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When fathers step in: Long-term consequences of early paternal involvement

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When Fathers Step In:
Long-Term Consequences of Early Paternal Involvement

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Abstract

We estimate the long-term impact of early paternal involvement by exploiting the 2002 Belgian paternity leave introduction as a natural experiment. Using a regression discontinuity design, we find that the reform significantly increased fathers' long-term time investment in childcare. Tracking children into early adulthood, we find precisely estimated null effects on a comprehensive set of outcomes, including educational attainment, labor market attachment, and family formation. These results hold across subgroups, including children of low and high-educated fathers. We conclude that while paternity leave may increase father involvement, it does not generate detectable advantages (or disadvantages) in children's early adult lives.

JEL Classification: J08, J13, J16, J18
Keywords : paternity leave, intergenerational effects, regression discontinuity design

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Previous research on parental investment in children has predominantly focused on mothers, a reflection of traditional caregiving roles. A number of studies have evaluated policy reforms dating back to the 1970s and 1980s (Baker & Milligan, 2010; Carneiro, Løken, & Salvanes, 2015; Dahl, Løken, Mogstad, & Salvanes, 2016; Danzer & Lavy, 2018; Dustmann & Schönberg, 2012; Ginja, Jans, & Karimi, 2020; Rasmussen, 2010). This body of literature has examined early maternal involvement triggered by paid maternity leave reforms, yielding a mixed picture of often limited impacts on child development. While Carneiro et al. (2015) found positive long-term effects in Norway,[1] other major studies report predominantly null results. Research in Austria (Danzer & Lavy, 2018),[2] Denmark (Rasmussen, 2010), Canada (Baker & Milligan, 2010), Germany (Dustmann & Schönberg, 2012), and on further expansions in Norway (Dahl et al., 2016) found no significant benefits for children's educational or early developmental outcomes. An exception highlighting nuanced intra-household effects comes from Sweden (Ginja et al., 2020), where benefits accrued only to older siblings.

Recent years have brought about a slow evolution in gender roles, with fathers increasingly dedicating more time to their children across countries (Altintas & Sullivan, 2017; Dotti Sani & Treas, 2016). As shown in Appendix Figure A1, data from the Multinational Time Use Study (Gershuny, Vega-Rapun, & Lamote, 2020) reveal that this increase has been primarily driven by college-educated fathers, who went from spending about 20 minutes per day on childcare in the 1960s to 80 minutes according to recent surveys. The introduction of paternity leave policies has accelerated this trend, and spurred a new literature seeking to understand the consequences of greater paternal engagement. Initial findings from existing research point to potential adverse

[1] A recent comment by Lillebø, Markussen, Røed, and Zhao (2024) raises important issues with this paper. Carneiro, Løken, and Salvanes (2024) present a revised analysis that yields similar results.

[2] Danzer and Lavy (2018) show that increased time with mothers in Austria did not harm children's PISA test scores, though effects diverged by maternal education. The subgroup effects have been found to be smaller in magnitude in a recent replication exercise by Troccoli (2024).

effects on young children's development (Avdic, Karimi, Sjögren, & Sundberg, 2023; Farré, González, Hupkau, & Ruiz-Valenzuela, 2024). Avdic et al. (2023) found negative impacts on boys' educational outcomes in Sweden, while Farré et al. (2024) reported detrimental effects on cognitive development among young children in Spain.[3] These results raise questions about the quality of paternal care and a potential lack of substitutability between mothers' and fathers' time that may worsen children's outcomes. Given these findings, a critical question emerges: Do public policies designed to increase fathers' childcare involvement have unintended consequences for their children's development and long-term outcomes?

To answer this question, we exploit the natural experiment provided by the 2002 Belgian paternity leave reform, combined with high-quality administrative data. The 2002 policy introduced a two-week, paid, and non-transferable leave for fathers, explicitly aiming to boost paternal involvement and promote a convergence of gender roles within the household. We show that the policy's take-up was high from its inception, with around 45% of new fathers utilizing it. Using data from the Belgian Time Use Survey conducted a decade after the reform, we provide evidence that the reform had a durable impact, significantly increasing the time fathers dedicated to daily childcare in the long run. This establishes the reform as a valid (and plausibly exogenous) shock to paternal involvement, allowing us to identify its causal effect on children's outcomes.

Leveraging population-level administrative data and a Regression Discontinuity Design (RDD) combined with a Difference-in-Differences (DiD) approach, we track a comprehensive set of outcomes for children born around the reform's implementation, who have since become adults and are 21 years old when observed in our data. Our analysis spans key life domains:

[3] Cools, Fiva, and Kirkebøen (2015) find that paternal leave quota in Norway improved school performance at age 16, but only for children of families where the father was more educated than the mother.

educational attainment (including middle school, high school completion, and PISA test scores), early labor market outcomes (employment and earnings), and family formation (cohabitation and teenage parenthood). Our data thus captures individuals at a formative stage, when early adult choices about career and family are being made. Exposure to different gender roles in childhood has the potential to significantly influence these decisions (Fernández, Fogli, & Olivetti, 2004).

In contrast to the adverse effects found in Spain and Sweden, our results consistently show no detrimental impact of increased father involvement. We find precisely estimated null effects across all outcomes. Our population-level dataset allows us to rule out even small effect sizes, such as a 1 percentage-point change in educational attainment or a 2 percent difference in earnings. Heterogeneity analyses further reveal that these null results hold even among children of less-educated or more 'traditional' fathers, as well as for the subgroups of high-educated and 'egalitarian' fathers who exhibited the largest increases in childcare time.

Our paper speaks to the emerging literature on the intergenerational effects of paternity leave (Avdic et al., 2023; Cools, Fiva, & Kirkebøen, 2015; Farré et al., 2024), demonstrating that its introduction does not necessarily produce negative spillovers on children. We extend previous analyses beyond education to include early labor market and family formation outcomes, capturing a broader picture of potential long-term impacts. We conclude that while the paternity leave reform successfully increased fathers' long-term involvement in childcare, this additional engagement did not create detectable advantages or disadvantages in their children's early adult lives.

More broadly, our paper contributes to the literature on the intergenerational transmission of preferences and skills (Black, Devereux, & Salvanes, 2005; Doepke & Zilibotti, 2017), and how increased time with children might be consequential (Zumbuehl, Dohmen, & Pfann, 2021). This body of work demonstrates that parents can shape their children's long-term outcomes through multiple channels. As argued by Doepke and Zilibotti (2017), this influence could operate both by molding children's preferences and by constraining their choice sets. Crucially, the effects of parental involvement may differ between mothers and fathers, as they can act as distinct role models, influencing educational attainment (Azam & Bhatt, 2015; Black et al., 2005; Humlum, Nandrup, & Smith, 2019; Philipp, 2023) and occupational choices (Lo Bello & Morchio, 2022), as well as health behaviors (Loureiro, Sanz-de-Galdeano, & Vuri, 2010; Thomas, 1994) or fertility decisions (Sipsma, Biello, Cole-Lewis, & Kershaw, 2010).

The remainder of the paper is structured as follows: Section I details the Belgian paternity leave reform and its long-term effects on fathers' time with children. Section II describes the data and empirical strategy. Section III presents the main results on the next generation's outcomes, and Section IV explores heterogeneity by fathers' education and family type. Section V offers concluding remarks.

I. Paternity leave and long-run fathers' involvement

A. The Belgian paternity leave reform

In July of 2002, Belgium introduced a two-week, job-protected, paid paternity leave for fathers. As shown in Appendix Figure A2, a substantial number of fathers utilized this policy following its introduction. The take-up rate among all new fathers hovered around 45% from the onset of the reform, with similar levels of uptake among both low and high-educated fathers (Panel A). We calculate the utilization rate as a share of all fathers, including those who were not eligible for the program. The reform initially applied only to salaried private-sector workers and contractual public-sector employees. Civil servants were covered under separate, parallel systems, and self-employed fathers did not gain access to a comparable paid paternity leave until 2019.

Prior to the 2002 reform, fathers were entitled to only 3 days of paid leave following the birth of a child, established by the "*Loi du 3 juillet 1978 relative aux contrats de travail*." This leave, known as "*Congé de circonstance"* was comparable to leave granted for events such as weddings or funerals, making it fundamentally different from the paternity leave introduced in 2002. The 2002 reform introduced an additional 7 working days of paternity leave, resulting in a total leave period of two weeks. A key feature of this leave is that it is an individual right of the father and is non-transferable to the mother.

Initially, fathers were required to take this leave within the first month after childbirth. This window was extended to 4 months in 2009, allowing fathers to schedule their leave after the mother's compulsory maternity leave. Regarding compensation, the first 3 days are fully paid by the employer, while the subsequent 7 working days are compensated by the Social Security at a rate of 82% of the father's gross salary.

This reform took place within a broader framework of Belgian family policies designed to support parents. Mothers already had access to a long-established maternity leave program, providing 15 weeks of paid leave since 1971. In addition to these leave entitlements, the system is characterized by a universal childcare allowance and the widespread availability of childcare services from a very young age. In the mid-2000s, for instance, Belgium's enrollment of children under three in formal childcare was more than 10 percentage points above the European average (ONE, 2009).

B. Consequences on paternal involvement with children

We now examine to what extent the Belgian paternity leave reform increased fathers' involvement in child-rearing. A growing body of empirical evidence demonstrates that even short paternity leave policies can effectively boost fathers' engagement in childcare in the long run (Farré & González, 2019; Kotsadam & Finseraas, 2011; Patnaik, 2019; Tamm, 2019).

To explore the long-term effects of the Belgian reform, we use data from the 2013 Time Use Survey (TUS), conducted a decade after the policy's introduction in July 2002. The full survey included 2,744 households. Our analysis focuses on the subsample of adults aged 18-55 who had a child born around the reform (between 1999 and 2005). This provides us with 310 individuals, 152 fathers and 158 mothers. Given the limited sample size, we employ a Difference-in-Differences design rather than a fully flexible Regression Discontinuity approach like in the rest of the paper. We compare parents of children born just before (April-June 2002) and just after (July-October 2002) the reform's cutoff, while accounting for potential seasonal effects by including parents of children born in non-reform years (1999-2001 and 2003-2005). The empirical strategy is detailed in Appendix B.

Our results, presented in Table 1, indicate that fathers eligible for paternity leave spent an additional 36 minutes per day on childcare—an effect that increases to 55 minutes in a Tobit specification. This represents more than double the time reported by ineligible fathers in the first column. We find no statistically significant changes in the time these fathers dedicated to other activities, such as work, sleep, or entertainment. Furthermore, as shown in Appendix Table A1, we observe no corresponding spillover effects on maternal childcare time. This suggests that the reform successfully increased paternal investment without displacing maternal involvement.

These findings are consistent with international evidence showing that paternity leave reforms have successfully increased the presence of fathers in the home after childbirth (Cools et al., 2015; Persson & Rossin-Slater, 2024), as well as their long-term involvement in childcare and household chores (Arnalds, Eydal, & Gíslason, 2013; Farré & González, 2019; Kotsadam & Finseraas, 2011; Pailhé, Solaz, & Tô, 2024; Patnaik, 2019; Tamm, 2019). For instance, Tamm (2019) found that German fathers who took paternity leave spent significantly more time with their families, with an additional 1.4 to 1.6 hours dedicated to childcare on weekends. Similarly, Farré and González (2019) reported that eligible Spanish fathers engaged in nearly an hour more of daily childcare. Research from France by Pailhé et al. (2024) also indicates that paternity leave led to a more equal division of child-rearing tasks, particularly among first-time parents.

Our estimates reinforce the international evidences that paternity leave policies can durably reshape household dynamics and promote greater fatherly involvement.[4] Crucially, we show that the Belgian paternity leave reform had a substantial and positive long-term effect on fathers' time investment in their children. The following sections explore the intergenerational consequences of this increased involvement.

II. Empirical strategy

A. Administrative data and sample

We exploit rich administrative data from Belgium, sourced from the *Banque Carrefour de la Sécurité Sociale* (BCSS). Through a data request to the *Comité sectoriel de la sécurité sociale*

[4] A recent study by Fontenay and González (2024) further substantiates the transformative effect of paternity leave on gender roles. Analyzing data from six European countries, including Belgium, we find that children whose fathers were eligible for paternity leave were significantly less likely to associate mothers exclusively with homemaking and fathers exclusively with careers in Implicit Association Tests.

et de la santé (Request 24/196), we obtained access to the complete population of individuals born in Belgium when the paternity leave reform was enacted (in 2002).

Our primary analysis focuses on the 2002 birth cohort, the year Belgium introduced paternity leave. To strengthen the empirical analysis and mitigate concerns related to birth seasonality, we extended the sample to include comparison cohorts from non-reform years—specifically, 1999, 2000, and 2001. The final estimation sample comprises 442,203 individuals born in Belgium between 1999 and 2002.

The key variable for determining exposure to the policy is the individual's birth date. The reform stipulated that only fathers of children born on or after July 1 of 200 were eligible for paternity leave. Due to data protection protocols, the *Comité sectoriel de la sécurité sociale et de la santé* provided only the year and month of birth, not the exact date. This limitation does not impede our analysis, as the reform took effect on the first day of the month. Consequently, we can classify individuals born in June 2002 as unexposed to a father on leave (control group) and those born in July 2002 as potentially exposed (treatment group), enabling a clear comparison across the policy cutoff.

We construct a comprehensive set of outcomes to assess the potential effects of increased father involvement on their children's future lives, spanning education, labor market outcomes, and family formation. Our data extend to December 2023, when all individuals in our primary sample had turned 21. This allows us to track early adult life decisions, a pivotal stage where exposure to different gender roles in childhood may influence consequential choices (Fernández et al., 2004).

Our education outcomes include middle school (8th grade) and high school (12th grade) completion. These correspond to two key milestones in the Belgian educational system: the "*Certificat d'Enseignement Secondaire du 1er Degré*" (typically earned at age 14) and the

"*Certificat de l'Enseignement Secondaire Supérieur*" (earned at age 18), which grants access to higher education. Due to delays in centralizing data between the Flemish and French-speaking communities, our dataset only covers educational records up to 2021. As a result, our sample was too young to have completed university studies, limiting our analysis to pre-tertiary education.

To complement these educational attainment measures, we use data from the Programme for International Student Assessment (PISA). Our primary data come from the 2018 wave, which tested 15-year-olds from the 2002 cohort. To control for birth-month seasonality in test scores, we also include data from the 2009, 2012, and 2015 waves, corresponding to the 1993, 1996, and 1999 birth cohorts. This allows us to measure competencies in reading, mathematics, and science, potentially detecting finer differences than grade completion alone.

Regarding labor market entry, we observe individuals' employment status in December 2023 and their quarterly earnings. Data preprocessing was minimal, as we primarily used the raw administrative records. One exception is quarterly earnings: due to data protection, the BCSS provides amounts in 180-euro bins. We multiply the reported bins by 180 to estimate quarterly earnings in euros. Given that the average quarterly earnings in our sample were approximately 7,000 euros, this procedure introduces only minimal measurement error.

We also track early family formation. Although our sample is relatively young (age 21), about 6% of women already have a child. We are particularly interested in capturing early fertility decisions, such as teenage motherhood, which are highly consequential for later life outcomes (Bronars & Grogger, 1994; Hotz, McElroy, & Sanders, 2005). We are also able to observe cohabitation with a partner. An advantage of the Belgian data is that individuals are required to register their residence, even if temporary. This allows us to identify couples even if they are not married or in a formal civil partnership. In our sample, about 15% of women were cohabiting with a partner at age 21.

Finally, we obtained data on the parents of individuals in our sample. We observe parental age and education level, which we use in heterogeneity analyses. Specifically, we distinguish between "traditional families," where the father is more than three years older and more educated than the mother, and "egalitarian families," where parents are of a similar age and educational level (or where the mother is more educated), following the classification used in prior literature (Folke & Rickne, 2020; González & Zoabi, 2021).

B. Regression Discontinuity Design

We employ a Regression Discontinuity Design (RDD) to analyze the impact of childhood exposure to a father on paternity leave. This approach exploits the sharp cutoff in Belgian legislation that made paternity leave available only to fathers of children born on or after July 1, 2002. Consequently, fathers of children born just before this date were ineligible for the newly introduced two-week leave. As outlined by Imbens and Lemieux (2008), the key identifying assumption of the RDD is that individuals cannot precisely manipulate the assignment variable. This assumption is plausible in our context, as the exact timing of birth is difficult to control. If this holds, then having a child just before or just after July 1 is effectively random, allowing for a causal comparison. These considerations motivate the estimation of the following RDD model:

$$(1)\quad y_i = \alpha + 1[t_i \geq c]\beta + 1[t_i \geq c] \cdot f_r(t - c, \gamma_r) + 1[t_i < c] \cdot f_l(c - t, \gamma_l) + \epsilon_i$$

Here, y_i is the outcome of interest for an individual i born in month t; c is the reform cutoff month (July); $1[\cdot]$ is an indicator function; and f_l and f_r are flexible functions that capture trends in the outcome on either side of the cutoff, with parameter vectors γ_l and γ_r. The coefficient β represents the estimated discontinuity at the cutoff. Under the assumption that parents lack precise control over birth dates around July 1, we can interpret β as the causal effect of the paternity leave reform.

All our specifications estimate Intent-to-Treat (ITT) effects. As Appendix Figure A2 shows, approximately 45% of fathers used the paternity leave immediately after the reform took place. Therefore, our ITT estimates represent a lower bound, and the effects on the treated fathers would be proportionally about twice larger. Our main specification uses linear polynomial regressions with a uniform kernel (i.e. no weighting), applied to a sample of children born in a six-month window around July 2002.

A remaining concern is the potential seasonality of our outcomes. Prior literature demonstrates that parents who give birth in different seasons differ systematically in their socioeconomic characteristics; for instance, Buckles and Hungerman (2013) find that younger, less-educated, and unmarried women are more likely to have winter births (in the US). Because these characteristics are very likely to correlate with children' outcomes, failing to account for birth month seasonality could bias our estimates. To address this, we combine our RDD with a Difference-in-Differences (DiD) approach, following other studies on parental leave (Avdic & Karimi, 2018; Bütikofer, Riise, & Skira, 2021; Danzer & Lavy, 2018; Dustmann & Schönberg, 2012; Lalive, Schlosser, Steinhauer, & Zweimüller, 2014). We apply this combined RDD-DiD framework to data from the reform year (2002) and three pre-reform years (1999-2001). This strategy is valid under the common trends assumption that the seasonal patterns in outcomes would have been similar in 2002 in the absence of the reform. Specifically, we extend the basic RDD model by incorporating pre-reform years and fully interacting the model with a reform year indicator (R), which equals 1 for 2002 and 0 otherwise. This expanded model also includes fixed effects for each pre-reform year λ_n :

$$(2) \quad y_i = \alpha + \sum_{s=0}^{1} 1[R_i = s] \cdot \{1[t_i \geq c]\beta_s + 1[t_i \geq c] \cdot f_r(t - c, \gamma_{rs}) + 1[t_i < c] \cdot f_l(c - t, \gamma_{ls})\} + \lambda_n + \epsilon_i$$

In this specification, our coefficient of interest is β_1, which captures the interaction between being born after July 1 and the 2002 reform year. This coefficient identifies the causal effect of the policy by comparing the discontinuity in 2002 to the seasonal patterns observed in the pre-reform years, thereby controlling for systematic differences between children born in different months.

C. Identifying assumptions

Before presenting our results, we assess the validity of our identification strategy. Our design hinges on the premise that assignment to treatment or control groups is as good as random for individuals born just before and just after the introduction of paternity leave on July 1, 2002. We test this by examining three key assumptions.

First, the RDD requires that no other concurrent policies or significant events at the cutoff date could confound our estimates. We are not aware of any such confounding factors in the Belgian context that coincided with the introduction of paternity leave in 2002.

Second, we test for potential manipulation of the assignment variable (the birth date). While the timing of natural births is difficult to control, parents could potentially schedule cesarean sections or induced labor to gain eligibility for the new benefit. Such "introduction effects" have been documented in other contexts (Gans & Leigh, 2009). Given that the paternity leave law was passed in August 2001, families had a window of a few months to potentially adjust birth timing.

To test for this, we use data from Statbel, the Belgian statistical office, on the number of daily births in 2002. This allows us to detect manipulation around the exact date, which is not possible with our monthly administrative data. Appendix Figure A3 provides graphical evidence, showing no abnormal bunching of births around the July 1 threshold. The variation observed is consistent with typical weekly patterns, such as fewer births on weekends. July 1 of 2002 was a Monday, which mechanically explains the higher number of births compared to the preceding weekend. We formally test for sorting by estimating regressions where the outcome is the log number of daily births. We control for day-of-the-week fixed effects and a linear trend. The results, presented in Appendix Table A2, show no statistically significant discontinuity in birth rates across various window sizes around the cutoff (from 7 to 42 days). This confirms that families did not systematically manipulate birth timing to gain eligibility for paternity leave.

Finally, we test for balance in pre-determined parental characteristics around the reform cutoff. We apply our main RDD-DiD model from equation (2) to a range of observable variables related to parental age and education. The results in Appendix Table A3 show that the vast majority of coefficients are statistically indistinguishable from zero. Out of 18 characteristics tested, only three ("Mother primary education," "Mother older than father," and "Mother more educated than father") show statistically significant differences. However, given our large population-level data, these tiny differences (less than one percentage point) are likely detected by chance and are too small to be substantively meaningful.

We extend this analysis to post-reform parental labor market outcomes, such as earnings, work volume, and job type. Appendix Table A4 shows no meaningful discontinuities across 20 outcomes for both fathers and mothers. This aligns with the existing literature, which finds limited long-run effects of paternity leave on parents' careers (Andresen & Nix, forthcoming; Ekberg, Eriksson, & Friebel, 2013; Kleven, Landais, Posch, Steinhauer, & Zweimüller, forthcoming). We can therefore conclude that parents on either side of the cutoff were similar in their pre-determined characteristics and that the reform itself did not create long-run divergences in their career trajectories. The primary difference introduced by the reform was a change in father involvement with children, without significant spillovers onto parental careers.

III. Consequences of early paternal involvement for children

This section analyzes the long-run effects of early paternal involvement on children's life outcomes, focusing on three key domains: educational attainment, labor market outcomes, and early family formation. We begin by presenting visual evidence of the policy's impact through RDD graphs (Figure 1, Figure 2 and Figure 3), which plot the trends of the outcomes across the July 2002 cutoff. We then present our formal RDD-DiD estimates in Table 2, which control for potential seasonal confounding and provide our main causal inferences.

A. Educational attainment

We begin our analysis with educational outcomes, following the chronological progression of the Belgian system. The first milestone is middle school completion (the "*Certificat d'Enseignement Secondaire du 1er Degré*"), typically attained at age 14. Figure 1 (Panel A) shows that this certificate is achieved by about 85% of our cohort born in 2002. A slight downward trend in completion rates is visible from the beginning to the end of the year. This recurring pattern across all our outcomes is consistent with the established literature on birth-season effects, where relatively older children in a cohort often demonstrate advantages in health (Banegas, Rodriguez-Artalejo, Graciani, & De La Cruz, 2001), cognitive skills (Yamaguchi, Ito, & Nakamuro, 2023) and labor market performance (Larsen & Solli, 2017), and which may also reflect underlying differences in parental socioeconomic status (Buckles & Hungerman, 2013).[5]

[5] For high school completion (Panel B of Figure 1), the more pronounced downward trend for later birth months also reflects a data limitation: as explained in subsection II.A, our educational records end in 2021, when the 2002

A visual inspection of Panel A in Figure 1 reveals no apparent discontinuity at the July 2002 policy cutoff. We formalize this finding using our RDD-DiD model, which controls for seasonal patterns using pre-reform cohorts from 1999-2001. As shown in Column 1 of Table 2, the estimated effect of paternal leave eligibility for paternity leave on middle school completion is a precisely estimated zero. The point estimate is -0.0035 (a 0.35 percentage point decrease) with a standard error of 0.006. The 95% confidence interval, ranging from -1.5 to 0.8 percentage points, allows us to rule out both meaningfully negative and positive effects.

The coefficient is larger for women (Panel C) than for men (Panel B), indicating a 1.4 percentage point reduction for the former, significant only at the 10% level. However, this potential effect does not persist at the next educational stage. For high school completion in the second column of Table 2, we find precisely estimated null effects for both men and women, with coefficients very close to and statistically indistinguishable from zero (0.014 and 0.002, respectively).

Since completion outcomes may mask more nuanced differences in skills, we turn to the 2018 PISA test, which directly assesses the cognitive abilities of our 2002 cohort at age 15. As shown in Appendix Figure A4 and Appendix Table A5, we find no evidence that exposure to paternal leave affected student competencies in mathematics, reading or science. The estimates allow us to confidently rule out even small effects on these standardized test scores.

In summary, we conclude that the paternity leave reform, triggering an increase in father involvement, had no discernible impact on children's educational attainment up to the end of high school or on their performance in the PISA test. While our data cannot speak to tertiary education, the "*Certificat de l'Enseignement Secondaire Supérieur*" is a mandatory gateway to

cohort was only 19. Students born later in the year are more likely to have delayed school entry, and thus may not have graduated by this cutoff. Our RDD-DiD design explicitly accounts for this age-grading phenomenon by incorporating pre-reform cohorts (1999-2001) and measuring completion at the same age, that is 19 years old.

higher education. Our null results on this key outcome indicate that the reform did not affect the probability of qualifying for university or other higher education institutions.

B. Labor market outcomes

We next examine labor market outcomes. A well-established literature posits that fathers serve as influential role models, shaping their children's career choices and economic outcomes (Aina & Nicoletti, 2018; Bowles & Gintis, 2002), with some studies suggesting a particularly strong influence on sons (Laband & Lentz, 1983; Lo Bello & Morchio, 2022). We test whether the increased father involvement prompted by the paternity leave reform had an early, measurable impact on this domain. Although our cohort was only 21 years old in 2023, early labor market decisions, such as forgoing higher education for employment or choosing specific career paths, are highly consequential and may already be observable.

We measure labor market participation using two outcomes: the probability of being employed in December 2023, and quarterly earnings conditional on employment. Figure 2 plots these outcomes against the birth month. We observe that approximately 30% of our 2002 cohort was employed at this age. A visual inspection reveals no apparent discontinuity at the July 2002 cutoff.

The formal RDD-DiD estimates in Table 2 confirm this null result (columns 3 and 4). For employment, we find a precisely estimated zero effect. The coefficient is 0.0008 (a 0.08 percentage point increase, SE=0.0076). The 95% confidence interval, ranging from -1.4 to 1.6 percentage points, allows us to rule out both meaningfully negative and positive effects. The analysis of quarterly earnings reveals a similar pattern. The point estimate indicates a negligible increase of €28.5 (SE=88.0), which represents just 0.4% of the average quarterly earnings (€7,050.5), and which is statistically indistinguishable from zero.

In summary, we find no evidence that early exposure to a father on paternity leave affected early-career labor market outcomes. The reform did not alter the probability of employment or the level of early earnings for either sons or daughters. This suggests that the policy-induced increase in father involvement did not create early advantages or disadvantages in their children's labor market trajectories.

C. Family formation

Finally, we consider outcomes related to family formation. Our motivation relies on previous research showing how fathers might influence their children's partnering (Jennings, Axinn, & Ghimire, 2012) or fertility decisions (Högnäs & Carlson, 2012; Tanskanen & Danielsbacka, 2021), including teenage parenthood (Sipsma et al., 2010).

We measure early family formation using two outcomes observed by age 21: the probability of cohabiting with a partner and the probability of having a child. Although our sample is relatively young, these decisions concern a non-negligible share of individuals, and are highly consequential for later life outcomes (Bronars & Grogger, 1994; Hotz et al., 2005). Figure 3 plots these outcomes against the birth month. We observe that approximately 5% of the 2002 cohort was cohabiting with a partner, and about 2% already had a child (the share is much larger for women, with 14.7% of the whole sample already partnered and 5.7% having a child, according to Table 2). A visual inspection reveals no apparent discontinuity at the July 2002 policy cutoff for either outcome.

The formal RDD-DiD estimates in Table 2 confirm this null result (columns 5 and 6). For cohabitation with a partner, the estimated coefficient is -0.0006 (a 0.06 percentage point decrease), with a standard error of 0.0047. For the probability of having a child, the coefficient is -0.0031 (a 0.31 percentage point decrease), with a standard error of 0.0030. Both estimates are statistically indistinguishable from zero. The 95% confidence intervals are sufficiently tight to rule out meaningfully positive or negative effects on these early family formation decisions. The results are also precisely estimated zero for both men and women when analyzed separately (Panels B and C).

In summary, we find no evidence that early exposure to a father on paternity leave affected partnership or fertility decisions. The reform did not alter the likelihood of cohabiting with a partner or becoming a parent by age 21 for either sons or daughters. This suggests that the policy-induced increase in father involvement did not accelerate or delay these pivotal life choices.

IV. Heterogeneity analysis

In this final section, we leverage our large-scale administrative data to explore potential heterogeneous effects of the paternity leave reform based on pre-determined household characteristics. We first distinguish between households where fathers have low (primary or secondary) versus high (tertiary) education levels. We then categorize families as "traditional," where the father is more than three years older and more educated than the mother, or "egalitarian," where parents are of similar age and education (or the mother is more educated), following previous authors (Folke & Rickne, 2020; González & Zoabi, 2021).

As shown in Appendix Figure A2, take-up of paternity leave was similar among low- and high-educated fathers, but approximately five percentage points higher in egalitarian (48%) versus traditional families (43%). Appendix Table A6 further reveals that the long-term increase in childcare time was driven by high-educated and egalitarian fathers. If paternal involvement were to affect children's outcomes, we would therefore expect to observe it most clearly within these subgroups.

However, the results in Appendix Table A7 and Table A8 indicate no such effects. For children of high-educated fathers (Appendix Table A7), we find precisely estimated null effects across all outcomes—education, employment, earnings, partnership, and fertility (including PISA score in Appendix Table A9). Similarly, Appendix Table A8 shows no statistically significant impacts for children raised in either traditional or egalitarian households. The confidence intervals rule out meaningful effect sizes in both subgroups.

These null findings suggest that even among fathers who responded most to the reform by increasing their long-term involvement, this additional engagement did not translate into detectable advantages or disadvantages in their children's early adult outcomes. This may indicate that the quality of paternal care provided by these fathers, while greater in quantity, did not differ substantively from the alternatives (such as maternal care or formal childcare) in ways that alter the developmental trajectories that we measure.

V. Conclusions

Based on our analysis of Belgium's 2002 paternity leave reform, we conclude that, while the policy successfully increased fathers' long-term involvement in childcare, this higher engagement did not produce detectable advantages (or disadvantages) in their children's outcomes all the way through adulthood.

We first confirm that the reform had a durable impact fathers' involvement, with eligible fathers spending significantly more time on daily childcare a decade after the policy's implementation. We leverage this exogenous shock to paternal involvement to estimate its causal intergenerational effects.

Tracking the children into their early twenties revealed a consistent pattern of null effects across a wide range of critical life domains. In terms of educational attainment, the reform had no impact on the likelihood of completing middle school or high school, nor on performance in standardized PISA tests for reading, mathematics, and science. Similarly, early labor market outcomes at age 21 (including employment status and quarterly earnings) were unaffected. Finally, early family formation decisions, such as the probability of cohabiting with a partner or having a child, also displayed no significant change.

We believe that these precisely estimated null results hold important implications. First, they challenge the findings from recent studies in Spain and Sweden, which reported negative effects on child development from increased paternal leave (Avdic et al., 2023; Farré et al., 2024). Our research shows that, when combining population-level administrative data with a long-run perspective, we can rule out even small effects on crucial life outcomes. Second, extensive heterogeneity analyses revealed that the null effects persist across different family types. Even among children of highly educated and "egalitarian" fathers (the subgroups that exhibited the largest increases in childcare time) no meaningful effects on child outcomes were detected.

In conclusion, we provide robust evidence that non-transferable paternity leave can effectively promote greater gender equality in childcare responsibilities within households without harming child development in the long run. This may indicate that the quality of the additional paternal care was comparable to that of maternal care or formal childcare. Verifying this mechanism is an important avenue for future research.

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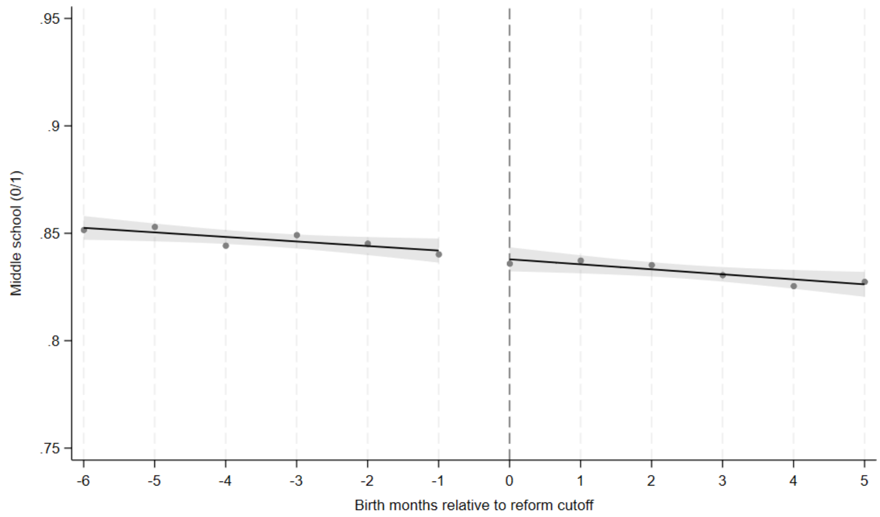
Panel A: Middle school completion (8th grade)



Panel B: High school completion (12th grade)

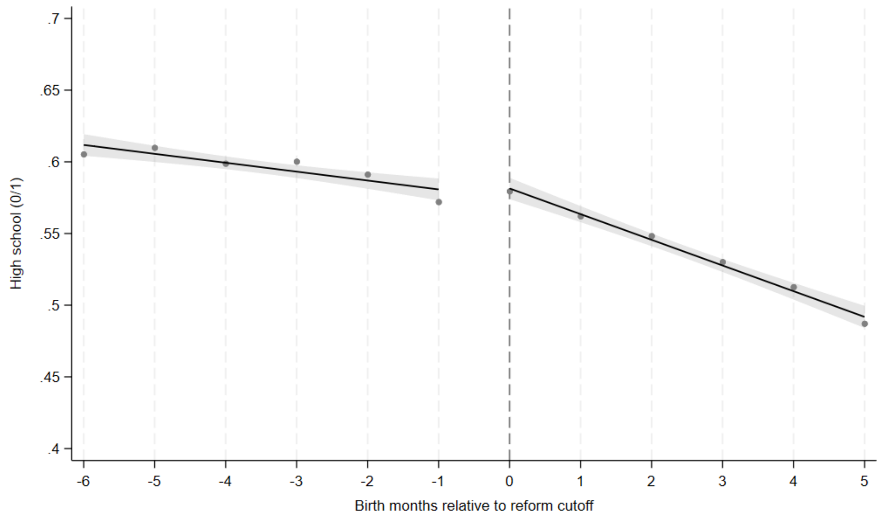
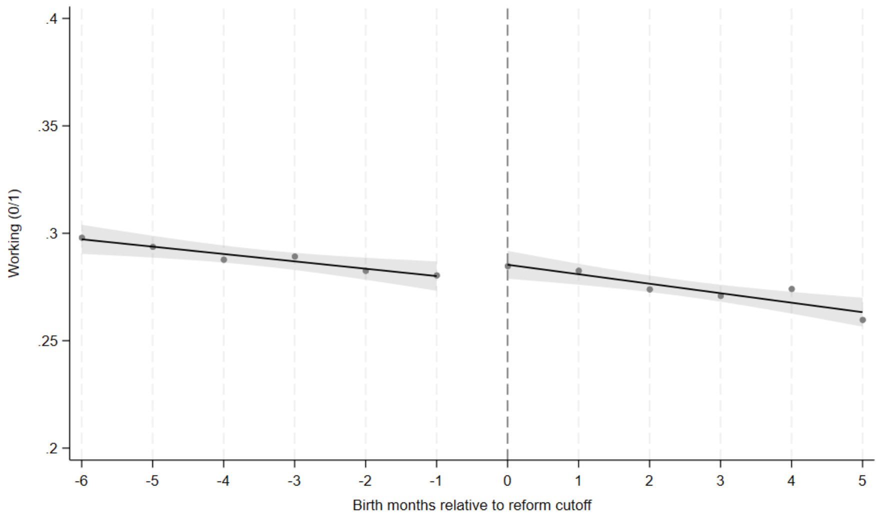


Figure 1: Local averages for grade completion by birth month

Notes: Average value by birth month. The vertical bar corresponds to the reform cutoff, normalized to 0 in July. The trends on each side of the cutoff are from linear regressions. Population born in 2002 in Belgium. The shaded area indicates the 95% confidence interval.

Panel A: Labor force participation (0/1)



Panel B: Quarterly earnings (euros)

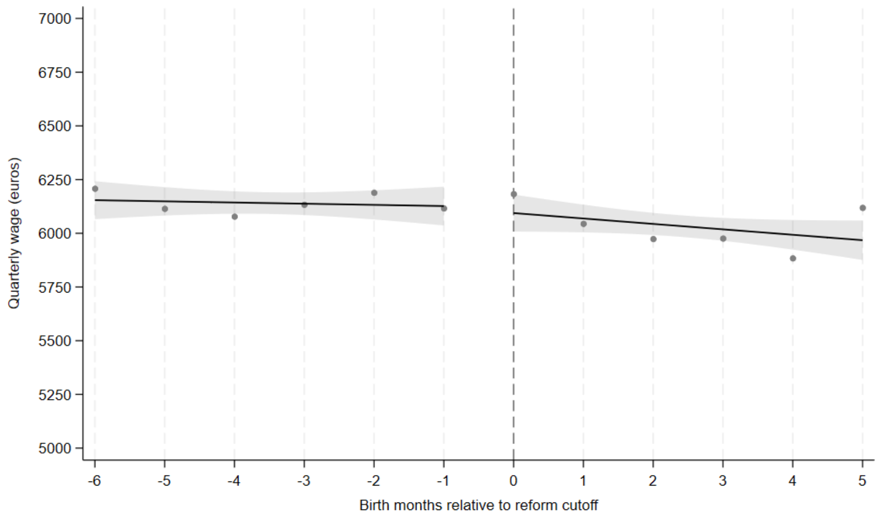
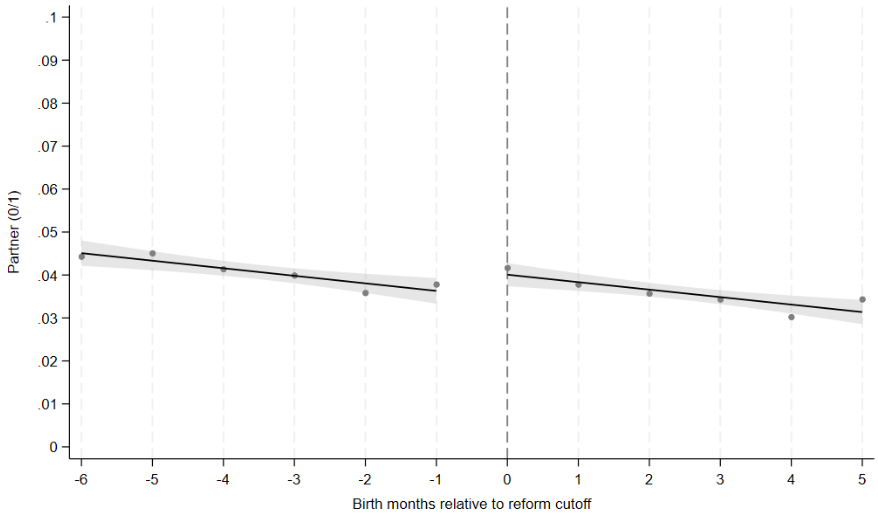


Figure 2: Local averages for labor market outcomes by birth month

Notes: Average value by birth month. The vertical bar corresponds to the reform cutoff, normalized to 0 in July. The trends on each side of the cutoff are from linear regressions. Population born in 2002 in Belgium. The shaded area indicates the 95% confidence interval.

Panel A: Currently living with a partner (0/1)



Panel B: Fertility - Have at least one child (0/1)

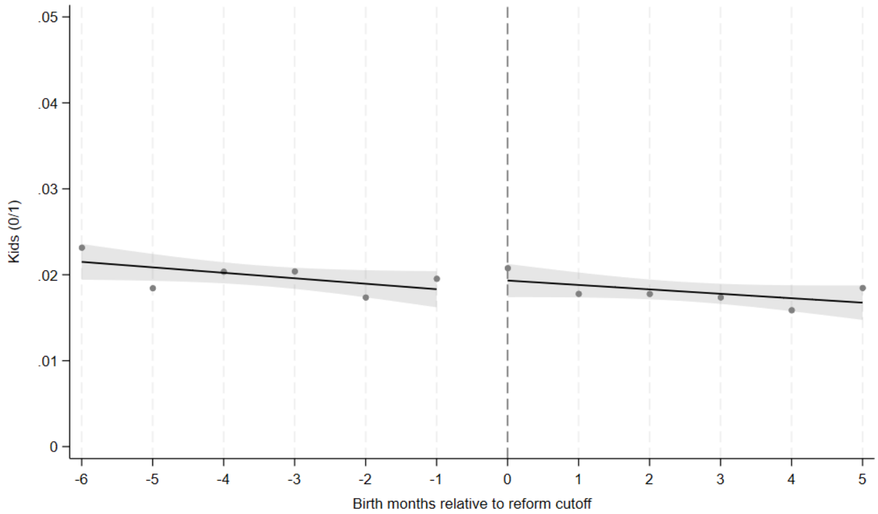


Figure 3: Local averages for family formation outcomes by birth month

Notes: Average value by birth month. The vertical bar corresponds to the reform cutoff, normalized to 0 in July. The trends on each side of the cutoff are from linear regressions. Population born in 2002 in Belgium. The shaded area indicates the 95% confidence interval.

Table 1: Time Use Survey - Fathers

	Fathers		
	Mean	OLS	Tobit
Childcare	**41.8**	**36.53 ***	**54.69 ****
		(20.65)	**(26.84)**
Sleeping	501.2	9.99	9.99
		(39.14)	(35.10)
Eating	106.7	4.37	4.37
		(22.51)	(19.71)
Work & social activities	297.2	0.98	-8.67
		(77.94)	(71.11)
Entertainment	246.9	-10.04	-10.04
		(63.36)	(54.45)
Residual	246.2	-41.83	-41.83
		(60.20)	(52.06)
Number of observations		152	152

Notes: The time dedicated to each activity is in minutes per day and comes from the 2013 Belgian Time Use Survey. This table reports DiD estimates based on equation (3). Regressions includes dummy variables which control for the fact to be employed, student, homemaker, disabled, unemployed, as well as Belgian or foreign born. The sample includes parents who had a first child between 1999 and 2005. Standard errors are reported in parentheses..

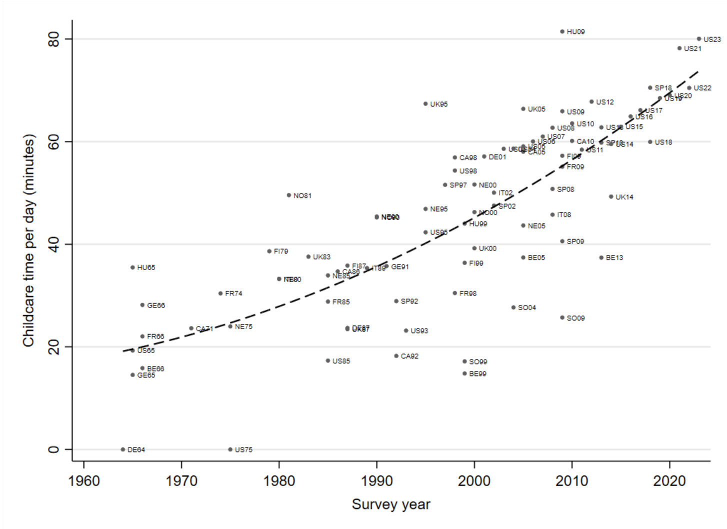
Table 2: Impact of the reform on the next generation's outcomes

	Middle school (0/1)	High school (0/1)	Working (0/1)	Quarterly earnings (euros)	Partner (0/1)	Children (0/1)
Panel A: All						
Coef.	-0.0035	0.0083	0.0008	28.5525	-0.0006	-0.0031
SE	(0.0062)	(0.0081)	(0.0076)	(88.0320)	(0.0047)	(0.0030)
Obs.	419863	419863	442913	177769	442913	442913
Mean	0.8235	0.5541	0.4714	7050.5457	0.1108	0.0398
Panel B: Men						
Coef.	0.0061	0.0140	0.0150	115.9023	-0.0025	0.0014
SE	(0.0091)	(0.0114)	(0.0108)	(116.8548)	(0.0056)	(0.0032)
Obs.	213654	213654	225914	95115	225914	225914
Mean	0.7995	0.4884	0.4976	7318.4142	0.0768	0.0236
Panel C: Women						
Coef.	-0.0137 *	0.0020	-0.0132	-133.8327	0.0010	-0.0079
SE	(0.0083)	(0.0113)	(0.0107)	(132.7315)	(0.0076)	(0.0051)
Obs.	206209	206209	216289	82654	216289	216289
Mean	0.8482	0.6219	0.4456	6749.7405	0.1466	0.0569

*Notes: This table reports RDD-DiD estimates based on equation (2) and using the reform (2002) and non-reform years (1999-2001). Standard errors are reported in parentheses. The last raw of each panel reports the mean of the outcome for the individuals born before the reform. ***p < 0.01,**p < 0.05, *p < 0.1.*

Appendix A

Panel A: High-educated fathers



Panel B: Low-educated fathers

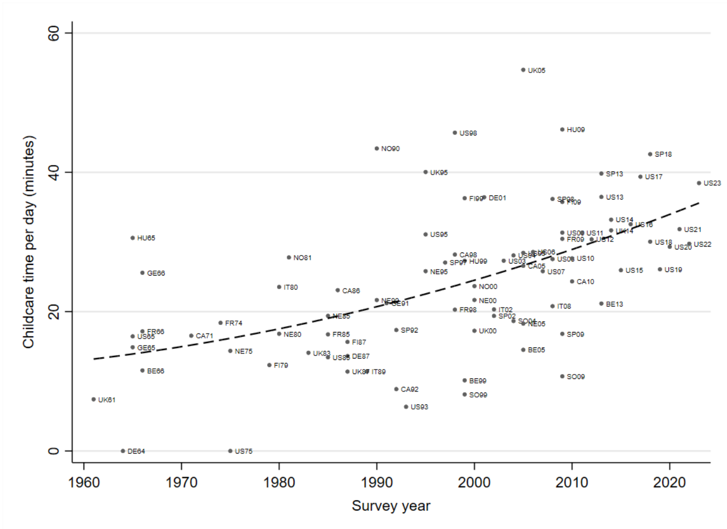
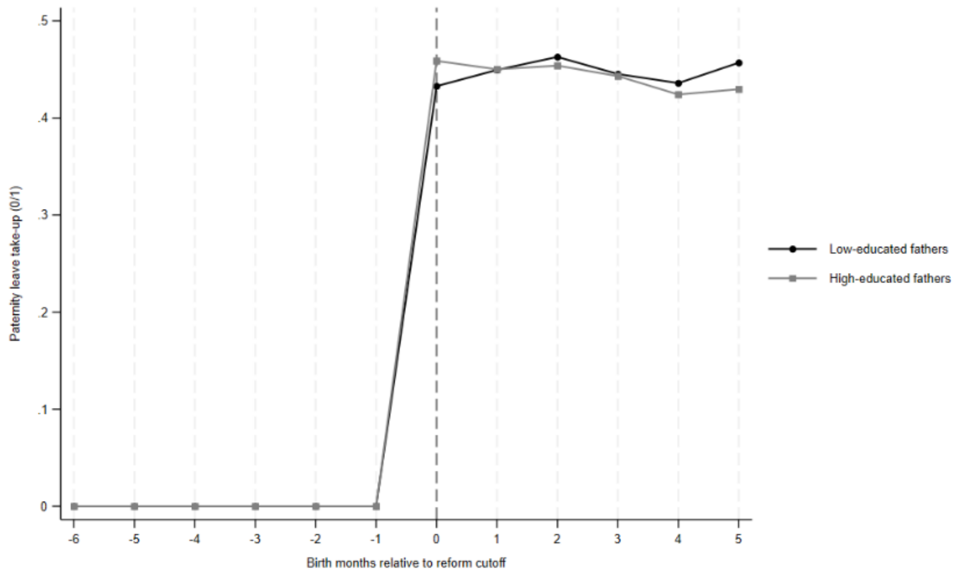


Figure A1: Trend in paternal childcare from Multinational Time Use Surveys

Notes: This figure presents data from the Multinational Time Use Study (Gershuny et al., 2020), which standardizes time-use diary data from national surveys. Each dot represents a survey wave from one of the 14 included countries for which at least two waves are available (Belgium, Canada, Denmark, Finland, France, Germany, Hungary, Italy, South Korea, Netherlands, Norway, Spain, United Kingdom, and the United States). The graphs plot the average daily minutes men spend on childcare activities (including physical care, medical care, supervision, routine care, play/sports, reading/talking, and helping with homework) against the survey year. The sample is stratified by fathers' education level: high-educated (college degree or equivalent) and low-educated (less than college). The trend lines illustrate the evolution of paternal childcare time over time for each group.

Panel A: Comparison between low and high-educated fathers



Panel B: Comparison between 'traditional' and 'egalitarian' fathers

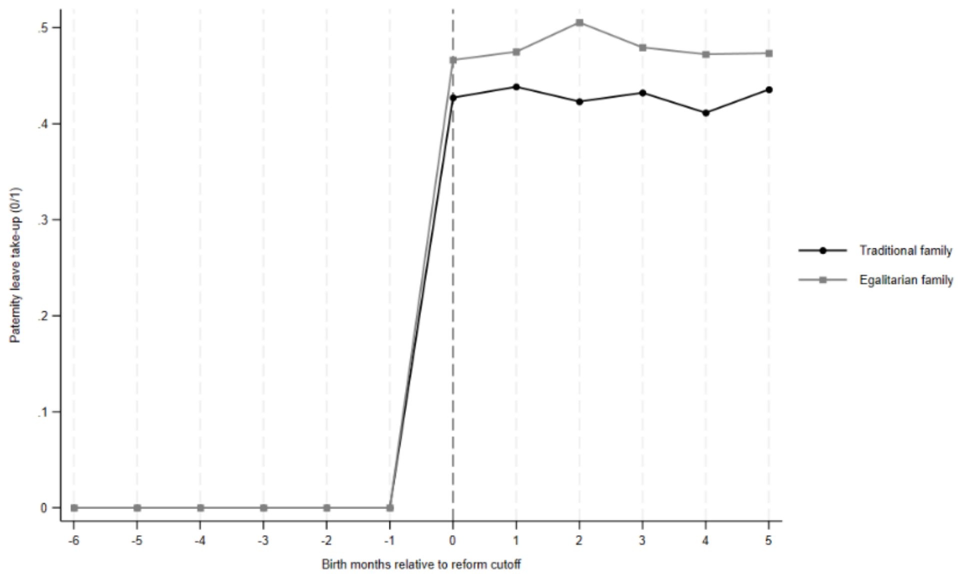


Figure A2: Local averages for paternity leave take-up by birth month

Notes: Average value by birth month. The vertical bar corresponds to the reform cutoff, normalized to 0 in July. The trends on each side of the cutoff are from linear regressions. Population of fathers with a child born in 2002 in Belgium.

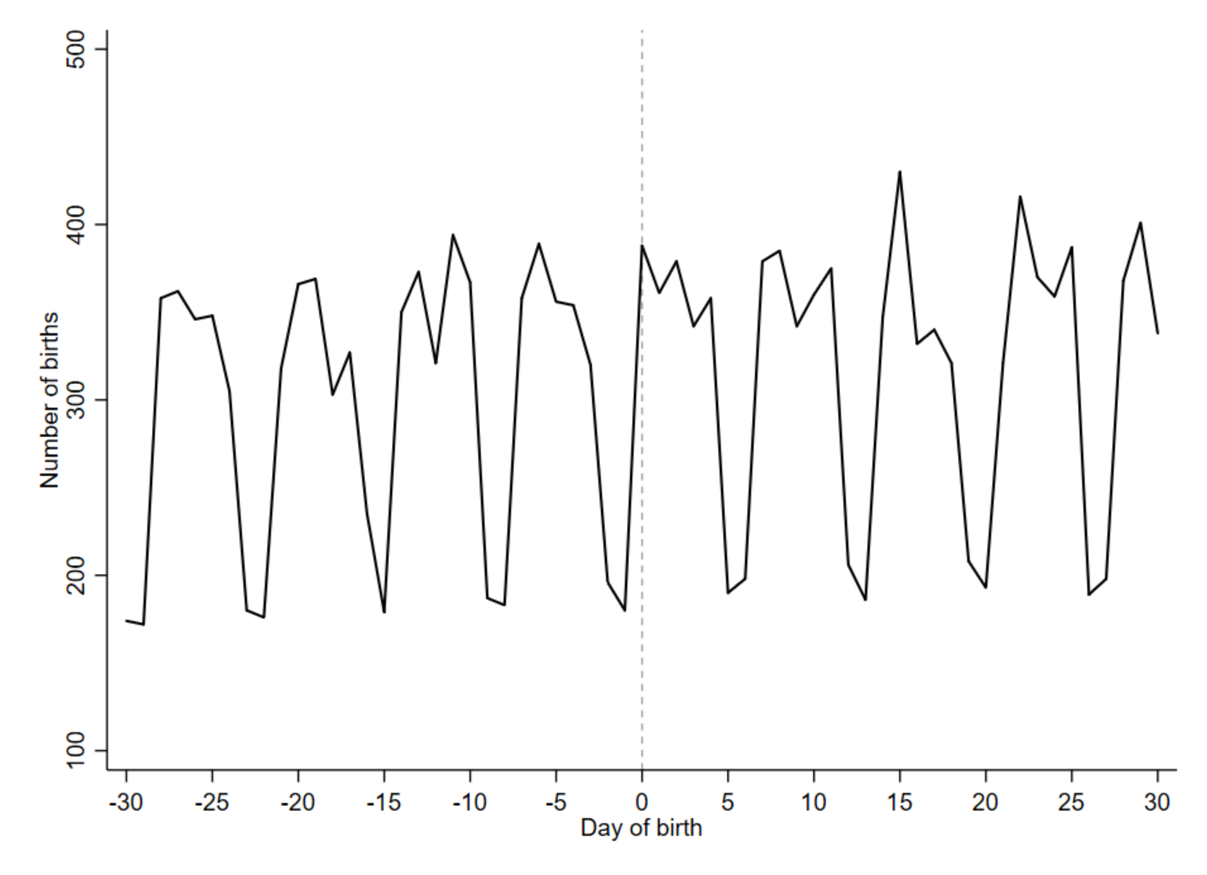
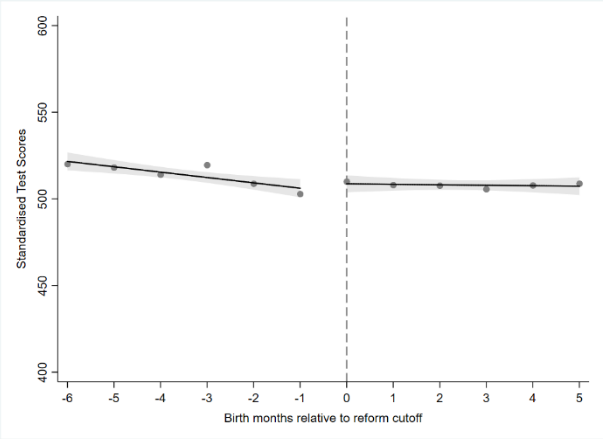


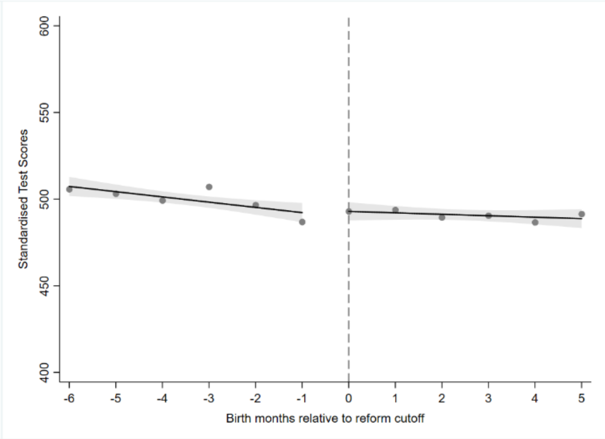
Figure A3: Daily number of births around reform cutoff

Notes: Daily number of births around the introduction of paternity leave. The day of birth is normalized to 0 for July 1st, 2002. Data source: Belgian statistical office – StatBel.

Panel A: Mathematics



Panel B: Reading



Panel C: Science

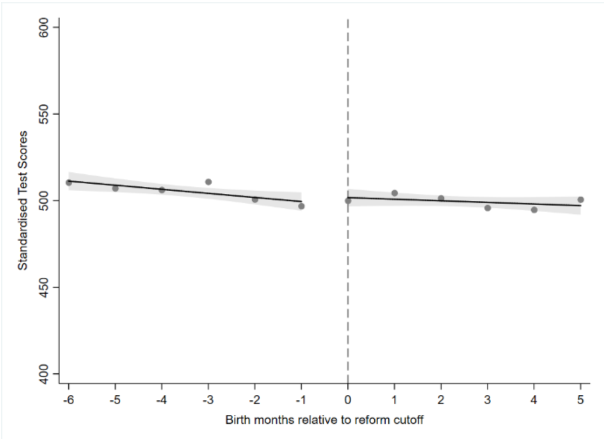


Figure A4: Local averages for PISA score by birth month

Notes: Average value by birth month. The vertical bar corresponds to the reform cutoff, normalized to 0 in July. The trends on each side of the cutoff are from linear regressions. Population born in 2002 in Belgium. The shaded area indicates the 95% confidence interval.

Table A1: Time Use Survey - Mothers

		Mothers	
	Mean	OLS	Tobit
Childcare	**71.8**	**-10.18**	**-8.44**
		(25.31)	**(29.69)**
Sleeping	525.8	45.68	45.68
		(34.83)	(28.62)
Eating	100.2	12.75	12.75
		(18.23)	(16.70)
Work & social activities	217.5	-54.91	-45.08
		(63.92)	(55.54)
Entertainment	170.7	15.49	14.87
		(42.08)	(41.10)
Residual	354.0	-8.83	-8.83
		(37.73)	(41.84)
Number of observations		158	158

Notes: The time dedicated to each activity is in minutes per day and comes from the 2013 Belgian Time Use Survey. This table reports DiD estimates based on equation (3). Regressions includes dummy variables which control for the fact to be employed, student, homemaker, disabled, unemployed, as well as Belgian or foreign born. The sample includes parents who had a first child between 1999 and 2005. Standard errors are reported in parentheses.

Table A2: Manipulation tests

Window	7 days	14 days	21 days	28 days	35 days	42 days
Log n. of births	0.030	0.029	0.032	0.013	-0.013	0.012
	(0.028)	(0.051)	(0.041)	(0.031)	(0.028)	(0.036)
Linear trend	N	Y	Y	Y	Y	Y
Day of the week	Y	Y	Y	Y	Y	Y
Number of observations	14	28	42	56	70	84

*Notes: This table reports RDD estimates from regressions of the form of equation (1). The outcome variable is the log daily number of births. The reported coefficients are from a binary indicator for birthdates on or after July 1st, 2002. The sample includes all days in the specified window around the date of the introduction of the paternity leave. In all but the first column, we control for a linear trend in date of birth (i.e. the running variable, centered at 0 in July 1st, 2002), interacted with the binary indicator. Standard errors are reported in parentheses. Data source: Belgian statistical office - StatBel. *** p<0.01, ** p<0.05, * p<0.1.*

Table A3: Balancing tests for parents' pre-determined characteristics

	Coef. /SE	Mean	Obs.
Father's age at childbirth	-0.0829	32.4194	394717
	(0.0925)		
Mother's age at childrbith	0.0562	29.5413	414946
	(0.0792)		
Father no formal education (0/1)	-0.0028	0.0167	388613
	(0.0022)		
Father primary education (0/1)	-0.0004	0.0622	388613
	(0.0041)		
Father lower secondary education (0/1)	-0.0088	0.1711	388613
	(0.0065)		
Father upper secondary education (0/1)	0.0092	0.3673	388613
	(0.0081)		
Father tertiary education (0/1)	0.0029	0.3826	388613
	(0.0081)		
Mother no formal education (0/1)	-0.0027	0.0163	408662
	(0.0021)		
Mother primary education (0/1)	-0.0085 **	0.0501	408662
	(0.0036)		
Mother lower secondary education (0/1)	-0.0073	0.1305	408662
	(0.0058)		
Mother upper secondary education (0/1)	0.0064	0.3285	408662
	(0.0078)		
Mother tertiary education (0/1)	0.0121	0.4745	408662
	(0.0082)		
Father older than mother (0/1)	-0.0077	0.3141	442913
	(0.0073)		
Mother older than father (0/1)	0.0062 **	0.0366	442913
	(0.0030)		
Parents have same age ± 3 years (0/1)	0.0066	0.5294	442913
	(0.0079)		
Father more educated than mother (0/1)	-0.0057	0.2078	442913
	(0.0065)		
Mother more educated than father (0/1)	0.0132 *	0.3031	442913
	(0.0072)		
Parents have same education (0/1)	-0.0031	0.3468	442913
	(0.0075)		

Notes: Column 1 reports results from RDD-DiD regressions based on equation (2) and using the reform (2002) and non-reform years (1999 -2001), with standard errors in parentheses. Column 2 reports the mean of the outcome for parents who had a child before the reform.

Table A4: Impact of the reform on parents' long run outcomes

	Coef. / SE	Mean	Obs.
Father is working (0/1)	0.0002	0.7501	424664
	(0.0071)		
Father is salaried employee (0/1)	-0.0038	0.7418	310913
	(0.0082)		
Father's quarterly earnings (euros)	47.9162	12633.4	229890
	(144.2978)		
Father is bluecollar (0/1)	-0.0105	0.3875	229890
	(0.0106)		
Father is white collar (0/1)	-0.0040	0.4390	229890
	(0.0108)		
Father is civil servant (0/1)	0.0145 *	0.1736	229890
	(0.0082)		
Father works fulltime (0/1)	0.0014	0.8518	229890
	(0.0080)		
Father is self employed (0/1)	0.0028	0.2518	310913
	(0.0081)		
Father is job seeker (0/1)	-0.0012	0.0284	424664
	(0.0026)		
Father is out of labor force (0/1)	0.0011	0.2215	424664
	(0.0069)		
Mother is working (0/1)	0.0120 *	0.7328	424664
	(0.0072)		
Mother is salaried employee (0/1)	-0.0049	0.8592	307010
	(0.0066)		
Mother's quarterly earnings (euros)	165.1346	10042.4	263058
	(119.3641)		
Mother is bluecollar (0/1)	-0.0001	0.1969	263058
	(0.0083)		
Mother is white collar (0/1)	0.0137	0.5719	263058
	(0.0101)		
Mother is civil servant (0/1)	-0.0137	0.2313	263058
	(0.0085)		
Mother works fulltime (0/1)	0.0091	0.4429	263058
	(0.0101)		
Mother is self employed (0/1)	0.0020	0.1284	307010
	(0.0064)		
Mother is job seeker (0/1)	-0.0006	0.0264	424664
	(0.0026)		
Mother is out of labor force (0/1)	-0.0114	0.2408	424664
	(0.0070)		

Notes: Column 1 reports results from RDD-DiD regressions based on equation (2) and using the reform (2002) and non-reform years (1999 -2001), with standard errors in parentheses. Column 2 reports the mean of the outcome for parents who had a child before the reform.

Table A5: Impact of the reform on the next generation's PISA score

	Mathematics	Reading	Science
Panel A: All			
Coef.	9.7	7.4	6.5
SE	(6.1)	(6.4)	(6.1)
Obs.	35224	35224	35224
Mean	520.1	509.1	512.0
Panel B: Men			
Coef.	9.8	8.3	8.8
SE	(8.8)	(9.1)	(8.9)
Obs.	17758	17758	17758
Mean	528.4	498.1	516.2
Panel B: Women			
Coef.	9.9	6.5	4.5
SE	(8.5)	(8.8)	(8.6)
Obs.	17466	17466	17466
Mean	511.7	520.1	507.9

*Notes: This table reports RDD-DiD estimates based on equation (2) and using the reform (2002-PISA wave 2018) and non-reform years (1993, 1996 and 1999-PISA waves 2009, 2012 and 2015). Standard errors in parentheses are clustered by school. Estimation weighted by individual inverse probability weights provided in the PISA data set. The last raw of each panel reports the mean of the outcome for the individuals born before the reform. ***p < 0.01,**p < 0.05, *p < 0.1.*

Table A6: Time Use Survey - Fathers heterogeneity

Panel A	**Low-educated fathers**			**High-educated fathers**		
	Mean	OLS	Tobit	Mean	OLS	Tobit
Childcare time	47.9	14.61	16.27	34.9	42.05 **	64.67 **
		(40.74)	(45.05)		(19.59)	(30.27)
Number of observations		76	76		76	76
Panel B	**Traditional fathers**			**Egalitarian fathers**		
	Mean	OLS	Tobit	Mean	OLS	Tobit
Childcare time	39.4	2.74	5.96	43.4	60.74 **	86.14 **
		(16.05)	(40.22)		(29.70)	(33.22)
Number of observations		58	58		94	94

Notes: The time dedicated to each activity is in minutes per day and comes from the 2013 Belgian Time Use Survey. This table reports DiD estimates based on equation (3). Regressions includes dummy variables which control for the fact to be employed, student, homemaker, disabled, unemployed, as well as Belgian or foreign born. The sample includes parents who had a first child between 1999 and 2005. Standard errors are reported in parentheses.

Table A7: Impact of the reform on the next generation's outcomes by father's education

	Middle school (0/1)	High school (0/1)	Working (0/1)	Quarterly earnings (euros)	Partner (0/1)	Children (0/1)
Panel A:						
High-educated fathers						
Coef.	-0.0116	-0.0028	-0.0071	-98.6836	-0.0011	-0.0022
SE	(0.0087)	(0.0125)	(0.0129)	(213.2721)	(0.0065)	(0.0023)
Obs.	130135	130135	131601	41734	131601	131601
Mean	0.9011	0.7531	0.3987	7547.2730	0.0636	0.0068
Panel B:						
Low-educated fathers						
Coef.	-0.0008	0.0109	-0.0005	19.8324	-0.0015	-0.0026
SE	(0.0084)	(0.0105)	(0.0101)	(101.6726)	(0.0068)	(0.0045)
Obs.	251398	251398	257012	121497	257012	257012
Mean	0.7992	0.4819	0.5433	6960.5282	0.1386	0.0534

*Notes: This table reports RDD-DiD estimates based on equation (2) and using the reform (2002) and non-reform years (1999-2001). Standard errors are reported in parentheses. The last raw of each panel reports the mean of the outcome for the individuals born before the reform. ***p < 0.01,**p < 0.05, *p < 0.1.*

Table A8: Impact of the reform on the next generation's outcomes by family type

	Middle school (0/1)	High school (0/1)	Working (0/1)	Quarterly earnings (euros)	Partner (0/1)	Children (0/1)
Panel A: Traditional family						
Coef.	-0.0309	-0.0160	-0.0242	186.2656	-0.0179	-0.0087
SE	(0.0229)	(0.0287)	(0.0271)	(332.9496)	(0.0165)	(0.0108)
Obs.	32655	32655	33343	12425	33343	33343
Mean	0.8010	0.4991	0.4408	6828.9207	0.1053	0.0414
Panel B: Egalitarian family						
Coef.	-0.0135	-0.0027	0.0181	-97.0927	-0.0021	0.0005
SE	(0.0087)	(0.0120)	(0.0118)	(131.4957)	(0.0072)	(0.0041)
Obs.	179863	179863	182427	79204	182427	182427
Mean	0.8550	0.6220	0.5136	7274.5434	0.1097	0.0313

*Notes: This table reports RDD-DiD estimates based on equation (2) and using the reform (2002) and non-reform years (1999-2001). Standard errors are reported in parentheses. The last raw of each panel reports the mean of the outcome for the individuals born before the reform. ***p < 0.01,**p < 0.05, *p < 0.1.*

Table A9: Impact of the reform on the next generation's PISA score by father's education

	Mathematics	Reading	Science
Panel A:			
High-educated fathers			
Coef.	5.5	6.0	-1.8
SE	(8.4)	(9.1)	(8.6)
Obs.	16303	16303	16303
Mean	545.9	535.1	538.2
Panel B:			
Low-educated fathers			
Coef.	5.3	1.7	6.9
SE	(9.1)	(9.4)	(9.3)
Obs.	15675	15675	15675
Mean	508.1	496.9	499.9

*Notes: This table reports RDD-DiD estimates based on equation (2) and using the reform (2002-PISA wave 2018) and non-reform years (1993, 1996 and 1999-PISA waves 2009, 2012 and 2015). Standard errors in parentheses are clustered by school. Estimation weighted by individual inverse probability weights provided in the PISA data set. The last raw of each panel reports the mean of the outcome for the individuals born before the reform. ***p < 0.01,**p < 0.05, *p < 0.1.*

Appendix B: Empirical strategy for time use analysis

To estimate the causal effect of the 2002 paternity leave reform on parents' time allocation, we estimate the following model separately for fathers and mothers using data from the 2013 Belgian Time Use Survey:

$$(3) \qquad y_i = \alpha + \sum_{s=0}^{1} 1[R_i = s] \cdot \{1[t_i \geq c]\beta_s\} + \delta X_i + \lambda_n + \varepsilon_i$$

Here, yi denotes the daily time in minutes that a parent spends on a specific activity (e.g., childcare), for a child i born in calendar month t. The reform cutoff c is July 2002. The vector Xi controls for individual characteristics, including employment status (employed, student, homemaker, disabled, unemployed) and nationality (Belgian-born or foreign-born). Year fixed effects, λn, account for aggregate time trends.

The key variable Ri is a dummy indicator distinguishing the reform year (Ri=1 for 2002) from the non-reform years (Ri=0 for 1999–2001 and 2003–2005). Our coefficient of interest is $\beta 1$, which captures the interaction between having a child in the reform year (Ri=1) and being born after the policy cutoff ($ti \geq c$). This coefficient can be interpreted as the Difference-in-Differences (DiD) estimate: it measures the differential change in time use for parents of children born just after July 1, 2002 (the treatment group: July–September births), compared to those born just before (the control group: April–June births), while netting out underlying seasonal patterns by leveraging data from non-reform years.

We apply this model to several daily time-use outcomes: childcare, sleeping, eating, work and social activities, entertainment, as well as a residual category for all other activities. Since a non-trivial share of parents reports zero minutes for activities like childcare or work, we estimate both linear and Tobit models. The Tobit specification accounts for left-censoring at zero minutes and right-censoring at 1,440 minutes (the total minutes in a day), providing estimates that are robust to this data structure.