The "other" Child Penalty: Work Disability after Motherhood and how Paternity Leave can help

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Abstract

Using Belgian administrative data from 2002-2016, we document the increased incidence rate of work disability among women after motherhood. Using an event study approach, we provide empirical evidence that the probability of women to enter disability diverges from the probability of men to do the same only after the birth of their first child. Surprisingly, this child penalty does not disappear over the long run and even up to eight years after childbirth a 1.2 percentage points gap remains. Building on this result, we then show that the provision of paternity leave is an effective public policy to moderate the probability of women to fall into disability after motherhood. We exploit a discontinuity in Belgian legislation, which opened paternity leave only to fathers of children born after the 1st of July 2002. By using a small window of births around this cutoff, we are able to evaluate the causal effect of the paternity leave reform on mothers in a regression discontinuity difference-in-differences (RD-DiD) framework. We find that mothers who had a child with a father eligible for paternity leave spent on average 22 fewer days on disability over a period of 12 years, which corresponds to a 21% decrease. This effect seems to be mostly driven by younger parents who had their first child during the reform year and by a reduction in musculoskeletal disorders, not mental conditions. Finally, we discuss the increased birth spacing induced by the reform, which seems to be one of the mechanisms behind our results.

JEL classification: J16, J13, I13, H55

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Introduction

It is well documented that women with children work and earn less than women without children, regardless of education and socioeconomic status (Angrist & Evans, 1998; Bertrand, 2011; Neumeier, Sørensen, & Webber, 2018). Recent studies have highlighted these so-called "child penalties" as decreasing women's earnings in the long-run by 12 % (Lundborg, Plug, & Rasmussen, 2017) to 20 % (Kleven, Landais, & Søgaard, 2018). In fact, according to Kleven *et al.* (2018), the fraction of gender wage inequality caused by "child penalties" has increased over time, from about 40 % in 1980 to about 80 % in 2013.

Our research shows that this child penalty also occurs in the context of Disability Insurance (DI) and that women are more likely to suffer from work disability after motherhood. This is not surprising given that pregnancy and childbirth have direct effects on women's health, whether it is physical complications (Cheng, Fowles, & Walker, 2006; Saurel-Cubizolles, Romito, Lelong, & Ancel, 2000) or depressive symptoms (Rubertsson, Wickberg, Gustavsson, & Rådestad, 2005). We, however, are among the first to show that there also long-term effects that take place long after the child delivery. We believe that this could reflect family arrangements detrimental to women's health and career. Our argument relies on the popular suggestion that working mothers face a "second shift" at home (Hochschild & Machung, 1990), since they still take on the lion's share of domestic work, including child care. This is well documented in the time use surveys, which show that in Belgium, for instance, working mothers spend on average more than double the time on child care than fathers (see table A1). At the same time, employed mothers spent less time in childfree leisure and personal care, as suggested by Craig (2007) in a study for Australia. Hence, the argument is that the combination of labor market participation and domestic work, also known as the "double burden", might affect women's health and career, as well as their likelihood to ultimately suffer from work disability.

We use an event study approach, similar to the one of Kleven *et al.* (2018), to show that having children increases women's probability to enter DI, while fathers are almost unaffected. Our approach is based on individual-level variations in the timing of first births and sharp changes that occur around childbirth. Our analysis reveals that this child penalty does not disappear over the long run and, even up to eight years after their first child's birth, women are 1.2 percentage points more likely than men to enter DI. We also demonstrate that the impact of children increases with the size of the family, with a gender gap that reaches 2.3 percentage points for parents with three children.

A recent study by Angelov, Johansson & Lindahl (2018) finds similar results for sick leave in Sweden. They use within-couple variations and show that following the birth of their first child, mothers more than double their sick leave compared with fathers.

Andresen and Nix (2019) also try to give a larger perspective on child penalties in the Norwegian context. Work disability is not their primary concern but they do measure sickness absences and find comparatively similar results to Angelov *et al.*'s and ours. Interestingly, they also measure the child penalty for lesbian couples. Their results are particularly noisy due to small sample size but they find a spike around childbirth for lesbian mothers bearing the child and no differential trend afterwards when compared to lesbian co-mothers who did not give birth. This gives support to our argument that long term effects may be driven by family arrangements and not by the biological cost of giving birth. However, the main limitation of their study is that their measure of sickness also includes absence for dependents, including young children. Therefore it is hard for them to disentangle pure health effects for the mother from days-off taken to take care of young children. Our study measures disability spells that have been validated by a doctor and concerns only the mother.

Building on the fact that the long-run probability for mothers to enter DI seems to be affected by family arrangements, we next turn to an evaluation of whether the provision of a paternity leave could be an effective public policy to moderate this effect. Indeed, policy makers have argued that paternity leave could increase the role of fathers in the household and alleviate the professional and economic costs of motherhood. Becker's (1985) study offers a theoretical framework to consider the impact of paid leave policies for fathers on the gendered division of household responsibilities. We argue that they could have both direct and indirect effects on the career of the spouse. First, in the short-run, they could help women during the period just after giving birth. Second, they could affect the decision-making and the division of tasks in households, with long-term consequences for women.

Numerous studies have shown empirically that paternity leave policies do increase fathers' share of domestic work and their involvement in childcare (Farré & González, 2019; Hook, 2010; Patnaik, 2019; Tamm, 2019), for as long as 13 years after the reform (Kotsadam & Finseraas, 2011). Hence, even short paternity leaves can produce a more equal division of domestic work many years after the paternity leave is taken. However, a complete balance between men and women might not be achieved, as Ekberg, Eriksson & Friebel (2013) show that fathers affected by a paternity leave reform in Sweden did not take more days-off to take care of sick children.

Evidences of the effects of paternity leave policies on labor market outcomes are more mixed. Two studies in Sweden and Norway did not find an effect of paternity leave reforms on both fathers' and mothers' labor supply and wages (Cools, Fiva, & Kirkebøen, 2015; Ekberg et al., 2013). On the contrary, Rege & Solli (2013) for Norway, Druedahl, Ejrnæs & Jørgensen (2019) and Andersen (2018) for Denmark, Dunatchik and Özcan (2019) for

Quebec, did find a reduction in gender wage gap, as well as an increased probability for women to participate in the labor force.

Recent studies have also reported effects on outcomes which are not directly related to the labor market, but might affect women's career. Farré & González (2019) found that the introduction of two weeks of paid paternity leave in Spain in 2007 led to delays in subsequent fertility. Avdic & Karimi (2018) show that couples who were affected by the introduction of a paternity leave quota in 1995 in Sweden, the so-called "daddy-month", increased their probability of separation compared to unaffected couples.

One study, more directly related to ours, did not find a significant effect on the sick leave of mothers after a paternity leave reform in Norway (Ugreninov, 2013), even though their results point in the expected direction. We believe that this study uses a rather limited empirical strategy,[1] that relies on too small of a sample and fails to account for seasonality in labor market outcomes. Another study by Persson and Rossin-Slater (2019) also tackles the question of father's involvement and maternal health. They evaluate a particular policy called "double days"[2] and found that increasing father's temporal flexibility reduces the risk of the mother experiencing physical postpartum health complications and improves her mental health (Persson & Rossin-Slater, 2019). However, both studies by Ugreninov (2013) and Persson and Rossin-Slater (2019) consider only short term health effects while our sample allows us to measure effects up to 12 years after childbirth.

Our evaluation tries to fill these gaps by using a robust empirical strategy, that relies on a large sample and a regression discontinuity difference-in-differences (RD-DiD) framework similar to Avdic *et al.* (2018). We exploit a discontinuity in Belgian legislation which opened paternity leave only to fathers of children born after the 1st of July 2002. By using a small window of births around this cutoff, we are able to evaluate the causal effect of the paternity leave reform. We show that it decreases the number of days on DI for women, up to 12 years after childbirth, by 21 percent. This effect seems to be mostly driven by the younger parents who had their first child during the reform year. In addition, we show that most of the long-term reduction in the number of days on DI for mothers is due to a decrease in disability related to musculoskeletal disorders, not mental conditions.

Finally, we show that the paternity leave reform in Belgium increased birth spacing

[1]The main issue with the Ugreninov (2013) study is that it does not use a regression discontinuity design but instead regress the outcome of interest on a dummy for the fact to be affected by the reform based on the child's birth date. This leaves them with a rather small sample of parents who had a child within one month of the reform date. In addition, they fail to account for the seasonality in sick leave absence, which as shown in our study, is particularly important.

[2]The reform allowed fathers to take up to 30 days of paid leave on an intermittent basis alongside the mother during the first year of the child, without affecting total leave duration

between the first two children. This result is perfectly in line with the findings of Farré & González (2019) after a similar reform in Spain. We explain why we think that the increased birth spacing could be one of the mechanisms behind the reduction of the time spent on DI for mothers. We demonstrate that both results exhibit similar time dynamics and are driven by the same sub-population, that is younger mothers who could decide to delay the birth of their second child. We conclude therefore that the timing of births for multiple-children families is key to reducing the problem of work disability of mothers at young ages.

The remainder of the paper will be organized as follows. Section 1 discusses the institutional context of disability insurance and parental leave policies in Belgium. Section 2 introduces the empirical framework of the event study and presents the results. Section 3 introduces the regression discontinuity difference-in-differences framework, presents the results, provides various robustness checks and discuss potential mechanisms. Section 4 concludes.

1 Institutional context

1.1 The Belgian Disability Insurance System

In Belgium, employed workers[3] satisfying some minimum amount of seniority[4] are insured against health shocks that affect their ability to work through the payment of disability benefits.

In this paper, we observe disability spells that last more than one month and are covered by the National Institute for Health and Disability Insurance. Shorter spells are fully paid by the employers and are therefore not reported in our data. In the remainder of the paper, we will also distinguish between those individuals who have spent less than a year on disability (called "short-term disabled") and those who have spent more than a year on disability (called "long-term disabled"). In practice, short- and long-term disabled are covered by two different programs which differ in terms of (i) administrative application and (ii) wage replacement rate.

Regarding the application for disability status, individuals must first be examined by a

[3]This also applies to currently unemployed workers. Self-employed have a distinct disability insurance program that we do not cover in this section and for which we do not have data.

[4]Full-time workers and unemployed workers must have fulfilled a minimum of 180 working days (or active days of job search for the unemployed) during the last twelve months to have access to disability insurance. For part-time workers, the condition is to have worked at least 800 hours over the last 12 months.

doctor designated by their health insurance fund.[5] In order to be recognized as "unable to work", applicants must fulfill two main criteria. First, the applicants' ability to work must be reduced by at leats 66% with respect to their previous occupational demands.[6] Second, applicants must have stopped all productive activity as a consequence of a deterioration of health that is not directly linked to their professional activity.[7] Then, after one year of short-term disability, the disabled workers may enter the long-term disability program. In practice, in order to be accepted into the long-term disability program, the applicants' doctor (who oversaw the applicant during the short-term period) submits the application to the NIHDI which can either directly approve the doctor's conclusions or run its own internal evaluation. In other words, there is no automatic transition from short- to long-term disability status.

The replacement rate also varies with the duration of disability. During the first year it amounts to 60% of the last wage payment received before becoming disabled.[8] After one year, when one enters the long-term disability program, the replacement rate depends on the last wage payment received,[9] as well as the position of the disabled in the household. To be precise, this share is 65% for heads of households, 60% for single households and 40% for cohabitants, with defined floor and ceiling amounts.[10]

Over the last decades, the number of persons deemed unable to work for health reasons and receiving DI benefits has increased substantially in Belgium, as well as in most OECD countries, creating an important challenge for the social security funding (OECD, 2010). This increase has been particularly substantial among women. This reflects, in part, their increased labor force participation, which contributed to expand the pool of insured workers, as more women had sufficient work history to qualify for DI. But according to Autor and Duggan (2006), this would explain only about one-sixth of the increase in the rate of female DI beneficiaries in the case of the United States. Figure 1 shows that, when taking into account eligible workers only, the incidence rate for women is still growing

[5]In Belgium, although the health care system is publicly supported at the national level, the reimbursement of medical expenses and short-term disability benefits are paid by the public health insurance fund called "mutuality", which are funded by the National Institute for Health and Disability Insurance (NIHDI) and act as intermediaries between this Institute and the disabled. In short, to benefit from the Belgian medical coverage, individuals must register at a health insurance fund. The most important health insurance funds in Belgium are the socialist health insurance fund, the Christian health insurance fund, the liberal health insurance fund and the neutral health insurance fund. Beyond their reimbursement role, health insurance funds also have a duty to accompany, inform and defend their members.

[6]Note that an important change occurs after 6 months of disability: the reduction in the ability to work is then evaluated with respect to any occupation that the worker could perform given his/her age, education and experience (instead of his/her previous occupation).

[7]This condition exists to establish a distinction between the disability insurance program and other programs such as the occupational injuries fund and the occupational diseases fund

[8]For unemployed, it is equal to the unemployment benefits.

[9]For unemployed workers, it is the last wage payment before unemployment.

[10]In 2019, the maximum short-term disability benefits were 2,052 euros per month, while the maximum long-term disability benefits were 2,223 euros per month.

faster than for their male counterparts. This is true for Belgium, whose data are used in this study, but also for the United-States and most OECD countries (OECD, 2010).

[Figure 1]

Another important trend in DI results from reforms[11] that expanded the eligibility criteria and induced major changes in the composition of the beneficiary population, with a notable shift towards younger workers. Autor and Duggan (2006) explain that the new legislation places more weight on "applicants' reported pain and discomfort", making it easier to qualify for certain impairments that used to be "hard to verify", such as back pain or depression (Liebman, 2015). The side effect of these reforms has been an increased incidence rate of disability at younger ages (Congressional Research Service, 2018). Indeed, mental and musculoskeletal disorders tend to have an early onset and low age-specific mortality (Autor & Duggan, 2006). As a result, those beneficiaries are likely to enter early on the DI program and experience a relatively long duration.

Hence, while work disability used to concern mostly older men prior to the 1990s, it is now increasingly affecting women, and particularly at young ages. Our study complements the existing literature on DI by shedding light on those gender inequalities for young adults. We show in particular that they might, in part, be explained by parenthood and how couples deal with the arrival of children in the household.

1.2 Parental leave

We now turn to the Belgian system for parental leave, whose main features are reported in table 1.[12]

[Table 1]

Paid maternity leave was introduced in 1971.[13] The legislation provides a maximum of 15 weeks[14] of paid leave for mothers of newborn children. Mothers can, to some extent,

[11]In the United-States, the major reform was implemented in 1984 by the Disability Benefits Reform Act (P.L. 98-460).

[12]Finally, both parents of children less than 12 years old are also entitled to 4 months of parental leave (8 months for a career interruption of 50% or 20 months for a career interruption of 20%- "Arrêté royal relatif à l'introduction d'un droit au congé parental dans le cadre d'une interruption de la carrière professionnelle", of October, 29th 1997). But workers who decide to use this other form of leave receive a fixed amount instead of a percentage of their salary, which makes it a less appealing system.

[13]"Loi sur le travail du 16 mars 1971"

[14]19 weeks for multiple births.

decide how to distribute those weeks before or after giving birth. However, they must take at least one week before the planned delivery day and cannot come back to work earlier than 9 weeks after birth. In other words, all mothers must stop working during a compulsory period of 10 weeks. Maternity leave is not universal and women are entitled to paid leave only if they have worked at least 120 days (or active days of job search for the unemployed) in the last 6 months.[15] The replacement rate is 82% of their gross salary during the first 30 days and 75% thereafter (capped at a ceiling of 2417 euros per month). Figure A1 shows the number of women that took a maternity leave as a percentage of the annual number of births between 2000 and 2016. Statistics for maternity leave do not include civil servants and self-employed workers who benefit from a different system.

Fathers of newly born children were, until the beginning of the 2000s, only entitled to 3 days of paid job absence. A real full-time, job-protected, paternity leave right was introduced in July 2002[16] for fathers[17] who have a salaried contract.[18] The paternity leave covers a period of 7 working days, which together with the 3 days of job absence, brings the leave period to 2 weeks. Initially, fathers could delay their paternity leave until one month after childbirth, but the time frame was extended to 4 months in 2009,[19] hence allowing fathers to take their paternity leave after the compulsory maternity leave period of mothers. The first 3 days of job absence are covered by the employer and are fully compensated. The replacement rate for the remaining 7 working days equals that of the mothers and corresponds to 82% of the gross salary. As shown in Figure A1, opting in to this policy was substantial from the introduction, with about 35% of new fathers using their paternity leave right during the second semester of 2002, and about 45% in the following years.[20] One should keep in mind that those statistics do not account for civil servants, who benefit from a different system, as well as self-employed workers who are not entitled to paid paternity leave. Hence, the take-up of eligible fathers was even higher.

It might be useful to put the Belgian system in perspective with other countries, notably the Scandinavian countries who were early adopters of government paid leave. In Sweden, for instance, the parental leave system was introduced in 1974 and was gender neutral. Both the mother and the father were given an equal number of paid leave for their children, but with the option of freely transferring paid leave days between each

[15] 400 hours if they work part-time.

[16] "Loi relative à la conciliation entre l'emploi et la qualité de vie", of August, 10th 2001.

[17] Paternity leave has been extended to co-parents in 2011; "Loi du 13 avril 2011 modifiant, en ce qui concerne les coparents, la législation afférente au congé de paternité", opening it to same sex couples.

[18] Their is no paid paternity leave for self-employed workers.

[19] "Loi-programme du 22 décembre 2008" which applies only to fathers of children born after the 1st of April 2009.

[20] Fathers who took only 3 days or less are not included in the figure since they need only to report to their employer.

other. The system was reformed in 1995 to encourage fathers to take a bigger share of the parental leave. A so-called "daddy-month" was introduced, reserving 1 month of paid leave to each parent, implying that 1 month of paid leave would be lost if either parent chose not to take any leave.

In Belgium, parental leave has never been transferable between parents. We believe that this feature is interesting since fathers can take paternity leave without automatic reduction for mothers. In other words, we can measure the net effect of providing paternity leave. Many studies in Scandinavian countries actually measure the combined effect of paternity leave provision and reduction of maternal leave (e.g. Ekberg et al., 2013; Avdic & Karimi, 2018). In some cases, the reforms are even combined with an increase of the total leave period for both parents (e.g. the second "daddy month" reform in Sweden), which makes it even harder to disentangle the estimated effects. In the context of work disability, the reduction of maternity leave could have detrimental effects on maternal health, which might not be balanced by the provision of paternity leave. This could be why Ugreninov (2013) does not find any effect of paternity leave provision on mothers' sick leave absence in Norway.

2 Event study

2.1 Empirical strategy

We use a rich set of administrative data from the Belgian Crossroads Bank for Social Security (CBSS). The latter puts together several administrative registers linked at the individual level (via personal identification numbers) and contains information on household composition, labor market outcomes of each member, as well as social security status. Most importantly, the data allows us to match children with their parents, as well as workers to their firms. Regarding the data on disability, we are able to observe the disability status during a given quarter, as well as the number of days of each disability spell and the amount of benefits received.

We obtained a large sample of 60% of all births during the years 2002 to 2013, with stratification at the provincial level to ensure representativity. We follow quarterly the career of the parents of those children over the period 2002 to 2016.[21]

For the event study analysis, we include all individuals who had a first child between

[21] The data for short-term disability insurance in 2002 are available only for the four (out of six) biggest health insurance funds.

2002 and 2013 and do not impose restrictions on the relationship status of the parents. This leaves us with an estimation sample of 371,875 parents.

Our first research question tries to evaluate to what extent children can impact the probability of their parents to fall into disability. As explained by Kleven *et al.* (2018) the ideal experiment would be to randomize fertility. But in the absence of such an experiment,[22] they propose instead an event study approach based on individual-level variation in the timing of first births and sharp changes that occur around the event. It is of course arguable that fertility choices and the timing of birth are not exogenous. However, as claimed by Kleven *et al.* (2018), outcomes should evolve smoothly over time, while the event of having a first child generates sharp changes that are likely to be orthogonal to unobserved determinants. In our case, it might be argued that women who invested in their education are more likely to have children later in life and are overall less likely to enter into disability. However, the effect of education on those outcomes should not generate sharp changes.

In addition, as explained by Kleven *et al.* (2018), the event study approach has the additional advantage of tracing out the full dynamic trajectory of the effects, and since we do not condition the sample on having one child in total, the long-run effects will also capture the impact of subsequent children. Previous studies using instruments for the number of children, such as twin births (Rosenzweig & Wolpin, 1980 ; Bronars & Grogger, 1994) or the gender breakdown of siblings (Angrist & Evans, 1998), only succeeded in estimating local effects of second of higher order children. Our approach will instead capture the total impact of having children on the probability to enter DI for mothers relative to fathers.

Event studies have been used in different contexts, such as the impacts of inheritances (Druedahl & Martinello, 2016), hospital admissions (Dobkin, Finkelstein, Kluender, & Notowidigdo, 2018) or family health shocks (Fadlon & Nielsen, 2019). In our specific setting, we foresee one limitation, the fact that this framework will not allow us to measure pre-child choices. For instance, if women invest less in education and career in anticipation of motherhood, then the estimated child penalties represent lower bounds on the total lifetime impacts of children (Kleven et al., 2018). In other words, our study will be able to identify the post-child effects of children conditional on pre-child choices.

We now turn to the econometric setting of our event study. For each individual in the data, we denote by t = 0 the quarter in which the father/mother has his/her first child and index all quarters relative to that time period. Our sample include parents who had

[22]Lundborg, Plug and Würtz Rasmussen (2017) have come up with a very convincing strategy that uses *in vitro* fertilization treatments.

a child in 2002-2013, that we follow over a period of up to 12 years, including up to 4 years prior to the birth and up to 8 years after.[23] In total, we observe each parent during 48 quarters. Our main outcome of interest is a dummy variable to indicate the receipt of disability benefits during a given quarter q for individual i of gender g and at event time t. We estimate the following equation separately for men and women:

$$y^g_{iqt} = \sum_{j \neq -4} \beta^g_j \cdot I[j = t] + \sum_k \gamma^g_k \cdot I[k = age_{iq}] + \sum_y \delta^g_y \cdot I[y = q] + \epsilon^g_{iqt} \qquad (1)$$

where we include a full set of event time dummies (first term on the right-hand side), age dummies (second term) and time period dummies (third term). We omit the event time dummy at t = -4, implying that the event time coefficients β^g_j measure the impact of children relative to four quarters before the first child's birth. We voluntarily chose a date not too close to childbirth, as we suspected that short-term disability would raise for women during their pregnancy. Following Kleven *et al.* (2018), we include a full set of age dummies to control non-parametrically for underlying life-cycle trends. Additionally, the age dummies improve the comparison between men and women, as women are on average a couple of years younger than men when having their first child. In addition, we include a full set of quarter dummies to control non-parametrically for time trends and seasonal effects.

We still observe slight differences in pre-trends between men and women even after controlling for age and quarter dummies. Since these smooth pre-trends distract from the breaks around childbirth, we follow again Kleven *et al.* (2018) and control for linear pre-trends. In other words, we estimate a linear trend separately for men and women using only pre-event data (i.e. from quarter -16 to quarter -4 before birth), and then we estimate the main event study specification from equation (1) residualizing the outcome variable with the estimated pre-trend.

2.2 Main results

Figure 2 plots the gender-specific impacts of children on disability status across event time. The outcome includes both short and long-term disability. As explained above, it corresponds to disability rates at event time t relative to the 4$^{\text{th}}$ quarter before the first

[23] Our sample includes parents who had a child between January 2002 and December 2013. We follow those parents until 2016. Our panel is therefore unbalanced because the follow-up period differs according to the birth date of the reference child. For parents who had a child in 2002, we do not have data on the four years before. For parents who had a child in 2013, we have pre-birth outcomes but a reduced follow-up period of 3 years. We run the estimations of a perfectly balanced panel and found similar results (available on request).

child's birth (t = -4), having controlled non-parametrically for age and time trends. The figure also includes 95% confidence bands around the event coefficients.

We see that women experience a sharp increase in their probability to enter disability starting 3 quarters before their first child's birth, that is at the beginning of the pregnancy. In the quarter right before giving birth, their probability to enter disability increases by about 6 percentage points[24] in comparison to the 4th quarter before giving birth. The effect then lowers during the quarter of childbirth and the next because most women who are ill are then covered by the maternity leave and not registered as disabled. However, the effect is not null since women who are ill during the last six weeks before childbirth are only entitled to 9 weeks of postpartum maternity leave. Thus, a woman who gave birth at the beginning of a given quarter might still enter disability insurance during the same quarter if she was only entitled to 9 weeks of maternity leave. Interestingly, we also observe another increase during the second and third year following the first child's birth. We recall that our event study design captures the total effect of all children, therefore it is most likely that the second increase is related to the arrival of subsequent children in the household.

[Figure 2]

From figure 2, we also conclude that men seem to be largely unaffected by children. We only detect a small increase in their probability to enter disability during the two quarters after their first child's birth. Most importantly, we observe that the probability of men and women to enter disability insurance never converges back and that 8 years after their first child's birth, a 1.2 percentage points gap remains. Since the average disability rate at t-4 was 2.8% for both women and men, the child penalty for women amounts to about 40%.

2.3 Heterogeneous effects

In this subsection, we want to observe how the effects measured in the event study vary depending on the total number of children in the household. Indeed, even though our event study is based on parents who had a first child between 2002-2013, the results presented in figure 2 are based on the full sample, irrespective of the total number of children they end up having. As already explained, this means that the dynamics we observe include the effects of children born after the first one. In other words, the estimated long-run

[24] The effect might be even larger given that women who are ill during the last six weeks before childbirth are already covered by their maternity leave and not registered as disabled.

impacts should be interpreted as capturing the total effect of all children. To explore the implications of multiple children, we replicate the event study in subsamples that condition on the total number of children parents end up having (1, 2, 3 or more children; respectively 31, 50, 15 and 4 percents of our sample for women), as of 2016.

We observe in figure 3 that the sharp increase around the birth of the first child is roughly similar in magnitude for the three subsamples. We also notice that the coefficient for mothers reverts to a level close to zero on the third quarter after childbirth for all types of families. However, the trends differ from the fourth quarter after childbirth. In families with a single child (panel A), the trends between parents are only slightly different. The gender gap eight years after the birth of their only child reaches only 0.8 percentage point. In families with two children (panel B), we observe an increasing gap between mothers and fathers in the second and third year following the birth of the first child. This very likely captures the effect of the second child. The gap between mothers and fathers up to eight years after the birth of their first child reaches 1.4 percentage points. It is expected that the two-child families look very much like the estimates for the whole sample in figure 2, since those families make up 50% of our sample. Finally, in families with three children (panel C), the gap between parents reaches 2.3 percentage points after eight years. Thus, we conclude that the probability for women to enter DI increases with the number of children.

[Figure 3]

One might ask, however, whether the increased probability for women to enter DI reflects merely the multiple pregnancies and deliveries or corresponds to the larger cost of having multiple children. To answer this concern, we replicate our event study around the second child's birth, conditioning our sample on having two children in total, as of 2016. From figure 3, we observe a spike in the probability of women to enter DI around the second childbirth that is similar in magnitude to figure 2 for the first child. We also see a small bump during the four years that precede the second child's birth. This is of course related to the first child's birth. It is a smooth bump rather than a sharp spike because the birth of the first child did not take place during the same quarter for all women. More interesting is the increase that follows the second child's birth. Since we conditioned our sample on a total fertility of two children, this subsequent increase cannot be attributed to other childbirths. We believe that it instead reflects the long-run effects for women of having multiple children. Thus, we conclude that beyond the short-term effects related to giving birth, there are indeed long-term effects of having children for women, which are reflected in their increased probability to enter DI even eight years after their second child's birth.

[Figure 4]

3 Regression discontinuity design

In the previous section, we provided empirical evidence that children have a large impact on the probability of mothers to enter disability. We now turn to the evaluation of whether paternity leave is an effective public policy to moderate the entry of women into disability after motherhood. To do so, we will exploit a discontinuity in Belgian legislation, which opens paternity leave only to fathers who had a child after the 1st of July 2002.

3.1 Empirical strategy

We use a regression discontinuity design to analyze the impact of paternity leave on maternal outcomes, exploiting the fact that fathers whose children were born before the 1st of July 2002 did not have access to the 2 weeks leave, while those whose children were born after could decide to take it. Since we do not know the exact day of birth of children, our running variable is instead the month of birth. We restrict our sample to a 6-month window around the reform, that is to parents of children born in 2002. In section 3.4, we test the sensitivity of our results to different bandwidth selection. As explained by Imbens (2008), the key assumption of this design is that individuals are unable to manipulate the assignment variable, here the birth date of their child. In our case, this seems like a reasonable assumption since birth dates are arguably difficult to manipulate. If this assumption holds, having a child right before or right after July 1st is as good as random. We test this formally in section 3.2, by checking whether family characteristics are balanced around the threshold. In addition, we also need to assume that there are no other important changes of relevance (such as other policy interventions) for parents of children born after the 1st of July. We are not aware of any such potential confounding factors.

The basic regression-discontinuity design[25] motivates estimation of the following cross-sectional regression model:

$$y_i^T = \alpha + 1[t_i \geq c]\beta + 1[t_i \geq c] \cdot f_r(t - c, \gamma_r) + 1[t_i < c] \cdot f_l(c - t, \gamma_l) + \epsilon_i \qquad (2)$$

[25]See Lee and Lemieux (2010) for a thorough exposition of the regression-discontinuity design econometric framework.

where y_i^T is the outcome of interest after T quarters for each parent of child i born in month t. c is the reform cutoff month, $1[\cdot]$ is the indicator function, f_l and f_r are unknown functions with parameter vectors γ_l and γ_r, capturing trends in the outcome of interest. We can interpret β as the estimated discontinuity for a given outcome when having children born just before and just after the 1st of July 2002. Moreover, if we assume that parents do not have exact control of when their children are born in a neighborhood around the 1st of July cutoff, we can interpret the estimated discontinuity as the causal effect of the paternity leave reform.

In order to address potential seasonality concerns,[26] we combine the regression discontinuity design of model 2 with a difference-in-differences approach similar to other research on the topic of parental leave (e.g. Lalive, Schlosser, Steinhauer, & Zweimüller, 2014; Farré & González, 2019; Dustmann & Schönberg, 2012; Danzer & Lavy, 2017; Avdic & Karimi, 2018; Cygan-Rehm, Kuehnle, & Riphahn, 2018). To do so, we include children born before and after July 1st of the treatment year (2002), as well as two years after the reform was implemented (2003 and 2004). This approach is valid under an additional common trends assumption that our outcomes' trends are comparable between reform and non-reform years. We cannot think of reasons why the seasonality pattern would change as a result of the introduction of the paternity leave.

Figure 5 shows that the number of disability days does indeed vary according to the moment that the child is born. We observe that women who had a child during the second part of the years 2003 and 2004 (non-reform years) always have on average a higher number of disability days. We therefore need to account for this seasonality when we measure the discontinuity in 2002.

[Figure 5]

This setting has the additional advantage of accounting for the fact that our outcomes of interest are measured quarterly while our running variable is monthly. This might have been problematic since the follow-up period will mechanically vary between couples whose children are born at the beginning or at the end of a quarter. For instance, if we observe parents' outcomes one quarter after birth, for those who had a child in June the follow-up period ranges from 3 to 4 months, while for parents of children born in July the follow-up period ranges from 5 to 6 months. This might be important since the discontinuity will be measured between June and July, which are respectively the end and the beginning of a quarter. Following Avdic and Karimi (2018), we therefore use non-reform years to wash out any such mechanical correlation.

[26] We can think for instance of child care accessibility that might vary according to the moment of the year, so that some parents need to stay longer with their new born at home.

We extend equation (3), using years 2003 and 2004, and additionally specifying an indicator R={0, 1}, equal to one for the reform year 2002 and zero otherwise, interacted with each included variable in the model:

$$\begin{aligned} y_i^T = \alpha + \sum_{s=0}^{1} 1[R_i = s] \cdot \{1[t_i \geq c]\beta_s + 1[t_i \geq c] \cdot f_r(t - c, \gamma_{rs}) \\ +1[t_i < c] \cdot f_l(c - t, \gamma_{ls})\} + \zeta X_i + \lambda_n + \epsilon_i \end{aligned} \quad (3)$$

Equation (3) is essentially a fully interacted version of (2) with separate effects for reform and non-reform years, with the exception of fixed effects for each non-reform year, represented by λ_n, and the vector of control variables X_i. Our coefficient of interest is still β_1, which is now the interaction between "having a child after July 1st" and the 2002 indicator (R). This new specification controls for systematic differences in outcome across families having a child in different (even if close) months of the year. Finally, we added a vector of control variables X_i, including age of parent, number of kids and region of living (indicator for living in Flanders) at the moment of the birth of the reference child. We know that those variables might affect the probability of entering disability and should therefore help us get more precise estimates. We test formally in section 3.2 that those predetermined outcomes are perfectly balanced between the treatment and control groups.

For the analysis, we restrict our sample to children born between January 2002 and December 2004 for which both parents are known at the time of birth but do not necessarily form a couple (in the sense of marriage or cohabitation) and might not even live together. Since we are primarily interested in the effects of paternity leave on mothers, we cannot use mono-parental families. We also restrict the sample to those households in which both parents were working at the time of birth. Since paternity leave is only available for salaried men, we do not want to include in our sample households in which the father was not working at the time of birth. This leaves us with an estimation sample of 101,735 households.[27]

[27]For the analyses in appendix, we only observe 99,502 of the fathers because for 2 percent of our sample we do not have information for one or more of the three control variables used in the estimations (i.e. number of children, age and region, at the moment of birth of the reference child)

3.2 Main results

From the literature review, we know that the introduction of a paternity leave policy could have long-term consequences for women by affecting the decision-making and the division of tasks in household. Therefore, we will analyze the trajectory of mothers over a substantive time period of 12 years after their reference child's birth.

As explained above, our design based on the provision of paternity leave for fathers of children born after July 1st 2002 should provide us with two groups that are as good as random. Table 2 shows evidences that control (Jan. - June) and treated (July - Dec.) parents are balanced in covariates across the threshold, in terms of their region of living (indicator for living in Flanders), size of household, number of children, whether the reference child is their first child, age, labor market status and type of employment, daily wage, all measured the quarter of birth of the reference child.[28] We apply equation (3) to these pre-determined outcomes and report the coefficient beta and the associated standard errors (between parentheses) on the right part of table 2. This provides a formal test showing that there is no discontinuity for the characteristics of parents of children born right before and right after the introduction of the paternity leave.

[Table 2]

Now that we have tested formally that our strategy based on the date of birth of the reference child is valid, we can turn to the analysis of the causal impact of the introduction of the paternity leave. We apply equation (3) to a set of outcomes in a RD-DiD framework, controlling for mothers' age, number of children and region of living, at birth. Table 3 reports our main results. We recall that we estimate intent-to-treat (ITT) effects, since we observe eligibility (month of birth) but not actual take-up of paternity leave at the individual level. To give a sense of the size of the effects, we also provide the average of the outcomes.

We have three main outcomes. The first one, reported in the tables as "Ever on DI" corresponds to the probability to have entered disability insurance at least once by a certain date. It is therefore a measure of the effect of the reform at the extensive margin. Two more outcomes combine the extensive and intensive margins by looking at the number of days on DI, as well as the amount of benefits received. Those outcomes are "cumulative", in the sense that they measure the total days/benefits from the moment of

[28]The wages are measured the quarter before the quarter of birth of the reference child. We had to limit the sample to a 3 months window (instead of 6 months), because our data starts in 2002. Therefore, we cannot observe outcomes the quarter before birth for those who had a child between Jan. and March 2002.

birth of the reference child to a specific date, usually twelve years after. Finally, all tables provide the overall effects, as well as a breakdown between short-term and long-term disability programs.

Table 3 shows that mothers who had a child with a father eligible for paternity leave spent on average 22 fewer days on DI, which corresponds to a reduction of 21% compared to the sample mean. The effect is perfectly in line with the one measured on disability benefits, which amounts to a reduction of 712 euros (about 18%).

[Table 3]

Interestingly, we observe that the effect on the short-term disability program is smaller (-6.3 days, that is a reduction of 11%) than that on the long-term program (-16.1 days, that is a reduction of 33%). This is good news for those women's career since the long-term disability program concerns individuals who have been away from the labor market for a long time (more than twelve months) and whose probability to go back to the same employer is almost null. Indeed, for short-term disability, the employer cannot terminate an open-ended contract during the first six months. In practice, many workers on short-term disability will therefore go back to the same employer when their health allows it. On the other hand, most workers on long-term disability have been laid off and need to find another job when their disability status ends.

We also provide figures that illustrate the dynamics of the effects over the whole follow-up period of twelve years. Figure 6 shows the causal effects of the reform on the cumulative number of days on DI for each quarter from the birth of the reference child (t=0) to the end of our sample (t=48). On the one hand, we observe that the effects on short-term disability (panel A) start in year 3 after the birth of the reference child. From this moment, we see a nearly continuous decreasing trend. On the other hand, we notice that the effects on long-term disability start later, from year 5 after the reference child's birth. Both graphs show that the effects are slowly building over time. They only differ by the moment when they start to kick in, which is logical since all individuals must first enter the short-term disability program before they can apply for the long-term one.

[Figure 6]

In appendix, we provide the same analyses on paternal outcomes. We do not observe a statistically significant reduction in the number of days on DI, nor the amount of benefits. But we do observe a smaller probability to enter in the short-term disability program at the extensive margin. Thus, we conclude that the reform has not been detrimental to fathers. If anything, there was a small reduction in DI entries.

3.3 Heterogeneous effects

We now examine the potential heterogeneity of the effects of the paternity leave reform. We are particularly interested in three sources of heterogeneity, the birth order of children, the age of mothers at childbirth and finally the medical condition of the DI applicants.

3.3.1 Effects by birth order

We first estimate heterogeneous effects depending on whether the reference child (i.e. the one born during the reform year) was the first child of the mother or a higher-order child. Our sample is therefore divided between first-time mothers, which represent 48% of our sample, and "experienced" mothers who were having a second or higher order child when the paternity leave reform entered into force. The results are presented in table 4. They indicate that the effects are largely driven by those who were having a first child. For the latter, the effects are almost twice as large and amount to 39 fewer days on disability and 1322 euros less of benefits, over a period of 12 years (both statistically significant at a one percent level). Interestingly, we also observe that the probability of first-time mothers to enter long-term disability (i.e. extensive margin) is 2 percentage points lower, a result which is now statistically significant at a five percents level. As explained above, this is good news since entry into long-term disability is almost always associated with a termination of the relationship with the previous employer and therefore a lower probability of returning to the labor market when the individual's health allows it.

[Table 4]

On the contrary, we find no statistically significant effects on the probability to enter disability, the number of days or the amount of benefits, for "experienced" mothers who had a second or higher-order child during the reform. The differences between first-time and experienced mothers is visible on figure 7. Panel A shows that the effect on first-time mothers starts after 2 years and slowly builds over time. For experienced mothers in panel B, the effect remains close to zero over the whole follow-up period.

[Figure 7]

3.3.2 Effects by mothers' age at childbirth

We now estimate heterogeneous effects depending on the age of the mother at the birth of her reference child. We split our sample between mothers who were aged less or more than 30 years old. The younger mothers represent 45% of our sample.

[Table 5]

The results, presented in table 5, indicate that the introduction of the paternity leave affected particularly younger mothers. For the latter, the effects amount to 30 fewer days on disability and 1179 euros less of benefits, over a period of 12 years (both statistically significant at a one percent level). We also observe that younger mothers are 1.6 percentage point less likely to have entered in the long-term disability program.

On the contrary, we find no statistically significant effects on entry to DI, number of days or the amount of benefits received for older mothers. Once again, the dynamic effects are unequivocal. Figure 8 (panel A) shows that the effect for younger mothers starts from the third year after the reference child's birth. Regarding older mothers (panel B), we do observe a slightly decreasing trend from year 4 but the effect is twice smaller and the confidence interval always includes zero.

[Figure 8]

From the analyses above, we understand that the reduction in disability prevalence observed after the introduction of the paternity leave was mainly driven by first-time mothers who were aged less than 30 years old. Older or more experienced mothers seemed not to be affected by the reform, or at least we could not detect statistically significant effects at conventional levels.

3.3.3 Effects by medical condition

We finally consider heterogeneous effects depending on the medical condition for which DI beneficiaries have obtained their status. We especially want to distinguish between mental and musculoskeletal disorders, which account for respectively 37% and 24% of the number of disability days registered in our sample. Unfortunately, we only know about the medical condition of the beneficiaries once they enter long-term disability, that is after 12 months on disability rolls. We have information on their medical condition based on the first digit of the International Statistical Classification of Diseases.[29] We group categories into (1) mental disorders, (2) musculoskeletal disorders and (3) others.

[Table 6]

From table 6, we remark that almost 50% of the reduction in long-term disability days for mothers is related to musculoskeletal disorders, while the effect on mental health is null. Figure 9 shows dynamic estimates during eleven years[30] following the reference child's birth. We observe that long-term disability days for musculoskeletal disorders (panel B) start decreasing already in year two after the reference child's birth and slowly accumulate over time until reaching a total of about 6 days.

We conclude from this heterogeneity analysis that the long-term reduction in the number of disability days for mothers is largely driven by the decrease in disability due to musculoskeletal disorders. This result is not surprising given that the prevalence rate of backaches remains high among mothers, even after the first postpartum year (Saurel-Cubizolles et al., 2000). We are well aware of the fact that musculoskeletal disorders are among the "hard to verify" impairments (Liebman, 2015). However, this is also the case for mental disorders for which doctors' diagnoses place more weight on "applicants' reported pain and discomfort" (Autor & Duggan, 2006). So we believe that the fact that we do not observe any effect on mental disorders reinforces the argument that it is primarily an health effect and not a moral hazard problem that was suggested by Angelov *et al.* (2018).

[Figure 9]

3.4 Robustness Checks

In this sub-section, we provide robustness checks for our RD-DiD design. First of all, we show that there is no evidence that parents could have anticipated the reform and self-select into the new paternity leave system. Manipulating the date for natural births is virtually impossible, but we want to rule out that planned cesarean sections or induced labor could have been rescheduled. To do so, we provide graphical evidence in figure A2 that the frequency of births by month had not been affected during the 2002 reform. We

[29] We have 17 categories for ICD-9 until 2015.

[30] Here, we restrict the analysis to 11 years because there was a change in the ICD classification from version 9 to 10 in 2016. Unfortunately, there exists no satisfactory table of conversation between these two versions.

observe that there is always a spike of births in July of each year, but for the reform year 2002 this spike is perfectly in line with the post-reform years. In other words, we do not observe bunching in the number of births in the month right after the cutoff.

We now turn to testing the sensitivity of our main results to bandwidth selection. For our main specification, we use a bandwidth of 6 months, which is the largest window available given that our data starts on January 2002. Here, we vary the bandwidth from 6 to 1 months around the threshold to observe how the coefficient for the causal effect of the reform changes. We also provide a donut-hole test, excluding births around the cutoff in June and July, in order to confirm, once more, that parents did not manipulate the birth date. The effects on maternal outcomes are qualitatively similar. When it comes to the cumulative number of days on disability insurance, the effect ranges from -19 to -31 days (table 7). Regarding the disability benefits, the coefficient ranges from -500 to -925 euros. We conclude from this table that our findings are robust to different bandwidth specifications.

[Table 7]

Then, we look into the sensitivity of our main results to different trend definitions in order to obtain an unbiased estimate of the discontinuity at the cutoff. Since we *a priori* do not know the functional forms of f_l and f_r in equation (3), we test for linear trend (our main specification), as well as quadratic and cubic trends. From table 8, we see that the reduction in disability days for mothers is very similar whether we use a linear or quadratic trend, respectively -22 and -21. When using a cubic trend, the reduction is larger and amounts to -51 days. Regarding disability benefits, the amount varies from -466 (quadratic) to -1541 (cubic). Altogether, our findings appear to be robust to specification checks.

[Table 8]

Finally, following Avdic & Karimi (2018), we use non-reform years in a "randomization inference design" and perform placebo analyses shifting artificially the reform cutoff by one month at a time. Thus, we estimate a placebo intervention 43 times between January 2003 and July 2006 using our RD-DiD design defined in equation (3). We estimate effects on our main outcome, that is the cumulative number of days on disability for mothers, but we restrain the period to 10 years after childbirth which is the maximum follow-up period in our sample for women who had a child in 2006. Figure 10 shows the distribution of point estimates from this procedure (panel A) and the cumulative distribution of t-values from the series of regressions (panel B) compared to a standard normal distribution. The point estimates from the placebo interventions are almost always higher than our estimated effect of -13.7 days (indicated by the doted vertical line) and, as expected, centered around zero ($\overline{\beta}_{placebo}$ = -0.1). Furthermore, we perform normality tests on the empirical distribution of the placebo coefficients (Skewness and kurtosis test), as well as the cumulative empirical distribution of the t-values (Kolmogorov-Smirnov test). Both tests cannot be rejected for any conventional significance level.

[Figure 10]

We conclude from this sub-section that our estimates of the effects of the paternity leave reform on maternal outcomes are largely robust to the choice of model specification and bandwidth. Placebo tests also reinforce the confidence in our findings.

3.5 Mechanisms

In this last sub-section, we explore the mechanisms that could have played a role in reducing the time that mothers have spent on DI following the introduction of the paternity leave in July 2002. We focus particularly on the effect of subsequent children because the event study has shown that the probability to enter disability was higher for women with more children. In addition, the event study suggested that after the initial sharp increase around the first childbirth, there was a subsequent raise in the likelihood to become disabled during the second and third years. We attributed this second increase to the arrival of more children in the household. We will test formally this assumption here.

We create a new outcome which corresponds to the probability for a mother to have another child during the years following the birth of the reference child. Unfortunately, we do not observe exactly the subsequent births from the same mother but only the fact that the household has one more child, which could also result from adoption or family recomposition. Furthermore, we create a second outcome which measures the total number of children in the household twelve years after the reference child's birth. We focus our attention on mothers whose reference child was their first child and that for two reasons. First, the median household in our sample has two children, so the key question for most couples is the timing of the second child. Second, we have seen in table 4 that the effects were largely driven by households who had a first child during the reform year.

[Table 9]

Table 9 shows the effect of the paternity leave introduction on mothers' subsequent fertility. We observe that the probability to have a second child for mothers, aged less than 30 and who had a first child with a father eligible for paternity leave, is about 5 percentage points lower in the second and third year after the first child's birth. This effect seems to reduce over time, so that we observe a difference close to zero twelve years after the reference child's birth. Figure 11 (panel A) shows the dynamic of those effects more precisely. We observe that there is a statistically significant negative effect on fertility in years two and three but then it gradually converges to zero. These results suggest that mother who had a first child with a father eligible for paternity leave took longer to have another child. In other words, birth spacing between the two first children has increased for treated women aged less than 30 years old at the moment of their first child's birth. However, the overall fertility seems to be unaffected since there is no difference in the total number of children they end up having 12 years after. Interestingly, the subsequent fertility of mothers aged more than 30 years old seems to be unaffected by the introduction of the paternity leave. We believe that those mothers are closer to the end of their fertile cycle and therefore cannot adjust birth spacing.

[Figure 11]

Our results echo those of Farré & González (2019) for Spain. They found that a similar reform, that is the introduction of a two weeks paternity leave, led to delays in subsequent fertility (Farré & González, 2019). However, they found that older mothers had fewer children on average, while we find that their total fertility is unaffected.

We believe that the increased birth spacing induced by the introduction of the paternity leave could be one of the mechanisms explaining our results on disability. Indeed, we observed in figure 6 that the number of short-term disability days starts diverging from zero only two years after the birth of the reference child. We think that this could be driven by the birth of a subsequent child, as the dynamics clearly match those of figure 11 (panel A). These findings are reinforced by the fact that our main results were driven by younger mothers who had a first child during the reform year. Those women were young enough to consider increasing birth spacing between their first two children.

We would like to make sure, however, that the results are not driven by the fact that mothers who delay their subsequent fertility are observed during more time with a single child since our twelve years follow-up period is indexed on the first child's birth. Indeed, one could argue that if the event of having a second child increases the probability to enter disability and if this event is delayed, this would automatically reduce the number of days on disability rolls during the twelve years following the first child's birth. Thus, in order to remove this mechanical effect, we change the follow-up period, which instead of being indexed on the first child's birth, is now indexed on the second child's birth. In other words, we ensure that all mothers are followed during a period of eight years for which they have at least two children. The figure A3 illustrates this new strategy. In addition, we recompute all our outcomes to capture only disability spells that took place after the second child's birth.

We reproduce our analyses on the sample of mothers who had their first child during the reform year and at least another child in the following 12 years. The results are reported in table 10. We observe that mothers, aged less than 30 years old and who had a first child with a father eligible for paternity leave, spent on average 35 fewer days on disability and received 1198 euros less in benefits since the birth of their second child. Those effects are really close in magnitude to the ones measured for first-time (table 4) and younger mothers (table 5). Furthermore, our results suggest again that older mothers are largely unaffected by the reform. This leads us to conclude that most of the reduction in disability has occurred after the birth of the second child and it is driven by young mothers who were found to have delayed the birth of their second child.

[Table 10]

We think that increased birth spacing could have improved both the labor market attachment of mothers and their health. The body of evidence on the effect of birth spacing on labor market outcomes is rather small. An empirical study for Sweden by Karimi (2014), using miscarriages between the first and second births as an instrument, finds that longer birth intervals have positive long-run effects on income and wage rates. On the other hand, Troske and Voicu (2013), using data for the United-States, show that increasing the time between the first and second childbirth worsens labor market outcomes for mothers by reducing their probability of working full-time. When it comes to the health effects, the evidences are more consistent. A recent review of 58 observational studies has shown that short intervals between pregnancies was associated with several adverse health conditions (Conde-Agudelo, Rosas-Bermudez, Castaño, & Norton, 2012).

Our study suggests that increased birth spacing, exogenously induced by the introduction of a paternity leave, might have lowered the time that women have spent on disability insurance in the long run. Of course the association between second birth timing and disability prevalence is purely correlational. However, the timing of the two effects match perfectly. In addition, the heterogeneity analyses have shown that the sub-populations that drive the results are exactly the same, that is young mothers who had a first child during the reform year. We also ruled out the potential mechanical effects by looking only at disability spells following the birth of the second child. Therefore, we conclude that the increased birth spacing is the most likely candidate mechanism for the long-term reduction in disability observed after the introduction of the paternity leave.

4 Conclusion

This paper has shed light on gender inequalities for young adults in the context of work disability. We focused our analysis on the impact of children, which have been shown to be a large contributor to inequalities between men and women on the labor market. Our argument was that the combination of women's labor market participation and their larger involvement in raising children, also known as the "double burden", might affect their health, as well as their likelihood to ultimately enter into disability insurance.

Using an event study methodology, we have provided empirical evidence that women's probability of entering disability insurance diverges from that of men's after the birth of their first child. This child penalty does not disappear over the long run and still represents a 1.2 percentage points gap between parents up to 8 years after their first child birth. In addition, we have showed that the impact of children increases with the size of the family, with a gender gap that raises to 2.3 percentage points in families with three children. We concluded that beyond the short-term health effects related to childbirth, the increased probability for women to enter disability insurance in the long-run was driven by the arrival of subsequent children.

Building on this result, we then turned to the evaluation of how the provision of paternity leave could be an effective public policy to reduce gender inequalities in the context of work disability at young ages. We have exploited a discontinuity in Belgian legislation, which opened paternity leave only to fathers of children born after the 1st of July 2002. By using a small window of births around this cutoff, we were able to evaluate the causal effect of the paternity leave reform on maternal outcomes in a regression discontinuity difference-in-differences framework. We have found that mothers who had a child with an eligible father spent on average 22 fewer days on disability insurance over a period of 12 years, which corresponds to a 21% decrease. This result seemed to be largely driven by younger women who had their first child during the reform year. Furthermore, we showed that the long-term reduction in the number of disability days for mothers is largely driven by the decrease in disability due to musculoskeletal disorders, not mental conditions.

Finally, we discussed the increased birth spacing induced by the introduction of the paternity leave in Belgium. We believe that this could be one of the mechanisms behind the reduction in the number of days on disability insurance for mothers. We demonstrated that both results are driven by the same sub-population, that is younger mothers who could decide to delay the birth of their second child. We concluded that the timing of births for families with multiple children is key to reducing the problem of work disability of mothers at young ages.

Recent discussions at the European Union level indicate that our findings could provide useful insights in the context of the work-life balance directive, which was adopted by the European Council on June, 13 2019 and should be implemented in all members states within three years. The directive introduces a paternity leave of 10 days for fathers. This corresponds exactly to the laws currently in place in Belgium, making it a very interesting case for research.

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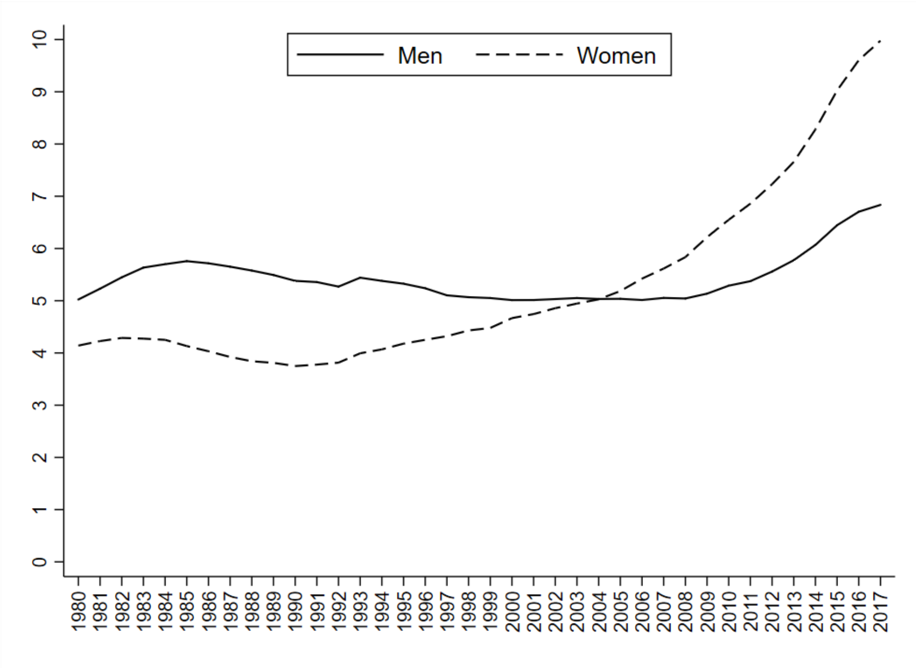
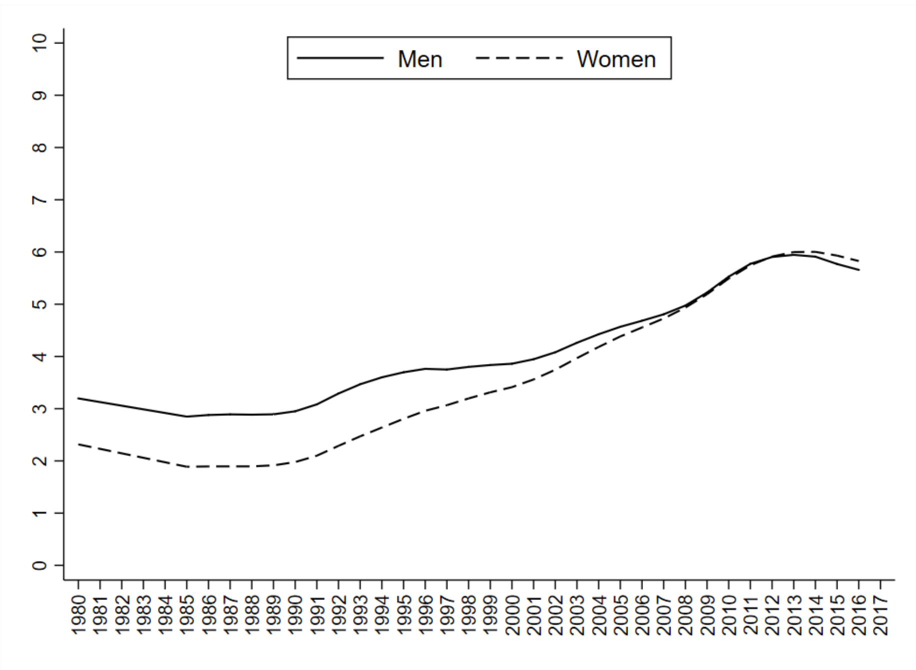
Figure 1: Percent of insured workers receiving (long-term) DI benefits

Panel A: Belgium

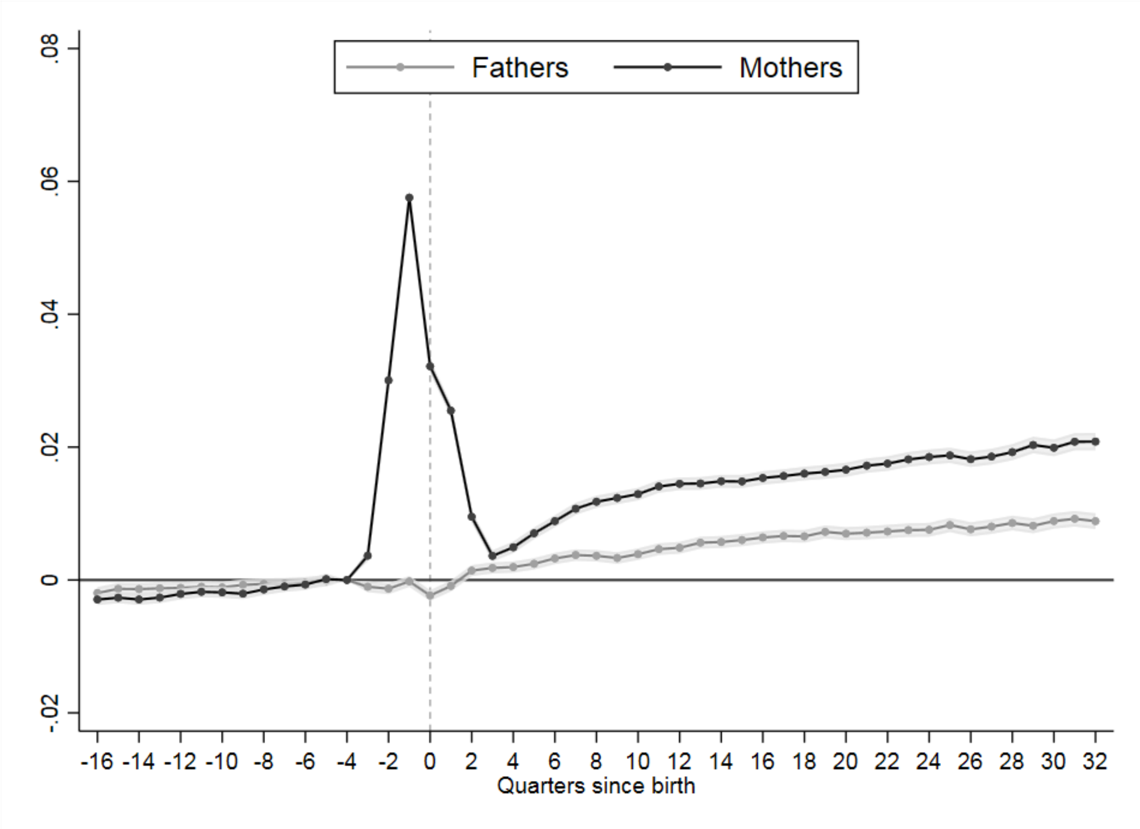


Panel B: United States



Data source for Belgium: National Institute for Health and Disability Insurance. Data source for the United States: Social Security Administration, 2017 Annual Statistical Supplement.

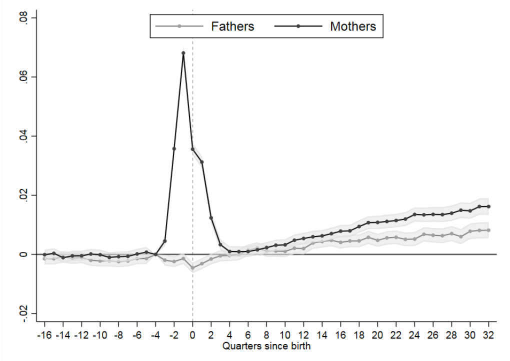
Figure 2: Impact of children on disability receipt (relative to event time -4)



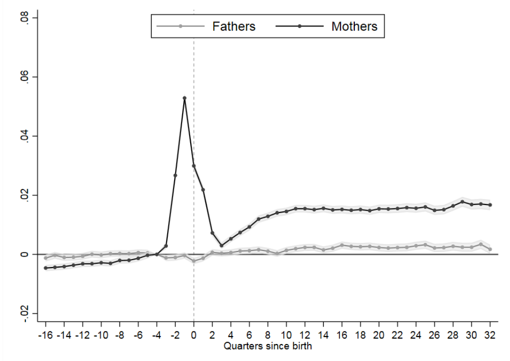
Notes: The figure shows event time coefficients estimated from equation (1) for men and women separately. All of these statistics are estimated on a sample of parents who had their first child between 2002-2013. The shaded 95% confidence intervals are based on robust standard errors.

Figure 3: Impact of children on disability receipt (relative to event time -4)
Heterogeneous effects by number of children

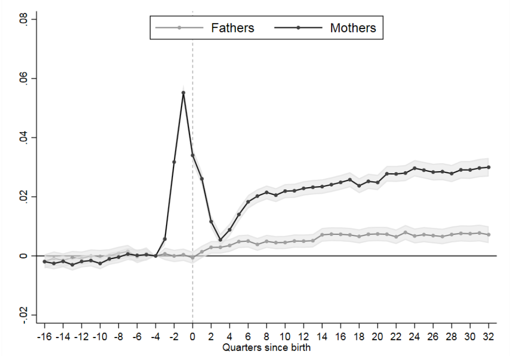
Panel A: One-child parents



Panel B: Two-child parents

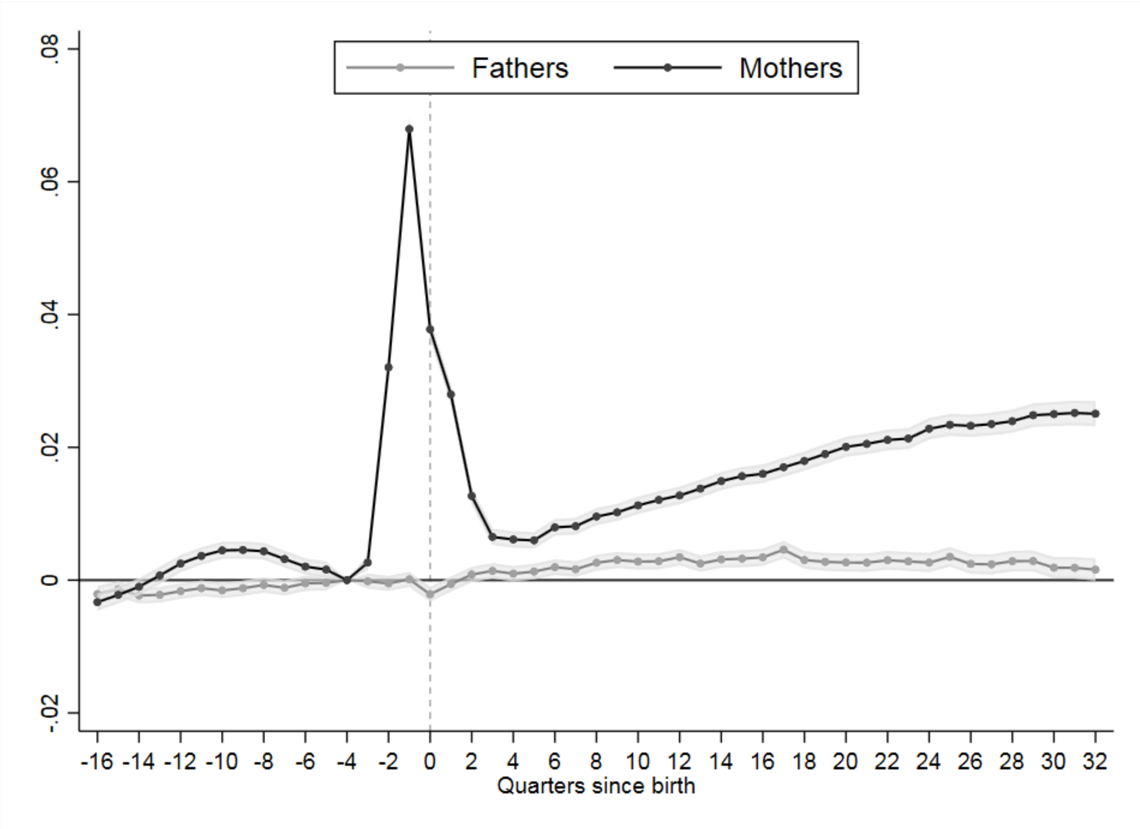


Panel C: Three-child parents



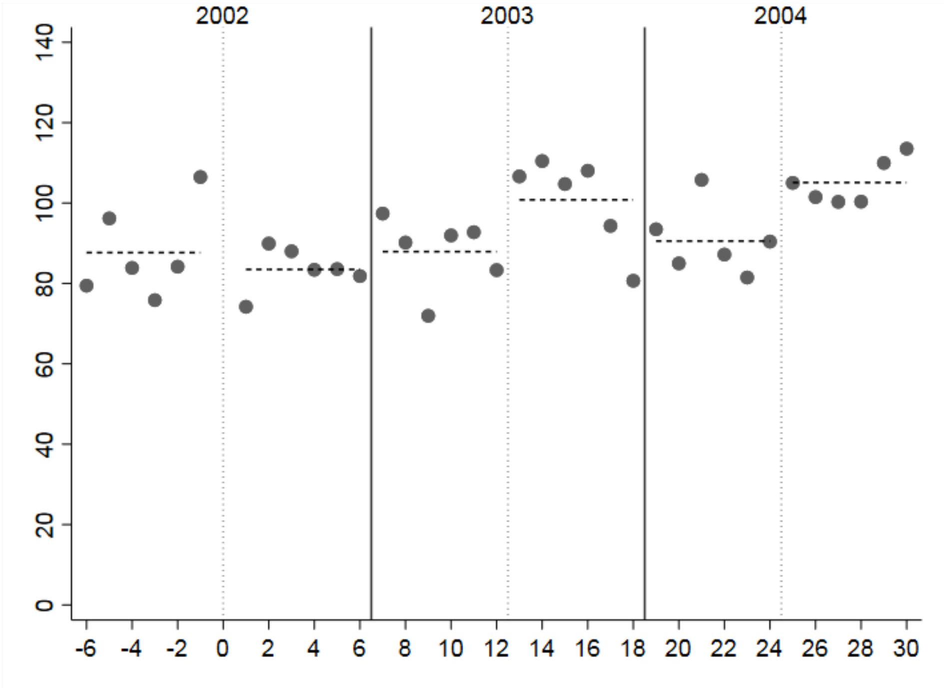
Notes: The figures show the impact of children on disability receipt as in Figure 2, but splitting the sample by the parents' total number of children as of 2016 (1,2 or 3 children).

Figure 4: Event study around second child's birth - Impact on disability receipt (relative to event time -4)



Notes: The figure shows event time coefficients estimated from equation (1) for men and women separately. t=0 is now the quarter of birth of the second child. All of these statistics are estimated on a sample of parents who have had two children in total as of 2016. The shaded 95% confidence intervals are based on robust standard errors.

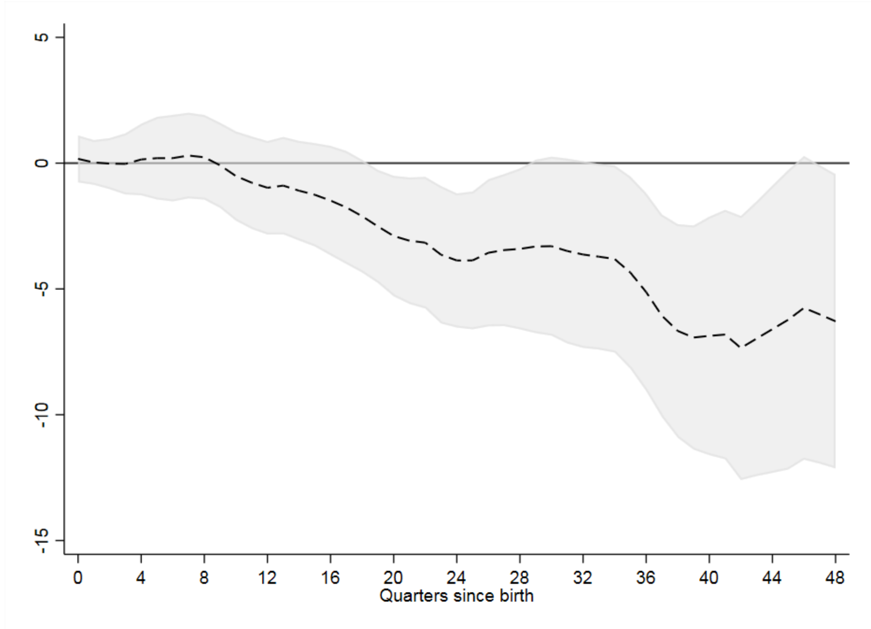
Figure 5: Seasonality - Total number of disability days 12 years after the reference child's birth



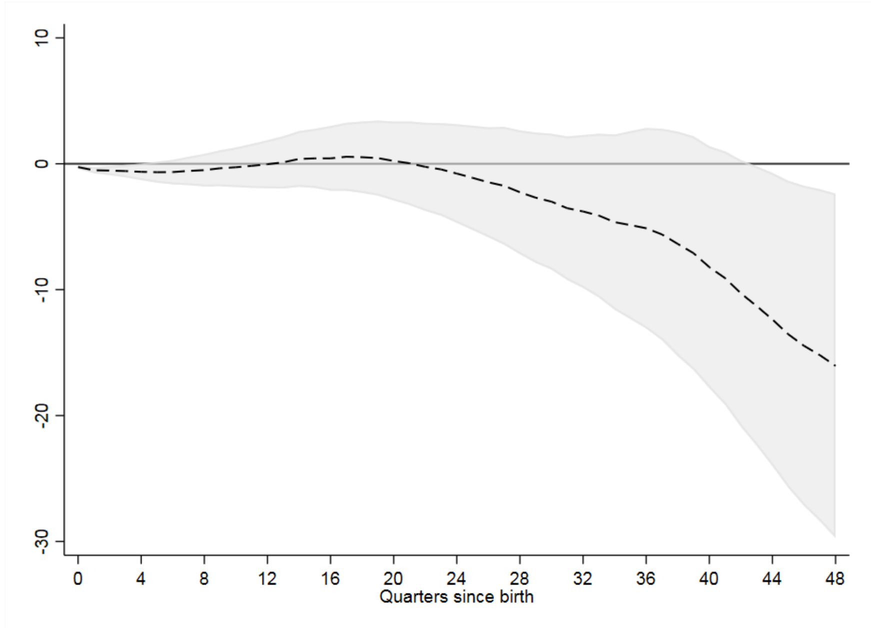
Notes: Sample of mothers who had a first child in 2002-2004. The horizontal dashed lines represent the average within a given semester. Time (horizontal axis) is indexed on the introduction of the paternity leave on the 1st of July 2002.

Figure 6: Cumulative effects of paternity leave reform on mothers' disability days

Panel A: Short-term disability



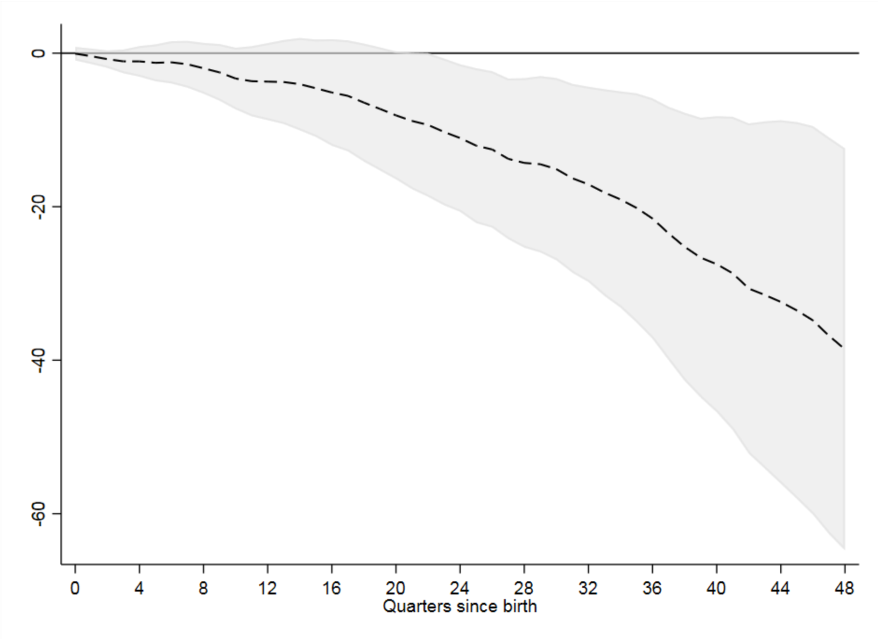
Panel B: Long-term disability



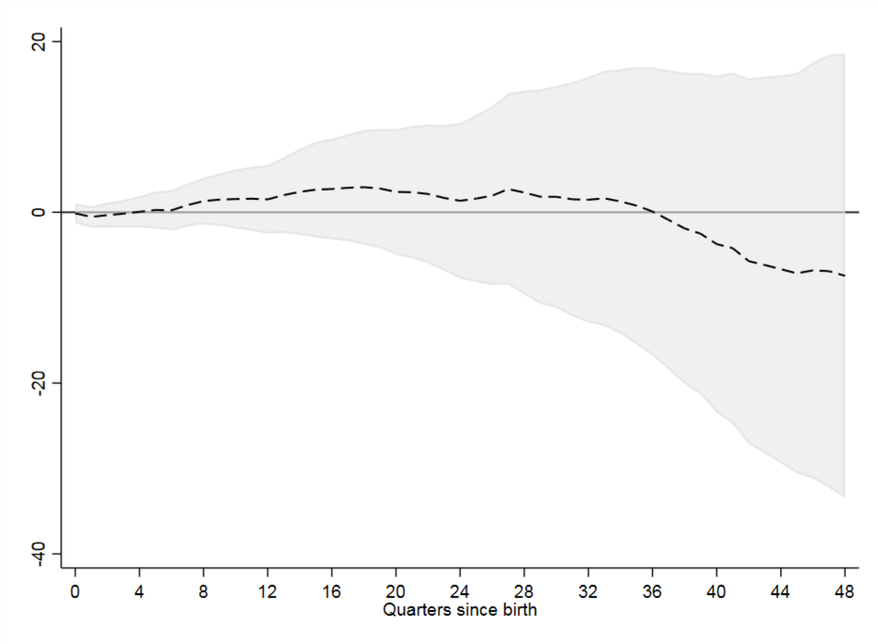
Notes: The figures show RD-DiD estimates from equation (3). All of these statistics are estimated on a sample of mothers who had a child between 2002 and 2004 and were employed at the time of the birth of the reference child. The shaded 95% confidence intervals are based on clustered standard errors at birth month level.

Figure 7: Cumulative effects of paternity leave reform on mothers' disability days (heterogeneous effects by birth order of the reference child)

Panel A: First child



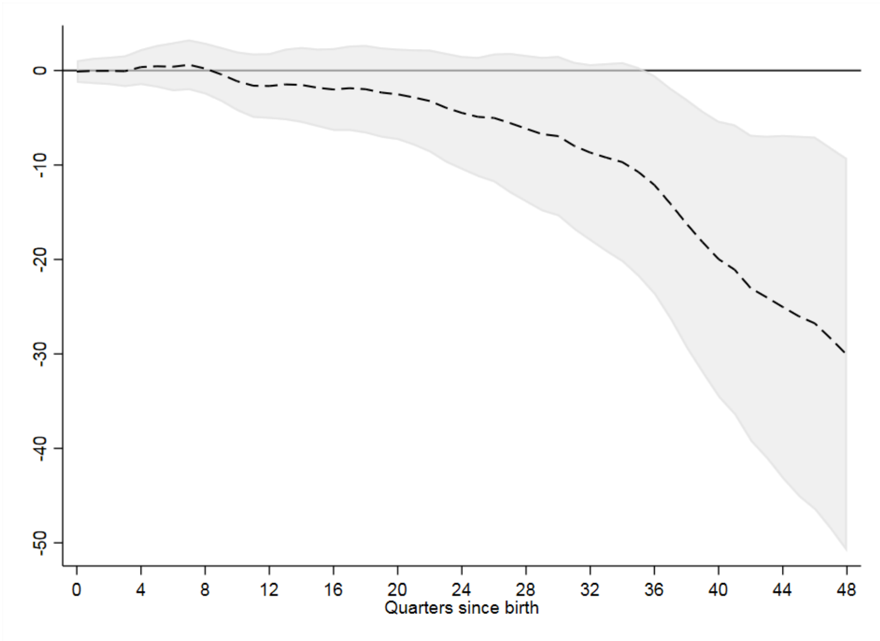
Panel B: Higher-order child



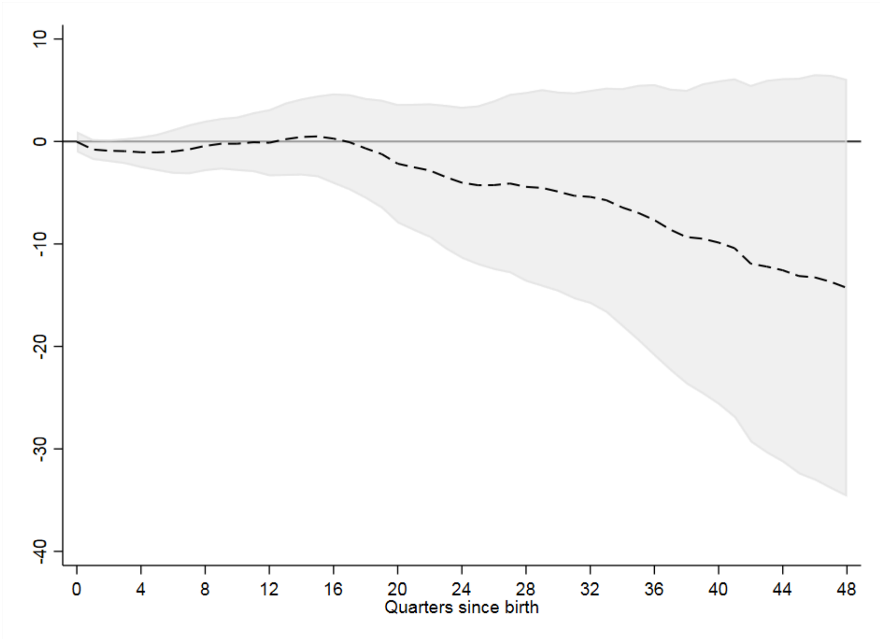
Notes: The figures show RD-DiD estimates from equation (3). All of these statistics are estimated on a sample of mothers who had a child between 2002 and 2004 and were employed at the time of the birth of the reference child. The shaded 95% confidence intervals are based on clustered standard errors at birth month level.

Figure 8: Cumulative effects of paternity leave reform on mothers' disability days (heterogeneous effects by age of mother at birth of the reference child)

Panel A: Mother < 30 years old



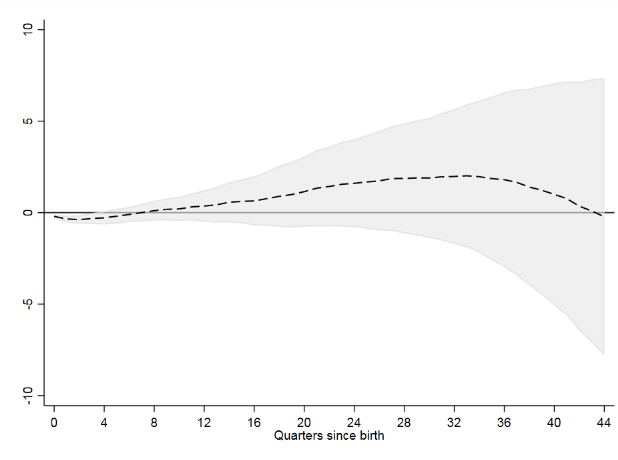
Panel B: Mother ≥ 30 years old



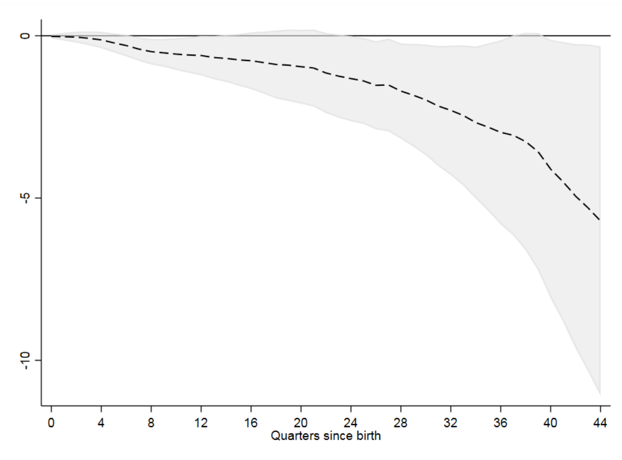
Notes: The figures show RD-DiD estimates from equation (3). All of these statistics are estimated on a sample of mothers who had a child between 2002 and 2004 and were employed at the time of the birth of the reference child. The shaded 95% confidence intervals are based on clustered standard errors at birth month level.

Figure 9: Cumulative effects of paternity leave reform on mothers' long-term disability days
(heterogeneous effects by medical condition)

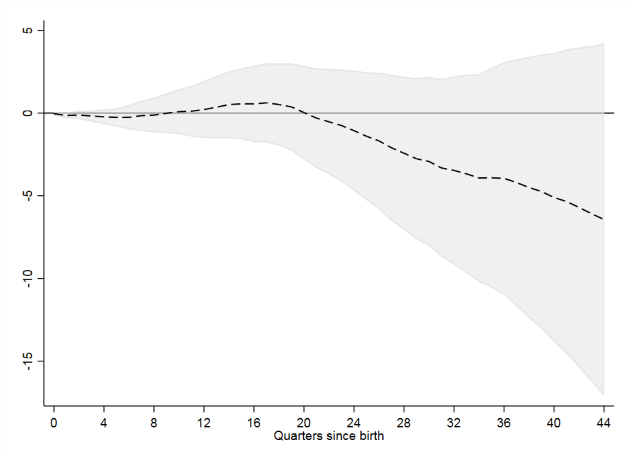
Panel A: Mental and behavioral disorders



Panel B: Musculoskeletal system or connective tissue diseases



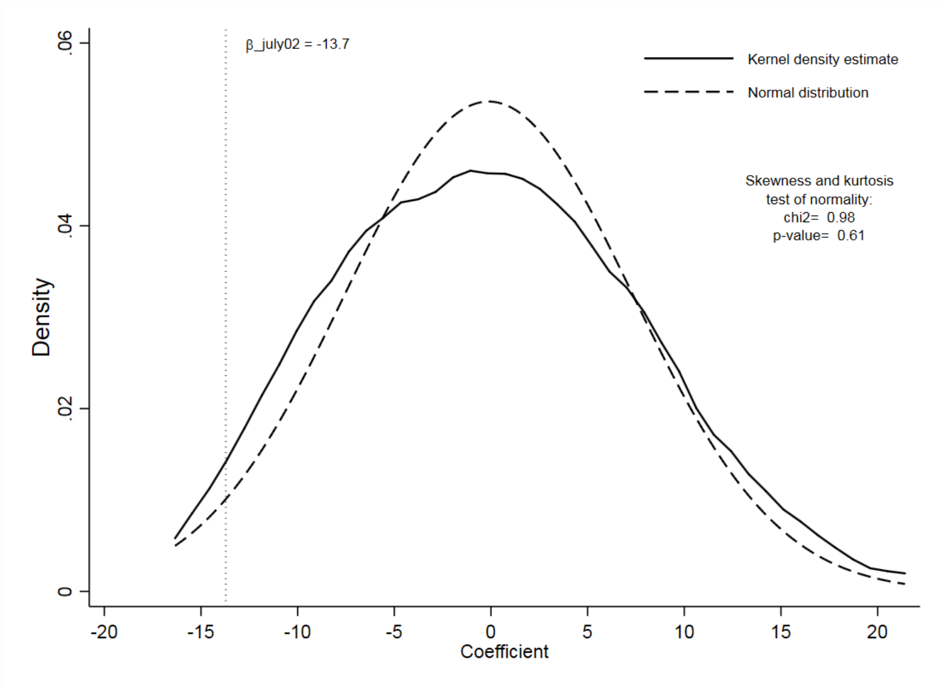
Panel C: Other diseases



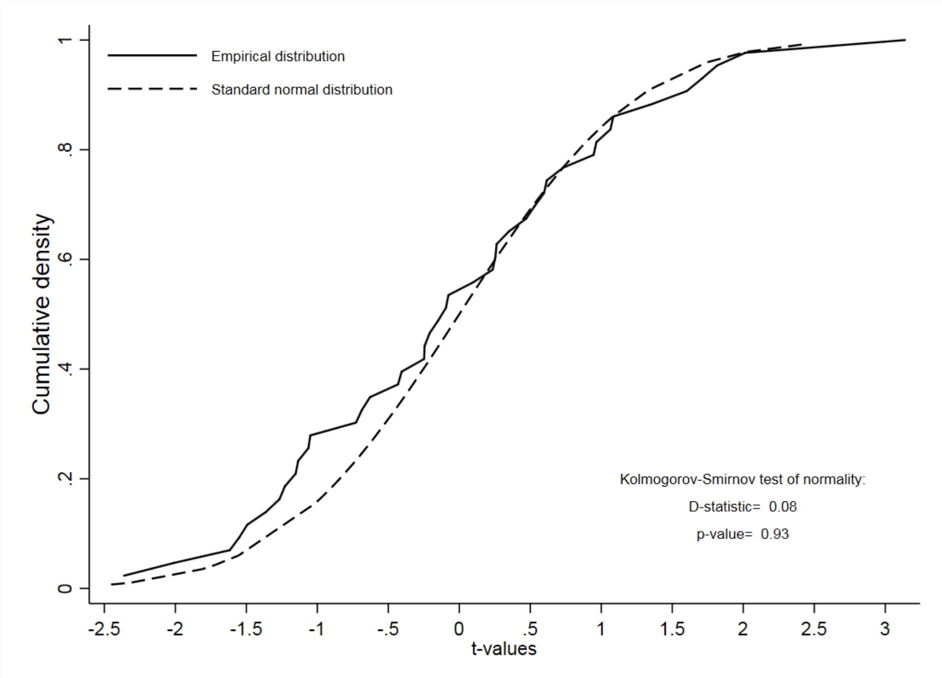
Notes: The figures show RD-DiD estimates from equation (3). All of these statistics are estimated on a sample of mothers who had a child between 2002 and 2004 and were employed at the time of the birth of the reference child. The shaded 95% confidence intervals are based on clustered standard errors at birth month level.

Figure 10: Placebo estimates for mothers' cumulative disability days after 10 years

Panel A: Placebo estimates



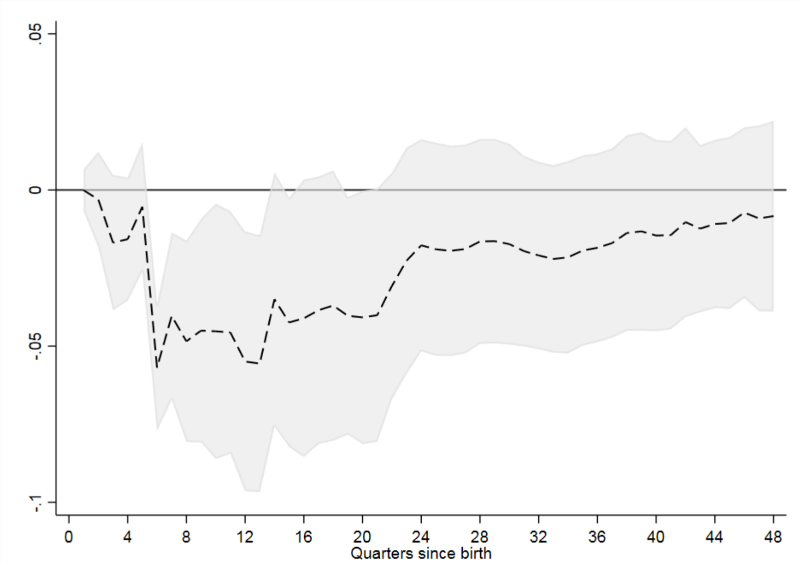
Panel B: t-values from placebo estimates



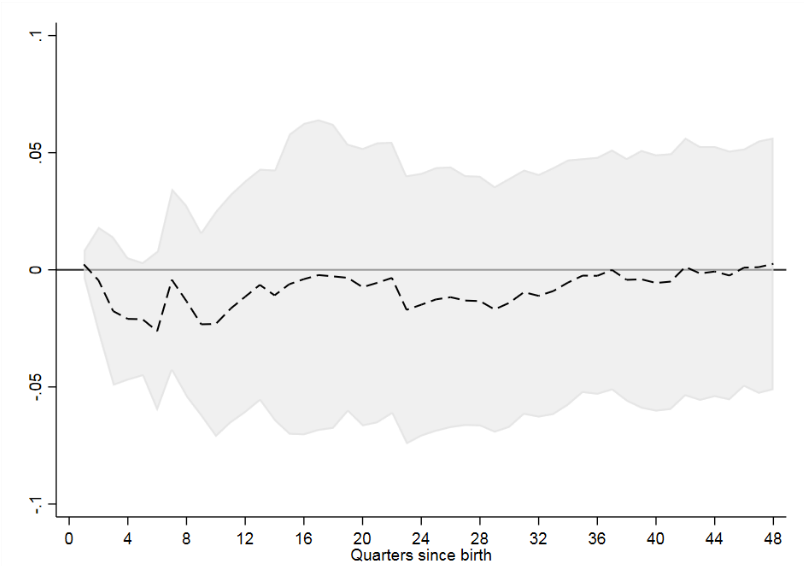
Notes: The figures show RD-DiD estimates from equation (3). All of these statistics are estimated on a sample of mothers who had a child between 2002-2006 and were employed at the time of the birth of the reference child.

Figure 11: Causal effects of the paternity leave on mothers' probability to have a second child

Panel A: Mothers < 30 years old at the birth of the reference child



Panel B: Mothers ≥ 30 years old at the birth of the reference child



Notes: The figures show RD-DiD estimates from equation (3). All of these statistics are estimated on a sample of mothers who had a first child between 2002 and 2004 and were employed at the time of the birth of the reference child. The shaded 95% confidence intervals are based on clustered standard errors at birth month level.

Table 1: Main features of the Belgian parental leave system

	Maternity leave	**Paternity leave**
Date of introduction	1971	July 2002
Duration	Max. 15 weeks (min. 1 before planned delivery + 9 after childbirth)	Max. 2 weeks (to be taken within 4 months after childbirth - initially 1 month)
Conditions	Only women who worked min. 120 days in last 6 months	Only fathers (co-parent) with salaried contract
Replacement rate	82% gross salary (first 30 days) 75% remaining days	First 3 days fully compensated Remain. 7 days 82% gross salary

Table 2: Descriptive statistics and balancing test
Control (Jan. - June) and Treament (July - Dec.)

		Mean		RDD-DiD		
		Control	Treatment	Coeff.	SE	Obs.
Household†	Live in flanders (0/1)	0,65	0,64	0,00	(0,02)	101735
	Both parents (0/1)	0,96	0,96	0,00	(0,01)	101735
	Size (#)	3,77	3,74	-0,01	(0,03)	101735
	Children (#)	1,73	1,69	0,01	(0,03)	101735
	First child (0/1)	0,46	0,49	-0,02	(0,02)	101735
Father	Age†	32,53	32,14	-0,05	(0,11)	99502
	Salaried employment (0/1)	0,83	0,83	-0,01	(0,01)	99502
	Blue collar (0/1)	0,40	0,40	-0,01	(0,02)	77946
	White collar (0/1)	0,51	0,50	0,01	(0,02)	77946
	Civil servant (0/1)	0,09	0,09	0,00	(0,01)	77946
	Self-employed (0/1)	0,15	0,16	0,00	(0,00)	99502
	Daily wage (euro)‡	96,25	94,64	-0,34	(1,54)	50765
Mother	Age†	30,35	29,91	-0,11	(0,14)	101735
	Salaried employment (0/1)	0,90	0,90	0,02	(0,01)	101735
	Blue collar (0/1)	0,17	0,18	-0,02	(0,02)	84993
	White collar (0/1)	0,72	0,73	0,02	(0,02)	84993
	Civil servant (0/1)	0,11	0,10	0,00	(0,01)	84993
	Self-employed (0/1)	0,09	0,09	-0,01	(0,01)	101735
	Daily wage (euro)‡	77,48	78,88	0,45	(2,32)	51840

*Notes: Columns 1-2 report means for the two groups who had a child in 2002. Columns 3-6 report results from RD-DiD regressions including also those who had a child in 2003 and 2004. All samples include fathers and mothers who were employed at the time of birth. Standard errors (reported in column 5) are clustered at birth month level. † Outcomes measured on Dec. 31 of each year. ‡ Outcomes measured the quarter before birth; sample limited to 3 months window. Since the data start in 2002, we cannot observe the outcomes for those who had a child between Jan. and March 2002. *** p<0.01, ** p<0.05, * p<0.1.*

Table 3: Effects of paternity leave reform on maternal outcomes 12 years after reference child's birth

	Coeff/SE		Mean
Ever on DI	0,005		0,400
	(0,012)		
Short-term (less than 12 months)	0,007		0,398
	(0,012)		
Long-term (more than 12 months)	-0,009		0,061
	(0,006)		
Cumulative days on DI	-22,3	**	104,7
	(8,9)		
Short-term (less than 12 months)	-6,3	**	55,4
	(3,0)		
Long-term (more than 12 months)	-16,1	**	49,3
	(7,0)		
Cumulative DI benefits	-712,2	**	4.055
	(302,2)		
Short-term (less than 12 months)	-156,7		2.197
	(118,9)		
Long-term (more than 12 months)	-555,5	**	1.858
	(227,1)		
Number of observations	101.735		

*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*

Table 4: Effects of paternity leave reform on maternal outcomes 12 years after reference child's birth (heterogeneous effects by birth order of the reference child)

	First child			Higher-order child	
	Coeff/SE		Mean	Coeff/SE	Mean
Ever on DI	-0,005		0,407	0,014	0,393
	(0,014)			(0,017)	
Short-term (less than 12 months)	-0,004		0,405	0,016	0,391
	(0,014)			(0,017)	
Long-term (more than 12 months)	-0,021	**	0,056	0,001	0,066
	(0,009)			(0,009)	
Cumulative days on DI	-38,6	***	96,8	-7,4	111,7
	(13,4)			(13,3)	
Short-term (less than 12 months)	-9,7	**	53,0	-3,1	57,5
	(4,7)			(3,9)	
Long-term (more than 12 months)	-28,9	***	43,8	-4,4	54,2
	(10,5)			(10,4)	
Cumulative DI benefits	-1.322,2	***	3.806	-159,3	4.270
	(431,1)			(452,8)	
Short-term (less than 12 months)	-297,6	*	2.146	-29,0	2.238
	(161,8)			(150,3)	
Long-term (more than 12 months)	-1.024,6	***	1.660	-130,3	2.032
	(360,1)			(348,6)	
Number of observations	48.505			53.230	

*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*

Table 5: Effects of paternity leave reform on maternal outcomes 12 years after reference child's birth (heterogeneous effects by age of mother at birth)

	Less than 30			More than 30 or equal	
	Coeff/SE		Mean	Coeff/SE	Mean
Ever on DI	0,005		0,450	0,006	0,358
	(0,014)			(0,013)	
Short-term (less than 12 months)	0,006		0,449	0,009	0,356
	(0,014)			(0,013)	
Long-term (more than 12 months)	-0,016	**	0,060	-0,003	0,062
	(0,008)			(0,007)	
Cumulative days on DI	-30,1	***	104,8	-14,3	104,7
	(10,6)			(10,4)	
Short-term (less than 12 months)	-8,5	*	61,6	-3,7	50,4
	(4,2)			(2,6)	
Long-term (more than 12 months)	-21,6	**	43,2	-10,6	54,3
	(8,3)			(8,6)	
Cumulative DI benefits	-1.179,0	***	3.962	-273,4	4.131
	(411,1)			(360,1)	
Short-term (less than 12 months)	-342,5	*	2.343	18,3	2.077
	(182,3)			(104,2)	
Long-term (more than 12 months)	-836,6	***	1.619	-291,7	2.054
	(296,0)			(294,9)	
Number of observations	45.751			55.984	

*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*

Table 6: Effects of paternity leave reform on maternal outcomes 11 years after reference child's birth (heterogeneous effects by type of disease)

	Cumulative days on long-term DI		
	Coeff/SE		Mean
All conditions	-12,4	**	39,9
	(5,9)		
Mental disorders	-0,2		14,8
	(3,9)		
Diseases of musculoskeletal system and connective tissue	-5,7	**	9,6
	(2,7)		
Other	-6,4		15,4
	(5,4)		
Number of observations	101.735		

*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*

Table 7: Effects of paternity leave reform on maternal outcomes 12 years after reference child's birth (varying bandwidth)

	6 Coeff/SE		5 Coeff/SE		4 Coeff/SE		3 Coeff/SE		2 Coeff/SE		1 Coeff/SE		Donut-hole Coeff/SE	
Ever on DI	0,005		0,022	**	0,022		0,016		-0,012		0,003		0,003	
	(0,012)		(0,010)		(0,013)		(0,014)		(0,013)		(0,006)		(0,017)	
Short-term	0,007		0,024	**	0,024	*	0,018		-0,008		0,005		0,004	
	(0,012)		(0,010)		(0,013)		(0,014)		(0,013)		(0,006)		(0,017)	
Long-term	-0,009		-0,011	**	-0,011	**	-0,011	*	-0,004		-0,008	***	-0,011	
	(0,006)		(0,004)		(0,004)		(0,005)		(0,004)		(0,000)		(0,010)	
Cumulative days on DI	-22,3	**	-19,5	**	-20,7	**	-28,1	***	-31,2	***	-20,5	***	-19,2	
	(8,9)		(7,9)		(7,4)		(7,5)		(9,2)		(3,4)		(11,9)	
Short-term	-6,3	**	-4,5	**	-3,0		-7,3	**	-3,7	*	-3,9	***	-8,0	*
	(3,0)		(2,1)		(2,1)		(2,7)		(1,9)		(0,2)		(3,9)	
Long-term	-16,1	**	-15,0	**	-17,7	**	-20,7	***	-27,4	***	-16,6	***	-11,2	
	(7,0)		(6,9)		(7,0)		(6,9)		(8,5)		(3,2)		(9,2)	
Cumulative DI benefits	-712,2	**	-588,2	**	-500,3	*	-833,4	***	-924,7	**	-626,4	***	-677,3	
	(302,2)		(245,7)		(280,2)		(282,1)		(371,3)		(148,5)		(424,7)	
Short-term	-156,7		-91,9		3,5		-188,1		-19,7		-67,7		-241,5	
	(118,9)		(85,7)		(104,2)		(113,3)		(118,3)		(39,8)		(165,0)	
Long-term	-555,5	**	-496,3	**	-503,8	*	-645,4	**	-905,1	***	-558,7	***	-435,8	
	(227,1)		(213,5)		(247,8)		(232,8)		(279,4)		(108,7)		(301,3)	
Number of observations	101.735		84.987		68.902		51.840		34.784		17.424		84.311	

Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8: Effects of paternity leave reform on maternal outcomes
12 years after reference child's birth
(varying polynomial order)

	Linear Coeff/SE		Quadratic Coeff/SE		Cubic Coeff/SE	
Ever on DI	0,005		0,041	**	-0,057	
	(0,012)		(0,019)		(0,035)	
Short-term (less than 12 months)	0,007		0,045	**	-0,055	
	(0,012)		(0,019)		(0,035)	
Long-term (more than 12 months)	-0,009		-0,013	*	0,000	
	(0,006)		(0,007)		(0,013)	
Cumulative days on DI	-22,3	**	-20,7	*	-51,5	**
	(8,9)		(11,5)		(19,6)	
Short-term (less than 12 months)	-6,3	**	-1,6		-11,1	
	(3,0)		(3,2)		(7,2)	
Long-term (more than 12 months)	-16,1	**	-19,2	*	-40,4	**
	(7,0)		(10,6)		(16,9)	
Cumulative DI benefits	-712,2	**	-466,1		-1.541,3	*
	(302,2)		(424,5)		(782,1)	
Short-term (less than 12 months)	-156,7		53,9		-266,6	
	(118,9)		(146,8)		(331,4)	
Long-term (more than 12 months)	-555,5	**	-520,0		-1.274,7	**
	(227,1)		(357,7)		(611,8)	
Number of observations	101.735		101.735		101.735	

*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*

Table 9: Effects of paternity leave reform on mothers' subsequent fertility

	Less than 30			More than 30 or equal	
	Coeff/SE		Mean	Coeff/SE	Mean
Other child					
After 1 year	-0,016		0,080	-0,021	0,077
	(0,010)			(0,013)	
After 2 years	-0,048	***	0,360	-0,013	0,309
	(0,016)			(0,021)	
After 3 years	-0,055	**	0,584	-0,011	0,472
	(0,021)			(0,025)	
After 6 years	-0,018		0,775	-0,015	0,607
	(0,017)			(0,029)	
After 12 years	-0,008		0,831	0,003	0,641
	(0,016)			(0,027)	
Total number of children after 12 years	-0,1		2,1	0,0	1,8
	(0,0)			(0,1)	
Number of observations	28.449			18.108	

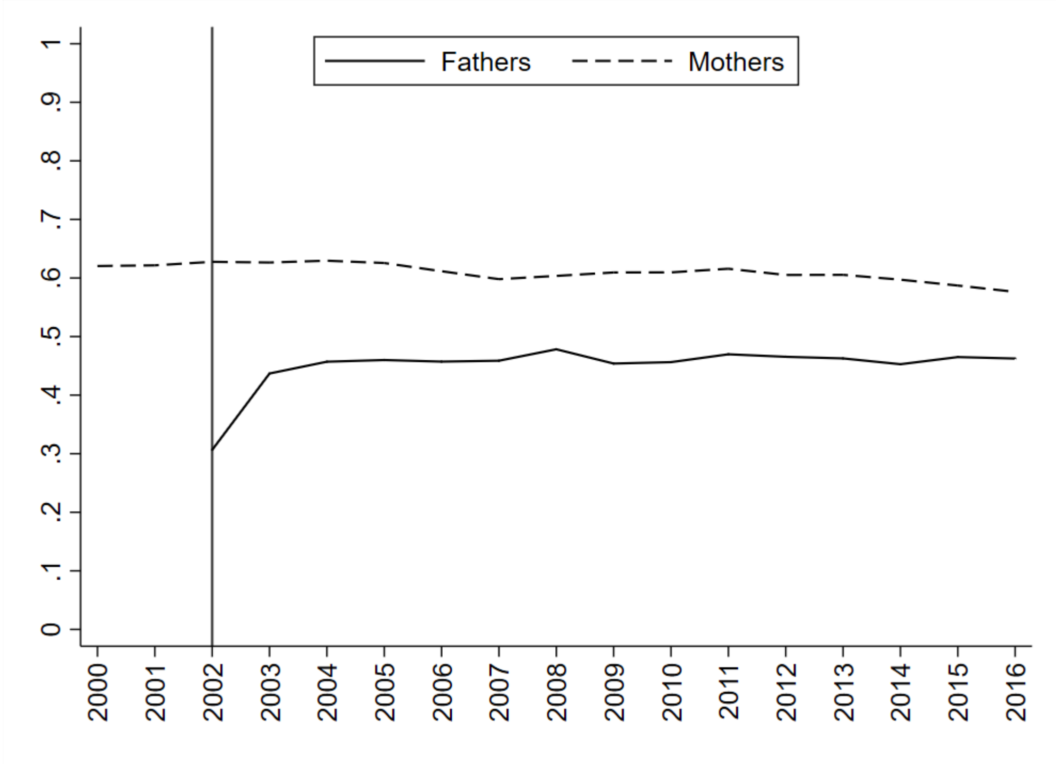
*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who had a first child between 2002 and 2004 and were employed at the time of the birth of the reference child. The dependent variable "other child" is an indicator for the mother having another child within the following years after the reference child's birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*

Table 10: Effects of paternity leave reform on maternal outcomes 8 years after the second child's birth (heterogeneous effects by age of mother at first child's birth)

	Less than 30			More than 30 or equal	
	Coeff/SE		Mean	Coeff/SE	Mean
Ever on DI	-0,010		0,329	0,049	0,254
	(0,021)			(0,038)	
Short-term (less than 12 months)	-0,011		0,328	0,046	0,251
	(0,020)			(0,039)	
Long-term (more than 12 months)	-0,020		0,035	-0,003	0,034
	(0,015)			(0,010)	
Cumulative days on DI	-34,8	**	55,9	4,5	49,7
	(16,9)			(13,7)	
Short-term (less than 12 months)	-10,7		35,6	-0,4	27,1
	(7,3)			(4,7)	
Long-term (more than 12 months)	-24,2	**	20,4	4,9	22,6
	(10,9)			(11,0)	
Cumulative DI benefits	-1.198,2	**	2.095	507,8	2.038
	(528,2)			(578,7)	
Short-term (less than 12 months)	-367,2		1.368	99,0	1.196
	(251,3)			(216,4)	
Long-term (more than 12 months)	-831,0	**	726	408,8	842
	(321,5)			(449,8)	
Number of observations	21.646			10.909	

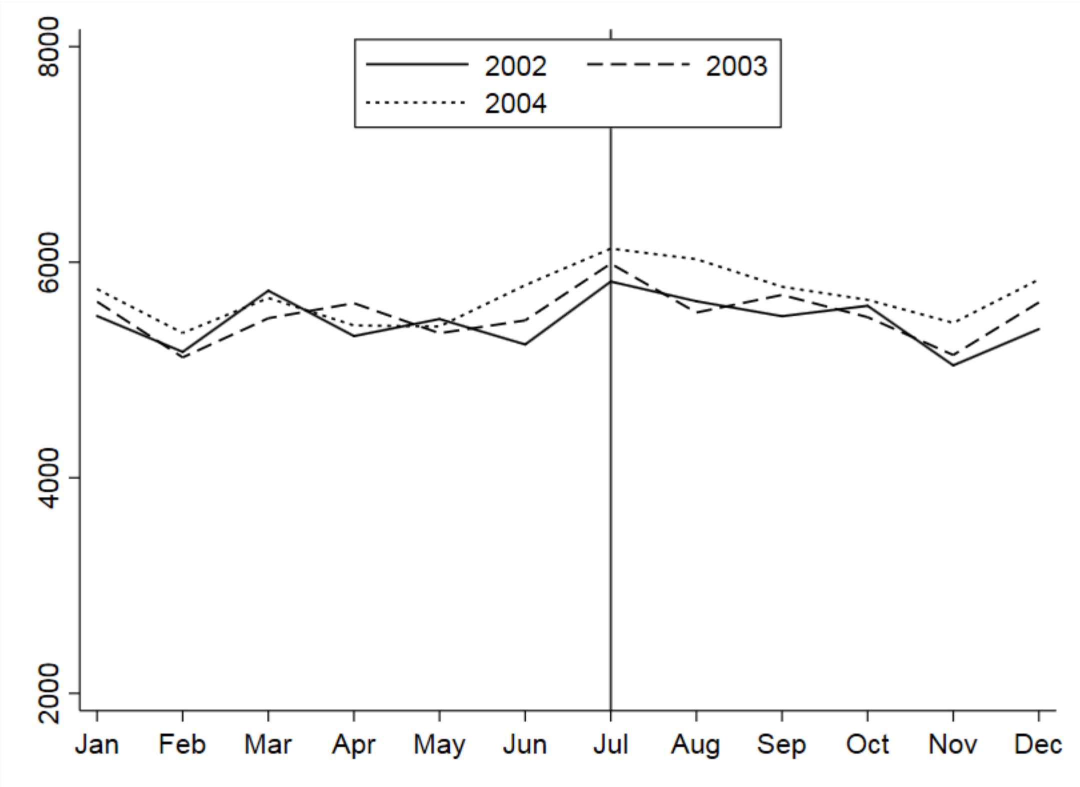
*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for mothers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes mothers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*

Appendix - Figure A1: Number of fathers/mothers taking paternity/maternity leave as a fraction of the annual number of births



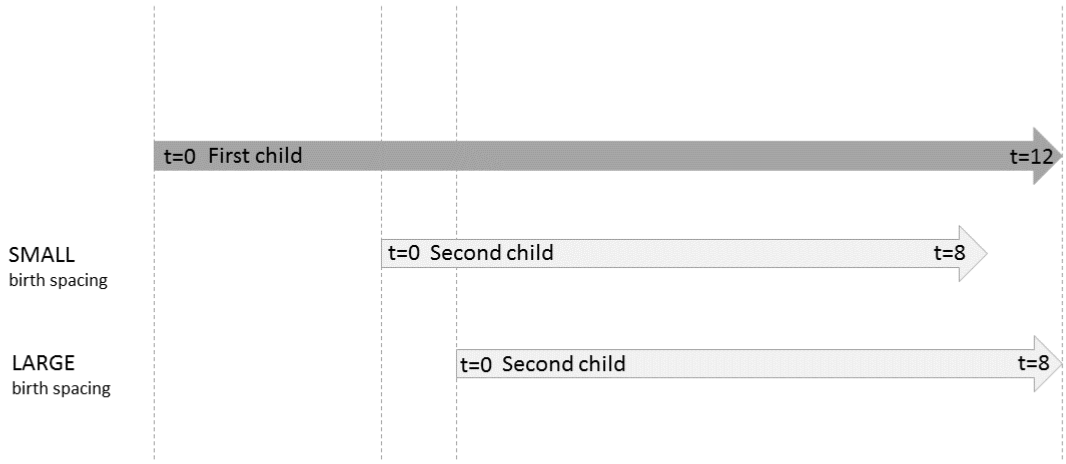
Notes: For the year of the reform, we only consider births from July to December 2002. Data sources: National Institute for Health and Disability Insurance (leave-takers) and StatBel (births).

Appendix - Figure A2: Number of births per month for reform year (2002) and non-reform years (2003 and 2004)



Data source: StatBel.

Appendix - Figure A3: New follow-up period indexed on second child's birth



Appendix - Table A1: Time use survey - Belgium 2013

	Men	Women	Diff.
Paid work	5:01	3:57	- 1:04
Household work	1:54	2:58	+ 1:04
Childcare and raising children	0:33	1:05	+ 0:32
Personal care	2:15	2:24	+ 0:09
Sleep and rest	8:17	8:29	+ 0:12
Education	0:06	0:06	+ 0:00
Social participation	1:14	1:10	- 0:04
Free time	3:13	2:23	- 0:50
Transportation	1:25	1:24	- 0:01
Other	0:03	0:05	+ 0:02

Notes: Household with both parents working and children.

Appendix - Table A2: Effects of paternity leave reform on paternal outcomes 12 years after the reference child's birth

	Coeff/SE		Mean
Ever on DI	-0,029	***	0,314
	(0,010)		
Short-term (less than 12 months)	-0,028	***	0,312
	(0,010)		
Long-term (more than 12 months)	-0,001		0,035
	(0,005)		
Cumulative days on DI	-2,6		65,5
	(6,0)		
Short-term (less than 12 months)	-4,3		37,4
	(2,7)		
Long-term (more than 12 months)	1,7		28,0
	(4,2)		
Cumulative DI benefits	-98,2		2.999
	(250,7)		
Short-term (less than 12 months)	-176,4		1.866
	(142,7)		
Long-term (more than 12 months)	78,2		1.133
	(158,4)		
Number of observations	99.502		

*Notes: This table reports RD-DiD estimates based on equation (3) and using the reform (2002) and non-reform years (2003 -2004). Regressions control for fathers' age, number of children, as well as region of living, at the moment of the birth of the reference child. The sample includes fathers who were employed at the time of birth. Standard errors (reported in parentheses) are clustered at birth month level. Sample means are reported in the second column. *** p<0.01, ** p<0.05, * p<0.1.*