

Effects of Extended Paternity Leave on the Relative Earnings and Union Stability of Parental Couples

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Abstract

Abstract Long paternity leaves have the potential to leave lasting effects on parental unions, potentially reducing specialization and increasing union stability. We put these hypotheses to a causal test, using an extension of the Norwegian parental leave father's quota from 6 to 10 weeks as a source of exogenous variation in fathers' leave uptake. We implement a Regression Discontinuity design, using full population data from Norwegian administrative registers of parents of children in a four month window around the reform ($N = 9\,516$). The reform significantly increased the amount of leave taken by fathers by about three weeks and reduced the amount of leave taken by mothers by about four weeks. Neither union stability nor his or her earnings were affected by the reform. The null finding suggests that extended paternity quotas do not equalize or stabilize unions -- nor do they intensify fatherhood penalties.

Keywords: Union dissolution, father involvement, quasi experiment
JEL codes: J12, J13, J16, J18

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1 Introduction

The vast increase in mothers' employment is among the seminal changes of Western societies in the last part of the 20th century. This increase spurred economic growth and established economic independence for women, transforming the everyday lives of families with children (Becker, 1991; Goldscheider et al., 2015; Lestaege, 2010). As mothers shifted time from unpaid to paid work, fathers increased their efforts at home. Still, parenthood continues to intensify gender differences in earnings (see e.g. Browning (1992); Cools and Strøm (2016); Lundberg and Rose (2000)), and while fathers do more at home, mothers continue to do most (Hook, 2006). This has given rise to a notion of a “stalled” (Hochschild and Machung, 2012) or “incomplete” (Goldscheider et al., 2015) gender revolution. Newer contributions have suggested that a deficit of gender equality in families could additionally reduce family well-being and, in turn, union stability (Goldscheider et al., 2015; Cooke, 2006; Esping-Andersen and Billari, 2015). This proposition is corroborated by studies showing that low father involvement is associated with lower female relationship satisfaction (Kaufman, 2000; Barstad, 2014) and lower union stability (Ruppanner et al., 2017; Sigle-Rushton, 2010). Specifically, for Norway, Sweden and Iceland, Lappegård et al. (2014, in Goldscheider et al. (2015)) find that shorter paternity leave correlates with lower union stability in all these countries. As fathers who are more committed to their partner may spend more time on care work, these associations need not indicate a causal effect.

This study addresses the potential of extended father quotas in equalizing and stabilizing parental unions. Gender equal and stable parental unions are considered politically desired outcomes, and while the division of unpaid work is outside the realm of state regulations, state compensated paternity leave constitutes a rare opportunity window for policy influence. Many countries have policies in place that incentivize fathers' participation in paid parental leave programs, often referred to as “father quota” or “daddy quota” policies (see e.g. Patnaik (2016) for an overview). The introduction of paternity quotas (typically of about one month length) effectively increase both the share of fathers taking leave and the number of leave days taken by fathers (cf. Cools et al. (2015) for Norway; Ekberg et al. (2013) for Sweden; Geisler and Kreyenfeld (2012) for Germany; Patnaik (2016) for Canada). Still, previous studies do not give consistent evidence that the introduction of paternity quotas have “advanced the gender

revolution”, as they are neither found to (consistently) increase mothers’ share of earnings, nor found to stabilize parental unions (Cools et al., 2015).

The current body of research leaves out policy changes and outcomes that are potentially of great importance when evaluating the effects of paternity quotas. Importantly, as the gendered division of labor is deeply entrenched in our culture, it may take time for new patterns of division of labor to take hold. Consequently, extensions of paternity quotas may have more profound effects than their introduction (i.e., effects of paternity quotas may be non-linear). Furthermore, within-couple dynamics may take time to respond, giving longer father’s quotas a larger potential to make lasting impacts on parental unions. To fully assess the equalizing potential of paternity quotas, studies that evaluate the extensions of paternity quotas are therefore called for. Furthermore, the potential of long paternity quota in stabilizing unions is clearly understudied, particularly with respect to the statistically less stable cohabiting unions.

This paper extends our knowledge on effects on paternity quotas by aiming to causally establish whether a government-induced extension of an existing paternity quota has consequences for the earnings, specialization and union stability of couples affected. We use an extension of the Norwegian paternity quota from 6 to 10 weeks, which took effect for parents of children born July 1st, 2009. The reform added two weeks to the total parental leave period, and shifted two weeks from the shared period to the period reserved for the father (NAV, 2015b)¹, incentivizing fathers to increase their time at home by four weeks and mothers to decrease their time at home by two weeks.

To estimate the effect of the extension of the father’s quota on leave uptake, earnings and union stability we employ a Regression Discontinuity design – a conservative identification strategy very unlikely to be biased by gradual changes in fathering practices over time. We compare couples with children born just before the extension of the father’s quota to couples who had a child just after this date. For precise estimation of effects for these relatively small subgroups, the sample size provided by full population data is crucial. Our final study sample consist of 9 516 parental couples who were coresiding prior to pregnancy, and where the mother was employed the year before the focal child was born (excluding most mothers not eligible for paid parental leave). All outcome variables are drawn from administrative registers, ensuring zero attrition and high validity.

Using the exogenous variation provided by the reform, we explore three main research questions. Together, these cast light on the efficiency of an extended fathers' quota in changing the gender balance in caring during the first year after the birth of a child, potentially affecting relative earnings and union stability. Our first research question regards the effect of the reform on parental leave uptake of mothers and fathers. While previous studies have demonstrated that the introduction of fathers quotas increase leave uptake by fathers, we do not know if families continue to comply as policy changes dictate relatively long absences from paid work for new fathers. Changes in the uptake of parental leave will closely reflect changes in caring practices in the first year, and are of interest in their own right, as active fathering has shown to enhance men's experience of becoming a father and spending time with their children (Rehel, 2014). If the reform moves leave uptake, this allows for testing for policy effects on specialization and stability of parental couples.

Our second research question regards the effect of an extended parental leave on the relative earnings development of mother and fathers. According to classic economic theory (Becker, 1991), the division of care for a small child can have lasting effects on the level of specialization between parents. We estimate effects of the reform on each parent's earnings, as well as the mothers' share of couple earnings, to disentangle the components that bring about any observed change in specialization and provide a more nuanced account of policy effects. Positive effects on her earnings are indicative of a more gender egalitarian division of paid and unpaid work. A decrease in fathers' earnings can be (and often is) interpreted as a(nother) sign of his increased efforts in unpaid work, and hence a desired policy outcome given the underlying goal of increasing gender equality. However, we note that a negative effect on his earnings also will emerge if extended paternity leave signals lower work commitment, leading to subsequent wage discrimination. Hence, a zero (or positive) effect of prolonged paternity quotas on fathers' earnings indicates that the reform has not introduced or intensified fatherhood wage penalties.

Our third and final research question regards the effect of father involvement on the intactness of the marriage or cohabiting union the focal child is born into. A more gender equal division of labor is associated with higher marital satisfaction and stability (Amato, 2007). To the best of our knowledge, the only example of a causal design to identify effects of father involvement on union stability to date is Cools et al. (2015), who find no significant effects of the introduction

of the four-week paternity quota in Norway in 1993 on marital stability when the focal child is 14 years old. We expand upon this study both by including the more fragile cohabiting unions, potentially more easily moved, and by investigating if a paternity leave of longer duration has more profound effects on union stability.

The results show that the reform induced fathers to immediately increase their leave length by three weeks, while mothers decreased their leave length by about four. In other words, families respond quickly to the extension of paternity quota, in line with what previous studies has shown for the introduction of such quotas. Despite this considerable change in leave uptake, we find no significant effects on any earnings outcome, nor union stability. A battery of robustness checks, including a placebo reform, supports a causal interpretation of our results.

Due to the large changes in caring practices invoked by the reform, our results can serve as a conservative empirical test of propositions of the potential effects on father involvement on labor market outcomes and union stability. While conclusions are necessarily tentative, our results yield little support to the potential of father involvement to equalize labor market outcomes and stabilize unions, and accentuate the potential importance of selection in producing the previously observed associations between father involvement on one side, and union stability and high maternal labor supply on the other.

Our results have important implications for policy. From a gender equality perspective, it might be seen as discouraging that our findings cast some doubt on the efficiency of extended paternity quotas in equalizing and stabilizing parental unions. Equally important, however, our results suggests that fathers who were moved by the reform to stay out of the labor market for an extended period of time experienced no (increased) fatherhood penalty. At least in a family friendly work culture, our results indicate that fathers may take prolonged work extensions to care for their newborn, without fear of facing subsequent earnings penalties.

2 Theoretical framework and previous research

Across Western societies, the birth of a(nother) child fuels gender specialization. Becker (1991, p. 39) suggests that this process is offset by women's small biological comparative advantage in nursing and care for newborns, and there is ample evidence that gendered expectations

and norms play a role in manifesting and strengthening specialization (Bittman et al., 2003; Brines, 1994; Hochschild and Machung, 2012; Ono and Raymo, 2006; West and Zimmerman, 1987). While the negative consequences of specialization for women’s career and economic independence are obvious, specialization was long expected to increase the gain from marriage and stabilize unions (Becker, 1991). Numerous empirical and theoretical contributions have later challenged that specialization is necessary for union stability (Esping-Andersen and Billari, 2015; Goldscheider et al., 2015; Oppenheimer, 1997). In this section, we first explore how long paternity leaves may affect the division of paid and unpaid work in the family, and then turn to the potential impact of paternity leave on union stability.

2.1 Paternity leave and gender specialization

If gender specialization is a cumulative or self-strengthening process, as suggested by Becker (1991), increasing fathers’ participation in non-market work after the birth of a child can have a lasting impact on division of paid and unpaid work in the family. Increasing the length of fathers’ parental leave may counteract the process of specialization by enabling mothers to return to paid work sooner, reducing her human capital depreciation, and/or by improving fathers’ domestic skills. If an extended father’s quota increases the father’s skills in home production, the process of specialization may be then slowed down or even stopped. In support of this, Rehel (2014) finds that fathers acquired both new domestic skills and strengthen their emotional bonds with children after about one month of parental leave. Similarly, using a resource-bargaining perspective, Lundberg and Pollak (1996) suggest that strengthening of non-market skills among fathers and market skills among mothers lead to lasting, de-specializing impacts of the division of labor within families. Both the bonding between father and child and the acquisition of household skills can be stronger if fathers spend their leave without the presence of the mother, who in most cases holds a comparative advantage in house and childcare work.

Numerous studies have assessed the relationship between paternity leave and various family and child outcomes (see e.g. Schober (2014); Patnaik (2016) for a review), and their findings primarily confirm that higher uptake of paternity leave is correlated with a more equal division paid and domestic work. Similarly, Hook (2006) shows in a cross-country comparison that average father’s time in childcare is higher in countries with generous paternity leave policies.

The fact that fathers who take longer leave also participates more in (other) household labor, is not evidence of a causal effect of paternity leave: these associations are vulnerable to endogeneity issues as parents who share the parental leave between them typically differ from those who opt for the mother taking all the leave. In Norway for instance, Naz (2010) show that married fathers use more leave than cohabitants, and that fathers' education, mothers' income and the number of preschool children positively affect fathers' use of both the paternity quota and the leave that can be shared freely between the parents. Lappegård (2008) shows that when fathers takes more leave when his and her earned income is approximately equal (though not when her income exceeds his, a finding probably related to characteristics of low-earnings fathers in these couples), and that his leave uptake increases in her, but not his, earned income.

A small number of quasi-experimental studies address the de-specializing and earnings-equalizing potential of father's quotas. Supporting the idea of de-specialization, some studies find positive effects on fathers' participation in child care (Cools et al., 2015; Schober, 2014) and house work (Kotsadam and Finseraas, 2011; Patnaik, 2016), and increases in mothers' labor supply (Kluve and Tamm, 2013; Patnaik, 2016). Meanwhile, other studies report negative effects on mothers' earnings (Cools et al., 2015) and labor supply (Ekberg et al., 2013), and increases in mothers' time spent on child care (Patnaik, 2016). Yet again, most studies find no causal effect on neither fathers' (Cools et al., 2015; Ekberg et al., 2013) nor mothers' (Rege and Solli, 2013) income, fathers' labor supply (Cools et al., 2015; Ekberg et al., 2013; Kluve and Tamm, 2013; Patnaik, 2016), or fathers' (Ekberg et al., 2013; Kluve and Tamm, 2013; Schober, 2014) or mothers' (Schober, 2014) participation in child care or house work. Notably, for Norway Rege and Solli (2013) identify a substantial negative effect of the 1993 introduction on fathers' earnings using a difference-in-difference design. Using a more conservative identification strategy, Cools et al. (2015) find no (negative) effects on father's earnings of the same reform.

In sum, while father's quotas seem to have led to a more gender egalitarian division of house and care work, there is not consistent evidence that this translates into a more egalitarian division of market work. On the contrary, it is striking that there even is some evidence that mothers *reduce* their efforts in market work as a consequence of increased paternity leave, indicating that paternal and maternal efforts at home may to some extent be complements rather than substitutes.

2.2 Father involvement and union stability

According to Amato (2007), conflict over unpaid work is among the major sources of marital dissatisfaction. Greenstein (2009) finds that a traditional division of unpaid labor is associated with lower relationship satisfaction for women in countries where men and women tend to share paid work. This pattern is confirmed in single country studies showing a negative association between traditional division of unpaid labor and women's relationship satisfaction (see e.g. Frisco and Williams (2003); Kaufman (2000) and Stevens et al. (2001) for the US; Kluwer et al. (1996) for the Netherlands; Barstad (2014) for Norway; Oláh and Gähler (2014) for Sweden). Men's efforts at home is also associated with lower union dissolution risks (Cooke (2006) for US; Sigle-Rushton (2010) for the UK; Ruppanner et al. (2017) for Sweden). Oláh and Gähler (2014) find that the combination of a gender equal ideology with a gender traditional practice lowers union stability among young Swedish coresidential couples. Similarly, Ruppanner et al. (2017) find that an unequal division of unpaid work is particularly detrimental to union stability if the extra work is put in by a woman, and not appreciated by her partner. These studies cannot be interpreted causally, as men who are satisfied with their union may be more inclined to do house and care work, and unmeasured characteristics such as personality traits may influence both men's housework and union stability. Still, they form the basis of the empirically testable prediction that his increased efforts at home will stabilize unions.

Mechanisms linking longer parental leave to increased union stability tend to depend on the parental leave invoking a lasting change in the division of household labor.² If the father increases his efforts at home, the mother's relationship satisfaction may increase, due to increased *perceived fairness* of the division of housework, and/or because house- and childcare may be more enjoyable as a shared than a solitary activity. The idea of perceived unfairness is rooted in equity theory (Adams (1965), see Lively et al. (2008) for applications to family research), which proposes that unfair social relationships give a feeling of distress, leading (particularly the discredited) actors to dissolve them.

There are few previous studies that address the effect of paternity leave on union stability. Lappegård et al. 2014 (in Goldscheider et al. 2015) find that a (somewhat) longer leave for fathers correlates with union stability throughout the Nordic countries (see also Oláh (2001) for

a similar result for Sweden only). The authors acknowledge that this finding may fully or partly be driven by selection, i.e. more stable couples sharing leave more equally. Cools et al. (2015) use exogenous variation in paternity leave uptake to estimate effects on a range of outcomes for parents and children, including marital dissolution when the child is 14 years old. Variation in paternity leave is obtained by analyzing the 1993 introduction of a paternity quota in Norway in a research set-up combining a discontinuity design and difference-in-difference approach, and the findings show no effect of the father’s quota introduction on marital stability. Our study is distinct from Cools et al. (2015) in two important aspects. First, as discussed above, longer parental leave may have a more profound impact on division of unpaid work, and hence also on the propensity to dissolve a union. Second, due to data limitations in older administrative registers in Norway, Cools et al. (2015) are only able to assess effects on union stability on married couples. Knowing that cohabiting unions are consistently less stable than marriages (see e.g. Hart et al. 2017), smaller changes in relationship quality due to changes in division of unpaid work may have a larger impact on union stability in this group.

2.3 Expectations

Our first research question regards the effect of changes in parental leave rights on parental leave uptake. To our knowledge there are no previous studies that estimate the effect of extending an existing father’s quota on the parental leave uptake of mothers and fathers. However, based on the general finding that parents are typically sensitive to leave policies that reserve a part of the leave for the father, as well as the incentive structure of this particular policy reform, we expect the policy to increase fathers’ leave uptake and decrease mothers’ leave uptake. If the reform affects uptake, this may in turn have effects on his and her earnings, as well as union stability.

Our second research question regards effects of changes in parental leave uptake on (relative) earnings. The reform we study provide clear incentives to strengthen fathers’ skills in unpaid work, while speeding up mothers’ return to paid work. Classical economic theory suggest that such changes have the potential to permanently reduce specialization in market work, i.e. increase her share of the couple’s earned income. The simplest example of such a shift is that the mother’s and fathers’ efforts at home are substitutes. As fathers shift time from market production to home production, lowering his earnings, time is freed up for mothers to increase

their efforts in paid work, increasing her earned income. Because parents could also shift time between paid/unpaid work and (pure) leisure, changes in relative earnings may also be driven by effects on one parent only. If he shifts time from leisure to unpaid work, she may be freed up to work and earn more, while his earnings are unmoved. Or, conversely, he may shift time from paid to unpaid work (earning less), while she responds to this by increasing time spent on leisure. An alternative mechanism for a negative effect on his earnings is signaling; that is, that fathers who are induced by the reform to take longer leave faces subsequent discrimination (“wage penalties”). In this sense, the absence of a negative effect on fathers’ earnings indicates that the reform has not introduced or strengthened a fatherhood wage penalty.

Our third and final research question regards the effect of parental leave uptake on union stability. Previous studies indicate a positive relationship between gender equality and union stability, pointing towards that a prolonged paternity quota would increase union stability. The mechanism for such a hypothesized effect is that his increased efforts at home could free up time for her paid work or leisure, increasing her relationship satisfaction. Still, there is little causal evidence in support of this mechanism, and the observed correlation may very well be confounded by differences in relationship satisfaction or family orientation. Our contribution lies in putting this well-founded hypothesis to a much sought for causal test. Contingent on a positive reform effect on her earnings, the standard microeconomic model yields a contrasting expectation: reduced specialization should reduce the gain from marriage, and hence increase dissolution risk (cf. Becker 1991).

3 Reform details

The Norwegian parental leave system ensures income replacement and job security so employed parents can care for their new child. With an explicit goal of strengthening the relationship between father and child, as well as to improve the gender equality in the division of paid and domestic work between the parents (Norwegian Ministry for Children and the Family (1992) p. 30), the Norwegian government introduced a father’s quota on April 1 1993. This policy reserved four weeks of leave exclusively³ for the father, and divided the parental leave into a mother’s quota, a father’s quota and a shared period which could be divided freely between the

parents. The duration of all three parts of the parental leave has gone through changes since 1993, and the developments are summarized in Table 1. Throughout these changes, parents can choose between 80 or 100 percent income replacement for a correspondingly longer or shorter leave. The social security system replaces earnings up to a cap of 6G⁴, but several employers, including the Norwegian public sector, top up parental leave compensation for incomes above this cap.

As we can see from the table, the father's quota was expanded from the original four weeks to five weeks in 2005 and then to six weeks in 2006, with a corresponding one-week expansion in the total leave period in both these years. In 2009, however, the father's quota was expanded by four weeks, wherein only two weeks were added to the total leave period and the remaining two were shifted from the shared leave. This rather substantial policy change prompted a significant political debate, and was criticized for "taking" leave from the mother and "giving" it to the father – an argument that reflects the strong tendency for mothers to take all or most of the shared leave (Dahl et al., 2014; Fougner, 2012).

All fathers whose child is born on or after the policy implementation date were eligible for the father's quota, as long as both parents had accumulated individual rights to paid parental leave. The eligibility criteria for paid parental leave have changed slightly over the period captured in the table, but for our sample (i.e. those who had a child close to July 1, 2009), eligibility depended on both parents having pensionable income for at least six out of the ten months before the child was born. Moreover, it was a requirement that the mother's eligibility was based on part time (50 percent) employment or higher (Norwegian Ministry for Children and the Family (2009): p.3).

There are requirements to the mother's labor market activity when the father uses the shared weeks in the paid parental leave scheme, however, this is not the case when the father uses the father's quota (NAV, 2016). The mother could therefore – if desirable – stay at home together with the father on either paid holidays, unpaid leave or graded leave (Norwegian Ministry for Children and the Family (2009): p.3). However, Norwegian holiday legislation (entitling all employees in full time position to five weeks of paid holidays a year), combined with the rules on flexible leave uptake (NAV, 2015a) and the now 10 weeks father's quota, implied that it was impossible for the mother to stay at home during the entire father's quota without the family

experiencing a substantial drop in disposable income. It is therefore likely that the 2009 reform increased not only the number of leave days taken by fathers, but also the number of days that fathers spent alone with their child. The 2009 extension of the quota from four to ten weeks may hence have caused a more profound change to the amount of time fathers spend at home caring for their young child than the 1993 implementation. Furthermore, the extension of the reform was implemented, 3 in 4 fathers were already taking some parental leave (Fougner, 2012). This “normalization” of paternity leave could potentially facilitate effects of further extensions. In comparison, the introduction of the Norwegian father’s quota increased take-up from 3 to 25 per cent, meaning that leave-taking remained a minority behavior among fathers also (immediately) after the introduction (Cools et al., 2015).

4 Methods and data

4.1 Identification strategy

The expansion of the father’s quota was implemented July 1 2009, and our empirical strategy takes advantage of this clear cutoff in eligibility and the fact that families with children born just before and just after the cut-off should be very similar. We use the increase in the father’s quota and the reduction in the shared leave (usually taken by the mother) as a discontinuous function of the birth date of the child, and estimate sharp Regression Discontinuity (RD) estimates to capture reform effects using the birth date of the child as our assignment variable. Sharp RD takes the following basic form (Angrist and Pischke, 2014):

$$Y_i = \alpha + \rho D_i + \gamma Z_i + \varepsilon_i$$

Where α is a constant term, γZ_i nets out general trends in the assignment variable, D_i is a dummy variable for treatment, and ρ gives the reform effect on the outcome. The equation is estimated on both uptake and outcome variables using the Stata command `rdrobust` (Calonico et al., 2016), which specifies a local polynomial regression for the running variable. The choice of bandwidth in local polynomial regression implies a trade-off between less bias (narrower bandwidth) and higher precision (wider bandwidth). Both binwidth and functional form is estimated independently before and after the cutoff. This flexible specification minimizes the likelihood that any bias from the running variable is retained. Our identifying assumption is

that the specification of the running variable (γZ_i) nets out all variation correlated with the outcome and the running variable that is not due to the reform.

Selection around the cut-off may compromise identification (Tamm, 2013; Cools et al., 2015). Such self-selection into (or out of) eligibility could happen for two main reasons; by parents timing the conception of a child in anticipation of the reform, and by expectant parents with due dates close to July 1 postponing/speeding up induced births or planned caesarian sections. Families where the father is more involved in family matters will presumably time the birth to after the introduction, whereas families where the father is less involved might want to time the birth to before the introduction. These different types of families may differ in factors relevant for specialization and union stability too. Hence, if such strategic timing exists, comparing families with children born just before and just after the cut-off will yield biased results.

The intention to expand the father’s quota to ten weeks was declared by the government already in 2005 (Soria Moria 2005, p 43), but the policy and its details (including date of implementation) was not proposed in the Council of State until April 3 2009 (Stortinget, 2015). This would leave less than nine months until the implementation, suggesting that strategic timing of conceptions should not be of major concern.⁵

Cools et al. (2015) find strong evidence of strategic timing of births two weeks before and after the 1993 introduction of the fathers quota (c.f. Brenn and Ytterstad (1997)). Using placebo tests (testing for “effects” on earnings in the year prior to the reform) we do also find some evidence of strategic timing, with high-income couples shifting into the treatment group. When we exclude parents of children born the 13 days before and the 13 days after the reform, no such evidence remains. Hence, we keep this restriction in our main results.⁶

4.2 Data

Study sample

We base all analyses on data from Norwegian population registers covering the time period between 2007 and 2016. Our main study sample is women who gave birth to a child in May, June, July or August 2009, and where the father and mother lived together as of 1.1. 2008 (before pregnancy). In Section 5.4, we test if restricting the sample to focal children born in June and

July yields similar results. From this main sample we make four further restrictions. First, as we take interest in gender specialization, same-sex couples are excluded. Second, as leave spells are registered to parents (and not children), we exclude couples who had another child 15 months before or after the focal child was born (see Appendix II for details on construction of the study sample). Third, as an exogenous proxy for parental leave rights, we include only focal children whose mothers had earnings the year prior to the reform. Finally, as multiple births give rise to correlated observations, only one focal child per birth (and parental leave spell) is included in the sample. The final study sample consists of 9 516 focal children, each with a registered mother and father. For this sample, we record leave taking and subsequent patterns of income for both the mother and the father for up to four years after the focal child was born (2014). Union status is recorded until January 1 the year the focal children turn seven (2016). This information is merged with other socio-demographic characteristics of the parents.

We also construct a separate sample for placebo analysis. Here, we retain couples based on the exact same criteria, shifted one year: The focal children are born in May, June, July or August 2008, parents must have co-resided as of 1.1.2007, and the mother must be registered by earnings in 2007. Same-sex couples and children born in the 26 days around the placebo cutoff (July 1st 2008) are excluded. The sample for placebo analysis consists of 9 110 couples.

4.2.1 Outcome variables

Measures of parental leave uptake As a first step, we establish whether our reform indeed has an effect on parental leave uptake among mothers and fathers. The main outcome of interest here is the number of paid leave days taken by the mother and father respectively.⁷ We also estimate effects on the number and average length of leave spells taken by the father and mother respectively, and each parent’s propensity to take part time leave.⁸ Together, these characteristics give an impression of whether the extended father’s quota induced longer uninterrupted paternity leave spells. The effect on parents’ leave uptake, if any, constitutes the mechanism or first stage through which effects on other outcomes are mediated. Descriptive statistics for all outcomes are shown in Table A.1. Details on the construction of parental leave data are given in Appendix II.

Earnings Our starting point of the analyses of changes in market work is the sum of earned income and primary and secondary business income (“yrkesinntekt”) (Steinkellner, 2003), an even better proxy of efforts in paid work than earned income alone. For brevity, we refer to this variable as earnings. Missing and zero earnings are set to 1, facilitating calculation of log earnings. We estimate effects both on the extensive margin (as captured by a dummy variable taking one if earnings exceed 1G, otherwise zero – see Footnote 4) and the intensive margin (log earnings), for both mothers and fathers. Earnings are measured from 2011 (the first year after the paid parental leave period ended, when focal children turn two years) to 2014 (when focal children turn five years).

Specialization in market work In addition to estimating effects on mothers’ and fathers’ earnings separately, we construct a measure for specialization in market work by dividing her earnings divided by the sum of her and his earnings.⁹ An increase in this outcome means a shift towards a less traditional division of labor in the couple. As for earnings we measure specialization from 2011 to 2014.

Union stability Our final outcome of interest is union stability, measured yearly January 1st from 2010 (before focal children turn one) to 2016 (before focal children turn seven). For each year, we construct a dummy variable taking the value one if the parental couple are still registered as living together, otherwise zero (see Appendix Table A.1 for descriptives). Unions can be dissolved by separation (for married couples) or registration of separate addresses (for cohabitators). This register measure ensures zero attrition, crucial for the validity of our results. The death of one partner is a rare case of union dissolution among couples with young children, and unlikely to be influenced by parental leave uptake, and we hence consider it unlikely to bias our results.

4.2.2 Control variables and subsample stratification

While a valid regression discontinuity design does not require inclusion of covariates beyond the running variable, covariates can both sharpen the precision of the estimates and provide a robustness check. Most importantly, we use information on observable characteristics measured

prior to the reform (in 2008) to conduct subgroup analysis. Based on register information of marriages, we construct an indicator taking the value one if the parental union is a marriage, otherwise zero. We also construct a set of dummies for parity of the focal child, distinguishing between first borns, second borns, and later borns (merged to retain subsamples of meaningful size). We obtain information on educational attainment and enrollment from the National Educational Database (NUDB). When used as a control variable, educational attainment is grouped into four levels: Basic (not completed high school), completed high school, higher education lower degree (BA), and higher education higher degree (MA or PhD). To retain test strength, we collapse these categories into lower (basic and high school) and higher (higher and lower degree) for the subsample analysis. Missing information on education is coded as a separate fifth category. Individuals are defined as students if they have been enrolled in education for at least one month during the current year. We also conduct subsample analysis for younger couples (both under age 30 the year the focal child is born) and older couples (at least one parent aged 30 or above the year the focal child is born). When included as covariates, mother’s and father’s age are each included with a linear and curvilinear term.

Table 2, Panel A, shows the distribution of background characteristics by treatment status. Reassuringly, the means are very similar, and never statistically different (95 per cent confidence intervals are overlapping). This corroborates the plausibility of our identification strategy.

5 Results

5.1 Reform effects on leave uptake

Our first research question regards the effect of the reform on the leave uptake of fathers and mothers. The reform incentivizes longer paid leave for fathers, and shorter paid leave for mothers. Effects on leave uptake are shown in Table 3, panels A and B. No controls indicate the basic model with no covariates (beyond the running variable), whereas full controls imply estimates from a model where all covariates are included. For fathers (Panel A), the estimates show a rather substantial increase of about 14 leave days, both statistically significant and robust to inclusion of covariates. Keeping in mind that the reform increase the number of days reserved for the father from 30 to 50 days, and that fathers pre-reform on average took 33 paid leave

days (Table 2) this is a strong yet plausible increase. A visual RD (Figure 1, left panel) confirms a clear jump in men’s leave days at the cutoff. Furthermore, the percent of fathers who takes 10 weeks of paid leave or more, increases with 50 percentage points (Table 3 and Figure 1, right panel), a massive increase from the pre-reform baseline of 12 percent (Table 2). The mean duration of each of the father’s parental leave spells is increased by 12 days, but there is no significant change in the number of spells taken.¹⁰ Neither fathers’ propensity to take leave, nor fathers’ propensity to take part time leave, are significantly affected.

The point estimates for mothers (Table 3, Panel B) show that the reform induced an average reduction in leave length of about 21 days, i.e. by about two weeks more than was incentivized by the reform. A visual RD for mother’s number of leave days (Figure 2) confirms a clear drop at the discontinuity. There is an equally large drop in the average duration of the leave spells for mothers, suggesting that mothers still tend to use all of their leave in one continuous break from the labor market. Unsurprisingly, neither the mother’s propensity to take leave or the average number of parental leave spells are affected.

As fathers earns more than mothers in 3 of 4 couples (Appendix Table A.1), 80 per cent compensation implies a larger income loss (in absolute terms) for a large majority of couples when he takes a larger share of the leave. As such, the reform strengthens the incentive to choose 100 percent income compensation, and couples respond to this incentive by decreasing their propensity of taking 80 percent compensation (Table 3, Panel A). In other words, treated couples on average take fewer (yet better compensated) leave days. This shift to shorter total leave length might explain why mothers’ number of leave days is reduced by more than the two weeks that were shifted to the father by the reform.

Subsample analysis (Appendix Table A.3) reveals a tendency of a stronger effect on his leave length if he or she have completed higher education, though the estimates are never statistically different by educational attainment (i.e. 95 per cent confidence intervals are overlapping). Differences in effects on his leave length by parity and union status are both substantially and statistically insignificant. For effects on her leave length, subsample estimates are imprecise, and never statistically different.

Taken together these findings show that the reform had a profound effect on the leave uptake of both mothers and fathers, confirming the findings from previous studies on the implementation

of fathers' quotas (Cools et al., 2015; Ekberg et al., 2013; Geisler and Kreyenfeld, 2012; Patnaik, 2016). This substantial shift in the distribution of leave between parents means that the policy change is well suited to identify causal effects of paternal involvement. As we observe changes in the leave uptake of both mothers and fathers, effects on other outcomes, if any, can be mediated by both fathers' increased time spent with a young child, and mothers' faster return to work after birth.

5.2 Effects of paternity leave on his and her market work

The profound change in leave uptake induced by the reform may have lasting effects on his and her earnings. Our second research question regards the effect of changes in leave uptake on his and her (relative) earnings, in the short and long run. His longer paternity leaves may reduce his earnings in the long run because he continues to pull more weight at home, and/or faces subsequent discrimination in the labor market. The combination of his longer and her shorter leave may strengthen her labor market outcomes. Our main outcome of interest is (relative) earnings when the focal child turns five (2014, Table 3). At this age, most Norwegian children are enrolled in a child care center, and a more permanent pattern of (absence of) specialization in the family is likely to have settled. We also assess yearly effects up to this age (Appendix Table A.2), starting in the year the focal child turns two.

We explore whether there are effects on his or her earnings at both the intensive (log earnings) and the extensive margin (the probability of being employed) the year the focal child turns five (Table 3, Panels D for fathers and E for mothers). Starting with the effects on log earnings, point estimates are negative for both fathers and mothers, but never statistically significant from zero. Discontinuity plots show no visual evidence for a discontinuity for log earnings (Appendix Figure A.1). For effects on his and her propensity to be employed, estimates are small, insignificant and close to zero. In the short run (the years the focal child turns two, three and four years) neither the propensity to be working nor log earnings are significantly affected for fathers (Appendix Table A.2, Panel B). For mothers (Panel C), we find no effects on the propensity to be working. A statistically significant negative effect on log earnings emerges for mothers the year the focal child turns two, but disappears when the focal child is three and four years. As one would expect some false positives with this number of statistical tests, we refrain

from substantive interpretations of this finding.

In sum, our results indicate that the reform did not induce lasting changes in labor supply on neither the extensive nor the intensive margin, and neither for mothers nor fathers. Consequently, specialization in market work remains unaffected by the reform (see Table 3 Panel C for the year the focal child turns 5, Appendix Table A.2, Panel A for the years before). The point estimates of specialization are small, and never differ significantly from zero. In other words, the tendency of mothers to provide about 40 percent of the household earnings remains unchanged throughout the period of observation. The results are robust to inclusion of exogenous controls, and a visual RD (Figure 3, left panel) confirms that trends in the running variable is well captured in our specification. Our results hence provide convincing evidence that the extension of the fathers' quota did not change the mother's share of couple earnings.

We have also split the sample by union type, parents' age, mother's education, father's education and the sex and parity of the focal child, in order to explore whether these overall findings hide heterogeneous policy adaptations in different families (Table 4). There is a small tendency for the reform to reduce specialization in families where the mother has higher education (significant after controls only) and if the mother had at least two children before the focal child; however, these effects are only significant at the 10 percent level. The number of significant estimates are about what one should expect from this number of tests at the ten percent level, and we hence refrain from substantive interpretations.

5.3 Effects of paternity leave on union stability

Our third research question regards the effect of changes in parental leave uptake on the union stability of parental couples. As (relative) earnings was not moved by the reform, effects (if any) must run through mechanisms other than changed division of market work. For union stability, our outcome of interest is whether the parental union is intact in a given year, with effects estimated from the year the focal child turns one (2010) to the year the focal child turns seven (2016). Positive estimates indicate a stabilizing effect. By construction of the sample, all unions are intact as of January 1 2008. Table A.1 shows that while 99% of the parental unions remain intact after two years (2010), the proportion gradually decreases to 87% before the focal child turns seven (2016).

Sharp RD estimates of the reform effect on the probability of the parental union to be intact the year the focal child turns seven are found in Panel C of Table 3. The estimates are negative, but small and not statistically different from zero on the 95 per cent level. This is confirmed by the lack of a visible change in union stability around the cutoff (Figure 3, right panel). We also test if the reform affects union stability in the short run, i.e. the years when the focal child is turns two to six years old (Appendix Table A.2). Again, point estimates are small and statistically insignificant. Hence, overall, the increase in father involvement that was induced by the extension of the paternity quota, had no stabilizing effect on unions when it was implemented.

As before it is possible that the absence of effects on the average may hide subgroup effects. Hence, we again split our sample by pre-reform characteristics and estimate effects on union stability seven years after the reform (2016) (Table 4). There is a tendency of a destabilizing effect for cohabiting couples and couples where the mother is without higher education, but both estimates lack precision and are significant at the ten percent level only. Differences between groups are far from statistically significant (i.e. 95 per cent confidence intervals overlap). In sum, our results give no support to the hypothesis that an extended paternity quota stabilizes parental unions – be it marital or cohabiting.

5.4 Robustness checks

In addition to inclusion of exogenous covariates, we have conducted three robustness checks. Our most important robustness test is the implementation of a “placebo reform” July 1st 2008. The construction of the placebo reform sample mirror the construction of the main sample exactly, with all criteria and measurements shifted one year back, including the outcome variables (see Section 4.2). Descriptive statistics for the placebo test sample is shown in Appendix Table A.4, and results are shown in Appendix Table A.5. The placebo reform effects are small, zero to the second decimal or negative, and never statistically significant at any conventional level. A discontinuity plot (Appendix Figure A.2, left panel) confirms the impression of no discontinuity in union stability measured before the child is seven years old at the placebo cutoff. This corroborates the validity of our results: There is no evidence of discontinuous differences in leave uptake, earnings, specialization or union stability by the child’s birth date in the absence

of a change in parental leave rights.

Second, we have estimated effects on pre-reform outcomes, that is, the effect of the reform on three market work outcomes (his and her log earnings, her share of couple earnings), and the couple’s propensity to be married one year prior to the reform (2008). Effects of these outcomes would indicate that the treatment and control groups are different in ways not captured by the running variable, meaning that our results do not have a causal interpretation. Results are shown Table 5 (see also Appendix Figure A.2, right panel). Reassuringly, we find no significant “reform effects”, neither for marriage or earnings outcomes measured prior to the reform.

Finally, to further ascertain that trends in the running variable does not bias our results, we restrict the study sample further to families of focal children born in June and July 2009, still excluding potential timers around the cutoff ($N=3\ 358$). Results are shown in Appendix Table A.6. The point estimates for effects on leave uptake are very similar those found in the main sample, but as expected, the lower sample size reduces precision and with wider confidence intervals estimates are no longer significant at the 5 per cent level. As expected we see no effects on neither earnings, specialization or union stability.

6 Concluding discussion

Increased father involvement has been suggested as a pathway to increasing mothers’ labor supply and stabilizing parental unions. Still, analyses of policies that most profoundly affect father involvement – the introduction of paternity quotas in various Western countries – do not show consistent evidence of an equalizing effect on earnings, and no effects are found on marital stability. In this paper, we suggest that while a short quota may be insufficient to change gendered patterns in care, extensions of existent quotas into longer durations may have potential to change dynamics of work and care in families. Furthermore, extensions of existent paternity quotas tend to happen in a context where male parental leave taking is normalized, and may as such have a larger potential of having an immediate effect on the behavior and practices on a large share of parents of newborns.

Our study utilizes an extension of the Norwegian paternity quota from 6 to 10 weeks, implemented on 1. June 2009, as a source of exogenous variation in the length of paternity leave. We

study reform effects on leave uptake, earnings and union stability in a Regression Discontinuity design, restricting our study sample to children born in the weeks around the implementation of the reform. The reform generated substantial and significant effects on leave uptake, inducing fathers who would have taken some leave regardless of the policy change to extend their leave with about 14 work days on average. The percent of fathers who takes at least 10 weeks of leave (the extended quota) increased with 50 percentage points from a baseline of 12 per cent. Mothers reduced their leave by about one month (21 work days) on average. In terms of changing the division of labor in a child's first year of life, and providing an opportunity to strengthen the bond between father and child, the reform was a success. A battery of robustness checks supports that the variation in father involvement is truly exogenous, i.e. not driven by self-selection or strategic timing.

If the changes in care patterns are lasting, one could expect that they would translate into effect on his and her earnings in the near or medium-term future. Being exposed to a longer paternity quota may entail a learning effect, making fathers efficient in and aware of household chores, and leading them to take on more unpaid work also when the parental leave has come to an end. If this is the case, one could legitimately expect that the treated fathers display a weaker earnings development than the control fathers, while treated mothers would have better earnings prospects than their control counterparts. We estimate effects on a range of earnings measures (his and her log earnings and probability of employment, her share of couple earnings), for each year up til the year the focal child turn five years. Contrary to expectations, we find no consistent evidence of significant effects.

The absence of effects is consistent with a similar study of the effect of the introduction of the Norwegian paternity quota (Cools et al., 2015). Hence, neither the introduction nor extensions of paternity quota in Norway seem to have immediate negative effects on his earnings development. Our results strongly indicate that the 2009 parental leave reform neither hampered the labor market position of the treated fathers, nor improved the position of the treated mothers. Hence, at this margin, there is no indication that the paternity quota had non-linear effects, starting to affect the division of paid work above a threshold length.

For potential effects on union dissolution, we hypothesized that treated fathers may permanently increase their efforts in unpaid work, resulting in higher relationship satisfaction among

treated mothers. This expectation is based in the empirical observations that mothers on average do more unpaid work than fathers, and that this is linked to relationship dissatisfaction. Knowing that earnings were not delectably moved by the reform, effects on union dissolution could still be mediated by treated fathers shifting time from (pure) leisure to house- and care work. We estimate effects on the probability of the union being intact up til the year the focal child turns seven years. Again, we find no evidence of statistically significant effects. The expectations that increased father involvement in child care, as induced by an extended father's quota, would stabilize parental unions are hence not supported. While union dissolution is not moved in a way we can measure, the reform may have caused incremental changes in relationship quality, not discernable with our data. To the best of our knowledge, no Norwegian data source combines the statistical strength required for an RD with self-reported data on relevant aspects of life in families – such as division of unpaid labor and relationship quality. A survey of such aspects, strategically administered to couples who had a child around the reform cutoff, would be invaluable to reform evaluation.

A general limitation of quasi-experimental design is the inability to capture dynamic effects – that is, reform-induced gradual societal changes that affect both the control and treatment group. In a Regression Discontinuity design such as ours, one is further able only to capture effects that are applicable to the first groups of parents treated by the reform. Regarding paternity quotas, one could speculate that such reforms induce gradual changes in fathering practices that affect both the treatment and control group, and/or that these gradual changes means that the reform effects are stronger for those treated later than for those treated first. Our findings provide some contrast to those of Rege and Solli (2013), who use a difference-in-difference design, and find that that introduction the paternity quota had a negative effect on the earnings of fathers – though only after a phase-in period. They reconcile their findings with the zero effect found by Cools et al. (2015) of the same reform in a more conservative design, arguing that reform effects are more profound when take up is already high, and/or a large proportion of fathers are moved by the reform. Our research design complies with both these criteria. As such, our reform context is favorable to identification of immediate effects, and our results provide credible, yet conservative, estimates of the reform effect.

Numerous scholars have suggested that increased father involvement stabilize parental unions

(Cooke, 2006; Esping-Andersen and Billari, 2015; Goldscheider et al., 2015; Sigle-Rushton, 2010). The idea is intuitively appealing: it reconciles ideals of gender equality with ideals of stability of parous unions, indicating that *more*, not less, gender equality is the receipt of for more children being raised in intact families. While far from a perfect test, our results cast some doubt on the claim that father involvement – at least as induced by changes in parental leave policies – stabilizes unions. Of course, several aspects of father involvement are unmoved by this reform, and it is possible that gradual changes in fathering practices remain causally related to union stability. Still, as long as evidence for this hypothesis remains limited to estimates with potentially strong selection bias, one might also ponder if “more gender equality” simply is not as effective a pathway to more stable parental unions as hypothesized.

On a more positive note, our results are reassuring for policy makers who ponder extensions of paternity quota, but are concerned that this may introduce or intensify fatherhood penalties. We find no evidence fathers moved by the reform to extend their parental leave experienced such penalties. Of course, some fathers may have anticipated earnings penalties, and taken shorter or no leave despite the reform (never-takers). Still, at least in the relatively family friendly Norwegian environment, fathers who make use of extended paternity quotas to bond with their young children are neither putting their career prospects nor the family income at risk.

Notes

¹The Nordic parental leave system offers parents a generous wage-compensation for staying home with a newborn child for around one year, and while the bulk of parental leave can be shared freely between the parents, it is in practice taken up mainly by the mother (Duvander and Lammi-Taskula, 2011; Lappegård, 2008).

²Because both mothers and fathers can shift time between paid/unpaid work and (pure) leisure, his increased effort at home may, but need not, be reflected in his lower earnings and/or her higher earnings.

³The father’s quota could not be transferred to the mother unless she was a single parent, the father was ineligible to paid parental leave, or the father was too sick or otherwise unable to care for the child.

⁴The base rate (G) of the Norwegian Social Insurance scheme is an annually adjusted amount used to define benefit eligibility and calculate pensions. As of 1. July 2009, the BA was 72 881 NOK, or 11 602 USD (calculated based on the exchange rate for 2009, https://www.norges-bank.no/en/Statistics/exchange_rates/currency/USD).

⁵It should be noted the public debate regarding the reform picked up in Norwegian newspapers as early as October 2008 (i.e. nine months prior to the implementation), but that it remains unlikely that future parents were able to guess the implementation date, as previous family policy reforms had been implemented on both April 1, May 1 and July 1.

⁶To avoid that the local polynomial regression adapts to the missing data around the cutoff, we add 13 to the running variable for all births before the cutoff, and subtract 13 to all births after the cutoff.

⁷Some parents are registered with a higher number of leave days than the parental leave system allows, potentially due to the erroneous registration of, e.g., sick leave days etc. during the paid parental leave period. Hence, we cap the leave duration at the maximum number of leave days available. The results are not sensitive to this.

⁸Tidskonto (“time account”) allows parents to take leave days part time, for instance may the mother stay at home with the child certain days of the week, and the father stay at home the remaining days, see <https://www.nav.no/fleksibeltuttak>.

⁹Couples without earnings have equal earnings, and are assigned a value of 0.5.

¹⁰Fougner (2012), on the contrary, finds that more fathers split up their leave spells in the period after the reform. The differences may emerge after 14 months after birth (at which time point our, but not Fougner’s, parental leave data are censored). Fougner applies a descriptive rather than quasi experimental design, and the differences could also be an artifact of time trends.

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Tables and Figures

Table 1: Development in the paid parental leave scheme, with 80/100 percent income coverage. Number of weeks.

Effective date	Reserved mother	Reserved father	To be shared	Total number of weeks
April 1 1992	9	-	24/33	33/42
April 1 1993	9	4	29/39	42/52
July 1 2005	9	5	29/39	43/53
July 1 2006	9	6	29/39	44/54
July 1 2009	9	10	27/37	46/56

Note: Of the weeks reserved for the mother, three weeks are to be used prior to giving birth, and an additional six immediately after. The father cannot take any of his leave days during this period. However, fathers may take 2 weeks of unpaid care leave during the first two weeks of the child's life. Several employers, including the Norwegian public sector, will allow the father to take paid leave these two weeks. This is unrelated to the father's quota.

Table 2: Mean differences by treatment status. Main sample. Balancing tests on pre-reform characteristics (Panel A) and description of parental leave uptake (Panel B).

	TREATMENT		CONTROL	
	MEAN	95% C.I.	MEAN	95% C.I.
PANEL A: BALANCING TESTS				
JOINT CHARACTERISTICS				
Parents married in 2008	0,48	[0,46;0,49]	0,47	[0,46;0,48]
FATHER CHARACTERISTICS				
Age	34,35	[34,20;34,51]	34,23	[34,08;34,38]
Educational attainment				
Basic	0,17	[0,16;0,18]	0,15	[0,14;0,16]
High school	0,39	[0,38;0,41]	0,41	[0,39;0,42]
Higher education, lower degree	0,28	[0,27;0,29]	0,27	[0,26;0,28]
Higher education, higher degree	0,14	[0,13;0,15]	0,16	[0,15;0,17]
Missing education info.	0,02	[0,01;0,02]	0,02	[0,01;0,02]
Enrolled in education	0,07	[0,06;0,08]	0,06	[0,06;0,07]
Earnings	472392,17	[464133,76;480650,58]	489399,85	[478974,52;499825,19]
MOTHER CHARACTERISTICS				
Age	31,52	[31,39;31,65]	31,55	[31,43;31,68]
Number of children	2,05	[2,02;2,08]	2,05	[2,02;2,07]
Educational attainment				
Basic	0,12	[0,11;0,13]	0,11	[0,10;0,12]
High school	0,27	[0,26;0,28]	0,28	[0,27;0,30]
Higher education, lower degree	0,44	[0,43;0,46]	0,44	[0,43;0,46]
Higher education, higher degree	0,15	[0,14;0,16]	0,14	[0,13;0,15]
Missing education info.	0,02	[0,02;0,03]	0,02	[0,02;0,03]
Enrolled in education	0,10	[0,10;0,11]	0,09	[0,08;0,10]
Earnings	311570,39	[306895,13;316245,66]	313522,27	[309172,19;317872,34]
PANEL B: PARENTAL LEAVE				
Father's leave days	47,05	[45,91;48,18]	33,09	[32,20;33,99]
Father takes comp. leave	0,78	[0,77;0,80]	0,77	[0,76;0,78]
Father takes >= 50 days leave	0,63	[0,62;0,64]	0,12	[0,11;0,13]
Father 80% compensation	0,45	[0,43;0,46]	0,55	[0,53;0,56]
Mother's leave days	201,93	[199,38;204,49]	217,58	[215,04;220,11]
Mother takes comp. leave	0,88	[0,87;0,89]	0,89	[0,88;0,90]
N	4617		4899	

Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Table 3: Reform effects on leave uptake and outcomes. Main sample.

	Est	SE		Est	SE	
A: LEAVE UPTAKE FATHERS						
Controls	No			Yes		
Number of days	14,27	(3,55)	***	14,51	(3,68)	***
Use time account	0,00	(0,03)		0,00	(0,03)	
Takes leave	-0,02	(0,05)		-0,01	(0,04)	
Takes \geq 50 days leave	0,49	(0,05)	***	0,50	(0,05)	***
Mean duration of spell	11,46	(3,29)	***	11,64	(3,40)	**
Number of spells	-0,09	(0,18)		-0,08	(0,17)	
80% compensation	-0,16	(0,06)	**	-0,15	(0,06)	**
B: LEAVE UPTAKE MOTHERS						
Number of days	-21,50	(8,61)	**	-20,82	(8,35)	**
Use time account	-0,02	(0,01)	‡	-0,02	(0,01)	‡
Takes leave	-0,02	(0,03)		-0,02	(0,03)	
Mean duration of spell	-23,38	(9,39)	**	-22,85	(9,22)	**
Number of spells	-0,03	(0,04)		-0,03	(0,04)	
C: JOINT OUTCOMES						
Mothers' share ch. 5 y	0,02	(0,02)		0,03	(0,02)	
Intact union ch. 7 y	-0,03	(0,04)		-0,03	(0,04)	
D: OUTCOMES FOR FATHERS						
Working ch. 5 y	-0,03	(0,03)		-0,02	(0,02)	
Ln(earn.) ch. 5 y	-0,30	(0,30)		-0,26	(0,27)	
E: OUTCOMES FOR MOTHERS						
Working ch. 5 y	-0,01	(0,03)		0,00	(0,03)	
Ln(earn.) ch. 5 y	-0,10	(0,25)		-0,12	(0,29)	

Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, ‡ $p < 0.1$.

Table 4: Reform effects on relative earnings (upper panel) and union stability (lower panel) in 2016. Subsample analysis.

	NO CONTROLS		FULL CONTROLS		STARS	N
	EST.	(S.E.)	EST.	(S.E.)		
A: HER EARNINGS SHARE						
BY MOTHER'S EDUCATION						
No higher education	0,01	(0,04)	0,02	(0,04)		3635
Higher education	0,03	(0,02)	0,04	(0,02)	§	5674
BY FATHER'S EDUCATION						
No higher education	0,02	(0,03)	0,03	(0,03)		4871
Higher education	0,02	(0,03)	0,02	(0,03)		4483
BY UNION TYPE IN 2008						
Cohabiting	0,01	(0,02)	0,02	(0,02)		5014
Married	0,03	(0,04)	0,03	(0,04)		4502
BY PARITY OF FOCAL CHILD						
First born	-0,01	(0,03)	-0,01	(0,04)		2603
Second born	0,01	(0,03)	0,01	(0,03)		4499
Higher order	0,10	(0,05)	* 0,07	(0,05)	‡	2414
BY CHILD SEX						
Girl	0,04	(0,03)	0,03	(0,03)		4628
Boy	0,01	(0,03)	0,02	(0,03)		4888
BY PARENT'S AGE						
Young parents	0,03	(0,02)	0,03	(0,02)		8036
Not young parents	-0,01	(0,05)	0,01	(0,05)		1480
B: UNION STABILITY						
BY MOTHER'S EDUCATION						
No higher education	-0,10	(0,07)	‡ -0,10	(0,07)	‡	3635
Higher education	0,00	(0,03)	0,00	(0,03)		5674
BY FATHER'S EDUCATION						
No higher education	-0,06	(0,06)	-0,08	(0,06)		4871
Higher education	-0,01	(0,04)	0,00	(0,04)		4483
BY UNION TYPE IN 2008						
Cohabiting	-0,06	(0,05)	-0,08	(0,05)	‡	5014
Married	0,00	(0,04)	0,02	(0,04)		4502
BY PARITY OF FOCAL CHILD						
First born	-0,07	(0,07)	-0,08	(0,06)		2603
Second born	-0,02	(0,05)	-0,01	(0,05)		4499
Higher order	-0,04	(0,06)	-0,03	(0,05)		2414
BY CHILD SEX						
Girl	0,00	(0,05)	-0,01	(0,05)		4628
Boy	-0,07	(0,05)	‡ -0,04	(0,04)		4888
BY PARENT'S AGE						
Young parents	-0,03	(0,04)	-0,02	(0,04)		8036
Not young parents	-0,07	(0,09)	-0,07	(0,08)		1480

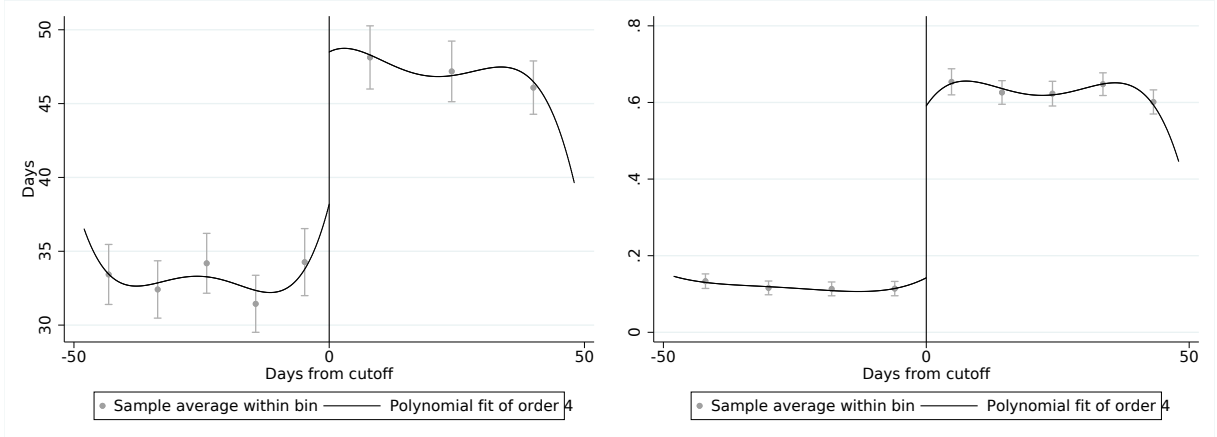
Note: N=9 516. For splits by union type and parity, subsamples sum to N=9 516. Splits by education sum to a lower N due to exclusion of individuals with missing educational attainment. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child. *** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.1.

Table 5: Robustness check: Reform effects on pre-reform outcomes. Main sample.

	NO CONTROLS		FULL CONTROLS	
	Est.	(SE)	Est.	(SE)
Her share earnings 2008	-0,01	(0,02)	0,00	(0,02)
Ln(earnings) father 2008	-16062,52	(18027,72)	-7588,39	(12879,74)
Ln(earnings) mother 2008	-38985,45	(25930,93)	-44922,53	(24331,99)
Married in 2008	-0,03	(0,05)	-0,02	(0,05)

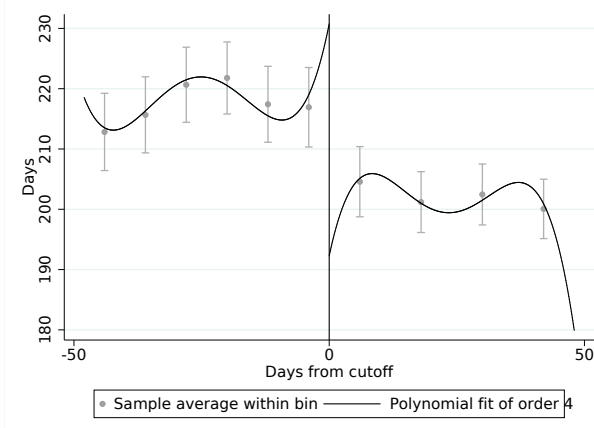
Note: N=9 516 for all models. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child. *** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.1.

Figure 1: Reform effect on father’s leave uptake in days (left panel) and the propensity to take at least 50 days of paid paternity leave (right panel). Estimates from Regression Discontinuity models. Bars show 95 per cent C.I. for bin-specific means.



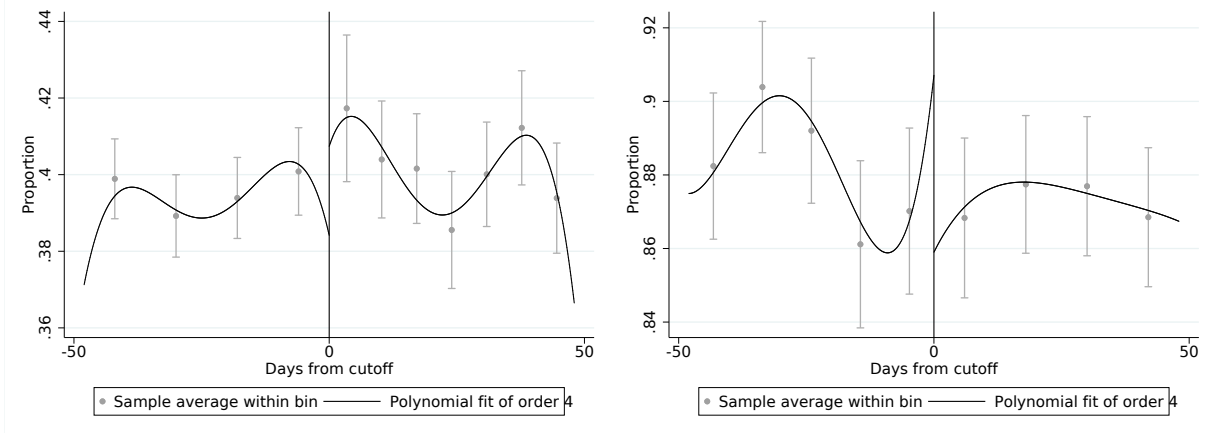
Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Figure 2: Reform effect on mother’s leave uptake in days. Estimates from Regression Discontinuity models. Bars show 95 per cent C.I. for bin-specific means.



Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Figure 3: Reform effect on probability of mothers’ share of earnings the year the focal child turns 5 (left panel) and the probability of the parental union being intact the year the focal child turns 7 (right panel). Estimates from Regression Discontinuity models. Bars show 95 per cent C.I. for bin-specific means.



Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Appendix I: Additional tables and figures

Table A.1: Descriptive statistics of outcome variables. Main sample.

	MEAN	SD	P25	Median	P75	MIN	MAX
JOINT CHARACTERISTICS							
UNION INTACT WHEN							
Child 1 y	0,99	0,09	1,00	1,00	1,00	0,00	1,00
Child 2 y	0,98	0,14	1,00	1,00	1,00	0,00	1,00
Child 3 y	0,96	0,20	1,00	1,00	1,00	0,00	1,00
Child 4 y	0,94	0,24	1,00	1,00	1,00	0,00	1,00
Child 5 y	0,92	0,27	1,00	1,00	1,00	0,00	1,00
Child 6 y	0,90	0,30	1,00	1,00	1,00	0,00	1,00
Child 7 y	0,88	0,33	1,00	1,00	1,00	0,00	1,00
MOTHER'S SHARE EARNINGS							
Before conc. (2008)	0,41	0,17	0,32	0,41	0,49	0,00	1,00
Child 2 y	0,39	0,19	0,29	0,40	0,48	0,00	1,00
Child 3 y	0,39	0,19	0,30	0,40	0,48	0,00	1,00
Child 4 y	0,39	0,19	0,30	0,40	0,49	0,00	1,00
Child 5 y	0,40	0,19	0,30	0,41	0,49	0,00	1,00
FATHER CHARACTERISTICS							
Leavedays	40,03	37,79	18,00	36,00	55,20	0,00	528,00
Leavedays capped	39,86	36,37	18,00	36,00	55,20	0,00	280,00
Takes compensated leave	0,78	0,42	1,00	1,00	1,00	0,00	1,00
Leave \geq 50 days	0,37	0,48	0,00	0,00	1,00	0,00	1,00
Number of spells	1,10	1,51	1,00	1,00	1,00	0,00	44,00
Mean duration of spells	34,40	34,45	10,00	30,42	50,00	0,00	528,00
Uses time account	0,09	0,29	0,00	0,00	0,00	0,00	1,00
80% compensation	0,50	0,50	0,00	0,00	1,00	0,00	1,00
WORKING WHEN							
Child 2 y	0,96	0,20	1,00	1,00	1,00	0,00	1,00
Child 3 y	0,96	0,20	1,00	1,00	1,00	0,00	1,00
Child 4 y	0,96	0,21	1,00	1,00	1,00	0,00	1,00
Child 5 y	0,95	0,21	1,00	1,00	1,00	0,00	1,00
LOG EARNINGS WHEN							
Child 2 y	12,81	2,01	12,89	13,11	13,40	0,00	16,09
Child 3 y	12,84	2,10	12,95	13,17	13,45	0,00	16,01
Child 4 y	12,86	2,20	12,99	13,23	13,51	0,00	16,22
Child 5 y	12,87	2,33	13,03	13,27	13,56	0,00	16,85
MOTHER CHARACTERISTICS							
Leavedays	218,52	102,07	180,00	258,00	282,00	0,00	796,80
Leavedays capped	209,99	89,89	180,00	258,00	280,00	0,00	280,00
Takes compensated leave	0,89	0,32	1,00	1,00	1,00	0,00	1,00
Number of spells	0,94	0,42	1,00	1,00	1,00	0,00	4,00
Mean duration of spells	212,36	102,59	176,00	246,00	280,80	0,00	796,80
Uses time account	0,02	0,12	0,00	0,00	0,00	0,00	1,00
WORKING WHEN							
Child 2 y	0,90	0,29	1,00	1,00	1,00	0,00	1,00
Child 3 y	0,91	0,29	1,00	1,00	1,00	0,00	1,00
Child 4 y	0,91	0,28	1,00	1,00	1,00	0,00	1,00
Child 5 y	0,91	0,28	1,00	1,00	1,00	0,00	1,00
LOG EARNINGS WHEN							
Child 2 y	12,08	2,56	12,34	12,73	12,97	0,00	15,32
Child 3 y	12,19	2,46	12,45	12,80	13,03	0,00	14,64
Child 4 y	12,20	2,59	12,49	12,85	13,08	0,00	15,82
Child 5 y	12,22	2,69	12,56	12,90	13,13	0,00	15,20

Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Table A.2: Reform effects on leave uptake and outcomes for all years, by age of focal child. Main sample.

	Est	SE	Est	SE
A: JOINT OUTCOMES				
UNION STABILITY				
Focal child 2 y	-0,01	(0,01)	-0,01	(0,01)
Focal child 3 y	-0,01	(0,02)	-0,01	(0,02)
Focal child 4 y	0,00	(0,02)	0,01	(0,02)
Focal child 5 y	-0,02	(0,03)	-0,01	(0,03)
Focal child 6 y	-0,04	(0,03)	-0,03	(0,03)
HER SHARE EARNINGS				
Focal child 2 y	-0,01	(0,02)	-0,01	(0,02)
Focal child 3 y	-0,02	(0,02)	-0,01	(0,02)
Focal child 4 y	0,01	(0,02)	0,01	(0,02)
B: FATHERS				
WORKING				
Focal child 2 y	0,01	(0,02)	0,00	(0,02)
Focal child 3 y	0,00	(0,02)	0,00	(0,02)
Focal child 4 y	-0,02	(0,02)	-0,01	(0,02)
LOG EARNINGS				
Focal child 2 y	-0,05	(0,24)	-0,06	(0,21)
Focal child 3 y	-0,01	(0,22)	0,01	(0,20)
Focal child 4 y	-0,19	(0,28)	-0,14	(0,25)
C: MOTHERS				
WORKING				
Focal child 2 y	-0,04	(0,03)	-0,04	(0,03)
Focal child 3 y	-0,04	(0,03)	-0,03	(0,03)
Focal child 4 y	0,00	(0,03)	0,00	(0,03)
Focal child 5 y	-0,01	(0,03)	0,00	(0,03)
LOG EARNINGS				
Focal child 2 y	-0,59	(0,27)	* -0,53	(0,26) *
Focal child 3 y	-0,17	(0,24)	-0,14	(0,22)
Focal child 4 y	-0,02	(0,23)	0,00	(0,23)
Controls				
	No		Yes	

Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child. *** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.1.

Table A.3: Reform effects on leave uptake. Subsample estimates.

	NO CONTROLS			FULL CONTROLS			N
	Est.	(S.E.)		Est.	(S.E.)		
FATHER'S LEAVE DAYS CAPPED							
BY MOTHER'S EDUCATION							
No higher education	10,54	(5,85)	‡	10,39	(5,98)		3635
Higher education	17,61	(4,27)	***	17,93	(4,26)	***	5674
BY FATHER'S EDUCATION							
No higher education	10,56	(4,66)	*	9,73	(4,92)	‡	4871
Higher education	18,25	(4,76)	***	17,74	(4,59)	***	4483
BY UNION TYPE IN 2008							
Cohabiting	12,97	(5,12)	*	13,24	(5,00)	*	5014
Married	14,51	(5,61)	**	17,57	(5,60)	**	4502
BY PARITY OF FOCAL CHILD							
First born	14,40	(6,57)	*	14,06	(6,62)	‡	2603
Second born	12,22	(5,22)	*	13,44	(5,33)	*	4499
Higher order	15,24	(6,74)	*	17,90	(6,29)	**	2414
MOTHER'S LEAVE DAYS CAPPED							
BY MOTHER'S EDUCATION							
No higher education	-25,25	(18,62)		-27,39	(18,72)		3635
Higher education	-20,97	(10,44)	*	-19,09	(10,11)	*	5674
BY FATHER'S EDUCATION							
No higher education	-29,79	(15,37)	*	-31,51	(15,81)	*	4871
Higher education	-13,47	(13,44)		-7,44	(13,41)		4483
BY UNION TYPE IN 2008							
Cohabiting	-22,89	(11,28)	*	-27,43	(11,84)	**	5014
Married	-19,02	(14,06)		-12,93	(13,62)		4502
BY PARITY OF FOCAL CHILD							
First born	-41,30	(20,56)	*	-40,28	(20,49)	*	2603
Second born	-14,80	(13,74)		-14,68	(13,55)		4499
Higher order	-24,93	(22,17)		-17,82	(19,53)		2414

Note: N=9 516. For splits by union type and parity, subsamples sum to N=9 516. Splits by education sum to a lower N due to exclusion of individuals with missing educational attainment. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child. *** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.1.

Table A.4: Descriptive statistics of outcome variables. Placebo analysis sample.

	MEAN	SD	P25	Median	P75	MIN	MAX
A: LEAVE UPTAKE							
Leave days fathers	33,12	32,26	15,00	36,00	36,00	0,00	280,00
Leave days mothers	217,13	92,81	189,00	276,00	280,00	0,00	280,00
B: OUTCOMES FATHERS							
Working ch. 5 y	0,95	0,21	1,00	1,00	1,00	0,00	1,00
Ln(earnings) ch. 5 y	12,86	2,27	13,01	13,24	13,52	0,00	15,90
C: OUTCOMES MOTHERS							
Working ch. 5 y	0,91	0,29	1,00	1,00	1,00	0,00	1,00
Ln(earnings) ch. 5 y	12,18	2,68	12,52	12,86	13,09	0,00	15,62
D: JOINT OUTCOMES							
Her share earnings ch. 5 y	0,39	0,19	0,29	0,40	0,49	0,00	1,00
Union intact ch. 7 y	0,88	0,33	1,00	1,00	1,00	0,00	1,00

Note: N=9 110 for all models. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 or July 15-August 31st. Couples must have co-resided as of 1.1.2007, and the mother must be registered with earned income in 2007, and siblings (if any) must be born at least 16 months before/after the focal child. *** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.1. Children turn 5 in 2013 and 7 in 2015.

Table A.5: Placebo reform effects on leave uptake and outcomes.

	No controls		Full controls	
	Est	(S.E.)	Est	(S.E.)
A: LEAVE UPTAKE				
Leave days fathers	-0,43	(3,55)	-1,26	(3,46)
Leave days mothers	-9,99	(10,40)	-6,02	(9,75)
B: OUTCOMES FATHERS				
Working ch. 5 y	0,01	(0,02)	0,01	(0,02)
Ln(earnings) ch. 5 y	0,10	(0,21)	0,09	(0,18)
C: OUTCOMES MOTHERS				
Working ch. 5 y	0,03	(0,03)	0,03	(0,03)
Ln(earnings) ch. 5 y	0,38	(0,31)	0,33	(0,30)
D: JOINT OUTCOMES				
Her share earnings ch. 5 y	0,04	(0,04)	0,03	(0,04)
Union intact ch. 7 y	0,01	(0,02)	0,02	(0,02)

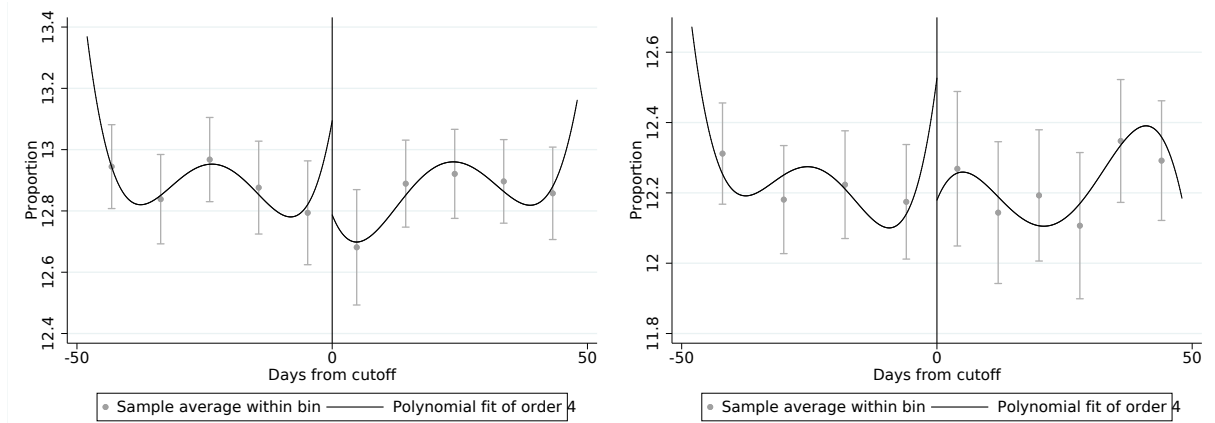
Note: N=9 110 for all models. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 or July 15-August 31st. Couples must have co-resided as of 1.1.2007, and the mother must be registered with earned income in 2007, and siblings (if any) must be born at least 16 months before/after the focal child.*** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.1. Children turn 5 in 2013 and 7 in 2015.

Table A.6: Reform effects on leave uptake and outcomes. Restricted sample (focal children born June and July).

	No controls		Full controls	
	Est	(S.E.)	Est	(S.E.)
A: LEAVE UPTAKE				
Leave days fathers	15,53	(9,15)	16,76	(8,26)
Leave days mothers	-26,36	(17,44)	-26,74	(19,22)
80% compensation	-0,12	(0,11)	-0,13	(0,10)
B: OUTCOMES FOR FATHERS				
Working (ch. 2y)	0,00	(0,07)	0,00	(0,07)
Working (ch. 5y)	-0,04	(0,06)	-0,03	(0,05)
Ln(earnings) ch. 2y	-0,28	(0,68)	-0,19	(0,63)
Ln(earnings) ch. 2y	-0,42	(0,80)	-0,32	(0,69)
C: OUTCOMES FOR MOTHERS				
Working (ch. 2y)	-0,06	(0,05)	-0,05	(0,05)
Working (ch. 5y)	0,01	(0,07)	0,00	(0,06)
Ln(earnings) ch. 2y	-0,57	(0,43)	-0,42	(0,41)
Ln(earnings) ch. 5y	-0,26	(0,60)	-0,24	(0,48)
D: JOINT OUTCOMES				
Mother's share ch. 2y	0,03	(0,05)	0,03	(0,03)
Mother's share ch. 5y	0,02	(0,05)	0,02	(0,04)
Union intact ch. 2y	-0,02	(0,02)	-0,02	(0,02)
Union intact ch. 7y	-0,06	(0,06)	-0,06	(0,06)

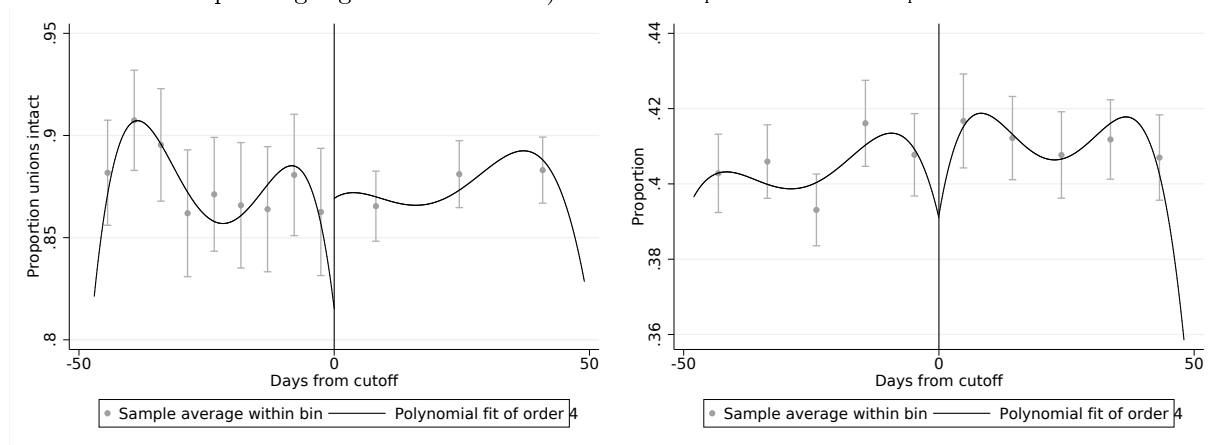
Note: N=3 358 for all models. The sample is opposite-sex couples with children born in 2009, either between June 1-June 18 or July 14-July 31st. Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child. *** p<0.001, ** p<0.01, * p<0.05, ‡ p<0.1.

Figure A.1: Reform effect on log of fathers' (left panel) and mothers' (right panel) earnings when the focal child turns five. Estimates from Regression Discontinuity models. Bars show 95 per cent C.I. for bin-specific means.



Note: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Figure A.2: Robustness tests: Left panel: Placebo reform “implemented” July 1st 2008 (regression results in Table A.5). Effects on probability of intact parental union the year the focal child turns 7 (2015). Right panel: Reform effects on mother’s share of of earnings pre-reform (See Table 5 for corresponding regression estimates). Bars show 95 per cent C.I. for bin-specific means.



Note to left panel: N=9 110. The sample is opposite-sex couples with children born in 2008, either between May 1-June 17 or July 15-August 31st. Couples must have co-resided as of 1.1.2007, and the mother must be registered with earned income in 2007, and siblings (if any) must be born at least 16 months before/after the focal child.
 Note to right panel: N=9 516. The sample is opposite-sex couples with children born in 2009, either between May 1-June 17 (control) or July 14-August 31st (treatment). Couples must have co-resided as of 1.1.2008, and the mother must be registered with earned income in 2008, and siblings (if any) must be born at least 16 months before/after the focal child.

Appendix II: Construction of parental leave data

Parental leave data are obtained from the FD Trygd Database (Akselsen et al., 2007), which contains information on receipt of a range of social transfers. FD Trygd consists of “spells” of transfer receipt. One parent’s leave after one birth can be composed of more than one spell. Spells are registered to parents, and must be linked to children by assumptions. With a maximum leave length of 56 (46) weeks at 80 (100) percent compensation, and few alternatives to parental care for children under 1 year in Norway, the vast majority of leave is taken within the first 1.5 year of the child’s life (Fougner, 2012). We assign leave spells to a child if the following two criteria are met:

1. The leave starts no earlier than the birth date (for fathers) or no earlier than [three] weeks before the birth date (for mothers). Fathers cannot take leave before a child is born. Norwegian expectant mothers are mandated by law to start their parental leave no later than three weeks before their due date. Births are medically induced 12 past due date the latest (<https://helsedirektoratet.no/retningslinjer/svangerskapsomsorgen>). Leaving some time for the birth to happen, the maximum duration between leave start and birth date should hence be five weeks.
2. The leave starts no later than 13 months and three weeks after the child is born.

The second restriction assigns leave spells accurately if children with a sibling born within 16 months of own birth are excluded from the sample. Children with closely spaced siblings differ systematically from children with siblings born further apart, faring somewhat worse on a range of health outcomes (Conde-Agudelo et al., 2006). Strictly speaking, we estimate the effect of the paternity quota for children with no closely spaced sibling. However, previous studies gives no clear foundations to expect effect heterogeneity for this group.