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Effects of extended paternity leave on family dynamics

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Abstract

Objective: Test if an extended paternity quota impacts couples' division of leave and paid work, and if these changes influence union stability, marriage propensity, and further childbearing.

Background: Influential empirical and theoretical works have linked unequal division of household and paid work to increasing divorce rates and falling fertility. This suggests that paternity quota reforms may affect family dynamics if they facilitate more time alone for a father and his young child.

Methods: We analyze an extension of the Norwegian parental leave father's quota from 6 to 10 weeks with a regression discontinuity design. Full population data of parents of children born in a 4-month window around the reform are drawn from Norwegian administrative registers ($N = 9757$).

Results: The reform significantly increased the amount of leave taken by fathers and reduced the amount of leave taken by mothers, while his and her subsequent earnings were unmoved. Neither union stability, fertility nor cohabiters' propensity to marry were affected by the change in leave uptake.

Conclusion: The reform succeeded in changing the division of paid parental leave between parents. However, these changes did not translate into changes in earnings, family stability, or parity progression. This suggests that policies that induce fathers to spend more time with their young child do not move the "stalled" gender revolution along.

KEYWORDS

family policies, fatherhood, fertility, gender roles, parental leave, relationship dissolution

INTRODUCTION

Several influential family theories suggest that the double burden of paid and unpaid work carried by mothers is an important driver of union instability and a reason why some couples stop short of their preferred number of children (Cooke, 2006; Esping-Andersen & Billari, 2015; Goldscheider et al., 2015). In dual-earner families, an unequal burden of unpaid work has been linked to both lower relationship satisfaction (Kaufman, 2000; Barstad, 2014) and union instability (Ruppanner et al., 2017; Sigle-Rushton, 2010; Amato, 2007). When a parental couple is dissolved, less household income and parental time tends to be available to each of the children. As pointed out by Meyer and Carlson (2014), this is of concern, as it potentially reduces the resources available for the care and socialization of children.

Many countries have so-called father's quota or daddy quota policies in place that incentivize fathers' participation in paid parental leave programmes (see, Patnaik 2016 for an overview). The introduction of such paternity quotas has been shown to increase both the share of fathers taking leave and the number of leave days taken by fathers (see Cools et al., 2015, for Norway; Ekberg et al., 2013, for Sweden; Geisler & Kreyenfeld, 2012, for Germany; Patnaik, 2016, for Canada; Farré & González, 2019, for Spain). In other words, paternity quotas bring fathers out of the labor market and into the home for a designated period of time. If the mother is engaged in paid work, the father will care for the child alone in her absence. Granted that fathers' improved skills and knowledge in childcare stick, paternity leave policies can thus help unlock a stalled gender revolution, to the potential benefit of both parents and their children.

In the Nordic countries, men who take somewhat longer paternity leave live in more stable unions (Lappegård et al., 2020) and are more likely to have a second child (Duvander et al., 2019). However, as fathers who are more committed to their partner may both spend more time on care work and be more likely to want another child, these associations need not indicate a causal relationship. Previous studies with a plausible causal design report mixed (and often null) findings for both union stability and fertility (cf. Avdic & Karimi, 2018; Cools et al., 2015; Farré & González, 2019). Hence, the impact of paternity quotas on family dynamics remains unclear. Importantly, most previous causal studies have focused on the introduction of relatively short paternity quotas of around 1 month. Qualitative studies suggest that shorter (1 month) and longer (2 months or more) paternity leaves are of a different nature (Brandt and Kvande, 2018). Whereas fathers on both shorter and longer leaves reportedly strengthened the bond with their child, only fathers on longer leave assumed the main responsibility for housework. These fathers reported an increased understanding for how demanding housework can be, and the efforts their partners put in. If such differences between shorter and longer leave apply generally, longer leaves will have a greater potential to change family dynamics than shorter ones do.

With this background, our article aims to provide evidence of a causal link between a government-induced extension of an existing paternity quota from 6 to 10 weeks and the subsequent union stability, marriage rates, and fertility of couples affected by the new law. An obvious challenge when studying the causal effect of paternity leave policies is that fathers that take more leave are different from fathers that do not in ways that we cannot observe in the data. If we simply compare the outcomes in families of fathers with short and long leave histories, our results will likely be biased by more engaged fathers also spending more time on leave with their child. Hence, such an approach would plausibly lead us to overestimate the importance of leave for family outcomes, as we would measure the combined effect of leave and an already engaged father. To overcome such selection issues, we study a Norwegian policy reform that took effect for parents of children born from July 1, 2009. The reform incentivized fathers to increase their time at home by 4 weeks and mothers to decrease their time at home by 2 weeks (NAV, 2015b). We evaluate reform effects in a regression discontinuity design, comparing co-resident couples

(married and unmarried) with children born just before the extension of the father's quota with couples who had a child just after this date ($N = 9757$). Our sample is restricted to opposite-sex couples, as the theoretical perspectives we build on a focus on their dynamics. The reform may affect the dynamics in same-sex couples differently, and we have insufficient statistical power to explore this empirically. All outcome variables are drawn from administrative registers, ensuring zero attrition and high validity. In a first step, we establish whether the extended quota affected the number of paid leave days and the (relative) earnings and propensity to work for the mothers and fathers. Then, in a second step, we estimate effects of the reform on union stability, fertility, and the propensity to marry 1–5 years following the policy implementation.

It is important to highlight that the extension of the paternity quota from 6 to 10 weeks in 2009 took place in a social and political climate where paternity leave had become more common than when the policy was first introduced in 1993, and the policy response was immediate: While 12% of fathers took 10 weeks or more of leave prior to the new policy, 63% of fathers with a child born just after the cutoff did the same. In contrast, the introduction of the 4-week paternity quota in Norway in 1993 affected a smaller and likely more selected group, increasing take-up from 3% to about 25% (Cools et al., 2015). Following the 2009 reform, the average father increased his leave by about 3 weeks, and this reform thus allows us to estimate effects of a longer paternity quota on family dynamics in the (quite literally) median family.

The most important contribution of this study is that we analyze the impact of an extended paternity quota on several demographic processes within a single institutional framework. Processes of fertility and union dissolution are strongly interlinked; a positive effect on fertility could be due to increased union stability entirely, and if the reform increases fertility, this could in turn stabilize unions.[1] As such, a more complete picture of these interrelated processes will allow us to better understand the mechanisms underlying effects (if any). For all these transitions, the gender revolution perspective (Goldscheider et al., 2015) suggests that extended paternity leave is part of a process making couples more gender egalitarian, thereby increasing fertility and reducing union dissolution rates. Our broad approach allows for a comprehensive test of whether extended paternity leave puts the mechanisms suggested by the gender revolution theory in motion.

In addition to this main goal, our analysis makes three important contributions to the literature. First, we pay particular attention to couples who we would theoretically expect to be the most responsive to changes in family dynamics, such as those in relationships with characteristics that are associated with a higher likelihood of dissolution and/or childbearing. This allows for a more finetuned understanding of the underlying mechanisms that might bring about any observed changes in family dynamics. Moreover, our study is—to the best of our knowledge—the first to assess effects of paternity leave reforms on the propensity to marry. The majority of Norwegian couples eventually get married (Wiik et al., 2009), with marriage having a "capstone" function (Holland, 2013). Cohabiters who intend to marry have a higher relationship quality than those who do not (Brown & Booth, 1996; Wiik et al., 2009). An increase in marriage rates could therefore signal an increase in commitment and relationship quality, to the benefit of both parents and their children. As the transition to marriage is a less life-changing decision than a union disruption or trying for another child, it may also be more easily impacted by smaller changes in relationship quality. Finally, we assess effects in both the short (1 year) and medium (5 years) term, based on an expectation that *when* transitions happen are more easily influenced than *whether* they happen (Gauthier, 2007).

REFORM DETAILS

The Norwegian parental leave system ensures income replacement and job security so that employed parents can care for their new child. The Norwegian government introduced a father's quota on April 1, 1993, with the explicit goals of strengthening the relationship between

father and child and increasing gender equality in the division of paid and domestic work between the parents (Norwegian Ministry for Children and the Family, 1992, p. 30). This policy reserved 4 weeks of leave exclusively[2] for the father, and divided the parental leave into a mother's quota, a father's quota and a period which could be divided freely between the parents. These three components have gone through several changes since 1993; see Table 1 for an overview of the development up until the reform we consider here (i.e., until 2009). At all time points, parents could choose between 80% and 100% income replacement for a correspondingly longer or shorter leave period.[3] The social security system replaces earnings up to a cap of 6G,[4] but several employers, including the Norwegian public sector, top up parental leave compensation for incomes above this cap.

As we can see from Table 1, the father's quota was expanded from the original 4 weeks implemented in 1993 to 5 weeks in 2005 and then to 6 weeks in 2006, with a corresponding 1-week expansion in the total leave period in both these years. The expansion in 2009 stands out because while the father's quota was extended by 4 weeks, only 2 weeks was added to the total leave period. The remaining 2 weeks were moved from the shared leave, and implied fewer weeks for the mother if she was to use the entire shared leave. This 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 shareable leave (Dahl et al., 2014; Fougner, 2012).

All fathers whose child was born on or after the policy implementation date were eligible for the increased 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 during 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 of the 10 months before the child was born. Moreover, it was required that the mother's eligibility was based on at least 50% employment (Norwegian Ministry for Children and the Family, 2009, p. 3).

Since the first father's quota was introduced, it has been required that the mother has a certain labor market activity if the father uses the shared weeks of paid parental leave. This is not the case when the father uses the father's quota (NAV, 2016). Prior to 2009, the family could in other words spend the paternity quota weeks together with the mother taking either paid holiday,[5] unpaid leave, or graded leave (Norwegian Ministry for Children and the Family, 2009, p. 3). After 2009, Norwegian vacation legislation (NAV, 2015a) in combination with the now 10-week father's quota meant that the average family would experience a drop in

TABLE 1 Development in the paid parental leave scheme, shown for 100% and 80% income coverage (number of weeks)

		100% coverage		80% income coverage	
Effective date	**Reserved father**	**To be shared**	**Total weeks**	**To be shared**	**Total weeks**
April 1, 1992	—	24	33	33	42
April 1, 1993	4	29	42	39	52
July 1, 2005	5	29	43	39	53
July 1, 2006	6	29	44	39	54
July 1, 2009	10	27	46	37	56

Note: In the period we study, the length of the paternity quota did not depend on income coverage. Throughout the period, 9 weeks are reserved for the mother, of which three are to be used prior to giving birth and six immediately after. The father cannot take any of his leave days during this 9-week period. However, fathers may take 2 weeks of unpaid care leave during the first 2 weeks of the child's life. Several employers, including the Norwegian public sector, will allow the father to take paid leave during these 2 weeks. This is unrelated to the father's quota.

income if the mother stayed at home with the father throughout the entire father's quota period. 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 fathers spent alone with their child. In the qualitative analyses by Østbakken et al. (2018), many fathers stressed that spending time alone with their child was important both for the development of domestic skills and for the opportunity to "bond" with the child during the leave period (see also Brandth & Kvande 2003, 2018).

THEORY AND EMPIRICAL BACKGROUND

Theoretical framework

Theories of the gender revolution (Goldscheider et al., 2015; see also Cooke, 2006; Esping-Andersen & Billari, 2015; Oppenheimer, 1997) see the increase in female labor supply as a first of two phases, which causes a double burden of paid and unpaid work for women and increases tension in couples around how best to organize the private sphere. This causes a "weakening of the family" (Goldscheider et al., 2010, p. 210), making couples more likely to split up, and perhaps also reluctant to have children. Other family theories, such as specialization theory (Becker, 1991), also describe such a weakening of the family as the immediate result of women's increased employment.

However, the gender revolution theory departs from specialization theory in that it emphasizes a second phase, where men increase their efforts in house and care work, ultimately making partners less specialized. The end point of this second phase is "a more equal relationship between men and women, together with increased commitment to each other and men's increased commitment to children" (Goldscheider et al., 2015, p. 211). This increased involvement of men in the home is expected to counteract falling fertility and decrease union dissolution rates, "strengthening the family" (p. 212). In this section, we outline how an extension of the paternity quota could put the mechanisms suggested by the gender revolution theory in motion, mainly by changing the division of paid and unpaid work.

As suggested by theories on the "first phase" of the gender revolution, longer paternity leave could influence fertility by having a lasting impact on the division of paid work. Across Western societies, women face a "motherhood penalty" in earnings (Budig & England, 2001; Cools & Strøm, 2016). This reduction in earnings due to childbearing, often referred to as an opportunity cost, is a major factor limiting family size. Increasing the length of fathers' parental leave may enable mothers to return to paid work sooner, reducing her human capital depreciation and opportunity cost of childbearing (Bergsvik et al., 2020). If the paternity quota permanently improves fathers' domestic skills, as suggested by Rehel (2014), mothers' improved labor market outcomes may be permanent (Lundberg & Pollak, 1996). We note, however, that if an earnings penalty for fathers emerges or is strengthened with the paternity quota, this could drive a negative effect on fertility.

Extended paternity leave may also pace up the second step of the gender revolution, changing the division of unpaid work in the family. Conflict over unpaid work is among the major sources of marital dissatisfaction (Amato, 2007), and men's efforts in housework are associated with lower risk of union dissolution (Cooke, 2006, for the US; Sigle-Rushton, 2010, for the UK; Ruppanner et al., 2017, for Sweden).[6] 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 (Greenstein, 2009); a pattern confirmed in several single country studies (see, Frisco & Williams, 2003; Kaufman, 2000; Stevens et al., 2001, for the US; Kluwer et al., 1996, for The Netherlands; Barstad, 2014, for Norway; Oláh & Gähler, 2014, for Sweden). Equity theory proposes that unfair social relationships give rise to a feeling of distress, leading (particularly the more unsatisfied) actors to dissolve them (Adams, 1965; Lively et al., 2008). If the

father increases his efforts at home as a result of the extended paternity quota, the mother's relationship satisfaction may increase due to increased perceived fairness of the division of housework, and/or because house care and childcare may be more enjoyable as a shared rather than solitary activity.

Based on the gender revolution theory, we have outlined mechanisms suggesting that longer paternity leave could correct a "gender equality deficit" in some couples and have a positive impact on their functioning, thereby influencing their propensity for further childbearing and union dissolution. If the mother wants her partner to step up more in childcare, and an extended paternity quota nudges him to do so, the couple may emerge as a happier and/or more efficient team. If his lack of involvement in house care and childcare has made her reluctant to have another child, or made her consider dissolving the union, these outcomes may be affected in turn. We note, however, that a longer paternity quota could contribute to a "father's penalty" in earnings, potentially reducing fertility by making fathers more reluctant to have another child.

Literature review

Quasi-experimental evidence on paternity leave and the division of paid and unpaid work

Studies of law changes in Germany and Quebec suggest that longer paternity leave give a more equitable division of house and care work. A large expansion of parental leave in Germany in 2007, also reserving 2 months for the father, increased fathers' time spent on childcare (Schober, 2014), albeit not on housework (Kluve & Tamm, 2013). Tamm (2019) shows that reform-induced variation in paternity leave for higher parities in Germany gave a lasting increase in fathers' time spent on childcare and housework. The introduction of paternity quotas in Quebec also gave a lasting shift toward a more equal division of household work in the family (Patnaik, 2016) and increased fathers' time solo parenting (Wray, 2020).

The German reforms that impacted unpaid work left paid work unmoved (Kluve & Tamm, 2013; Tamm, 2019), suggesting, perhaps, that unpaid work is in general more malleable. Most studies from the Nordic context support that paternity quotas neither increase maternal earnings nor reduce paternal earnings. Cools et al. (2015) find no (negative) effects of the paternity quota introduction on father's earnings or labor supply in Norway, and unexpectedly, a small negative effect on mother's earnings. Findings in a recent study by Østbakken et al. (2018) reveal no permanent effect of the Norwegian 2009 expansion of the paternity quota on a range of labor market outcomes using a difference-in-difference design. The implementation of the Swedish "daddy month" had no long-term effects on the earnings of fathers or mothers (Ekberg et al., 2013). Two studies do, however, suggest an equalizing effect. First, a study by Rege and Solli (2013) concludes that there is a substantial but delayed negative effect of the introduction of the Norwegian fathers' quota on fathers' earnings, while mothers' earnings are unmoved. While Rege and Solli (2013) argue convincingly that one can only expect an effect as time passes and a larger proportion of fathers is motivated by the reform, their difference-in-difference analysis is also more vulnerable to bias from time trends than that of Cools et al. (2015), which analyzes the effect of the same reform on couples who had children just before and after implementation. Second, analyzing five Danish parental leave reforms, Andersen (2018) finds that the mother's wage increases when the father's share of parental leave increases. However, since these reforms also changed other components of the parental leave system, it is difficult to rule out that these other changes also influenced the estimated effects.[7]

To summarize, the studies that are least likely to mistake other societal changes for reform effects suggest that paternity quotas do not influence wages or labor supply. The studies that do

suggest an effect are also more prone to mistaking time trends or other reform changes for effects of the paternity quota. Based on the current literature, we primarily expect to find no effects on these outcomes in our analysis.

Paternity leave and family dynamics

Few previous studies address the relationship between paternity leave and union stability. Lappegård et al. (2020) find that (somewhat) longer leave for fathers correlates with increased union stability in Norway, Sweden, and Iceland; an association that may be fully or partly driven by selection. Using a regression discontinuity/difference-in-difference design that handles such selection effects, Cools et al. (2015) find no effect from the introduction of the paternity quota in Norway on marital dissolution 14 years later, suggesting that the correlation between leave length and union stability is mainly due to selection. However, when subsequent studies have allowed the reform effect to vary by socioeconomic background, a different picture emerges. Avdic and Karimi (2018), using a regression discontinuity design, find that the introduction of a paternity quota (but not later expansions) in Sweden temporarily increased union dissolutions among women with lower earnings. They suggest that for some low-earning couples with traditional values, a longer paternity quota disrupts functional patterns of specialization, at the same time as the cost of her taking unpaid leave to compensate may be difficult to shoulder. Olafsson and Steingrimsdottir (2020) found that the introduction of an Icelandic daddy month increased union stability, but only for the higher educated. Margolis et al. (2021) suggest that reforms that increased fathers share of parental leave in Quebec increased union stability, an effect concentrated among couples with more gender egalitarian values.

To the best of our knowledge, no previous studies have investigated the effect of paternity quotas on cohabiting parents' propensity to marry. Married couples and cohabitants who intend to marry have higher relationship quality than cohabitants without such intentions (Brown & Booth, 1996; Wiik et al., 2009). As most Norwegian cohabitors with children intend to marry at some point, shifts in the timing of this marriage may be quite sensitive to changes in relationship quality.

Finally, for fertility, previous analyses indicate a positive association between paternity leave uptake and the risk of second births (Duvander et al. (2019) for Norway, Sweden, and Iceland). For higher order births, a negative relationship was found in Norway and Sweden and no relationship was found in Iceland (Duvander et al., 2019; Lappegård, 2010). Studies that credibly handle self-selection are, again, rare. Cools et al. (2015) find no effect of the introduction of the paternity quota on fertility after 14 years. However, as only a small proportion of fathers were induced to take more leave immediately after this reform, large effects could perhaps not be expected in their regression discontinuity model, which captures changes for families with children born just after the reform. Farré and González (2019) analyzed the introduction of a 2-week paternity leave in Spain using a regression discontinuity design, and found that the reform delayed childbearing and reduced higher-order births among women aged 30 years or older. The diverging findings in this study could suggest that paternity quotas may have a more negative effect when implemented in the South European context, where public support to families is scarce and family values are more traditional. However, features of the reform and design could also make it easier to detect existing effects of the Spanish reform compared to the Norwegian one.[8]

Hypotheses

The gender revolution theory, backed by empirical studies on unpaid work and relationship satisfaction, suggests that in dual-earner couples, her doing more unpaid work than him is a major

source of tension. A longer paternity quota equalizes the division of care for the new child and may also impact the division of unpaid work in the long run. We test the expectation that this could improve relationship satisfaction and reduce union dissolution risk in our first hypothesis:

H1: A longer paternity quota reduces union dissolution risk.

We note that this effect may be more pronounced for cohabiting couples. In general, cohabiting unions are more fragile than marriages, also when comparing couples with children (Hart et al., 2017; Lyngstad & Jalovaara, 2010). This means that cohabiting couples—on average—will be less committed to stay in the union, and closer to a "threshold point" where the union is dissolved. If a longer paternity quota has a (even small) positive effect on relationship quality across subgroups, some cohabiting unions who would otherwise have been dissolved, may remain intact.[9]

Relationship quality is a strong predictor of intentions to marry among cohabiting couples (Wiik et al., 2009). If a longer paternity quota also reduces tensions in cohabiting unions, as expected by the gender revolution theory, higher marriage rates may follow. This forms the basis of our second hypothesis:

H2: A longer paternity quota makes cohabiters more likely to marry.

If a more equal division of unpaid work reduces mothers' opportunity costs and increases relationship satisfaction, couples may be more likely to have another child. We test this expectation in our third and final hypothesis:

H3: A longer paternity quotas increase fertility.

In general, we expect short-term changes to be larger than medium-term changes: a small improvement in relationship quality may be more likely to pace up an already planned wedding, or make couples try earlier for an already planned child. We can investigate this empirically by assessing if couples affected by the extended quota are more likely to get married, break up or have a child in the first years following the reform than the (otherwise comparable) couples who had a child just before the quota took effect. If effects are transitory, they will appear in data as an effect after 1–3 years, with no differences remaining after 5 years.

Effect heterogeneity by socioeconomic status

Higher educated individuals tend to have more gender egalitarian attitudes than those with lower education (Goldscheider et al., 2015). Similarly, couples where she has higher earnings or education than him tend to divide unpaid work more equally (Bittman et al., 2003). These more egalitarian couples are potentially more positively affected by paternity leave reforms in the early phases of the gender revolution (Goldscheider et al., 2015; Lappegård et al., 2020; Margolis et al., 2021), as gender egalitarian values make them open to be "nudged" toward a more equal division of labor. The establishment of more traditional gender roles at the birth of a child is found to create role conflict and reduce relationship satisfaction most strongly in couples that are initially egalitarian (Margolis et al., 2021; Twenge et al., 2003). If a paternity quota corrects this dynamic, it would be of particular benefit to couples where she has higher education and/or relatively higher earnings. In contrast, some of the more traditional couples (where he or she has lower education, and/or he has a relatively higher education and income) may even be negatively impacted by longer paternity leave, as it interrupts a specialized division of labor that they are happy with (Avdic & Karimi, 2018).

The effect of the introduction of paternity quotas in contexts that are comparable to Norway are so far consistent with expectations for the early phases of the gender revolution. Paternity quota reform reduced union dissolution rates for higher educated couples in Iceland (Olafsson & Steingrimsdottir, 2020), increased dissolution rates for women with lower socioeconomic status in Sweden (Avdic & Karimi, 2018), and had a stabilizing effect on unions concentrated among egalitarian couples in Canada (Margolis et al., 2021).

In later phases of the gender revolution, however, other groups may be more affected (Lappegård et al., 2020). When the Norwegian paternity quota was expanded in 2009, that is, 16 years after its introduction, fathers already took relatively long parental leave in couples where the mother had a higher income than her partner (cf. Duvander, 2014; Lappegård, 2008).

The expansion nudged more fathers to take leave compared to the original reform, suggesting an impact not only on the most egalitarian couples. At this later stage, it is possible that the most egalitarian couples already share (other) unpaid work equally, so that an extended paternity quota matters little for their long-term wellbeing and practices, and hence for demographic outcomes, too.

We will test empirically whether the uptake and effects of the extended paternity quota on demographic outcomes vary between egalitarian and traditional couples (see Margolis et al., 2021). If the effect is strongest for egalitarian couples, we expect to see a larger effect in couples where at least one partner has higher education, and/or where she has relatively higher income and education. If effects are strongest among traditional couples, it should be most marked in couples where at least one partner has lower education, and/or where she has relatively lower income and education.

Differential effects by parity

Parents of a first born have not yet established gendered patterns of childcare (Craig & Mullan, 2011), and couples tend to become more gender traditional when they become parents (Baxter et al., 2008). For parental couples having their first child, roles and practices as parents are being cast. For these couples, the institutional structure—including the length of the paternity quota—may have larger influences than for couples with one or more older children. This leads to an expectation of a stronger effect on family dynamics if the focal child is the first born.

METHOD

The basic idea of using the regression discontinuity approach in our setting is that it allows us to compare similar families just before and after the extension of paternity leave. If we instead simply compared fathers with short and long leave histories, our results could be biased by the fact that more engaged fathers are more likely to spend more time on leave with their child. This would probably lead us to overestimate the importance of leave for family outcomes, as we would measure the combined effect of leave and an already engaged father. In the regression discontinuity model, we think of the policy as an exogenous shock to the family, where those having a child on or after July 1, 2009 are eligible for 4 weeks more of paternity leave, whereas otherwise similar families having a child just prior to the cutoff are not. Sharp regression discontinuity takes the following basic form (Angrist & Pischke, 2014):

$$Y_i = \alpha + \rho D_i + \gamma f(Z_i) + \varepsilon_i$$

where Y denotes the outcome, in our case as listed below: parental leave uptake, earnings, union stability, marriage propensity and fertility. α is a constant term, whereas D_i is a dummy

variable for treatment and takes the value one if the new child is born after the cutoff. ρ gives the effect of the reform on the outcome. Regression discontinuity analysis requires a "running variable" to indicate the observations before and after the "cutoff" (i.e., the date of the passage of the law). Here, the running variable is the focal child's birth date, transformed to days before and days after the passage of the law (indicated by negative values for each day before (first day observed before is −1, second day observed before is −2, etc.) and positive values for the days after (e.g., 1 and 2)). The function $\gamma f(Z_i)$ in our setting essentially nets out trends in child birth date; it controls, for example, for possible bias that arises if couple characteristics vary with the birth season of the child in ways that may also influence the division of labor in the family. Our identifying assumption is that the specification of the running variable ($\gamma f(Z_i)$) nets out all variation in the outcome correlated with birth date (the running variable) that is not due to the reform.[10] We also show discontinuity plots with a linear fit at each side of the reform cutoff.

Selection around the cutoff date may compromise identification (Cools et al., 2015; Tamm, 2013). Such self-selection into (or out of) eligibility could happen in two main ways: 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 cesarean sections. Families with clear preferences for shorter or longer paternity leave may also differ in terms of factors relevant for specialization and union stability. Hence, if such strategic timing exists, comparing families with children born just before and just after the cutoff will yield biased results.

The intention to expand the father's quota to 10 weeks was declared by the Norwegian government in 2005 (Soria Moria, 2005, p. 43), but the policy and its details (including date of implementation) were not proposed in the Council of State until April 3, 2009 (Stortinget, 2015). This would leave less than 9 months until the implementation, suggesting that the strategic timing of conception should not be a major concern.[11] Cools et al. (2015) find strong evidence of strategic timing of births 2 weeks before and after the 1993 introduction of the father's quota in Norway (see Brenn & Ytterstad, 1997), and by using placebo tests (testing for "effects" on earnings in the year prior to the reform), we also find some evidence of strategic timing. When we exclude parents of children born on the 13 days before and the 13 days after the reform, no such evidence remains, and we keep this restriction in our main analyses.[12] We also present "donut plots" showing how regression discontinuity estimates change when potential strategic timers are excluded from the sample day by day (Supporting Information Figures S1 and S2).

DATA

Study samples

We base all analyses on data from Norwegian population registers covering the time period between 2007 and 2016. Our starting point is parents of children born to parents of opposite sex in May, June, July, and August 2009 (N = 20,551).[13] As multiple births give rise to correlated observations, only one focal child per birth (and parental leave spell) is included in the sample. We make three further restrictions. First, we exclude couples where the father and mother did not live together as of January 1, 2008 (i.e., before the pregnancy) (N = 7246), because our interest lies in the dynamics of coresident couples.[14] As the reform could affect the propensity to enter and dissolve unions, union status must be measured prior to the reform to avoid endogenous conditioning breaking the randomization (Angrist & Pischke, 2009). This restriction means that on average, treated parents have lived together for a longer time at the time of conception.

We have tested whether this restriction influences the results by conditioning the sample on cohabitation as of January 1, 2007, resulting in a more similar relationship duration requirement (on a relative scale) across treatment and control groups. Reassuringly, this condition yields similar results (these are available upon request). Second, as an exogenous proxy for

parental leave rights, we exclude focal children whose mothers had missing or zero earnings the year prior to the reform ($N = 717$). Finally, and as described above, we exclude couples who had a child in the 4 weeks around the reform, due to issues of strategic timing ($N = 2831$). The final sample for analysis of sociodemographic outcomes (i.e., of earnings, union dissolution, marriage, and fertility) consists of 9757 couples.

Measurement of parental leave outcomes requires one additional restriction, as leave spells are registered to parents rather than children. Hence, in order to link leave spells to focal children, we exclude 241 couples who had another child 0–15 months before or after the focal child was born (see Supporting Information, Part B for details), and then assume that any parental leave taken within 15 months is linked to the focal child. The final parental leave sample consists of 9516 couples. This additional restriction may imply that we exclude a higher share of parents if the reform did indeed have a positive effect on (short-term) fertility, so that the parental leave sample is endogenously conditioned. We will pay close attention to this in our estimations.

Outcome variables

Measures of parental leave uptake

The main parental leave outcome is the number of paid leave days taken by the mother and father, respectively.[15] We also estimate the effects on the number and average length of each parent's leave spells, as well as their propensity to take part-time leave.[16] Together, these characteristics give an impression of whether the extended father's quota led to 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 parental leave outcomes, separately by reform status, are shown in Table 2. We provide additional details of the structure of the parental leave data and samples in the Supporting Information, Part B.

Earnings

To measure if longer paternity leave changes the opportunity costs of childbearing for mothers and fathers, we use data on earnings from administrative records. We use 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 the calculation of log earnings. We estimate effects both on the probability of being employed (defined as a dummy variable taking the value one if earnings exceed 1G, otherwise zero—see footnote 4) and log earnings, for both parents. Earnings are measured from 2010 (when the focal child turns one, and one parent is typically still on paid parental leave) to 2014 (when the focal child turns five). We also construct a measure of specialization in market work by dividing her earnings by the sum of her and his earnings.[17] An increase in this measure means a shift toward a less traditional division of paid work.

Union stability and marriage propensity

Union stability is measured annually on January 1 from 2011 (when the focal child is 1 year old) to 2015 (when the focal child is 5 years old). For each year, we construct a dummy variable taking the value one if the parental couple is still registered as living together, otherwise zero.

TABLE 2 Mean differences by reform status (outcome variables)

Parental leave sample	Post	Pre	Post–pre
Father's days of leave	47.05	33.09	13.95**
Father takes leave	0.78	0.77	0.01
80% compensation	0.45	0.55	−0.10***
Mother's days of leave	201.93	217.58	−15.64***
Mother takes leave	0.88	0.89	−0.01*
Observations	4617	4899	
Paid work sample			
Mother's share 5 years	0.40	0.39	0.01
Father working 5 years	0.95	0.95	−0.00
Father ln(earn) 5 years	12.84	12.87	−0.03
Mother working 5 years	0.91	0.91	−0.00
Mother ln(earn) 5 years	12.20	12.21	−0.01
Family dynamics			
Union intact ch. 5 years	0.89	0.90	−0.01
Married 5 y	0.70	0.71	−0.01
Has younger sib ch. 5 years	0.35	0.34	0.01†
Observations	4747	5030	

Note: The samples are opposite-sex couples with children born in 2009, either between May 1 and June 17 (control) or July 14 and August 31 (treatment). Couples must have cohabited as of January 1, 2008, and the mother must be registered as having earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.
***$p < 0.001$. **$p < 0.01$. *$p < 0.05$. †$p < 0.1$.

Unions are dissolved by registration of separate addresses. This register measure ensures zero attrition, which is 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 therefore consider it unlikely to bias our results. Marriage propensity is measured in the same year as a dummy variable taking the value one if the partners are registered as married, otherwise zero.

Fertility

We construct variables for the cumulative number of younger siblings born before the focal child's first (2010), second, third, fourth, and fifth (2014) birthdays. Based on these count variables, we construct dummies for having at least one younger sibling within each time frame.

Control variables and subsample stratification

While a valid regression discontinuity design does not require the inclusion of covariates other than the running variable, covariates can both sharpen the precision of the estimates and provide robustness checks. Most importantly, we use information on observable characteristics measured prior to the reform (in 2008) to perform subgroup analysis. We use marriage register information to construct an indicator taking the value one if the parental union is a marriage, otherwise zero. We also construct a dummy for the child's sex, and a set of dummies for parity of the focal child, distinguishing between the mother's first, second, and higher order births. We

obtain information on educational attainment and enrollment from the National Educational Database. When used as a control variable, educational attainment is grouped into four levels: basic (did not complete high school), completed high school, higher education lower degree (BA), and higher education higher degree (MA or PhD). Missing information on education is coded as a separate fifth category. Based on these five categories, we construct a measure of relative educational attainment, classifying couples as his education highest, her education highest, or the same educational level. We collapse these categories into lower (basic and high school) and higher (higher and lower degree) to retain test strength for the subsample analysis. Individuals are defined as students if they have been enrolled in education for at least 1 month during the current year. Mother's and father's ages are each included with linear and curvilinear terms. For subsample stratification, we also construct a dummy variable taking the value 1 if she earns more than him, and 0 if not.

RESULTS

Effects on leave uptake and paid work

Effects on leave uptake

The reform provided fathers with an incentive to take longer paid leave and mothers to take shorter paid leave. The effects on leave uptake are shown in Table 3. The first column presents the linear basic model with no covariates (other than the running variable), whereas the second column displays estimates from a model in which we include a more flexible control for trends. For fathers, the estimates show a substantial increase of about 14 days of leave. Both estimates are statistically significant and unaffected by the inclusion of covariates (results with covariates included are available upon request). Keeping in mind that the reform increased the number of days reserved for the father from 30 to 50 days, and that fathers took 33 paid leave days on average before the reform (Table 2), this is a strong and plausible increase. A visual regression discontinuity (Figure 1a) confirms a clear jump in men's leave days at the cutoff. Furthermore, the share of fathers that took 10 weeks of paid leave or more increased by 50 percentage points (Table 3), a massive increase from the prereform baseline of 12% (Table 2). Neither fathers' propensity to take any leave nor fathers' propensity to take part-time leave were significantly affected.

Estimating effects on leave uptake week by week (Supporting Information Figure S3) shows that the propensity to take 8 and 9 weeks also increased by 60 percentage points, suggesting that the reform predominantly moved fathers from taking just above 6 weeks of leave, to 10 weeks of leave. There were also some small effects above 10 weeks, showing that some fathers took even longer leave than the extension mandated after the reform was implemented.

The point estimates for mothers show that the reform reduced average leave length by about 21 days; that is, by about 2 weeks more than was incentivized by the reform. A visual regression discontinuity for mothers' number of leave days (Figure 1b) confirms a clear drop at the discontinuity. Mothers still tended to use their leave in one continuous break from the labor market, as the average duration of each leave spell and number of leave spells for mothers were unchanged (results available upon request). We note that the increase in total leave length might have made the shorter and better compensated option (i.e., of 100% income compensation) more popular, and that the longer paternity quota meant that the absolute cost of choosing 80% compensation increased for most couples.[18] In line with these changed incentives, the number of couples choosing 80% compensation (and a longer total leave) fell due to the reform (Table 3). This likely explains why the mother's number of leave days was reduced by more than the 2 weeks that were shifted to the father.

TABLE 3 Reform effects on leave uptake and outcomes

	Linear		Square	
	Est	**SE**	**Est**	**SE**
Parental leave				
Father's days of leave	15.39	(1.51)***	15.43	(1.51)***
Father takes leave	0.00	(0.02)	0.00	(0.02)
Father takes ≥ 10 weeks	0.54	(0.02)***	0.54	(0.02)***
80% compensation	−0.07	(0.02)***	−0.07	(0.02)***
Number of days mothers	−16.74	(3.17)***	−16.55	(3.11)***
Mother takes leave	−0.02	(0.01)*	−0.02	(0.01)*
Paid work				
Mothers share ch. 5 years	0.01	(0.01)	0.01	(0.01)**
Father working ch. 5 years	−0.01	(0.01)	−0.01	(0.01)
Father ln(earn.) ch. 5 years	−0.06	(0.10)	−0.07	(0.10)†
Mother working ch. 5 years	0.00	(0.01)	0.00	(0.01)
Mother ln(earn.) ch. 5 years	−0.01	(0.09)	−0.01	(0.09)
Family dynamics				
Intact union intact ch. 5 years	0.00	(0.01)	0.00	(0.01)
At least one younger sibling ch. 5 years	0.02	(0.02)	0.02	(0.02)
Married (cohabiters only) ch. 5 years	0.01	(0.03)	0.01	(0.03)

Note: Ordinary Least Squares/Linear Probability Model estimates from regression discontinuity models. $N = 9516$ for the parental leave sample and 9757 for the sociodemographic sample. The samples are opposite-sex couples with children born in 2009, either between May 1 and June 17 (control) or July 14 and August 31 (treatment). Couples must have co-resided as of January 1, 2008, and the mother must be registered with earned income in 2008. In the parental leave sample, siblings (if any) must be born at least 16 months before/after the focal child.
***$p < 0.001$. **$p < 0.01$. *$p < 0.05$. †$p < 0.1$.

Subsample estimates are shown in Figure 2. We expected that first time-parents would be more strongly impacted by the reform. To the extent that more egalitarian couples (where at least one is highly educated, she earns more than him/has higher education than him) already shared leave more equally, the leave-sharing in these couples may be less impacted by the reform. We found two statistically significant differences (based on a test of an interaction term in a joint model, $p < 0.05$): The effect on his leave length was larger when she had higher compared to lower educational attainment, and she reduced her leave more if they had the same educational level, as compared to her having the highest education. Overall, however, it seems the reform impacted different sociodemographic groups in a similar fashion, and there is no evidence that egalitarian couples were consistently less impacted.

Taken together, these findings show that the reform had a profound effect on the leave uptake of both mothers and fathers, and that effects are substantially larger than what previous studies on the introduction of fathers' quotas in several countries have found (Cools et al., 2015; Ekberg et al., 2013; Geisler & Kreyenfeld, 2012; Patnaik, 2016). The simultaneous reduction in leave days among mothers implies that the father's share of the total leave days increased substantially.

Effects on paid work

To capture effects on his and her opportunity costs, we estimated the effects on his and her earnings. In short, we found no significant effects on his, her, or relative earnings when the child

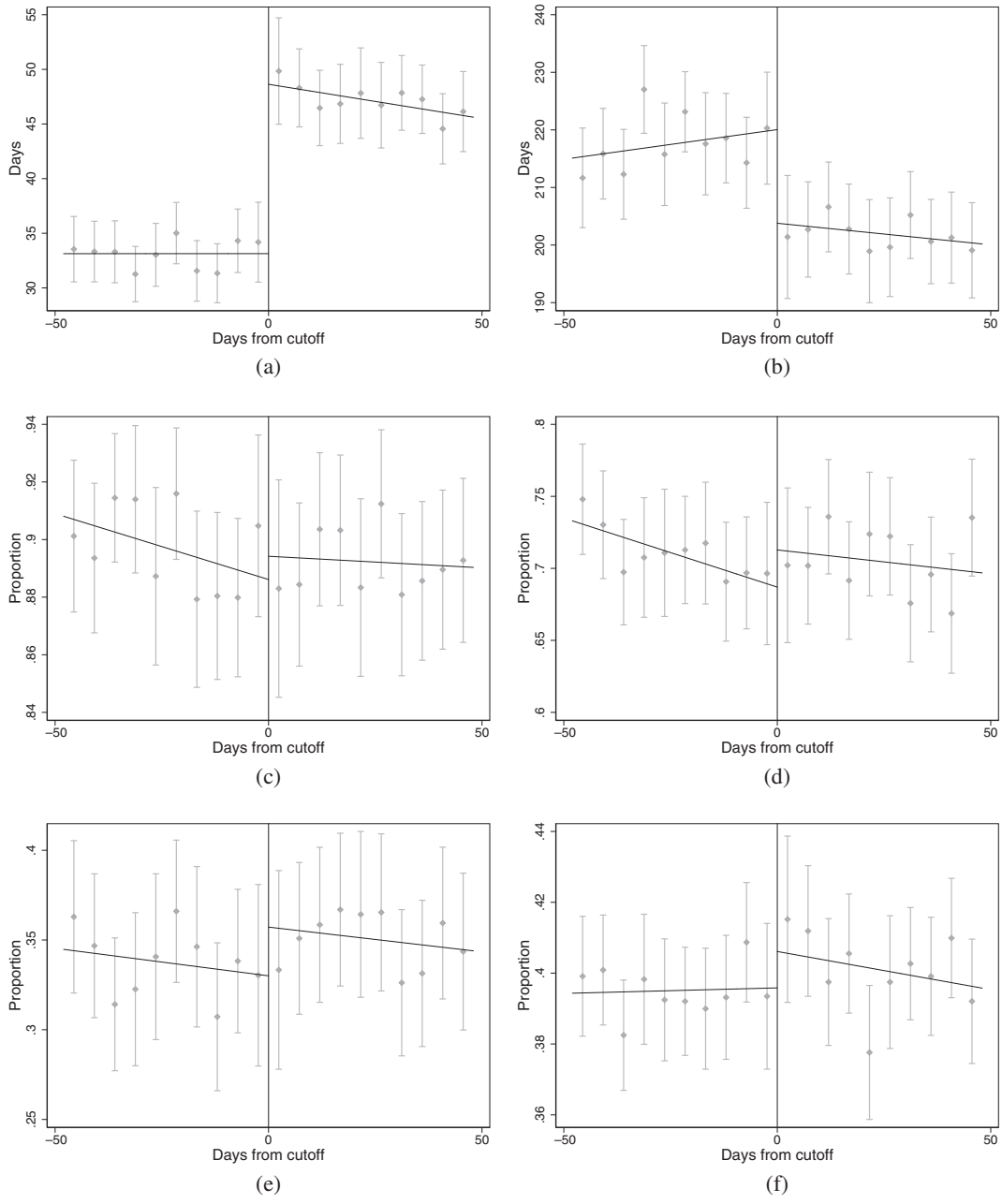

FIGURE 1 Reform effects on leave uptake, earnings, and demographic outcomes. Discontinuity plots. Lines give linear fit on each side of the cutoff. Points give bin-specific means, and error bars give their 95% confidence intervals. Note: N = 9516 for leave uptake sample, N = 9757 for sociodemographic sample. Both samples are opposite-sex couples with children born in 2009, either between May 1 and June 17 (control) or July 14 and August 31 (treatment). Couples must have cohabited as of January 1, 2008, and the mother must be registered as having earned income in 2008. For the leave uptake sample, it is an additional requirement that siblings (if any) must be born at least 16 months before/after the focal child. (A) Father's leave uptake in days. (B) Mother's leave uptake in days. (C) Union intact after 5 years. (D) Married after 5 years. (E) Younger sibling within 5 years. (F) Her share of earnings after 5 years

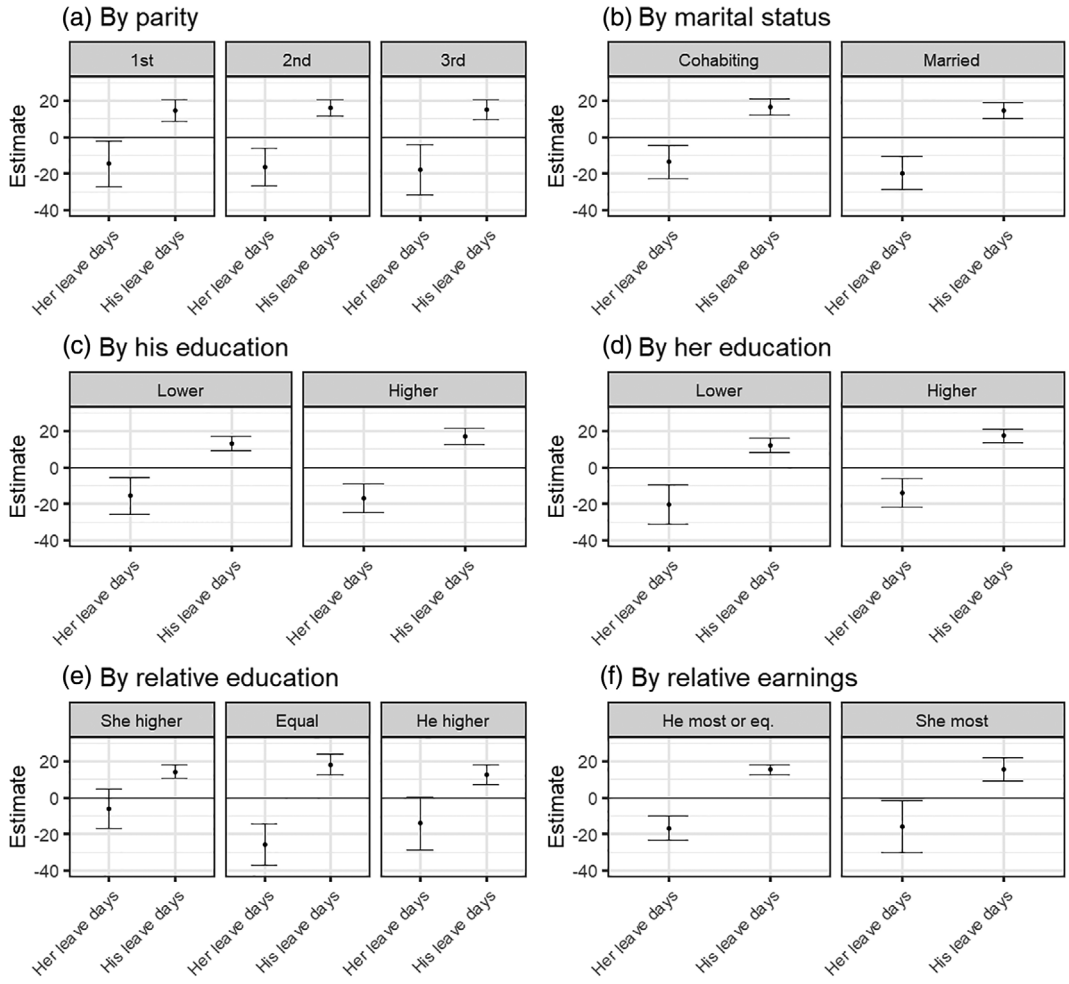

FIGURE 2 Effects on leave uptake. Subsample estimates. (A) By parity. (B) By marital status. (C) By his education. (D) By her education. (E) By relative education. (F) By relative earnings

was 5 years old (Table 3). At this age, most Norwegian children are enrolled in a childcare center, and a more permanent pattern of division of paid and unpaid work is likely to have settled.

Looking at the division of labor in the shorter run, we see a lower probability of being employed for mothers affected by the reform the year the focal child turned two. When the focal child was 4 years old, mothers were back working to the same extent as mothers unaffected by the father's quota (Figure 3). This may indicate that some families responded to shorter total paid leave with some unpaid leave or reduced working hours for the mother (see also Østbakken et al., 2018).[19] None of the other earnings outcomes were affected in the short run. As for leave, we tested whether paid work was differentially impacted in different subsamples. We found no evidence that the earnings of first-time parents were more strongly moved. The reform had a significantly more positive effect on her earnings if he had higher as compared to lower education ($p < 0.05$). However, the same pattern was not found for her higher education, nor her relatively higher education and earnings (results available upon request). As such, we did not find a consistent pattern where egalitarian couples are more (or less) affected by the reform. Our results thus add to the body of studies suggesting that paternity quotas have no permanent impact on the division of paid work in the family.

Paternity leave reform and family dynamics

The expansion of the paternity quota extended the period of paternal care but did not leave a permanent mark on the division of paid work in the family. However, the father's experience with childcare may still impact the family's everyday life in ways that improve relationship quality, and previous studies have shown that unpaid work can be affected even when paid work is not. We now go on to explore if these potential changes had an impact on three central components of family dynamics: union stability, the propensity for cohabiters to marry, and subsequent childbearing.

Union stability

Our first hypothesis stated an expectation that an extended paternity quota would reduce union dissolution rates. To also capture temporary effects, we estimated effects on whether the parental union was intact in the years from when the focal child was 1 (2010) through to 5 (2015) years old. Here, a positive estimate would indicate a stabilizing effect. Our sample included only unions that were intact as of January 1, 2008 by construction. While 98% of the parental unions remained intact when the focal child was one, the proportion gradually decreased to 90% when the focal child was five (Supporting Information Table S4).

We report sharp regression discontinuity estimates of the reform effect when the focal child was 5 years old in Table 3. The estimates are zero to the second decimal, and not sensitive to the inclusion of covariates (available upon request). This is confirmed by the lack of a visible change in union stability around the cutoff (Figure 1c). In the short run (focal child aged one through four), there was a tendency of higher union dissolution risk, but this is far from statistically significant (Figure 3).

We hypothesized that cohabiting couples may be more influenced by the reform, as they are more fragile and thereby more susceptible to changes in relationship quality. Figure 4b shows a tendency for a more negative effect on union stability for cohabiters compared to married couples, but the effects are not statistically different when tested in a joint model ($p > 0.05$). Moreover, there is no support for the expectation that first time parents were more strongly influenced than others (Figure 4a). We also tested whether egalitarian and traditional couples were differentially affected by the reform, and found no such differences.[20]

Propensity to marry

Our second hypothesis stated an expectation that an extended paternity quota would increase cohabiters propensity to marry. Contrary to this expectation, the propensity for cohabitors to marry within 5 years was unmoved by the reform (Table 3). However, we see a tendency for marriage plans to be postponed in the short term (Figure 3), with significantly lower marriage rates in the reform group 1 year after the reform, increasing gradually to a zero effect in the medium term. Such a postponement of marriage plans may suggest, contrary to what we were expecting based on gender revolution theory, that paternity leave has a temporary negative effect on relationship quality .

Parity progression

Finally, our third hypothesis stated an expectation that an extended paternity quota would increase fertility. At age 5, 34% of the focal children in the sample had at least one younger

sibling (Supporting Information Table S4). The average number of younger siblings at age 5 was 0.37, meaning that only a minority had more than one sibling. Reform effects on the probability of having a younger sibling at age 5 are negative, yet statistically insignificant (see Table 3). Supporting the absence of effects, we found no visual discontinuity at the cutoff in the probability of having a(nother) sibling within 5 years (Figure 1e). There was also no evidence of effects in the short term (Figure 3c).

Finally, we assessed the probability of having at least one younger sibling at age 5 in the same subsamples as above (Figure 4). In particular, the probability of having an additional sibling varied strongly with parity, with 75% of first-borns, 24% of second-borns, and 10% of third

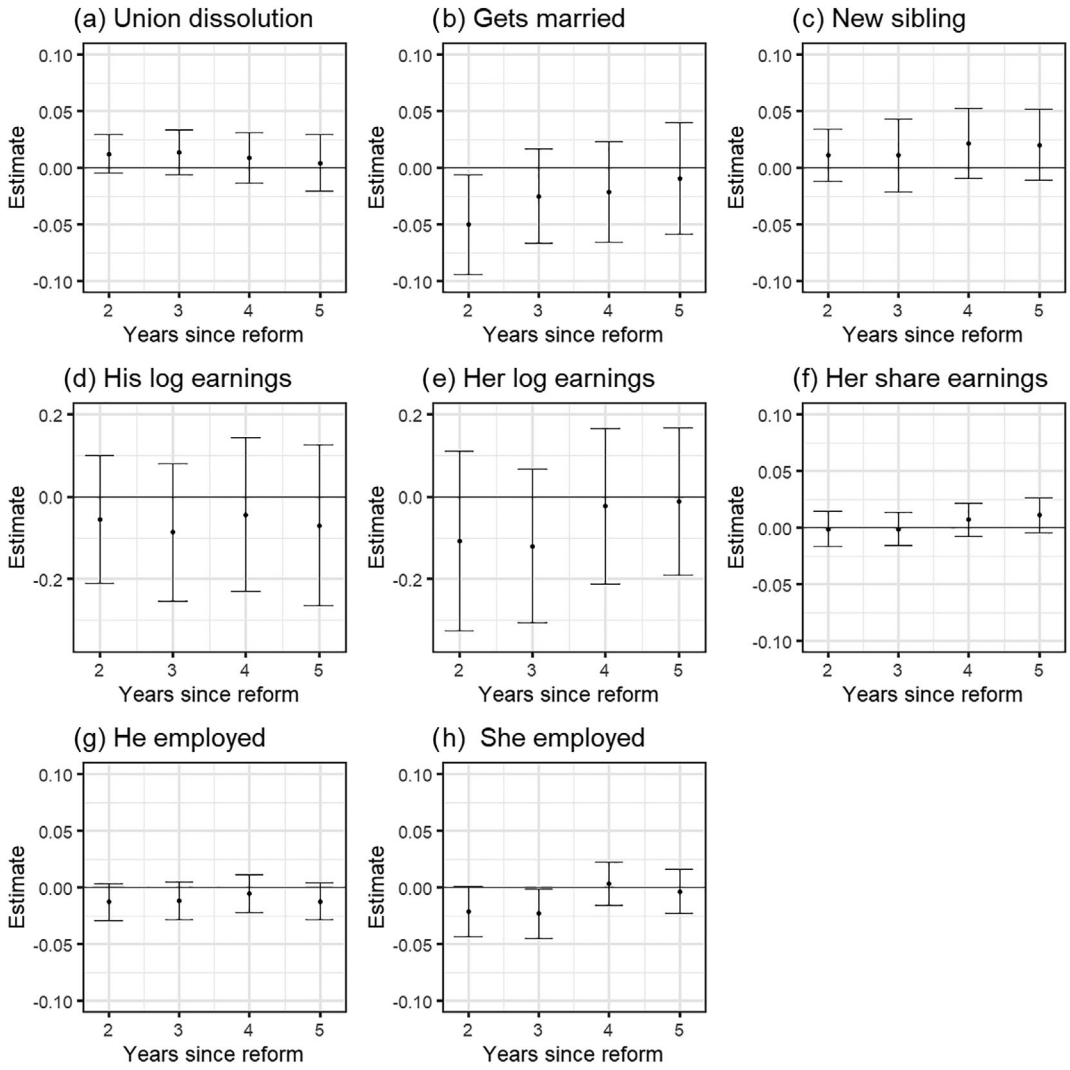

FIGURE 3 Reform effects on sociodemographic outcomes. Ordinary Least Squares/Linear Probability Model estimates from regression discontinuity models, estimated separately by the age of the focal child. Dots mark point estimates and error bars 95% confidence intervals. Black lines indicate basic model, and gray indicate full controls. Note: $N = 9757$. The sample consists of opposite-sex couples with children born in 2009, either between May 1 and June 17 (control) or July 14 and August 31 (treatment). Couples must have cohabited as of January 1, 2008, and the mother must be registered as having earned income in 2008. (A) Union dissolution. (B) Gets married. (C) New sibling. (D) His log earnings. (E) Her log earnings. (F) Her share earnings. (G) He employed. (H) She employed

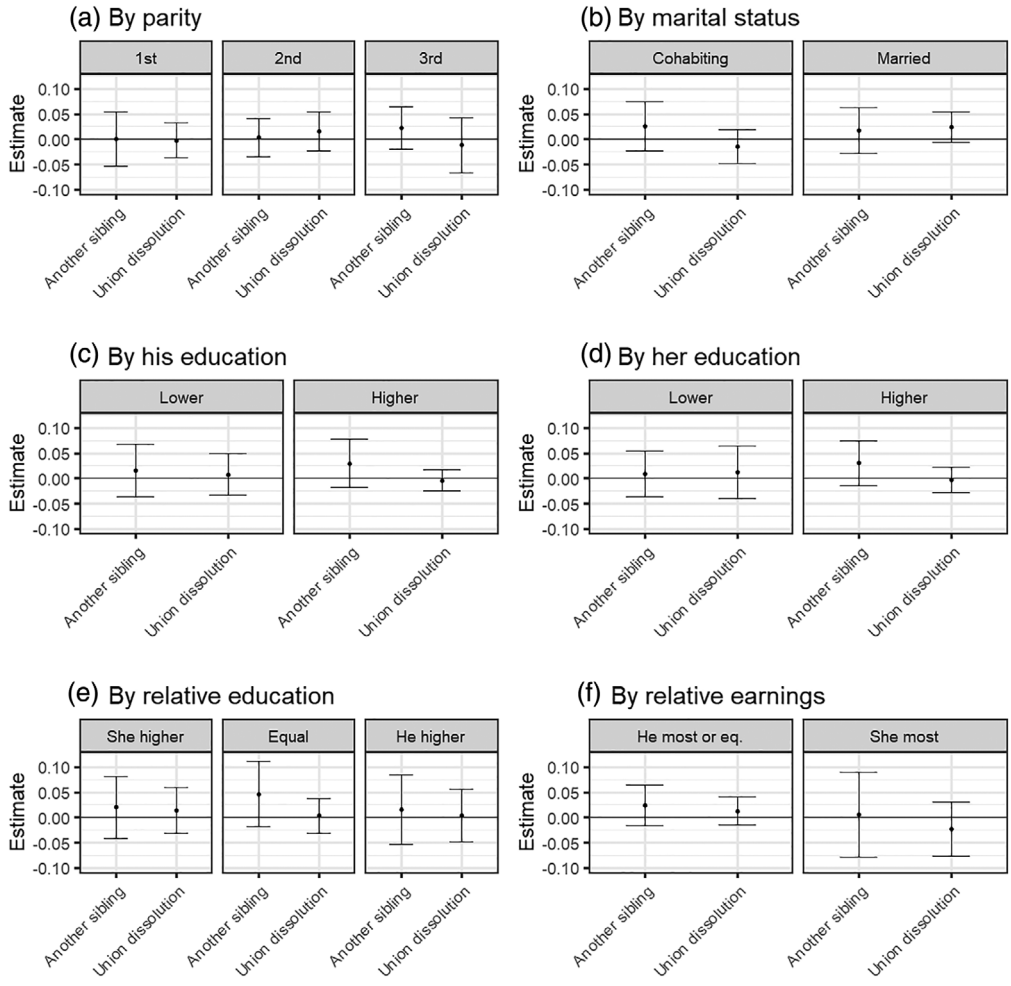

FIGURE 4 Effects on demographic outcomes. Subsample estimates. (A) By parity. (B) By marital status. (C) By his education. (D) By her education. (E) By relative education. (F) By relative earnings

or higher order having an additional sibling within 5 years. Again, estimates for subgroups are similar. As for differences by socioeconomic status, we found no evidence of differential effects between egalitarian and traditional couples.

Robustness tests

We have performed a number of tests to secure that our results are robust. First, we have varied the inclusion criteria for our two study samples to make sure that the results are not driven by a certain cutoff. If results are sensitive to cut-offs, it suggests that they may emerge by chance in our main sample. Moreover, we have tested whether the reform significantly affected pre-reform outcomes, which it reassuringly did not. We have also performed a "placebo reform," "pretending" that the reform took place at a different time than it actually did. Last, we have tested alternative specifications of the running variable. Details of and results for these robustness tests are shown in Supporting Information, Part A.

In sum, the robustness tests strengthen our interpretation that our main model correctly identifies the effects on father's leave taking behavior, and that these changes do not translate into changes in the division of paid work, union stability, marriage propensity, or fertility. The placebo reform suggests that the intervention group, absent of the reform, are three percentage points less likely to choose a longer, less compensated leave, giving on average four fewer leave days for the mother. This suggests that about half the shift toward shorter leave in the reform year, that is, 20% of the reduction in mothers' leave days, is due to seasonal differences rather than a reform effect. It is also noteworthy that the timing effect on cohabiters' propensity to marry is robust to varying exclusion around the cutoff.

CONCLUSION

Increased father involvement has been suggested as a potential pathway to more stable parental unions and increased fertility. This paper studies the effect of a reform extending the Norwegian paternity quota from 6 to 10 weeks on paid work and family dynamics. Institutional constraints and holiday laws mean that this reform is more likely to ascertain that the father spends time alone with his young child than the paternity quota introduction previously analyzed by Cools et al. (2015). Our results show that the reform caused an immediate and substantial increase in fathers' leave uptake. The reform moved about half the fathers to take about three work weeks longer leave; a substantial change affecting a large proportion of the population of eligible fathers. There was also a significant reduction in the length of mother's leave. However, this same paternity quota extension has no lasting effect on maternal or paternal paid work, and we find no medium-term effects on any earnings measure when the child is 5 years old. Our results thus indicate that even in a context where uptake is high, paternity quotas do not affect the earnings of either women or men. The patterns we detect for labor supply are in other words largely concurrent with previous research, which suggests that legal reforms have a strong impact on the division of parental leave days, but little lasting impact on paid work (Cools et al., 2015; Kluve & Tamm, 2013; but see Andersen, 2018; Rege & Solli, 2013, for exceptions). Given these findings, changes in his and her opportunity costs are unlikely mediators of any observed effects on family dynamics.

Our main contribution to the literature lies in exploring whether the meaningful change in the division of paid leave prompted by the reform impacted family dynamics; that is, union stability, cohabitors propensity to marry, and further childbearing. Even in the absence of change in the division of paid work, the father's experience in caring for the child could impact the quality of the parental union. Also, the experience of solo daytime care might leave the father more (or less) inclined to have a(nother) child soon. Based on theories of the gender revolution (Goldscheider et al., 2015; see also Cooke, 2006; Esping-Andersen & Billari, 2015), one could expect paternity leave expansions to promote a more equal division of paid and unpaid work, which in turn could reduce mothers' opportunity costs and increase relationship satisfaction. We test three hypotheses based on this theory.

Our first hypothesis is that *a longer paternity quota reduces union dissolution risk*. We find no support for a lower union dissolution risk for couples who are affected by the extended quota. We also explore whether the less stable unions of cohabitors are more likely to dissolve but find no evidence of such a pattern.

Our second hypothesis is that *a longer paternity quota makes cohabiters more likely to marry*. We study this by looking at the likelihood of marriage among cohabiters. Most Norwegian cohabiters with children plan to marry eventually (Wiik et al., 2009), and one can speculate that changes in marriage timing is the measure most easily moved by smaller changes in relationship quality: A small reduction in relationship quality may not be enough to make them split up their household, but may put wedding plans on hold. However, we find little support for this in

the data. If anything, there seems to be a small tendency for cohabiters to postpone their marriage in the short term, although this is not a lasting effect.

Finally, our third hypothesis is that *lpaternity quotas increase fertility*. We explore this by looking at whether couples eligible for the new paternity quota are more likely to have another child in the first 5 years after the focal child is born. We find no evidence of such increased fertility; couples with children born just before and just after the reform cutoff are just as likely to have another child. This is true also when we limit the sample to couples who had their first child around the reform.

We also test whether different socioeconomic groups responded differently to the reform, focusing on differences between egalitarian and traditional couples. We find a tendency of a larger change in the number of leave days in couples where she has higher education, but this is not mirrored by a stronger impact on family dynamics, and it was not found when using other measures of gender equality. This contrasts with studies from other contexts, which tend to find increased union stability for more egalitarian couples (Olafsson & Steingrimsdottir, 2020, for Iceland; Margolis et al., 2021, for Quebec), and even increased dissolution rates for women with lower socioeconomic status (Avdic & Karimi, 2018, for Sweden). Our results are, however, in line with studies of the effect of the introduction of the paternity quota in Norway (Cools et al., 2015), suggesting that context may matter for the impact of paternity quotas on union stability.

An important limitation of our study is that we are unable to measure two important mediators in our data: relationship quality and division of unpaid work. Previous research strongly suggests that paternity quotas alter the division of unpaid work. The postponement effects on marriage suggests a small immediate decline in relationship quality, and exploring effects on relationship quality directly is an important question for future research. This requires a data source that combines the sample size required for a credibly causal design with self-reported data on relevant aspects of life in families, such as the division of unpaid labor and relationship satisfaction.

It may very well be that there is a causal link between father involvement and family dynamics, but that the changes in father involvement induced by the reform are too small or local to invoke these causal mechanisms. As such, the reform effect is no perfect test of the effect of father involvement on family dynamics. Also, the absence of effects could be due to the reform setting offsetting mechanisms in motion. In about 40% of the couples, the father did not take extended leave despite the reform, shortening the total leave length available to the family. In these couples, the women will observe a majority of fathers stepping up in childcare, while her partner does not. This signaling effect might destabilize the union or disincentivize further childbearing.

Moreover, we study an extension of a paternity quota in the relatively family friendly Norwegian context, characterized by a regulated labor market and gender egalitarian values. Hence, we cannot rule out that a similar policy would provide other results in a less gender egalitarian context. On one hand, policies further equalizing parenting practices may matter less when the gender revolution has already proceeded quite far. On the other hand, equalizing policies implemented in more traditional contexts may even backfire and reduce fertility (see Farré & González, 2019, for Spain). To understand how welfare regimes shape reform effects, studies from a broad range of contexts is required. We believe that our study is one small yet valuable piece of evidence in understanding the larger picture of the gender revolution.

There is an extensive scholarly debate on the link between father involvement on one side and fertility and union stability on the other. The dominant perspective that nontraditional families would result in more union dissolutions and lower fertility (Becker, 1991) has been challenged by contributions suggesting the opposite effect (Cooke, 2006; Edin & Kefalas, 2011; Esping-Andersen & Billari, 2015; Goldscheider et al., 2015; Sigle-Rushton, 2010). The idea that father involvement strengthens families is intuitively appealing: it reconciles ideals of gender

equality with ideals of stable parous unions and relatively high fertility, indicating that *more*, not less, gender equality is the prescription 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 and increases fertility. On the other hand, we also find little evidence for the opposite, with the exception of a small effect on marriage postponement among cohabiters. Our results show that a strengthening of the father's leave, the most prominent political tool to increase fathers' involvement at home, did not affect family dynamics. This finding raises many important questions for further research, when it comes to the relationship among gender equality, perceived fairness, and relationship quality.

On a more positive note, our results are reassuring for policy makers who contemplate extending the paternity quota, but are concerned that this may introduce or intensify fatherhood wage penalties. We find no evidence that fathers incentivized by the reform to extend their parental leave experienced such penalties. At least in the relatively family-friendly Norwegian environment, fathers who make use of extended paternity quotas to bond with their young children are putting neither their career prospects nor the family income at risk.

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ENDNOTES

[1] This is a reasonable expectation in the Norwegian context, where intentions of not having a birth tend to be upheld (Noack & Østby, 2002), and intending to have a child in the near future is a strong predictor of having a child (Dommermuth et al., 2015).

[2] The father's quota could not be transferred to the mother unless she was a single parent, the father was not eligible for paid parental leave, or the father was too sick or otherwise unable to care for the child.

[3] Note that irrespective of which income compensation is chosen, all leave days have to be used before the child turns 3 years old. If a sibling is born before the leave days are used, and the parents qualify for parental leave also for this other child, the remaining leave days for the previous child are forfeited.

[4] The base rate (G) of the Norwegian Social Insurance scheme is an annually adjusted amount used to define benefit eligibility and calculate pensions. As of July 1, 2009, the base rate (G) was NOK 72881, or USD 11,602 (calculated based on the exchange rated for 2009, https://www.norges-bank.no/en/Statistics/exchange_ rates/currency/USD).

[5] All full-time employees are entitled to 5 weeks of paid holiday in Norway (NAV, 2015a).

[6] Men who are satisfied with their union may, of course, be more inclined to do house and care work than those who are not, and unmeasured characteristics such as personality traits may influence both men's housework and union stability.

[7] While this study is rigorous and covers a number of reforms and time periods using detailed data, we find the results to be inherently difficult to interpret. The reforms are used to instrument the fathers share of leave taking, which is calculated by dividing the father's leave days by the couple's total leave days. Included as a control variable, however, is the mother's leave length. To us, the length of the mother's leave appears to be an endogenous control variable in this setting (Angrist & Pischke, 2009, p. 64).

[8] In contrast to the reform studied by Cools et al. (2015), the take-up of the Spanish father's quota was large and immediate.

[9] Marital couples may experience the same improvement in relationship quality without a change in dissolution risk, as a lower number of couples will be on the margin to dissolve their union.

[10] For robustness, we also estimated a local linear regression for $f(Z)$ using triangular kernel density estimation (results available on request).

[11] It should be noted that the public debate regarding the reform picked up in Norwegian newspapers as early as October 2008 (i.e., 9 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 April 1, May 1, and July 1.

[12] To avoid the possibility 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.

[13] Administrative registers do not distinguish between births and adoptions. However, adoptions are very rare in Norway, and in 2009, there were 591 adoptions according to Statistics Norway (StatBank, 2021). It is very unlikely that this will impact our results. Restricting the sample to focal children born in June and July, or expanding it to focal children born in March–October, yields similar results (see Supporting Information A).

[14] If this restriction was made as of January 1, 2009, it would have hit the prereform and postreform groups quite differently: couples in the prereform group could have entered a union late in pregnancy and still be included in our sample, while couples in the postreform group would have needed to enter the union very early in or before the pregnancy to be included. Such compositional differences could bias our estimates, and this problem is reduced by measuring union status 1 year earlier, before any of the focal children were conceived.

[15] Some parents are registered with a higher number of leave days than the parental leave system allows, possibly due to the erroneous registration of, for example, 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.

[16] Tidskonto ("time account") allows parents to take leave days part-time. For instance, the mother may stay at home with the child certain days of the week and father stay at home the remaining days (see https://www.nav.no/fleksibeltuttak).

[17] Couples without earnings have equal earnings, and each is assigned a value of 0.5.

[18] Fathers earned more than mothers in three of four couples; see Supporting Information Table S4.

[19] Norwegian employees are entitled to a period of unpaid leave directly following parental leave to care for small children.

[20] The pattern also holds when we split the sample by her income in quartiles, and test for tempo effects by income quintile, following the design of Avdic and Karimi (2018) closely (available upon request).

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