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Parental Leave and Intimate Partner Violence*

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Abstract

We examine the impact of a 2002 Danish parental leave reform on intimate partner violence (IPV) using administrative data on assault-related hospital contacts. Using a regression discontinuity design, we show that extending fully paid leave increased mothers' leave-taking and substantially reduced IPV, with effects concentrated among less-educated women. The reform also lengthened birth spacing, while separations remained unchanged and earnings effects were modest. The timing and heterogeneity of impacts point to fertility adjustments—rather than exit options or financial relief—as the key mechanism. Parental leave policy thus emerges as an underexplored lever for reducing IPV.

Keywords: Intimate partner violence; parental leave
JEL Classifications: J12, I38

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1 Introduction

Intimate partner violence (IPV) is a pervasive global social problem, affecting women across both developed and developing countries, with severe consequences for victims and their families (WHO, 2021). Research on the transition to motherhood shows that IPV risk often dips during pregnancy and the immediate postpartum period before rebounding in the following years (Bergvall and Rodríguez-Planas, 2024; Chen et al., 2024). This rebound is especially concerning as it occurs while women are caring for very young children and may mark the onset of IPV becoming a persistent presence. Evidence from Mexico supports this view: Deschamps (2025) finds that young mothers face an elevated and more permanent IPV risk compared to their otherwise equivalent non-mother peers.[1] Exposure to IPV in the years after childbirth is strongly associated with adverse maternal physical and mental health—including depression, anxiety, PTSD, and long-term functional impairment—as documented in longitudinal studies (Brown et al., 2020) and systematic reviews (Howard et al., 2013).[2] Moreover, if IPV persists, its negative effects extend well beyond the early years after birth (Bhuller et al., 2024). Together, this evidence highlights the years following childbirth as a critical period for preventing the escalation of IPV risk.

Several policy approaches aim to reduce IPV specifically in the perinatal period, but the results have been mixed. Routine screening for IPV during antenatal care, often paired with referral, has not reduced subsequent IPV (O'Doherty et al., 2015). Nurse home-visitation programs, sometimes augmented with IPV-specific components, target high-risk pregnant women and provide ongoing support, but show mixed results with some studies reporting reductions in IPV and improvements in maternal well-being, and others finding limited effects (Prosman et al., 2015; Mejdoubi et al., 2013). Low-intensity informational interventions in antenatal settings have not been proven effective (Flaathen et al., 2022). In general, these findings underscore the need for alternative interventions that address the IPV risk faced by mothers.

In this paper, we turn to parental leave policy as a potentially important, yet largely unexplored, lever for reducing IPV. Paid leave may influence IPV through several channels: by alleviating financial stress during the perinatal period, by facilitating greater spacing between births, and by enhancing mothers' empowerment and exit options.[3] We focus on a 2002 reform to the Danish parental leave

[1] For the case of Brazil Britto et al. (2024) document that fathers' rate of being prosecuted for IPV increases steadily and substantially (over 200 percent) in the two years following childbirth, but show that the bulk of the increase reflects an increased reporting rate. The authors also look at the effect of eligibility for maternity benefits – an effective income transfer to the mother in the first four post birth months – but are unable to statistically confirm a mitigating effect on IPV incidence.

[2] IPV during pregnancy also has severe consequences for the children. Key studies by Aizer (2011) and Currie et al. (2022) indicate that maternal exposure to IPV during pregnancy adversely affects birth outcomes and infant health, which may have long-term implications for child development. Vickery et al. (2025) show that IPV in the first four years after birth negatively affects children's development of cognitive and socio-emotional skills.

[3] D'Inverno et al. (2018) recently suggested that parental leave policies may offer a complementary approach to mit-

system that substantially changed both the duration and the structure of paid leave. Prior to the reform, parents were entitled to 24 weeks of full-pay leave, with the option to extend up to 52 weeks at a reduced (60 percent) benefit rate. The reform increased fully compensated leave to 46 weeks and simultaneously eliminated the reduced-benefit extension, thereby extending the period of income security after childbirth.

We draw on administrative data covering the universe of mothers eligible for parental leave and use the introduction of the reform cutoff in a regression discontinuity design to estimate its effects on leave-taking and IPV incidence in the four years after childbirth. IPV is measured using hospital contact records that capture inpatient, outpatient, and emergency room visits across all public and private facilities, with assault-related contacts classified as a distinct category. Consistent with prior work examining the reform's effects on early childhood outcomes (Houmark et al., 2024; Beuchert et al., 2016), we find that the reform lengthened mothers' leave duration with negligible impact on fathers' leave. Our main results show that the reform significantly reduced the incidence of IPV, with effects concentrated among mothers with below-median education. Additional analyses indicate that these reductions were not driven by increased parental separation, and post-leave earnings effects were modest. Instead, the reform increased birth spacing, particularly among less-educated mothers, suggesting that longer intervals between births were a key channel behind the decline in IPV. Taken together, these findings highlight parental leave policy as a promising, previously underexplored tool for reducing IPV and improving maternal and child well-being.

Our study contributes to a growing literature in economics that evaluates policy tools for reducing IPV. Existing work has examined a wide range of approaches, including no-drop prosecution policies (Aizer and Dal Bó, 2009), victim risk assessments (Black et al., 2023), specialized IPV courts (García-Hombrados et al., 2024), and arrest policies (Amaral et al., 2023; Iyengar, 2009). While these interventions are important, they are not designed with mothers as the central target population. Our focus on parental leave policy highlights a different dimension: that benefit policy itself can shape IPV risk. In this respect, our study is related to Bhalotra et al. (2025), who show that laid-off men in Brazil are more likely to perpetrate IPV after their unemployment benefits expire, suggesting that income support and benefit duration can meaningfully affect IPV dynamics.

2 Potential Mechanisms Linking Parental Leave to IPV

The extension of paid parental leave entitlements can influence IPV through multiple channels. These mechanisms differ in whether they operate through entitlement effects—changes in the institutional environment that directly shift household dynamics regardless of changes in leave uptake—or through leave-taking effects, which depend on how individuals adjust their behavior in response

igating IPV.

to the availability of longer leave. Importantly, the theoretical predictions of these channels are not unidirectional: while some arguments suggest that longer leave reduces IPV risk, others imply that it may increase vulnerability. The net effect is therefore theoretically ambiguous.

A first mechanism centers on **exit options and bargaining** power within the household. The exit-option mechanism is rooted in household-bargaining theory (McElroy and Horney, 1981; Manser and Brown, 1980), emphasizing that the distribution of power depends on each partner's outside options. By guaranteeing extended earnings-related benefits and strengthening mothers' rights to return to work, expanded parental leave entitlements can improve women's fallback positions. Even if the additional leave is not fully taken, the existence of a stronger entitlement can shift bargaining dynamics by making exit from an abusive relationship more credible.[4] At the same time, extended leave-taking may have the opposite effect if it reduces women's long-run labor market attachment. Prolonged absences can slow career progression or lower lifetime earnings, thereby weakening bargaining power and potentially heightening IPV risk (Ruhm, 1998; Waldfogel, 1998).

Another mechanism operates through **economic security and household stress**. Financial strain is a well-documented trigger of IPV, shaping both the likelihood of perpetration (Schwab-Reese et al., 2016) and the risk of victimization (Breiding et al., 2017). Access to extended paid leave can mitigate the income loss associated with childbirth and allow improved consumption smoothing. By providing a stable and predictable benefit stream during a period of heightened expenses, longer paid leave may reduce IPV risk.[5] At the same time, if extended leave reduces household earnings in the longer run, financial pressures could rise later, potentially fueling conflict. The direction of the effect therefore depends on whether short-run security outweighs possible long-run costs.

Parental leave may also affect IPV by shaping **relationship quality and maternal well-being**. Postnatal mental health problems, such as postpartum depression, are strongly associated with relationship strain and IPV (Howard et al., 2013; Silverman et al., 2006). By enabling time away from work without jeopardizing employment, longer leave entitlements can improve physical recovery, lower stress, and reduce the risk of postpartum mental health problems. These improvements may reduce IPV by lowering the likelihood of conflict escalation and decreasing direct vulnerability to violence. At the same time, extended leave-taking increases the time that couples spend together at home. This can foster bonding and co-parenting, but it may also heighten opportunities for conflict

[4] Empirically, the link between women's economic empowerment and IPV has received mixed support. Some studies find supporting evidence that stronger outside options reduce IPV (Hidrobo et al., 2016; Aizer, 2010), while others document backlash effects and increased IPV risk (Bergvall, 2024; Erten and Keskin, 2018). Our setting differs in that parental leave entitlements improve women's fallback positions primarily through earnings replacement and legal rights to job-protected leave, rather than relative wage shocks and additional cash transfers. In this sense, the reform may plausibly strengthen mothers' bargaining power in ways less likely to provoke backlash.

[5] A direct route by which the specific reform delivered income security and smoothing was by the replacement of the previous option of extended leave on a reduced benefit with fully paid leave. Prior to the reform more than half of all mothers had leave durations between 24 and 52 weeks, durations that would have entailed some use of the reduced-benefit extended leave option.

if relationship tensions are already present (Noonan et al., 2007; Gelles, 1974; Goode, 1971).

Finally, **fertility behavior and birth spacing** provide another potential channel. Access to longer or more secure leave may reduce the immediate incentive to plan a subsequent birth, reflecting either a preference for prolonged family time or a deliberate strategy of allocating resources and attention to the current child before expanding the family. Alternatively, increased birth spacing could arise by delaying re-entry into employment and parents seeking to re-establish their position at work before undertaking another birth-related interruption. Longer birth spacing can lower IPV risk by reducing cumulative physical, emotional, and financial strain associated with rapid successive pregnancies (Barclay et al., 2024; Conde-Agudelo et al., 2006). On the other hand, some parents may strategically use leave entitlements to maximize benefit streams by shortening spacing between children. If clustering births allows families to stack paid leave periods, this could increase stress and conflict, with negative implications for IPV.

Taken together, these mechanisms underscore the ambiguous theoretical impact of extended parental leave entitlements on IPV. On one hand, longer leave can improve women's bargaining power, enhance financial security, promote maternal well-being, and lengthen birth spacing. On the other hand, it can also increase economic dependency, reinforce traditional gender roles, heighten opportunities for conflict, and in some cases encourage fertility behaviors that exacerbate household strain. Which of these forces dominates is ultimately an empirical question, likely shaped by contextual factors such as the generosity of benefits, the structure of labor markets, prevailing gender norms, and the availability of social services. This highlights the importance of careful empirical strategies that can isolate causal effects and consider heterogeneity across institutional settings.

3 Institutional Setup

The 2002 Parental Leave Reform. Our study exploits a major reform to the Danish parental leave system in 2002. The issue of parental leave was raised during the November 2001 general election campaign, but the reform was not passed until March 20, 2002. It applied retroactively to children born from January 1, 2002 onward, with mothers giving birth between January and March allowed to choose between the old and new schemes. In practice, take-up of the new system was nearly universal, which reduce concerns that parents could have manipulated the timing of childbirth to benefit from the reform.

The reform substantially altered both the duration of paid leave and the structure of benefits. Before the reform, mothers were entitled to 14 weeks of maternity leave, fathers to 4 weeks (two of which had to be taken immediately after birth and two at weeks 24–26), and parents could share an additional 10 weeks, giving 24 weeks at the full benefit rate. After the reform, parents retained the mother's 14 weeks but were granted 32 additional weeks at full benefits to be shared freely, while

fathers' entitlement was reduced to two weeks, both to be taken immediately after birth. In total, the period with full compensation rose from 24 to 46 weeks. At the same time, the so-called child-care leave, which had allowed parents to extend leave by up to 52 weeks at a reduced benefit rate (60 percent), was abolished. Instead, parents were permitted to extend their leave by up to 14 weeks without payment. The full benefit level corresponded to the unemployment benefit rate, which replaced roughly 50–75 percent of prior earnings depending on income, with many employees further covered by contracts that topped up wages to 100 percent for part of the leave period (Olivetti and Petrongolo, 2017). To be eligible for the parental leave scheme, parents had to be employed, self-employed, or unemployed but covered by an unemployment insurance fund. Appendix Figure 1 illustrates the pre- and post-reform benefit schedules.[6]

Intimate Partner Violence in Denmark. Recent survey evidence indicates that 4–5 percent of women in Denmark experience IPV within a given year, and more than 12 percent report lifetime exposure (Ottosen and Vernstrøm, 2022). Rates have shown little change over time. Compared internationally, Denmark's IPV prevalence is close to the European average (Sardinha et al., 2022). During our study period in the early 2000s, there was no specific legislation targeting partner violence and IPV was prosecuted under general assault provisions of the criminal code.

4 Empirical Strategy

We use a regression discontinuity design to estimate the effects of the 2002 Parental Leave Reform, exploiting the fact that parents whose children were born at the end of 2001 and beginning of 2002 were subject to different parental leave systems. Specifically, we estimate local linear regressions of the form:

$$y_i = f(b_i) + \beta D_i + \varepsilon_i,$$

where the unit of observation is the parent of child i. As a first step, we examine how the reform impacted the parental leave duration of each parent. We then turn to our main outcome, an indicator for whether the mother of child i experienced IPV within the first four years after birth. In this model, b_i denotes the running variable (the distance between the child's exact birth date and the reform date), $f(\cdot)$ is a first-degree polynomial in the running variable that is allowed to differ on either side of the cutoff, and D_i indicates whether child i was born on or after January 1, 2002. The coefficient β captures the intention-to-treat effect of the reform on IPV.[7]

[6] For further details on the Danish parental leave system and the 2002 reform, see Andersen (2018).

[7] We do not use the reform as an instrument for parental leave duration, as the reform may have a direct effect on IPV by changing victims' exit options even without a change in actual leave-taking.

In the baseline specification, we use a triangular kernel that assigns greater weight to observations closer to the cutoff. We restrict the sample to a 90-day bandwidth around the reform date to ensure that families on either side are comparable.[8] For inference, we follow the robust bias-correction procedure of Calonico et al. (2019, 2014), which centers confidence intervals on bias-adjusted rather than conventional estimates and uses standard errors from specifications with a higher-order (quadratic) polynomial in the running variable. Accordingly, we report both conventional estimates with robust standard errors and their bias-corrected counterparts, assessing significance based on robust confidence intervals. Finally, we use a rich set of child and parental characteristics to examine the validity of the RD design and to explore potential mechanisms. Data sources and sample construction are described in the next section.

5 Data and Analysis Sample

We use administrative data from Denmark covering the period 1998–2006. The dataset contains individual-level records with unique personal identifiers, which allow us to track individuals over time and link children to their parents. Appendix Table 1 provides definitions of all variables used in the analysis and their sources. Below, we briefly describe the key measures.

Outcomes. Our first outcome is parental leave, measured as the number of days taken after childbirth, separately for mothers and fathers. The main outcome of interest is intimate partner violence, which we measure using hospital contact records (inpatient, outpatient, and emergency) from both public and private facilities. Emergency room visits must specify a primary cause, and assault is classified as a distinct category. We define a mother as experiencing IPV if she had an assault-related ER contact within the first four years after the child's birth.[9]

Child Characteristics. The birth register records the exact date of birth, the child's gender, plurality, birth weight, and whether the delivery was by cesarean section. Using parental identifiers, we link children to their parents and siblings and determine birth order (parity).

Parental Characteristics. Several registers provide detailed information on parental background. For each parent, we observe age, marital or cohabitation status, immigration status, years of school-

[8] This bandwidth is slightly smaller than the optimal 100-day bandwidth suggested by Calonico et al. (2014).

[9] This is our preferred measure for three reasons. First, our measure of IPV is observed for the entire Danish population. This provides us with a large sample of mothers with a childbirth around the reform date, which is required when using a RD-design. Second, prior evidence indicates that police reports are more prone to reporting biases (Doyle and Aizer, 2018). Third, administrative victimization data from police records are only available from 2001, whereas some of our identification checks require data from earlier years. Nonetheless, we also demonstrate the robustness of our baseline results when using police records.

ing, and labor income from four years before birth through four years after birth. We also observe whether the mother had a prior incidence of IPV and whether the father had a prior arrest for crime.

Analysis Sample. The analysis sample consists of mothers eligible for parental leave who gave birth within 90 days of the reform implementation date, January 1, 2002, and for whom the child's father can be identified in the registers.[10] Appendix Table 2 reports descriptive statistics for this sample. About half of the children are girls, 43 percent are first-born, and 5 percent are of low birth weight. Mothers are on average 31 years old at birth, with nearly 15 years of schooling and pre-birth labor income of about 180,000 DKK. Fathers are on average 33 years old, have just under 15 years of schooling, and pre-birth labor income of about 272,000 DKK. 0.5 percent of mothers experienced IPV during the second to fourth years before the child's birth. 0.1 percent of fathers had a violent offense during the same period. Roughly 96 percent of parents cohabit or are married at birth, and in 26 percent of households the mother is the primary earner.

6 Results

6.1 Effects of the 2002 Parental Leave Reform on Leave Duration and IPV

Figure 1 presents visual evidence on the relationship between child birth date and our outcomes. The upper panel shows parental leave duration, while the lower panel focuses on IPV. Within each panel, the left figure is based on a linear specification and the right on a local-linear specification. The results indicate that mothers of children born shortly after the reform took substantially longer leaves, whereas there was no discernible impact on fathers' leave duration.[11] The figures also reveal a clear reduction in the incidence of IPV among mothers giving birth just after the reform.

Table 1 reports the corresponding regression estimates. Columns (1)–(2) present results for parental leave duration and Column (3) for IPV. Panel A displays estimates from linear specifications, while Panel B reports results from our baseline local-linear regression with a triangular kernel. Bias-corrected estimates are shown in square brackets, and robust standard errors in parentheses. We also report pre-reform means for each outcome. Consistent with the graphical evidence, we find that the reform increased mothers' leave duration by about 26 days, with no meaningful effect on fathers' leave. At the same time, the reform reduced the likelihood that a mother experienced IPV within four years of childbirth by 0.33 percentage points. This is a sizeable effect, corresponding to

[10] We classify mothers as eligible for parental leave if a post-birth parental leave record can be identified in the registers. Since all mothers are required to take the first two weeks of maternity leave after childbirth, parental leave payment records allow us to capture the full set of eligible mothers. Fathers can be identified for 99 percent of births.

[11] These findings are consistent with Houmark et al. (2024) and Beuchert et al. (2016), who document the effect of the 2002 reform on parental leave duration.

a decline of more than 60 percent relative to the pre-reform mean of 0.53 percent.

6.2 Robustness Checks

Tests of the Validity of the Regression Discontinuity Design. The validity of an RD design relies on the assumption that individuals cannot precisely manipulate the assignment variable. In our context, this requires that parents do not have precise control over the timing of their child's birth. A potential concern is that couples with due dates close to the reform cutoff may have postponed induced births or scheduled cesarean sections in order to qualify for the new leave scheme. To assess the plausibility of this local randomization assumption, we examine the frequency of births by child birth date within our bandwidth around the cutoff. Consistent with earlier studies using birth date cutoffs around the new year (Houmark et al., 2024; Avdic and Karimi, 2018), we find that the number of births declines in December and rises after January 1. Importantly, however, this pattern is not unique to the reform year (Appendix Figure 2).[12]

We next examine whether observable characteristics differ systematically across the cutoff by re-estimating our baseline model with covariates as dependent variables. Results are reported in Table 2. Column (1) presents conventional point estimates from local-linear regressions, Columns (2) and (3) show the bias-corrected estimates and robust standard errors, and Column (4) reports the mean of each covariate. Overall, the results suggest that families just below and just above the cutoff are similar. Although a few characteristics display statistically significant discontinuities, both the overall pattern and a joint significance test across covariates indicate little evidence of systematic birth timing.[13]

Robustness to bandwidth choice. Appendix Figure 4 examines the sensitivity of our findings to alternative bandwidths. The baseline results remain stable when varying the bandwidth from 30 to 120 days on each side of the cutoff. We also estimate donut RD models that exclude mothers of children born within one week of the cutoff on either side. As shown in Column (2) of Table 3, the results continue to indicate that mothers giving birth shortly after the reform faced a lower risk of IPV.

[12] Estimating a local-linear regression similar to our baseline model, with the number of births by child birth day as the dependent variable, we find an increase of about 18 births (16 percent) at the cutoff in the year of implementation. This estimate is robust to donut specifications excluding observations near the cutoff. Moreover, when implementing a difference-in-discontinuity design that uses adjacent cohorts as a control, the estimated increase is only 4 (4 percent) and is not statistically significant at conventional levels.

[13] Appendix Figure 3 provides visual evidence of covariate balance. In Table 3 we show that the results are robust to the inclusion of covariates and below we document that the baseline effects are driven primarily by lower-educated mothers.

Robustness to model specification. Column (3) of Table 3 adds controls for observable child and family characteristics, while Column (4) introduces a quadratic specification of the running variable. The estimated effects are somewhat smaller when controls are included and somewhat larger with the quadratic model, but in both cases they are statistically indistinguishable from the baseline estimate. We also conduct randomization inference by applying our RD design to adjacent non-reform cohorts, estimating placebo cutoffs for every month between October 1999 and April 2004.[14] Appendix Figure 5 shows that the placebo estimates are centered around zero and are almost always larger than our estimated effect (indicated by the vertical line). Finally, we implement a difference-in-discontinuity design, using different cohorts as controls. Column (5) of Table 3 uses pre-reform cohorts, Column (6) post-reform cohorts, and Column (7) all available cohorts. Across specifications, the results remain remarkably close in magnitude to the baseline RD estimate.

Robustness to alternative measures of IPV. In the final column of Table 3, we measure IPV using police reports. As expected, given under-reporting, the mean IPV rate is more than 30 percent lower than the rate based on ER data. The estimated effect is smaller in absolute terms, but still implies a 50 percent reduction in IPV. Although imprecisely estimated, the direction and magnitude align closely with our baseline results, reinforcing the conclusion that the reform lowered IPV risk.

Taken together, these results show that our finding of reduced IPV following the 2002 parental leave reform is robust to alternative bandwidths, model specifications, and outcome measures.

6.3 Heterogeneous Effects by Mother's Education

Households with different earnings opportunities often respond differently to benefit-related policies, and the same may hold for the 2002 parental leave reform. To examine this possibility, we assess heterogeneity in the reform's effects by maternal education, splitting the sample into mothers with years of schooling at or below the median (14 years) and those above the median[15]. Panel C of Table 1 reports the results.

For parental leave, we find broadly similar increases in leave duration across education groups. Mothers with below-median education extended their leave by an average of 22 days, while those with above-median education extended leave by 31 days. The difference between the two groups is, however, not statistically significant. Fathers' leave durations were unaffected in either group.

By contrast, the estimated effects on IPV diverge sharply by education. Among mothers with below-median education, the reform led to a sizeable and statistically significant reduction in IPV

[14] We exclude cutoffs that would generate non-constant exposure to the reform, i.e., dates too close to the true reform cutoff.

[15] Fewer than 0.5 percent of mothers in our sample are missing information on pre-birth education. We replace these missing values with the population median of 13 years, assigning them to the below/at-median group. The results are robust to instead assigning these mothers to the above-median education group.

incidence. This decline is large relative to the pre-reform incidence in this group, indicating a marked improvement in maternal safety in the years following birth. For mothers with above-median education, however, the estimated effect is essentially zero. These results suggest that the IPV-reducing effects of the reform were concentrated among mothers with lower educational attainment.

6.4 Additional Outcomes and Potential Mechanisms

In this section, we examine a set of additional outcomes to shed light on how changes in parental leave may have influenced family dynamics and economic security in ways that shape IPV risk. The results are reported in Table 4.

Timing of IPV. Our main analysis considers IPV incidence over the four years after childbirth. Since most mothers take less than a year of leave, much of this period occurs after they return to work. To pinpoint when the reform's effect emerges, we estimate impacts separately by post-birth year (Panel A of Table 4). We find no detectable effects in the first two years, but substantial reductions in years three and four. This pattern suggests the reform's IPV-reducing effect did not arise during the leave period itself, but in the subsequent years once most mothers had reentered the labor market.

Fertility. We next examine fertility within four years of the sample child's birth, considering both the number of additional births and the time to the next birth (Panel B of Table 4). The reform led to a small, statistically insignificant reduction in the total number of births across education groups. By contrast, conditional on having a subsequent birth, the reform significantly increased birth spacing—lengthening the interval by about 0.9 months on average, relative to a baseline of 31 months.[16] This effect is concentrated among mothers with below-median education, for whom spacing rose by nearly 1.5 months, compared to only two weeks among those with above-median education. These results suggest that extended leave affected fertility dynamics mainly by delaying subsequent births rather than reducing them outright.

Earnings. We also examine parental earnings in the three years after the leave period, excluding the first year after birth when leave-taking is concentrated (Panel C of Table 4). For mothers, the reform had small and statistically insignificant effects overall. However, estimates diverge by education: earnings rose slightly for mothers with below-median education but declined slightly for those with above-median education. For fathers, the reform reduced earnings by about 2.5 percent on average, driven by families in which the mother has below-median education, where fathers' earnings

[16] The finding of negligible effects of parental leave reforms on the total number of subsequent births (the "quantum effect") but clear effects on timing is consistent with the literature, see for instance Kleven et al. (2024).

fell by over 4 percent. Taken together, these results suggest that in lower-educated families, the reform modestly improved mothers' relative earnings positions, potentially shifting intra-household economic balances.

Separations. Finally, we test whether the reform affected family stability (Panel D of Table 4). We measure separations as whether the mother is no longer living with the father of the sample child four years after birth and complement this with data on cohabitation with any partner. Across both measures, we find little evidence of change. Separation rates did not increase in the treatment group for either education group, offering no support for the idea that the reform raised separations by empowering mothers. If anything, the estimates suggest a modest reduction in separations among mothers with above-median education. Similarly, the likelihood of cohabitation with a partner four years after birth is not reduced and even shows a small positive effect for higher-educated mothers.

To summarize, our results suggest that the parental leave reform increased mothers' leave-taking and reduced IPV in the four years after childbirth, with effects concentrated among mothers with below-median education and strongest in years three and four. The reform also modestly shifted relative earnings, left separation rates unchanged, and lengthened birth spacing. These findings provide a basis for evaluating the mechanisms outlined in Section 2, which we discuss next.

Exit options and bargaining power. Household bargaining theory suggests that a woman's IPV risk depends on her fallback position, with stronger outside options reducing abuse. The reform plausibly improved mothers' fallback positions by strengthening job protection and providing more generous earnings replacement, even without substantially longer leave-taking. If bargaining were the main mechanism, we would expect higher separation rates as stronger exit options enable women to leave abusive relationships. Instead, separations were essentially unchanged, if anything slightly lower among higher-educated mothers. This suggests the IPV decline was not driven by exercised exit options. Still, bargaining dynamics can shift without separations. Among less-educated mothers, relative earnings improved: mothers experienced modest post-leave gains while fathers' earnings declined. This shift could have strengthened women's bargaining positions within relationships, helping deter violence. The concentration of IPV reductions among less-educated mothers fits this interpretation, but the modest earnings changes and stable separation rates indicate that bargaining effects are at most a contributing factor rather than the primary driver.

Economic security and household stress. Another potential channel is that extended leave reduced IPV by smoothing income and easing financial stress around childbirth. By guaranteeing longer earnings replacement, the reform may have shielded families from strain that triggers conflict. If this channel were central, however, IPV declines should have been largest immediately after

birth, when financial relief was most salient, and earnings should have improved durably. Neither prediction holds. IPV reductions occurred mainly in years three and four, after most mothers returned to work. Earnings did not improve overall; less-educated mothers saw modest gains, but fathers' earnings fell, leaving household income little changed. These patterns suggest that while extended leave may have offered temporary relief, economic security is unlikely to be the main driver of the observed decline.

Relationship quality and maternal well-being. Extended leave could have reduced IPV by improving maternal recovery, mental health, and couple dynamics. If so, IPV reductions would have likely emerged soon after birth, when leave-taking directly supports recovery.[17] While temporary improvements in recovery or stress early on could have lasting consequences, lack of reductions in IPV soon after birth combined with unchanged separation rates suggests that this is unlikely to be the dominant channel.

Fertility behavior and birth spacing. The clearest evidence points to fertility as a central mechanism. The reform lengthened inter-birth intervals, especially among less-educated mothers, the group where IPV declines are concentrated. IPV reductions peak around three years after childbirth – typically when subsequent births occur – providing strong temporal alignment between fertility behavior and declines in IPV. Longer spacing may ease cumulative stress from rapid successive pregnancies, lowering conflict and IPV risk.[18] Importantly, the timing of effects aligns: IPV reductions were strongest in years three and four, precisely when differences in fertility and spacing matter most. This temporal match, together with the concentration of effects among less-educated mothers, suggests that adjustments in fertility spacing may be a key pathway through which the reform reduced IPV.

7 Conclusion

In this paper, we provide new evidence on how parental leave policy shapes intimate partner violence. We exploit a major 2002 reform to the Danish parental leave system and show that extending the period of full-pay leave significantly reduced IPV in the four years following childbirth. The effect

[17] Beuchert et al. (2016) find suggestive evidence that the reform improved mental health of mothers with particularly low educational attainment ($\leq$ 10 years of education), as measured by a reduced rate of hospitalization with depression and a reduced likelihood of receiving antidepressants within three years of birth.

[18] Data from our pre-reform control cohorts (cohorts 2000 and 2001 with births within 90 days on each side of January 1) are consistent with IPV risk decreasing with inter-birth spacing. For women having a further birth within two years, we observe an IPV rate of 0.0056 (95% CI 0.0033–0.0079), compared to 0.0033 (95% CI 0.0023–0.0043) for those with a subsequent birth in years 2–4. For comparison, the IPV rate for women with no further birth within four years is 0.0063 (95% CI 0.0054–0.0072).

is large in magnitude and concentrated among mothers with below-median education. We further analyze additional outcomes to shed light on mechanisms. We find no increase in separations and only modest shifts in relative earnings, suggesting that stronger exit options or improved household finances are unlikely to be the primary drivers. Instead, we identify fertility behavior as the most plausible channel. The reform lengthened birth spacing, particularly among less-educated mothers, and the timing of these fertility changes coincides with the largest IPV reductions in years three and four after birth.

Intimate partner violence remains a pervasive problem, with the World Health Organization estimating that about one in three women worldwide experience IPV during their lifetime (WHO, 2021). Our results suggest that parental leave reforms, often justified in terms of child development and maternal labor supply, may also deliver meaningful reductions in violence against women. More broadly, they underscore the importance of incorporating IPV into the evaluation of social policies.

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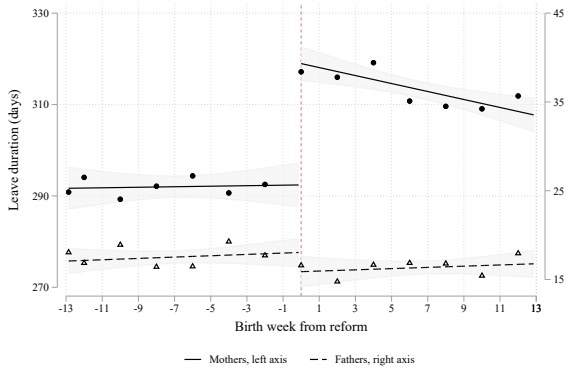
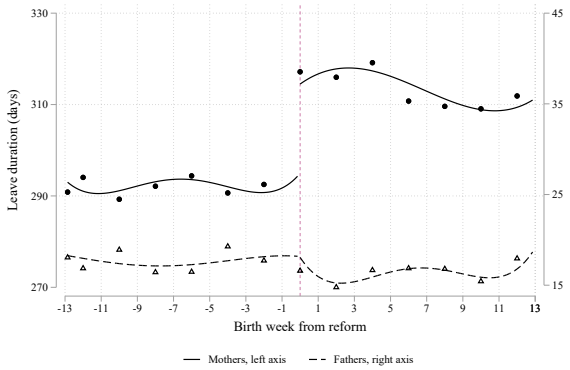
Figure 1: Effect of the 2002 reform on parental leave duration and intimate partner violence

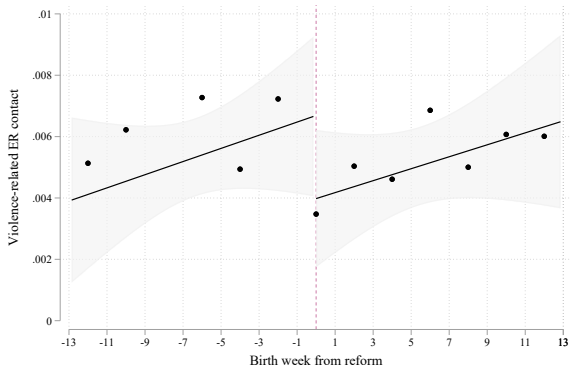
(a) Parental leave duration, Linear fit



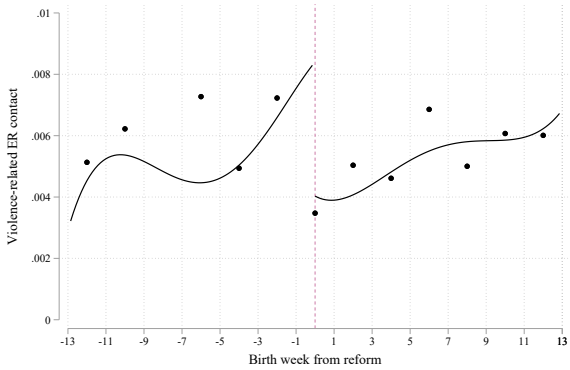
(b) Parental leave duration, Local linear regression



(c) Intimate Partner Violence, Linear fit



(d) Intimate Partner Violence, Local linear regression



Notes: Parents of children born within 90 days on either side of the reform implementation date (January 1, 2002), where mothers were eligible for parental leave benefits and both parents can be identified in the registers. Markers show the average leave duration (Panels a–b) and the share of mothers with a violence-related ER visit (Panels c–d) by week of birth, along with fitted lines. The first column presents linear estimates with separate trends, while the second column shows local linear estimates using a triangular kernel with separate trends.

Table 1: Effect of the 2002 reform on parental leave duration and intimate partner violence

	Mother's leave (1)	Father's leave (2)	IPV (3)
Panel A: Linear model (Full sample)			
Conventional estimate	26.55***	-2.17*	-0.0027
Robust SE	(3.19)	(1.24)	(0.0018)
Panel B: Local linear model (Full sample)			
Conventional estimate	26.61***	-2.20	-0.0033*
Bias-corrected estimate	[25.11]	[-2.35]	[-0.0048]
Robust SE	(4.72)	(2.13)	(0.0027)
Pre-reform mean	292.06	17.63	0.0053
N	22,114	22,114	22,114
Panel C: Heterogeneity by mothers education (local lin. model)			
Mother's education: below/at median			
Conventional estimate	22.40***	-3.87	-0.0081**
Bias-corrected estimate	[19.48]	[-3.54]	[-0.0106]
Robust SE	(7.01)	(2.35)	(0.0044)
Pre-reform mean	297.53	16.09	0.0063
N	11,129	11,129	11,129
Mother's education: above median			
Conventional estimate	31.25***	-0.53	0.0018
Bias-corrected estimate	[31.56]	[-1.16]	[0.0014]
Robust SE	(8.77)	(2.55)	(0.0034)
Pre-reform mean	286.54	19.19	0.0043
N	10,985	10,985	10,985

Notes: Parents of children born within 90 days on either side of the reform implementation date (January 1, 2002), where mothers were eligible for parental leave benefits and both parents can be identified in the registers. The table reports the estimated reform effect on mothers' and fathers' leave duration, as well as mothers' risk of IPV. Panel A presents linear specifications, while Panels B and C report local linear specifications. Panel A and B report the estimated reform effect for the full sample, whereas in Panel C the sample is split by mothers' educational level. The in-sample median education for mothers is 14 years. All standard errors are robust and clustered by date of childbirth. In the local-linear models, bias-corrected estimates appear in square brackets, with robust standard errors in parentheses below. The mean of the outcome is shown for individuals below the cutoff. Stars denote statistical significance (* p<0.1, ** p<0.05, *** p<0.01) based on robust confidence intervals centered on the bias-corrected estimates (Calonico et al., 2019, 2014).

Table 2: Distribution of covariates across the cutoff

	Conventional estimate (1)	Bias-corrected estimate (2)	SE (3)	Pre-reform mean (4)
A. Child characteristics (N=22,114)				
Girl	-0.0092	[-0.0039]	(0.0204)	0.5063
First-born	-0.0227	[-0.0221]	(0.0203)	0.4448
Multiple birth	-0.0015	[-0.0014]	(0.0068)	0.0294
Cesarean section	0.0197	[0.0362]	(0.0237)	0.1883
Birth weight	28.7	[9.5]	(23.9)	3520.2
Low birth weight (<2500 grams)	-0.0102	[-0.0142]	(0.0097)	0.0518
Joint χ^2	8.3120			
p-value	0.2161			
B. Mother's characteristics (N=22,114)				
Born abroad	-0.0104	[-0.0131]	(0.0087)	0.0485
Age at birth	0.221**	[0.391]	(0.192)	30.765
Education (years)	0.104**	[0.182]	(0.092)	14.759
Pre-birth labor income	0.09	[0.27]	(3.45)	179.62
Prior incidence of IPV	-0.0009	[0.0018]	(0.0028)	0.0054
Joint χ^2	6.7466			
p-value	0.2402			
C. Father's characteristics (N=22,114)				
Born abroad	-0.0068	[-0.0142]	(0.0104)	0.0633
Age at birth	-0.006	[0.023]	(0.262)	33.115
Education (years)	0.140*	[0.189]	(0.100)	14.461
Pre-birth labor income	2.98	[2.90]	(6.65)	271.38
Prior charge of violent offense	0.0005	[0.0012]	(0.0044)	0.0094
Joint χ^2	3.0881			
p-value	0.6864			
D. Household characteristics (N=22,114)				
Married/cohabiting at birth	0.0066	[0.0043]	(0.0099)	0.9570
Mother is the primary earner (pre-birth)	-0.0257	[-0.0327]	(0.0220)	0.2288
Joint χ^2	4.3164			
p-value	0.1155			
E. All characteristics (N=22,114)				
Joint χ^2	23.970			
p-value	0.156			

Notes: Parents of children born within 90 days on either side of the reform implementation date (January 1, 2002), where mothers were eligible for parental leave benefits and both parents can be identified in the registers. Column 1 reports the estimated reform effect from a separate local-linear regression with a triangular kernel of the characteristic listed in the row for the family member indicated in the panel heading. Column 2 reports the corresponding bias-corrected estimate, Column 3 the robust standard error, and Column 4 the mean of the variable in the row calculated among those below the cutoff. Stars denote statistical significance (* p<0.1, ** p<0.05, *** p<0.01) based on robust confidence intervals centered on the bias-corrected estimates (Calonico et al., 2019, 2014).

Table 3: Robustness checks

Outcome:	Violence-related ER contact							Police-reported IPV
	Baseline (1)	Donut (2)	Covariates (3)	Polynomial (4)	RD-DID (5)	RD-DID (6)	RD-DID (7)	Crime Data (8)
Conventional estimate	-0.0033*	-0.0037*	-0.0025	-0.0048	-0.0036*	-0.0034	-0.0035*	-0.0018
Biascorrected estimate	[-0.0048]	[-0.0064]	[-0.0038]	[-0.0051]	[.]	[.]	[.]	[-0.0022]
Robust SE	(0.0027)	(0.0035)	(0.0027)	(0.0034)	(0.0021)	(0.0024)	(0.0020)	(0.0030)
N	22,114	20,976	22,114	22,114	68,839	67,631	114,068	22,114
Pre-reform mean of dependent variable	0.0053	0.0052	0.0053	0.0053	0.0053	0.0053	0.0053	0.0036
Model	RD	RD-Donut	RD	RD	RD-DiD	RD-DiD	RD-DiD	RD
Cohorts included	2002	2002	2002	2002	2000-2002	2002-2004	2000-2004	2002
Control for family characteristics			X					
Polynomial order	1st	1st	1st	2nd	1st	1st	1st	1st
Kernel	Triangular	Triangular	Triangular	Triangular	Uniform	Uniform	Uniform	Triangular

Notes: Parents of children born within 90 days on either side of the reform implementation date (January 1, 2002), where mothers were eligible for parental leave benefits and both parents can be identified in the registers (Columns 1-4 and 8). Parents of children born within 90 days on either side of January 1 in the indicated cohorts, where mothers were eligible for parental leave benefits and both parents can be identified in the registers (Columns 5-7). Column 1 presents the baseline estimates. The sample in Column 2 excludes mothers giving birth in the week surrounding the reform date. The specification in Column 3 includes observable child and family characteristics described in Section 5, while the specification in Column 4 includes a second-degree polynomial. Columns 5-7 presents estimates from a difference-in-discontinuity design using the adjacent cohorts indicated as a control group. Column 8 implements the baseline model using an IPV measure based on crime records. All standard errors are robust and clustered by date of childbirth. In the local-linear models, bias-corrected estimates appear in square brackets, with robust standard errors in parentheses below. The mean of the outcome is shown for individuals below the cutoff in the reform cohort. Stars denote statistical significance (* p<0.1, ** p<0.05, *** p<0.01) based on robust confidence intervals centered on the bias-corrected estimates (Calonico et al., 2019, 2014).

Table 4: Mechanisms: IPV timing, fertility and earnings and separations

	(1) Estimate	(2) Bias-corrected estimate	(3) Robust SE	(4) Mean of dependent variable
Panel A: Yearly IPV incidence				
In 1st year from childbirth	-0.0003	[0.0002]	(0.0012)	0.0009
In 2nd year from childbirth	-0.0000	[0.0014]	(0.0013)	0.0014
In 3rd year from childbirth	-0.0013	[-0.0021]	(0.0021)	0.0020
In 4th year from childbirth	-0.0013**	[-0.0033]	(0.0014)	0.0012
Panel B: Subsequent fertility				
Total new births				
Baseline	-0.0236	[-0.0335]	(0.0219)	0.3911
Mother's education below/at median	-0.0356	[-0.0506]	(0.0334)	0.3476
Mother's education above median	-0.0125	[-0.0178]	(0.0280)	0.4351
Time to next birth				
Baseline	0.901***	[1.221]	(0.444)	31.260
Mother's education below/at median	1.436***	[2.363]	(0.841)	30.923
Mother's education above median	0.406	[0.163]	(0.666)	31.531
Panel C: Earnings 1-3 years from childbirth				
Mothers				
Baseline	1.9	[10.0]	(14.8)	605.6
Mother's education below/at median	12.8	[26.5]	(21.2)	525.6
Mother's education above median	-12.4	[-14.5]	(25.1)	686.5
Fathers				
Baseline	-23.7*	[-51.5]	(28.3)	987.1
Mother's education below/at median	-42.9**	[-79.1]	(39.0)	910.0
Mother's education above median	-6.1	[-28.3]	(42.5)	1065.0
Panel D: Separation and cohabitation (4 years after childbirth)				
Separated from father				
Baseline	-0.0071	[-0.0033]	(0.0135)	0.1173
Mother's education below/at median	0.0038	[0.0117]	(0.0219)	0.1372
Mother's education above median	-0.0178	[-0.0178]	(0.0180)	0.0975
Cohabit with a partner				
Baseline	0.0085	[0.0100]	(0.0140)	0.8861
Mother's education below/at median	-0.0082	[-0.0101]	(0.0229)	0.8678
Mother's education above median'	0.0254	[0.0302]	(0.0184)	0.9046

Notes: Parents of children born within 90 days on either side of the reform implementation date (January 1, 2002), where mothers were eligible for parental leave benefits and both parents can be identified in the registers. Column 1 reports the estimated reform effect from a separate local-linear regression with a triangular kernel using the outcome indicated in panel heading and for the subgroup listed in the row. The in-sample median education for mothers is 14 years. Column 2 reports the corresponding bias-corrected estimate, column 3 the robust standard error, column 4 the mean of the variable in the row calculated among (parents of) children born before the reform implementation date. Stars denote statistical significance (* p<0.1, ** p<0.05, *** p<0.01) based on robust confidence intervals centered on the bias-corrected estimates (Calonico et al., 2019, 2014).

Parental Leave and Intimate Partner Violence

Online Appendix

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Mette Ejrnæs
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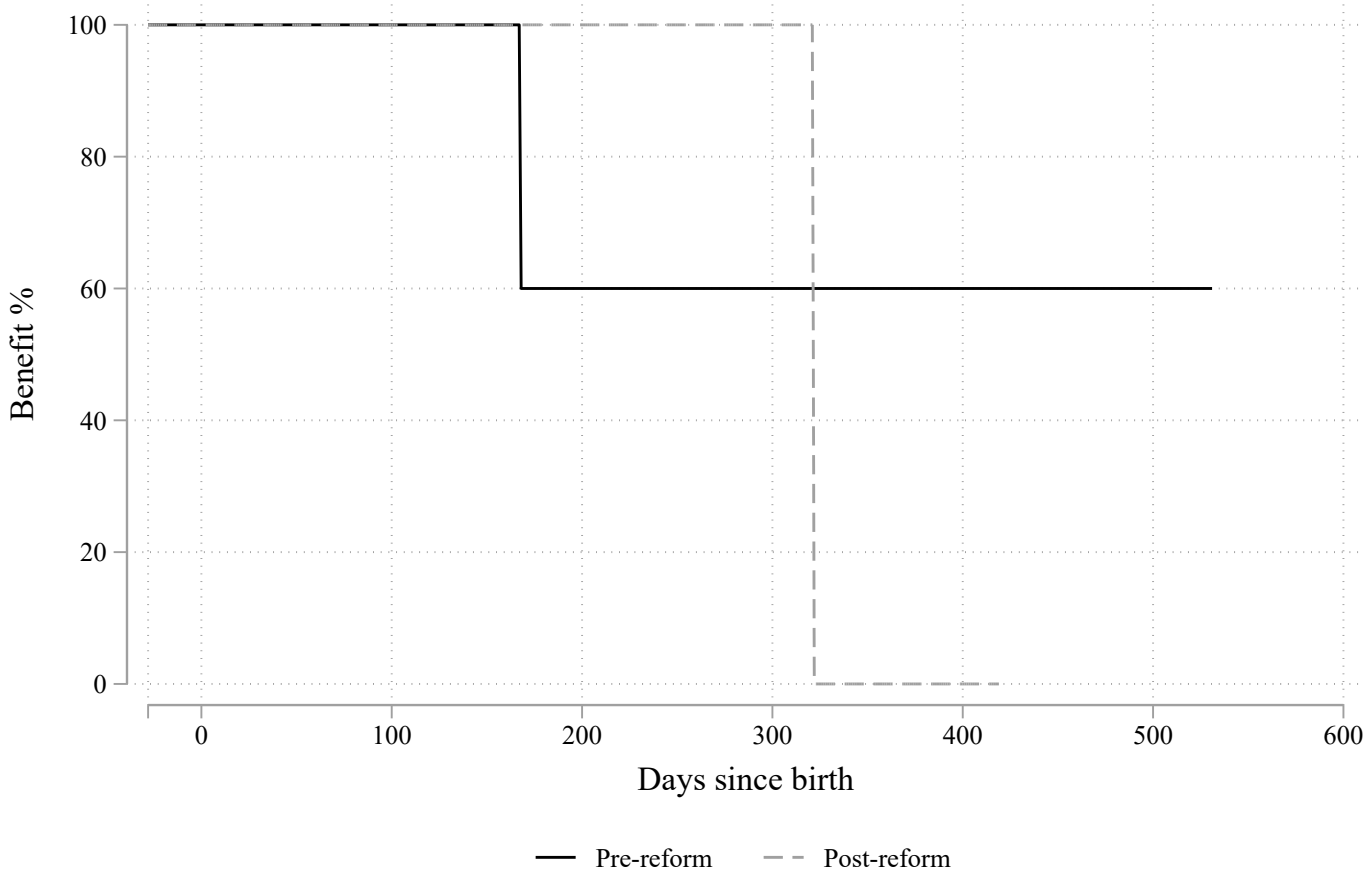
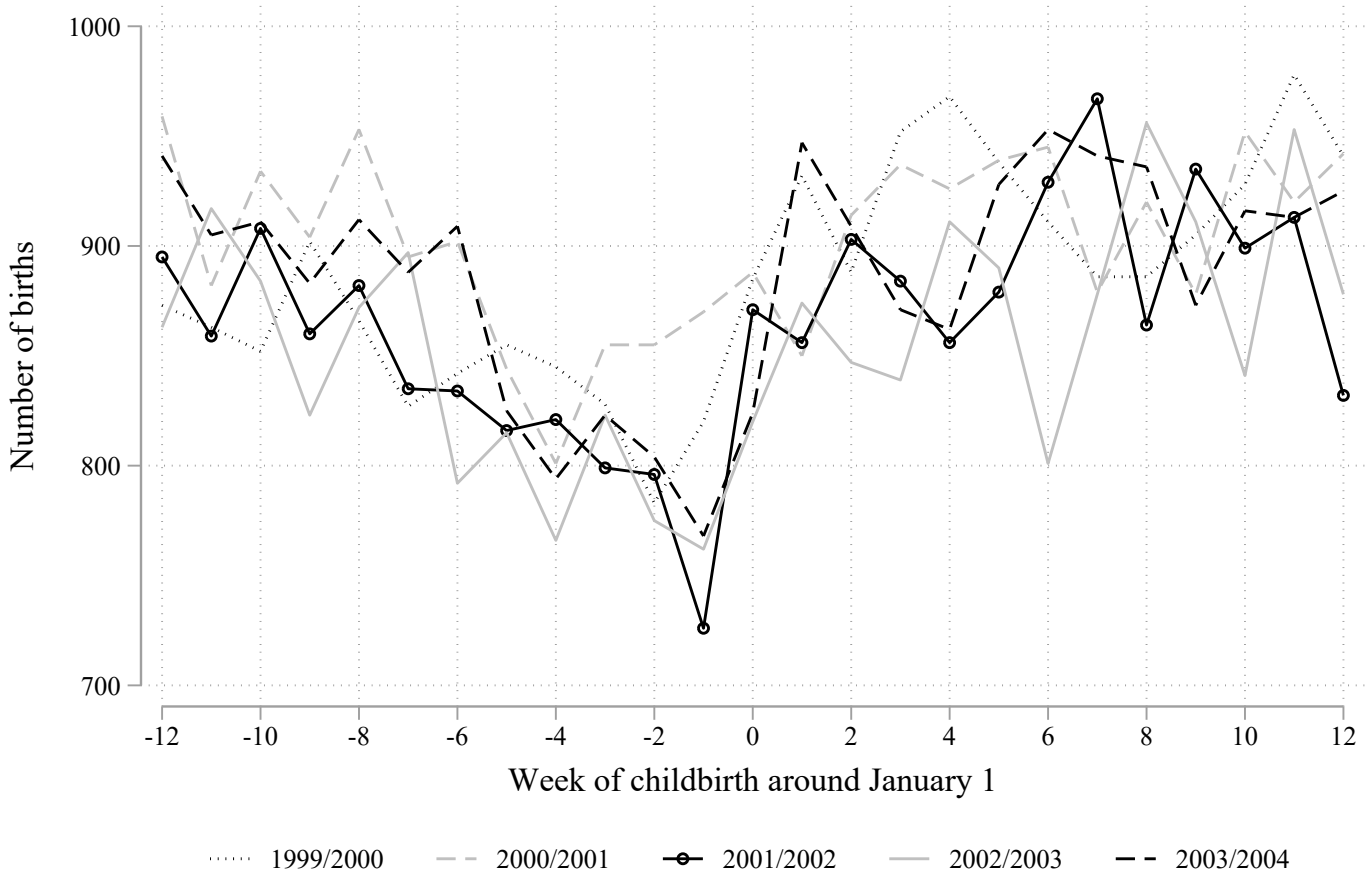
Appendix Figure 1: Mothers' leave benefit compensation



Notes: The graph presents mothers' leave benefit compensation under the pre- and post-reform policies in Denmark, 2001–2002

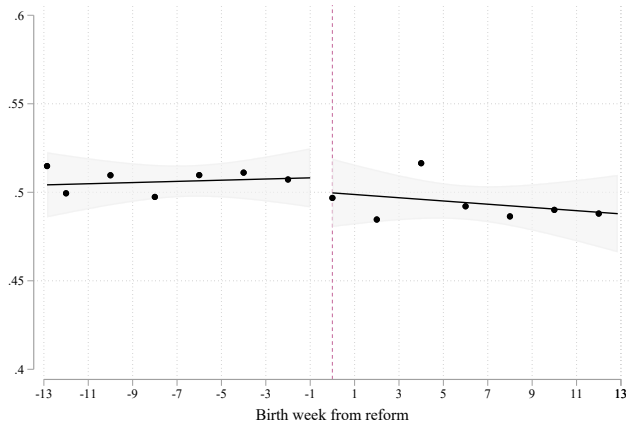
Appendix Figure 2: Frequency of births around January 1



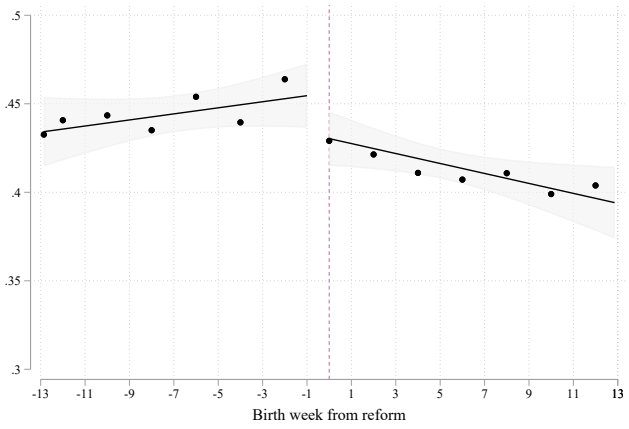
Notes: Sample includes parents of children born within 90 days on each side of January 1 during 2000–2004, where mothers were eligible for parental leave benefits and both parents can be identified in the registers. The figure shows the birth density in one-week bins centered on the turn of the year.

Appendix Figure 3: Evolution of observable characteristics around the reform date

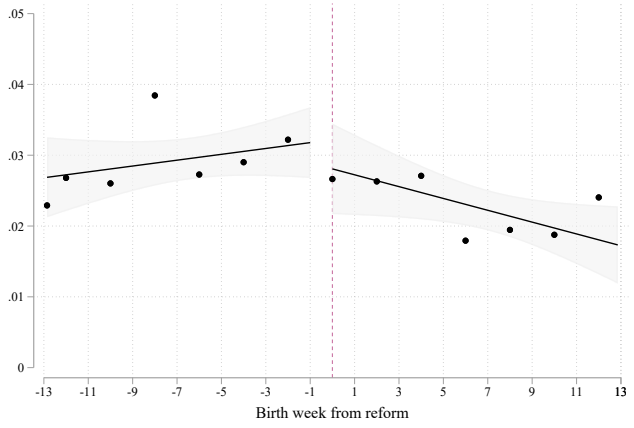
Child characteristics



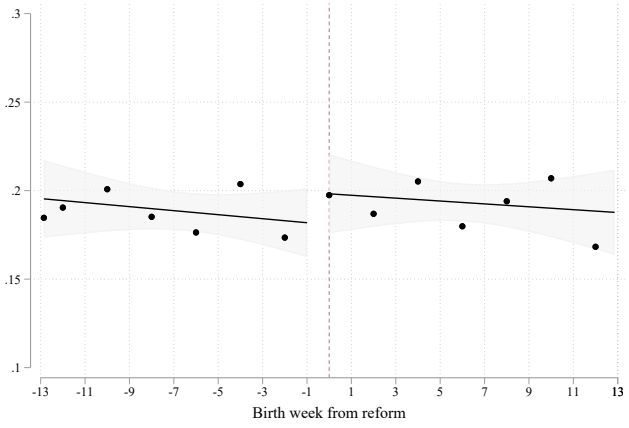
(a) Girl



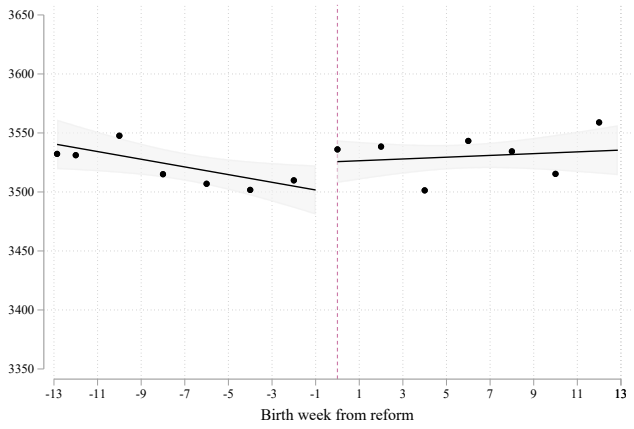
(b) First birth



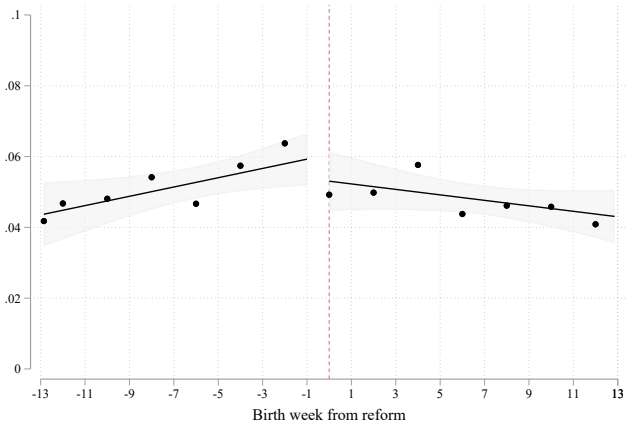
(c) Multiple birth



(d) Cesarean section



(e) Birth weight

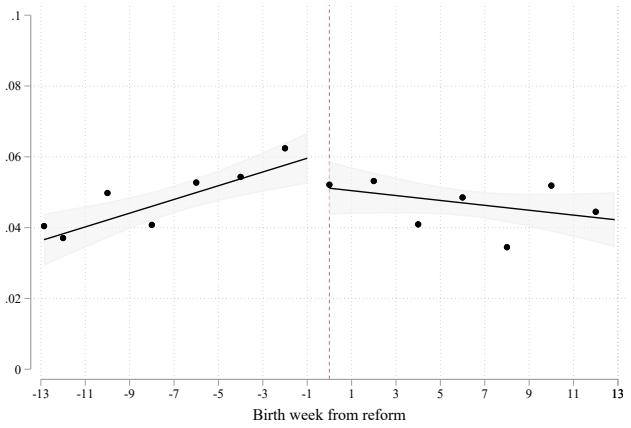


(f) Low birth weight

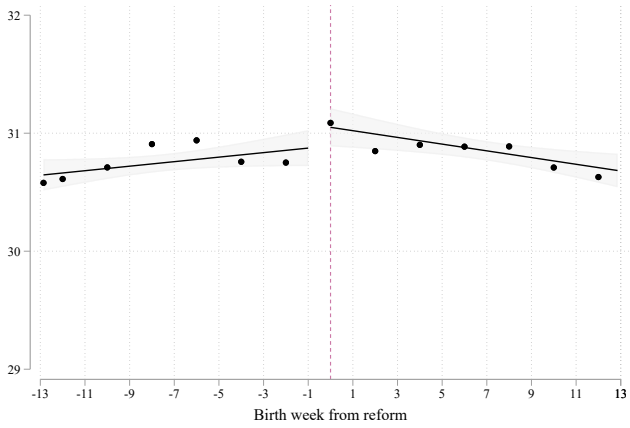
Notes: Families of children born within 90 days on either side of the reform implementation date (January 1, 2002), where mothers were eligible for parental leave benefits and both parents can be identified in the registers. Each marker represents the average of the variable indicated in the panel for 2-week bins around the reform implementation date. The lines plot a linear trend estimated separately on each side of the reform implementation date.

Appendix Figure 3 (cont.)

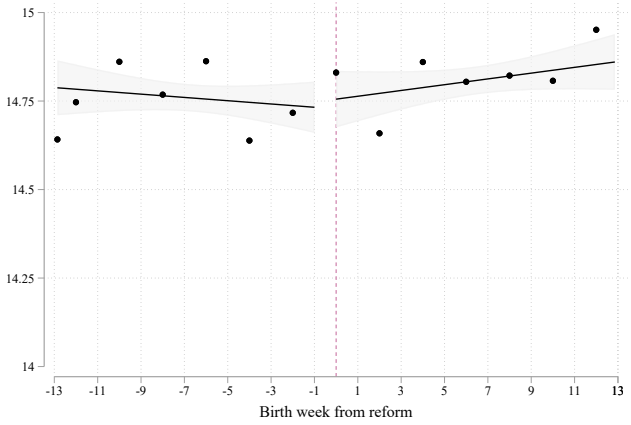
Mother's characteristics



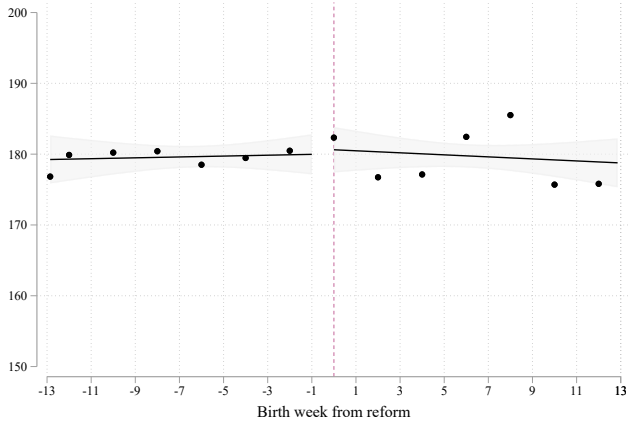
(g) Born abroad



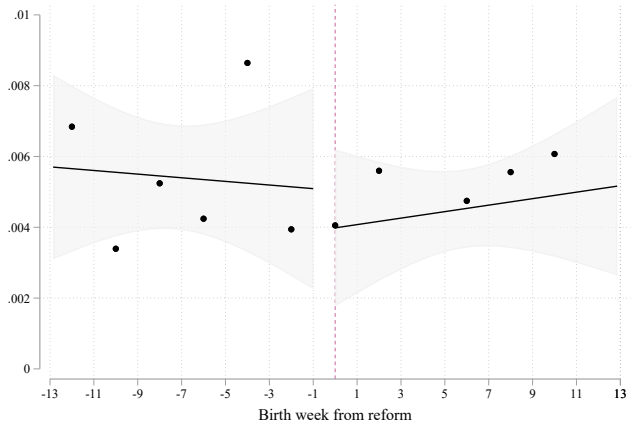
(h) Age



(i) Years of education



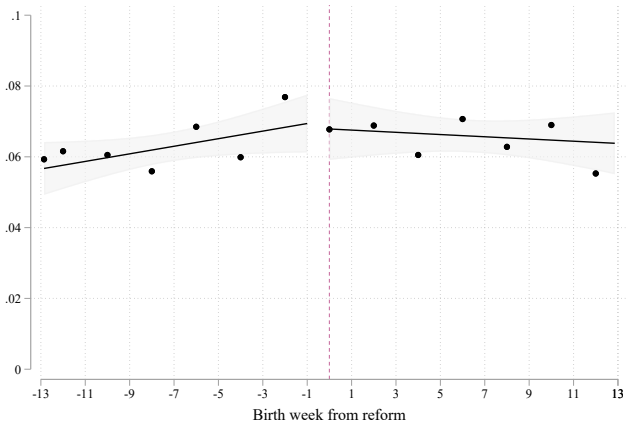
(j) Pre-birth labor income



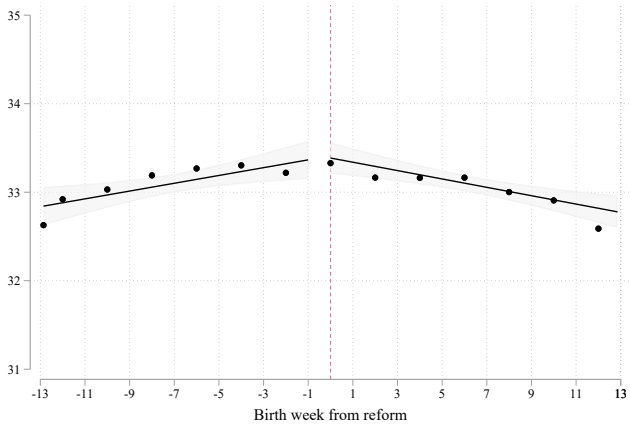
(k) Prior incidence of IPV

Appendix Figure 3 (cont.)

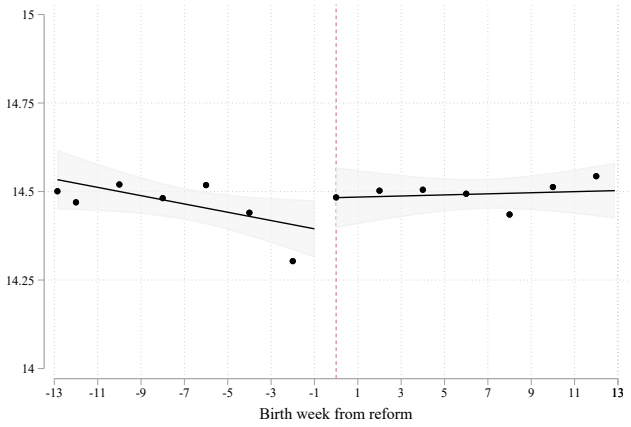
Father's characteristics



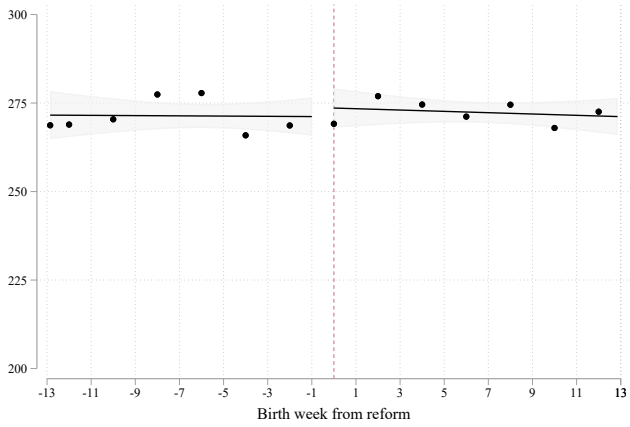
(l) Born abroad



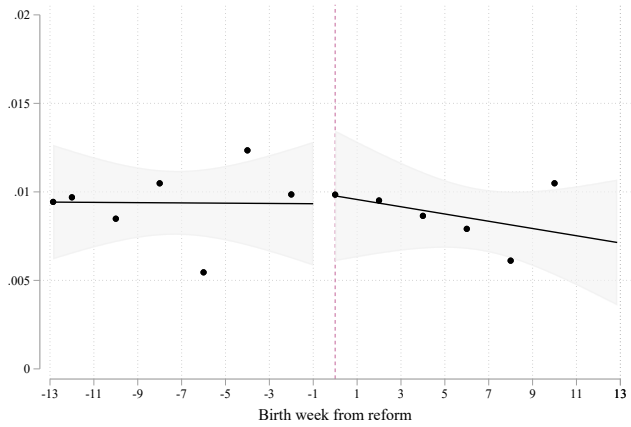
(m) Age



(n) Years of education



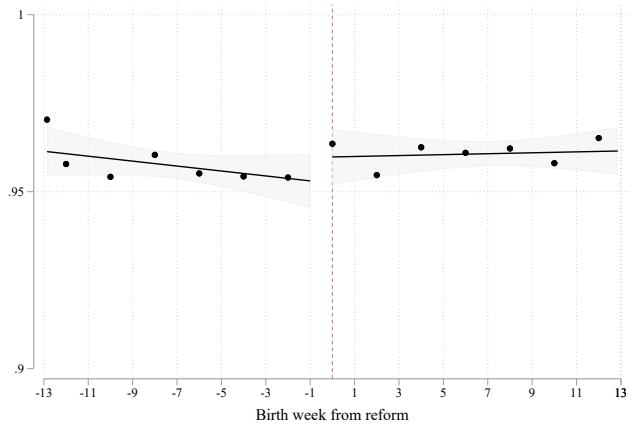
(o) Pre-birth labor income



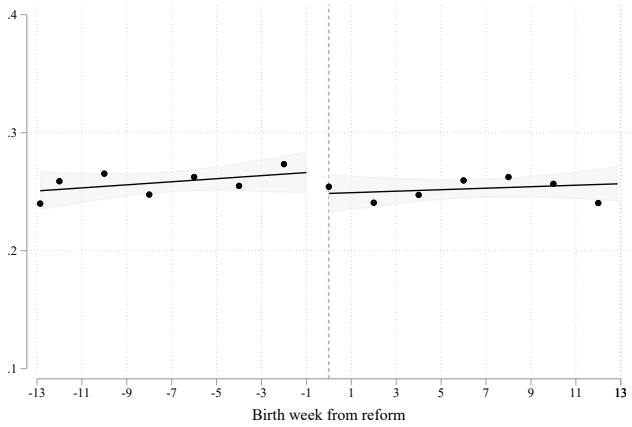
(p) Prior arrest for violent offense

Appendix Figure 3 (cont.)

Household characteristics

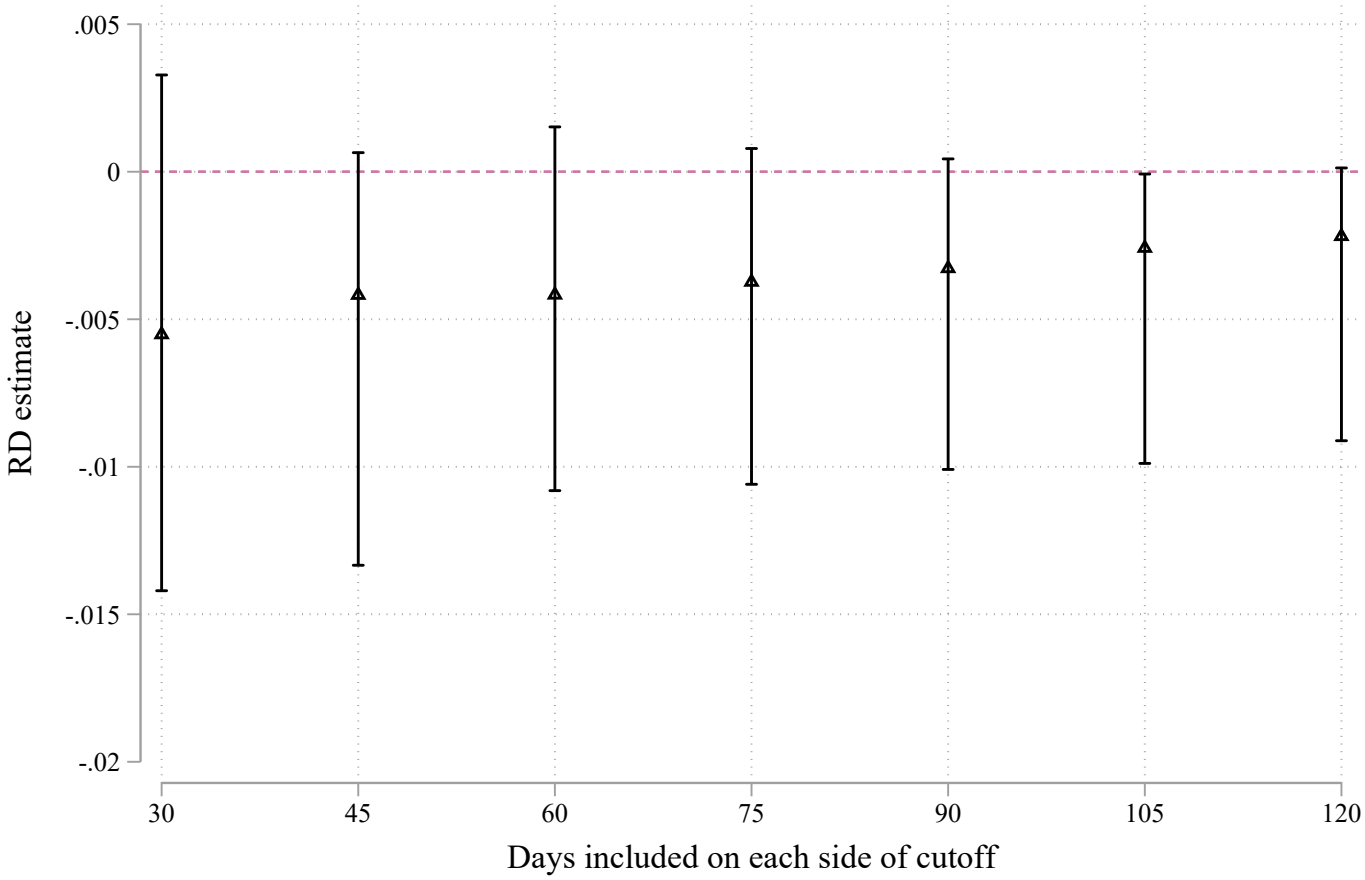


(q) Married/cohabiting at birth



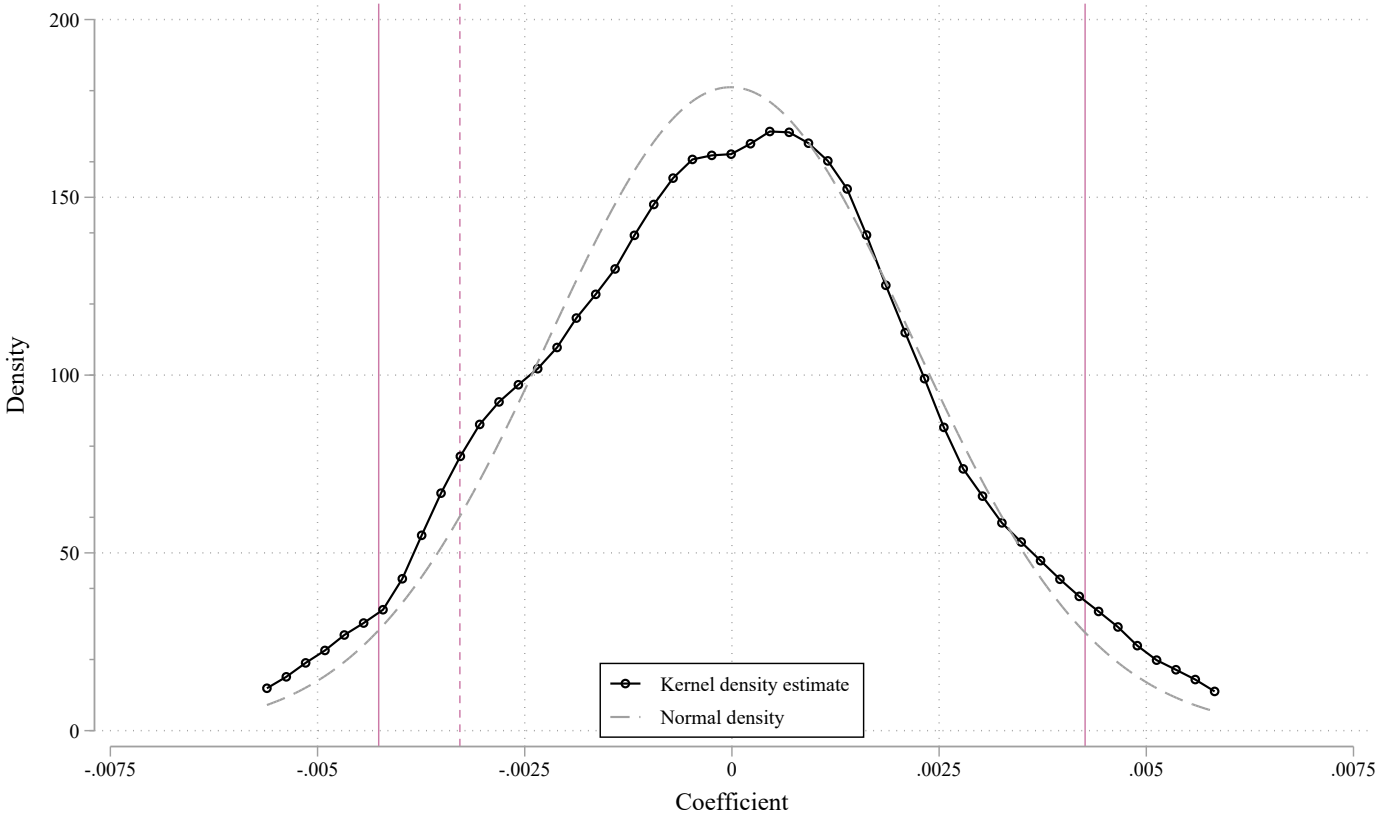
(r) Mother is the primary earner (pre-birth)

Appendix Figure 4: Sensitivity to choice of bandwidth



Notes: Parents of children born within up to 120 days on either side of the reform implementation date (January 1, 2002), where mothers were eligible for parental leave benefits and both parents can be identified in the registers. Each point plots the estimated RD effect of the parental leave reform on IPV for a range of selected bandwidths around the cutoff. Vertical lines represent 95% confidence intervals based on robust standard errors and centered on the bias-corrected estimate.

Appendix Figure 5: Placebo reform dates



Notes: The sample includes all mothers of children born between July 1, 1999, and June 30, 2004, who were eligible for parental leave benefits and where both parents could be identified in the registers. The figure shows the distribution of placebo estimates of the reform effect, obtained by shifting the policy cutoff one month at a time starting from October 1, 1999, to April 1, 2004. Cutoffs that would generate non-constant exposure to the actual reform (i.e., dates too close to the true reform cutoff) are excluded. This results in 47 estimates, that are obtained using local linear regression with a triangular kernel and separate linear trends on both sides of the cutoff. The dashed line marks the estimated reform effect at the true cutoff, while the solid lines indicate the 95% range of placebo estimates (±1.96 times the standard deviation).

Appendix Table 1: Data Sources

Variable	Definition	Years available	Register
A. Outcomes			
Parental leave	Total days receiving parental leave benefits after the birth of the focal child, measured until the next birth or 24 months post-birth (whichever occurs first). (DREAM codes 881, 412.)	1991–2021	Danish Register for Evaluation of Marginalization (DREAM)
Violence-related ER contact	Indicator for any emergency room contact where the recorded cause is violence (`c_kontaars = 3`). Measured both as yearly incidence (year 1, year 2) and cumulative incidence within the first four years.	1995–2018	National Patient Register (LPR)
Police-reported IPV	Indicator for being the victim of at least one police-reported violent crime where the identified perpetrator is the father of the focal child.	2001–2021	Criminal registers of victims and charges (KROF, KRSI)
Earnings	Annual labor income (wages and self-employment surplus) in thousands of DKK. Measured for the first year after childbirth and cumulatively for years 1–3.	1980–2021	Income Register (IND)
Subsequent fertility	Indicator for having a subsequent birth; number of subsequent births; and birth spacing (conditional on a subsequent birth).	1997–2021	Medical Birth Register (MFR)
Relationship outcomes	Indicator for separation from the focal child's father (conditional on cohabiting with the father at birth). Indicator for cohabitation with any partner (unconditional on being the father).	1986–2021	Population Register (BEF)
B. Control variables			
Child and birth characteristics	Indicators for gender, birth order, multiple birth, cesarean section, and low birth weight ($\leq$ 2,500g). For less than 1 percent we do not observe the birth weight. For these cases we import the cohort-specific full population mean.	1997–2021	Medical Birth Register (MFR)
Parents' demographics	Indicators for immigrant status, age at childbirth, years of education, and average annual labor income in the three years prior to childbirth. For less than 1 percent of mothers and fathers we do not observe the educational level before birth. For these cases we impute education as the gender-specific full population median.	1986–2021	Population Register (BEF); Education Register (UDDA); Income Register (IND)
Prior violent crime	Indicator for at least one official charge for a violent crime within four years prior to the birth of the focal child.	1980–2021	Criminal register of charges (KRSI)
Prior violence-related ER contact	Indicator for any violence-related emergency room contact (`c_kontaars = 3`) in the four years prior to the birth of the focal child.	1991–2018	National Patient Register (LPR)

Appendix Table 2: Summary statistics

	Mean	SD
A. Child characteristics		
Girl	0.4990	0.5000
First parity	0.4281	0.4948
Multiple birth	0.0262	0.1598
Cesarean section	0.1906	0.3928
Birth weight	3525.4	617.3
Low birth weight (<2500 g.)	0.0498	0.2176
B. Mother's characteristics		
Born abroad	0.0478	0.2132
Age at birth	30.822	4.388
Education (years)	14.7827	2.2136
Pre-birth labor income	179.7	93.9
Prior violence-related ER contact	0.0050	0.0704
C. Father's characteristics		
Not born in DK	0.0647	0.2460
Age at birth	33.100	5.291
Education (years)	14.4767	2.3663
Pre-birth labor income	271.8	165.2
Prior charge of violent offense	0.0090	0.0942
D. Household characteristics		
Married/cohabiting at birth	0.9586	0.1992
Mother is the primary earner (pre-birth)	0.2558	0.4363
N	22,114	

Notes: Means and standard deviations of parental characteristics for children born within 90 days of the reform implementation date (January 1, 2002), restricted to families where both parents can be identified in the registers and where mothers are eligible for parental leave benefits. Labor income (in 1,000s of 2015 DKK), prior IPV, and prior perpetration are measured during the second to fourth years before the child's birth.