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ABSTRACT

Motherhood and Domestic Violence: A Longitudinal Study Using Population-Wide Administrative Data*

Most empirical studies indicate that becoming a mother is an augmenting factor for the perpetration of intimate partner violence (IPV). Using rich population-wide hospital records data from Sweden, we conduct a stacked DiD analysis comparing the paths of women two years before and after the birth of their first child with same-age women who are several quarters older when giving birth to their first child and find that, in contrast to the consensus view, violence sharply decreases with pregnancy and motherhood. This decline has both a short-term and longer-term component, with the temporary decline in IPV covering most of the pregnancy until the child is 6 months old, mimicking a temporary decrease in hospital visits for alcohol abuse by the children's fathers. The more persistent decline is driven by women who leave the relationship after the birth of the child. Our evidence is not supportive of alternative mechanisms including suspicious hospitalizations, an overall reduction in hospital visits or selection in seeking medical care, mothers' added value as the main nurturer, or mothers' drop in relative earnings within the household. Our findings suggest the need to push for public health awareness campaigns underscoring the risk of victimization associated with substance abuse and to also provide women with more support to identify and leave a violent relationship.

JEL Classification: J12, J13

Keywords: motherhood, stacked difference-in-differences model, event study, individual fixed effects, administrative longitudinal records data, population-wide estimates

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I. Introduction

The United Nations states that "Violence against women and girls is one of the most pervasive human rights violations globally (António Guterres, UN 2020). Indeed, one in three women in EU-28 countries has experienced physical and/or sexual violence since age 15, with an estimated three quarters of all violence against women perpetrated by (ex-)domestic partners (FRA 2015). The consequences of violence against women are devastating for women's health (WHO 2013), productivity and employment (Adams et al. 2013; Browne et al. 1999; Lloyd and Taluc 1999), as well as the health and development of their children (Aizer 2011; Anderberg and Moroni 2020; WHO 2002). Furthermore, the economic costs of violence against women across the EU-27 are estimated to amount to €290 billion a year, with intimate partner violence (IPV) making up 52% (€152 billion) of this sum (European Institute for Gender Equality 2021).

In this paper, we ask whether having children is a protective or risk factor for the incidence and severity of IPV. While violence during pregnancy may simply be a continuation of pre-existing IPV, the increased stress of transitioning into parenthood may also trigger or increase IPV. This is especially problematic because pregnancy may increase women's emotional and/or economic dependence on their partners, making them more vulnerable within the relationship. From a sociological perspective, motherhood is often used as a tool of control and manipulation by the abuser, where the partner may exert control over the mother by threatening to harm her or take away the children. At the same time, a woman may feel trapped in an abusive relationship, reluctant to leave because she fears losing custody of the children or not being able to financially support them. Even when women leave the relationship, custody discussions and exchanging children may lead to post-separation violence (Brownridge 2006). From evolutionary theory, motherhood also plays a prominent role in IPV because men may use physical and emotional abuse to control their partners and ensure access to their offspring. Last but not least, an increase in partners' time together when the mother is more present at home during postpartum childcare may create more opportunity for abuse.

On the other hand, IPV may conceivably *decrease* with motherhood, as women become more valuable within the relationship as they bear and nurture the child. Alternatively, violence against mothers may also decrease as maternal leave and potential subsequent withdrawals from the labor force lower women's economic independence (male backlash theory) and/or exposure to other men (evolutionary theory), thus reducing their partners' stress and anxiety. Motherhood may also lead to a reduction in IPV if the arrival of the child decreases women's willingness to accept being victimized and gives her the strength to leave an abusive relationship. At the same time, as parenthood may increase both partners responsibilities towards their child, they may engage less in destructive behaviors and, hence, curb engagement in substance abuse, which is directly related with violence. Finally, frequent health-care visits during antenatal, perinatal, and toddler care offers an ideal setting for healthcare providers to identify women experiencing violence and address issues of abuse.

Understanding whether IPV increases or decreases with motherhood as well as the reasons driving this change is a top priority and crucial for designing public health policies that eradicate domestic violence. For example, if we find that it is the stress of transitioning into parenthood that triggers or increases IPV, policy makers ought to offer more resources and emotional support with prenatal care. If, instead, an increase in IPV is driven by men's need to control and manipulate their partners when children arrive, efforts ought to be directed at challenging societies' hegemonic masculine norms through public health awareness campaigns and by propagating alternative forms of masculinity by incorporating them in teaching curricula as early as in primary education. Similar norm-evolving policies should be implemented if IPV *decreases* with motherhood because of a reduction in men's stress and anxiety when their partners are complying with traditional gender norms by becoming home makers and primary caregivers. Alternatively, if the decrease in IPV is driven by women's higher and more frequent access to health-care providers, the policy implication would be to offer this type of services more frequently and broadly to all women. Meanwhile, a decrease in IPV driven by fathers' curbing of alcohol abuse would trump the need to air public health awareness campaigns, underscoring the risk of victimization and perpetration with substance abuse consumption. Finally, a decrease driven by women leaving violent partners after birth would prompt both public campaigns on recognizing early signs of violence and institutional mechanisms supporting the separation of toxic relationships.

Most empirical studies indicate that having children is an augmenting factor for IPV in women (Brownridge 2006; Geffner et al. 2000; Graham-Bermann & Edleson 2001). Small-sample hospital and clinical evidence finds that IPV prevalence increases during pregnancy (Jasinski 2004) and after the birth of a child (Vatnar & Bjørkly 2010). However, most of the available evidence uses small samples of women recruited from hospitals, police stations, family counselling, or shelters (Graham-Bermann & Edleson 2001), and hence composed of mostly women who have been victimized or with high risk of victimization. By contrast, nationally representative studies find that the risk of victimization is lower during pregnancy (Gelles 1998; Jasinski, Kantor 2001; Currie *et al.* 2022). Nonetheless, descriptive analysis of the prevalence of victimization around birth continues to show that violence increases after birth during the first two years of the child (Bowen *et al*. 2005; Charles and Perreira 2007),[1] and quantitative studies by Cesur *et al.* (2023)

[1] Even though both studies observe women's victimization during pregnancy up to the first couple of years after birth, their analysis is descriptive, focusing on the prevalence and correlates of inter-personal violence in the UK (Bowen *et al*. 2005) and the US (Charles and Perreira 2007).

and González and Rodríguez-Planas (2020) find that having children is associated with higher risk of victimization.[2]

To date, the empirical evidence on IPV and motherhood has been mostly descriptive and has not attempted to estimate the causal impact of motherhood on IPV. Most recently, however, quantitative studies by Massenkoff & Rose (2023) and Britto, Rocha, Pinotti & Sampaio (2024) using a stacked Difference-in-Differences design and longitudinal crime data for Washington State and Brazil, respectively, both find an increase in fathers' arrests for domestic violence after the conception of the first child, but especially after childbirth. Massenkoff & Rose (2023) show that the spike in fathers' arrests for fourth degree assaults, which is the least severe assault charge, is mostly driven by fathers who divorce five years later. Britto *et al.* (2024) suggest that the increase in domestic violence prosecution is driven by increases in both actual violence[3] and a higher propensity for women to report any incidents to the police. Both papers focus on understanding more generally the dynamics of parental criminal activity around the time of the conception and birth of their first child.

In the current paper, we analyze the dynamic effects of childbirth on domestic violence by using a population-wide longitudinal dataset covering the medical history of the entire population of Sweden from 2001-2016. For our main analysis, we follow Bergvall (2022) and use a broad measure for injuries caused by assaults; namely, hospital visits, which includes over-night stays. However, we also present results using alternative measures, including hospital visits for injuries caused by IPV (our concern here is that this may be under-reported due to fear of retaliation) and over-night stays for injuries caused by assault, which captures the most severe cases and was the measure used by Aizer (2010). To address potential concerns with selective care-seeking or misreporting at the hospital, we take advantage of detailed administrative data on different types of, and reasons for, hospital visits to conduct a battery of sensitivity tests. Because we observe the entire medical history of the Swedish population, our data also contains birth information, precluding attrition due to merging different datasets in crime data studies. In addition to medical records, we also have both partners' employment and earnings records, highest achieved degree, medical records on alcohol abuse, and couples' relationship status of either marriage, divorce, cohabitation or separation.

To address the selection issues related to the fact that mothers and childless women may differ in preferences, risk taking, and lifestyles, we follow Massenkoff & Rose (*forthcoming*), Meletyeva & Riedel

[2] Cesur et al. (2023) and González and Rodríguez-Planas (2020) use quantitative methods to causally identify the effect of the OBRA-93 expansion of the earned income tax credit on the risk of IPV of women with children relative to those without (the former) and the role of gender norms on the risk of victimization across 28 EU countries (the latter). Hence, they do not estimate the effect of motherhood on IPV, though their specifications control for having children and show that the children covariate is positively associated with a higher risk.

[3] Britto *et al.* (2024) use SINAN data, based on mandatory notifications of domestic violence (SINAN) filed by Brazilian health units whenever they know or suspect that a patient has been a victim of domestic violence to proxy the probability of women being victims of aggression.

(2023), and Britto *et al.* (2024) by estimating a stacked Difference-in-Differences model with quarterly data based on mothers' age-at-birth, comparing mothers who have their first child to women who have their first child within the next eight quarters. Our results are nevertheless robust to using alternative methodological approaches.

Consistent with the descriptive literature, the raw data displays an upward trend in hospital visit rates for our more restrictive measure of IPV (where the perpetrator of the assault is specifically recorded as an intimate partner) that begins several years before birth and continues as the child grows. Yet, once we account for mother's age at first child's birth with a stacked difference-in-differences approach, the event study analysis no longer shows any statistically significant differences between mothers' hospital visits for IPV and those of mothers who have children when they are between one 4 and 12 quarters older. At the same time and consistent with the recent crime-data quantitative studies by Massenkoff & Rose (2023) and Britto *et al.* (2024), with this measure of IPV and focusing on mothers who separate from their partner within 2 year of the birth of their first child, our stacked DID estimates show an increase in violence after birth with a peak at 9 months, consistent with evidence from Britto *et al.* (2024) that the increase in violence is likely driven by a higher propensity for women to denounce in order to improve their bargaining power at separation.

Most importantly, by using a broader measure of domestic violence than that reflected by domestic abuse calls, arrests, or overnight stays for assault, our study uncovers a singular and novel result: the probability that a woman visits a hospital with injuries caused by assault sharply declines with pregnancy, further declines with birth, and this lower level persists for the two years after the birth of the first child. With pregnancy, mothers' hospital visits for assault fall by 33% relative to the rates of later-treated mothers. This gap widens during pregnancy, reaching its largest level at birth, with a relative decline of 59%. During the first year of the child, mothers' hospital visits for assault continue to be between 48% and 26% lower than those of later-treated mothers. All these estimates are statistically significant at the 5% level. Once the child turns two years old, the gap remains negative, albeit only marginally statistically significant at the 10% level. These results are robust to a battery of sensitivity analyses, including alternative methodological approaches, alternative measures of victimization, and falsification tests.

Furthermore, we are able to identify the mechanisms behind this new stylized fact, showing that this decline has both a short-term and a longer-term component. The temporary decline covers most of the pregnancy and until the child is 6 months old. This decline comes hand in hand with a similarly-timed decline in hospital visits for alcohol abuse by the child's father. In contrast, the persistent decline is driven by women who leave the relationship within 2 years of the birth of the child, presumably leaving a toxic relationship. While we explore many alternative and/or complementary mechanisms, the evidence is *not* supportive of any of the following channels: suspicious hospitalization, an overall reduction in hospital visits, mothers' added value as main nurturer, or mothers' drop in relative earnings within the household.

To the best of our knowledge, this is the first study to analyze the dynamics of victimization around motherhood using the entire population of a European country.[4] Most importantly, the study uncovers a new stylized fact, showing not only that motherhood does *not* increase domestic violence, but *reduces* it, most likely because fathers curb their alcohol abuse during pregnancy and around the birth of the child. While this reduction in violence does not persist, the drop in hospital visits for assault persists for women who leave their partners within two years of the birth of the child, and hence were presumably in a toxic relationship. These findings suggest the need to push for public health awareness campaigns underscoring the risk of victimization associated with substance abuse and to also provide women with more support to identify and leave a violent relationship.

II. Administrative Hospital Records Data and Measuring Domestic Violence

In this study, we use data from the Swedish Interdisciplinary Panel (SIP), an annual multigenerational dataset covering all individuals born in Sweden between 1930 and 1985, as well as their children. We observe individuals from 2001 to 2016 and have access to detailed information on both partners' employment history including earnings, marital status, co-residence status[5], demographics and hospital records. The latter informs us on the history of both in-patient and out-patient visits[6], including emergency-room visits.

We focus on Swedish women born between 1965 and 1990 who gave birth to their first child between 2003 and 2016, and were between 18 to 40 years old at the time of their first birth (19 to 43 for the control group). This amounts to 543,593 women who, on average, were 29 years old when they had their first birth (shown in column 1, Table 1). Most of these women (89 percent) lived with a partner at birth, with a third (33 percent) married and the rest (56 percent) cohabitating. Close to two fifths of the mothers had a university degree before their first birth (42%), most of them were employed (83%) and their yearly income averaged 306,406 Swedish Krona (about $28,800 US dollars) that originated mostly from labor income.

[4] This adds to the contributions of Massenkoff & Rose (2023), which is the largest such study ever conducted in the United States on men's and women's criminal behavior around the birth of their first child, and Britto *el al.* (2024), which uses the universe of criminal cases in Brazil for the period 2009-2020.

[5] Co-residence status can only be observed for couples who have common children.

[6] In-patient records include hospital visits where the patient stays over-night, whereas out-patient records include any contacts with specialized care providers, excluding primary-care physicians or dentists.

Our main violence measure is a binary indicator of whether a woman visited a hospital due to an injury caused by assault (physical or sexual) during a given quarter. During the visit, medical personnel report the external cause of a woman's injury using ICD-10 diagnosis code, which allows us to identify the assault cases. Our IPV measure is constructed using codes X85 through Y09, which cover injuries caused by physical or sexual assault. Moreover, codes Y06 and Y07 explicitly identify whether the perpetrator is the partner or someone else, and we use this more restrictive measure of IPV as a secondary outcome. For the other codes identifying assault, we follow Bergvall (2022) and code them as of a domestic nature if they took place at home or in an unspecified location. As the code of unspecified location is used by medical personnel in emergency rooms when the situation is more stressful than average and does not allow for much questioning,[7] the perpetrator is most frequently someone close to the victim (Goodwin & Breen 1990; Frenzel 2014), so measurement error in coding this as domestic violence is unlikely, especially among pregnant women and mothers of young children. Our results are robust to a measure of the intensity of violence; specifically, the sum of the number of hospital visits due to an injury caused by assault during the last quarter.[8]

Because our measure of domestic violence mostly captures cases of violence with induced injuries that led to either in-patient or out-patient care, it is considerably broader than Aizer's (2010) measure based on hospital overnight stays for assault. It is also less prone to concerns about under-reporting due to fear of retaliation than a measure based solely on hospital visits for IPV. Panel A in Figure 1 plots the prevalence of these three measures and documents that, prior to childbirth, our measure is considerably more comprehensive than the other two measures. In the quarter prior to pregnancy, the prevalence of hospital visits due to an injury caused by an assault that took place at home or in an unspecified location averaged 0.312 visits per 1,000 for the focal mothers. In contrast, the respective rates for the other measures were 0.035 for overnight hospitalization and 0.041 for IPV hospitalization (see Table 1 for more descriptive statistics).

Even though our measure is broader than hospital visits for IPV and overnight stays for assault, it continues to capture domestic violence that requires a visit to the hospital, and hence, represents severe physical harm. In comparison, a national survey of 20,000 women conducted by the Swedish Agency for Crime Prevention found that 2.2% of the surveyed women had experienced physical violence by an intimate partner in 2012 (Frenzel 2014) but only 7.9% of these women had contact with the police following the

[7] In our sample, 67% of the hospital visits for assault are coded as occurring in an unspecified location.
[8] Multiple incidents of hospital visits due to injuries caused by assault are rare. Mother's pre-birth mean is 1.213 with a standard deviation of 59.347. While the maximum number of hospital visits for assault per year is 34, 77% of the sample with hospital visits for assault experience only one per year..

assault (about half of whom filed a police report) and only 12% visited a hospital for their injuries. However, of the women who experienced aggravated assault, the rate of hospital visitation was substantially higher, at 29% (Frenzel 2014). Thus, the numbers support the notion that measuring assault using hospital visits captures only the tip of the iceberg of the true violence against women, focusing primarily on more severe aggravated assaults.

Another advantage of our measure is that it is less susceptible to reporting bias than self-reported violence from surveys which is influenced by the ability of the women to identify gender-based violence (Ellsberg *et al.* 2001). Our measure is also a broader measure of domestic violence than that reflected by police-level administrative records on complaints about psychological, physical, and sexual abuse, which capture only those cases where the woman (or her neighbors) are willing to denounce.[9] Furthermore, our measure of domestic violence is more objective than the subjective notes of social workers who observe only high-risk populations or of general practitioners who are more prone to report certain subgroups in doctor visits. Crucially, our measure has two important advantages over other data sources in that it covers the whole population of Sweden and is longitudinal, as we observe individuals over a fifteen-year period.

Nonetheless, we conduct a series of robustness checks to ensure that our results are not driven by selection to seek medical care or untruthfully reporting or misreporting the causes of ones' injuries that led to seeking medical care. Note that any selection to seek medical care or misreporting of the causes of ones' injuries would only bias our results if they differed between the mothers and mothers-to-be in our comparison groups *and* that difference shifted with the arrival of the child. Concerns in other jurisdictions about limited access due to costs of health insurance are limited in our study, as Sweden has a health-care system that is quite close to universal health care. Risk of selective misreporting is addressed by re-estimating the specifications using data on hospital visits for injuries similar to those caused by assault but where the reported cause is said to be an accident as well as hospital visits for bone fractures. We also present estimates using hospital overnight stays for assault (Aizer's measure) and hospital visits for assault-related injuries that cause bone fractures or with wounds in need of stitches, as these are the most severe cases whereby it may not be possible to avoid medical assistance. To address concerns that mothers or mothers-to-be under-report the partner or ex-partner as the perpetrator, we re-estimate the specifications using data on all hospital visits for assault, regardless of who was the perpetrator or whether the perpetrator was identified on the hospital records or known by the victim. Finally, we conduct three falsification tests

[9] Anderberg, Rainer & Siuda (2022) find that police-recorded domestic violence incidents cannot reliably inform us about the scale of the domestic violence problem, especially during crises like COVID-19. Furthermore, using similar data sources as our study, Karimi *et al.* (2023) find that only 20% of the women who visit a hospital for assault-related injuries file a police report.

to ensure the findings are not driven by systematic differences between mothers and mothers-to-be: (1) any hospital visit (excluding for assault or directly related to childbirth); (2) hospital visits for benign tumors; and (3) hospital visits for bus accidents.

III. Raw Data and Empirical Approach

Raw data. Panel A in Figure 1 shows mothers' quarterly rates of hospital visits for different types of assaults. The next two panels zoom in for two of these rates: hospital visits for IPV and overnight stays for assault. The relevant time dimension is the age of the mother at the time of her first birth. The dashed vertical line indicates the beginning of the pregnancy ($t = -3$) and the vertical solid line indicates the birth of the first child ($t = 0$).[10] As discussed in the previous section, the frequency is considerably higher for hospital visits for assaults than for hospital visits for only IPV or overnight hospitalizations for assault. This is expected, as the former includes most emergency room visits in which there is no time to inquire and/or record the cause of the injury. Furthermore, overnight hospitalizations for assault displays only the most severe assaults requiring an overnight stay, while hospital visits for IPV are only those where the victim specifically identifies her partner or ex-partner as the perpetrator.

Interestingly, the raw data reveals a divergent pattern between women's hospital visits for IPV and the other two categories. Consistent with most of the literature documenting an increase in IPV with motherhood, Panel B displays an upward trend in hospital visit rates for IPV that begins several years before birth and continues as the child grows. Furthermore, Panel B shows two sharp peaks: one around the first quarter of pregnancy and the other one around the 9th month after birth. Thereafter, hospitalization rates for IPV stabilize around 0.05 per mille, well above pre-pregnancy levels averaging 0.03 per mille. Our raw data for hospital visits for IPV thus appears to be consistent with the view widely held in the literature.

In contrast, the other measures of violence diverge from this narrative. Women's hospital visits for assault (displayed in Panel A) show a sharp decline starting at the time of the pregnancy, becoming lowest at birth and then bouncing back with the child's first months of life to stabilize around 0.2 per mille, significantly below pre-pregnancy levels (which average 0.31 per mille). In Panel C, overnight hospitalization rates for assault also drop around birth, mimicking the decline observed in hospital visit rates for assault. Yet, in this case, overnight hospitalizations remain low during the child's first 2.5 years to increase shortly thereafter, albeit below pre-pregnancy levels.

[10] As the data do not allow us to observe gestational age of the child at birth, the dashed line is an estimation of the average length of a pregnancy and included for illustration purposes.

Accounting for aging. To account for aging, in Panels A, C and E in Figure 2, the green dashed lines shows hospital visit rates in the three categories for mothers over the 2 years before and after the birth of the child. These are our focal mothers, who are the early-treated individuals. They are compared to the later-treated group of mothers, who serve as the control before they become pregnant (not-yet-treated) and whose hospital visit rates are also displayed in the same panels in Figure 2. For the later-treated groups, their hospital visit rates are plotted at the same ages as the focal mothers in the blue line up until the women in the later-treated groups become pregnant. For example, for mothers who have a child at age 28, the focus-mothers' line would plot their hospital visit rates over ages 26 to 30, while the series for mothers 6 to 8 quarters older would instead plot the hospital visit rates of those who have a first child when 29.5 to 30 years old over the same age range (in this case, 26 to 28.75 or 29.25 years, as they are treated thereafter). Furthermore, for later-treated mothers, the zero on the x-axis corresponds to their hospital visit rates at age 28.

Appendix Figure A.1 illustrates how the baseline risk of a hospital visit for assault differs by a woman's age at the birth of her first child, which depicts a clear negative gradient, at least up until age 30. This underlying relationship between age of mother at childbirth and assault risk makes any comparison of mothers who give birth at very different ages problematic and highlights the importance of comparing mothers to a control group of not-yet-treated women who give birth at a fairly similar age as the focal mother. Comparing mothers to childless women is also problematic, as motherhood is clearly a choice and women who choose (and are able) to become mothers most likely differ from childless women in many ways. Furthermore, an observed childless woman in the data may in fact just be a woman who has a child at a later age if the data stops too early, before her window of fertility has passed.

Cohort-specific pre-trends. Given our identification strategy, it is crucial that we observe parallel trends in hospital visit rates between the early- and later-treated groups prior to the pregnancy of focal mothers. Trends over age closely follow each other in Panel A of Figure 2. While this is also true on average for panel C, the smaller frequency of the event itself creates more choppiness. To the extent that parallel trends are a bit more uncertain in Panel E, in the rest of the paper we present the results for the three categories, but put less weight on the results for overnight hospitalization for assault than the other two categories. Importantly, our analysis is restricted to two years before and after birth because if we were to use older comparison mothers, the parallel trend assumption for these older mothers would be violated. Hence, our analysis covers 8 quarters after birth, implying that our oldest comparison group has children between 2 years and 9 months and 3 years older than our focal mothers. In the robustness section, when presenting estimates comparing mothers to childless women, we present estimates up to 8 years before and after birth.

Differential patterns across treatment status and outcomes. Despite the similarity in trends between focal mothers and later-treated mothers prior to pregnancy, there are distinct changes in hospital visit rates between the two groups starting when our focal mothers become pregnant. For example, Panel C shows that focal mother hospital visits for IPV peak both with pregnancy and about 9 months after the birth of the child, with the trough covering both the birth and the child's first three months of life. After the child's first birthday, focal mother hospital visit rates for IPV decline and converge to that of women who have a child 1 to 3 years later.

In contrast to the peaks in hospital visits for IPV observed for focal mothers in Panel C, Panel A reflects a sharp *decrease* in hospital visit rates for assault for focal mothers with pregnancy and childbirth. Indeed, we observe a U-shape in the hospital visit rates for assault of focal mothers that begins with pregnancy, and the trough coincides with the child's birth. Thereafter, hospital visit rates for assault rebound sharply, but stabilize around the child's first birth at a level considerably lower than that observed among women who have a child 1.5 to 3 years later. Similar to Panel A, Panel E shows a sharp decline at birth and during the child's first quarter of life in the focal mother overnight hospitalization rates for assault. This decrease persists over time, leaving focal mother overnight hospitalization rates well below those of women who have their first child 1.5 to 3 years later even though focal mother overnight hospitalization rates peak during the second and third quarters of pregnancy, similar to the peak observed for hospital visit rates for IPV. There also appears to be a sharp decline about 6 months prior to conception, perhaps reflecting a "honeymoon" period.

In summary, the raw data reveal a distinct pattern based on the type of hospital visit or hospitalization experienced. Both pregnancy and the child's 9-month birthday come with a burst of hospital visit rates for IPV, with convergence of IPV hospital visit rates with those of later-treated mothers only occurring once the child is one year old (shown in Panel C). In contrast, the other two categories of hospital visits or hospitalizations for assault reveal a sharp drop with pregnancy (Panel A) or birth (Panel E), shifting mothers' age-assault-hospitalization rates to levels considerably below those observed among women who have their first child 1 to 3 years later.

Stacked Difference-in-Differences (Stacked DiD). Using the following stacked difference-in-differences estimator, with a rolling window of cohort-specific control groups of later-treated mothers over age at childbirth, we compare the prevalence of hospital visits due to an injury caused by assault of our early-treated mothers with that of later-treated ones over 16 quarters around the birth:

$$y_{iat} = \sum_{j=-8, j\neq -4}^{+8} \beta_j^t * 1(a - a_i^0 = j) * 1(a_i^0 = t) + \delta_{at} + \alpha_{it} + \varepsilon_{iat}, \quad (1)$$

where Y_{iat} is an indicator for hospital visits for assault of individual *i* who belongs to age-at-childbirth cohort *t* at age *a*. Note that the relevant time dimension here is the age of the mother instead of the commonly used calendar time. $1(a - a_i^0 = j)$ is a dummy equal to one if the difference between the age of a mother and the age when she had her first child a_i^0 is j quarters, and $1(a_i^0 = t)$ identifies each cohort of mothers and their assigned control units. We control for age-by-cohort fixed effects (δ_{at}) and individual-by-cohort fixed effects (α_{it}). Our quarter of reference is the focal mothers' quarter prior to becoming pregnant. The error term includes a random component ε_{iat}. Robust standard errors are estimated to correct for heteroskedasticity and clustered at the individual level.

Equation (1) estimates a dynamic DiD model in the form of a two-way fixed effects (TWFE) regression. Our coefficients of interest are the cohort-specific effects β_j^t. They identify how the outcome of interest (say, hospital visits for assault) evolves over time; that is, *j* quarters before and after the birth of her first child (at mother's age a_i^0) relative to how the outcome would have evolved for similar-age women who will have their first child 4-12 quarters later. As treatment effects and fixed effects are allowed to differ by cohort *t* (i.e. quarter of age at childbirth), the model is equivalent to estimating separate TWFE regressions for each cohort using a control group of not-yet-treated women, thus avoiding concerns raised by the recent literature on difference-in-difference estimation in staggered settings as surveyed by De Chaisemartin & d'Haultfoeuille (2023).[11] We weight all cohort-specific estimates by the sample shares of each cohort to retrieve an average estimate for all cohorts.

The critical identifying assumption is that, in the absence of the birth of the first child, the outcomes of interest of before-treated mothers would evolve as those of later-treated mothers. To assess the validity of this assumption, in the previous subsection we showed that our main outcomes of interest generally followed parallel trends across early- versus later-treated groups prior to our focal mothers giving birth to their first child for our two preferred measures, which are hospital visits for assault and hospital visits for IPV. In the results section, we directly test this assumption as, in the absence of any pre-existing differential trends between early-women with and without children, the estimated coefficients β_{-8}^t to β_{-4}^t should not be statistically significantly different from zero.

[11] Nevertheless, in Appendix Figure A3 we ensure that our results are robust to implementation of the relevant recent estimators surveyed by De Chaisemartin & d'Haultfoeuille (2023).

V. Results

Main Findings: Age-adjusted effects on mothers. Panels B, D and F of Figure 2 plot estimates for first-time mothers, normalized by the mean hospital visit rate of each category measured one quarter before birth. Overall, we cannot reject the parallel pre-trends assumption. Panel B shows parallel pre-trends, with coefficients β_{-8}^{t} through β_{-4}^{t} being very close to zero in magnitude and not statistically significant. Panels D and F also show non-statistically significant estimates prior to childbirth, although some of the estimates are larger in magnitude in part because of the low frequency of either outcome.

With pregnancy, hospital visit rates for assault fall by 33 percent relative to the rates of later-treated mothers, as shown in Panel B. This gap widens during pregnancy, reaching its largest level at birth with a relative decline of 59 percent. During the child's first year, the mothers' hospitalization rates for assault continue to be between 48 percent and 26 percent lower than those of later-treated mothers. All these estimates are statistically significant at the 5% level. Once the child turns two years old, the gap remains negative, albeit only marginally statistically significant at the 10% level (p-value of 0.056).

Crucially, once we account for age-related assault dynamics with women who will be mothers several quarters later, Panel B of Figure 2 documents a substantial decrease in focal mother hospital visits for assault at pregnancy. The sharpness of the responses documented in Panel B suggest that this change in focal mother hospital visits most likely reflects the impact of pregnancy on hospital visits for assault rather than other changes in the social context that might decrease hospital visits for assault. Furthermore, the fact that the decline occurs at pregnancy or childbirth, but not before, rules out that it is driven by behavioral changes associated with the mother's decision to have a child.

Moving our attention to the other two less frequent outcomes, hospital visits for IPV and overnight hospitalizations for assault, a similar pattern is revealed. In both cases, there is a decrease in hospitalization rates around the last quarter of pregnancy (Panel D) or childbirth (Panel F) which persists during the first 6 months of the child's life. We also observe a small increase in hospitalizations for IPV at the beginning of pregnancy and at the child's 9 month birthday (Panel D), as well as a small increase in overnight hospitalizations for assault during the middle of the pregnancy (Panel F) relative to later-treated mothers (as we had observed in the raw data in Panels C and E). However, neither of these differences are statistically significantly different from zero at conventional levels. While the estimated coefficients may give an indication of the pattern, it is important to keep in mind that both hospital visits for IPV and overnight hospitalizations for assault are very rare outcomes in the data. Thus, it is difficult to estimate these outcomes with precision.

Suspicious Hospitalization. Figure 3 addresses concerns that our findings on hospital visits for assault are driven by early-treated mothers or practitioners differentially reporting the cause of the injury once a woman is pregnant or just had a child. In Panels A and B, we replace hospital visits for assault with hospital visits for injuries similar to those caused by assault but where the cause is stated to be an accident. In Panels C and D, we replace hospital visits for assault with hospital visits for bone fractures. Note that the bias for a patient untruthfully reporting the cause of one's injuries could be in either direction, for while mothers or mothers-to-be may be more likely to lie about a domestic assault to protect the father of their child, hospital staff are obligated under Swedish law to report cases of aggravated assault if a child is believed to be in danger. Panels A and C of Figure 3 present the quarterly raw averages for the early- and late-treated groups, while Panels B and D display the stacked difference-in-differences estimates. In both DiD panels, the gap in hospital visits due to accidents (Panel B) or due to bone fractures (Panel D) follows the similar U-shape pattern as the one observed for hospital visits for assaults (Panel B of Figure 2). With pregnancy, focal mother hospital visits for accidents fall by 20 percent relative to the rates of later-treated mothers and this gap widens during pregnancy, becoming largest at childbirth with a relative decline of over 50 percent. Over the next two years, the gap converges to zero, losing statistical significance with the child's 18-month birthday. Hospital visits for bone fractures follows more of a hockey-stick shape, with a sharp decline during the first two quarters of pregnancy and milder decline at childbirth, then slowly rebounding during the child's first 15 months but staying at levels well below the approximately 30 percent rate for later-treated mothers.

In contrast to our main estimates of hospital visits for assault, which is one of domestic nature since we code it as such if hospital visits for assault were explicitly identified as perpetrated by the partner or ex-partner or if they took place at home or in an unspecified location, the estimates in Panels E and F of Figure 3 relax the assault location and use hospitalization for *all* assaults regardless of where they took place, who was the perpetrator, or even whether the perpetrator was identified on the hospital records or known by the victim. Panel F shows a similar drop with pregnancy and childbirth, consistent with the earlier estimates.

Selective Health Care Seeking/Falsification Tests

To explore whether the observed drop in IPV is driven by mothers drastically reducing their health care visits, Figure 4 presents estimates using the following as left-hand-side variables: (1) any hospital visit (except for assault or directly related to childbirth); (2) hospital visits for benign tumors; and (3) hospital visits for bus accidents. Neither "any" hospital visits nor hospital visits for benign tumors show a reduction in health care with pregnancy or childbirth. Hospital visits for bus accidents are very imprecisely estimated and show a decline at birth and during the first 3-month of the child (only the latter is statistically significant). Taken together, these results suggest that it is unlikely that our finding on hospital visits for assault is due to some spurious difference in the change in the rate of hospital visits between treated mothers and not-yet-treated women.

To further explore whether the decline is due to mothers being selective with their hospital visits, Figure 5 shows results where we categorized our key variable by the severity of the assault, from more to less serious. The idea is to see if the least severe assaults represent a sharper drop in reporting, which would be indicative that there is some selective care-seeking. We categorize hospital visits for assault into the following types: (1) the "least severe" measure involves hospital visits where the action is described as some sort of exam/documentation/photography following assault or rape; (2) the "most severe" measure involves hospital visits for assault where the injury is either a bone fracture or a wound that required stitches; and (3) the middle category, which excludes hospital visits for assault in the other two categories. Clearly, both the least and most severe cases show a similar pattern, making it difficult to reconcile the sharp decline in hospital visits for assaults with any selective health care seeking by mothers.

Alternative Methodological Approaches and Longer-Term Results

In addition to our main specification in equation (2), we also implement other event study identification strategies using childless women as in Kleven, Landais & Søgaard (2019) as well as recent methods proposed by Borusyak *et al.* (2024), Sun & Abraham (2021), and Callaway & Sant'Anna (2021). Appendix Figure A2 shows event study analysis separately for mothers and a sample of childless women for whom we have imputed placebo birth years of a first child following Kleven et al (2019). We show results for the three measures. While the event study analysis for childless women mostly shows non-statistically significant estimates around the 0 line, there is for mothers a sharp and persistent drop in hospital visits for assault (panel A) and overnight stays (panel C), albeit the latter one lacks precision. Panel B reveals a 10 percent statistically significant drop in hospital visits for IPV at childbirth, but no pattern among childless women.

Figure A3 in the Appendix presents the estimates resulting from a regular TWFE model with childless women as a never-treated control group (panel A), as well as event-study estimation methods suggested by Borusyak *et al.* (2024) (panel B), Sun and Abraham (2021) (panel C), and Callaway and Sant'Anna (2021) (panel D). Both approaches by Callaway and Sant'Anna (2021) and Sun and Abraham (2021) decompose the estimated effect into separate two-by-two DiD comparisons; hence, dropping all "forbidden comparisons" such as using already treated units to estimate the counterfactual. Their approaches are very similar to our main estimation strategy but with the difference that they use childless women as a never-treated control group. The method proposed by Borusyak *et al*. (2024) imputes the missing counterfactuals by predicting them from pre-treatment values.[12] Reassuringly, Appendix Figure A3

[12] See, for example, Rüttenauer & Aksoy (2024) for a more in-depth comparison between the different estimators.

demonstrates that all four alternative estimation methods produce results that are comparable to our main estimates.

Appendix Figure A4 utilizes the method by Kleven et al. (2019)—already estimated in Figure A2 for a shorter time span— but estimates the impact over a longer time span (up to 32 quarters, or 8 years, before and after birth of the first child). Importantly, even though not all estimated coefficients are statistically different from zero at the 5% level, the estimates show that the average decline in hospital visits for assault persists up to when the child is 8 years old. Finally, Appendix Figure A5 presents the estimates from the main stacked difference-in-differences model in Equation (1) but without individual and cohort fixed effects; and Appendix Figure A6 uses as outcome number of hospital visits for assault during the last quarter. Reassuringly, the results remain the same.

V. Reconciling our Findings with Longitudinal Crime Data Studies

Contrary to previous studies, we find that motherhood reduces IPV and that this decrease persists for at least the first two year after childbirth (and probably more). It is important to note that, as with this previous descriptive work, our raw data of hospitalizations for IPV also shows a positive association with motherhood. However, this positive association no longer holds when we remove confounding effects by using future mothers-to-be as a comparison group. Nonetheless, we still need to reconcile our findings with recent longitudinal studies, which use crime data and health worker reporting of IPV (Massenkoff & Rose 2023; Britto et al. 2024) to also observe a positive relationship between IPV and motherhood driven by those who divorce. In this section, we conduct subgroup analysis by whether the woman stays with the father or leaves him within the first two years after the birth of the child. Doing so reveals an increase in hospital visits for IPV after conception and motherhood for those who leave the father of the child, which is consistent with the observed increase in fathers' arrest for fourth degree assaults found by Massenkoff & Rose (2023) and with the increase in domestic violence prosecution and the higher propensity for women to report any incidents to the police found by Britto *et al.* (2024).

Most importantly, we find that conception and motherhood *decreases* violence when we use our broader measure of hospital visits for assault, both for women who stay with the father and for those who leave him. For both types, there is a temporary decline that mimics the father's decline in hospital visits for alcohol abuse. In addition, for women who leave the father of the child within two years of giving birth, we observe a persistent decline in IPV after the child's first birth, probably around the time the relationship was dissolved.

Couples that stay together. Among those couples that survive their first two years after the birth of their first child, the evidence is consistent with pregnancy causing a sharp drop in both mother hospital visits for assault during the first quarter ($t = -2$) of the pregnancy and for IPV during the last quarter ($t = -1$), as shown in Panels A and B of Figure 6. In both cases, the decline in hospital visits persists until the child's six- (for IPV) or nine- (for assault) month anniversary, though the estimates for hospitalization for IPV are not statistically significant for IPV. Prior to conception, the hospitalization rates for assault and IPV of early-treated women are very similar to those of later-treated women. Interestingly, Panel C shows a similar temporary drop in hospital visit rates for alcohol abuse among the fathers of these children, suggesting that a potential driver of the temporary decline in violence is fathers curbing their alcohol consumption during pregnancy, at childbirth, and during the child's first 9 months of life. By the time the child is 9 months old, the fathers' hospital visit rates for alcohol abuse converge toward those of later-treated men, just as the mothers' hospital visit rates for assault and IPV converge toward those of later-treated women. While Panel D shows that the mothers' hospital visit rates for alcohol abuse also decline with conception, pregnancy, and birth, they remain persistently lower than those of later-treated women for at least two years *after* childbirth, suggesting that mothers' lower alcohol consumption is not sufficient to reduce their victimization.

Couples that do not stay together. Figure 7 displays an event study analysis for mothers who leave the relationship within two years after the birth of their first child. In this case, we observe two distinct and apparently contradictory results. On the one hand, there is a sharp decline in hospital visits for assault that begins with conception (shown in Panel A) while, on the other hand, hospital visits for IPV increases from conception and during the first 9 months after birth, albeit only the peak at 9 months after birth is statistically significant at the 5% level (shown in Panel B). As we discuss below, the decline in hospital visits for assault for couples who break up is similar to that observed for mothers who stay in the relationship and for both is most likely due to the pregnancy. Fathers' lower intoxication, reflected by the decline in their hospitalization for alcohol abuse at ($-2 \leq t \leq 0$), would explain the mothers' lower levels of hospital visits for assault during pregnancy and continuing through the child's first quarter of life. This decline in maternal hospital visits for assault is also consistent with norms against assaulting pregnant women because violence may harm the developing fetus (Currie *et al.* 2022) and norms against harming mothers who are breastfeeding their child.

On the other hand, the second finding of an increase in hospital visits for IPV is not necessarily caused by the pregnancy, as it is preceded by another peak a year prior to conception ($-6 \leq t \leq -7$). It is noteworthy that the timing of this first peak is close in time to their partners' higher rate of hospital visits for alcohol abuse at ($-5 \leq t \leq -8$), which would be consistent with the literature that associates alcohol consumption and domestic violence (Markowitz 2000).

What, then, might explain the increase in hospital visits for IPV after conception? Since fathers' hospital visits for alcohol abuse decline during pregnancy and rebound after childbirth (Panel C of Figure 7), alcohol intoxication by their partners cannot be driving the increase in hospital visits for IPV ($t = -2$) by early-treated women. Most likely, this increase is therefore either a prolongation of violence within the relationship that had temporarily declined during the six-month "courtship" period prior to procreation, and/or a higher willingness of mothers to report the perpetrator, for this may give them a stronger case in a divorce or to obtain custody of the child. Furthermore, as fathers' hospital visits for alcohol abuse converge after childbirth to those of later-treated men, this suggests that the "honeymoon" might be over and violence resumes, which would also explain the third peak in hospitalizations for IPV when the child is about 9 months old ($t = +3$). This last increase is also consistent with anecdotal evidence that the risk of victimization increases with separation or divorce (Brownridge 2006) and that there is a higher propensity for women to report violence during a divorce (Britto *et al.* 2024). It is noteworthy that mothers' hospital visits for assault or IPV decline following the child's first birthday, most likely reflecting that mothers have left the toxic relationship.

Concerns that leaving a partner might be endogenous are addressed in Appendix Figure A.7 where we show an event study analysis for hospital visits for assault if the woman has a high or low ex-ante probability of leaving her partner (panel A) or being a victim of assault (panel B) based on pre-birth socio-demographic characteristics. Although we lose some precision as expected, the results are similar to those shown in panel A in Figures 6 and 7, mitigating any endogeneity concerns.

VI. Alternative Mechanisms

In this section, we discuss several alternative mechanisms that we tested but found no evidence that they were driving the observed sharp decline in hospital visits for assault after pregnancy and childbirth.

Women become more "valuable" within the household as they are now nurturing a child. To study this channel, we analyze whether the largest decline in the likelihood of hospital visits for assault occurs among mothers living with the father of the child compared to single mothers or those living with a partner who is not the father of the child. Evidence that the decline is driven by mothers living with the father would suggest that women's nurturing role increases their bargaining power within the household, reducing their risk of victimization. Panel A of Appendix Figure A.8 shows that all women, whether they are single, living with the father or not, experience a statistically significant decline in the likelihood of hospital visits for assault with the birth of their first child. Hence, there is no clear support for this mechanism.

Lower risk of 'male backlash' as the mother is now earning and working less outside of the household, reducing any potential threat felt by her partner. We also analyze both partners' employment and earning dynamics around the birth of the child and classify couples by whether the mother's relative decline in earnings compared to her partner upon the arrival of the child was below or above the median gender gap in earnings among partners. Again, as shown in Panel B of Appendix Figure A.8, we observe a decline in the likelihood of hospital visits for assault for both groups of women, with no statistically significant difference between them, making it difficult to support this mechanism.

Mothers' lower engagement in risky behaviors. As discussed in the previous section, women's hospital visits for alcohol abuse decline even prior to conception. Nonetheless, it is not clear that this is driving the decline in hospital visits for assaults, as the drop in hospital visits for alcohol abuse last for 24 months after the birth of the child for women who stay in a relationship but fades away before the child's 18-month birthday for women who leave the relationship. Yet, the drop in hospital visits for assault is short-lived for women who stay in the relationship and more persistent and long-lasting for those who leave the couple. For the latter, the drop in hospital visits for assault is likely the result of the mother leaving a violent relationship. Taken together, this evidence casts some doubt on the viability of mothers' reduction in alcohol abuse, proxied by hospital visits for alcohol consumption, being a sufficient driver of the decline in violence.

Another way of investigating mothers' behavioral change is observing the impact of a second child's birth on their mothers' rate of hospital visits for assault. If the decline in hospital visits for assault with motherhood was driven by a behavioral change, we would not expect to see the same pattern with the pregnancy and birth of the second child. Yet, Appendix Figure A.9 shows a similar short-term decline (albeit of a smaller magnitude) in hospital visits for assault during the pregnancy and birth of the second child, suggesting that it is unlikely driven by any behavioral change by mothers.

While the data do not allow us to test the extent to which the decrease in violence is driven by women's higher and more frequent access to health-care providers because antenatal, perinatal, and toddler care offers an opportunity for identifying women experiencing violence during pregnancy, post-partum, and the first years of the child, the fact that we observe a similar decline in hospital visits for assault around the birth of the second child suggests that higher access to health-care providers is unlikely the main mechanism driving our results..

Mothers' Economic Vulnerability. Appendix Figure A.10 presents an event study analysis for vulnerable versus non-vulnerable mothers, using different measures of vulnerability, all measured the year before pregnancy: (1) younger women; (2) those without a college degree; (3) women from the lowest three income quintiles; (4) immigrants; or (5) women not employed. As all subgroups display a similar drop in hospital visits for assault, with no clear pattern in any differences between vulnerable or less vulnerable groups, it is also unlikely that economic vulnerability (or lack of thereof) is driving our main findings.

VII. Conclusion

Using Swedish population-wide longitudinal administrative hospital records data and a stacked DiD approach, we document a large drop in hospital visits for assault with the pregnancy and birth of the first child. After ruling out that this is not driven by suspicious hospitalization, an overall reduction in hospital visits or selection in seeking medical care, we show that this decline has both a short-term and a longer-term component. The temporary decline covers most of the pregnancy and until the child is 9 months old, and mimics a temporary decline in hospital visits for alcohol abuse among the first children's fathers. The more persistent decline is driven by women who leave the relationship after the birth of the child. We find no evidence consistent with other potential mechanisms, including mothers' added value as the main nurturer, drop in relative earnings within the household, lower engagement in risky behaviors, or vulnerability. Our findings are novel because until now most of the evidence, comprising mostly descriptive studies and using small samples, supported a direct relationship between motherhood and intimate partner violence. In addition, our findings reveal the importance of using measures of violence that are broader than those reflected by domestic abuse calls, arrests, or overnight stays for assault, which capture only those cases where the woman (or her neighbors) are willing to report an incident.

Evidence that the decline may be temporary and tied to fathers' reduction in hospital visits for alcohol abuse suggests the need to air public health awareness campaigns underscoring the risk of victimization and perpetration with substance abuse consumption. Evidence that violence decreases with women leaving violent relationships would suggest the need to conduct campaigns that reduce the acceptance of violence as well as put in place a social network allowing couples to break free of toxic relationships.

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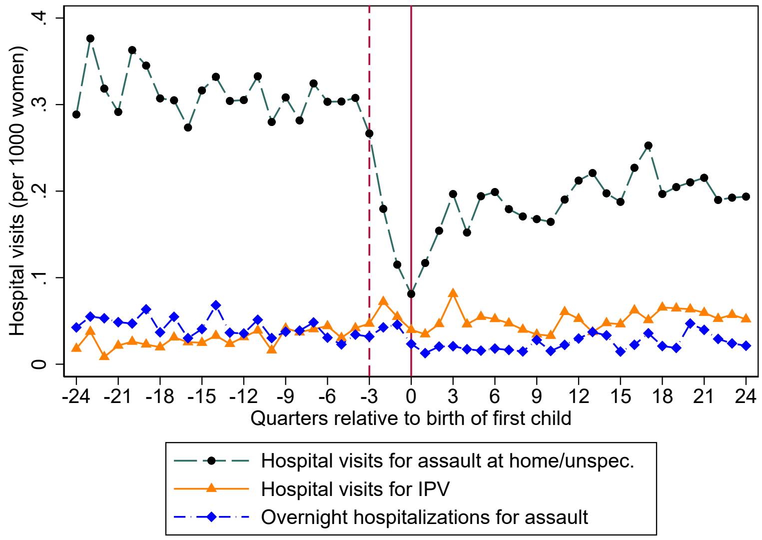
Table 1: Summary statistics

	Focal mothers	Future mothers
	Socio-demographic characteristics	
Cohort	1980	1980
	(6)	(6)
Birth year of first child	2009	2009
	(4)	(4)
Age at first birth	29	30
	(4)	(4)
University degree before pregnancy	0.42	0.42
	(0.49)	(0.49)
Employed before pregnancy birth	0.83	0.86
	(0.37)	(0.34)
Married at birth	0.33	0.33
	(0.47)	(0.47)
Cohabit at birth	0.56	0.57
	(0.50)	(0.50)
Yearly labour earnings	289,550	244,259
	(175,574)	(167,465)
Yearly total income	306,406	258,155
	(163,157)	(159,603)
	Hospital visits before pregnancy	
Hospital visits for assault at home or unspec. locations (HU)	0.312	0.298
	(17.650)	(17.271)
Hospital visits for IPV	0.041	0.030
	(6.375)	(5.523)
Over-night hospitalizations for assault	0.035	0.040
	(5.902)	(6.340)
Hospital visits for any assault	0.354	0.358
	(18.817)	(18.922)
Hospital visits for similar accidents	2.079	2.404
	(45.545)	(48.969)
Hospital visits for bone fractures	2.145	2.209
	(46.259)	(46.945)
Hospital visits for benign tumors	15.517	14.130
	(123.596)	(118.028)
Hospital visits for alcohol abuse	0.513	0.585
	(22.642)	(24.178)
Father's hospital visits for alcohol abuse	0.507	0.522
	(22.513)	(22.832)
Hospital visits for car accidents	0.825	0.900
	(28.703)	(29.988)
Hospital visits for bus accidents	0.015	0.013
	(3.935)	(3.540)
Any hospital visit	442.867	336.563
	(496.726)	(472.534)
No of women	543,593	533,995
Observations	9,173,514	56,200,230

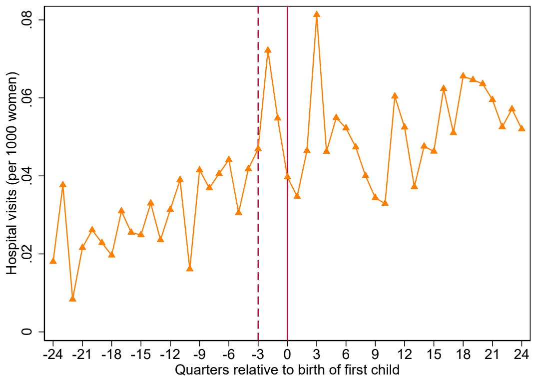
Notes: The table shows mean and standard deviations of main variables. Column (1) contains the focal mothers, column (2) contains the control group of future mothers who will give birth 4-12

Figure 1: Raw averages of hospital visits for assault around birth of the first child

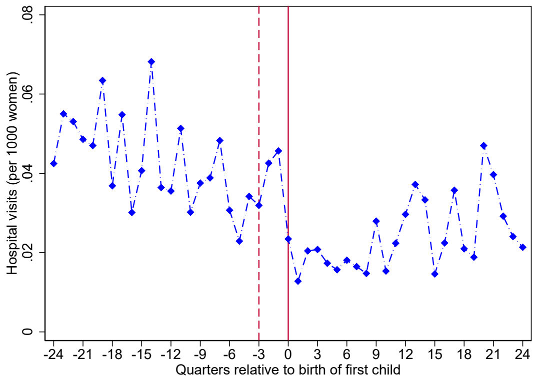
(a) Hospital visits for assault at HU



(b) Hospital visits for IPV



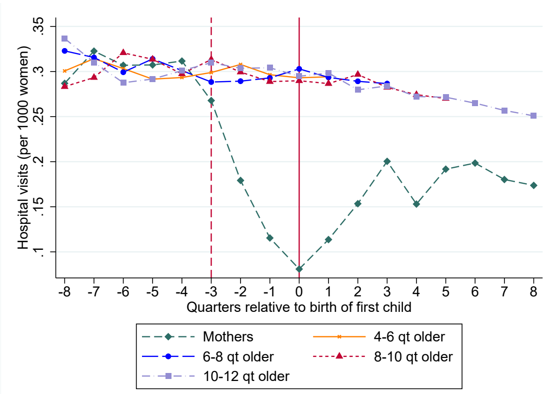
(c) Overnight hospitalizations for assault



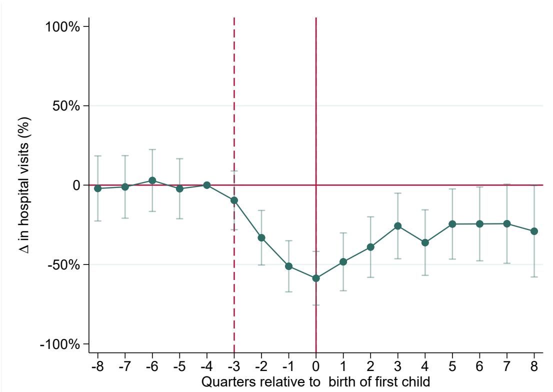
Notes: The figure shows the raw averages (per 1000 women) compared to quarter of birth of a woman's first child. The outcomes in panel (a) are hospital visits for assaults that took place at home or in unspecified locations (including IPV and overnight hospitalizations), hospital visits where the woman specifically reported that the perpetrator was her intimate partner, and overnight hospitalizations for assaults. Panel (b) and (c) separates out hospital visits for IPV and overnight hospitalizations for assault. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure 2: Raw averages and stacked difference in difference of hospital visits for assault around birth of the first child, for focal mothers and future mothers

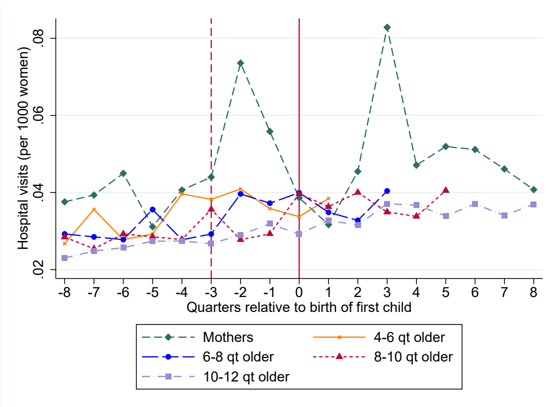
(a) Raw averages: hospital visits for assault at HU



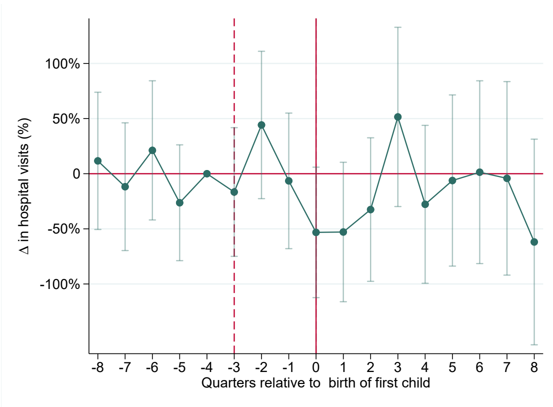
(b) DiD: hospital visits for assault at HU



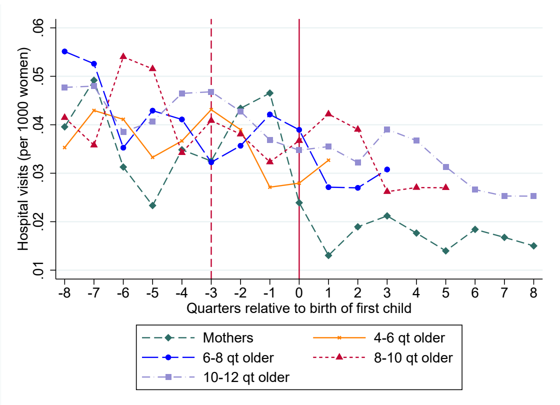
(c) Raw averages: hospital visits for IPV



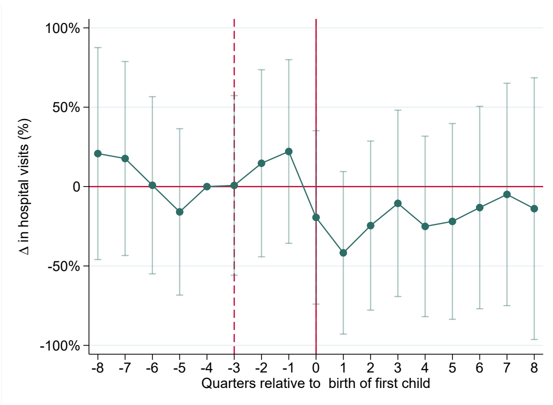
(d) DiD: hospital visits for IPV



(e) Raw averages: overnight hospitalizations for assault



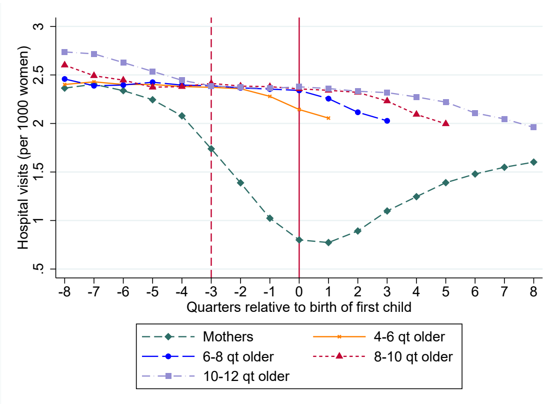
(f) DiD: overnight hospitalizations for assault



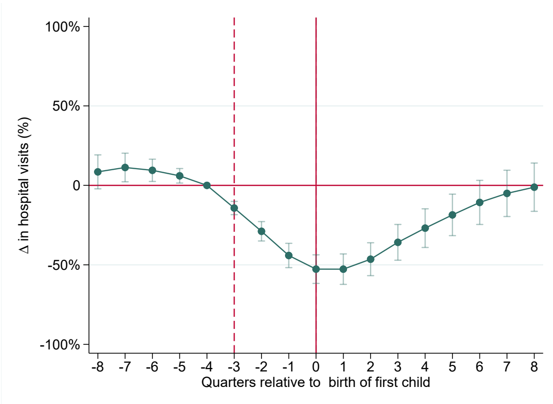
Notes: The outcomes are hospital visits for assaults that took place at home or in unspecified locations, including IPV and overnight hospitalizations, (panel (a) and (b)), hospital visits where the woman specifically reported that the perpetrator was her intimate partner (IPV) (panel (c) and (d)), and overnight hospitalizations for assaults (panel (e) and (f)). The graphs in the left column demonstrates the raw average of each outcome related to quarter of birth of the focal mother, for focal mothers and the control group of mothers who give birth 4-12 quarters later. The graphs in the right column show the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1). All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure 3: Raw averages and stacked difference in difference of hospital visits for other injuries around birth of the first child, for focal mothers and future mothers

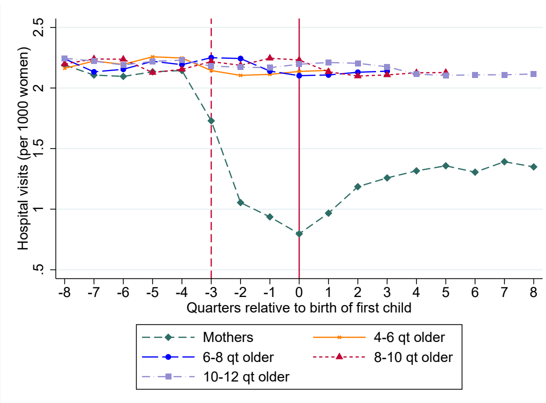
(a) Raw averages: hospital visits for similar accidents



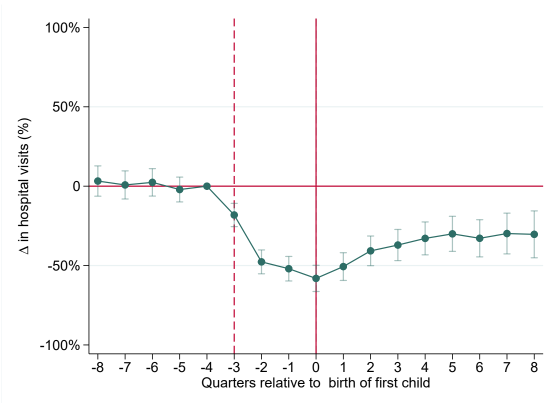
(b) DiD: hospital visits for similar accidents



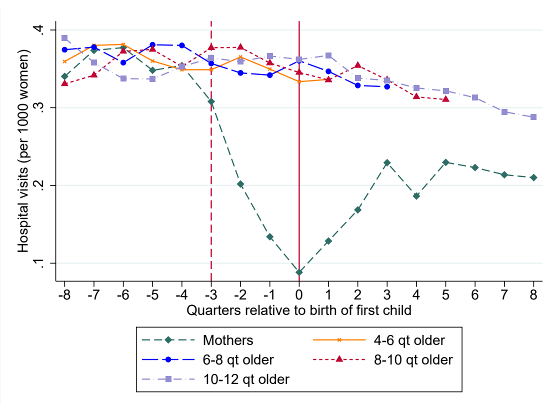
(c) Raw averages: hospital visits for bone fractures



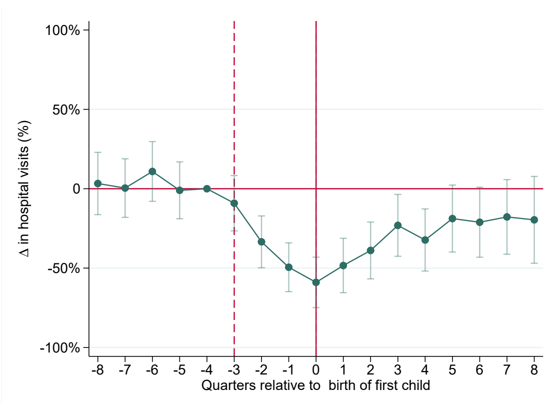
(d) DiD: hospital visits for bone fractures



(e) Raw averages: hospital visits for any assault



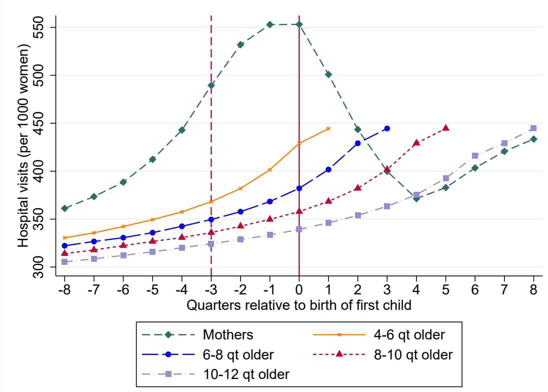
(f) DiD: hospital visits for any assault



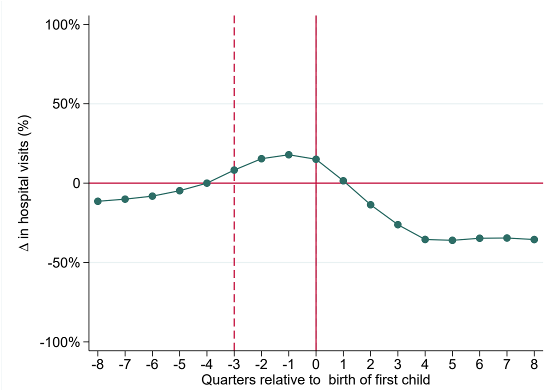
Notes: The outcomes are hospital visits with similar main diagnosis as those most common for assault, that took place at home or in unspecified locations, but where the cause is stated to be an accident (panel (a) and (b)), hospital visits for bone fractures (panel (c) and (d)), and hospital visits for assault regardless of where they took place (panel (e) and (f)). The graphs in the left column demonstrates the raw average of each outcome related to quarter of birth of the focal mother, for focal mothers and the control group of mothers who give birth 4-12 quarters later. The graphs in the right column show the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1). All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure 4: Raw averages and stacked difference in difference of hospital visits for placebo measures around birth of the first child, for focal mothers and future mothers

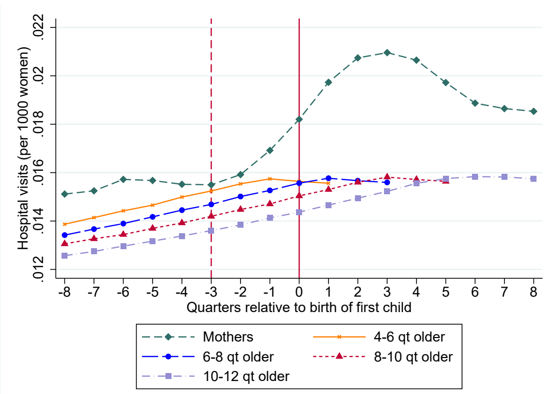
(a) Raw averages: any hospital visits (excl. assault and birth related)



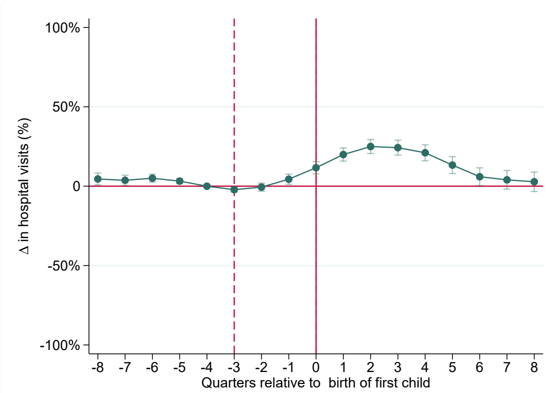
(b) DiD: any hospital visits (excl. assault and birth related)



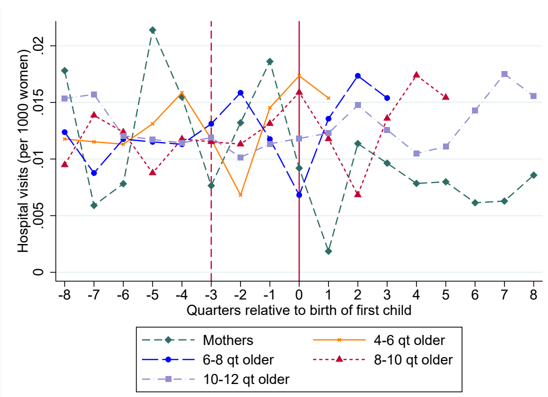
(c) Raw averages: hospital visits for benign tumors



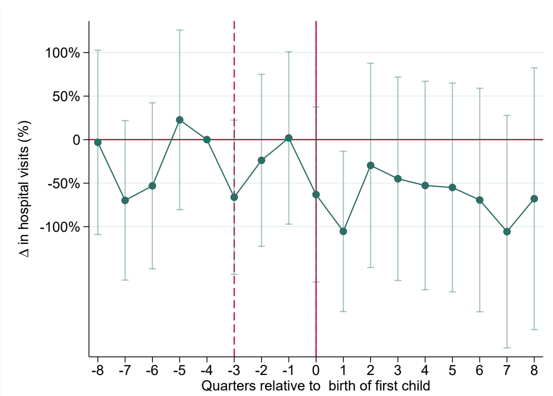
(d) DiD: hospital visits for benign tumors



(e) Raw averages: hospital visits for bus accidents

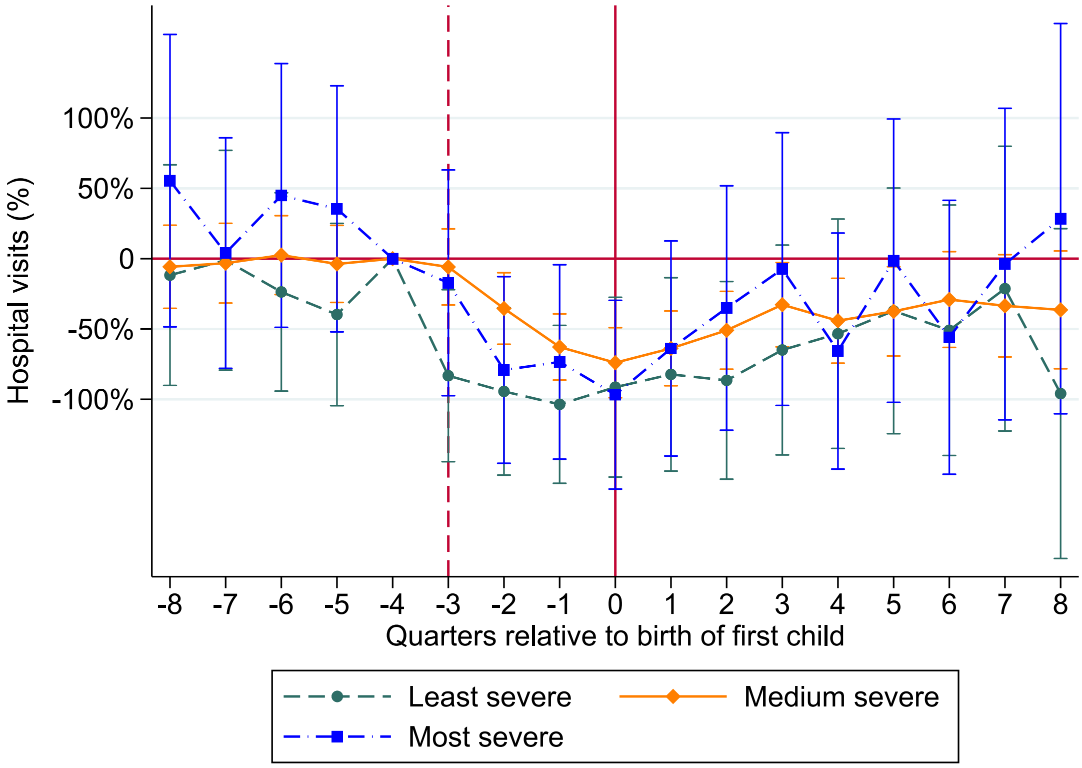


(f) DiD: hospital visits for bus accidents



Notes: The outcomes are any hospital visits excluding assault or birth related (panel (a) and (b)), hospital visits for benign tumors (panel (c) and (d)), and hospital visits for injuries caused by bus accidents (panel (e) and (f)). The graphs in the left column demonstrates the raw average of each outcome related to quarter of birth of the focal mother, for focal mothers and the control group of mothers who give birth 4-12 quarters later. The graphs in the right column show the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1). All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

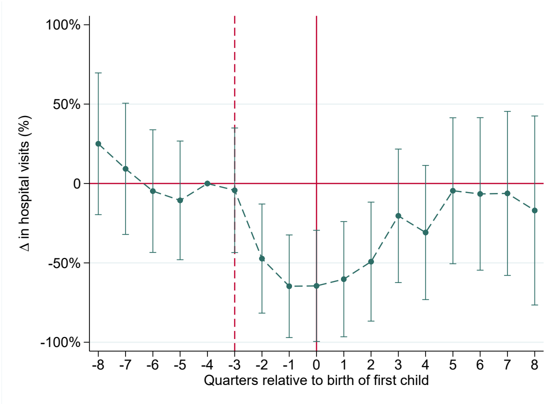
Figure 5: Stacked difference-in-difference around birth of the first child: hospital visit for assault of different severity



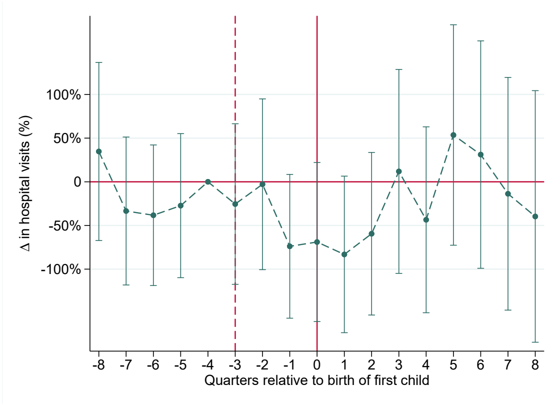
Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1), separately for hospital visits for assault where injuries are of different severity. The "Least severe" includes hospital visits for assault that did not result in an overnight stay and where the medical action was related to documentation of injuries (these medical actions include "AL003" (vaginal ultrasound), "AV008" (image documentation), "AV025" (exam following abuse) and "AV047" (exam after rape or alleged rape). "Most severe" includes only hospital visits and hospitalizations for assault where the injury was either a bone fracture or a wound in need of stitches. "Medium severe" includes the rest of the hospital visits for assault that did not result in an overnight stay. All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure 6: Stacked difference-in-difference of hospital visit for assault around birth of the first child, for mothers who stay with their partner 2+ years after birth

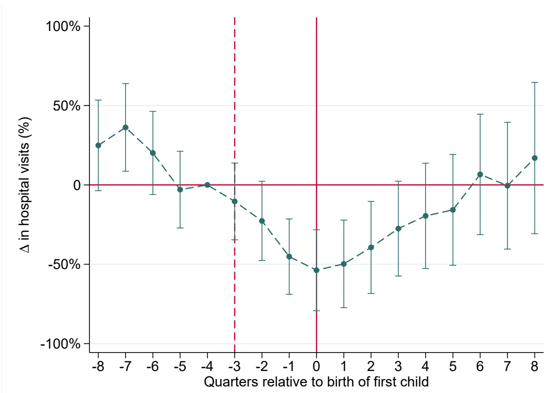
(a) Hospital visits for assault at HU



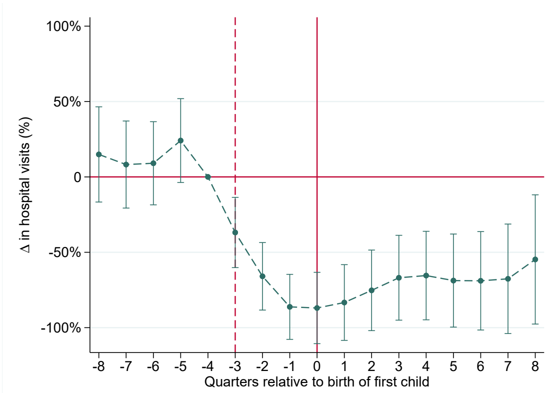
(b) Hospital visits for IPV



(c) Father's hospital visits alcohol abuse



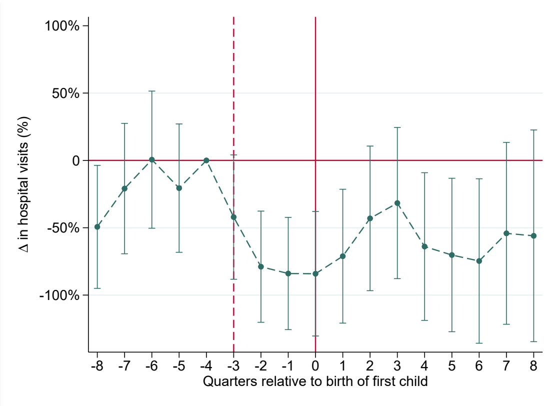
(d) Hospital visits for alcohol abuse



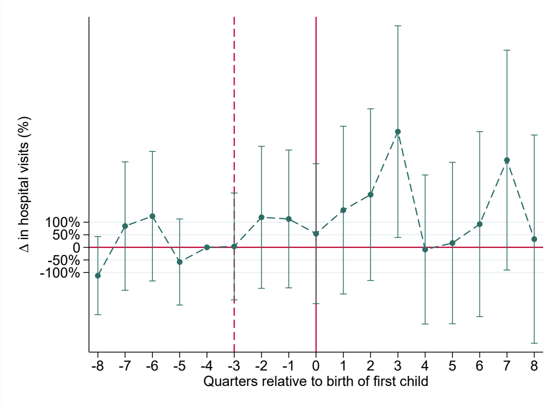
Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1), including only focal mothers and control women who stay with their partner for 2+ years after the birth of the first child. The outcomes are hospital visits for assault that took place at home or in unspecified locations (panel (a)), hospital visits for IPV (panel (b)), father's hospital visits for alcohol abuse (panel (c)), and mother's hospital visits for alcohol abuse (panel (d)). All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure 7: Stacked difference-in-difference of hospital visit for assault around birth if the first child, for mothers who leave their partner within 2 years after birth

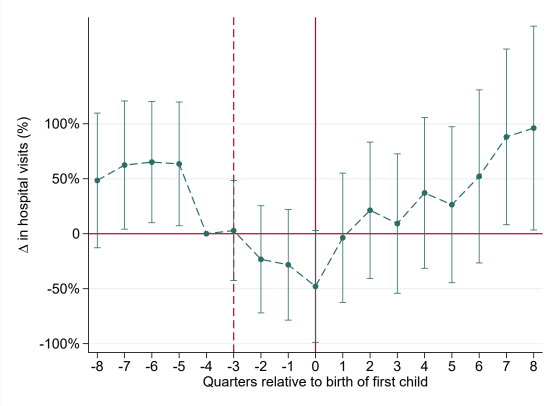
(a) Hospital visits for assault at HU



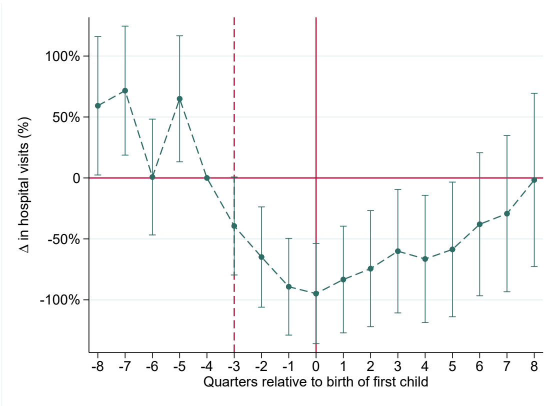
(b) Hospital visits for IPV



(c) Father's hospital visits alcohol abuse



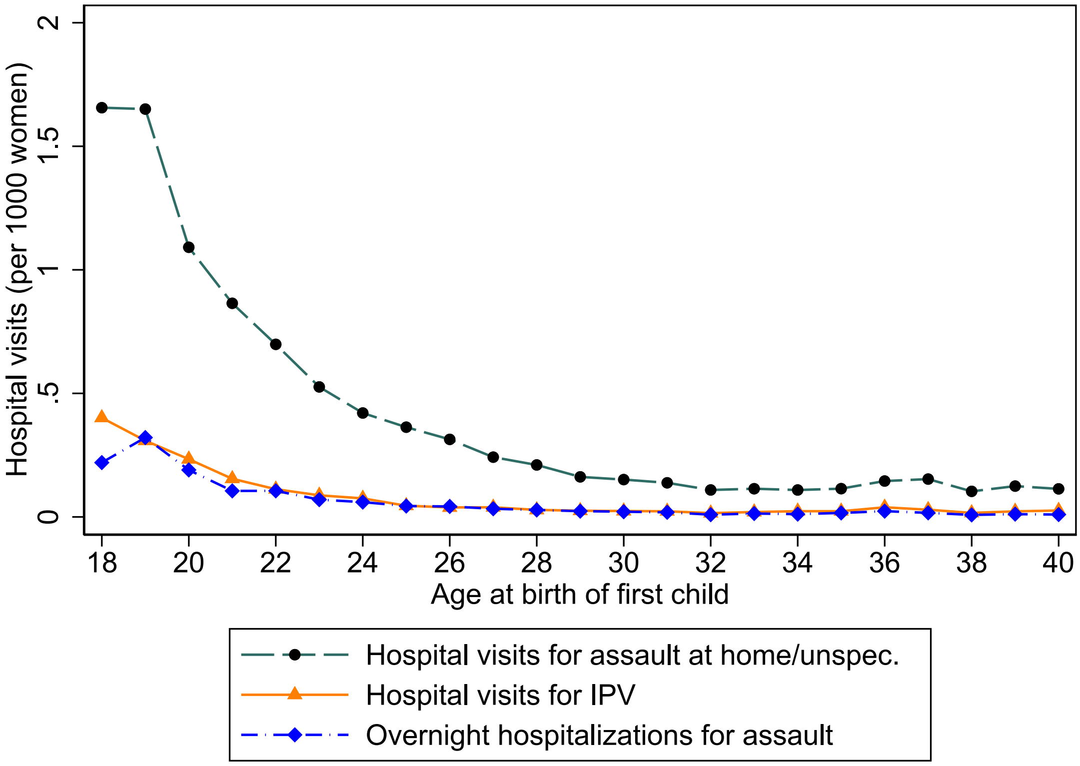
(d) Hospital visits for alcohol abuse



Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1), including only focal mothers and control women who leave their partner within 2 years after the birth of the first child. The outcomes are hospital visits for assault that took place at home or in unspecified locations (panel (a)), hospital visits for IPV (panel (b)), father's hospital visits for alcohol abuse (panel (c)), and mother's hospital visits for alcohol abuse (panel (d)). All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Appendix A

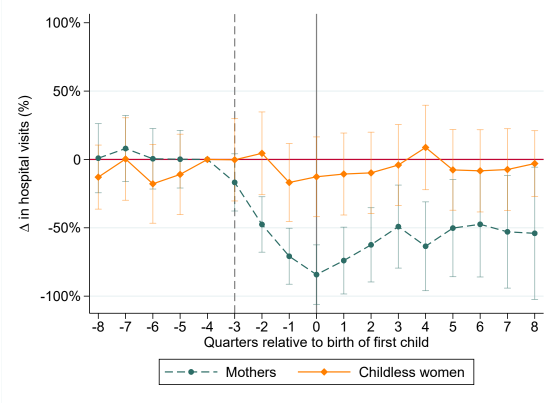
Figure A1: Average hospital visit for assault by age at first birth



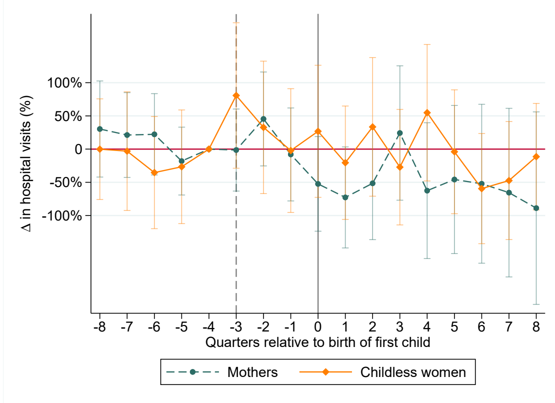
Notes: The figure depicts the average hospital visits for assault for women at different ages of first birth.

Figure A2: Event studies of hospital visits for assault around the birth of the first child, for mothers and childless women (following Kleven et al (2019))

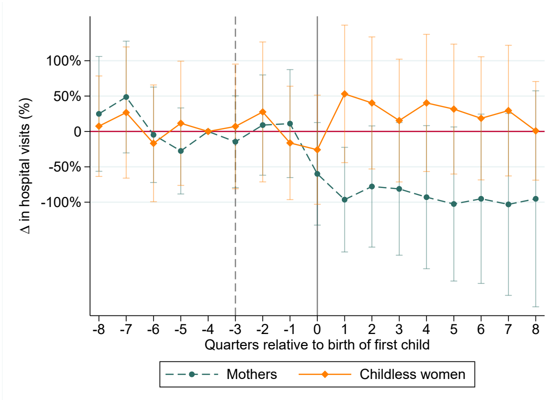
(a) Hospital visits for assault at HU



(b) Hospital visits for IPV



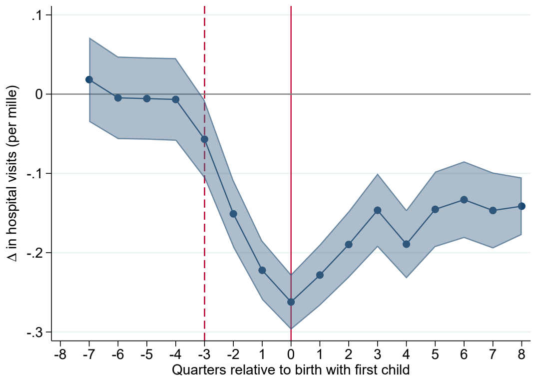
(c) Overnight hospitalizations for assault



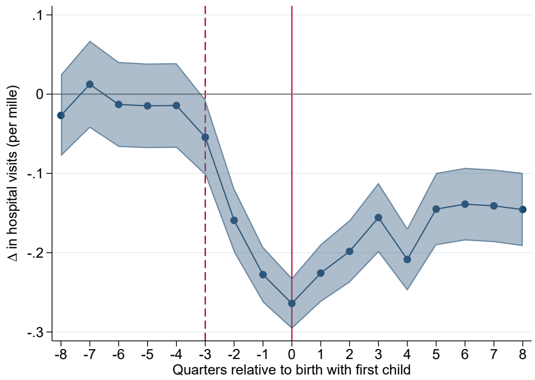
Notes: The figure depicts the point estimates and 95% confidence intervals of event study regressions controlling for individual, age and year fixed effects, following the methodology of Kleven et al (2019). The outcomes are hospital visits for assault that took place at home or in unspecified locations (panel (a)), hospital visits for IPV (panel (b)), and overnight hospitalizations for assault (panel (c)). Childless women are included if they are predicted to remain childless, based on socio-demographic characteristics. All regressions cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure A3: Event studies for hospital visits for assault around birth of the first child, for mothers and childless women (alternative estimators)

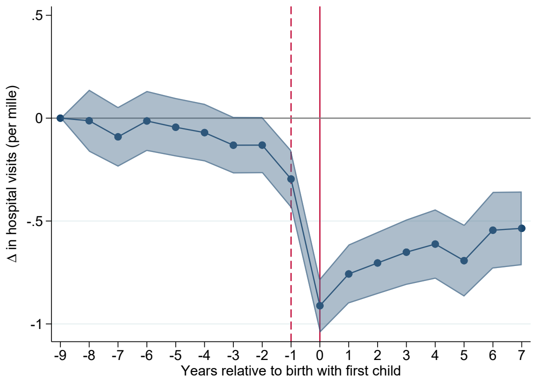
(a) Regular TWFE with never-treated control group



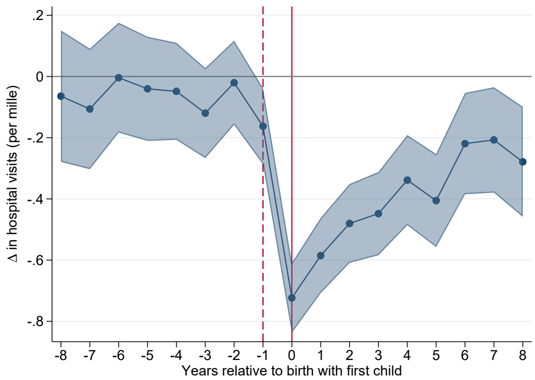
(b) Borusyak et al (forthcoming)



(c) Sun and Abraham (2021)



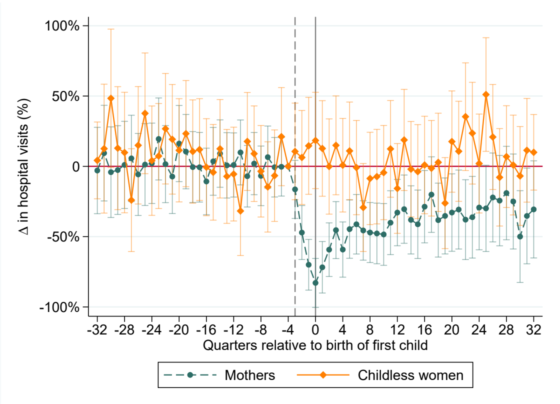
(d) Callaway and Sant'Anna (2021)



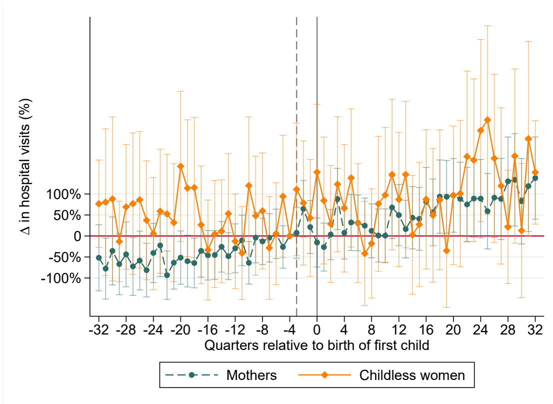
Notes: The figure depicts the point estimates and 95% confidence intervals of event study regressions controlling for individual, age and year fixed effects. The outcome in all panels is hospital visits for assault that took place at home or in unspecified locations. Childless women are included if they are predicted to remain childless, based on socio-demographic characteristics, and in all regressions they are included as a never-treated control group. Panels (a) and (b) use quarterly data. However, due to lack of computational power and a very large panel dataset, panels (c) and (d) use annual data. All regressions cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure A4: Long-term event studies of hospital visits for assault around birth of the first child, for mothers and childless women (following Kleven et al (2019))

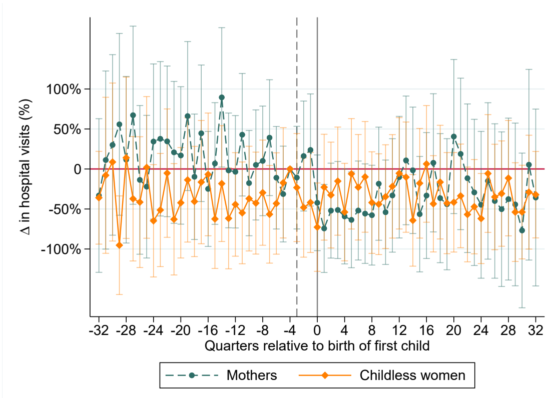
(a) Hospital visits for assault at HU



(b) Hospital visits for IPV



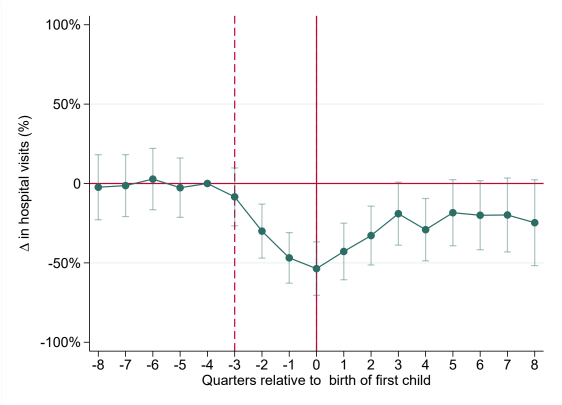
(c) Overnight hospitalizations for assault



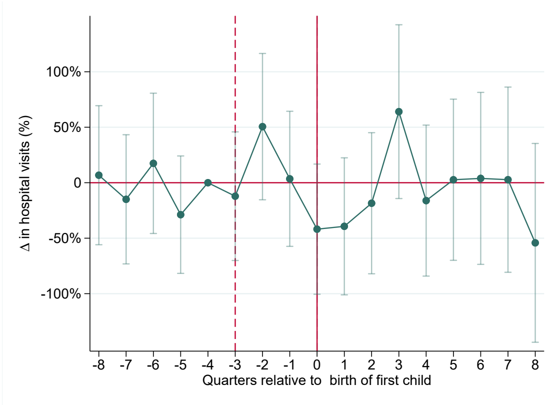
Notes: The figure depicts the point estimates and 95% confidence intervals of event study regressions controlling for individual, age and year fixed effects, following the methodology of Kleven et al (2019). The outcomes are hospital visits for assault that took place at home or in unspecified locations (panel (a)), hospital visits for IPV (panel (b)), and overnight hospitalizations for assault (panel (c)). Childless women are included if they are predicted to remain childless, based on socio-demographic characteristics. All regressions cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure A5: Stacked difference-in-difference of hospital visits for assault around birth of the firsct child, without individual and cohort fixed effects

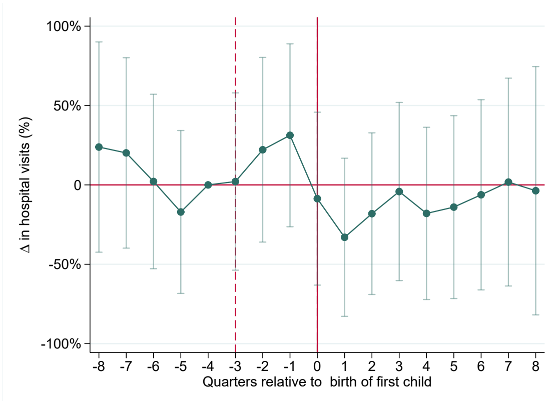
(a) Hospital visits for assault at HU



(b) Hospital visits for IPV

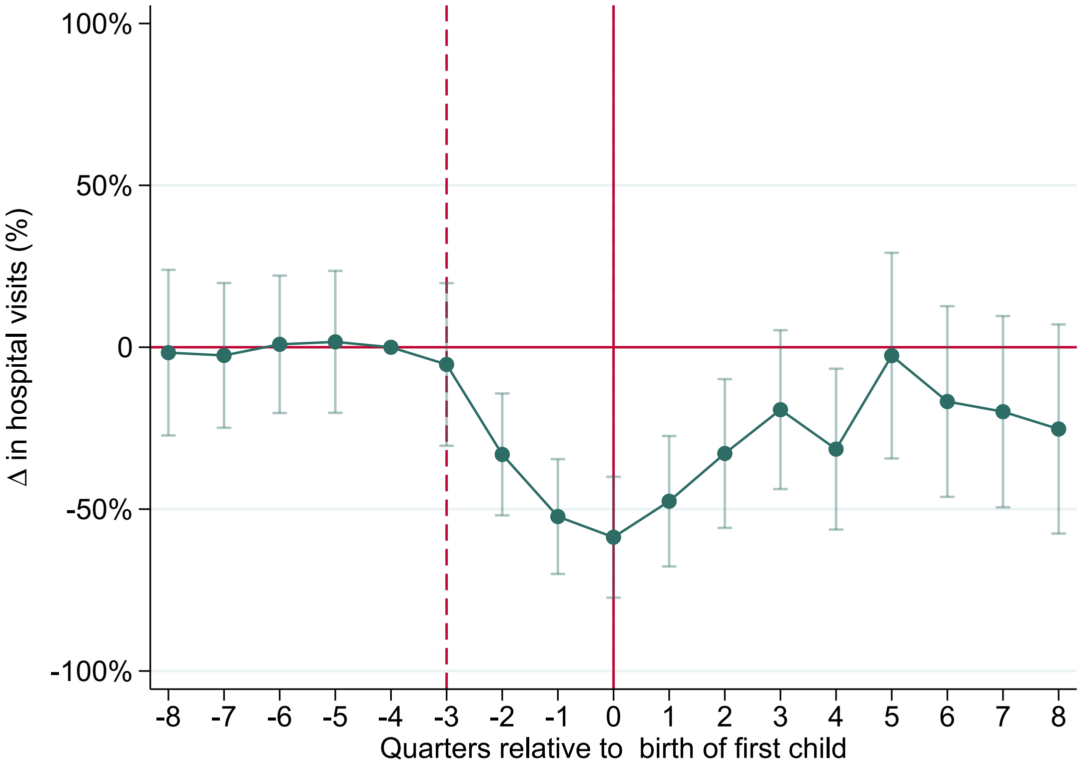


(c) Overnight hospitalizations for assault



Notes: The figure depicts the point estimates and 95% confidence intervals of a stacked difference-in-difference regression on focal mothers and future mothers. The outcomes are hospital visits for assault that took place at home or in unspecified locations (panel (a)), hospital visits for IPV (panel (b)), and overnight hospitalization for assault (panel (c)). The regressions only control for age fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

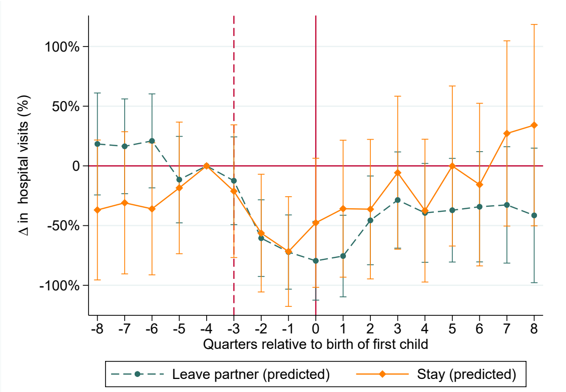
Figure A6: Stacked difference-in-difference of number of hospital visits for assaults around the birth of the first child



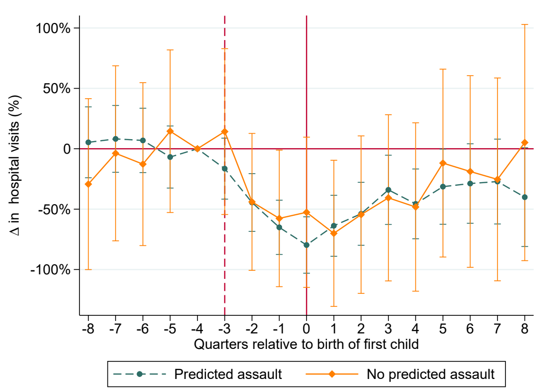
Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1). The outcome is a count measure of all times a woman visited a hospital for injuries caused by assault that took place at home or in unspecified locations in a given quarter. The regression controls for age by cohort and individual by cohort fixed effects and clusters standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure A7: Stacked difference-in-difference of hospital visit for assault around birth of the first child, splitting the sample by ex-ante probability of:

(a) Leaving the father within two years after birth



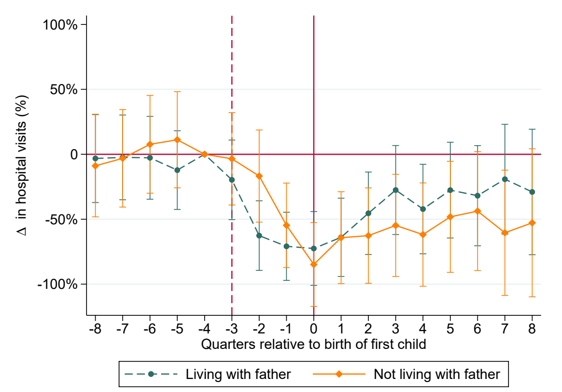
(b) Having a hospital visit for assault



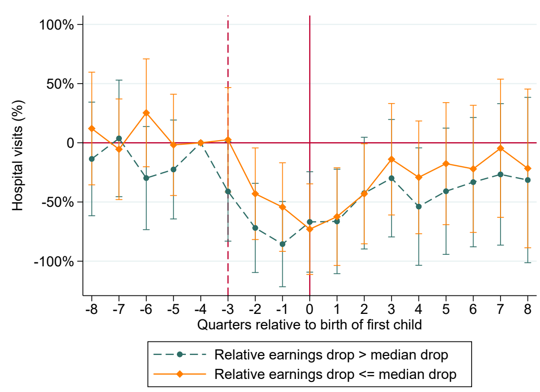
Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1), separate for women in different groups. The outcome is hospital visits for assault that took place at home or in unspecified locations. Panel (a) separates mothers (and future mothers) by their predicted probability of leaving the father of their child within two years after birth. Panel (b) separates by the predicted probability of having a hospital visit for assault. All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure A8: Stacked difference-in-difference of hospital visit for assault around birth of the first child, splitting the sample by:

(a) Whether mothers lives with the father at birth



(b) The relative earnings drop at birth

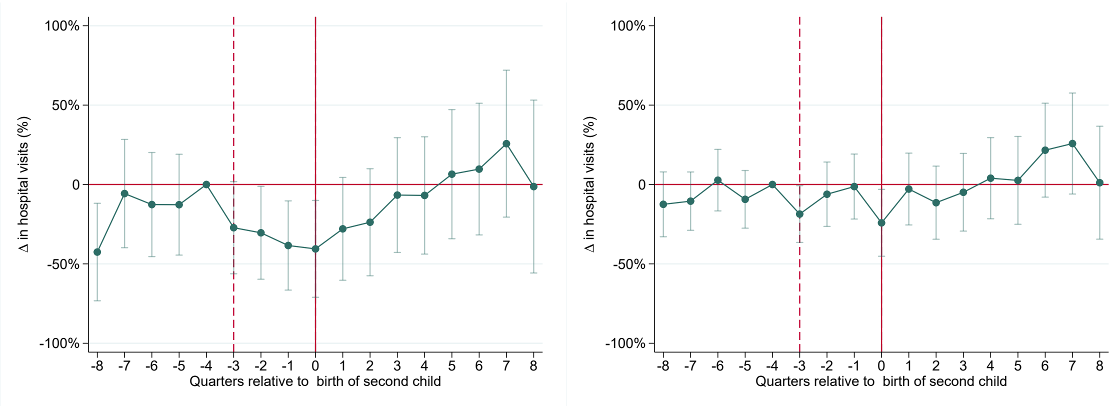


Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1), separate for women in different groups. The outcome is hospital visits for assault that took place at home or in unspecified locations. Panel (a) separates mothers (and future mothers) by whether they live with the father of their child at the time of birth or not. Panel (b) separates by whether the drop in relative income between mothers and fathers in the first year after the birth of the first child is above or below the median drop. All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure A9: Stacked difference-in-difference of hospital visit for assault around birth of the second child

(a) Hospital visits for assault at HU

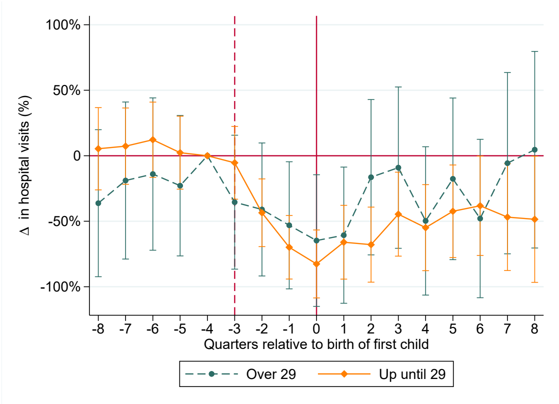
(b) Hospital visits for IPV



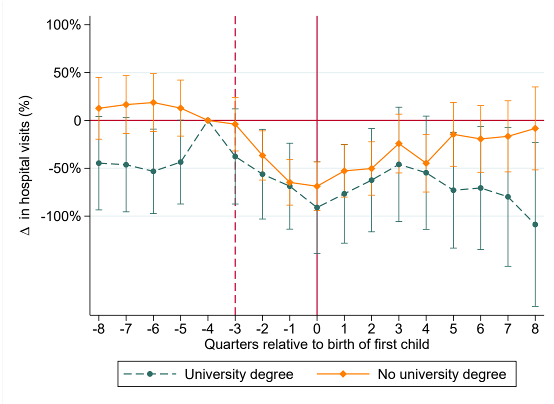
Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1), but when the event is the birth of a woman's second child. The outcomes are hospital visits for assault that took place at home or in unspecified locations (panel (a)) and hospital visits for IPV (panel (b)). All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.

Figure A10: Stacked difference-in-difference of hospital visit for assault around birth of the first child, splitting the sample by:

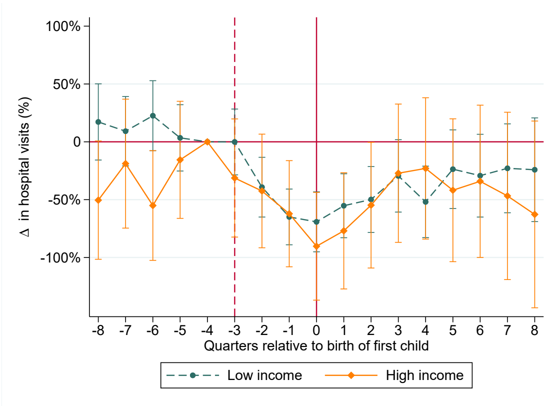
(a) Age at first birth



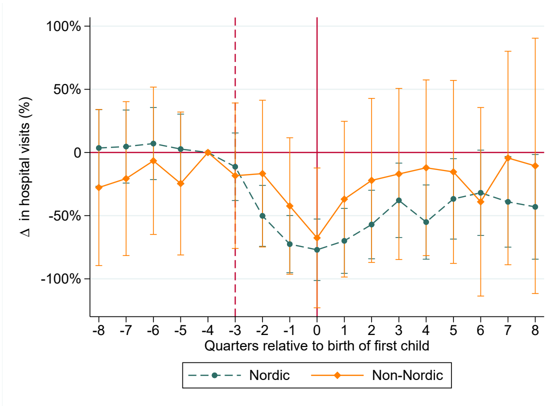
(b) Education level



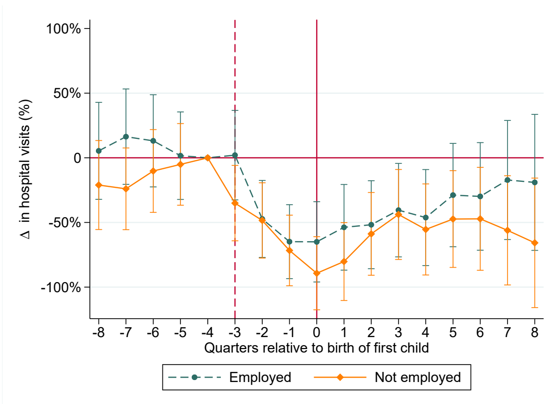
(c) Income



(d) Birth region



(e) Employment status



Notes: The figure depicts the point estimates and 95% confidence intervals of the stacked difference-in-difference regression described in equation (1), separate for women in different groups. The outcome is hospital visits for assault that took place at home or in unspecified locations. Panel (a) separates mothers (and future mothers) by age at first birth (above or below median). Panel (b) separates by whether the mother has a university degree before birth. Panel (c) separates by mother's income the year before pregnancy (above or below median). Panel (d) separates by mothers' birth region. Panel (e) separates by whether the mother was employed the year before pregnancy or not. All regressions control for age by cohort and individual by cohort fixed effects and cluster standard errors at the individual level. The estimates show the estimated impact as percentage compared to the pre-treatment mean of focal mothers. The dashed line indicates 3 quarters before birth (when most pregnancies start) and the solid line indicates quarter or birth.