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Family Formation and Crime

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ABSTRACT

We use administrative data from Washington State to perform a large-scale analysis of the impact of family formation on crime. Our estimates indicate that pregnancy triggers sharp declines in arrests rivaling any known intervention, supporting the view that childbirth is a "turning point" that reduces deviant behavior through social bonds. For mothers, criminal arrests drop precipitously in the first few months of pregnancy, stabilizing at half of pre-pregnancy levels three years after birth. Men show a sustained 20 percent decline in crime that begins at pregnancy, although arrests for domestic violence spike at birth. These effects are concentrated among first-time parents, suggesting that a permanent change in preferences---rather than transitory time and budget shocks---may be responsible. A separate design using parents of stillborn children to estimate counterfactual arrest rates reinforces the main findings. Marriage, in contrast, is not associated with any sudden changes and marks the completion of a gradual 50 percent decline in arrests for both men and women.

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Social dynamics are a fundamental determinant of crime (Glaeser et al., 1996). Interactions within neighborhoods (Damm and Dustmann, 2014), with potential criminal peers (Bayer et al., 2009), and with schoolmates (Billings et al., 2013) can meaningfully alter criminal trajectories, especially compared to traditional mechanisms such as the severity and immediacy of punishments (Becker, 1968; Chalfin and McCrary, 2017). A prominent literature argues that ties with children are critical as well. Parenthood is thought to serve as a “turning point” with the power to reduce criminal behavior through the added responsibility that comes with new social bonds (Laub and Sampson, 2001; Sampson et al., 2006). Indeed, parents with previous criminal justice contact frequently report in interviews that, without their children, they would be in prison or abusing drugs (Edin and Kefalas, 2011; Edin and Nelson, 2013; Sampson and Laub, 2009).

While the connection between family formation and crime has received substantial attention in the qualitative literature, quantitative evidence is sparse. Previous studies have focused on relatively small survey samples, leaving open the possibility that criminal desistance caused childbearing instead of the opposite. And despite the ubiquity and importance of family formation events—indeed, most people eventually have children or get married—empirical research in economics on the effects of family formation has focused on related but different questions such as the impact of child sex (Dahl and Moretti, 2008; Dustmann and Landersø, 2021) or effects on gender inequality (Kleven et al., 2019), with a few notable exceptions that focus on teen pregnancy (e.g., Hotz et al., 2005).

This paper uses administrative data covering more than a million parents to take an unprecedentedly close look at how criminal behavior changes when men and women have children. We implement a novel match between Washington State administrative records covering the universe of criminal arrests, births, marriages, and divorces—the largest such study ever conducted in the United States. Our comprehensive data allow us to highlight high-frequency changes in both the timing and type of arrests, distinguishing between desistance that occurs well before a child is conceived and changes after conception, for example. The scale of our data also allows us to precisely measure differences in effects across birth order, child sex, parents’ age, and other characteristics that speak to potential mechanisms and reinforce the robustness of the main results.

Several striking patterns immediately around childbirth are clear in the raw data. To carefully control for age trends in offending and provide point estimates of long-run effects, however, we develop a difference-in-differences estimator that compares mothers’ and fathers’ arrests rates over the three years before and after birth to arrest rates at the same ages of parents who have children when they are between one and five years older. These older parents tend to be arrested less frequently than the focal younger parents, a natural result of the fact that age at first birth is strongly correlated with overall arrest risk. We show, however, that age-crime profiles for these groups track each other closely, suggesting that older parents’ arrest rates can provide a useful

benchmark for counterfactual arrest rates in the absence of childbirth.

We begin our investigation with mothers. Both the raw data and difference-in-differences estimates show the same patterns: drug, alcohol, and economic arrests decline precipitously at the start of the pregnancy, bottoming out in the months just before birth. Shortly after birth, criminal arrests recover but ultimately stabilize at about 50 percent below pre-pregnancy levels. These effects are large compared to other commonly studied interventions. [Helland and Tabarrok \(2007\)](#) find that the threat of nearly 20 years of additional prison time decreases annual felony offenses by 15-20 percent, an elasticity of 0.05; [Lee and McCrary \(2005\)](#) calculate a similar deterrence elasticity for juveniles reaching the age of majority. Based on the summary assessment in [Chalfin and McCrary \(2017\)](#), mothers' 50 percent drop in crime after birth would correspond to the impact of more than doubling the police budget or prison population.

The sharpness of the response suggests that these declines reflect the impact of pregnancy rather than the onset of a romantic relationship or other coincident life events. There is no evidence of any anticipatory decline in arrest rates. We also find similar positive long-term impacts on teen mothers, for whom the vast majority of pregnancies are unanticipated ([Mosher et al., 2012](#)).¹ Still, our results apply only to mothers who carry their child to term and therefore appear in the birth records; offending patterns for parents who terminate their pregnancy may differ. If the timing of pregnancy itself is unconfounded and older parents provide an appropriate counterfactual, our estimates capture the causal effects of pregnancy on couples who elect to have the child. Recent evidence suggests that even among women who experience an unintended pregnancy, the majority do not receive an abortion ([Finer and Zolna, 2014](#)), suggesting this group accounts for a large share of all pregnancies. And unless mothers who do not carry their child to term have the *opposite* reaction to mothers who do, the unconditional effects of pregnancy would likely still entail large decreases in crime.

Mothers, however, experience physical effects of pregnancy that may change their propensity to engage in criminal activity independent of social interactions with their partners. Penalties for some criminal activities, including drug and excessive alcohol use, may be heightened while pregnant,² and after a child is born mothers may be more likely to be held legally responsible for the child's welfare. It is unclear which of these potential changes best explain the reductions in mothers' criminal behavior. Fathers, on the other hand, experience none of these changes and are typically less involved in childcare ([Drago, 2009](#)), making their outcomes a stronger test of how the social ties forged by family formation influence behavior. Our data provides a unique opportunity to study fathers because they are unusually well covered in Washington birth records, with 85%

¹Several previous studies have found no or negative effects of teen childbearing on conventional economic outcomes such as income and education ([Hotz et al., 2005, 1997](#); [Fletcher and Wolfe, 2009](#); [Kearney and Levine, 2012](#)), but have not studied crime.

²According to [Miranda et al. \(2015\)](#), Washington has prosecuted women for drug use during pregnancy although a 1996 appeals ruling determined that drug use during pregnancy is not criminal mistreatment.

of births to unmarried mothers in our data containing the father’s name and date of birth. In nationally representative data, births to unmarried mothers are twice as likely to be missing father information (Mincy et al., 2005).

We find that new fathers also exhibit substantial changes in criminal activity as a result of having children. Both in the raw data and difference-in-differences estimates, male arrests decrease sharply at the start of the pregnancy and remain at lower levels following the birth, with reductions around 20 percent for property, drug, and DUI arrests. As with mothers, the timing of fathers’ response suggests that pregnancy, not childbirth, is the primary inducement to decrease criminal behavior. The majority of the declines in fathers’ offending occurs 6-7 months prior to birth, when many soon-to-be fathers may first learn that their partner is pregnant. The results align closely with prior survey research, which suggests that many low-income men respond to pregnancy by radically reshaping their activities: “[M]en such as Byron are suddenly transformed. This part-time cab driver and sometime weed dealer almost immediately secured a city job in the sanitation department” (Edin and Nelson, 2013). Some research suggests that these effects should depend on whether the baby is male or female (Dahl and Moretti, 2008; Dustmann and Landersø, 2021). But when we split the sample based on infant sex, we find no differences in patterns of desistence for either mothers or fathers.

Not all changes brought on by family formation are positive, however. We find that men exhibit a large spike in domestic violence arrests at birth, with monthly rates increasing from below 10 arrests per 10,000 men in the months just before pregnancy to about 15 per 10,000 just after. This represents a 50-100% increase, depending on whether the change is compared to the lowest or highest point before birth. Eight percent of unmarried first-time fathers in our data are arrested for domestic violence some time in the two years following birth. These effects reverse half of the overall decline in arrests from other offenses, and are large relative to other known drivers of domestic violence. For example, Leslie and Wilson (2020) find that COVID-induced lockdowns increased domestic violence calls for service by 7.5%.

For both mothers and fathers, changes in offending after childbirth could result from a shift in preferences—a shift in time discounting to be more forward-looking, for example—or a temporary change arising from the time demands of raising young children. That men’s changes persist over several years points to an important role for preferences, since unmarried parents, who drive virtually all of the long-term crime declines, are highly likely to separate; five years after childbirth, only 18 percent are co-residing (Tach et al., 2010). An analysis of first- vs. second-time parents also supports a preferences interpretation, since the large permanent drops in crime are concentrated among first-time parents. In particular, there are no long-run effects for either mothers or fathers having their second child, despite short-lived declines starting with pregnancy.

To further probe the causal interpretation of our results, we supplement the difference-in-difference

evidence with results from an alternative strategy that isolates the effect of having a child by building a control group using records of stillbirths, which are reported if gestation exceeds 20 weeks. Though the sample size of stillbirths is small, these analyses reinforce the main findings. Relative to parents of stillborn children, fathers of liveborn children have increased domestic violence following the birth, whereas mothers and fathers of liveborn children show decreased arrest rates for property, drug, DUIs and other crimes. As in the main results, unmarried parents drive the effects. This suggests that having a child, and not just making the decisions that produce one, decreases most types of arrests and increases domestic violence.

In a final analysis, we turn to marriage, which is also a focus of the turning points literature (Sampson et al., 2006). The married parents in our sample are consistently less likely to be arrested for any offense, including domestic violence. To explore the effects of marriage itself, we construct similar difference-in-differences specifications that compare spouses' arrest rates around marriage to those of men and women who marry when they are older. For both sexes, crime decreases dramatically in the three years prior to marriage. This trend stops at the marriage date, after which offending is flat. Our data thus suggests that marriage marks the completion of a long relative decrease in crime, in line with the mothers quoted in Edin and Kefalas (2011) who want to settle down *before* marrying. Still, this analysis leaves open the possibility that romantic relationships more broadly construed can temper criminal behavior.

Our findings help clarify a large literature inspired by Sampson and Laub (1990)'s argument that key life events can serve as "turning points" that cause desistance by increasing social bonds. Their influential work reexamined data from Glueck and Glueck (1950), a longitudinal study of 500 delinquents in Boston, finding that spousal attachment, job stability, and economic aspiration were all associated with desistance. A large literature based primarily on smaller, selected samples builds on these results with conflicting findings, which we review in Table A.4. Most papers find no or minimal effects of motherhood on crime. Results for fathers have been similarly mixed.³ Though some prior work finds negative effects of marriage on crime, our result that long periods of desistance precede marriage suggests these effects may largely capture selection.⁴

Several more recent studies have used administrative data similar to ours to study the effects of marriage and childbirth on arrests. Most closely related are Skardhamar et al. (2014) and Skardhamar and Lyngstad (2009), which use Norwegian register data and find broadly similar trends at an annual level, but lack the ability to study the precise timing of the arrest reductions and address the possibility that coincident changes beyond family formation explain the observed desistance. Also related is Eichmeyer and Kent (2021), which provides complementary findings on the effects of parenthood for low-income mothers in a large county in Pennsylvania on a range of housing, healthcare, and government assistance outcomes. A smaller sample, however, limits

³For another recent review on mothers, see Giordano et al. (2011); for fathers, see Mitchell et al. (2018).

⁴For a critique and detailed review of the marriage effect, see Skardhamar et al. (2015).

precision and the time horizon over which effects can be measured.

The rest of this paper is organized as follows. [Section 2](#) describes the data collected and how it is linked together. [Section 3](#) presents the difference-in-differences strategy and the main effects on first-time mothers and fathers. [Section 4](#) analyses the impacts of second births. [Section 5](#) estimates effects by child sex, while [Section 6](#) analyzes domestic violence responses. [Section 7](#) provides evidence from stillbirths. [Section 8](#) analyzes arrests around marriage. And [Section 9](#) concludes.

2 Data

Our core analysis is based primarily on two administrative data sources from Washington state: the Washington State Institute for Public Policy’s criminal history database, a synthesis of data from the Administrative Office of the Courts (AOC) and the Department of Corrections (DOC); and still- and live-birth certificates from the Department of Health (DOH). We augment these data with Washington State marriage and divorce indexes acquired from the Washington State Archives, as well as additional DOC data covering incarceration and probation spells under state supervision.

The criminal history data covers every criminal charge made from 1992 to 2015, including the date of the alleged offense, the criminal code, and the name and date of birth of the defendant.⁵ We refer to a record in this data as an “arrest” or “charge” interchangeably, although some events may not involve apprehension by a police officer and jail booking (e.g., a citation for reckless driving).

The birth certificates span 1980 to 2009. The data includes the names and dates of birth of the mother and father, their races, the residential zip code of mother, and an indicator for whether the mother was married at birth. An average of 75,000 births happen every year in the sample period, for about one million births in total. To ensure that arrests are observed for a sufficiently long period before and after birth, we restrict to births after 1996. We also restrict to births when fathers and mothers are between 15 and 40 years old to focus on parents who are likely to be criminally active around the birth. Parents up to age 45 are used to estimate counterfactual arrest rates in the difference-in-difference strategy described below.

Washington is unusually good at recording fathers because it was one of the first states to implement in-hospital voluntary paternity establishment for unmarried mothers ([Rossin-Slater, 2017](#)).

⁵We also have access to a dataset covering all fingerprinted arrests from the Washington State Patrol’s Computerized Criminal History Database. Results change little when using this data instead or the union of two sources, though the State Patrol data contains less information on arrests and has known coverage issues ([Washington State Auditor’s Office, 2015](#)).

However, father’s information is still missing in about 5 percent of the birth certificates in the sample.⁶ We drop records without fathers’ names and dates of birth, since they cannot be matched to arrest data. In the primary analyses, we also restrict to the parent’s first birth as measured by matching parents within the birth records using the father’s full name and date of birth and the mother’s full (maiden) name and date of birth as reported on the birth certificates. Since the birth certificates begin in 1980, this means we will mislabel births as first births if someone in our sample had their first child in 1979 or earlier, implying a 17 year gap between births.

We also acquired separate records on stillbirths (i.e., fetal death certificates) from the Washington Department of Health covering the years 1997 to 2010. Stillbirths happen late in pregnancy and are only recorded if there were 20 weeks or more of gestation, after which hospitals are legally required to report them. There are about 500 stillbirths each year in our data, with an average estimated gestation of 29 weeks. These records include the full names and birth dates of the parents, allowing us to match them to the arrest data. However, some information reported on live birth records is missing for stillbirths, such as parent race and Medicaid enrollment status. Importantly for our purposes, stillbirth records have strong coverage of the fathers’ name and date of birth, which are only missing from 9 percent of observations.

We match arrest records to still and live birth records by implementing a fuzzy name match across parents and arrestees with the same date of birth. Records are considered as matched if the cosine similarity of 3-gram TF-IDF vectorizations of name strings is above 0.9.⁷ Mothers are matched based on both their maiden and legal names. We drop parents who are matched to multiple people in the arrest data, which tends to exclude a handful of very common names, but we include parents who have no matches at all. The never-arrested sample is kept to help identify age trends in the difference-in-difference analysis and so that the count results presented below can be interpreted as approximate population averages. The dropped records with ambiguously matched names constitute less than 10 percent of the birth certificates with fathers listed.

Finally, we combine state marriage and divorce records with our sample by merging them to birth certificates using a fuzzy string match of the combined names of the spouses. This match comes with the caveat that only couples who at some point have a child together will be included. Since the marriage certificates do not contain birth dates, married couples could not be linked to the arrest data without first linking to the birth certificates. When analyzing marriages, we use similar basic sample restrictions as in the analysis of births: marriages must fall between 1997 and 2010 and age at marriage must be between 15 and 40.

⁶Similar data in Michigan is missing the father in 16.5% of birth certificates (Almond and Rossin-Slater, 2013).

⁷TF-IDF stands for Term Frequency Inverse Document Frequency, a measure of how often a particular group of letters appears in the overall string or “document.” Traditional Levenshtein distance matching performs similarly, but is substantially slower than the TF-IDF approach, which can be computed efficiently using sparse matrix multiplication. Identical strings have a similarity of one, while strings with zero 3-grams in common have similarity zero.

Table 1 provides summary statistics on the main analysis samples of live- and stillbirths for first-time fathers and mothers. Most parents are white, but Hispanic and Asian parents comprise about 10% of births each.⁸ The average mother is 27 years old at birth, while the average father is about a year older. Over 70 percent of mothers are married at birth (the data do not specify whether they are married to the father). Parents of stillborn children have similar average ages to parents of live-born children. They live in zip codes with marginally lower median incomes, but are less likely to be on the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). Fathers are more likely to be arrested than mothers, with 34% (vs. 19%) acquiring at least one criminal charge in our sample period.

Table A.1 and Table A.2 shows how these sample characteristics change as we impose the restrictions mentioned above, starting with the entire sample of births in column (1). Column (2) restricts to parents aged 15 to 40; column (3) restricts to births where the parent is clearly matched (or not matched at all) to the arrest data; and column (4) adds the restriction that the birth is the parent’s first child. The final samples of first births are similar to the population of all births, though about two years younger on average. Omitting parents ambiguously matched to arrest records naturally decreases arrest rates.

The crimes represented in the data range from traffic infractions to murder. In most analyses, we focus on mutually exclusive groups of arrests based on categories constructed by the Washington State Institute for Public Policy. The main results focus on four groupings of crime categories: arrests that we call economic consist primarily of property crimes such as 3rd degree theft, 2nd degree burglary, trespassing, and forgery; drug crime categories include furnishing liquor to minors and possessing a controlled substance; driving under the influence, the most common arrest in the data, is treated as its own category; and destruction includes vandalism and property damage more broadly. In many analyses we simply consider an indicator for arrest for a crime in any of these four main categories.

Domestic violence arrests are analyzed separately because, as we show below, these offense have distinct patterns around childbirth. These arrests are most commonly fourth degree assaults, which is the least severe assault charge. We also omit a small share of other arrests that reflect ambiguous types of underlying activity. These include assaults coded as not related to domestic violence, since the coding appears to be unreliable, and obstruction of a police officer. We also separate driving-related offenses not related to DUIs, since these arrests are more likely related to levels of driving and commuting activity than criminal behavior. Figure A.1 shows that these restrictions are unlikely to substantively affect the results by plotting how arrest rates around birth change after successively removing these categories of crimes.

⁸Birth records record “Hispanic” as a distinct racial category as opposed to a separate measure of ethnicity.

3 Effects on first-time parents

3.1 Raw averages

We begin by plotting raw 30-day arrest rates for mothers and fathers in the three years before and after the birth of their first child using the main analysis samples described above. In this setup, $t=0$ marks the 30-day period beginning with the date of birth. Both this and all subsequent analyses use the date of the alleged offense, not the date of arrest, as the date of the criminal event. This partially addresses the concern that the offending patterns could be confounded if law enforcement officers are less likely to make an arrest in the case of a visible pregnancy.

Figure 1 Panel (a) shows arrest rates for mothers for our four primary categories of crimes. All series drop sharply during pregnancy and rebound slightly after birth. More specifically, they depict three consistent patterns: flat or slight positive trends leading up to the approximate date of the pregnancy (i.e., nine months before birth), large declines concentrated in the first few months of pregnancy, and a sharp rebound in arrests following the birth. Property and non-DUI drug arrests are lower than the pre-pregnancy averages three years after the birth, while DUI and property destruction arrests show less of a long-term decline.

Arrests related to alcohol and drugs show little evidence of anticipation ahead of the pregnancy. There are small declines in $t=-8$, when many mothers learn they are pregnant, and the largest decline in $t=-7$, by which time almost all mothers know (Branum and Ahrens, 2017). One reason could be that, based on self-report, pregnancy intention itself does not predict alcohol cessation (Terplan et al., 2014). However, another explanation is that not all pregnancies are intended and, as we explore below, these pregnancies likely drive our results.

Figure 1 Panel (b) shows the average monthly arrest rate of first-time fathers for the same four crime categories. Arrest rates for fathers are substantially higher than mothers' arrest rates, but show similar patterns.⁹ There are large drops in these raw averages after conception, especially for drug arrests. Between pregnancy and three years after birth, monthly drug arrests fall from over 20 to roughly 15 for every 10,000 men. Arrest rates remain substantially lower longer after birth. Economic (i.e., property) crimes show similar patterns. Arrests for DUI and destruction crimes, which include property vandalism and damage, are more rare but follow similar trends.

⁹According to the Federal Bureau of Investigation's Persons Arrested report, men accounted for 77% of all arrests, 69% of arrests for property crimes, and 82% of arrests for drug abuse violations nationally in 2002, approximately the mid-point of our sample (Federal Bureau of Investigation, 2002, Table 42). The numbers in Figure 1 suggest slightly lower male shares of arrests, e.g., about 66% for property crimes and 74% for drug crimes. The discrepancy could be due to several factors, including conditioning on parenthood and appearance on the birth record, our age restrictions, or the fact that our outcome is an indicator for *any* arrest within the month.

3.2 Difference-in-differences strategy

The simple raw averages provide clear evidence that pregnancy coincides with sharp changes in crime, but it is difficult to gauge long-run effects without accounting for the fact that women and men may be maturing independent of childbirth. We next introduce an event study framework that accounts for aging by comparing parents' arrest rates around childbirth to older parents' arrest rates at the same ages. We build an estimator that directly averages many such comparisons for every age-at-birth cohort of parents, avoiding potential complications with conventional staggered difference-in-differences estimators.¹⁰

The first panel of [Figure 2](#) illustrates the approach using mothers. The solid blue line plots monthly arrest rates for any crime in the four categories shown in [Figure 1](#) over the three years before and after childbirth, averaging all mothers without adjustment. The remaining lines plot average arrests rates of older mothers at the same ages as the focal mothers in the blue line. For example, if we were only examining patterns for mothers who have a child at age 21, the mothers line would plot their arrest rates over ages 18 to 24. The 1-2 years series would plot the arrest rates of mothers who have a child when 22-23 years old over the same ages, with zero on the x-axis still corresponding to their arrest rates at 21. The 2-3 years line plots the same for mothers who have a child at age 23-24, the 3-4 years line for mothers at age 24-25, and so on. Each comparison line stops nine months prior to when the youngest mother in the group would have a child themselves, ensuring that the means plotted correspond to average outcomes for a balanced sample of older mothers before their pregnancy. We construct these comparisons for each age-at-birth cohort of mothers and take the weighted average to produce the figure.

[Figure 2\(a\)](#) shows that focal mothers are arrested more often than older mothers, as should be expected given that women who give birth at older ages generally have lower arrest rates.¹¹ Despite level differences in arrest rates between mothers and older mothers, trends over age track closely across groups, especially for groups who gave birth when closest in age. Nine months before birth, mothers see sudden drops in arrests not shared by older mothers. Arrest rates then rebound sharply, but a year after birth have converged to that of women who have a child when two to four years older. In other words, childbirth shifts mothers' age-crime profile down to match that of women who have their first child when they are two to three years older.

Because older mothers can only be used to estimate counterfactual age trends before they become pregnant themselves, obtaining longer-run estimates requires using groups of mothers who give birth increasingly later in life and who are thus potentially more different from the focal mothers. Nevertheless, [Figure 2](#) shows that overall age trends for older comparison mothers are similar to

¹⁰For surveys covering these issues, see [Roth et al. \(2022\)](#) and [de Chaisemartin and D'Haultfœuille \(forthcoming\)](#).

¹¹Arrest rates are higher than the levels in [Figure 1](#) because the outcome is an indicator for arrest for *any* of the four main crime categories.

focal mothers’ trends, despite larger level differences. To obtain estimates of long-run effects up to three years after birth, women who give birth when at least three years and nine months older must be included as potential comparisons. Although effects could be estimated by making comparisons to these women alone, we use all women who give birth when one to five years older than focal mothers for precision. Results change little using alternative subsets of older mothers.

To construct point estimates of effects implied by these comparisons, we use a simple difference-in-differences estimator. Specifically, for each age-at-birth cohort c , we construct a panel of outcomes covering the ages three years before and after c . We stack this data onto a panel of outcomes over the same ages for all women who had their first child when they were between one and five years older than c , dropping all data after nine months prior to their own birth. We then estimate using ordinary least squares:

$$\mathbb{1}(\text{arrest})_{ia} = \alpha_a + \gamma_{g(i)} + \sum_{k \in [-36, -8] \cup [-10, 36]} \delta_{kc} \mathbb{1}(a - c = k) T_i + e_{ia} \quad (1)$$

where $\mathbb{1}(\text{arrest})_{ia}$ is an indicator for arrest at age a for mother i , α_a is a set of age effects, $\gamma_{g(i)}$ is a set of age-at-childbirth (in months) fixed effects, and $T_i = 1$ if the observation is a focal mother and 0 if the observation is for a comparison mother.

This specification corresponds to a standard difference-in-difference estimator, with the first difference comparing “treated” mothers who have a birth at age c to groups of older mothers, and the second difference comparing changes in arrest rates over age. The coefficients δ_{kc} in the summation measure differential changes in age-specific arrest rates for focal mothers relative to older mothers.¹² We normalize by dropping $k = -9$, so that all effects are measured relative to nine months prior to birth. We then repeat this data construction procedure for every age-at-birth cohort (measured in months) and average the δ_{kc} coefficients for each k , weighting by the number of focal mothers in each cohort c .¹³

3.3 Effects on mothers

Panel (b) of Figure 2 plots our primary estimates for first-time mothers, normalized by the mean arrest rate nine months before birth so that effects can be interpreted as proportional changes. Consistent with the patterns in Panel (a), mothers see flat arrest rates until nine months before birth, when arrests drop precipitously by nearly 100%. There is a slight rebound after birth and arrests eventually stabilize at levels 50% lower than rates nine months prior to birth. Arrest rates

¹²Effects of the indicators $\mathbb{1}(a - c = k)$ not interacted with T_i are co-linear with the age and age-at-birth effects.

¹³In practice, we stack the data sets of focal and comparison mothers for every cohort and estimate a single regression that interacts α_a and $\gamma_{g(i)}$ with a set of indicators for each comparison set. Standard errors are clustered over i , allowing us to account for the fact that mothers appear repeatedly in the stacked data.

then remain depressed for the next three years.¹⁴

These patterns will reflect the causal effects of pregnancy and childbirth if the onset of pregnancy does not coincide with other shocks that also affect arrests (e.g., the beginning of a romantic relationship) and if the age-crime trends of older parents provide an appropriate counterfactual for mothers' arrests rates in the absence of a pregnancy. The latter requirement is supported by the age trends shown in Panel (a). The former is supported by both panels of Figure 2, which show no evidence that pregnancy is anticipated by other arrest-reducing life changes for the mothers. We might have expected a decrease in crime ahead of pregnancy, reflecting the impact of mothers meeting potential fathers and reducing their criminal activity as a result. Instead, decreases in arrests coincide exactly with the onset of pregnancy.

This result also implies that it is unlikely that the effects reflect the *decision* to try to become pregnant rather than pregnancy itself. If decisions were an important time-varying omitted factor, we would expect at least some decided couples to fail to become pregnant immediately, generating dips in arrests before $t=-9$. Moreover, survey evidence suggests that the majority of births to unwed mothers, who as we show below drive our results, are unplanned (Mosher et al., 2012). And we obtain very similar results among teen mothers, for whom 78% of pregnancies are unintended (Mosher et al., 2012).¹⁵ For these populations, the exact timing of pregnancy is very likely a surprise, supporting the attribution of drops in arrest rates to the effects of pregnancy and the use of older mothers to estimate a counterfactual age-crime profile.

The magnitudes of arrest declines around childbirth are large compared to the effects of any known policy intervention. Causal evidence on most interventions has rarely estimated effects for men and women separately (Loeffler and Nagin, 2022). In combined samples, Rose and Shem-Tov (2021) find that an additional year of incarceration decreases the likelihood of any new offense within 5 years by 13% and cumulative new offenses by 14%. Chalfin and McCrary (2017) estimate the elasticity of property crime with respect to police manpower at -0.2, although estimates vary. In some of the largest effects in the literature, Heller (2014) find a 28-35% decrease in arrests for disadvantaged youth participating in a cognitive behavioral therapy program. A successful pregnancy appears to rival all of these interventions, consistent with the evidence from prior qualitative work. Unmarried mothers interviewed by Edin and Kefalas (2011), for example, frequently report that children changed their lives for good. "My kids, they've matured me a lot...I've always stayed off of drugs for them" (p. 130).

Figure A.3 Panel (a) reports the difference-in-differences estimates for the four main categories of crime underlying the main effects in Figure 2. The largest declines in pregnancy and in the

¹⁴Figure A.2 Panel (a) presents the results from a more traditional event-study specification that includes indicators for the 36 months before and after birth and additive controls for age. These estimates show similar patterns to the main effects, although pre-pregnancy increases are slightly more pronounced.

¹⁵These results are discussed in Section 3.3.3.

long-run occur for the the most common types of arrests, those for economic and drug crimes. Drug arrest show a particularly striking pattern; relative to the raw means in [Figure 1](#), post-birth declines reflect decreases in on the order of 50%. DUI and destruction arrests show similar patterns, but smaller pregnancy decreases and long-run differences in arrest rates, consistent with their overall lower pre-pregnancy prevalence.

3.3.1 Pregnancy termination

Our findings only apply to pregnancies that are carried to term. However, many pregnancies end with abortions. During our sample period, around 20 percent of pregnancies are estimated to have ended in abortion ([Finer and Henshaw, 2006](#)). This share is higher for younger, low-income women and for unintended pregnancies. However, recent evidence suggests that even among women who experience an unintended pregnancy, the majority do not receive an abortion: In 2008, 51% of pregnancies were unintended and 41% of unintended pregnancies resulted in termination ([Finer and Zolna, 2014](#)).

Criminal offending patterns for couples who elect to terminate their pregnancy may differ from the patterns for the mothers and fathers in our sample. Although the decision to carry a pregnancy to term is endogenous, we view timing of the pregnancy itself as plausibly exogenous given the lack of anticipation in the difference-in-difference estimates and the survey evidence on intendedness. Removing age effects, the before-after comparison implicit in our difference-in-differences estimates therefore identifies the causal effect of childbearing for couples who elect not to terminate a pregnancy. While the overall effects of pregnancy are also interesting, we view these effects as likely to be attenuated versions of those measured here unless couples who terminate exhibit substantially opposite patterns.

3.3.2 Alcohol offenses

Unlike the other three categories of crime, the raw averages of DUI arrests in [Figure 1](#) Panel (a) eventually return to pre-pregnancy levels. This appears to be due to the fact that women are more likely to be driving after having their first child. Partial evidence for this idea is that more innocuous arrests related to driving, such as driving without a license, increase steadily over the sample period (see [Figure A.4](#)). But what can we say about drinking behavior independent of the propensity to drive? For more insight on this question, we turn to the most common alcohol-related arrests for people under the age of 21: alcohol possession. We perform our difference-in-difference analysis for women who become mothers at or before the age of 20, which brings the sample size

down to 69,539 mothers.¹⁶ The plot of difference-in-differences effects on these alcohol arrests is shown in [Figure A.5](#). Similar to the non-alcohol drug arrests in the previous plot, the figure suggests a sharp, largely sustained desistance at the beginning of pregnancy. Thus, at least for this subgroup where we have a measure of drinking that is unconfounded with driving, there is a clear decline.

3.3.3 Teen mothers

Economists actively debate the consequences of teen pregnancy, which is uniquely high in the United States compared to peer countries in Europe ([Hoffman, 2008](#); [Kearney and Levine, 2012](#)). Influential research using miscarriage as an instrument finds minor negative and even some positive effects of teen childbearing ([Hotz et al., 2005, 1997](#); [Ashcraft et al., 2013](#)).¹⁷ However, [Fletcher and Wolfe \(2009\)](#) use a similar empirical design with different data and find strictly negative effects on education and income, leading to a recent summary that the “[n]egative consequences of teen childbearing are well documented” ([Yakusheva and Fletcher, 2015](#)).

We estimate effects of childbirth on teen mothers, defined as women who give birth before turning 20, using the same strategy as above. We plot the coefficients from the difference-in-differences specification for the four main crime categories in [Figure A.6](#), where the coefficients are normalized by the pre-pregnancy average to give the fractional change in arrest rates. Motherhood remains a large driver of desistance for this subgroup. As in the full sample, arrests show a sharp and largely sustained decreases to half of the pre-pregnancy levels. These results are also informative because 78% of teen mothers report that their births resulted from unintended pregnancies ([Mosher et al., 2012](#)), mitigating concerns that effects reflect the endogenous choice to become pregnant rather than pregnancy itself. The results provide perhaps the clearest evidence to date that childbearing is a turning point for even very young women.

3.4 Effects on fathers

While mothers’ arrests show striking changes around pregnancy, many of these shifts may reflect the immediate physical effects of pregnancy, as well as changes in potential legal and social sanctions for drug and alcohol abuse while pregnant. Fathers’ responses might better isolate the social or psychological changes that result from parenthood, since they are less affected by pregnancy

¹⁶As above, we continue to use mothers who have children later to define a counterfactual. However, all comparisons in our strategy are made between arrest rates of women who are the same age and thus subject to the same alcohol laws.

¹⁷For an overview of the causal effects of teen childbearing, see [Kearney and Levine \(2012\)](#), who conclude that “most rigorous studies on the topic find that teen childbearing has very little, if any, direct negative economic consequence.”

both physically and legally. As shown in Panel (b) of [Figure 1](#), men appear to also see declines in arrests after pregnancy that are sustained long after childbirth. But how robust are these patterns to accounting for the general age-crime profiles of first-time fathers?

[Figure 3](#) repeats the difference-in-difference exercise described above for all first-time fathers. Panel (a) shows that, as was the case with women, men who have children when they are younger generally have higher arrest rates than men who have children when they are slightly older. On average, men experience relatively stable or slightly decreasing arrest rates over the ages before childbirth, and older fathers show similar changes in arrests rates over the same ages. Despite the level differences, fathers and older fathers' arrest rates track each other closely, suggesting the latter may provide an appropriate counterfactual for arrest rates in the absence of childbirth. Pregnancy triggers sharp declines in arrests not shared by older fathers at the same ages. There is a slight rebound after birth, but the net effect of pregnancy is to knock fathers' age-crime profile down to the same level as men who have children when they are 1-2 years older.

Panel (b) of [Figure 3](#) plots estimated proportional effects on arrests from [Equation 1](#) based on these comparisons. The pregnancy decline constitutes a roughly 30% decrease in arrest rates. Arrest remain roughly 20% lower than pre-pregnancy levels over the next three years. As with mothers, the results show little evidence of any anticipatory responses. There are small declines in $t=-8$ and larger declines in $t=-7$ and $t=-6$, when many men may learn from their partners that they are expecting.¹⁸ It appears that men respond to this news by sharply altering their activities. These results are consistent with an observation in [Edin and Nelson \(2013\)](#): even in the case of unplanned pregnancies, men respond to the news with happiness. The researchers asked young, low-income fathers how they responded to the news of the pregnancy. “Unadulterated happiness—even joy—was by far the most common reaction...” (p. 68).

Men's declines in arrests compare favorably to the deterrent effects of exceptionally harsh punishments. For example, under California's three-strikes law, offenders with two strikes faced almost 20 years of additional prison time and exhibited a decrease in annual felony offenses of 15 to 20 percent ([Helland and Tabarrok, 2007](#)). In Italy, [Drago et al. \(2009\)](#) find that an increase in expected sentences among recently released prisoners by 25 percent would decrease re-arrests in 7 months by 18 percent. Our results on arrest rates are not directly comparable to estimates of recidivism for people recently released from prison. However, the probability of any arrest in a longer period shows the same large decline: among all of the first-time fathers in our sample, the share arrested for any drug offense goes from 1.7 percent in the year before pregnancy to 1.2 percent in the year after birth.

¹⁸[Figure A.2](#) Panel (b) presents the results from a more traditional event-study specification that includes indicators for the 36 months before and after birth and additive controls for age. Results change little, although long-run declines are marginally larger.

Figure A.3 Panel (b) reports the difference-in-differences estimates for the four main categories of crime underlying the main effects in Figure 3. The largest declines are for drug offenses, which decrease sharply during pregnancy and continue to decline afterwards. All other arrest categories show similar patterns, despite their large differences in baseline arrest rates shown in Figure 1. In terms of proportional effects, therefore, the smallest impacts are for economically motivated crimes, perhaps suggesting that income generation remains a priority for newly minted fathers.

Taken together, the results for first-time fathers suggest large positive changes concentrated during pregnancy, despite the fact that men do not directly experience any of its physical effects. While new to the quantitative literature, this response is consistent with qualitative research asking at-risk fathers how they reacted when they learned about a partner’s pregnancy. Edin and Nelson (2013) note that, “Men are drawn in—usually after the fact of conception...[and] usually work hard to forge a stronger bond around the impending birth” (p. 203). Fathers interviewed say they would “probably be in jail” or “out getting high” without their children (p. 74).

Sustained lower arrest rates are even more surprising in light of the fact that unmarried parents, who we show below drive the majority of these effects, are highly likely to separate; five years after childbirth, only 18 percent are co-residing (Tach et al., 2010). As co-residence declines, fathers may be less economically tied to their children and shoulder fewer child rearing responsibilities. Despite this, we see no increases in arrests rates. Family formation thus appears to be a persistent “turning point” for crime. This finding supports the view that having a child shifts preferences over criminal activity rather than simply causing temporary incapacitation effects, an idea we return to in our discussion of the effects of first vs. second births below.

3.5 Married vs. unmarried parents

Marital status at birth has long been a focal metric of policy makers. The descriptive statistics in Table 2 show clear differences in the probability of arrest and incarceration across the two samples. Unmarried fathers are twice as likely to have ever been arrested, and seven times as likely to have had an incarceration spell. Since married couples are already less prone to crime, the additional effect of childbirth may have a less stabilizing effect. On the other hand, single and cohabiting mothers experience a large negative shock to their income-to-needs ratio (Stanczyk, 2020), which could increase economic offenses similar to effects found for individuals who have exhausted food stamps (e.g. Carr and Packham, 2019).

Figure 4 presents difference-in-difference estimates of the effect of family formation splitting parents by the marital status reported on the birth certificate. We scale effects to correspond to arrest per 10,000 parents and add the omitted-period average to help make the stark level differences between married and unmarried parents clear. While both groups show pregnancy declines, the

size of the drops for unmarried fathers and mothers dwarfs married parents' changes. Moreover, arrest rates for married parents return to similar levels as before the birth after several years, while unmarried parents see permanent declines.

As in the main results, there are no signs of anticipation ahead of the pregnancy for either group. This might be expected for unmarried parents, where more than half of all births are unintended. However, for married parents only 23 percent of births are unintended (Mosher et al., 2012, Table 2), and many couples spend months trying to conceive (Keiding et al., 2002). The patterns in Figure 4 can thus be viewed as further evidence that the decision to have a child does not influence criminal activity. However, it could also be that the criminally-active married women who drive the estimates are much more likely to have unintended pregnancies.

3.6 Migration

An important potential confound in our setting is migration in or out of Washington State. Defining our sample by conditioning on a birth in Washington implies parents are most likely to be physically present in Washington around the time of conception. Since our data only cover arrests in Washington, it is possible that post-birth declines reflect migrations out of the state—and therefore unobservable attrition.¹⁹ The most immediate argument against this threat is the clear increase in domestic violence following the birth that we discuss further below. For migration to explain the decrease in other arrests, the men accounting for the spike in domestic violence would need to have a much lower propensity to be arrested for other offenses. However, arrests are correlated across offense types: men with more drug arrests tend to have more domestic violence arrests as well, for example.

As a more direct test of robustness to outmigration concerns, we estimate effects on men with greater attachment to the state in the post-birth period by restricting the sample to the 69,900 fathers who commit a DUI or traffic offense in the endpoints of our sample, i.e., 4-5 years after the birth. In Panel (a) of Figure A.11, we show that this sample, which should be much less contaminated by migration attrition, shows a similar 20 percent decrease in drug arrests. Panel (b) shows that we also find very similar effects on the sample of first births for men who ever have a second child in Washington State. If migration were driving the results and fathers physically present in Washington had stable levels of arrest rates post-birth, we would expect the decrease for both these group to be substantially smaller.

Similarly, it is possible that migration into Washington affects our estimates of arrest rates before pregnancy. Figure A.12 explores this concern by estimating effects on two sub-samples with pre-

¹⁹Incarceration poses an analogous attrition problem as men in our sample are least likely to be in prison ten months before the birth; results using only never-incarcerated fathers are identical, however.

existing activity in Washington and that we expect to be less affected by potential in-migration: men who, according to a match within the birth records, were born in the state (Panel (a)), and men who have a juvenile offense recorded in our data (Panel (b)). Both sets of results show similar patterns to the main effects, with flat pre-trends in the lead up to conception and sharp declines during pregnancy that are sustained for several years after birth.

4 First vs. second births

The results so far are consistent with two broad explanations. First, childbirth could initiate a permanent change in preferences. For instance, having a child could cause people to derive less utility from drug use or crime, or make them more future-regarding. An alternative explanation is that childbearing affects crime purely through its effect on time and resource budgets. The presence of a young child could create a temporary incapacitation effect due to childcare or housework, or force parents to reallocate activity in order to support them. One simple way to attempt to discriminate between these two hypotheses is to compare effects of first births to the effects of second births. A preferences-based explanation predicts that most changes should be concentrated in the first birth, while a transitory time and budget shocks channel suggests similar effects regardless of birth order.

In [Figure 5](#), we construct the same difference-in-differences estimates described above splitting the sample by first vs. second birth. In order to use a consistent set of parents in both figures, the underlying data retains all mothers and fathers whose first and second children are both born in the sample period. Older parent counterfactuals are constructed exactly as above, except that older parents also must have at least two births in the sample window and when examining effects of second births we use parents who have a second birth when between one and five years older without conditioning on the timing of the first birth.

The results show that, for both mothers and fathers, the bulk of desistance happens at the first birth. Three years after their second birth, mothers are arrested at levels similar or slightly higher to before pregnancy. Fathers experience a brief decrease after second birth that is not sustained, compared to a permanent 25-30 percent decrease after the first birth. The lack of any long run changes after a second birth for fathers is especially notable due to the fact that some men only start investing in children for later births, while this is less common for women ([Edin and Nelson, 2013](#)). Taken together, however, the results for both fathers and mothers are more consistent with the idea that becoming a parent permanently changes preferences.

5 Boys vs. girls

A preference-based explanation might also suggest the effects of childbirth would depend on the sex of the child. Previous studies have shown the importance of son preference for fathers (Dahl and Moretti, 2008), including in the degree of criminal desistance (Dustmann and Landersø, 2021). We replicate this analysis in our data by studying the cumulative offending rates of fathers and mothers split by sex in Figure 6. We focus on unmarried parents since they showed the largest response in the preceding heterogeneity analyses; the results are very similar for married parents, however.

Panel (a) shows cumulative arrests rates beginning five years before birth for mothers, split by infant sex and using a monthly indicator for any of the four main offending categories from Figure 1 as the crime outcome. A slight visual difference is present between mothers to daughters compared to sons: at 36 months after the birth, mothers to male infants have 0.006 more cumulative months with offenses, a 1.7% increase compared to the average of 0.349 for mothers to daughters. However, this difference is small and insignificant.

Panel (b) shows the same series for fathers to sons compared to daughters. The trends are nearly identical. At 36 months, fathers to sons have 0.909 cumulative months with offenses compared to 0.905 among fathers to daughters. These similarities persist when we study more at-risk subsamples such as fathers under the age of 20 (as in Dustmann and Landersø (2021)), and with other outcomes such as domestic violence. Taken together, this suggests that the infant’s sex has no bearing on the mother’s or, perhaps more surprisingly, the father’s criminal desistance.

6 Domestic violence

We next turn to a critical caveat to the previous “turning points” findings that, to our knowledge, has not received any explicit mention in the host of quantitative studies on crime and family formation. The decline in economic, drug, and DUI arrests for men around childbirth coincide with a large increase in domestic violence.

Figure 7 shows raw averages for domestic violence arrests among fathers in the full first birth sample, along with arrest rates for comparison older fathers. Domestic violence arrests increase up until the start of the pregnancy, decrease sharply, and then markedly spike in the month of the birth. The increase leading up to $t=-9$ may reflect conditioning on childbirth at $t=0$, as relationships and hence opportunities for domestic violence increasingly form ahead of the pregnancy. The decrease during pregnancy appears consistent with norms against assaulting pregnant women, when violence may also harm the developing fetus (Currie et al., 2018). Finally,

the spike at birth might help explain why recent studies found ambiguous effects of fatherhood on overall arrest rates (e.g. [Mitchell et al., 2018](#)). In [Figure A.8](#), we show, also using the raw averages, that a similar spike is visible around marriage.²⁰

Our data measure arrests with a high degree of accuracy, but the connection between arrests and violent behavior over the sample period is less certain if the propensity to report domestic violence changes after pregnancy and childbirth. Victimization surveys, which may more accurately track changes in behavior compared to measures based on law enforcement involvement, confirm the qualitative finding that domestic violence is more likely after the pregnancy than during: in a nationally representative survey, 1.7 percent of mothers reported physical violence during the pregnancy compared to 3.1 percent in the first postpartum year ([Charles and Perreira, 2007](#)).²¹ Even if some share of the arrest spike is driven by changes in reporting, the results clearly show that pregnancy generates large increases in criminal justice contact due to domestic violence complaints, itself an important policy outcome.

Other results suggest changes in behavior and not simply reporting drive these estimates, however. In particular, domestic violence is strongly linked to the likelihood of subsequent divorce. [Figure A.9](#) Panel (a) shows father’s domestic violence arrests split by divorce status five years later, normalized by pre-pregnancy means to account for large level differences between the two groups. Despite similar pre-trends, men destined for divorce show a much larger spike in domestic violence arrests following the birth. [Figure A.9](#) Panel (b) focuses on these divorced men, grouping them based on whether they divorced 1, 2, 3 or 4 years after the birth. The plot shows clearly that domestic violence spikes ahead of the divorce decree.

7 Evidence from stillbirths

The preceding sections provide evidence on the causal impact of childbirth assuming the onset of pregnancy does not coincide with other time-varying confounds and that older parents’ arrest rates can be used to construct an accurate counterfactual. In this section, we probe the robustness of these results using an alternative design that compares parents’ post-birth arrest rates to the outcomes of a sample of couples whose pregnancy ends in a late-stage miscarriage. If, in line with the previous results, the outcome of the pregnancy has a causal effect on arrests, parents of stillborn infants should show relatively higher rates of arrests post-pregnancy.

²⁰[Figure A.7](#) plots mothers’ domestic violence arrests around childbirth. Women are around four times less likely to be arrested for this crime in the three years after childbirth. Nevertheless, mothers show a drop to near-zero arrest rates around childbirth that rebound to pre-pregnancy levels shortly afterwards.

²¹Further, in an interview, a Seattle police officer said that the presence of children would not affect the likelihood of an arrest due to Washington’s strict mandatory arrest law. However, the evidence here is indirect, and a recent meta-analysis concluded that “the research community still does not know for sure whether pregnant women are at higher or lower risk of being physically abused” ([DeKeseredy et al., 2017](#)).

On the other hand, stillbirths are far less common than miscarriages and often have distinct causes affecting the health of the mother such as pre-eclampsia, bacterial and viral infections, other medical conditions, and possibly domestic violence (Lawn et al., 2016). Further, the experience of a stillbirth is often followed by a period of bereavement (Heazell et al., 2016). Some of the differences in arrests between parents to still- and live-born children may thus reflect the effects of losing a child rather than having one.²²

The last two columns in Table 1 describe the differences between the stillbirth sample and our primary analysis sample, restricting to stillbirths where the parents have a clear match in the arrest data and that are the mother’s or father’s first birth. Mothers to stillborn babies are 6-10 percentage points less likely to be married but are otherwise positively selected on characteristics that predict arrest risk, such as receipt of WIC. Mothers in our data who experience stillbirths exhibit greater variance in age than mothers to liveborn children, and the infants are likely to be male and twins, in line with medical studies on risk factors (Lawn et al., 2016). Parents of stillborn children are also less likely to be arrested, on average.

To illustrate the variation used to estimate effects, Figure 8 plots arrest rates for unmarried parents of liveborn and stillborn infants around birth. We aggregate time periods to the six-month level to reduce noise in the smaller stillbirth sample. The outcomes are indicators for whether any arrest for the specified offense occurred in the six-month period. To remove level differences between the two groups, we show differences relative to the pre-pregnancy average. We focus on unmarried parents following the main analysis presented above, which shows that effects of childbirth are concentrated in this group of parents.

Panel (a) of Figure 8 shows that for fathers, arrest rates for drug, DUI, economic and property crimes in each group closely follow each other up to pregnancy then subsequently diverge, with fathers of liveborn children showing substantial declines relative to parents of stillborn children. Childbirth thus appears to cause a large decrease in arrests, consistent with the results in the primary difference-in-differences analysis. Panel (b) shows that domestic violence arrests also trend similarly for both groups in the lead up to pregnancy. But they diverge afterwards, with parents to liveborn children showing significantly more domestic violence arrests in the months after birth. Finally, Panel (c) shows that mothers’ arrests also follow patterns consistent with the main results, with large gaps between the two groups emerging post-birth.²³

Since arrests are rare and our stillbirths sample is relatively small, we quantify these effects in a simplified difference-in-differences specification estimated on the panel of outcomes three years before and after each still and live birth collapsed to six-month periods. We estimate the following

²²We find similar effects looking at periods 6 months or more beyond birth, when such effects may be attenuated, however.

²³Mothers’ domestic violence arrests are too rare in the stillbirth sample for meaningful comparisons.

regression specification:

$$Crime_{it} = \alpha_i + \sum_{k \in \{-2, -1\}} \delta_k \mathbb{1}(t = k) + \delta_1 AfterBirth_{it} + \delta_2 AfterBirth_{it} \cdot Live_i + \mathbf{X}'_{it} \beta + \epsilon_{it} \quad (2)$$

where $Crime_{it}$ is either an indicator for any arrest or the count of arrests in six-month period t before/after the birth, α_i indicates person fixed effects, and the series δ_k captures the two six-month periods overlapping with pregnancy (i.e., the year before birth). We include these pregnancy effects to exclude any temporary declines during pregnancy from the pre-post comparison. $AfterBirth_{it}$ is an indicator for $t \geq 0$, and the indicator $Live_i$ is equal to 1 for normal births and 0 for stillbirths. There is no main effect for $Live_i$ because we include only first-time parents, making it colinear with the person fixed effects. The vector $\mathbf{X}_{it}$ includes a polynomial in age and dummies for being above age 18 and 21. Standard errors are clustered by person.

The results for fathers reported in [Table 3](#) show the same patterns of offending declines as in the main results. Columns (1) and (3) report results for all first-time fathers, while columns (2) and (4) restrict to unmarried fathers. The outcome in columns (1) and (2) is a count of arrests. In columns (3) and (4), the outcome is a binary indicator for having any arrest. Panel (a) shows that birth generates large decreases in arrests for fathers to liveborn children, and that this difference is especially pronounced among unmarried fathers. For instance, column (4) shows that unmarried fathers to liveborn children have a roughly 1 p.p. relative decrease in the probability of arrest in the months after birth, about 17% of the outcome mean. By contrast, Panel (b) shows that liveborn fathers experience sharply elevated rates of domestic violence arrests after birth, consistent with the previous results.

[Table 4](#) shows results of the same exercise for first-time mothers' economic, drug, destruction, and DUI arrests. The results show large post-birth declines in arrests for mothers to liveborn infants, mirroring the findings in the main analysis. Across columns, both the quantity of arrests and the monthly arrest rate decline relative to mothers of stillbirth children. For instance, column (4) shows that unmarried mothers experience a relative 0.7 p.p. reduction in their six-month arrest rates. This point estimate is slightly smaller as a fraction of the pre-birth mean than the preceding estimates in [Section 3.3](#), though confidence intervals include effects as large as 40% of the mean. Taken together, the results for both mothers and fathers support the interpretation that changes in arrest rates around childbirth reflect the causal effects of pregnancy and family formation.

8 The role of marriage

As noted earlier, there are large level differences in criminal arrests between parents who are married vs. unmarried at birth. Marriage itself is a prominent feature of the turning points framework (Laub et al., 1998). In qualitative studies, formerly delinquent men often attribute considerable weight to marriage: “If I hadn’t met my wife at the time I did, I’d probably be dead. It just changed my whole life...that’s my turning point right there” (Sampson and Laub, 2009, p. 41). Marriage is also emphasized in some economics research. For example, a long literature debates the content of the male marriage wage premium (e.g., Antonovics and Town, 2004).

To analyze criminal arrests around marriage, we take a similar approach to our analysis of child-birth. Figure 9 plots arrest rates for women and men in the three years before and after marriage, along with arrest rates for older spouses over the same ages. The series are constructed exactly as in Figure 2 and Figure 3, but using date of marriage to define cohorts instead of date of first birth. Older spouses’ outcomes are kept until the month of marriage, and each counterfactual line stops at the age when the earliest spouse in the group would be married. For example, the line for spouses who marry 1-2 years after focal spouses stops at $t = 12$, the age when those who marry when one year older than the focal spouses would celebrate their nuptials.

Both panels show that husbands and wives have similar arrest patterns in the run up to their marriage, albeit at starkly different levels. Men are nearly three times as likely to be arrested. Though both groups exhibit similar arrest rates to future spouses over the ages three years before their marriage, roughly two years prior a steady decline in arrests begins, bringing arrest rates ultimately below 0.05 percent for women and below 0.2 percent for men. Arrests rates then flatten out as future spouses continue their declines in anticipation of their own marriages.

We omit difference-in-differences estimates of these effects, since the patterns are clear from Figure 9. Marriage itself marks that end of a long-period of desistance rather than a turning point for criminal behavior. These results are consistent with reports from adults studied in the qualitative literature, where many subjects state that they view marriage as an outcome of financial success and relationship stability. In a representative comment, one subject says she would get married “[a]fter I have a house and a car and everything, and I’m financially stable” (Edin and Kefalas, 2011, p. 93). Surveys using larger samples find that stringent financial “prerequisites” for marriage are set by unwed couples (Gibson-Davis et al., 2005).

Still, some research has largely interpreted marriage effects as causal.²⁴ For instance, in recent work, Sampson and Laub (2009) write: “Selection into marriage appears to be less systematic than many think...[m]any men cannot articulate why they got married or how they began relationships, which often just seemed to happen by chance.” The plots suggest clearly that romantic

²⁴See Skardhamar et al. (2015) for a critical assessment, however.

partnerships are important, demarcating a large decrease in arrests, but the association could be either because of the relationship or other factors simultaneously decreasing crime and increasing the probability of marriage. In support of the relationship mechanism, [Sampson and Laub \(2009\)](#) note that some women condition marriage on men’s social behaviors: “Before marriage, Leonard’s wife also told him directly, ‘Your friends or me.’”

8.1 Good marriages, bad marriages

Economic models going back to [Becker et al. \(1977\)](#) posit that divorces happen in response to negative information about the expected gains from the union (for a more recent example see [Charles and Stephens, 2004](#)). In sociology, a core tenet of turning points theory is that marriage itself does not guarantee desistance—relationships are salutary to the extent that they are characterized by high attachment ([Sampson and Laub, 1992](#)). The turning points theory plainly predicts that desistance should be less pronounced for bad marriages. The model in [Becker et al. \(1977\)](#) implies that divorce should be preceded by some negative surprise.

To explore these ideas, we combine our data with statewide divorce data from Washington. We plot descriptive statistics for married couples and those who divorce within five years in [Table A.3](#). Parents who get divorced are younger, reside in poorer zipcodes, and are more likely to be white or black (and less likely to be Hispanic or Asian). Perhaps most importantly, men and women who are headed for divorce are both about twice as likely to have any arrest.

[Figure A.10](#) plots arrest patterns around births for still-married and eventually divorced couples. We compare couples still married five years after birth to those who have divorced by that time. To account for level differences we subtract and divide by the pre-pregnancy averages for each group. The outcome is an indicator for any of the four main categories of arrest.²⁵ Compared to their past levels of arrest rates, women headed for divorce have slightly higher rates of arrests post-birth, despite broadly similar trends leading up to the pregnancy. These same effects are present and much more pronounced for men.²⁶

These results are consistent with the idea that for married couples, “spousal attachment” is pivotal to maintaining desistance, although the parallel trends leading up to the birth suggest that preparation for a child can be just as impactful for couples who will eventually divorce ([Laub and Sampson, 2001](#)). The results are also broadly consistent with economic conceptions of marital dissolution as in [Becker et al. \(1977\)](#) arguing that divorce occurs in reaction to unexpected changes to the gains from the union. Of course, unobserved variables related to crime and divorce could be driving these results. For example, a spouse could lose their job, resulting in both increased

²⁵Results for crime-type specific arrests show similar patterns.

²⁶The results are very similar using marriages as the focal event.

crime and marital dissolution.

9 Conclusion

How does someone change when they become a parent or wed? This paper establishes several novel patterns in criminal arrests around childbirth and marriage, leveraging a detailed administrative sample and a difference-in-differences strategy comparing parents and spouses to men and women who give birth and marry later. The results provide clear evidence on the degree to which these events serve as “turning points” for criminal behavior. For mothers of all ages, childbirth is transformative, even with the rebound in arrests that occurs after pregnancy. A significant decrease in drug, DUI, and property offenses occurs for fathers as well. However, the increase in domestic violence around both births and marriage is a significant qualifier. Marriage, meanwhile, is reserved for couples who, in the words of [Edin and Kefalas \(2011\)](#), have “made it.”

Parenthood is not a policy, although governments take a wide range of actions in order to prevent teen pregnancy, support marriage through the tax code, and encourage father’s involvement in their children’s lives. Some of these policies may have important spillover effects on parents’ criminal activity. Our findings on the timing of desistance for fathers also suggest that pregnancy could be a uniquely favorable time for interventions promoting additional positive changes. On the other hand, the stark patterns in domestic violence arrests may argue for expanding the purview of home visitation programs in the postnatal period, which are typically directed towards the child’s welfare ([Bilukha et al., 2005](#); [Turnbull and Osborn, 2012](#)). Finally, our findings on teen mothers add some important qualifiers to the conventional wisdom around its consequences.

Our finding that drug arrests show large decreases after family formation implies that substance abuse may respond powerfully to incentives built around social bonds. While some views of addiction frame it primarily as the outcome of involuntary impulses,²⁷ addiction experts observe that some successful treatments, such as Alcoholics Anonymous, are based on promoting social cohesion and interdependence ([Heyman, 2009](#)). Though the experience of childbearing cannot easily be synthesized in an intervention, our results suggest social ties within the family may be a particularly potent source of support for combating addiction.

²⁷For example, the National Institute on Alcohol Abuse and Alcoholism (NIAAA), defines drug abuse as a disease: “Addiction is a chronic, often relapsing brain disease...[s]imilar to other chronic, relapsing diseases, such as diabetes, asthma, or heart disease.”

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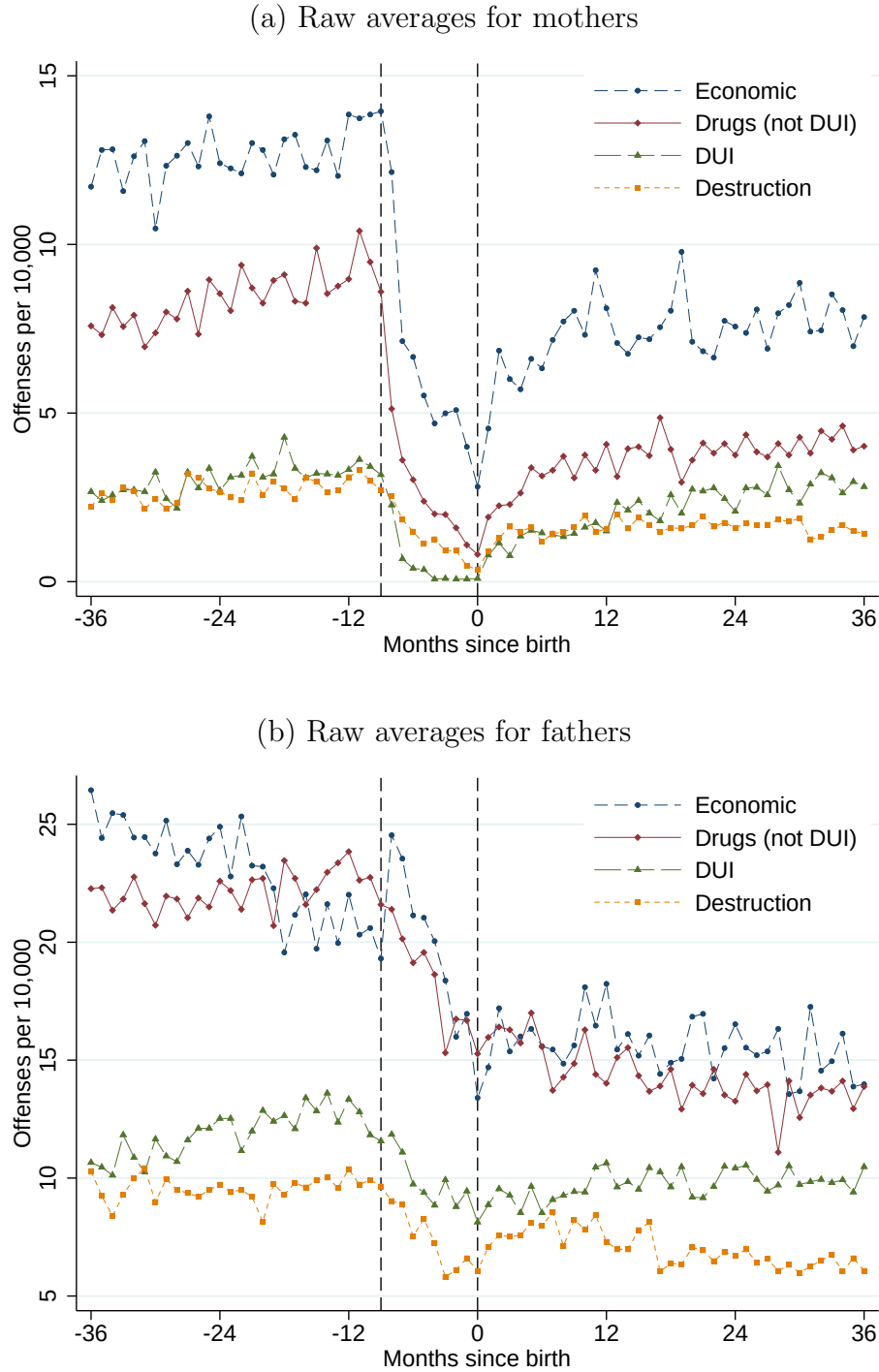
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Figures

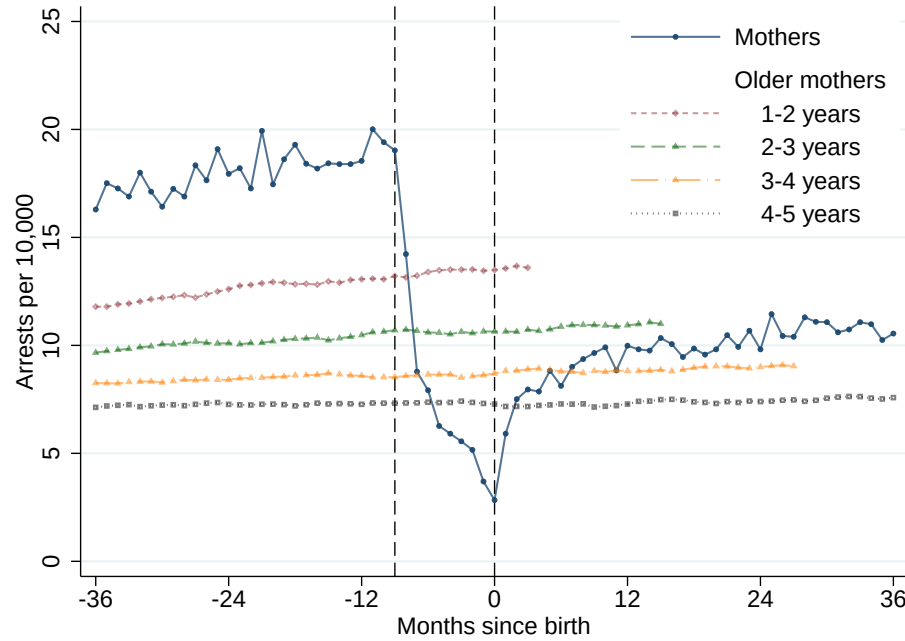
Figure 1: Monthly arrest rates around birth for first-time mothers and fathers



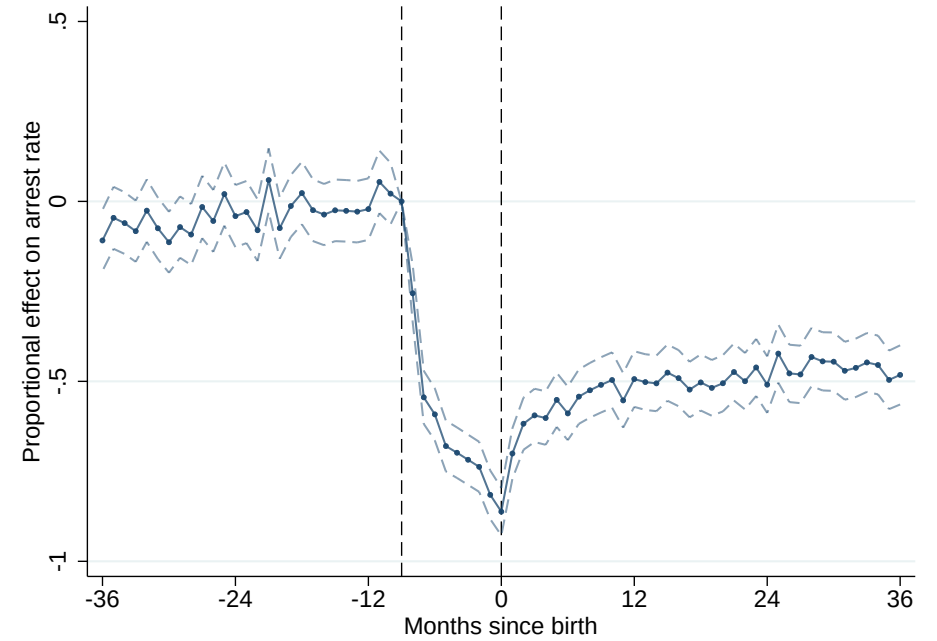
Notes: This figure plots unadjusted arrest rates over the three years before and after first birth for mothers and fathers. The outcomes in both panels are the average of an indicator for any offense from the specified category. Panel (a) includes the primary sample of 532,790 mothers. Panel (b) includes the primary sample of 502,900 first-time fathers. Economic arrests include theft, burglary, trespassing, and forgery. Drug arrests include primarily furnishing liquor to a minor and possession. DUI stands for driving under the influence. Destruction includes vandalism and property damage. In both panels, the vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 2: Effects for first-time mothers

(a) Mothers vs. older mother counterfactuals



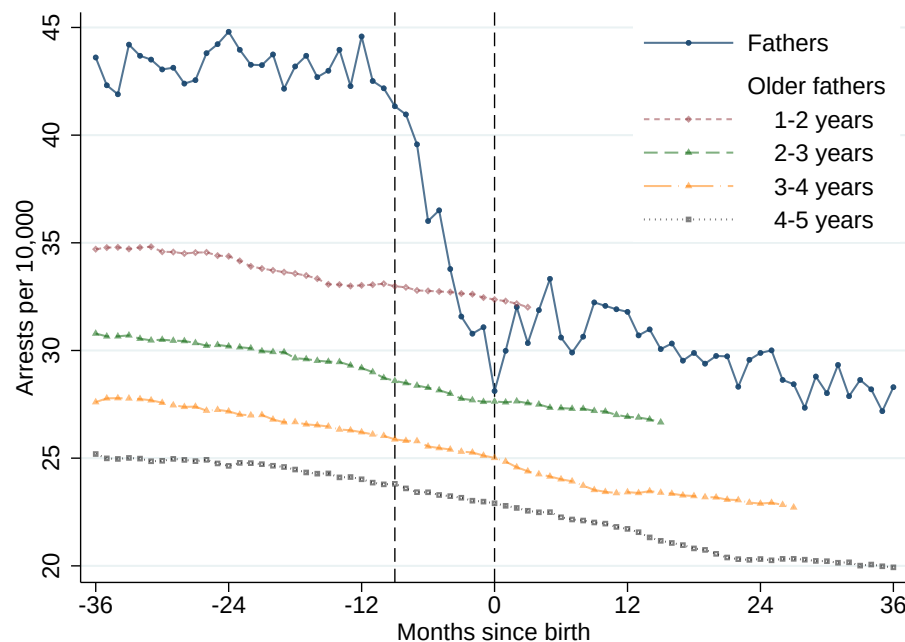
(b) Difference-in-differences estimates



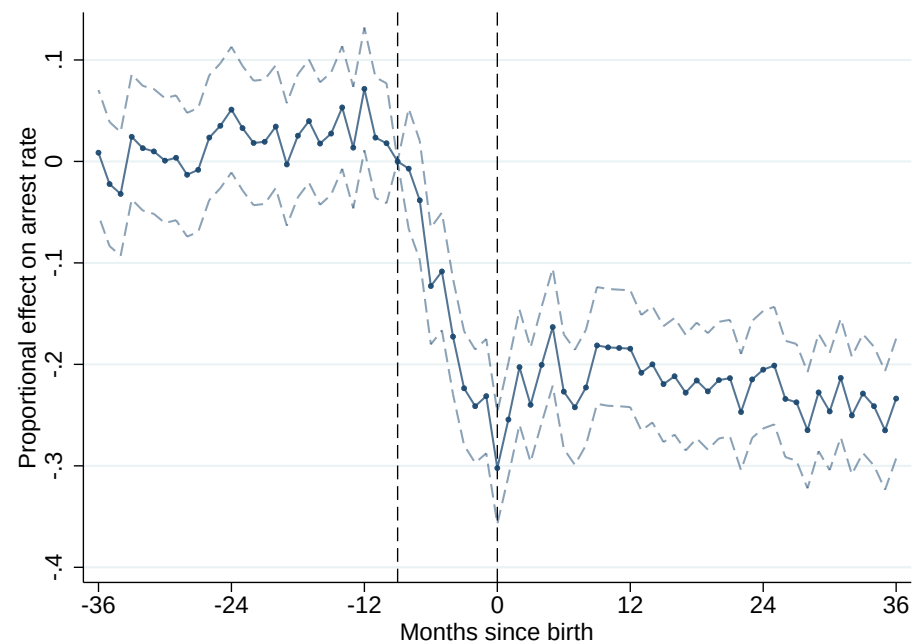
Notes: This figure shows difference-in-difference estimates of the effects of childbirth for mothers. Panel (a) shows average monthly arrest rates around childbirth for mothers and several comparison groups. The outcome is an indicator for any arrest for the crime types plotted in Figure 1. The solid blue line plots mothers' arrest rates in the three years before and after their first child is born. The other lines plot arrest rates over the same ages for women who have their first child 1 to 5 years later. For example, if restricted to the set of women who have their first birth at age 22, the blue line would plot arrest rates from ages 19 to 25. The red dashed line would plot arrest rates over the same ages for women who have their first child between ages 23 and 24. Separate comparisons are constructed for each age-at-birth cohort of mothers in the data and averaged, weighting by cohort size. Outcomes for members of each comparison groups are included until 9 months before the first birth in the group, so each counterfactual line stops 9 months before the youngest mother in the group's birth. Panel (b) plots averages of regression estimates of Specification 1, which measures effects of births relative to these counterfactuals. Regression effects are divided by the average arrest rates of mothers 9 months before birth to show proportional effects. In both panels, the vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 3: Effects for first-time fathers

(a) Fathers vs. older father counterfactuals



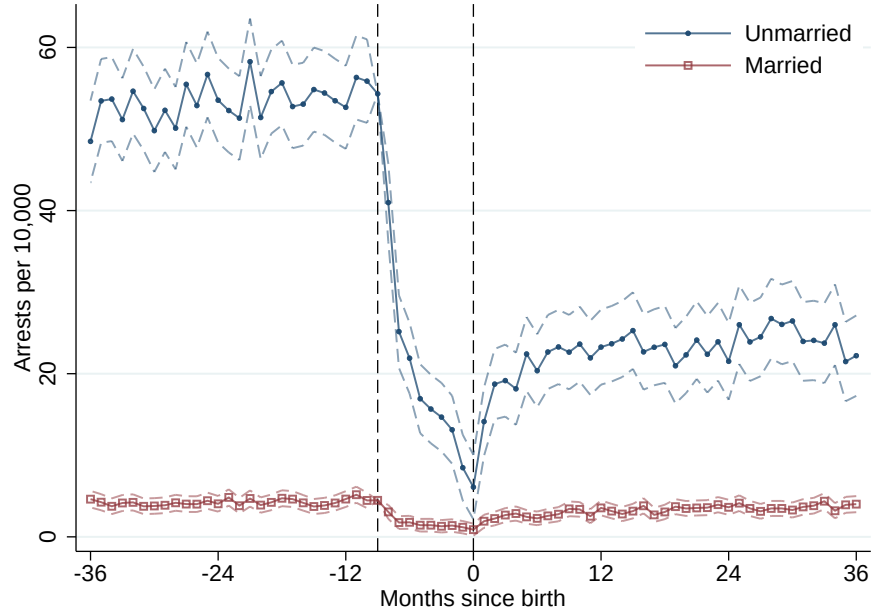
(b) Difference-in-differences estimates



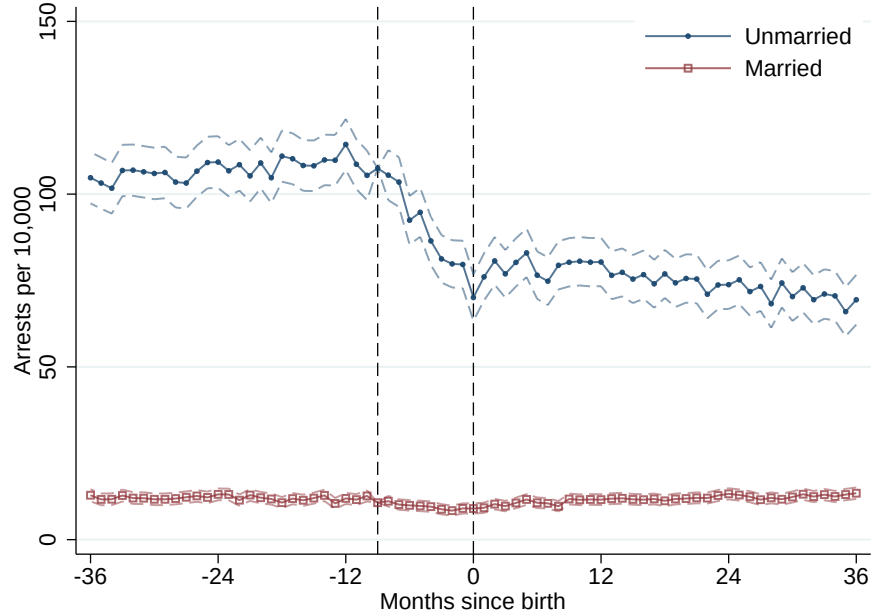
Notes: This figure shows difference-in-difference estimates of the effects of childbirth for fathers. Panel (a) shows average monthly arrest rates around childbirth for fathers and several comparison groups. The outcome is an indicator for any arrest for the crime types plotted in Figure 1. The solid blue line plots fathers' arrest rates in the three years before and after their first child is born. The other lines plot arrest rates over the same ages for men who have their first child 1 to 5 years later. For example, if restricted to the set of men who have their first birth at age 22, the blue line would plot arrest rates from ages 19 to 25. The red dashed line would plot arrest rates over the same ages for men who have their first child between ages 23 and 24. Separate comparisons are constructed for each age-at-birth cohort of fathers in the data and averaged, weighting by cohort size. Outcomes for members of each comparison groups are included until 9 months before the first birth in the group, so each counterfactual line stops 9 months before the youngest father in the group's birth. Panel (b) plots averages of regression estimates of Specification 1, which measures effects of births relative to these counterfactuals. Regression effects are divided by the average arrest rates of fathers 9 months before birth to show proportional effects. In both panels, the vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 4: Heterogeneity by marital status

(a) Difference-in-differences estimates for mothers



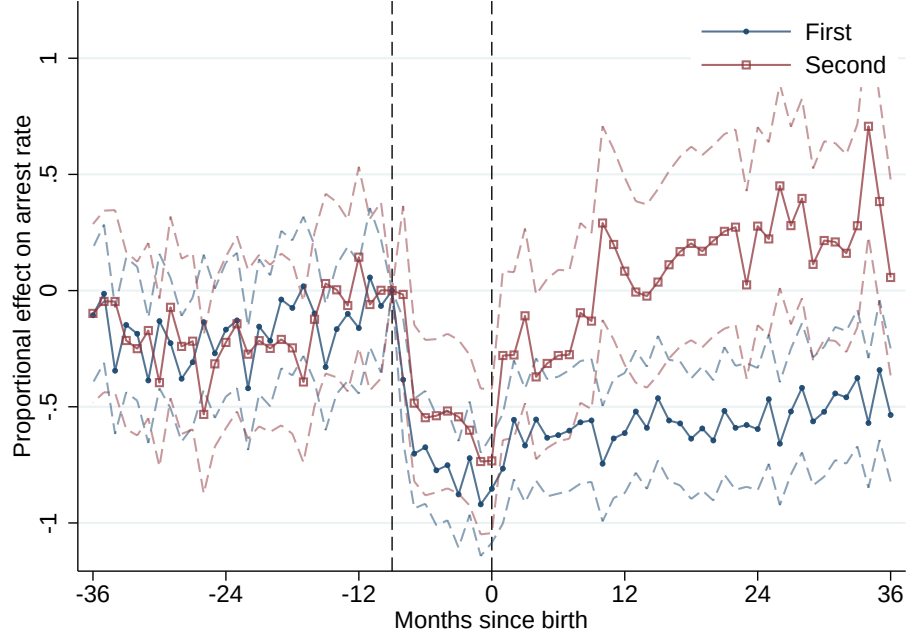
(b) Difference-in-differences estimates for fathers



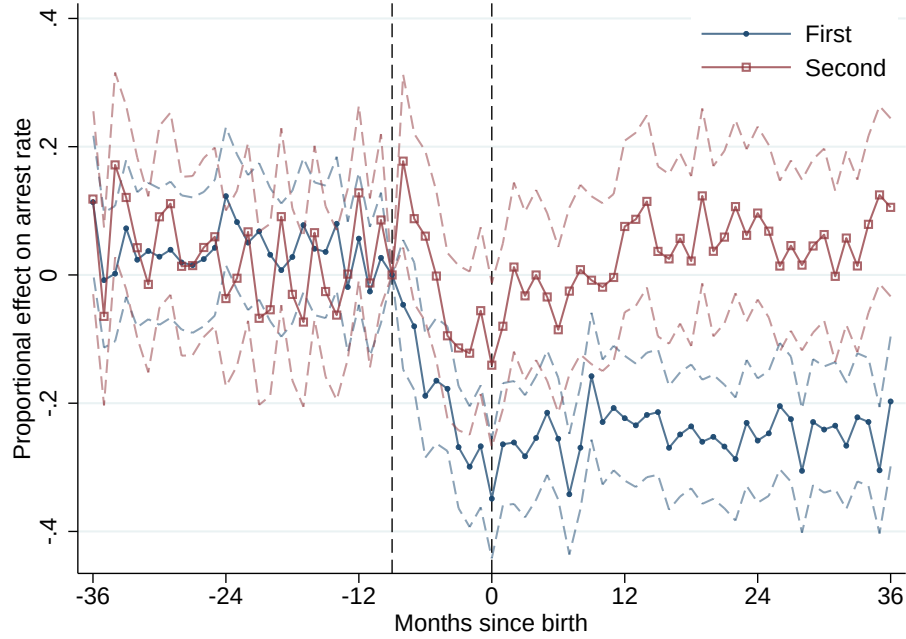
Notes: This figure shows heterogeneity in arrest rates and childbirth effects by parents' marital status. Each panel shows average difference-in-differences estimates from Specification 1, splitting the data by marital status at birth for mothers (a) and fathers (b). Estimates are scaled to reflect effects on arrests per 10,000 people. The outcome is an indicator for any arrest for the crime types plotted in Figure 1. Dots show point estimates and dashed lines show 95% confidence intervals based on standard errors clustered at the person level. The omitted period is nine months before birth and the arrest rate in the omitted period is added to the coefficients to show average arrest rates. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 5: Firsts vs. second births

(a) Difference-in-differences estimates for mothers

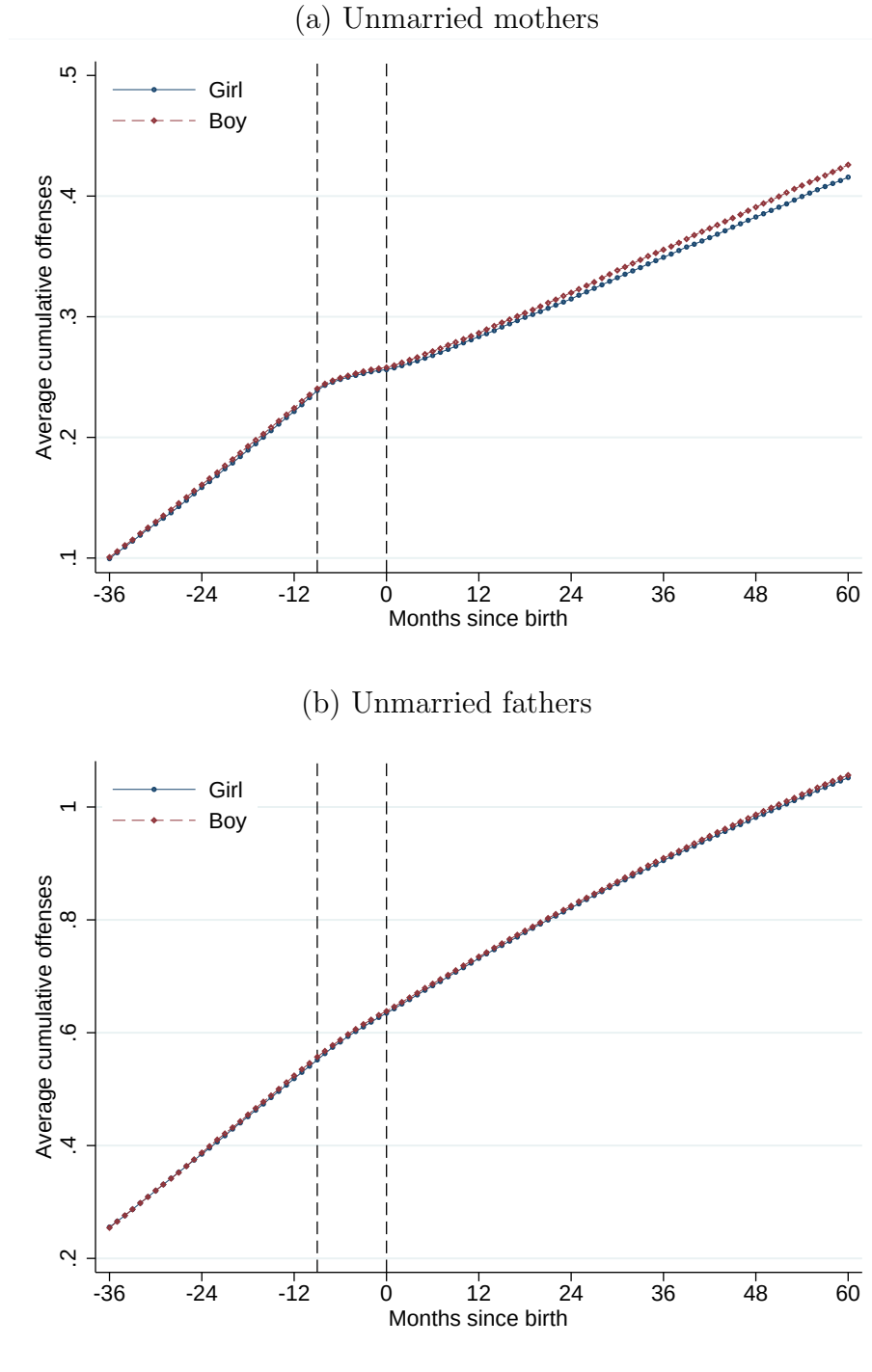


(b) Difference-in-differences estimates for fathers



Notes: This figure compares estimated effects of first vs. second births. Each panel shows difference-in-differences estimates of Equation 1 for mothers (a) and fathers (b). The sample includes all parents in the primary samples with at least two births. Estimated effects for second births come from analogous comparisons to those used to measure effects of first births; parents' outcomes before and after their second birth are compared to outcomes at the same ages for parents who have a second child when 1-5 years older. The outcome is an indicator for any arrest for the crime types plotted in Figure 1. Estimates are divided by mean arrest rates 9 months before birth, which serves as the omitted period, to measure proportional effects. Dots show point estimates and dashed lines show 95% confidence intervals based on standard errors clustered at the person level. The vertical dashed lines mark 9 months before the birth and the month of birth.

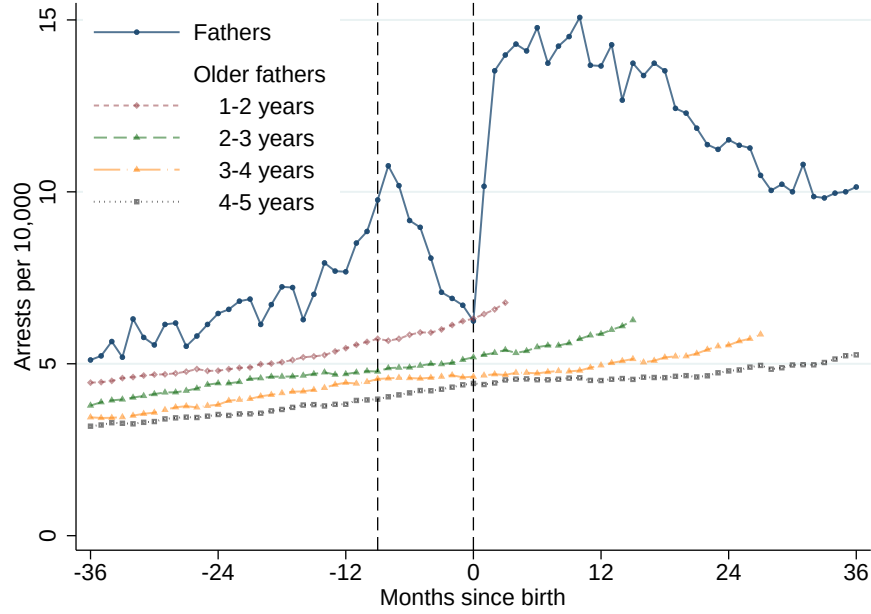
Figure 6: Effects of infant sex among unmarried first-time parents



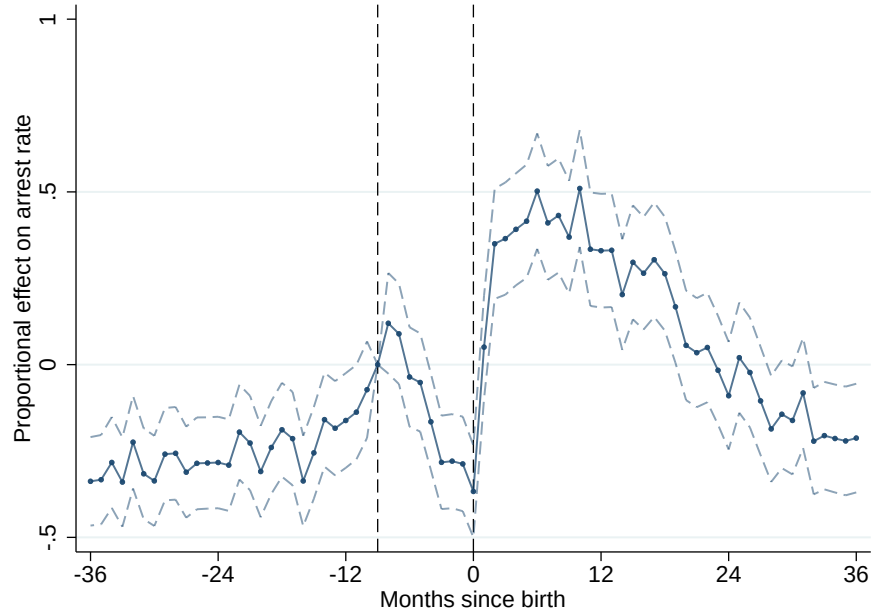
Notes: This figure tests for differences in the effects of childbirth by child sex. The samples includes the 152,499 unmarried first-time fathers and 153,854 unmarried first-time mothers in the primary samples. Both plots show the monthly averages of a cumulative count of an offense indicator, equal to 1 if the mother or father committed a drug, DUI, economic, or property destruction offense in a given month, beginning five years before birth.

Figure 7: Fathers' domestic violence

(a) Fathers vs. older father counterfactuals



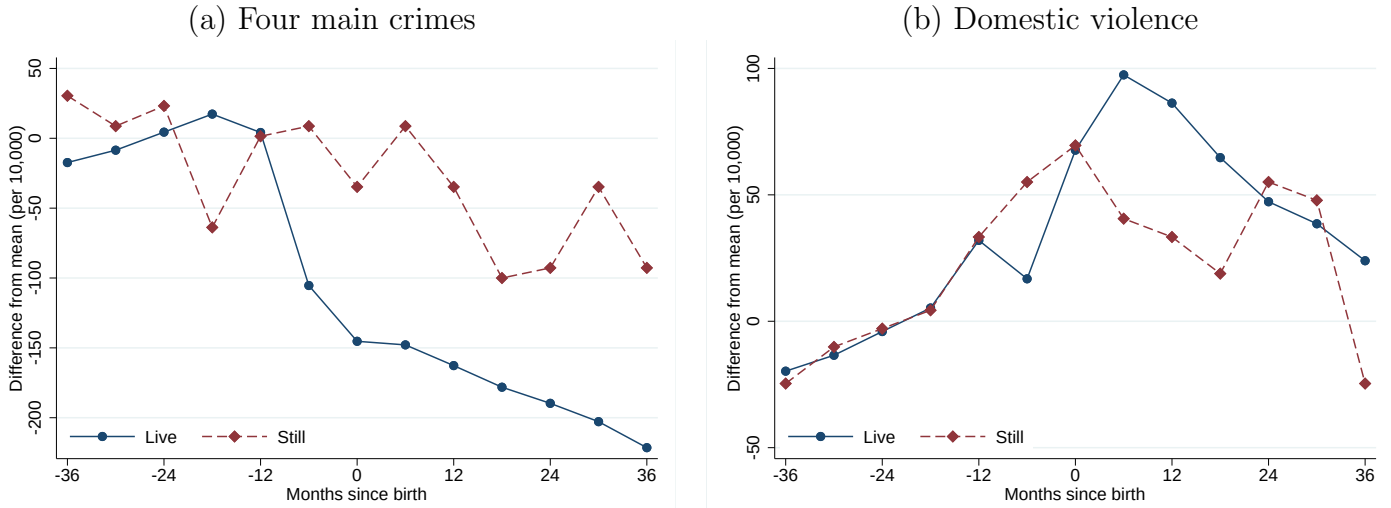
(b) Difference-in-differences estimates



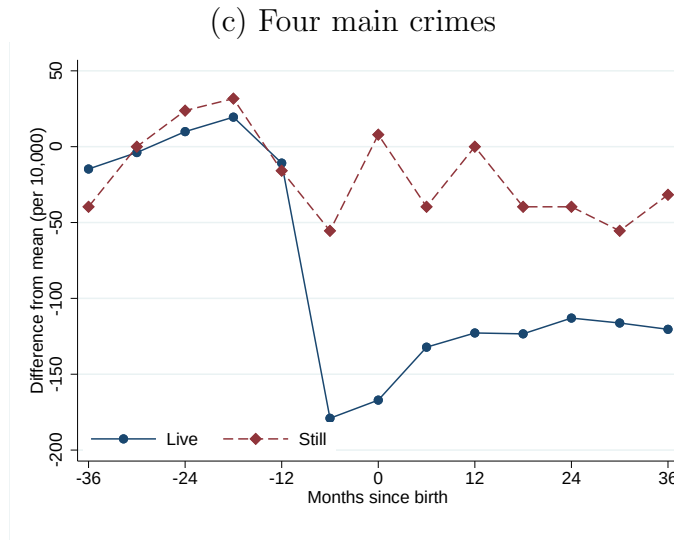
Notes: This figure shows effects of childbirth on father's domestic violence arrests. Panel (a) shows average monthly arrest rates around childbirth for fathers and several comparison groups. The outcome is an indicator for any domestic violence arrest. Fathers' and older fathers' outcomes are constructed as in [Figure 3](#). Panel (b) plots average of regression estimates of [Equation 1](#), which measures effects of births relative to these counterfactuals. Regression effects are divided by the average domestic violence arrest rates of fathers 9 months before birth to show proportional effects. In both panels, the vertical dashed lines mark 9 months before the birth and the month of birth.

Figure 8: Stillbirths vs. livebirths

Unmarried fathers

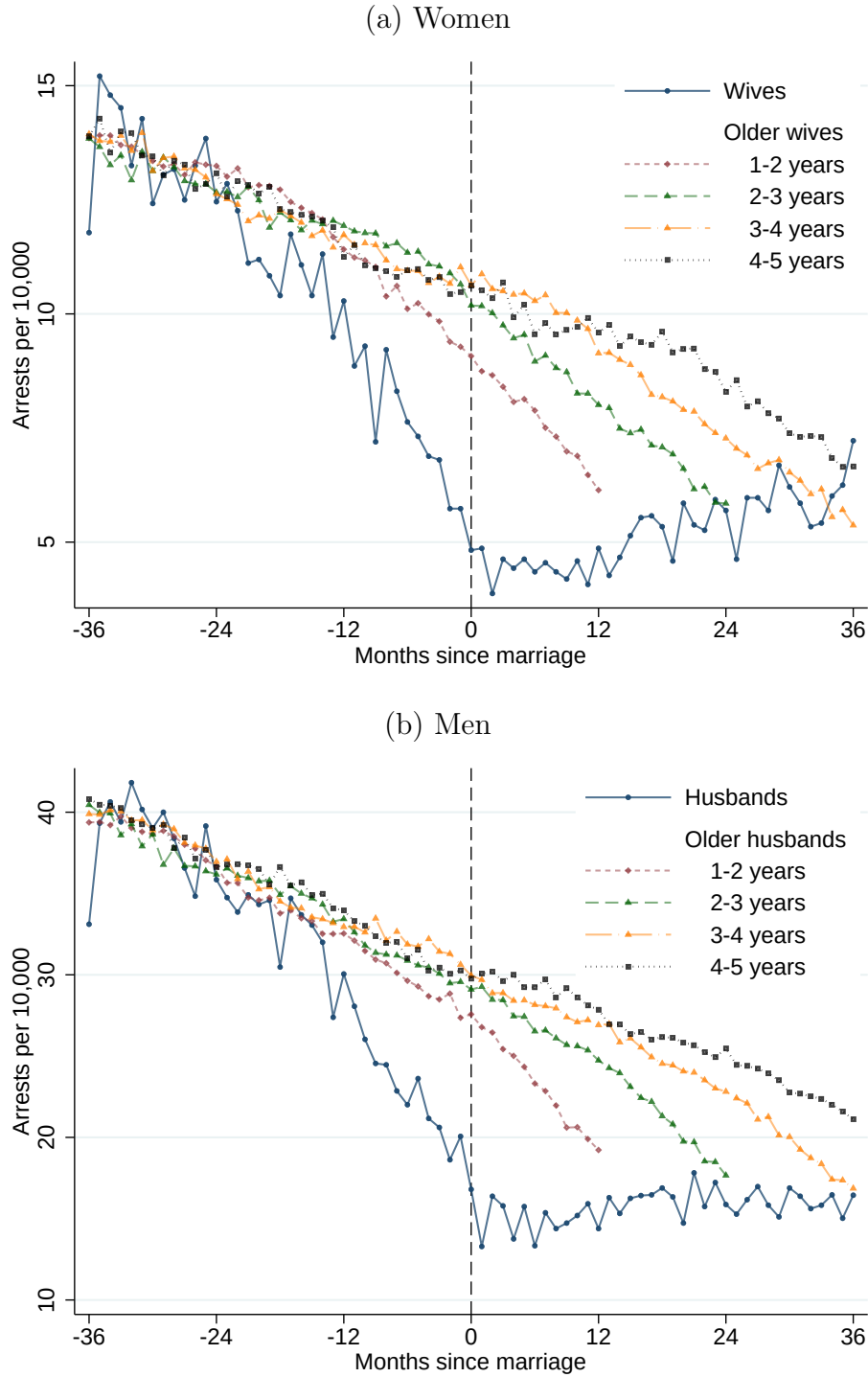


Unmarried mothers



Notes: This figure shows arrest patterns around birth for parents of live- and still-born children. Across panels, the data plotted is the average of an indicator for being charged with an offense in a six-month period, minus the average of the indicator for that group (either live or stillbirth) in the two years before birth. For example, the point at month 24 in Panel (c) indicates that, for every 10,000 single mothers to live-born infants, there were 40 fewer offenses compared to the pre-pregnancy average. We use the six-month period to compensate for the relatively small number of stillbirths and resulting noisy monthly arrest rate measures.

Figure 9: Arrest patterns around marriage



Notes: This figure plots arrest rates around marriage for spouses and several comparisons groups of older spouses. The sample includes all fathers ($N=243,570$) and mothers ($N=254,708$) from the birth data who are visible in the arrest data 3 years after and 3 years before their marriage. Each graph is constructed in the same way as Figures 2 and 3, except using age at marriage instead of age at first birth to define cohorts and potential comparison spouses. Comparison husbands and wives are included up until their month of marriage, and comparison group lines stop when the youngest spouse in the group marries. The outcome is an indicator for any arrest for the crime types plotted in Figure 1. The vertical dashed line marks the month of marriage.

Tables

Table 1: Descriptive statistics for analysis samples

	First birth		Still birth	
	Mothers (1)	Fathers (2)	Mothers (3)	Fathers (4)
Demographics				
White	0.69	0.65		
Black	0.04	0.05		
Hispanic	0.13	0.13		
Asian	0.10	0.08		
Other or missing	0.04	0.09		
Age	26.73 (5.75)	28.23 (5.52)	27.13 (6.11)	28.01 (5.79)
Birth				
Low birth weight (<2500g)	0.06	0.06	0.59	0.60
Twins+	0.02	0.02	0.05	0.06
Male infant	0.51	0.51	0.53	0.53
Marital				
Mother married at birth	0.71	0.70	0.65	0.60
Midpregnancy marriage	0.05	0.05	0.05	0.05
Divorce if married	0.18	0.18	0.22	0.22
Economic				
Median zipcode income	59,944 (18,110)	59,577 (17,924)	58,331 (17,906)	57,864 (17,544)
Mother on Medicaid	0.35	0.36		
WIC	0.34	0.35	0.25	0.27
Crime				
Any arrest	0.19	0.34	0.10	0.27
Father ever incarcerated	0.04	0.03	0.04	0.04
Father ever on probation	0.07	0.06	0.08	0.06
Observations	532,790	502,900	3,579	3,417

Notes: This table presents key descriptive statistics for the primary analysis samples of first live- and still-births. Each column shows averages of parent and birth characteristics, with standard deviations for non-binary variables shown in parentheses. Column (1) presents descriptives for the mother’s first birth sample, while column (2) reports statistics for the father’s first birth sample. Columns (3) and (4) report descriptives for the stillbirth sample for mothers and fathers, respectively. Median zipcode income is for the years 2006-2010 from the American Community Survey (ISR, 2019). WIC is an indicator for being on the Special Supplemental Nutrition Program for Women, Infants, and Children at the time of birth.

Table 2: Married vs. unmarried parents

	Married		Unmarried	
	Mothers (1)	Fathers (2)	Mothers (3)	Fathers (4)
Demographics				
White	0.72	0.72	0.63	0.48
Black	0.03	0.04	0.06	0.07
Hispanic	0.10	0.10	0.19	0.19
Asian	0.12	0.10	0.06	0.05
Other or missing	0.03	0.04	0.06	0.21
Age	28.27 (5.17)	29.60 (4.97)	22.95 (5.33)	25.08 (5.45)
Birth				
Low birth weight (<2500g)	0.05	0.05	0.06	0.06
Twins+	0.02	0.02	0.01	0.01
Male infant	0.51	0.51	0.51	0.51
Marital				
Mother married at birth	1.00	1.00	0.00	0.00
Midpregnancy marriage	0.06	0.07	0.00	0.00
Divorce if married	0.18	0.18		
Economic				
Median zipcode income	62,028 (18,819)	61,714 (18,676)	54,8023 (15,043)	54,657 (14,941)
Mother on Medicaid	0.22	0.22	0.65	0.66
WIC	0.23	0.24	0.61	0.62
Crime				
Any arrest	0.11	0.25	0.39	0.56
Father ever incarcerated	0.01	0.01	0.10	0.07
Father ever on probation	0.03	0.03	0.17	0.14
Observations	378,936	350,401	153,854	152,499

Notes: This table reports average parent and birth characteristics for married or unmarried first-time parents. Each column reports means, with standard deviations for non-binary variables shown in parentheses. Column (1) shows descriptives for the married mothers' first birth, while column (2) reports statistics for married fathers' first birth. Columns (3) and (4) show descriptives for unmarried mothers and fathers, respectively. Median zipcode income is for the years 2006-2010 from the American Community Survey ([ISR, 2019](#)). WIC is an indicator for being on the Special Supplemental Nutrition Program for Women, Infants, and Children at the time of birth.

Table 3: Stillbirth results, fathers

(a) Four main crime categories				
	(1) # Charges	(2) # Charges, Unmarried	(3) LPM	(4) LPM, Unmarried
After birth	-0.00067 (0.00341)	-0.00219 (0.00796)	-0.00008 (0.00138)	0.00098 (0.00314)
Live X After birth	-0.00523 (0.00337)	-0.01485* (0.00780)	-0.00306** (0.00136)	-0.00981*** (0.00308)
Pre-birth outcome mean	0.04027	0.10466	0.02242	0.05630
R-squared	0.180	0.175	0.204	0.201
N	6,582,121	2,000,427	6,582,121	2,000,427

(b) Domestic violence				
	(1) # Charges	(2) # Charges, Unmarried	(3) LPM	(4) LPM, Unmarried
After birth	0.00455*** (0.00148)	0.01415*** (0.00338)	0.00266*** (0.00075)	0.00794*** (0.00164)
Live X After birth	0.00264* (0.00144)	0.00508 (0.00323)	0.00152** (0.00074)	0.00316** (0.00159)
Pre-birth outcome mean	0.00515	0.01235	0.00345	0.00810
R-squared	0.135	0.135	0.148	0.150
N	6,582,121	2,000,427	6,582,121	2,000,427

Notes: These tables report estimates from the difference-in-differences specification in [Equation 2](#). Panel (a) uses criminal charges for drug, DUI, economic, or property destruction offenses in each six-month period as the outcome, while Panel (b) uses domestic violence offenses. Across panels, columns (1) and (3) report results for all first-time fathers in the sample (Number of men: 502,900 with normal births and 3,417 with stillbirths) and columns (2) and (4) report results restricting to unmarried fathers (N = 152,499 with normal births and 1,380 with stillbirths). The outcome in columns (1) and (2) is a count of charges. In columns (3) and (4), the outcome is a binary indicator for having any of those charges in the six-month period. Standard errors clustered at the person level are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

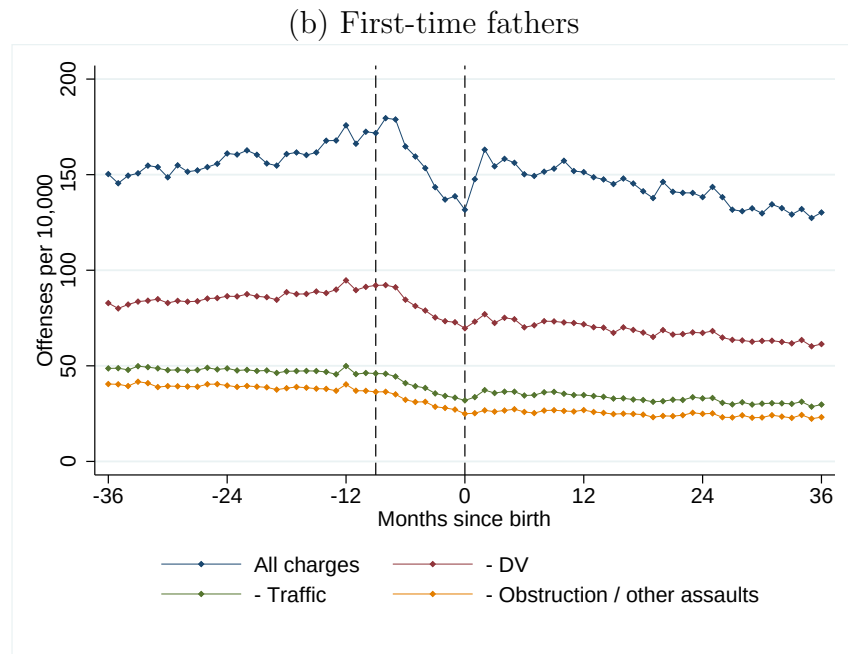
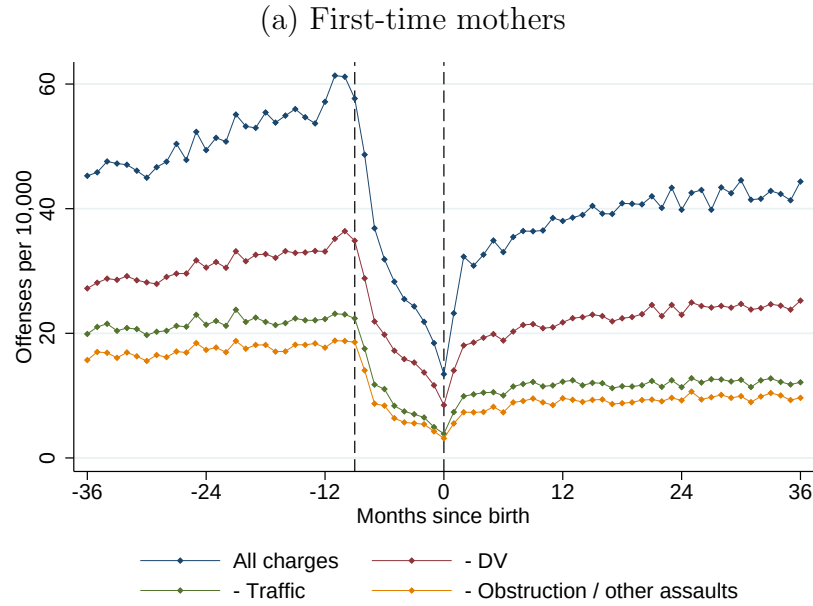
Table 4: Stillbirth results, mothers

(A) Four main crime categories				
	(1) # Charges	(2) # Charges, Unmarried	(3) LPM	(4) LPM, Unmarried
After birth	-0.00342 (0.00239)	-0.01681*** (0.00330)	-0.00299*** (0.00064)	-0.01018*** (0.00153)
Live X After birth	-0.00659*** (0.00242)	-0.01368*** (0.00315)	-0.00267*** (0.00063)	-0.00723*** (0.00148)
Pre-birth outcome mean	0.01583	0.04531	0.00961	0.02707
R-squared	0.133	0.132	0.156	0.155
N	6,972,797	2,016,508	6,972,797	2,016,508

Notes: This table reports estimates from the difference-in-differences specification reported in [Equation 2](#) using criminal charges for drug, DUI, economic, or property destruction offenses in each six-month period as the outcome. Columns (1) and (3) report results for all first-time mothers in the sample (Number of women: 532,790 with normal births and 3,579 with stillbirths). Columns (2) and (4) report results restricting to unmarried mothers (N: 153,854 with normal births and 1,262 with stillbirths). The outcome in columns (1) and (2) is a count of charges. In columns (3) and (4), the outcome is a binary indicator for having any of those charges in the six-month period. Standard errors clustered at the person level are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

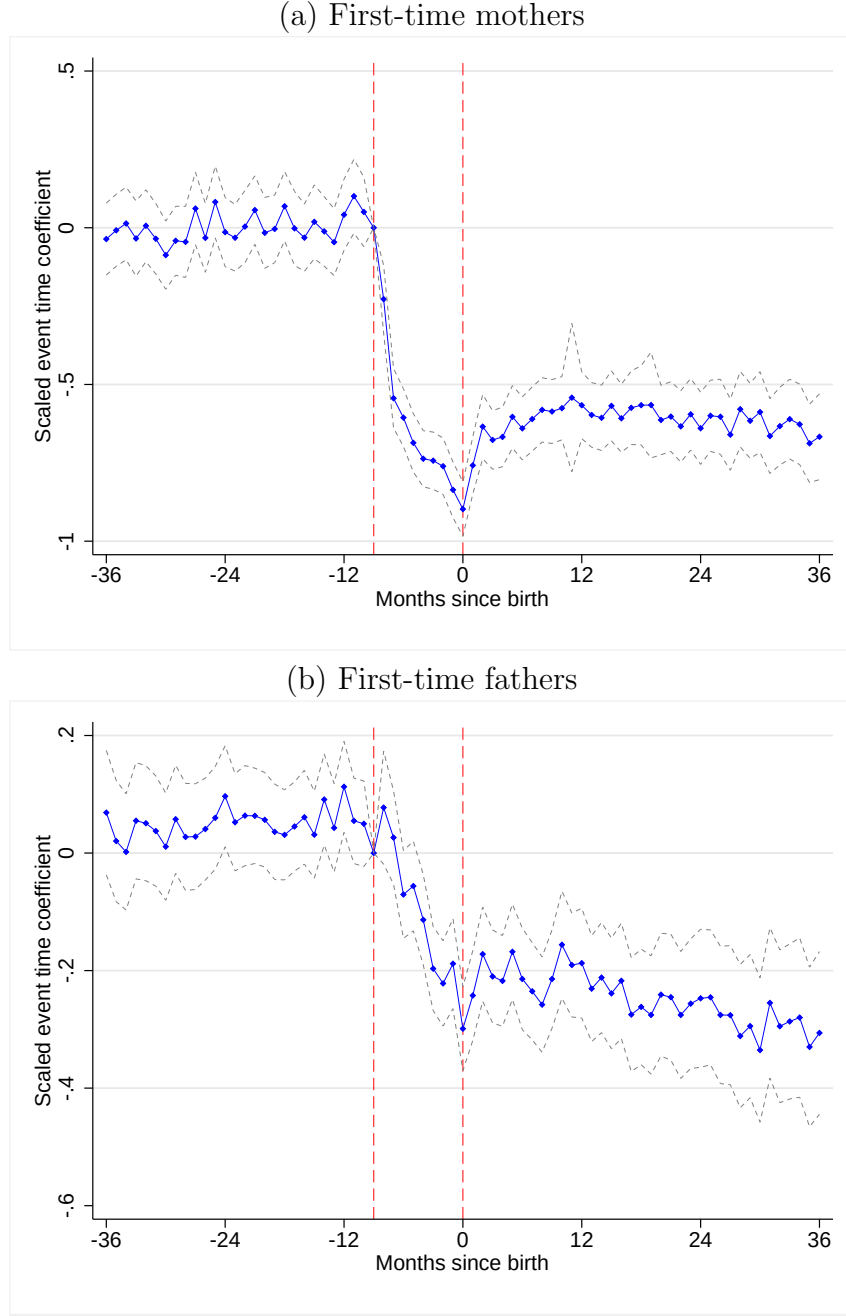
Appendix A Additional figures and tables

Figure A.1: Crime categories



Notes: This figure shows how trends in the raw averages of crime outcomes around childbirth vary as we sequentially implement our category restrictions. The top blue line shows all charges. Next, in the red line, we remove all domestic violence charges, a category which we study separately. The green line removes all non-DUI driving offenses, and the yellow line removes charges of obstruction and non-DV assaults. This last line is the main crime outcome we use in our analysis, consisting of economic crimes, drug crimes, DUIs, and destruction of property.

Figure A.2: Traditional event study estimates

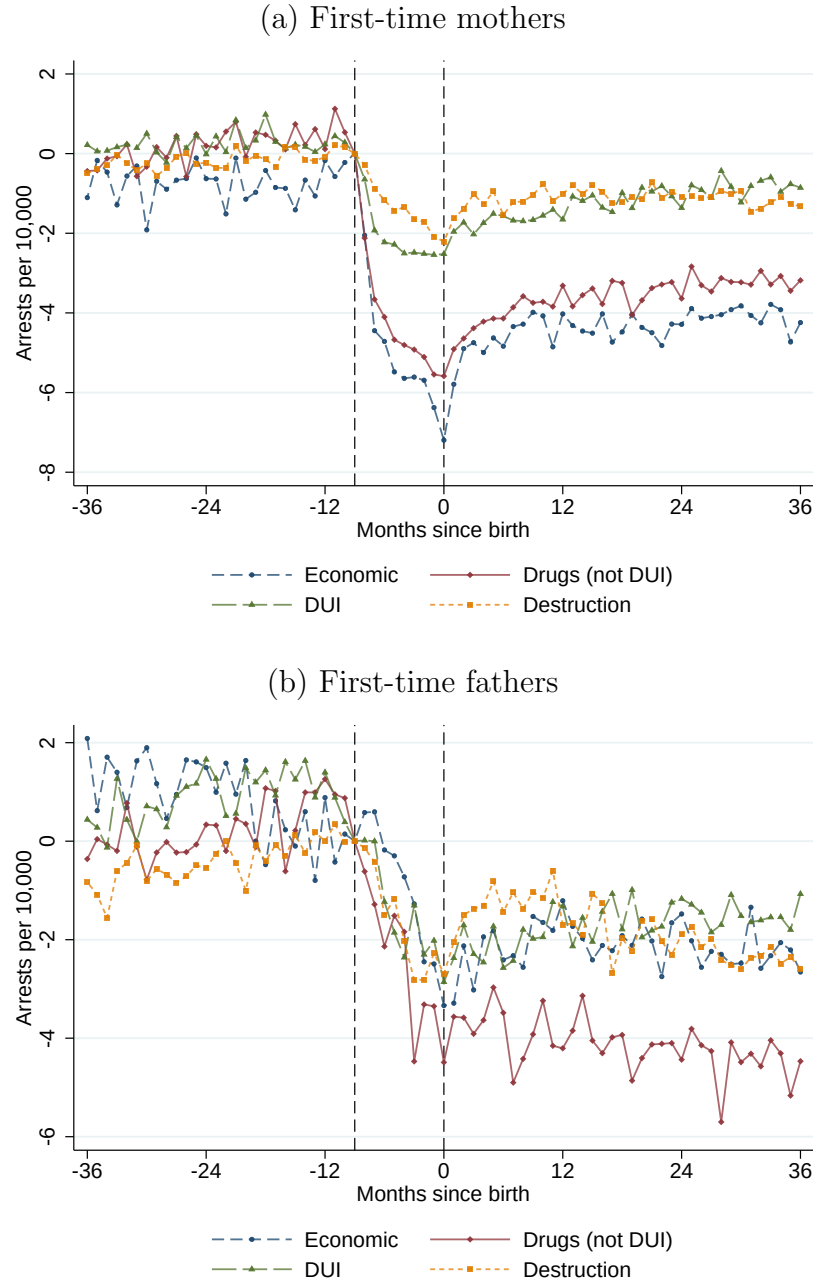


Notes: This figure plots estimated effects of childbirth from a traditional event study specification. In both panels, the dots show point estimates and the dotted lines give confidence intervals of event time coefficients from:

$$\mathbb{1}(arrest)_{it} = \alpha_i + \sum_{k \in S} \delta_k \mathbb{1}(t = k) + \mathbf{X}'_{it} \beta + \epsilon_{it}$$

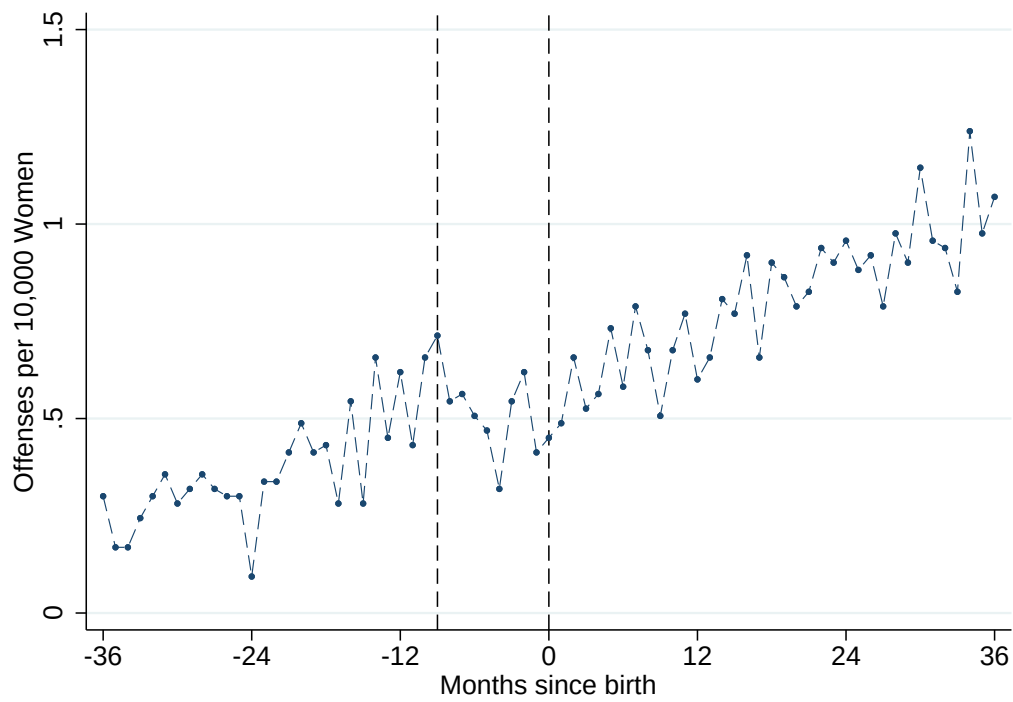
where $\mathbb{1}(arrest)_{it}$ is equal to one if person i committed one of the four main offenses in month t , α_i denotes person fixed effects and δ_k are the event time coefficients measuring effects k months relative to birth. The set S runs from 36 months before to 36 months after birth, omitting $k = -9$. We bin the endpoints before and after 37 months from birth and include these as separate controls. The controls in $\mathbf{X}_{it}$ include a 4th-order polynomial in age (measured in months) and dummies for being above 18 and 21 years of age. Panel (a) includes the first-time mothers sample and panel (b) includes the first-time fathers sample. The outcome is any offense from one of the four main crime categories. The estimates are divided by the average in the omitted period.

Figure A.3: Crime-type specific effects



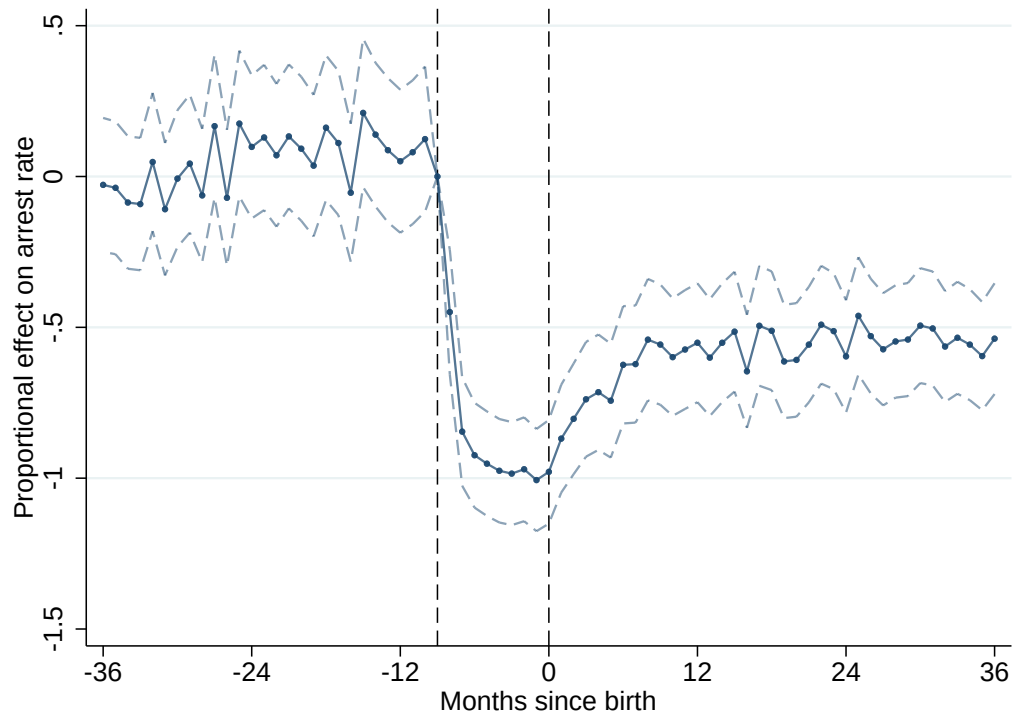
Notes: This figure plots averages of regression estimates of Specification 1, which measures effects of births relative to these older parents, for first-time and mothers and fathers. The outcome for each series is an indicator for any arrest for the crime type specified in the legend. Effects are scaled by 10,000. In both panels, the vertical dashed lines mark 9 months before the birth and the month of birth.

Figure A.4: Driving without a license, mothers



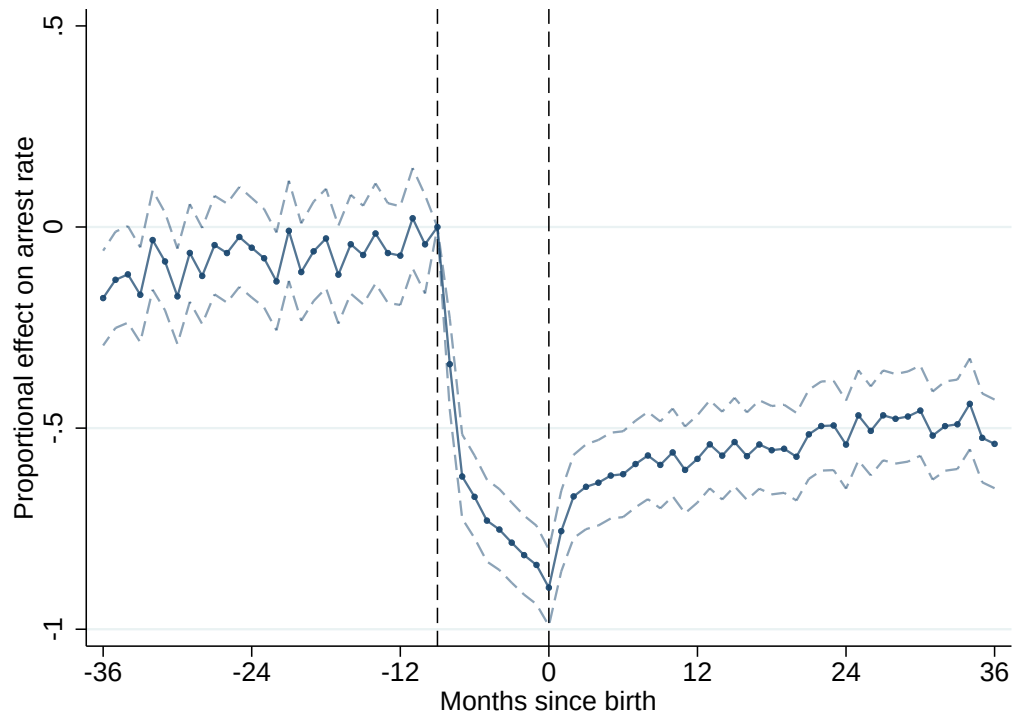
Notes: This figure shows the raw averages of an indicator for arrests for driving without a license, the most common non-DUI driving offense, around childbirth. Includes fully-balanced arrest data for 532,790 first-time mothers. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure A.5: Difference-in-differences estimates for alcohol offenses, mothers under 21 years old



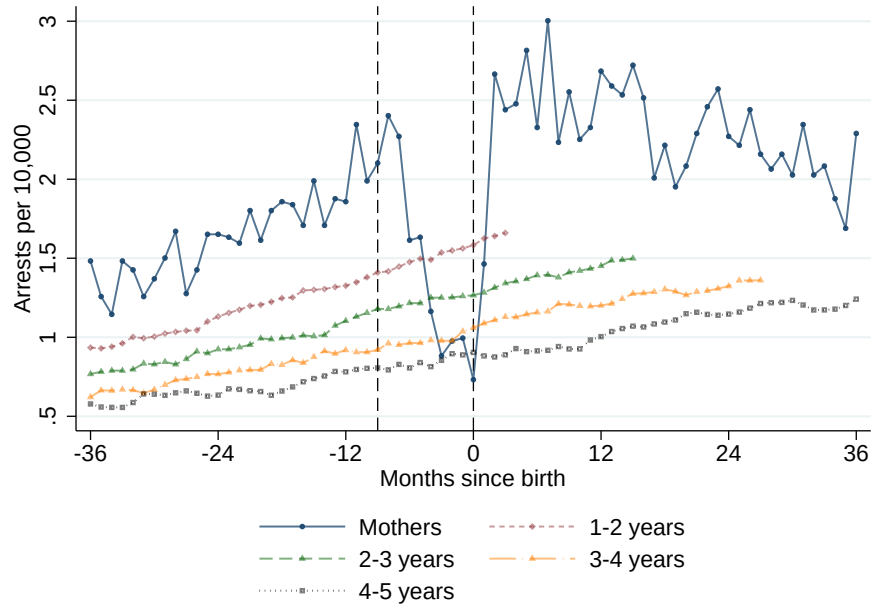
Notes: This figure plots regression estimates of effects of childbirth from Specification 1 with 95% confidence intervals derived from standard errors clustered at the mother level shown in dashes. The sample is restricted to mothers who gave birth before turning 21. The outcome is an indicator for any alcohol related arrest. Estimates are divided by average arrest rates 9 months before birth to show proportional effects. The dashed lines marks 9 months before the birth and the month of the birth.

Figure A.6: Difference-in-differences estimates for teen mothers



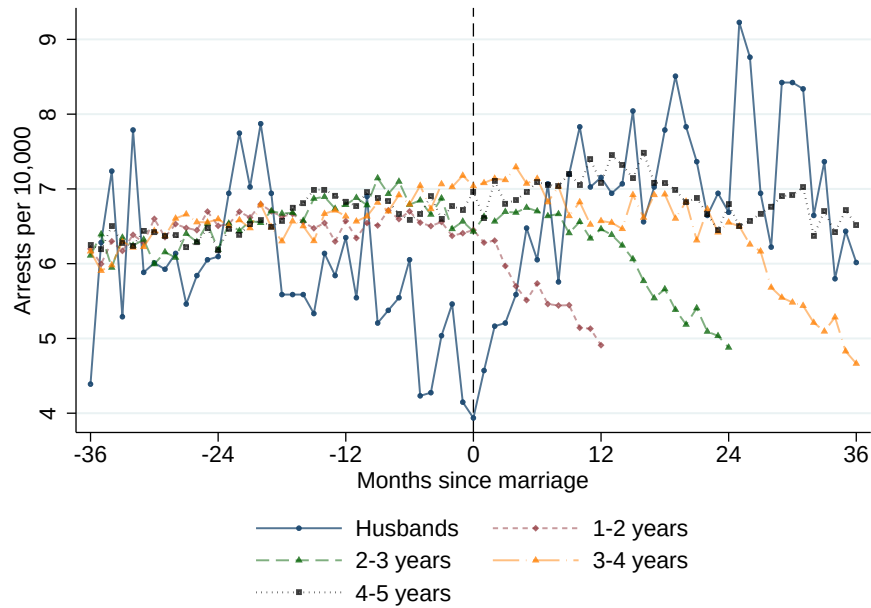
Notes: This figure plots regression estimates of effects of childbirth from Specification 1 with 95% confidence intervals derived from standard errors clustered at the mother level shown in dashes. The sample is restricted to first-time mothers who gave birth at age 19 or younger. The outcome is an indicator for any economic, drug, DUI, or property destruction offense within the month. Estimates are divided by average arrest rates 9 months before birth to show proportional effects. The dashed lines marks 9 months before the birth and the month of the birth.

Figure A.7: Mothers domestic violence



Notes: This figure plots average monthly arrest rates around childbirth for mothers and several comparison groups. The outcome is an indicator for any domestic violence arrest. Mothers' and older Mothers' outcomes are constructed as in Figure 2. The vertical dashed lines mark 9 months before the birth and the month of birth.

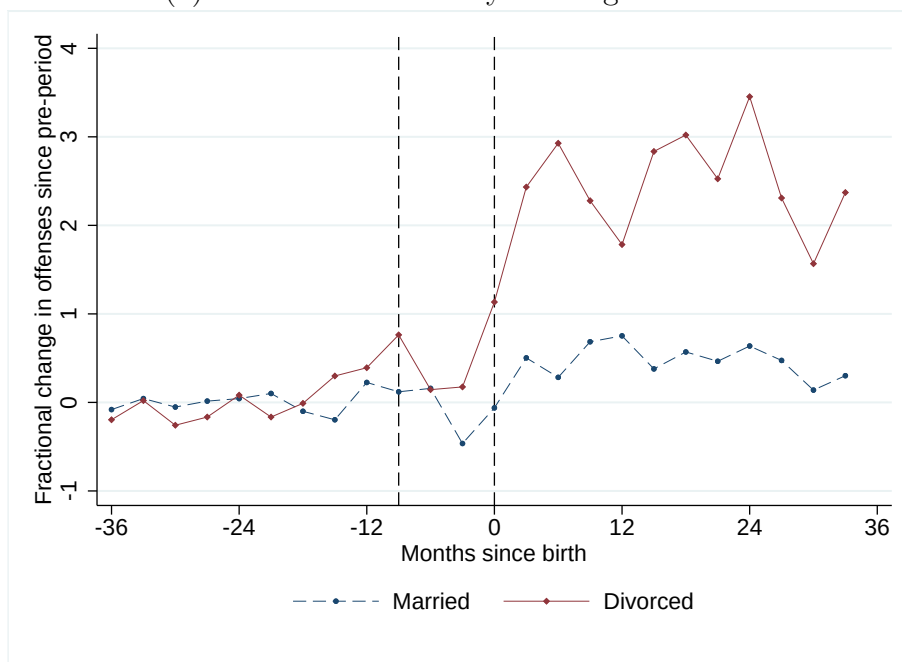
Figure A.8: Fathers' domestic violence around marriage



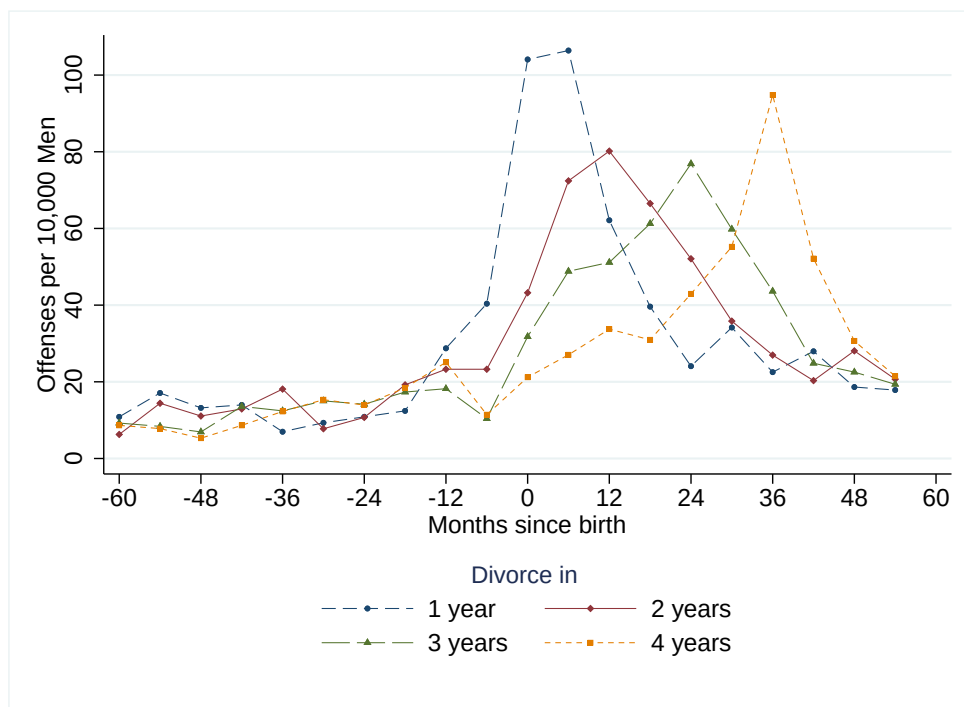
Notes: This figure shows average arrest rates around marriage for husbands and older husbands. It is also constructed using the same approach as in Figure 3, so that husband's arrest rates are compared to older husbands' over the same ages.

Figure A.9: Domestic violence vs. divorce

(a) Domestic violence by marriage outcome

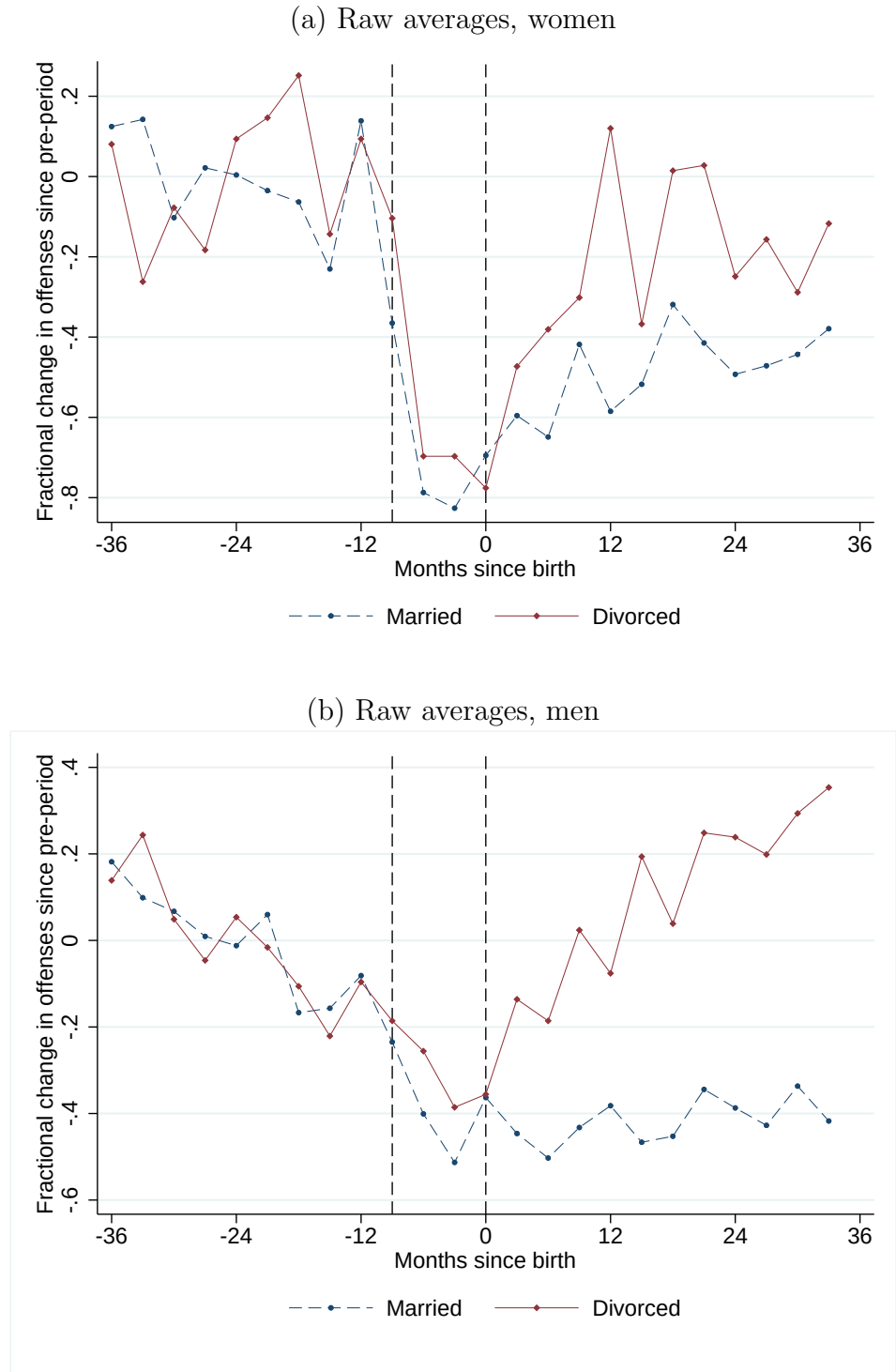


(b) Domestic violence by divorce timing



Notes: This figure examines how domestic violence arrests relate to divorce. Panel (a) includes 126,777 still-married men and 10,145 divorced men.. Panel (b) includes all men who were married for their first birth and then divorced 1-4 years after. Grouping is based on the rounded time in years between the child's birth date and date of the divorce decree (when the divorce is finalized). Sample sizes for the four groups are 2,146 (1 year), 4,511 (2 years), 5,768 (3 years), and 5,976 (4 years).

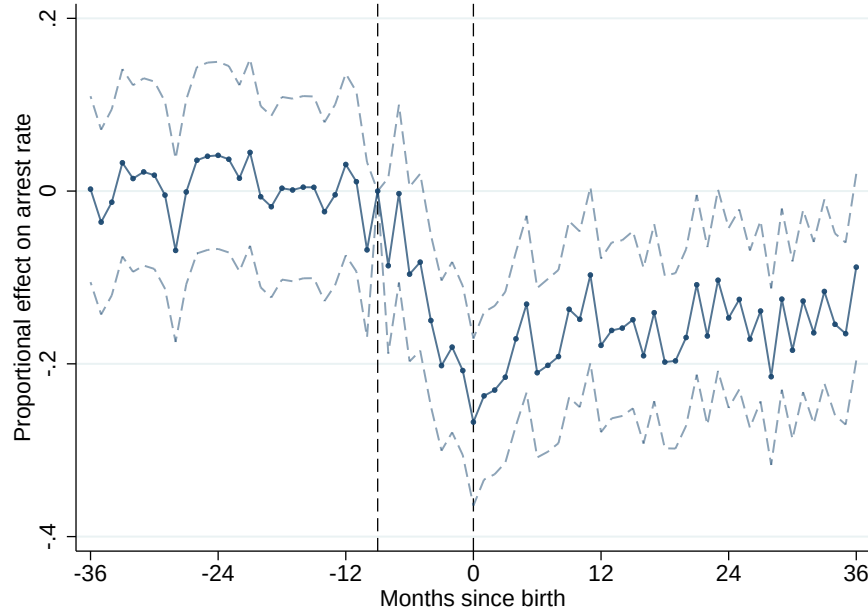
Figure A.10: Heterogeneity in the effect of childbirth between good marriages and bad marriages



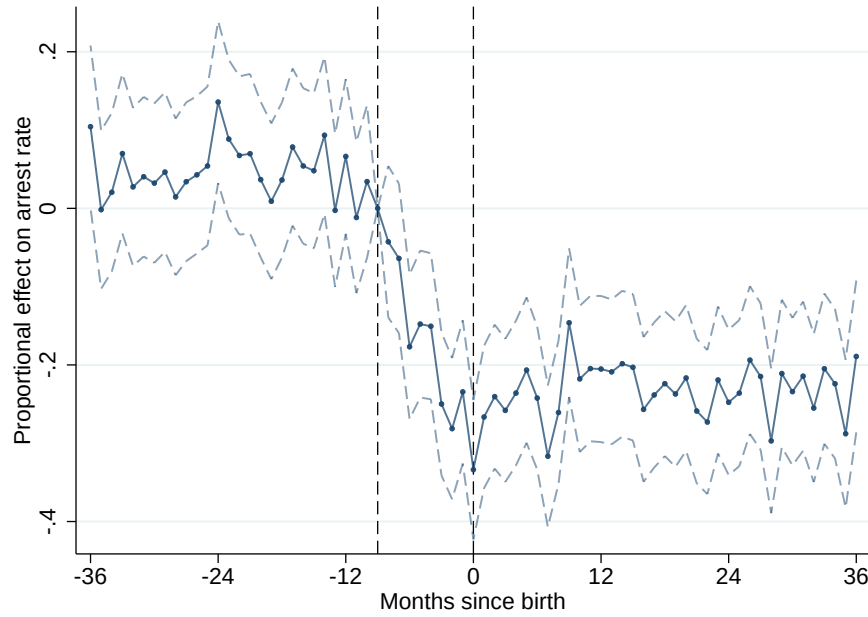
Notes: This figure shows differences in arrests around marriage for couples that divorce within five years of childbirth compared to those who remain married. Panel (a) includes data on 135,774 still-married women and 10,319 divorced women. Panel (b) includes arrest data on 126,777 still-married men and 10,145 divorced men. The outcome is any drug, DUI, economic, or property destruction arrest, divided by the pre-pregnancy average. Divorce classification is derived from a fuzzy match between the Washington state marriage and divorce indexes. The vertical dashed lines mark 9 months before the birth and the month of birth.

Figure A.11: Robustness to migration out of Washington State

(a) Difference-in-differences estimates for men with future crime

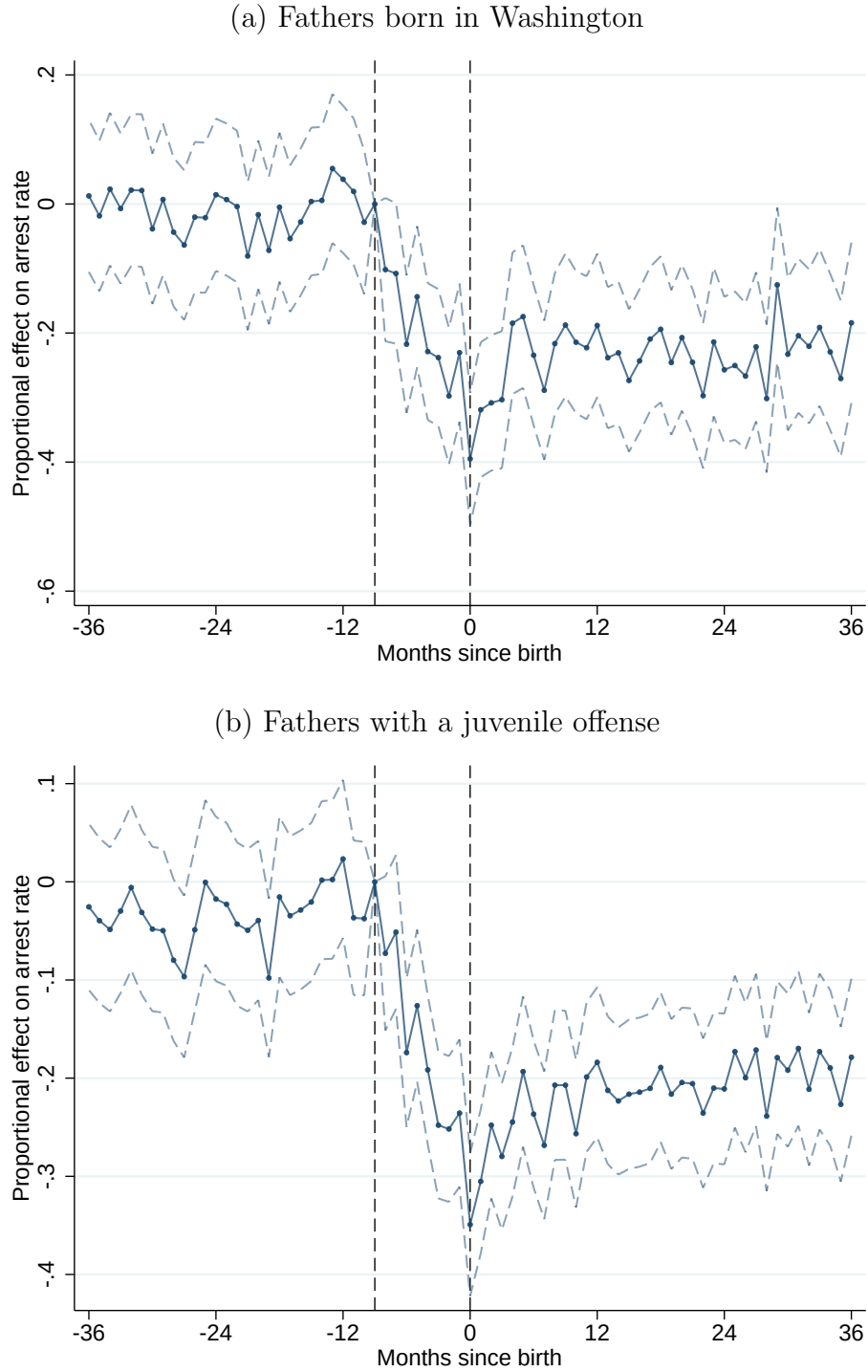


(b) Difference-in-differences estimates for men with future children



Notes: This figure plots regression estimates of effects of childbirth from Specification 1 with 95% confidence intervals derived from standard errors clustered at the father level shown in dashes. Panel (a) restricts to men charged with a driving-related (including DUI) offense 4-5 years after the birth. Panel (b) restricts to fathers who at some point have a second child in Washington State. The outcome is an indicator for any economic, drug, DUI, or property destruction offense within the month. Estimates are divided by average arrest rates 9 months before birth to show proportional effects. The dashed lines marks 9 months before the birth and the month of the birth.

Figure A.12: Robustness to migration into Washington State



Notes: This figure plots regression estimates of effects of childbirth from Specification 1 with 95% confidence intervals derived from standard errors clustered at the father level shown in dashes. Panel (a) includes all fathers determined to be born in Washington State through a link to an earlier birth record. Panel (b) includes all fathers with a juvenile arrest in Washington State. The outcome is an indicator for any economic, drug, DUI, or property destruction offense within the month. Estimates are divided by average arrest rates 9 months before birth to show proportional effects. The dashed lines marks 9 months before the birth and the month of the birth.

Table A.1: Effects of sample restrictions, mother sample

	All births	+Age restrictions	+Good match	+Mother's first
Demographics				
White	0.71	0.71	0.71	0.69
Black	0.04	0.04	0.04	0.04
Hispanic	0.11	0.12	0.11	0.13
Asian	0.09	0.09	0.09	0.10
Other or missing	0.04	0.04	0.04	0.04
Age	27.91 (6.01)	27.52 (5.63)	27.66 (5.59)	26.73 (5.75)
Birth				
Low birth weight (<2500g)	0.05	0.05	0.05	0.06
Twins+	0.02	0.02	0.02	0.02
Male infant	0.51	0.51	0.51	0.51
Marital				
Mother married at birth	0.73	0.73	0.75	0.71
Midpregnancy marriage	0.03	0.03	0.03	0.05
Divorce if married	0.18	0.18	0.18	0.18
Economic				
Median zipcode income	59834.99 (18187.96)	59661.75 (18093.91)	59900.66 (18191.49)	59944.48 (18110.35)
Mother on Medicaid	0.36	0.36	0.35	0.35
WIC	0.34	0.35	0.34	0.34
Crime				
Any arrest	0.25	0.25	0.22	0.19
Father ever incarcerated	0.04	0.04	0.04	0.04
Father ever on probation	0.09	0.09	0.08	0.07
Observations	983,687	955,914	908,480	532,790

Notes: This table shows how sample descriptives change as we implement restrictions sequentially for mothers. The first column includes all observations in the DOH birth records over the sample period, 1997 to 2009. The second column restricts to mothers between the ages of 15 and 40. The third column restricts to mothers who are unambiguously matched (or not matched) to the crime data. The final column restricts to the mother's first birth.

Table A.2: Effects of sample restrictions, father sample

	All births	+ Age restrictions	+Good match	+Father's first
Demographics				
White	0.66	0.66	0.67	0.65
Black	0.05	0.05	0.05	0.05
Hispanic	0.12	0.12	0.12	0.13
Asian	0.08	0.07	0.08	0.08
Other or missing	0.09	0.10	0.09	0.09
Age	30.21 (6.54)	28.99 (5.44)	29.15 (5.40)	28.23 (5.52)
Birth				
Low birth weight (<2500g)	0.05	0.05	0.05	0.06
Twins+	0.02	0.01	0.02	0.02
Male infant	0.51	0.51	0.51	0.51
Marital				
Mother married at birth	0.73	0.72	0.74	0.70
Midpregnancy marriage	0.03	0.03	0.03	0.05
Divorce if married	0.18	0.18	0.18	0.18
Economic				
Median zipcode income	59820.84 (18182.44)	59415.29 (17961.17)	59791.10 (18094.11)	59576.83 (17924.14)
Mother on Medicaid	0.36	0.37	0.35	0.36
WIC	0.34	0.35	0.34	0.35
Crime				
Any arrest	0.41	0.42	0.37	0.34
Father ever incarcerated	0.04	0.05	0.03	0.03
Father ever on probation	0.09	0.09	0.07	0.06
Observations	976,581	889,533	814,220	502,900

Notes: This table shows how sample descriptives change as we implement restrictions sequentially for fathers. The first column includes all listed fathers in the DOH birth records over the sample period, 1997 to 2009. The second column restricts to fathers between the ages of 15 and 40. The third column restricts to fathers who are unambiguously matched (or not matched) to the crime data. The final column restricts to the father's first birth.

Table A.3: Descriptives of married and divorced parents

	Married		Divorced	
	Mothers (1)	Fathers (2)	Mothers (3)	Fathers (4)
Demographics				
White	0.81	0.81	0.83	0.80
Black	0.02	0.03	0.03	0.05
Hispanic	0.05	0.05	0.03	0.05
Asian	0.09	0.07	0.07	0.06
Other or missing	0.03	0.03	0.04	0.04
Age	28.07 (5.02)	29.31 (4.88)	25.23 (5.07)	27.02 (5.11)
Birth				
Low birth weight (<2500g)	0.05	0.05	0.06	0.06
Twins+	0.02	0.02	0.01	0.01
Male infant	0.52	0.52	0.51	0.51
Marital				
Mother married at birth	1.00	1.00	1.00	1.00
Midpregnancy marriage	0.13	0.14	0.28	0.28
Divorce	0.00	0.00	1.00	1.00
Economic				
Median zipcode income	62839.21 (18206.16)	62454.59 (18074.11)	58908.64 (16025.63)	58587.03 (15785.47)
Mother on Medicaid	0.15	0.16	0.26	0.27
WIC	0.16	0.17	0.30	0.32
Crime				
Any arrest	0.12	0.29	0.29	0.54
Father ever incarcerated	0.01	0.01	0.05	0.04
Father ever on probation	0.03	0.03	0.12	0.09
Observations	135,774	126,777	10,319	10,145

Notes: This table reports summary statistics for mothers and fathers who were married vs. divorced five years after the birth. The overall sample includes all births in the primary sample matched to a marriage record and recorded as married on the birth certificate.

Table A.4: Papers on Crime and Childbearing or Marriage

Authors and Year	Journal	Data and sample size	Main results
Gottlieb and Sugie (2019)	Justice Quarterly	NLSY97, N=8,496	Both cohabitation and marriage are associated with reductions in offending
Mitchell et al. (2018)	American Journal of Criminal Justice	NLSY97, N=2,787 non-fathers, 1,772 fathers	Fatherhood is associated with decreased substance use but not the likelihood of any arrest
Pyrooz et al. (2017)	Criminology	NLSY97, N=629	Mothers and residential fathers have decreased likelihoods of gang membership and offending
Tremblay et al. (2017)	Journal of Child and Family Studies	Pathways to Desistance Study, N=1,170	Fatherhood is associated with greater risk exposure among serious juvenile offenders
Na (2016)	Journal of Developmental and Life Course Criminology	Pathways to Desistance Study, N=864 adolescents and N=476 young adults	Teen fathers report increased offending following childbirth; older fathers experience a slight decrease
Zoutewelle-Terovan and Skardhamar (2016)	Journal of Quantitative Criminology	Statistics Norway, N=289 & Netherlands' Municipal Population Register and Judicial Documentation, N=279	For at-risk mothers and fathers, decrease leading up to birth; increase to higher levels afterwards
Landers et al. (2015)	Journal of Child and Family Studies	NLSY 1997, N=478	Young fathers have decreased drug use controlling for individual fixed effects
Craig (2015)	Journal of Crime and Justice	Add Health, N=3,327	Marriage decreases offending among whites and Hispanics but not blacks; Parenthood only decreases whites' offending

Table A.4 – *Continued from previous page*

Authors and Year	Journal	Data and sample size	Main results
Theobald et al. (2015)	Australian & New Zealand Journal of Criminology	Australian & New Zealand Journal of Criminology & Cambridge Study in Delinquent Development, N=411	The number of convictions decreases after childbirth for men; this effect is greater if the child is born before or within nine months of marriage
Barnes et al. (2014)	Justice Quarterly	Add Health, N=15,701	Marriage is correlated with but does not cause desistance
Zoutewelle-Terovan et al. (2014)	Crime & Delinquency	Netherlands Ministry of Justice, N=540	Marriage and parenthood both promote desistance of serious offending for men but not women
Skardhamar et al. (2014)	The British Journal of Criminology	Norwegian Register, N=80,064	Offending declines the year before marriage followed by a slight increase after marriage; the rebound is due to those who split up
Craig and Foster (2013)	Deviant Behavior	Add Health, N=3,082	Marriage decreases delinquent behavior for both males and females
Monsbakken et al. (2012)	The British Journal of Criminology	Statistics Norway, N=208,296 persons (101,480 women and 106,816 men)	Offending declines permanently before childbirth despite slight rebound after
Bersani and Doherty (2013)	Criminology	NLSY97, N=2,838	Marriage decreases the likelihood of arrest; offending is higher when one is divorced than when one is married
Doherty and Ensminger (2013)	Journal of Research in Crime and Delinquency	The Woodlawn Project, N=965	Marriage reduces offending for men only
Jaffee et al. (2013)	Development and Psychopathology	Add Health, N=4,149	Marriage is associated with a lower rate of criminal activity

Table A.4 – *Continued from previous page*

Authors and Year	Journal	Data and sample size	Main results
Mercer et al. (2013)	European Journal of Criminology	Netherlands Ministry of Justice & Population Registration, N=540	Married males have a higher likelihood of committing violent offenses compared with non-married males; reverse is true for women
Barnes and Beaver (2012)	Journal of Marriage and Family	Add Health, N=2,284 sibling pairs	Marriage is associated with desistance; this effect decreases after controlling for genetic influences
Beijers et al. (2012)	European Journal of Criminology	Netherlands, N=971	Marriage is associated with desistance among high-risk men married after 1970 in the Netherlands
Salvatore and Taniguchi (2012)	Deviant Behavior	Add Health, N=4,880	Both marriage and parenthood reduce offending
Van Schellen et al. (2012)	Journal of Quantitative Criminology	Netherlands CCLS, N=4,615	Marriage is associated with decreased conviction frequency for women; only marriage to a non-convicted spouse is beneficial for men
Kerr et al. (2011)	Journal of Marriage and Family	US - Capaldi and Patterson (1989) Study, N=206	Men desist from crime and use alcohol and tobacco less frequently following childbirth
Giordano et al. (2011)	Journal of Criminal Justice	Toledo Adolescent Relationships Study (TARS), N=1,066	Mothers are more likely to desist from crime than fathers; parents from disadvantaged backgrounds have less desistance than those from advantaged ones

Table A.4 – *Continued from previous page*

Authors and Year	Journal	Data and sample size	Main results
Forrest and Hay (2011)	Criminology & Criminal Justice	NLSY79, N=2,325	Unlike cohabitation, marriage is associated with reduced crime, but effects decrease once controlling for self-control measures
Herrera et al. (2011)	Journal of Research on Adolescence	Add Health, N=1,267 opposite sex romantic pairs	Relationship quality and length are associated with decreased crime
McGloin et al. (2011)	European Journal of Criminology	Netherlands CCLS, N=4,612	The year of marriage and year after have the greatest effect on decreasing offending
Kreager et al. (2010)	Criminology	Denver Youth Survey, N=567	Teen and young adult motherhood is associated with decreased delinquency for disadvantaged women; controlling for motherhood and age, marriage is not associated with desistance
Petras et al. (2010)	Criminology	Netherlands CCLS, N=4,615	The effects of marriage on probability and frequency of conviction are both negative
Ragan and Beaver (2010)	Youth & Society	Add Health, N=1,884	Marriage is associated with marijuana desistance
Skardhamar and Lyngstad (2009)	Statistics Norway Discussion Papers	Norwegian Register (Marriage N=121,207; First birth=175,118)	Men desist from crime leading up to marriage/childbirth; some rebound for serious offenses
Bersani et al. (2009)	Journal of Quantitative Criminology	Netherlands CCLS, N=4,615	Marriage is associated with a decrease in the odds of a conviction; the effect for women is less than that for men

Table A.4 – *Continued from previous page*

Authors and Year	Journal	Data and sample size	Main results
Savolainen (2009)	The British Journal of Criminology	Statistics Finland, N=1,325	Cohabitation has a stronger effect on desistance than marriage; parenthood is associated with decreased crime
Thompson and Petrovic (2009)	Journal of Research in Crime and Delinquency	NYS, N=1,496	First childbirth increases odds of drug usage for men and women, except single mothers; marriage decreases odds of drug usage for men but women's drug usage depends on strength of relationship
Beaver et al. (2008)	Social Science Research	Add Health, N=1,555	Being married increases the odds of desisting
King et al. (2007)	Criminology	NYS, N=1,725	After accounting for selection into marriage, marriage has a significant but small effect on crime; the decrease is much greater for males than females
Massoglia and Uggen (2007)	Journal of Contemporary Criminal Justice	Youth Development Study, N=1,000	Relationship quality is positively correlated with desistance
Sampson et al. (2006)	Criminology	Glueck and Glueck study (1950), N=500 male delinquents and 500 male nondelinquents	Marriage is associated with a 35 percent reduction in the odds of crime for men
Maume et al. (2005)	Journal of Quantitative Criminology	NYS waves 5-6, N=593	Marriage promotes marijuana desistance only for those with high marital attachment

Table A.4 – *Continued from previous page*

Authors and Year	Journal	Data and sample size	Main results
Hope et al. (2003)	The Sociological Quarterly	Add Health, N=6,877	Adolescent girls who keep their babies reduce delinquent behavior compared to those with other pregnancy resolutions
Piquero et al. (2002)	Social Science Quarterly	California Youth Authority, N=524	Controlling for individual differences, marriage is negatively associated with violent, but not nonviolent, arrests
Graham and Bowling (1995)	Home Office Research Study	UK household survey, N=2,529	Having children is a strong predictor of desistance for females but not for males