Parenthood in Poverty

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Abstract

We provide comprehensive evidence on how new parenthood impacts the lives of economically disadvantaged women in the U.S. Using high-frequency individual-level administrative panel data from a large urban county combined with an event study design, we document how parenthood engenders far-reaching and persistent changes. New parenthood triggers large eligibility changes that sharply and greatly increase enrollment in Medicaid and SNAP by more than 50%, underscoring the profound importance of the social safety net for new mothers with low incomes. Simultaneously, new parenthood appears to motivate significant behavioral shifts, with increased uptake (+45%) in opioid use disorder treatment and notable reductions in criminal behavior (−48%). Yet, despite these positive changes, there are also significant challenges: first-time parenthood precipitates increased housing instability, causing a 50-70% in homeless shelter stays, and eventually leading to a large and lasting increase in reliance on public housing (+42%). Robustness checks, including two separate (matched) difference-in-differences analyses, suggest robustness to endogeneity in the timing of first parenthood.

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1. Introduction

Parenthood can profoundly reshape the lives of new parents, affecting far more than just the ability to work; it may alter housing needs, mental and physical health, the stability of relationships, and more. Of course, such disruptions might be more severe for individuals who are already facing unstable living conditions. Yet, for those individuals in particular, parenthood could also serve as a stabilizing influence, by providing better access to the social safety net acting, and by acting as a catalyst for positive change to "turn one's life around".

Despite their importance for safety net policy design, we have no large-scale empirical evidence on the effects of first-time parenthood on living conditions—beyond labor market effects—of new parents living in poverty. This lack of evidence is troubling because it concerns a significant part of the population: in the United States, 13.2% of families with children have incomes below the poverty limit, and this fraction rises to 31% for single-mother headed households, who make up 24% of all households with children (US Census Bureau, 2021).

In this paper, we investigate how pregnancy and parenthood shape key aspects of economic and psycho-social well-being among women of low socioeconomic status (SES) in the United States, aiming to reveal both the challenges and stabilizing forces that emerge. Using high-frequency administrative records from a large urban US county—Allegheny County, Pennsylvania—spanning the years 2005-2019, we capture a comprehensive view of new mothers' living conditions. The data includes detailed birth records, alongside information on health, housing, welfare benefits, and criminal justice involvement—allowing us to track transitions in areas like healthcare access and housing stability, as well as behavioral changes. Focusing on a sample of approximately 13,000 first-time mothers whom we identify as being of low socio-economic status based on their pre-pregnancy Medicaid enrollment, we explore how these transitions unfold with the onset of parenthood. We also cautiously extend the analysis to first-time fathers, acknowledging the limitations due to incomplete data on paternal identification.[1]

Our empirical strategy centers on an event-study design around first-time parenthood

[1] The birth records, which we use to identify parenthood, do often–38% of the time for children born to low SES mothers–not list a father, introducing selection concerns in the analysis of impacts of parenthood on men.

that controls for individual and time fixed effects, operating under the assumption that any endogenous confounds change smoothly around the timing of conception and birth. While this assumption is strong and may be violated in some cases, we use a four-pronged approach to reinforce the robustness of our findings. First, visual analysis of our raw outcomes reveals sharp, discontinuous shifts either at the discovery of pregnancy, at childbirth, or both, indicating strong event-related changes. Second, the raw data reveals a lack of pre-trends for most outcomes, and linear pre-trends for some—the latter of which are driven by general time trends that disappear with the inclusion of time fixed effects—further supporting the validity of our approach. Third, to further account for endogeneity in the timing of pregnancy, we perform two matched difference-in-differences analyses: one compares the outcomes of first-time mothers to a control group of similar demographics who have their first child two years later, while the other contrasts women who experience a live birth with those who have a miscarriage. Finally, we highlight that for many policy applications—particularly in "tagging" scenarios à la Akerlof (1978), where pregnancy or parenthood acts as a predictor for allocating services—the observed changes are intrinsically valuable, making the precise isolation of causal effects secondary in importance.

To circumvent issues with staggered event-study designs arising from treatment effects being heterogeneous across time or across treated units, we employ the "imputation estimator" by Borusyak, Jaravel and Spiess (2024) as our main estimator. It relies on estimating individual and time fixed effects based on pre-treated observations only. As shown in the robustness section, the results are unchanged when we use the traditional two-way fixed effects estimator.

We establish three main findings. First, new parenthood profoundly increases the use of key government assistance programs, highlighting the importance of the social safety net for first-time mothers living in poverty. Becoming a parent leads to a 28 percentage point increase in Medicaid coverage in the year following childbirth—a change that is more than twice the impact of the Affordable Care Act (ACA) expansion on this population. The enrollment in food assistance (SNAP) also increases substantially, by 16 percentage points. This rise in program participation is immediate—half of women enroll during the first trimester of pregnancy—and enduring, suggesting eligibility-driven changes in early pregnancy play a major role in providing access.

Second, parenthood triggers significant behavioral shifts, as evidenced by increased treatment for substance use disorders (SUD) and a profound reduction in criminal behavior. Treatment for opioid use disorder (OUD), the most prevalent SUD in our sample, rises by 45% (or 0.68pp) in the year after childbirth. This effect is not attributable to changes in insurance status, which we control for. The sharp onset of effects early on in pregnancy suggests that they are driven by women seeking treatment for pre-existing conditions, motivated by pregnancy (rather than with pregnancy leading women to increase their consumption of illicit substances). In terms of crime, we find that in the year following childbirth, criminal charges decrease by 48%, particularly for theft and drug offenses. This reduction is not driven by increased access to social programs, as it occurs regardless of Medicaid enrollment. Instead, the decrease likely reflects a combination of behavioral incapacitation and a desire to "turn one's life around"—a finding in line with the "turning point hypothesis" formulated by Sampson and Laub (1990).

However, not all changes are for the better. Our third main result shows that pregnancy and childbirth precipitate increased housing instability. During pregnancy, the incidence of homeless shelter stays—an extreme outcome in our data—jumps by 73% or 0.08 percentage points (pp), and stays in longer-term housing programs also increase after childbirth. Over time, this instability often translates into persistent reliance on public housing, with occupancy rising by 42% twelve months after childbirth. These effects are likely driven by real changes in housing needs rather than by eligibility changes resulting from pregnancy and parenthood: when studying the birth of a second child—an event that does not substantially change eligibility for homelessness services, since a child is already present throughout—we observe even stronger effects on the homelessness outcomes. They underscore the challenges new mothers of low socioeconomic status face in securing stable living conditions.

Together, our findings have significant policy implications. First, optimizing the *timing* of moving-to-opportunity and other housing mobility programs relative to life events like first-time parenthood could be extremely valuable. Given the increased residential mobility and reliance on housing assistance we document around first births, this period offers a window of opportunity for housing mobility programs to achieve higher take-up rates and willingness to move to high-opportunity neighborhoods, which can greatly improve child outcomes (Chyn and Katz, 2021). The increased housing instability we observe during this life period further suggests that such timing would yield particularly large benefits for both parents and children, aligning with research on the critical role of in-utero and early-childhood environments for development (Almond, Currie and Duque, 2018; Rossin-Slater and Persson, 2018), as well as the literature showing that the earlier children move to opportunity, the better their outcomes (Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018).

Second, the profound changes we document in criminal behavior and substance use treatment suggest that social factors like new parenthood may serve as a pivotal moment for fostering positive change. Thus, it is possible that also other social factors that provide a strong sense of meaning and purpose and that could be influenced by government programs—by returning social capital, economic opportunities, or both—might help improve individual welfare and generate positive externalities at the community level.

This paper contributes to the literature on the impact of parenthood by painting a more comprehensive and detailed picture of the effects of parenthood on the non-labor-market outcomes of low-SES individuals than has previously been possible. Most of the existing literature focuses on labor-market outcomes—that is, earnings and employment—with special attention to differences across gender (see the "Child Penalty Atlas" by Kleven and Leite-Mariante, 2024, for an overview). As far as non-labor-market outcomes are concerned, the closest papers to ours are Miller, Wherry and Foster (2023) and Massenkoff and Rose (2024).[2] The former study documents the effects of abortion denial among a sample of 600 women seeking to terminate their pregnancies. The authors find that abortion denial leads to increases in financial instability measured through credit reports, consistent with our finding of increased homelessness encounters. Massenkoff and Rose (2024) employ an event study design to investigate the effects of pregnancy on crime using administrative data from

[2] Also related is Stanczyk (2020), who studies the receipt of SNAP and TANF using self-reports from the SIPP in an event study framework. Furthermore, there are correlational studies on individual programs (Medicaid, SNAP, and SUD treatment) that largely rely on self-reported survey data (Daw et al., 2017; Adams et al., 2003; D'Angelo et al., 2015; Gordon, Lewis and Radbill, 1997; Kim, 2018; Wolfe et al., 2007). See Celhay, Meyer and Mittag (2021) for a discussion of systematic errors in self-reports for the case of government benefits.

Washington State and also find that pregnancy leads to large reductions in criminal behavior.[3] We contribute to this literature in three main ways: first, we study a broader set of domains than has previously been possible. Second, we employ a multi-domain approach that allows us to explore the effects of pregnancy and parenthood across multiple life dimensions relevant to well-being at once. This approach provides a unique and nuanced understanding of how social assistance uptake, behavioral changes, and housing transitions are interconnected and unfold over time. Third, except for Miller, Wherry and Foster (2023) and the study of criminal behavior, we can estimate more precise and robust effects and trace out changes at a high resolution, thanks to high-quality administrative data available at high frequency and encompassing a large sample.

This paper also contributes to a large and growing literature on the causes of economic distress. It is similar in methodology to studies about the economic consequences of adverse life events, such as health shocks or the death of a spouse (Dobkin et al., 2018; Fadlon and Nielsen, 2021). We add to this literature by providing evidence on how the major life event of parenthood affects life outcomes, by showing that parenthood can trigger significant shifts in social assistance use, behavioral health, and housing stability. Finally, this paper contributes to the literature on housing instability and homelessness.[4] Most closely related, Curtis et al. (2013) study how homelessness rates differ between families with a healthy child and those with a child born with a severe health condition. More recent work explores the role of evictions and eviction policies in causing homelessness (Collinson et al., 2024; Abramson, 2024). The remaining literature, rather than focusing on the causes of homelessness, largely focuses on evaluating different homelessness service programs and the expansion of funding for homelessness services (e.g. Lucas, 2017; Corinth, 2017). We contribute to this literature by providing evidence that pregnancy and childbirth are important drivers of housing instability and homelessness.

The rest of the paper proceeds as follows. Section 2 describes the setting, data, sample, and outcomes. Section 3 outlines our empirical strategy. Section 4 presents our main results,

[3] See Massenkoff and Rose (2024) for a detailed review of the literature related to criminal behavior and family formation, which has largely focused on the impact on fathers (e.g. Britto et al., 2024; Dustmann and Landersø, 2021; Savolainen, 2009).

[4] See Evans, Phillips and Ruffini (2019) for a review of the literature on homelessness prevention.

as well as mechanism and heterogeneity analyses; Section 5 presents robustness checks. Section 6 shows results for first-time fathers. Finally, Section 7 concludes.

2. Setting, Data, and Definitions

2.1 Setting and Data Sources

Setting We use a comprehensive set of administrative records for all residents of Allegheny County, a large US metropolitan area including the city of Pittsburgh, located in the state of Pennsylvania. Its 1.2 million residents—25% of them reside in Pittsburgh—stand out as strikingly representative of the US as a whole in terms of socioeconomic and demographic make-up. Based on 2015-2019 American Community Survey 5-year and US Census Bureau estimates presented in Table A.1, in Allegheny County (nationwide), the median household income is $60,000 ($61,000), the share of the population living below the federal poverty level is 13% (14%). The share of households with children headed by a single parent is 33% (32%), and 14% (13%) of the population is of Black race/ethnicity. Rent levels are also very similar to the national average, with a 2-bedroom apartment renting for $890 on average, compared to $980 nation-wide. The only notable differences are a much lower population share that is foreign-born (5% vs. 13% nationwide) and a much lower population share of Hispanic ethnicity (2% vs. 16% nationwide). Among all adult residents in the county, 19% are Medicaid-insured (Allegheny HealthChoices, 2017). Among all births in the county, 27% are to Medicaid-insured mothers (Pennsylvania Department of Health, 2018).

Data Source The data used for this analysis spans birth records, housing, health, public assistance program use, and crime, and covers the years 2005-2019. It is collected and stored in the Allegheny County Data Warehouse, a centralized data warehouse established by the county's Department of Human Services (DHS) in 1999 to improve DHS planning and decision-making (Kitzmiller, 2013). The data covers all individuals, who at any point between 2005-2019 resided in the county,[5] and includes a unique identifier that is used to link a resident's records across domains. Records were provided to the research team as anonymized individual-level panel data at a monthly frequency. We provide an overview of each data element in Table A.2, and describe each element in more detail in Appendix B.

From a data depth and breadth point of view, the Allegheny County data is ideal because it provides a comprehensive set of key markers of well-being and economic hardship at a high frequency and of high quality. It includes important domains that are traditionally difficult to observe in survey data (e.g. homelessness and substance use disorders) and to link across domains in administrative data.

2.2 Sample selection

Our primary aim is to study the effect of pregnancy and childbirth on the lives of economically disadvantaged individuals. We focus on this demographic group because they are particularly vulnerable to economic shocks (and thus may experience disproportionate impacts on economic security and well-being), they are particularly relevant for policy design (since more reliant on public assistance programs), and they are an underexplored population. Thus, from the sample of all county residents, we first identify the occurrence and date of first-time parenthood, and second identify the socioeconomic status.

Identifying first birth events Using birth record data covering all births in Allegheny County between 1999 and 2020, we extract records for all 248,000 children born between 2007 and 2020. This choice of period guarantees that we have at least two years of pre-birth outcome data for each parent since we observe all outcomes back to at least 2005.

For all but 130 children, a mother is identified on the birth record, yielding ca. 156,000 unique mothers. In contrast, no father is listed on 39,000, or 16%, of birth records and this fraction rises to 38% for economically vulnerable children—those whose birth is paid for through Medicaid. This sizeable, likely selective attrition of fathers on birth records motivates

[5] As common with administrative records at the sub-national level, we do not observe in- and out-migration (see, e.g. Grogger, 2013, for a discussion of this issue). Consequently, we perform several robustness checks in Section 5 that focus on sub-samples with ex-ante low likelihoods of out-migration, finding our results essentially unchanged.

our decision to focus on women for our main analysis; we report results for men in a shorter section after the main analysis.

We further restrict the sample to those ca. 100,000 women who have their *first* birth in the sample period. We focus on first births because we expect any changes to living conditions to be strongest for new parents.[6] We identify first-time mothers as those for whom no birth record from a date earlier than 2007 (and after 1998, the earliest year for which we observe birth records) exists, and whose birth record of the first observed birth between 2007 and 2020 lists the number of previous live births as zero. We further exclude the 2% of women who experience the relevant birth event at ages younger than 16 or older than 40 because of small cell sizes, resulting in a sample of 97,400 first time mothers.

Identifying individuals of low SES Since we do not observe education and income directly, we proxy for low SES with receipt of public assistance ahead of the first pregnancy. Specifically, we construct a low SES indicator that equals one if we observe the person is Medicaid-insured at any point during the five years leading up to the pregnancy. We choose this criterion because it captures a large fraction of low SES individuals: Medicaid is the largest means-tested program in the United States (Congressional Budget Office, 2013), and its eligibility cutoff for household income—138% of the Federal Poverty Level (FPL)—captures the 17% poorest households in Pennsylvania (US Census Bureau, 2018); furthermore, its take-up rate is relatively high—estimated at 70% among adults and 80-90% among children (Sommers et al., 2012)—and thus ensures we are not missing too many economically disadvantaged individuals. For completeness and robustness, we also provide results for the entire sample of first live births, as well as for alternative low SES criteria, such as pre-pregnancy SNAP receipt, pre-pregnancy Medicaid or SNAP receipt, and childhood Medicaid enrollment.[7] We focus on the five-year window before pregnancy because it is the longest window we can observe for all women in the sample since our Medicaid data starts in 2002 and we include births from 2007 onward.

[6] We explore differences in effects around first and second births in Section 4.3 to uncover the mechanisms behind the changes we observe around first birth.

[7] We find that the Medicaid criterion is indeed comprehensive. It captures 82% of individuals for whom we observe *any* use of public assistance programs in the five years leading up to pregnancy (including housing assistance, SNAP/TANF, and Medicaid).

It is important to note that low-income adults aged 21 and over without dependent children became eligible for Medicaid only later in our sample period. Pennsylvania's Affordable Care Act (ACA) expansion rendered this previously ineligible group eligible so long as household income is below 138% of FPL starting in June 2015 (Kaiser Family Foundation, 2021*b*).[8] Hence, relative to the full sample of first-time mothers in the county with incomes below 138% of FPL pre-pregnancy, our sample criterion misses older (> 25 years old) first-time moms with births before 2015. Furthermore, since we only capture the estimated 70-90% of Medicaid-eligibles who take up the benefit, the sample also skews towards those more familiar with government assistance.

Of the approximately 97,400 first birth events observed in our sample period, 16% are to women whom we identify as low SES. In our discussion of sample demographics in Section 2.3, we compare demographic characteristics of the low SES sample to its non-low SES counterpart, documenting clear markers of socio-economic vulnerability in the low SES sample relative to the non-low SES sample.

Selecting the event time window For our event study regression, we restrict observations to a window of one year before the approximate date of conception to one year after birth, covering a total of 33 months per individual.[9] Including "only" twelve pre-conception months allows us to control for more precise and accurate pre-trends in event time; restricting the post-birth observations to a one-year window, as opposed to a longer time horizon, ensures that our difference-in-differences imputation estimator, which predicts post-birth outcomes based on pre-conception observations, does not extrapolate out too far. Since our outcome data does not extend beyond September 2019, we estimate treatment effects only for individuals for whom we have complete panel data—that is, all 12,928 individuals whose first childbirth falls into the period January 2007 to September 2018.

[8]For individuals aged 6-20, the maximum household income threshold increased from 100% to 138% of FPL in 2014 (Kaiser Family Foundation, 2021*a*).

[9]We set the approximate date of conception to nine calendar months before the month of childbirth. This approximation is "conservative" in that pregnancies may last shorter than nine months, but rarely last longer.

Sub-sample for substance use disorder analysis Since we observe substance use outcomes only for Medicaid-insured individuals, we limit this sub-analysis to those continuously insured by Medicaid throughout the event time window. This approach ensures that observed changes around the birth event reflect actual changes in service receipt, rather than changes in the visibility of such services due to fluctuating insurance status. This restriction reduces the sample size to about 2,700 individuals, or 21% of the full sample. Compared to the overall low-SES cohort, women in this restricted sample are on average 1.3 years younger at the time of their first birth and display greater economic vulnerability, with 2.6% experiencing homelessness in the year before pregnancy (versus 1.7% in the broader low-SES sample).

2.3 Summary Statistics

In Table 1, we present summary statistics for our main event study sample of first-time mothers of low SES in column (1), alongside statistics for all other first-time mothers in column (2). As expected, the low-SES sample displays significantly greater economic vulnerability. Compared to non-low-SES mothers, they are much younger at first birth (average age 22 vs. 28), more likely to be underage (10% vs. 1%), more likely to be Black (52% vs. 8%), and more likely to have no father listed on the birth certificate (43% vs. 9%). Additionally, a higher percentage of these women receive SNAP benefits in the year before pregnancy (38% vs. 1%) and experience at least one homelessness encounter (2% vs. 0%) or contact with the criminal justice system (11% vs. 1%).

While our data does not explicitly record the fraction of births resulting from unintended pregnancies, we estimate that this figure is around 47%, based on studies of similar populations.[10]

2.4 Outcome Construction

Our analysis focuses on six primary outcomes spanning the domains of social assistance program use, behavioral outcomes related to substance use and crime, and housing. For each

[10] According to Finer and Zolna (2016), approximately 60% of viable pregnancies are unintended among American women aged 20-24 or with incomes below the poverty line, and around 60% of those unintended pregnancies are not terminated. Thus, in this demographic group, out of 76 live births, 36 are on average unintended.

outcome, we construct individual-month level indicators that equal one in case a given event occurred that month, and zero otherwise; we provide a short description below and more details, including on secondary outcomes, in Appendix B.

Social Assistance Program Use In the domain of social assistance, we observe enrollment in key programs for healthcare coverage, food assistance, and cash assistance available to individuals with low incomes in the United States: *Medicaid*, *SNAP*, and *TANF*. We consider the first two as primary and the last one as a secondary outcome. The outcomes are coded as dummies that equal one if the person (or their household, in the case of SNAP and TANF) is enrolled in a given program that month.

Substance Use Disorders Our mental health claims data captures treatment encounters for mental health disorders paid for through Medicaid. While this dataset allows studying a range of mental health outcomes, we focus specifically on SUDs for several reasons. SUDs are not a fringe issue: in our sample of first-time mothers of low SES, 11% have been treated for a SUD at least once in their life before their first pregnancy—33% of them for opioid use disorder (OUD), the most common substance use disorder observed in our data. These disorders substantially burden affected individuals, their children, and society as a whole (Degenhardt and Hall, 2012; Romanowicz et al., 2019; U.S. Department of Health and Human Services, 2016). Despite the availability of highly effective treatments, these services are often underutilized (Blanco et al., 2013). Moreover, there is limited quantitative evidence on the individual determinants of treatment uptake beyond correlational studies.

Our primary outcome of interest is a dummy that equals one in months in which a person receives treatment for opioid use disorder, the most prevalent SUD in our data (*OUD treatment*). As secondary outcomes, we consider different types of OUD treatment, treatment for any substance use disorder, as well as treatment for the next most common SUDs.

Criminal Behavior For our analysis of criminal behavior, the primary outcome indicator, *Criminal offense*, is defined as one in any month when a new criminal charge is filed in a county court and zero otherwise. As secondary outcomes, we differentiate between felony and misdemeanor cases. Among felony cases, we further categorize them into major types, including assault, theft, drug possession, DUI charges, and a combined category for all other charges.

Housing Housing is a key determinant of well-being and may be particularly sensitive to the changes brought on by having a child. For individuals of low SES, pregnancy and childbirth can lead to short-term housing instability if existing housing arrangements end suddenly without sufficient savings to secure new accommodations (e.g., due to expulsion from a parental home or conflict in a romantic relationship). In the longer term, the financial pressures of parenthood may drive a shift to more affordable housing options to manage increased space requirements and child-related expenses.

Our data enables us to capture both short-term and long-term aspects of housing transitions. We measure short-term housing instability through homeless shelter stays (*Homeless shelter*). For longer-term housing changes, we track reliance on key housing support programs observable in our data. These programs fall into two categories: medium-term support designed for individuals experiencing homelessness, such as Rapid Rehousing, Transitional Housing, and Permanent Supportive Housing—grouped under the secondary outcome *Long-term homeless*; and rental subsidy programs aimed at the low-income population, including residence in *Public housing*—our other primary housing outcome—and the receipt of *Section 8* rental assistance.

2.5 Program Eligibility Rules

To draw welfare conclusions we need to understand whether observed changes in outcomes reflect changes in *underlying need* (i.e. demand-side factors), *eligibility* (i.e. supply-side factors), or *information*. Eligibility criteria are the only elements readily observable to us. Hence, we collected information on program eligibility rules for each outcome in our data. We provide a detailed overview in Table A.3 and a short summary below.

Eligibility for Medicaid and SNAP increases substantially when individuals transition from a household with no dependent children, to pregnancy, to a household with dependent children. For example, in the case of Medicaid, the income eligibility threshold for a woman living alone changes from \$1,400 per month before pregnancy, to \$3,100 during pregnancy, to

$2,000 post childbirth.

The homelessness assistance environment also changes as individuals transition into parenthood, though not with additional children—a feature we will exploit to investigate mechanisms later. Both homeless shelters and long-term homeless housing are provided in separate facilities depending on whether a child is present, potentially altering the supply and quality of program slots for women as they become parents.

In contrast, eligibility for substance use disorder treatment (conditional on Medicaid enrollment) and public housing does not change significantly with family status. SUD treatment is covered by Medicaid for both pregnant and non-pregnant individuals. Furthermore, a recent RCT on opioid use disorder documents that pregnancy status does not increase treatment access conditional on attempting to make an appointment (Patrick et al., 2020). Public housing, allocated via waitlists that do not prioritize pregnant women or families with children, bases placement solely on the order of application submission (Allegheny County Housing Authority, 2021). However, larger households are eligible for bigger units, potentially affecting wait times (Allegheny County Housing Authority, 2020), and, although against official policy, housing authority administrators could use discretion to prioritize pregnant women or families with young children.

In summary, eligibility for Medicaid, SNAP, and homelessness assistance changes significantly during pregnancy and after childbirth. In contrast, eligibility for SUD treatment and public housing remains relatively unchanged with family status.

3. Empirical Strategy

The primary goal of this paper is to map the impact of parenthood on living conditions for economically vulnerable women. In the absence of a randomized experiment, we leverage detailed panel data in an event study framework based on sharp changes around pregnancy and childbirth to estimate causal effects.[11] However, clearly, unobserved changes to life

[11] Compared to instrumental variable approaches that leverage variation in access to abortion or IVF—neither of which are government-funded in the U.S. and thus neither are visible in administrative data—this method allows us to estimate average impacts across all first-time mothers, not just those on the margin of using these treatments.

circumstances may impact the decision to engage in "risky" sexual behaviors (for unplanned pregnancies) or to conceive a child (for planned pregnancies) and may also impact domains such as housing and crime. Under the assumption that such endogenous factors evolve smoothly around the exact time of conception/childbirth, we can recover the impact of parenthood via estimating discontinuous changes from such smooth trends at the event of childbirth (Kleven, Landais and Søgaard, 2019). The validity of this approach is much enhanced by using high-frequency, monthly-level individual panel data—a type of data not typically available in landmark studies on the childbirth penalty.

Our empirical approach proceeds in three steps. First, we analyze raw means of the outcome variables over time relative to a woman's first live birth. Plotting raw means allows us to visually assess the existence of pre-trends and the sharpness of changes upon discovery of pregnancy and childbirth. Second, we present event study estimates that net out overall time trends and fixed differences across individuals who give birth at different times. Third, we implement two complementary identification strategies—a one-to-one matching approach that matches observably similar women who give birth two years apart and a difference-in-differences analysis that compares the trajectories of women who experience a miscarriage to those who have a life birth.

3.1 Event Study Specification

For the baseline event study analysis, we follow recent advances in the econometrics literature by using the "imputation" estimator developed in Borusyak, Jaravel and Spiess (2024). It addresses limitations of conventional event study methods that may yield inconsistent estimates when treatment effects vary across time or units. We also present results using a traditional two-way fixed effects estimator, finding our main results unchanged. Following Borusyak, Jaravel and Spiess (2024), the imputation estimator is constructed in three steps: (1) estimate individual and time fixed effects using only pre-treatment (i.e. pre-conception) observations; (2) use these estimates to impute untreated potential outcomes for post-conception periods; and (3) calculate the treatment effect for a given relative period as the average difference between actual and imputed outcomes across all individuals.

In the first step, this approach relies on a simple two-way fixed effect model with individual and calendar year-month fixed effects, estimated among the *untreated observations* only, via OLS:

$$y_{it} = \alpha + \mu_i + \gamma_t + \epsilon_{it}, \tag{1}$$

where y_{it} is the outcome of interest for individual i in calendar year-month t, where μ_i and γ_t are individual and calendar year-month fixed effects. In our context, "untreated observations" are all those observed ahead of a woman's pregnancy that results in her first live birth. In robustness checks, we also control for a linear pre-trend in event time, finding that it leaves our results unchanged.

In the second step, we obtain observation-level causal effect estimates as the difference between actual and predicted outcomes, for each *treated observation*:

$$\hat{\tau}_{it} = y_{it} - \hat{y}_{it}, \tag{2}$$

where $\hat{y}_{it}$ is the prediction obtained from model Equation (1). Treated observations are all observations occurring at or after the onset of pregnancy.

Finally, our target treatment effects are then estimated as simple averages across observations for relative event time periods. We report results for two types of periods: First, to trace out dynamic effects in as much detail as possible, we show treatment effects for each month relative to conception in event study figures. Second, to summarize the magnitude of estimated effects, we bin relative event time months into two aggregate periods—pregnancy, and year post-birth—and report results in table-form. We report conservative standard errors clustered at the individual level, whose formula is derived and shown to be valid in large samples in Borusyak, Jaravel and Spiess (2024).

3.2 Assessing Raw Trends and Pre-Trends

Given that pregnancy likely occurs with a lag relative to any changes in living conditions that also influence the outcomes of interest (such as meeting a new partner), and given the high-frequency nature of our outcome data, we first visually and informally check for pre-trends in the raw data. The left panels in Figures 1-3 graph the time series of raw mean outcomes relative to the month of first childbirth.

Across all outcomes, the raw time series reveal either small and smooth linear trends or no trends leading up to the pregnancy, as well as sharp trend breaks either around the discovery of pregnancy in months 2-3, or around the month of childbirth, or both. For those outcomes with pre-trends in the raw data, we find that those trends disappear in the event study upon the inclusion of individual and time-fixed effects, suggesting that they are due to a combination of overall time trends and aging (and thus not biasing our event study results).[12]

To formally assess whether this specification accurately nets out any pre-trends, we test for and reject the presence of pre-trends across all our twelve outcome variables, using the pre-trend test derived by Borusyak, Jaravel and Spiess (2024).[13] Results from this test are reported in the bottom row of our main results Table 2.

3.3 Matching and Miscarriage Specifications

To the extent that the onset of pregnancy is correlated with sharp changes to living conditions, residual endogeneity could remain even in the absence of pre-trends. Therefore, we provide further evidence with a difference-in-differences analysis, comparing women who experience live births to those who experience miscarriages (similar to Massenkoff and Rose, 2024). This design addresses the potential endogeneity in the (sharp) timing of pregnancy. Finally, to directly net out any sudden age-related effects (that could bias our results in case pregnancy onset correlates with, for example, finishing high school), we employ a matched difference-in-differences analysis that compares a woman's change in outcomes around childbirth to the contemporaneous change of a matched control peer of the same cohort with similar demographic characteristics who gives birth two years later. We describe the two empirical strategies in detail in Appendix C and discuss their results, which are in line with those of

[12] Accordingly, in a robustness check that includes a linear pre-trend control in event time in our event study specification, we find that magnitudes remain unchanged.

[13] The test works as follows: first, estimate the model from Equation (1) on untreated observations via OLS, including dummies for each of the six (out of 12) months immediately preceding conception. Second, use the Wald test statistic to test whether the six pre-treatment dummies are jointly equal to zero.

our main specification, in Section 5.

4. Results

We first present results for our main outcome variables (sub-sections 4.1-4.3), followed by analyses of mechanisms, heterogeneity, and long-term effects (subsections 4.4-4.5).

4.1 Impacts on Social Assistance Program Use

In this section, we show that pregnancy and parenthood lead to very large increases in the use of social assistance programs, and that much of the increase is likely driven by eligibility changes.

Figure 1 shows raw scatter plots and event study results for the impact of pregnancy and parenthood on healthcare coverage and food assistance side-by-side. A summary of the corresponding effect sizes averaged over pregnancy and over the first year of parenthood is provided in columns 1-2 of Table 2. For both outcomes, the figures reveal extremely flat pre-trends, followed by sharp and very large increases at key moments around pregnancy and childbirth.

In terms of magnitudes, we find that having a child increases Medicaid and SNAP enrollment in the year after childbirth by 28pp and 16pp, respectively, corresponding to a more than 50% increase relative to the pre-pregnancy mean.[14] For the case of Medicaid, we benchmark this magnitude against two other events that greatly impact Medicaid eligibilty—the ACA expansion, and aging out of Child Medicaid. We find that effect of first-time parenthood on Medicaid enrollment trumps that of the former by a factor of more than two, while it is similar in magnitude to the effect of the latter (see Figure A.1).

4.2 Impacts on Behavioral Outcomes

SUD Treatment We find that new parenthood leads to a large, 45% increase in the treatment for Opioid Use Disorder, and that this increase is likely driven by individuals with

[14] We also find a 15pp increase in enrollment in the cash assistance program TANF, see Table A.4.

pre-existing disorders commencing treatment.

The top panel of Figure 2 presents a time series of raw means of treatment for OUD on the left, and the associated results from the event study specification on the right; a summary of the corresponding effect sizes in table form is provided in column 3 of Table 2. The event study figure shows that treatment for OUD starts increasing around four months after conception, and remains at a relatively stable level in the year after childbirth. The magnitude of the effect is substantial: we estimate an increase of 0.34pp (or 23% relative to the pre-pregnancy mean) during pregnancy, and an increase of 0.68pp (or 45%) in the year post-childbirth, compared to the no-pregnancy/no-child counterfactual.[15]

We present results for secondary SUD outcomes in Table A.4. In terms of treatment for any substance use disorder, we estimate a 1.21pp (or 47%) increase in the year post-childbirth, 56% of which is accounted for by OUD. When investigating different treatment types for OUD, we find large increases in medication-based treatment (such as methadone and buprenorphine), which has been shown in the medical literature to be highly effective in non-pregnant patients (Mattick et al., 2014), and is also strongly recommended in pregnant patients (World Health Organization, 2014).[16]

Criminal Behavior Next, we investigate the impacts of parenthood on criminal behavior, the direct and indirect consequences of which shape the lives of many individuals in economically vulnerable communities: in our sample of first-time mothers of low SES, 25% have been charged with a criminal offense at least once in their life before their first pregnancy. We begin by documenting overall effects on criminal behavior that are in line with findings from Massenkoff and Rose (2024), before analyzing mechanisms including the role of access to government assistance, such as healthcare coverage.

The bottom panels of Figure 2 show that pregnancy and childbirth lead to a substantial reduction in criminal behavior. Criminal behavior decreases gradually upon the discovery of

[15]In Appendix D.1 we combine this finding with our results from the previous section to discuss the implications of pregnancy-related health insurance churn.

[16]Furthermore, we find evidence of substitution from rehab-based to outpatient-based treatment (columns 3-4 of Table A.4). Considering the next most prevalent substance use disorders after opioid use disorder (cannabis, alcohol, and cocaine), we detect no statistically significant effects on treatment for any of the three disorders in the year after childbirth (columns 6-8).

pregnancy, reaches its lowest point in the month of birth (a 60% decrease from a base rate of 1.7% pre-pregnancy), to then increase again, but stays significantly below its pre-pregnancy level even one year after birth. Summarizing event study estimates into more aggregate time periods in Table 2, we find sizeable and statistically significant effect sizes of -0.67pp and -0.83pp during pregnancy and the year after birth, respectively. Relative to the pre-pregnancy mean of 1.74%, the decreases correspond to -39% and -48%, respectively.[17]

When distinguishing the two sub-components of criminal offenses, misdemeanor and felony offenses, we find significant reductions of similar magnitudes to both (see the first two columns of Table A.5). In terms of felony offenses, we observe the largest impact on criminal charges related to theft and controlled substances (columns 4-5).

4.3 Impacts on Housing

In this section, we first present results on short-term housing solutions in the form of homeless shelter visits, and then present results on long-term housing solutions.

Short-Term Emergency Housing Assistance While homeless shelter stays are a relatively rare occurrence even among economically vulnerable individuals—the cumulative risk of having at least one homeless shelter stay in the year before pregnancy is 2% in our sample—we find that pregnancy and new parenthood increase this risk substantially. The top panel of Figure 3 contains two graphs showing the use of homeless shelters surrounding pregnancy and childbirth: the left figure presents a time series of raw means; the right panel traces out average effects for each month relative to conception, obtained from event study analysis as described in Section 3. The figure shows significant evidence that shelter visits increase due to pregnancy and suggestive evidence that they also remain at a higher-than-baseline rate after childbirth. Column 5 of Table 2 summarizes causal effect estimates by averaging the monthly estimates into the two aggregate time periods of pregnancy and the year after birth. The magnitudes of the effects are substantial: during pregnancy, homeless-shelter visits increase by 0.08pp (73%) compared to the no-child counterfactual—an estimate that

[17]The magnitudes are consistent with Massenkoff and Rose (2024), who document effects on the order of a 70% decrease around birth on arrests among first-time mothers of Washington State.

is highly statistically significantly different from zero; the coefficient estimate for the year post-birth is of similar magnitude, but noisier. These results suggest that childbirth and especially pregnancy may generate substantial short-term housing disruptions for low SES women.

Long-Term Housing Assistance We present the raw time series and event study plot for public housing in the bottom panels of Figure 3; results in table-form are in column 6 of Table 2. We present results for secondary outcome (for long-term homelessness programs and Section 8) in columns 7-8 of Table A.5. For all three housing programs, we observe an increase in use after childbirth, but the magnitude and precision of the estimates vary considerably. The starkest pattern emerges for public housing: we find statistically significant, positive effects starting two months before childbirth that increase linearly with time such that, twelve months after childbirth, parenthood increases the share of women who live in public housing by 42% (or 2pp), compared to the no-child counterfactual. Results for our secondary housing outcome that proxies for living outside of one's parental household—using information on whether a person is registered as "head of household" in her subsidized housing—suggest that the effect is not driven by moves back into the parental household. It is more likely due to moves *out of* parental households straight into public housing: we find a large positive effect of new parenthood on the probability of heading a household in public housing. The effect size of 1.74pp (or 171% relative to the pre-pregnancy mean) is even larger than that observed for public housing residence, overall (Table A.5).

The effects on Section 8 rental subsidy receipt are more imprecisely estimated, begin later, and are about a quarter of the size of those on public housing in the year after birth. This suggests public housing more readily addresses women's short-term housing needs due to new parenthood. Most likely because it is *less* desirable than Section 8, making it more readily available: in Allegheny County, the average waitlist time for public housing is 9.2 months, compared to nearly three years for Section 8 (Deitrick et al., 2011). Given the persistence of housing choices and evidence that Section 8 rental assistance leads to better child outcomes than public housing (Chyn, 2018), the welfare loss from directing new mothers into public housing instead of prioritizing them for Section 8 could be substantial.

4.4 Mechanisms

In this section, we investigate the mechanisms driving the observed effects for each of our outcome variables, finding that mechanism vary widely across outcomes. The large increases in enrollment in key government assistance programs is likely largely eligibility-rule driven, while the increase in SUD treatment and decrease in criminal behavior is likely due to motivational factors; finally, the increase in homeless shelter stays is likely due to a real increase in housing instability.

Social assistance programs The observed increases in benefit enrollment around first-time parenthood could be purely eligibility rules-driven, or could be due to changes in other factors (such as income). Our findings suggest that eligibility expansions are the dominant force driving these trends. We see a sharp and significant rise in benefit uptake two to three months after conception—approximately when the pregnancy is discovered. This timing is crucial, as it precedes any significant decline in earnings but aligns with the more lenient eligibility criteria for pregnant women. Furthermore, benefit enrollment shows additional sharp changes at key program milestones: two months postpartum for Medicaid, and around the month of birth for SNAP, which coincide with further eligibility changes due to updated family status.[18]

These findings highlight that new parenthood is one of the most critical life events driving access to public benefit programs for individuals with low incomes in the U.S. They are consistent with Han, Meyer and Sullivan (2021), who highlight the important role of policy in explaining the diverging trends in consumption patterns of low-educated single mothers over the last 30 years, relative to trends among low-educated single women without children.

SUD treatment Our data does not allow us to determine with certainty whether the increased treatment for OUD is due to increased treatment for already preexisting, non-worsening OUDs, vs. new cases or a worsening of OUD caused by pregnancy and parenthood.

[18]For Medicaid, the income eligibility threshold becomes significantly stricter 60 days postpartum (Kaiser Family Foundation, 2021*c*). The drop in SNAP benefit receipt in the two months post-birth is due to a special nutrition program (WIC) for breastfeeding mothers that substitutes for SNAP benefits during the first three months after birth.

However, the timing of the increase points to the former story rather than the latter. Specifically, as shown in Figure 2, medical encounters for OUD increase sharply in months 3-4 of pregnancy, which is arguably when women find out about their pregnancy and begin to visit health providers more assiduously for pregnancy-related health checks. The increase is thus consistent with referral to treatment by medical providers at pregnancy-related encounters, as well as increased motivation on the part of the pregnant woman to treat her disorder to protect her unborn child. Qualitative evidence suggests an important role for such motivational factors: pregnant women in substance use disorder treatment report their pregnancy as the top treatment motivator (Jackson and Shannon, 2013).

Hence, our findings suggest that new parenthood can be an important push factor out of untreated substance use disorders.

Criminal behavior The breadth of our data allows us to investigate key mechanisms behind the observed decrease in criminal behavior. Specifically, on the one hand, the reduction in criminal behavior might be due to pregnant women's desire to "turn one's life around"—the so-called "turning point" hypothesis formalized by Sampson and Laub (1990); on the other hand, we document in Section 4.1 a large increase in access to key social assistance programs providing healthcare coverage, food and cash assistance, which may in turn decrease the need to engage in criminal behavior.[19] In particular, the crime-reducing effects of benefit receipt have been documented by Jácome (2022) for the case of healthcare coverage, Carr and Packham (2019) for the case of food assistance, and Foley (2011) and Deshpande and Mueller-Smith (2022) for the case of cash assistance.

To disentangle the two mechanisms, we split the sample into two distinct groups: those who had access to key government assistance programs all along (the *"Access all along"* group), and those who could potentially gain access due to new parenthood (the *"Gained access"* group).[20] We present time series of mean outcomes separately for each group in Figure A.2,

[19]A third channel, yielding similar predictions as the turning point hypothesis, is that of (physical) incapacitation due to late-stage pregnancy and/or childcare responsibilities.

[20]*"Access all along"* is defined as enrollment in a given government benefit program for at least 90% of the event time window (29 out of 33 months). *"Gained access"* is defined as having been enrolled at most 20% of the months preceding pregnancy (2 out of 12 months). The total sample size for this analysis includes 6,653 women, 50% of whom fall into the *Access all along* group.

adjusting for cohort, year of childbirth, and race. Panel B shows that the propensity for criminal offending follows a remarkably similar trajectory in both groups—including a marked decrease during pregnancy—suggesting that gaining access to Medicaid is not the primary driver behind the decrease.[21] This finding is consistent with access to healthcare coverage driving at most a small part of the negative effect of childbirth on crime observed in the period after childbirth for the average woman in our sample. Rather, the observed trajectories are more consistent with mechanisms of incapacitation, a motivation to turn one's life around, or both.

Homelessness We find that increase in homeless shelter stays due to first-time parenthood likely reflect real increases in housing disruptions, as opposed to changes in eligibility for/referral to homeless services due to changes in family status: when comparing effect sizes across first and second births for women for whom we observe two births, we find that effects of the second birth—where eligibility is unlikely to change substantially since a first child is already present—are at least as large if not larger than those observed around the first birth (see Figure A.3).[22]

4.5 Heterogeneity and Long-Term Effects

Heterogeneity in the impacts of first-time parenthood We analyze how the effects of new parenthood vary across different subgroups based on age and race—the two most readily observable demographic characteristics in our data. Understanding these variations can shed light on which subgroups are most affected, and in which domain, and thus help inform more tailored policy approaches to address their specific needs and challenges. We summarize results in Figure A.4. For government benefits like Medicaid and SNAP, older Black women exhibit the largest increases in enrollment, suggesting that this demographic might experience the largest change in access during this period. In contrast, reductions in criminal behavior appear similarly across all demographic groups, indicating that parenthood

[21] Similarly, for the case of SNAP, we find an equal-sized reduction in crime for SNAP-gainers and those who were enrolled in the benefit all along both during pregnancy and after childbirth, suggesting that newly-acquired access to food assistance is unlikely to contribute to the observed decrease in crime.

[22] Summary statistics for this sample and results in table-form are presented in Table A.6 and Table A.7, respectively. For power reasons, we do not restrict this analysis to women of low SES.

acts as a universal motivator for behavioral change (or universal incapacitator), regardless of age or race. The effects on OUD treatment are driven by older white women, aligning with the broader context of the opioid epidemic disproportionately affecting this group. Meanwhile, the effects on housing stability—both increased homeless shelter stays and transitions into public housing—are most significant for younger Black women, pointing to acute housing challenges for this subgroup.[23]

Long-Term Effects of First-Time Parenthood When examining the long-term effects of first-time parenthood up to five years after childbirth in Figure A.5, we find that the effects in all domains we measure—government benefits, behavioral outcomes, and housing—persist well beyond the immediate year after childbirth, suggesting that the challenges and opportunities of parenthood for individuals of low SES shape economic and social outcomes for years. Specifically, for Medicaid and SNAP enrollment, while the effects do decrease over time, they remain substantial even five years later, highlighting the enduring role of parenthood in connecting families to critical social support. In terms of criminal behavior, we find that the initial behavioral changes wane slowly and only lose statistical significance five years after childbirth. For public housing, effects peak 2-3 years after childbirth and stay high throughout, indicating that parenthood creates lasting moves into and needs for government-assisted housing. Results for homeless shelter stays become smaller and noisier in later periods. However, when considering longer-term homelessness assistance programs, we find large and persistent effects (see Figure A.6), suggesting that new parents in need transition from shelters into the latter type of housing assistance over the years.

The persistence of these effects across most outcomes reinforces the importance of timely interventions during pregnancy and early parenthood, as they have the potential to yield benefits that extend far into the future for both parents and children.

[23] We can also compare trajectories by whether the father is missing from the birth record—a proxy for single motherhood. With the caveat that this "moderator" obtains endogenously, at the moment of childbirth, we find a sizeable, 0.11pp increase in the homeless shelter encounter gap between those with no father listed and those with a father listed during pregnancy compared to pre-pregnancy—a 200% increase relative to the pre-pregnancy difference in average homeless shelter encounters across the two groups.

5. Robustness

Sample Selection and Model Specification Robustness Checks The event study results presented in the previous sections are robust to key specifications checks. These include a) changing our sample selection criterion in various ways (i. include all first-time mothers; ii. use alternative low SES criteria); b) robustness to "attrition" from in- and out-migration; c) excluding pre-conception months to rule out bias from "anticipatory effects"; d) including a linear pre-trend control; and e) using a standard two-way fixed effect estimator. We discuss these robustness checks in detail in Appendix C.1.

Matched DiD approach To account for age effects non-parametrically, we employ a matched DiD design similar to Fadlon and Nielsen (2021) and Mello (2023), who apply this method to estimate the effects of health shocks on labor supply and of traffic fines on financial wellbeing, respectively. This approach compares the evolution of outcomes for first-time mothers around childbirth (the "treatment group") with the simultaneous evolution for a matched control group of comparable individuals who have their first birth three years later. We match women based on age (that is quarter and year of birth), race, and Medicaid history. See Appendix C.2 for details. We report dynamic causal effect estimates in Figures A.7-A.9. We find matched pairs of 'treated' and 'control' women to be on parallel trends ahead of the treated peer's pregnancy and find sharp divergence in trends upon discovery of pregnancy, childbirth, or both. These patterns suggest that age effects do not bias our results in the main analysis. Effect sizes are summarized in table-form in Table A.18. In terms of both magnitudes and precision, the matched DiD results closely match those from our main event study.

Variation in pregnancy loss We present results from a robustness check that accounts for potential endogeneity in the timing of pregnancy by exploiting naturally occurring variation in pregnancy loss. Specifically, we conduct a difference-in-differences analysis that compares women who have a live birth to observably similar childless women who experience a miscarriage. See Appendix C.3 for details, including a discussion of the limitations of this analysis, especially that women experiencing a miscarriage are slightly disadvantageously selected. We report results from the DiD estimation based on 1,019 miscarriage events and 27,329 live birth events in Table A.19, and find them in line with results from our main analysis for most outcomes. Having a live birth, compared to a miscarriage, is associated with a statistically significantly larger increase in enrollment in Medicaid and SNAP, treatment for OUD, homeless shelter stays, and movement into public housing. Results for criminal behavior are noisier but show the same sign as in our main analysis.

6. Results for Men

In this section, we present our findings for first-time fathers, finding results that differ substantially from those for women across all outcomes.

The absence of fathers from birth records—38% of children born to Medicaid-insured mothers lack a father on the birth record—complicates this analysis due to likely selection bias. In Pennsylvania, unmarried parents must jointly sign a "Voluntary Acknowledgment of Paternity" form, often done immediately after birth, to establish legal rights and child support. Consequently, parents may not file this form—likely often in cases when the father is not present for the birth—for many reasons related to recent developments in the romantic relationship or economic situation of either parent. Whether parents are married (thus automatically listing the father on the birth record) is also likely influenced by similar factors. Consequently, men on better recent economic or psycho-social trajectories may be more likely to be listed. Due to this selection issue, the results in this section should be interpreted cautiously.

We present summary statistics for first-time fathers in Table A.20. A total of 5,046 first-time fathers satisfy our low SES criterion, making up 8.3% of all first-time fathers. They share similar characteristics to first-time mothers of low SES, except for a higher baseline rate of criminal charges (19.5% vs. 11%). We report results, estimated using our baseline imputation estimator, in Table A.21. Among men of low SES, new parenthood shows no statistically significant association with most outcomes and the sign of the association often contrasts with that found for women. There is no significant link between new parenthood and housing and OUD treatment outcomes after childbirth, a negative association with Medicaid enrollment (supporting the selection narrative), and a positive association with criminal behavior after birth. In the full sample of fathers, significance levels are similar (see Table A.22). However, coefficients for Medicaid and OUD treatment switch signs, thus directionally matching those found for women, consistent with higher rates of cohabitation and co-parenting (and thus co-movement of outcomes) among less economically disadvantaged new parents.

Acknowledging potential selection concerns, these results tentatively suggest that while it has been established that new parenthood leads to diverging trajectories of women and men in the labor market, on average, among individuals from economically disadvantaged backgrounds, having a child also has vastly different consequences for the overall living conditions of women relative to men, including in the domains of housing, social insurance use, and criminal behavior. These differences plausibly arise in contexts where many parents do not cohabit and parenting responsibilities are not equally shared.

7. Conclusion

In this paper, we traced out the impacts of pregnancy and parenthood on key markers of economic and psycho-social well-being of women of low socio-economic status in the United States. Our findings highlight that becoming a parent brings unique challenges and opportunities for individuals from this demographic group: on the one hand, we document significant strain in the domain of housing in the form of greater housing instability, as well as a large, persistent push into public housing. On the other hand, we find a tremendous increase in access to valuable government assistance programs for healthcare, food, and cash, as well as significant behavioral shifts towards commencing SUD treatment and reduced criminal offending, consistent with a motivational mechanism.

Our results should be interpreted with caution for several reasons. First, despite flat pre-trends and numerous robustness checks supporting our empirical approach, any remaining unobserved endogeneity in the timing of first-time parenthood could pose challenges to identification. However, for a range of policy questions, such as the allocation of homelessness services, observed changes are directly relevant, and isolating causal effects is less crucial. Second, our analysis is based on data from one large U.S. county. While representative of national demographics in many respects, effects may differ in other regions, and our findings are specific to the U.S. context. Lastly, as common when using administrative data to study drug use and homelessness, we can only cautiously interpret changes in the use of SUD treatment services and homeless shelter services in terms of an improvement vs. a worsening of the underlying state of substance use and housing stability.

With these caveats in mind, we believe the two most important implications of our results are the following: First, the time of new parenthood is a particularly important and suitable one for programs assisting vulnerable women in moving to stable housing in high-opportunity neighborhoods. Not only do we find that the period of new parenthood is one marked by increased mobility and reliance on housing assistance, suggesting a particularly high success rate in encouraging moves to high-opportunity neighborhoods; but we also find markers of increased housing instability during this period, suggesting that moving families to opportunity very early on could yield particularly large returns, including for children. Second, the profound changes we document in criminal behavior and substance use treatment suggest that social factors like new parenthood may serve as a pivotal moment for fostering positive change. Thus, other social factors that provide a strong sense of meaning and purpose and that could be influenced by government programs—by returning social capital, meaningful work opportunities, or both—may help improve individual welfare and spur positive externalities at the community level. Developing and evaluating such programs could provide an under-explored, potentially valuable complement to traditional government assistance programs.

Overall, we hope to complement important qualitative and mixed-method work such as Edin and Kefalas (2005) and DeLuca, Wood and Rosenblatt (2019) by providing more data-driven insights into the impacts of parenthood on economically disadvantaged parents, to inform policymakers in designing effective safety-net policies that help manage the disruptions of parenthood and leverage its opportunities for positive change. Given the ample evidence documenting the importance of a children's pre- and postnatal environment for their long-term health, well-being, and economic outcomes (Almond, Currie and Duque, 2018), such improvements could have very large positive externalities.

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Tables

Table 1: Sample Demographics

	(1) Low SES (main analysis sample) mean	(2) All others mean
Age	21.90	28.44
Age 16-17	0.10	0.01
Black	0.52	0.08
White	0.46	0.85
Father listed	0.57	0.91
Married	0.10	0.71
SNAP recipient	0.38	0.01
Homeless service encounter	0.02	0.00
Criminal offense charge	0.11	0.01
MHD treatment encounter	0.13	0.00
SUD treatment encounter	0.05	0.00
Observations	12928	66529

Notes: Table shows demographic characteristics of all women in Allegheny County who experienced a first live birth between 2007 and 2018 and were aged 16-40 at the time. Women proxied to be low SES (our main event study sample) are in column (1); all others are in column (2). Observations are at the individual level. Age and marital status are measured as of month of childbirth. "Father listed" indicates whether a father is listed on the birth record. "SNAP recipient" equals one if the individual received SNAP benefits in any month during the year before pregnancy. "Any homeless encounter" is one if the individual used at least one homelessness service in the year before pregnancy. "Criminal offense charge" is one if the individual was charged with a crime in an Allegheny court at least once in the year before pregnancy. "MHD treatment encounter" and "SUD treatment encounter" are dummies for having received treatment for any mental health disorder (excluding SUDs) or any SUD, respectively, at least once in the year before pregnancy, based on Medicaid records. See Section 2.2 for details on sample construction.

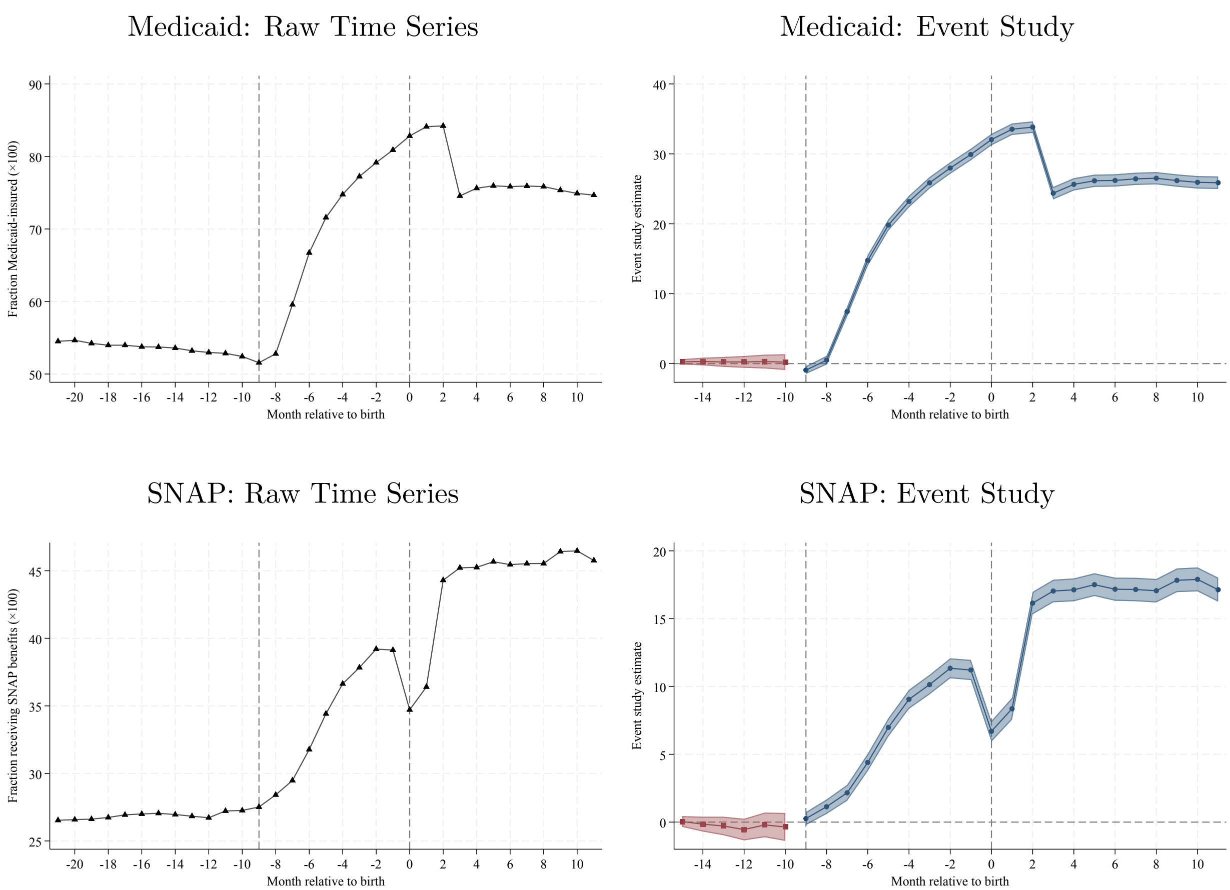
Table 2: Main Results on the Impact of First-time Parenthood

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	16.50***	6.30***	0.34***	-0.67***	0.08***	0.10
	(0.35)	(0.27)	(0.12)	(0.09)	(0.02)	(0.08)
Post-birth	27.72***	15.59***	0.68**	-0.83***	0.06	1.44***
	(0.77)	(0.61)	(0.29)	(0.19)	(0.04)	(0.21)
Mean	52.98	26.72	1.51	1.74	0.11	4.75
Observations	456756	457309	97823	380254	457309	457309
Individuals	12928	12928	2715	10593	12928	12928
Pre-trend p-value	0.89	0.37	0.90	0.16	0.61	0.43

Notes: This table shows estimates of the effect of pregnancy and the effect of parenthood in the first year after birth on our six primary outcomes, for our main analysis sample of first-time mothers of low SES detailed in Section 2.2. We estimate effects using the "imputation estimator" described in Section 3. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean" gives the mean of the dependent variable ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

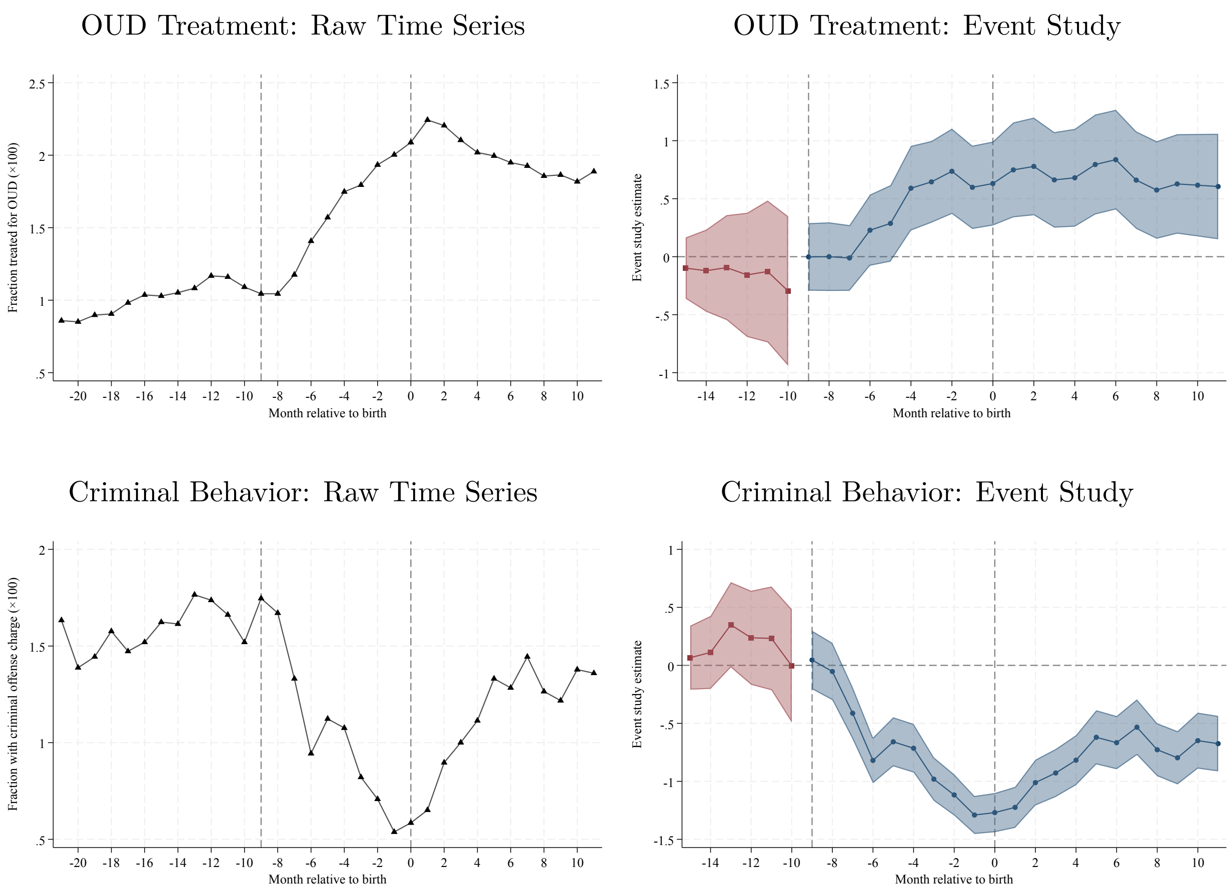
Figures

Figure 1: Government Benefit Use



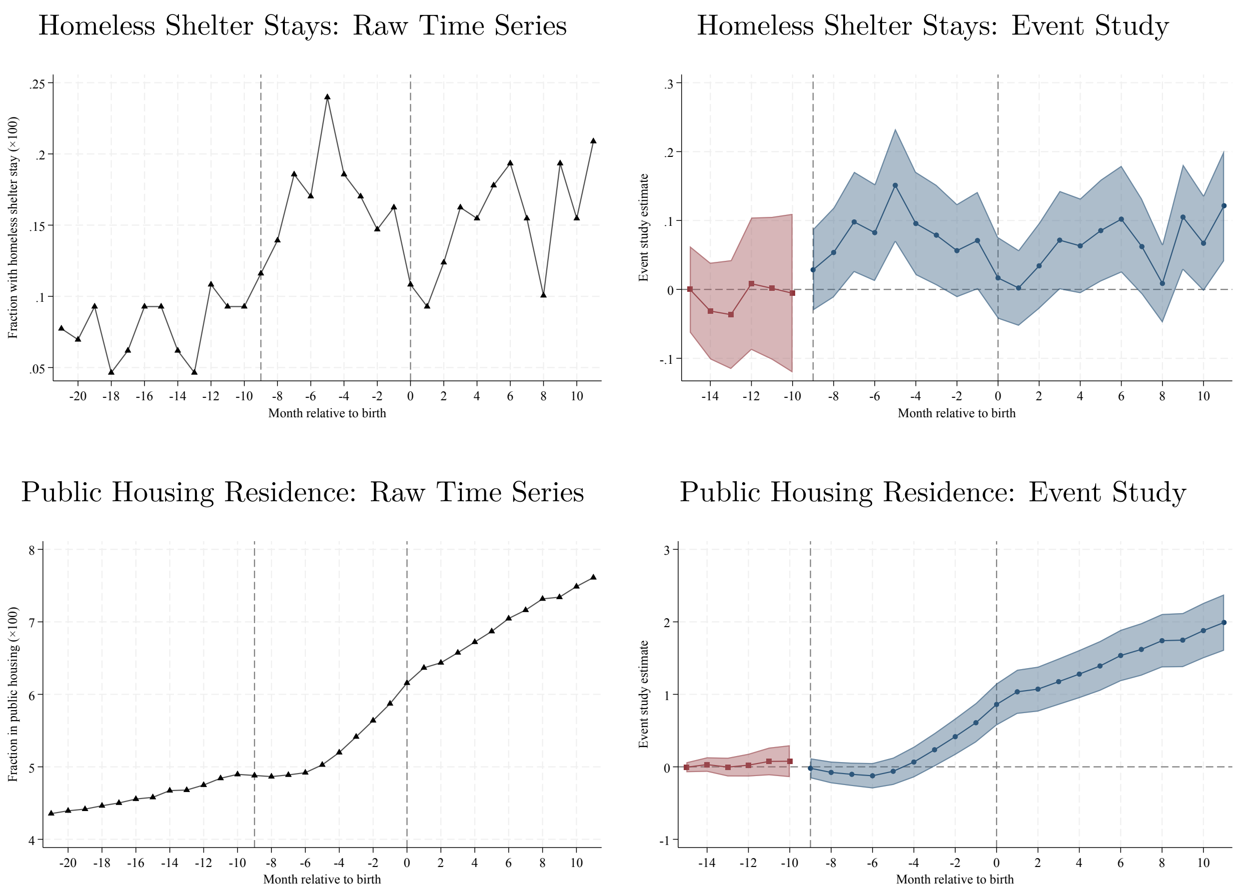
Notes: The figures on the left show raw means of primary government benefit outcomes (×100) by month relative to childbirth, for our main analysis sample of first-time mothers of low SES. The figures on the right show the corresponding event study estimates (×100), based on the "imputation estimator" described in Section 3. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. The left (right) vertical dotted line in each figure shows the approximate month of conception (childbirth).

Figure 2: Behavioral Outcomes



Notes: The figures on the left show raw means of primary behavioral outcomes (×100) by month relative to childbirth, for our main analysis sample of first-time mothers of low SES. The figures on the right show the corresponding event study estimates (×100), based on the "imputation estimator" described in Section 3. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. The left (right) vertical dotted line in each figure shows the approximate month of conception (childbirth).

Figure 3: Housing



Notes: The figures on the left show raw means of primary housing outcomes (×100) by month relative to childbirth, for our main analysis sample of first-time mothers of low SES. The figures on the right show the corresponding event study estimates (×100), based on the "imputation estimator" described in Section 3. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. The left (right) vertical dotted line in each figure shows the approximate month of conception (childbirth).

Appendix

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A. Appendix Tables and Figures

A.1 Appendix Tables

Table A.1: Allegheny County Characteristics

	Allegheny County mean	Rest of US mean
College plus	0.35	0.28
Foreign born	0.05	0.13
Median hshld income	60,055.76	61,287.21
Poor	0.13	0.14
White	0.81	0.64
Black	0.14	0.13
Hispanic	0.02	0.16
Asian	0.02	0.04
Single parent	0.33	0.32
Rent 2-bedroom	890.77	982.46
Population	1,223,348.00	1,094,111.02

Notes: Table shows mean demographic characteristics of Allegheny County residents (left column), as well as the average across all other US county-level means, weighted by county population (right column). "Poor" refers to share of individuals who fall below the federal poverty level. "Single parent" refers to the share of households with children that are headed by a female head (no husband present) or a male head (no wife present). Data comes from county-level estimates based on 2010 Census and ACS 5-year data (2006-2010, 2012-2016), provided by Opportunity Insights and collected in Chetty and Hendren (2018).

Table A.2: Overview of Data Elements

Type	**Population**	**Details**	**Years**
Birth records	All birth records filed in the county	Child ID, mother ID, father ID, birth weight, marital status of mom, number of previous live births of mom, calendar year-month of most recent non-live birth of mom.	1999-2019
Demographics	All*	Calendar year-month of birth, gender, race, state, and country of birth, flag for born in Allegheny County.	2005-2019
Medicaid, SNAP, TANF	All*	Month-level indicators of enrollment status for Medicaid, SNAP (household-level), TANF (household-level).	2002-2019
Housing Assistance	All*	Month-level indicators for residence in public housing and for Section 8 voucher receipt (household-level).	2005-2019
Homelessness Services	All*	Date and length of stay, type of encounter (shelter, rapid re-housing, transitional housing, permanent supportive housing).	2005-2019
Mental health and substance use treatment	Medicaid-insured or otherwise publicly funded	Date and type of each treatment received. Type includes psychotherapy, medication-based SUD treatment encounters (e.g. methadone receipt), inpatient stays in psychiatric hospitals and SUD treatment centers, and other services; includes diagnosis codes for each encounter.	2005-2019
Court records	All*	All criminal charges filed in Allegheny courts (Court of Common Pleas and Magisterial District Courts). Includes date, court type, and offense type (misdemeanor, felony, and within felony: assault, theft, drug possession, DUI). Outcome—such as "not guilty", "guilty", "guilty plea", "dismissal", "withdrawal"—listed for some cases.	2007-2019 (felonies), 2010-2019 (misdemeanors)
Physical health encounters	Medicaid-insured	Dates of all inpatient and outpatient encounters not covered by Medicaid Behavioral Health (i.e. excluding treatment of MHD and SUD), including diagnosis codes; does not include pharmaceutical claims.	2015-2019

Notes: Table provides an overview of all data elements used in this study. *All refers to all residents who have resided in Allegheny County at any point in the years of data coverage; we do not have information about when someone moved into or out of the county.

Table A.3: Eligibility Changes By Family Status

Program	**Eligibility Before first pregnancy**	**Eligibility During first pregnancy**	**Eligibility with one child in household**
Medicaid*	non-disabled adult age 21 or over: ineligible before 2015 and <$1,400 since 2015	<$3,100	non-disabled adult age 21 or over: <$580 before 2015 and <$2,000 since 2015
SNAP†	<$1,400, must participate in work program at least 20 hours per week to receive benefits for more than 3 months (waived 2009-2015)	<$1,400, no work requirement	<$2,250, no work requirement
TANF†	ineligible	<$205	<$316
Homeless Services§	12 shelters and 47 permanent/transitional housing programs for singles	Can access single shelters, plus 3 extra shelters for pregnant women	7 shelters and 55 permanent/transitional housing programs for families with children
Public Housing & Section 8‡	<$3,875, min. 18 year old household head	unchanged	<$4,429, min. 18 years old household head

Notes: All eligibility thresholds listed in US$ refer to gross monthly household income for a household with one adult (and one child, for the last column) unless otherwise noted, and correspond to 2020 eligibility thresholds for adult household members. The only program with a major change to eligibility thresholds over the sample period is Medicaid, which was expanded in 2015 to include households without children and to increase income thresholds for parents. "Unchanged" means no change relative to eligibility before first pregnancy. Under Medicaid Pennsylvania, for individuals aged 6-20 a household income threshold of 138% of FPL applies since 2014, corresponding to about $2,000 in a household of size two. Before 2014, the threshold was 100% of FPL (Kaiser Family Foundation, 2021*a*).

Sources: * Kaiser Family Foundation (2021*b*), Kaiser Family Foundation (2021*c*); † Pennsylvania Department of Human Services (2021); § Burger et al. (2015); ‡ Allegheny County Housing Authority (2020).

Table A.4: Pregnancy and Parenthood Effect Estimates for Secondary Outcomes I

	Benefit use	**SUD treatment**						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	TANF	Medication	Rehab	Psychotherapy	Any SUD	Cannabis	Alcohol	Cocaine
Pregnancy	4.20***	0.43***	-0.33***	0.33***	0.09	-0.17	-0.12**	-0.01
	(0.16)	(0.12)	(0.09)	(0.11)	(0.19)	(0.14)	(0.06)	(0.03)
Post-birth	15.06***	0.66**	-0.46*	0.64***	1.21**	0.40	-0.04	0.04
	(0.37)	(0.26)	(0.25)	(0.19)	(0.50)	(0.35)	(0.14)	(0.10)
Mean	5.38	1.14	0.33	0.37	2.58	0.66	0.18	0.11
Observations	457309	97823	97823	97823	97823	97823	97823	97823
Individuals	12928	2715	2715	2715	2715	2715	2715	2715
Pre-trend p-value	0.15	0.35	0.94	0.28	0.11	0.05	0.10	0.56

Notes: This table shows results from the same analysis as our baseline results reported in Table 2, but for our secondary outcomes in the domains of government benefits and SUD treatment.

Table A.5: Pregnancy and Parenthood Effect Estimates for Secondary Outcomes II

	Criminal behavior						Housing		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Misde-meanor	Felony	Assault	Theft	Drug offense	DUI	Long-term homeless	Sec. 8	Pub. Hous. (Head)
Pregnancy	-0.37***	-0.41***	-0.03	-0.15***	-0.09***	-0.06***	-0.01	-0.03	0.20***
	(0.10)	(0.07)	(0.03)	(0.04)	(0.03)	(0.02)	(0.04)	(0.09)	(0.05)
Post-birth	-0.26	-0.55***	0.04	-0.23***	-0.19***	-0.09**	0.14	0.43**	1.74***
	(0.19)	(0.15)	(0.07)	(0.08)	(0.07)	(0.04)	(0.10)	(0.21)	(0.16)
Mean	1.07	1.08	0.22	0.24	0.25	0.08	0.58	11.85	1.02
Observations	269110	380254	380254	380254	380254	380254	457309	457309	457309
Individuals	7225	10593	10593	10593	10593	10593	12928	12928	12928
Pre-trend p-value	0.29	0.21	0.24	0.42	0.34	0.84	0.38	0.26	0.47

Notes: This table shows results from the same analysis as our baseline results reported in Table 2, but for our secondary outcomes in the domains of criminal behavior and housing.

Table A.6: Summary Statistics: Two Live Births Sample

	mean
Age	26.55
Age 16-17	0.04
Black	0.16
White	0.80
Father listed	0.86
Low SES	0.18
Medicaid insured	0.13
SNAP recipient	0.08
Homeless service encounter	0.00
Criminal offense charge	0.02
MHD treatment encounter	0.02
SUD treatment encounter	0.01
Months between births	43.44
Observations	22683

Notes: Table shows summary statistics for all women with a first and second live birth in the sample period (2007-2018) that are at least 24 months apart. All time-varying variables are reported as of the month of first childbirth (or the year before first pregnancy, respectively).

Table A.7: First vs. Second Live Birth DiD Results

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy × 2nd child	-5.03***	-0.99***	-0.03	-0.07	0.01	-0.05
	(0.30)	(0.22)	(0.06)	(0.08)	(0.02)	(0.07)
Post-birth × 2nd child	-6.88***	-5.12***	0.14*	0.19**	-0.00	-0.25**
	(0.32)	(0.27)	(0.07)	(0.09)	(0.02)	(0.10)
2nd child	9.40***	4.92***	-0.08	-0.31***	-0.02	0.87***
	(0.33)	(0.27)	(0.08)	(0.07)	(0.02)	(0.13)
Pregnancy	11.03***	2.17***	0.14***	-0.13**	0.01	0.08
	(0.23)	(0.15)	(0.04)	(0.06)	(0.02)	(0.05)
Post-birth	12.10***	4.52***	0.15***	-0.24***	-0.00	0.43***
	(0.26)	(0.20)	(0.05)	(0.06)	(0.01)	(0.09)
Mean	13.81	8.75	0.29	0.33	0.02	1.47
Observations	1496789	1497078	1497078	1119558	1497078	1497078
Individuals	22683	22683	22683	16963	22683	22683

Notes: This table reports DiD estimates comparing the impact of first vs. second births among all women with a first and second live birth in the sample period that are at least 24 months apart. Based on the following event study specification: $y_{ijr} = \alpha + \sum_{r \neq -12}(\gamma_r \tau_r + \beta_r \tau_r T_{ij}) + \nu T_{ij} + \eta X_{ijr} + \epsilon_{ijt}$; where r is month relative to the month of childbirth, i is individual, and j denotes the series (either first or second birth). τ_r denotes relative event time dummies, T_{ij} is an indicator that equals one if the observation pertains to a second birth, and X_{ijr} is a set of controls (individual FE, age FE, and calendar year FE). Only observations in the event time window ($-21 \leq r \leq 11$) are included. Table shows coefficient estimates for $\beta_{-4}, \beta_3, \nu, \gamma_{-4}$, and γ_3 (in that order). "Mean" gives the mean of the dependent variable ($\times 100$) 12 months before childbirth. Cluster-robust standard errors clustered at the individual-by-birth level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Table A.8: Results for All First-Time Mothers

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	7.60***	1.66***	0.09***	-0.18***	0.02***	0.03**
	(0.08)	(0.05)	(0.01)	(0.02)	(0.00)	(0.01)
Post-birth	13.18***	4.73***	0.19***	-0.23***	0.01*	0.33***
	(0.16)	(0.12)	(0.03)	(0.04)	(0.01)	(0.04)
Mean	8.62	4.94	0.20	0.42	0.02	0.93
Observations	2810029	2813499	2813499	2308764	2813499	2813499
Individuals	79457	79457	79457	64162	79457	79457
Pre-trend p-value	0.90	0.55	0.69	0.18	0.74	0.35

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that the effects of pregnancy and parenthood are estimated on the full sample of first-time mothers (not just the sample of women of low SES).

Table A.9: Results using SNAP and Medicaid Low SES Criterion

	Benefit use		Behavioral outcomes		Housing	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	17.15***	6.08***	0.34***	-0.70***	0.08***	0.11
	(0.33)	(0.26)	(0.12)	(0.09)	(0.02)	(0.08)
Post-birth	28.74***	14.78***	0.68**	-0.86***	0.06	1.38***
	(0.73)	(0.61)	(0.29)	(0.18)	(0.04)	(0.20)
Mean	48.97	28.09	1.51	1.73	0.10	4.68
Observations	494491	495081	97823	413472	495081	495081
Individuals	13985	13985	2715	11512	13985	13985
Pre-trend p-value	0.89	0.44	0.90	0.23	0.78	0.38

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that the effects of pregnancy and parenthood are estimated for a slightly different sample. Instead of using the baseline low SES criterion of having been enrolled in Medicaid at some point in the 5 years preceding pregnancy, we use an alternative criterion of having been enrolled in either Medicaid or SNAP or both at some point in the five years leading up to pregnancy.

Table A.10: Results using SNAP Low SES Criterion

	Benefit use		Behavioral outcomes		Housing	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	16.61***	3.64***	0.23	-0.92***	0.11***	0.11
	(0.45)	(0.46)	(0.16)	(0.13)	(0.04)	(0.11)
Post-birth	27.64***	6.47***	0.66	-1.11***	0.07	1.37***
	(0.96)	(1.07)	(0.40)	(0.28)	(0.07)	(0.30)
Mean	54.59	53.54	1.80	2.18	0.15	6.39
Observations	263290	263672	64954	234962	263672	263672
Individuals	7337	7337	1780	6467	7337	7337
Pre-trend p-value	0.29	0.24	0.38	0.20	0.99	0.77

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that the effects of pregnancy and parenthood are estimated for a slightly different sample. Instead of using the baseline low SES criterion of having been enrolled in Medicaid at some point in the 5 years preceding pregnancy, we use an alternative criterion of having been enrolled in SNAP at some point in the five years leading up to pregnancy.

Table A.11: Results using Childhood Medicaid Low SES Criterion

	Benefit use		Behavioral outcomes		Housing	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	18.46***	6.68***	0.27**	-0.63***	0.08***	0.14*
	(0.34)	(0.27)	(0.11)	(0.09)	(0.02)	(0.08)
Post-birth	33.57***	17.06***	0.57**	-0.80***	0.08*	1.53***
	(0.71)	(0.61)	(0.24)	(0.18)	(0.04)	(0.21)
Mean	46.47	24.56	0.80	1.63	0.09	4.82
Observations	454813	455424	89662	391074	455424	455424
Individuals	12813	12813	2510	10863	12813	12813
Pre-trend p-value	0.70	0.69	0.62	0.26	0.68	0.64

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that the effects of pregnancy and parenthood are estimated for a slightly different sample. Instead of using the baseline low SES criterion of having been enrolled in Medicaid at some point in the 5 years preceding pregnancy, we use an alternative criterion of having been enrolled in Medicaid at any point before the 21st birthday (but before the first pregnancy).

Table A.12: Robustness to In-/Out-Migration I: Results for Sub-Sample with Local Service Records Before and After Event Time Window

	Benefit use		Behavioral outcomes		Housing	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	19.07***	7.23***	0.34***	-0.67***	0.11***	0.15
	(0.40)	(0.33)	(0.12)	(0.11)	(0.03)	(0.10)
Post-birth	37.44***	18.13***	0.68**	-0.72***	0.07	1.73***
	(0.87)	(0.76)	(0.29)	(0.24)	(0.05)	(0.26)
Mean	62.23	34.06	1.51	2.08	0.12	6.17
Observations	345073	345567	97823	287982	345567	345567
Individuals	9645	9645	2715	7900	9645	9645
Pre-trend p-value	0.75	0.52	0.90	0.06	0.70	0.34

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that the effects of pregnancy and parenthood are estimated for the sub-sample of low SES individuals who have an Allegheny DHS service encounter (that is, a Medicaid claim, court record, housing record, or welfare benefit record) in both the year before and the year after the event time window.

Table A.13: Robustness to In-/Out-Migration II: Results for Sub-Sample with Local Service Record in Childhood

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	14.77***	7.42***	0.12	-0.69***	0.10***	0.19*
	(0.46)	(0.42)	(0.09)	(0.12)	(0.03)	(0.11)
Post-birth	29.23***	18.24***	0.33	-0.81***	0.10*	1.92***
	(1.03)	(0.94)	(0.22)	(0.26)	(0.06)	(0.30)
Mean	61.93	31.39	0.42	1.94	0.09	6.51
Observations	249927	250281	69540	231339	250281	250281
Individuals	6979	6979	1919	6405	6979	6979
Pre-trend p-value	0.73	0.93	0.15	0.09	0.74	0.34

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that the effects of pregnancy and parenthood are estimated for the sub-sample of low SES individuals who have an Allegheny DHS service encounter (that is, a Medicaid claim, court record, housing record, or welfare benefit record) before age 17, and ahead of the event time window.

Table A.14: Robustness to In-/Out-Migration III: Results for Sub-Sample Born in Pennsylvania

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	16.58***	6.52***	0.34***	-0.68***	0.08***	0.17*
	(0.37)	(0.29)	(0.13)	(0.10)	(0.02)	(0.09)
Post-birth	28.60***	16.15***	0.71**	-0.80***	0.05	1.58***
	(0.82)	(0.67)	(0.29)	(0.21)	(0.04)	(0.23)
Mean	53.74	27.66	1.44	1.80	0.08	4.92
Observations	401235	401703	89894	332799	401703	401703
Individuals	11391	11391	2503	9303	11391	11391
Pre-trend p-value	0.77	0.30	0.91	0.32	0.89	0.45

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that the effects of pregnancy and parenthood are estimated for the sub-sample of low SES individuals who were born in Pennsylvania (information that is recorded on their child's birth record).

Table A.15: Robustness to Allowing for Anticipation Effects

	Benefit use		Behavioral outcomes		Housing	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	16.34***	6.12***	0.03	-0.77***	0.13***	0.18*
	(0.43)	(0.32)	(0.15)	(0.13)	(0.03)	(0.10)
Post-birth	27.51***	15.21***	0.11	-1.02***	0.14***	1.59***
	(0.90)	(0.74)	(0.31)	(0.29)	(0.06)	(0.23)
Mean	53.73	27.05	1.62	1.62	0.09	4.58
Observations	411339	411587	87872	341537	411587	411587
Individuals	12928	12928	2715	10593	12928	12928
Pre-trend p-value	0.80	0.67	0.87	0.37	0.52	0.62

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that we omit the three months immediately preceding conception from the estimation.

Table A.16: Robustness to Including Linear Pre-Trend Control

	Benefit use		Behavioral outcomes		Housing	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	16.52***	6.27***	0.36**	-0.73***	0.08***	0.09
	(0.48)	(0.38)	(0.17)	(0.12)	(0.03)	(0.10)
Post-birth	27.79***	15.54***	0.72*	-0.97***	0.07	1.42***
	(0.99)	(0.78)	(0.39)	(0.24)	(0.06)	(0.24)
Mean	52.98	26.72	1.51	1.74	0.11	4.75
Observations	456756	457309	97823	380254	457309	457309
Individuals	12928	12928	2715	10593	12928	12928
Pre-trend p-value	0.74	0.30	0.87	0.17	0.60	0.44

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference being that we include a (linear) control for relative event time.

Table A.17: Results with Standard Two-Way Fixed Effects Estimator

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy effect	13.09***	5.06***	0.29**	-0.59***	0.07***	0.08
	(0.32)	(0.26)	(0.12)	(0.07)	(0.02)	(0.09)
Post-birth effect	21.02***	12.60***	0.58***	-0.67***	0.03	1.39***
	(0.41)	(0.37)	(0.19)	(0.10)	(0.03)	(0.14)
Mean	52.98	26.72	1.51	1.74	0.11	4.75
Observations	426624	426624	89595	349569	426624	426624
N individuals	12928	12928	2715	10593	12928	12928

Notes: This table shows estimates of the effect of pregnancy and the effect of parenthood in the first year after birth on our six primary outcomes, for all first-time mothers of low SES in Allegheny County. The estimates obtain from the following standard two-way fixed effects model estimated via OLS: $Y_{it} = \beta_0 + \beta_1 \times Preg_{it} + \beta_2 \times Post_{it} + \mu_i + \gamma_{y(it)} + \epsilon_{it}$, where i denotes individual and t denotes calendar year-month. The regression includes controls for individual fixed effects (μ_i) and calendar year fixed effects ($\gamma_{y(it)}$). Columns 5 restricts to the sub-sample of first-time mothers who were Medicaid-insured throughout the event time window. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Table A.18: Matched DiD Results

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
T × Pregnancy	18.95***	7.49***	0.17	-0.57***	0.08***	0.35***
	(0.42)	(0.33)	(0.16)	(0.06)	(0.02)	(0.12)
T × Post-birth	34.27***	18.13***	0.44**	-0.62***	0.03	1.71***
	(0.54)	(0.45)	(0.21)	(0.06)	(0.02)	(0.20)
Mean T	53.40	26.97	1.54	1.67	0.09	4.79
Mean C	46.38	21.00	0.66	0.94	0.02	4.02
Observations	843769	843876	209880	998942	843876	843876
T individuals	12786	12786	3180	14547	12786	12786

Notes: Table reports causal effect estimates on interaction coefficients of treatment (i.e. individual in matched dyad who has the childbirth) and relative event time period dummies (treated peer's pregnancy time window and treated peer's year after childbirth, respectively) from a matched DiD regression detailed in Section 5. Regression includes controls for treatment, relative event period dummies, and their interaction. Sample is restricted to treated-control dyads in which the treated peer satisfies the low SES criterion (that is, is observed as Medicaid-insured in at least one month of the five years preceding pregnancy). "Mean T" and "Mean C" give the mean of the dependent variable ($\times 100$) of the treated and control peers, respectively, at 12 months before the treated peer's childbirth. Sample in column 3 is restricted to continuously Medicaid-insured individuals. Coefficient estimates and standard errors are multiplied by 100 for better readability. Cluster-robust standard errors clustered at the individual-by-treatment level are shown in parentheses. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Table A.19: Live Birth vs. Miscarriage DiD Results

	Benefit use		Behavioral outcomes		Housing	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy × Live birth	9.83***	3.11***	0.19	-0.44	0.07***	0.22
	(0.77)	(0.69)	(0.12)	(0.28)	(0.01)	(0.15)
Post-Pregn. × Live Birth	17.00***	9.68***	0.43***	-0.16	0.04	1.29***
	(1.05)	(0.77)	(0.15)	(0.20)	(0.02)	(0.24)
Pregnancy	1.93***	-0.44	0.05	0.10	-0.02	-0.16
	(0.75)	(0.68)	(0.11)	(0.27)	(0.01)	(0.14)
Post-Pregnancy	4.84***	-1.65**	0.03	-0.23	-0.00	-0.47**
	(1.02)	(0.75)	(0.15)	(0.20)	(0.02)	(0.22)
Mean live birth group	20.87	11.81	0.28	0.96	0.04	2.37
Mean miscarriage group	27.38	12.95	0.39	2.07	0.00	2.65
Observations	929177	929370	929370	718218	929370	929370
N indiv.-event tuples	28348	28348	28348	21922	28348	28348

Notes: Table shows effect estimates of having a live birth (vs. a miscarriage) obtained from OLS estimation of the difference-in-differences model detailed in Section 5. The regression includes controls for individual-by-event fixed effects and calendar year fixed effects. It is estimated off of the sample detailed in Appendix C.3. The "Mean" rows give the mean of the dependent variable (×100) two months before the approximate month of conception. Coefficient estimates and standard errors are multiplied by 100 for better readability. Cluster-robust standard errors clustered at the individual-event level are shown in parentheses. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Table A.20: Demographic Characteristics of First-Time Fathers

	(1)	(2)
	Low SES mean	All Others mean
Age	23.200	30.196
Age 16-17	0.050	0.003
Black	0.488	0.073
White	0.474	0.853
SNAP recipient	0.303	0.007
Homeless service encounter	0.011	0.000
Criminal offense charge	0.195	0.017
MHD treatment encounter	0.087	0.001
SUD treatment encounter	0.074	0.001
Observations	5046	55811

Notes: Table shows demographic characteristics of all men in Allegheny County at the time they first become parents, as identified via birth records. First-time parenthood is defined as the first birth record that lists the individual as the father, that is also the first birth to the child's mother, and that falls in the sample period (2007-2018). To keep in parallel with the study of women, the sample includes men aged 16-40 at the event only. Men identified as low SES are grouped into column (1). All other men are grouped into column (2). Observations are at the individual level. Outcomes are measured as of month of childbirth unless otherwise noted. Low SES is defined as being Medicaid-insured in at least one month within the five years preceding the mother's pregnancy leading up to the birth. Pregnancy onset is approximated as 10 months before the month of birth.

Table A.21: Results for First-Time Fathers of Low SES

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	-2.39***	0.56	-0.39**	0.20	-0.05*	-0.00
	(0.46)	(0.36)	(0.19)	(0.20)	(0.03)	(0.09)
Post-birth	-2.49**	0.52	-0.14	1.01**	-0.07	-0.02
	(1.10)	(0.86)	(0.45)	(0.42)	(0.06)	(0.23)
Mean	41.06	20.11	2.38	3.10	0.08	2.87
Observations	179312	179494	20546	149398	179494	179494
Individuals	5046	5046	547	4134	5046	5046
Pre-trend p-value	0.02	0.36	0.39	0.44	0.20	0.23

Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference is that the effects are estimated on a different sample. Namely, on all first-time fathers of low SES. "Pregnancy" refers to the period spanning nine months before the birth of the child.

Table A.22: Results for All First-Time Fathers

	Benefit use		**Behavioral outcomes**		**Housing**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Medicaid	SNAP	Opioid UD treatment	Criminal offense	Homeless shelter	Public Housing
Pregnancy	0.17***	0.14***	0.00	-0.04*	-0.00	-0.00
	(0.04)	(0.04)	(0.01)	(0.02)	(0.00)	(0.01)
Post-birth	1.21***	0.58***	0.06***	0.02	-0.00	0.01
	(0.10)	(0.09)	(0.02)	(0.05)	(0.01)	(0.02)
Mean	3.40	2.04	0.12	0.52	0.01	0.31
Observations	2158200	2160832	2160832	1775458	2160832	2160832
Individuals	60857	60857	60857	49179	60857	60857
Pre-trend p-value	0.02	0.38	0.80	0.14	0.27	0.61

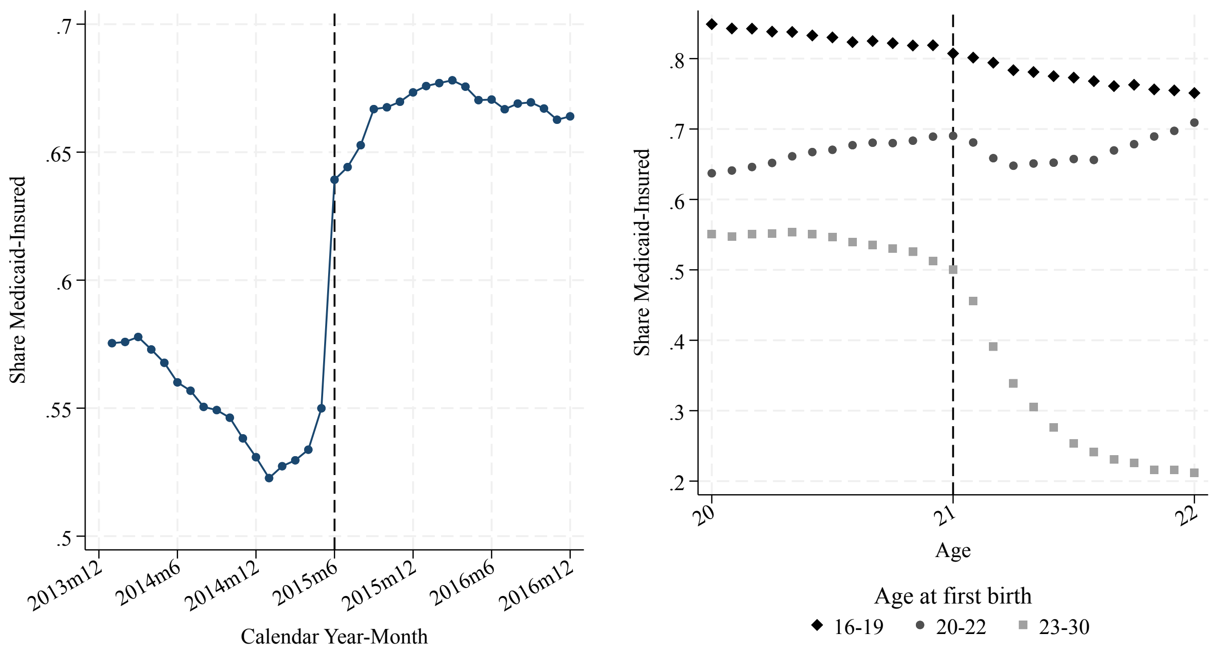
Notes: This table shows results from the same analysis as our baseline results reported in Table 2; the only difference is that the effects are estimated on a different sample. Namely, on all first-time fathers. "Pregnancy" refers to the period spanning nine months before the birth of the child.

A.2 Appendix Figures

Figure A.1: Benchmarking Effect Sizes for Medicaid Enrollment

(A) ACA Expansion

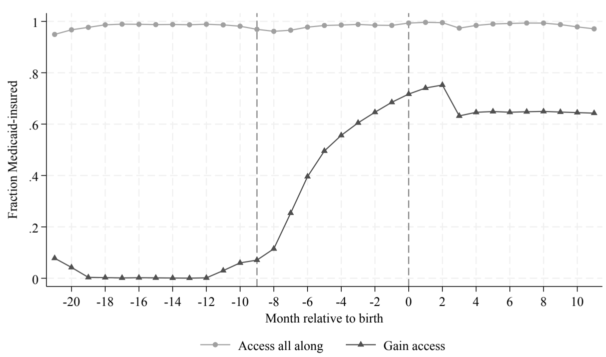
(B) Aging out of Child Medicaid



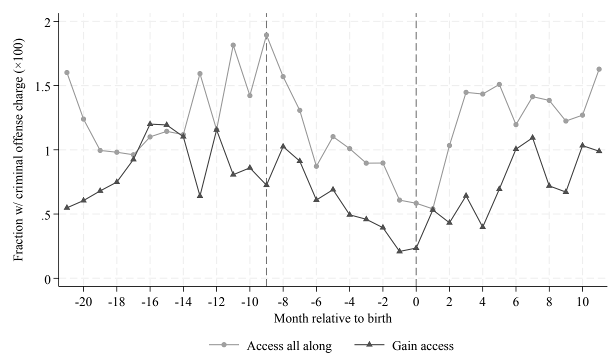
Notes: Panel A shows a time series of the share of women who are Medicaid insured around the time of the ACA-expansion (which took place in June 2015). Panel B shows a time series of the share of women who are Medicaid insured surrounding their 21st birthday (when stricter eligibility criteria go into effect for childless individuals). Both panels are for our main analysis sample detailed in Section 2.2; Panel B further restricts to those who had their first child before age 31 (such that we can observe their Medicaid enrollment going as far back as age 20). See Table A.3 for eligibility thresholds.

Figure A.2: Mechanism Analysis for Crime Results

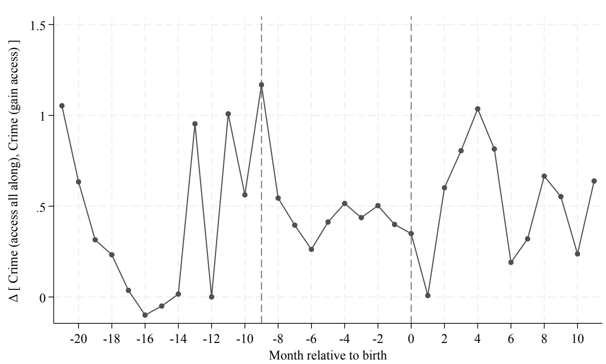
(A) Raw Means: Medicaid enrollment



(B) Raw Means: Criminal Behavior



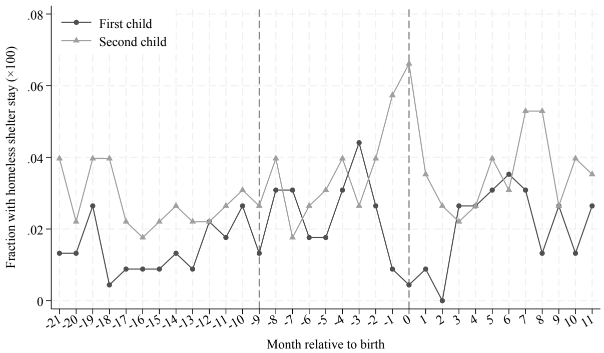
(C) Difference-In-Means: Criminal Behavior



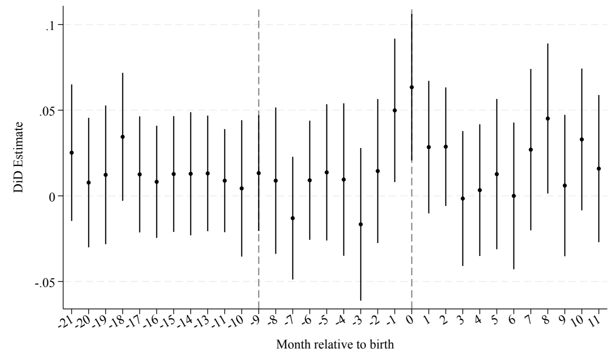
Notes: Figures A and B show means of Medicaid enrollment and criminal offending (×100), respectively, by month relative to first childbirth, separately for two sub-samples drawn from the full sample of first-time mothers: women who were continuously enrolled in Medicaid throughout the event time window ("Access all along", $N = 3,339$)—defined as being Medicaid enrolled in at least 29 out of the 33 months—and women who were not enrolled in Medicaid in the year preceding pregancy ("Gained access", $N = 3,314$)—defined as having been enrolled in at most 2 out of the 12 pre-pregnancy months. The two groups are matched based on year of childbirth, year of own birth, and race, as follows: means for each relative time period are computed for each demographic cell-by-access group separately and then averaged across demographic cells within an access group and relative time period by using weights equal to the total number of individuals in a demographic cell. Panel C shows the difference between the "access all along" average and the "gained access" average from panel B.

Figure A.3: Mechanism Analysis for Homelessness Results

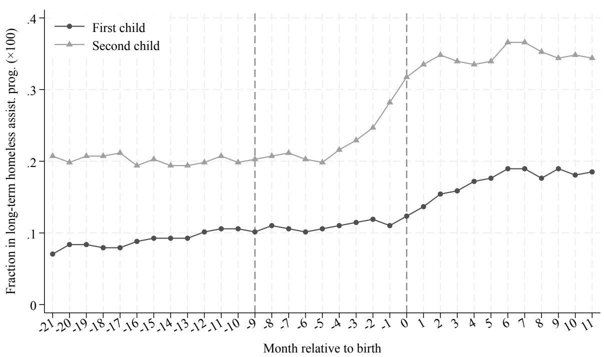
Shelter Stays: Raw Time Series



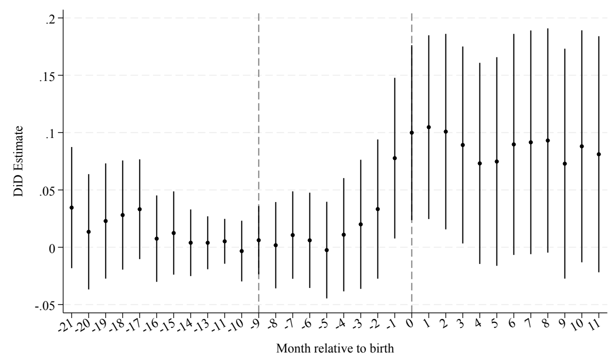
Shelter Stays: DiD Results



Long-term homeless assistance: Raw time series

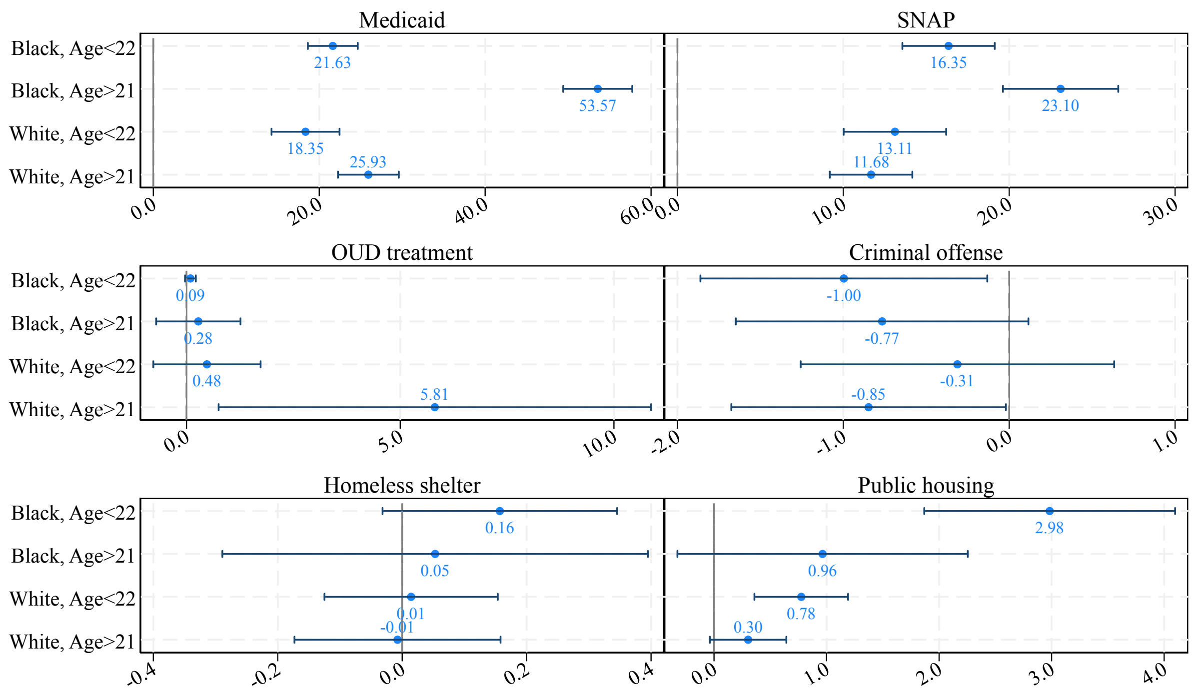


Long-term homeless assistance: DiD Results



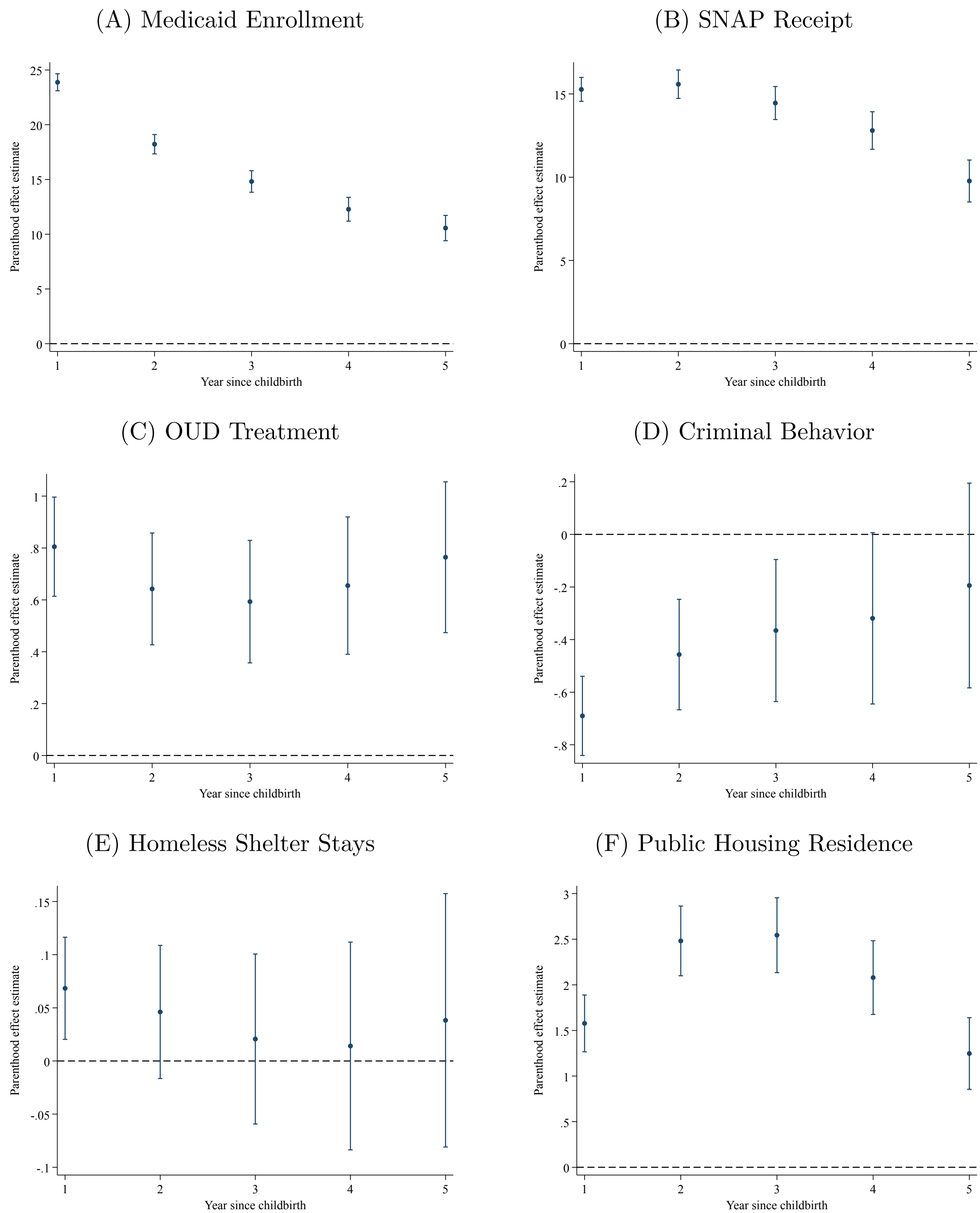
Notes: The figures on the left show raw means of our two homelessness-related outcomes (shelter stays at the top, and stays in long-term homelessness programs at the bottom) by month relative to childbirth, separately for first and second births. The figures on the right show the corresponding DiD estimates of the interaction coefficients from the following event study specification: $y_{ijr} = \alpha + \sum_{r \neq -12}(\gamma_r \tau_r + \beta_r \tau_r T_{ij}) + \nu T_{ij} + \eta X_{ijr} + \epsilon_{ijt}$; where r is month relative to the month of childbirth, i is individual, and j denotes the series (either first or second birth). τ_r denotes relative event time dummies, T_{ij} is a dummy for second birth, and X_{ijr} is a set of controls (individual FE, age FE, and calendar year FE). We plot the β_r's, which give the deviation from the baseline ($r = -12$) difference in outcomes across the events, at every month relative to childbirth. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. The sample is restricted to women with a first and second live birth in the sample period that are at least 24 months apart ($N = 22,890$ individuals). See Table A.7 for DiD estimation results in table-form.

Figure A.4: Heterogeneity by Race/Ethnicity and Age



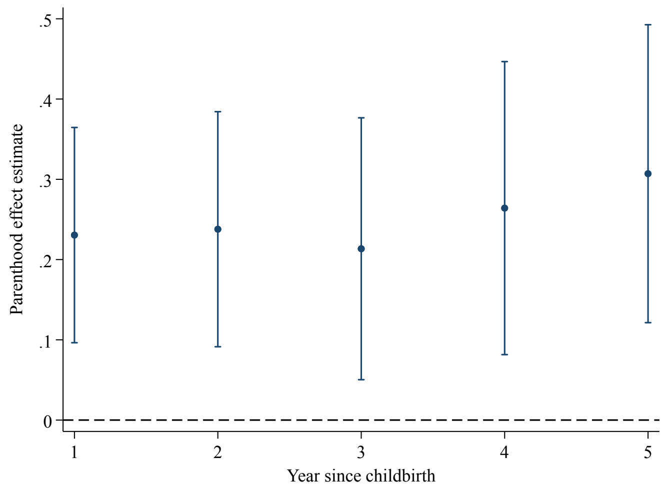
Notes: Each panel shows estimates of the causal impact of new parenthood on a given outcome (listed in the panel header) obtained from the "imputation estimator" described in Section 3, and estimated separately for four race-by age groups. Effect estimates are multiplied by 100 for better readability. Estimates are for the "Post-birth effect" (i.e. first year after childbirth). 95% confidence intervals based on cluster-robust standard errors clustered at the individual level are also shown.

Figure A.5: Longer Term Effects



Notes: Each figures shows the effect of first-time parenthood by year since childbirth, estimated via a two-way fixed effects regression of a given outcome on dummies for each year since childbirth (plotted above), a dummy for the pregnancy period, as well as individual- and calendar year fixed effects. The effect estimates are multiplied by 100 for better readability Based on our main analysis sample of first-time mothers of low SES; For each individual, all observations from twelve months before the onset of pregnancy to the end of the sample period are included.

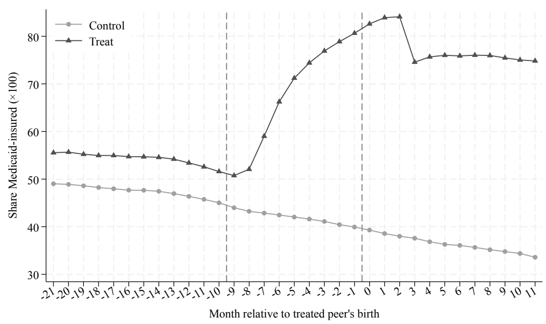
Figure A.6: Longer Term Effects on Residence in Long-Term Anti-Homelessness Programs



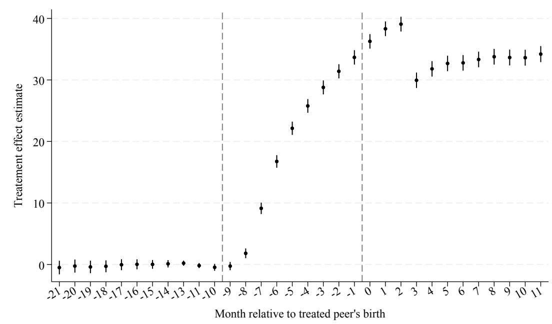
Notes: The figure shows the effect of first-time parenthood by year since childbirth, estimated via a two-way fixed effects regression of a given outcome on dummies for each year since childbirth (plotted above), a dummy for the pregnancy period, as well as individual- and calendar year fixed effects. The outcome is a dummy for residence in a long-term anti-homelessness program and the effect estimates are multiplied by 100 for better readability. Based on our main analysis sample of first-time mothers of low SES; For each individual, all observations from twelve months before the onset of pregnancy to the end of the sample period are included.

Figure A.7: Matched DiD Results for Benefit Outcomes

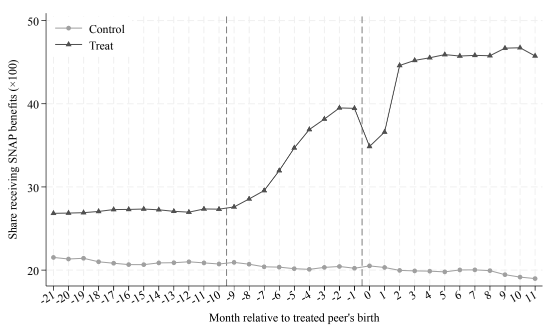
Medicaid: Raw Time Series



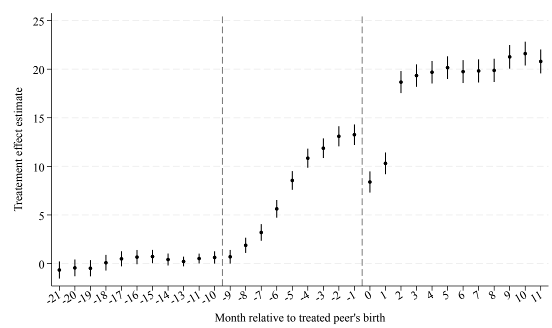
Medicaid: DiD Results



SNAP: Raw Time Series

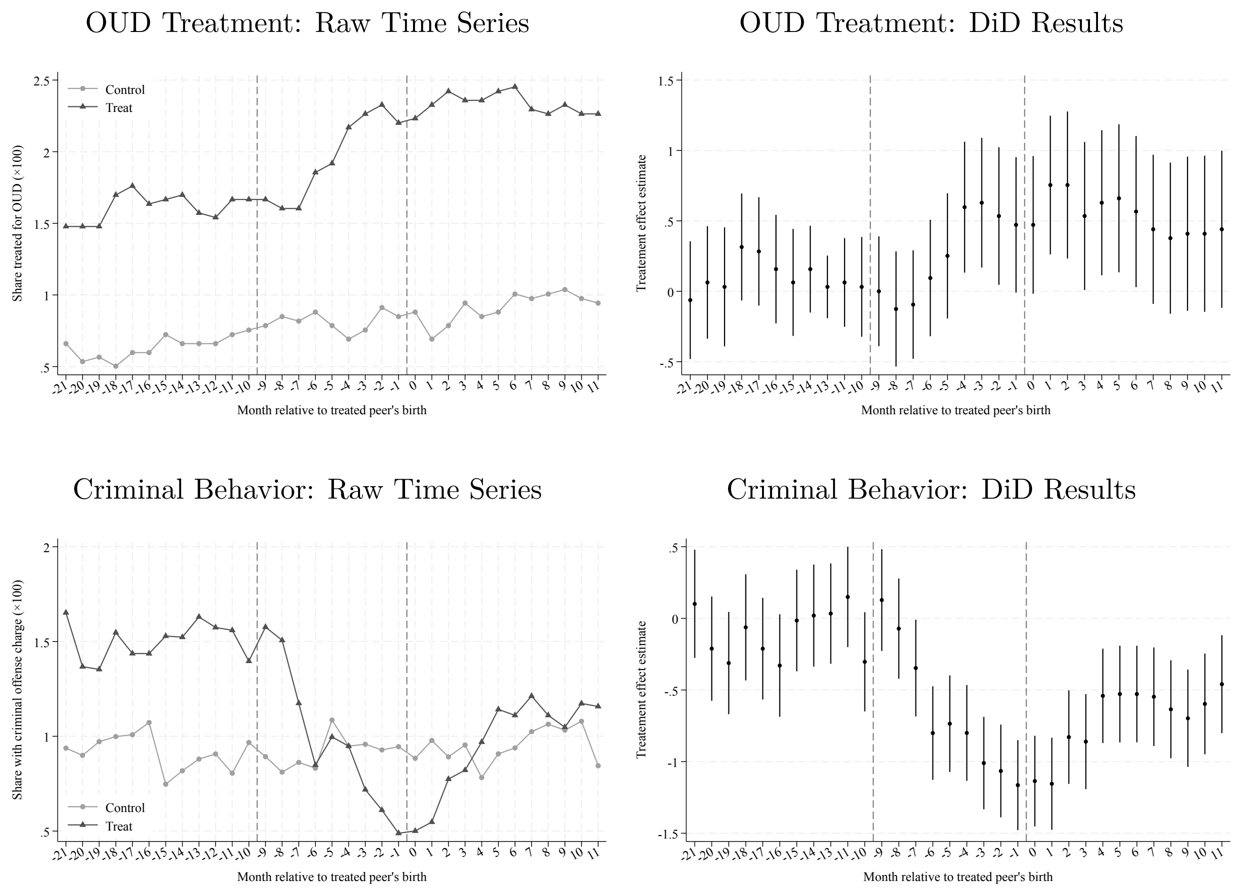


SNAP: DiD Results



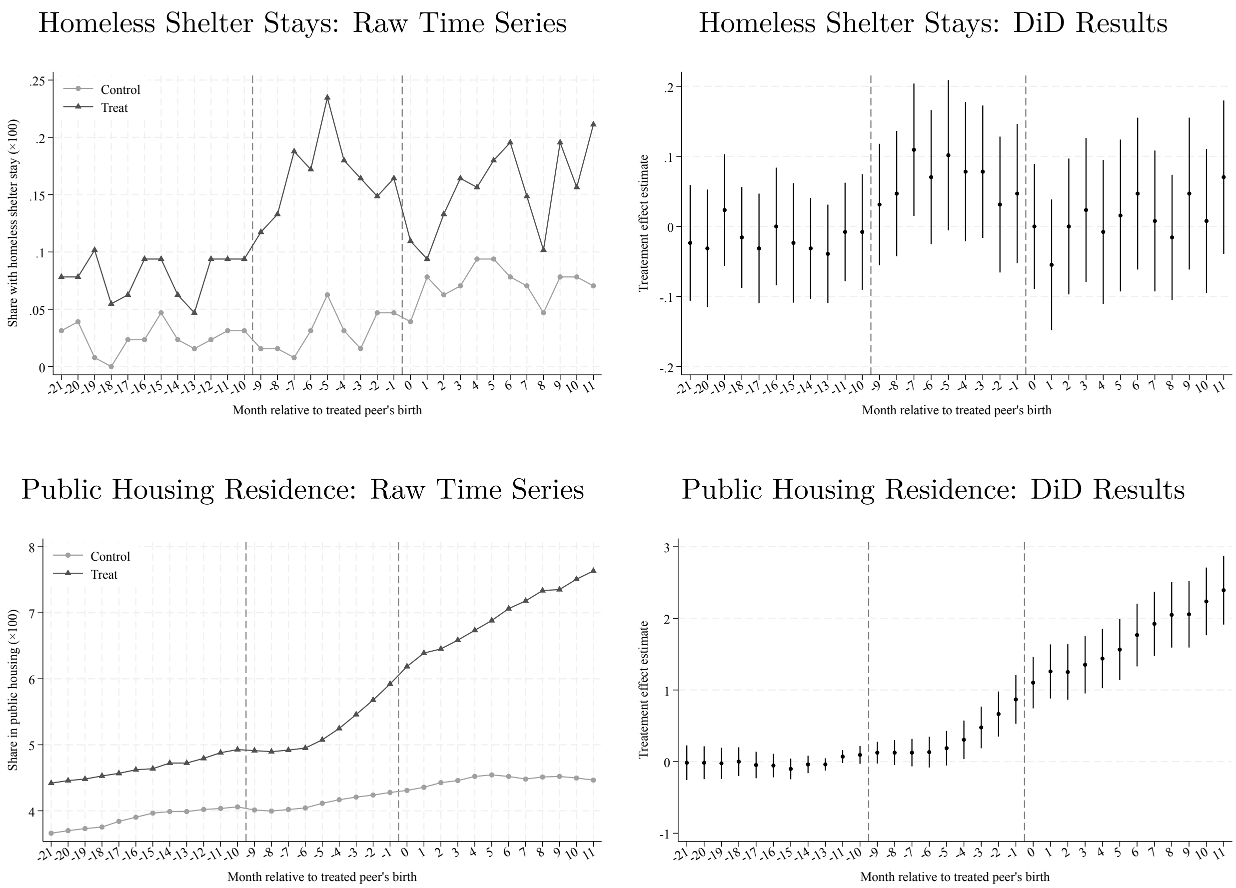
Notes: The figures on the left show raw means of primary government benefit outcomes ($\times 100$), separately for first-time mothers of low SES ("Treat") and for a one-to-one exactly matched control group who give birth two or more years later ("Control"), by month relative to the treated peer's month of childbirth. We match on age, race, and Medicaid history. The figures on the right show the corresponding causal estimates ($\times 100$) from an OLS regression of a given outcome on a treatment dummy, relative event time dummies, and their interaction. We plot the interaction coefficients. Month -12 relative to the treated peer's childbirth is the omitted category. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown. See Appendix C.2 for more details.

Figure A.8: Matched DiD Results for Behavioral Outcomes



Notes: The figures on the left show raw means of primary behavioral outcomes (×100), separately for first-time mothers of low SES ("Treat") and for a one-to-one exactly matched control group who give birth two or more years later ("Control"), by month relative to the treated peer's month of childbirth. We match on age, race, and Medicaid history. The figures on the right show the corresponding causal estimates (×100) from an OLS regression of a given outcome on a treatment dummy, relative event time dummies, and their interaction. We plot the interaction coefficients. Month −12 relative to the treated peer's childbirth is the omitted category. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown. See Appendix C.2 for more details.

Figure A.9: Matched DiD Results for Housing



Notes: The figures on the left show raw means of primary housing outcomes (×100), separately for first-time mothers of low SES ("Treat") and for a one-to-one exactly matched control group who give birth two or more years later ("Control"), by month relative to the treated peer's month of childbirth. We match on age, race, and Medicaid history. The figures on the right show the corresponding causal estimates (×100) from an OLS regression of a given outcome on a treatment dummy, relative event time dummies, and their interaction. We plot the interaction coefficients. Month −12 relative to the treated peer's childbirth is the omitted category. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown. See Appendix C.2 for more details.

B. Data and Outcome Construction

Birth Records: Identifying First Births

We use birth records to 1) identify and date the first life birth event for each woman, and 2) identify and date the most recent non-life birth event for women in our within-person dynamic difference-in-differences analysis.

Birth records cover all babies born alive in Allegheny County during 1999-2020. Each birth record has fields for mother, father, and child identifiers, month and year of birth, and information on how many previous life births the mother has had. For women with previous non-life birth events (such as abortions, miscarriages, and stillbirths) who had a subsequent life birth, the birth record of the life birth also lists the month and year of the most recent non-life birth.

To use as moderators and for summary statistics, we also extract information on whether a father is listed on the birth record, the marriage status of the mother at the time of birth, birth weight, and the principal payment method of the birth (Medicaid, private insurance, or other).

Welfare Benefit Programs

Welfare benefit records include indicators, for each year-month, for participation in each of the following state/federal programs for low-income individuals: Medicaid, Supplemental Nutrition Assistance Program (SNAP) colloquially referred to as food stamps, and Temporary Assistance for Needy Families (TANF) cash benefits. The data covers the years 2002-2019. Note that for the case of SNAP and TANF, our outcome indicators equal one for all household members within a household that receives these benefits.

Substance Use Disorders

We use Allegheny County Behavioral Health (i.e. mental health) claims records to measure mental health outcomes related to substance use disorder. The data pertains to all mental health treatment services paid for through public funds (including Medicaid, Medicare, and some care to uninsured individuals that is publicly funded), and covers the years 2005-2019. The vast majority of care (90%) we observe in this dataset for individuals in our sample is funded through Medicaid.

We use mental health records to construct month-level indicators for substance use disorder treatment encounters. Treatment encounters span psychotherapy, medication-based treatment, inpatient stays in psychiatric hospitals and addiction treatment centers, and other services (such as the use of county-based crisis hotlines, and peer support programs). Each treatment encounter recorded in the dataset features a diagnosis code, which we use to identify substance use-related encounters. Our main SUD treatment outcome studied in this paper is an indicator for receiving treatment for opioid use disorder (OUD)—the most common SUD observed in the data.

As secondary outcomes, we consider receiving treatment for *any* SUD, as well as for the next most commonly treated SUDs (cannabis, alcohol, and cocaine use disorder).[24] To gauge what *types* of treatment for SUD pregnancy and parenthood trigger, we also distinguish between the three main types of treatment for opioid use disorder: opioid use disorder medication treatment encounters (such as methadone treatment encounters), inpatient opioid use disorder treatment (i.e. rehab), psychotherapy for opioid use disorder, as well as unspecified outpatient encounters (which are typically either psychotherapy- or medication-related).

Criminal Behavior

We use court records to assess changes in criminal behavior. The records include data for all criminal charges filed in Allegheny courts—that is, in the Court of Common Pleas and Magisterial District Courts; the former handles felony cases only, while the latter handles both misdemeanor and felony cases. For each case, we observe its date, whether it is a felony or misdemeanor charge, and, among felony charges, the type of charge. We group felony charges into five broad categories: assault, theft, drug possession, DUI, and all others (such as terroristic threats, criminal trespassing, and prostitution). The verdict of the case is listed only in a small subset of cases, and hence we do not use this information. Expunged records are not included in this dataset. The data covers the years 2007-2019 for the Court of Common Pleas, and 2010-2019 for Magisterial District Courts. We combine data from both courts—that is, for a given individual and month, the criminal offense outcome dummy equals one in case a criminal charge was filed in at least one of the two types of courts. When we analyze the secondary outcome "Misdemeanor offense" (which is measured based on Magisterial District Court records only), we only consider the period 2010-2019, while analysis of all other primary and secondary outcomes in the domain of criminal behavior is based on the period 2007-2019.

Housing

To study housing instability, we use homelessness service records, Section 8 data, and public housing residence information; all data sources span the years 2005-2019. For every individual-month pair, we use indicators for whether an individual received a given type of housing assistance that month. Our main outcomes comprise homeless shelter stays, and residence in public housing. As secondary outcomes, we consider receipt of medium- to long-term homelessness assistance, and residence in a household that receives a Section 8 voucher.

Homelessness service records include the date of entry and exit, as well as the type of every individual encounter with the homelessness system in the county. To distinguish an acute housing crisis in its most severe form from more general housing instability, we distinguish between two outcomes: Homeless shelter stays, and participation in a medium- to long-term anti-homelessness program. The latter includes rapid rehousing, permanent supportive housing, and transitional housing. For both types of outcomes, we construct an indicator outcome from the entry- and exit dates such that it equals one if an individual is using a given homelessness service that month.

[24] The relevant ICD-9 and ICD-10 diagnosis codes are: opioid use disorder- 304.0x, 304.7, F11.x; alcohol use disorder- 303.x, F10.x; cocaine use disorder- 304.2x, F14.x; cannabis use disorder- F12.x, 304.3, 305.2; any substance use disorder- 303.x, 304.x, 305.x., F1x.x.

For the public housing and Section 8 outcomes, we construct indicators that equal one if an individual is registered as living in a public housing residence or in a household that receives Section 8 rental assistance in a given month, respectively. To further proxy for whether individuals relying on public housing or Section 8 vouchers live outside of their parental home vs. with their parents, we use secondary outcomes indicating whether the person is listed as the head of household for a given housing benefit. This information is available for approximately 73% of public housing and Section 8 participants, and we code missing as zeros.

C. Robustness Analyses

C.1 Sample Selection and Model Specification Checks

To probe the robustness of our results to sample selection criteria, we first omit our low SES criterion altogether and report results for all first-time mothers in the county in Table A.8. With this much larger sample of ca. 80,000 women, who are much less economically vulnerable on average (see Table 1), we find sign and statistical significance across virtually all our outcomes unchanged. While impacts are quite similar in relative terms across the two samples, the absolute magnitude of parenthood's impact on homelessness, public housing, and criminal behavior is, expectedly, much smaller in the full sample, highlighting the vastly different challenges and changes to environments that women of lower and higher incomes face as a result of parenthood. For example, pregnancy increases the propensity to stay at a homeless shelter by 0.02pp in the full sample compared to 0.08pp in the low SES sample. Similarly, expanding low SES to include those who received either Medicaid or SNAP benefits at any point in the five years leading up to conception (instead of using the Medicaid criterion only) does not alter results (Table A.9); neither does using a criterion of low SES that disregards Medicaid and only considers SNAP enrollment (Table A.10), or one that only considers Medicaid-enrollment before age 21—i.e. child Medicaid (Table A.11).

We report results from the remaining robustness checks in Table A.12-Table A.17, and find statistical significance levels and magnitudes largely unchanged. Table A.12-Table A.14 address potential concerns about in- and out-migration biasing results, by zooming in on sub-samples of i) individuals with Allegheny DHS service encounters in the year before *and* after the event time window, ii) individuals with Allegheny DHS service encounters during childhood, and iii) individuals born in Pennsylvania. Table A.15 employs our standard imputation estimator but omits the three months immediately preceding conception to rule out that any anticipatory effects enter the estimation of individual- and time fixed effects. Table A.16 also employs our standard imputation estimator but includes a linear pre-trend control. Table A.17 shows results from a standard two-way fixed effects estimator.[25]

[25] We estimate the following model based on the same data as our baseline estimation: $Y_{it} = \beta_0 + \beta_1 \times Preg_{it} + \beta_2 \times Post_{it} + \mu_i + \gamma_{y(it)} + \epsilon_{it}$, where i denotes individual, t denotes calendar year-month. The regression includes controls for individual fixed effects (μ_i) and calendar year fixed effects ($\gamma_{y(it)}$). $Preg$ and $Post$ are dummies for pregnancy and the first year after childbirth, respectively.

C.2 Matched Difference-In-Differences Analysis

To account for age effects, we perform a matched difference-in-differences analysis that follows Fadlon and Nielsen (2021) and Mello (2023), who apply this method to estimate the effects of health shocks on labor supply and of traffic fines on financial well-being, respectively. This approach matches each individual in the data to a comparable "control" peer who experiences the same event in the future (or never at all).

We use two criteria for matching: demographic characteristics (own birth cohort, race, and Medicaid history) and childbirth timing. Matching occurs in two steps: first, define the set of potential control matches based on exact matching on demographics. Second, among the potential matches, select the best possible match concerning childbirth timing.

Matching on demographics For each woman in our main analysis sample ("treated peers"), we define the *set of "potential control matches"* as the set of women who are of the same age cohort (as measured by quarter-year of own birth), race, and Medicaid history (ahead of the treated peer's first pregnancy). We match on Medicaid history to compare women of similar SES. Medicaid history is a categorical variable that can take four values, given by the quartile of the number of months a person is observed Medicaid insured in the years before a treated peer's first pregnancy.

Matching on childbirth timing Choosing a control peer whose own first childbirth date is as close as possible to that of a treated peer has the advantage of a higher similarity between treated and control individuals. At the same time, it limits the length of the event time window we can consider, since matching treated and control peers whose childbirth dates are close in time implies that the conception date of a control peer occurs shortly after the childbirth date of the treated peer. Because a minimum distance of three years in events within a matched pair allows us to consider the same event time window as in our main analysis (that is, spanning from twelve months before conception to twelve months post-childbirth) without introducing any contamination and while maintaining comparability as much as possible, we employ this threshold.

Accordingly, for each treated peer, we select as her control the peer from her *"potential control matches"* who has her first child as close as possible in time to the treated woman's first child but a minimum of 36 months after (selecting randomly in case there is more than one match); in case no such "control" exists, we match the treated woman to a woman who satisfies the demographic matching criteria, but who is childless (and not pregnant) as of the end of the sample period.

Sample and Summary Statistics Women can enter the sample only once as a treated peer, but several times as a control peer (the average control peer acts as a control for 2.2 treated peers). If an individual acts as a control for more than one peer, we keep one outcome series for each match. We find a control match for each treated woman; however, for 11 % of treated women, the only control match available is that of a woman for whom we observe no childbirth in the sample period (i.e. an always-childless control peer). Summary statistics are reported in Table C.23 below.

Table C.23: Summary Statistics for Matched DiD Sample

	(1) "Control" mean	(2) "Treated" mean
Never has child	0.11	0.00
Months to first childbirth	38.44	0.00
No. of T peers matched to	2.21	0.55
Age	21.87	21.87
Black	0.53	0.53
Medicaid-enrolled	0.58	0.69
SNAP receipt	0.29	0.38
Homeless encounter	0.01	0.02
Criminal offence charge	0.07	0.11
OUD treatment encounter	0.01	0.02
Observations	12786	12786

Notes: Table shows summary statistics for the sample of women entering the matched difference-in-differences analysis detailed in Appendix C.2. Observations are at the individual-event level (note that an individual can enter both in the treated group and the control group and can enter in the control group more than once). To construct the matching, we first identify all low SES women with a first childbirth in the sample period ("treated"). Low SES is defined, as before, as being Medicaid-insured at any point in the five years preceding pregnancy. Next, we match each treated woman to a woman of the same age (as measured by quarter-year of own birth), race, and Medicaid history (ahead of the treated peer's first pregnancy) who has her first child as close as possible in time to the treated woman's first child but a minimum of 36 months after (selecting randomly in case there is more than one match); in case no such "control" exists, we match the treated woman to a woman of the same age, race, and Medicaid history, but for whom we observe no childbirth at all (marked as "Never has child" in the summary statistics table above). The summary statistics table shows mean characteristics of the treated and control peers. For time-varying characteristics, we report them as of month of the treated peer's first childbirth (or pregnancy) event, as noted.

Estimating Equation The complete panel and one-to-one match design simplifies the difference-in-differences analysis considerably. In particular, it makes including individual fixed effects, date fixed effects, or age fixed effects obsolete. The simple estimating equation is given by:

$$y_{ijr} = \alpha + \sum_{r \neq -12} (\gamma_r \tau_r + \beta_r \tau_r T_{ij}) + \nu T_{ij} + \epsilon_{ijt}, \quad (3)$$

where r is month relative to the (placebo) month of childbirth, i is individual, and j denotes the series (treated or control), since individuals can enter with more than one series. τ_r denotes relative event time dummies, and T_{ij} is an indicator that equals one if the observation pertains to a treated peer. The objects of interest are the β_r's. They provide an estimate of the deviation from the baseline difference in outcomes between treated and control peers, at every month relative to the treated peer's month of first childbirth. Results from this specification are presented in the event study Figures A.7-A.9.

To summarize effect sizes into more aggregate periods, in table form, we replace the month-level dummies τ_r from the estimating equation above with two aggregate period dummies: one for the pregnancy period, and one for the year post-childbirth period. These results are presented in Table A.18.

C.3 Difference-in-Differences Miscarriage vs. Life Birth Analysis

To further account for the potentially endogenous timing in the onset of pregnancy, we present results from a robustness check that explores naturally occurring variation in pregnancy loss. Specifically, we conduct a difference-in-differences analysis that compares women who have a live birth to observably similar childless women who experience a miscarriage. This strategy was first employed in the teen birth literature (Hotz, Mullin and Sanders, 1997).

Sample Construction We identify miscarriage events via Medicaid claims and birth records. We find that Medicaid claims records likely provide a comprehensive sample of all miscarriage events that require medical attention and occur to Medicaid-insured women.[26,27]

Because we only have Medicaid claims records for the period 2015-2019, which is too short a period to provide enough sample, we supplement the sample of miscarriage events with non-live birth events identified via birth records spanning the whole sample period 2005-2019. We can only identify non-live births from birth records of subsequent live births. Each live birth record includes a field that lists the date of the most recent non-live birth event experienced by the mother listed on the birth record; this is the field we use to identify and date non-live births via birth records. Including such events increases the sample size but introduces two important limitations: first, birth records do not distinguish between causes for the non-live birth: a non-live birth could be a miscarriage (or stillbirth), or an abortion.[28] While abortions are likely heavily under-reported on birth records due to stigma and lack of documentation in patients' medical histories, we may still erroneously code some abortions as miscarriages.[29] Henceforth, we call all non-live birth events miscarriages, for simplicity. Second, by using subsequent live birth records to identify miscarriages, we are missing miscarriages experienced by women who do not have a subsequent live birth.

Among all miscarriage events, we keep those that are not preceded by a live birth. Because our low SES criterion is too strict to deliver a large enough sample of miscarriage events (a total of 500), we relax it by including all live birth and miscarriage events occurring to young women (as a proxy for low SES). That is, we only include women who have their first live birth or miscarriage event at age 25 or younger. Focusing on younger women also

[26]Medicaid physical health claims include records for every inpatient and outpatient encounter (such as Emergency Department visits, hospital stays, primary care encounters), including detailed diagnosis codes. We identify miscarriages through ICD-9 and ICD-10 diagnosis codes. The codes are "634.xx" for ICD-9 and "O03.xx" for ICD-10.

[27]Using the Medicaid and birth records, we find a ratio of miscarriages to live births of approximately 1:10.05; that is, miscarriages make up 9.95% of all (recorded) birth events. This statistic is slightly lower than the worldwide average of 15.3% of all recognized pregnancies, which includes miscarriage events that do not require medical attention (Quenby et al., 2021).

[28]Among non-live birth events not occurring by induced abortion, an event happening at < 20 weeks gestation is defined as a miscarriage; otherwise, it is considered a stillbirth.

[29]Unfortunately, no study exists that measures the extent to which induced abortions are under-recorded on birth certificates.

makes us more likely to zoom in on unplanned pregnancies. As in our main analysis, we exclude women younger than 16 at the event and we restrict to events for which we observe complete panel data covering one year before conception to one year after birth. For women in the miscarriage group, we only keep the first observed miscarriage in case we observe more than one. Note that a woman can enter this sample more than once: she can enter with a miscarriage event, and also with a subsequent live birth. The resulting sample includes 1,019 women who have a miscarriage and 27,329 women who have a live birth.

Summary Statistics Summary statistics for this sample are presented in Table C.24 below. Overall, the approximately 28,300 women in this sample have similar demographic characteristics (in terms of age and race) to those in our main event study sample of low SES first-time mothers, though only about 39% are identified as low SES based on our Medicaid criterion. Furthermore, within this sample, women who experience a miscarriage look very similar in terms of observable characteristics to women who experience a live birth: they have the same average age of 21, and a very similar racial/ethnic composition (33% are Black, in both samples). The sample of women who experience a miscarriage skews slightly more vulnerable on socioeconomic characteristics, as evidenced by slightly higher rates of pre-pregnancy SNAP use (19.5% vs. 16.6%), and slightly higher rates of homelessness (1.0% vs. 0.7%). Of note is that within this sample, among the women who have a miscarriage event, 27.6% also enter the sample with a subsequent live birth event.

Estimating Equation For simplicity and because our event study imputation estimator cannot readily be applied in a setting that dynamically differences out trends observed among a control group that itself gets "treated" by an event, we employ a simple difference-in-differences estimator following Massenkoff and Rose (2024). It is given by the following model:

$$Y_{ijt} = \alpha + \nu_{ij} + \gamma_{year(ijt)} + \beta_1 Pregnancy_{ijt} \times LB_{ij} + \beta_2 Post_{ijt} \times LB_{ij} + \gamma X_{ijt} + \epsilon_{ijt}, \quad (4)$$

where i indexes person, j indexes event (since a person can enter with both a miscarriage and a live birth event), and t indexes calendar year-month. Furthermore, ν_{ij} and $\gamma_{year(ijt)}$ denote individual-by-event and calendar year fixed effects, respectively; LB is a dummy that equals one for observations belonging to a live birth series; $Post_{ijt}$ is a dummy that equals one for months 0-11 since the birth event. $Pregnancy_{ijt}$ is a dummy that equals one for months 0-2 (0-8) since the approximate date of conception for miscarriage (live birth) events. The approximate month of conception is defined as four (ten) months before the birth event for miscarriages (live births). Finally, X_{ijt} contains the one-way interaction terms- that is a dummy for $Pregnancy$ and a dummy for $Post$.

Identification Assuming that conditional on pregnancy, having a miscarriage is not correlated with our outcomes of interest, this strategy helps control for unobservable, time-varying factors that are correlated with the timing of conception and influence our outcomes. Given the high-frequency event study setting with detailed data pre-pregnancy, level differences in the outcome variables during the pre-period among women who experience a miscarriage

Table C.24: Summary Statistics for Life Birth vs. Miscarriage DiD Sample

	Live birth mean	Miscarriage mean
Age	21.391	20.876
Age 16-17	0.074	0.126
Black	0.335	0.334
White	0.631	0.629
Low SES	0.387	0.399
Medicaid insured	0.272	0.337
SNAP recipient	0.166	0.195
Homeless service encounter	0.007	0.010
Criminal offense charge	0.059	0.104
MHD treatment encounter	0.052	0.080
SUD treatment encounter	0.018	0.022
(Also) has miscarriage	0.010	1.000
(Also) has live birth	1.000	0.276
Months between events	39.423	39.423
Observations	27329	1019

Notes: Table shows summary statistics for women in the sample for the difference-in-differences analysis comparing miscarriage events to live birth events as detailed in Appendix C.3. Observations are at the individual-event level (note that an individual can enter both the live birth group and the miscarriage group). The left column pertains to women with a first live birth in the sample period 2007-2018. The right column pertains to women with a miscarriage event within the same time frame (measured via Medicaid claims diagnosis codes and birth records) who have not had a previous live birth at the time of the event. The sample is restricted to likely unplanned pregnancies, by restricting to age at event of 25 or younger, and to live births to women with no miscarriage event in the preceding 24 months, and miscarriage events to women with no live birth event in the following 24 months. Outcomes are measured as of month of the event unless otherwise noted. Low SES is dummy that equals 1 if person is observed as Medicaid-insured at any point in the five years preceding the pregnancy leading up to the event. Pregnancy onset is approximated as nine months before the month of birth (for live birth events), and four months before the event (for miscarriage/non-live-birth events). "Months between events" is the number of months between the miscarriage event and the live birth event for the subset of women who enter the sample with two time series—one for each event.

compared to those who have a live birth are not a threat to identification. Those differences are simply differenced out.

Three key empirical concerns related to sample selection, endogeneity in the timing of miscarriages, and the shock of miscarriage itself persist that suggest the results from this analysis should be interpreted cautiously. The first relates to sample selection bias: miscarriage commonly happens early on in the pregnancy, before the decision about whether to have an abortion is made. Therefore, the sample of women who experience a miscarriage may include individuals who would have had an abortion had they not miscarried; while any such unobservable differences that are fixed over time get differenced out, differences in pre-existing trends across the two groups do not. The second one relates to an endogeneity concern: Miscarriage may be triggered by unobservable, negative life events, such as physical stress or psychological stress due to job loss, that also influence the outcomes of interest. The third relates to interpretation. Experiencing a miscarriage may itself be a traumatizing event with detrimental impacts on mental health (Rellstab, Bakx and Garcia-Gomez, 2022), and may thus not provide a suitable counter-factual when the counter-factual of interest is one of not having had a pregnancy at all. The last two points imply *negative* selection into the miscarriage sample relative to the live birth sample. Thus, for any negative change to living conditions, we find in the live birth group relative to the miscarriage group, it may be an underestimate in absolute terms; any positive change to living conditions we find in the live birth group relative to the miscarriage group is likely to be an overestimate of the impact of a live birth relative to the counterfactual of having no birth event at all.

Results We present results from the DiD estimation in Table A.19, and find them in line with results from our main analysis. The coefficients of interest are those on the two interaction terms *Pregnancy*× *Live birth* and *Post Pregnancy*× *Live birth*; they provide an estimate of the change in outcomes due to new parenthood after differencing out the change in outcomes observed among individuals who experience a miscarriage.

In terms of direction and statistical significance, the results obtained in our main event study analysis for homeless shelter stays, public housing residence, social assistance use, and OUD treatment also obtain in this robustness check. That is when controlling for the potentially endogenous timing of pregnancy via the inclusion of the miscarriage control group, we still find sizeable and statistically significant increases across all these outcomes. For example, we find that relative to women who experience a miscarriage, women with a live birth experience a 1.29pp larger increase in movement into public housing in the year after the birth event—compared to an effect size estimate of 1.44pp in our main event study analysis.

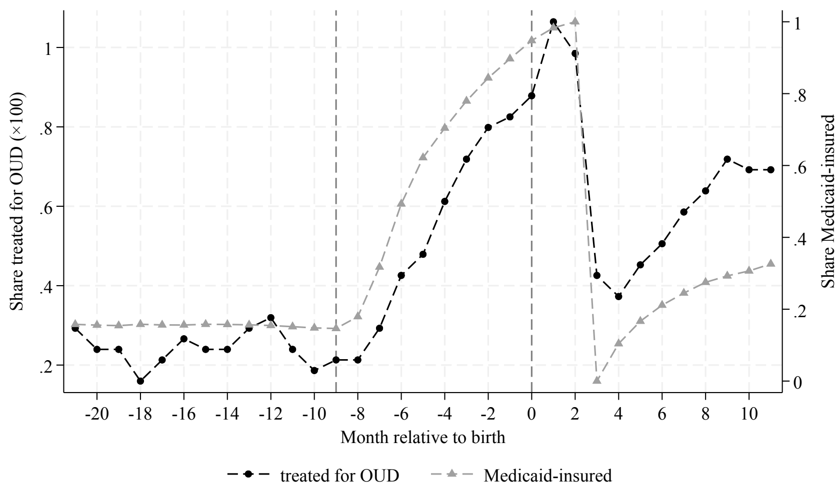
In contrast, the magnitude of the coefficients for the social assistance program use outcomes becomes smaller, consistent with the fact that the eligibility status of the miscarriage sample also changes with pregnancy. Furthermore, while results for criminal behavior retain the same sign as in our main event study analysis (in the sense that relative to the miscarriage control group, the live birth group experiences larger decreases in criminal behavior), the differences in effects of pregnancy and post-childbirth for the miscarriage and the live birth group are not statistically significant.

D. Additional Analyses

D.1 Health insurance churn and SUD treatment

Combining our findings in the domains of SUD treatment and Medicaid insurance enrollment, we can investigate the consequences of pregnancy-related health insurance churn. Figure 1 reveals that a substantial share of women—9%—abruptly loses Medicaid coverage at two months postpartum when stricter eligibility criteria come into effect. This period *precisely* coincides with the time in which women's propensities to enter SUD treatment are highest (see Figure 2). Accordingly, when we zoom in on the ca. 3,800 first-time mothers in our data who *lose* Medicaid at 60 days postpartum, we find an abrupt, 0.6 pp (or 60%) drop in publicly funded treatment for SUDs in the subsequent month (see Figure D.10 below). Even if many of the women who lose Medicaid might manage to become privately insured, they would likely have to change service providers and there might be a coverage gap. Experiencing disruptions or, worse, a complete loss of access to these services in a time of documented need could have adverse consequences for affected women (and their children). The fact that drug-related deaths are a major contributor to post-partum maternal mortality—they are found to be the second leading cause of mortality in the year after childbirth (Goldman-Mellor and Margerison, 2019)—underscores the importance of this issue. Therefore, expanding the post-birth Medicaid eligibility period, or providing alternative subsidies in the months after the end of Medicaid eligibility could help avoid disruptions in or loss of SUD treatment services during a very sensitive time period for parents and children. The findings thus lend support to a key reform of Medicaid enacted in March of 2021: the Postpartum Coverage Extension, a provision in the American Rescue Plan Act, which gives all states the new option to extend the postpartum coverage period under Medicaid from 60 days following pregnancy to a full year (Kaiser Family Foundation, 2021*d*).

Figure D.10: OUD Treatment and Loss of Medicaid at 60 Days Postpartum



Notes: This figure shows raw means of outcomes by month relative to childbirth for the sub-sample of women who lose Medicaid coverage at three months postpartum, when stricter income eligibility rules come into effect. The sample size is 3,757 individuals, 36.7% of whom are in our low SES sample. The dark dots show the share of women receiving OUD treatment (multiplied by 100 for better readability), while gray triangles give the share of women who are Medicaid-insured.