Parenthood in Poverty

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Abstract

Parenthood has profound effects on the lives of new parents in terms of both labor-market and non-labor-market outcomes. For low-SES individuals, who might lack resources to weather the shock of parenthood, non-labor market outcomes (e.g., housing stability) are likely to be as primary a concern as labor market outcomes. In this paper, we provide the most comprehensive and detailed evidence to date of the effects of pregnancy and parenthood on the non-labor-market outcomes of low-SES individuals in the United States. Our data consists of longitudinal, high frequency administrative records from a large urban U.S. county. Our outcomes span housing, treatment for substance use disorder (SUD), take-up of social assistance programs, and crime. We find that new parenthood leads to: i) short-term and long-term changes in the housing environment, including increases in short-term homeless-shelter stays, transition into longer-term homelessness programs, and transition into public housing; ii) an increase in treatment for SUDs; iii) large eligibility-driven increases in use of key government assistance programs for healthcare, food assistance, and cash assistance; iv) large reductions in criminal behavior driven at least in part by individuals gaining healthcare coverage. We also show that the effects of parenthood are heterogeneous by race and vulnerability to mental health disorders. A battery of robustness checks, including two separate (matched) difference-in-difference analyses, suggest our results are robust to potential endogeneity concerns about the timing of pregnancy.

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My daughter learned to walk in a homeless shelter. [...] Sometimes I cleaned the floors and toilets of homes whose owners I knew, friends who had heard I was desperate for money. They weren't rich, but these friends had financial cushions beneath them, something I didn't. A lost paycheck would be a hardship, not a start of events that would end with living in a homeless shelter. They had parents or other family members who could swoop in with money and save them from all of that. No one was swooping in for us. It was just Mia and me.

—Stephanie Land, *Maid*.

Parenthood has profound effects on the lives of new parents: it affects their ability to work, alters their housing needs, influences their mental and physical health, probes the stability of their relationships, and more. For individuals of low socio-economic status (SES), who may lack the resources to fully insure themselves against disruptions caused by having a child, the effects of parenthood on non-labor-market outcomes such as housing may be at least as relevant as the effects on labor-market outcomes. Despite their importance, such effects have to date been under-explored in the economics literature.

In this paper, we present the most comprehensive and detailed evidence to date on the effects of parenthood on the non-labor-market outcomes of low SES individuals in the United States. Coupling high-frequency administrative data from a large urban U.S. county with an event-study research design, we document the effects of parenthood in the domains of housing, treatment for drug addiction, take-up of social assistance programs, and crime. Our main analysis focuses primarily on mothers, because the subset (62%) of fathers who are listed on children's birth certificates is likely to be highly selected.[1]

Our analysis relies on high-frequency, detailed administrative records from a large urban US county (Allegheny County in Pennsylvania) spanning the years 2005 to 2019. Besides containing birth records for the universe of births in Allegheny County during our sample period, the data includes Medicaid mental and physical health-claims records, homelessness service records, public housing and Section 8 records, welfare benefit records (Medicaid,

[1] In this analysis, we use "women" and "mothers" to describe people who are pregnant or gave birth. We use these terms to align with the language in the birth record data, which we use to identify new parents. However, we acknowledge that not all people who become pregnant or give birth identify as women.

SNAP, TANF), and court records (misdemeanor and criminal offense charges) for all residents of Allegheny County. Our main sample consists of the universe of women—approximately 12,500—who have a first birth in the sample period and are of low SES as measured by their pre-pregnancy welfare-program enrollment. We also show results on the full sample of women and, keeping in mind the important selection caveat mentioned above, on the sample of low-SES men.

Our empirical strategy consists of an event-study design around pregnancy and childbirth. The identifying assumption is that any endogenous confounds evolve smoothly around the exact timing of conception/childbirth. We acknowledge the assumption is strong and, in some cases, possibly violated. We employ a four-pronged approach in support of our analysis. First, for most of our outcomes, we find compelling visual evidence of sharp, discontinuous changes at the discovery of pregnancy, at childbirth, or both. Second, in order to control for potential pre-trends at the individual level, we control for individual-specific linear pre-trends. Third, in robustness checks, we employ two separate matched difference-in-difference strategies that further account for endogeneity in the timing of pregnancy. The first matched difference-in-difference analysis compares the outcomes of women around the birth of their first child with the outcomes of a matched control group with similar demographics (including same age) who have their first child two years later; the second matched difference-in-difference analysis compares the outcomes of women who have a live birth to those of women who have a miscarriage. Fourth, we note that, for a variety of policy questions—especially those related to "tagging" (Akerlof, 1978) (e.g., using pregnancy/new parenthood as a predictor for outcomes when deciding how to allocate services)—observed changes to outcomes are of direct interest and precisely isolating causal effects is less relevant.

In order 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 et al. (2021). By estimating individual and time fixed effects based on pre-treated observations only, the Borusyak et al. (2021) estimator avoids direct comparisons between newly-treated units and already-treated units. As a result, it delivers consistent estimates even in the presence of heterogeneous treatment effects across time and treated units. As shown in the robustness section, the results are unchanged when we use the traditional two-way fixed effects estimator.

By means of our event-study analysis, we establish four main results. First, we find that, for low-SES women, pregnancy and childbirth lead to substantial changes to the housing environment. Specifically, we find that, during pregnancy, homeless shelter stays – a rare and extreme outcome in our data – increase by 100% or 0.1 percentage points (pp). Similarly, movement into longer-term housing programs for individuals experiencing homelessness increases by 67% or 0.4pp in the year after birth. The increased take-up of homelessness services is likely to be driven by real changes in housing needs rather than to eligibility changes resulting from pregnancy and parenthood; specifically, we observe even stronger effects around the birth of a second child, for which there are no eligibility changes. Finally, we find a gradual and persistent increase in public housing occupancy as a result of new parenthood: one year after childbirth, parenthood almost doubles the share of women who live in public housing, compared to the no-child counterfactual.

The effects of pregnancy and childbirth on new-mothers' housing environments are widely heterogeneous depending on the mother's race and vulnerability to mental health disorders. Specifically, increases in short-term homeless shelter encounters are primarily driven by Black women, while increases in long-term anti-homelessness housing assistance are driven by individuals with preexisting SUDs independent of race. These results are consistent with Black women having less access to informal housing insurance (for example through family) and with individuals with SUDs disproportionately requiring more intense assistance that long-term anti-homelessness programs are designed to provide.

Our second main result is that pregnancy and childbirth lead to increases in treatment for SUD, driven primarily by treatment for opioid-use disorder. Specifically, we find that treatment for any SUD (opioid use disorder) increases by 20% (29%) during pregnancy, and by 110% (129%) in the year after childbirth, on a base of 3% (1.7%) pre-pregnancy. The increase in treatment for opioid-use disorder is almost entirely driven by white women above the age of 22, the demographic group with the highest levels of pre-pregnancy opioid abuse in the sample. A priori, increases in observed SUD treatment could be due to at least three factors: changes in insurance status, increases in actual substance use, and increases in treatment for existing substance use disorder. We rule out the possibility that the detected effects are due to changes in insurance status by limiting our analysis to women who are continuously Medicaid-insured. In terms of the other two possible mechanisms, the timing and sharpness of the increase in treatment for opioid-use disorder early on in pregnancy is more consistent with pregnancy triggering treatment for a pre-existing disorder, rather than with pregnancy leading women to increase their consumption of illicit substances. This finding is in line with qualitative evidence documenting that current pregnancy is reported to be the top treatment motivator among pregnant women in SUD treatment (Jackson and Shannon, 2013). Overall, the results on substance use suggest that new parenthood can be a major push factor out of untreated drug addictions.

Our third main result is that parenthood leads to tremendous increases in the use of key government assistance programs (healthcare coverage, food assistance, and cash assistance). In terms of healthcare, we find that pregnancy and childbirth lead to a 26pp increase in Medicaid coverage. As a point of comparison, for the women in our sample, the impact of the Affordable-Care Act (ACA) expansion is one-third of the magnitude. The increases in SNAP (i.e., food stamps) and TANF (i.e., cash assistance) enrollment due to new parenthood are also large (17pp and 14pp respectively). The increase in government-program use is immediate—50% of women enroll within the first trimester of pregnancy—and lasting, suggesting that these programs are of great value to economically vulnerable women around the time of family formation. Furthermore, the immediacy of uptake in early pregnancy—a period marked by large changes to income eligibility thresholds because of pregnancy status—supports the notion that the increase in program use is in large part eligibility-driven as opposed to driven mainly by reductions in income due to reduced capacity to work.

Our fourth main result is that pregnancy and childbirth leads to large decreases in criminal behavior. Specifically, charges for criminal offenses decrease by 29%, on a base of 1.7%, with the largest decrease observed for drug charges. We find that the decrease in criminal behavior is larger for women gaining access to Medicaid than for those who had access all along, thus suggesting that at least part of the reduction in criminal behavior is due to better access to healthcare and, specifically, treatment for substance use.

Taken together, our findings have important implications for policy design. First, they suggest that optimizing the *timing* of housing mobility programs that help low-income families move to stable housing in high-opportunity neighborhoods could be extremely valuable. Our results show that the period of pregnancy and early-parenthood is marked by increased mobility and increased reliance on housing assistance. Therefore, targeting such housing mobility programs to individuals around the time of family formation might lead to high take-up rates and moves across neighborhoods. Furthermore, given our evidence of increased housing instability during this time period, such a program would likely yield particularly large benefits to both parents and children, as suggested by the expansive literature documenting the importance of in-utero and early-childhood environments for child development (Almond et al., 2018; Rossin-Slater and Persson, 2018), as well as the literature showing that the earlier children move to opportunity, the better their outcomes (Chetty and Hendren, 2018). Second, the significant heterogeneity in effects we find across individuals and outcomes highlights the value of customized assistance relative to one-size-fit-all solutions.[2] In particular, women with substance use disorders (11% in our sample) have distinct trajectories with regards to homelessness and criminal behaviors and are likely to require more intense social assistance. Third, the period of pregnancy and early-parenthood seems to be an effective time to connect women to social assistance programs and addiction treatment services, suggesting that outreach and cross-program referral efforts might be particularly impactful during this period. Fourth, our findings underscore the importance and potential of access to healthcare (and in particular treatment of substance use disorders) for reducing criminal behavior.

[2]This finding is in line with Bergman et al. (2020), who document the value of tailoring assistance to the specific needs of low-income housing-voucher recipients. It is also in line with Chetty et al. (2020), who document large heterogeneity in rates of social mobility, teenage pregnancy, and incarceration for individuals from different demographic groups who grow up in the same census tract and have similar parental socio-economic backgrounds.

This paper contributes to the economic 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. As discussed, most of the existing literature focuses on labor-market outcomes such as earnings and employment (including Angrist and Evans, 1998; Lundborg et al., 2017; Zohar and Brooks, 2021; Gallen et al., 2021), with a special focus on differences across gender (e.g. Blau and Kahn, 2017; Kleven et al., 2019a,b; Kuziemko et al., 2020).[3] As far as non-labor-market outcomes are concerned, the closest papers to ours are Miller et al. (2020) and Massenkoff and Rose (2020).[4] Miller et al. (2020), who study the effects of abortion denial among a sample of 600 women seeking to terminate their pregnancies, find that abortion denial leads to increases in financial instability and, according to self-reports, increased rates of living alone with children or living with a male partner. Massenkoff and Rose (2020) employ an event study design to investigate the effects of pregnancy on crime and, like us, find that pregnancy leads to large reductions in criminal behavior. We contribute to this literature in three main ways: first, we study a broader set of domains than has previously been possible. To the best of our knowledge, the effects of pregnancy and parenthood on public housing residence, homelessness, and substance use disorders have not been studied before in the economics literature.[5] Second,

[3]There is also a literature on the consequences of *teenage* parenthood for education and labor market outcomes. See Hotz et al. (1997), Fletcher and Wolfe (2009) and Kearney and Levine (2012).

[4]Also related are papers about government benefit use and family formation. Existing papers in this domain rely on self-reported survey measures, and do not adjust for overall time trends and individual-specific pre-trends. See Celhay et al. (2021) for a discussion of systematic errors in self-reports for the case of government benefits. *Medicaid*: Daw et al. (2017) rely on the Medical Expenditure Panel Survey ($N = 2,726$) and find a 20pp increase in self-reported Medicaid enrollment at delivery relative to the quarter before pregnancy (while we find a 15pp increase in our full sample). Also related are Adams et al. (2003) and D'Angelo et al. (2015), who rely on retroactive survey data collected after delivery. *TANF*: Kim (2018) relies on the SIPP survey and detects a 10pp increase in self-reported TANF receipt after childbirth compared to before pregnancy among low SES women. *SNAP*: Gordon et al. (1997) show average participation rates in the food stamps program by quarter/trimester relative to childbirth, based on self-reports from the 1990-91 SIPP survey waves.

[5]As far as SUD treatment is concerned, Wolfe et al. (2007) study a sample of women identified as having a SUD *at their delivery encounter*, and document changes in treatment for substance use disorder from pre-conception, to pregnancy, to the postpartum period. When aiming to identify the impact of pregnancy and parenthood on SUD treatment, this sample selection criterion introduces important selection concerns that we circumvent in our analysis by avoiding sample selection based on post-conception outcomes.

we use high-quality, high-frequency administrative data to carry out our analysis.[6] Our fine-grained high-quality data allows us to trace out changes, at a high resolution, over each month pre-pregnancy, during pregnancy, and post pregnancy. Third, we are able to explore the effects of pregnancy and parenthood across multiple domains at once. As discussed above, the high-dimensionality of our data allows us to show that different typologies of women face distinct challenges related to pregnancy and childbirth. Furthermore, it allows us to engage in a deeper exploration of mechanisms than has previously been possible. For instance, in the domain of criminal behavior, we add texture to the finding in Massenkoff and Rose (2020) by showing that the observed reduction in criminal behavior is likely in part driven by better access to social assistance programs, rather than solely by soon-to-be and new parents becoming motivated to "turn their life around" (the turning point hypothesis formalized by Sampson and Laub (1990)).

This paper also contributes to a large and growing literature on the causes of economic distress by focusing on parenthood as a major life event. 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). Similar to these shocks, new parenthood can have a large impact on domains ranging from housing to criminal behavior.

Finally, this paper contributes to the literature on housing instability and homelessness.[7] 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 explored the role of evictions and eviction policies in causing homelessness (Collinson et al., 2021; Abramson, 2021). The rest of the 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 (Lucas, 2017; Corinth, 2017). We contribute to this literature by providing evidence that pregnancy and childbirth are important drivers of housing instability

[6] With the exception of Massenkoff and Rose (2020) for the case of criminal behavior and Miller et al. (2020) for the case of financial stability, the existing literature on the effects of pregnancy on non-labor-market outcomes does not rely on high-frequency administrative data.

[7] See Evans et al. (2019) for a thorough review of the literature.

and homelessness.

The rest of the paper proceeds as follows. Section 1 describes the setting, data, sample, and outcomes. Section 2 outlines our empirical strategy. Section 3 shows our results. Section 4 presents various robustness checks that probe the robustness of our results. Section 5 shows results for first-time fathers. Finally, Section 6 concludes.

1. Setting, Data, and Definitions

1.1 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% nation-wide) and a much lower population share of Hispanic ethnicity (2% vs. 16% nation-wide). 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 in order 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, and includes a unique identifier that is used to link a resident's records across domains. Records were provided to the research team in the form of anonymized individual-level panel data.

The data includes the universe of birth records pertaining to births in Allegheny County, as well as Medicaid mental and physical health claims records, homelessness service records, public housing and Section 8 records, welfare benefit records (Medicaid, SNAP, TANF), and court records (misdemeanor and criminal offense charges) for all residents of Allegheny County. 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 breath point of view, the Allegheny County data is ideal because it provides a comprehensive set of key markers of well being and economic hardship– some previously unstudied– 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 mental health/substance use disorders), and typically non-linkable across domains (and thus to life events such as becoming a parent) in administrative data.

1.2 Sample selection

Our primary aim is to study the effect of pregnancy and childbirth on the lives of low-SES individuals. For reasons related to selection described in the next paragraph, we focus on women for the main analysis, and we discuss results for men only briefly. In order to identify low-SES women who become first-time parents, we first need to identify occurrence and date of first time parenthood, and second identify low SES status. In what follows, we lay out the details of both steps.

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 time period guarantees that we have at least two years of pre-birth outcome data for each parent, since our outcomes cover the time period from 2005 onward.

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 our decision to focus on women only for our main analysis, and report results for men only briefly in the very end, in Section 5.

We further restrict the sample to those ca. 99,500 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.[8] We identify first birth mothers as those for whom no birth record from a date earlier than 2007 exists, and whose birth record pertaining to 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 individuals.

Identifying low SES individuals 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.[9] 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), its take-up rate is relatively high, estimated at ca. 70% among adults and ca. 80-90% among children (Sommers et al., 2012), and its eligibility cutoff for household income—138% of the Federal Poverty Level (FPL)—captures the 17% poorest households in Pennsylvania (US Census

[8]We explore differences in effects around first and second births in Section 3.1 for the purpose of uncovering the mechanisms behind the changes we observe around first birth.

[9]For completeness and robustness, we also provide results for the entire sample of first live births (without the low SES restriction), for the smaller sample receiving SNAP at any point in the five years ahead of their first pregnancy, and for the slightly larger sample receiving SNAP or Medicaid at any point in the five years ahead of their first pregnancy; results are reported in the robustness Section 4.

Bureau, 2018). Note that the income eligibility threshold was stricter for those below age 21 until 2014, and for those at or above age 21 until 2015.[10,11] Therefore, relative to the full sample of first-time mothers in the county with incomes below 138% of FPL pre-pregnancy, our sample misses those with first births in the first half of the sample period who are the least poor and who are older. Furthermore, since we only capture the estimated 70-90% of Medicaid-eligibles who take up the benefit, the sample also skews towards those more affine to or familiar with government assistance. Of the approximately 97,400 first birth events observed in our sample period, 15%, or 12,500, are to women whom we identify as low SES. In our discussion of sample demographics in Section 1.3, we compare demographic characteristics of the low SES sample to its non-low SES counterpart, documenting clear markers of economic vulnerability—in terms of age at first birth, race, whether a father is listed on the birth record, pre-pregnancy SNAP receipt, and encounters with the homelessness and criminal justice system—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[12] to one year after birth, covering a total of 33 months per individual. Including "only" twelve pre-conception months allows us to control for more precise and accurate individual-specific linear pre-trends; restricting the post-birth observations to a one-year window, as opposed to a longer time horizon, ensures that our difference-in-difference imputation estimator, which predicts post-

[10]The threshold rose from 100% to 138% of FPL for individuals 6-20 years of age in 2014 (Kaiser Family Foundation, 2021a). It rose from 0% (i.e. categorically ineligible) to 138% of FPL for individuals older than 20 without disabilities and without dependent children as part of the ACA expansion, which took effect in Pennsylvania in June 2015 (Kaiser Family Foundation, 2021b)

[11]The Medicaid criterion captures a large fraction of individuals receiving any type government assistance for low-income individuals: in our data, it captures 82% of individuals whom we observe using *any* of the public assistance programs in the five years leading up to pregnancy (that is: residence in public housing, use of Section 8 rental assistance, homelessness encounter, use of SNAP benefits (i.e. food stamps), and Medicaid insurance status.

[12]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 almost never last longer. In our main analysis sample, 64% of pregnancies last 37-39 weeks, equivalent to 8.51-8.98 months (calculated as weeks of gestation as listed on birth record—that is, weeks from beginning of last menstrual period to moment of childbirth—minus two weeks, representing the time since fertilization). Only 0.6% last more than nine months (i.e. 39 weeks), and 11.27% of pregnancies last less than 35 weeks (8.05 months), and 24% last 35-36 weeks (8.05-8.28 months).

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 12,589 individuals whose first childbirth falls into the time period January 2007 to September 2018; for individuals for whom we do not observe the full 33 months (that is, individuals with childbirth dates after September 2018), we still include observations from the twelve months before conception in our estimation of date and individual fixed effects (i.e. step one of our imputation estimator) in order to estimate date fixed effects in 2018 and 2019 with a large-enough sample.

Sub-sample for substance use disorder analysis Finally, for substance use disorder outcomes only, we restrict the sample to individuals who were Medicaid-insured for at least 90% of the months in the event time window (that is, 30 months out of the 33 months). We make this restriction because we only observe substance use disorder treatment for Medicaid-insured individuals.[13] By restricting to continuously Medicaid-insured individuals, we can insure that any changes in those outcomes measured around the birth event are due to actual changes in service receipt (as opposed to changes in mere *visibility* of service receipt due to changes in insurance status). This restriction retains 36% of the sample, resulting in a sample size of ca. 4,500.

1.3 Summary Statistics

In Table 1, we show summary statistics for our main event study sample—low SES first-time mothers—in column (1), and statistics for all other first-time mothers in column (2). We observe a total of 12,589 first live births between 2007 and 2018 occurring to women we identify as low SES, and 68,124 first live births to non-low SES women. Indeed, our low SES sample shows much more pronounced markers of economic vulnerability than its non-low-SES counterpart: relative to the non-low SES sample, the low SES sample skews much younger

[13]We also observe care for a likely small number of uninsured individuals whose uncompensated care is paid for through publicly funds. It is estimated that about two thirds of uncompensated care to the uninsured is financed with public funds (Coughlin et al., 2014).

(average age at first birth of 22 years vs. 29 years), includes a much larger share of underage mothers (9.7% vs. 1.2%), a much larger share of women who are black (52.6% vs. 8.5%), whose child has no father listed on the birth certificate (43.8% vs. 9.2%), who receive SNAP benefits (i.e. food stamps) at any month in the year pre-pregnancy (38.4% vs. 1.1%), and who experience at least one homelessness encounter (1.7% vs. 0.0%) or encounter with the criminal justice system (11.1% vs. 1.0%) in the year before pregnancy.

We do not observe the fraction of births in our data that resulted from unintended pregnancy, however we estimate this amount to be around 40 percent based on studies of similar populations. This estimate comes from research by Finer and Zolna (2011), who use survey data to show that for American women with incomes below the poverty line, 62 percent of pregnancies were unintended, and 43 percent of unintended pregnancies resulted in an abortion.

1.4 Outcomes

We observe outcomes in four domains: housing, mental health and substance use, social assistance use, and criminal behavior. Outcomes in the first three domains are available for the full period, from January 2005 to September 2019. Outcomes in the last domain are only available from 2007 onward. For each outcome, we construct individual-month level indicators that equal one in case a given event occurred that month, and zero otherwise. We describe the construction of each outcome in brief below (and provide more details in Appendix B), followed by a brief overview of program eligibility rules.

Housing Housing is a key determinant of well-being that is likely heavily affected by having a child. For low SES individuals in particular, pregnancy and childbirth might lead to short-term housing instability when existing housing arrangements terminate abruptly but no savings exist to secure a new rental quickly (e.g. due to exile from the parental home); in the longer term, parenthood may lead to increased pressure to rely on "cheaper" housing solutions to accommodate the increased need for space and additional expenditures due to living with a child. Our data allows us to capture both of these aspects: we measure short-term housing instability by tracking homeless shelter stays (*Homeless shelter*); we measure changes to longer-term housing solutions by tracking reliance on the other key housing support programs observable in our data. These programs can be divided into those specifically designed for individuals experiencing homelessness and typically running for 6-24 months (namely, Rapid Rehousing, Transitional Housing and Permanent Supportive Housing—summarized into a single outcome labelled *Long-term homeless*),[14] and rental subsidy programs for the low-income population more generally (namely, residence in *Public housing* and household receipt of *Section 8* rental assistance).[15] To investigate whether women who start relying on public housing and Section 8 vouchers are forming their own households (vs. moving in with their parents), we consider as secondary outcomes whether the individual is listed as the household head for a given housing benefit.[16]

Substance Use Disorders In the domain of mental health, we focus on substance use disorders (and in particular opioid use disorder), because these disorders impose a very high burden on affected individuals, their children, and society (Degenhardt and Hall (2012), Romanowicz et al. (2019), U.S. Department of Health and Human Services (2016)), highly effective treatments exist for many of them but treatment is severely under-utilized (Blanco et al., 2013), and we have little quantitative evidence on individual (i.e. demand-side) determinants of treatment take-up beyond correlational evidence.[17] In particular, to the best

[14] Rapid Rehousing is a program providing primarily housing search and rental assistance to individuals at-risk of homelessness, for a duration of up to 24 months; Transitional Housing provides temporary housing in the form of a room or apartment in a residence with support services to individuals formerly experiencing homelessness, for up to 24 months; Permanent Supportive Housing provides housing search and rental assistance, as well as intensive support services to individuals who experience chronic homelessness, for unlimited duration (Allegheny County Human Services, 2021).

[15] Public housing provides rental subsidies in properties typically owned by the government, while Section 8 vouchers provide rental subsidies for privately-owned properties.

[16] This information is available for about 73% of public housing dwellers and Section 8 voucher users. We code it as a dummy variable that equals one if the person is listed as head of household, and zero otherwise (that is, if the information is missing or if the person does not make use of public housing or Section 8 that month).

[17] There is a sizeable correlational literature describing individual-level factors, such as age, gender, and onset of disorder, that are associated with treatment initiation among individuals with substance use disorders. See, for example, Blanco et al. (2015).

of our knowledge, only one study—Wolfe et al. (2007)—exists that studies the association of pregnancy and parenthood with addiction treatment using individual-level panel data; however, this study, which uses data from the late 1990s, is limited to a cohort of women identified as having a substance use disorder via diagnosis codes associated with their delivery encounter, introducing important selection concerns that we circumvent in our analysis by avoiding sample selection based on post-conception outcomes.

Our mental health claims data spans treatment encounters for mental health disorders paid for through public funds. That is mainly treatment of Medicaid and Medicare insured individuals, as well as (a likely small amount of) treatment of uninsured individuals whose uncompensated care costs are paid for through public funds. To avoid issues with interpretation (i.e. an inability to distinguish between changes to actual treatment vs. changes to mere visibility of treatment in our data due to changes in insurance status), our analysis of drug addiction treatment is based solely on the sub-sample of continuously Medicaid insured individuals. The following types of treatments are included in the data: outpatient psychotherapy, outpatient medication-based addiction treatment, inpatient stays in psychiatric hospitals and addiction treatment centers, and other treatment services (such as peer programs, detoxification, telephone crisis); each treatment encounter is associated with a diagnosis code that delineates the associated disorder.

As our main outcomes of interest, we consider i) treatment for any substance use disorder (*Any SUD treatment*), and ii) treatment for the most common substance use disorder observed in the data: *Opioid use disorder treatment*.

As secondary mental health outcomes, we consider treatment for the next most common substance use disorders (cannabis, alcohol, and cocaine use disorder), as well as the main sub-components of treatment for opioid use disorder: opioid use disorder medication treatment encounters (such as methadone treatment encounters), and inpatient opioid use disorder treatment (i.e. rehab).

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*; while Medicaid is an individual-level benefit program, food and cash assistance operate at the household-level. Hence, for the latter two outcomes, a person-month is coded as one if the household the person resides in receives the benefit.

Criminal Behavior Regarding criminal behavior, our main outcome indicator, *Criminal offense*, equals one in the month in which a new criminal charge is filed in a court of the county (the relevant courts include the Court of Common Pleas and Magisterial District Courts), and zero otherwise. As secondary outcomes, we distinguish between felony and misdemeanor cases (using a dummy for each), and among felony cases, we further distinguish major types of felonies, namely assault, theft, drug possession, DUI charges, and all other charges (such as terroristic threats, criminal trespassing, and prostitution).

1.5 Program Eligibility Rules

In order to draw welfare conclusions we need to understand to what extent the changes we observe around first-time parenthood reflect changes in *underlying need*, *eligibility*, or *information*. Eligibility criteria are the only elements readily observable to the researcher. Hence, we collect information on program eligibility rules for each outcome in our data. The data is summarized in Appendix Table A.3.

Eligibility for many government programs increases markedly when one becomes a parent. In particular, eligibility for the three large government benefit programs that provide health insurance, food assistance, and cash assistance (namely Medicaid, SNAP, and TANF) 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 increases from \$1,400 per month before pregnancy, to \$3,100 during pregnancy, to \$2,000 post childbirth. Therefore, if the bulk of the observed change in uptake of these programs occurs immediately and sharply around the dates in which eligibility changes due to family status, it is an indicator that the observed changes are likely largely eligibility-driven.

The homelessness assistance environment also changes as individuals change family status, although it does not increase with additional children. Namely, all main homelessness services (i.e. emergency shelter, transitional housing, and permanent supportive housing services) are provided in separate facilities, depending on whether a child is present, potentially changing the supply (and quality) of available program slots for women as they transition from single status to parent status. Accordingly, in our investigation of impacts of parenthood on homelessness in Section 3.1, we perform additional analyses beyond our baseline event study, in order to better isolate need-based changes in homelessness encounters due to childbirth. Specifically, we compare changes in housing outcomes across the first and second live birth event, for women of low SES who have at least two live births. The idea behind this approach is that for women who already have a dependent child, homeless service eligibility does not change with the second pregnancy/birth.

In contrast, eligibility for public housing and Section 8 vouchers does not change significantly as family status changes.[18] For both programs, assignment is based on wait lists that do not prioritize pregnant women or families with children; the order is determined by the date in which applications are received (Allegheny County Housing Authority, 2021). However, we cannot rule out completely that family status influences wait times: first, for public housing, wait times for apartments of different sizes may differ (and larger households can apply for larger appartments); second, it is possible that some individuals in the housing authority may discretionally prioritize pregnant women or families with small children, against the official policy.

Finally, eligibility for substance use disorder treatment does not vary by pregnancy/family status, conditional on Medicaid insurance status: such treatment is covered by Medicaid for

[18] A change in family status does affect the minimum and maximum size, in terms of bedrooms, that households are eligible for. It increases by one for every additional household member (Allegheny County Housing Authority, 2020).

both pregnant and nonpregnant patients, and, for the case of opioid use disorder, a recent RCT documents that pregnancy status does not increase treatment access: among simulated patient-callers who called outpatient opioid addiction treatment centers in ten U.S. states, those representing *nonpregnant* women were *more likely* to be granted an appointment than those representing pregnant women, while experiencing the *same wait times* conditional on receiving an appointment (on average 1-3 days) (Patrick et al., 2020). Therefore, any increases in treatment for substance use disorder we observe due to changes in family status are unlikely to be eligibility- or wait time-driven.

2. Empirical Strategy

The primary goal of this paper is to map out the impact of becoming a parent on living conditions for economically vulnerable women. In an ideal experiment aimed at identifying causal effects, first-time parenthood would be randomly assigned to a random subset of this population. In the absence of such an experiment, we exploit the detailed panel-nature of our data in an event study framework that is based on sharp changes around discovery of pregnancy and the birth of a first child.[19] Clearly, unobserved changes to life 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. However, such endogenous factors plausibly evolve smoothly around the exact time of conception/childbirth, and thus we can recover the impact of parenthood via estimating discontinuous changes from such smooth trends at the event of childbirth (Kleven et al., 2019b). In essence, we achieve this by employing a dynamic difference-in-difference approach with individual and time fixed effects using high frequency panel data and including controls for individual-specific pre-trends—that is, we measure changes in outcomes around pregnancy

[19] As highlighted by Kleven et al. (2019b), one advantage of this approach—besides delivering sufficient sample size and thus statistical power to study individuals of low SES—relative to instrumental variable (IV) approaches is that it allows for estimating the average impact across all first-time mothers in the data, as opposed to that local to individuals on the margin of abortion (as in Miller et al. (2020) and Zohar and Brooks (2021)) or IVF treatment (as in Lundborg et al. (2017)).

and childbirth relative to smooth trends at the individual level, differencing out overall time trends that are unrelated to childbirth using women who have children at different points in time. In this section, we first lay out the details of our event study design, and then discuss identification.

Our empirical approach proceeds in two steps: first, we graph raw means of the outcome variables over time relative to a woman's first live birth; second, we present event study estimates. Plotting raw means allows us to visually assess the existence of pre-trends, as well as the sharpness of changes upon discovery of pregnancy and upon childbirth. Furthermore, the visual inspection of the raw means inform the choice of functional form for the event study specification; specifically, it gives us a sense of whether individual pre-trends (if any) are linear, quadratic, etc. Under the assumptions discussed in detail below, the event study allows us to obtain causal effect estimates for each month relative to child birth, as well as summarize them into more aggregate periods.

For our event study analysis, we follow recent advances in the econometrics literature by applying an estimator that circumvents established issues of conventional event study estimation methods. Specifically, standard two-way fixed effect (TWFE) models that have typically been used to estimate treatment effects in settings like ours—that is, settings with "staggered adoption" of treatment across individuals over time—have been shown to deliver inconsistent estimates in the presence of treatment effect heterogeneity (see, for example, Borusyak et al. (2021), Sant'Anna and Roth (2021), Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020)). The issue arises because the treatment effect estimate obtained from a TWFE model is a weighted average of all possible 2×2 difference-in-differences (DD) comparisons between groups of units treated at different points in time (Goodman-Bacon, 2021). For example, already treated units may act as controls for later treated units; in this case, when treatment effects vary over time, changes in the treatment effect to already treated units get subtracted from the DD estimate, thus yielding potentially negative weights—an issue termed as "forbidden comparisons" by Borusyak et al. (2021). Such issues may even flip the sign of the estimate compared to the true effect. To circumvent such issues, we use Borusyak et al. (2021)'s "imputation" estimator—which is shown to be robust and efficient under treatment effect heterogeneity—as our main specification. In a nutshell, this method circumvents "forbidden comparisons" by only using pre-treatment observations to estimate individual and time fixed effects, thereby allowing for arbitrary treatment effect heterogeneity.[20] Nevertheless, for completeness, we also report results from a conventional two-way fixed effects estimator in the robustness section, and find our results virtually unchanged.

Following Borusyak et al. (2021), we construct the imputation estimator in three steps, which we summarize briefly here, and then describe in more detail below. The estimation relies on panel data with observations at the person-date level, where date corresponds to year-month. First, we estimate date and individual fixed effects by OLS on untreated (i.e. pre-conception) observations only. Second, we use these estimates to extrapolate/impute untreated potential outcomes for treated (i.e. post-conception) observations, and obtain the treatment effect estimate for each observation as the difference between actual and imputed outcome. Third, we estimate the target treatment effect for a given relative time period of interest (such as two months post childbirth) as the simple average of the treatment effect estimate for that relative time period across all individuals. As described in Section 1.2, our baseline specification limits the sample to a completely balanced panel with individual-month pairs that fall within 12 months before conception and 12 months after birth.[21]

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

[20] To the best of our knowledge, to date, it is the only valid estimator in the event study context under presence of heterogeneous treatment effects whose efficiency properties are known. Furthermore, the estimator allows for consistently estimating treatment effects aggregated across several periods—a feature that is key for our setting with high-frequency data and many post-treatment periods; other available estimators such as those proposed in Sun and Abraham (2020) and de Chaisemartin and D'Haultfœuille (2020) do not have this feature.

[21] In order to estimate calendar month fixed effects in 2018 and 2019 with a large enough sample in the first step of our estimation procedure, we also include observations falling into the twelve months before conception among those with incomplete panel data due to childbirth dates after September 2018. Those observations do not enter treatment effect estimation in later steps.

OLS:

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

where y_{it} is the outcome of interest for individual i in calendar year-month t, and where μ_i and γ_t are individual and calendar year-month fixed effects, respectively. In our context, "untreated observations" are all those observed ahead of a woman's pregnancy that results in her first live birth.[22] We allow for an individual-specific linear pre-trend, captured by δ. By including this linear pre-trend, the treatment effect estimates computed in step two give the change in the outcome following the onset of pregnancy and childbirth relative to any pre-existing linear trend. We report results from a model omitting this term in the robustness section, and find they remain unchanged.

In the second step, we obtain observation-level treatment 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, in order 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, in order 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 cluster at the individual-level, whose formula is derived

[22] We approximate the calendar year-month of pregnancy onset to fall nine months before the calendar year-month of birth. See footnote 12 for details.

and shown to be valid in large samples in Borusyak et al. (2021).[23]

2.1 Identification

Our empirical strategy relies on two standard assumptions: no anticipatory effects and parallel trends. No anticipation requires that there is no anticipatory response to pregnancy ahead of time—an assumption plausible in our setting, in which many pregnancies are unplanned and the timing of conception often cannot be predicted to the exact month. Nevertheless, we provide standard robustness checks that exclude the three months immediately preceding pregnancy from estimation (see Section 4).

Parallel trends requires that conditional on having a live birth in the sample period and on the included controls, in the absence of pregnancy and childbirth, the expectation of the outcome of interest follows the same path for all individuals and in all time periods available in the data. This assumption implies that the exact timing of conception is uncorrelated with changes to the outcome, conditional on controls. The main threat to our identification strategy is that the timing of pregnancy is correlated with other significant life events that also influence the outcome of interest, such as meeting a new partner. If this is the case, then we cannot interpret the change in outcomes from pre- to post-pregnancy as being *due to* the birth of a child.

Given that pregnancy likely occurs with a lag relative to any changes in living conditions (such as meeting a new partner) that also influence the outcomes of interest, and given the high-frequency nature of our outcome data, we start by visually and informally checking for pre-trends in the raw data. In addition, the sharp timing of the onset of pregnancy and of childbirth allows us to assess whether outcomes change discontinuously around these times. The left panels in Figure 1-Figure 5 graph the time series of raw mean outcomes relative to the month of first child birth. Across all outcomes, the raw time series reveal smooth linear or no trends leading up to the pregnancy, as well as sharp trend breaks either around the

[23]We use Borusyak et al. (2021)'s STATA packages "did_imputation" and "event_study" to obtain treatment effect estimates, standard errors, and event study plots.

discovery of pregnancy in month 2-3, or around the month of child birth, or both.

These findings inform our choice of controls for the event study specification from Equation (1). In particular, they suggest that a specification with an individual-specific linear pre-trend in event time is the most suitable functional form in order to control for pre-trends. By including this control, the coefficients on pregnancy and post-birth periods identify changes in outcomes net of any pre-existing trends. To formally assess whether this specification accurately nets out any pre-trends, we test for and reject the presence of pre-trends for across all our twelve outcome variables, using the pre-trend test derived by Borusyak et al. (2021).[24] Results from this test are reported in the bottom row of the results presented in table-form (Table 2-Table 5).

To the extent that the onset of pregnancy is correlated with sharp changes to living conditions, controls for linear pre-trends fail to account for such residual endogeneity. Therefore, we provide further evidence with a difference-in-difference analysis, comparing women who experience live births to those who experience miscarriages (similar to Massenkoff and Rose (2020)). This design addresses the potential endogeneity in the (sharp) timing of pregnancy. Finally, to directly net out any age effects, we employ a matched differenece-in-difference 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. These analyses are detailed in Section 4.

3. Results

3.1 Impacts on Housing

One of the fundamental non-labor-market outcomes that are likely to be heavily affected by having a child is housing. For low SES individuals in particular, pregnancy and childbirth might lead to short- and long-term housing disruptions. Pregnant women and new mothers

[24]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.

might require short-term housing assistance whenever their existing housing arrangements terminate abruptly; such abrupt terminations could happen, for instance, due to evictions or, in the case of teenage mothers, exile from the parental home. Pregnancy and childbirth are also likely to affect longer-term housing needs for reasons related to space, expenditures, changes in domestic relationships, etc. In this section, we first present results on short-term housing solutions in the form of homeless shelter visits, and then present results on medium-to-long-term housing solutions. We further investigate heterogeneity by race, age, and type of housing assistance.

Short-Term Housing Assistance The main programs providing short-term housing support in the United States consist of homeless shelters and emergency cash grants for rental assistance; we observe the former in our data and report results on homeless shelter use below.

The top panel of Figure 1 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 treatment effects for each month relative to conception, obtained from event study analysis as described in Section 2. 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. Table 2, which summarize average treatment effect estimates across the more aggregate time periods of pregnancy, and year post birth, tells a similar story: the coefficient estimate is positive and highly statistically significant during pregnancy. The magnitudes of the effects are substantial: during pregnancy, homeless-shelter visits increase by 0.1pp (100%) compared to the no-child counterfactual; the effect in the year post-birth is of equal magnitude, but more noisily estimated. These results suggest that childbirth and especially pregnancy may generate substantial short-term housing disruptions for low SES women. We find that these effects are likely reflecting real increases in housing disruptions, as opposed to changes in eligibility for homeless services due to pregnancy or new parenthood: when comparing effects across first and second births for the subset of women in our sample for whom we observe two births, we find that effects are as pronounced at the second birth—where eligibility stays constant (see Figure A.4).

Figure A.1 explores heterogeneity along age and race and shows that Black women, who might be more economically vulnerable than their white counterparts, are more likely to visit homeless shelters as a result of pregnancy and childbirth (while there is no heterogeneity by age). This finding is consistent with the intuition that the most vulnerable individuals might lack the resources to insure themselves against short-term disruptions to housing needs due to changes in family composition.

Medium-to-Long-Term Housing Assistance Pregnancy and childbirth are likely to also generate medium-to-long-term disruptions to housing needs. For instance, the arrival of a child might require new mothers to find more spacious housing solutions or, if young, to move out from their parents' homes. In the United States, various programs help individuals with low incomes obtain stable housing. As summarized in Section 1.4, the programs can be divided into those specifically designed for individuals experiencing homelessness and rental subsidy programs for the low-income population more generally. The bottom panel of Figure 1 and Figure 2 show that low SES women increase their take-up of medium-to-long-term housing assistance programs as a result of pregnancy. Once again, the left half of each figure displays a time-series of raw means and the right half displays event-study results based on the empirical strategy outlined in Section 2. We observe a clear pattern: pregnancy and new parenthood lead to stark increases in movement into public housing, as well as into medium-term homelessness housing programs. Relative to public housing residence, the effect on Section 8 voucher use is smaller and only statistically significantly different from zero starting about six months after childbirth. In terms of magnitudes, compared to the no-child counterfactual, public housing dwelling increases by 1.3pp (or 27%), Section 8 utilization increases by 0.6pp (or 5%), and residence in medium-to-long-term homelessness programs increases by 0.4pp (or 67%) in the year post childbirth (see Table 2, which summarizes the event study estimates in table form). The larger and earlier impacts on public housing compared to Section 8 are consistent with much higher overall demand yielding longer wait times for Section 8: in Allegheny County, the average length of time spent on the wait list for public housing is 9.2 months, compared to nearly three years for Section 8 vouchers (Deitrick et al., 2011). Such relatively long wait times render Section 8 housing assistance unsuitable for addressing short-term changes in housing needs.

Since the three forms of housing assistance provide distinct housing environments with likely different impacts on well-being and child development, it is worthwhile investigating the typology of women who enroll in the different housing assistance programs as a result of new parenthood.

Figure A.2 shows that Black women, who are once again more likely to be in a vulnerable position, show a disproportionate increase in movement into public housing (top left panel). Furthermore, it is disproportionately younger women who move into public housing due to parenthood (top right panel). Results for our secondary housing outcomes—proxies for living outside of one's parental household given by a dummy for whether a person is registered as "head of household" in her subsidized housing—suggest that the increased movement into public housing triggered by new parenthood is not driven by moves back into one's parent's household, but rather by moves *out of* parental households: we find a large positive effect of new parenthood on the probability to head a household in public housing-with 1.5pp, the effect size is even larger than that observed for public housing residence, overall (Table A.5).[25] Conversely, medium-to-long-term homelessness programs do not exhibit meaningful heterogeneity along the dimension of race (bottom left panel), while increased enrollment in such programs seems to be driven more by women above the median age at childbirth of 22 (bottom right panel). Due to the prevalence of substance use disorder among homeless individuals (Early, 2015), which may require more intense assistance, medium-to-long-term homelessness programs tend to be particularly geared towards individuals who

[25]In contrast, we do not detect a statistically significant impact of new parenthood on being the head of a Section 8 voucher using household. The associated estimated coefficient of 0.2pp is less than half of that observed for Section 8 voucher use, overall, suggesting that much of the increase in Section 8 voucher use could be driven by young women's returns into Section 8 voucher using parental households. It is consistent with long wait times rendering Section 8 housing assistance unsuitable for addressing short-term changes in housing needs.

experienced issues with substance use. Figure A.3 shows that, indeed, individuals who were ever treated for substance use disorder ahead of their pregnancy (11% of the sample) are disproportionately more likely to move into medium-to-long-term homelessness housing programs as a result of pregnancy and childbirth.

The last result about medium-to-long-term homelessness being driven primarily by people who experienced issues related to substance use suggests that pregnancy and childbirth could be a particularly promising time to connect such individuals to various government services, including ones for substance use disorder treatment. To that we turn next.

3.2 Impacts on Substance Use Disorder Treatment

The analysis presented in this section relies on outcomes measured via Medicaid claims records, which necessitates an additional sample restriction: to ensure that we are picking up changes in actual treatment/diagnosis, as opposed to changes in observed treatment due to switching from a private insurance plan to Medicaid, we restrict to the sub-sample of women in our live birth event study sample who were continuously Medicaid-insured throughout our event time window.[26]

In Section 3.1 we document that the increase in use of medium-to-long-term homelessness services due to new parenthood is primarily driven by women who experience issues related to substance use, especially opioid use disorder. Such finding suggests that pregnancy and childbirth could be a promising time to connect individuals who suffer from substance use to government services, especially treatment for substance use disorder.

Indeed, we find that new parenthood increases treatment for substance use disorder, and that this is driven by treatment for opioid use disorder, the most common substance use disorder observed in our data. Figure 3 presents a time series of raw means of treatment for *any* substance use disorder (top panel) and opioid use disorder specifically (bottom panel)

[26] We define "continuously insured" as being Medicaid insured in at at least 90% of months in our event time window, that is in the 33 months covering the year before pregnancy, nine months of pregnancy, and the year after birth. This selection criterion retains 36% of the original sample. Compared to the full sample, the resulting sub-sample is younger and more economically vulnerable.

in the left panel, and the associated results from the event study specification outlined in Section 2 in the right panel; a summary of the corresponding effect sizes in table form is provided in the the first two columns of Table 3. The event study figure shows that treatment for opioid use disorder 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 0.5pp (or 29%) during pregnancy, and an increase of 2.2pp (or 129%) in the year post childbirth, compared to the no-pregnancy/no-child counterfactual. When investigating different treatment types in Table A.6, 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 nonpregnant patients (Mattick et al., 2014), and is also strongly recommended in pregnant patients (World Health Organization, 2014).[27]

It is important to point out that our data does not allow us to determine with certainty whether the increased treatment for opioid use disorder is due to increased treatment for already preexisting opioid use disorders, vs. new cases of opioid use disorder caused by pregnancy and parenthood. The timing of the increase, however, points to the former story rather than the latter. Specifically, as shown in Figure 3, medical encounters for opioid use disorder increase sharply in month 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 in order 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).

[27] We report results for the remaining secondary substance use disorder outcomes in the other columns of Table A.6. We find evidence of substitution of rehab-based opioid use disorder treatment for outpatient medication-based treatment due to pregnancy and parenthood (columns 1-2). Considering the next most prevalent substance use disorders after opioid use disorder (cannabis, alcohol, and cocaine), we detect no effects on treatment for cannabis use disorder, and small, marginally significant negative effects on treatment for alcohol and cocaine use disorder due to pregnancy (columns 3-5).

In sum, our findings suggest that new parenthood can be a major push factor out of untreated drug addictions. Clearly, access to addiction treatment services is critical to realize such gains in treatment. In the next section, we examine how new parenthood impacts access to Medicaid, the key healthcare program providing addiction treatment services for low income populations.

3.3 Impacts on Social Assistance Program Use

In this section, we present evidence that pregnancy and parenthood lead to major increases in the use of social assistance programs, find that much of this increase is likely eligibility-driven, and link in results on treatment for substance use disorders to inform the policy debate on insurance design.

Figure 4 shows event study results for the impact of pregnancy and parenthood on healthcare coverage, food assistance, and cash assistance; a summary of the corresponding effect sizes in table form is provided in Table 4. We observe a 26pp increase in Medicaid insurance status due to childbirth, and a 17pp and 14pp increase in SNAP and TANF receipt, respectively. In terms of magnitudes, the impact of new parenthood on Medicaid insurance enrollment is more than two times as large as that of the ACA expansion for the women in our sample.[28] This finding highlights that in practice, new parenthood is one of the most significant life events determining access to public benefit programs for individuals with low incomes in the United States. It is in line with Han et al. (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.

As discussed in Section 1.5, wider eligibility is likely to translate directly to higher

[28] In Appendix Figure A.5, we plot Medicaid enrollment rates in the years surrounding the expansion, which took effect in June 2015. For the cohort most affected by the expansion among those in our sample (women who have a child in the household—that is women with a first child born by 2013), we observe a 10pp increase in Medicaid enrollment due to the expansion. The impact of new parenthood is also about twice the as large as the impact of "aging out" of child Medicaid for the women in our sample (plotted in Appendix Figure A.6).

enrollment rates. Accordingly, we see a sharp, significant increase in uptake in month two to three after conception—the approximate time of discovery of the pregnancy—a time when pregnancy is unlikely to lead to large drops in earnings, but when the significantly more lenient eligibility criteria for pregnant women go into effect for all three programs. Moreoever, we observe sharp changes in benefit enrollment three months postpartum (Medicaid), and around the month of birth (TANF and SNAP)—the exact months at which these programs institute further eligibility changes due to change in family status.[29] Large increases in program enrollment due to pregnancy and parenthood may thus be expected. Coupled with other outcomes in our dataset, however, the results on social assistance programs can help shed light on: a) potential concerns with the structure of existing social assistance policies and b) the mechanisms behind some of our findings (see our results on criminal behavior).

Combining our findings in the domains of substance use disorder treatment and Medicaid insurance enrollment, we can shed light on the consequences of pregnancy-related health insurance churn. Figure 4 reveals that a substantial fraction of women—9%—abruptly loses Medicaid coverage at two months postpartum, when stricter eligibility criteria come into effect. This time period *precisely* coincides with the time in which women's propensities to enter SUD treatement are highest (see Figure 3). Accordingly, when we zoom in on the ca. 4,000 first-time mothers in our data who *lose* Medicaid at 60 days postpartum, we find an abrupt, 0.8 pp (or 65%) drop in publicly funded treatment for substance use disorder in the subsequent month (Figure A.7).[30] Even if many of the women who lose Medicaid might manage to become privately insured, they would likely have to change service provider and there might be a gap in coverage. Experiencing disruptions in—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

[29] For Medicaid, the income eligibility threshold drops from 220% of FPL to 138% (38-58% in the pre-expansion years) of FPL at 60 days postpartum (Kaiser Family Foundation, 2021c). The sharp 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 in the first three months after birth.

[30] "Publicly funded" mental health care includes care for Medicaid/Medicare-insured, and uncompensated care for uninsured, about two thirds of which is financed with public funds (Coughlin et al., 2014).

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 addiction 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, 2021d).

The increased take-up of social assistance programs as a result of pregnancy can also shed light on the mechanisms driving some of the effects of pregnancy and childbirth as shown in the next section.

3.4 Impacts on Crime

The last outcome we investigate is crime-related behavior. We begin by documenting overall effects on criminal behavior that are in line with findings from Massenkoff and Rose (2020), before analyzing mechanisms suggesting an important role for Medicaid-access in explaining the overall effects.

Figure 5 shows that pregnancy and childbirth lead to a substantial reduction in criminal behavior. Criminal behavior decreases gradually upon the discovery of 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 5, we find sizeable and statistically significant effect sizes of -0.5pp or 29% (for the period of pregnancy) and -0.6pp or 35% (for the year post childbirth). When distinguishing the two sub-components of criminal offenses: misdemeanor and felony offenses, we find significant reductions of similar magnitudes to both (see Table A.7). Among the sub-components of felony offenses, we observe the largest impact on criminal charges related to controlled substances. Our overall findings on criminal behavior are consistent with Massenkoff and Rose (2020), who document effects of similar magnitudes (on the order of a 70% decrease around birth, with largest decreases for drug-related crimes) on arrests among first-time mothers of Washington State.

The breadth of our data allows us to go further and 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 3.3 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. In particular, the crime-reducing effects of benefit receipt have been documented by Jácome (2020) for the case of healthcare coverage, Carr and Packham (2019) for the case of food assistance, and Foley (2011) for the case of cash assistance. The multi-domain nature of our dataset helps us distinguish between the two mechanisms and suggests that at least part of the decrease in crime may be due to better access to social insurance programs, especially Medicaid.

In order 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 gained access (the "Gained access" group).[31] We show event studies estimated separately for each sample in Figure A.8. We find a more marked, and longer-lasting decrease in criminal behavior for those who gain Medicaid access (compared to those who had it all along), consistent with access to healthcare coverage indeed driving at least part of the negative effect of childbirth on crime. This finding is highly consistent with results from Jácome (2020), who documents a 15% (or 0.55pp) increase in likelihood of incarceration among young men due to losing Medicaid upon reaching maturity. On the contrary, for SNAP, we find an equal-sized reduction in crime for SNAP-gainers and those

[31] "Access all along" is defined as having continuously been enrolled in a given government benefit program in the 12 months before pregnancy, and the 12 months after birth. "Gained access" is defined as having never been enrolled in the 12 months before pregnancy, but having been continuously enrolled in the 12 months after birth.

who were enrolled in the benefit all along, suggesting that newly-acquired access to food assistance is not driving the decrease in crime.

4. Robustness

In this section, we report results from two kinds of robustness checks: i) checks related to sample selection and model specification; ii) supplementary DiD analyses: a matched DiD using observably similar women who give birth two years later as a control group (to net out age effects), and a DiD approach that explores variation in pregnancy loss (to further control for endogeneity in the onset of pregnancy).

4.1 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. define low SES as having Medicaid or SNAP history, iii. define low SES as having SNAP history); b) excluding pre-conception months to rule out bias from "anticipatory effects"; c) omitting the individual-specific linear pre-trend control; and d) using a standard two-way fixed effect estimator.

To probe the robustness of our results to sample selection criteria, we start by omitting 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, which skews much less economically vulnerable (as can be gauged from summary statistics presented in Table 1), we find sign and statistical significance across 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—that is a

100% increase over the mean of 0.02%—compared to a 0.1pp in the low SES sample on a base of 0.1%, also equaling a 100% increase. 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).

We report results from the remaining robustness checks in Table A.11-Table A.13, and find statistical significance levels as well as magnitudes largely unchanged. Table A.11 employs our standard imputation estimator, but omits the three months immediately preceding conception in order to rule out that any anticipatory effects enter the estimation of individual- and time fixed effects. Table A.12 also employs our standard imputation estimator, but drops the individual-specific linear time trend. Table A.13 shows results from a standard two-way fixed effects estimator.[32]

4.2 Additional Difference-in-Difference Results

Matched DiD approach To account for age effects non-parametrically, we employ a matched DiD design similar to Fadlon and Nielsen (2021) and Mello (2021), 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 with the simultaneous evolution for a matched control group of comparable individuals who have their first birth two years later. We match women based on the year they were born, their race, and their Medicaid history. See Appendix C for details. We report dynamic treatment effect estimates in Figure A.9-Figure A.13 and summarize results in table-form for our main analysis sample of low SES first time mothers in Table A.15, and for the sample of all first time mothers in Table A.14. The matched DiD results closely

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

match those from our main event study. We find matched pairs of 'treated' and 'control' women to be on parallel trends ahead of the (placebo) pregnancy, and find sharp divergence in trends either upon discovery of pregnancy, or childbirth, or both.

Variation in pregnancy loss Finally, 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-difference analysis that compares women who have a live birth to observably similar childless women who experience a miscarriage. See Appendix D for details, including a discussion of the limitations of this analysis. We report results from the DiD estimation in Table A.17, 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 larger increase in homeless shelter stays during pregnancy, a larger increase in movement into public housing and in treatment of opioid use disorder after the birth event, as well as the expected larger increases in enrollment in Medicaid, SNAP, and TANF. Due to the relatively small sample of women with a miscarriage event ($N = 981$), the effect estimates for long-term homelessness and criminal behavior are noisier; the only outcome to switch sign is that of any substance use disorder treatment, which is consistent with potential endogeneity in the occurrence of miscarriages.

5. Results for Men

We present event study results of the impact of first-time parenthood on men in this section, finding effects that differ substantially from those observed for women on almost all primary outcomes. It is important to preface the analysis focusing on men with an important caveat: we identify first-time parenthood via being listed as father or mother on birth certificates, but fathers are often not listed, likely selectively so. While a mother is listed on virtually every birth certificate in our data, a father is missing on 16% of them, and this fraction rises to 38% for low SES children (i.e. children whose birth is paid for through Medicaid).

The "attrition" of fathers from birth records is likely selective: in Pennsylvania, among unmarried parents, both parents need to agree voluntarily about who the biological father to the child is by signing a form called "Voluntary Acknowledgment of Paternity (VAP)" (Form PA-CS 611) in front of a witness; this often happens directly after birth in the hospital. Establishing paternity matters for securing custody and visitation rights, and entitles the child to financial support from the father. 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 at birth (and thus, according to state laws, the father gets listed on the birth record automatically), is likely to be highly endogenous to similar forces, too. Hence, it is plausible that among men with similar demographic characteristics, those on a better recent economic or psycho-social trajectory are more likely to be listed on the birth record. We cannot address such selection issues within our event study specification, and hence the results presented in this section should be taken with a grain of salt.

We present summary statistics for first-time fathers in Table A.18, present event study results for low SES first-time fathers in Table A.19, and for all first-time fathers in Table A.20. Using our Medicaid insurance criterion, we identify 4,800 first-time fathers of low SES in our data, making up 8.4% of all first-time fathers. Relative to the sample of low SES first time mothers, first time fathers have similar characteristics, on average—the exception being a much higher rate of criminal charges in the year before the child was conceived (20.2% vs. 11.1%).

Focusing on event study results among the low-SES sample of first time fathers, we find that new parenthood has no statistically significant association with many outcomes, and often shows an opposing association relative to that found for women. Specifically, we find no statistically significant association with housing and substance use disorder treatment either during the period of pregnancy or in the year post-birth, a sizeable negative association with Medicaid and SNAP enrollment in both periods (in line with a selection story, by which men whose economic trajectories improve during pregnancy are more likely to be listed on birth records), and a positive association with criminal behavior after birth. In terms of statistical significance, results look very similar in the sample of all first time fathers (i.e. dropping the low SES restriction), although the coefficients switch sign for Medicaid, SNAP, and TANF.

Acknowledging potential selection concerns, we believe the aforementioned results are consistent with the following, tentative, interpretation: while it has been established that new parenthood leads to diverging trajectories of women and men in the labor market, we find that among individuals from economically vulnerable, disadvantaged backgrounds, having a child also has vastly different consequences for the overall living conditions of women relative to men, including domains of housing, social insurance use, and criminal behavior. These differences plausibly arise in environments in which parents do not cohabit and one parent shoulders most parenting responsibilities.

6. Conclusion

In this paper, we studied the effects of pregnancy and parenthood on the non-labor market outcomes of low-SES individuals in the United States. Due to selection in terms of whether fathers are listed on children's birth certificates, most of our analysis focused on mothers. We showed that pregnancy and parenthood lead to short-term and long-term changes in the housing environment, including increases in short-term homeless-shelter stays, transition into longer-term homelessness programs, and transition into public housing. We also document increases in treatment for substance use disorders, large eligibility-driven increases in use of key government assistance programs, and large reductions in criminal behavior driven at least in part by mothers gaining healthcare coverage.

Our results should be interpreted with caution for several reasons. First, despite our event study strategy featuring controls for individual-level linear pre-trends and our robustness checks, the decision to have a child is endogenous at least for some women, which might pose challenges to identification. As discussed, however, for a variety of policy questions such as those related to the allocation of homelessness services, observed changes to outcomes are of direct interest and precisely isolating causal effects is less relevant. Second, our analysis relies on data from one large county in the U.S. Although the county looks representative of the U.S. as a whole in terms of most observable characteristics in our dataset, we cannot rule out that the effects might be different in other counties or in the U.S. as a whole. Furthermore, our results are tightly dependent on the institutional framework in the United States; therefore, the extent to which the insights presented in this paper apply to countries other than the United States is not immediately obvious. Third—as is often happens with mental health outcomes measured via claims records—it is hard to determine whether increased treatment for substance use disorders is due to worsening/new occurrence of such disorders, or to an increase in treatment only. We argue that the sharpness and the timing of the increase in treatment for substance use disorder suggests the results are due to an increase in treatment for pre-existing substance use disorder, but we acknowledge that our data does not allows us to provide a more concrete answer to that question.

With these caveats in mind, our results point to four policy implications that may improve the living conditions of young, economically-vulnerable women on the verge of parenthood. First, the time of family formation is a particularly important and suitable one for programs assisting vulnerable women in moving to stable housing in high-opportunity neighborhoods. Not only is the period of family formation one with higher propensity to accept government assistance in the domain of housing (as evidenced by increased reliance on anti-homelessness services and public housing); it is also one in which stable housing and residence in high-opportunity neighborhoods is known to be most beneficial for children.[33] Second, one-size-fit-all solutions may not work particularly well, because different demographic groups experience different challenges as a result of pregnancy and childbirth. For instance, a significant fraction of vulnerable women who become parents have substance use disorders (11% in our sample). Those women have distinct trajectories with regards to homelessness and criminal behavior and, as a consequence, may require more intense social assistance. Third, the period of pregnancy and new parenthood is a very effective time to connect women

[33]See Clark et al. (2019) and Sandel et al. (2018), who document a strong positive association between pre- and postnatal homelessness and child ill-health; see Chetty and Hendren (2018) who show that the earlier a child moves to a better neighborhood, the larger its positive impact on social mobility.

to social assistance programs as well as addiction treatment services, suggesting that outreach and cross-program referral efforts are particularly impactful during this period. Fourth, our findings underscore the importance and potential of access to healthcare (and in particular treatment of substance use disorders) for reducing criminal behavior, supporting the evidence put forward by Jácome (2020) in a different setting.

Overall, we hope this paper can complement important qualitative work such as Edin and Kefalas (2005) by shining a data-driven light on the challenges that low-SES individuals—especially women—face during pregnancy and early-parenthood. We hope the results can help policy makers design effective safety-net policies to help economically vulnerable individuals deal with the disruptions, and realize the opportunities, caused by parenthood. Given the ample evidence documenting the importance of a child's pre- and postnatal environment for long-term health, well-being, and economic outcomes summarized in Almond et al. (2018), such improvements could have immense positive externalities.

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Tables

Table 1: Sample Demographics

	(1) Main Analysis Sample: Low SES First Time Mothers	(2) All Other First Time Mothers
	mean	mean
Age	22.386	28.893
Age 16-17	0.097	0.012
Black	0.526	0.085
White	0.454	0.843
Dad listed on birth certificate	0.562	0.908
SNAP recipient in year before pregnancy	0.384	0.011
Any homeless encounter in year before pregnancy	0.017	0.000
Charged with crime in year before pregnancy	0.111	0.010
Any MHD encounter in year before pregnancy	0.124	0.003
Any SUD encounter in year before pregnancy	0.050	0.001
Observations	12589	68124

Notes: Table shows demographic characteristics of all women in Allegheny County who experienced a first live birth in the sample period (2007-2018), and who were age 16-40 at the time. Women identified as low SES, and thus constituting our main event study sample, are grouped into column (1). All other women 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 pregnancy leading up to the first birth. Pregnancy onset is approximated as 10 months before the month of birth. "SNAP recipient" is a dummy that equals one if individual received SNAP benefits in at least one months during the year before onset of pregnancy. "Any homeless encounter" is dummy that equals one if individual had at least one encounter with the homelessness system (that is: shelter encounter or participation in long-term anti-homelessness program as defined in Section 1.4) in the year before onset of pregnancy. "Charged with crime" is dummy that equals one if individual was charged with a crime in an Allegheny court at least once in the year before onset of pregnancy. "Any MHD encounter" ("Any SUD encounter") is dummy that equals one if individual received treatment for any mental health disorder excluding substance use disorders (any substance use disorder) at least once in the year before onset of pregnancy, as per medicaid behavioral health records. See Section 1.2 for details on sample construction.

Table 2: Event Study Results - Housing

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8
Pregnancy effect	0.001*	0.000	0.001	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
Post-birth effect	-0.000	0.003**	0.012***	0.005*
	(0.001)	(0.001)	(0.003)	(0.003)
Year-month FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes
Mean of dep. var	0.001	0.006	0.048	0.120
Obs	446278	446278	446278	446278
N individuals	12589	12589	12589	12589
Wald-statistic pre-trend p-value	0.629	0.199	0.455	0.228

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the main analysis sample of low SES first time mothers detailed in Section 1.2. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Table 3: Event Study Results - Treatment for Substance Use Disorder

	(1) Any SUD treatment	(2) Opioid Use Dis. treatment
Pregnancy effect	0.006**	0.006***
	(0.003)	(0.002)
Post-birth effect	0.033***	0.022***
	(0.008)	(0.005)
Year-month FE	Yes	Yes
Individual FE	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes
Mean of dep. var	0.030	0.017
Obs	161074	161074
N individuals	4524	4524
Wald-statistic pre-trend p-value	0.344	0.906

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2. Observations are at the individual-month level. Estimates are based on restricted sample of low SES first-time mothers who were Medicaid-insured at least 90% of the months spanning 12 months before conception to 12 months post-birth. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Table 4: Event Study Results - Healthcare, Food, Cash Assistance

	(1) Medicaid	(2) SNAP	(3) TANF
Pregnancy effect	0.161***	0.066***	0.040***
	(0.005)	(0.004)	(0.002)
Post-birth effect	0.271***	0.171***	0.144***
	(0.013)	(0.009)	(0.005)
Year-month FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes
Mean of dep. var	0.546	0.270	0.054
Obs	446278	446278	446278
N individuals	12589	12589	12589
Wald-statistic pre-trend p-value	0.867	0.250	0.163

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the main analysis sample of low SES first time mothers detailed in Section 1.2. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Table 5: Event Study Results - Criminal Behavior

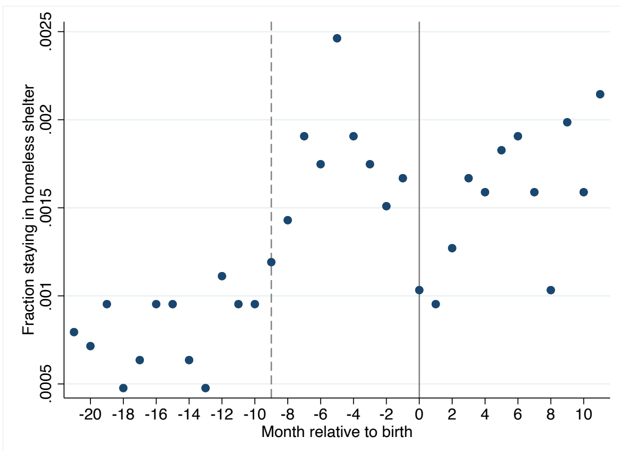
	(1) Criminal offense
Pregnancy effect	-0.006***
	(0.001)
Post-birth effect	-0.006*
	(0.004)
Year-month FE	Yes
Individual FE	Yes
Indiv.-spec. lin. time trend	Yes
Mean of dep. var	0.017
Obs	379189
N individuals	10556
Wald-statistic pre-trend p-value	0.243

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the main analysis sample of low SES first time mothers detailed in Section 1.2. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

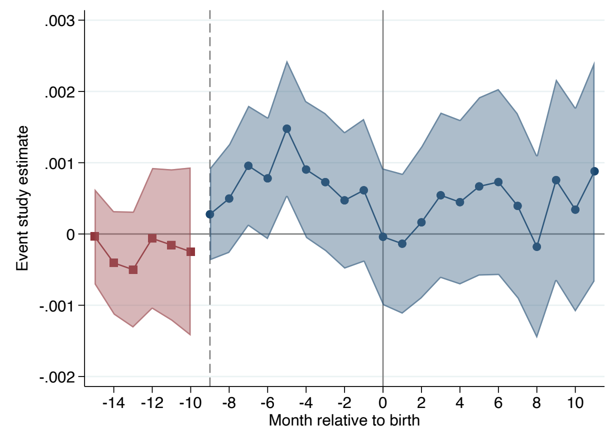
Figures

Figure 1: Homelessness: Raw Time Series and Event Studies

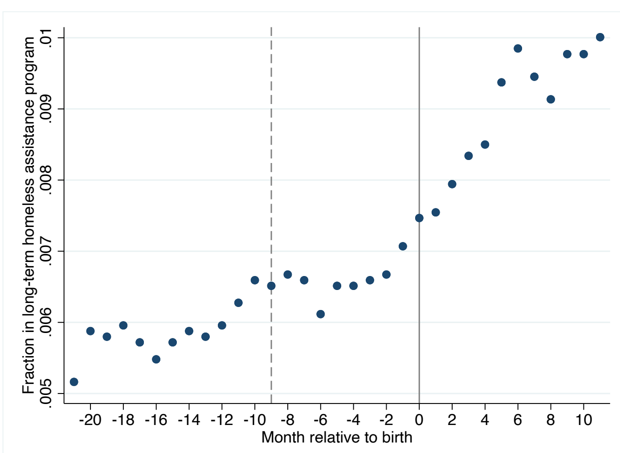
Homeless Shelter Stays: Raw Time Series



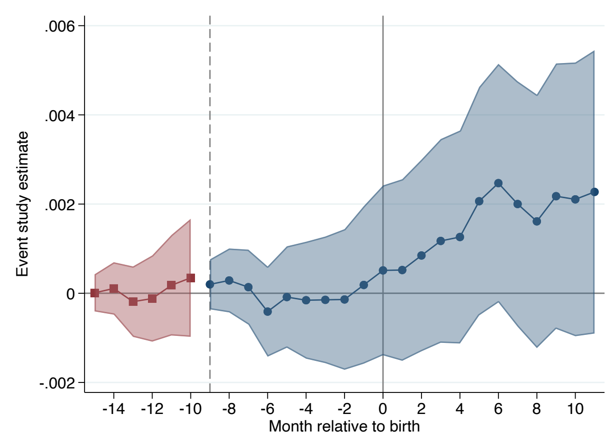
Homeless Shelter Stays: Event Study



Medium/long-term homelessness assistance: Raw Time Series



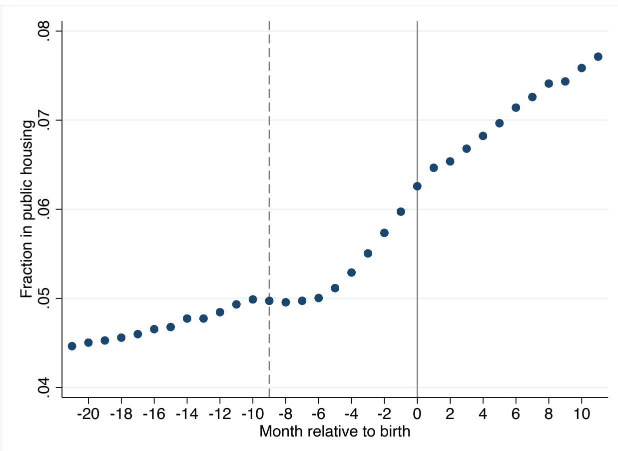
Medium/long-term homelessness assistance: Event Study



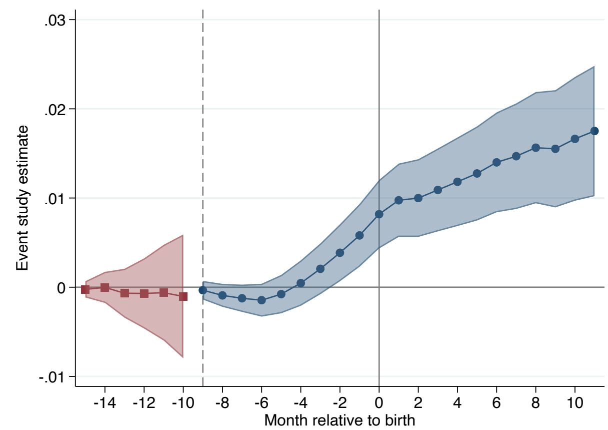
Notes: Figures show raw means of outcomes by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right), for the main analysis sample of low SES first time mothers detailed in Section 1.2. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth.

Figure 2: General Long-Term Housing Assistance: Raw Time Series and Event Studies

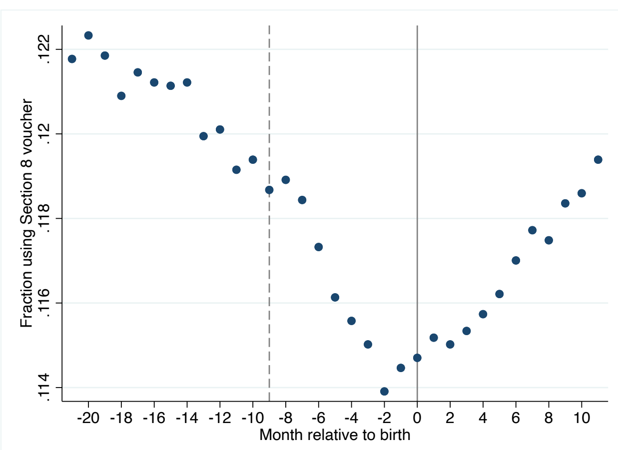
Public Housing Residence: Raw Time Series



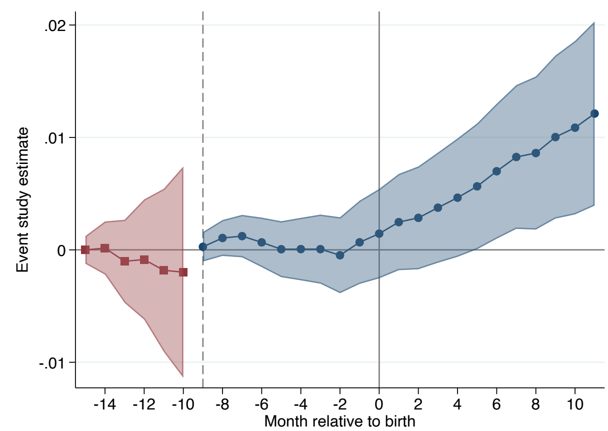
Public Housing Residence: Event Study



Section 8 Voucher Use: Raw Time Series



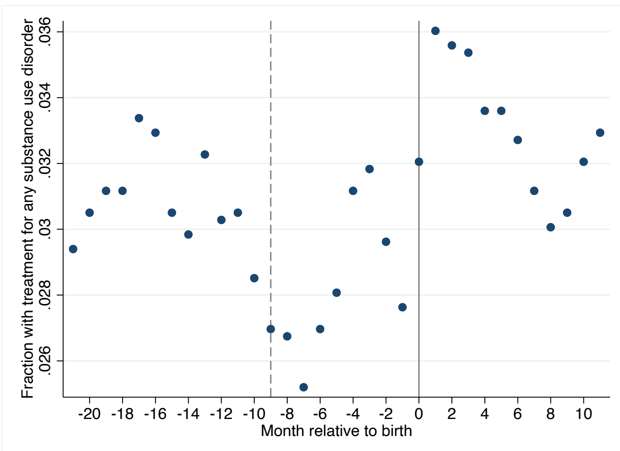
Section 8 Voucher Use: Event Study



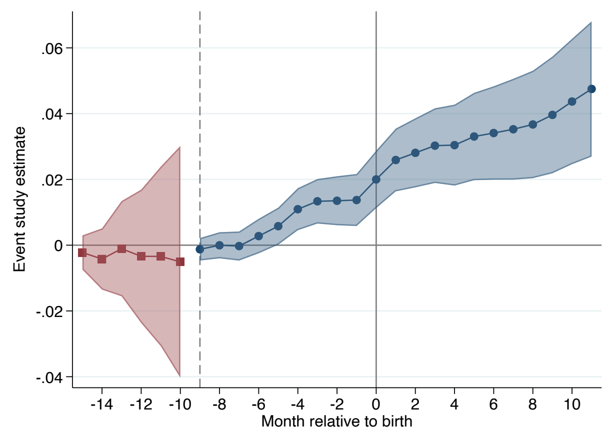
Notes: Figures show raw means of outcomes by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right), for the main analysis sample of low SES first time mothers detailed in Section 1.2. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth.

Figure 3: Substance Use Disorder: Raw Time Series and Event Studies

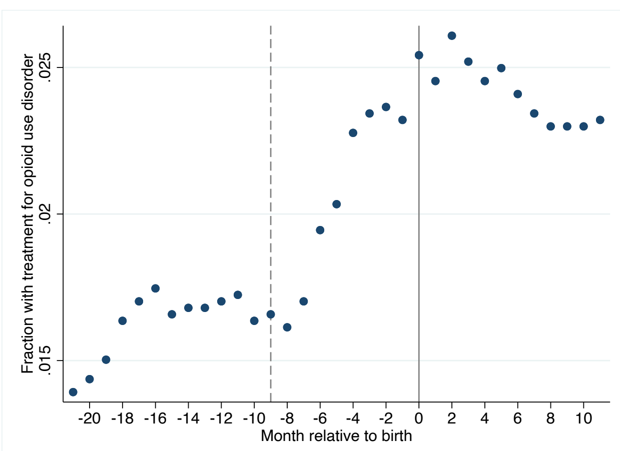
Any SUD Treatment: Raw Time Series



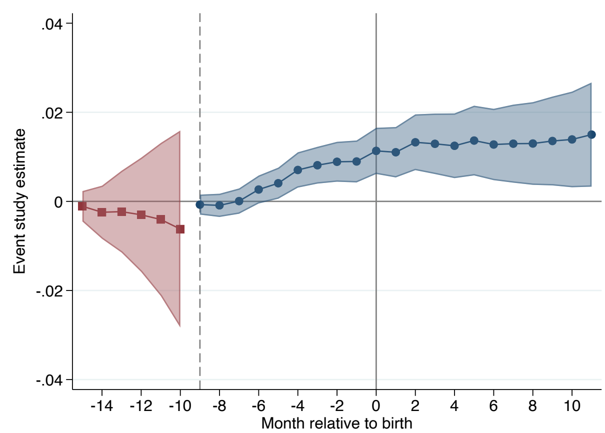
Any SUD Treatment: Event Study



Opioid Use Dis. Treatment: Raw Time Series

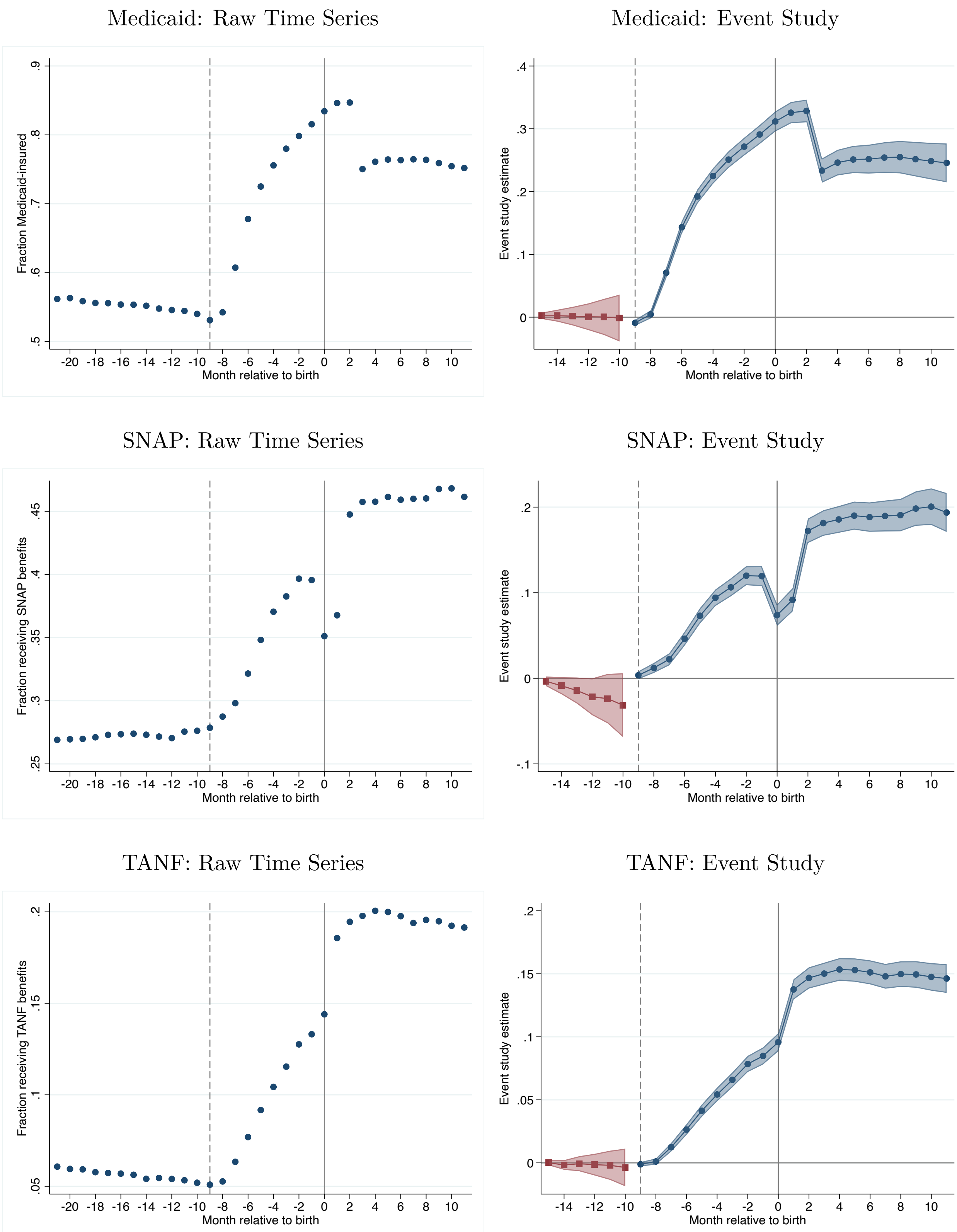


Opioid Use Dis. Treatment: Event Study



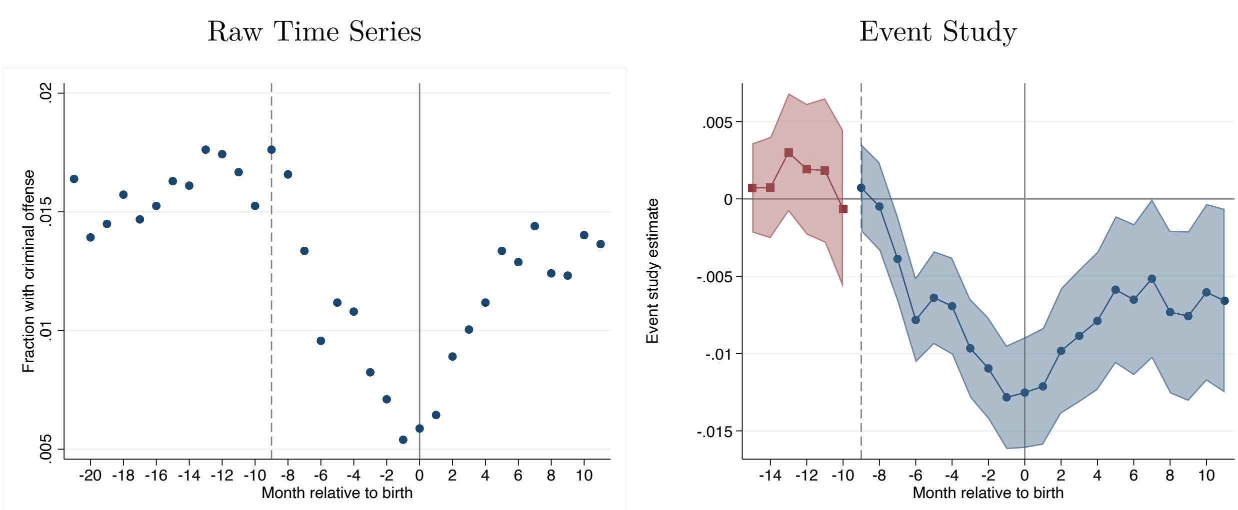
Notes: Figures show raw means of outcomes by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right). Estimates are based on restricted sample of low SES first-time mothers who were Medicaid-insured at least 90% of the months spanning 12 months before conception to 12 months post-birth. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth.

Figure 4: Government Benefit Use: Raw Time Series and Event Studies



Notes: Figures show raw means of outcomes by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right), for the main analysis sample of low SES first time mothers detailed in Section 1.2. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth.

Figure 5: Criminal Behavior: Raw Time Series and Event Studies



Notes: Figures show raw means of outcomes by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right), for the main analysis sample of low SES first time mothers detailed in Section 1.2. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth.

A. Appendix Tables and Figures

Appendix Tables

Appendix 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).

Appendix 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, marriage status, number of previous live births of mom, date of most recent non-live birth of mom.	1999-2019
Demographics	All*	Year/month of birth, gender, and race.	2005-2019
Medicaid, SNAP, TANF	All*	Month-level indicators of enrollment status for Medicaid, SNAP (household-level), TANF (household-level).	2005-2019
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 encounter, 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 addiction treatment encounters (e.g. methadone receipt), inpatient stays in psychiatric hospitals and addiction treatment centers, and other services; includes diagnosis codes for reach 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, offense type (misdemeanor, felony, and within felony: assault, theft, drug possession, DUI). Outcome of court case only 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.

Appendix 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 in order 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 age 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, 2021a).

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

Appendix Table A.4: Summary Statistics: Two Live Births Sample

	mean
Age	21.556
Age 16-17	0.118
Black	0.555
White	0.426
Dad listed on birth certificate	0.559
Low SES	1.000
Medicaid insured in year before pregnancy	0.740
SNAP recipient in year before pregnancy	0.395
Any homeless encounter in year before pregnancy	0.015
Charged with crime in year before pregnancy	0.098
Any MHD encounter in year before pregnancy	0.126
Any SUD encounter in year before pregnancy	0.048
Months between births	44.524
Observations	6967

Notes: Table shows summary statistics for women who have at least two live births in the sample period, and who are identified as low SES ahead of their first live birth. All time varying variables are reported as of at the first live birth event/first pregnancy.

Appendix Table A.5: Event Study Results - Secondary Housing Outcomes

	(1) Public Housing (Head)	(2) Sec. 8 (Head)
Pregnancy effect	0.001*	-0.001
	(0.001)	(0.001)
Post-birth effect	0.015***	0.002
	(0.002)	(0.002)
Year-month FE	Yes	Yes
Individual FE	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes
Meatrend	0.010	0.018
Mean of dep. var	469580	469580
Obs	12589	12589
N individuals	0.337	0.390

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the main analysis sample of low SES first time mothers detailed in Section 1.2. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.6: Event Study Results - Secondary Substance Use Disorder Outcomes

	(1) Opioid Use Dis. Medication	(2) Opioid Use Dis. Rehab	(3) Cannabis Use Dis. treatment	(4) Alcohol Use Dis. treatment	(5) Cocaine Use Dis. treatment
Pregnancy effect	0.004***	-0.005***	-0.001	-0.001**	-0.001*
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Post-birth effect	0.006**	-0.009**	0.002	-0.002	-0.001
	(0.003)	(0.004)	(0.005)	(0.002)	(0.002)
Year-month FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.012	0.006	0.007	0.002	0.001
Obs	169770	169770	169770	169770	169770
N individuals	4524	4524	4524	4524	4524
Wald-statistic pre-trend p-value	0.820	0.980	0.489	0.686	0.473

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2. Estimates are based on restricted sample of low SES first-time mothers who were Medicaid-insured at least 90% of the months spanning 12 months before conception to 12 months post-birth. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.7: Event Study Results - Secondary Criminal Behavior Outcomes

	(1) Felony	(2) Misde-meanor	(3) Felony: Assault	(4) Felony: Theft	(5) Felony: Drug poss.	(6) Felony: DUI	(7) Felony: Other
Pregnancy effect	-0.003***	-0.004***	-0.001	-0.001**	-0.001**	-0.000	-0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Post-birth effect	-0.004*	-0.009**	-0.001	-0.002*	-0.003**	0.001	0.000
	(0.002)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.011	0.011	0.002	0.002	0.003	0.001	0.003
Obs	402491	293228	402491	402491	402491	402491	402491
N individuals	10556	7245	10556	10556	10556	10556	10556
Wald-statistic pre-trend p-value	0.248	0.419	0.361	0.138	0.332	0.856	0.816

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the main analysis sample of low SES first time mothers detailed in Section 1.2. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

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

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.0002**	-0.0000	0.0004**	-0.0002	0.0035	0.0069***	0.0754***	0.0168***	0.0081***	-0.0018***
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0026)	(0.0016)	(0.0010)	(0.0007)	(0.0003)	(0.0003)
Post-birth effect	0.0000	0.0004	0.0035***	0.0008	0.0220***	0.0250***	0.1300***	0.0489***	0.0303***	-0.0026***
	(0.0002)	(0.0003)	(0.0006)	(0.0006)	(0.0082)	(0.0047)	(0.0023)	(0.0017)	(0.0008)	(0.0007)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.0002	0.0010	0.0092	0.0223	0.0303	0.0170	0.0851	0.0488	0.0097	0.0041
Obs	2856395	2856395	2856395	2856395	161074	161074	2856395	2856395	2856395	2349746
N individuals	80713	80713	80713	80713	4524	4524	80713	80713	80713	65360
Wald-statistic pre-trend p-value	0.811	0.255	0.360	0.264	0.344	0.906	0.848	0.360	0.326	0.243

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the full sample of all first live births to women (i.e. without restriction to low SES individuals). Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.9: Event Study Results with SNAP and Medicaid Low SES Criterion

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.0007**	-0.0000	0.0006	-0.0012	0.0057**	0.0045***	0.1695***	0.0640***	0.0400***	-0.0066***
	(0.0003)	(0.0005)	(0.0010)	(0.0011)	(0.0026)	(0.0016)	(0.0047)	(0.0038)	(0.0019)	(0.0012)
Post-birth effect	0.0003	0.0025*	0.0115***	0.0018	0.0312***	0.0146***	0.2834***	0.1621***	0.1433***	-0.0070**
	(0.0008)	(0.0014)	(0.0028)	(0.0031)	(0.0082)	(0.0047)	(0.0117)	(0.0086)	(0.0044)	(0.0035)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.0010	0.0058	0.0478	0.1193	0.0303	0.0170	0.5011	0.2871	0.0562	0.0173
Obs	486153	486153	486153	486153	161074	161074	486153	486153	486153	412827
N individuals	13709	13709	13709	13709	4524	4524	13709	13709	13709	11487
Wald-statistic pre-trend p-value	0.778	0.259	0.455	0.242	0.344	0.906	0.876	0.286	0.296	0.378

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sample of low SES individuals who been enrolled in Medicaid or SNAP at any point in the five years leading up to conception. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.10: Event Study Results with SNAP Low SES Criterion

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.0012**	-0.0008	-0.0004	-0.0030*	0.0051	0.0069***	0.1555***	0.0420***	0.0525***	-0.0080***
	(0.0005)	(0.0009)	(0.0015)	(0.0018)	(0.0034)	(0.0020)	(0.0061)	(0.0066)	(0.0033)	(0.0019)
Post-birth effect	0.0015	0.0014	0.0070*	-0.0013	0.0286***	0.0302***	0.2308***	0.0901***	0.1829***	-0.0061
	(0.0011)	(0.0025)	(0.0040)	(0.0045)	(0.0088)	(0.0051)	(0.0138)	(0.0145)	(0.0072)	(0.0047)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.0015	0.0095	0.0642	0.1644	0.0370	0.0221	0.5449	0.5353	0.0996	0.0217
Obs	264260	264260	264260	264260	104435	104435	264260	264260	264260	235616
N individuals	7353	7353	7353	7353	2895	2895	7353	7353	7353	6485
Wald-statistic pre-trend p-value	0.939	0.335	0.840	0.472	0.471	0.725	0.291	0.168	0.274	0.197

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sample of low SES individuals who have been enrolled in SNAP at any point in the five years leading up to conception. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.11: Event Study Results Allowing for Anticipation Effects

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.0017***	0.0006	0.0006	0.0004	-0.0005	0.0070***	0.1460***	0.0628***	0.0360***	-0.0136***
	(0.0005)	(0.0008)	(0.0015)	(0.0020)	(0.0045)	(0.0026)	(0.0074)	(0.0056)	(0.0026)	(0.0024)
Post-birth effect	0.0033***	0.0026	0.0109***	0.0066	0.0056	0.0189***	0.2248***	0.1549***	0.1291***	-0.0326***
	(0.0010)	(0.0017)	(0.0038)	(0.0048)	(0.0111)	(0.0069)	(0.0170)	(0.0117)	(0.0057)	(0.0061)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.0010	0.0057	0.0468	0.1211	0.0305	0.0166	0.5534	0.2740	0.0563	0.0163
Obs	401534	401534	401534	401534	144869	144869	401534	401534	401534	340544
N individuals	12589	12589	12589	12589	4524	4524	12589	12589	12589	10556
Wald-statistic pre-trend p-value	0.612	0.372	0.588	0.183	0.357	0.938	0.269	0.715	0.279	0.651

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, but omitting the three months immediately preceding conception, for our baseline analysis sample of low-SES first-time mothers detailed in Section 1.2. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 15 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.12: Event Study Results without Linear Pre-Trend Control

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.0008***	-0.0001	0.0012	-0.0005	-0.0028*	0.0013	0.1593***	0.0637***	0.0427***	-0.0066***
	(0.0002)	(0.0004)	(0.0008)	(0.0009)	(0.0017)	(0.0011)	(0.0036)	(0.0028)	(0.0016)	(0.0009)
Post-birth effect	0.0006	0.0015	0.0149***	0.0042**	0.0005	0.0024	0.2667***	0.1576***	0.1523***	-0.0081***
	(0.0004)	(0.0010)	(0.0021)	(0.0021)	(0.0040)	(0.0025)	(0.0079)	(0.0063)	(0.0038)	(0.0019)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	No	No	No	No	No	No	No	No	No	No
Mean of dep. var	0.0011	0.0060	0.0485	0.1201	0.0303	0.0170	0.5457	0.2705	0.0540	0.0174
Obs	446278	446278	446278	446278	161074	161074	446278	446278	446278	379189
N individuals	12589	12589	12589	12589	4524	4524	12589	12589	12589	10556
Wald-statistic pre-trend p-value	0.608	0.380	0.357	0.208	0.257	0.615	0.892	0.391	0.129	0.194

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2 but omitting the individual-specific linear pre-trend control, for our baseline analysis sample of low-SES first-time mothers detailed in Section 1.2. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.13: Event Study Results with Standard Two-Way Fixed Effects Estimator

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.0006**	-0.0006	-0.0039***	-0.0034***	-0.0017	0.0022*	0.1111***	0.0030	0.0091***	-0.0058***
	(0.0003)	(0.0005)	(0.0009)	(0.0010)	(0.0017)	(0.0012)	(0.0033)	(0.0029)	(0.0019)	(0.0009)
Post-birth effect	-0.0001	0.0003	0.0030**	-0.0022	0.0037	0.0043**	0.1700***	0.0317***	0.0828***	-0.0066***
	(0.0004)	(0.0007)	(0.0014)	(0.0014)	(0.0027)	(0.0018)	(0.0046)	(0.0046)	(0.0033)	(0.0013)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	No	No	No	No	No	No	No	No	No	No
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.0011	0.0060	0.0485	0.1201	0.0303	0.0170	0.5457	0.2705	0.0540	0.0174
Obs	415437	415437	415437	415437	149292	149292	415437	415437	415437	348348
N individuals	12589	12589	12589	12589	4524	4524	12589	12589	12589	10556

Notes: Table shows treatment effect estimates obtained from a standard two-way fixed effect estimator obtained from the following OLS model: $Y_{it} = \beta_0 + \beta_1 \times Preg_{it} + \beta_2 \times Post_{it} + \mu_i + \gamma_{y(it)} + \delta\mu_i \times r_{it} + \epsilon_{it}$, where i denotes individual and t denotes calendar year-month. The regression includes controls for individual fixed effects (μ_i), calendar year fixed effects ($\gamma_{y(it)}$), and an individual-specific linear control in event time ($\mu_i \times r_{it}$),. It is estimated off of the "live birth event study" sample detailed in Section 1.2. "Pregnancy effect" ("Post-birth effect") is the coefficient on a dummy that equals one in months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable twelve months before childbirth. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.14: Matched DiD Results - Low SES Sample

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy (5th month)	0.0008	0.0011	0.0004	0.0002	0.0007	0.0022	0.2118***	0.0834***	0.0389***	-0.0076***
	(0.0007)	(0.0008)	(0.0015)	(0.0018)	(0.0020)	(0.0015)	(0.0065)	(0.0060)	(0.0032)	(0.0024)
Post-birth (3rd month)	0.0001	0.0015	0.0139***	0.0062**	0.0074***	0.0074***	0.2796***	0.1956***	0.1484***	-0.0082***
	(0.0007)	(0.0011)	(0.0024)	(0.0026)	(0.0022)	(0.0017)	(0.0077)	(0.0073)	(0.0046)	(0.0024)
Mean of dep. var	0.0008	0.0064	0.0476	0.1259	0.0158	0.0090	0.5536	0.2580	0.0555	0.0151
Obs	379890	379890	379890	379890	379890	379890	379890	379890	379890	367476
N treated individuals	9045	9045	9045	9045	9045	9045	9045	9045	9045	8396
N control individuals	9045	9045	9045	9045	9045	9045	9045	9045	9045	8396

Notes: Table reports treatment effect estimates on interaction coefficients of treatment and relative event time dummies at -4 and 3 relative to month of childbirth obtained from a matched DiD regression detailed in Appendix C. Regression includes controls for treatment, relative event time dummies, and their interaction. Sample is restricted to women with at least one month of Medicaid enrollment in their Medicaid history (that is, in five years before their (placebo) conception year). "Mean of dep. var" gives the mean of the dependent variable 12 months before childbirth. 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 *** (**)[*].

Appendix Table A.15: Matched DiD Results - All First-Time Mothers

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy (5th month)	0.0002*	0.0001	0.0001	-0.0001	0.0008**	0.0010***	0.0881***	0.0198***	0.0076***	-0.0018***
	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0014)	(0.0010)	(0.0005)	(0.0004)
Post-birth (3rd month)	0.0001	0.0003*	0.0029***	0.0009**	0.0026***	0.0026***	0.1214***	0.0520***	0.0296***	-0.0018***
	(0.0001)	(0.0002)	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0016)	(0.0013)	(0.0008)	(0.0004)
Mean of dep. var	0.0001	0.0010	0.0083	0.0212	0.0025	0.0014	0.0802	0.0425	0.0088	0.0033
Obs	2677962	2677962	2677962	2677962	2677962	2677962	2677962	2677962	2677962	2580986
N treated individuals	63761	63761	63761	63761	63761	63761	63761	63761	63761	58783
N control individuals	63761	63761	63761	63761	63761	63761	63761	63761	63761	58783

Notes: Table reports treatment effect estimates on interaction coefficients of treatment and relative event time dummies at -4 and 3 relative to month of childbirth obtained from a matched DiD regression detailed in Appendix C. Regression includes controls for treatment, relative event time dummies, and their interaction. "Mean of dep. var" gives the mean of the dependent variable 12 months before childbirth. 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 *** (**)[*].

Appendix Table A.16: Summary Statistics for Miscarriage vs. Life Birth DiD Sample

	Live birth mean	Miscarriage mean
Age	21.607	21.128
Age 16-17	0.079	0.130
Black	0.348	0.336
White	0.620	0.629
Low SES	0.389	0.418
Medicaid insured in year before pregnancy	0.285	0.336
SNAP recipient in year before pregnancy	0.173	0.196
Any homeless encounter in year before pregnancy	0.007	0.010
Charged with crime in year before pregnancy	0.061	0.106
Any MHD encounter in year before pregnancy	0.051	0.073
Any SUD encounter in year before pregnancy	0.018	0.019
(Also) has miscarriage	0.010	1.000
(Also) has live birth	1.000	0.257
Months between events	38.889	38.889
Observations	25801	981

Notes: Table shows summary statistics for women in the sample for the difference-in-difference analysis comparing miscarriage events to live birth events as detailed in Appendix D. Observations are at the individual-event level (note that an individual can enter both in 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 10 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.

Appendix Table A.17: Live Birth vs. Miscarriage DiD Results

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy × Live birth	0.001***	-0.000	0.002	0.000	-0.009	0.001	0.102***	0.031***	0.015***	-0.004
	(0.000)	(0.001)	(0.002)	(0.002)	(0.007)	(0.003)	(0.008)	(0.007)	(0.003)	(0.003)
Post-Pregn. × Live Birth	0.000	0.002	0.014***	0.006	-0.003	0.007***	0.175***	0.100***	0.076***	-0.001
	(0.000)	(0.002)	(0.002)	(0.004)	(0.008)	(0.002)	(0.011)	(0.008)	(0.004)	(0.002)
Pregnancy	-0.000	0.000	-0.002	-0.003	0.005	0.002	0.016**	-0.004	-0.003	0.001
	(0.000)	(0.001)	(0.001)	(0.002)	(0.007)	(0.003)	(0.008)	(0.007)	(0.003)	(0.003)
Post-Pregnancy	-0.000	-0.001	-0.005**	-0.007*	0.004	-0.002	0.047***	-0.017**	-0.015***	-0.003
	(0.000)	(0.002)	(0.002)	(0.004)	(0.007)	(0.002)	(0.011)	(0.008)	(0.004)	(0.002)
Individual-Event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.000	0.003	0.025	0.059	0.020	0.008	0.221	0.124	0.029	0.010
Obs	877920	877920	877920	877920	132402	132402	877920	877920	877920	679275
N indiv.-event tuples	26782	26782	26782	26782	4040	4040	26782	26782	26782	20735

Notes: Table shows treatment effect estimates obtained from OLS estimation of difference-in-difference model detailed in Section 4.2. 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 D. "Mean of dep. var" gives the mean of the dependent variable two months before the approximate month of conception. 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 *** (**)[*].

Appendix Table A.18: Demographic Characteristics of First-Time Fathers

	(1) Low SES First Time Fathers mean	(2) All Other First Time Fathers mean
Age	23.663	30.669
Age 16-17	0.050	0.003
Black	0.491	0.075
White	0.470	0.850
SNAP recipient in year before pregnancy	0.313	0.007
Any homeless encounter in year before pregnancy	0.012	0.000
Charged with crime in year before pregnancy	0.202	0.017
Any MHD encounter in year before pregnancy	0.083	0.001
Any SUD encounter in year before pregnancy	0.073	0.001
Observations	4844	57018

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: 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 sampel 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.

Appendix Table A.19: Event Study Results for Low SES Men

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	-0.0001	-0.0001	-0.0013	-0.0003	0.0003	-0.0015	-0.0385***	-0.0154***	-0.0023	0.0030
	(0.0004)	(0.0007)	(0.0013)	(0.0016)	(0.0075)	(0.0044)	(0.0068)	(0.0055)	(0.0021)	(0.0030)
Post-birth effect	0.0006	-0.0012	-0.0052	0.0038	0.0100	-0.0032	-0.0792***	-0.0793***	-0.0061	0.0141
	(0.0011)	(0.0018)	(0.0038)	(0.0044)	(0.0224)	(0.0136)	(0.0201)	(0.0139)	(0.0053)	(0.0096)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.0008	0.0043	0.0297	0.0869	0.0500	0.0226	0.4292	0.2071	0.0283	0.0312
Obs	172912	172912	172912	172912	31216	31216	172912	172912	172912	148624
N individuals	4844	4844	4844	4844	840	840	4844	4844	4844	4108
Wald-statistic pre-trend p-value	0.263	0.260	0.607	0.140	0.305	0.114	0.077	0.796	0.238	0.465

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for low SES first-time fathers. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

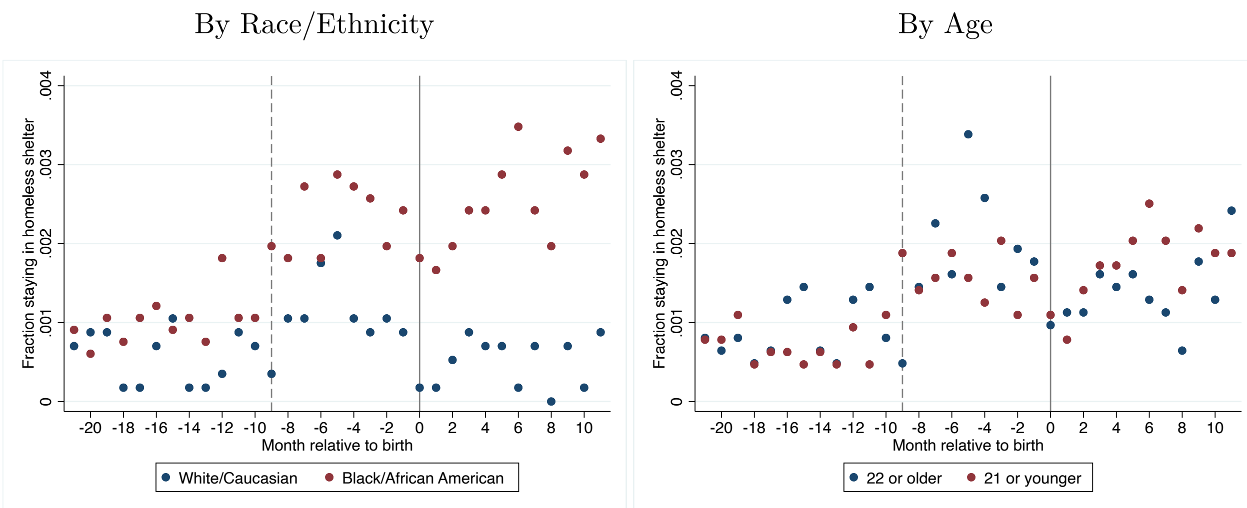
Appendix Table A.20: Event Study Results for All Men

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid Use Dis. treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.0000	0.0001	-0.0001	-0.0000	0.0003	-0.0025	0.0013**	0.0002	0.0001	-0.0003
	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0075)	(0.0044)	(0.0006)	(0.0005)	(0.0002)	(0.0003)
Post-birth effect	0.0001	0.0002	-0.0002	0.0002	0.0102	-0.0077	0.0107***	0.0011	0.0009*	0.0006
	(0.0001)	(0.0002)	(0.0004)	(0.0004)	(0.0224)	(0.0136)	(0.0018)	(0.0013)	(0.0005)	(0.0009)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Indiv.-spec. lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.0001	0.0004	0.0030	0.0083	0.0500	0.0226	0.0336	0.0202	0.0025	0.0051
Obs	2195181	2195181	2195181	2195181	31216	31216	2195181	2195181	2195181	1808454
N individuals	61862	61862	61862	61862	840	840	61862	61862	61862	50143
Wald-statistic pre-trend p-value	0.406	0.348	0.753	0.204	0.305	0.114	0.092	0.799	0.430	0.226

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for all first-time fathers regardless of SES. Observations are at the individual-month level. "Pregnancy effect" ("Post-birth effect") is the average treatment effect across months -9 to -1 (0 to 11) relative to month of childbirth. "Mean of dep. var" gives the mean of the dependent variable 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 with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Figures

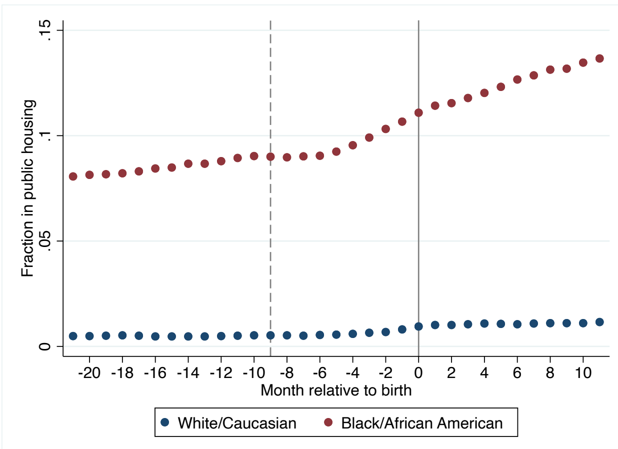
Appendix Figure A.1: Heterogeneity in Homeless Shelter Stays by Race/Ethnicity and Age



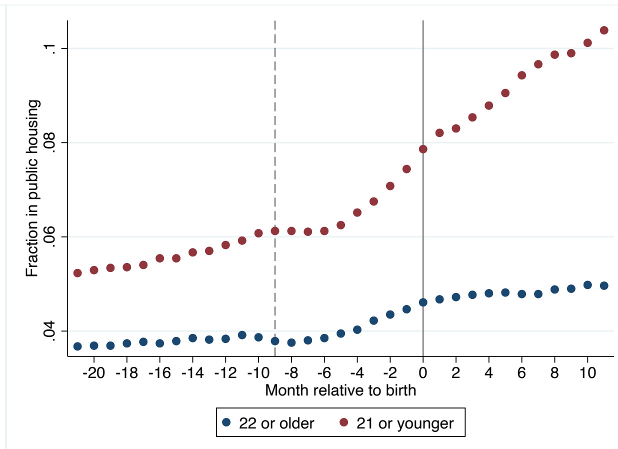
Notes: Figures show raw means of outcomes by month relative to first live birth event, separately by age/ethnicity and by above/below median age at childbirth (median is 22 years). Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth. Sample is restricted to first life birth event to mothers identified as low SES, as detailed in Section 1.2.

Appendix Figure A.2: Heterogeneity in Long-term housing Program Enrollment by Race/Ethnicity and Age

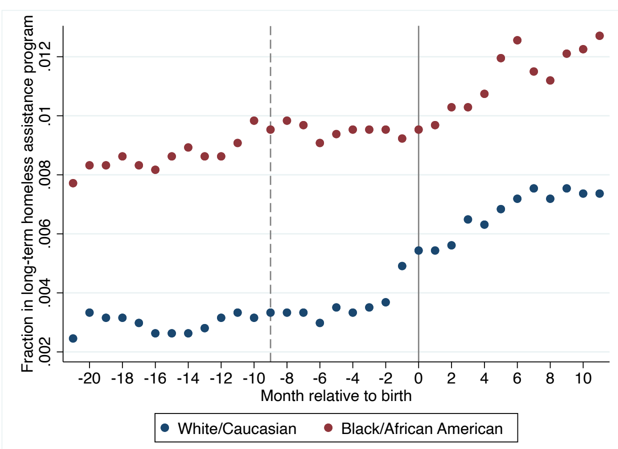
Public Housing: By race/ethnicity



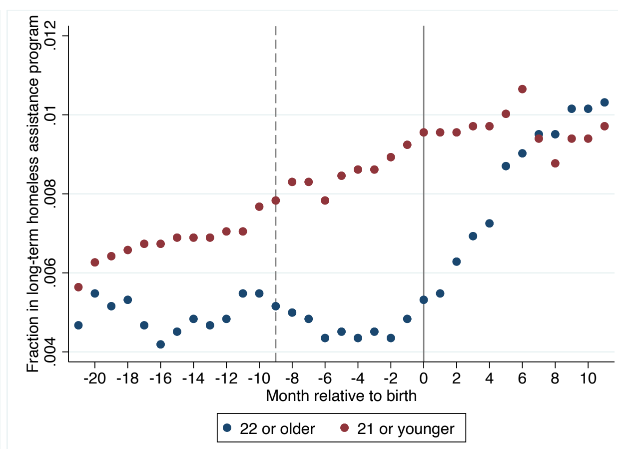
Public Housing: By Age



Medium/Long-Term Homelessness Assistance: By Race/Ethnicity

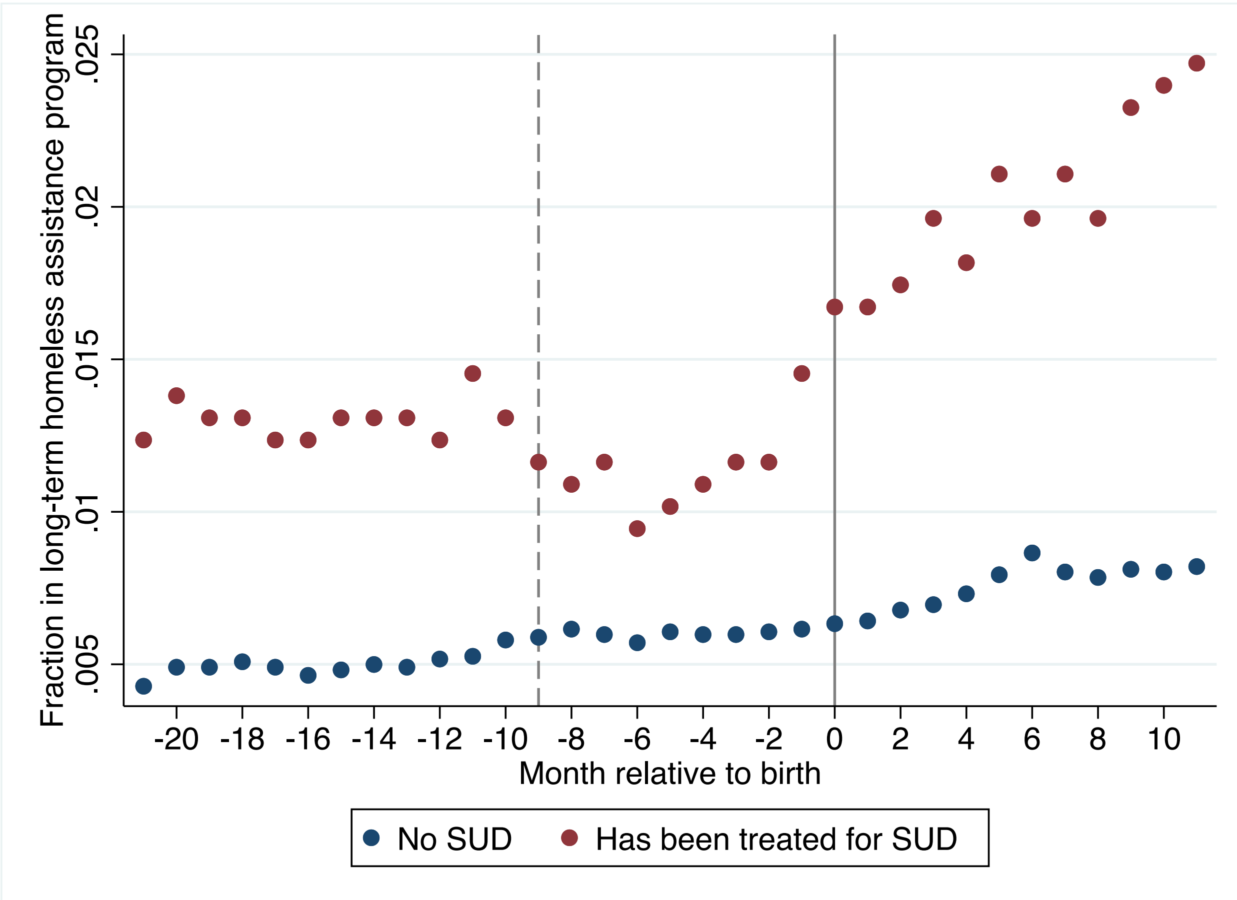


Medium/Long-Term Homelessness Assistance: By Age



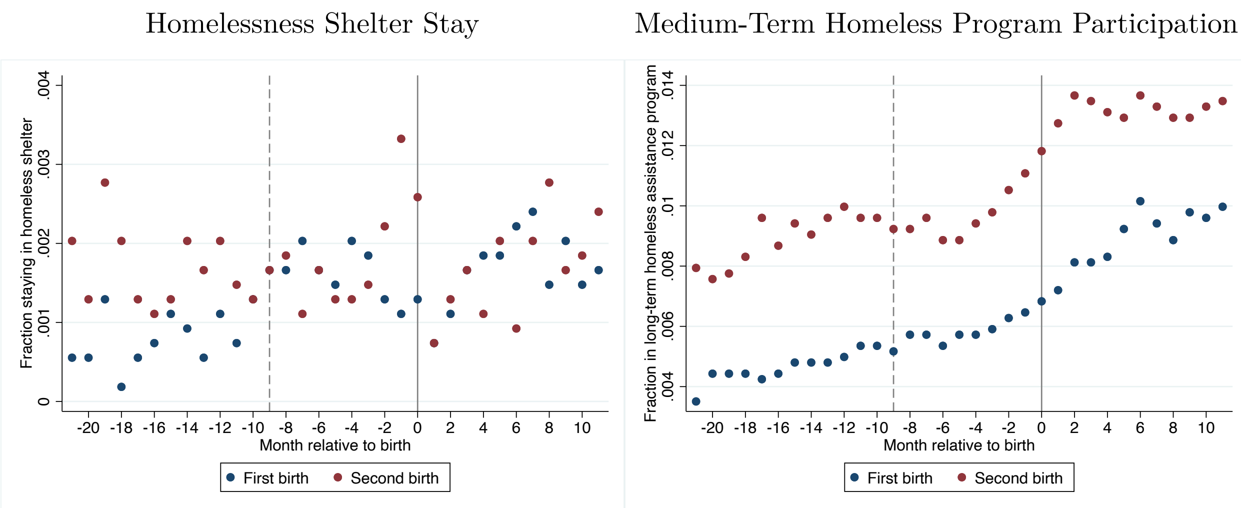
Notes: Figures show raw means of outcomes by month relative to first live birth event, separately by age/ethnicity and by above/below median age at childbirth (median is 22 years). Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth. Sample is restricted to first life birth event to mothers identified as low SES, as detailed in Section 1.2.

Appendix Figure A.3: Medium/Long-Term Homelessness Assistance: Heterogeneity by Substance Use Disorder



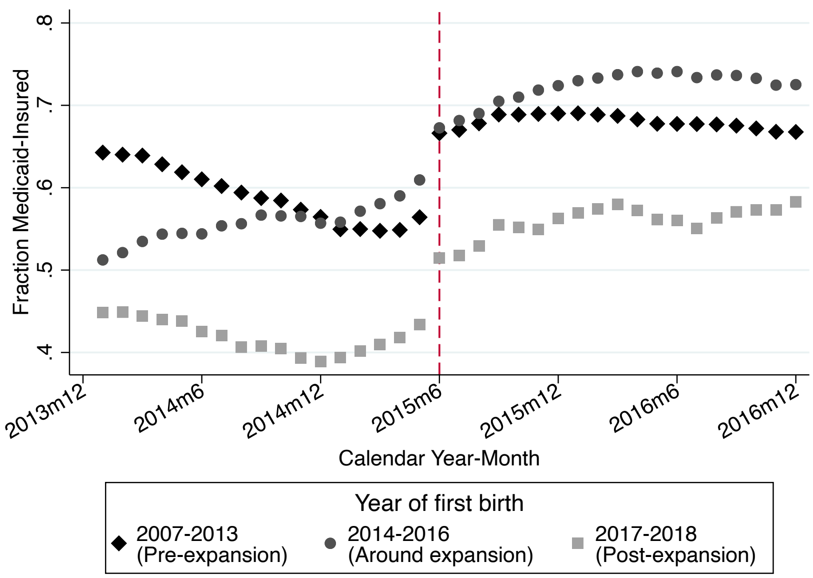
Notes: Figure shows raw means of outcomes by month relative to first live birth event. "No SUD" ("Has been treated for SUD") refers to sample of women with no (at least one encounter for) treatment for substance use disorder observed at any point before approximate commencement of the pregnancy. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth. Sample is restricted to first life birth event to mothers identified as low SES, as detailed in Section 1.2.

Appendix Figure A.4: Homelessness: First vs. Second Live Birth



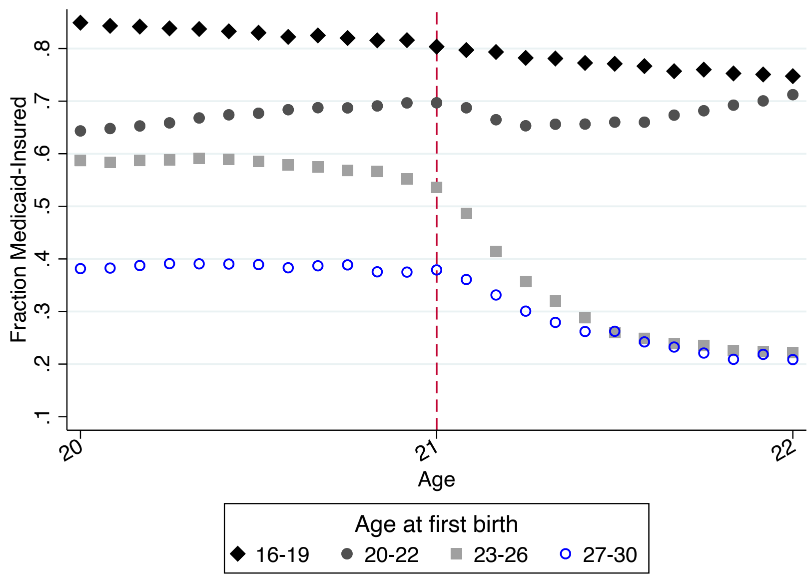
Notes: Figures show raw means of outcomes by month relative to first and second live birth event. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth. Sample is restricted to women who have at least two live births *and* who are in the main analysis sample (that is, who are identified as low SES ahead of their first pregnancy) as detailed in Section 1.2.

Appendix Figure A.5: Impact of Medicaid-Expansion on Medicaid Insurance Enrollment



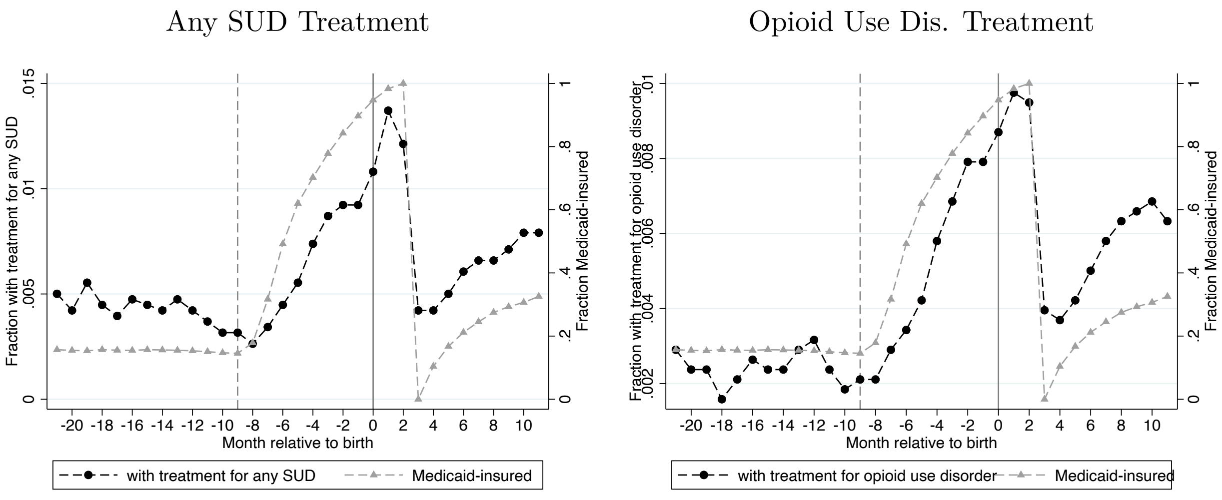
Notes: Figure shows time series of the fraction of women who are Medicaid insured in the years around the ACA-expansion. Separately for 3 sub-samples: those who had their first child pre-expansion, those who had it in the years surrounding the expansion, and those who had it post-expansion. The dashed red line denotes the date the expansion went into effect (June 2015). Sample is restricted to those who are in the main analysis sample—that is, low SES first time mothers—as detailed in Section 1.2. Time series are shown separately for three sub-samples because eligibility criteria changed differentially depending on family status (See Table A.3 for eligibility thresholds).

Appendix Figure A.6: Impact of "Aging Out" on Medicaid Insurance Enrollment



Notes: Figure shows fraction of women who are Medicaid insured by age in years and months, in the years around the 21st birthday (which marks the age-out date for the more generous child income threshold for Medicaid in Pennsylvania). Separately for 3 sub-samples: those who had their first child pre-aging out, those who had it in the years surrounding the age-out date, and those who had it post-aging out. The dashed red line denotes the month of turning 21 years old. Sample is restricted to those age 16-30 at first birth who are in the main analysis sample—that is, low SES first time mothers—as detailed in Section 1.2. Time series are shown separately for three sub-samples because eligibility criteria for Medicaid vary by family status (See Table A.3 for eligibility thresholds).

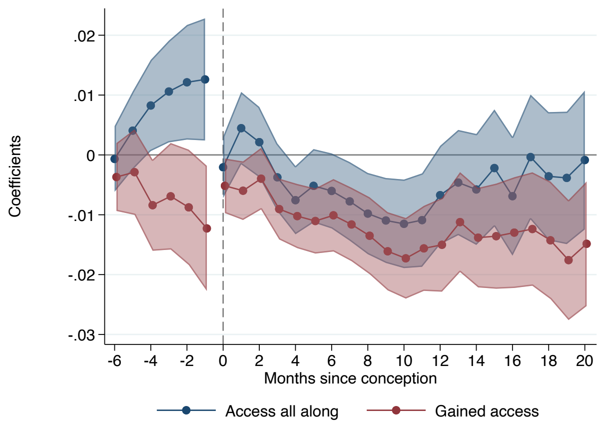
Appendix Figure A.7: Substance Use Disorder Treatment and Loss of Medicaid at 60 Days Postpartum



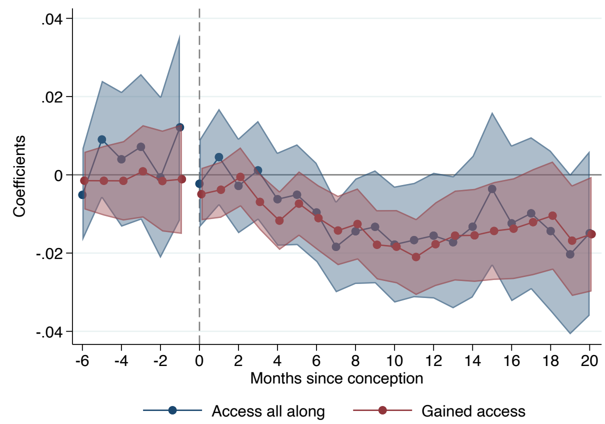
Notes: Figures show raw means of outcomes by month relative to childbirth for the sub-sample of women who lose Medicaid-coverage at three months postpartum, when stricter income eligibility rules come into effect. Sample size is 3,793 individuals, 12.6% of whom are in our low SES sample. Dark dots represent fraction receiving any SUD treatment (left panel) and opioid use disorder treatment (right panel), respectively. Light triangles represent fraction Medicaid-insured. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth.

Appendix Figure A.8: Heterogeneity in Impact of Parenthood on criminal behavior

(A) Heterogeneity by Medicaid Access



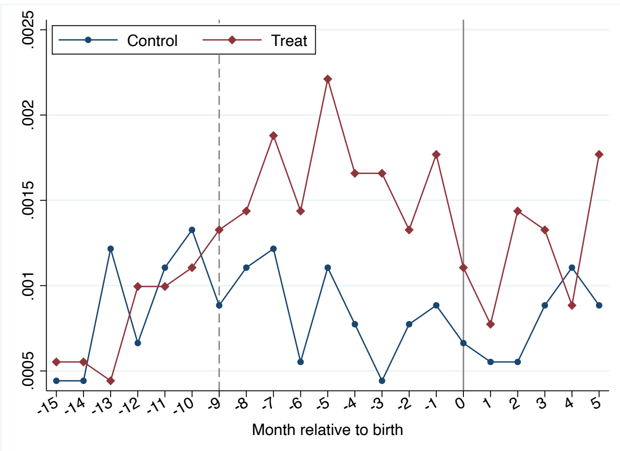
(B) Heterogeneity by SNAP Access



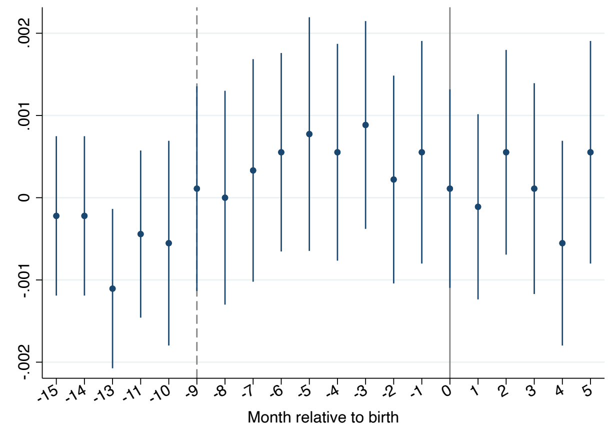
Notes: Figures show event study estimates from the "imputation estimator" described in Section 2, estimated separately on the following subsamples: women who were continuously enrolled in Medicaid (left panel) or SNAP (right panel) in the year pre-pregnancy and the year post-birth ("Access all along"), and women who were never enrolled in the respective program in the year pre-pregnancy but always enrolled in the year post-birth ("Gained acess"). Vertical dotted line shows approximate time of conception. Vertical solid line shows time of birth. Sample is restricted to first life birth event to mothers identified as low SES, as detailed in Section 1.2.

Appendix Figure A.9: Homelessness - Matched DiD Results

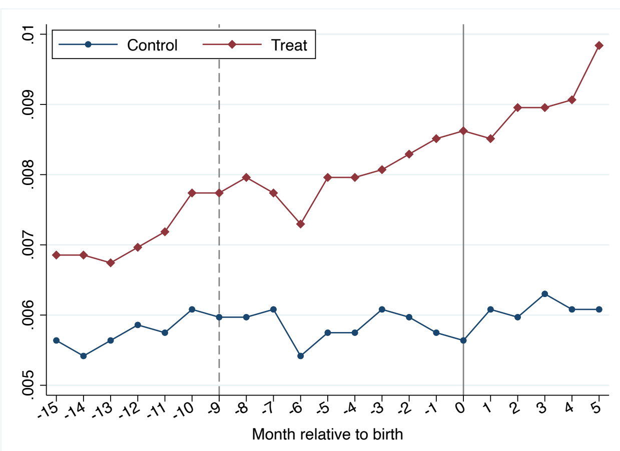
Homeless Shelter Stays: Raw Time Series



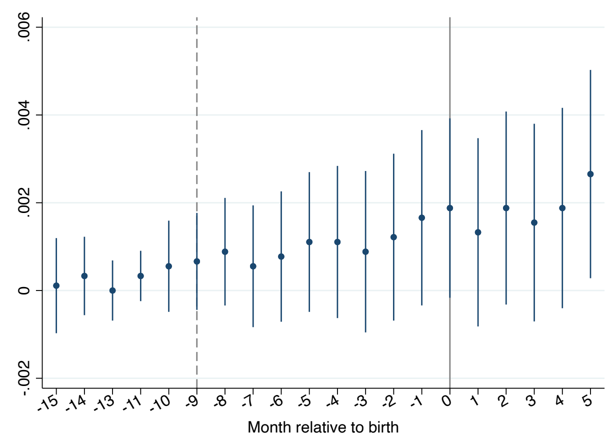
Homeless Shelter Stays: DiD Results



Medium/long-term homelessness assistance: Raw Time Series



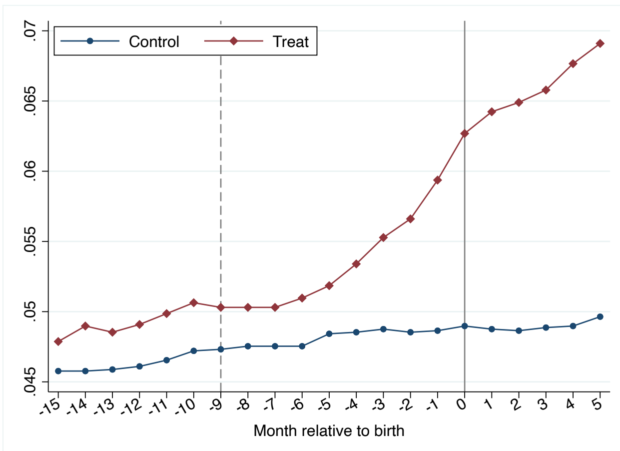
Medium/long-term homelessness assistance: DiD Results



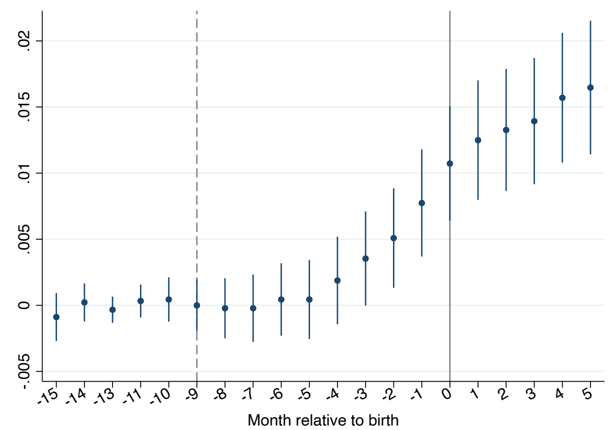
Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. Right figures report treatment effect estimates on interaction coefficients of treatment and relative event time dummies. Regression includes controls for treatment, relative event time dummies, and their interaction. Month −12 relative to childbirth is the omitted category. Sample is restricted to women with at least one month of Medicaid enrollment in their Medicaid history (that is, in five years before their (placebo) conception year). Vertical dotted line shows approximate month of conception of treated individuals. Vertical solid line shows month of childbirth of treated individuals. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown.

Appendix Figure A.10: General Long-Term Housing Assistance - Matched DiD Results

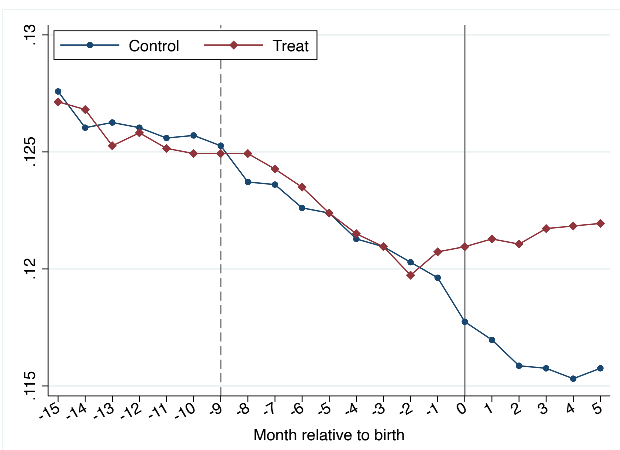
Public Housing Residence: Raw Time Series



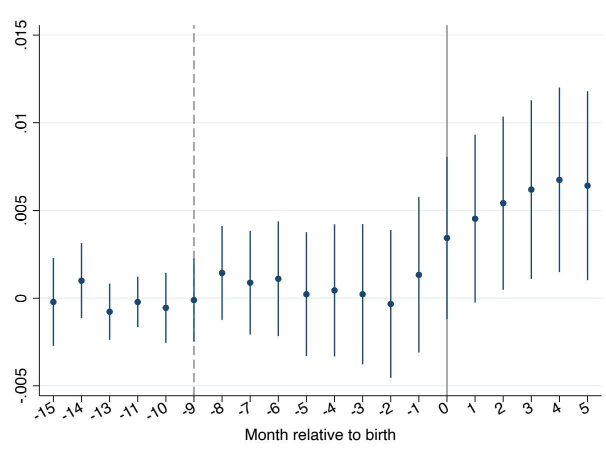
Public Housing Residence: DiD Results



Section 8 Voucher Use: Raw Time Series

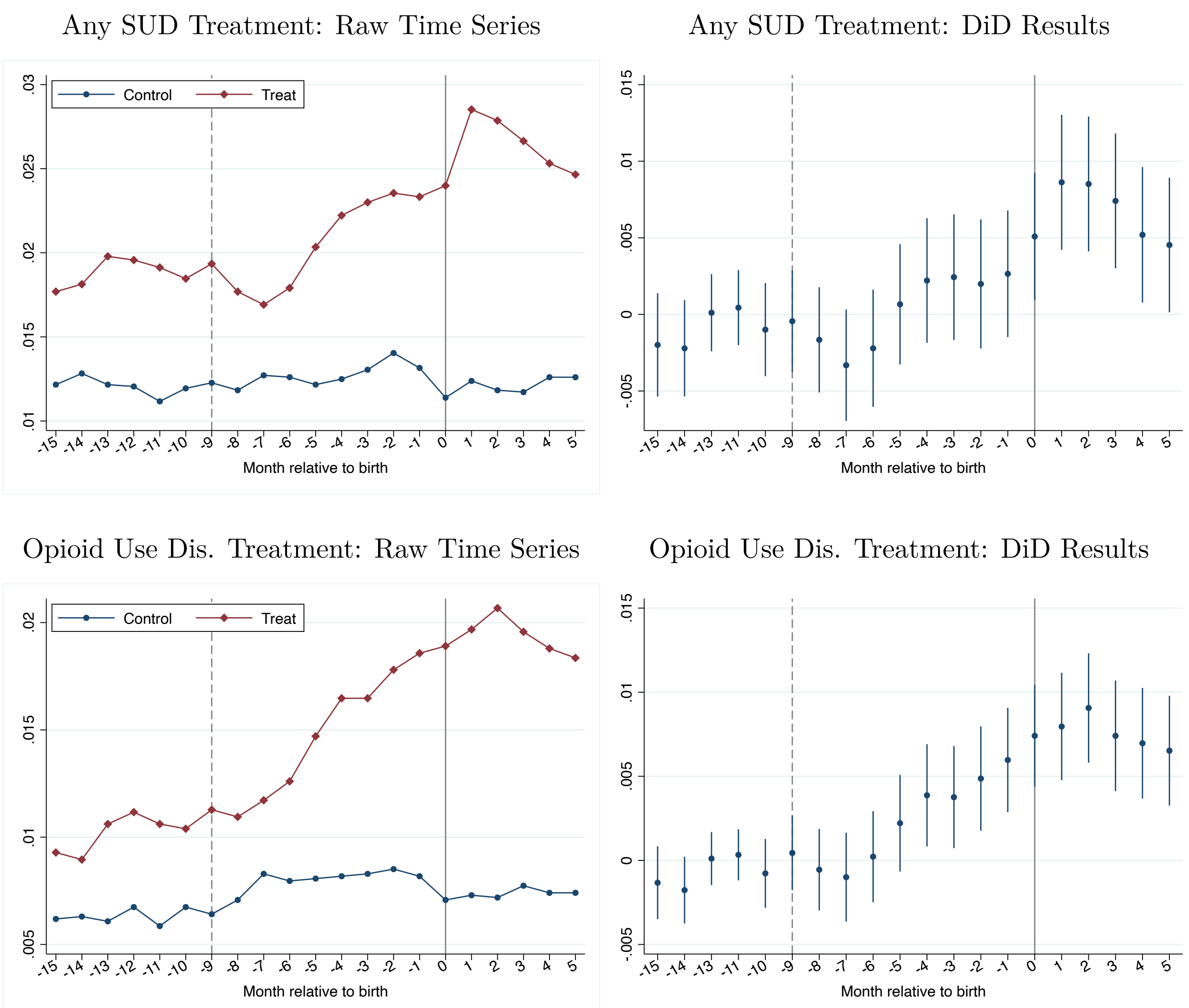


Section 8 Voucher Use: DiD Results



Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. Right figures report treatment effect estimates on interaction coefficients of treatment and relative event time dummies. Regression includes controls for treatment, relative event time dummies, and their interaction. Month −12 relative to childbirth is the omitted category. Sample is restricted to women with at least one month of Medicaid enrollment in their Medicaid history (that is, in five years before their (placebo) conception year). Vertical dotted line shows approximate month of conception of treated individuals. Vertical solid line shows month of childbirth of treated individuals. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown.

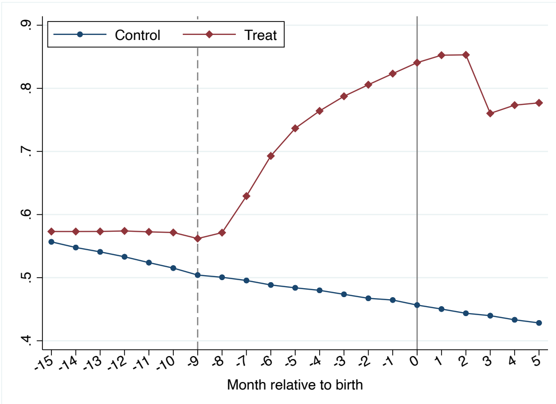
Appendix Figure A.11: Substance Use Disorder - Matched DiD Results



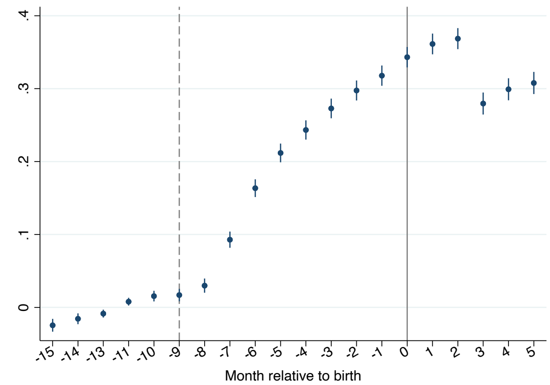
Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. Right figures report treatment effect estimates on interaction coefficients of treatment and relative event time dummies. Regression includes controls for treatment, relative event time dummies, and their interaction. Month -12 relative to childbirth is the omitted category. Sample is restricted to women with at least one month of Medicaid enrollment in their Medicaid history (that is, in five years before their (placebo) conception year). Vertical dotted line shows approximate month of conception of treated individuals. Vertical solid line shows month of childbirth of treated individuals. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown.

Appendix Figure A.12: Government Benefit Use - Matched DiD Results

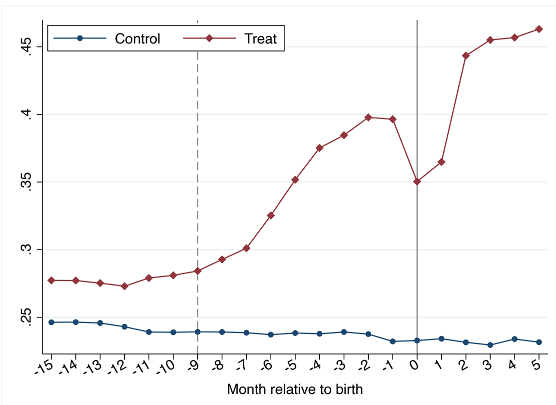
Medicaid: Raw Time Series



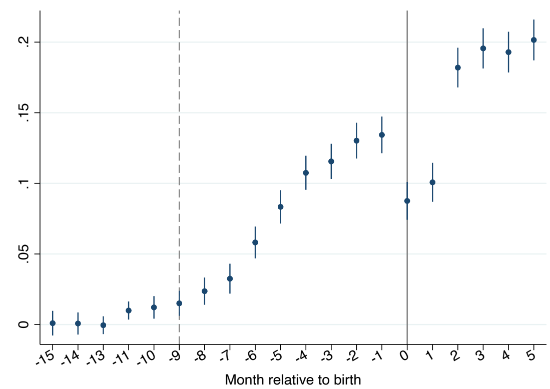
Medicaid: DiD Results



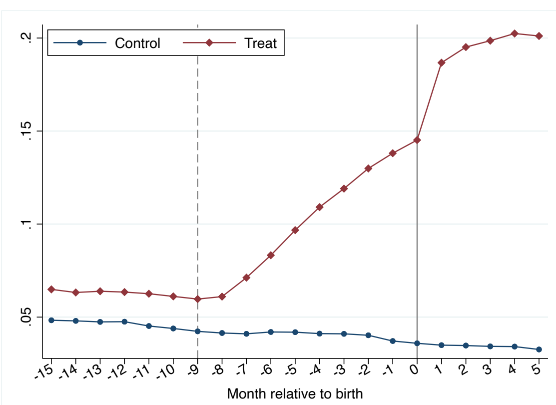
SNAP: Raw Time Series



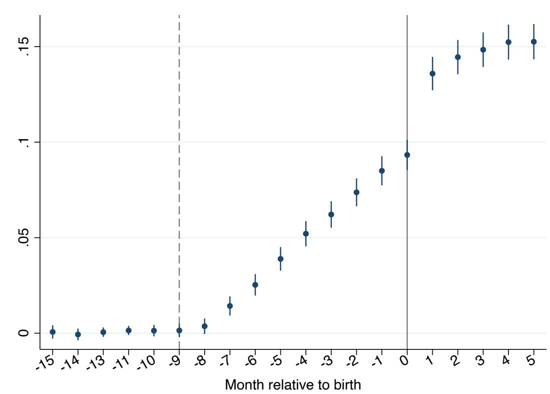
SNAP: DiD Results



TANF: Raw Time Series



TANF: DiD Results

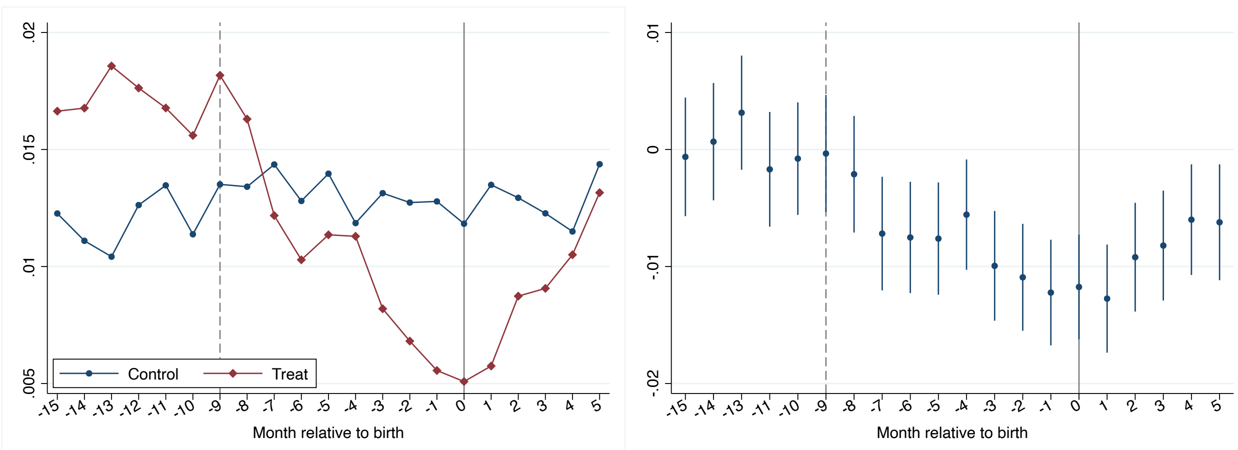


Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. Right figures report treatment effect estimates on interaction coefficients of treatment and relative event time dummies. Regression includes controls for treatment, relative event time dummies, and their interaction. Month -12 relative to childbirth is the omitted category. Sample is restricted to women with at least one month of Medicaid enrollment in their Medicaid history (that is, in five years before their (placebo) conception year). Vertical dotted line shows approximate month of conception of treated individuals. Vertical solid line shows month of childbirth of treated individuals. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown.

Appendix Figure A.13: Criminal Behavior - Matched DiD Results

Raw Time Series

DiD Results



Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. Right figures report treatment effect estimates on interaction coefficients of treatment and relative event time dummies. Regression includes controls for treatment, relative event time dummies, and their interaction. Month −12 relative to childbirth is the omitted category. Sample is restricted to women with at least one month of Medicaid enrollment in their Medicaid history (that is, in five years before their (placebo) conception year). Vertical dotted line shows approximate month of conception of treated individuals. Vertical solid line shows month of childbirth of treated individuals. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-treatment level are also shown.

B. Data and Outcome Construction

B.1 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-difference analysis.

Birth records cover all babies born alive in Allegheny County during the years 1999-2020. Each birth record has fields for mother, father and child identifiers, month and year of birth, as well as 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/or for summary statistics, we also extract information on whether a father is listed on the birth record, marriage status of mother at time of birth, birth weight, and the principal payment method of the birth (Medicaid, private insurance, or other).

B.2 Mental Health/Substance Use Disorder Outcomes

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.

In Pennsylvania, publicly-funded treatment for mental health disorders (including substance use disorders) is managed and financed separately from physical health care (so-called "Behavioral Health Carve-Out"). As a result, mental and physical claims records are collected and stored in separate places, and span different time periods. Mental health records are available from 2005 onward, while physical health records are available only from 2015. Only care that is publicly funded is included in this data; the vast majority is funded through Medicaid: we find that 90% of claims in the mental health records pertain to individuals who are Medicaid-insured in the month to which the claim pertains.

We use mental health records to construct month-level indicators for substance use disorder treatment encounters. We observe treatment encounters for psychotherapy, medication-based addiction treatment, inpatient stays in psychiatric hospitals and addiction treatment centers, and other services (such as use of county-based crisis hotlines, and peer support programs). We construct indicators for encounters for opioid, alcohol, cocaine and cannabis use disorder - the most common substance use disorders observed in the data -, as well as an indicator for *any* substance use disorder encounter.[34]

B.3 Housing Outcomes

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 a) homeless shelter stays, b) medium- to long-term homelessness assistance, c) residence in public housing, b) residence in household that receives Section 8 voucher.

Homelessness service records include date of entry and exit, as well as type of every individual encounter with the homelessness system in the county. We can distinguish the following types of encounters: Day shelter visit, emergency shelter visit, social worker outreach encounters, and program participation in any of the following medium- to long-term anti-homelessness programs: rapid rehousing, permanent supportive housing, and transitional housing. 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. 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.

B.4 Social Assistance Outcomes

Social assistance records are essential to our investigation because new parenthood increases one's eligibility for assistance while also likely increasing need. 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 2005-2019. Note that for the case of SNAP

[34]We identify respective encounters via their associated ICD-9 and ICD-10 diagnosis codes: 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.

and TANF, the indicators equal one for all household members within a household that receives those services.

B.5 Criminal Behavior Outcomes

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 handle 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 other (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 Magisteial 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.

C. Matched Difference-In-Difference Analysis

In order to account for age effects, we perform a matched difference-in-difference analysis that broadly follows Fadlon and Nielsen (2021) and Mello (2021), who apply this method to estimate the effects of health shocks on labor supply and of traffic fines on financial wellbeing, respectively. This approach matches each individual in the data to a comparable "control" peer who experiences the same event in the future.

Match Definition We match each woman to a "control" peer who has the same own year of birth, race, and Medicaid history, and who experiences her first live birth two years later (that is, two calendar years later, in the same half of the year). We focus our control group on women who give birth two years later in order to maximize comparability subject to the constraint of observing enough post-childbirth periods in which the control peer is not yet pregnant herself. We match on Medicaid history in order to compare women of similar SES. We require a match with respect to two aspects of Medicaid history: i) ever Medicaid enrolled, and ii) recently Medicaid enrolled. Ever Medicaid enrolled is defined as a dummy that equals one if the individual was ever enrolled in Medicaid in the five years preceding the (placebo) pregnancy, akin to our low SES criterion in the full sample. Recently Medicaid enrolled is a dummy that equals one if the individual was ever enrolled in Medicaid in the year preceding the (placebo) pregnancy. Importantly, for the set of control peers, we consider a "placebo" first childbirth date that falls two calendar years before the actual first childbirth, and construct Medicaid history relative to this "placebo" event date.

Event Time Window Choosing control peers whose event date is only two years in the future has the advantage of higher similarity between treated and control individuals. At the same time, it limits the length of the event time window we can consider, since the conception date of a control peer lies only about 14 months after the childbirth date of the treated peer. A distance of two years in events within a matched pair allows us to consider an even time window spanning from six months before conception to six months post childbirth without introducing any contamination. To rule out such contamination, we truncate the data and only include observations that fall into the event time window.

Sample Construction For each woman with a first live birth in the sample period, we consider a single exact match in terms of the criteria specified above. If there is more than one control match for a given individual, we randomly select one individual from the pool of potential matches. Women can enter the sample once (only as a treated peer or only as a control peer) or twice (as both a treated and control peer); in case they enter the sample twice, their two panel series are completely non-overlapping, by construction. Among all women with first live births in the sample period (2007-2018), we find a control match for 77%. This fraction drops to 70% for women identified as low SES (that is, with at least one month of Medicaid enrollment in the five years preceding conception). The final sample of all women with live births in the matched DiD analysis includes 63,761 "treated" women (and the same number of control peers); the final sample of "low SES" women, defined as having at least one month of Medicaid enrollment in the history period considered for matching, includes 9,045 "treated women" (and the same number of control peers).

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

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

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.

D. Difference-in-Difference 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-difference 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 et al., 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.[35,36]

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.

[35] 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.

[36] 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).

We can only identify non-live births from birth records pertaining to 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.[37] 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.[38] 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. By focusing on younger women, we are also more likely to zoom in on unplanned pregnancies. As in our main analysis, we exclude women for whom the event happens at age younger than 16, 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 981 women who have a miscarriage and 25,801 women who have a live birth.

Summary Statistics Summary statistics for this sample are presented in Table A.16. Overall, the approximately 27,000 women in this sample have similar demographic characteristics to those in our main event study sample of low SES first-time mothers, though the fraction of low SES individuals is slightly lower. 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 similar racial/ethnic composition. The sample of women who experience a miscarriage skew slightly more vulnerable on socioeconomic characteristics, as evidence by slightly higher rates of

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

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

pre-pregnancy SNAP use (19.6% vs. 17.3%), 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, 25.7% 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-difference estimator following Massenkoff and Rose (2020). 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 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 with caution. 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 et al., 2021), 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 under-estimate in absolute terms. By the same token, any positive change to living conditions we find in the live birth group relative to the miscarriage group are likely to be an over-estimate of the impact of a live birth relative to the counter-factual of having no birth event at all.

Results We present results from the DiD estimation in Table A.17, 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. We find that 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 (i.e. Medicaid, SNAP, TANF), and opioid use disorder 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 1.4pp larger increase in movement into public housing in the year after the birth event—compared to an effect size estimate of 1.3pp in our main event study analysis. However, the magnitude of the coefficients for the social assistance program use outcomes becomes much smaller, consistent with the fact that the eligibility status of the miscarriage sample also changes with pregnancy. On the other hand, while results for long-term homelessness assistance and crime 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 increases in long-term homelessness and 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. Only one outcome, that of any substance use disorder, delivers a different sign compared to our main analysis (with the interaction coefficients both during and after pregnancy being negative), but the coefficient estimates are not statistically significantly different from zero.