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PARENTHOOD IN POVERTY

Abstract

Parenthood could change economic and psycho-social trajectories profoundly, creating opportunities in some domains of life and strain in others. Individuals of low SES, who might lack resources to weather the disruptions caused by parenthood, may face distinct challenges, detailed knowledge of which would greatly aid better design of social assistance. We provide comprehensive evidence of the effects of new parenthood on key markers of economic and psycho-social well-being among women of low SES in the U.S. Using longitudinal, high frequency administrative records from a large urban county in combination with an event study design, 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 opioid use disorder likely driven by those with a pre-existing, formerly untreated disorder; iii) large eligibility-rule driven increases in use of key government assistance programs for healthcare, food assistance, and cash assistance; iv) large reductions in criminal behavior, unlikely to be driven by increased access to government assistance. Effects are heterogeneous by race and vulnerability to mental health disorders. Robustness checks, including two separate (matched) difference-in-differences analyses, suggest robustness to endogeneity in the timing of first parenthood.

JEL Classification: H00, I00

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Parenthood in Poverty

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March 18, 2023

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Abstract

Parenthood could change economic and psycho-social trajectories profoundly, creating opportunities in some domains of life and strain in others. Individuals of low SES, who might lack resources to weather the disruptions caused by parenthood, may face distinct challenges, detailed knowledge of which would greatly aid better design of social assistance. We provide comprehensive evidence of the effects of new parenthood on key markers of economic and psycho-social well-being among women of low SES in the U.S. Using longitudinal, high frequency administrative records from a large urban county in combination with an event study design, 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 opioid use disorder likely driven by those with a pre-existing, formerly untreated disorder; iii) large eligibility-rule driven increases in use of key government assistance programs for healthcare, food assistance, and cash assistance; iv) large reductions in criminal behavior, unlikely to be driven by increased access to government assistance. Effects are heterogeneous by race and vulnerability to mental health disorders. Robustness checks, including two separate (matched) difference-in-differences analyses, suggest robustness to endogeneity in the timing of first parenthood.

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Parenthood could profoundly change the lives of new parents: beyond affecting the ability to work, it may alter housing needs, influence mental and physical health, probe the stability of relationships, and more. For individuals of low socio-economic status (SES), who may lack the resources to fully insure themselves against the disruptions caused by having a child, such impacts in domains outside of the labor market, such as housing, may be at least as relevant for their economic and psycho-social trajectories as impacts on labor supply and labor income. Despite their importance, such effects have to date been under-explored in empirical research. This lack of evidence is troubling because it limits our ability to design better safety-net policies for a significant part of the population: in the United States, 13.2% of families with children have incomes below the poverty limit, and this fraction rises to 31% for single-mother headed households, who make up 24% of all households with children (US Census Bureau, 2021).

In this paper, we trace out the impacts of pregnancy and parenthood on key markers of economic and psycho-social well-being among women of low SES in the United States. 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. The data includes birth records for the universe of births in the county, as well as comprehensive records of residents' living conditions, spanning Medicaid mental and physical health-claims records, homelessness service records, public housing and Section 8 records, welfare benefit records, and court records. Our main sample consists of all women who have a first birth in the sample period and are of low SES as measured by their pre-pregnancy Medicaid enrollment—approximately 12,500 individuals. We also show results for the full sample of women, for alternative low SES criteria, and, subject to an important selection caveat, results for first-time fathers.[1]

Our empirical strategy involves 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,

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

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 leading up to conception, we control for a linear pre-trend in event time. Third, in robustness checks, we employ two separate matched difference-in-differences strategies that further account for endogeneity in the timing of pregnancy. The first 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 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 of 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, Jaravel and Spiess (2022) as our main estimator. It relies on estimating individual and time fixed effects based on pre-treated observations only. As shown in the robustness section, the results are unchanged when we use the traditional two-way fixed effects estimator.

We establish four main results. First, we find that, for low-SES women, pregnancy and childbirth lead to substantial changes to the housing environment, marked by short-term increases in housing instability, and long-term moves into public housing. During pregnancy, homeless shelter stays – a rare and extreme outcome in our data – increase by 77% or 0.083 percentage points (pp); in the year after childbirth, homeless shelter stays and stays in longer-term housing programs for individuals experiencing homelessness are also more frequent, although more noisily estimated. The increase is likely driven by real changes in housing needs rather than by eligibility changes resulting from pregnancy and parenthood: when studying the birth of a second child—an event that does not substantially change eligibility for homelessness services, since a child is already present throughout—we observe even stronger effects. Moreover, we find a gradual, persistent and large increase in public housing occupancy as a result of new parenthood: one year after childbirth, parenthood increases the share of women who live in public housing by 40%, compared to the no-child counterfactual.

Second, we find that pregnancy and childbirth lead to increases in treatment for substance use disorder (SUD), driven by opioid use disorder (OUD)—the most common SUD observed in our sample. Treatment for opioid use disorder increases by 48% (or 0.72pp) in the year after childbirth, on a base of 1.5% pre-pregnancy. The increase 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. We rule out the possibility that the detected effects are due to changes in treatment access or observability caused by changes to insurance status by limiting our analysis of substance use disorder outcomes to women who are continuously Medicaid-insured. Investigating mechanisms, the timing and sharpness of the increase in treatment early on in pregnancy are most 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), and with smaller scale panel studies documenting decreases in self-reported drug use after becoming a parent (Thompson and Petrovic, 2009; Fergusson, Boden and Horwood, 2012). In sum, new parenthood is likely to be an important push factor out of untreated substance use disorders.

Third, we find 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 becoming a parent leads to a 28pp increase in Medicaid coverage in the year after childbirth. As a point of comparison, for the women in our sample, the impact of the Affordable-Care Act (ACA) expansion is less than half of the above-mentioned magnitude. The increases in SNAP (i.e., food stamps) and TANF (i.e., cash assistance) enrollment due to new parenthood are also large (16pp and 15pp 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 first childbirth. 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 rules-driven as opposed to driven mainly by reductions in income due to reduced capacity to work.

Finally, we document that pregnancy and childbirth lead to large decreases in criminal behavior, and that the reductions is unlikely to be driven by better access to social assistance programs such as healthcare coverage. Specifically, charges for criminal offenses (measured by a month-level dummy for whether a criminal charge was filed in court) decrease by 56% on average in the year after childbirth, on a base of 1.7% pre-pregnancy. The biggest decrease is observed for theft and drug charges around the months of childbirth. Criminal behavior re-bounds a few months after childbirth, but stays at a permanently lower level. Leveraging the cross-domain nature of our data, we find similar-sized decreases among women who did vs. did not already have access to important benefit programs (such as Medicaid), and we find large decreases also for women who do not start any SUD treatment. It points to incapacitation or a (temporary) motivation to turn one's life around (the turning point hypothesis formalized by Sampson and Laub, 1990), or the combination of the two, as the main mechanisms at play.

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 first childbirth might lead to high take-up rates and higher willingness to move across neighborhoods to high-opportunity areas, which have been shown to produce better outcomes for children (see Chyn and Katz, 2021, for an overview of the neighborhood effects literature). Furthermore, given our evidence of increased housing instability during this time period, timing housing assistance this way 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 (see, e.g., Almond, Currie and Duque, 2018; Rossin-Slater and Persson, 2018), as well as the literature showing that the earlier children move to opportunity, the better their outcomes (Chetty, Hendren and Katz, 2016; Chetty and Hendren, 2018). Moreover, our findings underscore the importance of social factors for criminal desistance and engagement with substance use disorder treatment. In environments marked by low levels of economic opportunity and high levels of social isolation, programs that foster a strong sense of purpose and meaning—by returning social capital, economic opportunities, or both—are likely to improve individual welfare tremendously, and spur strong positive externalities at the community-level.

This paper contributes to the literature on the impact of parenthood by painting a more comprehensive and detailed picture of the effects of parenthood on the non-labor-market outcomes of low-SES individuals than has previously been possible. Most of the existing literature focuses on labor-market outcomes such as earnings and employment (including Adda, Dustmann and Stevens, 2017; Angrist and Evans, 1998; Lundborg, Plug and Rasmussen, 2017; Zohar and Brooks, 2022; Gallen et al., 2022), with a special focus on differences across gender (e.g. Blau and Kahn, 2017; Kleven et al., 2019; Kleven, Landais and Søgaard, 2019; Kuziemko et al., 2022).[2] As far as non-labor-market outcomes are concerned, the closest papers to ours are Miller, Wherry and Foster (2022) and Massenkoff and Rose (2022). The former study documents the effects of abortion denial among a sample of 600 women seeking to terminate their pregnancies. The authors find that abortion denial leads to increased rates of living alone or living with a male partner (according to self-reports), and to increases in financial instability (a composite measure that also includes evictions); the latter is consistent with our finding of increased homelessness encounters. Massenkoff and Rose (2022) employ an event study design to investigate the effects of pregnancy on crime using administrative data from Washington State and also find that pregnancy leads to large reductions in criminal behavior. Also related is Stanczyk (2020), who studies average amount of SNAP and TANF dollars received, using self reports from the SIPP in a specification with relative event time indicators, as well as calendar year and month fixed effects. Furthermore, there are important

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

correlational studies on Medicaid[3], SNAP[4], TANF[5] and SUD treatment[6], which report raw, un-adjusted rates of enrollment/use for individual programs and largely rely on self-reported survey data.[7] 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 outcomes of public housing residence and homelessness have not been studied before in the context of new parenthood. Second, we can estimate more precise and robust effects, thanks to high quality administrative data available at high frequency and encompassing a large sample: the latter two features allow us to apply an event study approach that accounts for pre-trends, as well as individual fixed effects, to better isolate causal effects.[8] Furthermore, 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. The high dimensionality of our data allows us to show that different groups 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).

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

[3]Daw et al. (2017) rely on the Medical Expenditure Panel Survey ($N = 2,726$) and find a 20pp higher self-reported Medicaid enrollment at delivery relative to the quarter before pregnancy (while we find a 13pp increase in our full sample, using our event study design). Also related are Adams et al. (2003) and D'Angelo et al. (2015), who rely on retroactive survey data collected after delivery.

[4]Gordon, Lewis and Radbill (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]Kim (2018) relies on the SIPP survey and detects 10pp higher self-reported TANF enrollment after childbirth compared to before pregnancy among low SES women.

[6]Wolfe et al. (2007) study a sample of 431 women identified as having a SUD *at their delivery encounter* (thereby introducing important selection concerns when aiming to identify the impact of pregnancy and parenthood on SUD treatment), and use administrative records to document rates of treatment for substance use disorder in the pre-conception, pregnancy, and postpartum period, respectively.

[7]See Celhay, Meyer and Mittag (2021) for a discussion of systematic errors in self-reports for the case of government benefits.

[8]With the exception of Miller, Wherry and Foster (2022) for the case of financial stability and papers on the impact of new parenthood on criminal behavior (including Massenkoff and Rose, 2022; Skarhamar and Lyngstad, 2009; Savolainen, 2009, — see the former for a detailed review of the literature related to criminal behavior and family formation), the other related papers (on Medicaid, SNAP, TANF, and SUD treatment) rely on reporting raw means of outcomes without carrying out causal analysis and/or rely on self-reports.

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.[9] 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., 2022; Abramson, 2022). 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 (e.g. Lucas, 2017; Corinth, 2017). We contribute to this literature by providing evidence that pregnancy and childbirth are important drivers of housing instability and homelessness.

The rest of the paper proceeds as follows. Section 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%

[9] See Evans, Phillips and Ruffini (2019) for a thorough review of the literature.

(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,[10] 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.

[10] As common with administrative records at the sub-national level, we do not observe in- and out-migration (see, e.g. Grogger, 2013, for a discussion of this issue). Consequently, we perform several robustness checks in Section 4 that focus on sub-samples with ex ante low likelihoods of out-migration, finding our results essentially unchanged.

1.2 Sample selection

Our primary aim is to study the effect of pregnancy and childbirth on the lives of low-SES individuals. Thus, from the sample of all county residents, 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 for our main analysis; we report results for men in a shorter section after the main analysis.

We further restrict the sample to those ca. 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.[11] We identify first birth mothers as those for whom no birth record from a date earlier than 2007 (and after 1998, the earliest year we observe birth records for) 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

[11] 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.

at any point during the five years leading up to the pregnancy.[12] 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 Bureau, 2018). Note that income eligibility thresholds changed during the sample period: they became less strict for those age 21 or older (below age 21) in 2015 (2014).[13,14] 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 familiar with government assistance. Of the approximately 97,400 first birth events observed in our sample period, ca. 16% 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.

[12]For completeness and robustness, we also provide results for the entire sample of first live births (without the low SES restriction), as well as for alternative low SES criteria, such as pre-pregnancy SNAP receipt, pre-pregnancy Medicaid or SNAP receipt, and childhood Medicaid enrollment; results are reported in the robustness Section 4.

[13]The threshold rose from 100% to 138% of FPL for individuals 6-20 years of age in 2014 (Kaiser Family Foundation, 2021*a*). 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, 2021*b*)

[14]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.

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[15] 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 pre-trends in event time; restricting the post-birth observations to a one-year window, as opposed to a longer time horizon, ensures that our difference-in-differences imputation estimator, which predicts post-birth outcomes based on pre-conception observations, does not extrapolate out too far. Since our outcome data does not extend beyond September 2019, we estimate treatment effects only for individuals for whom we have complete panel data—that is, all 12,928 individuals whose first childbirth falls into the 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 in the entire event time window (that is, in all 33 months spanning 12 months before approximate conception to 12 months after childbirth). We make this restriction because we only observe substance use disorder treatment for Medicaid-insured individuals.[16] 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 in our data due to changes in insurance status). This restriction retains 21% of the sample, resulting in a sample size of ca. 2,700. Compared

[15] 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% or pregnancies last more than nine months (i.e. 39 weeks), 24% last 35-36 weeks (8.05-8.28 months) and 11.27% last less than 35 weeks (8.05 months).

[16] 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).

to the complete low SES sample, the average woman from the resulting sub-sample is ca. 1.3 years younger at first birth and more economically vulnerable (e.g. 2.6% have a homelessness encounter at some point in the year preceding pregnancy, compared to 1.7% in the full low SES sample).

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 12,928 first live births between 2007 and 2018 occurring to women we identify as low SES, and 66,529 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 (average age at first birth of 22 years vs. 28 years), includes a much larger share of underage mothers (9.8% vs. 1.2%), a much larger share of women who are black (52.3% vs. 8.2%), whose child has no father listed on the birth certificate (43.4% vs. 9.1%), who receive SNAP benefits (i.e. food stamps) at any month in the year pre-pregnancy (37.8% vs. 1.1%), and who experience at least one homelessness encounter (1.7% vs. 0.0%) or encounter with the criminal justice system (10.8% 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 50% based on studies of similar populations. This estimate is based on statistics reported in Finer and Zolna (2016), who use survey data to show that for American women who are age 20-24, or who have incomes below the poverty line, ca. 60% of pregnancies are unintended, and ca. 60% of those unintended pregnancies result in a life birth.

1.4 Outcomes and Program Eligibility Rules

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).

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 (i.e. demand-side factors)*, *eligibility (i.e. supply-side factors)*, or *information*. Eligibility criteria are the only elements readily observable to the researcher. Hence, we also collect information on program eligibility rules for each outcome in our data. We provide a detailed overview in Table A.3, and discuss it for each outcome after detailing its construction.

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, or conflict in romantic relationships; 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 labeled *Long-term homeless*),[17] and rental subsidy programs for the low-income population more generally (namely, residence in

[17]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).

Public housing and household receipt of *Section 8* rental assistance).[18] 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.[19]

The homelessness assistance environment changes as individuals change family status, although it does not increase with additional children. Namely, both homeless shelters and long-term homeless housing 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 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.[20] 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 apartments); 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.

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

[19] 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).

[20] 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).

Substance Use Disorders In the domain of mental health, we focus on substance use disorders, 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.[21] In particular, to the best of our knowledge, only one study—Wolfe et al. (2007)—exists that studies the association of pregnancy and parenthood with SUD 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 captures treatment encounters for mental health disorders paid for through public funds. That is mainly treatment of Medicaid-insured individuals, as well as (a likely small number of) uninsured individuals whose uncompensated care costs are paid for through public funds. To avoid issues with interpretation discussed above, our analysis of SUD 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 SUD treatment, inpatient stays in psychiatric hospitals and SUD 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 commonly treated substance use disorders (cannabis, alcohol, and cocaine use disorder). To gauge what *types* of treatment for SUD pregnancy and parenthood trigger, we also distinguish between

[21] 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).

the three main types of treatment for opioid use disorder: opioid use disorder medication treatment encounters (such as methadone treatment encounters), inpatient opioid use disorder treatment (i.e. rehab), psychotherapy for opioid use disorder, as well as unspecified outpatient encounters (which are typically either psychotherapy- or medication-related).

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 both pregnant and non-pregnant patients. Further, for the case of opioid use disorder, a recent RCT documents that pregnancy status does not increase treatment access conditional on attempting to make an appointment: among simulated patient-callers who called outpatient opioid use disorder treatment centers in ten U.S. states, those representing *non-pregnant* 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 (in our sample of continuously Medicaid-insured individuals).

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 anyone in the household in which the woman resides receives the benefit.

Eligibility for 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.

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). Since we observe court records only starting in 2007, we exclude individuals with childbirth dates earlier than 2009 from the analysis of criminal outcomes.

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.[22] 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. Under the assumption that such endogenous factors evolve smoothly around the exact time of conception/childbirth, we can recover the impact of parenthood via estimating discontinuous changes from such smooth trends at the event of childbirth (Kleven, Landais and Søgaard, 2019). To most closely approximate a setting where this assumption holds, we employ a dynamic difference-in-differences approach with individual and time fixed effects using high frequency panel data and including a control for a linear pre-trend in event time—that is, we measure changes in outcomes around pregnancy and childbirth relative

[22] As highlighted by Kleven, Landais and Søgaard (2019), 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, Wherry and Foster, 2022; Zohar and Brooks, 2022) or IVF treatment (as in Lundborg, Plug and Rasmussen, 2017).

to smooth trends leading up to the pregnancy, 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 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.[23] We use Borusyak, Jaravel and Spiess (2022)'s "imputation" estimator—which is shown to be robust and efficient under treatment effect heterogeneity—as our main specification. In a nutshell, this method only uses pre-treatment observations to estimate individual and time fixed effects, thereby allowing for arbitrary treatment effect heterogeneity.[24] Nevertheless, for completeness, we also report results from a conventional two-way fixed effects estimator in the robustness section, and find our results virtually

[23]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, e.g., Borusyak, Jaravel and Spiess, 2022; Sant'Anna and Roth, 2022; 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, Jaravel and Spiess (2022). Such issues may even flip the sign of the estimate compared to the true effect.

[24]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.

unchanged.

Following Borusyak, Jaravel and Spiess (2022), 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.[25]

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

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

where y_{it} is the outcome of interest for individual i in calendar year-month t, where μ_i and γ_t are individual and calendar year-month fixed effects, respectively, and where r represents relative event time (e.g. $r_{it} = -12$ for the calendar year-month t that corresponds to 12 months before i's childbirth).[26] In our context, "untreated observations" are all those observed ahead of a woman's pregnancy that results in her first live birth.[27] We control for a linear pre-trend in event time, captured by δ. By including this linear pre-trend, the treatment

[25]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.

[26]We bin relative month -21 and -20 into a single value of $r = -20$ to avoid issues of co-linearity with γ_t.

[27]We approximate the calendar year-month of pregnancy onset to fall nine months before the calendar year-month of birth. See footnote 15 for details.

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 leading up to the pregnancy. 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 clustered at the individual-level, whose formula is derived and shown to be valid in large samples in Borusyak, Jaravel and Spiess (2022).[28]

2.1 Identification

Our empirical strategy relies on two assumptions: no anticipatory effects and parallel trends. No anticipation requires that there is no anticipatory response to pregnancy ahead of time—an assumption that may be 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

[28]We use Borusyak, Jaravel and Spiess (2022)'s STATA packages "did_imputation" and "event_study" to obtain treatment effect estimates, standard errors, and event study plots.

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 that also influence the outcomes of interest (such as meeting a new partner), 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 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 a 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 a pre-existing linear trend.[29] To formally assess whether this specification accurately nets out any pre-trends, we test for and reject the presence of pre-trends across all our twelve outcome variables, using the pre-trend test derived by Borusyak, Jaravel and Spiess (2022).[30] Results from this test are reported in the bottom row of the results presented in table-form (Table 2-Table 5).

[29]We also provide results from a specification excluding this control in the robustness section. Magnitudes stay essentially unchanged, while standard errors drop substantially, suggesting that the "pre-trends" visible in the raw figures are largely due to overall time trends, which are netted out via inclusion of year-month fixed effects.

[30]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.

To the extent that the onset of pregnancy is correlated with sharp changes to living conditions, a control for a pre-trend in event time fails to account for such residual endogeneity. Therefore, we provide further evidence with a difference-in-differences analysis, comparing women who experience live births to those who experience miscarriages (similar to Massenkoff and Rose, 2022). This design addresses the potential endogeneity in the (sharp) timing of pregnancy. Finally, to directly net out any "age" effects (that could bias our results in case pregnancy onset correlates with, for example, finishing high school), we employ a matched difference-in-differences analysis that compares a woman's change in outcomes around childbirth to the contemporaneous change of a matched control peer of the same cohort with similar demographic characteristics who gives birth two years later. 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 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 Emergency 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 stays below.

While homeless shelter stays are a relatively rare occurrence even among economically vulnerable individuals—the cumulative risk of having at least one homeless shelter stay ahead of the first pregnancy is 1.8% in our sample—we find that pregnancy and new parenthood increase this risk substantially. 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 summarizes treatment effect estimates by averaging the monthly estimates into the two aggregate time periods of pregnancy and year after childbirth. The magnitudes of the effects are substantial: during pregnancy, homeless-shelter visits increase by 0.083pp (77%) compared to the no-child counterfactual—an estimate that is highly statistically significantly different from zero; the coefficient estimate for the year post-birth is of similar magnitude, but noisier. These results suggest that childbirth and especially pregnancy may generate substantial short-term housing disruptions for low SES women. We find that these effects are likely reflecting real increases in housing disruptions, as opposed to changes in eligibility for homeless services due to changes in family status: when comparing effect sizes across first and second births for women for whom we observe two births, we find that first birth effects are similar to or less pronounced than those observed around the second birth—where eligibility is unlikely to change substantially, since a first child is already present (see Figure A.1).[31]

Figure A.2 explores heterogeneity along age and race and shows that Black women experience larger increases in homeless shelter visits as a result of pregnancy and childbirth (while there is no heterogeneity by age). This finding is consistent with Black women having less access to informal housing insurance (e.g. through family), and therefore being less able

[31]Summary statistics for this sample and results in table-form are presented in Table A.4 and Table A.5, respectively. For power-reasons, we do not restrict this analysis to women of low SES.

to weather short-term disruptions to housing needs due to changes in family composition.[32]

Medium-to-Long-Term Housing Assistance Pregnancy and childbirth may also generate longer 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, in the form of public housing and Section 8 vouchers.

We present the raw time series and event study plots side-by-side in the bottom panel of Figure 1 (for medium-to-long term homelessness programs), as well as in Figure 2 (for public housing and Section 8). For all three housing programs, we observe an increase in use after childbirth, but magnitude and precision of the estimates vary considerably. The starkest pattern emerges for public housing: we find statistically significant, positive effects starting two months before childbirth that increase linearly with time such that, one year after childbirth, parenthood increases the share of women who live in public housing by 40% (or 2pp), compared to the no-child counterfactual. The effects on Section 8 rental subsidy receipt are more noisily estimated, commence later, and, even one year post-birth, are only approximately half the size of those on public housing, in absolute terms. They suggest that, among the two programs, public housing more readily addresses women's short-term housing needs due to new parenthood—most likely not because it provides the more desirable housing environment, but rather the opposite: because it is *less* desirable than Section 8, it is more readily available: 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). Given the persistence of housing choices, as well as the evidence that

[32]We can also compare trajectories of women who do vs. do not have a father listed on their child's birth certificate—a proxy for whether they become a single parent or not. With the caveat that this "moderator" obtains endogenously, at the moment of childbirth, we find a sizeable, 0.11pp increase in the homeless shelter encounter gap between those with no father listed and those with a father listed during pregnancy compared to pre-pregnancy—a 200% increase relative to the pre-pregnancy difference in average homeless shelter encounters across the two groups.

Section 8 program enrollment produces better outcomes for children than public housing, on average—and that children do better the earlier they move from public housing to Section 8—(Chyn, 2018), the welfare loss from directing new mothers into public housing, rather than prioritizing them for Section 8, could be large.

Table 2 summarizes treatment effect estimates by averaging the monthly estimates into the two aggregate time periods of pregnancy and year after childbirth. Focusing on the year post childbirth, we find suggestive evidence of increased movement into medium-term homelessness housing programs, but the increase, while large at 0.157pp (or 27%), is not statistically significantly different from zero. Compared to the no-child counterfactual, public housing dwelling increases by 1.416pp (or 30%) on average in the year post childbirth; the equivalent coefficient estimate for Section 8 utilization is 0.407pp (or 3.4%).

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.3 shows that Black women and younger women show a disproportionate increase in movement into public housing (top two panels). 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 more likely due to moves *out of* parental households straight into public housing: we find a large positive effect of new parenthood on the probability to head a household in public housing- with 1.723pp (or 169% relative to the pre-pregnancy mean), the effect size is even larger than that observed for public housing residence, overall (Table A.6).[33] 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

[33] In contrast, we detect a much smaller impact of new parenthood on being the head of a Section 8 voucher using household (0.359pp).

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 experienced issues with substance use. Figure A.4 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

Substance use disorders (SUD), which are often very debilitating (Degenhardt and Hall, 2012; Romanowicz et al., 2019), are not a fringe issue: in our sample of low SES first-time mothers, 11% have been treated for a SUD at least once in their life before their first pregnancy—33% of them for opioid use disorder (OUD), the most common substance use disorder observed in our data. In particular for OUD, highly effective treatments exist, but are severely under-utilized (Blanco et al., 2013), and individual (i.e. demand-side) determinants of treatment take-up are poorly understood.

We find that new parenthood increases treatment for SUD, and that this increase is driven by treatment for OUD.[34] Figure 3 presents a time series of raw means of treatment for *any* SUD (top panel) and OUD specifically (bottom panel) 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 Table 3. The event study figure shows that treatment for OUD starts increasing around four months after conception, and remains at a relatively stable level in the year after childbirth. The

[34] Recall that, as detailed in Section 1.2, we restrict the sample for this analysis to the subset of women who are continuously Medicaid-insured throughout the event time window. While this restriction limits our sample size and thus reduces power, it allows us to rule out that increases in observed treatment receipt are merely due to changes in visibility of service receipt due to changes in insurance status (e.g. when switching from private insurance to Medicaid).

magnitude of the effect is substantial: for OUD treatment, we estimate an increase of 0.36pp (or 24% relative to the pre-pregnancy mean) during pregnancy, and an increase of 0.72pp (or 48%) in the year post childbirth, compared to the no-pregnancy/no-child counterfactual. For any substance use disorder, we estimate an effect of 1.151pp (or 45%) in the year post childbirth. When investigating different treatment types in Table A.7, we find large increases in medication-based treatment (such as methadone and buprenorphine), which has been shown in the medical literature to be highly effective in non-pregnant patients (Mattick et al., 2014), and is also strongly recommended in pregnant patients (World Health Organization, 2014).[35]

It is important to point out that our data does not allow us to determine with certainty whether the increased treatment for OUD is due to increased treatment for already preexisting, non-worsening opioid use disorders, vs. new cases or a worsening of OUD 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 OUD 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).

In sum, our findings suggest that new parenthood can be an important push factor out of untreated substance use disorders. Clearly, access to SUD 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 SUD treatment services for low income populations.

[35] We report results for the remaining secondary substance use disorder outcomes in the other columns of Table A.7. We find evidence of substitution of rehab-based OUD 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 statistically significant effects on treatment for any of the three disorders (columns 5-7).

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 28pp increase in Medicaid insurance status due to childbirth, and a 16pp and 15pp increase in SNAP and TANF receipt, respectively. In terms of magnitudes, the impact of new parenthood on Medicaid insurance enrollment is more than twice as large as that of the ACA expansion for the women in our sample.[36] 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, Meyer and Sullivan (2021), who highlight the important role of policy in explaining the diverging trends in consumption patterns of low-educated single mothers over the last 30 years, relative to trends among low-educated single women without children.

As discussed in Section 1.4, wider eligibility is likely to translate directly to higher 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. Moreover, 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

[36] 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 as large as the impact of "aging out" of child Medicaid for the women in our sample (plotted in Appendix Figure A.6).

institute further eligibility changes due to change in family status.[37] 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 investigate 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 treatment are highest (see Figure 3). Accordingly, when we zoom in on the ca. 3,800 first-time mothers in our data who *lose* Medicaid at 60 days postpartum, we find an abrupt, 0.6 pp (or 60%) drop in publicly funded treatment for substance use disorder in the subsequent month (Figure A.7).[38] 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 after childbirth (Goldman-Mellor and Margerison, 2019)—underscores the importance of this issue. Therefore, expanding the post-birth Medicaid-eligibility period, or providing alternative subsidies in the months after the end of Medicaid-eligibility could help avoid disruptions in or loss of SUD treatment services during a very sensitive time period for parents and children. The findings thus lend support to a key reform of Medicaid enacted in March of 2021: the Postpartum Coverage Extension, a provision in the American Rescue Plan Act, which gives all states the new option to extend the postpartum coverage period under Medicaid from 60

[37]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, 2021*c*). 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.

[38]"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).

days following pregnancy to a full year (Kaiser Family Foundation, 2021*d*).

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 that of criminal behavior, the direct and indirect consequences of which shape the lives of many individuals in economically vulnerable communities: in our sample of first-time mothers of low SES, 25% have been charged with a criminal offense at least once in their life before their first pregnancy. We begin by documenting overall effects on criminal behavior that are in line with findings from Massenkoff and Rose (2022), before analyzing mechanisms including the role of access to government assistance, such as healthcare coverage.

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 -0.73pp and -0.93pp during pregnancy and the year after birth, respectively. Relative to the pre-pregnancy mean of 1.74%, the decreases correspond to -42% and -56%, respectively. When distinguishing the two sub-components of criminal offenses: misdemeanor and felony offenses, we find significant reductions of similar magnitudes to both (see the first two columns of Table A.8). Among the sub-components of felony offenses, we observe the largest impact on criminal charges related to theft and controlled substances. Our overall findings on criminal behavior are consistent with Massenkoff and Rose (2022), 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.[39] In particular, the crime-reducing effects of benefit receipt have been documented by Jácome (2022) for the case of healthcare coverage, Carr and Packham (2019) for the case of food assistance, and Foley (2011) and Deshpande and Mueller-Smith (2022) for the case of cash assistance.

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).[40] We present time series of mean outcomes separately for each group in Figure A.8, adjusting for cohort, year of childbirth, and race. Panel (B) shows that the propensity to have a criminal offense charge decreases markedly during pregnancy in both groups, suggesting that access to Medicaid is not the primary driver behind the decrease (the average difference in mean criminal offence rates between the access all along and the gain access group in the year pre-pregnancy is 0.52 pp, compared to 0.47 pp during pregnancy). In the year after childbirth, we find a slightly smaller rebound in criminal behavior among the group that gains access to Medicaid during pregnancy: the average difference in means during this year is 0.57—a 0.05 pp increase relative to the pre-pregnancy difference.[41] This finding is consistent with access to healthcare coverage driving at most a small part of the negative effect of childbirth on crime observed in the period after childbirth for the average woman in our sample. Rather, the observed trajectories are more consistent with mechanisms of incapacitation, a (temporary) motivation to turn one's life around, or both.

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

[40] "Access all along" is defined as having been enrolled in a given government benefit program for at least 80% of the 12 months before pregnancy. "Gained access" is defined as having been enrolled at most 20% of the 12 months before pregnancy, and having been enrolled at least one out of the first five months of pregnancy. The total sample size for this analysis includes 8,200 women, of whom 47% fall into the access all along group.

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

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. use alternative low SES criteria); b) robustness to "attrition" from in- and out-migration; c) excluding pre-conception months to rule out bias from "anticipatory effects"; d) omitting the pre-trend control; and e) 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.9. With this much larger sample of ca. 80,000 women, who are much less economically vulnerable on average (as can be gauged from summary statistics presented in Table 1), we find sign and statistical significance across virtually all our outcomes unchanged. While impacts are quite similar in relative terms across the two samples, the absolute magnitude of parenthood's impact on homelessness, public housing, and criminal behavior is, expectedly, much smaller in the full sample, highlighting the vastly different challenges and changes to environments that women of lower and higher incomes face as a result of parenthood. For example, pregnancy increases the propensity to stay at a homeless shelter by 0.02pp in the full sample compared to 0.08pp in the low SES sample. Similarly, expanding low SES to include those who received either Medicaid or SNAP benefits at any point in the five years leading up to conception (instead of using the Medicaid criterion only) does not alter results (Table A.10); neither does using a criterion of low SES that disregards Medicaid and only considers SNAP enrollment (Table A.11), or one that only considers Medicaid-enrollment before age 21—i.e. child Medicaid (Table A.12).

We report results from the remaining robustness checks in Table A.13-Table A.18, and find statistical significance levels as well as magnitudes largely unchanged. Table A.13-Table A.15 address potential concerns about in- and out-migration biasing results, by zooming in on sub-samples of i) individuals with Allegheny DHS service encounters in the year before *and* after the event time window, ii) individuals with Allegheny DHS service encounters during childhood, and iii) individuals born in Pennsylvania. Table A.16 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.17 also employs our standard imputation estimator, but drops the control for the pre-trend in event time. Table A.18 shows results from a standard two-way fixed effects estimator.[42]

4.2 Additional Difference-in-Differences 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. 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. These patterns suggest that age effects are not biasing our results in the main analysis. Effect sizes are summarized in table-form for low SES first-time mothers in

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

Table A.20, and for the sample of all first-time mothers in Table A.21. In terms of magnitudes, the matched DiD results closely match those from our main event study, although estimates become noisier for outcomes related to homelessness, possibly due to the smaller sample size of 9,000 instead of 12,000 individuals and the reporting of coefficients on individual months relative to conception (instead of estimating the mean across all pregnancy months and across all months in the year post childbirth, as done in the imputation estimator) to preserve the estimator's validity.

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-differences 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, especially that women experiencing a miscarriage are slightly disadvantageously selected. We report results from the DiD estimation based on 1,019 miscarriage events and 27,329 live birth events in Table A.23, and find them in line with results from our main analysis for most outcomes: having a live birth, compared to a miscarriage, is associated with a statistically significantly larger increase in 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. Results for long-term homelessness and criminal behavior are noisier, but show the same sign as in our main analysis. The only coefficient to switch sign relative to the main results is that for any substance use disorder treatment during pregnancy, which switches from small and positive to small and negative (being statistically indistinguishable from zero in both analyses).

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.24, present event study results for low SES first-time fathers in Table A.25, and for all first-time fathers in Table A.26. Using our Medicaid insurance criterion, we identify 5,046 first-time fathers of low SES in our data, making up 8.3% 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 (19.5% vs. 10.8%).

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 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 and opioid use disorder treatment.

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 many parents do not cohabit and one parent shoulders most parenting responsibilities.

6. Conclusion

In this paper, we traced out the impacts of pregnancy and parenthood on key markers of economic and psycho-social well-being of women of low socio-economic status in the United States. Our findings highlight that becoming a parent brings unique challenges and opportunities for individuals from this demographic group: on the one hand, we document significant strain in the domain of housing in the form of greater housing instability, as well as a large, persistent push into public housing. On the other hand, we find a tremendous increase in access to valuable government assistance programs for healthcare, food, and cash, as well as improvements in the domains of crime and substance use, likely driven by motivational factors.

Our results should be interpreted with caution for several reasons. First, despite our event study strategy featuring a control for a pre-trend in event time 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, we believe the two most important implications of our results are the following: First, the time of new parenthood is a particularly important and suitable one for programs assisting vulnerable women in moving to stable housing in high-opportunity neighborhoods. Not only do we find that the period of new parenthood is one marked by increased mobility and reliance on housing assistance; we also find markers of increased housing instability during this period, suggesting that moving families to opportunity very early on could yield particularly large returns, including for children.[43]

Second, more generally, our findings underscore the importance of social factors for criminal desistance and engagement with substance use disorder treatment. In environments marked by low levels of economic opportunity and high levels of social isolation, programs that foster a strong sense of purpose and meaning—by returning social capital, economic opportunities, or both—are likely to improve individual welfare tremendously, and spur strong

[43]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, Hendren and Katz (2016) and Chetty and Hendren (2018) who show that the earlier a child moves to a better neighborhood, the larger its positive impact on social mobility.

positive externalities at the community-level. To be clear, we do not imply to encourage early parenthood, since it implies significant (economic) strain for parents and a potentially less stable environment for children. Instead, our results suggest that developing and evaluating programs that bring purpose and meaning through alternative channels—e.g. meaningful work opportunities, or investments in social capital—could provide an under-explored, potentially valuable complement to traditional government assistance programs.

Overall, we hope this paper can complement important qualitative and mixed-method work such as Edin and Kefalas (2005) and DeLuca, Wood and Rosenblatt (2019) by shining more 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, created 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, Currie and Duque (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	21.897	28.436
Age 16-17	0.098	0.012
Black	0.523	0.082
White	0.456	0.846
Dad listed on birth certificate	0.566	0.909
Married at birth	0.099	0.711
SNAP recipient in year before pregnancy	0.378	0.011
Any homeless encounter in year before pregnancy	0.017	0.000
Charged with crime in year before pregnancy	0.108	0.010
Any MHD encounter in year before pregnancy	0.128	0.003
Any SUD encounter in year before pregnancy	0.050	0.001
Observations	12928	66529

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	0.083***	0.001	0.092	-0.042
	(0.031)	(0.053)	(0.099)	(0.114)
Post-birth effect	0.070	0.157	1.416***	0.407
	(0.056)	(0.129)	(0.244)	(0.256)
Year-month FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Lin. event time control	Yes	Yes	Yes	Yes
Mean of dep. var	0.108	0.580	4.749	11.850
Obs	457309	457309	457309	457309
N individuals	12928	12928	12928	12928
Wald-statistic pre-trend p-value	0.597	0.235	0.439	0.263

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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with 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 UD treatment
Pregnancy effect	0.067	0.356**
	(0.305)	(0.172)
Post-birth effect	1.151*	0.718*
	(0.677)	(0.387)
Year-month FE	Yes	Yes
Individual FE	Yes	Yes
Lin. event time control	Yes	Yes
Mean of dep. var	2.578	1.510
Obs	97823	97823
N individuals	2715	2715
Wald-statistic pre-trend p-value	0.101	0.875

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 throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with 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	16.525***	6.275***	4.197***
	(0.483)	(0.376)	(0.195)
Post-birth effect	27.786***	15.540***	15.040***
	(0.989)	(0.783)	(0.428)
Year-month FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Lin. event time control	Yes	Yes	Yes
Mean of dep. var	52.978	26.717	5.376
Obs	456756	457309	457309
N individuals	12928	12928	12928
Wald-statistic pre-trend p-value	0.742	0.304	0.145

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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Coefficient estimates and standard errors are multiplied by 100 for better readability. 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

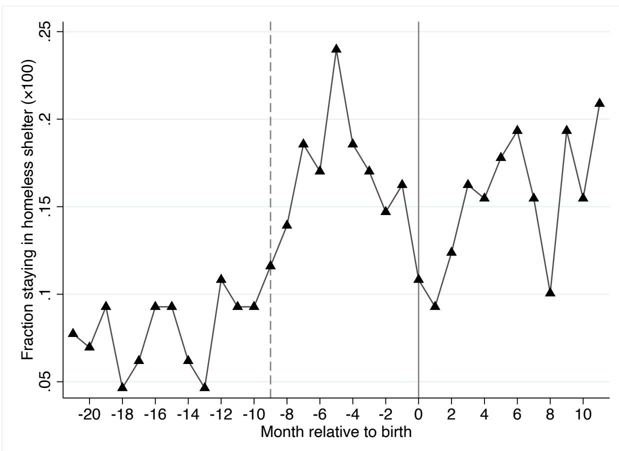
	(1) Criminal offense
Pregnancy effect	-0.732***
	(0.123)
Post-birth effect	-0.973***
	(0.236)
Year-month FE	Yes
Individual FE	Yes
Lin. event time control	Yes
Mean of dep. var	1.737
Obs	380254
N individuals	10593
Wald-statistic pre-trend p-value	0.167

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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

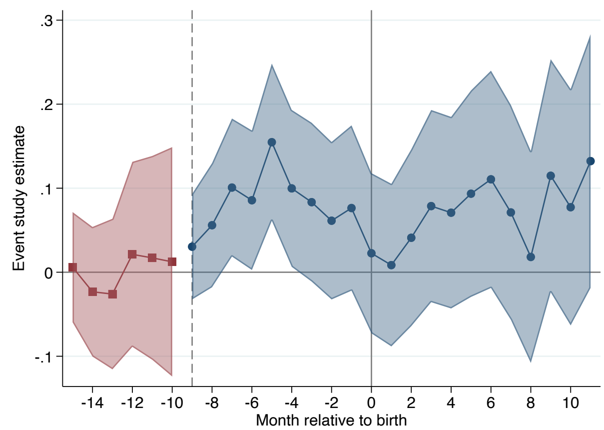
Figures

Figure 1: Homelessness: Raw Time Series and Event Studies

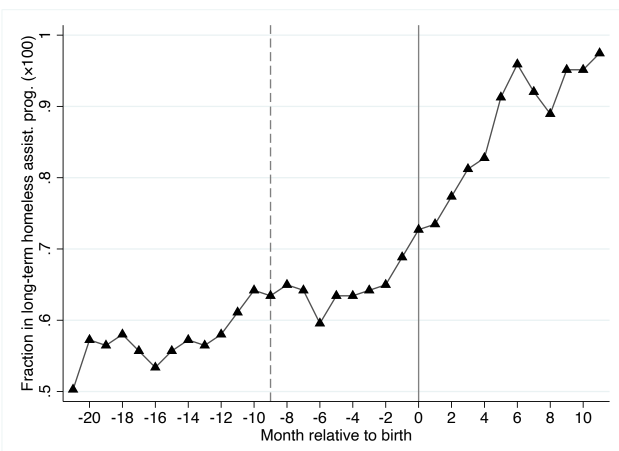
Homeless Shelter Stays: Raw Time Series



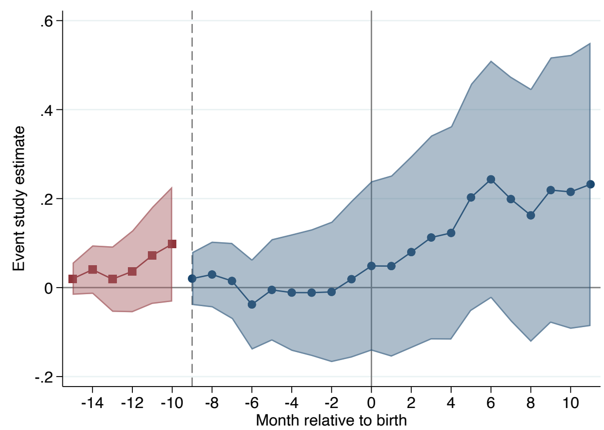
Homeless Shelter Stays: Event Study



Medium/long-term homelessness assistance: Raw Time Series

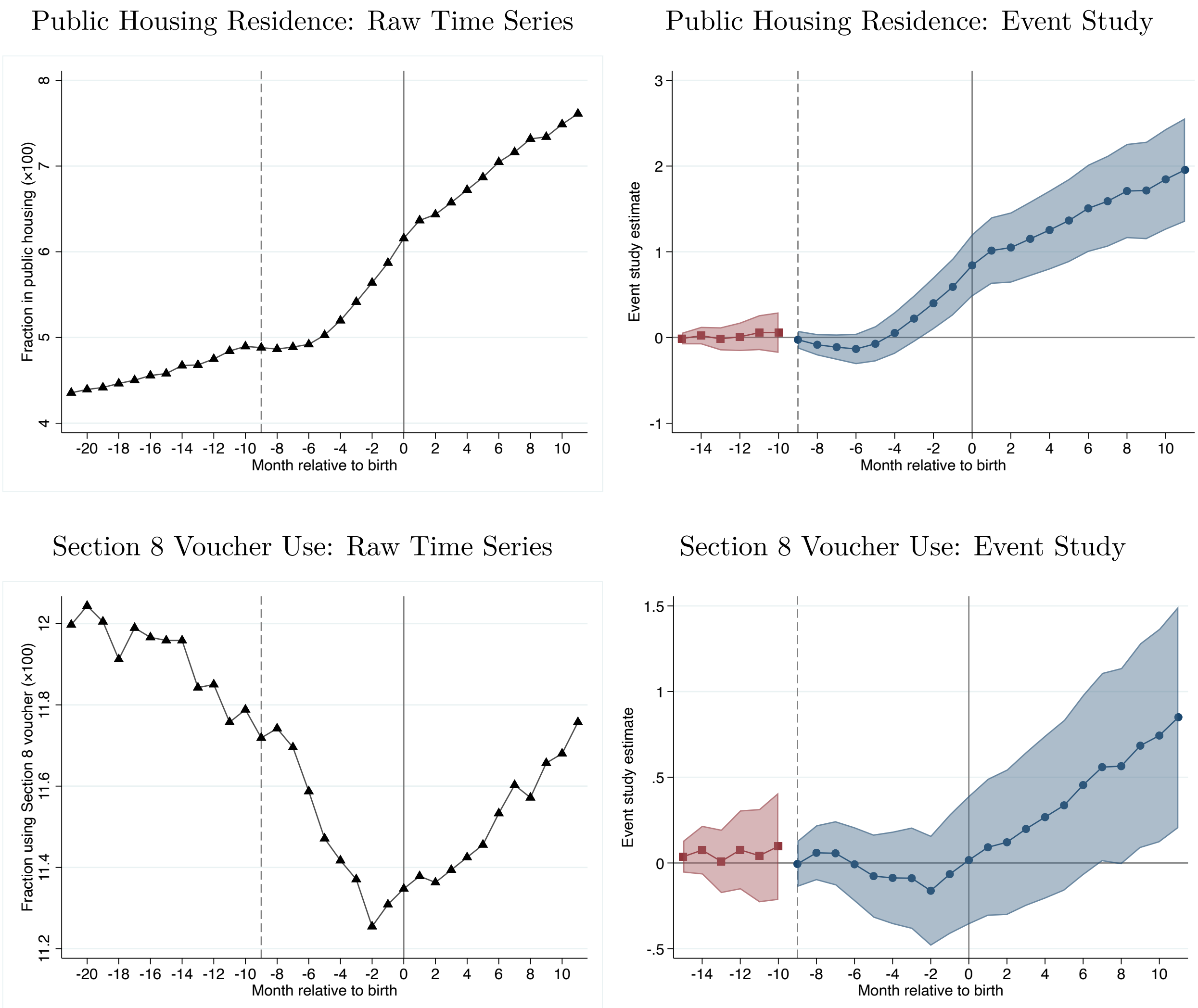


Medium/long-term homelessness assistance: Event Study



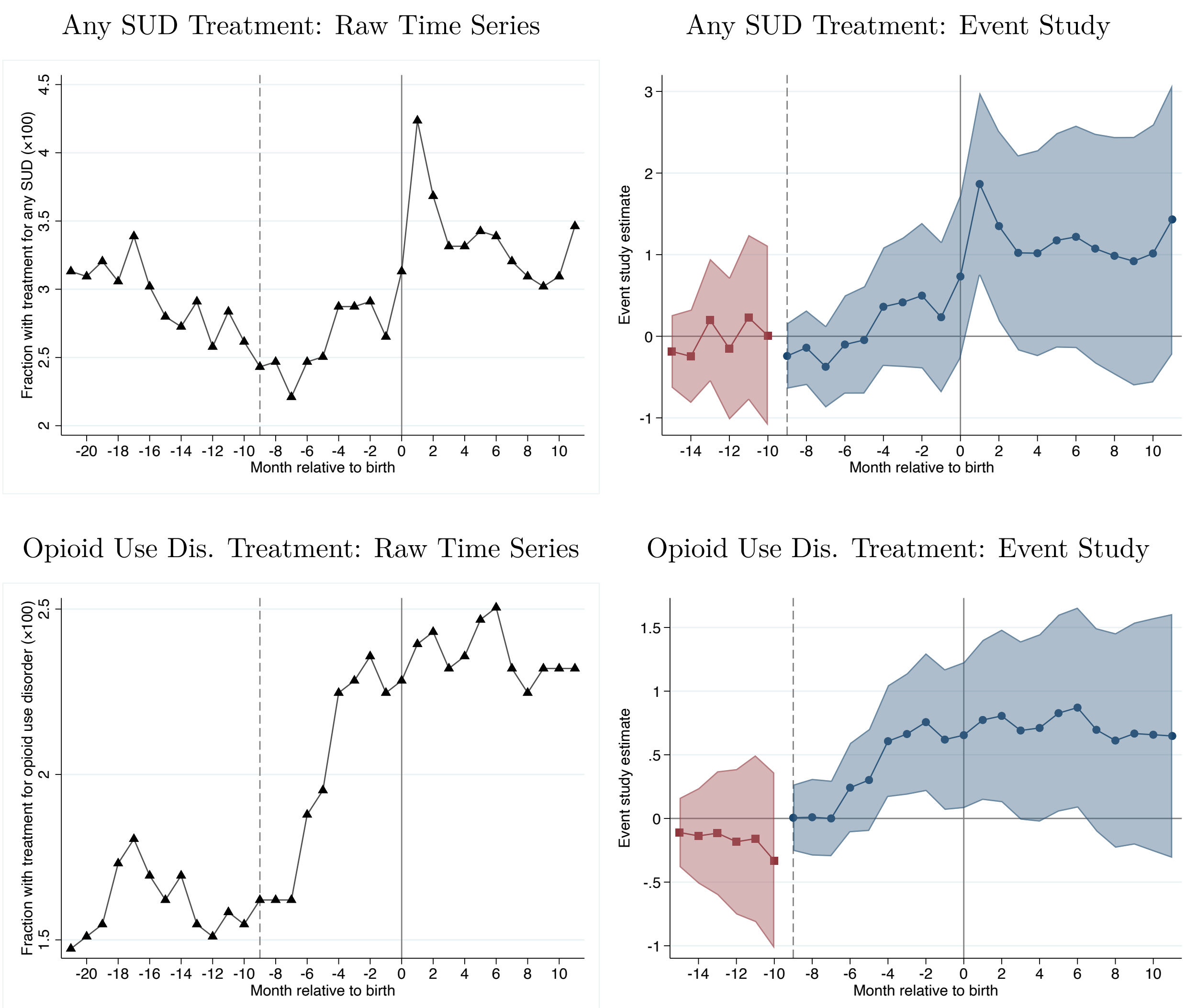
Notes: Figures show raw means of outcomes ×100, by month relative to first live birth event (left), by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right), for the main analysis sample of low SES first-time mothers detailed in Section 1.2. Event study estimates are based on outcome dummy multiplied by 100 for better readability. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. 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



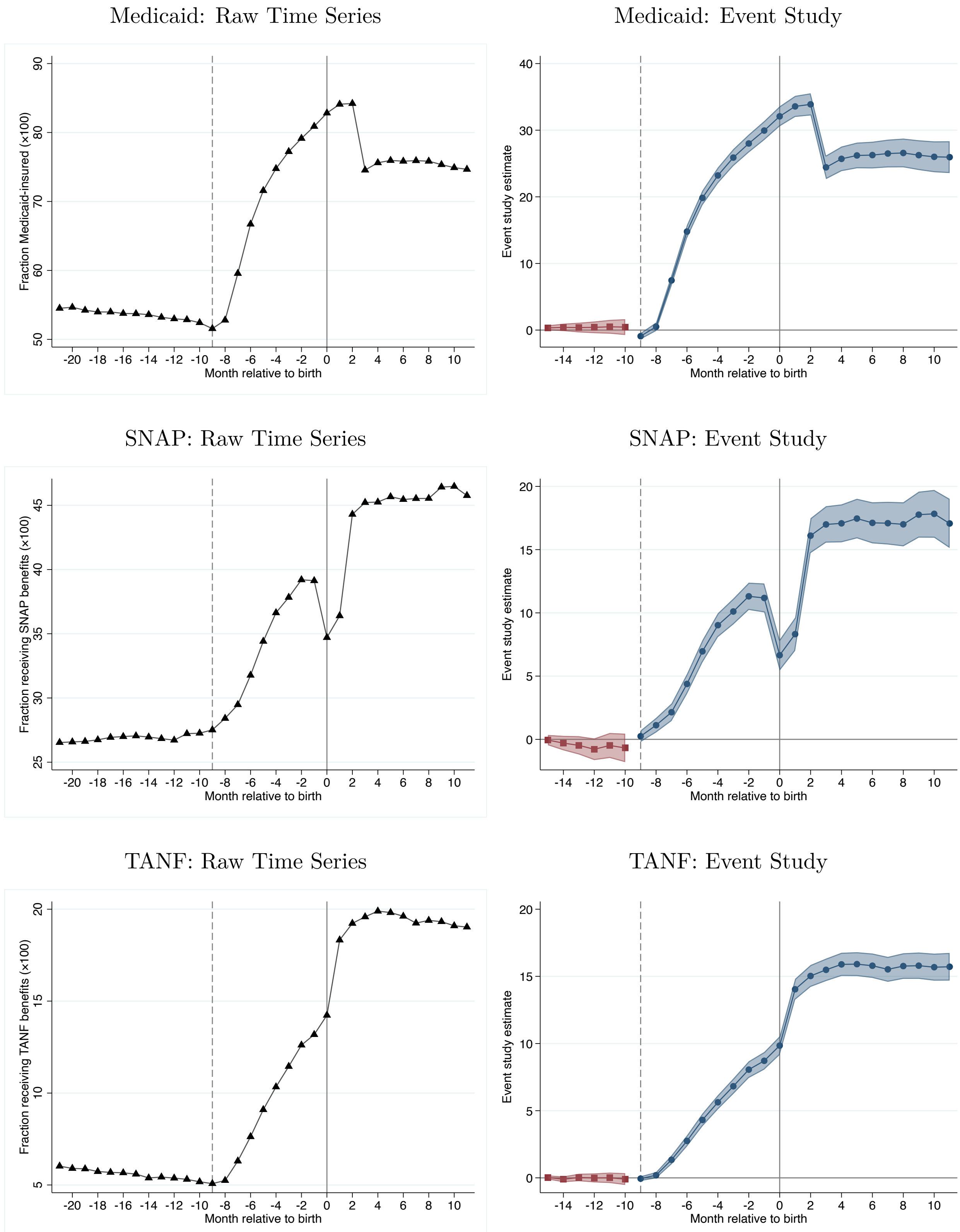
Notes: Figures show raw means of outcomes ×100, by month relative to first live birth event (left), by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right), for the main analysis sample of low SES first-time mothers detailed in Section 1.2. Event study estimates are based on outcome dummy multiplied by 100 for better readability. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. 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



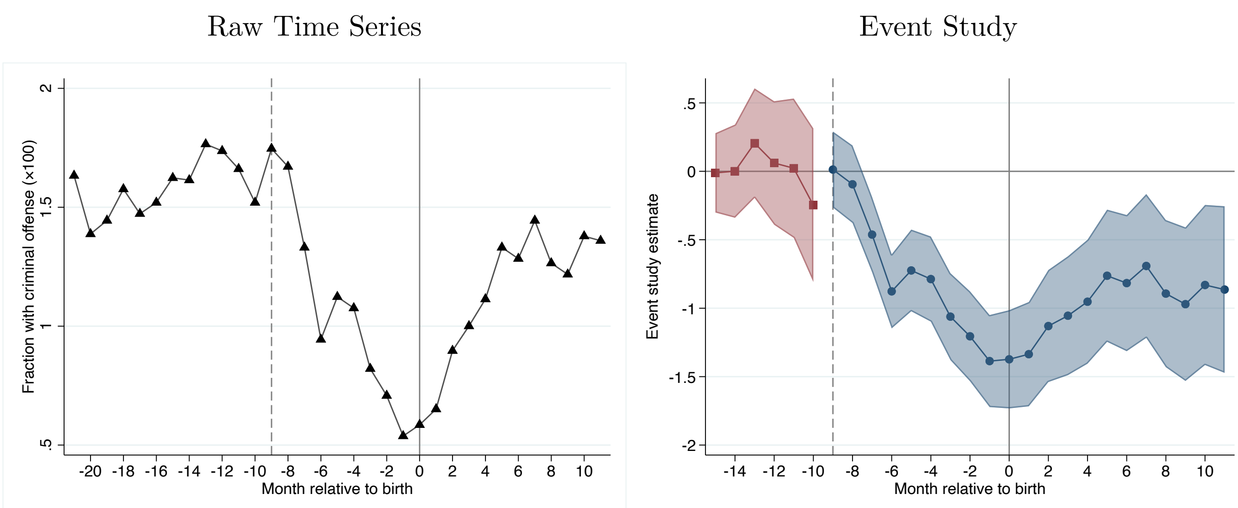
Notes: Figures show raw means of outcomes ×100, by month relative to first live birth event (left), by month relative to first live birth event (left) and event study estimates from the "imputation estimator" described in Section 2 (right). Estimates are based on restricted sample of low SES first-time mothers who were Medicaid-insured throughout the event time window. Event study estimates are based on outcome dummy multiplied by 100 for better readability. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. 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 ×100, by month relative to first live birth event (left), 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. Event study estimates are based on outcome dummy multiplied by 100 for better readability. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. 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 ×100, 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. Event study estimates are based on outcome dummy multiplied by 100 for better readability. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. 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, marital status of mom, 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).	2002-2019
Housing Assistance	All*	Month-level indicators for residence in public housing and for Section 8 voucher receipt (household-level).	2005-2019
Homelessness Services	All*	Date and length of 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 SUD treatment encounters (e.g. methadone receipt), inpatient stays in psychiatric hospitals and SUD 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, 2021*a*).

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

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

	mean
Age	26.546
Age 16-17	0.036
Black	0.159
White	0.799
Dad listed on birth certificate	0.861
Low SES	0.178
Medicaid insured in year before pregnancy	0.127
SNAP recipient in year before pregnancy	0.076
Any homeless encounter in year before pregnancy	0.002
Charged with crime in year before pregnancy	0.021
Any MHD encounter in year before pregnancy	0.024
Any SUD encounter in year before pregnancy	0.009
Months between births	43.442
Observations	22683

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

Appendix Table A.5: First vs. Second Live Birth Difference-in-Differences Results

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy (5th month) × 2nd childbirth	0.010	0.011	-0.051	0.044	-0.131*	-0.035	-5.035***	-0.992***	-0.598***	-0.066
	(0.023)	(0.025)	(0.068)	(0.073)	(0.070)	(0.059)	(0.298)	(0.224)	(0.146)	(0.079)
Post-birth (3rd month) × 2nd childbirth	-0.002	0.089**	-0.250**	0.217**	0.131	0.137*	-6.882***	-5.122***	-1.125***	0.192**
	(0.020)	(0.044)	(0.099)	(0.102)	(0.088)	(0.071)	(0.321)	(0.274)	(0.199)	(0.086)
2nd childbirth	-0.016	0.049	0.873***	-0.775***	-0.028	-0.082	9.396***	4.924***	3.124***	-0.313***
	(0.015)	(0.040)	(0.132)	(0.186)	(0.090)	(0.080)	(0.332)	(0.266)	(0.193)	(0.068)
Pregnancy (5th month)	0.006	0.002	0.080	-0.294***	0.133***	0.139***	11.026***	2.172***	1.424***	-0.134**
	(0.015)	(0.016)	(0.050)	(0.057)	(0.047)	(0.040)	(0.234)	(0.152)	(0.099)	(0.059)
Post-birth (3rd month)	-0.001	0.045	0.433***	-0.464***	0.223***	0.153***	12.099***	4.523***	4.018***	-0.240***
	(0.015)	(0.029)	(0.085)	(0.093)	(0.061)	(0.050)	(0.263)	(0.201)	(0.156)	(0.060)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age 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	0.022	0.150	1.475	3.031	0.441	0.293	13.808	8.753	2.301	0.327
Obs	1497078	1497078	1497078	1497078	1497078	1497078	1496789	1497078	1497078	1119558
N 2nd childbirths	22683	22683	22683	22683	22683	22683	22683	22683	22683	16963
N 1st childbirths	22683	22683	22683	22683	22683	22683	22683	22683	22683	16963

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

Appendix Table A.6: Event Study Results - Secondary Housing Outcomes

	(1) Public Housing (Head)	(2) Sec. 8 (Head)
Pregnancy effect	0.188***	-0.045
	(0.067)	(0.063)
Post-birth effect	1.723***	0.359**
	(0.184)	(0.151)
Year-month FE	Yes	Yes
Individual FE	Yes	Yes
Lin. event time control	Yes	Yes
Mean of dep. var	1.021	1.740
Obs	457309	457309
N individuals	12928	12928
Wald-statistic pre-trend p-value	0.437	0.350

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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.7: Event Study Results - Secondary Substance Use Disorder Outcomes

	(1) Opioid UD Medication	(2) Opioid UD Rehab	(3) Opioid UD Psychoth.	(4) Opioid UD Unspec: PsyTh/Medic	(5) Cannabis UD any treatment	(6) Alcohol UD any treatment	(7) Cocaine UD any treatment
Pregnancy effect	0.425***	-0.381**	0.407***	-0.095	-0.193	-0.132	0.016
	(0.149)	(0.179)	(0.138)	(0.108)	(0.231)	(0.093)	(0.067)
Post-birth effect	0.653*	-0.577	0.816***	-0.165	0.350	-0.065	0.088
	(0.336)	(0.353)	(0.249)	(0.204)	(0.487)	(0.214)	(0.134)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	1.142	0.331	0.368	0.295	0.663	0.184	0.110
Obs	97823	97823	97823	97823	97823	97823	97823
N individuals	2715	2715	2715	2715	2715	2715	2715
Wald-statistic pre-trend p-value	0.262	0.947	0.378	0.502	0.048	0.097	0.539

Notes: Table shows treatment effect estimates obtained from the “imputation estimator” described in Section 2. “UD” stands for “Use Disorder”. Estimates are based on restricted sample of low SES first-time mothers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.8: 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	-0.445***	-0.442***	-0.048	-0.167***	-0.087**	-0.063**	-0.100*
	(0.095)	(0.116)	(0.042)	(0.050)	(0.044)	(0.025)	(0.051)
Post-birth effect	-0.627***	-0.418**	-0.008	-0.275***	-0.180**	-0.097*	-0.101
	(0.184)	(0.208)	(0.082)	(0.095)	(0.084)	(0.050)	(0.097)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	1.076	1.066	0.217	0.236	0.255	0.085	0.283
Obs	380254	269110	380254	380254	380254	380254	380254
N individuals	10593	7225	10593	10593	10593	10593	10593
Wald-statistic pre-trend p-value	0.193	0.425	0.356	0.550	0.330	0.565	0.778

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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Coefficient estimates and standard errors are multiplied by 100 for better readability. 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 for All First-Time Mothers

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.020***	0.000	0.028	-0.021	0.068***	0.084***	7.607***	1.654***	0.855***	-0.182***
	(0.006)	(0.009)	(0.018)	(0.021)	(0.024)	(0.017)	(0.100)	(0.070)	(0.035)	(0.025)
Post-birth effect	0.015	0.042*	0.321***	0.084*	0.250***	0.182***	13.195***	4.720***	3.185***	-0.247***
	(0.010)	(0.023)	(0.046)	(0.047)	(0.048)	(0.034)	(0.189)	(0.147)	(0.079)	(0.047)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.018	0.106	0.928	2.268	0.334	0.204	8.620	4.944	0.985	0.416
Obs	2813499	2813499	2813499	2813499	2813499	2813499	2810029	2813499	2813499	2308764
N individuals	79457	79457	79457	79457	79457	79457	79457	79457	79457	64162
Wald-statistic pre-trend p-value	0.729	0.208	0.357	0.273	0.737	0.654	0.777	0.483	0.297	0.186

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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

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

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Medicaid	(6) SNAP	(7) TANF	(8) Criminal offense
Pregnancy effect	0.086***	-0.029	0.088	-0.096	17.173***	6.053***	4.129***	-0.751***
	(0.030)	(0.052)	(0.094)	(0.109)	(0.454)	(0.373)	(0.188)	(0.119)
Post-birth effect	0.072	0.115	1.339***	0.327	28.801***	14.733***	14.854***	-0.978***
	(0.055)	(0.125)	(0.233)	(0.246)	(0.922)	(0.776)	(0.410)	(0.227)
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.100	0.572	4.684	11.798	48.974	28.087	5.513	1.729
Obs	495081	495081	495081	495081	494491	495081	495081	413472
N individuals	13985	13985	13985	13985	13985	13985	13985	11512
Wald-statistic pre-trend p-value	0.763	0.270	0.446	0.251	0.752	0.391	0.284	0.248

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sample of individuals who have been enrolled in Medicaid or SNAP at any point in the five years leading up to conception. Substance use disorder-related results are omitted because the continuously Medicaid insured subsample precisely equals the one from the main results reported in Table 3. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.11: 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 UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.115**	-0.052	0.088	-0.299*	0.242	0.254	16.441***	3.597***	5.451***	-0.954***
	(0.051)	(0.095)	(0.142)	(0.175)	(0.393)	(0.230)	(0.593)	(0.649)	(0.324)	(0.179)
Post-birth effect	0.085	0.159	1.314***	-0.076	1.576*	0.712	27.252***	6.361***	19.072***	-1.184***
	(0.094)	(0.225)	(0.348)	(0.387)	(0.858)	(0.520)	(1.177)	(1.347)	(0.684)	(0.340)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.150	0.954	6.392	16.492	2.921	1.798	54.586	53.537	9.963	2.180
Obs	263672	263672	263672	263672	64954	64954	263290	263672	263672	234962
N individuals	7337	7337	7337	7337	1780	1780	7337	7337	7337	6467
Wald-statistic pre-trend p-value	0.984	0.234	0.841	0.432	0.106	0.312	0.401	0.215	0.267	0.215

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sub-sample of low SES women who have a service encounter in both the year before and the year after the event time window, and the year after been enrolled in SNAP at any point in the five years leading up to conception. Columns (5)-(6) further restrict to sample of first-time mothers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.12: Event Study Results with Childhood Medicaid Low SES Criterion

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.084***	0.023	0.131	-0.060	0.044	0.288**	18.442***	6.651***	3.990***	-0.667***
	(0.029)	(0.049)	(0.097)	(0.114)	(0.301)	(0.144)	(0.445)	(0.366)	(0.194)	(0.117)
Post-birth effect	0.080	0.121	1.513***	0.422*	1.131*	0.617**	33.536***	16.994***	14.577***	-0.871***
	(0.051)	(0.117)	(0.240)	(0.256)	(0.656)	(0.305)	(0.882)	(0.758)	(0.422)	(0.221)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.086	0.562	4.823	11.801	1.753	0.797	46.468	24.561	5.221	1.629
Obs	455424	455424	455424	455424	89662	89662	454813	455424	455424	391074
N individuals	12813	12813	12813	12813	2510	2510	12813	12813	12813	10863
Wald-statistic pre-trend p-value	0.635	0.348	0.639	0.126	0.241	0.575	0.619	0.573	0.229	0.241

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sample of individuals who were enrolled in Medicaid at any point before their 21st birthday (but before their first pregnancy). Columns (5) and (6) are restricted to sub-sample of continously Medicaid-insured 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.13: Robustness to In-/Out-Migration I: Event Study Results for Sub-Sample with Local Service Records Before and After Event Time Window

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.105***	0.001	0.131	-0.047	0.067	0.356**	19.125***	7.250***	5.025***	-0.768***
	(0.039)	(0.068)	(0.125)	(0.144)	(0.305)	(0.172)	(0.551)	(0.465)	(0.249)	(0.152)
Post-birth effect	0.070	0.180	1.653***	0.476	1.151*	0.718*	37.371***	18.152***	17.781***	-0.944***
	(0.069)	(0.163)	(0.305)	(0.323)	(0.677)	(0.387)	(1.110)	(0.968)	(0.541)	(0.289)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.122	0.745	6.100	15.351	2.578	1.510	61.567	33.609	6.987	2.045
Obs	350838	350838	350838	350838	97823	97823	350344	350838	350838	291933
N individuals	9804	9804	9804	9804	2715	2715	9804	9804	9804	8019
Wald-statistic pre-trend p-value	0.696	0.139	0.191	0.317	0.101	0.875	0.612	0.455	0.154	0.096

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sub-sample of low SES individuals who have a Allegheny DHS service encounter (that is, a Medicaid claim, court record, housing record, or welfare benefit record) in both the year before and the year after the event time window. Columns (5)-(6) further restrict to sample of first-time mothers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.14: Robustness to In-/Out-Migration II: Event Study Results for Sub-Sample with Local Service Record in Childhood

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.102***	0.071	0.185	-0.127	-0.305	0.144	14.714***	7.460***	4.726***	-0.749***
	(0.038)	(0.079)	(0.139)	(0.174)	(0.341)	(0.132)	(0.610)	(0.553)	(0.312)	(0.166)
Post-birth effect	0.093	0.171	1.914***	0.599	0.464	0.391	29.141***	18.467***	17.752***	-0.920***
	(0.067)	(0.189)	(0.340)	(0.388)	(0.725)	(0.282)	(1.241)	(1.125)	(0.667)	(0.310)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.085	0.854	6.491	15.758	1.506	0.415	61.794	31.359	7.203	1.940
Obs	251951	251951	251951	251951	69791	69791	251595	251951	251951	232778
N individuals	7025	7025	7025	7025	1926	1926	7025	7025	7025	6444
Wald-statistic pre-trend p-value	0.748	0.214	0.333	0.295	0.090	0.154	0.791	0.894	0.220	0.104

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sub-sample of low SES individuals who have a Allegheny DHS service encounter (that is, a Medicaid claim, court record, housing record, or welfare benefit record) before age 17, and ahead of the event time window. Columns (5)-(6) further restrict to sample of first-time mothers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.15: Robustness to In-/Out-Migration III: Event Study Results for Sub-Sample Born in Pennsylvania

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.079***	0.002	0.160	-0.003	0.155	0.350**	16.608***	6.540***	4.317***	-0.741***
	(0.030)	(0.058)	(0.108)	(0.124)	(0.317)	(0.175)	(0.509)	(0.404)	(0.213)	(0.134)
Post-birth effect	0.059	0.145	1.564***	0.592**	1.383**	0.733*	28.673***	16.186***	15.545***	-0.952***
	(0.053)	(0.139)	(0.265)	(0.278)	(0.694)	(0.394)	(1.037)	(0.846)	(0.466)	(0.257)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.079	0.597	4.916	12.492	2.517	1.438	53.735	27.662	5.610	1.795
Obs	401703	401703	401703	401703	89894	89894	401235	401703	401703	332799
N individuals	11391	11391	11391	11391	2503	2503	11391	11391	11391	9303
Wald-statistic pre-trend p-value	0.899	0.166	0.451	0.217	0.061	0.871	0.658	0.241	0.143	0.319

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for the sub-sample of low SES individuals who were born in Pennsylvania (information that is recorded on their child's birth record). Columns (5)-(6) further restrict to sample of first-time mothers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.16: 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 UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.148***	0.064	0.169	-0.004	-0.116	0.046	16.373***	6.017***	4.241***	-0.962***
	(0.046)	(0.077)	(0.144)	(0.175)	(0.468)	(0.256)	(0.687)	(0.554)	(0.259)	(0.210)
Post-birth effect	0.181**	0.274*	1.568***	0.493	0.920	0.157	27.582***	15.003***	15.085***	-1.405***
	(0.082)	(0.158)	(0.303)	(0.332)	(0.900)	(0.479)	(1.282)	(1.062)	(0.525)	(0.388)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.093	0.557	4.579	11.959	2.799	1.621	53.728	27.050	5.593	1.624
Obs	411587	411587	411587	411587	87872	87872	411339	411587	411587	341537
N individuals	12928	12928	12928	12928	2715	2715	12928	12928	12928	10593
Wald-statistic pre-trend p-value	0.489	0.506	0.638	0.142	0.094	0.805	0.461	0.776	0.209	0.704

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. Columns (5)-(6) restrict to subsample of first-time mothers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) 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 and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.17: 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 UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.079***	-0.007	0.105	-0.034	0.092	0.341***	16.496***	6.299***	4.204***	-0.667***
	(0.023)	(0.038)	(0.080)	(0.091)	(0.193)	(0.121)	(0.349)	(0.268)	(0.158)	(0.088)
Post-birth effect	0.062	0.140	1.444***	0.426**	1.206**	0.685**	27.721***	15.593***	15.056***	-0.827***
	(0.043)	(0.101)	(0.207)	(0.206)	(0.495)	(0.288)	(0.774)	(0.614)	(0.367)	(0.190)
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
Lin. event time control	No	No	No	No	No	No	No	No	No	No
Mean of dep. var	0.108	0.580	4.749	11.850	2.578	1.510	52.978	26.717	5.376	1.737
Obs	457309	457309	457309	457309	97823	97823	456756	457309	457309	380254
N individuals	12928	12928	12928	12928	2715	2715	12928	12928	12928	10593
Wald-statistic pre-trend p-value	0.611	0.382	0.435	0.262	0.105	0.897	0.885	0.371	0.147	0.162

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2 but omitting the control for the pre-trend in event time, for our baseline analysis sample of low-SES first-time mothers detailed in Section 1.2. Columns (5)-(6) restrict to subsample of first-time mothers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.18: 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 UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	0.070***	0.003	0.079	-0.379***	-0.240	0.292**	13.089***	5.060***	2.455***	-0.590***
	(0.023)	(0.043)	(0.085)	(0.096)	(0.179)	(0.117)	(0.319)	(0.264)	(0.167)	(0.071)
Post-birth effect	0.027	0.164***	1.388***	-0.268**	0.620**	0.580***	21.022***	12.599***	11.272***	-0.670***
	(0.030)	(0.060)	(0.138)	(0.135)	(0.290)	(0.189)	(0.405)	(0.371)	(0.289)	(0.097)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.108	0.580	4.749	11.850	2.578	1.510	52.978	26.717	5.376	1.737
Obs	426624	426624	426624	426624	89595	89595	426624	426624	426624	349569
N individuals	12928	12928	12928	12928	2715	2715	12928	12928	12928	10593

Notes: Table shows treatment effect estimates obtained from a standard two-way fixed effect estimator obtained from the following OLS model: $Y_{it} = \beta_0 + \beta_1 \times Preg_{it} + \beta_2 \times Post_{it} + \mu_i + \gamma_{y(it)} + \epsilon_{it}$, where i denotes individual and t denotes calendar year-month. The regression includes controls for individual fixed effects (μ_i) and calendar year fixed effects ($\gamma_{y(it)}$). It is estimated off of the "live birth event study" sample detailed in Section 1.2. Columns (5)-(6) restrict to subsample of first-time mothers who were Medicaid-insured throughout the event time window. "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 ($\times 100$) twelve months before childbirth. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.19: Summary Statistics for Matched DiD Sample

	Low SES Sample		Full Sample	
	(1)	(2)	(3)	(4)
	"Control" mean	"Treated" mean	"Control" mean	"Treated" mean
Age	21.214	21.241	26.639	26.654
Age 16-17	0.127	0.134	0.030	0.032
Black	0.524	0.524	0.144	0.144
White	0.468	0.468	0.798	0.798
Medicaid insured in year before pregnancy	0.658	0.719	0.102	0.106
SNAP recipient in year before pregnancy	0.338	0.390	0.057	0.068
Any homeless encounter in year before pregnancy	0.012	0.019	0.002	0.003
Charged with crime in year before pregnancy	0.090	0.121	0.020	0.028
Any MHD encounter in year before pregnancy	0.121	0.138	0.021	0.023
Any SUD encounter in year before pregnancy	0.036	0.051	0.006	0.008
Observations	9267	9267	62638	62638

Notes: Table shows summary statistics for the sample of women entering the matched difference-in-differences analysis detailed in Appendix C. Observations are at the individual-event level (note that an individual can enter both in the treated group and the control group). Columns (1) and (3) pertain to women with a first live birth in the sample period who are matched to a woman in the "treated" sample based on own year of birth, race, Medicaid history (ahead of the treated peer's pregnancy), as well as a childbirth date such that it falls two years after that of the matched treated peer. Outcomes are measured as of month of the treated peer's first childbirth (or pregnancy) event, as noted. Low SES is dummy that equals 1 if (matched) treated peer is observed as Medicaid-insured at any point in the five years preceding pregnancy.

Appendix Table A.20: Matched DiD Results - Low SES Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Homeless shelter	Long-term homeless	Public Housing	Sec. 8	Any SUD treatment	Opioid UD treatment	Medicaid	SNAP	TANF	Criminal offense
Pregnancy (5th month)	0.043	0.119	-0.054	0.054	-0.054	0.194	21.614***	7.511***	3.572***	-0.722***
	(0.063)	(0.077)	(0.144)	(0.174)	(0.198)	(0.146)	(0.648)	(0.596)	(0.303)	(0.267)
Post-birth (3rd month)	0.054	0.205*	1.360***	0.367	0.971***	0.939***	29.244***	18.528***	14.266***	-0.812***
	(0.067)	(0.117)	(0.235)	(0.255)	(0.223)	(0.165)	(0.763)	(0.711)	(0.449)	(0.264)
Mean of dep. var	0.086	0.610	4.613	12.491	1.538	0.858	54.003	25.353	5.331	1.637
Obs	389214	389214	389214	389214	389214	389214	389214	389214	389214	325794
N treated individuals	9267	9267	9267	9267	9267	9267	9267	9267	9267	7757
N control individuals	9267	9267	9267	9267	9267	9267	9267	9267	9267	7757

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 treated-control dyads in which the treated peer satisfies the low SES criterion (that is, is observed as Medicaid-insured in at least one month of the five years preceding pregnancy). "Mean of dep. var" gives the mean of the dependent variable ($\times 100$) 12 months before treated peer's childbirth. Coefficient estimates and standard errors are multiplied by 100 for better readability. Cluster-robust standard errors clustered at the individual-by-treatment level are shown in parentheses. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.21: Matched DiD Results - Full Sample

	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy (5th month)	0.016	0.014	0.002	0.005	0.064*	0.086***	8.854***	1.881***	0.720***	-0.174***
	(0.011)	(0.012)	(0.024)	(0.029)	(0.033)	(0.026)	(0.139)	(0.102)	(0.050)	(0.049)
Post-birth (3rd month)	0.021*	0.051***	0.291***	0.065	0.330***	0.305***	12.282***	5.099***	2.939***	-0.179***
	(0.012)	(0.019)	(0.040)	(0.042)	(0.040)	(0.031)	(0.166)	(0.130)	(0.079)	(0.048)
Mean of dep. var	0.014	0.099	0.828	2.168	0.255	0.144	8.140	4.283	0.865	0.367
Obs	2630796	2630796	2630796	2630796	2630796	2630796	2630796	2630796	2630796	2223732
N treated individuals	62638	62638	62638	62638	62638	62638	62638	62638	62638	52946
N control individuals	62638	62638	62638	62638	62638	62638	62638	62638	62638	52946

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 ($\times 100$) 12 months before treated peer's childbirth. Coefficient estimates and standard errors are multiplied by 100 for better readability. Cluster-robust standard errors clustered at the individual-by-treatment level are shown in parentheses. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.22: Summary Statistics for Life Birth vs. Miscarriage DiD Sample

	Live birth mean	Miscarriage mean
Age	21.391	20.876
Age 16-17	0.074	0.126
Black	0.335	0.334
White	0.631	0.629
Low SES	0.387	0.399
Medicaid insured in year before pregnancy	0.272	0.337
SNAP recipient in year before pregnancy	0.166	0.195
Any homeless encounter in year before pregnancy	0.007	0.010
Charged with crime in year before pregnancy	0.059	0.104
Any MHD encounter in year before pregnancy	0.052	0.080
Any SUD encounter in year before pregnancy	0.018	0.022
(Also) has miscarriage	0.010	1.000
(Also) has live birth	1.000	0.276
Months between events	39.423	39.423
Observations	27329	1019

Notes: Table shows summary statistics for women in the sample for the difference-in-differences analysis comparing miscarriage events to live birth events as detailed in Appendix 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 nine months before the month of birth (for live birth events), and four months before the event (for miscarriage/non-live-birth events). "Months between events" is the number of months between the miscarriage event and the live birth event for the subset of women who enter the sample with two time series—one for each event.

Appendix Table A.23: 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 UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy × Live birth	0.066***	-0.013	0.223	0.042	-0.080	0.186	9.825***	3.110***	1.482***	-0.444
	(0.015)	(0.108)	(0.148)	(0.200)	(0.170)	(0.119)	(0.772)	(0.691)	(0.299)	(0.276)
Post-Pregn. × Live Birth	0.036	0.155	1.291***	0.579	0.113	0.429***	16.999***	9.676***	7.452***	-0.155
	(0.023)	(0.192)	(0.236)	(0.378)	(0.227)	(0.153)	(1.050)	(0.768)	(0.388)	(0.198)
Pregnancy	-0.019	0.030	-0.165	-0.256	0.225	0.046	1.933***	-0.442	-0.261	0.105
	(0.012)	(0.105)	(0.141)	(0.193)	(0.165)	(0.115)	(0.749)	(0.680)	(0.290)	(0.275)
Post-Pregnancy	-0.004	-0.085	-0.472**	-0.725**	0.428*	0.027	4.840***	-1.655**	-1.557***	-0.231
	(0.023)	(0.188)	(0.222)	(0.369)	(0.224)	(0.148)	(1.022)	(0.749)	(0.364)	(0.199)
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	0.042	0.261	2.381	5.715	0.554	0.282	21.102	11.849	2.723	1.008
Obs	929370	929370	929370	929370	929370	929370	929177	929370	929370	718218
N indiv.-event tuples	28348	28348	28348	28348	28348	28348	28348	28348	28348	21922

Notes: Table shows treatment effect estimates obtained from OLS estimation of difference-in-differences 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 (×100) two months before the approximate month of conception. Coefficient estimates and standard errors are multiplied by 100 for better readability. Cluster-robust standard errors clustered at the individual-event level are shown in parentheses. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.24: Demographic Characteristics of First-Time Fathers

	(1) Low SES First Time Fathers mean	(2) All Other First Time Fathers mean
Age	23.200	30.196
Age 16-17	0.050	0.003
Black	0.488	0.073
White	0.474	0.853
SNAP recipient in year before pregnancy	0.303	0.007
Any homeless encounter in year before pregnancy	0.011	0.000
Charged with crime in year before pregnancy	0.195	0.017
Any MHD encounter in year before pregnancy	0.087	0.001
Any SUD encounter in year before pregnancy	0.074	0.001
Observations	5046	55811

Notes: Table shows demographic characteristics of all men in Allegheny County at the time they first become parents, as identified via birth records. First-time parenthood is defined as: 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.25: 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 UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	-0.040	0.024	-0.038	-0.111	-1.035	-0.493	-2.374***	0.408	0.066	0.151
	(0.043)	(0.066)	(0.118)	(0.151)	(0.887)	(0.408)	(0.655)	(0.534)	(0.206)	(0.278)
Post-birth effect	-0.045	0.025	-0.101	0.039	-1.232	-0.367	-2.450*	0.164	0.806*	0.897*
	(0.082)	(0.134)	(0.289)	(0.338)	(1.808)	(0.823)	(1.445)	(1.141)	(0.435)	(0.531)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.079	0.416	2.874	8.482	4.936	2.377	41.062	20.115	2.735	3.096
Obs	179494	179494	179494	179494	20546	20546	179312	179494	179494	149398
N individuals	5046	5046	5046	5046	547	547	5046	5046	5046	4134
Wald-statistic pre-trend p-value	0.203	0.202	0.354	0.089	0.617	0.442	0.018	0.368	0.137	0.456

Notes: Table shows treatment effect estimates obtained from the "imputation estimator" described in Section 2, for low SES first-time fathers. Columns (5)-(6) restrict to subsample of first-time fathers who were Medicaid-insured throughout the event time window. 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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

Appendix Table A.26: Event Study Results for All Men

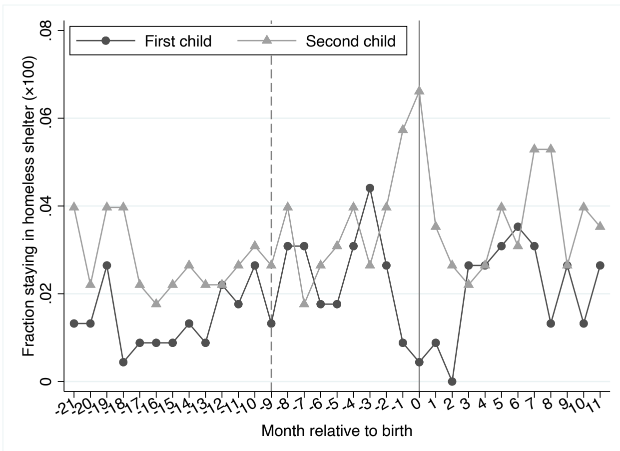
	(1) Homeless shelter	(2) Long-term homeless	(3) Public Housing	(4) Sec. 8	(5) Any SUD treatment	(6) Opioid UD treatment	(7) Medicaid	(8) SNAP	(9) TANF	(10) Criminal offense
Pregnancy effect	-0.002	0.004	-0.004	0.000	-0.049**	0.002	0.171***	0.130**	0.014	-0.048
	(0.005)	(0.006)	(0.011)	(0.014)	(0.024)	(0.014)	(0.058)	(0.052)	(0.018)	(0.032)
Post-birth effect	-0.000	0.006	-0.001	0.021	-0.000	0.066**	1.216***	0.561***	0.118***	0.010
	(0.009)	(0.013)	(0.026)	(0.032)	(0.047)	(0.029)	(0.130)	(0.111)	(0.039)	(0.060)
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
Lin. event time control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var	0.008	0.039	0.306	0.840	0.245	0.118	3.405	2.039	0.250	0.516
Obs	2160832	2160832	2160832	2160832	2160832	2160832	2158200	2160832	2160832	1775458
N individuals	60857	60857	60857	60857	60857	60857	60857	60857	60857	49179
Wald-statistic pre-trend p-value	0.282	0.293	0.781	0.281	0.713	0.817	0.021	0.399	0.326	0.143

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 ($\times 100$) twelve months before childbirth. The p-value of a Wald test statistic for a joint test of all six pre-conception month dummies being jointly equal to zero is reported in the last row. Cluster-robust standard errors clustered at the individual level are shown in parentheses. Coefficient estimates and standard errors are multiplied by 100 for better readability. Coefficient estimates with associated p-values < 0.01 (< 0.05) [< 0.1] are denoted by *** (**)[*].

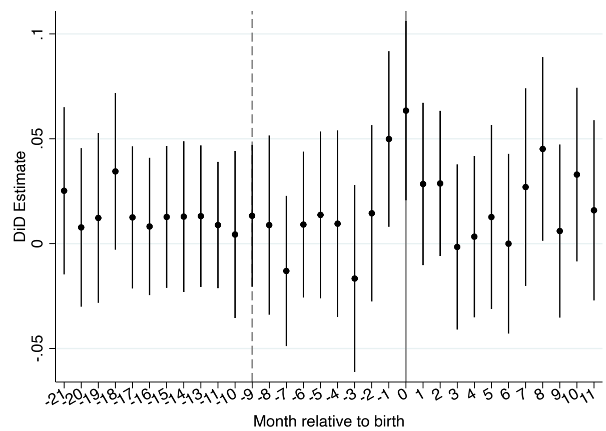
Appendix Figures

Appendix Figure A.1: Homelessness - First vs. Second Live Birth

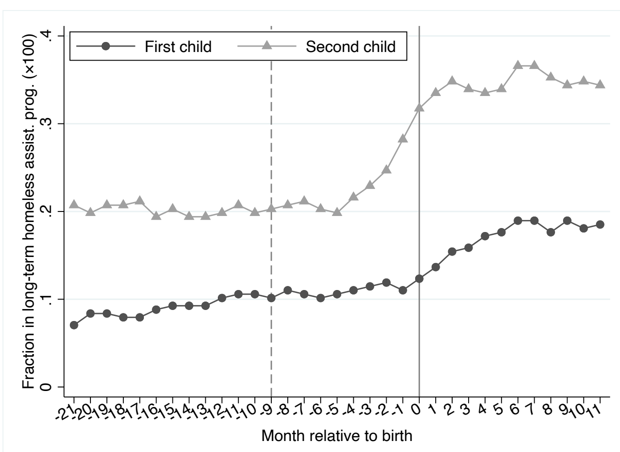
Homeless Shelter Stays
Raw Time Series



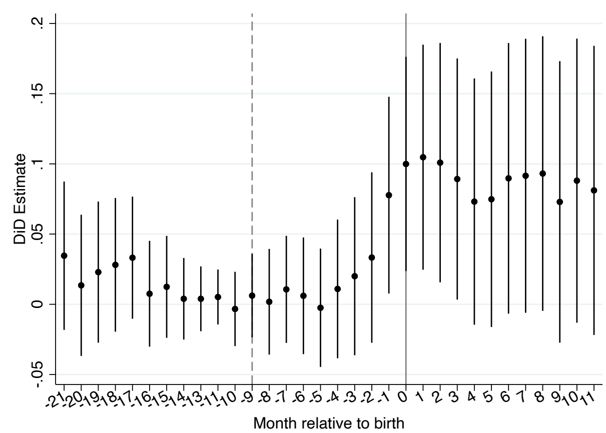
Homeless Shelter Stays
DiD Results



Medium/long-term homelessness assistance
Raw Time Series

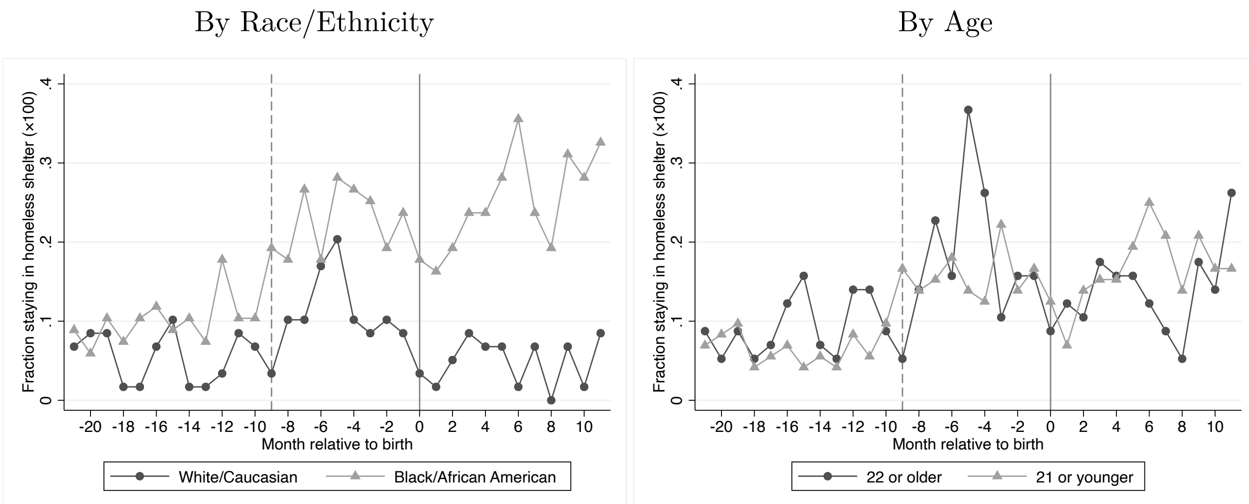


Medium/long-term homelessness assistance
DiD Results



Notes: Figures show raw means of outcomes by month relative to birth event (left) and estimates from a DiD regression (right). Based on outcome dummies multiplied by 100 for better readability. Sample is restricted to women with a first and second live birth in the sample period that are at least 24 months apart ($N = 22,890$ individuals). Right figures report treatment effect estimates on interaction coefficients of second birth dummy and relative event time dummies, from the following event study specification: $y_{ijr} = \alpha + \sum_{r\neq -12}(\gamma_r \tau_r + \beta_r \tau_r T_{ij}) + \nu T_{ij} + \eta X_{ijr} + \epsilon_{ijt}$; where r is month relative to the month of childbirth, i is individual, and j denotes the series (either first or second birth). τ_r denotes relative event time dummies, T_{ij} is an indicator that equals one if the observation pertains to a second birth, and X_{ijr} is a set of controls (individual FE, age FE, and calendar year FE). Only observations in the event time window ($-21 \leq r \leq 11$) are included. The objects of interest are the β_r's. They provide an estimate of the deviation from the baseline difference in outcomes between second and first childbirth, at every month relative to childbirth. Vertical dotted line shows approximate month of conception. Vertical solid line shows month of childbirth. 95% confidence bars based on cluster-robust standard errors clustered at the individual-by-birth level are also shown. See Table A.5 for DiD estimation results in table-form.

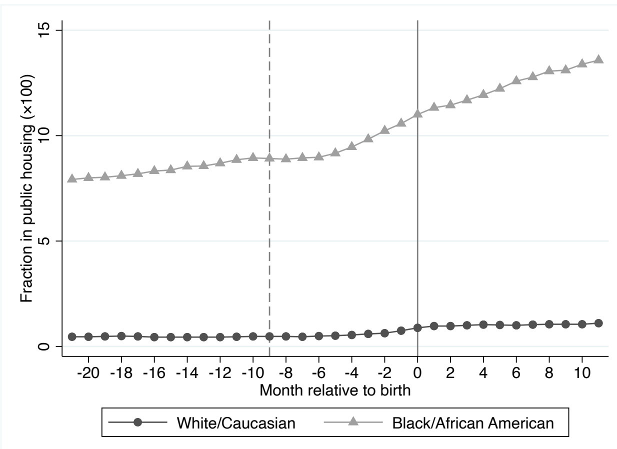
Appendix Figure A.2: Heterogeneity in Homeless Shelter Stays by Race/Ethnicity and Age



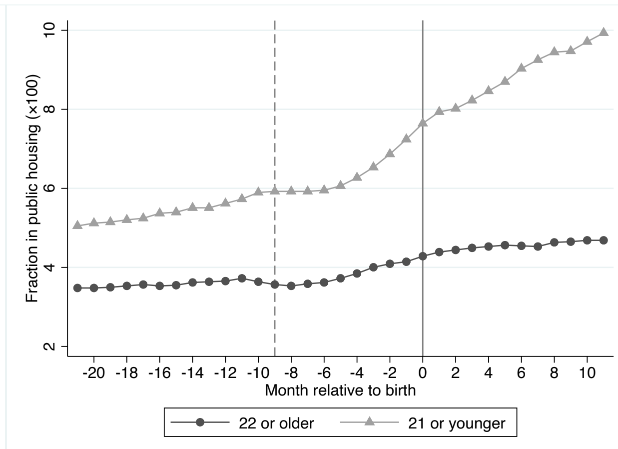
Notes: Figures show raw means of dummy for stay at homeless shelter (×100), by month relative to first live birth event, separately by age/ethnicity and by above/below median age at childbirth (median is 22 years). Vertical dotted line shows approximate month of conception. Vertical solid line shows 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: Heterogeneity in Long-term housing Program Enrollment by Race/Ethnicity and Age

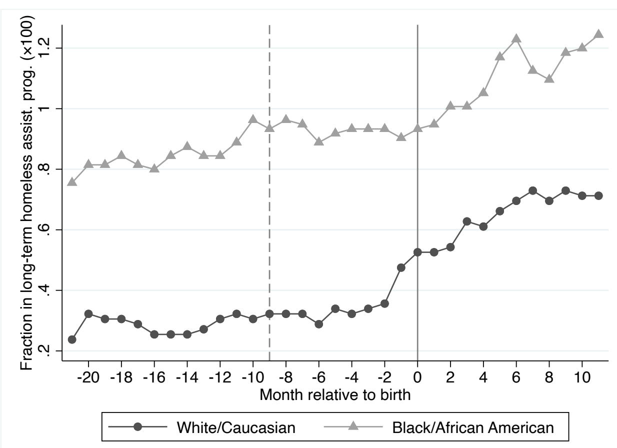
Public Housing: By race/ethnicity



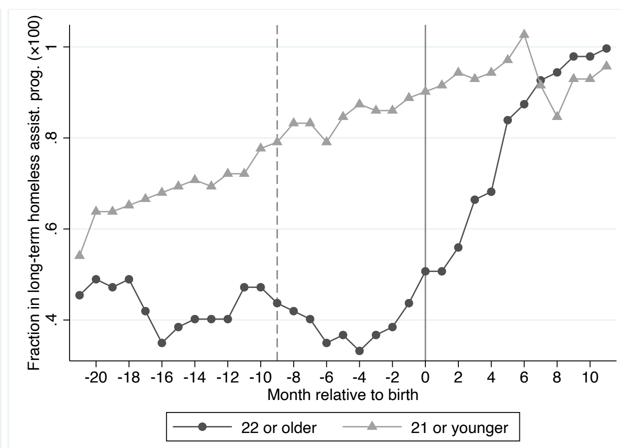
Public Housing: By Age



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

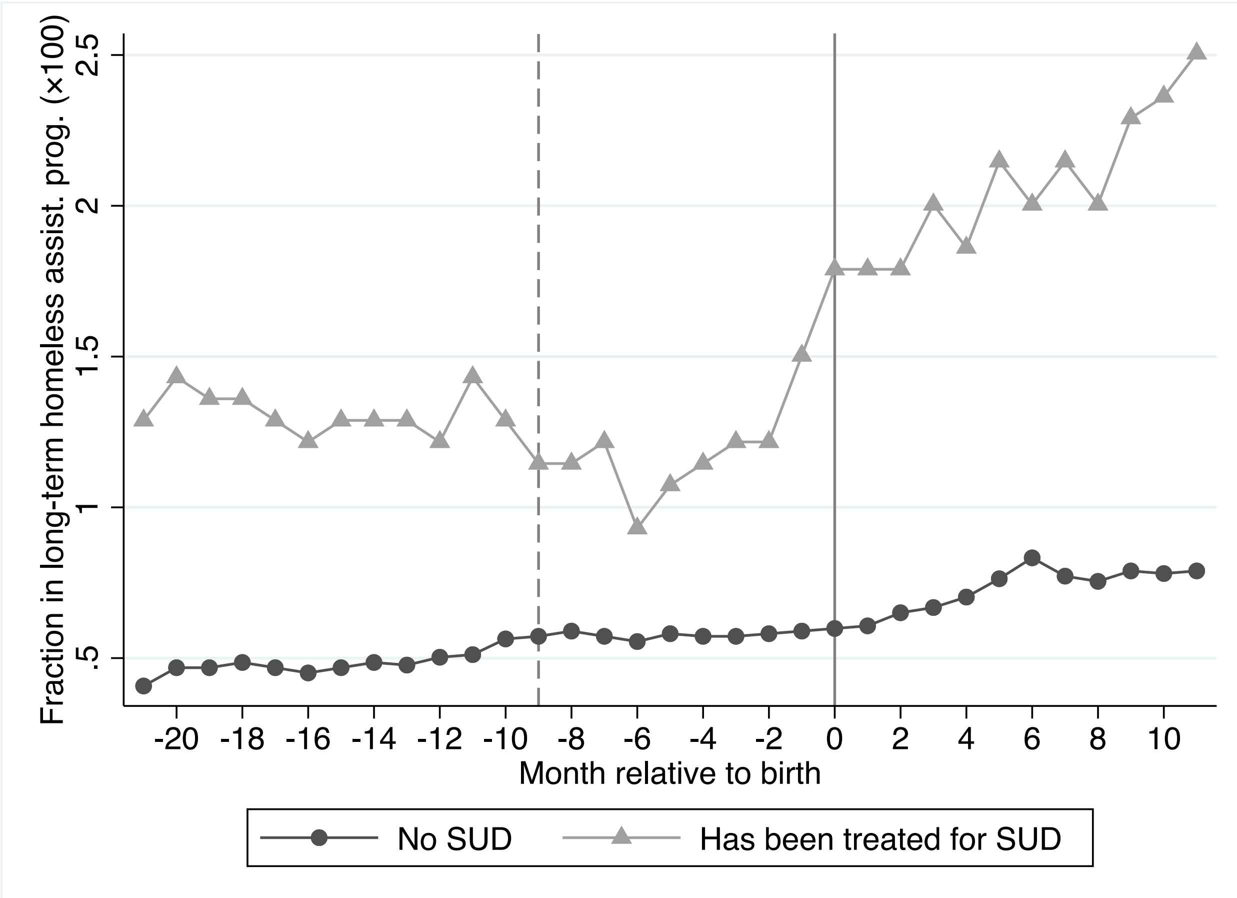


Medium/Long-Term Homelessness Assistance: By Age



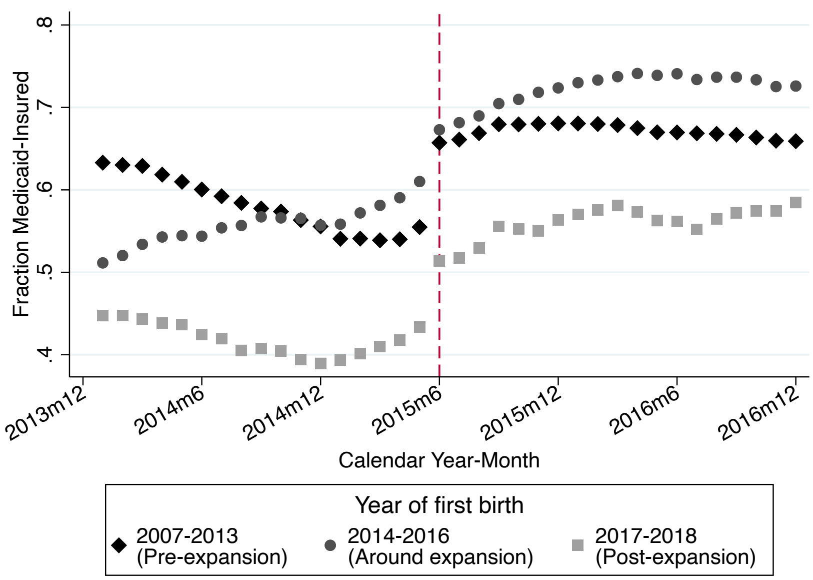
Notes: Figures show raw means of outcomes ($\times 100$), by month relative to first live birth event, separately by age/ethnicity and by above/below median age at childbirth (median is 22 years). Vertical dotted line shows approximate month of conception. Vertical solid line shows month of birth. Sample is restricted to first life birth event to mothers identified as low SES, as detailed in Section 1.2.

Appendix Figure A.4: Medium/Long-Term Homelessness Assistance: Heterogeneity by Substance Use Disorder



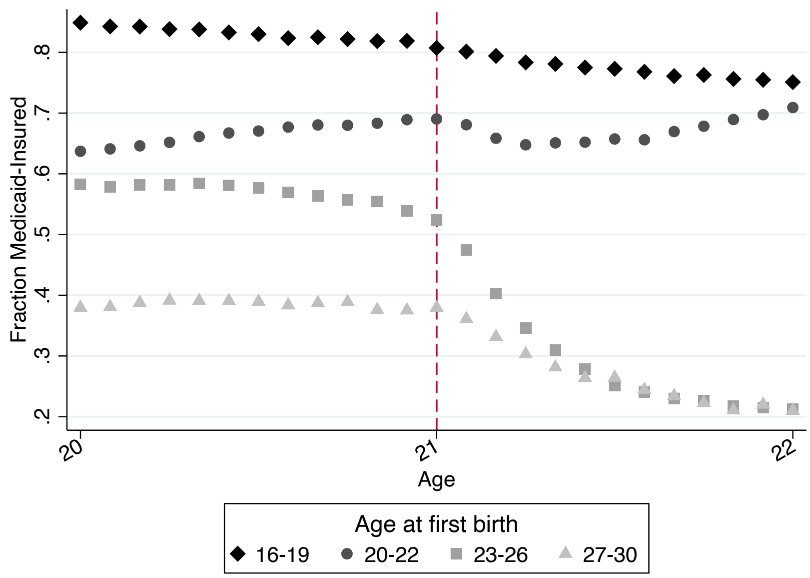
Notes: Figure shows raw mean of dummy for enrollment in long-term homeless assistance program (×100), by month relative to first live birth event. "No SUD" ("Has been treated for SUD") refers to sample of women with no (at least one encounter for) treatment for substance use disorder observed at any point before approximate commencement of the pregnancy. Sample sizes are 11,531 and 1,397, respectively. 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.5: Impact of Medicaid-Expansion on Medicaid Insurance Enrollment



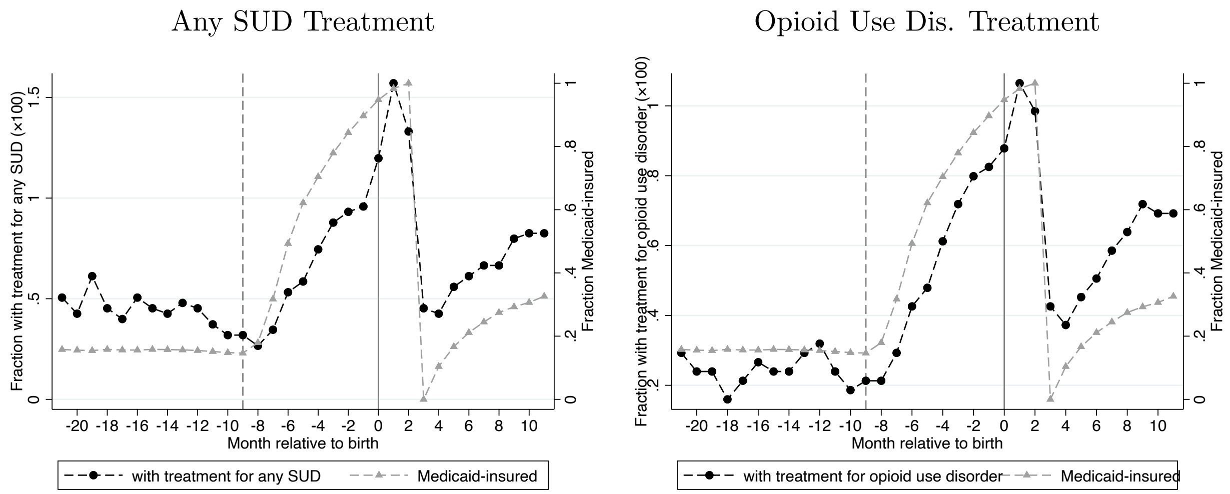
Notes: Figure shows time series of the fraction of women who are Medicaid insured in the years around the ACA-expansion. Separately for 3 sub-samples: those who had their first child pre-expansion, those who had it in the years surrounding the expansion, and those who had it post-expansion. The dashed 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).

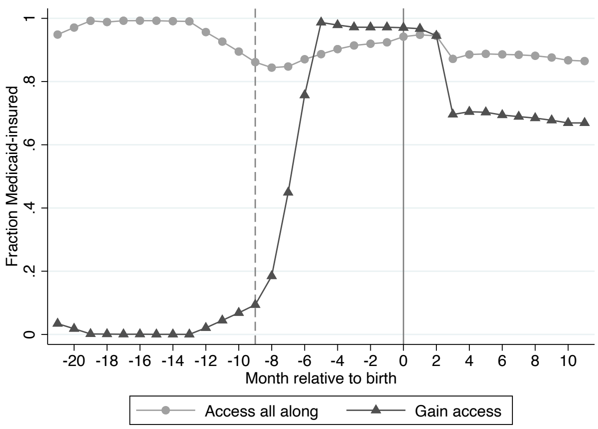
Appendix Figure A.7: Substance Use Disorder Treatment and Loss of Medicaid at 60 Days Postpartum



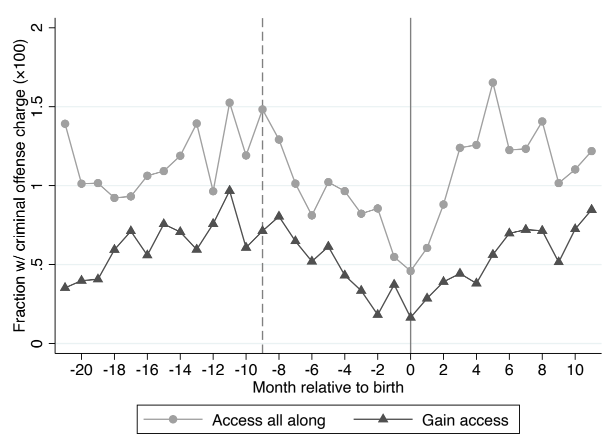
Notes: Figures show raw means of outcomes by month relative to childbirth for the sub-sample of women who lose Medicaid-coverage at three months postpartum, when stricter income eligibility rules come into effect. Sample size is 3,757 individuals, 36.7% of whom are in our low SES sample. Dark dots represent fraction receiving any SUD treatment (left panel), and fraction receiving opioid use disorder treatment (right panel), respectively (both are multiplied by 100 for better readability). 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

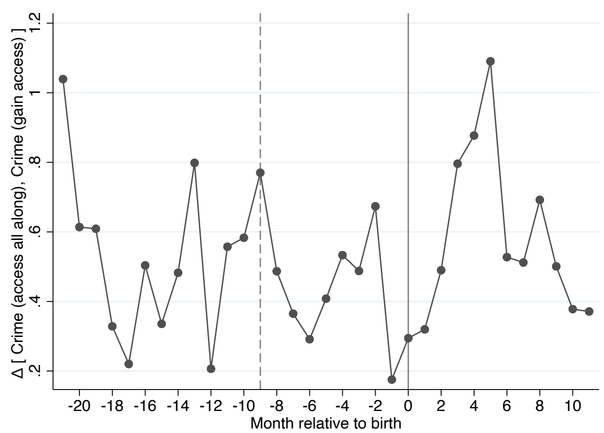
(A) Raw Means: Medicaid enrollment



(B) Raw Means: Criminal Behavior



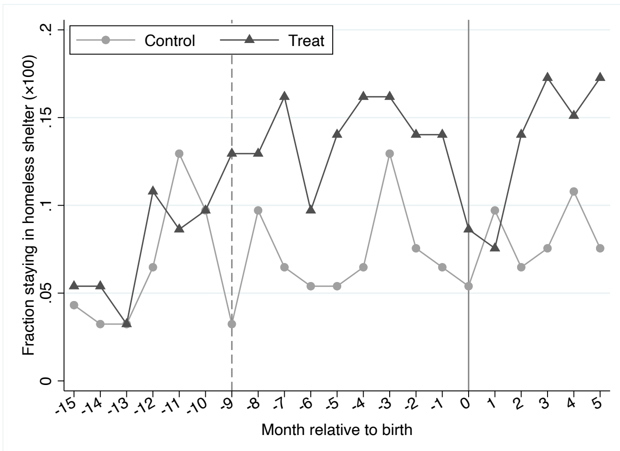
(C) Difference-In-Means: Criminal Behavior



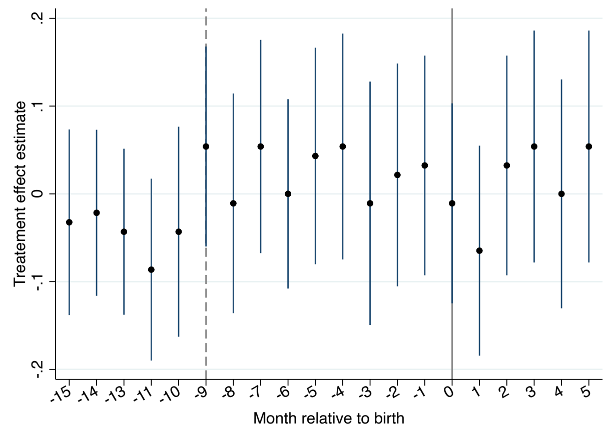
Notes: Figures (A) and (B) show means of Medicaid enrollment dummy and criminal offense charge dummy (×100), respectively, by month relative to first childbirth event, separately for two sub-samples: women who were continuously enrolled in Medicaid in the year pre-pregnancy ("Access all along", $N = 3,805$), and women who enrolled at some point in the first five months of pregnancy, but not before ("Gained acess", $N = 4,401$). The two groups are matched based on year of childbirth, year of own birth, and race (only demographic cells with at least 2 individuals per gain access and per access all along group are kept), as follows: means for each relative time period are computed for each demographic cell-by-access group separately, and then averaged across demographic cells within an access group and relative time period by using weights equal to the total number of individuals in a demographic cell. Panel (C) shows the difference between the access all along average (×100) and the gain access average (×100) from panel (B), for each relative time period. Vertical dotted line shows approximate time of conception. Vertical solid line shows time of birth. Sample is restricted to women with first life birth events in the sample period (with no restriction on SES).

Appendix Figure A.9: Homelessness - Matched DiD Results

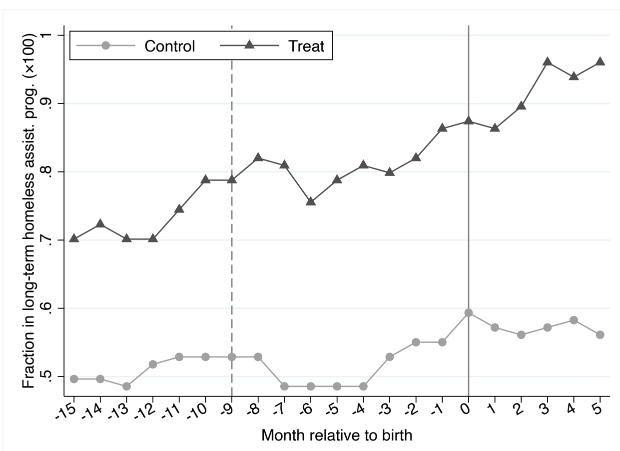
Homeless Shelter Stays: Raw Time Series



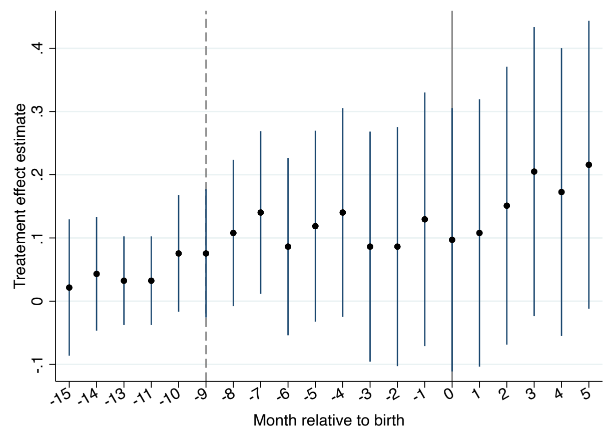
Homeless Shelter Stays: DiD Results



Medium/long-term homelessness assistance: Raw Time Series



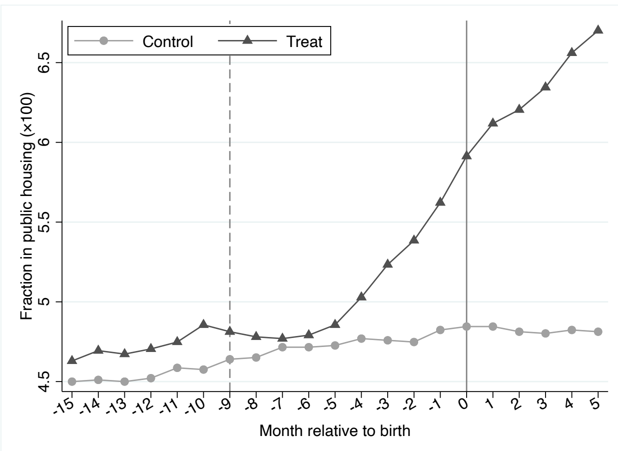
Medium/long-term homelessness assistance: DiD Results



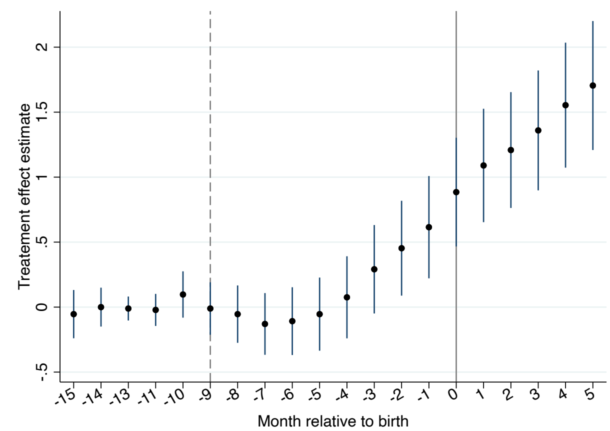
Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. All estimates are based on outcome dummy multiplied by 100 for better readability. 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 treated-control dyads in which the treated peer satisfies the low SES criterion (that is, is observed as Medicaid-insured in at least one month of the five years preceding pregnancy). 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

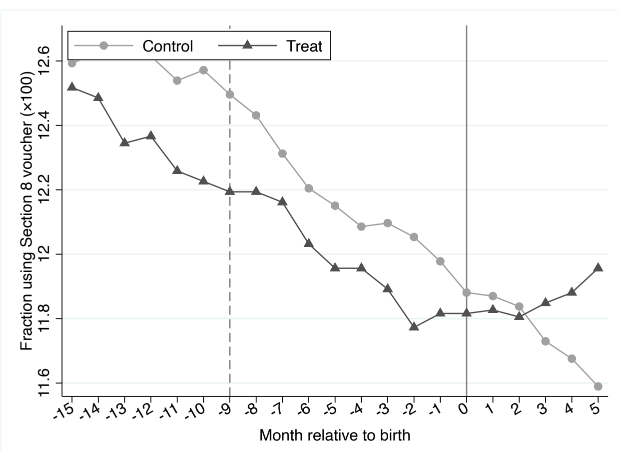
Public Housing Residence: Raw Time Series



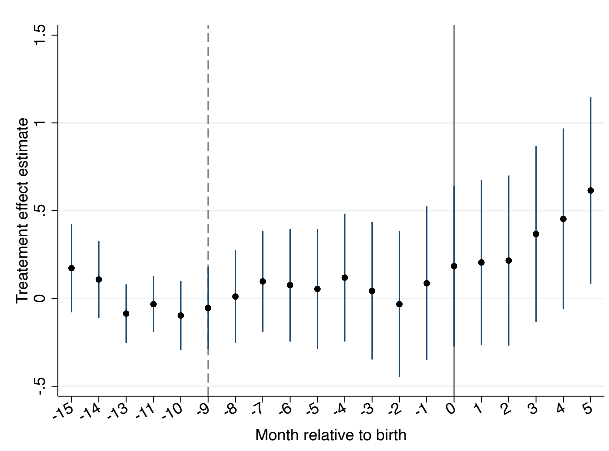
Public Housing Residence: DiD Results



Section 8 Voucher Use: Raw Time Series



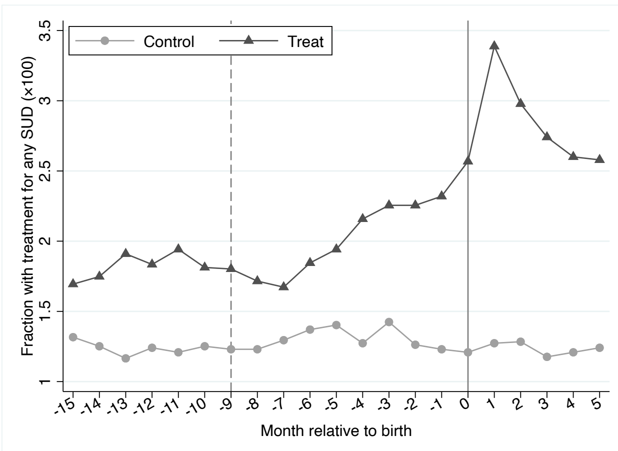
Section 8 Voucher Use: DiD Results



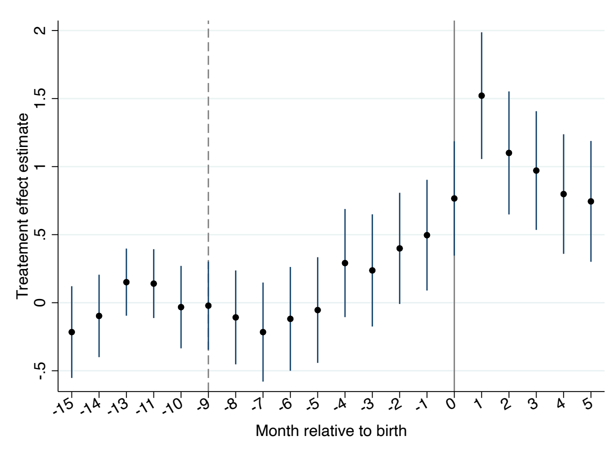
Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. All estimates are based on outcome dummy multiplied by 100 for better readability. 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 treated-control dyads in which the treated peer satisfies the low SES criterion (that is, is observed as Medicaid-insured in at least one month of the five years preceding pregnancy). 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

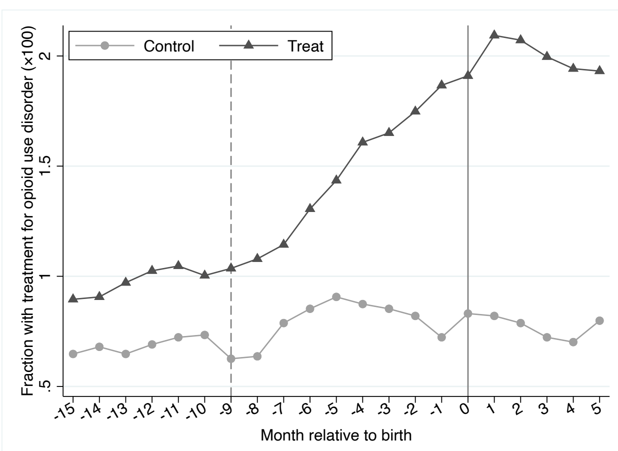
Any SUD Treatment: Raw Time Series



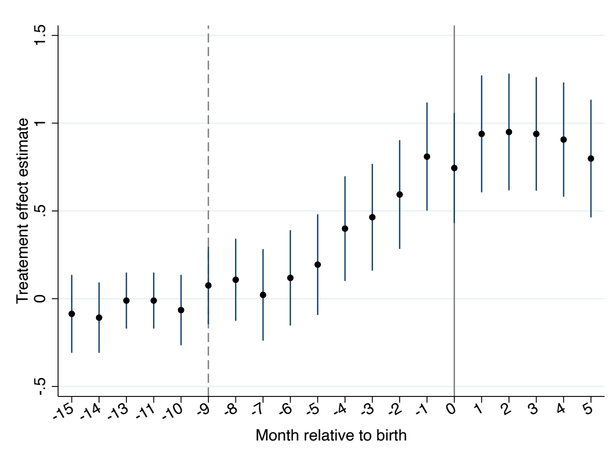
Any SUD Treatment: DiD Results



Opioid Use Dis. Treatment: Raw Time Series



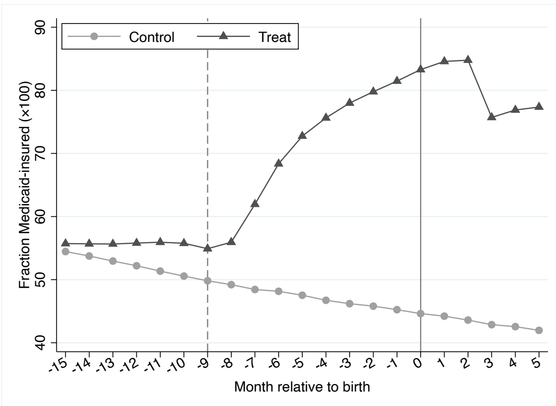
Opioid Use Dis. Treatment: DiD Results



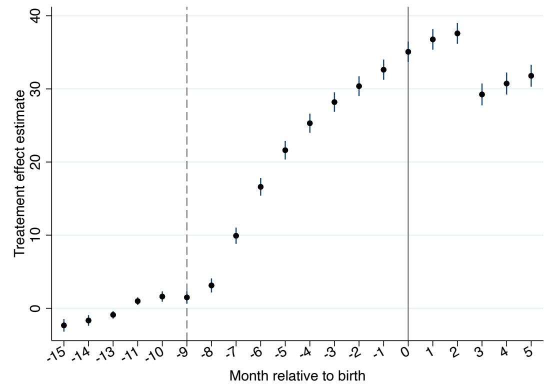
Notes: Figures show raw means of outcomes for treated and control group by month relative to first live birth event of treated individuals (left) and event study estimates from matched DiD regression (right), detailed in Appendix C. All estimates are based on outcome dummy multiplied by 100 for better readability. 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 treated-control dyads in which the treated peer satisfies the low SES criterion (that is, is observed as Medicaid-insured in at least one month of the five years preceding pregnancy). 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

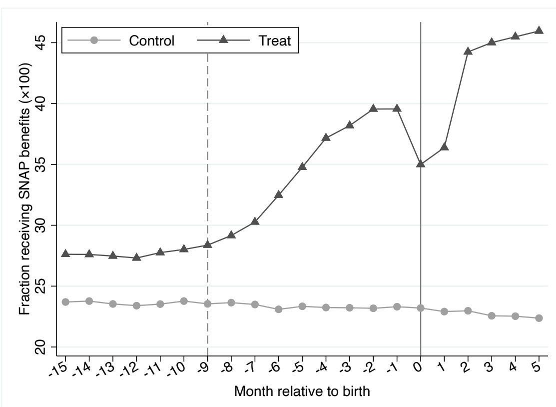
Medicaid: Raw Time Series



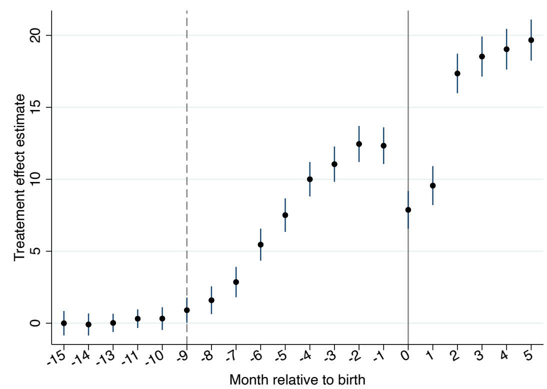
Medicaid: DiD Results



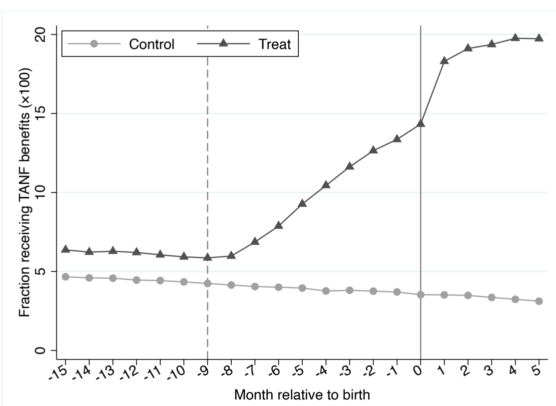
SNAP: Raw Time Series



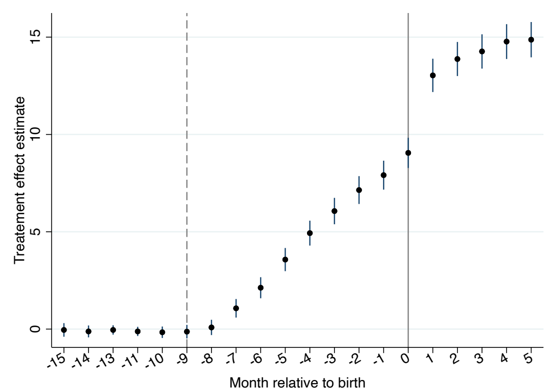
SNAP: DiD Results



TANF: Raw Time Series

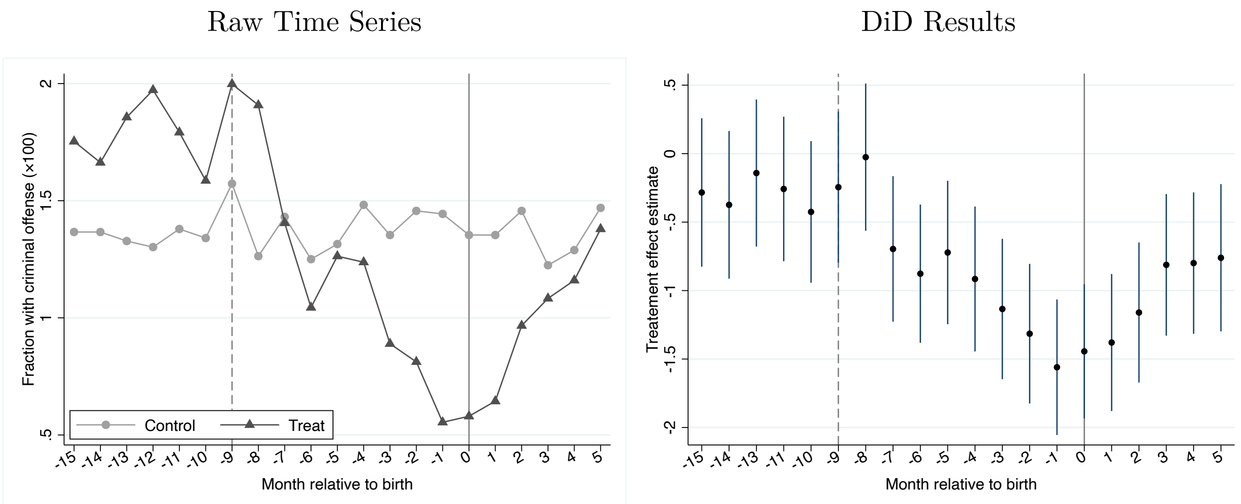


TANF: DiD Results



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. All estimates are based on outcome dummy multiplied by 100 for better readability. 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 treated-control dyads in which the treated peer satisfies the low SES criterion (that is, is observed as Medicaid-insured in at least one month of the five years preceding pregnancy). 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



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. All estimates are based on outcome dummy multiplied by 100 for better readability. Right figure reports 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 treated-control dyads in which the treated peer satisfies the low SES criterion (that is, is observed as Medicaid-insured in at least one month of the five years preceding pregnancy). 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-differences 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 SUD treatment, inpatient stays in psychiatric hospitals and SUD 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.[44]

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 2002-2019. Note that for the case of SNAP

[44] 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 Magisterial District Court records only), we only consider the period 2010-2019, while analysis of all other primary and secondary outcomes in the domain of criminal behavior is based on the period 2007-2019.

C. Matched Difference-In-Differences Analysis

In order to account for age effects, we perform a matched difference-in-differences 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 well-being, 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 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 79%. This fraction drops to 72% 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 62,638 "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 9,267 "treated women" (and the same number of control peers). Summary statistics are reported in Table A.19. Note that for this analysis, when considering SUD outcomes, we do not restrict the sample to individuals who are continuously Medicaid-insured. That is because such restriction would introduce major selection concerns, since new parenthood is a major determinant of Medicaid enrollment.

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

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

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-Differences Miscarriage vs. Life Birth Analysis

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

Sample Construction We identify miscarriage events via Medicaid claims and birth records. We find that Medicaid claims records likely provide a comprehensive sample of all miscarriage events that require medical attention and occur to Medicaid-insured women.[45,46]

[45]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.

[46]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).

Because we only have Medicaid claims records for the period 2015-2019, which is too short a period to provide enough sample, we supplement the sample of miscarriage events with non-live birth events identified via birth records spanning the whole sample period 2005-2019. We can only identify non-live births from birth records 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.[47] 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.[48] 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 1,019 women who have a miscarriage and 27,329 women who have a live birth.

Summary Statistics Summary statistics for this sample are presented in Table A.22. Overall, the approximately 28,300 women in this sample have similar demographic characteristics (in terms of age and race) to those in our main event study sample of low SES first-time mothers, though only about 39% are identified as low SES based on our Medicaid criterion.

[47]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.

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

Furthermore, within this sample, women who experience a miscarriage look very similar in terms of observable characteristics to women who experience a live birth: they have the same average age of 21, and a very similar racial/ethnic composition (33% are Black, in both samples). The sample of women who experience a miscarriage skew slightly more vulnerable on socioeconomic characteristics, as evidence by slightly higher rates of pre-pregnancy SNAP use (19.5% vs. 16.6%), and slightly higher rates of homelessness (1.0% vs. 0.7%). Of note is that within this sample, among the women who have a miscarriage event, 27.6% also enter the sample with a subsequent live birth event.

Estimating Equation For simplicity and because our event study imputation estimator cannot readily be applied in a setting that dynamically differences out trends observed among a control group that itself gets "treated" by an event, we employ a simple difference-in-differences estimator following Massenkoff and Rose (2022). 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, Bakx and Garcia-Gomez, 2022), and may thus not provide a suitable counter-factual, when the counter-factual of interest is one of not having had a pregnancy at all. The last two points imply *negative* selection into the miscarriage sample relative to the live birth sample. Thus, for any negative change to living conditions we find in the live birth group relative to the miscarriage group, it may be an 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.23, 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 1.3pp larger increase in movement into public housing in the year after the birth event—compared to an effect size estimate of 1.4pp in our main event study analysis. In contrast, the magnitude of the coefficients for the social assistance program use outcomes becomes smaller, consistent with the fact that the eligibility status of the miscarriage sample also changes with pregnancy. 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 a single coefficient, that on the interaction of pregnancy and live birth for the outcome of any substance use disorder, delivers a different sign compared to our main analysis (with the interaction coefficient being negative); however, the coefficient estimate is very small and not statistically significantly different from zero.