**The Cyclicality of Births and Babies' Health, Revisited:
Evidence from Unemployment Insurance**

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Abstract: This paper revisits the cyclical nature of births and infant health and investigates to what extent the relationship between aggregate labor market conditions and birth outcomes is mitigated by unemployment insurance (UI). We introduce a novel empirical test of standard neoclassical models of fertility that directly tests the prediction of opposite-signed income and intertemporal substitution effects of business cycles by examining the interaction of the aggregate unemployment rate with a measure of potential income replacement from UI. Our results show that as UI benefit generosity reaches 100 percent income replacement, there is no effect of the unemployment rate on births. This implies that the well-documented cyclical nature of births is about access to liquidity. We also provide novel evidence that infant health is countercyclical based on timing of conception, but procyclical based on time *in utero*. The negative relationship between the *in utero* aggregate unemployment rate and infant health also disappears when potential UI replacement rates reach 100 percent. Our results imply that the social insurance provided by UI has a pro-natalist effect and improves the health and economic well-being of the next generation.

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Introduction

In the US and across advanced economies, falling birth rates have led to concerns over the macroeconomic ramifications of declining population growth.[1] In addition, the importance of health at birth to lifetime health and economic well-being is now well understood.[2] As such, understanding how families' economic circumstances and government safety net programs affect childbearing outcomes and babies' health have become increasingly critical questions for economists and policy makers. In this paper, we address these questions by revisiting one of the most well-established empirical facts in economics and demography: the pro-cyclical nature of births.[3] Specifically, we revisit how births and infant health respond to the business cycle and investigate to what extent these relationships are mitigated by consumption smoothing income assistance, namely, unemployment insurance (UI).

A re-visiting of the cyclical nature of births and infant health advances our understanding of the mechanisms through which economic circumstances affect childbearing decisions and infant health outcomes. We find that births and infant health are pro-cyclical, and that these relationships are fundamentally about cyclical changes in liquidity. This highlights the critical role of access to liquidity in family formation decisions, and points to the potential for policies which boost disposable incomes to affect birth outcomes. It also highlights a

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[1] For a review, see Kearney and Levine (2022a).
[2] For a review, see Almond and Currie (2011).
[3] As early as the 1960s, procyclical fertility was described as "one of the most firmly based empirical findings in any of the social sciences" (Thomas, cited in Butz and Ward, 1979). Empirical studies on the topic include, for example, Galbraith and Thomas (1941); Silver (1965); Butz and Ward (1979); Ermisch (1988); Adsera (2005); Currie and Schwandt (2014); Schaller (2016); Dettling and Kearney (2014); Schaller, Fishback, and Marquardt (2020).

mechanism through which aggregate economic shocks are propagated to the next generation, that is, via changes in health at birth and cohort size. Our focus on unemployment insurance additionally informs our collective understanding of how social insurance mitigates potential effects of economic downturns on individuals. We find that when UI fully replaces lost incomes, births and infant health are no longer cyclical. This implies that UI is pro-natalist and improves the health and economic well-being of the next generation.

Since Becker (1960), economists have modelled couples' child-bearing decisions in a neo-classical decision-making framework in which potential parents decide on the optimal number of children that maximizes lifetime utility, subject to the budget constraint that they face.[4] Such a model generates testable predictions that have been borne out in empirical work: namely, that there is an increase in births associated with an increase in family income and with a decrease in the costs associated with having children.[5] The effect of exogenously determined changes in earnings on births is ambiguous, since increases in earnings increase family income, but also increase the opportunity cost of time devoted to raising children.[6]

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[4] In this paper we abstract away from the question about whether the decision to have a child is best modeled as the decision of an individual or a couple, as well as whether it is the outcome of one individual's dominant preferences, a consensus decision, or the result of interpersonal bargaining. Our reference to decisions made by a couple or by a woman when it comes to the decision to have a child is one of expositional convenience only.

[5] Studies documenting a causal link between shocks to income or liquidity and fertility rates include Lindo (2010), Black, Kolesnikova, Sanders, and Taylor (2013), Kearney and Wilson (2018); Cumming and Dettling (2020), and Goodman, Isen and Yannelis (2022). Studies showing a causal link between child-related prices and fertility rates include Milligan (2005); Cohen, Dehejia and Romanov (2013); and Dettling and Kearney (2014).

[6] Historically, economists have posited this off-setting substitution effect should apply to women's earnings (given historical gender patterns in time spent in childrearing), and they have treated increases in men's earnings as equivalent to increases in unearned income. However, as highlighted by Doepke et al, (2022), the historical negative correlation between women's earnings and childbearing under-pinning these types of assertions reversed in recent years and, as such, the economics of fertility has likely entered a "new era" in which distinctions based on traditional gender roles are increasingly less relevant.

This constrained optimization framework for the demand for children extends to decisions about the optimal timing of childbearing, including in response to the business cycle. The cyclical nature of fertility has been interpreted as reflecting optimal timing decisions in a life cycle model in which individuals respond to a temporary shock to income and wages. Assuming well-functioning capital markets, would-be parents could borrow and save to finance the cost of children and optimally choose when to have them over the life-cycle, unaffected by temporary changes in income (Hotz, Klerman, and Willis, 1997). But capital markets are imperfect, and the pro-cyclical nature of births is consistent with the proposition that liquidity or credit-constrained couples are more likely to choose to become parents when they have more disposable income available to pay for the associated costs of childbearing. Another common conjecture in the economics literature is that potential parents without access to savings or credit would opt into childbearing during an economic downturn when wages (and hence, the opportunity cost of time) are temporarily lower. This presumption is explicit in, for example, Dehejia and Lleras-Muney (2004) and Schaller (2016).[7]

This paper revisits the question of whether and why births track the business cycle by combining testable predictions from economic models of fertility with high frequency data on cyclical movements in economic conditions and unemployment insurance benefit generosity. Our analysis begins by confirming that births are indeed pro-cyclical through recent cycles. We

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[7] Dehejia and Lleras-Muney (2004) write, "If capital markets are perfect, women's fertility decisions will not depend on the path of wages of other members of the household. Furthermore, if skills do not depreciate, women will substitute fertility into periods in which their own-wage is low (page 1095)." Schaller (2016) writes, "In the case of perfect certainty and perfect capital markets, transitory fluctuations in wages do not alter expected lifetime income and thus should not impact expected total fertility. They do, however, impact the timing of fertility if couples respond to transitory fluctuations in female wages by choosing to give birth when wages are low (page 4)."

use administrative birth certificate data on the universe of live births in the United States from 2000-2019 and we date births to the likely month of conception. Guided by economic models of fertility and the empirical macroeconomics literature, we estimate OLS regressions relating monthly county-group birth outcomes to a broad array of cyclical economic variables which could plausibly influence child-bearing decisions. Specifically, our empirical analysis captures movements in employment opportunities with the county unemployment rate. We incorporate cyclical movements in unearned income with measures of asset prices –including county house prices and equity prices-- and allow for differential effects on groups with different ownership rates of such assets. We also include measures of consumer sentiment and expectations.[8] We both confirm and extend the existing literature, finding a separate role for movements in unemployment rates, asset prices, sentiment, and expectations. To the best of our knowledge, our paper is the first in the literature to take account of such a broad array of cyclical economic indicators.

We then move on to analyses of *why* fertility is observed to be pro-cyclical. We introduce a novel empirical test of standard neoclassical models of fertility (e.g. Becker 1960, Hotz et al, 1996). Specifically, we propose an explicit test for the prediction of opposite-signed income and intertemporal substitution effects of business cycles by interacting the aggregate unemployment rate with a measure of potential income replacement from UI. If unemployment rates affect

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[8] We do not include measures of output or consumer spending as explanatory variables because economic models of fertility highlight a role for movements in employment opportunities, income, and liquidity in child-bearing decisions, but there is no mechanism by which consumer spending or GDP would be expected to independently drive child-bearing decisions. If anything, we would expect consumer spending or GDP to be reverse-casually related to births as future parents make additional purchases in preparation for a new child (e.g., furniture, clothes, a larger vehicle, etc.). Consistent with his notion, Buckles et al (2021) provide suggestive evidence that conceptions lead changes in GDP.

births because of an income effect associated with a transitory shock we would expect income replacement from UI to offset those effects. And when UI replaces 100 percent of income, the intertemporal substitution effect would be expected to lead to a positive relationship between unemployment rates and births. To test these predictions, we construct a refined measure of UI generosity that captures differences over time across states in UI income replacement rates that are due to changes in state-level policies, as well as how state system progressivity differentially affects different groups, but importantly, our constructed measure is stripped of cyclical fluctuations in group-level incomes.

We find that as weekly UI benefits reach 100 percent income replacement, there is no negative effect of the unemployment rate on births. This implies that aggregate unemployment exerts a downward pressure on births entirely through a lost income channel. We find no evidence of a positive intertemporal substitution effect. In other words, the observed co-movement of births with transitory movements in unemployment rates is explained by movements in access to disposal income. We show that this pattern of a negative effect of unemployment on births and an offsetting effect of UI holds across alternative model specifications, including an analysis of individual-level data from the Current Population Survey (CPS) where we can observe an individual's own unemployment status.

Next, we revisit the question of whether infant health is pro-cyclical and provide novel evidence that US recessions are bad for infant health.[9] We confirm the finding of Dehejia and

[9] Dehejia and LLeras-Muney (2004) examine state-by-year variation in US unemployment rates from 1975-1999 and find that infant health is counter-cyclical. De Cao et al. (2022) find that births are pro-cyclical in England. Studies of the cyclicality of infant health in low to middle income countries tend to find evidence of pro-cyclical effects, as reported in De Cao et al. (2022).

Lleras-Muney (2004) that infants who were *conceived* during periods of elevated unemployment in the US tend to be healthier, but note that this finding is a downstream implication of the cyclical pattern of births, since higher SES women --who have healthier babies, on average-- are less likely to be liquidity constrained, and thus, are over-represented in the population of expectant mothers during downturns. When we narrow in on the effect of the aggregate unemployment rate *in utero,* we find that recessions lead to worse infant health – specifically, a higher incidence of low birth weight and preterm births – consistent with a causal negative effect of recessions on babies' health. We additionally find that these relationships are consistent with the unemployment effect being driven by income loss. Specifically, we find that when *in utero* UI income replacement rates reach 100 percent, there is no longer a negative effect of unemployment on infant health as measured by birth weight and gestation length. A back-of-the-envelope calculation using our results implies that it would take an average of $383/week of UI benefits to erase the cyclical pattern of pre-term births, or an additional $129 per week on top of the current average UI benefit level. We explore the role of health inputs in mediating this relationship – including an examination of individual-level data from the Behavioral Risk Factor Surveillance System (BRFSS) – and find no evidence in favor of prenatal care as a mechanism, but some evidence that a higher UI replacement rate is associated with improvements in maternal health behaviors such as a reduced incidence of maternal smoking and alcohol consumption.

Our paper makes important conceptual contributions to the economics of fertility literature by providing arguably one of the most direct tests of neoclassical models of fertility in the context of modern business cycles and considering a broader set of measures of economic conditions than has typically been used. We present novel results suggesting that *all* of the relationship between labor market conditions and births is driven by income and liquidity effects, not intertemporal substitution behavior.

Our paper also contributes to the literature on the economic effects of UI. Existing papers document the positive effects of UI on consumption smoothing (e.g., Gruber, 1997, Browning and Crossley, 2001; Chetty, 2008, East and Kuka, 2015).[10] Our paper documents that UI smooths cyclical fluctuations in births, pointing to a new "liquidity effect" of UI – allowing potential parents to optimally time their childbearing decisions.[11] Our result suggest that by smoothing cyclical fluctuations in births, UI generosity has downstream implications for outcomes where cohort sizes matter, such as the allocation of educational resources.[12] Our results also point to important long-run effects of UI, since temporary postponements in births often lead to permanent reductions in the number of children a woman will have (see, e.g., Currie and Schwandt, 2014). Our finding that UI mitigates the cyclical postponement of births in recessions implies that UI is an incidental pro-natalist policy. This finding is directly relevant to policy attempts to counter low fertility rates in high-income countries (see, for instance, Sobotka et al, 2019).

[10] There is also a large, tangential literature documenting the effects of UI on labor market outcomes and job search (e.g., Kroft and Notowidigo, 2016, or Schmieder, von Wachter, and Bender 2012). A couple of recent papers also document UI effects on other outcomes, such as averting mortgage default and foreclosures (Hsu et al, 2018) and increasing access to health care (Kuka, 2020).

[11] Chetty (2008) identifies an effect of access to additional cash income from UI that allows otherwise liquidity constrained unemployed individuals to spend longer looking for a desirable job and he labels this the UI liquidity effect. Chetty's naming of a liquidity effect of as distinct from a moral hazard effect of UI implies a higher optimal level of UI generosity.

[12] See, for example, Petrilli (2019) on how changing birth rates affect the education system.

In addition, our paper adds to the literature on infant health at birth. Existing research documents that health at birth plays a key role in explaining lifetime health, human capital attainment and earnings (Black et al, 2007; Oreopoulos et al, 2008; Royer, 2009; Almond and Currie, 2011) and there is growing causal evidence that infant health is affected by the pre-natal environment (Aizer and Currie, 2014; Aizer, Stroud and Buka, 2016). We add to this literature by providing novel evidence that US babies who are *in utero* during periods of high unemployment have worse health outcomes.[13] We also add to the body of evidence about how social policies indirectly affect infant health outcomes. For instance, Hoynes, Miller, and Simon (2015) demonstrate that the EITC has a beneficial effect on infant health outcomes. In recent recessions, UI has been the largest U.S. safety net program (Bitler and Hoynes, 2016). Our finding that UI ameliorates the negative effect of recessions on the health outcomes of babies who are affected in utero thus implies beneficial intergenerational effects of UI on economic well-being.

Background: Recessions, Birth Outcomes, and the Potential Role of UI

a. Recessions and Births

[13] To the best of our knowledge, related papers do not look separately at the link between infant health outcomes and aggregate unemployment rates at the time of conception versus the time in utero. There are other differences between our paper and existing papers on this question. De Cao et al (2022) examine the link between aggregate unemployment and infant health outcomes in the UK by comparing outcomes for siblings born in years with different unemployment rates; Dehejia and Lleras-Muney (2004) run a similar siblings-comparison specification in their paper using data on infants born in California. (Both of these papers measure unemployment at the year level.) The implicit assumption in a sibling comparison is that maternal characteristics that lead to differential selection into childbearing during recessions (i.e., the presence of liquidity constraints) and differential infant health outcomes (i.e., income, wealth, socioeconomic status) are fixed for a given mother over time. This strikes us as a dubious proposition.

Economic models of fertility indicate that births may follow the business cycle owing to temporary changes in income or wages, which can alter the optimal timing of childbearing for couples who are liquidity or borrowing constrained (Butz and Ward, 1979). Pro-cyclical fertility is consistent with the proposition that potential parents without access to credit or savings will be less likely to choose to become parents when they have less disposable income available to pay for the associated costs of childbearing (e.g. during unemployment). Consumption-smoothing income assistance from unemployment insurance would be expected to offset these effects.[14]

It is common in the cyclicality of fertility literature to encounter discussions that suggest that Becker's (1960) model implies that fertility could be counter-cyclical due to off-setting intertemporal substitution effects (e.g., Dehejia and Lleras-Muney, 2004; Schaller, 2016). Though such considerations likely apply to *permanent* changes in earned income, we propose they should not apply to short-run *transitory* changes in earned income. One obvious reason why is that such considerations are made with respect to the opportunity cost of time spent rearing children. But opting into pregnancy during an economic downturn likely means opting into having an infant or toddler during an economic recovery period. That typically would mean even greater child-related demands on parental time than during pregnancy. This raises

[14] Births could also follow the business cycle if accompanying changes in incomes are (believed to be) permanent, for example, due to wage scarring or if unemployment spells are expected to lead to permanent changes in employment. If that were the primary mechanism, we would not expect a differential change in childbearing among individuals who are liquidity constrained. Whether or not UI would fully or partially offset those effects would depend on the expected size of the future income losses and the discount rate individuals place on their future income. We follow the more common convention that business cycles are temporary in our exposition, and our empirical analysis is ultimately consistent with this notion since we find no effect of business cycles on groups that are not liquidity constrained.

practical doubt about the proposition often cited in previous papers that opting into pregnancy during a temporary economic downturn is potentially optimal.

Recessions and recoveries typically feature movements in more than just families' incomes and employment. Each recession and recovery is unique, and as such, there will be specific contextual factors that shape the relationship between aggregate economic conditions and the fertility response. Some recessions are accompanied by financial crises featuring dramatic swings in financial markets which reduce unearned income. Other recessions feature large declines in house prices, which have been shown to effect child-bearing decisions (Lovenheim and Mumford, 2011; Dettling and Kearney, 2014). Consumer sentiment and expectations also vary over the business cycle, which could potentially alter childbearing plans.[15] The fiscal and monetary policy environment could also shape how births move during an economic downturn (e.g., Cumming and Dettling, 2020; Kearney and Levine, 2022b).[16] Finally, more prolonged recessions and recoveries will mean that delayed or postponed births are more likely to lead to permanent reductions in completed fertility; this could result from biological fertility challenges associated with older age (Sommer, 2016) or from shifted preferences over time as one grows accustomed to their present family size.

b. Recessions and Infant Health

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[15] For example, in the context of the COVID-19 pandemic, Kearney and Levine (2022b) document that the recession in the early spring of 2020 led to a large decline in conceptions, which was amplified by the extent of local-area COVID cases.

[16] Cumming and Dettling (2020) document a link between central bank policy rates and births. In particular, they find that monetary easing during the Great Recession increased births in the United Kingdom by increasing household liquidity via mortgage rate pass-through. Kearney and Levine (2022b) find that during the COVID-19 pandemic, fertility rates increased when household spending recovered, which was bolstered by the fiscal policy response.

Aggregate economic conditions could affect infant health outcomes by shifting the demographic composition of mothers. Specifically, as noted above, economic models predict that liquidity or credit constrained mothers will opt out of childbearing during downturns. Because the incidence of liquidity and credit constraints is negatively correlated with socioeconomic status (e.g, Bhutta and Dettling, 2018), and lower socio-economic status mother's tend to have, on average, less healthy babies, this would potentially mean that the infants born during recessions would be of better relative health, on average. Indeed, Dehejia and Lleras-Muney (2004) show that babies conceived during periods of higher unemployment have a reduced incidence of low and very low birth weight, which they attribute in part to the shifted composition of mothers.

There are also direct ways in which an economic downturn that affected the income and employment of parents would lead to a change in infant health. First, job loss or household income loss during an economic downturn could, all else equal, reduce health inputs, leading to a reduction in infant health outcomes. This could be a result of people being income constrained to pay for adequate nutrition or prenatal care, or from unemployed parents losing access to health insurance that was attached to employment. [17] Second, recession-induced income losses may lead to either increases or decreases in either risky or healthy behaviors, depending on how time intensive such behaviors are or whether they are complements or substitutes with time in market work. Dehejia and Lleras-Muney (2004) document a decline in maternal smoking during recessions, which could reflect either a behavioral change related to unemployment or

[17] Kuka (2020) finds that UI benefit generosity is associated with greater health insurance coverage, which raises the possibility that more generous UI might increase prenatal care access.

the compositional shift in who becomes a mother during an economic downturn. Finally, unemployment (both at an individual or aggregate level) may affect maternal stress, and maternal stress levels have been shown to be negatively related to infant health outcomes (Aizer, Stroud, Buka, 2016).[18] UI benefits – by providing liquidity during downturns – could mitigate any or all of these channels.

Data and Summary Statistics

Our main source of data on births and infant health outcomes is National Center for Health Statistics (NCHS) Vital Statistics birth certificate data. We applied for and obtained a confidential version of the Vital Statistics data which includes the mother's state and county of residence. The data also includes information on mother's race and Ethnicity, age, and marital status, as well as a variety of measures of infant health outcomes including birth weight and length of gestation, which we use to infer if the infant was born prematurely. The data also has information on maternal health behaviors, including prenatal care usage though-out the pregnancy (including the month of initiation of care and number of visits) and whether the mother smoked while she was pregnant.[19] Our data set includes all births in all years from 2000-2019. We construct counts of births and health outcomes at the county-month-group-level, where demographic groups are defined by age category (18-34 and 35-49) and race/ethnicity (White, non-Hispanic, Black, non-Hispanic and Hispanic) and month refers to the month of

[18] The medical literature has also documented the effects of maternal stress -- specifically, elevated maternal cortisol levels-- on fetal development. See, for example, Hobel and Culhane (2003).
[19] Smoking and the month in which prenatal care usage began are missing in some states and year due to known issues with the Vital Stats data, as noted in Appendix A.

conception, since that is the time relevant to the decision to get pregnant. Appendix A includes more information on the construction of this data.

We link the birth certificate data to information on economic conditions and unemployment insurance benefits available in the mother's county of residence in the relevant month. Our main measure of local labor market conditions is the monthly unemployment rate in a mother's county of residence which we obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics (BLS LAU). Our models control for house prices at the level of county and month, as measured by the Zillow housing value index (ZHVI) which represents the value of a typical home in the county-month. We put this measure in real dollars using the CPI-U less shelter series. We measure equity prices using the average value of the Wilshire 5000 Index in each month and put them in real dollars by deflating by the CPI-U. To allow for differential effects of movements in asset prices amongst owners and non-owners of those assets (following the intuition in Dettling and Kearney, 2014), we construct we construct demographic group-level home and equity ownership rates using the 2001-2019 waves of the Survey of Consumer Finances (SCF). We use the Michigan Survey Research Center (SRC) Survey of Consumers measures of consumer sentiment and consumer expectations, which we construct at the level of month by group by region (the smallest geographic unit available).[20] The indices are defined as the percent of positive responses minus the percent of negative responses plus 100. Finally, we use the Centers for Disease Control (CDC) Surveillance,

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[20] We average the monthly region-level and age-group level indices to construct a region-group level monthly index.

Epidemiology and End Results (SEER) data to estimate female population in each county-group-year. Table 1 includes summary statistics for these variables.

We construct a refined measure of UI generosity which we refer to as a "potential weekly UI income replacement rate." We use data collected from Department of Labor (DOL) publications to construct a UI benefit calculator which factors in two key sources of state-by-year policy variation: weekly income replacement formulas and maximum benefit levels.[21] We use all women age 18-49 in the 2000 Census (five percent sample) and feed their wage and salary incomes through the UI benefit calculator for their state of residence in each year in our sample to estimate potential weekly benefit levels and income replacement rates.[22] We then calculate median potential UI replacement rates by state, year, and group, where the groups are defined as in the birth data. The key to this measure is that it captures differences over time across states in UI generosity due to changes in state-level policies, as well as how state system progressivity differentially affects different groups; but, by using a fixed sample of women from the year 2000, it is stripped of cyclical fluctuations in group-level incomes and employment.[23] In extensions and robustness checks, we estimate our regression models using potential

[21] Appendix A includes more information on the UI benefit calculator and construction of potential replacement rates. For ease of construction, for the 2000-2009 period we use the maximum benefit levels from the Hsu et al (2018) replication package, which were constructed from the same DOL source data we use in the same manner.

[22] We use the CPI-U to inflate the 2000 incomes to 2001-2019 dollars, since the maximum benefit levels are in nominal terms.

[23] This is similar in spirit to the simulated instrument method initially proposed in Currie and Gruber (1996). The application of this approach to our setting relies on the assumption that policy-induced variation in state-year UI replacement rates is not endogenously determined with movements in county-group-month conceptions. We note, however, that our primary purpose is not to examine the causal effect of UI replacement rates on fertility rates, but rather to estimate how the cyclicality of birth rates is mediated by the liquidity provided through unemployment insurance.

replacement rates for different subsamples (including men), and we also examine the sensitivity of results to other common measures of UI benefit generosity, such as maximum weekly benefit caps and maximum durations of UI eligibility.

Appendix Table 1 lists constructed UI replacement rates across states. As can be seen in the table, on average, weekly UI benefits are estimated to potentially replace 53 percent of weekly wage and salary income for the typical working woman ages 18 to 49. Across states, potential UI replacement rates range from below 50 percent in Alaska, Arizona, DC, Florida, DC, Illinois, and Mississippi to above 60 percent Hawaii, Oregon and Kentucky. Appendix Table 2 lists UI replacement rates constructed for different samples; later we will run robustness checks using these alternative constructions. Appendix Table 3 shows that there is considerable variation across years in our measure of potential UI replacement rate. Over time, potential replacement rates peak in the Great Recession at 58 percent and fall to 51 percent in 2019. Appendix Table 4 reports potential UI replacement rates across groups. Replacement rates are a bit higher for younger women than older women, reflecting typical life-cycle earnings profiles.

Our measure of UI benefit generosity has some key advantages over plausible alternatives like maximum durations of eligibility or maximum benefit caps for both conceptual and empirical reasons. Conceptually, our measure is designed to be interpretable as a percentage change in income, which is the parameter of interest in our conceptual framework. If we were to use, for example, maximum durations of benefits we would not be able to easily interpret those parameters within the economic models of fertility. Our measure also better captures the relevant features of state UI policy for the population we study than maximum benefit caps alone. Specifically, comparing our estimate of median weekly UI benefits for women of child-bearing age (column 2) with state maximum benefit caps (column 4) in Appendix table 1 we see that across all 50 states, column 2 falls below column 4. This implies that the maximum benefit caps do not bind for a typical woman of child-bearing age. Furthermore, we can see that our measure of potential UI replacement is only weakly correlated with maximum benefit levels.[24] For example, Louisiana has an average maximum benefit of $272 a week --the bottom 10 percent of all states in terms of maximums-- but at Louisiana's median women's income of $19,112, UI replaces 59 percent of income –placing Louisiana in the top 10 percent of all states in terms of potential UI replacement rates. In other words, states with more generous replacement formulas do not always have the highest maximum benefit caps, and vice versa. Thus, our measure –which combines both state replacement rate formulas and maximum benefit levels – is likely to better capture the relevant policy variation for the population we study than state maximums alone.[25]

Empirical Strategy

To examine the effects of recessions on births we estimate models of the following form:

$$\ln\left(births_{cgt} + 1\right) = \beta_1 UR_{c,t-3,t-1} + \mathbf{E}_{cgt-3,t-1}\boldsymbol{\beta_E} + \ ln(pop_{cgy}) + \delta_c + \delta_t + \delta_g + \epsilon_{igct} \tag{1}$$

Where $births_{cgt}$ refers to the number of conceptions leading to live births in county c and age-group by race/ethnicity group g in month t. We add one to the birth count so that cells with zero

[24] The correlation coefficient between column 2 and 4 is 0.16.

[25] Women of child-bearing age tend to have below average incomes. Studies focusing on higher income populations –e.g., homeowners (Hsu et al, 2018) – may not face these issues as the maximums will more often bind in higher income populations.

births are not dropped from the regression sample.[26] $UR_{c,t-3,t}$ is the mean unemployment rate in the county from month *t-3* through month *t-1* (e.g., the quarter leading up to conception). $\mathbf{E}_{cgt-3,t-1}$ is a vector of time-varying county-group level economic conditions, including county-month house prices interacted with group home ownership rates, stock prices interacted with group equity ownership rates, and group-region-month Michigan survey measures (sentiment, expectations). We include a control variable for the log of the female population (pop_{cgy}), defined by county, group, and year (y) (population counts by group are not available at the monthly level). Our baseline specification includes population as a control variable, rather than using it as the denominator for birth rates because it is an imperfect proxy for county-group monthly population and measurement error in the denominator of a dependent variable could introduce bias from non-classical measurement error.[27] δ_c are county fixed effects, δ_g are group fixed effects and δ_t are month fixed effects. We weight all regressions by the female population in each cell, and standard errors are adjusted for clustering at the county-level.[28]

In this paper we are interested in identifying a causal relationship between lagged economic conditions and conceptions, and our models are designed to overcome multiple identification challenges that might be present in a cross-sectional approach. A key feature of

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[26] About a third of the county-group-month cells have no births. As a robustness check, we also present results where we aggregate births and infant health outcomes at a somewhat higher level (county-age-group-month, instead of county-age-group-race/ethnicity group-month level), in which case, only about 10 percent of cells have no births. We find similar results (Appendix Table 8, Panel C, columns 3-4 and Appendix Table 11, Columns 9-10).

[27] For a discussion of bias arising from the "ratio problem" see Barlett and Partnoy (2020). As a robustness check, we include results based on a specification using the log of the fertility rate as a dependent variable and controlling for the log of population, as suggested in Bartlett and Partnoy (2020) to address such bias, in the Appendix Table 8, panel C.

[28] As a robustness check, we estimated models for which standard errors are adjusted for clustering at the state level (Appendix Table 8, Panel A, Columns 3-4 and Appendix Table 11, Columns 3-4)

our regression model is the inclusion of county fixed effects (δ_c), which capture time-invariant differences across counties in fertility preferences. This would include any sorting patterns by preferences for children according to fixed labor market or economic characteristics of different cities or neighborhood types, as well the potential effects of state-level social policies which might affect birth outcomes but were mostly unchanged over the period we study (e.g., abortion policies or welfare generosity).[29] Another important aspect of the model is the inclusion of month fixed effects (δ_t) which capture national trends in economic conditions and births, including how fertility decisions respond to national economic developments that might co-vary with the cycle, such as the monetary or fiscal policy response. Put together, this means β_1 and $\boldsymbol{\beta_E}$ are identified off within-county changes in economic conditions over time.

To investigate the mediating effect of unemployment insurance – or more generally income replacement – on the cyclicality of births, we augment the specification above with an interaction term between the unemployment rate and the potential UI replacement rate. In this case we augment equation (1) with the following additional terms: $\beta_2 UR_{c,t-3,t-1}{}^{*}\, UI_{s,g,t-3,t-1} + \beta_3 UI_{s,g,t-3,t-1}$, where $UI_{s,g,t-3,t-1}$ is our measure of the potential UI replacement rate. In the augmented model, β_2 describes the effects higher unemployment rates as UI approaches 100 percent income replacement. Conditional on β_2, β_1 then describes the effects of unemployment on birth outcomes when there is no income replacement from UI. In this augmented equation,

[29] In general, incremental changes in such policies have been found to have fairly small or negligible effects on aggregate fertility rates (e.g., Kearney, Levine, Pardue, 2022). Larger changes in abortion policy and Medicaid coverage happened outside the time period being studied here. We also estimate a robustness check with State-by-year fixed effects and find that the patterns are generally robust. (Appendix Table 8, Panel A, Columns 1-2 and Appendix Table 11, Columns 1-2).

testing for income effects of unemployment on births is thus a test of whether $\beta_2 > 0$ and testing for positive intertemporal substitution effects on births is tantamount to testing whether or not $\beta_1 + \beta_2 > 0$. We estimate a standard error on the latter using 500 bootstrap replications.

To study infant health outcomes we estimate models of the following form:

$$Y_{c,g,t} = \beta_1 UR_{c,t,t+8} + \beta_2 UR_{c,t-3,t-1} + \mathbf{X}_{cgt}\boldsymbol{\beta_x} + \mathbf{E}_{st,t+8}\boldsymbol{\beta_E} + \delta_c + \delta_t + \epsilon_{isct} \qquad (2)$$

Where $Y_{c,g,t}$ is the proportion of babies in each county *c*, group *g*, conceived in month *t* born with various health attributes at birth or maternal health behaviors during pregnancy. The main infant health outcome measures include, alternately, the proportion of babies that are low birth weight (under 2500 grams at birth) and the proportion born prematurely (before 37 weeks).[30] In models that use a measure of maternal health behavior as the outcome variable of interest, we consider the proportion of mothers who initiated prenatal care in the first trimester, who obtained at least 5 prenatal care visits, and who smoked while pregnant. In these specifications, most models include two measures of the unemployment rate, $UR_{c,t,t+8}$, which is the unemployment rate in the mother's county during the nine months following conception (i.e., while the baby was *in utero*) and $UR_{c,t-3,t-1}$, the unemployment rate in the mother's county of residence in the three months leading up to conception. The former is designed to capture the causal effect of experiencing a recession during pregnancy on infant health or maternal behavior, while the latter is designed to capture health effects owing to any differential selection into childbearing during recessions.

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[30] Appendix table 10 has alternative health measures, including the fraction of babies born with a very low birth weight (under 1500 grams), the proportion born very prematurely (before 32 weeks) and birth weight.

These models explicitly control for various infant and maternal characteristics in vector $\mathbf{X}_{cgt}$, which includes the proportion of babies who are boys and from multiple births (both of which affect birthweight), as well as the proportion of mothers who are married, since married women's incomes are in part insured by their husbands' earnings and could react differently to the business cycle and government social insurance.[31] The remaining terms in equation (2) are defined as in equation (1).

We also estimate models where we introduce an interaction term between unemployment rates and UI benefit generosity. Specifically, we augment equation (2) with the following additional terms: $\beta_3 UR_{c,t,t+8}$* $UI_{s,g,t,t+8}$+ $\beta_4 UI_{sg,t,t+8}$, where $UI_{s,t,t+8}$ are our measures of UI benefit generosity in state s, for group g, measured during pregnancy *(t,t+8)*. In these models, β_2 describes the effects of higher unemployment rates on infant health outcomes as UI approaches 100 percent income replacement. If experiencing a pregnancy during an economic downturn is, on net, bad for infant health because of various (indirect) effects of lost income, then a higher replacement rate should mitigate those negative effects, resulting in β_2 <0 (since the infant health outcomes captured in $Y_{c,g,t}$ indicate worse health).

Results

i. Unemployment rates and Births

Table 2 presents results from estimating equation (1). Column 1 starts with a relatively sparse specification omitting most of $\mathbf{E}_{cgt-3,t-1}$ and columns 2 and 3sequentially add the

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[31] We do not control for mother's education because of the well-known issue of birth certificates missing this information in nearly half of states during the period from 2009-2013 See Appendix A for more information on missing data.

various economic indicators in $\mathbf{E}_{cgt-3,t-1}$. The relationship between unemployment rates and conceptions is stable within a small range and statistically significant (at the 1 percent level). The estimates indicate that each percentage point increase in local unemployment rates reduces births by about 2 percent. The estimated effect is similar to that found in previous work (see, e.g., Kearney and Levine, 2022b).

Column 4 of table 2 reports the results of estimating equation (1) with the interaction term between unemployment rates and UI benefit generosity. Consistent with the notion that children are "normal goods" – that is, the demand for children increases in income -- β_2 is positive and statistically significant at the one percent level. The coefficient on β_2 implies that holding unemployment rates fixed, moving from zero to 100 percent income replacement increases births by 7.61 percent.[32] Note that while this extrapolation is useful to facilitate interpretation of the effect as a measure of income replacement, it is outside the bounds of our sample, where UI replacement rates range from 25 to 78 percent.[33] In column 4, β_1 is more negative than in column 3, indicating that when UI does not replace any income, each 1 percentage point increase in the unemployment rate is associated with a roughly 6 percent

[32] This pattern of results is robust across various alternative specifications, as shown in Appendix Table 5. These specifications use UI replacement rates calculated over the earned income of different samples, including women with positive wage income, women who worked at least 26 weeks last year (i.e., typical UI eligibility work requirements), men with positive wage income, and men who worked at least 24 weeks last year. It is also robust across specifications that use the mean instead of the median replacement rate, define the outcome only for marital births and use the replacement rate over married men's earned income, and using the maximum benefit level instead of the potential replacement rate. In a specification that replaces the replacement rate with just the maximum weeks of eligibility, the coefficient on the interaction term goes to zero.

[33] Furthermore, although 100 percent replacement rates are outside the scope of our sample, they are not completely unrealistic as policy experiment, as UI replacement rates reached well over 100 percent in 2020 (see, e.g., Ganong, Noel and Vavra, 2020)

reduction in births. Recall that testing for an intemporal substitution effect is tantamount to testing if $\beta_1 + \beta_2 > 0$. We test this formally by running 500 bootstrap replications to estimate a standard error on $\beta_1 + \beta_2$, and find that we cannot reject that the substitution effect is zero at the ten percent level (the point estimate is 0.0157 with standard error of 0.0097).[34]

Put differently, we can see from a comparison of the coefficients β_1 and β_2 in column 4 of Table 2 that the positive income effect on fertility associated with full replacement of income loss from unemployment insurance (i.e., at a UI replacement rate of 100%) fully cancels out the negative effect on fertility from an increase in unemployment. This suggests that the cyclical pattern of births is fully explained by liquidity effects. This need not have been true. First, as we have discussed above, standard economic models on fertility decisions have tended to assert the existence of a positive intertemporal substitution effect. Second, individuals might infer from an unemployment spell (or an increase in aggregate unemployment rates) the possibility or even likelihood of a permanent loss in income, which would lead to lower fertility. The data do not offer evidence in support of either of these phenomena.

Column 4 of Table 2 also indicates that some of the variables included in $\mathbf{E}_{cgt-3,t-1}$ exert independent and statistically significant effects on births above and beyond changes in unemployment rates. Local house price increases exert a positive effect on homeowners, and

[34] In a contemporaneous working paper, Lindo et al (2022) uses SIPP data to examine the relationship between state maximum UI benefit caps and child-bearing decisions in the years following a man or woman experiencing a spell out of the labor force. They find that such spells reduce the probability a man will have a child and increase the probability a woman will have a child two to three years after the initiation of the spell. Although it is not their main specification, when they focus on spells in which the potential parent is actively looking for work (i.e., unemployed and eligible for UI), they find that higher maximum benefits levels increase men's and women's childbearing one and half to two years after the onset of those spells.

negative effect on non-owners, confirming the results of Dettling and Kearney (2014). Increases in equity prices similarly exert a positive wealth effect on births among owners of equities. Improvements in consumer sentiment and consumer expectations do not enter the model with statistical significance.

A natural question arising from the results displayed in Table 2 is whether the estimated unemployment rate and UI effects on birth rates is driven by individuals who are unemployed and eligible to receive UI, or whether those effects capture other aspects of the recessionary environment not already included in our models which could also affect employed individuals (e.g., falling wages). To probe this, we estimated a modified version of equation (1) using individual-level data from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC) on recent births and unemployment spells.[35] Appendix Table 6 displays the results. Column 1 indicates that women who report having been unemployed in the prior year are 0.6 percentage points less likely to have had a recent birth (about a 10 percent decline at the mean of the dependent variable). The specification reported in Column 2 additionally includes a control for the local unemployment rate, indicating that the local unemployment rate does not exert a statistically significant effect on the probability of giving birth for individuals who are not unemployed. Finally, column 3 adds the interaction between own unemployment status and UI benefit generosity. Like the main results presented in Table 2, this specification shows that when UI benefits reach 100 percent income replacement, the negative effect of own unemployment on the probability of giving birth disappears. All told,

[35] Appendix A provides details about the CPS data construction and individual-level specification.

Table A6 indicates that the results in Table 2 hold at the individual-level and suggests the effects are driven by women who experience an unemployment spell and receive UI benefits.

Table 3 displays the results of estimating equation (1) and the augmented models by birth parity. There is a negative effect of unemployment across all parity of births. The interaction with UI replacement rate is positive across parities, however, the effects are quite a bit larger for third and higher births. Following the literature, which has proposed that third and higher births may be more "marginal" (e.g., Dettling and Kearney, 2014), we interpret this as potentially consistent with the notion that UI increases completed fertility.[36]

ii. Unemployment Rates and Infant Health

Table 4 presents results from estimating equation (2) for two infant health outcomes. Columns 1-2 present results where the outcome is the percent of infants born with a low birth weight (under 2500 grams) and columns 3-4 present results where the outcome is the percent of infants born prematurely (fewer than 37 weeks). Columns 1 and 3 display estimates of the baseline version of equation (2) without the interaction term with UI generosity. In both columns 1 and 3, the coefficient on the *in utero* unemployment rate ($UR_{c,t,t+8}$) is positive, implying that babies who are *in utero* during periods of high unemployment suffer worse health outcomes. The estimated effects indicate that each one percentage point increase in the *in utero*

[36] Appendix Table 7 reports results where we allow the effects of unemployment rates and potential UI replacement to vary by age group and race/Ethnicity group. The largest negative effects of the unemployment rate and positive effects of the interaction term with the UI replacement rate are for Black women and for younger women. This could be indicative of Black women and younger women being especially liquidity constrained and sensitive to labor market conditions and income replacement rates. Appendix Table 8, Panel B includes robustness checks on the specification including an additional lag and a lead on the unemployment rate to address possible serial correlation in unemployment rates over time, both of which are estimated to be statistically insignificant.

unemployment rate increases the fraction of babies born low birth rate by .04 percent points (statistically significant at the 10 percent level) and the fraction born prematurely by 0.14 percentage points (statistically significant at the 1 percent level). These effects represent a 2 and 1 percent increase, respectively, at the means of the dependent variables.

Although babies *in utero* during times of high unemployment suffer worse health outcomes, columns 1 and 3 indicate that babies *conceived* during times of high unemployment are a bit healthier. A 1 percentage point increase in the unemployment rate in the months leading up to conception ($UR_{c,t-3,t}$) reduces the fraction of babies born with low birth weight by 0.04 percentage points and the fraction of babies born prematurely by 0.07 percentage points (both statistically significant at the 1 percent level). We interpret this as a downstream implication of the change in the pool of expectant mothers documented in table 2. In particular, this result is consistent with higher SES mothers – who, on average, tend to have healthier babies—being over-represented in the population of mothers during downturns because the incidence of liquidity constraints is relatively lower among higher SES families (see, e.g., Dettling and Bhutta, 2018).

Columns 2 and 4 report results from the model augmented with the interaction term between the unemployment rate and the potential UI replacement rate. The results suggest there is a role for public policy to improve infant health outcomes by buffering income losses during recessions. The coefficients on β_1 implies that when there is no income replacement from UI, each 1 percentage point increase in the unemployment rate increases the proportion of infants that are low birth weight by 0.16 percentage point (about 2.5 percent at the dependent variable mean) and the proportion of births that are pre-term by 0.43 percentage point (about

3.5 percent at the dependent variable mean), both statistically significant at the one percent level. However, the coefficient on β_3 --the interaction between the *in utero* unemployment rate and potential UI replacement-- shows that, holding the unemployment rate fixed, higher UI replacement rates lead to better infant health outcomes --moving from zero to 100 percent UI replacement reduces the proportion of low birth weight births by 0.23 percentage point (2.6 percent) and preterm births by 0.53 percentage point (4.0 percent). In other words, at 100 percent UI replacement, the negative effect of the unemployment rate on infant health disappears.[37] Together, these estimates imply that UI would need to replace between 70-80 percent of incomes to offset the negative effects of higher aggregate unemployment rates on infant health –above typical replacement rates in our sample.[38]

To put this estimate in a cost/benefit perspective, we note that the average cost of a birth hospitalization was $62,931 for commercially insured preterm infants and $43,858 for Medicaid-insured preterm infants compared to $2,401 and $1,894 for commercially insured and Medicaid-insured full-term infants, respectively (McLaurin et al, 2019). Our results imply that it would

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[37] More formally, we run 500 bootstrap replications to estimate a standard error on $\beta_1 + \beta_3$ and find that when UI replaces 100 percent of income there is a small positive, but statistically insignificant (at the five percent level) effect of unemployment rates on infant health outcomes. For percent low birth weight, the coefficient is -0.07 with a standard error of 0.04. For percent born prematurely, the coefficient is 0.11 with a standard error of 0.07.

[38] Appendix Table 9 reports results using alternative measures of UI replacement, analogous to Appendix Table 5 for births. The pattern of results is generally robust. Appendix Table 10 displays results for alternate outcomes, including the proportion of births that are very low birth weight (less than 1500 grams), the proportion of births that are very preterm (less than 32 weeks), birth weight, gestational age, and the proportion of births that are male. The same patterns hold, indicating that UI has a protective effect both on average health at birth (birth weight) as well as more adverse outcomes (very low birth weight, very preterm). There is no effect on male share (Appendix Table 10, columns 5-6). Appendix table 11 Columns 5-8 also displays various robustness checks on the specification, including models with controls for mothers' education (which is missing in many state-years, as documented in Appendix A) and a lead on the unemployment rate (which is found to be statistically insignificant).

take an average of $383/week of UI benefits to erase the cyclical pattern of pre-term births, or an additional $129 per week on top of the current average UI benefit level of $254 per week. For the length of a full-time pregnancy (44 weeks), this would amount to nearly $17,000 in UI benefits. The implication is that the cost of replacing lost income among pregnant mothers has a positive social return based on the cost of a pre-term birth alone, not even accounting for the well-documented longer-term effects of infant health on lifetime health and income.

Columns 2 and 4 of Table 4 also point to some novel relationships between cyclical movements in asset prices and infant health which are also consistent with positive liquidity effect on infant health. Specifically, we find that higher house prices during pregnancy lead to improvements in infant health among groups with higher home ownership rates. This is consistent with a wealth effect on infant health, presumably also via an increase in liquidity.

Table 5 reports infant health results for a specification where we allow the effects unemployment rates and potential UI replacement rates to vary by demographic group.[39] For nearly all groups (except Hispanic mothers) there is evidence of positive selection effects from higher unemployment at the time of conception, as indicated by a reduction in these two negative health indicators at birth. And for all groups, the results indicate that unemployment rates during pregnancy lead to worse infant health outcomes (more pre-term births, and for Black and older mothers, more low birth weight births); higher potential UI replacement rates mitigates all these effects. Across groups, the largest magnitude effects are for Black women and for women 35-49 -- both groups which have a higher incidence of low birth weight and preterm

[39] In this specification, we interact indicators for the demographic group listed with $UR_{c,t,t+8}$* $UI_{s,g,t,t+8}$, $UI_{sg,t,t+8}$ $UR_{c,t,t+8}$ and $UR_{c,t-3,t}$.

births on average. For Black women, a 1 percentage point increase in the unemployment rate during pregnancy leads to 0.75 percentage point (5 percent at the mean) increase in the proportion of low birth weight births and 0.89 percentage point (44.7 percent) increase in the proportion of preterm births. For older women, a 1 percentage point increase in unemployment rates lead to 0.18 percentage point (2 percent at the mean) increase in the proportion of low birth weights and 0.31 percentage point (2.1 percent) increase in the proportion of preterm births. Holding the unemployment rate fixed, moving from zero to 100 percent income replacement from UI reduces the proportion of low-birth weight births among Black women by 0.1.1 percentage points (7.5 percent) and preterm births by 1.3 percentage points (6.9 percent), and among older women by 0.22 percentage point (2.3 percent) and preterm births by 0.31 percentage point (2.1 percent).

Table 6 reports results separately by trimester. The estimated conditional relationship between the unemployment rate and infant health outcomes is largest in the first trimester, while the point estimate of the effect of the interaction of the unemployment rate and the UI replacement rate is generally stable across pregnancy trimesters. This would be consistent with the protective impact of UI on infant health being strongest during the first trimester. One interpretation of this pattern of results is that they are consistent with unemployment affecting infant health development through elevated maternal stress (with higher UI benefits reducing stress), as stress earlier in pregnancy has been shown to have more pronounced harmful effects on fetal development (Hobel and Culhane, 2003).

Table 7 probes other possible explanations for the procyclicality of infant health by reporting the results for maternal health behavior during pregnancy. Columns 1-4 display two measures of prenatal care usage: obtaining prenatal care in the first trimester (columns 1-2) and having at least 5 prenatal care visits (columns 3-4). The coefficient on the *in utero* unemployment rate is either positive or not statistically different from zero in columns 1-4, indicating that prenatal care usage cannot explain the procyclicality of infant health.[40] This is consistent with the notion that prenatal care has only modest causal effects on babies' health (see Corman et al, 2019 for a review). Columns 5-6 displays the results for smoking during pregnancy. In column 5, there is no evidence that smoking is associated with aggregate unemployment either before or during pregnancy, however, in column 6, we find that higher *in utero* unemployment rates are associated with an increase in the proportion of mothers who smoked during pregnancy (marginally statistically significant at the 10 percent level), but that higher potential UI replacement rates mitigate those effects. We speculate that perhaps this pattern is related to maternal stress patterns, insofar as worse aggregate economic conditions exacerbate stress, which leads to increased rates of smoking, and income replacement mitigates that stress and associated smoking behavior.[41]

Appendix Table 12 further probes for changes in health behavior using individual-level data on pregnant women from the Behavior Risk Factor Surveillance System (BRFSS). The

[40] The coefficient on unemployment at the time of conception in columns 1-2 indicates increased prenatal care usage, which also consistent with positive selection into childbearing during recessions.

[41] We probe on the potential role of changes in self-reported mental and physical health status using individual-level data on pregnant women from the Behavior Risk Factor Surveillance System (BRFSS). The results of that supplementary analysis are reported in Appendix Table 11, panel A. The results are mostly imprecise and do not yield much insight into mechanisms. That said, the point estimates suggest that being unemployed is associated with more self-rated bad mental health days and more generous unemployment insurance exerts a downward pull on that relationship (though the effect is not statistically significant.)

results are mostly imprecise, but there is some suggestive evidence that being unemployed is associated with worse health behaviors among pregnant women, and that more generous UI offsets those effects.[42]

Conclusion

This paper provides arguably one of the most direct tests of economic models of fertility (e.g., Becker, 1960; Hotz et al, 1996) in the context of modern business cycles. We provide direct empirical evidence testing the separate proposed mechanisms through which higher aggregate unemployment could affect births – an income loss effect (negative) and an intertemporal substitution effect (positive). We find that when UI provides 100 percent replacement, unemployment rates exert essentially no effect on births, consistent with the notion that children are "normal" and widespread and binding liquidity constraints cause families to delay childbearing when their incomes are temporarily low. We find no evidence to support intertemporal substitution effects. This finding strikes us as being consistent with the practical observation that the opportunity cost of time spent child-rearing would be expected to occur well after the typical temporary unemployment spell is over (specifically, about nine months through eighteen years later).

This paper was written in the aftermath of the COVID-19 pandemic recession, which featured a dramatic fiscal expansion -- for the first time, UI benefit generosity was expanded so

[42] Kuka's (2020) study of the effects of UI generosity on health insurance coverage and health outcomes includes an analysis of BRFSS data. She finds little evidence of significant short-term effects of UI on risky behaviors, such as alcohol consumption or smoking, and no effects on health conditions such as diabetes and blood pressure, but her sample is not limited to pregnant women.

that replacement rates exceeded 100 percent of income (Ganong, Noel and Vavra, 2020), and personal incomes ultimately *rose* during the recession (Bhutta et al, 2021). New research indicates that the pandemic also initially featured the typical child-bearing response; as unemployment rates sky-rocketed, there was a reduction in conceptions, but this pattern quickly reversed (Kearney and Levine, 2022b). This reversal led some observers to speculate that perhaps the long-standing procyclical nature of fertility had broken down (Bailey, Currie and Schwandt, 2022). Our finding that procyclical fertility is ultimately about the cyclical nature of liquidity reconciles this apparent contradiction in the recent experience.

Our paper provides novel evidence that infant health is pro-cyclical in the US. We find that although differential selection into childbearing during recessions causes infants *conceived* during periods of high unemployment to be healthier, on average, babies who are *in utero* during periods of high unemployment experience worse health outcomes, namely, they are more likely to be low birth weight and are more likely to be born prematurely. However, when we allow for off-setting income replacement from UI, these effects disappear. In other words, loss of income during pregnancy harms babies' health and social insurance can offset the harmful effects.

This paper adds to the literature on the effects of UI on consumption smoothing by documenting that UI acts to smooth cyclical fluctuations in birth outcomes, allowing would be parents to maintain their intended rates of fertility and protecting infant health. Because births that are delayed are averted altogether and health at birth plays a key role in explaining lifetime health and human capital attainment, our results suggest that UI is pro-natalist and leads to intergenerational improvements in economic well-being.

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Table 1: Summary Statistics

	Mean	SD	Memo: Data Source
log(births$_{c,t,g}$+1)	4.09	1.57	Vital Statistics
Percent low birth weight$_{c,t,g}$	8.65	8.53	Vital Statistics
Percent preterm$_{c,t,g}$	13.05	10.42	Vital Statistics
unemp rate$_{c,t-3,t-1}$	5.97	2.54	BLS LAU
ui replacement$_{g,s,t-3,t-1}$	0.53	0.05	DOL and Census
log(population$_{c,t,g}$)	9.80	1.27	SEER
house prices$_{c,t-3,t-1}$ ($10,000)	25.81	16.84	Zillow ZHVI
equity prices$_{t-3,t-1}$	75.21	29.03	Wilshire Index
consumer sentiment$_{g,t-3,t-1}$	88.31	11.85	Michigan SRC
consumer expectations$_{g,t-3,t-1}$	80.49	11.36	Michigan SRC

Table 2. Unemployment, UI, and Births

	(1)	(2)	(3)	(4)
unemp rate$_{c,t-3,t-1}$	-0.0188	-0.0190	-0.0190	-0.0601
	(0.0065)***	(0.0065)***	(0.0067)***	(0.0151)***
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$				0.0761
				(0.0217)***
ui replacement$_{g,s,t-3,t-1}$				-0.5338
				(0.3089)*
house prices$_{c,t-3,t-1}$ * home own rate$_g$	0.0482	0.0485	0.0485	0.0484
	(0.0067)***	(0.0067)***	(0.0067)***	(0.0067)***
house prices$_{c,t-3,t-1}$	-0.0333	-0.0331	-0.0331	-0.0332
	(0.0064)***	(0.0064)***	(0.0065)***	(0.0065)***
equity prices$_{t-3,t-1}$ * equities own rate$_g$		0.0090	0.0090	0.0089
		(0.0010)***	(0.0010)***	(0.0010)***
consumer sentiment$_{g,t-3,t-1}$			0.0007	0.0006
			(0.0023)	(0.0023)
consumer expectations$_{g,t-3,t-1}$			-0.0005	-0.0005
			(0.0039)	(0.0039)
log(population)	0.7771	0.7827	0.7827	0.7826
	(0.0289)***	(0.0291)***	(0.0291)***	(0.0291)***
N	2,449,745	2,449,745	2,449,745	2,449,745

Notes: Estimated according to equation 1. Includes county, month of conception, and age-group-race/ethnicity group fixed effects. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.05 **p<0.01 ***p<0.001

Table 3. Unemployment, UI and Births, by Birth Parity

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	First Births		Second Births		Third+ Births	
unemp rate$_{c,t-3,t-1}$	-0.0182	-0.0432	-0.0158	-0.0491	-0.0138	-0.0616
	(0.0058)***	(0.0148)***	(0.0053)***	(0.0144)***	(0.0048)***	(0.0120)***
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$		0.0463		0.0616		0.0883
		(0.0230)**		(0.0226)***		(0.0176)***
N	2,449,745	2,449,745	2,449,745	2,449,745	2,449,745	2,449,745

Notes: Estimated according to equation 1. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; log(group population), house prices, equity prices consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.05 **p<0.01 ***p<0.001

Table 4. Unemployment, UI, and Infant Health

	Percent Low Birth Weight (<2500g)		Percent Pre-term (<37 Weeks)	
	(1)	(2)	(3)	(4)
unemp rate$_{c,t,t+8}$	0.0393	0.1634	0.1360	0.4249
	(0.0227)*	(0.0596)***	(0.0349)***	(0.0958)***
unemp rate$_{c,t-3,t-1}$	-0.0364	-0.0371	-0.0667	-0.0678
	(0.0127)***	(0.0127)***	(0.0193)***	(0.0195)***
unemp rate$_{c,t,t+8}$ * ui replacement$_{g,s,t,t+8}$		-0.2294		-0.5336
		(0.0874)***		(0.1411)***
ui replacement$_{g,s,t,t+8}$		1.4945		6.0357
		(0.6975)**		(1.3420)***
house prices$_{c,t,t+8}$ * home own rate$_g$	-0.0175	-0.0173	-0.0414	-0.0424
	(0.0045)***	(0.0044)***	(0.0068)***	(0.0067)***
house prices$_{c,t,t+8}$	0.0044	0.0048	0.0141	0.0151
	(0.0073)	(0.0073)	(0.0105)	(0.0104)
equity prices$_{c,t,t+8}$ * equities own rate$_g$	-0.0089	-0.0085	0.0054	0.0065
	(0.0035)**	(0.0035)**	(0.0047)	(0.0045)
consumer sentiment$_{g,t,t+8}$	-0.0473	-0.0464	-0.1013	-0.0921
	(0.0350)	(0.0345)	(0.0446)**	(0.0434)**
consumer expectations$_{g,t,t+8}$	0.0734	0.0731	0.1562	0.1508
	(0.0349)**	(0.0345)**	(0.0424)***	(0.0414)***
N	1,579,503	1,579,503	1,579,503	1,579,503

Notes: Estimated according to equation 2. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; the proportion of mothers who are married and the proportion of babies that are boys and twins . Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.05 **p<0.01 ***p<0.001

Table 5. Unemployment, UI, and Infant Health, by Demographic Group

	Percent Low Birth Weight (<2500g)		Percent Pre-term (<37wks)	
	(1)	(2)	(3)	(4)
Panel A: Age-group				
unemp rate$_{c,t,t+8}$				
X I(Age 18-34)	0.0214	0.1111	0.1343	0.4709
	(0.0227)	(0.0693)	(0.0329)***	(0.1125)***
X I(Age 35-49)	0.0610	0.1831	0.1411	0.3132
	(0.0252)**	(0.0732)**	(0.0399)***	(0.1091)***
unemp rate$_{c,t-3,t-1}$				
X I(Age 18-34)	-0.0285	-0.0273	-0.0829	-0.0817
	(0.0122)**	(0.0122)**	(0.0169)***	(0.0172)***
X I(Age 35-49)	-0.0458	-0.0485	-0.0493	-0.0528
	(0.0175)***	(0.0176)***	(0.0268)*	(0.0268)**
unemp rate$_{c,t,t+8}$ * ui replacement$_{g,s,t,t+8}$				
X I(Age 18-34)		-0.1689		-0.6261
		(0.0995)*		(0.1716)***
X I(Age 35-49)		-0.2233		-0.3075
		(0.1214)*		(0.1762)*
Dep. Var Mean, Age 18-34	7.7	7.7	11.4	11.4
Dep. Var Mean, Age 35-49	9.3	9.3	14.2	14.2
Panel B: Race-Ethnicity-group				
unemp rate$_{c,t,t+8}$				
X I(White - NH)	0.0332	0.0651	0.1230	0.3443
	(0.0225)	(0.0630)	(0.0316)***	(0.0877)***
X I(Black- NH)	0.1402	0.7457	0.1605	0.8932
	(0.0363)***	(0.1643)***	(0.0467)***	(0.2302)***
X I(Hispanic)	0.0060	0.1892	0.1467	0.4770
	(0.0321)	(0.1233)	(0.0561)***	(0.2441)*
unemp rate$_{c,t-3,t-1}$				
X I(White - NH)	-0.0347	-0.0335	-0.0716	-0.0724
	(0.0142)**	(0.0143)**	(0.0205)***	(0.0207)***
X I(Black- NH)	-0.0971	-0.1168	-0.0698	-0.0885
	(0.0304)***	(0.0332)***	(0.0393)*	(0.0412)**
X I(Hispanic)	-0.0176	-0.0152	-0.0515	-0.0439
	(0.0201)	(0.0197)	(0.0334)	(0.0336)
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$				
X I(White - NH)		-0.0600		-0.4066
		(0.0940)		(0.1294)***
X I(Black- NH)		-1.0786		-1.3189
		(0.2586)***		(0.3968)***
X I(Hispanic)		-0.3482		-0.6284
		(0.2049)*		(0.3986)
Dep. Var Mean, White - NH	7.8	7.8	12	12
Dep. Var Mean, Black - NH	14.6	14.6	18.8	18.8
Dep. Var Mean, Hispanic	7.2	7.2	12.5	12.5

Notes: Estimated according to equation 2. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; the proportion of mothers who are married and the proportion of babies that are boys and twins . Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. N=1,579,503. *p<0.05 **p<0.01 ***p<0.001

Table 6: Unemployment, UI, and Infant Health, by Trimester

	Percent Low Birth Weight (< 2500g)			Percent Premature (< 37 weeks)		
	(1)	(2)	(3)	(4)	(5)	(6)
unemp rate$_{c,t+l,t+k}$	0.1691	0.1476	0.1132	0.3741	0.3644	0.3400
	(0.0580)***	(0.0575)**	(0.0442)**	(0.0837)***	(0.0862)***	(0.0866)***
unemp rate$_{c,t+l,t+k}$ * ui replacement$_{g,s,t+l,t+k}$	-0.2129	-0.2189	-0.1974	-0.4562	-0.4950	-0.5044
	(0.0860)**	(0.0840)***	(0.0760)***	(0.1266)***	(0.1326)***	(0.1375)***
ui replacement$_{g,s,t+l,t+k}$	1.4680	1.3647	1.2362	5.5593	5.6215	5.7012
	(0.7098)**	(0.6786)**	(0.6168)**	(1.2499)***	(1.2795)***	(1.3165)***
unemp rate$_{c,t-3,t-1}$	-0.0528	-0.0284	-0.0125	-0.0687	-0.0340	-0.0202
	(0.0132)***	(0.0111)**	(0.0139)	(0.0184)***	(0.0161)**	(0.0180)
house prices$_{c,t+l,t+k}$ * home own rate$_{g}$	-0.0176	-0.0173	-0.0175	-0.0428	-0.0424	-0.0430
	(0.0045)***	(0.0044)***	(0.0044)***	(0.0068)***	(0.0067)***	(0.0067)***
house prices$_{c,t+l,t+k}$	0.0034	0.0048	0.0053	0.0155	0.0149	0.0126
	(0.0065)	(0.0072)	(0.0077)	(0.0094)*	(0.0104)	(0.0110)
equity prices$_{c,t+l,t+k}$ * equities own rate$_{g}$	-0.0080	-0.0073	-0.0073	0.0077	0.0090	0.0080
	(0.0035)**	(0.0033)**	(0.0033)**	(0.0045)*	(0.0045)**	(0.0044)*
consumer sentiment$_{g,t+l,t+k}$	0.0484	-0.0463	-0.0490	0.0458	-0.0820	-0.0539
	(0.0225)**	(0.0242)*	(0.0319)	(0.0282)	(0.0301)***	(0.0401)
consumer expectations$_{g,t+l,t+k}$	-0.0095	0.0579	0.0607	0.0220	0.1103	0.0926
	(0.0138)	(0.0231)**	(0.0312)*	(0.0184)	(0.0285)***	(0.0377)**
Memo: Trimester during which economic conditions and ui are measured	First l=0,k=2	Second l=3,k=5	Third l=6,k=8	First l=0,k=2	Second l=3,k=5	Third l=6,k=8
N	1,637,912	1,608,595	1,579,463	1,637,912	1,608,595	1,579,463

Notes: Estimated according to equation 2. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; the proportion of mothers who are married and the proportion of babies that are boys and twins; house prices, equity prices, consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.05 **p<0.01 ***p<0.001

Table 7. Unemployment, UI, and Maternal Health Behaviors

	(1)	(2)	(3)	(4)	(5)	(6)
	Obtained prental care in 1st Trimester		At least 5 prental care visits		Smoked during pregnancy	
unemp rate$_{c,t,\ t+8}$	0.2778	-0.2031	0.0190	0.0388	-0.1023	0.3186
	(0.0885)***	(0.4981)	(0.0386)	(0.1033)	(0.1063)	(0.2953)
unemp rate$_{c,t,\ t+8}$ * ui replacement$_{g,s,t,\ t-}$		0.8854		-0.0373		-0.7807
		(0.8992)		(0.1770)		(0.4480)*
unemp rate$_{c,t-3,t-1}$	0.2068	0.2067	0.0216	0.0204	0.0227	0.0204
	(0.0543)***	(0.0534)***	(0.0220)	(0.0219)	(0.0369)	(0.0373)
	N=1,499,852; Dep. Var. Mean = 81.5		N=1568,648; Dep. Var. Mean = 95.6		N=1,436,963 ; Dep. Var. Mean = 7.9	

Notes: Estimated according to equation 2. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; the proportion of mothers who are married; and the proportion of babies that are boys and twins; house prices, equity prices, consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.1 **p<0.05 ***p<0.001

Appendix for "The Cyclicality of Births and Babies Health, Revisited: Evidence from Unemployment Insurance"

Appendix A: Data Construction

Births and Infant Health Outcomes

To construct county-group-month births and infant health outcomes, we applied for and obtained access to the confidential Vital Statistics birth certificate data from NCHS, which includes information on the mother's state and county of residence.

We assign births to the month of conception using the month and year of birth and the reported weeks of gestation, where we assume all births occur mid-month (because day or week of birth is missing from the data). When weeks of gestation are missing, we assume 40.

For the infant health outcomes, we construct the proportion of births (dated to conception) in each county-group-month with a low birth weight (defined as a birth weight under 2500 grams) and that are born prematurely (defined as a gestational period less than 37 weeks). In robustness checks, we also consider the following health outcomes: very low birth weight (below 1500 grams), very preterm (less than 32 weeks), and birth weight and the following behavioral outcomes: if the mother obtained prenatal care during the first trimester of pregnancy, if the mother had at least 5 prenatal care visits, and if the mother smoked during pregnancy. We also calculate the proportion of births to married women, and the proportion of babies that are twins and boys.

There are well known issues with missing data in the Vital Statistics data. In particular, the CDC updated birth certificate reporting requirements in 2003, but not every state altered their birth certificates to conform to the new reporting standards right away leading to missing data from 2009-2013. The states with missing information during at least some of the 2009-13 period are: AL, AK, AZ, AR, CT, HI, IL, LA, ME, MD, MA, MN, MS, MO, NJ, NC, RI, VA, WV, WI. This missing information affects a number of variables, including mother's educational attainment, smoking during pregnancy and the month prenatal care began. For the latter two, which are variables we use as robustness check outcomes, we drop the missing state-years.

UI Benefit Generosity

We construct potential weekly UI replacement rates by first constructing a state-by-year weekly UI benefit calculator. The UI benefit calculator is based on information we collected on weekly benefit formulas found in Department of Labor publications from 2000-2019.[43] To

[43] For 2006-2020, these can be found at URLs of the following format (modifying the year): https://oui.doleta.gov/unemploy/pdf/uilawcompar/2020/monetary.pdf. For years prior to 2006, the format for the URL is: https://oui.doleta.gov/unemploy/content/sigpros/1990-1999/July1990.pdf. For the

construct the benefit calculator, we followed a similar procedure to Ganong, Noel and Vavra (2020) and recorded (a) the time period of wages the state uses (highest quarter, two highest quarters, three highest quarters, weekly, annual wages, etc.), (b) the percent of those period's wages used in the calculation of a weekly UI benefit, and (c) the cap on the maximum weekly benefit that can be received. When there were multiple formulas or maximums listed, we chose the higher formula or maximum.[44] When formulas include both a rate and intercept, we include both pieces of information in our calculation (in the table below, Pennsylvania is an example of such a formula). We do not use information on differences in benefits by the number of dependents.

The table below gives some illustrative examples of how we calculate the potential weekly UI replacement rate in various instances. Take the example of someone who earned $100,000 a year in Oregon. This person is eligible for the maximum benefit of $624 per week, since .0125 of their earnings exceeds that cap; the potential replacement rate is thus 624/(100,000/52) = .32. Now let's consider someone who earned $25,000 in the state of Florida. We assume evenly quarter earnings, so take ($25,000/4)*0.0385 = $240, which is below the cap of $275 and implies a potential replacement rate of 0.50.

Annual Wage and Salary Earnings	State	Year	Wage Concept (w)	Formula	Maximum	Weekly UI Benefit	Potential Replacement Rate
$100,000	OR	2019	Annual	0.0125w	$624	$624	0.32
$50,000	PA	2017	Highest Quarter	0.0392w+1.96	$569	$492	0.51
$25,000	KY	2007	Annual	0.0131w	$401	$328	0.68
$25,000	FL	2010	Highest Quarter	0.0385w	$275	$240	0.50

In our baseline we construct replacement rates using these benefit formulas for all women with non-zero wage and salary income between the age of 18-49 in the 2000 Census (based on the 5 percent sample, obtained from IPUMS-USA). We use a fixed sample of individuals to abstract away from cyclical fluctuations in population composition and realized incomes. We inflate this sample of women's wage and salary incomes using the CPI-U for each

maximum benefits, when possible, we used the Hsu, Matsa and Meltzer (2018) replication file data, which can be found at https://www.openicpsr.org/openicpsr/project/116160/version/V1/view.

[44] This differs from Ganong , Noel and Vavra (2020) who used the first formula listed. We chose the higher formula following Hsu, Matsa and Meltzer (2018), who chose the highest maximum benefit. The one exception to this rule is Alaska, where we used the lower formula and the highest maximum benefit. We did so because Alaska publishes tables with benefits amounts by income in each year, rather than using an explicit formula. When these tables are translated into a formula in the DOL publications it results in an unusually wide range, but only the very lowest income levels are eligible for the top of the range and the vast majority income levels are eligible for closer to the bottom of the range. Note that our results are robust to omitting Alaska entirely (not reported).

year of our sample (2000-2019), and then feed those annual real earned incomes through the UI benefit calculator based on each woman's state of residence for each year. Because Census records annual incomes, rather than quarterly incomes, we cannot exactly match many of the concepts used in the UI benefit calculator (e.g., highest quarter of the past four). Instead, we simply divide annual earned income by four to obtain quarterly income, two to two-quarter income, 52 to obtain weekly income, etc. We do not incorporate information on weeks or hours worked in these calculations. To account for federal expansions in benefit amounts, we add $25 a week to weekly benefits in 2009 and 2010, as provided by the Federal Additional Compensation program. Our constructed weekly UI replacement rate is the calculated weekly benefit amount divided by 1/52 times annual wage and salary income. Once we have the weekly benefit levels and replacement rates, we collapse by state, year, month, and group.

As noted above, in our baseline specification we construct replacement rates from earned income among women age 18 to 49 with non-zero wage and salary income. The collapsed median by state, year, and group thus reflects the replacement rate among this sample of women. In robustness checks, we use instead calculated means for this sample, as well as medians constructed from the following alternative samples: women who worked at least 26 weeks in the prior year (since many states have a 2-quarter work requirement for UI eligibility), men with non-zero wage and salary income, men who worked 26 weeks in the prior year, married men with non-zero wage and salary income, and unemployed men and women. Note that for the men's measures we use the exact same state-groups as we do for women, so the implicit assumption is that the economically dependent partner of a woman is in the same state-group as her.

We match the replacement rates to the birth data by calculating means of the each of these measures over (a) the three months prior to conception and (b) the nine months following (inclusive of) the month of conception. Although the data do not vary by month within in a state-year, many pregnancies scan multiple calendar years and by calculating means over these periods we allow for the possibility that state UI rules change prior to and within a pregnancy.

In other robustness checks, we also examine maximum durations of UI eligibility as an alternative measure of UI generosity. Regular and expansions to maximum durations were obtained from Farber, Rothstein and Valletta (2015) replication files. We use durations two ways, first, as a measure of generosity on its own and second, to scale our measure of *in utero* potential replacement rates to account for benefit exhaustion. That is, if maximum durations of benefit eligibility are 13 weeks, we create a scaled potential replacement rate of 13/40 * replacement rate.

Appendix tables 1-4 display summary statistics on the various calculations of UI potential replacement rates.[45] Replacement rates are in general lower for men than women, and for individuals who worked more of the previous year, reflecting higher median incomes. Mean replacement rates are lower than medians, since replacement rates fall when benefit caps bind. Appendix Table 2 shows state benefit levels in 2019, and in column 7, for comparison, we add Ganong, Noel and Vavra (2020) median replacement rates for unemployed individuals in 2020 (obtained from Appendix table A1 of Ganong, Noel, and Vavra, 2020). Although their measure is based on a different sample (no age restriction, only individuals who were unemployed in 2020 and eligible for UI based on their work history), the resulting replacement rates are quite similar.

Economic Variables

We collected information on county-level unemployment rates from the Bureau of Labor Statistics Local Area Unemployment Statistics. We collected information on county-month house prices from Zillow. Specifically, we used the Zillow Home Value Index (ZHVI) which represents the value of a typical home. We put this value in real dollars using the CPI-U-less shelter series, and put the series in the $10,000 units. We captured equity prices using the average monthly value of the Wilshire-5000 index, which we downloaded from the St. Louis Federal Reserve FRED database. This series was adjusted to real dollars using the CPI-U.

We collected information on consumer sentiment and expectations using the University of Michigan Survey of Consumers, which we obtained from the Michigan Survey Research Center. We construct these measures by age-group and region of residence, which is the lowest level of geography available in those data. To construct these measures at this level, we average the region-level and age-group level indices in order to construct a region-group level index. The indices are defined as the percent of positive responses minus the percent of negative responses plus 100, implying that a higher value of the index can be interpreted as more positive sentiment/expectations.

We constructed group-level measures of asset ownership rates using the 2001-2019 Survey of Consumer Finances (SCF) using the SCF Bulletin measures of home ownership and direct or indirect equity ownership, where indirect equity ownership refers to ownership via retirement accounts, mutual funds, etc.

Individual-level Fertility Data

[45] Because this data begins in 2000, our analysis sample begins in 2000Q2 since we use the prior quarters' UI benefit information for our analyses.

We downloaded Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC) data for 2001-2020 from IPUMS-CPS. The CPS ASEC data includes retrospective information on unemployment spells in the previous calendar year, the age of one's youngest child, various demographic information (age, race and ethnicity, marital status) and household information (home ownership), and state and county of residence (county is available for counties with over 100,000 residents). We define a birth as having occurred in the past year if the youngest own child in the home is under age 1 and we refer the prior calendar year as the "conception year". We limit the sample to women ages 18-49 as in our main analysis.

We merge our measure of unemployment insurance benefit generosity at the level of state and conception year. We also merge in information on the annual unemployment rates and median house prices at the level of conception year and county (when available) or state (when county is not available). For these measures, we define the state measure as the population weighted average of the county-level measures for all counties not reported in the CPS (that is, we omit counties for which we are able to merge the county-level data to the CPS from the state averages). Finally, we merge the measures of the sentiment and expectations by region and year and equity prices by year.

We use our state-group-year measure of UI benefit generosity to measure UI benefits rather than calculating individual-level UI benefits or using self-reported benefits for conceptual and practical reasons. Conceptually, as in our earlier analysis, we are interested in measuring changes in UI generosity owing to changes in policy rather than changes in the pool of unemployed individuals or changes in labor income owing to the economic environment. Practically, it would be difficult to construct such a measure because we only have information on income and hours worked for the portion of the prior calendar year that the individual was not unemployed, and furthermore, we do not know if that income and work occurred before or after the unemployment spell. This means we cannot accurately estimate a benefit level or replacement rate. We do not use information on self-reported UI income because of known issues with severe underreporting of such benefits.[46]

Individual-level health data from BRFSS

We use the 2000-2019 waves of the Behavioral Risk Surveillance System (BRFSS), obtained from the CDC, to study unemployed pregnant women's health behaviors and self-

[46] In our sample, only 20 percent of individuals who report being unemployed in the previous year report positive UI income. Similarly, only about 60 percent of individuals who report positive UI income report being unemployed last year.

rated mental and physical health.[47] We define a woman as pregnant if she reports that she is currently pregnant and we define a woman as unemployed if she reports that she is "out of work".[48] We examine the following health behaviors: whether a woman reports that she recently drank alcohol, that she recently smoked cigarettes (and if so, if she does so daily), and an index of the amount of fruits and vegetables she eats.[49] We also examine the following measures of self-reported health: the percent of bad mental or physical health days in the past month, and an indicator for having any bad mental or physical health days in the past month, similar to Evans and Garthwaite (2004). All specifications include state, interview year, interview month, race/Ethnicity, Age-group (in five-year categories), and marital status fixed effects, plus state-month-level house prices, state-month-level unemployment rates, and region-month level sentiment and expectations.

[47] The BRFSS survey design changed in 2011 and BRFSS warns samples are not directly comparable for the earlier and later periods of our sample. All of our specifications include year fixed effects and final survey weights to adjust for differences in the sampling frame.

48 We use current employment status, which has the following options: employed, self-employed, out of work for more than 1 year, out of work for less than 1 year, homemaker, student, retired, or unable to work. Note that we do not have information on whether she is looking for work in order to make this series comparable to an unemployment rate so we simply assign anyone out of work as unemployed.

49 The BRFSS questionnaire has changed over time and we harmonize the data where possible. For alcohol, the 2000-2014 data has number of alcoholic drinks consumed in the past month, and from 2015-2019, consumed in the past week. We assign all non-zero values as drinking while pregnant. For cigarette smoking, we assign individuals who respond that they currently smoke every day as daily smokers and those who respond that they currently smoke every day or some days as smokers. Fruit and vegetable consumption is available in 2000-2003 and odd years from 2003-2019. In 2000-2009 we use the fruit and vegetable index, which is defined less than one serving per day, one to three servings per day, three to five servings per day, and more than five servings per day. Beginning in 2011, the questionnaire contains separate counts of fruits and vegetables eaten per day which we transform into a comparable index.

Appendix B: Additional Tables and Figures

Appendix Table 1. Calculation potential UI replacement rates by state

	(1)	(2)	(3)	(4)
	ui replacement$_{g,s}$	ui benefit$_{g,s,t}$	income$_{g,s,t}$	max benefit $_{s,t}$
Alabama	0.51	200	20,718	246
Alaska	0.47	260	28,553	396
Arizona	0.49	206	22,791	232
Arkansas	0.51	186	19,063	410
California	0.52	246	24,785	420
Colorado	0.53	258	25,468	473
Connecticut	0.50	284	29,283	591
Delaware	0.55	280	27,102	328
District of Columbia	0.48	285	33,412	361
Florida	0.49	222	23,709	275
Georgia	0.58	270	24,481	316
Hawaii	0.62	294	24,451	510
Idaho	0.51	181	18,492	368
Illinois	0.48	235	25,429	528
Indiana	0.53	232	23,046	386
Iowa	0.58	247	22,281	477
Kansas	0.56	235	21,881	430
Kentucky	0.66	254	20,110	400
Louisiana	0.59	216	19,446	272
Maine	0.60	241	20,982	514
Maryland	0.53	309	30,369	374
Massachusetts	0.51	282	28,850	930
Michigan	0.54	244	23,378	354
Minnesota	0.51	253	25,990	555
Mississippi	0.48	187	20,686	235
Missouri	0.52	230	22,843	302
Montana	0.53	181	17,750	461
Nebraska	0.51	207	21,216	335
Nevada	0.53	252	24,923	379
New Hampshire	0.53	257	25,408	395
New Jersey	0.60	341	29,296	568
New Mexico	0.54	213	20,360	441
New York	0.51	255	26,181	408
North Carolina	0.51	230	23,647	422
North Dakota	0.51	194	19,774	544
Ohio	0.51	223	22,913	504
Oklahoma	0.57	208	18,929	388
Oregon	0.66	273	21,617	490
Pennsylvania	0.56	249	23,073	542
Rhode Island	0.58	253	22,896	633
South Carolina	0.51	217	22,301	313
South Dakota	0.51	206	21,141	347
Tennessee	0.50	210	21,855	284
Texas	0.53	229	22,656	416
Utah	0.50	181	18,838	452
Vermont	0.58	235	20,881	401
Virginia	0.52	258	25,538	353
Washington	0.51	233	23,799	576
West Virginia	0.55	191	18,011	403
Wisconsin	0.53	241	23,811	356
Wyoming	0.53	185	18,199	451

Note: Displayed are median potential weekly UI replacement rates (Column 1), median potential UI weekly benefits (Column 2), and median annual wage and salary income (Column 3) for 2000-2019, based on a sample of women of child-bearing age with non-zero wage and salary incomes in the 2000 Census. UI benefits calculated as detailed in appendix. Median incomes inflated to 2000-2019 dollars using CPI-U in Column 3. Column 4 list the average state maximum benefit from 2000-2019. Source: Census, DOL, BLS.

Appendix Table 2. Median UI Replacement Rates in 2019, Alternative Groups and Measures

	Women, Wages>0	Men, Wages>0	Women, Worked >26 weeks	Men, Worked >26 weeks	Unemployed women and men	Women, Wages>0, Mean	Unemployed, Eligible for UI (Ganong, Noel, and Vavra, 2020)
Alabama	0.48	0.38	0.47	0.37	0.50	0.43	0.47
Alaska	0.47	0.42	0.47	0.42	0.47	0.43	0.46
Arizona	0.47	0.37	0.42	0.34	0.51	0.41	0.34
Arkansas	0.50	0.50	0.50	0.50	0.50	0.48	0.50
California	0.50	0.46	0.50	0.46	0.50	0.46	0.50
Colorado	0.50	0.50	0.50	0.49	0.50	0.48	0.60
Connecticut	0.50	0.50	0.50	0.50	0.50	0.48	0.50
Delaware	0.51	0.43	0.50	0.40	0.54	0.46	0.57
District of Columbia	0.47	0.46	0.46	0.45	0.50	0.43	NA
Florida	0.47	0.40	0.46	0.38	0.50	0.42	0.47
Georgia	0.57	0.46	0.54	0.42	0.62	0.50	0.62
Hawaii	0.62	0.60	0.62	0.60	0.62	0.59	0.62
Idaho	0.50	0.47	0.50	0.46	0.50	0.48	0.50
Illinois	0.47	0.47	0.47	0.47	0.47	0.46	0.47
Indiana	0.47	0.42	0.47	0.42	0.47	0.44	0.47
Iowa	0.57	0.57	0.57	0.57	0.57	0.55	0.57
Kansas	0.55	0.51	0.55	0.51	0.55	0.52	0.55
Kentucky	0.62	0.57	0.62	0.57	0.62	0.58	0.62
Louisiana	0.64	0.46	0.59	0.41	0.68	0.57	0.39
Maine	0.59	0.59	0.59	0.59	0.59	0.58	0.59
Maryland	0.53	0.47	0.52	0.46	0.54	0.47	0.54
Massachusetts	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Michigan	0.53	0.43	0.52	0.40	0.53	0.47	0.53
Minnesota	0.50	0.50	0.50	0.50	0.50	0.49	0.50
Mississippi	0.47	0.38	0.46	0.36	0.50	0.42	0.41
Missouri	0.51	0.44	0.50	0.42	0.52	0.45	0.51
Montana	0.52	0.52	0.52	0.52	0.52	0.51	0.52
Nebraska	0.50	0.47	0.50	0.47	0.50	0.48	0.50
Nevada	0.52	0.49	0.52	0.49	0.52	0.49	0.52
New Hampshire	0.52	0.45	0.52	0.45	0.52	0.48	0.48
New Jersey	0.60	0.56	0.60	0.56	0.60	0.57	0.60
New Mexico	0.54	0.52	0.54	0.52	0.54	0.52	0.53
New York	0.50	0.45	0.50	0.45	0.50	0.45	0.50
North Carolina	0.50	0.45	0.50	0.44	0.50	0.45	0.50
North Dakota	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Ohio	0.50	0.50	0.50	0.50	0.50	0.49	0.50
Oklahoma	0.57	0.55	0.57	0.55	0.57	0.54	0.57
Oregon	0.65	0.62	0.65	0.61	0.65	0.62	0.51
Pennsylvania	0.51	0.51	0.51	0.51	0.52	0.51	0.51
Rhode Island	0.50	0.50	0.50	0.50	0.50	0.49	0.50
South Carolina	0.50	0.44	0.49	0.42	0.50	0.45	0.49
South Dakota	0.50	0.48	0.50	0.47	0.50	0.48	0.50
Tennessee	0.48	0.39	0.47	0.37	0.50	0.43	0.44
Texas	0.52	0.50	0.52	0.49	0.52	0.50	0.52
Utah	0.48	0.48	0.49	0.48	0.48	0.45	0.49
Vermont	0.58	0.55	0.58	0.55	0.58	0.55	0.58
Virginia	0.52	0.45	0.51	0.44	0.52	0.46	0.52
Washington	0.50	0.50	0.50	0.50	0.50	0.49	0.50
West Virginia	0.55	0.52	0.55	0.51	0.55	0.52	0.55
Wisconsin	0.52	0.45	0.52	0.43	0.52	0.47	0.52
Wyoming	0.52	0.50	0.52	0.50	0.52	0.51	0.52

Note: Table displays replacement rates across various subsamples in 2019, except the final column which displays median replacement rates in 2020 from Ganong, Noel, and Vavra (2020), Appendix table A1.

Appendix Table 3: Replacement Rates and Unemployment Rates by Year

	$ui\ replacement_{g,s,t}$		$unemp\ rate_{c,t}$	
Year	Mean	SD	Mean	SD
2000	0.53	0.06	3.91	1.27
2001	0.53	0.05	4.38	1.42
2002	0.53	0.04	5.70	1.54
2003	0.53	0.04	5.98	1.64
2004	0.53	0.04	5.56	1.56
2005	0.53	0.04	5.14	1.49
2006	0.52	0.04	4.72	1.44
2007	0.52	0.04	4.58	1.39
2008	0.52	0.04	5.48	1.66
2009	0.57	0.05	8.83	2.58
2010	0.58	0.05	9.67	2.54
2011	0.53	0.05	9.07	2.43
2012	0.52	0.04	8.19	2.29
2013	0.52	0.04	7.52	2.11
2014	0.52	0.05	6.35	1.84
2015	0.52	0.05	5.41	1.62
2016	0.52	0.05	4.95	1.51
2017	0.52	0.05	4.49	1.39
2018	0.52	0.05	3.98	1.23
2019	0.51	0.05	3.74	1.23

Appendix Table 4: Replacement Rates and Unemployment Rates by Group

	ui replacement$_{g,s,t}$		unemp rate$_{c,t}$	
Group	Mean	SD	Mean	SD
White (NH), 18-35	0.54	0.05	5.81	2.41
Black (NH), 18-35	0.54	0.05	6.16	2.43
Hispanic, 18-35	0.53	0.04	6.39	3.00
White (NH), 35-49	0.52	0.05	5.82	2.43
Black (NH), 35-49	0.53	0.05	6.19	2.44
Hispanic, 35-49	0.53	0.04	6.39	2.98

Appendix table 5: Different UI Replacement Rates, Births

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
unemp rate$_{c,t-3,t-1}$	-0.0601	-0.0391	-0.0604	-0.0648	-0.0625	-0.0552	0.0015	-0.0228	-0.0229
	(0.0151)***	(0.0103)***	(0.0162)***	(0.0162)***	(0.0158)***	(0.0145)***	(0.0202)	(0.0058)***	(0.0069)***
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$	0.0761	0.0355	0.0774	0.0939	0.0901	0.0782	0.0324	0.0008	0.0001
	(0.0217)***	(0.0130)***	(0.0247)***	(0.0294)***	(0.0292)***	(0.0292)***	(0.0398)	(0.0006)	(0.0000)***
Memo: Sample and measure used in ui replacement$_{g,s,t-3,t-1t}$	Women with Wage income>0	Women with Wage income>0, Mean instead of Median	Women, Worked 24 weeks last year	Men with Wage Income>0	Men who worked 24 weeks last year	Married Men with Wage Income>0	Married Men with Wage Income>0; Outcome is marital	Maximum Benefit Level ($100s)	Maximum Weeks of Eligibility
N	2,449,745	2,449,745	2,449,745	2,449,745	2,449,745	2,449,745	2,449,745	2,449,745	2,449,745

Notes: Estimated according to equation 1. Income measure refers to the group-level measure of income that is used to estimate UI replacement rates. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; house prices, equity prices, consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.1 **p<0.05 ***p<0.001

Appendix Table 6: Individual-level Unemployment, UI

	(1)	(2)	(3)
$\text{unemployed}_{i,t-1}$	-0.0060	-0.0072	-0.0232
	(0.0011)***	(0.0011)***	(0.0091)**
$\text{unemp rate}_{c,t-1}$		-0.0003	-0.0003
		(0.0003)	(0.0003)
$\text{unemployed}_{i,t-1}$ * $\text{ui replacement}_{g,s,t-1}$			0.0299
			(0.0171)*
$\text{ui replacement}_{g,s,t-1}$			-0.0432
			(0.0287)
Controls for house prices, sentiment and expectations	No	Yes	Yes
Observations	905,625	877,843	877,843

Notes: Estimated according to a modified version of equation 1 where the dependent variable is Pr(Baby Under Age 1) and the independent variable is an indicator for having been unemployed in the prior calendar year. Includes state, year, age, race/ethnicity group, and marital status fixed effects. Data sources is CPS ASEC, BLS LAU, Zillow, and Michigan SRC. Standard errors adjusted for clustering at the state-level in parentheses. *p<0.05 **p<0.01 ***p<0.001

Appendix Table 7: Unemployment, UI, and Births, by Demographic Group

	(1)	(2)
Panel A: Age-group		
unemp rate$_{c,t-3,t-1}$		
X I(Age 18-34)	-0.0118	-0.0343
	(0.0067)*	(0.0193)*
X I(Age 35-49)	-0.0269	-0.0499
	(0.0076)***	(0.0229)**
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$		
X I(Age 18-34)		0.0383
		(0.0278)
X I(Age 35-49)		0.0454
		(0.0381)
Panel B: Race-Ethnicity-group		
unemp rate$_{c,t-3,t-1}$		
X I(White - NH)	-0.0223	-0.0745
	(0.0069)***	(0.0187)***
X I(Black- NH)	-0.0349	-0.1321
	(0.0075)***	(0.0465)***
X I(Hispanic)	0.0021	0.0503
	(0.0130)	(0.0876)
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$		
X I(White - NH)		0.0955
		(0.0290)***
X I(Black- NH)		0.1759
		(0.0788)**
X I(Hispanic)		-0.0881
		(0.1543)
N	2449745	2449745

Notes: Estimated according to equation 1. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; log(group population), house prices, equity prices consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.05 **p<0.01 ***p<0.001

Appendix Table 8: Unemployment, UI and Births, Robustness of Specification and Data

	(1)	(2)	(3)	(4)
Panel A. Specification Adjustments				
unemp rate$_{c,t-3,t-1}$	0.0076	-0.1571	-0.0190	-0.0601
	(0.0053)	(0.0449)***	(0.0092)**	(0.0175)***
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$		0.3035		0.0761
		(0.0830)***		(0.0255)***
Memo: Adjustment	Add State-Year FE		Std Error Cluster by State	
	2,449,745	2,449,745	2,449,745	2,449,745
Panel B. Add lags and leads				
unemp rate$_{c,t-3,t-1}$	-0.0170	-0.0582	-0.0183	-0.0564
	(0.0029)***	(0.0132)***	(0.0065)***	(0.0145)***
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$		0.0762		0.0708
		(0.0217)***		(0.0223)***
unemp rate$_{c,t-6,t-4}$	-0.0023	-0.0024		
	(0.0053)	(0.0053)		
unemp rate$_{c,t,t+8}$			-0.0014	-0.0017
			(0.0030)	(0.0031)
	2,449,745	2,449,745	2,449,745	2,449,745
Panel C. Alternate dependent variables and groups				
unemp rate$_{c,t-3,t-1}$	-0.0095	-0.0239	-0.0203	-0.0464
	(0.0021)***	(0.0084)***	(0.0099)**	(0.0188)**
unemp rate$_{c,t-3,t-1}$ * ui replacement$_{g,s,t-3,t-1}$		0.0266		0.0481
		(0.0146)*		(0.0212)**
Memo:Adjustment	Dep. Var. is log(fertrate)		Groups by Age, County	
N	2,449,745	2,449,745	1,342,824	1,342,824

Notes: Estimated according to equation 1. Includes county, month of conception, and age-group-race/ethnicity group fixed effects; house prices, equity prices, consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC, and SCF. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.1 **p<0.05 ***p<0.001

Construction

Appendix Table 9: Different UI Replacement Rates, Infant Health

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Percent Low Birth Weight									
unemp rate$_{c,t,\ t+8}$	0.1634	0.1196	0.1587	0.1164	0.1114	0.1010	0.0615	0.0236	0.0168
	(0.0596)***	(0.0475)**	(0.0587)***	(0.0356)***	(0.0356)***	(0.0359)***	(0.0362)*	(0.0258)	(0.0360)
unemp rate$_{c,t,\ t+8}$ * ui replacement$_{g,s,t,\ t+8}$	-0.2294	-0.1410	-0.2231	-0.1561	-0.1480	-0.1324	-0.0056	0.0003	0.0447
	(0.0874)***	(0.0546)***	(0.0893)**	(0.0534)***	(0.0521)***	(0.0504)***	(0.0045)	(0.0002)	(0.0518)
unemp rate$_{c,t-3,t-1}$	-0.0371	-0.0381	-0.0371	-0.0376	-0.0376	-0.0378	-0.0369	-0.0385	-0.0364
	(0.0127)***	(0.0128)***	(0.0127)***	(0.0127)***	(0.0127)***	(0.0128)***	(0.0129)***	(0.0134)***	(0.0128)***
Panel B: Percent Premature									
unemp rate$_{c,t,\ t+8}$	0.4249	0.3068	0.3895	0.2577	0.2522	0.2206	0.2047	0.1003	0.1185
	(0.0958)***	(0.0643)***	(0.0893)***	(0.0486)***	(0.0477)***	(0.0460)***	(0.0547)***	(0.0397)**	(0.0592)**
unemp rate$_{c,t,\ t+8}$ * ui replacement$_{g,s,t,\ t+8}$	-0.5336	-0.2998	-0.4731	-0.2466	-0.2388	-0.1813	-0.0170	0.0008	0.0332
	(0.1411)***	(0.0777)***	(0.1321)***	(0.0690)***	(0.0667)***	(0.0657)***	(0.0081)**	(0.0003)**	(0.0935)
unemp rate$_{c,t-3,t-1}$	-0.0678	-0.0704	-0.0680	-0.0686	-0.0687	-0.0686	-0.0686	-0.0644	-0.0670
	(0.0195)***	(0.0195)***	(0.0194)***	(0.0193)***	(0.0193)***	(0.0193)***	(0.0195)***	(0.0198)***	(0.0194)***
Memo: Sample used to construct ui replacement$_{g,s,t,\ t+8}$	Women with Wage income>0	Women with Wage income>0, Mean instead of Median	Women, Worked 24 weeks last year	Men with Wage Income>0	Men who worked 24 weeks last year	Married Men with Wage Income>0	Maximum Benefit Level ($100s)	Maximum Duration	Women with wage income>0, Add Durations
N	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463

Notes: Estimated according to equation 2. Includes county, quarter of conception, and age-group-race/ethnicity group fixed effects; the proportion of mothers who are married; and the proportion of babies that are boys and twins; house prices, equity prices, consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.1 **p<0.05 ***p<0.001

Appendix Table 10: Unemployment, UI and Alternate Birth Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(5)	(6)	(5)	(6)
	Very Low Birth Weight (< 1500g)		Birth Weight		Very Preterm (<32 weeks)		Gestational Age		Share Male	
unemp rate$_{c,t,\ t+8}$	0.0420	0.0886	-1.2710	-4.6629	0.0599	0.1270	-0.0127	-0.0224	0.0036	0.0188
	(0.0175)**	(0.0406)**	(0.6005)**	(1.7241)***	(0.0181)***	(0.0415)***	(0.0037)***	(0.0099)**	(0.0218)	(0.0581)
unemp rate$_{c,t,\ t+8}$ * ui replacement$_{g,s,t,\ t+8}$		-0.0862		6.2740		-0.1240		0.0178		-0.0281
		(0.0485)*		(2.5922)**		(0.0526)**		(0.0145)		(0.0988)
unemp rate$_{c,t-3,t-1}$	-0.0202	-0.0204	0.6187	0.6471	-0.0277	-0.0279	0.0045	0.0045	0.0237	0.0236
	(0.0071)***	(0.0071)***	(0.3952)	(0.3919)*	(0.0085)***	(0.0085)***	(0.0020)**	(0.0020)**	(0.0185)	(0.0185)
	N=1,579,463 ; Dep. Var. Mean = 1.6		N=1,579,463; Dep. Var. Mean = 3291		N=1,579,463; Dep. Var. Mean = 2.1		N=1,579,463; Dep. Var. Mean = 38.5		N=1,579,463; Dep. Var. Mean = 51.1	

Notes: Estimated according to equation 2. Includes county, quarter of conception, and age-group-race/ethnicity group fixed effects; the proportion of mothers who are married; and the proportion of babies that are boys and twins; house prices, equity prices, consumer sentiment and expectations. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.1 **p<0.05 ***p<0.001

Appendix Table 11: Unemployment, UI and Infant Health, Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A: Percent Low Birth Weight													
unemp rate$_{c,t,\ t+8}$				0.0293	0.1354	0.0393	0.1634	0.0436	0.0588	0.0526	0.0862	0.0327	0.1181
				(0.0227)	(0.0547)**	(0.0356)	(0.0889)*	(0.0227)*	(0.0404)	(0.0441)	(0.0529)	(0.0143)**	(0.0446)***
unemp rate$_{c,t,\ t+8}$ * ui replacement$_{g,s,t,\ t+8}$					-0.1947		-0.2294		-0.0280		-0.0621		-0.1563
					(0.0891)**		(0.1156)*		(0.0619)		(0.0597)		(0.0711)**
unemp rate$_{c,t-3,t-1}$				-0.0510	-0.0512	-0.0364	-0.0371	-0.0419	-0.0420	-0.0366	-0.0370	-0.0330	-0.0335
				(0.0171)***	(0.0171)***	(0.0143)**	(0.0142)**	(0.0128)***	(0.0127)***	(0.0140)***	(0.0140)***	(0.0111)***	(0.0111)***
unemp rate$_{c,t+12,t+14}$										-0.0157	-0.0157		
										(0.0255)	(0.0255)		
Panel B: Percent Premature													
unemp rate$_{c,t,\ t+8}$				0.0641	0.0182	0.1360	0.4249	0.1417	0.0461	0.1195	0.0464	0.1211	0.4253
				(0.0317)**	(0.0840)	(0.0622)**	(0.1264)***	(0.0344)***	(0.0686)	(0.0567)**	(0.0753)	(0.0328)***	(0.0984)***
unemp rate$_{c,t,\ t+8}$ * ui replacement$_{g,s,t,\ t+8}$					0.0841		-0.5336		0.1770		0.1350		-0.5581
					(0.1426)		(0.1688)***		(0.1099)		(0.1050)		(0.1427)***
unemp rate$_{c,t-3,t-1}$				-0.0880	-0.0879	-0.0667	-0.0678	-0.0727	-0.0719	-0.0645	-0.0638	-0.0543	-0.0553
				(0.0261)***	(0.0261)***	(0.0282)**	(0.0295)**	(0.0191)***	(0.0191)***	(0.0205)***	(0.0205)***	(0.0183)***	(0.0185)***
unemp rate$_{c,t+12,t+14}$										0.0198	0.0199		
										(0.0402)	(0.0403)		
Memo: Specification				State-year FE	State-year FE	SE Clustering at State	SE Clustering at State	Add extra controls	Add extra controls	Add lead	Add lead	Group by county-age	Group by county-age
N				1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,579,463	1,222,124	1,222,124

Notes: Estimated according to equation 2. Includes county, quarter of conception, and age-group-race/ethnicity group fixed effects; the proportion of mothers who are married; and the proportion of babies that are boys and twins; house prices, equity prices, consumer sentiment and expectations, and credit supply. Data sources are Vital Statistics, SEER, BLS LAU, Zillow, Michigan SRC. Standard errors adjusted for clustering at the county-level in parentheses. *p<0.1 **p<0.05 ***p<0.001

Appendix Table 12: Unemployment, UI and Health Indicators from the BRFSS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Self-Rated Health								
	Any bad mental health days this month		Percent bad mental health days this month		Any bad physical health days this month		Percent bad physical health days this month	
unemployed$_{i,t-1}$	0.0177	0.1556	0.0300	0.0366	-0.0115	0.1146	0.0192	0.1032
	(0.0099)*	(0.1343)	(0.0063)***	(0.0765)	(0.0148)	(0.1386)	(0.0065)***	(0.0755)
unemp rate$_{c,t-1}$	-0.0095	-0.0092	-0.0036	-0.0035	0.0062	0.0066	-0.0008	-0.0006
	(0.0041)**	(0.0042)**	(0.0022)	(0.0023)	(0.0043)	(0.0047)	(0.0019)	(0.0020)
unemployed$_{i,t-1}$ * ui replacement$_{g,s,}$		-0.2590		-0.0125		-0.2368		-0.1576
		(0.2543)		(0.1414)		(0.2437)		(0.1378)
ui replacement$_{g,s,t-1}$		0.2676		0.0655		0.3751		0.0857
		(0.1094)**		(0.0538)		(0.1413)**		(0.0650)
Mean of Dep. Var.	0.36	0.36	0.11	0.11	0.31	0.31	0.08	0.08
Observations	48,315	48,315	48,315	48,315	48,108	48,108	48,108	48,108
Panel B: Health Behaviors								
	Drinks Alcohol		Smokes		Smokes Daily		Fruit and Vegetable Index (1-4)	
unemployed$_{i,t-1}$	-0.0180	0.0943	0.0770	0.0250	0.0551	-0.0397	-0.0602	-0.6154
	(0.0071)**	(0.0662)	(0.0120)***	(0.1113)	(0.0115)***	(0.1121)	(0.0239)**	(0.3292)*
unemp rate$_{c,t-1}$	-0.0012	-0.0012	-0.0034	-0.0034	-0.0013	-0.0013	-0.0109	-0.0101
	(0.0025)	(0.0023)	(0.0033)	(0.0033)	(0.0023)	(0.0023)	(0.0091)	(0.0086)
unemployed$_{i,t-1}$ * ui replacement$_{g,s,}$		-0.2108		0.0976		0.1779		1.0377
		(0.1242)*		(0.2029)		(0.2082)		(0.6140)*
ui replacement$_{g,s,t-1}$		-0.2174		-0.0066		0.0197		0.3982
		(0.0737)***		(0.0871)		(0.0840)		(0.3825)
Mean of Dep. Var.	0.1	0.1	0.1	0.1	0.06	0.06	3.02	3.02
Observations	47,951	47,951	49,771	49,771	49,771	49,771	26,926	26,926

Notes: Estimated according to a modified version of equation 2 where the dependent variable is listed in the column heading and the independent variable is an indicator for currently being unemployed. Sample is women who are currently pregnant. Includes state, year, month, age, race/ethnicity group, and marital status fixed effects, and state-month-level house prices, state-month-level unemployment rates, and region-month level sentiment and expectations. All regressions use survey weights. Data sources is 2000-2019 BRFSS, BLS LAU, Zillow, and Michigan SRC. Standard errors adjusted for clustering at the state-level in parentheses. *p<0.05 **p<0.01 ***p<0.001