Does the Delivery of Primary Health Care Improve Birth Outcomes? Evidence from the Rollout of Community Health Centers *

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Abstract

Introduced as part of the War on Poverty, Community Health Centers (CHCs) deliver primary care to underserved populations by locating sliding-scale clinics in economically disadvantaged areas. We investigate how this policy affected infant health using the rollout of CHCs and a flexible event study framework with Vital Statistics natality data. Our results show that average birth weight increased, and low birth weight incidence decreased after a CHC opened in the mother's county of residence. These improvements in infant health can be explained by increased access to early prenatal care and reductions in maternal smoking.

Keywords: Health care access, Infant health
JEL Codes: I38, J13, O15

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1 Introduction

Health disparities across socioeconomic groups are an important concern to policymakers because they start at birth and persist throughout one's lifetime, and health at birth is a key determinant of human capital, well-being, and economic self-sufficiency in the long run (e.g., Currie, 2011; Black et al., 2007). To address these gaps, the federal government has invested in demand- and supply-side polices that serve low income populations including pregnant women and young children. Demand-side policies to address gaps in nutrition and access to health insurance include Food Stamps, the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), and Medicaid. One of the critical supply-side polices that directly delivers primary care is Community Health Centers (CHCs). There is a large body of causal evidence that documents the effects of nutrition and health insurance programs on health outcomes across the life cycle (Almond et al., 2011a, 2017; Bailey et al., 2020; Hoynes et al., 2011; Currie and Gruber, 1996; East et al., 2017), but evidence is limited on the causal effects of the delivery of primary care services at scale on health outcomes (Bailey and Goodman-Bacon, 2015).

This paper examines the effects of the introduction of the CHC program on infant health outcomes and explores potential mechanisms. Established as part of the War on Poverty (WOP), CHCs have provided both primary and preventive care to medically underserved communities since 1965. While CHCs do not specifically target their services to pregnant women, almost all of them provide prenatal and postpartum care as well as voluntary family services (Goldman and Grossman, 1988). Indeed, women of childbearing age are the second-most common users of CHCs, representing around 30% of users by the end of the 1980s (Rosenbaum, 1987).[1] To understand how the increased availability of these primary care services affected infant health, we analyze birth weight and the incidence of low birth weight as our main outcomes, both of which are commonly used as proxies for health at birth.

[1]The first-most common group of CHCs users are children under the age of 14 (36% of the users).

Individuals who are healthier at birth are more likely to be healthier adults, with improved educational and labor market outcomes (Almond et al., 2017).

Our main outcome data are from Vital Statistics birth records between 1968 and 1988, which include rich information on infant health and maternal characteristics including county of residence, age, race, and, in some states and years, marital status and educational attainment. For the rollout of CHCs, we extend data on CHCs' introduction between 1965 and 1980 compiled by Bailey and Goodman-Bacon (2015) using the Federal Assistance Awards Data System (FAADS) from the US Census Bureau until 1988.[2] Combining the outcome data with the introduction of CHCs, we assign the treatment based on birth year and county of residence at birth. We also explore the effect of CHCs separately by maternal characteristics to assess whether the impacts are stronger for low socioeconomic status women, who are more likely to use CHC services. To analyze the potential mechanisms, we use data from the 1979 National Longitudinal Survey of Youth (NLSY79) and the Survey of Health Services Utilization and Expenditures (SHSUE) in addition to Vital Statistics.

Our empirical strategy uses a difference-in-differences design that exploits geographic and time variation in the introduction of CHCs. We use the robust difference-in-differences estimation strategy introduced by Sun and Abraham (2020) after diagnosing the presence of treatment effect heterogeneity associated with the standard two-way fixed effects approach (de Chaisemartin and D'Haultfœuille, 2020). We first present our estimates using static difference-in-differences and then dynamic event study models. To limit concerns about potential endogeneity of program placement, our preferred specification follows previous WOP studies and controls for county-of-birth fixed effects and 1960 county characteristics interacted with flexible trends (Bailey and Goodman-Bacon, 2015; Almond et al., 2011a; Bailey et al., 2020, 2021). Moreover, we show that the trends in outcomes are not statistically

[2]Our analysis spans up to 1988 because in 1989, the federally qualified health centers (FQHC) were created by Congress. With the new designation, the financing and reimbursements options changed in response to low Medicaid reimbursement rates paid in some states (Wolfe, 2013).

different between treated and control counties before the introduction of CHCs.

Our results show that the introduction of CHCs led to significant improvements in infant health. We find that maternal exposure to CHCs increased average birth weight between 3 to 6 grams (0.08% of the mean) and reduced the incidence of low birth weight by 1%. These intent-to-treat (ITT) estimates reflect the average effect for the whole population in a county. Scaling the ITT effects by approximate CHC participation rates among pregnant women (between 6.4% and 10.7%) implies that the average treatment effect on the treated (TOT) is between 25 and 42 grams for birth weight and between 9% and 16% for low birth weight. These effect sizes are comparable to other TOT estimates of policy interventions on infant health, including Food Stamps and WIC (Almond et al., 2011a; Hoynes et al., 2011).

Based on the target population of CHCs, we expect the effects to be concentrated among low socioeconomic status mothers. Indeed, we find that CHCs' impacts on infant health are stronger for low-educated mothers. Moreover, our findings are robust to including various control variables, alternative fixed effects and trends, and timing measures. Lastly, we show that these effects are not driven by fertility responses or changes in the composition of women giving birth.

To explain potential mechanisms underlying the effects on infant health, we show that CHCs' introduction improved access to prenatal care, in particular in the first trimester. In addition, we find that mothers exposed to CHCs before their child's birth experienced a large, statistically significant reduction in smoking during pregnancy.[3] This finding is critical because smoking is one of the primary risk factors for intrauterine growth restrictions, which leads to smaller infants. Moreover, maternal smoking is negatively related to access to prenatal care: women with reduced access to prenatal care, especially early in pregnancy, are more likely to report smoking during pregnancy (Evans and Lien, 2005). Additionally, we

[3]We also provide suggestive evidence that CHCs' introduction led to mothers reduce their alcohol use during pregnancy.

provide evidence that childbearing-age women without a high school degree living in areas with a CHC were more likely to have a regular source of health care, pointing to improved pre-conception health as another potential mechanism.

Our paper contributes to several strands of literature. In the CHC literature, the best available causal evidence of the effects of the CHC program comes from Bailey and Goodman-Bacon (2015), who study the causal effect of the rollout of CHCs from 1965 to 1974 on adult mortality (age 50 plus). They find significant reductions in age-adjusted adult mortality rates up to 15 years after CHCs' introduction. We contribute to this literature by studying another important beneficiaries of CHCs: pregnant women. We provide new causal evidence that CHCs improved several markers of birth outcomes up to seven years after their introduction. To understand the potential mechanisms underlying the long-run effects on infant health outcomes, we use several data sources that allowed us to examine the maternal and infant experiences following CHCs' introduction. This is important because the understanding of mechanisms can also inform the design of policies that are more effective.

Prior CHC literature provides descriptive evidence that these centers may improve infant birth outcomes. For example, Goldman and Grossman (1988) use cross-sectional analyses and find that an increase in the number of CHCs in the 1970s was associated with reductions in infant mortality rates in exposed counties, with larger effects for Black people than White people. Studies that analyze more recent CHC expansions find positive associations between primary care availability and infant health outcomes (e.g., Shi, Macinko, Starfield, Xu, Regan, Politzer, and Wulu, 2004; Shi, Stevens, Wulu, Politzer, and Xu, 2004). We build on this work by using modern panel data techniques to control for the non-random assignment of CHCs and reduce endogeneity bias.

Finally, our study contributes to the literature that shows that WOP programs have been effective in improving outcomes for individuals who were exposed to the programs' introduction (e.g., Bailey et al., 2020, 2021; Currie and Gruber, 1996; East et al., 2017;

Goodman-Bacon, 2018, 2021; Hoynes et al., 2016; Ludwig and Miller, 2007). While these studies have examined human capital investments related to early childhood education, nutrition, and health insurance, we study health investments related to direct provision of primary care services which improved prenatal care access. Interestingly, the magnitudes of our effects on birth outcomes are similar to those from Food Stamps, WIC and Medicaid, which suggests that different types of policies that promote different investments can generate similar effects.

The rest of the paper is organized as follows. In Section 2, we provide background information about CHCs' introduction, the prior CHC literature, and evidence on the exogeneity assumption. Section 3 describes our data sources, and Section 4 introduces the empirical methodology followed by the results in Section 5. In Section 6 and 7, we present potential testable mechanisms and the robustness of our results. Section 8 concludes.

2 The Community Health Center Program

2.1 Background

The CHC program was established with the passage of the Economic Opportunity Act in 1964, which also created the Community Action Program (CAP) as part of the Office of Economic Opportunity (OEO) (Wolfe, 2013). Focusing on a community empowerment approach, CHCs (known originally as Neighborhood Health Centers) provide primary and preventive health services as well as referrals to hospitals if additional care is needed (Erickson, 2013). Access to services is based on residence only, and the population they serve includes low-income families, the uninsured, migrants, and homeless people (Levitan, 1969).

The first CHCs were established in 1965 in Boston, Massachusetts, and Mound Bayou, Mississippi. The initial phase of grants funded CHCs mainly in urban areas with high concentrations of poverty. By August 1968, 32 centers were operating and 75% of them

were in urban areas (Levitan, 1969). By the early 1970s, the federal government had funded over 100 centers through the OEO grants (Erickson, 2013). However, federal involvement in supporting the provision of health services declined during Richard Nixon's presidency, and the OEO was dismantled. By 1974, grants were awarded through the regional Department of Health, Education, and Welfare offices rather than through the federal government (Erickson, 2013). Due to disagreements in the administration, the program was stagnant during this time, and the number of CHCs only increased slightly in the first half of the 1970s (Figure 1, Panel A).

In 1975, despite a lack of support from the Ford administration, Congress formalized the CHC program. This legislation required organizations receiving CHC grants to serve medically underserved areas, which were defined based on the percentage of the population below the federal poverty line, the infant mortality rate, the percentage of the population over 65, and the primary care physician to population ratio (Wolfe, 2013). This new legislation shifted the focus and funding of CHCs toward rural areas. Between 1975 and 1980, during the Ford and Carter administrations, the number of counties with a CHC increased significantly, and by 1980, 622 counties had at least one CHC.

In the early 1980s, the Reagan administration showed opposition to the CHC program. Reagan eliminated all categorical health service grants and created five block grants where CHC became part of a primary care block grant via the 1981 Omnibus Budget Reconciliation Act (Erickson, 2013). From 1984 to 1986, Congress continuously rejected a health center block grant that would be administered by states, and the CHC remained federally funded. In 1986, because of overwhelming Congressional support, Reagan signed the Health Services Amendment Act of 1986 that repealed the primary care block grant and authorized funding for CHCs as categorical grants through FY 1988 and increased state involvement in the program. During the 1980s, the creation of CHCs increased slightly (Erickson, 2013). Between 1981 and 1988, CHCs were introduced in 111 counties, and by 1988, 733 counties

in total had at least one CHC (Figure 1, Panel A).

Our analysis spans up to 1988 because in 1989, the federally qualified health centers (FQHC) were created by Congress. With the new designation, the financing and reimbursements options changed in response to low Medicaid reimbursement rates paid in some states (Wolfe, 2013).

CHC Services and Served Population

CHCs were designed to comprehensively serve the health needs of individuals living in medically underserved and poor areas. They focus on providing primary and preventive health services as well as health education, and some also have in-house pharmacies to ensure patients can access necessary medication. In cases where more specialized or acute care is needed, practitioners at CHCs can refer patients to local hospitals (Wolfe, 2013).

CHCs are staffed by multidisciplinary teams of clinical personnel including general practitioners, internists, OB/GYNs, pediatricians, nurses, and nurse practitioners/physician assistants (Dievier and Giovannini, 1998). While children make up the majority of patients, women of childbearing age are the second largest group, representing 28.6% of users (Rosenbaum, 1987). Consequently, prenatal care is an important part of the primary care services provided by CHCs. Obstetrics patients account for the second-most patient days and number of admissions (National Association of Community Health Centers, 1984). Almost all CHCs (85%) provide prenatal care, serving over 200,000 pregnant women per year by the end of our study period (Witwer, 1990).

Evidence from national-level surveys shows CHCs were an important source of prenatal care during the study time period. Appendix Figure A.1 shows the percentage of women who reported receiving prenatal care at a "clinic" in the first four cycles of the National Survey of Family Growth (NSFG). In the 1973 and 1976 cycles, all types of clinics were grouped together. Starting in 1982, the clinic category was broken into more detailed categories,

including CHCs specifically. Approximately 80% of pregnant women received care from a private practitioner (Institute of Medicine, 1999). But for the 20% of women who could not access these sources, CHCs and other public clinics were (and still are) an important source of prenatal care. The largest increase in the use of clinics in Appendix Figure A.1 occurred between the 1976 and 1982 waves of the NSFG. This corresponds to the largest increase in the number of counties with CHCs, which also occurred in the late 1970s. This is suggestive evidence that pregnant women received primary care services from CHCs during the rollout period we analyze.

Consistent with the goal of providing primary care to underserved populations, most CHC patients' family income was below the poverty level. According to a 1984 survey of CHC users, 60.8% had family incomes at or below the poverty line, while only 13.7% had an income above 200% of the poverty level. Their patients also had relatively low levels of education: 45.4% reported 1–8 years of education, and 39.3% reported 9–12 years. Only 15% reported more than a high school education (National Association of Community Health Centers, 1984). While we cannot observe income in our outcome data, we examine heterogeneity by maternal education as we expect stronger effects for low-educated mothers.

2.2 The Rollout of CHCs between 1965 and 1988

From 1965 to 1980, CHCs' introduction year at the county level comes from data collected by Bailey and Goodman-Bacon (2015). From 1965 to 1974, they use multiple sources of information including the National Archives Community Action Program, Public Health Services Reports, and OEO reports. From 1975 to 1980, they use data from the National Archives Federal Outlays files. By 1980, CHCs were established in 622 counties (Figure 1, Panel A).

Because our analyses cover the years between 1968 and 1988, we supplement the rollout compiled by Bailey and Goodman-Bacon (2015) by extending it up to 1988. We use data

from the FAADS to identify the counties of organizations awarded grants related to CHCs. These data allow us to identify the introduction of CHCs in 111 additional counties between 1981 and 1988 (Figure 1, Panel A).

Figure 1, Panel B depicts the rollout of CHCs across counties over time. The map clearly illustrates that there is considerable variation both across time and within states. As discussed above, the initial funding introduced CHCs in more urban areas, and after 1975, it shifted to establishing centers in more rural counties. Appendix Table A.1 presents 1960 characteristics of counties that received CHCs between 1965 and 1988. Counties where CHCs were established between 1965 and 1974 have 58% to 76% of their population in urban areas.

For counties that received CHCs after 1975, the urban population drops to 36%. Consistent with this, counties that introduced CHCs in the initial phases of funding had lower poverty rates (as measured by the share of families with incomes below $3,000, the 1960 poverty line for a family of four) than counties that introduced CHCs after 1975 or did not receive CHCs between 1965 and 1988. Early CHC counties were also more educated, more affluent and had a larger number of physicians per capita in 1960. These observed differences and the potential for unobservable time-invariant county characteristics correlated with establishing CHCs during this period motivate us to include county fixed effects in our analyses.

Figure 1, Panel B also illustrates the presence of significant within-state variation in the establishment of CHCs. This variation is useful for our analysis as it allows us to include state-by-year fixed effects, which absorb time-varying changes in state policies such as Medicaid expansions, access to contraception, and others that may be correlated with CHCs' rollout. We show this specification as part of our robustness checks in Section 7.

Our identification strategy requires that the rollout's timing is exogenous after conditioning on covariates. The previous discussion suggests that counties with certain characteristics

were more likely to receive CHCs in the earlier phases. This potential systematic variation in CHCs' introduction could contaminate our estimates of the CHCs' impacts if those characteristics are associated with differential trends in our outcomes of interest. To provide evidence on this assumption, we test whether 1960 county characteristics (pre-program) predict the timing of a county's CHC opening, following a similar approach used by other WOP studies (Hoynes and Schanzenbach, 2009; Bailey, 2012; Bailey et al., 2020).[4] The results, presented in Table 1, include models with and without state fixed effects, both unweighted and weighted by county population in 1960.

Columns 1–3 focus on all the counties that adopted CHCs between 1965 and 1988. They show that counties that are more urban, more non-White, have a higher number of MDs per capita, and have a larger fraction of poor families implemented CHCs earlier. Furthermore, counties with a larger fraction of educated adults introduced CHCs later. Some of these associations are no longer significant when we include state fixed effects (column 3) except for urban and non-White percentages as well as MDs per capita, which are consistent with the political economy of the program (Wolfe, 2013).

Because the Vital Statistics data start in 1968, we do not observe pre-treatment outcomes for counties that adopted a CHC before 1968 and do not include them in the analyses.[5] Columns 4–6 present models for our analytical sample. For this sample, urbanicity, the non-White share of the population, and the supply of physicians are associated with a CHC opening earlier. However, the quantitative importance of these predictors is smaller than for the full set of counties (in columns 1–3), and most of the variation remains unexplained (as illustrated by the low R-squared between 0.06 and 0.32). This benefits our identification strategy as there is no strong evidence of endogeneity in the timing of CHCs. However, unobservables may still be a concern. To ameliorate this issue, our preferred specification in-

[4]The dependent variable is the year of CHCs' introduction expressed as an index equal to one beginning in 1965.

[5]Only 39 counties introduced CHCs by 1968.

cludes interactions of significant 1960 pre-treatment county characteristics with flexible time trends to control for differential trends across counties that could be spuriously correlated with CHCs' effects.[6]

3 Data

Data on birth outcomes are from Vital Statistics birth records from 1968 to 1988.[7] These data are coded from individual birth records and represent either a 50% or 100% sample of births occurring in each state. Our primary outcomes of interest are birth weight and if the infant is considered low birth weight (birth weight $<$ 2,500 grams).

To better understand the effects and explore mechanisms, we examine several additional outcomes from the birth certificate: if the infant was classified as having a very low birth weight (birth weight $<$ 1,500 grams), the gestational age of the infant, if the infant was small for gestational age,[8] and whether the infant was born prematurely (before 37 weeks of gestation). We also examine if the mother received any prenatal care during pregnancy and if prenatal care began in her first trimester.

The birth certificate data also include information on the mother's age, race, and, in some states and years, marital status and educational attainment. We restrict the sample to White and Black women ages 15–39 at the time of the birth. The data are collapsed into cells due to computational intensity. To preserve our ability to use fixed effects to control for socioeconomic and demographic factors, cells are defined by county, mother's age group (15–19, 20–24,..., 35–39), mother's race (Black or White),[9] mother's marital status (unmarried, married, missing), mother's education (less than high school, high school, more than high

[6] A similar approach has been used by other studies on the impact of WOP programs, such as Food Stamps (Almond et al., 2011b).

[7] Vital Statistics micro data are available starting in 1968, and county identifies are publicly available through 1988.

[8] "Small for gestational age" is defined as being in the bottom 10% in birth weight of all infants born at the same gestational age in the same year.

[9] In early years of the Vital Statistics, races other than White or Black are often coded simply as "other," so we cannot track births to women of other races consistently throughout the sample period.

school, missing), parity, and year.

We combine these data with the CHCs' rollout and county-level characteristics. These include county-level transfers per capita, hospital beds per capita (Bailey and Goodman-Bacon, 2015), and the White and non-White share of Aid to Families with Dependent Children (AFDC) women and children at the state level as a proxy for Medicaid access compiled by Goodman-Bacon (2018). We also include information on the rollout of contemporaneous policies including Food Stamps from Almond et al. (2011a), family planning programs from Bailey (2012), and WIC.[10]

4 Research Design

We exploit time and geographic variation in the rollout of CHCs to estimate the causal effect of access to primary health care services on birth outcomes. We use a difference-in-differences design and a flexible event study framework to account for dynamic treatment effects. Our specification is the following:

$$Y_{ijt} = \sum_{y=-8^{+}}^{-2} \pi_y D_j 1\left(t - T_j^* = y\right) + \sum_{y=0}^{8^{+}} \tau_y D_j 1\left(t - T_j^* = y\right) + \mathbf{M}_{it}'\boldsymbol{\alpha} + \mathbf{X}_{jt}'\boldsymbol{\beta} + \theta_j + \delta_t + \varepsilon_{ijt} \tag{1}$$

where Y_{ijt} is the birth outcome of infants whose mothers are in cell i born in county j and year t. Cells are defined by parity and mother's age, race, marital status, and educational attainment. D_j is a binary treatment variable equal to one if the county ever received a CHC grant. $1(t - T_j^* = y)$ are event-year dummies that represent the seven years before and after the CHC was opened. Observations eight years or more before CHC establishment are captured by the dummy $1(t - T_j^* <= -8)$. Similarly, observations eight years or more after

[10] The WIC rollout data corresponds to Hoynes et al. (2011), supplemented by ongoing work with National Archives documents by Bitler, Horn, Kose, Rosales-Rueda, and Seifoddini.

are captured by the dummy $1(t-T_j^* >= 8)$. τ_y are the coefficients of interest, which show how the selected outcome evolves over time after a CHC was opened. π_y show the pre-treatment changes in birth outcomes in eventually treated counties relative to untreated counties. These estimates will allow us to test the assumption of parallel pre-trends that the timing and location of CHCs are uncorrelated with pre-program changes in birth outcomes. We expect that these coefficients will be small and statistically insignificant if birth outcomes in CHC and non-CHC counties were trending similarly before the program's introduction.

θ_j is a county fixed effect that absorbs time-invariant characteristics that could be correlated with the CHCs' rollout, and δ_t are year fixed effects that capture time-varying national changes.[11] $\mathbf{M}_{it}$ includes a constant and cell-level sociodemographic characteristics such as mother's education, marital status, and age. As male infants are, on average, heavier at birth (Lehre et al., 2013), we also control for the portion of births in the cell that are male.[12] $\mathbf{X}_{jt}$ includes time-varying county and state characteristics that could confound the effect of CHCs: county-level transfers per capita, hospital beds per capita, the rollout of Food Stamps and family planning programs, and the White and non-White share of AFDC women and children at the state level as a proxy for Medicaid access. Regressions are weighted by the number of births in the cell. Due to computational intensity, all results presented here are estimated at the cell level; the results are identical when we perform our analyses at the individual level.

The standard practice is to estimate these models using two-way fixed effects (TWFE). However, recent developments in econometrics have shown that this method may be biased in staggered designs such as the rollout used in this paper (Goodman-Bacon, 2021; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020). Causal interpretation in the standard setting fails if treatment effects vary across cohorts (groups of counties that adopted the

[11] We show robustness to state-by-year fixed effects in Section 7.

[12] It is worth noting that this male variable is not an endogenous control as it is not responding to the presence of CHCs. In fact, we do not find evidence that the birth cohort sex ratio is affected by exposure to CHCs.

policy in the same year) or across time. Bias arises because earlier-treated units are used as controls for later-treated units, which is only valid in the case of constant treatment effects. In the presence of heterogeneous treatment effects, this comparison will lead to bias since the parallel trends assumption no longer holds.

To assess whether treatment effect heterogeneity is a potential issue in our setting, we perform a diagnostic test proposed by de Chaisemartin and D'Haultfœuille (2020). The authors show that the TWFE estimator is a weighted sum of several difference-in-differences across cohorts and periods;[13] these weights sum to one, and some may be negative. Their proposed test first estimates the standard TWFE, extracts the weights, and examines whether these weights are correlated with factors that are associated with the size or the intensity of the treatment effects. In our context, these test results are presented in Appendix Table A.2. The TWFE weights are correlated with treatment year, maternal education, hospitals per capita, and state AFDC availability, indicating the presence of treatment effect heterogeneity. This evidence shows that TWFE may not be appropriate in our setting because heterogeneous treatment effects can lead to bias.

For this reason, we use a procedure developed by Sun and Abraham (2020) that accounts for dynamic treatment effects across cohorts. This methodology divides units into different cohorts depending on when they receive the policy, plus a never-treated group that is used as a comparison group. The estimation procedure identifies the cohort-specific average treatment effect on the treated, $\text{CATT}_{e,l}$, in relative time period l for the cohort who received the intervention at time e. Their interaction-weighted estimator uses a saturated regression, interacting relative time dummies with cohort dummies (excluding indicators for the comparison group). Then, the estimated CATTs are averaged across a given relative

[13]These difference-in-differences can be of the following types: for early treated groups using never treated as a comparison, for later treated groups using never treated as a comparison, for early treated groups using later treated as a comparison, and for later treated groups using early treated as a comparison. The last type is particularly problematic as early treated groups may not be a valid comparison for later treated groups in the presence of treatment effect heterogeneity (Goodman-Bacon, 2021).

time using the sample share of each cohort as weights. This method gives a consistent estimate of the treatment effect under the parallel trends and no anticipation assumptions. Sun and Abraham (2020) show that their estimator is robust to heterogeneous treatment effects.

5 Main Results

5.1 Full Sample

In this subsection, we present estimates from both static and dynamic difference-in-differences models for birth weight (in grams) and the incidence of low birth weight (less than 2,500 grams). We start with our baseline specification, which includes county and year fixed effects and demographic controls (Table 2, columns 1 and 4). We then add time-varying state and county controls (columns 2 and 5) and 1960 county variables interacted with year fixed effects (columns 3 and 6). While it is reassuring that our results are not sensitive to model specification, our preferred specification is the one that includes time-varying state and county controls as well as 1960 significant determinants interacted with flexible trends.

Table 2, Panel A presents the results using a static difference-in-differences design where we find that CHCs' introduction improved infant health outcomes. The results for birth weight indicate that CHCs led to a small increase in birth weight of 3 to 6 grams. Relative to the pre-period mean of 3,300 grams, our preferred specification in column 3 implies a 0.08% increase in average birth weight. Turning to the incidence of low birth weight, we also find that there was a reduction of 0.0009 percentage points with the exposure to a CHC during pregnancy. Relative to the mean, this corresponds to a 1% decrease.

Figure 2 presents the event study coefficients that capture dynamic effects before the introduction of CHCs (–7 to –2 years) and after the introduction (0 to 7) using bins to increase statistical precision (Bailey and Goodman-Bacon, 2015).[14] Importantly, for our

[14]Table 2, Panel B presents the coefficients and the standard errors on the underlying regression results

empirical strategy, the coefficients in the periods before the introduction of CHCs are small and do not show statistical evidence of differential trends in infant health outcomes, which confirms the parallel trends assumption. In the post-period, we see a sharp increase in birth weight that persists in the long run. For low birth weight, we see a gradual reduction that attenuates in the longer run.

Overall, the magnitudes of these ITT effects are small on the full population; however, recall that most pregnant women do not receive care at a CHC. In the next subsection, we will attempt to estimate the CHCs' impacts on the population that potentially received their services. Our ITT estimates on birth weight and low birth weight are within the ranges of those found by other WOP programs. For example, Almond et al. (2011b) find that the Food Stamp program's introduction increased birth weight by around 2 grams for White infants and 4.3 grams for Black infants while reducing the likelihood of low birth weight by 1% and 1.3% for White infants and Black infants, respectively. Similar to our effect sizes, Hoynes et al. (2011) estimate that the rollout of WIC increased birth weight between 2.3 and 2.7 grams with no significant impacts on the incidence of low birth weight. Regarding Medicaid, during the 1980's dramatic expansions to pregnant women, Currie and Gruber (1996) find that a 30 percentage point increase in eligibility was associated to a reduction of 1.9% in low birth weight.

5.2 Translating ITT Effects into TOT Estimates

To translate the ITT estimates into average TOT effects, we need information on the fraction of births to women who used CHC services during pregnancy in the 1970s and 1980s. To approximate that information, we gather a range of statistics based on historical reports and survey data. Witwer (1990) states that CHCs and migrant health centers served "an estimated 213,000 pregnant women per year." From the Vital Statistics data, we calculate that there were 3.3 million births per year, on average, between 1968 and 1988. Combining

that produce the figure.

these two statistics, we estimate that approximately 6.4% of pregnant women were served by CHCs per year. If we use the total births in counties that had a CHC by 1988 (1.98 million), we get a CHC participation rate of 10.7%. Using these two alternative potential participation rates to scale our ITT estimates gives an estimated TOT effect for birth weight between 25 and 42 grams, and for low birth weight between 0.008 and 0.014 percentage points (9% and 16% of the mean).

Alternatively, the 1982 and 1988 National Family Growth Survey asked specifically about prenatal care use at CHCs. Approximately 5.2% of pregnant respondents reported receiving care at a CHC. However, survey respondents tend to under-report their use of public programs, and Bound et al. (2001) document that for clinic visits, survey responses agree with provider records as little as 39% of the time. Adjusting for under-reporting, the CHC participation rate could be as high as 13%. Using both the unadjusted and adjusted CHC participation rates, we get an estimated TOT effect on birth weight between 21 and 52 grams. For low birth weight, these participation rates imply a TOT effect between 8% and 20%.

It is reassuring that the two alternative approaches yield similar ranges of average TOT effects. Furthermore, these effect sizes are comparable to other TOT estimates of policy interventions on infant health, including Food Stamps and WIC (Almond et al., 2011a; Hoynes et al., 2011).[15] This similarity is interesting since the policies we use as a comparison promote different types of health investments and some are more targeted than CHCs. For example, WIC specifically promotes nutrition investments for low-income pregnant women and infants, whereas CHCs provide primary health care access in under-served areas for population of all ages. While there is a large body of causal evidence that shows the significant impacts of targeted programs on infant health, we show that less-targeted policies can have similar effects on health outcomes at birth.

[15]We acknowledge that our estimated participation rates may be measured with error, leading to either higher or lower TOT estimates. Our approach represents our best guess given the information available.

5.3 Heterogeneity

5.3.1 Mother's Education

Based on the population served by CHCs, we expect its impacts to be concentrated among low socioeconomic status families. Using information available on maternal education in the Vital Statistics data, Figure 3 presents the results on infant health separately for mothers with less than high school, high school, and greater than high school education.[16] As expected, the results are stronger for children born to mothers with less than high school education, particularly for low birth weight. However, we cannot reject that the effect sizes are statistically different across the subsamples.

Our findings in Appendix Table A.3 suggest that among women with less than high school education, following CHCs' introduction, there is a sharp increase in birth weight and a gradual reduction in the fraction of low-birth-weight births. The average birth weight increases by 7 grams, while the incidence of low birth weight decreases by 0.002 percentage points. While the overall effect size on birth weight is small, the reduction in the incidence of low birth weight is a 1.9% decline relative to the mean. The magnitude of the overall effect for the low-educated sample is similar in size to the effects of Food Stamp access in high-poverty counties (Almond et al., 2011a) and WIC access to low-educated mothers (Hoynes et al., 2011).

5.3.2 Other Maternal Characteristics

Next, in Appendix Figure A.2 we explore treatment effect heterogeneity by maternal race (Panels (a) and (b)) and age groups (Panels (c) and (d)) for our main outcomes. On average, Black mothers are more likely to have a low-birth-weight delivery and more preterm births than White mothers (Aizer and Currie, 2014). These disparities persist through adulthood

[16] Following Kearney and Levine (2007), we restrict our sample to 36 states that include complete information about maternal education. The states with missing information on maternal education are AL, AR, CA, CT, DE, DC, FL, GA, ID, MD, NM, OR, PA, TX, and WA.

and are associated with later-life health and human capital accumulation (Currie, 2011). We hypothesize that CHCs may play a role in closing the health disparities between Black and White infants. Panels (a) and (b) present our infant health results separately for all mothers, White and Black mothers using our preferred specification, which includes time-varying state and county controls as well as 1960 significant determinants interacted with flexible trends. These results show that the effect of CHCs on birth weight and low birth weight is greater for Black infants than White infants in the short run (up to three years after CHCs' introduction). However, the effects disappear quickly after three years for Black infants.

Similarly, teenage mothers are more likely to have a low socioeconomic status, experience a low-birth-weight delivery, and have complications during birth (Geronimus and Korenman, 1993). It may be that a CHC's arrival to a county provides easy access to health care during early pregnancy for teenage mothers and improves infant health outcomes for this group. Panels (c) and (d) of Appendix Figure A.2 present the results on infant health outcomes separately for all mothers, teenage mothers, mothers who are ages 20–29 and 30–39, using our preferred specification. We show that infants of teenage mothers benefit from a CHC's introduction slightly more than other infants. However, these effects among the subgroups are not statistically different from one another.

6 Other Outcomes and Potential Mechanisms

6.1 CHCs and Other Birth Outcomes, Prenatal Care, and Maternal Behaviors

We also examine other birth outcomes in Figure 4: an indicator for very low birth weight ($<$ 1,500 grams), gestation in weeks, small for gestational age (below the tenth percentile of birth weight for gestational age), and an indicator for a premature birth ($<$ 37 weeks

gestation).[17] We present estimates from our preferred specification, which includes time-varying state and county controls as well as 1960 significant determinants interacted with flexible trends. Figure 4, Panel (a) shows that exposure to a CHC reduced the probability of very low birth weight by 1.4% of the mean, a small effect that we detect in the static model, which is noisier in the dynamic specification.

In addition, birth weight is partly determined by gestational length (Kramer, 1987a,b). While we do not find significant effects on average gestational age, we do find that CHCs reduced the probability of an infant being small for their gestational age by 1.3% (Panel (c)). The effects are concentrated between 2 and 7 years after a CHC opening. Additionally, we see evidence of a decrease in the likelihood of preterm delivery by around 1.2% between years 0 to 5 post CHC introduction (Panel (d)). Due to infants' increased vulnerability, preterm birth is one of the leading causes of infant mortality. While we do not find evidence of CHCs' effects on infant mortality (see Appendix Figure A.3), this finding is still relevant since preterm births are costly: in 2005, each preterm birth led to increased costs of over $50,000 (US Institute of Medicine, 2007).

Regarding the mechanisms underlying CHCs' effects on birth outcomes, prenatal care is one of the main services provided by these centers (Goldman and Grossman, 1988). Access to and use of prenatal care have been linked to length of gestation and birth weight (Rosenzweig and Schultz, 1983; Abrevaya and Dahl, 2008; Corman et al., 2019). In terms of the probability of any prenatal care, the average in our sample is very high (98%), and we find a small positive effect of CHCs (0.26%).

In this context, a more relevant dimension of prenatal care is its timing. Figure 4, Panel (f) shows that the introduction of CHCs led to a 1.3% increase in the probability of receiving prenatal care during the first trimester. The effect is immediate and only grows stronger throughout the post-period. During the first prenatal visit, the provider gathers information

[17] Appendix Table A.4 presents the point estimates and standard errors on the underlying regression results that produce the figure.

about the patient's health history, asks about alcohol and tobacco use, screens for conditions that could lead to complications, and counsels her on nutrition and health behaviors. This early screening can help prevent serious complications later in the pregnancy (National Institutes of Health, 2017). Additionally, receiving this information earlier allows women to engage in positive health behaviors for a larger fraction of their pregnancies, making early initiation of prenatal care especially important for later outcomes.

At that time, birth certificates did not report information on maternal health behaviors during pregnancy. Therefore, we turn to the NLSY79 to understand how access to a CHC affected women's behaviors during pregnancy. For each pregnancy, women were asked if they used alcohol or tobacco during the pregnancy. A pregnancy is considered treated if the child was born after a CHC opened in the county of birth. The effects were estimated using our preferred specification for pregnancies that occurred by 1988. Given the much smaller sample of the NLSY79, we only estimate static effects.

In Table 3 columns (1) and (3), we provide suggestive evidence that CHCs' introduction led to mothers reduce their alcohol use during pregnancy. This table also shows that access to a CHC before a child's birth significantly reduces the probability of the mother reporting smoking during her pregnancy by 11% of the mean (column 2). The decline is larger if we consider a sample of low-educated mothers (less than high school education), who are more likely to use the CHCs (column 4). This reduction in maternal smoking during pregnancy is consistent with the birth weight results. Smoking is the primary risk factor for intrauterine growth restrictions, which leads to smaller infants. Nationally, tobacco use during pregnancy accounts for 20% to 30% of low-birth-weight births (Ricketts et al., 2005). Maternal smoking is negatively related to access to prenatal care: women with reduced access to prenatal care, especially early in pregnancy, are more likely to report smoking during pregnancy (Evans and Lien, 2005). Reducing maternal smoking may be a primary mechanism through which improved access to care from a CHC could lead to improved infant outcomes.For example,

oxygen delivery to the fetus improves immediately upon stopping smoking, leading to improved fetal growth rates. Quitting earlier relative to later improves the in-utero environment for a longer portion of pregnancy (Centers for Disease Control and Prevention, 2017).

Taken together, we find suggestive evidence that the introduction of CHCs improved additional markers of health at birth. We show that smoking cessation and early access to prenatal care are plausible mechanisms.

6.2 CHCs and Women's Access to Health Care

As discussed in Section 2.1, national-level surveys show that clinics, including CHCs, were important sources of prenatal care and their use grew as the CHC program expanded. However, we are unable to observe if a woman lives near a CHC in the NSFG surveys. To investigate if living in an area with a CHC changes health care use, we use data from the 1963 and 1970 SHSUE to investigate if being near a CHC increased women's access to health care. The SHSUE is a nationally representative survey with data from 1963, before the CHC program began. The same areas were sampled again for the 1970 wave.

First, we identify the counties that make up the primary sampling units (PSUs) of the SHSUE. Some PSUs contain more than one county; in those cases, we consider women treated if any county in their PSU had a CHC by 1970. While we cannot specifically analyze pregnant women in these data, we limit the sample to women ages 15–39, which matches the ages of mothers in our Vital Statistics sample. We estimate a difference-in-differences linear probability model:

$$Y_{it} = \alpha + \beta_1 CHC_i + \beta_2 1(1970 = 1)_t + \beta_3 CHC_i * 1(1970 = 1)_t + \mathbf{X}'_{it}\theta + \varepsilon_{it} \qquad (2)$$

where i is an individual and t is the survey year. Y_{it} is whether the woman has a regular source of care or, for those with a regular source of care, if the source of care is a clinic, a

category that explicitly includes CHCs in 1970. The equation includes an indicator for if the woman lives in an area that receives a CHC by 1970, an indicator for the 1970 wave, and an interaction term, whose coefficient is reported in Table 4. $\mathbf{X}_{it}$ is a set of controls for age, marital status, race, and the same urban categories as used in the main regressions. Standard errors are clustered at the PSU level. Given the educational distribution of CHC users and the stronger effects for mothers without a high school degree discussed in Sections 2.1 and 5.3.1, we perform this analysis for the full sample and separately for women without a high school degree.

Table 4 shows that the opening of a CHC improved health care access for women without high school degrees. Those living in an area with a CHC by 1970 were significantly more likely to report having a regular source of care. This is a 13% increase relative to the mean. Much of this increase seems to be driven by increased use of clinics: the coefficient in column 4 represents almost a 100% increase in the portion of women reporting a clinic as their regular source of care. Individuals with a regular source of care are more likely to receive preventative care and less likely to delay visits for specific conditions. Having a regular source of care is more strongly predictive for these outcomes than having health insurance (Sox et al., 1998).

This pattern is consistent with our results on early prenatal care. Women who already have a provider will have a much easier time starting prenatal care early in their pregnancies. In addition to prenatal care, this result points to another potential mechanism for the observed improvements in infant health: pre-conception health. Regular access to health care may lead to general improvements in the health of childbearing-age women. Women who are healthier before pregnancy are more likely to carry to term and have healthier infants (Stephenson et al., 2018).

7 Composition, Sensitivity, and Robustness

7.1 Composition

Beyond the direct effect on infant health, access to health care through a CHC may have indirect effects on the health of observed infants if women respond to CHC access by changing their fertility patterns. This is plausible given that CHCs provide reproductive health services including family planning (Wolfe, 2013). If more disadvantaged women responded to these services by having fewer children, then our results may be biased downward. However, in Table 5 we see no evidence of changes to fertility: when measured by the birth rate or the natural log of the number of births, there is no measurable change in overall fertility.

The table also shows no effect on marital status, parity, maternal age, or the percentage of teen births. In CHC-exposed areas, women giving birth may be slightly more likely to be Black. This effect is not significant in the static analysis but is statistically different from zero for some of the post-period. Given that Black women are more likely to have adverse birth outcomes, any potential bias from this shift in maternal composition is against our finding of infant health improvements. Additionally, given the null effect on infant mortality rates (see Appendix Figure A.3), we can infer that the effects on birth outcomes are not driven by changes to survival.

7.2 Sensitivity Using Alternative Specifications

Our results are robust to alternative specifications, which are presented in Figure 5.[18] The results for birth weight and low birth weight in this figure follow the same pattern. The red square reproduces our preferred specification, which includes demographic and time-varying county characteristics as well as flexible trends for 1960 county determinants. Next, we estimate this specification using a TWFE model (the blue triangle). The effect is no longer

[18] Our results in table form are available in Appendix Tables A.5 and A.6.

statistically significant and there is some evidence of a pretrend. However, as discussed above, we present some evidence of potential treatment effect heterogeneity, and we use the Sun and Abraham procedure for the main results to minimize the bias from using earlier-treated units as comparison for later-treated units (Sun and Abraham, 2020). We consider this evidence in favor of our estimation method choice.

Next, we add state-by-year fixed effects (green circle), which reduce the magnitude of the static effect but have no qualitative effect on the estimates using the dynamic estimation. Finally, the estimate represented by the purple diamond redefines the timing of the intervention to be relative to the time of conception. Conception timing is calculated using the gestational age at birth; in our study period, this information is missing for 21.3% of the sample. When gestation information is not available, conception is calculated as nine months before the date of birth. The static effect using this timing is very similar to our preferred specification. However, the dynamic effect is attenuated, potentially due to the increase in measurement error from the conception calculation.

7.3 Additional Robustness

Beyond the sensitivity checks, we perform a randomization test to ensure the effects we report could not have been the result of random chance. We compute the static policy effect from 1,000 permutations of the treatment, holding the distribution of treatment dates constant. The distribution of coefficients from these permutations are reported in Appendix Figure A.4. For both birth weight and the incidence of low birth weight, the blue histogram represents the distribution of the randomized coefficients and the red line represents the estimated effect. For both outcomes, the estimated effect of the CHC is well into the tail of the distribution of randomized coefficients.

In a similar spirit, we want to be sure the dynamic results we report are not the result of unrelated trends in areas that receive a CHC. To test this, we re-estimate the CHC effects

with a change in the treatment timing. Instead of using the actual year a CHC opened, we assign the treatment timing to be four years earlier than the truth. If our results are driven by spurious trends in areas that received a CHC, these trends will now be picked up by the regression as the treatment effect. Because this requires us to have additional years of pre-period information, we can only include treated counties that received a CHC in 1977 or after in this robustness check. Appendix Figure A.5 presents these results and shows that there is no observable effect of an artificially early CHC arrival on average birth weight or the incidence of low-birth-weight births. In all three versions of the specification, the "treatment effect" is flat, statistically insignificant, smaller in magnitude, and opposite signed than the true treatment effects we find.

7.4 Sensitivity Using Alternative CHC Timing

In this paper, we extend the CHCs' rollout collected by Bailey and Goodman-Bacon, 2015 with information from the Federal Assistance Awards Data System (FAADS) for the years between 1980 and 1988. In this section, we examine whether our results are robust to only using the previous rollout data. To check this, we re-estimate our analysis for counties that adopted CHCs by 1980 (similar to Bailey and Goodman-Bacon, 2015) using the Vital Statistics data up to 1980. Appendix Table A.7 presents these results, which show that CHCs were effective in improving birth outcomes during this time period. In fact, we find larger effects of CHCs' introduction through 1980 than in our preferred analysis in Table 2. Overall, these estimates provide evidence that our results are robust to using only the previous rollout timing collected by Bailey and Goodman-Bacon (2015) and a shorter horizon of the natality data.

8 Conclusion

Reducing health disparities between children born to richer versus poorer families is an important policy goal given the long-run consequences to poor health (Currie, 2011). The introduction of the CHC program provides an opportunity to understand how direct provision of health care in medically underserved areas affected infant health. By leveraging the rollout of CHCs in a flexible difference-in-differences empirical framework, we show that infants born to mothers with access to a CHC in their county were significantly healthier at birth as measured by birth weight. On average, the introduction of CHCs increased birth weight by 3 grams and reduced the incidence of low birth weight by 1% of the mean. These ITT effects potentially translate into TOT effects between 25 and 42 grams for birth weight and between 9% and 16% for low birth weight. The magnitudes of these impacts are comparable to those from the other WOP programs including Food Stamps and WIC.

To examine potential mechanisms, in addition to the limited data available from the Vital Statistics, we use auxiliary data from the NLSY79 and the SHSUE. We identify early prenatal care use and smoking cessation as potential mechanisms behind the CHCs improvements on health at birth. Also, we show evidence of improvements in pre-conception health care for low-educated childbearing-age women as another mechanism. Our paper shows that interventions that do not specifically target mothers and infants, but instead focus on increasing access to health care in the general population, can improve health outcomes for the next generation.

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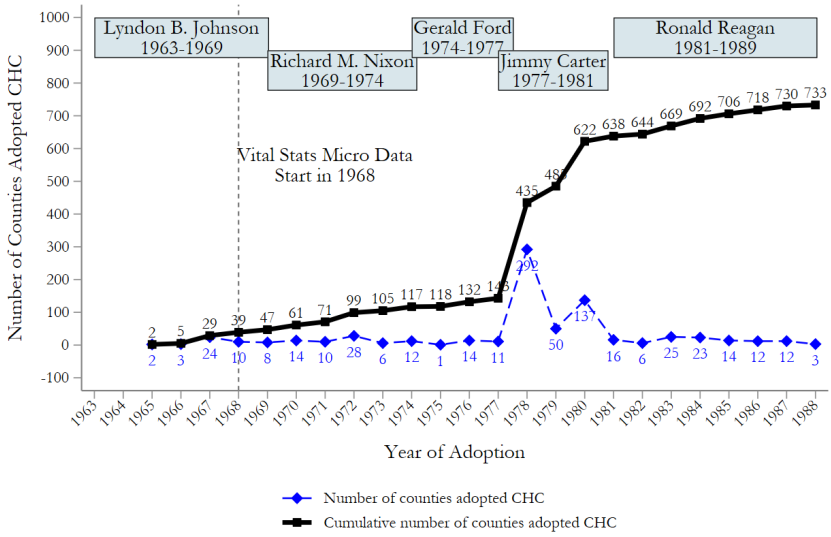
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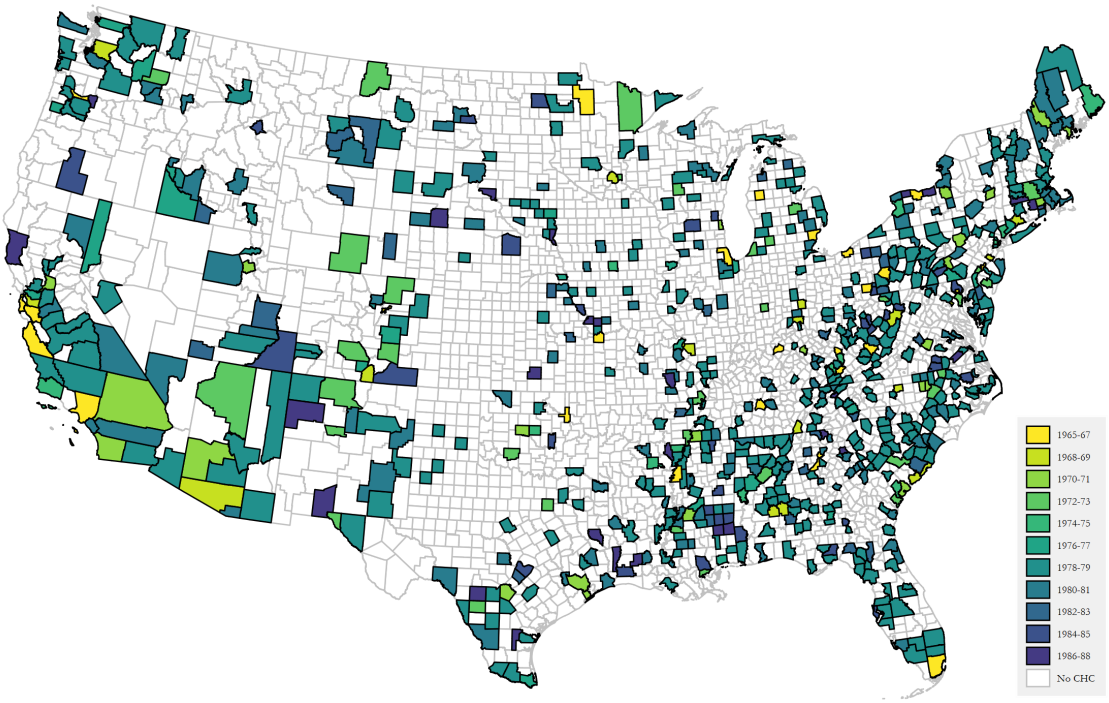
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Figures

Figure 1: Timing of CHC adoption and Geographic Variation



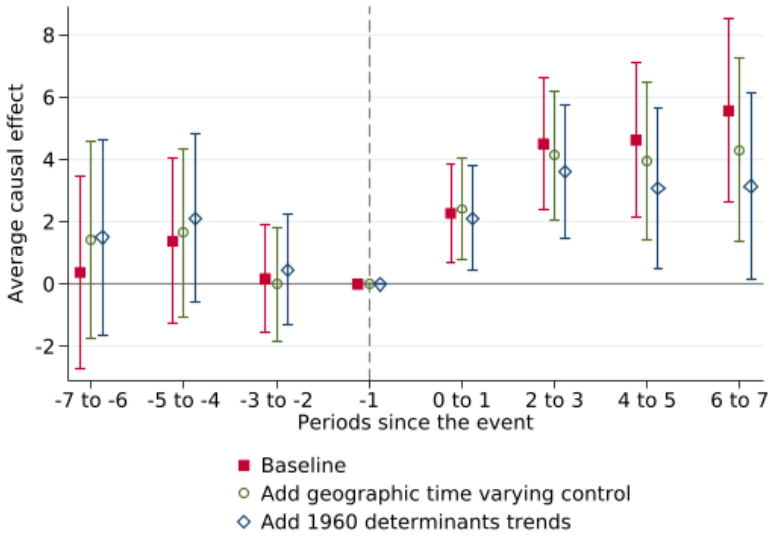
(a) Number of Counties Adopted CHC over Time



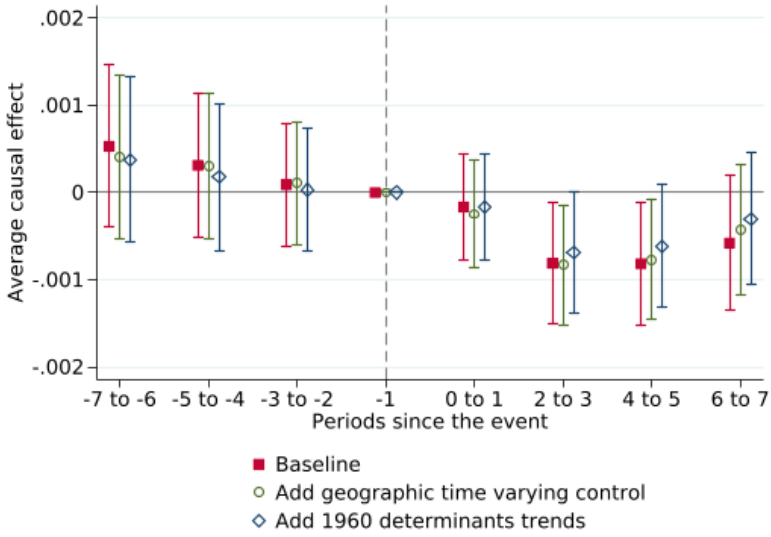
(b) Geographic Variation of CHC Timing

Notes: We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988.

Figure 2: The Effect of CHC on Infant Health - Main Outcomes
Full Sample



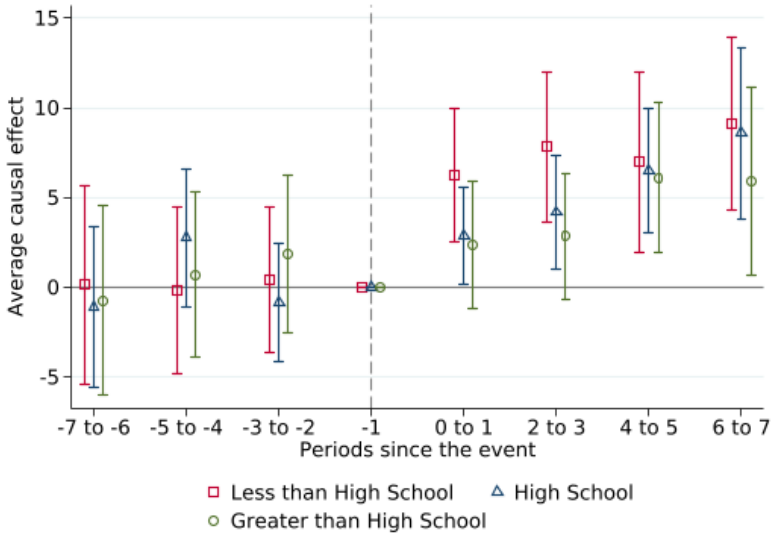
(a) Birth Weight (grams)



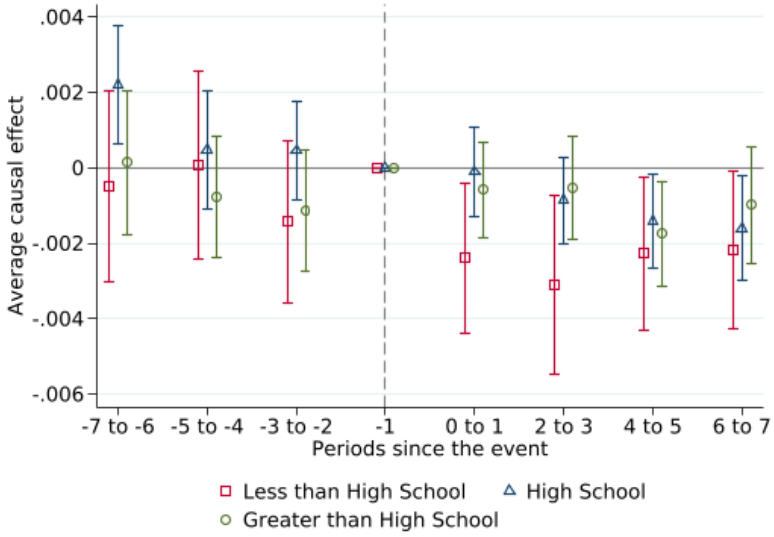
(b) Low Birth Weight

Notes: This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4.

Figure 3: The Effect of CHC on Infant Health - By Mothers' Education



(a) Birth Weight (grams)

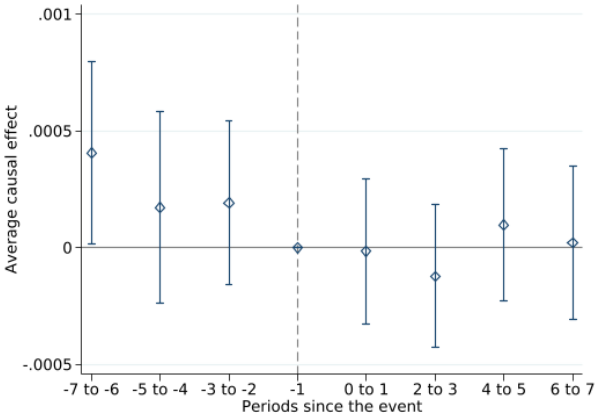


(b) Low Birth Weight

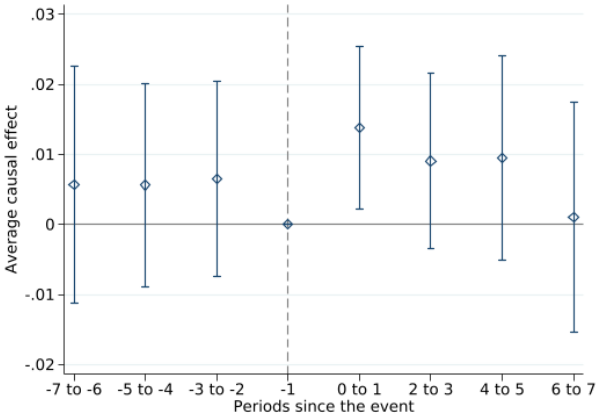
Notes: This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. For this analysis, we restrict our sample to 36 states that include complete information about maternal education following Kearney and Levine (2007). All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4.

Figure 4: The Effect of CHC on Other Birth Outcomes and Mechanisms

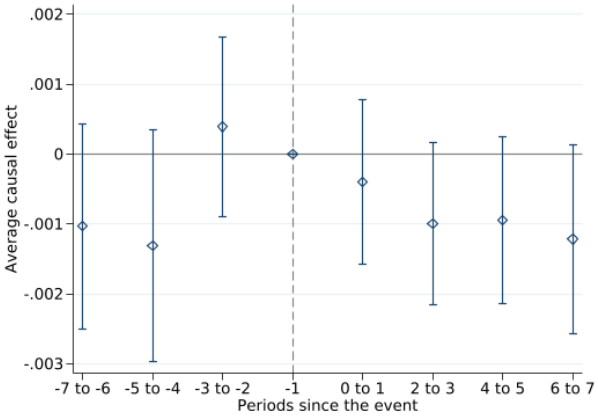
(a) VLBW



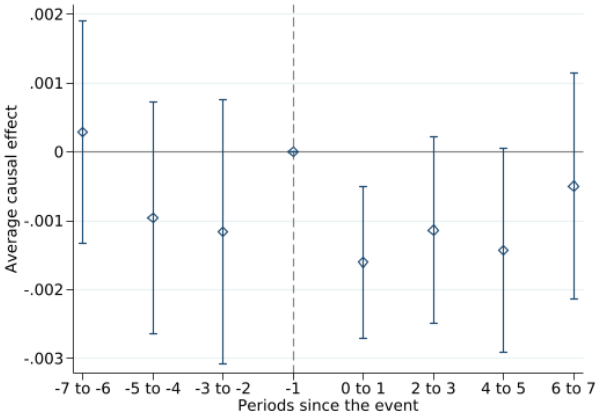
(b) Gestational age



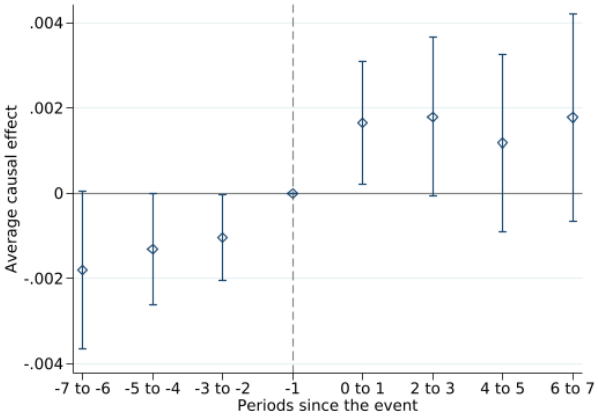
(c) Small for GA



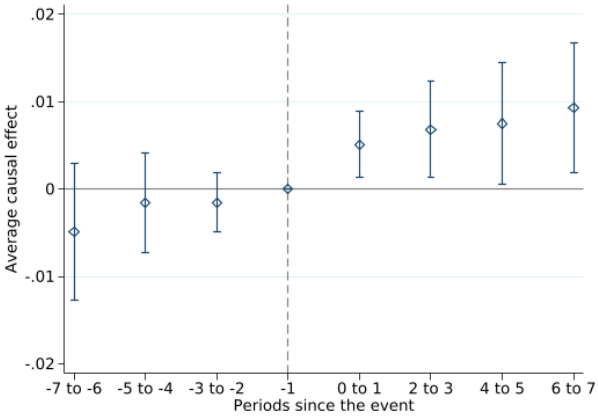
(d) Preterm



(e) Any PnC

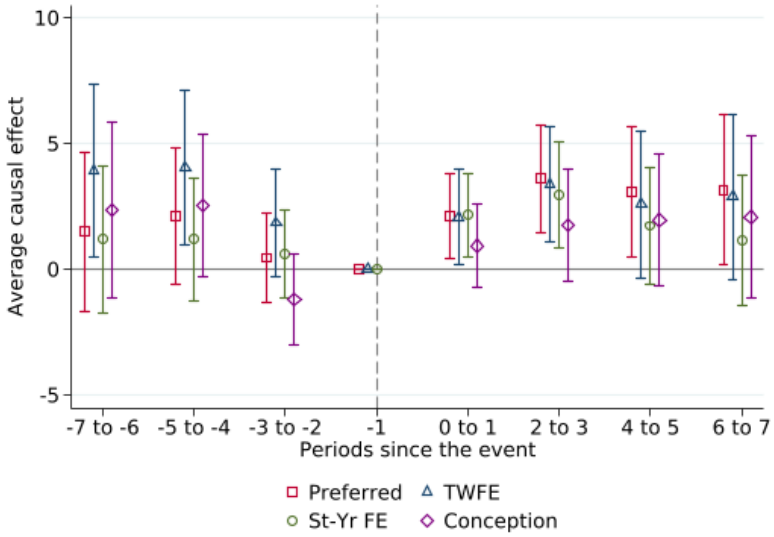


(f) First trim PnC

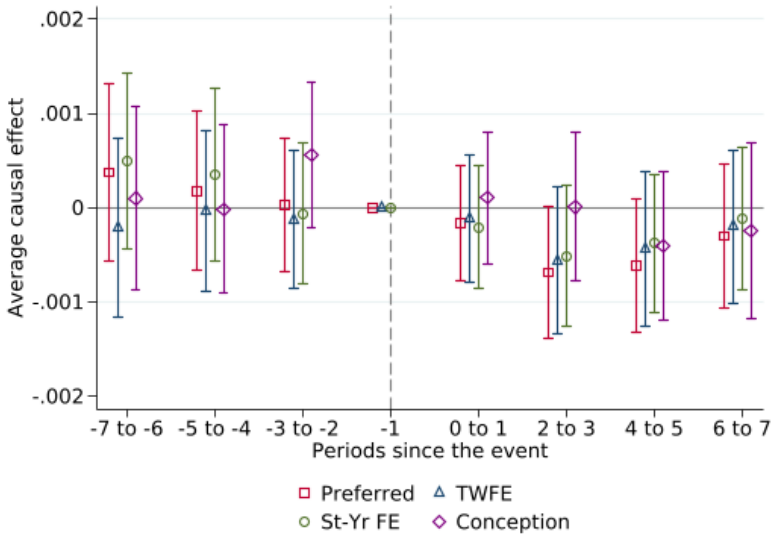


Notes: This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4.

Figure 5: The Effect of CHC on Infant Health, Alternative Specifications



(a) Birth Weight (grams)



(b) Low Birth Weight

Notes: This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2020), except for the estimates labeled "TWFE" which use twoway fixed effects. The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimations, see Section 7.

Tables

Table 1: Determinants of CHCs Rollout between 1965 and 1988

	1965-1988			1969-1988		
	(1)	(2)	(3)	(4)	(5)	(6)
Median family income- 1959	-0.0013	-0.0075***	-0.0005	-0.0003	-0.0024	-0.0002
	(0.0010)	(0.0027)	(0.0026)	(0.0008)	(0.0022)	(0.0021)
% w/ family income < $3000 - 1959	-0.1281**	-0.5948***	-0.1439	-0.0315	-0.1666	-0.0703
	(0.0640)	(0.1919)	(0.1718)	(0.0543)	(0.1503)	(0.1472)
% w/ family income $10000+ - 1959	-0.0775	0.4516*	-0.1627	-0.0509	0.2217	-0.0347
	(0.0992)	(0.2516)	(0.2521)	(0.0863)	(0.1984)	(0.2050)
% population aged 0-4 -1960	-0.0087	0.4596	0.6254	0.0569	0.1226	0.3528
	(0.1377)	(0.3777)	(0.4394)	(0.1191)	(0.2684)	(0.3109)
% population aged 65+ - 1960	0.0056	0.2335	0.2649	0.0788	0.2677*	0.2407
	(0.0823)	(0.2056)	(0.2649)	(0.0646)	(0.1589)	(0.1720)
% persons 25+ w/ <4 yrs sch. - 1960	0.0170	0.1227	0.0741	-0.0127	0.0309	0.0980
	(0.0353)	(0.0884)	(0.0989)	(0.0319)	(0.0797)	(0.0821)
% persons 25+ w/ 12+ yrs sch. - 1960	0.0276	0.0819	0.1288*	-0.0037	0.0319	0.0639
	(0.0296)	(0.0591)	(0.0730)	(0.0263)	(0.0559)	(0.0680)
% nonwhite - 1960	-0.0030	-0.0280	-0.0868***	0.0008	0.0023	-0.0710***
	(0.0109)	(0.0265)	(0.0312)	(0.0095)	(0.0247)	(0.0268)
% urban - 1960	-0.0203**	-0.1081***	-0.0853***	-0.0122	-0.0601***	-0.0487***
	(0.0088)	(0.0210)	(0.0206)	(0.0077)	(0.0164)	(0.0168)
% rural farm - 1960	0.0089	-0.0084	0.0291	-0.0026	-0.0124	0.0381
	(0.0133)	(0.0326)	(0.0379)	(0.0109)	(0.0238)	(0.0299)
Total Active MDs (per pop) - 1960	-1.6214***	-2.6439***	-2.3433***	-0.8431**	-1.6795***	-1.2697**
	(0.4742)	(0.5731)	(0.5482)	(0.3856)	(0.6449)	(0.5079)
AMR - All Ages - 1960	-0.0019*	-0.0033	-0.0027	-0.0011	-0.0017	-0.0016
	(0.0011)	(0.0038)	(0.0033)	(0.0010)	(0.0028)	(0.0028)
R-Squared	0.156	0.449	0.591	0.060	0.167	0.320
Obs	728	728	728	690	690	690
Weighted	N	Y	Y	N	Y	Y
State FE			Y			Y

Notes: The data are at the county level and the dependent variable is the year (normalized to one in 1965) that a CHC was established in the county. The covariates come from the City and County Data Book for 1960. Columns 2-3 and 5-6 are weighted by 1960 county population. Heteroskedasticity-robust standard errors in parenthesis. * p<0.10, ** p<0.05, *** p<0.01.

Table 2: The Effect of CHCs on Infant Health, Main Outcomes, Full Sample

	Birth Weight			Low Birth Weight		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Static Policy Effect						
CHC	5.7784***	4.7279***	2.7322**	-0.0014***	-0.0013***	-0.0009***
	(1.2796)	(1.3297)	(1.3622)	(0.0003)	(0.0003)	(0.0003)
B: Dynamic Policy Effect						
Years -7 to -6	0.3836	1.4201	1.5082	0.0005	0.0004	0.0004
	(1.5657)	(1.6111)	(1.6087)	(0.0005)	(0.0005)	(0.0005)
Years -5 to -4	1.3877	1.6603	2.1240	0.0003	0.0003	0.0002
	(1.3526)	(1.3856)	(1.3811)	(0.0004)	(0.0004)	(0.0004)
Years -3 to -2	0.1847	0.0024	0.4729	0.0001	0.0001	0.0000
	(0.8879)	(0.9243)	(0.9137)	(0.0004)	(0.0004)	(0.0004)
Years 0 to 1	2.2830***	2.4288***	2.1258**	-0.0002	-0.0002	-0.0002
	(0.8170)	(0.8371)	(0.8520)	(0.0003)	(0.0003)	(0.0003)
Years 2 to 3	4.5189***	4.1528***	3.6142***	-0.0008**	-0.0008**	-0.0007*
	(1.0821)	(1.0546)	(1.0915)	(0.0004)	(0.0003)	(0.0004)
Years 4 to 5	4.6288***	3.9445***	3.0741**	-0.0008**	-0.0008**	-0.0006*
	(1.2712)	(1.2948)	(1.3160)	(0.0004)	(0.0003)	(0.0004)
Years 6 to 7	5.5788***	4.3221***	3.1588**	-0.0006	-0.0004	-0.0003
	(1.5010)	(1.5090)	(1.5201)	(0.0004)	(0.0004)	(0.0004)
N	4242779	4236199	4235220	4242779	4236199	4235220
Mean Y	3298.3084	3298.4409	3298.4195	0.0859	0.0859	0.0859
Baseline	Y	Y	Y	Y	Y	Y
Geographic Time Varying Controls	N	Y	Y	N	Y	Y
1960 Determinants Trends	N	N	Y	N	N	Y

Notes: This table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. All regressions include county, year fixed effects, and individual demographic controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: The Effect of CHCs on Substance Use in Pregnancy

	Full Sample		Less than high school	
	Alcohol use	Tobacco use	Alcohol use	Tobacco use
	(1)	(2)	(3)	(4)
CHC	-0.0022	-0.0412**	-0.0552	-0.0715**
	(0.0163)	(0.0163)	(0.0356)	(0.0309)
N	6102	6097	1018	1017
Mean	0.3578	0.3612	0.3103	0.5227

Notes: This table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). The outcome data is from the National Longitudinal Survey of Youth 1979 Child files. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. These regressions correspond to our preferred specification, which include county, year fixed effects, individual demographic controls, geographic time varying controls and 1960 determinants trends. Standard errors are clustered at the county level. For more details about the estimation, see Section 4. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: The Effect of CHCs on Women's Health Care Access

	Full Sample		Less than high school	
	Regular Source of Care	Source is Clinic	Regular Source of Care	Source is Clinic
	(1)	(2)	(3)	(4)
CHCx1970	-0.0600	-0.0016	0.0872*	0.122*
	(0.120)	(0.970)	(0.0981)	(0.0511)
N	1949	1745	1121	967
Mean	0.884	0.134	0.812	0.115

Notes: Outcome data are from the 1963 and 1970 Surveys of Health Services Utilization and Expenditures. We restrict the sample to White and Black women ages 15-39. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. Regressions are weighted using SHSUE survey weights. Standard errors are clustered at the PSU level. For more details about the estimation, see Section 6.2. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: The Effect of CHCs on Maternal Composition

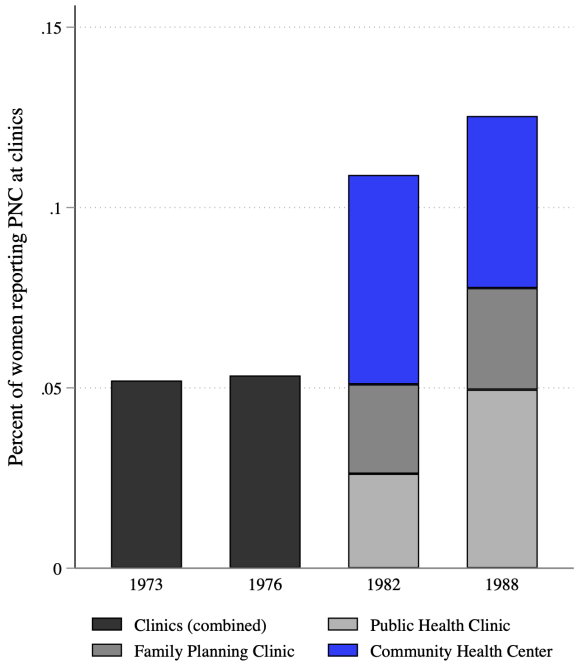
	Birth Rate	Ln(Births)	% Married	Parity	Mother's Age	% Teen Mom	% >= HS graduate	% Black
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Static Policy Effect								
CHC	-0.0005	0.0089	-0.0055	0.0024	-0.0163	0.0013	-0.0080	0.0046
	(0.0007)	(0.0175)	(0.0051)	(0.0055)	(0.0302)	(0.0016)	(0.0107)	(0.0034)
B: Dynamic Policy Effect								
Years -7 to -6	0.0015**	0.0114	0.0060	-0.0068	-0.0284	0.0006	0.0286	-0.0039
	(0.0008)	(0.0187)	(0.0088)	(0.0060)	(0.0247)	(0.0017)	(0.0216)	(0.0039)
Years -5 to -4	0.0006	-0.0157	0.0090	-0.0002	-0.0068	-0.0004	-0.0072	-0.0040
	(0.0006)	(0.0263)	(0.0073)	(0.0042)	(0.0187)	(0.0016)	(0.0137)	(0.0031)
Years -3 to -2	-0.0001	-0.0040	0.0028	-0.0011	-0.0063	-0.0004	-0.0117	-0.0007
	(0.0003)	(0.0060)	(0.0059)	(0.0027)	(0.0101)	(0.0007)	(0.0151)	(0.0009)
Years 0 to 1	-0.0001	0.0025	0.0044	0.0030	0.0007	0.0001	-0.0079	0.0020**
	(0.0003)	(0.0078)	(0.0064)	(0.0024)	(0.0110)	(0.0007)	(0.0108)	(0.0009)
Years 2 to 3	0.0001	0.0202	-0.0039	0.0016	-0.0210	0.0017	-0.0126	0.0058***
	(0.0006)	(0.0219)	(0.0068)	(0.0040)	(0.0197)	(0.0013)	(0.0149)	(0.0018)
Years 4 to 5	-0.0004	0.0002	-0.0062	0.0009	-0.0336	0.0020	0.0077	0.0067***
	(0.0008)	(0.0235)	(0.0072)	(0.0051)	(0.0255)	(0.0015)	(0.0154)	(0.0024)
Years 6 to 7	0.0001	0.0094	-0.0059	-0.0053	-0.0095	0.0011	-0.0171	0.0009
	(0.0010)	(0.0257)	(0.0081)	(0.0064)	(0.0419)	(0.0025)	(0.0122)	(0.0052)
N	62049	62049	59737	62049	62049	62049	14408	62049
Mean Y	0.0798	7.4376	0.8714	2.0519	24.7058	0.1644	0.7434	0.1129

Notes: This table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the county cell level. Only states that collect information on education levels are included in column 7. These regressions correspond to our preferred specification, which include county, year fixed effects, geographic time varying controls and 1960 determinants trends. Standard errors are clustered at the county level. For more details about the estimation, see Section 4. * p<0.10, ** p<0.05, *** p<0.01.

Appendix

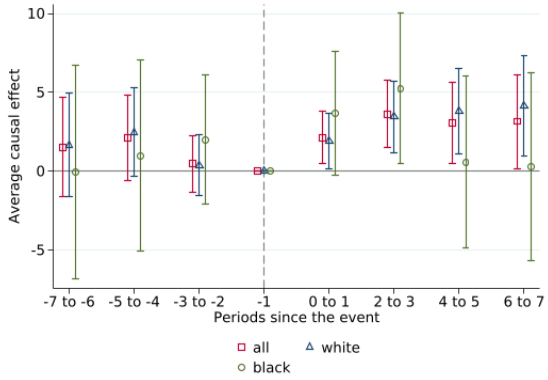
A Additional figures and tables

Figure A.1: Reported use of clinics for prenatal care

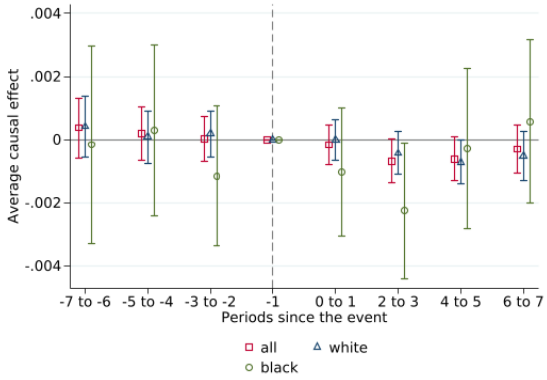


Notes: Authors calculations using cycles 1-4 of the National Survey of Family Growth. Percentages calculated using survey weights.

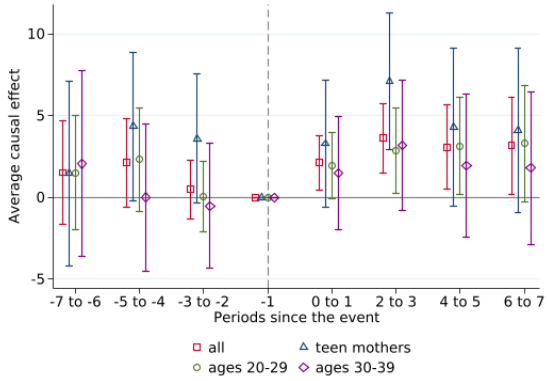
Figure A.2: Heterogeneity: The Effect of CHC on Infant Health - By Maternal Characteristics



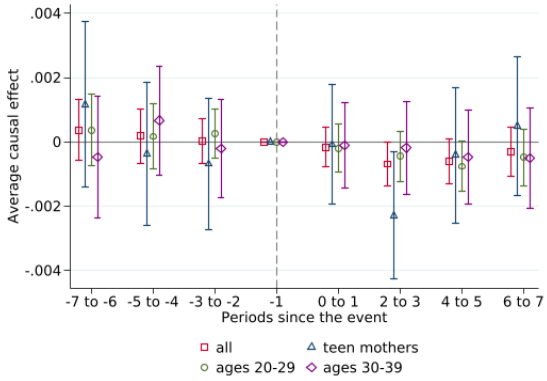
(a) Birth Weight (grams) - By Race



(b) Low Birth Weight - By Race



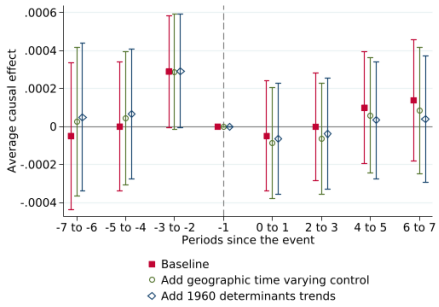
(c) Birth Weight (grams) - By Maternal Age



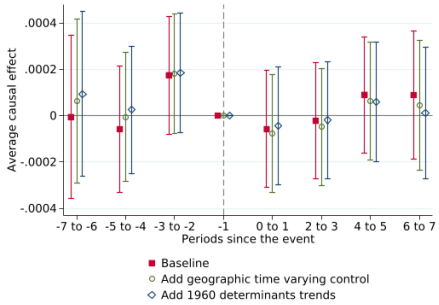
(d) Low Birth Weight - By Maternal Age

Notes: This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2020) with our preferred specification (Spec 3 of Table 2). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4.

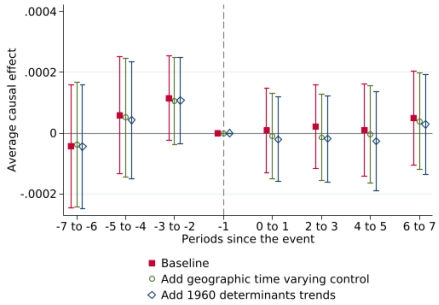
Figure A.3: The Effect of CHC on Infant Mortality Rates



(a) Infant Mortality Rate (combined)



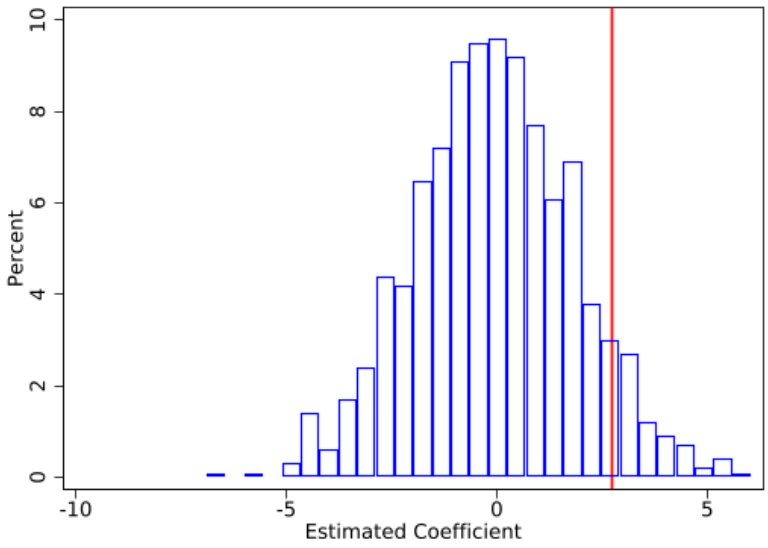
(b) Infant Mortality Rate (Neonatal)



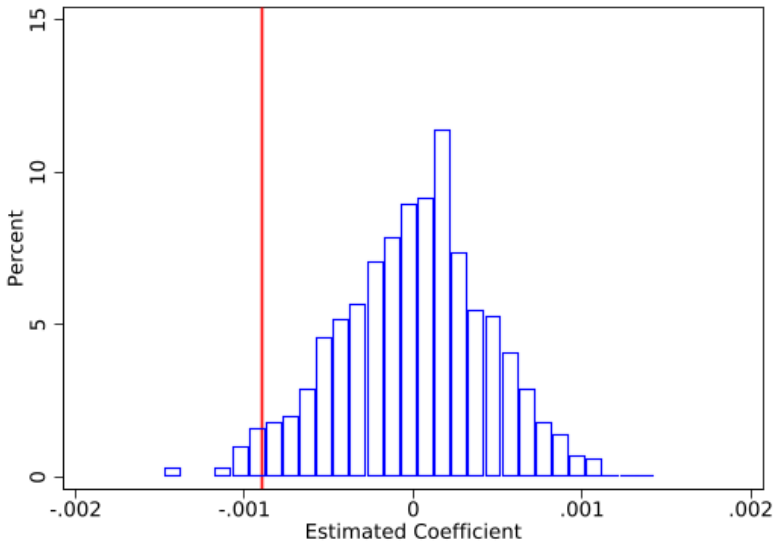
(c) Infant Mortality Rate (Post-neonatal)

Notes: Infant mortality rate is the number of deaths of infants less than one year old over the number of live births. Neonatal refers to deaths in the first 27 days after birth; post-neonatal refers to deaths between 28 days and one year after birth. This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Compressed Mortality File that covers the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4.

Figure A.4: The Effect of CHC on Infant Health, Randomized Treatment Timing



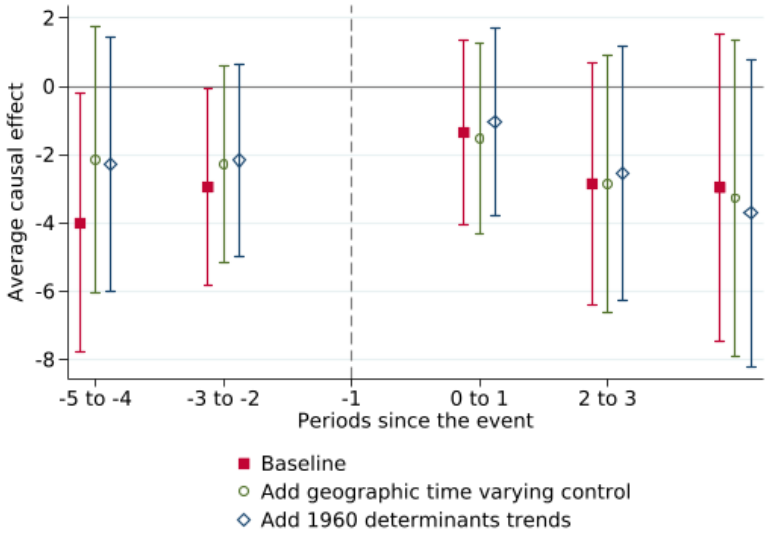
(a) Birth Weight (grams)



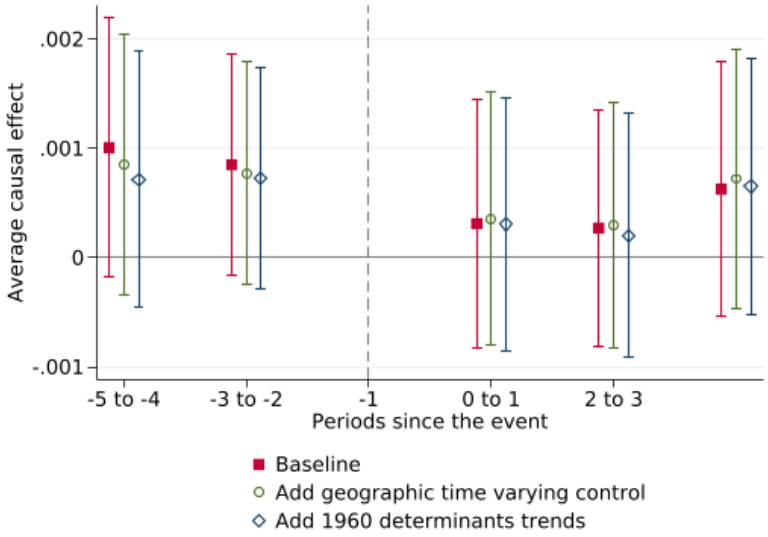
(b) Low Birth Weight

Notes: This figure plots the estimated coefficients from 1000 permutations of our preferred specification. The red line represents the true estimated effect. The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. For these figures, CHC timing has been randomized. All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 7.

Figure A.5: The Effect of CHC on Infant Health, Placebo Treatment Timing, For the Full Sample



(a) Birth Weight (grams)



(b) Low Birth Weight

Notes: This figure plots the estimated coefficients and 95% confidence intervals obtained from event study specification using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. For these figures, CHC timing has been artificially applied four years before actual treatment. All regressions include county, year fixed effects, individual demographic controls, and time varying county controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 7.

Table A.1: 1960 characteristics of counties with CHCs between 1965 and 1988, Full Sample

	CHC established in					Rest of counties
	(1) 1965-1968 (N=39)	(2) 1969-1970 (N=22)	(3) 1971-1974 (N=56)	(4) 1975-1980 (N=505)	(5) 1981-1988 (N=111)	(6) (N=2332)
Total population - 1960	1058587.4	411549.7	249451.7	76222.2	55731.7	29637.9
Median family income- 1959	5430.2	5577.7	4866.8	4112.0	4136.8	4134.8
% w/ family income < $3000 - 1959	24.84	20.92	29.66	37.02	36.85	35.92
% w/ family income $10000+ - 1959	15.04	14.04	12.15	7.982	8.021	7.626
% population aged 0-4 -1960	11.24	11.55	12.26	11.50	11.46	11.01
% population aged 65+ - 1960	9.458	8.345	8.412	9.647	9.861	10.99
% persons 25+ w/ <4 yrs sch. - 1960	10.64	8.823	13.22	13.54	13.21	10.51
% persons 25+ w/ 12+ yrs sch. - 1960	39.47	43.07	38.13	33.28	33.51	37.23
% nonwhite - 1960	18.40	12.39	18.28	15.99	14.35	9.001
% urban - 1960	76.02	75.68	57.82	36.12	37.38	28.75
% rural farm - 1960	4.718	2.782	10.29	18.93	18.37	24.63
Total Active MDs (per pop) - 1960	1.613	1.736	0.964	0.666	0.738	0.617
AMR - All Ages - 1960	1002.7	971.1	984.1	976.4	951.5	919.6

Notes: Source of county characteristics is the 1960 County and City Databooks (Haines and ICPSR 2005).

Table A.2: Diagnostic Test for Treatment Effect Heterogeneity: Regression of FE weights on year

	Correlation	Coef	S.E	T-stat
Year	-0.272	-1.572	0.200	-7.860
Mother's education LTHS	0.061	0.034	0.020	1.671
Hospitals per capita	0.168	0.003	0.000	6.779
Share of non-White women on AFDC	0.263	2.326	0.581	4.005

Notes: The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. This table reports results using a diagnostic test introduced by De Chaisemartin and D'Haultfoeuille (2020).

Table A.3: The Effect of CHCs on Infant Health, Main Outcomes

Mothers with Less than High School Degree

	Birth Weight			Low Birth Weight		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Static Policy Effect						
CHC	9.6162***	8.9934***	6.7582***	-0.0026***	-0.0025***	-0.0018**
	(1.9768)	(1.9241)	(1.9360)	(0.0009)	(0.0008)	(0.0009)
B: Dynamic Policy Effect						
Years -7 to -6	0.0710	-0.2057	-0.0073	-0.0002	-0.0001	-0.0005
	(2.8191)	(2.8101)	(2.8146)	(0.0013)	(0.0013)	(0.0013)
Years -5 to -4	0.0972	-0.2757	-0.2409	0.0001	0.0002	0.0001
	(2.3096)	(2.3150)	(2.3739)	(0.0012)	(0.0012)	(0.0013)
Years -3 to -2	0.2497	-0.1087	0.4116	-0.0014	-0.0013	-0.0014
	(2.0801)	(2.0489)	(2.0666)	(0.0011)	(0.0011)	(0.0011)
Years 0 to1	6.1171***	6.2051***	6.2380***	-0.0023**	-0.0024**	-0.0024**
	(1.9445)	(1.9192)	(1.8843)	(0.0010)	(0.0010)	(0.0010)
Years 2 to 3	8.7758***	7.9332***	7.8228***	-0.0035***	-0.0033***	-0.0031***
	(2.2073)	(2.2175)	(2.1386)	(0.0012)	(0.0012)	(0.0012)
Years 4 to 5	8.3657***	7.7164***	6.9869***	-0.0028**	-0.0026**	-0.0023**
	(2.4933)	(2.5202)	(2.5506)	(0.0011)	(0.0011)	(0.0010)
Years 6 to 7	11.0896***	9.9640***	9.1464***	-0.0027**	-0.0025**	-0.0022**
	(2.4885)	(2.3983)	(2.4432)	(0.0011)	(0.0011)	(0.0011)
N	981343	979326	979114	981343	979326	979114
Mean Y	3233	3233	3233	0.1027	0.1027	0.1027
Baseline	Y	Y	Y	Y	Y	Y
Geographic Time Varying Controls	N	Y	Y	N	Y	Y
1960 Determinants Trends	N	N	Y	N	N	Y

Notes: This table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. All regressions include county, year fixed effects, and individual demographic controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: The Effect of CHCs on Infant Health, Other Outcomes and Mechanisms

	VLBW	Gestational Age	Small for Gestation	Preterm	Any Prenatal Care	First Trimester
	(1)	(2)	(3)	(4)	(5)	(6)
A: Static Policy Effect						
CHC	-0.0002**	0.0022	-0.0014**	0.0005	0.0026*	0.0085**
	(0.0001)	(0.0067)	(0.0006)	(0.0009)	(0.0015)	(0.0038)
B: Dynamic Policy Effect						
Years -7 to -6	0.0004**	0.0057	-0.0010	0.0003	-0.0018*	-0.0049
	(0.0002)	(0.0086)	(0.0007)	(0.0008)	(0.0009)	(0.0040)
Years -5 to -4	0.0002	0.0056	-0.0013	-0.0010	-0.0013*	-0.0016
	(0.0002)	(0.0074)	(0.0008)	(0.0009)	(0.0007)	(0.0029)
Years -3 to -2	0.0002	0.0065	0.0004	-0.0012	-0.0010**	-0.0016
	(0.0002)	(0.0071)	(0.0007)	(0.0010)	(0.0005)	(0.0017)
Years 0 to 1	-0.0000	0.0138**	-0.0004	-0.0016***	0.0016**	0.0051***
	(0.0002)	(0.0059)	(0.0006)	(0.0006)	(0.0007)	(0.0019)
Years 2 to 3	-0.0001	0.0091	-0.0010*	-0.0011*	0.0018*	0.0068**
	(0.0002)	(0.0064)	(0.0006)	(0.0007)	(0.0010)	(0.0028)
Years 4 to 5	0.0001	0.0095	-0.0009	-0.0014*	0.0012	0.0075**
	(0.0002)	(0.0075)	(0.0006)	(0.0008)	(0.0011)	(0.0035)
Years 6 to 7	0.0000	0.0010	-0.0012*	-0.0005	0.0018	0.0093**
	(0.0002)	(0.0084)	(0.0007)	(0.0008)	(0.0012)	(0.0038)
N	4235220	3701912	3701912	3701912	3917747	3917747
Mean Y	0.0147	39.3207	0.1058	0.1116	0.9796	0.6667

Notes: This table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. The high impact sample corresponds to infants whose mothers had less than a high school education. These regressions correspond to our preferred specification, which include county, year fixed effects, individual demographic controls, geographic time varying controls and 1960 determinants trends. Standard errors are clustered at the county level. For more details about the estimation, see Section 4. * p<0.10, ** p<0.05, *** p<0.01.

Table A.5: The Effect of CHCs on Birth Weight, Alternative Specifications

	Main	TWFE	StYrFx	Conception
	(1)	(2)	(3)	(4)
A: Static Policy Effect				
CHC	2.7322**	0.4666	1.5305	2.5940*
	(1.3622)	(1.1080)	(1.1080)	(1.3678)
B: Dynamic Policy Effect				
Years -7 to -6	1.5082	3.9516**	1.1984	2.3706
	(1.6087)	(1.7528)	(1.5035)	(1.7705)
Years -5 to -4	2.1240	4.0435***	1.2023	2.5646*
	(1.3811)	(1.5640)	(1.2411)	(1.4391)
Years -3 to -2	0.4729	1.8562*	0.6192	-1.1770
	(0.9137)	(1.0788)	(0.9038)	(0.9304)
Years 0 to 1	2.1258**	2.0897**	2.1644**	0.9511
	(0.8520)	(0.9576)	(0.8547)	(0.8569)
Years 2 to 3	3.6142***	3.4046***	2.9756***	1.7602
	(1.0915)	(1.1775)	(1.0705)	(1.1280)
Years 4 to 5	3.0741**	2.5899*	1.7368	1.9760
	(1.3160)	(1.4940)	(1.1728)	(1.3281)
Years 6 to 7	3.1588**	2.8976*	1.1780	2.0919
	(1.5201)	(1.6704)	(1.3165)	(1.6354)
N	4235220	4235220	4235220	4311235

Notes: All columns except 2 in this table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). Column 2 uses a two-way fixed effects specification. The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. Column 1 reproduces our preferred specification, which includes county, year fixed effects, individual demographic controls, geographic time varying controls and 1960 determinants trends. Column 3 adds state-by-year fixed effects. Column 4 redefines the treatment timing based on estimated time of conception. Standard errors are clustered at the county level. For more details about the estimation, see Sections 4 and 7. * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: The Effect of CHCs on Low Birth Weight, Alternative Specifications

	Main	TWFE	StYrFx	Conception
	(1)	(2)	(3)	(4)
A: Static Policy Effect				
CHC	-0.0009***	-0.0003	-0.0007**	-0.0010***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
B: Dynamic Policy Effect				
Years -7 to -6	0.0004	-0.0002	0.0005	0.0001
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Years -5 to -4	0.0002	-0.0000	0.0004	-0.0000
	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Years -3 to -2	0.0000	-0.0001	-0.0001	0.0006
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Years 0 to 1	-0.0002	-0.0001	-0.0002	0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Years 2 to 3	-0.0007*	-0.0006	-0.0005	0.0000
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Years 4 to 5	-0.0006*	-0.0004	-0.0004	-0.0004
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Years 6 to 7	-0.0003	-0.0002	-0.0001	-0.0002
	(0.0004)	(0.0004)	(0.0004)	(0.0005)
N	4235220	4235220	4235220	4311235

Notes: Low birth weight defined as less than 2500 grams. All columns except 2 in this table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). Column 2 uses a two-way fixed effects specification. The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1988. We compile data on CHC timing from (1) Bailey and Goodman-Bacon (2015) for years 1965-1980 and (2) FAADS for years 1981-1988. The analysis is run using data at the cell level. Column 1 reproduces our preferred specification, which includes county, year fixed effects, individual demographic controls, geographic time varying controls and 1960 determinants trends. Column 3 adds state-by-year fixed effects. Column 4 redefines the treatment timing based on estimated time of conception. Standard errors are clustered at the county level. For more details about the estimation, see Section 4 and 7. * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: The Effect of CHCs on Infant Health, Main Outcomes

Sensitivity with Timing: CHC Adoption and Natality data by 1980

	Birth Weight			Low Birth Weight		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Static Policy Effect						
CHC	6.7400***	6.0028***	3.6715**	-0.0017***	-0.0016***	-0.0012***
	(1.3943)	(1.4221)	(1.4513)	(0.0003)	(0.0003)	(0.0004)
B: Dynamic Policy Effect						
Years -7 to -6	1.2401	1.9588	2.1090	0.0005	0.0005	0.0004
	(1.7157)	(1.7096)	(1.7029)	(0.0005)	(0.0005)	(0.0005)
Years -5 to -4	2.2720	2.3439	3.0254**	0.0002	0.0002	0.0001
	(1.4735)	(1.4583)	(1.4671)	(0.0005)	(0.0005)	(0.0005)
Years -3 to -2	0.4352	0.2733	0.8600	0.0001	0.0001	0.0000
	(0.9444)	(0.9452)	(0.9455)	(0.0004)	(0.0004)	(0.0004)
Years 0 to 1	2.6919***	2.6637***	2.2490**	-0.0001	-0.0002	-0.0001
	(0.9093)	(0.9314)	(0.9564)	(0.0003)	(0.0004)	(0.0003)
Years 2 to 3	6.3839***	5.8421***	4.8725***	-0.0015***	-0.0015***	-0.0013***
	(1.3705)	(1.3563)	(1.4220)	(0.0004)	(0.0004)	(0.0004)
Years 4 to 5	9.3798***	8.2746***	6.4702***	-0.0018***	-0.0018***	-0.0014**
	(2.0176)	(2.0742)	(2.1736)	(0.0005)	(0.0006)	(0.0006)
Years 6 to 7	10.3713***	9.0871***	6.9369***	-0.0017***	-0.0016***	-0.0013**
	(2.1508)	(2.2731)	(2.3932)	(0.0006)	(0.0006)	(0.0006)
N	2281181	2278654	2278112	2281181	2278654	2278112
Mean Y	3290.2325	3290.2908	3290.2660	0.0873	0.0873	0.0873
Baseline	Y	Y	Y	Y	Y	Y
Geographic Time Varying Controls	N	Y	Y	N	Y	Y
1960 Determinants Trends	N	N	Y	N	N	Y

Notes: This table reports the estimated coefficients and standard errors using the methods introduced in Sun and Abraham (2020). The outcome data is from the Vital Statistical Natality Files that cover the years between 1968 and 1980. We compile data on CHC timing from Bailey and Goodman-Bacon (2015) for years 1965-1980. The analysis is run using data at the cell level. All regressions include county, year fixed effects, and individual demographic controls. Standard errors are clustered at the county level. For more details about the estimation, see Section 4. * p<0.10, ** p<0.05, *** p<0.01.