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Healthy Babies: Does Prenatal Care Really Matter?

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ABSTRACT

Recent economic literature on child development has underscored the importance of giving babies a healthy start. Despite the widespread use of prenatal care, whether this early investment improves infant health is not well understood. This study provides new causal evidence on this crucial issue using 1.4 million sibling births. The baseline within-family analysis yields robust evidence that either early care onset or an increase in visits has a salient payoff in terms of newborn health stock. Furthermore, this study exploits two quasi-experiments to respectively deal with potential bias in the within-mother estimates and investigate the effectiveness of prenatal care under a Medicaid managed care reform. Overall, the results suggest it is important to improve care access for childbearing women especially those with low socioeconomic status. Moreover, caution is needed in design and delivery of managed care plans not to undermine provision of adequate prenatal care.

Keywords: prenatal care; infant health; early childhood environment; in utero health investments; Medicaid managed care; sibling data

JEL classification: I12; I18

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1. Introduction

Health at birth and the ways in which the prenatal period affects birth outcomes have received much attention in the economics literature of child development (Currie and Almond, 2011; Almond et al, 2017). The importance of having good birth outcomes and adequate prenatal investments have been underscored by the recent skill formation models, in which a higher level of health stock at birth will create a higher level of health and human capital postpartum as well as make subsequent parental investments more productive (Cunha and Heckman, 2007, 2008; Cunha et al. 2010; Almond and Currie, 2011). Moreover, the existing studies show the elasticity of substitution is usually low between prenatal and postnatal investments. As a result, it lowers the incentives for parents to remediate the disadvantage of an infant born less healthy in future periods, which may exacerbate the between-child health inequality within families (Currie and Almond, 2011; Almond and Mazumder, 2013).

Indeed, the lifetime burdens of poor health at birth have been documented by the recent causal analyses which use twin or sibling data from the U.S. or other developed counties. The short term excessive hospital costs and risk of one-year mortality are significantly higher for the low birth weight or preterm babies (Almond et al., 2005; Black et al., 2007; Oreopoulos et al., 2008). Poor birth outcomes also undermine childhood cognitive development and adolescent educational attainment (Oreopoulos et al., 2008; Figlio et al., 2014). In the long run, babies with lower birth weight have worse adult outcomes than their own twins or siblings in terms of education, health, labor market outcomes, and well-being of the next generation (Behrman and Rosenzweig, 2004; Black et al., 2007; Oreopoulos et al., 2008; Royer 2009; Bharadwaj et al., 2017). In particular, policy interventions beyond early childhood such as improvement on school quality are found to be ineffective to mitigate such persistent disadvantages which result from adverse birth outcomes and poor early-life investments (Cunha and Heckman, 2007; Figlio et al., 2014). Therefore, there is an emerging consensus on the importance of adequate investments over the nine months in utero which is one of the most critical periods in one's life.

Promoting prenatal care usage is an important strategy to improve infant health. Through check-ups, health professionals not only instruct women on nutritional diet, weight gain, smoking cessation, illness prevention, and healthy life style throughout the course of pregnancy; but also provide diagnoses and treatments for maternal health and physiological problems (ACOG, 2012), all of which are expected to benefit the infants. In fact, support for the dramatic expansion of Medicaid since the early 1980s has been largely based on the perception that prenatal care "works" for preventing poor birth outcomes and is more cost-effective than hospital services (Currie and Gruber, 1996). Use of prenatal care is also a key mechanism for other policy initiatives related to newborn health, such as enriched prenatal services (Reichman and Florio, 1996; Joyce 1999), welfare reform (Currie and Grogger, 2002; Kaestner and Lee, 2005), new service delivery and payment systems (Aizer et al., 2007; Jensen 2014; Hu et al., 2015), malpractice liability pressure (Dubay et al, 2001), and Medicaid physician fee increases (Gray 2001; Sonchak, 2015). In addition, investigating the efficacy of prenatal care usage in the context of household production provides insights on intra-family resource allocation and newborn health disparities across different socioeconomic groups (Corman et al., 1987; Rosenzweig and Wolpin, 1991; Rosenzweig and Wolpin, 1995; Currie, 2009).

However, empirical tests on whether prenatal care "works" are difficult, since immeasurable and unobserved mother- or family-level determinants of infant health affect usage of prenatal care. To address this endogeneity issue, researchers have applied two principal empirical strategies. The first approach uses instrumental variables (IV) which typically come from area-level (e.g., MSA-, state-, or county-level) characteristics or policies on the availability, access, and cost of prenatal care usage (Rosenzweig and Schultz, 1983; Grossman and Joyce, 1990; Rous et al., 2004; Conway and Deb, 2005; Wehby et al., 2009; Sonchak, 2015).[2] Nonetheless, one threat to the validity of such IVs is that health professionals and policymakers can alter the availability and accessibility of prenatal services according to the area-specific pregnancy outcomes and underlying health care needs of women. But this concern does not carry over to Evans and Lien (2005) which instruments for prenatal care by an unanticipated bus strike. However, endogenous residential location of urban dwellers will introduce a selection bias on the estimated impacts of the bus strike, which biases the corresponding IV results. Moreover, changes of area-level policies used as IVs may impact birth outcomes through multiple health inputs (not exclusively through prenatal care), or occur in conjunction with the dynamics of other

[2] In randomized controlled trials (RCTs) of prenatal care usage, the IV is essentially the assignment variable. However, the samples used in RCTs are usually selective (for instance, only low-risk women were recruited in several studies) and not large enough for analyzing adverse pregnancy outcomes with low incidence such as low birth weight (Carroli et al., 2001). In addition, previous RCTs often focus on the components/contents of care within different provision models while rarely address how to enhance cost efficiency (e.g., promoting early care onset) regardless of the type of care models (Symon et al., 2017).

policies and social programs (Currie and Grogger, 2002). In this case, a reduced form approach is more appropriate.

The second applies within-mother analysis to sibling births. Comparisons of sibling births will eliminate the source of confounding from the birth invariant mother- or family-level unobservables (Rosenzweig and Wolpin, 1991; Rosenzweig and Wolpin, 1995; Abrevaya, J., 2006; Abrevaya and Dahl, 2008; Aizer and Currie, 2014; Balsa and Triunfo, 2015). However, the previous studies along this line also have several limitations. One, the data used often track women's fertility over a short period of time and lack sufficient observations on mothers with more than three sibling births. Two, the within-mother estimates will be biased in the presence of feedback effects (previous birth outcomes impact the current prenatal care) or measurement error on care usage. Three, the underlying shocks for the cross-birth variation of prenatal care usage could be diversified. As such, the policy implications of the prior within-family analysis are unclear. Lastly, it is worth mentioning the existing causal evidence on effectiveness of prenatal care from either the IV or within-mother literature is mixed and inconclusive.

This study sheds new light on the role of prenatal care in improving birth outcomes, with a unique data of sibling births. The sample for the baseline analysis contains 1.4 million newborns delivered by about 0.6 million mothers over 22 years in the state of Pennsylvania. This large sample allows us to precisely estimate how different elements of prenatal care impact birth outcomes using a within-mother estimator. It also permits analysis on the heterogenous effects across different subgroups, the underlying mechanisms of prenatal care, and comparison with sibling results from another state.

More importantly, the long sample period of the Pennsylvania sibling data covers two interesting quasi-experiments on prenatal care usage. The first is the one-month (March 16 to April 13) Port Authority Transit strike in Allegheny County in 1992 ever examined in Evans and Lien (2005). Our approach differs from that study in two ways. One, we follow Currie and Schwandt (2016) to use mother fixed effects to deal with the endogenous residential location of metropolitan area residents, thereby improving the validity of the bus strike instrument. Moreover, exploiting the exogenous variation from the bus strike will address the potential bias in the within-mother estimates due to any feedback effect or measurement error on care usage. Two, we focus on the corresponding change on care visits for the less educated Allegheny women who rely heavily on public transit, consistent with the recent urban literature which suggests public transportation is a primary driving force for the poor's urbanization (Glaeser et al. 2008; Duranton and Puga, 2015).

The second is the phase-out of the HealthChoices program across several zones which offered mandatory managed care to the Pennsylvania Medicaid recipients. Specifically, we will explore whether this new system of service delivery impacted birth outcomes and to what extent this effect was accounted for by changes on prenatal care. This analysis complements Hu et al. (2015) which looks at the effects of this reform on maternal preventable complications and healthcare cost containment. With the HealthChoices initiative, the Medicaid eligible women are likely to change the way of self-selecting into the Medicaid program by maternal unobserved characteristics. To deal with this selection problem, we follow Aizer et al. (2007) to control for the mother fixed effects in the empirical model. Furthermore, we will compare the prenatal care pathway with changes on access to high-quality hospital care at the birth facility under the HealthChoices expansion. Overall, all the empirical tests yield robustness evidence that prenatal care makes a difference in newborn health.

2. Data

The primary data set for this study is constructed from the birth certificates of all birth occurrences in the state of Pennsylvania (PA) from 1989 to 2010.[3] Prior to 1989, important maternal health information such as gestational weight gain and prenatal smoking was not reported. And the data was available only through 2010 when we began this research project. To complement the PA analysis, we also make use of birth records from the Washington state (WA) from 2003 to 2006. In addition to prenatal care utilization, the birth records contain rich information on infant health, parental demographic characteristics, mother's county and city of residence, and the newborn birth facility, etc.

With access permission to the universe of birth files, we link consecutive singleton births to the same mother by mother's name, date of birth, race/ethnicity, and newborn parity. Then, we restrict the sample to PA residents and drop women with more than five births in the sample

[3] We do not have data on fetal deaths, since the birth certificates only record information of infants born alive. It could be the case that improved prenatal care reduces the risk of fetal loss and especially helps unhealthy fetuses to survive (healthy fetuses often survive even without adequate care). Therefore, the saved babies are likely to be small relative to the population. Consequently, our estimate from the live births only (without addressing the fetal selection) will be smaller in magnitude than the birth weight effect of improved prenatal care for all the fetuses.

period.[4] As a result, our de-identified full PA sibling data contains 611,107 mothers with two to five singleton births, in total 1,421,593 mother-infant pairs. A similar approach is used to construct a sample of 50,083 WA sibling births. Due to the short study period, this WA sample does not have observations on women having more than four singleton births. However, compared with the PA sibling data, it codes additional information on maternal employment status during last year (before the baby's birth) which is useful for sensitivity analysis.

To construct the sample for the Allegheny bus strike from the PA sibling data, we note this shock mainly impacts poor urban dwellers who use public transit at a much higher rate than other income groups (Linsalata, 1992). Glaeser et al. (2008) presents theoretical and empirical results on how public transportation explains urbanization of poverty.[5] With income not coded in birth records, we firstly restrict the sample to the women with no more than 12 years of education who usually have low income. Below, our empirical approach will deal with endogenous residential sorting of the less educated women who choose urban life into Allegheny and other high-density urbanized areas, and then compares the women with or without exposure to this bus strike on their usage of prenatal care and birth outcomes.[6] Secondly, we focus on 1989-1995 with about three years' worth of data before and after the strike, for a final bus strike sample of 215,371 mother-infant observations.

Our HealthChoices sample is related to three main phases of the HealthChoices expansion in 1997 (Southeastern zone, 5 counties), 1999 (Southwest zone, 10 counties), and 2002 (Lehigh/Capital zone, 10 counties).[7] This reform has the potential to impact birth outcomes, by

[4] Among all the women with multiple singleton births from PA birth records, less than two percent had more than five births. We have also compared the fertility pattern of the 15- to 44-year-old women from the PA universe birth records with the women ever with a child from the Current Population Survey. The propensity of having more than one baby of the former group in PA is about 14 percent less than the national average, partly due to the low percentage of the black and Hispanic populations in this state. Moreover, by looking at women over 35 who ever delivered a baby in both data sources, we find suggestive but consistent evidence that such women have a high probability (about 80 percent) of having in total two to five babies over the life cycle.

[5] Public transportation involves large time fixed costs. Such fixed costs encourage the poor with low opportunity costs of time to disproportionately use this transportation mode. There is also a high time cost per mile for public transit usage. Therefore, the comparative advantage of public transportation (relative to automobiles) is salient for short distance commutes to the city center, which incentivizes the poor to centralize (Glaeser et al., 2008; Duranton and Puga 2015).

[6] Additional analysis on the women with more than 12 years of education shows small and insignificant effect of the bus strike on prenatal care and infant health.

[7] The Southeast zone initiated the program on Feb 1, 1997, with the zone consisting of Bucks, Chester, Delaware, Montgomery, and Philadelphia counties. The Southwest zone included Allegheny, Armstrong, Beaver, Butler, Fayette, Greene, Indiana, Lawrence, Washington, and Westmoreland counties. This zone adopted the program on

altering the quality or quantity of various medical services (such as prenatal care) for the Medicaid mothers. In constructing this sample, we impose the following restrictions on the original PA sibling data. One, the sample period is limited to 1994 to 2004.[8] Two, while the birth files do not report maternal Medicaid eligibility or coverage, we focus on the native-born unmarried women with no more than 12 years of education. Such disadvantaged women were very likely to be eligible for Medicaid (Aizer et al., 2007). Three, we follow Hu et al. (2015) to drop all the mothers residing in the South and West Philadelphia where a pilot Medicaid managed care program (known as HealthPass) was operated before 1997 but plagued by a variety of problems (Johnston, 2003). The resulting full HealthChoices sample includes 81,588 mother-infant pairs.

We also use the birth facility information to introduce several hospital-level controls to the sample, including the number of beds staffed, the presence of neonatal intensive care unit (NICU) level 2 or 3.[9] Such birth and time variant variables on hospital capacity measures maternal access to high-quality hospital care and impacts medical treatment intensity (Picone et al., 2003; Aizer et al., 2007). In addition, we use hospital fixed effects to capture unobserved hospital characteristics which contribute to the quality of hospital care. Below, we will explore to what extent the channels of prenatal care and access to hospital care can account for the total HealthChoices effect. Moreover, because the birth facility information is unavailable after 2002, we limit the sample period to 1994-2002 and lose some useful variations of the HealthChoices expansion, especially for the Lehigh/Capital Zone. The final HealthChoices-Hospital sample includes 67,243 sibling births.

This study examines four measures of infant health. The first is birth weight, the primary birth outcome measure in most economic studies of newborn health and welfare. The second is low

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Jan 1, 1999 (Pennsylvania Department of Public Welfare, 1997). The Lehigh/Capital zone initiated mandatory enrollment on April 1, 2002. This zone served Adams, Berks, Cumberland, Dauphin, Lancaster, Lebanon, Lehigh, Northampton, Perry, and York counties (Pennsylvania Department of Health and Human Services, 2003). Moreover, 25 counties had made HealthChoices available to the Medicaid recipients on a voluntary basis before 1997: Bedford, Blair, Bradford, Cambria, Carbon, Clarion, Clearfield, Columbia, Crawford, Erie, Forest, Franklin, Jefferson, Lackawanna, Luzerne, Mercer, Monroe, Montour, Pike, Schuylkill, Somerset, Sullivan, Susquehanna, Warren, and Wyoming counties. The other 17 counties served the Medicaid enrollees through fee-for-service (FFS). Below we use the Voluntary and FFS counties together as the control counties.

[8] Since 2005, the state had expanded managed care by introducing the ACCESS Plus program (a primary care case management program) to the 42 control counties with a voluntary HealthChoices or FFS program (Lewin Group, 2005).

[9] The hospital data are available at: www.statistics.health.pa.gov/HealthStatistics/HealthFacilities/Hospital Reports.

birth weight (LBW, birth weight less than 2,500 grams), a key indicator for poor health at birth which adversely influences various lifetime outcomes such as health, education, and earnings. It is generally recognized that LBW results from either fetal growth restriction or shortened gestation. A commonly used measure for the former is small for gestational age (SGA, defined as birth weight below the 10th percentile for babies of the same gestational age in the full sibling sample); for the latter, we look at preterm birth (gestational age less than 37 weeks). As to prenatal care utilization, we consider three measures. The first is care onset beyond the first trimester.[10] Since timeliness is an important element of care adequacy, late care initiation is expected to negatively affect infant health. The second is the total number of care visits. The third one is "inadequate care" for the women receiving "inadequate" or "intermediate" care by the Kessner index. This index assesses care adequacy by taking into account the timing of care onset and number of visits conditional on gestation (Kessner et al., 1973). In addition, the control variables for this analysis include the characteristics of the infant (sex, birth order, birth year and month), the mother (race/ethnicity, age, education, marital status, pre-pregnancy hypertension, and previous termination of pregnancy), and the father (race/ethnicity, age, education).

Table 1 presents the descriptive statistics for the full PA sibling sample (column 1) and the three subgroups by trimester of care onset (columns 2-4).[11] By the first column, 5 percent of the infants are born at LBW and the incidences of SGA and preterm birth are 10 percent and 6 percent, respectively. Over all the mother-infant pairs, about 18 percent of the mothers smoked during pregnancy and 38 percent had inadequate or excessive weight gain. As to prenatal care, while on average the pregnant women took 11 visits during pregnancy, about 16 percent of them begun care utilization beyond the first trimester, 27 percent had inadequate care by the Kessner Index. There are also remarkable mother-level variations on care utilization (not shown), which is particularly helpful for us to apply a within-mother estimator. For instance, about 22 percent of the mothers (135,172 mothers) changed the timing of care onset at least once over past conceptions; 53 percent of them ever changed the number of care by more than two visits and 27 percent altered the total number of visits by at least four across different pregnancies.

The other columns of Table 1 demonstrate a striking monotonic relationship between the timing of care initiation and infant health. Mothers with care onset beyond the first trimester are

[10] We follow the literature to add the women receiving no care (extremely delayed care) into this late care onset group (Dubay et al., 2001; Kaestner and Lee, 2005).
[11] The summary statistics for the bus strike or HealthChoices sample are available upon request.

more likely to have LBW babies than those with first trimester care initiation, similarly for the other birth outcomes. Late care initiation also appears positively associated with fewer care visits, prenatal smoking, and inadequate gestational weight gain.[12] Moreover, comparing columns (3) and (4) with (2), we find women with lower socioeconomic status tend to have late care onset. Such women are more likely to be less educated, younger, black or Hispanic, and unmarried, relative to the women with early care onset.

[Insert Table 1 Here]

3. Method

We begin by the following empirical model on infant health production, using the full sample of PA sibling births:

$$Y_{ijctm} = \alpha_0 + \alpha_1 PNC_{ijctm} + \alpha_2 X_{ijctm} + \mu_i + \theta_c + \theta_t + \theta_m + \varepsilon_{ijctm}, \quad (1)$$

where Y_{ijctm} is a birth outcome such as birth weight for mother i with infant j born in county c in year t and month m, and PNC is a measure on prenatal care (e.g., late care onset) that this mother received during pregnancy. X is a vector of aforementioned infant, maternal and paternal controls. This model also includes a set of mother fixed effects μ_i, maternal county of residence fixed effects θ_c, infant birth year effects θ_t, and birth month effects θ_m. The coefficient of interest is α_1 which captures how prenatal care affects birth outcomes.

Estimating equation (1) by ordinary least squares (OLS) without controlling for μ_i will produce biased estimates for α_1, since there are immeasurable birth invariant mother- or family-level determinants of infant health which also affect prenatal care usage. Such unobservable characteristics can be cognitive and non-cognitive abilities, maternal health endowment, propensity for risky health behaviors, etc. Although imperfect, this OLS approach provides a useful starting point. We will apply OLS separately to the PA singleton births and sibling births, and then contrast the results with the within-mother estimates below.

To eliminate the birth-invariant factors μ_i, we demean the outcome and explanatory variables in equation (1) at the mother level and then run OLS to the demeaned sibling birth data. The resulting within-mother estimates for α_1 will be unbiased, given that PNC is strictly exogenous

[12] The indicator of inadequate gestational weight gain equals one in two cases. One, for term pregnancies, the total weight gain is below 7 kg (which is about the lower limit of recommended weight gain ranges across all the preconception BMI groups). Two, for preterm births, the weight gain rate is less than 0.3 kg/week in the second and third trimester. Likewise, we consider gestational weight gain to be excessive, if the total maternal weight gain is more than 18 kg for term pregnancies or the weight gain rate exceeds 0.5 kg/week beyond the first trimester for preterm births (Institute of Medicine, 1990; Yan, 2015).

conditional on the mother fixed effects (Wooldridge, 2010). Nevertheless, this approach is subject to several caveats. First, the within-mother results will be biased if unobserved birth-varying characteristics systematically drive prenatal care and birth outcomes. To address this concern, we will check whether the estimates are sensitive to additional birth-varying variables.

Second, strict exogeneity for PNC will fail if a negative health shock during the last pregnancy causes women to use more care in the current conception. It can be easily shown this feedback effect will bias up the mother fixed effect (FE) estimates on α_1 (Abrevaya, 2006; Balsa and Triunfo, 2015). One standard solution is to firstly assume PNC is sequentially exogenous (weaker than strict exogenous) which allows feedback effects, then first-difference equation (1) removing μ_i and apply lagged $PNCs$ to instrument for ΔPNC (Cameron and Trivedi, 2005; Wooldridge, 2010). Because sequential exogeneity is still a fairly strong restriction, preferably we can find an exogenous variable outside model (1) to instrument for PNC.

Third, there could be measurement error on care usage. For instance, the birth facility may record false information on the month or day for care initiation from the prenatal visit flow sheet. However, aggregating the care onset timing at the trimester level in this study will alleviate this problem. In contrast, the presence of classical measurement error (CME) on the coded total number of visits is a more serious concern. Using the within-mother estimator will exacerbate the attenuation bias due to CME, especially when the measurement noise is weakly correlated across births. It is notable that this source of downward bias and the upward bias from the feedback effect tend to cancel out each other. Again, an IV approach can address the CME issue.

The 1992 Allegheny County bus strike provides a good instrument for prenatal care, under certain conditions below. Before we get into the details, it is worth noting while the FE estimates from equation (1) apply to the women who change prenatal care by various (unknown) reasons, the IV results only capture the casual effect of prenatal care for the Allegheny mothers who lost care or delayed care onset in the wake of the bus strike. Now consider the following first stage equation, with the bus strike sample of the less educated women having sibling births:

$$PNC_{ijctm} = \beta_0 + \beta_1 STRIKE_{tm} + \beta_2 STRIKE_{tm} \times URBAN_c + \beta_3 STRIKE_{tm} \times ALLEGHENY$$
$$+\beta_4 X_{ijctm} + \nu_i + \lambda_c + \lambda_t + \lambda_m + \xi_{ijctm}, \qquad (2)$$

where we use the same variable subscripts as equation (1). The indicator $STRIKE$ equals 1 for the women across all the PA counties whose pregnancies covered part of or the entire period of the four-week bus strike. $URBAN$ is an indicator for the large urbanized areas (population in the central core>1 million) with high population density (>1,000 people per square mile) in the state of Pennsylvania, including Allegheny and the comparable counties, where public transit has played an important role serving the low-income residents.[13] $STRIKE \times URBAN$ controls for the common shocks to prenatal care utilization in the large urbanized counties during the treatment period. $STRIKE \times ALLEGHENY$ is the bus strike shock specific to Allegheny County and β_3 captures the corresponding change on prenatal care PNC.[14] Here, the demographic controls X contains additional maternal characteristic (marital status, pre-pregnancy hypertension, and previous termination of pregnancy). The model also includes mother fixed effects ν_i, county fixed effects λ_c, newborn birth year and month effects (λ_t, λ_m). Because errors are unlikely to be independent within counties over time, we cluster the standard errors at the county of maternal residence (Cameron and Miller, 2015).

Next, using $STRIKE \times ALLEGHENY$ to instrument for PNC in equation (2), we can apply Two-Stage Least Square (2SLS) to estimate the causal effect of prenatal care on infant health Y which is γ_3 in the following equation:

$$Y_{ijctm} = \gamma_0 + \gamma_1 STRIKE_{tm} + \gamma_2 STRIKE_{tm} \times URBAN_c + \gamma_3 PNC_{ijctm} + \gamma_4 X_{ijctm} + \phi_i + \pi_c + \pi_t + \pi_m + \sigma_{ijctm}, \quad (3)$$

where the set of fixed effects and control variables are defined in a way like equation (2). While we do not impose strict or sequential exogeneity on PNC, the identification of γ_3 requires the bus strike instrument meet two conditions: relevant in the first stage regression and uncorrelated with the error term in equation (3). The second one (the exclusion restriction) will be less plausible without the mother FE ϕ_i in equation (3), because there could be residential sorting of less educated women into Allegheny (due to the dynamics of local labor market conditions and amenities including public transit system) and then into the central city/suburban area by

[13] Two cases are considered for such large and high-density urbanized areas. The first is the baseline "three-county" case, where $URBAN = 1$ for Allegheny County (here the central city is Pittsburg and the suburbs are the within-county areas outside Pittsburg), Philadelphia County (the central city counterpart of Pittsburg. Note the city of Philadelphia extended the city borders to be coterminous with Philadelphia County in 1854), and Montgomery County (defined as the high-density suburban area of Philadelphia). The second is the "four-county" case, where we further add Delaware County to the high-density suburban area of Philadelphia for sensitivity analysis. The other two counties adjacent to Philadelphia were less densely populated and unlikely to capture counterfactual scenarios for the suburbs of Pittsburg during the bus strike period.

[14] The interaction term $STRIKE \times ALLEGHENY$ is equivalent to $STRIKE \times URBAN \times ALLEGHENY$. Also, applying OLS to equation (2) gives causal estimates of the bus strike effect (Lee, 2005).

maternal unobserved characteristics of health/ability ϕ_i.[15] As Currie and Schwandt (2016) points out, simply using county/neighborhood fixed effects and observable maternal socio-economic variables is not sufficient to capture the full story of compositional changes and sorting of local residents over time.

It is also important to include mother FE ν_i in the first stage regression. To see the point, consider a within-Allegheny setting where we use two finer bus strike instruments by the central city/suburban area. In the presence of job decentralization and spatial mismatch within urban labor markets (Ihlanfeldt, 2006), the Pittsburg low-skilled women are very likely to differ from the counterparts who live in or move to suburban neighborhoods in immeasurable characteristics ν_i, although all of them are frequent users of public transit. As such, the residential location choice by ν_i endogenously affects to what extent the central city and suburban women can differentially handle the bus strike, by altering women's family income, time cost of receiving prenatal care, etc. Consequently, it will bias the estimated heterogenous impacts of the strike on prenatal use for both groups of women, unless we control for the mother FE in equation (2). In addition, conditional on residential sorting within the high-density urbanized areas, the blacks generally use public transportation at a higher rate than the other race groups. Below, we will follow Evans and Lien (2005) to interact the Allegheny strike instrument with indicators of black and non-black residents and investigate the corresponding heterogenous changes on care usage.

Finally, the expansion of the PA HealthChoices program offers an interesting opportunity to explore the effectiveness of prenatal care under contemporary health care reform. We start with the following reduced form model by the HealthChoices sample:

$$Y_{ijctm} = \delta_0 + \delta_1 HealthChoices_{ctm} + \delta_2 X_{ijctm} + \delta_3 Zonetrend_{ct} + \psi_i + \chi_c + \chi_t + \chi_m + \zeta_{ijctm}, \quad (4)$$

where the variable subscripts are the same as equation (1) and Y for outcomes. $HealthChoices$ equals 1 for the Medicaid eligible mothers who conceived a baby after their residence counties implemented the mandatory HealthChoices for Medicaid recipients. The coefficient δ_1 captures the total effect of this program. X represents demographic controls. Below, we will control for prenatal care (a channel of the HealthChoices) by adding the term $\delta_4 PNC_{ijctm}$ to equation (4), where PNC is assumed to be strictly exogenous. In this parsimonious specification, we follow

[15] Most of the women in the sample who move into or away from Allegheny County or other counties of the high-density urbanized areas are metropolitan residents.

Hu et al. (2015) to include $Zonetrend$ a vector of four linear time trends (for the three HealthChoices zones plus one control zone).

Due to the HealthChoices initiative, the Medicaid eligible women in the treatment counties may alter the way of self-selecting into Medicaid for managed care services. Aside from the observed control variables, unobserved maternal characteristics also can contribute to the compositional change of Medicaid enrollees under the reform. Like Aizer et al. (2007), we deal with this selection issue by adding mother FE ψ_i in equation (4). The next section will also consider the case of maternal selective migration across counties by birth variant unobservables. The χ_c, χ_t, and χ_m are county, birth year, and birth month fixed effects. The robust standard errors are clustered at the maternal residence county level. In addition, we will examine several variants of equation (4) such as dropping the zone-specific time trends or using PNC as the dependent variable.

In addition to prenatal care, the HealthChoices can operate through access to high-quality hospital care at the birth facility.[16] To contrast the two pathways, we use the specification below:

$$Y_{ijchtm} = \rho_0 + \rho_1 HealthChoices_{ctm} + \rho_2 Hosp_{htm} + \rho_3 X_{ijctm} + \rho_4 Zonetrend_{ct} + \kappa_i$$
$$+\omega_h + \omega_c + \omega_t + \omega_m + \tau_{ijctm}, \qquad (5)$$

where h indexs hospitals. $Hosp$ represents strictly exogenous time-variant indicators on hospital capacity, which capture maternal access to intensive treatments at the birth facility: NICUs (level 2 or above, level 3 or above), staffed beds>200 (or 400). The model also includes hospital fixed effects ω_h for time invariant hospital-level determinants of high-quality care which can vary by birth (women may switch hospitals between sibling births). The other variables and fixed effects (for mother, county, birth year/month) are the same as the counterparts in equation (4).

Note we use equation (5) and the HealthChoices-Hospital sample to obtain an estimated health effect of the HealthChoices (ρ_1), while controlling for maternal access to high-quality hospital care. This estimate should be smaller than the one (total effect) from equation (4) without the hospital-level controls, based on the same sample. The difference of such two

[16] For instance, childbearing women on Medicaid who have developed serious health problems such as acute gestational hypertension usually seek health services from hospitals before giving births. However, as introducing managed care plans and capitated payment by the HealthChoices provides clear incentives to manage treatment costs and limit care utilization, it may restrict such women's access to high-quality and expensive hospital services (such as NICUs or intensive care from large hospitals).

estimates capture the strength of the access to hospital care pathway. [17] A similar exercise, with the same sample, will provide insights on the prenatal care pathway. In the final step, we will compare the magnitude of such two mechanisms.

4. Results

Table 2 reports the baseline results using the full PA sibling birth data.[18] By Panel A, care onset beyond the first trimester reduces newborn birth weight by 43.5 grams (g) and increases the risk of LBW by 1.2 percentage points, without controlling for the mother FE (columns 1 and 4). The corresponding two within-mother estimates of late care onset are smaller but still highly significant: -30 g on birth weight and a 0.8 percentage-point or 16 percent increase on LBW (columns 2 and 5). Similar patterns emerge for the other two adverse birth outcomes (Panel C). The mother FE estimates suggest late care initiation increases the risk of SGA by 7 percent and preterm birth (or prematurity) by 15 percent, relative to the sample means.

Moreover, for each birth outcome, the OLS and FE estimates above are statistically distinguishable from each other. The smaller magnitude of the FE estimates suggests women with unobserved disadvantages tend to have late care onset, while recall Table 1 presents a similar pattern of negative selection by observables. Taken together, we find pregnant women with lower socioeconomic status are more likely to exacerbate rather than compensate for transmission of their overall disadvantages to the offspring by having poor prenatal care, consistent with previous studies (Rosenzweig and Wolpin, 1991; Rosenzweig and Wolpin, 1995; Abrevaya, J., 2006). The within-mother estimates are similar in the presence of additional birth-varying variables of marital status, pre-pregnancy hypertension, and previous termination of pregnancy (columns 3 and 6).

[Insert Table 2 Here]

The other two panels look at the number of care visits. The OLS results suggest beneficial effects on all the four birth outcomes (columns 1 and 4). Adding mother FE effects, the other columns indicate one more visit significantly increases newborn birth weight by about 22 g, lowers the LBW risk by 0.8 percentage points and the incidence of preterm birth by 1.1

[17] To the extent some time-varying elements on access to hospital care are unobserved and not controlled for, this comparison gives a lower bound of this channel.

[18] We also use all the singleton births from the original PA data to estimate equation (1) by OLS. The results are very similar to those based on the sibling births (columns 1 and 4).

percentage points, but has a small and insignificant effect on SGA. In addition, we have also tried clustering at the maternal residence county since the county fixed effects may not control for all the within-county error correlation. While this leads to larger standard errors, all the precisely estimated coefficient in Table 2 retain significance at 1 percent.

One might wonder whether the significant health effects of prenatal care can be seen in other states. Appendix A reports the within-mother estimates using the WA sibling birth data. By columns (1) and (4), late care onset is associated lower birth weight of -32 g plus an increased LBW risk by 0.8 percentage points, while one additional visit increases birth weight by 18 g and reduces the LBW incidence by 0.5 percentage points. The estimates are not sensitive to the inclusion of the same additional controls for the PA analysis (columns 2 and 5). Furthermore, changes on maternal employment status across births can affect prenatal care usage (through changes on income, insurance coverage, time use, etc.) and infant health. The WA data allows us to further control for this important birth variant variable in columns (3) and (6).[19] The results are almost unchanged.

Table 3 considers two simple extensions, where we focus on birth weight and LBW to save space. One, the late care onset indicator is broken into two by trimester (Panel A). We find that care onset beyond the second trimester (including no care, the case of extremely delayed care) has a stronger effect on birth weight than onset in the second trimester. Similar differential effects of care initiation by trimester are evident for LBW. The estimated effects become smaller when we control for the mother FE. Two, we apply the care adequacy measure which integrates care onset with the number of visits (Panel B). The within-mother estimates suggest inadequate prenatal care lowers newborn birth weight by about 50 g and increases the LBW risk by 1.5 percentage points.

[Insert Table 3 Here]

Table 4 explores heterogenous effects of prenatal care by race/ethnicity and by the number of sibling births that mothers had over the study period. We control for mother FE in all the regressions and again concentrate on the results about birth weight and LBW. Columns (1) to (3) focus on the three major racial/ethnic groups in the full sibling sample. They show both the timing of care onset and the number of visits matter for newborn birth weight. A similar story

[19] Due to data limitations, we have no information on paternal employment. Also, the data does not record the reasons for changes of maternal employment status or whether such changes were permanent or temporary.

emerges for LBW, except for the insignificant effect of late care onset on the Hispanic infants. Columns (4) to (7) split the sample into four groups by the number of births. We find either dimension of prenatal care makes a difference in birth weight and LBW. Within each panel (A to D), the estimated effects of care utilization do not vary much by the number of births. Besides, all the 16 coefficient estimates here are statistically significant at 1 percent.[20]

[Insert Table 4 Here]

There are a variety of channels in the overall causal chain between prenatal care and birth outcomes. For instance, two standard components of the routine prenatal visits are smoking cessation counselling and promotion of healthy weight gain during pregnancy. We then consider to what extent the two channels accounts for the total effect of prenatal care. The results are shown in Table 5, where we impose strict exogeneity for smoking and weight gain in all the regressions with mother FE. Panel A shows the effect of care onset on birth weight and LBW shrink by about 4-5 percent, as the model controls for prenatal smoking. When we further add gestational weight gain measures, the estimated effects decrease by about 13 to 15 percent. Panel B reports smaller reductions (about 3 percent) on the health effects of taking additional prenatal visits, after we control for smoking and weight gain.[21] The other mechanisms (such as stress management and control of infections) which prenatal care works through merit consideration in future research.

[Insert Table 5 Here]

Table 6 investigates the first stage relation between the Allegheny bus strike and usage of prenatal care. Column (1) shows the bus strike has little effect on late care onset for the less educated women in Allegheny County. Nonetheless, by the next column, it reduces prenatal visits by 0.48 visit, with the three counties included in the PA high-density urbanized areas (see footnote 13). The point estimate is similar in column (3), with the alternative definition of urbanized areas (the "four-county" case). The other columns report stronger effects of the bus strike on the Pittsburg residents and blacks. The same pattern is found, when we further consider

[20] Additional subsample analysis by education indicates prenatal care significantly affects newborn birth weight and LBW of both the mothers with no more than 12 years of education (≤12 years) and the higher educated mothers who had completed more than 12 years of education (results available upon request).

[21] We also run regressions with mother FE effects using prenatal smoking or weight gain as the dependent variable. The results (not shown) suggest late care initiation is associated with a higher probability of prenatal smoking and inadequate weight gain but has little impact on excessive gestational weight gain. Similar patterns emerge from having additional care visits (albeit smaller effects).

the central city and suburban specific common shocks during the bus strike period in the specification (not shown). Notably, since the strike instruments are strongly correlated with care visits by the large F statistics (columns 2-6), the corresponding 2SLS estimates below are unlikely to be biased by weak instruments (Angrist and Pischke, 2008).

[Insert Table 6 Here]

Table 7 demonstrates the reduced form estimates. To save space, we focus on the case with a single bus strike instrument. Regressions of birth weight on the bus strike yield significant estimates of about -15 g. Combining this with the first stage results (Table 6) favors the interpretation that the bus strike lowered birth weight exclusively through the number of visits (not the timing of care onset).[22] In contrast, the estimated effects are small and insignificant for the risks of LBW and SGA. The last two columns present robust evidence that the bus strike significantly increases prematurity by 0.9 percentage points.

[Insert Table 7 Here]

Table 8 reports the IV estimates for birth weight and prematurity.[23] The first two columns show, for the less educated Allegheny women who had altered care utilization due to the bus strike, one additional visit increases newborn birth weight by 31-32 g and reduces the risk of preterm birth by 1.8-2 percentage points when we employ a single instrument and either definition on the large and high-density urbanized areas. The 2SLS estimates are smaller but still highly significant with two IVs of the central city and suburban area (columns 3 and 5). Column (4) indicates the estimates are much larger and somewhat implausible, without controlling for the mother FE in equations (2) or (3). For instance, it suggests one more visit reduces prematurity by about 40 percent.[24]

Column (6) of Table 8 estimates the model by two-step generalized method of moments (GMM). The results are quite similar to the 2SLS estimates in column (3), with smaller standard

[22] The bus strike could have influenced birth outcomes through other channels, aside from prenatal care usage. It could be the case that this strike lowered earnings or caused inconvenience in grocery shopping, thereby independently impacting pregnant women's nutrition intake and use of different infant health inputs, for a given number of care visits. To examine this possibility, we run regressions of inadequate or excessive gestational weight gain against the bus strike instrument while controlling for the total number of care visits and mother FE. The coefficient estimates on the bus strike are not significant.

[23] The IV estimates for LBW and SGA are small and insignificant. We do not report them here for brevity.

[24] The results are similar with one bus strike instrument and no inclusion of mother FE in equation (2) or (3). Moreover, we also run native OLS regressions without ϕ_i in equation (3) for the Allegheny residents only and find one additional visit leads to an increase of 39 g in birth weight and a decrease of 1.9 percentage points in prematurity.

errors as expected. The last two columns apply two race-specific strike instruments. The IV estimates are slightly smaller than those from the strike instruments by city/suburb but again statistically significant: one additional visit corresponds to an increase of 23-24 g in birth weight and a decrease of 1.3 to 1.4 percentage points in prematurity (base: 0.08 for the Allegheny women in the sample). The results are almost the same when we further break the strike instrument in equation (2) into four by race and residential area (not shown).

[Insert Table 8 Here]

Table 9 explores how the HealthChoices impacts prenatal care and birth outcomes. The upper panel examines the case without adding zone-specific time trends. For the Medicaid eligible women, implementing this program for mandatory managed care at their counties of residence increases late prenatal care onset by 4.9 percentage points but has little effect on the total number of visits, by the first two columns.[25] As such, below we use the care onset variable only to explore the prenatal care pathway. The other columns indicate that, one, introduction of the HealthChoices significantly lowers newborn birth weight and increases the risk of LBW and prematurity but not SGA; two, the estimated effects on birth weight, LBW, and prematurity shrink by 4 to 8 percent when we control for late care onset. The lower panel includes the zone-specific time trends in the empirical model, which somewhat alters the magnitude of the estimates above but not the statistical significance. Column (1) reports new estimated effect on late care initiation of a 2.8 percentage-point or percent increase. Moreover, by the results across the other columns, we find the prenatal care channel now accounts for about 2 to 4 percent of the total effects of the HealthChoices on newborn birth weight, LBW, and prematurity.

[Insert Table 9 Here]

As a sensitivity analysis, we use either the FFS or Voluntary counties as the control counties and redo the regressions with the zone-specific trends. The pattern (Appendix B) is similar to Table 9. We have also considered two-way clustered standard errors at the county and the mother level (Cameron et al., 2011). In this case, the standard errors become only slightly larger and all

[25] On one hand, the HealthChoices program encourages utilization of preventive care. This can be especially helpful for prenatal care promotion among the newly enrolled beneficiaries with the mandatory managed care introduced, who used to receive limited care. On the other hand, as stated above, the capitated setting incentivizes the HealthChoices managed care organizations to constrain the amount of healthcare services. As such, it may lead to provision of inadequate prenatal care. Moreover, steering the enrollees to a limited set of health professionals by the HealthChoices can result in excessive demand for prenatal care for some network providers, which practically undermines the efforts to achieve “early and often” prenatal care provision. Put together, the expected net effect of the HealthChoices program on prenatal care utilization is not obvious.

the precisely estimated effects above retain significance (not shown). Third, migration of Medicaid eligible women across the mandatory HealthChoices and control counties can be related to mother- or family-level unobserved birth variant characteristics. As such, adding mother FE is not sufficient to deal with this type of selective migration. To address this concern, we assign women the Medicaid health delivery system (HealthChoices, Voluntary, or FFS) that they would have been exposed to had they stayed in the county where we initially observed them in the sample. Again, the estimated effects of the HealthChoices on prenatal care usage and birth outcomes are similar to Table 9 (the results available upon request).

Table 10 contrasts the prenatal care pathway with access to high-quality hospital care at the birth facility, where all the regressions control for the zone-specific time trends and use the smaller HealthChoices-Hospital sample. Consistent with Table 9, the HealthChoices is found to negatively influence prenatal care onset and newborn birth weight (columns 1 and 2). The estimated effect of this program on birth weight falls in absolute value by 3 percent, with the late care onset variable controlled for. Moreover, the direct effect of late care onset is -28 g for birth weight (column 3). Column (4) alternatively controls for NICU level 2 or above, hospital staffed beds>200, and the hospital fixed effects, which reduces the estimated HealthChoices impact by about 8 percent. In column (5), we instead add into the model NICU level 3 or above, staffed beds>400, plus the hospital fixed effects. Consequently, the coefficient estimate of the HealthChoices shrinks by 9 percent.[26] Altogether, the magnitude of the prenatal care pathway is about 33 to 38 percent as much as the one on access to high-quality hospital care at the birth facility, when the outcome of interest is birth weight.

The regressions for LBW and SGA do not yield significant impact estimates on the HealthChoices (not shown). However, column (6) shows switching from a FFS system to mandatory managed care significantly increases the risk of having preterm births for Medicaid eligible women by 1.66 percentage points. Comparing this with column (7) suggests that the elevated incidence of late care onset accounts for about 3 percent of the total effect of the HealthChoices on prematurity. Likewise, access to hospital care is responsible for about 8 to 11 percent of the overall HealthChoices impact, when we contrast column (6) to the last two columns which add the hospital-level controls at the birth facility. Put together, when

[26]We suppress the coefficient estimates on the hospital level variables for brevity.

investigating the HealthChoices effect on prematurity, we find suggestive evidence that the magnitude of the prenatal care channel is about 27 to 38 percent of access to hospital care.

[Insert Table 10 Here]

5. Conclusion

Recent economic research on child development have stressed the need for giving babies a good "start". Of all the in-utero health investments, use of prenatal care is regarded as an important and cost-efficient one. However, to date, the literature has not provided consistent evidence on the causal effects of prenatal care usage on infant health. The present study revisits this crucial issue using 1.4 million sibling births. Our within-mother estimates by the full sibling sample suggest a modest effect of prenatal care on the mean birth weight but large effects on adverse outcomes at the lower end of the birth weight distribution. Similar results hold in almost all the subgroups, whether women are stratified by race/ethnicity, education, or the number of births. Such effects on birth outcomes are also pronounced when we consider different dimensions of care utilization, explore the mechanisms, or use sibling data from another state.

We also exploit two quasi-experiments on care usage for the disadvantaged women. The first one is used to deal with the potential bias in the baseline within-mother estimates. The corresponding IV-Mother FE estimates suggest one more prenatal care visit at the mean increases newborn birth weight by 23-24 g and lowers the risk of prematurity by 1.3-1.5 percentage points. The second one allows us to investigate the effectiveness of prenatal care in a Medicaid manage care reform, where the empirical model uses mother FE to deal with selection into Medicaid by maternal unobservables. We find implementation of the HealthChoices does not alter the number of visits but increases late care onset by 9 percent. The higher risk of delayed care accounts for a small fraction (about 2-4 percent) of the total HealthChoices impact on birth weight, LBW, and prematurity. Nevertheless, there is suggestive evidence that the magnitude of this prenatal care channel is about 30-40 percent of the one on access to high-quality hospital care.

Future studies need to link the two quasi-experimental shocks on prenatal care usage to subsequent parental responsive investments and child development within the household. Two, more research remains to be done on other elements of newborn health capacity (head circumference, brain weight, etc.) which also matter for childhood cognitive and noncognitive skill formation. Three, we want to understand more about the demand and supply side causes for maternal inadequate prenatal care utilization. Overall, this study suggests usage of prenatal care makes a difference in newborn health stock formation. As an immediate policy implication, it is important to improve care access for childbearing women especially those with low socioeconomic status. Furthermore, caution is needed in design and delivery of managed care not to undermine provision of adequate prenatal care.

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Table 1. Descriptive statistics

	(1) Full sample (Num of mothers=611,107)		(2) Care onset in the first trimester		(3) Care onset in the second trimester		(4) Care onset beyond the second trimester	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Birth weight (grams)	3399.38	547.19	3418.60	539.82	3320.11	561.15	3209.82	611.69
Low birth weight	0.05	0.21	0.04	0.20	0.06	0.24	0.10	0.30
Small for gestation	0.10	0.30	0.09	0.29	0.13	0.34	0.15	0.36
Preterm birth	0.06	0.24	0.06	0.23	0.07	0.26	0.13	0.33
Care onset beyond the first trimester	0.16	0.36	0.00	0.00	1.00	0.00	1.00	0.00
Number of prenatal care visits	10.94	3.55	11.56	3.14	8.63	3.24	3.97	3.34
Inadequate care (by the Kessner Index)	0.27	0.44	0.14	0.34	1.00	0.00	1.00	0.00
Prenatal smoking	0.18	0.38	0.16	0.36	0.26	0.44	0.33	0.47
Inadequate gestational weight gain	0.11	0.31	0.10	0.30	0.15	0.35	0.18	0.38
Excessive gestational weight gain	0.27	0.44	0.27	0.44	0.26	0.44	0.33	0.47
Mother's age	27.45	5.68	27.93	5.49	24.91	5.91	24.61	5.97
Mother non-Hispanic White	0.85	0.36	0.88	0.33	0.70	0.46	0.57	0.49
Mother non-Hispanic Black	0.10	0.30	0.08	0.26	0.21	0.41	0.33	0.47
Mother Hispanic	0.04	0.19	0.03	0.17	0.07	0.26	0.08	0.26
Mother Asian	0.01	0.12	0.01	0.11	0.02	0.14	0.02	0.13
Mother education ≤12 years	0.49	0.50	0.45	0.50	0.70	0.46	0.74	0.44
Mother education=13-15 years	0.22	0.42	0.23	0.42	0.18	0.38	0.15	0.36
Mother education ≥16 years	0.28	0.45	0.31	0.46	0.11	0.31	0.08	0.27
Mother married	0.71	0.45	0.77	0.42	0.46	0.50	0.33	0.47
Father's age	29.99	6.18	30.36	6.02	27.98	6.64	28.12	6.77
Father non-Hispanic White	0.80	0.40	0.84	0.36	0.63	0.48	0.48	0.50
Father non-Hispanic Black	0.11	0.31	0.09	0.28	0.22	0.41	0.32	0.47
Father Hispanic	0.04	0.20	0.04	0.19	0.08	0.27	0.08	0.27
Father Asian	0.01	0.11	0.01	0.11	0.02	0.13	0.01	0.12
Father education (years)	13.30	2.40	13.52	2.34	12.15	2.41	12.02	2.43
Infant male	0.51	0.50	0.51	0.50	0.52	0.50	0.51	0.50
Infant birth order	2.02	1.06	1.96	0.97	2.25	1.32	2.58	1.64
Number of sibling births	1,421,593		1,198,251		174,966		48,376	

Notes: The full sample consists of all the mothers with two to five live births in the state of Pennsylvania in 1989-2010. Additional maternal control variables (not shown) include chronic hypertension and previous termination of pregnancy.

Table 2. Prenatal care and infant health: baseline results

	Birth Weight (Mean=3399.38)			LBW (Mean=0.05)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: care onset						
Care onset beyond the first trimester	-43.540***	-30.067***	-29.259***	0.012***	0.008***	0.008***
	(1.445)	(1.484)	(1.484)	(0.001)	(0.001)	(0.001)
Panel B: number of visits						
Number of prenatal care visits	23.492***	22.083***	22.085***	-0.008***	-0.008***	-0.008***
	(0.169)	(0.185)	(0.185)	(0.0001)	(0.0001)	(0.0001)
	SGA (Mean=0.1)			Preterm Birth (Mean=0.06)		
Panel C: care onset						
Care onset beyond the first trimester	0.017***	0.007***	0.007***	0.012***	0.009***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel D: number of visits						
Number of prenatal care visits	-0.002***	0.0001	0.0001	-0.011***	-0.011***	-0.011***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y
Mother fixed effects	N	Y	Y	N	Y	Y
Additional maternal controls	N	N	Y	N	N	Y
N (mothers)	611,107	611,107	611,107	611,107	611,107	611,107
N (sibling births)	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593

Notes: Each coefficient estimate comes from a separate regression. The sample includes all the mothers with two to five sibling births in PA from 1989-2010. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include race/ethnicity, age, education of the mother and father, where maternal race/ethnicity variables are dropped with mother fixed effects controlled for. The infant characteristics are infant gender and birth order. The additional maternal controls are marital status, pre-pregnancy hypertension, and previous termination of pregnancy. Robust standard errors clustered at the mother's level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 3. Prenatal care and infant health: care onset by trimester and care adequacy

	Birth Weight (Mean=3399.38)			LBW(Mean=0.05)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: care onset						
Care onset in the second trimester	-27.436***	-22.528***	-21.787***	0.006***	0.005***	0.005***
	(1.523)	(1.554)	(1.554)	(0.001)	(0.001)	(0.001)
Care onset beyond the second trimester	-106.879***	-66.101***	-64.999***	0.034***	0.022***	0.022***
	(3.005)	(3.019)	(3.017)	(0.001)	(0.002)	(0.002)
Panel B: care adequacy						
Inadequate care (by the Kessner index)	-64.808***	-50.074***	-49.636***	0.018***	0.015***	0.015***
	(1.148)	(1.193)	(1.193)	(0.0005)	(0.001)	(0.001)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y
Mother fixed effects	N	Y	Y	N	Y	Y
Additional maternal controls	N	N	Y	N	N	Y
N (mothers)	611,107	611,107	611,107	611,107	611,107	611,107
N (sibling births)	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593

Notes: Each coefficient estimate comes from a separate regression. The sample includes all the mothers with two to five sibling births in PA from 1989-2010. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include race/ethnicity, age, education of the mother and father, where maternal race/ethnicity variables are dropped with mother fixed effects controlled for. The infant characteristics are infant gender and birth order. The additional maternal controls are marital status, pre-pregnancy hypertension, and previous termination of pregnancy. Robust standard errors clustered at the mother's level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 4. Prenatal care and infant health: by race/ethnicity and by the number of births

	Birth Weight			Birth Weight			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Non-Hispanic White	Non-Hispanic Black	Hispanic	Mothers with 2 births	Mothers with 3 births	Mothers with 4 births	Mothers with 5 births
Panel A: care onset							
Care onset beyond the first trimester	-28.352***	-36.985***	-20.197***	-26.211***	-36.331***	-26.395***	-27.385***
	(1.693)	(3.841)	(5.723)	(2.058)	(2.619)	(4.329)	(7.163)
Panel B: number of visits							
Number of prenatal care visits	21.709***	26.452***	17.198***	22.930***	21.883***	20.069***	19.585***
	(0.205)	(0.540)	(0.753)	(0.248)	(0.329)	(0.576)	(1.055)
Mean (birth weight)	3435.42	3166.59	3269.20	3394.57	3408.38	3403.53	3414.72
	LBW			LBW			
	Non-Hispanic White	Non-Hispanic Black	Hispanic	Mothers with 2 births	Mothers with 3 births	Mothers with 4 births	Mothers with 5 births
Panel C: care onset							
Care onset beyond the first trimester	0.007***	0.014***	0.003	0.007***	0.008***	0.009***	0.010***
	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.004)
Panel D: number of visits							
Number of prenatal care visits	-0.008***	-0.012***	-0.007***	-0.008***	-0.008***	-0.007***	-0.007***
	(0.0001)	(0.0003)	(0.0004)	(0.0001)	(0.0002)	(0.0003)	(0.0006)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y	Y
Mean (LBW)	0.04	0.10	0.06	0.05	0.05	0.05	0.05
N (mothers)	518,197	59,992	17,446	450,963	126,817	27,419	5,908
N (sibling births)	1,203,756	143,432	53,558	901,926	380,451	109,676	29,540

Notes: Each coefficient estimate comes from a separate regression for a specific subsample, where the original full sample includes all the mothers with two to five sibling births in PA from 1989-2010. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. Robust standard errors clustered at the mother's level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 5. Prenatal care and infant health: mechanisms

	Birth Weight (Mean=3399.38)				LBW (Mean=0.05)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: care onset								
Care onset beyond the first trimester	-30.067***	-28.555***	-26.132***	-26.141***	0.008***	0.0077***	0.0068***	0.0068***
	(1.484)	(1.481)	(1.474)	(1.474)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: number of visits								
Number of prenatal care visits	22.083***	21.946***	21.403***	21.403***	-0.0078***	-0.0078***	-0.0076***	-0.0076***
	(0.185)	(0.185)	(0.183)	(0.183)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Prenatal smoking	N	Y	Y	Y	N	Y	Y	Y
Inadequate gestational weight gain	N	N	Y	Y	N	N	Y	Y
Excessive gestational weight gain	N	N	N	Y	N	N	N	Y
Maternal and paternal controls	Y	Y	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
N (mothers)	611,107	611,107	611,107	611,107	611,107	611,107	611,107	611,107
N (sibling births)	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593	1,421,593

Notes: Each coefficient estimate comes from a separate regression. The sample includes all the mothers with two to five sibling births in PA from 1989-2010. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. Robust standard errors clustered at the mother's level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 6. Allegheny bus strike and prenatal care: first stage estimates

	(1) Late care onset	(2) Number of Visits	(3) Number of Visits	(4) Number of Visits	(5) Number of Visits	(6) Number of Visits
Strike × Allegheny	0.001	-0.482***	-0.466***			
	(0.002)	(0.151)	(0.135)			
Strike × Allegheny × City				-0.565***	-0.553***	
				(0.149)	(0.131)	
Strike × Allegheny × Suburbs				-0.432***	-0.421***	
				(0.156)	(0.138)	
Strike × Allegheny × Black						-0.738***
						(0.148)
Strike × Allegheny × Non-Black						-0.382**
						(0.156)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y
Additional maternal controls	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y
F statistics for instruments	0.37	10.14	11.90	18.83	20.95	85.86
	[0.5433]	[0.0022]	[0.001]	[<0.0001]	[<0.0001]	[<0.0001]
Urbanized areas (counties with high population density)	Three counties	Three counties	Four counties	Three counties	Four counties	Three counties
Mean (outcome variables)	0.26	10.32	10.32	10.32	10.32	10.32
Mean (outcome variables, Allegheny women)	0.20	10.68	10.68	10.68	10.68	10.68
N (mothers)	99,442	99,442	99,442	99,442	99,442	99,442
N (sibling births)	215,371	215,371	215,371	215,371	215,371	215,371

Notes: Each coefficient estimate comes from a separate regression. The bus strike sample consists of all the mothers with no more than 12 years of education who had two to five sibling births in PA from 1989 to 1995. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. The additional maternal controls are marital status, pre-pregnancy hypertension, and previous termination of pregnancy. For the urbanized areas, the "three counties" are Allegheny, Philadelphia, and Montgomery counties while the "four counties" are Allegheny, Philadelphia, Montgomery, and Delaware counties (see footnote 13 for details). P-values for the F statistics are reported in square brackets. Robust standard errors clustered at the maternal residence county level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 7. Allegheny bus strike and infant health: reduced form estimates

	(1) Birth Weight	(2) Birth Weight	(3) LBW	(4) SGA	(5) Preterm Birth	(6) Preterm Birth
Strike × Allegheny	-15.094***	-14.883***	0.001	-0.004	0.009***	0.009***
	(3.789)	(3.212)	(0.002)	(0.008)	(0.003)	(0.003)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y
Additional maternal controls	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y
Urbanized areas (counties with high population density)	Three counties	Four counties	Three counties	Three counties	Three counties	Four counties
Mean (outcome variables)	3358.73	3358.73	0.06	0.13	0.07	0.07
Mean (outcome variables, Allegheny)	3319.71	3319.71	0.07	0.13	0.08	0.08
N (mothers)	99,442	99,442	99,442	99,442	99,442	99,442
N (sibling births)	215,371	215,371	215,371	215,371	215,371	215,371

Notes: Each coefficient estimate comes from a separate regression. The bus strike sample consists of all the mothers with no more than 12 years of education who had two to five sibling births in PA from 1989 to 1995. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. The additional maternal controls are marital status, pre-pregnancy hypertension, and previous termination of pregnancy. For the urbanized areas, the "three counties" are Allegheny, Philadelphia, and Montgomery counties while the "four counties" are Allegheny, Philadelphia, Montgomery, and Delaware counties. Robust standard errors clustered at the maternal residence county level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 8. Prenatal care and infant health: IV estimates by the Allegheny bus strike

	Birth Weight (Sample Mean=3358.73, Allegheny Mean=3319.708)							
	(1) 2SLS One IV	(2) 2SLS One IV	(3) 2SLS Two IVs (City and Suburbs)	(4) 2SLS Two IVs (City and Suburbs)	(5) 2SLS Two IVs (City and Suburbs)	(6) GMM Two IVs (City and Suburbs)	(7) 2SLS Two IVs (Black and Non-Black)	(8) GMM Two IVs (Black and Non-Black)
Number of care visits	31.303***	31.928***	23.481***	46.288***	24.813***	24.200***	23.107***	23.604***
	(5.228)	(5.412)	(4.368)	(2.641)	(3.902)	(4.262)	(4.203)	(4.155)
F statistics for instruments	10.14	11.90	18.83	23.58	20.95	18.83	85.86	85.86
	[0.002]	[0.001]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]
	Preterm Birth (Sample Mean=0.07, Allegheny Mean=0.08)							
Number of care visits	-0.020***	-0.018***	-0.015***	-0.031***	-0.014***	-0.015***	-0.014***	-0.013***
	(0.004)	(0.005)	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)	(0.004)
F statistics for instruments	10.14	11.90	18.83	23.58	20.95	18.83	85.86	85.86
	[0.002]	[0.001]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]	[<0.0001]
Maternal and paternal controls	Y	Y	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y	Y	Y
Additional maternal controls	Y	Y	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	N	Y	Y	Y	Y
Urbanized areas (counties with high population density)	Three counties	Four counties	Three counties	Three counties	Four counties	Three counties	Three counties	Three counties
N (mothers)	99,442	99,442	99,442	99,442	99,442	99,442	99,442	99,442
N (sibling births)	215,371	215,371	215,371	215,371	215,371	215,371	215,371	215,371

Notes: Each coefficient estimate comes from a separate regression. The bus strike sample consists of all the mothers with no more than 12 years of education who had two to five sibling births in PA from 1989 to 1995. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. The additional maternal controls are marital status, pre-pregnancy hypertension, and previous termination of pregnancy. For the urbanized areas, the "three counties" are Allegheny, Philadelphia, and Montgomery counties while the "four counties" are Allegheny, Philadelphia, Montgomery, and Delaware counties. P-values for the F statistics are reported in square brackets. Robust standard errors clustered at the maternal residence county level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 9. Medicaid managed care, prenatal care, and infant health: the case of the HealthChoices

	Regressions without Zone-Specific Time Trends								
	(1) Late Care Onset	(2) Number of Visits	(3) Birth Weight	(4) Birth Weight	(5) LBW	(6) LBW	(7) SGA	(8) Preterm Birth	(9) Preterm Birth
HealthChoices	0.049***	-0.139	-15.879*	-14.667*	0.0089**	0.0085*	-0.0054	0.0147**	0.0140**
	(0.012)	(0.133)	(8.284)	(8.365)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
Late care onset				-24.906***		0.0072***			0.0150***
				(4.185)		(0.003)			(0.003)
	Regressions with Zone-Specific Time Trends								
	Late Care Onset	Number of Visits	Birth Weight	Birth Weight	LBW	LBW	SGA	Preterm Birth	Preterm Birth
HealthChoices	0.028**	0.036	-16.957*	-16.194*	0.0092*	0.0090*	-0.0021	0.0183***	0.0179***
	(0.011)	(0.114)	(9.259)	(9.312)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)
Late care onset				-27.279***		0.0074***			0.0157***
				(4.203)		(0.002)			(0.003)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mean (outcome variables)	0.30	10.21	3223.27	3223.27	0.09	0.09	0.16	0.09	0.09
N (mothers)	36,938	36,938	36,938	36,938	36,938	36,938	36,938	36,938	36,938
N (sibling births)	81,588	81,588	81,588	81,588	81,588	81,588	81,588	81,588	81,588

Notes: Each coefficient estimate comes from a separate regression. The HealthChoices sample consists of all the native-born unmarried mothers with no more than 12 years of education who had two to five sibling births in PA from 1994 to 2004. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. Robust standard errors clustered at the maternal residence county level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Table 10. Pathways of the HealthChoices: prenatal care utilization and access to hospital care

	(1) Late Care Onset	(2) Birth Weight	(3) Birth Weight	(4) Birth Weight	(5) Birth Weight	(6) Preterm Birth	(7) Preterm Birth	(8) Preterm Birth	(9) Preterm Birth
HealthChoices	0.028***	-25.256**	-24.449**	-23.287**	-22.983**	0.0166**	0.0162**	0.0148**	0.0153**
	(0.009)	(11.744)	(11.900)	(11.209)	(11.396)	(0.0073)	(0.0072)	(0.0069)	(0.0068)
Late care onset			-28.399***				0.0145***		
			(4.824)				(0.0034)		
Maternal and paternal controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zone-specific time trends	Y	Y	Y	Y	Y	Y	Y	Y	Y
Hospital staffed beds > 200	N	N	N	Y	N	N	N	Y	N
Hospital staffed beds > 400	N	N	N	N	Y	N	N	N	Y
NICU level 2 or above	N	N	N	Y	N	N	N	Y	N
NICU level 3 or above	N	N	N	N	Y	N	N	N	Y
Hospital fixed effects	N	N	N	Y	Y	N	N	Y	Y
Mean (outcome variables)	0.30	3221.21	3221.21	3221.21	3221.21	0.09	0.09	0.09	0.09
N (mothers)	30,836	30,836	30,836	30,836	30,836	30,836	30,836	30,836	30,836
N (sibling births)	67,243	67,243	67,243	67,243	67,243	67,243	67,243	67,243	67,243

Notes: Each coefficient estimate comes from a separate regression. The HealthChoices-Hospital sample consists of all the native-born unmarried mothers with no more than 12 years of education who had two to five sibling births in PA from 1994 to 2002. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. Robust standard errors clustered at the maternal residence county level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Appendix A. Prenatal care and infant health: WA estimates

	Birth Weight			LBW		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: care onset						
Care onset beyond the first trimester	-32.057***	-31.744***	-31.769***	0.008**	0.008**	0.008**
	(6.995)	(6.990)	(6.994)	(0.003)	(0.003)	(0.003)
Panel B: number of visits						
Number of prenatal care visits	18.322***	18.285***	18.314***	-0.005***	-0.005***	-0.005***
	(1.204)	(1.204)	(1.204)	(0.001)	(0.001)	(0.001)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y
Additional maternal controls	N	Y	Y	N	Y	Y
Maternal employment status	N	N	Y	N	N	Y
Mean (outcome variables)	3438.73	3438.73	3438.73	0.04	0.04	0.04
N (mothers)	24,607	24,607	24,607	24,607	24,607	24,607
N (sibling births)	50,083	50,083	50,083	50,083	50,083	50,083

Notes: Each coefficient estimate comes from a separate regression. The WA sample consists of all the mothers with two to four births in 2003-2006. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. The additional maternal controls are marital status, pre-pregnancy hypertension, and previous termination of pregnancy. Robust standard errors clustered at the mother's level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.

Appendix B. The HealthChoices and infant health: alternative control counties

	The Control: FFS Counties								
	(1) Late Care Onset	(2) Number of Visits	(3) Birth Weight	(4) Birth Weight	(5) LBW	(6) LBW	(7) SGA	(8) Preterm Birth	(9) Preterm Birth
HealthChoices	0.032***	0.041	-16.940*	-16.037*	0.0091*	0.0089*	-0.0047	0.0181**	0.0175**
	(0.011)	(0.117)	(9.404)	(9.470)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)
Late care onset				-28.582***		0.0086***			0.0165***
				(4.634)		(0.003)			(0.004)
Mean (outcome variables)	0.32	9.96	3215.05	3215.05	0.09	0.09	0.16	0.10	0.10
N (sibling births)	64,163	64,163	64,163	64,163	64,163	64,163	64,163	64,163	64,163
	The Control: Voluntary Counties								
	Late Care Onset	Number of Visits	Birth Weight	Birth Weight	LBW	LBW	SGA	Preterm Birth	Preterm Birth
HealthChoices	0.028**	0.041	-18.785**	-17.950*	0.0099*	0.0096*	-0.0064	0.0186***	0.0181***
	(0.011)	(0.118)	(9.187)	(9.251)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)
Late care onset				-29.395***		0.0079***			0.0165***
				(4.222)		(0.003)			(0.003)
Maternal and paternal controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Infant characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mother fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zone-specific time trend	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mean (outcome variables)	0.31	10.13	3219.76	3219.76	0.09	0.09	0.16	0.09	0.09
N (sibling births)	75,484	75,484	75,484	75,484	75,484	75,484	75,484	75,484	75,484

Notes: Each coefficient estimate comes from a separate regression. The upper panel use a sample of all the native-born unmarried mothers with no more than 12 years of education who had two to five sibling births from 1994 to 2004 in the mandatory HealthChoices counties and FFS counties; the lower panel uses a sample of mothers of the same characteristics in the mandatory HealthChoices counties and Voluntary counties. All the regressions also control for infant birth year and month fixed effects and maternal county of residence fixed effects. The maternal and paternal controls include mother's age and education, father's race/ethnicity, age and education. The infant characteristics are infant gender and birth order. Robust standard errors clustered at the maternal residence county level are reported in parentheses. *Significant at 10% level. ** Significant at 5% level. *** Significant at 1% level.