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The impact of mothers' earnings on health inputs and infant health[☆]

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ABSTRACT

This paper investigates the impact of mothers' earnings on birth weight and gestational age of infants in the U.S. It also analyzes the impact of earnings on mothers' consumption of prenatal medical care, and their propensity to smoke and drink during pregnancy. The paper uses census division-year-specific skill-biased technology shocks as an instrument for mothers' earnings and employs a two-sample instrumental variables strategy. About 14 million records of births between 1989 and 2004 are used from the Natality Detail files along with the CPS Annual Demographic Files from the same period. The results reveal that an increase in weekly earnings prompts an increase in prenatal care of low-skill mothers (those who have at most a high school degree) who are not likely to be on Medicaid, and that earnings have a small positive impact on birth weight and gestational age of the newborns of these mothers. Specifically, if a mother's earnings double, this produces a weight gain of the newborn by about 100 g and an increase in gestational age by 0.7 weeks. An increase in earnings does not influence the health of newborns of high-skill mothers (those with at least some college education). Variations in earnings have no impact on birth weight for mothers who are likely to be on Medicaid.

1. Introduction

Child health is an important ingredient in human capital formation, and poor health at birth impacts adult outcomes. For example, low birth weight reduces educational attainment (Case et al., 2005; Currie and Hyson, 1999). Low birth weight also has a negative impact on labor market outcomes (Black et al., 2007; Currie and Hyson, 1999) and on health in adulthood (Behrman and Rosenzweig, 2004).

The seminal work of Grossman (1972) provides the theoretical framework of a human capital model through which the production of health can be analyzed. In this model individuals' health capital depreciates over time and gross investment in health can be produced by a household production function that uses the person's own time, and health inputs such as medical care and healthy diet. Health inputs may include those with negative marginal products such as cigarette and alcohol consumption.[1] The initial health endowment is an important determinant of the future stock of health. This endowment is not only determined by genetics, but it can be impacted by in utero exposure to disease, and detrimental environmental factors such as air pollution (Almond, 2006; Currie and Walker, 2011).

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[1] As described in Almond and Currie (2011), different approaches to health production exist; e.g. Heckman (2007).

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In this context it is important to investigate, both from a scientific and public policy perspective, the extent to which an increase in maternal income during pregnancy impacts infant health. The issue, however, is complicated because of the endogeneity of income. For example, in the analysis of the impact of mothers' income on birth outcomes, it is difficult to find exogenous variations in income that could help identify the causal impact of income on birth weight. Consequently, one line of research has focused on aggregate units such as the rate of low birth weight infants at the state level, and analyzed how this aggregate is impacted by state unemployment rates. For example, Dehejia and Lleras-Muney (2004) found that higher unemployment rates were associated with *improved* health outcomes of infants as measured by the rate of low birth weight. This result is consistent with the findings of Ruhm (2000), who reported that health behaviors improved during bad economic times, leading to better health outcomes.[2]

Birth weight is a key birth outcome, and there are two channels through which pregnant women's earnings may affect birth weight of their newborn. First, if child health is a normal good, then an increase in income increases the derived demand for health inputs. For example, pregnant women may increase the consumption of prenatal care, and they may initiate prenatal medical care earlier during the pregnancy. In this case, increases in prenatal care consumption will lead to increases in birth weight. On the other hand, prenatal care is a time intensive activity and an increase in the opportunity cost of time may result in mothers seeking less prenatal care. Dehejia and Lleras-Muney (2004) show that the average number of prenatal care visits by pregnant women *increases* during times of *high* unemployment and they argue that the decline in the opportunity cost of time during recessions (when incomes go down) is the underlying reason for this decline. They report that a one percentage point *increase* in the unemployment rate results in a 0.26–0.5% *reduction* in the low birth weight rate, and they attribute the improvement of birth outcomes to the implied increase of prenatal care consumption during recessions. However, as pointed out by Lindo (2011), Dehejia and Lleras-Muney (2004) are not able to isolate the impact of income on infant health from the impact of other factors that are associated with periods of high unemployment.

Almond et al., 2011 explained the county-level average birth weight as a function of the introduction of the Food Stamp Program (FSP) in the 1960s. Exploiting the fact that the FSP became operational in different counties in different time periods, they find that FSP had a positive impact on birth weight, with larger impacts among African American mothers. Although food stamps can be used only to purchase certain food items, Hoynes and Schanzenbach (2009) report that the food stamp recipients behave as if the benefits were paid in cash, suggesting that the receipt of food stamps is equivalent to an income transfer. On the other hand, Hoynes and Schanzenbach (2012) find that the food stamp program leads to reductions in employment and hours worked, especially among families headed by single women. They show that the impact on the treated is 500–600 fewer hours of work per year. This suggests that the increase in disposable income due to the food stamp receipt is counterbalanced to some extent by a decline in labor supply triggered by the food stamp program, and therefore the net effect on household income may not be substantial.

Hoynes et al., 2015 use changes in the Earned Income Tax Credit (EITC) policy to identify exogenous changes in income. They use birth certificate data collapsed into cells defined by state, month, parity of birth, education, marital status, race, and age of the mother to identify the amount of EITC for which the family is eligible. Using a difference in difference specification to capture the effect of an expansion of the EITC in 1993, the authors conclude that increases in EITC income resulted in a lower incidence of low birth weight as well as an increase in mean birth weight.

An alternative strategy is to investigate the impact of income on infant health using micro data, and to find arguably exogenous variations in income. One such example is Lindo (2011), who used the job loss of a husband in the past as an exogenous shock to household income. Using data from the Panel Study of Income Dynamics and controlling for individual fixed effects, the paper found that a husband's job loss in the past has a strong negative effect on infant health, reducing birth weight by about 4.5%. Although this is an interesting result, the magnitude of the decline in income due to job loss is unknown, so is the extent to which job loss is correlated with stress in the household, which can also have a detrimental effect on birth outcomes. Along the same lines, Chung et al. (2015) used payouts of dividends from the Alaska Permanent Fund during the 1980s as a source of exogenous variation in family income and found a very small positive effect of family income on birth weight. The magnitude of the estimated effect was only about 18 g of additional birth weight per $2331 additional income in 2011 dollars.

In summary, to get around endogeneity of income, most studies analyzed aggregate indicators of infant health (e.g. county-level low birth weight rates) and tied them to proxies of aggregate income (such as unemployment rates or expansions of the EITC program). There are only a handful of micro-level studies that aim to analyze the impact of personal income on birth weight. This is because of two reasons. First, birth certificates do not contain information on income. Second, data sets that have information on both birth weight and family or personal income are limited in sample size, and more importantly, it is difficult to find exogenous variation in personal income that is not related to birth weight.

In this paper we employ data from the United States Detail Natality files for the period of 1989–2004 and use information on about 14 million births to unmarried mothers to estimate the causal impact of mothers' earnings at the time of conception on the birth weight of the newborns using an instrumental-variables strategy.[3] We focus on unmarried mothers because, as explained below, our instrument is conceptually less relevant for married women.

[2] Although Ruhm (2015) points out the sensitivity of the findings regarding countercylicality of good health outcomes.

[3] We use birth certificates for births that occurred between 1989 and 2004. Depending on the birth month, this means that conception will have occurred between 1988 and 2004. See Section 3 for details.

A contribution of the paper is the introduction of a novel instrument to identify the impact of income on birth weight. Earnings of pregnant women may be correlated with their unobserved attributes that may also impact birth outcomes. Thus we use a well-defined measure of skill-biased technology as an instrument for earnings. Because earnings information is not available on birth certificates, we use micro data from the CPS for the same time period to estimate first-stage earnings equations. The reduced form equations are based on birth certificates where birth weight of the newborn depends on exogenous mother characteristics and the skill-biased technology parameter determined at the census division level by year. This two-sample instrumental variables design enables us to recover the structural estimate of the impact of mothers' earnings on birth weight and gestational age of their newborn.

We also created the instrument at the state-level (as opposed to the census division-level), by considering all states in a state's census division except for the state itself. This "leave own-state out" instrument provided similar results, displayed in the Appendix.

Another contribution of the paper is to estimate input demand functions within the same coherent empirical framework. Specifically, we estimate input demands for smoking, drinking, and consumption of prenatal medical care using data provided by birth certificates. Together, the results reveal insights into not only the impact of income on birth weight, but also on the pathways through which the impact of income operates. For example, we find that in case of low-skilled pregnant women (those with education levels of high school or less) who are unlikely to be covered by Medicaid, the increase in income produces an increase in prenatal care consumption, which results in a small improvement in birth weight and gestational age. A different result is obtained for high-skilled pregnant women. The demand for prenatal care is not sensitive to income for high-skilled women, and the effect of income on birth weight in the sample of high-skill women is zero.

The impact we identify in the paper represents the net influence of earnings on infant health and health inputs. Earnings are determined by both hours of work and wages, but our goal is not to separately identify the relative importance of wages or hours of work.[4]

[4] The spirit of the interpretation of our estimate is similar to those obtained from many similar papers, such as Almond et al. (2011) who find that food stamp expansions reduced the incidence of low birth weight. The particular mechanism through which food stamps influence birth weight remains unknown. Potential channels include food expenditures and labor supply reactions. Although the authors are not able to disentangle the particular mechanism through which food stamps impact birth weight, the reduced form impact is still provides valuable information. Another example is the impact of education on health outcomes. Because education is endogenous to health production, any health outcome regression should employ an instrument or some exogenous variation in education. An unbiased estimate of education, so obtained, reveals however (in most cases) the net impact of education on health. This is because although an increase in education can have a direct impact of health production (productive efficiency), it can also impact health through other changes such as input re-allocation (allocative efficiency), the change in time preference, or income. While it is very challenging to identify the particular channel through which education impacts health, the question of whether there is a causal net impact of education on health is an important one.

The rest of the paper is structured as follows. In the next section we describe the empirical framework and introduce the instrument. Section 3 describes the data, and Section 4 presents the results. Section 5 consists of the conclusion and discussion.

2. Theoretical framework and empirical strategy

Following the standard framework of a birth weight production function as outlined in Grossman and Joyce (1990), Corman et al. (1987), and Corman and Grossman (1985), we assume that parents' utility function depends on consumption, the number of births, and the birth outcome. Maximization of this function subject to production and budget constraints generates the demand for birth outcome; and the production function of birth outcome determines the demand for inputs such as medical care. The birth weight production function can be depicted as

$$b = f(m, a, z) \tag{1}$$

where m is the use of prenatal care, a is the use of contraceptive and abortion services, and z represents maternal risk factors and productive efficiency of the mother (Altindag et al., 2011; Grossman, 2000, 2006). Input demand functions obtained in this framework are given by Eqs. (2) and (3)

$$m = g_1(p, y, z) \tag{2}$$

$$a = g_2(p, y, z) \tag{3}$$

where p is the vector of prices and availability and y represents income. Substitution of (2) and (3) into (1) yields

$$b = h(p, y, z) \tag{4}$$

Eq. (4) is the reduced form demand function for the birth outcome, where birth outcome b depends on prices, income and maternal risk factors. We estimate (4) to identify the impact of income on infant health at the mother level. We also estimate (2) to identify the role played by income in inputs demand functions. The input demand functions are also reduced form equations because they are obtained by maximizing a utility function subject to production and resource constraints (Corman and Grossman, 1985).

Although (4) is a reduced form, its estimation is complicated using micro data (birth certificates) for two reasons. First, the birth certificate data do not contain information on mother's income (y). Second, even if income information were available on birth certificates, mother's income (or family income) in Eq. (4) is endogenous if more productive mothers with higher incomes have better health outcomes due to unobservable productivity. Therefore we develop an instrument for y to employ in Eqs. (4) and (2). The details of the instrument are described below.

An analysis of the impact of income on birth weight necessitates linking birth weight information of the baby to the income information of the mother or the family. Birth certificates provide the largest and most reliable data on birth weight, but they do not contain information on income. Thus, we employ data from two different sources and use a two-sample instrumental variables strategy. This strategy allows us to estimate the structural parameters of interest. We use income data for women of child bearing age from the Current Population Survey (CPS) for the years 1989–2005, covering earnings for the years 1988–2004 to estimate the following first stage regression.

$$\mathit{Ln\,Earnings}_{is}^{t} = \beta_1(\text{Skill biased tech change})_c^t + \beta_2 X_{is}^t + \epsilon_{is}^t, \quad (5)$$

where $\mathit{Earnings}_{is}^{t}$ represents real weekly earnings of woman (i) in state (s) in year (t), and X_{is}^t stands for a vector of individual level characteristics. It also includes state fixed effects, year fixed-effects and state-specific time trends. Note that Eq. (5) does *not* represent a panel data structure. Instead, it depicts the models to be estimated based on repeated cross sections using the CPS data, and the superscript (t) indicates the year of the CPS survey.

As will be detailed below, in Eq. (5) skill-biased technological change in year t and census division c negatively affects earnings of unskilled women (women with high school education or less). On the other hand, earnings go up in response to skill-biased technological change for skilled women (women with at least some college education). This means that in the sample of unskilled (skilled) mothers, β_1 is expected to be negative (positive). X includes race indicators and the age of the woman. Women in the low-skill samples have at most a high school education. Therefore in these regressions we include a dummy variable to control for whether the woman has a high school degree. The regressions using the sample of high-skilled women include an indicator to control for the receipt of a college diploma.

Both low-skill and high-skill samples include only unmarried women. The reason for focusing on unmarried women is because the validity of the instrument can be in question in case of married women. More specifically, the instrument has an impact on women's earnings but it would also influence the earnings of the husbands if husbands are working. This means that in the outcome equation (e.g. the birth weight equation) where married women's earnings are used as an endogenous explanatory variable, the instrument would have a direct impact on the outcome. This is because the error term of the outcome equation would contain husbands' earnings and thus the instrument would be correlated with the error term.[5]

The second data set pertains to almost 14 million birth certificates in the United States for the years 1989–2004, covering conceptions for the years 1988–2004. These data are employed to estimate the reduced form Eq. (6)

$$\mathit{Outcome}_{is}^{t} = \alpha_1(\text{Skill biased tech change})_c^t + \alpha_2 X_{is}^t + \eta_{is}^t, \quad (6)$$

where $\mathit{Outcome}_{is}^{t}$ represents various outcomes such as the birth weight of the child, an indicator variable if the newborn is of low birth weight (less than 2500 g), gestational age, the extent of prenatal care consumption during pregnancy, measures of late initiation of prenatal care, and indicators of smoking and drinking behavior of mother i who gave birth in state s during year t. Note that the vector of explanatory variables X is identical in both the reduced form and first stage Eqs. (5) and (6) for the two-sample instrumental variable strategy to be viable (Inoue and Solon, 2010).

Taking the ratio of the coefficient of the instrument from the reduced form estimates using birth certificate data (α_1 in Eq. (6)), and the coefficient of the instrument from the first stage estimation using CPS data (β_1 in Eq. (5)) provides the two-sample instrumental variables estimate of the impact of earnings on birth outcomes.[6] That is, we calculate $\gamma = \alpha_1/\beta_1$. We use the delta method to calculate the standard error of the estimate of γ (Inoue and Solon, 2010; Dee and Evans, 2003). Specifically, assuming that the covariance of β_1 and α_1 is zero, the variance of the estimated two-sample IV coefficient is

$$\begin{aligned}\operatorname{var}(\gamma) &= \left(\frac{\partial\gamma}{\partial\beta_1}\right)^2 \operatorname{var}(\beta_1) + \left(\frac{\partial\gamma}{\partial\alpha_1}\right)^2 \operatorname{var}(\alpha_1)\\ &= \left(-\frac{\alpha_1}{\beta_1^2}\right)^2 \operatorname{var}(\beta_1) + \left(\frac{1}{\beta_1}\right)^2 \operatorname{var}(\alpha_1)\\ &= \left(\frac{1}{\beta_1}\right)^2\left[\operatorname{var}(\alpha_1) + \frac{\alpha_1^2}{\beta_1^2}\operatorname{var}(\beta_1)\right].\end{aligned}$$

Our analysis investigates the impact of mothers' earnings on infant health. Earnings, of course, are determined by the interplay between labor force participation, hours of work and the wage rate. Our goal is not to determine the relative contributions of these factors. Rather, our aim is to examine the net effect of earnings. Because we exclude part-time workers and focus on those who work full-time (35 hours or more per week), much of the variation in earnings plausibly comes from variation in wages. Of course, workers adjust their work hours in response to changes in wages. This means that, as mentioned above, we investigate the reduced form, or net, effect of earning on infant health.

[5] The Census Bureau publishes statistics about child support that is due to be paid to custodial parents. In 2003 there were about 10.7 million unmarried parents living with their own children under the age of 21 whose other parent was not living in the home. About half of these parents had some child support that was due to be paid to them by the other parent. Out of those individuals who were due to be paid some child support, only 44% actually received all payments that were due, while the remainder either received partial child support payments, or none at all. Non-payment of child support varies by education of the parent. For example, 28% of low-skill parents (those with a high school diploma or less education) who are due to receive some child support actually received no payments at all. For high skill parents (those with at least some college education) this statistic is lower: 19% of parents with child support due actually received nothing.

[6] The two-sample instrumental variables approach was pioneered by Angrist and Krueger (1992), who used the two stage instrumental variables estimator to estimate the effect of age at school entry on educational attainment. Other applications of this estimator can be found in Lindo and Stoecker (2010) who investigated the criminal propensity of Vietnam veterans, Dee and Evans (2003) who examined the impact of drinking on the educational attainment of teenagers, as well as Currie and Yelowitz (2000) who analyzed the impact of housing project on the welfare of children. For a succinct technical discussion of the estimator see Inoue and Solon (2010).

To put this more concretely using an example, consider Almond et al., 2011 who find that food stamp expansions reduce the incidence of low birth weight. In a separate paper Hoynes and Schanzenbach (2009) show that the same food stamp program reduces out-of-pocket food spending of consumers, and these authors show in a different paper that the food stamp program had an impact on employment and hours worked (Hoynes and Schanzenbach, 2012). These three papers together reveal that the food stamp program had an impact on child health, but that the program influenced labor supply and unearned income as well. Mothers work less and consume more food after the receipt of food stamps. More food consumption would improve birth weight, and not working and avoiding stress may also improve birth weight. We do not know through which channel low birth weight was impacted, but it was impacted nevertheless. Our design is similar in that it does not allow us to investigate whether earnings go up because wages rise holding labor supply constant, or earnings go up despite the reduction in labor supply. Even though we cannot disentangle these mechanisms, it is important to analyze whether or not there is a net effect of earnings.

2.1. The instrument

We use a measure of census-division- and year-specific skill-biased technological change as an instrument for mothers' earnings. Let aggregate output, Y_{ct}, produced in a *census division c* during year *t* be described by the following CES production function

$$Y_{ct} = \left[(A_{H_{ct}}H_{ct})^{\frac{\sigma-1}{\sigma}} + (A_{L_{ct}}L_{ct})^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}},$$

where H and L stand for efficiency-adjusted high-skill and low-skill labor inputs, respectively. A_H and A_L are factor-augmenting technology terms. The parameter σ is the elasticity of substitution between low-skilled and high-skilled labor and based on previous work, it is assumed to be greater than one. Following Autor et al. (2008) we set $\sigma = 1.6$.

Assuming competitive factor markets, the first order conditions result in the following relationship between the relative wage of skilled and unskilled workers, $W_{H_{ct}}/W_{L_{ct}}$, and the relative supply of skills, H_{ct}/L_{ct}:

$$\frac{W_{H_{ct}}}{W_{L_{ct}}} = \left(\frac{A_{H_{ct}}}{A_{L_{ct}}}\right)^{\frac{\sigma-1}{\sigma}}\left(\frac{H_{ct}}{L_{ct}}\right)^{-\frac{1}{\sigma}},$$

where W_H and W_L represent efficiency-adjusted wages of skilled and unskilled labor, respectively. Using data on wages and labor supply of both low-skilled and high-skilled labor from the CPS, we back out the value of $A_{H_{ct}}/A_{L_{ct}}$. Following Autor et al. (1998), Autor et al. (2008), and Goldin and Katz (2007), we use $\ln(A_{H_{ct}}/A_{L_{ct}})$ as an index for skill-biased technological change. We employ this index of skill-biased technological change as an instrument for mothers' earnings.

We use the nine census divisions (New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain and Pacific) as the relevant geographic units in which technology is determined. This means that we aggregate H, L and W to the census division-level in each year and obtain the skill-biased technological change index $\ln(A_{H_{ct}}/A_{L_{ct}})$ by census division and year. The skill-biased technology index, of course, can be calculated at the state-level as well. The reason to obtain the index at the census division-level, rather than the state level, is that it may be argued that technological change at the state level could be influenced by states' labor market conditions, making it potentially endogenous. Calculating the skill-biased technology index at the census division level circumvents this potential complication. Put differently, in our framework, it is assumed that technological change is determined at the regional level and that the states which make up a particular region are exposed to the technology shocks of that region.

We, however, also calculated the instrument at the state-level (as opposed to the census division-level), by considering all states in a state's census division except for the state itself. This alternative version, "leave own-state out" instrument, provided similar results as displayed in Appendix Tables A1 and A2.

Although a change in $\ln(A_{H_{ct}}/A_{L_{ct}})$ can arise for a number of reasons, ranging from variations in the relative prices of non-labor inputs to the evolution of labor market institutions, the consensus in the literature is that the primary driver of $\ln(A_H/A_L)$ is skill-biased technological change (Autor et al., 2008; Goldin and Katz, 2007). A related point is whether skill-biased technological change and the resultant change in the relative demand for skilled workers would induce a policy reaction, which would render our instrument invalid. For example, if state governments increase minimum wages in reaction to a change in technology favoring skilled workers, the instrument would be invalid to the extent that the minimum wage has a direct impact on infant health. However, the scenario that states increase the level of minimum wages in response to technology shocks does not seem realistic because minimum wages are not adjusted frequently. Mocan and Unel (2011) provide detailed evidence on the validity of this instrument. The construction of the instrument using the CPS data is explained in the Appendix.

3. Data

We use individual-level data from two sources. First, we use birth certificates of the universe of births in the United States for the years 1989–2004, obtained from the Natality Detail Files of the National Center for Health Statistics. The birth certificate data contain a record for each child born in the United States, and each record includes information regarding the child's birth weight, as well as demographic characteristics of the mother, such as age, education, race, and marital status. In addition, information regarding the mother's use of prenatal care and the mother's smoking and drinking behavior are available. We use only birth certificates for singleton births to unmarried mothers who are at least 20 years old. We use data only until 2004 because geographic identifiers are not available in the public use data starting in 2005. Geographic identifiers—in particular the state of residence of mothers—are essential in order to be able to match our measure of skill-biased technology shocks described above with individual mothers in the data set. We use data starting with the 1989 birth certificates because some states did not report important demographic information on their certificates prior to that year. Specifically, California and Texas did not report mothers' education until 1989. Education is a crucial variable for our analysis, and to avoid excluding populous states of California and Texas we start the analysis using 1989 birth certificates.

We use the birth weight of the infants (recorded in grams) from the birth certificates as a measure of infant health. A second measure is an indicator variable that takes the value of one if the infant is low birth weight (less that 2500 g). We also use gestational age as an indicator of the newborn's health because gestational age is correlated with birth weight. Since one potential mechanism for how income can affect infant health is prenatal care, we make use of several measures of prenatal care consumption contained in the birth certificates. There are two particular variables of interest: the number of prenatal care visits attended by the mother, and the month of the pregnancy during which prenatal care was initiated. The number of prenatal care visits is the actual number of times that the mother visited a physician for prenatal consultations. The month of the pregnancy when the first prenatal care visit took place is an indication for how early the mother sought prenatal care.[7] Since it is important that prenatal care starts early in the pregnancy we also create a dummy variable that indicates whether prenatal care was initiated late. We consider prenatal care to have commenced late if the first prenatal care visits occurred after the first trimester of the pregnancy, i.e. if prenatal care was initiated in the fourth month of the pregnancy or thereafter.

Smoking behavior is recorded using a dummy variable indicating whether the mother smoked during the pregnancy, and drinking behavior is similarly captured using a dummy variable indicating whether the mother consumed alcohol during the pregnancy. While most states started reporting smoking and drinking information in 1989, some states started reporting this information later. Louisiana and Nebraska started reporting smoking and drinking behavior information in 1990, Oklahoma started in 1991, New York in 1995, and Indiana in 1999. California and South Dakota never reported data on smoking or drinking.[8]

After restricting the birth certificate data to records of only singleton births to unmarried mothers aged 20–49 where demographic information of the mother is available, we end up with a data set of 13,756,856 birth certificates.[9] We merge these birth certificates with the measure of skill-biased technological change described in Section 2 using the year of conception and the census division of residence of the mother.

In our two-sample design, we combine information from the birth certificates regarding the circumstances of each birth with information regarding mothers' earnings. We obtain individual-level earnings data from the Current Population Survey's Annual Demographic File. Since the birth certificate data are for mothers of age 20–49, we only use the earnings for females who are between 20 and 49 years old in the CPS data. The CPS contains an income measure indicating annual personal income from wages and salaries for the calendar year prior to the survey. We construct the measure of real weekly earnings by dividing the real value (in 2005 Dollars) of annual personal income from wages and salaries in the previous calendar year by the number of weeks worked in the previous calendar year.[10] For women in the CPS sample, the year during which the reported income was earned is therefore the year prior to the CPS survey year.

In order to match the conception date of infants with the date when personal income was earned by the mother, we subtract nine months from the date of birth of infants from the birth certificates and match the resulting year with the year during which income was earned by women in the CPS sample.[11] For example, if a child was born in November of 2000, then conception occurred in February of 2000. Real weekly earnings during February of 2000 are then obtained from the 2001 survey year of the CPS sample.

After restricting the CPS sample to women between the ages of 20 and 49, and dropping observations with missing demographic information, the resulting data set contains 124,491 observations. We merge these observations with the measure of skill-biased technological change described in Section 2 using the year during which income was earned and the census-division of residence of the woman.

We also perform analyses by Medicaid receipt of the mother. We can directly identify Medicaid recipients in the CPS data but Medicaid status is not observed on birth certificates. Therefore, we use parity as a proxy when using birth certificate data. First-born children are less likely to be Medicaid eligible than parity of two or greater. This is due to the fact that the income threshold for Medicaid eligibility is a function of family size, and the larger the family, the lower the income threshold for Medicaid eligibility.

As described earlier, we use the census division of the state of residence of the mother to match skill-biased technology shocks with births. There are some births in the data that occurred in a state other than the state of residence of the mother. For example, while a mother may be a resident of California, she may have given birth in Illinois. In this case, the birth certificate would be reported by the state of Illinois and will include all items reported by

[7] If no prenatal care took place, then the month of the first prenatal care visit is coded to be equal to 10.

[8] Beginning in 2003, some states adopted a revised version of the standard birth certificate that changed the way in which smoking behavior of the mother is recorded. Specifically, the revised version contains smoking participation information separately for each trimester of the pregnancy. In those cases we recode data on the different trimesters to be consistent with the measure of smoking used during the other years.

[9] We drop women whose marital status was imputed.

[10] We exclude women who were employed less than full time in the previous calendar year, and also exclude women who are self-employed.

[11] In our main results we assume that a pregnancy lasts nine months because gestational length is often missing on the birth certificates, and it is measured with error.

the state of Illinois. Some items reported by Illinois may, however, not be reported by the state of California. For example, smoking information is never reported by California, but is included in the Illinois birth certificates starting in 1989. We exclude such cases from our analyses. In order to check whether women who give birth in states other than their state of residence significantly influence the results, we also estimated specifications that include mothers who gave birth in a state different from the state where they reside. The results did not change.

Since our empirical strategy described above relies on estimating the impact of earnings on mothers' behavior and the impact of earnings on health outcomes of newborns separately for low-skill mothers and high-skill mothers, we present summary statistics separately for low-skill mothers (Tables 1 and 3) and for high-skill mothers (Tables 2 and 4). We assign the skill level of women by using information about their educational attainment. We classify women as being low-skilled if they have at most a high school diploma. Women are considered high-skilled if they have at least some college education. This information is available both on birth certificates and in the CPS data.

Table 1 presents descriptive statistics of the variables obtained from the birth certificate data pertaining to low-skilled women, and Table 2 displays the same information for high-skilled women. Tables 3 and 4 present the descriptive statistics of the CPS data related to these two groups, respectively. The instrument (the measure of skill-biased technology change described above) will be used both in the reduced-form regression using the birth certificates and also in the first-stage regression using the income data from the CPS. Thus the instrument is merged with both the birth certificates data and the CPS data. The first rows of Tables 1–4 show that the descriptive statistics of the instrument are very similar between the two data sets.

Tables 1 and 2 show that the average birth weight for children of low-skill mothers (3232 g) is less than the average birth weight of children of high-skill mothers (3277 g) at all parity levels. This is a difference of 45 g, or about 1.6 ounces. The same difference exists between first-born babies of low-skill vs. high skill mothers and higher parity levels. On the other hand, the gestational age is the same between high-skill and low-skill mothers. The tables also show that low-skill mothers have one fewer prenatal care visits during their pregnancy in comparison to high-skill mothers (10 vs. 11 visits). Moreover, low-skill mothers initiate prenatal care later than high-skill mothers and the percentage of low-skill mothers who smoked during their pregnancy (27%) is much higher compared to the percentage of high-skill mothers who smoked during their pregnancy (16%).

Smoking and drinking are dummy variables to indicate if the mother smoked cigarettes or consumed alcohol during pregnancy. *Prenatal Care Late* is a dummy variable that takes the value of one if the prenatal care was initiated after the first trimester during pregnancy. *Prenatal Care Visits* is the number of visits to a prenatal care provider during the pregnancy. *Prenatal Care Delay* represents the delay in the receipt of prenatal care in months. For example, if the mother started receiving prenatal care in the fifth month of her pregnancy, this variable takes the value of five. If she never received prenatal care, the variable is assigned the value of 10.

Average real weekly earnings of low-skill unmarried women in the CPS sample presented in Table 3 is about $456 in 2005 dollars, and it is $700 in the sample of high-skill unmarried women in Table 4. There are differences in the

Table 1
Summary statistics of birth certificate data (low-skill unmarried women).

Variable	Low-skill unmarried women								
	All Parity			First-borns			Parity > 1		
	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*
ln(*AH*/*AL*)	1.2376	0.4497	10,183,953	1.2423	0.4463	3,217,825	1.2359	0.4512	6,924,031
Birth weight (g)	3232.2	604.2	10,183,953	3214.0	601.1	3,217,825	3241.0	605.1	6,924,031
Low birth weight (<2500 g)	0.0890	0.2847	10,183,953	0.0903	0.2866	3,217,825	0.0881	0.2835	6,924,031
Gestation (weeks)	38.789	2.8921	10,046,232	38.985	2.8582	3,184,013	38.700	2.9017	6,823,681
Number of Prenatal Care Visits	10.146	4.4999	9,776,757	10.939	4.1781	3,104,256	9.7803	4.5933	6,645,032
Prenatal Care Delay[a]	3.3182	2.1271	9,866,760	2.9960	1.8394	3,130,218	3.4667	2.2305	6,707,581
Late prenatal care[b]	0.3312	0.4707	9,866,760	0.2629	0.4402	3,130,218	0.3629	0.4808	6,707,581
Smoking	0.2727	0.4454	7,815,745	0.2308	0.4214	2,499,417	0.2927	0.4550	5,288,901
Drinking	0.0303	0.1713	7,890,548	0.0211	0.1439	2525,557	0.0345	0.1825	5,338,787
Age	25.32	4.9488	10,183,953	23.54	4.142	3,217,825	26.15	5.071	6,924,031
Less than high school education	0.4141	0.4926	10,183,953	0.2980	0.4574	3,217,825	0.4681	0.4990	6,924,031
High school diploma	0.5859	0.4926	10,183,953	0.7020	0.4574	3,217,825	0.5319	0.4990	6,924,031
White	0.6428	0.4792	10,183,953	0.7210	0.4485	3,217,825	0.6068	0.4885	6,924,031
Black	0.3197	0.4664	10,183,953	0.2411	0.4278	3,217,825	0.3559	0.4788	6,924,031
Other race	0.0375	0.1899	10,183,953	0.0379	0.1909	3,217,825	0.0373	0.1895	6,924,031

Birth certificates are for the years 1989–2004, covering conceptions from 1988–2004. Mothers are at least 20 years of age. Low-skill means that the mother has a high school diploma or less education. The means of the variables are different from each other between the "All Parity" "First-borns" and "Parity > 1" samples with $p = 0.00$.

[a] If no prenatal care took place, prenatal care delay is coded = 10.

[b] Late initiation of prenatal care means that prenatal care was initiated after the first trimester, conditional on having any prenatal care.

Table 2
Summary statistics of birth certificate data (high-skill unmarried women).

Variable	High-skill unmarried women								
	All Parity			First-borns			Parity > 1		
	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*
ln(*AH/AL*)	1.2789	0.4279	3,572,903	1.2769	0.4278	1,846,587	1.2814	0.4284	1,711,421
Birth weight (g)	3276.9	608.6	3,572,903	3261.4	606.0	1,846,587	3294.0	610.6	1,711,421
Low birth weight (<2500 g)	0.0788	0.2694	3,572,903	0.0804	0.2719	1,846,587	0.0770	0.2665	1,711,421
Gestation (weeks)	38.825	2.7732	3,538,342	38.945	2.764	1,831,675	38.696	2.776	1,692,843
Number of Prenatal Care Visits	11.264	4.2067	3,453,765	11.589	4.028	1,792,199	10.915	4.361	1,651,574
Prenatal Care Delay[a]	2.8239	1.7846	3,481,043	2.6961	1.6504	1,805,021	2.9612	1.9072	1,665,373
Late prenatal care[b]	0.2272	0.4190	3,481,043	0.1963	0.3972	1,805,021	0.2605	0.4389	1,665,373
Smoking	0.1587	0.3654	2,845,059	0.1223	0.3276	1,474,176	0.1985	0.3989	1,360,207
Drinking	0.0219	0.1462	2,852,885	0.0191	0.1368	1,480,386	0.0248	0.1556	1,362,486
Age	26.89	5.437	3,572,903	25.438	5.096	1,846,587	28.456	5.358	1,711,421
Some college education	0.7779	0.4156	3,572,903	0.7344	0.4416	1,846,587	0.8253	0.3797	1,711,421
College degree	0.2220	0.4156	3,572,903	0.2656	0.4416	1,846,587	0.1747	0.3797	1,711,421
White	0.5853	0.4927	3,572,903	0.6363	0.4810	1,846,587	0.5303	0.4990	1,711,421
Black	0.3672	0.4820	3,572,903	0.3167	0.4651	1,846,587	0.4217	0.4938	1,711,421
Other race	0.0474	0.2126	3,572,903	0.0470	0.2116	1,846,587	0.0480	0.2137	1,711,421

Birth certificates are for the years 1989–2004, covering conceptions from 1988 to 2004. Mothers are at least 20 years of age. High-skill means that the mother has at least some college education. The means of the variables are different from each other between the "All Parity" "First-borns" and "Parity > 1" samples with $p = 0.00$.

[a] If no prenatal care took place, prenatal care delay is coded = 10.

[b] Late initiation of prenatal care means that prenatal care was initiated after the first trimester, conditional on having any prenatal care.

average characteristics of women in the CPS samples compared to the characteristics of mothers obtained from the birth certificates. Women in the CPS sample tend to be older on average, though the minimum age is 20 and the maximum age is 49 in both samples. Comparing Table 1 with Table 3 shows that the proportion of women with no high school diploma is larger in the low-skill sample of the birth certificate data and comparing Table 2 with Table 4 shows that the proportion of women with a college degree is higher in the CPS sample of the high-skill women.

Pregnant women on Medicaid might not alter their consumption of prenatal care in reaction to a change in income as they already have access to care. Thus, we estimate the models by likely Medicaid status of mothers, and Tables 3 and 4 also provide descriptive statistics by Medicaid receipt of mothers.

The means of the variables pertaining to first-borns in Table 1 are different from those of parity > 1. Similarly, the means of the variables in the "All Parity" sample are different from those in the "First-born" sample with *p*-values of 0.00. This is the case even when the means are close to each other in magnitude, because the sample sizes are in the millions. The same is true for all variables reported in Tables 2–4.

Table 3
Summary statistics of CPS data (low-skill unmarried women).

Variable	Low-skill unmarried women								
	All			Medicaid non-recipients			Medicaid recipients		
	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*
ln(*AH/AL*)	1.2008	0.4819	51,517	1.1911	0.4816	45,771	1.2838	0.4754	5673
Real weekly earnings (2005 $)	456.27	254.40	51,517	470.89	255.15	45,771	323.86	205.45	5673
Age	33.44	8.68	51,517	33.85	8.715	45,771	30.14	7.630	5673
Less than high school education	0.1955	0.3966	51,517	0.1781	0.3826	45,771	0.3326	0.4712	5673
High school diploma	0.8045	0.3966	51,517	0.8219	0.3826	45,771	0.6673	0.4712	5673
White	0.7563	0.4287	51,517	0.7736	0.4185	45,771	0.6280	0.4834	5673
Black	0.1976	0.3982	51,517	0.1835	0.3870	45,771	0.3094	0.4623	5673
Other race	0.0451	0.2076	51,517	0.0430	0.2028	45,771	0.0626	0.2422	5673

CPS sample for the years 1989–2005, covering earnings for 1988–2004. Women are at least 20 years of age. Low-skill means that the mother has a high school diploma or less education. The number of observations of Medicaid recipients and Medicaid non-recipients add up to less than the total number of observations for low-skill unmarried women in the CPS sample because we drop observations with imputed Medicaid recipient status. The means of the variables are different from each other between the three samples listed in the table with $p = 0.00$.

Table 4
Summary statistics of CPS data (high-skill unmarried women).

Variable	High-skill unmarried women								
	All			Medicaid non-recipients			Medicaid recipients		
	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*	Mean	Std. Dev.	*N*
ln(*AH*/*AL*)	1.2761	0.4534	72,974	1.2721	0.4539	69,949	1.3763	0.4264	2983
Real weekly earnings (2005 $)	700.57	435.49	72,974	712.55	436.64	69,949	422.51	295.234	2983
Age	33.28	8.391	72,974	33.36	8.408	69,949	31.36	7.740	2983
Some college education	0.5496	0.4975	72,974	0.5373	0.4986	69,949	0.8354	0.3709	2983
College degree	0.4504	0.4975	72,974	0.4626	0.4986	69,949	0.1646	0.3709	2983
White	0.7914	0.4063	72,974	0.7984	0.4012	69,949	0.6306	0.4827	2983
Black	0.1494	0.3564	72,974	0.1438	0.3509	69,949	0.2769	0.4475	2983
Other race	0.0592	0.2360	72,974	0.0578	0.2333	69,949	0.0925	0.2898	2983

CPS sample for the years 1988–2004, covering earnings for 1988–2004. Women are at least 20 years of age. High-skill means that the mother has at least some college education. The number of observations of Medicaid recipients and Medicaid non-recipients add up to less than the total number of observations for high-skill unmarried women in the CPS sample because we drop observations with imputed Medicaid recipient status. The means of the variables are different from each other between the three samples listed in the table with $p = 0.00$.

4. Results

Table 5 presents the results pertaining to low-skilled women. We report the results for the birth weight equation, the equation for the probability of having a low birth weight baby (lighter than 2500 g), for gestational age of the baby at birth, for mother's smoking and drinking during pregnancy as well as three measures of prenatal care. All regressions control for individual demographic characteristics, as well as state fixed-effects,

Table 5
The impact of mothers' earnings: low-skill, unmarried women.

		All Parity		Firstborns	
	First stage	Reduced form	IV	Reduced form	IV
Birth weight (g)	−0.107***	−13.20***	123.42***	−10.09**	94.297*
	(0.0288)	(3.3368)	(45.550)	(5.0242)	(53.381)
	[51,400]	[10,183,953]		[3,217,825]	
Low birth weight (<2500 g)	−0.107***	0.00438***	−0.0410**	0.00491**	−0.0459**
	(0.0288)	(0.0013)	(0.0161)	(0.0019)	(0.0219)
	[51,400]	[10,183,953]		[3,217,825]	
Gestation (weeks)	−0.107***	−0.0721***	0.6742***	−0.0749***	0.7006**
	(0.0288)	(0.0155)	(0.2320)	(0.0245)	(0.2966)
	[51,400]	[10,046,232]		[3,184,013]	
Prenatal Care Visits	−0.107***	−0.249***	2.3242***	−0.2282***	2.1335**
	(0.0288)	(0.0504)	(0.7829)	(0.0654)	(0.8384)
	[51,400]	[9,776,757]		[3,104,256]	
Prenatal Care Delay	−0.107***	0.2034***	−1.9015***	0.176***	−1.6463***
	(0.0288)	(0.0329)	(0.5967)	(0.0407)	(0.5838)
	[51,400]	[9,866,760]		[3,130,218]	
Prenatal Care Late	−0.107***	0.0278***	−0.2601***	0.0210***	−0.1965**
	(0.0288)	(0.0052)	(0.0855)	(0.0067)	(0.0817)
	[51,400]	[9,866,760]		[3,130,218]	
Smoking	−0.0771**	0.0100*	−0.1301	0.00938	−0.1206
	(0.0286)	(0.0055)	(0.0862)	(0.0061)	(0.0907)
	[44,945]	[7,815,745]		[2,499,417]	
Drinking	−0.0991***	−0.00008	0.00083	0.00107	−0.0121
	(0.0287)	(0.0015)	(0.0151)	(0.0018)	(0.0206)
	[47,988]	[7,890,548]		[2,525,557]	

Note. Standard errors clustered at the census division by age group level are in parentheses, sample sizes in brackets.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey with earnings for the years 1988–2004. Regressions also include a quadratic term in age, controls for having a high school diploma, race, state dummies, year dummies, and state-specific year trends. Reduced form results were obtained using birth certificate data covering conceptions during years 1988–2004; regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data. Prenatal Care Visits are the number of times the mother visited a health care provider for prenatal consultations during the pregnancy. Prenatal Care Delay is the number of months that the mother waited before seeking prenatal care. Prenatal Care Late is a dummy variable indicating whether the mother initiated prenatal care after the first trimester of the pregnancy. Smoking is a dummy variable for whether the mother smoked in during the pregnancy. Drinking is a dummy variable for whether the mother drank during the pregnancy.

year fixed-effects and state-specific trends. We report standard errors that are clustered at the census division by age group level in order to allow for correlated error terms for mothers of similar age within a census division.[12]

For each outcome, Table 5 presents two sets of results. Column 2 and 3 present the results obtained from regressions that use all births, regardless of parity. In other words, these regressions use all newborns irrespective of whether they are the first child of the mother, or whether they represent the second birth, and so on. The table also presents the results obtained from first-born infants, displayed in the two right-most columns.

Two-sample IV requires both samples to include the same variables. The following variables, which exist both in the CPS and the birth certificates, are included as control variables in each regression: the race of the mother, the age of the mother, whether the mother has a high school diploma (in the low-skill sample), whether she has a college diploma (in the high-skill sample).

The first row of Table 5 shows the first-stage results, as well as the reduced form and the IV estimates pertaining to the sample of all births (all parity) and the sample of first-borns. The first stage is the same in both samples as it is obtained from the CPS.[13] In fact, the first stage is the same for all outcomes other than smoking and drinking. The reason why the first-stage regression is different in smoking and drinking regressions is that some states do not report smoking or drinking information on birth certificates in some years. For example, California never reports smoking on birth certificates. Louisiana and Oklahoma started reporting smoking information on birth certificates in 1990. In cases like these, birth certificates from these states cannot be used in reduced form regressions for smoking. For consistency, we omitted the same state and years from the CPS data in running the first-stage regressions.

The instrument is strong with an *F*-value of almost 14. The birth weight is in levels, and the real weekly earnings are in logarithms. Column 3 of Table 5 shows that in the sample of all births, the IV estimate indicates that a 10% increase in real weekly earnings increases the birth weight of children of unskilled unmarried women by only 12.3 g, which is a small impact. The same increase in earnings produces an increase in birth weight by about 9 g in the sample of first-born babies. The probability of low birth goes down by 4-to-5 percentage points for low-skill women due to doubling in earnings and the gestational age goes up by about 0.7 weeks.

Row 4 of Table 5 shows that an increase in income has a positive effect on prenatal care consumption. Specifically, if real weekly earnings double the number of prenatal care visits during pregnancy goes up by about two visits both in the sample of birth at all parity level as well as in the sample of low-skill mothers of firstborns. The same increase in income generates a shortening in the delay of the initiation of prenatal care (i.e. women start consuming prenatal care sooner after getting pregnant), shown in row 5. Consistent with these results, we also observe that an increase in income of low-skilled unmarried women reduces their propensity to initiate late prenatal care (after the first trimester). Table 5 also shows that an increase in income has no impact on drinking during pregnancy or on smoking.[14]

The upshot of Table 5 is that in case of low-skilled unmarried mothers, an increase in income, triggered by a regional shock to the relative demand for skilled labor, increases the consumption of prenatal care. This produces a small increase in gestational age of the newborn and a very small positive impact on birth weight.

Table 6 displays a different picture in case of high-skilled unmarried women. In this case, an increase in real weekly earnings has no statistically significant impact on birth weight or prenatal case consumption. The reduced forms are not significant in birth weight equations. Furthermore, the instrument is not powerful in the first-stage regressions. The age interval of women in the high-skill sample is also 20-to-49 as was the case in the low-skill sample. The high-skill sample, by definition, consists of women who have at least some college education. Some of these women may still be enrolled in college at the age of 20. Therefore, we also ran the regressions of high-skilled women sample with those who are 25 or older. The results were very similar.

4.1. State-level instrument

We consider census divisions as the geographical area in which technology is determined and then adopted by the states that encompass those regions.

[12] We define age groups in two-year intervals (i.e. 20 and 21 years old, 22 and 23 years old, etc.) Combined with nine census divisions, this results in 135 clusters.

[13] Observations with imputed earnings are not used. Birth certificates reveal the birth day of the child; thus we are able to impute the month of conception which is used to match the timing of the CPS survey. This way, the year in which earnings information is obtained from the CPS is aligned with the year in which the woman was pregnant.

[14] While there exists an extensive literature on the effect of cigarette prices on smoking (Tekin et al., 2009; Cawley et al., 2004; Colman et al., 2003; Becker et al., 1994), the evidence on the income elasticity of smoking is scant. Maternal smoking behavior has also received attention (Fingerhut et al., 1990). However, the focus of the research has again been the effect of price changes on smoking behavior of women, not on income (Evans and Ringel, 1999; Evans et al., 1999; Ringel and Evans, 2001). For example Ringel and Evans (2001) investigate how women's smoking behavior during pregnancy is affected by cigarette taxes and find that higher cigarette taxes reduce smoking rates among pregnant women. They find that the quit behavior of pregnant women is more sensitive to changes in the prices of cigarettes than the quit behavior of non-pregnant women. The authors acknowledge that income is an essential control variable, but they are unable to control for it using only data from the Natality Detail Files. Limited evidence on the income elasticity of smoking suggests that whether income elasticity is positive or negative varies systematically across time periods, countries, and demographic groups. For high-income countries like the U.S. the sign appears to have reversed over time, so that cigarettes appear to have switched from being a normal good to an inferior good (Cheng and Kenkel, 2010; Wasserman et al., 1991). Kenkel et al. (2014) use data on multiple waves of the Current Population Survey's Tobacco Use Supplement matched with income data from the Annual Social and Economic Supplement from 1993 to 2007. The authors find that while the income elasticity of smoking in a cross sectional OLS specification is negative, that income elasticity is positive in the IV specification.

Table 6
The impact of mothers' earnings: high-skill, unmarried women.

		All Parity		Firstborns	
	First stage	Reduced form	IV	Reduced form	IV
Birth weight (g)	0.0352	−4.288	−121.78	−7.872	−223.56
	(0.0215)	(4.2808)	(142.60)	(6.4886)	(229.51)
	[72,803]	[3,572,903]		[1,846,587]	
Low birth weight (<2500 g)	0.0352	0.00168	0.0478	0.00538**	0.1529
	(0.0215)	(0.0019)	(0.0611)	(0.0027)	(0.1215)
	[72,803]	[3,572,903]		[1,846,587]	
Gestation (weeks)	0.0352	−0.0478**	−1.3569	−0.0560*	−1.5898
	(0.0215)	(0.0217)	(1.0333)	(0.0297)	(1.2868)
	[72,803]	[3,538,342]		[1,831,675]	
Prenatal Care Visits	0.0352	−0.289***	−8.1936	−0.287***	−8.1465
	(0.0215)	(0.0484)	(5.1994)	(0.0560)	(5.2329)
	[72,803]	[3,453,765]		[1,792,199]	
Prenatal Care Delay	0.0352	0.174***	4.9425	0.148***	4.2023
	(0.0215)	(0.0252)	(3.1085)	(0.0305)	(2.7135)
	[72,803]	[3,481,043]		[1,805,021]	
Prenatal Care Late	0.0352	0.0295***	0.8372	0.0241***	0.6850
	(0.0215)	(0.0049)	(0.5306)	(0.0060)	(0.4529)
	[72,803]	[3,481,043]		[1,805,021]	
Smoking	0.0462**	0.00524	0.1134	0.00417	0.0904
	(0.0219)	(0.0048)	(0.1175)	(0.0050)	(0.1158)
	[63,373]	[2,845,059]		[1,474,176]	
Drinking	0.0386*	0.00007	0.0018	0.00088	0.0220
	(0.0218)	(0.0016)	(0.0417)	(0.0020)	(0.0520)
	[67,206]	[2,852,885]		[1,480,386]	

Note. Standard errors clustered at the census division by age group level are in parentheses, sample sizes in brackets.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey with earnings for the years 1988–2004. Regressions also include a quadratic term in age, controls for having a college degree, race, state dummies, year dummies, and state-specific year trends. Reduced form results were obtained using birth certificate data for conceptions during the years 1988–2004; regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Prenatal Care Visits are the number of times the mother visited a health care provider for prenatal consultations during the pregnancy. Prenatal Care Delay is the number of months that the mother waited before seeking prenatal care. Prenatal Care Late is a dummy variable indicating whether the mother initiated prenatal care after the first trimester of the pregnancy. Smoking is a dummy variable for whether the mother smoked in during the pregnancy. Drinking is a dummy variable for whether the mother drank during the pregnancy.

This procedure minimizes the concern that technology could be endogenous at the state level, driven by local labor market conditions. Nevertheless, to investigate the sensitivity of the results, we also created the instrument at the state-level (as opposed to the census division-level). In this procedure, we created the instrument for a state by considering all states in a state's census division except for the state itself. The results obtained from this ''leave own-state out'' instrument, are displayed in Appendix Tables A1 and A2. In some cases the estimated impacts are greater in magnitude, but they are consistent with those reported earlier.

4.2. Results by Medicaid status of mothers

Low-skilled (low-educated) women might be covered by Medicaid, and therefore an increase in income might not translate to increased prenatal care consumption. To further investigate which type of women are impacted by an increase in income, we ran the birth weight, gestational age and input demand regressions for both low-skill and high-skill women by their Medicaid status. For example, Table 7 presents the results obtained from birth weight regressions that use the sample of *Medicaid Non-recipients*. These are unmarried women who are *not* on Medicaid.[15] Their weekly earnings are higher than earnings of women who are on Medicaid. The descriptive statistics in Table 3 show that for low-skilled unmarried women the average real weekly earnings are $471 if they are not on Medicaid, and it is about $324 if they are Medicaid recipients. Similarly, Table 4 shows that average real weekly earnings are $423 for high-skilled Medicaid recipients, and it is about $713 for high-skilled women who are not on Medicaid.

In Table 7 as well as the rest of the paper we employ the sample of first-born babies. The results of the regressions that used all births provided very similar results. Table 7 shows that an increase in weekly earnings, triggered by a skill-biased technology shock has an impact on birth weight in case of low-skilled *Non-Medicaid Recipient* low-skill mothers. Earnings have no impact on birth weight in case of high-skilled mothers.

[15] As described earlier, we can directly identify Medicaid recipients in the CPS data, but Medicaid status is not observed in the birth certificate data. Thus, we approximate the Medicaid status on birth certificates using parity.

Table 7
The impact of mothers' earnings on birth weight (Medicaid non-recipients & firstborns).

	Birth weight					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH*/*AL*)	−0.0926***	−10.09**	108.89*	0.0385*	−7.872	−204.59
	(0.0287)	(5.0242)	(63.866)	(0.0214)	(6.4886)	(203.38)
Age	0.0626***	12.00***		0.112***	7.330***	
	(0.0043)	(2.4858)		(0.0042)	(2.3054)	
Age-squared	−0.0007***	−0.350***		−0.00130***	−0.243***	
	(0.0001)	(0.0437)		(0.0001)	(0.0393)	
Race: Black	−0.0519***	−190.1***		−0.0701***	−201.6***	
	(0.0080)	(4.3978)		(0.0082)	(3.7141)	
Race: Other	−0.0596***	−57.41***		−0.0263**	−112.4***	
	(0.0120)	(10.6302)		(0.0109)	(9.7512)	
Education: HS	0.297***	55.51***		–	–	
	(0.0099)	(2.1458)		–	–	
Education: College	–	–		0.325***	47.42***	
	–	–		(0.0071)	(2.7022)	
F-Stat	10.43			3.24		
Observations	45,666	3,217,825		69,785	1,846,587	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

Table 8 displays the results of the analysis of the propensity of having a low birth weight baby in the sample of mothers who are not on Medicaid. Consistent with Table 7, an increase in earnings lowers the probability of having a low birth weight baby in the sample of *low-skill* women who are not on Medicaid. The magnitude of the impact is small: doubling of earnings lowers the probability of having a low birth weight baby by only five

Table 8
The impact of mothers' earnings on low birth weight (Medicaid non-recipients & firstborns).

	Low birth weight (=1 if <2500 g)					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH*/*AL*)	−0.0926***	0.00491**	−0.0530**	0.0385*	0.00538*	0.1399
	(0.0287)	(0.0019)	(0.0266)	(0.0214)	(0.0027)	(0.1052)
Age	0.0626***	−0.00330***		0.112***	0.000553	
	(0.0043)	(0.0011)		(0.0042)	(0.0009)	
Age-squared	−0.0007***	0.000138***		−0.00130***	0.00005***	
	(0.0001)	(0.0000)		(0.0001)	(0.0000)	
Race: Black	−0.0519***	0.0556***		−0.0701***	0.0526***	
	(0.0080)	(0.0018)		(0.0082)	(0.0015)	
Race: Other	−0.0596***	0.00644***		−0.0263**	0.0146***	
	(0.0120)	(0.0023)		(0.0109)	(0.0018)	
Education: HS	0.297***	−0.0113***		–	–	
	(0.0099)	(0.0011)		–	–	
Education: College	–	–		0.325***	−0.0171***	
	–	–		(0.0071)	(0.0010)	
F-Stat	10.43			3.24		
Observations	45,666	3,217,825		69,785	1,846,587	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

Table 9
The impact of mothers' earnings on gestation (Medicaid non-recipients & firstborns).

	Gestational age (weeks)					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH/AL*)	−0.0926***	−0.0749***	0.8090**	0.0385*	−0.0560*	−1.4549
	(0.0287)	(0.0245)	(0.3643)	(0.0214)	(0.0297)	(1.1170)
Age	0.0626***	−0.0147		0.112***	0.00165	
	(0.0043)	(0.0099)		(0.0042)	(0.0097)	
Age-squared	−0.0007***	0.00109***		−0.00130***	0.00066***	
	(0.0001)	(0.0002)		(0.0001)	(0.0002)	
Race: Black	−0.0519***	−0.632***		−0.0701***	−0.594***	
	(0.0080)	(0.0184)		(0.0082)	(0.0157)	
Race: Other	−0.0596***	−0.198***		−0.0263**	−0.267***	
	(0.0120)	(0.0249)		(0.0109)	(0.0256)	
Education: HS	0.297***	0.0492***		–	–	
	(0.0099)	(0.0070)		–	–	
Education: College	–	–		0.325***	0.0901***	
	–	–		(0.0071)	(0.0103)	
F-Stat	10.43			3.24		
Observations	45,666	3,184,013		69,785	1,831,675	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

percentage points. A similar inference is obtained when using gestational age as an indicator for infant health, shown in Table 9.

Tables 10–12 display the results pertaining to models where various measures of prenatal care are used as dependent variables. Once again, we focus on mothers who are not Medicaid recipients. Table 10, which displays the results for prenatal visits, shows that an increase in weekly earnings increases total prenatal care visits during pregnancy in the sample of low-skilled women, but it

Table 10
The impact of mothers' earnings on the number of prenatal medical care visits (Medicaid non-recipients & firstborns).

	Prenatal Care Visits					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH/AL*)	−0.0926***	−0.228***	2.4637**	0.0385*	−0.287***	−7.4553*
	(0.0287)	(0.0654)	(1.0393)	(0.0214)	(0.0560)	(4.3908)
Age	0.0626***	0.0805***		0.112***	0.142***	
	(0.0043)	(0.0114)		(0.0042)	(0.0215)	
Age-squared	−0.0007***	−0.0014***		−0.00130***	−0.00162***	
	(0.0001)	(0.0002)		(0.0001)	(0.0004)	
Race: Black	−0.0519***	−0.451***		−0.0701***	−0.135***	
	(0.0080)	(0.0606)		(0.0082)	(0.0416)	
Race: Other	−0.0596***	−0.735***		−0.0263**	−0.811***	
	(0.0120)	(0.0381)		(0.0109)	(0.0275)	
Education: HS	0.297***	0.798***		–	–	
	(0.0099)	(0.0438)		–	–	
Education: College	–	–		0.325***	0.114***	
	–	–		(0.0071)	(0.0258)	
F-Stat	10.43			3.24		
Observations	45,666	3,104,256		69,785	1,792,199	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

Table 11
The impact of mothers' earnings on Prenatal Care Delay (Medicaid non-recipients & firstborns).

	Prenatal Care Delay					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH/AL*)	−0.0926***	0.176***	−1.9011***	0.0385*	0.148***	3.8458
	(0.0287)	(0.0407)	(0.7345)	(0.0214)	(0.0305)	(2.2791)
Age	0.0626***	−0.0880***		0.112***	−0.132***	
	(0.0043)	(0.0058)		(0.0042)	(0.0080)	
Age-squared	−0.0007***	0.00151***		−0.00130***	0.00181***	
	(0.0001)	(0.0001)		(0.0001)	(0.0001)	
Race: Black	−0.0519***	0.0630***		−0.0701***	−0.0374***	
	(0.0080)	(0.0197)		(0.0082)	(0.0113)	
Race: Other	−0.0596***	0.290***		−0.0263**	0.309***	
	(0.0120)	(0.0164)		(0.0109)	(0.0170)	
Education: HS	0.297***	−0.328***		–	–	
	(0.0099)	(0.0196)		–	–	
Education: College	–	–		0.325***	−0.0993***	
	–	–		(0.0071)	(0.0134)	
F-Stat	10.43			3.24		
Observations	45,666	3,130,218		69,785	1,805,021	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

decreases the prenatal care visits of high-skill women. Table 11 demonstrates that a 50% increase in weekly earnings of low-skill women reduces the delay in the initiation of prenatal care by about a month and Table 12 shows that a 50% increase in income of the same group of women reduces the probability of initiating prenatal care late (after the first trimester of pregnancy) by 11 percentage points. Earnings have no impact on prenatal care consumption of high-skill mothers regardless of how prenatal care is measured.

Table 12
The impact of mothers' earnings on the probability of initiating Prenatal Care Late (Medicaid non-recipients & firstborns).

	Initiating Prenatal Care Late					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH/AL*)	−0.0926***	0.0210***	−0.2269**	0.0385*	0.0241***	0.6269
	(0.0287)	(0.0067)	(0.1005)	(0.0214)	(0.0060)	(0.3820)
Age	0.0626***	−0.0236***		0.112***	−0.0315***	
	(0.0043)	(0.0012)		(0.0042)	(0.0019)	
Age-squared	−0.0007***	0.000390***		−0.00130***	0.000441***	
	(0.0001)	(0.0000)		(0.0001)	(0.0000)	
Race: Black	−0.0519***	0.0174***		−0.0701***	0.00314***	
	(0.0080)	(0.0041)		(0.0082)	(0.0027)	
Race: Other	−0.0596***	0.0691***		−0.0263**	0.0741***	
	(0.0120)	(0.0042)		(0.0109)	(0.0045)	
Education: HS	0.297***	−0.0703***		–	–	
	(0.0099)	(0.0037)		–	–	
Education: College	–	–		0.325***	−0.0216***	
	–	–		(0.0071)	(0.0026)	
F-Stat	10.43			3.24		
Observations	45,666	3,130,218		69,785	1,805,021	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

Grossman and Joyce (1990) find a small impact of prenatal care on birth weight. Specifically, they report that a month of prenatal care delay causes a reduction of birth weight by 37 g for black mothers, and it has not a statistically significant effect in case of white mothers. Our finding is consistent with their results. We find that mothers' income has a small impact on the use of prenatal medical care. Given that prenatal care has a small impact on birth weight reported by Grossman and Joyce (1990), the impact of income on birth weight through the channel of prenatal care is expected to be low.

Previous research has shown a detrimental impact of smoking while pregnant on birth weight. Much of this research is based on the intensity of maternal smoking; i.e. the number of cigarettes smoked during pregnancy (e.g. Grossman and Joyce, 1990; Rosenzweig and Schultz, 1983), although the inference obtained from Evans and Ringel (1999) is based on smoking participation during pregnancy as we do in this paper. Researchers either considered cigarette consumption as an exogenous variable, or investigated the impact of cigarettes on birth weight driven by changes in cigarette prices. Information on the response of maternal smoking to income is limited. Rosenzweig and Schultz (1983) found that the elasticity of maternal smoking to husband income is small, with an elasticity of 0.07. Table 13 shows that in the sample of mothers who are not Medicaid recipients, weekly earnings have no impact on smoking regardless of the skill level of the mother. Therefore, a change in smoking participation (initiation or cessation of smoking during pregnancy) due to a change in income is not an important avenue through which birth weight is impacted. Similarly, Table 14 shows that alcohol consumption during pregnancy does not react to variations in mothers' earnings. Caution should be exercised here because the first-stage regressions are not powerful in smoking and alcohol consumption.

We also estimated the same set of regressions using the sample of mothers who are on Medicaid. The results are available upon request. Weekly earnings have no impact on birth weight or on the probability of having a low-birth baby either for high-skill or low-skill mothers in this sample. Similarly, income has no impact on smoking or drinking. The impact of weekly earnings has a small positive impact on prenatal care consumption for low skill mothers with p-values of about 0.09, and the same is true for gestational age. However, the first-stage regressions of these models are very weak with an F-statistic of 4.1. That is, the instrument is not powerful enough to explain the variation in weekly earnings of Medicaid-recipient mothers. Consequently, no reliable inference can be made on this sample.

4.3. Immigration

In some states Hispanic immigration could be an important aspect of the low-skill labor market. Large inflow of low-skilled immigrant workers would impact wages and earnings of all low-skilled workers, and the health status of babies born to immigrant workers may be systematically different. To investigate the sensitivity of

Table 13
The impact of mothers' earnings on the probability that a mother smokes during pregnancy (Medicaid non-recipients & firstborns).

	Smoking					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH*/*AL*)	−0.0584**	0.00938	−0.1606	0.0526**	0.00417	0.0793
	(0.0284)	(0.0063)	(0.1301)	(0.0227)	(0.0050)	(0.1004)
Age	0.0626***	0.00565**		0.110***	0.0316***	
	(0.0045)	(0.0025)		(0.0042)	(0.0020)	
Age-squared	−0.00073***	−0.00004		−0.00127***	−0.00046***	
	(0.0001)	(0.0000)		(0.0001)	(0.0000)	
Race: Black	−0.0618***	−0.160***		−0.0794***	−0.112***	
	(0.0078)	(0.0105)		(0.0084)	(0.0035)	
Race: Other	−0.0694***	−0.118***		−0.0246*	−0.0690***	
	(0.0141)	(0.0086)		(0.0118)	(0.0031)	
Education: HS	0.281***	−0.0624***		–	–	
	(0.0098)	(0.0086)		–	–	
Education: College	–	–		0.322***	−0.0896***	
	–	–		(0.0075)	(0.0038)	
F-Stat	4.22			5.39		
Observations	39,971	2,499,417		60,717	1,474,176	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

Table 14
The impact of mothers' earnings on the probability of a mother drinking alcohol during pregnancy (Medicaid non-recipients & firstborns).

	Drinking					
	Low-skill women			High-skill women		
	First stage	Reduced form	IV	First stage	Reduced form	IV
ln(*AH/AL*)	−0.0725**	0.00107	−0.0148	0.0447**	0.000875	0.01958
	(0.0289)	(0.0018)	(0.0255)	(0.0222)	(0.0020)	(0.04614)
Age	0.0622***	0.00499***		0.111***	0.00368***	
	(0.0045)	(0.0005)		(0.0042)	(0.0005)	
Age-squared	−0.00071***	−0.00006***		−0.00129***	−0.00004***	
	(0.0001)	(0.0000)		(0.0001)	(0.0000)	
Race: Black	−0.0596***	−0.00068		−0.0767***	−0.0116***	
	(0.0080)	(0.0007)		(0.0083)	(0.0005)	
Race: Other	−0.0650***	0.00226		−0.0204*	−0.00650***	
	(0.0140)	(0.0024)		(0.0114)	(0.0011)	
Education: HS	0.292***	−0.00561***		–	–	
	(0.0098)	(0.0005)		–	–	
Education: College	–	–		0.323***	−0.00488***	
	–	–		(0.0075)	(0.0006)	
F-Stat	6.27			4.0591		
Observations	41,431	2,525,557		62,247	1,480,386	

Note. Standard errors clustered at the census division by age group level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey, covering earnings for the years 1988–2004. Regressions also include state dummies, year dummies, and state-specific year trends. The reduced form results were obtained using the birth certificate data for conceptions during the years 1988–2004 and regressions include identical control variables as the first stage regressions. The Two-Sample IV estimate is the ratio of the reduced form coefficient to the first stage coefficient from two different samples since earnings are unavailable in birth certificate data Low-skill refers to women with a high school education or less; high-skill refers to women with at least some college education. Standard errors for the IV estimate were calculated using the Delta method.

the results to immigration, for each state we calculated the percentage change in the proportion of immigrants in state population between 1990 and 2010. The data on immigration are calculated using census surveys [5% extracts] from the IPUMS. All individuals who were not U.S. citizen at the time of their birth considered as immigrants. Census weights are used to calculate the number of immigrants in each state.

The top 10 states where the proportion of immigrant population grew the fastest over this period are Texas, North Carolina, Georgia, Arkansas, Tennessee, Kansas, Nebraska, Minnesota, Virginia and Kentucky. We dropped these states and re-estimated the models. The results, which are available upon request, showed that our inference is not altered by estimating the models using mothers in states that did not experience a significant increase in immigrant population.

5. Conclusion and discussion

Although the impact of income on infant health is important to investigate both from a scientific and public policy perspective, the analysis is complicated because of the endogeneity of income. For example, maternal income or household income is likely to be correlated with mother attributes and household characteristics that may directly impact the birth weight of the infant. In this paper we use a two-sample instrumental variables strategy to identify the causal impact of mothers' income on the birth weight and gestational age of newborns. We use birth record data obtained from almost 14 million births between 1989 and 2004, which contains information about mother characteristics, the birth weight of the newborn and the location of the birth.

Following the literature on skill-biased technological change and wage inequality, we create a census division- and year-specific measure of skill-biased technological change as an instrument in the first-stage earnings regressions. Because earnings information is not available on birth certificates, we use micro data from the Current Population Survey (CPS) to estimate first-stage earnings equations for women who are observationally similar to the mothers of the 14 million newborns. Specifically, the CPS women and the mothers on the birth certificates are similar in such dimensions as state of residence, age, race, marital status and education. The reduced form equations are based on birth certificates where the birth weight of the newborn depends on exogenous mother characteristics and the census-division level skill-biased technology parameter. This two-sample instrumental variables design allows us recover the structural estimate of the impact of mothers' earnings on birth weight and on gestational age.

We also estimate input demand functions for smoking, drinking, and prenatal medical care consumption of mothers using data provided by birth certificates. Together, these results reveal insights into not only the impact of income on infant health, but also on the pathways through which the impact of income operates.

The results show that an increase in weekly earnings has no impact on prenatal care consumption or the demand for alcohol or cigarettes for mothers who are likely to be on Medicaid. For low-skilled mothers who are *not* likely to be on Medicaid, an increase in weekly earnings generates a small increase in prenatal care consumption, and it reduces the delay in the initiation of prenatal care. Consequently, the increase in income produces an improvement in birth outcomes of newborns through the mechanism of prenatal care consumption and increased gestational age, although the magnitude of the impact is small. Specifically, if a mother's earnings double, triggered by a regional shock to the relative demand for skilled labor, her prenatal visits during pregnancy go up by about two, and this produces an increase in gestational age by 0.7 weeks, and a weight gain of the newborn by about 100 g. This implies an earnings elasticity of birth weight by 0.03. The propensity to smoke and to consume alcohol is not impacted by mothers' earnings.

It is difficult to directly compare the magnitudes of the effects we obtain in this study to those in the literature because methodologies and samples differ substantially between studies. For example, Chung et al. (2015) used dividend payouts from the Alaska Permanent Fund as a source of exogenous income variation, and they estimated an increase in birth weight by 47 g for a ten percent income shock, with an implied income elasticity of 0.13. The effects reported in Lindo (2011), which is based on the assumption that husbands' job losses affect birth weight only through family income in the year prior to birth, provide an income elasticity of 0.32.

An increase in mothers' earnings has no impact on input demand or birth weight of newborns in the sample of mothers who are likely to be on Medicaid, regardless of whether they are high-skilled or low-skilled. The results do not change qualitatively when we calculate the instrument at the state-level, as opposed to the division-level or when we exclude the top 10 states that have experiences the largest increase in their immigrant population.

Our results are generated by an increase in real weekly earnings, due to a skill-biased technical change. Earnings are generated by wages and hours worked; and an increase in wages would increase labor supply. Therefore, an increase in earnings in our analysis could be accompanied with more hours worked, which may induce additional stress to the pregnant woman, and this may be detrimental to infant health. Put differently, our identification strategy captures the net effect of increased earnings, part of which might stem from increased hours worked in the labor market. Thus, our estimates might potentially differ from those of a hypothetical experiment that would involve an income transfer to pregnant women without affecting their labor supply.

Appendix. Construction of the efficiency-adjusted labor inputs to create the index of skill-biased technological change

We use the March Current Population Survey (CPS) files from 1978 to 2010 (covering earnings from 1977 to 2009) for full-time workers (those who work 35 or more hours a week) ages 16–64. Self-employed people are dropped from the sample, as are allocated earnings observations (using individual earnings allocation flags). In constructing the key variables, we closely follow the previous labor literature on wage inequality (Katz and Murphy, 1992; Krusell et al., 2000; Card and DiNardo, 2002; and in particular, Autor et al., 2008).

Each individual's average weekly earnings are formed by dividing annual income from wages and salaries by the number of weeks worked during the previous year. Earnings are deflated using the Personal Consumption Expenditure Index with a base year of 2005. We make two adjustments for topcoded earnings. First, following Autor et al. (2008) income of workers with top coded earnings is imputed by multiplying the annual topcode amount by 1.5. Second, starting in 1996, topcoded earnings values are assigned the mean of all topcoded earners. In these cases, we simply reassign the topcoded values to all such observations and again multiply by 1.5. Workers whose weekly earnings below \$70 in 2005 dollars are dropped, as are those non-full-year workers (i.e. those who work less than 40 weeks) whose weekly earnings exceed 1/40th the top-coded value of weekly earnings.

We construct the series for high-skill and low-skill labor input and wages as follows. The data in each year in each state are divided into 24 distinct groups characterized by 2 sexes, 4 education categories (Education $\leq$ 11 years, Education = 12 years, 13 $\leq$ Education $\leq$ 15 years, and Education $\geq$ 16 years) and four potential experience categories (0–9 years, 10–19 years, 20–29, 30+ years) as in Unel (2010). Potential experience are calculated as Min{age–years of schooling–6, age–16} following Autor et al. (2008). In calculating each group's average weekly earnings, earnings are weighted by the product of the corresponding CPS sampling weight and weeks worked.

We assume that the high-skill labor class consists of college or college-plus workers and the workers with some college; and the low-skill labor class consists of those who have no college education. Groups within a class are assumed to be perfect substitutes and we use group relative weekly earnings of full-time workers as weights for the aggregation of labor inputs into skilled and unskilled classes. Standard in this literature is the assumption that relative wages equal relative efficiencies of labor. More specifically, following Autor et al. (2008), we choose the group that contains male workers with less than 12 years of education and with less than 10 years of potential experience as the base group. A relative wage measure is then constructed by dividing each group's average weekly earnings by the average weekly earnings of the base group. The relative efficiency index measure for each group, q_g, is computed as the arithmetic mean of the relative wage measures in that group over 1977–2009. Then the total efficiency-adjusted labor input in each class is given by

$$H_t = \sum_{g \in G_H} q_g H_{gt} \tag{A.1}$$

$$L_t = \sum_{g \in G_L} q_g N_{gt} \tag{A.2}$$

where N_{gt} represents the total labor weeks used in production by group g in year t. Since H and L are efficiency-adjusted labor inputs, the corresponding earnings W_H and W_L are also efficiency-adjusted. Following Krusell et al. (2000), they are calculated as

$$W_{H_t} = \sum_{g \in G_H} \frac{\omega_{gt} N_{gt}}{H_t} \tag{A.3}$$

$$W_{L_t} = \sum_{g \in G_L} \frac{\omega_{gt} N_{gt}}{L_t}, \tag{A.4}$$

where ω_{gt} represents the average weekly earnings of group g in year t.

Table A1

Table A2

Table A1
The impact of mothers' earnings: low-skill, unmarried women, state-level instrument.

	First stage	All Parity Reduced form	All Parity IV	Firstborns Reduced form	Firstborns IV
Birth weight (g)	−0.0520***	−11.131***	213.89**	−11.688***	224.60*
	(0.0190)	(3.1262)	(98.5884)	(2.6545)	(96.6439)
	[51,005]	[10,183,953]		[3,217,825]	
Low birth weight (<2500 g)	−0.0520***	0.0037**	−0.0719	0.0047**	−0.0903*
	(0.0190)	(0.0018)	(0.0439)	(0.0018)	(0.0473)
	[51,005]	[10,183,953]		[3,217,825]	
Gestation (weeks)	−0.0520***	−0.0495**	0.9510*	−0.0535**	1.0287*
	(0.0190)	(0.0189)	(0.5020)	(0.0221)	(0.5671)
	[51,005]	[10,046,232]		[3,184,013]	
Prenatal Care Visits	−0.0520***	−0.2104**	4.0436	−0.2310**	4.4390**
	(0.0190)	(0.1038)	(2.4819)	(0.0933)	(2.4180)
	[51,005]	[9,776,757]		[3,104,256]	
Prenatal Care Delay	−0.0520***	0.1496***	−2.8749**	0.1366***	−2.6255**
	(0.0190)	(0.0445)	(1.3549)	(0.0482)	(1.3333)
	[51,005]	[9,866,760]		[3,130,218]	
Prenatal Care Late	−0.0520***	0.0224***	−0.4309**	0.0186**	−0.3572*
	(0.0190)	(0.0071)	(0.2085)	(0.0083)	(0.2062)
	[51,005]	[9,866,760]		[3,130,218]	
Smoking	−0.0498**	0.0078**	−0.1569	0.0063	−0.1269
	(0.0243)	(0.0037)	(0.1069)	(0.0047)	(0.1135)
	[44,611]	[7,815,745]		[2,499,417]	
Drinking	−0.0660**	−0.0013	0.0204	0.0016	−0.0292
	(0.0261)	(0.0017)	(0.0276)	(0.0017)	(0.0332)
	[47,627]	[7,890,548]		[2,525,557]	

Note. Standard errors clustered at the state level are in parentheses, sample sizes in brackets.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey with earnings for the years 1988–2004. Regressions also include a quadratic term in age, controls for having a high school diploma, race, state dummies, year dummies, and state-specific year trends. Reduced form results were obtained using birth certificate data covering conceptions during years 1988–2004; regressions include identical control variables as the first stage regressions. The IV estimate is the ratio of the reduced form coefficient over the first stage coefficient since earnings are unavailable in birth certificate data. Prenatal Care Visits are the number of times the mother visited a health care provider for prenatal consultations during the pregnancy. Prenatal Care Delay is the number of months that the mother waited before seeking prenatal care. Prenatal Care Late is a dummy variable indicating whether the mother initiated prenatal care after the first trimester of the pregnancy. Smoking is a dummy variable for whether the mother smoked in during the pregnancy. Drinking is a dummy variable for whether the mother drank during the pregnancy.

Table A2
The impact of mothers' earnings: high-skill, unmarried women, state-level instrument.

	First stage	All Parity Reduced form	All Parity IV	Firstborns Reduced form	Firstborns IV
Birth weight (g)	0.0056	−3.2893	−583.13	−8.7209*	−1546.1
	(0.0167)	(3.9639)	(1866.9)	(4.8036)	(4664.1)
	[72,227]	[3,572,903]		[1,846,587]	
Low birth weight (<2500 g)	0.0056	0.0014	0.2407	0.0042*	0.7459
	(0.0167)	(0.0018)	(0.7801)	(0.0022)	(2.2477)
	[72,227]	[3,572,903]		[1,846,587]	
Gestation (weeks)	0.0056	−0.0328*	−5.8183	−0.0467**	−8.2742
	(0.0167)	(0.0203)	(17.63)	(0.0231)	(25.8812)
	[72,227]	[3,538,342]		[1,831,675]	

Table A2 (*Continued*)

		All Parity		Firstborns	
	First stage	Reduced form	IV	Reduced form	IV
Prenatal Care Visits	0.0056	−0.2348***	−41.619	−0.2596***	−46.03
	(0.0167)	(0.0873)	(124.41)	(0.0951)	(137.56)
	[72,227]	[3,453,765]		[1,792,199]	
Prenatal Care Delay	0.0056	0.1260***	22.3371	0.1057**	18.74
	(0.0167)	(0.0379)	(66.59)	(0.0398)	(56.03)
	[72,227]	[3,481,043]		[1,805,021]	
Prenatal Care Late	0.0056	0.0217***	3.8428	0.0173**	3.0621
	(0.0167)	(0.0068)	(11.461)	(0.0034)	(9.1655)
	[72,227]	[3,481,043]		[1,805,021]	
Smoking	0.0161	0.0046	0.2873	0.0037	0.2322
	(0.0207)	(0.0037)	(0.4369)	(0.0034)	(0.3660)
	[62,932]	[2,845,059]		[1,474,176]	
Drinking	0.0081	0.0015	0.1818	0.0008	0.0739
	(0.0186)	(0.0020)	(0.4840)	(0.0022)	(0.2408)
	[66,703]	[2,852,885]		[1,480,386]	

Note. Standard errors clustered at the state level are in parentheses, sample sizes in brackets.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
First stage results were obtained using data from the Annual Demographic File of the Current Population Survey with earnings for the years 1988–2004. Regressions also include a quadratic term in age, controls for having a college degree, race, state dummies, year dummies, and state-specific year trends. Reduced form results were obtained using birth certificate data for conceptions during the years 1988–2004; regressions include identical control variables as the first stage regressions. The IV estimate is the ratio of the reduced form coefficient over the first stage coefficient since earnings are unavailable in birth certificate data. Prenatal Care Visits are the number of times the mother visited a health care provider for prenatal consultations during the pregnancy. Prenatal Care Delay is the number of months that the mother waited before seeking prenatal care. Prenatal Care Late is a dummy variable indicating whether the mother initiated prenatal care after the first trimester of the pregnancy. Smoking is a dummy variable for whether the mother smoked in during the pregnancy. Drinking is a dummy variable for whether the mother drank during the pregnancy.

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