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What All Women (and Some Men) Want to Know: Does Maternal Age Affect Infant Health?

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Abstract

The incidence of many commonly-studied birth outcomes, such as infant mortality and prematurity, varies by maternal age. In particular, both older and younger women face an increased probability of an unfavorable birth outcome. Whether this association is causal is an open question, as a mother's age is correlated with other determinants of infant health, such as socioeconomic and marital status. If age does directly affect infant health, then delays in childbearing, as have occurred during the past thirty years, could have important implications for health outcomes and costs.

In this paper I examine the relationship between maternal age and infant health. To control for unobservable differences across mothers, I create a unique large-scale panel data set of over one million sibling births from the universe of births in Texas between 1989 and 2001. Comparing outcomes across siblings using a generalized correlated random-effects model, I find that both younger and older mothers give birth to infants who are less healthy along several dimensions. Women over age 35 and under age 18 are 30 percent more likely to have a preterm delivery when compared to women between the ages of 26 and 29. Women giving birth at older ages also face elevated risks of infant death and of having a child with an abnormal condition.

Keywords: infant health, delayed childbearing, correlated random effects
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I. Introduction

The average age at childbirth among women in the United States has increased steadily over the past thirty years. This trend is illustrated in Figure 1, which traces the evolution of the maternal age distribution over time. In 1968, the distribution was tightly centered around age 20: half of all first births were to women between the ages of 18 and 22. By 1998, the distribution had become bimodal, with one peak at 20 and a second emerging at 27.[1] Although this trend toward later childbirth has been recognized in the popular press (e.g., a 2002 *Time* magazine cover story), most of the economics literature has addressed the causes of the rise in maternal age (e.g., Blau (1998)), rather than the consequences.

In this paper I address the question: do changes in the maternal age distribution affect infant health?[2] Simple cross-sectional comparisons suggest that the answer is yes. For mothers 40 and older, the likelihood of a premature delivery is 53 percent higher than that for women between the ages of 25 and 29 (Figure 2). At the other end of the age distribution, women are also at increased risk: the rate of premature birth for mothers under 15 is nearly 2.5 times that for women in their late twenties. Since premature infants spend longer in the hospital at birth and tend to have worse health later in life (Bhutta et al., 2002), changes in the age distribution would seem to have important implications for health care costs and health. However, many other characteristics vary by age (Figure 3). In particular, younger mothers are more likely to be African-American or Hispanic, to have lower levels of education, and are less likely to be married than older mothers.

This confounding may account for the mixed findings of previous studies. Berkowitz et al. (1990) conclude that delayed childbearing imposes no risk on birth outcomes, while Cnattingius et al. (1992) find that older mothers have elevated risks of premature delivery and low birthweight babies. Estimates for teenage mothers are equally inconclusive. These studies leave open the question of whether maternal age is an important determinant of infant health.

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[1] Meanwhile, the percent of births to teenage mothers has remained stable over time.

[2] In particular, I am interested in estimating the biological risk associated with age. Other age-associated problems with pregnancy and conception such as infertility and miscarriage receive considerable attention, but I do not address them in this paper. These outcomes are topics for future research. Instead, I focus on the cost of caring for infants in poor health, which arguably exceeds the costs arising from age-related pregnancy and conception problems.

To circumvent the selection problems associated with the endogeneity of age at childbirth, I use a new panel data set of over a half million mothers to examine changes in the health outcomes of children born to the same woman at different ages. The sample consists of mothers with two or more births in Texas between 1989 and 2001 and is one of the largest panel data sets ever assembled for births in the United States.[3] Infant health outcomes are based on reports from birth certificates, avoiding problems of recall error or biased reporting in retrospective data. Using these data, I estimate a generalized fixed-effects model that controls for any permanent differences in maternal characteristics, while relaxing some of the strong restrictions imposed by a simple first-difference estimator.[4]

This study provides three key improvements over existing studies (Rosenzweig (1986) and Rosenzweig and Wolpin (1995)) that use panel data to estimate the effect of maternal age. First, the Texas data set is far larger than samples used in previous literature.[5] This is especially important because existing studies suffer from a lack of power that makes it difficult to draw strong conclusions about the contribution of maternal age to infant health. Second, the quality and detail of the infant health outcomes in this data set are arguably superior to the information available in other samples. Third, the estimation model I use is more general than those adopted in the past. In particular, I allow the effect of age to vary by birth order, an important consideration suggested by the biomedical literature and confirmed by the data.

Using this new data set and estimation method, I find that old and young mothers face significantly higher risks of adverse birth outcomes. When compared to women aged 26 to 29, women over age 35 have approximately a 30 percent higher risk of a preterm delivery, a 57 percent higher incidence of an infant death, and a 27 percent increased risk of bearing a child with an abnormal condition.[6] Women younger than 18 have a 28 percent higher risk of a premature birth relative to 26 to 29 year olds. These estimates of the impact of maternal age are different than those derived from simple cross-sectional comparisons. In

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[3] Time and cost prevent me from creating a similar panel data set for other states. I utilized Texas data due to its availability from previous research (Royer, 2003).

[4] I will later refer to the first-difference estimator as a fixed-effects estimator and the generalized fixed-effects estimator as a correlated random-effects estimator, as is standard in the literature (Jakubson (1991) and Ashenfelter and Zimmerman (1997)).

[5] In contrast, Rosenzweig and Wolpin (1995) use a sample of 1,600 mothers from the National Longitudinal Survey of Youth.

[6] I describe these abnormal conditions in Appendix A.

particular, longitudinal estimates of the effect of relatively young ages are *smaller* than the cross-sectional estimates, while the longitudinal estimates of the effect of relatively older ages are *larger*. This pattern is consistent with the difference in observable maternal characteristics: young mothers tend to have characteristics associated with worse infant health outcomes while the opposite is true for older women.

Although panel data methods for inferring the effect of maternal age are appealing, they suffer from some potential problems. One is the endogeneity of the decision to have a second child. If a parent's decision to have an additional child is related to the health of the first child, panel-based methods possibly lead to biased estimates. I deal with this issue using a conventional selection correction procedure, a reweighting method, and estimation on a subset of mothers with high fertility rates whose childbearing decisions appear to be less affected by the health of the first child. Another potential source of bias is selective abortion. If a mother learns that her fetus is unhealthy, she may decide to seek an abortion. To address this concern, I estimate the age effects for mothers who do not receive an amniocentesis or ultrasound. If anything, these robustness checks suggest that I may be understating the effects of age for older women and overstating them for younger women.

This paper proceeds as follows. Section II reviews the maternal age literature. Section III describes the empirical methodology. I follow with a description of the constructed panel data set in Section IV and present the main estimation results in Section V. I perform several robustness checks in Section VI, decompose the age effects in Section VII, and estimate the economic costs of teenage and delayed childbearing in Section VIII. Section IX concludes.

II. A Sketch of the Literature on the Effects of Maternal Age on Birth Outcomes

A burgeoning literature has documented that the incidence rates for many adverse birth outcomes are higher for women at the ends of the age spectrum. Yet there remains intense debate about whether this association is causal. In this section, I summarize the existing studies that examine the links between maternal age and infant health.

This area of research can be categorized into two strands. The first strand describes the biological mechanisms by which a mother's age affects her ability to bring a healthy baby fetus to term. This research provides a sense of why one might believe that age is an important determinant of infant health. The second strand encompasses epidemiological and economic studies, which quantify the relationship between maternal age and infant health.

A. *How Might Maternal Age Affect Birth Outcomes?*

Biologically speaking, early or late childbearing may be detrimental to the health of the fetus because of impaired functioning of a woman's reproductive system (Ananth et al., 1996). If a woman is either too young or too old, her uterus and cervix may be unable to sustain a normal pregnancy. As Fraser et al. (1995) write, "immaturity of the uterine or cervical blood supply may predispose teenage mothers to subclinical infection, an increase in prostaglandin production, and a consequent increase in the incidence of preterm delivery." Also, since a teenage woman is "biologically immature" (Strobino et al., 1995), the needs of her developing body compete with the demands of the fetus. Thus, she may be incapable of providing the fetus with adequate nutrients.

For an older woman, changes in the circulatory system are important but less serious than the declining quality of her eggs as she ages. Studies of in vitro fertilized embryos suggest that the majority of a woman's eggs are chromosomally abnormal by the age of 40 (Velde and Pearson, 2002; Gianaroli et al., 1999; Wells and Delhanty, 2000).[7] As a result, women who hope to conceive in their late thirties may be unsuccessful because these defective eggs are not viable. Even if the fetus is carried to term, the infant may suffer from a chromosomal abnormality such as Down's syndrome.[8]

Older women also face circulatory difficulties, as do younger women. To test whether birth-related problems among older women are due solely to a woman's eggs rather than a combination of circulatory and

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[7] These clinical studies are subject to selection biases. For instance, the study population in Gianaroli et al. (1999) consists of patients who were classified as having a low predicted probability of carrying a fetus to term. To the degree that their aging is different from other women, these findings may overstate the percentage of eggs that are chromosomally abnormal.

[8] Owing to these deficient eggs, infertility problems and rates of spontaneous abortion are also elevated among older women.

egg deficiencies, Pellicer et al. (1995) randomly assigned eggs to women of varying ages by means of in vitro fertilization. They discovered that there was no difference in the success of implantation between older and younger mothers, but that older women (over 40) had a lower probability of carrying the fetus to term. They concluded that the reason for this elevated miscarriage rate is that among older women, the arrangement of blood vessels neighboring the uterus is defective.

Another obstacle to a healthy pregnancy, especially among older women who have given birth earlier, is parity (i.e., birth order). Due to damage to the reproductive system from earlier pregnancies, a multiparous woman may experience circulatory problems, which can affect the health of her unborn child. For instance, a past pregnancy may harm the blood vessels where the placenta is attached (Goplerud, 1986). This can lead to placenta previa, a severe condition where the placenta blocks the opening to the birth canal. The risks of placenta previa include uterine bleeding, preterm birth, infection, and birth defects. As both parity and maternal age influence the circulatory capabilities of the reproductive system, the interaction of maternal age and parity is likely to be biologically important.[9]

Beyond these elements, however, the existing biomedical research on the effects of age on birth outcomes is hazy. Ideally this literature would allow one to separate infant health outcomes into two categories: those affected by maternal age and those not. But with the exception of chromosomal abnormalities, miscarriage, and conception – all of which appear to be clearly affected by age – little is known. Part of the reason for this dearth of evidence is that the causes of some outcomes (e.g., preterm delivery) are largely unknown (Buekens and Klebanoff, 2001).

B. Quantifying the Effect of Maternal Age on Birth Outcomes

To evaluate whether the changes in a woman's reproductive system throughout her life cycle affect infant health, researchers have focused primarily on cross-sectional comparisons. Although the methodologies used in these studies are similar, the findings are not. For example, Fraser et al. (1995) find that teenage mothers have higher risks of premature and low birthweight births than do women in their

[9] However, studies have found it difficult to disentangle the role of parity from age because parity and age move in tandem. Mechanically, a woman's age for her second birth will be higher than that for her first birth.

twenties. On the other hand, the results of Strobino et al. (1995) suggest the opposite. After controlling for confounding factors such as poverty and minority status, which Fraser et al. (1995) omit, their results reveal no statistical difference in birthweight between teenage mothers and mothers in their mid-20's. Likewise, at the other end of age spectrum, age is of disputed importance. Using data on first births in Sweden, Cnattingius et al. (1992) conclude that older women face a higher probability of having an infant in poor health, but Berkowitz et al. (1990) do not.

There are several plausible reasons for these discrepancies across studies. First, since the age at which a mother gives birth is closely associated with other determinants of infant health, the estimated age effects are sensitive to the choice of control variables. Most previous studies rely on data from birth records, which provide minimal background information. Using the National Longitudinal Survey of Youth, which collects a richer set of demographic and economic data than do birth record data, Geronimus and Korenman (1993) and Strobino et al. (1995) observe that the apparent detrimental effects of early childbearing disappear after accounting for family background.[10] In addition, several studies suffer from overcontrolling: the authors account for the medical risk factors of the mother, such as diabetes and hypertension. Since one of the mechanisms by which age affects birth outcomes is through the health of the mother, accounting for her health status may understate the total age effect.

Another source of the apparently contradictory cross-sectional results is sample selection. To remove bias stemming from unobserved and observed differences in maternal characteristics that are correlated with age at birth, most authors limit their estimation sample to a homogenous population. But because these samples are highly selected and unlikely to be representative of all mothers, the external validity of these results is questionable. For example, Fraser et al. (1995) restrict their sample to white adolescent mothers in Utah; the family upbringing of these mothers is not typical of the average teenage mother. Due to such sample restrictions, which limit the sample size and the number of births to women either early or late in their childbearing years, the cross-sectional regression estimates are often imprecise.

[10] Although the National Longitudinal Survey of Youth (NLSY) data have more extensive background information, the infant health data are less comprehensive than the birth record data. The infant health information in the NLSY is also susceptible to retrospective biases.

As an alternative to the cross-sectional approach, Rosenzweig (1986) and Rosenzweig and Wolpin (1995) use several within-family comparisons (e.g., cousins and siblings) to estimate how age affects infant health. Rather than analyzing the age effects themselves, these authors investigate primarily how resources are allocated within a family. As I will discuss in detail in Section III, they consider how the dynamic behavior of families biases fixed-effects estimates of the effect of maternal age on birth outcomes.

These longitudinal studies have four primary limitations. First, Rosenzweig and Wolpin (1995) focus on the effects of teenage childbearing rather than the effects of childbearing in general. Second, their instrumental variables strategy for the fixed-effects model may be invalid. Third, the outcomes they study – gestation, birthweight, and fetal growth – are not universally meaningful. That is, one is usually concerned only if gestation or birthweight is below a certain threshold. Fourth, their estimated effects are imprecisely measured because the number of observations is small.

This paper overcomes many of the problems inherent in the existing cross-sectional and longitudinal studies of maternal age. To control for unobserved heterogeneity across mothers, I use a panel data set of mothers with two or more births constructed from the universe of births in Texas. Since the match rate of mothers across births is high, this study does not suffer from the sample size problems that plague earlier studies. Furthermore, I improve upon prior literature by more carefully modeling the roles and interaction of age and parity. The medical literature suggests that these interactions are important.

III. Empirical Strategy

As highlighted earlier, the simple cross-sectional relationship between maternal age and infant health is potentially misleading because a mother's age is associated with other unobservable and observable factors that also influence birth outcomes. As a result, it is difficult to isolate the direct effect of maternal age on birth outcomes. In an "ideal," if impractical, world, one would measure this by comparing outcomes across women randomly assigned (without their foreknowledge) to give birth at different ages.

In the "non-ideal" world, there are two possible approaches to solve this endogeneity problem – panel data and instrumental variables. In this context, however, the panel data approach is preferable to instrumental variables. It is improbable that one could find an instrument that affects maternal age but is unrelated to infant health. First, the unadjusted risks for many adverse outcomes (e.g., preterm birth and infant mortality) are higher only for women at either end of the reproductive spectrum. Thus, a valid instrumental variable procedure rests on the availability of exogenous variables that affect the childbearing decisions of these women. Second, several possible instruments – business cycle conditions, the gap between cessation of birth control and conception, and the age at start of menses – are all potentially related to birth outcomes. As far as I am aware, no one has found a useful instrumental variable to estimate the effect of age on birth outcomes.

In this section, I discuss several panel data approaches to estimating the age gradient, starting with a fixed-effects specification. This model restricts the age effects to be independent of parity, which can lead to serious biases. I subsequently consider a less restrictive correlated random-effects model, of which the fixed-effects model is a special case.

A. *Fixed-Effects Estimation*

I begin with a simple relationship between maternal age and birth outcomes:

(1) $$y_{ij} = \alpha + f(a_{ij})\beta + \boldsymbol{x}_{ij}\delta + \mu_i + \varepsilon_{ij}$$

where y_{ij} is the outcome for birth j of mother i, $f(a_{ij})$ is a vector such that $f(a_{ij})\beta$ is a flexible function of the mother's age which allows for a nonlinear effect of age,[11] and $\boldsymbol{x}_{ij}$ is a vector of observable characteristics.[12] The error term in equation (1) is composed of two parts: a mother-specific component μ_i and an infant-specific component ε_{ij}. This mother fixed effect μ_i captures time-invariant, unobservable characteristics of

[11] For example, if the relationship between age and birth outcomes is quadratic, $f(a_{ij})$ would be $[a_{ij} \quad a_{ij}^2]$. Later, I will express $f(a_{ij})$ as a vector of age dummies.

[12] I will assume throughout this paper that β does not vary across time. It is possible that the effect of age varies over time as medical technology improves. But as Velde and Pearson (2002) point out, the ability of technology to reverse reproductive problems associated with aging is overstated. Thus, the assumption that β does not vary across time may be relatively innocuous. Moreover, since I only examine births between 1989 and 2001, birth technologies are somewhat fixed relative to looking at a longer time period. In the regression specifications, I do include year dummies for the year of birth of the infant to capture changes in medical technologies over time.

the mother that are related to y_{ij}. These characteristics include her family background, her desire for healthy children, her ability, and her genetic health endowment.

As is well-known, least squares estimates of β will be biased if the mother fixed effect μ_i is correlated with $f(a_{ij})$. Since the age at which a woman gives birth is not random and is related to unobservable characteristics such as her earnings potential, such bias is likely. The direction of this bias depends on the correlation between the error term, $\mu_i + \varepsilon_{ij}$, and the age of the mother. If y_{ij} denotes a bad infant outcome, μ_i is arguably above average for teenage mothers.[13] In this case, the OLS estimate of the age effect for teenage pregnancies will be upward-biased. On the other hand, as illustrated in Figure 3, women who have their first child near the end of their childbearing years tend to have more education. If the same pattern holds for unobserved maternal characteristics, such as ability or earnings potential, the correlation between late childbearing and the error term will be positive, leading to a downward bias in the age effect for older mothers.

With a sample in which each mother has more than one birth, I can remove the bias resulting from the omitted factor μ_i by differencing equation (1) across first and second births as follows:

(2) $$y_{i2} - y_{i1} = (f(a_{i2}) - f(a_{i1}))\beta + (\boldsymbol{x}_{i2} - \boldsymbol{x}_{i1})\delta + (\varepsilon_{i2} - \varepsilon_{i1})\,.$$

$\hat{\beta}$ will be a consistent estimate of the age effects if $Cov((\varepsilon_{i2} - \varepsilon_{i1}), (f(a_{i2}) - f(a_{i1}))) = 0$. It is possible that this covariance assumption does not hold. For example, as a woman grows older, she may adopt more healthy behaviors to insure the health of her unborn child.

B. Problems with the Fixed-Effects Model

Rosenzweig (1986) and Rosenzweig and Wolpin (1995) criticize the fixed-effects approach and argue that such an analysis will be biased because parents respond to the idiosyncratic shocks of previous births. To illustrate this, Rosenzweig (1986) develops a sequential model of childbearing and shows that under parental utility-maximizing behavior, the shock to the birth outcome of the first child affects the resources

[13] Low socioeconomic status women have a high μ_i because y_{ij} is a bad health outcome.

devoted to the second child, implying that $Cov(\varepsilon_{i1}, f(a_{i2})) \neq 0$ and $Cov(\varepsilon_{i1}, \boldsymbol{x}_{i2}) \neq 0$. As a point of clarification, these inputs are meant to be interpreted broadly and to include behavioral inputs such as age. In addition, due to informational constraints, Rosenzweig contends that the idiosyncratic shock to any given birth outcome is unknown before that birth. That is, $Cov(\varepsilon_{i1}, f(a_{i1})) = 0$ and $Cov(\varepsilon_{i2}, f(a_{i2})) = 0$.[14] Thus, under these assumptions and Rosenzweig's additional assumption that age has a linear effect on birth outcomes,[15]

(3) $$Cov(a_{i2} - a_{i1}, \varepsilon_{i2} - \varepsilon_{i1}) = -Cov(a_{i2}, \varepsilon_{i1}) \neq 0.$$

Given this argument, the fixed-effects method (estimation of equation (2)) leads to biased and inconsistent estimates of the age effects because of this non-zero covariance. Parents who experience a bad first birth shock (i.e., large ε_{i1}) might delay the birth of a second child $(Cov(a_{i2}, \varepsilon_{i1}) > 0)$, and the fixed-effect estimate is likely biased downward.

To solve this problem and get a consistent estimate of the effect of age, Rosenzweig proposes using the characteristics from the first birth, $f(a_{i1})$ and $\boldsymbol{x}_{i1}$, as instruments for the differences in inputs across births, $f(a_{i2})$ - $f(a_{i1})$ and $\boldsymbol{x}_{i2}$ - $\boldsymbol{x}_{i1}$. In this model, this instrument set is valid because of the informational constraints assumed above: the error term for the kth birth outcome is uncorrelated with the inputs for the first through kth births. This implies that

(4) $$Cov(\varepsilon_{i2} - \varepsilon_{i1}, a_{i1}) = Cov(\varepsilon_{i2}, a_{i1}) - Cov(\varepsilon_{i1}, a_{i1}) = 0.$$

Under a less restrictive model, the validity of these instruments is questionable. Looking back at equation (1), I assumed that the effects of age and other covariates did not vary across births. This model is over-identified since one can relax these restrictions and still estimate the longitudinal model (as Chamberlain (1982) observed). If I allow the coefficients in equation (1) to differ across births, then the analog to equation (2) is

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[14] One potential problem with this is that there is an implicit assumption of perfect information, or that people know how their behaviors will affect the birth outcomes of their children. Scientifically, relatively little is known about the determinants of poor birth outcomes.
[15] To be clear, Rosenzweig (1986) does not assume a linear relationship between birth outcomes and age. He instead estimates a log-log specification. Therefore, to be consistent with his model, one should interpret y_{ij} as the log of the birth outcome and a_{ij} as the log of maternal age. This does not change the point of the argument above.

(5) $$y_{i2} - y_{i1} = (\alpha_2 - \alpha_1) + f(a_{i2})\beta_2 - f(a_{i1})\beta_1 + \boldsymbol{x}_{i2}\delta_2 - \boldsymbol{x}_{i1}\delta_1 + (\varepsilon_{i2} - \varepsilon_{i1}).$$

Differencing across births still removes the omitted variables bias due to the unobserved mother fixed-effect μ_i but allows the effects of age to differ by parity.[16]

If the model represented by equation (5) is the true data generating process, then maternal age at first birth enters the outcome equation directly, making it an invalid instrument for the difference in maternal age across births. Rewriting equation (5):

(6) $$\begin{aligned} y_{i2} - y_{i1} = (\alpha_2 - \alpha_1) + (f(a_{i2}) - f(a_{i1}))\beta_2 + f(a_{i1})(\beta_2 - \beta_1) + \\ (\boldsymbol{x}_{i2} - \boldsymbol{x}_{i1})\delta_2 + \boldsymbol{x}_{i1}(\delta_2 - \delta_1) + (\varepsilon_{i2} - \varepsilon_{i1}). \end{aligned}$$

Note that equation (6) implies that the fixed-effects model (2) potentially suffers from omitted variables bias via the exclusion of $f(a_{i1})$ and $\boldsymbol{x}_{i1}$. Appendix B discusses this omitted variables bias further.[17]

C. Correlated Random-Effects Model

i. Two-Birth Model

Given the potential problems with the fixed-effects approach, I instead estimate a more general model that overcomes some of these limitations. Consider the following general structural model for two-birth mothers:

(7) $$y_{i1} = \theta_{10} + f(a_{i1})\theta_{11} + f(a_{i2})\theta_{12} + \boldsymbol{x}_{i1}\theta_{13} + \boldsymbol{x}_{i2}\theta_{14} + \mu_i + \varepsilon_{i1}$$

(8) $$y_{i2} = \theta_{20} + f(a_{i1})\theta_{21} + f(a_{i2})\theta_{22} + \boldsymbol{x}_{i1}\theta_{23} + \boldsymbol{x}_{i2}\theta_{24} + \mu_i + \varepsilon_{i2}$$

where $f(a_{ij})$ and $\boldsymbol{x}_{ij}$ are assumed to be strictly exogenous conditional on μ_i.[18] That is,

(9) $$E(\varepsilon_{ij} \mid f(a_{ij}), \boldsymbol{x}_{ij}, \mu_i) = 0, \;\; j = 1, 2.$$

Later in this section I will show that further restrictions must be placed on the parameters of this model to make it estimable.

[16] The model represented by equation (5) nests the fixed-effects model as a special case (i.e., where $\beta_1=\beta_2$ and $\delta_1=\delta_2$). By testing whether $\beta_1=\beta_2$ and $\delta_1=\delta_2$, one can test a crucial assumption of the fixed-effects model.
[17] In Appendix B, I also demonstrate that the fixed-effects estimate of the age profile need not be a weighted average of the correlated random-effects model estimates of the first and second birth age profiles.
[18] The intercepts in these equations allow for fixed differences in birth outcomes by parity.

Each of the coefficients in equations (7) and (8) has a causal interpretation.[19] This model differs from equation (1) in that the effects of age and the other inputs are allowed to vary across first and second births and that the birth inputs for past and future births are permitted to affect the current outcome. As before, the unobserved mother fixed effect μ_i is potentially correlated with the age of the mother as well as with the other inputs. To model this correlation, I follow Chamberlain's (1982) approach and decompose the mother fixed effect into its linear projection onto the explanatory variables for the two births and a residual:[20]

$$\mu_i = \phi_0 + f(a_{i1})\phi_1 + f(a_{i2})\phi_2 + \boldsymbol{x}_{i1}\phi_3 + \boldsymbol{x}_{i2}\phi_4 + \eta_i . \tag{10}$$

By definition, the error term in equation (10) is uncorrelated with $f(a_{i1})$, $f(a_{i2})$, $\boldsymbol{x}_{i1}$, and $\boldsymbol{x}_{i2}$. Plugging equation (10) into equations (7) and (8) yields the following reduced-form equations:

$$y_{i1} = \pi_{10} + f(a_{i1})\pi_{11} + f(a_{i2})\pi_{12} + \boldsymbol{x}_{i1}\pi_{13} + \boldsymbol{x}_{i2}\pi_{14} + \xi_{i1} \tag{11}$$

$$y_{i2} = \pi_{20} + f(a_{i1})\pi_{21} + f(a_{i2})\pi_{22} + \boldsymbol{x}_{i1}\pi_{23} + \boldsymbol{x}_{i2}\pi_{24} + \xi_{i2} \tag{12}$$

where

$$\begin{array}{ll}
\pi_{10} = \theta_{10} + \phi_0 & \pi_{20} = \theta_{20} + \phi_0 \\
\pi_{11} = \theta_{11} + \phi_1 & \pi_{21} = \theta_{21} + \phi_1 \\
\pi_{12} = \theta_{12} + \phi_2 & \pi_{22} = \theta_{22} + \phi_2 \\
\pi_{13} = \theta_{13} + \phi_3 & \pi_{23} = \theta_{23} + \phi_3 \\
\pi_{14} = \theta_{14} + \phi_4 & \pi_{24} = \theta_{24} + \phi_4 \\
\xi_{i1} = \eta_i + \varepsilon_{i1} & \xi_{i2} = \eta_i + \varepsilon_{i2}
\end{array}$$

The system of equations defined by (11) and (12) is not identified since there are fifteen structural parameters but only ten reduced-form coefficients.[21] To estimate this system of equations, I therefore impose the following four restrictions:[22]

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[19] Since the inputs for the second birth occur after the first birth outcome, it seems logical to assume that θ_{12} and θ_{14} are zero. These coefficients could be non-zero if a woman makes all of her childbearing decisions prior to the birth of her first child. For example, suppose that a woman decides to have two children before the age of twenty. This imposes a constraint on the ages at which the mother has her first and second child. Thus, it is plausible that her age at the second birth affects the infant health of her first child. However, individuals are unlikely to be so forward-looking, so it is reasonable to believe θ_{12} and θ_{14} are zero.

[20] This can be done if μ_i and all of the explanatory variables have finite second moments (Wooldridge, 2002). The second moment condition insures consistency of the ϕ's.

[21] To be precise, the number of parameters depends on the dimensions of $f(a_{i1})$, $f(a_{i2})$, $\boldsymbol{x}_{i1}$, and $\boldsymbol{x}_{i2}$. Therefore, the numbers presented in the text represent the minimum number of structural and reduced-form parameters.

[22] Under these restrictions, all parameters of the model are identified except ϕ_0, θ_{10}, and θ_{20}, which are not parameters of interest.

$$
\theta_{12} = 0 \\
\theta_{14} = 0 \\
\theta_{21} = 0 \\
\theta_{23} = 0 \tag{13}
$$

Referring back to equations (7) and (8), these first two restrictions allow neither the mother's age at the second birth nor other second birth inputs to have a causal effect on the first birth outcome. These assumptions are reasonable since the second birth must follow the first birth and the timing of the second birth is presumably unknown at the time of the first birth. The last two restrictions are more debatable. They imply that inputs from previous births do not affect the current birth outcome. If, as argued in the public health literature (Khoshnood et al., 1998), the interval between the first and second births affects the second birth outcome,[23] then this assumption is violated. As will be shown later, however, these last two assumptions can be relaxed, and I can still identify the second-birth age profile but not the first-birth age profile.

Ignoring these concerns for the moment, and substituting these four restrictions into equations (11) and (12) gives:

$$y_{i1} = \pi_{10} + f(a_{i1})\pi_{11} + f(a_{i2})\pi_{12} + \boldsymbol{x}_{i1}\pi_{13} + \boldsymbol{x}_{i2}\pi_{14} + \xi_{i1} \tag{14}$$

$$y_{i2} = \pi_{20} + f(a_{i1})\pi_{21} + f(a_{i2})\pi_{22} + \boldsymbol{x}_{i1}\pi_{23} + \boldsymbol{x}_{i2}\pi_{24} + \xi_{i2} \tag{15}$$

where

$$
\begin{array}{ll}
\pi_{10} = \theta_{10} + \phi_0 & \pi_{20} = \theta_{20} + \phi_0 \\
\pi_{11} = \theta_{11} + \phi_1 & \pi_{21} = \phi_1 \\
\pi_{12} = \phi_2 & \pi_{22} = \theta_{22} + \phi_2 \\
\pi_{13} = \theta_{13} + \phi_3 & \pi_{23} = \phi_3 \\
\pi_{14} = \phi_4 & \pi_{24} = \theta_{24} + \phi_4 \\
\xi_{i1} = \eta_i + \varepsilon_{i1} & \xi_{i2} = \eta_i + \varepsilon_{i2}
\end{array}
$$

and all parameters other than ϕ_0, θ_{10}, and θ_{11},[24] which are not of direct interest, are exactly identified. Details on how to estimate this system of equations are in Appendix C.

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[23] The correlation between the interpregnancy interval and the risk of an adverse birth outcome could be driven primarily by selection. In particular, low socioeconomic status women have short birth intervals. Controlling for the unobservable, time-invariant characteristics of the mother may remove this selection bias.

[24] Although I cannot identify θ_{10} and θ_{11}, I can identify their difference (θ_{11} - θ_{10}).

The identification of the age effects in this model is complicated by two factors: the functional form of *f*(*a*) and the absence of the fixed-effects restriction that the coefficients on the inputs for the first and second birth be identical. To understand how the age effects are identified in this model, it helps to transform it into a difference-in-differences model. The main insight of this transformation is that the second-birth age effect is identified from comparing observationally-identical women who had their first birth at the same age but their second at different ages. Performing the opposite comparison identifies the first birth age effect. To see this, without loss of generality, suppose *f*(*a*) is a set of two age interval dummy variables, one for births to women between the ages of 12 and 25 and the other for those to women older than 25.[25] Substituting this functional form restriction in for *f*(*a*), one can rewrite equations (14) and (15) as:

$$(16)\qquad y_{i1} = (\theta_{10} + \phi_0) + D^1_{i1}(\theta_{11} + \phi_1) + D^1_{i2}\phi_2 + \boldsymbol{x}_{i1}(\theta_{13} + \phi_3) + \boldsymbol{x}_{i2}\phi_4 + \eta_i + \varepsilon_{i1}$$

$$(17)\qquad y_{i2} = (\theta_{20} + \phi_0) + D^1_{i1}\phi_1 + D^1_{i2}(\theta_{22} + \phi_2) + \boldsymbol{x}_{i1}\phi_3 + \boldsymbol{x}_{i2}(\theta_{24} + \phi_4) + \eta_i + \varepsilon_{i2}$$

where D^1_{ij} is a dummy variable for the first age category for individual i for birth j. The D^2_{ij} dummy variable is omitted. Taking the difference between equations (16) and (17) gives

$$(18)\qquad y_{i2} - y_{i1} = (\theta_{20} - \theta_{10}) - D^1_{i1}\theta_{11} + D^1_{i2}\theta_{22} - \boldsymbol{x}_{i1}\theta_{13} + \boldsymbol{x}_{i2}\theta_{24} + \varepsilon_{i2} - \varepsilon_{i1}.$$

In this setting, a simple way of identifying θ_{11}, the first birth age profile, is to compare the differences in outcomes across births for women with identical $\boldsymbol{x}_1$ and $\boldsymbol{x}_2$ and who had their second birth after age 25, but some of whom had their first birth before age 25 and some of whom had it after age 25. That is, dropping subscripts i for visual clarity,

$$(19)\qquad \begin{aligned} &E(y_2 - y_1 \mid D^1_1 = 0, D^1_2 = 1, \boldsymbol{x}_1, \boldsymbol{x}_2) - E(y_2 - y_1 \mid D^1_1 = 1, D^1_2 = 1, \boldsymbol{x}_1, \boldsymbol{x}_2) \\ &\quad = [\theta_{20} - \theta_{10} + \theta_{22} - \boldsymbol{x}_1\theta_{13} + \boldsymbol{x}_2\theta_{24}] - [\theta_{20} - \theta_{10} - \theta_{11} + \theta_{22} - \boldsymbol{x}_1\theta_{13} + \boldsymbol{x}_2\theta_{24}] \\ &\quad = \theta_{11} \end{aligned}$$

Similarly, one can estimate the second-birth age effect by comparing the difference in outcomes for observationally-identical mothers who have their first birth within the same age interval but differ with respect to the age at which they had their second birth.

[25] In my analysis, I divide the age distribution into seven categories rather than two.

As discussed previously, a shortcoming of this correlated random-effects model is that it assumes that previous birth inputs do not affect the current birth outcome: θ_{21}=0 and θ_{23}=0. This assumption is unlikely to hold. For example, a woman may receive nutritional advice when she attends prenatal care during her first pregnancy, which helps her second pregnancy. As alluded to earlier, however, I can relax these assumptions and still identify the age profile of the second birth. If θ_{21} and θ_{23} are non-zero, the two-birth reduced-form model is as follows:

(20)
$$\begin{aligned} y_{i1} = &(\theta_{10} + \phi_0) + f(a_{i1})(\theta_{11} + \phi_1) + f(a_{i2})\phi_2 \\ &+ \boldsymbol{x}_{i1}(\theta_{13} + \phi_3) + \boldsymbol{x}_{i2}\phi_4 + \eta_i + \varepsilon_{i1} \end{aligned}$$

(21)
$$\begin{aligned} y_{i2} = &(\theta_{20} + \phi_0) + f(a_{i1})(\theta_{21} + \phi_1) + f(a_{i2})(\theta_{22} + \phi_2) \\ &+ \boldsymbol{x}_{i1}(\theta_{23} + \phi_3) + \boldsymbol{x}_{i2}(\theta_{24} + \phi_4) + \eta_i + \varepsilon_{i2}. \end{aligned}$$

Differencing these two equations yields:

(22) $$y_{i2} - y_{i1} = (\theta_{20} - \theta_{10}) + f(a_{i1})(\theta_{21} - \theta_{11}) + f(a_{i2})\theta_{22} + \boldsymbol{x}_{i1}(\theta_{23} - \theta_{13}) + \boldsymbol{x}_{i2}\theta_{24} + \varepsilon_{i2} - \varepsilon_{i1}.$$

Note that the effect of age at the second birth, θ_{22}, remains identified but that the effect of age at the first birth, θ_{11}, does not. The functional form of this regression is identical to that for the more-restrictive correlated random-effects model defined by equations (14) and (15), but the interpretation of the estimated coefficients is different. If this model is correct, only the age profile for the second birth is identifiable.

ii. Three-Birth Model

The preceding structural model can be easily extended to cases where each mother has three (or more) births. The overidentification of the three-birth model allows me to test the validity of the identifying assumptions analogous to (13). In particular, the three-birth structural model is

(23) $$y_{ij} = \theta_{j0} + f(a_{i1})\theta_{j1} + f(a_{i2})\theta_{j2} + f(a_{i3})\theta_{j3} + \boldsymbol{x}_{i1}\theta_{j4} + \boldsymbol{x}_{i2}\theta_{j5} + \boldsymbol{x}_{i3}\theta_{j6} + \mu_i + \varepsilon_{ij} \quad j = 1,2,3$$

(24) $$\mu_i = \phi_0 + f(a_{i1})\phi_1 + f(a_{i2})\phi_2 + f(a_{i3})\phi_3 + \boldsymbol{x}_{i1}\phi_4 + \boldsymbol{x}_{i2}\phi_5 + \boldsymbol{x}_{i3}\phi_6 + \eta_i.$$

If I impose restrictions analogous to the earlier constraints (i.e., characteristics for other births do not influence the current birth outcome), the three reduced-form equations are of the following form

$$(25)\quad y_{ij} = \boldsymbol{\pi}_{j0} + f(a_{i1})\boldsymbol{\pi}_{j1} + f(a_{i2})\boldsymbol{\pi}_{j2} + f(a_{i3})\boldsymbol{\pi}_{j3} + \boldsymbol{x}_{i1}\boldsymbol{\pi}_{j4} + \boldsymbol{x}_{i2}\boldsymbol{\pi}_{j5} + \boldsymbol{x}_{i3}\boldsymbol{\pi}_{j6} + \xi_{ij} \quad j = 1,2,3$$

Appendix D provides the equations that relate the reduced-form parameters to the structural parameters. In this model, there are 21 reduced-form parameters but only 16 structural parameters, allowing a test of the identifying assumptions.[26]

Using mothers who have three births, I can also estimate a less restrictive model, one for which I allow both current and past birth inputs to affect the current birth outcome. The corresponding reduced-form equations are identical in form to equation (25), although the relationships between the reduced-form and structural parameters are different. Further details are in Appendix D. In this model, the effects of the first-birth inputs ($\boldsymbol{\theta}_{11}$ and $\boldsymbol{\theta}_{14}$) are not identified, but the effects of the inputs for other births are.

IV. Data

In this analysis, I use the 1989 to 2001 Texas linked birth and infant death files constructed by the Texas Department of Health.[27,28] These files cover the universe of all births occurring in Texas, approximately 300,000 per year. Records are composed of two parts: natality information collected from birth certificates and infant mortality information from the death certificate for those infants who died within the first year.[29] At birth, each mother and her health care provider complete a survey that includes questions on education, marital status, the delivery method, medical risk conditions, pregnancy history, prenatal care utilization, and birth outcomes.

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[26] Once again, the number of parameters depends on the dimensions of $f(a_{i1})$, $f(a_{i2})$, $\boldsymbol{x}_{i1}$, and $\boldsymbol{x}_{i2}$. Therefore, the numbers presented in the text represent the minimum number of structural and reduced-form parameters.

[27] To address the concern of generalizability, it should be noted that the health of infants born in Texas patterns that of infants in the rest of the United States.

[28] Prior to 1989, the exact birth date of the mother was not collected on the birth certificate. Since I use the mother's birth date to match siblings, I can only use births that occurred in 1989 or later.

[29] From 1990 to 1999, between 93 and 98 percent of annual infant deaths in Texas were linked to a birth certificate (Texas Department of Health, 2001).

Under special permission from the Texas Department of Health, I have acquired access to the parents' names as recorded on the birth certificate. Using a mother's name and her exact birth date, I match births to the same woman over time, thus creating a panel data set of mothers.[30]

To assemble the panel, I first create a stacked data set of all Texas births sorted by a mother's first and maiden names and her birth date. For each unique name and birth date combination, I include the mother in my sample if four criteria are met. First, since the birth certificate lists the parity of the birth, I only include mothers whose more recent births report a higher parity than earlier births. Second, I keep only those women with at least two births and a gapless birth history (i.e., if the first and third birth are observed, so is the second birth). Third, I exclude mothers who have had at least one plural birth.[31] Fourth, I drop mothers with more than one child assigned to the same parity value. Of the 4,366,316 births occurring in Texas between 1989 and 2001, 1,890,494 births (43.3 percent) meet these criteria. In Appendix E, I itemize how many observations were dropped from the matched sample for not meeting each of the selection rules above and describe the composition of the matched sample.

Imperfect matches will introduce bias in the regression estimates. If a match is incorrect, then differencing across births will not remove the omitted variables bias stemming from the mother-specific fixed effect. To evaluate the success of this matching, I check whether time-invariant characteristics of the mother (e.g., race) are consistent across births. Table 1 reveals that false matches are minimal. Each number represents the percentage of mothers who report a consistent cross-birth value for that characteristic. For instance, 98.3 percent of the matched mothers report the same birth state across all births. The high percentages in Table 1 indicate that measurement error resulting from imperfect matching will be low.

To further evaluate the success of the matching algorithm, I estimate the percentage of second births that are unmatched (Table 2). Second births are unmatched to first births for several reasons – migration into

---

[30] I have also acquired mothers' scrambled Social Security numbers, but these numbers are only available starting in 1994 and are not a requirement on the birth certificate. For matches based on names and birth dates, the Social Security number matches 96.3 percent of the time for mothers I observe giving birth twice. Most of the non-matches appear to be typographical errors: 66.9 percent of the non-matches match for eight of the nine digits of the Social Security number.

[31] I restrict the matched-mothers sample to women who have had only singleton births for two reasons: the age effects are identified from differences in the mother's age across births and plurality is an independent risk factor for adverse birth outcomes.

Texas between the first and second birth, typographical and reporting errors in the data, or the occurrence of a first birth before 1989, the first year of the data. Since the date of last live birth is recorded on the birth certificate, I can restrict the data to mothers who have their first birth in 1989 or later. Among this population, the matching algorithm performs decently: over 70 percent of second births are matched. According to estimates from the 2000 Census, mobility appears to be responsible for most of the missed matches. Using calculations of mobility rates of mothers living in Texas at the time of the 2000 Census, I estimate that about 75 percent of the missed matches result from interstate migration.[32]

Table 3 provides descriptive means for key demographic variables for the samples of one-birth, two-birth, and three-birth mothers.[33] As used here, one-birth mothers are mothers whose name and birth date are unique within the sample of 1989 to 2001 Texas births. Two-birth mothers are mothers whose first and second births are matched, and three-birth mothers are mothers whose first through third births are matched.[34] Two-birth mothers are positively selected in relation to one- and three-birth mothers. In particular, they are more educated and more likely to be married. Some of these differences could be the result of systematic mismatching, an issue I will address in section VI.

For these same mothers, Table 4 presents descriptive statistics for birth-related variables. As expected, given their demographic characteristics, two-birth mothers have better birth outcomes than one- or three-birth mothers. For example, the infant mortality rate for first births of two-birth mothers is 3.78, but the respective rates for one- and three-birth mothers are 4.68 and 6.60.[35,36]

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[32] To derive this, I first calculate the number of mothers living in Texas who have only two children, both of who are under five and one of whom is less than one year old using the 2000 Census. Among this population, I calculate the percentage of mothers who moved to Texas in the last five years. This estimate is 18.6 percent. Then, I choose a comparable population of women from the Texas birth records. In particular, I select all women who had a second birth between April 1999 and March 2000 (up to one year prior to the date of the Census) and a first birth after April 1995. Among this group, the percentage of second births that are unmatched to a first birth is 24.8 percent. Therefore, the estimate of the percentage of nonmatches due to migration into Texas is 18.6/24.8, or 75 percent.

[33] I do not present the results from testing the differences in the means in these descriptive tables because the sample sizes are large enough that even small differences in the means are statistically significant.

[34] Although I refer to these samples as one-birth, two-birth, and three-birth mothers, this does not mean that these mothers are in fact one-birth, two-birth, and three-birth mothers. For instance, the one-birth mother sample consists of both true one-birth mothers and mothers who have or will have additional children. Likewise, some mothers in the two-birth sample may have more than two births.

[35] The improvement of birth outcomes with higher parities may seem contradictory to the hypothesis that outcomes deteriorate with age. However, in the Texas data, the average two-birth mother has her two births during the set of ages where the risk of an adverse birth outcome declines with age.

V. Estimates of the Effect of Maternal Age on Birth Outcomes

In this section, I present the main set of estimation results of the correlated random-effects model using two-birth mothers. For comparison, I also include cross-sectional and fixed-effects estimates of the age profile.

Throughout the analysis, I focus on the effect of maternal age on the probability of a premature birth, although I discuss later how maternal age affects other birth outcomes such as infant mortality and low birthweight. I concentrate on prematurity for several reasons. First, prematurity is one of the most common adverse birth outcomes: nearly ten percent of births are premature. Second, there are high medical costs associated with premature births. In 1998, initial hospital care costs for a preterm infant totaled $58,000 relative to $4,300 for a full-term infant (March of Dimes, 2003a). Third, maternal age and prematurity are strongly correlated, as shown in Figure 2. Fourth, the consequences of prematurity appear to be long-term. In a meta-analysis examining the link between preterm delivery and later outcomes, Bhutta et al. (2002) found that children who were born premature had lower test scores and were more likely to have an attention-deficit disorder. Fifth, prematurity is generally believed to be an important negative birth outcome. For example, the March of Dimes recently invested $75 million in hopes of reducing the incidence of prematurity (March of Dimes, 2003a). The president of the March of Dimes, Dr. Jennifer L. Howse, argued for passage of new congressional funding for prematurity research by stating, "Premature birth is one of the most common, serious and costly problems facing America's infants, responsible for about half of all infant hospitalization charges" (March of Dimes, 2003c).

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[36] The infant mortality rates in Table 4 are lower than the national average. Infant mortality rates for the entire population of Texas births, matched or unmatched, are more consistent with the national trends. For the overall population of Texas births, the infant mortality rates falls from 8.37 in 1989 to 5.41 in 2001. This decline is comparable to the decrease for the United States over this same period. Why are the infant mortality rates so much lower for the matched sample? Infant mortality rates computed from matching birth records to infant death records are lower than rates computed by dividing the number of infant deaths by the number of live births. However, matching cannot completely explain this phenomenon. The quality of data is arguably better among the matched sample, implying that matched births are more likely to be linked to an infant death record than are unmatched births. One plausible explanation is that an infant death discourages families from having an additional child. If this is true, though, one should not see differences in infant mortality rates for unmatched and matched second births. But once again, the infant mortality rate among the unmatched sample is higher than among the matched sample. Hence, the differences in death rates between the matched and unmatched samples may suggest that, as seen by their observable characteristics, matched mothers are a selected population of women.

A. Age and Prematurity

Table 5 presents estimates of the age profile for the sample of two-birth mothers (i.e., mothers whom I only observe twice in the Texas natality files) for premature births.[37,38,39] Columns (1) through (4) give the cross-sectional estimates, and columns (5) through (8) show the longitudinal estimates. To allow for a non-linear age profile, the estimated models include maternal age group dummies separately by parity.[40,41] The excluded category is the 26 to 29 year-old age group. Hence, each entry in Table 5 represents the excess risk of a premature birth for that age category, relative to the risk for a similar woman between 26 and 29 years old. To get a sense of the shape of these age profiles, I plot the estimated age dummy coefficients from Table 5 in the top panel of Figure 4. Recall from the discussion of the empirical model that the estimates of the second-birth age profile are more robust than those for the first-birth age profile because they are impervious to the inclusion of prior birth covariates as controls.

The first two columns of Table 5 reveal the same pattern seen earlier in Figure 2: the incidence of preterm delivery is highest among younger and older women. The next two columns confirm that this association between age and prematurity is confounded by other determinants of birth outcomes, which are correlated with a mother's age when she gives birth. Columns (3) and (4) include controls for maternal

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[37] When I estimate these models for all mothers observed giving birth in Texas at least two times, the results are similar as discussed later in Section VI. I choose to estimate the model for mothers observed giving birth twice because the two-birth mother is the modal mother.

[38] Although I match 1989 to 2001 births, I restrict the estimation sample to births occurring between 1991 and 2001 because two of the included regressors – maternal drinking and smoking behavior – are only available starting in 1991.

[39] I do not present the estimated age dummy coefficients for the sample of unmatched mothers in Table 5 but later I discuss the differences in the age profiles for unmatched and matched mothers in the robustness check section.

[40] Note that amongst women who give their first and second birth within the same age interval, the first and second birth age dummies are identical. Thus, the age effects are identified from women who give their first birth within a different age interval than their second birth. Since the age intervals span several years, one may be worried that the age effects are identified off of women with a relatively long interpregnancy interval. The average time between births for two-birth mothers is 3.26 years. For the sample of two-birth mothers, the age interval differs across births for 60 percent of them. I chose to use age intervals rather than a complete set of age dummies to reduce the standard errors of the point estimates. If the age intervals are smaller, the age gradients are nearly identical except for younger mothers. For these mothers, the slope of the age profile is steeper but imprecisely measured with the shorter age intervals.

[41] To choose the age dummies, I examined the maternal age distribution and selected cutoffs such that the age groups at the tails of the distribution had a sufficient number of observations to estimate the age effects precisely.

education, marital status, paternal absence, maternal smoking and drinking behavior, maternal race/ethnicity, residential zip code, and birth year of infant.[42]

As anticipated by the earlier discussion of the potential bias in the OLS estimates of the age profile, younger women have a *lower* risk of a premature birth than the unadjusted age profile suggests, but the effect of age on prematurity does not disappear completely. For example, a woman under 18 has a 2.4 percentage point higher risk of a preterm first birth than a similar 26 to 29 year old woman. This risk is less than half the estimated excess risk from the unadjusted cross-sectional model.

On the other hand, the probability of a premature birth for older women *rises* after controlling for covariates. As discussed earlier, a natural explanation for this empirical finding is that the attributes of older mothers (e.g., educational attainment) are negatively correlated with poor infant health outcomes and bias the unadjusted cross-sectional age profile for these mothers downward. For first births, the excess risk for a woman in her late thirties is nearly identical to that for a teenager.

Given the potential biases that may remain in the cross-sectional estimates due to omitted unobserved factors, columns (5), (6a), and (6b) present fixed-effects and correlated random-effects estimates of the age profiles without controls for covariates. The correlated random-effects estimates are very similar to the adjusted cross-sectional estimates. The only exception is the excess risk estimate for second-birth mothers younger than 18, which is lower than the corresponding cross-sectional estimate. Selection probably drives this difference. A woman who gives birth for the second time as a teenager is likely more negatively selected than a similarly-aged first-birth mother. The covariates in the cross-sectional regression, however, may not adequately soak up this selection bias, which may be driven by unobservable characteristics. In contrast, the correlated random-effects model may more sufficiently control for this selection bias, as it removes the source of bias arising from a mother's unobservable, time-invariant characteristics.

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[42] I do not control for maternal behaviors that are endogenously related to age. These behaviors include prenatal care and obstetric procedures (e.g., ultrasound). Older mothers may recognize that they are at higher risk for a poor birth outcome and seek such care. Thus, regression models that include controls for these behaviors may be overcontrolled in the sense that these behaviors are a function of the age-associated risks. Since these behaviors arguably mitigate the effect of age on infant health, these overcontrollled regressions will lead to downward-biased estimates of the age profile.

Unlike the correlated random-effects estimates, the fixed-effects results suggest that age does not pose a risk for older women. This highlights one of the potential problems with the fixed-effects model, for which identification is more directly tied to the interval between births. However, the interpregnancy interval is an independent risk factor for an adverse pregnancy outcome. In particular, if there is a short time span between births, a mother may experience depleted nutritional resources, hormonal problems, and postpartum stress (Stephansson et al., 2003).

To see how estimation of the age effect in the fixed-effects model is related to the interval between births, suppose, for simplicity, that age has a linear effect on birth outcomes. In the first-difference specification of the fixed-effects model, the influence of age for both the first and second birth (restricted to be identical in this model) is precisely estimated from the gap between births. On the other hand, under this same scenario, the second-birth age effect in the correlated random-effects model is identified from the interpregnancy interval after controlling for age at first birth. Once I include the delay between births as an additional control in the fixed-effects model, the estimates resemble the correlated random-effects results (not shown). The correlated random-effects estimates are insensitive to the inclusion of the interpregnancy interval as a control. It is also worth noting that the naïve assumption that the fixed-effects estimates of the age profile are a weighted average of the correlated random-effects estimates does not hold. I discuss why this does not occur in further detail in Appendix B.

Columns (7), (8a), and (8b) display the fixed-effects and correlated random-effects estimates after controlling for the same time-varying covariates as in columns (3) and (4). Both sets of estimates are essentially unchanged by the addition of these controls. Although this evidence alone is not conclusive, it suggests that the bias in the correlated random-effects estimates due to time-varying unobserved factors, such as income, is small.

From these estimates one can draw inferences about the age that minimizes the likelihood of a premature first birth.[43] The biased unadjusted cross-sectional estimates indicate that for first births, the women with the lowest incidence of prematurity are mothers between the ages of 26 and 29. For second

[43] Other factors such as household resources and time constraints also affect the optimal age of childbearing.

births, this group is slightly older – between 30 and 33. However, as judged by the adjusted correlated random-effects estimates, the "best" age for first and second births is between 22 and 25.

One of the primary motivations for estimating the correlated random-effects model is that it allows for the age gradients to differ by parity. As evidenced in Figure 4, this appears to be an important consideration for both the controlled and uncontrolled specifications. The effect of age is generally smaller for first births than second births (except in the extreme tails) but increases slightly more quickly with age. An F-test (not reported) rejects the equivalence of the first-birth and second-birth age profiles.

Prematurity is a binary outcome and the effects of age may not be best estimated with a linear model. To see if the estimates in Table 5 are sensitive to this linearity assumption, I estimate the correlated random-effects model as a bivariate probit model; the estimated age profiles are nearly identical (Table 6).[44] See Appendix C for a detailed discussion of how to estimate the system of equations as a bivariate probit model.

Estimates of the effects of the non-age covariates are independently worthy of mention, especially given the scarcity of large-scale panel data studies in this area of research. These estimates also provide a sense of the magnitude of the age effects. Table 7 presents the coefficient estimates for the covariates included in the Table 5 models. Most striking is the strong and significant effect of the father's absence. This variable is inferred from the birth certificate data by the lack of his birth date and education level. If the father is not present, an infant has an approximately one-percentage point higher likelihood of being born prematurely. Paternal presence may proxy for household resources or reduce a pregnant mother's stress. Recent studies (Catalano, 2003) suggest that maternal hormones introduced by stress influence a fetus' health. The effect of paternal presence is surprising given that a mother's marital status has little influence.[45] Of the time-varying covariates, a mother's drinking behavior during pregnancy is the strongest predictor of a

[44] It should be noted that in the linear case, rewriting the fixed effect as its linear projection onto the explanatory variables and an orthogonal residual is not restrictive. In the non-linear case, this is not true. There are two important assumptions underlying this projection. First, the expected value of the fixed effect conditional on the explanatory variables is linear and second, that the error term in the projection equation has a normal distribution (Chamberlain, 1984).

[45] The difference in the estimated effect for marital status and the estimated effect for presence of father could be due to measurement error. In Texas, between 1989 and 1993, mothers who reported paternal information on the birth certificate were considered married. Starting in 1994, the birth certificate specifically asked mothers whether they were married (Ventura et al., 2000). When I restrict the sample of two-birth mothers to those who had their first birth after 1993, the coefficients for marital status and paternal presence are comparable. Hence, the potential bias due to measurement error seems to be unimportant.

premature birth. The influence of drinking is roughly equivalent to the effect of delaying childbirth from the 26 to 29 to the 34 to 37 age range. Surprisingly, smoking behavior appears unimportant.

The estimated impact of education on prematurity follows an unusual pattern. In the cross-section, higher levels of education are associated with reductions in the probability of an early delivery. The correlated random-effects estimates of the returns to education do not exhibit this monotonic pattern for first births. Instead the risk of a premature first birth for a college-educated woman is nearly identical to that for an otherwise similar woman with fewer than nine years of schooling.[46] These estimates of the effect of education should be interpreted with caution, as 12.7 percent of mothers report lower education levels at their second birth than at their first birth. These results are consistent with the findings of McCrary and Royer (2003) who use school-age entry laws to identify the effect of education on birth outcomes and find that education does not affect birth outcomes.

B. Looking Beyond Prematurity: The Age Profiles for Other Birth-Related Outcomes

Thus far, I have only examined the age-related risks of prematurity, but there are other birth outcomes for which age may play an important role. The age profiles for other outcomes may differ from that for prematurity since there are several plausible mechanisms by which age affects infant health. For example, the aging of a woman's eggs leads to chromosomal abnormalities among mothers aged 35 and older, whereas pregnancy complications are more often due to the malfunctioning of a woman's reproductive system. In this subsection, I explore the effects of age on six additional outcomes: infant death, low birthweight, newborn abnormal conditions, pregnancy-associated hypertension, congenital anomalies, and labor and delivery complications.[47] There is persistent debate about the association of age and the likelihood of these outcomes (Berkowitz et al., 1990; Strobino et al., 1995).

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[46] As a matter of comparison to Currie and Moretti (2002), I estimated Currie and Moretti's panel data model specification using their sample restrictions. They constructed a pseudo panel of births using the national Vital Statistics Detailed Natality Files to study the effect of education on birth outcomes. My estimate of the effect of education, as measured in years, on the likelihood of a premature birth is substantially smaller than theirs and is opposite in sign (0.0000537 compared to –0.0032).

[47] Another outcome is APGAR score, which is based on a baby's reflexes, color, muscle tone, heart rate, and respiratory effort at birth. APGAR scores are not collected on Texas birth certificates.

In Appendix Tables A1 and A2, I present cross-sectional and panel data estimates with infant death as the outcome variable.[48] These tables are analogous to Tables 5 and 7. As is the case with prematurity, the unadjusted cross-sectional relationship between infant death and maternal age is U-shaped (see Figure 4). This pattern is less distinct when covariates are included. Both the adjusted cross-sectional and correlated random-effects estimates indicate that infant death is more common among older mothers; for younger women, the age-associated risk is less evident.

One consistent pattern across the specifications is the effect of maternal education. An increase from 12 to 16 or more years of schooling reduces the probability of infant death by approximately 50 percent, roughly equivalent to the reduction in risk from not drinking. This result, combined with the finding that education has no impact on prematurity, suggests that there are several pathways by which education affects birth outcomes. Infant mortality reflects not only the infant's health at birth but also the care the infant receives after birth. Conditional on the birth outcome, a more-educated mother may be more knowledgeable or may have better access to resources to care for a sick infant.

Appendix Tables A3 and A4 display the estimates for low birthweight, another birth outcome more prevalent among younger and older women.[49] One again sees that the adjusted correlated random-effects estimates are qualitatively consistent with the adjusted cross-sectional estimates (see Figure 4). The results suggest that women at the tails of the age distribution have a higher risk of a low birthweight birth, although these risks are imprecisely measured. Smoking and drinking behaviors increase the probability of a low birthweight birth and have a larger effect than age does.

The unadjusted probability of a newborn abnormal condition does not exhibit the U-shaped pattern observed for prematurity, low birthweight, and infant death; it increases monotonically with age (Figure 5).[50] Abnormal conditions include anemia, assisted ventilation, and fetal alcohol syndrome.[51] The most common

[48] One potential concern about these results is that because infant death is a rare binary outcome, the point estimates from the linear correlated random-effects model may be misleading. When I estimate the model as a bivariate probit system, the point estimates are nearly identical.
[49] It is debatable whether birthweight is a good measure of infant health. In a study of twins, Almond et al. (2002) find that birthweight is a poor predictor of infant mortality.
[50] This monotonic pattern is present among the sample of first and second births. For second births, the incidence is slightly higher for women under 18 years old than those between 18 and 21.
[51] These conditions are described further in Appendix A.

condition is assisted ventilation under 30 minutes, which occurs for approximately 2 percent of all births.[52] For newborn abnormal conditions, the cross-sectional and correlated random-effects estimates are relatively similar (Appendix Tables A5 and A6). According to the correlated random-effects estimates, women aged 30 and older are at a one to three percentage point higher risk of delivering an infant with an abnormal condition. Maternal smoking is nearly as important as maternal age in predicting newborn abnormal conditions.

For this outcome, the correlated random-effects estimates are very sensitive to the addition of controls. In particular, while the unadjusted estimates indicate that age is strongly associated with the probability of a newborn abnormal condition, the adjusted profile suggests a much weaker relationship. The difference appears to be due to a sharp increase in the incidence of these conditions over time. Among the two-birth mother sample, 2.6 percent of 1991 births are classified as having a newborn abnormal condition. By 2001, this figure rose to 4.3 percent. When I include the birth year of the infant in the regression in addition to controls for maternal age, the estimates resemble those in columns (8a-b) (not shown). The difference between the adjusted and unadjusted estimates in columns (6a-b) and (8a-b) could also signal that time-varying omitted variables may potentially be contaminating the correlated random-effects estimates.

Appendix Tables A7 and A8 present estimates for pregnancy-associated hypertension. The prevalence of hypertension increases with a mother's age, particularly for first births (see Figure 5). The differences across parity for hypertension suggest the importance of the less-restrictive correlated random-effects model in this context. Within the same population of mothers, the probability of hypertension for first births is nearly twice that for second births; this is consistent with the hypothesis that a woman's stress is lower for her second birth since the birthing process is no longer a new experience. The cross-sectional estimates of the effect of age on hypertension are essentially invariant to the inclusion of controls; both imply that the risk increases with age. On the other hand, the correlated random-effects estimates change when controls are added. The unadjusted estimates map out a steeper age profile than seen from the cross-

[52] On the birth certificate, there is a write-in category for abnormal newborn conditions, which I have excluded. When the other conditions category is added, the percent of births with an abnormal condition doubles and the age profiles are slightly more convex. For approximately 16 percent of infants with an unknown abnormal condition, prematurity is the listed condition. Since these other conditions are not categorized, it is difficult to know what they represent.

sectional model, but the slope of the adjusted correlated random-effects age profile is slight. None of the adjusted age coefficients are statistically different from zero. Once again, this discrepancy may reflect the omission of factors that are correlated with age. Covariates such as education, smoking, drinking, and marital status do not appear to affect the incidence of pregnancy-associated hypertension (Appendix Table A8).

The next birth outcome I consider is congenital anomalies (birth defects), which are widely cited as a potential consequence of giving birth at a later age. Some of the more commonly-known congenital anomalies include club foot and Down's syndrome.[53,54] Appendix Table A9 and Figure 5 confirm the conventional wisdom about the age-related prevalence of these conditions: the unadjusted age gradient is flat and rises sharply for the upper tail of the age distribution.[55] Women aged 38 and older have nearly a 40 percent higher probability of having an infant with a congenital anomaly than do women as old as 34 to 37.

In the cross-section, this upward-sloped age profile remains after I include controls. In contrast, the correlated random-effects estimates, both unadjusted and adjusted, exhibit no such gradient. In fact, the estimated age coefficients for older women are negative rather than positive. Since congenital anomalies are so rare, the coefficients estimates tend to be imprecise. Therefore, the point estimates should be regarded with caution. The covariate that exhibits the strongest association with congenital anomalies is drinking (Appendix Table A10). A mother who drank during pregnancy has about a 50 percent higher risk of giving birth to an infant with a congenital anomaly.

Of all the outcomes mentioned thus far, labor and delivery complications are by far the most common. Fifteen percent of all births in the two-birth sample experienced at least one such complication.[56]

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[53] For further details on congenital anomalies, see Appendix A.
[54] Since there is strong biomedical and empirical evidence that advanced maternal age is causally related to chromosomal abnormalities, it would be interesting to examine the effect of age on these abnormalities. Unfortunately, since these abnormalities are so rare, such analysis provides little insight on this relationship. For my two-birth sample, I only observe 226 Down's syndrome births.
[55] Since these anomalies are rare, I also estimate the correlated random-effects model as a bivariate probit. The point estimates are essentially unchanged.
[56] The most frequent complications were the presence of moderate/heavy meconium (6 percent of births) and dysfunctional labor (6 percent of births). Meconium is dark fetal fecal matter usually discharged at the time of birth. An infant who inhales meconium is susceptible to meconium aspiration syndrome, a severe pulmonary disease (Cunningham et al., 2001). Dysfunctional labor occurs when labor is stalled. If labor is dysfunctional, there is an increased likelihood of a neonatal infection. Dysfunctional labor is also associated with increased maternal and fetal mortality (Cunningham et al., 2001). I exclude the "other labor/delivery complication" write-in category from the calculation of whether the mother had labor or delivery complications. Fourteen percent of all births have a

As shown in Appendix Table A11 and Figure 5, the occurrence of these complications varies by both age and parity. The unadjusted and adjusted cross-sectional results suggest that older mothers, especially first-time mothers, have higher risks of labor/delivery complications. The risk of complications, such as dysfunctional labor, may decline with parity because first birth may have helped the body's reproductive and muscular system adjust to the birthing process. For first births, the unadjusted age profile is upward-sloping: women aged 38 and older are 57 percent more likely to have a complication than are teenage women. But these differences disappear in the correlated random-effects model (see Figure 5).[57] However, according to all models, smoking increases the likelihood of complications (Appendix Table A12).

These findings lead to the question of why the correlated random-effects and adjusted cross-sectional estimates are qualitatively similar (accounting for sampling error) for all outcomes other than labor and delivery complications. Many of these complications, particularly dysfunctional labor, may be related to a woman's body structure and the capabilities of her uterus and cervix (Cunningham et al., 2001). These characteristics are unobservable to the researcher and are, to some degree, fixed over time. Moreover, such factors may be less important for other birth-related outcomes. It could be argued that the functioning of a woman's uterus and cervix affects preterm delivery, but not to the degree that they influence labor and delivery complications. The March of Dimes (2003b) asserts that the four leading causes of premature births are infection, bleeding, stress, and overstretching of the uterus. A woman's susceptibility to these factors can change over time, whereas her body structure is effectively fixed.

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labor/delivery complication classified in this category. Ten percent of write-in responses for the other category are for conditions relating to the umbilical cord, such as the cord being wrapped around the infant's head. If included, the estimated age profile is steeper. The complete list of labor and delivery complications can be found in Appendix A.

[57] It is plausible that the reason that the estimated risks of complication do not rise with age is due to the frequency of cesarean section among older women. A woman of advanced maternal age, who is perceived to be at high risk, may receive a cesarean section as a precautionary measure. This use of cesarean section is consistent with the findings in Appendix Table A11. To distinguish the importance of the preemptive use of cesarean section, I have also estimated this model conditioning on women who do not receive a cesarean. The results exhibit the same general pattern: older women do not have an increased risk of complications. However, the sample of older mothers who do not receive a Cesarean section may be positively selected in terms of their underlying risk of complications. A better test may be to study the relationship between age and the risk of complications and Cesarean section as a single outcome.

VI. Robustness of Findings

In this section, I examine the robustness of the results for two-birth mothers with respect to alternative specifications and different estimation samples. First, I compare the age profiles for two-birth mothers with those for other mothers. Second, I investigate how selective abortion affects the estimated age profiles. Finally, I estimate the age profiles for three-birth mothers under two different specifications of the correlated random-effects model to test how misspecification may bias my results.

A. How Representative is the Two-Birth Mothers Sample?

A concern about the panel data approach is external validity. In this subsection, I explore this issue of whether the two-birth mother results generalize to other groups of mothers. To begin, I compare the earlier results for two-birth mothers with those for mothers whom I observe at least twice. Since these results are similar, I conclude that the main age profile estimates are good representations of the first- and second-birth age gradients for mothers with.more than two births.

I then concentrate on whether the age profile estimates generalize for the remaining mothers – one-birth mothers. First, I contrast the first-birth age profiles for one- and two-birth mothers and find some differences in these gradients. I discuss several reasons for this. One is the endogenous decision to have an additional child. If a woman's first birth is bad, she may decide not to bear another child. Based on a mean reversion argument, I argue that this type of selection bias would cause the estimated age effects for older women, as estimated from the two-birth model, to be downward-biased. I address this type of selection in three different ways – by employing a conventional selection correction, by assigning weights to the two-birth sample to make it more similar to the one-birth sample, and by examining a population with high fertility for whom the deterrence effect is less operative. These results suggest that the direction of the bias is opposite of that predicted from mean reversion. As such, I conclude that this deterrence bias is not a major concern.

A second source of selection bias relates to matching. In particular, if mobility is endogenous, then two-birth mothers are a selected sample of true two-birth mothers. To distinguish the importance of this issue, I look exclusively at Texas natives, a less mobile sample. I find that the correlated random-effects estimates for this population are consistent with that for the overall sample. In light of all these results, I conclude that these sources of selection bias, if they exist, lead to an overestimate of the influence of age for younger mothers and an underestimate for older mothers.

i. Generalizability to Other Groups of Mothers

These concerns aside, the earlier results (specifically, Table 5) are applicable to a substantial proportion of women: the modal mother in both the 1998 June CPS and 1990 Census has given birth twice (Table 8). Nearly one-fourth of Texas women past their childbearing years had exactly two live births. To check if the results for two-birth mothers vary when mothers with three or more births are considered, I repeat the estimation using the sample of all mothers with at least two births. The estimated age coefficients for the prematurity regressions are presented in Appendix Table A13 and are very similar to those in Table 5.

In terms of generalizability, the other population of interest is the sample of one-birth mothers for which the panel data model is inestimable. A simple approach to test the external validity of the main results is to compare first birth outcomes of mothers who have one birth to first birth outcomes of mothers who have two births. This is impossible with the constructed panel. First, I cannot compute the exact number of children born to each woman. Migration, name changes, and typographical errors in names and birth dates prevent me from identifying all of a particular woman's births. Second, mothers who had not completed their fertility by the end of 2001 would be incorrectly classified.

As an alternative, I compare first-birth age profiles across one- and two-birth mothers using first births occurring between 1991 and 1995.[58] Thus, I remove a large portion of the sample that eventually has another birth; misclassification will be due mostly to migration and data errors rather than incomplete fertility. The results of this exercise are reported in Table 9. Columns (1) through (2) display the unadjusted cross-sectional estimates of the effect of age on prematurity for first births for mothers who are observed with one or two births in the Texas panel. Columns (3) through (4) present the parallel adjusted estimates. The age dummy coefficients for the adjusted cross-sectional regressions are plotted in Figure 6.

[58] The percent of first births that are matched is constant across years for first births occurring before 1996 but falls in later years.

Both the unadjusted and adjusted age profiles for these groups of mothers are distinctly different, as indicated by the F-test results at the bottom of Table 9. Compared to one-birth mothers, the age gradient for two-birth mothers is steeper at the bottom tail of the age distribution but more gradual at the upper tail. However, the differential risk of prematurity for a teenager relative to a mother aged 26 to 29 is consistent across these two groups. If these differences across mothers were visible only in the unadjusted cross-sectional results, one might be comfortable that the correlated random-effect estimates for two-birth mothers, which are qualitatively similar to the adjusted cross-sectional results (see Table 5) are a good representation of the true age effect on the prematurity of the first birth.

Why are these age profiles different? Below I discuss two potential explanations. One is the endogeneity of the choice to bear an additional child and the other is the ability to match births.

ii. The Decision to Have a Second Child May Be Endogenous

A bad first pregnancy may deter a woman from having another child. This deterrence could account for the higher prevalence of preterm delivery among one-birth mothers. Figure 7 plots the percent of first births that are matched to a second birth by mother's age at first birth and by prematurity status. From this graph, it is clear that the matching rate declines with age and that at each age, the matching rate is lower for women with preterm first births. Even after controlling for a wide range of factors that could influence a woman's decision to have another child, such as her marital status and her age, women who have a preterm first birth have a statistically significant 3 to 4 percentage point lower probability of being matched to a second birth (regression results not shown). These results must only be taken as suggestive, since it is impossible to disentangle whether this difference is due to true deterrence or to matching problems.

In the presence of this discouragement effect, mothers who have more children experience better outcomes for their first birth than those who do not, and there is the typical mean reversion problem. In some sense, women who had a good first birth experienced a positive idiosyncratic shock; since the expected value of this shock is zero, their second birth will look worse relative to their first birth. Hence, for the matched sample, the upper tail of the age distribution will be an upward-biased estimate of the true age profile. At the other end of the age distribution, since the probability of having an additional child is less contingent on the outcome of the first birth, the potential for bias is small. In what follows, I find that the direction of the bias, apparently small, is the opposite of what this intuitive argument implies. Therefore, deterrence may not be an important source of selection bias.

To consider this selection bias more closely, I add a conventional selection correction into my original correlated random-effects model. In the first step of this selection correction, I estimate the probability of being matched for exactly two births using the samples of both one- and two-birth mothers. My exclusion restriction is based on the sex of the first child. Past studies have documented a link between a couple's behavior and the sex of their firstborn. For example, both Bedard and Deschênes (2003) and Dahl and Moretti (2003) find that if a married couple's firstborn is a girl, the marriage is more likely to end in divorce. In this case, one would predict that the probability of a second child is higher if the firstborn is a male because there is a higher likelihood that the marriage remains intact. Other studies predict the opposite. If parents have a gender preference for boys, as seen throughout much of the developing world (Leung, 1991), families with a firstborn male have a lower expected probability of a second child. Regardless of the mechanism, in the Texas data I observe that families with a firstborn son are more likely to have a second child. This effect is statistically significant with a t-ratio of 3.74.

Table 10 displays the selection-corrected correlated random-effects estimates for two-birth mothers. I include the estimated inverse Mills ratio from the selection equation. The estimated age profiles are consistent with the original results, before controlling for selection (see Table 5). Furthermore, in the new specification, the difference in the selection parameter across births – the coefficient on the inverse Mills ratio – is not significantly different from zero. This suggests that the differences observed earlier in the age profiles for one- and two-birth mothers are not driven by selection.

To address this potential selection bias in a different way, I estimate a weighted version of the basic correlated random-effects model. Using the samples of one- and two-birth mothers, I first match women on the basis of age, race, Hispanic origin, immigrant status, education, the presence of the father, and prematurity status of the first birth. I then assign weights to the two-birth mothers based on the relative proportions of one- to two-birth women in each cell. The weighted regression results are presented in Appendix Table A14. Figure 8 also shows the age profiles for the weighted and unweighted (from Table 5) regressions. After weighting, the age-associated risks are larger for older women and smaller for younger women, which, based on the comparison of the cross-sectional results for one- and two-birth mothers, matches the intuitive direction of the original results' bias but not the direction suggested by the mean reversion argument. However, considering the imprecision of the estimates, these differences are minor.

As a third and final way of assessing the magnitude of this potential selection bias, I estimate the age profiles for a group of women with high fertility rates: Hispanic mothers. The age-specific fertility rates for Hispanics exceed those for non-Hispanics (Hamilton et al., 2003). Among this population, the decision to have additional children may be more affected by tastes than by the outcomes of previous pregnancies. Differences in the matching probabilities based on the outcome of the first birth observed earlier (Figure 7) do persist for this sample (not shown), but the differences are smaller when conditioned on age. Hence, the selection bias inherent from the choice to have a second child may less important for this sample of mothers. In Table 11, I present regression estimates of the age profile for prematurity among Hispanic women. The point estimates are smaller for younger women and larger for older women than are those in the full sample of two-birth mothers (Table 5).[59] These differences are considerable for women who have delayed childbearing, but the standard errors of the coefficients are nearly three times the size of those for the complete sample. These findings confirm those for the earlier weighted exercise. Together, these three attempts to correct for any potential selection bias due to deterrence suggest that if the two-birth age profiles are biased, they are biased downward for older women and upward for younger women, but that any such bias is minimal.

[59] One of the puzzling findings in the public health literature (Rumbaut and Weeks (1996)) is that despite their economic disadvantage, immigrants have better birth outcomes than natives. The disparity in the results between the full sample and the Hispanic sample, of which 38 percent are immigrants, may reflect heterogeneity in the effect of age.

iii. *Ability to Match Births May Result in Selection Bias*

The second mentioned source of selection bias is non-random matching. The sample of two-birth mothers represents women for whom I observe two births – identified as the first and second births on the birth certificate. Similarly, one-birth mothers are those with only one observed birth (identified as the first). Yet many "one-birth mothers" may have children that I do not observe – either because the mother moved out of Texas, had a birth after 2001, or because of data errors. Given the matching rates for second births in Table 2, one might expect that approximately one-fourth of first births belonging to the one-birth mother sample are births to mothers with more than one child, most of this misclassification is likely due to mobility in light of the interstate migration patterns from the 2000 Census discussed earlier in Section IV.

As a first pass of examining the magnitude of selection bias due to mobility, I estimate the prematurity age profiles for second births to unmatched and matched mothers, restricting the sample to those women born in Texas who had their first live birth between 1991 and 2001. By selecting only Texas natives, I reduce the percentage of non-matches resulting from migration. According to the 2000 Census, approximately 18.6 percent of Texas mothers moved into the state between their first and second birth. For mothers born in Texas, this percentage drops to 3.9 percent.[60] Hence, the twelve percent missed match rate reported in Table 2 will be mostly driven by typographical errors. As seen in Table 12, the estimated cross-sectional age gradients for matched and unmatched Texas native mothers are alike. For the adjusted regression, the age profiles are statistically indistinguishable.[61] This suggests that among Texas natives the mobility-related selection bias may be small.

To reduce any bias due to selective migration between births, which may contaminate the correlated random-effects estimates, in Table 13 I present age profile estimates comparable to those in Table 5 but for

[60] These figures are computed using the sample of mothers living in Texas who have only two children under 5, one of whom is less than one year old, in the 2000 Census. Using this sample, I calculate the percentage of women who moved to Texas within the last five years. These calculations are overestimates of mobility rates between the first and second birth, as I cannot determine exactly when a mother moved. In particular, a mother may have moved to Texas before the birth of her two children or after the birth of her two children – both leading to overcounts of the number of women moving between the first and second birth. This is the best estimate of migration patterns between the first and second birth possible from the Census since the Census only has information on whether a mother has moved in the last five years.

[61] One caveat for not rejecting that the profiles are identical for Texas natives might be due to a loss of power when restricting the sample to Texas natives.

Texas natives. Here I again estimate the cross-sectional, fixed-effects, and correlated random-effects models. Table 14 presents estimates of the effect of the non-age covariates on the probability of a preterm birth for this Texas native sample. In comparison to the original results (Tables 5 and 7), the age profiles and the estimated effects of the non-age covariates are nearly identical. One minor difference is that among Texas natives, the age-associated risks of prematurity for women between 34 and 37 are slightly higher than those for the full population of two-birth mothers. The similarity of these results suggests that although the matching algorithm appears to result in a selected sample, this selection does not significantly bias the age profile of two-birth mothers.

B. Selective Abortion

All of the results presented thus far are conditional on the mother having a live birth, but not all conceptions end in a live birth. One remaining question is whether the age profiles are biased by selective abortion.[62] Suppose, for instance, that a woman decides to have an abortion because she learns that her fetus is unhealthy. If such selective abortions are common and the incidence of fetal health problems rises with maternal age, then the upper tail of the estimated age profiles could be biased downward by selection.

Regardless of the reason, if women who have abortions vary systematically from those who do not, then all (not just selective) abortions are a potential source of selection bias. In the following analysis, I focus exclusively on selective abortion because the bias associated with such abortions is likely larger than that for general abortions. First, selective abortion is a clear, direct threat to identification, whereas any randomly chosen abortion is less closely linked to fetal health. Second, the correlated random-effects model adjusts for time-invariant observable maternal characteristics, which may adequately control for systematic differences between women who seek an abortion and those who do not. However, if a woman's preference for

[62] A related issue is how the age profiles would change if all conceptions ended in a live birth. This question is not as relevant since some fetal deaths (e.g., miscarriages and stillbirths) are involuntary, not selective; thus, there is no reasonable counterfactual live-birth outcome. Instead, it may be more useful to examine the probability of a live birth by age as a separate outcome. An older woman may be equally worried about carrying her fetus to term and giving birth to a healthy infant. This is a topic for future research and will not be addressed here.

abortion changes over her lifecycle, the correlated random-effects model will still be biased. If the bias due to selective abortions is small, one may reasonably believe that the overall abortion bias is minor.

The Texas natality records do not allow me to address the selective abortion issue directly. Data on abortions are limited. In Texas, a fetal death certificate, which is nearly identical to a birth certificate, is only completed if the pregnancy lasts longer than twenty weeks. Since 98 percent of abortions occur before this cutoff, extensive abortion data are not available. Texas law requires abortion facilities to provide a report of each abortion performed, but the reporting requirements are minimal. Facilities need only supply the mother's year of birth, race, marital status, place of residency, and basic information about the abortion procedure. Hence, it is impossible to create a panel data set of all conceptions in Texas.[63]

Despite the limited abortion data, I can use aggregate abortion statistics for Texas to gain a sense of the frequency of abortion by age. If the selective abortion scenario holds, one would expect abortion rates to rise with age, since older women have a higher probability of carrying an unhealthy fetus. Figure 9, which plots the percent of pregnancies that end in abortion by the mother's age, supports this hypothesis.[64] For the oldest women, abortion rates increase monotonically with age.

Although Figure 9 is consistent with the selective abortion hypothesis, older women may have other reasons for seeking an abortion. For instance, when an older woman has an unplanned pregnancy, she may decide to have an abortion if she has already attained her desired family size. Survey evidence does suggest that most older women decide to have an abortion for reasons other than their own health or the health of their fetus. In 1987, the Alan Guttmacher Institute surveyed 1,900 women receiving abortions and asked them about the reasons contributing to their decision to abort. For women 30 and older, the four primary reasons for abortion were anxieties about life changes after having a baby, the inability to afford a child, the lack of desire to raise a child in a single-parent household, and satisfaction with their current family size (Torres and Forrest (1988)). Each of these reasons was reported by over fifty percent of the sample. In

[63] It is possible to add fetal deaths to the panel data set since the mother's name and birth date are reported on the fetal death certificate. I plan to acquire these data in the future. The number of reported fetal deaths is not high. In 1992, there were 320,714 births, 2,073 reported fetal deaths, and 87,230 abortions in Texas (Texas Department of Health, 1992).

[64] In this context, pregnancies are the sum of abortions and live births. Fetal deaths other than abortions (e.g., miscarriages and stillbirths) are not included in the pregnancy totals.

comparison, only 17 and 15 percent, respectively, claimed that their own health or the health of their fetus factored into their decision. As expected, the importance of these health-related concerns increased with age. However, even among the highest age ranges, these concerns were of lesser importance than the other four considerations. This finding may, however, be driven by the relative infrequency of fetal and maternal complications. As stated earlier, even the most common adverse health outcome considered in this analysis – pregnancy/delivery complications – occurred in only 17 percent of all births in the two-birth sample. Arguably the only outcome reflecting fetal abnormalities observable in utero (congenital anomalies) occurred in a minimal one percent of all births.

Given the data in the Texas birth records, there is an alternative means for testing the selective abortion theory. Since ultrasound and amniocentesis are the primary methods by which a woman learns about the development of her fetus, I can indirectly test whether the age profile is biased by selective abortion by examining the gradient for women who did not receive an amniocentesis or an ultrasound.[65] Such women presumably do not know the fetus' health. In Table 15, I estimate age profiles for this sample of mothers. The cross-sectional age profiles for the mothers who do not undergo amniocentesis or ultrasound are nearly identical to those for all mothers.[66] The age dummy coefficients for the upper tail of the maternal age distribution are larger than those for the full sample, but because most older mothers receive ultrasound or amniocentesis, the point estimates are imprecise. These results suggest that the estimates of excess risk for older women relative to women in their late twenties are biased downwards by at most 1 to 2 percentage points.

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[65] The frequency of ultrasound and amniocentesis may be underreported in the birth record data (Doria-Rose et al., 2003).

[66] My prior is that the bias due to selective abortion is higher for outcomes other than for prematurity. Outcomes such as Down's syndrome are more predictable during development. Even though a premature birth is fairly unpredictable, it is associated with conditions that can be detected from ultrasound. Vaginal bleeding is one such example. Because these problems are correlated with the risk of a preterm delivery, women who are more likely to have a premature birth may be more likely to abort. An arguably more interesting outcome is congenital anomalies. I do not present the results for this outcome because of the imprecision of the point estimates for the overall sample of two-birth mothers. When I condition on those mothers who did not receive ultrasound or amniocentesis, the risk of a congenital anomaly rises for second births to older mothers but remains negative for first births.

C. *Three-Birth Mother Sample*

The large size of the three-birth sample allows me to perform another specification check. Specifically, I estimate the effect of age on the probability of a premature birth using the two three-birth models outlined in equation (25).[67] The former is the more restrictive correlated random-effects model in which only current inputs are allowed to affect the infant health outcomes. In the latter, less restrictive model, both current and past inputs can affect outcomes. In what follows, I find that the estimation demands of this model are tremendous because of the multicollinearity of the age dummies. The age effects that are precisely estimated agree with the earlier estimates.

In the first three columns of Table 16, I present the regression estimates for the model in which I allow only the current birth inputs to affect the birth outcome. In the last two columns, I display the regression estimates for the less-restrictive model in which I include the previous birth inputs as controls.[68,69] Figure 10 plots these estimates.

For the more restrictive model, the estimated age profiles for the first and second birth are not consistent with the earlier findings. Neither exhibits the expected clear U-shaped pattern seen earlier. There is a curved relationship in both cases, but the risks do not increase monotonically with age for the older age groups. For the first birth, the coefficient estimate for the 30 to 33 age group is 0.013, whereas for the 34 to 37 category, the coefficient is a small and insignificant 0.001. However, the third-birth age gradient exhibits the same shape as seen for the first and second births of the two-birth sample.

When I estimate the less restrictive model by including the inputs of previous births as controls, the age profiles change. The second-birth age profile becomes nearly horizontal relative to the age profiles for the two-birth mothers, although the age effects at the tails of the age distribution are imprecisely measured.

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[67] I estimate the three-birth model as a restricted system of seemingly unrelated regressions.

[68] I exclude controls for maternal smoking and drinking behavior and residential zip code from the estimated regressions. The omission of these variables has a negligible effect on the estimated age coefficients in the two-birth mother sample. I exclude smoking and drinking as regressors because information on these behaviors is only available for 1991 and onward. By limiting the sample of 3-birth mothers to those who had their first birth 1991 or later, I lose nearly 25 percent of the 3-birth mother sample. With age dummies for the three births in each of the model equations a large sample is needed because of the potential multicollinearity problem. I omit residential zip code as a control for computational ease. Including dummy variables for the residential zip code complicates the estimation without changing the results.

[69] As discussed previously, the first-birth age effects are not identified in this model.

As with the more restrictive model, the estimated third-birth age profile resembles the U-shaped pattern from the original models. The only key difference between the two models is that in the less restrictive model, the risk associated with teenage childbearing is higher. The strong effect of previous teenage childbearing on subsequent birth outcomes may explain this difference.

There are several plausible explanations for the curious flattening of the first- and second-birth age profiles. First, many of the model estimates are imprecise and the true trend might be masked. Multicollinearity is a big concern in the three-birth model, as each equation includes three sets of age dummies (i.e., a separate set for the first, second, and third births). The unusual estimates, such as the second-birth age effect for women 38 and older, have large standard errors. The third-birth estimates, which follow the pattern predicted from the two-birth results, are more precise.[70] One future avenue is to collect additional data from a bigger state such as California.

A second potential explanation is that the age effects are heterogeneous across mothers. To distinguish whether the unusual patterns for first and second births are an artifact of the model or the sample, I estimate the original two-birth correlated random-effects model using the first and second births or the second and third births (not shown). Neither set of estimates is consistent with the two-birth mother estimates. Hence, the three-birth sample likely drives the atypical age profiles, but not much can be inferred from these results as the estimates have large sampling errors.

A third possible explanation for the aberrant three-birth results is selection. To control for potential sample selection bias in the two-birth model, I estimate a conventional two-sided selection model. This controls for selection from two births to three births in addition to the previously-considered selection from one birth to two births. I estimate two separate selection equations: first I model the probability that a mother is observed giving birth more than once, and then I model the decision to have a third child conditional on already having two children. As before, the exclusion restriction in the first selection equation is based on the sex of the firstborn. In the second equation, identification rests on the sex of the first two children. Angrist and Evans (1998) found that parents whose first two children are of the same gender are

[70] In an attempt to circumvent the multicollinearity problem, I replaced the age dummies with a flexible polynomial function of age. The shape of the age profiles remains the same.

more likely to have a third child, since parents have a desire for both a son and a daughter. This same pattern is found in the Texas data: the same sex variable has a strong influence on the probability of having a third child. The associated t-statistic is 28.09.

By assuming that the error terms in the selection and outcome equations follow a multivariate normal distribution, the estimation of selection-corrected correlated random-effects model amounts to including the inverse Mills ratios from both selection equations additively in the correlated random-effects model. These results for two-birth mothers are displayed in Table 17. The point estimates agree with the main estimates for two-birth mothers. Additionally, the coefficients on the selection terms are fairly precise zeros.

Overall, the age profiles for two-birth mothers are generally impervious to the robustness checks, suggesting that they may be good estimates of the true age profiles. The suggested direction of the bias, if it exists, is downward for older women and upward for younger women.

VII. What Does Maternal Age Represent?

The estimated age profiles are intended to capture the biological risks associated with maternal age. To get a sense of what the age effect estimates represent, I decompose the change in the estimated age effects from a restricted regression with no covariates, to an unrestricted regression, which includes a full set of covariates. Some of these covariates are a function of a mother's age (e.g., maternal risk factors) (Gelbach, 2003).[71,72] Suppose the restricted regression is of the following form:

(26) $$Y_i = X'_{1i}\beta_{1r} + \varepsilon_i.$$

In this scenario, X_{1i} represents the set of age dummies and β_{1r} is the effect of age on the birth outcome Y_i. If I then add covariates X_2 to the regression equation:

(27) $$Y_i = X'_{1i}\beta_{1u} + X'_{2i}\beta_2 + \varepsilon_i,$$

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[71] To derive this decomposition, one can use the Frisch-Waugh-Lovell theorem and general rules of matrix algebra.
[72] The unrestricted regression is an overcontrolled specification. As mentioned earlier, to capture the biological risks associated with age, one should not control for health-related factors directly associated with age. For instance, since the increasing prevalence of health conditions as people age reflects the biological risks of age, including controls for maternal risk factors may understate the true age effect.

$\hat{\delta} = \hat{\beta}_{1r} - \hat{\beta}_{1u}$ can be decomposed into its components,

(28)
$$\hat{\delta} = \sum_{k \in K} \hat{\delta}_k$$

where K is the set of covariates in X_2. In this equation, $\hat{\delta}_k$ is the projection coefficient vector from a regression of $X'_{2k}\hat{\beta}_{2k}$ on X_1, where $X'_{2k}\hat{\beta}_{2k}$ is the inner product of the kth variable in the X_2 matrix and the estimated coefficient vector on X_{2k} from the unrestricted regression. Following such an approach, Table 18 presents the estimated influence of each covariate on the age gradient, $\hat{\delta}_k$, for both the cross-sectional and correlated random-effects models. For the cross-sectional results, $\hat{\delta}_k$ represents both the factors underlying the adjusted age profile and the characteristics correlated with a mother's age that bias the cross-sectional relationship, as the restricted regression includes only age controls. On the other hand, since the correlated random-effects model removes the source of bias stemming from a mother's fixed characteristics, $\hat{\delta}_k$ does not capture this bias.

For the cross-sectional model, as expected, the age profile for older mothers captures the influence of both maternal risk factors and birth technologies, such as amniocentesis, fetal monitoring, and ultrasound. Maternal risk factors include health conditions such as diabetes, which develop as people age. For younger mothers, the unadjusted first-birth age profile is mainly a function of the number of prenatal care visits. For second births to teenagers, however, the number of previously terminated pregnancies explains nearly all of the elevated age-associated risk of a premature birth. This is probably a selection effect rather than a biological risk effect. As discussed earlier, women who have a second birth as a teenager are highly negatively selected. Furthermore, for second births, teenage pregnancy is highly correlated with the number of previously terminated pregnancies.

In the correlated random-effects model, the age profiles change only slightly following the inclusion of covariates into the regression. For older women, maternal medical risk factors and, most importantly, the birth year of the infant are the predominant factors altering the age profile.[73] As with the cross-sectional regression for first births, prenatal care visits heavily influence the age effects for younger mothers.

VIII. The Economic Costs of Maternal Age on Infant Health

Once one has estimates of the maternal age effects, it is possible to address the cost-related concerns mentioned earlier. One of the motivations of this paper was the remarkable evolution in the maternal age distribution over time. How has this shift impacted infant health costs? Using the age profile estimates from Table 5, the prematurity-related initial hospital care costs (March of Dimes, 2003a), and the age distribution of births, I estimate that this shift amounted to roughly $461 million in additional costs for first and second births.

I can also address how costs would change under other scenarios (Table 19). In particular, I consider how costs would decrease in two settings: (1) if all first and second births to women under 18 are postponed until they are between the ages of 18 and 21 (the "cost of teenage childbearing") and (2) if all first and second births to women 34 and older instead occurred when these mothers were between the ages of 30 and 33 (the "cost of delayed childbearing"). These cost estimates are underestimates of the long-run costs of prematurity, since they only capture initial hospital costs.[74]

The costs of teenage childbearing and delayed childbearing are not small, especially given the concern placed on other factors affecting infant health. Using the correlated random-effects estimates, one observes that delaying childbearing imposes over $200 million per year in additional prematurity-related health care costs. The cost of teenage childbearing is of the same magnitude. However, the unadjusted cross-sectional estimates overstate this teenage childbearing cost by nearly 50 percent. Taken together, nearly a half a billion dollars, or approximately 5 percent of United States public expenditures on maternal and child health (United

[73] This may be related to sample selection. In particular, all mothers who have a first birth late in the sample period must have their second birth shortly thereafter. Women with a short duration between pregnancies are negatively selected. Alternatively, since prematurity rates rose over the 1990's, the unadjusted age effects may partially pick up this trend. So it is not surprising that the inclusion of the birth year of the infant as a control has an effect on the estimated age profiles.

[74] They also represent partial equilibrium effects. By delaying childbirth, a woman may have fewer children. This effect would reduce health care costs.

States Census Bureau, 2000), are spent caring for premature infants born to young and old mothers. The magnitude of these costs may seem minor, but in comparison to the costs of smoking – often a target of public policy – they are not. Using the upper bound estimate of the effect of smoking on prematurity from Table 7, smoking imposes a cost of $88.8 million or 20 percent of the combined cost of early and delayed childbearing.[75]

These costs associated with delayed childbearing may be offset by the income a woman potentially gains by staying in the labor force during her prime childbearing years, when her age profile of earnings is upward-sloping. Yet, given the endogeneity of the timing of childbirth, it is difficult to estimate the increase in lifetime income due to such a delay. Miller (2003) finds that a one-year delay in fertility translates into a nine percent increase in lifetime earnings. However, the gains from increased earnings are arguably private, whereas infant health care costs are public.[76] Thus, there remains an appropriate concern about the costs imposed by the decision to delay childbirth.

IX. Conclusion

In this paper, I provide new evidence on the influence of maternal age on birth outcomes using a newly created, large-scale panel data set of mothers in Texas. Exploiting differences in sibling outcomes, I find that maternal age has an effect on a multitude of birth-related outcomes and that the age profiles differ by parity. For teenagers, an increase in age decreases the likelihood of a premature birth. A mother under 18 has a two-percentage point, or 25 percent, higher probability of having a preterm birth than does a similar 26 to 29 year old woman. Older women confront higher risks of premature birth, infant death, and having an infant born with abnormal conditions. Women 30 and older have a one to three percentage point increased

[75] This cost estimate may be understated if there is significant measurement error in maternal smoking rates. If smoking is mismeasured, this will result in underestimates of the effect of smoking on prematurity and of the number of smokers. However, to be cautious, I used the largest estimated smoking effect in my calculation.
[76] It should be noted that older women are often covered by private insurance, as they have higher income on average, so some of the health-related costs of delayed childbearing are private. But if older women with private health insurance postpone childbearing, the health costs resulting from their delay may raise overall private health insurance premiums. Thus, in some sense, even with private insurance, this postponement is a public cost.

risk of a preterm birth, representing a 12 to 31 percentage increase in risk, and a 25 to 100 percent elevated probability of an infant death.

Knowing the magnitude of the age-associated risks is important economically because if age affects infant health, then the changes in the maternal age distribution over the last thirty years could have imposed large health costs. The estimates in this paper suggest that the age-related health costs of childbearing are not negligible. Given these estimates, the costs of delayed childbearing have now become equal to those of teenage births and will likely increase as more women have children later in life. In the future I plan to estimate the total age-related health costs by linking hospital discharge data to the birth certificate data.

It should be noted that by focusing only on the health costs, these estimates fail to take into account some of the benefits of delayed childbearing. For example, by postponing the start of a family, a woman may have more resources to take care of her children. Nevertheless, as women consider their childbearing decision, they should be aware that delays do not come without jeopardizing their infant's health.

Appendix A – Definitions

Abnormal conditions of the newborn: anemia, fetal alcohol syndrome, hyaline membrane disease (a lung disorder that affect mostly premature infants and makes it difficult for them to breathe), meconium aspiration syndrome (when a newborn inhales stool mixed with amniotic fluid), assisted ventilation under 30 minutes, assisted ventilation 30 minutes or longer, seizures, and other unspecified conditions

Congenital anomalies (conditions present at birth): anencephalus (neural tube defect affecting the skull), spina bifida/meningocele (incomplete closure and spine and back bone before birth), hydrocephalus (accumulation of cerebrospinal fluid around the brain), microcephalus (an underdeveloped head and brain), other unspecified central nervous system anomalies, heart malformations, other circulatory/respiratory anomalies, rectal atresia/stenosis (incomplete development of the anus and rectum), tracheo-esophageal fistula/esophageal atresia (incomplete development of the esophagus), omphalocele/gastroschisis (the bulging of internal organs through the abdominal wall), other unspecified gastrointestinal anomalies, malformed genitalia, renal agenesis (absence of one or more kidneys), other unspecified urogenital anomalies, cleft lip/palate, polydactyl/syndactyly (non-normal number of toes of fingers or joined fingers), limb reduction (missing limbs), club foot, diaphragmatic hernia (protrusion of abdominal organs into the chest cavity), other unspecified musculoskeletal/integumental anomalies, Down's syndrome, other unspecified chromosomal abnormalities, and other unspecified congenital anomalies

Fetal death: a conception that does not manifest into a live birth

Induced abortion: a premature expulsion of a fetus unfit for survival

Infant death: death occurring in the first year of life

Labor and delivery complications: fever, the presence of meconium (dark fecal matter), premature rupture of membrane, abruptio placenta (premature abruption of the placenta), placenta previa (placenta covers part of the cervix), bleeding, seizures during labor, labor under 3 hours, labor over 20 hours, dysfunctional labor, fetus breeched, cord prolapse (the umbilical cord emerges from the birth canal before the rest of infant's body), and other unspecified complications

Low birthweight: infant weighing less than 2500 grams (5.5 pounds)

Medical risk factors: anemia, cardiac disease, acute or chronic lung disease, diabetes, hydramnios (too much amniotic fluid)/oligohydramnios (too little amniotic fluid), hemoglobinopathy (hemoglobin disorder such as sickle cell), chronic hypertension, pregnancy-associated hypertension, eclampsia (seizures during pregnancy), incompetent cervix, renal disease (disease related to kidneys), blood group isoimmunization (the compatibility of Rh factor of the mother and the infant), sexually-transmitted diseases, and other unspecified risk factors

Miscarriage: the spontaneous end to fetal life

Parity: birth order

Prematurity: birth before 37 weeks of gestation (40 weeks is a normal pregnancy)

Stillbirth: birth of a dead fetus

Very low birthweight: infant weighing less than 1500 grams (3.25 pounds)

Appendix B – Problems with the Fixed-Effects Model

It naively appears that the omitted variables bias from estimating the fixed-effects specification (2) is small if the less restrictive model, where one allows the effects to vary by parity, is the true data-generating process. First, note that the excluded variables $f(a_{i1})$ and $\boldsymbol{x}_{i1}$ are multiplied by $(\beta_2 - \beta_1)$ and $(\delta_2 - \delta_1)$ respectively when they enter into the first difference equation (6). Hence, if $(\beta_2 - \beta_1)$ and $(\delta_2 - \delta_1)$ are not large, one may assume the exclusion of $f(a_{i1})$ and $\boldsymbol{x}_{i1}$ does not severely bias the coefficient estimates. However, the omitted variables bias is also a function of the correlations between $f(a_{i1})$ and $\boldsymbol{x}_{i1}$ and each of the dependent variables. The probability limit of the estimated age effect if $f(a_{i1})$ and $\boldsymbol{x}_{i1}$ are excluded from estimation of equation (6) is

(1') $$plim\ \hat{\beta} = \beta_2 + \pi$$

where π is the probability limit of the projection coefficient on $f(a_2) - f(a_1)$ from an auxiliary regression of the omitted variables, $(\beta_2 - \beta_1) f(a_1) + (\delta_2 - \delta_1)\boldsymbol{x}_1$, on $f(a_2) - f(a_1)$ and $\boldsymbol{x}_2 - \boldsymbol{x}_1$. In the simple case where the effect of the inputs do not differ across births ($\delta_2 = \delta_1$), this bias term simplifies to

(2') $$plim\ \hat{\beta} = \beta_2 + (\beta_2 - \beta_1)\theta$$

where θ is the probability limit of the coefficient on $f(a_2) - f(a_1)$ from the regression of $f(a_1)$ on $f(a_2) - f(a_1)$ and $\boldsymbol{x}_2 - \boldsymbol{x}_1$. If θ is big, even small differences in the age effects are magnified.

Equation (2') underscores another often-ignored issue with fixed-effects estimation. The fixed-effect estimate is not necessarily a weighted average of the age effects for first and second births. If β is a scalar, then in order for $plim\ \hat{\beta}$ in equation (2') to lie between β_2 and β_1, it must be true that $-1 \leq \theta \leq 0$, but this condition does not necessarily hold.

In summary, the assumptions implicit in a fixed-effects model are frequently ignored but are a potential source of bias. If the effects of the independent variables vary for the first and second birth (or more generally, across the exploited variation), then the fixed-effects estimates will be biased. Even if the difference in the effects across births seems miniscule, the bias can still be large. Note that one of the crucial assumptions of the over-identified fixed-effects model can be easily tested using an F-test.

Appendix C – Estimation of the Correlated Random-Effects Model

There are several possible methods of estimating the structural parameters in the system of equations defined by the correlated random-effects model. For the linear two-birth model, if the ϕ parameters are viewed as nuisance parameters, then one can difference equations (15) and (14) to estimate the structural parameters (excluding the ϕ parameters and the intercept terms, θ_{10} and θ_{20}). That is:

(1'') $$y_{i2} - y_{i1} = (\theta_{20} - \theta_{10}) - f(a_{i1})\theta_{11} + f(a_{i2})\theta_{22} - \boldsymbol{x}_{i1}\theta_{13} + \boldsymbol{x}_{i2}\theta_{24} + \varepsilon_{i2} - \varepsilon_{i1}$$

This is equivalent to the earlier model that allows the effects of maternal age and other inputs to vary by birth order (equation (5)). However, I cannot estimate the ϕ parameters using this approach, and thus cannot determine the extent of bias in the OLS estimates due to the unobserved mother fixed effect.

An alternative method allows me to estimate all of the identified structural parameters. This method is useful for estimating the two-birth model as a bivariate probit model. This is because the bivariate probit model is non-linear, so I cannot take advantage of the linearity of the model as I do in equation (1''). The first step of this method is to estimate the reduced-form equations (14) and (15) as a system of seemingly unrelated regressions (SUR). Next, the reduced-form parameters are written as a function of the structural parameters. That is,

(2'') $$\Pi = \Gamma\Theta$$

where Π is a matrix of the reduced-form parameters, Θ is a matrix of the structural parameters, and Γ is a matrix of known constants. Then, the optimal minimum distance estimator of Θ, provided that Γ is full-column rank, is computed:

(3") $$\hat{\Theta} = \underset{\Theta}{\operatorname{argmin}} (\hat{\Pi} - \Gamma\Theta)' \hat{V}^{-1} (\hat{\Pi} - \Gamma\Theta)$$

where $\hat{V} = V(\hat{\Pi})$ is the estimated variance of $\hat{\Pi}$ from the first step. Writing $\hat{V}^{-1}$ as its Cholesky decomposition (i.e., $\hat{V}^{-1} = C'C$), the minimum distance estimator in (3") can be expressed as the following:

(4") $$\hat{\Theta} = \underset{\Theta}{\operatorname{argmin}} (C\hat{\Pi} - C\Gamma\Theta)'(C\hat{\Pi} - C\Gamma\Theta)$$

which is equivalent to the least squares estimator

(5") $$\begin{aligned} \hat{\Theta} &= ((C\Gamma)'(C\Gamma))^{-1}(C\Gamma)'(C\hat{\Pi}) \\ &= (\Gamma'\hat{V}^{-1}\Gamma)^{-1}\Gamma'\hat{V}^{-1}\hat{\Pi} \end{aligned}$$

<u>Appendix D – The Relationship Between the Reduced-Form and Structural Parameters in the Three-Birth Model</u>

The relationships between the reduced-form and the structural parameters for the more-restrictive three-birth model that does not allow the inputs of prior births to affect the current birth outcome are:

$$\begin{array}{lll}
\pi_{10} = \theta_{10} + \phi_0 & \pi_{20} = \theta_{20} + \phi_0 & \pi_{30} = \theta_{30} + \phi_0 \\
\pi_{11} = \theta_{11} + \phi_1 & \pi_{21} = \phi_1 & \pi_{31} = \phi_1 \\
\pi_{12} = \phi_2 & \pi_{22} = \theta_{22} + \phi_2 & \pi_{32} = \phi_2 \\
\pi_{13} = \phi_3 & \pi_{23} = \phi_3 & \pi_{33} = \theta_{33} + \phi_3 \\
\pi_{14} = \theta_{14} + \phi_4 & \pi_{24} = \phi_4 & \pi_{34} = \phi_4 \\
\pi_{15} = \phi_5 & \pi_{25} = \theta_{25} + \phi_5 & \pi_{35} = \phi_5 \\
\pi_{16} = \phi_6 & \pi_{26} = \phi_6 & \pi_{36} = \theta_{36} + \phi_6 \\
\xi_{i1} = \eta_i + \varepsilon_{i1} & \xi_{i2} = \eta_i + \varepsilon_{i2} & \xi_{i3} = \eta_i + \varepsilon_{i3}
\end{array}$$

The relationships between the reduced-form and structural parameters for the less-restrictive three-birth model that allows the inputs of prior births to affect the current birth outcome are:

$$\begin{array}{lll}
\pi_{10} = \theta_{10} + \phi_0 & \pi_{20} = \theta_{20} + \phi_0 & \pi_{30} = \theta_{30} + \phi_0 \\
\pi_{11} = \theta_{11} + \phi_1 & \pi_{21} = \theta_{21} + \phi_1 & \pi_{31} = \theta_{31} + \phi_1 \\
\pi_{12} = \phi_2 & \pi_{22} = \theta_{22} + \phi_2 & \pi_{32} = \theta_{32} + \phi_2 \\
\pi_{13} = \phi_3 & \pi_{23} = \phi_3 & \pi_{33} = \theta_{33} + \phi_3 \\
\pi_{14} = \theta_{14} + \phi_4 & \pi_{24} = \theta_{24} + \phi_4 & \pi_{34} = \theta_{34} + \phi_4 \\
\pi_{15} = \phi_5 & \pi_{25} = \theta_{25} + \phi_5 & \pi_{35} = \theta_{35} + \phi_5 \\
\pi_{16} = \phi_6 & \pi_{26} = \phi_6 & \pi_{36} = \theta_{36} + \phi_6 \\
\xi_{i1} = \eta_i + \varepsilon_{i1} & \xi_{i2} = \eta_i + \varepsilon_{i2} & \xi_{i3} = \eta_i + \varepsilon_{i3}
\end{array}$$

Appendix E – Creation of the Matched Sample

To create the matched sample, I follow the procedure described in the data section. Below I detail the composition of the data set.

Sample	Number of births
Births to Texas residents (1989-2001)	4,366,316
Unmatched births:	2,475,822
Missing name or birth date	76,650
No name and birth date matches (One-birth mothers)	1,782,673
Multiple births of the same parity	199,144
Non-consecutive birth histories	165,829
Missing parity	144,798
At least one pregnancy was a plural birth	105,114
Missing plurality	1,614
Matched births:	1,890,494
Birth histories starting with first birth:	1,441,378
Two-birth mothers (i.e, observe first and second births)	928,102
Three-birth mothers (i.e., observe first, second, and third births)	392,100
Four-birth mothers	97,652
Five-birth mothers	18,815
Six-birth mothers	3,762
Seven-birth mothers	798
Eight- and higher-birth mothers	149
Birth histories that do not start with first birth:	449,116
Second and third birth observed	214,826
Third and fourth birth observed	77,668
Second, third, and fourth birth observed	62,661
Fourth and fifth birth observed	23,182
Remaining births	70,779

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Table 1: Match Statistics (Matched Sample from the Texas Linked Birth/Infant Death Records (1989-2001))

Characteristic	Percentage of mothers who report a consistent cross-birth value
Mother's race	99.1
Mother's Hispanic origin	97.9
Mother's birth state	98.3
Mother's Social Security number	95.8
Mother's date of last live birth	96.0

Notes:

1. All percentages are computed using non-missing values.
2. The mother's Social Security Number is reported beginning in 1994 but is often missing for the years in which it is reported. For 1994-2001, 9.9 percent of births belonging to the matched-mother sample are missing the mother's Social Security Number.
3. In the two-birth matched mothers sample, there are 464,051 mothers. Forty-eight percent report their Social Security number for two births.
4. If missing values are included as valid (e.g., a mother whose race is missing for all births is classified as a mother with a consistent cross-birth value), these percentages change only slightly.

Table 2: Matching of Second Births (Texas Linked Birth/Infant Death Records (1989-2001))

	Percent of singleton second births that are matched	
Sample	Using the sample of all non-matched mothers	Using the sample of selected non-matched mothers
Mothers whose first birth occurred between 1989 and 2001	72.3	77.8
Texas-born mothers whose first birth occurred between 1989 and 2001	82.1	88.4

Note: The selected non-matched mothers include only those mothers for whom there were no other observations with the same name and birth date.

Table 3: Demographic Characteristics of One-Birth, Two-Birth, and Three-Birth Mothers (Texas 1989-2001)

	Information on Mother			Information on Father		
	One-Birth Mothers	**Two-Birth Mothers**	**Three-Birth Mothers**	**One-Birth Mothers**	**Two-Birth Mothers**	**Three-Birth Mothers**
Number of mothers	886135	464051	130700	886135	464051	130700
Race at first birth						
White	0.84	0.86	0.85	0.85	0.89	0.90
Black	0.12	0.11	0.13	0.10	0.08	0.08
Asian	0.04	0.03	0.01	0.04	0.03	0.02
Hispanic at first birth	0.39	0.35	0.43	0.38	0.34	0.43
Age at first birth	23.99	23.46	21.40	27.34	26.69	24.88
Age at second birth		26.72	24.04		29.68	27.23
Age at third birth			27.04			30.13
Education at first birth						
Less than 9 years	0.08	0.06	0.09	0.06	0.04	0.06
9-11 years	0.21	0.21	0.32	0.13	0.13	0.17
12 years	0.32	0.31	0.29	0.26	0.27	0.25
13-15 years	0.19	0.18	0.14	0.15	0.16	0.12
16+ years	0.19	0.22	0.15	0.18	0.22	0.16
Married at first birth	0.64	0.73	0.69			
Married at second birth		0.80	0.74			
Married at third birth			0.77			
Father matches between 1st and 2nd birth					0.67	0.59
Father matches between 2nd and 3rd birth						0.67
Father information missing at 1st birth				0.20	0.16	0.23
Father information missing at 2nd birth					0.10	0.14
Father information missing at 3rd birth						0.12

Notes:

1. One-birth mothers are first-birth mothers whose name and birthdate are unique. Two-birth mothers are mothers who are matched across their first and second births. Three-birth mothers are mothers who are matched across their first, second, and third births.
2. A father matches across births if his first and last name and birth date are the same across births.
3. A father's information is missing if his education and his birth date are missing.

Table 4: Means of Birth-Related Variables for One-Birth, Two-Birth, and Three-Birth Mothers (Texas 1989-2001)

	One-Birth Mothers	**Two-Birth Mothers**	**Three-Birth Mothers**
Number of mothers	886135	464051	130700
Number of prenatal care visits			
First birth	11.27	11.55	10.91
Second birth		11.63	10.96
Third birth			11.24
Number of previously terminated pregnancies			
First birth	0.20	0.19	0.16
Second birth		0.31	0.26
Third birth			0.35
Mother has at least one medical risk factor			
First birth	0.30	0.29	0.30
Second birth		0.27	0.26
Third birth			0.28
Mother has labor/delivery complications			
First birth	0.20	0.21	0.20
Second birth		0.12	0.13
Third birth			0.12
Low birth weight birth (<2500 grams)			
First birth	0.07	0.06	0.06
Second birth		0.04	0.05
Third birth			0.05
Premature birth (<37 weeks of gestation)			
First birth	0.09	0.08	0.09
Second birth		0.08	0.08
Third birth			0.09
Baby has congenital anomaly			
First birth	0.010	0.010	0.012
Second birth		0.009	0.009
Third birth			0.009
Infant mortality rate (infant deaths per 1000 births)			
First birth	4.68	3.78	6.60
Second birth		3.24	5.83
Third birth			4.06

Notes:

1. One-birth mothers are first-birth mothers whose name and birthdate are unique. Two-birth mothers are mothers who are matched across their first two births. Three-birth mothers are mothers who are matched across their first three births.

2. For details on medical risk factors, labor/delivery complications, and congenital anomalies, see Appendix A.

Table 5: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Two-Birth Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.056	0.089	0.024	0.057	0.021	0.027	0.039	0.013	0.023	0.024
	(0.002)**	(0.005)**	(0.002)**	(0.005)**	(0.002)**	(0.004)**	(0.007)**	(0.004)**	(0.005)**	(0.007)**
Mother's age 18-21	0.021	0.037	-0.000	0.016	0.003	0.002	0.015	-0.002	0.000	0.006
	(0.001)**	(0.001)**	(0.002)	(0.002)**	(0.002)	(0.003)	(0.003)**	(0.003)	(0.004)	(0.004)
Mother's age 22-25	0.006	0.012	-0.004	0.000	-0.000	-0.006	0.005	-0.003	-0.006	0.000
	(0.001)**	(0.001)**	(0.001)**	(0.001)	(0.001)	(0.002)*	(0.002)*	(0.002)	(0.002)*	(0.002)
Mother's age 30-33	0.004	-0.004	0.007	0.004	0.003	0.010	0.006	0.005	0.011	0.009
	(0.002)**	(0.001)**	(0.002)**	(0.001)**	(0.002)	(0.003)**	(0.002)**	(0.002)*	(0.003)**	(0.002)**
Mother's age 34-37	0.016	0.004	0.020	0.014	0.003	0.020	0.013	0.007	0.024	0.018
	(0.003)**	(0.002)*	(0.003)**	(0.002)**	(0.003)	(0.005)**	(0.004)**	(0.003)	(0.005)**	(0.004)**
Mother's age 38+	0.018	0.009	0.022	0.021	0.001	0.020	0.021	0.007	0.025	0.029
	(0.006)**	(0.003)**	(0.006)**	(0.003)**	(0.005)	(0.010)*	(0.006)**	(0.006)	(0.010)*	(0.007)**
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	358394	358394	358394	358394	358394	358394	358394	358394	358394	358394
Mean of dependent variable	0.0818	0.0799	0.0818	0.0799	0.0818	0.0799	0.0818	0.0799	0.0818	0.0799

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 6: Bivariate Probit Correlated Random-Effects Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Two-Birth Mothers)

	1st birth	2nd birth	1st birth	2nd birth
	(1a)	(1b)	(2a)	(2b)
Mother's age <18	0.023	0.034	0.019	0.022
	(0.003)**	(0.006)**	(0.004)**	(0.007)**
Mother's age 18-21	0.001	0.014	0.000	0.006
	(0.002)	(0.003)**	(0.003)	(0.004)
Mother's age 22-25	-0.006	0.005	-0.006	0.000
	(0.002)**	(0.002)*	(0.002)**	(0.003)
Mother's age 30-33	0.012	0.007	0.013	0.011
	(0.004)**	(0.002)**	(0.004)**	(0.003)**
Mother's age 34-37	0.023	0.015	0.028	0.024
	(0.006)**	(0.004)**	(0.007)**	(0.005)**
Mother's age 38+	0.022	0.024	0.028	0.038
	(0.011)*	(0.007)**	(0.013)*	(0.009)**
<u>Controls</u>				
Maternal education	N	N	Y	Y
Marital status	N	N	Y	Y
Absence of father	N	N	Y	Y
Smoking/drinking behavior	N	N	Y	Y
Maternal race/ethnicity	N	N	NA	NA
Birth year of infant	N	N	Y	Y
Observations (mothers)	358394	358394	358394	358394
Mean of dependent variable	0.0818	0.0799	0.0818	0.0799

Notes: This table presents marginal effect estimates with their associated standard errors in parentheses. The marginal effect is computed as the mean marginal effect, as opposed to the marginal effect at the mean. The standard errors are computed using the delta method. For computational ease, the residential zip code is excluded from the regressions as a control variable. In the linear specification (Table 5), the inclusion of the residential zip code as a control does not affect the age profile estimates. The sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 7: Cross-Sectional and Panel Data Estimates of the Effect of Non-Age Covariates on the Probability of a Premature Birth (Matched Two-Birth Mothers)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8a)	(8b)
Mother has less than 9 years of education	0.010	0.010	0.010	0.009	0.014
	(0.002)**	(0.003)**	(0.004)**	(0.005)*	(0.005)**
Mother has 9-11 years of education	0.002	0.005	0.002	0.000	0.003
	(0.002)	(0.002)**	(0.002)	(0.003)	(0.003)
Mother has 13-15 years of education	-0.004	-0.003	0.002	0.004	0.001
	(0.001)**	(0.001)*	(0.002)	(0.002)	(0.002)
Mother has 16+ years of education	-0.016	-0.020	0.001	0.007	-0.001
	(0.001)**	(0.001)**	(0.003)	(0.003)*	(0.003)
Father absent	0.007	0.013	0.008	0.008	0.009
	(0.002)**	(0.002)**	(0.002)**	(0.003)**	(0.003)**
Mother married	-0.012	-0.011	-0.002	-0.002	-0.004
	(0.002)**	(0.002)**	(0.002)	(0.002)	(0.002)
Mother smoked during pregnancy	0.004	0.005	0.002	-0.003	0.006
	(0.002)	(0.002)**	(0.003)	(0.003)	(0.003)
Mother drank during pregnancy	0.015	0.013	0.015	0.013	0.019
	(0.005)**	(0.005)*	(0.005)**	(0.007)	(0.007)**
Mother black	0.050	0.045			
	(0.002)**	(0.002)**			
Mother American Indian	-0.011	0.022			
	(0.010)	(0.011)			
Mother Asian	0.013	0.024			
	(0.003)**	(0.003)**			
Mother other race	0.008	-0.005			
	(0.027)	(0.021)			
Mother Hispanic	0.011	0.014			
	(0.001)**	(0.001)**			
Mother immigrant	-0.009	-0.010			
	(0.001)**	(0.001)**			
Observations (mothers)	358394	358394	358394	358394	358394
Mean of dependent variable	0.0818	0.0799	0.0799	0.0818	0.0799

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Table 5, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 8: Fertility and Expected Fertility

Estimates from the 1998 June Supplement of the Current Population Survey

For Women Aged 18-39 who have Clear Fertility Intentions			For Women Aged 18-44		
Expected Number of Births	**Frequency**	**Cumulative Frequency**	**Number of Live Births**	**Frequency**	**Cumulative Frequency**
0	10.89	10.89	0	19.03	19.03
1	14.31	25.20	1	17.33	36.36
2	45.37	70.57	2	35.81	72.17
3	20.35	90.92	3	18.20	90.37
4	6.46	97.38	4	6.10	96.47
5	1.66	99.04	5	2.22	98.69
6	0.55	99.59	6	0.78	99.47
7	0.18	99.77	7	0.40	99.86
8	0.12	99.90	8	0.07	99.93
9	0.03	99.93	9	0.04	99.97
10	0.02	99.95	10	0.03	100.00
11	0.03	99.98	11	0.00	100.00
12	0.01	99.98			
13	0.02	100.00			
Number of Observations:		13871			26994

Notes:

1. All frequencies are weighted by the CPS person weights.

2. Women with clear fertility intentions include those planning on having no children.

Estimates from the 1990 Census

For Texas Women Aged 45+		
Number of Live Births	**Frequency**	**Cumulative Frequency**
0	13.10	13.10
1	13.21	26.32
2	24.35	50.67
3	20.35	71.01
4	12.16	83.17
5	6.36	89.53
6	3.97	93.50
7	2.18	95.68
8	1.49	97.16
9	0.91	98.07
10	0.65	98.72
11	0.48	99.20
12	0.80	100.00
Number of Observations:		27812

Notes:

1. All frequencies are weighted by the Census person weights.

Table 9: Cross-Sectional Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (First Births Occurring between 1991 and 1995)

	Sample:			
	One-birth mothers	Two-birth mothers	One-birth mothers	Two-birth mothers
	(1)	(2)	(3)	(4)
Mother's age <18	0.049	0.055	0.019	0.027
	(0.003)**	(0.002)**	(0.003)**	(0.003)**
Mother's age 18-21	0.011	0.021	-0.007	0.001
	(0.002)**	(0.002)**	(0.002)**	(0.002)
Mother's age 22-25	0.001	0.004	-0.007	-0.005
	(0.002)	(0.002)*	(0.002)**	(0.002)**
Mother's age 30-33	0.011	0.005	0.014	0.008
	(0.002)**	(0.002)*	(0.002)**	(0.002)**
Mother's age 34-37	0.025	0.017	0.030	0.022
	(0.003)**	(0.003)**	(0.003)**	(0.003)**
Mother's age 38+	0.038	0.018	0.044	0.023
	(0.004)**	(0.008)*	(0.004)**	(0.009)**
Controls				
Maternal education	N	N	Y	Y
Marital status	N	N	Y	Y
Absence of father	N	N	Y	Y
Smoking/drinking behavior	N	N	Y	Y
Maternal race/ethnicity	N	N	Y	Y
Residential zip code	N	N	Y	Y
Birth year of infant	N	N	Y	Y
Observations (mothers)	224261	203239	224261	203239
Mean of dependent variable	0.095	0.079	0.095	0.079

F-test for equality of age profiles	F-stat	P-value
Unadjusted regression (columns (1) & (2))	45.15	0.00
Adjusted regression (columns (3) & (4))	20.42	0.00

Notes: Sample only includes mothers residing in Texas who had their birth between 1991 and 1995. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 10: Selection Correction Correlated Random-Effects Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Two-Birth Mothers)

	1st birth	2nd birth	1st birth	2nd birth
	(1a)	(1b)	(2a)	(2b)
Mother's age <18	0.026	0.039	0.022	0.024
	(0.004)**	(0.005)**	(0.005)**	(0.006)**
Mother's age 18-21	0.001	0.015	-0.000	0.005
	(0.003)	(0.003)**	(0.004)	(0.004)
Mother's age 22-25	-0.006	0.004	-0.006	0.000
	(0.002)*	(0.002)*	(0.003)*	(0.002)
Mother's age 30-33	0.010	0.006	0.012	0.009
	(0.003)**	(0.002)**	(0.003)**	(0.003)**
Mother's age 34-37	0.022	0.014	0.025	0.019
	(0.005)**	(0.004)**	(0.006)**	(0.004)**
Mother's age 38+	0.024	0.022	0.027	0.029
	(0.010)*	(0.006)**	(0.011)*	(0.007)**
Difference in selection parameter across births	0.006		0.001	
	(0.004)		(0.006)	
Controls				
Maternal education	N	N	Y	Y
Marital status	N	N	Y	Y
Absence of father	N	N	Y	Y
Smoking/drinking behavior	N	N	Y	Y
Residential zip code	N	N	Y	Y
Birth year of infant	N	N	Y	Y
Observations (mothers)	358394	358394	358394	358394
Mean of dependent variable	0.0818	0.0799	0.0818	0.0799

Notes: Sample only includes Texas-born mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Difference in selection parameter is the estimated coefficient on the inverse Mills ratio in the differenced version of the correlated random-effects model (see Appendix C). Hence, the coefficient represents the difference in the coefficients on the inverse Mills ratio across births. The selection correction model is estimated by maximum likelihood. Variables in the selection equation include maternal age, the sex of first child, mother's immigrant status, marital status, maternal education, maternal race, and maternal ethnicity. The exclusion restriction is based on the sex of the first child. The selection equation is estimated using only one-birth and two-birth mothers. Observation counts include only two-birth mothers. Robust standard errors are presented in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 11: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Hispanic Two-Birth Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.043	0.078	0.027	0.060	0.012	0.020	0.036	0.002	0.005	0.013
	(0.003)**	(0.006)**	(0.003)**	(0.006)**	(0.004)**	(0.006)**	(0.009)**	(0.007)	(0.008)	(0.010)
Mother's age 18-21	0.015	0.024	0.005	0.013	-0.003	-0.000	0.005	-0.010	-0.011	-0.010
	(0.002)**	(0.002)**	(0.003)	(0.003)**	(0.003)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
Mother's age 22-25	0.002	0.006	-0.002	0.001	-0.005	-0.005	-0.002	-0.008	-0.010	-0.009
	(0.003)	(0.002)**	(0.003)	(0.002)	(0.002)*	(0.004)	(0.004)	(0.003)*	(0.005)*	(0.004)*
Mother's age 30-33	0.009	0.002	0.011	0.005	0.010	0.010	0.009	0.013	0.015	0.015
	(0.004)*	(0.003)	(0.004)**	(0.003)	(0.003)**	(0.006)	(0.005)*	(0.004)**	(0.007)*	(0.005)**
Mother's age 34-37	0.026	0.018	0.027	0.023	0.023	0.037	0.022	0.029	0.048	0.033
	(0.007)**	(0.004)**	(0.007)**	(0.004)**	(0.006)**	(0.013)**	(0.008)**	(0.007)**	(0.013)**	(0.009)**
Mother's age 38+	0.048	0.031	0.051	0.036	0.046	0.065	0.055	0.054	0.079	0.071
	(0.018)**	(0.008)**	(0.018)**	(0.008)**	(0.011)**	(0.027)*	(0.015)**	(0.013)**	(0.028)**	(0.016)**
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	212922	212922	212922	212922	212921	212922	212923	212924	212925	212926
Mean of dependent variable	0.0871	0.0853	0.0871	0.0853	0.0862	0.0871	0.0853	0.0862	0.0871	0.0853

Notes: Sample only includes Hispanic mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses.
* denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 12: Cross-Sectional Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Second Births)

	Sample:			
	Texas Natives Who Had First Birth Between 1991 and 2001			
	Unmatched	Matched	Unmatched	Matched
	(1)	(2)	(3)	(4)
Mother's age <18	0.106	0.089	0.073	0.057
	(0.010)**	(0.005)**	(0.011)**	(0.005)**
Mother's age 18-21	0.038	0.034	0.015	0.013
	(0.004)**	(0.002)**	(0.005)**	(0.002)**
Mother's age 22-25	0.009	0.009	-0.002	-0.002
	(0.004)*	(0.001)**	(0.004)	(0.002)
Mother's age 30-33	-0.001	-0.005	0.008	0.003
	(0.004)	(0.002)**	(0.004)	(0.002)
Mother's age 34-37	0.010	0.007	0.021	0.016
	(0.006)	(0.002)**	(0.006)**	(0.002)**
Mother's age 38+	0.029	0.021	0.042	0.032
	(0.011)**	(0.004)**	(0.011)**	(0.004)**
Controls				
Maternal education	N	N	Y	Y
Marital status	N	N	Y	Y
Absence of father	N	N	Y	Y
Smoking/drinking behavior	N	N	Y	Y
Maternal race/ethnicity	N	N	Y	Y
Residential zip code	N	N	Y	Y
Birth year of infant	N	N	Y	Y
Observations (mothers)	46436	258428	46436	258428
Mean of dependent variable	0.096	0.084	0.096	0.084

F-test for equality of age profiles between matched and unmatched mothers

F-stat	F(6,304851)=6.45	F(6,304706)=0.61
Associated p-value	0.00	0.73

Notes: Sample only includes mothers residing in Texas. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 13: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Two-Birth Mothers Born in Texas)

	Estimation Method:									
	Cross-Section				Panel Data					
					Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.054	0.090	0.020	0.056	0.021	0.027	0.044	0.013	0.023	0.026
	(0.002)**	(0.005)**	(0.003)**	(0.006)**	(0.003)**	(0.005)**	(0.008)**	(0.006)*	(0.006)**	(0.009)**
Mother's age 18-21	0.019	0.035	-0.004	0.012	0.003	-0.000	0.017	-0.002	-0.001	0.004
	(0.002)**	(0.002)**	(0.002)	(0.002)**	(0.002)	(0.004)	(0.004)**	(0.004)	(0.005)	(0.005)
Mother's age 22-25	0.005	0.011	-0.004	-0.002	0.000	-0.007	0.005	-0.002	-0.007	-0.001
	(0.002)**	(0.002)**	(0.002)*	(0.002)	(0.002)	(0.003)*	(0.003)	(0.003)	(0.003)*	(0.003)
Mother's age 30-33	0.009	-0.004	0.011	0.005	0.005	0.019	0.009	0.006	0.021	0.012
	(0.002)**	(0.002)*	(0.002)**	(0.002)*	(0.002)*	(0.004)**	(0.003)**	(0.003)*	(0.004)**	(0.003)**
Mother's age 34-37	0.022	0.008	0.025	0.017	0.004	0.029	0.022	0.008	0.034	0.027
	(0.004)**	(0.003)**	(0.004)**	(0.003)**	(0.004)	(0.008)**	(0.005)**	(0.005)	(0.008)**	(0.006)**
Mother's age 38+	0.017	0.014	0.022	0.025	-0.003	0.014	0.026	0.002	0.020	0.033
	(0.010)	(0.004)**	(0.010)*	(0.004)**	(0.007)	(0.016)	(0.009)**	(0.009)	(0.016)	(0.010)**
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	212922	212922	212922	212922	212922	212922	212922	212922	212922	212922
Mean of dependent variable	0.0871	0.0853	0.0871	0.0853	0.0862	0.0871	0.0853	0.0862	0.0871	0.0853

Notes: Sample only includes Texas-born mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses.
* denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 14: Cross-Sectional and Panel Data Estimates of the Effect of Non-Age Covariates on the Probability of a Premature Birth (Matched Two-Birth Mothers Born in Texas)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8)	
Mother has less than 9 years of education	0.018	0.010	0.016	0.017	0.021
	(0.004)**	(0.005)	(0.005)**	(0.007)*	(0.008)**
Mother has 9-11 years of education	0.003	0.006	0.001	-0.001	0.003
	(0.002)	(0.002)**	(0.003)	(0.003)	(0.003)
Mother has 13-15 years of education	-0.004	-0.004	0.004	0.007	0.002
	(0.002)*	(0.002)*	(0.002)	(0.003)*	(0.003)
Mother has 16+ years of education	-0.014	-0.021	0.004	0.014	0.001
	(0.002)**	(0.002)**	(0.004)	(0.004)**	(0.004)
Father absent	0.008	0.014	0.010	0.012	0.010
	(0.002)**	(0.003)**	(0.002)**	(0.003)**	(0.004)**
Mother married	-0.011	-0.009	-0.003	-0.001	-0.006
	(0.002)**	(0.002)**	(0.002)	(0.003)	(0.003)*
Mother smoked during pregnancy	0.002	0.006	0.000	-0.006	0.005
	(0.003)	(0.002)*	(0.003)	(0.004)	(0.004)
Mother drank during pregnancy	0.015	0.017	0.014	0.010	0.021
	(0.007)*	(0.007)*	(0.006)*	(0.009)	(0.009)*
Mother black	0.052	0.049			
	(0.002)**	(0.002)**			
Mother American Indian	-0.009	0.007			
	(0.014)	(0.016)			
Mother Asian	0.018	0.002			
	(0.020)	(0.018)			
Mother other race	-0.101	-0.084			
	(0.009)**	(0.005)**			
Mother Hispanic	0.012	0.016			
	(0.002)**	(0.002)**			
Mother immigrant	0.000	0.000			
	(0.000)	(0.000)			
Observations (mothers)	212922	212922	212922	212922	212922
Mean of dependent variable	0.0871	0.0853	0.0862	0.0871	0.0853

Notes: Sample only includes Texas-born mothers residing in Texas who gave birth between 1991 and 2001. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Table 13, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 15: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Two-Birth Mothers who Receive No Amniocentesis or Ultrasound)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.050	0.081	0.020	0.051	0.022	0.028	0.039	0.011	0.017	0.019
	(0.003)**	(0.007)**	(0.004)**	(0.007)**	(0.004)**	(0.007)**	(0.011)**	(0.008)	(0.009)*	(0.012)
Mother's age 18-21	0.022	0.037	0.002	0.017	0.006	0.003	0.017	-0.002	-0.004	0.003
	(0.002)**	(0.003)**	(0.003)	(0.003)**	(0.003)	(0.005)	(0.005)**	(0.006)	(0.007)	(0.006)
Mother's age 22-25	0.004	0.010	-0.005	-0.001	-0.000	-0.005	0.003	-0.004	-0.007	-0.004
	(0.003)	(0.002)**	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Mother's age 30-33	-0.001	-0.003	0.002	0.005	0.004	0.008	0.007	0.007	0.012	0.012
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.005)*	(0.005)**
Mother's age 34-37	0.016	0.010	0.019	0.019	0.011	0.024	0.019	0.018	0.033	0.028
	(0.005)**	(0.004)**	(0.005)**	(0.004)**	(0.005)*	(0.010)*	(0.007)**	(0.007)**	(0.011)**	(0.008)**
Mother's age 38+	0.017	0.008	0.020	0.018	0.004	0.029	0.019	0.014	0.042	0.033
	(0.013)	(0.006)	(0.013)	(0.006)**	(0.010)	(0.021)	(0.013)	(0.012)	(0.021)*	(0.014)*
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	105723	105723	105723	105723	105723	105723	105723	105723	105723	105723
Mean of dependent variable	0.0840	0.0823	0.0840	0.0823	0.0832	0.0840	0.0823	0.0832	0.0840	0.0823

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 16: Correlated Random-Effects Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Three-Birth Mothers)

	Model:				
	Only Current Inputs Allowed to Affect Birth Outcome			Both Current and Previous Inputs Allowed to Affect Birth Outcome	
	Dependent Variable:				
	1st Birth Premature	2nd Birth Premature	3rd Birth Premature	2nd Birth Premature	3rd Birth Premature
Mother's Age at First Birth					
<18	0.001 (0.006)			NA	NA
18-21	-0.017 (0.005)**			NA	NA
22-25	-0.014 (0.004)**			NA	NA
30-33	0.013 (0.005)*			NA	NA
34-37	0.001 (0.011)			NA	NA
38+	0.039 (0.037)			NA	NA
Mother's Age at Second Birth					
<18		0.022 (0.007)**		0.004 (0.009)	-0.032 (0.010)**
18-21		0.000 (0.005)		-0.010 (0.006)	-0.012 (0.007)
22-25		-0.008 (0.004)*		-0.012 (0.004)**	-0.003 (0.005)
30-33		0.010 (0.004)*		0.009 (0.005)	-0.003 (0.006)
34-37		0.012 (0.007)		0.009 (0.009)	-0.004 (0.011)
38+		0.005 (0.016)		-0.001 (0.022)	-0.007 (0.023)
Mother's Age at Third Birth					
<18			0.023 (0.018)		0.044 (0.019)**
18-21			0.000 (0.005)		0.011 (0.006)
22-25			-0.006 (0.003)		0.000 (0.004)
30-33			0.012 (0.004)**		0.009 (0.004)*
34-37			0.025 (0.005)**		0.023 (0.007)**
38+			0.037 (0.009)**		0.036 (0.012)**
Observations (mothers)	124302	124302	124302	124302	124302
Mean of dependent variable	0.085	0.083	0.089	0.083	0.089

Notes: Sample only includes mothers residing in Texas who gave birth between 1989 and 2001. Controls for birth year, presence of father, and marital status are included. Age dummies for mother's age 26-29 years are excluded. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 17: Two-Sided Selection-Correction Correlated Random-Effects Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Two-Birth Mothers)

	1st birth	2nd birth	1st birth	2nd birth
	(1a)	(1b)	(2a)	(2b)
Mother's age <18	0.026	0.040	0.022	0.024
	(0.004)**	(0.008)**	(0.005)**	(0.006)**
Mother's age 18-21	0.002	0.016	-0.000	0.005
	(0.003)	(0.004)**	(0.004)	(0.004)
Mother's age 22-25	-0.006	0.005	-0.006	0.000
	(0.002)*	(0.002)*	(0.003)*	(0.002)
Mother's age 30-33	0.010	0.006	0.012	0.009
	(0.003)**	(0.003)*	(0.003)**	(0.003)**
Mother's age 34-37	0.022	0.013	0.025	0.019
	(0.005)**	(0.005)**	(0.006)**	(0.004)**
Mother's age 38+	0.023	0.020	0.027	0.029
	(0.010)*	(0.008)*	(0.011)*	(0.007)**
Difference in selection parameter across births for probability of more than one birth	0.005		0.002	
	(0.004)		(0.008)	
Difference in selection parameter across births for probability of more than two births	0.002		-0.001	
	(0.007)		(0.010)	
Controls				
Maternal education	N	N	Y	Y
Marital status	N	N	Y	Y
Absence of father	N	N	Y	Y
Smoking/drinking behavior	N	N	Y	Y
Residential zip code	N	N	Y	Y
Birth year of infant	N	N	Y	Y
Observations (mothers)	358393	358393	358393	358393
Mean of dependent variable	0.0818	0.0800	0.0818	0.0800

Notes: Sample only includes Texas-born mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Difference in selection parameter is the estimated coefficient on the inverse Mills ratio in the differenced version of the correlated random-effects model (see Appendix C). Hence, the coefficient represents the difference in the coefficients on the inverse Mills ratio across births. There are two selection equations: one for the probability of having one or more births and one for the probability of having two or more births. Variables in the selection equation for having one or more births include maternal age, the sex of first child, the mother's immigrant status, marital status, maternal education, maternal race, and maternal ethnicity. The exclusion restriction in this equation is based on the sex of the first child. Variables in the selection equation for having two or more births include maternal age, whether the first two children are of the same sex, the mother's immigrant status, marital status, maternal education, maternal race, and maternal ethnicity. The exclusion restriction is based on whether the first two children are of the same sex. Selection equations are estimated using exclusively one-birth, two-birth, and three-birth mothers. Observation counts include only two-birth mothers. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Table 18: Covariate Contributions to the Difference in Age Dummies*

Cross-Sectional Model

	First Birth						*Second Birth*					
	Age of Mother						**Age of Mother**					
	<18	**18-21**	**22-25**	**30-33**	**34-37**	**38+**	**<18**	**18-21**	**22-25**	**30-33**	**34-37**	**38+**
Year of birth	0.0010	0.0004	-0.0002	0.0000	0.0003	0.0009	0.0006	0.0004	0.0000	-0.0001	-0.0003	-0.0001
Maternal race/ethnicity	0.0047	0.0029	0.0006	0.0000	-0.0002	-0.0006	0.0021	0.0047	0.0031	0.0015	-0.0004	-0.0002
Maternal marital status	0.0037	0.0022	0.0007	-0.0001	-0.0001	-0.0001	0.0001	0.0001	0.0000	0.0000	0.0000	0.0000
Maternal education	-0.0005	0.0029	0.0022	-0.0007	-0.0008	-0.0012	0.0029	0.0035	0.0029	-0.0028	-0.0035	-0.0039
Residential zip code	0.0000	-0.0004	-0.0004	0.0005	0.0007	0.0010	0.0006	0.0004	0.0000	-0.0001	-0.0003	-0.0001
Paternal information missing	-0.0019	-0.0011	-0.0004	0.0001	0.0001	0.0001	-0.0008	-0.0005	-0.0001	0.0001	0.0006	0.0015
Number of prenatal care visits	0.0302	0.0198	0.0076	-0.0018	-0.0033	-0.0039	0.0020	0.0008	0.0004	-0.0002	-0.0003	-0.0001
Maternal medical risk factors	-0.0001	-0.0005	-0.0004	0.0003	0.0023	0.0055	-0.0022	-0.0013	-0.0005	0.0001	0.0011	0.0025
Maternal smoking and drinking behaviors	0.0001	0.0001	0.0000	0.0000	0.0001	0.0001	-0.0001	-0.0002	-0.0001	0.0000	0.0001	0.0001
Number of previously terminated pregnancies	-0.0010	-0.0005	-0.0001	0.0003	0.0008	0.0014	0.0350	0.0188	0.0068	-0.0029	-0.0041	-0.0039
Maternal weight gain during pregnancy	-0.0007	-0.0009	-0.0001	0.0007	0.0017	0.0024	0.0011	0.0004	0.0006	-0.0004	-0.0002	0.0003
Labor/delivery complications	-0.0010	-0.0006	-0.0003	0.0003	0.0006	0.0008	-0.0009	-0.0005	-0.0004	0.0004	0.0010	0.0012
Obstetric procedures	-0.0005	-0.0004	-0.0002	0.0003	0.0031	0.0053	0.0001	0.0001	0.0001	0.0000	0.0010	0.0024
Total change	0.0339	0.0240	0.0091	-0.0001	0.0053	0.0116	0.0348	0.0181	0.0068	-0.0030	-0.0010	0.0037

Correlated Random-Effects Model

	First Birth						*Second Birth*					
	Age of Mother						**Age of Mother**					
	<18	**18-21**	**22-25**	**30-33**	**34-37**	**38+**	**<18**	**18-21**	**22-25**	**30-33**	**34-37**	**38+**
Year of birth	0.0048	0.0039	0.0020	-0.0017	-0.0032	-0.0041	-0.0032	-0.0031	-0.0020	0.0016	0.0033	0.0048
Maternal marital status	0.0012	0.0007	0.0003	-0.0001	-0.0002	-0.0002	0.0012	0.0006	0.0002	0.0000	0.0001	0.0001
Maternal education	0.0049	0.0030	0.0016	-0.0006	-0.0009	-0.0011	0.0024	0.0013	0.0006	-0.0002	0.0000	0.0002
Residential zip code	0.0014	0.0008	0.0006	-0.0003	-0.0003	-0.0006	0.0008	0.0000	0.0001	0.0000	-0.0002	-0.0001
Paternal information missing	0.0005	0.0003	0.0001	0.0000	-0.0001	-0.0001	0.0004	0.0002	0.0001	0.0000	0.0000	0.0001
Number of prenatal care visits	-0.0204	-0.0135	-0.0048	0.0017	0.0032	0.0040	0.0235	0.0132	0.0048	-0.0018	-0.0033	-0.0035
Maternal medical risk factors	0.0013	0.0012	0.0006	-0.0005	-0.0018	-0.0032	-0.0026	-0.0015	-0.0008	0.0005	0.0013	0.0013
Maternal smoking and drinking behaviors	-0.0001	-0.0001	0.0000	0.0000	0.0001	0.0001	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Maternal weight gain during pregnancy	0.0003	0.0014	0.0011	-0.0006	-0.0008	-0.0008	0.0021	0.0011	0.0008	-0.0002	-0.0001	-0.0004
Number of previously terminated pregnancies	0.0002	0.0001	0.0000	-0.0001	-0.0001	0.0000	-0.0001	0.0000	0.0000	0.0001	0.0001	0.0000
Labor/delivery complications	0.0005	0.0001	-0.0001	0.0000	0.0000	0.0000	-0.0003	0.0000	0.0000	-0.0001	0.0000	-0.0001
Obstetric procedures	0.0002	0.0002	0.0002	0.0000	-0.0006	-0.0014	0.0003	0.0002	0.0001	-0.0001	0.0005	0.0002
Total change	-0.0053	-0.0018	0.0015	-0.0021	-0.0048	-0.0074	0.0246	0.0119	0.0038	-0.0002	0.0017	0.0028

Notes:

1. Sample includes only two-birth mothers.
2. The estimation sample is limited to 1991 to 2001 as several of the covariates (weight gain, prenatal care visits, smoking and drinking behaviors, labor complications, and obstetric procedures) are only available starting in 1991.
3. Obstetric procedures include amniocentesis, electronic fetal monitoring, and ultrasound.
4. For more information on labor/delivery complications, see Appendix A.

* Covariate contributions are to the difference between the uncontrolled prematurity regression estimates and controlled prematurity regression estimates.

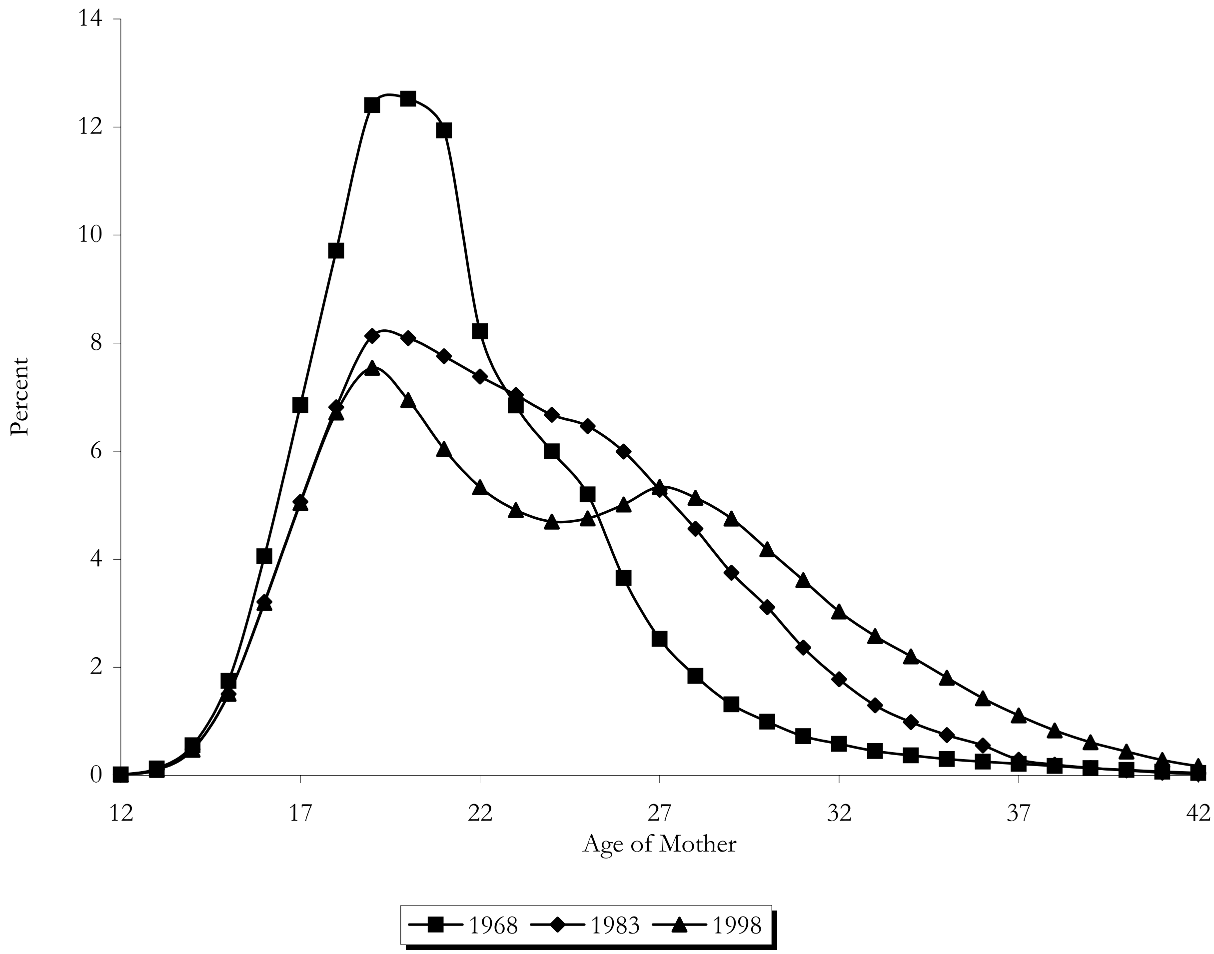
Table 19: Age-Related Annual Initial Hospital Costs Due to Prematurity for First and Second Births in the United States (Millions of Dollars)

Difference in Cost (Actual-Counterfactual)

	Cross-Sectional Estimates		Correlated Random-Effects Estimates	
Counterfactual	Unadjusted	Adjusted	Unadjusted	Adjusted
Births to Mothers Younger than 18				
If Births Postponed to Ages 18-21 :	348.2	243.6	236.0	205.6
Births to Mothers 34 and Older				
If Births Had Occurred When Ages 30-33 :	196.7	231.8	193.6	246.0
Total	544.9	475.4	429.6	451.6

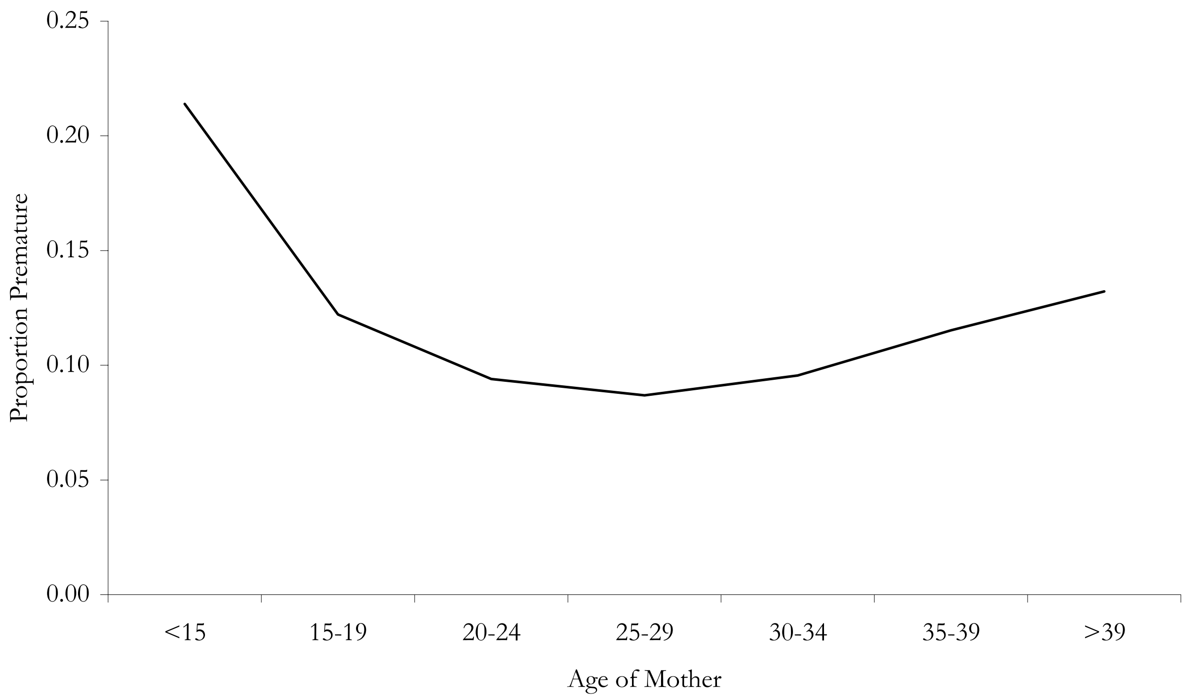
Notes: Cost estimates are based on the 1998 distribution of first and second births in the United States, the additional hospital care costs for a premature birth, which amounted to $53,700 in 2000 (March of Dimes, 2003a), and the estimated age dummy coefficients from Table 5.

Figure 1: Age Distribution of First-Birth Singleton Mothers - United States



Notes: Figure gives percent of births to mothers of a specified age. The 1968 file is a 50% random sample of births occurring in that year. The 1983 and 1998 files contain the universe of births. The source of the data is the Natality Detail Files from the National Center for Health Statistics.

Figure 2: Rates of Prematurity by Age of Mother for Singleton First Births - United States (1995-1997)



Notes: Figure gives the proportion of births that are premature, defined as a live birth occurring before 37 weeks of gestation. Gestation is computed from the date of last menses when this date is non-missing and from the clinical estimate of gestation otherwise. The source of data is the Birth Cohort Linked Birth/Infant Death Data from the National Center for Health Statistics.

Figure 3: Average Years of Education for Singleton First-Birth Mothers by Age - Texas (1989-2001)

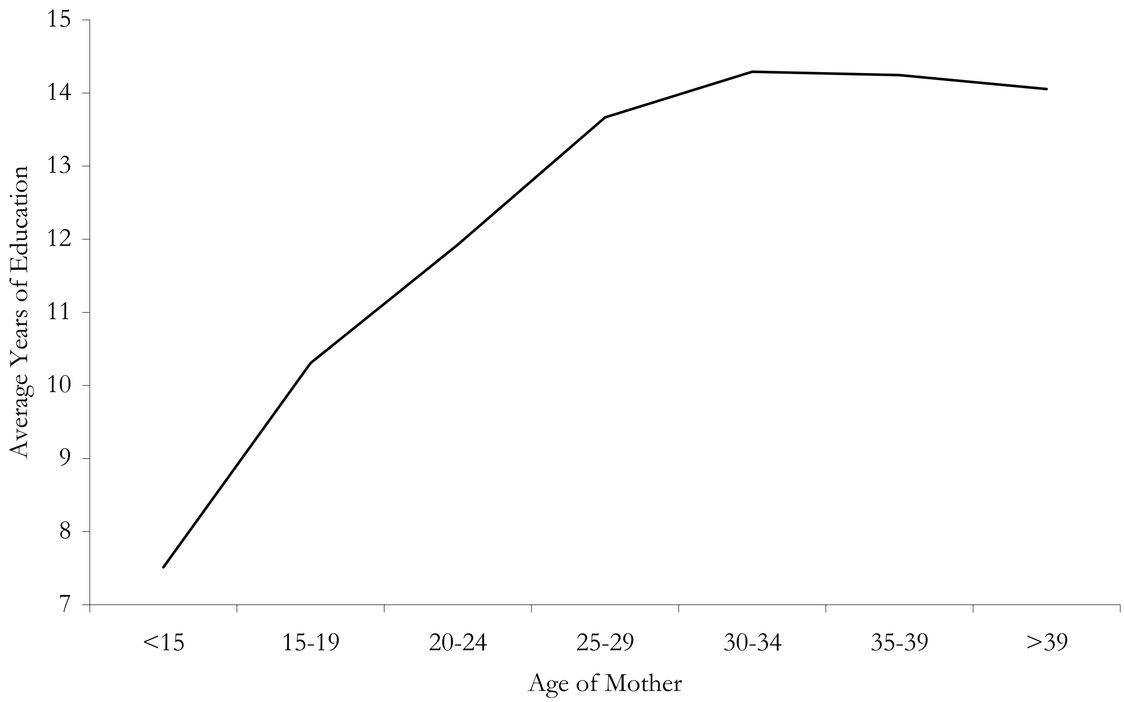
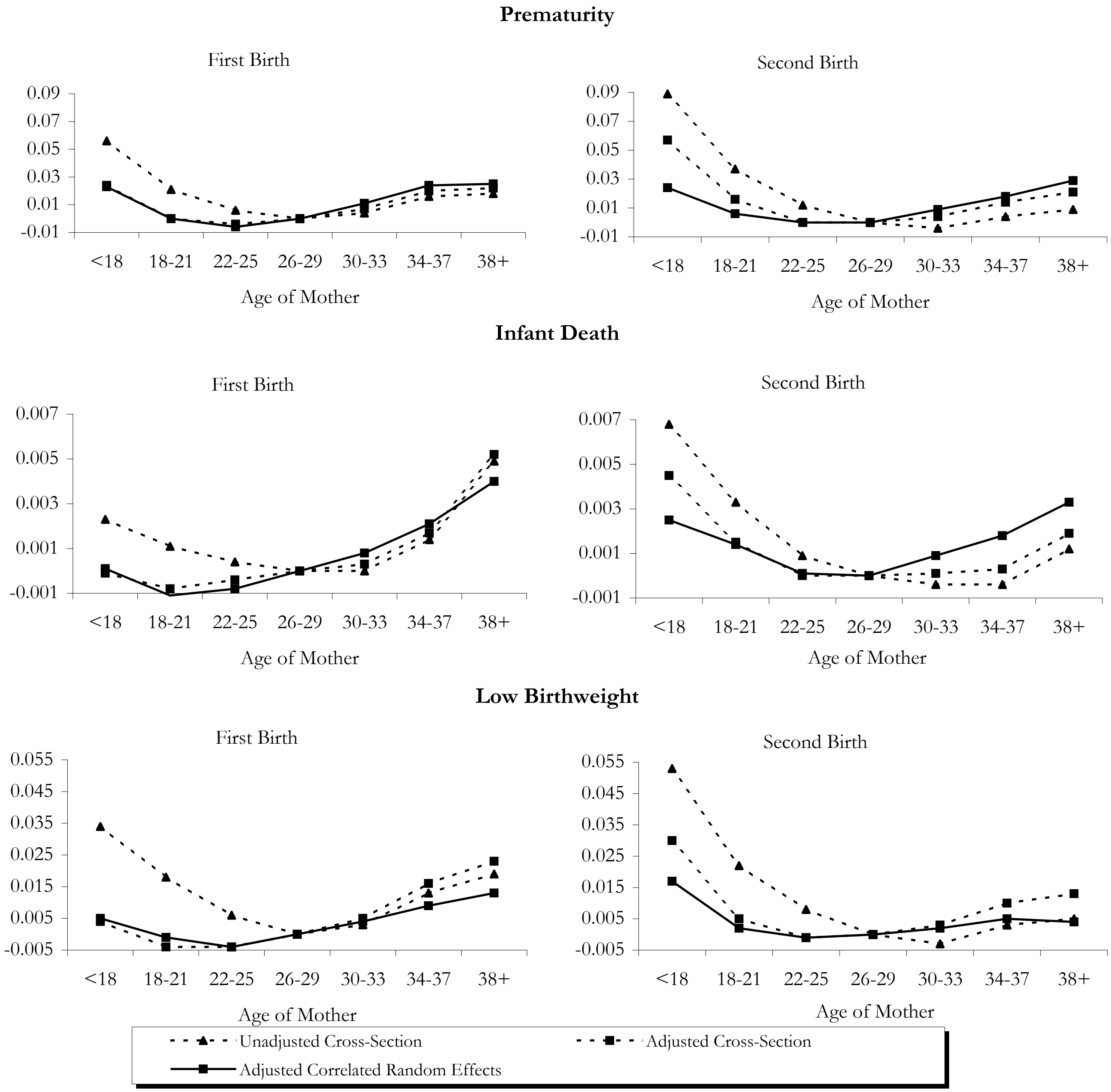
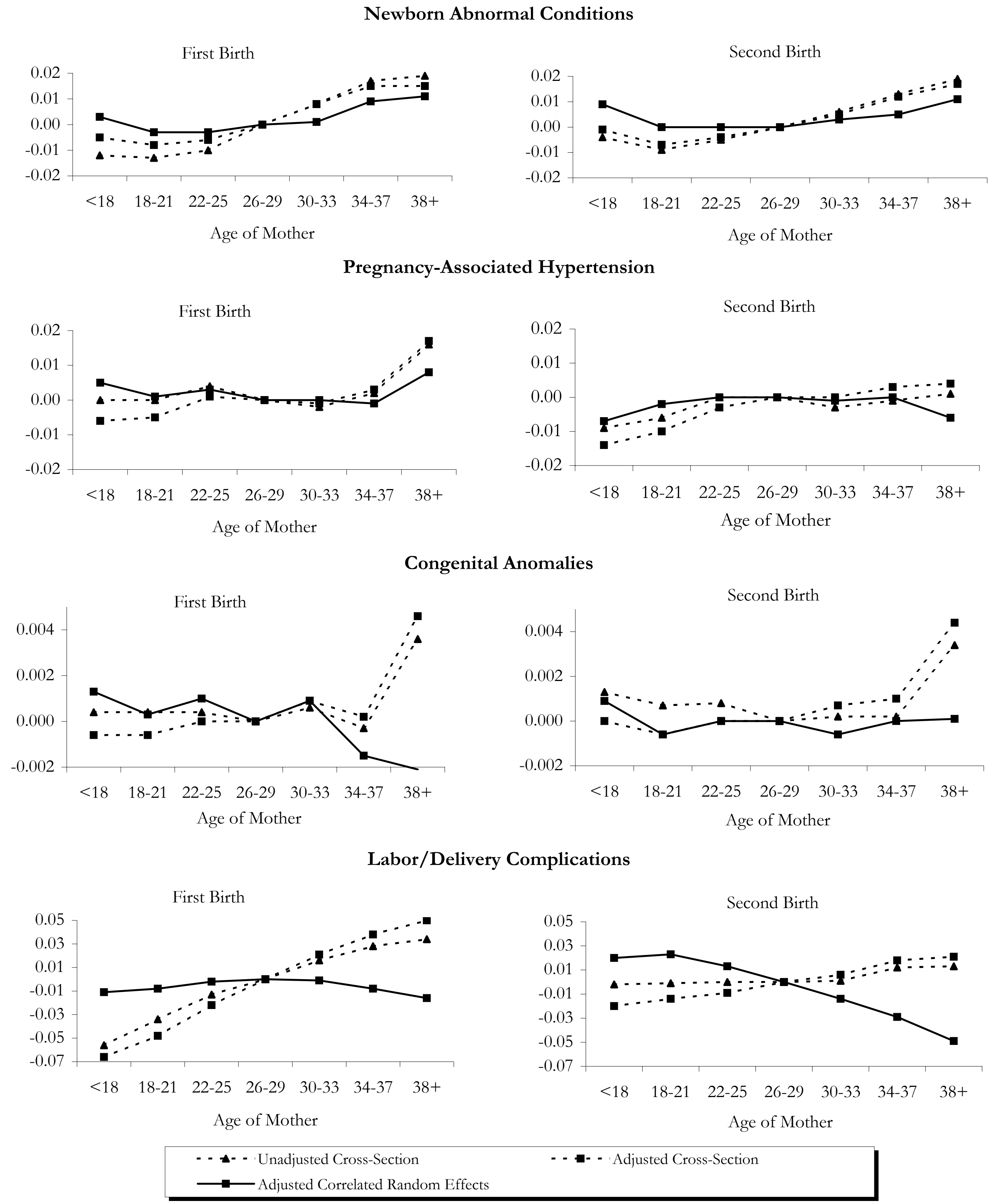


Figure 4: Estimated Age Profiles (Matched Two-Birth Mothers)



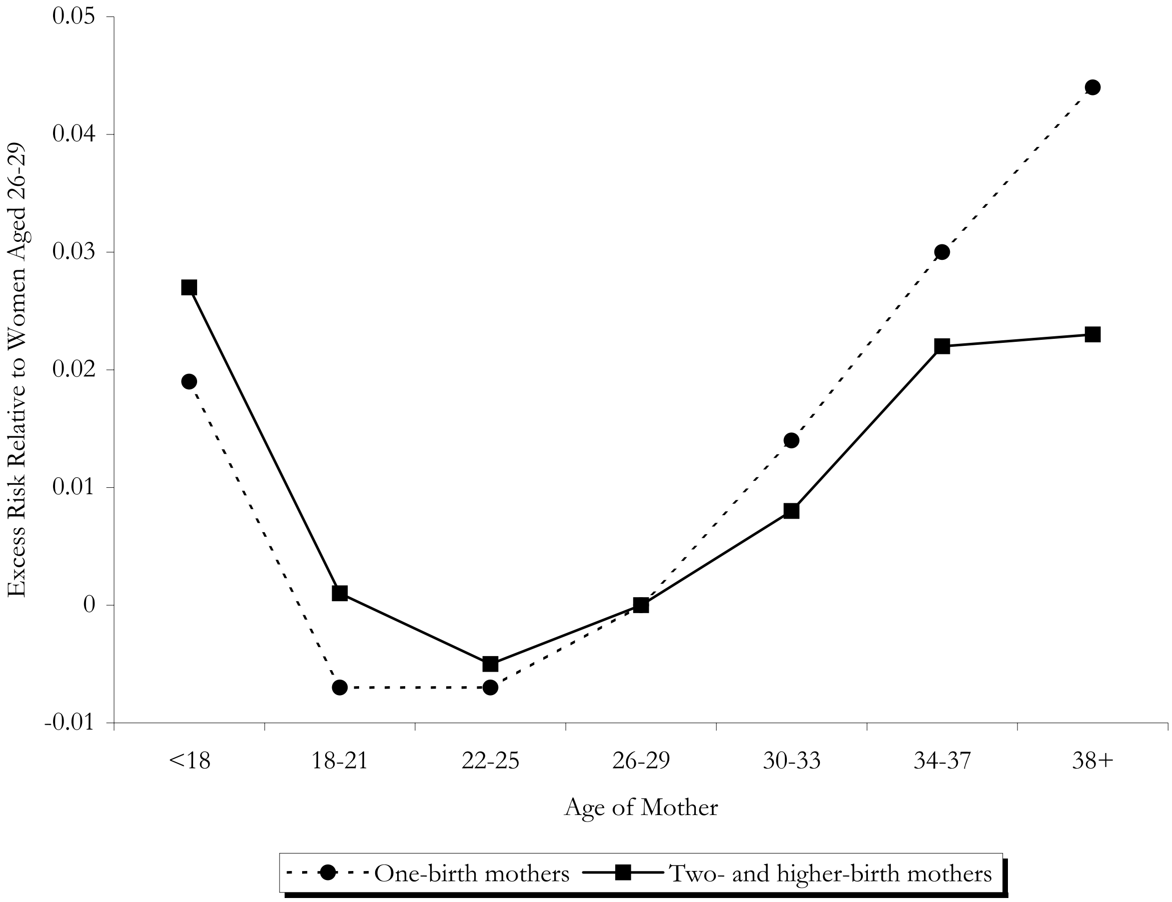
Notes: These graphs are plots of the age dummy coefficients from Table 5 and Appendix Tables A1 and A3. Each point represents the risk for a woman of that age range relative to a woman aged 26-29. The unadjusted plots represent the regression estimates unadjusted for covariates whereas the adjusted plots represent the regression estimates adjusted for covariates.

Figure 5: Estimated Age Profiles (Matched Two-Birth Mothers)



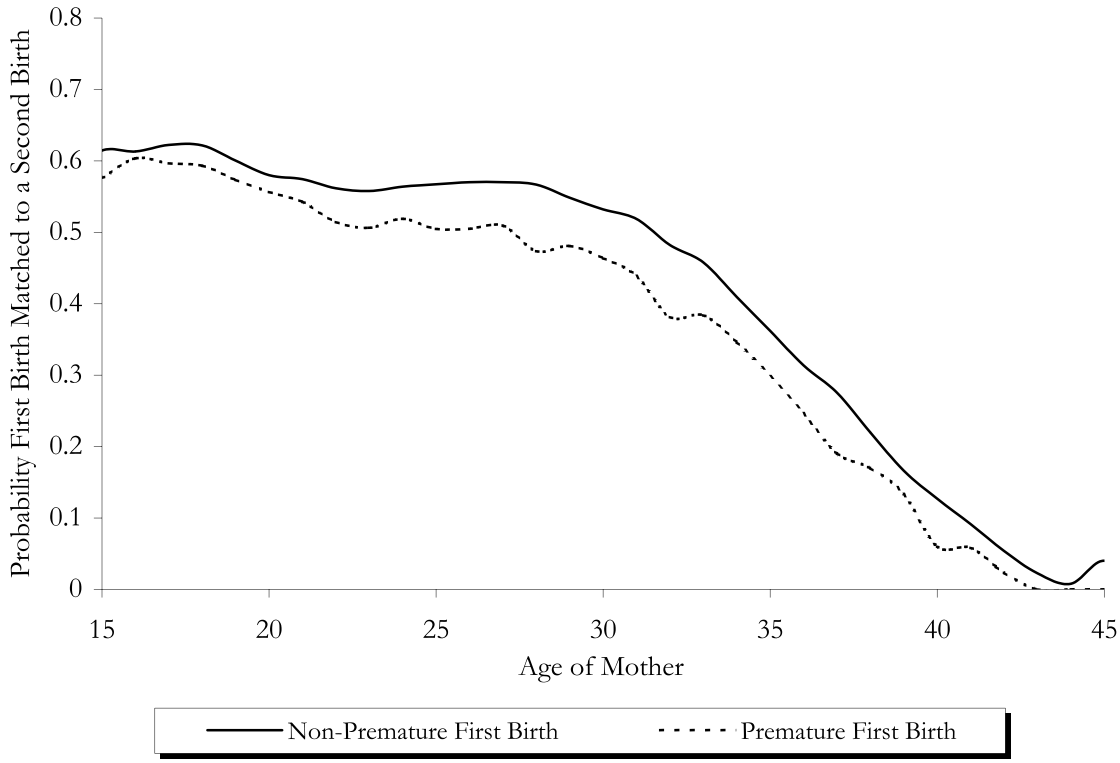
Notes: For further details, see Figure 4. These graphs are plots of the age dummy coefficients from Appendix Tables A5, A7, A9, and A11.

Figure 6: Regression-Adjusted Estimates of the First-Birth Age Profile for Prematurity (First Births Occurring Between 1991 and 1995)



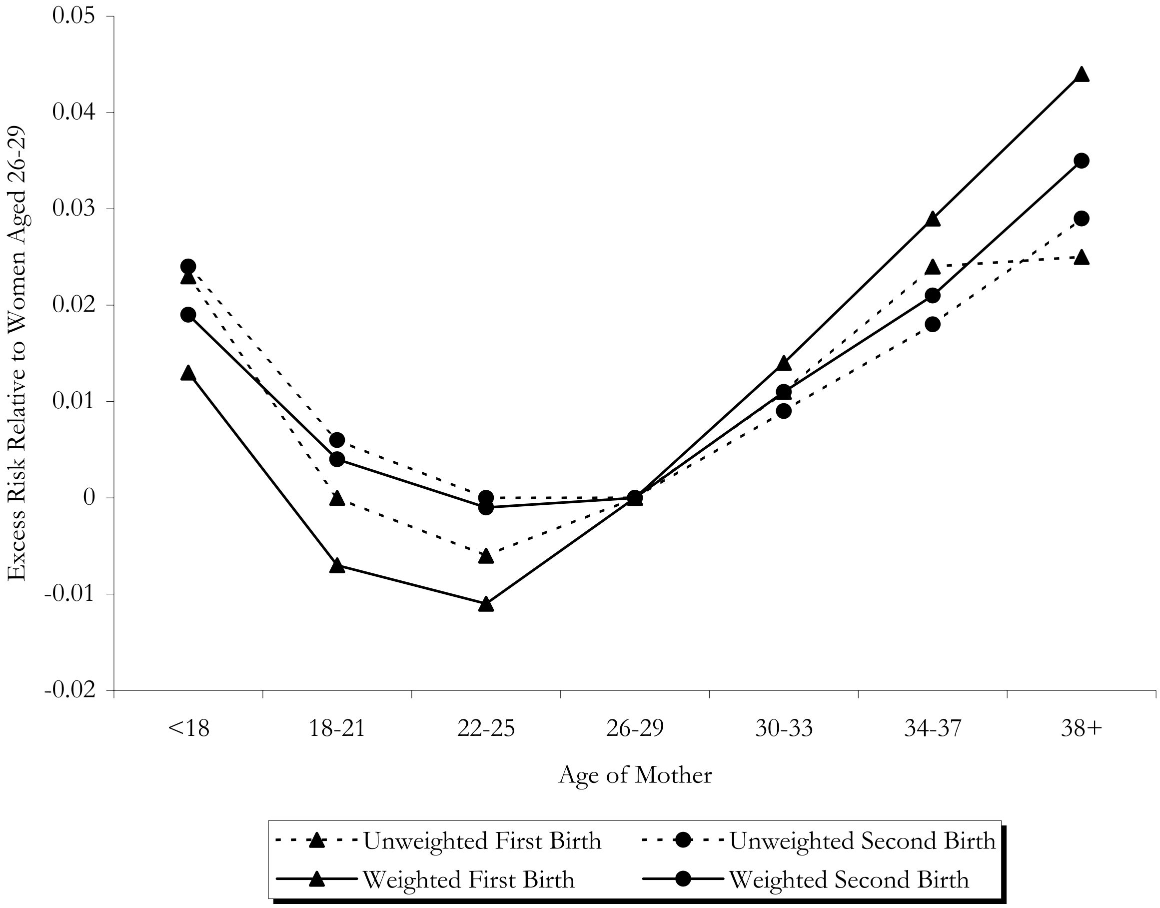
Notes: These graphs are plots of the age dummy coefficients from Table 9. This figure gives the additional risk of prematurity associated with that age group. In particular, each point represents the risk for a woman of that age range relative to a woman aged 26-29.

Figure 7: Matching Rates by Age and by First Birth Outcome (First Births Occurring Between 1989 and 1995)



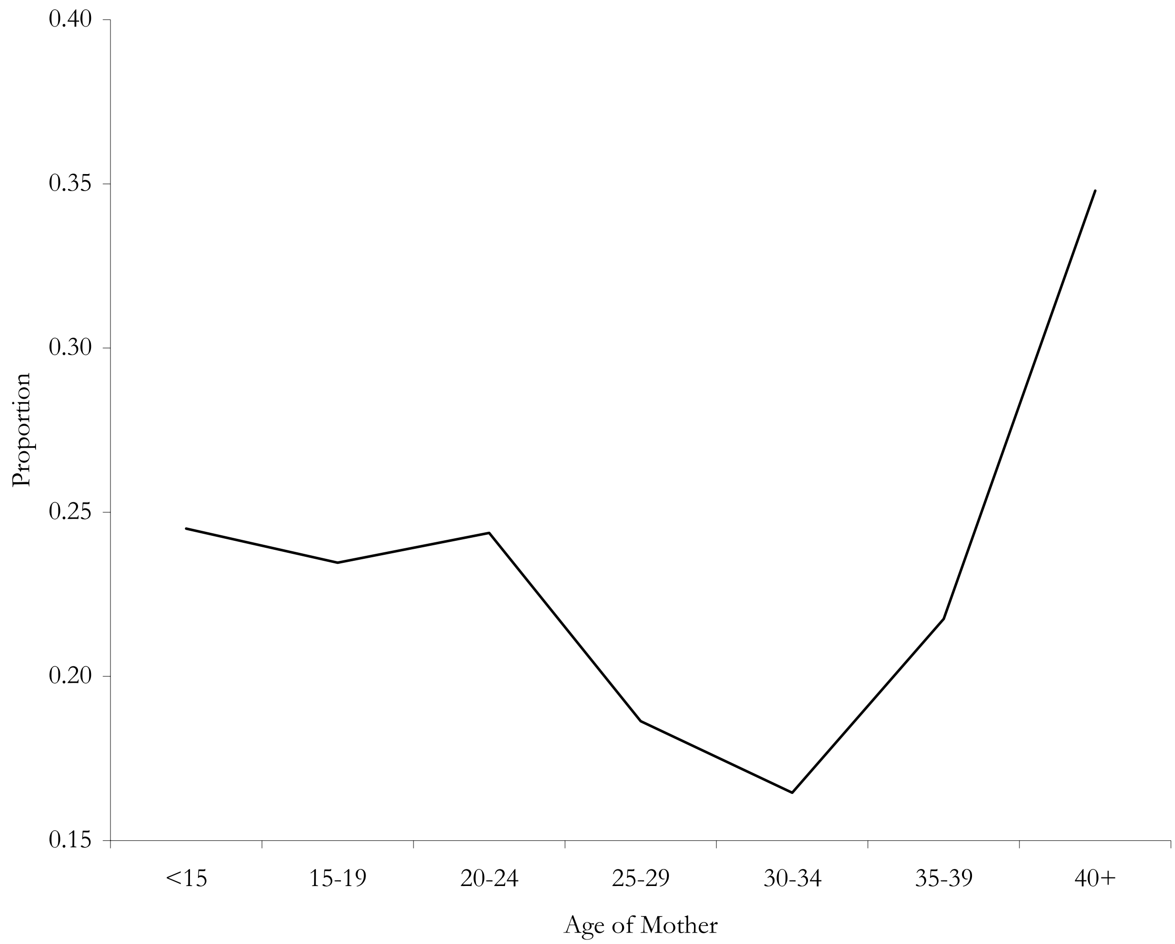
Notes: This figure is a plot of the percent of first births that are matched to a second birth conditional on the first birth outcome for the sample of one-, two-, and three-birth mothers.

Figure 8: Weighted and Unweighted Age Profiles for Prematurity (Matched Two-Birth Mothers)



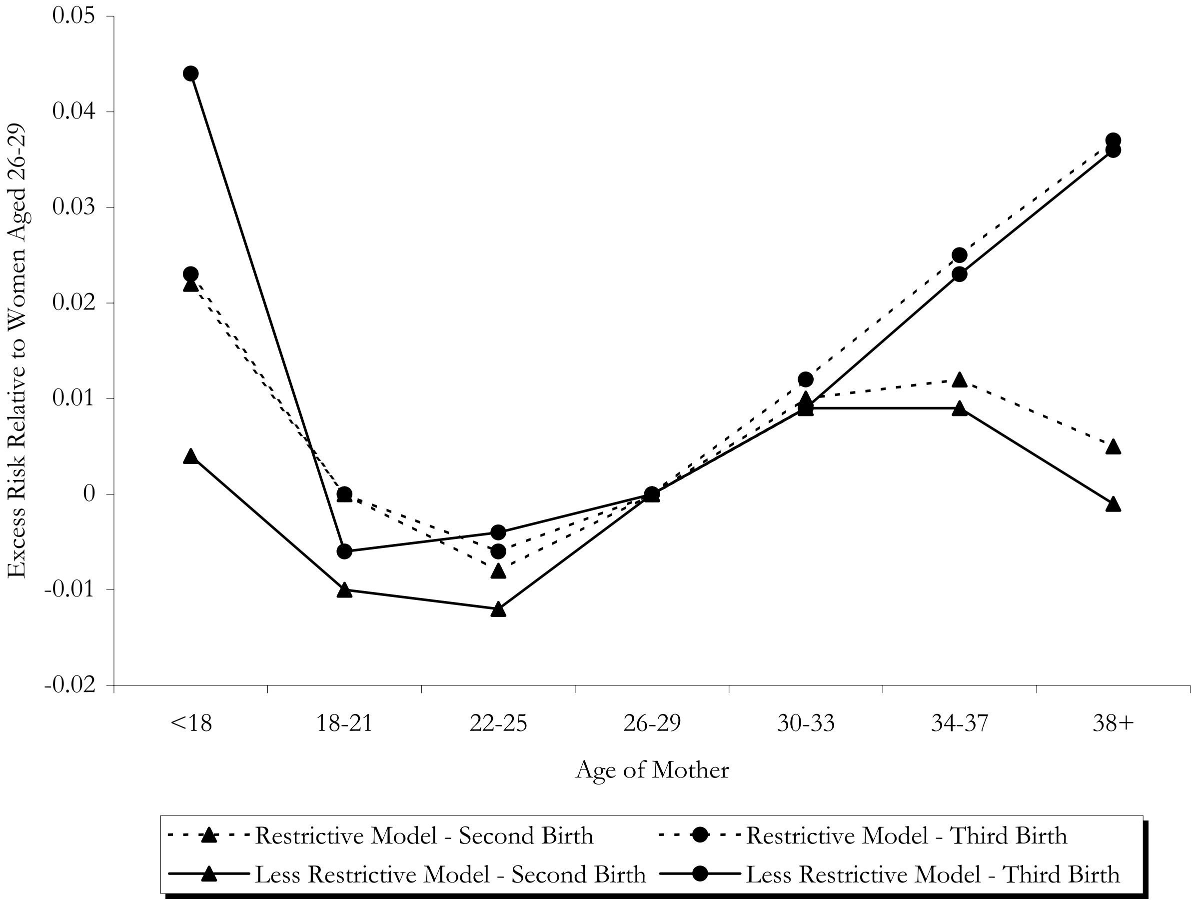
Notes: This graph is a plot of the age dummy coefficients from the correlated random-effects model in Tables 5 and Appendix Table A14. The age profiles represent the additional risk of prematurity associated with that age group. In particular, each point represents the risk for a woman of that age range relative to a woman aged 26-29. For further details on the weighting, see Appendix Table A14.

Figure 9: Proportion of Pregnancies that End in Abortion (Texas 1990-1996)



Notes: Pregnancies are the sum of abortions and live births; fetal deaths such as miscarriages and stillbirths are not included. Proportions computed using Texas natality files and aggregate abortion statistics from Texas Vital Statistics.

Figure 10: Regression-Adjusted Estimates of the Age Profiles for Prematurity for Three-Birth Mothers



Notes: These graphs are plots of the age dummy coefficients from Table 16. This figure gives the additional risk of prematurity associated with that age group. In particular, each point represents the risk for a woman of that age range relative to a woman aged 26-29. The restrictive model does not allow the characteristics of previous births to affect current birth outcomes. The less restrictive model allows the characteristics of previous births to affect subsequent birth outcomes.

Appendix Table A1: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of Infant Death Within the First Year of Life (Matched Two-Birth Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.0023	0.0068	-0.0001	0.0045	-0.0001	-0.0039	-0.0005	0.0003	0.0001	0.0025
	(0.0004)**	(0.0012)**	(0.0005)	(0.0012)**	(0.0006)	(0.0009)**	(0.0018)	(0.0010)	(0.0011)	(0.0019)
Mother's age 18-21	0.0011	0.0033	-0.0008	0.0015	0.0002	-0.0036	-0.0009	0.0003	-0.0011	0.0014
	(0.0003)**	(0.0003)**	(0.0004)*	(0.0004)**	(0.0004)	(0.0007)**	(0.0007)	(0.0007)	(0.0008)	(0.0009)
Mother's age 22-25	0.0004	0.0009	-0.0004	0.0000	0.0001	-0.0019	-0.0013	0.0002	-0.0008	0.0001
	(0.0003)	(0.0003)**	(0.0003)	(0.0003)	(0.0003)	(0.0005)**	(0.0005)*	(0.0004)	(0.0006)	(0.0006)
Mother's age 30-33	0.0000	-0.0004	0.0003	0.0001	-0.0001	0.0018	0.0021	-0.0002	0.0008	0.0009
	(0.0003)	(0.0002)	(0.0004)	(0.0002)	(0.0004)	(0.0006)**	(0.0005)**	(0.0005)	(0.0007)	(0.0005)
Mother's age 34-37	0.0014	-0.0004	0.0017	0.0003	0.0000	0.0043	0.0041	-0.0003	0.0021	0.0018
	(0.0006)*	(0.0003)	(0.0006)**	(0.0003)	(0.0006)	(0.0012)**	(0.0008)**	(0.0008)	(0.0012)	(0.0008)*
Mother's age 38+	0.0049	0.0012	0.0052	0.0019	0.0003	0.0069	0.0067	-0.0002	0.0040	0.0033
	(0.0019)*	(0.0006)	(0.0019)**	(0.0006)**	(0.0011)	(0.0028)*	(0.0014)**	(0.0013)	(0.0028)	(0.0015)*
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	360756	360756	360756	360756	360756	360756	360756	360756	360756	360756
Mean of dependent variable	0.0038	0.0033	0.0038	0.0033	0.0035	0.0038	0.0033	0.0035	0.0038	0.0033

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A2: Cross-Sectional and Panel Data Estimates of the Effect of Non-Age Covariates on the Probability of Infant Death Within the First Year of Life (Matched Two-Birth Mothers)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8a)	(8b)
Mother has less than 9 years of education	0.0000	0.0006	-0.0002	-0.0005	0.0013
	(0.0005)	(0.0006)	(0.0009)	(0.0011)	(0.0011)
Mother has 9-11 years of education	0.0001	0.0003	-0.0005	-0.0008	-0.0001
	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.0006)
Mother has 13-15 years of education	-0.0007	-0.0011	-0.0008	-0.0003	-0.0012
	(0.0003)*	(0.0003)**	(0.0004)	(0.0005)	(0.0005)*
Mother has 16+ years of education	-0.0025	-0.0016	-0.0017	-0.0020	-0.0014
	(0.0003)**	(0.0003)**	(0.0007)*	(0.0007)**	(0.0007)*
Father absent	0.0007	0.0014	0.0009	0.0004	0.0020
	(0.0005)	(0.0005)*	(0.0004)*	(0.0006)	(0.0008)*
Mother married	-0.0005	-0.0012	-0.0001	-0.0002	0.0004
	(0.0004)	(0.0004)**	(0.0004)	(0.0005)	(0.0006)
Mother smoked during pregnancy	0.0014	0.0027	0.0011	0.0002	0.0015
	(0.0005)*	(0.0005)**	(0.0006)	(0.0009)	(0.0009)
Mother drank during pregnancy	0.0037	0.0008	0.0017	0.0017	0.0018
	(0.0015)*	(0.0012)	(0.0011)	(0.0021)	(0.0016)
Mother black	0.0026	0.0014			
	(0.0005)**	(0.0004)**			
Mother American Indian	-0.0015	0.0053			
	(0.0021)	(0.0037)			
Mother Asian	0.0003	0.0008			
	(0.0006)	(0.0006)			
Mother other race	-0.0034	0.0041			
	(0.0003)**	(0.0068)			
Mother Hispanic	-0.0007	-0.0001			
	(0.0003)*	(0.0003)			
Mother immigrant	-0.0006	-0.0003			
	(0.0003)	(0.0003)			
Observations (mothers)	360756	360756	360756	360756	360756
Mean of dependent variable	0.0038	0.0033	0.0035	0.0038	0.0033

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Appendix Table A1, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A3: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of a Low Birthweight Birth (Matched Two-Birth Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.034	0.053	0.004	0.030	0.050	-0.001	0.008	0.009	0.005	0.017
	(0.002)**	(0.004)**	(0.002)	(0.004)**	(0.002)**	(0.003)	(0.005)	(0.003)**	(0.004)	(0.006)**
Mother's age 18-21	0.018	0.022	-0.004	0.005	0.028	-0.005	-0.004	-0.001	-0.001	0.002
	(0.001)**	(0.001)**	(0.001)*	(0.001)**	(0.001)**	(0.002)*	(0.002)	(0.002)	(0.003)	(0.003)
Mother's age 22-25	0.006	0.008	-0.004	-0.001	0.011	-0.006	-0.004	-0.003	-0.004	-0.001
	(0.001)**	(0.001)**	(0.001)**	(0.001)	(0.001)**	(0.002)**	(0.002)*	(0.001)*	(0.002)*	(0.002)
Mother's age 30-33	0.003	-0.003	0.005	0.003	-0.013	0.006	0.005	0.001	0.004	0.002
	(0.001)*	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**	(0.002)**	(0.002)	(0.002)	(0.002)
Mother's age 34-37	0.013	0.003	0.016	0.010	-0.027	0.013	0.010	0.001	0.009	0.005
	(0.002)**	(0.001)*	(0.002)**	(0.001)**	(0.002)**	(0.004)**	(0.003)**	(0.003)	(0.004)*	(0.003)
Mother's age 38+	0.019	0.005	0.023	0.013	-0.046	0.019	0.011	-0.005	0.013	0.004
	(0.006)**	(0.002)*	(0.006)**	(0.002)**	(0.004)**	(0.008)*	(0.005)*	(0.005)	(0.008)	(0.005)
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	360504	360504	360504	360504	360504	360504	360504	360504	360504	360504
Mean of dependent variable	0.0626	0.0427	0.0626	0.0427	0.0527	0.0626	0.0427	0.0527	0.0626	0.0427

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A4: Cross-Sectional and Panel Data Estimates of the Effect of Non-Age Covariates on the Probability of a Low Birthweight Birth (Matched Two-Birth Mothers)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8a)	(8b)
Mother has less than 9 years of education	0.008	0.005	0.001	0.003	0.002
	(0.002)**	(0.002)**	(0.003)	(0.004)	(0.004)
Mother has 9-11 years of education	0.005	0.005	-0.003	-0.002	-0.002
	(0.001)**	(0.001)**	(0.002)	(0.002)	(0.002)
Mother has 13-15 years of education	-0.006	-0.004	-0.000	-0.001	-0.001
	(0.001)**	(0.001)**	(0.002)	(0.002)	(0.002)
Mother has 16+ years of education	-0.016	-0.013	0.001	-0.002	0.000
	(0.001)**	(0.001)**	(0.002)	(0.002)	(0.002)
Father absent	0.001	0.011	0.003	-0.001	0.009
	(0.002)	(0.002)**	(0.001)*	(0.002)	(0.002)**
Mother married	-0.010	-0.005	-0.005	-0.007	0.002
	(0.001)**	(0.001)**	(0.001)**	(0.002)**	(0.002)
Mother smoked during pregnancy	0.037	0.035	0.013	0.010	0.015
	(0.002)**	(0.002)**	(0.002)**	(0.003)**	(0.003)**
Mother drank during pregnancy	0.007	0.017	0.022	0.010	0.034
	(0.005)	(0.004)**	(0.004)**	(0.006)	(0.005)**
Mother black	0.055	0.043			
	(0.002)**	(0.002)**			
Mother American Indian	-0.011	-0.000			
	(0.008)	(0.008)			
Mother Asian	0.030	0.023			
	(0.003)**	(0.002)**			
Mother other race	0.001	-0.007			
	(0.022)	(0.014)			
Mother Hispanic	0.010	0.008			
	(0.001)**	(0.001)**			
Mother immigrant	-0.007	-0.007			
	(0.001)**	(0.001)**			
Observations (mothers)	360504	360504	360504	360504	360504
Mean of dependent variable	0.0626	0.0427	0.0527	0.0626	0.0427

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Appendix Table A3, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A5: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of an Infant with an Abnormal Condition (Matched Two-Birth Mothers)

	Estimation Method:									
	Cross-Section				Panel Data					
					Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	-0.012	-0.004	-0.005	-0.001	-0.013	-0.027	-0.017	0.002	0.003	0.009
	(0.001)**	(0.002)	(0.002)**	(0.003)	(0.002)**	(0.003)**	(0.004)**	(0.003)	(0.003)	(0.004)*
Mother's age 18-21	-0.013	-0.009	-0.008	-0.007	-0.014	-0.025	-0.019	-0.003	-0.003	0.000
	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**	(0.002)**	(0.002)	(0.003)	(0.003)
Mother's age 22-25	-0.010	-0.005	-0.006	-0.004	-0.007	-0.015	-0.010	-0.002	-0.003	0.000
	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**	(0.002)**	(0.001)	(0.002)	(0.002)
Mother's age 30-33	0.008	0.006	0.008	0.005	0.004	0.010	0.012	-0.000	0.001	0.003
	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**	(0.002)**	(0.001)	(0.002)	(0.002)
Mother's age 34-37	0.017	0.013	0.015	0.012	0.008	0.026	0.022	-0.001	0.009	0.005
	(0.002)**	(0.001)**	(0.002)**	(0.001)**	(0.002)**	(0.004)**	(0.003)**	(0.003)	(0.004)*	(0.003)
Mother's age 38+	0.019	0.019	0.015	0.017	0.012	0.033	0.036	-0.002	0.011	0.011
	(0.005)**	(0.002)**	(0.005)**	(0.002)**	(0.003)**	(0.008)**	(0.005)**	(0.004)	(0.008)	(0.006)*
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	360756	360756	360756	360756	360756	360756	360756	360756	360756	360756
Mean of dependent variable	0.0413	0.0429	0.0413	0.0429	0.0421	0.0413	0.0429	0.0421	0.0413	0.0429

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. These newborn conditions exclude other unspecified conditions. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A6: Cross-Sectional and Panel Data Estimates of the Effect of Covariates on the Probability of an Infant with an Abnormal Condition (Matched Two-Birth Mothers)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8a)	(8b)
Mother has less than 9 years of education	-0.005	-0.000	-0.003	-0.002	-0.001
	(0.001)**	(0.001)	(0.003)	(0.003)	(0.003)
Mother has 9-11 years of education	-0.003	-0.000	-0.001	-0.001	-0.001
	(0.001)**	(0.001)	(0.002)	(0.002)	(0.002)
Mother has 13-15 years of education	0.005	0.006	0.003	0.003	0.003
	(0.001)**	(0.001)**	(0.001)*	(0.002)	(0.002)
Mother has 16+ years of education	0.007	0.004	0.002	0.003	0.001
	(0.001)**	(0.001)**	(0.002)	(0.003)	(0.002)
Father absent	-0.002	-0.001	-0.001	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Mother married	0.002	0.002	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Mother smoked during pregnancy	0.006	0.007	0.009	0.006	0.011
	(0.002)**	(0.001)**	(0.002)**	(0.002)**	(0.002)**
Mother drank during pregnancy	0.005	-0.003	0.001	0.005	-0.003
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Mother black	0.004	0.006			
	(0.001)**	(0.001)**			
Mother American Indian	0.000	0.004			
	(0.008)	(0.008)			
Mother Asian	-0.007	-0.008			
	(0.002)**	(0.002)**			
Mother other race	0.014	-0.022			
	(0.020)	(0.007)**			
Mother Hispanic	-0.006	-0.006			
	(0.001)**	(0.001)**			
Mother immigrant	-0.009	-0.011			
	(0.001)**	(0.001)**			
Observations (mothers)	360756	360756	360756	360756	360756
Mean of dependent variable	0.0413	0.0429	0.0421	0.0413	0.0429

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. These newborn conditions exclude other unspecified conditions. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Appendix Table A5, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A7: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of Pregnancy-Associated Hypertension (Matched Two-Birth Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	-0.000	-0.009	-0.006	-0.014	0.060	-0.019	-0.025	0.007	0.005	-0.007
	(0.001)	(0.002)**	(0.002)**	(0.002)**	(0.002)**	(0.003)**	(0.004)**	(0.003)*	(0.003)	(0.005)
Mother's age 18-21	0.000	-0.006	-0.005	-0.010	0.036	-0.015	-0.016	-0.001	0.001	-0.002
	(0.001)	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.002)**	(0.002)**	(0.002)	(0.003)	(0.003)
Mother's age 22-25	0.004	-0.000	0.001	-0.003	0.018	-0.005	-0.008	-0.001	0.003	-0.000
	(0.001)**	(0.001)	(0.001)	(0.001)**	(0.001)**	(0.002)**	(0.002)**	(0.001)	(0.002)	(0.002)
Mother's age 30-33	-0.002	-0.003	-0.001	-0.000	-0.020	0.006	0.005	-0.002	-0.000	-0.001
	(0.001)	(0.001)**	(0.001)	(0.001)	(0.001)**	(0.002)**	(0.002)**	(0.001)	(0.002)	(0.002)
Mother's age 34-37	0.002	-0.001	0.003	0.003	-0.038	0.011	0.014	-0.002	-0.001	0.000
	(0.002)	(0.001)	(0.002)	(0.001)*	(0.002)**	(0.004)**	(0.003)**	(0.003)	(0.004)	(0.003)
Mother's age 38+	0.016	0.001	0.017	0.004	-0.063	0.024	0.013	-0.009	0.008	-0.006
	(0.006)**	(0.002)	(0.006)**	(0.002)*	(0.003)**	(0.007)**	(0.005)**	(0.004)*	(0.007)	(0.005)
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	360756	360756	360756	360756	360756	360756	360756	360756	360756	360756
Mean of dependent variable	0.0572	0.0298	0.0572	0.0298	0.0435	0.0572	0.0298	0.0435	0.0572	0.0298

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A8: Cross-Sectional and Panel Data Estimates of the Effect of Covariates on the Probability of Pregnancy-Associated Hypertension (Matched Two-Birth Mothers)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8a)	(8b)
Mother has less than 9 years of education	0.001	0.001	-0.001	-0.004	0.002
	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)
Mother has 9-11 years of education	-0.001	-0.001	-0.002	-0.003	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Mother has 13-15 years of education	0.001	-0.001	0.004	0.004	0.002
	(0.001)	(0.001)	(0.001)**	(0.002)*	(0.002)
Mother has 16+ years of education	-0.011	-0.010	0.003	0.000	0.002
	(0.001)**	(0.001)**	(0.002)	(0.002)	(0.002)
Father absent	0.001	0.004	-0.004	-0.003	0.000
	(0.001)	(0.001)**	(0.001)**	(0.002)	(0.002)
Mother married	0.001	0.001	-0.004	0.001	0.000
	(0.001)	(0.001)	(0.001)**	(0.002)	(0.002)
Mother smoked during pregnancy	-0.013	-0.006	0.002	-0.001	0.004
	(0.002)**	(0.001)**	(0.002)	(0.002)	(0.002)*
Mother drank during pregnancy	-0.001	-0.002	0.005	0.004	0.004
	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)
Mother black	0.002	0.006			
	(0.001)	(0.001)**			
Mother American Indian	0.005	0.027			
	(0.010)	(0.009)**			
Mother Asian	-0.020	-0.010			
	(0.002)**	(0.001)**			
Mother other race	-0.036	-0.008			
	(0.009)**	(0.010)			
Mother Hispanic	-0.002	0.001			
	(0.001)	(0.001)			
Mother immigrant	-0.016	-0.008			
	(0.001)**	(0.001)**			
Observations (mothers)	360756	360756	360756	360756	360756
Mean of dependent variable	0.0572	0.0298	0.0435	0.0572	0.0298

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Appendix Table A7, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A9: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of an Infant with a Congenital Anomaly (Matched Two-Birth Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.0004	0.0013	-0.0006	0.0000	0.0049	0.0016	0.0028	0.0009	0.0013	0.0009
	(0.0006)	(0.0012)	(0.0008)	(0.0013)	(0.0009)**	(0.0014)	(0.0021)	(0.0016)	(0.0018)	(0.0023)
Mother's age 18-21	0.0004	0.0007	-0.0006	-0.0006	0.0029	0.0006	0.0006	-0.0002	0.0003	-0.0006
	(0.0005)	(0.0005)	(0.0006)	(0.0005)	(0.0007)**	(0.0012)	(0.0011)	(0.0011)	(0.0014)	(0.0013)
Mother's age 22-25	0.0004	0.0008	0.0000	0.0000	0.0020	0.0011	0.0006	0.0004	0.0010	0.0000
	(0.0005)	(0.0004)	(0.0005)	(0.0005)	(0.0005)**	(0.0009)	(0.0008)	(0.0007)	(0.0010)	(0.0009)
Mother's age 30-33	0.0006	0.0002	0.0009	0.0007	-0.0019	0.0006	-0.0010	-0.0004	0.0009	-0.0006
	(0.0006)	(0.0005)	(0.0006)	(0.0005)	(0.0006)**	(0.0011)	(0.0009)	(0.0007)	(0.0011)	(0.0010)
Mother's age 34-37	-0.0003	0.0002	0.0002	0.0010	-0.0041	-0.0021	-0.0007	-0.0011	-0.0015	-0.0000
	(0.0009)	(0.0006)	(0.0009)	(0.0006)	(0.0010)**	(0.0020)	(0.0015)	(0.0013)	(0.0021)	(0.0016)
Mother's age 38+	0.0036	0.0034	0.0046	0.0044	-0.0041	-0.0030	-0.0009	0.0002	-0.0021	0.0001
	(0.0025)	(0.0011)**	(0.0025)	(0.0011)**	(0.0017)*	(0.0043)	(0.0025)	(0.0022)	(0.0043)	(0.0027)
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	360756	360756	360756	360756	360756	360756	360756	360756	360756	360756
Mean of dependent variable	0.0102	0.0084	0.0102	0.0084	0.0093	0.0102	0.0084	0.0093	0.0102	0.0084

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A10: Cross-Sectional and Panel Data Estimates of the Effect of Covariates on the Probability of an Infant with a Congenital Anomaly (Matched Two-Birth Mothers)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8a)	(8b)
Mother has less than 9 years of education	-0.0007	0.0009	-0.0004	-0.0009	0.0009
	(0.0007)	(0.0008)	(0.0014)	(0.0014)	(0.0014)
Mother has 9-11 years of education	0.0005	0.0004	-0.0002	-0.0004	-0.0001
	(0.0005)	(0.0005)	(0.0008)	(0.0009)	(0.0009)
Mother has 13-15 years of education	-0.0007	-0.0002	-0.0010	-0.0011	-0.0009
	(0.0005)	(0.0005)	(0.0007)	(0.0008)	(0.0008)
Mother has 16+ years of education	-0.0008	-0.0024	-0.0010	-0.0001	-0.0016
	(0.0006)	(0.0005)**	(0.0012)	(0.0013)	(0.0012)
Father absent	0.0010	-0.0000	0.0007	0.0014	0.0002
	(0.0006)	(0.0007)	(0.0007)	(0.0009)	(0.0010)
Mother married	0.0000	-0.0003	-0.0001	0.0006	-0.0005
	(0.0005)	(0.0005)	(0.0006)	(0.0008)	(0.0008)
Mother smoked during pregnancy	0.0034	0.0023	0.0019	0.0025	0.0014
	(0.0009)**	(0.0008)**	(0.0010)	(0.0013)	(0.0012)
Mother drank during pregnancy	0.0044	0.0040	0.0045	0.0033	0.0055
	(0.0023)	(0.0021)	(0.0018)*	(0.0030)	(0.0028)*
Mother black	0.0017	0.0027			
	(0.0007)*	(0.0006)**			
Mother American Indian	0.0007	0.0023			
	(0.0042)	(0.0042)			
Mother Asian	-0.0007	0.0007			
	(0.0010)	(0.0010)			
Mother other race	-0.0076	-0.0070			
	(0.0005)**	(0.0004)**			
Mother Hispanic	-0.0009	-0.0006			
	(0.0005)	(0.0004)			
Mother immigrant	-0.0008	-0.0003			
	(0.0005)	(0.0005)			
Observations (mothers)	360756	360756	360756	360756	360756
Mean of dependent variable	0.0102	0.0084	0.0093	0.0102	0.0084

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Appendix Table A9, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A11: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of Labor/Delivery Complications (Matched Two-Birth Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	-0.056	-0.002	-0.066	-0.020	0.195	-0.003	0.049	-0.009	-0.011	0.020
	(0.002)**	(0.004)	(0.003)**	(0.004)**	(0.003)**	(0.005)	(0.007)**	(0.006)	(0.007)	(0.008)*
Mother's age 18-21	-0.034	-0.001	-0.048	-0.014	0.150	0.005	0.040	0.003	-0.008	0.023
	(0.002)**	(0.002)	(0.002)**	(0.002)**	(0.002)**	(0.004)	(0.004)**	(0.004)	(0.005)	(0.005)**
Mother's age 22-25	-0.013	-0.000	-0.022	-0.009	0.081	0.006	0.020	0.007	-0.002	0.013
	(0.002)**	(0.002)	(0.002)**	(0.002)**	(0.002)**	(0.003)	(0.003)**	(0.003)*	(0.004)	(0.003)**
Mother's age 30-33	0.016	0.001	0.021	0.006	-0.099	-0.009	-0.019	-0.026	-0.001	-0.014
	(0.003)**	(0.002)	(0.003)**	(0.002)**	(0.002)**	(0.004)*	(0.004)**	(0.003)**	(0.004)	(0.004)**
Mother's age 34-37	0.028	0.012	0.038	0.018	-0.208	-0.023	-0.041	-0.064	-0.008	-0.029
	(0.004)**	(0.002)**	(0.004)**	(0.002)**	(0.004)**	(0.008)**	(0.006)**	(0.005)**	(0.008)	(0.006)**
Mother's age 38+	0.034	0.013	0.050	0.021	-0.329	-0.038	-0.066	-0.114	-0.016	-0.049
	(0.010)**	(0.004)**	(0.010)**	(0.004)**	(0.007)**	(0.015)*	(0.009)**	(0.008)**	(0.015)	(0.010)**
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	360756	360756	360756	360756	360756	360756	360756	360756	360756	360756
Mean of dependent variable	0.2146	0.1209	0.2146	0.1209	0.1678	0.2146	0.1209	0.1678	0.2146	0.1209

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A12: Cross-Sectional and Panel Data Estimates of the Effect of Covariates on the Probability of Labor/Delivery Complications (Matched Two-Birth Mothers)

	Estimation Method:				
			Panel Data		
	Cross-Section		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth		1st birth	2nd birth
	(3)	(4)	(7)	(8a)	(8b)
Mother has less than 9 years of education	-0.009	0.012	-0.017	-0.024	0.010
	(0.003)**	(0.003)**	(0.005)**	(0.006)**	(0.006)
Mother has 9-11 years of education	-0.002	0.005	-0.003	-0.007	0.003
	(0.002)	(0.002)**	(0.003)	(0.003)	(0.003)
Mother has 13-15 years of education	0.001	-0.002	0.004	0.004	0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Mother has 16+ years of education	-0.016	-0.011	0.003	-0.005	0.003
	(0.002)**	(0.002)**	(0.004)	(0.005)	(0.005)
Father absent	-0.005	0.004	-0.010	-0.009	0.002
	(0.002)	(0.002)	(0.003)**	(0.003)**	(0.004)
Mother married	-0.003	-0.005	-0.008	-0.000	-0.002
	(0.002)	(0.002)**	(0.002)**	(0.003)	(0.003)
Mother smoked during pregnancy	0.027	0.031	0.026	0.023	0.029
	(0.003)**	(0.002)**	(0.004)**	(0.005)**	(0.004)**
Mother drank during pregnancy	0.013	0.019	0.017	0.017	0.017
	(0.008)	(0.006)**	(0.007)**	(0.009)	(0.010)
Mother black	0.025	0.030			
	(0.003)**	(0.002)**			
Mother American Indian	0.017	0.029			
	(0.016)	(0.014)*			
Mother Asian	0.020	0.002			
	(0.005)**	(0.004)			
Mother other race	0.008	0.013			
	(0.038)	(0.028)			
Mother Hispanic	0.027	0.022			
	(0.002)**	(0.002)**			
Mother immigrant	-0.013	0.009			
	(0.002)**	(0.002)**			
Observations (mothers)	360756	360756	360756	360756	360756
Mean of dependent variable	0.2146	0.1209	0.1678	0.2146	0.1209

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. These labor/delivery complications exclude other unspecified conditions. The excluded group includes white, native, non-Hispanic women with 12 years of education. This table is a continuation of Appendix Table A11, which presents the maternal age coefficients. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A13: Cross-Sectional and Panel Data Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (2+ Birth Matched Mothers)

	Estimation Method:									
					Panel Data					
	Cross-Section				Fixed Effects	Correlated Random Effects		Fixed Effects	Correlated Random Effects	
	1st birth	2nd birth	1st birth	2nd birth		1st birth	2nd birth		1st birth	2nd birth
	(1)	(2)	(3)	(4)	(5)	(6a)	(6b)	(7)	(8a)	(8b)
Mother's age <18	0.056	0.090	0.023	0.057	0.017	0.025	0.042	0.006	0.014	0.022
	(0.001)**	(0.003)**	(0.002)**	(0.003)**	(0.002)**	(0.003)**	(0.005)**	(0.004)	(0.004)**	(0.006)**
Mother's age 18-21	0.023	0.040	0.001	0.017	0.003	0.001	0.018	-0.004	-0.007	0.002
	(0.001)**	(0.001)**	(0.001)	(0.001)**	(0.002)	(0.003)	(0.003)**	(0.003)	(0.003)*	(0.003)
Mother's age 22-25	0.007	0.013	-0.003	0.001	-0.000	-0.006	0.004	-0.003	-0.009	-0.003
	(0.001)**	(0.001)**	(0.001)*	(0.001)	(0.001)	(0.002)**	(0.002)*	(0.002)	(0.002)**	(0.002)
Mother's age 30-33	0.005	-0.005	0.008	0.004	0.002	0.008	0.006	0.004	0.013	0.011
	(0.001)**	(0.001)**	(0.001)**	(0.001)**	(0.001)	(0.002)**	(0.002)**	(0.002)*	(0.003)**	(0.002)**
Mother's age 34-37	0.016	0.004	0.020	0.014	-0.001	0.014	0.010	0.005	0.023	0.020
	(0.002)**	(0.002)*	(0.002)**	(0.002)**	(0.002)	(0.005)**	(0.003)**	(0.003)	(0.005)**	(0.004)**
Mother's age 38+	0.020	0.010	0.024	0.021	-0.003	0.016	0.016	0.005	0.029	0.031
	(0.006)**	(0.003)**	(0.006)**	(0.003)**	(0.005)	(0.009)	(0.006)**	(0.006)	(0.010)**	(0.006)**
Controls										
Maternal education	N	N	Y	Y	N	N	N	Y	Y	Y
Marital status	N	N	Y	Y	N	N	N	Y	Y	Y
Absence of father	N	N	Y	Y	N	N	N	Y	Y	Y
Smoking/drinking behavior	N	N	Y	Y	N	N	N	Y	Y	Y
Maternal race/ethnicity	N	N	Y	Y	N	N	N	NA	NA	NA
Residential zip code	N	N	Y	Y	N	N	N	Y	Y	Y
Birth year of infant	N	N	Y	Y	N	N	N	Y	Y	Y
Observations (mothers)	466564	466564	466564	466564	466564	466564	466564	466564	466564	466564
Mean of dependent variable	0.0830	0.0819	0.0830	0.0819	0.0825	0.0830	0.0819	0.0825	0.0830	0.0819

Notes: Sample only includes the first and second births of mothers residing in Texas who gave birth between 1991 and 2001. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.

Appendix Table A14: Correlated Random-Effects Weighted Estimates of the Effect of Maternal Age on the Probability of a Premature Birth (Matched Two-Birth Mothers)

	1st birth	2nd birth	1st birth	2nd birth
	(1a)	(1b)	(2a)	(2b)
Mother's age <18	0.020	0.032	0.013	0.019
	(0.004)**	(0.007)**	(0.006)*	(0.008)*
Mother's age 18-21	-0.003	0.013	-0.007	0.004
	(0.003)	(0.003)**	(0.004)	(0.004)
Mother's age 22-25	-0.009	0.004	-0.011	-0.001
	(0.003)**	(0.002)	(0.003)**	(0.003)
Mother's age 30-33	0.011	0.008	0.014	0.011
	(0.004)**	(0.003)**	(0.004)**	(0.003)**
Mother's age 34-37	0.024	0.015	0.029	0.021
	(0.007)**	(0.005)**	(0.007)**	(0.005)**
Mother's age 38+	0.037	0.027	0.044	0.035
	(0.014)**	(0.008)**	(0.014)**	(0.009)**
Controls				
Maternal education	N	N	Y	Y
Marital status	N	N	Y	Y
Absence of father	N	N	Y	Y
Smoking/drinking behavior	N	N	Y	Y
Residential zip code	N	N	Y	Y
Birth year of infant	N	N	Y	Y
Observations (mothers)	360070	360070	360070	360070
Mean of dependent variable	0.0934	0.0841	0.0934	0.0841

Notes: Sample only includes mothers residing in Texas who gave birth between 1991 and 2001. All observations are weighted to have the same distribution across cells based on age, race, Hispanic origin, immigrant status, maternal education, presence of father, and first-birth prematurity status as one-birth mothers. An age dummy for mother's age 26-29 years is excluded. Robust standard errors are in parentheses. * denotes significance at the 5% level and ** denotes significance at the 1% level.