The Effect of Female Education on Fertility and Infant Health: Evidence From School Entry Policies Using Exact Date of Birth*

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Abstract

This paper uses age-at-school-entry policies to identify the effect of female education on fertility and infant health. We focus on sharp contrasts in schooling, fertility, and infant health between women born just before and after the school entry date. School entry policies affect female education and the quality of a woman's mate and have generally small, but possibly heterogeneous, effects on fertility and infant health. We argue that school entry policies manipulate primarily the education of young women at risk of dropping out of school.

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1 Introduction

Education is widely held to be a key determinant of fertility and infant health. From a theoretical perspective, several causal channels have been emphasized. First, education raises a woman's permanent income through earnings, tilting her optimal fertility choices toward fewer offspring of higher quality (Becker 1960, Mincer 1963, Becker and Lewis 1973, Willis 1973). Second, under positive assortative mating, a woman's education is causally connected to her mate's education (Behrman and Rosenzweig 2002), so that the effect of education on household permanent income is augmented through a multiplier effect. Third, education may improve an individual's knowledge of, and ability to process information regarding, fertility options and healthy pregnancy behaviors (Grossman 1972).

On the empirical side, an extensive literature documents associations between education and fertility and infant health (Strauss and Thomas 1995). However, whether these associations represent causal relationships has been the subject of debate. Early quasi-experimental infant health research using differences in education between sisters who become mothers points toward more muted effects than the cross-sectional relationship, suggesting an important role for selection (Wolfe and Behrman 1987). On the other hand, more recent quasi-experimental infant health research focused on primary school construction programs in Taiwan (Chou, Liu, Grossman and Joyce 2007) and Indonesia (Breierova and Duflo 2004), and on college openings in the United States (Currie and Moretti 2003), finds that there is a causal effect, and that observational comparisons may even understate the true causal effect. Recent quasi-experimental fertility papers (Black, Devereux and Salvanes 2008, Leon 2004) similarly suggest the causal effect is as large as the partial correlation.[1]

In this paper, we present new evidence on the effect of female education on fertility and infant health in the United States using school entry policies as an instrument for education. In particular, we exploit the fact that the year in which a person starts school is a discontinuous function of exact date of birth. For example, in California and Texas, our two study states, one must be 5 years old

[1]Oreopoulos, Page and Stevens (2006) present some evidence to the contrary.

on December 1st (California) or September 1st (Texas) in order to begin kindergarten.[2] As a consequence of these policies, individuals born within a day of one another enter school at different ages and have different levels of education throughout school enrollment. Assuming individuals born near in time are similar along non-education related dimensions, differences in education at motherhood for women born near the entry date are exogenous. The crux of our identification strategy is to compare fertility and infant health outcomes for mothers born just before and after the school entry date and to relate the magnitude of these differences to the schooling discontinuity.

Using large samples of birth records, we find:

1. School entry policies have large effects on schooling at motherhood: one-fourth of young Texas mothers born after the school entry date have a year less education than they otherwise would, had they been born before the entry date. For California, our estimate is one-seventh.

Furthermore, using this variation in education due to the school entry policies, we reach two key conclusions:

1. Education does not significantly impact fertility: women born just before and after the school entry date are equally likely to become mothers and give birth at similar ages.
2. Education has generally small, but possibly heterogeneous, effects on infant health: women born just before and after the entry date give birth to children of similar health, as proxied by birth weight and prematurity. There is some suggestive evidence of different effects of education on low birth weight by race and ethnicity.

Along the dimension of mate quality, we also find that women born just after the entry date have younger and less educated mates than women born just before. We hypothesize that much of this effect is due to the way in which school entry policies manipulate an individual's peer group. Girls who are born after the entry date will start school at older ages and hence will have young peers.

Implementing our identification strategy requires information on date of birth, which is unavailable in most public-use files. We use a recent administrative data set on all births in California and Texas with information on mother's date of birth and education, infant health, pregnancy behaviors

[2]For both California and Texas, school entry policies pertain to the typical age of kindergarten entry. However, kindergarten is not mandatory in either state. See `http://www.ecs.org./html/educationIssues/EarlyLearning/KDB_intro.asp`. Nevertheless, according to the 1980 Census, over 80 percent of females in California and Texas who are age eligible for kindergarten attend kindergarten.

(e.g., smoking and drinking), and paternal characteristics. These data allow us to focus contrasts narrowly around the school entry date, a challenge for earlier analyses in which either exact date of birth or large sample sizes were wanting (Angrist and Krueger 1991, Cascio and Lewis 2006).

A narrow focus on individuals born near the school entry date builds on the quarter of birth approach of Angrist and Krueger (1991). First, it sidesteps the criticisms of Bound, Jaeger and Baker (1995) regarding seasonality of birth (assuming seasonal patterns are continuous at the school entry date). Second, it leads to a precise estimate of the relationship between within-year birth timing and educational attainment, circumventing statistical problems associated with weak instruments (Staiger and Stock 1997).

The crucial assumption underlying this approach is that for dates near the school entry date, an individual's date of birth is random. This assumption is plausible a priori, since parents are unlikely to strategically plan the exact date of birth of their child. Moreover, this assumption is testable—women born just before and after school entry dates should be similar in terms of predetermined, observable characteristics. We find that they are.

Proper interpretation of our estimates requires consideration of several features specific to our approach. First, not all children will begin school in the year predicted by school entry policies. The parents of a child born before the school entry date may hold their child back by a year, and the parents of a child born after the school entry date may petition for their child to start school a year before typically allowed, or may start their child in private school. For neither type of child will schooling progression be affected by school entry policies. This suggests that our estimates may disproportionately reflect the experience of women from low socio-economic backgrounds, whose parents are somewhat more likely to comply with school entry policies (Elder and Lubotsky 2009).

Second, even if school entry policies affect a woman's schooling progression, they may not affect education at motherhood. School entry policies affect education at motherhood for two types of women: (i) those still enrolled in school, for whom the effect is primarily mechanical, and (ii) those who have already completed schooling, whose school-leaving decision was age-dependent (i.e., not just schooling-dependent). For example, a woman who drops out of school at the earliest age allowed under a typical compulsory schooling law will have fewer years of education if she starts school late (Angrist and Krueger 1992).[3] This suggests that our estimates may be most relevant for women at risk of dropping out of school. Such women are likely to give birth at earlier ages than women intent on attaining a specific level of schooling, such as a college degree. Empirically, we find that school entry policies exert the greatest impact on the education of women giving birth at young ages. Thus, we stratify most of our analysis by age, focusing on women age 23 or younger, for whom our first stage relationship is strongest.

Third, school entry policies potentially affect not just education at motherhood, but also age at motherhood. This would present an identification problem, since it would lead to two endogenous regressors, rather than just one. However, surprisingly, we document that school entry policies affect neither the probability of becoming a mother nor age at motherhood. This is substantively interesting, and also implies that our approach identifies an education effect unconfounded by selection into motherhood and unconfounded by age at motherhood.

Fourth, school entry policies represent a dual manipulation of schooling and age-for-grade. This dual manipulation feature of our research design is shared by nearly all schooling research designs, because education (as conventionally measured) takes time. Hence, conceptual manipulations of education entail either starting an individual in school earlier, or keeping an individual in school longer. As we discuss in Section 6, for fertility and infant health outcomes, other research designs for schooling answer different questions than our research design.

Fifth, education at motherhood may differ from completed education if women return to school after childbirth. This is important because a temporary reduction in schooling will not necessarily affect permanent household income, whereas a permanent reduction in schooling would be expected to, because of the labor and mating market returns to schooling. While temporary and permanent reductions in schooling may have different effects on income, both temporary and

[3] Age at school leaving laws are not the only plausible reason for dropout decisions to depend on age. Additional plausible mechanisms include a desire to begin working life, perhaps triggered by minimum work age policies (Lleras-Muney 2002), the availability of welfare, or contraceptive failure. Indeed, for some years, Texas' compulsory schooling law requires individuals to finish the grade they start when they become compulsory schooling age (Texas Education Code, Section 21.032, 1984, Section 25.085, 1995). In such a circumstance, compulsory school leaving laws do not lead to differences in education for those starting school at different times.

permanent reductions in schooling may affect learning and the ability to process information, the causal pathway emphasized by Grossman (1972), Glewwe (1999), and Lleras-Muney (2002). Auxiliary analysis of the National Longitudinal Survey of Youth suggests that older mothers are more likely to have completed their education, raising the possibility of heterogeneity by age in the effects of education on infant health. We examine this issue empirically, but find little evidence of age-based heterogeneity.

The remainder of the paper is organized as follows. In Section 2 we describe the mechanisms by which education could affect fertility and infant health and briefly summarize the existing literature on the topic. In Section 3, we discuss our identification strategy, as well as our approach to nonparametric estimation, model selection, and inference. After describing the data we use in Section 4, we present the results of our estimation in Section 5. Section 6 presents evidence on heterogeneous effects and discusses a variety of important interpretation issues. Section 7 concludes.

2 Conceptual Issues

2.1 Why Should Education Matter?

In broad terms, education may affect a woman's fertility and child-investment choices through either income or learning (Michael 1973). Education increases a woman's income stream through both the labor market and the mating market, the latter through assortative mating. In addition to the income channel, education may improve a woman's stock of knowledge regarding contraceptive technologies or healthy pregnancy behaviors, either because it augments her knowledge directly (i.e., educational curricula are important), or because it improves her ability to absorb and process information generally. We next describe each of these mechanisms in turn.

The income channel operates through the well-documented effect of education on labor earnings. The notion that an exogenous increase in a woman's income may lead to reduced fertility is present in the earliest treatments of the neoclassical model of fertility (Mincer 1963, Willis 1973). In these models, households do not value children *per se*, but what Willis terms "child services"—the product of the number of children and the average quality of those children. A key idea is that production of child services is time-intensive relative to other activities for the woman. As the value of a woman's time rises, she generally substitutes away from consumption that is highly time-intensive (Becker 1965) and hence desires fewer children. These predicted effects of education on fertility map naturally into predicted effects on child quality. Assuming child services are a normal good, falling fertility in response to rising income requires that child quality be an increasing function of income. Cross-price effects such as these were first emphasized by Becker and Lewis (1973) and Willis (1973).

Predictions based on the income channel are further sharpened by positive assortative mating, or the tendency for men and women of similar education to pair (Behrman and Rosenzweig 2002). Under this type of stratification, an exogenous increase in a woman's education leads to a mate of higher education, further increasing household permanent income through a multiplier effect.

In addition to the income channel, the literature has stressed the role of education in augmenting an individual's stock of health knowledge (Willis 1973). With respect to fertility, Rosenzweig and Schultz (1989) provide evidence that a woman's education explains ability to effectively use contraception. With respect to infant health, Thomas, Strauss and Henriques (1991) show that education predicts a woman's ability in regards to, or perhaps interest in, information acquisition and processing. One of the most frequently-cited examples of this mechanism is smoking (Currie and Moretti 2003). Through anti-smoking campaigns in schools or health class, children could learn about the dangers of smoking and be discouraged from adopting the habit. Glewwe (1999) argues that the most important mechanism for knowledge gain is not directly via curricula; rather the skills obtained in school facilitate the acquisition of health knowledge. Grossman (1972) formalizes these ideas by viewing education as a productivity shifter in the household production function for health.

Since education can affect infant health through several different channels and the intensity of these channels may not be the same for all levels of education nor for all subpopulations, the effect of education on infant health may differ across studies. For example, Currie and Moretti (2003) use college openings to study the effect of maternal education on infant health. The women whose schooling attainment at motherhood is affected by college openings are those women with a high level of education generally. As we show below, our study focuses on the causal role of education for women with a low level of education generally. Educational levels that appear to be affected in our study are in the range of eighth to twelfth grade, with a muted effect on the first two years of college. This subpopulation is of interest for several reasons. First, the observational infant health return to education is declining in the level of education. Second, the labor market return to education is declining in education (Card 1999). Third, young women at risk of dropping out of school are frequently the target of specific policies aimed at reducing fertility and improving infant health.

2.2 What Does the Effect of Education Represent?

The model of fertility and child investment outlined above suggests that infant health is a function of (i) maternal choice variables (e.g., smoking while pregnant) and (ii) maternal endowments (e.g., genetic makeup). A general health production function takes the form $Y = f(X, W)$, where Y is a measure of the health of a particular mother's newborn child, X is a vector of maternal choice variables, and W is a vector of maternal endowments. Elements of W are fixed from the mother's perspective. However, a mother's schooling could affect her health inputs, elements of X. Demand for health inputs may be expressed as a general function of resources, endowments, and the demand for schooling, $X = g(S, I, W)$, where S denotes schooling and I denotes resources. Resources are meant to be interpreted broadly as non-schooling factors that affect a mother's choice of health inputs (e.g., income). Combining, we have

$$Y = f(g(S, I, W), W) \tag{1}$$

This simple formulation suggests thinking of schooling as potentially affecting infant health through different mechanisms. First, additional schooling can be thought of as a productivity shifter (i.e., changing f). Second, schooling could impact a mother's health inputs such as mate selection, income, prenatal care, and smoking (i.e., changing X, or g). Analogous expressions may be developed relating female education to fertility decisions.

The first idea of schooling as a productivity shifter is the focus of Grossman's (1972) model of health capital. In particular, it is the effect of education on health inputs via health knowledge and the ability to process information. The second idea of schooling as altering health inputs may be termed an indirect effect of education.

In this paper, we are unable to distinguish between the direct/Grossman effect and the indirect effect. Nevertheless, this distinction is important. It highlights the potential for heterogeneous education effects, as there are several mechanisms by which education could potentially improve infant health.

3 Methodology

Following the literature, consider a partially linear approximation to equation (1),

$$Y_{ij} = \theta S_{ij} + \tau(W_{ij}) + \varepsilon_{ij} \tag{2}$$

where θ captures the effect of schooling on infant health holding ε_{ij} and W_{ij} fixed. Here, $\tau(\cdot)$ is a function, and the residual ε_{ij} is meant to capture unobserved factors potentially affecting infant health. The subscripts emphasize the grouping structure of our data, with many mothers (indexed by i) observed with the same birthday (indexed by j), even within single birth cohorts.

3.1 Identification

Identifying the effect of education on infant health requires solving two difficult problems. The first problem is the endogeneity of schooling. The second problem is sample selection. This second problem may arise if, for example, education affects a woman's decision to have children, leading to a selected sample of those observed giving birth. A regression discontinuity approach will, under continuity assumptions to be discussed, circumvent the endogeneity problem. However, except in unusual circumstances, it will not circumvent the sample selection problem.

Consider first the endogeneity problem, and suppose that mothers are a random sample of women. We free up this assumption when we discuss sample selection, below. Linearly project

S_{ij} and Y_{ij} in the conditional expectation sense:

$$Y_{ij} = m(R_j) + \alpha D_j + u_{ij} \quad (3)$$

$$S_{ij} = n(R_j) + \beta D_j + v_{ij} \quad (4)$$

where $D_j = \mathbf{1}(R_j > 0)$ indicates birth after the school entry date and R_j denotes an individual's day of birth relative to the school entry date for the state in which the individual begins school. For example, $R_j = 5$ for an individual born 5 days after the school entry date. The function $n(r)$ is defined to be continuous so that any discontinuity at $r = 0$ in the conditional expectation of S_{ij} is captured by the parameter β. It is straightforward to show that $\alpha = \theta\beta$, by linear projection. Assuming that $\beta \neq 0$, then, identifying θ requires simply identifying α. We refer to the continuity in r of the conditional distribution function of W_{ij} given $R_j = r$ as "smoothness". Under smoothness, $m(r) \equiv \theta n(r) + E[\tau(W_{ij})|R_j = r]$ is continuous at $r = 0$, and α captures any discontinuity at $r = 0$ in the conditional expectation of Y_{ij}. Hence θ is identified by the ratio of the discontinuity at $r = 0$ in Y_{ij} to the discontinuity at $r = 0$ in S_{ij}. Thus, under smoothness and assuming $\beta \neq 0$, the regression discontinuity approach circumvents the endogeneity problem. These basic points are formalized in Hahn, Todd and van der Klaauw (2001, Theorem 1).

Consider now the problem of sample selection. We only observe infant health for the subset of women who decide to become mothers. Nonetheless, under a standard one-sided selection model, we can consistently estimate population conditional expectations with the inclusion of an additively separable control function (Gronau 1974, Heckman 1976, 1979). Consistent with this literature, consider next an estimation equation analogous to the outcome equation (3) but based only on the observed data, with P_j the conditional probability of giving birth given R_j:

$$Y_{ij} = m(R_j) + \alpha D_j + \lambda(P_j) + \nu_{ij} \quad (5)$$

where the control function $\lambda(P_j)$ corrects for sample selection. The specific functional form of $\lambda(\cdot)$ depends on distributional assumptions. For example, under bivariate normality of ν_{ij} and the unobserved component of the decision to become a mother, $\lambda(p) \propto \phi(\Phi^{-1}(p))/p$ or the inverse Mills ratio (Heckman 1979, Ahn and Powell 1993, Das, Newey and Vella 2003).

Under general conditions, $\lambda(\cdot)$ is continuous. Continuity of $\lambda(\cdot)$ and $m(\cdot)$ imply that if the probability of motherhood is smooth in the mother's day of birth, then $\widetilde{m}(\cdot)$ is continuous, where $\widetilde{m}(R_j) \equiv m(R_j) + \lambda(P_j)$. We may thus rewrite equation (5) as

$$Y_{ij} = \widetilde{m}(R_j) + \alpha D_j + \nu_{ij} \tag{6}$$

This clarifies that if the probability of motherhood is unaffected by school entry policies, the observed discontinuity in infant health identifies α. However, if the probability of motherhood were affected by school entry policies then $\widetilde{m}(\cdot)$ would be discontinuous and point identification of α would not be possible without further modeling.[4] An analogous argument shows that if the probability of motherhood is unaffected by school entry policies, the observed discontinuity in maternal schooling identifies β. Hence, if the probability of motherhood is unaffected by school entry policies, there is equivalent sample selection from the left and from the right, and the regression discontinuity approach circumvents both the endogeneity problem and the sample selection problem.

We document that the probability of motherhood is a smooth function of day of birth (see Section 5, below). This is surprising in light of the negative association between education and fertility documented in other work (e.g., Hotz, Klerman and Willis 1997). Nonetheless, the substantive implication of these results is supported by our analysis of age at first birth, which shows that age at motherhood is similarly a smooth function of day of birth.

To the best of our knowledge, school entry policies are the only educational intervention studied in the literature that do not affect fertility. This simplifies interpretation of our infant health results for two reasons. First, an effect on the probability of giving birth would create sample selection problems, as discussed. Second, an effect on age at birth would lead to ambiguities of interpretation. For example, an educational intervention inducing women to attend college would delay

[4]If there were a discontinuity in the probability of motherhood in day of birth and no instrument for observation were available, the approach of Lee (2005) could be used to bound the treatment effect.

fertility mechanically. Since a woman delaying fertility from 18 to 22 on average improves her baby's health at birth (Royer 2004), this would again lead to more endogenous regressors than instruments. School entry policies are thus a unique setting in which it is possible to isolate the effect of education on infant health, holding constant fertility choices. However, as we discuss in detail in Section 6, school entry policies are a simultaneous manipulation of schooling and age relative to one's peer group during schooling, and this has implications for the appropriate interpretation of our estimates.

Much of the recent program evaluation literature adopts a perspective which views α, β, and θ as random variables rather than as constants in the population (e.g., Card 1999, Appendix A.2). This leads to additional identification difficulties. However, it is still possible to characterize what is estimable. As emphasized by Hahn et al. (2001), there is a direct analogy between the probability limit of a regression discontinuity estimator and the local average treatment effect interpretation of the instrumental variables estimator (Imbens and Angrist 1994). In particular, under a monotonic effect of school entry policies on schooling, a regression discontinuity estimator will identify the effect of schooling on fertility and infant health for those persons whose educational attainment is causally affected by school entry policies (cf., Angrist and Imbens 1995). This subpopulation is not necessarily representative of the overall population of interest.

Monotonicity is not guaranteed. The effect of school entry policies on schooling would not be monotonic if, for example, a woman's parents would choose to delay her entrance into school if she were born before the school entry date, but would choose to petition the school district to allow her to begin school early if she were born after the school entry date. To take another example, monotonicity would be violated if a woman would eventually complete more schooling if she were born after the school entry date than she would if she were born before the school entry date. This could occur if, for example, being older throughout school progression made it easier to complete more schooling.

On the other hand, as emphasized by Angrist and Imbens (1995), monotonicity is partially testable, because it implies that at each point of the education distribution, the probability of attaining at least that level of education for individuals born before the cutoff date must exceed the probability for those born after the date.[5] In Section 5, below, we present results from a regression discontinuity analogue to the estimator given in Angrist and Imbens (1995) for the average causal response weights. These results corroborate the monotonicity assumption.

3.2 Estimation

Estimation of equation (6) may be accomplished in a variety of ways. The recent empirical regression discontinuity literature has focused on global polynomial estimators (see, for example, the references given in Lee and Card 2006). However, Hahn et al. (2001) and Imbens and Lemieux (2008) advocate an adaptation of local linear regression (cf., Fan and Gijbels 1996). These two estimation approaches are generally competitive, with differing strengths and weaknesses. As a practical matter, we have estimated all of our models using both approaches and obtained nearly identical results. We follow the recommendations of the theoretical literature and present estimates based on local linear methods. For both reduced-form and instrumental variables estimates, these local linear methods can be understood as method of moments estimators. Throughout this subsection, we use the method of moments framework to describe our exact estimation strategy to avoid confusion over details of our implementation.

Our analysis consists of two parts. First, we estimate the effect of school entry policies on fertility behaviors including the probability of motherhood and the timing of motherhood. Second, after establishing that school entry policies do not affect fertility, we turn to estimation of the effect of education on infant health.

Throughout the empirical analysis, our estimated reduced-form school entry effects are based on cohort-specific estimates, where each cohort is defined symmetrically about the school entry cutoff date. For example, the 1975 birth cohort for California is the set of baby girls born in California 182 days before and after December 1, 1975. These cohort-specific estimates are not as precise as those that pool the information across cohorts. To improve precision and to economize the pre-

[5]That is, the distribution function of schooling for those born after the school entry date must lie entirely to the left (or right) of the distribution function of schooling for those born before the school entry date. The key condition is that the distribution functions cannot cross.

sentation, we also present pooled estimates overall, along with a test of the implied cross-cohort restrictions, using minimum chi-square techniques (Malinvaud 1970, Ruud 2000). We generally fail to reject the restrictions, supporting the choice of pooling across cohorts. Informed by our conclusion that the reduced-form analysis supports pooling across cohorts, we base our instrumental variables estimates on the entire main estimation sample. This allows us to avoid estimating cohort-specific first stage regression models, which is known to lead to statistical problems with weak instruments (Bound et al. 1995).

A minor complication regarding estimating the effect of school entry policies on the probability of motherhood arises, because we do not know whether a particular female born on a specific date later gives birth. We do know, however, the overall fraction of women born on a given day in California (Texas) observed giving birth in our administrative data for California (Texas), which proxies for the probability of motherhood and is sufficient for estimation at the group-data level. The construction of this proxy is described in greater detail in Section 4, below. The estimated effect of school entry policies on the probability of motherhood corresponds to $\hat{\alpha}$ in the method of moments problem

$$0 = \sum_{j=1}^{J} \{P_j - \hat{\alpha} D_j - \hat{\pi}_0 - \hat{\pi}_1 R_j - \hat{\pi}_2 D_j R_j\} K_h(R_j)(1, D_j, R_j, D_j R_j)' \tag{7}$$

where P_j denotes the fraction of women born on day j who we observe and $(1, D_j, R_j, D_j R_j)'$ is a (column) vector of covariates including a constant, an indicator for being born after the school entry date, day of birth relative to the school entry date, and the interaction of the indicator with relative day of birth. The weighting function $K_h(r) = h^{-1}K(r/h)$ is based on the triangle kernel $K(t) = \max\{0, 1 - |t|\}$, which is known to be boundary optimal (Cheng, Fan and Marron 1997).

For outcomes where we possess individual-level control variables (e.g., schooling and low birth weight), we estimate our models at the micro-data level for additional precision. For these outcomes, denoted Y_{ij}, the method of moments problem is

$$0 = \sum_{j=1}^{J} \sum_{i=1}^{n_j} \left\{ Y_{ij} - \hat{\alpha} D_j - \hat{\pi}_0 - \hat{\pi}_1 R_j - \hat{\pi}_2 D_j R_j - \mathbf{X}_{ij}' \hat{\pi}_3 \right\} K_h(R_j) \left(1, D_j, R_j, D_j R_j, \mathbf{X}_{ij}' \right)' \quad (8)$$

where $\mathbf{X}_{ij}$ is a (column) vector of background characteristics which are smooth functions of R_j: the mother's (i) race/ethnicity and (ii) age (for outcomes other than age). The inclusion of these controls has no substantive effect on our discontinuity estimates. Web Appendix Table 3 shows that mother's race/ethnicity and other background characteristics are related smoothly to day of birth.

Finally, to compare the magnitude of our estimated effects, and to see what kind of effect sizes our data provide evidence against, we report instrumental variables estimates of the effect of schooling on infant health outcomes using the discontinuity as the excluded instrument. These estimates correspond to $\hat{\theta}$ in the method of moments problem

$$0 = \sum_{j=1}^{J} \sum_{i=1}^{n_j} \left\{ Y_{ij} - \hat{\theta} S_{ij} - \hat{\pi}_0 - \hat{\pi}_1 R_j - \hat{\pi}_2 D_j R_j - \mathbf{X}_{ij}' \hat{\pi}_3 \right\} K_h(R_j) \left(1, D_j, R_j, D_j R_j, \mathbf{X}_{ij}' \right)' \quad (9)$$

As noted above, we do not estimate our instrumental variables models separately by cohort. We instead use the entire main estimation sample and redefine $\mathbf{X}_{ij}$ to include indicators for the mother's (i) race and ethnicity, (ii) age, and (iii) birth cohort.

3.3 Bandwidth Selection

Implementing local linear regression requires choosing a bandwidth, h. There are many automatic bandwidth selectors for nonparametric regression. Fan and Gijbels (1996, Section 4.2) provide a simple automatic procedure which we adapt to the regression discontinuity context.[6] We also implement the Imbens and Lemieux (2008) procedure. This procedure generally corroborates the Fan and Gijbels procedure, but occasionally chooses the largest considered bandwidth ($h = 180$).

[6] This procedure fits a fourth-order global polynomial separately on the left and the right of the point of discontinuity. For either side, the rule-of-thumb bandwidth is $c \left[\check{\sigma}^2 (b-a) \big/ \sum \check{m}''(R_j)^2 \right]^{1/5}$ where $\check{\sigma}^2$ is the mean squared error for the regression, $b - a$ is the range of R_j, $\check{m}''(R_j)$ is the estimated second derivative of the global polynomial evaluated at R_j, the summation is over the data, and $c \doteq 3.438$ is a kernel-dependent constant (see equations (4.3), (3.20), and (3.22) of Fan and Gijbels (1996)).

The results of both procedures are presented in Web Appendix Table 2. Each of these automatic bandwidth selectors chooses a bandwidth that is overly wide for the purposes of hypothesis testing (Pagan and Ullah 1999, Horowitz 2001). We thus opt for a more conservative, under-smoothed bandwidth of 50 days throughout. Web Appendix Figure 3 presents a profile of discontinuity estimates in bandwidths, for our key outcomes of low birth weight, prematurity, and schooling.

3.4 Inference

The local linear regressions described in equations (7) through (9) are weighted least squares and weighted instrumental variables procedures and hence are amenable to standard regression inference procedures (cf., Imbens and Lemieux 2008). However, our data have a grouping structure, with many observations having the same value of the running variable R_j. In such a context, Lee and Card (2008) suggest the use of clustering on the running variable.[7] Following their suggestion, we cluster our standard errors at the level of the running variable and further employ the finite sample ("HC3") adjustment suggested by MacKinnon and White (1985).[8] We have assessed the accuracy of this inference approach using simulation, focusing on local linear regression with $h = 50$ applied to data generating processes that mimic our own data. The simulation evidence suggests that the tests presented in this paper (of 5 percent nominal size) enjoy size of 5-6 percent.

4 Data and Sample

We use confidential 1989-2001 Texas and 1989-2002 California natality data, acquired from each state's Department of Health. We focus on recent natality data since the standard birth certificate started collecting the mother's exact date of birth beginning in 1989. Information on the mother's exact date of birth is suppressed on the public-use national Natality Detail Files compiled by the National Center for Health Statistics. By special permission we obtained access to a version of the California and Texas data files with this information.

[7] For our data, the Lee-Card correction factor, adapted to our local linear context, is nearly always zero and is always small.

[8] As a practical matter, it is not always easy to obtain correct HC3 standard errors in software, particularly in the weighted regression case. In such cases we instead use the jackknife at the level of the birthday. MacKinnon and White (1985) note that HC3 is an approximation to the jackknife.

These natality files cover the universe of all births occurring in these states, approximately 800,000 births per year. At birth, each mother along with her health care provider completes an extensive survey, which inquires about maternal and paternal demographic characteristics, maternal behaviors during pregnancy (e.g., prenatal care), and the health of the infant at birth. For Texas, but not for California, our natality data are merged with infant mortality information from death certificates for those infants who died within the first year.

We impose four main sample restrictions. First, our sample consists exclusively of mothers born in the state in which they gave birth.[9] Third, for our infant health analysis, we limit our sample to mothers who are 23 years old or younger.[10] When analyzing the probability of motherhood or age at birth, we make no age restriction, as we first need to verify that there is no effect on either before conditioning on age. Third, we focus on first-time mothers. As emphasized by Wolpin (1997), poor infant health at first birth may causally affect a woman's decision regarding subsequent fertility and child investment choices. In the absence of additional modeling, it will not be possible to separate the effect of education from the effect of the observed health of the first child. Analyzing first births also strengthens the plausibility of independence assumptions and leads to a more homogeneous sample that is more comparable to those used in the literature. Fourth, for California (Texas) we focus exclusively on potential mothers born between 1969 and 1987 (1986).

Our other sample restrictions affect the estimation sample only slightly. We exclude non-singleton births, as the meaning or significance of infant health measures such as low birth weight may vary by plurality (2 percent of the total). Finally, we purge those records missing information on education and their own day of birth (also 2 percent of the total).

Table 1 provides descriptive statistics for our study states. Throughout our analysis, we examine Texas and California separately. To get a sense of how selective is our main estimation sample, we present summary statistics for the overall sample of mothers with singleton births (first column for each state), the sample of first-time mothers (second column), and the young native mothers

[9] An ideal analysis would use information on the state in which a mother began her education. We view state of birth as a reasonable proxy for the state where education begins. According to the 2000 Census, 89.5 percent of 5 year olds born in California still lived in California, and 89.8 percent of 5 year olds born in Texas still lived in Texas.

[10] The education discontinuity induced by school entry policies is smaller for older women, as noted above.

sample (i.e., those born in the state in which they birth and who are 24 years old or younger) the sample used in our main analysis (last column).

Relative to the other sample of mothers, our estimation sample is somewhat negatively selected. The first-time young native mothers are considerably younger and have worse birth outcomes. Comparing California and Texas, the years of schooling, age at motherhood, and rates of prematurity are similar, but the rate of low birth weight is roughly 1 percentage point higher in Texas than in California. In terms of race and ethnicity, African American mothers comprise 14 (19) percent of our main estimation sample for California (Texas), and for both states over 40 percent of the mothers are Hispanic.

For our analysis of the probability of motherhood, we merge the number of first-time mothers in our administrative data born in California (Texas) between January 1, 1969, and December 31, 1988, with the number of women born in California (Texas) on those same dates, calculated from the public-use Natality Detail Files, 1969-1988, the only years for which daily birth counts by state are available. The number of women in our administrative data relative to those at risk for being observed proxies for the probability of motherhood. This measure is more accurate for older cohorts, because women in more recent cohorts are not observed in our administrative data unless they give birth at a young age.

5 Results

We present our results in six subsections. First, we consider the impact of school entry policies on fertility. We find no difference in fertility behaviors for those born just before and after the cutoff dates. Second, as we observe no differences in fertility behaviors related to school entry policies, we examine the impact of school entry policies on education at motherhood. These effects are visually apparent, economically important, and precisely estimated. Third, we examine the impact of school entry policies on infant health, as proxied by birth weight, gestational length, and infant mortality. We find little evidence of differences in these outcomes for those born just before and after the cutoff dates. Fourth, we present instrumental variables estimates of the effect of female education on infant health. Fifth, we examine the impact of school entry policies on several risk factors for poor infant health. Sixth, we discuss robustness.

5.1 School Entry Policies and Fertility

The effect of education on fertility could manifest itself in terms of the probability of ever becoming a mother, the number of children, and the timing of childbearing. As discussed below, for several cohorts of women we observe a direct estimate of the probability of becoming a mother. We do not observe completed fertility, as our observation window is too short. However, we observe age at first birth, a fertility timing measure.

To examine the effect of school entry policies on fertility, we begin with a graphical presentation of the relationship between a female's day of birth and the probability of motherhood separately for California and Texas in Figure 1. Vertical bars are placed at the school entry cutoff date for each cohort. If school entry policies affect fertility in a consistent way, we should expect to see a discontinuity in the probability of giving birth at most vertical bars. For California, there is no consistent pattern. For Texas, there are some suggestive jumps in the figure, but these may not be significantly different from zero.

To assess whether the jumps in Figure 1 are consistent with sampling variability, we have estimated the jump at the school entry date for each cohort for each state, and we have further disaggregated these effects into discontinuities in the probability of giving birth at any specific age. This approach is flexible, but produces a great many estimates (162 for California and 143 for Texas). We provide a complete presentation of all 305 discontinuity estimates in Web Appendix Tables 1A through 1D. Because the 305 estimates are typically small and statistically insignificant, we focus on summary measures here to economize on space.

Our summary measures are of two forms. The first is a state-specific Wald test for the null hypothesis that the discontinuity estimates across all cohorts and all available ages are jointly zero. These test statistics, which are distributed chi-square asymptotically, are 122.57 for California (162 d.o.f., p-value = 0.99) and 153.29 for Texas (143 d.o.f., p-value = 0.26).

The second summary measure from this analysis is a series of age-specific discontinuity estimates, pooled across cohorts. These estimates are presented in Table 2. The pooled estimates are weighted averages of the cohort specific discontinuities presented in Appendix Tables 1A-1D, and can be understood as minimum chi-square estimates of the assumed common effect across cohorts. These pooled estimates are generally small in magnitude, statistically insignificant, and of varying signs. The data contain little evidence against the pooling restrictions. The p-value for the test of the cross-cohort restrictions is given below each estimate in brackets; these are above 5 percent for all but one of the 37 pooled estimates.

Finally, the lower right-hand corner of Table 2 presents summary measures for the probability of motherhood at age 23 or younger, and for the probability of motherhood at any age. These overall estimates are estimated with a great deal of precision and give no indication that a woman's fertility choices are affected by the timing of her birth relative to the school entry date in her state.[11]

In summary, we find little evidence that school entry policies affect either the probability of motherhood or age at first birth. This conclusion has both substantive and statistical implications. Substantively, the lack of impact of school entry policies on these fertility outcomes indicates a limited causal role for education in a woman's fertility planning among women desiring to have a family young enough that schooling is potentially a binding constraint on age at first birth. For example, these results are consistent with a biological model in which age of menarche, not educational attainment, determines sexual activity and in which use of contraception is unrelated to the amount of schooling completed to date.[12] Statistically, the lack of an impact of school entry policies on fertility means that women born just before and after the school entry date form an equivalently-selected sample and hence can be used to study the effect of education on infant health without sample selection corrections.

[11]Further analysis of the effect of school entry policies on fertility is presented in Web Appendix Figures 1 and 2. Web Appendix Figure 1 presents an estimate of the conditional expectation of age at first birth (among those observed) given birthday. Web Appendix Figure 2 presents the distribution of age effects, and is precisely analogous to Figure 4, discussed below.

[12]It is possible that women born after the cutoff date are more likely to become pregnant but also more likely to obtain an abortion than women born before the date. While we cannot directly test this hypothesis as we have no direct data on abortions, women in our sample born just before and after the cutoff date report similar numbers of prior pregnancies (results available from authors).

5.2 School Entry Policies and Education

Having determined that school entry policies do not appear to alter the probability of motherhood in our observation window, we can proceed to examine the impact of these policies on educational attainment. We begin with a graphical presentation of the relationship between schooling and day of birth separately for California and Texas in Figure 2.[13]

We highlight two aspects of the estimates in Figure 2. First, for young mothers in both California and Texas, there is a marked discontinuity in education at motherhood exactly at the school entry date, as expected. Second, there is no evidence of a discontinuous relationship at any other day of birth. The juxtaposition of the smoothness of the conditional expectation away from the school entry date and the sharpness of the discontinuity at the entry date supports the interpretation of the education discontinuity as directly attributable to school entry policies.

Discontinuity point estimates are given in Table 3. The estimate for California is -0.14, while that for Texas is -0.24. Note for Texas, we reject the assumption of homogenous effects across cohorts. This is because, as we discuss later, the first-stage estimates vary with age and age at observation differs systematically by cohort. The magnitudes of the effect of school entry policies on education are large relative to other benchmark differences in education. For example, according to the 2000 Census, the national black-white education gap for women is -0.88. To interpret the magnitude of the education discontinuities, suppose that school entry policies affect schooling by one year or not at all (i.e., being born after the school entry date reduces schooling by at most one year). Under this assumption, the education discontinuity estimates the fraction of young women whose education at motherhood is affected by school entry policies (cf., Angrist and Krueger 1992). Thus, school entry policies affect education at motherhood for a large 14 (24) percent of young first-time native mothers in California (Texas).[14] Estimates of the impact of school entry policies for young women are precise, with t-ratios ranging from 9 to 16.

As noted, an interesting pattern in the data is that the education discontinuity is strongest for the

[13]For these cohorts, the school entry date was fixed at December 1 (California) and September 1 (Texas).

[14]Unreported results for first-time mothers of all ages are about 30 to 40 percent smaller than that for the main estimation sample of mothers 23 or younger.

youngest mothers and weakest for the oldest mothers. Figure 3 provides separate education discontinuity estimates for different ages.[15] We supplement these disaggregated discontinuity estimates with female school enrollment rates for our two study states, calculated from the 2000 Census. The figure shows discontinuity estimates that decline in magnitude with age as enrollment rates fall.

The age gradient in the education discontinuities is consistent with two stories. One story is that, for women in the cohorts we study, school entry policies have no impact on completed education, but do manipulate education at motherhood for those whose pregnancy interrupts their schooling. A second story is that, for women in these cohorts, young mothers are those who drop out of school as soon as possible, and that older mothers are those whose educational attainment would not be affected by when they started their schooling because they stop schooling based on completed schooling rather than age.

Under the first story, our fertility and infant health estimates are due to the direct/Grossman effect of education. Indirect effects of education through income will not be as relevant because a woman who anticipates returning to and finishing school will have approximately equal permanent income as a woman who completes that same level of schooling prior to beginning her family. The direct/Grossman effect is operative, however, because one cannot know what one has not yet learned. Under the second story, our fertility and infant health estimates are due to both a direct/Grossman effect and an indirect effect.[16] The second story thus implies a stronger effect of education on fertility and infant health than the first.

An important issue for further interpretation of our estimates is the range of education levels manipulated by school entry policies. This issue is addressed by the curve presented in Figure 4 (Angrist and Imbens 1995). Each open circle at schooling level s represents the estimated percent of women age 23 or younger who would complete fewer than s years of schooling if born after the

[15] For this figure only, we use information from pre-1969 cohorts. The inclusion of these additional cohorts greatly improves the precision of the estimates for older mothers.

[16] It is possible that the women whose educational attainment at motherhood is affected by school entry policies have little foresight regarding their permanent incomes. For example, some of these women may be too young to have ever received any earnings. If such women do not have sufficient foresight, then the effect of education would operate primarily through a learning channel.

school entry date but would complete s or more years of schooling if born before.[17]

If school entry policies manipulate schooling by at most one year, each open circle represents the estimated percent of young women who would complete $s-1$ years of schooling if born after the school entry date but would complete s years of schooling if born before. Under this latter, sharper interpretation, 4 percent of California young women and 6 percent of Texas young women would complete high school if born before the school entry date, but would fail to do so if born after. In both states, being born before the school entry date induces 2 percent young women to complete a year of college, when otherwise they would have only completed high school. Figure 4 thus indicates that school entry policies affect not just the number of years of high school a woman has completed by the time of her first births, but also the number of years of college.

As noted above, Figure 4 is also important because it provides a test of the monotonicity assumption. Under monotonicity, the distribution functions of schooling for those born just before and after the school entry date should not cross. This pattern is corroborated by Figure 4, because the curves are positive throughout the support of education.

5.3 School Entry Policies and Infant Health

Proceeding next to our analysis of the policies on health outcomes, we examine the reduced-form effects of the policies on the incidence of low birth weight, a widely-cited risk factor for poor infant health (Figure 5).[18] As with our analysis of education, we report results for mothers 23 years old or younger.

Because schooling declines at the school entry date, we expect to see an increased likelihood of low birth weight at the school entry date. However, the data indicate no obvious break in behavior. This visual impression is confirmed by point estimates which are generally small and statistically insignificant (Table 3). The effect for California (Texas) is -0.0006 (-0.0051), which is small rela-

[17] Practically, these estimates are based on the difference in the empirical cumulative distribution function of schooling for those born before and after the school entry date, where the empirical cumulative distribution function is defined using a sharp, rather than the more traditional weak, inequality. We construct these by using a series of linear probability models with dependent variables $\mathbf{1}(S_{ij} < s)$, each estimated according to equation (8), with s ranging over the support of schooling.

[18] See Almond, Chay and Lee (2005) for references.

tive to the overall incidence of low birth weight of 6 (8) percent.

We next consider the impact of school entry policies on the incidence of premature birth, defined as gestational length of less than 37 weeks. Figure 6 gives an estimate of the conditional expectation of prematurity in mother's day of birth. Because prematurity is a negative health outcome, we expect to see a rise in prematurity at the school entry date. However, the data indicate no break in behavior. The estimated discontinuities are again small and statistically insignificant (Table 3). The estimate for California (Texas) is -0.0012 (-0.0029), which is small relative to the overall incidence of prematurity of 10 (11) percent.[19]

Our results for low birth weight and prematurity are somewhat surprising in light of the existing literature. For comparison, a conventional estimate of the effect of education on low birth weight and prematurity is -0.01 (Currie and Moretti 2003). Given our first stage estimates, we would expect reduced form impacts of school entry policies on low birth weight and prematurity of 0.0014 (0.0024) for California (Texas). We return to this issue below.

For Texas, information on infant mortality is available. The plot of infant mortality against mother's day of birth (available upon request) provides no obvious visual evidence of discontinuity at the school entry date. However, this may be due to low statistical power—infant mortality is only one-tenth as likely as low birth weight or premature. Consistent with this, the estimated discontinuity is large in economic terms (0.0013 compared with an overall incidence of 0.0067 (i.e., an infant mortality rate of 6.7 infant deaths per 1000 births)), but statistically indistinct from zero.

Compared to the prior literature (e.g., Chou et al. 2007, Currie and Moretti 2003), our estimates are surprisingly consistent with a null hypothesis of no effect of education on infant health, as noted. One possible explanation for this pattern is lack of statistical power. Without a large number of observations local to the cutoff, in our case the school entry date, the regression discontinuity design may not have sufficient power to rule out economically interesting hypotheses.

In the Web Appendix, we present a detailed discussion of the sample sizes required to rule out different null hypotheses, focusing on outcomes studied in the recent literature. Here we mention

[19] As with the effects for low birth weight, we have examined the effects for a variety of cutoffs (20 weeks, 25 weeks, etc.) and found no effects for these other cutoffs.

the setup and conclusions of these power calculations briefly. For a fixed point estimate and null hypothesis, we compute the minimal percent increase in sample size (relative to our original sample) required to reject the null hypothesis. Web Appendix Table 4 presents the calculations. Using our own point estimates as a guide and the Currie and Moretti (2003) estimates as our null hypothesis, our sample is sufficiently large to reject such null hypotheses for maternal smoking, low birth weight, and prematurity. For prenatal care, our positive estimates actually exceed the size of the Currie and Moretti (2003) estimate, but to distinguish our estimate from theirs, we need roughly a 30 percent increase in sample size. For infant death, we would require 3 to 4 times as large a sample to detect an effect given a reasonable null hypothesis (e.g., a null hypothesis of zero). The estimates of Currie and Moretti (2003) are quite sizable and their use as a relevant null hypotheses may overstate our ability to detect meaningful economic effects. As such, we consider other null hypotheses. In many cases (e.g., smoking and low birth weight), we have enough power to detect effects half of the size of Currie and Moretti (2003).[20] See the Web Appendix for further discussion of power.

5.4 The Effect of Education on Infant Health

To understand the magnitude of the reduced-form effects, we now turn to instrumental variables estimates. These estimates are reported in Table 4 for the infant health outcomes of low birth weight, prematurity, and infant death (Texas only).

The low birth weight estimate for California (Texas) is 0.0036 (0.0199), with a standard error of 0.0161 (0.0118). As noted above, these estimates are somewhat surprising in light of the findings in the literature. Pooling our low birth weight estimates for California and Texas provides an overall estimate for the two states with greater precision. The pooled estimate is 0.0142 (standard error of 0.0095), and there is little evidence against the pooling restriction. At the 5 percent level, we are able to reject all null hypotheses involving an effect size more negative than -0.0014. This is based on a one-sided test with an alternative hypothesis that the effect is larger than the null. Thus, for low birth weight, our data and research design provide evidence against the conventional point

[20] In unreported results, similarly small and insignificant effects are estimated for the incidence of very low birth weight (birth weight less than 1500 grams), very very low birth weight (birth weight less than 1000 grams), and high birth weight (more than 4000 grams).

hypothesis of -0.01.

For prematurity, estimates for both states are smaller in magnitude and estimated with somewhat less precision. The estimate for California (Texas) is 0.0076 (0.0100) with a standard error of 0.0241 (0.0141). The pooled estimate of the effect of female education on prematurity is 0.0094 (standard error of 0.0122), and there is little evidence against the pooling restriction. At the 5 percent level using one-sided tests, we rule out point hypotheses more negative than -0.011. Thus, for prematurity, our data and research design are somewhat consistent with the conventional point hypothesis of -0.01.

For infant death, we only have information for Texas. As noted above, this estimate is of the expected sign and is large in economic magnitude, but is also estimated with very little precision. While the imprecision in our estimate cautions against strong interpretation, it is interesting to note that our point estimate is of the same sign and magnitude as that of Chou et al. (2007). These authors study the effects of junior high school expansion in Taiwan on schooling and infant health.

5.5 School Entry Policies and Risk Factors

Female schooling affects infant health to the extent that schooling affects a mother's behavior and that behavior affects the health of her child. To better understand these mechanisms in the context of our study, we turn now to reduced-form estimates of the impact of school entry policies on risk factors for poor infant health (Table 3). These may be particularly important in comparing our results to those in the literature, as "the effect of education" may mean different things in different studies.

The risk factors we consider in Table 3 may be thought of as falling into three key categories. The first category, which we term "risky maternal behaviors", encompasses maternal smoking, drinking, and sexually transmitted diseases. The second category is comprised of several prenatal care measures: care during pregnancy, care during the first trimester, and number of visits. The third category of risk factors pertain to the quality of the infant's father, as proxied by presence of father's information on the birth certificate, his age, and his education.

Estimated impacts of school entry policies on maternal behavior are generally small, of mixed sign, and often statistically insignificant. The estimated impacts on maternal smoking in California suggests that women born after the school entry date are statistically significantly more likely to smoke than women born before (t-ratio=2.4). While this finding is consistent with conventional estimates from the literature (e.g., de Walque 2004), the effects for Texas are of the opposite sign.[21] For other risk factors, women born before and after the school entry date have similar rates of sexually transmitted diseases in both California and Texas. For Texas, where we observe a measure of maternal drinking, the effect is the opposite of the expected sign and insignificant.

Turning to the estimates for prenatal care, we see that four of the six estimates are of the expected sign. Mothers with less education are somewhat less likely to receive prenatal care in the first trimester, and receive somewhat less of it. However, the estimates are modest in magnitude when compared to the sample mean. The estimates for Texas are on the cusp of significance, but those for California are consistent with sampling variability.

In contrast, paternal quality effects are sizable. These estimates show that women born just subsequent to the school entry date have mates who are younger and less educated, on average, than the mates of women born just before the entry date. These point estimates for both California and Texas are large and statistically distinct from zero.

These effects are not surprising given the nature of this educational intervention. School entry policies impact one's peer group. On average, individuals born immediately before the school entry date will have older peers in their grade, whereas individuals born after the school entry date will have younger peers. Our findings are consistent with the notion that mate selection is primarily grade-based.

5.6 Robustness

Our identification of the effects of female education hinges on the assumption that women born before and after the cutoff dates have similar pre-determined characteristics. We can test this assumption by testing the continuity of pre-determined characteristics in day of birth for potential

[21] The measurement of maternal smoking on the California birth certificate is less direct than that on the Texas birth certificate. As such, the California measure of smoking may be less reliable.

mothers. Aside from race and ethnicity, most of the characteristics we observe in our administrative data could be viewed as a response to assignment to starting grade and therefore are not useful for testing the research design. However, we may test for continuity of a variety of pre-determined characteristics using the public-use Natality Detail Files, which record information on infants and their parents as of birth. We can thus verify the smoothness of a variety of maternal and grandparental characteristics for women in the risk set for becoming mothers in our sample.

Web Appendix Table 3 gives estimated discontinuities for selected pre-determined characteristics of mothers. Each entry is a discontinuity estimate for a different pre-determined characteristic, calculated in the same manner as for those in Table 3, but using no auxiliary controls. We find little evidence of any discontinuity in the maternal characteristics we measure: fraction Hispanic, fraction black, low birth weight and first month of prenatal care. Similarly, we find little evidence of discontinuity in the grandparental characteristics we measure: native, parity, child mortality, and age.[22]

Finally, we show the profile of our reduced-form discontinuity estimates in the bandwidth chosen, for selected outcomes (Web Appendix Figure 3). As discussed, the data suggest that the appropriate bandwidth for these data is likely in the range 50-100 for most outcomes (Web Appendix Table 2). Over this range, our estimates are quite stable.

6 Discussion

In this section, we interpret our findings in light of the existing literature. Our comments fall into two broad categories: sources of potential heterogeneity in the effect of female education on fertility and infant health, and the potential role of age-for-grade effects in our estimates.

6.1 Heterogeneity

The effect of female education on fertility and infant health is plausibly heterogeneous for several reasons: (i) background characteristics, such as race; (ii) the level of schooling manipulated, such as high school versus college; (iii) the mechanisms by which schooling affects infant health, such

[22]We proxy child mortality by the fraction of the grandmother's live-born children who were still living at the time of the mother's birth.

as via a direct/Grossman effect or an indirect effect; (iv) the persistence of the schooling differences induced, since the behavior of forward-looking individuals may depend on both current and future human capital; and (v) the type of policy manipulation, such as school entry policies which manipulate when a child begins school, versus school exit policies which manipulate when a child ends school. We next elaborate on these points.

First, schooling interventions may not impact all subpopulations equally. For instance, Currie and Moretti (2003) document effects of college openings on white women's schooling, but note that there is little to no effect on black women's schooling.[23] In contrast, school entry interventions seem to have more homogenous effects on schooling. Table 5 presents estimates of the effect of school entry policies on schooling, low birth weight, and prematurity, separately by race/ethnicity.[24] The table indicates generally statistically similar effects on all three outcomes in both states, but there are some interesting differences. For example, for both states, the effect on education is somewhat smaller for black women than it is for the other two groups. On the other hand, these differences are consistent with sampling variability. Effects on prematurity are of similar magnitude for different racial/ethnic backgrounds for both states. Effects on low birth weight likewise are consistent with homogeneity for Texas. The strongest evidence of race/ethnicity differences in the effects of school entry policies is for low birth weight in California. For black women, the effect is consistent with education improving well-being and is statistically significant, while the effect for white non-Hispanic women is of the opposite sign and also statistically significant. The effect for white non-Hispanic women could be consistent with some of the stress hypotheses discussed in the medical literature (e.g., Hedegaard, Henriksen, Sabroe and Secher 1993)—i.e., more educated women may work in more stressful jobs, leading to an elevant incidence of prematurity.

[23] Angrist and Krueger (1991) similarly document much stronger effects of compulsory schooling for white men compared to black men, and Lleras-Muney (2005) echoes this conclusion for changes in child labor laws and compulsory schooling laws. Goldin and Katz (2003) argue that continuation schools, an important factor in the rise in educational attainment for 1910 to 1940, have similar effects for blacks and whites.

[24] Because of the smaller sample sizes underlying the estimates in this table, we use a slightly larger bandwidth of 70 days throughout. This is appropriate for a bandwidth selector of order $n^{-1/5}$ (cf., Porter 2003, Theorem 3(b)), since several of our estimates are based on 20 percent subsamples ($50 \times 0.2^{-1/5} \approx 70$).

Second, if the relationship between schooling and infant health is non-linear, the effect of education will depend on the level of education manipulated by the intervention. Observational comparisons suggest such a nonlinear relationship, with the biggest health returns concentrated amongst the lowest-educated. As we have discussed, school entry policies primarily affect the number of years of high school education (cf., Figure 4). This might suggest that our estimates should be larger than many of those in the literature, where the recent focus has been more on research designs that impact college attendance.

Third, for different interventions, the effect of education may operate through different channels. For example, suppose understanding the fetal health implications of smoking while pregnant is the dominant mechanism behind education's impact on infant health, and suppose that exposure to college is required for women to appreciate these fetal health implications (as might occur through peer effects). Then educational manipulations affecting college attendance only negligibly may have negligible impacts on infant health. In this example, interventions targeting college would have larger direct/Grossman effects on infant health than would interventions targeting high school. In addition, interventions targeting college could have larger indirect effects on infant health than those targeting high school if the number of years of college is more important for a woman's financial resources, as might occur through non-linearities in the labor and/or mating market return to schooling.

These effects may depend on the degree of foresight in the subpopulation affected by the manipulation. Our study focuses on fertility and maternal investment behavior for women 23 or younger. These women might not invest in maternal behaviors protective of infant health if they fail to anticipate the labor and mating market returns to their schooling. Other studies focus on fertility and maternal investment behaviors observed at older ages, where women may have already appreciated the returns to their schooling.

Nonetheless, our data provide some suggestive evidence that the women in our study, while young, already anticipate the labor and mating market returns to their schooling. For California, using school entry cutoff dates as an instrument for education, we find that an extra year of education reduces the likelihood of public payment for delivery (e.g., Medicaid) and raises the likelihood of private payment (e.g., private insurance), leaving self-payment (i.e., out-of-pocket) unaffected. In most cases, eligibility for public funding is dependent on income. This suggests that, already at motherhood, the women in our study are experiencing differences in income due to their education. Alternatively, they might anticipate future income differences and exert more effort in becoming eligible (e.g., completing paperwork). For Texas, again using the school entry dates as an instrument, we find that an extra year of education lowers the likelihood that a woman receives prenatal care in a hospital and raises the likelihood that she receives care in a private clinic, leaving unchanged the likelihood of care in a public health clinic (results available upon request).

Fourth, there is an distinction between education at motherhood and completed education. The women in our study are young and may have had their educational progression interrupted. This raises the possibility that these women might return to school after childbearing, in which case our research design would not capture the effect of completed education, but rather the effect of education at motherhood. For other research designs, childbearing might occur at an age where a return to schooling is unlikely and the research design might measure the effect of completed education (e.g., Currie and Moretti 2003). The effect of completed education on infant health may be different than the effect of education at motherhood on infant health. For example, an intervention that affects education at motherhood but not completed education should not have an indirect effect of schooling due to resources (assuming foresight). As another example, an intervention that affects completed education but not education at motherhood should not have direct/Grossman effects, because a woman cannot yet know what she has not yet learned.

To understand the dynamics of female schooling decisions following first births, we examined the patterns of school enrollment and school completion among the sample of women from the 1979 National Longitudinal Survey of Youth (NLSY79) (results available on request). Women having their first birth before 18 are substantially more likely to return to school than women having their first birth after 18. This pattern suggests that for mothers younger (older) than 18, our estimates isolate the effect of education at motherhood (completed education).[25] Table 5 presents separate estimates for these two age groups. As suggested by Figure 3, the education discontinuity is smaller for 18-23 year olds than for those below 18. However, for low birth weight and prematurity, the estimates are statistically indistinct across the two age groupings. This suggests that both the effect of education at motherhood and the effect of completed education are small for this study.

Fifth, and finally, the effect of schooling may differ depending on the type of intervention involved. Consider two broad types of policies that could increase years of schooling: (i) those affecting school exit decisions (e.g., raise the minimum dropout age) and (ii) those affecting school entrance decisions (e.g., lower the age at school entry). Even if these policies exert similar effects on educational attainment, the impacts on fertility and maternal investment behaviors could plausibly differ. Moreoever, even if fertility and maternal investment impacts are quantitatively similar for school entry and school exit interventions, the economic interpretations may differ.

To understand this point, consider a woman who desires to have children early in life (but after completing schooling), and who wants to avoid violating compulsory schooling laws (i.e., she will drop out of school as soon as she lawfully can). A school exit intervention extending the compulsory school leaving age by one year will likely lead such a woman to delay childbearing by a year. However, this effect represents not just the direct/Grossman effect and the indirect effect, but also the mechanical delay associated with the woman's desire to comply with the law. Hence, for this type of woman, the meaning of the fertility effect of schooling is different depending on the type of intervention under discussion. Further, this mechanical delay in fertility creates problems for the identification of the effect of education on infant health, since maternal age is believed to causally affect infant health. Similar interpretation differences arise when applying these two research designs to other types of women, as well.[26]

[25] Using a longitudinal Texas data set, we estimate that the schooling discontinuity for second-time mothers is 70 percent as large as the schooling discontinuity for those same mothers at first birth. Unfortunately, this panel is too small to estimate precise reduced-form effects.

[26] As a second example, consider a woman who has an unplanned pregnancy (e.g., contraceptive failure) while a freshman in high school and who, due to time constraints, finds herself unable to return to school after childbearing. Lowering the age at school entry for this type of woman would ensure that she completed all of ninth grade and part of tenth grade. This additional education would lead to direct/Grossman effects and, assuming foresight, indirect effects. In contrast, raising the compulsory schooling age for such a woman from 16 to 17 will leave her education

Thus, school entry interventions do not mechanically affect fertility in the same manner as school exit interventions, and hence sidestep some of the identification problems outlined. However, school entry interventions are a dual manipulation of schooling and age relative to peer group, and this may create identification problems of its own. We take up this issue in the next subsection.

6.2 Age-for-Grade Effects

Our research design exploits the fact that, due to the timing of her birth, a woman born before the school entry date will typically enter school a year ahead of when she would have entered, had she been born after the school entry date. However, entering school early implies not just getting ahead of the pack, but also being younger than the pack. Thus, school entry policies amount to a dual manipulation of schooling and age relative to peers.[27]

If relative age is unimportant for behaviors and outcomes, then our comparisons highlight the effect of schooling alone. However, if relative maturity is important, it could potentially explain why we find small and insignificant effects of education on fertility and infant health. Within the economics literature, the consensus is that children who are older in their class perform better in school than children who are younger (Bedard and Dhuey 2006, Elder and Lubotsky 2009, Cascio and Schanzenbach 2007, Kawaguchi and Kenkyūjo 2006, Fredriksson and Öckert 2005). In general, old-for-grade students have higher test scores, are less likely to repeat grades, and complete more schooling than young-for-grade students.[28] We might thus expect that the effect of schooling on fertility and infant health and the effect of being young relative to peers on fertility and infant health are of opposite sign, potentially leading estimates of small magnitude, possibly sufficiently small as to be consistent with no effect.

While we cannot entirely rule out the hypothesis that age-for-grade effects are offsetting pure

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unaffected. Thus, a compulsory schooling research design cannot be used to learn about the infant health impacts of schooling for this type of woman. A complete discussion of the impacts of these two policy interventions on different types of women requires an articulated formal model of schooling and fertility and maternal investment behaviors. We do not have space to present such a model here.

[27] As noted, this dual manipulation feature of an instrumental variables approach to schooling is intrinsic. For example, as noted above, changes in compulsory schooling policies are dual manipulations of schooling and absolute age.

[28] Because these observations are largely based on within-grade comparisons, there is an ongoing debate whether this observation should be interpreted as an age-for-grade effect or an absolute age at school entry effect.

schooling effects, we argue against it on three grounds. First, in terms of test scores, the performance gap between younger and older students declines with age—suggesting that the long-run effects of age-for-grade may be small. Second, in the United States, unlike in some other countries (Kawaguchi and Kenkyūjo 2006, Fredriksson and Öckert 2005), individuals born immediately after the school entry cutoff date acquire fewer years of schooling than the individuals born immediately before (Dobkin and Ferreira 2007). This may imply a weakened role of age-for-grade in the long run for the United States relative to other countries.[29]

Third, for both California and Texas, we find small and insignificant differences in the probability of becoming a mother, age at first birth, or infant health. Stipulating that the age-for-grade effect was of the opposite sign of the education effect, it would be surprising if, in each of these contexts, the effects were close enough in magnitude as to make the net effect small. Indeed, our estimated effects of school entry policies on the probability of becoming a mother, age at first birth, and infant health are approximately zero *for all cohorts*. It would be all the more surprising if the age-for-grade effects for each of these outcomes were of the opposite sign of the education effect for all cohorts.

While there exists this extensive literature on the effect of age-for-grade on education-related outcomes, we know of no research on age-for-grade effects on fertility or infant health. Moreover, at least for fertility, the direction of bias to an age-for-grade effect is theoretically ambiguous. On the one hand, being old for one's grade could affect social development. In this case, the age-for-grade effect could be protective against pregnancy, with mature girls resisting the advances of persuasive boys. On the other hand, being old for one's grade could make pregnancy more likely if older girls are more popular than younger girls and if sexual activity is increasing in popularity.[30]

Even if both education and age-for-grade effects are operative, our empirical results continue to have an interpretation as the program evaluation of postponing schooling as it pertains to fer-

[29] Many factors contribute to these cross-country differences. We leave the explanation of these differences to future research.

[30] In addition, within a grade, younger girls may look up to older girls and mimic their behaviors. Mimicry renders ambiguous the sign of the age-for-grade effect, because of dependence on the magnitude of the pure age effect. Similar ambiguities surround age-for-grade effects on behaviors, such as maternal smoking.

tility and infant health. This policy evaluation is relevant both to the private decisions of parents contemplating when their children should start school, as well as to the current debate regarding the appropriate entry date. Several states have recently moved, or are currently debating moving, these dates from late in the year to the early Fall (Datar 2006), with the stated rationale of raising the age of the average kindergartner (Aizenman 2002). While starting children at older ages may help them cope with the demands of an increasingly rigorous kindergarten curriculum, our results suggest that, for some girls, doing so makes it more likely that pregnancy will interrupt school progression at an earlier grade. To the extent that these schooling differences will be permanent, our results suggest this will lead to reduced completed schooling, mates of lower education and earnings ability, and diminished lifetime income.[31]

7 Conclusion

We have argued that, for some women, education may play a more muted role in fertility and child investment decisions than suggested by the previous literature. Our evidence is based on comparisons of outcomes between women born just before and after the school entry date. Compared to women born just before the school entry date, women born just after the entry date (i) have substantially lower schooling, as expected, (ii) are equally likely to become mothers, (iii) give birth at similar ages, and (iv) give birth to similarly healthy infants. That we do not document differences in infant health is surprising, given the assortative mating results: school entry policies lead to economically important differences in the age and education of a woman's mate. These comparisons are credible to the extent that confounders are smooth in day of birth for females who are potential mothers. On prior grounds we find it credible that two individuals born near in time are similar. To substantiate this point, we have provided evidence that measured pre-determined characteristics are similar for women born just before and after the school entry date.

[31] However, it may be difficult to infer from this policy experiment what might be the effects of large changes in the school entry cutoff date. Our estimates are most closely tied to a policy involving adjusting the school entry date by a small margin (e.g., from December 1 to November 30). Ideally, we would like to forecast the effects of a policy which adjusts the school entry date by a larger margin (e.g., from December 1 to September 1, in line with recent policy changes). However, this is a more challenging identification problem. Such a policy shift alters the age distribution of the entire classroom and would almost surely be combined with a policy to alter curriculum accordingly. While our evidence may shed light on the expected effects of such a policy reform, it does not provide a fully credible evaluation.

Our estimates are specific to the subpopulation of women whose education at motherhood is affected by school entry policies. These women may be negatively selected, for several reasons. First, their parents were willing to comply with school entry policies, as is more common among parents of low socio-economic status. Second, school entry policies affect education at motherhood for those women giving birth at young ages with low education generally. Thus, these results may be difficult to generalize to other subpopulations.[32]

On the other hand, this may mean that our results are relevant for specific policies. The National Campaign to Prevent Teen Pregnancy, a non-profit and non-partisan initiative, emphasizes the importance of schooling in reducing rates of teenage pregnancy. Our results suggest that such emphasis may be misplaced. When policymakers envision expensive interventions to raise female education, they should think carefully of how they expect increases in education to improve well-being, particularly with teenagers.

Finally, these estimates directly address the fertility and infant health consequences of starting school early. Parents of children with birthdays near the school entry date may be interested in these findings, particularly if they view their child as at risk of dropping out of school. Moreover, there continues to be an active policy debate regarding the appropriate age at school entry, and several states have changed the school entry date to earlier in the year in order to raise the average age of kindergartners. Our results suggest that even if moving back the entry date does succeed in improving the preparedness of some children for an increasingly intensive kindergarten curriculum, such a policy shift is not without costs and may create both winners and losers.

[32] In addition, we have emphasized further features of our approach that problematize extrapolation to other contexts.

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Table 1. Descriptive Statistics

	California (1989-2002)			Texas (1989-2001)		
	All Mothers	First-Time Mothers	First-Time Native Mothers Under 24	All Mothers	First-Time Mothers	First-Time Native Mothers Under 24
% Mothers White Non-Hispanic	26.97	29.32	36.70	35.97	38.57	39.06
% Mothers White Hispanic	55.76	52.60	45.35	47.69	45.44	41.14
% Mothers Black	8.26	8.09	14.04	14.19	13.52	19.38
Mother's education (years)	11.19	11.47	11.57	11.25	11.45	11.19
	[3.00]	[2.90]	[1.68]	[2.66]	[2.64]	[1.84]
Mother's age (years)	22.44	21.11	18.94	21.87	20.64	18.83
	[4.02]	[3.90]	[2.23]	[3.81]	[3.67]	[2.26]
% Low birth weight (<2500 grams)	5.07	5.88	6.24	6.38	7.17	7.96
% Premature (<37 weeks gestation)	9.67	9.66	10.09	9.72	9.55	10.49
Infant mortality rate (deaths before 1 year per 1K births)	NA	NA	NA	0.60	0.58	0.67
% Mothers smoking during pregnancy	1.93	1.70	2.83	8.03	6.66	8.49
% Mothers drinking during pregnancy	NA	NA	NA	0.90	0.87	0.97
% Mothers with STDs	1.26	1.39	1.68	2.76	2.90	3.85
% Mothers with prenatal care	98.80	99.00	99.05	97.06	97.51	98.02
% Prenatal care began in 1st trimester	74.77	75.42	73.09	68.83	70.33	67.58
Number of prenatal care visits	11.23	11.38	11.39	10.56	10.81	10.78
	[4.10]	[4.08]	[4.07]	[4.56]	[4.48]	[4.36]
% Father present	87.71	85.90	83.57	78.22	75.42	68.74
Father's education (years)	11.19	11.39	11.46	11.64	11.86	11.63
	[3.42]	[3.45]	[2.67]	[2.85]	[2.78]	[1.94]
Father's age (years)	25.97	24.58	21.90	25.49	24.16	21.97
	[5.66]	[5.50]	[4.09]	[5.45]	[5.24]	[4.12]
% Having first birth	52.02	100.00	100.00	52.02	100.00	100.00
Observations	3,264,615	1,698,232	641,557	2,112,017	1,098,598	533,165

Notes: Table reports means and standard deviations (brackets) for mothers in 1969 to 1987 (1986) cohorts for California (Texas). Mothers with missing education, parity, or birth date values or non-singleton births are excluded. Native subsample includes only mothers born in that state. Father's presence is measured by the presence of his educational attainment and birthdate on the birth certificate.

Table 2. Discontinuity in Probability of Giving Birth at Specific Ages

Age	California	Texas	Age	California	Texas
13	-0.0003 (0.0002) [0.96] {0.0004}	-0.0001 (0.0003) [0.67] {0.0008}	24	-0.0017 (0.0019) [0.26] {0.0097}	-0.0005 (0.0015) [0.01] {0.0120}
14	-0.0004 (0.0004) [0.44] {0.0027}	-0.0018 (0.0008) [0.98] {0.0044}	25	-0.0003 (0.0015) [0.57] {0.0081}	-0.0030 (0.0019) [0.65] {0.0102}
15	-0.0001 (0.0007) [0.54] {0.0083}	-0.0017 (0.0009) [0.96] {0.0117}	26	0.0008 (0.0015) [0.67] {0.0070}	0.0005 (0.0023) [0.85] {0.0083}
16	0.0004 (0.0010) [0.68] {0.0164}	0.0012 (0.0012) [0.64] {0.0224}	27	0.0003 (0.0014) [0.04] {0.0059}	-0.0007 (0.0028) [0.63] {0.0066}
17	0.0004 (0.0013) [0.76] {0.0228}	-0.0003 (0.0017) [0.22] {0.0320}	28	-0.0015 (0.0015) [0.66] {0.0051}	-0.0021 (0.0026) [0.92] {0.0051}
18	-0.0006 (0.0017) [0.74] {0.0286}	-0.0036 (0.0021) [0.65] {0.0380}	29	0.0002 (0.0026) [0.50] {0.0040}	-0.0024 (0.0025) [0.74] {0.0035}
19	0.0002 (0.0017) [0.61] {0.0318}	-0.0025 (0.0023) [0.08] {0.0390}	30	0.0024 (0.0023) [0.61] {0.0031}	0.0015 (0.0038) NA {0.0022}
20	-0.0009 (0.0019) [0.96] {0.0263}	-0.0019 (0.0023) [0.39] {0.0332}	31	0.0035 (0.0042) NA {0.0020}	
21	-0.0027 (0.0016) [0.69] {0.0200}	0.0027 (0.0021) [0.93] {0.0253}			
22	-0.0003 (0.0017) [0.90] {0.0154}	0.0030 (0.0020) [0.07] {0.0193}	Observed at 23 or Younger	-0.0018 (0.0039) [0.99] {0.1848}	-0.0048 (0.0051) [0.88] {0.2411}
23	0.0005 (0.0015) [0.99] {0.0121}	0.0001 (0.0018) [0.55] {0.0148}	Observed at Any Age	-0.0019 (0.0048) [0.99] {0.2307}	-0.0072 (0.0060) [0.99] {0.2903}

Note: Standard errors in parentheses. P-values on cross-cohort restrictions in brackets below standard errors. Sample means in braces below p-values. See text for details. For California (Texas), there are 950,272 (664,058) individuals in our study cohorts born within 50 days of the school entry date.

Table 3. Effects of School Entry Policies:
First Stage and Reduced Form Estimates

California			Texas		
Fraction Observed	**Maternal Age**	**Maternal Education**	**Fraction Observed**	**Maternal Age**	**Maternal Education**
-0.0019	0.0127	-0.1436	-0.0072	0.0147	-0.2427
(0.0048)	(0.0306)	(0.0150)	(0.0060)	(0.0297)	(0.0144)
[0.99]	[0.70]	[0.18]	[0.90]	[0.09]	[0.01]
{0.23}	{20.45}	{11.58}	{0.27}	{20.06}	{11.19}
951,164	214,608	172,256	664,058	188,692	156,879
Birth Outcomes			**Birth Outcomes**		
Low Birthweight	**Prematurity**	**Infant Death**	**Low Birthweight**	**Prematurity**	**Infant Death**
-0.0006	-0.0012	NA	-0.0051	-0.0029	0.0013
(0.0025)	(0.0033)		(0.0030)	(0.0033)	(0.0010)
[0.89]	[0.48]		[0.14]	[0.01]	[0.53]
{0.06}	{0.10}		{0.08}	{0.11}	{0.01}
172,248	164,773		156,771	156,195	156,879
Risky Maternal Behaviors			**Risky Maternal Behaviors**		
Mother Smokes	**Mother Drinks**	**Mother Has STDs**	**Mother Smokes**	**Mother Drinks**	**Mother Has STDs**
0.0041	NA	0.0020	-0.0013	-0.0020	0.0007
(0.0017)		(0.0015)	(0.0034)	(0.0011)	(0.0025)
[0.90]		[0.70]	[0.85]	[0.69]	[0.15]
{0.03}		{0.02}	{0.08}	{0.01}	{0.04}
172,194		164,978	138,852	138,663	141,575
Prenatal Care			**Prenatal Care**		
Any Care	**Care in First Trimester**	**Number of Visits**	**Any Care**	**Care in First Trimester**	**Number of Visits**
0.0002	-0.0002	-0.0150	0.0020	-0.0095	-0.0928
(0.0010)	(0.0051)	(0.0446)	(0.0017)	(0.0055)	(0.0512)
[0.61]	[0.67]	[0.37]	[0.51]	[0.77]	[0.28]
{0.99}	{0.74}	{11.40}	{0.98}	{0.69}	{10.77}
170,879	170,364	167,770	153,845	153,834	149,083
Paternal Characteristics			**Paternal Characteristics**		
Father Present	**Father's Age**	**Father's Education**	**Father Present**	**Father's Age**	**Father's Education**
0.0005	-0.1183	-0.0779	0.0013	-0.2163	-0.0916
(0.0040)	(0.0410)	(0.0277)	(0.0052)	(0.0477)	(0.0236)
[0.55]	[0.08]	[0.14]	[0.37]	[0.55]	[0.39]
{0.84}	{21.90}	{11.45}	{0.68}	{21.98}	{11.63}
172,256	148,743	151,331	156,879	107,197	105,747

Note: Standard errors in parentheses. P-values on cross-cohort restrictions in brackets below standard errors. Sample means in braces below p-values. Number of observations below sample means. See text for details.

Table 4. IV Effects of Female Education on Infant Health

	California	Texas	Pooled
Low Birthweight	0.0036	0.0199	0.0142
	(0.0161)	(0.0118)	(0.0095)
	{0.06}	{0.08}	{0.41}
	172,248	156,771	
Prematurity	0.0076	0.0100	0.0094
	(0.0241)	(0.0141)	(0.0122)
	{0.10}	{0.11}	{0.93}
	164,773	156,195	
Infant Death		-0.0056	
		(0.0045)	
		{0.01}	
		156,879	

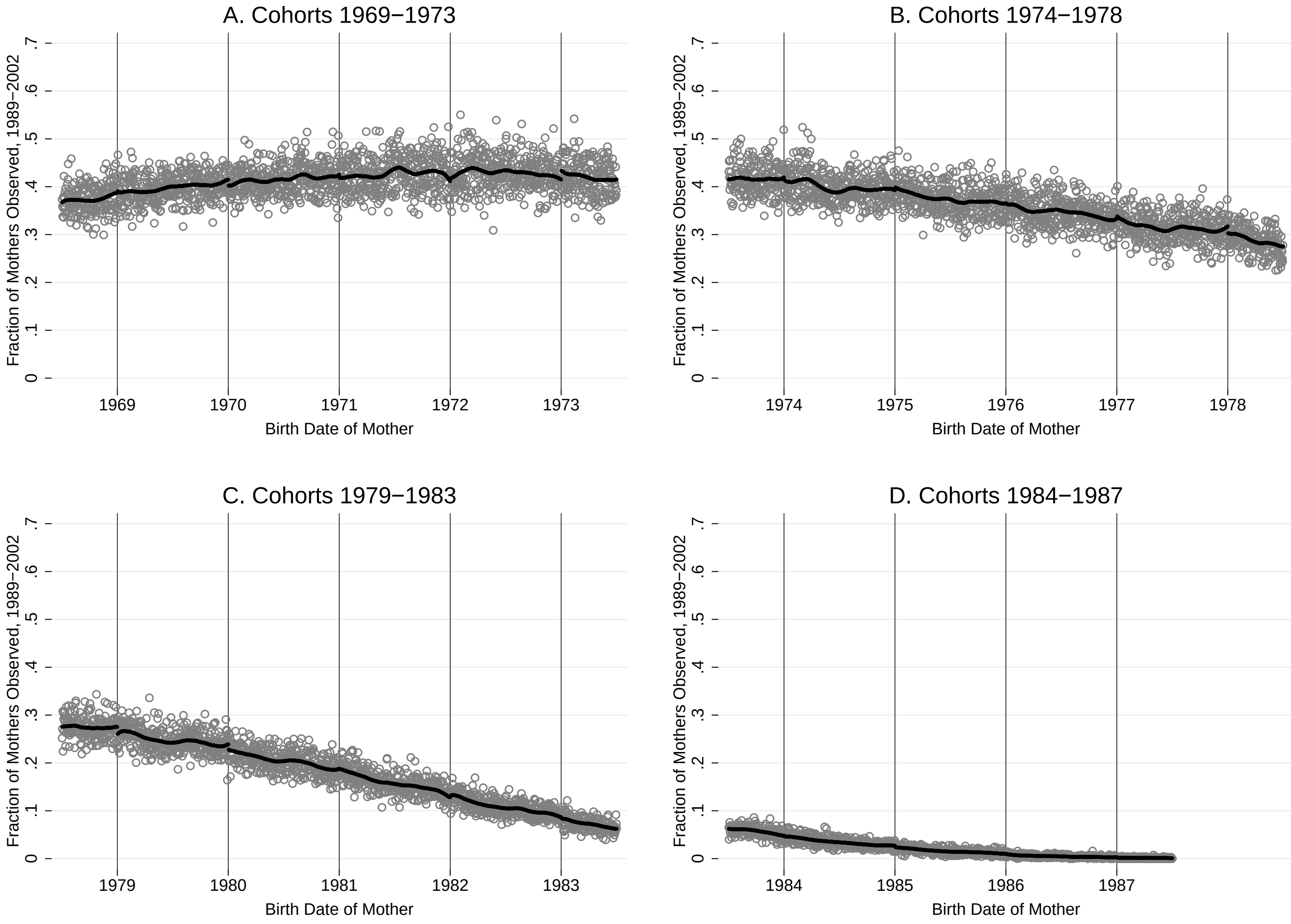
Notes: Standard errors in parentheses. For California and Texas, sample means in braces below standard errors, and sample sizes below sample means. For pooled estimates, p-values on cross state restrictions in brackets beneath standard errors.

Table 5. Heterogeneity in Effects of School Entry Policies

	By Race/Ethnicity				By Age		
	White, Non-Hispanic	White Hispanic	Black	Test of Equality	Less Than 18 Years Old	18-23 Years Old	Test of Equality
A. California							
Education	-0.1455	-0.1634	-0.1209	p=0.45	-0.2832	-0.1006	p<0.001
	(0.0195)	(0.0182)	(0.0290)		(0.0203)	(0.0146)	
	[0.09]	[0.14]	[0.06]		[0.34]	[0.52]	
	{11.87}	{11.31}	{11.65}		{10.10}	{12.13}	
	88,853	110,440	34,572		66,816	176,882	
Low Birthweight	-0.0071	-0.0017	0.0155	p=0.02	-0.0009	-0.0007	p=0.97
	(0.0032)	(0.0029)	(0.0074)		(0.0042)	(0.0024)	
	[0.85]	[0.44]	[0.34]		[0.78]	[0.59]	
	{0.05}	{0.06}	{0.10}		{0.07}	{0.06}	
	88,849	110,438	34,568		66,813	176,875	
Prematurity	0.0007	-0.0064	-0.0024	p=0.48	-0.0050	-0.0001	p=0.45
	(0.0042)	(0.0040)	(0.0083)		(0.0058)	(0.0030)	
	[0.85]	[0.16]	[0.32]		[0.88]	[0.01]	
	{0.09}	{0.10}	{0.13}		{0.12}	{0.09}	
	85,065	105,841	32,954		63,303	169,870	
B. Texas							
Education	-0.2837	-0.2438	-0.2059	p=0.06	-0.3764	-0.1984	p<0.001
	(0.0213)	(0.0202)	(0.0246)		(0.0195)	(0.0155)	
	[0.49]	[0.00]	[0.52]		[0.27]	[0.70]	
	{11.59}	{10.79}	{11.23}		{9.56}	{11.87}	
	83,173	89,147	42,411		63,680	151,991	
Low Birthweight	-0.0071	-0.0056	0.0017	p=0.52	0.0006	-0.0073	p=0.18
	(0.0037)	(0.0041)	(0.0069)		(0.0051)	(0.0029)	
	[0.76]	[0.58]	[0.44]		[0.31]	[0.50]	
	{0.06}	{0.07}	{0.12}		{0.09}	{0.07}	
	83,120	89,107	42,366		63,642	151,891	
Prematurity	-0.0019	-0.0078	0.0064	p=0.25	-0.0028	-0.0024	p=0.95
	(0.0042)	(0.0046)	(0.0073)		(0.0064)	(0.0031)	
	[0.06]	[0.45]	[0.32]		[0.04]	[0.29]	
	{0.08}	{0.11}	{0.15}		{0.13}	{0.10}	
	82,863	88,785	42,161		63,345	151,400	
Infant Death	0.0010	0.0005	0.0012	p=0.92	0.0005	0.0010	p=0.82
	(0.0014)	(0.0011)	(0.0020)		(0.0017)	(0.0009)	
	[0.73]	[0.30]	[0.87]		[0.19]	[0.26]	
	{0.01}	{0.01}	{0.01}		{0.01}	{0.01}	
	83,173	89,147	42,411		63,680	151,991	

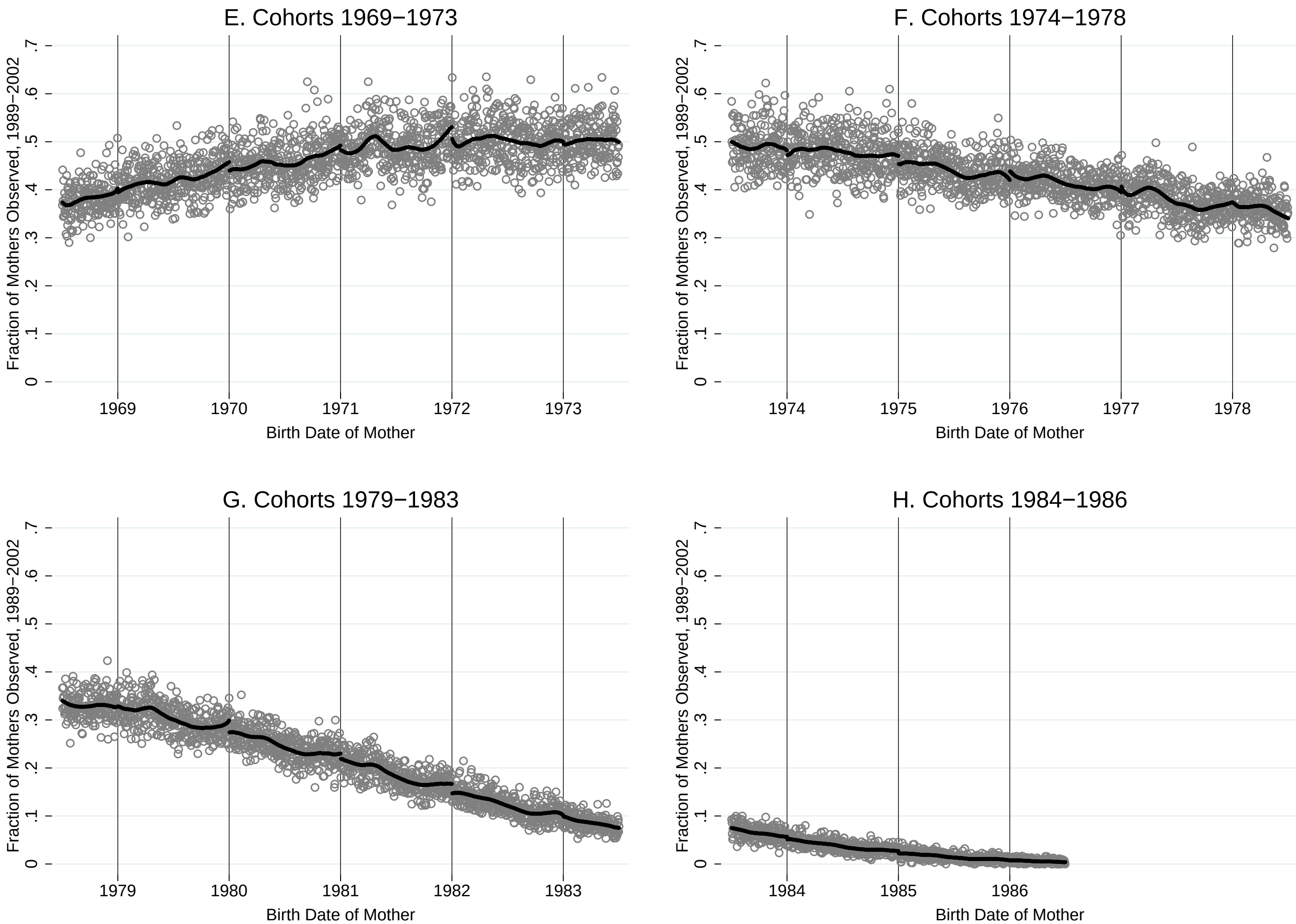
Notes: Standard errors in parentheses. P-value for test of cross-cohort restrictions in brackets below standard error. Sample mean in braces below p-value. Sample size below sample mean. Estimates based on bandwidth of 70 days. Column 4 (7) presents p-value on restriction that discontinuity is equal for the 3 different race/ethnic groups (2 different age groups).

Figure 1. Fraction of Birth Cohort Observed At Childirth: California



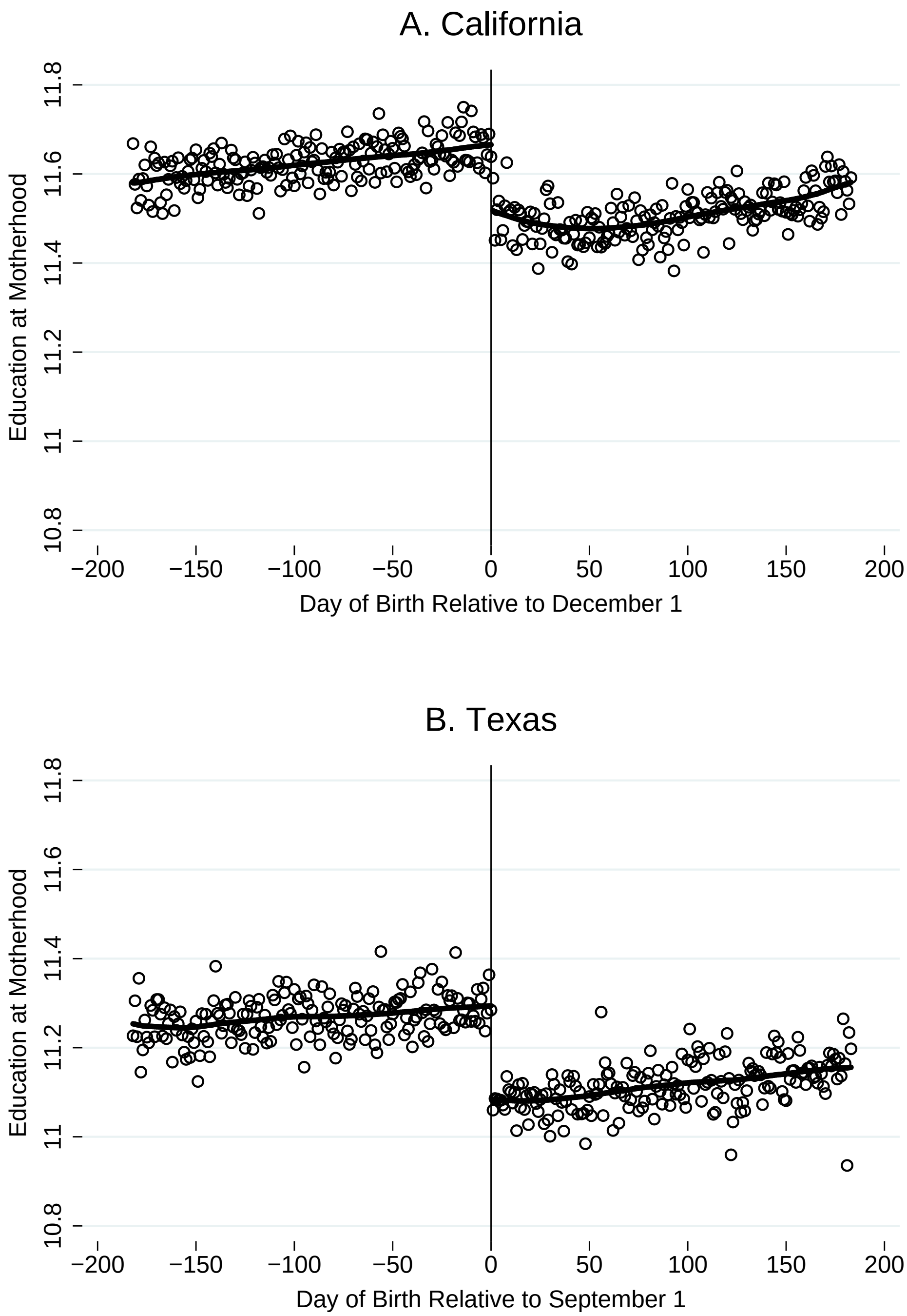
Note: Open circles represent the fraction of all baby girls born in California on the given day observed giving birth in California between 1989 and 2002. Solid curve is a local linear smoother fit separately for each cohort ($h = 50$). Cohorts defined symmetrically about school entry dates, which are indicated by vertical lines. See text for details.

Figure 1 (cont.). Fraction of Birth Cohort Observed At Childirth: Texas



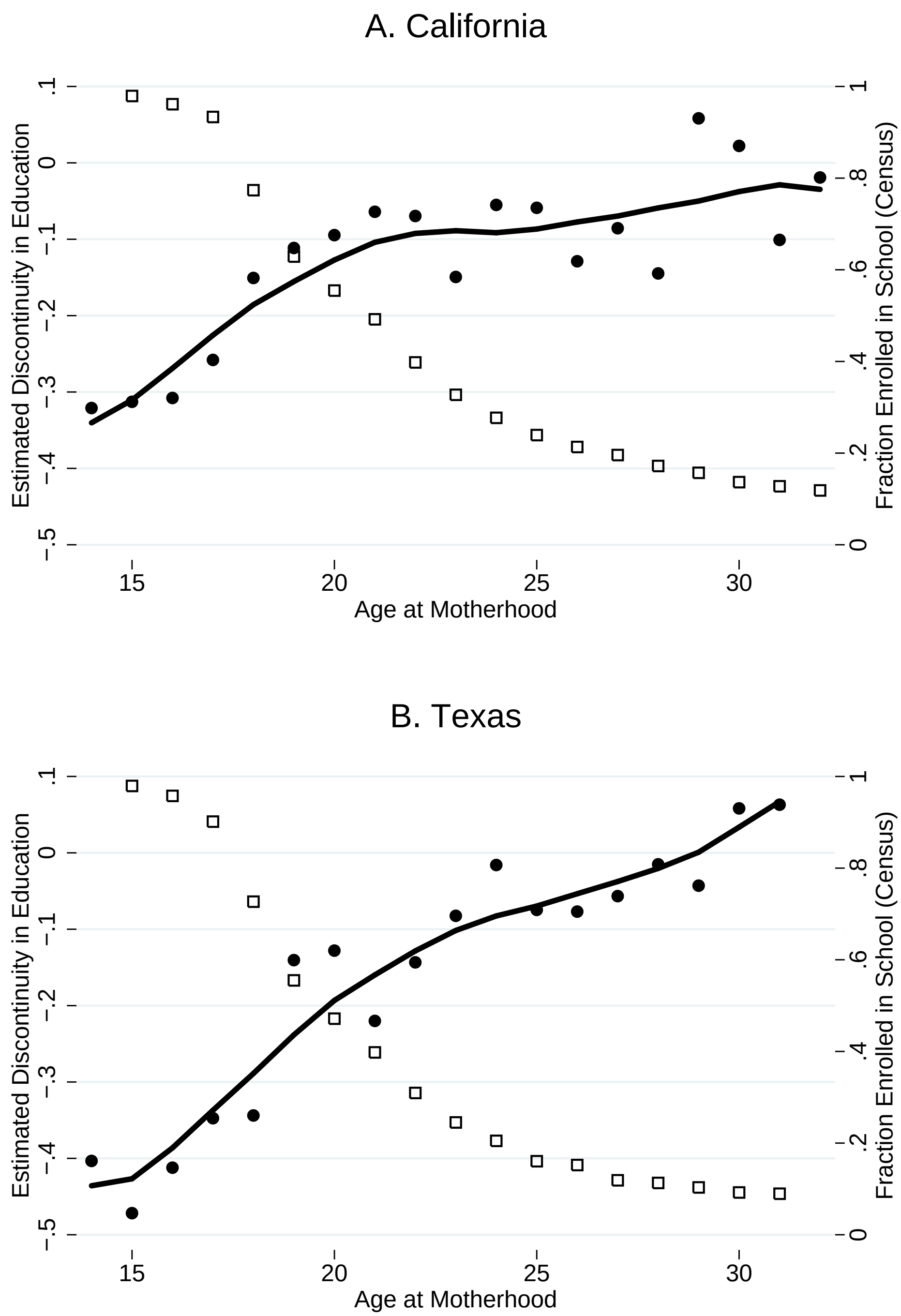
Note: Open circles represent the fraction of all baby girls born in Texas on the given day observed giving birth in Texas between 1989 and 2001. Solid curve is a local linear smoother fit separately for each cohort ($h = 50$). Cohorts defined symmetrically about school entry dates, which are indicated by vertical lines. See text for details.

Figure 2. Education at Motherhood



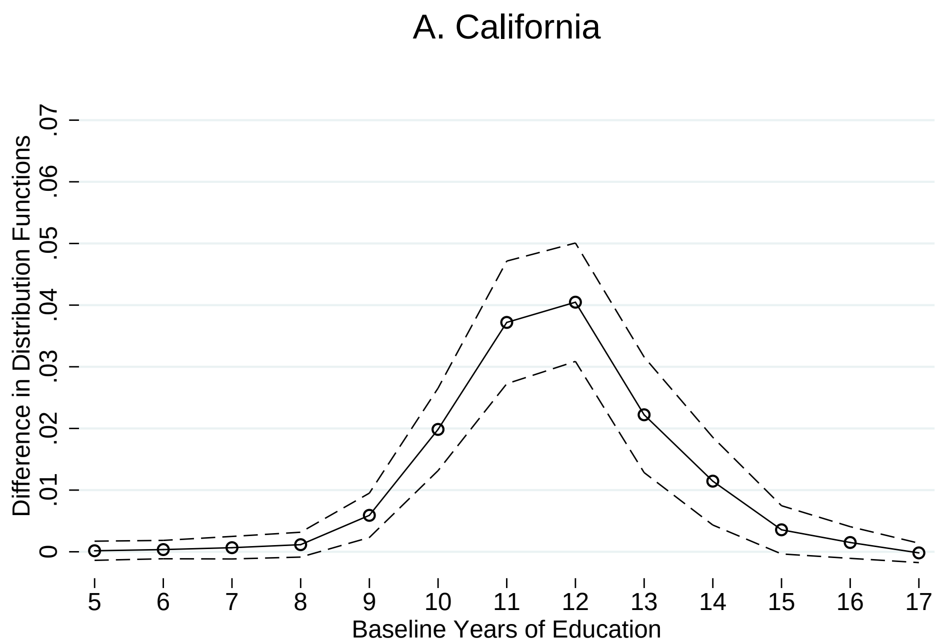
Note: Open circles are unconditional averages. Solid curve is a local linear smoother ($h = 50$). Estimates based on young women from post-1969 cohorts. See text for details.

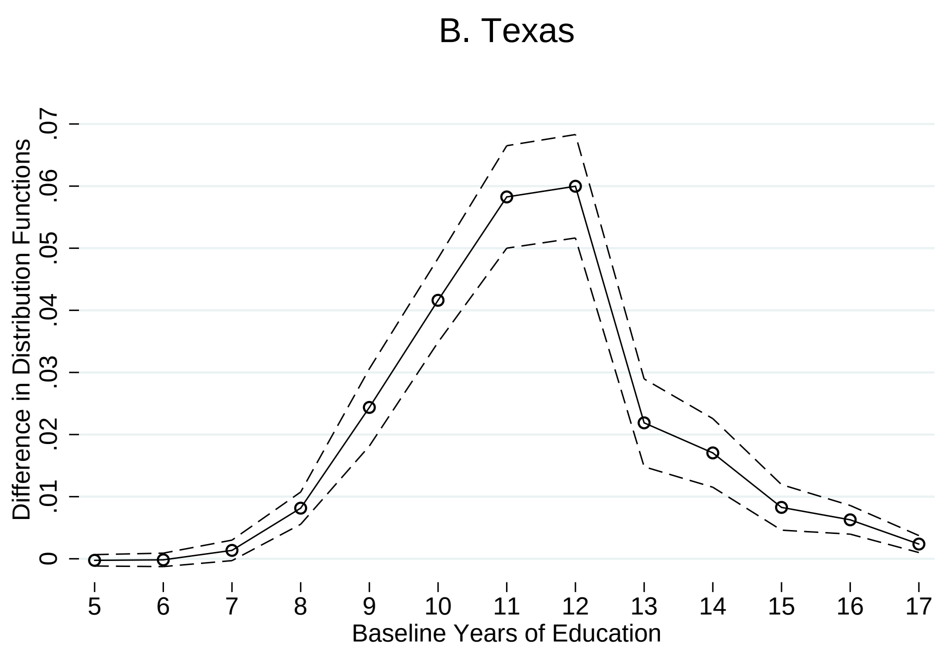
Figure 3. Age Profile of Education Discontinuity



Note: Open squares are estimates from the 2000 Census of the fraction of individuals of the specified age who were enrolled in school. Solid circles are age-specific estimated discontinuities in maternal education at the school entry date, based on a bandwidth of 75 days. The solid curve smooths the age-specific estimates using local linear smoothing using a bandwidth of 5 years. Estimates based on all available cohorts. See text for details.

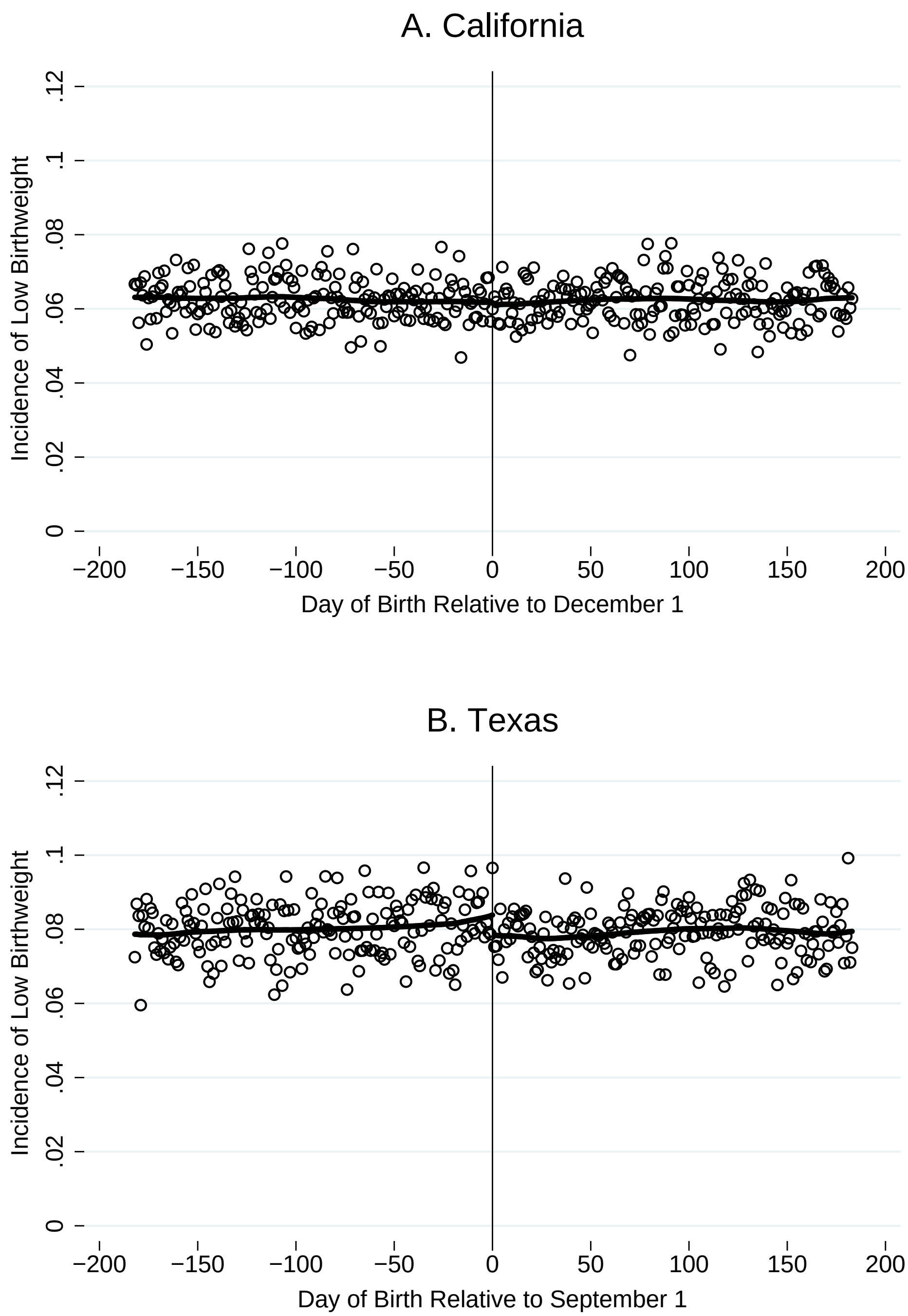
Figure 4. Distribution of Education Effects





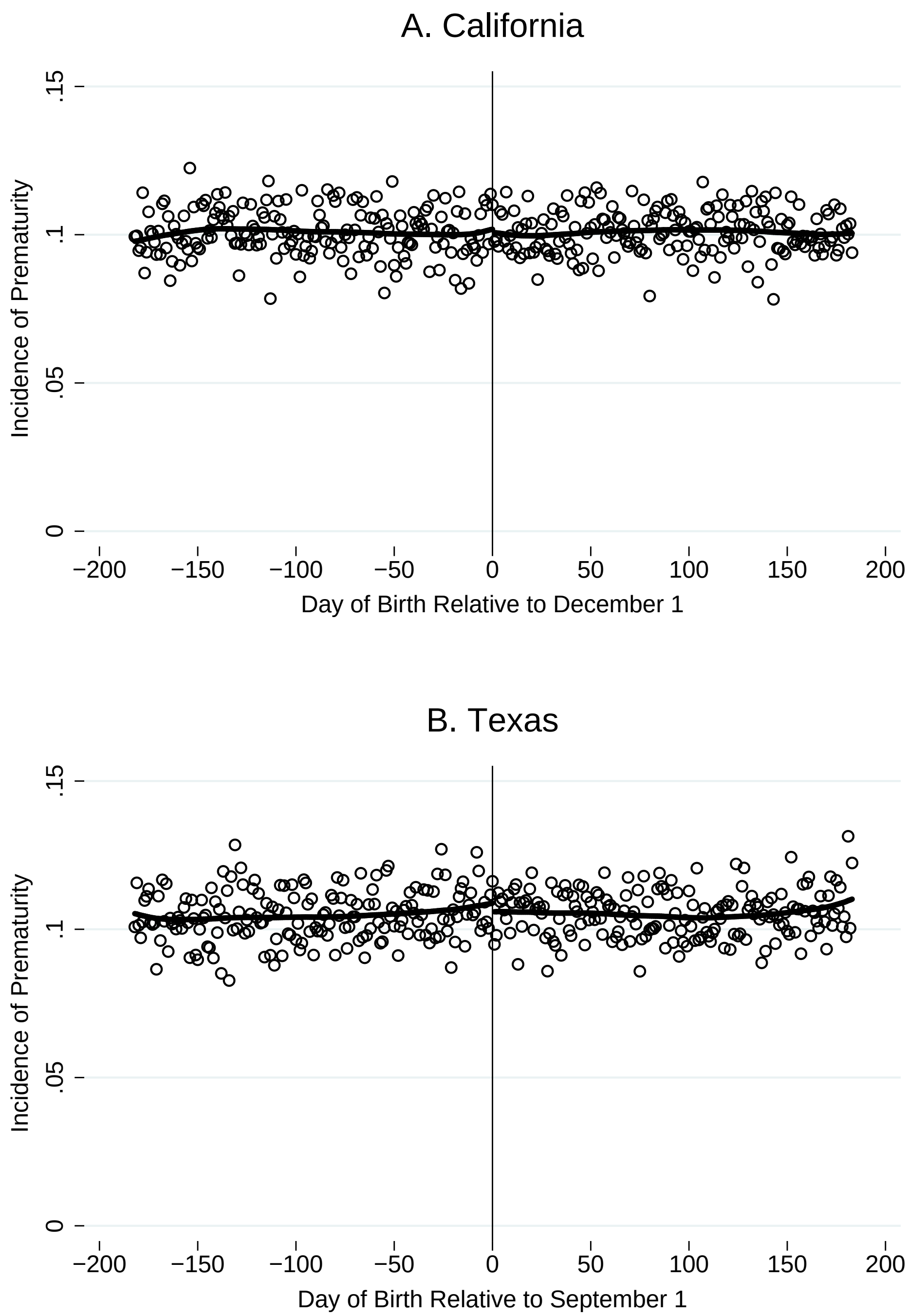
Note: Open circles represent differences in distribution functions for education for those born before and after the school entry date. Estimates based on young women from post-1969 cohorts. Dashed lines indicate pointwise confidence regions. See text for details.

Figure 5. Incidence of Low Birthweight



Note: Open circles are unconditional averages. Solid curve is a local linear smoother ($h = 50$). Estimates based on young women from post-1969 cohorts. See text for details.

Figure 6. Incidence of Prematurity



Note: Open circles are unconditional averages. Solid curve is a local linear smoother ($h = 50$). Estimates based on young women from post-1969 cohorts. See text for details.

Web Appendix

1 Estimates of the Probability of Motherhood by Age and Cohort

Here we expand on the description of Web Appendix Table 1. The table consists of 4 separate panels: 1A, 1B, 1C, and 1D. Panels 1A and 1B pertain to California; panels 1C and 1D pertain to Texas. For each panel, a row represents a cohort, and each column represents a specific age at which a mother could be observed in our administrative data.[1] The entries of the table are discontinuity estimates, with standard errors in parentheses.

The marginal rows at the bottom of the table present joint tests of no effect across cohorts, pooled estimates of the cohort-specific discontinuities, tests of the cross-cohort restrictions, and sample means.

The last column of panels 1B and 1D gives cohort-specific discontinuities corresponding to Figure 1 for California and Texas, respectively. None (one) of the cohort-specific discontinuities for California (Texas) are significantly different from zero. The lower right hand corner of panels 2B (2D) displays the pooled estimated discontinuity in the fraction observed at any age of -0.0019 (-0.0072). Compared to the overall fraction observed of 0.23 (0.29), these are very small discontinuities. Neither is statistically significant at conventional levels of significance.

The second-to-last columns of panels 2B and 2D present similarly small and insignificant estimates for the fraction observed at 23 or younger (i.e., selection into our estimation sample). The pooled discontinuity estimate for the probability of giving birth at 23 or younger is -0.0018 (-0.0048) for California (Texas), which is small relative to the mean of 0.18 (0.24).

The other columns of the table present age-specific probabilities of observation. These are useful to consider, because it is possible that there could be no aggregate change in the fraction observed, but individuals could be observed at different ages. The age-specific discontinuity estimates do not support this hypothesis, however. The estimated discontinuities are generally small, of fluctuating sign, and statistically indistinguishable from zero. For ex-

[1] Beneath the cohort label, we present the number of women in these cohorts who are born within 50 days of the school entry date.

ample, 2 (3) of the 162 point estimates for California are positive (negative) and significant, and 2 (4) of the 143 point estimates for Texas are positive (negative) and significant.

The third-to-last columns of panels 2B and 2D present tests of the hypothesis that the discontinuity estimates are jointly zero across all ages, for a given cohort ("test of no effect").[2] For each state and for each cohort, we fail to reject the hypothesis.

For each panel, the third-to-last block of rows presents tests of the hypothesis that the discontinuity estimates are jointly zero across all cohorts, for a given age ("test of no effect"). For California, we reject one of these hypotheses (age 28) and for Texas, we reject two of these hypotheses (ages 22 and 25). These rejections may be spurious, since we are testing many hypotheses. To test this idea, we also present a test of the hypothesis that the discontinuity estimates are jointly zero across all cohorts and all ages (bold entry in lower right of panels 2B and 2D). We fail to reject this stringent null hypothesis for both California and Texas.

The second-to-last block of rows presents pooled estimates of the discontinuity in the probability of being observed, for a given age. These pooled estimates impose potentially false cross-cohort restrictions. We present the p-value for the test of the hypothesis that the discontinuity is equal across cohorts (brackets). For all but one age in California and one age in Texas, we fail to reject the cross-cohort restrictions.

2 Power

In this section, we discuss ex-ante estimates of the sample sizes required to reject point hypotheses our own study is not able to rule out. For example, our data do not rule out the possibility that a one-year increment to schooling reduces infant mortality by -0.001.

We next explain our approach to computing the requisite sample sizes. Because they are

[2]Estimates and tests pooled across ages ignore the mechanical negative correlation between the indicator for being age a and the indicator for being age $a' \neq a$. Because we have many age categories, this negative correlation is negligibly small (cf., McCrary 2008). The simulation evidence we have examined shows that ignoring the negative correlation leads to extremely minor size distortions.

nonparametric in nature, our instrumental variables estimates have variances of the form

$$V\left[\widehat{\theta}\right] = \frac{c}{nh} \tag{A.1}$$

where n is our existing sample size, h is the bandwidth and c is a complicated function of the design matrix and prediction errors.

The estimated standard errors from our data provide a preliminary estimate of c that can be used to forecast what kinds of magnitudes of standard errors would be associated with point estimates from larger sample sizes. For example, doubling the sample size is expected to yield a standard error 70 percent as large as our estimated standard errors, holding the bandwidth fixed at $h = 50$.

An alternative approach to forecasting standard errors shrinks the bandwidth with the sample size. The theoretical econometrics literature suggests that the bandwidth should be of the order $n^{-1/5}$ (Porter 2003, Theorem 3(b)). Write $h = kn^{-1/5}$ for some k and note that this implies

$$V\left[\widehat{\theta}\right] = \frac{\tilde{c}}{n^{4/5}} \tag{A.2}$$

where $\tilde{c} = c/k$. The estimated standard errors from our data and our chosen bandwidth of $h = 50$ together furnish an estimate for $\tilde{c}$.

We thus have 2 approaches to power calculations for the regression discontinuity context. The first approach holds the bandwidth fixed (e.g., $h = 50$ regardless of sample size). The second approach shrinks the bandwidth at the theoretically prescribed rate. We turn now to calculating the sample size needed to reject a specific point hypothesis $\theta = \theta_0$ under both approaches, with a focus on tests of 5 percent size.

Under the first approach, for a two-sided test, we write

$$\frac{|\widehat{\theta}_N - \theta_0|}{\mathrm{se}_N} > 1.96 \Longleftrightarrow \frac{N-n}{n} > \frac{1.96^2}{(\widehat{\theta}_N - \theta_0)^2}\widehat{\mathrm{se}}^2 - 1 \tag{A.3}$$

where θ_0 is the point hypothesis to be tested, se_N is the standard error forecast for the new, larger sample size N, $\widehat{\text{se}}$ is the estimated standard error from our data, and $\widehat{\theta}_N$ is the point estimate we expect to obtain in the larger sample size.[3] (For example, we might choose to set $\widehat{\theta}_N$ equal to the estimate based on our data (i.e., $\widehat{\theta}$), or we might choose to set $\widehat{\theta}_N$ to zero.)

The right-hand side of the second inequality gives the predicted smallest percent increase in the sample size that will allow rejection of the point null hypothesis $H_0 : \theta = \theta_0$ in favor of the alternative $H_a : \theta \neq \theta_0$.

The second approach to power calculations for the regression discontinuity context shrinks the bandwidth with the sample size. Under this approach, for a two-sided test, we write

$$\frac{|\widehat{\theta}_N - \theta_0|}{\text{se}_N} > 1.96 \Longleftrightarrow \frac{N-n}{n} > \left(\frac{1.96^2}{(\widehat{\theta}_N - \theta_0)^2}\widehat{\text{se}}^2\right)^{5/4} - 1 \tag{A.4}$$

Intuitively, if the fixed bandwidth approach suggests that twice as much data (i.e., 100 percent more) is required to reject a particular hypothesis of interest, then the shrinking bandwidth approach suggests that 138 percent as much data is required ($2^{5/4} - 1 \approx 1.38$).[4]

Appendix Table 4 presents these calculations for selected outcomes, point hypotheses of interest, and hypothetical point estimates, $\widehat{\theta}_N$, that would obtain in the larger sample. For selected outcomes of interest—maternal smoking, prenatal care in the first trimester, low birth weight, prematurity, and infant death—we present typical point estimates from the literature (column 1) alongside IV estimates using our data (column 2). The IV estimates are pooled estimates for California and Texas.

The remaining columns of the table present our power calculations. Column 3 reports the point estimate we might expect to obtain in the larger sample. Column 4 gives potential point hypotheses of interest, and column 5 gives the percent increase in sample size needed

[3] Note c in Equation A.1 will be the same in the old and new sample. Also, we have subtracted 1 from both sides of the inequality to make this expression in percentage terms.

[4] Under either approach, these results can be adapted to suit power calculations for one-sided tests. One-sided tests have alternative hypotheses of the form $H_a : \theta < \theta_0$ or $H_a : \theta > \theta_0$. The first type of alternative hypothesis is interesting when $\widehat{\theta}_N - \theta_0$ is positive, and the second type is interesting when $\widehat{\theta}_N - \theta_0$ is negative. One can show that the predicted smallest required percent increase in the sample size continues to have the form given by these inequalities, but with 1.64 replacing 1.96.

to reject that point hypothesis, using a two-sided test and assuming that the bandwidth is held fixed at $h = 50$ (cf., A.3). Column 6 mimics column 5, but reports the percent increase in the sample size required if the bandwidth were to be smaller than that we use here (cf., A.4). Point hypotheses from Currie and Moretti (2003) and Chou, Liu, Grossman and Joyce (2007) are presented for comparison purposes.

Rows 1 through 4 pertain to maternal smoking. For this outcome, our IV estimate is the same sign as that in the literature and statistically significant, but smaller in magnitude. We are interested in knowing what kinds of point hypotheses could be ruled out in larger samples, assuming that the point estimate in the larger sample was the same as that we obtain (-0.016). Columns 5 and 6 report that the point hypothesis -0.06 is rejected by our data, suggesting a smaller impact of schooling on smoking than in the literature. On the other hand, our data also rule out 0 as a plausible hypothesis. Our power calculations suggest that only 20 percent more data would be required to rule out a point hypothesis of -0.03. However, hypotheses such as -0.01 which are close to our point estimate, would be difficult to rule out even with very large samples.

Rows 5 through 8 pertain to prenatal care in the first trimester. Our IV estimate is again of the same sign as that in the literature. However, for this outcome, the estimate is not quite statistically significantly different from zero. We are thus interested in knowing what kind of a sample size would be required to rule out zero. Columns 5 and 6 report that even a 30-40 percent increase in sample size would be sufficient to rule out zero, assuming that the point estimate in the larger sample was the same as that we obtain (0.031). Ruling out the Currie and Moretti (2003) estimate of 0.02 seems infeasible, as does ruling out point hypotheses in the neighborhood of 0.04. On the other hand, ruling out a large point hypothesis such as 0.06 would be feasible with 50-70 percent more data.

Rows 9 through 16 pertain to low birth weight. For this outcome, our data provide substantial evidence against the Currie and Moretti (2003) estimate. In particular, our analysis rejects the point hypothesis of -0.01 and the much smaller point hypothesis of -0.005.

Indeed, if a 70 percent larger sample were collected and the point estimate was equal to what it is in our data (0.014), we could rule out the extremely small point hypothesis of -0.001.

Rows 13 through 16 consider a similar set of thought experiments regarding power, but change the assumptions. In particular, in these 4 rows, we assume that in the larger sample size, we would obtain a point estimate of zero. With such a point estimate, it would be possible to reject the Currie and Moretti (2003) estimate only with much more data, and rejecting smaller point hypotheses such as -0.005 and -0.001 is likely not feasible.

Rows 17 through 24 pertain to prematurity. For this outcome, the typical point hypothesis from the literature is on the edge of the confidence region based on our data. For example, the two-sided confidence region is $[-0.014, 0.028]$ and the one-sided confidence region is $[-0.011, 0.009]$. In a 50-70 larger sample with the same point estimate, we would reject the Currie and Moretti (2003) estimate of -0.01. To rule out a hypothesis of -0.005 would require much more data than we have.

Rows 21 through 24 consider a similar set of thought experiments, but assume that in the larger sample the point estimate would be zero rather than 0.009. Ruling out point hypotheses of -0.02 and -0.01 would be feasible under such a scenario, but ruling out smaller point hypotheses would be unlikely.

Rows 25 through 32 pertain to infant death. Currie and Moretti (2003) do not estimate the effect of education on this outcome. While the country context may be quite different, Chou et al. (2007) report an estimate of -0.005. The table shows that, using our research design, very large sample sizes are necessary to make precise statements about the effect of schooling on infant death.

Web Appendix Table 1A. Discontinuity in Fraction Giving Birth at Specific Ages, by Cohort: California, Ages 13 through 23

	13	14	15	16	17	18	19	20	21	22	23
1969 cohort								-0.003	-0.007	0.002	0.001
46,190								(0.009)	(0.004)	(0.008)	(0.003)
1970 cohort							0.004	-0.005	-0.005	-0.005	-0.003
46,190							(0.006)	(0.005)	(0.007)	(0.003)	(0.005)
1971 cohort						0.000	-0.002	-0.008	0.001	0.001	0.000
40,464						(0.004)	(0.009)	(0.009)	(0.004)	(0.004)	(0.003)
1972 cohort					0.008	-0.005	-0.005	0.007	0.001	0.001	0.002
39,762					(0.004)	(0.011)	(0.004)	(0.009)	(0.004)	(0.007)	(0.007)
1973 cohort				0.006	0.000	0.010	0.000	-0.002	0.002	-0.003	0.001
38,594				(0.003)	(0.008)	(0.006)	(0.005)	(0.004)	(0.004)	(0.006)	(0.003)
1974 cohort			-0.001	0.001	0.000	-0.002	0.011	-0.002	-0.006	-0.003	0.003
41,154			(0.002)	(0.006)	(0.004)	(0.006)	(0.005)	(0.004)	(0.005)	(0.008)	(0.003)
1975 cohort		-0.002	0.006	0.001	-0.002	0.003	-0.004	0.004	0.003	-0.002	0.001
41,666		(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.010)	(0.005)	(0.003)	(0.003)
1976 cohort	0.000	-0.001	-0.003	-0.002	0.003	0.002	0.001	0.004	-0.001	0.002	-0.001
44,132	(0.000)	(0.003)	(0.002)	(0.007)	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)	(0.007)
1977 cohort	0.000	0.001	0.000	0.001	0.005	-0.002	-0.002	0.000	-0.005	0.002	0.002
45,410	(0.001)	(0.002)	(0.002)	(0.004)	(0.006)	(0.007)	(0.007)	(0.005)	(0.004)	(0.004)	(0.003)
1978 cohort	-0.001	0.001	-0.004	-0.002	-0.001	0.002	-0.002	0.000	-0.006	0.000	
46,940	(0.001)	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)	(0.010)	(0.003)	(0.008)	(0.004)	
1979 cohort	-0.001	0.001	0.001	-0.002	-0.001	-0.007	-0.002	-0.002	-0.005		
50,968	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.005)		
1980 cohort	0.000	-0.001	-0.002	-0.004	-0.006	0.001	0.002	-0.004			
52,566	(0.001)	(0.001)	(0.002)	(0.003)	(0.006)	(0.005)	(0.004)	(0.004)			
1981 cohort	-0.001	0.001	0.001	0.002	0.000	-0.006	0.001				
54,956	(0.000)	(0.001)	(0.002)	(0.002)	(0.006)	(0.007)	(0.004)				
1982 cohort	0.000	-0.001	-0.001	0.002	0.003	0.001					
55,898	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)					
1983 cohort	-0.001	-0.001	0.001	0.002	-0.001						
56,090	(0.000)	(0.001)	(0.004)	(0.003)	(0.002)						
1984 cohort	0.000	-0.001	0.000	0.001							
58,884	(0.001)	(0.001)	(0.003)	(0.002)							
1985 cohort	0.000	-0.002	0.000								
61,919	(0.000)	(0.001)	(0.001)								
1986 cohort	0.000	0.001									
62,574	(0.000)	(0.001)									
1987 cohort	0.000										
65,915	(0.000)										
Test of no effect	9.88	13.02	10.05	8.98	7.69	7.74	9.20	4.71	9.74	4.32	2.40
p-value, no effect	[0.63]	[0.37]	[0.61]	[0.70]	[0.81]	[0.80]	[0.69]	[0.97]	[0.55]	[0.93]	[0.98]
Pooled Estimates	-0.0003	-0.0004	-0.0001	0.0004	0.0004	-0.0006	0.0002	-0.0009	-0.0027	-0.0003	0.0005
Standard Errors	(0.0002)	(0.0004)	(0.0007)	(0.0010)	(0.0013)	(0.0017)	(0.0017)	(0.0019)	(0.0016)	(0.0017)	(0.0015)
p-value, cohort restrictions	[0.96]	[0.44]	[0.54]	[0.68]	[0.76]	[0.74]	[0.61]	[0.96]	[0.69]	[0.90]	[0.99]
Means	{0.0004}	{0.0027}	{0.0083}	{0.0164}	{0.0228}	{0.0286}	{0.0318}	{0.0263}	{0.0200}	{0.0154}	{0.0121}

Web Appendix Table 1B. Discontinuity in Fraction Giving Birth at Specific Ages, by Cohort: California, Ages 24 through 31

	24	25	26	27	28	29	30	31	Test of No Effect [p-value]	Observed at 23 or Younger	Observed at Any Age
1969 cohort	-0.006	0.001	0.001	0.000	0.000	0.001	0.004	0.004	10.28	-0.003	0.004
46,190	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	[0.59]	(0.026)	(0.038)
1970 cohort	-0.001	-0.004	-0.003	0.001	0.000	0.004	0.001		8.42	-0.015	-0.012
46,190	(0.003)	(0.006)	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)		[0.75]	(0.007)	(0.013)
1971 cohort	-0.005	0.005	0.004	0.004	-0.004	-0.005			9.38	-0.005	-0.006
40,464	(0.005)	(0.003)	(0.005)	(0.003)	(0.003)	(0.007)			[0.67]	(0.027)	(0.040)
1972 cohort	0.006	0.001	-0.002	-0.007	-0.003				16.62	0.010	0.005
39,762	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)				[0.16]	(0.034)	(0.045)
1973 cohort	-0.004	0.002	0.001	0.002					8.65	0.017	0.019
38,594	(0.008)	(0.003)	(0.003)	(0.003)					[0.73]	(0.019)	(0.030)
1974 cohort	-0.002	-0.004	0.003						12.93	0.000	-0.005
41,154	(0.005)	(0.004)	(0.002)						[0.37]	(0.025)	(0.034)
1975 cohort	-0.002	-0.002							8.17	0.008	0.006
41,666	(0.009)	(0.003)							[0.77]	(0.016)	(0.024)
1976 cohort	0.001								4.38	0.004	0.001
44,132	(0.004)								[0.98]	(0.028)	(0.028)
1977 cohort									3.95	0.002	0.005
45,410									[0.97]	(0.021)	(0.020)
1978 cohort									3.80	-0.014	-0.015
46,940									[0.96]	(0.021)	(0.021)
1979 cohort									9.46	-0.015	-0.015
50,968									[0.40]	(0.017)	(0.017)
1980 cohort									6.16	-0.011	-0.011
52,566									[0.63]	(0.023)	(0.023)
1981 cohort									4.52	-0.001	-0.001
54,956									[0.72]	(0.016)	(0.016)
1982 cohort									2.59	0.003	0.003
55,898									[0.86]	(0.010)	(0.010)
1983 cohort									2.02	-0.002	-0.002
56,090									[0.85]	(0.011)	(0.011)
1984 cohort									0.99	-0.002	-0.002
58,884									[0.91]	(0.006)	(0.006)
1985 cohort									7.29	-0.003	-0.003
61,919									[0.06]	(0.003)	(0.003)
1986 cohort									2.96	-0.001	-0.001
62,574									[0.23]	(0.002)	(0.002)
1987 cohort									0.00	-0.001	-0.001
65,915									[0.98]	(0.001)	(0.001)
Test of no effect	2.40	9.89	4.87	3.72	9.97	2.50	2.14	1.05	**122.57**	10.38	6.24
p-value, no effect	[0.97]	[0.19]	[0.56]	[0.59]	[0.04]	[0.47]	[0.34]	[0.30]	**[0.99]**	[0.94]	[0.99]
Pooled Estimates	-0.0017	-0.0003	0.0008	0.0003	-0.0015	0.0002	0.0024	0.0035		-0.0018	-0.0019
Standard Errors	(0.0019)	(0.0015)	(0.0015)	(0.0014)	(0.0015)	(0.0026)	(0.0023)	(0.0042)		(0.0039)	(0.0048)
p-value, cohort restrictions	[0.26]	[0.57]	[0.67]	[0.04]	[0.66]	[0.50]	[0.61]	NA		[0.99]	[0.99]
Means	{0.0097}	{0.0081}	{0.0070}	{0.0059}	{0.0051}	{0.0040}	{0.0031}	{0.0020}		{0.1848}	{0.2307}

Web Appendix Table 1C. Discontinuity in Fraction Giving Birth at Specific Ages, by Cohort: Texas, Ages 13 through 23

	13	14	15	16	17	18	19	20	21	22	23
1969 cohort								0.006	-0.006	-0.005	0.001
32,522								(0.007)	(0.009)	(0.008)	(0.004)
1970 cohort							-0.005	-0.014	0.005	0.001	-0.004
33,938							(0.013)	(0.012)	(0.008)	(0.010)	(0.006)
1971 cohort						-0.009	-0.005	-0.006	0.003	0.011	-0.002
33,258						(0.008)	(0.005)	(0.005)	(0.010)	(0.004)	(0.005)
1972 cohort					0.000	-0.007	-0.004	-0.006	0.002	0.010	-0.003
31,286					(0.005)	(0.006)	(0.011)	(0.009)	(0.006)	(0.005)	(0.004)
1973 cohort				0.000	0.002	0.005	-0.003	-0.002	0.004	-0.007	0.008
30,924				(0.004)	(0.007)	(0.004)	(0.009)	(0.011)	(0.005)	(0.004)	(0.005)
1974 cohort			-0.001	0.005	-0.003	-0.006	-0.001	-0.010	0.001	0.006	0.004
31,806			(0.003)	(0.004)	(0.006)	(0.009)	(0.005)	(0.006)	(0.005)	(0.004)	(0.004)
1975 cohort		-0.002	-0.006	0.002	0.000	-0.001	-0.003	0.004	-0.002	0.000	-0.002
31,794		(0.005)	(0.005)	(0.004)	(0.007)	(0.006)	(0.010)	(0.004)	(0.005)	(0.005)	(0.004)
1976 cohort	-0.002	-0.003	-0.001	0.002	-0.010	0.002	0.015	0.003	0.004	0.006	-0.001
32,776	(0.002)	(0.003)	(0.003)	(0.006)	(0.006)	(0.005)	(0.005)	(0.010)	(0.006)	(0.005)	(0.007)
1977 cohort	0.000	-0.001	0.000	0.005	-0.001	-0.007	-0.002	0.009	0.008	0.004	
33,298	(0.001)	(0.003)	(0.004)	(0.003)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.005)	
1978 cohort	0.001	-0.002	-0.004	0.004	0.010	-0.003	-0.002	-0.005	0.006		
35,035	(0.001)	(0.002)	(0.003)	(0.004)	(0.005)	(0.008)	(0.005)	(0.007)	(0.004)		
1979 cohort	0.001	0.000	-0.003	-0.002	0.009	-0.008	-0.001	0.000			
37,521	(0.001)	(0.002)	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)			
1980 cohort	0.001	0.001	-0.002	0.004	-0.006	0.000	-0.013				
39,878	(0.001)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)	(0.005)				
1981 cohort	-0.001	-0.003	0.002	-0.004	0.001	-0.003					
41,689	(0.001)	(0.001)	(0.003)	(0.003)	(0.005)	(0.009)					
1982 cohort	0.001	-0.003	-0.001	0.001	-0.004						
43,526	(0.001)	(0.002)	(0.002)	(0.004)	(0.003)						
1983 cohort	-0.001	-0.003	-0.002	-0.002							
42,507	(0.002)	(0.002)	(0.002)	(0.004)							
1984 cohort	0.000	-0.002	-0.002								
44,303	(0.001)	(0.002)	(0.002)								
1985 cohort	0.000	-0.002									
44,143	(0.001)	(0.002)									
1986 cohort	0.000										
43,854	(0.001)										
Test of no effect	7.77	14.72	7.16	9.33	13.22	10.54	17.95	10.73	6.41	18.88	5.92
p-value, no effect	[0.73]	[0.20]	[0.79]	[0.59]	[0.28]	[0.48]	[0.08]	[0.47]	[0.78]	[0.03]	[0.66]
Pooled Estimates	-0.0001	-0.0018	-0.0017	0.0012	-0.0003	-0.0036	-0.0025	-0.0019	0.0027	0.0030	0.0001
Standard Errors	(0.0003)	(0.0008)	(0.0009)	(0.0012)	(0.0017)	(0.0021)	(0.0023)	(0.0023)	(0.0021)	(0.0020)	(0.0018)
p-value, cohort restrictions	[0.67]	[0.98]	[0.96]	[0.64]	[0.22]	[0.65]	[0.08]	[0.39]	[0.93]	[0.07]	[0.55]
Means	{0.0008}	{0.0044}	{0.0117}	{0.0224}	{0.0320}	{0.0380}	{0.0390}	{0.0332}	{0.0253}	{0.0193}	{0.0148}

Web Appendix Table 1D. Discontinuity in Fraction Giving Birth at Specific Ages, by Cohort: Texas, Ages 24 through 30

	24	25	26	27	28	29	30	Test of No Effect [p-value]	Observed at 23 or Younger	Observed at Any Age
1969 cohort	-0.001	-0.003	-0.002	0.005	-0.001	-0.002	0.001	3.87	-0.008	-0.009
32,522	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	[0.97]	(0.016)	(0.038)
1970 cohort	0.005	0.002	0.000	-0.001	-0.003	-0.003		5.93	-0.017	-0.018
33,938	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)		[0.88]	(0.031)	(0.043)
1971 cohort	0.009	-0.006	0.004	-0.005	-0.002			18.61	-0.007	-0.009
33,258	(0.005)	(0.004)	(0.005)	(0.009)	(0.006)			[0.07]	(0.008)	(0.022)
1972 cohort	-0.003	-0.006	0.002	-0.001				9.74	-0.008	-0.023
31,286	(0.005)	(0.004)	(0.008)	(0.004)				[0.55]	(0.044)	(0.047)
1973 cohort	-0.001	-0.005	-0.002					9.29	0.010	-0.006
30,924	(0.003)	(0.007)	(0.004)					[0.60]	(0.038)	(0.040)
1974 cohort	-0.003	-0.001						9.24	-0.005	-0.010
31,806	(0.004)	(0.003)						[0.60]	(0.035)	(0.042)
1975 cohort	-0.010							16.32	-0.011	-0.017
31,794	(0.003)							[0.13]	(0.034)	(0.037)
1976 cohort								14.72	0.016	0.019
32,776								[0.20]	(0.024)	(0.030)
1977 cohort								9.27	0.021	0.014
33,298								[0.51]	(0.031)	(0.033)
1978 cohort								11.24	0.000	0.000
35,035								[0.26]	(0.021)	(0.021)
1979 cohort								9.28	0.001	0.001
37,521								[0.32]	(0.021)	(0.021)
1980 cohort								12.70	-0.025	-0.025
39,878								[0.08]	(0.014)	(0.014)
1981 cohort								10.66	-0.011	-0.011
41,689								[0.10]	(0.024)	(0.024)
1982 cohort								5.55	-0.020	-0.020
43,526								[0.35]	(0.009)	(0.009)
1983 cohort								3.50	-0.003	-0.003
42,507								[0.48]	(0.008)	(0.008)
1984 cohort								1.90	-0.005	-0.005
44,303								[0.59]	(0.005)	(0.005)
1985 cohort								1.35	-0.005	-0.005
44,143								[0.51]	(0.008)	(0.008)
1986 cohort								0.11	0.000	0.000
43,854								[0.74]	(0.002)	(0.002)
Test of no effect	5.92	18.91	6.14	1.37	1.76	1.20	1.14	**153.29**	12.85	11.90
p-value, no effect	[0.66]	[0.01]	[0.41]	[0.93]	[0.78]	[0.75]	[0.57]	**[0.26]**	[0.80]	[0.99]
Pooled Estimates	-0.0005	-0.0030	0.0005	-0.0007	-0.0021	-0.0024	0.0015		-0.0048	-0.0072
Standard Errors	(0.0015)	(0.0019)	(0.0023)	(0.0028)	(0.0026)	(0.0025)	(0.0038)		(0.0051)	(0.0060)
p-value, cohort restrictions	[0.01]	[0.65]	[0.85]	[0.63]	[0.92]	[0.74]	NA		[0.88]	[0.99]
Means	{0.0120}	{0.0102}	{0.0083}	{0.0066}	{0.0051}	{0.0035}	{0.0022}		{0.2411}	{0.2903}

Web Appendix Table 2. Details on Bandwidth Selection

	California							Texas						
	Imbens-Lemieux	Rule-of-Thumb	Percent increase in RMSE compared to IL choice					Imbens-Lemieux	Rule-of-Thumb	Percent increase in RMSE compared to IL choice				
			ROT	30	40	50	60			ROT	30	40	50	60
Fraction Observed	180	73	0.4	4.0	2.2	1.3	0.8	56	57	0.0	2.3	0.8	0.1	0.1
Maternal Age	142	91	0.3	4.6	2.8	2.0	1.6	126	70	0.4	3.4	2.4	1.3	0.8
Maternal Education	92	95	0.0	3.1	1.8	1.2	0.7	180	68	1.2	4.3	3.1	2.3	1.5
Low Birthweight	180	90	0.7	5.1	3.5	2.4	1.6	180	81	0.7	4.6	3.2	2.3	1.7
Prematurity	180	80	0.8	4.8	3.4	2.3	1.6	180	89	0.6	5.1	3.5	2.4	1.6
Infant Death								180	82	0.7	4.5	2.8	1.8	1.3
Mother Smokes	166	62	0.7	4.0	2.3	1.3	0.8	180	135	0.2	5.5	3.8	2.7	1.8
Mother Drinks								180	69	0.5	3.7	1.9	1.0	0.7
Mother Has STDs	180	73	0.5	3.4	1.6	1.0	0.7	180	66	1.1	4.5	2.8	1.9	1.3
Any Care	180	92	0.6	4.5	2.9	1.9	1.5	180	105	0.6	4.2	2.6	1.8	1.4
Care in First Trimester	180	71	0.9	4.7	2.9	1.9	1.3	180	74	0.7	4.0	2.5	1.4	0.9
Number of Visits	180	64	1.4	3.4	2.2	1.7	1.4	180	77	1.1	5.6	3.6	2.5	1.7
Father Present	180	122	0.4	3.5	2.6	2.0	1.7	156	129	0.1	3.3	2.1	1.7	1.2
Father's Age	180	86	0.6	3.5	2.5	1.9	1.6	180	74	1.2	5.1	3.5	2.4	1.8
Father's Education	180	69	0.6	3.3	1.9	1.3	0.9	180	75	0.9	5.0	3.4	2.5	1.6

Notes: First 2 columns present the results of different model selection methods. The Imbens-Lemieux ("IL") method estimates the RMSE directly and chooses the bandwidth that minimizes the estimated RMSE. The Rule-of-Thumb ("ROT") method is based on global polynomial approximations. The final 5 columns present the log difference between the estimated RMSE at the minimizer (IL) and RMSE at the stated bandwidth. See text for details.

Web Appendix Table 3. Tests of Overidentification: Continuity of Baseline Characteristics

California

Maternal Characteristics				
Hispanic	Black	Black*	Low Birthweight*	First Month Prenatal Care*
-0.0031	-0.0025	-0.0017	-0.0012	-0.0036
(0.0069)	(0.0044)	(0.0025)	(0.0023)	(0.0160)
[0.25]	[0.52]	[0.96]	[0.55]	[0.65]
{0.42}	{0.13}	{0.10}	{0.07}	{2.76}
214,608	214,608	576,421	575,213	576,421

Grandparental Characteristics*				
			Age at Childbirth	
Native Born	Parity	Child Mortality	Mother	Father
0.0049	0.0188	0.0003	0.0255	0.0607
(0.0044)	(0.0127)	(0.0004)	(0.0546)	(0.0534)
[0.14]	[0.22]	[0.59]	[0.62]	[0.87]
{0.70}	{2.08}	{0.01}	{25.68}	{28.60}
513,213	575,379	573,466	576,421	558,994

Texas

Maternal Characteristics				
Hispanic	Black	Black*	Low Birthweight*	First Month Prenatal Care*
0.0051	-0.0020	0.0030	-0.0001	-0.0300
(0.0077)	(0.0056)	(0.0035)	(0.0023)	(0.0188)
[0.93]	[0.68]	[0.77]	[0.71]	[0.91]
{0.39}	{0.18}	{0.15}	{0.08}	{2.95}
188,692	188,692	551,294	550,760	551,294

Grandparental Characteristics*				
			Age at Childbirth	
Native Born	Parity	Child Mortality	Mother	Father
0.0023	0.0015	0.0003	-0.0248	-0.0481
(0.0040)	(0.0109)	(0.0004)	(0.0635)	(0.0664)
[0.99]	[0.57]	[0.08]	[1.00]	[0.96]
{0.85}	{2.15}	{0.01}	{24.61}	{27.86}
502,761	551,074	551,069	551,294	475,450

Notes: Standard errors in parentheses. P-values for null hypothesis that discontinuity equal across cohorts in brackets beneath standard errors. Sample means in braces below p-values. Sample sizes below sample means. Stars indicate that estimates are based on public-use files.

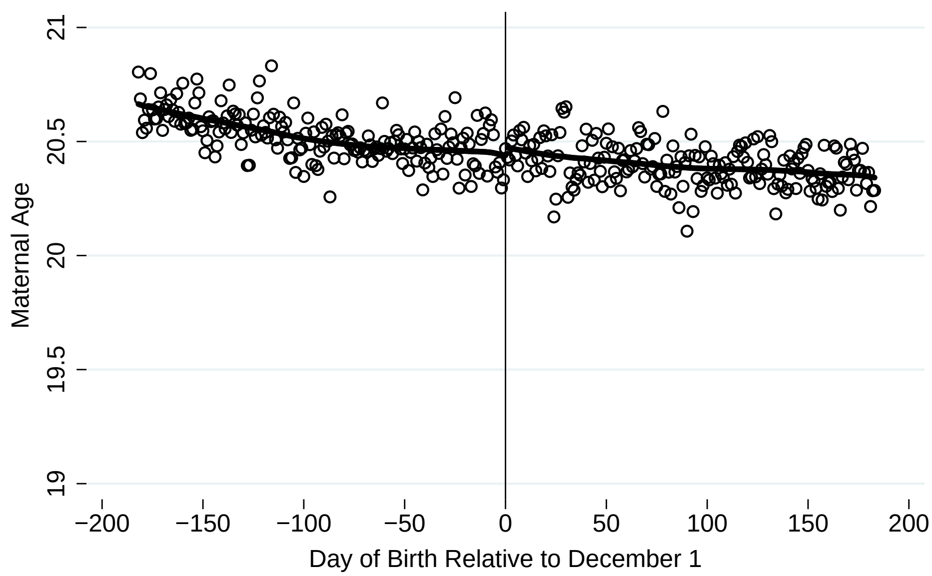
Web Appendix Table 4. Power Calculations

Outcome	Row	Currie and Moretti	Estimate (Std. Err.)	Presumptive Point Estimate (θ_N)	Point Hypothesis (θ_0)	Needed Increase in Sample Size: Fixed Bandwidth Approach	Needed Increase in Sample Size: Shrinking Bandwidth Approach
Maternal Smoking	1	-0.06	-0.016	-0.016	-0.060	Rejected	Rejected
	2		(0.008)		-0.030	16%	21%
	3				-0.010	660%	1162%
	4				0	Rejected	Rejected
Prenatal Care in 1st Trimester	5	0.02	0.031	0.031	0	29%	37%
	6		(0.018)		0.020	910%	1702%
	7				0.040	1471%	3027%
	8				0.060	49%	65%
Low Birth Weight	9	-0.01	0.014	0.014	-0.010	Rejected	Rejected
	10		(0.010)		-0.005	Rejected	Rejected
	11				-0.001	50%	65%
	12				0	71%	96%
	13			0	-0.010	247%	373%
	14				-0.005	1287%	2577%
	15				-0.001	34577%	149540%
	16				0	NA	NA
Prematurity	17	-0.01	0.009	0.009	-0.020	Rejected	Rejected
	18		(0.012)		-0.010	52%	68%
	19				-0.005	175%	255%
	20				0	548%	933%
	21			0	-0.020	42%	55%
	22				-0.010	469%	779%
	23				-0.005	2177%	4875%
	24				0	NA	NA
Infant Death	25	NA	-0.006	-0.006	-0.010	294%	455%
	26		(0.004)		-0.005	22653%	88270%
	27				-0.001	266%	407%
	28				0	147%	209%
	29			0	-0.010	Rejected	Rejected
	30				-0.005	208%	307%
	31				-0.001	7589%	22667%
	32				0	NA	NA

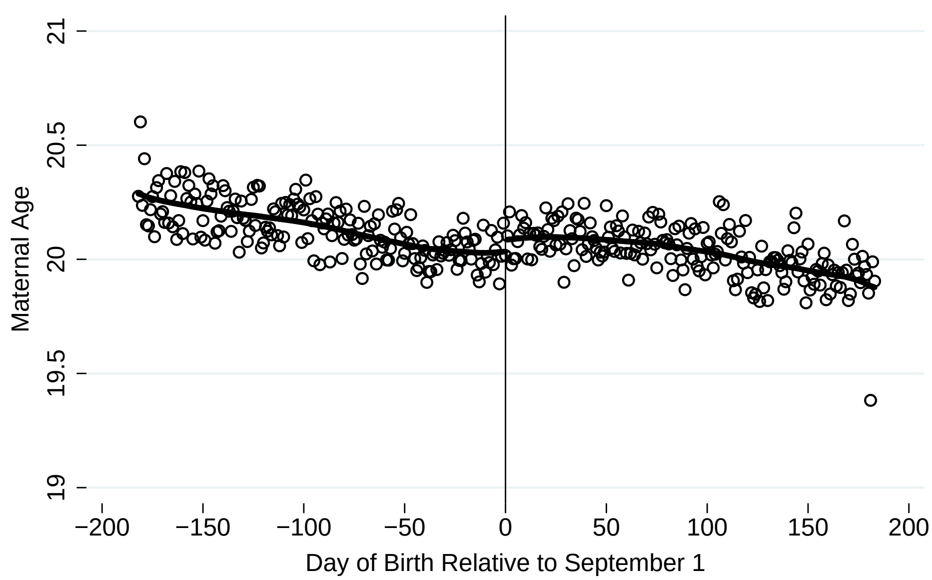
Notes: Table presents estimated percent increases in sample size necessary to reject specified hypotheses, under various assumptions on the point estimate that would be obtained in a larger sample. For details on calculations, see text.

Web Appendix Figure 1. Age at First Birth

A. California

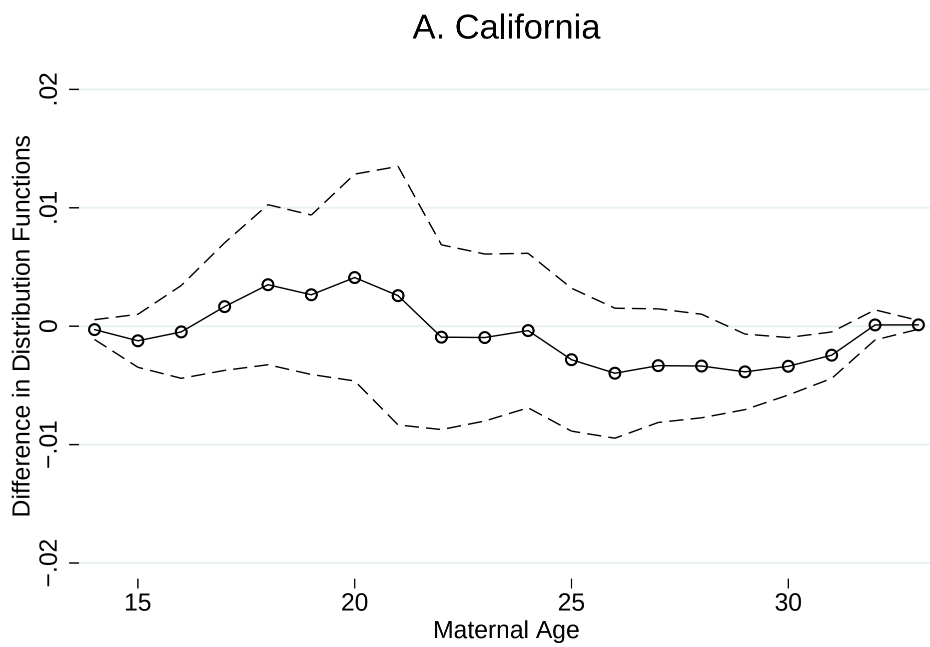


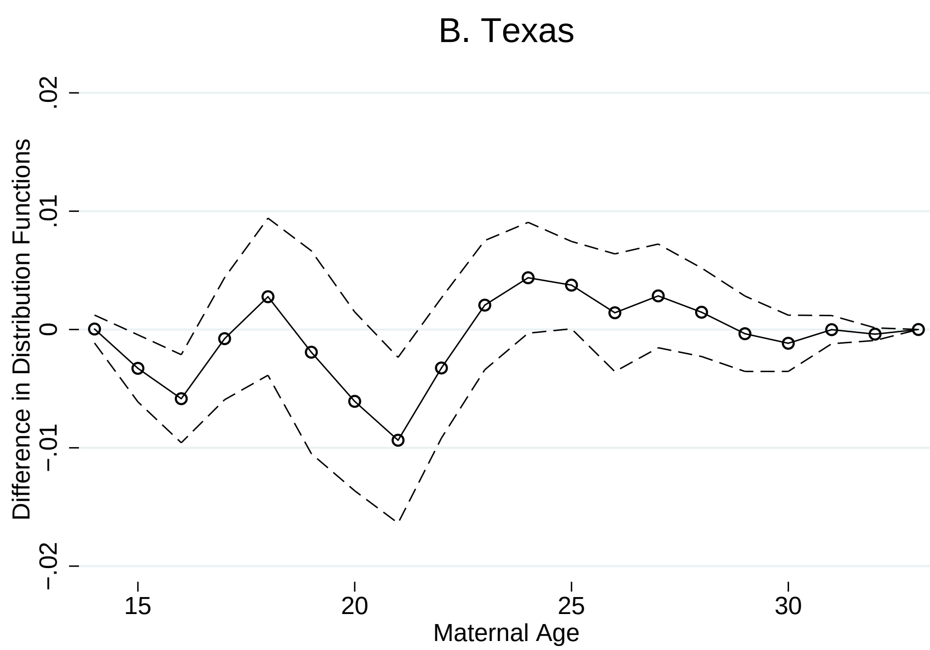
B. Texas



Note: Open circles are unconditional averages. Solid curve is a local linear smoother ($h = 50$). Estimates based on post-1969 cohorts. See text for details.

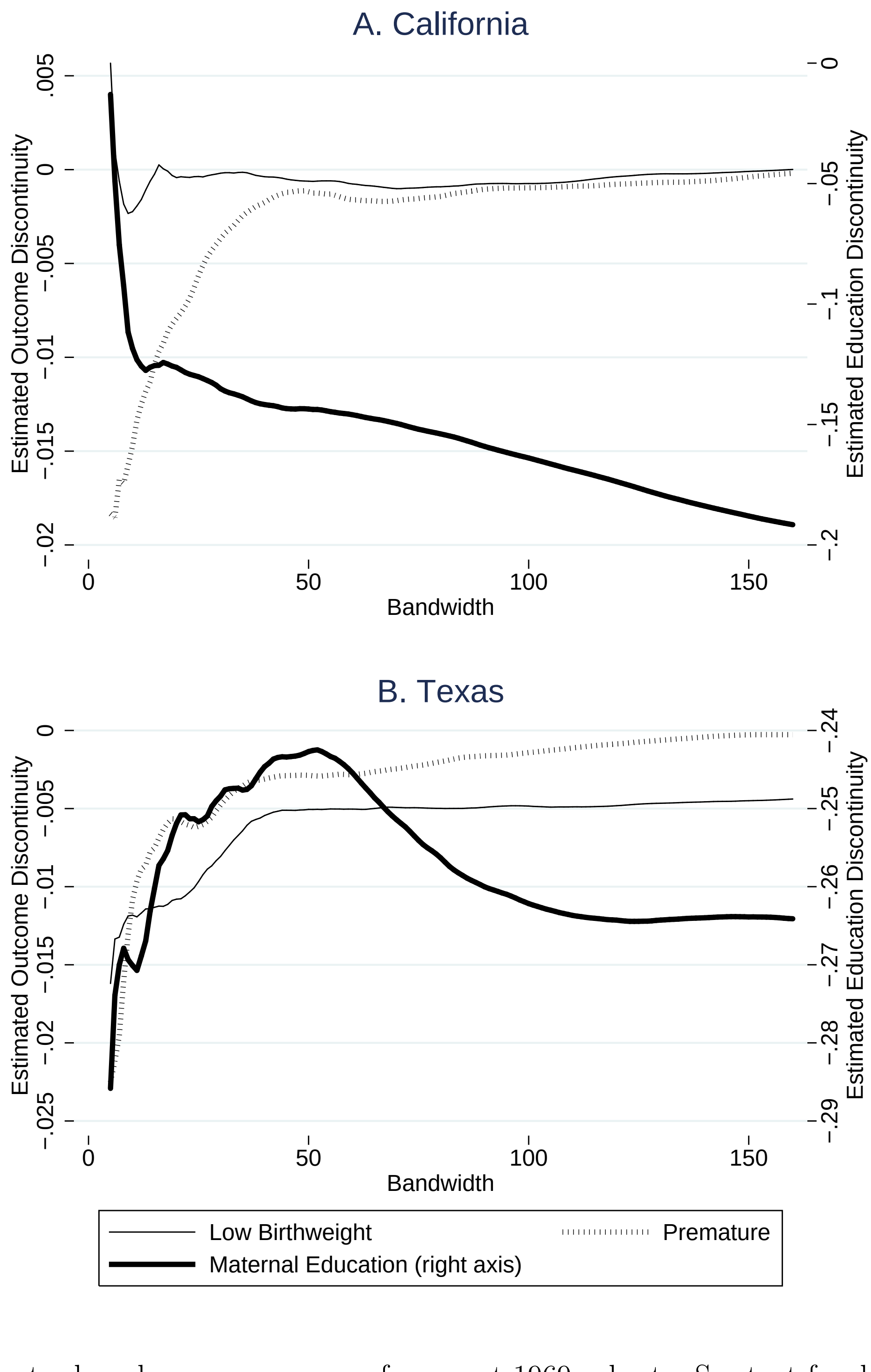
Web Appendix Figure 2. Distribution of Age Effects





Note: Open circles represent differences in distribution functions for age for those born before and after the school entry date. Estimates based on post-1969 cohorts. Dashed lines indicate pointwise confidence regions. See text for details.

Web Appendix Figure 3. Selected Reduced Form Discontinuities by Bandwidth



Note: Estimates based on young women from post-1969 cohorts. See text for details.

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