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Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings and Longitudinal Data

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Abstract

We estimate the effect of maternal education on birth outcomes using data from the Vital Statistics Natality files for 1970 to 1999. We also assess the importance of four potential channels through which maternal education may improve birth outcomes: use of prenatal care, smoking behavior, marriage, and fertility. In an effort to account for unobserved characteristics of women that could induce spurious correlation, we pursue two distinct empirical strategies. First, we construct panel data by linking women in different years of the Vital Statistics records and examine the effects of changes in education on changes in birth outcomes.

Second, we have compiled a new data set on openings of two and four year colleges between 1940 and 1990. We use data about the availability of colleges in the woman's county in her 17th year as an instrument for maternal education. Several specification tests lend credibility to the instrument. For example, the opening of four year colleges increases four year graduation rates, but does not affect the probability of having some college, while the opening of two year colleges increases the probability of having some college but has a much smaller effect on four-year graduation rates. Also, while the opening of a college in the mother's 17th year increases her educational attainment, openings in her 25th or later years do not. Furthermore, when male-only colleges become coed, maternal education increases, but when female-only colleges become coed, we find no effect on women's education.

Our findings using the two approaches are similar. Higher maternal education improves infant health, as measured by birthweight and gestational age. It also increases the probability that a new mother is married, reduces parity, increases use of prenatal care, and reduces smoking, suggesting that these are important pathways for the ultimate effect on health.

Introduction

Over the past half century, American women have experienced tremendous increases in average educational attainment. Among first-time mothers, average education increased from 12.1 in 1969 to 13.2 in 1999. Returns to education are generally quantified in terms of increases in wages. However, in addition to earning more, well educated women marry better, and may use any given set of health inputs more efficiently, by for example, reducing unhealthy behaviors such as smoking. If higher maternal education does indeed improve child health outcomes, then conventional estimates of the returns to college, which focus only on wages, understate the social benefits.[1] Moreover, to the extent that healthier children go on to be more productive and more highly educated adults themselves, there will be an important inter-generational spillover that analyses of wage effects alone will not capture.

On the other hand, college educated women tend to have fewer children (and sometimes none at all), and some researchers have suggested that by increasing the opportunity cost of a woman's time, gains in women's education above some level might negatively affect some measures of child well-being. Thus, the extent to which gains in maternal education have translated into improved child quality is an open question.

This paper makes two contributions. We estimate the effect of maternal education on birth outcomes using data from individual birth certificates from the Vital Statistics Natality files for 1970 to 1999. We also assess the importance of four potential channels through which maternal education may improve birth outcomes. First, we estimate the effect of education on the use of prenatal care. Because more educated women earn more, they may be able to afford more or better health care. Second, more educated women are also likely to marry higher earning men, which will further raise family income. We estimate to what extent an increase in a woman's education raises the probability of marrying a highly educated man. Third, education may induce women to have healthier behaviors. In particular we analyze the effect of education on the probability of smoking during pregnancy. Finally, we look at the effect of education on fertility. As Becker's quality/quantity trade-

[1] Lleras-Muney (2001) makes this point in a somewhat different context, showing that there is a return to education in the form of longer life-expectancy among adults.

off suggests, education may induce women to have fewer children of higher quality.

Infants of college educated women have better outcomes, measured in terms of both birth weight and the incidence of prematurity. But it is not obvious that this relationship is causal since better educated women differ in many ways from less educated ones. In an effort to isolate the causal effect of education, we pursue two distinct empirical strategies. First, we construct panel data by linking women in different years of the Vital Statistics and examine the effects of *changes* in education on *changes* in birth outcomes. These longitudinal models control for permanent unobserved factors such as genetic endowment, family background, ability, and discount rates. However, they are not necessarily robust to the presence of transitory shocks that may be correlated with the decision to go back to school after the first birth.

Second, we turn to an instrumental variable strategy. Following the literature that uses institutional features of the college market as instruments for educational attainment, we have compiled a new data set on openings of two and four year colleges between 1940 and 1990. We use this data about the availability of colleges at the county level as an instrument for maternal education in models of birth outcomes and potential mediating factors such as prenatal care, smoking behavior, marriage, and fertility. Our instrumental variables (IV) models control for many potentially unobserved confounding factors by including county-year fixed effects, so that our estimates are identified by differences in the availability of educational services among different cohorts of women delivering in the same county and year.[2]

One could argue that colleges tend to open in counties where residents' education is already increasing or is expected to increase, and therefore are not a cause but an effect of increasing education. Although we cannot completely rule out this possibility, we provide six pieces of evidence which suggest that college openings do in fact increase educational attainments. First, we find that maternal education and birth outcomes have no clear trend in the ten years before the opening of a college, but improve afterwards. Second, while the opening of a college in the mother's 17th year increases her educational attainment, openings in her 25th or later years do not. Third, the

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[2] We focus on white women. The instrumental variable strategy does not work as well for black women. Black women do not appear to be as strongly affected by new college openings as white women, presumably because of segregation in the first part of the period under consideration. We hope to explore the relationship between education and birth outcomes among black women in future work.

opening of four year colleges increases four year graduation rates, but does not affect the probability of having some college, while the opening of two year colleges increases the probability of having some college but has a much smaller effect on four-year graduation rates. Fourth, when male-only colleges become coed, maternal education increases, but when female-only colleges become coed, we find no effect on women's education. Finally, the opening of public colleges has a larger effect than the opening of private colleges, and college openings matter more in relatively "college poor" counties.

Although the instrumental variable and longitudinal model estimates are based on completely different samples and identifying assumptions, our findings are remarkably similar using both methods. Higher maternal education improves child quality, as measured by birth weight and gestational age. It also increases the probability that a new mother is married, is associated with higher husband education, reduces parity, increases use of prenatal care, and reduces smoking, suggesting that these are all important pathways for the ultimate effect on health. As one might expect, the estimated effects are larger for less educated women, but remain substantial even among college graduates.

Panel data estimates are similar to OLS, while instrumental variables coefficients are larger than OLS coefficients. This latter comparison is consistent either with a larger marginal benefit of schooling for those women whose education is affected by local college openings, or with county-level spillovers in the effects of education between women. Our estimates provide some support for both hypotheses.

The rest of the paper is laid out as follows. Section 2 provides essential background, while Section 3 lays out a simple model showing the inter-relationships between the outcomes we examine. Section 4 discusses the data and Section 5 describes our empirical strategy. Results appear in Section 6 and 7, which are followed by the conclusion.

2. Background

Our work builds on a large literature examining the role of maternal education in the "production" of child quality, as well as the effect of female education on marriage and fertility. In addition, to the extent that better child health is viewed as a "return" to maternal education, our work is in the tradition of research measuring the returns to college using changes in the accessibility of college as an instrument. This section briefly reviews key findings from these literatures.

a) Maternal Education and Child Quality

Many studies report a correlation between maternal education and measures of child health. In fact, this robust relationship was one factor underlying the World Bank's drive over the past decade to promote maternal education in developing countries (World Bank, 1993). Much of the existing literature differs from this paper in that it focuses on developing countries, emphasizes the effects of improvements in relatively low levels of education, and does not attempt to establish whether the effect of maternal education is causal.[3]

There is little work exploring the relationship between maternal education and child health in developed countries. There is however, some work exploring the effects of maternal education on children's test scores and schooling attainment in the U.S. Rosenzweig and Wolpin (1994) use a sample of siblings to examine the effect of maternal schooling on children's achievement test scores. They find that mothers who continued their schooling between births increased the test scores of the younger siblings without harming the older ones. We will pursue a similar strategy using the panel data set we have constructed.

On the other hand, Behrman and Rosenzweig (2002) conclude that much of the positive cross-sectional relationship between the schooling of mothers and children is due to heritable ability as well as assortative mating. They note that increased schooling of mothers reduces the amount of time mothers spend in the home (i.e. time inputs into child production) which could well have negative effects on child outcomes. In keeping with this hypothesis, studies that have directly examined the effect of maternal employment on measures of child well-being often find some evidence of negative effects, though the results are inconsistent across gender and age groups and/or outcome measures, and it is difficult to control for the endogeneity of maternal employment.[4]

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3 Two papers that do address the question of causality are Desai and Alva (1998) and Thomas, Strauss and Henriques (1991). The first suggests that much of the return to female education may be through higher father's earnings, while the second finds that female education is associated with factors such as reading newspapers, and thus seems to have a direct effect on the ability to gather information about health.

4 See Blau and Grossberg, 1992; Desai, Chase-Lansdale, and Michael, 1989; Neidell, 2000; Parcel and Menaghan, 1994; Ruhm, 2000; Waldfogel, Han, and Brooks-Gunn, 2002.

If maternal education does affect child health, then this may have significant intergenerational effects. Poor child health affects adult health, and may also have indirect effects through reductions in cognitive functioning and/or schooling. There are many studies linking low birth weight to future cognitive deficits, for example (see Currie, 2000 for a review of some of this literature). Currie and Hyson (1999) show using data from the 1958 British cohort study that low birthweight also has long-term negative impacts on test scores, employment probabilities, and wages among young adults.

Thus, the existing literature suggests that increases in even high levels of maternal education might have a positive effect on infant health by improving maternal behaviors that are linked to poor infant health outcomes. On the other hand, increases in the value of maternal time with education may militate against time-consuming investments in infant health. The literature also highlights the dangers in interpreting any observed correlations between maternal education and child health inputs or outputs as causal. Hence, the relationship between increases in college attendance among American women and infant health outcomes remains an open question.

b) Education, Assortative Mating, and Fertility

Education may affect child outcomes either directly or indirectly through its effects on the choice of father. Becker (1981, 1985) predicted that more educated men would be likely to marry less educated women because of gains to specialization in "household production".[5] However, the empirical evidence suggests the opposite: Highly educated men and women tend to marry each other. Mare (1991) examines data from five decades and concludes that the tendency towards "positive assortative mating" became stronger between the 1930s and the 1970s, and then remained fairly constant during the 1980s. Much of the effect seems to be due to the fact that college educated people find potential spouses among their college classmates.

Goldin (1992, 1995) examines the implications of positive assortative mating on a woman's "returns" to higher education. She finds that for women, much of the return to college may come in the form of a higher earning spouse. For the cohort that graduated between 1945 and 1960, roughly half of the gain to household income was through this channel. Moreover, of women who pursued a career subsequent to college, some remained childless, suggesting that in our sample of women

5 Since Becker, several authors have developed theoretical explanations for positive assortative mating. See Lam (1988) and Oppenheimer (1988).

bearing children, a large part of the return to college was through enhancements in spouses' earnings rather than increases in their own earnings.[6]

Education may also affect child outcomes indirectly through its effects on fertility. Becker and Lewis's (1973) discussion of tradeoffs between the "quantity" and "quality" of children desired is often given as an explanation of the negative relationship between women's education and their fertility. The model also implies that increases in education will be associated with improvements in child quality.

In summary, there is surprisingly little evidence available about whether increases in maternal education in a country like the United States have a causal effect on infant health outcomes. The literature suggests that in addition to any direct effects of education on child health production, it is important to consider indirect effects through assortative mating and fertility decisions.

c) College Availability and Estimates of the Returns to College

Given that we examine both two and four-year college openings, studies by Kane and Rouse (1995) and Rouse (1995) are particularly relevant. Kane and Rouse were the first to demonstrate that there was indeed a labor market return to attendance at two-year colleges. Rouse shows that access to two year colleges increases overall educational attainment, though it does not affect the probability of attaining a four year degree. She also finds that increasing access to two-year colleges tends to divert some students from four-year to two-year colleges. Our results on the effects of two-year relative to four-year colleges on women educational attainment are consistent with Rouse's findings.

Card (1995) uses college proximity as an instrumental variable to estimate returns to schooling. He shows that instrumental variables estimates are larger than OLS estimates, which is due to there being high returns to education among those most constrained by lack of college availability. We return to this point below.

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[6] Were we to use these data to study maternal health, selection induced by education would be a significant problem. However, we have the whole universe of infants born. Even if college education causes some women not to bear children, it may still have a positive effect on the health of those infants who are born.

3. A Simple Model of the Effects of Maternal Schooling on Family Structure, Health Inputs, and Child Health Outcomes.

The model in this section shows the relationships between the different outcomes we will examine. It is based on the idea that women "produce" child health outcomes by combining inputs in ways that are circumscribed by the available production technology.[7] Women are assumed to derive utility from child "quality", as well as from their own consumption and leisure:

(1) $U = U(Q, C, L; X, u)$,

where Q is the stock of child quality, C is consumption of other goods, L is leisure, X is a vector of individual characteristics, and u is a vector of neighborhood characteristics that may affect tastes such as average family size in the area. Women's utility is increasing in all three arguments. $Q = f(N,q)$ where N is the number of children and q is the quality per child. It is not important that any particular functional form be assumed. Rather, as Becker and Lewis (1973) point out, what is important is that "an increase in quality is more expensive if there are more children because the increase has to apply to more units; similarly, an increase in quantity is more expensive if the children are of higher quality, because higher-quality children cost more" (page S280).

Utility is maximized subject to the following set of constraints:

(2) $q = q(G, V; S, X, v, e)$,

where G represents goods invested in child quality, V represents time invested in producing child quality, S is the mother's schooling, v is a vector of neighborhood characteristics such as medical services that may affect child quality, and e is a random shock. Equation (2) can be thought of as a production function for child quality. Education is expected to improve the productivity of mothers. That is, the more educated mother is assumed to be able to produce a higher "quality" child with a given budget of goods and time.

Mothers also face the following set of constraints:

(3) $C = Y - P^*G$,

where P are exogenously determined prices,

[7] The model derives from Grossman's (1970, 1999) model of health as human capital. For early examples of its application to infant health see Rosenzweig and Schultz (1982, 1983, 1988).

(4) $Y = Y_f + w*H$,

where Y_f is the amount of income that the woman receives from the father of her child,

(5) $w = w(S; z)$,

where z is a vector of local labor market conditions, $w'(S)>0$, and

(6) $L + N*V + H + M = 1$,

where M is the time cost of being married (if a woman is married) and the amount of time available is arbitrarily normalized to one. If a woman is married, then M>0 and $Y_f=Y_f(S)$ where $Y'(S)>0$. That is, we assume that a woman can find a better quality husband if she is more educated. If she is not married, then M=0 and $Y_f=0$. In order to solve this problem, the woman must determine whether her maximum utility will be higher in the married or unmarried state.

The mother's choice variables are S, M, N, G, V, and H. We will observe analogues of all of these measures except H in our data. S and M are directly observed, and while we do not know the completed number of children, we can observe the birth order of each child in our data to see whether, on average, less educated women are having births of higher parity, and whether more educated women are delaying first births. G and V are proxied by the whether or not prenatal care began in the first trimester of pregnancy, which can be viewed as an indicator of the mother's willingness to invest in the unborn child and also by whether or not the mother smokes.

It is interesting to compare the effects of these two health inputs. Prenatal care is costly so that increases in prenatal care with education might reflect a relaxation of the budget constraint that occurs with increased wealth. However, refraining from smoking saves money and time so that reductions in smoking are unlikely to be accounted for by increases in wealth alone, and may reflect an effect of education via the production function. Moreover, smoking is the leading cause of low birthweight in the United States, so it is an interesting outcome to examine in its own right: Lightwood, Phibbs, and Glantz (1999) estimate that up to a quarter of low birthweight infants born in the 1980s were adversely affected by maternal smoking while pregnant.

We can now consider the effect of reducing the cost of obtaining a college education on each of these outcomes. Other things being equal, a reduction in cost should increase the quantity of education, S, consumed. The increase in schooling increases the expected wage, increasing the opportunity cost of time spent in non-work activities such as child rearing and marriage-building.

On the other hand, schooling increases the payoff to investing in marriage by increasing the value of the spouse that can be attracted. These considerations suggest that the effect of education on marriage rates, M, is theoretically ambiguous while the effect on the total amount of time devoted to child-raising, N*V, is negative.

Child-raising time can be reduced either by having fewer children, or by devoting less time to each child, or both. However, decreases in V must be offset with increases in G if a given level of q is to be maintained. Of course, to the extent that better educated women are able to combine G and V more efficiently in the production of q, quality may rise even with G and V constant. Age at first birth is not an explicit choice variable in this static model, but predictions can be obtained by assuming that women must complete their educations before they begin child raising. In this case, the reduction in the cost of schooling will increase schooling and postpone child rearing.

The model highlights the many pathways through which increased maternal schooling can affect our measures of child quality, q, which are birthweight and gestational age. [8] Increased schooling will influence the probability of marriage, family income and family size, which will in turn influence the choice of inputs into child development. In addition, education may change the production function, allowing mothers to combine a given set of time and money inputs into outputs more efficiently.

The model sketched above can be solved to yield conditional demand functions for our indicators of M, N, G, V, and for our measures of child quality, q, of the following form:

(7) M, N, G, V, q = f(S, X, P, u, v, z, e)

where the demands/outputs are conditional on the endogenously chosen level of education, S, which in turn is assumed to depend on the availability of college. In most of our work, we will proxy the neighborhood-specific variables P, u, v and z using county-year fixed effects and mother-cohort trends. In the next section, we discuss the application of this model to our data.

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[8] We also examined the probability that infants were low birthweight conditional on being full-term, since the effect of inter-uterine growth retardation is different than that of preterm delivery, although both may result in low birthweight. The results were similar to those we report for low birth weight.

4. Data

a) Vital Statistics Data

Our primary source of data is individual-level Vital Statistics Natality records from 1970 to 1999. These data are drawn from birth certificates and are thought to cover virtually all births in the United States. Most states currently report the mother's county of residence, age, race, marital status and education, information about when she began prenatal care during pregnancy, whether she smoked during pregnancy, information about the fathers' education, and information about birth outcomes including birthweight and gestational age. One limitation of the Vital Statistics data is that the number of states reporting some types of information has increased over time, and thus we do not have a balanced panel of counties over time, and the number of counties is not the same for all our outcome measures. In the Data Appendix we describe which of the variables are not available in all years. The effect of this selection on our estimates is mitigated because we include either mother fixed effects or county*year dummies in all of our models.

Because there are well known differences in average outcomes by birth order, we focus most of our analyses either on first births (in the OLS and IV) or on changes between first and second births (in the panel). (The sole exception is when we examine parity, where we include births of all parities in the sample). Because our instrument is weak for blacks, (presumably because of segregation in the first part of the period under consideration) and the panel available is smaller, we focus on white mothers.

In this paper, we use two separate identification strategies: longitudinal models and instrumental variables. The samples used in the two strategies are different, although they are both drawn from the Vital Statistics files. We now describe the two samples. Our first set of analyses is based on a panel data set constructed from Vital Statistics records. The Vital Statistics system does not link mother's records between births, and we are not aware of previous attempts to create a panel from these data. However, it is possible to link first and second births with a fair degree of certainty for a subset of mothers. In particular, we use information about the mother's year and state of birth, and county of residence, as well as each child's birth year, birth month, and the interval between births (which was reported on the birth certificate until 1993, after which it was dropped as a result of budget cuts) in order to match first and second births. For this analysis, we use all women aged 16 to 45 at first birth, since we want to capture all women who experienced a change in educational attainments between the births. We include all exact one-to-one matches. The sample is therefore a panel of mothers with two children, observed at first and second birth. Because we are including only one-to-one matches, mothers from smaller counties are more likely to be in the sample.[9] See the Data Appendix for more details.

Summary statistics for all first births in the Vital Statistics sample are shown in column (1) of Table 1. Although we don't use this sample in our analysis, column (1) is reported for the sake of comparison with the panel data and IV samples. Column (2) shows first births from the panel of first and second births that we are able to match. The matched longitudinal sample is not perfectly representative of the population. The differences arise because the women we can match one-to-one are more likely to be from rural areas than the full sample of women, and because mothers who change county between births cannot be matched. The comparison of column (1) and (2) indicates that the mothers we can match are slightly less educated than the full sample, and are less likely to have low birth weight or pre-term babies even though they have lower income and are also less likely to get prenatal care in the first trimester. They are however, more likely to be married than the full sample of mothers. It is also a somewhat older sample, which is accounted for by the fact that birth intervals are only reported up until 1993. Not surprisingly, mothers in the matched sample are from counties that are 51% urban in the year when they are 17, compared to 71% in column (1).

Column (3) shows means for the second births in our panel. On average, women in the panel gained a half year of education between the first and second birth. These differences are explored further in Appendix Table A2, which shows that there are a substantial number of mothers who change education levels between the births. For example, 12.6% of the mothers add one year, 5.5% add 2 years, and 5.92% gain 3 or more years. There is a striking reduction in the probability of low birthweight or prematurity between first and second births, which provides a justification for our focus on first births in our subsequent analysis. Table 1 also shows that the probability of using prenatal care use in the first trimester and of being married increased between first and second births.

9 We have re-estimated all longitudinal models using a sample that includes *all* cells that can be matched, not only cells with size equal to one. Results did not change.

We now turn to the sample used to estimate the IV models. While in the longitudinal sample we need mothers of school age (since identification comes from mothers whose education changed between the first and second birth), in our instrumental variables strategy we focus on first-time mothers 24 to 45 years old in order to select women whose education is likely to have been completed. We take a 10% random sample of the full sample of women in this category for ease of estimation.[10] Column (4) of Table 1 shows means for this sample. Compared to the full sample of first-time mothers, these older mothers are more educated, less likely to have negative outcomes, more likely to get prenatal care in the first trimester, much more likely to be married, and less likely to smoke. They also have husbands who are more educated on average. However, the median income in their county in the year when they are 17 is more similar to the full sample of mothers than to that of the mothers in the panel, and the number of two and four year colleges is also comparable to the full sample.

The last four columns break out this sample of older mothers by education. In all cases, more education is associated with more desirable birth outcomes. The monotonic relationship between favorable birth outcomes and maternal education is very striking. These improvements in outcomes may come about through many channels, as illustrated by the monotonic relationship between education and early prenatal care, probability of smoking during pregnancy, probability of being married, husband education and fertility. Just to take one example, only 2% of college educated mothers smoke during pregnancy. The corresponding figure for mothers with some college, high school and less than high school are 8%, 17% and 34%. Not surprisingly, college educated women are also more likely to come from counties with a higher per capita number of four-year colleges in the year when they were 17.

The low percentage of high-school drop outs and the high percentage of college graduates in the IV sample reflect the fact that only first-time mothers 24 or older are included, and they tend to be more educated than the average. In Appendix Table A1 we show that the education distribution in the Vital Statistics data is consistent with the education distribution of first time mothers in the Census.

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10 Since our instrument varies at the county-cohort of birth level, the effective sample size for the instrumental variable estimator is the number of cohorts times the number of counties. Using only 10% of all births greatly reduces computation time but has no effect on the effective sample size.

b) Data on College Openings

Our IV analysis combines Vital Statistics natality data with a unique data set on openings of two and four-year colleges that we compiled for this research.11 The construction of this data set is described in the Data Appendix. Figure 1 shows the tremendous rise in the number of colleges between 1940 and 1996. In 1940, there were 346 two-year colleges in our sample and 1301 four-year colleges. By 1996, these figures had risen to 1436 and 1808 two and four-year colleges respectively. The pattern for two-year colleges is more "S-shaped" than that for four-year colleges reflecting the great expansion of the two-year college system during the 1960s and 70s.12

We use these data to construct measures of the availability of two and four-year colleges. Each measure is the number of colleges which existed in the woman's county when she was 17 years old, divided by the estimated number of 18 to 22 year olds in the county in that year (in thousands). This instrument takes into account the fact that cohort size is likely to have an impact on the availability of college given any fixed number of schools (c.f. Card and Lemieux, 2001; Welch, 1979).13 We construct separate measures for two and four-year colleges.

The most important limitation of our data is that we observe mothers' residences at the time of the birth (of the baby), rather than at the time when she was 17. Thus, we are forced to assume that her county of residence is the same at the birth as it was at 17. Because young women have high mobility rates, this assumption is potentially problematic. If mothers randomly change location between age 17 and the time of the birth, then we will tend to understate the extent to which college openings affect women's educational attainment in the first stage regressions. However, 17 year old women may move in order to attend college and then stay in the new location, or counties that experience college openings may be become more attractive to college educated women.

We cannot completely rule out these two possibilities. But we present two pieces of evidence that suggest that this type of mobility is not driving our results. First, if college educated women

11 The ideal instrumental variable would account not only for the number of colleges, but also for their size. Information on the availability of classroom space or capital expenditures is not available for part of the period under consideration. Information on enrollment is available, but enrollment reflects both the supply of college places and the demand for these places, and thus is not a valid instrument.

12 Virtually all of the two-year colleges in our sample are public institutions, while roughly half of four-year colleges are public.

13 The number of individuals 18-22 years old is from the Census. See Data Appendix. Note that measurement error in

tended to move to counties with college openings (for example, because of employment oppportunities), then we would see a correlation between college openings and college availability measured at ages greater than 17. We test for this possibility by examining the relationship between maternal education and college availability at 25, 30, 35 and 40. We find no evidence of a positive relationship between maternal education and college availability at older ages.

It is still possible that our results are driven by the migration of 17 or 18 year old women seeking education to counties with recent college openings. While highly selective institutions recruit students from across the country, most of the four year and virtually all of the two year colleges in our sample are not of this type. A typical new institution is more likely to resemble Coleman College (La Mesa, CA) or Aims Community College (Greeley, CO) than Harvard or Princeton. Although most of the schools in our sample are non-selective institutions that one would expect to enroll mainly local women, it is still possible that at least some of our results are explained by the endogenous mobility of college students. While we cannot completely rule out this possibility, we use Census data in an attempt to quantify the magnitude of the bias introduced by endogenous migration. In interpreting these findings, one should keep in mind that Census data are far from ideal, because they are available only for a limited number of years, and identify "PUMAs", not counties. See the Data Appendix for details.

One final caveat is needed. In the panel, we include women who have two births, who are more likely to be married than women in the full sample of 16-45 year old women (column 1). Similarly, in the IV sample, we include women who are 24 or older who, again, are more likely to be married. Therefore our results on marriage may be less generalizable than some of the other results.

5. Identification Strategies

In this paper we adopt two separate identification strategies to estimate the effect of schooling on child quality, and the channels through which schooling affect child quality. Although neither identification strategy is based on incontrovertible assumptions, the two strategies depend on very different assumptions. Therefore, we hope to shed some light on the validity of our identification assumptions by comparing estimates from the two strategies.

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our denominator should not carry over to our left hand side variable since they are measured in 2 different sources.

a) Longitudinal Models

Using panel data, it is straightforward to estimate models that control for *permanent* unobserved characteristics of mothers by estimating equations of the following form:

(1) $d\text{OUTCOME} = a_0 + a_1 * d\text{EDUC} + a_2\text{AGE}_1 + a_3\text{AGE}_2 + a_4\text{COHORT} + a_5\text{COUNTY} + \text{v}$,

where *d*OUTCOME is the change in one of our outcome measures, *d*EDUC is the change in the mother's years of education between the births, AGE_1 is a vector of dummy variables for the mother's age at the time of the first birth, AGE_2 is a vector of dummy variables for the mother's age at the time of the second birth, COHORT is a vector of indicators for the mother's decade of birth (e.g. 1920-29, 1930-39, etc.), and COUNTY is a vector of indicators for the mother's county. This specification of the age variables allows the effect of birth interval to differ by the mother's age at first birth. It is important to control for age, since the risk of poor infant outcomes as well as marriage and fertility decisions depend on age (very young mothers are less likely to be married, and both very young and very old mothers are more likely to have poor outcomes).

While cohort and county dummies would fall out of a straight fixed-effects model, model (1) allows the effects of changes in education to differ for different cohorts and counties. The cohort of the mother proxies for many factors such as attitudes towards education, marriage, female employment, and child bearing, while the county dummies capture changes in the availability of medical technology. We obtained very similar estimates with the more restrictive mother fixed effect model which did not include cohort and county effects.

The model in equation (1) is identified by comparing mothers whose education does not change with mothers whose education increases between the first and second birth. The model is based on the assumption that the only source of heterogeneity that affects outcomes is *permanent*. In particular, model (1) controls for differences in ability, genetic endowment, and family background. However, it is possible to think of cases where this assumption fails. The presence of *transitory* shocks that affect both education and birth outcomes could be problematic. For example, women who decide to go back to school after having the first child may be experiencing improvements in their economic conditions relative to women who decide not to go back to school. In this case, model (1) would overestimate the effect of schooling. For this reason we now turn to a different identification strategy, based on instrumental variables. Although the IV estimator is based on a different set of assumption, we will see that IV estimates are consistent with estimates obtained from longitudinal models.

b) Instrumental Variables Models

The first stage in our instrumental variables regressions take the form:

(2) EDUC = $b_0 + b_1$IV-2 + b_2IV-4 + b_3AGE + b_4COHORT + b_5COUNTY*YEARBRTH + b_6 INCOME17 + b_7 URBAN17 + u

where IV-2 and IV-4 denote our instrumental variables for two and four-year colleges; AGE is a vector of single year of maternal age dummies as in (1); COUNTY*YEARBRTH is a vector of indicators for the county and year of the child's birth; INCOME17 and URBAN17 control for the median income and the percent urban in the county when the woman was 17, and u is a random error term.

The county/year effects control for many characteristics of the local area that may affect outcomes, such as the availability and quality of medical services, the local business cycle, pollution, etc. Identification comes from the fact that *within each county and year of birth of the baby*, there are mothers who were 17 before a college opening, and mothers who were 17 after a college opening.

The income and urbanicity measures control for some of the factors likely to affect the educational attainment of the mother's cohort. In some of the models, we test the robustness of our results by including state-mother cohort trends, which allow the cohort effects to vary from state to state, and by including the average education of fathers in the county (of all ages) when the mother was 17 as a measure of demand for education (as discussed further below).

Our second stage regression models take the following form:

(3) OUTCOME = $c_0 + c_1$PEDUC + c_2AGE + c_3COHORT +c_4COUNTY*YEARBRTH + c_5 INCOME17 + c_6 URBAN17+ z,

where PEDUC is the predicted years of education from (2), and z is a random error. In all the models, the standard errors allow for potential correlations between the errors within county-year at 17 clusters.

This specification does not capture the part of the return to schooling that arises through residential location. More educated women can probably afford to live in areas with better hospitals and better environmental quality. By including county*year effects, we absorb the effect of these geographical factors on birth outcomes, and in this sense, we underestimate the benefit of schooling.

c) Validity of the Instrument and its Limitations

One important concern is that college openings do not occur randomly. It is possible that college openings reflect rising demand for education/human capital that affects both demand for education and outcomes. In this case, educational attainment would drive college openings, not vice-versa, and our estimates could overestimate the effect of schooling on birth outcomes.

On the other hand, it is also possible that state legislatures place colleges in a compensatory way, i.e. in under-served areas that have low college attendance. For example, legislation establishing the new University of California Merced campus begins: "The San Joaquin Valley is the most populous region of the state without a University of California campus, and has one of the lowest rates of college participation of all regions in California. Access to postsecondary education is determined, in significant measure, by a student's proximity to college campuses" (California State Education Code, Section 92160). Similarly, other state plans for higher education emphasize increasing access to education as a main goal of public investments.[14] These policy statements suggest that to the extent that there is any correlation, the location of new public colleges may be negatively rather than positively correlated with average academic attainments in the area. This would lead us to underestimate the effect of education on birth outcomes.

The location of private colleges may be governed by different considerations. On the one hand, it is possible that private colleges are located where demand is anticipated to grow. On the other hand, private college location could be affected by land prices, which would dictate location in less developed areas.[15]

We assess the validity of our instrument with seven specification checks. First, we look at whether education appears to be increasing before college openings. If openings determine education rather than vice-versa, we should find little evidence of a pre-trend in schooling in the ten years

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14 See examples on www.sheeo.org/govern/gov-panrep.htm.

15 We have examined the sensitivity of our IV results to the exclusion of private 4-year colleges (most 2-year colleges are public). We found that we could not reject the hypothesis that the estimates were the same whether or not these

before the opening of a new college. We also look for the presence of pre-trends in birth outcomes.

Second, we ask whether openings of two and four-year colleges affect educational attainments in the way predicted by Rouse (1995). That is, we expect new four-year colleges to have a stronger effect on the completion of four-year college degrees and vice-versa.

As discussed above, a third test of the validity of our instruments is to estimate first-stage models that include measures of college availability when the mother was 25 or older in addition to measures of college availability when the woman was 17. If our identification strategy is valid, then measures of opportunities at age 17 should have a larger effect on the woman's educational attainment than measures of college availability taken when she is past the usual age of college attendance. Conversely, if college educated women 25 or older move to counties that experience college openings, then measures of college availability taken at 25 or older should be stronger predictors of a woman's educational attainment than our measure of college availability measured at age 17.

Fourth, we turn to single-sex colleges. We cannot look at openings of single-sex colleges, because there are only a handful of new single-sex colleges over our sample period. But we do look at four-year colleges that turn from being single-sex to being coeducational. Male-only colleges that become coed increase women's access to college and hence should have a positive effect on female education; but if our identifying assumption is correct, female-only colleges that become coed should have no positive effect on women's education.

Fifth, we ask whether the opening of a new college has a greater effect on educational attainments in regions that are "college poor". That is, we expect that the opening of a new college should have a smaller effect if there are many colleges close by than if there are few. We also ask whether the opening of public colleges has a greater impact than the opening of private colleges, as one might expect if our instrument captures reductions in the cost of attendance for the marginal woman.

Sixth, we control for the average educational attainment of fathers (of all ages) in the county when the woman was 17 years old. If new colleges were more likely to be built in areas with high demand for college education, then part of this latent demand for college education by women will

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colleges were excluded.

be captured by measures of the educational attainments of men.

Finally, we try to assess the magnitude of the bias introduced by endogenous mobility using data from the 1980 and 1990 Census Public Use Micro Samples (PUMS). We are worried that some women move to areas with higher college availability in order to attend college. The potential for endogenous mobility is probably the main limitation of the instrument. We try to replicate our first stage regression in the Census sample using information on enrollment status of women 19-21 and the location of residence 5 years prior to the Census. The information on residence 5 years prior to the Census allow us to assign women the number of colleges that were available in the location of residence at age 14-16, that is *before* women reach college age. We regress enrollment status on this measure and compare the results with those from a regression of enrollment status on the number of colleges available when the woman was 14-16 in the current location of residence. The location of residence at age 14-16 is less likely to be contaminated by endogenous mobility than current location of residence.

d) Comparison of OLS and IV Estimates

If education is endogenous, and mothers of better "quality" tend to have higher education, OLS will tend to overestimate the true effect of schooling on birth outcomes. Hence, OLS estimates will be larger than valid IV estimates. On the other hand, there are at least two reasons why IV may exceed OLS.

First, the marginal benefit of schooling for individuals whose education has been affected by college openings may be larger than the average benefit for the population. Our IV estimates reflect the effect of education for women who would not have gone to college had it not been for the fact that a college opened in their county of residence. These women are likely to come from disadvantaged backgrounds relatively to those who were not constrained by college location. Card (2000, 2001) reviews a series of recent studies that measure the effects of education on earnings using instrumental variables such as compulsory schooling laws, differences in the accessibility of schools, and other institutional features of the education market. These studies typically find that instrumental variables estimates *exceed* OLS estimates of the returns to education, suggesting that the marginal returns to education among the groups affected by changes in the instrument are higher than the average returns in the population.

This is relevant when estimating the effect of schooling on child quality. The women affected by college openings may have high marginal returns to schooling, both in terms of earnings and the adoption of healthier behaviors, so that we may find IV estimates that are larger than OLS. Some of the specification tests described in the previous section shed light on whether women's educational choices appeared to be constrained by lack of college availability.

A second potential reason for IV to exceed OLS is the existence of spillovers. It is possible that increases in educational attainment have spillover effects on other women if, for example, pregnant women talk to other pregnant women and new mothers about infant health (c.f. Aizer and Currie, 2002). Alternatively, spillover effects could work more indirectly, by changing the standard of care practiced by doctors and hospitals. That is, if educated women demand that certain practices are adopted, less educated women may also benefit from those practices.

Lochner and Moretti (2001) show that spillovers can provide an alternative explanation for IV estimates that are high relative to OLS. If an individual's decision to engage in healthy behaviors or to buy health inputs depends on average education levels or the average behavior of other individuals in their cohort and county, IV (using county-cohort level instruments as we do) will estimate the combined effect of own education on outcomes as well as the effect of average cohort education on outcomes. That is, IV will estimate the sum of the individual effect and the spillover effect. If cross-county and cohort variation in average education are small relative to overall variation in education, then OLS will only estimate the individual effect of education. In Appendix B we present a formal analysis of the expected relationship between OLS, IV, and grouped OLS estimates when there are spillovers.

Empirically, we try to determine whether differences between OLS and IV can be attributed to education spillovers by aggregating our data to the county-cohort-level: OLS county-cohort-level estimates which exceed OLS estimates obtained using individual-level data are consistent with the hypothesis that IV exceeds OLS because of spillover effects.

A final reason for IV to exceed OLS is that the OLS estimates are contaminated by measurement error. However, if this were the case, then one might expect to see longitudinal estimates smaller than cross sectional estimates, which we do not find.

6. Estimates from Longitudinal Models

Table 2 presents estimates of the longitudinal model (equation 1) obtained using the sample of mothers 16-45 years old. Column 1 of Table 2 presents OLS estimates of the effects of maternal education on the probability of low birthweight, prematurity, the use of prenatal care in the first trimester, and marital status, using the pooled first and second births from the panel data set. (Estimation using only the first births produced very similar results). The models are similar to equation (1), except that they are estimated using levels rather than changes in outcomes/education. The estimates suggest that education has a strong positive effects on all four indicators. For example, an increase in education of one year would reduce the probability of low birthweight by almost 10% relative to the means shown in Table 1.

Column (2) of Table 2 presents estimates from models of the form (1) (changes on changes). The estimates are strikingly similar to those in column (1) and suggest that mothers who increase their education between the first and second births reduce the probability of low birth weight and prematurity as well as increasing their use of early prenatal care and marriage probabilities. The estimates in column (2) are for the full sample of women 16 to 45 at the time of the first birth. They imply that an increase in education of one year would reduce the probability of low birthweight by about a half of a percentage point. Relative to the baseline estimates in Table 1, this translates into a reduction of almost 10%. The effect on the probability of preterm birth is smaller, at 5.5%. Some of the improvements in birth outcomes may be mediated through increases in the use of prenatal care, and through the increases in the probability of marriage.

The next two columns of Table 2 divide the sample into those who started with a high school degree at the time of the first birth, and mothers who ended with a high school degree at the time of the second birth. The motivation for this division is twofold. First, it is possible that the effect of an extra year of schooling is different depending on the initial level of schooling. For example, the means in Table 1 suggest that an extra year of schooling at low levels of education has a larger impact than an extra year at high levels of education. Second, this division is intended to facilitate the comparison of the panel data estimates with the IV estimates to be presented below, since the IV estimates are based on increases in schooling from relatively high initial levels of education (from high school to community college or to four year college).

The estimates in columns 3 and 4 suggest that the effects of education are significant for both groups, although they are substantially larger for less educated women, as one might expect if decreasing returns to education set in after some point. In particular, the estimated effects of education on low birth weight and the use of prenatal care are roughly double in the less educated group, while the effect of education on the probability of marriage almost triples from an increase of 1 to 3 percentage points per year of additional education.

7. Instrumental Variable Estimates

a) Graphical Analysis

We begin our analysis of college openings by graphically showing how maternal education in the sample of first-time mothers 24 and older changed in the ten years before and after the opening of a four-year college. Figure 2 shows the average years of schooling before and after a four year college opening, after controlling for college dummies and dummy variables for mother's cohort of birth.[16] For purposes of illustration, we select openings where there were no other openings in the same county either 10 years before or 10 years after the year of the index college opening. (In the formal analysis below we use all the openings). The Figure shows that, on average, maternal education levels are higher in the 10 years following the opening than they were in the 10 years preceding. Moreover, there is no particular trend in maternal education in the county prior to the opening of a new four-year college.

The four panels of Figure 3 show the same story in more detail. The pattern for four-year college attendance is quite similar to the pattern for average years of education shown in Figure 2. This is consistent with the hypothesis that most of the increase in education that arises from the opening of a four year college comes from increases in the fraction of women with a four year college education. Furthermore, most of the increase in the fraction of women with a four year college degree appears to be coming from a reduction in the number of women who have only a high

16 In particular, we regressed years of schooling on dummies for time equal to t-10, t-8, t-6, t-4, t-2, t, t+2, t+4, t+6, t+8, t+10, county dummies and cohort dummies. The Figure shows the coefficients on dummies for time equal to t-10, t-8, t-6, t-4, t-2, t, t+2, t+4, t+6, t+8, t+10, where t is the time of college opening. Figures 2 to 4 are obtained in a similar way.

school degree. This lends some credibility to the hypothesis that the college opening induced some women who would have otherwise stopped studying after graduating from high school to go to college. There is little trend in either the number of women with some college (but no four-year degree) or in the number of high school dropouts.

Detecting pre-trends in outcomes is even more important than detecting pre-trends in education. Figure 4 shows the impact of college openings on the incidence of low birthweight and premature birth. Figure 4 shows that while there were no clear trends in the incidence of low birthweight or prematurity in the 10 years prior to the opening of a new college in the county, there is a drop in the incidence of these negative outcomes following the opening.

Figure 5 is like Figure 4, but includes college openings that occur when the mother is 25. Most women should be unaffected by a college opening that occurs past their college age. As expected, we see no clear effect of these later openings on outcomes.

b) First Stage Estimates of the Effects of College Openings on Maternal Education

We now turn to a more formal analysis of the effect of college openings on schooling. Our first stage results are shown in Table 3. Column (1) of panel 1 shows that an increase of one four-year college per 1,000 persons 18 to 22 would result in almost a full year more of maternal education among women 24 and over at the time of first birth.The mean of this variable in our sample is .082 indicating that at the mean, new college openings increased maternal education by approximately .08 years. The effect of a new two-year college is substantially smaller: An increase of one college per 1,000 young adults would increase schooling by about a fifth of a year. Evaluated at the sample mean of .05 two year colleges per 1,000, this coefficient implies that increases in the availability of two-year colleges increased maternal schooling by about .01 years.[17]

Column 2 shows that adding state-cohort trends to the model has little effect on the estimates. Column 3 indicates that controlling for the average education of fathers in the county when the

17 A second way to interpret these estimates is to ask how many more college-educated women resulted from the opening of, for example, a new four-year college? Model 2 of Table 3 indicates that such an opening increased the probability that a first-time mother 24 or older had 4 years of college by about 20 percentage points. Since the average county has approximately 10,000 18 to 22 year olds, this implies that one new four year college would increase the probability that these mothers had a college education by 2 percentage points. Relative to the baseline of 42%, this is an increase of about 5% in the probability of college. Since the average county in our sample has 165 first time moms 24 to 45, a 5% increase would imply that approximately 8 more of women per county per year received a college education

mother was 17 reduces the coefficients–in this specification a new four year college (per 1,000) increases maternal schooling by .7 rather than .95 years--but the effect remains sizeable and statistically significant.

The remaining panels of Table 3 show how the change in educational attainment break down. New four-year colleges apparently have little effect on the probability that only "some college" is completed. On the other hand, a new two-year college (per 1,000) increases the probability of obtaining a four-year degree by 2.5 percent, but has a larger effect on the probability of some college, increasing it by 3.2 percent. Again, most of this increase is coming from a decrease in the number of women obtaining only a high school degree. Estimates in panels 2 to 5 of Table 3, and in particular the comparison of model 2 with model 3, are inconsistent with the hypothesis that colleges open in areas where education is increasing at all levels. Model 2 and model 3 suggests that the opening of a four or two year college affects the education distribution at the "right" level, and has little spillover onto other levels of schooling.[18]

Table 4 shows alternative specifications of our first stage equation (2), in which measures of college availability when the mother was 25 or older are added to the base specification shown in Table 3. The coefficients on college availability at age 17 remain very similar to those in Table 3, confirming that this measure of college availability has a positive effect on maternal education. While a few coefficients in column 1 are precisely estimated, column 3 shows that once state-mother cohort time trends and fathers' average educational attainment when the mother was 17 are added to the model, not a single one of the measures of college availability at older ages is statistically significant. We perform an F-test to test whether the coefficients on college availability at 17 are equal to the coefficients on college availability at later ages, and we always reject this restriction. Thus, Table 4 provides evidence that it is college availability at 17 rather than at older ages that

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as a result of the college opening

18 We have also estimated first stage models separately for three different cohorts of mothers: Those who were 17 between 1940 and 1960, those who were 17 between 1960 and 1980, and those who were 17 between 1980 and 2000 (results are not reported in the table). In Table 3, the coefficient on four-year colleges was .95. The comparable coefficients for the effect of four-year colleges on the three cohorts are: 1.19, .903, and 1.53 (all statistically significant). The comparable coefficients for the effects of two-year colleges are: .007, .173, and .214 (but only the middle coefficient is statistically significant). Thus the effect of having a four-year college open in ones own county is not noticeably weaker for the younger cohorts than for the older ones, whereas the effects of two-year colleges are greatest for those cohorts who experienced the large boom in two-year college construction. We speculate that part of the reason for this pattern may be the increase in the average size of colleges over the period under consideration.

matters. This suggests that our results regarding the effects of college openings on maternal education are not driven by underlying trends in college enrollments. It also suggests that our results are not driven by the migration of women older than 24 seeking jobs (or even husbands) in the county of the new college. We present further specification checks in section c below.

b) OLS and IV Estimates of the Effect of Maternal Education

OLS estimates of the effects of maternal education on infant health, marriage, husband's education, probability of smoking, parity and age at first birth are shown in column 1 of Table 5 for our sample of older mothers. These estimates are very similar to those in Table 2. For example, they suggest that an additional year of maternal education reduces the probability of low birth weight by ten percent, and lowers the risk of prematurity by 6 percent. The increases in college education induced by openings of four-year colleges are estimated to have reduced the risk of low birth weight by about one percent, while openings of two-year colleges reduced the risk of low birth weight by about half of one percent. The comparable figures for reductions in the risk of prematurity are half of one percent for four-year colleges and are very small for two-year colleges.

In this sample, we can examine a wider range of outcomes, including the education of the husband, the age at first birth, parity, and the incidence of smoking. Note that the model for age at first birth differs slightly from equation (3) in that it does not include maternal age dummies while the model of parity includes all births rather than first births only. Table 5 suggests that much of the positive impact of higher education on birth outcomes may be coming through reductions in smoking, which is the single leading cause of low birth weight and prematurity: An additional year of education reduces the probability of smoking by more than thirty percent. The increases in four and two-year colleges are estimated to have reduced smoking by three percent and two percent, respectively.

Some of the effect may also be coming through changes in the budget constraint since an additional year of education increases the probability of marriage by one percentage point and increases husband's education by .6 of a year. Effects on the use of prenatal care may be interpreted either as the result of higher income, or as a change in behavior. In any case, an additional year of education increases the probability that prenatal care began in the first trimester by one percentage point. However, marriage and prenatal care utilization rates are high for all groups in our sample, which means that there is less scope for education to affect these outcomes than smoking.

Instrumental variables estimates are shown in column 2 of Table 5. We do not present IV estimates for husband's education, since it is not plausible that our instruments affect women's education but not men's. It is striking that the IV estimates are roughly double the OLS estimates for all outcomes except marriage and parity, where the IV estimates are somewhat lower than the OLS. These estimates imply then, that educated women have characteristics which also make them more marriageable and likely to have fewer children. Controlling for these characteristics reduces the estimated effect of education on marriage and parity but does not eliminate it. On the other hand, IV estimates of the effects of education on prenatal care utilization and birth outcomes exceed OLS estimates which suggests either that there are large spillover effects of education on these outcomes or that there is a higher than average return to educating the marginal woman in terms of infant health.

To help in interpreting the magnitude of the estimated effects, consider that the increase in maternal education between the cohort of women who went to college in the 1940s and the 1950s and the cohort of women who went to college in the 1980s is about 1.6 years. The probability of low birth weight and pre-term birth decreased by 6 percentage points and 3 percentage points, respectively. Our estimates suggest that 12% of the decrease in the probability of low birth weight and 20% of the decrease in the probability of pre-term birth can be attributed to increased maternal education.

The third column of Table 5 shows estimates of equation (2) using data aggregated to the county-year level. As discussed above, if spillovers are important, then we expect these estimates to significantly exceed those in column (1). And if spillovers are to explain the fact that IV estimates exceed OLS estimates, then the estimates in column (3) should resemble those in column (2). On the basis of these comparisons, the outcome for which spillovers appear to be most important is the use of prenatal care in the first trimester. There is also some evidence consistent with spillovers in the estimates for pre-term birth and mother's age at first birth, although for these outcomes IV estimates exceed those from grouped data.

Table 6 presents some evidence regarding the robustness of our OLS and IV results to various changes in specification. The odd columns present estimates which control for state-mother cohort effects, while the even columns also control for average fathers' education when the mother was 17.[19] Table 6 shows that with the exception of mother's age at first birth, our results are remarkably robust to these changes in specification. On the other hand, the estimated effect of maternal education on age at first birth is quite sensitive to the inclusion of state-cohort trends, suggesting that trends in age at first birth in this group of mothers 24 and older varied considerably from place to place in a way that was correlated with educational outcomes.

d) Additional IV Specification Checks

We now turn to the additional specification checks described in Section 5c. We begin by using single-sex colleges as a specification check. If our identification strategy is valid, then the transformation of a male only college into a coed college should increase maternal education, while the transformation of a female only college into a coed college should have no effect (or a negative effect if women are crowded out) on maternal education. In our sample, there are 104 cases of four year colleges switching from male-only to coed, and 85 cases of four year colleges switching from female-only to coed.20 Table 7 shows that having a college change from being male-only to being coed, has an effect on maternal education that is remarkably similar to the effect of a new four year college opening shown in Table 3. (The specification is similar to the one used in Table 3, column 1 except that the measures of coed colleges are included instead of the measures of two and four year college availability). The effect of a having a college change from being female only to coed is smaller, and not statistically significant.

We now turn to a specification check based on the availability of other colleges in nearby counties. If our identification strategy is valid, then the opening of a college in a county in an area far from any college should have a larger impact than the opening of a college in a county close to many colleges. In Table 8 we ask whether the effects of new college openings are greater in

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[19] We also estimated alternative models that included region*cohort dummies in addition to the state*cohort trends. These specifications allow cohort effects to vary over regions in a non-linear way, while the state-cohort time trends constrain differences between state to be linear. In any case, the estimates including region*cohort effects were very similar to those shown in Table 6.

20 Unfortunately, we could not find data on changes in gender policy for two years colleges.

locations far from other colleges. In the first specification (Model 1), we divide counties into two groups according to whether the number of colleges (normalized by cohort size) in adjacent counties is above or below the median. These variables are then interacted with our measures of two and four-year college availability. The results indicate that new four-year colleges do have a greater effect in college-poor areas, but the difference between college-rich and college-poor areas is not statistically significant. On the other hand, new two-year colleges are found to have a much greater impact on maternal education in college-poor areas.

In a second specification (Model 2 of Table 8), we adopt a continuous measure of the availability of colleges in adjacent counties, defined as the average number of colleges in adjacent counties (again normalized by the relevant cohort sizes). This specification suggests that the effect of a new college falls off rapidly with the number of colleges in contiguous areas: An increase of one four-year college per 1,000 residents 18 to 22 is estimated to increase maternal education by almost a year in areas with no colleges in contiguous counties. But the effect falls to zero as the number of colleges in contiguous counties rises to one per 1,000 residents in the relevant age cohort.

Table 9 shows the separately estimated effects of the opening of public and private four-year colleges. As discussed above, if college openings in a woman's county affect maternal education by reducing the costs of attendance, then we might expect to see a larger effect for public colleges than for private colleges, given the usual differences in tuition and the larger average size of public colleges. Table 9 shows that this is indeed the case. The effect of a new public college is over three times greater than the effect of a new private college.[21]

e) Endogenous Mobility

Finally, we turn to the issue of endogenous mobility. There are two types of endogenous mobility that are potentially problematic for our instrumental variables strategy. First, 17 or 18 year old women may move to counties that experience college openings to attend college. Second, college educated women may move to counties that experience college openings after they graduate from college but before they have their first child. In Table 4 we presented evidence that indicates that the second type of mobility does not appear to be very significant. Mobility of the first type is more

[21] Information about the capacity and tuition of colleges is not available on a consistent basis over time.

worrisome. It is undoubtably the case that some women move to areas with higher college availability in order to attend college. The question is whether such mobility is important enough to account for a significant portion of the documented correlation between number of colleges and maternal education (i.e. our first stage coefficient). If mobility explains a significant portion of our first stage coefficient, then our instrument is invalid. We want to make clear that, because of data limitations, we cannot completely rule out this possibility. We do have some indirect evidence that we hope may shed some light on the magnitude of the problem.

First, as we mentioned earlier, colleges in our sample appear to be, for the most part, new non-selective institutions that are unlikely to attract a significant portion of the student body from far away. This is, of course, just a conjecture, since data on the county of residence of the students prior to their enrollment do not exist. The only available data report the state of residence prior to enrollment. In Table 10 we report the percentage of college students who are from the state where the college is located, by the age of the college, in 1998.[22] The Table confirms that, while old, more established institutions attract a significant number of out of state students, new, less established colleges attract mostly in state students. For example, while more than a third of the students enrolled in four year colleges that are 100 years or older are from out of state, only 9% of students enrolled in colleges 9 years or younger are from out of state. The corresponding figures for two year colleges are even lower. Unsurprisingly, community college students are overwhelmingly local, irrespective of the age of the college.

Although suggestive, these numbers are less than ideal. On one hand, the figures shown are for 1998. Since student mobility has steadily increased since 1940 (Hoxby, 1996), mobility in most of the years we consider is likely to be even lower. On the other hand, Table 10 reports student mobility across states, and is likely to underestimate mobility across counties.

For a second piece of indirect evidence, we turn to the 1980 and 1990 five percent Public Use Micro Samples of the Census. We try to assess the effect of the instrumental variable on college enrollment using the county of residence at ages younger than college age. We compare the estimated effect with the effect of the instruments on college enrollment using current county of residence. Specifically, we take all white women 19-21 who report information on the location of

22 Data are from 1998 IPEDS and include 2303 institutions. We obtain similar results when we look at 1992 or 1994. Data for earlier years were not available.

residence 5 years prior to the Census (that is, when the respondent was 14-16). We then regress a dummy equal to one if the respondent is enrolled in school on the (normalized) number of four and two year colleges that existed when the respondent was 14-16 in the location where she lived when she was 14-16. We also regress the enrollment dummy on the (normalized) number of four and two year colleges that existed when the respondent was 14-16 in the location where she currently lives. Both models include year and location fixed effects.

Ideally, we would like to replicate our first stage regressions using Census data to check whether estimates differ when county of residence at age 14-16 is used instead of current county of residence. However, data limitations preclude an exact replication. First, the dependent variable is enrollment, not schooling achievement, because the individuals for whom we have information on county of residence at age 14-16 may still be in college at the time of the Census. In other words, while the Natality IV sample includes women 24 or older, the Census sample includes women 19 to 21 years old. Second, we include all women, not only mothers, since we have no way of knowing which of the women will end up having children. Third, the smallest geographical identifier in the Census is the PUMA, not the county. PUMA are smaller than counties in urban areas, and larger than counties in rural areas. For example, for the nine-county San Francisco Bay Area, there are 48 PUMAs. We aggregate PUMAs in urban areas to the level of metropolitan area. See the Data Appendix text for more details. Finally, the Census reports the information on PUMA of residence 5 year ago only in 1980 and 1990. For these reasons, Census estimates are not exactly comparable with the first stage estimates in Table 3.

Estimates in the top panel of Table 11 indicate that the coefficient on four year colleges that existed when the respondent was 14-16 in the location where she lived at age14-16 is 0.104, indicating that an increase in the (normalized) number of four year colleges in the location of residence *before* the respondent reaches college age is associated with an increase in the probability of enrollment. Estimates in the bottom panel indicate that the coefficient on four year colleges that existed when the respondent was 14-16 in the location where the respondents is at the time of the Census is 0.106, not very different from the corresponding coefficient in the upper panel. The location of residence *before* the respondent reaches college age is less likely to be contaminated by endogenous mobility than current location of residence. The similarity of the two estimates lends some credibility to the assumption that endogenous mobility is not solely responsible for our first stage results for four years colleges, although the difference between Natality and the Census samples precludes firm conclusions.

The similarity between upper and lower panel estimates does not extend to two year colleges. The coefficient for two year colleges in the upper panel is 0.011. The fact that the coefficient for two year colleges is smaller than the corresponding coefficient for four year colleges is consistent with results in Table 3. However, the coefficient for two year colleges in the lower panel is negative. This last finding is surprising and stands in sharp contrast both with the coefficient in the upper panel and the first stage coefficient in Table 3. We don't have a definitive explanation for it. But it should be noted that only a very limited number of new two years colleges enter the sample between 1980 and 1990. Most of the new two years colleges in the sample open during the 1960s and 1970s (Figure 1).

In summary, our estimates of the effects of college openings on maternal education suggest that four year colleges have a greater effect on four year college completion than two year colleges (and vice versa); that the education of women who are 25 or older when a college opening occurs is not affected by the opening; that female-only colleges increase women's education, but male-only colleges do not; that public colleges have a greater impact than private colleges; and that college openings in "college poor" locations have larger effects. All these findings suggest that the estimated effects of college openings are unlikely to simply reflect an omitted third factor. Moreover, our Census results suggests that increases in the number of college educated women following the opening of a four year college are unlikely to be *solely* due to inflows of women seeking education. Thus, while it is impossible to definitively prove the validity of our instrumental variables strategy, the specification checks provide some additional evidence which is not inconsistent with our identification assumptions.

7. Conclusions

We provide new evidence regarding the effect of maternal education on infant health, and on a series of other factors that are likely to influence infant health such as smoking, use of prenatal care, marriage, and fertility behavior. Our estimates suggest that increases in maternal education over the past 30 years have had large positive effects on birth outcomes. We estimate that an additional year of education reduces the incidence of low birth weight by approximately ten percent, and reduces the incidence of pre-term birth by six percent, on average. These effects arise because education affects maternal behavior (by reducing smoking by more than 30 percent, for example); it increases earnings, possibly relaxing budget constraints; it improves women's marriage markets; and it reduces fertility. As expected, the estimated effects are larger for less educated women, but remain substantial even among high school graduates and college graduates. We also find some indirect evidence of educational spillovers, particularly in the use of prenatal care in the first trimester.

Although we cannot rule out alternative explanations—in particular, we cannot completely rule out endogenous mobility—the qualitative results are robust to the use of longitudinal data and instrumental variables methods. The fact that longitudinal models and instrumental variable estimates depend on very different assumptions lends some credibility to our findings.

In addition, our first stage estimates are of interest in their own right, since they demonstrate that the boom in the construction of new colleges during the 1960s and 1970s had a significant impact on the education of mothers. Specifically, the mean county experienced an increase of .08 four-year colleges and .05 two-year colleges per 1,000 young people 18 to 22. These improvements in the education infrastructure increased average maternal schooling by .08 years and .01 years for four and two-year colleges respectively.

On average, a change in .09 years of education reduced low birthweight by about one percent, and reduced the incidence of preterm birth by about half of one percent, according to our OLS and panel data estimates. If we use the IV estimates to calculate the impact on those most likely to have been affected, then the increase in education induced by the college openings is estimated to have reduced the incidence of low birthweight and preterm delivery by closer to two percent and one percent, respectively. While these may seem like small improvements, the costs of low birthweight and prematurity are large. For example, it is estimated that between birth and age 15, low birthweight children incur an additional \$5.5 to \$6 billion more in health, education, and other costs than children of normal birthweight (March of Dimes, 2002).

Traditionally, research on the benefits of schooling has focused on the *private* returns to education, i.e. the effect of a worker's education on her own wage. There is growing evidence that the *social* return to education may exceed the private return.[23] Improved birth outcomes are another benefit of education that is not reflected in the wages of educated individuals. Hence, our results suggest that estimates of the returns to education which focus only on increases in wages significantly understate the social return. Moreover, if educating mothers improves the health, educational attainment, and labor market outcomes of children, then this is a significant inter-generational benefit.

23 Education has been shown to benefit productivity, innovation, and lower crime rates. See for example Moretti 2000; and Lochner and Moretti 2002.

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Table 1: Summary Statistics

	All	Panel Sample		IV Sample				
				All	College	Some College	High School Only	High School Drop Out
	1st births 16-45	1st births 16-45	2nd births 18-45	1st births 24-45	1st births 24-45	1st births 24-45	1st births 24-45	1st births 24-45
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother Education	12.95 (2.22)	12.77 (2.22)	13.24 (2.15)	14.21 (2.08)	16.3 (0.47)	14.27 (0.44)	12 (0)	9.47 (2.15)
Low Birth Weight	0.054	0.047	0.032	0.049	0.041	0.047	0.057	0.089
Pre-term Birth	0.079	0.069	0.058	0.069	0.062	0.068	0.075	0.100
Prenatal Care	0.827	0.816	0.846	0.921	0.945	0.925	0.894	0.759
Smoked During Pregnancy	.145	--	--	0.078	0.023	0.080	0.168	0.340
Married	.772	0.889	0.946	0.923	0.968	0.930	0.890	0.721
Husband's Education	13.1 (2.31)	--	--	14.20 (2.65)	15.55 (1.65)	14.39 (1.96)	12.88 (1.94)	10.87 (2.7)
Mother Age	23.83 (5.14)	23.29 (4.6)	26.29	28.12 (3.56)	29.02 (3.6)	27.80 (3.1)	27.31 (3.3)	27.30 (3.6)
Year Mother was 17	1980 (8.7)	1971.0 (5.8)	1971.0 (5.8)	1974.8 (7.5)	1975.0 (7.7)	1975.6 (7.4)	1974.1 (7.3)	1973.4 (8.6)
Per Capita Number of Four Year Colleges in County, when Mother is 17	0.0796 (0.0984)	0.0838 (0.129)	0.0838 (0.129)	0.0819 (0.0925)	0.0856 (0.0856)	0.0799 (0.0903)	0.0785 (0.0932)	0.0766 (0.0974)
Per Capita Number of Two Year Colleges in County, when Mother is 17	0.0548 (0.0850)	0.0633 (0.120)	0.0633 (0.120)	0.0502 (0.0696)	0.0492 (0.0492)	0.0519 (0.0672)	0.0497 (0.0739)	0.0514 (0.0850)
Median Family Income in County when Mother is 17	32510 (7150)	27156 (7768)	27156 (7768)	33273 (7467)	34082 (7716)	33670 (7120)	32359 (7176)	29942 (7751)
Percentage County that is Urban when Mother is 17	0.71 (0.27)	0.51 (0.31)	0.51 (0.31)	0.77 (0.24)	0.80 (0.23)	0.78 (0.23)	0.74 (0.26)	0.70 (0.29)
Sample Size	20,422,042	817,165	817,165	671,468	279,574	169,916	204,394	17,584
Parity (sample includes all Births rather than 1st births)	2.0 (1.3)	1 (0)	2 (0)	2.4 (1.2)	2.0 (1.1)	2.3 (1.2)	2.5 (1.2)	3.4 (1.2)

Notes: For smoking, N=166,183. For husband's education, N-= 486,255. See text

Table 2

Longitudinal Models: The Effect of Changes in Maternal Education on Changes in Infant Health, Health Inputs and the Marriage Market

	Cross Section	Changes		
	All Mothers	All Mothers	Mothers with High School Degr. at First Birth (Educ. Range: 12-16+)	Mothers with High School Degr. at Second Birth (Educ. Range: 0-12)
	(1)	(2)	(3)	(4)
1. Change in Low Birth Weight	-0.0043	-0.0046	-0.0037	-0.0071
	(0.0001)	(0.0002)	(0.0004)	(0.0007)
2. Change in Pre-Term Birth	-0.0037	-0.0038	-0.0038	-0.0042
	(0.0001)	(0.0003)	(0.0005)	(0.0009)
3. Change in Prenatal care	0.0137	0.0122	0.0105	0.0195
	(0.0001)	(0.0005)	(0.0008)	(0.0014)
4. Change in Marital Status	0.0142	0.0210	0.0120	0.0284
	(0.0001)	(0.0003)	(0.0002)	(0.0009)

Notes: Standard errors in parentheses. The coefficients reported in columns 2 to 4 are the coefficients on changes in mother education. The sample was obtained as follows: first, women with parity =1 were selected. Then, women with parity = 2 were matched to women with parity =1 based on county, date of birth of first child (month and year), mother's age at first birth, mother's year of birth and mothe's state of birth. Finally, only cells with size equal 1 in both years were kept (one-to-one matches). The sample is therefore an actual panel of mothers with two children, where the same mother is observed at the time of the first birth and the time of the second birth. Smoking is missing because it was reported first in 1989, while the month of birth for the last birth was reported only until 1993. All models include unrestricted mother age effects at first birth, mother age at second birth, unrestricted mother cohort of birth effects, and county dummies. See text for details.

Table 3
The Effect of College Openings on Maternal Education

	(1)	(2)	(3)
Model 1: Years of Schooling			
Four Year Colleges	0.950	0.952	0.716
	(0.046)	(0.046)	(0.042)
Two Year Colleges	0.176	0.158	0.145
	(0.046)	(0.046)	(0.046)
R squared	0.11	0.12	0.12
Model 2: 4 Years or More of College			
Four Year Colleges	0.198	0.199	0.150
	(0.010)	(0.010)	(0.009)
Two Year Colleges	0.025	0.021	0.018
	(0.010)	(0.010)	(0.010)
R squared	0.10	0.10	0.10
Model 3: Some College			
Four Year Colleges	0.000	0.046	0.000
	(0.006)	(0.006)	(0.006)
Two Year Colleges	0.032	0.032	0.032
	(0.007)	(0.007)	(0.007)
R squared	0.02	0.03	0.03
Model 4: High School Only			
Four Year Colleges	-0.156	-0.156	-0.120
	(0.009)	(0.009)	(0.009)
Two Year Colleges	-0.053	-0.051	-0.051
	(0.010)	(0.010)	(0.010)
R squared	0.07	0.07	0.07
Model 5: Less Than High School			
Four Year Colleges	-0.026	-0.026	-0.026
	(0.003)	(0.003)	(0.003)
Two Year Colleges	-0.003	-0.003	-0.003
	(0.004)	(0.004)	(0.004)
R squared	0.03	0.03	0.03
State-Mother Cohort Trends		Yes	Yes
Men's Average Education			Yes

Notes: Standard errors in parentheses. All models include mother age effects, mother cohort of birth effects, county*year effect (where year is year of birth of the child), median county income and percent urban in year when mother was 17,. "Four year" is the number of four year colleges existing in a county in the year when the mother was 17 normalized by the size of mother cohort (number of 17-20 year olds in the county, in thousands). "Two year" is the number of two year colleges existing in a county in the year when the mother was 17 normalized by the size of the mother's cohort. See text for details.

Table 4
The Effect of College Openings at Ages Later than 17 on Maternal Years of Schooling

	(1)	(2)	(3)
Model 1: College Opens at Age 25			
Four Year College Opens at Age 17	1.259	1.330	0.941
	(0.161)	(0.161)	(0.153)
Two Year College Opens at Age 17	0.317	0.299	0.191
	(0.104)	(0.103)	(0.102)
Four Year College Opens at Age 25	-0.371	-0.454	-0.264
	(0.180)	(0.178)	(0.171)
Two Year College Opens at Age 25	-0.180	-0.177	-0.057
	(0.105)	(0.105)	(0.107)
Effect at age 17 = Effect at age 25 (p-value)	0.000	0.000	0.000
Model 2: College Opens at Age 30			
Four Year College Opens at Age 17	1.304	1.304	0.857
	(0.139)	(0.140)	(0.131)
Two Year College Opens at Age 17	0.279	0.241	0.071
	(0.100)	(0.100)	(0.098)
Four Year College Opens at Age 30	-0.444	-0.449	-0.246
	(0.157)	(0.158)	(0.149)
Two Year College Opens at Age 30	-0.122	-0.095	0.063
	(0.100)	(0.100)	(0.100)
Effect at age 17 = Effect at age 30 (p-value)	0.000	0.000	0.000

Table 4 - *continued*

Model 3: College Opens at Age 35			
Four Year Colleges Opens at Age 17	1.221	1.234	0.800
	(0.151)	(0.152)	(0.145)
Two Year Colleges Opens at Age 17	0.250	0.236	0.098
	(0.106)	(0.106)	(0.105)
Four Year Colleges Opens at Age 35	-0.325	-0.344	-0.169
	(0.179)	(0.180)	(0.174)
Two Year Colleges Opens at Age 35	-0.184	-0.174	-0.056
	(0.105)	(0.105)	(0.106)
Effect at age 17 = Effect at age 35 (p-value)	0.000	0.00	0.005
Model 4: College Opens at Age 40			
Four Year Colleges Opens at Age 17	1.120	1.127	0.722
	(0.176)	(0.178)	(0.176)
Two Year Colleges Opens at Age 17	0.197	0.167	0.080
	(0.153)	(0.154)	(0.150)
Four Year Colleges Opens at Age 40	-0.050	-0.064	0.043
	(0.229)	(0.233)	(0.229)
Two Year Colleges Opens at Age 40	-0.247	-0.220	-0.107
	(0.146)	(0.147)	(0.148)
Effect at age 17 = Effect at age 40 (p-value)	0.002	0.002	0.17
State-Mother Cohort Trends		Yes	Yes
Men's Average Education			Yes

Table 5

The Effect of Maternal Education on Infant Health, the Marriage Market and Fertility

	OLS (1)	IV (2)	OLS Grouped (3)
1. Low Birth Weight	-0.0050 (0.0001)	-0.0098 (0.0038)	-0.0057 (0.0008)
2. Pre-Term Birth	-0.0044 (0.0001)	-0.010 (0.0044)	-0.0078 (0.0009)
3. Prenatal care	0.0114 (0.0001)	0.0234 (0.0055)	0.0230 (0.0010)
4. Smoked During Pregnancy	-0.0305 (0.0004)	-0.0583 (0.0118)	-0.0345 (0.0030)
5. Married	0.0206 (0.0002)	0.0128 (0.0040)	0.0135 (0.0008)
6. Parity	-0.121 (0.000)	-0.092 (0.010)	-0.0577 (0.0030)
7. Husband's Education	0.607 (0.0019)	--	0.690 (0.0081)
8. Mother's Age at First Birth	0.120 (0.002)	0.535 (0.057)	0.221 (0.018)

Notes: Standard errors in parentheses. All models include unrestricted mother age effects, unrestricted mother cohort of birth effects, county*year effect (where year is year of birth of the child), median county income in year when mother was 17, and percent of the county that is urban in year when the mother was 17. Models in row 8 do not control for mother age. See text for details.

Table 6
The Effect of Maternal Education on Infant Health, the Marriage Market and Fertility: Alternative Specifications

	OLS (1)	OLS (2)	IV (3)	IV (4)
1. Low Birth Weight	-0.0050	-0.0050	-0.0099	-0.0110
	(0.0001)	(0.0001)	(0.0038)	(0.0054)
2. Pre-Term Birth	-0.0044	-0.0046	-0.010	-0.010
	(0.0001)	(0.0001)	(0.0044)	(0.0062)
3. Prenatal care	0.0116	0.0117	0.0241	0.0251
	(0.0001)	(0.0001)	(0.0054)	(0.0077)
4. Smoked During Pregnancy	-0.0305	-0.0365	-0.0623	-0.0607
	(0.0004)	(0.0004)	(0.0118)	(0.0204)
5. Married	0.0207	0.0206	0.0129	0.0130
	(0.0002)	(0.0002)	(0.0040)	(0.0054)
6. Parity	-0.121	-0.120	-0.088	-0.062
	(0.000)	(0.000)	(0.010)	(0.015)
7. Husband's Education	0.604	0.601	--	--
	(0.0019)	(0.0019)		
8. Mother's Age at First Birth	0.006	0.006	0.017	-0.041
	(0.000)	(0.000)	(0.008)	(0.014)
State-Mother Cohort Trends	Y	Y	Y	Y
Men Avg. Education		Y		Y

Notes: Standard errors in parentheses. All models include unrestricted mother age effects, unrestricted mother cohort of birth effects, county*year effect (where year is year of birth of the child), median county income in year when mother was 17, and percent of the county that is urban in year when the mother was 17. Models in row 8 do not control for mother age. See text for details.

Table 7
The Effect of the Transformation of Male-Only Colleges or Female-Only Colleges into Coed Colleges on Maternal Education

From Male-Only College to Coed College	0.976
	(0.318)
From Female-Only College to Coed College	0.305
	(0.411)

Notes: The coefficients in row 1 show the effect of the transformation of male only four years colleges into coed colleges on mothers' education. There are 106 changes between 1968 and 1983. The coefficients in row 2 show the effect of the transformation of female-only four years colleges into coed colleges on mothers' education. Thee are 87 changes between 1968 and 1983. The specification is the one used in Table 3 (model 1 column 1). Data on single sex two year colleges are not available.

Table 8
The Effect of College Openings on Maternal Education
by Number Of Colleges Available in Contiguous Counties

	Coefficient for Four Year Colleges (1)	Coefficient for Two Year Colleges (2)
Model 1: Dividing Counties in Two Groups		
Contiguous counties have less than median number of colleges	1.074	0.299
	(0.086)	(0.069)
Contiguous counties have more than median number of colleges	0.884	0.027
	(0.051)	(0.060)
Model 2: Linear Interaction		
Number of Colleges in own County	1.078	0.288
	(0.076)	(0.062)
Number of Colleges in own County* Number of Colleges in Contiguous Counties	-1.236	-1.154
	(0.465)	(0.498)

Notes: Standard errors in parentheses. The four coefficients for each model come from one regression. All models include main effects for the number of colleges in contiguous counties, unrestricted mother age effects, unrestricted mother cohort of birth effects, county*year effect (where year is year of birth of the child), median county income in year when mother was 17, and percent of the county that is urban in year when the mother was 17. Number of colleges is normalized by cohort size. See text for details.

Table 9

The Effect of College Openings on Maternal Education: Separate Estimates for Public and Private Four-year Colleges

	Coefficient for Four Year Colleges (1)	Coefficient for Two Year Colleges (2)
Public Colleges	1.814	
	(0.075)	0.181
		(0.046)
Private Colleges	0.503	
	(0.053)	

Notes: Standard errors in parentheses. All coefficients come from one regression. The model includes unrestricted mother age effects, unrestricted mother cohort of birth effects, county*year effect (where year is year of birth of the child), median county income in year when mother was 17, and percent of the county that is urban in year when the mother was 17. Number of colleges is normalized by cohort size. See text for details.

Table 10
The Percentage of In-State Students, by Age of College

	Four Year Colleges	Two Year Colleges
College is 0-9 years old	91	94
College is 10-19 years old	91	98
College is 20-29 years old	84	95
College is 30-39 years old	79	95
College is 40-49 years old	71	96
College is 50-59 years old	75	94
College is 60-69 years old	75	91
College is 70-79 years old	75	93
College is 80-89 years old	74	94
College is 90-99 years old	77	92
College is 100+ years old	64	94

Notes: Entries show the percentage of students enrolled in four year or two year colleges who are from the state where the college is located. Data are from 1998 IPEDS.

Table 11
The Effect of College Openings on Enrollment,
by Current Residence and Residence at age 14-17,
1980 and 1990 Census

	Enrolled in School (1)
Model 1	
Four Year Colleges in Location of Residence at age 14-17	0.104
	(0.057)
Two Year Colleges in Location of Residence at age 14-17	0.011
	(0.005)
Model 2	
Four Year Colleges in Current Location of Residence	0.106
	(0.012)
Two Year Colleges in Current Location of Residence	-0.125
	(0.018)

Notes: Sample includes white women 19-21 from the 1980 and 1990 5% Censuses of Population. The dependent variable is a dummy equal one if the respondent is enrolled in school. Entries in model 1 show the coefficient on four and two year colleges that existed when the respondent was 14-16 in the city where she lived at age was 14-16. Entries in model 2 show the coefficient on four and two year colleges that existed when the respondent was 14-16 in the city where she currently lives. Both models include year and location fixed effects. N= 305,225. See text and Data Appendix for details on the samples.

Figure 1: Number of Four and Two Year Colleges

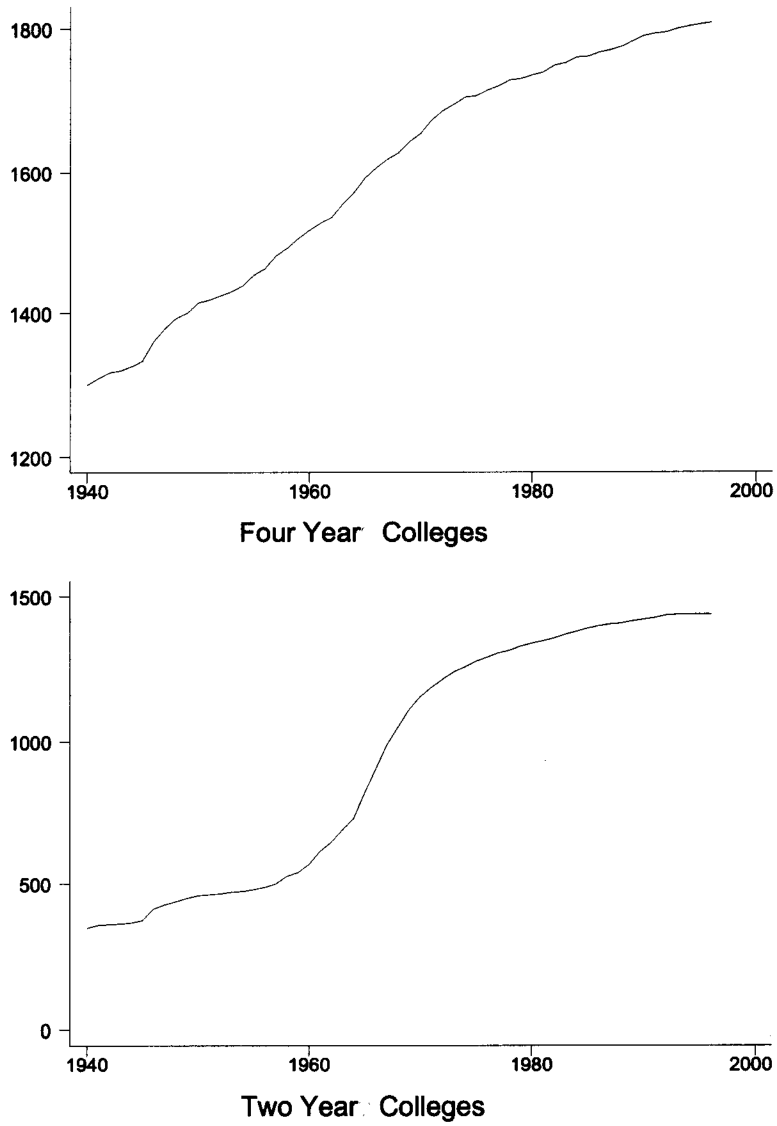
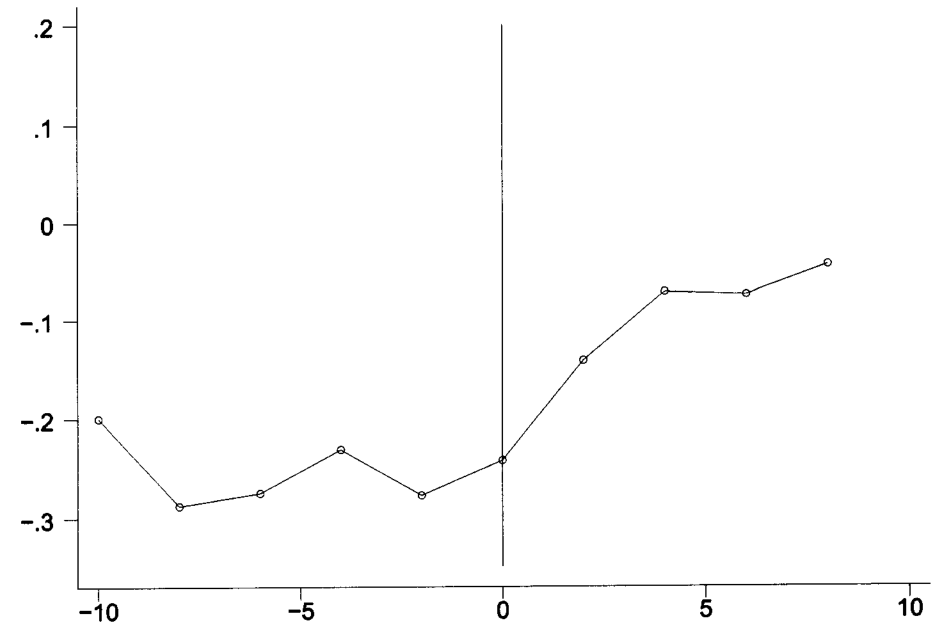
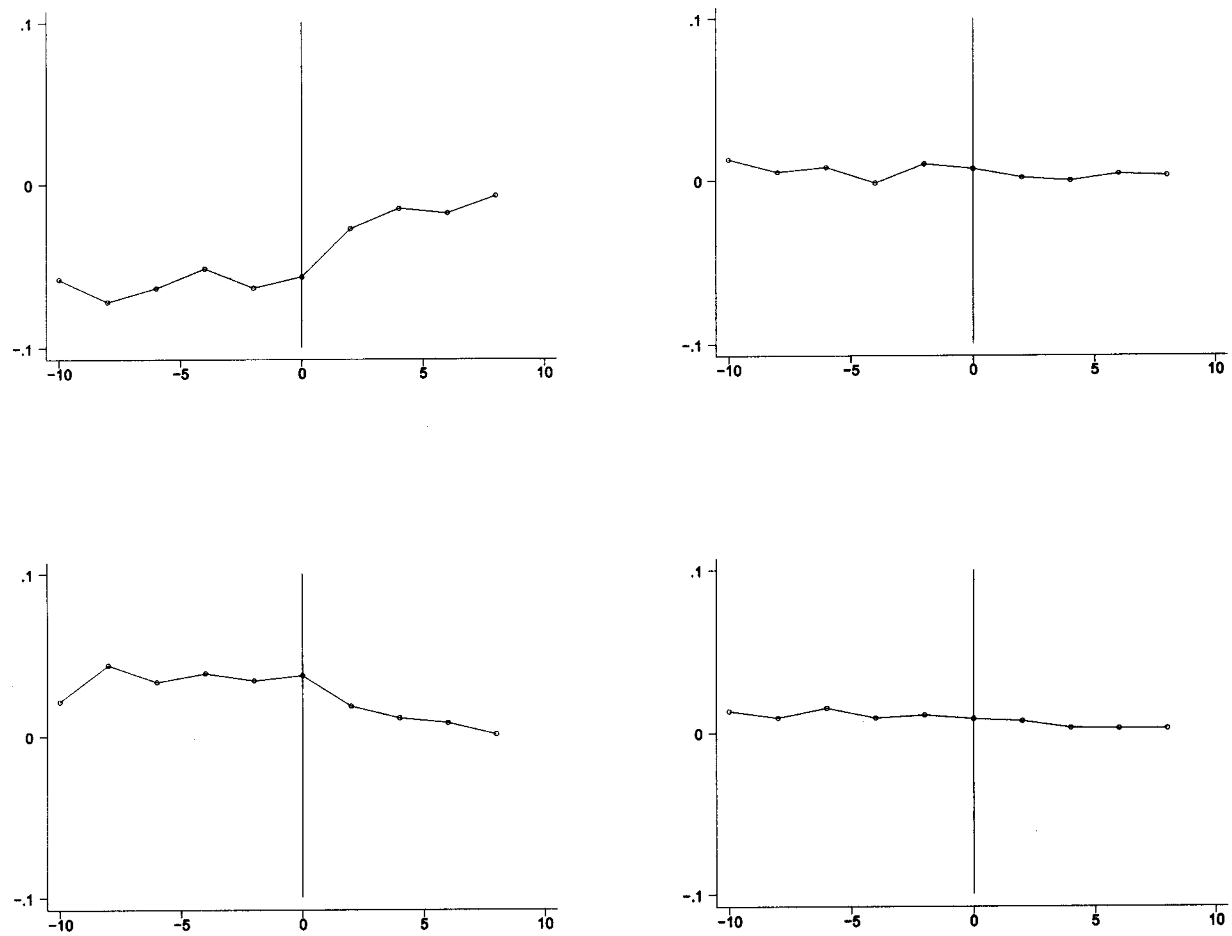


Figure 2: Average Years of Schooling of Women, Before and After the Opening of a Four Year College



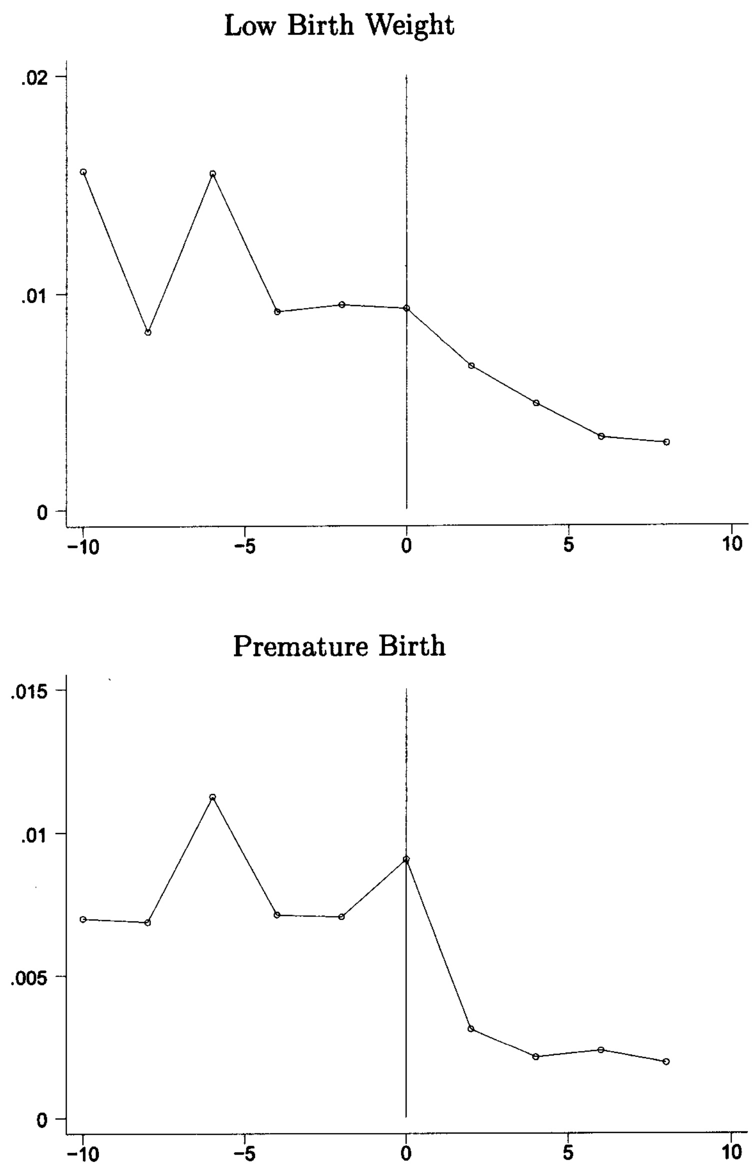
Notes: The Figure shows average years of schooling in the years preceding and the years following the college opening, controlling for county dummies, and cohort dummies. The figures includes only college openings that occurred at least 10 years apart from other openings in the same county. Time 0 is when the college opens and the mother is 17 years old.

Figure 3: Probability of College, Some College, High School and Less than High School, Before and After the Opening of a Four Year College



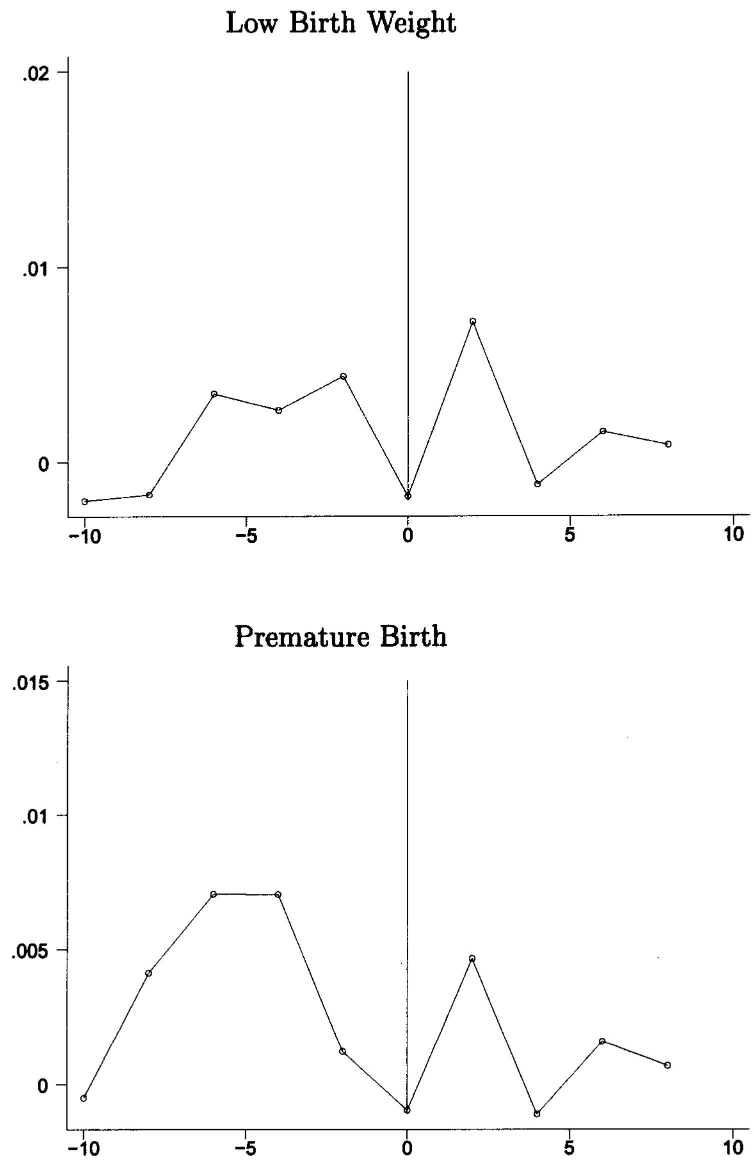
Notes: The top-left panel shows the conditional probability of college education in the years preceding and the years following the opening of a four year college, controlling for county dummies and mother cohort dummies. The top-right panel is for commuity college; the bottom left panel is for high school; the bottom-right panel is for less than high school. Time 0 is when the college opens and the mother is 17 years old. The figures includes only college openings that occurred at least 10 years apart from other openings in the same county.

Figure 4: Average Birth Outcomes, Before and After the Opening of a Four Year College - Mother is 17 When the College Opens



Notes: The top panel shows the conditional probability of low birth weight in the years preceding and the years following the college opening, controlling for county dummies, and cohort dummies. The bottom panel shows the conditional probability of premature birth in the years preceding and the years following the college opening, controlling for county dummies, and cohort dummies. In both figures, time 0 is when the college opens and the mother is 17 years old. The figures include only college openings that occurred at least 10 years apart from other openings in the same county.

Figure 5: Average Birth Outcomes, Before and After the Opening of a Four Year College - Mother is 25 When the College Opens



Notes: The top panel shows the conditional probability of low birth weight in the years preceding and the years following the college opening, controlling for county dummies, and cohort dummies. The bottom panel shows the conditional probability of premature birth in the years preceding and the years following the college opening, controlling for county dummies, and cohort dummies. In both figures, time 0 is when the college opens and the mother is 25 years old. The figures include only college openings that occurred at least 10 years apart from other openings in the same county.

Data Appendix

a) Quality of Vital Statistics Data

States began reporting maternal education at different points, so that we do not have a balanced panel of states. Appendix Table A3 shows which states that did not report maternal education over time. In addition, it is important to note that the National Vital Statistics data do not report birth intervals after 1993, so that we cannot incorporate later waves of the Vital Statistics into our panel. Vital Statistics did not begin reporting smoking until 1989. In 1989, 43 states report. States that do not report include CA, IN, LA, NB, NY, OK, SD. By 1992, the states that were not reporting were down to CA, IN, NY, SD. In the most recent years, only CA and NY are missing. Finally, husband education is not reported in detail after 1991. Models examining smoking or husband education are estimated using a smaller data set than those examining other outcomes.

While we have no reason be believe that education is systematically misreported in the Vital Statistics data, we have compared education in the Vital Statistics to Census reports. The results are in Appendix Table A1. White mothers 24-45 with parity equal to 1 are included. The first row reports the distribution of education in all years of the Natality Files. These figures correspond to the sample sizes reported at the bottom of Table 1. Note that the low percentage of high-school drop outs and the high percentage of college graduates reflect the fact that the sample includes first-time mothers 24 or older, who tend to be more educated than the average. The remaining rows report the distribution of education in Census years. Columns 1 to 4 show the distribution of maternal education in the Natality Files. Columns 5 to 8 show the distribution of education in the Census. The Natality File figures are generally consistent with Census figures, although the percentage of college graduates tends to be somewhat higher in the Natality Files, while the percentage of high school drops out tend to be lower.

b) Changes in Years of Schooling in the Vital Statistics Panel.

In the longitudinal sample, we include all white women aged 16 to 45 at first birth. We assign all women to a given cell based on the woman's year and state of birth, her county of residence, the first child's birth year, the child's birth month, and the interval between births (which was reported on the birth certificate until 1993). We then drop all cells that have more than one mother. (We have repeated the analysis keeping all cells, and found similar results). Appendix Table A2 shows that 24.3% of women in the panel report changes in education between their first and second births. About 11% of women who reported negative changes were deleted from this sample. This is a lower bound of the measurement error in years of schooling.

c) Construction of the College Openings Data Set

We began with a listing of accredited two and four year institutions that existed in fall 1996 from the National Center for Education Statistic's Integrated Postsecondary Education Data System (IPEDS) and excluded very small schools (those with less than 200 students in 1996). For four-year colleges, we then searched the Peterson's Guide to Four-Year Colleges (1999) and the Barron's Profiles of American Colleges (1996) for information about the starting date of each college. If the college was listed in both sources and the founding date was the same in the two guides, then we accepted that date. If these conditions were not met, then we searched the internet for information about the founding date of the school.

In reading through college histories, we attempted to choose a date that was as close as possible to the date when actual undergraduate academic instruction began. For example, if the university began as a vocational school and later added academic instruction, we chose the later date. However if the school began as a "Normal school" or a teacher's college we did use that date. Similarly, if the founding date of the school was listed as the date when land was purchased, we used the date when instruction actually began. If the school began as a graduate school or divinity school and later added undergraduate instruction, we used the later date. If a university was formed through the merger of two schools, we used the date at which it opened its doors to the public.

We excluded the following categories of schools: Schools of psychology, law schools, seminaries, Bible colleges and other mainly religious schools, schools that offer only distance or only internet learning, medical schools and medical centers, graduate schools and schools that were purely research facilities, and foreign universities offering degrees in the U.S. in their native languages.

Information on two-year colleges was collected in a similar fashion. There were essentially two types of two-year colleges that existed in 1996: Those that offered one and/or two year degree programs as well as transfer programs to four year schools, and those that offered only very specific vocational programs. However, we found that most two-year colleges offering broader programs now began as vocational institutions. Thus, for the sake of consistency, we decided to keep all two year colleges, excluding only hairdressing and beauty schools. Excluding institutions that did not offer an Associates Degree had very little effect on our results, however.

An important potential problem is that some schools that existed over part of our sample period may have exited the sample by 1996. In order to assess the severity of this problem, we used data generously supplied by David Card about colleges that were in existence at 5 year intervals over our sample period. This data base was compiled using the Department of Education's CASPAR data base. We estimate that roughly 11% of schools that were in existence at some point had exited by 1996. Adding these schools to our data base, did not affect the estimates reported below.

Our instrumental variable is the number of colleges which existed in the woman's county when she was 17 years old divided by the estimated number of 18 to 22 year olds in the county in that year. Since population numbers are not available by age group, county, and year, we impute the number of 18 to 22 year olds in the county using the county population in each year (interpolating between Census years) and information about the number of 18 to 22 year olds in the state in each Census year. That is, we assume that 18 to 22 year olds are distributed across each state roughly in proportion to county populations. We have experimented with two alternative imputation strategies, and found similar results.

d) *Census Data*

We use the 1980 and 1990 5% sample of the Census (PUMS) to identify place residence in 1975 and 1980 (in 1980 Census) and 1985 and 1990 (in 1990 Census). The main limitation of the data is that counties are not reported. The smallest geographical identifiers available in the PUMS are called Public Use Microdata Areas, or PUMAs and identify areas of no less than 100,000 population. In rural areas, PUMAs are larger than counties, while in densely populated areas, PUMAs are smaller than counties. For example, for the nine-county San Francisco Bay Area, there are 48 PUMAs. The PUMS also report metropolitan areas identifiers, although the definition changes between 1980 and 1990. We assign individuals located in metropolitan areas the total number of colleges and of individuals 18-22 in the metropolitan area. We make the metropolitan area definition in 1990 consistent with the one in 1980. The codes are available on request. We assign individuals located outside metropolitan areas, the total number of colleges and of individuals 18-22 in the PUMA. In the1980 sample, not everyone was asked about location of residence in 1975, so that the sample size is smaller in 1980 than in 1990.

Appendix Table A1

Comparison of Education of Mothers of any Parity in the Natality Files and the Census

	NATALITY FILES				CENSUS			
	College (1)	Some College (2)	High School (3)	Less than High School (4)	College (5)	Some College (6)	High School (7)	Less than High School (8)
All Years (1970-99)	40%	27%	30%	3%				
1970, 1980, 1990	38%	26%	32%	4%	33%	30%	31%	5%
1990	40%	27%	30%	3%	35%	34%	26%	4%
1980	37%	26%	34%	3%	30%	26%	36%	6%
1970	33%	20%	41%	6%	26%	18%	42%	12%

Notes: Columns 1 to 4 show the distribution of maternal education in the Natality Files. First time white mothers 24-45 are included. Columns 5 to 8 show the distribution of education in the Census. First time white mothers age 24-45 who had a child in the last year are included.

Appendix Table A2: Distribution of Changes in Years of Schooling for Mothers in the Panel

	Percent of Sample	Percent who is HS Drop Out at Second Birth	Percent who is HS Graduate at Second Birth	Percent who has Some College at Second Birth	Percent who is College Grad. at Second Birth
Change in years of schooling = 0	75.7	10	47	16	24
Change in years of schooling = 1	12.6	18	23	38	20
Change in years of schooling = 2	5.5	9	35	32	16
Change in years of schooling = 3	2.6	5	35	31	27
Change in years of schooling = 4	2.3	1	15	9	74
Change in years of schooling = 5	0.8	1	7	14	78
Change in years of schooling = 6	0.1	1	11	29	57

Appendix Table A3: States Missing Maternal Education in the Vital Statistics Data, By Year

1969	AL	AR	CA	CT	DE	DC	FL	GA	ID	MD	NM	PA	TX	WA
1970	AL	AR	CA	CT	DE	DC		GA	ID	MD	NM	PA	TX	WA
1971	AL	AR	CA	CT	DE	DC		GA	ID	MD	NM	PA	TX	WA
1972	AL	AR	CA	CT		DC		GA	ID	MD	NM	PA	TX	WA
1973	AL	AR	CA						ID	MD	NM	PA	TX	WA
1974	AL	AR	CA						ID		NM	PA	TX	WA
1975	AL	AR	CA						ID		NM	PA	TX	WA
1976		AR	CA						ID		NM		TX	WA
1977		AR	CA						ID		NM		TX	WA
1978											NM		TX	WA
1979											NM		TX	WA
1980-1988													TX	WA
1989-1991														WA
1992+							All states report.							

Appendix 2: IV and OLS in a Model with Spillovers

We show that OLS and IV estimates differ when spillovers are introduced. This is a simplified version of an argument that originally appeared in Lochner and Moretti (2002) Consider the a simple model of birth outcomes with county-level spillovers:

$$y_{ic} = \alpha + \beta S_{ic} + \gamma S_c + \varepsilon_{ic},$$

where y_{ic} is the birth outcome of mother i in county c; S_{ic} is a dummy equal one if individual i is a college graduate; S_c represents college graduation rate in county c; and ε_{is} is a mean zero random error term assumed to be independent of S_{is} and S_s. To simplify the analysis, assume we have a balanced panel with n individuals in each of C Counties. We also define S the average graduation rate in the entire economy.

OLS estimates of a regression of y_{ic} on S_{ic} is

$$\begin{aligned}
\hat{\beta}_{ols} &= \beta + \gamma \left(\frac{\sum_c \sum_i (S_{ic} - S)(S_c - S)}{\sum_c \sum_i (S_{ic} - S)^2} \right) + \frac{\sum_c \sum_i \varepsilon_{ic}^2}{\sum_c \sum_i (S_{ic} - S)^2} \\
&= \beta + \gamma \left(\frac{\sum_c (S_c - S)(S_c - S)}{\sum_c \sum_i (S_{ic} - S)^2} \right) + \frac{\sum_c \sum_i \varepsilon_{ic}^2}{\sum_c \sum_i (S_{ic} - S)^2} \\
&\to \beta + \gamma \left(\frac{V(S_c)}{V(S_{ic})} \right).
\end{aligned}$$

IV estimates, using a valid county-level instrument z_c (with sample mean z) are:

$$\begin{aligned}
\hat{\beta}_{IV} &= \beta + \gamma \left(\frac{\sum_c \sum_i (z_c - z)(S_c - S)}{\sum_c \sum_i (z_c - z)(S_{ic} - S)} \right) + \frac{\sum_c \sum_i (z_c - z)\varepsilon_{ic}}{\sum_c \sum_i (z_c - z)(S_{ic} - S)} \\
&= \beta + \gamma \left(\frac{\sum_c (z_c - z)(S_c - S)}{\sum_c (z_c - z)(S_c - S)} \right) + \frac{\sum_c \sum_i (z_c - z)\varepsilon_{ic}}{\sum_c \sum_i (z_c - z)(S_{ic} - S)} \\
&\to \beta + \gamma.
\end{aligned}$$

For $\gamma > 0$, we will observe $\beta_{IV} \geq \beta_{ols}$ since $V(d_s) \leq V(d_{is})$. For small cross-county variation in graduation rates (i.e. $V(d_s) \approx 0$), OLS will estimate the own-effect of drop out on criminal participation while IV estimates using state-level instruments will estimate the combined own-effect and spillover effect of average drop out rates.

Defining y_c the average birth outcome in a county, estimates from a regression of y_c on average graduation rates in a county will produce estimates of the combined own and spillover effects, since $y_c = \alpha + (\beta + \gamma)S_c + \varepsilon_c$.