Parental Education and Child Health: Evidence from a Natural Experiment in Taiwan

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December 2003
(Preliminary: Do not cite)

Abstract:

Many studies have documented that parental education, especially maternal education, has a significant positive impact on child health. This study exploits a natural experiment to estimate the causal impact of education in Taiwan. In 1968, the Taiwan government extended compulsory education from six to nine years, which required all school-age children (between six and fifteen) to attend elementary school for six years and junior high school for three years. To accommodate the expected increase in enrollment in junior high schools, the government opened 140 new junior high schools, a seventy-percent increase, in 1968. This education reform created the largest expansion in junior high school constructions and student enrollment in Taiwan. Our natural experiment exploits variations across cohorts in exposure to compulsory education reform and across regions in newly established school density. We estimate the impact of mother's education on child health by using cohort and newly established school density interactions as instruments for parents' education. Our main data are annual birth and death certificates from 1978 to 1999. Our 2SLS estimates suggest that mother's schooling has larger effects on child health outcomes than father's schooling. Parental schooling reduces the probability of low birthweight, very low birthweight and prematurity, but has no significant impact in lowering neonatal, infant and postneonatal mortality.

I. Introduction

Improvements in child well-being are widely accepted public policy goals in developing and developed countries. Not only are these improvements viewed as desirable in their own right, but there is mounting evidence that schooling and health--two key components of well-being and of the stock of human capital--are crucial determinants of economic growth (Topel 1999; Bloom, Canning, and Sevilla 2001, 2002; Krueger and Lindahl 2001). Many studies point to the importance of family background in general and mother's schooling in particular in child health (Haveman and Wolfe 1995; Grossman and Kaestner 1997 and contained in the Appendix of this application; Behrman 1999). This finding is related to a larger literature that shows that an individual's own schooling is positively related to his or her own health (Grossman 1972a, 1972b, 1975; Grossman and Kaestner 1997; Grossman 2000 and contained in the Appendix of this application). The positive correlation between mother's schooling and child health in numerous studies was one factor behind the World Bank's campaign in the 1990s to encourage increases in maternal education in developing countries (World Bank 1993). In a 2002 issue of Health Affairs devoted primarily to the nonmedical determinants of health, Deaton (2002) argues that policies to increase education in the U.S. and to increase income in developing countries are very likely to have larger payoffs in terms of health than those that focus on health care, even if inequalities in health rise. Since more education typically leads to higher income, policies to increase the former appear to have large returns for more than one generation throughout the world.

Efforts to improve the health of an individual by increasing the amount of formal schooling that he or she acquires or that try to improve child well-being by raising maternal schooling assume that the schooling effects reported in the literature are causal. A number of investigators have argued, however, that omitted “third variables” may cause schooling and health or well-being to vary in the same direction. For example, Fuchs (1982) argues that persons who are more future oriented (who have a high degree of time preference for the future or discount it at a modest rate) attend school for longer periods of time and make larger investments in their own health and in the well-being of their children. Thus, the effects of schooling on these outcomes are biased if one fails to control for time preference. The time preference hypothesis is analogous to the hypothesis that the positive effect of schooling on earnings, explored in detail by Mincer (1974) and in hundreds of studies since his seminal work (see Card 1999, 2001 for reviews of these studies), is biased upward by the omission of ability. Behrman and Rosenzweig (2002) present an argument that is even more closely related to ability bias in the earnings-schooling literature. In their model parents with favorable heritable endowments obtain more schooling for themselves, are more likely to marry each other, and raise children with higher levels of well-being. In turn these endowments reflect ability in the market to convert hours of work into earnings and childrearing talents in the nonmarket or household sector.

Governments can employ a variety of policies to raise the educational levels of their citizens. These include compulsory schooling laws, new school construction, and targeted subsidies to parents and students. If proponents of the third variable hypothesis are correct, evaluations of these policies should not be based on studies that relate adult health or child well-being to actual measures of schooling because these measures may be correlated with unmeasured determinants of the outcomes at issue. In this paper we propose to use techniques that correct for third-variable bias to evaluate the effects of a policy initiative that radically altered the school system in Taiwan and led to a dramatic increase in the amount of formal schooling acquired by the citizens of that country during a period of very rapid economic growth.

The paper proceeds as follows. Second II provides some background on education reform in 1968 in Taiwan, followed by a review of the related literature in Section III. Section IV outlines a conceptual framework. Section V describes the empirical strategy and empirical specification. Section VI reports the estimation results and Section VII concludes.

II. Background

A. 1968 Education Reform in Taiwan

In 1968, the Taiwan government extended compulsory education from six to nine years, which required all school-age children (between 6 and 15) to attend elementary school for six years and junior high school for three years. To accommodate the expected increase in enrollment in junior high schools, the government opened 140 new junior high schools, a seventy-percent increase, at the beginning of the academic year 1968-69. This education reform created the largest expansion in junior high school construction and student enrollment in Taiwan's history (Chang 1991; Clark and Hsieh 2000. Unless otherwise noted the material in this section is drawn from these sources, especially from Clark and Hsieh. Note, however, that we have revised some of their data.).

Primary school education in Taiwan was nearly universal by the mid-1960s, but approximately one-half of primary school graduates did not obtain additional education because enrollment in junior high school was restricted by a competitive national examination and by the limited number of junior high schools, especially in rural areas. The 1968 reform abolished the junior high school entrance examination and made it possible for all primary school graduates to continue their education. Children who had previously ended their education after primary school also were allowed to continue their education as long as they were under the age of 15 in 1968 but were unlikely to do so.

The large number of new junior high schools that opened in 1968 increased the number of these schools from 0.8 schools per thousand primary school graduates in the academic year 1967-1968 to 1.3 schools per thousand graduates in the academic year 1968-1969 (see Figure 1). The immediate impact was to increase the percentage of primary school graduates who entered junior high school from 56 percent in 1967 to 77 percent in 1968 (see Figure 2).

Chang (1991) estimates that the 1968 compulsory schooling law increased the mean number of years of formal schooling completed by members of the labor force by 1.3 years by 1978 and by 3.4 years by 1988. Concurrently, there was a two-fold increase in real per capita gross domestic product in U.S. dollars from 1968 to 1978 and a four-fold increase from 1968 to 1988 (Heston, Summers, and Aten 2002). Hence, growth in the two decades following the nine-year compulsory schooling law was much more rapid than the two-fold expansion in real per capita gross domestic product in the two decades prior to its enactment.

A notable aspect of the school construction program was that its intensity varied across regions of Taiwan. Table 1 contains the number of new junior high schools that opened in 1968 per thousand children between the ages of 12 and 14 in 1967 in each of the 21 cities or counties of Taiwan.[1] Program intensity varied from 0.02 in Kaohsiung City to 0.76 in Penghu County. Hence, the nine-year compulsory schooling legislation provides a “natural experiment” to evaluate the impacts of parents’ schooling on the health of their children. In particular, those over the age of 11 in 1968 were unlikely to be affected by school reform and constitute a control group. On the other hand, those 11 years of age and under in 1968 were very likely to have been affected by school reform and constitute a treatment group. Moreover, the effects of school reform on the number of years of formal schooling completed in the treatment group should be larger the larger is the program intensity measure in city or county of birth. Clark and Hsieh (2000) present strong evidence in support of this proposition for men, and we present similar evidence for women in Section V. Thus, we employ the products of cohort indicators and the program intensity measure in Table 1 as instruments for schooling. Greater intensity among younger cohorts should lead to more schooling but should be uncorrelated with unmeasured determinants of the well-being of the offspring of these cohorts. Unlike Clark and Hsieh (2000) who are forced to predict schooling based on county of current residence of males between the ages of 30 and 50, we have information on county of birth. Based on our computations from the Taiwan Panel Survey of Family Dynamics, less than 10 percent of the population attended junior high school in a county that differed from their county of birth in the period after 1968.

Finally, we present some evidence that underscores the validity of our instrument. Taiwanese authorities planned to allocate more new junior high schools in regions where

[1] In Taiwan large cities are separate local entities. Hence Taipei County pertains to the region outside of Taipei City. Unlike Clark and Hsieh (2000), we present separate program intensity data for Taipei City, Kaohsiung City, Taichung City, and Tainan City. This is appropriate

initial enrollment in junior high school was low (Clark and Hsieh 2000). This suggests that unmeasured determinants of schooling might be correlated with program intensity. Figure 3, however, shows no relationship between the junior high school enrollment rate in the county in 1966 and program intensity. Figure 4 shows no relationship between intensity and an alternative measure of schooling needs--the percentage of the labor force in agriculture in 1967. These two figures suggest that program intensity was random rather than systematically correlated with initial schooling levels. They imply no regional differences in the growth of schooling over time.

B. Data and Sample

Our data collection consists of all birth certificates and infant and child death certificates for the years 1978 through 1999. There were more than 300,000 births a year in Taiwan during this period. Birth and death certificates will be linked through national identification numbers received by each person born in Taiwan. We consider the following outcomes from these data: the probabilities of low- (less than 2,500 grams) and very low-weight (less than 1,500 grams) births; the probabilities of neonatal, postneonatal, and infant, and preterm births.

Low and very-low birthweights have extremely strong associations with infant morbidity and mortality. In a proximate sense, birthweight and low birthweight are caused by prematurity and slow growth in utero. Premature births have gestational ages (the difference between the date of birth and the date of the mother's last menstrual cycle) of less than 37 weeks. The general consensus in the clinical literature is that relatively little is known about the causes of preterm delivery (for example, Hack and

because children who reside in Kaohsiung City, for example, do not attend school in the part of Kaohsiung County that is outside the city.

Merkatz 1995) and that most interventions designed to prevent these deliveries are not successful (for example, Goldenberg and Rouse 1998). On the other hand, fetal growth offers more scope for intervention since it is known to be linked to maternal smoking and maternal weight gain (see Joyce 1999 for a review of this evidence). Neonatal deaths pertain to deaths within the first month of life, while postneonatal deaths pertain to deaths between the ages of one month and one year. Infant deaths are the sum of those occurring in the neonatal and postneonatal periods. We distinguish between neonatal and postneonatal mortality because their causes are very different. Most neonatal deaths are caused by congenital anomalies, prematurity, and complications of delivery, while most postneonatal deaths are caused by infectious diseases and accidents. Infants who die within the first month of life will be excluded from estimation for the probability of postneonatal death.

In addition to birthweight and gestational age, birth certificates contain the following information that is relevant for our research: gender; parity; mother’s city or county of birth; father’s city or county of birth; mother’s marital status; mother’s age; mother’s schooling; father’s age; and father’s schooling.

Throughout our study, we consider the women (or men) who were between the ages of less than 1 and 20 in 1968 and between the ages of 22 and 45 when they (or their wives) gave birth in the period from 1978 through 1999. We will use all births for estimation.

III. Literature Review

Many studies point to the importance of family background in general and mother’s schooling in particular in child health and educational outcomes in developed and developing countries (Edwards and Grossman 1979, 1983; Shakotko, Edwards, and Grossman 1983; Haveman and Wolfe 1995; Grossman and Kaestner 1997; Behrman 1999; Glewwe 1999). This finding is related to a larger literature that shows that an individual's own schooling is positively related to his or her own health (Grossman 1972a, 1972b, 1975; Grossman and Kaestner 1997; Grossman 2000). Parents' schooling also may have important indirect effects on their children's educational attainment because poor health in early childhood has been linked to unfavorable educational outcomes (Edwards and Grossman 1979; Shakotko Edwards, and Grossman 1983; Chaikind and Corman 1991; Currie 2000; Alderman et al. 2001).

The research just mentioned underscores parents' schooling as the key determinant of the intergenerational transmission of well-being, as reflected by the health and education of children. While most of the studies control for many variables that are correlated with schooling, the causal nature of the schooling effect has been questioned on the grounds that schooling is endogenous. Hence, omitted "third variables" such as a future orientation or heritable endowments may be correlated with adult schooling, adult health, and child well-being (Fuchs 1982; Behrman and Rosenzweig 2002). In the case of adults, Fuchs (1982) measures time preference in a telephone survey by asking respondents questions in which they chose between a sum of money now and a larger sum in the future. He is not able to demonstrate that the schooling effect is due to time preference. However, the results must be regarded as preliminary because they are based on one small sample of adults on Long Island and on exploratory measures of time preference.[2]

[2] Becker and Mulligan (1997) develop a model in which time preference is endogenous and caused by schooling. In their model schooling might have no effect on health or educational

Behrman and Rosenzweig (2002) control for unmeasured third variables by examining differences in years of formal schooling completed by the offspring of 424 female and 244 male identical (monozygotic) twins in the U.S. While mother's schooling has a positive and significant effect on children's schooling in the cross section, the within-twin estimate either is insignificant or negative and marginally significant. On the other hand, the coefficient of father's schooling is positive and significant in both cases. They argue that their findings with regard to mother's schooling may be attributed to the increased amount of time that educated women spend in the labor market and consequently the reduced amount of time that they spend with their children.

While the study by Behrman and Rosenzweig is novel and provocative, several considerations suggest that their findings should not be viewed as definitive. First, it is based on a small sample. Second, differencing between twins exacerbates biases due to measurement error in schooling (Griliches 1979; Bound and Solon 1999; Neumark 1999), although Behrman and Rosenzweig do attempt to adjust for these biases. Third, it is well known that more educated women have fewer children (for example, Becker 1981) so that their increased time in the labor market does not necessarily mean that they spend less time with their offspring than less educated women. Finally, Bound and Solon (1999) stress that variation in schooling between identical twins may be systematic rather than random.

Our research is related to a large literature that uses the technique of

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attainment with an endogenous measure of time preference held constant, but schooling might have a very important indirect or reduced form impact that operates through time preference. Even with an adequate measure of time preference, estimation of their model would be difficult if instrumental variables to investigate the causal impact of schooling on earnings (see Card 1999, 2001 for reviews) and to a much smaller literature that employs the same technique to investigate the causal impact of adult schooling on adult health or of parents' schooling on children's health. The idea here is to find variables, termed instruments, that are correlated with schooling but not correlated with third variables such as ability, other inherited genetic traits, and time preference. In the context of two-stage least squares estimation and its variants, the instruments are used to predict schooling in the first stage. Then predicted schooling replaces actual schooling in the earnings or health equation.

In the health area, the earliest studies to employ the instrumental variables (IV) technique are by Berger and Leigh (1989), Sander (1995a, 1995b), and Leigh and Dhir (1997). These four studies are discussed in detail by Grossman (2000). All four studies employ U.S. data and pertain to adults. In almost all cases the IV coefficients are at least as large in absolute value as the ordinary least squares coefficients. These findings point to causal impacts of schooling on health. This conclusion should be interpreted with caution because some of the instruments employed (parents' schooling, parents' income, and cognitive test scores) may be correlated with omitted third variables.

Very recent work by Lleras-Muney (2002), Adams (2002), Arendt (2002), Arkes (2001), Currie and Moretti (2002), Breierova and Duflo (2002), and de Walque (in progress) address the schooling-health causality issue by using compulsory education laws, new primary school construction, unemployment rates during a person's teenage

one wanted to allow that measure and schooling to be endogenous and to allow schooling to enter the structural well-being equation.

years, or new college openings to obtain consistent estimates of the effect of schooling on health. Lleras-Muney (2001) employs state-specific compulsory education laws in effect from 1915 to 1939 to obtain consistent estimates of the effect of education on mortality in synthetic cohorts of successive U.S. Censuses of Population for 1960, 1970, and 1980. This instrument is highly unlikely to be correlated with unobserved determinants of health, especially because she controls for state of birth and other state characteristics at age 14. Her ordinary least squares estimates suggest that an additional year of schooling lowers the probability of dying in the next ten years by 1.3 percentage points. Her IV estimate is much larger: 3.6 percentage points.

Adams (2002) uses the same instrument as Lleras-Muney in the first wave of the Health and Retirement Survey, conducted in 1992. He restricts his analysis to individuals between the ages of 51 and 61 and measures health by functional ability and self-rated health. He finds positive and significant effects of education on these positive correlates of good health and larger IV coefficients than the corresponding OLS coefficients.

Arendt (2002) capitalizes on compulsory school reform in Denmark in 1958 and 1975 to study the impact of schooling on self-rated health in the 1990 and 1995 waves of the Danish National Work Environment Cohort Study. Respondents were between the ages of 18 and 59 in 1990. His results are similar to those of Adams.

Arkes (2001) focuses on white males aged 47 to 56 in the 1990 Census of Population. His instrument for schooling is the state unemployment rate during a person's teenage years. With state per capita income held constant, he argues that a higher unemployment rate should lead to greater educational attainment because it reduces the opportunity cost of attending school. From two-stage least squares probit models, he finds that an additional year of formal schooling lowers the probability of having a work-limiting condition by 2.6 percentage points and reduces the probability of requiring personal care by 0.7 percentage points. Both estimates exceed those that emerge from probit models that treat schooling as exogenous.

Currie and Moretti (2002) examine the relationship between maternal education and birthweight among U.S. white women with data from individual birth certificates from the Vital Statistics Natality files for 1970 to 2000. They use information on college openings between 1940 and 1990 to construct an availability measure of college in a woman's 17th year as an instrument for schooling. They find that the positive effect of maternal schooling on birthweight increases when it is estimated by instrumental variables. They also find that the negative IV coefficient of maternal schooling in an equation for the probability of smoking during pregnancy exceeds the corresponding OLS coefficient in absolute value. Since prenatal smoking is the most important modifiable risk factor for poor pregnancy outcomes in the United States (U.S. Department of Health and Human Services 1990), they identify a very plausible mechanism via which more schooling causes better birth outcomes. Finally, parity falls and the probability of being married rises as maternal education rises. The OLS coefficients are somewhat larger than the IV coefficients, although both sets are significant. These results suggest other mechanisms via which more schooling leads to better infant health outcomes.

Breierova and Duflo (2002) capitalize on a primary school construction program in Indonesia between 1973 and 1978. In that period 61,000 primary schools were constructed. Program intensity, measured by the number of new schools constructed per primary-school age child in 1971, varied considerably across the country's 281 districts. Hence this school construction program bears much in common with the 1968 Taiwan program that is the focus of our research. In a study of the effects of schooling on earnings, Duflo (2001) shows that average educational attainment rose more rapidly in districts where program intensity was greater. She also argues that the program had a bigger effect for children who entered school later in the 1970s and no effect for children who entered school before 1974. Therefore, she uses the interaction between year of birth and program intensity as an instrument for schooling for male wage earners in the 1995 intercensal survey of Indonesia who were between the ages of 2 and 24 in 1974. This instrument turns out to be an excellent predictor of schooling.

Breierova and Duflo (2002) use the instrument just described to estimate the effects of mother's and father's education on fertility and child mortality in the same survey employed by Duflo. They employ fertility histories of approximately 120,000 women between the ages of 23 and 50 in 1995. They find that female education is a stronger determinant of age at marriage and early fertility than male education. They also find that mother's and father's schooling have about the same negative effects on infant mortality. Some, but not all, of the IV coefficients exceed the corresponding OLS coefficients. The authors treat their results as very preliminary.

de Walque (in progress) examines the effect of schooling on the probability that males 20 years of age and older smoke cigarettes in Indonesia. His data come from the 1993 Indonesia Family Life Survey. His instrument for the number of years of formal schooling completed is the level of school that was available to an individual in his district when he was of school age. His preliminary results suggest that the schooling effect is larger in absolute value in the IV models, although it is not precisely estimated in some specifications.

The results of the seven very recent studies just reviewed suggest causality from more schooling to better health. The finding that the IV estimates exceed the OLS estimates may arise because the instruments are based on policy interventions that affect the educational choices of persons with low levels of education (Card 2001). If different individuals face different health returns to education, IV estimates reflect the marginal rate of return of the group affected by the policies (Angrist, Imbens, and Rubin 1996). Card (2001) points out: "For policy evaluation purposes…the average marginal return to schooling in the population may be less relevant than the average return for the group that will be impacted by a proposed reform. In such cases, the best available evidence may be IV estimates of the return to schooling based on similar earlier reforms (p. 1157)."

A second explanation of the larger IV than OLS estimates is that the schooling variable contains random measurement error, which leads to a downward bias in the OLS estimates. As long as the instruments for schooling are not correlated with this error, the IV procedure eliminates this bias (Card 1999, 2001). A third explanation is that there may be spillover effects in the sense that the health outcome of an individual depends on the average schooling of individuals in his or her area as well as on his or her own schooling or that of his or her parents (Acemoglu 1996; Acemoglu and Angrist 2000). Currie and Moretti (2002) show that IV estimates of this combined effect based on area-level instruments are consistent, while OLS estimates understate it.

In summary, the seven very recent studies that we have discussed in detail underscore the utility of employing IV techniques with area-level instruments to obtain consistent estimates of the effects of schooling on health and other measures of well-being. We will use IV methods to examine many child health outcomes using a large data set. Our health outcomes will be objective measures obtained from birth certificates, and from merging these certificates with infant and child death certificates. This is in contrast to Breierova and Duflo (2002) who rely on women's reports of deaths of their infants and children. These reports are likely to contain errors. We will examine the effects of increased schooling in Taiwan during its period of rapid economic growth. Clark and Hsieh (2000) have successfully employed the same instrument that we use in research on the impacts of schooling on the earnings of Taiwanese males.

IV. Conceptual Framework

Following Becker (1965), Grossman (1972a, 1972b), Becker and Lewis (1973), Willis (1973), Becker (1981), we employ a model of family decisionmaking in which the utility function of parents depends on their own consumption or standard of living, the number of children in the family, and the well-being of each child. In turn, well-being depends on health and cognitive development outcomes. These outcomes are produced with inputs of goods and services purchased in the market (for example, medical care services in the case of health and books and other types of educational material in the case of cognitive development) and with the parents' own time. There also may be production functions for items that determine the parents' living standard.

Maximization of the utility function subject to production and resource constraints yields demand functions for health and cognitive development outcomes, for the inputs that help determine these outcomes, for the number of children in the family, and for parents' consumption. These demand functions depend on income or wealth, input prices, efficiency in production, tastes including time preference, and heritable endowments of health and cognitive development. Note that some inputs in the production function such as cigarette smoking and excessive alcohol use have negative effects on health and cognitive development. They are demanded at least by some individuals because their negative effects on child well-being are more than offset by their positive contributions to other commodities that enter the utility function. In addition, children may consume these goods with their own resources because their utility functions do not coincide with those of their parents.

Grossman (1972a, 1972b, 1975, 2000) and others show that causality from higher levels of parents' schooling to improved child health or cognitive development results when the more educated are more efficient producers of these outcomes. (In the remainder of this section, statements made pertaining to health also pertain to cognitive development.) This efficiency effect can take two forms. Productive efficiency pertains to a situation in which the more educated obtain a larger health output from given amounts of endogenous (choice) inputs. Allocative efficiency pertains to a situation in which schooling increases information about the true effects of the inputs on health. For example, the more educated may have more knowledge about the harmful effects of cigarette smoking or about what constitutes an appropriate diet.

Increases in allocative efficiency due to higher schooling levels improve health to the extent that they lead to the selection of a better input mix. Increases in productive efficiency raise child health through two mechanisms. First, the "shadow prices" of commodities that enter the utility function fall, and real income or wealth rises with money income held constant. If child health has a positive real income elasticity, the quantity demanded by parents will rise. There may also be a substitution effect in favor of child health if efficiency in the health production function rises by a larger percentage than in other production functions. Since mothers typically allocate more time to childcare than fathers, it is natural to obtain separate estimates of the effects of mother's schooling and father's schooling. Especially in the case of the former, one wants to take account of increases in the wage or the value of time associated with schooling. Willis (1973) assumes that the production of child quality or well-being is more intensive in the wife's time than the production of the parents' standard of living. But child quality rises with the wage while family size falls because child well- being and parents' standard of living are complements in consumption. Becker and Lewis (1973) get the same result for a different reason. In their model, there are fixed costs (costs that do not depend on child quality) associated with the number of children. They show that an increase in the value of the wife's time lowers the price of quality relative to that of number of children, although it raises the price of quality relative to parents' standard of living. Thus, they predict a small negative or even a positive effect of an increase in the wage on child well-being. The point we wish to emphasize is that in both models wage or value of time effects are extremely unlikely to reverse productive efficiency effects and are very likely to reinforce these effects.

In this paper we do not attempt to distinguish between the allocative and productive efficiency effects. In addition, we do not attempt to test the efficiency hypotheses against the alternative hypotheses that education causes health because it changes tastes or time preference in a manner that favors health relative to certain other commodities. Instead, our aim is to investigate whether the education effect is in fact causal or whether it is due to omitted third variables. These variables are relevant because parents' education is endogenous in a fuller model of decisionmaking across several generations. For example, a mother with a favorable health endowment may obtain more schooling and may transmit the endowment to her offspring. Alternatively, a more future oriented mother may obtain more schooling for herself and allocate more resources to improving the health of her offspring.

In the next section we outline and defend an instrumental variables methodology to obtain causal estimates of the effects of parents' schooling on child health and cognitive development. The idea is to predict parents' schooling based on variables that determine the price of schooling such as compulsory minimum schooling laws and school availability in the area in which a person grows up. These variables influence schooling decisions but should not be correlated with unmeasured third variables.

V. Effect of 1968 Education Reform on Education

A. Basic Approach

As indicated in the previous section, we propose to use aspects of the nine-year compulsory schooling law enacted in Taiwan in 1968 as an instrument for schooling. Consider students from families who would only send them to school for six years in the absence of the law. Enactment of the legislation essentially reduces the price of three additional years of schooling for these students because it forces their parents "to become more 'generous'" (Becker 1993, p. 140). Of course the price may not fall to zero because the law may not be fully enforced. But the presumption is that the law should encourage some students who otherwise would not have attended junior high school to do so. The opening of many new junior high schools in Taiwan in 1968 reinforces this effect.

Following Duflo (2001), Clark and Hsieh (2000), and Breierova and Duflo (2002), we will incorporate variations in the intensity of the school program in Taiwan across counties, measured by the county-specific number of new junior high schools that opened in 1968 per thousand children between the ages of 12 and 14 in 1967, in the instrument for schooling. Note that the legislation also may have had effects on the probabilities of completing schooling levels beyond junior high school. Thus, the number of years of formal schooling completed is the most comprehensive measure of its impact.

To construct our instrument, we first form control and treatment groups using women or men between the ages of less than 1 and 20 in 1968. Those between the ages of 15 and 20 were very unlikely to have been affected by school reform and constitute the control group. On the other hand, those under the age of 12 were very likely to have been affected by school reform and constitute the treatment group. Children between the ages of 12 and 14 in 1968 who had previously ended their education after primary school were allowed to return to school but were unlikely to do so. Hence, we also distinguish between this group and those under the age of 12 in some analyses.

The following regression model incorporates the notion that the impact of reform on the treatment group should be larger the larger are the number of new junior high schools that opened in the city or county of residence at the beginning of the 1968 school year:

$$S_{ijt} = \sum_k a_{1k} C_{ik} + b(P_j \times T_i) + \sum_{k=2}^{21} a_{2k} R_{jk} + \sum_{k=79}^{99} a_{3k} Y_{tk} + \sum_k \sum_{l=79}^{99} a_{4k} R_{jk} Y_{tl} + e_{ijt} \,. \qquad (1)$$

Here e S_{ijt} is the number of years of formal schooling completed by mother (or father) i born in city/county j with her/his child born in year t. We also include cohort dummies (C_i), where the number of cohort dummies depends on the definitions of treatment and control groups described below. Region (city or county) of birth dummies (R_j) are included to capture the regional fixed effects. To control for trend effects, we include dichotomous indicators (Y_t) for each year from 1978 to 1999, where 1978 is the omitted year. The regression also includes interactions between region of birth dummies and year dummies to capture different trend effects in different regions. Finally, T_i is a dummy indicating whether mother *i* belongs to the treatment group as described in more details below, and P_j denotes the program intensity in her region of birth. The coefficient b estimates the impact of an additional junior high school (per thousand children aged 12-14) on mother's education in the treatment group. We estimate the equation separately for mothers and fathers.

To evaluate our identification strategy, we form multiple treatment and control groups based on differential impacts of the policy on different cohorts. This idea illustrated by Figure 2 According to the figure , it obviously took time to establish mechanisms to enforce the additional schooling required by the law. The percentage of primary school graduates who entered junior high school rose from 56% in the academic year 1967-68 to 77% in academic year 1968-69, to 87% in academic 1975-76, and to 99% in 1978-79. Thus, the law was not fully effective until the youngest members of the treatment group were about to enter junior high school. Since the 1968 legislation undoubtedly had important lagged effects, it is plausible to expect that the reform had the largest effect on the youngest members of the treatment group. We first estimate models that are limited to 0-5 year olds in 1968 (the treatment group) and 15-20 year olds in 1968 (the control group) and alternatively 6-11 year olds and 15-20 year olds. Our identification strategy will be validated if the increase in schooling is larger among 0-5 year olds than among 6-11 year olds.

We also form our control group using women (or men) aged 12-14 in 1968. As mentioned above, this group was allowed to return for free junior high school education, but was less likely to do so since they may have begun to work. We compare this control group to a treatment group which is restricted to those between the ages of 9-11 in 1968. This comparison (9-11 versus 12-14) yields similar number of births in each group and also minimizes the number of years that must elapse before a treatment group woman gives birth at the same age as a control group woman. Finally, we compare those aged 0-11 in 1968 (treatment group) with those aged 12-20 and 15-20 in 1968 (control groups).

Table 2 presents estimates of the interactions between the treatment dummy and program intensity using all births to these women (or men) who were between the ages of 22 and 45 when they (or their wives) gave birth in the period from 1978 through 1999. Program effects for mother's and father's educational attainment are presented in panels A and B, respectively. In column 1 of panel A, the control group is the cohort aged 15-20 in 1968 and the treatment group is a cohort aged 0-5 in 1968.. The estimate suggests that an additional junior high school (per thousand children aged 12-14) increases mother's education by 1.03 years. In column 2 the estimate for the treatment group of mothers aged 6-11 in 1968 of 0.45 years is smaller. These results strengthen our identification strategy, since the education reform not only has a positive impact on the educational attainment of the treatment groups, but also has a larger impact for the younger women. The program effect is not statistically significant when the treatment group consists of the cohort aged 9-11 and the control group consists of the cohort aged 12-14 (panel A, column 3). This result implies that the 12-14 year olds may not be a pure control group because some individuals in this group might go back to junior high schools after the reform. A comparison of the estimates in columns 4 and 5 yields a similar implication. The impact of the reform on the treatment group is smaller when the control group contains women aged 12-14 (0.332 compared with 0.461).

The estimates in panel B, which pertain to males, are larger and more significant in a statistical sense than the corresponding estimates in panel A. This suggests that the education reform has a bigger impact on father's educational attainment than on mother's educational attainment. For example, column 5 indicates that an additional junior high school per thousand children aged 12-14 increases the number of years of schooling completed by fathers aged 0-11 by 0.78 years compared to 0.46 years for mothers. In addition, the results in panel B suggest that the education reform had a larger impact on the education of younger fathers (i.e. 1.09 years for ages 0-5 and 0.77 years for ages 6-11) which is consistent with our expect.

B. Full Specification

We can generalize eq. (1) to allow for a full set of program intensity and cohort interactions as follows:

$$S_{ijt} = \sum_{k=0}^{19} a_{1k} C_{ik} + \sum_{k=0}^{19} b_k (P_j \times C_{ik}) + \sum_{k=2}^{21} a_{2k} R_{jk} + \sum_{k=79}^{99} a_{3k} Y_{tk} + \sum_{k=2}^{21} \sum_{l=79}^{99} a_{4k} R_{jk} Y_{tl} + e_{ijt} \,.$$

We estimate the above equation separately for mothers and fathers. Parental years of schooling is regressed on 20 cohort dummies, 20 region of birth dummies and interactions between cohort dummies and program intensity for mothers (or fathers) between the ages of less than 1 and 20 in 1968. Mothers (or fathers) aged 20 in 1968 form the control group, and this dummy is omitted from the regression. Each coefficient of b_k can be interpreted as an estimate of the impact of the education reform on a given cohort k. We also include 21 years of child birth year dummies and 420 interactions between region of birth dummies and year of birth dummies.

If there are time-varying and region-specific unobservables that are correlated with the education reform, the estimates of coefficients b_k are biased. For example, if the allocation of new junior high schools is negatively related to the initial enrollment rate or positively related to skilled labor demand, the estimates b_k will underestimate the effect of education reform in the former case and overestimate in the latter case. We have shown in Figures 4 and 5 that the program intensity and junior high school enrollment rate and the percentage of agricultural workers are not correlated. But we still want to control for these omitted effects by including interactions between cohort dummies (C_k, $k = 0, \ldots 19$) and the initial enrollment rate in junior high school in 1966 and interactions between cohort dummies and the percentage of agricultural workers in 1967 in our estimation.

Table 3 shows regression coefficients and t-ratios of the 20 interactions between age in 1968 and program intensity. Controlling for the initial enrollment rate and the percentage of agricultural workers does not affect the estimates of the coefficients significantly, but it improves the efficiency of these estimates. Since the cohort aged 15 and older in 1968 did not benefit from the reform, the cohort aged between 12 and 14 was allowed to return to school but may not do so, and the cohort aged 11 and under was completely exposed to the new policy with strong lagged effect as discussed above, we could expect the pattern of the coefficients b_k to be 0 for $k \geq 15$, to be significantly positive for k = 0 to 11 and decreasing from k = 0 to 11, and to be ambiguous for k = 12 to 14. We plot the coefficients of b_k of mothers in Figure 5 and of fathers in Figure 6 to confirm these predictions.

In Figures 5 and 6, it is obvious that the coefficients b_k start to decrease sharply when k=13, and fluctuates near zero for k=14 to 19. The coefficients b_k are all positive for k=0 to 11, and decrease from 0 to 11. The pattern is consistent with our expectation that the reform had no impact on the cohort not exposed to it, and had the largest effect on the youngest cohort. These results further validate our identification strategy.

In the specification that controls for the enrollment rate and the share of the agricultural worker in Table 3, the average program coefficient in the regression for mother's schooling is 1.33 between the ages of 0 and 5, 1.01 between the ages of 6 and 11, 0.71 between the ages of 12 and 14, and 0.31 between the ages of 15 and 19. The corresponding coefficients in the regressions for father's schooling is 1.07 between the ages of 0 and 5, 0.95 between the ages of 6 and 11, 0.68 between the ages of 12 and 14, and 0.12 between the ages of 15 and 19. Those estimates confirm the patterns shown in Figures 7 and 8 that the magnitude of the coefficients decrease with age in 1968. The sets of coefficients for the 0-5 and 6-11 year olds are significant for both the sample of mothers and fathers. For mothers, the F-ratios are 8.06 and 5.17, respectively. For fathers, the F-ratios are 7.86 and 6.80, respectively. The same tests applied to the 12-14 year olds and 15-20 year olds yield insignificant F-ratios for both mothers and fathers. Given the results shown in Tables 2 and 3, we delete the cohort aged 12-14 from our sample in the following analyses.

C. Restricted Estimation

Since the education reform had a smaller impact on individuals aged 12 and older in 1968, we impose the restriction that b_k are equal to zero for $k \geq 15$ and estimate the following equation separately for mothers and fathers:

$$S_{ijt} = \sum_{k=0}^{19} a_{1k} C_{ik} + \sum_{k=0}^{11} b_k (P_j \times C_{ik}) + \sum_{k=2}^{21} a_{2k} R_{jk} + \sum_{k=79}^{99} a_{3k} Y_{tk} + \sum_{k=2}^{21} \sum_{l=79}^{99} a_{4k} R_{jk} Y_{tl} + e_{ijt} \qquad (2)$$

The reference group comprises of mothers (or fathers) aged 15 to 20 in 1968. Estimates of coefficients b_k are reported in Table 4.

In general, the coefficients decrease with age in 1968. The estimated coefficients are all statistically significant, except for some coefficients reported in column (1). The F-statistics presented at the bottom of the table test the hypothesis that the coefficients of the interaction terms are jointly zero. The F-ratios are 15.73 and 16.96 for mother's and father's samples, respectively, when the enrollment rate and the percentage of agricultural share are employed as regressors. These numbers are larger than the critical values proposed by Bound, Jaeger and Baker (1995), Staiger and Stock (1997), and Stock and Yogo (2002) required to avoid biases in TSLS coefficients due to weak instruments.

For mothers, the estimates in column (2) suggest that on average the cohorts aged 0 and 5 and 6 to 11 in 1968 received 1.00 and 0.72 additional years of education for every junior high school constructed per 1000 children between the ages of 12 and 14, respectively. For fathers, the estimates in column (4) suggest that on average the cohorts aged between 0 and 5 and 6 to 11 in 1968 received 0.84 and 0.77 additional years of education for every junior high school constructed per 1000 children between the ages of 12 and 14, respectively. Clark and Hsieh (1999) report estimates for males aged 6-11 of 0.45 in the 1994-97 Manpower Utilization Survey and 0.95 in the 1994-1997 Survey of Personal Income Distribution. It is comforting that our estimate falls in this range.

Evaluating at the mean, our estimates imply that the education reform increased the years of schooling by xx for women aged 0-11 and by xx for men aged 0-11.

D. Effect of Reform on Different Levels of Education

We examine the impact of the reform at different levels of education by estimating the following linear model for the probability of completing different levels of education:

$$S_{ijt}^{m} = \sum_{k=0}^{19} a_{1k} C_{ik} + \sum_{k=0}^{11} b_k^{m} (P_j \times C_{ik}) + \sum_{k=2}^{21} a_{2k} R_{jk} + \sum_{k=79}^{99} a_{3k} Y_{tk} + \sum_{k=2}^{21} \sum_{l=79}^{99} a_{4k} R_{jk} Y_{tl} + e_{ijt}\,, \quad (3)$$

where S_{ijt}^{m} is a dummy variable that indicates whether the individual i born in city/county j with her child born in year t completed the mth level of education. We consider four levels of education attainment: elementary school or more (6 years +), junior high school or more (9 years +), senior high school or more (12 years +) and university or more (16 years +). The b_k^{m} measures the impact of reform on the mth level of education on a given cohort k, for k = 0 to 11.

Table 5 presents estimates of b_k^{m} and Figures 7 and 8 show the plots of the coefficients. Figure 7 clearly shows that the education reform has the largest positive effect on the completion of junior high school for women, which is the target of the reform. However, the reform induced more men to complete senior high school education (Figure 8). Our results suggest that 1968 education reform had spillover effects. On average an additional junior high school built for every 1000 children in 1968 increased the probability of completing junior high school by 13%, senior high school by 9% and college by 4% for women. For men, the figures are 7%, 14% and 6% for junior high school, senior high school and college, respectively.

VI. Effect of Parental Education on Child Health Outcomes

A. Two-Stage Least Squares Estimates

We have shown in the previous section that the education reform increased the years of formal schooling completed by parents. The next question is whether the higher education attainment of parents leads to better health outcomes of their children. Consider the following equation which relates a child health outcome to parents' schooling and other observable characteristics:

$$H_{ijt} = wS_{ijt} + \sum_{k=1}^{19} a_{1k} C_{ik} + \sum_{k=2}^{21} a_{2k} R_{jk} + \sum_{k=79}^{99} a_{3k} Y_{tk} + \sum_{k=1}^{19} \sum_{l=79}^{99} a_{4k} R_{jk} Y_{tl} + h_{ijt}, \qquad (4)$$

where H_{ijt} represents child health outcome and the coefficient w will measure the impact of parents' schooling on the outcome.

If h_{ijt} in equation (4) and e_{ijt} in equation (1) or (2) are correlated, the application of ordinary least squares (OLS) to equation (4) will produce inconsistent coefficient estimates. Consistent estimates of equation (4) and in particular of the causal effect of S on H can be obtained by the method of instrumental variables (IV). Estimates of equation (1) or (2) in the previous section provide the first stage of the IV estimation. The interactions between the treatment dummy and program intensity or between the cohort dummies and program intensity--which represent the price of schooling--serve as the instrument or instruments for schooling in the IV procedure.

Tables 6 reports the OLS and 2SLS estimates using the IV shown in Table 2. Under OLS estimation, regardless of the definition of treatment and control groups, higher parental educational attainments significantly reduce neonatal mortality, infant mortality, postneonatal mortality, the probability of low birth weight, very low birth weight, and the risk of prematurity. For example, the estimates suggest that an additional year of mother's education reduces neonatal mortality, infant mortality and postneonatal mortality by 0.01 percentage points (0.1 deaths per thousand live births), 0.03 percentage points, and 0.02 percentage points for mothers aged 0 to 11 in 1968, respectively panel A, column 5). It also reduces the probability of low birthweight, very low birthweight and prematurity by 0.18 percentage points, 0.10 percentage points and 0.09 percentage points, respectively. Under the 2SLS estimation, mother's years of schooling shows no significant impacts on neonatal mortality, infant mortality, and postneonatal mortality. Mother's educational attainment significantly reduces the probabilities of low birthweight, very low birthweight, and prematurity (columns 2, 4, and 6 in Panel A). For mothers aged 0 to 11 in 1968, an additional year of mother's schooling reduces the probability of low birthweight, very low birthweight, and prematurity by 1.66 percentage points, 1.13 percentage points, and 1.65 percentage points respectively. The 2SLS estimates are much bigger than the r the OLS estimates. This finding is consistent with the findings in previous studies mentioned in Section III. Explanations of these results are also discussed in that section.

Under the OLS estimation, father's years of schooling has similar effects on child health as mother's years of schooling. However, under TSLS estimation, father's years of schooling has smaller effects than mother's years of schooling in reducing the probability of low birthweight, very low birthweight, and prematurity. An additional year of father's schooling reduces the probabilities of low birthweight, very low birthweight and prematurity by 1.28 percentage points, 0.88 percentage points,, and 0.91percentage points. For neonatal, infant and postneonatal mortalities, father's and mother's years of schooling have similar impacts. Mother's schooling may have a larger impact in the

TSLS estimation than father's schooling because her schooling has a greater impact on efficiency in the production of healthy fetuses and decisions with regard to medical and nonmedical prenatal inputs.

Table 7 presents OLS and TSLS results using the interactions between cohort dummies and program intensity as the IV. Similar to the findings from Table 6, mother's and father's years of schooling have significant impacts on child health when these impacts are estimated by OLS. However, the TSLS estimates show that mother's years of schooling has no impact on mortality but father's years of schooling significantly reduces infant and postneonatal mortality. Both mother's and father's years of schooling significantly reduce the probabilities of low birthweight, very low birthweight, and prematurity. As in Table 6, the mother's schooling coefficients in these outcomes are larger in absolute value than the father's schooling coefficients. These findings are not sensitive to the inclusion of initial enrollment and the percentage of agricultural employment

B. Joint Estimation of Parental Education Effects

Finally, instead of investigating mother's and father's education in separate regressions, we include mother's years of schooling (S^m) and father's years of schooling (S^f) in the same regression as follows:

$$H_{ijt} = w^m S^m_{ijt} + w^f S^f_{ijt} + \sum_{k=0}^{19} a^m_{1k} C^m_{ik} + \sum_{k=2}^{21} a^m_{2k} R^m_{jk} + \sum_{k=79}^{99} a_{3k} Y_{tk} + \sum_{k=0}^{19} \sum_{l=79}^{99} a^m_{4k} R^m_{jk} Y^m_{tl}$$
$$+ \sum_{k=0}^{19} a^f_{1k} C^f_{ik} + \sum_{k=2}^{21} a^f_{2k} R^f_{jk} + \sum_{k=0}^{19} \sum_{l=79}^{99} a^f_{4k} R^f_{jk} Y^f_{tl} + h_{ijt}$$

We also include mother's county of birth (C_m), father's county of birth (C_f), dichotomous indicators for mother's (R_m) and father's (R_f) year of birth or for mother's and father's age in 1968, dichotomous indicators for year of infant's birth (*Y*), and interactions between infant's year of birth and mother's and father's county of birth. Since husbands typically are older than their wives, we include fathers who were aged less than 1 to 25 in 1968 and were 22 to 50 when their wives gave birth. We also exclude cohort aged between 12 and 14 in 1968 for both mothers and fathers. Mother's schooling and father's schooling are both treated as endogenous and are predicted along the lines described in the previous section. To be specific, the instruments for mother's schooling are the 12 interactions between program intensity in her region of birth and cohort dummies. The instruments for father's schooling are 12 interactions between program intensity in his region of birth and cohort dummies. In this procedure the 24 instruments are used to predict both variables. It should be pointed out that the inclusion of father's schooling raises the issue of how to deal with women who are not married. In fact, this is not an important issue in this study, because births to unmarried women accounted for less than 5% of all births in Taiwan in the 1978-1999 period. Births to unmarried women are deleted from the regressions.

In Table 8, we present estimates of the partial effects of father's and mother's education on child health outcomes. , Mother's and Father's schooling have almost similar effects on all health outcomes, when these effects are obtained by OLS. The TSLS estimates suggest, however, that mother's schooling has more significant and bigger impacts than father's schooling the probabilities of very low birthweight and prematurity. An additional year of mother's schooling lowers the probabilities of very low birthweight and prematurity by 0.7 percentage points and 1.28 percentage points, respectively (column 8).

VII. Conclusion

It is well-documented that parents' schooling is the key determinant of the intergenerational transmission of well-being, as reflected by the health of children. While most of the studies control for many variables that are correlated with schooling, the causal nature of the schooling effect has been questioned on the grounds that schooling is endogenous. Hence, omitted "third variables" such as a future orientation or heritable endowments may be correlated with adult schooling and child well-being. This study exploits a natural experiment to estimate the causal impact of mother's and father's education in Taiwan.

Our 2SLS estimates suggest that mother's schooling has larger effects on child health outcomes than father's schooling. Parental schooling reduces the probability of low birthweight, very low birthweight and prematurity, but has no significant impact in lowering neonatal, infant and postneonatal mortality.

We also find that IV estimates in general exceed the OLS estimates. One explanation is that the instruments are based on policy interventions that affect the educational choices of persons with low levels of education (Card 2001). If different individuals face different health returns to education, IV estimates reflect the marginal rate of return of the group affected by the policies (Angrist, Imbens, and Rubin 1996). A second explanation of the larger IV than OLS estimates is that the schooling variable contains random measurement error, which leads to a downward bias in the OLS estimates. As long as the instruments for schooling are not correlated with this error, the IV procedure eliminates this bias (Card 1999, 2000). A third explanation is that there may be spillover effects in the sense that the health outcome of an individual depends on the average schooling of individuals in his or her area as well as on his or her own schooling or that of his or her parents (Acemoglu 1996; Acemoglu and Angrist 2000). Currie and Moretti (2002) show that IV estimates of this combined effect based on area-level instruments are consistent, while OLS estimates understate it.

The evidence that parental schooling improves child health outcomes implies that the returns to education, measured only in terms of earnings increases, are substantially underestimated. Our next step is to examine the pathways of influence of parental education on child health.

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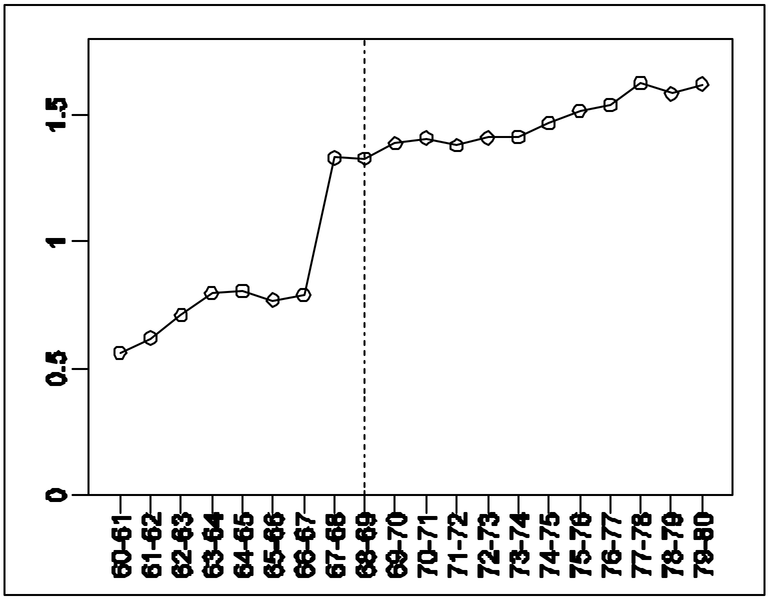
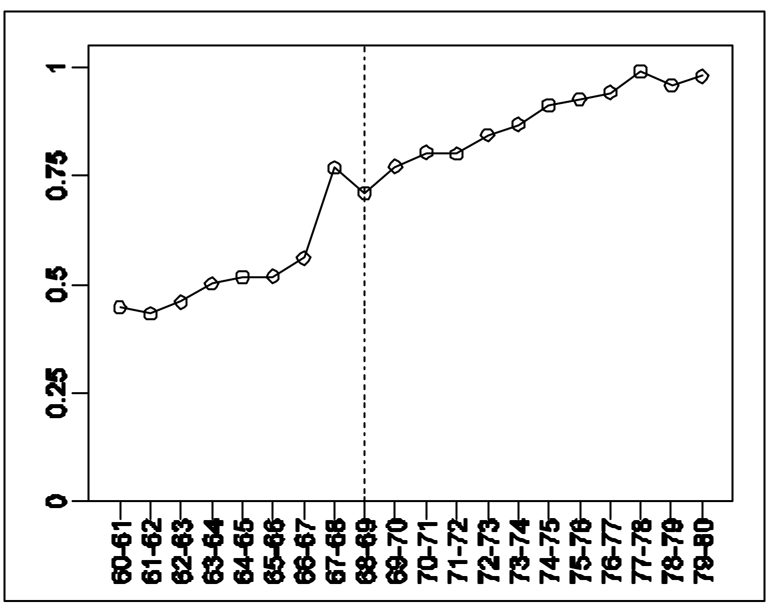
Figure 1: Junior High Schools per Thousand Primary School Graduates



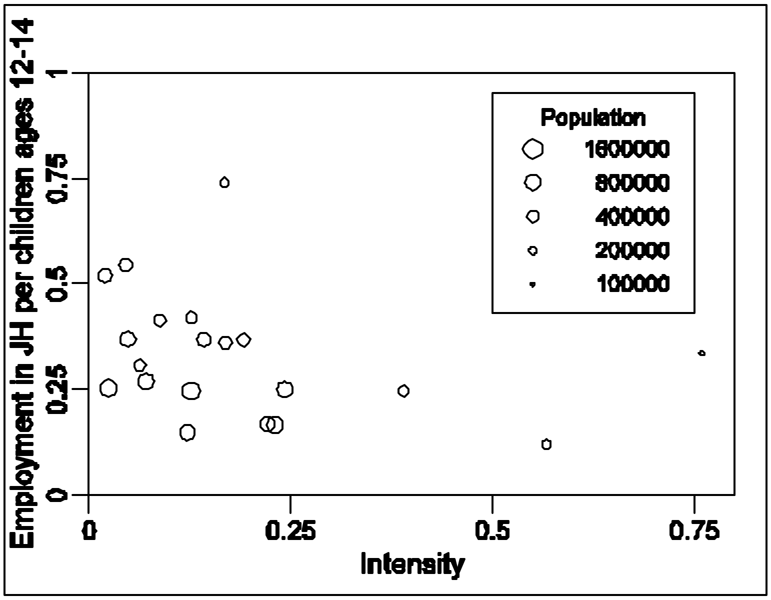
Source: Ministry of Education, Educational <u>Statistics of Republic of China</u>.

Figure 2: Number of First Year JH Students to Number of Primary School Graduates



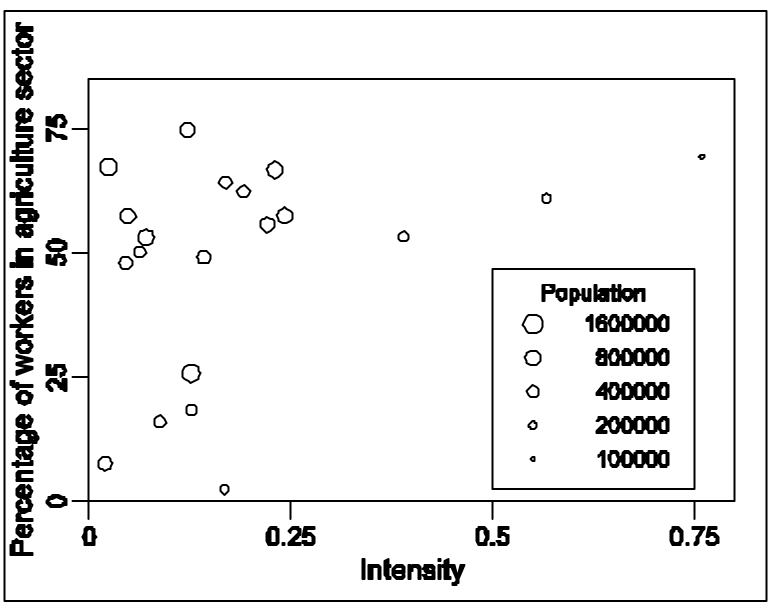
Source: Ministry of Education, <u>Educational Statistics of Republic of China</u>.

Figure 3: Enrollment Rate in 1966 vs. Program Intensity



Source: The data on enrollment rate are from the Educational Statistics of the Republic of China.

Figure 4: Percentage of Workers in Agriculture in 1967 vs. Program Intensity



Source: The data on percentage of workers in agriculture are from the Taiwan Agricultural Yearbook.

Figure 5: Coefficients of the Interactions between the Age in 1968 and Program Intensity in the Years of Schooling Regression (Mothers)

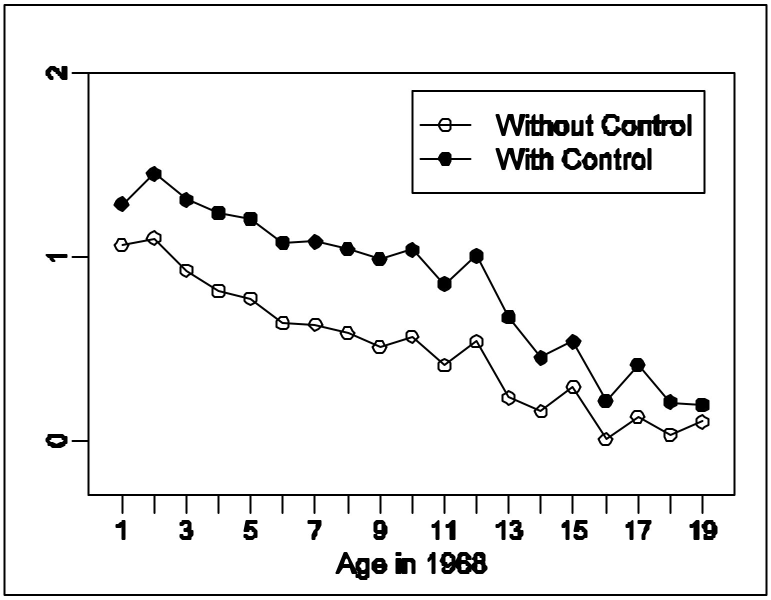


Figure 6: Coefficients of the Interactions between the Age in 1968 and Program Intensity in the Years of Schooling Regression (Fathers)

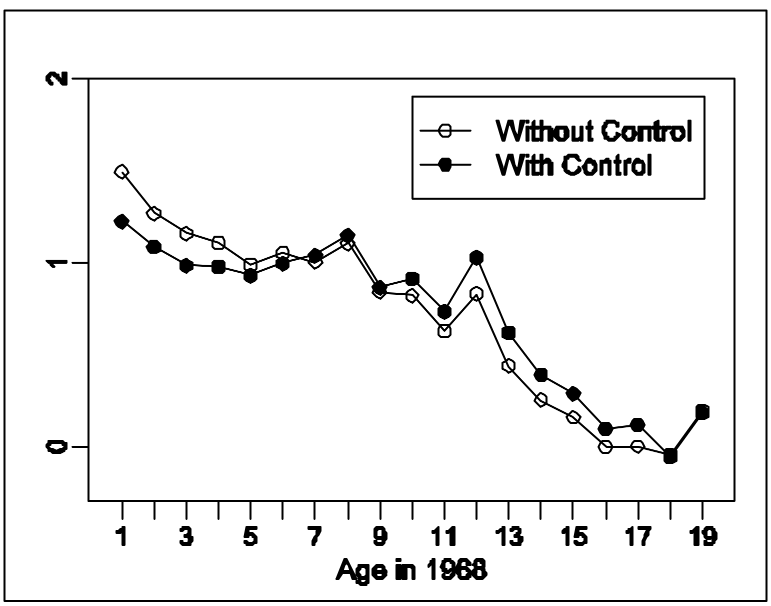


Figure 7: Coefficients of the Interactions between the Age in 1968 and Program Intensity in the Level of Education Completed Regression (Mothers)

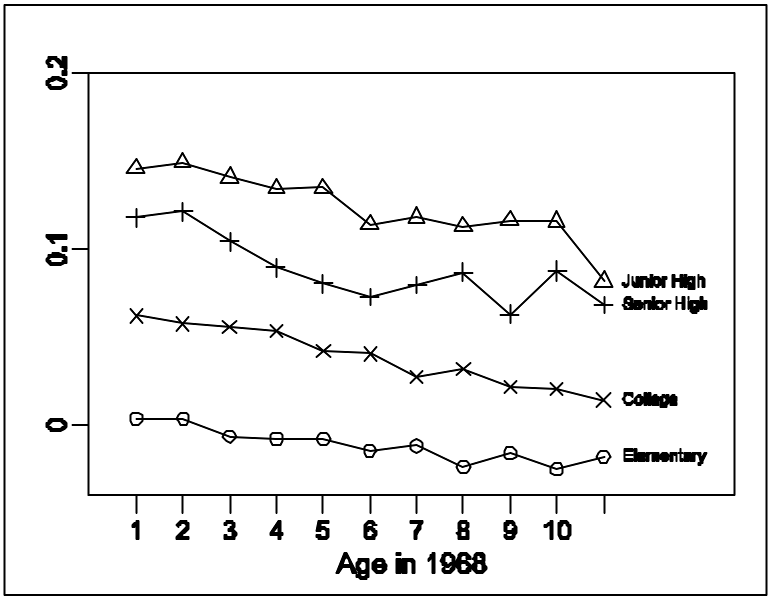


Figure 8: Coefficients of the Interactions between the Age in 1968 and Program Intensity in the Level of Education Completed Regression (Mothers)

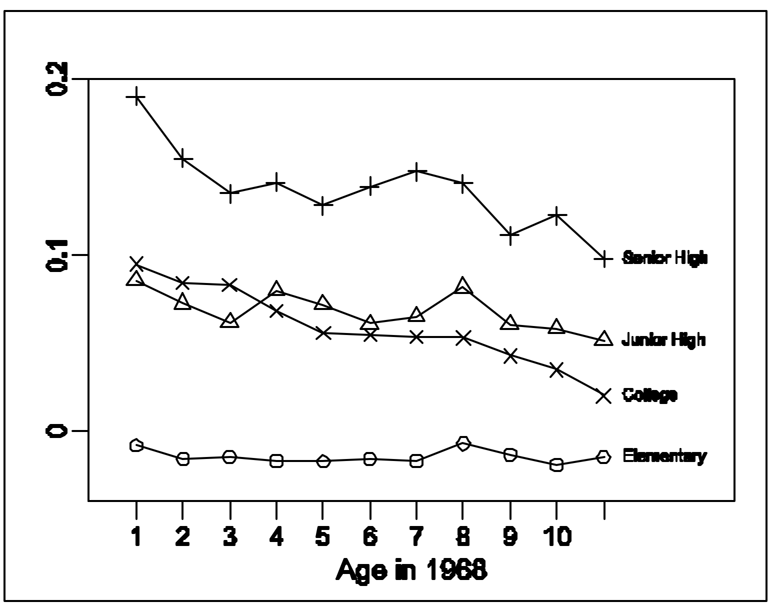


Table 1 Program Intensity in 1968

City/county	Intensity of the 1968 program
Taipei city	0.23
Kaohsiung city	0.02
Taipei county	0.13
Ilan county	0.06
Taoyuan county	0.14
Hsinchu County	0.05
Miaoli county	0.19
Taichung County	0.22
Changhua county	0.02
Nantou county	0.17
Yunlin county	0.12
Chaiyi county	0.07
Tainan county	0.23
Kaohsiung county	0.05
Pingtung county	0.24
Taitung county	0.57
Hualien county	0.39
Penghu county	0.76
Keelung city	0.17
Taichung city	0.13
Tainan city	0.09

Program intensity is defined as number of new junior high schools in 1968 per thousand children ages 12-14 in 1967.

Table 2 Effects of Education Reform on Parents' Educational Attainment (Basic Approach)

	Panel A: First Stage Results on Mother's Education				
	(1)	(2)	(3)	(4)	(5)
Control group (mothers ages in 1968)	15-20	15-20	12-14	12-20	15-20
Treatment group (mothers ages in 1968)	0-5	6-11	9-11	0-11	0-11
Treatment dummy*Program intensity	1.029	0.450	0.098	0.332	0.461
	(2.07)	(1.76)	(0.54)	(1.71)	(1.69)
Sample size	2,531,550	2,909,818	2,025,177	5,576,868	4,591,774
R-square	0.189	0.141	0.140	0.170	0.165
% of sample belongs to the treatment group	66.44	70.80	51.36	67.10	
	Panel B: First Stage Results on Father's Education				
	(1)	(2)	(3)	(4)	(5)
Control group (fathers ages in 1968)	15-20	15-20	12-14	12-20	15-20
Treatment group (fathers ages in 1968)	0-5	6-11	9-11	0-11	0-11
Treatment dummy*Program intensity	1.094	0.771	0.098	0.580	0.782
	(2.43)	(2.94)	(0.54)	(3.12)	(2.84)
Sample size	2,883,256	3,585,365	2,025,177	6,099,832	4,963,381
R-square	0.118	0.100	0.140	0.106	0.110
% of sample belongs to the treatment group					

Sample includes mothers or fathers who were between 22 and 45 when mothers gave birth in the period from 1978 through 1999. All regressions also include 20 region of birth dummies, 21 year of birth dummies, 20 cohort dummies, and 420 interactions between region of birth dummies and year of birth dummies. T-statistics are in parentheses and are based on robust standard errors that allow for clustering by region of birth and age in 1968.

Table 3 Effects of Education Reform on Parents' Educational Attainment (Full Specification)

	Dependent Variable: Mother's Years of Schooling				Dependent Variable: Father's Years of Schooling			
Cohort dummies*Program intensity								
Cohort0 (age=0 in 1968)	1.295	(1.57)	1.481	(2.90)	1.558	(2.25)	1.251	(2.95)
Cohort1 (age=1 in 1968)	1.060	(1.29)	1.284	(2.53)	1.491	(2.28)	1.221	(2.98)
Cohort2 (age=2 in 1968)	1.099	(1.40)	1.450	(2.96)	1.265	(2.01)	1.082	(2.69)
Cohort3 (age=3 in 1968)	0.924	(1.20)	1.309	(2.72)	1.157	(1.86)	0.982	(2.43)
Cohort4 (age=4 in 1968)	0.812	(1.08)	1.236	(2.64)	1.103	(1.85)	0.975	(2.52)
Cohort5 (age=5 in 1968)	0.771	(1.05)	1.207	(2.58)	0.987	(1.66)	0.929	(2.38)
Cohort6 (age=6 in 1968)	0.637	(0.88)	1.076	(2.32)	1.050	(1.82)	0.993	(2.59)
Cohort7 (age=7 in 1968)	0.631	(0.87)	1.081	(2.36)	1.002	(1.78)	1.037	(2.72)
Cohort8 (age=8 in 1968)	0.583	(0.81)	1.040	(2.23)	1.101	(1.97)	1.145	(2.95)
Cohort9 (age=9 in 1968)	0.506	(0.71)	0.985	(2.18)	0.837	(1.52)	0.862	(2.26)
Cohort10 (age=10 in 1968)	0.566	(0.80)	1.036	(2.28)	0.819	(1.53)	0.911	(2.46)
Cohort11 (age=11 in 1968)	0.407	(0.59)	0.850	(1.92)	0.626	(1.19)	0.732	(2.03)
Cohort12 (age=12 in 1968)	0.537	(0.78)	1.003	(2.24)	0.828	(1.60)	1.025	(2.81)
Cohort13 (age=13 in 1968)	0.231	(0.33)	0.669	(1.55)	0.438	(0.85)	0.616	(1.73)
Cohort14 (age=14 in 1968)	0.156	(0.22)	0.449	(1.02)	0.251	(0.49)	0.388	(1.14)
Cohort15 (age=15 in 1968)	0.289	(0.41)	0.536	(1.23)	0.157	(0.30)	0.286	(0.84)
Cohort16 (age=16 in 1968)	0.005	(0.01)	0.213	(0.45)	-0.002	(0.00)	0.094	(0.27)
Cohort17 (age=17 in 1968)	0.128	(0.17)	0.411	(0.89)	0.000	(0.00)	0.116	(0.31)
Cohort18 (age=18 in 1968)	0.031	(0.04)	0.209	(0.43)	-0.044	(-0.08)	-0.057	(-0.14)
Cohort19 (age=19 in 1968)	0.101	(0.11)	0.193	(0.36)	0.194	(0.31)	0.181	(0.45)
Controls for:								
JHS enrollment rate*T	No		Yes		No		Yes	
% in agricultural share*T								
F-statistic	1.16		6.7		3.89		7.49	
R-square	0.17		0.17		0.11		0.11	
Sample size	5,576,868		5,576,868		6,099,832		6,099,832	

Sample includes mothers or fathers who were between 22 and 45 when mothers gave birth in the period from 1978 through 1999. All regressions also include 20 region of birth dummies, 21 year of birth dummies, 20 cohort dummies, and 420 interactions between region of birth dummies and year of birth dummies. T-statistics are in parentheses and are based on robust standard errors that allow for clustering by region of birth and age in 1968. The F-statistics test the hypothesis that the coefficients of the interaction terms are jointly zero.

Table 4: Effects of Education Reform on Parents' Educational Attainment (Restricted Specification)

Cohort dummies*Program intensity	Dependent Variable: Mother's Years of Schooling				Dependent Variable: Father's Years of Schooling			
	(1)		(2)		(3)		(4)	
Cohort0 (age=0 in 1968)	1.152	(2.22)	1.144	(3.61)	1.409	(2.74)	1.011	(3.58)
Cohort1 (age=1 in 1968)	0.919	(1.80)	0.951	(3.03)	1.344	(2.91)	0.986	(3.81)
Cohort2 (age=2 in 1968)	0.963	(2.15)	1.120	(3.97)	1.121	(2.62)	0.849	(3.41)
Cohort3 (age=3 in 1968)	0.793	(1.85)	0.983	(3.65)	1.015	(2.45)	0.750	(3.01)
Cohort4 (age=4 in 1968)	0.688	(1.76)	0.915	(3.70)	0.969	(2.54)	0.751	(3.29)
Cohort5 (age=5 in 1968)	0.654	(1.78)	0.890	(3.54)	0.858	(2.31)	0.711	(3.12)
Cohort6 (age=6 in 1968)	0.531	(1.51)	0.770	(3.12)	0.928	(2.70)	0.785	(3.59)
Cohort7 (age=7 in 1968)	0.528	(1.56)	0.777	(3.29)	0.890	(2.73)	0.839	(3.92)
Cohort8 (age=8 in 1968)	0.491	(1.49)	0.746	(3.00)	1.001	(3.17)	0.961	(4.28)
Cohort9 (age=9 in 1968)	0.427	(1.46)	0.705	(3.30)	0.750	(2.52)	0.693	(3.29)
Cohort10 (age=10 in 1968)	0.490	(1.73)	0.759	(3.52)	0.746	(2.73)	0.758	(3.88)
Cohort11 (age=11 in 1968)	0.322	(1.30)	0.568	(3.10)	0.560	(2.22)	0.592	(3.39)
Controls for:								
JHS enrollment rate*Cohort	No		Yes		No		Yes	
% in agricultural share*Cohort								
F-statistic	4.11		15.73		9.49		16.96	
R-square	0.16		0.17		0.11		0.11	
Sample size	4,591,774		4,591,774		4,963,381		4,963,381	

Sample includes mothers or fathers who were between 22 and 45 when mothers gave birth in the period from 1978 through 1999. All regressions also include 20 region of birth dummies, 21 year of birth dummies, 20 cohort dummies, and 420 interactions between region of birth dummies and year of birth dummies. T-statistics are in parentheses and are based on robust standard errors that allow for clustering by region of birth and age in 1968. The F-statistics test the hypothesis that the coefficients of the interaction terms are jointly zero.

Table 5: Effects of Education Reform on Different Levels of Education

	Dependent Variable: Mother's Educational Attainment (Binary Variable)							
Cohort dummies*Program intensity	Elementary		Junior High		Senior High		College	
Cohort0 (age=0 in 1968)	0.002	(0.07)	0.161	(2.79)	0.156	(2.73)	0.073	(3.79)
Cohort1 (age=1 in 1968)	0.001	(0.03)	0.144	(2.47)	0.117	(2.30)	0.063	(3.31)
Cohort2 (age=2 in 1968)	0.001	(0.03)	0.148	(2.91)	0.120	(2.88)	0.058	(3.35)
Cohort3 (age=3 in 1968)	-0.009	(-0.32)	0.139	(2.75)	0.103	(2.59)	0.056	(3.48)
Cohort4 (age=4 in 1968)	-0.010	(-0.38)	0.133	(2.79)	0.088	(2.47)	0.054	(3.41)
Cohort5 (age=5 in 1968)	-0.011	(-0.41)	0.133	(3.11)	0.079	(2.22)	0.043	(2.72)
Cohort6 (age=6 in 1968)	-0.017	(-0.69)	0.112	(2.69)	0.071	(2.17)	0.041	(2.83)
Cohort7 (age=7 in 1968)	-0.015	(-0.58)	0.116	(2.83)	0.078	(2.51)	0.028	(2.02)
Cohort8 (age=8 in 1968)	-0.027	(-1.17)	0.111	(2.75)	0.085	(2.83)	0.032	(2.57)
Cohort9 (age=9 in 1968)	-0.019	(-0.82)	0.114	(3.19)	0.061	(2.35)	0.023	(1.91)
Cohort10 (age=10 in 1968)	-0.028	(-1.39)	0.114	(3.01)	0.086	(3.19)	0.022	(1.96)
Cohort11 (age=11 in 1968)	-0.021	(-1.13)	0.080	(2.38)	0.067	(2.80)	0.015	(1.46)
F-Statistics	0.3		11.34		10.4		10.1	
	Dependent Variable: Father's Educational Attainment (Binary Variable)							
Cohort dummies*Program intensity	Elementary		Junior High		Senior High		College	
Cohort0 (age=0 in 1968)	-0.011	(-0.65)	0.096	(1.56)	0.188	(2.95)	0.095	(3.78)
Cohort1 (age=1 in 1968)	-0.008	(-0.47)	0.085	(1.45)	0.191	(3.77)	0.096	(4.40)
Cohort2 (age=2 in 1968)	-0.016	(-0.97)	0.073	(1.27)	0.155	(3.69)	0.085	(3.85)
Cohort3 (age=3 in 1968)	-0.015	(-0.92)	0.061	(1.10)	0.135	(3.30)	0.084	(3.86)
Cohort4 (age=4 in 1968)	-0.017	(-1.06)	0.080	(1.42)	0.142	(4.04)	0.069	(3.80)
Cohort5 (age=5 in 1968)	-0.018	(-1.04)	0.072	(1.28)	0.129	(4.01)	0.057	(3.12)
Cohort6 (age=6 in 1968)	-0.016	(-1.05)	0.061	(1.14)	0.140	(4.72)	0.056	(3.55)
Cohort7 (age=7 in 1968)	-0.017	(-1.19)	0.065	(1.32)	0.148	(4.90)	0.055	(3.62)
Cohort8 (age=8 in 1968)	-0.007	(-0.54)	0.082	(1.68)	0.141	(5.25)	0.055	(3.80)
Cohort9 (age=9 in 1968)	-0.014	(-1.10)	0.060	(1.34)	0.112	(4.20)	0.044	(3.42)
Cohort10 (age=10 in 1968)	-0.019	(-1.57)	0.058	(1.31)	0.123	(4.96)	0.036	(2.84)
Cohort11 (age=11 in 1968)	-0.015	(-1.18)	0.051	(1.28)	0.098	(4.15)	0.022	(1.91)
F-Statistics	1.23		2.28		25.44		18.45	

Table 6: Effects of Parental Schooling on Child Health Outcomes: OLS and TSLS (Basic Approach in the First Stage)

	P*T (0-5 vs 15-20)		P*T (6-11 vs 15-20)		P*T (0-11 vs 15-20)	
Panel A: Mother's Years of Schooling on Child Health Outcomes						
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Neonatal Mortality	-0.0001	0.0007	-0.0001	0.0004	-0.0001	-0.0006
	(-6.08)	(0.52)	(-6.04)	(0.32)	(-9.18)	(-0.39)
Infant Mortality	-0.0003	-0.0020	-0.0003	-0.0020	-0.0003	-0.0030
	(-18.99)	(-0.91)	(-18.94)	(-1.00)	(-26.58)	(-1.26)
Postneonatal Mortality	-0.0002	-0.0027	-0.0002	-0.0024	-0.0002	-0.0024
	(-19.79)	(-1.63)	(-19.75)	(-1.58)	(-26.91)	(-1.32)
Low Birthweight	-0.0019	-0.0262	-0.0019	-0.0211	-0.0018	-0.0166
	(-40.61)	(-3.76)	(-40.62)	(-3.47)	(-52.37)	(-2.36)
Very Low Birthweight	-0.0012	-0.0159	-0.0012	-0.0141	-0.0010	-0.0113
	(-47.70)	(-4.29)	(-47.60)	(-4.35)	(-57.11)	(-3.13)
Premature	-0.0011	-0.0195	-0.0011	-0.0187	-0.0009	-0.0165
	(-22.92)	(-2.93)	(-22.94)	(-3.17)	(-26.64)	(-2.45)
Panel B: Father's Years of Schooling on Child Health Outcomes						
	P*T (0-5 vs 15-20)		P*T (6-11 vs 15-20)		P*T (0-11 vs 15-20)	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Neonatal Mortality	0.0000	0.0014	0.0000	0.0018	0.0000	-0.0008
	(-4.22)	(1.36)	(-4.27)	(1.25)	(-6.29)	(-1.02)
Infant Mortality	-0.0003	-0.0001	-0.0003	-0.0003	-0.0003	-0.0028
	(-19.76)	(-0.07)	(-19.70)	(-0.12)	(-24.79)	(-2.25)
Postneonatal Mortality	-0.0002	-0.0016	-0.0002	-0.0020	-0.0002	-0.0020
	(-22.32)	(-1.25)	(-22.20)	(-1.22)	(-27.06)	(-2.08)
Low Birthweight	-0.0018	-0.0115	-0.0018	-0.0113	-0.0018	-0.0128
	(-42.17)	(-2.38)	(-42.22)	(-1.75)	(-52.21)	(-3.54)
Very Low Birthweight	-0.0010	-0.0087	-0.0010	-0.0106	-0.0008	-0.0088
	(-42.71)	(-3.39)	(-42.65)	(-3.05)	(-46.10)	(-4.90)
Premature	-0.0007	-0.0108	-0.0007	-0.0095	-0.0007	-0.0091
	(-17.48)	(-2.32)	(-17.53)	(-1.54)	(-21.64)	(-2.63)

All regressions include mother’s years of schooling, 20 region of birth dummies, 21 year of birth dummies, 20 cohort dummies, and 420 interactions between region of birth dummies and year of birth dummies. T-statistics are in parentheses.

Table 7: Effects of Parental Schooling on Child Health Outcomes: OLS and TSLS (Restricted Estimation in the First Stage)

Mother's Education on Child Health Outcomes	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS
Neonatal Mortality	-0.0001	0.0001	-0.0001	-0.0002
	(-9.18)	(0.12)	(-9.22)	(-0.23)
Infant Mortality	-0.0003	-0.0001	-0.0003	-0.0012
	(-26.58)	(-0.09)	(-26.66)	(-0.82)
Postneonatal Mortality	-0.0002	-0.0002	-0.0002	-0.0009
	(-26.91)	(-0.21)	(-26.98)	(-0.87)
Low Birthweight	-0.0018	-0.0139	-0.0018	-0.0139
	(-52.37)	(-3.21)	(-52.63)	(-3.37)
Very Low Birthweight	-0.0010	-0.0080	-0.0010	-0.0072
	(-57.11)	(-3.65)	(-57.12)	(-3.47)
Premature	-0.0009	-0.0081	-0.0009	-0.0137
	(-26.64)	(-1.97)	(-27.01)	(-3.46)
Controls for:				
JHS*T & % agricultural*T	No	No	Yes	Yes

Father's Education on Child Health Outcomes	OLS	2SLS	OLS	2SLS
Neonatal Mortality	0.0000	-0.0005	0.0000	-0.0003
	(-6.29)	(-0.67)	(-6.30)	(-0.30)
Infant Mortality	-0.0003	-0.0020	-0.0003	-0.0028
	(-24.79)	(-1.97)	(-24.75)	(-2.20)
Postneonatal Mortality	-0.0002	-0.0016	-0.0002	-0.0026
	(-27.06)	(-2.00)	(-26.99)	(-2.61)
Low Birthweight	-0.0018	-0.0112	-0.0018	-0.0126
	(-52.21)	(-3.69)	(-52.39)	(-3.36)
Very Low Birthweight	-0.0008	-0.0073	-0.0008	-0.0076
	(-46.10)	(-4.88)	(-46.15)	(-4.09)
Premature	-0.0007	-0.0056	-0.0007	-0.0074
	(-21.64)	(-1.94)	(-21.86)	(-2.07)
Controls for:				
JHS*Cohort & % agricultural*Cohort	No	No	Yes	Yes

All regressions include mother's years of schooling, 20 region of birth dummies, 21 year of birth dummies, 20 cohort dummies, and 420 interactions between region of birth dummies and year of birth dummies. T-statistics are in parentheses.

Table 8: Effect of Parental Education on Child Health Outcomes

	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mother's Education	Father's Education	Mother's Education	Father's Education	Mother's Education	Father's Education	Mother's Education	Father's Education
Neonatal Mortality	0.0000	0.0000	0.0010	-0.0005	0.0000	0.0000	-0.0007	-0.0019
	(-4.61)	(-1.63)	(0.64)	(-0.31)	(-4.62)	(-1.63)	(-0.52)	(-1.09)
Infant Mortality	-0.0001	-0.0001	0.0025	-0.0032	-0.0001	-0.0001	-0.0008	-0.0053
	(-8.84)	(-8.53)	(1.01)	(-1.20)	(-8.90)	(-8.49)	(-0.37)	(-1.92)
Postneonatal Mortality	-0.0001	-0.0001	0.0015	-0.0026	-0.0001	-0.0001	-0.0001	-0.0034
	(-7.65)	(-9.77)	(0.78)	(-1.30)	(-7.73)	(-9.71)	(-0.04)	(-1.61)
Low Birthweight	-0.0012	-0.0014	-0.0061	-0.0016	-0.0012	-0.0014	-0.0091	-0.0066
	(-24.20)	(-30.31)	(-0.83)	(-0.21)	(-24.34)	(-30.36)	(-1.44)	(-0.82)
Very Low Birthweight	-0.0004	-0.0004	-0.0081	0.0002	-0.0004	-0.0004	-0.0070	0.0006
	(-17.15)	(-17.42)	(-2.20)	(0.05)	(-17.29)	(-17.38)	(-2.23)	(0.14)
Premature	-0.0004	-0.0005	-0.0057	0.0024	-0.0004	-0.0005	-0.0128	-0.0058
	(-9.57)	(-12.16)	(-0.80)	(0.32)	(-9.75)	(-12.19)	(-2.10)	(-0.75)
Controls for: JHS*T & % agricultural*Cohort	No	No	No	No	Yes	Yes	Yes	Yes
Sample size	3,624,585	3,624,585	3,624,585	3,624,585	3,624,585	3,624,585	3,624,585	3,624,585

All regressions include mother’s years of schooling, father’s years of schooling, 20 mother’s region of birth dummies, 20 father’s region of birth dummies, 20 mother’s cohort dummies, 24 father’s cohort dummies, 21 year of birth dummies 420 interactions between mother’s region of birth dummies and year of birth dummies, and 420 interactions between father’s region of birth dummies and year of birth dummies. T-statistics are in parentheses.