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Can Basic Maternal Literacy Skills Improve Infant Health Outcomes? Evidence from the Education Act in Nepal

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Abstract

The National Education System Plan (NESP), which was implemented in 1971, reshaped the education system of Nepal and increased access to education among females. I use this dramatic change in Nepal's education system as a quasi-natural experiment to identify the effect of maternal literacy skills such as the ability to read, write, and the highest level of schooling on infant and child mortality outcomes. The results suggest that the reform improved educational attainment among females of school-going-age during the time of the reform but had no effect on male's educational attainment. Using within cohort and across district variations in educational outcomes due to the reform, I find that one more year of maternal schooling reduces under five mortality rate by 4.5 percentage points.

Key words: Mother's literacy, Child Mortality, Health Returns to Education.
JEL Codes: I10, I26, I15

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1 Introduction

Educating women is viewed as one of the most effective policy measures in improving human development outcomes in developing nations. To highlight the importance of educating girls, Kofi Annan (2003) mentioned, "No other policy is as likely to raise economic productivity, lower infant and maternal mortality, improve nutrition and promote health."[1] Studies claim that mothers pass on benefits of education to their children in forms of enhanced cognitive development, improved academic success, higher income, and better health outcomes (Behrman and Rosenzweig, 2002; Behrman et al., 2017; Conti et al., 2010). Based on such a robust relationship, the World Bank initiated a campaign in 1990s, which was dedicated to promote maternal education in developing nations (Mundial, 1993).

Simultaneously, the concern of child mortality has received a widespread attention, particularly, with the Fourth Millennium Development Goal (MDG 4) aiming to reduce under-five child mortality rate by two thirds between 1990 and 2015. Children in developing countries are eight-ten times more likely to die before their fifth birthday compared to developed nations and the majority of these deaths are due to preventable diseases such as pneumonia, diarrhea, and birth asphyxiation (You et al., 2015). Although child mortality has shown a drastic decline since 1990 across both developed and developing nations, the gap in under-five mortality is still large and has not converged over the years.[2]

This study evaluates the effect of basic maternal literacy skills such as the ability to read, write, and the highest level of formal education on infant and child mortality outcomes. To answer the question on hand, I use a dramatic change in Nepal's education system as a quasi-natural experiment. I use data from the Nepal Living Standard Surveys (NLSS) of years 1995 and 2003. The adoption of the National Education System Plan (NESP) in 1971 established a framework for universal education, which influenced educational achievement of mothers in NLSS 1995 and 2003. To overcome issues of prior existing education system such as a lack of uniformity in curriculum and textbooks, inadequate school infrastructure, absence of trained teachers, and a strong prejudice against educating females and people from lower castes, the NESP nationalized education in the country. To expand access to quality education for an average Nepali individual, the Education Act of 1971, a part of the NESP, brought upon several changes which went into effect throughout the nation by 1976. Some important changes include: 1) A provision of free primary education (grades 1 to 5) with textbooks provided by the government, 2) The implementation of a standardized curriculum and textbooks, 3) Supply of trained teachers; and 4) Construction of new schools.

I use the two stage least squares (2SLS) to identify the effect of maternal education on child health

[1] `https://www.un.org/press/en/2003/sgsm8662.doc.htm`

[2] In 2015, the under-five mortality rate of developed regions on average was 0.6 percent compared to mortality rate of 4.7 percent of developing nations.

outcomes. First, I implement a difference-in-differences model by using variations in the intensity of the education reform (measured by schools per 1,000 of 6-9 year olds in 1975) and cohorts that are likely to be affected by the reform (0-9 years of age in 1975) to identify the effect of the reform on female educational outcomes. Second, using variation in educational outcomes due to the reform, I estimate the causal effect of maternal educational outcomes on infant and child mortality.

I find that the reform improved educational outcomes among females. Specifically, one school per 1,000 children following the reform increased the ability to read by 14.1 percentage points and improved the highest years of formal education attainment by 0.857 years. The 2SLS findings suggest that one more year of basic education at the primary level (1^{st} to 5^{th} grade) reduces under five child mortality rate by 4.5 percentage points. The results suggest that the reform did not affect contemporaneous male's education outcomes. While investigating some mechanisms through which maternal education can affect children's health outcomes, I find evidence of assortative matching on education, with relatively better educated women marrying men with higher levels of schooling. Also, the findings provide some evidence indicating that schooling improves a mother's health condition by reducing the probability of having chronic illness. Although I find no direct evidence suggesting that basic schooling improved women's fertility outcomes such as age of first marriage and early fertility, the results indicate that schooling enhances a woman's knowledge regarding contraceptives.

The identification relies under an assumption that in absence of the reform educational outcomes between females born in high intensity districts and females born in low intensity districts would not be systematically different. I use three different approaches to evaluate the given assumption: 1) An event-study method, which presents non-parametric estimates of the reform by female's age during the time of the reform, 2) Placebo tests, and 3) Accessing the effects of the reform on parental educational outcomes.

The event-study method follows the specification outlined in (Duflo, 2001) and estimates the effect of the reform by age (in 1975). The results confirm that the effects of the reform decreases with age and the estimates pertaining to age groups that already surpassed the primary-school-going age (unexposed group) during the time of the reform (10-27 years of age) are very close to zero. This points to lack of pre-existing differences in trends in educational outcomes across low and high intensity districts. To further access the validity of the identification, I formulate a placebo exercise that forms an artificial exposed group by including 10-25 year olds in 1975 and the unexposed group comprises 26-35 year olds (in 1975). The estimates pertaining to placebo exercise that uses educational outcomes as the dependent variables are close to zero and lack both economic and statistical relevance. Next, although I present estimates by estimating specifications that controls for parental literacy (father's and mother's literacy), I directly estimate the effect of the reform on parental literacy skills. Note that parental literacy skills for females aged 0-27 in 1975 are predetermined and should not be affected by the reform. The results confirm such hypothesis. I replicate the

event study and placebo exercises by using child mortality outcomes instead of mother's education outcomes to estimate the reduced form model. The results indicate that the reform improved child mortality outcomes and demonstrate lack of pre-existing trends.

Grossman (1972) developed a theoretical model regarding the demand for health capital. The author investigated several determinants of health besides medical care, including knowledge, which is proxied by years of formal schooling. Although a huge body of research indicates a positive association between education and health, there is relatively less consensus as to whether this positive association demonstrates a causal link. This is because the determinants of educational attainment such as discount rate and ability can jointly determine health outcomes. Moreover, health can itself be viewed as a form of human capital, which can in return determine educational outcomes and earnings (Almond and Currie, 2011; Almond and Mazumder, 2011; Currie and Vogl, 2013; Grossman, 1972). Ehrlich and Yin (2013) develops a human capital-based endogenous growth model that considers population aging and economic growth as endogenously determined. Using an overlapping generations model the authors show that health and education are jointly determined; health investment for children acts as a determinant of long-term economic growth, as such investment preserves the return in knowledge formation or education. In line to such results, improvements in parent's education is also likely to improve children's human capital formation particularly through improved education.

There exists a growing body of literature that addresses the causal relationship between mother's education and child health. Currie and Stabile (2003) uses availability of colleges in women's county of residence in her 17^{th} birthday as instruments and find that higher maternal education among white women improves infant health in the United States. However, by using age-at-school-entry policies in California and Texas to identify the effect of education on fertility and infant health, McCrary and Royer (2006) conclude that higher maternal education does not significantly impact fertility decisions and has a small but heterogeneous effect (by race and ethnicity) on infant health. Likewise, Lindeboom et al. (2009) find little evidence that maternal schooling has a beneficial effect on child health outcomes in the United Kingdom. In contrast, Breierova and Duflo (2004) use a massive school construction program in Indonesia between 1973 and 1978 as instruments and find that improvement in maternal education reduces child mortality. Similarly, by using junior high school openings in Taiwan due to an extended compulsory education from 6 to 9 years as instruments, Chou et al. (2010) find that increases in schooling due to the reform decreased child mortality. Also, using expansion of access to secondary schooling in Zimbabwe, Grépin and Bharadwaj (2015) find that an additional year of schooling reduces a probability of child death by almost 20%. Among these studies, only the Breierova and Duflo (2004) and Grépin and Bharadwaj (2015) studies focus in developing countries. However, the average years of formal education among female is 6.67 years (Breierova and Duflo, 2004)

and 5.96 (Grépin and Bharadwaj, 2015) compared to 0.83 in this study (Source: NLSS data survey years 1995 and 2003), which depicts a stark difference in educational outcome. In Grossman (2015) meta-analysis that reviews the existing literature evaluating the relationship between education and health, he writes, "I conclude that there is enough conflicting evidence in the studies that I have reviewed to warrant more research on the question of whether more schooling does in fact cause better health outcomes."

This study contributes to the existing literature by evaluating the effect of basic maternal literacy skills such as the ability to read, write and the highest level of formal education on infant and child mortality by utilizing the education reform that was implemented during a period when female education was close to non-existent. The Preston curve states that an additional increment in per capita income at the lower levels of per capita income is associated with larger gains in life expectancy, compared to additional increases at the higher levels. Consistent with the Preston curve and the law of diminishing returns, it is reasonable to expect differential effects of varying levels of maternal education on child health outcomes. An increase in one additional year of schooling at the primary level (between 1^{st} to 5^{th} grade) may have higher health benefits compared to an additional year of increase in education at the college level.

The study is structured as follows. Section 2 provides essential discussion regarding the role of maternal education on child health in developing countries, while section 3 provides a brief history about Nepal's education system and the National Education System Plan (NESP). Section 4 describes the identification strategy and data, followed by the first stage results. Sections 5 discusses the main results and potential mechanisms; section 6 provides robustness checks, section 7 compares the findings with earlier studies and section 8 concludes.

2 Maternal Education and Child Health in Developing Countries

In general, improvements in mother's education is likely to increase both marginal product of medical care and marginal product of time spent on health – two inputs that directly affect the stock of health.[3] This will shift the marginal efficiency of health capital (MEC) curve to the right, which increases health stock for educated mothers and their children. Also, schooling can increase health knowledge among mothers and in return improve allocative efficiency by allowing mothers to allocate appropriate inputs in producing health (Kenkel, 1991). Kenkel (1991) finds that the relationship between schooling and health behaviors such as consumption of cigarettes, alcohol and exercise is partly explained by differences in health knowledge. However, schooling effects on health differences still remain after accounting for health knowledge, suggesting that although allocative efficiency may be a part of the mechanism, several other pathways can explain the

[3]See (Grossman, 1972) for more detail.

schooling effects on health. Perhaps a major one being Fuchs' hypothesis that schooling and health are both determined by unobserved factors such as time preferences (Fuchs, 1980).

In developing nations, the majority of child mortality occurs due to preventable or curable diseases such as pneumonia, birth asphyxiation, diarrhea, and preterm birth complications (WHO and UNICEF, 2013). Better educated women are more likely to use preventive measure such as vaccines, chlorination, and mosquito nets as they may possess an increased ability to absorb health-related knowledge (Ndjinga and Minakawa, 2010). Glewwe (1999) finds that although health knowledge is not directly taught in classes in Morocco, mothers use their literacy skills to obtain health-related knowledge, which can in return improve child health. Also, health status of women during pregnancy plays a vital role in determining newborns' health. Hence, prenatal care can significantly affect both mother's and newborn's health. Currie and Stabile (2003) provide evidence of increases in prenatal care visits among mothers with higher schooling. Besides prevention as a potential pathway, educated women may be relatively more knowledgeable in terms of diagnosing diseases and delivering a sick child to a health clinic for timely treatment. Thomas (1999) concludes that women with better comprehensive skills are more likely to use and assimilate information in the community. Hence, mother's literacy can potentially increase the marginal product of medical care available in one's community.

The other direct pathway which can prominently affect child health is through a reduction in fertility. Osili and Long (2008) (in Nigeria) and Breierova and Duflo (2004) (in Indonesia) show that increases in maternal years of education lowers fertility rate. Relating to Becker and Lewis (1973), as a result of a reduction in fertility, a household can focus on quality of children rather than quantity, which in return is likely to improve child health.

Access to clean water supply can massively affect an infant's health. Focusing in the U.S., Cutler and Miller (2005) and Alsan and Goldin (2015) show that improvements in clean water supply in the 19^{th} century significantly reduced infant mortality. These findings are directly prevalent to developing nations, where roughly 783 million people do not have access to clean water.[4] Mothers who are literate may be more likely to realize the importance of clean water supply on an infant's health.

Another pathway through which maternal education may impact child health is through issues related to gender inequality which is highly prevalent in developing countries such as Nepal. Brinda et al. (2015) uses data for 138 countries and present a robust positive correlation between gender inequality index and child mortality. Gender inequality can weaken maternal health by restricting women's access to fundamental human rights including nutrition, health care, and autonomy. Maternal under-nutrition during pregnancy increases instance of stillborn babies and babies with low birth weight, which makes a child vulnerable to

[4] http://www.who.int/water_sanitation_health/monitoring/jmp2012/fast_facts/en/

infectious diseases (Shah, 2010). Jejeebhoy and Sathar (2001) provide evidence that education increases autonomy among women (in India and Pakistan) by influencing female leadership and decision making. Similarly, women's physical mobility is correlated with their use of antenatal care and safe delivery care (Bloom et al., 2001). Education can improve the status and role of women in a society and at the same time abridge gap in gender inequality, which can help improve infant and child health.

In a patriarchal society like that of Nepal, selection of a spouse for marriage is often arranged by families (Pillari, 2005). Given the low levels of labor market participation among females, marriage can itself be regarded as one of the most important decisions for a woman's economic future due to increased dependency of women's financial and social status on men. Important factors in match-making include woman's decorum, astrological paring (of potential husband and wife), social etiquette, skin color, and education (Jayakar, 1994; Lee, 2000). If education allows a woman to select relatively educated husbands, it increases assurance regarding her general well-being. Hence, educational assortative matching can saliently affect health outcomes of children through father's education and household wealth.

3 The National Education System Plan and the Education Act of 1971

Prior to 1951 Nepal was under the Rana regime, which was established in 1866 and lasted until 1950. The Rana leaders thrived on maintaining absolute internal power and were concerned that providing education to the common public could make people conscious of their rights (Shakya, 1977). School opportunities were extremely restricted during the Rana regime and severe penalties were inflicted on those attempting to educate the populace. The literacy rate of the country is estimated at 5 percent in 1951 (Parajuli and Das, 2013).

Formal educational practices in Nepal was introduced only in 1951 with the downfall of the Rana regime and the establishment of democracy. Introduction of formal education in the country began with formation of the Ministry of Education of Nepal in 1951. The country experienced a boom in school construction in the second half of the 20^{th} century. In 1951 Nepal had 321 primary schools, whereas the number of primary schools increased to 25,927 by 2004 (MOE, 2013).

Despite of the introduction of formal education and a rise in schooling infrastructure, the growth of educational outcomes was "un-purposive and lopsided" (Ministry of Education, 1971). The provision of education was biased in terms of the level of wealth, sex, and caste. A general belief of the society deemed education as unnecessary for females and marginalized castes. Savada (1991) claims that educational

opportunities had been monopolized by the members of wealthier and higher castes, and there existed a strong prejudice against educating females. The education system was haphazardly operated without any uniformity in adaptation of curriculum, teacher-training, and text materials across the nation (Savada, 1991). The teachers lacked professional training; a typical primary school teacher had only four to six years of schooling (Wood, 1959). Recognizing these issues associated with the ongoing education system, the Education-Advisory Council in 1968 considered Nepal's educational tradition as archaic and warranted necessary changes.

The adoption of the National Education System Plan (NESP) in 1971 nationalized education in the country with an objective of expanding access to education and providing quality education through reliable funding-sources and technical support (Upadhyaha et al., 2009). NESP, as a supply side policy, emphasized on developing adequate schooling infrastructure and established a framework for universal education. With adoption of NESP, the government intervened Nepal's education system by taking over community-based schools, built additional schools, and arranged provisions for training teachers.[5] Such a change shifted the burden of financing and managing schools to the government and was conducted through the District Education Offices (DEOs) established in each district. The Education Act of 1971, a part of the NESP, is a pioneer of all educational reforms implemented in the nation. It was established with aims of promoting access to quality education. The Education Act brought upon several changes in the education sector of Nepal. Some valuable changes are: 1) Primary education (grades 1 to 5) provided by public schools will be free of costs and textbooks for students enrolled in public schools will be provided by the government as well, 2) A provision of free secondary education is made to girls obtaining education in public schools, 3) Teachers are to be provided with professional training, 4) An implementation of the standardized curriculum and textbooks as approved by the government; and 5) A formation of the District Education Committee in each district to manage and supervise schools within the district. This act was enacted in two districts of Nepal in 1971, thirteen districts in 1972, and by 1976 all seventy five districts of the country were governed by this act. School facilities, teachers, and other education-related materials were provided by the government through the Ministry of Education.

The declaration of free primary level education in 1975 marks a significant step towards obtaining primary education for an average Nepali. Between 1980 and 1985, the Nepali government allocated 2.6 percent of the Gross National Product (GNP) on education (US $ 73 million in 1995 dollars). Approximately, one-third of the annual education budget in the 1980s was allocated towards primary education, mainly on teachers salaries (UNESCO, 1987). By 1980, to resolve the problem of shortages of teachers in rural areas, the government provided incentive for teachers to teach in rural areas by paying them 110 percent of the

[5]The Institute of Education, that operated from 14 campuses throughout the country took responsibility of training teachers.

actual salary (UNESCO, 1987).

The long standing prejudice against educating women created a huge gender gap in educational outcomes such as the ability to read, write, and years of formal schooling completed. The literacy rate in 1961 was 16.3 percent for males and 1.8 percent for females. The literacy rate among males increased to 34 percent in 1981 and 54.5 percent in 1991 compared to 12 percent and 25 percent among females in those respective years (Balatchandirane, 2007). Both demand and supply side factors can potentially explain such disparity. The patriarchal society that regards females responsible for household work, early age marriage, societal restriction on female sexuality and mobility, and the dowry system are some factors that lessens the demand for education for girls. Similarly, schools lacking an adequate number of female teachers, teachers attitude towards educating females, the availability of appropriate physical facilities (toilets) in schools, and longer home to school distance are some supply side factors that may contribute to an existing gender disparity in literacy. Adhikari (2008) concludes that lack of toilets in schools of Nepal is one of the factors responsible for a high dropout rate among girls. Beaman et al. (2012) provide evidence from a randomized natural experiment that female leadership increases educational outcomes among females.

As shown in Figure 1a, the number of schools constructed increased sharply in the late 1960s (due to the first democratic revolution of 1951) and the first half of 1970s following the NESP, which marked as a first-step of a supply side policy aimed at increasing access to schooling.[6] Figure 1b shows a dramatic rise in education expenditure over 1970s. Given the resource constraints, school construction depended on availability of labor and raw materials. Materials required for school buildings (during 1970s) such as stones, wood, bamboo joists, beams, and mud mortar could either be found in the locality, or would have to be transported outside of the locality (UNESCO, 1978). Although in extremely remote areas construction is relatively expensive in terms of raw materials, such higher costs can be balanced by inexpensive local labor with lower opportunity cost.

A notable fact about the rise in school construction in the second half of the 20^{th} century is that the intensity varied across districts. The district-level map constructed in Figure 2 presents the the number of schools available in rural areas in 1975 by districts based on the sample of communities used in this study. By 1975, a district on average had 16 schools available. The number of schools available varied from 55 schools in Jhapa and 1 school in Dolpa. Even among the districts in the Eastern Development Region, there existed considerable variability in the number of schools available; Jhapa and Morang received more schools (55 and 39, respectively), whereas Okhaldhunga and Solukhumbu had lower number of schools (5 and 6, respectively). The effect of reform's implementation should have been hampered by lack of proper schooling

[6] Figure 6e shows that the number of schools increased significantly in early 1990s, following the second democratic revolution. However, this episode of school increment is likely to be demand driven, rather than oriented through a supply side policy.

infrastructure. In contrast, districts that were relatively well equipped with schooling infrastructure will have reaped high benefits of the reform. Hence, the number of schools available in 1975 (per 1,000 of 6 to 9 year olds) is used as the intensity measure of the reform.

Figures 3a and 3b plot the unconditional averages of the ability to read and the highest level of education by age in 1975 across high and low intensity districts among females, respectively.[7] It is visible from the figure that females who were 10 years or older in 1975 (past the school going age) were not affected by the reform (across both low and high intensity districts). The trend in educational outcomes among individuals 10 years or older (in 1975) is remarkably similar across high and low intensity districts. This provides some evidence that school construction following the NESP was not demand-driven. In contrast, females younger than 10 years (those of school going age) were affected by the reform and the effect is pronounced in high intensity districts compared to low intensity districts. For instance, females born in 1975 in a high intensity district on average are approximately 12 percentage points more likely to be able to read and are likely to have more than two years of schooling compared to one year for females born in a low intensity districts. Figures 3a and 3b motivate the following identification strategy.

4 Empirical Setting

4.1 First Stage – The Effect of the Reform on Educational Outcomes

The level of exposure to the reform is dependent on: 1) An individual's age in 1975; and 2) The district of birth. The education reform including the provision of free education enacted in 1975 affected younger and older cohorts differently, which generates the first level of variation. Nepali students typically start attending primary school when they are 6-9 years old (Savada, 1991). Young individuals between the ages of 0 and 9 in 1975 – termed as the exposed cohort from hereon – were affected by the provision of free education and NESP. In contrast, older individuals between the ages of 10 and 25, who had surpassed the school going age, did not benefit from the reform (unexposed cohort).

The second level of variation comes from the intensity of the reform. In districts with an adequate number of schools, NESP along with the provision of free primary education can reduce monetary costs associated with obtaining education. Moreover, other changes brought upon by the reform such as a provision of free of cost textbooks and implementation of uniformly prescribed curriculum should affect educational outcomes among children living in districts with more schools. Hence, the effect of the reform should be stronger in districts that have more schools available in 1975 (per 1,000 children). Using these two different

[7] The median of intensity measure is used as the cut-off point to determine high and low intensity districts for this purpose.

variations, the basic identification strategy, which is similar to that of Duflo (2001), can be written in a differences-in-difference framework as:

$$E_{idbw} = \alpha + \beta T_i * S_{d1975} + \eta X_{idbw} + \gamma_b + \lambda_d + \theta_w + \epsilon_{idbw}, \quad (1)$$

where, E_{idbw} represents the educational outcome (the ability to read, write, and formal years of highest schooling completed) of an individual i born in district d in year b and residing in ward w (during the survey year), T_i is "1" if an individual is 0 to 9 years old (exposed) in 1975 and 0 if she is 10 to 27 years old in 1975 (unexposed), and S_{d1975} is the district specific number of schools available in 1975 per 1,000 of 6 to 9 year old children, which measures the intensity of the reform across districts. X_{idbw} is a vector of individual specific characteristics which include ethnicity, religion, father's literacy status, and mother's literacy status (these variables are discussed in more detail in the Data section), which affects education outcomes. Additionally, the specifications include the interactions between such control variables and the indicator representing exposed cohort. γ_b is a vector of mother's year of birth dummies which will capture birth-year specific common trend in education across the nation. λ_d is district of birth fixed effects and θ_w represents ward of residence fixed effects.[8] The ward level fixed effects absorb the influence of time invariant ward specific characteristics across wards in a district. Equation (1) is estimated by using the OLS and to account for the correlation between error terms within a district, standard errors are clustered at the district of birth.[9]

If an exposed cohort from a high reform intensity district tends to have better educational outcomes then the estimate of β will be positive and is interpreted as the effect of the reform on educational outcomes. The causal interpretation of β relies on the identification assumption that in absence of the reform, educational outcomes of people born in high reform intensity districts would not have been systematically different from educational outcomes of people born in low reform intensity districts. This identification should not be taken for granted. I provide an extensive set of robustness checks to increase confidence towards a belief that the identification is not based on flawed assumption.

One serious concern is that the number of schools built between 1951 and 1975 can be demand driven. In other words, districts receiving a higher number of schools between 1951 and 1975 may have a populace who are more willing to be educated. In this case, the estimate of β will be overstated. On the other hand, it is also likely that legislatures allocated schools in a way that districts with high illiteracy rates received higher

[8] A ward is the electoral subdivision of the nation. A district is divided into several wards and on average nine wards make up a village development committee (VDC). Wards are also the primary sampling units of NLSS.

[9] Standard errors are clustered at the district of birth level since intensity of the reform is measured at the district level. However, clustering the standard errors at the ward level does not change the findings of the study. Such results are not presented but are available upon request.

number of schools (per capita). This would understate the effect of the reform on educational outcomes. Controlling for parental educational status in equation (1) resolves this issue to a certain extent. As previously mentioned, Figures 3a and 3b lend some support that school construction is not demand driven.

The identification will also be violated if other governmental programs aimed at promoting education are correlated with the availability of schools in 1975. Only 3 percent of individuals in the sample reported having obtained education through governmental programs or NGOs, and these individuals are excluded from the analyses. The implication of identification strategy can be tested to provide suggestive evidence regarding the reliability of the identification assumption. First, I consider the following falsification exercise. Individuals aged 10 years or older in 1975 are not fully exposed to the reform. Hence, the reform should not affect educational outcomes of the unexposed cohorts systematically across low and high intensity districts. I conduct a falsification test where 10- to 25 year-olds in 1975 are considered as a pseudo treated group and 26- to 35 year-olds as control cohort. The estimated difference-in-differences should be close to zero if the main results are not spuriously driven.

Next, I directly use father's and mother's literacy status (of individuals in sample used to estimate equation 1) as the dependent variable, respectively. If pre-treatment differences in educational outcomes between low and high intensity districts are not driving the results, the estimated effect of the reform should be close to zero when parental literacy status is used as the dependent variable as parent's literacy status is predetermined at the time of the reform.

Following Duflo (2001), equation (1) can be expanded to an interaction term analysis as follows:

$$E_{idbw} = \alpha + \beta_j \sum_{j=0-1}^{22-23} T_{ij} * S_{d1975} + \eta X_{idbw} + \gamma_b + \lambda_d + \theta_w + \epsilon_{idbw}, \tag{2}$$

where, all variables are similar to those in equation (1), except T_{ij} indicates whether an individual i is of the age category j in 1975. Estimates of β_j should be a decreasing function of age, and estimates of β_j should be equal to zero for values of $j \geqslant 10$.

4.2 Data

This study uses data from the Nepal Living Standard Survey (NLSS) 1995 and 2003. The NLSS is a detailed face-to-face survey conducted by the collaborative effort of the Center Bureau of Statistics and the World Bank. The NLSS survey comprises of two main files 1) Individual file; and 2) Community level file. The survey is further divided by urban and rural residential areas. Approximately 85 percent of individuals from the sampled households were born in rural regions and more than 70 percent of them still lived in rural areas of the nation during the survey year. I use two waves of repeated cross-sectional surveys, including the survey

years 1995 and 2003. I use both individual-level and community-level files for the purpose of this study. The number of households interviewed in 1995 and 2003 surveys are 3,388 and 4,008, respectively. The NLSS reports the district of birth and age of respondents, which are used to form exposed and unexposed cohorts as described in the previous section.

The questions related to educational outcomes are asked to individuals who are five years or older. There are three main variables of educational outcomes which are of interest: 1) The ability to read (whether an individual can read), 2) The ability to write (whether an individual can write); and 3) The highest years of formal schooling. The ability to read and write, which both represents basic literacy skills, are denoted by a binary variable and indicate the measures used to calculate official literacy statistics by the World Bank. The highest years of schooling obtained is consistent with the education variable used by Duflo (2001) and Chou et al. (2010). To address the potential issue of individuals obtaining education through institutions other than schools such as the NGO literacy programs and government campaigns aimed at promoting literacy, I drop those individuals who obtained education through literacy programs.

A section of the NLSS survey that focuses on health requires mothers to list all children who were born starting from the first to last birth order, regardless of whether or not the child is alive. Then a mother (or the respondent) is required to provide information on whether the child is currently alive or not. First, I construct a binary variable where 0 represents the status of a respective mother's child being alive during the survey year; otherwise the value given is 1. Similarly, I create indicators for: 1) Infant mortality, where infant mortality is defined as death of a child before his/her first birthday; and 2) Under five child mortality. Since this study investigates the effect of maternal education on child mortality, the final sample when evaluating mortality outcomes comprise mothers who gave birth to at least one child.

The community level files of the NLSS survey is designed to measure community characteristics and market prices to supplement the individual level survey files. A section of the community level file provides detailed information regarding the construction of schools in the sampled communities in the NLSS. The survey provides a list of schools available in communities where interviews were conducted, including the name of the school, the year of establishment, and school type (pubic versus private). I pool the school data from both survey years, linking communities to appropriate districts where they belong. Then I calculate the district-specific total number of schools available in 1975 per 1000 children (aged 6-9), which is used as a measure of the intensity of the education reform. The population data is obtained from the 1971 Nepal Population Census. The community level file is merged to the individual level file by the district of birth of mothers during survey years 1995 and 2003.

In specifications, I include other individual level characteristics that are plausibly exogenous to the availability of schools in 1975. Such controls include: 1) Ethnicity – a categorical variables for Brahmins,

Chhetris, Newars, and Others; and 2) Religion (Hindu, Buddhist, and Others).[10] The variation in availability of schools in 1975 can itself be influenced by the demand for education. To a certain extent, such an issue can be addressed by controlling for parental educational levels. I create dummy variables to indicate whether an individuals father and mother are literate, respectively.

While analyzing the effect of maternal education on child health, it is necessary to pay attention towards improvement in health sector in the nation. The implementation of the National Health Policy in 1991, which increased access to medical services in rural areas, is of a main concern. Similarly, development projects such as the Water Resource Act (1992) and construction of schools in the 1990s after the advent of the second democratic movement of 1990 poses serious concerns. These potential concerns are thoroughly discussed in the proceeding sections.

4.3 Results (Effect of The Education Reform on Maternal Educational Outcomes)

Table 1 shows the summary statistics of mothers where Group (1) pertains to the whole sample and Groups (2) and (3) represent unexposed (10-to 27-year-olds in 1975) and exposed (0-to 9-year-olds in 1975) cohorts, respectively. Group (2) suggests that only 7 percent of females in older cohort can read. The ability to read among exposed cohort increased by 16 percentage points, compared to older cohort. Similarly, the ability to write improved by 16 percentage points and child mortality rate decreased by 4 percentage points within cohorts (See Table 2). Such within cohort differences in means are statistically significant at a 1 percent level. Mostly importantly, the observed characteristics such as ethnicity and religion are similar between the older and younger cohorts. Table 2 highlights very poor child mortality outcomes, with 12 out of 100 children dying before their 5^{th} birthday.

Table 3 presents the results after estimating equation (1) where the outcome variables are the ability to read, the ability to write, and the highest level of formal education obtained. The results show that the reform improved educational outcomes among children aged 0 to 9 years in 1975. Specifically, one school per 1,000 children following the reform increased the ability to read and write among younger cohort by 13.7 (Column 1) and 12.3 (Column 3) percentage points, respectively. These coefficients are statistically significant at a 1 percent level. Similarly, the coefficients on the highest level of formal schooling completed shown in Columns (5 and 6) are positive, suggesting that the reform improved formal schooling.

The F-statistic pertaining to the interaction between the reform intensity and younger cohort in 1975

[10]Ethnicity is a strong predictor of educational outcomes as the caste system based on Hinduism prefers educating people of higher caste (Brahmins and Chhetris) and deems schooling as unnecessary for people in lower caste due to occupational segregation.

for each specifications are presented in the bottom of Table 3. Such F-statistics are greater than 10 for all of the model specifications, which signifies the strength of the instruments, suggesting that the IV estimates are unlikely to be biased towards the OLS estimates when evaluating child mortality outcomes. However, this requires that the results depicted in Table 3 represent the causal impact of the reform on educational attainment.

In the 1980s, the education policies of the nation was tied to the foreign donors. Two major projects were introduced with an assistance of the foreign donors: 1) The Education for Rural Development Project (ERDP or Seti Project) in 1981; and 2) The Primary Education Project (PEP). The ERDP was a pilot program developed and designed by the UNICEF, UNESCO, and UNDP. The main goal of the project was to demonstrate how education can be promoted in rural parts of the nation through effective means. Three districts of the Far-Western Development Region: Doti, Bajhang, and Bajura, were included in this project. The PEP was the first intervention of the International Development Association (IDA) in educational sector of Nepal and was initiated in a larger scale than the ERDP. The three main goals of the PEP were: 1) To provide low-cost improvement in primary education, 2) Strengthen the administrative and technical aspects of educational sectors; and 3) Initiate programs to improve adult literacy. The project was implemented in several phases between 1985 and 1993 in six accessible districts Dhankuta, Jhapa, Kaski, Tanahu, Surkhet and Dang. Evidence suggests that academic achievement of the PEP students are significantly higher than the non-PEP students. To address the effect of the Seti Project and the PEP, in alternative specifications, I include the interaction term between younger cohort in 1975 and district indicators where the Seti Project and the PEP was initiated, respectively. The results from such model specification provide similar estimates as to Table 3 and are included in the Appendix section (Table A1).

The causal interpretation of the reform on educational outcomes relies upon an assumption that there are no time varying district specific factors omitted in the specification that are correlated with both allocation of schools in 1975 and educational outcomes. If allocation of schools in 1975 is demand-driven, to a certain extent controling for parental literacy accounts for school allocation due to differences in demand for education across districts. Next, I discuss the potential concern of endogeneity in school placement across districts.

Figures 3a and 3b show descriptive summary of educational attainment by age in 1975 across low and high intensity districts. The figures suggest that the percentage of females able to read and the highest level of formal education in control group (10- to 25- year-olds in 1975) who were born in high intensity districts are similar to the percentage of females able to read and years of education in low intensity group of the same age group. As visible in figures, differences among these variables expand between the low intensity and high intensity districts for exposed cohort (0 to 9 years in 1975), with younger individuals born in high intensity

districts achieving better educational outcomes. Although the evidence shown is suggestive, it is assuring to know that trends in educational outcomes are similar across high and low intensity regions among the cohort who had surpassed school-going-age during the time of the reform.

I address the potential issue of endogeneity in school placement across the districts of Nepal in three different ways. First, I additionally include fathers literacy status, which is a strong predictor of district-specific literacy, in the model specifications.[11] Including father's literacy status will account for across-district heterogeneity in education prior to the reform. Second, I conduct a falsification exercise by comparing 10- to 25 year-olds (pseudo exposed group) with 26- to 35 year-olds (control group) in 1975. These individuals had surpassed their school going age during the time of the reform and their educational outcomes should not be affected by the reform. Any results suggesting otherwise will raise concerns regarding the underlying assumption governing the identification strategy. Third, I directly test whether pre-reform educational outcomes among people born in high-reform intensity districts are systematically different from educational outcomes of people born in low-reform intensity districts by using fathers and mothers literacy status as the dependent variable.

Table 4 shows the results from a falsification exercise, which compares females who are of 10- to 25 year-olds with 26- to 35 year-olds in 1975. Both cohorts should not be affected by the reform. If educational outcomes among females was in an upward trajectory in high reform intensity districts prior to the reform, then the coefficient on the interaction between 10- to 25 year-olds in 1975 and the intensity of the reform should be positive and statistically significant in this falsification exercise. If this is the case, the results shown in Table 3 can be spurious – educational outcomes of young cohort in 1975 may have improved even in absence of the reform. The coefficients on the interaction term between younger cohort and the reform intensity from this falsification exercise, as shown in Table 4, are not statistically significant at any conventional levels and are close to zero. As an additional falsification exercise, I shift the age boundary to create pseudo-cohorts and treat them as pseudo-exposed groups. I focus on three pseudo-exposed and unexposed groups: (i) 5-15 year olds in 1975 as the pseudo-exposed group and 16-25 as unexposed group; (ii) 7-17 as exposed and 18-27 as unexposed; (iii) 9-19 as pseudo-exposed and 20-29 as unexposed group. The effects should be diluted as we move from formation (i) to (iii). The results from this falsification, as presented in Table A4, when compared with the main results in Table 3 show that the effects of the reform are narrowly confined to individuals who are most likely to be affected by the reform but are absent among other groups. It should be noted that these falsification exercises provide suggestive evidence that the identification is not obtained from a flawed assumption.

[11] This is mainly due to strong gender prejudice in educational attainment. According to district specific indicators from the Census 1971, male and female literacy rate were 21.5 percent and 3.1 percent, respectively.

Next, I directly use father's and mother's literacy status (respectively) as a dependent variable and estimate equation (1). Conditional on the covariates, the reform should have no effect on parental literacy status. The coefficient on the interaction term between the treatment group and reform intensity are close to zero and statistically insignificant, suggesting that the pre-treatment level of literacy is not systematically different across high and low reform intensity districts (conditioned on the covariates).[12]

Next, in spirit of Duflo (2001), I estimate equation (2), where the coefficients of interest are the interactions between age category in 1975 and the reform intensity. The coefficient β_j describes the effect of the intensity of the reform on individuals who are of j years old in 1975. Figure 4 plots the coefficients of β_j for the outcome variable the ability to read, write, completion of fifth grade and highest schooling attainment, along with the 95 percent confidence levels. Here, j represents every two years of interval (0-1, 2-3, 4-5 year olds and so on in 1975) and extends from 0-1 to 26-27, where 24-27 year olds in 1975 are used as a comparison group. The grouping of two years is preferred as to one because using a single year assigns a low number of people in each group, which results to imprecise and volatile estimates. In Figure 4, the coefficients are above 0 for individuals less than 8 years of age. The coefficient drops for ages 8-9 and 10-11 (in 1975), after which they fluctuate around zero. As hypothesized, the figures show that the effect of the reform decreases by age in 1975.[13]

In the next section, I estimate the reduced form model to evaluate the effect of the education reform on child mortality. Then, I use variation in mother's ability to read, the ability to write, and the highest years of schooling obtained due to the reform to estimate the effect of maternal schooling on infant and child mortality.

5 Effect of Maternal Education on Child Mortality

5.1 Two-Stage-Least Square (2SLS) Estimates

The intensity of the reform can be used to identify the effect of maternal education on child mortality under two assumptions: 1) The reform affected educational outcomes among mothers; and 2) The reform had no effect on child mortality other than improving mother's educational outcomes. Results from the previous section suggests that the reform improved educational outcomes among females of ages 0 to 9 in 1975.

To proceed, I first estimate a basic specification which investigates the relationship between maternal

[12]The results are included in the Appendix Table A5 and shows no evidence of the reform influencing parental educational outcomes.

[13]As a suggestive exercise, I inspect the correlation between reform intensity and employment measure in manufacturing, hydro-power, and commerce sectors using the district level aggregated data from the Census 1971. The results shown in Figure A3 in the Appendix section indicates that the reform intensity is not correlated with employment in manufacturing and hydro-power sectors and negatively correlated with employment in commerce sector.

educational outcomes and child mortality by using the OLS. The specification is given as follows:

$$C_{cidbw} = \alpha + \beta E_{idbw} + \eta X_{idbw} + \gamma_b + \lambda_d + \theta_w + \kappa_c + gender_c + \upsilon_{cidbw}, \tag{3}$$

where, C_{cidbw} is a measure of child mortality specific to a child c from the mother i who was born in district d in year b, currently residing in ward w. E is mother's educational outcome. X includes other control variables (ethnicity, religion). γ_b, λ_d, and θ_w stands for mother's year of birth fixed effects, district of birth fixed effects, and ward of residence fixed effects, respectively. κ_c represents child's birth year fixed effects, which capture year-specific mutual trend in mortality across the nation. Additionally, I control for child's gender to account for gender disparity between sons and daughters.

The OLS estimate of β, which evaluates the effect of maternal education on child mortality can be biased if there exist a correlation between E_{idbw} and υ_{cidbw}. I estimate the following second stage of the 2SLS,

$$C_{cidbw} = \alpha + \beta \widehat{E}_{idbw} + \eta X_{idbw} + \gamma_b + \lambda_d + \theta_w + \kappa_c + gender_c + \xi_{idbw}, \tag{4}$$

where, mother's educational outcome portrayed in equation (3) is replaced by the predicted values of educational outcome after estimating the first stage of the 2SLS by equation (1). Note that the estimates presented in Table 3 represent the first stage estimates.

The 2SLS estimates are biased if there are unobserved variables affecting children's health which are also correlated with the reform intensity. Although no major policies (to the best of my knowledge) were correlated with the reform during the time when the reform was enacted, Nepal went through a major political change in 1991, which marks the year of the second democracy. Three events after the democracy should be emphasized: 1) The establishment of the National Health Policy in 1991; 2) The establishment of the Water Resource Act in 1992; and 3) Spike in school construction in the early 1990s. It is of a serious concern if these policies are systematically related with the intensity of the 1975 reform. I discuss the severity of such potential concerns below.

Figure 6a shows that the number of health posts increased dramatically after the establishment of the National Health Policy in 1991. It is likely that districts experiencing higher intensity of the education reform may eventually receive more health posts following the establishment of the National Health Policy. This likely is a case if better schooling infrastructure and educational outcomes attract superior health infrastructure. In such a case, not accounting for health infrastructure may overstate the 2SLS estimates. On the other hand, legislatures may place more health posts in districts with lower number of schools (per 1000 children) to compensate lack of schooling infrastructure with better health infrastructure. Similarly,

Figures 6c and 6e depict increases in the number of water projects and new schools in the 1990s.[14]

The scatter plots shown in Figures 6b, 6d and 6f plot the relationship between the reform intensity in 1975 and the establishment of new health posts, the number of water projects, and the number of new schools built between 1990-1995. These figures suggest that the allocation of health posts following the health reform, water projects, and new schools across the country in 1990s are not systematically related to the reform intensity. An auxiliary set of regressions that regress the total number of district specific health posts built between 1990 and 1995 (per 100 households), the number of new water projects, and new schools constructed between 1990 and 1995 on the reform intensity measure reveals no statistically significant relationship between the reform intensity and development projects in the 1990s (Table A7). This further validates that the later developments in the 20^{th} century are not systematically related with the reform intensity. Nevertheless, I control for the interaction between the number of health posts established (per 100 households) and younger cohort in 1975 when estimating child mortality outcomes in alternate model specifications.

Along with the number of schools available in 1975, it is ideal to have information regarding the quality of schools such as classroom capacity, availability of facilities, and teacher-student ratio. Information regarding the school quality in 1975 is not available and I view this as a limitation to a certain extent. It has to be noted that missing information regarding school quality does not necessarily overstate the effect of the reform. For example, if the Education Act of 1971 improved school quality as well and if districts with more schools (per 1,000 children) systematically faced positive improvements in school quality, then such an effect will be embedded within the measure of reform intensity used in this study. However, if there is a quality-quantity trade-off, and if districts with lower number of schools per 1,000 children faced better improvement in terms of school quality, the effect of the reform intensity will be understated. Nevertheless, since the first stage is precisely estimated, although the instruments used are imperfect, it provides enough power and seems adequate to identify the effect of maternal education on child health outcomes.

5.2 Reduced Form and the 2SLS Results

Before discussing the 2SLS estimates, I turn attention towards the reduced form estimates of the reform on child mortality outcomes, presented in Table 5. The estimates on the interaction term between younger cohort and the reform intensity are negative and statistically significant at the conventional levels. In contrast, the

[14]It has to be noted that the spike in school construction was not because of any new education policies but rather to meet the population growth and demand for education. To inspect whether the spike in school construction in the early 1990s affects the first-stage estimates, I re-estimate equation (1) by additionally controlling for an interaction between the number of new schools established between 1990-1995 (per 100 people) and exposed cohort (0-9 year olds in 1975). The results are similar to Table 3 (not presented but available upon request), further confirming that school construction in early 1990s is not systematically related to the reform intensity in 1975.

coefficients on the interaction term from the falsification exercise shown in Table 6 that compares artificial exposed group of 10-25 year olds in 1975 with 26-35 year olds are close to zero and statistically insignificant at any conventional levels.

Next, I estimate the event study model specification for child mortality outcomes. The results are presented in Figure 5. The coefficients presented in Figure 5a demonstrates a decreasing trend with lessening of age for age groups below 10-11 (expect for age group 2-3). The coefficients pertaining to age groups 10-11 and above are small and fluctuate around zero. When the dependent variable used is child alive indicator ("1" for if the child is alive), the coefficients show an increase in trend with a reduction in mother's age in 1975 for age groups below 10-11. As a whole the figures suggest that the reform improved under five mortality of children belonging to exposed mothers. Moreover, these figures mirror the trends from the event study model shown in Figure 4. Now, we proceed with the 2SLS results.

Table 7 shows the effect of maternal education on child mortality outcomes by estimating both OLS and IV methods. Columns (1) and (2) use infant mortality as the dependent variable, Columns (3) and (4) use under five mortality, and Columns (5) and (6) use child alive status. Columns (1), (3), and (5) present the OLS estimates, whereas the even columns show the IV results. Panel A uses the ability to write as the independent variable and Panel B uses the highest level of schooling.

The OLS results in Column (1) shows that the ability to write is associated with a reduction in infant mortality rate by 1.6 percentage points. Similarly, the highest level of schooling attained, shown in Columns (1), (3) and (5), negatively affects infant mortality, under five mortality, and child mortality. One more year of schooling is associated with a reduction in infant mortality rate by 0.26 percentage points.

The 2SLS estimates are shown in Columns (2), (4), and (6) of Table 7. All the 2SLS estimates have same signs as the OLS estimates. Columns (2) and (4) in Panels A suggest that a percentage point increase in the ability to write reduces infant and under five child mortality by 0.2 and 0.3 percentage points, respectively. Similarly, the 2SLS estimates when the highest years of schooling is instrumented, as shown in Columns (2) and (4) in Panel B, suggest that one more year of schooling at the lower level reduces infant and under five mortality rates by 3.12 and 4.5 percentage points, respectively.

To interpret the results, let us consider an actual scenario that maternal literacy rate increased from 3.9 percent in 1971 to 25 percent in 1991, an increase of 21.1 percentage points (Bajracharya and Khakha, 2013). Similarly, under five mortality rate decreased from 25 percent in 1971 (Pebley, 1992) to 9.2 percent in 1991[15] – a reduction of 15.8 percentage points. Considering the ability to write as a basic literacy skill, the 2SLS estimate suggests that approximately 40 percent of a reduction in under five mortality can be explained by improvements in literacy rate among women between 1971 and 1991. However, it has to be

[15]See `https://data.worldbank.org/indicator/SH.DYN.MORT`

noted that the 2SLS estimates are interpreted as the local average treatment effect (LATE), which identifies the impact of educational outcomes among the subset of individuals who obtained education because of the reform and otherwise would not have obtained education in absence of the reform.

For the 2SLS method, it is important that the instruments are strong enough, so that the 2SLS estimates are not biased towards the OLS estimates. One way to determine the strength of the instruments is to focus on the F-Statistics from the first stage. As reported in Table 3, the F-Statistics are larger than 10 – a value corresponding to Staiger and Stock (1997) rule of thumb. Also, these test statistics are higher than the critical values for weak instruments as proposed by Stock (2005). Based on these tests, the first stage has good power and the potential issue of weak instruments can be ruled out in this case.

The magnitude of the 2SLS estimates shown in Table 7 are larger than the OLS estimates. There are at least three reasons why the 2SLS estimates can exceed the OLS estimates in this case. First, random measurement error in the variables representing educational outcomes may cause the OLS estimates to be biased towards zero. Second, Card (1999, 2001) shows that the 2SLS estimate does not necessarily demonstrate the average treatment effect but more of an estimate of the local average treatment effect. It is likely that the marginal returns to education may be higher for those individuals who otherwise would have no schooling in absence of the education reform. Such individuals are likely to be from relatively disadvantaged background. In this case, a reasonable comparison between the OLS estimate and the IV estimate may not be feasible. Card (1999, 2001) reviews a handful of studies that estimates the effect of education on earnings by using the quarter of an individual's birth, assignment of lottery numbers during the Vietnam draft, accessibility to schools, and compulsory schooling laws as instruments. The 2SLS estimates from these studies are usually larger than the OLS estimates, lending support to the argument that the marginal returns to education among the groups affected by such institutional changes are higher than the average treatment effect of the population. Hence, it has to be emphasized that the 2SLS interpretation in this study refers to those women who went to school due to the education reform and otherwise would not have attended school in absence of the reform (and would have been illiterate). The returns to basic level of education is likely to be higher for these women. Third, Card (1999) shows that the IV estimates can be biased upwards relative to the OLS estimates if unobserved differences in earnings between the treatment and control group is divided by small differences in schooling. However, Card notes that such a problem is less worrisome if instruments use large variation in education (prevalent within cohorts in this study).

Next, to evaluate whether increases in health posts following the National Health Policy (NHP) in 1991 is influencing the results presented in Table 7, I re-estimate the OLS-IV results by including the control for the interaction between younger cohort in 1975 and new health posts constructed (per 100 households) between 1990 and 1995. The results are shown in Table A8 (reduced form) and Table A9 (IV estimates).

Both the OLS and IV estimates are very similar to the estimates presented in Table 4, which is consistent with Figure 6b showing that the establishment of health posts following the NHP is not systematically correlated with the reform intensity.[16]

As mother's health is closely related to child's health, I estimate the effects of mother's education on her health condition (whether she has any chronic illness). The results are presented in Table 8. Both OLS and IV estimates are negative – the IV estimate in Column (4) indicates that an additional year of education reduces the probability of a mother having chronic illness by 1.2 percentage point. However, both OLS and IV estimates are imprecisely estimated.

5.3 Mechanisms

In this section, I investigate three potential channels through which maternal education can affect child health outcomes: 1) Assortative matching by education, 2) Fertility outcomes; and 3) Household's landholdings.

In developing nations such as Nepal, where the majority of women do not participate in labor market, marriage is arguably the single most assertive determinant of women's economic well-being. Improvements in female's education due to the reform can increase her value in an arranged marriage setting; given the patriarchal society this can lead to marrying husbands with relatively higher educational attainment. Table 9 presents both OLS and IV estimates and provides evidence of educational assortative matching.[17] Columns (1) (OLS) and (2) (IV) in Panel B suggest that an increase in one more year of female's schooling increases husband's schooling level by 0.58 and 0.8 years, respectively. Similarly, estimates in Columns (3) (OLS) and (4) (IV) in panel B indicate that one more year of female's education increases the probability of marrying a husband who has completed 5^{th} grade by 5.6 and 10 percentage points, respectively. These estimates are statistically significant at a 1 percent level.

The evidence of educational assortative matching raises the question of what if the reform directly affected male's educational attainment. If such is the case, the IV estimates capturing the effects of mother's education on child mortality will most likely be biased upwards in failure to properly account for improvements in male's educational attainment. To access the effects of the reform on male's educational attainment, I estimate equation (1) by using male's educational outcomes as the dependent variable. The results are presented in Table 10. The coefficients on the interaction term are negative, close to zero and statistically

[16] To further access whether NHP is driving the main findings, I specifically restrict the sample to only include children born after 1990. NHP will have improved access to health care across cohorts of both exposed and unexposed mothers (0-9 and 10-27 year olds in 1975) in the 1990s within a district, except that exposed mothers will have higher educational attainment. Hence, the effects of NHP will be differenced out by using the unexposed cohort. However, the marginal product of medical services (through NHP) may be higher for exposed mothers due to higher educational attainment compared to unexposed mothers. The IV estimates from such a restricted sample are similar in magnitude to that shown in Table 7. Results are not shown but are available upon request.

[17] I am able to match about 70 percent of women in the sample with their respective spouse using the spouse ID code.

insignificant at the conventional levels for basic literacy variables. Furthermore, I estimate the event-study model given by equation (2) for males. Such results are presented in the Appendix Figure A1. The coefficients on the age-specific interaction terms fluctuate around zero and none of them are statistically significant at the conventional levels. These results indicate that the reform did not affect male's educational outcomes.[18]

Osili and Long (2008) provide instrumental variable estimates indicating that an increase in a mother's education by one year reduces early fertility by 0.26 births. Married women of NLSS are asked specific questions regarding their family planning and fertility decisions, including knowledge regarding contraceptives. Table 11 provides the OLS and IV estimates on the following fertility-related outcomes: i) A woman's age of marriage; ii) Her knowledge regarding contraceptives; iii) Current usage of contraceptives; iv) Whether a health worker visited her home to talk about family planning; v) The total number of children; vi) The total number of children before age 25 (early fertility); vii) A woman's age at the first birth; and viii) Average birth spacing between children (in years). Although the OLS results presented in Table 11 indicate that an increase in women's education is correlated with improvements in fertility outcomes, the IV estimates suggest that improvements in educational attainment due to NESP did not affect the reported fertility outcomes such as the total number of children, early fertility, age of first birth, and birth spacing. This is in contrast to the findings from Osili and Long (2008). The IV estimate pertaining to age of marriage is actually negative, although statistically insignificant. However, IV estimates pertaining to knowledge regarding contraceptives and usage of contraceptives are positive in Columns (2) and (3) of Table 11. This set of finding is interesting as it suggests that although women's education due to NESP did not alter actual fertility outcomes, improvements in education increased knowledge regarding contraceptive devices. In context of a patriarchal society, a female's education improvements at very low levels may not alter fertility decisions of a household, since such decisions may still depend on husbands; nevertheless schooling can improve health knowledge.

When considering the effects of educational outcomes on household landholdings – the major source of wealth in developing nations – the OLS estimates in Table 12 are positive and statistically significant at the 1 percent level in Panel A. The IV estimates shown in Panel B are positive suggesting that improvements in female's education is associated with an increase in her household's likelihood of owning a piece of land, the number of plots of land owned, and the number of cultivable plots. However, the IV estimates are relatively imprecisely estimated compared to the OLS estimates in Panel A. These results provide some evidence that a woman's education can improve financial well-being, which can further improve child health (see Case et al. (2008,0); Currie et al. (2007); Currie and Stabile (2003)).

[18] In a patriarchal society, human capital investment in sons is preferred more than investment in daughters. Hence, sons are likely to be educated regardless of the reform. However, females are the marginalized sub-group, and the effects of the reform such as distribution of free textbooks for females (directly targeted towards reducing gender-gap in education) and free-primary-education provision are likely to be more binding for females.

In summary, the results discussed in this section provide suggestive evidence indicating that education improves economic well-being of females through educational assortative matching. Moreover, females who were affected by the reform are likely to belong to a relatively wealthier households later on in their lives. Also, education can improve health knowledge among females, which can in fact increase mother's awareness in activities that can directly determine children's outcomes. Together these channels can lead to improvements in child mortality outcomes.

6 Threats Against Validity

Several threats against validity should be acknowledged.

Assessing Validity of the Identification. The identification crucially requires that school construction following NESP is not demand driven. I provide a brief summary regarding several lines of evidence developed to support the exogeneity of school construction discussed throughout the paper.

To assess the main findings of the study, I conduct a falsification exercise where 10 to 25 year olds in 1975 are used as the treated pseudo cohort and 26 to 35 year olds comprise of control cohort. The DD estimates from such an exercise, as shown in Table 4, are close to zero and statistically insignificant. As follows, I estimate an event study specification as given in equation 2. The findings from the event study model are portrayed in Figure 4. The coefficients are a decreasing function of age and they are close to zero for individuals of age 10 and over in 1975 – age group unlikely to be affected by the reform. Next, to further assess the validity of the identification, I focus on three pseudo-exposed and unexposed groups: (i) 5-15 year olds in 1975 as the pseudo-exposed group and 16-25 as unexposed group; (ii) 7-17 as exposed and 18-27 as unexposed; (iii) 9-19 as pseudo-exposed and 20-29 as unexposed group. Table A4 provides evidence that the effects of school construction following NESP are narrowly focused on a group of children who are most likely to be affected by NESP and absent among other groups.

NESP reports a respondent's parental educational status, regardless of their current residential status. Parental educational outcomes of individuals aged 0 to 27 year olds in 1975 are predetermined and not affected by the reform. I directly evaluate the effects of the reform on parental literacy status. As shown in Table A5, the coefficient on the interaction term of the DD estimate is close to zero and statistically insignificant. If the episode of school construction were demand-driven, such coefficients would be positive. Finally, Figure A3 suggests that the intensity measure is uncorrelated with employment in manufacturing and hydropower industry in 1971 and actually slightly negatively correlated with employment in commerce industry. Overall, these pieces of evidence suggests that school construction following NESP was not demand-driven and they strengthen the validity regarding the identification strategy used in this study.

Measurement error in child mortality. Since a mother is asked to report the mortality status of a child, it creates a potential issue of measurement error. If misreporting is random across different educational levels, then such type of measurement error will only inflate the standard errors, while still leading to unbiased estimates. It is problematic when misreporting varies systematically with educational outcomes. Such an event can occur if mothers with basic literacy skills are more likely to precisely report child mortality compared to mothers lacking literacy skills. In this case, the OLS and 2SLS estimates will be biased downwards. This happens if mothers who lack basic education fails to report an incidence of infant or child mortality. In contrast, if mothers who lack literacy skills are more likely to correctly report child mortality, the estimates of this study will be biased upwards. However, there is no reason to believe that such might be the case.

Migration. The identification strategy of this study relies upon an implicit assumption that individuals attend school in their district of birth. If across district mobility is random, it induces measurement error which will downward bias the estimate to zero (increase signal to noise ratio). It is more problematic if families migrate across district from low intensity to high intensity districts in search of better education for children. Is mobility endogenous?

Given the high cost of migration such as lack of proper transportation, inadequate roadways, and undulating landscape of the country, it is unlikely that an issue of migration is problematic. However, the potential issue of migration should not be taken for granted.

NLSS 2003 provides a detailed profile regarding migration, which includes district migrated from, age when migrated and reason for migration. Using migration questionnaires, I plot histograms of age of migration (to the current location) and age of marriage in Panels A2a and A2b in Appendix Figure A2. The majority of migration happens after age 14 (82 percent), by when an individual should have surpassed her primary school going age. Moreover, the distribution of age of migration in Panel A2a overlaps with the distribution of marital age in A2b. Figure A2c confirms that the majority of migration happens due to marriage, when a female leaves her initial home. Evidence presented in Figure A2 supports the claim that migration is not endogenous to the reform; it very unlikely for a female to migrate across district for educational purposes.

Accessing Neighborhood Characteristics. It can be the case that the findings of mother's schooling on child mortality is driven by differences in neighborhood or community characteristics that are systematically correlated with the reform intensity. This is feasible if exposed mothers in high reform intensity districts share relatively better infrastructure compared to unexposed mothers. Controlling for ward-level fixed effects remove any influence of time invariant characteristics, including community characteristics that may have influenced children's health. Even in a given ward, groupings by education is possible with edu-

cated mothers residing in households closer to basic amenities. Given the data set, a more direct analysis is plausible.

Both NLSS 1995 and 2003 provide information regarding access to facilities measured by the household's distance to the closest facility including primary school, health post/hospital, paved road, and dirt road.[19] Table A11 provides results estimating the effects of mother's education on distance to the closest facility from both OLS and IV method. Both OLS and IV coefficients suggest that education does not systematically affect allocation of a mother's household with respect to neighborhood amenities.

One concern previously discussed is that the National Health Policy (1991) could have differentially affected exposed mothers born in high reform intensity districts compared to unexposed mothers by allocating health post facilities closer to households of mothers exposed to the reform. However, the results presented in Table A11, Panel C, speaks against such a potential claim. There is no evidence that the construction of health posts following the NHP disproportionately affected exposed mothers in high reform intensity districts. The results that the neighborhood characteristics are not systematically different within exposed and unexposed cohorts across high and low intensity districts is reassuring on behalf of the identification.

Measurement error in availability of schools. Another concern is whether and how many schools that were available in 1975 exited the sample by 1995 and 2003 (survey years). In presence of classical measurement error, being a case where existing of schools are random across districts, the first stage estimates of this study will be biased downwards due to an increase in signal to noise-ratio. In the case of non-classical measurement error, when exiting of schools are positively correlated to the actual number of schools in 1975 (per 1,000 children), the first-stage estimates will be biased upwards. In contrast, when exiting of schools are negatively correlated with the actual number of schools in 1975 (per 1,000 children), the first-stage will be biased downwards.

Unfortunately, due to the lack of availability of data, I am unable to directly address the severity of this problem. Is potential existing of schools between 1975 and the survey years a major concern? Two facts lend support to a belief that exiting of schools between 1975 and the survey year is not a major concern. First, school construction boomed in the second half of the 20^{th} century in Nepal, especially in latter decades (1990s). The country needed more schools to keep par with population growth, improve the literacy rate, and meet demands of people in need of education. Caddell (2007) argues that opening schools became a symbol of the modern, developing nation and a medium through which to transmit this revised vision of the state to the populace. It is unlikely that a significant portion of schools would exit in such circumstances. Second, if exiting of schools posed a serious concern, it unlikely that we would observe a change of trend in educational outcomes between individuals living in high and low intensity reform districts exactly starting

[19] The measure of distance used is in minutes and by foot.

from 8 to 9 years old in 1975 (as shown in Figure 4, the age group who were exposed to the reform).

Sample selection. The effect of mother's education on children's mortality outcomes is estimated in a sample of women who had given birth before the survey – 82% of women in the sample of females in NLSS aged 0-27 years in 1975 gave birth to a child before the survey.[20] To evaluate whether and to what extent sample selection is driving the results, I follow Heckman (1977) and account for sample selection. In the first step, I predict the probability of giving birth (any birth) on individual and demographic characteristics. Then I control for the inverse of Mill's ratio in the 2SLS specification to account for the selection problem. The results from such exercise are presented in Table A14. The point estimates are of similar magnitude to the IV estimates in Table 7.

Two-way clustering of standard errors. Until now the standard errors are clustered at mother's district of birth to account for correlation in the error term within a district. To consider correlation in error terms within cohorts as well as district of birth, I present the standard errors after two-way clustering on district of birth and cohort-level following Cameron et al. (2008). The finding are reported in Tables A12 (education) and A13 (reduced form on child mortality). The standard errors obtained from two-way clustering are in fact slightly smaller compared to the standard errors presented in the main findings, Table 3 (education) and Table 5 (child mortality).

7 Comparison with Earlier Literature

How do the estimates of this study compare to those in the existing literature? This is a difficult question to answer, mainly because all of the previous empirical work is focused on much higher levels of maternal education than being used in this study. One of the contributions of this paper is that it investigates the effect of basic level of maternal education on infant and child mortality. Currie and Stabile (2003) and McCrary and Royer (2006) investigate the effect of maternal education on child health outcomes in the United States. Currie and Moretti focus on college level education and McCrary and Royer consider high school level or more. Breierova and Duflo (2004) and Chou et al. (2010) investigate the effect of education on child health outcomes in Indonesia and Taiwan, respectively. The average years of schooling in the sample of these studies are 6.67 (Breirova and Duflo) and 9.53 (Chou et al.), which are still considerably greater but relatively closer to this study. If education directly affects health by improving health production function (Grossman, 1972), following the law of diminishing returns, it is expected that improvements in education at the lower levels may have higher returns to health.

[20] Given that relatively small portion of women (18%) did not give birth to a child, the potential issue of sample selection may not be severe.

First, I compare my 2SLS estimates of increases in the highest level of formal schooling with Breirova and Duflo's findings. Breirova and Duflo focus in a developing nation and use significant increases in school establishments as instruments to identify the casual effect of education on child health outcomes. The average infant mortality rate in Indonesia as reported by Breirova and Duflo (6 percent) is lower than the average of this study. Their 2SLS estimates suggest that one more year of schooling reduces the number of children who died before the first birthday by 0.06. This corresponds to a reduction in infant mortality rate by approximately 2 percentage points (calculation performed on averages). This result is strikingly similar to the findings reported in Table 7, Column (2) (Panel B) of this study, which indicates that one more year of maternal schooling reduces infant mortality by 2.6 percentage points. Second, I relate my findings with the meta-analysis performed by Gakidou et al. (2010). The authors use 915 sources of nationally representative data from 219 countries including Nepal. The authors estimate that 51.2 percent of a reduction in under five mortality between 1970 and 2009 can be attributed to improved educational outcomes among women. Specifically, in South Asia, women's education accounts for 39.1 percent of a reduction in child deaths. Although, this study differs from Gakidou et al.'s in several aspects such as timeframe, empirical method, and data usage, referring to the 2SLS estimates of this study, approximately 40 percent of a reduction in under five mortality can be attributed to improvements in women's literacy rate between 1971 and 1991. In summary, the findings of this study are quite similar to the meta-analysis.

8 Conclusion

This paper adds to the existing literature by investigating the effect of basic maternal educational outcomes such as the ability to read, the ability to write, and the highest level of schooling on infant and child mortality outcomes. Given the law of diminishing returns, the Preston curve, and the policy importance provided in educating females, it is pertinent to understand the heterogeneous effects of mother's education on children's health. The estimates suggest that increases in maternal literacy rate following the educational reform of 1971 had both economic and statistically significant impact on child mortality. The 2SLS coefficient suggest that an increase in mother's ability to write by one percentage point and one more year of schooling reduces under five mortality by 0.3 and 4.5 percentage points, respectively. In developing nations, mothers play an important role in preventing or curing children from diseases such as pneumonia, pre-term birth complications, diarrhoeal diseases, and malaria, which are some of the leading causes of child mortality.

However, I caution that the 2SLS estimates should not be interpreted as the average treatment effects. The marginal benefit of schooling for females who are affected by the education reform may be higher than the effect among the general population. Hence, the 2SLS estimates should be interpreted as the effect of

education on child mortality outcomes specific to mothers who otherwise would not have received even basic level of education had the education reform not been implemented.

Caution should be provided while relating the study's findings in context of other nations. In 1971, infant mortality in Nepal was 17.5 percent compared to 1.9 and 1.8 percent in the United States and the United Kingdom, respectively. The findings of this study can be related to developing nations in South East Asia and Africa where improvements in educational outcomes and child health continue to be a leading priority. Reducing child mortality by two-thirds between 1990 and 2015 was one of the objectives set by the Millennium Development Goals (MDG). Although child mortality has been reduced by almost half over the past two decades, it remains as one of the goals lagging farthest behind in comparison to other MDGs. The majority of child deaths are preventable or curable and occurs during the first five years of birth. This study provides evidence that improvements in basic levels of maternal education can help improve child health outcomes. As education and health capital are jointly determined by a household, improvements in mother's education in developing nations due to a supply side reform can also affect children's educational outcomes. Empirical studies along such lines can shed more light on jointly determined nature of two major forms of human capital – health and education, as highlighted by Ehrlich and Yin (2013).

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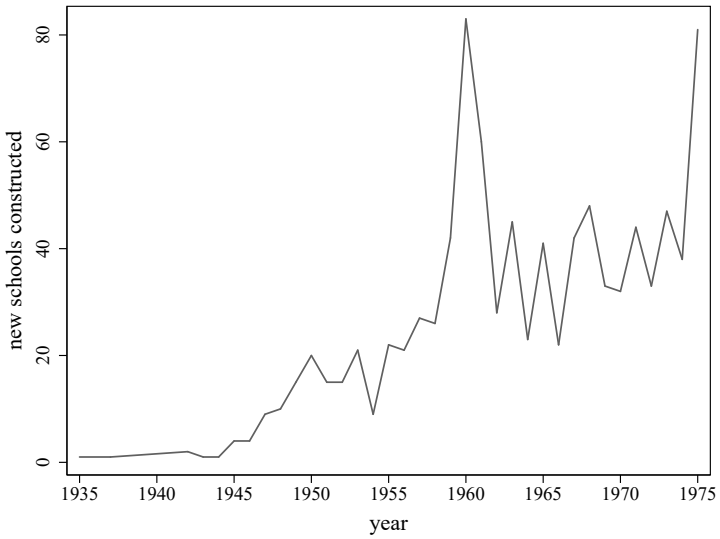
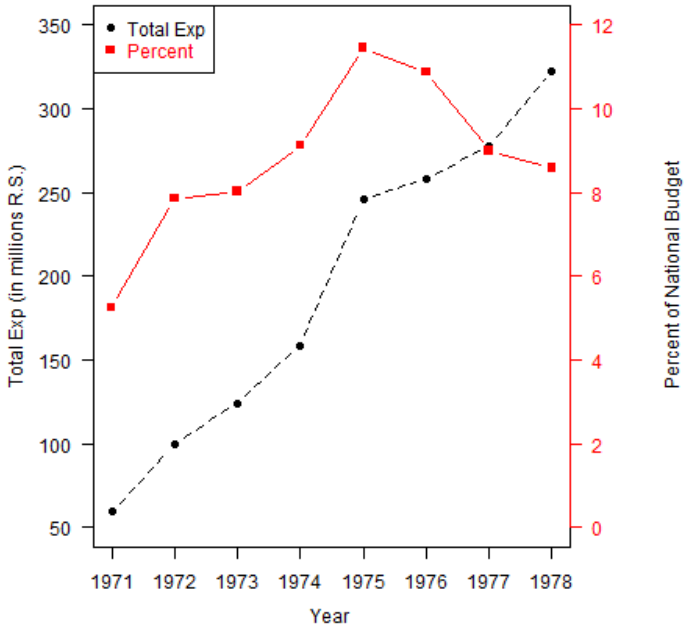
9 Figures and Results

FIGURE 1: New Schools Constructed and Expenditure Over Time

(A) New Schools

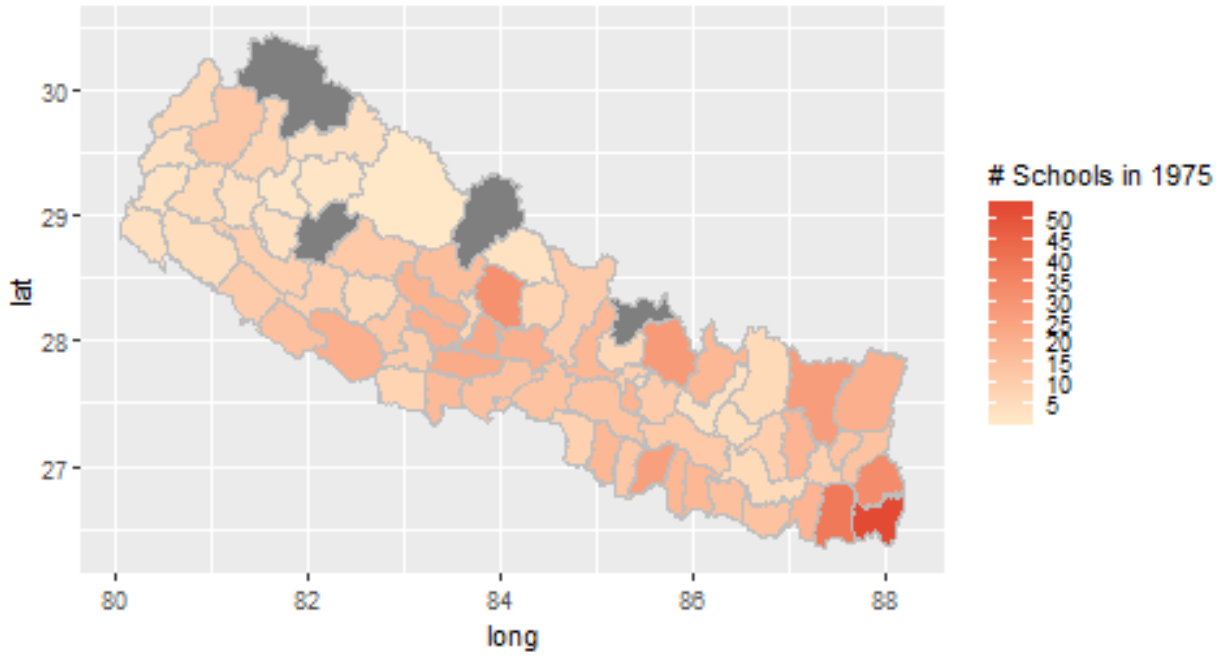


(B) Expenditure



Note: The data for year of school operation is extracted from the Nepal Living Standard Survey (NLSS) community files 1995 and 2003. The community files provide a detailed information regarding the year of school's operation, type of school (private versus public) and quality measures (but only during the survey year). The data for expenditure comes from the Ministry of Education in Nepal.

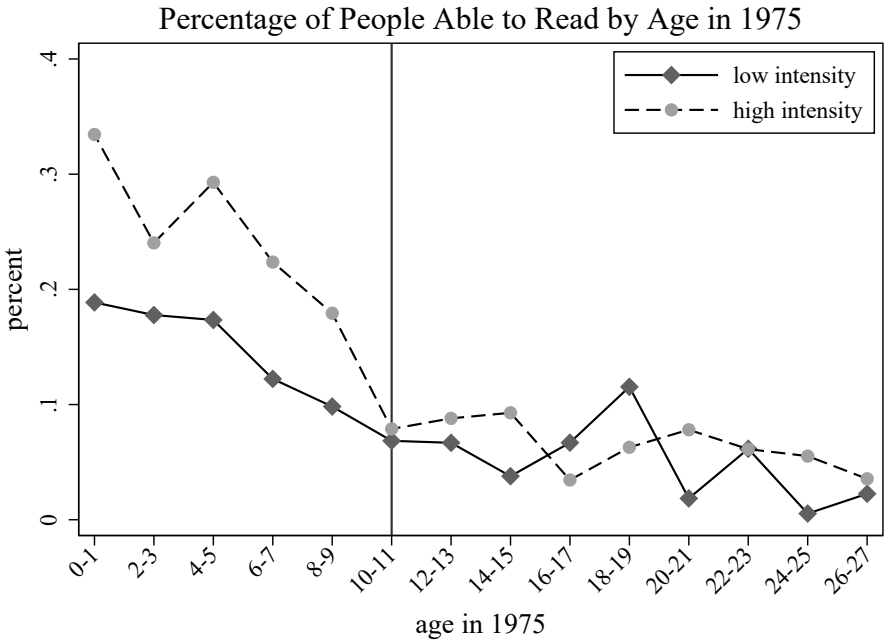
FIGURE 2: Availability of Schools Across Districts



Note: The map above shows the number of schools available in 1975 across districts using community level files from NLSS 1995 and 2003. Insufficient data on school availability for four districts: Humla, Mustang, Jajarkot, and Rasuwa.

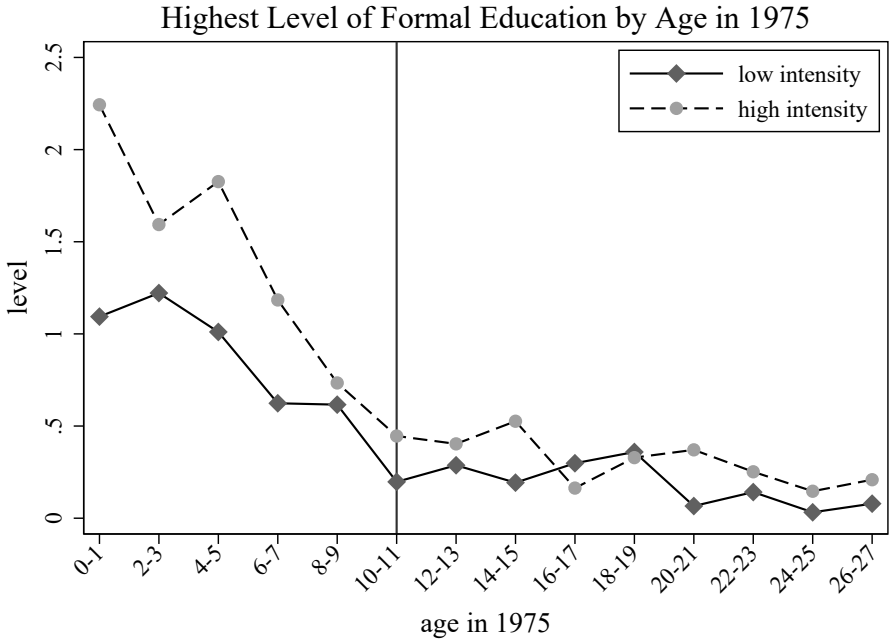
FIGURE 3: Educational Outcomes by Age Groups and Intensity Regions

(A) Ability to Read



Source: NLSS 1995 and 2003

(B) Highest Level of Schooling

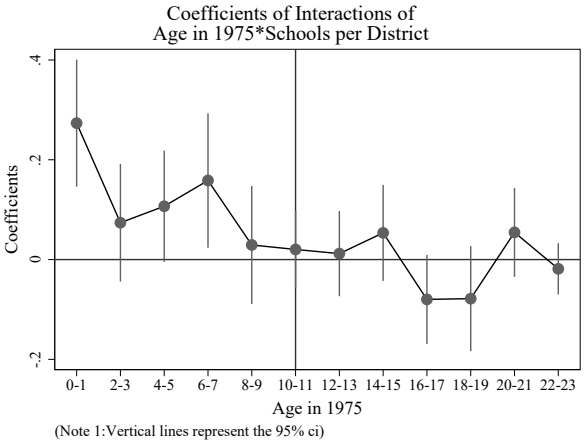


Source: NLSS 1995 and 2003

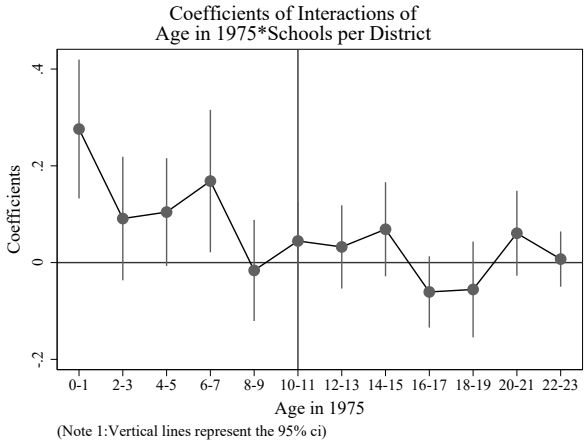
Note: The figures above show the unconditional trends in educational outcomes by age groups in 1975 across high and low intensity regions. Here, high intensity regions are classified as districts having more schools (per 1,000 children) in 1975 than the median. These figures are used precisely to motivate the layout of identification; the continuous measure of reform intensity is used when conducting estimations.

FIGURE 4: Event Study

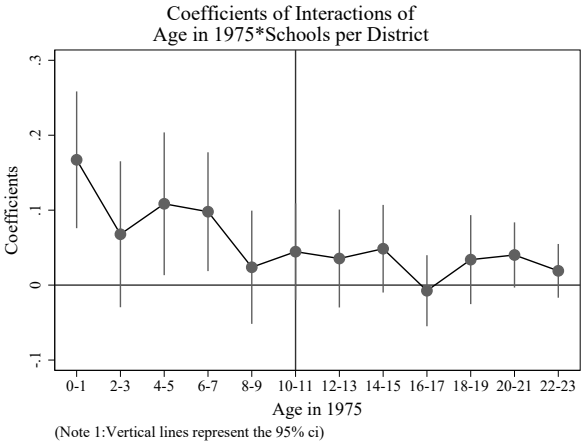
(A) Ability to Read



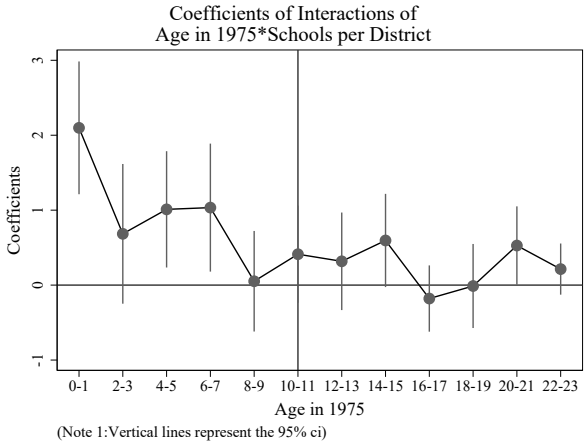
(B) Ability to Write



(C) Fifth Grade



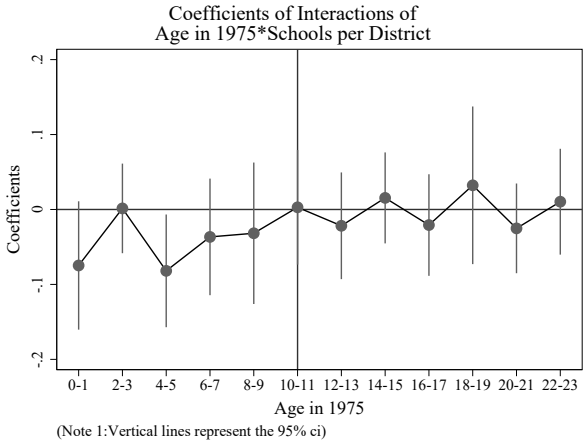
(D) Highest Education



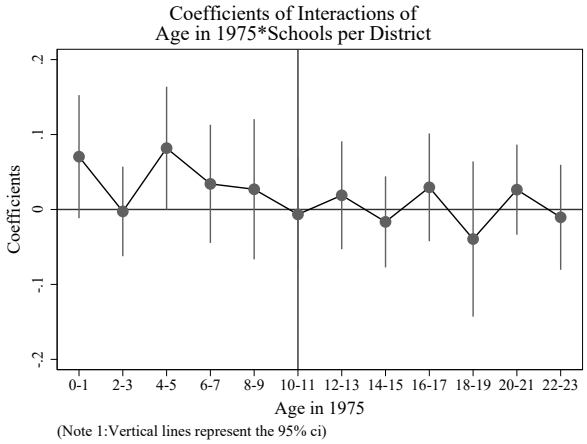
Note: The figures in panels above represent the coefficients from the event study model specifications from equation (2) when measures of women's educational attainment (the ability to read, write, fifth grade completion, and the highest level of schooling), obtained during the survey year, are used as the dependent variable. Age groups 24-27 in 1975 are omitted. The bars represent the 95 percent confidence intervals. The model includes ethnicity dummies, religion dummies, mother's literacy status, survey year indicator, year of birth fixed effects , district of birth and ward of residence fixed effects. The model also includes interactions between exposed cohort and i) ethnicity, ii) religion, and iii) parent's literacy status, respectively. Standard errors are obtained by clustering at the district of birth level.

FIGURE 5: Event Study (Mortality Outcomes)

(A) Under 5 Mortality



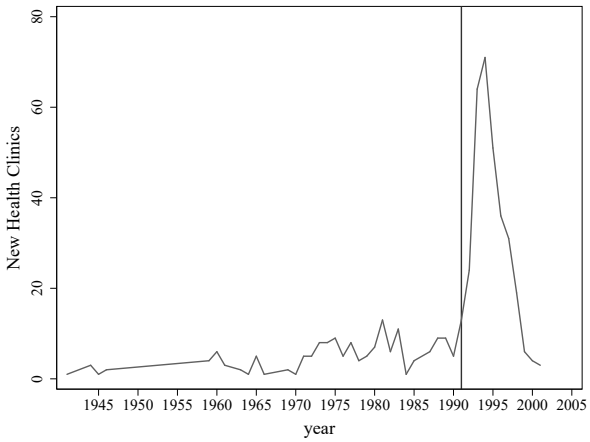
(B) Child Alive



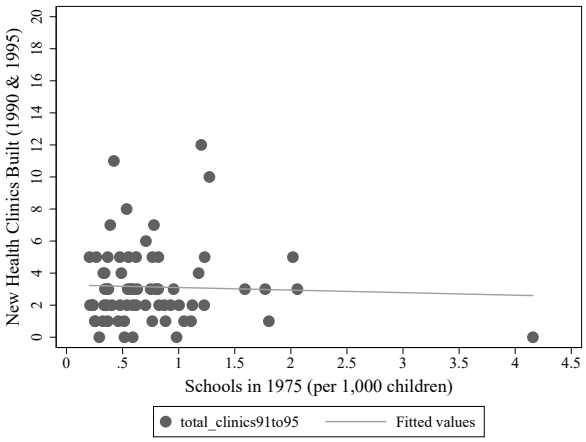
Note: The figures in panels above represent the coefficients from the event study model specifications when measures of child mortality (under five mortality and dead-alive status of a child) obtained during the survey year are used as the dependent variable. Age groups 24-27 in 1975 is the omitted category. The bars represent the 95 percent confidence intervals. The model includes ethnicity dummies, religion dummies, mother's literacy status (child's grandmother's), father's literacy status (child's grandfather's) survey year indicator, year of birth fixed effects (both mother's and child's), district of birth and ward of residence fixed effects. The model also includes interactions between exposed cohort and i) ethnicity, ii) religion, and iii) parent's literacy status, respectively. Standard errors are obtained by clustering at mother's district of birth level.

FIGURE 6: National Health Policy (1991), Water Projects and Schools in 1990s

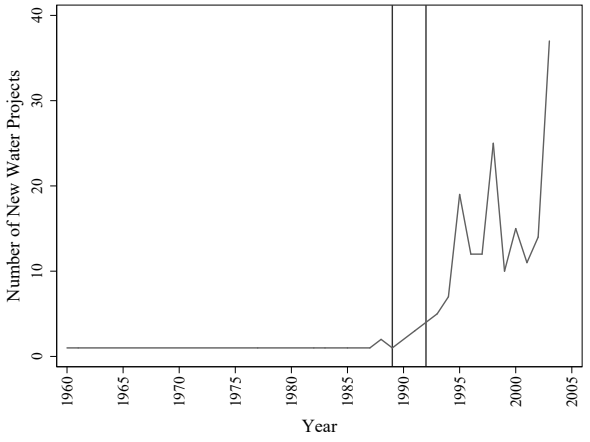
(A) Health Posts Constructed Over Time



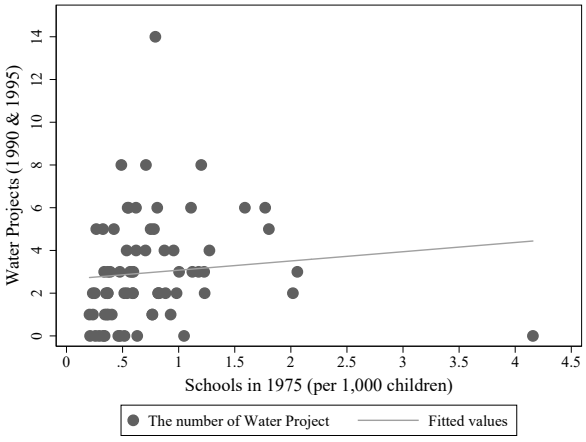
(B) Health Posts and Intensity Measure (1975)



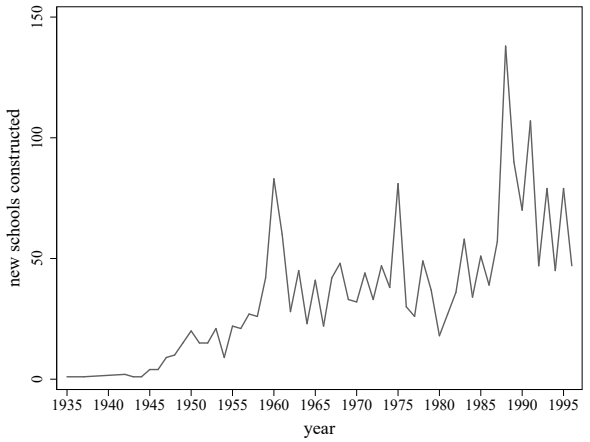
(C) Water Projects Over Time



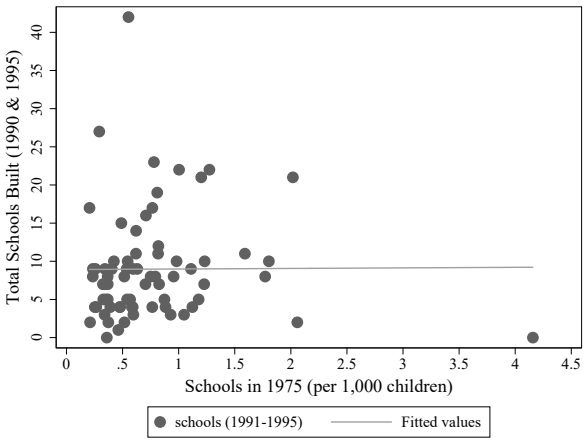
(D) Water Projects and Intensity Measure (1975)



(E) School Construction by Year



(F) Schools Built and Intensity Measure (1975)



Note: Figure 6a shows the number of new health posts in operation by year. A massive increase in health posts is visible after the National Health Policy in 1991, implemented with the goal of increasing access to medical services in rural parts of the nation. Data is extracted from the community level files of NLSS 1995 and 2003. Figure 6b presents a scatter plot between the reform intensity (schools in 1975, per 1000 children) and new health posts established between 1990 and 1995 along with the best fit line. Figure 6c shows the establishment of water projects by year and figure 6d plots the scatter plot between new water projects and the reform intensity along with the best fit line. Figure 6e shows new schools built by year and figure 6f plots the relationship between the intensity measure and new schools built between 1990 and 1995.

Table 1: Summary Statistics (Mother's Sample)

	(1) 0-27 Yrs		(2) 10-27 Yrs.		(3) 0-9 Yrs	
	Mean	St.dev.	Mean	St.dev.	Mean	St.dev.
Ability to Read	0.139	0.346	0.067	0.250	0.230	0.421
Ability to Write	0.124	0.329	0.051	0.219	0.215	0.411
Highest Schooling	0.832	2.410	0.301	1.419	1.496	3.125
Father Literate	0.160	0.366	0.111	0.314	0.221	0.415
Mother Litetate	0.023	0.151	0.017	0.130	0.031	0.174
Chhetri	0.198	0.399	0.190	0.392	0.209	0.407
Brahmin	0.138	0.345	0.141	0.348	0.135	0.342
Newar	0.043	0.203	0.043	0.204	0.043	0.202
Other Ethnicity	0.620	0.485	0.626	0.484	0.614	0.487
Hindu	0.846	0.361	0.844	0.363	0.849	0.358
Buddhist	0.077	0.267	0.082	0.275	0.071	0.257
Other Religion	0.076	0.266	0.074	0.262	0.080	0.271
Age in 1975	11.179	8.032	17.230	5.229	3.574	2.709
Schools per 1000 children	0.718	0.451	0.733	0.474	0.699	0.420
Total Schools in 1975 (in sample)	15.631	9.584	15.434	9.292	15.878	9.937
Total Health Posts in 1975	0.315	0.639	0.122	0.358	0.539	0.800
Heath Posts (per 100 hh)	0.470	0.357	0.489	0.358	0.447	0.356
Literacy Rate 1971	12.429	5.388	12.526	5.410	12.305	5.358
Population of 6-9 year olds (1975)	23757.278	10781.749	23276.358	10679.925	24361.670	10881.141
Wave	1.468	0.499	1.480	0.500	1.453	0.498
N	4184		2330		1854	

Note: The table presents summary statistics for the main sample used in the study. Group (1) pertains to the whole sample (0-27 year olds in 1975); group (2) comprises of unexposed cohort (10-27 year olds in 1975), who are likely to have surpassed primary school attending age during the time of the reform. Group (3) includes exposed cohort of 0-9 year olds in 1975. One's ability to read and write are basic literacy outcomes and are used by the World Bank and the UNESCO to measure literacy rates in developing nations.

TABLE 2: Summary Statistics (Children's Sample)

	(1)		(2)		(3)	
	0-27 Yrs		10-27 Yrs.		0-9 Yrs	
	Mean	St.dev.	Mean	St.dev.	Mean	St.dev.
Child Alive	0.875	0.331	0.857	0.350	0.907	0.290
Infant Mortality	0.092	0.289	0.099	0.298	0.079	0.269
Under 5 Mortality	0.121	0.326	0.137	0.344	0.090	0.286
Child Gender	0.519	0.500	0.523	0.499	0.513	0.500
Birth Order	2.971	1.857	3.286	1.993	2.392	1.403
N	13466		8714		4752	

Note: The table presents summary statistics for mortality status of children born to females reported in Table 1. The NLSS 1995 and 2003 reports detailed profile of child's outcomes for all married women, including date, month and year of birth, child alive status, if dead – age of death (in days, months, and year), which is used to calculate infant and under five mortality.

Table 3: Effect of the Reform on Educational Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Read	Read	Write	Write	Highest Educa.	Highest Educa.
0-9 (1975)*Intensity	0.137***	0.141***	0.123***	0.127***	0.835***	0.857***
	(0.0358)	(0.0340)	(0.0381)	(0.0364)	(0.210)	(0.191)
Mother Literate	0.311***	0.238***	0.333***	0.267***	2.442***	2.061***
	(0.0703)	(0.0735)	(0.0693)	(0.0728)	(0.553)	(0.586)
Father Literate		0.135***		0.123***		0.717***
		(0.0312)		(0.0314)		(0.232)
N	4184	4184	4184	4184	4136	4136
$r2$	0.362	0.406	0.367	0.411	0.410	0.452
F	14.66	17.07	10.48	12.06	15.84	20.21

Note: Additionally, all specifications control for religion and ethnicity fixed effects, and the interactions of religion and ethnicity indicators with the cohort of 0-9 year olds in 1975 (exposed cohort). All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and its interaction with the exposed cohort; the even columns include both father's and mother's literacy status and their interactions with the exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE 4: Effect of the Reform on Educational Outcomes (Falsification Exercise)

	(1)	(2)	(3)	(4)	(5)	(6)
	Read	Read	Write	Write	Highest Educa.	Highest Educa.
10-25 (1975)*Intensity	-0.0026	-0.0128	0.0049	-0.0027	0.0620	0.0245
	(0.0123)	(0.0132)	(0.0142)	(0.0148)	(0.1051)	(0.1066)
Mother Literate	0.3033***	0.2279***	0.3348***	0.2677***	2.5315***	2.1533***
	(0.0689)	(0.0711)	(0.0670)	(0.0684)	(0.5195)	(0.5318)
Father Literate		0.1277***		0.1136***		0.6402***
		(0.0286)		(0.0284)		(0.2042)
N	2926	2926	2926	2926	2891	2891
r2	0.3023	0.3214	0.3139	0.3336	0.3533	0.3676

Note: The pseudo exposed group comprises of 10-25 year olds (pseudo-exposed cohort) and unexposed group includes 26-35 year olds. Additionally, all specifications control for religion and ethnicity fixed effects, and the interactions of religion and ethnicity indicators with the exposed cohort. All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and its interaction with the exposed cohort; the even columns include both father's and mother's literacy status and their interactions with the exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table 5: Effect of the Reform on Child Mortality

	(1)	(2)	(3)	(4)	(5)	(6)
	Infant Mort.	Infant Mort.	U5 Mort.	U5 Mort.	Child Alive	Child Alive
0-9 (1975)*Intensity	-0.0254*	-0.0256*	-0.0369**	-0.0376**	0.0355**	0.0362**
	(0.0134)	(0.0133)	(0.0156)	(0.0154)	(0.0156)	(0.0154)
Mother Literate	-0.0508*	-0.0479*	-0.0406	-0.0288	0.0508	0.0409
	(0.0264)	(0.0278)	(0.0365)	(0.0377)	(0.0360)	(0.0371)
Father Literate		-0.0054		-0.0211		0.0179
		(0.0159)		(0.0179)		(0.0194)
N	13466	13466	13466	13466	13466	13466
r2	0.0865	0.0865	0.1022	0.1023	0.1026	0.1027

Note: Columns (1) and (2) use infant mortality, (3) and (4) use under five mortality, and (5) and (6) use child alive status as the dependent variable. Additionally, all specifications control for religion and ethnicity fixed effects, and the interactions of religion and ethnicity indicators with the cohort of 0-9 year olds in 1975 (exposed cohort). All specifications include survey year dummy, year of birth dummies (both mother's and child's), district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and mother's literacy status interacted with the exposed cohort; the even columns include both father's and mother's literacy status and their interactions with the exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE 6: Effect of the Reform on Child Mortality (Falsification)

	(1)	(2)	(3)	(4)	(5)	(6)
	Infant Mort.	Infant Mort.	U5 Mort.	U5 Mort.	Child Alive	Child Alive
10-25 (1975)*Intensity	-0.0001	-0.0053	-0.0013	-0.0133	0.0031	0.0135
	(0.0131)	(0.0156)	(0.0137)	(0.0146)	(0.0136)	(0.0153)
Mother Literate	-0.0596*	-0.0611*	-0.0465	-0.0373	0.0557	0.0483
	(0.0311)	(0.0329)	(0.0433)	(0.0442)	(0.0433)	(0.0441)
Father Literate		0.0001		-0.0207		0.0179
		(0.0168)		(0.0195)		(0.0213)
N	10503	10503	10503	10503	10503	10503
r2	0.1036	0.1041	0.1210	0.1221	0.1204	0.1215

Note: The pseudo exposed group include mothers of 10-25 year olds and unexposed group includes 26-35 year olds in 1975. Additionally, all specifications control for religion and ethnicity fixed effects, and the interactions of religion and ethnicity indicators with the pseudo-exposed cohort. All specifications include survey year dummy, year of birth dummies (both mother's and child's), district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and its interaction with the exposed cohort; the even columns include both father's and mother's literacy status and their interactions with the exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table 7: OLS and IV Estimates

(a) Panel A

	(1) Infant Mort (OLS)	(2) Infant Mort (IV)	(3) U5 Mort (OLS)	(4) U5 Mort (IV)	(5) Child Alive (OLS)	(6) Child Alive (IV)
OLS (write)	-0.0158		-0.0125		0.0156	
	(0.00955)		(0.0119)		(0.0122)	
IV (write)		-0.202*		-0.297**		0.286**
		(0.105)		(0.122)		(0.122)
N	13466	13466	13466	13466	13466	13466
r2	0.0864	0.0865	0.102	0.102	0.102	0.103

(b) Panel B

	(1) Infant Mort (OLS)	(2) Infant Mort (IV)	(3) U5 Mort (OLS)	(4) U5 Mort (IV)	(5) Child Alive (OLS)	(6) Child Alive (IV)
OLS (highest_educa)	-0.00263**		-0.00281*		0.00310**	
	(0.00131)		(0.00152)		(0.00153)	
IV (highest_educa)		-0.0312*		-0.0453**		0.0435**
		(0.0158)		(0.0182)		(0.0183)
N	13312	13312	13312	13312	13312	13312
r2	0.0879	0.0880	0.103	0.104	0.104	0.104

Note: The table presents OLS and IV estimates. The dependent variable is infant mortality (in Columns 1 and 2), under five mortality (in Columns 3 and 4), and child mortality (in Columns 5 and 6). The IV specifications use the interaction between the exposed cohort (0-9 years in 1975)*reform intensity as instruments (from equation 1). Ability to write and the highest level of schooling is instrumented in Panels A and B, respectively. All specifications control for father's literacy status, mother's literacy status, interactions of father's and mother's literacy with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies (both mother's and children's), district of birth and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE 8: OLS and IV Estimates (Chronic Illness)

	(1)	(2)	(3)	(4)
	Chronic	Chronic	Chronic	Chronic
OLS (write)	-0.0122			
	(0.0224)			
IV (write)		-0.0820		
		(0.0715)		
OLS(highest_education)			-0.0043	
			(0.0032)	
IV (highest_educa)				-0.0118
				(0.0101)
N	3429	3429	3386	3386
r2	0.2260	0.2262	0.2270	0.2268

Note: The table presents OLS and IV estimates. The dependent variable is mother's health condition (chronic illness). The IV specifications use the interaction between the exposed cohort (0-9 years in 1975)*reform intensity as instruments (from equation 1). Ability to write and the highest level of schooling are instrumented. All specifications control for father's literacy status, mother's literacy status, interactions of parental literacy status with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies, district of birth and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE 9: Women's Education and Husband's Education Attainment (Assortative Matching)

	(1)	(2)	(3)	(4)	(5)	(6)
	Highest Educ. (OLS)	Highest Educ. (IV)	Fifth Grade (OLS)	Fifth Grade (IV)	Tenth Grade (OLS)	Tenth Grade (IV)
OLS (write)	3.662***		0.357***		0.244***	
	(0.287)		(0.0351)		(0.0319)	
IV (write)		5.853**		0.729***		0.405**
		(2.823)		(0.270)		(0.179)
N	2903	2903	2903	2903	2903	2903
r2	0.508	0.459	0.426	0.389	0.372	0.317

	(1)	(2)	(3)	(4)	(5)	(6)
	Highest Educ. (OLS)	Highest Educ. (IV)	Fifth Grade (OLS)	Fifth Grade (IV)	Tenth Grade (OLS)	Tenth Grade (IV)
OLS (highest_educa)	0.575***		0.0560***		0.0449***	
	(0.0394)		(0.00475)		(0.00503)	
IV (highest_educa)		0.794**		0.101***		0.0538**
		(0.380)		(0.0358)		(0.0238)
N	2873	2873	2873	2873	2873	2873
r2	0.514	0.456	0.433	0.388	0.406	0.311

Note: The table presents OLS and IV estimates. The dependent variable is spouse's educational outcomes: highest education (in Columns 1 and 2), completion of fifth grade (in Columns 3 and 4), and completion of tenth grade (in Columns 5 and 6). The IV specifications use the interaction between the exposed cohort (0-9 years in 1975)*reform intensity as instruments (from equation 1). Ability to write and the highest level of schooling is instrumented in Panels A and B, respectively. All specifications control for father's literacy status, mother's literacy status, interactions of parental literacy status with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies, district of birth and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE 10: The reform did not affect male's educational attainment

	(1)	(2)	(3)	(4)	(5)	(6)
	Read	Read	Write	Write	Highest Educa.	Highest Educa.
0-9 (1975)*Intensity	-0.0259	-0.0221	-0.0308	-0.0184	0.113	0.0644
	(0.0328)	(0.0330)	(0.0323)	(0.0312)	(0.302)	(0.328)
Mother Literate	0.143*	-0.00129	0.149*	0.00279	2.133***	0.744
	(0.0853)	(0.0929)	(0.0861)	(0.0939)	(0.796)	(0.762)
Father Literate		0.327***		0.331***		3.088***
		(0.0396)		(0.0367)		(0.316)
N	3530	3530	3530	3530	3480	3480

Note: Additionally, all specifications control for religion and ethnicity fixed effects and their interactions with the exposed cohort. All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and its interaction with the exposed cohort. The even columns include both mother's and father's literacy status, and interactions of parental literacy status with the exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE 11: Women's Education and Fertility Outcomes

(A) Panel A

	(1) Age Marriage	(2) Know Contraceptive	(3) Use Contraceptive	(4) Health Worker Visit	(5) Total Children	(6) Total Children (25)	(7) Age First Birth	(8) Birth Space
OLS (highest_educa)	0.203***	0.0248***	0.0124***	0.0113***	-0.0844***	-0.0672***	0.120	0.0148
	(0.0302)	(0.00401)	(0.00405)	(0.00391)	(0.0129)	(0.0104)	(0.110)	(0.0128)
N	3505	3523	3523	3523	4136	4136	3624	3386
r2	0.322	0.402	0.353	0.309	0.360	0.282	0.163	0.235

(B) Panel B

	(1) Age Marriage	(2) Know Contraceptive	(3) Use Contraceptive	(4) Health Worker Visit	(5) Total Children	(6) Total Children (25)	(7) Age First Birth	(8) Birth Space
IV (highest_educa)	-0.348	0.0345	0.0530*	0.0196	-0.00325	0.0520	0.110	0.0909
	(0.398)	(0.0422)	(0.0290)	(0.0350)	(0.177)	(0.136)	(1.515)	(0.186)
N	3505	3523	3523	3523	4136	4136	3624	3386
r2	0.312	0.394	0.350	0.306	0.355	0.275	0.163	0.235

Note: The table presents OLS (Panel A) and IV (Panel B) estimates. The dependent variable comprises of fertility outcomes: i) Age of marriage, ii) Knowledge regarding contraceptives (an indicator for whether a woman knows about contraceptives), iii) Whether a woman uses contraceptive (an indicator), iv) Whether a health worker visited her home to talk about family planning, v) The total number of children, vi) The total number of children (before 25), vii) Age of first birth, and viii) Birth spacing. The IV specifications use the interaction between the exposed cohort (0-9 years in 1975)*reform intensity as instruments (from equation 1). All specifications control for father's literacy status, mother's literacy status, interactions of parental literacy status with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies, district of birth and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table 12: Women's Education and Household's Landholdings

	(1)	(2)	(3)
	Own Land	Number of Plots	Number of Cultivable Plots
OLS (highest_educa)	0.00959***	0.0809***	0.0697***
	(0.00336)	(0.0224)	(0.0167)
N	4136	4136	4136
r2	0.322	0.446	0.397
	(1)	(2)	(3)
	Own Land	Number of Plots	Number of Cultivable Plots
IV (highest_educa)	0.0280	0.347*	0.181
	(0.0229)	(0.180)	(0.110)
N	4136	4136	4136
r2	0.319	0.445	0.394

Note: The table presents OLS (Panel A) and IV (Panel B) estimates. The dependent variable refer to landholdings: i) Whether a household owns any land, ii) The total number of plots owned, and iii) The total number of cultivable plots. The IV specifications use the interaction between the exposed cohort (0-9 years in 1975)*reform intensity as instruments (from equation 1). The results present estimates when the highest years of schooling is instrumented. All specifications control for father's literacy status, mother's literacy status, interactions of parental literacy status with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies, district of birth and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

10 Online Appendix

Can Basic Maternal Literacy Skills Improve Infant Health Outcomes? Evidence from the Education Act in Nepal

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10.1 Additional Robustness Exercise

Several additional robustness exercises are conducted to validate the main results of the paper, which are briefly summarized below and the results are presented in the Appendix Section.

Region of Birth and Year of Birth Interaction Term. I also include region of birth indicators interacted with year of birth in an alternative model specification. The alternative model specification includes the following interaction terms: 1) Region of birth dummies (i. Eastern, ii. Central, iii. Western, iv. Mid-western, v. Far-western) interacted with year of birth, respectively; 2) Ethnicity dummies interacted with an indicator of whether an individual is of age 0-9 in 1975 (treated cohort); and 3) Mother's and father's literacy status interacted with the treated cohort, respectively. The results from this specification is shown in Table A2 in the Appendix section. The coefficients on the interaction term are positive, statistically significant at the 1 percent level (except for column (3), significant at the 5 percent level), and consistent to the main findings of the study.

Using Variable based on the Median of Intensity Measure. Table A3 in the Appendix section shows the results when using an indicator variable to represent the reform intensity. The indicator takes a value "1" if intensity measure is above the median, otherwise the value given is "0." This new form of intensity measure is interacted with cohort indicator representing 0-9 year olds in 1975. The DD results presented in Table A3 are consistent with the main findings of the paper.

Forming Pseudo Cohorts. As an exercise, I shift the age boundary to create pseudo-cohorts and treat them as the exposed group. I focus on three pseudo-exposed and unexposed groups: (i) 5-15 year olds in 1975 as the pseudo-exposed group and 16-25 as unexposed group; (ii) 7-17 as exposed and 18-27 as unexposed; (iii) 9-19 as pseudo-exposed and 20-29 as unexposed group. The effects should be diluted as we move from formation (i) to (iii).

Table A4 in the Appendix shows the results from this robustness exercise. The results are diluted as the age boundary are shifted from (i) to (iii). Moreover, the findings presented in Table 4, falsification exercise, depicts the null result. These pieces of evidence together support the identification strategy used in this study.

Using Parental Educational Outcomes as Predetermined Variables. One advantage of using the data from the Nepal Living Standard Survey (NLSS) is that the survey provides a detailed information regarding educational attainment of a respondent's parent regardless of their current living status. Parent's educational outcomes of individuals aged 0-27 year olds in 1975 are predetermined and not influenced by NESP. I directly estimate the effect of the reform on parent's literacy status. Given that the intensity measure used in this study is demand-driven, we expect to see positive estimates on the coefficient representing the interaction between 0-9 year olds in 1975 and the intensity measure when the dependent variable used is parental literacy status. However, the results presented in Table A5 (Appendix) show that the coefficients on the interaction term are both economically and statistically close to zero. This provides additional evidence that the demand for education did not drive school construction following the reform.[21]

Balance Exercise. Educational reform is unlikely to affect parent's work status (wage versus self employ-

[21]This point has been highlighted in footnote 12 of the main paper.

ment), ethnicity or religion status. I conduct a balance exercise by using these variables as the dependent variables. The results, as shown in Table A6, indicate that the coefficients on the DD estimates are close to zero and statistically insignificant at any conventional levels.

Table A1: Effect of the Reform on Educational Outcomes (Including Seti and PEP Projects)

	(1)	(2)	(3)	(4)	(5)	(6)
	Read	Read	Write	Write	Highest Educa.	Highest Educa.
0-9 (1975)*Intensity	0.140***	0.144***	0.126***	0.130***	0.873***	0.905***
	(0.0352)	(0.0345)	(0.0380)	(0.0374)	(0.209)	(0.204)
Mother Literate	0.318***	0.185**	0.340***	0.215***	2.484***	1.587***
	(0.0705)	(0.0708)	(0.0694)	(0.0698)	(0.553)	(0.580)
0-9 (1975)*Seti Project	-0.166***	-0.147***	-0.166***	-0.148***	-1.192***	-1.064***
	(0.0579)	(0.0528)	(0.0577)	(0.0531)	(0.413)	(0.382)
0-9 (1975)*PEP Project	0.0322	0.0371	0.0306	0.0352	0.0892	0.113
	(0.0650)	(0.0679)	(0.0697)	(0.0721)	(0.373)	(0.394)
Father Literate		0.224***		0.212***		1.494***
		(0.0235)		(0.0227)		(0.175)
N	4184	4184	4184	4184	4136	4136
$r2$	0.364	0.403	0.370	0.408	0.412	0.447
F	15.73	17.45	11.03	12.16	17.36	19.77

Note: The Seti project was implemented in several phases between 1985 and 1993 in six accessible districts – Dhankuta, Jhapa, Kaski, Tanahu, Surkhet and Dang. The PEP Project was conducted in three districts of the Far-Western Development Region: Doti, Bajhang, and Bajura. Additionally, all specifications control for religion and ethnicity fixed effects and their interactions with exposed cohort. All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. The odd columns include mother's literacy status and an interaction between mother's literacy status and exposed cohort; the even columns include both mother's and father's literacy status and interactions between parental literacy status and the exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table A2: Effect of the Reform on Educational Outcomes (Controlling for Year of Birth Interacted with Birth Region)

	(1)	(2)	(3)	(4)	(5)	(6)
	Read	Read	Write	Write	Highest Educa.	Highest Educa.
0-9 (1975)*Intensity	0.0989***	0.107***	0.0816**	0.0892***	0.524***	0.581***
	(0.0309)	(0.0301)	(0.0324)	(0.0318)	(0.161)	(0.151)
Mother Literate	0.323***	0.245***	0.346***	0.274***	2.542***	2.114***
	(0.0715)	(0.0743)	(0.0704)	(0.0735)	(0.562)	(0.593)
Father Literate		0.141***		0.130***		0.770***
		(0.0304)		(0.0303)		(0.225)
N	4184	4184	4184	4184	4136	4136
$r2$	0.373	0.414	0.381	0.422	0.424	0.464
F	10.29	12.67	6.337	7.869	10.59	14.76

Note: All specifications control for religion and ethnicity fixed effects; the specifications include the following interaction terms: 1) Region of birth dummies (i. Eastern, ii. Central, iii. Western, iv. Mid-western, v. Far-western) interacted with year of birth, respectively; 2) Ethnicity dummies interacted with an indicator of whether an individual is of age 0-9 in 1975 (treated cohort); and 3) Mother's literacy status interacted with the exposed cohort. All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. Columns (2), (4), (6) additionally include father's literacy status and an interaction term between father's literacy status and the treated cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table A3: Effect of the Reform on Educational Outcomes (Using Median of Reform Intensity)

	(1)	(2)	(3)	(4)	(5)	(6)
	Read	Read	Write	Write	Highest Educa.	Highest Educa.
0-9 (1975)*Intensity	0.134***	0.137***	0.119**	0.121***	0.758**	0.781***
	(0.0447)	(0.0404)	(0.0495)	(0.0456)	(0.308)	(0.274)
Mother Literate	0.304***	0.231***	0.328***	0.262***	2.404***	2.029***
	(0.0696)	(0.0725)	(0.0689)	(0.0721)	(0.553)	(0.584)
Father Literate		0.133***		0.121***		0.702***
		(0.0312)		(0.0313)		(0.232)
N	4235	4235	4235	4235	4187	4187
$r2$	0.361	0.404	0.366	0.410	0.408	0.451
F	8.973	11.43	5.735	7.059	6.058	8.135

Note: Additionally, all specifications control for religion and ethnicity fixed effects, and the interactions of religion and ethnicity indicators with the cohort of 0-9 year olds in 1975 (exposed cohort). All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and its interaction with the exposed cohort; the even columns include both father's and mother's literacy status and their interactions with the exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table A4: Effect of the Reform on Educational Outcomes (Using Different Pseudo Exposed Groups)

(a) Panel A

	(1) Write	(2) Write	(3) Write	(4) Fifth Grade	(5) Fifth Grade	(6) Fifth Grade	(7) Hightest Educa	(8) Hightest Educa	(9) Hightest Educa
Pseudo-Age Group*Intensity	0.0644**	0.0316	-0.000485	0.0369	0.0188	0.0119	0.422*	0.194	0.0859
	(0.0311)	(0.0217)	(0.0156)	(0.0224)	(0.0143)	(0.0105)	(0.221)	(0.134)	(0.109)
Mother Literate	0.315***	0.269**	0.203*	0.278***	0.260**	0.230**	2.480***	2.219***	1.846*
	(0.102)	(0.104)	(0.103)	(0.0986)	(0.0988)	(0.115)	(0.811)	(0.787)	(0.977)
N	2980	2767	2521	2941	2730	2487	2941	2730	2487
$r2$	0.363	0.344	0.347	0.359	0.339	0.335	0.409	0.384	0.380

(b) Panel B

	(1) Write	(2) Write	(3) Write	(4) Fifth Grade	(5) Fifth Grade	(6) Fifth Grade	(7) Hightest Educa	(8) Hightest Educa	(9) Hightest Educa
Pseudo-Age Group*Intensity	0.0670**	0.0288	-0.00146	0.0381*	0.0176	0.0109	0.435**	0.176	0.0755
	(0.0307)	(0.0208)	(0.0154)	(0.0223)	(0.0140)	(0.0104)	(0.218)	(0.129)	(0.110)
Mother Literate	0.284***	0.228**	0.178*	0.275***	0.248**	0.235**	2.359***	2.030**	1.781*
	(0.102)	(0.101)	(0.101)	(0.0979)	(0.0967)	(0.113)	(0.814)	(0.771)	(0.957)
Father Literate	0.0683	0.0781**	0.0534*	0.0159	0.0238	-0.00218	0.326	0.375	0.186
	(0.0423)	(0.0353)	(0.0309)	(0.0217)	(0.0238)	(0.0172)	(0.238)	(0.226)	(0.191)
N	2980	2767	2521	2941	2730	2487	2941	2730	2487
$r2$	0.399	0.370	0.376	0.378	0.346	0.343	0.438	0.404	0.402

Note: As an additional robustness exercise, different pseudo-exposed groups are tested. Columns (1), (3) and (6) treats 5-15 year olds in 1975 as the exposed group and 16-25 as unexposed group; in columns (2), (4) and (8) 7-17 as exposed and 18-27 as unexposed; and in columns (3), (6) and (9) 9-19 as exposed and 20-29 as unexposed group. All specifications control for religion and ethnicity fixed effects, mother's literacy status; the specifications include the following interaction terms: 1) Ethnicity and religion dummies interacted with an indicator of whether an individual is of age 0-9 in 1975 (exposed cohort); and 2) Mother's literacy status interacted with the treated cohort. All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. Panel B additionally includes father's literacy status and an interaction term between father's literacy status and exposed cohort. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table A5: Effect of the Reform on Parent's Literacy

(a) Panel A

	(1) Mother Literate	(2) Father Literate
0-9 (1975)*Intensity	0.00918	-0.0188
	(0.0130)	(0.0251)
N	4184	4184
$r2$	0.203	0.260

(b) Panel B

	(1) Mother Literate	(2) Father Literate
IV (highest education)	0.00993	-0.0311
	(0.0144)	(0.0242)
N	4136	4136
$r2$	0.211	0.316

Note: The table presents results from specifications that estimate the effect of the reform on parent's literacy skills. Parents here refers to parents of 0-27 year olds in 1975. Parent's literacy skills are predetermined during the time of the reform (in 1975). Given that the validity of the identification strategy is maintained the reform should not affect parental literacy skills. All specifications include ethnicity and religion dummies, their interactions with exposed cohort, survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table A6: Balance Exercise

	(1) Mother Work	(2) Father Work	(3) Brahmin	(4) Chhetri	(5) Hindu	(6) Buddhist
0-9 (1975)*Intensity	-0.0182	-0.00772	-0.00542	-0.00842	0.0166	0.00132
	(0.0249)	(0.0398)	(0.0296)	(0.0262)	(0.0246)	(0.0229)
N	2093	2126	4235	4235	4235	4235
$r2$	0.322	0.324	0.536	0.427	0.541	0.587

Note: The table presents results from specifications that estimate the effect of the reform on parent's employment status, ethnicity and religion categories. Given that the validity of the identification strategy is maintained the reform should not affect such dependent variables. All specifications include ethnicity and religion dummies, their interactions with exposed cohort, survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE A7: Reform Intensity in 1975 and Later Developments

	(1)	(2)	(3)
	schools 1990-1995	clinics 1990-1995	water project 1990-1995
Reform Intensity	0.0733	-0.160	0.432
	(1.452)	(0.492)	(0.508)
N	71	71	71

Note: The table presents results from specifications that estimate the effect of the reform on school construction, health post construction, and water projects between 1990 and 1995 at the district level. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE A8: Effect of the Reform on Child Mortality (accounting for the rise in health posts in the early 1990s)

	(1)	(2)	(3)	(4)	(5)	(6)
	Infant Mort.	Infant Mort.	U5 Mort.	U5 Mort.	Child Alive	Child Alive
0-9 (1975)*Intensity	-0.0234*	-0.0235*	-0.0333**	-0.0336**	0.0316**	0.0319**
	(0.0129)	(0.0125)	(0.0148)	(0.0142)	(0.0148)	(0.0143)
Mother Literate	-0.0368**	-0.0339**	-0.0300	-0.0218	0.0357*	0.0277
	(0.0152)	(0.0163)	(0.0189)	(0.0202)	(0.0188)	(0.0197)
Father Literate		-0.0047		-0.0133		0.0130
		(0.0103)		(0.0119)		(0.0124)
N	13466	13466	13466	13466	13466	13466
r2	0.0847	0.0848	0.1009	0.1012	0.1012	0.1016

Note: The table replicates the results shown in Table 2. Additionally, specifications control for religion and ethnicity fixed effects, and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies (both mother's and child's), district of birth fixed effects and ward of residence fixed effects. The odd columns include an interaction between mother's literacy status and the exposed group. The even columns include both mother's and father's literacy status, and the interactions between parental literacy status and exposed group. Additionally, the specifications control for the interaction between the number of new health post established between 1990 and 1995 (per 100 households) and 0-9 years cohort in 1975. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE A9: Effect of the Reform on Child Mortality (accounting for rise in health posts in the early 1990s)

	(1)	(2)	(3)	(4)	(5)	(6)
	Infant Mort (OLS)	Infant Mort (IV)	U5 Mort (OLS)	U5 Mort (IV)	Child Alive (OLS)	Child Alive (IV)
OLS (write)	-0.0172*		-0.0136		0.0167	
	(0.0095)		(0.0120)		(0.0123)	
IV (write)		-0.1867*		-0.2648**		0.2515**
		(0.1013)		(0.1141)		(0.1148)
N	13466	13466	13466	13466	13466	13466
r2	0.0847	0.0847	0.1009	0.1011	0.1013	0.1015

	(1)	(2)	(3)	(4)	(5)	(6)
	Infant Mort (OLS)	Infant Mort (IV)	U5 Mort (OLS)	U5 Mort (IV)	Child Alive (OLS)	Child Alive (IV)
OLS (highest_educa)	-0.00292**		-0.00292*		0.00317**	
	(0.00126)		(0.00154)		(0.00157)	
IV (highest_educa)		-0.0276*		-0.0390**		0.0371**
		(0.0147)		(0.0165)		(0.0166)
N	13312	13312	13312	13312	13312	13312

Note: The table replicates results from 7. Additionally, specifications control for religion and ethnicity fixed effects, and their interactions with the exposed cohort. All specifications control for father's literacy status, mother's literacy status, interactions of parental literacy status with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies (both mother's and child's), district of birth fixed effects and ward of residence fixed effects. Additionally, the specifications control for the interaction of new health posts constructed between 1990 and 1995 (per 100 households) and mothers who were of age 0-9 years in 1975. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table A11: Effect of the Reform on Neighborhood Characteristics

	(1) School Distance	(2) Health Post Distance	(3) Distance Dirt Road	(4) Distance Paved Road
OLS (highest_educa)	-0.00289	-0.0115	-0.00213	-1.927
	(0.00281)	(0.00722)	(0.0467)	(2.783)
N	4136	4136	4136	4117
r2	0.205	0.444	0.861	0.877
	(1) School Distance	(2) Health Post Distance	(3) Distance Dirt Road	(4) Distance Paved Road
IV (highest_educa)	-0.0502	0.142	-0.340	4.714
	(0.0571)	(0.197)	(0.650)	(39.92)
N	4136	4136	4136	4117
r2	0.205	0.444	0.861	0.877
	(1) School Distance	(2) Health Post Distance	(3) Distance Dirt Road	(4) Distance Paved Road
0-9 (1975)*Intensity	-0.0451	0.125	-0.286	3.037
	(0.0497)	(0.172)	(0.564)	(34.97)
N	4184	4184	4184	4165
r2	0.203	0.445	0.861	0.877

Note: The table presents the results when the dependent variables used include the distance from household to school, health post, nearest dirt road and paved road (in minutes by foot). It is likely that infrastructure development over time is systematically correlated with the reform intensity in 1975. All specifications control for father's literacy status, mother's literacy status, interactions of parental literacy status with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. All specifications include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test. The results from Table A11 suggest that there are no systematic differences in neighborhood characteristics between exposed and unexposed cohorts.

TABLE A12: Effect of the Reform on Educational Outcomes (Two-Way Clustering)

	(1)	(2)	(3)	(4)	(5)	(6)
	Read	Read	Write	Write	Highest Educa.	Highest Educa.
0-9 (1975)*Intensity	0.137***	0.141***	0.123***	0.127***	0.835***	0.857***
	(0.0300)	(0.0286)	(0.0306)	(0.0290)	(0.179)	(0.161)
Mother Literate	0.311***	0.238***	0.333***	0.267***	2.442***	2.061***
	(0.0543)	(0.0506)	(0.0515)	(0.0489)	(0.510)	(0.482)
Father Literate		0.135***		0.123***		0.717***
		(0.0226)		(0.0234)		(0.163)
N	4184	4184	4184	4184	4136	4136
$r2$	0.362	0.406	0.367	0.411	0.410	0.452

Note: Additionally, all specifications control for religion and ethnicity fixed effects, and the interactions of religion and ethnicity indicators with the cohort of 0-9 year olds in 1975 (exposed cohort). All specifications also include survey year dummy, year of birth dummies, district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and its interaction with the exposed cohort; the even columns include both father's and mother's literacy status and their interactions with the exposed cohort. Robust standard errors clustered at the district of birth and cohort-level are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

Table A13: Effect of the Reform on Child Mortality (Two-Way Clustering)

	(1)	(2)	(3)	(4)	(5)	(6)
	Infant Mort.	Infant Mort.	U5 Mort.	U5 Mort.	Child Alive	Child Alive
0-9 (1975)*Intensity	-0.0254**	-0.0256**	-0.0369***	-0.0376***	0.0355***	0.0362***
	(0.0124)	(0.0124)	(0.0143)	(0.0141)	(0.0137)	(0.0135)
Mother Literate	-0.0508***	-0.0479**	-0.0406**	-0.0288	0.0508***	0.0409**
	(0.0184)	(0.0187)	(0.0206)	(0.0217)	(0.0196)	(0.0203)
Father Literate		-0.0054		-0.0211*		0.0179
		(0.0117)		(0.0125)		(0.0138)
N	13466	13466	13466	13466	13466	13466
r2	0.0865	0.0865	0.1022	0.1023	0.1026	0.1027

Note: Columns (1) and (2) use infant mortality, (3) and (4) use under five mortality, and (5) and (6) use child alive status as the dependent variable. Additionally, all specifications control for religion and ethnicity fixed effects, and the interactions of religion and ethnicity indicators with the cohort of 0-9 year olds in 1975 (exposed cohort). All specifications include survey year dummy, year of birth dummies (both mother's and child's), district of birth fixed effects and ward of residence fixed effects. The odd columns also include mother's literacy status and its interaction with the exposed cohort; the even columns include both father's and mother's literacy status and their interactions with the exposed cohort. Robust standard errors clustered at mother's district of birth and cohort-level are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

TABLE A14: Effect of the Reform on Child Mortality (accounting for sample selection)

(A) Panel A

	(1)	(2)	(3)
	Infant Mortality	U5 Mortality	Child Alive
IV (write)	-0.1890*	-0.2686**	0.2549**
	(0.1004)	(0.1122)	(0.1125)
N	13466	13466	13466
r2	0.0839	0.0990	0.0992

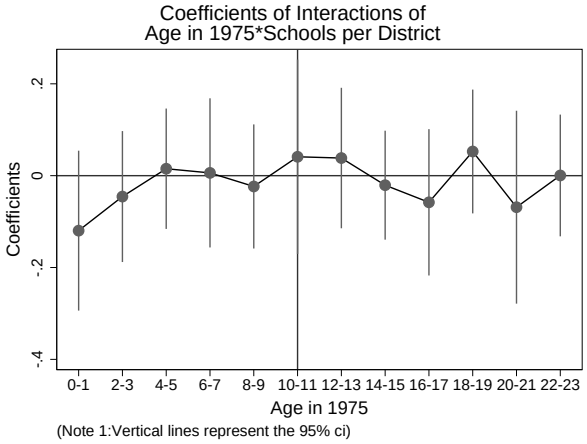
(B) Panel B

	(1)	(2)	(3)
	Infant Mortality	U5 Mortality	Child Alive
IV (highest education)	-0.0281*	-0.0397**	0.0377**
	(0.0145)	(0.0162)	(0.0162)
N	13312	13312	13312
r2	0.0855	0.1005	0.1009

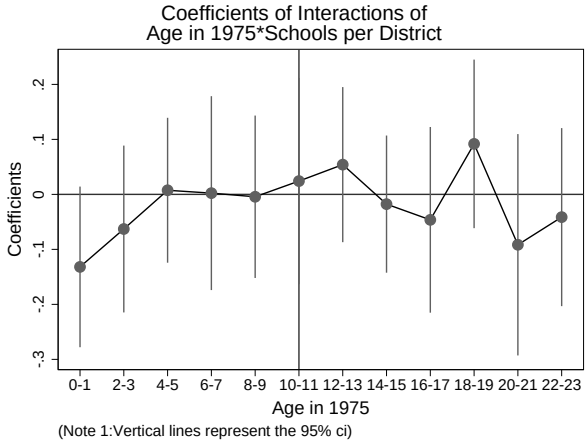
Note: The table replicates the IV results from Table 7 by accounting for sample selection following Heckman (1977). In the first step, an indicator of whether a woman has given birth before the survey period is regressed on covariates such as fathers literacy status, mothers literacy status, interactions of parental literacy status with the exposed cohort, religion dummies and ethnicity dummies and their interactions with the exposed cohort. Then the inverse of mills ratio is calculated and controlled for in specification pertaining to 2SLS. Additionally, all 2SLS specifications include survey year dummy, year of birth dummies (both mother's and child's), mother's district of birth fixed effects and ward of residence fixed effects. Robust standard errors clustered at the district of birth are reported in parenthesis. ***, **, and * represent significance at the 1 percent, 5 percent, and 10 percent levels, respectively, in a two-tailed test.

FIGURE A1: Event Study (Male's Education Attainment)

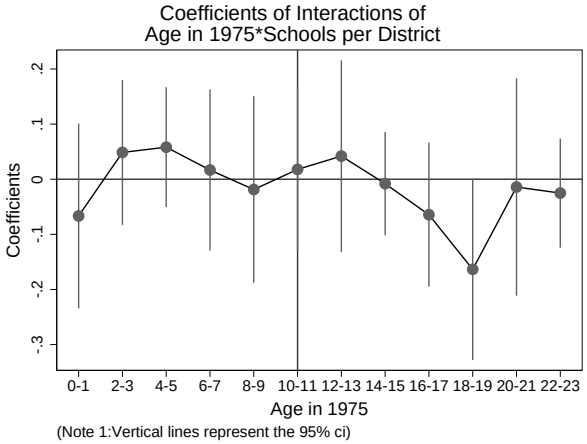
(A) Ability to Read



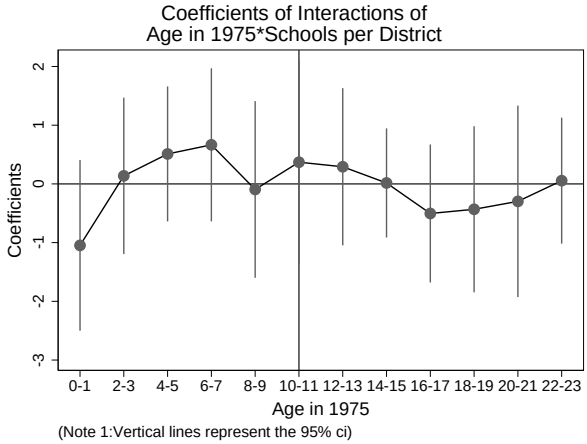
(B) Ability to Write



(C) Fifth Grade



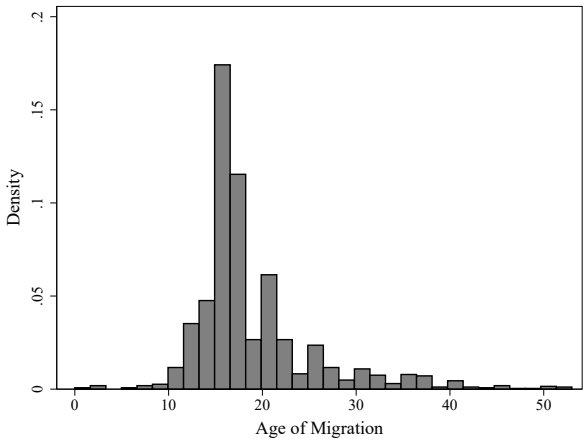
(D) Highest Education



Note: The figures in panels above represent the coefficients from the event study model specifications when measures of male's educational attainment (ability to read, write, fifth grade completion, and highest level of schooling) obtained during the survey year are used as the dependent variable. Age groups 24-27 in 1975 is the omitted category. The bars represent the 95 percent confidence intervals. The model includes ethnicity dummies, religion dummies, mother's literacy status, survey year indicator, year of birth fixed effects, district of birth and ward of residence fixed effects. Standard errors are obtained by clustering at the district of birth level.

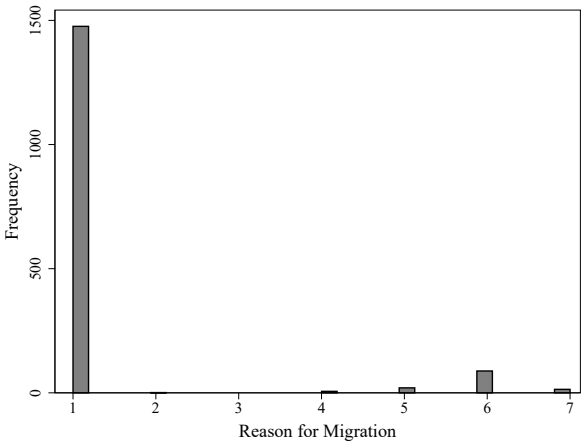
FIGURE A2: Migration

(A) Age Migrated



(B) Age of Marriage

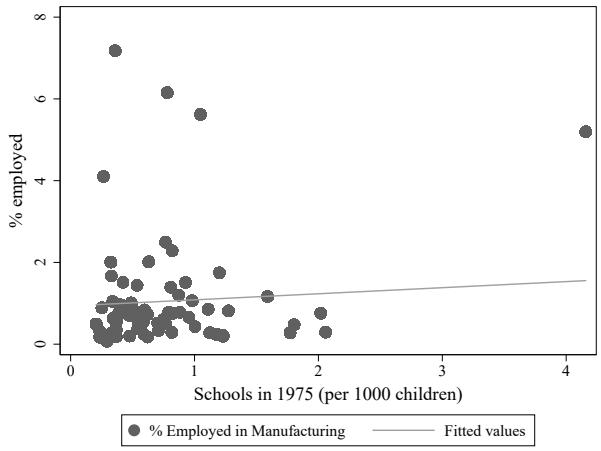
(C) Reason for Migration



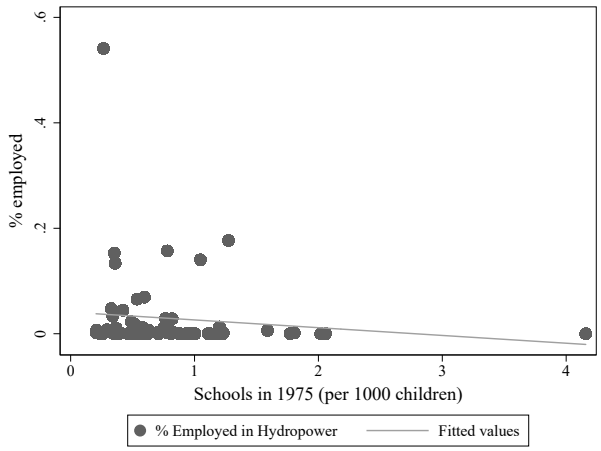
Note: The figures above pertain to females in the main sample (0-27 year olds in 1975) in survey year 2003. Survey year 1995 in NLSS lacks detailed questionnaire regarding migration. In NLSS 2003, 76.11 percent of females reported migrating to their current place of residence out of which approximately 40 percent migration happened across district. However, about 92 percent migrated due to family reason and less than 0.5 percent migrated for educational purposes. The distribution of migration age and age of marriage coincides with one another, suggesting that the majority of migration among females happen due to marriage. Code 1 in Figure A2c represents migration due to family reasons (marriage), 2 – educational and training purposes, 3 – political reasons, 4 – natural disaster, 5 – work-related migration, 6 – easier lifestyle, and 7 other reasons.

FIGURE A3: Reform Intensity and Employment (various sectors)

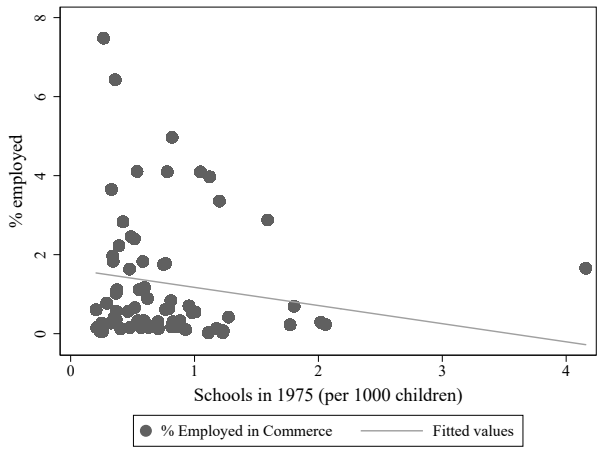
(A) Manufacturing and Intensity Measure



(B) Hydropower and Intensity Measure



(C) Commerce and Intensity Measure



Note: Data is extracted from NLSS 1996 and 2003 community level files and 1971 Census. The employment measures are at the district level.