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Intergenerational Effect of Education Reform Program and Maternal Education on Children's Educational and Labor Outcomes: Evidence from Nepal

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We examine a potential intergenerational determinant of child labor by investigating the effect of maternal education on children's educational and labor outcomes. To account for endogeneity of mother's education, we use the Nepal Education System Plan (NESP) (1971), one of the first education reforms in the country, as an exogenous source of variation. We find that NESP increased educational outcomes among females that were most likely affected by the reform due to their birth year and district of birth. Furthermore, an increase in mother's highest level of schooling increases a child's probability of finishing 5^{th} grade only among mothers from a higher caste households. We find modest effects of mother's education on child labor outcomes, with the IV estimate indicating that a year increase in mother's education reduces a child's weekly work by approximately an hour. The IV estimates are about two-fold larger than the OLS estimates in most cases. We caution that exclusion based on social hierarchy should be considered when promoting maternal education as a medium to improve children's well-being in developing nations like Nepal.

Key words: Returns to Education, Maternal Education, Child Labor, Schooling
JEL Code: I26, J20, I30

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1 Introduction

The importance of improving women's education cannot be overstated and yet girls have difficulty accessing education in many parts of the world (Azam and Kingdon, 2013). Several factors contribute to the limited access to education among females in developing nations including the prevalence of labor market discrimination reducing incentives for schooling, gender-biased school infrastructure (such as lack of availability of female teachers), and parental preference for male education, which manifests in lower allocation of household expenditure towards female education (Azam and Kingdon, 2013). While gender parity in education is an end in itself, it is also a means to achieve other development goals. The potential role of women's education in the production of non-market goods such as child health and human capital, in addition to market goods, has been widely cited as one of the many reasons to improve female's access to education (Chou et al., 2010; Currie and Moretti, 2003; Grépin and Bharadwaj, 2015; World Bank, 1993). In situations when positive externalities associated with females' education are not fully recognized, government policy plays an important role.

Child labor is an important issue that is connected with human capital formation. Despite national laws against child labor and ratification declared in compliance with the key standards enforced by the International Conventions, child labor is prevalent in developing nations.[1] Approximately 168 million children in the world of ages 5 to 17 were involved in child labor in 2012 (Diallo et al., 2013). Asia-Pacific has the highest stock of child laborers: 77.7 million children aged 5 to 17 as compared to 59 million in Sub-Saharan Africa and 12.5 million in Latin America. Reports suggest that child labor is associated with a lower rate of schooling. The International Labour Organization (ILO) reports that a large share of children drop out of school before age 15 – approximately 24 million 7-14 year old children in India, Pakistan, and Bangladesh alone are not enrolled in school. The issue is particularly severe in Nepal; drawing on recent statistics, the International Labor Organization notes that "[in] relative terms, children in Nepal face the highest risk of being in child labour than elsewhere in South Asia, with over one-quarter (26 per cent) of all 5-17 year old engaged in child labour."[2] High prevalence of child labor not only harms the child, but it can impose long-term effects as well. Without accumulation of general human capital, the child will not be able to adjust as the labor market reorients towards the service sector, where skills have high returns. From a policy perspective, reducing child labor is closely related to sustainable economic growth.

A large literature is devoted to understand the determinants of child labor so that appropriate policies can be devised. Potential determinants include poor education, high unemployment rate, poverty, and traditional

[1] Ratification of International Conventions on child labor includes: 1) Minimum Age; 2) Worst forms of child labor; 3) Optional Protocol on Armed Conflict; 4) Optional Protocol on the sale of children, child prostitution and child pornography; and 5) Palermo protocol on trafficking in persons.

[2] http://www.ilo.org/newdelhi/areasofwork/child-labour/WCMS_300805/lang--en/index.htm

societal norms that suppress children's education, especially that of girls. It is likely that child labor is a deeply-rooted problem in developing nations brought upon by necessity due to poor socio-economic status and cannot be tackled with just labor laws. However, the exact determinants of child labor is still a subject of debate. The theoretical models consider a unitary household that decides about a child's time use based on a trade-off between child's current income and future income through additional schooling (Yang, 2008). Poor households tend to have a higher marginal utility of current income (wealth), which may influence a parent from poor socio-economic background to involve her children in labor. Indeed, poverty is considered to be the most direct cause of child labor (Basu et al., 2010). But the literature is yet to explore the role played by parental education.

In this study we explore a potential intergenerational determinant of child labor by evaluating the effects of a mother's education on a child's schooling and labor outcomes. Several studies conclude that improvements in mother's education improves overall well-being of a child, including child health-related outcomes (Breierova and Duflo, 2004; Chou et al., 2010; Currie and Moretti, 2003). Higher education increases income, creating a noticeable wealth effect, which expands a household's budget constraint.[3] Such increases in income can make substitutable activity, i.e. schooling, more affordable and subsequently reduce opportunity costs of working in a farm.[4] So, if a mother's higher years of schooling increases her income, then we might observe better child outcomes. Qian (2008) finds such an effect among farm households in China, where increases in mother's income improved schooling attainment of children. Studies have also shown that educated mothers tend to have lower fertility (Breierova and Duflo, 2004; Osili and Long, 2008). A reduction in fertility due to improvements in mother's education can indirectly affect per-capita wealth of the household. According to Becker and Lewis (1973) and De Tray (1972)'s quality-quantity trade-off, an increase in mother's education can improve quality of children through a reduction in quantity. Mother's education may also improve her bargaining position in the household, which has been associated with improvement in child's outcomes. Chen (2012) shows that girls' school enrollment increases in households where mothers have greater bargaining power. Moreover, in developing countries, education improves parents' awareness and personal preferences, which can positively affect children's schooling choices and reduce child labor. However, it is important to note that schooling might not be a perfect substitute for child labor as children can be deployed to work the same number of hours by reducing their leisure time.

Even in presence of all these channels hinting towards a reduction in child labor, it can still persist in some contexts. One theory posits that the relationship between child labor and a household's economic condition

[3](Duflo, 2001) finds an economic returns to education of 6.8 to 10.6 percent. The IV estimates suggest that the education returns are 20-40 percent higher than the OLS estimates (Card, 1999).

[4]Edmonds and Shrestha (2014) mention that substitutability between child labor and schooling depends on hazardous versus non-hazardous employment. The authors report that substitution between hazardous work and schooling may be weak, with non-hazardous work being an obvious alternative.

is non-linear. For instance, Bhalotra and Heady (2003) argue that child work increases with an increment in household's landholdings. Therefore, if improvements in mother's education increases landholdings, a major source of wealth in developing nations, it may create an opportunity or even obligations for household members, including children, to participate in agricultural activities. Additionally, due to strong societal preferences and cultural norms in developing nations attesting child labor as a state of normality, especially for girls, improvements in mother's education may not spillover to the next generation if these cultural factors exert a strong opposite effect. Hence, how mother's education affects children's schooling and labor outcomes remain an empirical question.

A primary issue in estimating the effect of mother's education on child labor is that both mother's education and child labor could be jointly determined or driven by unobserved common factors, leading to omitted variable bias. These unobserved variables can be divided into two categories, depending on whether they are determined before or after the mother's education has been determined. The former set of variables can confound the relationship between the mother's and her child's outcomes. For example, educated mothers are themselves likely born in richer households with beneficial early life circumstances.[5] As such, these mothers may possess higher ability, which they can pass on to their off-springs. In this case, unobserved ability drives both mother's schooling and her child's education and labor. On the other hand, variables that are determined after mother's education has been decided, such as the characteristics of her spouse and her fertility decisions (through assortative matching), are parts of the causal mechanism through which mother's education may affect children's well-being. However, these pathways are likely to be correlated with unobserved characteristics which initially determines mother's education (e.g. ability, household characteristics, and endowment).

We overcome these problems in this paper by using a quasi-experimental variation in mother's education created by a dramatic change in education policy in Nepal. Although formal education was introduced in 1951 following the first democracy in Nepal, access to education was discriminated by gender, wealth, and caste (Savada, 1991). The National Education System Plan (NESP) of 1971 is among the first education acts which was implemented across the nation by 1976 (United Nations Educational, Scientific and Cultural Organization, 2015). NESP nationalized education in Nepal with an aim of counter-acting the elitist bias of the inherited system of education by linking it more effectively to productive enterprises and egalitarian principles (Ministry of Education, 1971). The main thrusts of NESP were: 1) To promote equal access to quality education for all children; 2) The implementation of a standardized textbook and curriculum; and 3) Provision of trained teachers. The Education Act of 1971, a part of NESP, went into effect throughout the nation by 1976. This included a provision of free primary education (grades 1 to 5) with textbooks provided

[5]See Heckman (2011) for a brief review on importance of early child education.

by the government, which was made explicit in late king Birendra's coronation address in 1975 (Graner, 2006).

We use exposure to reform due to mother's year of birth and district of birth as arguably exogenous source of variation in mother's education. Mothers (in survey years) who were past schooling-going-age at the time of the reform (1976) will not benefit from the program. Likewise, intensity of the reform varied across districts, based on variation in enrollment rates among males in 1971 (prior to the reform). Discussed in greater detail in the Empirical Strategy section, we argue that females in districts with higher male enrollment rate should be more affected by the reform due to logistical and cultural reasons. In our identification design, we compare children of mothers who were of schooling age during the reform in high-intensity districts to those past schooling age and those in low intensity district. However, when evaluating children's outcomes, we have to deal with one additional issue – we restrict the sample of children between 10 and 15 years of age, due to which children belonging older cohort (unaffected) mothers are more likely to be of higher birth order. Existing research has shown that birth order affects human capital accumulation (Hotz and Pantano, 2015). To account for birth order difference, we use multiple years of survey as an additional source of difference and implement a Difference-in-Difference-in-Differences (DDD) specification.

We use data from the Nepal Housing and Population Census 2001 and 2011 (hereafter census 2001 and 2011, respectively) to study the impact of the reform on mothers' and children's educational outcomes and Nepal Living Standard Survey (NLSS) 1996 and 2004 to study labor outcomes. The need for using different surveys arise because the census does not contain a comprehensive data on child labor. We briefly explain our empirical strategy here using example of census data; our strategy with NLSS is similar with appropriate adjustment for mother's age. In census 2011, mothers aged 35-44 years (0-9 years old in 1976) are exposed to the reform where as those 45-54 year olds (10-19 years old in 1976) were too old to benefit from the reform. The issue is that children of mothers in the older cohort will have older siblings. This sibling effect may be district-specific or correlated with reform intensity, thus violating the difference-in-differences assumption. To account for this, we take an additional difference between children of younger and older mothers in 2001. Note that in 2001, both groups were too old (10-19 and 20-29 in 1976, respectively) to benefit much from the program and therefore any difference in the outcome must be due to district-specific birth order effect. The assumption behind the triple difference strategy is that any unobserved district-specific time varying factors between two survey years should not have differential effects across younger (35 to 44 year olds) and older cohorts (45-54 year olds).

We conduct three primary analyses. As a first stage, we evaluate the effect of the reform on educational outcomes among mothers who were of school going age in 1976. Second, we estimate a reduced form model to evaluate the effect of the reform on children's educational and labor outcomes. Third, we use the reform

as an instrument to evaluate the causal effect of mother's education on child labor participation.

We find that one standard deviation increase in intensity of the reform increases mother's years of schooling by 0.4 years. The effects are more pronounced among mothers from higher castes (Brahmin and Chhetri). The reform increased the likelihood of children completing their fifth grade only among children from higher caste mothers, but we fail to reject the null hypothesis of no effect of the reform on child labor participation. Using the reform as an instrument, we find that on average one more year of a mother's schooling increases a child's likelihood of finishing 5^{th} grade by 0.3 percentage points. We find modest effects of maternal education on child labor. The findings suggest that on average one more year of mother's schooling leads to a reduction in the total number of hours worked by a child in the past week by 0.62 units.

2 Literature Review

A large body of literature is concerned with better understanding the determinants of child labor in developing countries. While the role of household poverty is taken as axiomatic (Basu et al., 2010), other studies have found counter-intuitive results. For instance, Bhalotra and Heady (2003) found that child work increases with household's landholdings. Basu et al. (2010) provide a counter-argument that children in the poorest households are unable to engage in employment activities due to lack of employment opportunities close by and not due to lack of incentives. The implication is that local labor markets mediate the relationship between household poverty and child labor. Basu et al. (2010) further posit a U-shaped relationship between landownership and child labor: as land holding grows, child labor increases first and then begins to decline.

A body of empirical literature estimates the impact of income shocks on child labor (Kruger, 2007; Yang, 2008). These studies focus on changes in income due to price shocks or exchange rate shocks. For example, Yang (2008) finds that a favorable exchange rate shock that increased the value of remittances in migrant's household increases the likelihood of schooling and lowers the number of hours worked. Edmonds and Schady (2012) study the relationship between family economic status and child labor using experimental data from Ecuador. The authors evaluate the outcome of an unconditional cash transfer that was randomly allocated to a fraction of the poor households and given to the mother. Despite lack of conditionality, the authors find a large reduction in child labor among recipients of the cash transfer, indicating that household income is an important determinant of child labor. However, Edmonds and Shrestha (2014) find that stopping such transfers also led to resumption of child labor in Nepal. Therefore, reducing child labor would require a permanent increase in household income, which mother's higher education is maybe able to provide. This raises an important question about the relationship between mothers education and child labor. How

large are the effects of mothers education by itself? Or does female education have to be accompanied to employment opportunities that raise household income?

Children's schooling and labor supply are affected by family background. Black and Devereux (2011) summarize the literature on intergenerational correlation on socio-economic status. In discussing the methodological difficulty in establishing causal links, they note that "as it is often the case that any particular parental attribute is correlated with a variety of parental characteristics, many of which cannot be observed in the data." Besides studies that show strong correlations between outcomes of parents and children, papers attempt to arrive at causal estimates using three distinct methodologies. One set of papers study outcomes of children of twin mothers who share genetic and environmental conditions but attain different levels of education (Behrman and Rosenzweig, 2002). Another approach in the literature has been to treat adoption as randomly assigning children to parents and studying the correlation between the parents and adopted children. These studies have exclusively been carried out in developed countries with rich administrative datasets and therefore cannot inform policy in developing countries. The third approach uses random variation in parental income and education that are uncorrelated with their characteristics to study impacts on children. The papers that are particularly relevant to this study use policy-induced changes affecting educational attainment of parents.

Black et al. (2005) posit two possible explanations for correlation between parental education and their children's outcomes. One is selection - high ability parents have high ability children. Another is causality - parents who get more education cause their children to receive more education. Improvements in education of one generation having spillover effects on the next generation creates a scenario of postive externality, which calls for greater investment in public education. To establish a causal link, Black et al. (2005) use changes in compulsory education laws in Norway in 1960s, which was implemented across municipalities at different times. The paper uses exposure to the reform as an instrument for parental education. The study finds little evidence of a causal relationship between parental education and children's education outcomes. Similar strategy has also been used by Oreopoulos et al. (2006) in the case of U.S. to find that a one year increase in parental education reduces the probability of a child repeating a grade by between 2 and 7 percentage points. Likewise, Chevalier (2004) uses a change in the compulsory schooling laws in Britain that occurred in 1972. The author also finds a large positive effect on continued attendance beyond compulsory education.

Exploiting an alternative source of variation in maternal education, Carneiro et al. (2013) use differences in direct and opportunity cost of college at the time the mother was growing up to identify the effect of maternal education on a variety of childrens outcomes, including behavioral problems, achievement, grade repetition, and obesity. The authors argue that one causal link between maternal education and children outcome is home environment. The authors highlight the dual role of maternal education it improves the

mothers capacity to take care of their children. But in the context of developed countries like the U.S. where this study is based, it also means increased maternal employment, which could leave less time for child rearing. Increased maternal education may also lead to better home environment through assortative matching. The empirical strategy in this paper looks at variation in direct and opportunity cost of schooling across countries of mothers birth and cohorts, and uses a limited information maximum likelihood (LIML) method to account for weak instrument. The authors also point out that papers using changes to compulsory education laws estimate the impact among parents who are at the lower end of the education distribution. Furthermore, these studies focus on developed countries, where compulsory education laws preclude any possibility of child labor.

In a developing country setting, Fafchamps and Wahba (2006) find that proximity to urban areas plays an important role in encouraging child labor. The authors make a distinction between wage work in the market and labor supply in the farm to find that proximity to urban market increase the likelihood of wage work by children, but this takes up less time than working in farms. Furthermore, schooling also increases with urban proximity. Another channel through which mothers education may impact child labor is by improving her bargaining position. In general, a greater bargaining position for women has been shown to improve child outcomes. Basu and Ray (2002) use relative education of women and their contribution to total household income as measures of women's bargaining position.

Our study fills several gaps in the current literature. Intergenerational determinant of child labor has been rarely studied in the literature. A limitation of studies focused in developed countries in this regard is that they do not consider child labor as a possible outcome variable. Compulsory education laws and strict enforcement of laws means that child labor is rarely an issue in developed countries. However, given its importance in developing countries, the spillover effect of mothers education on child labor warrants investigation.

The intergenerational impact in the context of our study may be different than those found in developed countries. On the one hand, we are investigating the impact of inducing improvements at the lower end of the education spectrum (primary years of schooling). This may not have contributed to the overall human capital development of mothers. In contrast, the majority of current intergenerational spillover literature use changes to compulsory education laws in developed countries where mothers already obtained a lot more years of schooling than developing countries. Furthermore, the labor market for females are also not well-developed in our context.[6] On the other hand, at low-levels of education, the marginal benefits of education

[6]Due to societal norms that depict women as care-takers, women in Nepal are much less likely to work outside of the household. Although 40 percent of women are economically active, the majority of them work in agricultural sector and are used as unpaid family workers (Women in Nepal, ADB 1999). According to the Census Bureau of Statistics (CBS, 2002), 18.9 percent of literate women are engaged in paid work, compared to 30.9 percent of males (Bhadra and Shah, 2007). Due to such reasons, monetary returns to education among women may be very low.

may be quite large. Another contribution is that sibling effect is usually ignored in the literature. The existing studies mostly compare older mothers to younger mothers but having children of different ages. Even if we control for birth order in this situation, differences in marriage and childbearing decisions of mothers belonging to different cohorts may be correlated with determinants of children's human capital.

3 Background: Child Labor and Education Reform in Nepal

3.1 Child Labor in Nepal

In our study context, child labor is an important issue. In the mid-1990s, child labor in the export-oriented carpet weaving sector came to the forefront of public attention, leading to policy changes. Nepal enacted a law prohibiting child labor in risky business, including carpet weaving, tourism, and public transport, which was termed as the Child Labour Act, 2000. However, the act does not seem to have been effective in reducing the intensity of child work. A report by the Central Bureau of Statistics (CBS) based on a household survey found that labor force participation rate was 47% among children aged 10-14 in 2014 (Central Bureau of Statistics, 2015). The rate of child labor is higher in rural areas, for females, and in the lowest income quintile.

There has been a growing interest in understanding the effects of social protection on child labor (Dammert et al., 2017). A survey of the literature by Dammert et al. (2017) found that programs that reduce vulnerability of households work well, but the effect are heterogeneous. Furthermore, little is known about the persistent impact of these programs. In our study context, Edmonds and Shrestha (2014) found that providing scholarship and support for school-related expenses reduced involvement in hazardous forms of child labor, but the impact lasted only during the year support was provided. The lack of sustained impact suggests that there might be other more important factors driving child labor.

3.2 The National Education System Plan (NESP 1971) and Educational Policies

Although formal education in Nepal was introduced in 1951 after dethroning of Rana rulers and establishment of the first democracy, education was highly monopolized based on gender, wealth, and ethnicity (Savada, 1991). Even decades after the establishment of formal education, society still viewed educating females and people from marginalized class as unnecessary. In the context of Nepal, poverty is a major factor that impedes promotion of education (United Nations Educational, Scientific and Cultural Organization, 1987). Furthermore, parents generally prefer educating sons over daughters due to a societal norm that depicts girls

as future housewives and mothers. The literacy rate in 1961 was 16.3 percent for males and 1.8 percent for females. Moreover, the literacy rate for males increased to 34 and 54.5 percent in 1981 and 1991 compared to 12 percent and 25 percent among females in these respective years (Balatchandirane, 2003).

With aims of creating an exploitation-free education system based on egalitarian principles, which could be achieved by "counter-acting the elitist bias" of the concurrent education system, NESP nationalized education in 1971 and was implemented across the country by 1976 (Ministry of Education, 1971). NESP established a framework for universal education and is regarded as the pioneer of education reforms in the country. The reform created an organized structure of education, with primary school extending from grade 1 to 3, lower secondary from class 4 to 7, and secondary from class 8 to 10. The main thrusts of the reform were: 1) Promote equal access to quality education among all children regardless of gender, wealth, and caste; 2) Improve quality of education through provision of trained teachers; 3) Implementation of standardized curriculum across the nation; 4) Provision of standard textbooks free of cost to children attending primary schools in remote areas; and 5) Emphasize vocational education. Although NESP was focused to increase enrollment at all levels, the main focus of the reform was at the primary school level. From 32 percent enrollment rate in 1970, NESP set a target of 64 percent enrollment in three-year primary education by 1976 (United Nations Educational, Scientific and Cultural Organization, 1984).

Public expenditure in education sector increased significantly after the implementation of NESP. The proportion of education development funds reached 12.4 percent of the total budget in 1976 and remained around 9 percent in 1980s (United Nations Educational, Scientific and Cultural Organization, 1984). The total government expenditure on education was R.S. 6.3 crores ($ 611,650 in today's exchange rate) in fiscal year 1971/1972 and increased to R.S. 24.4 crores ($2,368,932 in today's exchange rate) in 1975/1976, with implementation of NESP amounting to R.S. 66 crores ($ 97,087 in today's exchange rate) between 1971 to 1976 (Ministry of Education, 1977). Provision of free primary textbooks and teachers' salaries comprised a significant portion of expenses allocated for education purposes. Between 1970 and 1980, the total number of schools increased by 30.3 percent (7,275 primary schools in 1970 and 10,130 schools in 1980), and the number of teachers increased by 48.9 percent (United Nations Educational, Scientific and Cultural Organization, 1987).The minimum qualification required for a primary school teacher was the School Leaving Certificate (SLC), whereas in 1950s a typical primary school teacher had only four to six years of schooling (Wood, 1959).[7] To meet the shortage of teachers in rural areas, local teachers were trained and provided allowance that amounted to 110 percent of their salaries.

As a part of NESP, the Institute of Education provided one to two years of training courses, after which individuals completing one-year course was classified as primary school teachers and those finishing two-year

[7]However, it is reported that about one third of teachers did not have the required qualification.

course became lower secondary school teachers. The institute also conducted a distance learning program and radio teacher training program, which made it possible for teachers in local communities to remain in their households and continue teaching. The distant learning programs facilitated provision of self-learning materials and followed up with contact sessions for final examinations.

The implementation period of NESP coincided with the coronation ceremony of late King Birendra Bir Bikram Shah Dev in 1975 – the year that marks the declaration of free primary education in the nation (United Nations Educational, Scientific and Cultural Organization, 1987). This provided a significant step to facilitate education among common people. A report published by the Ministry of Education (1977) mentions that primary enrollment rate had reached 59 percent in 1976 and such an increase had been "facilitated to a considerable extent by the implementation of free primary education since 1975." To reduce the wide gap in enrollment between girls and boys, enforcement measures such as distribution of free textbooks to girls and free schooling provision at the secondary level were established. The fifth development plan policies (1975 to 1980) immediately followed the implementation period of NESP. The educational policies indicated in this plan were the same as policies specified in NESP, and the development plan further emphasized the extension of free primary education with an importance given to increase access to education among females. In the final year of the plan, the government increased duration of primary education until the fifth grade (grade 1 to 5) with an objective of providing opportunity to receive higher levels of education, as children could now complete fifth grade free of cost.

There exists a substantial across-district variation in enrollment rate in both pre and post NESP periods. Enrollment was highest in the Central Development region and lowest in Mid-western region. The total primary school enrollment increased by 161.4 percent between 1970 and 1980 and female enrollment increased from 15.8 percent in 1970 to 28 percent in 1980 (as a portion of total enrollment) (United Nations Educational, Scientific and Cultural Organization, 1984).

4 Data

The data for this study is sourced from the fifteen percent sample of Nepal National Population Census (census 2001, 2011) and Nepal Living Standards Survey (NLSS 1996, 2004). The need for using different datasets arise from advantages and disadvantages of each source. The census data is larger and covers all locations in Nepal, which allows exploring the effects by castes, but contains sparse information on labor market participation.[8] On the other hand, NLSS, modelled after the World Bank's Living Standards Measurement Survey, collected detailed data on labor market activities. However, the data was collected

[8]Caste is a dimension along which severe discrimination persists in Nepal in several grounds, including access to health and education (Gurung, 2005). Therefore, we also present results that investigate caste heterogeneities.

by a stratified random sample methodology and includes approximately 4000 households in each survey. Analysis from both datasets provides a fuller understanding of the relationship between education reform, female education, and children's outcomes.

We focus on children aged between 10 and 15 at the time of the survey. The constraints to education start to bind during this age group, as opportunity cost of education increases. For instance, Azam and Kingdon (2013) find that parents start discriminating between boys' and girls' education during this age group.

4.1 Nepal-National Population Census (2001 and 2011)

The dataset includes information on age, gender, education, and other personal characteristics (caste, religion), and economic activity. Questions related to education ask whether an individual ever went to school, current school attendance status, and levels completed. In Nepal, students begin schooling at age 6 and attend 5 years of primary school (level 1 to 5), 3 years of lower secondary school (6 to 8), and 2 years of upper secondary school (level 9 and 10). After the 10^{th} grade, each student takes the School Level Certificate (SLC) exam, a national exam that determines eligibility to continue post-secondary schooling. Individuals between 10-15 year olds are usually between levels 5 and 10, although it is not uncommon for students to begin schooling later and/or repeat some levels. When evaluating the effect of NESP on mother's education, we use completion of fifth grade and years of schooling as the main variables of interest. Since the goal of NESP was to improve primary education, we expect the greatest impact to be at the lower end of education distribution. When conducting analyses pertaining to children's educational outcomes, we focus on current attendance and the completion of fifth grade. We drop mothers with missing value for years of schooling (0.4 percent of the sample).

Our analysis from the Census data focuses among children of household heads as we can assign information about mothers by using the relationship codes. We have information on the mother's district of birth and age, which allows us to assess her exposure to NESP. To assist our identification strategy, we restrict the mother's age to be between 35 and 54 at the time of the survey.

The census collected information on the number of months an individual engaged in various activities over the last twelve months for individuals over 10 years of age. However, we are unable to construct a measure of child labor that is generally used in the literature. In the best case, measure of child labor from the census data only focuses on the extensive margin (participation). International Labor Organization and Central Bureau of Statistics (2011) has technical statistical definition of child labor (pg. 30). It defines child labor as employment that lasts greater than 15 hours in a reference week. "Work" has been broadly defined

to not only include employment in farm or household business, but also household chores such as making mats and fetching water. Unfortunately, due to the lack of detailed data on work-related activities in the previous week, we are bound to use monthly information to construct measures of child labor. To overcome such drawbacks imposed by lack of data availability regarding detailed measures of work-related activities in the Census (e.g, days worked per week, hours worked), we turn to Nepal Living Standard Survey.

4.2 Nepal Living Standard Survey (1995 and 2004)

We use two waves of NLSS data of survey years 1995 and 2004. The educational variables used pertaining to a mother's educational outcomes are similar to that of the Census. Although NLSS includes smaller sample compared to the census, a notable advantage of using NLSS data is that the surveys contain detailed information regarding employment and economic activity of an individual over 10 years of age. This allows us to construct measures of child labor that defines the intensity of labor. The NLSS presents both monthly and weekly measures of work-related activities. To reduce recall biases, we focus on activities performed in the past week. We focus on three main measures that describes child work: 1) Days worked per week; 2) Hours worked per day in a week; and 3) The total number of hours worked in a week.

Another benefit of using NLSS data is that it includes detailed information regarding infrastructure composition (e.g., schools, health clinics) in area of residence during the survey year. Nepal underwent the second political revolution in 1991, which led to the establishment of democracy in lieu of the Panchayat regime. Such a political change created a rapid expansion of the number of schools established across the nation. A part of NLSS data collects detailed information regarding schools in the primary sampling area (ward), which includes information on the year of establishment of schools. NLSS also includes information regarding the distance from the household to the nearest school. These variables allow us to properly control for the district-specific effect of the political change on education-related outcomes, while analyzing children's educational and work-related outcomes. Note that these changes are irrelevant while evaluating the effect of NESP on mother's education, as mothers affected by NESP will have completely surpassed the school-going-age in 1991. While conducting analyses pertaining to child outcomes, birth order is of a specific relevance. Edmonds (2006) finds that siblings differ in comparative advantage in household production. NESP survey records maternity history of all married women who have given birth. Usi'ng this we control for birth order effects when analyzing child outcomes.

5 Empirical Strategy

Let y_{ihd} be the outcome (education or labor) of a child i in household h belonging to a mother born in district d and m_{ihd} be mother's education for a respective mother born in district d. The relationship between a child's outcome and mother's education is given by:

$$y_{ihd} = \alpha + \beta m_{ihd} + \kappa X_{ihd} + \gamma_h^y + \delta_d + \epsilon_{ihd}^y. \tag{1}$$

Here, X_{ihd} represents observed household specific characteristics, γ_h^y represents a vector of unobserved household characteristics that are correlated with mother's education and child outcomes, δ_d is location specific effect, and ϵ_{ihd}^y is the error term. Parameter β represents the causal impact of mother's education on a child's outcome given that unobserved factors are controlled for. Since we cannot feasibly control for all relevant household characteristics that may both affect mother's education and child's outcome, we identify β by using variation in m_{ihd} created by arguably exogenous factors determining exposure to the reform (e.g., birth year and district of birth).

In our study context, geographic variation in male enrollment rate in 1971, a proxy for variation in intensity of the education reform, is the first source of variation. Likewise, mother's age at the time of the survey is the second variation. Finally, appending samples from the 2001 and 2011 census data (1995 and 2004 for NLSS data) provides the third source of variation.

5.1 Using Geographic Variation in Male Enrollment Rate to Identify the Intensity of NESP

The first source of variation is to compare changes in outcomes between districts that were highly affected by the National Education System Plan (NESP) with districts which were less affected by NESP. The reform was drafted in 1971 and was implemented throughout the country by 1976. Prior to the implementation of NESP, substantial geographic variation in enrollment rate existed. Broadly speaking, enrollment rate was higher for districts located in the Eastern and Central region, and lower in the Far Western region (according to the 1971 Census, 93 and 91 percent of 6 to 14 year olds in Karnali and Seti zones were not enrolled in school, respectively).[9]

We use geographic variation in enrollment rate among 6 to 14 year old males pertaining to primary (1-5th grade) and middle school (6-8th grade) as an intensity measure. This is because the effect of the reform would be higher for females in districts with higher pre-reform enrollment rate for males. Even after the

[9] http://cbs.gov.np/nada/index.php/catalog/24

introduction of formal education in Nepal following the first democracy of 1951, the societal norms viewed educating females as unnecessary (Shrestha et al., 1986; Stash and Hannum, 2001). The literacy rate in 1961 was 1.8 percent for females compared to 16.3 percent for males.[10]

In 1971, the difference between male and female enrollment rate was substantial. On average, male enrollment rate was 16 percentage points higher than female enrollment. Many districts had relatively high male enrollment rates, but relatively low female enrollment rates, and the relationship between the two was not linear (see Figure 1). Among districts with close to 30% male enrollment, none had female enrollment over 14%. Only a handful of districts had female enrollment over 10%, highlighting the stark gender difference in attitude towards education.

A severe discrimination between sons and daughters explains a historical fact that improvements in female educational outcomes have been trailing males' educational outcomes. Districts with low enrollment rate among males would mean lack of adequate resources necessary to fully absorb the effect of the education reform. Whereas, districts with relatively higher enrollment rate among males would signal adequate resources and infrastructure to better absorb the effect of the reform. In other words, given the severe magnitude of gender discrimination, it is unlikely that educational outcomes among females will improve substantially before improvements in male educational outcomes (particularly in districts with low enrollment rate among sons). Variation in male enrollment rate will proxy for the number of schools in the district and societal perception regarding education, which would mediate the effect of NESP. Hence, the effect of the reform should be concentrated in districts with higher pre-reform enrollment rate among males.

By studying the change in literacy rate between 1971 and 1981, we can show that male education was a necessary condition for improvement in female education. Figure 2 plots increase in literacy rate among 10 to 14-year-old males and females against male literacy rate in 1971. It is clear from the figure that in districts with lower male literacy, the gain in female literacy after the program was lower. In contrast, the gains in male literacy is much higher in districts with low 1971 male literacy rate. Therefore, a high pre-existing male education was a pre-requisite for females to benefit from the program.

However, using geographic variation alone to identify the effect of the reform is problematic as pre-reform enrollment rate among males is not randomly assigned. Therefore, females in high male enrollment districts are also more likely to have higher education due to other unobserved differences across district that might later influence our outcome of interest. Figure 1 shows that the correlation between male and

[10]Such a huge gender disparity in educational outcomes can be explained by both demand and supply factors. A 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.

female enrollment prior to the program is weak but still positive. Using the 1971 Census data, we find that literacy rate is higher in the Central and Eastern regions of Nepal. Districts that differ in socio-economic status may have different labor market conditions and perception regarding child labor, which can affect the magnitude of children's labor and schooling outcomes. To isolate the effect of such unobserved factors which are correlated with district enrollment rate prior to the full implementation of the reform and outcome variables, we make use of within cohort differences.

5.2 Variation Within Cohorts

The exposure to the reform depends on one's year of birth. Precisely, girls who were past normal school-starting-age would not gain any benefit from the program. Although the program aimed to improve access to education at all levels, as previously mentioned, the main focus of the program was to expand primary education. Older females past certain age will not only be less likely to attend primary school but additional responsibilities of carrying out household chores will prevent them from enrolling at older age. As such, age in 1975 is another dimension through which the reform will have differential effect. Children in Nepal typically start schooling between 6 and 9 years (Savada, 1991). As our second source of variation we compare cohorts of women who were affected by the reform to those who were too old to benefit. The former group includes women who are born between 1967 and 1976 (aged 0-9 in 1976), and the latter includes women who are born between 1957 and 1966 (aged 10-19 in 1976). These women are aged 35-54 in 2011, out of which the cohort of 35-44 year olds are more likely to benefit from the program. We restrict our sample to mother's who are in this age group.

Until now we have two sources of variations including across district variation of reform intensity and within cohort variation. Figure 3 provides a descriptive evidence regarding our decision to make use of these two identification variations. The district-specific difference in female primary school completion rate in 1981 between 6-14 year olds (exposed to the reform) and 20-29 year olds (unexposed) is positively and strongly associated with male enrollment rate of 1971. Although Figure 1 shows a week correlation between male and female enrollment in 1971, females exposed to the reform and living in high male enrollment districts in 1976 faced much larger increases in primary enrollment rate when measured in 1981 (See Figure 3). In contrast, there is no such pattern when focusing among males (Figure 4).

5.3 Using Within Cohort Variation Across Survey Years

In situations when the variable of interest is not systematically different between exposed and unexposed mothers, estimation could be carried out using a single cross-sectional data. However, comparing children's

outcome such as child labor in any given survey year between mothers aged 0-9 and 10-19 in 1976 is problematic as the older cohort will have older children during the survey years. For any given survey year, only 26% of the sample are children of mothers from the older cohort (unexposed). This could lead to endogeneity concerns due to birth order effects and sample selection. If the analysis is restricted to a single survey, the children of older cohort that are in our sample are likely to have higher birth order (i.e., they already have older siblings and they are 2^{nd}, 3^{rd}, 4^{th}... child). Having an older sibling engaged in labor market may reduce the likelihood of child labor. On the other hand, younger or exposed mothers are more likely to have children engaged in child labor due to birth order effects. This means that our sample includes a mix of children with various birth order which is systematically related to exposed and unexposed cohort. It is particularly problematic if such systematic differences between younger and older cohorts differ across high and low intensity districts.

To account for such systematic differences in birth order among children from mothers in older and younger cohorts, we use multiple cross-sectional surveys and employ a triple-difference strategy. We describe the intuition behind our identification strategy in Table 1 with a simple 2x2x2 framework.[11] If we use a single survey, then we could compare children of Cohort A and Cohort C, or Cohort B and Cohort D. We can estimate a difference-in-differences (DD) model by comparing Cohort B and Cohort D, but mothers in Cohort B will have much younger children with lower birth order than Cohort D. This makes comparing child labor and schooling outcomes of children belonging to mothers of these two cohorts problematic.

Now, consider another sample where none of the mothers in relevant age groups are exposed to the program (Cohorts C and E in survey year 2001) but are of respective age groups as of Cohorts B and D. Since neither of the cohorts C and E were exposed to the program (as individuals in these groups had surpassed school-going-age during the time of the reform), we should not find any direct effect of NESP. However, we can use Cohorts C and E to difference out systematic differences between older and younger cohort due to birth order effects resulting from using a single cross-sectional survey of 2011. This leads to a difference-in-difference-in-differences specification. Thus, the difference within mother's cohorts, program intensity in mother's district of birth across districts, and survey years will help isolate the direct impact of mother's education on child outcomes.

5.4 Difference-in-Differences

We begin with a difference-in-differences (DD) specification, in which we compare Cohort B to Cohort D. Assuming linearity, mother's education can be written as a function of intensity of the reform and her

[11] Note that this pertains to census data but the strategy is similar when using NLSS data, except age adjustments to mothers from respective survey years (1995 and 2004).

exposure to the reform, which depends on her age during the time of reform. We estimate the following specification.

$$E_{icd} = \alpha + \beta Exposed_c * Intensity_d + \gamma birthyear_i + \lambda district_d + \eta X_{icd} + \epsilon_{icd}, \tag{2}$$

where E_{icd} denotes educational outcomes of mother i (completion of 5^{th} grade and the highest level of schooling) of cohort c, born in district d. $Exposed_c$ is an indicator variable taking a value 1 if an individual i belongs to younger cohort in 2011 survey (Cohort B), which is interacted with geographic differences in intensity of the reform, denoted by $Intensity$ and measured by enrollment rate at the primary (1-5^{th} grade) and middle school (6-8^{th} grade) levels of 6 to 14 year old males in 1971. Variables $birthyear_i$ and $district_d$ represents year of birth and district of birth fixed effects. X is a vector of other exogenous factors that influences educational outcomes (caste and religion). Both of these variables are strong predictors of education. Due to caste-based discrimination, certain caste members enjoy greater privileges including access to education. Note that $Exposed_c$ is collinear with birthyear dummies and is not included by itself. The above equation is estimated by using the OLS and standard errors are clustered at the district level. The coefficient of interest is β, which presents the difference-in-differences estimate.

Parameter β represents the causal estimate of the reform under an assumption that educational outcomes among younger and older mothers across districts that were highly affected by the reform and districts less affected by the reform would follow a similar trend in absence of the reform. One potential concern is that districts that were highly affected by the reform may have experienced improvements in female educational outcomes prior to the reform and educational outcomes may already have been upward trending. To provide a suggestive empirical test regarding this assumption, in spirit of Duflo (2001), we expand equation 2 to form a case study analysis as given below.

$$E_{ijd} = \alpha + \beta_j \sum_{j=0}^{17} [Age\_1976_{ij} * Intensity_{id}] + \gamma birthyear_i + \lambda district_{id} + \eta X_{icd} + \epsilon_{icd}, \tag{3}$$

where the analysis is restricted to individuals of ages 35 to 54 year olds in 2011 survey year, giving us individuals of ages 0 to 19 in 1976. Age indicator in 1976 (Age_1976_{ij}) is interacted with the intensity measure. The comparison group is 18 and 19 year olds in 1976. If the identification strategy outlined above is valid, the magnitude of β_j should be a decreasing function of age, decline sharply for $j = 10$, and remain close to 0 for $j > 10$.

In the DD specification, exposed and unexposed groups belong to Cohorts B and D, respectively, when using 2011 survey year. As previously mentioned, it is problematic if systematic differences in children of

mothers from younger and older cohorts are correlated to child outcomes and reform intensity. To alleviate such concerns, we use the difference-in-difference-in-differences (DDD) as our preferred specification.

5.5 Difference-in-Difference-in-Differences (DDD)

By using Cohorts C and E as shown in Table 1, we incorporate the third level of variation created by cohorts unaffected by the reform in 2001 survey year. Both these groups were too old to benefit from the policy, and therefore, their education level should not be systematically different across low and high intensity districts between the two survey years.[12] This allows us to estimate a difference-in-difference-in-differences (DDD) specification, which is more robust than the DD specification as it captures systematic differences, which originates in the DD specification given by equation 2 when comparing child outcomes from mothers of younger and older cohorts in survey year 2011. To operationalize the identification strategy, we first estimate the following DDD model, where the dependent variable used is mother's educational outcomes similar to equation 2.

$$E_{idcs} = \beta Exposed_c * Intensity_d * Survey\_2011 + \delta_{cd} + \tau_{cs} + \phi_{sd} + e_{idcs}. \tag{4}$$

Subsequently, we estimate a reduced form to evaluate the effect of the reform on children's education and labor outcomes, which is given as follows:

$$y_{idcs} = \beta Exposed_c * Intensity_d * Survey\_2011 + \delta_{cd} + \tau_{cs} + \phi_{sd} + e_{idcs}. \tag{5}$$

where, y_{idcs} is child outcome (education or child labor) of a child i who belongs to a mother born in district d, of cohort c, and in survey year s. $Exposed_c$ takes a value 1 if an individual is 35-44 year olds (Cohorts B and C as shown in Table 1) and 0 if 45-54 year olds (Cohorts D and E), and is interacted with reform intensity of mother's district of birth (d) and survey year (2011). The equation includes a full set of double interaction between cohort and mother's district of birth denoted by δ_{cd} which captures cohort specific effect within a district; τ_{cs}, interaction between cohort and survey year; and ϕ_{sd} represents interaction between survey year and mother's district of birth. It is not necessary to include mother's district of birth fixed effects by itself once δ_{cd} is included. Also, it is unnecessary to include survey year and cohort fixed effects individually. β is the coefficient of interest, which measures the effect of reform on outcome variables for 35-44 year olds in 2011 survey year relative to 2001. We consider the above specification (DDD) as our preferred model as it differences out systematic differences within mothers from older and younger cohorts in 2011 survey, which may be correlated with reform intensity and child outcomes by using unaffected mothers from 2001 survey

[12] We show that such is the case in an auxiliary specification. Results are not shown but are available upon request.

year. Standard errors obtained from two-way clustering at the district and cohort level are presented.

The identifying assumption for the DDD specification is that there are no district-cohort specific changes between 2001 and 2011 survey years that are correlated with the reform and district-cohort specific child outcomes, conditional on the covariates we include in the model. In other words, any unobserved time varying district specific changes between two survey years affecting higher and lower intensity districts differently should not systematically affect child outcomes of 35-44 year old and 45-54 year old mothers.

5.6 Potential threats to identification by DDD

Any changes between 2001 and 2011 that will disproportionately impact children of younger mothers in high intensity districts in 2011 will lead to possible bias in the result. There were some important economic and political changes that took place in Nepal in the first decade of 2000. The most important of these is perhaps the end of a decade long civil war and restoration of democracy in 2006. While the 1990s was the period of economic decline in manufacturing sector, the 2000s brought in a wave of low-skilled temporary labor migration to the Middle East and Malaysia. If these changes had differential effects on children belonging to younger cohort mothers in districts with high program intensity, then our estimate will pick up these effects. These concerns are discussed in more detail below.

5.6.1 Boom in labor migration

Were there changes in economic conditions in Nepal between 2001 and 2011 that may be correlated with 1971 male enrollment rates and affected children of younger mothers differently? Labor market conditions were vastly different between the two surveys. In the context of Nepal, labor migration to the Middle East and Malaysia for low-skilled jobs expanded rapidly in the first decade of 2000s. Therefore, children in 2011 were facing different family circumstances and labor markets than children in 2001. Remittances from foreign migration may have reduced the need for child labor at home. Furthermore, households maybe were able to afford the direct and opportunity cost of schooling. On the other hand, absence of household member (most of the migrants are male) may have led to labor shortages, requiring greater use of child labor in family farms. Figure A1a in the appendix shows the correlation between fraction of district population absent in 2011 and 1971 male enrollment rate. We see a positive relationship, indicating that any effects of the boom in labor migration may be picked up by our measure of reform intensity. To account for boom in labor migration, we control for the triple interaction of district-specific total count of foreign migrants in 2011 interacted with exposed cohort and the survey year 2011.

5.6.2 Civil War (1996-2006)

A decade long civil war between the Communist Party of Nepal (Maoist) and the government of Nepal led to a massive destruction of property and human lives. It is estimated that more than 17,000 people (including both civilians and armed forces) were killed during the conflict and more than 100,000 individuals were displaced.[13] There is evidence that the conflict had positive impact on female education (Valente, 2013). In Appendix Figure A1b, we see that conflict was less intense in areas with high enrollment rate. So, the positive effect of low conflict may confound the impact of higher reform. To account for the effect of the war, we control for the triple interaction term between district-specific count of displaced individuals (per10,000), exposed cohort, and the survey year 2011.

5.7 Summary statistics

Tables 2 shows the summary statistics divided by samples in two survey years using the census and NLSS data, respectively. As expected mothers have higher educational outcomes in 2011 compared to individuals in 2001 survey year. Mostly importantly, it is noticed that other characteristics such as ethnicity, religion, child's age, and gender are virtually similar across two waves, suggesting that sample composition across waves are similar (on observed characteristics).

6 The Effect of NESP on Mother's Education

Our first set of results show that NESP had a strong impact on mother's education. We estimate the impact of NESP in three ways. First, following the traditional DD method, we use a single cross section and exploit variation across age (cohort) and reform intensity. In 2011 (2001), the exposed cohort include mothers aged 35-44 (25-34) and the unexposed cohort include mothers aged 45-54 (35-44). These results are reported in Table 3. In the second DD estimate, we define exposed and unexposed cohort differently. In this case, the exposed cohort include women aged 35-44 in 2011, whereas the unexposed cohort include women aged 35-44 in 2001. This classification allows us to compare women of the same age group (and hence with same family structure). In the third approach, we combine multiple survey years and within cohort variation obtained from each survey years along with variation in intensity of the reform across districts and estimate a DDD specification, which is our preferred specification and is used as a first-stage estimation of instrumental variable technique.

[13] https://www.insightonconflict.org/conflicts/nepal/conflict-profile/

6.1 Results from Single-Survey-Year Difference-in-Differences Estimation

The estimates from using a single cross-sectional data and using variation within age cohorts and reform intensity to identify the effect of NESP for a full sample are shown in Table 3. The first four columns (Columns 1 to 4) uses the survey year 2001 and Columns 5 to 8 uses the survey year 2011. The odd columns present results without additional district-level control variables, whereas the even columns include district-level control variables. For the full sample, the results show that a percentage point increase in male school enrollment rate in 1971 increased the likelihood of 5^{th} grade completion and the highest level of education of younger cohort by 0.41 percentage points and 0.038 units (Columns 2 and 4), respectively, after controlling for personal and district characteristics. The results are comparable across both 2001 and 2011 survey years.

By using a case study method as shown in equation 3 but after pooling both survey years together, we estimate the effect of the reform for individuals of each age in 1976 from 0 to 17 year olds (comparison group is 18 and 19 year olds). The age-specific estimates of the effect of the reform are plotted in Figure 5. The coefficients show a decreasing trend over age, falls sharply for 10 year olds in 1976, after which they remain close to zero. We also conduct case studies by using each survey year individually. The coefficients on the interaction terms are plotted in Appendix section, Figure A3, for survey year 2011. The results remain similar.

6.2 Results from DD Estimation Combining Multiple Surveys

As previously discussed, relying upon a single cross-sectional survey is problematic when evaluating child outcomes due to systematic differences in family structure between the exposed and unexposed mother's households. Table 4 shows the effect of NESP on females' completion of fifth grade and the highest level of schooling obtained after using between survey and across district variation of reform intensity in a slightly different way than depicted in Equation 2. Here, the exposed cohort is obtained from the 2011 survey year and are 35 to 44 year olds (0-9 year olds in 1976), whereas the unexposed cohort comprises of 35 to 44 year olds but come from 2001 survey year (10-19 year olds in 1976). This allows us to compare outcomes from exposed and unexposed individuals of same age groups. The specifications shown in Columns (1) and (3) include the survey year fixed effects and mother's district of birth fixed effects and personal characteristics.[14] The specifications in Columns (2) and (4) include additional control variables such as the district-specific total absentee count and the total individuals displaced (per ten thousand people) interacted with 2011 survey year, respectively. Since the main objective is to analyze child outcomes, the sample from hereon is

[14] In additional specification we use birth year fixed effects instead of survey year fixed effects, which is also the cohort fixed effects in this case. The results are similar. We prefer using cohort fixed effects instead of birth year fixed effects to reduce the number of parameters that needs to be estimated in DDD specification (i.e., cohort and district interaction versus birth year and district interaction). Birth year and district interaction yields a lot more parameters that needs to be estimated.

restricted to mothers with children of ages 10 to 15.

The coefficient of interest, which is the interaction between 2011 survey year and intensity, as shown in Table 4, is positive and statistically significant at the 1 percent level across all columns. Moreover, the magnitude of the coefficient is economically relevant. The standard deviation of the reform intensity is 9.8. Specifically, the coefficients in Columns (2) and (4) suggest that one standard deviation increase in reform intensity increases completion of fifth grade by 7 percentage points and improves the highest level of schooling attained by 0.6 units. To further investigate the validity of our findings we estimate the case study specification as shown in equation 3. The age-specific estimates of the reform are plotted in Figure 6. The trend in coefficients on β_j, as shown in Figure 6, is similar to the trend shown in Figure 5, which lends support to the validity of identification strategy used.

Next, we turn to a difference-in-difference-in-differences (DDD) specification by using three different variations: 1) Across district variation in reform intensity; 2) Between survey year; and 3) Exposure to the reform, determined by age in 1976. The DDD specification relies upon a lighter set of assumption than difference-in-differences (DD) model. We provide a detailed discussion in section 6.3.

6.3 Results from DDD (Combining within cohort variation with variations from different survey years and across district reform intensity)

Table 5 presents the results from the DDD specification that shows the effect of NESP on female's educational outcomes by using the Census data. Columns (1), (2), (5) and (6) include people from all caste, whereas other Columns restrict the sample to Brahmins and Chettris (mothers from higher caste households). The DDD estimates are positive and statistically significant across all columns. For instance, Columns (2) and (6) suggest that one unit increase in standard deviation of reform intensity increases the probability of completing fifth grade by 3.9 percentage points and increases the highest years of schooling by 0.36 units. The F-Statistic pertaining to test that the DDD estimate is significant equals 25.17 when the highest years of schooling is used as the dependent variable. This is much larger than the critical F-Statistic of 10, which is used to access the strength of instruments. The DDD estimates are larger in magnitude when the sample is restricted to Brahmins and Chettris. This suggests that the effects of NESP on educational outcomes are higher among mother's from higher caste. The F-Statistic associated with significance of the DDD estimate when using the highest years of schooling as the dependent variable in the restricted sample equals 13.07 (in Column 8). Similarly, Table 6 shows the DDD estimates estimated in a similar way as shown in Table 5 but by using NLSS data. Although the DDD estimates are slightly smaller than the DD estimates (as shown in 4), the estimates from both datasets (Census and NLSS) are of similar magnitudes.

As previously mentioned, the validity of the DDD estimates rest on a lighter assumption compared to the underlying assumption governing the unbiasedness of DD estimates. The stricter DD assumption that in absence of the reform, trends in educational outcomes between exposed (35 to 44 in 2011) and unexposed (45 to to 54 in 2011) cohorts across low and high intensity reform districts should not vary systematically is no longer required. In the DDD specification, unobserved cohort-specific factors which are correlated with the reform intensity are allowed to affect exposed (35 to 44 in 2011) and unexposed (45 to 54 in 2011) cohorts across low and high intensity districts differently. These effects are captured by the district-cohort fixed effects. Any unobserved changes within cohort across districts are absorbed by district-cohort fixed effects. Similarly, any common changes affecting younger cohort differently compared to older cohort between two survey years are captured by cohort-survey fixed effects. The underlying assumption now is that unobserved factors correlated with the reform intensity will not affect younger (35 to 44 year olds) and older (45 to 54 year olds) cohorts differently in a district between two survey years. Any cohort-specific unobserved differences between younger and older cohorts in 2011 survey year in a particular district will be differenced out by using unexposed cohorts but of same age groups in 2001 survey given that unobserved changes affect both cohorts in a district similarly between two survey years.

As stated in section 5.6 of the identification strategy, we note two main possible threats to the DDD estimates: 1) Loss due to civil war; and 2) Increase in migration between 2001 and 2011. We note that these changes are unlikely to affect mother's educational outcomes since mother's education is predetermined during the onset of these events, but these events surely poses concerns while using child outcomes as the dependent variable. To address these concerns we include the following triple interaction between (a) count of displaced individuals between 1996 and 2006 (per ten thousand people) interacted with survey year (2011) and cohort (35-44 year olds); and (b) the total count of district specific migrants in 2011 interacted with survey year and cohort.

The results obtained after including these interaction terms are presented in Columns (2), (4), (6) and (8) in Table 5 and Columns (2) and (4) in Table 6. Although these controls address potential omitted variable bias, we caution that these given control variables could be endogenous. For instance, migration could be determined by education, with improvement in education leading to an increase in migration. In contrast, lack of education can increase migration for low-skilled jobs as well. Similarly, educational status of a district can determine intensity of the civil war. It is reassuring that the results shown in Tables 5 and 6 are robust after including these district-specific controls.

It is problematic if NESP also affected males' educational outcomes similar to females' education as in such a case it would be difficult to isolate effect of mother's education on child outcomes from improvements in father's educational outcomes. For robustness check, we carry out the same DD and DDD procedure

for male household head or spouse of household head. The estimates are plotted against age category in Appendix Figure A2. The results show that male education is not differentially impacted by intensity of the reform, confirming our earlier claim using district-level data (see Figure 2). The coefficients for all age categories are statistically not different from zero and close to one another. This increases our confidence that any impact of the program on the education of next generation would come from improvement in female education.

7 Effect of NESP on Child Outcomes

7.1 Reduced Form Estimates

Table 7 presents reduced form estimates of the effect of NESP on children's educational outcomes from the DDD specification after using the Census data. All columns include controls for the double interaction terms between survey-cohort, cohort-district, district-survey fixed effects, and personal characteristics. Additionally, the even columns include district-specific controls given by the set of district-level triple interaction terms used in Table 5.

Referring to the findings from the previous section that NESP improved status of maternal education, we may expect to see a decrease in labor and an increase in educational outcomes among children belonging to mothers affected by NESP. However, Table 7 indicate that NESP had no effect on child educational outcomes given by the status of attending school and completion of fifth grade. Our estimates supporting a null hypothesis of no effect is surprising at first hand compared to prior literature demonstrating positive effects of maternal education on child outcomes including health (Chou et al., 2010; Currie and Moretti, 2003). Our measures of educational outcomes are self-reported, which could potentially create desirability bias if parents perceive schooling to be socially desirable. However, even if such biases are present, they are unlikely to explain the results in Table 7. For measurement issue to be of a concern, misreporting should be correlated with the reform and should differ between younger and older cohorts in a district between two survey years 2001 and 2011.

We emphasize that caste hierarchy, a social concept that has profuse influence over determining societal norms, may be influential in explaining child outcomes. Table 7 include individuals from all castes. It is particularly important to understand the caste structure of Nepal in this context to understand the results supporting the null hypothesis of no effect in Table 7.

Briefly, Nepali caste system is dependent on the varna model of social stratification based on birth. There are four major varnas defined as: 1) Brahmin (priests, scholars, and educators); 2) Kshatriya (soldiers,

governors, kings); 3) Vaishya (merchants, farmers, and artisans); and 4) Sudra (laborers, artisans, and service providers). Severe discrimination persists in the nation across these castes. Although Nepal's constitution of 1990 guarantees equality by stating that the State will not discriminate against citizens based on "religion, color, sex, caste, ethnicity or belief," such rights are bounded by the clause that traditional practices at religious places should not be considered discriminatory (Gurung, 2005). This means that untouchables still have no access to religious places and shrines. Hence, the 1990 constitution of Nepal reaffirms the confinement with Hindu ideology of the caste system.

Such a rigid structure of Nepali caste system creates social inequality and allows exclusion of millions of lower caste people in various spheres including access to education and health care. The data available provides a high positive correlation between caste rank and illiteracy. According to the 1991 Census, Nepal had a literacy rate of 39.3 percent; the average literacy rate for the oppressed caste (Dalit) was only 22.8 percent.[15] The national literacy rate had increased to 54 percent in 2001, but the literacy rate among the oppressed caste (Dalit) remained 33.8 percent. A study conducted on caste-based discrimination in schools concluded that the oppressed caste (Dalit) faced exclusion in school from both teachers and fellow students. The teachers participated in indirect form of discrimination such as neglect, repeated blaming, and labeling of Dalit students as weak performers (Bishworma, 2010). The extreme poverty under which most Dalit live is another reason that explains such poor educational outcomes. Children from households of lower castes typically have a higher opportunity cost of attending schools due to severe poverty. Given such a rigid social context of Nepal's caste system, we expect differential effect of NESP on children from higher caste households. Lack of differentiation across caste may explain the zero effect shown in Table 7.

This variation by caste is clearly depicted in Figure 7. The figure shows age-education profile for males and females of low and high caste born in districts with high and low intensity of NESP reform. We provide separate figures for 2001 (7a) and 2011 (7b). Several salient points of Nepali education is evident from the graph. First, male education was higher than female education among the oldest cohort, but low caste males had more education than high caste females. Progress in male education over time is approximately linear in both low and high intensity districts. The gain in education is most pronounced for high caste females in high intensity districts, whereas the trend for low caste females does not appear to be very different across the two categories of districts.

We expect to see positive effects of NESP on child outcomes among children from mothers of higher caste mainly because they typically face lower direct and opportunity costs of schooling. Table 8 presents

[15]The Dalit population comprised of 12.8 percent of the total population of Nepal (Census 2001). This statistic is highly contended by researchers as they view that this figure is undervalued. The practice of untouchability is highly common in the nation, where Dalits are not allowed to touch public drinking water when Non-Dalits are in queue. The Dalit populace cannot afford private health care and also are not able to get an easy access to government subsidized health care due to caste-based discrimination (see Bhattachan et al., 2009).

estimation similar to Table 7 but only include Brahmins and Chhetris mothers who belong to a higher spectrum in the caste-hierarchy, hence, not exposed to caste-based discrimination. The results from Table 8 shows that NESP reform has a positive effect on children's completion of fifth grade. The DDD coefficients from Columns (7) and (8) indicate that one standard deviation increase in intensity of the reform results to an average increase of a child's fifth grade completion by 1 and 1.5 percentage points. These estimates are modest in magnitude but are statistically significant at the 1 percent level.

Now we turn to estimates of the impact of NESP on child labor, which is estimated using the NLSS survey. The results from analyses are presented in Table 9. NLSS contains both monthly and weekly (past week) variables pertaining to work-related activities. We note that recall bias is lower in variables pertaining to the past week, hence, we prefer weekly variables in favor of monthly variables. We focus on three weekly variables: 1) Days worked in the past week; 2) Hours worked per day in the past week; and 3) The total number of hours worked in the past week. These variables are listed in Table 9. The DDD estimates presented in Table 9 are negative across all columns and are consistent with the belief that education reform reduces child labor. For instance, the DDD estimate in Columns (4) and (6) suggests that one standard deviation increase in reform intensity reduces hours worked per day in the past week and total weekly hours worked by 0.2 and 1 units, respectively. Such reductions are of a modest size. The control variables included in the model are similar to the variables used in Table 7 and Table 8.

7.2 IV Estimation

To understand the relevance of the reduced-form estimates, we turn to instrumental variable estimates. The estimates are reported in Table 10 (using the Census data) and 11 (using NLSS data) for children's education and labor outcomes.

Table 10 instruments mother's highest level of schooling by using the reform and presents both OLS and IV estimates when the sample is restricted to mothers from Brahmin and Chhetri families. The OLS estimates treat mother's schooling as exogenous, whereas the IV estimates treat schooling as endogenous and use the interaction between cohort, survey year and reform intensity as the instrument. The OLS estimates show that increases in maternal years of schooling is associated with a reduction in child labor participation. In contrast, the IV estimates pertaining to child labor participation are close to zero and statistically insignificant at any conventional levels. This lends support to the null hypothesis that increases in mothers years of schooling has no effect on child labor participation. There are two main explanations to such differences between the OLS and IV estimates. First, IV estimates do not represent the average treatment effect but reflects the local average treatment effect (LATE). In this context, the IV interpretation

is limited to the effect of mother's education on child outcomes only among those mothers affected by NESP. Educated mothers who were not affected by NESP can potentially share different family background compared to mothers affected by NESP. They would have been educated regardless of NESP and are likely to be from the higher spectrum of socio-economic background. The OLS estimate picks up effect of these mothers as well, whereas the IV estimation is precisely concerned with mothers who were affected by NESP. Second, the OLS estimates are likely to be driven by other unobserved factors correlated with both child labor and mother's education.

When turning to educational outcomes, both OLS and IV estimates have positive signs. The OLS estimates are statistically significant at the 1 percent level, indicating that increases in maternal years of schooling on average is associated with an increased likelihood of a child's school attendance and completion of fifth grade. Although positive, the IV estimate pertaining to school attendance is not statistically significant at any conventional levels for specification including district-level interaction terms. The IV estimate referring to completion of fifth grade is positive and statistically significant at a 1 percent level. Specifically, the IV estimate in Column (8) suggests that an increase in mother's schooling by a year on average increases the likelihood of a child's completion of fifth grade by 3 percentage points among mothers affected by NESP. The magnitude of IV estimate is twice as that of the OLS estimate.[16]

Table 11 shows the effect of mother's highest level of schooling on comprehensive measures of child labor outcomes in the past week by presenting both the OLS and IV estimates from NLSS data. Both the OLS and IV estimates in Table 11 suggest that increases in mother's schooling reflects less time spent on child labor. Although the IV estimates have much larger standard errors compared to the OLS estimates, the estimates pertaining to hours per day and the total weekly hours (Columns 4 and 6) are statistically significant at a 1 percent level. The magnitude of these IV estimates are approximately two fold larger than the OLS estimates. Although larger than the OLS estimates, we note that the magnitude of IV estimates reports to a modest reduction in hours worked in the past week. For instance, Column (6) indicates that an additional year of an increase in mother's education on average reduces child's total number of hours worked in the past week by 0.62 units.

8 Conclusion

The previous literature on intergenerational effect of parent's schooling on children's education outcomes is focused on developed nations. Due to compulsory education laws and strict enforcement of laws, child

[16]To access whether the standard errors are underreported in the second stage, additionally we estimate standard errors from 200 bootstrap replications. The results remain similar. The results are not shown but are available upon request from the authors.

labor is rarely an issue in these settings. In this study, we focus in the context of Nepal, where child labor is still widely prevalent. We estimate the intergenerational effect of the Nepal Education System Plan (NESP), which enhanced educational outcomes among females who were of school-going-age during the implementation period of the reform (1976), on children's education and labor outcomes. Subsequently, using exposure to the reform as an instrument, we identify the effect of mother's education on children's education and labor outcomes.

Our preferred identification strategy is based on difference-in-difference-in-differences (DDD) specification, which uses three sources of variations for identification: 1) Intensity of the reform across districts, 2) Age of the mother at the time of reform, and 3) Survey years. This identification strategy allows us to difference out unobserved differences between children belonging to younger and older cohorts, such as birth order and family structure, which may arise from using only one cross-sectional survey. We thus address the concern that comparing parents of different ages (older versus younger) while evaluating intergenerational effects is likely to suffer from endogeneity due to differences in birth order between children from younger and older parents.

We find that mothers exposed to the reform were more likely to complete fifth grade and attain a higher level of schooling. Among mothers exposed to the reform, one standard deviation increase in reform intensity on average increased the probability of completing fifth grade by 3.9 percentage points and increased the highest years of schooling by 0.36 units. Given that the average years of schooling among females in the sample is 1.43 and only 15 percent of mothers have completed fifth grade (from 2011 Census), the effect of the reform is economically significant. Furthermore, the reform is more effective among mothers belonging to privileged caste status (Brahmin and Chhetri). When evaluating the effect of NESP on children's education outcomes we find no effect in the whole sample and the intergenerational effect on children's education persists only among children born to mothers who belong to higher castes (Brahmins and Chhetri). The IV estimates indicate that mothers' education leads to an increase in children's likelihood of completing fifth grade but again the results are restricted to mothers from higher caste households. Improvement in mothers' education creates a modest reduction in child's total hours worked in the past week by approximately an hour.

The findings reported here are a matter of interest because they show that implementation of a universal education system can be successful in generating higher educational outcomes, which can follow through to younger generation. However, in the context of our study, caste hierarchy is a specific matter of concern and homogeneous implementation of policies framed to enhance education would be relatively less useful if rigid caste structure is not accounted for. Caste-based exclusion persists in many parts of South Asia besides Nepal including India, Bangladesh, and Sri Lanka. Given the findings, we caution that policies designed to

improve access to education in developing nations such as Nepal should thoroughly consider societal structure before implementing such policies in a major scale.

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9 Tables

Table 1: Illustration of identification strategy

	Age in 2001	Age in 2011
Treated in 1976 (Aged 0-9)	25-34	35-44
	(Cohort A)	(Cohort B)
Not treated in 1976 (Aged 10-19)	35-44	45-54
	(Cohort C)	(Cohort D)
Not treated in 1976 − Placebo group	45-54	55-64
(Aged 20-29)	(Cohort E)	

Note: Cohorts A and B are exposed to the reform, whereas individuals belonging to Cohorts C, D, and E had surpassed their school-going-age during the implementation of the reform as a whole in 1976.

TABLE 2: Summary statistics of key variables

	Mean	SD	Mean	SD
Panel A: Nepal census				
	2001		2011	
5th grade complete (mother)	0.0787	0.269	0.15	0.357
Highest level of schooling (mother)	0.71	2.396	1.428	3.256
Attending school (child)	0.788	0.409	0.885	0.319
5th grade complete (child)	0.416	0.493	0.599	0.49
Brahmin	0.18	0.384	0.176	0.381
Chhetri	0.139	0.346	0.103	0.304
Hindu	0.805	0.396	0.799	0.401
Buddhist	0.121	0.326	0.103	0.304
Age (child)	12.58	1.705	12.69	1.703
Sex (child)	1.478	0.5	1.495	0.5
Birth order (child)	2.433	1.16	2.355	1.135
Male school enrollment (1971)	22.93	9.764	21.79	8.913
displacement (per 10,000)	32.41	137.3	55.52	286.7
Total number of absentee	29902.1	21797.7	28603.6	21575.2
Panel B: NLSS				
	1996		2004	
5th grade complete (mother)	0.0923	0.289	0.142	0.349
Highest level of schooling (mother)	0.882	2.644	1.31	3.08
Days worked (per week)	1.495	2.689	1.775	2.749
Hours worked (per day)	1.262	2.479	1.135	1.888
Total hours worked (weekly)	7.198	15.19	5.392	10.07
Chhetri	0.196	0.397	0.18	0.384
Brahmins	0.177	0.382	0.147	0.354
Hindu	0.848	0.359	0.824	0.381
Buddhist	0.0685	0.253	0.108	0.31
Child age	12.39	1.682	12.42	1.721
Child gender	0.478	0.5	0.473	0.499
Birth order	2.993	1.455	2.749	1.413
Male school enrollment (1971)	24	10.97	23.48	10.53
Displacement (per 10,000)	51.6	290.6	35.19	163.4
Total number of absentee	33187.7	24601.1	31320.9	22599.2

Table 3: Difference-in-Differences (Effect of NESP on Mother's Education Using Individual Survey Year in a Full Sample)

	Census 2001	Census 2001	Census 2001	Census 2001	Census 2011	Census 2011	Census 2011	Census 2011
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	m5grade	m5grade	mlevel	mlevel	m5grade	m5grade	mlevel	mlevel
Cohort*Intensity	0.00397***	0.00407***	0.0373***	0.0384***	0.00445***	0.00480***	0.0416***	0.0444***
	(0.000637)	(0.000719)	(0.00621)	(0.00689)	(0.000788)	(0.000887)	(0.00708)	(0.00811)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	641154	641154	641154	641154	680149	680149	680149	680149

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: All model specifications control for cohort and district of birth fixed effects, caste and religion fixed effects. Additionally, specifications with district level controls (even Columns) include district-specific total absentee count and the number of individuals displaced (per ten thousand people) due to the civil war interacted with exposed cohort indicator. Robust standard errors clustered at the district of birth are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

Table 4: Difference-in-Differences (Effect of NESP on Mothers Education Combining Two Survey Years)

	(1)	(2)	(3)	(4)
	m5grade	m5grade	mlevel	mlevel
2011*Intensity	0.00582***	0.00651***	0.0575***	0.0630***
	(0.000908)	(0.00107)	(0.00878)	(0.0105)
Controls	No	Yes	No	Yes
N	301631	301631	301631	301631

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: All model specifications control for survey year and district of birth fixed effects, caste and religion fixed effects. Note that survey year fixed effects are perfectly collinear with cohort fixed effects. Additionally, specifications with district-level controls include district-specific total absentee count and the number of individuals displaced (per ten thousand people) due to the civil war interacted with 2011 survey year indicator. Robust standard errors clustered at the district of birth are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

Table 5: DDD (Effect of NESP on Female Education, Using Census 2001 and 2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	m5grade	m5grade	m5grade	m5grade	mlevel	mlevel	mlevel	mlevel
2011*Exposed*Intensity	0.00322***	0.00392***	0.00542***	0.00502***	0.0295***	0.0360***	0.0553***	0.0490***
	(0.000789)	(0.000758)	(0.00133)	(0.00135)	(0.00760)	(0.00718)	(0.0137)	(0.0136)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	422163	422163	125004	125004	422163	422163	125004	125004

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: Columns (3), (4), (7) and (8) restricts the sample from Brahmin and Chhetri households. All specifications include a full set of double interaction between cohort and mother's district of birth, cohort and survey year, and survey year and mother's district of birth fixed effects. Addtionally, all specifications include caste and religion fixed effects. The specifications with district level controls (even Columns) include triple interaction terms between the total number of absentee due to migration, cohort, and survey year; and the total number of displaced individuals (per ten thousand people), cohort, and survey year. Two-way clustered robust standard errors at mother's district of birth and cohort are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

Table 6: DDD (Effect of NESP on Female Education, Using NLSS 1995 and 2003)

	(1)	(2)	(3)	(4)
	m5grade	m5grade	highest_educa	highest_educa
2011*Exposed*Intensity	0.00507***	0.00524***	0.0440***	0.0445***
	(0.00140)	(0.00141)	(0.0122)	(0.0121)
Controls	No	Yes	No	Yes
N	4450	4450	4450	4450

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: All specifications include a full set of double interaction between cohort and mother's district of birth, cohort and survey year, and survey year and mother's district of birth fixed effects. Additionally, all specifications include father's literacy status, caste and religion fixed effects. The specifications with district level controls include triple interaction between the total number of absentee due to migration, cohort, and survey year; and the total number of displaced individuals (per ten thousand people), cohort, and survey year. Two-way clustered robust standard errors at mother's district of birth and cohort are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

Table 7: DDD (Effect of NESP on Children's Education, Using Census 2001 and 2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	labor1	labor1	labor2	labor2	attending	attending	level5	level5
2011*Exposed*Intensity	0.000389	0.000146	0.0000179	0.0000399	0.00000694	0.0000200	-0.000121	0.000118
	(0.000289)	(0.000393)	(0.000330)	(0.000411)	(0.000235)	(0.000271)	(0.000290)	(0.000316)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	422163	422163	422163	422163	420954	420954	422163	422163

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: All specifications include a full set of double interaction between cohort and mother's district of birth, cohort and survey year, and survey year and mother's district of birth fixed effects. Additionally, all specifications include child's age, birth order fixed effects, gender, caste and religion fixed effects. The specifications with district level controls include triple interaction terms between the total number of absentee due to migration, cohort, and survey year; and the total number of displaced individuals (per ten thousand people), cohort, and survey year. Two-way clustered robust standard errors at mother's district of birth and cohort are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

Table 8: DDD (Effect of NESP on Child Outcomes among Brahmins and Chhetris, Using Census 2001 and 2011)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	labor1	labor1	labor2	labor2	attending	attending	level5	level5
2011*Exposed*Intensity	0.000731	0.000943	0.000419	0.00117	0.000756**	0.000486	0.00125***	0.00145***
	(0.000454)	(0.000624)	(0.000568)	(0.000766)	(0.000316)	(0.000428)	(0.000430)	(0.000533)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	125004	125004	125004	125004	124338	124338	125004	125004

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: The sample is restricted to mothers from Brahmin and Chhetri households. All specifications include a full set of double interaction between cohort and mother's district of birth, cohort and survey year, and survey year and mother's district of birth fixed effects. Additionally, all specifications include child's age, birth order fixed effects, gender, caste and religion fixed effects. The specifications with district level controls include triple interaction terms between the total number of absentee due to migration, cohort, and survey year; and the total number of displaced individuals (per ten thousand people), cohort, and survey year. Two-way clustering of robust standard errors at mother's district of birth and cohort are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

TABLE 9: DDD (Effect of NESP on Child Labor, NLSS sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	days_perweek	days_perweek	hours_perday_weekly	hours_perday_weekly	total_hours_weekly	total_hours_weekly
2011*Exposed*Intensity	-0.0254***	-0.0253***	-0.0161*	-0.0170*	-0.0952*	-0.0953*
	(0.00920)	(0.00966)	(0.00945)	(0.00986)	(0.0544)	(0.0569)
Controls	No	Yes	No	Yes	No	Yes
N	4450	4450	4450	4450	4450	4450

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: All specifications include a full set of double interaction between cohort and mother's district of birth, cohort and survey year, and survey year and mother's district of birth fixed effects. Additionally, all specifications include father's literacy status, caste, gender, child's age and religion fixed effects. The specifications with district level controls include triple interaction between the total number of absentee due to migration, cohort, and survey year; and the total number of displaced individuals (per ten thousand people), cohort, and survey year. Two-way clustering of robust standard errors at mother's district of birth and cohort are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

Table 10: OLS and IV Estimates of the Effect of Mother's Education on Child Outcomes using the Census Data, Brahmins and Chhetris

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	labor1	labor1	labor2	labor2	attending	attending	level5	level5
OLS	-0.00814***		-0.0109***		0.00239***		0.0150***	
	(0.000523)		(0.000735)		(0.000301)		(0.000640)	
IV		0.0192		0.0238		0.00991		0.0296***
		(0.0127)		(0.0156)		(0.00874)		(0.0109)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	125004	125004	125004	125004	124338	124338	125004	125004

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: The sample is restricted to mothers from Brahmin and Chhetri households. All specifications include a full set of double interaction between cohort and mother's district of birth, cohort and survey year, and survey year and mother's district of birth fixed effects. Additionally, all specifications include child's age, birth order fixed effects, gender, caste and religion fixed effects. The specifications with district level controls include triple interaction terms between the total number of absentee due to migration, cohort, and survey year; and the total number of displaced individuals (per ten thousand people), cohort, and survey year. Two-way clustering of robust standard errors at mother's district of birth and cohort are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

Table 11: OLS and IV Estimates of the Effect of Mother's Education on Child Labor Outcomes using NLSS Data

	(1)	(2)	(3)	(4)	(5)	(6)
	days_perweek	days_perweek	hours_perday_weekly	hours_perday_weekly	total_hours_weekly	total_hours_weekly
OLS	-0.108***		-0.0690***		-0.406***	
	(0.0136)		(0.0117)		(0.0589)	
IV		-0.0963*		-0.160***		-0.622***
		(0.0536)		(0.0323)		(0.184)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	4450	4450	4450	4450	4450	4450

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

Note: All specifications include a full set of double interaction between cohort and mother's district of birth, cohort and survey year, and survey year and mother's district of birth fixed effects. Addtionally, all specifications include child's age, birth order fixed effects, gender, caste and religion fixed effects. The specifications with district level controls include triple interaction terms between the total number of absentee due to migration, cohort, and survey year; and the total number of displaced individuals (per ten thousand people), cohort, and survey year. Two-way clustering of robust standard errors at mother's district of birth and cohort are presented in parenthesis. * p<0.1, ** p<0.05, *** p<0.01

10 Figures

FIGURE 1: Correlation between Male and Female Enrollment in 1971

Source: Nepal Census 1971. The figure shows correlation between males' and females' enrollment rates, aged 6-14 in 1971.

FIGURE 2: Change in Literacy Rate among 10-14 Year Olds between 1971 and 1981 by 1971 Male Enrollment Rate

Source: Nepal Census 1971 and 1981. The vertical axis plots the percentage point difference in literacy rate of 10 to 14 year old males and females between 1971 and 1981. The horizontal axis plots enrollment rates of males aged 6-14 in 1971.

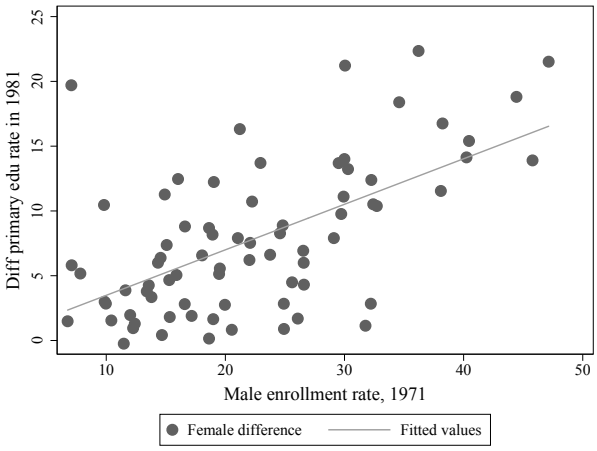


FIGURE 3: Difference in 1981 Primary School Completion Rate between Females 6-14 and 20-29 by Male's Enrollment Rate in 1971

Source: Nepal Census 1971 and 1981

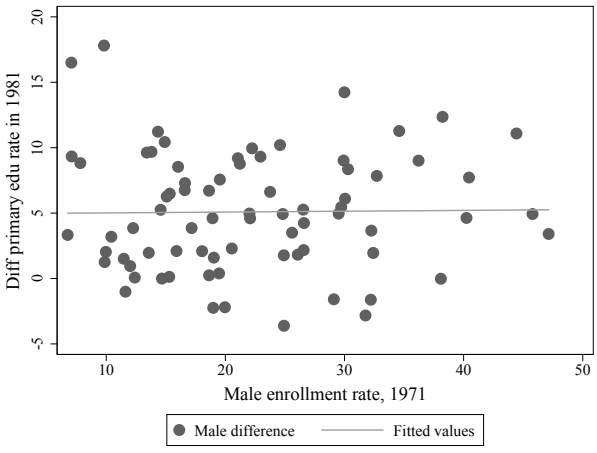


FIGURE 4: Difference in 1981 Primary School Completion Rate between Males 6-14 and 20-29 by Male's Enrollment Rate in 1971

Source: Nepal Census 1971 and 1981.

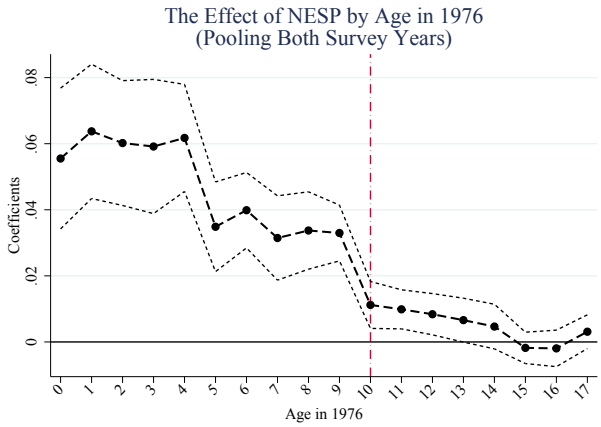


FIGURE 5: The Effect of NESP by Age (pooling across surveys 2001 and 2011)

Note: The figure show coefficients on the interaction term after estimating equation 3 and uses both 2001 and 2011 survey years from the census.

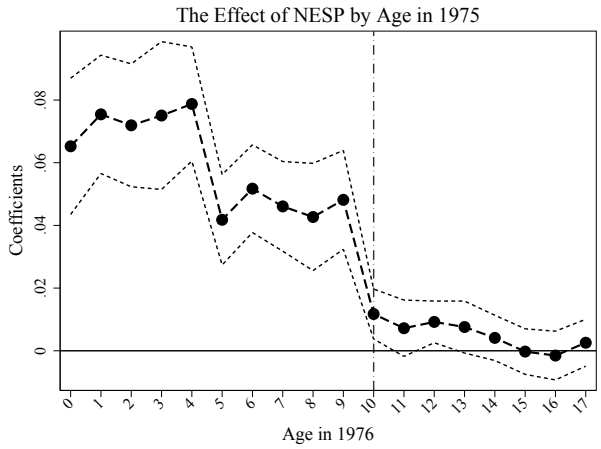
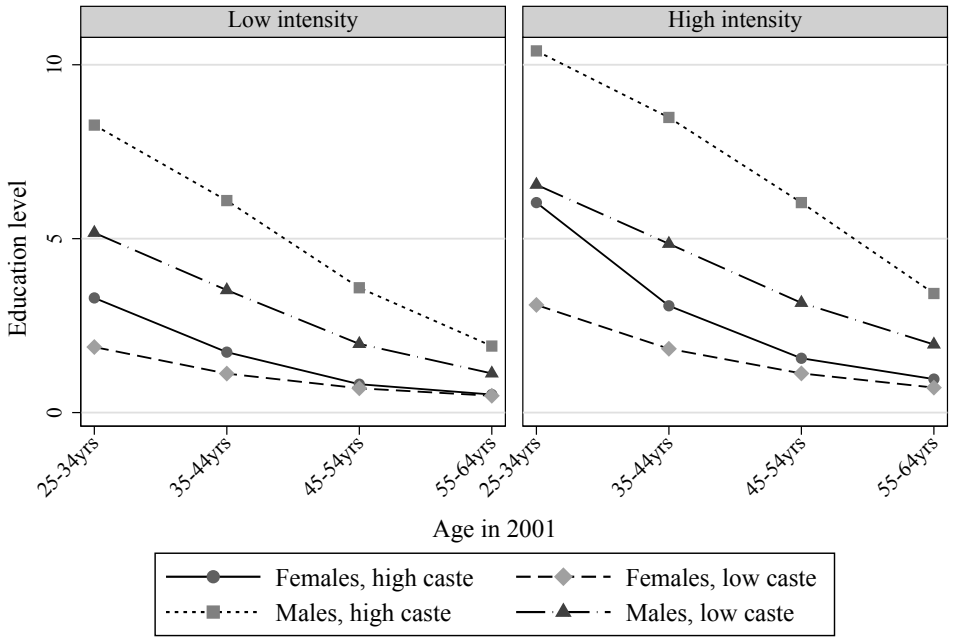
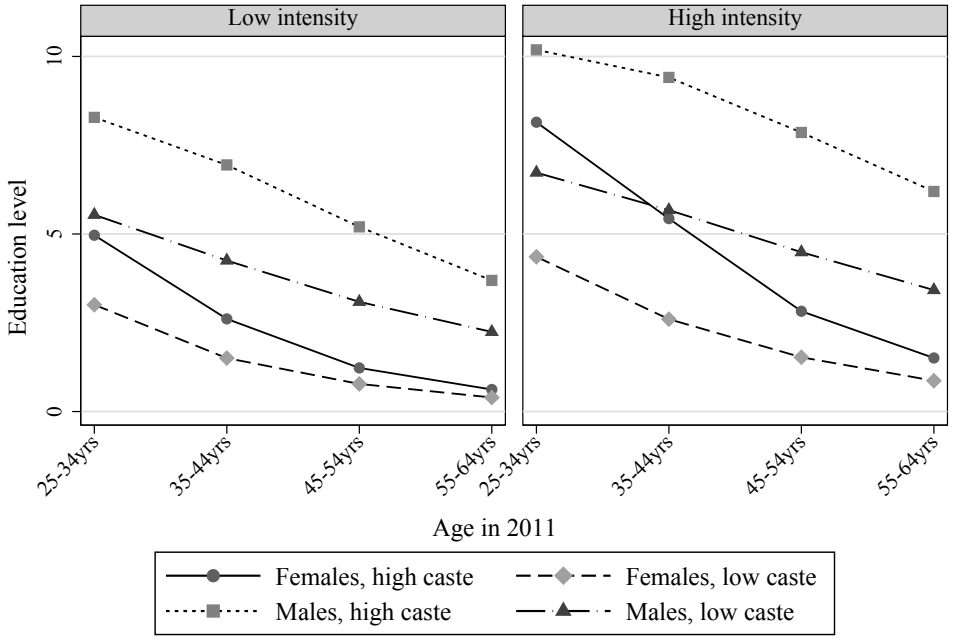


FIGURE 6: The Effect of NESP by Age

Note: The figure show coefficients on the interaction term from a case study where exposed and unexposed groups come from 2011 and 2001 survey years, respectively, and are 35-44 year olds during the time of survey. The estimation strategy is similar to equation 3 except that we use Cohort B as exposed cohort and Cohort C as unexposed cohort (in Table 1).



(A) Census 2001

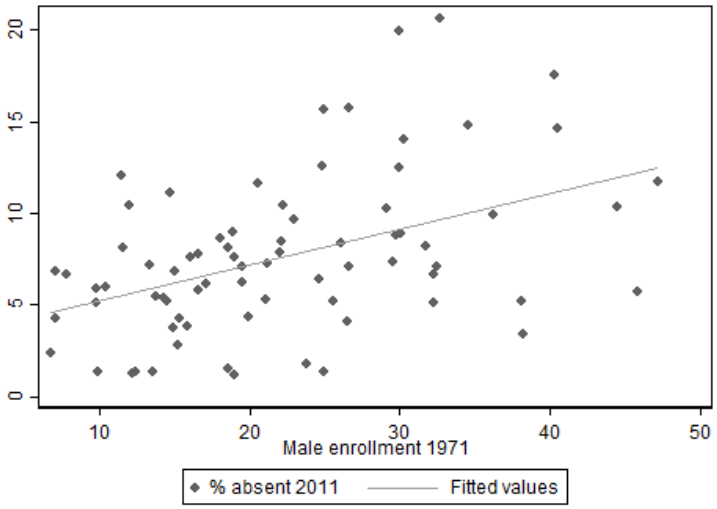


(B) Census 2011

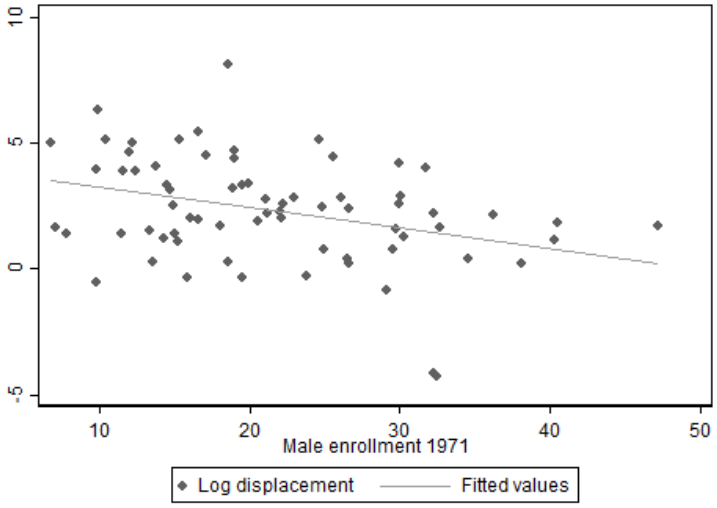
FIGURE 7: Education Levels of Age Cohorts for Females and Males of High and Low Caste by Reform Intensity.

Appendix

Figures

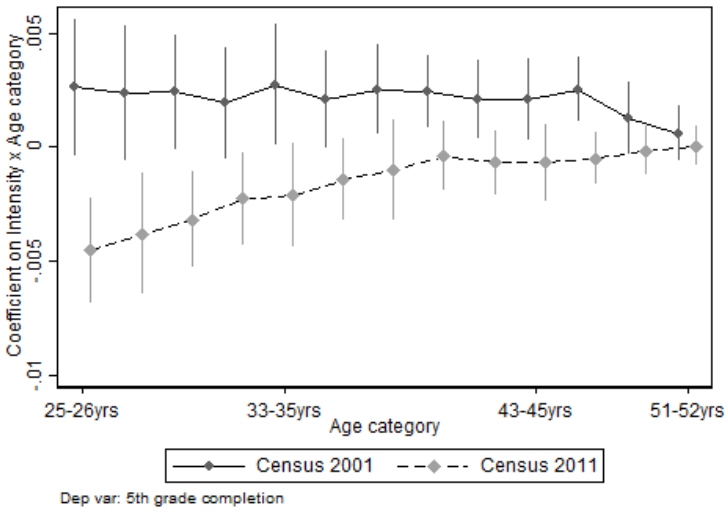


(A) Proportion of absent population (Source: Census 2011)

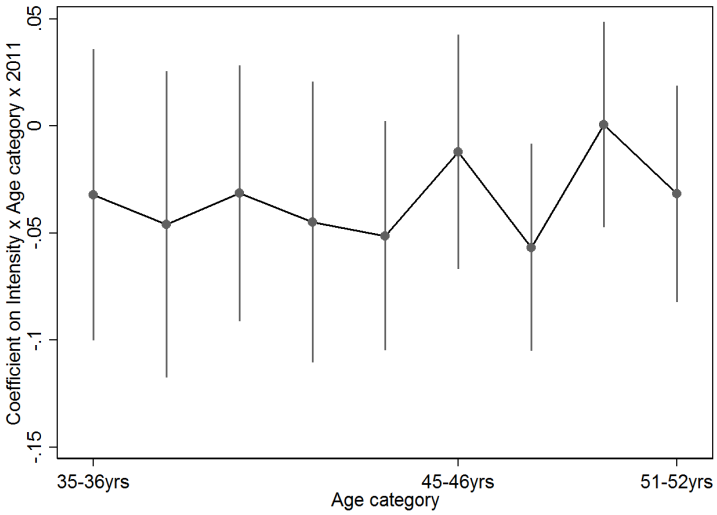


(B) Conflict-related displacement 1996-2006 (Source: INSEC)

FIGURE A1: Correlations between school enrollment rate in 1971 and recent changes in migration and conflict-related displacement



(A) Coefficients estimates from single year DD



(B) Coefficients estimates from DDD

FIGURE A2: Coefficient estimates of DD and DDD strategy using sample of male household heads

Figure shows coefficients obtained from regressing education level on (1) interaction between reform intensity and age category for panel (A) and (2) interaction between reform intensity, age category and year 2011 dummy for panel (B), controlling for appropriate fixed effects. The model excludes other control variables.

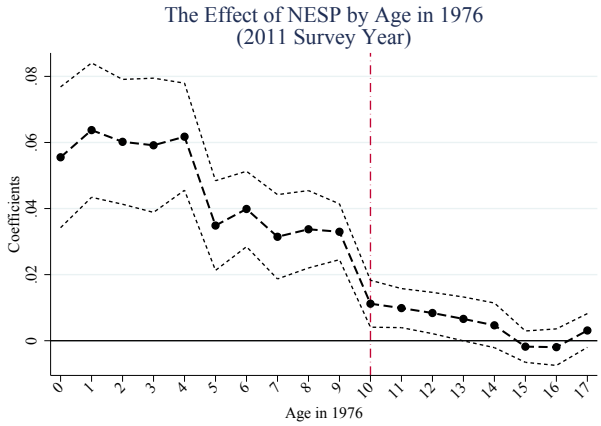


FIGURE A3: The effect of NESP by age (using 2011 survey)

Note: The figure show coefficients on the interaction term after estimating equation 3 and uses only 2011 survey year from the census.

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