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The Long-Term Health Effects of Educational Expansion: Evidence from India's DPEP

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Empirical research has sought to causally estimate the long-run effects of education on adult health behaviors worldwide. However, the existing evidence is largely concentrated in developed country contexts - primarily the U.S., U.K., and Scandinavia - with limited evidence from low- and middle-income countries, particularly India. This study exploits the variation introduced by the District Primary Education Program (DPEP), which targeted districts with female literacy rates below the 1991 national average using a difference-in-difference approach with fixed effects. Using data from the 1991 Census and the National Family Health Survey (NFHS Round 4), I estimate the causal impact of DPEP on educational attainment and downstream effects on health and substance use outcomes using an Instrumental Variables approach. The study contributes to the literature by providing the first causal evidence on the relationship between schooling expansion and adult health outcomes in India. The results indicate that DPEP led to improvements in educational attainment but the effects on health and substance use behavior are very modest or close to zero.

Keywords: School Expansion Program, Adult Health Outcomes, Substance Use Behavior

JEL: I12, I15, I38

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AI Declaration

For this thesis, AI has been utilised to support the generation of econometric Stata code, focusing on debugging and syntax corrections. Furthermore, generative AI has been used to enhance the grammar, punctuation, and spellcheck to improve readability of the paper. For all the aforementioned tasks, ChatGPT (OpenAI, free model) and Claude Sonnet 4.6 (Anthropic) have been utilised.

All AI help that was taken was reviewed and appropriately modified or rejected by the author. All empirical decisions, analytical choices, results discussed and any remaining errors are my own.

Contents

List of Abbreviations Used

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Abbreviation	Full Form
BMI	Body Mass Index
CD	Cragg-Donald
DiD	Difference-in-Differences
DHS	Demographic and Health Survey
DPEP	District Primary Education Program
IV	Instrumental Variable
KP	Kleibergen-Paap
LATE	Local Average Treatment Effect
NCD	Non-Communicable Disease
NFHS	National Family Health Survey
OLS	Ordinary Least Squares
SC	Scheduled Caste
SHRUG	Socioeconomic High-Resolution Rural-Urban Geographic Dataset
SSA	Sarva Shiksha Abhiyan
ST	Scheduled Tribe
WHO	World Health Organisation

1 Introduction

Education is increasingly being recognized as a key determinant of long-run socioeconomic outcomes including health beyond its direct effects on earnings and labor market performance. Better educated individuals could have better health outcomes due to improved knowledge on health risks, enhanced cognitive skills, better income and access to healthcare, and better peer environments. A substantial body of literature documents a strong education gradient in health, where individuals with more schooling are likely to exhibit lower mortality, better physical health, and reduced engagement in risky substance use behaviors. Theoretical explanations of the correlation between education and health are diverging. Grossman (1972) develops a model based on education entering the health production function as a factor and suggests that education improves access to health-related information and better cognitive ability to process this information and make health-related decisions. However, Fuchs (1982) claim that this observed correlation between health and education is not causal but rather due to unobservables, such as discount factor and time preferences or ability, which could simultaneously drive both duration of education and healthier lifestyles. Establishing causality of the effect of education on health is thus central in the empirical economics literature in this area.

Deriving causal estimates of the relationship between education and health remains challenging. This is because in most cases, education is not randomly assigned: individuals who attain more schooling could differ systematically from others in several unobserved characteristics. These include preferences, discount rates, cognitive ability, parental education, early-life health endowments and household income etc. One or more of these factors independently influence health outcomes generating serious endogeneity concerns for the estimation. For instance, children with better educated parents are both more likely to receive more education and have better access to healthcare which will not be reflected in a naive OLS estimation of this relationship. To address such endogeneity concerns, a lot of empirical work has relied on instrumental variable strategies, twin studies and compulsory schooling reforms as ways to generate exogenous variation in education. The existing evidence consists of very mixed results that vary by context, identification strategies and sample size. While some of these studies find significant impacts of schooling on mortality and health behaviors, others report small or statistically insignificant impacts, especially for health conditions and biological markers. A lot of this evidence also comes from high-income countries and relatively less is known about long-run health consequences of education in developing countries. These countries have different labor market conditions, disease environments, infrastructure and baseline health and education levels which makes it important to study them.

This study examines the relationship between schooling and long-run adult health and substance use behavior using evidence from India's District Primary Education Program (DPEP).

DPEP was a large-scale policy intervention introduced in 1994 that aimed to expand access to primary education in educationally disadvantaged districts and to bridge socioeconomic gaps in educational attainment. The program focused on improving infrastructure and schooling quality through new school construction and teacher training. Eligibility to DPEP was determined at the district level using a threshold rule based on female literacy rates from the 1991 Population Census. Districts falling below the national average female literacy were eligible for DPEP. However, as the program rule was not perfectly enforced, I exploit the variation in educational attainment introduced by DPEP using a Difference-in-Differences approach to answer the following research questions:

1. Did DPEP generate meaningful improvements in educational attainment?
2. Do improvements in educational attainment translate into better adult health outcomes and substance use behaviors?

This paper contributes to two strands of literature. First, it contributes to addressing the limited empirical evidence on the relationship between education and adult health in developing country settings. To my knowledge, this is the first quasi-experimental analysis evaluating the impact of a schooling program in India on adult health outcomes and substance use behavior. Second, it extends the evaluation of DPEP beyond outcomes that have previously been studied. Existing research has examined program effects on educational attainment and enrollment, labor market outcomes, and social outcomes such as marriage patterns and domestic violence. This paper broadens that evaluation to long-run health. The findings indicate that DPEP led to increases in educational attainment when measured at the district-birth year cohort level, and downstream effects on adult health outcomes and substance use behaviors are generally modest and statistically insignificant. These results suggest that improvements in primary and upper primary schooling alone may not be sufficient to generate long-run health gains in contexts like India. Complementary improvements in healthcare access, public health infrastructure and education beyond basic literacy level may be required for educational gains to translate into meaningful long-run improvements. These null results are consistent with prior empirical literature documenting limited effects of schooling on health, and speak to the debate on whether education policies can serve as effective tools for improving population health in settings where returns to both education and healthcare spending are uncertain (Gupta et al., 2002; Cutler and Lleras-Muney, 2012).

The rest of the paper is organized as follows. Section 2 provides institutional background on DPEP and the Indian health context. Section 3 reviews the relevant literature. Section 4 describes the data, cohort and variable construction. Section 5 outlines the empirical strategy and Section 6 presents the main results, followed by robustness checks and heterogeneity analyses in Sections 7 and 8. Section 9 discusses the findings and Section 10 concludes.

2 Background

2.1 District Primary Education Program (DPEP)

The DPEP was launched in 1994 and was implemented across 219 Indian districts (according to 1991 boundaries - bifurcations increased the number in 2011) in total (Anukriti et al., 2023). The primary goal of DPEP was to universalise primary education and improve learning outcomes. This was done through school construction, enrollment drives, establishment of resource centers and teacher training (Agarwal et al., 2024). It also aimed to reduce disparities in education access for girls and children from Scheduled Castes (SC), Scheduled Tribes (ST), and other disadvantaged communities. The program led to creation of about 160,000 new schools and trained about 1 million teachers across India (Azam and Saing, 2017). The DPEP adopted an "area-specific" assignment rule for treatment with districts as the unit of planning (Government of India, 2008). Because the program had a particular focus on female education, it was targeted to districts that had female literacy rates below the cutoff determined by the national average of 39.3 in the 1991 Census of India. Additionally, the central government also specified successful implementation of the Total Literacy Campaign (TLC), a program that aimed at improving literacy levels (Rao, 1991) as the second criteria for treatment. However, the TLC had been successfully implemented in almost all districts in India by 1994 and hence did not matter for selection into DPEP (Jalan and Glinskaya, 2015). DPEP was implemented in a staggered manner in districts dispersed across the country (Figure 1).

The first phase of the program (1994-2001) included 42 districts, 80 more were added in phase-2 (1996-2002), 27 in phase-3 (1998-2003) and the remaining in the remaining phases starting 1999-2002 (Azam and Saing, 2017). The central government covered 85 percent of program expenditure, aided by international donors including the World Bank and the UK Department for International Development, with the remaining 15 percent borne by state governments. The funding was under a condition that existing education budgets remain unchanged to ensure resources are not diverted from existing budget allocations. This rule created a large DPEP-induced increase in government expenditure (Sunder, 2020). In some districts, DPEP implementation coincided with the launch of the Sarva Shiksha Abhiyan (SSA) program in 2002. To address this confounding, I conduct a robustness check by excluding districts that began implementation in 2002 or later.

2.2 Adult Health in India

Non-communicable diseases (NCDs) have now become a major public health concern globally, but more specifically in low-middle income countries (LMICs). Approximately 18 million

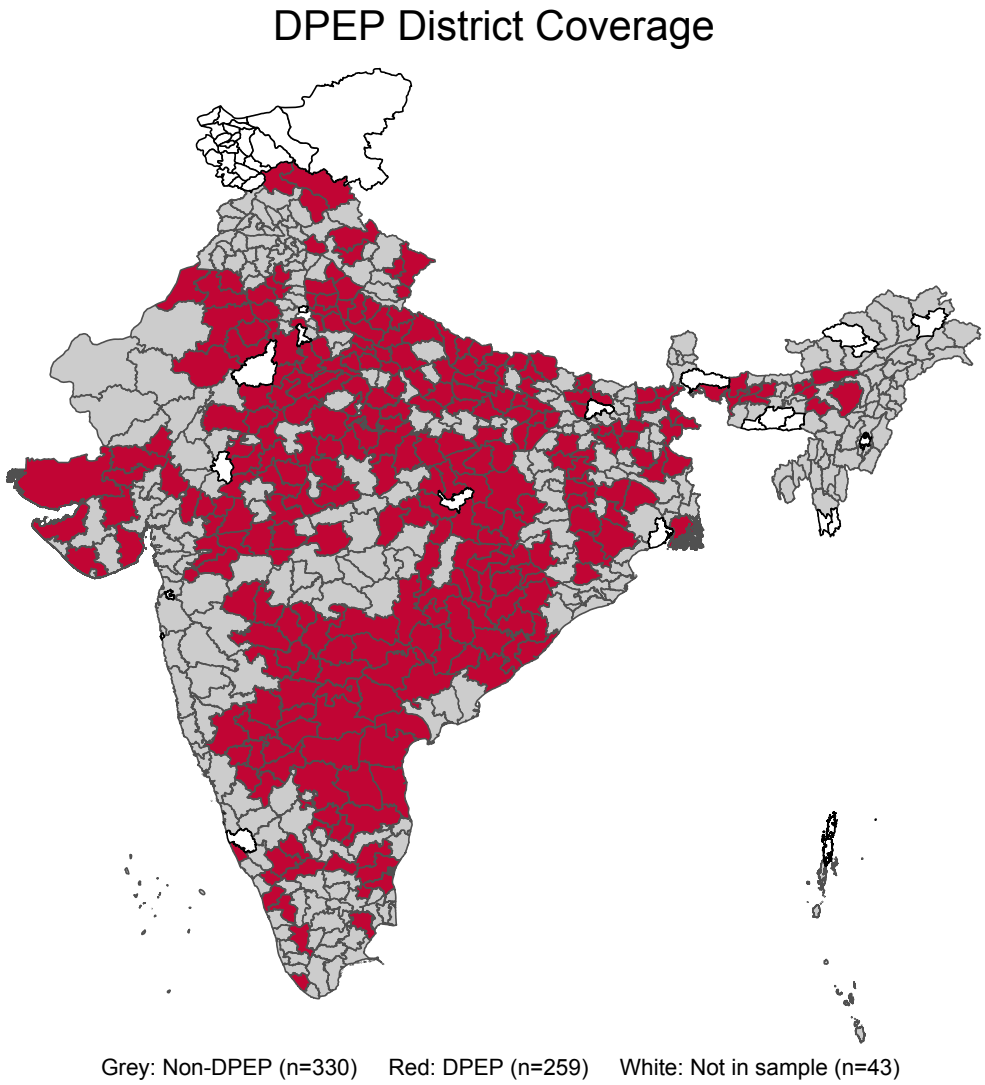

Figure 1: DPEP Coverage Across Districts (2011 Boundaries)

Source: Author's rendering of DPEP district coverage data from Azam and Saing (2017) and Census of India 2011 district boundaries.

people died from an NCD before the age of 70 in 2021 and deaths in LMICs accounted for 82% of these (World Health Organization, 2023). Cardiovascular diseases and diabetes are the leading causes of NCD-related mortality here and behavioral factors such as tobacco use, unhealthy diets and harmful alcohol consumption are known to significantly increase the risk of premature death from NCDs.

Over the last several decades, India has experienced substantial improvements in disability-adjusted life years (DALYs) and reductions in mortality from communicable diseases. However, the overall disease burden in the country has shifted toward non-communicable diseases and injuries (India State-Level Disease Burden Initiative Collaborators, 2017). Chronic conditions such as cardiovascular diseases, mental health disorders and diabetes account for a large share of deaths in India (Patel et al., 2011). Furthermore, India has the second largest population of individuals living with diabetes in the world (International Diabetes Federation, 2023),

and recent evidence suggests a growing trend of hypertension coexisting alongside diabetes (Viswanathan, 2017). National prevalence of regular use of smoking tobacco was estimated to be 16.2% and alcohol consumption 4.5% with men being more likely than women to regularly consume both (Neufeld et al., 2005). Lastly, the prevalence of type 2 diabetes has increased significantly among the young adult group in India (20-39 year olds) (Nanditha et al., 2024). Research also shows that the burden of chronic diseases in India is expected to increase over time which further worsens the severity of health challenges faced by the population.

Health outcomes and health-related challenges in India also exhibit strong socioeconomic gradients. Chronic diseases are often more prevalent in rural and disadvantaged communities, as well as among individuals with little to no formal education (Saikia and Debbarma, 2020). There is also a clear income gradient, with regular substance use increasing among lower income quintiles (Neufeld et al., 2005). Education can influence health through several pathways, including improved health knowledge, higher earning potential, and better access to healthcare services. Individuals with higher levels of education tend to have better health outcomes and are more likely to engage in healthier behaviours. In particular, educated women are more likely to use contraception and access antenatal care services.

It is this collection of evidence about adult health in India that motivates the choice of outcome variables for this study which are detailed in Section 4.

3 Literature Review

The literature review has three main sections. The first section reviews the empirical literature examining the relationship between education policies and adult health outcomes. A large body of research predominantly in developed economies has studied whether increases in educational attainment lead to improvements in long-run health. These papers usually use policy reforms such as compulsory schooling laws as sources of exogenous variation. The second section focuses more specifically on studies linking education to health-related behaviors, including smoking, alcohol consumption and other lifestyle choices, in both developed and developing country contexts. Finally, the third section reviews literature evaluating the impacts of the District Primary Education Program (DPEP) in India. This literature has primarily focused on educational attainment, labor market outcomes and social outcomes such as marriage and domestic violence.

3.1 Education and Schooling on Adult Health

The idea that compulsory schooling laws provide a natural experiment was first formalized by Angrist and Krueger (1991). Since then, a large empirical literature has studied the causal impact of education on health, primarily using instrumental variable approaches and compulsory schooling reforms as sources of exogenous variation. Lleras-Muney (2005) uses successive U.S. censuses to estimate the impact of education on mortality by constructing synthetic cohorts and finds substantial reductions in mortality associated with additional years of schooling. Oreopoulos (2006) implements a regression discontinuity design around the British school-leaving age reform and finds large returns to education in terms of earnings and employment, and modest effects on health outcomes. However, several other studies report small and often statistically insignificant causal effects. Clark and Royer (2013) exploit two major compulsory schooling reforms in Britain and find strong impacts on educational attainment and wages, but little evidence of effects on health and mortality. Fletcher (2015), reexamining U.S. compulsory schooling laws using large survey data, finds limited and insignificant health effects. Jürges et al. (2013) incorporate biological marker data to complement self-reported measures and document strong correlations between education and health, but find statistically insignificant causal estimates when using compulsory schooling reforms as instruments. These biomarker-based analyses are particularly relevant to this study, which also examines objective health indicators in addition to behavioral outcomes.

Estimated impacts of schooling and education on health in the broader quasi-experimental literature are similarly inconclusive and often point to small effects. Meghir and Palme (2001) analyze a Swedish schooling reform that increased compulsory schooling, implemented a

national curriculum, and abolished ability-based selection into academic and non-academic streams. They find substantial gains in educational attainment and earnings, especially among disadvantaged groups, although health effects were not central to their analysis. Kemptner et al. (2011) find some evidence in Germany for the effect of additional schooling on long-term illness for men but not for women. They also find weak effects of education on the likelihood of weight problems and almost no evidence for a causal effect on health behaviors. Galama et al. (2018) review the health literature and conclude that evidence for causal effects of education on outcomes such as obesity is limited. Mazumder (2011) similarly reviews the literature on schooling and health and suggests that much of the recent research finds little evidence of strong causal effects, particularly outside the U.S. context. Overall, while some IV-based studies suggest meaningful health returns to education, others point to small or insignificant effects, highlighting differences across contexts and identification strategies.

In lower- and middle-income settings, schooling expansions have been shown to affect fertility and reproductive health behaviors. The most comprehensive quasi-experimental evidence of the economic benefits of education is provided by Duflo (2001), which studies the Indonesian school construction program by exploiting variation in schooling exposure due to differences in region and date of birth and finds significant wage gains. Osili and Long (2008) find that female education reduces early fertility in Nigeria, while Chen and Guo (2022) show that increased female schooling in China permanently lowers fertility and affects marriage timing and child mortality. De and Tümay (2024), using a fuzzy RD design in Turkey, find improvements in certain reproductive health behaviors, though effects on commonly studied outcomes such as fertility and contraceptive use are weak. Duflo et al. (2026) exploit a randomised controlled trial of secondary school scholarships and find that female scholarship recipients experience delayed fertility, reduced unwanted pregnancies, and significant improvements in child outcomes including a 50% reduction of under-one mortality and cognitive development gains of 0.24 standard deviations by age 5 among their children. The authors also identify changes in parenting practices (higher stimulation, preventive care etc.) as more likely to be the main mechanism and not income effects. This suggests education operates through health literacy and behavioral channels even if labor market returns to education are modest thus providing motivation to study downstream health consequences of primary education expansion in India. In summary, these studies suggest that contextual heterogeneity is particularly pronounced in developing country settings, and that causal impacts of education on health outcomes beyond fertility-related behaviors remain relatively understudied.

3.2 Education Policies on Health-Related Behaviors

This subsection reviews the literature examining whether education causally impacts adult health and reduces engagement in behaviors harmful for health such as smoking and harmful alcohol consumption. Early instrumental variable studies focus on using exogenous variation from natural experiment settings to look at smoking behaviors. De Walque (2007) uses variation in U.S. college attendance caused by draft avoidance during the Vietnam War to examine impacts on smoking. Using risk of induction in Vietnam as an IV for college education, they find that education significantly reduces current smoking and increases smoking cessation. In parallel, Grimard and Parent (2007) use cohort-based differences in male educational attainment during the Vietnam War and find that additional schooling reduces the probability of becoming a smoker, although effects on smoking cessation are less robust.

Other quasi-experimental approaches suggest positive health-related returns to education. Currie and Moretti (2003) use variation in college openings as an instrument for maternal education and find that higher maternal education leads to improvements in birth weights and reduces smoking during pregnancy. Van Kippersluis et al. (2011) find that additional schooling due to a Dutch compulsory schooling reform reduces mortality at older ages and suggest that such long-run health returns are due to benefits from behavioral changes accumulating over time. In contrast, some studies find weaker or context-dependent effects. For example, Kemptner et al. (2011) report limited evidence of schooling effects on smoking behavior, and Clark and Royer (2013) find little impact of British compulsory schooling reforms on adult health outcomes despite strong educational effects.

Twin and sibling studies offer a very robust identification strategy in the absence of random assignment as they control for shared genetic and family background. Koning et al. (2015), using Australian twin data, find that education reduces smoking duration even after accounting for unobserved heterogeneity. Similarly, Johnson et al. (2011) show that higher educational attainment is associated with healthier smoking and drinking patterns among Danish twins, and that education may reduce the distribution of risky behaviors by altering environmental influences.

3.3 DPEP Literature

A number of studies evaluate the impact of the District Primary Education Program (DPEP) on educational attainment and related socioeconomic outcomes in India. Khanna (2023) provides a very comprehensive evaluation of the DPEP program, exploiting the female literacy threshold in a fuzzy regression discontinuity design. Using census, school infrastructure and household survey data, he finds that DPEP significantly increased years of schooling and primary school

completion among cohorts exposed to DPEP. These educational gains further translated into higher earnings, although the estimated returns to education are partly reduced by general equilibrium effects in local labor markets. Roodman (2025) questions the robustness of these findings. More specifically, he shows that including four districts near the treatment cutoff that were omitted from Khanna (2023)'s dataset significantly reduces the impacts on both schooling and wages. He also points out that methodological adjustments, including clustering and alternative cutoff specifications, further increase standard errors. This suggests that the magnitude of DPEP's effects as estimated particularly by Khanna (2023) could be sensitive to specification and sample choices.

Earlier studies primarily focus on the educational impacts of the program. Azam and Saing (2017) examine education effects of DPEP using a DID framework and find increased primary school participation with slightly larger effects for girls in terms of attending and completing primary school. The results also show that although the program improved educational access for SC/ST groups, effects were larger for non-SC/ST groups. Jalan and Glinskaya (2015), however, use Propensity Score Matching and report a negative effect on school attendance for girls aged 6-10 but a positive effect for 11-13 year olds, suggesting the program encouraged previously out-of-school girls to re-enroll at older ages.

More recent work has examined broader social outcomes of the program. Agarwal et al. (2023) evaluate program effects on marital and post-marital well-being outcomes. They find that age at marriage decreases but post-marriage well-being in terms of violence, household wealth and use of contraception improves. Similarly, Agarwal et al. (2024) examine the impact of DPEP on domestic violence against women in India using a fuzzy RDD and data from NFHS-4. They find that the program increased women's education by around 0.9 years on average and led to substantial reductions in the probability of women experiencing domestic violence in treated districts. Looking at the mechanisms, their results suggest that education improved gender attitudes, reduced justification of violence, improved partner quality and access to information. Educated women are also more likely to seek help from formal authorities and tend to marry wealthier partners with more progressive gender beliefs.

Finally, Sunder (2020) examines the intergenerational impact of DPEP recipients and finds that women who benefitted from the program were healthier, had enhanced bargaining power and invested more in their children's education compared to women in non-DPEP districts. He also finds that female beneficiaries transfer these benefits to their children, who perform better in school, are more likely to be enrolled and achieve smoother progression through grades. No such effect is found for families where only the father benefited.

4 Data

The study combines information from the 1991 Population Census with survey data from the National Family Health Survey (NFHS), specifically round 4 (2015-16). The NFHS individual recode provides comprehensive data for women aged 15-49 and is the primary data source for the analysis. To examine whether program effects differ by gender, I also use the men's recode from NFHS-4, which provides data for men aged 15-54 on a subset of outcomes.

Between 1991 and the NFHS-4 survey year, there were a substantial number of changes in district boundaries - several new districts were formed by splitting older ones and some were merged together into larger districts. The number of districts in India in 1991 was 452 but by 2011 the count increased to 640. This creates discrepancies between district definitions at the time of program implementation and the survey year used to assess long-term outcomes. To ensure geographic consistency across datasets, I harmonise district identifiers using the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG). SHRUG provides location codes - called SHRIDs - that are consistent across the 1991 and 2011 Census boundaries, and pre-built crosswalk files that link these SHRIDs to both 1991 and 2011 district identifiers. I use these crosswalks to bridge the two boundary definitions. First, I attach relevant 1991 Census variables - female population, female literates, and female population aged 0-6 - to SHRUG sub-district units using the SHRUG-to-1991 Census crosswalk. Second, I link these units to 2011 district identifiers using the SHRUG-to-2011 Census crosswalk, restricting to matched locations and collapsing to the 2011 district level. Third, 2011 district names are attached using SHRUG's location names file. The 1991 female literacy rate is constructed as the share of literate females among those aged 7 and above, expressed as a percentage. The resulting district-level dataset is merged with the NFHS-4 dataset based on state and district name, with naming conventions manually reconciled to ensure consistency with Census naming. This harmonisation ensures that all datasets used in the final analysis are consistently aligned to the 2011 district boundaries, while preserving the mapping required for determining DPEP eligibility.[1]

District-level information on DPEP implementation is obtained from Azam and Saing (2017), who provide a phase-wise classification of DPEP districts based on official government records. I use this data to construct a binary treatment indicator and a categorical variable indicating implementation phase in each district. Post-2001 districts created by administrative bifurcation inherit their parent district's DPEP status. Union territories are excluded from the analysis as DPEP was not implemented there, and Jammu and Kashmir is excluded for the same reason along with geopolitical complexity. The final analysis sample covers 589 districts across 29 states, comprising 259 DPEP districts and 330 non-DPEP districts.

[1] NFHS-4 uses 2011 district boundaries according to DHS.

NFHS is a nationally representative household survey conducted by the Ministry of Health and Family Welfare that collects detailed information on adult health and health-related behaviours. My main analysis relies on the women's questionnaire (individual recode), which provides comprehensive data for women aged 15–49. Educational outcomes include total years of schooling, an indicator for being literate, and an indicator for completing primary schooling. For substance use behaviors, I examine alcohol consumption and tobacco use. Alcohol consumption is measured as a binary indicator for any alcohol use. Smoking is defined as an indicator for current use of any tobacco product including cigarettes, bidis, or smokeless tobacco, constructed from self-reported responses in the NFHS questionnaire. Objective health outcomes include hypertension, overweight and underweight status, hemoglobin levels, height(in cm), weight(in kg) and diabetes. Hypertension is defined using the average of multiple blood pressure readings and coded according to the World Health Organisation (WHO) and Demographic and Health Survey (DHS) definition. Overweight and Underweight status are constructed from measured height and weight using body mass index (BMI ≥ 25 and BMI ≤ 18.5 respectively). Hemoglobin levels are obtained from blood samples and I use both raw and altitude-adjusted measures. Diabetes is defined using measured blood glucose levels based on standard WHO thresholds for random (non-fasting) readings.

I also examine program and downstream effects on the male sample to assess gender differences. The male sample analysis is more limited in outcomes due to differences between the women's and men's questionnaires. Education outcomes - years of schooling, literacy, and primary completion - are identical across both samples. Health outcomes are more limited for men as BMI and hemoglobin measures are not collected for male respondents. Substance use outcomes for men include alcohol consumption and tobacco use.

Since panel data outcomes for the same individuals or repeated cross-sections covering these outcomes across pre- and post-program years are not available, I use cohort-based variation in program exposure within NFHS-4. The analysis uses data from NFHS-4 aggregated to thedistrict × age × birth year level for both the female and male samples, with cell-level means weighted by cell population size in all regressions. This three-way aggregation is necessary to resolve an ambiguity in treatment assignment caused by the survey's design. Since NFHS-4 was conducted across both 2015 and 2016, collapsing solely by birth year creates cells containing respondents of different ages depending on their interview year. This generates a non-binary treatment definition for the boundary cohort cells that split the sample into treatment and control groups. Collapsing by district × age × birth year resolves this by ensuring all women within a cell share the same age at interview and treatment status. This design closely follows the DiD implemented by Azam and Saing (2017). The treatment cohort consists of women aged 27–34 at the time of NFHS-4, corresponding to those who were approximately 6–13 years old at the time of DPEP's 1994 launch and therefore of primary school age during program implementation.

The control cohort consists of women aged 35–44, who were 14–23 years old at launch and too old to have directly benefited from primary school interventions. This age-based cohort definition follows the approach used by Azam and Saing (2017), Khanna (2023), and Agarwal et al. (2024). Cohorts too old to have changed their schooling decisions are used as placebo comparisons in falsification tests. Validation of these cohort definitions is discussed in Section 5.

Outcome Categories and Variables

Category	**Variables**
Panel A: Female Sample	
Education (DiD)	Years of Schooling, Primary Completion, Literacy
Substance Use (IV)	Alcohol Consumption, Partner Alcohol Consumption, Smoking
Adult Health (IV)	Diabetes, Hypertension, Overweight, Underweight, BMI, Hemoglobin, Height (cm), Weight (kg)
Panel B: Male Sample	
Education (DiD)	Years of Schooling, Primary Completion, Literacy
Substance Use (IV)	Alcohol Consumption, Smoking
Adult Health (IV)	Diabetes, Hypertension

Notes: DiD = Difference-in-Differences. IV = Instrumental Variables, instrumenting years of schooling with the DiD treatment interaction. Male sample excludes BMI and Hemoglobin due to data unavailability in the NFHS-4 mens recode.

Table 1: Summary Statistics by Age Cohort and DPEP Status

	Young (ages 6–13 in 1994)				Old (ages 14–23 in 1994)			
	DPEP	Non-DPEP	Diff	p-val	DPEP	Non-DPEP	Diff	p-val
Panel A: Female Sample								
Years of Education	5.519	7.418	1.899	0.000	3.704	5.692	1.988	0.000
Literate	0.498	0.673	0.175	0.000	0.341	0.532	0.191	0.000
Primary Completion	0.090	0.070	-0.020	0.000	0.077	0.078	0.001	0.289
Urban	0.240	0.346	0.106	0.000	0.236	0.352	0.116	0.000
Scheduled Caste	0.871	0.779	-0.092	0.000	0.875	0.788	-0.087	0.000
Scheduled Tribe	0.099	0.183	0.085	0.000	0.095	0.174	0.079	0.000
Muslim	0.131	0.088	-0.043	0.000	0.126	0.084	-0.042	0.000
Panel B: Male Sample								
Years of Education	8.285	9.002	0.717	0.000	7.021	8.070	1.049	0.000
Literate	0.755	0.822	0.068	0.000	0.659	0.749	0.090	0.000
Primary Completion	0.079	0.064	-0.015	0.000	0.085	0.072	-0.014	0.000
Urban	0.256	0.370	0.114	0.000	0.265	0.361	0.095	0.000
Scheduled Caste	0.857	0.800	-0.058	0.000	0.856	0.795	-0.061	0.000
Scheduled Tribe	0.108	0.164	0.056	0.000	0.107	0.169	0.062	0.000
Muslim	0.125	0.086	-0.040	0.000	0.123	0.078	-0.045	0.000

Notes: Summary statistics by age cohort and DPEP district status. Young cohort: ages 6–13 in 1994 (ages 27–34 at survey). Old cohort: ages 14–23 in 1994 (ages 35–44 at survey). Difference = DPEP minus Non-DPEP. p-value from a two-sample t-test of equality of means. DPEP district status assigned following Azam and Saing (2017).
Source: NFHS-4 individual and men's recodes (2015–16).

5 Empirical Strategy

5.1 Difference-in-Differences

My main identification strategy compares education and health outcomes for women of primary school age during the DPEP rollout to those who were too old to benefit, across DPEP and non-DPEP districts. The focus on females as my main sample is because DPEP was designed to target districts with a female literacy rate lower than the national average in an attempt to bridge the gender gap in education. However, since the program was a school construction program, there is high possibility of spillover effects to the male sample as well and I present the results for men as a heterogeneity analysis rather than a primary contribution.

The treatment cohort consists of women aged 27–34 at the time of the NFHS-4 survey (2015-16), corresponding to ages 6–13 in 1994 when DPEP was launched. The standard primary school age window in India consists of ages 6–11. However, following the previous DPEP literature Azam and Saing (2017), Agarwal et al. (2024), I include individuals upto 13 years of age to account for late admission into school and re-enrollment both of which were common in the DPEP time period. It is also important to note that individuals aged 1–5 in 1994 could have also benefited from DPEP but this cohort is excluded as they would have also heavily benefited from the Sarva Shiksha Abhiyan (SSA) Program. The control cohort consists of women aged 35–44 at survey, corresponding to ages 14–23 in 1994, who were too old to have benefited from DPEP primary school interventions. More precisely, the first difference exploits cohort variation (between young / exposed and old / non-exposed individuals) and the second difference exploits geographic variation created by DPEP targeting. The DiD coefficient therefore captures the additional educational and health gains accrued by women who were of primary school age in districts that received DPEP.

District treatment status is assigned using the district-phase mapping from Azam and Saing (2017), which identifies all districts that received DPEP funding across phases. A district is classified as treated if it received DPEP funding in any phase, and untreated otherwise.[2]

The main specification is:

$$Y_{dct} = \alpha + \beta(\text{Young}_c \times \text{DPEP}_d) + \gamma X_{dct} + \delta_d + \lambda_t + \rho_s \times t + \varepsilon_{dct} \tag{1}$$

where Y_{dct} is the outcome for district d, age group c, and birth year t, Young_c is an indicator equal to one for the treatment cohort (ages 27–34 at survey), DPEP_d is an indicator equal to

[2]The analysis is conducted separately for women and men. The male sample uses the NFHS-4 mens recode and follows an identical identification strategy, with male-specific outcome variables as described in section 4 .

one for districts that received DPEP funding, X_{dct} is a vector of cell-level controls including urban share, SC/ST share, and Muslim share, δ_d are district fixed effects, λ_t are birth year fixed effects, and $\rho_s \times t$ are state-specific linear cohort trends following Azam and Saing (2017). All regressions are weighted by cell population size. The coefficient of interest is β, which captures the differential change in outcomes for the treatment cohort in DPEP districts relative to the control cohort and relative to non-DPEP districts. Standard errors are clustered at the district level to account for within-district correlation in outcomes across cohorts.

Although DPEP was introduced in a staggered fashion across districts, this study pools all potentially treated cohorts without explicitly accounting for variation in calendar-time adoption. This approach follows the majority of existing literature on the program and reflects the limited availability of data from the period of implementation. I draw some support for my identification strategy from Azam and Saing (2017), who conduct an age at exposure analysis for each age group and find effects for cohorts considered here. Replicating that specification is not feasible here because the aggregated dataset used reduces effective sample size by too much.

The control variables in the specification are included for the following reasons. DPEP districts were predominantly rural and lower income and so without controlling for urban status, any urban-rural composition differences between the set of DPEP and non-DPEP districts could confound treatment effect. DPEP explicitly targeted the SC/ST subgroup as they face significant structural barriers to education and social discrimination. Without this control, the treatment effect would partly reflect caste composition differences across treated and untreated districts which could inflate DPEP impact. Historically in India, Muslim women have had lower educational attainment driven by cultural, economic and institutional factors. Muslim population shares vary across districts and could correlate with DPEP targeting through the literacy criterion and so omitting this could bias the estimate.

District and birth year fixed effects are included to absorb all time-invariant district characteristics and national cohort trends respectively. This is done to ensure the estimate identifies within-district variation across cohorts and is not confounded by regional and cohort specific differences.

State-specific linear trends are added because even after district and birth year FE, states with more DPEP districts may have different educational trajectories before program implementation. For example, states that invested more in education could both be more likely to have DPEP districts and to show faster improvement between cohorts. State-specific trends allow each state to follow its own linear trajectory over time. Identification then comes from deviations around these state-level trends - that is, whether districts exposed to DPEP improved more than expected given their state's general trajectory. This relaxes the parallel trends assumption to requiring parallel trends conditional on each state's linear time path (Angrist and Pischke, 2009), which

is particularly important given that DPEP targeting was based on 1991 female literacy rates, which varied systematically across states.

Migration behavior is a concern because the data only allows me to observe and control for current place of residence. However, Topalova (2010) and Munshi and Rosenzweig (2009, 2016) document that this was a period in India with very low migration rates. Deshingkar and Anderson (2004) show that rural-urban migration is much lower in India than other countries. Many studies on India ignore migration, as the numbers are low (Das Gupta (1987); Foster and Rosenzweig (1996); Anderson (2005); Duflo and Pande (2007); Banerjee et al. (2008)). Lastly, Khanna (2023) also incorporates migration decisions in his model and fails to detect a migration response empirically. I also present results from a robustness check excluding the urban status control and find consistent results.

The coefficient of interest is β, which captures the differential change in outcomes for the treatment cohort in DPEP districts relative to the control cohort and relative to non-DPEP districts. Standard errors are clustered at the district level to account for within-district correlation in outcomes across cohorts.

5.1.1 Identification

The main identifying assumption for the DiD estimate is the parallel trends assumption. In the context of this study, it means that in the absence of DPEP, treated and control cohorts would have followed similar (parallel) educational trajectories across DPEP and non-DPEP districts. While this assumption is not directly testable because the same young cohort who were not exposed to the program are not observed, I provide the following supporting evidence:

First, I conduct a falsification test by estimating my main specification using a fake potential treatment group aged 15–19 in 1994 (*Fake Young*) and a control group aged 20–24 in 1994. Both these groups are older than primary school age and hence did not benefit from DPEP.

The following equation is estimated for the falsification

$$Y_{dct} = \alpha_1 + \beta_1(\text{FakeYoung}_c \times \text{DPEP}_d) + \gamma_1 X_c + \delta_d + \lambda_t + \rho_s \times t + \varepsilon_{dct} \tag{2}$$

The specification is similar to 1 except *FakeYoung* is an an indicator equal to one for women aged 36–40 at survey (aged 15–19 in 1994), with the fake control group consisting of women aged 41-45 (aged 20–24 in 1994). Under parallel trends, the coefficient of the fake treatment interaction should be statistically indistinguishable from zero. I find no significant effects for any education outcome in the falsification test supporting the identifying assumption (see Table

2).

Second, I present event study evidence to validate my cohort selection and reinforce parallel trends following Azam and Saing (2017). The event study estimates the interaction of DPEP district indicator with age in 1994 dummies, with age 23 as the reference category using :

$$Y_{idt} = \alpha + \sum_{k=5}^{22} \delta_k (\mathbf{1}[\text{age}_{i,1994} = k] \times \text{DPEP}_d) + \sum_{k=5}^{22} \mu_k \mathbf{1}[\text{age}_{i,1994} = k] + \gamma X_i + \delta_d + \lambda_t + \rho_s \times t + \varepsilon_{idt} \tag{3}$$

where δ_k is the coefficient on the interaction of the DPEP indicator with a dummy for being age k in 1994, and μ_k captures the main effect of cohort k. The reference category is age 23 in 1994, the oldest cohort in my main sample who were surely too old to benefit from DPEP. If the parallel trends assumption holds, δ_k should be close to zero for cohorts aged 14 and above in 1994, who were too old for primary school at DPEP launch. Figures 2 and 3 present the event study results for years of education and primary completion respectively.

Table 2: Falsification Test

	Years of Education	Literate	Primary Complete
	(1)	(2)	(3)
DPEP × Fake Young	−0.017	0.000	0.005
	(0.055)	(0.005)	(0.003)
Observations	10,423	10,423	10,423
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Controls	Yes	Yes	Yes
R^2	0.744	0.720	0.236

Notes: Robust standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Falsification test using fake treatment group aged 36–40 in 2011 (aged 15–19 in 1994) and fake control group aged 41–44 (aged 20–23 in 1994). Neither group was of primary school age during DPEP implementation. Observations weighted by district-age-birth year cell population. Controls include urban share, SC/ST share, and Muslim share.
Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

Figures 2 and 3 plot the event study results for the female sample. For years of education, coefficients are close to zero for ages 14–22 in 1994, consistent with the parallel trends assumption. Post-treatment coefficients are positive for ages 5–9 in 1994, although statistically significant

only for selected age groups. For ages 10–13, no clear effects are visible, but this likely reflects imprecision rather than a true null effect given the birth year-level aggregation, resulting in wide confidence intervals. The primary completion indicator shows a cleaner pattern where pre-treatment coefficients are statistically indistinguishable from zero throughout, while post-treatment coefficients trend upward for younger cohorts, with the largest effects concentrated among those aged 5–13 in 1994. This monotonically increasing exposure gradient lends support to my choice of treatment and control cohorts.

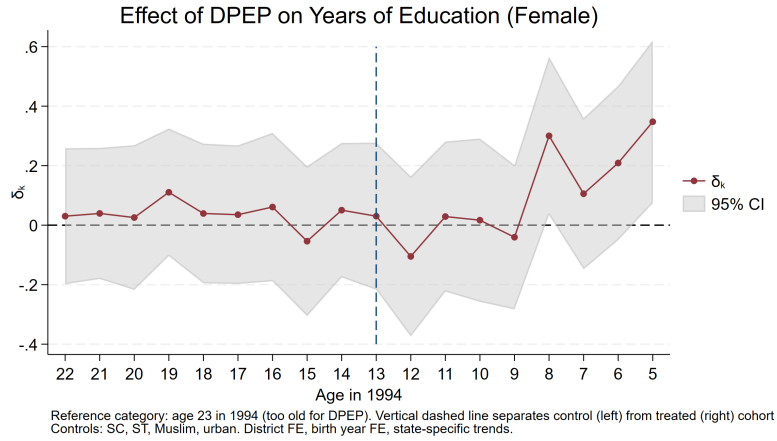

Figure 2: Event Study: Effect of DPEP on Years of Education - Women

Notes: δ_k refers to the coefficient of the interaction term of the DPEP program with age k. See equation (3) in main text for details. The estimation controls for district fixed effects, birth year fixed effects, state-specific linear trends, and indicators for urban residence, scheduled castes, scheduled tribes, and Muslims. Robust standard errors clustered at district level. Shaded area represents 95% confidence intervals.

Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

A potential threat to identification is the overlapping rollout of the SSA program launched in 2001-02. This was a follow-on education program and targeted several of the same districts as DPEP. To address this, I conduct a robustness check where I exclude phase 4 districts which are the ones most likely to have experienced significant SSA overlap. I find that the education results are robust to this exclusion, but the instruments get weaker in Section 7.

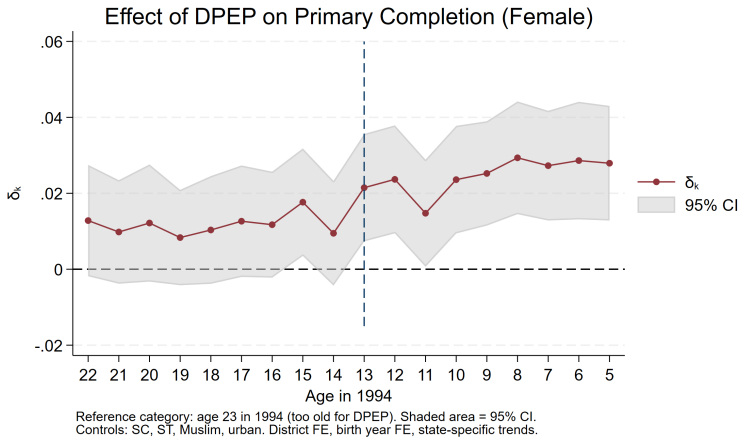

Figure 3: Event Study: Effect of DPEP on Primary School Completion - Women

Notes: δ_k refers to the coefficient of the interaction term of the DPEP program with age k. See equation (3) in main text for details. The estimation controls for district fixed effects, birth year fixed effects, state-specific linear trends, and indicators for urban residence, scheduled castes, scheduled tribes, and Muslims. Robust standard errors clustered at district level. Shaded area represents 95% confidence intervals.

Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

5.2 Instrumental Variables

To estimate the causal effect of education on health behaviors and biomarker outcomes, I use the DiD treatment interaction ($\text{Young}_c \times \text{DPEP}_d$) as an instrument for years of schooling, following Khanna (2023) and Azam and Saing (2017). All outcome variables are expressed as cell-level means and regressions are weighted by cell population size.

The first stage regression is:

$$S_{dct} = \alpha + \pi(\text{Young}_c \times \text{DPEP}_d) + \gamma X_{dct} + \delta_d + \lambda_t + \rho_s \times t + \varepsilon_{dct} \tag{4}$$

where S_{dct} is average years of schooling in cell dct. The second stage uses predicted years of schooling $\hat{S}_{dct}$ to estimate the effect on health and substance use outcomes:

$$H_{dct} = \alpha + \tau \hat{S}_{dct} + \gamma X_{dct} + \delta_d + \lambda_t + \rho_s \times t + \varepsilon_{dct} \tag{5}$$

where H_{dct} is a cell-level mean health outcome and τ identifies the Local Average Treatment Effect (LATE) of an additional year of education on health outcomes for women induced into more schooling by DPEP exposure. The same district, birth year, and state-specific trend fixed effects are maintained as in the main DiD specification.

The exclusion restriction for the IV requires that DPEP affects health outcomes only through its

effect on years of schooling (education), conditional on the fixed effects and controls included. This is plausible given DPEP's primary focus was on improving school enrollment and educational outcomes with no direct health intervention components. This is unlike the SSA program which integrated the provision of sanitation and meals as part of a holistic approach to primary education. One remaining concern is the Mid-Day-Meal Scheme which was implemented nationally from 1995 and provided cooked meals to primary school children. However, since the scheme was implemented across all districts (both DPEP and non-DPEP), any direct nutritional effects do not differentially impact treatment and control districts. The DiD design therefore ensures the IV estimates are robust to the overlap of the Mid-Day-Meal Scheme.

Instrument strength or relevence is assessed using the Kleibergen-Paap rk Wald F-statistic, which is the appropriate weak instrument test under clustering (Stock and Yogo, 2005). The Kleibergen-Paap rk LM statistic is used to test for underidentification. Given that the instrument is just-identified - one instrument for one endogenous variable - the Hansen J overidentification test is not applicable. First stage results and instrument strength statistics are reported and interpreted alongside the IV estimates in Section 6.

6 Results

6.1 Education Outcomes

Table 3 presents the main DiD estimates for female education outcomes. DPEP exposure generates significant improvements across most education measures. Women in the treatment cohort residing in DPEP districts completed 0.173 additional years of schooling relative to the control cohort in non-DPEP districts, significant at the 1% level. Literacy rates increased by 2.2 percentage points and primary completion rates by 1.2 percentage points, both significant at the 1% level. These estimates are comparable to Azam and Saing (2017), who find that DPEP increased the probability of attending and completing primary school by approximately 2 percentage points and years of schooling by 0.16 years. The small difference between my estimates and those of Azam and Saing (2017) can be attributed to the fact that their specification includes wealth quintiles as controls, which could attenuate the estimates given the strong link between education and income.

These estimates are, however, substantially smaller than those reported in RDD-based studies. Khanna (2023) and Agarwal et al. (2024) find effects approaching a full additional year of schooling using regression discontinuity designs that exploit the female literacy threshold used for DPEP targeting. There are two main concerns with these larger estimates. First, Roodman (2025) notes that RDD-based inference in this setting is sensitive to the choice of clustering, and

that standard errors clustered at the district level yield substantially wider confidence intervals. Second, the enforcement of the literacy cutoff was weak. Figure 4 plots the probability of DPEP treatment against the centered 1991 female literacy rate. The figure replicates Agarwal et al. (2024) and reveals substantial non-compliance with the eligibility threshold (compliance is around 25 percent). This means a large share of districts above the cutoff received DPEP funding, while many eligible districts below the cutoff did not. This weak compliance undermines the credibility of the RDD design and widespread non-compliance can cause RDD estimates to conflate effect of crossing the threshold with actual program receipt. The DiD approach adopted in this paper is robust to this concern, as identification exploits cohort variation within districts rather than discontinuous treatment assignment at the threshold. The summary statistics in The summary statistics in Table 1 show that educational attainment increases across both DPEP and non-DPEP districts, suggesting that the DiD identifies a modest effect after accounting for broader trends in education unrelated to the program.

Table 3: DiD Estimates: Female Education Outcomes

	Years Edu	Literate	Primary
	(1)	(2)	(3)
DPEP × Young	0.173***	0.022***	0.012***
	(0.051)	(0.005)	(0.003)
Observations	22079	22079	22079
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean	5.779	0.529	0.076
R-squared	0.786	0.752	0.220

Notes: Robust standard errors clustered at district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Treatment: Ages 27–34 at survey (fully exposed). Control: Ages 35–44 at survey. Observations weighted by district-age-birth year cell population. Controls: urban share, SC/ST share, Muslim share.

Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

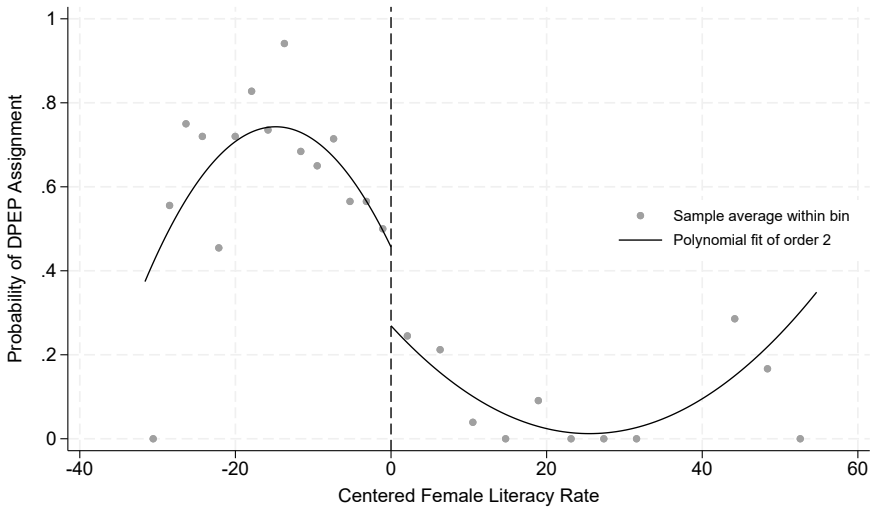

Figure 4: Probability of DPEP Treatment by Centered Female Literacy Rate (1991)

Notes: Each point represents the sample average probability of DPEP treatment within a bin of the centered 1991 female literacy rate. The solid line shows a second-order polynomial fit. The dashed vertical line at zero indicates the eligibility threshold. Replicates Agarwal et al. (2024), Figure 1.

6.2 Health Outcomes

Tables 5 and 4 present the IV estimates of the effect of years of schooling on health biomarker outcomes and substance use behaviors respectively, instrumenting years of education with the DiD treatment interaction. The Kleibergen-Paap rk Wald F-statistic across main specifications exceeds the Stock-Yogo 15% critical value of 8.96 but falls marginally below the 10% threshold of 16.38, and the Kleibergen-Paap rk LM underidentification test strongly rejects the null of underidentification ($p < 0.001$), confirming instrument relevance. Results should therefore be interpreted with appropriate caution regarding instrument strength.

Table 5 presents the IV estimates for biomarker outcomes. An additional year of schooling increases BMI by 0.435 units, significant at the 5% level, and increases the likelihood of diabetes by 1.3 percentage points, significant at the 10% level. Overweight and underweight indicators are very small in magnitude and are not significant at conventional levels. In Panel B, both hemoglobin measures show no significant effects, suggesting education does not meaningfully impact iron deficiencies in this sample. Weight and height are insignificant with weight showing a negative point estimate while height shows a positive one. This pattern of results where BMI shows a significant change but both height and weight do not is puzzling but likely reflects a feature of using an aggregated dataset. Cell-level mean BMI is computed from individual-level BMI values prior to collapsing and thus captures within-cell variation in the weight-to-height ratio that is not easily recoverable from separately averaged weight and height measures. The BMI finding is thus treated as the primary measure of body composition, with weight and

height serving as supplementary decomposition evidence. Following this, the height (in cm) and weight (in kg) variables are also not considered in Robustness Checks and Heterogeneity Analysis.

Looking at substance use behaviors in Table 4, an additional year of schooling does not generate any significant improvements in alcohol consumption, smoking likelihood or likelihood of partner consuming alcohol. This result is expected of the female sample because alcohol consumption and tobacco use among women in India was extremely low at baseline to start with (Neufeld et al., 2005). The program thus in this setting, did not have much scope to generate improvement across these outcomes.

Table 4: Female Sample - Substance Use Behaviour

	Drinks Alcohol	Partner Drinks	Smokes
	(1)	(2)	(3)
Years of schooling (women)	-0.004	-0.018	-0.001
	(0.007)	(0.060)	(0.002)
Observations	22079	16372	22079
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean	0.031	0.340	0.004
KP F-stat	15.343	13.369	15.343
CD F-stat	28.542	22.380	28.542
R-squared	0.010	0.006	-0.005

Notes: Robust standard errors clustered at district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Treatment: Ages 27–34 at survey. Control: Ages 35–44 at survey. Endogenous variable: Average years of education. Instrument: DPEP × Young. Observations weighted by district-age-birth year cell population. Controls: urban share, SC/ST share, Muslim share. KP F-stat = Kleibergen-Paap F-statistic. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individual's recode data (2015–16)

Table 5: IV Estimates: Biomarker Health Outcomes

	Hypertensive	Diabetic	BMI	Overweight	Underweight
	(1)	(2)	(3)	(4)	(5)
Panel A: Hypertension, Diabetes and BMI					
Years of Schooling	0.020	0.013*	0.435**	0.031	0.002
	(0.018)	(0.008)	(0.220)	(0.022)	(0.017)
Observations	22068	22076	22070	22070	22070
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean	0.137	0.026	22.582	0.248	0.165
KP F-stat	11.323	11.314	11.299	11.299	11.299
CD F-stat	21.121	21.091	21.069	21.069	21.069
R-squared	-0.077	-0.142	-0.007	0.016	0.003

	Hemoglobin	Hemoglobin Adj.	Weight (kg)	Height (cm)
	(6)	(7)	(8)	(9)
Panel B: Hemoglobin, Weight and Height				
Years of Schooling	2.146	-1.396	-4.579	1.903
	(3.958)	(0.888)	(4.313)	(3.032)
Observations	22076	22068	22079	22076
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean	128.778	117.092	66.673	159.774
KP F-stat	11.314	11.310	11.309	11.314
CD F-stat	21.091	21.094	21.077	21.091
R-squared	-0.002	-0.212	-0.048	-0.004

Notes: Robust standard errors clustered at district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Treatment: Ages 27–34 at survey. Control: Ages 35–44 at survey. Endogenous variable: average years of education instrumented by DPEP × Young. Observations weighted by district-age-birth year cell population. Controls: urban share, SC/ST share, Muslim share. KP F-stat = Kleibergen-Paap rk Wald F-statistic, the appropriate weak instrument test under clustering. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

7 Robustness Checks

This section presents a series of robustness checks to assess the sensitivity of the main IV estimates to alternative specifications and potential threats to identification. I test robustness to five main concerns:

1. Contamination of the treatment effect by the concurrent rollout of the SSA program in Phase 4 districts;
2. Sensitivity of estimates to the inclusion of state-specific linear cohort trends;
3. Sensitivity to the inclusion of urban share as a control variable;
4. The choice of endogenous variable - replacing years of schooling with primary school completion likelihood as an alternative measure of the education impact of DPEP;
5. Multiple hypothesis testing across outcomes, addressed using the Romano-Wolf stepdown correction.

For all robustness checks, I present IV estimates in this section and first-stage education estimates in Appendix C1. I report results only for the key outcomes - BMI, diabetes, hypertension, alcohol consumption, and smoking - with the full set of outcomes available in Appendix C2. IV results for the specification using Primary Completion Rate as the instrument are provided in Appendix C3. Romano-Wolf corrected p-values for the main sample DiD and IV estimation are reported in Appendix C4 and confirm that results are robust to multiple hypothesis testing concerns. Lastly, I also present a narrower cohort specification for the first-stage effect to check sensitivity of education effects to treatment cohort choice.

Table 6 presents IV estimates excluding Phase 4 DPEP districts which are most likely to have experienced concurrent exposure to the Sarva Shiksha Abhiyan (SSA) program. The first-stage results in Appendix C1 show that DPEP exposure continues to increase years of schooling by 0.155 years, significant at the 5% level, suggesting the education effect is not driven by SSA contamination. However, the Kleibergen-Paap F-statistic falls to approximately 3 across all outcomes, indicating that the instrument loses substantial power after the Phase 4 exclusion. The second-stage IV estimates are therefore unreliable and should be treated as suggestive only.

Table 7 presents estimates from a specification that excludes state-specific linear cohort trends, and including only district and birth year fixed effects. This tests whether the main results are driven by the inclusion of state-specific trends rather than reflecting genuine program effects. The first-stage results for this specification given in Appendix C1 are slightly stronger than the main specification. This is consistent with the idea that state-specific trends absorb

some variation that is attributable to states with more DPEP districts having faster improving educational trajectories rather than clear program effects. The main results hold across both specifications and including state trends is the more conservative specification. The hypertension estimate in this specification should be interpreted with caution given the weak instrument, as IV estimates are more sensitive to instrument strength for outcomes where the reduced form effect is small.

Table 6: Robustness Check: SSA Exclusion - Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Years of Schooling	-0.012	0.002	0.005	0.016	0.240
	(0.019)	(0.005)	(0.034)	(0.015)	(0.383)
Observations	18522	18522	18512	18519	18514
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean	0.032	0.005	0.140	0.025	22.680
KP F-stat	3.116	3.116	3.129	3.118	3.114
CD F-stat	5.550	5.550	5.575	5.554	5.545
R-squared	-0.097	-0.038	-0.003	-0.221	0.073

Notes: Robust standard errors clustered at district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Endogenous variable: average years of education instrumented by DPEP × Young. Treatment: Ages 27–34 at survey. Control: Ages 35–44 at survey. Phase 4 districts excluded. KP F-stat = Kleibergen-Paap rk Wald F-statistic; below conventional weak instrument thresholds due to reduced sample.
Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

Table 8 presents estimates from a specification that does not control for urban residence. The first-stage results in Appendix C1 are stronger than the main specification, which is expected - by omitting the urban share control, the specification assigns equal weight to rural and urban variation in DPEP exposure, whereas the main specification accounts for the fact that urban areas have a higher educational baseline that could moderate the treatment effect. The IV estimates on the key health outcomes are consistent with the main specification, and the Kleibergen-Paap F-statistic comfortably exceeds the Stock-Yogo 10% critical value of 16.38 across all outcomes, confirming instrument strength in this alternative specification.

Table 7: Robustness Check: No State-Specific Trends - Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Years of Schooling	-0.002	0.004	0.072**	0.014*	0.604*
	(0.011)	(0.004)	(0.031)	(0.008)	(0.313)
Observations	22079	22079	22068	22076	22070
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	No	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes
Mean	0.031	0.004	0.137	0.026	22.582
KP F-stat	8.238	8.238	8.250	8.247	8.235
CD F-stat	26.682	26.682	26.736	26.714	26.680
R-squared	0.012	-0.105	-1.086	-0.191	-0.200

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Specification excludes state-specific linear cohort trends. Endogenous variable: average years of education instrumented by DPEP × Young. Treatment: Ages 27–34 at survey. Control: Ages 35–44 at survey. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

Table 9 presents IV estimates using primary school completion likelihood as the endogenous variable in place of years of schooling. The first-stage results in Appendix C1 confirm that DPEP exposure significantly increases primary completion rates, and the Kleibergen-Paap F-statistic for the IV estimation is substantially higher in this specification than in the main specification, confirming instrument relevance. The direction and significance of the health effects are consistent with the main specification - diabetes remains marginally significant and BMI shows a positive and significant effect. However, the point estimate on BMI is considerably larger and should be carefully interpreted. Because the first-stage effect of DPEP on primary completion has a small point estimate, the IV scaling factor is large and could amplify the second-stage coefficients substantially. The results are therefore best interpreted as directionally consistent with the main specification rather than as precise estimates of the health returns to education.

Table 8: Robustness Check: No Urban Control - Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Years of Schooling	-0.007	0.000	0.017	0.011*	0.450**
	(0.007)	(0.002)	(0.014)	(0.006)	(0.175)
Observations	22079	22079	22068	22076	22070
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean	0.031	0.004	0.137	0.026	22.582
KP F-stat	16.415	16.415	16.408	16.420	16.381
CD F-stat	30.349	30.349	30.372	30.364	30.313
R-squared	-0.018	0.000	-0.054	-0.108	-0.018

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Specification excludes urban share as a control. Endogenous variable: average years of education instrumented by DPEP × Young. Treatment: Ages 27–34 at survey. Control: Ages 35–44 at survey. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

Table 10 presents estimates using a narrower cohort definition, restricting the treatment group to women aged 27–32 at survey (ages 6–11 in 1994) and the control group to women aged 35–41 (ages 14–20 in 1994). This specification sharpens the treatment window to cohorts with the clearest primary school age exposure to DPEP, excluding older treated cohorts who were at the upper boundary of the primary school age range at program launch. Literacy increases by 1.4 percentage points and primary completion by 1.4 percentage points, significant at the 5% and 1% levels respectively, and broadly consistent with the main specification. The years of schooling coefficient of 0.075 is positive but loses significance and magnitude relative to the main estimate of 0.173, which reflects reduction in sample size and consequent loss of statistical power for a continuous outcome variable. The stability of the binary education outcome estimates across the narrow and main specifications provides support for treatment effects not being sensitive to precise definition of cohort boundaries.

Table 9: Robustness Check: Primary Completion as IV - Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Primary Completion	-0.117	0.004	0.290	0.187*	6.205**
	(0.131)	(0.034)	(0.251)	(0.099)	(3.123)
Observations	22079	22079	22068	22076	22070
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean	0.031	0.004	0.137	0.026	22.582
KP F-stat	21.738	21.738	21.741	21.738	21.741
CD F-stat	35.618	35.618	35.606	35.615	35.609
R-squared	-0.020	0.000	-0.049	-0.091	-0.101

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Endogenous variable: primary school completion likelihood instrumented by DPEP × Young. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.
Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

Table 10: Robustness Check: Narrow Cohort Specification

	Years of Edu	Literate	Primary
	(1)	(2)	(3)
DPEP × Young (Narrow)	0.075	0.014**	0.014***
	(0.058)	(0.006)	(0.003)
Observations	294329	294329	294329
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
R-squared	0.279	0.226	0.021

Notes: Robust standard errors clustered at district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Narrow treatment group: Ages 26–32 (age 5–11 in 1994). Control group: Ages 35–41 (age 14–20 in 1994). Individual controls include urban residence, SC/ST status, and Muslim religion.
Source: Author's rendering of NFHS-4 individual's recode data (2015–16).

8 Heterogeneity Analysis

To assess whether DPEP effects are concentrated among historically disadvantaged subgroups that were targeted by the program and to assess spillover effects to male children as mentioned previously, I estimate the main DiD and IV specifications separately for these groups. DPEP was specifically designed to improve educational access for rural women and women belonging to Scheduled Castes and Scheduled Tribes (SC/ST) communities (Aggarwal (1998)). I also present results for urban and non-SC/ST women to verify if the program was successful in generating stronger effects in the intended subgroups.

8.1 Male Sample

For the male sample, the event studies in Appendix B present ambiguous event-study plots. Pre-treatment coefficients are flat and close to zero, supporting parallel trends. However, there is no clear effect on the post-treatment coefficients unlike what was observed for women. This suggests that while the parallel trends assumption holds for men, the male DiD estimates lack causal strength - in the absence of a clear gradient, it is difficult to attribute the average effect to DPEP exposure.

Table 11 presents the DiD estimates for male education outcomes. The point estimates suggest stronger effects for men than for women in terms of years of schooling (0.572) while primary completion effects are insignificant. This pattern is consistent with Azam and Saing (2017), who find higher impacts on primary school completion for women, while years of schooling completed are marginally higher for men. However, as discussed previously, these effects are to be taken as suggestive evidence as the individual age cohorts do not display a strong gradient in effects. Moreover, the higher years of education for men could reflect differential school dropout behavior between the genders - female students are likely to dropout earlier as they face more constraints to educational uptake.

Table 12 presents the IV second stage estimates for male health outcomes. An additional year of schooling decreases tobacco use by 2.2 percentage points and increases likelihood of being diabetic by 1 percentage point for men, significant at the 10% level. No significant effects are found for hypertension, or alcohol consumption. The stronger tobacco effect for men relative to women once again reflects baseline gender differences in tobacco use rates (Neufeld et al., 2005). Education-induced shifts in behavioral norms and health awareness therefore have greater scope to reduce consumption among more educated men.

Table 11: DiD Estimates: Male Education Outcomes

	Years of Edu	Literate	Primary
	(1)	(2)	(3)
DPEP × Young	0.572***	0.039***	-0.008
	(0.092)	(0.008)	(0.005)
Observations	48076	48076	48076
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
R-squared	0.161	0.115	0.029
Mean	8.267	0.759	0.072

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Treatment group: Ages 27–34 at survey. Control group: Ages 35–44 at survey. Individual controls include urban residence, SC/ST status, and Muslim religion.
Source: Author's rendering of NFHS-4 men's recode data (2015–16).

Table 12: IV Estimates: Male Health Outcomes

	Hypertensive	Diabetic	Drinks Alcohol	Smokes
	(1)	(2)	(3)	(4)
Years of Education	0.000	0.010*	-0.016	-0.022*
	(0.014)	(0.006)	(0.016)	(0.013)
Observations	46779	47248	48076	48076
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Mean	0.196	0.031	0.399	0.174

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. IV second stage: years of education instrumented by DPEP × Young. Treatment group: Ages 27–34 at survey. Control group: Ages 35–44 at survey. Individual controls include urban residence, SC/ST status, and Muslim religion.
Source: Author's rendering of NFHS-4 men's recode data (2015–16).

8.2 Rural and Urban Subsamples

Table 13 presents the IV estimates for health outcomes for the rural subsample. The first-stage estimates in Appendix D1 show that DPEP exposure increases years of schooling by 0.118 years among rural women, significant at the 5% level, and literacy and primary completion rates are roughly comparable to the main specification. The IV estimates are not significant for any of the outcomes examined and also suffer from weak instrument issues. Table 14 show the IV estimates of the urban subsample and the first-stage results are presented in Appendix D1. The education effects are stronger for the urban subpopulation with years of education improving by 0.259 years significant at the 1% level. However, it is also important to note that primary education completion shows no effect for this group. Together these results suggest that while DPEP was successful in increasing primary completion rates in rural areas as intended, this did not translate to long-term education gains or health gains and that these regions may need a more comprehensive policy. Due to lack of documentation about the number of schools built within rural neighborhoods under DPEP, it is also possible that the intervention was not granular enough to address heterogeneous within-district barriers for rural women. Supply-side improvements in school access are not sufficient on their own for sustained educational attainment for women in India.

Table 13: Rural Subsample: Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Years of Schooling	-0.010	0.000	0.023	0.012	-0.097
	(0.020)	(0.005)	(0.037)	(0.014)	(0.363)
Observations	21500	21500	21485	21492	21485
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean Outcome	0.034	0.004	0.132	0.019	22.035
KP F-stat	2.961	2.961	2.982	2.957	2.959
CD F-stat	5.731	5.731	5.775	5.726	5.729
R-squared	-0.040	-0.000	-0.083	-0.134	-0.051

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Rural subsample. Endogenous variable: average years of education instrumented by DPEP × Young. Treatment group: Ages 27–34 at survey. Control group: Ages 35–44 at survey. Controls: SC/ST share, Muslim share. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individuals recode data (2015–16).

Table 14: Urban Subsample: Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Years of Schooling	-0.000	0.001	0.004	0.009	0.430
	(0.005)	(0.002)	(0.022)	(0.011)	(0.270)
Observations	18390	18390	18280	18348	18275
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean Outcome	0.018	0.004	0.145	0.041	24.019
KP F-stat	9.121	9.121	9.052	8.982	9.286
CD F-stat	11.444	11.444	11.357	11.280	11.633
R-squared	0.008	-0.011	-0.004	-0.038	-0.055

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Urban subsample. Endogenous variable: average years of education instrumented by DPEP × Young. Treatment group: Ages 27–34 at survey. Control group: Ages 35–44 at survey. Controls: SC/ST share, Muslim share. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.

8.3 SC/ST and Non-SC/ST Subsamples

The first-stage education effects in Appendix D1 for the SC/ST subpopulation are notably stronger than both the full sample and rural subgroup results. DPEP exposure increases years of schooling by 0.184 years among SC/ST women, significant at the 1% level and larger in magnitude than the full sample estimate of 0.173. Literacy increases by 2.3 percentage points and primary completion by 1.2 percentage points, both significant at the 1% level. The downstream effect on health outcomes in Table 15 for this group are also broadly consistent with the main specification with the exception of the diabetes indicator losing precision. The stronger effects for SC/ST women are consistent with DPEP's explicit targeting of historically marginalized communities. SC/ST women faced the greater baseline barriers to education - lower enrollment rates, higher dropout rates, and greater social and economic constraints on schooling - providing the most scope for program-induced improvements.

Table 15: SC/ST Subsample: Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Years of Schooling	-0.011	0.000	0.018	0.011	0.437**
	(0.009)	(0.002)	(0.017)	(0.007)	(0.211)
Observations	22048	22048	22037	22043	22038
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean Outcome	0.032	0.004	0.137	0.026	22.574
KP F-stat	12.089	12.089	12.080	12.094	12.067
CD F-stat	23.068	23.068	23.075	23.080	23.045
R-squared	-0.067	-0.000	-0.059	-0.101	-0.009

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. SC/ST subsample. Endogenous variable: average years of education instrumented by DPEP × Young. Treatment group: Ages 27–34 at survey. Control group: Ages 35–44 at survey. Controls: Muslim share, urban share. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.
Source: Author's rendering of NFHS-4 individuals recode data (2015–16).

No significant effects are observed for the non-SC/ST subsample on either education or health outcomes, and the near-zero Kleibergen-Paap F-statistic is a result of the absence of a meaningful first stage for this group. This is consistent with DPEP's explicit targeting criterion - districts were selected based on low female literacy rates, which are strongly correlated with high SC/ST population shares, implying that non-SC/ST women in the same districts faced lower baseline barriers to schooling and were less affected at the margin by program-induced supply improvements.

These findings suggest that DPEP was broadly successful in reaching its intended beneficiaries, generating the largest educational gains among the most disadvantaged subpopulations, consistent with Azam and Saing (2017) who find that program effects are concentrated among groups with the lowest pre-program educational attainment. For the rural subpopulation, the weaker years of schooling effect relative to the binary education outcomes suggests that while DPEP improved access to and completion of primary schooling, translating these gains into additional years of education may require complementary interventions addressing demand-side barriers such as transport costs, early marriage, and household opportunity costs that a school

construction and teacher training program alone cannot resolve.

Table 16: Non-SC/ST Subsample: Health Outcomes

	Drinks Alcohol	Smokes	Hypertensive	Diabetic	BMI
	(1)	(2)	(3)	(4)	(5)
Years of Schooling	0.206	0.025	0.026	0.775	0.003
	(1.635)	(0.210)	(0.720)	(9.703)	(6.348)
Observations	4698	4698	4640	4673	4644
District FE	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean Outcome	0.023	0.003	0.159	0.036	22.944
KP F-stat	0.016	0.016	0.017	0.006	0.016
CD F-stat	0.017	0.017	0.018	0.007	0.018
R-squared	-47.956	-5.014	-0.102	-360.747	0.034

Notes: Robust standard errors clustered at district level in parantheses. * p<0.10, ** p<0.05, *** p<0.01. Non-SC/ST subsample. Endogenous variable: average years of education instrumented by DPEP × Young. Treatment group: Ages 27–34 at survey. Control group: Ages 35–44 at survey. KP F-stat = Kleibergen-Paap rk Wald F-statistic; instrument is essentially irrelevant for this subsample consistent with DPEP targeting SC/ST districts. *Source:* Author's rendering of NFHS-4 individuals recode data (2015–16).

9 Discussion

The findings of this study are relevant to India's ongoing demographic and epidemiological transition, characterised by a substantial increase in the prevalence of chronic diseases. Even though prior literature has tried to establish a causal link between health and education, the estimates of this study reveal that this relationship is not straightforwardly positive in the Indian context. While DPEP-induced education gains represent meaningful progress – particularly for SC/ST women – the downstream health effects are more complex and require nuanced policy targeting. Rather than reducing the risk of non-communicable diseases, additional years of education manifest as higher body mass index scores and marginally higher diabetes risk. The positive IV estimates for BMI and diabetes among women require careful contextual interpretation. Given India's stage of epidemiological transition and the fact that DPEP targeted disproportionately low-literacy, low-income districts where baseline nutritional status was also likely lower, these findings can have two interpretations. The first is a nutrition transition narrative: as these women move up the socioeconomic ladder, they enter an environment with higher caloric intake and are no longer engaged in physically demanding livelihoods. Griffiths and Bentley (2001) documented that Indian women in higher socioeconomic groups and urban areas are more likely to be overweight, suggesting that as education raises living standards, women move into the overweight risk category. The second is a malnutrition reduction narrative: given India's historically high rates of undernutrition among women, an increase in BMI among previously underweight women may reflect a shift toward healthier weight rather than overnutrition. Saikia and Debbarma (2020) note that India faces a double burden of malnutrition, with underweight and overweight coexisting even within the same districts and demographic groups.

The other outcomes assessed provide some insight into which interpretation is more likely. Diabetes prevalence also increases, although less significant and this movement is directionally inconsistent with a pure malnutrition reduction story. If the BMI increase was driven largely by a shift away from underweight, we would not expect a corresponding rise in diabetes likelihood. The tendency of South Asian populations to develop diabetes at lower BMI thresholds than Western populations further supports this narrative (Nanditha et al., 2016). The overweight, underweight indicators and weight and height components of BMI are individually uninformative in this setting, as their cell-level means are unable to recover the individual-level BMI gradient due to the aggregation structure of the data. Given the relative imprecision of the diabetes estimate and the inability to separately identify rural and urban health effects causally, some uncertainty remains, and India's documented double burden suggests both channels may operate simultaneously within this observed transition. The findings thus suggest that education expansion and NCD prevention need to be treated as complementary policy priorities rather than substitutes in the Indian context.

Taken together, the full pattern of results are suggestive of which mechanisms are and are not likely at work in this setting. The findings on health outcomes highlight the limited relevance of the canonical Grossman (1972) framework in low-income settings. The Grossman model suggests that education improves health by increasing the efficiency of health production - more educated individuals make better health decisions, have higher health literacy, and are better able to navigate healthcare systems. In this context, the framework would predict that DPEP-induced education gains reduce risky health behaviors and improve biomarker outcomes. The evidence, however, is not so black and white. The pattern of results in this study - rising BMI and diabetes risk alongside null substance use effects - points to a distinctive feature of the education-health relationship in the Indian setting: where behavioral risks are already suppressed by social norms rather than health literacy, education operates on health primarily through the living standards and consumption channel. This suggests that the Grossman framework may have limited predictive power in contexts where cultural constraints on substance use behavior suppress effects from the health literacy mechanism. More broadly, it underscores why the health returns to education documented in the developed country literature cannot be assumed to travel to low-income settings where the baseline behavioral environment is fundamentally different.

9.1 Policy Implications

The findings of this study carry several policy implications for education and health investments in developing countries, particularly in the Indian context.

First, the results provide support for continued investment in targeted primary education programs in low-literacy districts. DPEP generated significant and robust improvements in female literacy and primary completion, particularly among SC/ST women who face significant structural barriers to education. The stronger effects for historically marginalised subgroups suggest that well-targeted education programs can be effective tools for reducing inequality in human capital accumulation, and that the district-level targeting criterion used by DPEP - female literacy rates below the national average - was broadly successful in directing resources toward the most disadvantaged populations.

Second, the gender-specific pathways from education to health documented here have important implications for program design. For women, education operates through a broad set of channels - household bargaining, marriage market outcomes, fertility decisions and health literacy - that are not equally available to men. The null health effects for men despite positive education gains are consistent with the idea that male health is more strongly determined by occupational exposure and labor market outcomes, channels that a primary education program may be insufficient to impact meaningfully. Policy designs that aim to leverage education for health improvements should therefore account for these gender-specific mechanisms rather than

assuming a uniform education-health gradient across men and women.

Third, the rural subgroup results point to the need for more granular, sub-district level implementation to translate district-level education investment into sustained schooling gains. The marginally weaker years of schooling effect for rural women suggests that supply-side improvements in school access are necessary but not sufficient - complementary investments in transport infrastructure and last-mile connectivity are needed to ensure that expanded school supply translates into actual attendance and completion, particularly for girls in dispersed rural communities (Muralidharan and Prakash, 2017). Demand-side constraints including household poverty, early marriage, and opportunity costs of schooling for girls in rural areas further compound these barriers and require targeted sub-district interventions (Aggarwal, 1998).

9.2 Limitations

The following limitations of this study are acknowledged. First, the identification strategy relies on a single cross-section of data, as panel data tracking the same individuals before and after DPEP implementation is not available. The cohort-based difference-in-differences design addresses this by exploiting variation across birth cohorts within districts, but the absence of pre-treatment outcome data means that the parallel trends assumption cannot be directly tested and must instead be supported by the falsification test and event study evidence presented in Section 5.

Second, the instrumental variables estimates should be interpreted with some caution regarding instrument strength. The Kleibergen-Paap rk Wald F-statistic across some main specifications exceeds the Stock-Yogo 15% maximal IV size critical value but falls marginally below the 10% threshold (Stock and Yogo, 2005), indicating that the instrument is not very strong by conventional standards. The Kleibergen-Paap rk LM underidentification test strongly rejects the null of underidentification ($p < 0.001$), confirming instrument relevance. The IV estimates should therefore be interpreted as a local average treatment effect for women induced into additional schooling by DPEP exposure.

Third, the concurrent rollout of the SSA in Phase 4 districts creates a potential source of contamination for the treatment effect. While the SSA exclusion robustness check shows that results are mostly unchanged for education after dropping Phase 4 districts, the IV estimations lose significant instrument strength in that restricted sample. To the extent that SSA contamination affects other DPEP phases by expanding schooling alongside DPEP, the estimated education gradient could be partially attributable to SSA rather than DPEP alone. This would mean that the IV estimates could reflect partially combined effect of both programs. The robustness of education results to dropping Phase 4 districts - where SSA overlap was most pronounced -

provides some reassurance, though the loss of instrument strength in that restricted sample means this check is imperfect. Residual SSA contamination therefore remains a caveat.

Fourth, the male results should be treated as suggestive. The male event study shows no clear monotonic gradient in education gains with age at DPEP exposure, unlike the clean pattern observed for women, suggesting that the male DiD estimates may not be as cleanly attributable to DPEP primary school exposure specifically. However, considering the female sample was the primary target of the program examined, this makes sense.

Finally, the external validity of the findings is limited to the type of districts targeted by DPEP - predominantly low-literacy, low-income, and rural. The effects documented here may not generalise to higher-literacy districts or to education interventions in more developed contexts, where the mechanisms linking education to health outcomes may operate differently.

10 Conclusion

This study estimates the causal effects of the District Primary Education Program on female educational attainment and adult health outcomes in India, using a difference-in-differences design exploiting variation in program exposure across birth cohorts and districts. Using data from the 1991 Census and NFHS-4, DPEP exposure led to significant improvements in female educational attainment - increasing years of schooling by 0.173 years, literacy by 2.2 percentage points, and primary completion by 1.2 percentage points - with effects concentrated among SC/ST women. Additional education induced by DPEP is associated with higher BMI and marginally higher diabetes risk among women, consistent with India's ongoing nutrition transition, while effects on substance use are null and attributed to the low baseline prevalence of these behaviours among Indian women rather than an absence of health literacy effects. Effects are strongest for SC/ST women, consistent with DPEP's targeting of historically marginalised communities, while the rural subgroup shows weaker and less precisely estimated schooling gains, suggesting that district-level implementation was insufficient to overcome the within-district barriers facing rural women.

The findings contribute to a growing literature on the long-run effects of education on health in developing countries, and provide the first causal evidence on this relationship in the Indian context. Primary education expansion is an effective tool for improving human capital among the most disadvantaged populations, but its downstream health effects are shaped by the epidemiological and socioeconomic context in which they operate and cannot be assumed to be identical to those documented in high-income and developed countries.

Four main directions stand out for future work. First, having panel data that follows the same individuals over time would make it possible to directly test whether pre-treatment trends were actually parallel, and would give cleaner estimates of how DPEP affected health outcomes specifically. Second, a better identification of the mechanisms that link education to health outcomes remains unanswered. More granular data on how DPEP affected intermediate outcomes such as health knowledge, preventive care utilisation, and occupational choice would help trace the pathway through which education exposure translates into health improvements. Third, this study focuses on non-communicable disease outcomes, and extending the analysis to infectious disease outcomes such as malaria, tuberculosis, HIV etc., would provide a more complete picture of how primary education affects adult health. Finally, data on precise proportion of DPEP-designated schools within districts would enable cleaner separation of DPEP's effects from concurrent programs such as SSA.

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Appendix

A. Sensitivity Analysis: Selection on Unobservables

To assess the robustness of the main estimates to selection on unobservables, I follow Oster (2019) and compute the degree of proportional selection (δ) required to drive treatment effect to zero using the *psacalc* command in Stata (Oster, 2013). The resulting bounds are reported in Table 17. For years of education, $\delta = 3.423$ indicates that selection on unobservables would need to be more than three times larger than selection on observables to explain the treatment effect. For literacy $\delta = 4.421$ which is an even stronger result. Both estimates comfortably exceed the threshold of $\delta > 1$. The negative δ for primary completion reflects that adding observable controls attenuates the treatment effect (equation with no controls shows a larger effect). Given DPEP targeted the exact subpopulations used as controls in the main specification, this negative estimate is expected. Taken together, the Oster bounds suggest that the main DiD estimates are unlikely to be driven by selection on unobservables providing confidence to causal interpretation of results.

Table 17: Oster Bounds: Selection on Unobservables

	Years of Edu	Literate	Primary
Coefficients			
Uncontrolled $\tilde{\beta}$	0.228	0.026	0.012
Controlled $\dot{\beta}$	0.173	0.022	0.0116
R-squared			
Uncontrolled $\tilde{R}$	0.203	0.180	0.018
Controlled $\dot{R}$	0.277	0.225	0.020
$R_{max} = 1.3\dot{R}$	0.360	0.293	0.026
Bound			
δ	3.423	4.421	-10.925

Notes: Following Oster (2019), δ reports the degree of selection on unobservables relative to observables required to drive the treatment effect to zero. R_{max} is set to $1.3\dot{R}$ following Oster's recommendation, capped at 0.9. $\delta > 1$ indicates the result is robust to unobservable selection. Uncontrolled model includes treatment and fixed effects only. Controlled model adds SC/ST status, Muslim religion and urban residence. Both models absorb district, birth year and state-specific linear trend fixed effects.
Source: Author's rendering of NFHS-4 individuals recode data (2015–16).

B. Identification Tests - Male Sample

Table 18: Falsification Test: Male Education Outcomes

	Years of Edu	Literate	Primary
	(1)	(2)	(3)
DPEP × Fake Young	0.114	0.007	-0.004
	(0.151)	(0.014)	(0.009)
Observations	19495	19495	19495
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes
R-squared	0.169	0.126	0.049

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Falsification test: Fake treatment group ages 36-40. Fake control: ages 41-44. Neither group was of primary school age during DPEP implementation. Individual controls include urban residence, SC/ST status, and Muslim religion.

Source: Author's rendering of NFHS-4 men's recode data (2015–16).

The falsification test assigns fake treatment to cohorts aged 36-40 relative to a fake control of cohorts aged 41-45 at the aggregate level. Neither group was of primary school age during DPEP implementation. No significant effects are found across any education outcome, consistent with the individual-level falsification results.

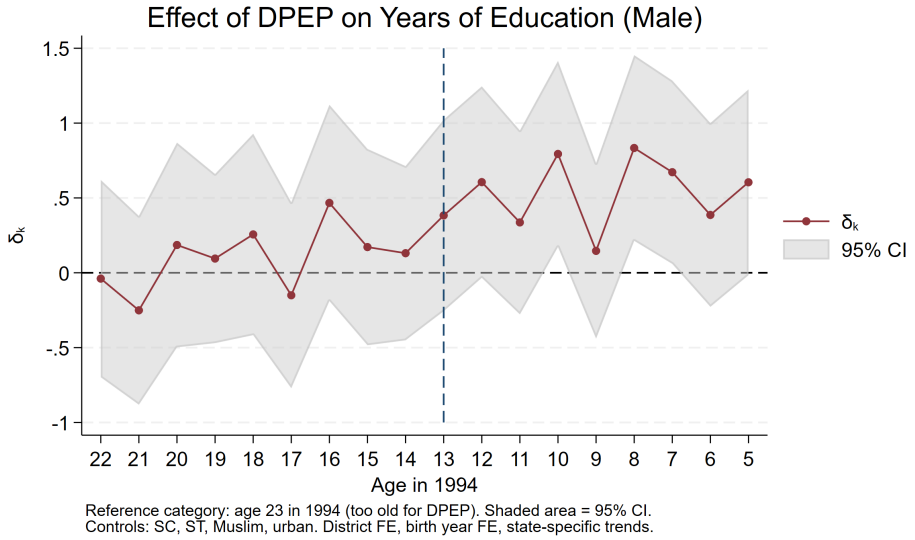

Figure 5: Event Study: Effect of DPEP on Years of Schooling - Men

Notes: δ_k refers to the coefficient of the interaction term of the DPEP program with age k. See equation (3) in main text for further details. The estimation controls for district fixed effects, birth year fixed effects, state-specific linear trends, and indicators for urban residence, scheduled castes, scheduled tribes, and Muslims. Robust standard errors clustered at district level. Shaded area represents 95% confidence intervals.

Source: Author's rendering of NFHS-4 men's recode data (2015–16).

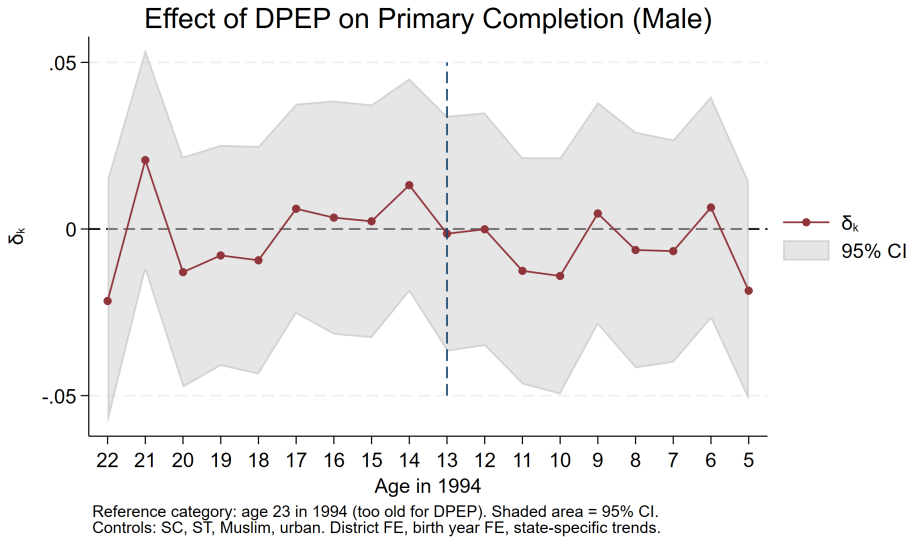

Figure 6: Event Study: Effect of DPEP on Primary School Completion - Men

Notes: δ_k refers to the coefficient of the interaction term of the DPEP program with age k. See equation (3) in main text for further details. The estimation controls for district fixed effects, birth year fixed effects, state-specific linear trends, and indicators for urban residence, scheduled castes, scheduled tribes, and Muslims. Robust standard errors clustered at district level. Shaded area represents 95% confidence intervals.

Source: Author's rendering of NFHS-4 men's recode data (2015–16).

C. Robustness Checks

C1. Robustness Checks - First Stage Results

Table 19: Robustness Checks: First Stage Results

	Years Edu	Literate	Primary
	(1)	(2)	(3)
Panel A: SSA Exclusion			
DPEP × Young	0.147**	0.021***	0.015***
	(0.061)	(0.006)	(0.003)
Observations	18484	18484	18484
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Controls	Yes	Yes	Yes
R-squared	0.783	0.752	0.225
Panel B: No State-Specific Trends			
DPEP × Young	0.180***	0.026***	0.021***
	(0.063)	(0.006)	(0.003)
Observations	22079	22079	22079
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	No	No	No
Controls	Yes	Yes	Yes
Mean Outcome	5.779	0.529	0.076
R-squared	0.776	0.744	0.200
Panel C: No Urban Control			
DPEP × Young	0.219***	0.025***	0.012***
	(0.054)	(0.005)	(0.003)
Observations	22079	22079	22079
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean Outcome	5.779	0.529	0.076
R-squared	0.760	0.734	0.217

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Panel A excludes Phase 4 districts to address potential SSA contamination. Panel B excludes state-specific linear cohort trends. Panel C excludes urban share as a control. Treatment: Ages 27–34 at survey. Control: Ages 35–44. Observations weighted by district-age-birth year cell population. Controls: urban share, SC/ST share, Muslim share (except Panel C which excludes urban share).

Source: Author's rendering of NFHS-4 individuals recode data (2015–16).

C2. Robustness IV - Complete Outcomes

All tables in this section report IV estimates using average years of education as the endogenous variable instrumented by DPEP × Young, unless otherwise noted. Standard errors are clustered at the district level. All specifications include district fixed effects, birth year fixed effects, and state-specific linear cohort trends unless noted. Observations are weighted by district-age-birth year cell population. Controls include urban share, SC/ST share, and Muslim share.

Table 20: Robustness Check: No State-Specific Trends - IV Estimates (Additional Outcomes)

	Partner Drinks	Overweight	Underweight	Hemoglobin Adj.
	(1)	(2)	(3)	(4)
Years of Schooling	0.012	0.067**	0.014	-2.395**
	(0.058)	(0.033)	(0.018)	(1.113)
Observations	16372	22070	22070	22068
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.340	0.248	0.165	117.092
KP F-stat	9.392	8.235	8.235	8.241
CD F-stat	26.160	26.680	26.680	26.707
R-squared	-0.004	-0.374	-0.075	-0.605

Notes: Robust standard errors clustered at district level in parantheses. * p<0.10, ** p<0.05, *** p<0.01. Specification excludes state-specific linear cohort trends. Endogenous variable: average years of education instrumented by DPEP × Young. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individuals recode data (2015–16).

Table 21: Robustness Checks: IV Estimates (Additional Outcomes)

	Partner Drinks	Overweight	Underweight	Hemoglobin Adj.
	(1)	(2)	(3)	(4)
Panel A: SSA Exclusion				
Years of Schooling	0.135	0.011	0.050	-1.625
	(0.176)	(0.039)	(0.046)	(1.843)
Observations	13806	18514	18514	18512
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.334	0.254	0.158	117.382
KP F-stat	2.603	3.114	3.114	3.120
CD F-stat	4.169	5.545	5.545	5.558
R-squared	-0.320	0.052	-0.646	-0.287
Panel B: No Urban Control				
Years of Schooling	0.025	0.033*	-0.003	-1.065
	(0.056)	(0.018)	(0.013)	(0.673)
Observations	16372	22070	22070	22068
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.340	0.248	0.165	117.092
KP F-stat	15.358	16.381	16.381	16.393
CD F-stat	25.784	30.313	30.313	30.340
R-squared	-0.019	0.004	0.014	-0.145

Notes: Robust standard errors clustered at district level in parantheses. * p<0.10, ** p<0.05, *** p<0.01. Endogenous variable: average years of education instrumented by DPEP × Young. Panel A excludes Phase 4 districts. Panel B excludes state-specific linear cohort trends. Panel C excludes urban share as a control. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individuals recode data (2015–16)

C3. Primary Completion Rate as IV

Table 22: Robustness Check: Primary Completion as IV - IV Estimates (Additional Outcomes)

	Partner Drinks	Overweight	Underweight	Hemoglobin Adj.
	(1)	(2)	(3)	(4)
Primary Completion	0.461	0.442	0.031	-19.917*
	(0.870)	(0.304)	(0.243)	(11.383)
Observations	16372	22070	22070	22068
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.340	0.248	0.165	117.092
KP F-stat	27.485	21.741	21.741	21.738
CD F-stat	39.404	35.609	35.609	35.602
R-squared	-0.003	-0.045	0.011	-0.106

Notes: Robust standard errors clustered at district level in parantheses. * p<0.10, ** p<0.05, *** p<0.01. Endogenous variable: primary school completion likelihood instrumented by DPEP × Young. Controls: urban share, SC/ST share, Muslim share. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.

Source: Author's rendering of NFHS-4 individuals recode data (2015–16)

C4. Romano Wolf Multiple Hypothesis Correction

A key concern when estimating the effect of education on multiple health outcomes simultaneously is the risk of spurious findings driven by multiple hypothesis testing. When a large number of outcomes are tested, the probability of rejecting at least one true null hypothesis by chance (type 1 error) increases substantially, inflating the familywise error rate (FWER).

To address this, I apply the Romano-Wolf stepdown correction (Romano and Wolf, 2005, 2016), which controls the FWER while accounting for the dependence structure across outcomes through a bootstrap resampling procedure. The correction is implemented using the `rwolf2` command in Stata (Clarke et al., 2020), with 1,000 bootstrap replications and a fixed seed of 12345 to ensure replicability. Table 23 report the Romano-Wolf corrected p-values alongside the original model p-values for the three groups of outcomes considered - education, health biomarkers and substance use behaviors. The Romano-Wolf corrected p-values remain close to

the main specification p-values across all outcomes, indicating that the main results are robust to multiple hypothesis testing concerns.

Table 23: Romano-Wolf Multiple Hypothesis Correction

Outcome	Model p-value	Romano-Wolf p-value
Panel A: Education Outcomes		
Years of Education	0.0020	0.0010
Literate	0.0001	0.0010
Primary Complete	0.0004	0.0010
Panel B: Behaviour Outcomes		
Drinks Alcohol	0.7253	0.6164
Partner Drinks	0.5225	0.5724
Smokes	0.3860	0.4555
Panel C: Biomarker Outcomes		
Hypertensive	0.0729	0.0430
Diabetic	0.4374	0.6074
BMI	0.0893	0.0549
Overweight	0.1477	0.1179
Underweight	0.6883	0.9041
Hemoglobin	0.9940	0.9950
Hemoglobin (adj.)	0.0376	0.0310
Weight	0.9247	0.9910
Height	0.3865	0.5155

Notes: Romano-Wolf stepdown p-values correcting for multiple hypothesis testing within each family of outcomes. Panel A reports results for the first-stage, where the independent variable is DPEP × Young. Panels B and C report IV estimates where the independent variable is years of education, instrumented by DPEP × Young. All models include district and birth year fixed effects, state-specific linear trends, and controls for urban share, SC/ST share, and Muslim share. Standard errors clustered at district level. 1,000 bootstrap replications with seed 12345.

C5. Covariate Balance Test Results

Baseline differences visible in Table 1 could raise concerns about comparability of DPEP and non-DPEP districts that are not captured by the set of fixed effects and controls included. The following Regression Discontinuity validation style Covariate Balance Test provides support to their comparability.

Table 24: Balance Test Covariates

	Unweighted					Weighted				
	(1) Sex Ratio (F/M×1000)	(2) SC/ST Share	(3) Age 0–6 Share	(4) Literate Share	(5) Worker Part.	(6) Sex Ratio (F/M×1000)	(7) SC/ST Share	(8) Age 0–6 Share	(9) Literate Share	(10) Worker Part.
RD_Estimate	16.487	0.0249	-0.0069	0.0124	0.0148	26.196	0.0979	-0.0026	0.0087	0.0126
	(17.167)	(0.0738)	(0.0089)	(0.0090)	(0.0255)	(18.392)	(0.0845)	(0.0065)	(0.0094)	(0.0211)
Observations	452	452	452	452	452	452	452	452	452	452

Standard errors in parentheses

Each column reports RD estimates for predetermined district-level covariates.

D. Heterogeneity Analysis

D1. Heterogeneity - First Stage Results

Table 25: Heterogeneity: First Stage Results

	Years Edu	Literate	Primary
	(1)	(2)	(3)
Panel A: Rural Subsample			
DPEP × Young	0.118**	0.014**	0.014***
	(0.058)	(0.006)	(0.003)
Observations	21500	21500	21500
Mean Outcome	4.844	0.461	0.083
R-squared	0.755	0.710	0.200
Panel B: Urban Subsample			
DPEP × Young	0.259***	0.028***	0.005
	(0.086)	(0.007)	(0.004)
Observations	18390	18390	18390
Mean Outcome	8.243	0.714	0.063
R-squared	0.429	0.390	0.099
Panel C: SC/ST Subsample			
DPEP × Young	0.184***	0.023***	0.012***
	(0.053)	(0.005)	(0.003)
Observations	22048	22048	22048
R-squared	0.782	0.749	0.214
Panel D: Non-SC/ST Subsample			
DPEP × Young	0.024	-0.001	0.014
	(0.193)	(0.020)	(0.009)
Observations	4698	4698	4698
Mean Outcome	6.359	0.587	0.060
R-squared	0.463	0.400	0.142
District FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. All specifications include district fixed effects, birth year fixed effects, and state-specific linear cohort trends. Observations weighted by district-age-birth year cell population. Controls for Panels A and B: SC/ST share, Muslim share. Controls for Panels C and D: urban share, Muslim share.

Source: Author's rendering of NFHS-4 individuals recode data (2015–16)

D2. Heterogeneity IV - Complete Outcomes

I. Rural and Urban Subsamples

Table 26: Heterogeneity: IV Estimates (Additional Outcomes) - Rural and Urban

	Partner Drinks	Overweight	Underweight	Hemoglobin Adj.
	(1)	(2)	(3)	(4)
Panel A: Rural				
Years of Schooling	0.066	-0.022	0.011	-3.114
	(0.144)	(0.038)	(0.041)	(2.363)
Observations	13752	21485	21485	21483
Mean Outcome	0.357	0.202	0.190	116.825
KP F-stat	2.980	2.959	2.959	2.978
CD F-stat	5.150	5.729	5.729	5.769
R-squared	-0.081	-0.133	-0.050	-0.804
Panel B: Urban				
Years of Schooling	0.051	0.022	0.002	-0.196
	(0.070)	(0.026)	(0.017)	(0.927)
Observations	7977	18275	18275	18255
Mean Outcome	0.296	0.366	0.096	117.795
KP F-stat	9.079	9.285	9.285	9.161
CD F-stat	11.159	11.633	11.633	11.490
R-squared	-0.108	0.015	-0.009	-0.010
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Endogenous variable: average years of education instrumented by DPEP × Young. Controls: SC/ST share, Muslim share. KP F-stat = Kleibergen-Paap rk Wald F-statistic. CD F-stat = Cragg-Donald F-statistic.
Source: Author's rendering of NFHS-4 individuals recode data (2015–16)

II. SC/ST and non-SC/ST Subsample

Table 27: Heterogeneity: IV Estimates (Additional Outcomes) - SC/ST and Non-SC/ST

	Partner Drinks	Overweight	Underweight	Hemoglobin Adj.
	(1)	(2)	(3)	(4)
Panel A: SC/ST				
Years of Schooling	0.047	0.028	0.000	-1.356
	(0.069)	(0.021)	(0.016)	(0.848)
Observations	16150	22038	22038	22036
Mean Outcome	0.344	0.247	0.165	117.053
KP F-stat	11.336	12.067	12.067	12.079
CD F-stat	19.596	23.045	23.045	23.071
R-squared	-0.048	0.028	0.010	-0.205
Panel B: Non-SC/ST				
Years of Schooling	-0.173	0.537	-0.039	-1.799
	(0.480)	(4.086)	(0.629)	(56.585)
Observations	947	4644	4644	4629
Mean Outcome	0.220	0.283	0.150	118.437
KP F-stat	0.177	0.016	0.016	0.005
CD F-stat	0.214	0.018	0.018	0.005
R-squared	-0.987	-29.958	-0.113	-0.307
District FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State-Specific Trends	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at district level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Endogenous variable: average years of education instrumented by DPEP × Young. Panel A controls: Muslim share, urban share. Panel B controls: Muslim share, urban share. KP F-stat = Kleibergen-Paap rk Wald F-statistic; instrument is essentially irrelevant for the Non-SC/ST subsample. CD F-stat = Cragg-Donald F-statistic.
Source: Author's rendering of NFHS-4 individuals recode data (2015–16)