

Improving primary school education in India: An impact assessment of DPEP-Phase I¹

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Abstract

The District Primary Education Program (DPEP) was launched by the Government of India (GOI) in 1994 with the objectives of providing primary school access for all children, reducing overall dropout rates and gaps in enrollments, dropouts and learning achievements across gender and social groups. To date, this has been the most intensive primary school education intervention by the central government both in terms of resources and planning. The GOI and its various international development partners (World Bank, ECU, DFID, UNICEF) have committed resources to the tune of US dollars 1.62 billion for the project. As of December 2001, the school system under DPEP covered 51.3 million children and 1.1 million teachers.

Has this massive resource mobilization in primary school education improved education outcome indicators? We evaluate Phase I of the DPEP, in terms of three specific outcome indicators: enrollment rates, completion rates and progression to higher levels of education. Program districts show a “small” improvement in outcome indicators. However, most of the benefit accrued to male children and primarily in the state of Madhya Pradesh where concurrent to the DPEP two substantial state level primary school programs were also initiated. Impacts on the scheduled caste group were also noticeable. Finally, contrary to the program’s goals, there has been virtually no impact on girl’s primary school education or on children belonging to the scheduled tribe.

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1 Introduction

Since independence, “education for all” children 14 years and younger has been the *mantra* of all political parties and governments in India. Several policy initiatives have been undertaken over the years by the central government to fulfill this objective.² These are in addition to initiatives taken by individual state governments.³ However, education outcome indicators for the overall population, and in particular, for disadvantaged groups like females, scheduled castes and scheduled tribes continue to be dismal. In the 1991 census, literacy rate among rural women was 30.6 while the national female literacy rate for India was 39.3. In some states, the female literacy rate in rural areas was as low as 11.6. Scheduled castes and tribes had even lower literacy rates, enrollment and achievement rates, and higher dropout rates than the general population.

It is against this backdrop that the Government of India launched a major primary school education initiative through the District Primary School Education Program (DPEP) in 1994. Under the scheme, different districts with poor educational indicators have been selected to receive financial assistance towards improvement of school infrastructure, teacher training, textbook improvement, etc.. The principal objectives of the DPEP are to provide access for all children to primary school or its equivalent non-formal education, to reduce overall dropout rates, and to reduce gaps in enrollments, dropouts, and learning achievements among gender and social groups.

The DPEP is a centrally sponsored scheme where 85 percent of the approved project cost within each district is borne by the central government, and the remainder by the state government. The central government's total contribution in the DPEP is funded in its entirety by international aid which includes an IDA loan from the World Bank, and grants from the European Union (for Madhya Pradesh), Government of Netherlands (for Gujarat),

² These schemes include Non-formal Education program (1979-80), Operation Blackboard for small rural schools (1986), Total Literacy Campaigns (1988), District Institutes of Education and Training (1988), Minimum Levels of Learning (1989).

³ Some of these schemes include *Shiksha Karmi* and *Lok Jumbish* schemes in Rajasthan, the Bihar education Project, Education Guarantee Scheme in Madhya Pradesh, Uttar Pradesh Basic Education Project (UPBEP), Andhra Pradesh Primary Education Project. In fact the UPBEP project was considered to be a pilot for the subsequent DPEP initiative. Under UPBEP, US\$165 million was provided under IDA terms to the Government of Uttar Pradesh.

Department of International Development (DFID) (for West Bengal and Andhra Pradesh), and United Nations International Children's Fund (UNICEF) (for Bihar).

To date, these international institutions have committed resources to the tune of Rs. 81.10 billion (\$1.62 billion) for the project of which the World Bank's commitment has been Rs. 51.07 billion (slightly over \$1 billion). More than 271 districts spread over 18 states have been brought under the coverage of the DPEP. The system covers 51.3 million children and 1.1 million teachers in 3,75,000 schools. More than 40,000 new school buildings have been constructed, about 66,000 works have been undertaken to provide ancillary facilities like drinking water, toilets, etc. Grass root level participation has been strengthened considerably with several thousands of village education committees and school management committees established under the auspices of the program. To date, 1 million teachers and 3 million community members have been trained under the program. All this makes the DPEP the most intensive intervention by the central government in India in primary school education, both in terms of resources and in terms of target population. (15th joint Review Mission Report, DPEP Bureau).

Given that the DPEP is perhaps the largest ever program of primary education development in the world, it is important to evaluate to what extent the program is achieving its stated objectives. In this paper, we intend to evaluate the first phase of the DPEP (DPEP-I) in terms of its stated objectives: (a) whether primary school attendance rates have increased since the inception of the project among all 6-13 year olds and for specific sub-populations (female children, and children from scheduled caste and tribes); (b) whether more children complete the primary school cycle now (1999-00) than earlier (1993-94); and (c) whether the program has encouraged students to remain in the schooling system beyond primary school.

The first phase of DPEP was started in 1994, where 42 districts (primarily in rural India) across the seven states of Assam, Haryana, Karnataka, Kerala, Madhya Pradesh, Maharashtra, and Tamil Nadu were chosen as DPEP districts. Program placement was not random but was primarily on the basis of low female literacy rates in 1991.⁴ As of date,

⁴ The DPEP guidelines indicate successful implementation of the Total Literacy Campaign (TLC) in the districts along with low female literacy rate as the two criteria used to choose districts. However, by 1994, TLC had been implemented in almost all districts in India.

DPEP Phase-I has been in place for more than 5 years and has thus covered at least one cycle of primary education among school-going children. Given this, it is an appropriate time to evaluate the project. Phases II and III of the DPEP were started in 1996-97 and 1997-98 respectively and Phase IV of the project has just started in 1999-00. Given that these phases have begun only a few years ago, it is too early to evaluate their program impacts and hence, our paper focuses on the impact evaluation of Phase I of the program.

The original DPEP guidelines put significant emphasis on research, monitoring and evaluation of the program. The Research, Evaluation and Studies Unit (RESU), which became functional from March 1995, compiled 255 research abstracts in primary education between 1994-2000 conducted at the national, state and district levels. Some of the nationwide studies include those by Pandey (2000), Aggarwal (2000a, 2000b), Menon (2001). A defining feature of all these studies is that they have focused primarily on changes in educational outcomes that have happened *within* DPEP districts. On this basis, these studies have by and large declared the DPEP to be a success in terms of its stated objectives.

However, the important point to note is that since independence, there has been a secular “improvement” in the various educational indicators in India.⁵ So, within any district selected under the DPEP, we would expect the primary school indicators to improve over time *even if the district did not* get DPEP assistance. Thus, in evaluating the DPEP, we have to determine the extent of improvements in primary school education in a DPEP district over and above the improvements that would have happened in the “natural course of events”. In other words, we have to determine the *net impact* of the program. If instead, we (like some of the studies mentioned above) measure the *gross* improvements in the program districts, we will probably be seriously overestimating its impact.

We use propensity score matching (PSM) methods to estimate the net impacts of the DPEP. PSM balances the distributions of observed covariates between a treatment group and a control group based on similarity of their predicted probabilities of being declared a project district (their “propensity scores”). The method does not require a parametric model linking

⁵ For example, literacy rates in India have steadily increased over time from 18.3 percent in 1952 to 52.2 percent in 1991 (World Bank, 1997).

program placement to outcomes, and thus allows estimation of mean impacts without arbitrary assumptions about functional forms and error distributions.

We combine data from the 1991 census with two rounds of the all-India household data from the National Sample Survey (Round 50,1993-94 and Round 55, 1999-2000) collected routinely by the National Sampling Survey organization (NSSO) Department of Statistics and Program Implementation, Government of India. We report the impacts for all the seven states where DPEP-I was started. We then report the impacts for the state of Madhya Pradesh separately. We do this because in Madhya Pradesh, two substantive state level primary school interventions -- Alternative Schooling (AS) and Education Guarantee Scheme (EGS) -- were started at around the same time as DPEP-I.

Our results show a “small” positive net impact of the DPEP on primary school attendance rates, stock of completed primary school education, and progression into higher levels of education beyond primary school. However, the impacts are not uniform. The more striking impacts are noticed for male children and for children living in the state of Madhya Pradesh. Our analysis clearly shows that the *net impact* of the program is substantially smaller than the *gross improvements*. Further, it matters how the net impact is calculated. A simple method to measure the net impact would be to compare the average outcomes in treatment districts with those in non-treatment districts. Aggarwal (2000) does precisely this and finds that between 1993-94 and 1996-97, the net impact of DPEP-I on enrollments was 5 percent, with it being as much as 16.8 percent in Assam. In contrast, our propensity score matching results indicate that at best, there was an impact of one percent on the attendance rates of primary school age children, and that too at the behest of significant improvements in Madhya Pradesh.

Apart from increasing access, and reducing dropouts in the overall population, an avowed aim of DPEP is to reduce gender and social disparity in access, retention and dropouts. Our results show that in this respect, the program’s performance has not been very successful. DPEP-I has had a small positive net impact on the three outcome indicators for the scheduled caste group, none or marginal net impact for the scheduled tribes, and most surprisingly, there has been virtually no impact on female children.

Our paper is organized as follows. In the next section we discuss the various aspects of the DPEP. In Section 3, we provide an intuitive description of the statistical methodology that we use in our analysis (we provide details about the methodology in a Technical Appendix), and Section 4 describes our data sources. Our results are presented in Section 5. Section 6 contains some concluding comments.

2 The DPEP Guidelines

The DPEP project was approved in early 1994 as a centrally sponsored intervention in primary school education in India. Under the first phase of the project, 42 districts spread over the seven states of Assam, Haryana, Kerala, Karnataka, Madhya Pradesh, Maharashtra and Tamil Nadu were chosen as project sites. At that time, it was envisaged that the project would subsequently cover *all districts with female literacy rates below the national average*.⁶ Today the project covers more than 240 districts spread over 18 states of India.

As Table 1 indicates, the choice of the initial 42 districts to a great extent satisfied the above criterion. With the exception of Kerala where no district has a female literacy rate below the national average, in all other states, all chosen districts had low female literacy rates. However, Table 1 also shows that the districts chosen were not necessarily the ones with the worst female literacy rates.⁷

Keeping the principles of the National Policy of Education (1986, updated in 1992) in mind, the charter of the DPEP sought to operationalize the following:

“... efforts would be made to develop district specific projects, with specific activities, clearly defined responsibilities, definite time-scheduled and specific targets. Each district project will be prepared within the majority strategy framework and will tailored to the specific needs and possibilities in the district. Apart from effective universalization of elementary education, the goals of each project would

⁶ Explicitly, two criteria had to be satisfied for a district to be chosen as a project district: low female literacy rate and districts where Total Literacy campaigns had been successfully implemented. However, the latter criterion was satisfied by nearly all districts in India at the start of the program.

⁷ In DPEP-I, in addition to the stated criteria, the policymakers implicitly chose to pick districts where they felt that there was considerable scope for the project to be successful (Pandey, 2000).

include the reduction of existing disparities in educational access, the provision of alternative systems of comparable standards to disadvantaged groups, a substantial improvement in the quality of schooling facilities, obtaining a genuine community involvement in the running of schools, and building up local level capacity to ensure effective decentralization of educational planning. That is to say, the overall goal of the project would be reconstruction of primary education as a whole in selected districts instead of piecemeal implementation of schemes. An integrated approach is more likely to achieve synergies among different program components". (*DPEP Principles, 1994*)

To achieve its stated goals, the program adopted measures to strengthen the capacities of national, state and district institutions for planning, management and evaluation of primary education. At the national level, the Ministry of Human Resource Development, Department of Education is responsible for overall control and management of the Program; it established a DPEP bureau headed by a Joint Secretary and six Directors/Deputy Secretaries. At the State level, the program is implemented through registered state level autonomous societies with the Chief Minister of the state as *ex-officio* chairperson. At the district and sub-district levels, program implementation plans are undertaken in consultation with District Project Implementation Committee, and the Block Project Implementation Committees. Action plans and budgets are developed at the district level with active participation from the community, NGOs, and teachers.

To get financial assistance under the program, states have to maintain their 1991-92 expenditure levels (in real terms) on elementary education. The state's 15 percent contribution to the DPEP cannot be included as part of these expenditures. Expenditures on civil works were restricted to 30% of the total (construction component 24% and management cost 6%). DPEP would not finance non-educational incentives like free uniforms, incentives for attendance, nutrition etc. except under special circumstances.

The DPEP recognized the role of local community initiatives in promoting enrollment, retention, achievement and school effectiveness. It encouraged the formation of Village Education Committees (VEC) and local bodies like Mother-Teacher Associations (MTA), which would in turn create awareness campaigns for providing the necessary institutional

infrastructure for increasing enrollment, retention, and for facilitating schools and Non-Formal Education (NFE) centers.

Another innovation of the DPEP was to provide for integrated education of disabled children. It proposed to involve the community to identify type, degree and extent of disabilities amongst the primary level age group. Necessary skills would be imparted to the parents and the disabled children. Teachers would be trained to detect disabilities, use aids and appliances, and implement individualized education plans.

The program emphasized on initiatives to strengthen educational planning and management capacities in the project states. This would involve setting up of separate State Institute of Educational Management and Training (SIEMAT) to augment state level structures like State Council of Educational Research and Training (SCERT). Training activities of teachers in private and aided schools, pre-primary teachers other than those under Integrated Child development Scheme (ICDS), VEC/MTA members and NGOs would also be financed under the project.

All the above initiatives were taken to (a) provide access for all children to primary school or its equivalent non-formal education, (b) to reduce overall dropout rates to less than 10 percent, and (c) to reduce gaps in enrollments, drop-outs and learning achievements among gender and social groups to less than 5 percent.

In this paper, our aim is limited. We intend to determine the net impact (between 1993/94 and 1999/2000) of the first phase of the DPEP on the three stated objectives described above. Ideally, we would like to know the net impact of the DPEP on the scholastic achievements of primary school students in the participating districts, and also the precise mechanics by which the DPEP affects these outcomes. However, answering these difficult questions must await further data availability and further research.

3 Methodology

We want to assess whether the DPEP has had a “net impact” on primary schooling outcomes. In principle, the net impact would be the difference between the observed outcomes in the

treatment districts and the (unobserved) outcomes in the same districts if there was no DPEP. In practice, of course, the exact counterfactual of a declared DPEP district is non-existent.

A simple method to get around the above problem would be to compare the average outcomes in treatment districts with those in non-treatment districts. However, program placement is not random and observed outcomes of the program may themselves be influenced by the selection criteria. In our case, there are non-program districts with high female literacy rates, and good educational facilities. To the extent that both these factors influence educational outcome indicators, the observed educational outcomes of the non-treatment districts, on average, do not measure the outcome indicators of the treatment districts in absence of the program. Therefore, differences in average outcomes across treatment and non-treatment districts do not isolate the impacts of the program on the treated. Such “selection bias” will generally arise in situations where there is a direct relationship between outcomes and choice of treatment districts.

There are several approaches used to deal with the selection bias problem. One could randomly pick the treatment districts so that the non-treatment districts are the true counterfactuals and there is no selection bias problem. In practice, however, such random experiment designs are not feasible especially in a large-scale program like the DPEP. Alternatively, one could use econometric techniques to “adjust out” the systematic differences between the non-experimental comparison group and the treatment group. Propensity score matching technique (the method we adopt in this paper) is one such econometric method where statistical models are used to generate a comparison group that is similar in all respects to the members in the participant group, except that they do not get the treatment.

A propensity score matching estimator pairs each program participant with an observably equivalent non-participant and then interprets the difference in their outcomes as the mean effect of the program on the treated. The probability of being selected as a project site – the propensity score function – is predicted as a function of a set of observed covariates that influence the program placement and outcomes, but are not influenced by outcomes themselves. The “no-treatment” district is declared a counterfactual for “treatment” district when the difference between the predicted probabilities is the minimum among the set of all

potential matches. The difference in outcome indicators between treatment district and its “matched” counterpart is the net impact of the program.⁸

In estimating the propensity score, female literacy rates would certainly be a determinant in our model. However, since we want to find a non-DPEP district that is similar to a DPEP district in all observed characteristics, we include other observed characteristics like population density, scheduled caste/scheduled tribe (SC/ST) concentration, female literacy rates for overall population and specifically for SC/ST population, proportion of people who are born in the enumeration area, housing indicators of the households (electricity, water, toilet, fuel usage, roofing material, floor material), village infrastructure characteristics (whether village has bank, communication systems, education facilities, etc.).

In our particular case, some additional issues need to be noted. First, since there are institutional differences in educational systems across states, we choose treatment and control districts from within the same state. Second, the DPEP project was implemented in a phased manner all-over India. DPEP-I was implemented in late 1994 and three years later, 40 additional districts in the same states were chosen as project sites under DPEP-II.⁹ Given that our two comparison periods are 1993-94 and 1999-00, we have to exclude the additional districts under DPEP-II from the set of potential candidates to construct the counterfactual; otherwise we will underestimate the impact of the program.¹⁰ Therefore, out of a total of 163 districts in the seven DPEP-I states, we select out the 42 districts that were project districts in 1994 and the 38 additional districts that were brought under the DPEP framework in phase II.¹¹ We have a sample of 93 districts remaining to find the appropriate counterfactuals for the DPEP-I districts.

⁸ To ensure that we have the best possible model to predict the propensity scores, several checks and balances are introduced in our methodology (See, Technical Appendix for details).

⁹ Six additional states – Andhra Pradesh, Uttar Pradesh, West Bengal, Orissa, Himachal Pradesh and Gujarat – were also declared project states under DPEP-II. However, we only focus on the DPEP-I states in our study.

¹⁰ However, the staggering of the projects can also be used to our advantage to look at another aspect of the evaluation of DPEP (Jalan, 2002).

¹¹ Based on Census of India data for 1991.

The propensity score estimator assumes that once we have matched on the predicted propensity score, the entire difference in the outcome indicators can be ascribed to the program. However, this may not be true. There could be some criterion in the selection of districts that is unobserved by the analyst. In the DPEP-I program itself, Table 1 clearly shows that low female literacy rate was not the *only* criterion used in the choice of program district. In almost all states, capability of districts to absorb the program was also kept in mind while selecting them as program districts. (See Pandey, 2000) The latter criterion is, however unobserved by us. In these cases, a difference-in-difference (DID) matching estimator that takes the difference between outcomes before and after program in project areas, minus the corresponding difference in the matched comparison areas, is the appropriate choice. The DID estimator allows for temporally invariant differences in outcomes between participants and non-participants.¹²

We need district data at two points in time to get around the problem of unobserved temporally invariant factors influencing participation and because the program is a district-level intervention. However, such data is not currently available.¹³ Lack of district-level data at two points in time creates a methodological problem. One solution is to create district averages by aggregating up sample household data to the district level. However, the sample design of the available household data sets are not stratified with respect to districts. As a result, aggregating households within the district to construct indicators at the district level may introduce significant sampling error in the variables.

If we match households across treatment and control districts and then report the impacts at the state level, we can avoid this problem. But then we encounter another problem. The program is a district level intervention, with selection of project districts based on certain district characteristics. If we use household level data to choose the

¹² In existing empirical work (for example, Smith and Todd (2000)), the DID estimator is found to be more robust than the cross-sectional matching estimators. A DID estimator however, requires data on periods after and before the program start data.

¹³ While the 1991 census data provides us with an ideal source of pre-program district data, data from 2001 census are not yet available. The only relevant district statistic available so far is the gender specific literacy rate for the overall population, which is not one of the explicit objectives of the program. Similarly, the sixth all-India education survey (SAIES) provides us with district data for 1993-94, but there has been no subsequent education survey conducted to give us information on the post-program statistics of the districts.

counterfactual, whereby all households in a treatment district are declared treatment households, we cannot control for district level characteristics. So we may end up matching households in treatment districts with households living in non-treatment districts using the propensity score model at the household level but if these matches are aggregated to the district level, we may end up with different matches. Below, we propose a way to get around this problem.

Since the district identifiers are available in the household data that we use, we can distinguish households residing in a project district versus those living in non-project areas. All households within a project district are declared to be project households. However, we still need to control for the district program selection criteria. We adopt a two stage matching method to get around this problem. First, we use the 1991 district level census data to match program districts with non-program districts. Once we have matched the districts, we use household level data to match households within the matched districts. For example, if district A is matched to district B using propensity score matching methods at the district level, in the second stage, we match households in district A with only households in district B. This way, we circumvent the problem of not being able to control for district level characteristics in the household matching stage.

Finally, the data that we use is not a panel data set, but are independent cross-sections over time. So we use methods appropriate for time-series of cross-sections as discussed in Heckman et. al. (1998) (See Technical Appendix for further details) in our analysis.

4 Data and Variables

We combine the 1991 census data with household level data to estimate the impact of the program. For the household level data, we use the 1993-94 and the 1999-2000 National Sample Survey (NSS) household surveys that collect data on age-specific enrollments and are available at two points in time (i.e. pre and post-program dates). The NSS adopts a two-stage stratified sampling design, but in neither stage is the district a stratification unit. Two issues may arise as a result of this. First, not all districts may be in the survey. This may mean deleting some of the program districts and/or choosing the second (or even third or higher) best matched counterfactual. Second, aggregation of households within districts to construct

district level statistics could lead to significant sampling error. In this paper, while the first is a non-issue, our econometrics methodology takes care of the second.

Table 2 reports literacy indicators from the 1991 census. We divide all districts in the seven project states into project districts (either under DPEP-I or DPEP-II) and non-project districts. The numbers suggest that at least on the face of it, the program implementers have chosen the program districts prudently. In the aggregate as well as in all sub-categories, literacy rates in the DPEP districts are worse than those in the non-program districts.

The first period of the household data is treated as pre-program data and the latter period as post-program information. We use data from the seven DPEP-I states. Excluding the expansion districts under DPEP-II in these states, our data covers 41 of the 42 project districts (Kaithal district in Haryana is covered by the NSS but for the 50th round there is no way to separate the households in Kaithal from the households in Kurukshetra.). We also exclude the district of Karbi Anglong in Assam from our set of treatment districts because available financial data indicates that flow of DPEP funds into this district started from 1997, nearly 3 years after the start of the project in other DPEP-I districts. Thus we have households residing in 40 treatment and 87 non-project districts.

Descriptive statistics on the sample used in the paper is reported in Table 3. There are approximately 5,900 households residing in the project districts and 12,500 households in the non-project districts. The per capita expenditures of households in these districts are lower when compared with the non-project districts. One striking feature is the higher percent of Scheduled Tribes residing in the project districts as compared to the non-project districts. This is congruent with the 1991 census data that indicate that in at least 4 of the 7 DPEP-I states, scheduled castes/tribes constituted more than 50% of total population.

We construct three main outcome variables: (i) current primary school attendance, (ii) the highest educational level completed and (iii) progression from primary to higher levels of education. Each measure was constructed at the household level.

Current attendance measures the proportion of children in 6 to 10 and 11-13 year old group in a household, currently attending primary school. A 1992 all-India schools survey

estimates that 21 percent of boys and 19 percent of girls enrolled in grade 5 were overage (World Bank, 1997). It is precisely to capture the possibility of late entry of children into primary education that we also include 11-13 year olds in our analysis.

The highest completed education level indicator measures the proportion of individuals with no education (illiterate), non-formal education, incomplete primary, and primary. This is based on individual's responses about their highest education level completed in the survey. We defined these indicators for all children in 11-13 and 14-15 age groups, as well as for males and females separately as well as for scheduled caste and tribe households.

Our final measure on progression rates uses cohort data from the two surveys. Using data from the 50th round, we calculate proportion of 6-7 year olds in a household who attended primary or pre-primary school in 1993; using 55th round data, we calculate the same measure for this cohort (i.e., those aged 12-13 in 1999-00) that attended middle or secondary school in 1999. The "drop out" outcome variable is then calculated as difference between these two measures. We construct similar measures for those aged 8-9 in 1993-94 and 14-15 in 1999-00. We thus construct two "drop out" measures, one for the younger — those aged 6 to 7 in 1993 and consequently aged 12-13 in 1999, and one for the older — those aged 8 to 9 in 1993 and consequently aged 14 15 in 1999.

5 Program Impacts

5.1 Comparison of change in trends between DPEP and non-DPEP districts

Figures 1-12 graph averages of the outcome indicators (defined in the earlier section) for DPEP and non-DPEP districts over the two time periods: 1993-94 and 1999-2000. These graphs demonstrate the following points:

(i) Initial mean levels (i.e. levels in 1993-94) of the outcome indicators in DPEP districts were considerably lower than the initial averages in non-DPEP districts. For example, the difference in primary school attendance among 6-10 year old scheduled caste children between DPEP and non-DPEP districts was more than ten percentage points.

(ii) Between the two periods, mean levels of the outcome indicators improved in both DPEP and non-DPEP districts across the different sub-populations (overall, males, females,

scheduled caste and scheduled tribes). Average primary school attendance by children in the age-group 6-10 years increased from less than 75 percent to approximately 80 percent in DPEP districts and from a little over 80 percent to nearly 88 percent in non-DPEP districts.

(iii) The rate of change in the mean outcome levels in DPEP districts was higher than that in non-DPEP districts. For example, the rate of growth in primary school attendance among 11-13 year olds was 9 percent in DPEP districts compared to barely 3 percent in non-DPEP districts. Similarly, change in mean proportion of 11-13 year olds reporting completed levels of primary education was 5.3 percent as compared to 1.1 percent in non-DPEP districts.

(iv) Despite improvements in mean outcomes over time in DPEP districts and at a rate faster than that observed in non-DPEP districts, the levels of outcome indicators in 1999-2000, failed to exceed the 1993-94 levels for the non-DPEP districts. Even though the rate of change in the proportion of 11-13 year olds reporting completed primary school levels was five times higher in DPEP districts than in non-DPEP districts, the mean percent of kids in DPEP districts in 1999/2000 reporting completed levels of primary school education was 49.8 percent; this is considerably lower than the average of 53.6 percent recorded for non-DPEP districts in 1993-94.

The above observations demonstrate substantial improvements in education indicators in DPEP districts. At the same time, they also show improvements in the non-DPEP districts. The issue then is to determine the marginal contribution of DPEP-I to primary school education, i.e., its net impact. To do this, we need to construct the “correct” counterfactual comparison group for the DPEP districts. This is what we do in the next subsection.

5.2 *Impact estimators of the DPEP*

We used a two stage matching procedure to estimate the impacts of the DPEP. First, we matched districts within states and then matched households within the matched districts. Among the set of observed characteristics included in the district propensity score model, the only statistically significant variables were the state dummy variables and female literacy rate.^{14,15} We therefore, matched project with non-project districts only on the basis of

¹⁴ The “balancing property” test of the propensity score model is satisfied.

observed female literacy rates within a particular state. However for robustness reasons, we also matched districts using the predicted propensity scores from the estimated model. We found that the matched control districts picked by the two alternative methods for the treatment districts were almost always identical.^{16,17} We matched households in treatment districts with households in matched control districts in the second stage.

We report the net impacts on the three different aspects of primary school education (access, completion levels and progression rates) for various population sub-groups (females, males, scheduled caste/tribe etc.). The underlying stochastic process for the different outcome indicators defined over each sub-population is different. Therefore, for the impact reported for each outcome indicator, we estimated a separate propensity score model to find similar households in treatment and non-treatment districts. Details on the individual logit models are available from the authors.

The impacts are reported for two sub-samples: full sample (includes households from 40 districts spread over the seven states), and separately for those households residing in Madhya Pradesh (Tables 5-12). We do this because in Madhya Pradesh, two substantive state level interventions called the Alternative Schooling (AS) and the Education Guarantee System (EGS) were started in 1995 and 1997 respectively. The AS started with 418 centers across the 19 DPEP-I districts, which by the year 2000, had expanded to 2,970 centers in the same districts (Jha, 2000). The EGS on the other hand has more than 16,000 centers in entire state (Gopalakrishnan and Sharma (1998)).¹⁸

¹⁵ The other variables included in our specification are: district population density, proportion of houses which are permanent and semi-permanent in district, proportion of households in district with power, and access to primary school, proportion of immigrants in district, scheduled caste/tribe concentration in district, and female literacy rate among scheduled caste/tribe.

¹⁶ We restrict the matching of districts to within states only to control for state specific rules and regulations in individual states. For example, in the state of Assam there is automatic promotion until primary school. This is not true in the other states.

¹⁷ In the paper we do not report the logit model at the district level. However this is available from the authors on request.

¹⁸ AS and EGS are two large-scale decentralized, non-formal primary school education systems sponsored by the government of Madhya Pradesh. Both these schemes cost less to the state government than setting up a formal primary school. The objectives of both these schemes were to provide primary school facilities to the most needy sections of society and those that cannot join the formal school system (for example, migrant children, children living in remote areas, those involved in labor etc.). Both these schemes emphasized on the importance of local community participation. For example, under both schemes, the

The data at hand (or even independent sources) do not help us distinguish enrollments in a DPEP school from enrollments in either of these schemes. In many cases, these alternative educational interventions have worked in a complementary manner and often under the umbrella of the same administrative body viz. *Rajiv Gandhi Shiksha Mission*.¹⁹ Given this, the best we can do is present the impacts for MP separately and see whether the pattern in MP is different from the other states.²⁰

Impacts on attendance rates. Tables 4 and 5 report the impacts on primary school attendance among 6-10 and 11-13 year olds for the overall population, for males and females separately and for the two minority groups – scheduled caste and tribes. The impact on attendance rates is defined as: $(P^T - P^c)_{1999-00} - (P^T - P^c)_{1993-94}$ where P^i ($i=Treatment, Control$) is the proportion of 6-10 or 11-13 year olds currently attending primary school.

In the aggregate, DPEP-I has had a “small” positive effect on attendance rates among children 6-10 years olds. This is true for all sub-populations except for girl students in this age category. It is only in the state of Madhya Pradesh that there has been impressive improvement in attendance rates. So much so that if we compare the overall impacts with those in Madhya Pradesh, in many cases it turns out that improvements in the other six states have been non-existent. For example, the impact on all children is estimated to be 0.95 percentage points when we take households in all the seven states together while the same for only those households in Madhya Pradesh is 1.21 percentage points. These numbers clearly suggest that in states other than Madhya Pradesh, the program has had a limited effect on attendance rates among 6-10 year olds.

A goal of the DPEP has been to improve primary school access for disadvantaged groups like scheduled castes and tribes. In this regard, the program has been successful in

local community body (*Panchayat*) is responsible for recommending the names of teachers, and the village education committee (or administrative bodies with similar functional responsibilities) is the managing authority with various responsibilities. AS has now been merged into the EGS schools.

¹⁹ For example, the pedagogical guidelines advocated by the DPEP are best represented in the Alternative Schools.

²⁰ The impacts for MP are weighted averages of the overall sample, the weights being the number of households in each category as a ratio of the overall sample.

substantially increasing primary school attendance rates among 6-10 year olds belonging to the scheduled caste group. However, no impact is observed for the scheduled tribes.

Another striking result of our analysis is that while reducing gender disparity in attendance rates was an avowed aim of the program, in reality quite the opposite has happened. While the impact on primary school attendance among 6-10 year old male children is considerable, there has been a negative net impact for girl children in age group 6-10 years. We do however find a positive net impact on primary school attendance for girls in the 11-13 year age group (See, Table 5). An explanation for this can be that the program succeeded in persuading “out of school” girls to attend school at an older age; however, we cannot be certain of this, as we cannot separate out the “late starters” from the “repeaters”.

Impacts on completed levels of education. In Tables 6-9, we report impacts on highest level of completed education among 11-13 and among 14-15 year olds to assess the impact of the program on the stock of completed levels of education. We chose 11-13 and 14-15 years as the relevant age-categories, to capture students who have completed their primary school education in stipulated years and to allow for some repetition and late starters. The impact on completed education levels is defined as: $(P_i^T - P_i^C)_{1999-00} - (P_i^T - P_i^C)_{1993-94}$ where P_i^j ($j=$ Treatment, Control) is the proportion of 11-13 or 14-15 year olds with ($i=no$ education, non-formal, incomplete primary, primary) levels of education.

For this outcome indicator, the net impact of DPEP-I is more impressive. The stock of education (measured in terms of highest level of education achieved) has improved considerably among 11-13 year old male children. The estimates show that the proportion of children reporting “no-education” or “incomplete primary education” have gone down while those reporting “completed primary school” education have increased significantly. This is true for children in Madhya Pradesh as well as the six other DPEP-I states. However, the “gender gap” is again not reduced. For the female sub-population, the net impact on education stock is at best zero and at worst negative. For children in the age-group 14-15 years, we find a pattern similar to that observed for the younger age group, though the impacts are distinctly smaller than before.

Tables 8 and 9 report the impacts for the scheduled caste and tribes. For 11-13 year olds, scheduled caste households have benefited from the program. The impacts are more muted for the 14-15 year olds. For the scheduled tribes, the picture is somewhat less heartening. The one noticeable result however, is that a higher proportion of scheduled tribe children in DPEP districts report completed levels of non-formal education.²¹

Impacts on cohort progression from primary to middle (or higher) level. Finally in Tables 10 and 11 we report impact estimates on the proportion of 6-7 (8-9 year olds) year olds attending primary school in 1993-94 who progressed to middle school by 1999-00. The impact on cohort progression is defined as: $(M_i^T - M_i^C)_{1999-00} - (P_j^T - P_j^C)_{1993-94}$ where M is the proportion of $i=(12-13, 14-15)$ year olds enrolled in middle school or higher in 1999-00 and P is the proportion of $j=(6-7, 8-9)$ year olds enrolled in primary school in 1993-94.

Our impact estimates show that there is a net increase in the proportion of 6-7 year old cohort who progress to middle school by 1999-2000 in the program districts. The magnitudes of the impacts are larger than that for attendance rates and for completion rates. This is observed in the aggregate and among all sub-populations with the exception of female children. In line with the patterns observed for the other two indicators, the net impacts are smaller for the older cohort (8-9 years) and the impacts are stronger for treatment households in Madhya Pradesh.

In summary, our analysis of the net impacts of DPEP-I on the different primary school education outcome indicators indicates the following results. First, the impact of this “massive” program, while positive on average, is surprisingly small. The average impact is the largest for “cohort progression”, suggesting that the program may have been instrumental in substantially reducing dropout rates among attending students, and the average impact is the smallest for “attendance rates”.

²¹ This is not very surprising because in several DPEP-I states like Assam, Kerala, Madhya Pradesh, alternative schools were started to improve access and retention among tribal children. In Kerala for example, according to the 15th Joint Review Mission, of the 136 alternative school centers opened in Kasaragod, Wayanad and Malappuram, 79 centers were opened in the tribal concentrated areas catering to 2,000 odd students. In contrast, few of these centers were placed in non-tribal non-coastal centers.

Second, a substantial part of the positive impact of DPEP-I emanates from the improvements recorded within the state of Madhya Pradesh. This is especially true for attendance rates. As mentioned before, Madhya Pradesh had other education programs like the EGS and AS running concurrently with DPEP-I, and it is not possible to disentangle the effects of the different programs.

Third, the program has been reasonably successful in improving the various educational outcomes of a particular disadvantaged group – the scheduled caste group. However, its impact on another disadvantaged group – the scheduled tribes – has been marginal.²²

Finally, our most surprising result is a virtual absence of any program impact on female children, whether the outcome indicator is attendance rates, or completion rates, or cohort progression rates. On the other hand, DPEP-I has substantially improved primary school indicators for male children. These results indicate that DPEP's objective of reducing the gender gap in enrollments, completed levels of education, and dropouts was certainly not achieved in the DPEP-I districts by the year 2000.

6 Conclusions

In this paper we have evaluated the DPEP-I education project using propensity score matching methods. The unexpected element in our program impact estimates is not that we find positive impacts of the program on various outcome indicators for different sub-populations *but* that the impacts are not as substantial as warranted by the massive amount of resources invested in the program or as claimed in the existing DPEP evaluation literature. Net program impacts on male children and scheduled caste population are the most impressive but here too, the net improvements in attendance rates are only 2.27 and 11.3 percent over the six years.

²² We do not want to make much of the scheduled tribe result, as they are mostly concentrated in only one state in our data.

Our analysis falls short in two ways. First, given the data at hand, we cannot assess the quality of education imparted in these primary schools. One would need standard test scores in both the control and treatment districts to comment on this aspect of the program. While terminal assessment scores (TAS) are available for the treatment districts, they were not simultaneously administered to the students in the non-treatment districts.

Second, we have estimated the average treatment impacts on the treated in our paper. However, even among the treated, there is heterogeneity in impacts. What explain these differences in impacts across treatment districts is an interesting and an important question from the point of view of program implementation. Simple correlations between the estimated net program impacts and several initial district education variables indicate a high correlation between attendance rates and presence of permanent school structures. However, examining the correlation matrix is too simplistic a method to ascertain conclusively the factors explaining the heterogeneity in impacts across treatment districts. Further research is required, and is being carried out at this time.

7 References

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Technical Appendix

We use the “counterfactual framework” proposed by Rubin (1974) and subsequently used by both statisticians and econometricians (Rosenbaum and Rubin (1983), Imbens and Angrist (1994), Heckman, Ichimura and Todd (1997) among others) to estimate the average treatment effects.

Let y_1 denote the outcome with treatment and y_0 outcome without treatment. Recognize that a unit cannot simultaneously be in both states. So, we cannot observe both y_1 and y_0 at the same time for the same unit. The econometrics problem that we have at hand is that of “missing data”.

Let t be a binary indicator, where $t=1$ indicates participation in the program and $t=0$ otherwise. (y_1, y_0, t) represents a random vector from the population of interest. For a random draw i from the population, the relevant vector is (y_{1i}, y_{0i}, t_i) . The implicit assumption that we make is that treatment of unit i affects only the outcome of unit i and does not affect any other unit’s outcome. Moreover, (y_1, y_0) could be correlated with t .

To measure the impact of the program, we are interested in the difference of outcomes with and without treatment. Several possible estimators are possible. We use the standard estimator of the average treatment on the treated (ATE) defined as: $ATE = E(y_1 - y_0 | t=1)$. That is, the mean effect of the program on the participants. Furthermore, if \underline{x} is a vector of observed covariates, ATE can be redefined as: $ATE = E(y_1 - y_0 | t=1, \underline{x})$.

Right at the beginning of the discussion we had posed the econometric problem underlying the estimation of program impacts as that of “missing data”. That is, for each treated (non-treated), at any point in time, we observe only y_1 (y_0). The observed outcome is: $y = y_0 + t(y_1 - y_0)$. Question therefore remains as to what do we do about the “missing data” problem?

There are different approaches for imputing the counterfactuals. Some of the non-experimental estimators are as follows. The cross-section mean difference indicators use data on $t=0$ as a proxy for missing data. In this case, the impact estimator is:

$$t_{cs} = E(y_1 | t=1) - E(y_0 | t=0) + [E(y_0 | t=1) - E(y_0 | t=0)]$$

[Evaluation Bias]

where the term in the parentheses is the evaluation bias. The before-after estimators treat pre-program data on participants to proxy for post-program outcomes in the no-treatment state. The impact estimator in this case is

$$t_{bl} = E(y_1 | t=1) - E(y_0 | t=1) + [E(y_0 | t=1) - E(y'_0 | t=1)]$$

[Evaluation Bias]

where y'_0 is the missing data. The term in parentheses is the evaluation bias. Advantage of the before-after estimator over the cross-sectional estimator is that it allows for unobserved fixed effects which difference out. However, the estimator is sensitive to time effects and to *Ashenfelter's Dip* problem. Finally, one can also compute the difference-in-difference estimator that takes differences over before-after outcomes for participants and non-participants. The impact estimator in this case is

$$t_{b2} = E(y_1 | t=1) - E(y_0 | t=1) + [E(y_0 - y'_0 | t=1) - E(y_0 - y'_0 | t=0)]$$

[Evaluation Bias]

This estimator takes care of the sensitivity to time effects problem in the before-after estimator.

Matching estimators pair each program participant with an observably similar non-participant and interpret the difference in their outcomes as the effect of the program. Matching estimators require that conditional on a vector of observed characteristics, \underline{x} , (y_1, y_0) are independent of t . This is the “ignorability” condition introduced by Rosenbaum and Rubin (1983).

Matching may be difficult to implement when the set of conditioning variables \underline{x} is large. Rosenbaum and Rubin (1983) define what they term as strong “ignorability” conditions that help in reducing the dimensionality problem of the conditioning variables while implementing the matching method.

Let $p(\underline{x}) = \text{Prob}(t=1|\underline{x})$ be the probability of treatment given the covariates \underline{x} . $p(\underline{x})$ is the propensity score function in the evaluation literature. Given $p(\underline{x})$, and under the assumptions (i) $E(y_1 - y_0 | t=1, \underline{x}) = E(y_1 - y_0 | \underline{x})$ (ii) $0 < p(\underline{x}) < 1$ for all \underline{x} , Rosenbaum and Rubin (1983) show that $E(y_1 - y_0 | t=1, p(\underline{x})) = E(y_1 - y_0 | p(\underline{x}))$. This implies that when the outcomes are independent of participation conditional on \underline{x} , they are also independent of participation conditional on the propensity score function.

What does the condition $0 < p(\underline{x}) < 1$ for all \underline{x} imply? Basically it imposes restrictions on the probability function such that ATE is identified. While it is still possible that ATE is identified when $p(\underline{x})=0$ for some \underline{x} , from a practical point of view we want to exclude units that have zero probability almost surely of being treated. We also exclude units with $p(\underline{x})=1$. We cannot estimate the ATE by including population units that are treated with certainty, conditional on \underline{x} .

If the above two assumptions are satisfied, then the y_0 distribution observed for the matched non-participant group can be substituted for the missing y_0 distribution for participants. The mean impact of the program can thus be written as:

$$\Delta = E(y_1 - y_0 | t=1) = E(y_1 | t=1) - E_{\underline{x} | t=1} \{E_y(y | t=1, \underline{x})\} = E(y_1 | t=1) - E_{\underline{x} | t=1} \{E_y(y | t=0, \underline{x})\}$$

where the second term is estimated from the mean outcomes of the matched comparison group.

For non-experimental data, we may not find a set of observed conditioning variables for which the conditions hold. This is the problem of *no common support* region. If there are regions where the support of \underline{x} does not overlap for the treated and non-treated, then there may be a fraction of program participants for whom no match could be found in the data. According to various studies by Heckman, Ichimura and Todd (1997, 1998), matching on the *no common support* region is the primary cause of a bias in a matching estimator.

Let $P = \Pr(t=1|\underline{x})$ be the estimated propensity score. Then the matching estimator is defined as

$$\iota_m = \frac{1}{n_1} \sum_{i \in I_1 \cap S_p} [y_{i1} - \hat{E}(y_{0i} | t = 1, P_i)]$$

where $\hat{E}(y_{0i} | t = 1, P_i) = \sum_{j \in I_0} W(i, j) y_{0j}$. I_1 denotes the set of program participants, I_0 set of non-program participants, S_p the region of common support, n_1 the number of persons in the intersection of the region of common support and the set of program participants. The match for each participant is constructed as a weighted average over the outcomes of the non-participants, where the weights depend on the distance between P_i and P_j .

Let $C(P_i)$ be a neighborhood for each i in the participant sample. Neighbors for i are non-participants $j \in I_0$ for $P_j \in C(P_i)$. The persons matched to person i are those in set A_i where $A_i = \{j \in I_0 | P_j \in C(P_i)\}$. Alternative matching estimators differ on the basis of the construction of the weights $W(\dots)$ and how the neighborhood is defined.²³

Some of the commonly defined matching estimators are the nearest neighbor matching estimator where $C(P_i) = \min_j \|P_i - P_j\|, j \in I_0$. That is, the non-participant with the value of P_j that is closest to P_i is selected as the match. In this case the $W(\dots)$ function is unity.²⁴ Similarly, there are the caliper matching estimators, where a match is declared only if the $C(P_i) = \min_j \|P_i - P_j\|, j \in I_0$ is restricted to be within a certain bound. Kernel and local linear matching estimators uses a kernel weighted average over multiple persons in the comparison group.

The estimators described above assume that after conditioning on a set of observed covariates \underline{x} , the mean outcomes are conditionally mean independent of participation in the program. However, this may not necessarily hold which could lead to the violation of ignorability conditions required for matching. In case of the DPEP, in the first phase of the

²³ Jalan and Ravallion (2000b) discuss the choice further, and find that their results for estimating income gains from an anti-poverty program are reasonably robust to the choice.

²⁴ Rubin and Thomas (2000) use simulations to compare the bias in using the nearest five neighbors to just the nearest neighbor; no clear pattern emerges.

project, program administrators identified project districts on the basis of low female literacy rates but also took the potential capability of the district to absorb the project while declaring it a project site. The latter criterion is very difficult to observe and if ignored, the correlation between program participation and the outcomes would still persist even if conditioned on female literacy rates and other observed district characteristics. In these cases, a difference-in-difference matching estimator (DID) allows for temporally invariant differences in outcomes between participants and non-participants. The DID estimator requires that $E(y_{0t} - y_{0t'}|P, t=1) = E(y_t - y_{t'}|P, t=0)$, where t and t' are time periods before and after the program enrollment date. This estimator also requires the support condition to hold in both periods. The impact of the project is thus defined as the difference between outcomes in the project areas after the program and before it, minus the corresponding outcome difference in the matched comparison areas.

If repeated cross-section data are available instead of information on the same households, the estimator can be implemented as:

$$DID = \frac{1}{n_{1t}} \sum_{i \in I_{1t} \cap S_p} [Y_{it}^1 - \sum_{j \in I_{0t} \cap S_p} W(i, j) Y_{jt}^0] - \frac{1}{n_{1t'}} \sum_{i \in I_{1t'} \cap S_p} [Y_{it'}^1 - \sum_{j \in I_{0t'} \cap S_p} W(i, j) Y_{jt'}^0]$$

$I_{1t}, I_{1t'}, I_{0t}, I_{0t'}$ denote the treatment and comparison group datasets in each time period. In our analysis, we use the matching estimator defined for the repeated cross-section estimator.

The success of any matching estimator depends on the availability of observable data to construct the conditioning set such that the “ignorability” conditions are satisfied. Furthermore, choice of variables to estimate the propensity score is also an integral component of the matching procedure. While theory does not suggest ways in which to choose the \underline{x} vector, the balancing property proposed by Rosenbaum and Rubin (1983) help determine interactions and higher-order terms to be included for a given set of \underline{x} such that the conditions are met. The intuition behind this proposition is that after conditioning on $\text{Prob}(t=1|\underline{x})$, additional conditioning on \underline{x} , should not provide any new information about t . The test therefore is whether or not there are differences in \underline{x} between $t=1$ and $t=0$ groups.

In this paper, we follow Dehejia and Wahba (1998, 1999) and divide the observations into strata based on the estimated propensity scores. These strata are chosen so that there is

no statistical difference in the mean of the estimated propensity scores between the treated and the untreated observations within each stratum. From a practical point of view, we start with a very coarse strata and make it finer until the above condition is satisfied. Once the strata are fixed, t-tests are used to detect mean differences in each x variable between the treated and non-treated groups. When significant differences are found for particular variables, higher order and interaction terms are added to the logistic model and the testing procedure repeated until no further differences emerge.

Table 1: DPEP-I Districts

ASSAM

(Districts in 1991: 23)

1. Dhubri (23.)
4. Darrang (30.)
5. Karbi Anglong (30.)
11. Marigaon (37.)

HARYANA

(Districts in 1991: 16)

2. Kaithal (23.68)
3. Jind (24.35)
4. Hisar (24.38)
6. Sirsa (27.43)

KARNATAKA

(Districts in 1991: 20)

2. Raichur (16.48)
6. Kolar (29.06)
7. Belgaum (31.07)
8. Mandya (32.12)

KERALA

(Districts in 1991: 14)

2. Kasaragod (75.31)
3. Wayanad (77.64)
5. Malappuram (83.91)

MADHYA PRADESH

(Districts in 1991: 47)

3. Rajgarh (9.46)
4. Guna (10.12)
5. Sidhi (11.40)
7. Surguja (12.50)
8. Shahdol (12.85)
11. Ratlam (13.94)
12. Chattarpur (14.12)
13. Panna (14.85)
15. Sehore (15.07)
17. Tikamgarh (15.39)
18. Dhar (15.64)
25. Mandsaur (19.88)
26. Raisen (20.45)
27. Bilaspur (20.92)
29. Satna (22.19)
30. Rajnandgaon (22.24)
32. Rewa (22.81)
33. Raigarh (23.48)
40. Betul (26.71)

MAHARASHTRA

(Districts in 1991: 30)

2. Parbhani(22.80)
3. Nanded (24.30)
6. Aurangabad (28.39)
8. Latur (35.35)
10. Osmanabad (35.80)

TAMIL NADU

(Districts in 1991: 22)

1. Dharmapuri (31.86)
2. South Arcot (34.29)
5. TSambuvarayar(35.9)

Note: Number preceding the name of the district indicates the rank of the district in the state with respect to its female literacy rate. A smaller number indicates lower literacy rate. Numbers in parentheses are the observed female literacy rates in 1991 from census data

Table 2: Descriptive statistics on education indicators in DPEP and non-DPEP districts

Indicator	DPEP – I	Non-DPEP - I *
Female literacy rates	0.2648 (0.157)	0.3923 (0.181)
Male literacy rates	0.5566 (0.124)	0.6669 (0.116)
Female literacy rates for scheduled castes	0.2100 (0.149)	0.3449 (0.195)
Male literacy rates for scheduled castes	0.4878 (0.135)	0.6097 (0.143)
Female literacy rates for scheduled tribes	0.1589 (0.131)	0.2768 (0.198)
Male literacy rates for scheduled tribes	0.3776 (0.199)	0.4841 (0.228)

Source: Census 1991

*This category excludes the expansion districts under DPEP-II in the DPEP-I states

Table 3: Descriptive statistics of the NSS sample

	50 th Round (1993-94)		55 th Round (1999-00)	
	DPEP	Non-DPEP*	DPEP	Non-DPEP*
<i>Sample Size</i>				
States: Assam, Haryana, Karnataka, Kerala, Madhya Pradesh, Maharashtra, Tamil Nadu				
Number of districts	41**	87	42	87
Number of households	5,986	12,576	5,438	13,027
<i>Demographic Structure</i>				
Household size	4.92	4.65	5.1	4.6
Proportion of children: 6-10 years	13.1	11.7	12.2	9.5
11-13 years	7.6	7.4	6.7	5.9
14-15 years	6.0	5.2	4.1	3.9
Proportion of girls: 6-10 years	6.1	5.5	5.6	4.4
11-13 years	3.5	3.4	2.9	2.7
14-15 years	2.2	2.3	1.8	1.7
<i>Social Status</i>				
Percent of scheduled tribes	16.7	9.0	18.8	10.6
Percent of scheduled castes	18.0	16.8	19.6	19.4
<i>Employment status</i>				
Percent of self-employed in non-agriculture	10.4	11.6	9.3	12.3
Percent of agricultural labor	36.6	35.9	37.1	36.5
Percent of other (non-agricultural) labor	5.9	11.5	6.5	12.1

Source: Estimates based on NSS data

* : excludes districts which were declared as project districts under DPEP-II

** : Kaithal district in Harayana was excluded from the sample because the data does not allow us to distinguish it from Kurukshetra, a non-project district

Figure 1
1993-1999: Current Attendance of 6-10 year old (percent of all children attending school)
by DPEP status

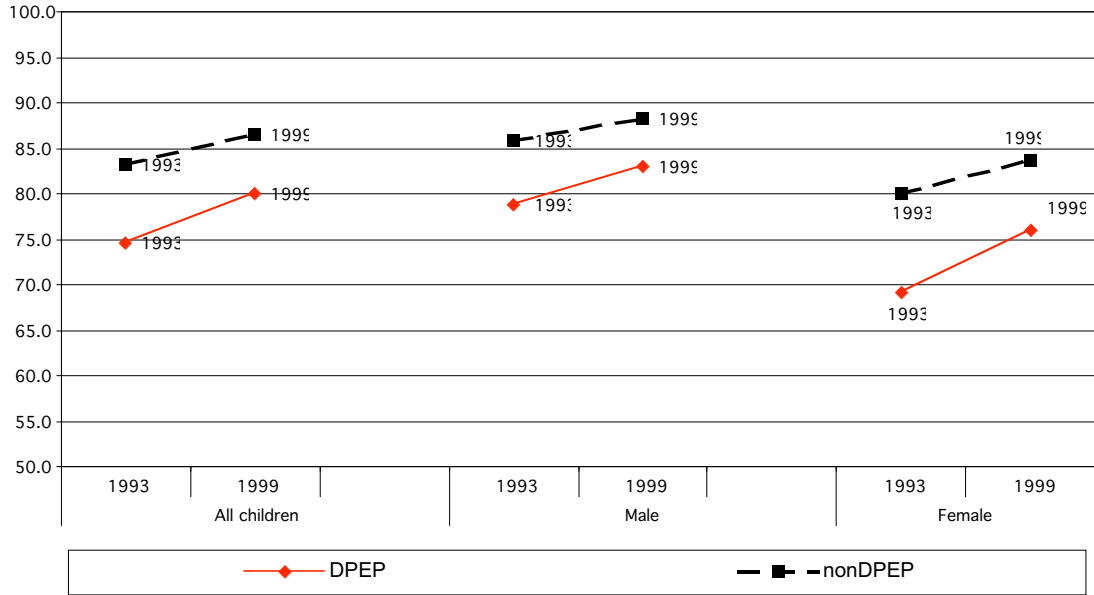


Figure 2
1993-1999: Current Attendance of 11-13 year old (percent of all children attending school)
by DPEP status

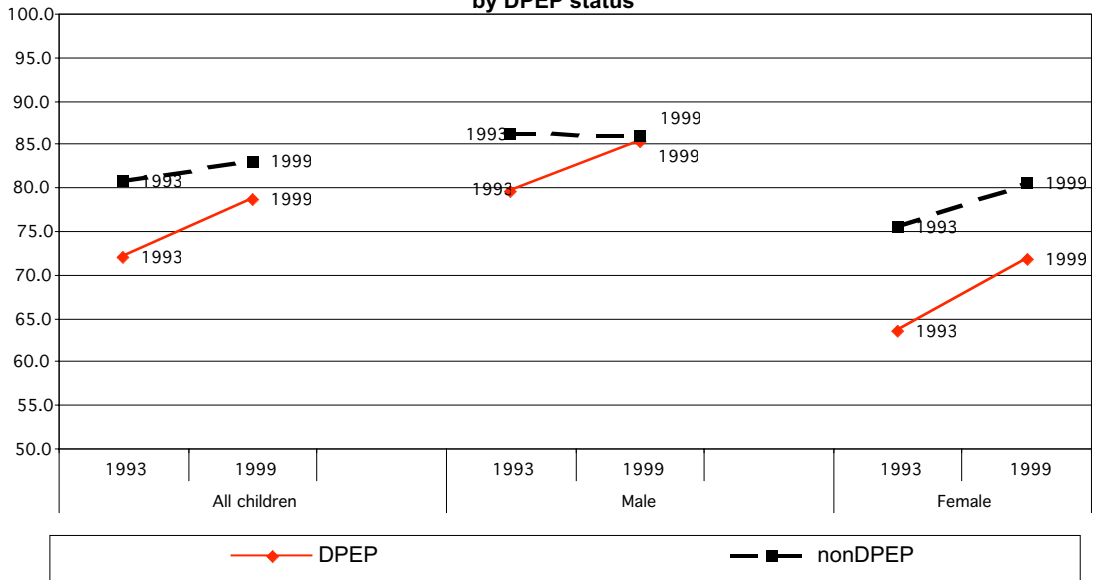


Figure 3

1993-1999: Current Attendance of 6-10 year old (percent of all children attending school) by DPEP status

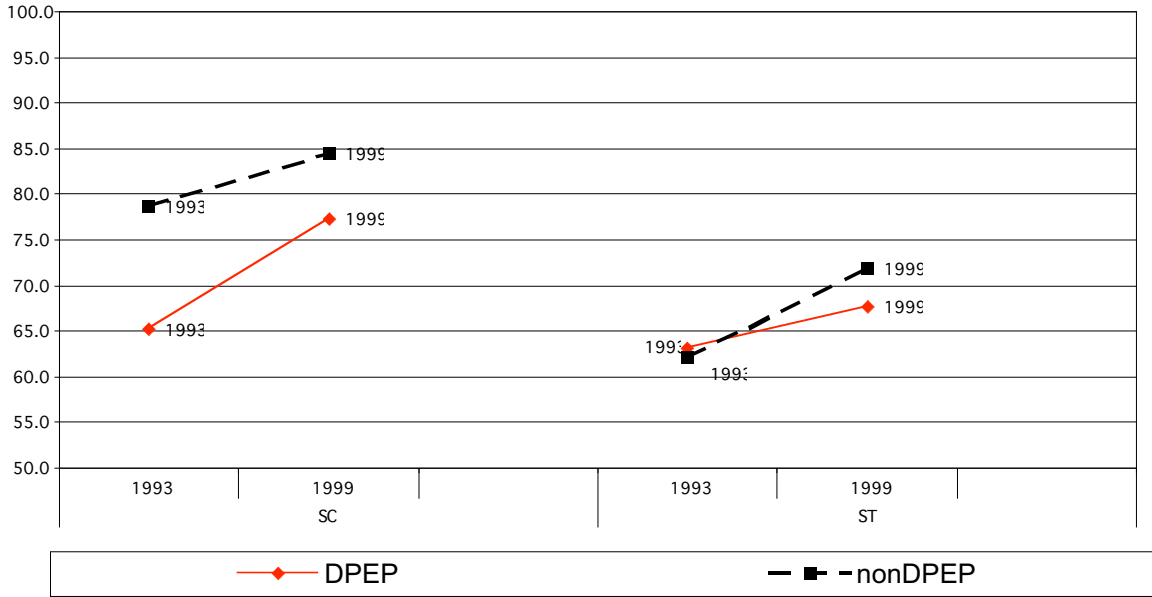


Figure 4

1993-1999: Current Attendance of 11-13 year old (percent of all children attending school) by DPEP status

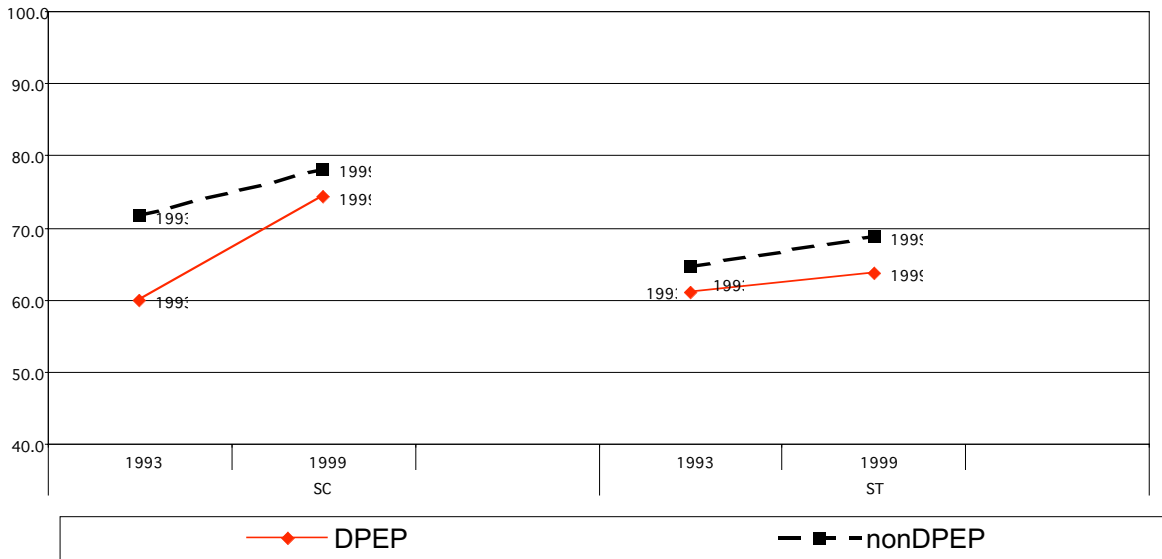


Figure 5

Change in level of education among 11-13 years old between 1993 & 1999
(change in proportion of all children with level of education)

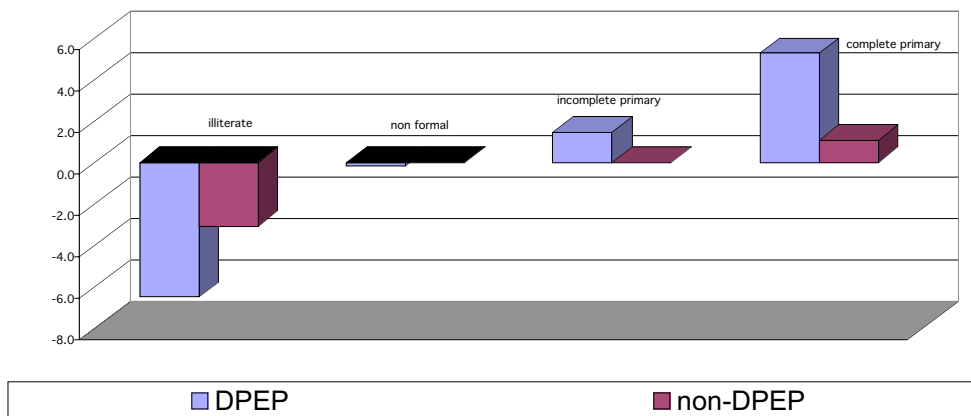


Figure 6

Change in level of education among 11-13 years old girls between 1993 & 1999
(change in proportion of girl children with level of education)

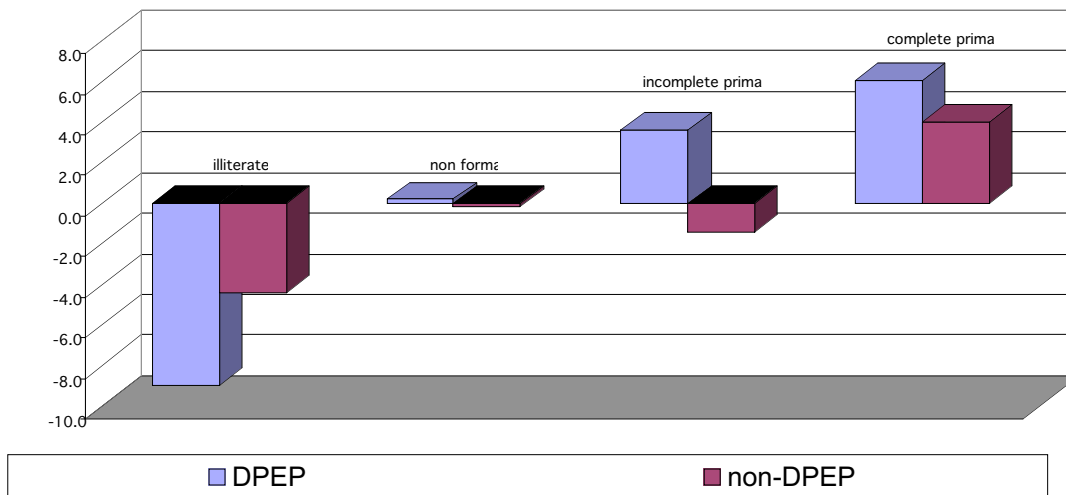


Figure 7

Change in level of education among 11-13 years old Scheduled Caste population
between 1993 & 1999
(change in proportion of SC children with level of education)

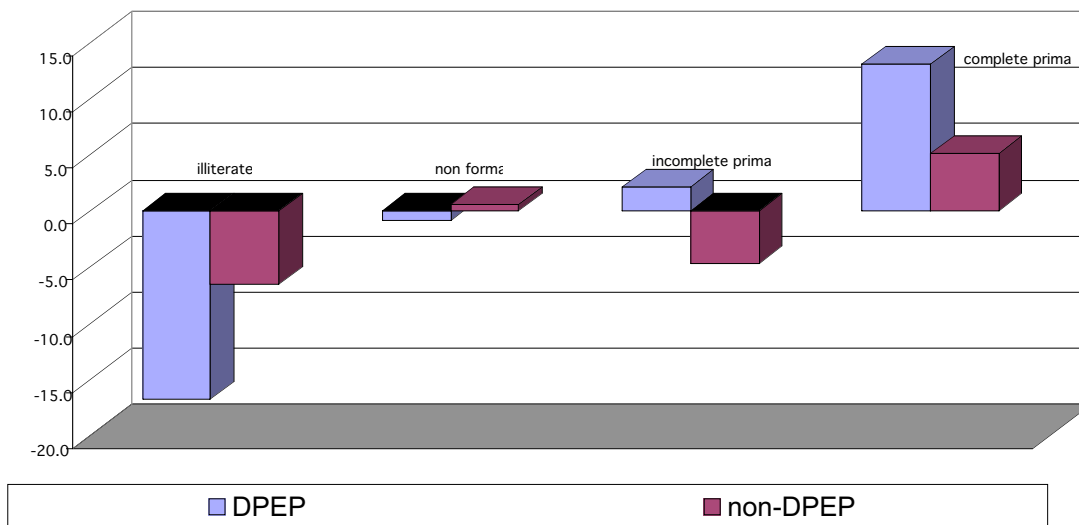


Figure 8

Change in level of education among 11-13 years old Scheduled Tribes population
between 1993 & 1999
(change in proportion of ST children with level of education)

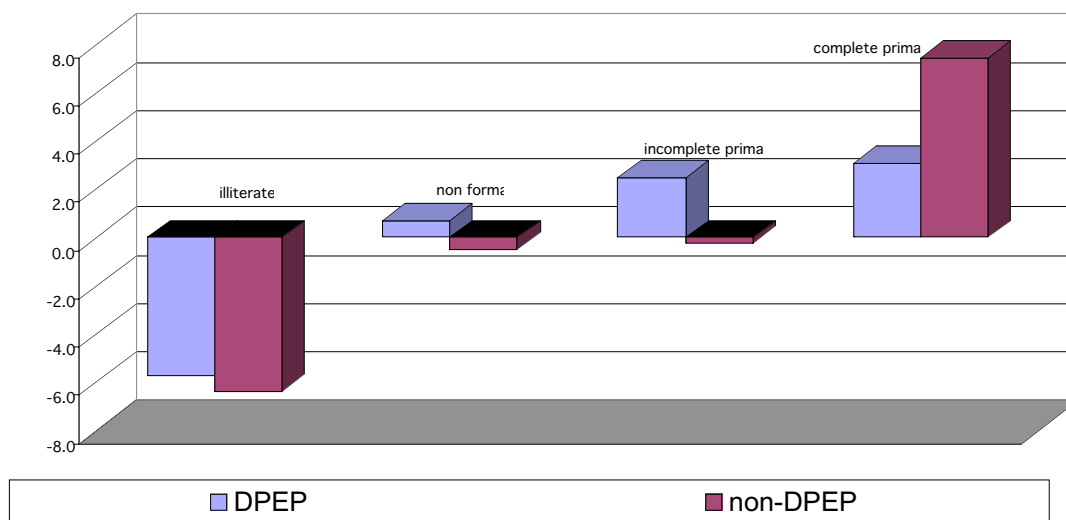


Figure 10: Cohort progression by DPEP status
 (8-9 year cohort enrolled in primary or pre-primary in 1993 and the same cohort enrolled in middle or secondary in 1999)

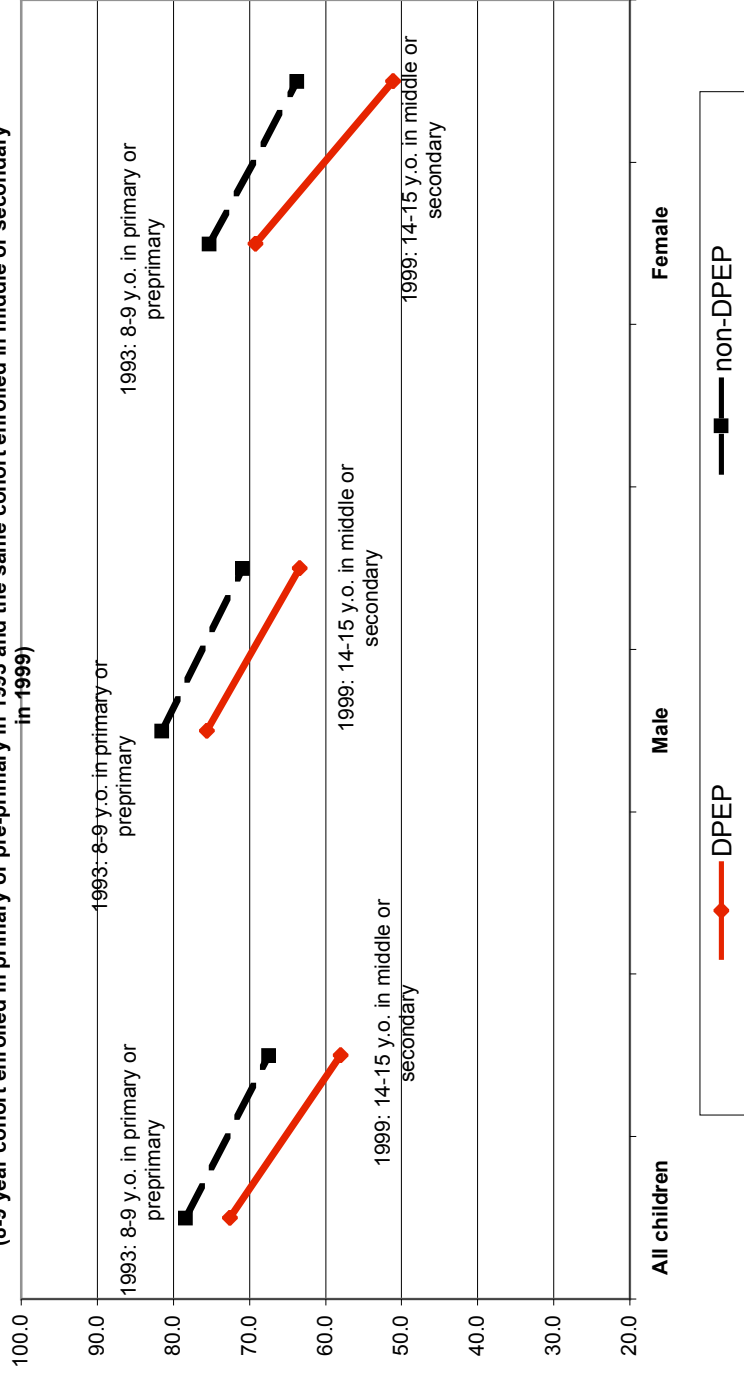


Figure 11: Cohort progression by DPEP status (6-7 year old cohort enrolled in primary or pre-primary in 1993 and the same cohort enrolled in middle or secondary in 1999)

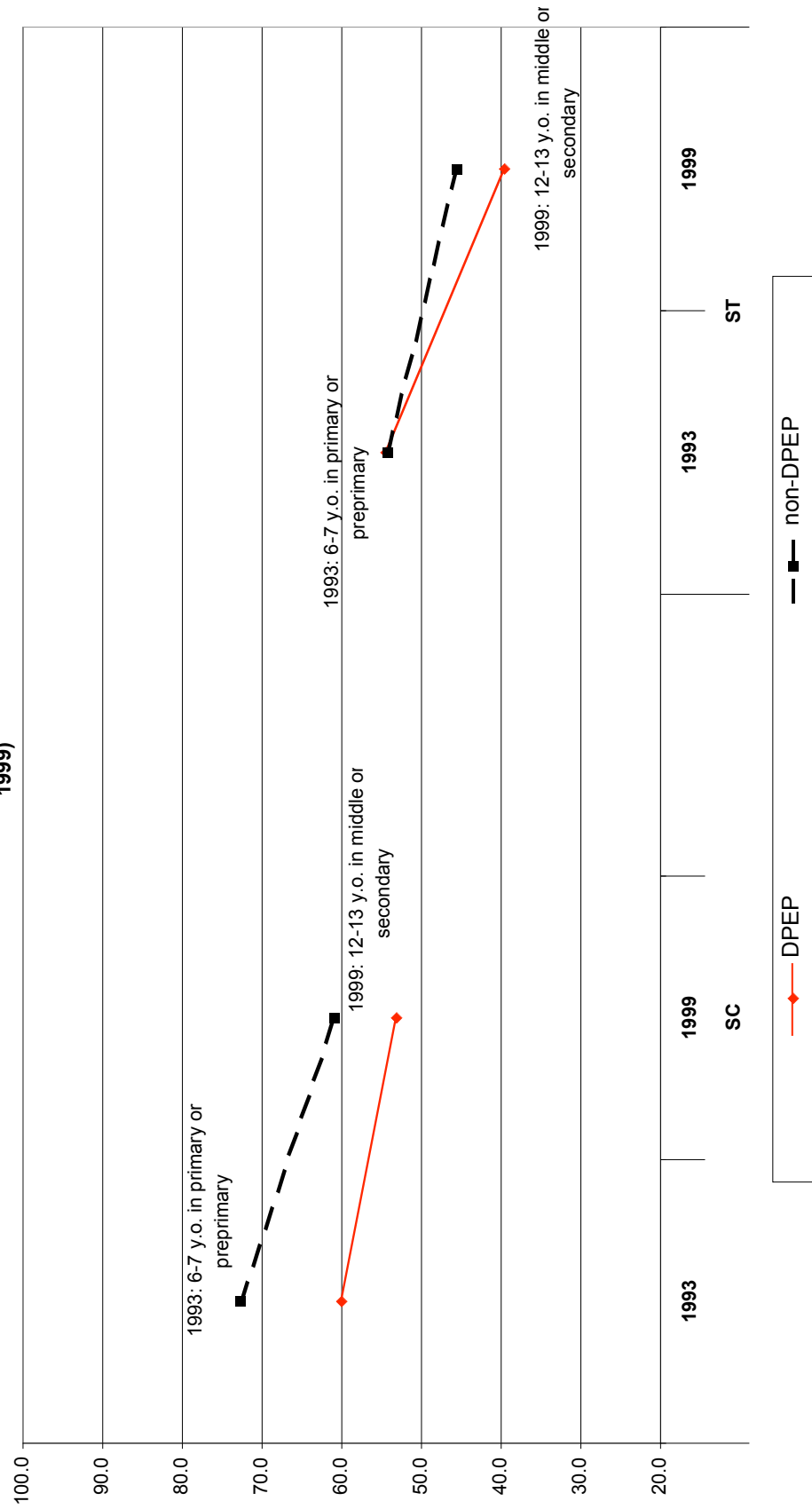


Figure 12: Cohort progression by DPEP status (8-9 year old cohort enrolled in primary or pre-primary in 1993 and the same cohort enrolled in middle or secondary in 1999)

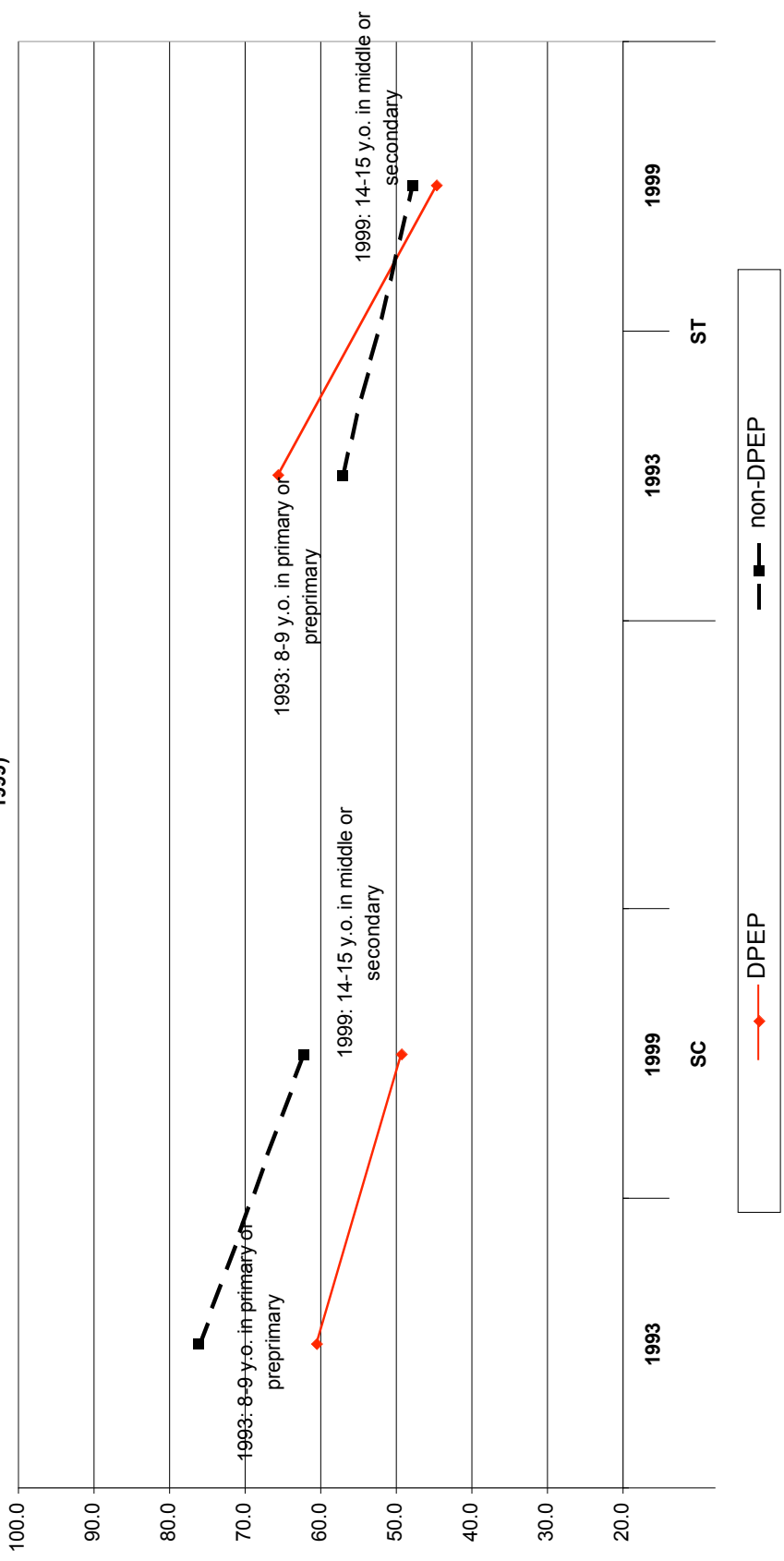


Table 4: Net impact of DPEP on school attendance of children 6-10 years

Population group	Full sample	Madhya Pradesh
All children	0.0095* (3.092)	0.0121* (22.427)
Girls	-0.0372* (-7.721)	-0.0222* (-23.578)
Boys	0.0227* (5.340)	0.0227* (27.302)
Scheduled Tribe	-0.0073 (-0.534)	0.0200* (5.381)
Scheduled Caste	0.1134* (8.331)	0.1040* (28.081)

Table 5: Net impact of DPEP on school attendance of children 11-13 years

Population group	Full sample	Madhya Pradesh
All children	-0.0261* (-6.587)	-0.0300* (-36.936)
Girls	0.0138* (2.048)	0.0297* (18.416)
Boys	-0.0290* (-4.600)	-0.0258* (-18.901)
Scheduled Tribe	-0.0445* (-2.220)	-0.0678* (-10.972)
Scheduled Caste	-0.1170* (-6.033)	0.0226* (3.828)

Notes: Figures in parentheses indicate associated t-statistics
 * indicates significance at 5% or lower

Table 6: Net impact of DPEP on highest completed level of schooling among 11-13 year olds

	Full sample	Madhya Pradesh
<i>All children</i>		
No education	0.0190* (5.004)	0.0006 (0.833)
Non-formal education		
Incomplete primary education	-0.0357* (-8.914)	-0.0122* (-14.652)
Primary education	0.0394* (9.012)	0.0152* (18.503)
<i>Female children</i>		
No education	0.0113 (1.632)	-0.0005 (-0.305)
Non-formal education	-0.0075* (-2.909)	0.0002 (0.518)
Incomplete primary education	-0.0034 (-0.500)	0.0007 (0.408)
Primary education	-0.0276* (-3.658)	-0.0002 (-0.136)
<i>Male children</i>		
No education	-0.0511* (-9.131)	-0.0404* (-34.618)
Non-formal education	-0.0007 (-0.330)	-0.0012* (-10.101)
Incomplete primary education	-0.0347* (-5.427)	0.0038* (2.624)
Primary education	0.1041* (14.908)	0.0344* (23.635)

Notes: Figures in parentheses indicate associated t-statistics

* indicates significance at 5% or lower

Table 7: Net impact of DPEP on highest completed level of schooling among 14-15 year olds

	Full sample	Madhya Pradesh
<i>All children</i>		
No education	0.0065 (1.222)	-0.0151* (-12.437)
Non-formal education	0.0056* (2.817)	0.0034* (44.228)
Incomplete primary education	0.0706* (15.039)	0.0402* (38.858)
Primary education	0.0008 (0.138)	0.0130* (10.187)
<i>Female children</i>		
No education	-0.0100 (-0.945)	-0.0002 (-0.057)
Non-formal education	0.0029 (0.804)	0.0078* (16.602)
Incomplete primary education	0.0158 (1.778)	0.0187* (8.278)
Primary education	-0.0029 (-0.270)	0.0030 (1.145)
<i>Male children</i>		
No education	0.0022 (0.284)	-0.0310* (-16.838)
Non-formal education	0.0128* (4.273)	0.0015* (16.941)
Incomplete primary education	0.0348* (5.090)	0.0064* (4.261)
Primary education	-0.0083 (-0.979)	0.0319* (14.901)

Notes: Figures in parentheses indicate associated t-statistics
 * indicates significance at 5% or lower

Table 8: Net impact of DPEP on highest completed level of schooling among 11-13 year olds

	Full sample	Madhya Pradesh
<i>Scheduled caste children</i>		
No education	-0.0764* (-3.766)	Small sample
Non-formal education	-0.0043 (-1.000)	
Incomplete primary education	-0.0479* (-2.405)	
Primary education	0.1010* (4.712)	
<i>Scheduled tribe children</i>		
No education	0.1194* (5.464)	0.1021* (14.880)
Non-formal education	0.0435* (5.285)	0.0397* (31.175)
Incomplete primary education	-0.0538* (-2.574)	-0.1290* (-20.054)
Primary education	-0.0903* (-4.390)	-0.0023 (-0.419)

Notes: Figures in parentheses indicate associated t-statistics

* indicates significance at 5% or lower

Table 9: Net impact of DPEP on highest completed level of schooling among 14-15 year olds

	Full Sample	Madhya Pradesh
<i>Scheduled Caste</i>		
No education	-0.1884* (-6.163)	
Non-formal education	0.0000 (0.000)	
Incomplete primary education	0.2114* (7.829)	Small sample
Primary education	-0.1288* (-4.532)	
<i>Scheduled Tribe</i>		
No education	0.0062 (0.199)	-0.0022 (-0.209)
Non-formal education	0.0117* (18.548)	0.0054* (10.909)
Incomplete primary education	0.1032* (3.719)	0.0292 (3.467)
Primary education	0.1800* (6.298)	0.1581* (16.550)

Notes: Figures in parentheses indicate associated t-statistics
* indicates significance at 5% or lower

Table 10: Net impact of DPEP on cohort progression of 6-7 year old students in 1993

	Full Sample	Madhya Pradesh
All children	0.0413* (8.143)	0.0341* (33.121)
Female children	-0.0104 (-1.159)	0.0068* (3.181)
Male children	0.0766* (9.349)	0.0449* (22.512)
Scheduled caste	0.0594* (2.558)	Small sample
Scheduled tribe	0.0791* (3.463)	0.1635* (25.293)

Table 11: Net impact of DPEP on cohort progression of 8-9 year old students in 1993

	Full Sample	Madhya Pradesh
All children	-0.0286* (-5.486)	0.0193* (15.803)
Female children	-0.1110* (-11.892)	-0.0045* (-1.908)
Male children	0.0862* (10.780)	0.0737* (35.999)
Scheduled Caste	0.2726* (10.596)	Small sample
Scheduled Tribe	0.0383 (1.524)	0.0360* (4.891)

Notes: Figures in parentheses indicate associated t-statistics
 * indicates significance at 5% or lower