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ABSTRACT

Trade Liberalization and Human Capital Accumulation: Evidence from Indian Census

We exploit the pre-reform employment composition of Indian districts and differential tariff cuts across industries introduced by the 1991 trade liberalization to examine the impact of liberalization on human capital accumulation measured by completion of different stages of schooling and aggregate schooling. Using Census 2011 data, we divide age cohorts that attended school before and after liberalization to implement cohort wise difference-in-difference strategy. We also construct a district-level panel using four decennial censuses that covers 1981-2011 and get an alternative difference-in-difference estimate by looking at the pre and post liberalization outcomes. We find that once we allow for the differential state policies, there is no evidence that the Indian trade liberalization has any impact on either aggregate schooling or on the attainment at different stages of schooling. We find suggestive evidence that positive effect of the increased returns to education was mitigated by the increased opportunity cost of schooling.

JEL Classification: O15, 019, J24, F16, F63

Keywords: liberalization, human capital accumulation, difference-in-difference

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1 Introduction

The effects of trade liberalization on different economic outcomes has attracted considerable attention. A number of studies have focused on human capital and the results are mixed. Increased exports may increase returns to skills (e.g., Hanson and Harrison, 1999; Goldberg and Pavcnik, 2007) increasing the incentive for higher education. However, export sector expansion in developing countries may also increase demand for low-skilled labor raising the opportunity cost of higher education. For example, Atkin (2016) exploits variation in the timing of manufacturing plant openings across municipalities in Mexico and finds increased high school dropout rates among those who were eligible for employment as job market opportunities increased in municipalities with new plants.[1] Hence, the impact of trade liberalization on human capital remains unambiguous theoretically and is an empirical question.

Indian trade liberalization presents a perfect setting to study the impact of liberalization on human capital accumulation. India introduced trade liberalization in July 1991 where import tariffs were reduced dramatically.[2] The introduction of liberalization was as a result of major balance of payments crisis and a bailout from the International Monetary Fund (IMF). The maximum tariff was reduced from 400 percent to 150 percent in July 1991, and later reductions brings the maximum tariff down to roughly 45 percent by 1997–98. Mean tariffs, which were 128 percent before July 1991 had fallen to roughly 35 percent by 1997–98 (Hasan, Mitra, and Ural, 2007). Edmonds, Pavcnik, and Topalova (2010), Topalova and Khandelwal (2011), and Topalova (2007, 2010) argue that set largely by the 1991 agreement with the International Monetary Fund (IMF), the tariff changes over the 1991–1997 period were not the result of the usual political economy process and were unlikely to have been anticipated

[1] In developed country setting, Greenland and Lopresti (2016) show that increased competition from Chinese imports led to decreased market opportunities for individuals without a high school degree and increased local high school graduation rates in the US commuter zones that were more exposed to Chinese imports compared to the commuter zones that were less exposed to Chinese imports.

[2] On 24 July 1991, Indian Finance Minister presented the budget in Indian Parliament which outlines the broader reforms.

by the labor markets. Using firm-level data, Topalova (2004) finds no correlation between future tariffs and current productivity and productivity growth for the 1989–1996 period. For the time period after 1997, however, she does find that future tariffs are negatively correlated with current productivity. A number of papers has exploited the exogenous nature of Indian trade liberalization to study the impact of trade liberalization on school attendance among 10-14 year old (Edmonds, Pavcnik, and Topalova, 2009, 2010), poverty (Topalova, 2007, 2010), firm-level productivity (Topalova and Khandelwal, 2011), unemployment (Hasan, Mitra, Ranjan, and Ahsan, 2012), industry wage premium (Kumar and Mishra, 2008), and fertility (Anukriti and Kumler, 2019).

The existing literature on human capital and Indian trade liberalization focuses on school attendance among 10-14 year old, and the evidence regarding impact on aggregate schooling is lacking. Using two waves of nationally representative National Sample Survey (NSS) data collected in 1987 and 1999-00, Edmonds, Pavcnik, and Topalova (2010, EPT henceforth) look at school attendance among 10-14 year old in rural India. Comparing pre and post 1991 data, they find that rural areas of districts that were more exposed to the tariff reduction experienced a lower increase in school attendance among 10-14 year old. EPT (2009) find similar results for primary school attendance among 10-14 year old in urban areas using the tariff exposure variation across 74 NSS state-regions.[3] Thus both EPT (2009) and EPT (2010) look at the attendance among 10-14 year old. A decrease at extensive margin—participation in education—will not necessarily translate into lower accumulation of human capital (aggregate schooling) if individuals stay at school longer. A change in average schooling will depend on both the extensive margin and intensive margin—how long students stay at school. The period of the 1990s also saw increased returns to schooling (Kijima, 2006) and increased college premium (Azam, 2010) in urban India providing individuals incentive to stay longer in school.

[3]The state-regions are constructed by National Sample Survey by grouping contiguous districts within same state with similar agroclimatic conditions and socioeconomic characteristic. The entire country is divided into 77 regions in the 1987 NSS data.

In this paper, we revisit the issue of the impact of Indian trade liberalization on human capital accumulation using district level panel constructed from the four Indian decennial censuses covering 1981-2011 period. We not only look at the aggregate years of schooling but also the completion of different stages of schooling as outcomes. It is important to consider the completion of different stages of schooling to have a complete picture of change in human capital. In a setting where a large fraction of the school going population is out of school, a mere increase in enrollment will lead to improvement in schooling. However, it will not automatically translate into change in quality of human capital. For example, if the enrollment rate at primary stage is improved and a large fraction of students drop out before completion of primary school or just after completing primary school, this will lead to improvement in aggregate schooling, but not much change in population with secondary or higher degrees. Similar to other papers on Indian trade-liberalization, we also exploit the fact that different industries witnessed differential tariff-cuts, and the impact of tariff-cuts were different for different districts based on their pre-liberalization (1991) industrial composition of employment. We carry out two different empirical strategies to get alternative estimates of the impact. Our first empirical strategy exploits district-level variation in change in tariff exposure over 1991-1997 and age (birth) cohorts whose schooling decisions were taken before and after 1991 to find the impact of the trade liberalization. As discussed later, the use of 2011 Census for cohort wise analysis allows different age groups to have completed their schooling cycle. Our second empirical strategy is a traditional difference-in-difference set up where we compare districts that were more exposed to tariff reduction to districts that were less exposed to tariff reduction using pre and post 1991 data. Moreover, in our analysis we allow the outcomes to vary across states to account for differential policies of states over time.

Our paper differs from EPT (2009, 2010) in following ways. First, unlike EPT (2009, 2010) who look at the school attendance among 10-14 year old only, our measure of human capital is achievement (completion) that captures both changes at intensive and extensive

margins. Moreover, we not only look at the primary school completion, but also secondary and tertiary school completion. In addition, we also consider aggregate years of schooling as one of the measures of human capital. Second, while both EPT (2009) and EPT (2010) use household survey data to construct their school attendance among 10-14 year old, we use census data to construct our district wise completion rates at different stages of schooling. The use of census data allows us to exploit district wise variation in change in tariff exposure in urban areas also to examine the impact of liberalization. In contrast, EPT (2009) exploit state-region variation for urban areas due to constraint of survey data. Third, while our traditional DID strategy is similar to EPT (2009, 2010), we carry out the placebo at the same district level as for our main results.[4] Fourth, we also conduct a cohort wise analysis to examine the impact of liberalization on achievements of different age cohorts since different age cohorts might be exposed to liberalization differently depending on when they attended school and which districts they reside. Fifth, we allow for differential state policies in our empirical specifications by introducing differential state-specific trends.

Allowing differential state-specific trends while exploiting district wise variation in trade exposure is quite demanding, however, given India's federal structure where majority of administrative power lie with the state governments, it is important that we account for state level policy differences. We show that not accounting for differential state policies over time may lead to erroneous causal inferences. Recent works on impact of trade liberalization (e.g., Dix-Carneiro and Kovak, 2017; and Carneiro, Soares, and Ulyssea, 2018) emphasize the importance of allowing differential state trends in the federal structure. In the case of educational outcomes in India, accounting for changing state policies is very important as prima facie education is a state subject. In 1976, an amendment was added to the Article 42 of the constitution moving education to the concurrent list which enabled the central government also to legislate in for education.[5] Nonetheless, state governments continue to

[4]While EPT (2010) main results are based on the district wise exposure, because of data limitations, theirs placebo results are based on the state-region level exposure where state-regions are aggregation of several districts.

[5]Indian Constitution clearly demarcates legislative/administrative powers for federal and state govern-

have the primary responsibility with more than 80 percent of education expenditure is borne by the state governments. For example, 2,688 billion out of a total 3,403 billion Indian Rupees expenditure on education during the year 2013-14 incurred by the education departments of both central and state governments came from state coffers (Government of India, 2019). Moreover, there were substantial variation in the growth of per capita education expenditure across states during the 1980s and 1990s (see online appendix Figure A1) that also need to be accounted for when assessing the growth of human capital across districts that belong to different states. World Bank (2004) finds that inter-state changes in public spending on elementary education per child during 1980-1999 were positively associated with changes in enrollment rates among 6-14 years over time.

Moreover, a strand of existing literature on trade liberalization in India is based on state-level policy differentials. For example, Besley and Burgess (2004) examine the role of labor market regulation in explaining differential manufacturing performance in Indian states between 1958 and 1992. Aghion, Burgess, Redding, and Zilibotti (2008) find that following delicensing, industries located in states with pro-employer labor market institutions grew more quickly than those in pro-worker environments. Chaudhuri and Ravallion (2006) find substantial cross-state variation in growth of state domestic product between 1978 and 2004. The state of Bihar only experienced a growth rate of 2.2% during this period, while the best performer state, Karnataka, experienced annual growth rate of 7.2%. Since change in policy or administrative capacity over time differs across states, there is no reason to believe that there is a common national trend, especially in the educational outcomes where states remain the main policy maker and budget provider.

The findings of the paper are striking. Using the cohort wise analysis, we find that reduction in tariff protection has no impact on either aggregate schooling or completion of different stages of schooling. We also find that the use of point estimates from the unaffected cohorts as placebo may not be sufficient evidence of causal relation in case of

ments by providing union and state lists, separately. It also provide a concurrent list which includes the powers to be considered by both the federal and state governments.

omitted variables. It helps if an additional cross-section data is available which can be used to carry out additional placebo. In our case, the placebo carried out on pre-liberalization period census, i.e. 1991 Census, is able to demonstrate strong pre-existing trends, helping us avoid making erroneous inferences based on the cohort wise analysis form a single cross-section data. Our difference-in-difference estimates from the traditional set up, i.e. comparing pre and post data, also do not find any impact of trade liberalization on human capital accumulation. We also find evidence of increased job opportunities for school going age group, especially in urban areas. Given no impact on schooling outcomes, we conjecture that increased returns of schooling may have mitigated the negative effect of increased opportunity cost of schooling. We provide suggestive evidence to support our conjecture.

The paper is organized as follows. Section 2 describes the data and construction of variables. Section 3 states the empirical framework, while section 4 presents the results. Section 5 explores potential mechanisms, and Section 6 concludes.

2 Data

We use four decennial Indian censuses collected in 1981, 1991, 2001, and 2011 as our main data source. We construct a district-level panel using the decennial censuses covering four decades from 1981-2011. A district is an administrative division of a state in India and various studies in India have used districts as local labor markets to study the impacts of trade exposure on various outcomes (e.g., Topalova, 2007, 2010; Edmonds, Pavcnik, and Topalova, 2010). Topalova (2010) documents little migration across Indian districts and show that neither migration nor the level of population are correlated with the change in the trade protection that a district experienced over the 1990s. Overtime new districts are created either by dividing or reorganizing existing districts. We trace the 2001 and 2011 districts back to the 1991 Census districts by combining the broken up districts, and our full sample comprises of 452 Census 1991 districts. In 1991, there were 462 districts in India,

however, the 1991 Census did not cover the state of Jammu and Kashmir (14 districts). We also use the 1981 census to conduct a placebo for our difference-in-difference analysis.[6] The 1981 census did not cover the state of Assam (23 1991 Census districts) further reducing the number of districts to 423 for placebo analysis. The Indian census provide information aggregated for different age groups and we work with the provided age groups to define our cohorts. Indian census reports education in terms of completion of different stages of schooling. We also create years of schooling variable using the stage of schooling completed information. Table 1 provides primary and tertiary completion rates among different age groups in 1991, 2001, and 2011 census data.[7] It also reports the years of schooling completed by different age groups in different censuses.[8]

Our tariff data is obtained from the UNCTAD Trade Analysis and Information System (TRAINS) at the 6-digit level of the Indian Trade Classification Harmonized System for around 5000 product lines. Similar to Topalova (2007, 2010), we match these product lines to National Industrial Classification (NIC) codes using the concordance table provided in Debroy and Santhanam (1993), to get a relatively precise measure of average industry-level tariffs. We use the 3-digit NIC industrial composition of employment at the district level from the 1991 Census to construct our employment weight.

2.1 Measurement of district exposure to the trade liberalization

Following the extensive literature (e.g., Topalova, 2010; EPT, 2010), a district's tariff protection is calculated as the 3-digit NIC industry level tariff weighted by the 1991 Census

[6]The 1981 Census is provided by Reeve and Barnes (2000). Few Census 1991 districts were created by breaking up the Census 1981 districts. In that case we break up the Census 1981 districts into the Census 1991 districts by using the population share of 1991 district in the parental 1981 district.

[7]For the 1981 Census, we do not have access to age group wise data for education completion at the district level. The PDF documents related to 1981 Census available on Census of India web page do not contain this information.

[8]The years of schooling are assigned in following way: illiterate- 0; literate without formal degree/ below primary school- 1; primary school- 5; middle school- 8; secondary- 10; higher secondary- 12; non-technical diploma- 12; technical diploma- 13; and graduate- 15. A very small fraction of population is categorized as unclassified education. Zero years of schooling is assigned for unclassified category.

3-digit industry employment in the district.

$$TR_{d,t} = \frac{\sum_i worker_{d,i,1991}}{Total\ worker_{d,1991}} * Tariff_{i,t}$$

where $TR_{d,t}$ is effective tariff protection for district d at time t, $worker_{d,i,1991}$ is total employment in industry i in district d in 1991, $Total worker_{d,1991}$ is the total number of workers in district d, and $Tariff_{i,t}$ is tariff applicable to industry i at time t. In the above measure, similar to Topalova (2010) and others (e.g. EPT, 2009, 2010), non-traded industries (services, trade, transport, and cultivation of cereals and oil seeds) are assigned a zero tariff change for the entire period.[9] Using the terminology of Topalova (2010), we call the above tariff measure "scaled tariff".

As argued in the literature (e.g., Topalova, 2010; EPT, 2009, 2010), the above scaled tariff measure may be correlated with initial poverty levels, and Topalova (2010) and others have used *"nonscaled tariffs"* to instrument for "scaled tariff." The *nonscaled tariffs* ignores the workers in the non-traded production sectors. Instead, it uses only those employed in traded production sectors to weight the tariff measure. Thus, a district in which 2 percent of the workers are employed in traded production sectors will have the same measure of nonscaled tariff as a district in which all workers are in traded production sectors if the sectoral composition within traded production sectors is the same in both districts. (Topalova, 2010). The nontraded tariff, $TrTR_{d,t}$, defined as

$$TrTR_{d,t} = \frac{\sum_{i \in traded} worker_{d,i,1991}}{\sum_{i \in traded} worker_{d,i,1991}} * Tariff_{i,t}$$

$TrTR_{d,t}$, *nonscaled tariffs*, ignores the workers in the nontraded production sectors (services, trade, transport, and cultivation of cereals and oilseeds). Instead, it uses only those employed

[9]Topalova (2010) argues that during their study period (1987-1999), the imports of cereals and oilseeds remained canalized and only government agencies were allowed to import these items. Moreover, no change in their tariff rates was observed (the tariff rate for cereals was set at zero). Thus, they were de facto nontraded goods.

in traded production sectors to weight the tariff measure (Topolva, 2010). For cohort-wise analysis the change in tariff is calculated in following way:

$$\Delta TR_d = \frac{\sum_i worker_{d,i,1991}}{Total\ worker_{d,1991}} * \{tariff_{i,1997} - tariff_{i,1991}\}$$

Table 2 presents the change in effective tariff protection enjoyed by the districts. The district at the tenth percentile of effective tariff change distribution faced a tariff reduction of 1.4 percentage points, while the district at the ninetieth percentile of the effective tariff change distribution faced a 12.9 percentage points reduction in tariff protection. On average, districts faced a tariff cut of 5.9 percentage points.

Our measure of reduction in protection focuses on tariff only and ignore non-tariff barriers (NTBs). EPT (2009, 2010) and Anukriti and Kumler (2019) also ignore the changes in NTBs citing data availability issues. As pointed out by Anukriti and Kumler (2019) and EPT (2009, 2010), ignoring NTBs is potentially problematic if the trends in NTBs were in the opposite direction compared to tariffs. However, tariffs and NTBs during the 1990s are positively correlated (EPT, 2010, Topalova, 2010). Hence, some of the impact that we attribute to tariff declines may be because of NTBs declines.

2.2 Identification of cohorts affected by the trade liberalization

Indian education system is divided into different stages of schooling: primary (grades 1-5), middle (grades 6-8), secondary (grades 9-10), senior secondary (grades 11-12), and tertiary (grades 13-15 or more). The relevant age groups for primary, secondary, senior secondary, and tertiary are 6-10, 11-13, 14-15, 16-17, and 18-20 or more, respectively. However, the period of 1990s was also characterized by over age enrollment. Hence a significant proportion of students started schooling later than age 6. The completion rate of primary rate is highest among 14 year old age cohort across all three censuses (Table 1). If students complete primary school on time, by age 11, we expect higher completion rate among 11 or 12 year old cohort

compared to 13 or 14 year old age cohort, assuming that there is a secular improvement in the completion rate over time. Similarly, from Table 1, it is clear that the tertiary completion rate is highest among 25-29 year old cohort.

Table 3 identifies the period when different age-cohorts in the 2011 Census were attending/finishing different stages of schooling. So, anyone aged 40 and above in 2011 should have completed all stages of schooling before 1991 and should not be affected by the liberalization. For the primary school completion outcome, individuals aged 13-29 in 2011 were likely have been affected from liberalization regarding their primary schooling decision. Because of over age enrollment, some fraction of individuals aged 30-34 in 2011 may also have been exposed to liberalization during their primary attendance period. Age groups 20-24 and 25-29 in 2011 should be the main beneficiaries for primary school completion outcome as they attended primary school in the 1990s. Similarly, for secondary school completion outcome, individuals aged 15-34 in 2011 were likely to be exposed to the liberalization. For tertiary school outcomes, individuals in age group 20-39 were exposed to the liberalization. Obviously for tertiary school outcome, the years of schooling individuals attended post 1991 varies for different age groups. Individuals in age group 20-29 in 2011 attended all years of schooling post-1991, while individuals aged 35-39 only attended tertiary post-1991. One advantage of our generalized specification (discussed in the next section) is that it allows the impact to vary across different age cohorts potentially capturing the impacts of differential exposure to the liberalization.

3 Empirical Framework

3.1 Cohort wise analysis

Following Duflo (2001), we exploit the fact that different districts experienced different effective tariff reductions and few cohorts attended schools after liberalization and few cohorts

already have completed their schooling by the time of liberalization.[10] The cohorts most likely to be affected by liberalization for different stages of schooling is identified in Table 1 and discussed in the last section. Taking a generalized interaction form:

$$y_{cds} = \alpha + \tau_c + \varphi_d + \sum_{c=1}^{k-1}(\Delta TR_d.d_c)\gamma_{1c} + \sum_{c=1}^{k-1}(X_{d91}.d_c)\gamma_{2c} + \sum_{s=1}^{j-1}\delta_s.trend + \epsilon_{cds} \qquad (1)$$

where y_{cds} is share of individuals in cohorts c living in district d and state s that have achieved a particular level of schooling, τ_c is cohort fixed effects, and φ_d is district fixed effect. d_c is an indicator variable that takes a value of one if birth cohort is c, and ΔTR_d is the change in effective tariff protection that a district d experienced between 1991 and 1997. X_{d91} is a matrix of district level characteristics in 1991 and include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, employed in services (construction is the omitted category), the share of district's population that is a Scheduled Caste/Tribe, and the percentage of literate population in a district. δ_s is an indicator for state s and $trend$ is a trend that captures secular change across cohorts.[11]

$\delta_s.trend$ allows the secular trend to vary across states and account for state policies that may change at different rates across states.[12] There are many reasons for allowing state-specific trends. First, as pointed out in the introduction that most of the education expenditure comes from state coffers and the growth of per child elementary education varied considerably across states in the 1980s and 1990s. Second, education policy decisions

[10] As stated earlier, the inter-district migration remains low in India. Hence for the majority of individuals, the reported residence districts should be the districts where they attended schools.

[11] We have 18 state indicators that contain 16 major states (97.4% of total population). Since union territories are administered directly by the central government, we grouped the union territories of Chandigarh, Delhi, Pondicherry, Andaman & Nicobar islands, Dadra & Nagar Haveli, Daman & Diu, and Lakshadweep (each one contains 1-3 districts in the 1991 Census) together as a single state. Similarly, we grouped the smaller northeastern states of Arunachal Pradesh, Manipur, Meghalaya, Mizoram, Nagaland, Sikkim, and Tripura together as one state. Our results are invariant to restricting the sample to 16 major states. The trend variable is based on age cohorts. It takes a value of 1 for 55-59, 2 for 50-54, ..., 9 for 15-19, and 10 for 13-14 age cohort.

[12] Alternatively, we also control for state-age cohort fixed effects that allows much flexible changes across states. The results are similar.

such as curriculum, new capital investments, and teacher salary or hiring are made at state level with little say of district administration. Third, given the federal structure in India, majority of the policy making and administrative powers remain with states, and institutional/administrative efficiency differs not only across states but also over time for the same state. Hence, allowing for state-specific trends accounts for varying state-level policies over time, which are also likely to be correlated with educational outcomes in districts subject to same policy changes. Adding state-specific trends means our focus in on the variation in tariff protection across districts within the same state.

Our sample include individuals in 13-59 age group in the 2011 Census. The indicator for cohort aged 55-59 in 2011 and all its interaction terms are dropped. In the generalized form, the γ_1's capture the impact of change in tariff on the completion of cohort c schooling. Since ΔTR_d is a change in tariff over 1991 and 1997, a negative γ_1 will imply that tariff-reduction lead to increased school completion. To account for the correlation in outcomes across districts but within state-age group, we cluster our standard errors at the state-age group level. For outcomes, we consider completion of different stages of schooling and aggregate years of schooling. In the generalized form, the γ_1's for cohorts that completed schooling before 1991 can serve as placebo, and one would expect a zero coefficient on interaction terms for cohorts who completed schooling before liberalization. In addition, we carried out an additional placebo by estimating the equation (1) using the 1991 census data. As reform was yet to be implemented in 1991, we expect the coefficients on interaction terms, $\Delta TR_d * d_c$, to be not different than zero for all cohorts. Following the literature, we also instrument change in scaled tariff (ΔTR_d) by change in nonscaled tariff ($\Delta TrTR_d$).

3.2 Pre and post analysis

We also use a traditional difference-in-difference setup using the pre and post reform data. We estimate the following equation

$$y_{dst} = \alpha + \beta.TR_{d,t} + \varphi_d + \lambda_t + X_{d91}.\lambda_t + \sum_{s=1}^{j-1} \delta_s.\lambda_t + \epsilon_{dst} \quad \forall t = 1991, 2001, \text{or } 2011 \qquad (2)$$

where y_{dst} is either share of individuals in district d and state s that have achieved a particular level of schooling in census t or aggregate years of schooling in district d in census t. λ_t is a indicator that takes a value 1 for post-1991 period, and account for the average change in outcome in the omitted state. The district fixed effect, φ_d, account for time-invariant district characteristics. The pre-reform conditions are interacted with the post reform indicator to capture any differential trend based on pre-reform conditions in a district. In addition, we also allow for differential trends across states to account for changes in state-specific policies (for example, differential growth in per capita education expenditure). The parameter of interest is β, and is identified under the assumption that unobserved district-specific time varying shocks that affect schooling outcomes are uncorrelated with changes in district tariffs over time, and a negative β would suggest that tariff reductions over time are associated with an increase in schooling in districts that experienced relatively larger tariff cuts compared to districts that experienced smaller tariff cuts. Similar to others, we use the 1991 tariff for year 1991 ($TR_{d,1991}$), but the 1997 tariff for year 2001 or 2011 data (i.e, $TR_{d,1997}$).[13] As stated earlier the existing literature has shown that only changes in tariff between 1991 and 1997 can be taken as exogenous. To account for the correlation in outcomes across districts, we cluster the standard errors on state-year level.[14]

[13]Others such as EPT (2009, 2010) and Topalova (2010) also use the 1997 tariff for their 1999 data.

[14]There are 36 state-year clusters. Cameron, Gelbach, and Miller (2008) argue that clustered standard errors are correct as the number of clusters approaches infinity. However, in practice, having 30 to 40 clusters is like approaching infinity. Alternatively we also clustered the standard errors at the state-region (74 clusters) level. State-regions are constructed by National Sample Survey by grouping contiguous districts within same state with similar agroclimatic conditions and socioeconomic characteristic. Overall, the conclusions remain same.

Equation (2) is similar to the specification used in EPT (2009, 2010) with the exception that we allow state-specific trends. Since EPT (2009, 2010) use individual level survey data for their school attendance outcome among 10-14 year old, they control for few individual characteristics in their regression. In contrast, we use aggregated census data and do not account for individual heterogeneity.[15] We also carry a placebo analysis estimating the same equation (2) on 1981 and 1991 data falsely assuming 1991 is the post reform period.

4 Results

4.1 Cohort wise analysis

In Table 4, we report the results for our cohort wise analysis for the primary school completion outcome. All the models in Table 4 control for the district and cohort fixed effects, and for brevity we only report the coefficients on interaction terms of cohort indicators with change in tariff measure which are our main interest parameters. Column (1) are estimates from the OLS model that does not allow for cohort effects to vary by initial district conditions or differential state trends. The coefficients are positive, and importantly those are statistically significant for only the age groups whose schooling decision may have been affected by trade-liberalization either at intensive or extensive margins or both except for the age group 25-29. The coefficients on interaction terms for age cohorts 30-34 or higher are relatively small and statistically insignificant. Positive and statistically significant coefficient on an age-group implies that the age-group that reside in a district that experienced a relatively larger tariff reduction experienced a lower primary school completion compared to the same age-group that reside in a district than experienced lower tariff reduction. In column (2), we allow for cohort effects to vary based on district's initial conditions (interaction of cohort indicators with district's 1991 characteristics). Strikingly, the coefficients on interaction terms flip

[15] Topalova (2010) use aggregated mean of consumption expenditure at the district level. The variation in tariff is at district-year level and is similar to EPT (2009, 2010) and Topalova (2010).

signs. Moreover, the coefficients also become statistically insignificant except for age 13 and age 14. In column (3), allowing state-specific differential trends reduces the magnitude of the negative coefficients, and none of the coefficients remain statistically significant.

In column (4) of Table 4, following EPT (2009, 2010) and Topalova (2010), we instrument change in scaled tariff (ΔTR_d) with change in nonscaled tariff ($\Delta TrTR_d$).[16] Column (4) allows for cohort effects to vary by initial district conditions, but does not allow differential state trends. The column (4) specification is similar to EPT (2010) and Topalova (2010) with the exception that they compare pre and post reform data for their difference-in-difference estimate. The effects of change in tariff are positive and show statistical significance only for the age-groups who we believe should have been affected by the reforms. Recall that the age group 20-24 and 25-29 in 2011 were the most likely beneficiary for the primary school outcome (Table 3). The coefficients on both terms are positive, and both economically and statistically significant. While the coefficients on individual age categories for ages 13-18 are positive and large, they are not statistically significant at 5% significance level. Individuals in age group 13-18 in 2011 Census attended their primary schooling in early to late-2000s (Table 3). The IV results without state trends (column, 4) also show that coefficients on interaction terms are not statistically significant and relatively small for age group 30-34 and older. Normally, the results of the column (4) are considered strong evidence of causal effects as the coefficients on interaction terms are only different than zero for cohorts that are exposed to liberalization and statistically insignificant for cohorts whose educational decisions should not have been affected as they already have completed primary school by the time of liberalization. Importantly, these results are in line with the school attendance results of EPT (2009, 2010) i.e., the districts that experienced a larger decline in the tariff experienced a slower growth in primary completion.

We carry out additional placebo to validate the results reported in column (4) of Table 4. We basically estimate the similar specification using the 1991 Census in place of the 2011

[16]Our first stage correlation is statistically significant and similar to what is reported in the literature. Results are available on request.

Census. The results are reported in column (9) of Table 4. Since the outcome information was collected in 1991, none of the cohorts should have been impacted by trade-liberalization, and ex-ante we expect the coefficients on interaction terms should not be different than zero. However, the results from the column (9) are striking and show that the coefficients on interaction terms using the 1991 Census almost mimic the coefficients using the 2011 Census reported in column (4).[17] The coefficients on interaction terms for age group 20-24 and 25-29 are positive and statistically significant. The coefficients on interaction terms for age 40-44 and above are statistically insignificant. These results rather than validating the column (4) causal interpretation cast serious doubts. In column (5), we allow for differential state trends in the IV estimation using the 2011 Census, and all the coefficients on interactions terms except for the coefficient for age 14 lose statistical significance. Importantly, the coefficients for single age categories for age 13-19 flip signs, while the magnitude of the coefficients for age 20-24 and 25-29 becomes almost zero. One could argue that allowing state trends reduces the variation in the change in tariff variable, however, flipping the signs of the coefficients suggest that column (4) estimates are not stable. In column (10), we estimate similar specification as column (5) using the 1991 Census. None of the coefficients on interaction terms are statistically significant. Based on the results including state-specific trends and placebo carried out using the 1991 Census, it is evident that trade-liberalization did not have an impact on the primary school completion.

In Table 5, we provide estimates for rural and urban areas, separately. Column (2) provides IV estimates without state trends for rural areas. As evident, the coefficients on interaction terms are positive and statistically significant at 5% significance level for all age groups below age 30 with the exception for age group 16 which is statistically significant at 10% level. These are the age groups that are supposed to be affected by trade liberalization for the primary school completion outcome (Table 3). The coefficients for age groups that completed schooling before 1991 are statistically insignificant. These results are very similar

[17]The 1991 Census does not provide disaggregated information for age 15-19, hence a grouped 15-19 category is used in the analysis.

to EPT (2010) results for rural areas. They find rural areas of districts that experienced a larger tariff reduction saw slower growth in school attendance among 10-14 year old compared to the national trend. In column (5), we estimate the same IV model without state trends with the 1991 Census and find a very similar pattern in the coefficients of interaction terms. Hence, we cannot infer that the point estimates on interaction terms in column (2) are picking up causal relation. In column (3), we allow for differential state trends. The signs of the coefficients flip and none of the coefficients are statistically significant with the exception of coefficient on age 14 term. Thus, we do not find evidence of any impact of trade liberalization on primary school completion in rural areas.

In column (8) of Table 5, we provide IV estimates for urban areas without state specific trends. The coefficients on interaction terms for age groups that are supposed to be affected by trade liberalization are not significant suggesting no impact in urban areas. This is in contrast to the results of EPT (2009) for urban areas. They find that school attendance among 10-14 year old is attenuated by 8 percentage points in the urban areas of state-regions with the average tariff change relative to urban areas of state-region with no change. It is worth mentioning that their estimation specification is different than our specification. In the next section, we will discuss a similar specification as theirs. Controlling state trends in column (9) of Table 3 does not change the conclusion. So, in urban areas also, we do not find evidence of an impact of trade liberalization on primary school completion.

In Table 6, we present the results for secondary school completion. A person should complete secondary school (grade 10) by the age of 16. Hence, we expect anyone in age group 15-34 in 2011 would have attended secondary school post 1991 (Table 3). Age group 25-29 and 30-34 in 2011 Census attended their secondary school during the 1990s. One would expect that the age group 15-19 and 20-24 also benefit though they attended secondary during 2000s as they started their schooling in the 1990s. Column (2) of Table 6 presents the results of IV estimation without state-specific trends. The coefficients on interaction terms for age group 15-19, 20-24, and 25-29 are positive and relatively large, although only the

coefficient for age group 25-29 is statistically significant. These results provide weak evidence of a negative impact of liberalization on secondary school completion. However, a placebo analysis using the 1991 Census throws up similar results negating any casual interpretation (column 5 of Table 6). In Column (3) of Table 6, we introduce state specific trends. The coefficients of interaction terms flip sign from positive to negative. Only the coefficient for age group 15-19 and 40-44 are statistically significant. Although, a placebo with state trends using the 1991 Census show no significant coefficient for interaction terms (column, 6), given the absence of any pattern in the impact in column (3), we conclude that there is no impact of liberalization on the secondary school completion. Column (7) of Table 6 that focuses on rural areas show similar result, i.e. only the coefficients on age group 15-19 and 40-44 are statistically significance. For urban areas, the coefficient for age group 25-29 is statistically significant and a negative sign of the coefficient suggests positive effect of trade liberalization on secondary completion (column 9 of Table 6). Given the absence of any clear pattern of impact on other age groups that potentially attended secondary school after 1991, it is not clear that the negative coefficient of one age-group is picking up the causal relationship.

In Table 7, we present the results for tertiary completion and focus on IV specifications with state trends. We expect that schooling decisions of individuals in age 20-34 potentially might have been affected as they attended tertiary after 1991 (Table 3). However, since getting enrolled in tertiary is conditional on finishing senior secondary, we expect the impact should be concentrated mostly in age group 20-24 and 25-29 as they took all schooling decisions post 1991. Only for age group 20-24 the coefficient of interaction term is statistically significant (column 3 of Table 7). Moreover, a placebo using the 1991 Census does not show any significant trend (column 6 of Table 7). Although the sign of the coefficient for 20-24 age group is negative suggesting positive impact of liberalization for tertiary outcome for age 20-24, the magnitude of the impact remains small. A district which witnessed an average tariff cut of 5.8 percentage points will have a higher tertiary completion by 0.7 percentage points among 20-24 old compared to a district that did not experience any tariff reduction.

In column (7) and column (10) of Table 7, we present the results for tertiary completion for rural areas and urban areas, separately. None of the coefficients on interaction terms are statistically significant in urban areas. For rural areas, the coefficients on interaction terms for age 20-24 and age 40-44 are statistically significant and have similar sign and magnitude making it difficult to get a conclusion. Nonetheless, the magnitude on the coefficient for age group 20-24 is very small.

In Table 8, we present the results for years of schooling outcome and focus on IV results. The IV estimates from specification that does not account for state specific trends (column, 2) finds a positive and statistically significant coefficients for interaction terms for age group 20-24 and 25-29. Moreover, the coefficients on interaction terms for age 40 and above is relatively very small in magnitude and statistically not different than zero. However, an additional placebo using the 1991 Census (column, 5) shows a pre-existing trend negating any causal interpretation in column (2). Adding state-specific trends flips signs of the IV estimates and there is no evidence that trade liberalization had any impact on the years of completed schooling.

Overall, based on cohort wise analysis, it is evident that trade liberalization did not have any impact either on the completion of different stages of schooling or on the aggregated years of schooling. The analysis also emphasizes accounting for state level differences in policies in a federal structure. The specifications that did not account for state level differences lead us to the conclusion that trade liberalization has a negative impact on human capital accumulation. However, a placebo using the past census before liberalization basically gives the same conclusion invalidating the causal interpretation of the specification without differential state trends. In the next subsection, we present the results using pre and post difference-in-difference strategy.

4.2 Pre and post analysis

Table 9 presents the results for primary completion using the traditional difference-in-difference set up with district-level panel data. For primary completion we focus on age group 13-19. A person should complete primary education by age 11. However, to allow for over age completion we chose a lower cut of 13 years of age. Since the 1991 Census, which serves as our baseline data, only reports aggregated information for age 15-19, we could only look at the age 13-19 group. As shown in appendix Table A1, 13-19 age cohort in 2001 Census should have started their schooling between 1988 and 1994, and should have finished their primary school between 1992 and 1998 assuming that they started schooling on time (at age 6). Thus, the 13-19 age cohort in 2001 Census consists of two types of individuals 1) whose enrollment and completion of primary school was done post 1991; 2) who were already enrolled but their completion decision was influenced by liberalization.

Panel A of Table 9 reports results for all areas. Column (1) presents the estimates of OLS model that do not include state-specific trends and initial district conditions interactions. The OLS estimate is statistically significant and positive suggesting that larger tariff-cuts is associated with lower primary school completion rate. Controlling for initial district conditions reduces the OLS coefficient significantly and it is no more statistically significant (column, 2). Adding state-specific trends reduces the OLS estimate to about zero. The IV estimate in column (4) that does not allow state-specific trends is positive and statistically significant. Moreover, the estimate is economically significant. A district that experienced tariff cut at 90th percentile will have 4.5 percentage points lower primary completion compared to a district that experienced tariff cut at the 10th percentile. Column (8) presents a placebo analysis using the 1981 and 1991 data. As stated earlier the placebo is carried out for all ages because of data availability. The IV estimate for the placebo in column (8) is positive and statistically significant. Since placebo is reported for all ages, in column (6) we report IV estimate without state-specific trends using all ages to see whether the different sample is driving the results. The IV estimate for all ages in column (6) is positive and sta-

tistically significant suggesting that moving from 13-19 age group sample to all ages sample does not affect the sign of the impact. Understandably, the magnitude of the impact is lower for all ages sample than the estimate in column (4) for 13-19 age group. The similar signs and statistical significance of IV and placebo estimates for all ages (column 6 and column 8) rule out causal interpretation of the IV estimate in column (4).

In column 4(a), Panel A of Table 9 we add state-specific trends to account for differential state policies while excluding interactions of district indicators with initial conditions. The point estimate not only loses its statistical significance, but the magnitude of the point estimate gets very small. This indicates that states that faced larger reduction in effective tariff also displayed other time varying characteristics that contributed to slower growth in primary completion biasing the coefficient.[18] In column (5), Panel A of Table 9, we add both interactions of district indicators with district initial conditions and state-specific trends. The IV estimate flips sign from positive to negative and is statistically significant only at the 10% significance level. Moreover, the magnitude of the coefficient is reduced significantly. Column (9) present placebo IV estimate with state-specific trends. The placebo estimate is small but positive and statistically insignificant suggesting that the IV estimate with the state-specific trends is picking up a causal relation. IV estimate using all ages also maintain negative sign further increasing confidence in the IV estimate in column (7). Although the coefficient is not large enough to make much economic impact (note that on average, districts experienced only 5.8 percentage points tariff cut), the conclusions drawn is totally different once we allow differential state governments polices over time.

In Panel B of Table 9, we present the results for rural areas. We find qualitatively similar results as for all areas. EPT (2010) whose main specification is same as reported in column (4) finds positive and statistically significant IV estimate. However, their placebo was carried out at the state-region level because of data constraint. Their placebo IV estimate

[18]We are not precluding the impact of liberalization on state level primary completion, however, in this setup it is difficult to eliminate the effects of state-policies to tease out the impact at the state-level educational outcome.

was negative and statistically insignificant. Based, on that they conclude that the trade-liberalization has negative effect on school attendance among 10-14 year old in rural areas. Although our outcome and data are different, a placebo at the same (district) level picks up the pre-existing trend. Moreover, allowing for state-specific trends results in flipping of sign of the IV estimate from positive to negative.

In Panel C of Table 9, we present the results for urban areas. Using the comparison between 1991 and 2001, none of the estimates are statistically significant. Moreover, the IV estimate flips sign once we allow for state-specific trends. These results are consistent with our cohort wise analysis, but quite different than the EPT (2009). It is worth mentioning out that EPT (2009) use state-region level aggregation for urban areas and their outcome, primary school attendance among 10-14 year old, is based on sample data. Their main specification is similar to specification reported in column (4), i.e., IV without state-specific trends. Their finding suggests negative impact of liberalization in urban areas too similar to EPT (2010) finding for rural areas. We do not find any impact of liberalization in urban areas on primary completion even in IV estimation without state-specific trends.[19]

In column (10) and (11) of Table 9, we present the IV estimates comparing the 2011 Census with the 1991 Census. We focus on age group 20-24 as individuals aged 20-24 in the 2011 Census should have started primary schooling during 1993-1997 and finished up the primary school during 1997-2001 (appendix Table A1). Hence, liberalization should have influenced both the enrollment and completion decisions for this age group. Similar to our earlier results, the IV estimates without state-specific trends are positive and statistically significant in both rural and urban areas. Recall that similar estimate for urban areas using 13-19 age group sample from 2001 Census was not significant. Nonetheless, the introduction of state-specific trends flips the signs of the IV estimates for rural and all areas. Although the positive sign of IV estimate holds for urban areas, the magnitude of the estimate becomes much smaller, and it loses its statistical significance. So, these results support the earlier

[19] A lower attendance among 10-14 year old may not necessarily leads to a lower primary school completion if primary students stay longer in schools in districts that experienced larger tariff reductions.

findings from the cohort wise analysis that the liberalization has no impact on the primary school completion.

In Table 10, we present the results for the tertiary completion outcome. Since largest proportion of tertiary completion is reported in age group 25-29 (Table 1), we focus on this group in the analysis.[20] As shown in appendix Table A1, the age group 25-29 in 2001 Census should have attended tertiary education during 1991-94, while the age category 25-29 in 2011 Census should have started schooling during 1988-1992 and hence should have been influenced by liberalization at each stage of schooling. Column (4) presents the IV estimate without state-specific trends that look at the changes between 1991 and 2001. The IV estimate is positive and statistically significant for all areas and rural areas. Adding state-specific trends reduces the magnitude of the IV estimate considerably although it maintains the positive sign. In practical terms the impact is very small given the average change in tariff of 5.8 percentage points. Column (10) and column (11) present the IV estimates that look at changes between 1991 and 2011. The coefficients are positive and statistically significant for all areas and rural areas. Adding state-specific trends not only reduces the magnitude of the coefficients considerably but also flips the signs suggesting no impact on tertiary completion in rural areas. The IV estimate for urban areas retains the negative sign but remain statistically insignificant at 5% significance level. So, we cannot infer a positive effect of trade liberalization on tertiary school completion in urban areas.

In Table 11, we report the results of years of schooling outcome. For this we focus on age group 20-29. One should complete education by age 21 since the highest category reported in data is graduate degree. Moreover, age 20-29 in Census 2001 should have attended secondary/tertiary in the 1990s, while age group 20-29 in Census 2011 should have been influenced by liberalization for most of their education (appendix Table A1). Results for years of schooling outcome for rural areas are similar to the results presented earlier. The IV estimate without state-specific trends is positive and statistically significant. Adding state-

[20] One should complete tertiary degree by age 21 or later assuming no delay in schooling. The age categories given by census is either 20-24 or 25-29.

specific trends flips the sign of the IV estimate and it is no more statistically significant. For urban areas, when we look at the 2011 Census, the IV estimate with state-specific trends is negative and statistically significant suggesting positive impact of liberalization on years of schooling outcome (column, 11 of Table 11). However, the magnitude of the impact is quite small to make any practical difference. Districts that experienced an average tariff cut (5.8 percentage points) will have 0.1 more year of schooling compared to districts that do not experienced tariff cuts.

Overall, our pre and post difference-in-difference estimates provide similar conclusions as provided by the cohort wise difference-in-difference estimates. Based on these results, we conclude that the trade liberalization in India did not have any noteworthy impact on the human capital accumulation in India.

5 Mechanisms

In literature, two potential mechanisms are discussed through which liberalization can affect human capital. The first is returns to schooling and the second is the opportunity cost of schooling. In Table 12, we look at the effect of change in tariff protection on log of number of workers, log of population, and workforce participation rate among 5-14 and 15-19. The use of 5-14 age group is necessitated by the fact that the 1991 Census provides workforce information based on 5-14 age aggregation. The results in Table 12 are based on comparison of 2001 Census against 1991 Census (Equation 2), and we focus on IV results with state-specific trends. Since we do not have similar age wise data from 1981 Census, we do not carry out the placebo. Hence, we avoid making causal inference and emphasize that the results reported in Table 12 are only suggestive evidence. Column (1)-(4) of Table 12 presents results for number of workers. The IV estimates (with state trends) are negative for both rural and urban areas suggesting that number of jobs among both age groups increased more in districts that experienced larger decline in tariff protection. However, the

IV estimate is statistically significant only for age group 5-14 in urban areas. Notably, the increases in number of jobs are not driven by population changes as the point estimates for population outcome are relatively small and, in few cases, have opposite signs (column 6 and 8). Consistent with the findings of EPT (2009), the workforce participation decreased less in urban areas of districts that experienced larger tariff decline (column 10 and 12).[21] Using the state-region variation in tariff, they find that regions that saw a smaller decline in tariff protection experienced a larger decline in the probability of a child working in age group 10-14.

EPT (2009) conjecture that one of the potential explanations for less decline in workforce participation among 10-14 is attenuated decline in poverty in urban regions with larger tariff declines found by Topalova (2010). Although, using state-level data, Hasan, Mitra, and Ural (2007) conclude that states whose workers are more exposed to foreign competition tend to have lower rural, urban and overall poverty rates (and poverty gaps). Since Census do not collect income and consumption data, we cannot confirm the poverty findings for urban India at district-level. Hence, we cannot preclude the role of poverty on slower reduction in workforce participation in urban areas. However, our results also suggest increased job opportunities for children in urban areas raising the opportunity cost of schooling. In rural areas also, there are suggestive evidence of increased opportunity cost of schooling. We find weak evidence that job opportunities for children grew more in districts that witnessed larger reduction in tariff protection. Moreover, Topalova (2010) finds that poverty declined less in rural areas of districts that saw larger tariff reductions reinforcing the increased opportunity cost arguement. Recent evidence from India's flagship National Rural Employment Guarantee Scheme also show that increasing employment opportunity lowers relative school enrollment and increases child labor in treated districts (e.g., Shah and Steinberg 2019; Li and Sekhri, 2020). Using similar data and methodology as EPT (2010), Bai and Wang (2020)

[21]The Child Labor (Prohibition and Regulation) Act, 1986 makes employing a child, defined as a person below the age of 14, for any work a cognizable criminal offense. However, 10.1 million out of the total of 259.64 million children in 5-14 age group in 2011 Census are reported to be working.

find that reduction in tariff protection for child intensive crop (equivalent to decreased opportunity cost of schooling) is associated with increased attendance among 10-14 year old in Rural India.

In our case, increased opportunity cost of schooling with no effects on schooling outcomes suggests that probably the negative effects of increased opportunity cost of schooling on schooling outcomes are mitigated by the potential positive effects of increased returns to schooling on schooling outcomes. Increasing returns to schooling is associated with improved schooling outcomes. For example, arrivals of high-skilled job opportunities have shown positive enrollment impacts (e.g., Shastry, 2012; Jensen, 2012; Oster and Steinberg, 2013). There is evidence of increasing college premium (Azam, 2010) and returns to schooling (Kijima, 2006) during the 1990s in urban India, however, establishing a link between returns of schooling in districts and tariff exposure is challenging given the availability of wage data in India. Census do not collect the wage information, and one has to rely on wage data collected by NSS employment surveys. Topalova (2010, footnote 7) states that given the NSS methodology in urban areas, it is not possible to create representative aggregate at the district level. Since wages are reported for only wage employees (while a large employment falls under self-employment), a district-level analysis using wage information for rural areas is also not feasible. EPT (2010) present some indirect evidence from rural India by looking at expenditure based on household head, adult employment based on literacy status, and school quality data. They conclude that if anything, the evidence is more consistent with increasing, rather than decreasing, returns to education.

We also carried out an exploratory exercise using the NSS data to see whether returns to education show some association with tariff reductions. For this, we estimated the following

regression at the state-region level.

$$log(wage_{irt}) = \alpha + \beta TR_{rt} + \lambda_t + \gamma_1 Medu_{irt} + \gamma_2 Hedu_{irt} + \gamma_{11} Medu * \lambda_t + \gamma_{21} Hedu * \lambda_t + \pi_1 Medu * \lambda_t * TR_{rt} + \pi_2 Hedu * \lambda_t * TR_{rt} + \eta X_{irt} + \varphi_r + X_{r91}.\lambda_t + \sum_{s=1}^{j-1} \delta_s.\lambda_t + \varepsilon_{irt} \ \forall t = 1987, 1999$$

where $wage_{irt}$ is the real wage for individual i residing in state-region r, TR_{rt} is the tariff protection for region r at time t. $Medu$ is an indicator for education level being primary, middle or secondary, while $Hedu$ captures education level of higher secondary or more. Standard errors are clustered at state-year level. In the above setup, the parameters π_1 and π_2 captures whether the change in returns for medium education and high education differs based on change in tariff protection.

Table 13 presents the estimates from the above equation. Column (3) and column (6) presents results from IV estimates with state-specific trends for urban and rural areas, respectively. It worth mentioning that the wage data for rural sample in 1987 NSS data has unusually high missing information. It reports wages only for 22 percent of the wage/salary employees while the other year NSS data report wage information for 80-90 percent of wage/salary employees. Since, we estimate the impact at region-level, estimation is feasible but estimates remain only suggestive.

The point estimates for π_1 and π_2 are negative for both urban and rural areas. Moreover, the magnitudes of the coefficients are economically significant suggesting that the returns for medium education compared to low education decreased less in districts that experienced more tariff cuts. Similarly, return to higher education increased more in the districts that experienced more tariff reductions. Given that we only have 74 state-regions and standard errors are clustered at state-year, the standard errors are large making both the coefficients statistically insignificant precluding any conclusive inference. But the signs of the coefficients suggest role for increased returns to schooling.

6 Conclusion

In this paper, we exploit the exogeneity of Indian trade liberalization to study the impact of Indian trade liberalization, introduced in 1991, on human capital accumulation measured by the completion of different stages of schooling and aggregate years of schooling. Using census data covering four decades, and multiple estimation strategy and outcomes, we find that the trade liberalization has no impact on either aggregate years of schooling or completion of different stages of schooling. The analysis also emphasizes the importance of accounting for differential state policies in federal structure.

We also find suggestive evidence of increased jobs and workforce participation among school going children age group in districts that experienced larger reduction in tariff protection. This suggests increased opportunity cost of schooling. We conjecture that the negative effects of increased opportunity cost of schooling on schooling outcomes may have mitigated by the positive effects of increased returns to schooling. We provide suggestive evidence to support our conjecture. The returns to higher education (higher secondary or more) increased more in state-regions that experienced larger decline in tariff protection.

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Table 1: Descriptive statistics

	Primary completion rate			Tertiary completion rate			Years of schooling		
Age group	1991	2001	2011	1991	2001	2011	1991	2001	2011
11	0.517	0.503	0.588	NA	NA	NA	3.0	2.9	3.3
12	0.504	0.562	0.727	NA	NA	NA	3.0	3.2	4.1
13	0.644	0.711	0.818	NA	NA	NA	4.1	4.3	5.0
14	0.641	0.712	0.839	NA	NA	NA	4.5	4.8	5.8
15-19	0.601	0.701	0.814	0.008	0.000	0.000	5.0	5.9	7.2
20-24	0.523	0.640	0.744	0.056	0.076	0.118	4.8	6.1	7.5
25-29	0.461	0.568	0.675	0.062	0.095	0.139	4.2	5.4	6.7
30-34	0.431	0.505	0.620	0.058	0.082	0.117	3.9	4.8	6.1
35-39	0.415	0.461	0.562	0.053	0.069	0.096	3.7	4.3	5.4
40-44	0.366	0.445	0.514	0.046	0.068	0.087	3.2	4.1	4.9
45-49	0.330	0.423	0.476	0.036	0.064	0.079	2.8	3.9	4.5
50-54	0.274	0.382	0.452	0.028	0.058	0.075	2.3	3.5	4.3
55-59	0.262	0.327	0.426	0.023	0.044	0.073	2.1	2.9	4.0
60-64	0.196	0.251	0.348	0.013	0.029	0.052	1.5	2.2	3.2
65-69	0.211	0.232	0.309	0.013	0.024	0.041	1.6	2.0	2.8
70-74	0.174	0.202	0.268	0.009	0.019	0.035	1.3	1.7	2.4
75-79	0.203	0.225	0.274	0.012	0.020	0.036	1.6	1.9	2.5
All ages	0.320	0.385	0.478	0.025	0.037	0.056	2.7	3.4	4.4

Note: District wise ratios/years of schooling (452 1991-Census districts) are weighted by district-age group population. The sample does not include state of Jammu & Kashmir. NA: Not Applicable.

Table 2: Change in district tariff between 1991 and 1997

			Percentile				
		Mean	10th	25th	50th	75th	90th
Panel A: Change in Tariff (Scaled)							
452 districts	All India	-0.058	-0.014	-0.021	-0.038	-0.071	-0.129
442 districts	Urban	-0.092	-0.038	-0.052	-0.078	-0.118	-0.167
446 districts	Rural	-0.050	-0.011	-0.016	-0.027	-0.049	-0.105
Panel B: Change in tariff (Nonscaled)							
452 districts	All India	-0.460	-0.371	-0.405	-0.450	-0.497	-0.540
442 districts	Urban	-0.447	-0.359	-0.405	-0.447	-0.496	-0.532
446 districts	Rural	-0.456	-0.369	-0.397	-0.445	-0.494	-0.542

Table 3: Cohorts based on age categories that potentially attended schools post 1991- liberalization

	2011 Census			
	Primary		**Secondary**	**Graduate**
Age cohort	***Start***	***Finish***	***Finish 10 years***	***Finish 15 years***
(1)	(2)	(3)	(4)	(5)
13- 14	2003- 2004	2008- 2009	2013- 2014	2018- 2019
15- 19	1998- 2002	2003- 2007	2008- 2012	2013- 2017
20- 24	1993- 1997	1998- 2002	2003- 2007	2008- 2012
25- 29	1988- 1992	1993- 1997	1998- 2002	2003- 2007
30- 34	1983- 1987	1988- 1992	1993- 1997	1998- 2002
35- 39	1978- 1982	1983- 1987	1988- 1992	1993- 1997
40- 44	1973- 1977	1978- 1982	1983- 1987	1988- 1992
45- 49	1968- 1972	1973- 1977	1978- 1982	1983- 1987
50- 54	1963- 1967	1968- 1972	1973- 1977	1978- 1982
55- 59	1958- 1962	1963- 1967	1968- 1972	1973- 1977
60- 64	1953- 1957	1958- 1962	1963- 1967	1968- 1972

Cohorts benefitted from Reforms for primary completion outcome
Cohorts benefitted from Reforms for secondary completion outcome
Cohorts benefitted from Reforms for graduate completion outcome

Table-4: Impact of liberalization on primary completion, cohort-wise analysis

	2011 Census					Placebo: 1991 Census				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IV	**NO**	**NO**	**NO**	**Yes**	**Yes**	**NO**	**NO**	**NO**	**Yes**	**Yes**
Cohort*1991 conditions	**NO**	**Yes**	**Yes**	**Yes**	**Yes**	**NO**	**Yes**	**Yes**	**Yes**	**Yes**
State* Trend	**No**	**No**	**Yes**	**No**	**Yes**	**NO**	**NO**	**Yes**	**NO**	**Yes**
Δtariff*age 13	0.395**	-0.301**	-0.107	0.414*	-0.206	-0.030	0.005	-0.022	0.369**	-0.128
	(0.193)	(0.132)	(0.111)	(0.220)	(0.132)	(0.190)	(0.139)	(0.114)	(0.185)	(0.134)
Δtariff*age 14	0.521***	-0.247**	-0.052	0.360*	-0.263**	0.024	0.070	0.046	0.450***	-0.053
	(0.192)	(0.118)	(0.109)	(0.204)	(0.128)	(0.176)	(0.135)	(0.109)	(0.157)	(0.101)
Δtariff*age 15	0.528***	-0.217*	-0.043	0.339	-0.206					
	(0.183)	(0.117)	(0.109)	(0.214)	(0.138)					
Δtariff*age 16	0.569***	-0.201*	-0.030	0.330	-0.209					
	(0.189)	(0.113)	(0.108)	(0.209)	(0.138)					
Δtariff*age 17	0.615***	-0.181*	-0.016	0.380*	-0.133					
	(0.198)	(0.108)	(0.107)	(0.205)	(0.122)					
Δtariff*age 18	0.514***	-0.150	0.025	0.413*	-0.133					
	(0.180)	(0.117)	(0.114)	(0.240)	(0.167)					
Δtariff*age 19	0.483***	-0.151	0.014	0.467**	-0.045					
	(0.179)	(0.112)	(0.111)	(0.223)	(0.136)					
Δtariff*age 1519						0.026	0.109	0.089	0.412***	-0.032
						(0.153)	(0.129)	(0.107)	(0.149)	(0.095)
Δtariff*age 2024	0.367**	-0.112	0.036	0.460**	0.016	-0.075	0.093	0.076	0.330**	-0.057
	(0.167)	(0.109)	(0.111)	(0.213)	(0.133)	(0.144)	(0.126)	(0.108)	(0.142)	(0.093)
Δtariff*age 2529	0.196	-0.071	0.057	0.377**	0.008	-0.069	0.137	0.123	0.367**	0.034
	(0.154)	(0.100)	(0.111)	(0.182)	(0.112)	(0.144)	(0.131)	(0.114)	(0.143)	(0.096)
Δtariff*age 3034	0.077	-0.084	0.023	0.177	-0.120	-0.127	0.130	0.120	0.358**	0.079
	(0.151)	(0.094)	(0.104)	(0.167)	(0.099)	(0.147)	(0.124)	(0.106)	(0.143)	(0.096)
Δtariff*age 3539	0.082	-0.062	0.025	0.089	-0.139	-0.059	0.129	0.122	0.355**	0.129
	(0.153)	(0.098)	(0.104)	(0.184)	(0.111)	(0.142)	(0.123)	(0.104)	(0.141)	(0.094)
Δtariff*age 4044	0.067	-0.047	0.021	0.024	-0.145	-0.071	0.101	0.095	0.242*	0.074
	(0.166)	(0.106)	(0.105)	(0.210)	(0.126)	(0.149)	(0.131)	(0.110)	(0.144)	(0.092)
Δtariff*age 4549	0.054	-0.048	0.002	0.016	-0.101	-0.016	0.048	0.044	0.114	-0.002
	(0.172)	(0.115)	(0.106)	(0.216)	(0.122)	(0.165)	(0.143)	(0.120)	(0.167)	(0.113)
Δtariff*age 5054	0.054	0.005	0.034	0.015	-0.046	0.011	0.063	0.057	0.057	0.008
	(0.187)	(0.118)	(0.116)	(0.216)	(0.109)	(0.178)	(0.145)	(0.122)	(0.175)	(0.105)
Observations	6,780	6,780	6,780	6,780	6,780	4,972	4,972	4,972	4,972	4,972

Note: The dependent variable is primary completion rate in different age groups in Indian districts (452 1991 Census districts). The sample consists age group 13-59, age group 55-59 is the excluded category. All the models include district fixed effects and age group fixed effects. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe

or reside in urban areas and the percentage of literate population. Standard errors (in parenthesis) are clustered at the state-age group level. The regressions are weighted by the relevant age group population in the districts.

Table-5: Impact of liberalization on primary completion by urban/rural, cohort-wise analysis

	Rural						Urban					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		2001 Census			Placebo-1991 Census			*2001 Census*			Placebo-1991 Census	
IV	**No**	**YES**	**Yes**	**No**	**YES**	**Yes**	**No**	**YES**	**Yes**	**No**	**YES**	**Yes**
Cohort*1991 conditions	**YES**	**YES**	**Yes**	**YES**	**YES**	**Yes**	**YES**	**YES**	**Yes**	**YES**	**YES**	**Yes**
State* Trend	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**
Δtariff*age 13	-0.080	0.532**	-0.250*	-0.069	0.254	-0.257	-0.332**	0.068	0.118	-0.205	-0.310	-0.169
	(0.118)	(0.254)	(0.136)	(0.100)	(0.203)	(0.167)	(0.163)	(0.300)	(0.195)	(0.159)	(0.274)	(0.220)
Δtariff*age 14	-0.028	0.469**	-0.319**	-0.016	0.364**	-0.148	-0.301**	-0.053	-0.000	-0.130	-0.267	-0.123
	(0.116)	(0.219)	(0.131)	(0.091)	(0.155)	(0.116)	(0.142)	(0.264)	(0.154)	(0.159)	(0.286)	(0.227)
Δtariff*age 15	-0.019	0.452**	-0.239*				-0.284**	-0.037	0.015			
	(0.116)	(0.221)	(0.130)				(0.136)	(0.247)	(0.148)			
Δtariff*age 16	-0.003	0.417*	-0.265*				-0.271**	-0.030	0.023			
	(0.116)	(0.220)	(0.140)				(0.125)	(0.238)	(0.141)			
Δtariff*age 17	0.003	0.451**	-0.193				-0.222*	-0.025	0.034			
	(0.115)	(0.220)	(0.127)				(0.116)	(0.217)	(0.126)			
Δtariff*age 18	0.047	0.497**	-0.194				-0.243**	0.004	0.063			
	(0.123)	(0.249)	(0.173)				(0.115)	(0.216)	(0.119)			
Δtariff*age 19	0.027	0.542**	-0.099				-0.218**	-0.009	0.057			
	(0.120)	(0.236)	(0.138)				(0.110)	(0.201)	(0.115)			
Δtariff*age 1519				0.036	0.350**	-0.110				-0.121	-0.190	-0.063
				(0.090)	(0.141)	(0.101)				(0.151)	(0.249)	(0.203)
Δtariff*age 2024	0.046	0.534**	-0.027	0.031	0.301**	-0.102	-0.208*	0.020	0.083	-0.132	-0.176	-0.052
	(0.120)	(0.230)	(0.139)	(0.093)	(0.136)	(0.100)	(0.107)	(0.192)	(0.120)	(0.151)	(0.236)	(0.204)
Δtariff*age 2529	0.076	0.455**	-0.014	0.094	0.382***	0.035	-0.280***	-0.148	-0.094	-0.137	-0.218	-0.110
	(0.118)	(0.196)	(0.118)	(0.095)	(0.137)	(0.100)	(0.105)	(0.186)	(0.119)	(0.160)	(0.241)	(0.210)
Δtariff*age 3034	0.044	0.246	-0.134	0.099	0.358***	0.067	-0.268***	-0.145	-0.101	-0.158	-0.151	-0.063
	(0.113)	(0.180)	(0.107)	(0.087)	(0.136)	(0.100)	(0.103)	(0.176)	(0.110)	(0.153)	(0.237)	(0.204)
Δtariff*age 3539	0.043	0.148	-0.148	0.097	0.330**	0.095	-0.267**	-0.205	-0.170	-0.118	-0.150	-0.082
	(0.113)	(0.196)	(0.117)	(0.086)	(0.133)	(0.099)	(0.104)	(0.181)	(0.113)	(0.155)	(0.237)	(0.207)
Δtariff*age 4044	0.034	0.056	-0.165	0.069	0.233*	0.060	-0.197*	-0.138	-0.111	-0.171	-0.234	-0.185
	(0.114)	(0.228)	(0.127)	(0.093)	(0.139)	(0.099)	(0.106)	(0.189)	(0.115)	(0.162)	(0.239)	(0.222)
Δtariff*age 4549	0.011	0.022	-0.135	0.029	0.104	-0.012	-0.143	-0.085	-0.067	-0.129	-0.247	-0.215
	(0.115)	(0.236)	(0.124)	(0.102)	(0.161)	(0.119)	(0.110)	(0.203)	(0.128)	(0.161)	(0.245)	(0.224)
Δtariff*age 5054	0.038	0.008	-0.078	0.042	0.076	0.025	-0.031	0.012	0.016	-0.126	-0.224	-0.204
	(0.126)	(0.241)	(0.110)	(0.099)	(0.167)	(0.098)	(0.125)	(0.220)	(0.134)	(0.177)	(0.284)	(0.229)
Observations	6,690	6,690	6,690	4,906	4,906	4,906	6,630	6,630	6,630	4,862	4,862	4,862

Note: The dependent variable is primary completion rate in different age groups in Indian districts (1991 Census districts, 446 rural, 442 urban) of India. The sample consists age group 13-59, Age group 55-59 is the excluded category. All the models include district fixed effects and age group fixed effects. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population. Standard errors (in parenthesis) are clustered at the state-age group level. The regressions are weighted by the relevant age group population in the districts.

Table 6: Impact of liberalization on secondary completion, cohort-wise analysis

	All						Rural		Urban	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Census		**2011-Census**			**Placebo: 1991 Census**		**2011**	**1991**	**2011**	**1991**
IV	**NO**	**YES**	**YES**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Cohort*1991 conditions	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State* Trend	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**
Δtariff*age 1519	-0.036	0.529	-0.254**	0.060	0.231***	0.024	-0.307**	0.006	0.053	0.204
	(0.094)	(0.332)	(0.128)	(0.048)	(0.082)	(0.073)	(0.121)	(0.050)	(0.190)	(0.206)
Δtariff*age 2024	0.049	0.495*	-0.175	0.080*	0.194***	0.014	-0.195	0.013	-0.007	0.302
	(0.105)	(0.265)	(0.122)	(0.045)	(0.057)	(0.066)	(0.129)	(0.050)	(0.127)	(0.203)
Δtariff*age 2529	0.055	0.462**	-0.102	0.070	0.140***	-0.013	-0.091	0.032	-0.317**	0.077
	(0.107)	(0.194)	(0.095)	(0.052)	(0.047)	(0.061)	(0.117)	(0.046)	(0.134)	(0.205)
Δtariff*age 3034	0.012	0.263	-0.189	0.080	0.111**	-0.014	-0.182	0.014	-0.187	0.086
	(0.100)	(0.198)	(0.116)	(0.050)	(0.048)	(0.060)	(0.132)	(0.045)	(0.134)	(0.196)
Δtariff*age 3539	-0.022	0.134	-0.214*	0.108**	0.133**	0.035	-0.232*	0.029	-0.178	0.058
	(0.099)	(0.221)	(0.122)	(0.046)	(0.053)	(0.061)	(0.130)	(0.046)	(0.116)	(0.186)
Δtariff*age 4044	-0.026	0.028	-0.226**	0.084*	0.091*	0.020	-0.262**	0.026	0.010	-0.047
	(0.102)	(0.243)	(0.112)	(0.045)	(0.055)	(0.059)	(0.114)	(0.042)	(0.112)	(0.205)
Δtariff*age 4549	-0.033	0.014	-0.150*	0.058	0.062	0.012	-0.187*	0.014	0.000	-0.070
	(0.100)	(0.244)	(0.088)	(0.047)	(0.055)	(0.060)	(0.100)	(0.042)	(0.116)	(0.196)
Δtariff*age 5054	-0.007	-0.007	-0.087	0.036	0.022	0.005	-0.117	0.013	0.070	0.033
	(0.111)	(0.250)	(0.090)	(0.053)	(0.056)	(0.068)	(0.110)	(0.044)	(0.127)	(0.213)
Observations	4,068	4,068	4,068	4,068	4,068	4,068	4,014	4,014	3,978	3,978

Note: The dependent variable is secondary completion rate in different age groups in Indian districts (452 1991 Census districts: 446 rural districts, 442 urban districts). The sample consists age group 15-59, Age group 55-59 is the excluded category. All the models include district fixed effects and age group fixed effects. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population. Standard errors (in parenthesis) are clustered at the state-age group level. The regressions are weighted by the relevant age group population in the districts.

Table 7: Impact of liberalization on tertiary completion, cohort-wise analysis

	All						Rural		Urban	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Census	2011-Census			Placebo: 1991 Census			2011	1991	2011	1991
IV	NO	YES	YES	NO	YES	YES	YES	YES	YES	YES
Cohort*1991 conditions	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
State* Trend	YES	NO	YES	YES	NO	YES	YES	YES	YES	YES
Δtariff*age 2024	-0.048	0.164	-0.121**	-0.000	0.052*	-0.015	-0.093**	-0.012	0.064	0.184*
	(0.030)	(0.117)	(0.056)	(0.013)	(0.031)	(0.024)	(0.039)	(0.011)	(0.136)	(0.096)
Δtariff*age 2529	0.001	0.177*	-0.064	-0.007	0.050**	-0.008	-0.034	-0.004	-0.239	0.113
	(0.034)	(0.092)	(0.056)	(0.010)	(0.020)	(0.018)	(0.042)	(0.010)	(0.174)	(0.083)
Δtariff*age 3034	0.005	0.108	-0.086	-0.013	0.033	-0.014	-0.055	-0.009	-0.088	0.103
	(0.030)	(0.073)	(0.055)	(0.013)	(0.021)	(0.021)	(0.043)	(0.010)	(0.152)	(0.095)
Δtariff*age 3539	-0.010	0.070	-0.080	-0.002	0.035	-0.002	-0.061	-0.004	-0.010	0.082
	(0.030)	(0.078)	(0.058)	(0.013)	(0.022)	(0.020)	(0.047)	(0.011)	(0.133)	(0.097)
Δtariff*age 4044	-0.006	0.026	-0.082	0.001	0.010	-0.017	-0.079**	-0.009	0.079	0.066
	(0.032)	(0.083)	(0.053)	(0.011)	(0.021)	(0.018)	(0.039)	(0.009)	(0.132)	(0.096)
Δtariff*age 4549	-0.006	0.013	-0.057	0.002	-0.013	-0.032	-0.065*	-0.016*	0.090	-0.071
	(0.031)	(0.084)	(0.049)	(0.012)	(0.026)	(0.022)	(0.037)	(0.009)	(0.145)	(0.124)
Δtariff*age 5054	-0.007	0.001	-0.031	0.002	-0.013	-0.021	-0.041	-0.011	0.076	0.034
	(0.033)	(0.093)	(0.054)	(0.013)	(0.027)	(0.021)	(0.040)	(0.009)	(0.163)	(0.104)
Observations	3,616	3,616	3,616	3,616	3,616	3,616	3,568	3,568	3,536	3,536

Note: The dependent variable is tertiary completion rate in different age groups in Indian districts (452 1991 Census districts: 446 rural districts, 442 urban districts). The sample consists age group 20-59, Age group 55-59 is the excluded category. All the models include district fixed effects and age group fixed effects. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population. Standard errors (in parenthesis) are clustered at the state-age group level. The regressions are weighted by the relevant age group population in the districts.

Table 8: Impact of liberalization on years of schooling, cohort-wise analysis

	All						Rural		Urban	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Census		**2011-Census**			**Placebo: 1991 Census**		**2011**	**1991**	**2011**	**1991**
IV	**NO**	**YES**	**YES**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
Cohort*1991 conditions	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State* Trend	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**
Δtariff*age 1519	-0.817	5.026*	-3.518**	0.503	3.312***	-0.544	-3.862***	-1.025	1.375	1.310
	(1.133)	(2.651)	(1.425)	(0.832)	(1.025)	(0.765)	(1.205)	(0.672)	(2.154)	(2.301)
Δtariff*age 2024	0.430	6.437**	-0.806	0.683	3.050***	-0.302	-1.362	-0.759	1.042	1.957
	(1.214)	(2.881)	(1.573)	(0.825)	(0.982)	(0.737)	(1.436)	(0.665)	(1.410)	(2.327)
Δtariff*age 2529	0.747	5.433**	-0.630	0.930	2.985***	0.102	-0.818	0.190	-2.977*	0.360
	(1.208)	(2.193)	(1.100)	(0.894)	(0.984)	(0.710)	(1.029)	(0.641)	(1.639)	(2.405)
Δtariff*age 3034	0.309	2.843	-2.039*	0.933	2.705***	0.303	-2.142**	0.249	-1.877	0.781
	(1.108)	(2.049)	(1.058)	(0.841)	(0.962)	(0.697)	(1.015)	(0.637)	(1.519)	(2.396)
Δtariff*age 3539	0.054	1.497	-2.274*	1.140	2.817***	0.888	-2.450**	0.537	-1.991	0.502
	(1.117)	(2.250)	(1.213)	(0.814)	(0.989)	(0.738)	(1.136)	(0.658)	(1.380)	(2.308)
Δtariff*age 4044	0.019	0.432	-2.332*	0.869	1.830*	0.389	-2.676**	0.280	-0.327	-0.764
	(1.156)	(2.471)	(1.229)	(0.855)	(1.001)	(0.709)	(1.077)	(0.638)	(1.284)	(2.442)
Δtariff*age 4549	-0.148	0.189	-1.647	0.473	0.822	-0.176	-2.092**	-0.247	-0.219	-1.847
	(1.150)	(2.557)	(1.079)	(0.909)	(1.163)	(0.847)	(0.930)	(0.759)	(1.424)	(2.562)
Δtariff*age 5054	0.179	0.033	-0.885	0.502	0.394	-0.032	-1.271*	0.127	0.657	-1.121
	(1.252)	(2.607)	(0.963)	(0.963)	(1.198)	(0.851)	(0.773)	(0.604)	(1.643)	(2.646)
Observations	4,068	4,068	4,068	4,068	4,068	4,068	4,014	4,014	3,978	3,978

Note: The dependent variable is years of completed schooling by age groups in Indian districts (452 1991 Census districts: 446 rural districts, 442 urban districts). The sample consists age group 15-59, Age group 55-59 is the excluded category. All the models include district fixed effects and age group fixed effects. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population. Standard errors (in parenthesis) are clustered at the state-age group level. The regressions are weighted by the relevant age group population in the districts.

Table 9: Primary completion pre- and post-reforms

	(1)	(2)	(3)	(4)	(4a)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform*	*Pre Reform**	*Pre Reform**	Pre and Post Reform	Pre and Post Reform
									Placebo			
Sample	***13-19 1991 vs 2001***	***13-19 1991 vs 2001***	***13-19 1991 vs 2001***	***13-19 1991 vs 2001***	***13-19 1991 vs 2001***	***13-19 1991 vs 2001***	***All 1991 vs 2001***	***All 1991 vs 2001***	***All 1981 vs 1991***	***All 1981 vs 1991***	***20-24 1991 vs 2011***	***20-24 1991 vs 2011***
Initial district condition*Post	**NO**	**YES**	**YES**	**YES**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State*Post	**NO**	**NO**	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**	**NO**	**YES**	**NO**	**YES**
IV with traded tariff	**NO**	**NO**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**Yes**	**YES**	**YES**	**YES**
A. All												
tariff	0.328**	0.069	0.005	0.387***	0.047	-0.114*	0.193***	-0.094***	0.112**	0.039	0.461**	-0.058
	(0.122)	(0.092)	(0.038)	(0.135)	(0.090)	(0.063)	(0.072)	(0.036)	(0.046)	(0.058)	(0.194)	(0.115)
Observations	904	904	904	904	904	904	904	904	856	856	904	904
1991 Districts	452	452	452	452	452	452	452	452	428	428	452	452
B. Rural												
tariff	0.212	0.082	-0.002	0.435***	-0.057	-0.115*	0.189**	-0.099**	0.092**	-0.011	0.492**	-0.064
	(0.128)	(0.099)	(0.042)	(0.144)	(0.067)	(0.060)	(0.080)	(0.040)	(0.039)	(0.056)	(0.219)	(0.109)
Observations	892	892	892	892	892	892	892	892	834	834	892	892
1991 Districts	446	446	446	446	446	446	446	446	417	417	446	446
C. Urban												
tariff	0.115*	-0.031	-0.041	0.054	-0.021	-0.153	0.170	-0.062	0.021	-0.032**	0.340**	0.070
	(0.066)	(0.126)	(0.115)	(0.144)	(0.062)	(0.112)	(0.167)	(0.099)	(0.023)	(0.013)	(0.134)	(0.078)
Observations	886	886	886	886	886	886	886	886	886	886	886	886
1991 Districts	443	443	443	443	443	443	443	443	443	443	443	443

Note: The dependent variable is primary completion rate in districts of India. Standard errors (in parenthesis) are clustered at the state-year level. The regressions are weighted by the relevant age group population in the districts. All the models include district fixed effects and an indicator for post 1991. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population.

Table 10: Tertiary completion pre and post reforms

	(1) Pre and Post Reform	(2) Pre and Post Reform	(3) Pre and Post Reform	(4) Pre and Post Reform	(4a) Pre and Post Reform	(5) Pre and Post Reform	(6) Pre and Post Reform	(7) Pre and Post Reform*	(8) *Pre Reform**	(9) *Pre Reform**	(10) Pre and Post Reform	(11) Pre and Post Reform
									Placebo			
Sample	*25-29*	*25-29*	*25-29*	*25-29*	*25-29*	*25-29*	***All***	***All***	***All***	***All***	*25-29*	*25-29*
	1991 vs 2001	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1981 vs 1991***	***1981 vs 1991***	***1991 vs 2011***	***1991 vs 2011***
Initial district condition*Post	**NO**	**YES**	**YES**	**YES**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State*Post	**NO**	**NO**	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**	**NO**	**YES**	**NO**	**YES**
IV with traded tariff	**NO**	**NO**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
A. All												
tariff	-0.045*	0.025	0.010	0.090***	0.012	0.038**	0.023***	0.006	0.005	0.008**	0.170**	-0.052
	(0.023)	(0.015)	(0.012)	(0.025)	(0.013)	(0.017)	(0.008)	(0.005)	(0.009)	(0.004)	(0.080)	(0.038)
Observations	904	904	904	904	904	904	904	904	856	856	904	904
1991 Districts	452	452	452	452	452	452	452	452	428	428	452	452
B. Rural												
tariff	-0.017	0.019	0.004	0.089***	0.013*	0.028**	0.019***	0.004	-0.013*	0.000	0.163**	-0.015
	(0.021)	(0.017)	(0.008)	(0.024)	(0.008)	(0.013)	(0.006)	(0.003)	(0.008)	(0.002)	(0.069)	(0.033)
Observations	892	892	892	892	892	892	892	892	844	844	892	892
1991 districts	446	446	446	446	446	446	446	446	422	422	446	446
C. Urban												
tariff	0.087**	0.055	0.093*	0.009	0.067***	0.022	0.037	0.006	0.021	-0.032**	-0.326	-0.353*
	(0.035)	(0.045)	(0.047)	(0.055)	(0.024)	(0.065)	(0.029)	(0.029)	(0.023)	(0.013)	(0.209)	(0.182)
Observations	882	882	882	882	882	882	882	882	834	834	884	884
1991 districts	441	441	441	441	441	441	441	441	417	417	442	442

Note: The dependent variable is tertiary completion rate in districts of India. Standard errors (in parenthesis) are clustered at the state-year level. The regressions are weighted by the relevant age group population in the districts. All the models include district fixed effects and an indicator for post 1991. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population.

Table 11: Years of schooling, pre and post reforms

	(1)	(2)	(3)	(4)	(4a)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform	Pre and Post Reform*	*Pre Reform**	*Pre Reform**	Pre and Post Reform	Pre and Post Reform
									Placebo			
Sample	***20-29***	***20-29***	***20-29***	***20-29***	***20-29***	***20-29***	***All***	***All***	***All***	***All***	***20-29***	***20-29***
	1991 vs 2001	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1991 vs 2001***	***1981 vs 1991***	***1981 vs 1991***	***1991 vs 2011***	***1991 vs 2011***
Initial district condition*Post	**NO**	**YES**	**YES**	**YES**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**	**YES**
State*Post	**NO**	**NO**	**YES**	**NO**	**YES**	**YES**	**NO**	**YES**	**NO**	**YES**	**NO**	**YES**
IV with traded tariff	**NO**	**NO**	**NO**	**YES**	**YES**	**YES**	**YES**	**YES**	**Yes**	**YES**	**YES**	**YES**
A. All												
tariff	1.437	0.189	0.324	2.699***	0.832	-0.395	1.582***	-0.558**	0.920***	0.511	4.914**	-1.006
	(1.044)	(0.630)	(0.378)	(0.821)	(0.809)	(0.590)	(0.522)	(0.251)	(0.310)	(0.341)	(2.211)	(1.161)
Observations	904	904	904	904	904	904	904	904	856	856	904	904
1991 Districts	452	452	452	452	452	452	452	452	428	428	452	452
B. Rural												
tariff	0.302	0.121	0.038	2.960***	-0.659	-0.627	1.598***	-0.640**	0.388	0.062	5.420**	-1.104
	(0.936)	(0.703)	(0.366)	(0.782)	(0.413)	(0.518)	(0.528)	(0.290)	(0.247)	(0.321)	(2.297)	(0.993)
Observations	892	892	892	892	892	892	892	892	844	844	892	892
1991 Districts	446	446	446	446	446	446	446	446	422	422	446	446
C. Urban												
tariff	1.999**	-0.481	0.229	0.209	1.060	-1.140	1.745	-0.388	1.316	0.378	0.990	-1.935**
	(0.886)	(1.163)	(1.368)	(1.689)	(0.714)	(1.051)	(1.429)	(0.768)	(1.065)	(1.257)	(1.979)	(0.958)
Observations	882	882	882	882	882	882	882	882	834	834	884	884
1991 Districts	441	441	441	441	441	441	441	441	417	417	441	441

Note: The dependent variable is years of schooling in districts of India. Standard errors (in parenthesis) are clustered at the state-year level. The regressions are weighted by the relevant age group population in the districts. All the models include district fixed effects and an indicator for post 1991. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population.

Table 12: Impact of trade liberalization on workforce, population, and workforce participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Log of workers				Log of Population				Workforce Participation Rate (WFPR)			
Sample	***5-14***	***5-14***	***15-19***	***15-19***	***5-14***	***5-14***	***15-19***	***15-19***	***5-14***	***5-14***	***15-19***	***15-19***
	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001	1991 vs. 2001
State*Post	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
A. Total												
tariff	-2.106*	-1.393	-0.202	-0.379	-0.156	-0.034	0.065	-0.061	-0.148**	-0.094*	-0.171	-0.147
	(1.222)	(1.050)	(0.371)	(0.299)	(0.167)	(0.102)	(0.129)	(0.094)	(0.061)	(0.053)	(0.132)	(0.105)
1991 WFPR									0.053		0.354	
Observations	904	904	904	904	904	904	904	904	904	904	904	904
B. Rural India												
tariff	-1.435	-1.034	0.069	-0.273	-0.095	0.094	0.214	-0.002	-0.125**	-0.078	-0.129	-0.126
	(1.175)	(0.928)	(0.379)	(0.235)	(0.173)	(0.078)	(0.159)	(0.090)	(0.060)	(0.049)	(0.141)	(0.092)
1991 WFPR									0.063		0.417	
Observations	892	892	892	892	892	892	892	892	892	892	892	892
C. Urban India												
tariff	-2.658*	-3.184***	-1.388*	-0.913	-0.529	0.184	-0.716**	-0.081	-0.067***	-0.090***	-0.165*	-0.183***
	(1.421)	(1.130)	(0.720)	(0.573)	(0.495)	(0.464)	(0.340)	(0.391)	(0.022)	(0.017)	(0.084)	(0.067)
1991 WFPR									0.021		0.181	
Observations	882	882	882	882	882	882	882	882	882	882	882	882

Note: Standard errors (in parenthesis) are clustered at the state-year level. The regressions are weighted by the relevant age group population in the districts. All the models include district fixed effects and an indicator for post 1991. Initial 1991 district conditions include percentage of workers in a district employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of district's population that is Schedule Caste/Tribe or reside in urban areas and the percentage of literate population.

Table 13: Impact of liberalization on returns to education

	(1)	(2)	(3)	(4)	(5)	(6)
		Urban			**Rural**	
Tariff	-0.569	1.323	-1.644	-0.719***	-0.938	-0.859**
	(0.675)	(1.144)	(1.091)	(0.238)	(1.189)	(0.354)
Post (1999=1, 1987=0)	0.441	-0.294	0.521	0.482	1.331	0.544
	(0.351)	(0.247)	(0.384)	(0.993)	(1.506)	(0.972)
Medium education	0.538***	0.540***	0.538***	0.677***	0.672***	0.678***
	(0.032)	(0.032)	(0.032)	(0.051)	(0.052)	(0.051)
High education	1.175***	1.180***	1.175***	1.404***	1.421***	1.405***
	(0.038)	(0.037)	(0.038)	(0.105)	(0.103)	(0.103)
Medium education*Post	-0.061	-0.026	-0.040	-0.356***	-0.343***	-0.363***
	(0.122)	(0.133)	(0.140)	(0.053)	(0.062)	(0.061)
High education*Post	0.004	0.044	0.029	-0.296**	0.027	0.031
	(0.112)	(0.123)	(0.129)	(0.128)	(0.241)	(0.242)
Medium education*Post*Tariff	-0.547	-1.016	-0.814	-1.060***	-1.433	-1.024
	(1.573)	(1.771)	(1.846)	(0.350)	(1.533)	(1.353)
High education*Post*Tariff	-0.732	-1.288	-1.052	-3.444**	-14.899*	-14.523*
	(1.315)	(1.528)	(1.590)	(1.659)	(8.736)	(8.591)
Observations	65,494	65,494	65,494	49,039	49,039	49,039
Adjusted R-squared	0.470	0.468	0.470	0.490	0.478	0.484
Sample	*NSS* ***1987 vs 1999***	*NSS* ***1987 vs 1999***	*NSS* ***1987 vs 1999***	*NSS* ***1987 vs 1999***	*NSS* ***1987 vs 1999***	*NSS* ***1987 vs 1999***
Initial region condition*Post	YES	YES	YES	YES	YES	YES
State*Post	Yes	NO	YES	Yes	NO	Yes
IV with traded tariff	NO	YES	YES	NO	YES	YES

Note: The dependent variable is log of real wages. The standard errors (in parenthesis) are clustered at the state-year level. The sample consists of individuals age more than 20 and having wage/salary jobs. All the models control for state-regions (74) fixed effects; individual characteristics age, age squared, gender, and caste; and interactions of regions with region 1991 characteristics such percentage of workers in region employed in agriculture, employed in mining, employed in manufacturing, employed in trade, employed in transport, and employed in services (construction is the omitted category), the share of region's population that is Schedule Caste/Tribe and the percentage of literate population. Medium education: primary, middle, or secondary; High education: Secondary and above, and omitted education category consists individuals with below primary education.

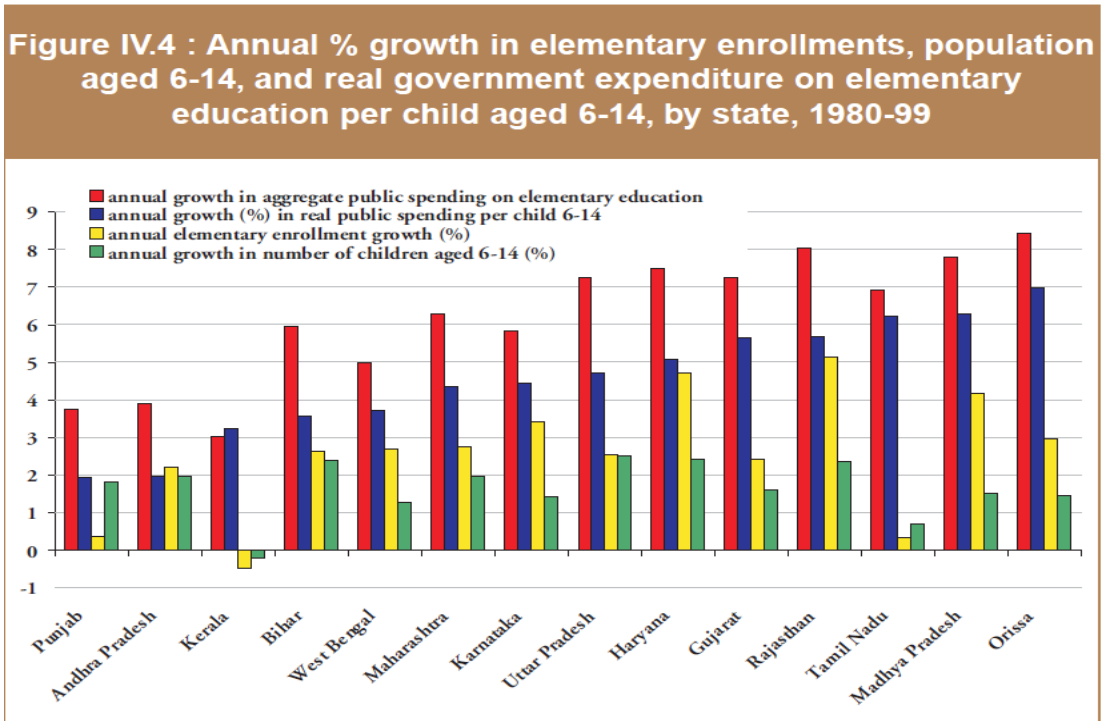
Appendix

Table A1: Age cohort schooling years by census

		Primary (1-5)				Secondary (9-10)				Tertiary (13-15)			
Census	Age Cohort	Start		Finish		Start		Finish		Start		Finish	
2001	13-19	1988	1994	1992	1998	1996	2002	1997	2003	2001	2007	2003	2009
2001	20-24	1983	1987	1987	1991	1991	1995	1992	1996	1996	2000	1998	2002
2001	25-29	1978	1982	1982	1986	1986	1990	1987	1991	1991	1995	1993	1997
2011	13-19	1998	2004	2002	2008	2006	2012	2007	2013	2011	2017	2013	2019
2011	20-24	1993	1997	1997	2001	2001	2005	2002	2006	2006	2010	2008	2012
2011	25-29	1988	1992	1992	1996	1996	2000	1997	2001	2001	2005	2003	2007

Online Appendix

Figure A1: State wise variation in education expenditure on elementary education



Source: World Bank (2004). India - Attaining the Millennium Development Goals in India: role of public policy and service delivery. Washington, DC: World Bank, page 71.