Access to Colleges, Human Capital, and Empowerment of Women*

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Abstract

Women's enrollment in higher education in developing countries is staggeringly low. Due to cultural norms and concerns for safety, women scarcely attend colleges outside their home districts and within home districts supply constraints are often binding contributing to the low enrollment. In this paper, we assess whether access to colleges in home districts improves human capital and the agency of women. Under a model degree colleges construction program commenced in India in 2008, eligible districts of the country received aid for college construction. The threshold for eligibility of districts for the grants was set at GER lower than the national average in 2001. We use the variation generated by this program, which is one of the largest college construction programs in the world, to estimate the effects of college access on women's outcomes. The college expansion following the policy led to an increase in number of years of schooling and college enrollment and completion for girls. Boys too benefited but the benefit for the girls is twice as large. Probability of employment and salary earnings increase for women and we also find evidence of benefits in the marriage market. This increase was accompanied by a decline in child marriage, fertility, an increase in age at marriage, and mobile phone ownership for women. Women also gained more autonomy in household decision making.

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1 Introduction

College enrollment in developing countries, especially in South Asia and Sub-Saharan Africa, remains strikingly low despite many policy efforts. Data from UNESCO's Institute for Statistics show that the gross enrollment ratio (GER) in higher education stood at 10 percent in 2012 in the low-income countries.[1] More saliently, there was a significant gender gap in the college enrollment rates in South Asia and Sub-Saharan Africa – with 81 and 64 girls enrolled against 100 boys, respectively – contradicting the global trend of more girls attending colleges than boys (Ilie and Rose, 2016). Supply side constraints are often considered a major culprit in giving rise to this pattern. A popular policy prescription to combat this issue has been the construction of more local colleges. Since, traditionally girls do not attend educational institutes that are too far (Sutton, 1998) due to gender norms and safety concerns, constructing more colleges in the educationally lagging regions may benefit girls more than boys.

However, a different school of thought identifies lower demand for education and prejudices as the main impediments to increasing girls' access to higher education (see for example, Jones (2008)). Children in developing countries often have to perform household chores or/and engage in farm enterprise. Since these household or economic activities are different for boys and girls, the opportunity cost of attending colleges can be higher for girls (Sutton, 1998). There is also a perception that the returns to higher education for women are low: young age has a premium in the marriage market, labor market opportunities are worse for women and girls typically move to the husband's house after they get married. These enduring factors can preclude girls from attending colleges despite adequate supply of colleges.[2]

Our study informs this debate by evaluating the consequences of a massive college expansion policy in India. With the aim of promoting access to higher education and making it more inclusive and equitable, the University Grant Commission (UGC) of India (the apex body that regulates higher education) introduced an innovative program in 2008 to provide financial assistance by way of grants for constructing new model colleges in the 374 eligible Educationally Backward Districts (EBD) of India. These were identified as the districts where the gross enrollment ratio in higher education was lower than the national average in 2001. We explore the variation in college expansion generated by this program to isolate

[1] GER in higher education is defined as the ratio of enrolled to total population in the age group 18-23.

[2] For instance, Krakauer (2018) documents that most schools remain unused after a school construction program in Afghanistan and Pakistan.

the causal impact of college access on human capital of girls and age at marriage as well as fertility choice decisions of potentially impacted women.

In Figure 1, we plot the time series of the total number of colleges in India since 1951.[3] The growth in colleges was increasing, albeit slowly, until 2001. However, there was a very stark acceleration in the decade from 2001 to 2011. Our identification harnesses this temporal trend change. Analogous to this, in Figure 2 we observe a steady increase in college enrollment until 2001. The liberalization of the Indian economy in 1991, which increased the demand for skilled labor in India, is followed by an uptick in college construction and enrollment. However, the notable sharp change in enrollment occurs in the decade of 2001-2011. This also escalated the previously dismal enrollment of girls in colleges. In this paper, we first try to establish causal increase in enrollment of girls and then attempt to highlight the consequences.

The key identification challenge while estimating the impact of college access is that college placement is non-random. To address this issue, we exploit the fact that exposure to college expansion was different across region and year of birth across cohorts. A sharp threshold was used for eligibility. Districts with GER lower than the national average in 2001 were eligible for the college construction grants. We identify the eligible districts by computing the 2001 district level GER using the Census of India 2001. We employ the same algorithm that formed the basis for the government program. In light of the fact that there are only 640 districts in India (less so in 2001), and most of expansion happened in districts with very low GER to begin with, we harness the panel dimension of the data for identification instead of relying on a regression discontinuity design exclusively. We employ a trend-break approach to identify the causal impact of the college expansion program by comparing the outcomes in the 374 eligible districts (Treatment) with those in the 266 districts not eligible for the grants (Control) before and after the expansion policy. We use a long panel of district by year new colleges (ranging from 1980-2016) for estimating the effect on college construction. We then employ cohort-analysis for estimating the overall and post-policy trends by treatment status. We use birth years 1965-1998 (ages 18-51) for education and other outcomes, and birth years 1965-2000 (ages 18-53) for employment and wages. Our empirical strategy controls for district and year fixed effects which absorb the time-invariant district characteristics and year-specific shocks common across all districts, respectively. Treatment was based solely on the eligibility criterion. However, the districts

[3]This figure plots total colleges by decade from 1951 to 2011 and then every year until 2017 based on UGC reports. The data from various UGC reports is available in this format. India started an annual survey of colleges in 2011 after which the data is collated by the UGC from that survey annually. See details in the data section.

with low GER (educationally backwards districts) are not necessarily similar to the high GER districts. The low GER districts exhibit a striking negative trend prior to the policy in education outcomes and college construction. This makes the use of a differences-in-differences strategy less suitable. Instead, we rely on a trend-break model.

Our reduced form analysis first establishes (using the panel data from 1980-2016) that there was a change in college expansion in treated areas right after the policy was implemented. College construction was less rapid in the treated areas before the policy but this trend reversed immediately after. Having established that the policy indeed increased the supply of colleges, we turn to examining the consequences for human capital using a panel of cohorts born between 1965 and 1998 using a nationally representative survey National Family Health Survey (NFHS-4) conducted in 2016-2017.[4] We find that years of education, college enrollment, and completion for girls exhibit the same trend in the treated area: much slower trend prior to the policy and a trend reversal with an acceleration in the post-policy period. Boys too gain from the policy as the expansion was based on overall GER and not gender-specific GER. However, the gains for girls are twice as large. With respect to number of years of schooling - there was an annual increase of 0.097 for females as compared to an annual increase of 0.046 for the males.

In conjunction to a change in college education, we also find other improvements in the welfare of girls. Expansion lowered the prevalence of child marriage among girls and increased the age at marriage. In the treated areas, we observe a decline in fertility and desired fertility for potentially affected cohorts. Women's ownership of major assets such as land and houses does not change, but there is a surge in mobile ownership. There is also an improvement in women's autonomy in making household decisions. Intimate partner violence is not affected. Labor market outcomes for girls improve significantly. Salaried employment estimated using the Periodic Labor Force Survey (PLFS) of India 2017-18 saw a positive trend after the policy.[5] However, there was an immediate fall in salaried employment (a statistically negative intercept shift) plausibly due to higher college enrolment and investment in human capital. Analogously, salary earnings also showed a positive post trend. We also detect evidence of benefits in the marriage market. On examining the characteristics of the spouses

[4]Our development outcomes come from this survey as well. Using different data from a nationally representative labor force survey, the Periodic Labor Force Survey (PLFS) of India, conducted in 2017-18, we are able to use the data for cohorts born between 1965 and 2000. Our results are not sensitive to the survey used or the birth years in the sample.

[5]This is the typical type of work that college educated would perform in India. While 32 percent of salaried workers are college graduates, only 1.73 percent wage workers are college graduates. College educated women comprise 39 percent of the salaried workforce but only 0.59 percent of the wage earning workforce.

in the post period, we find plausible positive assortative matching in terms of education and an increase in the likelihood of employment of the husband driven by non-agricultural work using the National Family Health Survey (NFHS wave 4).[6] Our findings reveal that higher college access can increase college participation and completion among women in developing countries and improve their labor and marriage market outcomes. This occurs concurrently with delay in marriage age which affects not only the welfare of women (Field and Ambrus, 2008; Jensen and Thornton, 2003) but also those of their children (Sekhri and Debnath, 2014).

In a series of robustness checks, we also show that our results are not sensitive to trends in economic growth (proxied by night time luminosity), poverty, and years included in the analysis. We find evidence of heterogeneity in college education outcomes by wealth but not by share of rural population.

Our paper complements and extends two strands of literature. First, it relates to a growing literature examining the effect of infrastructure investments in developing countries. Much of the focus of this work has been on productivity gains and reducing barriers to trade and market integration (Banerjee et al., 2012, Duflo and Pande, 2007, Lipscomb et al., 2013 Donaldson, 2018, Jensen, 2007, Asher and Novosad, 2019). Gender and educational outcomes have received scant attention. Limited number of studies have focused on education or gender and, to the best of our knowledge, none on the education outcomes of women. Duflo (2001) studies expansion of primary schools in Indonesia and finds a positive effect of primary school construction on education and earnings of men. Jagnani and Khanna (2019) study the consequences of elite college expansion on K-12 schooling markets. Rural electrification has been shown to increase female employment by releasing women from home production (Dinkelman, 2011).[7] We innovate by studying gender-based benefits of college expansion in developing countries where college access for women is limited and document that relaxing the supply constraints increases college participation and graduation of girls, improves the labor market outcomes, and betters other welfare enhancing development outcomes for women.

The second strand of literature we complement relates to geographic proximity of colleges and returns to educational attainment. A number of papers focusing on the US have used presence (or absence) of a local college or distance from a college as instruments

[6]Education investments can have returns in both labor and marriage markets. See for example, Chiappori et al. (2009).

[7]Fertility choice consequences of a German college expansion program is studied by Kamhöfer and Westphal (2017). A strand of work also focuses on consequences of more education for empowerment of women (Kremer et al., 2009; Friedman et al., 2015).

to estimate the returns to education (Card, 1993; Kane and Rouse, 1995; Carneiro et al., 2011; Carneiro and Heckman, 2002; Doyle and Skinner, 2016). Majority of these papers have focused on the rapid higher education expansion phase in US in the 1960s and 70s. The idea behind this approach is that proximity to college improves access and reduces costs as students can commute as opposed to being in residence at college (Kerr, 1991). However, research has also documented that this is less relevant for academically capable students (Hoxby, 1997) and for more recent cohorts of students who are less space bound due to technological innovations (Allen and Seaman, 2013). We contribute to this literature in a variety of ways. First, we evaluate the benefits of college proximity in a developing country. There are no prior estimates of benefits of college proximity for developing countries where supply constraints are still binding. Second, unlike most of this previous work, our focus is on access to higher education for women.[8] In our study, we examine a variety of social outcomes for women in addition to educational outcomes and labor market outcomes. A central innovation of our analysis is that we also shed light on the benefits in the marriage market.

Finally, our paper also contributes to the debate concerning investment on higher education in developing countries. International donor agencies (for example, the World Bank), have in the past considered investment in higher education as equity-detracting and have espoused allocation of scarce government resources to primary education as it provides higher social return than higher education (Psacharopoulos and Patrinos, 2004; World Bank, 2000). However, such arguments often ignore potential development spillover (Jagnani and Khanna, 2019) or "public good" function (Birdsall, 1996) of higher education. From a public finance perspective, investments in women's education have been documented to be more profitable than men (Psacharopoulos, 1994). We find that constructing colleges not only increases college enrollment and labor market outcomes but also reduces child marriage and fertility. To the extent that the confluence of these factors improves welfare of women, college expansion can have gender-based distributional effects and can aid in reducing gender disparities in the developing countries.

The rest of the paper is organized as follows. Section 2 provides the cultural context and background information on establishing the model colleges in the educationally backward districts in India. Section 3 describes the data. We discuss the empirical methodology to identify the causal impact of college construction program in section 4. Section 5 reports the empirical results and the falsification tests and section 6 concludes.

[8] Doyle and Skinner (2016) examine the effects on wages for women and finds that women's labor market outcomes are better.

2 Background

While India's knowledge economy is fueled by its massive college educated workforce, college education still remains elusive to its masses. In Asia, India trailed China in GER in higher education, which was 25.2 percent compared to 43.39 percent in China in 2016-17. By way of comparison, USA's GER is 85.8.[9] In addition to lagging behind many other economies, India faces another challenge of uneven access across states. Though in the past six decades, there has been a visible and substantial growth in colleges in India, it has not been spatially uniform. While some states have seen seen massive increases, others have trailed behind. College density, defined as the number of colleges for every one hundred thousand people in the age-group of 18-23 years, varied from 7 in Bihar to 59 in Telangana compared to an all India average of 28 (All India Survey on Higher Education, AISHE 2015-16). In 2016-17, the GER in higher education ranged from 14.9% in Bihar to 46.9% in Tamil Nadu.

Indian policy makers opine that proximity to colleges can go a long way in addressing this disparity. The 11th five-year plan of India emphasized increasing access to higher education in educationally backward districts. According to the 2001 Census of India, 374 districts had gross enrollment ratio in higher education lower than the national average of 12.4 percent. The apex higher education regulating body, the University Grant Commission (UGC) of India, along with the Ministry of Human Resource Development (MHRD) initiated a program to provide financial assistance for establishing new model colleges in these 374 educationally backward districts in 2008.[10].

To receive financial assistance under this program, the college had to be set up on or after 1st January 2008 in any of these 374 eligible districts. While setting up model colleges, preferences was given to the following areas: (a) Hilly or border regions populated with higher official share of minority and tribal population, (b) Areas with no college within a radius of 10 kilometres, and (c) Rural areas with reasonably good transportation facilities. The model colleges established under this scheme had to be either a constituent unit or permanently/temporally affiliated with a university covered under section 12B of the UGC Act.

Colleges established under this program received grants from the state as well as central government/ government funded bodies. The state government had to provide land to

[9]https://indianexpress.com/article/education/indias-gross-enrolment-ratio-in-higher-education-up-by-0-7-5012579/

[10]This program was termed as *providing financial assistance to New Model Colleges in Educationally Backward Districts.*

establish the model college. Two-thirds of the non-recurring and entire recurring expenditures were also to be funded by the state government with a provision for further appreciation in capital expenditure in future.

The affiliating universities were responsible for ensuring that the funds provided to the colleges were being utilized for infrastructural development. While establishing the model colleges is a collaborative project between the UGC and the state governments, the latter were allowed to collaborate with non-profit bodies or to enter into partnership arrangements with a for-profit organization under a *public-private partnership* arrangement clause.

In 2013, this program was subsumed under the new centrally sponsored 'Rashtriya Uchchatar Shiksha Abhiyan'(RUSA). The primary objective of RUSA was to improve access, equity and quality in higher education through planned development of higher education at the state level. The perception was that this will be an instrument to harmonize national program for funding state universities and colleges through a single over-arching umbrella body. RUSA's key actions and funding areas included construction of new model colleges (general and professional) as before. In addition, the policy was also geared towards upgradation of existing autonomous colleges to universities, conversion of colleges into cluster universities and provision of infrastructure grants to colleges and universities.[11]

3 Data

3.1 College Expansion and Eligibility

Data on the year of establishment for all colleges comes from the All India Survey on Higher Education (AISHE). This is an annual web-based survey conducted by the Ministry of Human Resource Development (MHRD) from all institutions engaged in higher education and is available from 2011 onwards. This survey collects information about the year of establishment, programs offered, teachers, infrastructure and so on. We use 2018 AISHE data for our analysis, which contains an exhaustive list of all the colleges of India, their location and their year of establishment. From this data, we construct a panel dataset of number of new colleges constructed in each district by year between 1980 to 2017.[12]

[11] All state institutions were eligible for these grants regardless of the GER of the districts.

[12] One caveat of this data is that if a college exited or closed during this period, we will not observe it. However, college construction has been increasing over time in India. The empirical strategy we employ is reliant on trends, and hence less susceptible to bias from this caveat.

Information about the treatment districts was obtained from the University Grant Commission of India. Out of the 593 districts in the country (as per the 2001 census), 374 districts with GER lower than 12.4 percent were identified as educationally backward districts. These are the districts eligible for the grant. For our analysis, we consider these districts as treatment districts and the remaining as controls. Hence, our estimation will yield Intent-To-Treat (ITT) estimates. In order to compute the 2001 GER for the districts, we rely on the same algorithm used by the government to establish eligibility. This algorithm uses the Census of India 2001 data.

3.2 Educational and Other Outcomes

We use household data from the fourth round of the National Family and Health Survey (NFHS-4), conducted in 2015-16. By this time, Indian districts had expanded to 640 in total. The survey covers all the 640 districts of India spanning the 29 states and 6 union territories. NFHS has four broad modules: (a) Household survey, (b) Woman's survey, (c) Man's survey, and (d) Bio-markers. We rely on the woman's survey for most of our analysis. The woman's module collected data on 699,686 women from about 568,200 households on various demographic and socio-economic characteristics such as education, age of marriage, asset ownership, employment, autonomy in decision making and domestic violence. It also covered a variety of health-related outcomes, such as fertility, knowledge and use of contraceptives, and child health. In addition, for a subset of women, the survey also collected information on the respondent's husband's education and employment.

3.3 Employment and Wages

Data on employment and wages comes from the Periodic Labor Force Survey (PLFS) conducted by NSSO during the period July 2017 to June 2018. [13] Geographically, the survey covers the whole of India except villages in Andaman and Nicobar Islands. Similar to the earlier Employment and Unemployment rounds of the NSSO, PLFS uses a stratified multi-stage design for sampling. However, certain changes were introduced in the survey methodology, data collection mechanism and sampling design of the PLFS vis-a-vis the earlier rounds,

[13] Relative to the earlier quinquennial rounds of the NSSO employment-unemployment survey, the PLFS is designed to provide more frequent estimates of labor force indicators in urban and rural areas (quarterly and annually, respectively). In addition, PLFS also provides estimates on additional aspects of the data (such as hours worked) which were not available earlier.

making comparison across the different rounds difficult.[14] The 2017-18 wave collected information from 102,113 households and spanned 433,339 persons (246,800 in rural areas and 46,000 in urban areas).

3.4 Other Ancillary Data

District-level economic and demographic data (e.g., literacy rate, unemployment rate, gender ratio, percentage of population residing in rural areas) come from the 2011 Census of India. We use 2011-12 poverty estimates by Mohanty et al. (2016), who calculated these by pooling the 66th and 68th rounds of consumption expenditure carried out by the National Sample Survey. Night light data comes from National Geophysical Data Center (NGDC) at the National Oceanic and Atmospheric Administration (NOAA).[15]

3.5 Summary Statistics

We report the summary statistics of the outcome variables for the full sample in Table 1. The summary statistics highlight poor socio-economic condition of women. The average years of schooling is around 7 years with only 11 percent of the women enrolling in colleges. On average, females get married at younger ages with roughly 44 percent marrying before 18, the legal age of marriage in India. Women's participation in the labor market is very low at only 30 percent. The actual and desired fertility is also quite high - 1.88 and 2.27, respectively.

Panel A of Appendix Table A.1 compares women in the treated and control districts with respect to several outcome variables for the cohort born after 1998 in our sample. We can see that women in treated districts perform much worse in proxies for educational attainment and child marriage. This is not surprising, because by design the treated districts are educationally backward districts and are therefore, the lagging regions. The backwardness in educational attainment is also reflected in 2001 population census (Panel B of Appendix Table A.1).

[14] Both surveys use a stratified multi-stage design, but there are differences in the criterion used for the stratification in the first and second stages. Moreover, a rotational panel sampling design is used in urban areas where each selected household is visited four times. There are no revisits in the rural sample. For more details on the sampling design and differences relative to the previous rounds, refer to the annual report of the PLFS: http://www.mospi.gov.in/sites/default/files/publication$_reports/Annual$%20$Report$%2C%20$PLFS$%202017–18$_3$1052019.pdf

[15] For a detailed discussion of the night light data, see Henderson et al. (2012).

4 Empirical Strategy

The main identification challenge here is that colleges are not randomly located across regions. For example, local economic or political conditions can influence college investment as well as the human capital and status of women.

In order to address this endogeneity issue, we rely on policy generated sharp variation in college expansion in India. As mentioned before, the government's 'model college policy' provided college construction grants. This college expansion subsidy program commenced in 2008 and was targeted based on GER in higher education in 2001 in the various districts. The government used the national average GER in 2001 as a threshold for allocating the subsidy. Districts with GER below this threshold were eligible for these grants. There were a total of 374 such districts. Note that the program was based on the overall GER and not the gender-specific GER. Figure 3 shows the map of the eligible districts to highlight this spatial variation. We utilize these two sources of variation (temporal and spatial) to identify the causal estimates of access to colleges on development outcomes of women.[16]

We compare the 374 eligible districts to the remaining 266 districts of India before and after this policy. Though the use of a sharp threshold to identify eligible districts warrants using a regression discontinuity design, there are four reasons why we do not rely on it as our main identification strategy. One, there is a relatively small sample of districts in our data so we are not adequately powered for this analysis (640 districts). Second, preferential treatment was accorded to some areas (described in the background section), which undermines the use of this approach. Third, the educationally backward districts (those with GER below the national average) exhibited a statistically significant differential trend relative to the other districts prior to 2008. Finally, the expansion after the policy happened in the tails in districts with very low GER. In Appendix Figure A.1, we plot the number of new colleges constructed in a district-year as a share of respective district's total number of colleges in 2001 (sample period 2009-2017) in Panel A on either side of the cutoff of 12.4 GER. In panel B, we plot the growth in total number of colleges between 2001 and 2018 on either side of the threshold. The plots for the treated districts are steeper than the control and are downward sloping indicating that the highest growth of colleges occurred in the tails of the treated districts rather than close to the threshold of 12.4. These districts with low GER are plausibly not similar to the other districts.

These reasons motivate our trend-break approach which allows for selection on ob-

[16] College expansion continues to happen in all districts for variety of other reasons as well.

servables.[17]

We define the variable Treatment as[18] :

$$Treatment = \begin{cases} 1, & \text{if } I(GER < NationalAverage_{2001}) = 1 \\ 0, & \text{otherwise} \end{cases}$$

The empirical model is as follows:

$$\begin{aligned} C_{it} = \alpha_i + \beta_t + \gamma_1(Treatment \times [t - 1980]) + \gamma_2(Treatment \times [t - 2008]) \\ + \gamma_3(Treatment \times Post) + \epsilon_{it} \end{aligned} \tag{1}$$

Where, $[t - 1980]$ and $[t - 2008]$ are linear time trends denoting the overall trend for the entire sample period and post-trend after the policy change, respectively. We interact these two trends with the treatment indicator. γ_1 and γ_2 are the estimates.

For college expansion (number of new colleges), we use a panel spanning 1980-2017. We also conduct a sensitivity check and verify that the results are not sensitive to the period chosen.[19] *Post* is a dummy variable taking a value of 1 for the years after the policy change (2009 and after) and 0 otherwise. This variable enters the regression interacted with treatment status. The coefficient on this interaction, γ_3, captures the intercept change post-policy. We cluster the standard error at the district level to account for the possible serial correlation in errors.

Figure 3 plots the estimates of an event study analysis (blue circles with the solid line).[20] Recall that treated districts are the educationally backward districts with lower GER in 2001 and we expect these to have lower college access. The figure bears out our conjecture about college expansion in India. Treated districts experienced a slower college expansion before 2009. However, we observe a sharp trend reversal in 2009, which coincides with the year the policy became effective. The maroon squares in solid line plot the implied yearly

[17] This approach does not require parallel pre-trends assumption like the Differences-in Differences strategy. Burgess and Pande (2005) use this type of approach to identify causal impact of rural banks on poverty.

[18] Between 2001 and 2009, some of the eligible districts were split into 2. The parent district is considered as treated and the 27 offshoot districts are in the control group. However, results are not sensitive to including or excluding these districts.

[19] The periods of our different samples are specified in the results section.

[20] These are year-by-year differences-in-differences estimates.

estimates from the trend break coefficients.[21]

To bolster our analysis, we use structural trend break in time series to (i) test whether the year-specific coefcients γ_t exhibit a structural break in 2008, and (ii) what date do these coefficients exhibit the structural break. To this end, we perform a supremum Wald test.[22] The results indicate strong support for our identification design: (1) We reject the null hypothesis of no structural break in 2009 at the 1 percent significance level, and (2) The data reveals a structural break in year 2008, precisely when the policy came into effect.

5 Main Reduced Form Results and Discussion

5.1 College Expansion

We report the trend-break model results estimating equation 1 in Column 1 of Table 2. We can see that on an average, treated districts get 0.04 fewer colleges each year before the policy change. But annual college construction reversed trend in treated districts and spiked to 0.18 more colleges an average. The implied year-specific treatment effects from this trend-break estimation are depicted by the maroon squares in the solid line in Figure 2.

To show that the trend reversal observed in Table 2 is driven by the policy change, we augment the empirical analysis by conducting the trend-break estimation as a function of the 2001 district GER (instead of treatment). Using the base year 1980, reassuringly, Appendix Table A.2 reveals that the districts with higher GER in 2001 had a higher college expansion trajectory relative to the educationally backward districts until 2008. However, this trend reversed in 2009.

To shed light on whether other confounding variables lead to a spurious indication of a trend-break, we add a number of controls to our specification. First, we control for local economic conditions proxied by trends in night-time average luminosity. The concern is that for an alternative reason, the economic growth trajectory reverses direction and that causes the trend-reversal. However, Column 2 of Table 2 reveals that the results remain unchanged. Related to this, we also control for trends in poverty. We use different measures of poverty for 2011 – poverty gap (Column 3), squared poverty gap (Column 4), and headcount ratio (Column 5) – all interacted with the time trend. The trend reversal remains significant

[21] These coefficients have been imputed from the trend break estimates, Column 1, Table 2.

[22] Each supremum test statistic is the maximum value of the test statistic that is obtained from a series of Wald or LR tests over a range of possible break dates in the sample.

regardless of the poverty measure used. Finally, we test whether our results are sensitive to different sample periods. We report the result in Appendix Table A.3. It is evident that the estimates are not sensitive to the time period and remain statistically significant for a range of sample periods: 1980-2017, 1990-2017 and 2000-2017.

5.2 Educational Outcomes

Our reduced form analysis indicated that there is a sharp trend reversal in college expansion in 2009. Districts with low GER had a sharper increase in college expansion due to the college grants. We now turn to evaluating how this affected educational and other development outcomes for women. To this end, we compare cohorts of girls of college going age before and after the policy reform in a similar trend-break model. Our sample period includes birth years from 1965 to 1998. Individuals born between 1990 and 1998 were 18 years or younger at the time of the policy change (as our survey data is from 2016, we are able to use seven cohorts that are younger than the college eligible age of 18 in India. The youngest cohort is 11 years old in 2009 and 18 years old in 2016).[23]

We rely on reduced form regressions of the following form:

$$\begin{aligned} Y_{whit} = \alpha_i + \beta_t + \lambda_1(Treatment \times [t - 1965]) + \lambda_2(Treatment \times [t - 1990]) \\ + \lambda_3(Treatment \times Post) + \delta \times X_{hit}\epsilon_{it} \end{aligned} \tag{2}$$

We index an individual girl by w, household by h, district by i and individual's birth year by t. Y is the outcome variable of interest. $Post$ is a dummy variable taking a value of 1 if the individual is born in 1991 or later and 0 otherwise. X_{hit} denotes a vector of household characteristics such as household assets, number of family members, rural/urban dummy and ethnicity. We cluster the standard error at the district level.

We first see the impact on educational outcome of girls. In Panel (a) of Figure 5, we plot the estimates analogous to the two specifications in Figure 2. Blue circles with solid line are the estimates from an event study analysis of the effect of treatment on number of years of schooling. We see a remarkably similar trend for number of years of schooling: negative trend in number of years of schooling for cohorts born in or before 1990 (19 years or older in 2009, and thus not likely to benefit from the grant program). However, we observe

[23] If individuals born before 1990 are affected by this policy, say because of grade repetition, they become college eligible at a later age. In that case, we would get an attenuated result and effects would be larger post policy change.

a sharp trend reversal for cohorts born in 1991 and after. The Wald test rejects the null of no structural break in 1990 at the 1 percent significance level. A similar pattern of trend reversal is observed in case of college enrollment (Panel (b)) and college completion (Panel (c)) .

Table 3 reports the results of the trend-break analysis estimating equation 2 with the standard errors. Number of years of schooling (Column 1) and college enrollment (any college, not necessarily completed degree) (Column 4) reflect an analogous change in trend as college expansion. Prior to the policy change, both variables were trending negatively for the educationally backward districts (treatment districts). In these districts, girls attained 0.015 less years of schooling in each year, compared to the control districts. However, after the policy change, girls in treatment districts had higher educational attainment. Thus, we observe a trend reversal and the F-test reported at the bottom of the table to test the statistical significance, shows that the trend reversal is indeed significant. We also see a similar trend reversal in girl's enrollment into college (Column 4) and college completion (Column 5). These results are robust to inclusion of different controls (Appendix Tables A.4 and A.5). Highest level of education is primary, or secondary (Columns 2 and 3, respectively) shows an opposite trend and its reversal.

5.2.1 Heterogeneity

We compare the effect of the college construction grants program across gender. The results are reported in Appendix Table A.6.[24] Similar to girls, we see a trend reversal in college enrollment and the number of years of schooling for boys as well. However, the trend reversal in the number of years of schooling is more pronounced for girls indicating that girls benefit more than boys when educational institutes are made closer to home. To see whether the trend reversal is statistically significantly different for boys and girls, we perform a Chow test and find that male and female experience statistically different trend reversal in the number of years of schooling. However, the trend reversal in college enrollment is not significantly different for males and females.[25]

We then explore the impact of the grants program on educational outcome of girls across rural and urban locations. Table 4 suggests that the gains from the program with

[24]For this analysis, we use NFHS household roster, which collects information on all the males and females of the household. Hence the number of women in the household survey is higher than the number of women covered in the woman's survey. We limit our sample to individuals aged between 18 and 51.

[25]The results remain qualitatively similar when we use PLFS data which includes more cohorts in the post period (Table A.7).

respect to the number of years of schooling is twice as large in rural areas than the urban areas – an annual increase of 0.13 in the rural areas compared to an annual increase of 0.06 in the urban areas. The Chow test for statistical equivalence of the two estimates indicates that the difference is statistically significant. We also observe a larger trend reversal in the rural areas compared to urban areas with respect to college enrollment.[26]

Next, we look at the heterogeneity in the effect of the program on educational attainment across different wealth quantiles of the households. Results are reported in Table 5. It is evident that all types of households benefited almost equally from the college expansion program with respect to the number of years of schooling.[27] On the contrary, college enrolment across wealth groups does vary: the college enrollment of girls monotonically increased with the wealth level of the households.[28]

5.3 Returns to Education: Labor Market and Marriage Market Outcomes

We use the PLFS 2017-18 to examine the labor market outcomes. In Table 6, we find that the likelihood of employment for females changes trend and becomes positive after the policy change (Column 1). While self employment and wage employment do not exhibit statistically significant changes (Columns 2 and 3), salaried employment changes to a positive and statistically significant trend post policy (Column 4). In India, around 80 percent of women in the age group of 18-59 are not in the labor force (Census of India), a trend that has also received attention from researchers (Afridi et al., 2018; Fletcher et al., 2017). Access to local colleges increases the likelihood of completing college and participating in the labor force for women. Advancing measures that enhance access to colleges could have a bearing on this trend.

In Appendix Table A.9, we document the trends for males. Here we observe a positive trend in employment which reverses to a small negative trend post policy change. However, as our F-test at the bottom of the table indicates, this is not a statistically significant trend change. A notable difference for men is that there is a reduction in self-employment and an increase in salaried employment with both trend reversals being significant. This indicates

[26] However, the Chow test suggests that this difference across the urban and rural areas is not statistically significant.

[27] The Chow tests do not reject the equality of the coefficients across different wealth groups.

[28] Chow test validates that the difference in trend reversal between the poorest or poorer household with the richer or richest households are statistically significant.

substitution for men from self-employment to salaried work post policy change.

In Table 7, we summarize the analysis for earnings for both females and males. Consistent with the increase in salaried employment of females reported in Table 6, we find a positive trend emergent after the policy change for salary earnings of women (Column 2) but not wage earnings (Column 1). The trend break is also statistically significant for salary earnings (F-test at the bottom of the Table). For males, the trend changes sign from negative to positive for salary earnings but is not significant. Wage earnings become negative post policy reform and the post trend is statistically significant. However, the F-test indicates an insignificant trend break.

We also shed light on the traits of the husbands subsequent to the change in policy. In Appendix Table A.8, we first examine education in terms of number of years of schooling and enrollment in college. We find evidence of premium on 'less education' in the marriage market. More educated men were marrying the less educated women in the treated areas. There is a negative trend (Column 1) in the post period albeit it is not significant. In the short run, men do not change their behavior and plausibly men with more years of schooling marry the more educated women after the policy change. Prior to the policy, husband's being employed was trending negatively (Column 3). However, there is a trend reversal in husband's employment with more husband's participating in non-agricultural work (Columns 3 and 5). Hence, it seems there are benefits in the marriage market.

5.4 Other Development Outcomes

We are interested in examining whether this improvement in educational outcomes then generates other synergistic improvements in development outcomes of interest. Social norms can influence women's labor market participation. However, there can be other empowering aspects of women's life that change for the better due to access to colleges.

5.4.1 Marriage Age and Child Marriage

A vast literature focuses on benefits of delayed age of marriage of girls (e.g., Field and Ambrus, 2008; Jensen and Thornton, 2003). In Appendix Figure A.2, we plot the distributions of the age of marriage for the marriages convened in 2005 and 2012 using the NFHS wave 4 survey data. We observe that the age of marriage has shifted to the right in 2012. We ascertain whether college expansion has contributed to this change in marriage age of girls.

In Table 8, we document the results of our trend-break specification for child marriage. Age of marriage was consistently lower in the treated districts for cohorts eligible for college prior to the policy reform. But it switched in 2009 and the trend reversed. There was also a statistically significant positive intercept shift. Legal age of marriage in India is 18. Higher proportion of girls were being married before the age of 18 before the policy in the treated districts and subsequently this reversed direction. In terms of magnitude, this is a modest annual gain of 2 percent of a standard deviation.

5.4.2 Fertility

We then examine the effect of college expansion on actual and desired fertility. Earlier literature suggests increased level of female education increases the age of marriage and reduces fertility (Currie and Moretti, 2003; Breierova and Duflo, 2004). We report the effect of expanded college access on fertility in Table 9. We can see that treated districts experienced faster decline in number of children ever born (Column 1) or in the number of living children (Column 2) compared to those of the control districts post the policy change. Similarly, the rate of decline in the number of desired children was higher in the treated districts after the policy change (Column 3). However, we find no changes in son preference (Column 4). We also investigate the use of contraceptives. Availability of contraceptives in US increased professional education for women and raised the age at first marriage (Goldin and Katz, 2002). In the Indian context, we examine if college access influences the use of contraceptives and do not find an effect on the knowledge of modern contraceptives and use (Columns 5 and 6, respectively).

5.4.3 Asset Ownership

In Table 10, we summarize our findings about asset ownership. Ownership of larger durable assets such as land or house by women was no different in the treated areas prior to the introduction of the policy and that did not change in the post policy period either. Having a bank account exhibited the same pattern. One change that did occur was mobile phone ownership. Research points out that college going women are concerned about their safety (Borker, 2017) and safety enhancing investments are made by women when empowered (Li et al., 2019). This trend could be on account of such safety enhancing investments by girls enrolled in colleges.

5.4.4 Autonomy in Decision Making

Unlike developed countries, women in developing countries are not key decision makers in the household. They typically have little to no say in making large purchases or even seeking medical care. They are also not allowed to go out of the house unaccompanied or without permission from husband or his family. We examine if college access also increases women's autonomy at making such decisions. Results are reported in Table 11. We see a trend reversal in decision making regarding health care, large purchase and visiting relatives. This indicates an increased role of women within the household regarding important decision making. College access results in empowerment of women in certain socio-economic spheres.

5.4.5 Domestic Violence: Incidence and Reporting

Intimate partner violence is also commonly accepted in India. Domestic violence effects not only the women and their children but also has externalities for the peers of the exposed children (Carrell et al., 2018). We examine the consequences of college access on four categories of domestic violence reported in the NFHS wave 4. These are emotional, less severe physical, more severe physical, and sexual violence.[29] In Table 12, we observe a trend change for less severe violence. However, the Benjamini–Hochberg 'false discovery rate' q-values (Benjamini and Hochberg, 1995) reported at the bottom of the table to account for multiple hypothesis testing indicates that none of these categories have a significant trend break.

6 Conclusion

This research sheds light on the consequences of access to local colleges for women in a developing country setting. While a body of work featuring US college expansion focuses on understanding returns to education in the labor market, there is not much evidence from developing countries. Our paper provides novel evidence that access to college improves educational attainment for girls. Consistently, we find evidence of returns in the labor market. Unlike this prior literature, we also find evidence of returns in the marriage market. College access in such settings can have other welfare enhancing consequences such as enhancing the

[29]Emotional violence includes humiliation in front of others or threat of harm or or insult or made to feel bad by husband/partner. Less severe physical violence includes being pushed or shaken or slapped, or having arms twisted or hair pulled by husband/partner. More severe physical violence includes being kicked or dragged, strangled or burnt, and being threatened with knife/gun or other weapon by husband/partner. Finally, sexual violence includes being physically forced into unwanted sex or sexual acts by husband/partner.

autonomy of women, pushing the age of marriage, and declining fertility. We shed light on these outcomes and find that access to local colleges increases women's autonomy, reduces fertility and increases the age at marriage.

From policy perspective, our findings negate the long standing donor community view that investment in higher education is equity-detracting and has lower social return. We show that the effect of increased college access on education is higher for girls compared to boys. We also document positive developmental spillover of investing in higher education across a number of welfare facets of women. Thus, investment in higher education in the form of increased access to local colleges has the potential to empower women and reduce the gender disparities in developing countries.

This paper also furthers our understanding of labor market trends for women in India. A salient finding of our paper is that the increased years of schooling and college enrollment and completion translate into higher employment opportunities and salaried earnings for girls. India has witnessed a secular decline in women's labor market participation despite continual increase in girls' schooling over time (Afridi et al., 2018; Fletcher et al., 2017). This puzzling pattern has been subject of many speculative explanations. Amid rising concerns for women's safety, lack of local educational opportunities might be affecting women human capital thus affecting the returns to labor market. According to a recent global poll conducted by Thompson-Reuters in 2018, India has been voted the unsafest place for women in the world. Borker(2019) shows that safety concerns affect quality decisions of females in India. Our findings imply that improving access to education alone is may not be effective at increasing employment opportunities for girls. Proximity of the higher education institutions may matter for not only reducing the cost of access but also the cost of safety. Complementarity between increased local college access and other demand side interventions such as increasing local recruiting efforts (Jensen, 2012) might be better suited to increasing women's labor force participation. Investigating how these levers align is an important avenue for future research.

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Table 1: Full Sample Outcomes Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A: National Family and Health Survey					
Years of schooling	699,686	6.73	5.19	0	20
Highest level of education is primary	699,686	0.13	0.33	0	1
Highest level of education is Secondary	699,686	0.48	0.50	0	1
Enrolls into college	699,686	0.11	0.32	0	1
Age at marriage	514,112	18.27	4.30	0	49
Marriage before 15 years of old	514,112	0.15	0.36	0	1
Marriage before 18 years of old	514,112	0.44	0.50	0	1
Currently working	122,351	0.23	0.42	0	1
Member of workforce	122,351	0.30	0.46	0	1
Participate in agricultural work	122,351	0.15	0.36	0	1
Participate in non-agricultural work	122,351	0.15	0.36	0	1
Owns house	122,351	0.39	0.49	0	1
Owns land	122,351	0.30	0.46	0	1
Has a bank account	122,351	0.52	0.50	0	1
Has a mobile phone	122,351	0.46	0.50	0	1
Children ever born	699,686	1.88	1.82	0	17
ideal no of children	693,891	2.27	1.00	0	6
Has son preference	692,894	0.20	0.40	0	1
Knows about modern contraceptive method	699,686	0.98	0.15	0	1
Uses modern contraceptive method	534,746	0.43	0.50	0	1
Panel B: Periodic Labor Force Survey					
Participates in labor force	114,307	0.22	0.42	0	1
Self employed	114,307	0.05	0.21	0	1
Wage worker	114,307	0.04	0.20	0	1
Salaried worker	114,307	0.07	0.26	0	1
Wage earning	114,196	70.3	389.4	0	4400
Salary earning	113,901	1023.1	4021.7	0	40000

Table 2: Effect Of the Grants policy on Number of Colleges Constructed

	Number of College Constructed				
VARIABLES	(1)	(2)	(3)	(4)	(5)
Treatment x (T-1980) trend	-0.0403***	-0.0536*	-0.0354**	-0.0374***	-0.0320**
	(0.0141)	(0.0281)	(0.0140)	(0.0142)	(0.0137)
Treatment x (T-2009) trend	0.183***	0.187***	0.179***	0.185***	0.168***
	(0.0485)	(0.0661)	(0.0483)	(0.0489)	(0.0474)
Treatment x Post	-0.0170	0.0829	-0.155	-0.141	-0.173
	(0.198)	(0.241)	(0.205)	(0.205)	(0.206)
Control	**None**	**Night Light**	**Poverty gap**	**Squared poverty gap**	**Head count ratio**
Observations	24,088	15,825	23,442	23,442	23,442
R-squared	0.420	0.495	0.434	0.431	0.436
F stat	13.05	7.345	13.20	13.70	12.11
Sig. level	0.000327	0.00691	0.000304	0.000234	0.000538

Notes: Data is from 2018 AISHE. The unit of analysis is at district-year. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for years 2009 and after and 0 otherwise. All regressions include district and year fixed effects. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 3: Effect of the Grants Policy on Educational Attainment of Girls

VARIABLES	(1) No of years of schooling	(2) Highest level of education is primary	(3) Highest level of education is Secondary	(4) Enrolls into college	(5) Finishes college
Treatment x Overall trend	-0.0158***	0.00183***	0.00174***	-0.00222***	-0.00205***
	(0.00543)	(0.000359)	(0.000532)	(0.000361)	(0.000336)
Treatment x Post Policy trend	0.134***	-0.00441***	-0.00586***	0.0142***	0.0144***
	(0.0170)	(0.000841)	(0.00171)	(0.00174)	(0.00170)
Treatment x Post	-0.0345	0.00262	0.0560***	-0.0430***	-0.0239***
	(0.0659)	(0.00392)	(0.00866)	(0.00739)	(0.00637)
Observations	650,773	650,773	650,773	650,773	650,773
R-squared	0.470	0.036	0.186	0.206	0.181
F stat	63.32	11.67	7.081	61.32	73.50
Sig. level	0	0.000675	0.00799	0	0

Notes: Data is from NFHS woman's survey. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 4: Effect of the Grants Policy on Educational Attainment of Girls by Region

	Years of Schooling		College Enrollment	
	Urban	Rural	Urban	Rural
VARIABLES	(1)	(2)	(3)	(4)
Treatment x Overall trend	0.00721	-0.0278***	-0.000365	-0.00217***
	(0.00634)	(0.00630)	(0.000565)	(0.000345)
Treatment x Post Policy trend	0.0645***	0.126***	0.00895***	0.0118***
	(0.0212)	(0.0197)	(0.00281)	(0.00181)
Treatment x Post	-0.0535	-0.0463	-0.0187	-0.0436***
	(0.0941)	(0.0824)	(0.0119)	(0.00840)
Observations	191,854	458,919	191,854	458,919
R-squared	0.387	0.468	0.204	0.161
F stat	12.70	36.55	11.28	37.83
Sig. level	0.000392	2.56e-09	0.000832	1.37e-09

Notes: Data is from NHFS woman's survey. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 5: Effect of the Grants Policy on Educational Attainment of Girls by Wealth

VARIABLES	(1) Poorest	(2) Poorer	(3) Middle	(4) Richer	(5) Richest
Panel A: Dependent variable is Number of years of schooling					
Treatment x Overall trend	-0.0284***	-0.0130*	-0.00123	-0.00274	0.0146*
	(0.00667)	(0.00679)	(0.00787)	(0.00788)	(0.00763)
Treatment x Post Policy trend	0.0503**	0.0630**	0.0732***	0.0637***	0.0591***
	(0.0237)	(0.0251)	(0.0254)	(0.0241)	(0.0218)
Treatment x Post	-0.174	0.0318	-0.0667	-0.0132	-0.0130
	(0.146)	(0.127)	(0.117)	(0.113)	(0.111)
Observations	121,901	137,518	136,770	130,026	124,558
R-squared	0.325	0.351	0.355	0.310	0.213
F stat	1.023	5.111	9.866	7.389	12.57
Sig. level	0.312	0.0241	0.00176	0.00674	0.000420
Panel A: Dependent variable is College enrollment					
Treatment x Overall trend	-0.000310**	-0.000573**	-0.000585	-0.000875	-0.000709
	(0.000132)	(0.000237)	(0.000406)	(0.000569)	(0.000625)
Treatment x Post Policy trend	**0.00217***	**0.00463*****	**0.00545*****	**0.00912*****	**0.0111*****
	(0.00122)	(0.00166)	(0.00211)	(0.00294)	(0.00325)
Treatment x Post	-0.0156**	-0.0231**	-0.0297***	-0.0333**	-0.0154
	(0.00714)	(0.00900)	(0.0109)	(0.0133)	(0.0149)
Observations	121,901	137,518	136,770	130,026	124,558
R-squared	0.039	0.062	0.091	0.120	0.150
F stat	2.673	7.198	6.305	9.501	11.30
Sig. level	0.103	0.00749	0.0123	0.00214	0.000820

Notes: Data is from NHFS woman's survey. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 6: Effect of the Grants Policy on Female Employment

VARIABLES	(1) Any employment	(2) Self-employment	(3) Wage employment	(4) Salaried employment
Treatment x Overall trend	-0.000371	2.56e-05	-0.000327	0.000524
	(0.000586)	(0.000322)	(0.000283)	(0.000347)
Treatment x Post Policy trend	0.00304*	-0.000253	-0.000142	0.00287***
	(0.00161)	(0.000631)	(0.000587)	(0.00110)
Treatment x Post	-0.0202*	0.00677	0.00178	-0.0179***
	(0.0112)	(0.00512)	(0.00504)	(0.00674)
Observations	113,503	113,503	113,503	113,503
R-squared	0.157	0.063	0.084	0.051
F stat	3.125	0.179	0.807	12.77
Sig. level	0.0776	0.673	0.369	0.000379

Notes: Data is from PLFS survey. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 7: Effect of the Grants Policy on Wage Earnings

	Female		**Male**	
VARIABLES	Logarithm of wage Earning (1)	Logarithm of salary Earning (2)	Logarithm of wage Earning (3)	Logarithm of salary Earning (4)
Treatment x Overall trend	-0.00184	0.00476	0.00433	-0.00117
	(0.00190)	(0.00402)	(0.00317)	(0.00730)
Treatment x Post Policy trend	0.000343	0.0209*	-0.0174*	0.0193
	(0.00394)	(0.0114)	(0.00974)	(0.0218)
Treatment x Post	-0.0205	-0.116	-0.0431	0.139
	(0.0352)	(0.0713)	(0.0646)	(0.122)
Observations	113,392	113,097	112,088	113,620
R-squared	0.079	0.071	0.067	0.280
F stat	0.183	6.589	2.274	1.103
Sig. level	0.669	0.0105	0.132	0.294

Notes: Data is from PLFS survey. The unit of analysis is at individual level. The dependent variables are in Logarithmic form. We exclude a few observations (top 1 percent of wage/salary distribution) as outliers. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 8: Effect of the Grants Policy on Child Marriage

VARIABLES	(1) Age at marriage	(2) Married before 18 years of age
Treatment x Overall trend	-0.0136**	0.00179***
	(0.00537)	(0.000465)
Treatment x Post Policy trend	0.0525***	-0.00753***
	(0.0174)	(0.00260)
Treatment x Post	0.166**	0.00339
	(0.0692)	(0.00869)
Observations	512,391	512,391
R-squared	0.177	0.132
F stat	5.602	5.026
Sig. level	0.0182	0.0253

Notes: Data is from NHFS woman's survey. Sample includes only ever-married women. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 9: Effect of the Grants Policy on Fertility and Child Preference

VARIABLES	(1) Total children ever born	(2) Number of living children	(3) Ideal number of children	(4) Son prefernece	(5) Knows about modern contraceptive method	(6) Uses modern contraceptive method
Treatment x Overall trend	0.00223	0.00377*	7.12e-05	-6.11e-05	3.13e-05	0.000582
	(0.00252)	(0.00203)	(0.000969)	(0.000298)	(8.30e-05)	(0.000594)
Treatment x Post Policy trend	-0.0168**	-0.0171***	-0.00411*	-0.000911	0.000117	-0.00247
	(0.00655)	(0.00595)	(0.00234)	(0.000909)	(0.000728)	(0.00197)
Treatment x Post	-0.0367*	-0.0326*	-0.0100	-0.00373	-0.00266	0.00182
	(0.0197)	(0.0186)	(0.0102)	(0.00481)	(0.00194)	(0.00804)
Observations	526,570	526,570	645,974	645,034	699,686	526,458
R-squared	0.449	0.443	0.278	0.087	0.073	0.183
F stat	5.062	5.118	3.630	1.199	0.0433	0.890
Sig. level	0.0248	0.0240	0.0572	0.274	0.835	0.346

Notes: Data is from NHFS woman's survey. Column (1), (2) and (6) is based on only ever-married women whereas column (3)-column (5) is based on the whole sample. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 10: Effect of the Grants Policy on Asset Ownership by Female

VARIABLES	(1) Owns house	(2) Owns land	(3) Has a bank account	(4) Has a mobile phone
Treatment x Overall trend	0.000512	-0.000497	3.88e-05	-0.00100*
	(0.000506)	(0.000452)	(0.000559)	(0.000524)
Treatment x Post Policy trend	0.00218	-0.000573	0.00154	0.00613**
	(0.00205)	(0.00196)	(0.00264)	(0.00259)
Treatment x Post	-0.00424	0.0158	-0.00639	-0.00134
	(0.0122)	(0.0116)	(0.0138)	(0.0124)
Observations	113,612	113,612	113,612	113,612
R-squared	0.152	0.131	0.139	0.258
F stat	1.812	0.315	0.392	4.307
Sig. level	0.179	0.575	0.532	0.0384

Notes: Data is from NHFS woman's survey. Information on asset ownership was collected for a subset of all surveyed women. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 11: Effect of the Grants Policy on Female Decision Making

	Autonomy to Make Decisions Regarding				
	Health care	Large purchase	Visit relatives	Spend husbands' earning	Spend respondents' own earning
VARIABLES	(1)	(2)	(3)	(4)	(5)
Treatment x Overall trend	-0.000967	-0.00179	-0.000648	-0.00149	0.00107
	(0.00124)	(0.00128)	(0.00122)	(0.00137)	(0.00223)
Treatment x Post Policy trend	0.0265**	0.0267**	0.0293**	0.00989	-0.00612
	(0.0124)	(0.0131)	(0.0121)	(0.0137)	(0.0289)
Treatment x Post	-0.0980**	-0.0905*	-0.132***	-0.0358	0.0270
	(0.0437)	(0.0466)	(0.0424)	(0.0516)	(0.106)
Observations	86,618	86,618	86,618	85,982	20,034
R-squared	0.066	0.083	0.082	0.066	0.103
F stat	4.316	3.643	5.709	0.377	0.0305
Sig. level	0.0382	0.0568	0.0172	0.539	0.861
BH q-value	**0.095**	**0.095**	**0.086**	**0.674**	**0.861**

Notes: Data is from NHFS woman's survey. Information on autonomy in decision making was collected for a subset of all surveyed women. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. Column (5) is conditional on respondent's earning. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table 12: Effect of the Grants Policy on Domestic Violence

VARIABLES	(1) Experienced any emotional violence	(2) Experienced any less severe violence	(3) Experienced any severe violence	(4) Experienced any sexual violence
Treatment x Overall trend	0.000137	0.00101*	7.43e-05	-0.000162
	(0.000430)	(0.000539)	(0.000313)	(0.000302)
Treatment x Post Policy trend	-0.00310	-0.00909**	-0.00161	0.00242
	(0.00342)	(0.00454)	(0.00252)	(0.00263)
Treatment x Post	0.0154	0.0256	0.00779	7.00e-05
	(0.0131)	(0.0177)	(0.00975)	(0.0100)
Observations	65,899	65,899	65,899	65,899
R-squared	0.053	0.115	0.059	0.046
F stat	0.769	3.168	0.379	0.743
Sig. level	0.381	0.0756	0.538	0.389
BH q-value	**0.519**	**0.303**	**0.538**	**0.519**

Notes: Data is from NFHS woman's survey. Information on domestic violence was collected for a subset of all surveyed women. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.1: Co-variate Balance Between Treatment and Control Districts

VARIABLES	Control mean	Difference between Treatment and Control	Standard error of difference	Observations
Panel A: Household Data				
Years of schooling	9.232	-0.659***	0.104	25,128
Highest level of education is primary	0.056	0.0234***	0.005	25,128
Highest level of education is Secondary	0.872	-0.0494***	0.011	25,128
Enrolls into college	0.021	-0.00243	0.003	25,128
Age at marriage	15.770	-0.310**	0.126	2,648
Marriage before 15 years of old	0.153	0.0513***	0.019	2,648
Marriage before 18 years of old	0.913	0.0200	0.018	2,648
Currently working	0.135	0.00577	0.013	4,426
Member of workforce	0.176	0.0263*	0.016	4,426
Participate in agricultural work	0.099	0.0181	0.013	4,426
Participate in non-agricultural work	0.078	0.00814	0.011	4,426
Owns house	0.273	0.00270	0.020	4,426
Owns land	0.218	0.0101	0.017	4,426
Has a bank account	0.432	-0.0120	0.020	4,426
Has a mobile phone	0.264	-0.0295*	0.017	4,426
Children ever born	0.025	0.00597	0.004	25,128
ideal no of children	1.959	0.0237	0.032	24,734
Son Preference	0.118	0.0171**	0.007	24,704
Knows about modern contraceptive method	0.940	-0.00532	0.006	25,128
Uses modern contraceptive method	0.067	0.00112	0.011	3,063
Panel B: 2001 Census Data				
Male-female ratio	0.930	0.005	0.005	578
Percentage of SC/ST population	0.145	0.006	0.007	578
Percentage literate	0.592	-.09***	0.009	578
Grosse enrollment ratio in college	18.080	-9.93***	0.412	578
Labor force participation rate	0.385	.036***	0.005	578

Table A.2: College Expansion as a Function of 2001 GER

	Number of College Constructed		
Variables	(1)	(2)	(3)
GER in 2001 x (T-1980) trend	0.00411***	0.00454**	0.00564
	(0.00131)	(0.00189)	(0.00397)
GER in 2001 x (T-2009) trend	-0.0163***	-0.0168***	-0.0179***
	(0.00434)	(0.00484)	(0.00682)
GER in 2001 x Post	-0.00169	-0.00602	-0.0138
	(0.0143)	(0.0144)	(0.0174)
Observations	23,273	17,612	11,322
R-squared	0.424	0.479	0.544
F stat	13.22	13.10	12.83
Sig. level	0.000300	0.000320	0.000368
Sample period	**1980-2017**	**1990-2017**	**2000-2017**

Notes: Data is from 2018 AISHE. The unit of analysis is at district-year. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for years 2009 and after and 0 otherwise. All regressions include district and year fixed effects. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.3: Effect Of the Grants policy: Sensitivity to different Sample Period

	Number of College Constructed		
Variables	(1)	(2)	(3)
Treatment x (T-1980) trend	-0.0406***	-0.0462*	-0.0552
	(0.0141)	(0.0244)	(0.0560)
Treatment x (T-2009) trend	0.185***	0.190***	0.199**
	(0.0483)	(0.0562)	(0.0836)
Treatment x Post	-0.0281	0.0287	0.104
	(0.193)	(0.217)	(0.287)
Observations	24,016	17,696	11,376
R-squared	0.420	0.480	0.546
F stat	13.54	13.41	13.13
Sig. level	0.000253	0.000271	0.000313
Sample period	**1980-2017**	**1990-2017**	**2000-2017**

Notes: Data is from 2018 AISHE. The unit of analysis is at district-year. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for years 2009 and after and 0 otherwise. All regressions include district and year fixed effects. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.4: Effect of the Grants Policy on Years of Schooling with Different Controls

	Number of Years of Schooling				
VARIABLES	(1)	(2)	(3)	(4)	(5)
Treatment x Overall trend	-0.0158***	-0.0141**	-0.00972*	-0.0114**	-0.0120**
	(0.00543)	(0.00563)	(0.00532)	(0.00536)	(0.00540)
Treatment x Post Policy trend	0.134***	0.111***	0.118***	0.125***	0.128***
	(0.0170)	(0.0172)	(0.0161)	(0.0162)	(0.0164)
Treatment x Post	-0.0345	-0.00363	-0.0416	-0.0445	-0.0455
	(0.0659)	(0.0679)	(0.0703)	(0.0695)	(0.0691)
Control	**None**	**Night Light**	**Poverty gap**	**Squared poverty gap**	**Head count ratio**
Observations	650,773	647,796	635,437	635,437	635,437
R-squared	0.470	0.470	0.472	0.471	0.471
F stat	63.32	42.18	56.53	62.25	63.35
Sig. level	0	1.67e-10	0	0	0

Notes: Data is from NFHS woman's survey. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.5: Effect of the Grants Policy on College Enrollment with Different Controls

	College Enrollment				
VARIABLES	(1)	(2)	(3)	(4)	(5)
Treatment x Overall trend	-0.00222***	-0.00165***	-0.00178***	-0.00192***	-0.00199***
	(0.000361)	(0.000349)	(0.000325)	(0.000337)	(0.000345)
Treatment x Post Policy trend	0.0142***	0.0114***	0.0129***	0.0135***	0.0137***
	(0.00174)	(0.00175)	(0.00171)	(0.00172)	(0.00174)
Treatment x Post	-0.0430***	-0.0326***	-0.0402***	-0.0419***	-0.0426***
	(0.00739)	(0.00756)	(0.00777)	(0.00768)	(0.00767)
Control	**None**	**Night Light**	**Poverty gap**	**Squared poverty gap**	**Head count ratio**
Observations	650,773	647,796	635,437	635,437	635,437
R-squared	0.206	0.207	0.210	0.209	0.209
F stat	61.32	39.89	51.85	56.34	57.66
Sig. level	0	5.05e-10	0	0	0

Notes: Data is from NFHS woman's survey. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.6: Effect of the Grants Policy on Educational Attainment by Gender

	Number of years of schooling			Enrolls into college		
	All	Male	Female	All	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment x Overall trend	-0.00283	0.0157***	-0.0208***	-0.00133***	-0.000592*	-0.00208***
	(0.00395)	(0.00408)	(0.00476)	(0.000296)	(0.000333)	(0.000307)
Treatment x Post Policy trend	0.0718***	0.0457***	0.0973***	0.00615***	0.00647***	0.00575***
	(0.0102)	(0.00981)	(0.0131)	(0.000972)	(0.00110)	(0.00106)
Treatment x Post	-0.00980	-0.0394	0.0236	-0.0244***	-0.0246***	-0.0229***
	(0.0456)	(0.0482)	(0.0613)	(0.00465)	(0.00580)	(0.00518)
Observations	1,366,393	674,119	692,274	1,366,393	674,119	692,274
R-squared	0.337	0.301	0.424	0.170	0.164	0.194
F stat	63.72	53.17	46.31	32.33	37.13	15.31
Sig. level	0	0	0	1.98e-08	1.91e-09	0.000101

Notes: Data is from NFHS Household survey. This analysis cover all male and female aged between 18 to 51. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.7: Effect of the Grants Policy on Educational Attainment by Gender Using PLFS

	Female		**Male**	
	Enrolls into collge	College graduate	Enrolls into collge	College graduate
VARIABLES	(1)	(2)	(3)	(4)
Treatment x Overall trend	-0.000735	-0.000506	0.000527	0.000394
	(0.000541)	(0.000466)	(0.000575)	(0.000539)
Treatment x Post Policy trend	0.00664***	0.00966***	0.00334*	0.00545***
	(0.00204)	(0.00189)	(0.00173)	(0.00162)
Treatment x Post	-0.0344***	-0.0380***	-0.00871	-0.0119
	(0.0117)	(0.0113)	(0.0114)	(0.0105)
Observations	113,503	113,503	115,634	115,634
R-squared	0.112	0.092	0.077	0.069
F stat	10.47	30.27	6.097	17.55
Sig. level	0.00128	5.49e-08	0.0138	3.20e-05

Notes: Data is from PLFS. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.8: Effect of the Grants Policy on Husband's Education and Employment

VARIABLES	(1) No of years of schooling	(2) Enrolled into college	(3) Member of workforce	(4) Participate in agricultural work	(5) Participate in non-agricultural work
Treatment x Overall trend	0.0126**	3.55e-05	-0.000546**	-0.000632	8.63e-05
	(0.00565)	(0.000408)	(0.000212)	(0.000466)	(0.000479)
Treatment x Post Policy trend	-0.0132	0.000165	0.00411*	-0.00266	0.00677*
	(0.0341)	(0.00272)	(0.00225)	(0.00338)	(0.00355)
Treatment x Post	0.146	0.00240	-0.00610	0.0239*	-0.0300*
	(0.153)	(0.0123)	(0.00808)	(0.0145)	(0.0155)
Observations	91,719	91,719	91,161	91,161	91,161
R-squared	0.357	0.177	0.034	0.244	0.222
F stat	0.000327	0.00565	2.485	0.971	3.804
Sig. level	0.986	0.940	0.115	0.325	0.0516

Notes: Data is from NFHS woman's survey. Information on husband's education and employment was collected for a subset of all surveyed women. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. All regressions control for household assets, no of family members, rural/urban dummy and Caste. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.9: Effect of the Grants Policy on Male Employment

VARIABLES	(1) Any employment	(2) Self-employment	(3) Wage employment	(4) Salaried employment
Treatment x Overall trend	0.00117***	-0.000988	0.000173	0.000943
	(0.000404)	(0.000693)	(0.000452)	(0.000764)
Treatment x Post Policy trend	-0.00295*	-0.00420***	-0.000603	0.00551**
	(0.00165)	(0.00133)	(0.00142)	(0.00239)
Treatment x Post	0.0426***	0.00462	0.00305	0.00592
	(0.0112)	(0.0114)	(0.00875)	(0.0123)
Observations	115,634	115,634	115,634	115,634
R-squared	0.433	0.190	0.071	0.118
F stat	1.452	20.73	0.118	10.50
Sig. level	0.229	6.36e-06	0.731	0.00126

Notes: Data is from PLFS. The unit of analysis is at individual level. Treatment is a dummy variable taking a value of 1 if a district is eligible for the college construction grants i.e., the GER is lower than national average in 2001. Post is an indicator variable that takes the value 1 for individual born on or after 1991, (18 years or younger in 2009, when the intervention took place) and 0 otherwise. Overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of survey). Post trend goes from cohort birth years 1991 to 1998. All regressions include district and year fixed effects. F stat reports the F statistic from a test of equality of the coefficients in overall trend in treated districts (row 1) and post trend in the same (row 2). Standard errors are clustered at the district level. *** indicates significance at 1, ** at 5, and * at 10 percent level.

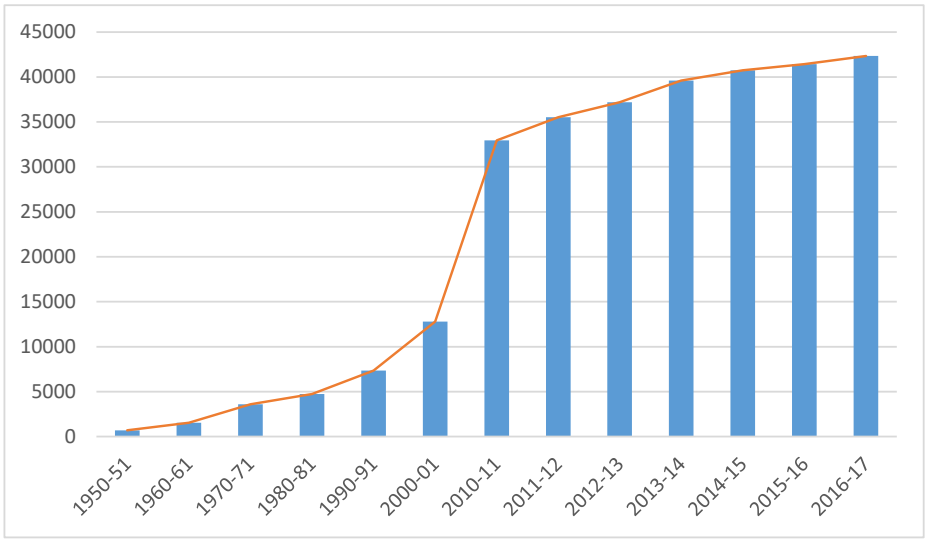


Figure 1: Total Number of Colleges in Different Years

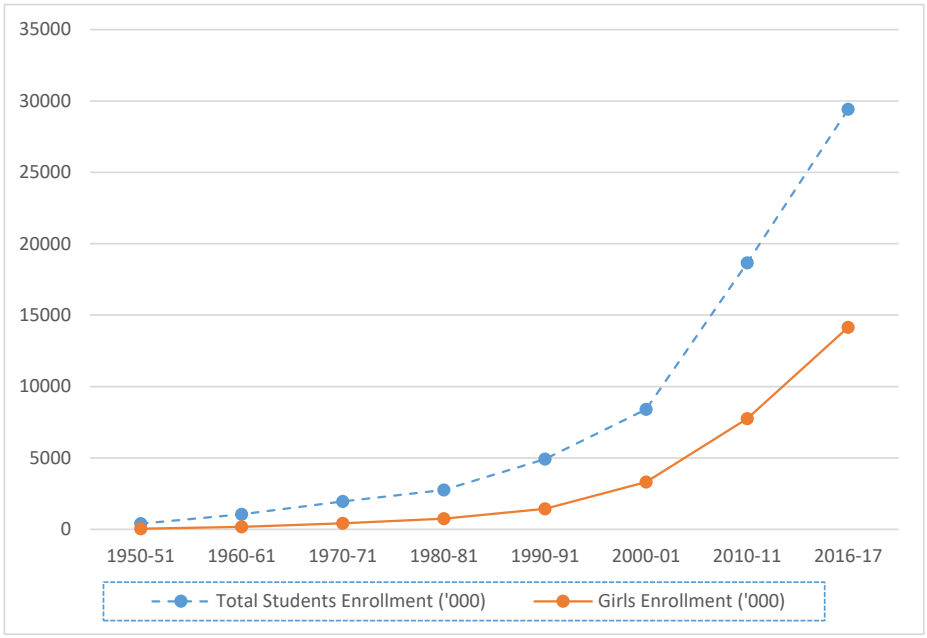


Figure 2: College Enrollment in Different Years

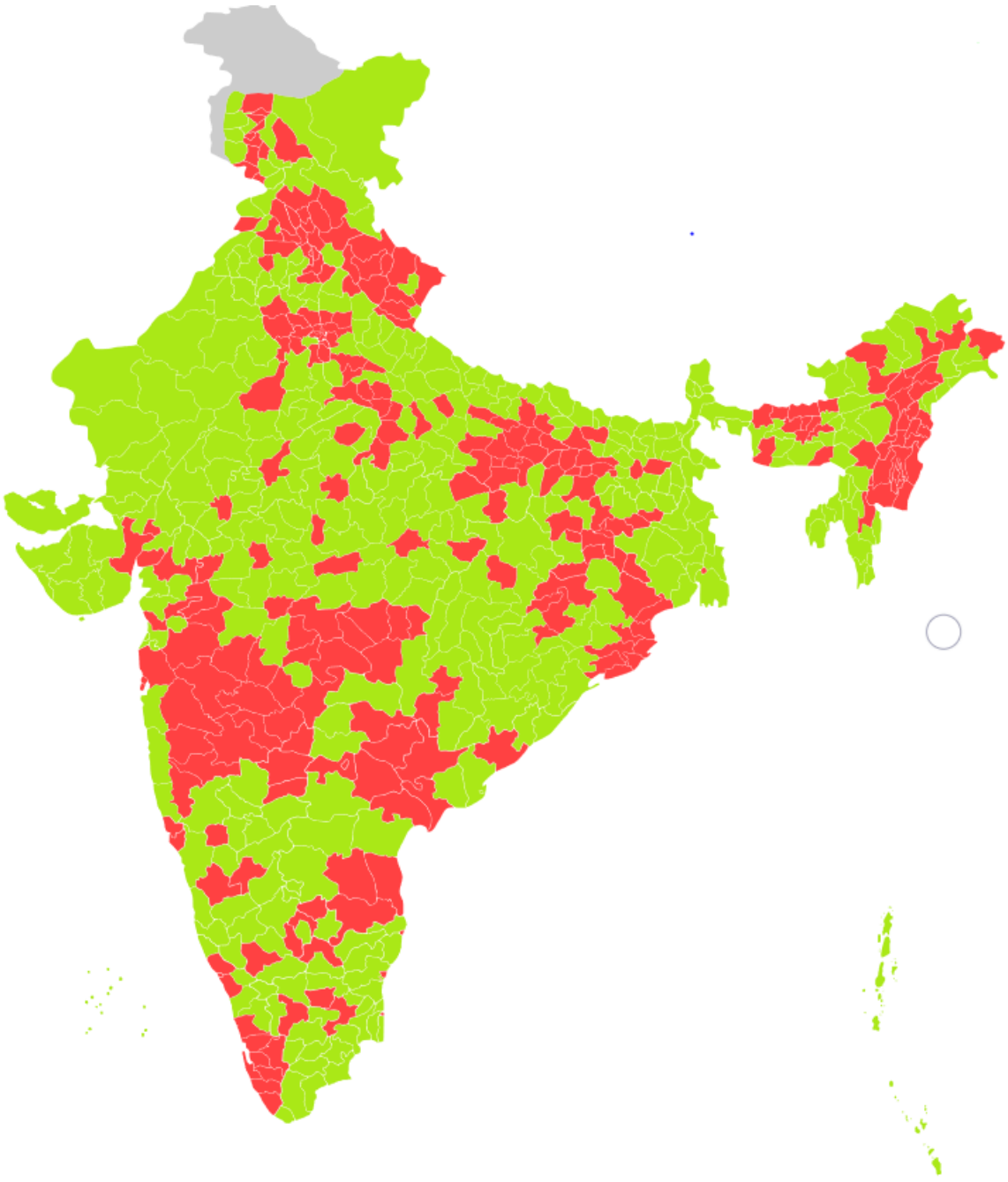


Figure 3: Treated Districts

Note: Treated districts are colored green while the control districts are colored red.

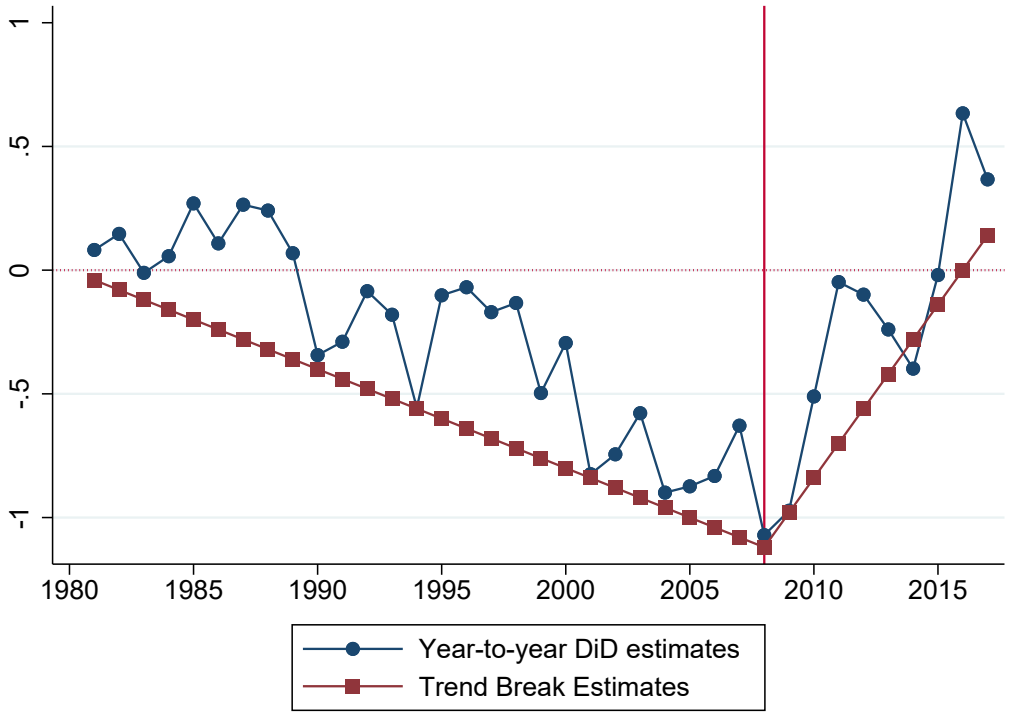
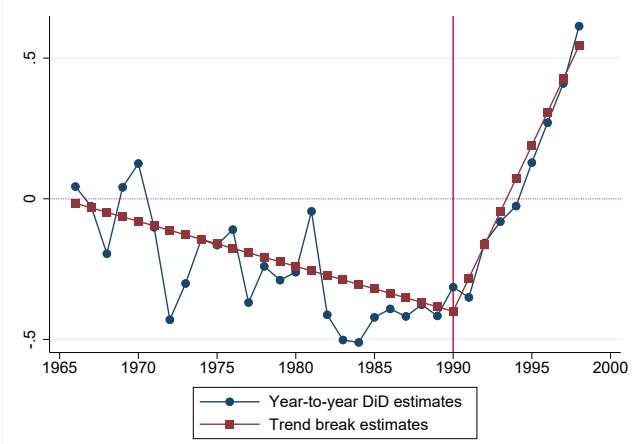
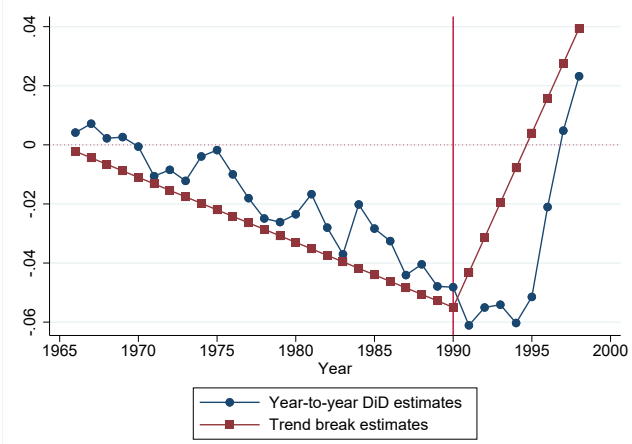


Figure 4: College Expansion by Treatment Status

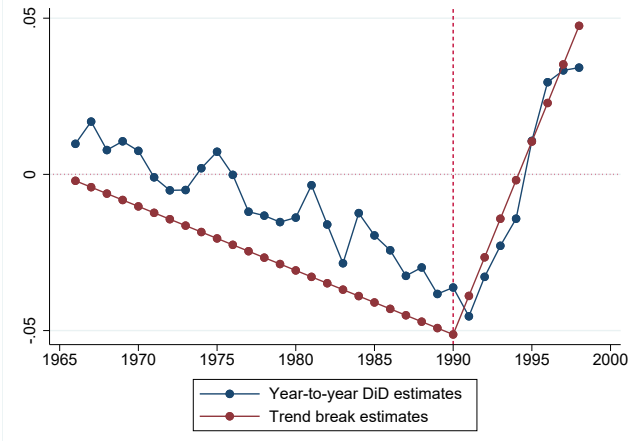
Notes: The series "Year-to-year DID estimates" graphs the annual coefficients on treatment from a Difference-in-difference regression of the form described in equation (1). The series "Trend break estimates" graphs the annual coefficient implied by the trend break model, column (1), Table 2 . In both cases, the dependent variable is the number of college constructed in a district.



(a) Number of years of schooling



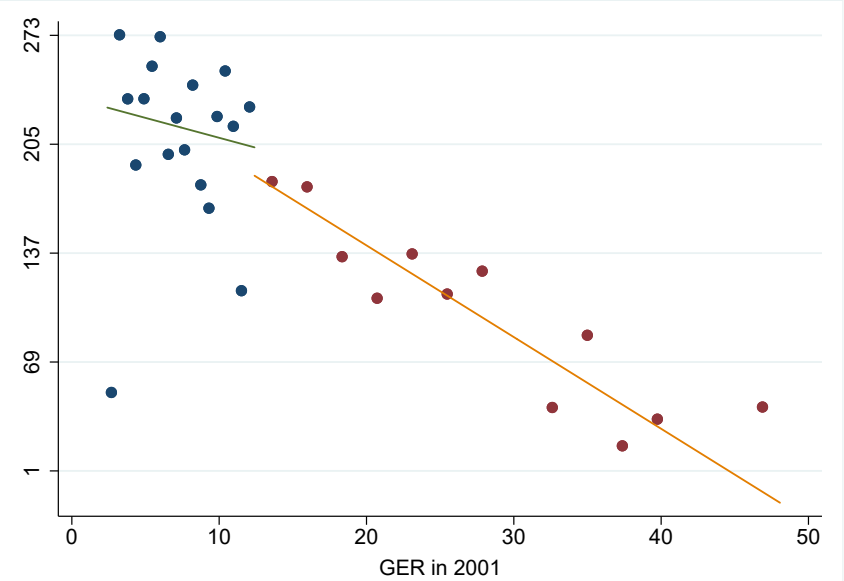
(b) College enrollment



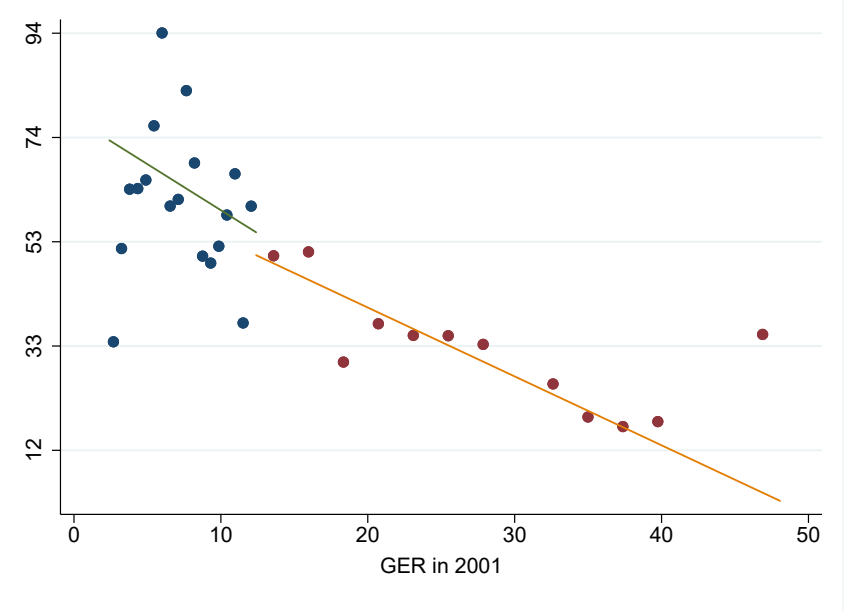
(c) College completion

Figure 5: Educational Attainment by Treatment Status

Notes: The series "Year-to-year DID estimates" graphs the annual coefficients on treatment from a Difference-in-difference regression of the form described in equation (1). The series "Trend break estimates" graphs the annual coefficient implied by the trend break model, Table 3. The dependent variable is number of years of schooling, college enrollment and college completion in panel (a), (b), and (c) respectively.



(a) Growth in colleges between 2001-08



(b) Growth in colleges between 2001-08

Figure A.1: 2001 GER and Growth in Colleges

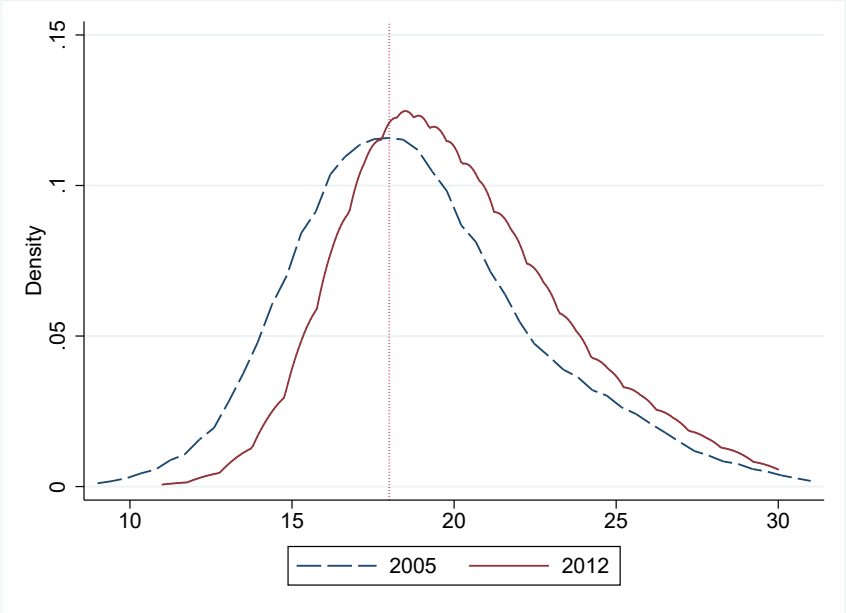


Figure A.2: Kernel Density of Age at Marriage