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 gross enrollment ratio lower than the national average in 2001. We use the variation generated by this program, 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 the number of years of schooling and college enrollment and completion for girls with positive spillovers for the neighboring districts. Boys too benefited, but the benefit for 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. JEL codes: I23, I22, I26, I24

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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 in low-income countries stood at 10 percent in 2012.[1] More saliently, there was a significant gender gap in the college enrollment rates in both South Asia and Sub-Saharan Africa – with 81 and 64 girls enrolled for 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 factor in giving rise to this pattern. As a result, a popular policy prescription to combat this issue has been the construction of more local colleges. Since girls traditionally 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.

A different school of thought identifies lower demand for education, and misplaced expectations 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 and/or engage in the 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 an adequate supply of colleges.[2]

Our study informs this debate by evaluating the consequences of a massive college expansion policy in India. With the objective 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 'Educationally Backward Districts' (EBD)

[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.

of India. These were identified as the districts where the GER in higher education was lower than the national average in 2001. We explore the variation in college expansion generated by this program to isolate the causal impact of college access on the human capital of girls, age at marriage as well as fertility choices 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 until 2001 was increasing, albeit slowly.In contrast, there was a very stark acceleration in the decade from 2001 to 2011. Our identification strategy 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, was followed by an uptick in college construction and enrollment. However, a notable sharp change in enrollment occurred during the period 2001-2011. Previously, women's enrollment in colleges was dismally low despite increases in overall enrollment. However, the decade between 2001 and 2011 saw an escalation in women's enrollment. In this paper, we first try to establish a causal increase in enrollment of women due to the expansion in college construction and then attempt to highlight its effects.

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 regions and birth cohorts. A sharp threshold was used for determining eligibility for the college construction grants. Districts with GER lower than the national average in 2001 were eligible for these grants. Average district GER, and not gender-specific GER, was used to ascertain eligibility. 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. Because there are only 640 districts in India (less so in 2001) and most of the expansion happened in districts with very low GER to begin with (in the tails of the GER distribution), we harness the panel dimension of the data for identification instead of relying on a regression discontinuity design exclusively. Even though treatment was based solely on the eligibility criterion, the districts with low GER (EBDs) were not necessarily similar to the high GER

[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 are 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.

districts. The low GER districts exhibited a striking negative trend in the education outcomes prior to the policy and resulting precipitous increasing in college construction. This makes the use of a differences-in-differences strategy less suitable.

Instead, we rely on a trend-break model. To identify the causal impact of the college expansion program, we compare 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. We use a long district-by-year panel (from 1980 to 2016) for estimating the changes in college construction. We then employ cohort-analysis for estimating the overall and post-policy trends in outcomes 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 due to differential data availability. Our empirical strategy controls for district and year fixed effects that absorb the time-invariant district characteristics and year-specific shocks common across all districts, respectively. We also account for an overall secular trend to absorb the pre-trends (albeit negative) in the treated districts.

Our reduced-form analysis first establishes 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 college completion for girls exhibit similar trends in the treated areas: much slower growth prior to the policy and a trend reversal with an acceleration in the post-policy period. Boys gain too from the policy as the expansion was based on overall GER and not gender-specific GER. The gains for girls, however, are much larger than boys.

With respect to the number of years of schooling, we find an annual increase of 0.069 for the treated cohorts (those born between 1991 and 1998) in the post-policy period. This translates to an average increase of 0.55 years of schooling (approximately 11 percent gain over the baseline

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

levels).These estimates are comparable to those documented for the US. Doyle and Skinner (2016) find an average increase of 0.56 years of education for individuals with a four-year college in their county at age 17. Similarly, Card (1993) find positive effects on education attainment of 0.32 to 0.38 years for those growing up near a college.

These effects vary by gender with a larger increase of 0.61 additional years of schooling for women born between 1991 and 1998 relative to a 0.5 years of additional schooling for men in analogous years. Likelihood of college enrollment and completion increase in an economically significant manner. For females in the aforementioned age group,the probability of enrolling in college increases 0.60 percentage points or 4.7 percent relative to the baseline average and probability of college completion goes up by 7.28 percent, respectively. [5]

In addition to changes 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 an improvement in women's autonomy in making household decisions. Intimate partner violence is not affected. Labor market outcomes for women improve significantly. Salaried employment estimated using the Periodic Labor Force Survey (PLFS) of India, conducted in 2017-18, saw a positive trend after the policy.[6] 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 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 NFHS-4).[7] Our findings reveal that better college access can increase college participation and completion among women in developing countries and improve their labor

[5] These estimates are comparable to female enrollment rates for populations that practice bride price in Indonesia documented by Ashraf et al. (2020).

[6] This is the type of work that college-educated would typically perform in India. While 32 percent of salaried workers are college graduates, only 1.73 percent of 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.

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

and marriage market outcomes. This occurs concurrently with delay in marriage age, which not only affects the welfare of women (Field and Ambrus, 2008; Jensen and Thornton, 2003), but that of their children as well (Sekhri and Debnath, 2014).

In a series of robustness checks, we bolster our estimation by showing that our results are not sensitive to trends in economic growth (proxied by night time luminosity) and poverty. Structural time series trend-break analysis indicates a break in trend in college construction and educational outcomes in 2009. We also rule out political changes or migration as alternative explanations. We find evidence of heterogeneity in college education outcomes by wealth but not by share of the rural population. We also find evidence of spatial spillovers. Women in adjacent neighboring districts that did not receive a new college also saw an improvement in educational outcomes. If we exclude these districts from our control group, the treatment effects are twice as large.

Our paper complements and extends three 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. A limited number of studies have focused on education or gender, but with the exception of Ashraf et al. (2020), no prior research focuses on the education and marriage outcomes of women. Duflo (2001) studies the 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).[8] Ashraf et al. (2020) implicate cultural practices as an important determinant of whether primary-school construction benefits girls or not. 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.

[8]Fertility choice consequences of a German college expansion program are studied by Kamhöfer and Westphal (2017). A strand of work also focuses on the consequences of more education for the empowerment of women (Friedman et al., 2016)

The second strand of papers debate whether social norms and traditional institutions impede women and girls from reaping the benefits of expanding educational opportunities by way of improved infrastructure or global economic integration. Evidence is mixed. While Cheema et al. (2018) and Jacoby and Mansuri (2011) indicate a *boundary effect* precluding women from utilizing improved educational facilities, Munshi and Rosenzweig (2006) show that traditional institutions disadvantage boys whereas girls gain from higher economic returns to education resulting from economic integration. Ashraf et al. (2020) document the mediating role of cultural norms on marriage and educational outcomes of school expansion. Our paper contributes to this literature by exploring the geographic spillovers of college construction and shows that women are able to benefit across district boundaries in a time period characterized by a high degree of globalization of the economy.[9]

The third strand of literature we complement relates to the geographic proximity of colleges and returns to educational attainment. A number of papers focusing on the United States (US) have used the presence (or absence) of a local college or distance from college as instruments to estimate the returns to education (Card, 1993; Kane and Rouse, 1995; Carneiro et al., 2011; Carneiro and Heckman, 2002; Doyle and Skinner, 2016). The majority of these papers have focused on the rapid higher education expansion phase in the US during 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 the 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.[10] In our study, we examine a variety of social outcomes for women in addition to educational outcomes and labor market outcomes. We also shed light on benefits in the marriage market.[11]

[9]FDI inflows to India rose dramatically post 2005 following the liberalization of FDI policy in the country. See Li et al. (2019) for details.

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

[11]A handful of studies have highlighted the market gains from education for women. Goldin (2006) argues that greater levels of human capital investment by women increased labor market returns for women relative to men post

Finally, our paper also contributes to the debate concerning investment in higher education in developing countries. International donor agencies like 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 spillovers as we demonstrate, other development spillovers for school education (Jagnani and Khanna, 2019) and "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 the welfare of women, college expansion can have gender-based distributional effects and can aid in reducing gender disparities in 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 the college construction program in section 4. Section 5 reports the empirical results and the falsification tests, and section 6 concludes.

2 Background

While India's knowledge economy is fueled by its massive college-educated workforce, college education still remains elusive to its masses. In 2016-17, with a higher education GER of only 25.2 percent, India trailed Asian countries like China which had a GER of 43.39 percent. By way of comparison, the GER for the US was as high as 85.8 percent.[12] In addition to lagging behind many other economies, India faces another challenge of uneven access across states. In the past six decades, there has been a visible and substantial growth in colleges in India but it has not been spatially uniform. While some states have seen massive increases, others have trailed behind.

1970. Similarly, Chiappori et al. (2017) find evidence for marital premium for college educated women.

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

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 the state of Bihar to 59 in Telangana compared to an all India average of 28 (according to the All India Survey on Higher Education, 2015-16). In 2016-17, the GER in higher education ranged from 14.9 percent in Bihar to 46.9 percent in Tamil Nadu.

Indian policymakers opine that proximity to colleges can go a long way in addressing this disparity. The 11$^{\text{th}}$ 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 a gross enrollment ratio in higher education lower than the national average of 12.4 percent. The apex higher education regulating body, the University Grants 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.[13]

To receive financial assistance under this program, the college had to be set up on, or after, 1$^{\text{st}}$ January 2008 in any of these 374 eligible districts. While setting up model colleges, preferences were given to the following areas: (a) hilly or border regions populated with a higher official share of minority and tribal population, (b) areas with no college within a radius of 10 kilometers, 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 the central government/government-funded bodies. The state government had to provide land to 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 the 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 was 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.

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

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 the planned development of higher education at the state level. The perception was that this would be an instrument to harmonize the national program for funding state universities and colleges through a single over-arching umbrella body. RUSA's key actions and funding areas included the 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.[14]

3 Data

We use several datasets to conduct our empirical analysis. There are three main sources of data pertaining to college expansion and eligibility, educational and marriage market outcomes, and employment and wages. To these sources, we add the demographic data from the Census of India and poverty and night light (average luminosity) to control covariates in our empirical specifications. Below we describe these data in detail.

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 for 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. We construct a panel dataset of the number of new colleges constructed in each district by year between 1980 to 2017.[15]

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

[15] One caveat of this data is that if a college exited or closed during this period, we would 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 districts were 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 all the districts, we rely on the methodology used by the government to establish eligibility. We use Table C-10 from 'C–Series: Social and Cultural Tables' of the 2001 Census of India to obtain district-wise gross enrollment in higher education. This is the total population enrolled in colleges in India. We combine this with district-wise population in the 18-23 age-group. The latter is imputed using the age- and district-wise population data from the Census 2001. This data provides population information for ages 18, 19 and 20-24. We assume a uniform distribution of the population in the 20-24 age group to get an approximation for the population in the 20-23 age group and combine it with that for ages 18 and 19. [16]

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 six 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.

[16] These approximations yield GER values very close to the ones used by the MHRD.

3.3 Employment and Wages

Data on employment and wages comes from the Periodic Labor Force Survey (PLFS) conducted by National Sample Survey Organization (NSSO) during the period July 2017 to June 2018.[17] 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 sampling design, survey methodology, and data collection mechanism of the PLFS viz-a-viz the earlier rounds, making a comparison across the different rounds difficult.[18] 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 (like 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 NSSO. Night light data comes from the National Geophysical Data Center (NGDC) at the National Oceanic and Atmospheric Administration (NOAA).[19]

3.5 Summary Statistics

We report the summary statistics of the outcome variables for the full sample in Table I. The summary statistics highlight the poor socio-economic condition of women. The average years of schooling are less than seven years, with only 11 percent of the women enrolling in colleges. On

[17] 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.

[18] 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$_r$*eports/Annual*%20*Report*%2C%20*PLFS*%202017 − 18$_3$1052019.*pdf*

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

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 are 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 cohorts 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 the 2001 population census (Panel B of Appendix Table A.1).

4 Empirical Strategy

The main identification challenge for our analysis is that colleges are not randomly located across regions. Local economic or political conditions, for example, can influence college investment as well as the human capital and status of women. In order to address this endogeneity issue, we rely on the policy generated sharp variation in college expansion across 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.[20]

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

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

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. That is, in districts with very low GER. In Panel A of Appendix Figure A.1, we plot the number of new colleges constructed in a district-year as a share of the respective district's total number of colleges in 2001 on either side of the GER cutoff of 12.4 (sample period 2009-2017). In panel B, we plot the growth in the total number of colleges between 2001 and 2018 on either side of the same threshold. The plots for the treated districts are steeper than the control districts and are downward sloping, indicating that the highest growth of colleges occurred in the tails of the treated districts rather than the neighbourhood of the 12.4 threshold. 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 observables.[21] We define the variable Treatment as[22]

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

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 both trends with the treatment indicator. γ_1 and γ_2 are the respective estimates. *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

[21] 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 the causal impact of rural banks on poverty.

[22] 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. The results are not sensitive to including or excluding these districts.

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. For college expansion, we use a panel spanning 1980-2017. We also conduct sensitivity checks to verify that the results are not sensitive to the period chosen.[23]

Figure 3 plots the estimates of an event study analysis (blue circles in solid line).[24] Recall that treated districts were 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 estimates from the trend break coefficients.[25]

To bolster our analysis, we use structural trend break in time series to test: (i) whether the year-specific coefcients γ_t exhibit a structural break in 2008, and (ii) when do these coefficients exhibit the structural break. To this end, we perform a supremum Wald test.[26] 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 the 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 II. We can see that treated districts get 0.04 fewer colleges on average each year before the policy change. But annual college construction reversed the 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.

[23] The periods of our different samples are specified in the results section.
[24] These are year-by-year differences-in-differences estimates.
[25] These coefficients have been imputed from the trend break estimates, Column 1, Table II.
[26] 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.

To show that the trend reversal observed in Table II 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, Appendix Table A.2 reassuringly 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 average night-time luminosity. The concern is that the economic growth trajectory reverses direction for an alternative reason, which causes the trend-reversal. However, Column 2 of Table II 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 regardless of the poverty measure used. Finally, we test whether our results are sensitive to different sample periods. We report the results 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 was a sharp trend reversal in college expansion after 2009. Districts with low GER had a sharper increase in college expansion due to the college grants. We now turn to evaluate how this affected educational and other development outcomes for women. To this end, we compare the 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. Since our survey data is from 2016, we are able to use seven cohorts that are younger than the college eligible age of 18. The youngest cohort is 11 years old in 2009 and 18 years old in

2016).[27] We rely on reduced-form regressions of the following form:

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

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 the educational outcome of girls. In Panel (a) of Figure 5, we plot the estimates analogous to the two specifications in Figure 2. Blue circles in the solid line are the estimates from an event study analysis of the effect of treatment on the number of years of schooling. We see a remarkably similar trend: a negative trend in the 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 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 the case of college enrollment (Panel (b)) and college completion (Panel (c)).

Table III reports the results of the trend-break analysis estimating Equation 2 along with the standard errors. The number of years of schooling (Column 1) and college enrollment (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 0.016 fewer years of schooling 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 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). The high-

[27] 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.

est level of education is primary, or secondary (Columns 2 and 3, respectively) shows an opposite trend and its reversal.

Do we need to add magnitudes and comparisons here too?

5.2.1 Robustness to Other Potential Confounders

To allay concerns about other potential confounding variables exhibiting the same trends and thus leading to a spurious relationship between college expansion and changes in human capital, we show that our results are robust to a variety of controls. In Appendix Tables A.4 and A.5, we include the trends in nightlights (average luminosity as a proxy for GDP) in Column 2 and various measures of poverty (poverty gap, its square, and headcount ratio, respectively) in Columns 3-5. The results remain unchanged.

Political changes can also affect outcomes, which then are attributed to college access via expansion. Since there was an election for the national government in 2009 and the political representation could have changed around this period, we investigate if there is a change in the local representation affiliated with the party forming the federal government. Such affiliation can bring in more resources (Asher and Novosad, 2017) and affect local labor markets. In so far as nightlights absorb the local growth trends, our results are robust to including these (as shown in Appendix Tables A.4 and A.5).

We also directly assess whether there is a change in the local representation that can potentially affect resource allocation. In India, the local state assemblies nest within districts. In the 2009 elections, a coalition of parties formed the national government as no single party secured the majority. Hence, we compute the proportion of local state assembly elected representatives from each district that align with either the alliance forming the national government or the major party in the national government before and after the policy reform using the state assemblies political representation and the national government's political representation data from 2005 to 2017.[28] The results from a differences-in-differences specification are summarised in Appendix Table A.6. Conditional on district fixed effects and district-specific linear trends, the proportion of local elected

[28]Some states also had elections that year, and there was a switch in the largest party in the coalition at the federal level. Hence, there can be a lot of variation in the alignment with the federal government.

representatives affiliated with national government forming alliance did not change in the treated areas post-treatment (Column 1) and those affiliated with the largest party in the coalition actually declined, albeit by a small number marginally significant at the 10 percent significance level.

We also rule out migration as the principal driver of our results. Using the two waves of the India Human Development Surveys from 2005 and 2012, we investigate whether the probability of having migrated to the current residence in the last five years changes in the treated districts after the policy reform. We have a panel of households and hence include household fixed effects in our specification reported in Column 1 of Appendix Table A.7.[29] The probability of migration in the last five years is negligible and statistically insignificant. Column 2, using district fixed effects, also shows the same results. Hence, there is no evidence of in- or out-migration in the treated districts around the time of the policy that could be driving our results.

5.2.2 Heterogeneity

We compare the effects of the college construction grants program across gender. The results are reported in Appendix Table A.8.[30] 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 males and females 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.[31]

We then explore the impact of the grants program on the educational outcomes of girls across rural and urban locations. Table IV suggests that the gains from the program with respect to the number of years of schooling are twice as large in rural areas than the urban areas – an annual increase

[29] Note that the question in the survey pertains to the household migration. Unlike the US, young unmarried girls of college-going age do not move out of their residence without their family. Migration for education is negligible among women, but they do move to their husband's place of residence after getting married.

[30] For this analysis, we use the 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.

[31] The results remain qualitatively similar when we use PLFS data, which includes more cohorts in the post-period (Table A.9).

of 0.13 in rural areas compared to an annual increase of 0.06 in 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.[32]

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 V. 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.[33] On the contrary, college enrolment across wealth groups does vary: the college enrollment of girls monotonically increased with the wealth level of the households.[34]

5.3 Spillovers

In South Asia, women are precluded from crossing village and district boundaries to access education or skill development opportunities (Cheema et al., 2018; Jacoby and Mansuri, 2011). Enduring social norms are the main drivers of this *boundary effect*. With rapid globalization, however, economic incentives have favored more compelling education opportunities for girls (Munshi and Rosenzweig, 2006) challenging traditional norms. In light of this backdrop, it is unclear whether there would be spillovers for women across geographic boundaries. On the one hand, crossing districts might be especially challenging. So the women in geographic neighbors of districts receiving colleges may not benefit from this expansion in infrastructure. On the other hand, given rapidly rising returns to education in India, access to nearby colleges may also benefit the women in close proximity who live in the neighboring untreated districts.

We explore this in three steps. First, we estimate the same specification as Equation 2 for girls but now restricting the sample to only the treated districts and their immediate neighbors. The estimates reported in Table VI are marginally smaller in magnitude but statistically similar to those reported in Table III. Second, we restrict the sample only to the controls and compare the neighboring control districts to the non-neighboring control districts before and after the intervention.

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

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

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

Results reported in Table VII are similar to those reported in Table III. Finally, we estimate this specification for a sample comparing treated districts to non-neighboring controls. Now the estimates reported in Table VIII are twice as large for the number of years of schooling relative to those reported in Table III. For all other outcomes, the effect size is larger. These are also statistically different. This implicates a role for spill-overs indicating that relative to non-neighboring controls, neighbors experience a treatment effect as large in magnitude. In other words, in the absence of spillover effects, the treatment effect would have been twice as large. Rapidly globalizing Indian economy just preceding the college construction (Li et al., 2019) could have triggered an erosion of the boundary effect in the Indian case.

5.4 Returns to Education: Labor and Marriage Market Outcomes

We use the PLFS 2017-18 to examine the labor market outcomes. In Table IX, 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 after the policy change (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 Table X, we document the trends for males. Here we observe a positive trend in employment, which reverses to a small negative trend post the 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 substitution for men from self-employment to salaried work post the policy change.

In Table XI, we summarize the analysis for earnings for both females and males. Consistent with the increase in salaried employment of females reported in Table VI, 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 after the 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 Table XII, we first examine education in terms of the 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) after the policy change, 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, the husband's being employed was trending negatively (Column 3). However, there is a trend reversal in the husband's employment with more husbands participating in non-agricultural work (Columns 3 and 5). Hence, it seems there are benefits in the marriage market as well.

5.5 Other Development Outcomes

We are interested in examining whether this improvement in educational outcomes 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 lives that change for the better due to access to colleges.

5.5.1 Marriage Age and Child Marriage

A vast literature focuses on benefits of delayed age of marriage of girls (Field and Ambrus, 2008; Jensen and Thornton, 2003). In Appendix Figure A.2, we plot the distributions of the age of marriage for marriages convened in 2005 and 2012 using NFHS-4. We observe that the age of marriage shifted to the right in 2012. We ascertain whether college expansion contributed to this change in marriage age. In Table XIII, we document the results of our trend-break specification for child marriage. The 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. The legal age of marriage in India is 18. A 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.

5.5.2 Fertility

We then examine the effect of college expansion on actual and desired fertility. Earlier literature suggests that 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 XIV. We can see that treated districts experienced a faster decline in the number of children ever born (Column 1) and 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). The availability of contraceptives in the 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 but do not find any effect on the knowledge of modern contraceptives and use (Columns 5 and 6).

5.5.3 Asset Ownership

In Table XV, 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.

5.5.4 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 not allowed to go out of the house unaccompanied or without permission from the husband or his family. We examine if college access affects women's autonomy in making such decisions. Results are reported in Table XVI. We do see a trend reversal in decision-making regarding health care, large purchases, and visiting relatives. This indicates an increased role of women within the household regarding important decisions. College access, therefore, results in the empowerment of women in certain socio-economic spheres.

5.5.5 Domestic Violence: Incidence and Reporting

Intimate partner violence is an age old phenomenon in India. Domestic violence affects 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 NFHS-4. These are emotional, less severe physical, more severe physical, and sexual violence.[35] In Table XVII, 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 the prior literature, we also find evidence of returns in the marriage market. College access in such settings can have other welfare-enhancing consequences. We shed light on some of these outcomes and find that access to local colleges increases women's autonomy, reduces fertility, and increases the age at marriage.

[35]Emotional violence includes humiliation in front of others or threat of harm 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 weapons by husband/partner. Finally, sexual violence includes being physically forced into unwanted sex or sexual acts by husband/partner.

From a 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 and that there are spatial spillovers. We also document positive developmental spillovers 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 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 a continual increase in girls' schooling over time (Afridi et al., 2018; Fletcher et al., 2017). This puzzling pattern has been subjected to many speculative explanations. Amid rising concerns for women's safety, lack of local educational opportunities might be affecting women's human capital, thus affecting the returns to the 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 (2017) shows that safety concerns affect the quality decisions of females in India. Our findings imply that improving access to education alone may not be effective at increasing employment opportunities for girls. The 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 I: 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 age	514,112	0.15	0.36	0	1
Marriage before 18 years of age	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 number 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 methods	699,686	0.98	0.15	0	1
Uses modern contraceptive methods	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 II: Effect of the Grants Policy on Number of Colleges Constructed

	Number of Colleges Constructed				
VARIABLES	(1)	(2)	(3)	(4)	(5)
Grant Policy x (T-1980) trend	-0.0403***	-0.0536*	-0.0354**	-0.0374***	-0.0320**
	(0.0141)	(0.0281)	(0.0140)	(0.0142)	(0.0137)
Grant Policy x (T-2009) trend	0.183***	0.187***	0.179***	0.185***	0.168***
	(0.0485)	(0.0661)	(0.0483)	(0.0489)	(0.0474)
Grant Policy 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 lights	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. Grant Policy 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 the 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 III: 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
Grant Policy x Overall trend	-0.0158***	0.00183***	0.00174***	-0.00222***	-0.00205***
	(0.00543)	(0.000359)	(0.000532)	(0.000361)	(0.000336)
Grant Policy x Post Policy trend	0.134***	-0.00441***	-0.00586***	0.0142***	0.0144***
	(0.0170)	(0.000841)	(0.00171)	(0.00174)	(0.00170)
Grant Policy 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 the NFHS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 IV: 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)
Grant Policy x Overall trend	0.00721	-0.0278***	-0.000365	-0.00217***
	(0.00634)	(0.00630)	(0.000565)	(0.000345)
Grant Policy x Post Policy trend	0.0645***	0.126***	0.00895***	0.0118***
	(0.0212)	(0.0197)	(0.00281)	(0.00181)
Grant Policy 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 the NHFS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 V: 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					
Grant Policy x Overall trend	-0.0284***	-0.0130*	-0.00123	-0.00274	0.0146*
	(0.00667)	(0.00679)	(0.00787)	(0.00788)	(0.00763)
Grant Policy x Post Policy trend	0.0503**	0.0630**	0.0732***	0.0637***	0.0591***
	(0.0237)	(0.0251)	(0.0254)	(0.0241)	(0.0218)
Grant Policy 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 B: Dependent variable is College enrollment					
Grant Policy x Overall trend	-0.000310**	-0.000573**	-0.000585	-0.000875	-0.000709
	(0.000132)	(0.000237)	(0.000406)	(0.000569)	(0.000625)
Grant Policy x Post Policy trend	0.00217*	0.00463***	0.00545***	0.00912***	0.0111***
	(0.00122)	(0.00166)	(0.00211)	(0.00294)	(0.00325)
Grant Policy 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 the NHFS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 VI: Effect of the Grants Policy on Educational Attainment of Girls (Controls: Neighboring Districts Only)

	(1)	(2)	(3)	(4)	(1)
VARIABLES	No. of years of schooling	Highest level of education is primary	Highest level of education is secondary	Enrolls into college	Finishes college
Grant Policy x Overall trend	-0.0183***	0.00145***	0.00150***	-0.00218***	-0.00202***
	(0.00549)	(0.000374)	(0.000524)	(0.000341)	(0.000316)
Grant Policy x Post Policy trend	0.116***	-0.00347***	-0.00668***	0.0128***	0.0132***
	(0.0181)	(0.000793)	(0.00142)	(0.00162)	(0.00174)
Grant Policy x Post	-0.0134	0.00236	0.0534***	-0.0380***	-0.0212***
	(0.0709)	(0.00406)	(0.00899)	(0.00770)	(0.00676)
Observations	655,743	655,743	655,743	655,743	609,607
R-squared	0.460	0.036	0.225	0.203	0.181
F stat	40.68	8.203	18.40	62.26	56.59
Sig. level	3.60e-10	0.00433	2.09e-05	0	0

Notes: Data is from the NFHS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. We exclude control districts that are not neighbor to any treatment districts. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 VII: Educational Attainment of Girls in Neighboring and Non-Neighboring Control Districts

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	(1) Finishes college
Neighbor controls x Overall trend	0.0189	0.00212***	0.00109	0.000184	0.000101
	(0.0142)	(0.000605)	(0.00176)	(0.00129)	(0.00124)
Neighbor controls x Post Policy trend	0.133***	-0.00294***	0.000425	0.00774*	0.00728
	(0.0351)	(0.00111)	(0.00431)	(0.00424)	(0.00491)
Neighbor controls x Post	-0.217	-0.00774	0.0370**	-0.0299**	-0.0188
	(0.155)	(0.00619)	(0.0171)	(0.0149)	(0.0132)
Observations	303,909	303,909	303,909	303,909	283,628
R-squared	0.434	0.039	0.182	0.212	0.188
F stat	23.94	0.779	0.239	5.910	3.590
Sig. level	1.72e-06	0.378	0.625	0.0157	0.0592

Notes: Data is from the NFHS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. We exclude control districts that are not neighbor to any treatment districts. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 VIII: Effect of the Grants Policy on Educational Attainment of Girls (Controls: Non-Neighboring Districts Only)

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	(1) Finishes college
Grant Policy x Overall trend	0.000497	0.00358***	0.00266	-0.00204	-0.00195
	(0.0139)	(0.000603)	(0.00172)	(0.00127)	(0.00122)
Grant Policy x Post Policy trend	0.252***	-0.00647***	-0.00604	0.0206***	0.0205***
	(0.0337)	(0.00111)	(0.00421)	(0.00409)	(0.00478)
Grant Policy x Post	-0.228	-0.00551	0.0894***	-0.0672***	-0.0395***
	(0.151)	(0.00609)	(0.0166)	(0.0144)	(0.0127)
Observations	438,890	438,890	438,890	438,890	407,543
R-squared	0.464	0.035	0.241	0.191	0.169
F stat	72.42	9.320	1.234	35.36	24.33
Sig. level	0	0.00241	0.267	5.82e-09	1.18e-06

Notes: Data is from the NFHS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. We exclude control districts that are not neighbor to any treatment districts. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 IX: Effect of the Grants Policy on Female Employment

VARIABLES	(1) Any Employment	(2) Self-Employment	(3) Wage Employment	(4) Salaried Employment
Grant Policy x Overall trend	-0.000371	2.56e-05	-0.000327	0.000524
	(0.000586)	(0.000322)	(0.000283)	(0.000347)
Grant Policy x Post Policy trend	0.00304*	-0.000253	-0.000142	0.00287***
	(0.00161)	(0.000631)	(0.000587)	(0.00110)
Grant Policy 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 the PLFS survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 X: Effect of the Grants Policy on Male Employment

VARIABLES	(1) Any employment	(2) Self-employment	(3) Wage employment	(4) Salaried employment
Grant Policy x Overall trend	0.00117***	-0.000988	0.000173	0.000943
	(0.000404)	(0.000693)	(0.000452)	(0.000764)
Grant Policy x Post Policy trend	-0.00295*	-0.00420***	-0.000603	0.00551**
	(0.00165)	(0.00133)	(0.00142)	(0.00239)
Grant Policy 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 the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 XI: 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)
Grant Policy x Overall trend	-0.00184	0.00476	0.00433	-0.00117
	(0.00190)	(0.00402)	(0.00317)	(0.00730)
Grant Policy x Post Policy trend	0.000343	0.0209*	-0.0174*	0.0193
	(0.00394)	(0.0114)	(0.00974)	(0.0218)
Grant Policy 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 the PLFS survey. The unit of analysis is at the individual level. The dependent variables are in Logarithmic form. We exclude a few observations (top 1 percent of wage/salary distribution) as outliers. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 XII: 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
Panel A: All women					
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
Panel B: Women with at least a secondary education					
Treatment x Overall trend	0.0152**	0.00189**	-0.000657**	0.000403	-0.00106
	(0.00717)	(0.000770)	(0.000330)	(0.000672)	(0.000703)
Treatment x Post Policy trend	-0.0289	-0.00340	0.00281	-0.00654*	0.00935**
	(0.0379)	(0.00384)	(0.00287)	(0.00385)	(0.00419)
Treatment x Post	0.0378	0.00139	-0.00225	0.0240	-0.0262
	(0.165)	(0.0172)	(0.0101)	(0.0166)	(0.0180)
Observations	47,358	47,358	47,142	47,142	47,142
R-squared	0.233	0.161	0.045	0.223	0.200
F stat	0.138	0.166	0.564	2.584	4.024
Sig. level	0.711	0.684	0.453	0.108	0.0453

Notes: Data is from the NFHS woman's survey. Information on the husband's education and employment was collected for a subset of all surveyed women. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 XIII: Effect of the Grants Policy on Child Marriage

VARIABLES	(1) Age at marriage	(2) Married before 18 years of age
Grant Policy x Overall trend	-0.0136**	0.00179***
	(0.00537)	(0.000465)
Grant Policy x Post Policy trend	0.0525***	-0.00753***
	(0.0174)	(0.00260)
Grant Policy 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 the NHFS woman's survey. The sample includes only ever-married women. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 XIV: 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
Grant Policy 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)
Grant Policy 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)
Grant Policy 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 the NHFS woman's survey. Column (1), (2), and (6) are based on only ever-married women, whereas column (3)-column (5) is based on the whole sample. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 XV: Effect of the Grants Policy on Asset Ownership by Females

VARIABLES	(1) Owns house	(2) Owns land	(3) Has bank account	(4) Has mobile phone
Grant Policy x Overall trend	0.000512	-0.000497	3.88e-05	-0.00100*
	(0.000506)	(0.000452)	(0.000559)	(0.000524)
Grant Policy x Post Policy trend	0.00218	-0.000573	0.00154	0.00613**
	(0.00205)	(0.00196)	(0.00264)	(0.00259)
Grant Policy 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 the NHFS woman's survey. Information on asset ownership was collected for a subset of all surveyed women. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 XVI: 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)
Grant Policy x Overall trend	-0.000967	-0.00179	-0.000648	-0.00149	0.00107
	(0.00124)	(0.00128)	(0.00122)	(0.00137)	(0.00223)
Grant Policy x Post Policy trend	0.0265**	0.0267**	0.0293**	0.00989	-0.00612
	(0.0124)	(0.0131)	(0.0121)	(0.0137)	(0.0289)
Grant Policy 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 the 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 the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 XVII: 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
Grant Policy x Overall trend	0.000137	0.00101*	7.43e-05	-0.000162
	(0.000430)	(0.000539)	(0.000313)	(0.000302)
Grant Policy x Post Policy trend	-0.00310	-0.00909**	-0.00161	0.00242
	(0.00342)	(0.00454)	(0.00252)	(0.00263)
Grant Policy 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 the NFHS woman's survey. Information on domestic violence was collected for a subset of all surveyed women. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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: Covariate Balance between Treatment and Control Districts

VARIABLES	Control mean	Difference b/w 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 the 2018 AISHE. The unit of analysis is at district-year. Grant Policy 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 the 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 Periods

	Number of College Constructed		
Variables	(1)	(2)	(3)
Grant Policy x (T-1980) trend	-0.0406***	-0.0462*	-0.0552
	(0.0141)	(0.0244)	(0.0560)
Grant Policy x (T-2009) trend	0.185***	0.190***	0.199**
	(0.0483)	(0.0562)	(0.0836)
Grant Policy 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. Grant Policy 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 the 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)
Grant Policy x Overall trend	-0.0158***	-0.0141**	-0.00972*	-0.0114**	-0.0120**
	(0.00543)	(0.00563)	(0.00532)	(0.00536)	(0.00540)
Grant Policy x Post Policy trend	0.134***	0.111***	0.118***	0.125***	0.128***
	(0.0170)	(0.0172)	(0.0161)	(0.0162)	(0.0164)
Grant Policy 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 the NFHS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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)
Grant Policy x Overall trend	-0.00222***	-0.00165***	-0.00178***	-0.00192***	-0.00199***
	(0.000361)	(0.000349)	(0.000325)	(0.000337)	(0.000345)
Grant Policy x Post Policy trend	0.0142***	0.0114***	0.0129***	0.0135***	0.0137***
	(0.00174)	(0.00175)	(0.00171)	(0.00172)	(0.00174)
Grant Policy 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 the NFHS woman's survey. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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: Changes in Political Affiliation by Treatment Status

VARIABLES	(1) Affiliation with ruling alliance	(2) Affiliation with ruling party
Grant Policy x Post	-0.00743	-0.0413*
	(0.0308)	(0.0249)
Observations	6,686	6,686
R-squared	0.725	0.758

Notes: Data is from the Election Commission of India. The sample period is 2005-2017. The unit of analysis is a district by year. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for years 2009 and after and 0 otherwise. Columns 1 and 2 presents estimated coefficients from Difference-in-difference estimates of the intervention (college construction) on the fraction of total constituencies in a district that is affiliated with the ruling alliance and the ruling party, respectively. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.7: Effect of the Grants Policy on Migration

VARIABLES	(1) Migrated in last 5 years	(2) Migrated in last 5 years
Grant Policy x Post	0.00710	0.00709
	(0.00680)	(0.00481)
Post	-0.0257***	-0.0257***
	(0.00606)	(0.00429)
Constant	0.0318***	0.0318***
	(0.00156)	(0.00110)
Control	HH FEs	District FEs
Observations	77,092	77,092
R-squared	0.532	0.045

Notes: Data is from IHDS 2005 and 2012. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 if the year of the survey is 2012, and 0 if the year of the survey is 2005. Each column presents estimated coefficients from Difference-in-difference estimates of the intervention (college construction) on the probability of migration. *** indicates significance at 1, ** at 5, and * at 10 percent level.

Table A.8: Effect of the Grants Policy on Educational Attainment by Gender

	No. of years of schooling			Enrolls into college		
	All	Male	Female	All	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Grant Policy 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)
Grant Policy 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)
Grant Policy 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 the NFHS Household survey. This analysis covers all males and females aged between 18 to 51. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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 Educational Attainment by Gender Using PLFS

	Female		**Male**	
	Enrolls into collge	College graduate	Enrolls into collge	College graduate
VARIABLES	(1)	(2)	(3)	(4)
Grant Policy x Overall trend	-0.000735	-0.000506	0.000527	0.000394
	(0.000541)	(0.000466)	(0.000575)	(0.000539)
Grant Policy x Post Policy trend	0.00664***	0.00966***	0.00334*	0.00545***
	(0.00204)	(0.00189)	(0.00173)	(0.00162)
Grant Policy 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 the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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.10: Effect of the Grants Policy on Husband's Education and Employment by Religion

	Hindu		Muslim	
VARIABLES	No of years of schooling	Enrolled in college	No of years of schooling	Enrolled in college
	(1)	(2)	(3)	(4)
Grant Policy x Overall trend	0.0187***	0.000389	-0.0191	-0.00179**
	(0.00620)	(0.000462)	(0.0150)	(0.000906)
Grant Policy x Post Policy trend	-0.0126	0.00200	-0.0228	-0.00932
	(0.0374)	(0.00316)	(0.0935)	(0.00636)
Grant Policy x Post Policy trend	0.00923	-0.0130	0.689*	0.0601**
	(0.168)	(0.0141)	(0.356)	(0.0269)
Observations	68,874	68,874	12,539	12,539
R-squared	0.378	0.197	0.366	0.170
F stat	0.0274	0.593	0.207	3.128
Sig. level	0.869	0.442	0.650	0.0775

Notes: Data is from the NFHS woman's survey. Information on the husband's education and employment was collected for a subset of all surveyed women. The unit of analysis is at the individual level. Grant Policy 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 the national average in 2001. Post is an indicator variable that takes the value 1 for individuals born on or after 1991 (18 years or younger in 2009, when the intervention took place) and 0 otherwise. The overall trend goes from cohort birth year 1965 to cohort birth year 1998 (18 years or older in 2016–the year of the survey). The 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.

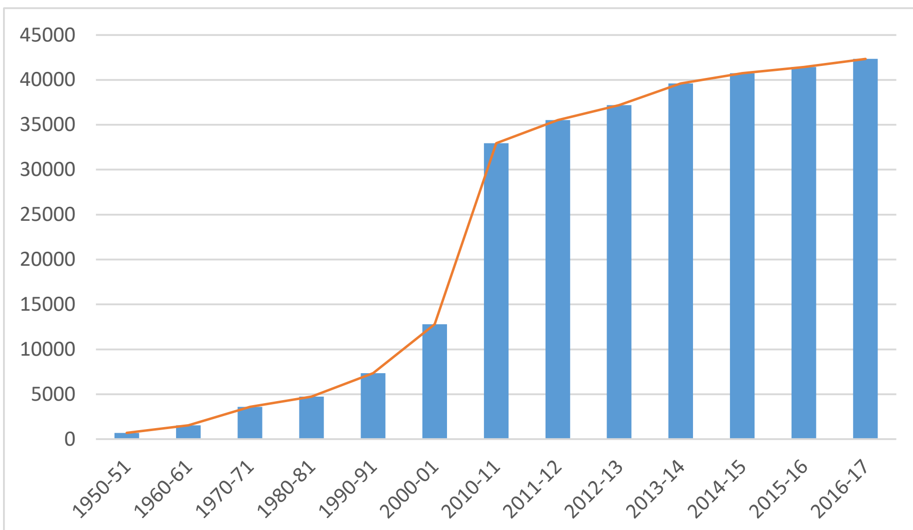


Figure 1: Total Number of Colleges in Different Years

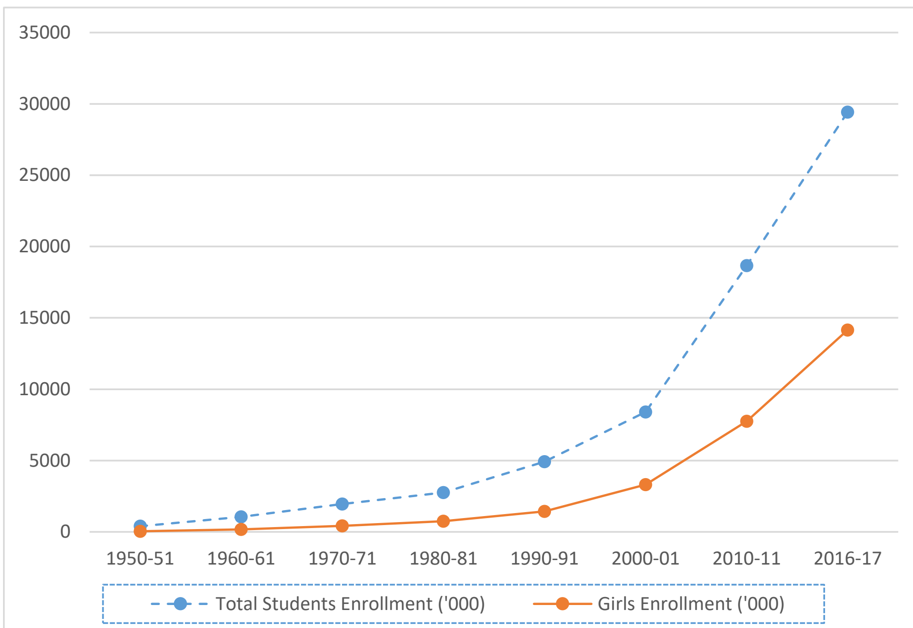


Figure 2: College Enrollment in Different Years

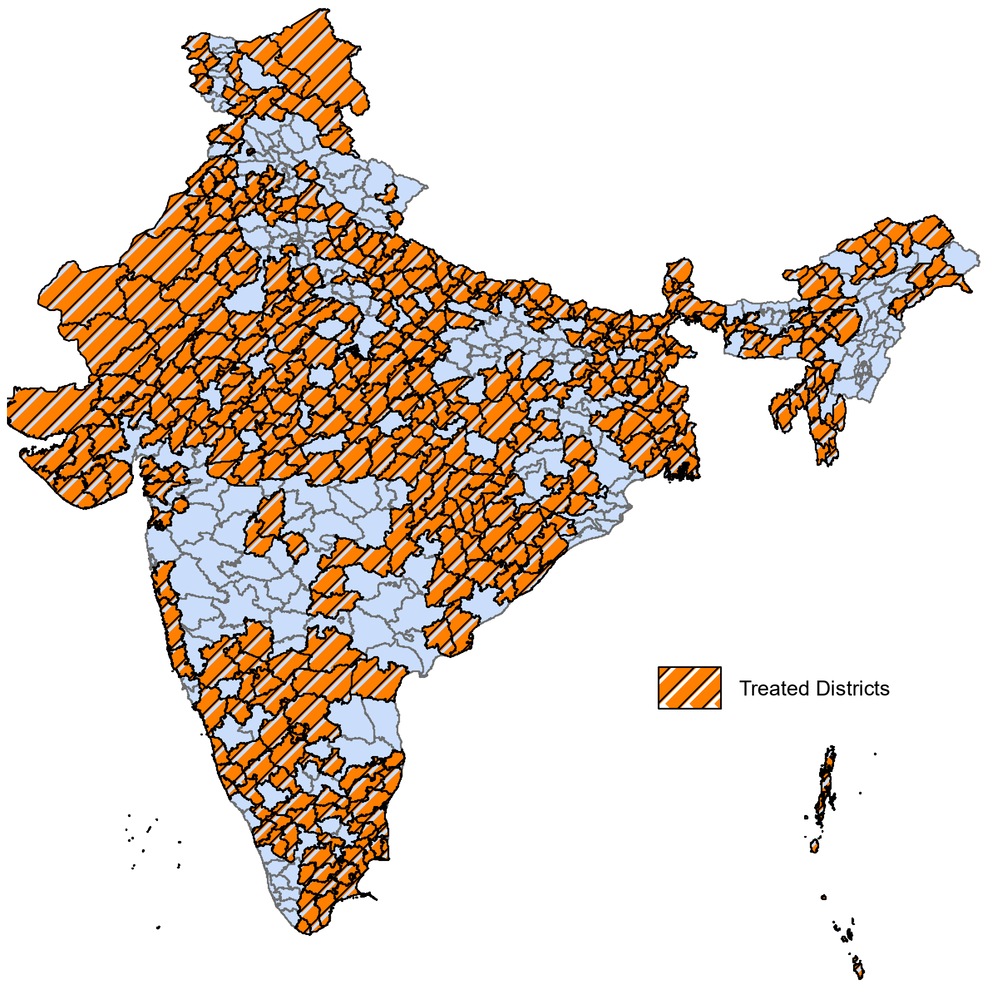


Figure 3: Treated Districts

Note: Treated districts are colored orange while the control districts are colored grey.

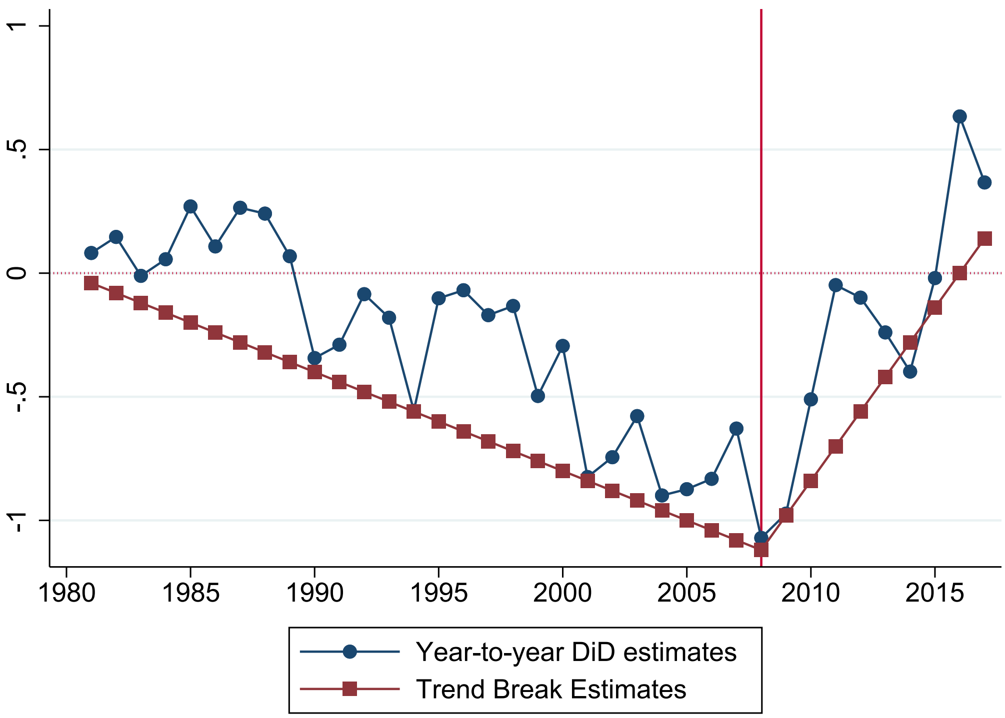
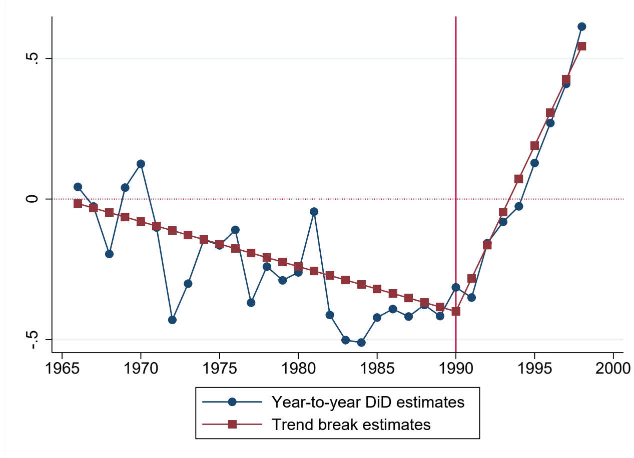
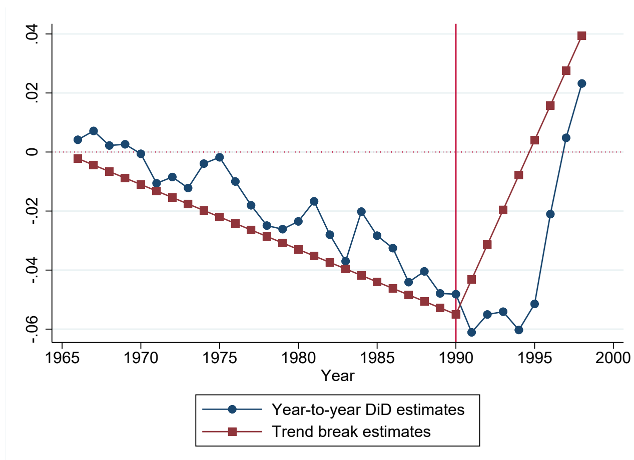


Figure 4: College Expansion by Treatment Status

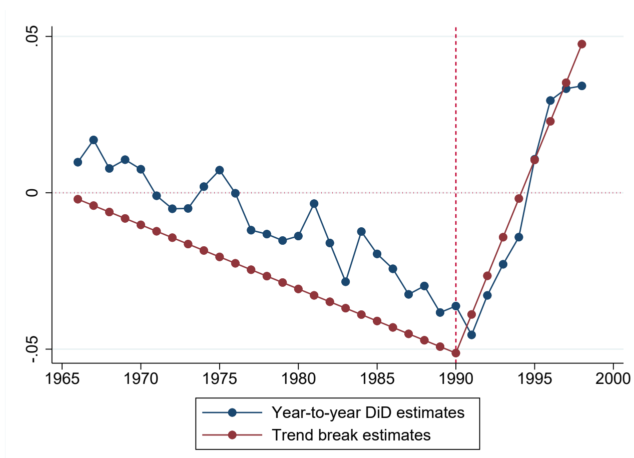
Notes: The series “Year-to-year DID estimates” graphs the annual coefficients on treatment from a Difference-in-difference regression of the form described in equation (1). The series “Trend break estimates” graphs the annual coefficient implied by the trend break model, column (1), Table II . In both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



(a) Number of years of schooling



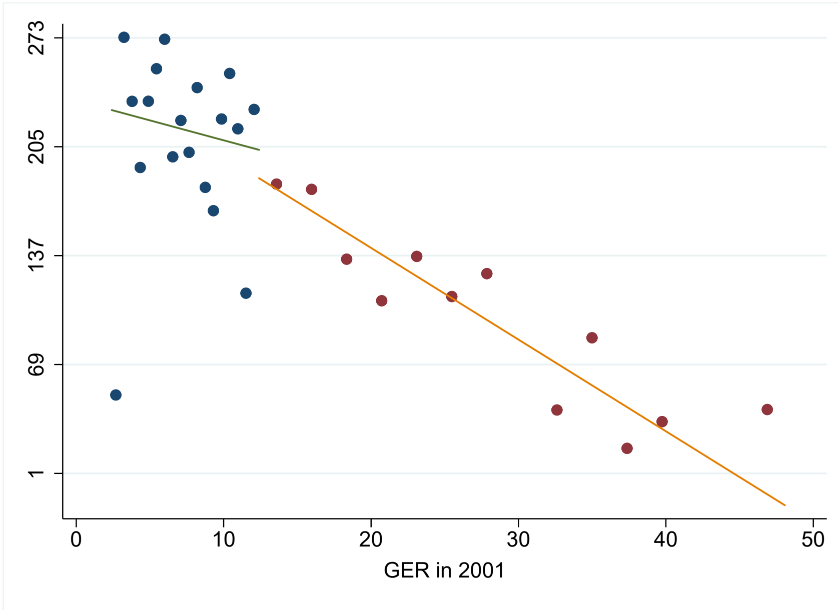
(b) College enrollment



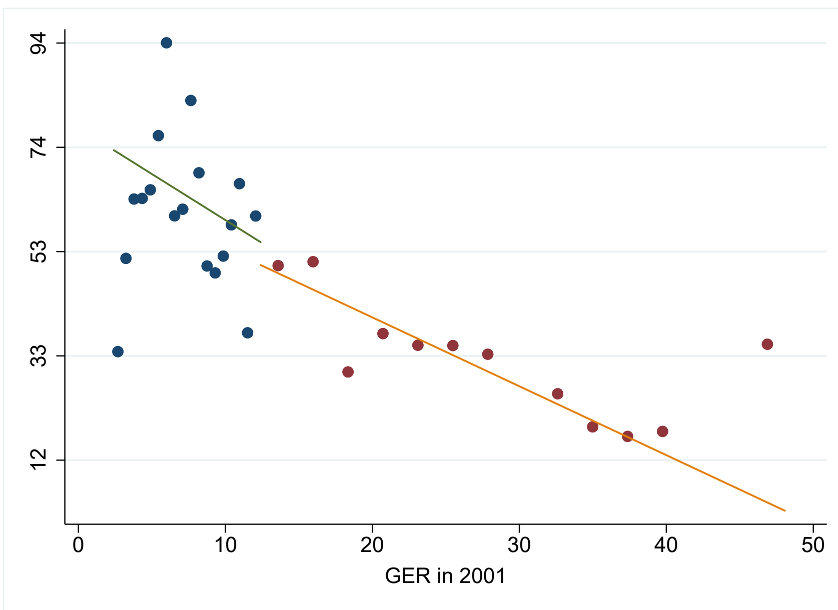
(c) College completion

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 III. The dependent variable is the 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

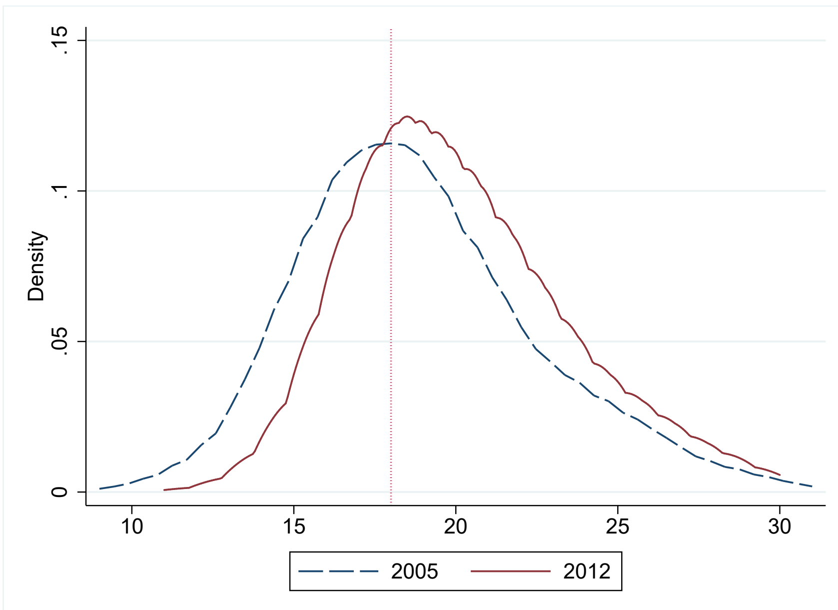


Figure A.2: Kernel Density of Age at Marriage