China's College Expansion and Inequality: A Heterogeneity Test

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Abstract

We investigate the causal effects of China's 1999 College Expansion ("the Expansion") and the college attainment it induced, using sharp Regression Discontinuity Designs. We find heterogeneous treatment effects across different regions and *hukou* status while not across gender and parental backgrounds. The *fake-cutoff* placebo test suggests the college attainment is unique and solely attributable to the Expansion for most subgroups, but mild time trends observed among individuals with agricultural *hukou* and those with CCP-affiliated parents suggest caution when interpreting subgroup heterogeneity for institutional characteristics. Whether these findings can be extended to predict the consequences of aggressive expansion at the graduate and doctoral levels in recent years remains uncertain and warrants further research.

1 Introduction

Most countries have experienced the expansion of higher education[1], however, whether it amplifies or weakens inequality remains debated (Bennett et al., 2024). This study investigates that question through the first wave of China's College Expansion in 1999 (hereafter "the Expansion"), which increased college enrollment capacity by around 44% (Li et al., 2014; Li et al., 2017; Dai et al., 2022), dramatically improving access to college for high school graduates. Based on data from the China Statistical Yearbook 2023 (Figure 1), both the average college admission rate and total enrollment increased sharply. Most existing research focuses on the Expansion's impact on higher education attainment and income. However, rigorous econometric evidence on how it causally influences inequality – particularly using nationally representative data – remains scarce, with little attention to subgroup heterogeneity.

For instance, Yang and Gao (2018) apply the Fortin–Lemieux–Firpo (FLF) decomposition method to distinguish between structural effects and price effects stemming from differences in the returns to individual characteristics, such as gender, education, and regional residence. Nonetheless, causal interpretation of these effects relies on strong assumptions, such as the absence of selection bias, thereby raising concerns regarding potential endogeneity. Similarly, Wu et al. (2020) implement a logit model and provide descriptive fact. Despite the research above using nationally representative dataset like Chinese Household Income Project (CHIP) or Chinese General Social Survey (CGSS), the causality and the direction of it are still not identified, in the context of study.

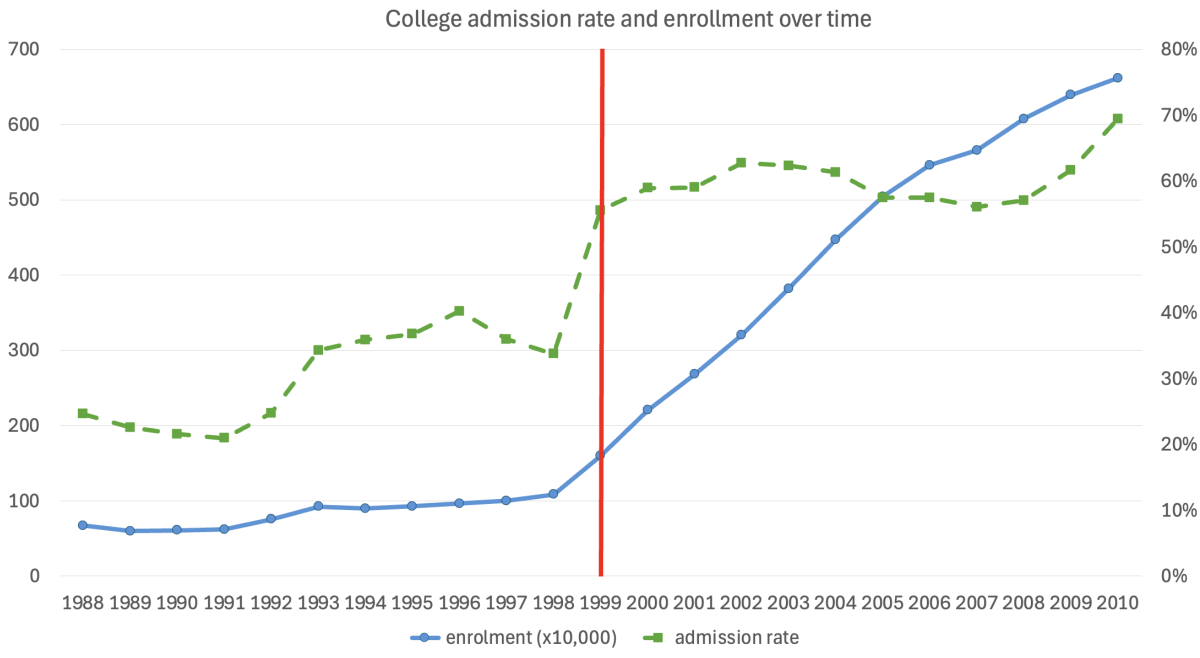

Figure 1: National average of college admission rate and total number of college enrollment.

Another group of studies, though employing rigorous econometric techniques, suffer from data limitations due to the lack of nationally representative samples. For instance, Dai et al. (2022) examine the policy effect of the Expansion using the China Urban

[1] Japan, South Korea, The US, India, etc.

Labour-force Survey (CULS), applying a fuzzy RDD to estimate the potentially heterogeneous impacts of increased higher education attainment – provided by the Expansion – across gender and *hukou* status[2]. However, because the CULS covers only the urban labor force, the sample consists solely of urban locals and rural migrants, thereby potentially generating selection bias and limiting the external validity of the results.

This study aims to fill the current gap. We use two nationally representative datasets – the 2010 China Family Panel Studies (CFPS 2010) and six waves of CGSS[3]. Methodologically, we implement the Regression Discontinuity Designs (RDD) where month of birth serves as a natural running variable. We take advantage of the strict age cutoff of primary school entry in China, allowing us rely on less restrictive assumptions for identification and have more power than the DID or the Fuzzy RDD (IV) approaches that most of the other study construct. Since the Expansion treats all regions simultaneously, the treatment effect for the whole country, on both years of education and college attainment probability, would be too large to ignore. In all specifications, we include dataset, year, and county fixed effects to account for dataset-specific variations, time-varying factors, and time-invariant unobservables not captured by other covariates. Additionally, we include month of birth fixed effect to control for the cohort heterogeneity, childbirth seasonality (Yang, 2021). The control for parental background (education level and political status) and individual characteristics are also included.

We find that the mean college attainment rate increased by significantly around the cutoff point of the Expansion, and no significant effects at nearby *fake cutoffs*, indicating that the result is unique to those exposed to the policy. To assess whether these gains differed across groups, we conduct a heterogeneity analysis by region, *hukou* status[4], gender, and parental background. The largest improvements occurred among groups with already-high attainment, such as residents of economically advanced eastern and coastal provinces, while disadvantaged groups – including those from inland areas, with agricultural *hukou*, or from less-educated families – saw smaller or even negative effects. Overall, the pattern suggests that the Expansion widened rather than narrowed educational inequality.

Specifically, those in the East and coastal areas already enjoyed higher college attainment rates than their Northern and inland counterparts before the Expansion – in the relatively educated Eastern and coastal regions, the Expansion increased the college degree attainment rate. For those who reside in the inland regions, they are less likely to get the college degree after the Expansion. The results are qualitatively and quantitatively similar regardless of how we define region – whether broadly, as the individual's location at the time of data collection, or more strictly, by additionally requiring that the individual has never migrated out of their province of birth. Additionally, for those who hold the agricultural *hukou*, the college degree attainment rate reduces significantly. However, go contrary with the existing literature, we do not find the treatment effect to be asymmetric in terms of gender (e.g., Zhang & Zhu, 2024).

We contribute to the empirical literature on the Expansion in at least two ways. Firstly, we are the first study on the Expansion's impact on inequality in China using the

[2] *Hukou* is a household registration system that restricts internal migration for work or residence, thereby reflecting an individual's place of residence.

[3] Due to the restriction of data provider, we are not able to link the same individual across the year in the CFPS, which makes it impossible for us to cluster the standard error at individual level.

[4] *Hukou* is China's household registration system that links access to public services to one's registered location.

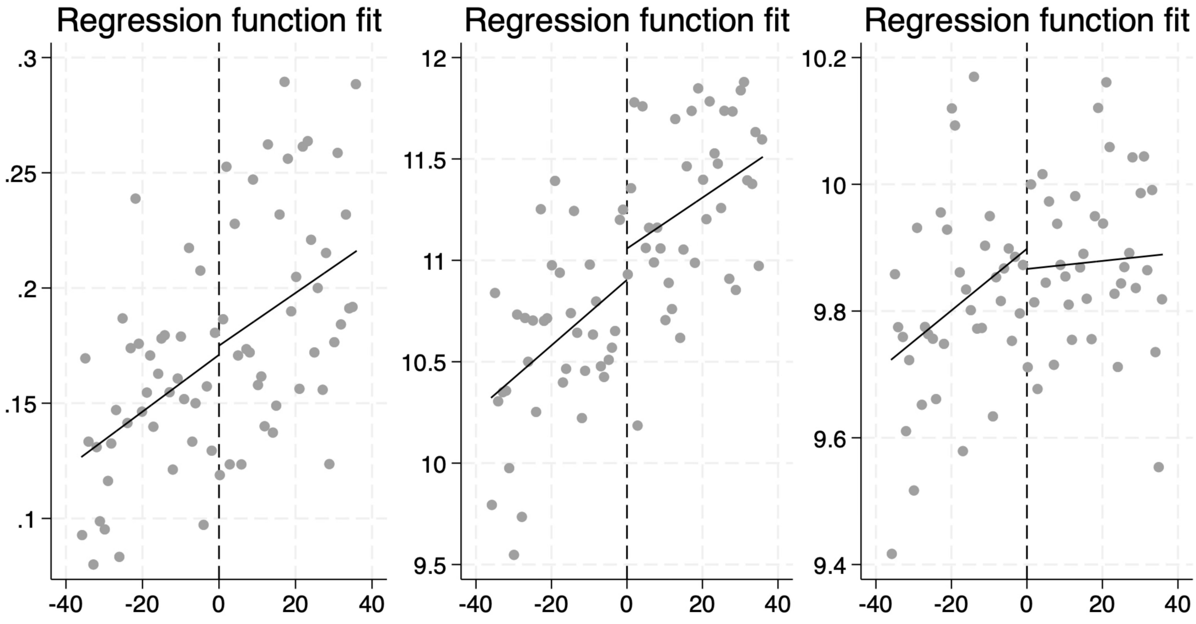

Figure 2: RDD Plot of college degree attainment, years of education, and the natural log of income.

rigorous RDD approach and nationally representative datasets. Secondly, we contribute novel evidence to the debate on whether the Expansion was economically justifiable on the inequality aspect, and whether the authority should follow the same path considering that the college expansion has now been more aggressive to the graduate and even doctoral level. The remainder of the paper is structured as follows. Section 2 provides background information on the policy context. Section 3 introduces the two nationally representative datasets and explains the construction of key variables. Section 4 outlines the research design, including identification assumptions and potential threats to validity. Section 5 presents the main empirical results. Finally, Section 6 concludes and discusses policy implications.

2 Background

With the end of the Cultural Revolution in 1977, the National College Entrance Exam (*gaokao*) was officially resumed after being paused for 10 years. It is highly selective and remains the most important measure for universities to select students with a strict bar. Furthermore, the growth of college enrollment was limited by the Ministry of Education (MOE) by setting provincial, university, and subject quotas annually (OECD, 2016). In January 1999, the MOE originally announced an admission plan of 1.3 million for three- and four-year college programs, a 20% increase over 1998. The following June, however, it revised the admission plan to 1.56 million, an unprecedented increase of 44% over the previous year. According to Che & Zhang (2018), college enrollment had only increased by an average of 4.7% annually between 1995 and 1998.

Therefore, such unprecedented expansion largely benefited those who born around 1981 and took the *gaokao* in 1999 and entered college that year. This is because in the Chinese education system, school entry is governed by a strict cutoff date of September

1st, whereby students must turn six years old before this date to enroll in primary school that year. As a result, this cutoff translates into predictable school entry and progression patterns, providing a quasi-natural experiment. For example, a child born on August 31, 1981 would have taken the *gaokao* in 1998 and missed the Expansion, while a child born just one day later, on September 1, 1981, would have taken the *gaokao* in 1999 and been fully exposed. For brevity, the full detail could be found in the Appendix 8.1.

3 Data

This study draws on two nationally representative Chinese datasets to examine the effects of the Expansion. First, we use the 2010 China Family Panel Studies (CFPS 2010), a large-scale, nationally representative panel survey conducted by the Peking University. In CFPS 2010, around 15,000 households are randomly chosen by multistage probability sampling, and all the individuals residing in the household are interviewed independently. In total, CFPS 2010 provides 33,600 completed adult observations from 25 provinces of China, representing about 95% of the population in mainland China. There are only 6 provinces excluded from the sample – Hainan, Inner Mongolia, Ningxia, Qinghai, Tibet, and Xinjiang. Second, we incorporate data from six waves of the Chinese General Social Survey (CGSS) conducted between 2010 and 2021[5], a repeated cross-sectional survey administered by Renmin University and the Hong Kong University of Science and Technology. The CGSS 2010 covers both urban and rural areas across all 31 provincial-level regions in mainland China, thereby enhancing the geographic coverage beyond that of the CFPS, as well as capturing similar individual-level variables.

The final merged dataset includes more than 85,000 observations with detailed information on individual income, gender, county of residence, ethnicity, years of education, highest degree obtained, parental background (education levels and political affiliation), height, *hukou* status (urban vs. rural household registration), and month and year of birth. These variables enable the construction of precise running variables, covariates, and subgroup identifiers for causal and heterogeneity analysis. The combined dataset provides a broad and demographically diverse sample well-suited for studying heterogeneity in years of education, college degree attainment, and natural log of income.

We exploit individuals' month of birth as the running variable within the Regression Discontinuity Design (RDD) framework. We construct a running variable that measures the number of months between an individual's birth month and the September 1981 cutoff for school entry. Specifically, we calculate the distance in months from September 1981 using year and month of birth. Meanwhile, considering the case in China and to get the cleanest treatment effect, the sample is then restricted to individuals born within 3 years before and after the cutoff to ensure local comparability as well as usable sample size. After the selection of observations who are potentially affected by the Expansion or just missed it, there are around 6,328 observations left.

To implement the RDD, there should be no manipulation around the cutoff, and the treated group and control group should be balanced. As shown in the Table 3 in Appendix 8.2, the treated group and control group are mostly balanced except for the illiteracy rate of parents, which would be controlled. In the meantime, some recent study brings up the endogenous seasonality of childbirth in China (Yang, 2021). To be more specific, workers

[5] Specifically, the 2010, 2011, 2012, 2013, 2015, and 2021 waves. Data from other years are not included because they lack information on month of birth, which prevents the construction of the running variable.

reunite with their family in annual spring festival around January (the Lunar New Year), and therefore, there is a big baby bump after October each year especially for the migrant workers, as shown in the Figure 4 in Appendix 8.3. The result of McCrary Density Test illustrates this pattern around our cutoff, and hence, we will control for the month of birth in the analysis.

4 Research Design

We use the sharp RDD and then interact treatment with subgroups to get the causal effects, which can analyze the baseline effect from the Expansion per se and the differential effects on different subgroups. The outcome of interests are individuals' years of education, dummy variable of having college degree or not, and the log of income.

4.1 Plain Sharp RDD

To analyze the differential treatment effect of the Expansion, we have to focus on the individuals who just got exposed to it versus those who just missed it. Therefore, using the date September 1st, 1981 as the cutoff of the running variable in our RDD.

$$\mathrm{Y}_i = \alpha_0 + \alpha_1 \cdot \mathrm{Post}_i + \alpha_2 \cdot f(R_i) + X_i'\alpha_3 + \epsilon_i$$

The variable Post_i is an indicator equal to one if the individual i was born on or after the cutoff date of September 1, 1981. R_i denotes the running variable, defined as the number of months from the cutoff. X_i' is a vector of covariates, including individual characteristics and the fixed effects of year, month of birth, and county to take into the unexplained variations into account. ϵ_i is the error term.

4.2 Interaction with Subgroups

Similarly, we could implement the following specification to indicate the potentially heterogeneous effects of how the Expansion influences different subgroups.

$$\begin{aligned}\mathrm{Y}_i = \beta_0 &+ \beta_1 \cdot \mathrm{Post}_i + \beta_2 \cdot (\mathrm{Post}_i \times \mathrm{Group}_i) + \beta_3 \cdot \mathrm{R}_i + \beta_4 \cdot (\mathrm{Post}_i \times \mathrm{R}_i) \\ &+ \beta_5 \cdot (\mathrm{R}_i \times \mathrm{Group}_i) + \beta_6 \cdot \mathrm{Group}_i + \beta_7 \cdot (\mathrm{Post}_i \times \mathrm{R}_i \times \mathrm{Group}_i) + X_i'\beta_8 + \varepsilon_i\end{aligned}$$

where the variable Group_i indicates the subgroup to which observation i belongs, which in our analysis could be gender, region, parental background, etc. ε_i is the error term.

In this specification, the coefficient β_1 captures the baseline effect of the Expansion, while β_2 measures the differential average treatment effect for the subgroup. More specifically, β_2 indicates whether belonging to Group_i modifies the magnitude of the treatment effect. For example, it shows whether being female amplifies or attenuates the impact of the Expansion.

5 Results

5.1 Plain Sharp RDD

The Sharp RDD serves as a sanity check, helping to verify whether the effect of the Expansion is large enough to be detected, and unique for the specific year 1999. As shown in the RDD plot and the results in Figure 2 and Table 1, individuals born after September 1, 1981 – who progressed through school normally and were exposed to the Expansion – are 5.4%[6] more likely to obtain a college degree, despite the significance being modest (p<0.1). One should be aware that the results of RDD is local, and the outcome of individuals should not be directly compared with national aggregate results. Therefore, the impact of the Expansion, as we believe, should still be large enough to observe.

Table 1: Sharp RDD Estimates at the True and *Fake* Cutoffs

	Dependent Variable		
	Years of Education	College Degree	ln(Income)
Panel A: *Fake cutoff*, June 1981			
Treatment Effect	0.5723^{*}	-0.0094	0.0858
	(0.3075)	(0.0359)	(0.1061)
Panel B: True cutoff, September 1981			
Treatment Effect	-0.2859	0.0547^{*}	0.0717
	(0.2517)	(0.0318)	(0.0856)
Panel C: *Fake cutoff*, December 1981			
Treatment Effect	-0.6168^{**}	-0.0333	-0.1234
	(0.2333)	(0.0337)	(0.0990)

Notes: This table reports conventional Sharp RDD estimates of the effect of exposure to the 1999 college expansion using a cutoff of September 1, 1981. Covariates include gender, parental characteristics, ethnicity, region, and year-of-birth fixed effects. Standard errors are reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, and $^{***}p < 0.01$. Significance is based on the p-value from the Robust method in the `rdrobust` package.

However, because school entry policy in China follows a September-based academic calendar as aforementioned, the RDD cutoff may coincide with cohort boundaries, raising concerns that the estimated discontinuity may reflect cohort effects rather than the policy effect itself. To address this concern, I conduct placebo tests using alternative cutoffs three months before and after the true cutoff (i.e., June 1981 and December 1981). These alternative cutoffs neither correspond to any policy change nor school-entry cohorts. If significant discontinuities are found at these placebo cutoffs, it would suggest that cohort effects may confound the estimates. We expect, for the college degree attainment, is only significant at the true policy cutoff and not at the placebo cutoffs, this provides stronger evidence that the observed jump is driven by the policy itself, rather than pre-existing cohort differences. As shown in Table 3, the placebo tests using the same specification but applying *fake cutoffs* show no significant effects on college degree attainment — the Expansion only has an effect on college attainment around the true cutoff. Although we observe some discontinuity in years of education, the placebo results suggest that this is not driven by college degree attainment and, therefore, not attributable to the Expansion, which should have had no effect at those alternative cutoffs.

[6]Recent papers suggest that the polynomial order should be kept linear, quadratic, or other smooth function to avoid misleading results (Gelman & Imbens). So, we only report the most conservative linear specification with polynomial order 1 to avoid manipulation or overfitting. Despite not reporting here, when we use quadratic specification, we would obtain an increase in college degree attainment around the true cutoff by 13%.

5.2 Interaction with Subgroups

Although the Expansion is national and simultaneous, the potential heterogeneous treatment effect is expected. For example, the discrimination against agricultural *hukou*, certain underdeveloped region, and female in education is well-documented for (Meng & Zhang, 2001; Kidd & Meng, 2003; Hannum, 2005). The benefits of interacting the treatment indicator and group indicator is that we can observe the differential treatment effect compared with the baseline. In this section, we interact the treatment variable with region indicator (North, East, coastal, and inland), *hukou* status indicator (agricultural and non-agricultural), gender, and parental educational and political background. The same specifications using *fake cutoff* are conducted as well serving as a placebo, and the results are shown in the Appendix 8.4 Table 5 and 6 as well, and we observe no significant effects on college degree attainment for the regions, while some time trends for gender, *hukou* status, and parental political background are found.

The differential treatment effect on ethnicity is not our focus based due to limited observations (N = 603). Moreover, ethnic minority groups in China receive preferential treatment in the *gaokao*, further complicating efforts to isolate the causal impact of the Expansion. While one might consider treating all 55 officially recognized minority groups as a single category, the substantial cultural, regional, and institutional heterogeneity (from regional ethnic autonomy system or even distinctive affirmative policies) among them renders such an approach of limited interpretative value. As a result, heterogeneity analysis based solely on minority status provides little meaningful insight for understanding the policy effect among ethnic minorities (Mackerras 2003; Gladney 2004; Harrell et al., 1995).

5.2.1 Regional Heterogeneity

As shown in Table 2 Panel A, there is significant heterogeneity on the attainment of college degree. For those who reside in the Eastern area or the coastal area, the Expansion increased their probability of college attainment by 12.2% and 5.9% respectively. Compared with the baseline, the numbers are 18.3% and 12%. While the inland counterpart shows a 1.7% decrease in the college attainment rate, and the differential decrease is as large as 12.7%.

But, there is significant heterogeneity in the attainment of a college degree. For those who reside in the Eastern area or the coastal area, the Expansion increased their probability of college attainment by 18.3% and 12% relative to the baseline, corresponding to total effects of 12.2% and 5.9%, respectively. The inland counterpart shows a 12.7% relative decrease compared with the baseline, yielding a total effect of -1.6%.

However, this linear RDD provides limited evidence that the increase in college attainment translated into higher income or additional years of schooling. For these outcome variables, we only find that residents in the Northern region experienced a 40.4% increase in income following the Expansion, relative to the baseline. The fact that this income gain does not appear to stem from increased educational attainment remains an open question.

Table 2: RDD Interaction Effects by Region (Panel A) and by Group (Panel B)

	Panel A: Region				**Panel B: Group**			
	East	Coastal	North	Inland	Agri *hukou*	Male	Parent CCP	Parent Illit.
Years of Education					*Years of Education*			
pos	0.126	0.534	0.212	0.728	−0.122	0.132	0.377	0.260
	(1.713)	(1.718)	(1.721)	(1.781)	(1.490)	(1.479)	(1.466)	(1.474)
pos × group	0.707	-0.276	0.474	0.133	0.473	0.204	−0.596	−0.036
	(0.587)	(0.591)	(0.581)	(0.607)	(0.473)	(0.472)	(0.489)	(0.471)
College Degree					*College Degree*			
pos	-0.008	0.015	0.057	0.111	0.148	0.077	0.034	0.059
	(0.178)	(0.178)	(0.180)	(0.186)	(0.173)	(0.172)	(0.170)	(0.170)
pos × group	0.183***	0.120**	0.024	-0.127**	−0.153***	−0.032	0.039	−0.013
	(0.061)	(0.061)	(0.061)	(0.063)	(0.055)	(0.055)	(0.057)	(0.054)
Log Income					*Log Income*			
pos	-0.297	-0.268	-0.461	-0.183	0.035	−0.097	−0.153	0.081
	(0.610)	(0.610)	(0.609)	(0.648)	(0.529)	(0.526)	(0.523)	(0.522)
pos × group	0.092	-0.000	0.404**	-0.092	−0.283*	−0.054	0.039	−0.482***
	(0.209)	(0.210)	(0.206)	(0.221)	(0.168)	(0.168)	(0.174)	(0.167)
Observations	1342	1342	1342	1342	1933	1934	1933	1934
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓
Parental Controls	✓	✓	✓	✓	✓	✓	✓	✓
County-Time-Dataset FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Panel A reports regional heterogeneity (East, Coastal, North, Inland); Panel B reports group heterogeneity (Agricultural *hukou*, Male, Parent CCP Member, Parent Illiterate). Each cell reports coefficients from a separate RDD regression. Outcomes are years of education, a college degree indicator, and log income. All regressions control for individual characteristics (height, month of birth), parental background (parents' education and CCP membership), and include county, time, and dataset fixed effects. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

To interpret the results as the differential treatment effect of the Expansion for each region, we need to strongly assume that residents in each regions are not migrated from other places. Otherwise, we are not able to determine whether this is the effect of the Expansion, or simply the demand of college graduates from these area, not to mention the 2014 *hukou* reform that has largely decreased the cost of migration and increase the labor mobility (Jin & Zhang, 2021; Liu et al., 2025; Chen et al., 2024; An et al., 2024). To solve this potential selection bias, for the regional heterogeneity test, I only use the observations (68.8% of the total sample, N = 4359) who have never migrated out of the province where they were born. I also relax that restriction and run the same specification with the full sample (N = 6328). In Table 4 in the Appendix 8.4, we show that the results are qualitatively and quantitatively similar for the college degree attainment rate if not more significant, which is consistent with our prior expectation and the reality that cross-regional migration is rare and usually hard because of cultural shocks and different institutional contexts.

5.2.2 ***Hukou*, Gender, and Parental Background Heterogeneity**

The second set of heterogeneity analyses examines differences by *hukou* status and gender. Prior literature has consistently documented that female and those who hold the agricultural *hukou* face systemic disadvantages in both educational and labor market opportunities. As we report in Table 2 Panel B, we find that the Expansion reduced the likelihood of obtaining a college degree by 15.3% for agricultural *hukou* holders, corresponding to the total effect of -5%. In the meantime, there is mild evidence suggesting that they suffer from a 28.3% decrease in income, corresponding to the total effect of -24.8%, potentially attributable to the Expansion that differentially reduces their probability of getting a degree. In contrast, different from the existing literature, we find no statistically significant evidence of heterogeneity by gender, which is surprising (e.g., Zhang & Zhu, 2024).

We then analyze whether parental backgrounds, be they parental education or political status (defined as being the member of the CCP), amplify or attenuate the effect of the Expansion. The result is inconclusive in terms of the college degree attainment as well as the years of education. Nonetheless, individuals who have at least one illiterate parent – defined as having zero years of education – experience a 48.2% reduction in income, and the total effect, after adding the results of the baseline, is still as large as 40.1%.

One potential concern is raised by our placebo test using a fake cutoff three months after the actual one: agricultural *hukou* holders still show a statistically significant 13.9% decrease ($p<0.1$) in college attainment (Appendix Table 6 Panel B). We interpret this not as a threat to our identification strategy, but rather as consistent with the policy context—namely, that the Expansion was not a one-time shock but a gradual increase in enrollment quotas. Thus, observing significant effects around nearby cutoffs reinforces, rather than undermines, the credibility of our findings.

The more notable placebo result is that individuals whose parents are CCP members begin to show a 19.1% increase in college attainment at the fake cutoff. This suggests that the political advantage embedded by CCP membership may not have been immediate, but instead emerged gradually as the policy was implemented over time.

Taken together, these results imply that while the Expansion may have equalized access to higher education across many observable dimensions, substantial inequality in income persists—particularly for those from disadvantaged family backgrounds, such as children of illiterate parents. This underscores the importance of examining not just educational access, but also material outcomes in evaluating the long-term effects of education policy.

6 Conclusion and Discussion

We examine the causal effect of China's 1999 College Expansion (hereafter, the Expansion) using Regression Discontinuity Designs that exploit month of birth as the running variable. Leveraging two nationally representative datasets, we focus on heterogeneity across multiple subgroups.

First, we find pronounced regional disparities in the policy's impact. Residents in economically advanced regions – particularly the Eastern and coastal areas – experienced clear educational gains in attaining a college degree by 18.3% and 12% respectively. By contrast, individuals from underdeveloped regions such as the North and inland areas did not share in these benefits; in fact, they suffered significantly lower rates of college attainment – by as much as 12.7%. These findings suggest that, rather than mitigating regional inequality, the Expansion may have exacerbated it in terms of educational outcomes. Interestingly, we also document a puzzling 40.4% increase in income among Northerners, which warrants further investigation.

Second, we explore differential effects by *hukou* status and gender, motivated by well-established disadvantages faced by individuals with agricultural *hukou* and by women in both educational and labor market contexts. We find no evidence of a gender gap in the policy's effect on any outcome, including education and income. However, individuals with agricultural *hukou* experienced a 15.3% decline in the likelihood of obtaining a college degree, alongside a marginally significant 28.3% ($p<0.1$) reduction in income. These results reinforce the notion that systemic institutional barriers continue to constrain rural populations, even under large-scale education reforms in 1999 and the *hukou* reform in 2014.

Third, we examine heterogeneity by parental background – specifically, whether one's parents were members of the Chinese Communist Party or were illiterate. We detect no significant effects on education outcomes in either subgroup. However, individuals with illiterate parents experienced a striking 48.2% reduction in income, highlighting the long-lasting economic consequences of intergenerational educational disadvantage.

Overall, our findings reveal that the Expansion, while substantially increasing higher education access in aggregate terms, did not serve as an equalizing force. On the contrary, it appears to have widened existing inequalities across regional, institutional, and socioeconomic lines – especially in college attainment and income.

These insights have important implications for current and future education policy, particularly the postgraduate expansion initiative that was launched in 2024. Given the higher selectivity and resource demands of master's and doctoral programs, there is a serious risk that such policies may disproportionately benefit already-advantaged students – those with stronger prior education, greater financial support, and better access to information. Without intentional redistribution mechanisms, e.g., allocating more graduate slots to students from underrepresented areas, providing needs-based scholarships, and expanding graduate education infrastructure in less-developed provinces, the 2024 expansion may reproduce or even deepen the stratifying effects observed in 1999. Policymakers should draw on the lessons of the past to ensure that future expansions of higher education promote not only access, but also equity.

7 Reference

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8 Appendix

8.1 Education Timeline

This Appendix provides institutional details on the structure of China's education system and the role of school entry cutoffs in enabling our regression discontinuity design (RDD).

8.1.1 School Entry Cutoff

In China, children are eligible to enter primary school only if they turn six years old before September 1st of the enrollment year. This rule is strictly enforced by education bureaus nationwide (Ministry of Education, 2009). For instance, a child born on August 31, 1981 would be eligible to enroll in primary school in September 1987, while a child born one day later, on September 1, 1981, would have to wait until September 1988 to start school. Despite postponing to seven years old is possible, it is rare and not encouraged.

This policy generates a sharp discontinuity in school entry based on date of birth, leading to highly predictable patterns of educational progression.

8.1.2 Mapping Birth Cohorts to *gaokao* Cohorts

The typical Chinese educational trajectory consists of:

- 6 years of primary school,
- 3 years of junior high (lower secondary, grade 7 to 9) school,
- 3 years of high (upper secondary, grade 10 to 12) school.

Therefore, students generally take the *gaokao* at the end of their 12th year of schooling, usually around the age of 18, in June.

As a result:

- Students who started primary school in September 1988 would have entered high school in 1997 and taken the *gaokao* in June 1999.
- These students were born between September 1, 1981 and August 31, 1982.

Hence, this birth cohort was the first to be fully exposed to the 1999 college expansion policy. In contrast, those born just earlier (e.g., between September 1, 1980 and August 31, 1981) would have taken the *gaokao* in 1998, before the expansion took place. Again, despite postponing primary education by a year is possible, it is not common and not encouraged. We further assume that parents do not have the information that the Expansion would have happened in 1999 and hence intentionally delay children's school year by a year.

This sharp and natural assignment mechanism based on the date of birth (in our study, month of birth) provides the basis for our RDD identification strategy.

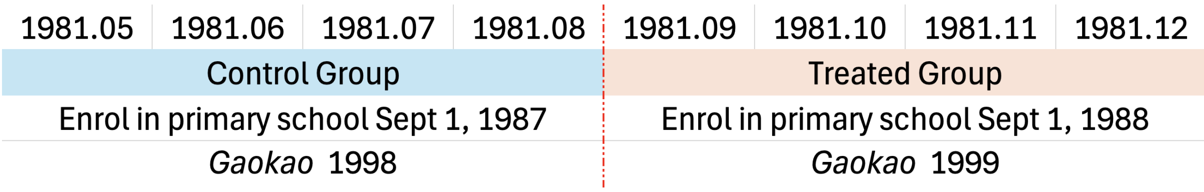

Figure 3: Timeline Graph of China's Education System and Identification of Control and Treated Group.

8.2 Balance Table

Table 3: Balance Check: Standardized Differences

	pos=:Mean_Control	pos=:Mean_Treatment	.:Std_Diff
Gender	.5343721	.5233959	.0219885
Height (cm)	166.017	166.7331	-.0896991
minor	.103758	.0892399	.0491785
Hukou status	1.893859	1.909378	-.0155939
agri	.5530706	.545311	.0155939
Northern province (1=yes, 0=no)	.4691109	.476999	-.0157982
east	.4571952	.4846989	-.0551161
west	.2476627	.2386969	.0208982
central	.1985335	.1879566	.0267874
inland	.4188605	.3972922	.0438908
coas	.3950504	.4232971	-.0574675
parent_illiterate	.4636114	.3447187	.2440517
dad	.2883593	.2785785	.0217015
mom	.2001833	.2114511	-.0278702
Dataset source	.6410632	.6345508	.0135486

8.3 Manipulation Plot

Figure 4: McCrary Density Test

8.4 Relaxed definition of region and placebo tests with *fake cutoffs*

Table 4: RDD Interaction Effects by Region (loosely defined) and Outcome

	East	Coastal	North	Inland
Years of Education				
pos	-0.148	0.242	0.194	0.434
	(1.471)	(1.472)	(1.478)	(1.519)
pos × group	0.842*	-0.031	0.096	0.196
	(0.468)	(0.471)	(0.466)	(0.495)
College Degree				
pos	-0.027	0.012	0.055	0.091
	(0.171)	(0.170)	(0.172)	(0.177)
pos × group	0.191***	0.115**	0.016	-0.106*
	(0.054)	(0.054)	(0.054)	(0.058)
Log Income				
pos	-0.233	-0.192	-0.223	-0.082
	(0.523)	(0.521)	(0.524)	(0.549)
pos × group	0.211	0.141	0.220	-0.115
	(0.166)	(0.167)	(0.165)	(0.179)
Observations	1934	1933	1933	1933
Individual Characteristics	✓	✓	✓	✓
Parental Background	✓	✓	✓	✓
County-Time-Dataset FE	✓	✓	✓	✓

Note: The dependent variables are individuals' years of education, a college degree indicator, and the log of income. Regions follow the Chinese government's definitions. The definition of region here refers to the respondents' location at the time of the survey. All regressions control for individual characteristics (height, month of birth), parental background (both parents' years of education, both parents' CCP membership), and include county, time, and dataset fixed effects. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: June 1981 Placebo RDD Interaction Effects by Region (Panel A) and by Group (Panel B)

	Panel A: Region				**Panel B: Group**			
	East	Coastal	North	Inland	Agri *hukou*	Male	Parent CCP	Parent Illit.
Years of Education					*Years of Education*			
pos	0.143	-0.148	0.568	-0.193	−0.534	0.037	−0.298	−0.000
	(0.577)	(0.570)	(0.588)	(0.586)	(0.600)	(0.595)	(0.559)	(0.576)
pos × group	-0.419	0.384	-1.069**	0.561	1.036**	−0.010	0.988*	0.068
	(0.484)	(0.494)	(0.482)	(0.507)	(0.491)	(0.479)	(0.507)	(0.480)
College Degree					*College Degree*			
pos	-0.032	-0.065	-0.072	-0.074	−0.079	−0.118*	−0.112*	−0.075
	(0.066)	(0.065)	(0.067)	(0.067)	(0.069)	(0.068)	(0.064)	(0.066)
pos × group	-0.073	0.009	0.023	0.030	0.038	0.109**	0.158***	0.046
	(0.055)	(0.056)	(0.055)	(0.058)	(0.056)	(0.055)	(0.058)	(0.055)
Log Income					*Log Income*			
pos	-0.125	-0.200	0.149	0.106	0.035	−0.080	0.065	0.054
	(0.197)	(0.194)	(0.201)	(0.203)	(0.205)	(0.203)	(0.192)	(0.196)
pos × group	0.253	0.533***	-0.267	-0.195	−0.031	0.174	−0.152	−0.073
	(0.165)	(0.168)	(0.164)	(0.175)	(0.168)	(0.164)	(0.174)	(0.164)
Observations	2042	2042	2042	1916	2042	2042	2042	2042
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓
Parental Controls	✓	✓	✓	✓	✓	✓	✓	✓
County-Time-Dataset FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Panel A reports regional heterogeneity (East, Coastal, North, Inland); Panel B reports group heterogeneity (Agricultural *hukou*, Male, Parent CCP Member, Parent Illiterate). Each cell reports coefficients from a separate RDD regression using the placebo cutoff of June 1981 (3 months before the real cutoff). Outcomes are years of education, a college degree indicator, and log income. All regressions control for individual characteristics (height, month of birth), parental background (parents' education and CCP membership), and include county, time, and dataset fixed effects. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: December 1981 Placebo RDD Interaction Effects by Region (Panel A) and by Group (Panel B)

	Panel A: Region				**Panel B: Group**			
	East	Coastal	North	Inland	Agri *hukou*	Male	Parent CCP	Parent Illit.
Years of Education					*Years of Education*			
pos	0.204	0.102	0.671	0.286	0.009	0.281	−0.194	0.602
	(0.524)	(0.501)	(0.561)	(0.548)	(0.585)	(0.573)	(0.495)	(0.537)
pos × group	0.248	0.697	-0.621	0.218	0.607	0.135	1.435*	−0.550
	(0.715)	(0.737)	(0.710)	(0.733)	(0.716)	(0.709)	(0.744)	(0.707)
College Degree					*College Degree*			
pos	-0.008	-0.038	-0.026	-0.022	0.070	−0.069	−0.083	0.007
	(0.062)	(0.059)	(0.066)	(0.065)	(0.069)	(0.068)	(0.058)	(0.063)
pos × group	-0.010	0.074	0.033	-0.017	−0.139*	0.115	0.191**	−0.033
	(0.084)	(0.087)	(0.084)	(0.087)	(0.084)	(0.084)	(0.088)	(0.083)
Log Income					*Log Income*			
pos	-0.250	-0.269	0.110	0.284	0.235	−0.064	0.093	0.264
	(0.191)	(0.182)	(0.205)	(0.203)	(0.214)	(0.210)	(0.182)	(0.196)
pos × group	0.629**	0.839***	-0.162	-0.497*	−0.361	0.178	−0.202	−0.477*
	(0.261)	(0.268)	(0.259)	(0.271)	(0.262)	(0.259)	(0.273)	(0.258)
Observations	2249	2249	2249	2105	2249	2249	2249	2249
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓
Parental Controls	✓	✓	✓	✓	✓	✓	✓	✓
County-Time-Dataset FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Panel A reports regional heterogeneity (East, Coastal, North, Inland); Panel B reports group heterogeneity (Agricultural *hukou*, Male, Parent CCP Member, Parent Illiterate). Each cell reports coefficients from a separate RDD regression using the placebo cutoff of December 1981 (3 months after the real cutoff). Outcomes are years of education, a college degree indicator, and log income. All regressions control for individual characteristics (height, month of birth), parental background (parents' education and CCP membership), and include county, time, and dataset fixed effects. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.