The Effect of Education on Voter Turnout in China's Rural Elections

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Abstract

Conventional wisdom and evidence from democracies suggest that more education should increase voter turnout. This paper revisits this issue by analyzing turnout in China's rural elections. Employing an instrumental variable strategy, I find that more education reduces turnout in rural elections. I provide suggestive evidence that more educated people may face higher opportunity costs of voting, which explain about 44% of cross-province variations in education-turnout links. I also discuss the role of other factors, including liberal attitudes, Confucian culture, and election stakes.

Keywords: Education, Turnout, Rural Elections, China, Compulsory Schooling Law
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1 Introduction

The relationship between education and political participation is an enduring research topic for students of political economy (see Willeck and Mendelberg, 2022 for review). In mature democracies, the education-participation link is closely related to representation and government accountability (arguably, by the best-informed people in a society), which, in turn, are crucial for political stability. The link also has implications for transitional societies: the modernization theory famously envisions that improvements in a country's education would lead to more democratic politics through promoting participation (Glaeser et al., 2007). As such, it is crucial to understand how education affects political participation in different contexts.

Conventional wisdom suggests that education is positively related to political participation (e.g., Almond and Verba, 1963; Nie et al., 1996; Rosenstone and Hansen, 1993), and some studies have established causality (Dee, 2004; Milligan et al., 2004; Sondheimer and Green, 2010). Education is even labeled as "the best individual-level predictor of participation" (Putnam, 1995). However, most evidence comes from *democracies*, with little attention paid to other settings. It could be useful to further investigations to *autocracies*, as they also feature various forms of political participation, including elections.

This paper is built upon the Chinese context. Specifically, I study the effect of education on a particular form of participation: turnout in rural elections. Since the 1980s, China has allowed villagers to elect their village leaders to address local governance challenges (Martinez-Bravo et al., 2022).[1] These elections are the only meaningful elections that have ever existed in China, thus providing a unique lens to study political participation in China. However, as with previous studies, identifying the causal effect of education on turnout is complicated by the endogeneity of educational attainment. For instance, omitted variable bias is one important concern: a range of factors can influence education and political behavior simultaneously (Kam and Palmer, 2008). To address this issue, I employ an instrumental variable (IV) strategy. I exploit cohort-level variation in exposure to China's Compulsory Schooling Law (CSL), which mandates compulsory schooling for children between ages 6 and 15. I show that exposure to the CSL strongly predicts improvements in educational attainment. Then, relying on CSL-induced variation in education, my primary finding is that education *reduces* turnout in China's rural elections, contrary to conventional wisdom. The effect is sizeable. The reduced-form estimates show that full exposure to the CSL reduces the probability of voting in rural elections by 14.3–14.7 percentage points. The two-stage least squares (2SLS) estimates imply that, on average, a one-year increase in schooling reduces the probability of voting by 10.8–13.8 percentage points.

The validity of the IV strategy hinges on the exclusion restriction that the CSL affects turnout only through education. The main concern is that exposure to the CSL may be correlated with unobserved factors influencing turnout. I conduct a battery of checks to alleviate this concern.

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[1]See Section 2.1 for discussions of these elections in detail.

First, I estimate an event study model to examine the reduced-form effects of the CSL on turnout cohort by cohort. There is no evidence of pretrends of turnout leading up to cohorts affected by the CSL. Second, I use different ways to control for potential confounders. My findings survive the inclusion of province-specific trends in birth cohorts. In addition, I show that the results are robust to restricting the analysis to different samples where confounders would be of lesser concern: (i) I consider a narrow bandwidth of cohorts who are more similar apart from exposure to the CSL; (ii) I look at individuals from provinces where the CSL was most effective so that other factors should have played a relatively minor role; (iii) I use a matching approach to construct a paired sample in which each pair of individuals are matched on observed characteristics. Lastly, I use the methodology developed by Conley et al. (2012) to assess the robustness of the 2SLS estimate to violations of the exclusion restriction. The negative link between education and turnout could withstand large violations of the exclusion restriction.

I provide suggestive evidence for one potential explanation of my findings, which concerns the opportunity cost of voting. Education may be linked to higher returns from production efforts, resulting in a higher cost associated with spending time on voting. Supporting this opportunity cost explanation, I observe that education has a more negative effect on voter turnout for people in regions with high returns to education and in non-agricultural sectors, where education is better compensated. These results align with the cross-country findings of Campante and Chor (2012). Also, education reduces turnout more when social trust, or trust in other people, is low. This is consistent with the notion that social capital is related to the willingness to bear participation costs (Nannicini et al., 2013). Furthermore, I find that the opportunity cost of voting, as measured by the returns to education, may account for approximately 44% of cross-province variations in the effect of education on turnout.

This paper speaks to the vast literature on the relationship between education and political participation. As mentioned earlier, conventional wisdom contends that more education would increase participation, as it improves abilities necessary for participation (Almond and Verba, 1963; Campbell et al., 1980; Wolfinger and Rosenstone, 1980; Carpini and Keeter, 1996), cultivates a sense of civic duty (Campbell et al., 1980), or places people in networks that encourage participation (Nie et al., 1996; Rosenstone and Hansen, 1993). However, some recent papers offer more nuanced insights, emphasizing the importance of contextual factors in shaping the relationship between education and participation. For instance, Campante and Chor (2012) document that the link between education and participation varies across countries and depends on factors such as land, human capital, and cultures; thus, to account for these patterns, they propose a theory in which the opportunity cost of participation plays a central role. Another relevant study is Croke et al. (2016). They study Zimbabwe's national elections and find that education reduces turnout; their explanation is that educated, democratic-minded people deliberately disengage to avoid legitimizing the autocrat. This paper adds to these new insights by offering evidence from China's rural elections. Notably, I show that the opportunity cost helps explain education's negative effect

on turnout, and unlike Croke et al. (2016), I do not find evidence that education fosters liberal attitudes in China.

This paper also relates to the literature on China's rural elections. Most previous research has focused on the impacts of rural elections on local governance and villagers' livelihoods (Zhang et al., 2004; Shen and Yao, 2008; Mu and Zhang, 2014; Martinez-Bravo et al., 2022). However, few studies have investigated the issue of causal determinants of participation in these elections. This issue could be interesting, given that rural elections represent a unique instance of institutionalized political participation in China. I contribute to this inquiry by shedding light on how education affects participation in rural elections.

The remainder of this paper proceeds as follows. Section 2 introduces the institutional background and data. Section 3 presents the research design. Section 4 reports the results of my analyses. Section 5 concludes.

2 Background and Data

2.1 China's Rural Elections

Rural China has undergone significant institutional changes since the reform era began in 1978. Regarding rural governance, the 1982 Chinese Constitution granted villagers the autonomy to manage their villages by electing a village committee as the "grassroots self-governing body". However, it was only after the *Organization Law of Village Committees* (OLVC) was introduced in 1987 that these elections were institutionalized and widely implemented (Shen and Yao, 2008), despite the Constitution's explicit terms about direct elections of village committees.[2]

As per the OLVC, a village committee comprises 3-7 members, including a chairperson, a vice chairperson, and several committee members. The members must be directly elected by villagers in competitive elections where the number of candidates exceeds the number of posts. The village committee's tenure is three years, with no imposed term limits, which in theory creates reelection incentives. Village committees are expected to deal with a range of local issues, such as public goods provisions (e.g., irrigation, schools, and roads), resource allocation (e.g., land, collective properties), dispute resolution, and enforcement of mandated policies (e.g., one-child policy). Thus, rural elections become the only meaningful elections that have ever existed in China, as people can choose officials who enforce policies relevant to their livelihoods.

Voluminous studies have provided empirical accounts of the impacts of rural elections on villagers' livelihoods. Generally speaking, rural elections have aligned village officials' actions with villagers' preferences. For instance, Martinez-Bravo et al. (2022) find that rural elections led

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[2]Alongside the village committee, the village branch of the Communist Party of China (CPC) is another important governing body of a village. Although the reforms introduced direct elections of village committee members, upper-level authorities continued appointing village party officials.

to improved implementation of popular policies, such as the provision of public goods, while weakening the implementation of less popular policies like the one-child policy. However, rural elections also have some limitations in local governance. One factor that has garnered significant attention in previous studies is local clans, the traditional informal institutions that govern rural life. The interplay between clans and rural elections could significantly impact local governance. There is evidence that candidates backed by large clans are more likely to win elections (Shen and Yao, 2008); and when a large clan controls office, public investment tends to increase considerably due to either better coordination facilitated by clan networks (Xu and Yao, 2015) or informal accountability mechanisms among clan members (Tsai, 2007).

While there is a wealth of information on the policy consequences of rural elections, there has been relatively little focus on the issue of people's participation. The OLVC provides detailed regulations regarding participation in rural elections. To be eligible to vote in a village election, a person must be at least 18 years old and have their *hukou* (official residency registration) registered in the village. Individuals without a local *hukou* may still be eligible to vote if they meet certain requirements, such as having lived or worked in the village for over a year and obtaining approval from the election organizing body after submitting an application to participate in the election. Additionally, a person may only vote in one village, either where they have their *hukou* or where they live or work. Given these institutional barriers to voting and the challenges associated with changing *hukou* registration, most people can only vote in their *hukou* village.

Typically, voters should vote in person, in the village, and on the election day. While proxy voting is permitted, it is subject to certain restrictions. There are no specific regulations governing the selection of the election day; thus, it could fall on workdays.[3]

Previous studies have documented some patterns of participation in rural elections. Their findings indicate that when deciding whether to vote, Chinese villagers carefully weigh the benefits and costs. For instance, Oi and Rozelle (2000) find that turnout is higher in agricultural villages, where elections are more important because the elected officials can allocate land. Similarly, Hu (2005) notes that economic development increased collective revenues controlled by village committees, thereby inspiring more participation in elections. Another illuminating anecdote is that in order to boost turnout rates, some localities proposed compensating voters for their lost labor and travel costs, as many villagers were reluctant to forgo labor revenues and thus did not engage in elections (Wong et al., 2020).

To summarize, rural elections have tangible influences on villagers' livelihoods, and villagers are sophisticated when making turnout decisions. That said, voting in rural elections is not trivial, so it would be interesting to examine the effect of education on turnout in China, provided that education has been labeled as "the best individual-level predictor of participation" (Putnam, 1995) in the West.

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[3]For instance, Xinshi village in Hainan held its election on Friday, March 5, 2021 (see https://www.163.com/dy/article/G4FTPJAS053469JX.html, in Chinese, retrieved on August 22, 2022).

2.2 Compulsory Schooling Law

Identifying the causal effect of education on voter turnout requires exogenous variation in education, as education is likely associated with other factors that encourage participation (Kam and Palmer, 2008). For instance, both education and voting require cognitive abilities to acquire and process information. An individual's family background, such as parents' state employment, may influence both educational attainment and political behavior (Wang and Sun, 2017). Thus, endogeneity in educational attainment may bias the estimation of education's effect on voter turnout. To address this issue, the study employs China's Compulsory Schooling Law (CSL) as an instrument for educational attainment. This approach is commonly used in the literature to examine the causal impact of education policies on various outcomes, including political behavior (Croke et al., 2016; Marshall, 2016b).

The central government enacted the Compulsory Schooling Law (CSL) in 1986, making it the first law to formally specify national education policy in China (Fang et al., 2012; Huang, 2015). The law was quickly adopted by every province. Figure 1 displays the timing of the CSL's implementation across the provinces included in my sample. I obtained this information from Du et al. (2021), who used the issue dates of official documents. To study the causal effect of education on voter turnout, I employ the CSL as an instrument for educational attainment, following a large body of literature that uses variation in education policy to examine the impact of education on a range of outcomes, including political behavior (e.g., Croke et al., 2016; Marshall, 2016b).
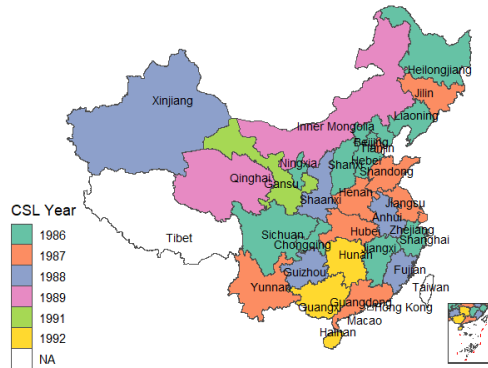

Figure 1. Rollout of the CSL

Note: This figure displays the rollout of the CSL across provinces in the sample (NA = excluded from the sample). Details of sample construction are provided in Section 2.3.

My research design relies on two features of the CSL. First, the law makes nine years of schooling mandatory, including six years of primary school and three years of middle school education under China's education system. This mandate creates plausibly exogenous improvements in educational attainment, especially for those who otherwise would not have received formal education. When the CSL was promulgated, a large Chinese population had little formal education. According to the 1982 Chinese Census, only 22.8% of people above 25 years old had completed middle school or

6

above. Secondly, the CSL requires children to attend school from age 6 and stay until age 15. Several measures are implemented to facilitate the enforcement of compulsory education, including a ban on the employment of children between the ages of 6 and 15 and the collection of education taxes to finance compulsory education. Local officials are held accountable for compulsory education enrollment. Enrollment rates might even be included in the evaluations that determined officials' promotions, thus creating high-powered political incentives to enforce the CSL. As a result, local education authorities require those under 15 who have already left school by the CSL's effective date to return to school and stay until they turn 15 (Fang et al., 2012).

The CSL imposes a significant constraint on the educational attainment of children aged 6 to 15, with younger children being subject to a more stringent law due to their longer exposure period between these ages. I construct a variable *Exposure* to measure an individual's level of exposure to the CSL. This measure is derived from the proportion of the 10 years between the ages of 6 and 15 that the CSL was in effect for the individual. This coding strategy is consistent with previous research using age-based variation to capture the impact of policy interventions (e.g., Hoynes et al., 2016). Figure 2 illustrates this measure as a function of the individual's age when the CSL was adopted. Individuals aged 16 or older received a *Exposure* status of 0 as they were not exposed to the CSL. Individuals aged between 5 and 15 were partially exposed, with *Exposure* ranging from 0.1 to 0.9, while those aged 6 or younger were fully exposed and received a *Exposure* status of 1. Therefore, *Exposure* captures individuals' varying degrees of exposure to the CSL in a linear fashion.

Figure 2. Exposure to the CSL Visualized

Note: This figure visualizes the measure of exposure to the CSL, *Exposure*, defined as the share of years between ages 6 and 15 that the CSL was in effect. The figure depicts *Exposure* as a function of age when the CSL became effective. The negative sign means being born before the CSL was adopted.

2.3   Data

Sample Construction. The main data source for this study is the China General Social Survey (CGSS). Modeled after the renowned General Social Survey (GSS) in the United States, the CGSS

project aims to track the evolution of Chinese society and has been regularly conducted by Renmin University of China since 2003. Each wave draws a cross-sectional sample of 6,000 to 10,000 individuals from rural and urban areas of 31 provinces in mainland China. The CGSS uses a multi-stage stratified sampling design adapted to the most recent population census.[4] In Appendix I, I compare the demographic characteristics of the CGSS sample with those of the national census, showing that the CGSS is nationally representative.

I assemble six waves of CGSS data collected between 2008 and 2017.[5] To ensure the relevance of the sample for the study, several criteria are considered in sample construction. Firstly, the sample is restricted to rural respondents, as they are the focus of the rural elections examined in this study. Secondly, only individuals between the ages of 25 and 55 at the time of the survey are included. This group mostly has completed their education and is not too old to be inactive, allowing for accurate measurement of educational attainment and proper definition of constituents of a village's election. With these restrictions applied and excluding observations with missing values, the final sample comprises 16,145 respondents from 30 provinces (with Tibet excluded).

Variable Definition. Subsequent analysis includes the following key variables.

1. Turnout. I measure turnout in rural elections using the following question:

"Did you vote in the most recent village committee election? Yes/No."

I define a dummy variable, *Turnout*, as one if a person has voted in the most recent election and zero otherwise.

It is worth noting that the variable of interest, turnout, is self-reported. In Western democracies, it is well known that many people, especially more educated people, tend to over-report their turnout in surveys due to social desirability. People consider voting socially desirable and wish to appear engaged even if they did not vote (e.g., Silver et al., 1986; Bernstein et al., 2001). When it comes to autocratic states like China, people may also over-report turnout due to a different mechanism of social desirability: voting in autocracies is desired not because of democratic values of civic engagement but due to reverence for the state (e.g., Reuter, 2021). However, some scholars suggest that for Chinese villagers, turnout in rural elections is not tied to political loyalty, so they are free to abstain (Burns, 1988). This should reduce over-reporting incentives associated with social desirability. In addition, in the sample, it does not appear that people overwhelmingly report that they have voted: the mean of *Turnout* is 49.4%.[6] As such, the survey responses from the CGSS can still provide some useful variation to explore.

2. Educational Attainment. The treatment of interest in this study is education. The CGSS records education completion as whether a person has completed literacy class, primary school, middle

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[4]For instance, the CGSS 2005 and 2008 use the 1 percent population census in 2005 as the sampling frame.

[5]These surveys were conducted in 2008, 2010, 2012, 2013, 2015, and 2017.

[6]An ideal way to determine the size of misreporting is to compare sample turnout rates to official statistics. However, official statistics are not available to the best of my knowledge.

school, high school, junior college, college, or graduate school. I convert education completion to years of schooling so that the estimates would be easier to interpret and comparable to existing literature.[7]

Furthermore, Marshall (2016a) suggests that it may be preferable to code education as a continuous variable (e.g., years of schooling) instead of a binary variable (e.g., middle school completion) in settings that use education reforms to instrument for education. This is because such reforms may affect all margins of educational attainment, but binary coding overlooks this fact and only considers a single margin, which can lead to biased estimates.[8] Nonetheless, I demonstrate the robustness of my results to alternative measures of educational attainment.[9]

3. Exposure to the CSL. To operationalize the proposed measure of exposure to the CSL as shown in Figure 2, I need to calculate a person's age when the CSL was adopted. To do so, I use two pieces of information: (i) one's birth year, and (ii) the province where one lives as of the survey, which links to the CSL's effective year as illustrated in Figure 1. Ideally, I would like to use the province where one received compulsory education, i.e., where she lived between ages 6 and 15. However, the CGSS does not provide such information, so I use the current residence province as a proxy.

4. Covariates. I collect other variables from the CGSS, such as gender, ethnicity, parents' educational attainment, and parents' memberships of the Communist Party of China (CPC). These variables are likely to influence an individual's educational attainment and political attitudes; hence, I include them in the regression analysis as controls. Note that all the variables are not outcomes of a person's education, avoiding the "bad control problem" (Angrist and Pischke, 2009).

Furthermore, my analysis investigates other outcomes besides turnout and uses additional variables to study heterogeneity. I will introduce them when they become pertinent.

2.4 Summary Statistics

Table 1 presents summary statistics of the main variables, separately for the full sample, those exposed to the CSL ($Exposure > 0$), and those unexposed to the CSL ($Exposure = 0$).

Panel A presents personal characteristics. As expected, the exposed group is younger than the unexposed group. Notably, there are significant differences between the exposed and unexposed groups regarding family backgrounds, such as parental education and CPC membership.

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[7]The conversion is as follows: illiterate = 0, literacy class = 3, primary school = 6, middle school = 9, high school = 12, junior college = 15, college = 16, and graduate school = 18.

[8]Intuitively, if education is coded as a binary variable, such as middle school completion, and is instrumented by the CSL, the exclusion restriction requires that the CSL affects turnout *only* through middle school completion. However, this assumption may be overly strong, as the CSL may improve other margins of education besides middle school, albeit with varying intensities (as shown in Figure 4), and these margins of education may also influence turnout. Therefore, binary coding may lead to violations of the exclusion restriction and biased estimates.

[9]In Table A4, I show the results using the completion of middle school or above, high school or above, and college or above as alternative measures of education. All echo the finding discussed later that education reduces turnout in rural elections.

9

Although these differences may be due to cohort heterogeneity, they may likely relate to unobserved factors and confound the CSL's effects. Therefore, I control for these variables in the regressions. Additionally, I conduct several tests to ensure that my results are not driven by omitted variable bias (see Section 4.2).

Table 1. Summary Statistics

	Full Sample (1) N = 16145	Exposed (2) N = 7114	Unexposed (3) N = 9031	Diff. (2)-(3) (4)
Panel A: Personal Characteristics				
Age	41.526	33.728	47.668	-13.940***
	[8.502]	[5.209]	[4.713]	(0.314)
Female	0.512	0.518	0.508	0.010
	[0.500]	[0.500]	[0.500]	(0.008)
Han Chinese	0.901	0.908	0.896	0.011
	[0.298]	[0.290]	[0.305]	(0.012)
Father schooling	5.575	7.009	4.445	2.564***
	[4.666]	[4.326]	[4.613]	(0.124)
Mother schooling	3.675	5.165	2.501	2.664***
	[4.423]	[4.479]	[4.006]	(0.135)
Father CPC member	0.152	0.154	0.151	0.003
	[0.359]	[0.361]	[0.358]	(0.006)
Mother CPC member	0.026	0.031	0.022	0.009***
	[0.158]	[0.172]	[0.146]	(0.003)
Panel B: Education and Turnout				
Schooling	9.264	10.462	8.320	2.142***
	[4.096]	[3.925]	[3.979]	(0.142)
Turnout	0.494	0.408	0.561	-0.153***
	[0.500]	[0.492]	[0.496]	(0.012)

Note: Columns (1)–(3) report the variables' means and standard deviations (in brackets) in corresponding (sub)samples. Column (4) reports the differences between Columns (2) and (3) and their standard errors clustered at the province-by-birth-year level (in parentheses).
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Panel B presents the two key variables: education and turnout. The education gap between the exposed and unexposed groups is noticeable. On average, those exposed to the CSL have 9.264 years of schooling, while the average schooling for those unexposed is 8.320 years, indicating the CSL's effectiveness in improving educational attainment. Regarding turnout in rural elections, the gap between the exposed and unexposed groups is equally significant, with the exposed being 15.3 percentage points less likely to vote than the unexposed. These observations suggest a negative relationship between education and turnout. Figure 3 depicts a steep education gradient in turnout, further demonstrating that turnout is negatively related to educational attainment: the turnout rate dropped by 28 percentage points among college-educated people compared to those with the lowest education level (below primary school). These patterns motivate a more thorough analysis.
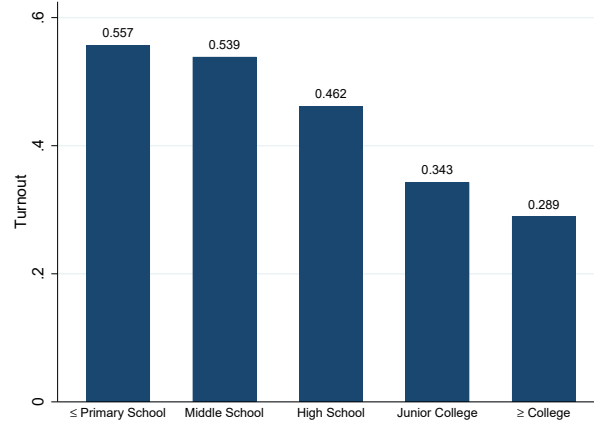
Figure 3. Turnout Rates by Education Level

Note: This figure depicts the education gradient in turnout. Individuals in the sample are grouped according to their educational attainment: below primary school, middle school, high school, and above college. Each bar is the turnout rate of a group. The number above each bar is the turnout rate.

3   Research Design

Specification(s). The primary challenge in identifying the causal effect of education on turnout is the endogeneity of educational attainment. As Section 2.2 mentions, education may correlate with many unobserved traits influencing turnout. To address this endogeneity issue, my research design uses exposure to the CSL as an instrument for educational attainment and examines the effects of CSL-induced changes in education on turnout. Formally, I estimate the following equations using two-stage least squares (2SLS):

$$Schooling_i = \alpha_0 + \alpha \cdot Exposure_{bp} + X_i'\delta + \lambda_b \times \phi_t + \mu_p \times \phi_t + \nu_i, \tag{1}$$

$$Turnout_i = \beta_0 + \beta \cdot Schooling_i + X_i'\rho + \lambda_b \times \phi_t + \mu_p \times \phi_t + \varepsilon_i. \tag{2}$$

Here, i indexes individuals, b indexes birth cohorts, p indexes provinces, and t indexes survey years. $Schooling_i$ denotes individual i's years of schooling, while $Exposure_{bp}$ is exposure to the CSL, depending on one's birth cohort b and residence province p. $Turnout_i$ is a dummy variable that equals one if individual i has voted in the most recent rural election. X_i is a set of control variables, including gender, ethnicity, parental schooling, and parental CPC membership. λ_b is the cohort fixed effect binned by five years.[10] μ_p is the province fixed effect. Both λ_b and μ_p are interacted with the survey year fixed effect ϕ_t to allow for heterogeneity across surveys. ν_i and ε_i are the error terms. Standard errors are clustered at the province-by-birth-year level — same as the

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[10]Unrestrictive cohort fixed effects largely reduce statistical power because there is not much variation in the timing of the CSL across provinces (all provinces in the sample adopted the CSL within six years). Instead, I use the five-year binned cohort fixed effects to control for cohort heterogeneity. I conduct various exercises to ensure my results are not driven by omitted variable bias (to be discussed in Section 4.2).

level of variation that the instrumentation leverages — to account for common shocks experienced by people in the same cohort and province.

I also estimate the following reduced-form regression to study the CSL's effect on turnout:

$$Turnout_i = \theta_0 + \theta \cdot Exposure_{bp} + X_i'\xi + \lambda_b \times \phi_t + \mu_p \times \phi_t + u_i. \tag{3}$$

Identification. For the 2SLS estimate of β to be valid, the instrument, exposure to the CSL, must strongly predict schooling. More importantly, the exclusion restriction should be met: exposure to the CSL affects turnout *only* through education. This assumption would be violated if exposure to the CSL picks up unobservables correlated with turnout. I deal with this issue in several ways. First, the fixed effects included help control for confounding factors. Table A2 shows that the differences in observed characteristics in Table 1 are largely eliminated when conditioning on fixed effects. Second, I shed light on the exclusion restriction's plausibility by estimating event-study specifications of the first-stage and reduced-form regressions, which allow me to examine pretrends in education and turnout (see Section 4.1). Third, I conduct several robustness checks that purge omitted variable bias (see Section 4.3). Furthermore, I implement a sensitivity test developed by Conley et al. (2012) to assess the robustness of 2SLS estimates to violations of the exclusion restriction.

4 Results

This section presents the results of this paper. Section 4.1 reports the basic results. Section 4.2 discusses several robustness checks. Section 4.3 provides some extensions that help explain the effects of education on turnout in China's rural elections.

4.1 Basic Results

4.1.1 Effects of the CSL on Education

Table 2 displays the CSL's effects on education, i.e., estimates of α in the first-stage regression (Equation 1) with some variants. Column (1) is a minimum specification that only includes fixed effects. Column (2) includes covariates, and Column (3), which is the preferred specification, further interacts covariates with cohort-by-year fixed effects ($\lambda_b \times \phi_t$) to allow for differential impacts of covariates across cohorts. All estimates imply that the CSL significantly improves educational attainment. More concretely, the most conservative estimate in Column (3) indicates that, on average, people fully exposed to the CSL would have 1.063 years more schooling than those unexposed to the CSL, *ceteris paribus*. This effect amounts to 11.5% of the sample mean years of schooling (9.264) and accounts for about 49.6% of the gap in schooling between people exposed to the CSL and not (2.142), which demonstrates the CSL's power in improving education.

In Table A3, I find that the CSL enhances educational attainment across different groups, and expectedly, the CSL has larger effects for those with traditionally disadvantaged backgrounds (e.g., females, individuals with less educated parents, and individuals from underdeveloped areas). These patterns ascertain that the CSL has played a causal role in improvements in educational attainment. They also support the IV monotonicity that exposure to the CSL should universally increase educational attainment, though with varying degrees; thus, the 2SLS estimate later can be interpreted as a local average treatment effect (LATE, Imbens and Angrist, 1994), which represents the average effect of education on turnout among compliers of the CSL (i.e., individuals whose schooling increases in the wake of the CSL).

Table 2. Effects of the CSL on Education

	(1) Schooling	(2) Schooling	(3) Schooling
Dependent mean = 9.264			
Exposure	1.367***	1.160***	1.063***
	(0.237)	(0.226)	(0.235)
Cohort-by-year FE	Y	Y	Y
Province-by-year FE	Y	Y	Y
Covariates		Y	
Inteacted covariates			Y
Obs.	16145	16145	16145

Note: The dependent variable is years of schooling. All regressions include noted controls. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

In Figure 4, I examine the effects of the CSL on different levels of educational attainment. The figure reveals that the CSL has a positive effect on all levels of educational attainment, with a greater impact on middle school completion, which matches the CSL's design that mandates middle school completion (see Section 2.2). Similar heterogeneity has also been identified by Huang (2015) and Du et al. (2021). This implies that some individuals who have completed middle school because of the CSL also continue to pursue higher levels of education. Using years of schooling as a treatment incorporates the CSL's extensive impacts.

To further examine the effects of the CSL on education, I estimate an event-study specification adapted from Equation 1. The results are visualized in Figure 5A. The CSL significantly increases the schooling of exposed individuals. In contrast, there are no strong effects on the schooling of those not exposed to the CSL. However, I note slight upward pretrends among people aged 19 or older when the CSL was adopted. I remain agnostic about the factors driving these pretrends but implement the sensitivity test proposed by Rambachan and Roth (2022) to assess the robustness of event-study results. Figure A4 shows that I can reject the null that the CSL did not affect schooling even if presuming the pretrends linearly persisted to cohorts exposed to the CSL. In Section 4.2, I conduct additional checks to alleviate concerns that the pretrends could reflect confounders.
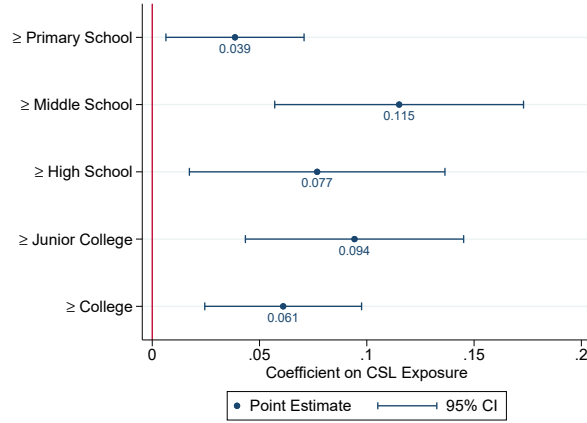
Figure 4. Effects of the CSL on Different Levels of Education

Note: This figure presents the CSL's effects on different education levels. I estimate Equation 1, replacing the dependent variable to be the dummy variable for (i) completing primary school or above, (ii) completing middle school or above, (iii) completing high school or above, or (iv) completing college or above. Both OLS and BJS estimates are derived. Standard errors are clustered at the province-by-birth-year level.
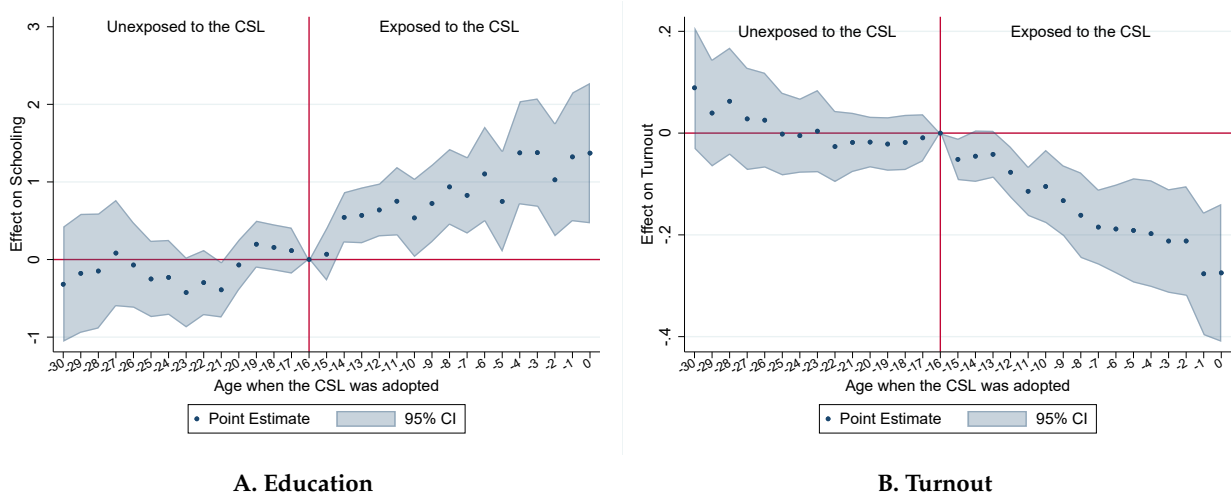

A. Education　　　　　　　　　　　**B. Turnout**

Figure 5. Dynamic Effects of the CSL on Education and Turnout

Note: This figure displays the dynamic effects of exposure to the CSL on education and turnout, using event-study models adapted from Equation 1 and Equation 3, respectively. The solid dots are point estimates, and the caps are 95 percent confidence intervals. Robust standard errors are clustered at the province-by-birth-year level.

4.1.2 Effects of Education on Voter Turnout

In this section, I turn to examine the effects of education on turnout, exploiting the CSL-induced variation in schooling. I first examine the reduced-form relationship between the CSL and voter turnout in rural elections, captured by the estimate of π from Equation 3. Columns (1)–(3) of Table 3 present the results, which show that, all else equal, individuals fully exposed to the CSL are 14.3–14.7 percentage points less likely to vote in rural elections compared to those who were not exposed. To investigate the CSL's dynamic effects on voter turnout, I estimate an event study

model, and Figure 5B displays the estimates. There are no discernible pretrends in voter turnout among those who were not exposed to the CSL, whereas voter turnout drops significantly among those who were exposed.[11]

Table 3. Effects of Education on Turnout in Rural Elections

	Full Sample						Stayers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout	Turnout
Exposure	-0.147***	-0.143***	-0.147***				-0.113***	
	(0.033)	(0.033)	(0.034)				(0.039)	
Schooling				-0.108***	-0.123***	-0.138***		-0.092**
				(0.031)	(0.037)	(0.042)		(0.036)
DV mean	0.494	0.494	0.494	0.494	0.494	0.494	0.551	0.551
F stat.				33.280	26.399	20.407		18.505
tF 95% CI				[-0.177, -0.038]	[-0.211, -0.036]	[-0.243, -0.034]		[-0.183, -0.001]
Cohort-by-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province-by-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates		Y				Y		
Inteacted covariates			Y	Y			Y	Y
Obs.	16145	16145	16145	16145	16145	16145	12861	12861

Note: The dependent variable is the turnout dummy. Columns (1)–(6) use the full sample, while Columns (7) and (8) use the sample of individuals who stay in their *hukou* townships. All regressions include noted controls. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses. *tF* confidence intervals are computed using the methodology developed by Lee et al. (2022).
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

These results suggest that more education, induced by the CSL, reduces turnout in rural elections. In Table 3, Columns (4)–(6) present 2SLS estimates of β from Equation 2, which quantify the marginal effect of education on turnout. The 2SLS estimates show a significant negative relationship between schooling and turnout. The results are robust to alternative *tF* inference for IV proposed by Lee et al. (2022) (see *tF* confidence intervals at the bottom of Table 3).[12]

The effect of education on turnout is sizeable. Specifically, the estimate in Column (6) implies that a one-year increase in schooling lowers the probability of voting by 13.8 percentage points, *ceteris paribus*. Such a large effect can be due to a combination of the steep education gradient in turnout (Figure 3) and the CSL's extensive effects such that people continue to obtain higher levels of education (Figure 4). This marginal effect amounts to 27.9% of the sample mean turnout rate (49.4%). The effect size is also comparable to the impact of parents' political background. With at least one parent being a CPC member, an individual is 10.8 percentage points more likely to vote, potentially reflecting party discipline or mobilization. However, this effect could be easily offset by a one-year increase in schooling.[13]

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[11]I also conduct the sensitivity test proposed by Rambachan and Roth (2022) for this event study, which shows that the CSL's effects on voter turnout are robust to a linear extrapolation of pretrends (see Figure A4).

[12]Lee et al. (2022) show that in some cases, the first-stage F statistic needs to be at least 142.6 to maintain the t-ratio test using conventional critical values. Thus, they propose an alternative tF test that adjusts critical values according to the first-stage F statistic.

[13]One needs to interpret the effect of CPC membership with caution since it is due to people's self-selection. Were CPC members the people who are inherently more likely to vote, the negative effect of education on turnout would be even more remarkable.

The effect I find in China is larger than what Croke et al. (2016) find in Zimbabwe. Croke et al. (2016) find that a one-year increase in schooling reduces turnout in *national* elections by 3–6 percentage points. Their interpretation is that educated citizens are democratic-minded and disengage in elections to avoid legitimizing the autocrat. The deliberate disengagement story may not be at play in the Chinese context since China's rural elections are just instrumental for *local* governance and have limited implications for regime legitimacy. One possible explanation for the large effect in China is that education increases the opportunity cost of voting (in low-stake local elections), which people are more responsive to. I elaborate this more in Section 4.2.

Columns (7) and (8) in Table 3 replicate reduced-form and 2SLS estimates using the sample of individuals who have stayed in their *hukou* townships as of the survey ("stayers"). They show that education reduces turnout despite a smaller effect size. However, these results should be taken cautiously since migration is an endogenous outcome of education.[14] Nonetheless, they help rule out some alternative interpretations of my findings. First, the negative link between education and turnout exists even among the local constituents, so my findings are not simply mechanical due to a combination of education-induced migration and institutional barriers that most people can only vote in *hukou* villages.[15] Second, these results suggest that my findings are not due to the measurement error due to using residence provinces to proxy for the provinces where people received compulsory schooling.

4.2 Robustness Checks

Thus far, the results imply that education possibly causes lower turnout in China's rural elections. This subsection provides several robustness checks for the findings.

Controlling for Confounders. The most severe threat is that exposure to the CSL may have picked up factors that drive education and turnout, thus contaminating the results. I conduct the following exercises to purge confounders further.

1. Province-Specific Trends. One concern is that exposure to the CSL correlates with differential socioeconomic conditions across cohorts (e.g., China's college expansion in 2000, Che and Zhang, 2018). To address this concern, I include province-specific birth-year trends ($\mu_p \times b$). Columns (1) and (2) report the reduced-form (RF) and 2SLS estimates with the inclusion of province-specific trends, confirming that education reduces turnout. However, the first-stage F statistic is below the conventional cutoff for 2SLS estimation (Staiger and Stock, 1997). To address weak-instrument issues, I implement an Anderson-Rubin test for the 2SLS estimate and reject the null of a zero effect at the 5% level, with a 95% confidence interval of [-0.538, -0.082].

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[14]The 2SLS estimate suggests that one-year more schooling increases the probability of (cross-township) migration by 7.68 percentage points (*p*-value = 0.015).

[15]Tellingly, in my sample, a stayer is on average twice as likely to vote as a migrant: sample turnout rate 55.1% (stayers) versus 27.0% (migrants).

2. Cohort Bandwidth. I restrict the sample to a narrow bandwidth of cohorts born around the time when the CSL was adopted. These individuals are presumably similar apart from exposure to the CSL. Specifically, for unexposed individuals, I exclude those aged 19 or older when the CSL was adopted, whose schooling has exhibited some trending (see Figure 5A). For exposed individuals, I include those between ages 6 and 15 when the CSL was adopted, covering all possible exposure intensities. Therefore, the resulting sample includes individuals within the interval [-19, -6] in Figure 2. In Table 4, Columns (3) and (4) display estimates using this sample, which confirms education's negative effect on turnout.

3. "CSL-Strong Sample". In this exercise, I focus on a "CSL-strong sample", where the CSL had a particularly salient impact on educational attainment, so influences of unobserved factors should have been relatively minor. I use a data-driven method to select the "CSL-strong sample" (details are discussed in Appendix III). To illustrate, consider Figure 2. In an ideal experiment, the CSL is expected to create a distinct break in the schooling trend for those exposed to it, if no other factors affect schooling. Based on this idea, I select fifteen provinces with the most prominent trend breaks in their schooling (birth cohort) trends. In Table 4, Columns (5) and (6) display estimates using the "CSL-strong sample". Reassuringly, we see that more education leads to lower turnout.

4. Matching. Using a matching approach, I create a sample where each CSL-exposed individual is matched with another individual from the same province, born in similar cohorts, and with comparable observable characteristics, *but* having weaker exposure to the CSL (see Appendix IV for details). This method ensures that each pair of individuals differ only in their levels of CSL exposure, provided that observable characteristics are informative about unobserved characteristics. Using this matched sample, I estimate reduced-form and 2SLS models that include *pair fixed effects* to exploit only within-pair variations. The results are presented in Table 4's final two columns, revealing a clear negative relationship between education and turnout.

5. Sensitivity Test. Uncontrolled confounders can lead to violations of the exclusion restriction, but it is challenging to find proxies for all possible confounding factors. To examine the sensitivity of my 2SLS estimate to violations of the exclusion restriction, I use the methodology developed by Conley et al. (2012). This approach allows the instrument, exposure to the CSL, to enter the second stage of the model (Equation 2) with a coefficient of γ, which measures the extent to which the exclusion restriction is violated and is set by the researcher. I test whether instrumented schooling significantly affects voter turnout for different values of γ. Since I find schooling has a negative effect, violations of the exclusion restriction are problematic only when γ is negative. Therefore, I calculate the largest negative value of γ such that the resultant 2SLS estimate is still significant at the 5% level. This value is denoted by $\bar{\gamma}$ and it is scaled by the exposure to the CSL's reduced-form effect on voter turnout, α_1. The ratio $\bar{\gamma}/\alpha_1$ represents the maximum hypothetical violation of the exclusion restriction that can be allowed while the 2SLS estimate is still statistically significant. This

Table 4. Robustness Checks

	Include Provincial Trends		Bandwidth [-19, -6]		CSL-Strong Sample		Matching	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Exposure	-0.150***		-0.210***		-0.130**		-0.222***	
	(0.042)		(0.038)		(0.053)		(0.077)	
Schooling		-0.181**		-0.157***		-0.087**		-0.119**
		(0.079)		(0.041)		(0.038)		(0.049)
DV mean	0.494	0.494	0.480	0.480	0.476	0.476	0.472	0.472
F stat.		8.803		24.749		19.857		14.875
Obs.	16145	16145	7249	7249	9826	9826	6656	6656

Note: The dependent variable is the turnout dummy. Both reduced-form (RF) and 2SLS estimates are reported. All regressions include cohort-by-year and province-by-year fixed effects as well as covariates interacted with cohort-by-year fixed effects. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Columns (1) and (2) use the full sample and include province-specific birth year trends in regressions. Columns (3) and (4) restrict the sample to people born 6 to 19 years before the adoption of the CSL. Columns (5) and (6) use the CSL-strong sample, where the CSL brought a significant increase in schooling. Columns (7) and (8) use a matching sample in which each CSL-exposed individual is paired with another who is observationally similar but less exposed to the CSL; pair fixed effects are included in regressions. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

exercise yields a ratio of 0.447, indicating that my findings are robust to substantial violations of the exclusion restriction.[16]

Alternative Estimator. Recent econometric literature has pointed out that the OLS estimator of fixed effects models may be biased for causal parameters of usual interest (e.g., average treatment effect) and results in misleading interpretations, because it aggregates heterogeneous treatment effects using insensible weights (see de Chaisemartin and D'Haultfoeuille (2022) and Roth et al. (2022) for review). Therefore, as a robustness check, I use the robust estimator proposed by Borusyak et al. (2021) (BJS) to re-estimate the first-stage and reduced-form regressions (Equation 1 and Equation 3), as well as their event-study specifications. In Appendix II, I show that BJS and OLS estimates are similar.

Standard Errors. So far, I have clustered standard errors at the province-by-birth-year level. In Table A5, I show that my results are robust to using alternative clustering of standard errors, including clustering at the provincial level and two-way clustering at both province-by-birth-year and province-by-survey-year levels, which allow for correlations of error terms in other dimensions. In addition, statistical inferences appear to be most conservative when clustering standard errors at the province-by-birth-year level, as in the main results.

Other Checks. In Table A6, I present several additional checks. First, I exclude four centrally administered municipalities (Beijing, Shanghai, Tianjin, and Chongqing), and the results do not change. Second, my findings are robust to excluding minority autonomous regions (Xinjiang, Ningxia, Inner Mongolia, Guangxi). Finally, I show robustness when conducting analysis separately

[16]Conley et al. (2012) does not provide a rule-of-thumb cutoff for γ/α_1. However, using Conley et al. (2012)'s approach, researchers have demonstrated the robustness of their 2SLS estimates given the following $\bar{\gamma}/\alpha_1$ ratios: 0.3 in Fatás and Mihov (2013) and 0.46 in Bentzen et al. (2017).

for years under different national leaders, i.e., Hu Jintao (before 2013) and Xi Jinping (after 2013). These results confirm that my findings are not driven by particular political environments due to regions or times.

4.3 Extensions

4.3.1 Potential Explanations

Why does more education make people less likely to vote? One potential explanation is that more educated people have higher opportunity costs of voting, which overwhelm potential benefits from participation (Palfrey and Rosenthal, 1985). As the anecdotal evidence in Section 2.1 suggests, villagers' participation decisions involve careful cost-and-benefit calculations.

To shed light on the opportunity cost story, I examine how the effect of education on turnout varies with the opportunity cost of voting. First, I use returns to education as a proxy for opportunity costs. When returns to education are high, it would be more profitable for more educated people to devote time to work efforts rather than voting. To measure returns to education, I run a regression of log income on schooling with rich controls for each province in my sample, and then I employ the coefficient on schooling to measure returns to education. Next, I divide the sample into high-return provinces (above median) and low-return provinces (below median). Columns (1) and (2) in Table 5 display the respective effects of education on turnout in the two groups of provinces. In line with the opportunity cost story, education has a more negative effect on turnout in provinces with higher returns to education, although the effects are not statistically distinguishable between the two groups of provinces. Second, I use non-agricultural employment as a second proxy for the opportunity cost of voting. Non-agricultural sectors compensate for education better than the agricultural sector, thus increasing the costs of time spent on voting. Tellingly, Columns (3) and (4) in Table 5 show that education reduces turnout more when people are employed in non-agricultural sectors. These results have the same spirit as the findings of Campante and Chor (2012), which document that the effect of education on participation negatively relates to a country's skill premium and individual employment in skilled occupations.

Prior research emphasizes the role of social capital in political participation, as high social capital could increase individuals' willingness to bear the costs of participation (e.g., Nannicini et al., 2013). Following previous literature, I measure social capital using a CGSS question that asks whether respondents are generally trustful or distrustful of other people (Tabellini, 2010). Notably, Columns (5) and (6) in Table 5 show that the negative effect of education effect on turnout is primarily driven by individuals who have low trust in other people and so are potentially more sensitive to high opportunity costs.

I also probe into the role of ideology. Existing literature suggests that education is a critical factor in shaping ideology (Lott, 1999; Bandiera et al., 2019; Alesina et al., 2021; Cantoni et al., 2017), which in turn would affect political behavior. Recall that Croke et al. (2016) find that in Zimbabwe,

Table 5. Potential Explanation: Opportunity Costs of Voting

	Returns to Education		Occupation		Social Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
	High	Low	Non-Ag.	Ag.	Trustful	Distrustful
Panel A: 2SLS						
Schooling	-0.151***	-0.089*	-0.150***	-0.083	-0.066	-0.218**
	(0.046)	(0.050)	(0.048)	(0.073)	(0.043)	(0.087)
F stat.	18.593	10.016	18.661	5.920	12.604	9.670
Equality test p-value		0.363		0.468		0.134
Panel B: Reduced-Form						
Exposure	-0.210***	-0.102**	-0.202***	-0.075	-0.083*	-0.241***
	(0.044)	(0.051)	(0.049)	(0.056)	(0.050)	(0.057)
DV mean	0.494	0.493	0.413	0.574	0.519	0.452
Equality test p-value		0.109		0.102		0.036
Obs.	7600	8545	8016	8129	8895	5843

Note: The dependent variable is the turnout dummy. Panel A reports 2SLS estimates and Panel B reports reduced-form estimates. Columns (1) and (2) compare individuals in provinces with high returns to education versus those in provinces with low returns to education (above or below the sample median). Columns (3) and (4) compare individuals employed in the non-agricultural sector versus those in the agricultural sector. Columns (5) and (6) compare individuals who are trustful of other people versus those who are distrustful of other people. All regressions include cohort-by-year and province-by-year fixed effects and covariates interacted with cohort-by-year fixed effects. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

despite its autocratic system, education fosters liberal attitudes and thus makes people deliberately disengage in unfair elections to protest the autocrat. In light of their deliberate disengagement theory, I examine the effects of education on political attitudes in China. Table 6 displays the results. The first two columns show that education has little effect on the likelihood of joining the CPC, which is associated with exposure to party indoctrination. The remaining columns examine liberal attitudes. The CGSS includes questions on whether one believes the government should not intervene in (i) open criticism of the government (freedom of criticism), (ii) how many children one wants to have (freedom of fertility), and (iii) where one wants to live and work (freedom of migration). All these questions correspond to the Chinese state's interventions in political and social life (i.e., censorship, one-child policy, and *hukou* system). However, Columns (3)–(8) show no significant effects of education on these liberal attitudes. Taken together, these results contrast with the findings of Croke et al. (2016), and suggest that education may not have affected turnout through ideology.

4.3.2 Cross-Province Variation in Education-Turnout Links

This section explores cross-province variations in the link between education and turnout. In particular, I investigate how these variations are explained by the opportunity cost of voting and other factors.

Table 6. Effects of Education on Other Outcomes

	CPC Member		Freedom of Criticism		Freedom of Fertility		Freedom of Migration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Exposure	-0.003		0.066*		0.011		0.014	
	(0.022)		(0.037)		(0.032)		(0.037)	
Schooling		-0.003		0.063		0.011		0.013
		(0.021)		(0.040)		(0.030)		(0.035)
DV mean	0.083	0.083	0.324	0.324	0.253	0.253	0.688	0.688
F stat.		20.407		16.873		18.120		18.768
Obs.	16145	16145	14564	14564	14680	14680	14675	14675

Note: The dependent variable is: CPC membership (Columns (1) and (2)), support for freedom of criticism (Columns (3) and (4)), support for freedom of fertility (Columns (5) and (6)), and support for freedom of migration (Columns (7) and (8)). Both reduced-form (RF) and 2SLS estimates are reported. All regressions include cohort-by-year and province-by-year fixed effects and covariates interacted with cohort-by-year fixed effects. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

I start by estimating the following regression modified from Equation 3:

$$Turnout_i = \theta_0 + \sum_p \theta_p \cdot (Exposure_{bp} \times \mu_p) + (X_i' \cdot \mu_p)\xi + \lambda_b \times \phi_t + \mu_p \times \phi_t + u_i. \quad (4)$$

This yields estimates $\hat{\theta}_p$'s, i.e., the CSL's effects on turnout by province. Figure 6A presents $\hat{\theta}_p$'s, showing large variations across 30 provinces in the sample. Next, I follow Finkelstein et al. (2016)'s approach to investigate factors that explain variations in $\hat{\theta}_p$'s. Specifically, I run the following univariate regressions:

$$\hat{\theta}_p = a + bZ_p + \eta_p. \quad (5)$$

Z_p is a provincial-level factor, so the coefficient b captures its association with the CSL's effects on turnout. η_p is the error term. Due to the small sample size (30 provinces), standard errors are bootstrapped.

Figure 6B presents estimates from Equation 5, which discusses several provincial-level factors. The estimated b's are standardized to reflect the effect of one SD change in Z_p on $\hat{\theta}_p$. First, returns to education are negatively related to $\hat{\theta}_p$'s, consistent with the findings above that education lowers turnout due to high opportunity costs.

The second factor that I examine is Confucianism. As China's traditional political philosophy, Confucianism features a benevolent dictator model that emphasizes obedience to virtuous leaders and thus could discourage political participation; some scholars even cite the Confucian culture as an explanation for China's lasting autocratic history (Huntington, 1991; Acemoglu and Robinson, 2020, 2021). As Confucius put it:

"When the Way [good governance] prevails, commoners do not debate matters of government."

To study Confucianism's influences on the education-turnout link, following previous literature (Kung and Ma, 2014; Alm et al., 2022), I use the density of Confucian temples, obtained from Chen et al. (2020), as a proxy for local intensities of Confucian culture. The second line in Figure 6B shows that education leads to a more pronounced decline in turnout in provinces with stronger Confucianism. The conformity and political apathy implied by Confucianism, which are more accepted by more educated people, may have resulted in these patterns, echoing the cross-country findings of Campante and Chor (2012) that obedience cultures weaken education's (positive) impact on turnout in democracies.

Lastly, I examine two other factors related to election stakes, as high election stakes may moderate participation costs. First, clan densities (Dincecco and Wang, 2021) may capture the degree to which informal institutions substitute rural elections in local governance. To the extent that people within the same clan share similar interests, a higher clan density may relate to more conflicts of interests, thus increasing electoral competitiveness. Figure 6B shows that education's negative effect on turnout is slightly weaker when the clan density is high. Second, people may care about the policy consequences of their votes. I use the lens of the one-child policy (OCP), as Martinez-Bravo et al. (2022) find that rural elections increase the number of OCP exemptions. Using the fine rates for OCP violations (Ebenstein, 2010) to measure statutory enforcement of the OCP, the final line in Figure 6B shows that education reduces turnout more in provinces with stricter statutory enforcement of the OCP. One likely explanation is that educated people have lower fertility rates (Bleakley and Lange, 2009), so they value OCP exemptions under strict statutory enforcement to a lesser degree and are less incentivized to vote.

I assess the explanatory power of the four factors for cross-province variations in education-turnout links, denoted as $\hat{\theta}_p$. For instance, the gap in $\hat{\theta}_p$ between below-median and above-median provinces is -0.187, and the gap in returns to education is 1.037 SD. Assuming the estimated b from Equation 5 is causal, equalizing returns to education across provinces would reduce the gap in $\hat{\theta}_p$ by $\frac{-0.079 \times 1.034}{-0.187} = 0.437$. Therefore, returns to education can account for 43.7% of the cross-province variation in education-turnout relations. Figure 6C illustrates this analysis for all four factors, revealing that returns to education have the highest explanatory power. Additionally, Figure 6D displays a strong linear fit of the relationship between returns to education and education-turnout links ($R^2 = 0.431$).

These findings emphasize that the opportunity cost of voting is a significant factor in shaping the effect of education on turnout. However, other channels are also likely at play, such as Confucianism, which explains 10.2% of cross-province variations in education-turnout links. It is worth noting that some measurements in this analysis are relatively coarse due to data limitations (e.g., the CGSS does not elicit Confucian beliefs), which may underestimate the importance of some channels. Future research could use more comprehensive measurements to explore the determinants of education-turnout links.

A. Reduced-Form Effects by Province

B. Correlates of Effects by Province
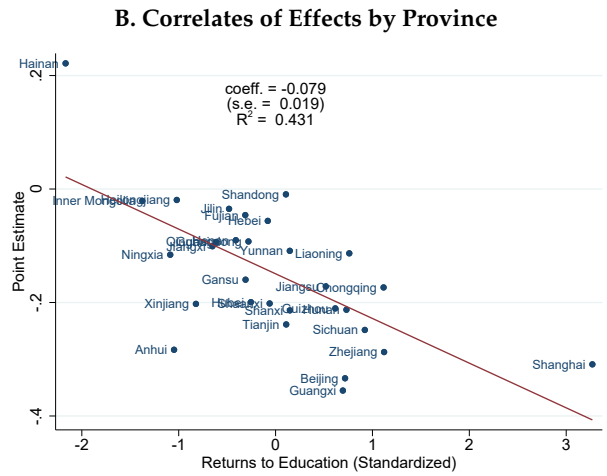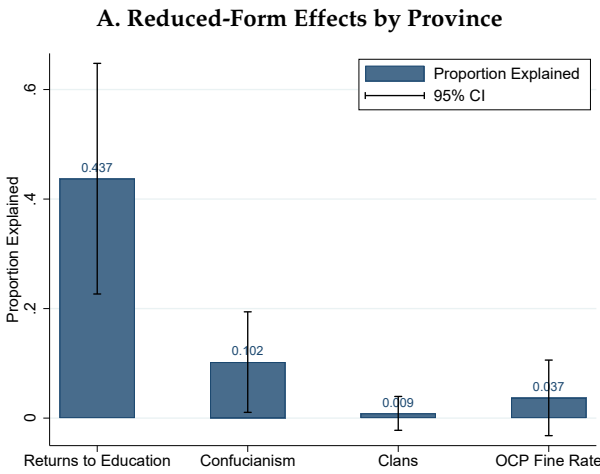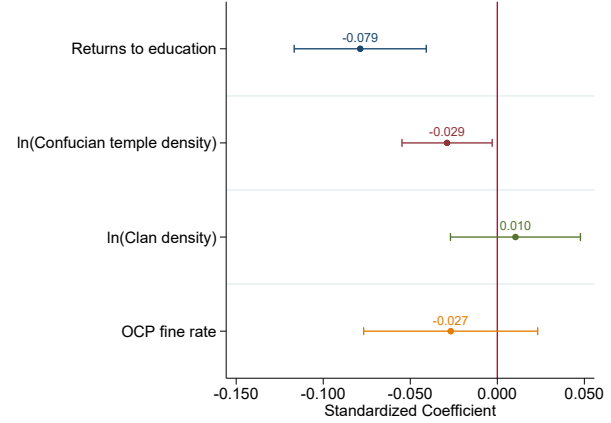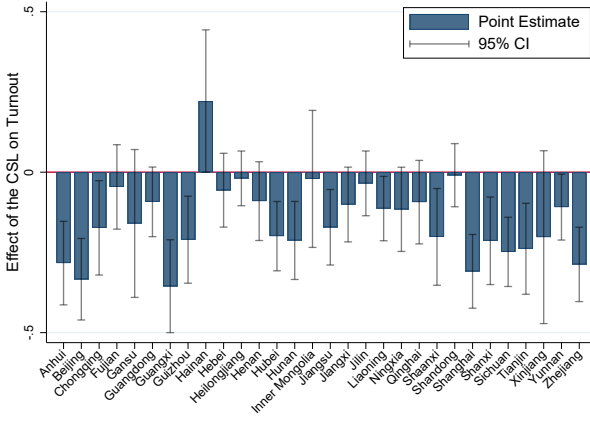
C. Variation in Effects Explained

D. By-Province Effects and Returns to Education

Figure 6. Effects of the CSL on Turnout by Province and Their Correlates

Note: Figure 6A presents the CSL's reduced-form effects on turnout by province (Equation 3 is estimated separately by province). Figure 6B reports results for bivariate regressions of by-province effects on provincial-level factors. Figure 6C displays variations in by-province effects that each provincial-level factor can explain. Figure 6D depicts the relationship between the effects of the CSL on turnout and returns to education.

5   Concluding Remarks

This paper investigates the causal relationship between education and voter turnout in China's rural elections. In contrast to conventional wisdom, I find that education has a strong negative effect on voter turnout, and I provide suggestive evidence that more educated people are less likely to vote because they face higher opportunity costs.

I close this paper by noting three limitations. Firstly, the external validity may be limited due to the uniqueness of China's rural elections, which are highly local and not comparable to elections in democracies or high-profile elections in some autocracies. Secondly, the opportunity cost of voting is an important explanation for the negative effect of education on turnout, but it only explains

about half of cross-province variations in education-turnout links. Future studies could employ novel designs and measures to gauge other explanations. Finally, due to data limitations, this paper only looks at the effect of education on one specific form of political participation in China — voting in rural elections. It could be a promising avenue for future research to investigate the impacts of education affects other forms of political participation in China, such as petitions, protests, and increasing online political participation via social media (King et al., 2013; Qin et al., 2017).

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Online Appendix

(Not for Publication)

I   CGSS versus Census

The sampling scheme of the CGSS follows the most recent population census. For instance, the 2008 CGSS follows the 1% population (mini) census in 2005. To ensure that the sample is nationally representative, I compare demographic and socioeconomic variables between the 2008 CGSS and the 2005 census. In the comparison, I impose restrictions on the census dataset such that it covers the same provinces and cohorts in my 2008 CGSS sample. Table A1 reports the means and standard deviations (in parentheses) of several variables in the 2008 CGSS and the 2005 census, which are similar between the two datasets, though respondents in the CGSS appear to be relatively more educated and less likely to migrate.

Table A1. CGSS versus Census

	(1)	(2)
	CGSS 2008	Census 2005
Birth year	1966.2	1968.0
	(8.064)	(8.460)
Female	0.525	0.496
	(0.500)	(0.500)
Han Chinese	0.900	0.903
	(0.300)	(0.296)
High school	0.131	0.0884
	(0.338)	(0.284)
Working	0.881	0.872
	(0.324)	(0.334)
Staying in hukou township	0.951	0.907
	(0.216)	(0.290)
N	1407	386965

 Note: This table compares demographic and socioeconomic variables between the 2008 CGSS and the 2005 census. Both means and standard deviations (in parentheses) are presented.

II   BJS Estimator

To ascertain that my results are not due to insensible aggregation of heterogeneous treatment effects in OLS estimation, I implement Borusyak et al. (2021)'s (BJS) robust estimator. The BJS estimator allows for calculating the treatment effect for each individual exposed to the CSL, and then one can aggregate these treatment effects using proper weights to recover causal parameters of interest.

Figure A1A and Figure A1B report the event-study results for the CSL's impacts on schooling and turnout, respectively. Note that the "window" used here is narrower than the one used by OLS (cf. Figure 5A and Figure 5B). This is because implementing the BJS estimator will drop some observations for which effects cannot be properly estimated. Reassuringly, Figure A1A and Figure A1B display patterns similar to those using OLS: the CSL improves education, and meanwhile, it reduces turnout; there are also no strong pretrends in education and turnout, lending confidence to the IV strategy.
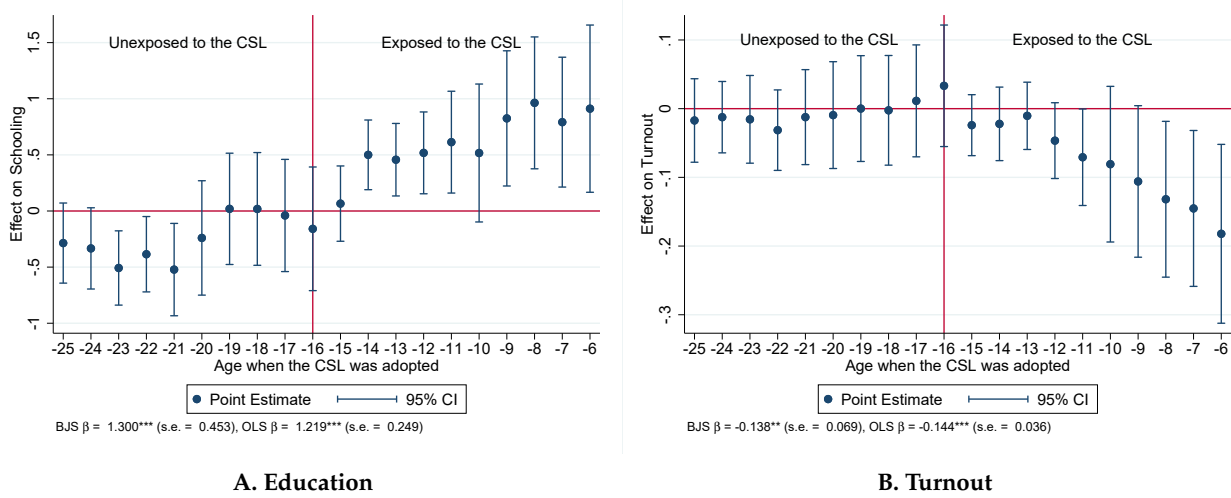

A. Education   **B. Turnout**

Figure A1. BJS Estimates of Effects of the CSL on Education and Turnout

Note: This figure displays the dynamic effects of exposure to the CSL on education and turnout, using event-study models adapted from Equation 1 and Equation 3, respectively. Borusyak et al. (2021)'s estimator is implemented. The solid dots are point estimates, and the caps are 95% confidence intervals. Robust standard errors are clustered at the province-by-birth-year level. At the bottom of Figure A1A (Figure A1B), BJS and OLS estimates of the CLS's effect on schooling (turnout) are displayed.

I also aggregate treatment effects on schooling and turnout to obtain BJS estimates of the first-stage effect (of the CSL on schooling, α in Equation 1) and reduced-form effect (of the CSL on turnout, β in Equation 2). Then, I compare them to the results using OLS. I do this following the instruction of BJS (Section 5.2 "Non-Binary Treatments"), which takes into account the fact that exposure to the CSL, *Exposure*, is non-binary, i.e., individuals are treated with different intensities. As can be seen at the bottom of Figure A1A and Figure A1B, the BJS and OLS estimates are similar, though OLS estimates seem more efficient.

Taken together, my results should not have been driven by aggregation issues in OLS estimation.

III CSL-Strong Sample

I use a data-driven approach to select a "CSL-strong sample", where the CSL brought the most salient improvement in education among those exposed to it, thus, the role of confounders would be relatively minor.

To illustrate, the goal is to pick provinces like Zhejiang in Figure A2 — schooling increases across cohorts, but there is a discernible upward trend break among cohorts born after 1970, i.e., those exposed to the CSL in Zhejiang. To operationalize this idea, I run the following regression using the sample of individuals who were unexposed or partially exposed to the CSL ($0 \leq Exposure < 1$), separately for each province (thus all parameters get subscript p):

$$Schooling_{ip} = \beta_p \left(b \times \mathbb{1}\{Exposure_{bp} > 0\} \right) + \delta_p b + \psi_p \mathbb{1}\{Exposure_{bp} > 0\} \\ + (X_i \times b)' \gamma + \lambda_b \times \phi_t + \varepsilon_{ip}, \tag{A1}$$

where β_p is the parameter of interest, capturing the CSL-induced break in the (linear) schooling trend. The interactions, $X_i \times b$, are also included to avoid inflating the CSL's contributions to trend breaks. I keep 15 provinces, half of the provinces covered by my sample, with the highest t-ratios of estimated β_p's. Consequently, the CSL-strong sample includes Beijing, Tianjin, Liaoning, Jilin, Heilongjiang, Shanghai, Zhejiang, Anhui, Jiangxi, Shandong, Henan, Hubei, Guizhou, Yunnan, and Gansu.
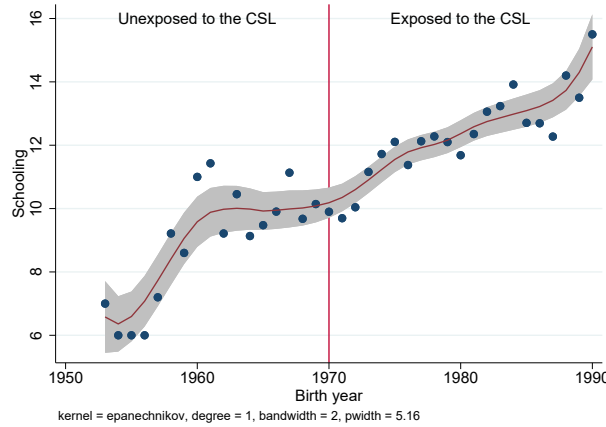

Figure A2. Education Across Cohorts: Zhejiang

IV Matching

I use a matching approach to create a sample in which each CSL-exposed individual is paired with an individual who is observationally similar but less exposed to the CSL. As such, the within-pair comparison would be cleaner, provided that observables represent unobserved characteristics. I conduct matching following the steps below.

First, simulating exposure to the CSL. In the spirit of propensity score matching, I estimate the following model of exposure to the CSL:

$$Exposure_{ibp} = \mu_p + W_i'\Gamma + \varepsilon_{ibp}. \tag{A2}$$

$Exposure_{ibp}$ is exposure to the CSL of individual i of birth cohort b and from province p, ranging from 0 to 1 (as defined in Figure 2). μ_p is the province fixed effect. W_i includes a set of variables that help explain individual i's exposure to the CSL. Because exposure mainly depends on birth cohorts, W_i contains variables that are likely to determine whether one was born earlier or later: indicators of han ethnicity (for which the family planning policy is stricter), gender (related to gender selection), parental CPC memberships (related to family planning enforcement), parental educational attainment (Bleakley and Lange, 2009), and parental birth cohorts. Note that all the variables in W_i are predetermined or exogenous. ε_{ibp} is the error term. Equation A2 exhibits good predictive power: $R^2 = 0.567$ (0.541 if excluding province fixed effects). The fitted value, $\widehat{Exposure}_{ibp}$, is simulated exposure to the CSL.

Second, matching. Each CSL-exposed individual i is paired with a single individual j satisfying the following conditions:

(i) from the same province;
(ii) having similar simulated exposure to the CSL, i.e., $\widehat{Exposure}_{ib_ip}$ and $\widehat{Exposure}_{jb_jp}$ are close;
(iii) born 1–5 years ahead of individual i and thus having *strictly weaker* exposure to the CSL, i.e., $Exposure_{jb_jp} < Exposure_{ib_ip}$.

These conditions would ensure that paired individuals are observationally similar (conditions (i) and (ii)), but differ in *actual* exposure to the CSL because of idiosyncratic reasons such that one was born earlier (condition (iii)). For example, Beijing adopted the CSL in 1986, thereby an individual born in 1971 was 15 in 1986 and thus received a 0.1 exposure level. She would be paired with one from Beijing, having similar simulated exposure, and born between 1966 and 1970 (actual exposure = 0). Matching is performed without replacement.

In Figure A3, I show the goodness of matching. Specifically, the treatment group includes all individuals exposed to the CSL. The pre-matching comparison group in Figure A3A includes individuals who satisfy condition (iii) and thus have weaker exposure to the CSL, while the post-matching comparison group in Figure A3B includes individuals who satisfy all three conditions

(i)–(iii). Tellingly, the pre-matching distributions are distinct between the two groups, while they become very similar after matching (the Kolmogorov–Smirnov test yields a p-value = 0.993).
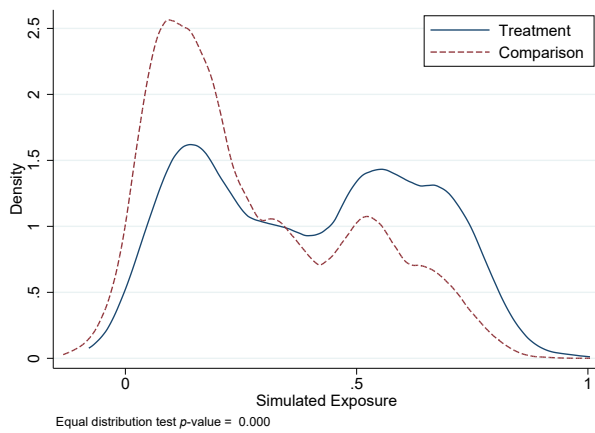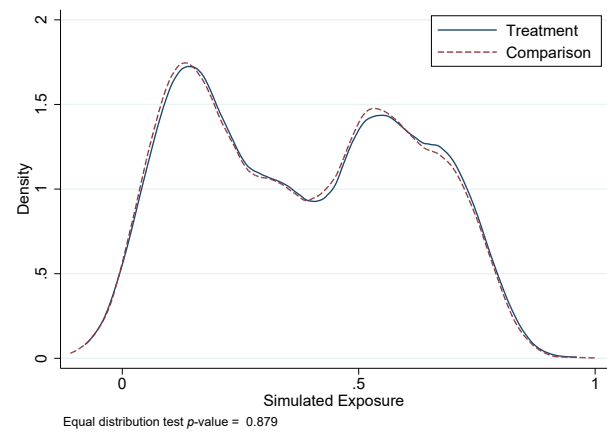
A. Pre-Matching   B. Post-Matching

Figure A3. Simulated Exposure to the CSL: Pre-Matching versus Post-Matching

V Additional Tables

Table A2. Covariate Balances

	(1) Without FEs	(2) With FEs
Female	0.022***	-0.001
	(0.006)	(0.020)
Han Chinese	-0.004	0.009
	(0.008)	(0.009)
Father Schooling	3.660***	0.226
	(0.103)	(0.146)
Mother Schooling	3.632***	0.296**
	(0.113)	(0.148)
Father CPC member	0.044***	0.006
	(0.005)	(0.012)
Mother CPC member	0.013***	0.008
	(0.002)	(0.006)

Note: Column (1) replicates Column (4) of Table 1, showing unconditional differences in covariates between the exposed ($Exposure > 0$) and the unexposed ($Exposure = 0$) cohorts. Column (2) displays the differences conditional on cohort-by-year fixed effects ($\lambda_b \times \phi_t$) and province-by-year fixed effects ($\mu_p \times \phi_t$). Robust standard errors are clustered at the province-by-birth-year level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A3. Effects of the CSL on Education by Subsample

	Female?		Educated Parents?		Developed Provinces?	
	(1)	(2)	(3)	(4)	(5)	(6)
	No	Yes	No	Yes	No	Yes
Exposure	1.359***	0.964***	2.023***	0.857***	1.524***	0.871***
	(0.359)	(0.313)	(0.553)	(0.262)	(0.348)	(0.330)
DV mean	8.581	9.980	7.059	10.364	9.840	8.735
Equality test p-value		0.398		0.059		0.173
Obs.	8273	7872	5372	10773	7736	8376

Note: The dependent variable is years of schooling. All regressions include cohort-by-year and province-by-year fixed effects and covariates interacted with cohort-by-year fixed effects. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A4. Alternative Specifications

	(1) ≥ Middle school	(2) Turnout	(3) ≥ High school	(4) Turnout	(5) ≥ College	(6) Turnout
Exposure	0.097***		0.078**		0.068***	
	(0.033)		(0.031)		(0.019)	
≥ Middle school		-1.521**				
		(0.697)				
≥ High school				-1.898**		
				(0.769)		
≥ College						-2.176***
						(0.716)
DV mean	0.692	0.494	0.351	0.494	0.080	0.494
F stat.		8.395		6.289		12.362
Obs.	16145	16145	16145	16145	16145	16145

Note: The dependent variable is years of schooling. All regressions include cohort-by-year and province-by-year fixed effects and covariates interacted with cohort-by-year fixed effects. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A5. Alternative Standard Errors

	(1) Schooling 1st Stage	(2) Turnout Reduced-Form	(3) Turnout 2nd Stage
Exposure	1.063	-0.147	
	(0.235)***	(0.034)***	
	[0.205]***	[0.041]**	
	{0.213}***	{0.035}***	
Schooling			-0.138
			(0.042)**
			[0.034]***
			{0.039}***
Obs.	16145	16145	16145

Note: This table presents first-stage, reduced-form, and second-stage estimates, using different standard errors to conduct statistical inferences. All regressions include cohort-by-year and province-by-year fixed effects and covariates interacted with cohort-by-year fixed effects. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in *parentheses*. Robust standard errors clustered at the province level are reported in *brackets*; *p*-values are computed through a wild bootstrap-*t* procedure following Cameron et al. (2008), due to the small number of clusters (30). Robust standard errors clustered at both province-by-birth-year and province-by-survey-year levels are reported in *braces*.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A6. Other Checks

	Aut. Regions Dropped		DCM Dropped		Before 2013		After 2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RF	2SLS	RF	2SLS	RF	2SLS	RF	2SLS
Exposure	-0.129***		-0.170***		-0.119**		-0.173***	
	(0.035)		(0.038)		(0.048)		(0.049)	
Schooling		-0.133***		-0.159***		-0.119**		-0.154**
		(0.048)		(0.049)		(0.055)		(0.061)
DV mean	0.502	0.502	0.494	0.494	0.495	0.495	0.492	0.492
F stat.	14135	14135	15042	15042	7974	7974	8171	8171

 Note: The dependent variable is years of schooling. All regressions include cohort-by-year and province-by-year fixed effects and covariates interacted with cohort-by-year fixed effects. Covariates include indicators of gender, han ethnicity, parental educational attainment, and parental CPC memberships. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
 * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

VI Additional Figures
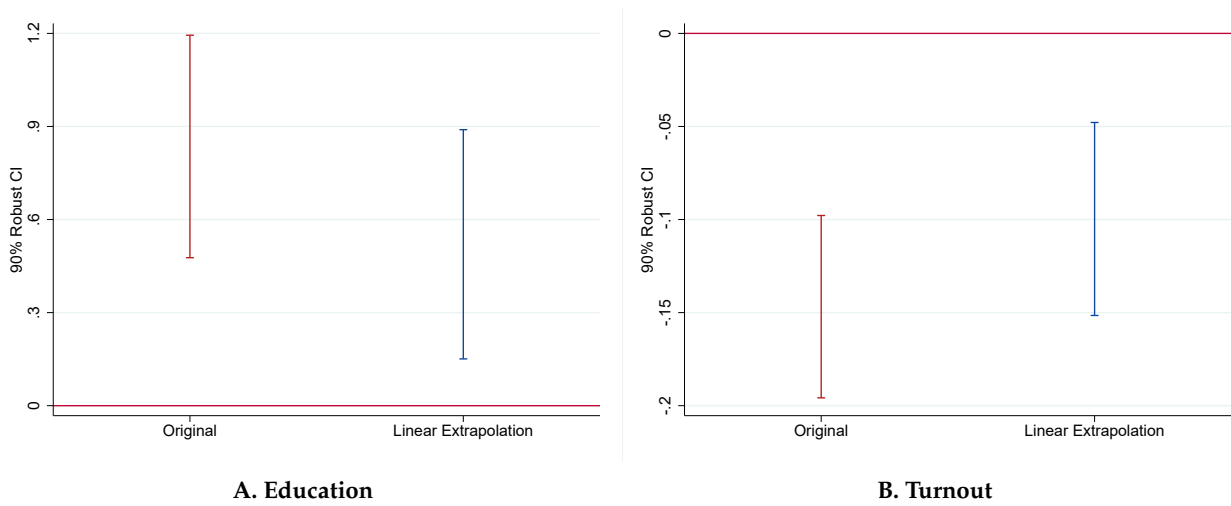

A. Education   B. Turnout

Figure A4. Sensitivity Test for Event Studies

Note: This figure presents the results of the sensitivity test for event studies, proposed by Rambachan and Roth (2022). Figure A4A (Figure A4B) is for the event study of education (turnout). It assesses whether a weighted average of post-treatment effects is significant when assuming linear pre-treatment trends linearly persist to post-treatment periods.

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