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 a quarter of cross-province variations in education-turnout links. I also discuss the role of other factors, including Confucian culture and and election stakes.

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

JEL Classifications: D72, I28, P52

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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 a 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 rual governance challenges (Pei, 2006; 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 the ages of 6 and 15. I show that exposure to the CSL strongly predicts

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

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 18.6–19.9 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 10.2–15.9 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 that also influence turnout. I conduct a battery of checks to alleviate this concern. 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 rule out the influences of potential confounders. The results are confirmed by a regression to kink design that exploits sharp change in exposure to the CSL. In addition, I show that the results are robust to restricting the analysis to different subsamples where confounders would be of lesser concern: (i) I consider a narrow bandwidth of cohorts who are presumably more similar apart from exposure to the CSL; (ii) I look at individuals from provinces where the CSL was most effective in enhancing schooling 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, thus resulting in a higher cost associated with spending time on voting. In line with this opportunity cost explanation, I find that education has a more negative effect on voter turnout for people in regions with high returns to education and for people employed 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 capital, as captured by clan density, is low. This is consistent with the notion that rich social capital enhances 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 a quarter of variation in the effect of education on turnout across provinces.

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 democratic 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 local elections. Notably, I show that the opportunity cost helps explain education's negative effect on turnout even in an autocratic setting.

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 during the reform era since 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, these elections were institutionalized and widely implemented only after the *Organization Law of Village Committees* (OLVC) was introduced in 1987 (Shen and Yao, 2008).[2]

According to the OLVC, a typical village committee has 3–7 members, including a chairperson, a vice chairperson, and several committee members. The members must be directly elected by villagers through competitive elections, where the number of candidates exceeds the number of available positions. The term for a village committee is three years, and there are no term limits, which in theory creates reelection incentives. Village committees are tasked with a range of local issues, such as public goods provisions (e.g., irrigation, schools, and roads), resource allocation (e.g., land, collective assets), dispute resolution, and enforcement of mandated policies (e.g., one-child policy, social assistance programs) (Pei, 2006; Zhang et al., 2004; Shen and Yao, 2008; Martinez-Bravo et al., 2022; He and Wang, 2017). As such, rural elections represent the sole meaningful elections in China, which allow people to pick officials who enforce policies relevant to their livelihoods.

Existing literature has provided rich 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 have improved the implementation of popular policies, such as the provision of public goods, while

[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, village party officials are still appointed by upper-level authorities.

weakening the implementation of unpopular 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 imposed by clan members (Tsai, 2007).

While there is a wealth of insights into the policy consequences of rural elections, there has been relatively little focus on the issue of people's participation. The OLVC has detailed provisions regarding participation in rural elections. To be eligible for voting in an election, a person must be at least 18 years old and have their *hukou* (official residency registration) registered in the village. People 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 more than a year and having obtained the election organizing body's approval for their request 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 various restrictions. There are no specific rules governing the selection of the election day; thus, it is possible to fall on workdays.[3]

Previous studies have documented some patterns of participation in rural elections. Their findings indicate that when it comes to whether to vote, Chinese villagers do 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 elected officials are in charge of land allocation, a critical input of agricultural production. Similarly, Hu (2005) notes that economic development increased collective revenues controlled by village committees, thereby inspiring more participation

[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).

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 earnings and thus did not engage in elections (Wong et al., 2020).

To summarize, rural elections have some tangible impacts 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.

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, this 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 impacts of education on a range of outcomes, including political behavior (e.g., among others, 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 Chinese provinces. I obtained this information from Du et al. (2021), who used the issuance dates of official documents.

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 education 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 above. Secondly, the CSL requires children to attend school from age 6 and stay until age 15. Several measures are implemented to facilitate enforcing 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 determine 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).

Therefore, the CSL imposes a significant constraint on the educational attainment of children between the ages of 6 and 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 defined as 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 evaluate the impacts of policy interventions (e.g., Hoynes et al., 2016).

Figure 2 depicts *Exposure* as a function of the individual's age when the CSL was adopted. In the figure, the age when the CSL was adopted is negatively signed so that cohorts born later are further to the right. Individuals aged 16 or older when the CSL was adopted received an *Exposure* status of 0, as they were not exposed to the CSL. Individuals aged between 5 and 15 when the CSL was adopted were partially exposed, with their *Exposure* ranging from 0.1 to 0.9. Those aged 6 or younger when the CSL was adopted were fully exposed and received an *Exposure* status of 1. In sum, *Exposure* captures individuals' varying degrees of exposure to the CSL in a linear fashion.

2.3 Data and Variables

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 it has been regularly conducted by the 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 show that the demographic characteristics are comparable between the CGSS sample and the national census, confirming 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 respondents with rural *hukou*, who are pertinent to the rural elections examined in this study. Secondly, the sample only includes individuals between the ages of 25 and 55 at the time of the survey. Individuals of this group mostly have completed their education and are not too old to be inactive, allowing for accurate measurement of educational attainment and appropriate definition of constituents for a village's election. Applying these restrictions and excluding observations with missing values, the final sample comprises 19,892 respondents from 29 provinces (with Tibet and Hainan 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 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, albeit due to a different mechanism of social desirability: voting in autocracies is desired not because of democratic values

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

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

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 50.9%.[6] As such, the survey responses from the CGSS should 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 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] In favor of the conversion, 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 an alternative measure 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.

[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.

[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 below 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 A7, I show the results using the completion of middle school or above as an alternative measure of education, which confirm the findings discussed later that education reduces turnout in rural elections.

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

Panel B presents the two key variables in this study: education and turnout. The education gap between the exposed and unexposed groups is noticeable. On average, those exposed to the CSL have 9.045 years of schooling, while the average schooling for those unexposed is 6.952 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 17 percentage points less likely to vote than the unexposed. These observations suggest a negative relationship between education and turnout. Figure 3 depicts the education gradient in turnout. Turnout increases when educational attainment increases from illiteracy to primary school, however, turnout drops significantly as educational attainment becomes higher. For instance, the turnout rate is by 29.2 percentage points lower among college-educated people than among those who have only completed primary school. In addition, Figure A6 shows that an year of schooling in high school or above is negatively associated with turnout. These patterns motivate a more thorough analysis for the causal relationship between education and turnout.

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 employs exposure to the CSL as an instrument variable (IV) 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):

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

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

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. $Exposure_{bp}$ is exposure to the CSL, depending on one's birth year b and residence province p (see Figure 2). $Turnout_i$ 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, han ethnicity, parents' schooling, and parents' CPC membership. λ_b is the cohort fixed effect binned by five years.[10] μ_p is the province fixed effect. ϕ_t is the survey year fixed effect. The interaction $\lambda_b \times \mu_p \times \phi_t$ allows for flexible heterogeneity across cohorts, provinces, and surveys. ν_i and ε_i are the error terms. Standard errors are clustered at the province-by-birth-year level — same as the level of variation that the instrumentation leverages — to account for common shocks experienced by people in the same cohort and province. Observations are weighted by sampling weights.

[10] Unrestricted 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).

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

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

Identification. For the 2SLS estimate of β to be valid, the IV, 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 granular fixed effects included help control for confounding factors. Table A4 shows that the differences in observable characteristics in Table 1 are largely eliminated when conditioning on fixed effects. Second, I shed light on the exclusion restriction's plausibility using 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 (see Section 4.3).

Interpretation. The effect of schooling on turnout is likely heterogeneous. The IV strategy identifies the local average treatment effect (LATE, Imbens and Angrist, 1994), i.e., the causal effect of schooling on turnout for individuals on the margin of attaining an additional year of schooling in the absence of the CSL (who are thus *compliers* of the CSL). It should be kept in mind that the LATE can be very different than the average effect on general population. The LATE interpretation requires the monotonicity, namely, exposure to the CSL should universally increase educational attainment, albeit with potentially different degrees.

4 Results

This section presents the results of this paper. Section 4.1 reports the basic results. Section 4.2 provides several robustness checks. Section 4.3 presents some extensions that help explain the effects of education on turnout in China's rural elections. Section 4.4 discusses the results' implications.

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 1.253 years more schooling than those unexposed to the CSL, *ceteris paribus*. This effect amounts to 15.9% of the sample mean years of schooling (7.882) and accounts for about 59.9% of the gap in schooling between people exposed to the CSL and not (2.092), demonstrating the CSL's power in improving educational attainment.

In Table A5, 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 provinces). These patterns ascertain that the CSL has played a causal role in improving educational attainment. They also suggest that the monotoncity assumption is likely to be met, thus, the subsequent 2SLS estimate can be interpreted as a LATE, the average effect of education on turnout among compliers of the CSL.

In Figure 4, I examine the effects of the CSL on different margins of educational attainment. The figure reveals that the CSL has a positive effect on all levels of educational attainment, but the effect is the most pronounced for 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). It 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 5(a). 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 A7 shows that I can reject the null that the CSL has no effect on schooling even if conditioning on the pretrends linearly extrapolated to cohorts exposed to the CSL. In Section 4.2, I conduct additional checks to alleviate concerns that the pretrends could reflect confounding factors.

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 start by examining the reduced-form relationship between the CSL and voter turnout in rural elections, captured by the estimate of θ 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 18.6–19.9 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 5(b) 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]

These results indicate 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 schooling on turnout. The 2SLS estimates show a significant negative relationship between schooling and turnout. 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 15.9 percentage points, *ceteris paribus*. Such a large effect can be due to a combination of the steep

[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 A7).

education gradient in turnout (Figure 3) and the CSL's extensive effects such that people continue to obtain higher levels of education (Figure 4). In addition, recall that the IV strategy identifies the average effect on compilers, who may experience substantial change socioeconomic status than the counterfactual due to the CSL and thus change voting behavior distinctively. This marginal effect amounts to 31.2% of the sample mean turnout rate (50.9%). 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 9.5 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.[12]

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 is unlikely at play in the Chinese context since China's rural elections are just instrumental for *local* governance and have limited implications for regime legitimacy.[13] One more plausible 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 sensitive to. I elaborate this argument 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* counties as of the survey ("stayers"). The results are similar to the full sample estimates. However, these results should be taken with caution 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

[12]One needs to interpret the effect of CPC membership with caution since people self-select into joining the CPC and CPC members are have higher educational attainment. Were CPC members those who are inherently more likely to vote, the negative effect of education on turnout would be even more remarkable.

[13]Nevertheless, in Table A6, I examine the effects of education on political attitudes in China. I find education has little effect on the likelihood of joining the CPC, which can be associated with exposure to party indoctrination. But education does enhances liberal attitudes. However, controlling for liberal attitudes has negligible influence on the estimated effect of education on turnout.

[14]The 2SLS estimate suggests that one year more schooling increases the probability of (cross-county) migration by 13.11 percentage points (p-value = 0.001).

vote in their *hukou* villages.[15] Second, these results suggest that my findings are not driven by the non-classical 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 foremost concern is that exposure to the CSL may have picked up other factors that drive education and turnout, thus contaminating the results. I conduct several exercises to ascertain the the results are not sensitive to confounders.

1. 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 more similar apart from exposure to the CSL. Specifically, for unexposed individuals, I include those aged 19 or younger when the CSL was adopted, whose schooling exhibits no significant trending (see Figure 5(a)). 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 (1) and (2) display estimates using this sample, which confirms education's negative effect on turnout.

2. "CSL-Strong Sample". In this exercise, I focus on a "CSL-strong sample", where the CSL had a particularly salient impact on educational attainment, thus, the 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, provided that there are no other factors affecting 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 (3) and (4) display estimates using the "CSL-strong sample." Reassuringly, more education leads to lower turnout.

[15]Tellingly, in my sample, a stayer is on average four times as likely to vote as a migrant: the sample turnout rate is 56.6% for stayers and 14.7% for migrants.

3. 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 close 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.

4. 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 IV, 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 has a significant effect on 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 exercise yields a ratio of 0.41, indicating that my findings are robust to substantial violations of the exclusion restriction.[16]

5. Regression to Kink. In Appendix V, I implement a regression to kink (RK) design that exploits the sharp local variation in the slope of exposure to the CSL at -16 (cf. Figure 2). I show that the CSL leads to a significantly positive change in the slope of schooling and a significantly negative change in the slope of turnout. By contrast, there are no significant slope changes in other covariates

[16] Conley et al. (2012) do 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).

potentially associated with schooling and turnout. These results confirm the main findings are due to the CSL's own distinctive impacts rather than mechanical smooth changes across cohorts.

Peer Effects. An individual's turnout decisions are likely to be influenced by their peers (e.g., Grácio and Vicente, 2021). Specifically, if one's turnout and their peers' turnout are complements, then previous results, which leverage cohort variation, likely overestimate the effect of education on turnout. To address this concern, I control for the peer turnout rate in the regressions. When calculating the peer turnout rate, an individual herself is left out, and calculation is done at the village-by-cohort level, the level at which peer spillovers are most likely to occur. The CGSS data do not contain village identifiers for 2008 and 2007 waves. In Table A8, for comparison, I show that the main results hold after excluding the two waves. The remaining columns present results with the peer turnout rate controlled for. A higher peer turnout rate is indeed associated with a higher probability to vote. However, the inclusion of the peer turnout rate does not markedly change the estimated effect of education on turnout.

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 reviews). 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.

Alternative Statistical Inference. So far, I have clustered standard errors at the province-by-birth-year level. In Table A9, 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. For the second-stage regression, I also implement the "*tF* inference" for IV proposed by Lee et al. (2022), confirming the statistical significance of the 2SLS estimate (see the tF confidence interval at the bottom of Table A9).[17]

Other Checks. In Table A10, 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 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 Explanation: Opportunity Cost

Why does more education make people less likely to vote? One potential explanation is that more educated people have higher opportunity costs of voting: they have to forgone higher production income to engage in political activities. According to classical turnout theory (Palfrey and Rosenthal, 1985), an individual will not vote if the cost of voting overwhelms potential benefits from participation. The reason to focus on the opportunity cost is twofold. First, as the anecdotes in Section 2.1 suggest, villagers' participation decisions do involve careful cost-and-benefit calculations. Second, the opportunity cost of voting has been found to play an important role in explaining cross-country variation in the effect of education on turnout or political participation in general (Campante and Chor, 2012).[18] I will explore other factors in Section 4.3.2.

To understand the role of the opportunity cost channel, I examine whether the effect of education on turnout varies with the opportunity cost of voting. If the opportunity cost channel is at play, one would expect that education has a more pronounced negative effect on turnout in an environment

[17] 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.

[18] Campante and Chor (2012) document that the effect of education on participation negatively relates to a country's skill premium and individual employment in skilled occupations.

that features a greater opportunity cost of voting for educated people. In the spirit of Campante and Chor (2012), I use two measures to capture the degree of opportunity costs.

First, I employ returns to education as a measure for opportunity costs, which captures the cost specifically depending on the level of educational attainment. When returns to education are high, it would be more profitable for more educated people to devote time to work efforts rather than to voting. To measure returns to education, for each province in my sample, I estimate a Mincer regression of log income on years of schooling, controlling for rich covariates; to address endogeneity of schooling, I use the exposure to the CSL as an IV.[19] I employ the estimated coefficient on schooling to measure each province's 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. The second measure for the degree of opportunity costs is non-agricultural employment. Non-agricultural sectors compensate for education better than the agricultural sector, thus increasing the costs of time spent on voting. In addition, people employed in non-agricultural sectors, whose livelihoods are less reliant on the elected village committees, may perceive lower benefits from participating in rural elections (Oi and Rozelle, 2000). As a result, the cost of voting vis-à-vis can be more salient for non-agricultural workers, particularly when they have a high level of educational attainment. Columns (3) and (4) in Table 5 investigates the heterogeneous effects of education on turnout by non-agricultural employment. In line with the opportunity cost channel, education exhibits a much larger negative effect on turnout among non-agricultural workers than among agricultural workers, though the effects are not statistically distinguishable between the two groups of workers.[20]

[19]When estimating these regressions, I use the 2005 population census on individuals aged 25–55. Covariates includes indicators for cohort groups, gender, ethnicity, marital status, and employment status (employer or employee).

[20]In Table A11, I provide some robustness checks for these results. First, to further purge confounding factors in estimated returns to education, I control for occupation and city fixed effects in the Mincer regression. Using this new measure for returns to education, Columns (1) and (2) confirm that education (or exposure to the CSL) has a more negative effect on turnout in high-return provinces. Second, to supplement the heterogeneity by individual non-agricultural employment, I examine the heterogeneity by a village's reliance on agriculture. The reliance index is the share of non-agricultural workers in a village. Reassuringly, the results in Columns (3) and (4) show that education lowers turnout more in villages less reliant on agriculture, where the cost of voting is more salient because of low importance of village committees.

Existing research has highlighted the role of social capital in facilitating political participation, as high social capital could increase individuals' willingness to bear the cost of participation (e.g., Nannicini et al., 2013). Therefore, one would expect that social capital can countervail education's negative effect on turnout. To probe into this hypothesis, I measure social capital using the clan density in a province (Wang, 2020), which captures the degrees of social cohesion and mobilization in elections. In Table 5, Columns (5) and (6) show that the negative effect of education effect on turnout is primarily driven by individuals who are in low social capital provinces and thus are potentially more sensitive to high opportunity costs.

4.3.2 Cross-Province Variation in Education-Turnout Links

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

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

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

This yields estimates $\hat{\theta}_p$'s, i.e., the CSL's effects on turnout by province. Figure 6 presents $\hat{\theta}_p$'s, showing large variation across 29 provinces in the sample.

Next, I follow Finkelstein et al. (2016)'s approach to investigate how the variation in $\hat{\theta}_p$'s is explained by different factors. Specifically, I run the following bivariate regression:

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

Z_p is a provincial-level factor, thus, coefficient b captures its association with the CSL's effects on turnout. The estimated b is rescaled to reflect the effect of one SD change in Z_p on $\hat{\theta}_p$. η_p is the error term. Due to the small sample size (29 provinces), standard errors are bootstrapped. Figure 7 presents estimates from Equation 5. I explore several provincial-level factors. In addition to the cost of voting discussed above, and I also investigate another two factors related to (i) the utility of the action of voting and (ii) the benefit engendered by preferred electoral outcome, which are the two components of the benefit of voting's in classical turnout theory (Palfrey and Rosenthal, 1985).

First, in Figure 7(a), 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 I examine is Confucianism, which relates to the utility of voting. 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."*

The conformity and political apathy embedded in Confucianism may reduce the utility of voting, thus amplifying the opportunity cost channel. It is also likely that the education system directly imparts Confucian doctrines and lowers turnout, and such indoctrination can be more successful if Confucianism has been readily accepted by the local population. To study Confucianism's influence on the link between education and turnout, 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. Figure 7(b) shows that education has a more negative effect on turnout in provinces with stronger Confucianism.[21] These patterns corroborate the conformity and political apathy implied by Confucianism, and they also echo the cross-country findings by Campante and Chor (2012) that obedience cultures weaken education's (positive) impact on turnout in democracies.

Lastly, I examine a factor related to election stakes, given that high election stakes may moderate participation costs. I focus on the enforcement of the unpopular one-child policy (OCP), which the elected village committees have some discretionary to intervene. Tellingly, Martinez-Bravo et al. (2022) find that rural elections increase the number of OCP exemptions, indicating that elected officials align with voters' preferences. Using the fine rate for OCP violations (Ebenstein, 2010) to

[21] Reassuringly, Columns (1) and (2) in Table A12 conduct a similar heterogeneity exercise as in Table 3, showing that the CSL and education have more pronounced negative effects on turnout in highly Confucian provinces (above-median Confucian temple density).

measure statutory enforcement of the OCP. The exemptions made by village committees would be more valuable when the statutory enforcement is harsher. Figure 7(c) shows that education weakly 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 thus are less incentivized to vote. However, this association is statistically insignificant and quantitatively small.[22]

A natural question is how much these factors explain the cross variation in education's effect on turnout. From Figure 7, we see that returns to education and Confucianism appear to be two important factors, having tight linear relationships with turnout and explaining 19.9% and 16.4% of the total variance of education's effect on turnout (in terms of R^2). I also follow Finkelstein et al. (2016) to calculate the amount of effect dispersion that can be closed by equalizing a factor. For instance, in the distribution of $\hat{\theta}_p$, the gap between below-median and above-median provinces is -0.5131, and the gap in returns to education is 0.8786 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 $\frac{-0.1464 \times 0.8786}{-0.5131} = 0.251$. Therefore, returns to education can account for 25.1% of the dispersion in education-turnout links across provinces. Figure 8 reports this analysis for all three factors. Echoing Figure 7, returns to education and Confucianism have relatively high explanatory power.

The findings above confirm that the opportunity cost of voting is a significant factor in shaping the effect of education on turnout. In the meantime, other channels are also likely at play, such as Confucianism, which explains 22.8% of cross-province variation in the link between education and turnout. By contrast, the election stake, as captured by the OCP enforcement, does not seem to play an important role. However, it should be noted 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. Also, there can be other dimensions of election stakes other than the OCP exemptions. I hope the findings here can be instrumental for future research to explore the determinants of education-turnout links with more comprehensive measurements.

[22]Columns (3) and (4) in Table A12 show that the effect of education on turnout does not appear to vary strongly by the OCP stringency.

4.4 Discussions

Three remarks on the findings are in order. First, the fact that education lowers turnout due to associated high opportunity costs suggests that the Chinese voters act upon their cost-benefit calculations. This can be encouraging because self-interest, or more broadly, the awareness of individual rights is a building block of democracy — this is perhaps even more meaningful in the Chinese context, given that the absence of the awareness of individual rights is cited as a key obstacle for democracy to flourish in China (Goodnow, 1915; Huntington, 1991),[23] and that propaganda invariably emphasizes responsibilities over rights to justify authoritarianism.

Second, the results substantiate the difference of China's education system from the democratic counterpart. The negative relationship between education and turnout I find in China's rural elections is in stark contrast to the positive relationship found in democracies. In democracies, though the opportunity cost channel is also at play (Campante and Chor, 2012), on balance, education still has a positive impact on turnout because it cultivates a civic duty that encourages political participation (Campbell et al., 1980).[24] Tellingly, a large body of nation-building literature demonstrates that an important goal of national education policy in democracies is to impart civic values that underpin democratic institutions (Weber, 1976; Ramirez and Boli, 1987; Aghion et al., 2019; Alesina et al., 2021; Bandiera et al., 2019). China's education system does not pass on civic values, on the contrary, it imparts viewpoints that uphold autocratic institutions (see, e.g., Cantoni et al., 2017; Jiang, 2016). This feature can amplify the salience of opportunity costs associated with education or directly discourage turnout.

Lastly, given that educated people disengage in rural elections because of high costs, one may wonder how the government should respond to it. To answer this question, it is key to consider the government's preferences, social benefits (vis-à-vis costs), and political constraints. It is reasonable

[23] It is worth mentioning that Frank Goodnow and Samuel Huntington make this point at different times and from different angles. Goodnow wrote his essay shortly after the 1911 Chinese Revolution that overthrew the last imperial dynasty and established the Republic of China; he was concerned that China's lack of the rule of law brought about "an almost complete absence in the minds of the Chinese people of the idea of individual rights." When Huntington wrote his essay, China was (and still is today) under the communist rule; he argued that the collectivist Confucian culture suppressed the sense of individual rights. In modern-day China, the rule of law is by no means adequately established and Confucianism is not yet relinquished, nevertheless, my results document a pattern in line of the awareness of individual rights.

[24] For the role of civic duty in turnout, see, among others, Ali and Lin (2013).

to assume that the government favors high turnout due to legitimacy or popularity considerations (Wong et al., 2020). So, it is in the interest of the government to implement measures that mitigate costs of voting and boosts educated people's turnout (e.g., setting the election day on holiday, absentee ballot, or even compensation for turnout), but these measures are not necessarily socially optimal. It is unclear how effective these measures can be, because the opportunity cost associated with education may far exceed the payoff from voting. The pure utility of the action of voting is low due the education system's omission of civic values, and the benefit of inducing attractive policy outcomes via voting is limited due to the extremely local nature of rural elections. In addition, rural elections are subject to increasing controls and manipulations by the authorities (Pei, 2006; Martinez-Bravo et al., 2022), thus, there may not be meaningful changes in the political equilibrium even if those turnout-enhancing measures are successful. Taken together, it appears that the central issue here may not be turnout-enhancing measures *per se*, but the empowerment of rural elections and reforms of the education system so that any kind of turnout-enhancing measures can be meaningful. However, this is a rather difficult task under China's autocratic regime, and the possible pathway is far from obvious.

Taken together, my findings offer some insights for Chinese people's political behavior and the nature of China's education system. The policy implication is not a straightforward one: it relates to the broader issue of China's political modernization, which is out of the scope of this paper but is interesting and important on its own for future research to explore.

5 Conclusions

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. First, 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. Second, the opportunity cost of voting is an important explanation for the negative effect of education on turnout, but it only explains about a quarter of 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 on 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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Figures

Figure 1. Rollout of the CSL

Note: This figure displays the rollout of the CSL across provinces.

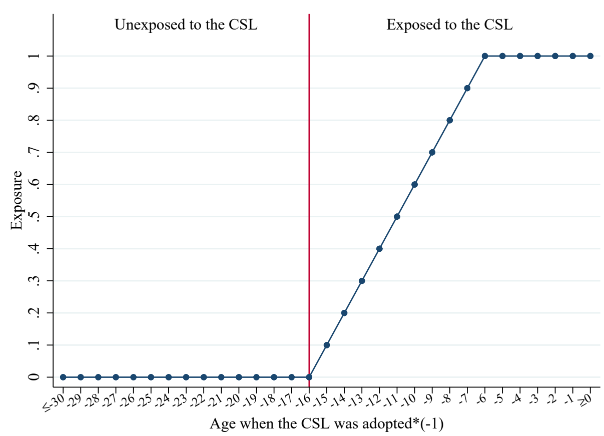


Figure 2. Exposure to the CSL Visualized

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 was adopted (negatively signed). The negative sign means being born before the CSL was adopted.

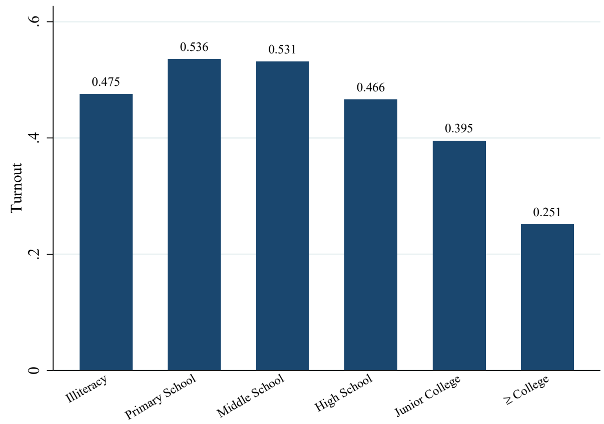


Figure 3. Turnout Rates by Education Level

Note: This figure depicts the education gradient in turnout. Individuals in the sample are grouped according to their educational attainment: illiteracy, 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.

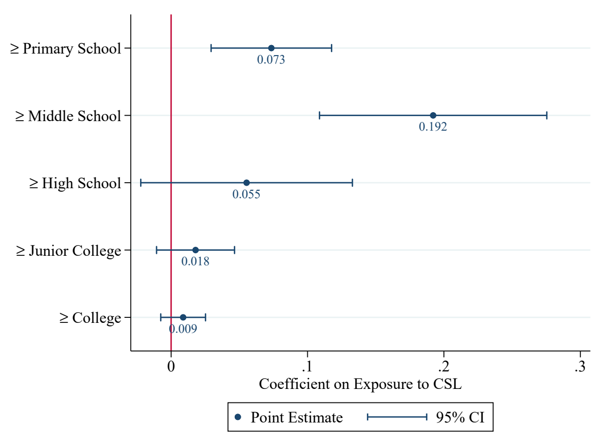
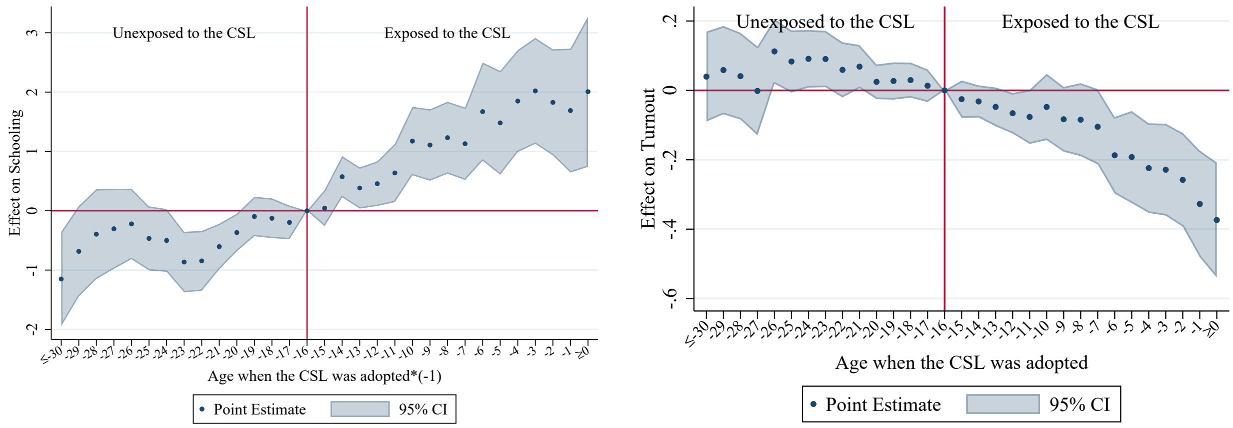


Figure 4. Effects of the CSL on Different Margins of Education

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. 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.

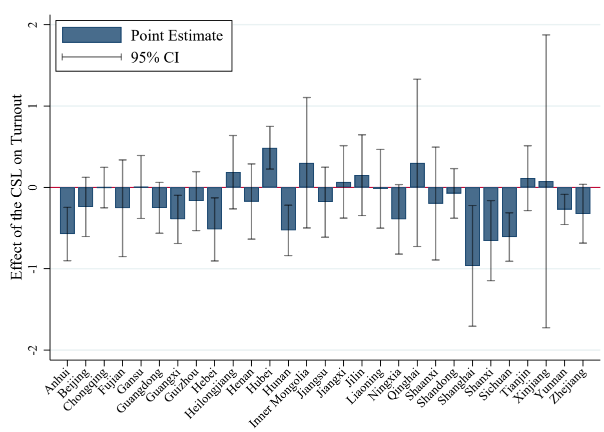
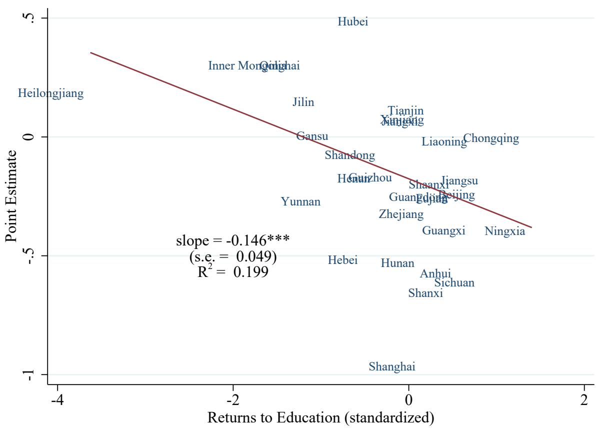
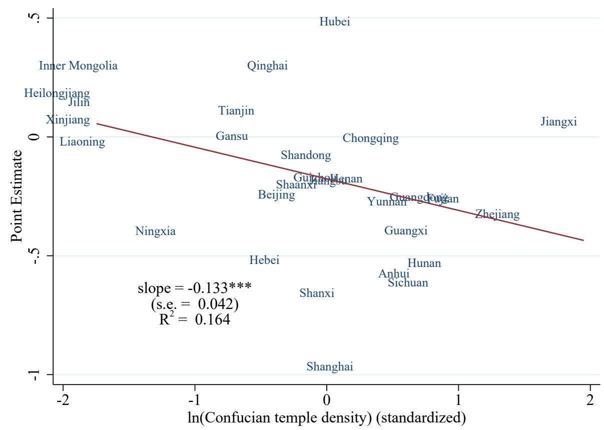


Figure 6. Effect of the CSL on Turnout by Province

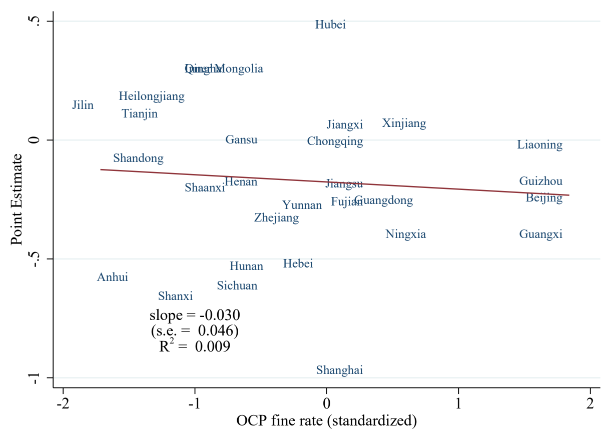
Note: This figure presents the CSL's reduced-form effects on turnout by province based on Equation 4.



(a). Returns to Education



(b). Confucianism



(c). OCP Fine Rate

Figure 7. Correlates of Provincial-Level Effects of the CSL on Turnout

Note: This figure visualizes the results of estimating Equation 5 for three provincial-level factors: returns to education, Confucianism, and the OCP fine rate. The standard errors are bootstrapped due to the small sample size.

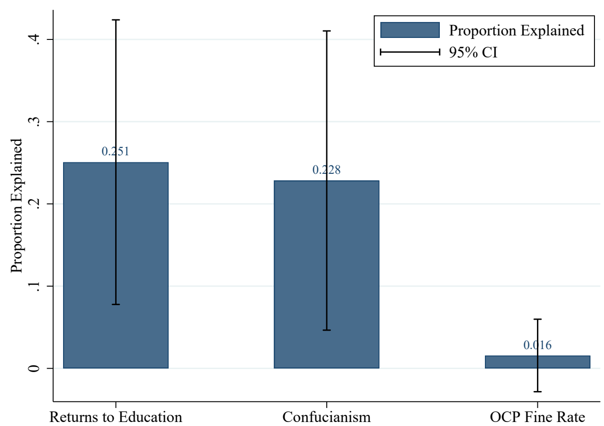


Figure 8. Variation in Effects Explained

Note: This figure displays the proportion of cross-province dispersion in the CSL's effect on turnout that can be explained by each provincial-level factor. The proportion explained is defined as the fraction of the effect size gap between below-median and above-median provinces that can be closed by equalizing a factor (see Section 4.3.2 for an example).

Tables

Table 1. Summary Statistics

	Full Sample (1)	Exposed (2)	Unexposed (3)	Diff. (2)-(3) (4)
Panel A: Personal Characteristics				
Age	41.119	33.185	47.464	-14.279***
	[8.717]	[5.460]	[4.723]	(0.089)
Female	0.511	0.513	0.510	0.003
	[0.500]	[0.500]	[0.500]	(0.009)
Han ethnicity	0.894	0.904	0.887	0.017***
	[0.307]	[0.295]	[0.317]	(0.005)
Father CPC member	0.099	0.092	0.104	-0.012**
	[0.298]	[0.289]	[0.306]	(0.005)
Mother CPC member	0.011	0.013	0.010	0.003
	[0.106]	[0.113]	[0.099]	(0.002)
Father schooling	4.702	6.119	3.569	2.550***
	[4.403]	[4.128]	[4.286]	(0.070)
Mother schooling	2.775	4.135	1.688	2.447***
	[3.911]	[4.029]	[3.447]	(0.064)
Panel B: Education and Turnout				
Schooling	7.882	9.045	6.952	2.092***
	[3.605]	[3.367]	[3.519]	(0.059)
Turnout	0.509	0.414	0.584	-0.170***
	[0.500]	[0.493]	[0.493]	(0.008)
Obs.	19,892	8,889	11,003	

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 (in parentheses).
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 2. Effects of the CSL on Education

	(1) Schooling	(2) Schooling	(3) Schooling
Exposure	1.830***	1.352***	1.253***
	(0.290)	(0.288)	(0.292)
DV mean	7.882	7.882	7.882
FEs	Y	Y	Y
Covariates		Y	
Inteacted covariates			Y
Obs.	19,892	19,892	19,892

Note: The dependent variable is years of schooling. All regressions include noted controls. Fixed effects (FEs) are province-by-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 3. Effects of Education on Turnout in Rural Elections

	Full Sample						Stayers	
	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout	(6) Turnout	(7) Turnout	(8) Turnout
Exposure	-0.187***	-0.186***	-0.199***				-0.186***	
	(0.048)	(0.048)	(0.048)				(0.054)	
Schooling				-0.102***	-0.137***	-0.159***		-0.161**
				(0.033)	(0.050)	(0.056)		(0.066)
DV mean	0.509	0.509	0.509	0.509	0.509	0.509	0.564	0.564
F stat.				39.722	22.077	18.341		13.824
FEs	Y	Y	Y	Y	Y	Y	Y	Y
Covariates		Y			Y			
Inteacted covariates			Y			Y	Y	Y
Obs.	19,892	19,892	19,892	19,892	19,892	19,892	17,243	17,243

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. Fixed effects (FEs) are province-by-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 4. Controlling for Confounders

	Bandwidth [-19, -6]		CSL-Strong Sample		Matching	
	(1) RF	(2) 2SLS	(3) RF	(4) 2SLS	(5) RF	(6) 2SLS
Exposure	-0.189***		-0.159**		-0.167**	
	(0.049)		(0.065)		(0.068)	
Schooling		-0.148***		-0.095**		-0.122**
		(0.054)		(0.048)		(0.062)
DV mean	0.504	0.504	0.491	0.491	0.491	0.491
F stat.		18.607		18.248		12.091
Obs.	8951	8951	10995	10995	10634	10634

Note: The dependent variable is the turnout dummy. Both reduced-form (RF) and 2SLS estimates are reported. All regressions include province-by-cohort-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) restrict the sample to people born 6 to 19 years before the adoption of the CSL. Columns (3) and (4) use the CSL-strong sample, where the CSL brought about a significant increase in schooling (see Appendix III for details). Columns (5) and (6) use a matching sample in which each CSL-exposed individual is paired with another who is observationally similar but less exposed to the CSL (see Appendix IV for matching procedures); pair fixed effects are included in regressions. Robust standard errors clustered at the province-by-birth-year level are reported in parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table 5. Potential Explanation: Opportunity Costs of Voting

	Returns to Education		Occupation		Social Capital	
	(1)	(2)	(3)	(4)	(5)	(6)
	High	Low	Non-Ag.	Ag.	High	Low
Panel A: 2SLS						
Schooling	-0.227**	-0.047	-0.194**	-0.128	-0.092	-0.176***
	(0.089)	(0.064)	(0.093)	(0.113)	(0.073)	(0.066)
F stat.	9.843	7.819	7.418	4.444	6.798	12.886
Equality test p-value		0.100		0.649		0.394
Panel B: Reduced-Form						
Exposure	-0.334***	-0.051	-0.231***	-0.108	-0.104	-0.247***
	(0.065)	(0.063)	(0.069)	(0.072)	(0.069)	(0.063)
DV mean	0.487	0.532	0.415	0.571	0.482	0.541
Equality test p-value		0.002		0.223		0.124
Obs.	9,794	10,098	7,943	11,949	10,183	9,709

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 province-by-cohort-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.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

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 the demographic and socioeconomic variables between the 2008 CGSS and the 2005 census. In the comparison, I restrict the census dataset so that it covers the same provinces and cohorts in the 2008 CGSS sample. Table A1 reports several variables' means and standard deviations (in parentheses) in the 2008 CGSS and the 2005 census. These variables are on average similar between the two datasets, though respondents in the CGSS appear to be relatively more educated in terms of high school completion.

Table A1. CGSS versus Census

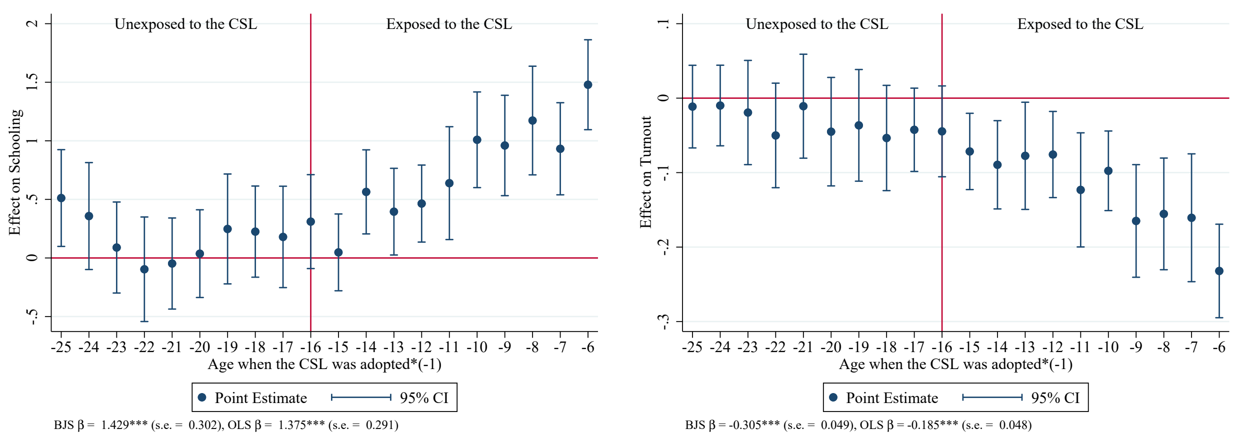
	(1) CGSS 2008	(2) Census 2005
Birth year	1966.9	1967.8
	(8.396)	(8.444)
Female	0.540	0.514
	(0.498)	(0.500)
Han ethnicity	0.904	0.909
	(0.294)	(0.288)
High school	0.136	0.0864
	(0.343)	(0.281)
Working	0.861	0.879
	(0.347)	(0.327)
Staying in hukou township	0.862	0.873
	(0.345)	(0.333)
Obs.	2,052	844,722

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 A1(a) and Figure A1(b) 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 5(a) and Figure 5(b)). This is because implementing the BJS estimator will drop some observations for which effects cannot be properly estimated. Reassuringly, Figure A1(a) and Figure A1(b) 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 A1(a) (Figure A1(b)), 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 A1(a) and Figure A1(b), 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):

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

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.

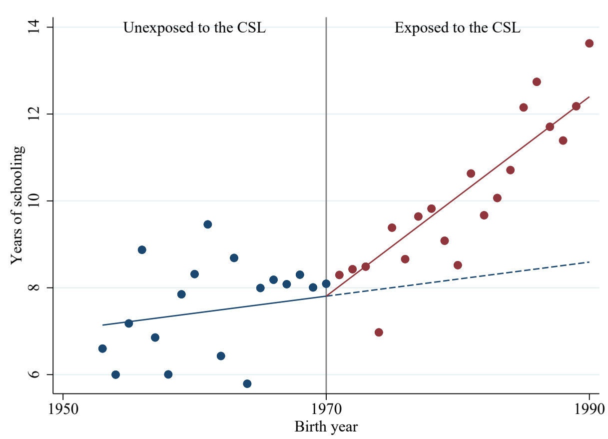


Figure A2. Education Across Cohorts: Zhejiang

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: $R^2 = 0.366$. The fitted value, $\widehat{Exposure}_{ib_ip}$, is simulated exposure to the CSL of individual i of cohort b_i.

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

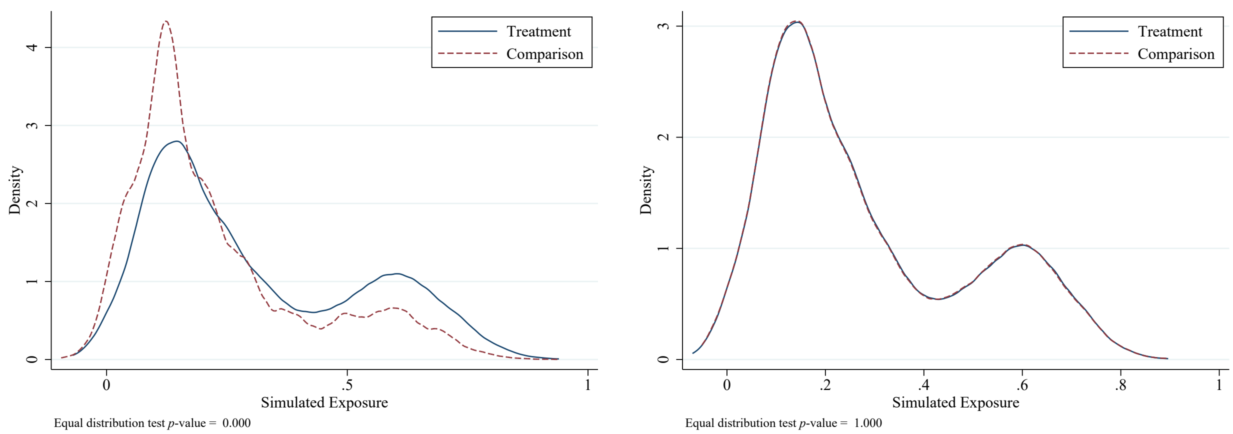
(i) from the same province;

(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 A3(a) includes individuals who satisfy condition (iii) and thus have weaker exposure to the CSL, while the post-matching comparison group in Figure A3(b) 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 Regression to Kink Design

Note that there is a kink point at -16 in Figure 2, where the slope of exposure to the CSL changes discontinuously. If other factors change smoothly around this kink point, one would expect to see a sharp change in schooling and turnout. Therefore, I implement a regression to kink (RK) design Card et al. (2015) to exploit sharp change the sharp change in exposure to the CSL around the kink point. I estimate the following specification:

$$y_i = \alpha + \delta(16 - AgeCSL_i) + \gamma(16 - AgeCSL_i) \times Exposed_i + \varepsilon_i, \tag{A3}$$

$-AgeCSL_i$ is individual i's (negatively signed) age when the CSL was adopted in their province: the higher $-AgeCSL_i$, the greater exposure to the CSL (cf. Figure 2). $16 - AgeCSL_i$ thus centers $-AgeCSL_i$ around the kink point -16. $Exposed_i = \mathbb{1}\{16 - AgeCSL_i > 0\}$ is an indicator for non-zero exposure to the CSL. The parameter of interest in Equation A3 is γ. It captures the change in the slope of outcome y_i when $16 - AgeCSL_i$ varies from below 0 to above 0, leading to a change in the slope of exposure to the CSL. I estimate Equation A3 using observations satisfying $-AgeCSL_i \in [-25, -6]$, which omits possible changes on the right of the second kink point at 0 in Figure 2.

Table A2 displays the main RK estimates. Columns (1) and (2) show significant slope changes in schooling and turnout, with a sharp increase in schooling and a sharp decrease in turnout. Column (3) displays the IV estimation, where turnout is regressed on predicted schooling based on the (first-stage) regression in Column (1). It again confirms a negative effect of schooling on turnout. Figure A4 visualizes the RK estimates, confirming the sharp slope changes around the kink point.

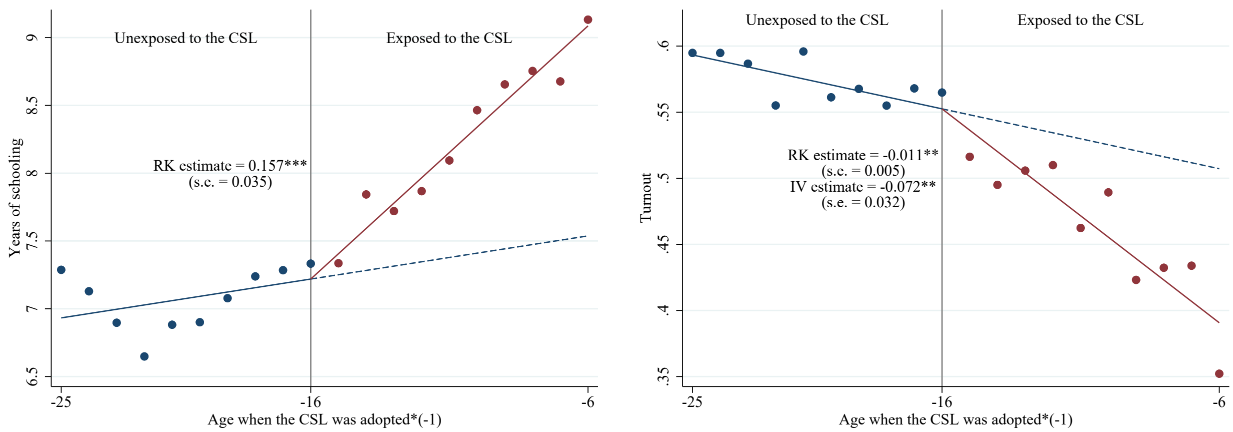
The identification assumption for the RK design is that other factors associated with schooling and turnout should not have discontinuous slope changes around the kink point. Table A3 shows that the smoothness in a range of variables. In addition, I conduct a classical McCrary's test for density continuity near the kink point (see Figure A5): it shows smoothness in the density function, implying a lack of strategic sorting regarding status of exposure to the CSL.

Taken together, these results indicate that the CSL has its own distinctive effects on schooling and turnout.

Table A2. RK Estimates

	(1) Schooling	(2) Turnout	(3) Turnout
(16 - AgeCSL) × Exposed	0.157***	-0.011**	
	(0.035)	(0.005)	
Schooling			-0.072**
			(0.032)
DV mean	7.508	0.532	0.532
F stat.			20.799
Obs.	13921	13921	13921

Note: This table presents the RK estimates from Equation A3. Standard errors are clustered at the province-by-birth-year level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$



(a). Education

(b). Turnout

Figure A4. Regression to Kink Estimates

Note: This figure visualizes the regression to kink (RK) estimates. Standard errors are clustered at the province-by-birth-year level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table A3. RK Estimates: Smoothness in Covariates

	(1) Female	(2) Han ethnicity	(3) Father CPC member	(4) Mother CPC member	(5) Father schooling	(6) Mother schooling
(16 - AgeCSL) × Exposed	-0.002	-0.001	-0.003	-0.000	0.037	0.045
	(0.003)	(0.006)	(0.002)	(0.001)	(0.037)	(0.036)
DV mean	0.517	0.898	0.104	0.012	4.452	2.491
Obs.	13,921	13,921	13,921	13,921	13,921	13,921

Note: This table presents the RK estimates from Equation A3, using a set of individual covariates as dependent variables. Standard errors are clustered at the province-by-birth-year level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

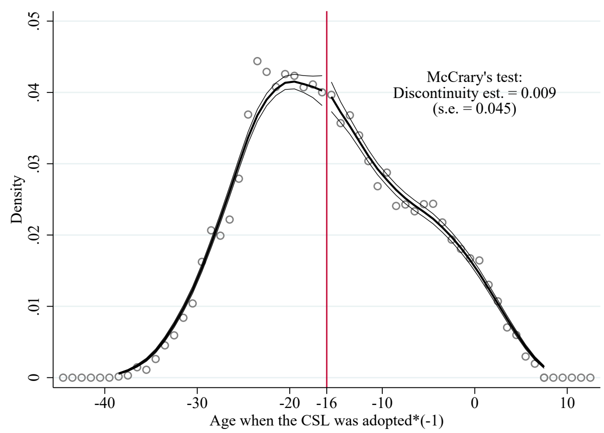


Figure A5. McCrary's Test

Note: This figure presents McCrary's test for discontinuity in the density of the running variable in Equation A3, $-AgeCSL_i$.

VI Supplementary Figures

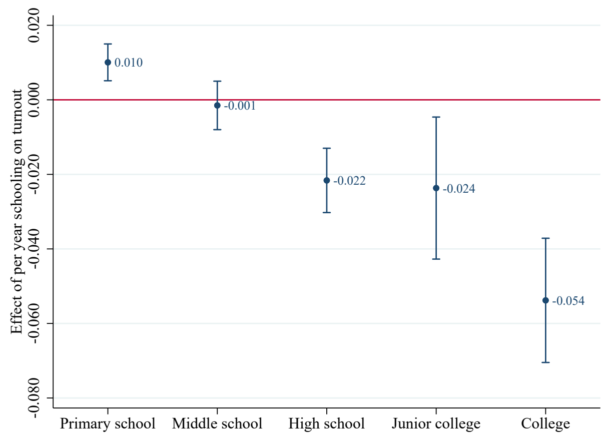
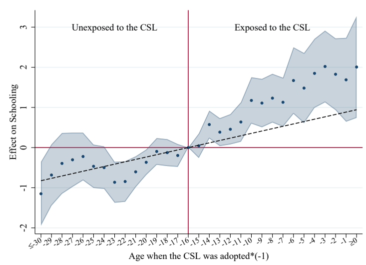


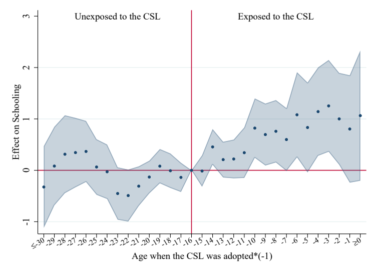
Figure A6. Effects of Per Year Schooling on Turnout by Education Stage

Note: This figure presents the association of per year schooling with turnout by education stage. The solid dots are point estimates, and the lines are 95% confidence intervals. I estimate the regression: $Turnout_i = \alpha + \sum_e \beta_e \cdot Schooling_{ei} + \varepsilon_i$, where $Schooling_{ei}$ is individual i's years of schooling in education stage e (e.g., 6-year primary school, 3-year middle school, 3-year high school, 3-year junior college, and 4-year college). Note that illiteracy is the omitted reference group, and college and graduate school are consolidated into the same category (with 4 years of schooling). Thus, coefficients β_e's capture the associations of per year schooling with turnout by education stage. Robust standard errors are used to construct the 95% confidence intervals.

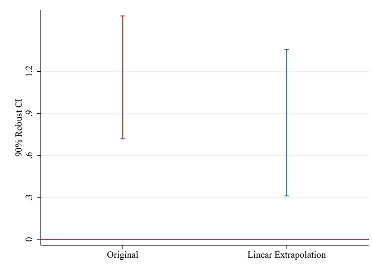
Panel A | Panel B | Panel C



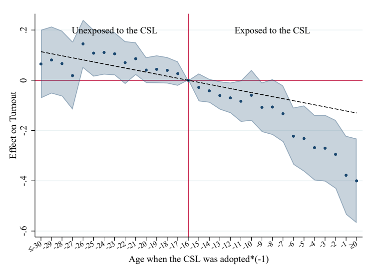
(a). Schooling



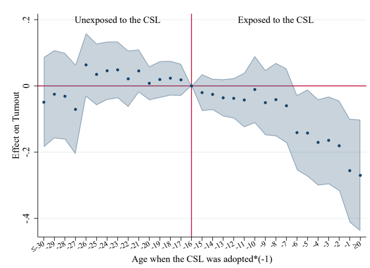
(c). Rotated Event Study



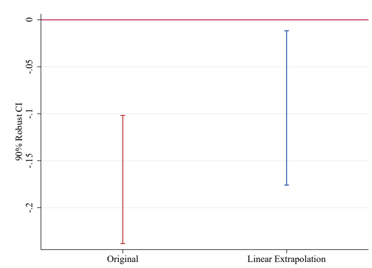
(e). Sensitivity



(b). Turnout



(d). Rotated Event Study



(f). Sensitivity

Figure A7. Sensitivity Tests for Event Studies

Note: This figure presents the results of the sensitivity test for event studies, proposed by Rambachan and Roth (2022). The test formally examines whether treatment effects are still significant when conditioning on extrapolated pretrends, which serve as a proxy of violations of parallel trends. I consider linear extrapolation. In Panel A, I overlay the event-study coefficients with a linear trend estimated using data on unexposed cohorts, extending it to the exposed cohorts. Panel B illustrates the deviations of the event-study coefficients depicted from the trend line. Finally, Panel C presents the confidence intervals outlined in Rambachan and Roth (2022) for the average of all coefficients for exposed cohorts, conditioning on the linearly extrapolated pretrend.

VII Supplementary Tables

Table A4. Covariate Balance

	(1) Without FEs	(2) With FEs
Female	0.003	0.022
	(0.009)	(0.028)
Han Chinese	0.017***	0.012
	(0.005)	(0.017)
Father Schooling	2.550***	0.230
	(0.070)	(0.244)
Mother Schooling	2.447***	0.060
	(0.064)	(0.201)
Father CPC member	-0.012**	0.038**
	(0.005)	(0.016)
Mother CPC member	0.003	0.001
	(0.002)	(0.008)

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-province-by-year fixed effects ($\lambda_b \times \mu_p \times \phi_t$). Robust standard errors are reported in the parentheses.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A5. Effects of the CSL on Education by Subsample

	Female?		Educated Parents?		Developed Provinces?	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes
Exposure	1.612***	0.869**	1.239***	0.966*	1.438***	1.174***
	(0.451)	(0.391)	(0.376)	(0.518)	(0.453)	(0.383)
DV mean	7.124	8.638	7.305	9.444	7.346	8.393
Equality test p-value		0.202		0.661		0.657
Obs.	10,645	9,247	14,508	5,384	9,714	10,178

Note: The dependent variable is years of schooling. For Columns (1) and (2), the sample is divided by gender. For Columns (3) and (4), the sample is divided by whether at least one parent had completed middle school. For Columns (5) and (6), the sample is divided according to whether the provincial-level share of population that has completed middle school (measured using the 1982 census) is above or below median. All regressions include province-by-cohort-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 A6. Education, Ideology, and Turnout

	Liberal Attitude		Turnout			
	(1)	(2)	(3)	(4)	(5)	(6)
	RF	2SLS	RF	RF	2SLS	2SLS
Exposure	0.510***		-0.209***	-0.204***		
	(0.108)		(0.050)	(0.050)		
Schooling		0.351***			-0.144***	-0.143***
		(0.098)			(0.048)	(0.049)
Liberal attitude				-0.011***		-0.002
				(0.004)		(0.006)
DV mean	0.011	0.011	0.518	0.518	0.518	0.518
F stat.		22.333			22.333	21.475
Obs.	17,440	17,440	17,440	17,440	17,440	17,440

Note: The dependent variable is: liberal attitude index (Columns (1) and (2)) and turnout (Columns (3)–(6)). The liberal attitude index is measured based on a respondent's response to CGSS questions: whether they believe the government should not intervene in (i) open criticism of the government (freedom of criticism), (ii) how many children a person wants to have (freedom of fertility), and (iii) where a person wants to live and work (freedom of migration). I take the first principal component of the answers. Both reduced-form (RF) and 2SLS estimates are reported. All regressions include province-by-cohort-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 A7. Alternative Specification

	(1)	(2)
	≥ Middle school	Turnout
Exposure	0.192***	
	(0.042)	
≥ Middle school		-1.034***
		(0.354)
DV mean	0.593	0.509
F stat.		20.468
Obs.	19,892	19,892

Note: The dependent variables are the indcator for middle school completion and turnout. All regressions include province-by-cohort-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 A8. Controlling for Peer Effects

	Main Specifications		+ Peer Turnout	
	(1)	(2)	(3)	(4)
	Turnout	Turnout	Turnout	Turnout
Exposure	-0.190***		-0.182***	
	(0.057)		(0.054)	
Schooling		-0.134***		-0.130***
		(0.051)		(0.050)
Peer turnout			0.211***	0.178***
			(0.013)	(0.022)
DV mean	0.530	0.530	0.530	0.530
F stat.		17.142		17.078
Obs.	14,177	14,177	14,177	14,177

Note: The dependent variable is turnout. All regressions include province-by-cohort-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 A9. Alternative Statistical Inference

	(1)	(2)	(3)
	Schooling	Turnout	Turnout
	1st Stage	Reduced-Form	2nd Stage
Exposure	1.253	-0.199	
	(0.292)***	(0.048)***	
	[0.299]***	[0.062]***	
	{0.313}***	{0.053}***	
Schooling			-0.159
			(0.056)***
			[0.064]**
			{0.060}***
tF 95% CI			[-0.173, -0.031]
Obs.	19,892	19,892	19,892

Note: This table presents first-stage, reduced-form, and second-stage estimates, using different standard errors to conduct statistical inferences. All regressions include province-by-cohort-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*. In Column (3), the tF test for IV proposed by Lee et al. (2022) is implemented.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table A10. Other Robustness 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	-0.202***		-0.188***		-0.155**		-0.242***	
	(0.051)		(0.049)		(0.075)		(0.061)	
Schooling		-0.157***		-0.151***		-0.247		-0.129***
		(0.058)		(0.058)		(0.208)		(0.045)
DV mean	0.517	0.517	0.508	0.508	0.538	0.538	0.482	0.482
F stat.		17.991		16.805		2.445		23.038
Obs.	18,627	18,627	18,459	18,459	9,252	9,252	10,640	10,640

Note: The dependent variable is the turnout dummy. All regressions include province-by-cohort-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 A11. Robustness: Opportunity Costs of Voting

	Returns to Education (alt.)		Village Reliance on Agriculure	
	(1)	(2)	(3)	(4)
	High	Low	Low	High
Panel A: 2SLS				
Schooling	-0.287**	-0.016	-0.195***	0.020
	(0.119)	(0.059)	(0.069)	(0.111)
F stat.	7.771	9.647	15.655	1.810
Equality test p-value		0.041		0.107
Panel B: Reduced-Form				
Exposure	-0.373***	-0.019	-0.349***	0.015
	(0.062)	(0.069)	(0.079)	(0.080)
DV mean	0.466	0.554	0.435	0.627
Equality test p-value		0.000		0.001
Obs.	9,949	9,943	7,182	6,995

Note: The dependent variable is the turnout dummy. All regressions include province-by-cohort-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 A12. Heterogeneous Effects by Confucianism and OCP Enforcement

	Confucian Temple Density		OCP Fine Rate	
	(1)	(2)	(3)	(4)
	High	Low	High	Low
Panel A: 2SLS				
Schooling	-0.268**	-0.095*	-0.137**	-0.181
	(0.136)	(0.051)	(0.060)	(0.115)
F stat.	5.914	14.672	13.849	4.677
Equality test *p*-value		0.235		0.733
Panel B: Reduced-Form				
Exposure	-0.273***	-0.149**	-0.196***	-0.187**
	(0.065)	(0.072)	(0.056)	(0.077)
DV mean	0.496	0.527	0.475	0.553
Equality test *p*-value		0.201		0.925
Obs.	10,808	9,084	10,886	9,006

Note: The dependent variable is the turnout dummy. All regressions include province-by-cohort-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.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

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