Ideological Curriculum Reform, Financial Literacy, and Long-Run Consequence of Political Education

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Abstract

This paper studies the economic consequences of ideologically oriented schooling in an authoritarian setting. Between 2004 and 2010, China introduced a new high school politics curriculum with the explicit goal of shaping students' ideology. Using the staggered rollout of the reform across provinces and comparing adjacent student cohorts, I estimate its causal effects on financial knowledge, preferences, and behavior. Three main findings emerge. First, exposure to the reformed curriculum durably reduces financial literacy, measured by standardized questions on basic economic and financial concepts. Second, affected students exhibit higher relative risk aversion, lower participation in equity markets, and a reduced propensity to engage in entrepreneurship. Third, placebo analyses show no similar patterns for cohorts that did not attend high school, and no declines in language or mathematics scores, ruling out changes in overall student ability or instruction quality as explanations. These results highlight the welfare costs of politicized education and show how state control over curricular content can generate persistent economic distortions.

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Keywords: Ideological curriculum reform, Financial literacy, Political education, Household finance

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1 Introduction

Public education is central to human capital accumulation, economic development, and state building. Compared with democracies, non democratic governments invest in schooling with different objectives. Formal education can serve as a vehicle for regime indoctrination that raises trust in and support for the state. To this end, authorities can shape the content of instruction, especially in the humanities and social sciences such as politics and history, in order to strengthen the regime. A growing body of work shows that beliefs, attitudes, and preferences can be shaped by what is taught in school.

The welfare meaning of such paternalistic information control is less straightforward compared with documenting results of shifts in attitudes. Within a consumer sovereignty framework, even if preferences are manipulated by an autocratic state, individuals still optimize given the information they receive, leading to an equilibrium that may be internally consistent yet socially undesirable. If curricular manipulation suppresses useful information and tilts decision making away from privately beneficial choices, there may exist a more informed allocation that is Pareto improving. This paper provides causal evidence on the economic consequences of ideological curricula in a non democratic system, quantifying the welfare relevant costs of ideological propaganda style education.

I study a nationwide reform of the high school politics curriculum introduced by the Chinese Communist Party, implemented prospectively for entering cohorts and phased across provinces between 2004 and 2010, with the explicit objective of shaping students ideology. As part of a comprehensive overhaul of upper secondary curricula, the Politics subject was reorganized into three modules labeled *Economic Life*, *Political Life*, and *Cultural Life*. Textbooks were centrally authored and adopted almost uniformly, and college entrance examination syllabi were aligned with the new content, making exposure binding for teachers and students. The revised economic and financial materials placed instruction within a state centered narrative that emphasized the distinctiveness of the socialist market economy and the features of state led development, while reducing the space devoted to standard economics and finance as social science. I focus on *Economic Life* because it concentrates instruction on markets and finance and was the primary target of ideological adjustment. In a system that stresses specialized university training rather than broad general education, high school is often the only stage at which non finance major students receive systematic instruction in economic and financial concepts, which makes exposure during this period particularly consequential.

To isolate exogenous variation in exposure to the new curriculum, I exploit the province level staggered rollout and the fact that only entering cohorts were taught under the revised

textbooks, following the identification logic in Cantoni et al. (2017). Using nationally representative micro survey data, I estimate difference in differences and event study specifications that compare cohorts entering high school before and after the reform within province and cohort cells. Exposure to the reformed curriculum substantially lowers long run financial literacy as measured by standardized questions on core economic and financial concepts. Using a lottery choice module, I construct a measure of relative risk aversion and find that exposed cohorts are more risk averse, less likely to participate in the stock market, and less likely to become entrepreneurs. These are real economic outcomes with clear welfare content, consistent with the literature linking financial knowledge to market participation, portfolio choice, and household welfare (Calvet et al., 2007; Van Rooij et al., 2012; Lusardi and Mitchell, 2014).

I conduct placebo tests using individuals who did not attend high school and assign pseudo treatment dates based on province and expected schooling trajectories; the dynamic estimates are flat and statistically insignificant for financial literacy, risk preferences, and subsequent behaviors, which supports the identification. I also consider alternative mechanisms. If the reform had broadly lowered instructional quality or altered the composition of students entering high school, one would expect declines in general academic performance. In the data, standardized verbal and mathematics scores are unchanged across the reform, which is inconsistent with a general deterioration in teaching quality or student ability. Finally, the effects are heterogeneous by ability. Using mathematics based terciles, impacts concentrate among the middle and high groups, while the low group shows no detectable response. This distributional pattern implies disproportionate costs for students most likely to proceed to college and to enter high influence occupations, magnifying the social consequences of politicized schooling.

This paper advanced the research on how non democratic regimes use information and schooling to shape beliefs and preferences. A growing literature studies informational autocracy and the role of propaganda in sustaining support for the regime (Lott, 1999; Guriev and Treisman, 2019). Alesina and Fuchs-Schündeln (2007) demonstrated that historical exposure to communist rule has persistent effects on preferences for redistribution and state intervention. Beyond political attitudes, media and information interventions can causally shift beliefs and behavior, as shown by work on television and radio access in modern and historical settings (DellaVigna and Kaplan, 2007; Enikolopov et al., 2011; Adena et al., 2015). The closest paper is Cantoni et al. (2017), who exploit the staggered adoption of a new high school politics curriculum in China and, using a survey of Peking University undergraduates, document causal effects of curricular content on political views.

I extend this line of research by moving from preferences and attitudes to welfare relevant outcomes in household finance. Leveraging nationally representative micro data, I quantify changes in financial knowledge, relative risk aversion, entrepreneurship, and equity market participation. The results show a clear trade off. Ideological schooling that serves regime objectives depresses financial literacy and market engagement, generating real economic costs for households and potential losses for society in long term.

Secondly, this paper extends the literature that examines how school curricula shape the long run effects of education. Curricular content structures the worldview acquired in school and thereby influences beliefs that persist into adulthood. Existing studies show that changes in curricula can durably affect college major choice (Görlitz and Gravert, 2018), gender and racial attitudes (Adukia et al., 2023; Agarwal and Sen, 2022), religious beliefs and practice (Arold, 2024; Adukia and Harrison, 2025), and political preferences (Fuchs-Schündeln and Masella, 2016; Chen et al., 2023; Costa-Font et al., 2024). In this spirit, I show that China's ideologically oriented reform of the high school politics curriculum reduced students' financial literacy and, through this channel, altered household finance behavior, including risk taking, entrepreneurship, and participation in equity markets.

Last but not least, this paper relates to research on financial literacy and on interventions designed to improve it. Field evaluations in both developed and developing countries show that financial education programs can raise knowledge and modify financial behaviors (Bruhn et al., 2016; Lührmann et al., 2018; Carpena et al., 2019; Kaiser et al., 2022), and school based programs are increasingly adopted across countries (Cole et al., 2016; Frisancho, 2023).[1] In contrast to this intervention oriented literature, I document that politicized instruction in basic economics and finance durably reduces students' financial literacy and bring negative effects on financial behavior. The empirical setting of this paper focus on China, the world's most populous country, where each year cohorts totaling approximately eight million high school students are exposed to the politics curriculum.

The remainder of the paper is organized as follows. Section 2 discusses institutional background of China's high school education system and the ideological oirented curriculum reform. Section 3 describes the survey data I use and identification strategy. Section 4 shows empirical results of long term effect of curriculum reform on financial literacy and behavior. Section 5 shows robustness check of main results. Section 6 concludes.

[1]The U.S. Consumer Financial Protection Bureau recommends introducing and building key financial education concepts throughout the K 12 school years (CFPB, 2013). The United Kingdom made financial education part of the national secondary school curriculum in September 2014, and Australia has adopted similar measures. Many developing countries, including Brazil, Colombia, India, Indonesia, and Uganda, are introducing financial education in their school systems as well.

2 Institutional Background

2.1 China's Educational System and Curriculum Reform

China's modern education system is characterized by high centralization and standardization. Following nine years of compulsory education, students either enter academic senior high schools or vocational tracks, with the former primarily serving as the pipeline for college entrance. The Gaokao tightly links performance in upper secondary school to university access and, through that channel, to later labor market opportunities and social mobility (Tsang, 2000; Hannum, 2003). This institutional linkage raises the returns to academic achievement and induces exceptional investment of time and resources by households and students. Empirically, the college admission system in China is organised around score cutoffs, with access to elite institutions determined by narrow margins that have sizeable wage consequences, underscoring the high stakes nature of the exam (Chen et al., 2020; Jia and Li, 2021).

Consistent with these incentives, comparative evidence shows very long study time among Chinese students. PISA based statistics report that students in the four participating provinces of China devote among the highest classroom hours per week, with B S J Z reporting about 31.8 classroom hours and ranking near the top across participating systems (OECD, 2019). Combined with extensive after school preparation, this time intensity makes the high school stage both a crucial period for human capital formation and a powerful conduit for sustained ideological transmission, reinforcing the political socialization described above.

Political and ideological education has historically been embedded in this system. Since the founding of the People's Republic, the state has explicitly tasked schools with the mission of "cultivating successors for socialism." Textbooks are centrally authored and reviewed by the Ministry of Education to ensure nationwide ideological consistency (Pepper, 2000). High school students are required to take courses in "Ideological and Political Studies," and their performance in these subjects is assessed in the *Gaokao*, reinforcing continuous exposure to the state's official narrative (Brady, 2009).

China's Eighth Curriculum Reform, launched in 2001 and phased into high schools thereafter, is widely viewed as more than a pedagogical modernization. Building on the nationwide patriotic education drive that the central government initiated in the 1990s to consolidate regime legitimacy and recalibrate youth political socialization after 1989, the reform explicitly reconfigured the high school politics curriculum to shape students' beliefs about China's political institutions, the rule of law, and the country's state-led development path. Official guidance for textbook writing emphasized ideological struggle and the need to resist

adverse external influences, while revised gaokao frameworks and new modules embedded these priorities in assessment. In parallel, curriculum standards reveal a persistent tension between globalization-oriented human capital goals and the transmission of socialist values, with the fine-tuned standards strengthening Sino-centric cultural identity and the role of Party leadership. Collectively, this trajectory is consistent with interpreting the 2001 reform as a deliberate counterweight to the liberalizing intellectual currents of the 1980s and 1990s, achieved through a renewed, system-wide apparatus of political socialization.[2]

2.2 Changes in Textbook Content

The curriculum reform led to a substantial restructuring of the high school political education framework. The subject previously known as "Politics" was reconfigured into three modules: *Economic Life*, *Political Life*, and *Cultural Life*. This reorganization created a more standardized and centralized structure, ensuring that students across provinces followed nearly identical materials. More importantly, the restructured framework emphasized the integration of ideological guidance with the teaching of basic knowledge, reflecting the state's intention to use education as a primary vehicle for political socialization (Pepper, 2000; Brady, 2009).

A closer look at the new textbooks reveals key shifts in the narrative and content emphasis. First, the economic modules increasingly portrayed markets as operating under the guidance of state macro-control, with explicit language highlighting the Party's leadership in economic development. Discussions of market forces and competition became more constrained, often framed within a narrative of socialist market economy. Second, case studies were heavily politicized: examples used in exercises and illustrations frequently focused on state-owned enterprises, technological achievements driven by government programs, or the success of national policies, while references to foreign models or Western experiences were minimized. Third, the materials incorporated explicit warnings about "Western ideological infiltration" and the risks of "uncritically importing capitalist ideas," further embedding ideological vigilance into the curriculum (Tsang, 2000; Brady, 2009).

These changes collectively redefined the cognitive schema through which students understood economic and financial systems. By downplaying the role of individual agency and private financial decision-making while amplifying the narrative of state primacy, the

[2]For discussions of the post-1989 patriotic education campaign and its role in state-led nationalism, see Zhao (1998) and Wang (2008) On the stated ideological aims and measurable attitudinal effects of the high school curriculum reform, see Cantoni et al. (2017) . On the globalization–nationalism tension within the 2001 and 2011 standards, see Law (2014).

reform created an environment in which students were less likely to internalize basic concepts of financial and economic knowledge. Such ideological framing provides a plausible channel through which the curriculum reform could have long term effects on students' financial literacy and economic behavior.

3 Data and Methodology

3.1 Main Data

This section describes the primary data sources used in this study.

3.1.1 China Family Panel Studies (CFPS)

I rely primarily on microdata from the China Family Panel Studies (CFPS), a nationally representative longitudinal survey of Chinese communities, families, and individuals launched in 2010 and conducted biennially since then. The survey covers 25 out of 31 provinces, municipalities, and autonomous regions, representing approximately 95% of China's population in 2010. CFPS is implemented by the Institute of Social Science Survey at Peking University and employs computer assisted personal interviewing (CAPI) technology provided by the Survey Research Center at the University of Michigan, ensuring high data quality. Compared to surveys with similar questionnaire structures such as the PSID and UKHLS, CFPS achieves internationally comparable response and tracking rates.[3]

For the main analysis, I use the 2018 wave of CFPS, which includes a financial literacy module and thus serves as the primary source for the financial literacy outcome variable. Financial literacy is measured by the number of correctly answered questions. In addition, 2018 is sufficiently distant from the timing of the high school curriculum reform, so treated students have typically completed schooling or entered the labor market, facilitating the study of long term reform effects on behavior. CFPS also provides a rich set of individual and household level control variables to approximate socioeconomic status and to examine potential within household spillover effects.

3.1.2 China Household Finance Survey (CHFS)

To assess the robustness of the curriculum reform's impact on financial literacy and subsequent behaviors, I also use the 2018 wave of the China Household Finance Survey (CHFS).

[3]The 2010 CFPS household response rate was 84.14%, the 2018 household level cross wave tracking rate was 86.6%, and the individual level cross wave tracking rate was 80.8%.

CHFS is a nationally and provincially representative survey that collects micro level information on household income, expenditures, assets, liabilities, insurance and securities, social insurance, intergenerational transfers, demographics, employment, and credit history. Sampling is conducted at the household level with design weights to ensure representativeness.

3.1.3 2015 Mini Census Microdata

To estimate the effect of high school curriculum reform exposure on individuals' later occupational choices, I link the provincial rollout schedule of the policy to the 2015 China mini census microdata, the most recent publicly accessible census microdata. This dataset contains extensive individual and household information, including demographic characteristics, highest attained education level, and occupation codes.

3.2 Descriptive statistics

Table 1 presents the descriptive statistics for our estimation sample. Panel A focuses on several measures for the initial track recommendation. Our baseline outcome variable y_t is a dummy variable that equals one if the track recommendation is at least equal to the college track. Enrollment in the college or university track is most relevant from a policy perspective, since it implies the student is tracked towards higher education. Panel A shows that 49.7% of the students receive a track recommendation equal to, or higher than, the college track.

3.3 Identification Strategy

I study the effect of the high school curriculum reform on financial literacy and related behaviors using a cohort difference in differences design with two way fixed effects. The design exploits that provinces adopted the reform in different cohort years and that exposure is determined at the high school entry year. The baseline two way fixed effects model compares adjacent entry cohorts within the same province around the first cohort year in which the reform took effect, while absorbing province and entry cohort fixed effects and controlling for individual covariates:

$$Y_{i,p,c} = \alpha + \beta \,\text{Reform}_{p,c} + X'_{i,p,c}\gamma + \delta_p + \tau_c + \sum_c \phi_c\big(\text{Age}_{i,p,c} \times 1\{c\}\big) + \varepsilon_{i,p,c} \tag{1}$$

Here $Y_{i,p,c}$ refer to the outcome for individual i who entered high school in province p and cohort c. $X_{i,p,c}$ collect individual level controls. δ_p and τ_c denote province and co-

Table 1: Descriptive statistics

	(1) Mean	(2) SD	(3) Min	(4) Max
Panel A: Demographics and education				
Age	31.585	5.739	21.000	45.000
Male	0.548	0.498	0.000	1.000
Education	6.079	0.887	5.000	9.000
Panel B: Financial literacy and preferences				
Financial literacy	2.262	0.703	1.000	3.000
Relative risk aversion	0.238	0.274	-0.159	0.500
Panel C: Financial outcomes				
Stock market participation	0.186	0.389	0.000	1.000
Consumption borrowing	0.124	0.330	0.000	1.000
Entrepreneurship	0.080	0.271	0.000	1.000
Income	8.189	4.552	0.000	13.122
Panel D: Cognitive scores				
IQ	5.781	1.170	1.000	7.000
Mathematics score	14.547	4.919	0.000	24.000
Verbal score	28.849	4.150	0.000	34.000
Observations	2074			

Notes: this table reports descriptive statistics for the estimation sample. Means and standard deviations are computed using non missing observations. Sample sizes vary by variable: Relative risk aversion $N = 1972$, IQ $N = 2072$, Mathematics score and Verbal score $N = 1190$, all others $N = 2074$. Education is coded on a discrete scale from 5 to 9 as in the survey instrument.

hort fixed effects. The term $\sum_c \phi_c(\text{Age}_{i,p,c} \times 1\{c\})$ allows each entry cohort to have its own linear age slope; this flexibly absorbs within cohort age dispersion that arises around the school entry cutoff month and addresses the fact that financial literacy may covaries with age. Treatment exposure is coded at entry: $\text{Reform}_{p,c} = 1$ if $c \geq T_p$ and 0 otherwise, where T_p denote the first cohort year in which the reform applied in province p. Standard errors are clustered at the province level, and I show robustness to two way clustering by province and cohort (Athey and Imbens, 2022; Abadie et al., 2023).

The identifying variation comes from cross cohort differences within province in a narrow window around T_p, relative to contemporaneous cohorts in provinces not yet exposed.

Province fixed effects remove time invariant differences across provinces, such as difference in ideological environments, school inputs, or local financial market development conditions. Cohort fixed effects absorb nationwide cohort shocks, for example macro trends in economic growth, financial access or financial education outside school. The cohort specific age slopes address contamination from mechanical age differences between adjacent cohorts. Because exposure is determined at the beginning of high school, any related instructional content delivered in later grades would attenuate differences between pre and post reform cohorts, so estimates are conservative in magnitude.

The assumption requried for my approach is that, without curriculum reform, the financial literacy outcomes of students attending high school in different states would have evolved along parallel trends, and that treatment effects are homogeneous over time. To provide evidence of the parallel trends restriction and trace dynamics effects of reform, I estimate an event study version that views each provincial adoption as a discrete event. Define the event time $k = c - T_p$ and event dummies $D_{k,i,p} = 1$ if $c - T_p = k$ and 0 otherwise, with one pre reform period omitted as the reference. The event study specification is

$$Y_{i,p,c} = \alpha + \sum_{k=-K}^{L} \beta_k D_{k,i,p} + X'_{i,p,c}\gamma + \delta_p + \tau_c + \sum_c \phi_c \big(\text{Age}_{i,p,c} \times 1\{c\}\big) \tag{2}$$

In robustness specifications I allow for province specific linear cohort trends, so that reform effects are identified off discrete within province jumps around T_p; this is demanding in terms of power and accommodates province specific smooth evolutions in attitudes or markets.

Even when there are no confounding shocks, staggered adoption may bias two way fixed effects if already treated cohorts serve as controls for later treated cohorts and treatment effects are not homogeneous vary over time (Goodman-Bacon, 2021; Sun and Abraham, 2021). I therefore complement the CSDID specifications that exclude already treated cohorts from the control group and use not yet treated cohorts instead (Callaway and Sant'Anna, 2021). Across these checks, the pattern and magnitude of the estimates remain stable.

4 Results

This section shows how ideological curriculum reform affect student's financial literacy in long term as well as their economic preference and financial behavior.

4.1 The effect of curriculum reform to financial literacy

Following equation (1), I first report two way fixed effects estimates of the curriculum effect on financial literacy. As shown in Table 2, Column (1) includes province fixed effects and high school entry cohort fixed effects only. The point estimate on exposure is -0.269 and statistically significant at 1% levels, indicating that cohorts exposed to the reformed curriculum scored lower on the financial literacy test.

Columns (2) to (4) sequentially add individual level covariates. Column (2) controls for gender and indicators of socio economic status such as parental education and household income. To address the concern that within cohort age dispersion may correlate with financial literacy, Column (3) augments the specification with cohort specific age slopes, which absorb systematic age variation within entry cohorts. Because provinces adopted the reform at different times and the main survey wave is in 2018, exposure mechanically induces cross provincial differences in the age composition of observed cohorts. To remove any contamination from province specific age patterns, Column (4) further includes an Age × Province trend.

Across all specifications, the exposure coefficient remains stable in magnitude and negative. The stability from Column (1) through Column (4) suggests that neither observed individual characteristics nor age related composition differences are driving the result, supporting the conclusion that the high school curriculum reform reduced financial literacy.

4.2 Event Study Results

Having shown that ideological curriculum reform has a negative impact on financial literacy, I next study the dynamics of this effect to understand whether it is temporary or persistent across time. To do so, I estimate specification (2) and display the estimates of the event study dummies in Figure 1. The estimates are not statistically significant in the years prior to the curriculum reform, which support for parallel trend assumption required in this identification strategy. Instead, the estimates for the years following the curriculum reform are negative, statistically significant. The impact of the curriculum reform is persistent over time.

4.3 Treatment Effect Heterogeneity

Recent studies highlight that in staggered difference-in-differences settings, it is important to account for potential biases arising from treatment effect heterogeneity, where the timing of treatment varies across individuals (De Chaisemartin and d'Haultfoeuille, 2020; Sun and

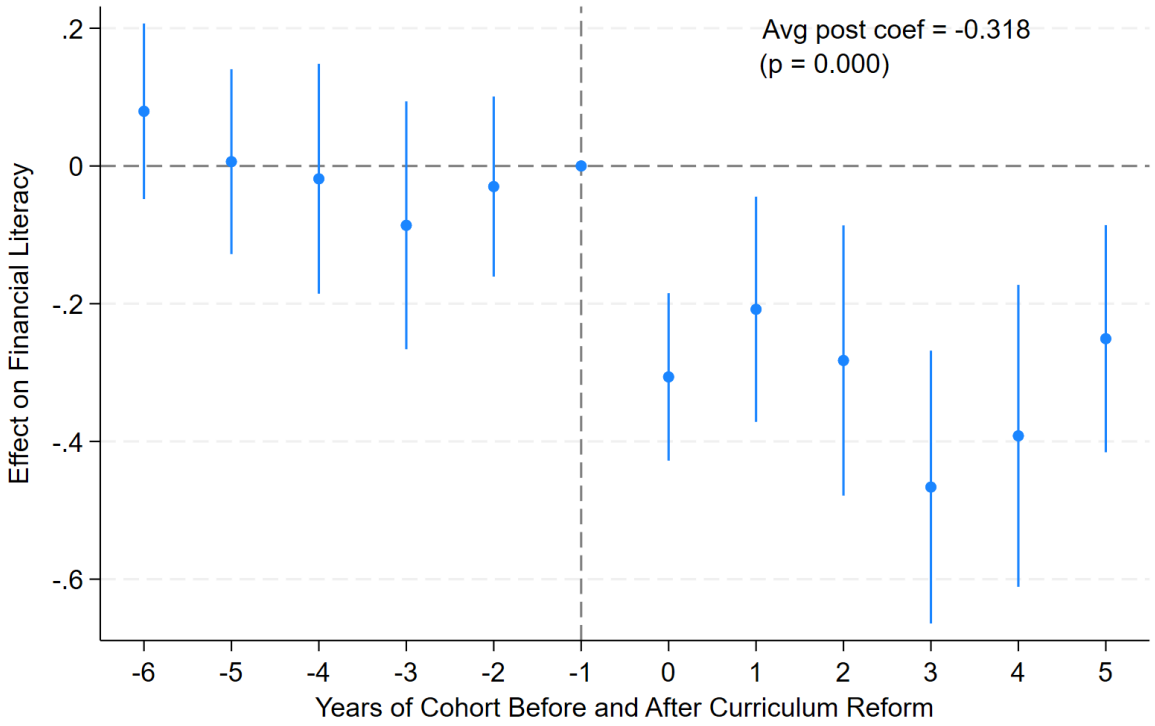
Table 2: TWFE Estimates of Curriculum Reform Effects On Financial Literacy

	Dependent variable: Financial literacy			
	(1)	(2)	(3)	(4)
Reform	-0.269***	-0.256***	-0.240***	-0.216***
	(0.065)	(0.063)	(0.053)	(0.059)
Province FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls		✓	✓	✓
Age×Year			✓	
Age×Province				✓
Mean of Dep. Var.	2.29	2.29	2.29	2.29
Std. Dev. of Dep. Var.	0.67	0.67	0.67	0.67
Observations	1963	1963	1963	1963

Notes: This table reports estimates of equation (1) using the two way fixed effects (TWFE) estimator. The dependent variable is *Financial Literacy*, measured as the number of correct answers in a standardized financial knowledge test. The key explanatory variable *Reform* equals 1 if students in province p and cohort c, individuals were exposed to the reformed curriculum, and 0 otherwise. All specifications include province (δ_p) and cohort-year (τ_c) fixed effects. Columns (2) to (4) additionally control for individual-level covariates such as gender and socio-economic status. Column (3) further adds an Age × Year trend to account for within cohort age variation in financial literacy, while Column (4) includes an Age × Province trend to absorb province-specific age patterns. Across all specifications, the coefficient on *Reform* is consistently negative and statistically significant, indicating that the curriculum reform substantially reduced financial literacy. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels. Numbers in parentheses are standard errors clustered at the province level.

Abraham, 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). Treatment effect heterogeneity can come from the impact of the curriculum reform being heterogeneous over time or varying across the different entry cohorts of high school students exposed to new curriculum at different times, and I complement the TWFE estimates using the estimator proposed by Callaway and Sant'Anna (2021). In the current setting, all children are affected by the new curriculum at specific years depending on province origin, and there are thus no never treated individuals. I use not yet treated student as control group as suggested by Callaway and Sant'Anna (2021). I present the estimates in Table 3. The dependent variable is financial literacy, measured by correct number of question answered in questionaire.

Figure 1: Dynamic Impact of Curriculum reform on Financial Literacy



Notes: The figure reports the estimates from specification (2) and shows the dynamics of the impact of curriculum reform on financial literacy, as well as whether there were pre-trends in financial literacy prior to the reform. The independent variables of interest are a set of event dummies for the number of years before or after the introduction of curriculum reform at each province. The dependent variable is number of questions about financial literacy answered correctly. Province and cohort entry year fixed effect are included in regression. The lines are 95% confidence intervals.

Column (1) include province and entry year fixed effect, while column 2 additionally add individual level controls. The results are statistically negative and are similar to previous TWFE estimates at Table 2 in magnitudes.

4.4 Impact On Other Behavior

A large body of literature documents that financial literacy shapes a wide range of financial behaviors, including retirement planning and savings decisions (e.g., Van Rooij et al., 2012; Deuflhard et al., 2019), stock market participation and portfolio allocation (Van Rooij et al., 2012; Bucher-Koenen et al., 2021), debt management and credit use (Disney and Gathergood, 2013; Gorbachev and Luengo-Prado, 2019), and the uptake of insurance and other complex financial products (Behrman et al., 2012; Lusardi et al., 2017).

Table 3: CSDID Estimates of Curriculum Reform Effects On Financial Literacy

	Dependent variable: Financial literacy	
	(1)	(2)
ATT of Reform	-0.385***	-0.313***
	(0.092)	(0.094)
Province FE	✓	✓
Year FE	✓	✓
Controls		✓
Mean of Dep. Var.	2.29	2.29
Std. Dev. of Dep. Var.	0.67	0.67
Observations	1963	1963

Notes: This table reports estimates from the Callaway and Sant'Anna (2021) estimator (CSDID). The dependent variable is *Financial Literacy*, measured as the number of correct answers in a standardized financial knowledge test. Numbers in parentheses are standard errors clustered at the province level. Both columns include province and year fixed effects; Column (2) additionally controls for individual level covariates. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels.

In this section, I examine how the ideological curriculum reform, through its adverse impact on students' financial literacy, also altered their financial behaviors in the long term. Specifically, I study high school cohorts exposed to the reform and evaluate its effects on four outcomes: consumption borrowing ratio, relative risk aversion, entrepreneurship, and stock market participation.

First, I investigate whether the reform altered individuals' tendency toward consumption borrowing. The survey asks respondents: "To what extent do you agree with the following statement: I would borrow money to satisfy my consumption needs." Responses range from (1) "completely disagree" to (5) "completely agree." I code responses 1–2 as 0 and 4–5 as 1. Consumer borrowing markets in China are underdeveloped, and only about 10% of respondents report any borrowing.

Second, I study the reform's effect on students' risk-aversion attitudes. Following the standard approach in the household finance literature (e.g., Barsky et al., 1997; Dohmen et al., 2005), I use a series of hypothetical lottery choice questions to recover each individual's coefficient of relative risk aversion (RRA). In each question, respondents choose between a certain payoff (e.g., 100 RMB for sure) and a risky option with a 50% chance of

receiving a higher payoff (e.g., 250 RMB) and a 50% chance of receiving nothing. By varying the magnitude of the high payoff across questions, I can identify the switching point at which a respondent moves from the safe to the risky option, which reveals the range of their RRA under a constant relative risk aversion utility function $u(c) = \frac{c^{1-\rho}}{1-\rho}$. Higher estimated ρ corresponds to greater risk aversion. This method has been widely applied in survey-based studies of financial decision-making and preference formation, and is particularly suited to contexts where observed market behavior may not fully capture underlying risk preferences.

Third, I investigate the effect of the reform on students' entrepreneurship, using self-employment status as the dependent variable. This is consistent with a body of research that treats entrepreneurship or self-employment as the outcome of interest (Sobel et al., 2010; Wang, 2012; Bianchi, 2012; Liang et al., 2018). I define "entrepreneurship" as reporting self-employed status and estimate reform effects using the same event study framework (2) described above.

Fourth, I examine the reform's effect on participation in the stock market. The survey asks respondents about the composition of their wealth portfolio. From these questions, I construct an indicator variable equal to one if the respondent reports holding any individual stocks, or if they own stocks indirectly through mutual funds or other bank-issued investment products, and zero otherwise. Stock market participation is closely related to individuals' financial literacy and risk aversion (Vissing-Jørgensen, 2002; Vissing-Jørgensen and Attanasio, 2003), and it has profound implications for financial development and wealth inequality within an economy (Brav et al., 2002; Melcangi and Sterk, 2025). China exhibits a relatively low rate of stock market participation compared to developed markets. Based on my calculations from the 2019 China Household Finance Survey, direct stock ownership is 6.42% and indirect ownership is 11.51%. In contrast, in the United States, the 2019 Survey of Consumer Finances reports direct participation at 15% and indirect participation at 53%. This measure captures an important dimension of household financial market involvement that may be directly shaped by the financial knowledge and attitudes formed during high school.

The results are presented in Figure 2, where all four outcomes are estimated using the event study specification in (2). I control for province and cohort entry year fixed effects, as well as Age × Province trends to absorb province-specific age patterns that may arise due to differences in reform timing across provinces in our single cross-sectional data. The four panels correspond to: (i) consumption borrowing ratio, (ii) risk aversion, (iii) entrepreneurship, and (iv) stock market participation rate. Consumption borrowing ratio shows no significant post-reform effect, suggesting that the curriculum change did not materially al-

ter students' intertemporal choice behavior. Panel B shows that risk aversion significantly increased after the reform (post-reform joint test coefficient 0.067, $p = 0.082$), indicating that the ideological shift in curriculum content made individuals more risk averse. Further, Panel C examines the effect on entrepreneurship. Entrepreneurial behavior is closely related to risk preference: the entrepreneur's role is to make decisions under uncertainty and bear the associated consequences, which is the source of entrepreneurial profit and can be considered the rent to risk preference. This view can be traced back to Knight (1921) and was formalized by Kihlstrom and Laffont (1979) and Evans and Jovanovic (1989). The results in Panel C support this link, as individuals exposed to the ideological curriculum reform are less likely to engage in entrepreneurship (coefficient -0.160, $p = 0.040$). Finally, Panel D shows the effect on stock market participation, which is also closely related to risk preferences, with an estimated coefficient of -0.069 ($p = 0.091$).

In summary, these findings are consistent with a growing empirical literature documenting how experiences under communism shape individual preferences and behaviors. For example, Schaewitz et al. (2022) show that exposure to communism in Germany had long-lasting effects on individuals' risk and time preferences; Fritsch et al. (2023) and Ivlevs et al. (2021) find persistent negative effects of communist experience on entrepreneurship in Germany and Eastern Europe; and Laudenbach et al. (2024) document that exposure to communism limits participation in financial markets. The reform I study, by increasing the ideological content of high school curricula, appears to have influenced financial literacy, risk preferences, and economic behaviors in ways similar to those found in these studies.

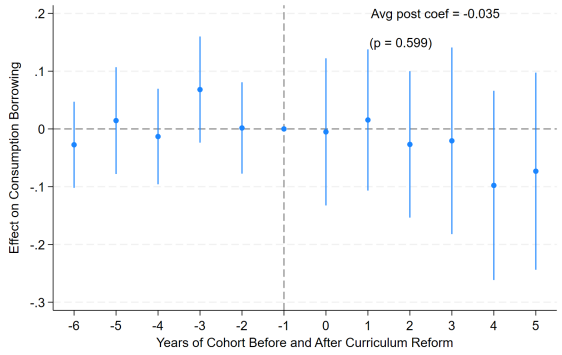
4.5 Heterogeneous analysis

I investigate which students are most affected by the politicized curriculum reform by dividing the sample into three groups based on their standardized mathematics test scores from the CFPS cognitive module: the low-ability tercile (Q1), the middle-ability tercile (Q2), and the high-ability tercile (Q3). For each group, I estimate the average treatment effect over the post-reform window using specification (2) and plot the results in Figure 3.

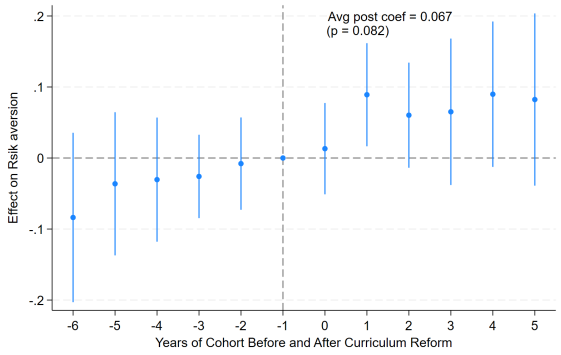
Understanding this distributional effect is important because if high-ability students are systematically more exposed to ideological bias, the long-run real effects of propaganda could be larger than what the average treatment effect suggests. High-ability students are more likely to attend college, enter high-income or high-influence occupations, and accumulate greater wealth. Lower financial literacy among this group could therefore have disproportionately negative economic consequences.

A substantial literature documents that mathematics ability is a strong predictor of over-

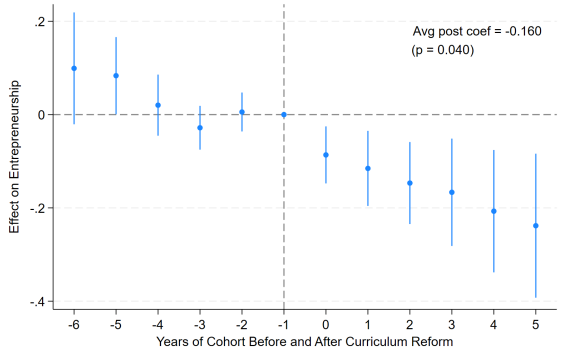
Figure 2: Event Study Estimates: High School Entry Cohort Outcomes



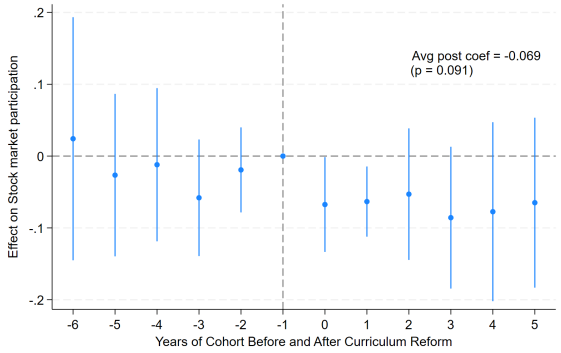
(a) Panel A: Consumption Borrowing Ratio



(b) Panel B: Relative Risk Aversion



(c) Panel C: Entrepreneurship

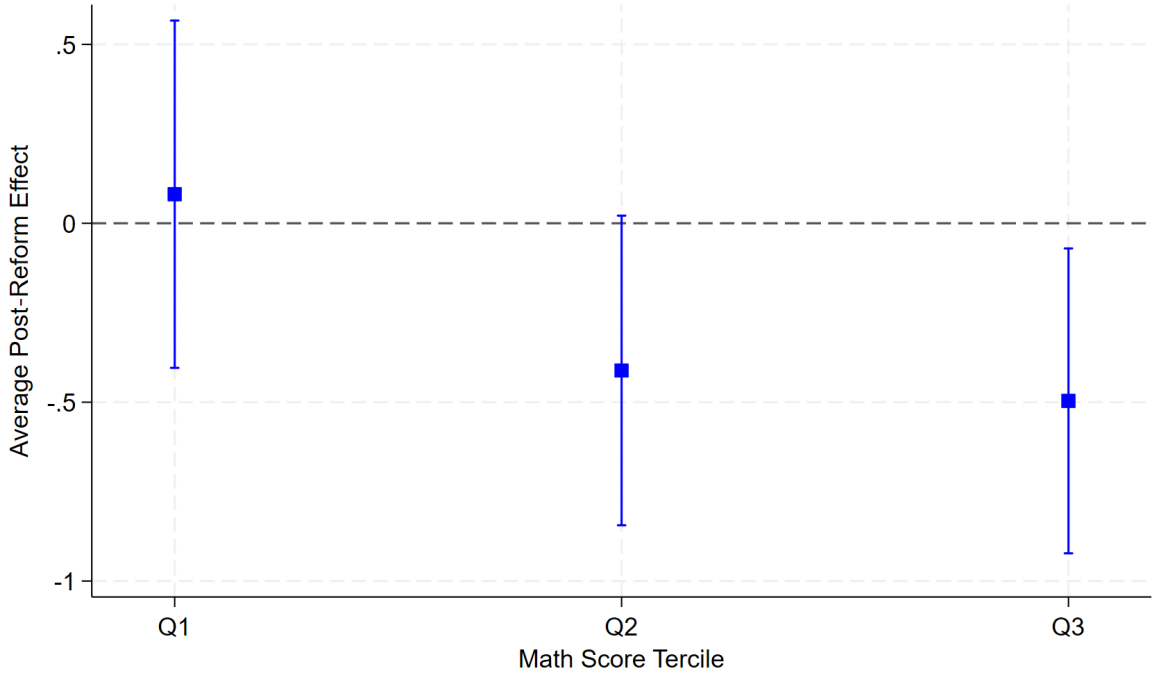


(d) Panel D: Stock Market Participation

Notes: This figure presents event study estimates for high school entry cohorts relative to reform implementation across four outcomes: consumption borrowing ratio (Panel A), relative risk aversion from lottery responses (Panel B), probability of self employment (Panel C), and stock market participation rate (Panel D). Vertical bars depict 95% confidence intervals clustered at the province level.

all academic performance and future labor-market outcomes. I use each respondent's mathematics test score from the CFPS Guttman-scaled cognitive assessment to assign tercile ranks. The results, displayed in Figure 3, show that the main impacts are concentrated among Q2 and Q3 students, while the estimated effect for the Q1 group is small and statistically insignificant. This pattern suggests that the politicized curriculum reform disproportionately affected students with higher cognitive ability. Given that these students are more likely to advance to higher education, enter influential occupations, and accumulate greater income and wealth, a reduction in their financial literacy could have particularly large long run real consequences for the economy.

Figure 3: Heterogeneous on Math Score



Notes: The sample is split into three groups according to standardized math test scores: Q1 (low-ability tercile), Q2 (middle-ability tercile), and Q3 (high-ability tercile). The points represent the estimated average treatment effect for event times 0–5, and the vertical bars show 95% confidence intervals with standard errors clustered at the province level.

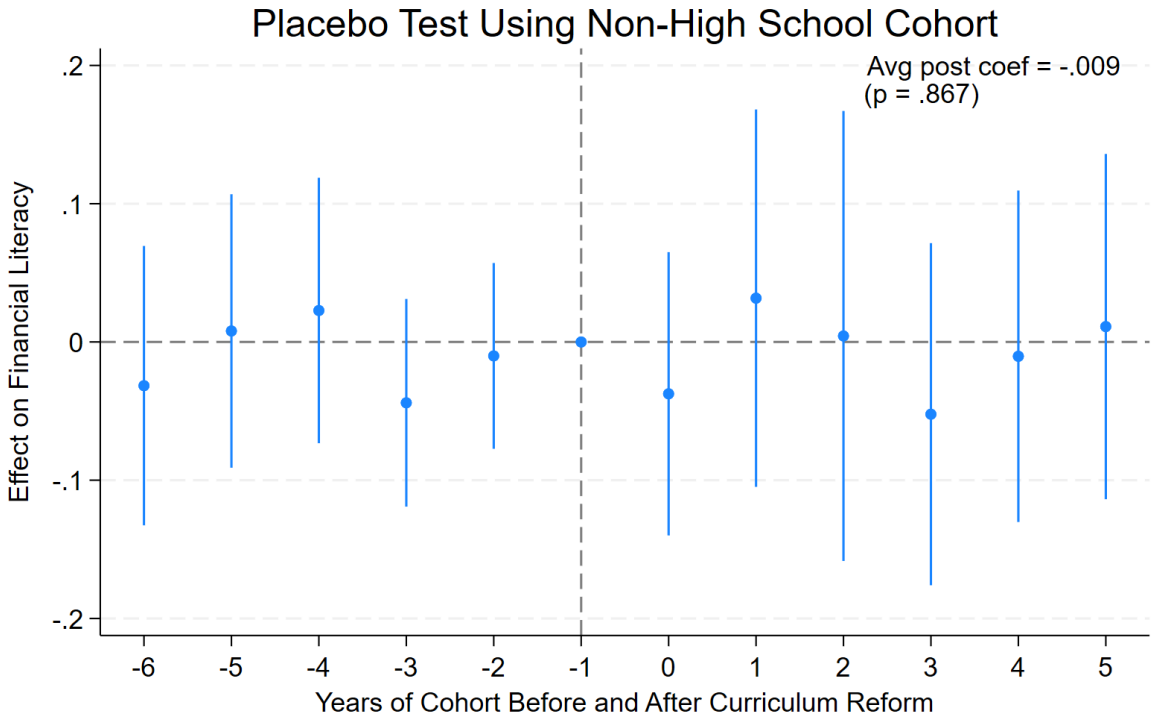
5 Robustness

This section presents additional robustness checks to validate the causal interpretation of the main results. I first implement placebo event-study exercises on individuals who did not attend high school, for whom the reform should have no effect. I then examine whether the observed patterns can be explained by a decline in overall teaching quality rather than by the ideological bias introduced by the reform.

5.1 Placebo Tests

Individuals who did not attend high school were not exposed to the ideological curriculum reform and therefore should not have been affected by it. To verify this, I follow specification (2) and construct a placebo event study for the non–high-school group, assigning them

Figure 4: Placebo Dynamic Impact of Curriculum reform on Financial Literacy



Notes: This figure reports estimates from specification (2) for individuals who did not attend high school. The independent variables of interest are a set of event-time indicators for the number of years before or after the (placebo) curriculum reform date assigned to each individual based on their province and expected schooling trajectory. The dependent variable is the number of financial literacy questions answered correctly. Province and cohort entry year fixed effects are included in all regressions. The lines represent 95% confidence intervals with standard errors clustered at the province level.

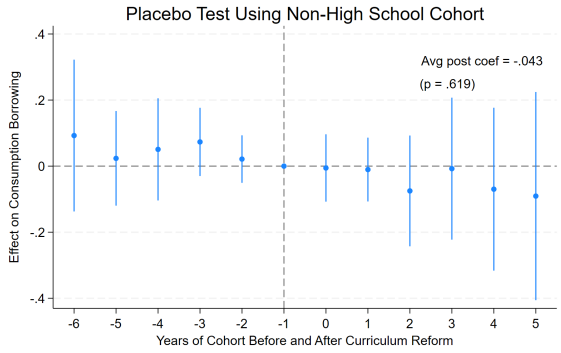
a "pseudo-treatment" date based on their province of residence and age profile.[4]

I first test the placebo effect of the reform on *Financial Literacy*. The regression results are reported in Figure 4. For the non highschool group, the dynamic event-study estimates around the placebo treatment date are statistically insignificant, indicating no detectable pre-trends or post-treatment changes in financial literacy.

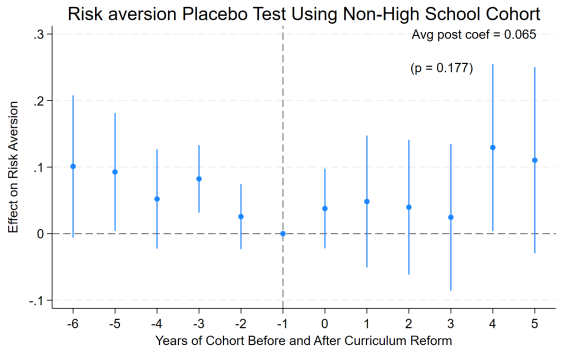
I then perform the same placebo exercise for the other behavioral outcomes, ensuring

[4]For example, suppose the curriculum reform was implemented in province p in 2000, and that a student who graduated from junior high school in 1999 would have entered high school in 1999 and thus been treated in the original sample. For an individual who dropped out after junior high school, I use the expected high-school entry year implied by their age to assign a pseudo-treatment date. For those who did not attend junior high school, such as primary school graduates, I approximate the pseudo-treatment date by adding three years to the age at which they left school. This procedure ensures that the placebo treatment timing is aligned with the hypothetical schooling trajectory of each individual.

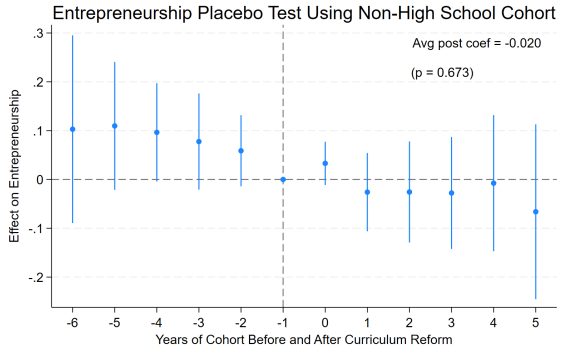
Figure 5: Placebo Event Study Estimates: Non High School Entry Cohort Outcomes



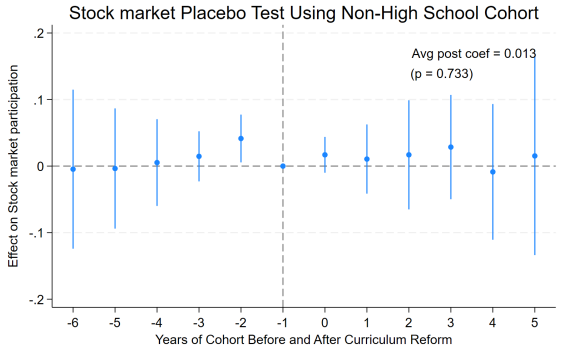
(a) Panel A: Consumption Borrowing Ratio



(b) Panel B: Relative Risk Aversion



(c) Panel C: Entrepreneurship



(d) Panel D: Stock Market Participation

Notes: This figure reports placebo event-study estimates from specification (2) for individuals without a high school education, who were not exposed to the ideological curriculum reform. Each panel corresponds to a separate outcome: consumption borrowing ratio (A), relative risk aversion from lottery choices (B), self-employment status (C), and stock market participation (D). The placebo treatment timing is assigned based on province of residence and expected schooling trajectory same as Figure 4. Province and cohort entry year fixed effects are included in all regressions. The vertical bars represent 95% confidence intervals, with standard errors clustered at the province level.

comparability with the main event-study estimates in Figure 2: *Consumption Borrowing Ratio*; *Relative Risk Aversion*; *Entrepreneurship* and *Stock Market Participation.* The results are summarized in Figure 5. Across all four outcomes, the event study coefficients for the non highschool group remain small in magnitude and statistically insignificant in both the pre and post placebo periods. Individuals who were not exposed to the ideological curriculum reform show no systematic changes in financial behavior around the placebo treatment date, supporting the validity of the identification strategy and alleviating concerns about spurious correlations.

5.2 Ruling Out Alternative Hypotheses: Teaching Quality

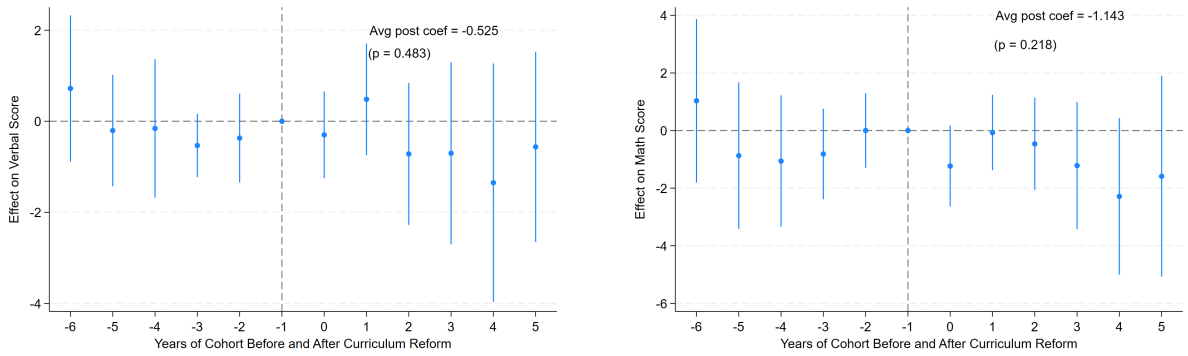
The core argument of this paper is that the ideological curriculum reform lowered students' financial literacy and economic knowledge, thereby shaping long-term preferences and behaviors. A potential competing hypothesis is that the observed decline in financial literacy may instead reflect a general deterioration in academic performance caused by a temporary drop in teaching quality. The reform required teachers to adapt to new curricular content, potentially reducing instructional effectiveness during the transition period.

Two pieces of evidence mitigate this concern. First, the decline in financial literacy is *persistent*, remaining significant for up to six year post-reform cohorts, which is inconsistent with a short-lived disruption effect expected. Second, I empirically test this hypothesis by estimating the impact of the reform on unrelated academic skills verbal and mathematics performance. If the fall in financial literacy were driven by an overall decline in teaching quality, one would expect reductions in both verbal and mathematics scores.

The CFPS survey includes a cognitive test module adapted from the U.S. Health and Retirement Study. All individuals aged ten or above are administered a literacy and a numeracy test during the self-completed individual interview. Both tests follow the psychometric *Guttman scale* framework. In the literacy test, the enumerator presents up to thirty-four Chinese characters in order of increasing difficulty; the test is terminated after three consecutive errors, and the position of the most difficult correctly answered character determines the score. In the numeracy test, the respondent answers up to twenty-four mathematics problems in increasing difficulty; again, the test stops after three consecutive errors, with the score determined by the position of the hardest correctly solved problem.

Figure 6 reports the event study estimates for standardized verbal and mathematics test scores. The coefficients are close to zero and statistically insignificant in all periods, indicating no detectable pre trends or post reform effects on general academic performance. This evidence rules out the alternative explanation that the main results are driven by a broad decline in teaching quality rather than the ideological bias of the reform.

Figure 6: Event Study Estimates: Exam Scores



(a) Panel A: Verbal Scores

(b) Panel B: Mathematics Scores

Notes: This figure shows event study estimates of cohort entry year relative to reform implementation on standardized cognitive test scores. The dependent variable in Panel A is verbal scores and mathematics scores in Panel B. Province and cohort entry year fixed effect are included in regression. The lines are 95% confidence intervals.

6 Conclusion

This paper shows that what schools teach can shape economic knowledge, preferences, and behavior well into adulthood. I study a nationwide reform of the high school curriculum in China that was introduced with an explicit ideological objective and rolled out at different times across provinces. Using cohort by province variation in exposure with difference in difference method, I find that exposure to the revised curriculum reduces financial literacy and is associated with higher relative risk aversion, lower participation in equity markets, and a lower propensity to become an entrepreneur. These results are stable across specifications, they survive placebo exercises on individuals who did not attend high school, and they are not accompanied by declines in verbal or mathematics scores, which argues against a general fall in teaching quality.

The evidence contributes to the debate on the consequences of political socialization in schools. Much of the literature has focused on attitudes and stated beliefs. I extend this line by documenting effects on outcomes with direct welfare content in household finance. Financial literacy is a key input in household decision making. Lower literacy is linked to limited participation, suboptimal risk taking, and foregone diversification. The pattern I find therefore points to real costs for households and to potential losses in aggregate dynamism.

The results also speak to the political economy of curriculum choice. When curricular content is selected with the aim of shaping ideology rather than building general human capital, there can be a trade off between regime objectives and the formation of economically useful knowledge. In the setting I study, textbooks and assessment frameworks place economic facts within a state centered narrative. The findings suggest that such framing can crowd out the acquisition of basic concepts in economics and finance and leave a lasting imprint on behavior that matters for savings, investment, and entry into self employment.

The policy implications are twofold. For education policy, the results underscore that curriculum standards and textbook framing are not neutral. They shape what students learn and how they carry knowledge into markets. For financial education policy, the results suggest that remedial programs should target cohorts and places where ideological framing depressed financial knowledge. School based financial education is often promoted as a tool to improve household decisions. The evidence here shows the mirror image: politicized instruction in basic economics and finance can reduce literacy and depress market engagement.

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