

FAMILY MATTERS: EDUCATION AND THE (CONDITIONAL) EFFECT OF STATE INDOCTRINATION IN CHINA

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Abstract When and how does state indoctrination work? Building upon research on motivated reasoning and family socialization, I argue that only those individuals whose parents have connections to political patronage are subject to state indoctrination because their pro-regime biases transmitted from parents induce higher receptivity prior to government messages. Focusing on political education in China, I conduct a quasi-experimental analysis exploiting the sharp variation in textbook content generated by China's most recent curriculum reform. Results based on a national survey show that the new politics textbooks successfully affected only those individuals whose parents had worked for the government. The finding survives extensive robustness checks and falsification tests. I also consider several alternative explanations of the effects: preference falsification, selective attention, parental indoctrination, and educational quality. This paper not only highlights the role of intergenerational transmission in moderating the effectiveness of state indoctrination but also casts doubt on the actual degree to which regimes can change minds by changing educational content.

Formal education is a central tool for government-sponsored indoctrination. In both democracies and autocracies, political elites choose and adjust educational content to promote state-sanctioned ideologies, increase compliance with authoritarian rule (Lott 1999; Voigtländer and Voth 2015;

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Cantoni et al. 2017; Testa 2018), mold national identity and citizenship (Naval, Print, and Veldhuis 2002; Darden and Grzymala-Busse 2006; Nozaki 2008; Huang 2019), stir up patriotism during political crises (Rosen 1993; Ben-Porath 2007), and in democracies to teach the ideas of civil liberty, procedural fairness, and voting (Niemi and Junn 1998).

How effective is this state indoctrination? A study by Cantoni et al. (2017) uses China's most recent high school curriculum reform to examine the causal effect of a set of new politics textbooks on students' attitudes. The reform brought notable variation in the content of political education offered to students; more importantly, the new curriculum was introduced to provinces in different years, helping researchers account for unobservable cross-cohort and cross-province differences that may otherwise confound the impact of the curriculum change. Cantoni and his colleagues concluded that the new politics textbooks successfully changed students' political attitudes in the direction intended by the Chinese government.

Notably, Cantoni and associates' conclusions (2017) are based solely on a study of students from China's most prestigious university, Peking University. This raises the question of whether their results can be generalized to the effects of the new textbooks on other Chinese students exposed to them. I use a national survey with a more representative sample to answer this question. A wide variety of respondents in the national survey also enable me to evaluate which segments of the population are most susceptible to propaganda. In addition, while most students surveyed in their study were just out of high school, I utilize data mostly composed of university graduates, providing the leverage to examine whether the treatment effect (if any) is sticky over time.

I argue that the receptivity of individuals to government-sponsored indoctrination is conditional on their familial connections to state patronage. Scholars of information processing have claimed that people's susceptibility to persuasion depends on whether the messages tap into their predispositions (Kunda 1990). Political socialization scholarship has also established that parents play a pivotal role in shaping the political predispositions of their children (Hyman 1959). When parents are connected to state patronage, the child is indirectly attached to the regime and possesses higher receptivity *ex ante* to government messages. More generally, family socialization could moderate young people's responses to political propaganda.

I employ a generalized difference-in-differences design that leverages provincial variation in the timing of curriculum reform and cohort variation in new curriculum eligibility. I restrict attention to individuals who started high school around the curriculum reform years. Because these people straddled the period in which the reform was implemented, they were differentially exposed to the textbook content depending on school entry years. I compare attitudes targeted in the new politics textbooks of those who were

just young enough to study the new textbooks and those who were just too old to study them. Under the assumption that the group characteristics are effectively identical, disparity in targeted attitudes across the two groups could be attributable to the curriculum change.

Based on changes in textbook content and data availability, I examine three main political attitudes that the government wished to shape. Results show that the new textbooks affected only those whose parents had worked for the government. Among the government-affiliated students (hereafter affiliated students), those learning from the new textbooks are more inclined than those learning from the old ones to (1) support government intervention in citizen life, (2) accept socialist democracy, and (3) trust government officials. By contrast, the new textbooks had no demonstrable effect on those whose parents are not government employees — the great majority of the population. Indeed, when using the sample where both affiliated and nonaffiliated students are pooled together, the effects are nearly null. My findings are robust to a wide range of additional analyses. Two falsification tests further confirm the identification of the treatment effect. I also discuss four main alternative explanations of the results in this paper.

For the majority of the respondents whose parents are unaffiliated, the textbook change made no difference whatsoever. My finding thus contrasts with the conclusion of [Cantoni et al. \(2017\)](#). Why do the two studies yield different results? One possibility is that Peking University may overrepresent affiliated students because it is the most prestigious university in the country. Indeed, 54 percent of Cantoni's sample had parents in the Chinese Communist Party. A recent survey from Peking University also shows that *cadre* in party-government organs and public institutions has become the most common occupation among the students' parents since 1997 ([Liang and Lee 2012](#)). Add in students whose parents are connected to state patronage and the Cantoni sample becomes something of a complement to the national survey used in this paper. In the concluding section, I discuss two other possible reasons that may explain the different results, including different years of reform covered and effect duration.

When and How Is Government Propaganda Effective?

MOTIVATED POLITICAL REASONING

People process and examine new messages in a biased manner to uphold their political priors ([Lord, Ross, and Lepper 1979](#); [Kunda 1990](#); [Ditto and Lopez 1992](#); [Nickerson 1998](#); [Taber and Lodge 2006](#)). People are less skeptical consumers of a message consistent with their priors: they expend less cognitive effort to evaluate the validity of the message, judge it as

relevant and reliable, and give undue weight to evidence in the message that supports their expectations. By contrast, people allocate more cognitive resources to thinking about a challenging message, focus on its weaknesses, and scrutinize its argument hypercritically.¹ The existence of motivated reasoning suggests that state propaganda may exert its intended effects only among people with pro-regime biases but fail to do so among others.

Although the argument that propaganda is effective among people with higher receptivity *ex ante* is not new, individuals' political predispositions in most prior studies are crudely inferred from macro-level factors, such as political regimes and resident districts (an exception is Peisakhin and Rozenas 2018). For instance, some studies claim that people's past socialization under a certain political regime nurtures their priors against a new regime's propaganda (Geddes and Zaller 1989; Bleck and Michelitch 2017). Other studies show that people born in districts where antisemitism was historically high were particularly susceptible to Nazi indoctrination because of their existing prejudices (Adena et al. 2015; Voigtländer and Voth 2015). In contrast to these studies, I focus on a micro-level factor—individuals' family socialization—that better captures the nature of people's political predispositions.

INTERGENERATIONAL TRANSMISSION OF POLITICAL PREDISPOSITIONS

Families and parents are commonly viewed as the “foremost among agencies of socialization into politics” (Hyman 1959, p. 69). Parents transmit attitudes that they consider valuable for their children to hold, presenting examples or models that children may emulate (Hess and Torney-Purta 1967). When parents have a close link to the polity (in the form of public employment for instance), they more enthusiastically promulgate values that support political authority (Merelman 1980). In addition to value transmission, parents situate their children in a sociopolitical environment where the latter develop attitudes as a result of the life experiences that accompany the inherited environment. The shared environment facilitates parent-child attitudinal similarity (Hout 1984). As far as utility maximization is concerned, people think like their parents politically because they expect to have experiences with the regime similar to those of their parents (Achen 2002).

Parental transmission of political predispositions is a staple in the field of political socialization. Although many studies have shown that parent-child correspondence on political values is more limited than socialization researchers expected, they mostly agree that children's political attachments

1. This does not mean that people never accept persuasion that challenges their priors, but because people react to the messages with excessive skepticism, the messages require stronger and more unanticipated evidence than necessary to induce people to believe them (Chiang and Knight 2011; Huang 2015a).

are highly congruent with those of their parents (Jennings and Niemi 1968; Niemi and Jennings 1991; Alford, Funk and Hibbing 2005; Jennings, Stoker, and Bowers 2009).

COOPTATION IN THE FORM OF POLITICAL PATRONAGE

What kinds of parents are likely to hold pro-regime biases? A large body of literature has shown that elites in developing and authoritarian states can exploit public employment via such methods as controls over recruitment, promotion, and retirement of government posts to create stakeholders in the status quo (Greene 2007; Kitschelt and Wilkinson 2007; Blaydes 2010; Svolik 2012). Public employment is a main channel through which governments allocate state patronage to garner popular support (Gimpelson and Treisman 2002; Calvo and Murillo 2004; Remmer 2007).

The Chinese Communist Party (CCP) uses the *bianzhi* system to control the amount of official employment in managing the scope of state patronage (Burns 2003, p. 777); it comprises all positions officially created. Following previous work, I define government employees as personnel serving in the Party, the government, and public institutions (Ang 2012).² In China, working units in the Party and governmental organs can be divided into core bureaus (*jiguan danwei*) and public institutions (*shiye danwei*). Core bureaus, which are responsible for political, administrative, and regulatory work, have a cluster of public institutions that perform such delegated tasks as administration, provision of public services, and commercial activities.

In sum, I hypothesize that government-sponsored indoctrination influences only those students whose parents are government employees because they are predisposed to accept government messages as a result of intergenerational transmission. Before testing this hypothesis with a rigorous research design, I show supporting evidence to two observable implications of my argument. The first observable implication is that government employees should possess more pro-regime attitudes than nonemployees. Using data from the China Survey 2008, [Supplementary Material Appendix A](#) presents the evidence that government employees are significantly more inclined than nonemployees to trust government officials, feel satisfied with government performance, and feel pride in being Chinese, all things being equal. The other observable implication is that affiliated students should have stronger political attachment to the Party than nonaffiliated students. Focusing on the respondents analyzed in this study who started high school around the curriculum reform years, [Supplementary Material Appendix A](#) shows that affiliated

2. This definition excludes personnel in the military and state-owned enterprises, a practice commonly adopted in existing studies because they are managed differently from public bureaucracies.

students are significantly more likely than nonaffiliated students to submit membership applications to the CCP, viewed as a display of pro-regime bias.

Empirical Strategy

To formally evaluate my hypothesis, I use variation in the content of political education generated by China's most recent high school curriculum reform. I identify the causal effects of political education in high school by comparing attitudes targeted in the new politics textbooks of those who were just young enough to study the new textbooks and those who were just too old to study them. One has no reason to suspect a substantial difference in personal characteristics in these students after accounting for common characteristics of province of origin and cohort. Thus, disparity in targeted attitudes between students studying the old and new textbooks is likely caused by state indoctrination efforts.

POLITICAL EDUCATION IN HIGH SCHOOLS IN CHINA

China's political education in high school is a canonical example of government-sponsored indoctrination, where courses aim to "help students recognize correct values and grasp correct political direction."³ It is part of thought work in schools, aiming to shape the political and social beliefs of students to promote their faith in the CCP leadership and socialist system. Under the current Chinese educational system, high school students are required to complete four political education courses in their first two years of high school, spending two hours a week taking these courses. The four courses are Economic Life, Political Life, Cultural Life, and Philosophy, each taught with a textbook bearing the name of the course as its title and focusing on one specific topic. I refer to these four textbooks as politics textbooks throughout the paper.

THE EIGHTH CURRICULUM REFORM IN CHINA

The Eighth Curriculum Reform, the most recent one, was officially initiated after the Ministry of Education issued its "Outline of Basic Education Curriculum Reform" in 2001. According to this document, the primary goal of the reform is to facilitate the moral and ideological education necessary in the current political, economic climate.⁴ The reform was described by government officials as "historically important" and one of the most significant

3. This quotation is a translated excerpt from a government document: "Curriculum Framework for the Senior High School Politics Subject," <https://tinyurl.com/y6tcsa5n>.

4. Available at http://www.gov.cn/gongbao/content/2002/content_61386.htm.

changes in educational policy since China's economic reform in the late 1970s.

Between 2004 and 2010, the government implemented the curriculum reform, bringing substantial changes in textbook content. Three features make the curriculum reform an appealing case to study the impact of state indoctrination. First, the initial cohort of students studying the new curriculum would have an entirely different three-year curriculum from those who started high school just a year earlier. This also means that the older, pre-reform cohorts of students would not switch to study the new textbooks. This reform thus generates sharp variation in educational content offered to students who started high school around the reform years.

Second, in contrast to educational reforms occurring at a "critical juncture," such as regime change (e.g., from Weimar to Nazi Germany) or political crisis (e.g., after the Tiananmen Incident), which often coincide with other socioeconomic changes that may confound the effect of curriculum reform, the Eighth Curriculum Reform was not followed by any major political changes in China. Third, the Chinese government introduced the new curriculum to provinces during different years.⁵ This incremental approach creates two types of cross-sectional variations—cross-cohort variation within provinces and cross-province variation within cohorts—that enable me to account for cohort-level and province-level differences that may confound the impact of government-sponsored indoctrination.

CHANGES IN TEXTBOOK CONTENT

A text analysis conducted by [Cantoni et al. \(2017\)](#) reveals that the old and new politics textbooks maintain the same core content, but the new ones significantly shift content on (1) Chinese political institutions, (2) Chinese economic institutions, (3) governance, (4) ethnic identity, and (5) the environment. Given the data availability of the national survey used in this study, my analysis focuses on the first three categories. I discuss each of them below.

Chinese political institutions: The new textbooks emphasize teaching students about socialist democracy, whose core element is political participation under the leadership of the Party. The new politics textbooks encourage students to participate in "democratic elections," advocating that citizens exercise their voting rights when they have the opportunity. In addition, the new textbooks intend to draw a line between orderly (i.e., institutionalized participation) and disorderly civil participation (i.e., unfettered political expression), noting that citizens' political lives will become chaotic if they

5. The introduction dates of the new curriculum were not randomly assigned across provinces. I address the potential selection issue in the robustness check section.

ignore the rules, regulations, and procedures put in place by the government. These changes in the content of Chinese political institutions correspond to the changes in word frequency used in the new textbooks: compared to the old textbooks, the term *participation* is used 497 percent more often in the new textbooks. Even greater increases occurred in the frequency of the use of *democracy* (2,057 percent) and *elections* (4,948 percent).

Chinese economic institutions: The new textbooks underscore the importance of socialist market economy for economic and social development. In contrast to a free-market economy, it highlights state intervention in the economy. Many newly added sections deliver the idea that markets are complemented or corrected by government regulation and state institutions. Notably, the new textbooks widely use everyday citizens' personal economic behavior, such as buying goods and working in labor units, as examples to elaborate why state intervention in citizens' personal lives is imperative for the socialist market economy.⁶ Consistent with these changes in textbook content, the term *government* is mentioned 360 percent more times in the new textbooks than in the old ones.

Governance: The new textbooks emphasize institutions that legitimize the Chinese government and its officials, especially adherence to the rule of law and administrative supervision. One of the main objectives of teaching students about the rule of law is to promulgate the virtue of "loving the CCP and the nation."⁷ Numerous added sections in the new textbooks note that government officials exert their power and duties according to the law, providing information about the methods by which citizens can supervise the government's power. Reflecting the revisions, the term *legal institution* is mentioned 497 percent more frequently in the new textbooks than in the old textbooks.

The content changes follow the objectives that the Chinese government outlined in curriculum reform documents.⁸ The content shifts also are consistent with changes that the Chinese government made to the college entrance exam.⁹ [Supplementary Material Appendix B](#) provides numerous translated excerpts from the new politics textbooks to show how the new textbook

6. Scholars of China's curriculum design have noted that a key feature distinguishing the new curriculum from the old one is that the new politics textbooks use examples from citizens' daily experience to increase the relevance of the textbook materials in the eyes of students ([Wang 2008](#)).

7. This objective is noted in a memo by the Ministry of Education available at <https://tinyurl.com/yavvosbk>.

8. [Cantoni et al. \(2017\)](#) in their text analysis show that the language used in a curriculum reform document issued by China's State Council is more prevalent in the new textbooks than in the old ones; the specific terms related to the five categories identified show even sharper changes in prevalence across curricula.

9. [Supplementary Material Appendix B](#) presents a full item-by-item discussion of each category of interest.

content related to the outcome of interest was presented under the new curriculum.

Based on changes in textbook content, I examine three political attitudes: views on state intervention in citizen life, views on democracy, and trust in government officials. If political education works as intended, people studying the new textbooks should be more inclined than those studying the old textbooks to support state intervention in their personal lives (politically and economically). They should also see people's participation in elections as the defining characteristic of democracy in a more affirmative manner. Finally, they should have higher trust in government officials, who are described as adherents of the rule of law and under administrative supervision.

DATA AND VARIABLES

The data used in the analysis derive from the Chinese General Social Survey (CGSS), a nationally representative survey run by the Renmin University in China. The CGSS, a part of the International Social Survey Program, is regarded as one of the most professionally managed surveys in China. Its sampling design is a three-stage stratified design, with county as the primary sampling units, community the secondary units, and household the third-level units. The CGSS uses face-to-face interview; the average interview time is about one and a half hours. I use all the available data from four independent waves of the CGSS, one from each of the following years: 2010, 2012, 2013, and 2015.¹⁰

To implement the identification strategy I discuss below, I analyze only respondents who fulfilled the following criteria. First, their highest level of education completed is at least high school, which removes respondents not attending high school. Second, their high school entry years were close to the years in which the new curriculum was introduced. Third, they either never migrated to other provinces since birth or had migrated to the province in which they currently live before age 15 (i.e., before students begin senior high school). Because the CGSS does not ask where respondents attended high school, I removed those whose migration histories I could not use to infer the places they attended high school.

New Curriculum is the treatment, a binary variable coded as 1 if respondents studied the new politics textbooks and 0 otherwise. Because CGSS does not ask respondents whether they followed the new curriculum or not, I use

10. The analysis begins with the 2010 wave because it is the first round to include a meaningful number of respondents studying the new textbooks. I drop the 2011 wave because it did not ask about the work units of respondents' parents; nor did it measure any outcome variables of interest. The 2014 wave has not been released. The response rate is 74.3 percent for the 2010 wave, 71.5 percent for the 2012 wave, and 72.2 percent for the 2013 wave (the response rate for the 2015 wave is not publicly available). I do not employ weights in the empirical analysis.

respondents' birth year to infer their treatment conditions under the assumption that students start high school at age 15.¹¹ I consider respondents as "treated" if their high school entry year coincides with, or occurs after, the introduction year of the new curriculum. By contrast, if respondents' high school entry year is prior to the introduction year, I consider them as "untreated." Among the 2,092 respondents under analysis, 868 of them were treated (41.5 percent), and 1,224 of them (58.5 percent) were untreated by the new curriculum.

For the outcome variables, *Intervention* measures respondents' attitudes toward state intervention in citizen life. It aggregates three survey questions measured on a five-point Likert scale ranging from completely disagree to completely agree. Specifically, the CGSS asks respondents how much they agree with the following statements:

- When an individual criticizes the government in public, the government should not intervene.
- How many children people want to have is a personal matter; the government should not intervene.
- People have the freedom to decide where to work and live; the government should not intervene.

These items indicate different aspects of state intervention in citizen life, including people's political (the first item) and economic lives (the second and third items).¹² Together, this index provides a comprehensive indication of people's general views on state intervention in their personal lives.¹³

The next variable is *Democracy*, which measures respondents' views on democracy. It is operationalized using the following question: a political system can be considered a democracy as long as citizens have the right to elect their representatives, who discuss critical national and local issues on

11. In the Chinese education system, high school or secondary education is intended for students aged 15 and 18. Granted, not every student starts high school at age 15, but because the CGSS does not ask respondents when they started high school, this assumption is necessary for my analysis. I show in the robustness check section that my results seem not sensitive to this assumption.

12. The birth planning program and the household registration system in China are both important measures that the government uses to serve its economic objectives and macroeconomic control. Thus, the second and third questions are well suited to measure attitudes toward the role of the government in the economy.

13. The Cronbach's alpha estimate is 0.48, which may be acceptable given the limited number of items. For transparency, I report the results using the three items separately in the [Supplementary Material, Appendix C](#). Results do not qualitatively change—the estimates are all in the right direction and follow a consistent pattern. I also reestimate the baseline model by using principal component analysis and find that the results are robust.

behalf of citizens. This variable is coded as 1 if respondents agreed with this statement and 0 if not. The next variable is *Trust*, which measures respondents' trust in government officials on a four-point Likert scale ranging from a great deal to not at all. It is operationalized using the following question: To what extent do you trust local officials? I recode these variables in a way that a higher value means greater consistency with the content of the new textbooks. I standardize each outcome variable for ease of comparison.

Because the CGSS may change questions across survey rounds, different rounds may be used to test disparate outcome variables. Specifically, the *Intervention* questions were asked in all four waves. The *Democracy* question was measured only in the 2013 wave. The *Trust* question has fewer observations because only a subset of respondents in the 2012 wave was asked the question.¹⁴

For *Affiliated Student*, I use the survey item asking the work units of respondents' parents when the respondents were 14. I define those whose parents (either or both of them) had worked in the Party, the government, or public institutions as affiliated students. If neither of their parents worked in those units, I define them as nonaffiliated students. Notably, public institutions in China can be fully or partially state funded or wholly self-funded; that is, not all public employees in public institutions are on the official state payroll. I consider working in the public institutions as working for the government only if the units are at least partially state funded. In total, 11.4 percent of the respondents are affiliated students. Among these students' parents, 10.7 percent worked in party-government organs and 89.3 percent in public institutions. [Table 1](#) presents summary statistics of the main variables.

The fact that respondents cannot preselect parents employed by the state lessens many sorts of confounders that would affect their political predispositions. In addition, if I follow a common practice used in prior studies that relies on survey items directly asking respondents' political predispositions, a concern is that people's current attitudes almost inevitably affect how they answer these questions. This issue is problematic when respondents' recall of predispositions was influenced by their treatment conditions because having this variable in the model raises red flags associated with posttreatment biases in what aims to be a causal analysis. By contrast, 14-year-old respondents' treatment conditions cannot affect their parental occupations.

14. [Supplemental Material Appendix C](#) provides the information about which questions were asked in each wave and the associated number of observations. It also presents additional analyses regarding *Intervention* to address the concern about changes in sampling across waves. For example, I restrict each model to the smallest number of observations across indicators in that wave. I also include survey year fixed effects in the model to account for time-varying factors that may affect respondents differently across waves.

Table 1. Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	<i>N</i>
Treated (New Curriculum = 1)	0.42	0.493	0.00	1.00	2,092
Affiliated Students (Yes = 1)	0.11	0.318	0.00	1.00	2,092
Intervention (Standardized)	0.00	1.000	-2.48	2.72	2,075
Intervention (Unstandardized)	8.73	2.306	3.00	15.00	2,075
Democracy (Standardized)	0.00	1.000	-2.18	0.46	536
Democracy (Unstandardized)	0.83	0.379	0.00	1.00	536
Trust (Standardized)	0.00	1.000	-1.81	1.99	275
Trust (Unstandardized)	2.43	0.791	1.00	4.00	275
Gender (Male = 1)	0.50	0.500	0.00	1.00	2,092
Ethnicity (Han = 1)	0.94	0.238	0.00	1.00	2,090
Height (in centimeters)	167.70	7.986	120.00	192.00	2,090
Residence (Rural = 1)	0.22	0.413	0.00	1.00	2,092
Education	8.93	2.584	5.00	13.00	2,086
Father in CCP	0.36	0.480	0.00	1.00	2,071
Mother in CCP	0.30	0.457	0.00	1.00	2,074
Father Education	4.99	2.310	1.00	13.00	2,063
Mother Education	4.37	2.228	1.00	13.00	2,061

NOTE.—Data come from the Chinese General Social Survey.

IDENTIFICATION STRATEGY

Using provincial variation in the curriculum reform and cohort variation in new curriculum eligibility, I estimate a generalized difference-in-differences model as follows:

$$Y_{icp} = \sum_c \gamma_c + \sum_p \delta_p + \beta_1 \text{New Curriculum}_{cp} + \beta_2 \text{Affiliated Student}_{cp} + \beta_3 (\text{New Curriculum}_{cp} \times \text{Affiliated Student}_{cp}) + \epsilon_{icp}, \tag{1}$$

where Y_{icp} is an individual survey question (i denotes the individual, c the high school entry cohort, and p the province of high school attendance). γ_c and δ_p are full sets of cohort and province fixed effects. The coefficient β_1 captures the treatment effect among nonaffiliated students, conditional on fixed differences across cohorts and provinces of origin. The coefficient β_2 captures the conditional expected values of y among affiliated students who studied the old textbooks; β_3 indicates by how much the effect of the new textbooks changes when respondents are affiliated students. The error terms, ϵ_{icp} , are clustered at the province \times cohort level to account for correlated disturbances across individuals within a province \times cohort cell (the level at which the curriculum varies).

This model can then be used to calculate the marginal effect of the new textbooks on the outcome variables of interest:

$$\frac{\partial(\textit{Targeted Attitude})}{\partial(\textit{New Curriculum})} = \beta_1 + \beta_3 \textit{Affiliated Student} \quad (2)$$

Unlike in purely linear models, β_1 has to be interpreted as a conditional coefficient representing the effect among nonaffiliated students (i.e., *Affiliated Student* = 0). Conversely, the sum of β_1 and β_3 captures the effect among affiliated students (i.e., *Affiliated Student* = 1). I use listwise deletion to address missing values because the magnitude of missingness is small and will show that my results are robust to imputed data in the robustness check section.

To augment the causal inference, I focus on respondents whose high school entry year occurred around the curriculum reform. In the analysis, all respondents come from four cohorts of students who entered high school around the reform year from each side of the curriculum.¹⁵ Table 2 describes the birth cohorts of students under analysis and their treatment conditions. Granted, narrowing the bandwidth would avoid more potential biases from selection for treatment, yet because my main hypothesis involves two levels for two factors (i.e., curriculum version and familial ties), narrower bandwidth will result in fewer observations for each combination of the two factors. In the robustness check, I show that the results are insensitive to bandwidth selection.

This identification strategy addresses various methodological concerns. First, province-level differences could confound the influences of the new curriculum because they are likely correlated with people's attitudes. Pan and Xu (2017), for example, find that regional economic composition, including trade openness and urbanization level, correlates with Chinese citizens' ideologies; however, these provincial differences cannot drive my results because I control for province fixed effects and exploit cross-cohort variation *within* provinces.

Second, time-varying provincial heterogeneity could be another concern; for example, differences in economic growth rates across provinces may differentially affect provinces and thus bias the impact of the new curriculum. However, most province-specific, time-varying factors seem unlikely to have very different effects across the neighboring cohorts within a province

15. To illustrate, I use Shandong as an example. Because the new curriculum was introduced in 2004, the first entry cohort of students receiving the new textbooks comprised those born in 1989, and the last entry cohort of students receiving the old textbooks comprised those born in 1988. For Shandong, the analysis focuses only on the cohorts of students born between 1985 and 1992, defining those born between 1985 and 1988 as the control group (the last four pre-reform cohorts) and those born between 1989 and 1992 as the treatment group (the first four postreform cohorts).

Table 2. Reform years across provinces and cohorts under analysis

Years	Provinces under reform	Cohorts analyzed
2004	Shandong, Ningxia, Hainan, Guangdong	1985–1988 (C) 1989–1992 (T)
2005	Jiangsu	1986–1989 (C) 1990–1993 (T)
2006	Tianjin, Zhejiang, Fujian, Anhui, Liaoning	1987–1990 (C) 1991–1994 (T)
2007	Hunan, Jilin, Shaanxi, Heilongjiang, Beijing	1988–1991 (C) 1992–1995 (T)
2008	Shanxi, Jiangxi, Henan, Xinjiang	1989–1992 (C) 1993–1996 (T)
2009	Hebei, Hubei, Yunnan, Inner Mongol	1990–1993 (C) 1994–1997 (T)
2010	Sichuan, Gansu, Guangxi, Qinghai, Tibet, Chongqing, Guizhou	1991–1994 (C) 1995–1998 (T)

NOTE.—Year refers to dates of introduction of the new textbooks. Provinces refer to the locations in which the reform was implemented. Cohorts Analyzed describes the treatment conditions of the birth cohorts analyzed, with C denoting control group and T denoting treatment group.

because the cross-cohort variation exploited falls within a narrow window (i.e., people who entered high school around the reform year). In the robustness check, I employ a tighter identification to address the unobserved province and cohort covarying characteristics.

Third, the natural evolution of attitudes across cohorts of students may explain attitudinal differences between treated and untreated students even in the absence of the new textbooks. This concern, however, is alleviated by including cohort fixed effects in the model that can zero out cross-cohort changes in attitudes. Fourth, one could argue that the curriculum reform might accompany other policies introduced to a reformed province; the policies could bias the results if they differentially affected the treated and untreated respondents. I consulted newspapers and no such policy exists.

Results

I first show that respondents under analysis who followed the old and new textbooks are statistically indistinguishable. Because respondents studying

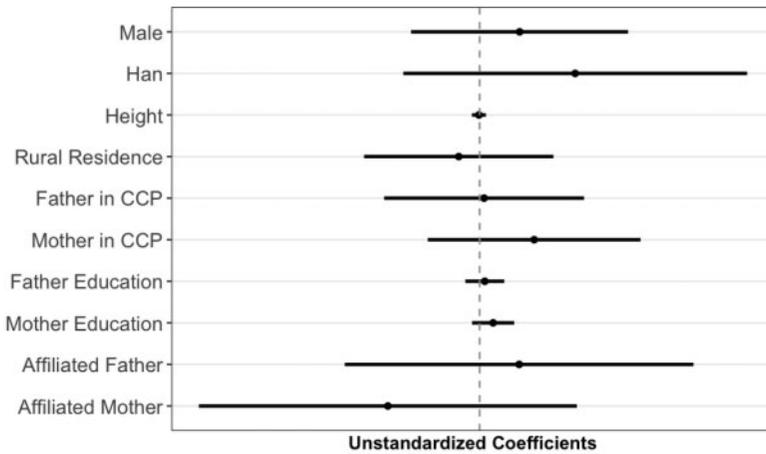


Figure 1. Balance test. This coefficient plot shows the OLS estimates of personal characteristics on exposure to the new curriculum. The dots represent the point estimates and the bars 95% confidence intervals. All regressions include a full set of province and cohort fixed effects. $N = 2,026$.

the new textbooks are younger and come from provinces where the new curriculum was introduced earlier, I include province and cohort fixed effects in the balance check to account for common characteristics in the province of origin and average characteristics of a cohort. [Figure 1](#) shows that the treated and untreated respondents are similar in a battery of personal characteristics. [Supplementary Material Appendix D](#) reports the estimates.

The main result supports my argument that the effectiveness of state indoctrination is conditional. [Figure 2](#) reports the estimated marginal treatment effects for affiliated and nonaffiliated students with corresponding confidence intervals. It shows that the new textbooks successfully affected only affiliated students. Compared to the untreated affiliated students, affiliated students exposed to the new textbooks exhibit more positive attitudes toward government intervention in citizen life ($p \leq 0.05$), view participation in elections as a defining characteristic of democracy ($p \leq 0.05$), and feel greater trust in local officials ($p \leq 0.1$). By contrast, the new textbooks did not affect nonaffiliated students in the same manner. Among these students, those studying the new textbooks seem indistinguishable across the three political attitudes that the government aimed to change from those studying the old textbooks. The estimates for *Democracy* and *Trust* even have a wrong sign.

Moreover, the estimates of the interaction variable are statistically significant at the 0.1 level (see the [Supplementary Material Appendix D](#)), meaning that the treatment effects on affiliated students seem larger than those on nonaffiliated students. Note that my argument about the conditional

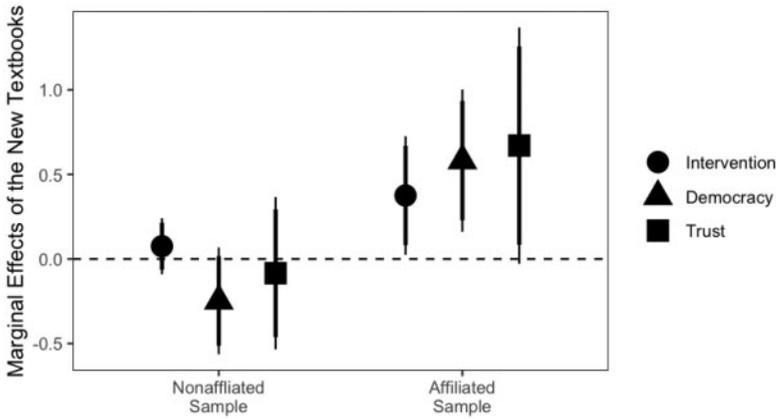


Figure 2. Marginal effects of the new textbooks. This plot shows the effects of the new textbooks on the targeted attitudes by affiliation status. The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All regression coefficients account for province and cohort fixed effects. Standard errors are clustered at the province \times cohort level. $N = 2,075$ (Intervention); $N = 536$ (Democracy); $N = 275$ (Trust).

indoctrination does not necessarily mean that the new textbooks had larger effects on affiliated students than on nonaffiliated students, yet such evidence bolsters my argument that the effectiveness of state indoctrination depends on receivers' familial connections to the regime.

One should also expect that the overall effects of the new textbooks are small because the vast majority of the respondents analyzed in this paper are nonaffiliated students. This expectation is consistent with Huang (2015b), who finds that political education in Chinese colleges does not indoctrinate students. To test this claim, I remove the conditioning variable and the interaction term from the baseline model, which allows me to interpret the regression coefficients associated with *New Curriculum* as the average effects of the new textbooks on students' attitudes.

The OLS estimates in figure 3 support my expectation, showing that overall the new textbooks had no demonstrable effect on people's political attitudes. In addition, the coefficient signs are unstable: although the estimate for *Intervention* and *Trust* is positive, it is negative for *Democracy*. In short, I find that the new textbooks did not persuade the majority of people in a manipulative fashion, casting doubt on the actual degree to which the Chinese regime can change minds by changing school content. [Supplementary Material Appendix D](#) presents the full estimates.

My finding that indoctrination works only for children of regime stakeholders is important. One might contend that these students may have been

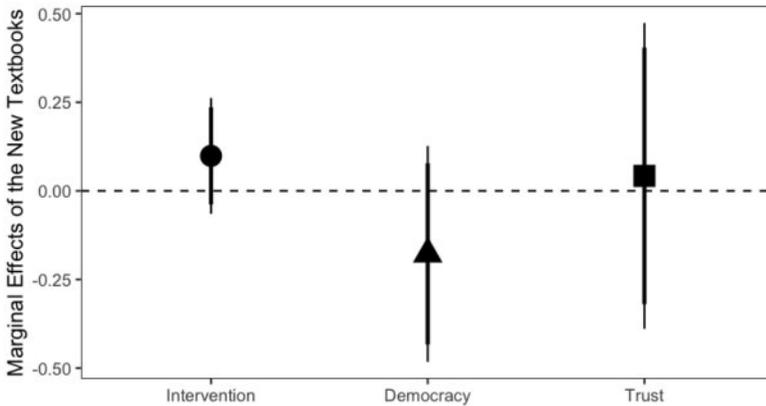


Figure 3. Average treatment effects. This plot shows the estimated effects of the new textbooks using the pooled sample. The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All coefficients are OLS estimates and account for province and cohort fixed effects. Standard errors are clustered at the province \times cohort level. $N = 2,075$ (Intervention); $N = 536$ (Democracy); $N = 275$ (Trust).

fairly aligned with the government in attitudes that the Chinese authorities attempt to change, so making them more aligned with the government seems unimportant; yet the data show that the untreated affiliated students have significantly *lower* scores on the three targeted attitudes than the untreated non-affiliated students (not reported). The benchmark differences indicate that my results are consequential.

FALSIFICATION TESTS

To corroborate the treatment effect identification, I conduct two falsification tests. First, I reanalyze the data using attitudes not targeted in the new textbooks. Because the placebo attitudes were not what the new textbooks aimed to change, one should observe no effect. I select five attitudes related to trust (ordinary people, relatives, neighbors, bank staff, and journalists). Panel (a) in [figure 4](#) shows that the new textbooks had no impact on the placebo attitudes, even among the affiliated student sample where I find indoctrination works. Second, I reanalyze the data by moving the introduction dates of the new curriculum three years before its actual dates. That is, the last three cohorts of students studying the old textbooks in real life are considered here as the first three cohorts studying the new textbooks. Under the placebo reform years, none of the cohorts of students analyzed was exposed to the new

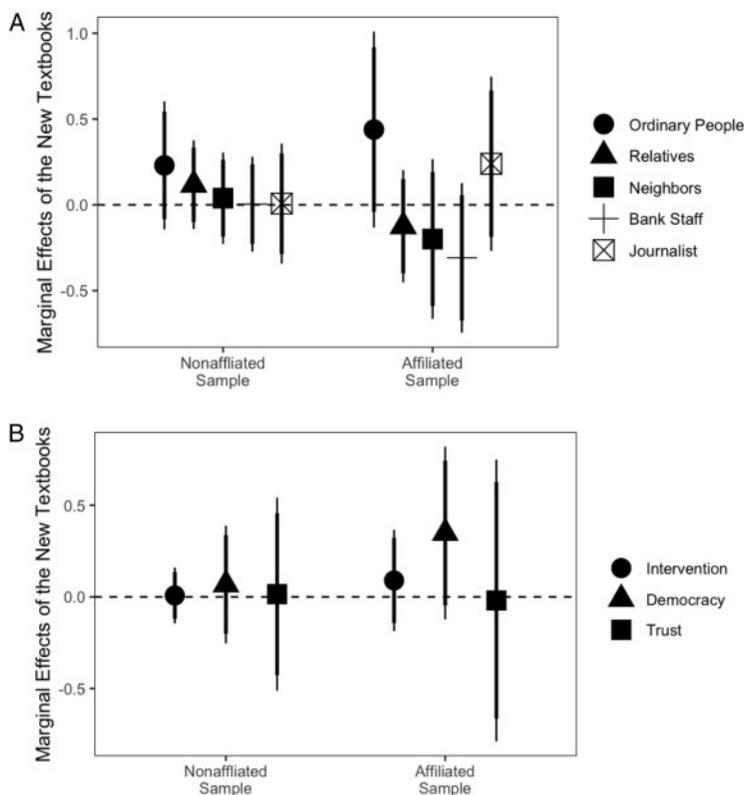


Figure 4. Falsification tests. Panel (a) shows the effects of the new textbooks on nontargeted attitudes. Panel (b) presents the effects of the new textbooks using placebo reform. The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All regression coefficients are OLS estimates and account for province and cohort fixed effects. Standard errors are clustered at the province \times cohort level. [Supplementary Material Appendix D](#) reports the regression coefficients.

textbooks, so no effect should occur. Panel (b) in [figure 4](#) shows that the effects identified previously disappear in the falsification test.

ROBUSTNESS CHECK

I conduct seven sets of additional analysis to show that my results are robust.¹⁶ First, I examine whether the results are sensitive to the assumption

16. In [Supplementary Material Appendix E](#), I use tables to report the full results based on the baseline model and coefficient plots to show the marginal treatment effects by affiliation status.

that students start high school at age 15. Second, I address the concern that parental occupation is endogenous. Third, I reanalyze the data by controlling for individual-level covariates. Fourth, I address the concern that the introduction dates of the new curriculum were not randomized across provinces. Fifth, I reanalyze the data by taking province-specific, cross-cohort trends into account. Sixth, I use a more demanding model to address the unobservable province and cohort covarying characteristics that could bias the results. Seventh, I reanalyze the data using multiple imputed data for missing values. [Supplementary Material Appendix E](#) discusses these analyses in more detail and reports the results.

Alternative Explanations

I discuss four alternative explanations of the effects. First, affiliated students may tend to falsify their preferences ([Jiang and Yang 2016](#); [Truex and Tavana 2019](#)): they may be more likely than nonaffiliated students to express politically correct views as suggested by the new textbooks. If so, the effects are detected even when affiliated students are not truly indoctrinated by the textbooks' content. Second, affiliated students may pay better attention to political education or are more academically competent than nonaffiliated students. The effects may thus reflect the fact that they memorize lines from the content of the new textbooks better rather than believe it.

Third, a complementarity may exist between family influence and school education: the new textbooks alone may not be able to affect affiliated students unless the textbooks' content is complemented by family influence. For example, state-related parents may discuss with their children the exact same lessons the new textbooks purposely emphasized, "teaching" the textbook content outside the classroom. The content of the new textbooks is reinforced by such family influence to indoctrinate affiliated students.¹⁷ Fourth, the introduction of the new curriculum was likely accompanied by better educational resources allocated to students, such as improved textbook quality, teaching practice, and school spending. Consistent with the results, the textbook effects can be conditional on predispositions because affiliated students may respond more positively than nonaffiliated ones to government actions that benefit them. If so, the

17. Although I cannot rule out this explanation directly, I have suggestive evidence to refute another related claim: affiliated students may have been affected by the new curriculum in the family before they attend high school and thus the results may be better interpreted as the effects of "new curriculum parents," not the new curriculum per se; yet if the effects are based on pre-high school indoctrination, affiliated and nonaffiliated respondents who did not attend high school around the curriculum reform years should express different targeted attitudes. Results reported in table SI-15 do not support this conjecture.

effects may reflect changes in educational quality rather than changes in educational content.

The first three alternative explanations put a slightly different spin on the interpretations of the effects but may not change the treatment effect identification per se. By contrast, if the fourth alternative explanation is true, my estimates could be biased because it suggests that a confounder (i.e., educational quality), rather than the claimed treatment (i.e., educational content), caused my results. Two analyses show that this alternative explanation is unlikely to explain my finding.

I first reanalyze the data by controlling for provincial spending on secondary education at the province-cohort level (calculated as a province's average level of spending during the three years of senior high school for each cohort). [Figure 5](#) shows that the estimates controlling for school spending are nearly identical to the baseline results. Even if school spending may have been greater for the cohorts in provinces exposed to the new curriculum, better educational quality accompanying the curriculum reform does not drive the result.

To complement the quantitative evidence, I also conducted semi-structured interviews with high school teachers and graduates from Beijing, Fujian, and Sichuan in China. My interviews reveal that students always get brand-new textbooks each year even in the old design. This is true in all urban and rural areas; the only exception is perhaps extremely poor regions. In other words, textbooks in the old design do not mean that they are dilapidated books; nor did the teachers think teaching practices were substantially changed as a result of the reform because teachers' and students' incentives were still directed toward the memorization of textbook content to succeed in the college entrance exam. This view concurs with observations from many Chinese scholars of curriculum design and public education ([Guo 2010](#); [Mao 2018](#)). The qualitative evidence could alleviate the concern that improved textbook quality and teaching practice may confound the effect of the curriculum change.

Conclusion

I find consistent evidence that the new politics textbooks worked only among respondents whose parents had been employed by the government, but not on the vast majority of the respondents. In light of the findings of [Cantoni et al. \(2017\)](#) that the new politics textbooks were successful in shifting people's political attitudes in an intended direction, the results of this paper seem to disagree.

My results, however, are fairly compatible with theirs once taking their survey respondents' family backgrounds into consideration. Their student sample comes from Peking University (PKU), in which the most common occupation type among the students' parents since 1997 is cadres (*ganbu*) in

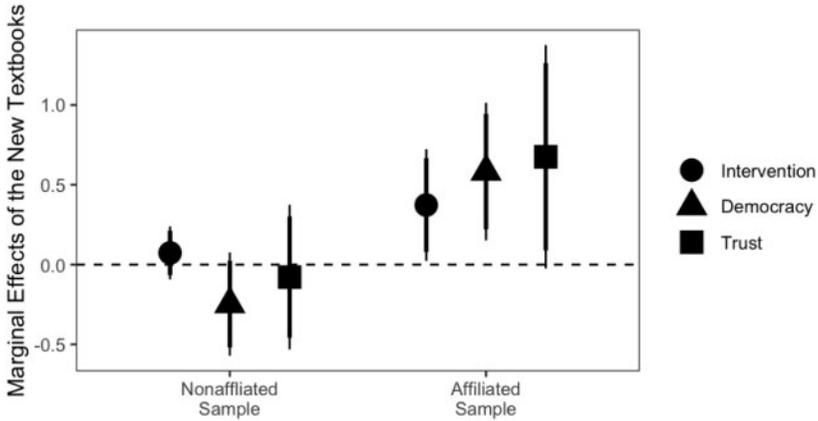


Figure 5. Marginal effects of the new textbooks controlling for spending on secondary education at the province \times cohort level. The bullet symbols represent the standardized coefficients and the bars 95% (90%) confidence intervals. All regression coefficients include a full set of province and cohort fixed effects. Standard errors are clustered at the province \times cohort level.

party-government organs and public institutions (Liang and Lee 2012).¹⁸ Because many parents of PKU students have close ties to state patronage, their sample seems likely to show the effect of the new textbooks. My finding not only corroborates their study but places important scope conditions on their conclusions. In doing so, it casts doubt on the idea that the Chinese regime can change minds by changing school textbooks on a large scale.

Besides the different sample adopted, two main differences between this study and that of Cantoni and his colleagues may explain why I find no effect on the general population. The first pertains to the difference in reform years covered: the Cantoni sample covers only students from provinces where the reform occurred in 2007, 2008, and 2009 because they surveyed only students in college. This study, by contrast, covers the entire time period of the reform. If provinces that introduced the new curriculum later did better in the implementation of the reform because they learned from the provinces that implemented earlier, the Cantoni sample may have much leverage to find the effect. I reanalyze the data with only these latecomer provinces but still find no effect on the general population. The second reason relates to the duration of effects: because Cantoni and his colleagues

18. According to their data, 39.8 percent of PKU students' parents from 1995 to 1999 were cadres in party-government organs and public institutions. Only 1.7 percent of the Chinese population were cadres in these work units in 2000.

surveyed only college students, the case could be made that an effect for respondents in college indeed exists and that the effect decreases over time for those out of college for longer periods of time. Given the data at hand, I cannot evaluate this hypothesis. The only statement I can make is that the effect on those who have familial ties to the regime seems lasting over time.¹⁹

One limitation of this paper is that the CGSS does not include items measuring respondents' family dynamics that would affect transmission rates. According to the extant literature, the transmission of political predispositions from parents to children is higher in families where political discussion among family members is frequent (family politicization). Parent-to-child transmission rates are also higher when the family forms a collective body, not a mere assemblage of fortuitously related individuals (unity). This paper is focused primarily on the direction of political attachments (i.e., attached to the CCP), but the intensity of political attachments resulting from family politicization and unity could also be relevant. I leave this empirical inquiry to future researchers.

Despite limitations, this paper contributes to existing scholarship in three chief ways. First, my finding is not only consistent with new studies showing that hard propaganda has a limited effect on attitudes (Bush et al. 2016; Huang 2015b, 2018; Selb and Munzert 2018) but sheds light on what segment of the population on which propaganda works best. Second, my finding highlights the importance of people's familial ties to the regime in moderating their responses to propaganda, suggesting that the political contexts in which people were socialized may affect their receptivity to propaganda. Future research can further examine the micro-level mechanisms underlying the results.

Finally, my finding suggests an alternative account for reasons that authoritarian regimes still engage in propaganda when most citizens seem not to believe it. The prevailing view is that propaganda signals government strength in maintaining social control and political order (Huang 2015b; Wedeen 1999). I add that political elites invest resources in state indoctrination because it could reinforce existing believers. This claim also speaks to a new body of research on the presence of propaganda as a strategy of control (Chen and Xu 2015; Guriev and Treisman 2015). My finding implies that state indoctrination could make a regime durable not because it expands the

19. This claim is based on the fact that many respondents in this study have graduated from college. I further explore this issue by focusing on the early cohorts studying the new textbooks. Results based on the sample with cohorts just one year above and below the reform year show that the effects are still discernible among the affiliated students who constitute the earliest cohort in their province to study the new curriculum. This is stronger evidence of the duration of treatment effect. [Supplementary Material Appendix E](#) reports the results.

popular base of the regime but because it consolidates those already aligned with the regime.

Data Availability Statement

REPLICATION DATA AND DOCUMENTATION are available at <https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/RVQ248>.

Supplementary Material

SUPPLEMENTARY MATERIAL may be found in the online version of this article: <https://doi.org/10.1093/poq/nfab007>.

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