

Does Knowledge of Constitutional Principles Increase Support for Civil Liberties? Results from a Randomized Field Experiment

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For decades, scholars have argued that education causes greater support for civil liberties by increasing students' exposure to political knowledge and constitutional norms, such as due process and freedom of expression. Support for this claim comes exclusively from observational evidence, principally from cross-sectional surveys. This paper presents the first large-scale experimental test of this proposition. More than 1000 students in 59 high school classrooms were randomly assigned to an enhanced civics curriculum designed to promote awareness and understanding of constitutional rights and civil liberties. The results show that students in the enhanced curriculum classes displayed significantly more knowledge in this domain than students in conventional civics classes. However, we find no corresponding change in the treatment group's support for civil liberties, a finding that calls into question the hypothesis that knowledge and attitudes are causally connected.

For more than a half-century, research on tolerance has documented the powerful correlation between education and support for civil liberties.¹ Since the 1955 publication of Stouffer's *Communism, Conformity, and Civil Liberties*, this topic has attracted attention from researchers in multiple disciplines and has influenced both theory and policymaking. Using a variety of measurement strategies, researchers have consistently found that willingness to extend civil liberties to unpopular target groups increases with formal education.² Researchers have typically interpreted this correlation in causal terms, claiming that more education leads people to appreciate and embrace principles and policies that protect civil liberties.

Most accounts suggest one of three potential causal mechanisms through which education may

increase tolerance. One school of thought attributes increased tolerance to the cognitive skills developed through education (Bobo and Licari 1989; Prothro and Grigg 1960). Proponents of this view, such as Nie, Junn, and Stehlik-Barry, argue that "formal education encourages cognitive development and enables citizens to understand the long-term trade-offs necessary in democracy" (1996, 6). A second explanation attributes education's influence to students' exposure to intellectual and social diversity in school. Stouffer, for example, argues that "schooling puts a person in touch with people whose ideas and values are different from one's own" (1955, 127). This explanation implies that the social and cultural milieu of formal education contributes to willingness to extend civil liberties to controversial groups or activities. Finally, and most importantly for our

¹We follow Stouffer (1955) and Gibson (1992) in defining tolerance as support for civil liberties.

²Support for civil liberties in the abstract is typically quite high, but support for civil liberties in situations involving unpopular minority groups is much lower, and it is this second type of support on which the tolerance literature focuses (Prothro and Grigg 1960, 284–86). Both types of support for civil liberties are correlated with formal education.

purposes, there is the claim that education increases tolerance by augmenting students' political knowledge. Some studies emphasize the impact of accurate factual knowledge on attitudes (Galston 2007; Peffley and Sigelman 1990; Sniderman, Brody, and Tetlock 1991), while others emphasize awareness of democratic norms and procedures (Delli Carpini and Keeter 1996; Galston 2001, 2007; McClosky and Brill 1983). The acquisition of knowledge is by far the most commonly cited causal mechanism; even studies that emphasize the importance of cognitive skills or the experience of diversity acknowledge the mediating role of political knowledge (Nie, Junn, and Stehlik-Berry 1996; Stouffer 1955).³

The correlation between education and support for civil liberties is consistently positive and strong. However, before attaching a causal interpretation to this correlation, one must address the possibility that unobserved factors may influence both years of schooling and levels of support for civil liberties. Although most researchers propose one or more causal mechanisms through which education may affect support for civil liberties, no studies have tested these mechanisms convincingly. Some simply assume that more years of schooling lead to greater knowledge, greater cognitive sophistication, or exposure to diverse ideas (Nunn, Crockett, and Williams 1978; Prothro and Grigg 1960; Stouffer 1955). Others adduce additional survey evidence to show a correlation between the specified mechanism and tolerance. For instance, both Bobo and Licari (1989) and Nie, Junn, and Stehlik-Berry (1996) use a vocabulary test to measure cognitive skills, while McClosky and Brill (1983) and Peffley and Sigelman (1990) incorporate measures of political knowledge into their studies. Although these correlations are suggestive, there remains the question of whether formal schooling causes greater cognitive acuity and political knowledge or whether, instead, formal schooling is

a marker for unmeasured attributes that are associated with cognitive skills and political awareness.⁴

Also relevant to understanding the relationship between education and tolerance is the literature on civic education. Whereas the aforementioned studies tend to examine the effects of education as measured by years of schooling, the civic education literature examines the impact of specific curricula. Here, too, the question of causality has been addressed primarily with nonexperimental data. To our knowledge, apart from Phillips's (2003) small experimental study of a Long Beach high school,⁵ all of the studies on civic education use observational research designs (see, for example, Litt 1963; Langton and Jennings 1968; Niemi and Junn 1998; Stroupe and Sabato 2004). Studies such as Gimpel, Lay, and Schuknecht (2003) and Campbell (2005) rely on cross-sectional surveys to draw causal inferences about the effects of past exposure to civics coursework or about the causal pathway by which education more generally affects political tolerance (Delli Carpini and Keeter 1996). These cross-sectional studies rely on strong *ceteris paribus* assumptions when comparing people who were and were not exposed to information. The same is true for studies such as Brody (1994), which do not measure students' knowledge or attitudes prior to exposure to a specific civics curriculum and therefore must rely on strong assumptions about the comparability of students who take different classes, often in different schools. Even studies that use a panel survey design (Hartry and Porter 2004) must still make strong assumptions about the over-time trajectories of students who attend different schools or different classes within schools. The conclusions drawn by these studies are not necessarily incorrect, but the potential for bias remains.

In an effort to overcome the uncertainties of drawing inferences from observational data, the current study implements an experimental research design. By randomly assigning the curricula to which students are exposed, we eliminate sources of bias that arise when students are sorted into classrooms or

³Many accounts make reference to more than one of these mechanisms; for example, Nunn, Crockett, and Williams (1978, 75) contend that "higher education affects political tolerance primarily through increasing people's cognitive skills, knowledge, and cultural sophistication," and Campbell (2006, 122–24, 198–99) maintains that a heterogeneous social context and schools' explicit teaching of tolerance as a norm both lead to more tolerant attitudes. On the other hand, Sullivan, Piereson, and Marcus (Chapter 5, 1982) have argued that some of the apparent statistical relationship between education and tolerance is spurious, reflecting the fact that those with higher levels of education tend to feel less threatened by the particular political groups that have traditionally been mentioned in surveys on tolerance.

⁴Dee (2004) uses child labor laws as an instrumental variable for years of education in order to gauge the impact of schooling on political tolerance. However, the observed effect may be an artifact of unmeasured factors that are related to both labor laws and levels of tolerance.

⁵Phillips utilized a randomized design to study the impact of extracurricular clubs and a civic practicum on the civic engagement of high school students. Two experiments were conducted based on a small number of classrooms, and no statistically significant impacts were found on either general political knowledge or tolerance of diversity.

schools. The present study represents the first successful attempt to conduct a large-scale randomized experiment that directly tests the causal impact of education on political knowledge and support for civil liberties. Our experiment involves roughly one thousand high school students, 59 classrooms, and 10 public schools of widely varying socioeconomic characteristics. Not only is our sample size large enough to draw reliable conclusions, but the variety of schooling environments also gives us a strong case for external validity.

By design, our experiment focuses on one causal mechanism: the effect of gaining relevant political knowledge—that is, knowledge about civil liberties and constitutional norms—on support for extending civil liberties to controversial groups or activities. We chose to test the impact of political knowledge because not only is it the most commonly posited mechanism in the tolerance literature, it is also the mechanism with the most far-reaching theoretical implications. Outside the domain of tolerance research, Bartels (1996) and Gilens (2001) have presented statistical models to predict how support for various policies would change if the public were as well-informed as the best-informed survey respondents. Furthermore, of all the mechanisms discussed, knowledge is arguably the most susceptible to potential policy interventions; it is easier to conceive of policies that would spread knowledge, such as increasing basic civics requirements or launching a public awareness campaign, than policies that would alter students' cognitive skills or expose them to a broader array of people and experiences.

Following Peffley and Sigelman, who advise that, “the most conclusive demonstration that knowledge has no bearing on tolerance would involve a measure of knowledgeability specifically tailored to the issue under consideration” (1990, 96), we sought to increase students' political knowledge in the domain of civil liberties by randomly exposing them to a curriculum that emphasizes the history and content of the Bill of Rights and Bill of Rights related jurisprudence. Such a design is also supported by Delli Carpini and Keeter's (1996, 221–24) contention that knowledge of Supreme Court decisions, as distinct from general political knowledge, leads people to embrace tolerant norms.

Our findings clearly demonstrate that the treatment did increase relevant political knowledge. Knowledge about civil liberties was significantly higher in classrooms that were recently exposed to the treatment curriculum. General knowledge about politics was unaffected by the treatment. The question of central theoretical interest is whether this

exogenous increase in relevant knowledge translated into increased support for civil liberties. Our experiment reveals no effect on support for civil liberties, a finding that challenges the longstanding hypothesis that exposure to civil liberties norms per se increases tolerance. Furthermore, we found that as the exogenous increase in knowledge dissipates one year and two years after the treatment, support for civil liberties remained unaffected, thus casting doubt on the functional interdependence (Converse 1964, 207) of political knowledge and tolerance.

This essay is structured as follows. We begin by providing an overview of the experiment—the curriculum, the research sites, and the experimental design. Next, we describe the surveys used to gauge preintervention equivalence of treatment and control groups and to measure postintervention experimental effects. After describing the statistical model used to estimate the direct treatment effects on knowledge and the indirect effects on support for civil liberties, we demonstrate the strong and statistically robust relationship between the experimental intervention and knowledge about constitutional rights. However, contrary to the hypothesis that knowledge induces appreciation and acceptance of tolerant norms, we find no relationship between exposure to the experimental curriculum and support for civil liberties, regardless of whether we examine the immediate effect or the effect one or two years afterward.

Data and Methods

This section describes the experimental program, the method by which students were assigned to experimental groups, and the pre- and postintervention surveys used to measure outcomes.

Experimental Curriculum

In order to gauge the effects of an exogenous increase in students' level of relevant political knowledge, we began by identifying curricula that stressed constitutional principles related to civil liberties and provided teachers with an accessible way of conveying these principles. We collaborated with the Bill of Rights Institute (BRI), whose mission is to educate high school students and teachers about the Constitution and to promote an informed and active citizenry. With these goals in mind, BRI developed a series of 21 lessons for high school teachers called *The Bill of Rights for Real Life* (BRRL). The lessons are written at

a level that is designed to be accessible to a broad spectrum of high school students, including students who are less academically inclined and unlikely to go on to college. The curriculum emphasizes classroom discussion and active student participation, both of which have been linked to tolerance and other civic attitudes in previous research (on classroom discussion, see Campbell 2008; Gibson and Levine 2003; Torney-Purta et al. 2001; on active student participation, see Finkel and Ernst 2005; Gibson and Levine 2003). The course content (as shown in the online appendix) combines history, current events, constitutional case studies, and landmark court decisions in an effort to underscore the meaning and relevance of constitutional principles such as freedom of speech.⁶

Treatment teachers participating in the experiment were asked to present all 21 *BRRL* lessons as a supplement to their civics curriculum. Although they were allowed to integrate the *BRRL* lessons into their curriculum in any order at any time during their respective civics courses, most teachers opted to cover the 21 lessons sequentially as one continuous unit on the Bill of Rights. In practice, teachers took an average of one and a half months to complete the *BRRL* curriculum. Based on a survey, to which 12 of 18 treatment teachers responded, the mean number of lessons teachers reported completing in full was 15.7, with a mean of 4 lessons completed in part, and 1.3 lessons skipped.

Control Curricula

Teachers in control classes were given no instruction as to course content or curricular guidelines for those classes, other than to *not* use lessons or materials from the *BRRL* curriculum. Since civics courses may vary across schools and across teachers, it is difficult to generalize about the content of the control curricula. From teacher questionnaires completed in May and June of 2007 (around the same time as the 2007 posttreatment survey), we know that the content of the control curriculum typically included topics such as the three branches of government, checks and balances, the Revolutionary War and ratification of the Constitution, the Bill of Rights, citizenship, elections, and state and local government.

As a manipulation check, we hired observers to visit 58 of the 59 classrooms, each on multiple

occasions, in order to measure classroom attributes and monitor the curriculum. These observers were asked to describe the content of what was taught and to score the class on multiple dimensions, including classroom environment, the type and frequency of technology in use, the level of student engagement (active vs. passive learning), and individual teaching style. We expected that the emphasis on civil liberties in the *BRRL* curriculum would increase the time spent on content related to constitutional knowledge and civil liberties. This expectation was confirmed by teacher evaluation questionnaires and classroom observations. Otherwise, raters' observations suggested that treatment and control were similar in terms of classroom environment, with treatment classes being somewhat less reliant on technology, more structured in terms of lesson plan, and more likely to invite student involvement and discussion.

Research Sites

A diverse array of schools in an eastern state participated in this study. These schools represent a broad spectrum of secondary educational settings ranging from urban to suburban to rural-regional. They include both traditional public high schools and public vocational-technical schools. Table 1 presents descriptive information from each high school's 2005–2006 profile: the range in enrollment, number of students in the experiment, socioeconomic status, ethnic diversity, overall academic achievement, dropout rates, and postgraduation outcomes.

Table 1 indicates that three of the schools are quite affluent, with fewer than 5% of the student body eligible for free or reduced-price meals. Two of the schools have a more economically disadvantaged student body, with over 35% of students eligible for free or reduced-price meals. The remaining schools are scattered in-between, with four schools falling in the 18–25% range. Test scores are positively correlated with this measure of affluence, while employment rates among students are negatively correlated. The proportion of graduates from these high schools who go on to college ranges widely, from 28% to 88%. All 10 schools participated in the pretreatment survey and the 2007 posttreatment survey. Seven of the ten original schools participated in all four waves of surveys. Attrition occurred in schools IV and VI because the students in the study were initially juniors and seniors; due to the difficulty of tracking students who had since graduated, we elected not to attempt to survey these students after the 2007

⁶See online appendix for an overview of the experimental curriculum.

TABLE 1 Sociodemographic Profiles of the 10 Participating High Schools

	I	II	III	IV	V	VI	VII	VIII	IX	X	State High School Average
Total Enrollment	1,460	1,130	1,160	480	1,490	1,360	830	880	580	760	N/A
Number of Students Assigned	86	86	65*	87**	225	100**	146	121	120	179	N/A
% Eligible for Free/ Reduced Price Meals	22	19	3	2	2	23	25	43	7	38	22
% Total Minority Population	32	14	7	3	8	40	30	49	6	43	N/A
% of K-12 Students with Non-English Home Language	6	6	1	0	2	2	9	7	2	12	11
% Grade 10 Meeting State Goal in Reading Across the Disciplines	43	35	75	71	70	44	35	28	66	36	47
% Grade 10 Meeting State Goal in Writing Across the Disciplines	60	43	68	76	85	52	40	44	76	33	52
Cumulative Four-year Dropout Rate for Class of '05	8	0	1	3	0	5	0	11	6	2	7
% Graduates from Class of '05 attending Two- or Four-year Colleges	87	69	85	87	88	78	30	82	78	28	78
% Graduates from Class of '05 Employed or in Military	8	20	9	7	7	16	67	7	12	58	14

Figures are for the 2005–2006 school year. One digit of precision was removed to preserve anonymity.

*School chose not to continue after Posttreatment Survey 2007.

**Schools where students took civics in the Junior or Senior year, and were thus not surveyed after Posttreatment Survey 2007.

posttreatment survey. School III initially indicated interest in continuing with the project but then did not respond to further attempts at contact.

Experimental Design

The feature of this study that sets it apart from most analyses of civic education is the manner in which students were assigned to the civic education treatment. In contrast to the observational studies that make up the bulk of this literature, the students in our study were randomly assigned to civics instruction with or without the *Bill of Rights for Real Life* curriculum. The use of random assignment ensures that, in advance of the study, there is no systematic tendency for students in the treatment group to score higher or lower than the control group on any outcome measure. This research design neutralizes some of the most common threats to inference in education research: heterogeneity across schools and self-selection of students into particular courses.

In each high school, we randomly assigned students to exposure to a *BRRL*-enhanced civics course or to a control group in which civics was taught in its usual manner. Civics is a mandated requirement for

high school graduation in the state where the experiment was conducted. Most of the participating high schools in this study require that civics be taken in the sophomore year, but two of the schools offer the option to take civics in either the junior or senior year. Six schools track students into civics levels based on academic performance; the remaining four schools do not group students by academic proficiency. In the high schools where tracking occurs, the *BRRL* curriculum was used mainly with the lower level classes (Levels 2 and 3) although some Level 1 classes were included in the study.

Prior to the start of the experiment, we recruited school administrators and teachers willing to use the *BRRL* curriculum. We then employed one of three different randomization procedures so that we could match teachers' needs with the requirements of the study. The three options for randomization were: (1) a teacher teaching more than one civics class or section allowed us to randomly assign at least one class to the treatment group and at least one to the control group; (2) a teacher willing to teach either all classes as treatment classes or all classes as control classes allowed us to randomly pick which experimental group the teacher would be assigned to; or (3) students were assigned by their school's registrar to one civics class or another in a manner that was

effectively random.⁷ In the last case, teachers could choose which experimental condition they wanted, so long as the choice was made before teachers knew which specific sections they were teaching or which students were in which section. The treatment group teachers were asked to teach all *BRRL* lessons while the control group teachers used their usual set of lessons.

The net result of this process over two semesters was 19 distinct clusters of classrooms, or strata, within which randomization occurred. Each stratum is composed of all classes in a single school that were in the same randomization pool. Within each stratum, every classroom had an equal probability of receiving the treatment. We have, in effect, nineteen distinct experiments. When estimating the treatment effect, we include fixed effects for strata to account for the fact that assignment occurred within strata but not across strata. Because classrooms, rather than specific individuals, were assigned to treatment and control groups, the statistical analysis that follows takes account of clustered random assignment for purposes of calculating standard errors. We return to this issue below, when discussing the statistical model.

Pretreatment Survey Measurement

Both treatment and control students completed a precurriculum questionnaire. The precurriculum survey instrument allowed us to establish that the two groups had similar background attributes, as one would expect given random assignment. A randomization check was performed to ensure these results by regressing treatment assignment on the following pretest measures: strata, survey attendance, year of

⁷We interviewed school registrars in order to gather details about how students were assigned to civics classes at a given level. In these schools we learned that 90% or more of the students are assigned arbitrarily to alternative sections of civics, with the remainder being reassigned by hand in order to alleviate unexpected scheduling conflicts. In advance of teaching the course, the teachers had no inkling of which sections would have stronger students, and the pretest surveys confirm that, prior to treatment, the classes had statistically equivalent baseline scores. It should be noted that the nature of the "treatment" is potentially different in sites where students, rather than teachers, were randomly assigned. When randomly assigning teachers, we assure that there is no systematic relationship between the curriculum and teacher quality. When quasi-randomly assigning students, we permit the possibility that more effective teachers seek out innovative curricula, in which case the treatment effects could be stronger. From our standpoint, this does not represent a problem, as we are interested in the downstream effects of exogenous shifts in knowledge, regardless of whether they are produced by teachers or the curriculum or the conjunction of the two.

birth, interest in politics, frequency of political discussions at home, party identification, having a job during the school year, mother's highest level of education, and seven factual questions measuring political knowledge. An F-test was then performed for the null hypothesis that all of the coefficients (with the exception of strata) were equal to zero. Because this test can be overly sensitive in small samples with many variables, we generated 20,000 simulated random assignments in order to calculate where our test statistic falls in the sampling distribution and found that 6.3% of these possible randomizations yielded F-values that were more extreme than the actual randomization. Although just short of statistical significance, this test suggests slight imbalance between treatment and control groups. As we will see below, this imbalance proves to be immaterial, as our findings are substantively unchanged by the addition of covariates.

Posttreatment Survey Measurement

Three postcurriculum surveys served as the primary evaluation tool for the study. The first postcurriculum survey was administered in 2007, shortly after students had completed the civics course (whether treatment or control). For students taking the civics course in the fall, surveys were administered in January, and for students taking the course in the spring, surveys were administered in April, May, or June. The second survey was administered the following year, 2008, during mid-to-late May or early June, and the third survey was administered a year after that, in 2009, also during mid-to-late May or early June. These surveys included questions testing both students' knowledge of the Bill of Rights and students' knowledge of other aspects of the Constitution and government. Students' capacity to understand and apply constitutional principles was tested using several question formats: short answer, fill in the blank, true/false, multiple choice, matching, and open-ended.

Care was taken in the administration of the questionnaires to maintain the symmetry between treatment and control conditions. First, the person who distributed the surveys to students had no apparent connection to the use (or absence) of the *BRRL* curriculum. The survey administrator presented the surveys to students in both treatment and control classrooms using the same introduction and instructions. The short introduction indicated that the surveys were meant to gauge what students know and think about government and politics. The

questionnaires were described as voluntary and confidential; students were asked to give them their attention but were told that their answers in no way affected their grade and would not be seen by their teacher. This procedure, coupled with a schedule of administration that surveyed students in both experimental groups at the same time, was designed to generate similar rates of nonresponse and attrition across experimental groups.

Table 2 reports the number of students within each experimental stratum who completed each survey. By “stratum,” we mean groups of courses (either taught by a single teacher or cluster of teachers) within which assignment took place.⁸ Across all of the strata, 30 courses and 627 students were assigned to the treatment group, and 29 courses and 588 students were assigned to the control group. There was no significant difference in the attrition rates between the treatment and control groups. Of the students in control classrooms, 87.4% completed the 2007 posttreatment survey, compared to 84.7% of the students in treated classrooms. By the last post-treatment survey in 2009, among those whose schools were still participating in the experiment, 53.0% of the controls completed the survey, compared to 52.5% of the treated.

Characteristics of the Courses and Teachers Studied

The evaluation involved 25 teachers in 10 high schools. Participating teachers ranged in age from 24 to 61 years, with two-thirds under 35. Teachers were almost evenly divided by gender, 13 female and 12 male. About one-third of the teachers had one to four years of teaching experience; another third had five to nine years; the remaining third had spent 10+ years in the classroom, with the longest term of service being 40 years. All of the participating teachers had completed a bachelor’s degree and earned professional certification to teach social studies at the secondary level. Slightly more than half of these teachers had also earned a master’s degree in education or a field related to social studies. As noted above, after the completion of the treatment or control curriculum and the administration of the 2007 survey, three schools did not continue in the experiment. The 2008 and 2009 surveys were thus

completed by students in seven schools, taught by 19 of the original 25 teachers. Teachers whose students participated in the follow-up study had an average age of 35. In all other respects, the demographics of teachers whose students participated in all four surveys were similar to the demographics of the original pool.

Outcome Measures

Our survey measures a range of political beliefs and orientations, but we focus on three categories of outcomes. The first category comprises relevant political knowledge, or knowledge about civil liberties, the Bill of Rights, and Bill of Rights related jurisprudence and Supreme Court cases covered by the *BRRL* curriculum. As shorthand, we refer to this domain as “civil liberties knowledge.” The other two categories are general political knowledge and support for civil liberties. For each of these categories, we created simple additive indices based on answers from each of the surveys. We later return to the issue of scale construction and show that our results do not change when we focus on individual items rather than indices.

In order to assemble the factual knowledge indices, we culled items from other surveys of civic knowledge as well as from the *BRRL* curriculum.⁹ Questions were selected to provide varying degrees of difficulty. For example, questions were as simple as “What do we call the first ten amendments to the U.S. Constitution?” and as complex as correctly identifying examples of infringements of constitutional protections. We chose not to include questions that focused narrowly on historical details (e.g., “Who was the father of the Bill of Rights?”) or pure recall (e.g., “Which rights are protected in the Second Amendment?”). Instead, we chose questions that touched on the definitions of key terms and the application of principles found in the Constitution and the Bill of Rights. We also asked students to correctly identify constitutional issues associated with landmark Supreme Court cases.

Although all of the surveys contained some questions covering general political knowledge, Post-treatment Surveys 2008 and 2009 contained a higher number of these questions in direct response to teachers’ concerns that the *BRRL* curriculum required them to spend more time on the Bill of Rights to the detriment of other areas of political knowledge, such as the structure of the federal government and

⁸Consider, for example, Stratum H, consisting of three courses taught by an instructor. Suppose this instructor indicates that he/she is willing to teach two *BRRL* courses. We randomly assign two courses of the three to the treatment group and the remaining course to the control group.

⁹See online appendix for a detailed listing of the survey items as well as their sources.

TABLE 2 Response Frequencies by Experimental Group

Stratum	Control						Treatment					
	Classes Assigned	Total Students Assigned	No. of Students in Pretreatment Survey	No. of Students in Posttreatment Survey 07	No. of Students in Posttreatment Survey 08	No. of Students in Posttreatment Survey 09	Classes Assigned	Total Students Assigned	No. of Students in Pretreatment Survey	No. of Students in Posttreatment Survey 07	No. of Students in Posttreatment Survey 08	No. of Students in Posttreatment Survey 09
	A	1	24	23	20	17	10	1	23	22	18	12
B	1	20	18	18	14	15	1	10	8	9	7	7
C	1	20	20	16	0	0	1	24	19	15	0	2
D	1	27	27	25	0	5	1	15	14	10	0	4
E	1	21	21	20	0	0	1	20	20	18	0	0
F	1	15	14	15	0	0	1	9	8	8	0	0
G	2	38	33	30	0	0	1	25	23	20	0	0
H	1	10	10	9	0	0	2	31	28	25	0	0
I	1	28	28	23	22	21	2	44	44	41	37	34
J	1	25	24	21	20	19	2	49	48	44	33	38
K	1	24	20	18	12	15	2	52	46	42	31	34
L	1	19	17	16	11	9	1	20	17	18	9	9
M	1	24	19	18	13	12	1	21	17	12	7	12
N	1	12	11	8	0	0	2	34	30	27	0	0
P	1	15	9	11	0	0	1	22	16	15	0	0
Q	7	128	118	118	94	94	3	51	47	48	40	39
R	3	75	70	73	47	14	4	104	91	97	64	19
S	1	19	16	17	13	6	1	27	23	24	21	7
T	2	44	39	38	33	33	2	46	37	40	34	34
Total	29	588	537	514	296	253	30	627	558	531	295	255

the roles played by state and local government. If this were true, students in control classes would be expected to perform better on general political knowledge than students in treatment classes, a hypothesis that we test below. Regarding both civil liberties knowledge and general political knowledge, it is important to note that teachers were not allowed to see any of our survey questions and were therefore prevented from “teaching to the test.” We also changed the questions used in each survey wave so that the content of the assessment would not be something that teachers could anticipate.

Table 3 describes the ranges and distributions of the scales used in the three posttreatment surveys. The Cronbach’s alpha indicates the reliability of each scale, or the squared correlation between the observed scale and the underlying factor that it is designed to measure. The knowledge scales tend to be more reliable than the scales measuring support for civil liberties.¹⁰ It should be stressed that this pattern does not imply that experimental results are biased for outcome measures that have lower reliability; rather, low reliability among scales used here as dependent variables increases the standard errors of our estimated treatment effects.¹¹ Given the large *N* of our experiment, our standard errors remain manageable despite low reliability. Moreover, our results are quite robust, and our substantive conclusions remain unchanged when each of the scale items is analyzed separately as a dependent variable.

One interesting feature of our survey results is the strong correlation we observe among the three outcome measures. In the first follow-up survey in 2007, for example, both the Civil Liberties Knowledge Index and the General Political Knowledge Index are significantly correlated with each other ($r=.42$) and with the Support for Civil Liberties Index ($r=.30$

and $r=.16$, respectively). The same pattern of significant correlations holds in the 2008 and 2009 surveys as well. These correlations are noteworthy in two respects. First, the strong correlation between knowledge in two different domains should be kept in mind as we present the experimental results, as only one of these two correlated scales will be shown to move in the wake of the experimental intervention. Second, the significant correlation between knowledge and support for civil liberties reproduces the pattern observed in prior observational studies. Moreover, this correlation addresses the concern that measures of support for civil liberties are too unreliable to be informative. The fact that this index is predicted by knowledge indicates again that the measures of support for civil liberties, though noisy, behave as one would expect.

Statistical Model

The focus of our analysis is the treatment effect on the outcome measures listed in the previous section. The basic model is a straightforward generalization of a comparison of means across treatment and control groups. The sole complication is the need to control for strata, because assignment occurred within rather than across strata. These controls are easily implemented by adding dummy variables for all strata but one.

Thus, our model has the form:

$$Y_i = \beta_0 + \beta_1 X_i + \sum_{k=1}^{K-1} \gamma_k D_k + u_i, \quad (1)$$

where X_i is scored 0 for control and 1 for treatment, the $K-1$ dummy variables marking each of the K strata are denoted D_k , and u_i denotes the unobserved disturbance term. The key parameter of interest in equation (1) is β_1 , which represents the average treatment effect on Y_i , which represents one of the three outcome measures: domain-specific knowledge, general knowledge, or support for civil liberties. This model may be extended in a straightforward manner to include as controls covariates measured during the pretest. Inclusion of these covariates has the potential to improve the efficiency with which the treatment effects are estimated. As will be shown below, the inclusion of these covariates has little effect on the estimates or their standard errors. In addition, because assignment was carried out at the level of the classroom rather than the level of the individual, our estimator must allow for the possibility that individuals within classrooms share unobserved

¹⁰In spring semester of 2010, we conducted a survey in two of the participating high schools in order to better understand the measurement properties of the items used in the four-wave panel study. Using a fresh set of respondents ($N=251$), the survey included a large proportion of the closed-ended items used in preceding surveys. The results confirm the reliability estimates presented in Table 3 and suggest that each scale is a valid measure of a single dominant factor.

¹¹It is common but mistaken to dismiss dependent measures whose reliability falls below some threshold, say, 0.5. So long as the measure of Y is valid, low reliability is not a source of bias. Consider, for example, a regression in which Y is measured with random error e such that $Y^* = Y + e$. A regression of Y^* on a random treatment X , as in equation (1) below, still yields an unbiased estimate of the average treatment effect. This result holds regardless of whether the variance in e is large and the reliability of Y^* is low because e is simply absorbed into the disturbance term.

TABLE 3 Description of Outcome Variables

	Civil Liberties Knowledge Index	General Political Knowledge Index	Support for Civil Liberties Index
Posttreatment Survey 2007			
Number of Questions	15	5	3
Range	0–15	0–5	0–3
Mean	8.47	1.93	2.36
N	1045	1045	1045
Standard Deviation	2.85	1.32	0.80
Cronbach’s Alpha	0.67	0.48	0.37
Posttreatment Survey 2008			
Number of Questions	18	22	7
Range	0–18	0–22	0–7
Mean	8.20	9.85	4.05
N	591	591	591
Standard Deviation	2.98	3.30	1.50
Cronbach’s Alpha	0.62	0.66	0.45
Posttreatment Survey 2009			
Number of Questions	16	10	7
Range	0–16	0–10	0–7
Mean	6.84	4.48	4.06
N	508	508	508
Standard Deviation	3.10	2.26	1.27
Cronbach’s Alpha	0.68	0.61	0.31

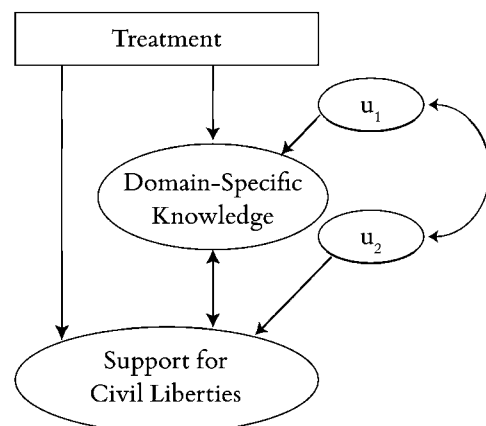
Notes: All questions rescaled to 0–1 for additive index.

attributes. We use Generalized Least Squares (GLS) regression with random effects at the classroom level to estimate average treatment effects (Green and Vavreck 2008). Because our treatment assignment is orthogonal to classroom-level effects, this method of estimation is efficient and consistent.

The central theoretical question in this study is whether an exogenous increase in civil liberties knowledge and understanding produces a corresponding increase in support for civil liberties. This causal relationship is depicted in Figure 1. Unlike traditional path models, the model in Figure 1 does not stipulate a simple recursive relationship among the variables. Rather, the system of equations is agnostic about whether unobserved factors that affect support for civil liberties are correlated with unobserved factors that affect knowledge. The key feature of the diagram is the fact that the treatment, which is randomly assigned, is assumed to be statistically independent of these two disturbance terms. If we assume that the treatment only influences support for civil liberties indirectly by influencing knowledge, then we can estimate the local average treatment

effect of knowledge by using randomly assigned curriculum as an instrumental variable (Angrist, Imbens, and Rubin 1996). However, this approach misestimates the effect of knowledge if the treatment

FIGURE 1 Depiction of the Causal Relations among Assigned Treatment, Domain-Specific Knowledge and Support for Civil Liberties



curriculum has a direct influence on support for civil liberties.

A more conservative estimation approach is to examine the reduced form relationship between random assignment and support for civil liberties. This regression model takes the same form as equation (1), but now the dependent variable is the Support for Civil Liberties Index. If knowledge and understanding mediate the effects of the treatment, we should obtain a positive estimate of β_1 . As Figure 1 suggests, estimating the reduced form provides a *conservative* test, because if the treatment leads to a direct increase in support for civil liberties (i.e., course content encourages support for norms of free expression and association) or indirectly through channels other than knowledge (e.g., course discussion promotes appreciation for dissenting viewpoints), we would attribute these effects to the mediating role of knowledge and understanding.

Results

Table 4 presents the primary findings of our experiment.¹² The treatment, exposure to the *BRRL* curriculum, has a highly statistically significant effect ($p < 0.001$) on the Civil Liberties Knowledge Index in both the 2007 and 2008 posttreatment surveys. In the 2007 posttreatment survey, the treatment yielded an improvement of 1.220 correct answers (SE = 0.155) out of 15 questions about the Constitution, (Cohen's $d = 0.49$). In the 2008 posttreatment survey, this treatment effect drops to 0.762 additional correct answers (SE = 0.226) out of 18 questions.¹³ In the 2009 posttreatment survey, we find a positive but

¹²These analyses were performed including all students who took each survey. Similar results obtain when the sample is restricted to the students that participated in all three waves of posttreatment surveys. See the online appendix.

¹³We included one open-ended short essay question on the 2007 posttreatment survey and one on the 2008 posttreatment survey. Students were presented with a hypothetical dilemma and asked for their thoughts and what/whose rights were at stake. The essays were scored by raters who were blind to the subjects' treatment assignment. Raters identified and summed the number of relevant constitutional principles identified. In the 2007 posttreatment survey, students in the treatment group performed significantly better than students in the control group, indicating that students were able to apply the constitutional knowledge they had gained. On average, treated students identified .182 (SE=0.068) additional rights out of a possible 13. However, there was no significant difference between treatment and control in the 2008 survey (-.007 rights, SE = 0.128). This finding comports with the general trend of declining differences between treatment and control groups over time.

statistically insignificant treatment effect of 0.221 correct answers (SE = 0.269) out of 16 questions. This suggests that the treatment induces a short-term increase in civil liberties knowledge that gradually dissipates over the course of two years. Because each of the survey waves used different questions to measure knowledge, the index means are not directly comparable over time. In order to calibrate the indices, we conducted the 2010 survey described in footnote 10, which included most or all of the index questions on a single questionnaire (See Table A4). After correcting for the changing difficulty of the items over time, we find that the control group means were essentially unchanged during the three postintervention waves, while the treatment group showed an initial surge followed by a gradual decline to the control group mean.

The treatment had no effect, positive or negative, on general political knowledge. The estimates hover near zero in each wave, regardless of whether the model controls for covariates. This null finding is important, because if the treatment had caused a decrease in general civic knowledge, the net effect on support for civil liberties would be unclear: the effects of augmenting specific knowledge might have been offset by a deficit of general knowledge. The fact that general knowledge is unaffected by the treatment allays this concern. The treatment increased knowledge in the domain that the experimental curriculum addressed without subtracting from knowledge in other domains.

The central theoretical question in this study is whether an exogenously induced increase in knowledge produces an increase in support for civil liberties. Our experimental results provide no support for this hypothesis. Support for civil liberties is unaffected by the treatment in all three posttreatment surveys. In order to ensure that these results were robust to our construction of the additive index, we also estimated the same regressions using each of the tolerance questions as the dependent variable. None of these regressions revealed a statistically significant treatment effect (see online appendix). We see no increase in support for civil liberties in the first two posttreatment waves, the period during which there were significant gains in civil liberties knowledge. Conversely, as the treated students' advantage in civil liberties knowledge decayed, their relative standing in terms of support for civil liberties remained unaffected.

The findings imply that students can become more knowledgeable about the Constitution and workings of government without experiencing a concomitant shift in their support for free speech,

TABLE 4 Generalized Least Squares Estimates of Experimental Treatment Effects on Civil Liberties Knowledge, General Political Knowledge, and Support for Civil Liberties

	2007 Posttreatment Survey		2008 Posttreatment Survey		2009 Posttreatment Survey	
Civil Liberties Knowledge Index						
Treatment	1.248 (0.181)	1.220 (0.155)	0.796 (0.228)	0.762 (0.226)	0.492 (0.268)	0.221 (0.269)
Covariates [†]	No	Yes	No	Yes	No	Yes
Strata [‡]	Yes	Yes	Yes	Yes	Yes	Yes
N	1045	1045	591	591	508	508
General Political Knowledge Index						
Treatment	0.058 (0.113)	0.023 (0.076)	0.055 (0.279)	0.051 (0.259)	-0.004 (0.204)	-0.037 (0.202)
Covariates [†]	No	Yes	No	Yes	No	Yes
Strata [‡]	Yes	Yes	Yes	Yes	Yes	Yes
N	1045	1045	591	591	508	508
Support for Civil Liberties Index						
Treatment	-0.061 (0.050)	-0.071 (0.051)	0.026 (0.150)	-0.073 (0.131)	0.022 (0.118)	-0.080 (0.119)
Covariates [†]	No	Yes	No	Yes	No	Yes
Strata [‡]	Yes	Yes	Yes	Yes	Yes	Yes
N	1045	1045	591	591	508	508

Notes: Standard errors in parentheses. Clusters for random effects model are at the classroom level.

[†]Covariates are dummy variables generated from the Pretreatment Survey: survey attendance, year of birth, interest in politics, frequency of political discussions at home, party identification, having a job during the school year, mother's highest level of education, and seven factual questions measuring political knowledge.

[‡]Models include dummy variables for strata (coefficients not reported).

dissent, or due process. This pattern of results is striking both because this experiment has ample power to detect even small effects and because, as noted above, the statistical test is biased in favor of finding a mediating relationship. The theoretical implications of this finding are profound. Evidently, it is possible to increase awareness and understanding of civil liberties without producing an increase in support for those civil liberties. This finding therefore calls into question the longstanding argument that beliefs and attitudes are causally linked in this domain.

Conclusion

The present study represents the first large-scale experiment testing the effects of civics instruction. On a basic level, the study sheds light on longstanding questions about whether civics instruction "works." Consistent with several survey-based studies, we find that the experimental curriculum's enhanced attention to constitutional rights did indeed convey factual information about the Constitution, government, and history. But this investigation is not primarily concerned with the

evaluation of a specific curriculum or teaching approach. Rather, it centers on a broader theoretical question, the causal relationship between knowledge and attitudes.

From this theoretical vantage point, the central finding is that randomly induced increases in relevant political knowledge did not produce corresponding changes in support for civil liberties. Although the correlation between political knowledge and support for civil liberties is strong in our survey and in many other surveys conducted over several decades, our experiment indicates that these two variables may not be causally connected. As such, this finding casts doubt on the causal inference that previous authors have drawn from the correlation between education and political tolerance in observational data.

Our central substantive finding is subject to multiple interpretations, each suggesting a somewhat different line of follow-up research. One interpretation, drawing on the work of Angrist, Imbens, and Rubin (1996), is that this finding reflects the idiosyncrasies of a particular set of subjects. This line of argument takes notice of the fact that the statistical effects of increases in knowledge are identified only

among the subgroup of students whose knowledge of civil liberties is augmented by the treatment curriculum. It may be that support for civil liberties among students who are affected on the margin by the instructional program is for some reason unaffected by increased knowledge of the Constitution. The empirical task for future research then becomes one of inducing knowledge effects in different ways or on different types of people, perhaps by intensifying the educational program or changing its focus. Our search for heterogeneous treatment effects came up with no convincing evidence of subgroup differences; partitioning the sample by political interest, frequency of political discussion at home, party ID, age, mother's level of education, employment status and seven factual questions yielded no significant interactions in models predicting knowledge or support for civil liberties.¹⁴

Another interpretation is that knowledge and attitudes are causally disconnected. One cannot increase support for civil liberties simply by teaching students about the provisions of law that are designed to protect these liberties. If this hypothesis were correct, the puzzle would then become figuring out why education and support for civil liberties are correlated. It may be that education promotes support for civil liberties through one of the other proposed causal mechanisms, in which case future experiments should be designed with an eye towards varying the teaching of cognitive skills or the level of exposure to diverse viewpoints. It may also be that there is another as-yet-unspecified factor that promotes higher levels of education and support for civil liberties. Regardless of which interpretation one accepts, advancing our understanding of the causal linkage between civic knowledge and attitudes requires a broad-ranging experimental research program. The current study represents a first step in this direction.

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¹⁴We tested for heterogeneous treatment effects by interacting the treatment term with each of the individual-level covariates. None of these effects were significant after taking into account multiple comparisons using the Holm-Bonferroni method.

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