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Adult Culture Wars and Student Academic Achievement

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Adult Culture Wars and Student Academic Achievement*

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Abstract

How do adult "culture wars" in education affect student learning in the classroom? I explore this question by combining information on nearly 500 school district political controversies with data on state test scores. Leveraging variation in the location and timing of these events as the basis for a difference-in-differences design, I show that student achievement declines in the wake of adult political battles. The effects are concentrated in math achievement—the equivalent of approximately 10 days of lost learning—and persist for at least four years. The declines are particularly pronounced for controversies surrounding racial issues and the teaching of evolution. These results suggest that well-intentioned education advocacy efforts focused on salient social justice issues may backfire, producing in unintended negative impacts on student achievement, and raise new questions about the adequacy of local democratic processes for the governance of public schools.

Keywords: culture wars, education politics, student achievement, local control of schools

In a well-functioning democracy, the decisions and actions of government officials must reflect the wishes of the governed.[1] Designing political institutions to achieve this goal, however, can require difficult tradeoffs if increased responsiveness to public opinion results in the politicization of bureaucracies that actually implement policies and impedes their performance (e.g., Gailmard and Patty 2007, Lewis 2008). Since at least the Progressive era, when reformers sought to insulate many aspects of public administration from excessive political meddling, how to balance these competing goals has been the subject of ongoing political and scholarly debate. For example, some critics have argued that Progressive reforms implemented in local government at the turn of the 20th Century—including the use of non-partisan, off-cycle, and at-large elections and the appointment of professional city managers—did little to improve government efficiency and instead shifted political power from lower-income, immigrant communities to wealthier, more advantaged constituencies (e.g., Bridges 1997, Trounstine 2006).[2]

Balancing political accountability with effective service delivery poses particularly difficult challenges in the context of public education. Although public schools exist to serve the educational needs of children, students generally cannot vote in the elections through which key policymakers are chosen (Chubb and Moe 1990). Recent research examining detailed voter file records has also found that most voters who participate in local school board elections do not themselves have kids and often look quite different in terms of race, ethnicity, and income than the children local schools educate (Kogan, Lavertu and Peskowitz 2021). Concern that political considerations can encourage school board members to prioritize adult interests at the expense of student learning motivated many of the educational reforms adopted in the early decades of the 20th Century—from the hiring

[1] There is considerable debate among philosophers about whether public officials should be most responsive to the (sometimes uninformed) *opinions* or to the *interests* of constituents (Pitkin 1967).

[2] Others have argued that off-cycle elections have empowered public employees and other narrow interest groups (Anzia 2011, Berry 2009, Moe 2006).

of professionally trained superintendents to teacher tenure and licensing laws designed to limit the influence of patronage in school employment—that continue to characterize the American public education system to the present day (Tyack 1974).

Although the effects (and efficacy) of these reforms remain debated, it is clear that they have not fully shielded public education from adult political conflicts. As exemplified most prominently by the 1925 Scopes Monkey trial, American voters have, at various times, found themselves deeply divided over educational issues such as the teaching of evolution, recitation of prayers in schools, sex education, and school curriculum (e.g., Berkman and Plutzer 2010, Boryczka 2009, Rhodes 2012). Often, these disagreements have played out publicly through the democratic process. Recent political arguments over the accommodation of gender identity in school athletics and instruction on topics related to the origins and persistence of racial inequities in American society arguably represent a continuation of such adult culture wars.

In this paper, I examine how such adult political conflicts ultimately affect student learning in the classroom. Theories of democratic accountability do not provide clear expectations in this regard. On one hand, by bringing attention to the inner workings of the school system, high-profile controversies may pique voter interest in local school elections, increasing accountability pressures and diluting the corrosive influence of special interest groups who would otherwise play a dominant role in what are typically low-interest, low-turnout elections (e.g., Berry 2009, Anzia 2011, Moe 2006). On the other, by shifting attention away from student outcomes to issues of interest to adults—including adults without children—the controversies could distract teachers and district administrators and create unnecessary chaos and staff turnover, disrupting student learning.[3]

[3]For example, Berry and Howell (2007) note that media coverage of student achievement plays an important role in making academic outcomes salient to voters and the weight voters put on achievement when deciding whether to re-elect incumbents. By making culture war issues more salient, controversies may cause voters to place less weight on academic achievement.

The empirical analysis combines a decade of data on high-profile "culture war" controversies in local public education that have been tracked as part of the Cato Institute's Public Schooling Battle Map with information on student academic achievement in grades three through eight from the Stanford Education Data Archive. I leverage variation in the timing and location these controversies to estimate the causal effect of such conflicts. Specifically, the analysis relies on a difference-in-differences design that compares the trajectory of student test scores in the affected districts before vs. after the outbreak of each controversy to contemporaneous trends in a control group of school districts that do not experience a similar controversy over the same time period.

To preview the findings, I show that local political controversies hurt student performance, particularly in mathematics. The decline in achievement, on the order of 0.02 to 0.03 standard deviation units, is roughly equivalent to 10 days of learning out of a standard 180-day school year. Moreover, these effects persist for at least four years after the event. Examining potential heterogeneity both in the nature of the controversies and the students affected, I find that the learning disruption is particularly pronounced in the wake controversies related to racial issues and the teaching of evolution, which both reduce math achievement by between 0.06 and 0.07 standard deviations. However, I do not find evidence that the disruptions impact disadvantaged students more than their peers, leaving the magnitude of the achievement gap between low-income and other students largely unchanged. These results are robust to newer methods that address potential bias in by two-way fixed effects models when examining interventions with staggered timing.

The study contributes to several distinct literatures. First, it is related to the growing body of research on voter behavior in local school board elections. These studies generally focus on the extent to which voters in such elections pay attention to student achievement—as opposed to other considerations that may be of interest to adults—and hold school board

incumbents accountable for performance, as theories of retrospective voting would predict (see Berry and Howell 2007, Kogan, Lavertu and Peskowitz 2016, Payson 2017, Thompson 2019). This work largely takes student achievement and school quality as exogenously determined—overlooking the possibility that the causal arrow may point in the opposite direction, with political turmoil potentially disrupting student learning.[4] While education researchers have shown that both teacher and administrative turnover can negatively impact student achievement (e.g., Bartanen, Grissom and Rogers 2019, Ronfeldt, Loeb and Wyckoff 2013), prior studies do not examine whether local political controversies can serve as the precipitating events that lead to disruptive staffing changes.

Second, the study is also related to a growing literature that examines how political controversies affect the quality of public services at the local level. Much of this work focuses on policing and has found that public protests and investigations in the wake of high-profile police shootings of civilians can reduce officer effort and proactivity, resulting in higher crime rates (Devi and Fryer 2020, Campbell 2021, Rivera and Ba 2022, Shi 2009). To my knowledge, this is the first paper to examine similar dynamics in the context of public education.

Data Overview

The data on local education controversies comes from the Cato Institute's Public Schooling Battle Map, described in more detail in McCluskey (2019). The database was assembled by Neal McCluskey, the director of Cato's Center for Educational Freedom, using a combination of Google News alerts, direct reports from individuals, and the ChoiceMedia.tv

[4] A growing literature examining the causal effect of local tax and bond referenda demonstrates how the decisions voters make on Election Day can impact student achievement, flipping the direction of causality assumed in much of the existing work on school board elections (e.g., Abott et al. 2020, Baron 2022, Cellini, Ferreira and Rothstein 2010, Hong and Zimmer 2016, Kogan, Lavertu and Peskowitz 2017).

"Newswire," an aggregator website for education-related news. As such, the data is largely limited to sufficiently high-profile controversies that attract media attention and likely misses many smaller conflicts in districts with less media coverage. While the database includes incidents dating back to 2005, it is most complete starting in 2011. Each controversy is classified into one of nine categories: (1) freedom of expression; (2) religion; (3) curriculum; (4) reading material; (5) race/ethnicity; (6) moral values; (7) gender equity; (8) sexuality; and (9) human origins.[5]

To examine how these controversies affect student academic achievement, I merge the Battle Map dataset with records from the Stanford Education Data Archive (SEDA) (Fahle et al. 2021). This collection includes achievement estimates based on test scores in math and English language arts in grade three through eight that states report to the U.S. Department of Education.

Because the SEDA data covers the years 2009 through 2018, I impose several data restrictions to avoid bias due to treatment effect dynamics and heterogeneity. First, I exclude from the analytic sample districts that are "always-treated"—dropping all districts with a controversy observed in 2009 or earlier years. Second, I limit the sample to districts that experience only one controversy during this time period, excluding school systems with more than one event in the data. In addition, I exclude state-level controversies that are likely to affect all districts in a given state.[6] Thus, the analytic leverage comes from comparing districts that experience one local controversy during the years included in SEDA compared to a control group made up of "never-treated" school districts that do not

[5]Events are coded into categories based on the dimension of the controversy that is judged to be most central. For example, several cases in which districts prohibited students from wearing rosary beads are coded as "freedom of expression," although the expression in those cases is religious in nature. Similarly, complaints about books are coded as "reading material" disputes even if the specific books in question are controversial because they deal with racial or sexual topics.

[6]As discussed below, my statistical model controls for state-by-year fixed effects that absorb the effects of these statewide controversies.

experience a controversy. The final sample includes approximately 520 local controversies, summarized in Table 1. The total number of events ranges from 30 to 80 per year and the most common type of controversy focuses on freedom of expression, which accounts for nearly a third of all of the events in the dataset.

Table 1: **Controversies by Year and Type**

Year of Event	Curriculum	Freedom of Expression	Gender Equity	Human Origins	Moral Values	Race/ Ethnicity	Reading Material	Religion	Sexuality	Total
2010	1	9	0	0	3	6	10	4	2	35
2011	2	8	0	2	0	0	12	8	1	33
2012	1	27	1	1	4	4	8	8	1	55
2013	8	21	2	1	4	10	14	12	3	75
2014	5	18	0	0	7	4	14	17	3	68
2015	5	20	6	4	6	7	6	13	2	69
2016	2	14	6	1	4	8	1	9	0	45
2017	5	19	9	0	8	6	4	14	0	65
2018	11	27	3	1	15	4	4	9	2	76
Total	40	163	27	10	51	49	73	94	14	521

Empirical Strategy

An obvious challenge to studying the effects of political controversies is that such events are unlikely to be random. For example, larger districts located in the core of their newspaper circulation area or television media market are probably more likely to see a run-of-the-mill, small-scale conflict escalate after attracting press coverage. In addition to district size, McCluskey (2019) finds that both student racial composition and the partisanship of each district's surrounding county predict the outbreak and frequency of events (see also Table 2 below).

My empirical strategy directly accounts for such selection. Specifically, I estimate a difference-in-differences model (Angrist and Pischke 2009) that controls for time-invariant factors that could affect both the probability of an outbreak of a political controversy and student achievement. In most specifications, I use the following OLS model:

$$\mathrm{Y}_{dgst} = \alpha_d + \beta \mathrm{Controversy}_{dst} + \delta_{st} + \varphi_g + \epsilon_{dgst}$$

where Y_{dgst} represents test scores of students in grade g in district d located in state s during year t. The model includes both district (α_d) and state-by-year (δ_{st}) fixed effects. The latter accounts for both potential changes in the assessments used by individual states over time as well as the impact of state-level policy changes and statewide education-related political controversies. I pool data across all tested grades and some specifications also include grade-specific fixed effects (φ_g), although I show that the results are not sensitive to this choice. $Controversy_{dst}$ is a binary indicator that takes the value of one starting in the year of each controversy. The variable remains "on" in all subsequent years, although I also estimate more flexible event study specifications that examine the dynamics more carefully. All standard errors are clustered by school district to account for serial correlation

(Bertrand, Duflo and Mullainathan 2004).

The primary quantity of interest is the estimate of β. This can be interpreted as the causal effect of controversy on student academic achievement under the assumption that student performance in the affected districts would have followed parallel trends as in districts that did not experience such an event. To probe the plausibility of this assumption, I examine the trajectory of achievement in the affected districts compared to never-treated controls in the years leading up to the controversy using the following event study specification:

$$\mathrm{Y}_{dgst} = \alpha_d + \sum_{j \leq -5}^{\geq 5} \pi_j \mathrm{Controversy}_{dst} \cdot 1(\tau_{dst} = j) + \delta_{st} + \varphi_g + \epsilon_{dgst}$$

Given the staggered nature of the treatment and the relatively short duration of the achievement panel, I bin the event window end points, combining all years at least five years before and five years after each event into two categories.

Recent work has shown that the two-ways fixed-effects models described above can be biased in applications with a staggered treatment when the effects are heterogeneous across time or units (Callaway and Sant'Anna 2021, de Chaisemartin and D'Haultfœuille 2020, Goodman-Bacon 2021). While much of the focus has been on the conventional difference-in-differences model, Sun and Abraham (2021) show that similar biases can impact event study estimates as well. I address this issue in several ways. First, as noted above, I exclude "always-treated" units from the analytic sample, ensuring that they do not contaminate the estimated treatment effects. Second, I show that results are quite similar when using alternative specifications proposed in this literature.

Table 2 provides a summary of the full dataset as well as the analytic sample. While each district is observed for multiple years and test scores are reported across multiple grades, the table summarizes district characteristics in 2010 and student achievement in

the fifth grade. Several patterns clearly stand out in the data. First, districts that experience a controversy attracting media attention are considerably larger, more urban, and enroll a smaller share of white students than districts that serve as the control group. On the other hand, treated districts are also somewhat wealthier, as measured by the share of students designated as economically disadvantaged, and have achievement levels that are modestly higher than for the districts in the control group. In addition, the treated districts have somewhat larger achievement gaps between higher- and lower-SES students. Second, districts that experience multiple events during the period of the study and are thus excluded from the analytic sample are particularly large—enrolling more than 17,000 students on averages in the tested grades—and are especially urban. Since these large urban districts are excluded from the analysis, caution is warranted in generalizing the findings to these types of districts.

Table 2: **Summary Statistics, Measured in 2010**

		Excluded from Analysis		Included in Analysis	
	Full SEDA Sample	Always-Treated	Multiple-Events	Never-Treated	Treatment Districts
Total Enrollment (Grades 3-8)	1,695.40	4,022.44	17,581.48	1,089.52	3,666.09
Ave. Enrollment per Grade	287.10	670.88	2,930.25	184.93	612.43
Attending Urban Schools %	0.06	0.15	0.34	0.04	0.14
Attending Suburban Schools %	0.20	0.40	0.40	0.18	0.35
Attending Town Schools %	0.17	0.20	0.06	0.18	0.20
Attending Rural Schools %	0.57	0.26	0.19	0.60	0.31
White %	0.75	0.70	0.60	0.75	0.69
Black %	0.08	0.12	0.16	0.07	0.11
Hispanic %	0.12	0.13	0.17	0.12	0.15
Econ. Dis. (ED) %	0.48	0.43	0.43	0.49	0.44
English Learners %	0.05	0.06	0.08	0.04	0.06
Special Ed %	0.14	0.14	0.13	0.14	0.14
Ave. Math Achievement (Grade 5)	-0.00	0.07	0.09	-0.01	0.07
Ave. ELA Achievement (Grade 5)	-0.01	0.05	0.08	-0.01	0.04
Math NonED-ED Achievement Gap (Grade 5)	0.48	0.55	0.64	0.47	0.53
ELA NonED-ED Achievement Gap (Grade 5)	0.52	0.58	0.67	0.50	0.56
Districts	13105	185	367	12031	521

Results

Table 3 reports the standard difference-in-differences estimates. The first two column show results for math scores while the latter two columns present comparable estimates for English language arts (ELA) achievement. Overall, student math achievement declines by approximately 0.018 standard deviations in the years after a local political controversy. The estimates are the same regardless of whether the model includes grade-level fixed-effects. By contrast, there does not appear to be any impacts on ELA achievement. While the divergence between math and ELA scores may be surprising, it is entirely consistent with other education policy research showing that student performance in math is much more sensitive to policy interventions than ELA achievement (see Fryer 2014 for overview).[7] Although education scholars have offered a number of plausible explanations for this pattern, the most likely is that students acquire a much larger share of their mathematical knowledge inside the classroom, in contrast to activities outside of school that complement formal reading instruction.

It is useful to put the effect size into more intuitive units. Hill et al. (2008) provide national benchmark for typical learning gains in both math and ELA during each year of schooling. Focusing on math achievement growth in just grades three through eight—those covered by the SEDA data—student scores increase by about 0.39 standard deviations per year, on average. The estimated effect of 0.018 corresponds to roughly 5 percent of annual gains, which translates to about 10 days of learning assuming a typical 180-day school year.

Figure 1 presents estimates from the event study specification. The top panel reports results for math achievement while the bottom panel focuses on ELA scores. Encouragingly, there is no evidence that test scores begin to decline in the years prior to the outbreak of

[7] Nearly every analysis of pandemic-related learning losses has found much larger test score declines in math than in ELA, for example.

Table 3: **Effect of Local Culture War Controversy on Student Achievement**

	(1) Math	(2) Math	(3) ELA	(4) ELA
Controversy	-0.0183***	-0.0183***	-0.00557	-0.00556
	(0.00668)	(0.00668)	(0.00484)	(0.00484)
Observations	522,359	522,359	548,946	548,946
R-squared	0.035	0.037	0.041	0.046
Number of Districts	11,215	11,215	11,252	11,252
Time FE	State-Year	State-Year	State-Year	State-Year
Grade FE	No	Yes	No	Yes

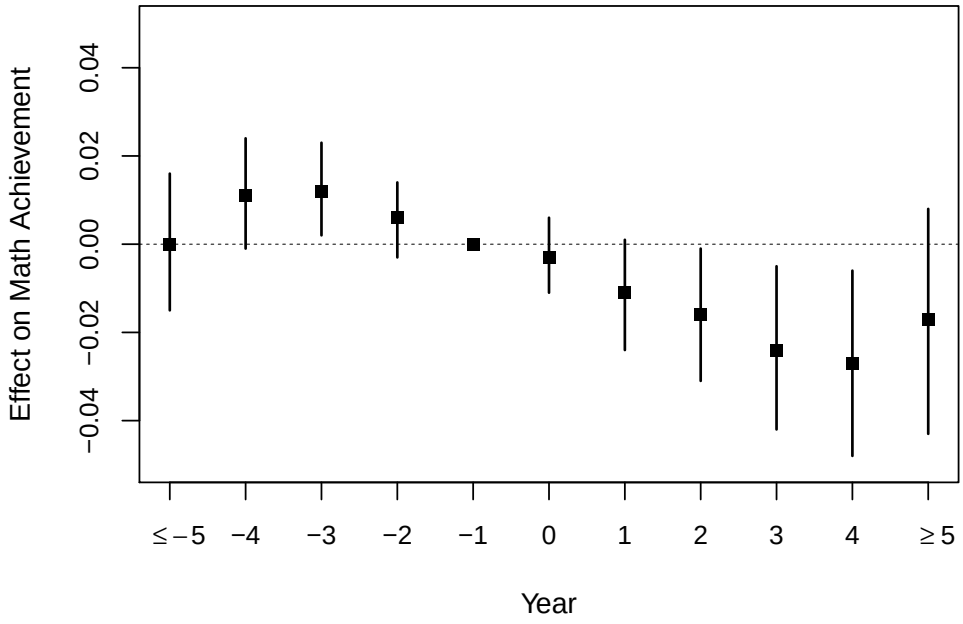
Robust standard errors clustered by district in parentheses
*** p<0.01, ** p<0.05, * p<0.1

local controversy. However, math scores show a clear decline starting in the year after the event and they remain significantly lower for at least four years. Although the individual point estimates are quite noisy, there is no evidence of a rebound in the short term—if anything, the impact appears to grow more negative over the first three years.

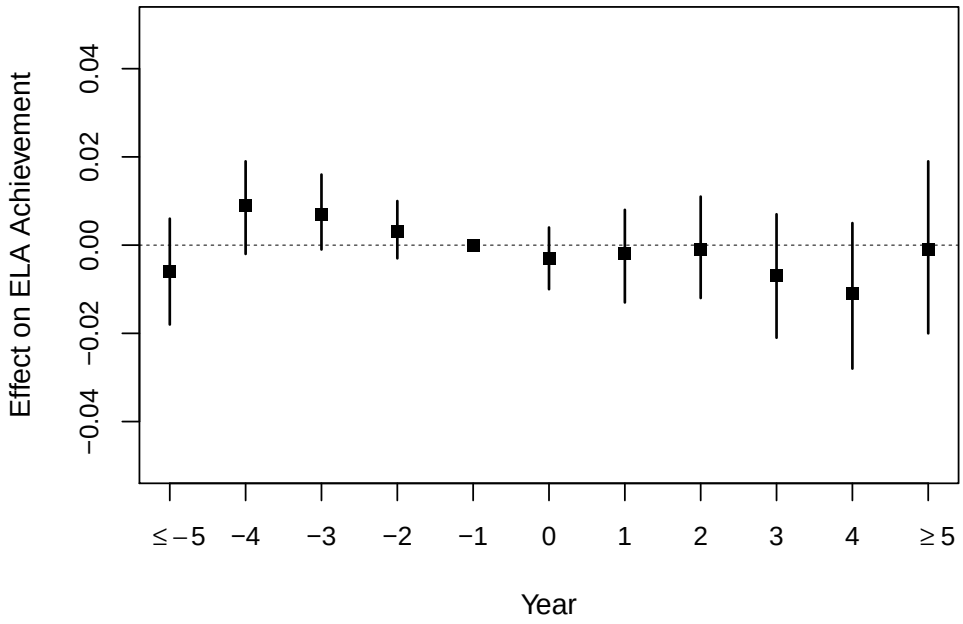
It is important to note that the Cato dataset records only the calendar year of each event, with no additional information about the precise day or month for most of the observations, while the test score data is reported based on academic years.[8] Thus, controversies that occur in the fall of calendar year t can affect achievement no earlier than the following spring, the first testing cycle after the event, which would show up as school year $t+1$ in the SEDA data. That may explain why effects for year zero are relatively small and not significant and why the impact does not appear until the following spring testing window.

To address concerns about bias due to staggered treatment and heterogeneity, I present alternative estimates using a specification in the spirit of de Chaisemartin and D'Haultfœuille (2020). Specifically, I group affected districts by the calendar year of each controversy and

[8]The database began including full dates starting in 2017. For the available years, more than 60 percent of the events occurred in June or later, after the testing cycle had already concluded for that calendar year.



(a) Math



(b) ELA

Figure 1: Event Study Plot for Effect of Local Controversy on Student Achievement

create a separate dataset that include each cohort of affected districts as well as all of the never-treated districts. I then estimate the difference-in-differences model separately for each cohort. These results are reported in the Supplemental Appendix. Although the individual estimates are more noisy due to the smaller sample sizes, the point estimates are almost all in the neighborhood of -0.02 to -0.03, suggesting minimal bias in the standard two-way fixed-effects model.[9]

Heterogeneity

The results presented thus far focus on average achievement. It is important to consider whether the learning disruptions disproportionately affect high-risk students. Because the SEDA dataset reports subgroup-specific achievement estimates only for student subgroups with at least 200 students, it is not possible to examine race-specific data in most districts. However, many districts to enroll a sufficient number of economically disadvantaged students, proxied in most states by their participation in the federal free- and reduced-price meals program and family participation in cash welfare and Medicaid.

These estimates appear in Table 4, which shows nearly identical declines in the math scores of both economically disadvantaged students and students not identified as such. As a result, there is no meaningful change in the achievement gap between these student subgroups. Consistent with the above results, there are no effects on ELA achievement for either group. The Supplemental Appendix also reports separate estimates for each tested grade. The declines in math achievement appear to be concentrated in elementary school grades, which is consistent with prior research showing that the learning of younger students is more sensitive to educational disruptions compared to the impact on older

[9]Events from both 2010 and 2011 stand out, neither resulting a negative point estimates. There are two possible explanations. First, as noted above, the data for these years is likely to be incomplete. Second, we observe the most number of post-event years for these cohorts, so it is possible that achievement ultimately recovers before the end of the panel, muting the difference-in-differences estimate.

students.

Mechanisms

Local political controversies can affect student learning through a variety of possible channels. First, school boards may replace senior leadership that becomes the focus of community opprobrium, producing turnover in administrative ranks and subsequent disruption in district operations. Voters may also replace current school board incumbents, which can also set in motion a chain reaction of other personnel changes. Second, the controversy can attract a disproportionate share of attention, energy, and resources, distracting school and district leaders from their primary focus on improving instruction and student outcomes. Third, if the controversy surrounds specific curriculum or teaching materials, districts may respond by adopting new curriculum, resulting in significant retraining needs, prep time, and transition costs for educators, which may negatively impact their productivity in the short term. Fourth, the negative publicity may cause higher-achieving students to leave the districts, mechanically reducing the average achievement.

That the decline in test scores is of comparable size for both economically disadvantaged and wealthier students—who are much more likely to have the resources necessary exit the public schools for private options or to move to a different district—provide some evidence against the last explanation. The Supplemental Appendix provides additional results that speak to this question as well. Examining total enrollment, I find no evidence that enrollment declines in the districts after a political controversy. In fact, total enrollment actually increases by between 2 and 3 percent after each event. There are also no changes in the demographic and socioeconomic composition of students served that would explain the changes in achievement.[10]

[10] Although there is a marginally significant decrease in the share of white students, the absolute magnitude of this decline is very small—less than one percentage point—and so cannot explain the decrease in

Table 4: **Effect of Local Culture War Controversy on Achievement Gap by Economic Disadvantage Status**

	(1) Math	(2) Math	(3) Math	(4) ELA	(5) ELA	(6) ELA
Controversy	-0.0155**	-0.0119*	0.00257	-0.00660	0.00156	0.00257
	(0.00700)	(0.00701)	(0.00468)	(0.00539)	(0.00517)	(0.00468)
Observations	402,566	396,568	332,963	423,616	417,621	332,963
R-squared	0.047	0.050	0.023	0.076	0.063	0.023
Number of Districts	9,707	9,531	8,472	9,742	9,566	8,472
Time FE	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year
DV	Not Disadvantaged	Disadvantaged	Achievement Gap	Not Disadvantaged	Disadvantaged	Achievement Gap

Robust standard errors clustered by district in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Unfortunately, there are no national data on staff, administrator, or school board turnover nor on curricula used by individual school districts, so it is not possible to directly test the remaining three possibilities. However, estimating separate effects for each type of controversy can provide some suggestive evidence about the underlying mechanisms.

These results are presented in Figure 2. Interestingly, most types of controversies appear to have little if any effect on student learning. Importantly, this is true for controversies surrounding instructional curriculum and those related to reading materials, suggesting that transition costs related to changes in pedagogy or teaching materials are unlikely to be driving the pooled results. Instead, large negative effects appear only for two types of controversies—those dealing with human origins and evolution and disputes surrounding race. Note that the latter category mostly includes fights unrelated to the actual content of instruction. For example, it covers potentially offensive statements made by school board members, disputes over school mascots, and community outrage in response to racially insensitive school artistic performances or themed events. In addition, there are smaller but still significant declines following controversies dealing with freedom of expression.[11] Overall, these results suggest that staff turnover and/or the diversion of attention and resources away from student academic outcomes are likely to represent the most plausible explanations for the observed declines.

It is also important to note that the point estimates for the declines linked to human origin and racial controversies are considerably larger, on the order of 0.06 to 0.07 standard deviations. These are substantively significant, corresponding to nearly 1.5 months of learning in mathematics in the affected grades. For these types of controversies, there is also evidence of declines in ELA scores, although the point estimates is considerably smaller

test scores.

[11] Although the decrease is much smaller than for racial and human origins controversies, there are far more events in this category, so the aggregate impact on achievement is nevertheless substantial.

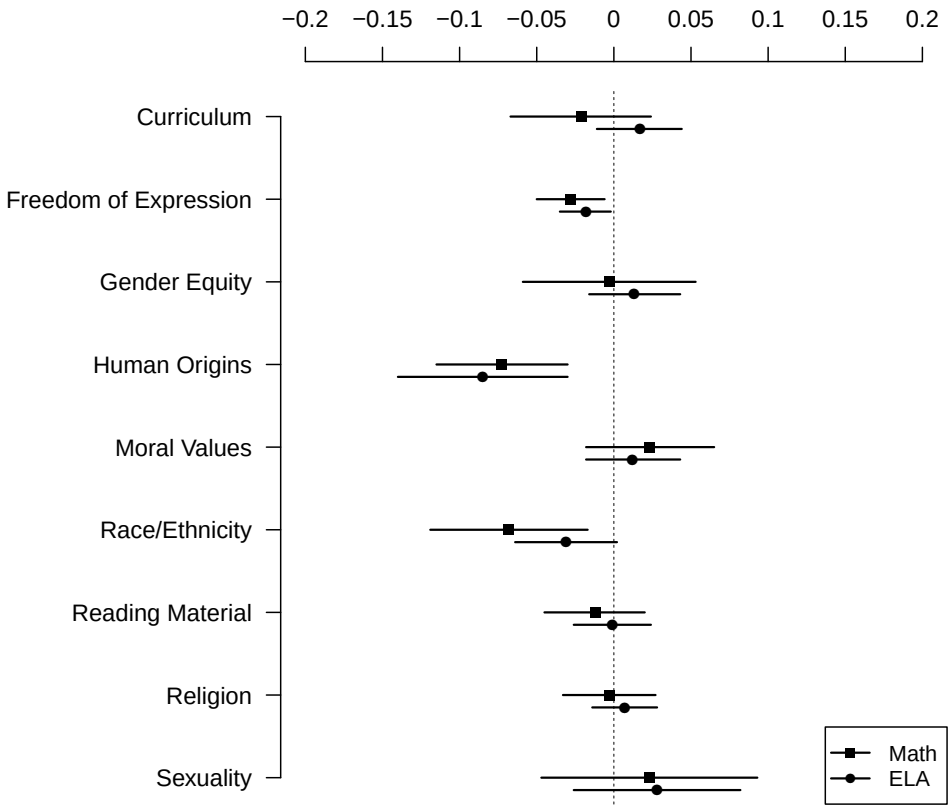


Figure 2: Student Achievement Impact by Topic of Controversy

for ELA achievement for racial controversies.

Conclusion

This study presents the first systematic evidence on how adult culture wars affect student academic outcomes. I show that local educational controversies significantly reduce student achievement, particularly in mathematics and especially in the wake of political battles over the teaching of evolution and disputes related to race and ethnicity. Moreover, student learning does not appear to catch up in the short term, with the test score declines persisting

for at least four years.

An important limitation is that the analysis focuses only on relatively high-profile cases that attract media attention. The results may not generalize to smaller-scale controversies that go unreported, and thus unrecorded in the data. To the extent that such controversies also affect student learning, however, they almost certainly bias my estimates toward the null since many of the district I use as the untreated control group likely experience such lower-profile events as well.

These results have broader implications for debates surrounding educational governance and policy. On the policy front, they offer caution about well-meaning efforts to push local schools to confront important social justice concerns. Although the most salient current educational policy controversies—including debates about Critical Race Theory and instruction about sexual orientation and identity—have a conservative bent, many of the controversies in the Cato database originated from progressive complaints and umbrage. Such forms of activism may be counterproductive. In recent years, for example, many have raised objections over racial disparities in student discipline (e.g., Liu, S.Hayes and Gershenson 2022) and the presence of armed school resource officers in school buildings (e.g., Weisburst 2019). I have little doubt that advocates working on these issues are sincere and genuinely concerned about student interests. However, it is important to consider the unintended consequences and learning disruptions produced by local controversies that can arise surrounding debates over such issues. Even if the advocacy is ultimately successful, it is not obvious whether the net effect for students is positive in the end.

In terms of governance, the findings underscore a central point first made by Chubb and Moe (1990): The interests of adult and those of students are not always aligned. This is true even when adults claim their policy advocacy is motivated by concern about students. Since only adults are able to vote in local school board elections, democratic processes that

empower adult voices may ultimately come at the expense of students and their learning needs. Given the troubling demographic divides between local school board electorates and the students taught in the public schools, as demonstrated by Kogan, Lavertu and Peskowitz (2021), local democracy may not be the optimal mechanism for providing high quality education to students. By reducing the scope of political conflict over educational policy issues, mechanisms of choice that empower parents and families directly—including school vouchers and charter schools—may benefit students in part by shielding them from the academic fallout that occurs in the wake of local political battles in the public schools.

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Online Appendix for Adult Culture Wars and Student Academic Achievement

Supplemental Online Appendix

Table A1: **Effect of Local Culture War Controversy on Student Achievement by Grade**

Panel A: Math						
	(1)	(2)	(3)	(4)	(5)	(6)
	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
Controversy	-0.0236***	-0.0231**	-0.0240***	-0.0134	-0.0128	-0.00125
	(0.00869)	(0.00927)	(0.00890)	(0.00826)	(0.00821)	(0.00907)
Observations	92,657	92,596	91,532	90,664	80,174	74,736
R-squared	0.095	0.063	0.053	0.049	0.054	0.056
Number of Districts	10,916	10,907	10,907	10,910	10,647	10,617
Panel A: ELA						
	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8
Controversy	-0.00573	-0.0117*	-0.000869	-0.00122	-0.00812	-0.00551
	(0.00693)	(0.00707)	(0.00685)	(0.00644)	(0.00611)	(0.00602)
Observations	92,622	92,617	92,494	91,760	90,384	89,069
R-squared	0.086	0.065	0.064	0.056	0.060	0.077
Number of Districts	10,914	10,909	10,907	10,910	10,847	10,853
Time FE	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year
Robust standard errors clustered by district in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Table A2: **Effect of Local Culture War Controversy on Student Achievement by Year of Event**

Panel A: Math								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Year 2010	Year 2011	Year 2012	Year 2013	Year 2014	Year 2015	Year 2016	Year 2017
Controversy	0.00555	0.0182	-0.0389***	-0.0224	-0.0204	-0.0302	-0.0319	-0.00810
	(0.0153)	(0.0194)	(0.0136)	(0.0189)	(0.0178)	(0.0191)	(0.0217)	(0.0181)
Observations	488,742	488,747	489,764	490,633	490,506	490,433	489,196	490,375
R-squared	0.037	0.037	0.037	0.037	0.037	0.037	0.037	0.037
Number of Districts	10,549	10,548	10,570	10,590	10,583	10,584	10,559	10,580
Panel A: ELA								
	Year 2010	Year 2011	Year 2012	Year 2013	Year 2014	Year 2015	Year 2016	Year 2017
Controversy	0.0163	0.0106	-0.0116	-0.00959	-0.0124	-0.00967	-0.000920	0.00601
	(0.0155)	(0.0162)	(0.0113)	(0.0133)	(0.0122)	(0.0132)	(0.0155)	(0.0125)
Observations	513,502	513,477	514,641	515,512	515,348	515,405	513,949	515,123
R-squared	0.045	0.045	0.045	0.045	0.045	0.045	0.045	0.045
Number of Districts	10,586	10,585	10,607	10,627	10,620	10,621	10,596	10,617
Time FE	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year
Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors clustered by district in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A3: **Student Enrollment and Composition**

	(1) log(Enrollment) (Tested Grades)	(2) log(Enrollment) (Total)	(3) Per-Grade Enrollment	(4) White (%)	(5) ED (%)	(6) ELL (%)	(7) Special Ed (%)	(8) Student-Teacher Ratio
Controversy	0.0319***	0.0216***	13.84***	-0.00918***	0.00133	0.00196*	-0.000805	0.279
	(0.00471)	(0.00387)	(3.534)	(0.00161)	(0.00258)	(0.00106)	(0.000933)	(0.242)
Observations	123,572	121,562	123,572	123,572	118,915	123,120	123,120	118,534
R-squared	0.032	0.063	0.400	0.104	0.275	0.196	0.178	0.013
Number of Districts	12,481	12,362	12,481	12,481	12,291	12,455	12,455	12,353
Time FE	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year	State-Year

Robust standard errors clustered by district in parentheses
*** p<0.01, ** p<0.05, * p<0.1