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The Unintended Effects of Common Core State Standards on Non-Targeted Subjects

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Abstract

From 2010 onwards, most US states have aligned their education standards by adopting the Common Core State Standards (CCSS) for math and English Language Arts. The CCSS did not target other subjects such as science and social studies. We estimate spillovers of the CCSS on student achievement in non-targeted subjects in models with state and year fixed effects. Using student achievement data from the NAEP, we show that the CCSS had a negative effect on student achievement in non-targeted subjects. This negative effect is largest for underprivileged students, exacerbating racial and socioeconomic student achievement gaps. Using teacher surveys, we show that the CCSS caused a reduction in instructional focus on non-targeted subjects.

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1 INTRODUCTION

Student achievement in the United States has been lagging behind the student achievement of many other industrialized countries for a number of decades (Hanushek et al., 2012; Shakeel and Peterson, 2021). The adoption of rigorous centralized education standards as a means of aligning basic elements of local school curricula has long been proposed to raise US student achievement (Costrell, 1994; Bishop, 1997). In 2008, a report published by the National Governors Association titled "Benchmarking for Success" suggested that US states should adopt a common core of internationally benchmarked education standards (Jerald, 2008). Such a standard, named the "Common Core State Standards" (CCSS), was subsequently developed for math and English Language Arts (ELA). The CCSS did not include other subjects such as science and social studies. From 2010 onwards, states could voluntarily adopt the CCSS. By 2021, 42 states had adopted the CCSS (Achieve Inc., 2013; CCSSI, 2021).

The theoretical literature on the effects of centralizing education standards does not offer a clear prediction on whether adopting the CCSS increases student achievement.[1] The empirical literature on the effects of the CCSS on student achievement has so far documented zero to modest positive effects on student achievement in the targeted subjects math and ELA. We replicate this analysis in our setting and come to largely the same conclusion, although our results suggest that the prior literature rather overestimates than underestimates any positive effects on student achievement in targeted subjects.[2]

[1]On the one hand, a centralized education standard could overcome the problem that education standards emerging from a decentralized process of setting standards tend to have inefficiently low degrees of rigor, potentially harming student achievement. This lack of rigor is caused by a free-riding problem induced by mobility of high school graduates across states and their pooling in the labor market (Costrell, 1994). This problem was of special relevance in the years before the adoption of the CCSS, as states had an incentive to adopt inefficiently low standards under the No Child Left Behind Act (NCLB) as means to increase pass rates of standardized tests (R. A. Maranto and A. G. Maranto, 2004; McCluskey and Coulson, 2007). On the other hand, a centralized education standard could also decrease student achievement by being less tailored to state-level preferences, and by abolishing the 'laboratory federalism' and competition between states for better education standards (Tiebout, 1956; Oates, 1999).

[2]Most of the early studies are correlational and find zero to modest positive associations between CCSS and student achievement in targeted subjects (Schmidt and Houang, 2012; Loveless, 2014, 2015, 2016). Exploiting quasi-experimental settings, more recent papers confirm the zero to modest positive effects (depending on study and subgroups therein) for Kentucky (Xu and Cepa, 2018), California (Gao

However, the main focus of our paper is on spillovers of the CCSS on student achievement in non-targeted subjects. Such spillovers have, to the best of our knowledge, not yet been studied in a causal framework. We argue that studying such spillovers is essential for evaluating the overall success of the CCSS in terms of student achievement and for guiding future reforms of education standards in general.

To close this research gap, we estimate the effect of the CCSS on student achievement in non-targeted subjects such as science and social studies. In theory, it is unclear whether any spillovers of the CCSS on student achievement in non-targeted subjects are positive or negative (or zero). On the one hand, the CCSS could be beneficial beyond its target subjects if, for example, skills acquired in a targeted subject such as math help students to perform well in a non-targeted subject such as science. On the other hand, the CCSS could have caused a reduction of instructional focus on the non-targeted subjects, possibly leading to a decline in student achievement in those subjects.

Simple correlations between CCSS adoption and student achievement in non-targeted subjects likely do not yield a causal answer to our research question. States that adopted the CCSS plausibly differ from states that did not adopt the CCSS in ways that affect student achievement through many channels other than education standards, for example through differences in political preferences or human capital. To overcome this identification problem, we estimate the effect of the CCSS on student achievement in non-targeted subjects in a two-way fixed effects difference-in-differences (DD) framework. This approach builds on the idea that states without CCSS adoption in a certain year act as counterfactuals for states with CCSS adoption in that year, after accounting for time-invariant differences between states and national differences between years.

To run these DD models, we combine state-level data on the adoption of the CCSS with

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and Lafortune, 2019), Chicago (Allensworth et al., 2021) and a subset of US states (Bleiberg, 2021). An exception is Song et al. (2019) who find negative effects on student achievement in targeted subjects of the adoption of general College and Career Readiness Content Standards (CCRCS). This study cannot be directly compared to the other studies, as the CCRCS include the CCSS but also other education standards. In our paper, we also estimate the effect of the CCSS on student achievement in targeted subjects in our setting and, again, find zero to modest positive effects. This finding largely confirms the conclusions of the prior literature, although our robustness checks suggest that the prior literature rather overestimates than underestimates any positive effects, see Appendix B for more details.

individual-level student achievement data from the National Assessment of Educational Progress (NAEP). The NAEP is known as The Nation's Report Card and provides unique student-level test score data for a large number of years, grades and subjects. In particular, it covers a range of subjects not targeted by the CCSS, namely science and different social studies (civics, economics, geography, and history). It is comparable across states and over time and covers the relevant years before and after the adoption of the CCSS. The NAEP student and teacher surveys complement the test score data by providing information on student characteristics, teacher characteristics and classroom instruction.

We find a significant negative effect of the CCSS on student achievement in non-targeted subjects. More specifically, being exposed to the CCSS for the entire school career (at the time of testing) as opposed to not being exposed to the CCSS at all decreases student achievement in non-targeted subjects on average by 0.08 units of a standard deviation. The effect size can be interpreted as a loss of learning worth approximately 25 percent to 30 percent of a school year. We regard this finding as reduced-form evidence that the CCSS induced a reduction of instructional focus on non-targeted subjects.

Next, we hypothesize that the negative effect is over-proportionally large for underprivileged students as these students (or their parents and environments in general) might be less able to compensate for the reduction of instructional focus. To test this hypothesis, we conduct subgroup analysis by student characteristics and find that the negative effect is mostly driven by Black and Hispanic students, and by students with free or reduced price lunch status, English language learner status, and disability status. We also conduct subgroup analyses by subject and grades and find that the achievement losses are most pronounced for science and for students in grade 4.[3] In sum, we conclude that the CCSS mainly reduced student achievement in non-targeted subjects among underprivileged students. This decline in student achievement exacerbates racial/ethnic and socioeconomic student achievement gaps as well as the achievement gap between

[3] In addition to the CCSS, some states adopted the Next Generation Science Standards (NGSS). The NGSS were released in 2013 with slow take-up rates by states relative to the CCSS. In appendix A, we demonstrate that our results are robust to controlling for NGSS adoption.

students with and without disabilities in the non-targeted subjects.

A series of robustness checks supports our main results. We show that the results are robust to event-study specifications and specifications with state-specific time trends. To account for state-specific shocks simultaneous to the adoption of the CCSS, we run a triple-difference model with students from private schools as an additional control group for which the CCSS was never mandatory. We also control explicitly for a large list of educational reforms. Furthermore, we account for recent developments in the econometric literature on two-way-fixed effects models and time-varying treatment effects and show that our results are not driven by negative weights (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2020; Athey and Imbens, 2021; Baker et al., 2021; Borusyak et al., 2021; Roth and Sant'Anna, 2021). Another set of robustness checks defines treatment based on information about actual CCSS implementation in the different states to account for the fact that CCSS adoption and CCSS implementation could diverge.

To uncover the mechanisms behind these results, we aim to understand what has changed in the classrooms of the students due to the CCSS. To this end, we draw on the NAEP teacher survey data on instructional focus. We find that the CCSS reduced teacher-reported instruction time, instructional resources, and some dimensions of the quality of teacher-student interactions for the non-targeted subjects. This finding suggests that the exclusion of science and social studies from the CCSS has signaled a lower relative importance of these subjects, resulting in a reduction of instructional focus.

Our paper contributes to the small but growing quasi-experimental literature on how the content of education standards affects individuals. Recently, it has been demonstrated that the content of US state education standards affects students' skills, attitudes, and occupational choice (Arold, 2021). Beyond education standards, the content of education in general influences skills (Cortes and Goodman, 2014; Goodman, 2019; Conger et al., 2021), labor market outcomes (Altonji et al., 2012; Fuchs-Schündeln and Masella, 2016) as well as identity, preferences and beliefs (Clots-Figueras and Masella, 2013; Cantoni et al., 2017; Bazzi et al., 2020).

Our outcome variable student achievement is not only interesting in its own right, but also an important predictor of economic outcomes at the individual and societal level. Student achievement has been found to affect earnings, income distribution, and economic growth (Hanushek and Woessmann, 2008, 2012). Notably, the predictive power of student achievement is much stronger than that of traditional measures of human capital used in the literature such as literacy rates (Romer, 1990), school enrollment (Barro, 1991), or years of education (Barro and Lee, 2013). Similarly, student achievement gaps between races/ethnicities (which we document for the non-targeted subjects) have been shown to account for relevant shares of the racial/ethnic gap in adulthood social and economic outcomes (Fryer, 2011). Although student achievement does not adequately capture non-cognitive skills (Heckman and Kautz, 2012; Jackson, 2018) which are increasingly important in the labor market (Deming, 2017), student achievement has been shown to be a strong predictor of not only cognitive skills, but also a broad set of individual-level outcomes including physical and mental health, and voting behavior (Borghans et al., 2016).

The paper proceeds as follows. Section 2 provides institutional background about the adoption and implementation of the CCSS. Section 3 outlines the empirical approach, while Section 4 describes the data. Section 5 reports the main results and heterogeneities of the effect of CCSS exposure on student achievement in non-targeted subjects. A series of robustness checks is presented in Section 6. Section 7 shows additional analyses of mechanisms, and Section 8 concludes.

2 INSTITUTIONAL BACKGROUND

2.1 Background and Data on the Adoption of the CCSS

The idea of centralizing education standards in the US has been discussed for decades (Costrell, 1994, 1997; Betts, 1998). In 2008, the National Governors Association (NGA), the Council of Chief State School Officers (CCSSO), and Achieve Inc. jointly published a report titled "Benchmarking for Success: Ensuring U.S. Students Receive a World-Class Education." (Jerald, 2008). The report prescribed that US states adopt a common core of internationally benchmarked standards in math and ELA to raise US achievement levels on international assessments. A number of philanthropic organizations, including the Bill & Melinda Gates Foundation, provided resources to enable the states to establish a common core of standards. Subsequently, numerous state governments, teachers' unions and other interest groups advocated for a systemic change in education standards across the nation. In 2009, a consortium of the National Governors Association and the Council of Chief State School Officers, with support from the U.S. Department of Education, set incentives for states to adopt the CCSS. If a state adopted the CCSS, it could get a waiver from some of the No Child Left Behind (NCLB) regulations.[4]

Our primary source for state-level data on the adoption of the CCSS is Achieve Inc. (2013), with an updated version provided by CCSSI (2021). CCSSI (2021) is the website of the Common Core State Standards Initiative provided by the National Governors Association Center for Best Practices and the Council of Chief State School Officers. They report if and when a state has adopted the CCSS.[5] Based on this data source, 42 states have adopted the CCSS permanently. Of the states that have adopted the CCSS permanently, most states adopted it in 2010, while a number of states adopted it in 2011 and one state, Wyoming, adopted it in 2012. In contrast, Alaska, Florida, Indiana, Nebraska, Oklahoma, South Carolina, Virginia and Texas have not adopted the CCSS permanently. In our baseline coding, we code students in these 8 states as never having been treated by the CCSS.[6] To account for the fact that some of those 8 states had

[4]The No Child Left Behind (NCLB) Act was a federal legislation signed in 2002 by President Bush. The act compelled states to design school accountability systems based on annual student assessments in math and reading that were linked to state standards. In December 2015, President Obama signed the Every Student Succeeds Act (ESSA). ESSA replaced the NCLB act. ESSA shifted NCLB's federal accountability aspect to the states.

[5]Throughout the paper, we treat Washington D.C. as a state.

[6]Minnesota is a special case as it did not adopt the CCSS for math, but for ELA, which we code accordingly.

adopted the CCSS temporarily, we present robustness checks in which we treat temporary adopters as treated from the year of the temporary adoption, even if they repealed/revised the CCSS later on. In the latter coding, only the states that never adopted the CCSS, Alaska, Nebraska, Virginia and Texas, remain in the control group, based on data from Bleiberg (2021) and CCSSI (2021). The map presented in Figure 1 illustrates the CCSS adoption graphically.

2.2 Background and Data on the Implementation of the CCSS

The implementation of the CCSS was not straightforward. There is anecdotal evidence that the CCSS presented challenges in teaching and testing to schools. Some teachers had difficulty adjusting to the new curriculum, and CCSS-based standardized tests were not always suitable. A case study in New York found that the CCSS led to exceedingly long and difficult exams. The rigor of the standardized tests exceeded the level of college readiness and represented more of an early college level (Polleck and Jeffery, 2017). In addition, some states did not have assessments and textbooks aligned with the CCSS until 2013 or later (Polikoff, 2017). Although not all challenges of the implementation of the CCSS have been overcome everywhere (Polikoff, 2015; Bay-Williams, 2016), more recent surveys show that most teachers feel prepared to teach the CCSS (Scholastic, 2014), have acquired good or excellent knowledge of the CCSS (Kane et al., 2016), base their curricula on the CCSS (Opfer et al., 2016), and use textbooks based on the CCSS (Blazar et al., 2019). Still, the challenges surrounding the implementation of the CCSS warrant robustness checks of the treatment coding in which we base the definition of students' CCSS exposure on the implementation of the CCSS, not just its adoption.

To account for this issue, we draw on a variety of data sources to create alternative treatment indicators based on CCSS implementation. They incorporate information on states' legal CCSS implementation requirements, actual CCSS implementation strategies, effectiveness of CCSS implementation, temporal CCSS implementation, and CCSS-aligned standardized testing. Detailed explanations of the different treatment indicators and their data sources are provided when reporting the corresponding results in Section 6 on robustness.

3 IDENTIFICATION STRATEGY

To estimate the effect of the CCSS on student achievement in non-targeted subjects, we run a two-way fixed effects difference-in-differences model (DD), and several extensions including models with state-specific time trends and a triple-difference model (DDD). The DD model takes advantage of the fact that some states did not adopt the CCSS. This approach builds on the idea that states without reforms in a given year act as counterfactuals for states with reforms in that year, after accounting for time-invariant differences between states and national differences between years. To capture this idea econometrically, we estimate a DD model as follows:

$$T_{istuv} = \beta * CCSS\_Exposure_{istuv} + \gamma * X_{istuv} + \mu_s + \lambda_t + \theta_u + \kappa_v + \epsilon_{istuv} \tag{1}$$

where T_{istuv} captures standardized student achievement of student i who goes to public school in state s, and takes the test in year t, grade u and subject v. Our main estimates pool all subjects that are not targeted by the CCSS, across all available grade levels. The treatment parameter $CCSS\_Exposure_{istuv}$ captures the dosage of CCSS exposure of student i attending public school in state s, and taking the test in year t, grade u and subject v. Unless noted otherwise, it is defined as the share of schooling years in which a student was exposed to the CCSS (at the time of the survey). It has the same domain (between 0 and 1) for students of different grades, making effect sizes of students from different grades comparable.[7] β is the parameter of interest capturing the effect on student achievement of being exposed to the CCSS for the entire school career until the survey date (exposure=1) relative to never being exposed to the CCSS until the survey

[7] In Section 6 we show robustness checks for different treatment definitions.

date (exposure=0). In our preferred treatment coding, we define a year in a given state as exposing a student to the CCSS if the state had adopted the CCSS permanently before that year or in the same year. In robustness checks, we employ other treatment definitions.

A vector of student-level control variables X_{istuv} includes indicator variables for gender, race/ethnicity, subsidized lunch status (indicator variable equals one if student receives free or reduced price lunch), English language learner status, disability status, parental education, and home possessions (separate indicator variables for computer and books). State fixed effects μ_s, test year fixed effects λ_t, grade fixed effects θ_u, subject fixed effects κ_v, and an error term ϵ_{istuv} complete the model. Note that test year and grade jointly define each cohort. Throughout the paper, all standard errors are clustered at the state level to account for potential correlation of error terms across years within states. Regressions are weighted to be population representative. We run the main DD estimations on a sample of students attending public schools (district and charter schools) only, as the implementation of the CCSS was never mandatory for private schools.

This baseline model addresses a variety of concerns about our ability to estimate the causal effect of CCSS exposure on student achievement in non-targeted subjects. First, one might be worried that state-level differences in domains such as returns to education, cultural characteristics that promote educational success, genetic endowments, or preferences for centralizing policies are correlated with CCSS exposure and affect student achievement. The state fixed effects eliminate all constant differences between states. Hence, we exploit cross-cohort variation within states. Second, one might be concerned that national trends in student achievement, for example fueled by overall economic development or national education policies, appear as effects of CCSS exposure. However, our year fixed effects capture all variation in student achievement that occurs nationwide between years. In addition, our individual-level control variables ensure that the students we compare are similar with regards to demographic and socioeconomic characteristics. For these reasons, our DD model yields a causal effect of CCSS exposure on student achievement in non-targeted subjects if the main identifying assumption about parallel trends holds. It assumes that in the absence of states adopting the CCSS, the change in student achievement in treated states would have been the same as that in non-treated states.

Although this assumption cannot be directly tested, we perform a series of robustness checks to assess its plausibility. We begin with running non-parametric event-study specifications, in which the adoption of the CCSS in a given state and year is defined as the event. In contrast to the DD model, the event-study model can assess non-linear pre-reform trends in student achievement. If student achievement prior to the adoption of the CCSS was trending in the direction of the estimated CCSS effects, this could indicate a bias from underlying trends in the data. Another advantage of the event-study model is that the time course of effects of the adoption of the CCSS can be assessed by disentangling effects which occur directly at the time of the CCSS adoption from those which occur gradually after the CCSS adoption. Specifically, we estimate the effect of the CCSS adoption in year t_s on student achievement k years before and after CCSS adoption, as captured by the parameter vector β_k, see equation (2). These effects are estimated relative to the year of reform k=0.[8]

$$T_{istuv} = \sum_{k=-6}^{6} 1(t_{is} = t_s + k)\beta_k + \gamma * X_{istuv} + \mu_s + \lambda_t + \theta_u + \kappa_v + \epsilon_{istuv} \tag{2}$$

Although the non-parametric specification captures an overall pre-trend, it does not account for state-specific trends. To address this, we perform analyses in which we add state-specific linear time trends, linear and quadratic time trends, as well as linear, quadratic and cubic time trends to equation (1). The state-specific linear time trend variable interacts each state fixed effect with a re-scaled year variable that equals one in the first year of observation, two in the second year of observation, and so forth. The corresponding reform effect is identified from within-state deviations in student achievement from smooth linear trends that coincide with the different timing of

[8]To smooth the numbers of observations across years, the observations are grouped together to bins of 2 years for all pre- and post reform years except for the bins at the beginning (end) of the domain which additionally include the years prior to (following) the domain's starting (ending) year.

CCSS adoption across states. State-specific quadratic and cubic time trends are defined analogously. Taken together, the event-study model and the models with state-specific trends address concerns about underlying trends in the data.

Even if there are no underlying trends in the data, shocks or events that occur simultaneously to the adoption of the CCSS remain a threat to the parallel trends assumption. To address this issue, we first run a triple difference model (DDD). For this analysis, we add an additional control group of private school students to our DD sample of public school students. The CCSS has never been mandatory for private schools. Correspondingly, we code private school students as not being exposed to the CCSS, even if their school is located in a state that has adopted the CCSS for public schools in some years of the school career of the students. Given the possibility that some private schools might have voluntarily implemented some elements of the CCSS, we should interpret DDD effects as lower-bound estimates,[9] (at least under the assumption of no endogenous selection between public and private school students as we discuss below).

Econometrically, we capture our third difference using a school type indicator variable (public school vs. private school). The full DDD follows equation (1) but adds a baseline indicator for school type as well as a full set of fixed effects interactions. This set includes school-type-by-state fixed effects (for example to control for state-specific time-constant regulation differences between private and public schools), school-type-by-year fixed effects (for example to control for changes in the national funding of public schools), and state-by-year fixed effects (for example to control for state-specific policies and programs directed at students or their families regardless of school type). The DDD uses variation at the school-type-by-state-by-year level to identify the effect of the CCSS on student achievement from differences in student achievement of students who attend public school compared to student achievement of students who attend private school coincident with the timing of the CCSS adoption in each state. The identifying assumption

[9] If some private schools implemented elements of the CCSS, we would code some students as untreated although they have in fact received some treatment. Hence, we would erroneously difference out some part of the real effect of the CCSS.

of the DDD requires that there is no other school type specific variable correlated with the CCSS adoption that affects student achievement. This identifying assumption is substantially weaker than that of the DD model, as it cancels out all confounding variables that affect public and private school students equally.

Still, policies that occurred simultaneously to the adoption of the CCSS which affect public and private school students differently and which have an effect on student achievement could bias the models presented so far. To address this concern, we collect data on reforms of public schooling policies and private schooling policies. Examples of public schooling policy controls include public education expenditures (as measured by the district-by-year-level per-pupil education expenditures in logarithmized dollars), waivers from NCLB/ESSA accountability requirements, and the adoption of the Next Generation Science Standards. Examples of private schooling policy controls include states' control of private school licensure, of private school curricula, or publicly funded voucher laws. Similarly, we also add controls for policies on homeschooling and compulsory schooling as a robustness check, as they might indirectly and differently affect public and private schools. The extent of recordkeeping requirements for homeschooling is an example of a homeschooling policy, and the number of compulsory schooling years is an example of a compulsory schooling policy. Adding these policy variables as controls can alleviate many concerns about simultaneous policies biasing the results.

Another threat to validity is that the adoption of the CCSS might have caused heterogeneous selection of specific groups of students into school types. For example, estimates could be biased if students with politically conservative parents left the public school system at the same time that the CCSS was adopted, and if political conservatism of parents affects student achievement. Neither the DDD model, nor the DD models based on a sample of public school students, are immune to this selection issue. We address this concern by running a DD model with a joint sample of public and private school students, whose reform effects are net of any heterogeneous selection between school types.

Even if the parallel trends assumption holds, the previously presented two-way fixed effects models could yield biased results due to time-varying treatment effects. If already-treated students act as controls for later-treated students in settings with staggered treatment timing, time-varying treatment effects can bias results away from the true effect. This issue, also referred to as "negative weighting", has received much attention in the recent econometric literature (Goodman-Bacon, 2018; Callaway and Sant'Anna, 2020; Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2020; Athey and Imbens, 2021; Baker et al., 2021; Borusyak et al., 2021; Roth and Sant'Anna, 2021). We address this issue by conducting a robustness check in which we exclude all 2x2 DD comparisons from the sample in which already-treated students act as controls. In sum, each of the presented approaches in this section has different identifying assumptions and addresses concerns about underlying trends in the data, simultaneous shocks, selection, or negative weights, in a different way. In our view, robust insights about the effect of the CCSS on student achievement in non-targeted subjects can be obtained if the different approaches yield similar results.

4 DATA

4.1 Student Achievement Data

We merge data on the adoption and implementation of the CCSS with standardized student achievement data. The data sources on the adoption and implementation of the CCSS are described in Section 2 on the institutional background and in Appendix C. For student achievement, we use the restricted-use individual-level dataset of the NAEP. The NAEP is a congressionally mandated project which is nationally representative of the US student body. It has measured the knowledge of US students in various subjects since 1990, and is also known as The Nation's Report Card.[10] The assessments are administered by the National Center for Education Statistics (NCES), an institution within the Institute of Education Sciences (IES) and the US Department of Education. Notably, there is a

[10]Throughout the paper we use the Main-NAEP and not the Long Term Trend NAEP, as the Main-NAEP has much larger sample sizes and is state-representative.

significant overlap between CCSS and NAEP items (Daro et al., 2015). For example, 79 percent of items on grade 4 and 87 percent of items on grade 8 of the 2015 NAEP math assessment were also covered by the CCSS.

The NAEP individual-level dataset has several advantages for our analysis. First, it provides information on student achievement at the individual level for a large number of years, grades and subjects for all US states. Second, the NAEP test is comparable across states and over time, which allows for consistent standardization and two-way fixed effects difference-in-differences estimations. Third, it includes a rich set of individual-level control variables such as student gender, race/ethnicity, and various socio-economic background variables, among others. Hence, we can control flexibly for students' pre-reform characteristics and perform subgroup analyses. We set missing values of controls to zero and add separate explanatory binary variables to all regressions to account for these missing values, unless noted otherwise.[11] Fourth, the NAEP also administers student achievement tests and surveys in private schools, which we can exploit for identification given that the CCSS was never mandatory for private schools. Fifth, the NAEP has also surveyed the teachers of a subset of the students in the sample. We can make use of the teacher data to investigate changes in classroom instruction resulting from the CCSS, as presented in Section 7 on mechanisms.

For our analysis, we use the NAEP data from 2005 to 2015 for all available grade levels, namely grades 4, 8 and 12. We exclude data from 2004 and before, as NAEP sampling increased tremendously after 2001 (and no testing was done in non-targeted subjects between 2002 and 2004 and after 2015). Besides, this sample cut provides pre- and post reform periods of roughly equal duration. We use the NAEP data from science, civics, economics, geography, and history to capture student achievement in non-targeted subjects.[12] Table A.1 lists the grades in which the NAEP tests were administered for each of the five subjects for each year between 2005 and 2015 (and for which state identifiers of students are available). We use the student-level data from all these subject-year-grade

[11] Our results are robust to not imputing the missings, see Table A.10.

[12] Other tests such as theater, visual arts, and music were not tested in the relevant years.

combinations in our main analysis. The resulting sample consists of more than one million students.[13] [14]

4.2 Descriptive Statistics

Table A.2 presents the mean, standard deviation, minimum and maximum of the main variables. The main outcome variable is student achievement, which we standardize to have a mean of zero and a standard deviation of one in the first year of each available grade-subject combination. This standardization allows the mean and variance to flexibly evolve in the following years, which explains why their overall mean and standard deviation reported in Table A.2 are close but not equal to zero and one, respectively. Regarding student characteristics, we note that about half of the sample is female and almost 60 percent is White. The shares of Black and Hispanic students are 15 percent and 20 percent, respectively. 5 percent of the students in the sample are Asian students (including Pacific Islanders). About 6 percent of the sample have English language learner status, and 11 percent disability status. To assess what share of students come from a low socioeconomic background, we can look at the shares of students receiving subsidized lunch (43 percent), having parents who did not finish high school (8 percent), having no computer at home (10 percent), or having less than 10 books at home (13 percent). We also show descriptive statistics for variables measuring the instructional focus on non-targeted subjects and teacher characteristics (both based on the NAEP teacher surveys), which we analyze in more detail in Section 7.

[13]Throughout the paper, we report the number of observations rounded to the nearest ten digit to comply with data protection regulations of the NCES.

[14]In the NAEP, no student takes the entire student achievement test. Instead, the NAEP reports plausible values for overall student achievement on a test estimated from the sample of questions that were administered to a student. We make the arbitrary choice of selecting the second plausible value the NAEP provides. The raw correlation between the second plausible value and the average of the first five plausible for student achievement in, for example, science equals 0.95 in our sample. Our results are robust to estimating effects using any other plausible value.

5 RESULTS

5.1 Main Results

Table 1 presents estimates of the statistical relation between CCSS exposure and student achievement in non-targeted subjects for different sets of control variables. In column 1, there are no control variables. The positive and statistically significant correlation between CCSS exposure and student achievement in non-targeted subjects implies that students exposed to the CCSS perform better in non-targeted subjects than those not exposed to the CCSS. This correlation could be caused by CCSS exposure improving student achievement in non-targeted subjects, for example through positive spillovers. It could also be caused by above-average student achievement in non-targeted subjects leading to CCSS exposure. This reverse causality could occur, for example, if above-average student achievement in non-targeted subjects before the reform promotes confidence in national education policies, thereby encouraging states to adopt the CCSS. Moreover, the observed positive correlation between CCSS exposure and student achievement in non-targeted subjects could also be driven by third variables such as parental education. This would be the case, for example, if states with a high proportion of students with highly educated parents are more likely to adopt the CCSS, for instance if these parents vote disproportionately for parties that endorse the CCSS, and if parental education itself increases student achievement in non-targeted subjects.

To isolate the effect of CCSS exposure on student achievement, we add control variables in columns 2-3. The positive correlation of CCSS exposure and student achievement in CCSS subjects remains almost unchanged conditional on student-level control variables (column 2). In the full model with both student-level controls as well as state and year fixed effects, the positive correlation becomes negative (column 3). The full model is our preferred model, as it flexibly accounts for demographic and socioeconomic differences between students as well as time-invariant differences between states and national differences between years. Unless noted otherwise, all further models presented in this paper are full models.

More specifically, exposure to the CCSS during the entire school career (at the time of testing), as opposed to no CCSS exposure at all, decreases student achievement in non-targeted subjects on average by 0.08 units of a standard deviation. This effect is statistically significant at the 5 percent level. To illustrate the effect size, we draw on the literature on education production functions, which suggests that the gain in learning from one year of schooling is equivalent to about one-quarter to one-third of a standard deviation increase in student performance on standardized tests (Woessmann, 2016). Correspondingly, the CCSS-induced learning loss in non-targeted subjects is equivalent to approximately 25 percent to 30 percent of a school year.

5.2 Subgroup Analysis

The negative effect of the CCSS on student achievement in non-targeted subjects need not be evenly distributed across subgroups. We hypothesize that students from underprivileged backgrounds may be disproportionately disadvantaged as it is more difficult for themselves, their parents, or their social environments in general to compensate for the reduction of instructional focus on non-targeted subjects.[15] For example, parents from underprivileged backgrounds might be less able or might have less time to help their children with homework themselves or pay for private tuition. To test this hypothesis, we conduct subgroup analyses by students' demographic and socioeconomic characteristics.

As reported in Table 2, we find that the negative effect on student achievement in non-targeted subjects is not evenly distributed across subgroups. With respect to race/ethnicity, the student achievement of Hispanics and Blacks in non-targeted subjects is reduced disproportionately as a consequence of the CCSS, while there are almost no reform effects for Whites and Asians. Race and ethnicity aside, the negative effect is larger for students who qualify for subsidized lunch than for those who do not. Furthermore,

[15] In Section 7 on mechanisms, we use outcomes measuring instructional focus based on teacher survey data to show explicitly that the CCSS caused a reduction of instructional focus on non-targeted subjects.

students with English language learner status or disability status lose disproportionately. Taken together, these results indicate that students from groups typically regarded as socially, economically or physically underprivileged suffer most from CCSS exposure in terms of their achievement in non-targeted subjects.

We also perform subgroup analyses by subjects and grades. As far as subjects are concerned, the negative effect on student achievement in non-targeted subjects comes mostly from science, as we show in Table A.3. In terms of grades, the negative effect is mostly due to students from grade 4, see Table A.4. These subgroup effects should be interpreted with caution as testing frequencies and sample sizes are much larger for science (compared to civics, economics, geography, and history) for grades 4 and 8 (compared to grade 12). Still, the large subgroup effect for students in grade 4 makes intuitive sense as teachers in elementary school have the greatest flexibility in shifting the instructional focus. [16]

6 ROBUSTNESS

In this section, we test the robustness of the main result to account for four general types of concerns. First, we test the plausibility of the identifying assumption about parallel trends between treatment and control groups in the two-way fixed effects DD model in various ways in a series of econometric robustness tests. Second, we assess whether negative weighting induced by time-varying treatment effects affects our two-way fixed effects DD estimates and their interpretation. Third, we test whether re-defining our treatment variable to incorporate information about CCSS implementation changes our results. Fourth, we conduct a series of further specification checks to ensure that our

[16]Yet another subgroup analysis we perform is motivated by the hypothesis that states with high average student achievement levels before the adoption of CCSS might suffer more from CCSS adoption in terms of student achievement in non-targeted subjects relative to states that had been low-performing to begin with. To test this hypothesis, we perform subgroup analysis by quartiles of the states' pre-CCSS student achievement level. As presented in Table A.5, there is no linear effect pattern across quartiles. Although the effect difference between the lowest quartile and the highest quartile is negative which could support our hypothesis, we do not emphasize this subgroup finding as it does not hold in robustness checks on the econometric and treatment specifications as described in Section 6.

results do not hinge on arguably arbitrary specification choices in the main regression.

6.1 Robustness Tests on Parallel Trends Assumption

A first plausibility test for the identifying assumption of parallel trends in outcomes between treatment and control groups are event-study specifications. Here, the adoption of the CCSS in a given state and year is defined as an event. As depicted in Figure 2, no statistically significant pre-trend in student achievement in non-targeted subjects can be identified prior to the adoption of the CCSS. If at all, student achievement in these subjects was improving before the reform. In contrast to this insignificant positive pre-trend, student achievement declined substantially after the reform, both in terms of size and significance. This finding supports the validity of the parallel trends assumption and confirms the negative effect of the CCSS on student achievement in non-targeted subjects reported in the main analysis.

To further account for the possibility of state-specific trends and simultaneous shocks, Table 3 presents econometric robustness tests for the main regression results (shown again in column 1 to facilitate comparison) by first adding state-specific linear time trends (column 2), then adding state-specific quadratic time trends (column 3), and finally adding state-specific cubic time trends (column 4). In column 5, DDD estimates are reported in which a set of data on private school students is added to the sample. Here, all state-specific time trends are replaced with a fixed effect for school type (public school vs. private school), as well as with school-type-by-state, school-type-by-year, and state-by-year fixed effects. In column 6, we run the main two-way fixed effects DD model with a sample of all students from public and private schools.

We find that the negative and significant main effect is robust across specifications and becomes even slightly more negative in the models with state-specific time trends of various orders. The same is true for the DDD model. The model which is estimated based on the sample of both public and private school students yields an effect that is slightly smaller than that of the main model, but still negative and significant. This series of econometric robustness checks demonstrates that the main effects are not affected by underlying state-specific trends in the data, endogenous selection between public and private school students, and shocks that occur simultaneously to the adoption of the CCSS as long as they affect both public and private school students equally.

While a large variety of state-specific shocks, including policy reforms in many areas, plausibly affect public and private schools students equally, the parallel trends assumption could still be violated by state-specific policies for public and private schooling. To test for this, we perform robustness checks which explicitly control for state-specific time-varying policies for public schooling (Table A.6) and private schooling (Table A.7), respectively. The results are robust throughout. In addition, policies on homeschooling and compulsory schooling could indirectly affect public and private schooling decisions. To account for this possibility, we also present robustness checks in which we add controls for homeschooling and compulsory schooling. As can be seen in Table A.8 for homeschooling and Table A.9 for compulsory schooling, the results are robust. Taken together, the findings in this subsection show that neither underlying trends in the data nor shocks occurring simultaneously to the adoption of the CCSS give rise to our main result.

6.2 Robustness Tests on Time-Varying Treatment Effects

Another potential threat to identification of the presented two-way fixed effects DD models are time-varying treatment effects. The main estimate is a weighted sum of the average treatment effects in each state and year (i.e. of each 2x2 comparison), with weights that may be negative. These negative weights can cause the main regression coefficient to be negative although all the average treatment effects are positive. Weights can be negative if already-treated units act as controls for later-treated units, in settings with time-varying treatment effects and staggered reform adoption.

Our event-study graphs presented above provide first insight into the potential bias induced by time-varying treatment effects, as they allow us to separate instantaneous from gradual reform effects. Event-studies are immune to bias from time-varying treatment effects as long as the pattern of effects is the same for all treatment cohorts. To explicitly explore the issue of time-varying treatment effects, we create a sample in which already-treated students never act as controls. Creating this sample is relatively straightforward in our setting as most states adopted the CCSS in 2010 or did not adopt the CCSS at all. By excluding the six states which adopted the CCSS in 2011 and 2012 from the sample, we transform our staggered setting into a non-staggered setting that is immune to negative weights. As shown in column 1 of Table A.10, the negative significant effect of the CCSS on student achievement in non-targeted subjects remains in this modified sample. This finding demonstrates that the main result is not driven by time-varying treatment effects and negative weights.

6.3 Robustness Tests on Treatment Definition

A different type of concern is that CCSS adoption and CCSS implementation could diverge. In our preferred treatment coding, we count all years as causing CCSS exposure for a student in a given state, in which the state had permanently adopted the CCSS before that year or at most in the same year. However, states that have adopted the CCSS permanently may not have implemented the CCSS comprehensively and thus may not be creating actual exposure. Conversely, states that have not adopted the CCSS permanently may have adopted and/or implemented the CCSS temporarily or partially.

To test whether our results hold if we define treatment based on CCSS implementation, we re-run our main regression using five different treatment variables, each capturing different information about the implementation of the CCSS. Under these treatment definitions, a school year in a given state is defined as a school year with CCSS exposure if that state (i) expects teachers to fully incorporate the CCSS in their classroom instruction, (ii) followed at least two out of three CCSS implementation strategies (professional development, new instructional materials, joined testing consortium), (iii) observed an effective change in state standard content due to the adoption of the CCSS, which we define to mean that no state standard existed that closely resembles the CCSS before the adoption of the CCSS, (iv) adopted and/or implemented the CCSS at least temporarily, or (v) mandated standardized tests aligned to the CCSS. Further information on each treatment definition, its construction, data sources, including a table containing state-specific coding information for all treatment definitions are provided in Appendix C.

We present the results for each treatment definition (including our main result to facilitate comparison) for the entire sample and the subset of students in grade 4, for which we have observed the largest subgroup effects in the main analysis. As shown in Table 4, we find a negative point estimate in all specifications, ranging from -0.088 to -0.035 units of a standard deviation for the overall sample and ranging from -0.177 to -0.098 units of a standard deviation for the subsample of students in grade 4. For the latter subgroup, all effects are statistically significant. Taken together, these findings suggest that results using treatment definitions based on CCSS implementation rather than CCSS adoption lead to the same overall conclusion as the main results in Section 5.

6.4 Further Specification Checks

In addition, we want to assess whether our results are robust to a number of modifications of our main regression. As indicated before, we set missing values of controls to zero and add separate explanatory binary variables to account for these missing values in our main regressions. The shares of missing values for the student control variables are below 10 percent for all variables except for parental education. For the latter approximately 40 percent of the values are missing, which can be mostly explained by the fact that this question was not asked in grade 4. To test whether the parental education control and its imputation affect the results, we run our main regression without controlling for parental education. As shown in column 2 of Table A.10, the effects do not differ meaningfully. As an additional robustness check, we do not impute missing values of any control variables (in addition to leaving parental education out of the set of control variables). As can be seen in column 3, the results are robust.

Moreover, we test the robustness of our main regression by modifying the definition of the treatment variable that captures the dosage of a student's exposure to the CCSS. So far, we have defined this variable as the share of schooling years a student was exposed to the CCSS (at the time of the survey). Alternatively, we now define exposure to the CCSS as the number of schooling years a student was exposed to the CCSS (at the time of the survey). As shown in column 4 of Table A.10, the negative effect is now insignificant and much smaller, but has a similar interpretation. In particular, we find that a one-year increase in CCSS exposure reduces student achievement in non-targeted subjects by 0.006 units of a standard deviation. Assuming 12 years of schooling, the total effect of CCSS exposure throughout the entire school career, as opposed to no exposure, equals 0.072 units of a standard deviation (0.006*12). This is close to the result of our main regression (0.079 units of a standard deviation) in which we define the treatment variable as a share of years. In addition, we show that our results are robust to excluding charter schools from the sample of public schools, or omitting population weights, respectively, see columns 5 and 6.

7 MECHANISMS

To study what gave rise to the observed effect on student achievement in non-targeted subjects, we examine what changed in students' classrooms in these subjects due to the CCSS. To this end, we draw on teacher survey data, provided by the NAEP for a subset of waves and classrooms. This data is suitable for our analysis for several reasons. First, it contains a rich set of subject-specific questions on instructional focus in the classroom comprising instruction time, instructional resources, five measures of differentiated instruction, and four measures of the quality of teacher-student interactions. We note that the instructional focus outcomes could be endogenous to the reform and hence should be interpreted as changes in teachers' perceptions of classroom instruction rather than evidence based on administrative data (which is not available for these outcomes at the subject-state-year-level).

Second, the NAEP includes teacher background characteristics which we can use as control variables and for subgroup analyses. Third, the NAEP teacher surveys are linked to the NAEP student achievement tests and student surveys. This link allows us to examine how instructional practices changed in non-targeted subjects according to the teachers who taught precisely the tested students from our main analysis. Fourth, the teacher surveys are standardized in the same way as the student surveys and achievement tests, making them comparable across states and years and thus suitable for a two-way fixed effects difference-in-differences approach. In fact, we can keep the empirical framework from the previous sections largely unchanged, but we use instructional focus outcomes instead of student outcomes and add teacher controls, thus ensuring methodological consistency with the previous sections.

Table 5 presents the results of CCSS exposure for instructional focus in non-targeted subjects. Overall, we find that the CCSS caused a reduction in instructional focus on the non-targeted subjects. Specifically, we observe negative significant effects of the CCSS on weekly instruction time, provision of instructional materials and resources, and two dimensions of the quality of teacher-student interactions. These two dimensions are setting and discussing goals with students. The extent of differentiated instruction did not change meaningfully. To illustrate the interpretation of the reported point estimates, we note that teachers of students who are fully exposed to the CCSS are 17 percentage points less likely to teach these students more than five hours per week in non-targeted subjects than teachers of students with no CCSS exposure, conditional on teacher characteristics, student characteristics as well as state, year, grade and subject fixed effects.[17]

The reduction in instructional focus on non-targeted subjects does not have to be evenly distributed across teacher subgroups. Understanding which subgroups of teachers drive the effects is interesting in itself and can be useful for tailoring policy advice to specific groups of teachers. We perform subgroup analyses for the four instructional focus outcome variables which were most affected by the CCSS, namely instruction time,

[17] The answer categories of the instructional outcome variables were coded differently in different survey waves of the NAEP. Hence, we code the variables as reported in the footnote of Table 5 to ensure consistency across waves.

instructional resources, and teacher-student interactions (setting and discussing goals). We conduct subgroup analyses by teacher characteristics which include teacher race/ethnicity, teacher education, teacher certification, and teacher experience. The subgroup pattern is not evenly distributed across instructional focus outcomes, but in general we find the largest reductions in instructional focus on non-targeted subjects for White teachers and teachers without a certification, see Tables A.11, A.12, A.13 and A.14, respectively for the four instructional focus outcomes.

Altogether, these results show that the adoption of the CCSS has shifted the instructional focus away from the non-targeted subjects. This finding is in line with the results from Section 5, which show a decline in student achievement in these subjects. It is also consistent with previous literature showing that instructional inputs affect student achievement. Increases in instruction time (Taylor, 2014), instructional resources (Holden, 2016), and the quality of teacher-student interactions (Allen et al., 2011) have all been shown to positively affect student achievement. These instructional inputs can also interact. For example, the effect of instruction time on student achievement depends on student-teacher interactions (Rivkin and Schiman, 2015).

8 CONCLUSION

Since 2010, the majority of US states have aligned their math and ELA education standards by adopting the CCSS. This paper estimates the effect of CCSS adoption on student achievement in non-targeted subjects. We find that the CCSS decreased student achievement in non-targeted subjects, particularly for underprivileged students. This is not only harmful to long-term individual and economic development (Hanushek and Woessmann, 2008, 2012), but also implies that the CCSS increased racial/ethnic and socioeconomic student achievement gaps in non-targeted subjects, with potentially long-lasting consequences. For example, racial/ethnic student achievement gaps account for relevant portions of adulthood racial/ethnic gaps with respect to income, unemployment, incarceration, health, and other important social and economic outcomes (Fryer, 2011).

With respect to mechanisms, we find that the negative spillover of the CCSS on student achievement in non-targeted subjects was accompanied by a reduction of instructional focus on these subjects. This result mirrors previous findings on the effects of NCLB, which also only focused on math and ELA and caused a reduction in instruction time in non-targeted science (Reback et al., 2014). In sum, our results allow to evaluate the CCSS more comprehensively, with, at best, modest positive effects on student achievement in targeted subjects (also previously documented in Bleiberg (2021)) at the expense of student achievement in non-targeted subjects.

In terms of education policy, our results suggest that the CCSS might have been more beneficial if it had been adopted for all school subjects. Such a policy might have prevented the negative spillover of the CCSS on non-targeted subjects, arguably by avoiding the perception that these subjects are less relevant and receive less instructional attention. At the same time, such a policy might also have reduced any positive effects on student achievement in the targeted subjects. Adopting a centralized education standard which covers all subjects requires that the participating states agree on the educational content for each subject. To achieve this goal, political challenges need to be overcome as exemplified by the controversies around the history curriculum (Cohen, 2020) or around the treatment of evolution theory in US State Science Education Standards (Lerner, 2000; Arold, 2021).

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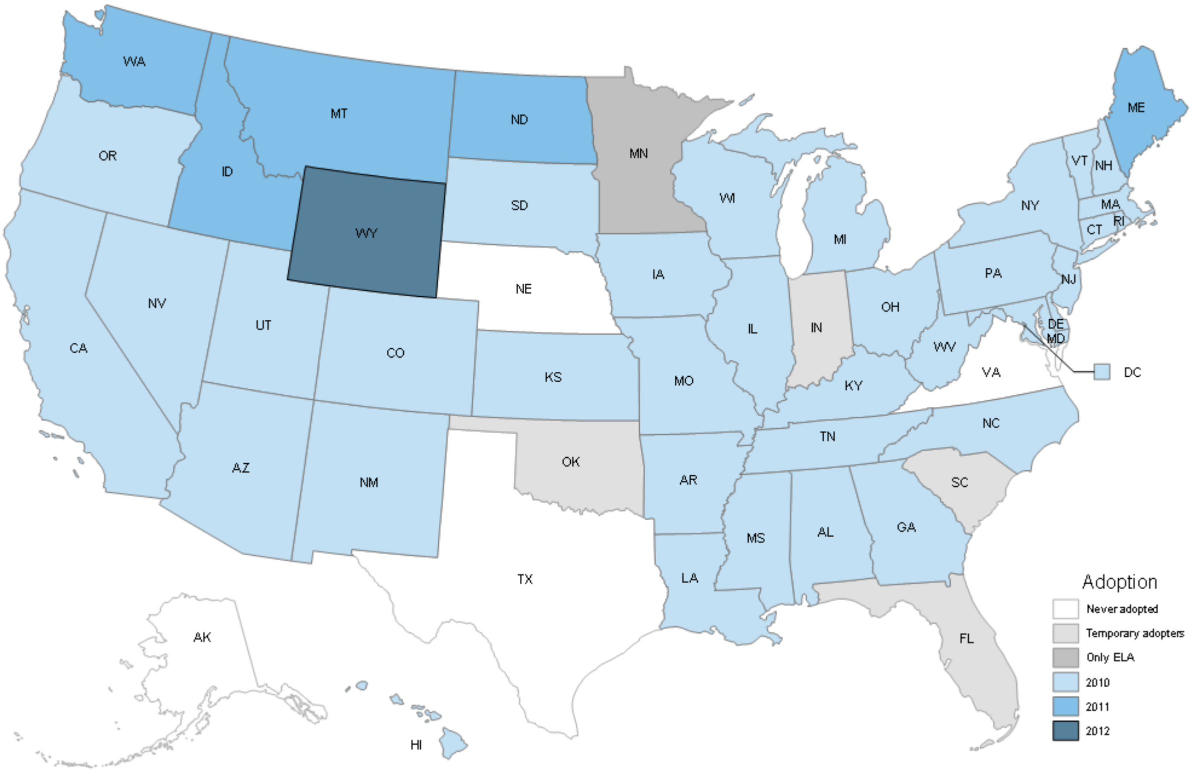
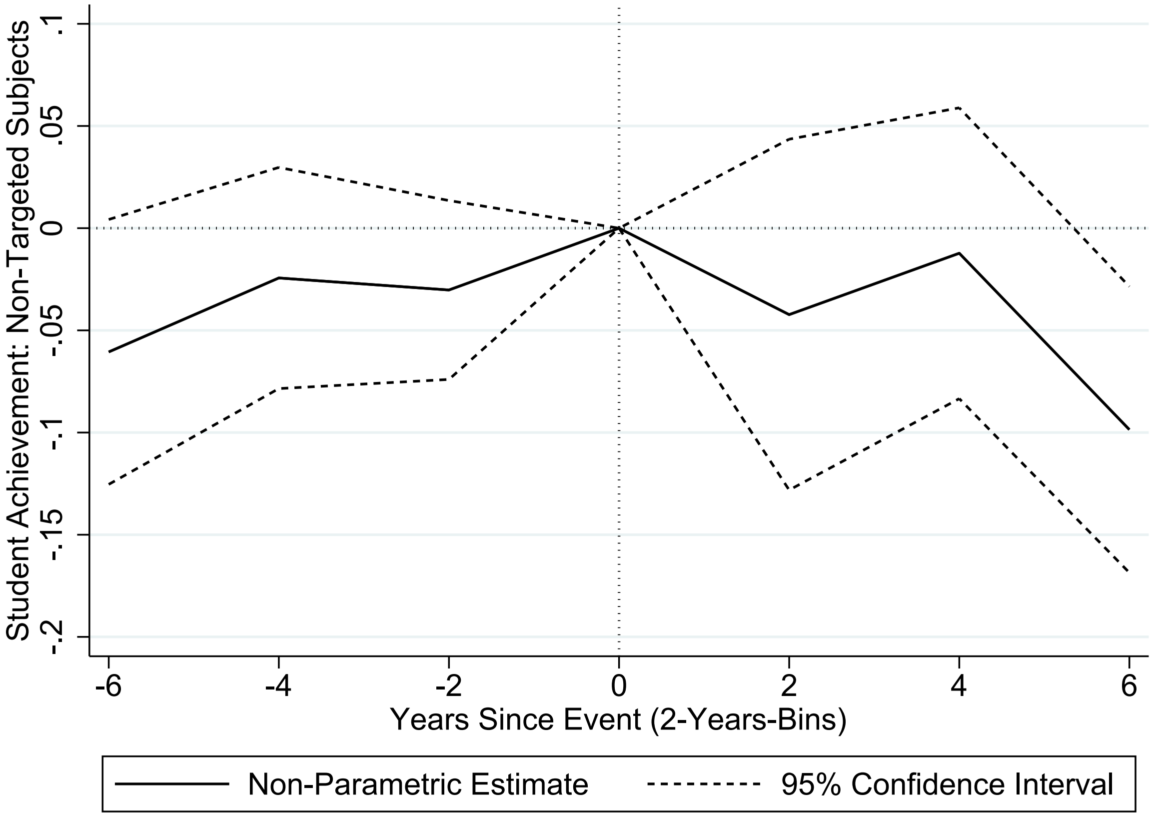
MAIN FIGURES AND TABLES

Figure 1 – CCSS Adoption Map



Note: Map depicts state-level adoption of CCSS. Data sources: Achieve Inc. (2013), Bleiberg (2021), and CCSSI (2021)

Figure 2 – Event-study graph: Non-targeted subjects



Note: Coefficients from non-parametric event-study regressions and their 95% confidence intervals. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as state, test year, grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Numbers on horizontal axis refer to respective two-year bins; i.e. 2 = first two years of treatment (year 0 = excluded category). The p values of omnibus hypothesis tests of zero pre- and post-event effects are 0.312 and 0.001, respectively. Data source: U.S. Department of Education, National Center for Education Statistics, National Assessment of Educational Progress

Table 1 – Effect of CCSS exposure on student achievement in non-targeted subjects

	(1)	(2)	(3)
CCSS Exposure	0.105***	0.117***	-0.079**
	(0.036)	(0.043)	(0.036)
State and Year FEs	NO	NO	YES
Controls	NO	YES	YES
Adj. R-squared	0.001	0.379	0.390
Observations	1,103,630	1,103,630	1,103,630

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table 2 – Effect of CCSS exposure on student achievement in non-targeted subjects, subgroups by student characteristics

	Gender		Race/Ethnicity					Subsidized Lunch Status		English Languange Lerner Status		Disability Status	
	(1) Female	(2) Male	(3) White	(4) Black	(5) Hispanic	(6) Asian	(7) Other	(8) Yes	(9) No	(10) Yes	(11) No	(12) Yes	(13) No
CCSS Exposure	-0.079**	-0.080**	-0.016	-0.111***	-0.181***	-0.012	0.016	-0.096***	-0.041	-0.201***	-0.065*	-0.176***	-0.068*
	(0.037)	(0.036)	(0.039)	(0.037)	(0.041)	(0.061)	(0.071)	(0.031)	(0.044)	(0.073)	(0.035)	(0.049)	(0.037)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.395	0.390	0.272	0.241	0.331	0.392	0.362	0.303	0.301	0.182	0.348	0.308	0.353
Observations	544,410	559,210	631,640	184,680	194,250	55,130	37,930	513,910	589,710	69,190	1,034,440	128,590	975,030

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table 3 – Effect of CCSS exposure on student achievement in non-targeted subjects, econometric robustness

	(1) DD	(2) (1) + linear state trends	(3) (2) + quadratic state trends	(4) (3) + cubic state trends	(5) DDD	(6) DD with private schools
CCSS Exposure	-0.079**	-0.117**	-0.100**	-0.095**	-0.090**	-0.074**
	(0.036)	(0.044)	(0.047)	(0.045)	(0.044)	(0.033)
State and Year FEs	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.390	0.391	0.392	0.393	0.394	0.390
Observations	1,103,630	1,103,630	1,103,630	1,103,630	1,135,960	1,135,960

Note: Each entry is from a separate two-way fixed effects regression model, where Model (1) is the baseline model, Models (2), (3), (4) subsequently add linear, quadratic, and cubic state-specific time trends, and Model (5) presents a triple-difference model where school type (public vs. private school students), school type*state, school type*year, and state*year fixed effects replace all state-specific time trends. Model (6) estimates the basic two-way fixed effects model on a sample of public and private school students. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table 4 – Effect of CCSS exposure on student achievement in non-targeted subjects, robustness using different definitions of treatment implementation

	CCSS adoption		CCSS implementation requirement		CCSS implementation strategies		Effective CCSS implementation		Include temporary CCSS adopters and implementers		CCSS-aligned testing	
	(1) All grades	(2) Only grade 4	(3) All grades	(4) Only grade 4	(5) All grades	(6) Only grade 4	(7) All grades	(8) Only grade 4	(9) All grades	(10) Only grade 4	(11) All grades	(12) Only grade 4
CCSS Exposure	-0.079**	-0.134***	-0.088	-0.177**	-0.042	-0.098*	-0.075**	-0.107***	-0.035	-0.111**	-0.085	-0.118**
	(0.036)	(0.036)	(0.079)	(0.076)	(0.041)	(0.050)	(0.035)	(0.039)	(0.045)	(0.055)	(0.057)	(0.054)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.390	0.376	0.390	0.376	0.390	0.376	0.390	0.376	0.390	0.376	0.390	0.376
Observations	1,103,630	434,440	1,103,630	434,440	1,103,630	434,440	1,103,630	434,440	1,103,630	434,440	1,103,630	434,440

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variables: Share of schooling years a student was exposed to CCSS (at the time of testing), where in Models 1 and 2 (CCSS adoption, baseline model) each schooling year counts as exposed in a given state in which the state adopted the CCSS permanently before that year or in the same year according to Achieve Inc. (2013) and CCSSI (2021), where Models 3 and 4 (CCSS implementation requirement) each schooling year counts as exposed in a given state in which the state expects teachers to fully incorporate CCSS into classroom instruction in grades K-12 in English language arts and mathematics according to Achieve Inc. (2013) and CCSSI (2021), where in Models 5 and 6 (CCSS implementation strategies) each schooling year counts as exposed in a given state if state education agency officials report that their state pursued at least two out of three CCSS implementation strategies (professional development, new instructional materials, joined testing consortium) as reported in Webber et al. (2014), where in Models 7 and 8 (Effective CCSS implementation) each schooling year counts as exposed in a given state in which the state implemented an effective change in state standard content through the adoption of CCSS which we define as not having had a state standard in place before the adoption of CCSS whose academic rigor is "too close to call" in comparison with CCSS (Carmichael et al., 2010) for the set of states adopting CCSS according to Achieve Inc. (2013) and CCSSI (2021), where in Models 9 and 10 (Include temporary CCSS adopters) each schooling year counts as exposed in a given state in which a state adopted and/or implemented CCSS at least temporarily according to Bleiberg (2021); and where in Models 11 and 12 (CCSS-aligned testing) each schooling year counts as exposed in a given state in which the state adopted CCSS-aligned standardized testing including field and transitional tests according to our own research (see Table C.17 for state-specific details of CCSS-aligned testing). Table C.18 provides state-specific coding information on all treatment definitions. Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table 5 – Effect of CCSS exposure on instructional focus in non-targeted subjects

	Instruction Time	Instructional Resources	Differentiated Instruction					Teacher-Student Interactions			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
			Standards	Material	Activities	Methods	Pace	Discuss Students' Performance	Set goals	Discuss goals	Adjust teaching
CCSS Exposure	-0.171***	-0.116***	-0.027*	0.021	-0.022	-0.015	0.006	-0.017	-0.035*	-0.047**	-0.020
	(0.055)	(0.030)	(0.015)	(0.016)	(0.013)	(0.022)	(0.016)	(0.035)	(0.019)	(0.023)	(0.023)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.090	0.043	0.053	0.073	0.064	0.167	0.122	0.109	0.091	0.089	0.058
Observations	847,830	785,660	555,490	556,350	556,070	556,440	556,110	554,820	554,490	554,360	554,250

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variables, by columns: Probability that students' teachers report about subject in question (Model 1) that students receive more than five hours of weekly instruction, (Model 2) that their school system provides them with all or most materials and other resources they need for the instruction, (Model 3) that they set differentiated standards for some students at least to a moderate extent, (Model 4) that they use differentiated materials for some students at least to a moderate extent, (Model 5) engage some students in differentiated activities at least to a moderate extent, (Model 6) that they use differentiated methods for some students at least to a moderate extent, (Model 7) change pace for some students at least to a moderate extent, (Model 8) that they discuss the student's current level of performance at least once a month, (Model 9) that they set goals for specific progress the student would like to make at least once a month, (Model 10) that they discuss progress the student has made toward goals previously set at least once a month, (Model 11) that they determine how to adjust their teaching strategies to meet the student's current learning needs and to reflect the student's future goals at least once a month. Explanatory variable: Share of schooling years teacher's students were exposed to CCSS (at the time of testing). Controls: Indicator variables for teacher race/ethnicity, teacher education, teacher certification (separate indicator variables for certification of National Board for Professional Teaching Standards, and for alternative certification), and teacher experience, as well as student controls for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

A SUPPLEMENTARY FIGURES AND TABLES

Table A.1 – List of grades of NAEP tests for non-targeted subjects

	Non-targeted Subjects				
Year	Science	Civics	Economics	Geography	History
2005	4, 8				
2006		4, 8, 12	12		4, 8, 12
2007					
2008					
2009	4, 8, 12				
2010		4, 8, 12		4, 8, 12	4, 8, 12
2011	8				
2012			12		
2013					
2014		8		8	8
2015	4, 8, 12				

Note: NAEP student achievement data in non-targeted subjects at the subject-by-year-by-grade level. Data source: See Figure 2

Table A.2 – Descriptive statistics

	Mean	Std. Dev.	Min.	Max.
Student Achievement Outcomes:				
Science	0.08	1.03	-4.76	4.51
Civics	0.03	0.98	-4.49	3.16
Economics	0.02	0.97	-4.33	3.70
Geography	0.00	1.00	-5.15	3.90
History	0.04	0.98	-4.64	3.32
Student Controls:				
Female	0.49	0.50	0.00	1.00
Race/Ethnicity: White	0.57	0.49	0.00	1.00
Race/Ethnicity: Black	0.15	0.36	0.00	1.00
Race/Ethnicity: Hispanic	0.20	0.40	0.00	1.00
Race/Ethnicity: Asian	0.05	0.22	0.00	1.00
Race/Ethnicity: Other	0.03	0.16	0.00	1.00
English Language Learner	0.06	0.24	0.00	1.00
Disabled	0.11	0.31	0.00	1.00
Subsidized Lunch	0.43	0.50	0.00	1.00
Parental Education: Did not finish High School	0.08	0.28	0.00	1.00
Parental Education: Graduated High School	0.18	0.39	0.00	1.00
Parental Education: Some education after High School	0.20	0.40	0.00	1.00
Parental Education: Graduated College	0.53	0.50	0.00	1.00
Computer at Home	0.90	0.30	0.00	1.00
Books at Home: 0-10	0.13	0.34	0.00	1.00
Books at Home: 11-25	0.22	0.41	0.00	1.00
Books at Home: 26-100	0.35	0.48	0.00	1.00
Books at Home: >100	0.30	0.46	0.00	1.00

Note: Continuation on next page

Descriptive statistics (continued)

	Mean	Std. Dev.	Min.	Max.
Instructional Focus Outcomes:				
Instruction Time	0.41	0.49	0.00	1.00
Instructional Resources	0.60	0.49	0.00	1.00
Differentiated Instruction: Standards	0.44	0.50	0.00	1.00
Differentiated Instruction: Material	0.65	0.48	0.00	1.00
Differentiated Instruction: Activities	0.41	0.49	0.00	1.00
Differentiated Instruction: Methods	0.62	0.49	0.00	1.00
Differentiated Instruction: Pace	0.59	0.49	0.00	1.00
Teacher dedication: Discuss students' performance	0.56	0.50	0.00	1.00
Teacher dedication: Set goals	0.40	0.49	0.00	1.00
Teacher dedication: Discuss goals	0.41	0.49	0.00	1.00
Teacher dedication: Adjust teaching	0.64	0.48	0.00	1.00
Teacher Controls:				
Teacher Race/Ethnicity: White	0.83	0.38	0.00	1.00
Teacher Race/Ethnicity: Black	0.07	0.25	0.00	1.00
Teacher Race/Ethnicity: Hispanic	0.06	0.24	0.00	1.00
Teacher Race/Ethnicity: Asian	0.02	0.15	0.00	1.00
Teacher Race/Ethnicity: Other	0.01	0.11	0.00	1.00
Teacher Education: Bachelor or less	0.50	0.50	0.00	1.00
Teacher Education: Master or more	0.50	0.50	0.00	1.00
NBPTS Teacher Certificate: Yes	0.13	0.34	0.00	1.00
NBPTS Teacher Certificate: Working towards	0.02	0.15	0.00	1.00
NBPTS Teacher Certificate: No	0.85	0.36	0.00	1.00
Alternative Teacher Certificate: Yes	0.13	0.33	0.00	1.00
Alternative Teacher Certificate: No	0.87	0.33	0.00	1.00
Teacher Experience: 2 years or less	0.09	0.28	0.00	1.00
Teacher Experience: 3-5 years	0.14	0.34	0.00	1.00
Teacher Experience: 6-10 years	0.22	0.42	0.00	1.00
Teacher Experience: 11-20 years	0.27	0.44	0.00	1.00
Teacher Experience: 21 years or more	0.28	0.45	0.00	1.00

Note: Descriptive statistics (mean, standard deviation, minimum, maximum) for main treatment, outcome, and control variables. Data source: See Figure 2

Table A.3 – Effect of CCSS exposure on student achievement in non-targeted subjects, subgroups by subjects

	(1) Science	(2) Civics	(3) Economics	(4) Geography	(5) History
CCSS Exposure	-0.096**	0.010	-0.052	-0.036	-0.004
	(0.042)	(0.087)	(0.307)	(0.079)	(0.097)
State and Year FEs	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Adj. R-squared	0.402	0.391	0.381	0.429	0.380
Observations	931,600	55,150	19,930	32,130	64,810

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subject indicated in the column header. Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table A.4 – Effect of CCSS exposure on student achievement in non-targeted subjects, subgroups by grades

	(1) Grade 4	(2) Grade 8	(3) Grade 12
CCSS Exposure	-0.134***	-0.007	-0.005
	(0.036)	(0.062)	(0.072)
State and Year FEs	YES	YES	YES
Controls	YES	YES	YES
Adj. R-squared	0.376	0.433	0.370
Observations	434,440	582,590	86,600

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table A.5 – Effect of CCSS exposure on student achievement in non-targeted subjects, subgroups by quartiles of states' student achievement before 2010

	(1) Quartile 1 (lowest)	(2) Quartile 2	(3) Quartile 3	(4) Quartile 4 (highest)
CCSS Exposure	0.004	-0.097*	-0.061	-0.041
	(0.046)	(0.048)	(0.051)	(0.029)
State and Year FEs	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Adj. R-squared	0.413	0.365	0.353	0.380
Observations	315,160	334,740	233,430	220,300

Note: Each entry is from a separate two-way fixed effects regression model. Sample of Quartile 1 subgroup includes students from states in the lowest quartile with respect to average student achievement in years before 2010. Sample of Quartile 2 subgroup includes students from states in the second lowest quartile with respect to average student achievement in years before 2010. Quartile 3 and 4 defined accordingly. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table A.6 – Effect of CCSS exposure on student achievement in non-targeted subjects, robustness with additional controls for public schooling policies

	Control for:						
	(1) Expenditures	(2) NGSS Adoption	(3) Teacher Policies	(4) School Choice	(5) Evolution	(6) Charter Schools	(7) NCLB/ESSA Waivers
CCSS Exposure	-0.077**	-0.066*	-0.098**	-0.075**	-0.079**	-0.080**	-0.074**
	(0.035)	(0.034)	(0.037)	(0.037)	(0.036)	(0.036)	(0.035)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.391	0.390	0.397	0.390	0.390	0.390	0.390
Observations	1,103,620	1,103,630	769,410	1,099,880	1,099,880	1,099,880	1,103,630

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Additional policy controls (state-by-year level, unless otherwise stated): Model 1 controls for district-by-year-level per-pupil education expenditures in logalithmized dollars; Model 2 controls for adoption of Next Generation Science Standards or of standards based on Next Generation Science Standards framework; Model 3 controls for index of teacher quality policies; Model 4 controls for public school choice laws; Model 5 controls for laws permitting public school teachers to teach 'weaknesses of evolution'; Model 6 controls for charter school laws; Model 7 controls for NCLB/ESSA requirements waiver. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: National Center for Education Statistics (Local Education Agency (School District) Finance Survey F-33); Ross et al. (2017); Sorens et al. (2008); Jordan and Grossmann (2020); See Figure 2

Table A.7 – Effect of CCSS exposure on student achievement in non-targeted subjects, robustness with additional controls for private schooling policies

	Control for:					
	(1) State Approval	(2) Licensure of Teachers	(3) Registration	(4) Curriculum	(5) Tax Credits	(6) Vouchers
CCSS Exposure	-0.079**	-0.080**	-0.075**	-0.080**	-0.088***	-0.064*
	(0.036)	(0.036)	(0.036)	(0.036)	(0.028)	(0.035)
State and Year FEs	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.390	0.390	0.390	0.390	0.390	0.390
Observations	1,099,880	1,099,880	1,099,880	1,099,880	1,099,880	1,099,880

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Additional policy controls (state-by-year level, unless otherwise stated): Model 1 controls for mandatory state approval, where state has discretion, licensing, or accreditation of private schools; Model 2 controls for mandatory state licensure of private school teachers; Model 3 controls for mandatory registration or licensing of private schools (note: if approval is required, registration is also coded as being required); Model 4 controls for extent of private school curriculum control; Model 5 controls for tax credit/deduction law for scholarship contributions or educational expenses of parents; Model 6 controls for publicly funded voucher laws. Standard errors clustered at the state level. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: Sorens et al. (2008); Jordan and Grossmann (2020); See Figure 2

Table A.8 – Effect of CCSS exposure on student achievement in non-targeted subjects, robustness with additional controls for homeschooling policies

	Control for:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Curriculum	Statute	Notice Extent	Notice Frequency	Notice Index	Recordkeeping	Testing	Teachers
CCSS Exposure	-0.082**	-0.080**	-0.079**	-0.086**	-0.089**	-0.080**	-0.079**	-0.080**
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.035)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.390	0.390	0.390	0.390	0.391	0.390	0.390	0.390
Observations	1,099,880	1,099,880	1,099,880	1,099,880	1,099,880	1,099,880	1,099,880	1,099,880

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Additional policy controls (state-by-year level, unless otherwise stated): Model 1 controls for subjects/curriculum requirement for homeschoolers; Model 2 controls for whether homeschooling is explicitly permitted by statute; Model 3 controls for extent of homeschooling notice requirement; Model 4 controls for frequency of homeschooling notice requirement; Model 5 controls for homeschooling notification index (Extent of homeschooling notice requirement * Frequency of homeschooling notice requirement); Model 6 controls for extent of homeschool recordkeeping requirements; Model 7 controls for standardized testing or other official evaluation requirement of homeschooling; Model 8 controls for homeschooling teacher qualifications requirement. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: Sorens et al. (2008); Jordan and Grossmann (2020); See Figure 2

Table A.9 – Effect of CCSS exposure on student achievement in non-targeted subjects, robustness with additional controls for compulsory schooling policies

	Control for:			
	(1)	(2)	(3)	(4)
	Compulsory school age, lower bound	Compulsory school age, upper bound	Compulsory school years	Kindergarten attendance
CCSS Exposure	-0.079**	-0.079**	-0.081**	-0.082**
	(0.037)	(0.035)	(0.036)	(0.036)
State and Year FEs	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Adj. R-squared	0.390	0.390	0.390	0.390
Observations	1,099,880	1,099,880	1,099,880	1,099,880

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Additional policy controls (state-by-year level, unless otherwise stated): Model 1 controls for compulsory school age, lower bound (minimum standard if set by local school district; age at which parental waivers not permitted); Model 2 controls for compulsory school age, upper bound (minimum standard if set by local school district; age at which parental waivers not permitted); Model 3 controls for compulsory school years (Compulsory school age, upper bound – Compulsory school age, lower bound); Model 4 controls for kindergarten attendance requirement. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: Sorens et al. (2008); Jordan and Grossmann (2020); See Figure 2

Table A.10 – Effect of CCSS exposure on student achievement in non-targeted subjects, further robustness checks

	(1) Exclude already-treated states from controls	(2) Controls: No parental education	(3) Controls: No parental education & No imputation of missings	(4) Treatment: Number of years of CCSS Exposure	(5) Sample: No Charter Schools	(6) No weights
CCSS Exposure	-0.082**	-0.079**	-0.090**	-0.006	-0.079**	-0.093***
	(0.036)	(0.036)	(0.036)	(0.007)	(0.036)	(0.025)
State and Year FEs	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.392	0.380	0.381	0.390	0.390	0.399
Observations	996,390	1,103,630	1,067,950	1,103,630	1,077,420	1,103,630

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects not targeted by the CCSS (Pool of science, civics, economics, geography, and history). Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Model 1 excludes states which adopted the CCSS in 2011 and 2012 from the sample which implies that no students from already-treated states act as controls; Model 2 excludes parental education from set of control variables; Model 3 excludes parental education from set of control variables and does not impute other missing control variables; Model 4 defines the explanatory variable as the number of schooling years a student was exposed to CCSS (at the time of testing); Model 5 excludes charter schools from the sample of public schools; Model 6 does not use population weights. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table A.11 – Effect of CCSS exposure on instruction time in non-targeted subjects, subgroups by teacher characteristics

	Teacher Race/Ethnicity					Teacher education		Teacher certification (NBPTS)			Teacher experience in years				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	White	Black	Hispanic	Asian	Other	Bachelor or less	Master or more	Yes	Working towards	No	2 or less	3-5	6-10	11-20	21 or more
CCSS Exposure	-0.201***	-0.050	0.033	-0.081	0.049	-0.145**	-0.226***	-0.147**	0.120	-0.202***	-0.170*	-0.074	-0.207***	-0.004	-0.351***
	(0.048)	(0.083)	(0.052)	(0.169)	(0.182)	(0.055)	(0.064)	(0.067)	(0.124)	(0.061)	(0.088)	(0.057)	(0.067)	(0.075)	(0.088)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.093	0.164	0.134	0.288	0.365	0.089	0.120	0.153	0.310	0.106	0.146	0.138	0.127	0.123	0.132
Observations	696,350	63,680	39,040	24,130	15,010	404,030	443,810	77,450	16,460	502,330	88,620	128,950	190,000	247,230	192,820

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Probability that students' teacher reports about subject in question that students receive more than five hours of weekly instruction. Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for teacher race/ethnicity, teacher education, teacher certification (separate indicator variables for certification of National Board for Professional Teaching Standards, and for alternative certification), and teacher experience, as well as student controls for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table A.12 – Effect of CCSS exposure on instructional resources in non-targeted subjects, subgroups by teacher characteristics

	Teacher Race/Ethnicity					Teacher education		Teacher certification (NBPTS)			Teacher experience in years				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	White	Black	Hispanic	Asian	Other	Bachelor or less	Master or more	Yes	Working towards	No	2 or less	3-5	6-10	11-20	21 or more
CCSS Exposure	-0.121***	-0.038	-0.220***	-0.071	0.177***	-0.121***	-0.103***	-0.132***	-0.163*	-0.122***	-0.127**	-0.054**	-0.148***	-0.110**	-0.115***
	(0.028)	(0.047)	(0.028)	(0.091)	(0.061)	(0.030)	(0.031)	(0.033)	(0.096)	(0.039)	(0.056)	(0.026)	(0.029)	(0.045)	(0.028)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.044	0.051	0.046	0.078	0.095	0.038	0.050	0.056	0.080	0.039	0.049	0.038	0.047	0.049	0.058
Observations	647,100	58,340	34,910	23,110	13,460	373,640	412,020	71,420	15,520	464,110	82,790	120,210	174,310	229,130	179,120

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Probability that students' teacher reports about subject in question that their school system provides them with all or most materials and other resources they need for the instruction. Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for teacher race/ethnicity, teacher education, teacher certification (separate indicator variables for certification of National Board for Professional Teaching Standards, and for alternative certification), and teacher experience, as well as student controls for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table A.13 – Effect of CCSS exposure on the quality of teacher-student interactions (setting goals) in non-targeted subjects, subgroups by teacher characteristics

	Teacher Race/Ethnicity					Teacher education		Teacher certification (NBPTS)			Teacher experience in years				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	White	Black	Hispanic	Asian	Other	Bachelor or less	Master or more	Yes	Working towards	No	2 or less	3-5	6-10	11-20	21 or more
CCSS Exposure	-0.029*	-0.064	-0.026	0.009	-0.195	-0.030	-0.046**	0.010	-0.079	-0.047**	-0.054	-0.012	-0.050	-0.022	-0.024
	(0.016)	(0.048)	(0.038)	(0.170)	(0.210)	(0.022)	(0.023)	(0.037)	(0.085)	(0.020)	(0.052)	(0.028)	(0.044)	(0.017)	(0.038)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.059	0.071	0.149	0.106	0.101	0.099	0.086	0.117	0.140	0.078	0.102	0.101	0.094	0.083	0.095
Observations	458,530	39,930	24,280	16,870	10,040	250,090	304,390	71,120	15,490	462,530	57,820	81,750	123,090	170,430	121,310

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Probability that students' teacher reports about subject in question that she sets goals for specific progress the student would like to make at least once a month. Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for teacher race/ethnicity, teacher education, teacher certification (separate indicator variables for certification of National Board for Professional Teaching Standards, and for alternative certification), and teacher experience, as well as student controls for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table A.14 – Effect of CCSS exposure on the quality of teacher-student interactions (discussing goals) in non-targeted subjects, subgroups by teacher characteristics

	Teacher Race/Ethnicity					Teacher education		Teacher certification (NBPTS)			Teacher experience in years				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	White	Black	Hispanic	Asian	Other	Bachelor or less	Master or more	Yes	Working towards	No	2 or less	3-5	6-10	11-20	21 or more
CCSS Exposure	-0.033	-0.086	-0.054	-0.042	-0.251	-0.053	-0.042**	0.007	-0.123	-0.059**	-0.070	-0.052	-0.029	-0.046**	-0.038
	(0.020)	(0.052)	(0.036)	(0.167)	(0.200)	(0.033)	(0.017)	(0.034)	(0.079)	(0.027)	(0.069)	(0.043)	(0.030)	(0.022)	(0.040)
State and Year FEs	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.060	0.076	0.127	0.116	0.116	0.094	0.086	0.113	0.140	0.077	0.094	0.104	0.094	0.080	0.096
Observations	458,460	39,940	24,270	16,870	10,020	250,020	304,350	71,120	15,480	462,400	57,780	81,770	123,080	170,360	121,290

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Probability that students' teacher reports about subject in question that she discusses progress her student has made toward goals previously set at least once a month. Explanatory variable: Share of schooling years a student was exposed to CCSS (at the time of testing). Controls: Indicator variables for teacher race/ethnicity, teacher education, teacher certification (separate indicator variables for certification of National Board for Professional Teaching Standards, and for alternative certification), and teacher experience, as well as student controls for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

B ANALYSIS OF THE EFFECTS OF THE CCSS ON TARGETED SUBJECTS

We show evidence that the CCSS had, at best, modestly positive effects on student achievement in the targeted subjects math and ELA. This analysis largely confirms the conclusions Bleiberg (2021) has drawn on this question, although our findings (using more data, among other conceptual differences) suggest that Bleiberg (2021) rather overestimates than underestimates the positive effects of the CCSS on student achievement in targeted subjects.

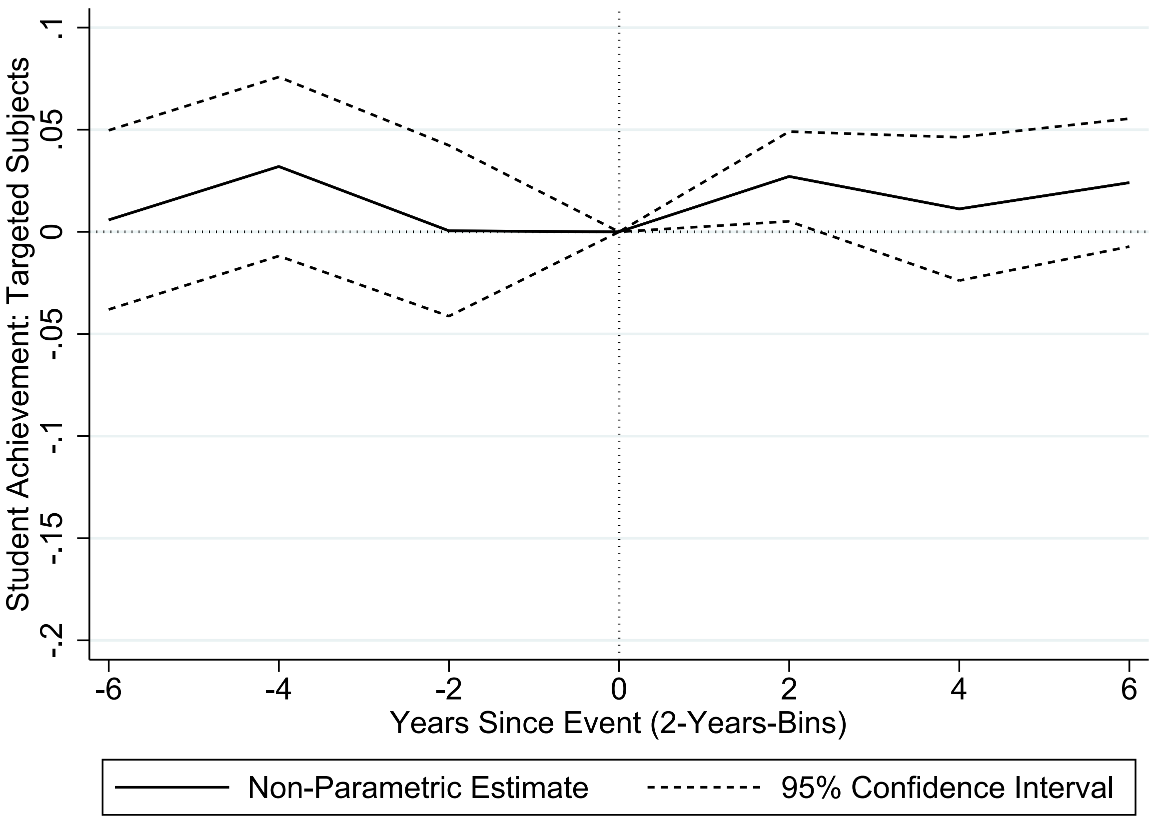
First, we visualize the modest positive effects in an event-study graph depicted in Figure B.1. The estimation equation follows Equation 2 presented in Section 3, with T_{istuv} now pooling standardized student achievement in all subjects that are targeted by the CCSS, across all available grades.[18] The event-study graph shows a modest increase in student achievement after the adoption of the CCSS that is marginally significant directly after adoption and insignificantly different from zero thereafter. Using a second dataset on student achievement from the Stanford Education Data Archive SEDA 4.0 (Reardon et al., 2021) with a shorter pre- but longer post-period relative to the NAEP, we find basically null effects, see Figure B.2. SEDA does not contain data on non-targeted subjects which is why we cannot use it for the main analysis.

Second, we also show parametric two-way fixed effects DD results for our preferred treatment indicator and the set of further treatment indicators described in Section 6 and Appendix C. The estimation equation follows Equation 1 presented in Section 3, with T_{istuv} pooling standardized student achievement in all subjects that are targeted by the CCSS, across all available grades (as above). As shown in Table B.15, we find zero to modestly positive effects across specifications.[19]

[18] Table B.16 provides the list of grades in which the NAEP tests were administered for each of the four targeted subjects math, reading, writing, and vocabulary (and for which state identifiers of students are available). We use the student-level data from all these subject-year-grade combinations in our analysis.

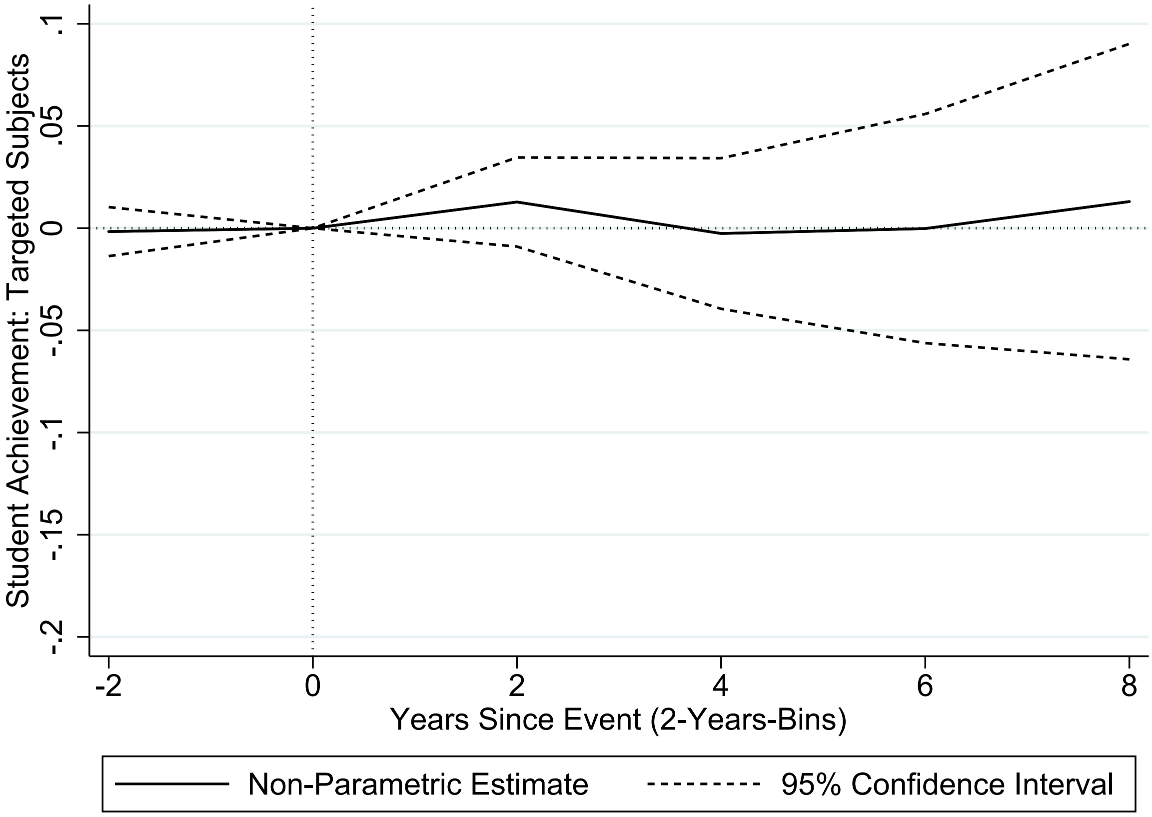
[19] Further estimations including specifications with state-specific trends and triple-difference models yield similar results (available on request).

Figure B.1 – Event-study graph: Targeted subjects (NAEP)



Note: Coefficients from non-parametric event-study regressions and their 95% confidence intervals. Dependent variable: Standardized student achievement in subjects targeted by the CCSS (Pool of math, reading, vocabulary and writing). Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as state, test year, grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Numbers on horizontal axis refer to respective two-year bins; i.e. 2 = first two years of treatment (year 0 = excluded category). The p values of omnibus hypothesis tests of zero pre- and post-event effects are 0.003 and 0.075, respectively. Data source: See Figure 2

Figure B.2 – Event-study graph: Targeted subjects (SEDA)



Note: Coefficients from non-parametric event-study regressions and their 95% confidence intervals. Dependent variable: Standardized student achievement in subjects targeted by the CCSS (Pool of math and ELA). Controls: District shares of races/ethnicities, English language learner status, disability status, subsidized lunch status, economic disadvantage, rural location, as well as state, test year, grade and subject fixed effects. Regressions use precision weights (the inverse of the standard error of average student achievement in math and ELA squared) and standard errors clustered at the state level. Numbers on horizontal axis refer to respective two-year bins; i.e. 2 = first two years of treatment (year 0 = excluded category). The p values of omnibus hypothesis tests of zero pre- and post-event effects are 0.782 and 0.190, respectively. Data source: Reardon et al. (2021)

Table B.15 – Effect of CCSS exposure on student achievement in targeted subjects using different definitions of treatment implementation

	(1) CCSS adoption	(2) CCSS implementation requirement	(3) CCSS implementation strategies	(4) Effective CCSS implementation	(5) Include temporary CCSS adopters and implementers	(6) CCSS-aligned testing
CCSS Exposure	0.010	-0.002	0.019	-0.001	0.046***	-0.007
	(0.023)	(0.027)	(0.019)	(0.021)	(0.017)	(0.032)
State and Year FEs	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Adj. R-squared	0.352	0.352	0.352	0.352	0.352	0.352
Observations	6,392,940	6,392,940	6,392,940	6,392,940	6,392,940	6,392,940

Note: Each entry is from a separate two-way fixed effects regression model. Dependent variable: Standardized student achievement in subjects targeted by the CCSS (Pool of math, reading, vocabulary and writing). Explanatory variables: Share of schooling years a student was exposed to CCSS (at the time of testing), where in Models 1 (CCSS adoption, baseline model) each schooling year counts as exposed in a given state in which the state adopted the CCSS permanently before that year or in the same year according to Achieve Inc. (2013) and CCSSI (2021), where Model 2 (CCSS implementation requirement) each schooling year counts as exposed in a given state in which the state expects teachers to fully incorporate CCSS into classroom instruction in grades K-12 in English language arts and mathematics according to Achieve Inc. (2013) and CCSSI (2021), where in Models 3 (CCSS implementation strategies) each schooling year counts as exposed in a given state if state education agency officials report that their state pursued at least two out of three CCSS implementation strategies (professional development, new instructional materials, joined testing consortium) as reported in Webber et al. (2014), where in Models 4 (Effective CCSS implementation) each schooling year counts as exposed in a given state in which the state implemented an effective change in state standard content through the adoption of CCSS which we define as not having had a state standard in place before the adoption of CCSS whose academic rigor is "too close to call" in comparison with CCSS (Carmichael et al., 2010) for the set of states adopting CCSS according to Achieve Inc. (2013) and CCSSI (2021), where in Models 5 (Include temporary CCSS adopters) each schooling year counts as exposed in a given state in which a state adopted and/or implemented CCSS at least temporarily according to Bleiberg (2021); and where in Models 6 (CCSS-aligned testing) each schooling year counts as exposed in a given state in which the state adopted CCSS-aligned standardized testing including field and transitional tests according to our own research (see Table C.17 for state-specific details of CCSS-aligned testing). Table C.18 provides state-specific coding information on all treatment definitions. Controls: Indicator variables for gender, races/ethnicities, English language learner status, disability status, subsidized lunch status, parental education, home possessions (separate indicator variables for computer and books) as well as grade and subject fixed effects. Regressions use population weights and standard errors clustered at the state level. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Data source: See Figure 2

Table B.16 – List of grades of NAEP tests for targeted subjects

	Targeted Subjects			
Year	Math	Reading	Vocabulary	Writing
2002		4, 8, 12		8, 12
2003	4, 8	4, 8		
2004				
2005	4, 8, 12	4, 8, 12		4, 8, 12
2006				
2007	4, 8	4, 8		8, 12
2008				
2009	4, 8, 12	4, 8, 12	4, 8, 12	
2010				
2011	4, 8	4, 8	4, 8	8, 12
2012				
2013	4, 8	4, 8		
2014				
2015	4, 8, 12	4, 8, 12		
2016				
2017	4, 8,	4, 8		

Note: NAEP student achievement data in targeted subjects at the subject-by-year-by-grade level. Data source: See Figure 2

C BACKGROUND INFORMATION ON TREATMENT DEFINITION ROBUSTNESS

To test whether our results hold if we define treatment based on CCSS implementation, we re-run our main regression using five different treatment variables each capturing different information about CCSS implementation in Section 6. This appendix provides background information about the construction and data sources of these five alternative treatment definitions.

First, we collect information on CCSS implementation requirements, from Achieve Inc. (2013) and CCSSI (2021). Here, the year of full implementation of CCSS is defined as the school year the respective state expects teachers in grades K-12 in math and ELA to incorporate the standards into classroom instruction. The time between adoption and full implementation varies between 1 to 4 years across adopting states, with an average of about 3 years.

Second, we note that state expectations about teachers implementing the CCSS into classroom instruction do not necessarily have to be aligned with actual state efforts to implement the CCSS. However, the latter might be more relevant for ultimate exposure of students to the CCSS and potential effects on student achievement than formal state expectations. To incorporate this idea into our analysis, we make use of a survey of state education agency officials provided by Webber et al. (2014). They conducted a survey of state education agency officials which collects information on actual state efforts towards CCSS implementation. Specifically, the survey respondents answer questions about whether the state has provided, guided or funded professional development on the CCSS, whether it has provided curriculum or instructional materials for the CCSS, and whether it has worked with a federally funded consortium to develop assessments aligned with the CCSS. In this treatment coding, we count a schooling year in a given state as being exposed to the CCSS if this state has adopted the CCSS according to Achieve Inc. (2013) and CCSSI (2021) and pursued at least two out of three CCSS implementation strategies as reported by the state education agency officials.

Third, we calculate a treatment indicator capturing effective CCSS implementation. Here, we build on the idea that effective change of the state standard can only be induced by the CCSS if the state standard in place prior to the adoption of the CCSS in the state in question is sufficiently different from the CCSS. To this end, we make use of a comparison of academic rigor of the CCSS with the respective state standards in place prior to the CCSS provided by Carmichael et al. (2010). We code students from states as being in the control group at all years if their pre-CCSS state standards are "too close to call" in both math and ELA in a comparison with the CCSS (in addition to coding students from states that did not adopt the CCSS according to Achieve Inc. (2013) and CCSSI (2021) as being in the control group at all years).

Fourth, we account for the fact that some states may have adopted and/or implemented some elements of the CCSS temporarily, even when they are listed as non-adopters and non-implementers in Achieve Inc. (2013) and CCSSI (2021). According to Bleiberg (2021), four of the eight non-permanent adopters of the CCSS in the coding based on Achieve Inc. (2013) and CCSSI (2021) have implemented at least some elements of the CCSS temporarily. The map presented in Figure 1 depicts them as temporary adopters. In this treatment coding, we count a schooling year as being exposed to the CCSS if the state in question adopted the CCSS temporarily or permanently.

Fifth, we argue that the relevant criterion for actual CCSS implementation might be the alignment of the content of state-mandated standardized testing with the CCSS. To assess this hypothesis, we did own background research to find out which state mandated what type of standardized test for each grade group and year. State-specific details on which tests (including field and transitional tests) are mandated when and for which grade are reported in Table C.17. Subsequently, we assessed which of these tests are aligned with the CCSS. This analysis allowed us to infer the year in which CCSS-aligned standardized testing was mandated in a given state. In the corresponding treatment coding, we count a schooling year in a given state as being exposed to the CCSS if this state has mandated

CCSS-aligned standardized testing in any group grade in that year.

Table C.18 presents the treatment status for each state for the baseline definition of CCSS adoption and the five definitions of CCSS implementation. In particular, it shows whether schooling years in a given state never count as being exposed to the CCSS ("always control"), or, if they do, from which year onwards.

Table C.17 – State-mandated tests, by state and grade group from 2010 onwards (based on own research)

State	3-8 grades	High school
Alabama	**2010**: Alabama Reading and Math Test (ARMT+) **2014**: ACT Aspire	**2010**: Alabama High School Graduation Exam (AHSGE) **2014**: ACT End of course
Alaska	**2010**: Standards-Based Assessments (SBAs) **2015**: Alaska Measures of Progress (AMP) **2017**: Performance Evaluation for Alaska's Schools (PEAKS)	**2010**: SBAs **2015**: AMP; ACT, SAT, or WorkKeys **2017**: PEAKS; ACT, SAT, or WorkKeys
Arizona	**2010**: Arizona Instrument to Measure Standards (AIMS) **2014**: Field test PARCC **2015**: AzMerit	**2010**: AIMS **2014**: Field test PARCC **2015**: AzMerit
Arkansas	**2010**: Arkansas Comprehensive Testing, Assessment, and Accountability Program (ACTAAP) **2013**: Arkansas Benchmark **2014**: Field test PARCC **2015**: PARCC **2016**: ACT Aspire	**2010**: Arkansas Comprehensive Testing, Assessment, and Accountability Program (ACTAAP) **2013**: Arkansas Benchmark **2014**: Field test PARCC **2015**: PARCC **2016**: ACT Aspire
California	**2010**: Standardized Testing and Reporting (STAR) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: Standardized Testing and Reporting (STAR) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced

Table C.17 - Aligned Testing Details (continued)

State	3-8 grades	High school
Colorado	**2010**: Colorado Student Assessment Program (CSAP) **2012**: Transitional Colorado Assessment Program (TCAP) **2014**: Field test PARCC **2015**: PARCC	**2010**: Colorado Student Assessment Program (CSAP) **2012**: Transitional Colorado Assessment Program (TCAP) **2014**: Field test PARCC **2015**: PARCC **2016**: PSAT, ACT **2017**: SAT
Connecticut	**2010**: Connecticut Mastery Test (CMT) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: Connecticut Academic Performance Test (CAPT) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2016**: SAT
Delaware	**2010**: Delaware Comprehensive Assessment System (DCAS) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: Delaware Comprehensive Assessment System (DCAS) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2016**: SAT
District of Columbia	**2010**: District of Columbia Comprehensive Assessment System (DC CAS) **2012**: DC CAS revised (transitional test) **2013**: DC CAS revised **2014**: Field test PARCC **2015**: PARCC	**2010**: District of Columbia Comprehensive Assessment System (DC CAS) **2012**: DC CAS revised (transitional test) **2013**: DC CAS revised **2014**: Field test PARCC **2015**: PARCC
Florida	**2010**: Florida Comprehensive Assessment Test (FCAT) **2011**: FCAT 2.0 **2014**: Florida Standards Assessment (FSA)	**2010**: FCAT **2011**: Florida End-of-Course (EOC) Assessments **2014**: FSA or Next Generation Sunshine State Standards (NGSSS) **2016**: FSA

Table C.17 - Aligned Testing Details (continued)

State	3-8 grades	High school
Georgia	**2010**: Criterion-Referenced Competency Tests (CRCT) **2015**: Georgia Milestones Assessment System (GMAS)	**2010**: End of Course Test (EOCT) **2015**: GMAS
Hawaii	**2010**: Hawaii State Assessment (HSA) **2014**: Part-HAS Part-Smarter Balanced (transition test) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: Hawaii State Assessment (HSA) **2014**: Part-HAS Part-Smarter Balanced (transition test) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced
Idaho	**2010**: Idaho Standards Achievement Test (ISAT) **2013**-14: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: Idaho Standards Achievement Test (ISAT) **2013**-14: Field test Smarter Balanced **2015**: Smarter Balanced in 10th grade; Choice of ACT, SAT or ACT Compass for 11th grade.
Illinois	**2010**: Illinois Standards Achievement Tests (ISAT) **2014**: Field test PARCC **2015**: PARCC	**2014**: Field test PARCC **2015**: PARCC **2016**: SAT
Indiana	**2010**: Indiana Statewide Testing for Educational Progress Plus (ISTEP+)	**2010**: ISTEP+, end-of-course tests
Iowa	**2010**: Iowa Test of Basic Skills (ITBS) **2011**: Iowa Assessments **2014**: Field test Smarter Balanced	**2010**: Iowa Test of Educational Development (ITED) **2011**: Iowa Assessments **2014**: Field test Smarter Balanced
Kansas	**2014**: No test **2015**: Field test Kansas State Assessment (KSA) **2016**: KSA	**2014**: No test **2015**: Field test Kansas State Assessment (KSA) **2016**: KSA
Kentucky	**2010**: Kentucky Performance Rating for Educational Progress (K-PREP)	**2010**: K-PREP, ACT QualityCore, ACT

Table C.17 - Aligned Testing Details (continued)

State	3-8 grades	High school
Louisiana	**2006**: Louisiana Educational Assessment Program (LEAP) and iLEAP **2013**: LEAP and iLEAP revised (transitional test) **2014**: Field test PARCC **2015**: PARCC **2016**: Mix of PARCC and LEAP	**2010**: End-of-course tests, ACT, ACT Plan **2013**-14: End-of-course revised (transitional test) **2015**: End-of-course revised
Maine	**2010**: New England Common Assessment Program (NECAP) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2016**: Maine Educational Assessments (MEA)	**2010**: SAT **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2015**: SAT
Maryland	**2010**: Maryland State Assessment (MSA) **2014**: Field test PARCC **2015**: PARCC	**2010**: Maryland High School Assessment (HSA) **2014**: Field test PARCC **2015**: PARCC
Massachusetts	**2010**: Massachusetts Comprehensive Assessment System (MCAS) **2014**: Field test PARCC **2014**: Districts choose between PARCC or MCAS **2016**: Mix of PARCC and Next Generation MCAS **2017**: Next Generation MCAS	**2010**: Massachusetts Comprehensive Assessment System (MCAS) **2014**: Field test PARCC **2014**: Districts choose between PARCC or MCAS **2015**: MCAS
Michigan	**2010**: Michigan Educational Assessment Program (MEAP) **2014**: Field test Smarter Balanced **2015**: Michigan Student Test of Educational Progress (M-STEP)	**2010**: Michigan Merit Exam (MME: includes SAT, WorkKeys) **2015**: MME, PSAT

Table C.17 - Aligned Testing Details (continued)

State	3-8 grades	High school
Minnesota	**2010**: Minnesota Comprehensive Assessments (MCA)	**2010**: MCA
Mississippi	**2010**: Mississippi Curriculum Test (MCT) **2014**: Field test PARCC **2015**: PARCC **2016**: Mississippi Academic Assessment Program (MAAP)	**2010**: Subject Area Testing Program (SATP) **2014**: Field test PARCC **2015**: PARCC **2016**: ACT
Missouri	**2010**: Missouri Assessment Program (MAP) **2014**: MAP revised (transitional test) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2016**: MAP	**2010**: Missouri End-of-Course Assessments **2014**: End of Course (EOC) revised (transitional test) **2015**: EOC revised, ACT
Montana	**2010**: Montana's Criterion Reference Test (Montana's CRT) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: Montana's CRT **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2016**: ACT
Nebraska	**2010**: Nebraska State Accountability Tests (NeSA)	**2010**: NeSA **2017**: ACT
Nevada	**2010**: Nevada's Criterion Reference Test (Nevada's CRT) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: High School Proficiency Examination (HSPE) **2015**: ACT
New Hampshire	**2010**: New England Common Assessment Program (NECAP) **2013**: NECAP revised (transitional test) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: NECAP **2013**: NECAP revised (transitional test) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2016**: PACE, SAT

Table C.17 - Aligned Testing Details (continued)

State	3-8 grades	High school
New Jersey	**2010**: New Jersey Assessment of Skills and Knowledge (NJASK) **2014**: NJASK revised (transitional test) **2014**: Field test PARCC **2015**: PARCC	**2010**: High School Proficiency Assessment **2014**: Field test PARCC **2015**: PARCC
New Mexico	**2010**: New Mexico Standards-based Assessment (NMSBA) **2014**: Field test PARCC **2015**: PARCC	**2010**: NMSBA **2014**: Field test PARCC **2015**: PARCC
New York	**2012**: Field test **2013**: New York State English Language Arts and Mathematics Tests **2014**: Field test PARCC **2016**: New York State Assessments	**2013**: Regents Exams **2014**: Regents revised **2014**: Field test PARCC
North Carolina	**2012**: Field test **2013**: End-of-grade tests **2014**: Field test Smarter Balanced	**2012**: Field test **2013**: End-of-course tests, ACT PLAN, ACT, WorkKeys **2014**: Field test Smarter Balanced
North Dakota	**2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2014**: Field test Smarter Balanced **2015**: Smarter Balanced
Ohio	**2014**: Ohio Achievement Assessments **2014**: Field Test PARCC **2015**: PARCC **2016**: Ohio State Tests (OST)	**2014**: Ohio Graduation Tests **2014**: Field Test PARCC **2015**: PARCC **2016**: OST, Ohio Graduation Test **2017**: End-of-course tests, SAT/ACT
Oklahoma	**2010**: Oklahoma Core Curriculum Test (OCCT) **2017**: Oklahoma School Testing Program (OSTP)	**2010**: End-of-course tests **2017**: OSTP

Table C.17 - Aligned Testing Details (continued)

State	3-8 grades	High school
Oregon	**2010**: Oregon Assessment of Knowledge and Skills **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: Oregon Assessment of Knowledge and Skills **2014**: Field test Smarter Balanced **2015**: Smarter Balanced
Pennsylvania	**2010**: Pennsylvania System of School Assessment (PSSA) **2013**: Field test PSSA revised **2015**: PSSA revised	**2010**: Pennsylvania System of School Assessment (PSSA) **2013**: Keystone Exams
Rhode Island	**2010**: New England Common Assessment Program (NECAP) **2014**: Field test PARCC **2015**: PARCC	**2010**: New England Common Assessment Program (NECAP) **2014**: Field test PARCC **2015**: PARCC
South Carolina	**2010**: South Carolina Palmetto Assessment of State Standards (SCPASS) **2015**: ACT Aspire **2016**: SC Ready	**2014**: ACT Plus Writing, ACT WorkKeys **2015**: End-of-course tests, ACT
South Dakota	**2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2014**: Field test Smarter Balanced **2015**: Smarter Balanced
Tennessee	**2014**: Tennessee Comprehensive Assessment Program (TCAP) **2014**: Field test PARCC **2015**: TNReady	**2014**: Tennessee Comprehensive Assessment Program (TCAP) **2014**: Field test PARCC **2015**: TNReady
Texas	**2015**: State of Texas Assessments of Academic Readiness (STAAR)	**2015**: STARR
Utah	**2014**: Field test Student Assessment of Growth and Excellence (SAGE) **2015**: SAGE	**2014**: Field test Student Assessment of Growth and Excellence (SAGE) **2015**: SAGE, ACT
Vermont	**2010**: New England Common Assessment Program (NECAP) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: New England Common Assessment Program (NECAP) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced

Table C.17 - Aligned Testing Details (continued)

State	3-8 grades	High school
Virginia	**2010**: Standards of Learning (SOL)	**2010**: SOL
Washington	**2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2010**: High School Proficiency Exam **2014**: Field test Smarter Balanced **2015**: Smarter Balanced
West Virginia	**2014**: Field test Smarter Balanced **2015**: Smarter Balanced	**2014**: Field test Smarter Balanced **2015**: Smarter Balanced
Wisconsin	**2010**: Wisconsin Knowledge and Concepts Exam (WKCE) **2014**: Field test Smarter Balanced **2015**: Smarter Balanced **2016**: Wisconsin Forward	**2015**: ACT, ACT Aspire
Wyoming	**2010**: Proficiency Assessments for Wyoming Students (PAWS), Student Assessment of Writing Skills (SAWS) **2013**: Field test PAWS revised **2014**: PAWS revised, Field test Smarter Balanced **2017**: Wyoming Test of Proficiency and Progress (WY-TOPP)	**2016**: ACT Aspire (9-10), ACT (11) **2017**: Wyoming Test of Proficiency and Progress (WY-TOPP), ACT

Table C.18 – Treatment codings by state

State	Main treatment: CCSS Adoption	CCSS implementation requirement	CCSS implementation strategies	Effective CCSS implementation	Include temporary CCSS adopters and implementers	CC-aligned testing
Alabama	2010	2013	2010	Always Control	2010	2014
Alaska	Always Control	Always Control	Always Control	Always Control	Always Control	Always Control
Arizona	2010	2013	2010	2010	2010	2014
Arkansas	2010	2013	2010	2010	2010	2014
California	2010	2014	2010	2010	2010	2014
Colorado	2010	2013	2010	2010	2010	2012
Connecticut	2010	2013	2010	2010	2010	2014
Delaware	2010	2012	2010	2010	2010	2014
D.C.	2010	2012	Always Control	2010	2010	2012
Florida	Always Control	Always Control	2010	Always Control	2010	Always Control
Georgia	2010	2014	2010	Always Control	2010	2015
Hawaii	2010	2013	2010	2010	2010	2014
Idaho	2011	2013	2010	2011	2011	2013
Illinois	2010	2013	2010	2010	2010	2014
Indiana	Always Control	Always Control	2010	Always Control	2010	Always Control
Iowa	2010	2012	2010	2010	2010	2011
Kansas	2010	2013	2010	2010	2010	2015
Kentucky	2010	2011	2010	2010	2010	2010
Louisiana	2010	2013	Always Control	2010	2010	2013
Maine	2011	2012	2010	2011	2011	2010
Maryland	2010	2013	2010	2010	2010	2014
Massachusetts	2010	2013	2010	Always Control	2010	2010
Michigan	2010	2012	2010	2010	2010	2010
Minnesota	2010	2012	Always Control	2010	2010	Always Control
Mississippi	2010	2013	2010	2010	2010	2014
Missouri	2010	2014	2010	2010	2010	2014
Montana	2011	2013	Always Control	2011	2011	2014

Treatment codings by state (continued)

State	Main treatment: CCSS Adoption	CCSS implementation requirement	CCSS implementation strategies	Effective CCSS implementation	Include temporary CCSS adopters and implementers	CC-aligned testing
Nebraska	Always Control	Always Control	Always Control	Always Control	Always Control	Always Control
Nevada	2010	2013	2010	2010	2010	2014
New Hampshire	2010	2014	2010	2010	2010	2013
New Jersey	2010	2013	2010	2010	2010	2014
New Mexico	2010	2013	2010	2010	2010	2014
New York	2010	2013	2010	2010	2010	2012
North Carolina	2010	2012	2010	2010	2010	2012
North Dakota	2011	2013	2010	2011	2010	2015
Ohio	2010	2013	2010	2010	2010	2014
Oklahoma	Always Control	Always Control	2010	Always Control	2010	Always Control
Oregon	2010	2014	2010	2010	2010	2014
Pennsylvania	2010	2013	2010	2010	2010	2013
Rhode Island	2010	2013	2010	2010	2010	2014
South Carolina	Always Control	Always Control	2010	Always Control	2010	Always Control
South Dakota	2010	2014	Always Control	2010	2010	2014
Tennessee	2010	2013	2010	2010	2010	2014
Texas	Always Control	Always Control	Always Control	Always Control	Always Control	Always Control
Utah	2010	2013	2010	2010	2010	2014
Vermont	2010	2013	2010	2010	2010	2014
Virginia	Always Control	Always Control	Always Control	Always Control	Always Control	Always Control
Washington	2011	2014	Always Control	2011	2012	2014
West Virginia	2010	2014	2010	2010	2010	2014
Wisconsin	2010	2014	2010	2010	2010	2014
Wyoming	2012	2014	Always Control	2012	2012	2013

Note: Table shows whether schooling years in a given state are never coded as exposed to CCSS ("always control"), or, if they are, from which year onwards, for different treatment definitions. Data sources: Achieve Inc. (2013) and CCSSI (2021) (CCSS Adoption and CCSS implementation requirements); Webber et al. (2014) (CCSS implementation strategies); Carmichael et al. (2010) (Effective CCSS implementation); Bleiberg (2021) (Include temporary CCSS adopters and implementers); Own research, see also C.17 (CCSS-aligned testing)