Information Shocks and Internet Silos: Evidence from Creationist-Friendly Curriculum*

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Abstract

How the Internet affects the ability of its users to seek out information which either supports or contradicts their existing beliefs remains an open question. To examine this, we analyze the effect of the Louisiana Science Education Act (2008), which allowed the teaching of creationism as an alternative 'theory' to evolution in Louisiana schools, on students' science test performance in nationally administered tests. Using detailed data on Louisiana schools, we employ a difference-in-differences strategy to document that science test achievement declined after the law relative to schools in neighboring Texas. The effect of the law was driven by regions with high Internet penetration and low parental education levels. After the change in policy, Louisiana students were more likely to seek out information on the Internet using search terms which led them to web pages that reinforced a creationist message. Moreover, in line with the baseline results, we find that increased search intensity comes from low-education areas in Louisiana and that it persists outside the school calendar (during test preparation months), implying a continuing effect on students.

Keywords: Evolution, Creationism, Internet, Online Search, Test Scores, Louisiana Science Education Act

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1 Introduction

Over the past two decades, the amount of content, and especially of contentious content, has spiraled online. However, it remains unclear how Internet users draw on sources which might provide facts to either support or challenge their existing beliefs. Do such sources help web users to correct inaccurate beliefs, or reinforce their prior, possibly flawed, beliefs (Sunstein (2001), Sunstein (2007) Gentzkow and Shapiro (2011), Boxell et al. (2017))? On the one hand, the Internet should be able to supply information which might correct falsifiable beliefs. On the other hand, as users control the manner of their search, they may find sources which support their beliefs, even if those beliefs go against the mainstream consensus. Sunstein (2001) forcefully argued that with the vast amount of content available online, people would restrict themselves to information consistent with their existing facts and beliefs.[1] Van Alstyne and Brynjolfsson (2005) argue that the Internet could either lead to a global village online or to cyber-balkanization, based on how individuals choose to use its plethora of information.

To explore this question, we use a shift in the 'facts' people were exposed due to the passing of Louisiana Science Education Act. In June 2008, the Louisiana legislature passed the 'Science Education Act,' which allowed teachers in public schools to use 'supplemental materials' in science class while covering topics such as evolution.[2] This policy has been criticized by scientists and educators, including Nobel laureates, who suggest that it implicitly allowed religious beliefs such as creationism to be taught alongside scientific theories of evolution in the classroom. There is anecdotal evidence that this Act has indeed led to the teaching of creationism[3] in schools across Louisiana.[4]The law also provides a useful

[1]Moreover, he points out that this would be most relevant to people from different political ideologies, with liberals potentially only interacting with other liberals and similarly for conservatives.

[2]Even though the law implicitly allowed the use of additional teaching materials to challenge other phenomena backed by scientific evidence such as global warming, critics mainly saw it as a tool to introduce the teaching of creationism in science classrooms. See http://www.nola.com/education/index.ssf/2017/03/science_evolution_standards.html for more on this.

[3]We acknowledge that creationism, along with its variations (such as Young Earth and Old Earth Creationism) and Intelligent Design, are distinct theories. Teasing out these differences and their effect on students is beyond the scope of this paper.

[4]See http://www.slate.com/articles/health_and_science/science/2015/04/creationism_in_louisiana_public_school_science_classes_school_boards_and.html for anecdotal evidence. See http://www.salon.com/2015/

setting to examine how the internet modifies behavior in response to the introduction of potentially contentious information. The Internet gives students greater access to information in general. However, it is not clear whether students use the internet to contradict and correct beliefs conveyed in the classroom setting (Cantoni et al., 2017), or to reinforce those beliefs.

We analyze how the introduction of information supporting different beliefs regarding science and theories of evolution affects student performance on science tests. Specifically, we analyze the effect of the Louisiana Science Education Act on student performance in high school science tests as part of the nationally administered American College Testing (ACT) standardized tests for students in Louisiana relative to students in Texas which did not have a policy change. We use school-level ACT science test score data between 2003 and 2013 to see whether the policy had any effect on student performance.[5]

We then explore the heterogeneous effects of this law across regions with different levels of Internet penetration and education. If the Internet does play a role in this process then we should expect to see heterogeneous effects of the law depending on the level of Internet usage. We hypothesize that higher Internet penetration being associated with science test achievement would provide us with suggestive evidence of people seeking out creationist information on the Internet in line with what was being taught in the classroom. On the other hand, if higher Internet penetration leads to better test performance, then this would be indicative of information online being used to broaden horizons. To explore this we use Federal Communications Commission (FCC) data on the number of Internet providers, and assess how the effect of the law on science test performance varied with Internet penetration.[6]

06/11/its_official_louisiana_public_schools_are_using_the_book_of_genesis_in_high_school_science_classes/, which discusses how the Book of Genesis is being used in at least one school district's science classes. http://www.bjupress.com/resources/science/grade-5/ is a typical text book which emphasizes God's role - 'Science 5 focuses on man's use of God's creation and design as well as a study of minerals and rocks, fossils, matter and heat, sound and light, weather, biomes, ecosystems, and the respiratory and circulatory systems.'

[5] We follow the literature by looking at test scores as the measure of performance. While test scores might not capture all facets of student knowledge, they are widely used as the performance yardstick by policy makers for allocation of grants, as well as universities in their admission process.

[6] We use the number of Internet service providers (ISPs) as a proxy for Internet adoption. There is a large

After the law was enacted, science test performance worsened in Louisiana schools relative to those in Texas. Quantitatively, the policy change lead to about one third of a standard deviation decline in science test scores. This effect is primarily driven by schools located in relatively underprivileged areas, that is, those with low levels of parental or family education. Moreover, it is schools in underprivileged areas with high Internet penetration which drive our results.[7]

Our identifying assumption for this analysis is that the trends in science test achievement between Louisiana and Texas, that are unexplained by school and time fixed effects, would have remained the same in the absence of the policy being enacted in Louisiana. By carrying out a series of placebo checks, we rule out obvious alternative channels which could explain the change in test scores around the time the law was passed. First, we do not find any similar decline in math test performance after the law passed. This rules out a general decline in student performance in analytical subjects such as science and math around the same time. Second, to rule out a general downward trend in science test performance, we assign fake policy dates while analyzing data from the pre-policy years. We do not find any statistically significant effect when the policy date is falsely assigned to any of the pre-policy years. Last, there is no discontinuous change in other observables related to school education revenues or expenditures, especially associated with science instruction in the classroom. We also show that variation in other underlying demographic variables such as population size, population density and commuting times cannot generate the same effect on test scores as high internet penetration areas. Overall, results in all the placebo specifications are reassuring that what we are measuring is linked with the passing of the Louisiana Science Education Act.

We then investigate the mechanism. The decline of science scores in high-internet penetration areas suggests that information sought out online reinforces supplementary creationism materials being added to school teachings, consistent with the echo chamber

amount of evidence, which we document in Section 2.2, showing that the number of ISPs is highly positively correlated with Internet adoption and usage.

[7] This effect size magnitude we measure is comparable to those documented by Belo et al. (2013), who find a negative effect of Internet use on middle school student test scores in Portugal.

hypothesis (Sunstein (2001), Sunstein (2007), Van Alstyne and Brynjolfsson (2005), Halberstam and Knight (2016)). In line with this, we find that there was an increase in Google search intensity of keywords related to creationism in Louisiana relative to Texas after the law was introduced. There was a significant increase in creationism-related online search intensity even when measured relative to evolution-related search terms. Analyzing search intensity at the city level, we show that this increase in creationism related search was driven by low education regions such as Lafayette and Shreveport and not by high education regions such as New Orleans and Baton Rouge, which maps back into our baseline results. Furthermore, this increase in search intensity exists among students even when schools break for vacation but they have to prepare for the ACT which suggests a persistence in belief in the importance of the topic among students. This finding also allows to separate the effect from being a purely teacher-driven effect where the increased searches are coming primarily from searches in the classroom or day-to-day home assignments.

Our findings contribute to three streams of the Information Systems and Economics literature. The first literature investigates how the Internet affects educational outcomes. Some of this literature is optimistic. Banerjee et al. (2007) find that computer-aided programs aimed at improving math scores in urban Indian schools lead to better student performance in the short run. Other studies such as Machin et al. (2007), Jackson et al. (2014), Angrist and Lavy (2002) and Malamud and Pop-Eleches (2011) also analyze the impact of ICTs on student test scores to find mixed results. In general, though more specific studies suggest that Internet access does not increase test scores. In the Information Systems literature, Belo et al. (2013) find that Internet use in Portuguese schools decreased student test scores, mainly because of time away from work on websites such as Youtube. Goolsbee and Guryan (2006) analyze data on California schools to find no effect of Internet subsidies on student performance along different dimensions. Our paper shows a nuanced view of the internet where students use of the internet may exacerbate the potential effects of non-standard information being given in the classroom.

The second literature is the literature on whether online content leads to ideological segregation or broadened horizons. These studies also have mixed results. In the Information

Systems literature, Van Alstyne and Brynjolfsson (2005) address this issue theoretically to find that the internet could lead to 'balkanization' or to broadening of horizons. Gentzkow and Shapiro (2011) do not find evidence for a substantial level of online segregation in news consumption. Lelkes et al. (2015), on the other hand, find that access to broadband Internet increases political polarization. Our results suggest that access to the internet may not mitigate polarization because the nature of internet search means that people may shape the information they see with their choice of words or emphasis.

Finally, we add to a limited number of studies which attempt to identify the causal effect of school curriculum and educational content on student outcomes. In particular, Cantoni et al. (2017) use a textbook reform in China to find that it led students to feel more favorable towards the Chinese government and have greater skepticism about the free market. Analyzing how the Internet affects the way classroom teaching is processed by students is a fundamental issue that Cantoni et al. (2017) notes but leaves entirely unexplored.[8]

Our paper has implications for both managers of online content platforms and policymakers. They highlight that in the debate about the exposure to content online, internet users shape the nature of the content they are exposed to by the particular phrases they use. For example, when an individual searches for 'Intelligent Design' then www.intelligentdesign.org, which is the second search result, highlights the 'science' of Intelligent Design (see Figures 3 and 4). This extends beyond the passive models of content exposure which often underlie debates regarding 'fake news' online. One potential policy solution is that, if a user's choice of search phrase leads to disputed content, it could be labeled as disputed. Disputed content labels have been shown to reduce the spread of false facts or news online (Friggeri et al., 2014).[9]

In terms of policy, the fact that we measure a decline in science test scoring, and that the decline is driven by Internet penetration, has implications more generally for

[8]Clots-Figueras and Masella (2013) analyze a language policy change in the Catalan education system instituted by the Catalan government and demonstrate that students who were exposed to more years of compulsory education in Catalan identified more with being Catalan than Spanish.

[9]For more on the spread of false news, see Vosoughi et al. (2018).

policy regarding promoting the STEM sector. Such a negative effect on science education test scores that are required for college applications, during a time when the country is facing a shortage of STEM graduates to fill jobs in the government and private sector, is concerning.[10] Our results also suggest that policies focused on promoting internet use may not substitute for the particular curriculum that is pursued in the classroom.

The paper proceeds as follows. Section 2 describes the data and while Section 3 lays out our empirical strategy. Section 4 reports the regression results with Section 5 looking at some robustness checks, and Section 6 concludes.

2 Data

For our empirical analysis we use data on: i) ACT scores, ii) Internet penetration, iii) School district level finances, and iv) other socio-economic indicators at the school district level.

2.1 School Level ACT Scores

In June 2008, the Science Education Act was passed by the Louisiana State Legislature and was signed into law by Governor Bobby Jindal. The law was aimed at allowing teachers in public schools to question and critique existing scientific theories such as evolution and global warming. In addition, teachers were indirectly allowed to present alternate theories such as creationism. Creationism is a belief that the earth originated through an act of God, rather than through natural processes. Young Earth creationists believe that the Earth is less than ten thousand years old. Old Earth creationists interpret their sacred texts to permit a conclusion that matches that of scientific analysis, that the Earth is 4.5 billion years old.

Louisiana was the first state to pass such a law and still remains the only state to have such an education policy in place. Other states, including Texas, have tried and failed

[10] See https://www.usnews.com/info/blogs/press-room/articles/2016-05-17/the-us-news-raytheon-stem-index-shows-america-will-have-to-depend-on-foreign-workers-to-fill-stem-jobs and https://www.theguardian.com/commentisfree/2013/may/01/louisiana-cost-teaching-creationism for more information on the specific consequences for Louisiana and for the US labor market more generally.

to pass similar bills. Since lawmakers in Texas have also attempted to enact such a law several times[11] and because of their empirical similarity prior to Louisiana's shift in policy, Texas seems to serve as the most natural control group relative to Louisiana.

As a later robustness check, we also show that our results replicate if we use Mississippi as a control group.

The ACT is a standardized test taken by high school students in the U.S. in order to apply for college. It is a competitor of the Scholastic Assessment Test (SAT). All four-year college degree-granting institutions accept ACT test scores. In 2011, the ACT overtook the SAT in terms of the number of students taking it. The ACT consists of tests on four subjects: English, Math, Reading and Science, with each subject being graded on a scale of 1-36. By contrast, the SAT does not test Science as one of its subjects.

We obtain comprehensive school-level ACT scores from the State Education Boards of Texas and Louisiana from 2003 to 2013.[12] For each academic year, we have information on the average ACT grade achieved in each school separately for Science, Math, English and Reading.

To ensure confidentiality of student information, average scores are not made available for schools in Texas with fewer than five students taking the ACT in a particular year; in Louisiana, average scores are unavailable for schools with fewer than ten ACT students in a particular year. Each test is graded on a scale of 1-36. The average Science score over the sample period across all schools is 19.76. The mean test scores across different subjects are very similar, at 19.88 for Math, 19.74 for Reading and 18.77 for English.

The science test has 40 multiple choice questions, which need to be answered within 35 minutes. They are mainly related to the analysis of different scientific concepts based on passages provided during the test. While the ACT test requires specific preparation, they still assume that the students will have some knowledge of the material taught in science lessons in school.[13] Advanced knowledge of scientific theories is not required for

[11] For more information on attempts to introduce similar bills and enact these laws in different states, see https://www.aibs.org/public-policy/evolution_state_news.html.

[12] For a large majority of the sample period, this data is publicly available, while for the missing years it was made available by the Boards upon request.

[13] More details on science test questions can be found here: http://www.act.org/content/act/en/products-

the test, though given the time crunch, it is evident that prior information about the concepts could give a substantial competitive edge.[14] This view is echoed by Kaplan Test Prep, one of the most widely used test preparations website, which notes that "...you do not have to be an excellent Science student to score highly on the ACT Science test; some knowledge of the concepts tested and a familiarity with the presentation of certain concepts will almost certainly lead to better scores."[15] In Figure 1, we show an example of a typical passage (taken from Kaplan's webpage) which would appear as part of an ACT science test question. The questions would be based on the evolution-related passage. The passage highlights two differing hypotheses of evolution- 'The Multi-Generational Hypothesis' and 'The Out of Africa Hypothesis.' While students could answer questions based solely on the information provided in the passage, without any prior knowledge of these competing hypotheses, it is evident that familiarity with topic would definitely give them an edge in terms of time and ultimately in test scores.[16] Another example science test passage hosted on Kaplan's website, sourced from www.act.org, requires analysis of passages related to DNA and genetics - concepts which are central to evolutionary theories (https://www.kaptest.com/study/act/the-act-science-test-biology-basics/). Given this background, it is evident that if science teachers spend a significant amount of time using supplemental materials, it will distract students from core topics like human evolution which appear in the ACT, which would in turn, presumably, affect their performance in the ACT science tests.

The ACT takes place about six times during the year between September and June of the academic year and is available in all U.S. states. Students are free to appear for the test on a date of their choice and can re-take it if they do not feel satisfied with their performance. The timing of the law plays a role in terms of how we define the post-policy period. The law was passed in June 2008 and we define the post-policy period as starting

and-services/the-act/test-preparation/science-practice-test-questions.html.

[14] For more information on what exactly students need to do for preparing for the ACT science test, see http://blog.prepscholar.com/the-only-actual-science-you-have-to-know-for-act-science.

[15] See Kaplan's webpage for more information https://www.kaptest.com/study/act/the-act-science-test-biology-basics/.

[16] The link to the passage is here: https://www.kaptest.com/study/act/act-science-conflicting-viewpoints/

in the 2008-09 academic year, which begins in August 2008. Since this implies immediate effects from the law on test-taking performance, we also in a later section show our results hold if we allow a lag of a year for the effect to be measurable.

2.2 Internet Penetration

To arrive at a measure of Internet penetration or connectivity, we use data on the number of high speed Internet service providers (ISPs) in a zip code made available by the FCC (through Form 477). A provider is counted if there is at least one subscriber in the zip code. This data is available only till 2008 and hence we use the number of ISPs at the end of 2007, which is right before the law was enacted, as a measure of Internet penetration. The mean number of ISPs in a zip code is 9.03, with 9 being the median. If the number of high speed ISPs is greater than zero but less than 3, then the exact number is not available in the data. Following Larcinese and Miner (2012), we normalize this to 2, which is the average. We define high internet and low penetration using the median number of ISPs.

The number of ISPs appears a good proxy for overall Internet adoption and usage. Kolko (2010) uses survey data from Forrester Research to find that there is a monotonic relationship between the number of high speed ISPs and the rate of Internet adoption across zip codes in the United States.

Larcinese and Miner (2012) also find a similar relationship between penetration and usage based on FCC data. While formally the number of ISPs is a measure of Internet penetration, because it is highly correlated with usage, we use it as a plausible proxy for Internet adoption.

There is reason to think that the variation in Internet adoption, after controlling for differences in observable demographics, at the end of 2007, is a somewhat exogenous process. Cross-sectional variation in Internet adoption, conditional on observables, is often driven by exogenous factors such as weather, terrain, pre-existing telecommunication cables and right of way laws (see Kolko (2010), Larcinese and Miner (2012), Belo et al. (2013) and Gavazza et al. (2015)). We expect school fixed effects to capture much of this variation.[17]

[17] In line with this hypothesis, we carry out placebo checks below which show that, conditional on a variety

2.3 Google Trends

We use data from Google Trends on the online search intensity of keywords related to creationism and evolution. Google Trends provides historical search volume data at various geographic levels. In our baseline analysis, we use state level keyword search to ensure that there is sufficient volume of searches. While search intensity at finer geographic levels is hard to come by, we get enough searches at the level of a city for Shreveport and Lafayette (low-education areas in Louisiana) and New Orleans and Baton Rouge (high-education areas in Louisiana) which allows us to provide additional evidence on the mechanism. It is important to note that Google Trends does not report the absolute volume of searches, but only an index ranging from 0 to 100 which is based on the number of queries of the words in question relative to the overall number of queries over a period of time in a geographical area. If, for example, a search is "20" on Google Trends, it does not mean there are only 20 searches in that region for that keyword. Instead it means that the region has a relative index of 20, which is a scale-free measure of the relative popularity of the measure. Hence, we can only make qualitative statements about the direction of change in search intensity. Google Trends data has been used in various studies to measure consumer interests (Choi and Varian (2012), Wu and Brynjolfsson (2009)), as well as public attitudes (Stephens-Davidowitz, 2014).[18]

2.4 School District Finances

Information on finances such as total revenues and expenditures accruing to schools in a district, is publicly available at an annual level. The U.S. Census Bureau collects such fiscal data as part of the Annual Survey of Government Finances. We use these variables as controls in our regressions. This data also provides detailed information on different sub-categories of revenue and expenditures, which we use in some of our placebo checks.

of fixed effects and controls, the underlying demographic characteristics of the school district do not affect test scores in the same way as high rates of Internet penetration or adoption do.

[18] Lack of search intensity below state level is a potential drawback faced by most papers using Google Trends data and is not particular to ours. Creationism keywords related search intensity is at par with search intensity for other topics analyzed in these related papers. See descriptive statistics in Table A6 in the Appendix.

Annual estimates on the number of children and the number of children in poverty in a school district is collected by the Census Bureau as part of the Small Area Income and Poverty Estimates program (SAIPE). A school district on average has about 5,600 children.

2.5 Other Socio-Economic Variables

We use the American Community Survey (ACS), administered by the U.S. Census Bureau, to get information on other economic and demographic characteristics at the level of the school district. The ACS provides information on the racial composition, adult education and income levels of the school district. The ACS does not take place annually, and hence we use time-invariant 2007 levels of these variables to control for differences in these characteristics across school districts. On average, a school district has 18% of its adult population with less than a high school degree. We use this measure of educational qualification and its variation across districts to analyze how the law had heterogeneous effects depending on the level of education in a district. We define low and high education levels at the median. Similarly, we define high and low income based on the median levels of the average household income in the sample. In terms of ethnic composition, the average school district is 81% White, while African-Americans account for 8% of the population on average. We control for these differences in racial composition of school districts in our regressions.

3 Empirical Framework

To analyze how the introduction of the Science Education Act influenced the trend of science test performance in Louisiana, we use a difference-in-differences setup with schools in Texas serving as the control group and estimate the following baseline specification:

$$\Delta Science_{it} = \alpha_i + \beta_t + \theta_1 Louisiana_i \times After_t + \theta_2 X_{dt} + \theta_3 Z_d \times After_t + \epsilon_{it}$$

The outcome variable of interest $\Delta Science_{it}$ is the change in science test scores in school i in year t relative to year $t-1$ ($Science_{it} - Science_{it-1}$). This is called the 'gain score' in the literature which accounts for unobserved heterogeneity which might lead to persistence in test scores. For more details on specifications analyzing test scores in the education literature, see Gelman and Hill (2006).[19] α_i are school fixed effects which capture any time-invariant differences across schools, and, in particular, the way science might be taught across schools. β_t are year fixed effects which capture aggregate trends affecting both Louisiana and Texas, such as a change in a federal policy linked to education. Our coefficient of interest is θ_1, which captures the effect of the education policy on science scores in Louisiana schools relative to those in Texas, which did not experience the policy change. The main effect of $After_t$ is collinear with year fixed effects and hence gets dropped from the regression. Similarly, the direct effect of $Louisiana$ is collinear with school fixed effects and is thus dropped from the regression.

We include two sets of controls which vary at the level of the school district (d) to account for differences in socio-economic characteristics across districts. X_{dt} consists of the child population, total education revenue and expenditure, which vary at the level of the district-year. Z_d consists of time-invariant controls (at 2007 levels) including the proportion of Whites, the proportion of African-Americans, median household income, the proportion of the population which has less than a high school degree, the proportion of the population which has some educational qualification, the ratio of poor income to the average income, and the number of Internet providers.

Finally, in order to account for the error term being serially correlated between schools within a particular school district, even after accounting for school fixed effects, we cluster standard errors at the school district level. This ensures that we do not overestimate the precision of our results.[20]

[19]This regression equation is also structurally similar to a specification with a lagged dependent variable used in Jackson et al. (2014) and Chetty et al. (2014). We use terms such as test performance, test score gains and changes in test scores interchangeably. For robustness to alternative functional forms, see Tables 2 and **??**.

The difference dependent variable, apart from addressing potential autocorrelation, also allows us to compare magnitudes with papers in the literature including Goolsbee and Guryan (2006) and Belo et al. (2013).

[20]Clustering at the level of the city, identified through the second and the third digits of the zip code, or at

4 Benchmark Results, Placebos and Mechanism

4.1 Baseline Estimates

4.1.1 Estimates for the Full Sample

We begin our analysis with the main specification (1) to evaluate the effect of the Science Education Law in Louisiana on the change in science test scores in Louisiana schools relative to those in Texas. The main independent variable of interest is the interaction term of whether the school is in Louisiana and whether it is a post-policy period.

The baseline results for the whole sample, displayed in Table 2, show a decline in ΔScience in Louisiana relative to Texas. In column (1), which has no controls, Louisiana $\times$ After is negative and statistically significant at the 1% level. As we add year fixed effects (column (2)), socio-economic variables (column (3)) and school fixed effects (column (4)), the effect remains statistically significant at conventional levels. Once we add both year and school fixed effects in column (4), only the interaction effect remains, since the direct effects of both Louisiana and After get absorbed by the two sets of fixed effects. In terms of the magnitude of the effect for the whole sample, the law reduces test score gains by 0.12 of a standard deviation in our most stringent specification (column (5)). We also look at alternative functional forms to assess the stability of our results. In column (6), we use science scores in levels instead of ΔScience to find similar results. In column (7), we use the science score as a proportion of the total as the dependent variable, finding qualitatively similar results to when we use ΔScience.

4.1.2 Test Scores by Internet Penetration and Parental Education

After establishing results for the full sample, we report estimates which form the core of our paper focusing on how the effect of this law varies across different areas with different levels of Internet penetration and adult (or parental) education.[21] In particular, what role does the Internet play in this process? Does access to information online mitigate or

the state $\times$ year level leaves the results unchanged as reported in Table A2.

[21] We use variation in adult education across different regions as a measure of family (education) quality rather than school quality. The two would, of course, be correlated.

exacerbate the effect of the law? Does the law hurt students who come from areas with low levels of parental education? These questions are of first-order importance because of the widespread use of the Internet in school-related work. In particular, students seeking out information online which is consistent with classroom creationist teachings could harm their academic performance. Moreover, it is well documented that the level of parental education influences their children's outcomes, and hence would affect the effect of the law.

Results in Table 3 provide a clear picture on both these issues. When we split the sample into high Internet and low Internet areas (i.e., the number of Internet providers is above and below the median respectively), we find that Louisiana $\times$ After term is negative and statistically significant only for high Internet areas (column (1)) and insignificant for low Internet areas (column (2)). Moreover, in low education areas with high Internet penetration, the law had a statistically significant negative effect (at the 1% level) on science test performance of schools in Louisiana relative to those in Texas (column (3)). In terms of the magnitude, a coefficient of -0.608 corresponds to approximately one third of a standard deviation decline in the change in science test scores.[22]

Quantitatively, these estimates are in line with those found in (Belo et al., 2013), who also document a 0.7 standard deviation decline in student scores due to the availability of the Internet. We do not find any statistically significant effect of the education law on test scores in low education and low Internet penetration areas (column (4)), which indicates that the Internet does indeed play a vital role in the ways students access and use information related to school work. The size of the coefficient (-0.284) is also less than half of what we found for schools in high Internet regions.[23] Results for high education regions show that the law had no statistically significant effect on science test performance irrespective of whether there was high Internet penetration (column (5)) or low Internet penetration (column (6)). This is in line with intuition, as one would expect families with

[22] We discuss the results by high and low education splits in Section 5.3.1 where we analyze different demographic characteristics which might affect test scores.

[23] When we analyze the change in test scores in areas with low income levels and high (and low) Internet penetration, we do not find statistically or economically significant results, unlike what we find for low education and high Internet areas. This indicates that, as we hypothesized, it is the interaction of low education and access to the Internet which seems to be driving the effect. For more see Table A3 in the Appendix.

strong educational backgrounds to ensure that their children are not adversely affected in any nationally administered test due to a policy shift at the state level. Quantitatively, the coefficients are also a fraction (-0.165 and 0.112 in columns (5) and (6) respectively) of the effect found for regions with low education and high Internet.

Finally, we provide some graphical evidence in Figure 2 which complements our baseline regression results. Focusing on areas with high Internet penetration and low levels of education, we plot the coefficients of a regression of the change in science test scores in schools in Louisiana relative to Texas before and after the policy, conditional on school and year fixed effects and a few controls. There are two main takeaways from this picture. First, before the policy there is no evidence of any systematic differences in science test performance between Louisiana and Texas schools. In other words, there are no pre-trends. Second, it is clear that after the law was introduced in Louisiana, ΔScience in Louisiana schools relative to Texas fell in a statistically significant way. This provides a strong check for our identification strategy since schools across the two states in these areas were comparable before the law and then ΔScience fell in Louisiana schools.

4.2 Placebo Checks

Our identifying assumption is that the trends in the change in science test scores (and other variables) of schools in Louisiana relative to Texas would have remained the same in the absence of the Science Education Law being passed in Louisiana. This assumption cannot be tested directly, but we do carry out a series of placebo checks to ensure that the results from our data are consistent with the identifying assumption.

4.2.1 Effect on Math and other subject scores

Since the law was mainly aimed at influencing the teaching of science in classrooms, if we are indeed capturing the causal effect of the law, we should not see a similar change in other subject test scores. In particular, an important check in our favor would be to rule out any effect on math test performance which would indicate that there was no general tendency of Louisiana schools to under-perform in analytical subjects around the time of the law being passed. Relatedly, analyzing whether the law had an effect on the change in math test scores, especially in regions with high Internet penetration, would be a check on whether high Internet usage served as a general distraction, hindering performance across different subjects in line with what Belo et al. (2013) find looking at Portuguese schools. Our hypothesis would indicate that we should not find any effect of the law on math test score gains in low education and high Internet penetration regions.

Table 4 shows the results of this placebo check. We can see that the law had no effect on the change in math test scores as Louisiana $\times$ After is insignificant. This holds for the full sample (column (1)) as well as high and low internet areas (columns (2) and (3)). When we split the sample further, we find no effect on math test scores in low education and high Internet penetration regions (column (4)), which were driving the reduction in the change in science test scores in our baseline results. This gives us confidence that the presence of the Internet is not leading to a general decline in test performance, but that there is an effect specific to science related performance. Moreover, reassuringly, there is also no change in math test scores in other regions as well (columns (5)-(7)).

As a further check, we look at the policy's effect on English and Reading test performance in Louisiana relative to Texas. The results reported in columns (8) and (9) show that there was no change in test score gains post the policy. This gives us further confidence that we are indeed picking up something relevant to the effect of the law on science instruction in Louisiana classrooms and that the Internet played a role in the decline of science test score gains but did not have a negative effect on performance across all subjects.

4.2.2 False Policy Dates

We now analyze our results related to low education and high Internet penetration areas in more detail. In particular, we want to assess whether these regions were inherently more likely to fare worse than schools in Texas with similar characteristics. Figure 2 shows that since there were no pre-trends, there was no systematic difference between schools in such regions across the two states. We examine this further by analyzing data from the pre-policy years and assigning the policy year to each of those pre-policy years one by one to see whether we can generate the same results as we do with the actual post-policy data.

The results in Table 5 give us confidence in our estimates. When the policy year is assigned to 2004 (column (1)), implying that the post-policy period is 2005-2008, we do not see any statistically significant effect of the policy on Louisiana science test score gains relative to Texas. Additionally, the sign of the coefficient is positive, which would go against our hypothesis that these regions in Louisiana were prone to performing relatively worse in science tests. We find similar null results when the policy year is assigned to 2005 (column (2)), to 2006 (column (3), and finally to 2007 (column (4)).[24]

Overall, these results, along with Figure 2, suggest that regions with high Internet penetration but low levels of education in Louisiana were not systematically under-performing in their science tests relative to their Texas counterparts in the pre-policy period.

4.2.3 Placebo Check with Socio-Economic Observables

As another check to assess whether we are indeed capturing the causal effect of the Science Education Law on test performance in Louisiana, we investigate whether there were any discontinuous changes in other observables at the same time as the law was passed. This could imply that we are merely picking up the effect on science test scores of some other observable, which is moving at the same time as the education policy was changed. Alternatively, there could be a related unobservable which affects both the observable and test scores, for example a general change in attitude towards the sciences.

We estimate the baseline specification while altering the outcome variable of interest in every column in Table 6 as a falsification test.[25] In column (1) where the outcome variable of interest is the total population in the school district, we find that the coefficient on Louisiana $\times$ After is statistically insignificant. In column (2), we find a similar null effect when the dependent variable is the total number of children in the school district, which

[24] To ensure that these results are not driven due to a smaller sample size, we carry out another check where we look at different sub-samples which have a comparable number of observations as in our baseline heterogeneity results. Focusing on 2006 and 2007 as the false policy years (time period right before the policy), we demonstrate the robustness of our results in Table A1.

[25] Since these variables are defined at the level of the school district, our unit of observation in this analysis is school district-year.

we also explicitly control for in all our regressions. In columns (3) and (4), we find no statistically significant change in total revenue or total expenditure of Louisiana schools after the law was passed. In columns (5) and (6), we look at certain sub-categories of sources where the school district is getting its revenue from. In column (5), we find that there was no change in the amount of revenue earmarked for science related activities at the school district level. Moreover, there was no change in the amount of local revenue generated (column (6)) for the school district after the law was passed in Louisiana.[26]

As a final check on observables, we address the concern that this negative effect on science test performance might be driven simply by more (potentially low quality) students enrolling in schools which might be emphasizing creationism in science class. Using data on the number of students taking the ACT in each school-year, we can test this claim. Results in Table 7 show that there was no significant change in the number of students taking the ACT on average after the policy change came in. These results hold for the full sample (column (1)) as well as for the different sub-samples including areas of low education with high internet penetration (column (2)).[27] [28]

4.2.4 Alternative Control Group

Another natural question is whether Texas, though a bordering state that has tried to pass similar legislation that has similar pretrends, is a convincing control group. Therefore, we obtained data from the Mississippi educational authority.[29] The caveat to this data is that we were only able to obtain test score data for a shorter time series (starting from 2006). Despite the sparsity, we are able to replicate our baseline results with Mississippi as an

[26]The R-squared for these regressions is high and is mainly driven by the fixed effects. This is intuitive since one would expect these variables to move very slowly. We find similar insignificant results for the placebos if we analyze demographic variables in first differences but which a much lower R-squared exactly because changes are harder to predict using fixed effects.

[27]We do not have similar data on teachers potentially moving schools, though teacher mobility is mostly determined by student characteristics. See http://educationnext.org/the-revolving-door/ for more on this. Given that we do not see significant change in students moving schools, we can reasonably infer that the law did not affect the teacher mobility either.

[28]Additionally, in the robustness section below, we show that our results are not driven by low or high performing outliers.

[29]Arkansas collects school level data very sporadically with its time series starting only in 2010 making it unsuitable for our analysis.

alternative control group.[30]

Overall, while we cannot test our identifying assumption directly, the placebo checks suggest that unobserved heterogeneity and events happening simultaneously are not driving our key findings.

4.3 Test Scores, Demographics and Google Search

4.3.1 Test Scores and Demographics

Our baseline results indicate that interactions between higher Internet usage and low levels of education are part of the mechanism which drives the decline in test scores. While this provides suggestive evidence for the Internet and education playing a significant role in the process, it is also true that these factors can be correlated with a variety of underlying demographic characteristics of the regions which might drive the results. Hence, we carry out a set of checks to assess whether our results are being driven by population size or density, commuting times or income, or simply reflect a metropolitan-non metropolitan area divide.

In Table 8, in columns (1) and (2), we show that neither high nor low population can generate the same results as high Internet penetration. Similarly, differential levels of child population are not drivers of results which are observationally equivalent to those generated by high Internet penetration areas since the coefficient on Louisiana $\times$ After is statistically insignificant in both columns (3) and (4). The coefficient on Louisiana $\times$ After is insignificant when we split the sample based on high population density (column (5)) and low population density (column (6)) as well as high and low commuting times (columns (7) and (8) respectively). Columns (9) and (10) show that the high Internet results cannot be generated by simply looking at metropolitan and non-metropolitan areas separately. We also find that different levels of Internet penetration does not reflect an income divide with the coefficients for both high income (column (11)) and low income (column (12)) being insignificant. Finally, we also look at high and low levels of education to find that, as in the baseline, high education areas remain unaffected by this law (column (13)). In low

[30] See Table A4 in the Appendix for details.

education areas, the effect of the law is negative and statistically significant (column (14)) as in the case of high Internet penetration areas.

This shows that there is an interaction between low levels of education and the availability of information online due to high Internet penetration which leads to a decline in science test score gains. More generally, these results suggest that we are measuring something meaningful about the synergies between Internet use and education which is not confounded by, and goes beyond, other underlying demographics of the different school districts.

4.3.2 Google Trends for Creationism Search Terms

Our baseline results have demonstrated the negative effect of the Science Education Act on science test performance in Louisiana schools. Our results suggest that the Internet did have an adverse effect on test scores by potentially leading to information silos online rather than broadening students' horizons. To highlight this mechanism cleanly, we would require Internet browsing data at the level of the household and school, which unfortunately is unavailable. In the absence of this detailed data, we consider an alternative way to pin down the mechanism.

We provide evidence for the Internet being used to confirm creationist teachings in the classroom by analyzing Google Trends data. A simple Google search shows how there is easy access to information in line with creationist theories. For example, in Figure 3, we show how a Google search for 'Intelligent Design' brings up results which highlight the case for the 'Science of Intelligent Design', while Figure 4 shows how the second-ranked search result (after the Wikipedia entry), www.intelligentdesign.org, tries to debunk evolution.

State Level Analysis: As an initial step, we use state-level search intensity on keywords associated with creationism and evolution before and after the law was initiated in the Louisiana legislature. At this exploratory step, we collapse the data into a pre and post legislation period.[31] Our hypothesis of access to the Internet allowing people

[31] We focus on three years before and after the policy, and in particular we define the pre-policy period from 05/01/2005 to 05/01/2008 and the post-policy period from 05/02/2008 to 05/01/2011. It is in May 2008

to seek out creationism-related information, due to creationism being taught in the classroom, would imply that we should see an increase in creationism-related search intensity in Louisiana relative to Texas after the law was instituted relative to before. We create a list of creationism- and evolution-related search terms which we validate and supplement based on a Google keyword rank checker tool.[32]

Table 9 shows that the search intensity of creationism-related keywords provide evidence in line with our hypothesis. In particular, the search intensity for 'Creationism' before the law was 10 in Louisiana and 8 in Texas ($\delta_b = 2$) while after the law it was 12 for Louisiana and 6 for Texas ($\delta_a = 6$), which implies that search intensity between Louisiana and Texas, after the law increased with $\delta_a - \delta_b = 4$. We find similar results for search terms such as 'Intelligent Design' ($\delta_a - \delta_b = 2$) which is essentially a synonym for creationism. Other keywords terms related to creationism also see an increase in search intensity, such as 'Young Earth Creationism', 'Flat Earth' and 'Book of Genesis'. Terms which are related to religiosity in general, such as 'Bible', 'Christianity' and 'Catholic Church' also saw an increase in search intensity. This is in line with the idea that creationism is primarily a religious theory, the teaching of which can lead to an increase in religiosity in general.

A concern with this model-free evidence could be that it is possible that search intensity for all kinds of terms went up in Louisiana relative to Texas after the law. In particular, we would like to rule out that search intensity for keywords related to evolution also increased after the law. Reassuringly, we find that evolution-related terms such as 'Homo Sapiens', 'Human Evolution', 'DNA', 'Adaptation' and 'Darwin' either stayed constant or declined in search intensity.

Next, to carry out a more rigorous exercise, we take our keyword list and run a difference-in-differences regression with Google search data aggregated at the state level with year and state fixed effects using the whole sample period (2004-2013). We estimate the following regression equation at the level of the state-year:

that it became clear that the Louisiana Science Education Act would come into effect, since it was passed by the Senate on 04/28/2008. Results are robust to alternative cutoffs. For more on the different stages of the passage of the bill, see https://www.aibs.org/public-policy/evolution_state_news

[32]In particular we use serps.com, which provides keyword ranks related to a search term in Google.

$$Log(Search)_{wit} = \alpha_i + \gamma_w + \beta_t + \theta_1 Louisiana_i \times After_t + \epsilon_{wit}$$

In particular, we look at how search intensity for word w changes after the law is passed controlling for state and year fixed effects (α_i, β_t) as well as word fixed effects (γ_w). Results in Table 10 are in line with our model-free evidence. There was a statistically significant increase in 'Creationism' searches in Louisiana relative to Texas after the policy change (column (1)). Similarly, there is a statistically significant increase in search intensity for 'Intelligent Design' (column (2)), 'Christianity' (column (3)), 'Bible' (column (4)) and 'Church' (column (5)). In column (6), we group all creationism-related keywords together and run the same difference-in-differences regression by additionally allowing for keyword fixed effects. The coefficient on Louisiana × After is positive and statistically significant at the 1% level. Finally, in column (7), we take all creationism- and evolution-related terms and analyze the triple interaction term Louisiana × After × Creationism Words. The coefficient is positive and statistically significant at the 1% level, implying that there was an increase in searches related to creationist keywords relative to evolution-related words after the law in Louisiana.

City Level Analysis: One drawback of using state level search intensity is that we don't know which regions are driving this increase. Our baseline test score results suggest that the increase in creationist search intensity should come from the low education areas and not high education areas. Sub-state level Google search intensity is hard to come by because of measurement issues. But we are able to get search intensity for creationism related keywords for four Louisiana cities: Lafayette, Shreveport, New Orleans and Baton Rouge. Lafayette and Shreveport perform badly in terms of their levels of adult education while New Orleans and Baton Rouge have high levels of adult education.[33] If our search results are to tie in with the baseline results then we should see an increase in search intensity for creationist keywords only for Lafayette and Shreveport. We re-run the diff-in-diff at the

[33] For more see http://www.theadvertiser.com/story/news/local/education/2017/08/01/what-least-educated-region-louisiana/529409001/

city level:

$$Log(Search)_{wit} = \alpha_i + \gamma_w + \beta_t + \theta_1 LouisianaCity_i \times After_t + \epsilon_{wit}$$

The results at the city level are re-assuring as can be seen in Columns (1) and (2) of Table 11. The statistically significant increase in search intensity for creationism related terms only comes from Lafayette and Shreveport (column (1)) while it is insignificant (and of the wrong sign) when we analyze search coming from New Orleans and Baton Rouge. We also carry out a placebo check to assess whether we can replicate this effect using pre-policy data for both these sets of cities. Using creationism related search intensity data from 2004-2008 and assigning a fake policy date of January 2006, we find that the true effect of the policy cannot be replicated for Lafayate and Shreveport as seen in column (1) of Table 12. Similarly, New Orleans and Baton Rouge also do not show a statistically significant increase or decrease in search intensity (column (2)).

As one final step to shed light on our mechanism, we try to assess whether this might be a completely teacher driven phenomenon without any persistent effects on students. In particular, the increase in creationism related search could simply be driven by teachers making students search out creationism related information in the classroom or as a home assignment. We want assess whether these classroom teachings have a persistent impact on how important the topic is perceived by the student. To get at this, we use the fact that two rounds of the ACTs take place in September and October.[34] Students are encouraged to prepare for these tests over the summer which is when the school calendar schedules its summer breaks. Hence, if we see an increase in creationism related search terms in this period then we can indeed attribute it directly to student activity online as schools generally re-open for the new academic year at the end of August or the beginning of September.

Using this variation in timings of the school and ACT calendar, we find that results, in Table 11, are in line with our hypothesis. In particular, we see a rise in creationism related

[34] For example, see the schedule for 2010-2011 here https://www.studypoint.com/ed/act-test-dates-2010-2011/.

search terms post the law in Lafayette and Shreveport when we restrict the sample to the 'pre-exam period' which is July, August and September of each year (column (3)). We do not see a similar increase for New Orleans and Baton Rouge (column (4)). Moreover, we cannot replicate these estimates for the low education areas using only pre-policy data as seen in column (3) of Table 12. Additionally, in Figure 5 we can see the same result graphically for Shreveport and Lafayette. There are again two main takeaways from this picture: (1) There are no discernible pre-trends and (2) there is a significant increase in search intensity during the pre-exam months after the law was passed.[35]

Overall, analyzing the Google Trends data provides suggestive evidence in line with our hypothesis that Internet users in Louisiana were seeking out information related to creationism which was allowed to be taught in classrooms after the Science Education Act was passed.

5 Robustness Checks

5.1 2009 as the Policy Year

So far, in our analysis, we have treated 2008 as the policy year with 2009-2013 being the post-policy period. The policy was passed in June 2008, which could imply that students taking the ACT in the 2008-09 academic year might not be severely affected by classroom instruction since preparations for the ACT start a little in advance. To ensure that our results are robust to this concern, we re-run the baseline estimation using 2009 as the policy year with 2010-2013 serving as the post-policy period. Table 13 shows that these results are in line with our baseline estimates. Only areas with high Internet penetration and low education experience a statistically significant (at the 1% level) decline in ΔScience. The coefficients are quantitatively similar to the baseline as well. Moreover, no other region experienced a similar significant decline in the 2010-2013 post policy period.

[35] Additionally, the fact that searches do not increase in the 2008 calendar year also implies that our search intensity results are not by people simply searching for these terms around the time that the law was passed. In a robustness exercise, we explicitly add a control for a window around when the law was passed in Louisiana in all the regressions which use search intensity. All our results are robust to this control. Results available on request.

5.2 Using Counties Near The Louisiana-Texas Border

Our difference-in-differences approach goes a long way in addressing potential endogeneity concerns and establishing the causal effect of the law in Louisiana on science test performance. School fixed effects and other time-varying control variables account for a variety of factors which could be driving the observed result. Moreover, our placebo checks bolster our causal claims. Nevertheless, the fact that the enforcement of the law depends on the potentially changing characteristics of schools means that endogeneity remains a concern since we can't observe all the relevant variables that could simultaneously determine a change in our outcome variables around the time the law was passed.

To test the robustness of our results, we take an additional step by focusing on a sub-sample of the data which consists of schools located in only those counties which lie close to the Texas-Louisiana border. The underlying assumption is that the schools in these counties would be more similar in their unobservable characteristics relative to all schools across the two states. We restrict our attention to approximately one quarter of the sample to ensure we have enough observations in these counties to assess the heterogeneity of the law's effects, which maps back into our baseline estimates. Using this sub-sample of schools in counties close to either side of the Louisiana-Texas border, we estimate our baseline specification again.[36]

The results in Table 14 show that the estimates based only on schools in these counties is qualitatively similar to what we find in the full sample. The law has a negative and significant effect on the change in science test scores in low education and high Internet penetration areas (column (1)). Additionally, the law has no effect on science test performance of schools in these border counties in areas with low education and low levels of Internet (column (2)), with high education and high Internet levels (column (3)) and high education and low Internet levels (column (4)). These results, which look at schools in more geographically proximate areas across Louisiana and Texas, are in line with the baseline estimates, providing more confidence in our causal claims.[37]

[36] We also carry out propensity score matching to find that our baseline results are robust. Results available upon request.

[37] Our results are also robust to explicitly excluding schools in the New Orleans area which took steps to

5.3 Excluding Outliers

Finally, we also analyze whether our baseline results are robust to the exclusion of outliers and to different functional forms.

In column (1) of Table 15, we drop schools with science test scores above the 95th percentile in low education and high Internet penetration regions while estimating the baseline equation. The coefficient on Louisiana × After is still negative and statistically significant at the 1% level. Similarly, when we drop schools in the bottom 5th percentile (column (2)), results remain qualitatively and quantitatively in line with the baseline.

Next, while considering high Internet penetration areas, we drop schools in areas with Internet penetration above the 95th percentile (column (3)) and below the 5th percentile (column (4)) with no qualitative difference in the effect of the policy relative to our benchmark.[38]

To ensure that our results are not driven by a particular year, we drop observations from 2013. Column (5) indicates that 2013 does not exclusively drive the results, since Louisiana × After is still negative and statistically significant (at the 5% level). In column (6), we drop 2004 and find similar results.

6 Conclusion

In this paper, we quantify the effect of the Louisiana Science Education Act on student performance in nationally administered science tests. We also analyze how this effect varies by the degree of Internet penetration across different areas. Using Texas schools as the control group, we find that the law had a negative and statistically significant effect on science test performance in Louisiana, but that this effect is limited to less-educated regions with high levels of Internet connectivity. We establish that this effect is causal by demonstrating that the law did not affect performance in other subjects. Moreover, we

prohibit the teaching of Creationism and Intelligent Design as part of the Science curriculum in 2012. For details see https://ncse.com/news/2012/12/louisiana-board-bans-creationism-0014665. Results available upon request.

[38] We provide more robustness related to potential internet related (as well as other) outliers and thresholds in Table A5 in the Appendix.

use placebos to demonstrate that there was no similar effect in the pre-policy period and that we do not see any movement in other observables which rules out unobservable factors driving our results.

To identify the mechanism, we find that creationism-related search terms online increased significantly in Louisiana relative to Texas after the policy was introduced. This increase in online search was also relative to a baseline of evolution-related search terms. Moreover, we show that this increase in creationism related search terms was driven by low education areas such as Lafayette and Shreveport. We also show that the decline in test score gains in high Internet areas cannot be generated by characteristics such as population size, density or income, or by whether the area is metropolitan or not, implying that the Internet is not a mere proxy for some underlying demographic attribute of the region. Overall, this provides suggestive evidence that information online is being used to seek out information related to creationist teachings in the classroom.

Our study has limitations, especially related to data availability. First, while we attribute the decline in test scores to teaching in the classrooms, we do not observe exactly what was taught. Further research and more detailed data is required to directly pin down the effect of different teaching tools or types of alternative content on student performance. Second, we do not have Internet penetration data for the entire sample period and hence we analyze the heterogeneity in the effect on test scores using cross-sectional variation in Internet availability. Finally, we present suggestive evidence relating to the change in internet search behavior in affected regions but are not able to tie this data to high school student behavior in particular. Notwithstanding these limitations, this paper represents a useful first step in understanding the interaction between the ability of the Internet to reinforce or change the set of beliefs that a person adopts.

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Table 1: Summary Statistics

	Observations	Mean	Std. Deviation	Min.	Max.
Science Test Score	15,387	19.76	1.97	9.2	31
ΔScience Test Score	13,680	.012	1.36	-9.9	10.3
Math Test Score	15,387	19.88	2.20	9	33.3
ΔMath Test Score	13,680	.092	1.32	-9	9
Reading Test Score	15,387	19.74	2.46	4.5	34
ΔReading Test Score	13,680	.017	1.78	-15.3	12
English Test Score	15,387	18.77	2.59	6	36
ΔEnglish Test Score	13,680	-.033	1.75	-14	13.5
District Education Revenue	10,531	52428.44	131231.7	666	2221585
District Education Expenditure	10,531	54793.86	138150	580	2355857
Child population	10,287	5652.944	15207.26	27	266882
No. of Internet Providers	1,218	9.03	2.95	2	16
Prop. of White	842	0.813	0.136	0.18	1
Prop. of African-American	842	0.077	0.112	0	0.73
Prop. with Less than High School Degree	842	0.176	0.10	0	0.62
Prop. with some education degree	842	0.315	0.07	0.094	0.62
Household Income	842	30731.37	9053.857	11586	109907
Ratio of Poor Income to Average Income	991	0.160	0.084	0.02	0.49

Table 2: Baseline Results: Science Test Scores

VARIABLES	(1) ΔScience	(2) ΔScience	(3) ΔScience	(4) ΔScience	(5) ΔScience	(6) Science	(7) $\frac{Science}{Total}$
Louisiana × After	-0.322***	-0.322***	-0.241***	-0.174***	-0.162**	-0.220**	-0.0016***
	(0.0290)	(0.0290)	(0.0459)	(0.0616)	(0.0789)	(0.087)	(0.0005)
Louisiana	0.0491***	0.0491***	0.122***				
	(0.0165)	(0.0165)	(0.0460)				
After	0.0711***						
	(0.0176)						
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
School FE	No	No	No	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes	Yes
Controls × Internet	No	No	No	No	Yes	Yes	Yes
Observations	13,680	13,680	12,077	12,077	10,124	11,338	11,338
R-squared	0.003	0.013	0.124	0.273	0.288	0.760	0.398

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences in columns (1)-(5), in levels in column (6) and as a proportion of the total score in column (7). Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity. Column (7) does not include school district test scores in other subjects as a control since the dependent variable is a normalized measure.

Table 3: Baseline Results: Heterogeneity in Science Test Scores

VARIABLES	High Internet (1) ΔScience	Low Internet (2) ΔScience	Low Educ+High Int (3) ΔScience	Low Educ+Low Int (4) ΔScience	High Educ+High Int (5) ΔScience	High Educ+Low Int (6) ΔScience
Louisiana × After	-0.247***	-0.0963	-0.608***	-0.284	-0.165	0.112
	(0.0835)	(0.149)	(0.183)	(0.204)	(0.103)	(0.247)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,793	4,331	2,972	2,151	2,821	2,180
R-squared	0.227	0.332	0.213	0.311	0.267	0.363

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, school district expenditures, child population, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 4: Placebo: Math and Other Test Scores

	Full Sample	High Internet	Low Internet	Low Educ+High Int	Low Educ+Low Int	High Educ+High Int	High Educ+Low Int	Low Educ+High Int	Low Educ+High Int
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	ΔMath	ΔMath	ΔMath	ΔMath	ΔMath	ΔMath	ΔMath	ΔEnglish	ΔReading
Louisiana × After	0.0315	-0.00337	0.0957	-0.179	0.106	0.0852	0.243	-0.325	0.097
	(0.0773)	(0.0862)	(0.146)	(0.186)	(0.215)	(0.110)	(0.219)	(0.245)	(0.228)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,124	5,793	4,331	2,972	2,151	2,821	2,180	2,972	2,972
R-squared	0.247	0.194	0.285	0.186	0.292	0.226	0.290	0.156	0.135

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average math test score in first differences in columns (1)-(7) while it is the english test score in column (8) and the reading test score in column (9) (both in first differences). Controls include the number of Internet providers interacted with After; Time varying controls including school district revenue, school district expenditures, child population, school district test scores in English and reading (columns (1)-(7)), school district test scores in math (columns (8)-(9)); and time invariant controls including proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 5: Placebo: Fake Policy Dates for High Internet and Low Education Areas

VARIABLES	Year=2004 (1) ΔScience	Year=2005 (2) ΔScience	Year=2006 (3) ΔScience	Year=2007 (4) ΔScience
Louisiana × After	0.278	0.152	-0.214	0.0908
	(0.277)	(0.228)	(0.271)	(0.347)
Year FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes
Observations	1,470	1,470	1,470	1,470
R-squared	0.267	0.271	0.268	0.264

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. The sample is restricted to 2008 which is the pre-policy period. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 6: Placebo: Other Observables for High Internet and Low Education Areas

VARIABLES	(1) Overall Population	(2) Child Population	(3) Total Revenue	(4) Total Expenditure	(5) Revenue for Science	(6) Total Local Revenue
Louisiana $\times$ After	186.9	-432.2	79.32	-3,283	-7,325	2,001
	(1,252)	(687.1)	(142.0)	(9,602)	(7,733)	(2,984)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School District FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Controls $\times$ Internet	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,622	1,622	1,622	1,622	1,622	1,622
R-squared	0.998	0.997	0.931	0.997	0.995	0.997

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Unit of observation is school district-year. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After (except when the variable itself is the dependent variable). Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 7: Placebo: Student Count in Exams

VARIABLES	Full Sample (1) Student Count	Low Educ+High Int (2) Student Count	Low Educ+Low Int (3) Student Count	High Educ+High Int (4) Student Count	High Educ + Low Int (5) Student Count
Louisiana × After	-1.506	-11.08	-4.222	6.404	0.862
	(5.50)	(9.831)	(6.524)	(14.26)	(5.425)
Year FE	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes	Yes
Observations	10,500	3,016	2,254	2,975	2,255
R-squared	0.955	0.954	0.964	0.939	0.974

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 8: Science Test Scores and Demographics

VARIABLES	High Population (1) ΔScience	Low Population (2) ΔScience	High Child Pop. (3) ΔScience	Low Child Pop. (4) ΔScience	High Density (5) ΔScience	Low Density (6) ΔScience	High Commute (7) ΔScience	Low Commute (8) ΔScience	Metro (9) ΔScience	Non Metro (10) ΔScience	High Income (11) ΔScience	Low Income (12) ΔScience	High Educ. (13) ΔScience	Low Educ. (14) ΔScience
Louisiana × After	-0.0754	-0.0852	-0.0707	-0.0799	-0.0876	-0.119	-0.130	-0.0787	-0.0984	-0.104	-0.0300	-0.0856	-0.065	-0.440***
	(0.0848)	(0.160)	(0.0882)	(0.161)	(0.146)	(0.0912)	(0.122)	(0.125)	(0.118)	(0.119)	(0.0876)	(0.1599)	(0.109)	(0.135)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	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
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,247	4,876	5,251	4,872	4,544	5,580	5,101	5,023	6,266	3,858	5,195	4,929	5,001	5,123
R-squared	0.197	0.338	0.196	0.339	0.317	0.247	0.315	0.275	0.273	0.320	0.192	0.337	0.327	0.264

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable in columns (1) and (2) is high and low levels of total population, in (3) and (4) it is high and low levels of child population, in columns (5) and (6) it is high and low population density, in columns (7) and (8) it is high and low commuting times, whether the zip code is in an urban (column (9)) or rural area (column (10)), and high (column (11)) and low (column (12)) levels of household income. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 9: Google Trends Search Intensity

	Before the Law			After the Law			
Search Word	Louisiana$_b$	Texas$_b$	δ_b	Louisiana$_a$	Texas$_a$	δ_a	$\Delta = \delta_a - \delta_b$
Creationism	10	8	2	12	6	6	4
Intelligent Design	5	4	1	13	9	3	2
Young Earth Creationism	2	2	0	4	2	2	2
Bible	7	6	1	15	9	6	5
God	9	8	1	22	11	11	10
Christianity	6	6	0	12	6	6	6
Catholic Church	5	2	3	12	4	8	5
Flat Earth	0	8	-8	4	4	0	8
Book of Genesis	1	1	0	3	2	1	1
Dinosaur	32	27	5	42	35	7	2
Darwinism	11	7	4	10	5	5	1
Human Evolution	10	6	4	11	8	3	-1
DNA	31	21	10	36	28	8	-2
Adaptation	9	6	3	20	18	2	-1
Darwin	13	6	7	15	9	6	-1
Homo Sapiens	24	16	8	34	26	8	0
Hunter-Gatherer	1	0	1	1	1	0	-1
Genetics	27	15	12	33	22	11	-1
Sexual Selection	1	1	0	2	2	0	0

Table 10: Google Trends Regressions

VARIABLES	Creationism (1) Search Intensity	Intelligent Design (2) Search Intensity	Christianity (3) Search Intensity	Bible (4) Search Intensity	Catholic Church (5) Search Intensity	All Creationism Words (6) Search Intensity	All Words (7) Search Intensity
Louisiana × After	0.692**	0.726***	0.667**	0.610*	1.169***	0.399***	0.0616
	(0.277)	(0.254)	(0.277)	(0.332)	(0.297)	(0.0825)	(0.0772)
Louisiana × After × Creationism Words							0.337***
							(0.113)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Words FE	No	No	No	No	No	Yes	Yes
Observations	242	242	242	242	242	2,904	4,840
R-squared	0.111	0.247	0.353	0.290	0.202	0.398	0.559

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the (logarithm) of Google search intensity related to each term mentioned.

Table 11: City Level Creationism Keywords Regressions

VARIABLES	Full Sample (1) Lafayette+ Shreveport	Full Sample (2) New Orleans+Baton Rouge	Exam Period (3) Lafayette+ Shreveport	Exam Period (4) New Orleans+Baton Rouge
Louisiana City × After	0.184**	-0.013	0.411***	-0.115
	(0.0798)	(0.102)	(0.159)	(0.241)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Words FE	Yes	Yes	Yes	Yes
Observations	3,840	3,960	960	990
R-squared	0.470	0.484	0.497	0.496

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the (logarithm) of Google search intensity of creationism related keywords.

Table 12: Fake Policy Date (2006) for City Level Google Searches

VARIABLES	Full Sample (1) Lafayette+ Shreveport	Full Sample (2) New Orleans+Baton Rouge	Exam Period (3) Lafayette+ Shreveport	Exam Period (4) New Orleans+Baton Rouge
Louisiana City × After	0.0784	-1.242	0.412	0.959
	(0.389)	(0.778)	(0.769)	(1.560)
Year FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Words FE	Yes	Yes	Yes	Yes
Observations	1,536	1,584	384	396
R-squared	0.428	0.426	0.450	0.403

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the (logarithm) of Google search intensity of creationism related keywords.

Table 13: Robustness: Alternative Policy year (2009)

VARIABLES	Low Educ+High Int (1) ΔScience	Low Educ+Low Int (2) ΔScience	High Educ+High Int (3) ΔScience	High Educ+Low Int (4) ΔScience
Louisiana × After	-0.757***	-0.203	-0.240	0.0482
	(0.209)	(0.223)	(0.147)	(0.226)
Year FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes
Observations	2,972	2,151	2,821	2,180
R-squared	0.216	0.312	0.267	0.362

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 14: Robustness: Counties near Texas-Louisiana Border

VARIABLES	Low Educ+High Int (1) ΔScience	Low Educ+Low Int (2) ΔScience	High Educ+High Int (3) ΔScience	High Educ+Low Int (4) ΔScience
Louisiana × After	-1.013**	-0.489	-0.0115	0.0460
	(0.454)	(0.628)	(0.182)	(0.315)
Year FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes
Observations	759	436	801	828
R-squared	0.192	0.369	0.251	0.392

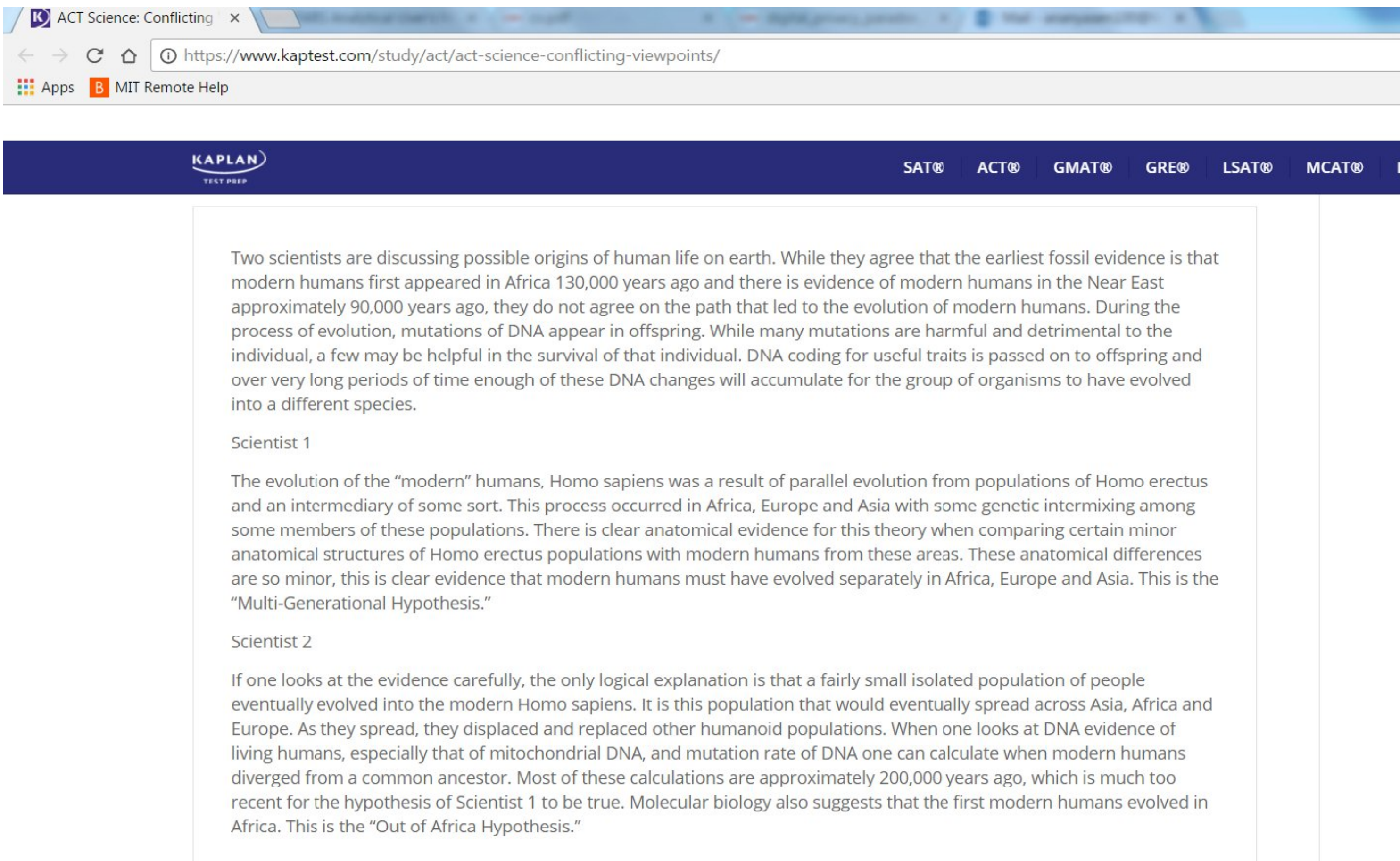
Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table 15: Robustness: Outliers

	Exclude Top Scores	Exclude Lowest Scores	Exclude Highest Internet	Exclude Lowest Internet	Exclude 2013	Exclude 2004
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ΔScience	ΔScience	ΔScience	ΔScience	ΔScience	ΔScience
Louisiana × After	-0.565***	-0.687***	-0.600***	-0.453**	-0.421**	-0.624***
	(0.194)	(0.187)	(0.182)	(0.201)	(0.191)	(0.202)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,873	2,645	2,619	2,516	2,681	2,682
R-squared	0.220	0.253	0.228	0.205	0.217	0.222

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the change in science test scores. Controls in columns (1)-(7), include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Figure 1: Evolution Related Passage



Two scientists are discussing possible origins of human life on earth. While they agree that the earliest fossil evidence is that modern humans first appeared in Africa 130,000 years ago and there is evidence of modern humans in the Near East approximately 90,000 years ago, they do not agree on the path that led to the evolution of modern humans. During the process of evolution, mutations of DNA appear in offspring. While many mutations are harmful and detrimental to the individual, a few may be helpful in the survival of that individual. DNA coding for useful traits is passed on to offspring and over very long periods of time enough of these DNA changes will accumulate for the group of organisms to have evolved into a different species.

Scientist 1

The evolution of the “modern” humans, Homo sapiens was a result of parallel evolution from populations of Homo erectus and an intermediary of some sort. This process occurred in Africa, Europe and Asia with some genetic intermixing among some members of these populations. There is clear anatomical evidence for this theory when comparing certain minor anatomical structures of Homo erectus populations with modern humans from these areas. These anatomical differences are so minor, this is clear evidence that modern humans must have evolved separately in Africa, Europe and Asia. This is the “Multi-Generational Hypothesis.”

Scientist 2

If one looks at the evidence carefully, the only logical explanation is that a fairly small isolated population of people eventually evolved into the modern Homo sapiens. It is this population that would eventually spread across Asia, Africa and Europe. As they spread, they displaced and replaced other humanoid populations. When one looks at DNA evidence of living humans, especially that of mitochondrial DNA, and mutation rate of DNA one can calculate when modern humans diverged from a common ancestor. Most of these calculations are approximately 200,000 years ago, which is much too recent for the hypothesis of Scientist 1 to be true. Molecular biology also suggests that the first modern humans evolved in Africa. This is the “Out of Africa Hypothesis.”

Figure 2: Policy Effect in High Internet and Low Education Regions

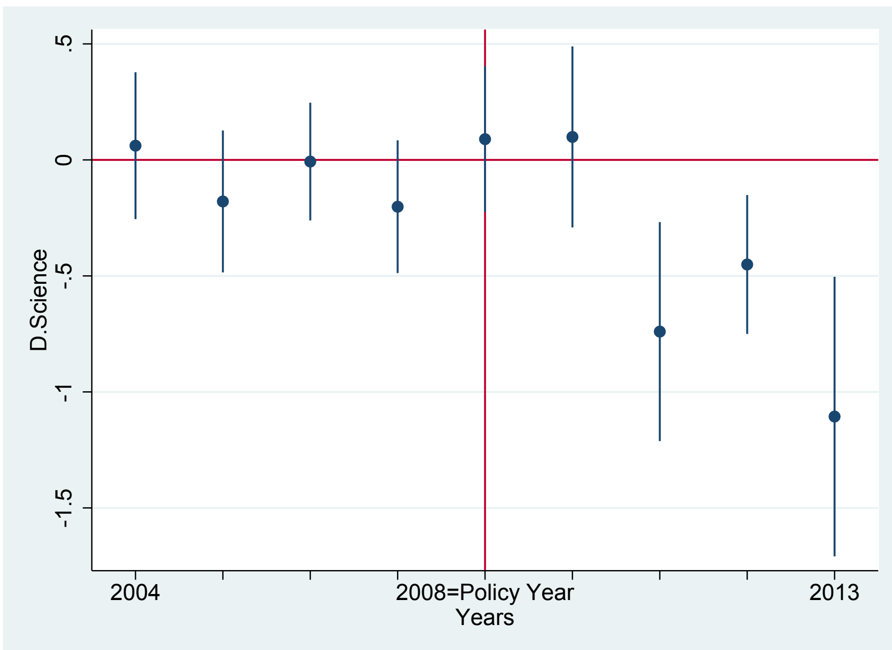


Figure 3: Google Search Result for 'Intelligent Design'

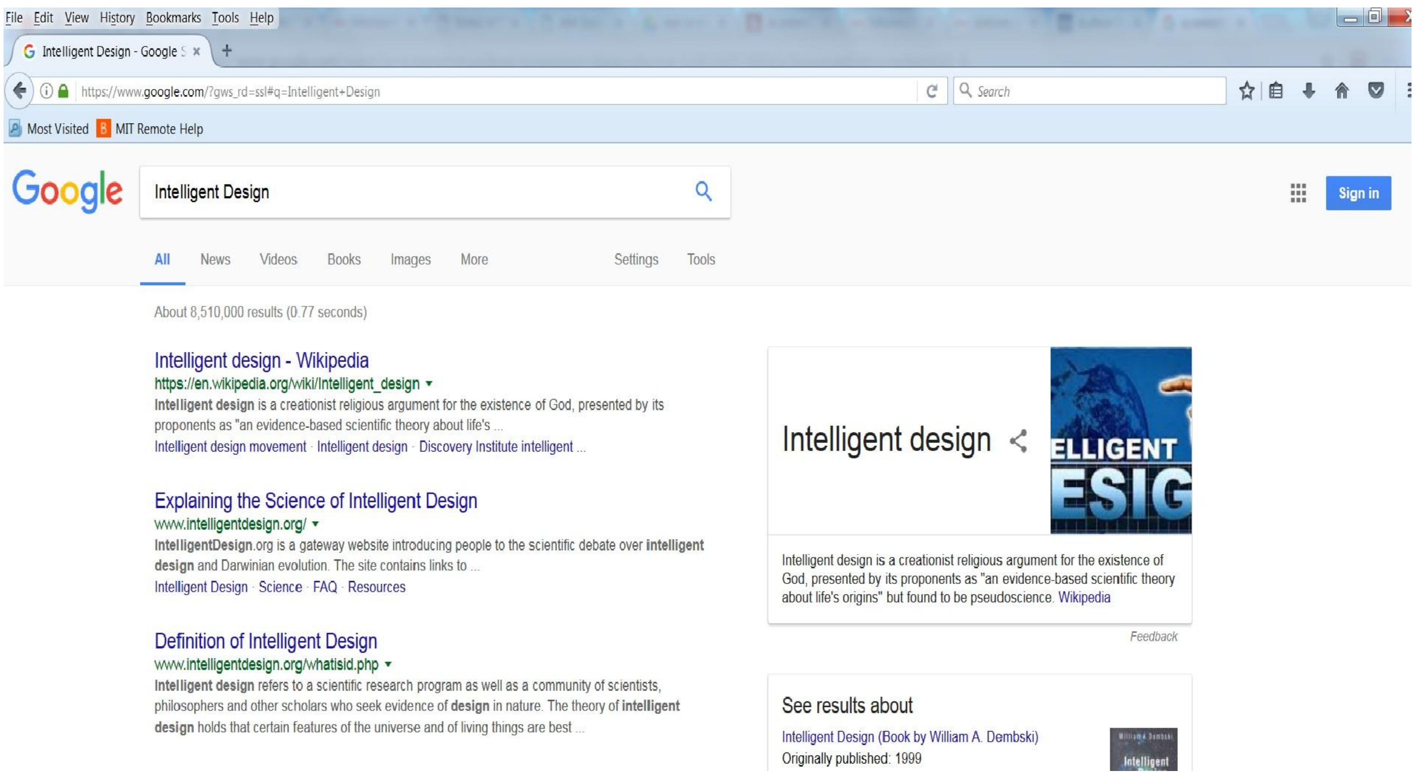


Figure 4: Website for 'Intelligent Design'

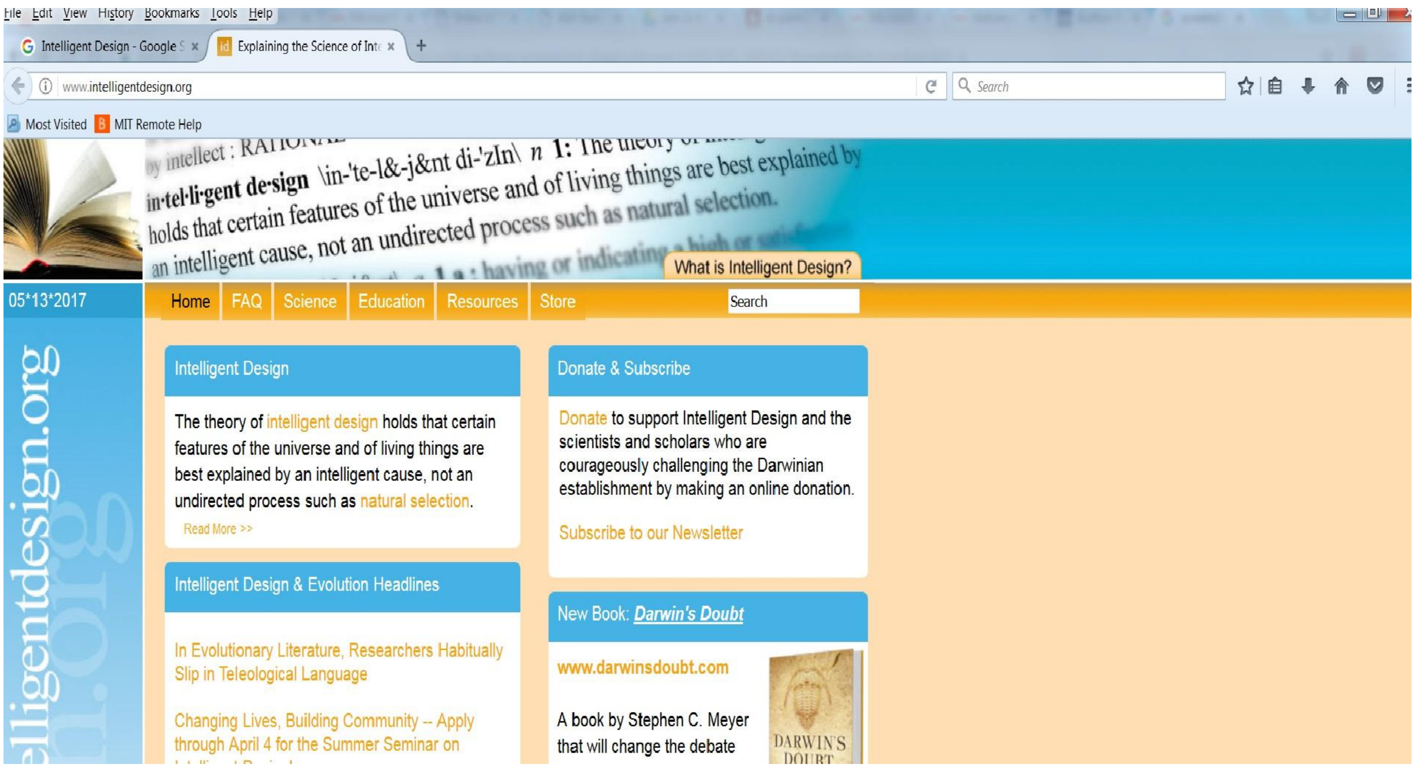
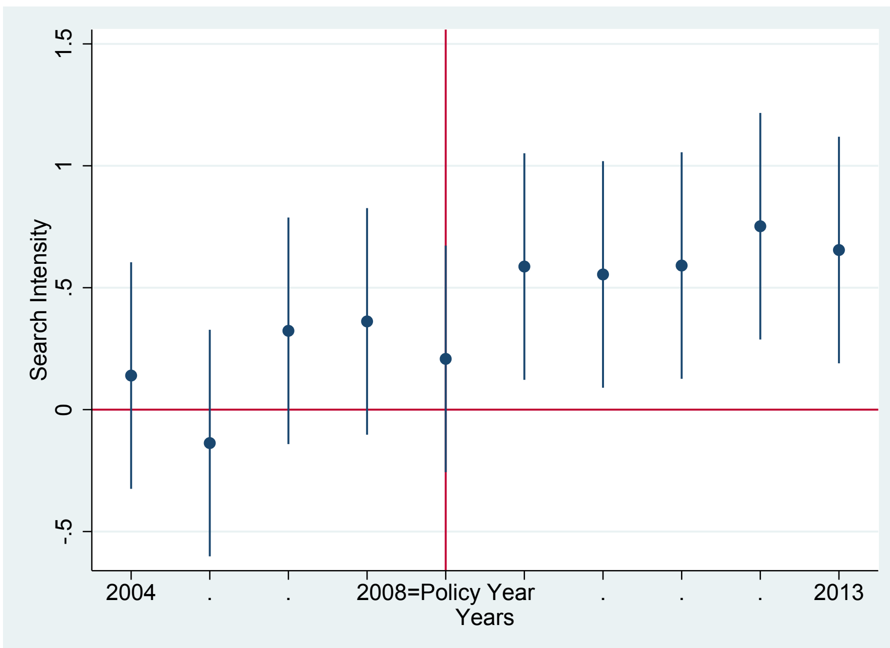


Figure 5: Creationism Searches Before Exams



Appendix : Supplementary Evidence

Table A1: Placebo: Fake Policy Dates for Different Sub-Samples

VARIABLES	Full Sample Year=2006 (1) ΔScience	Full Sample Year=2007 (2) ΔScience	High Internet Year=2006 (3) ΔScience	High Internet Year=2007 (4) ΔScience	Low Internet Year=2006 (5) ΔScience	Low Internet Year=2007 (6) ΔScience	Low Education Year=2006 (7) ΔScience	Low Education Year=2007 (8) ΔScience	High Education Year=2006 (9) ΔScience	High Education Year=2007 (10) ΔScience
Louisiana × After	-0.0990	0.141	0.0339	0.172	-0.297	0.170	-0.336	0.189	-0.0747	0.316
	(0.128)	(0.142)	(0.162)	(0.163)	(0.250)	(0.311)	(0.238)	(0.279)	(0.166)	(0.197)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,991	4,991	2,875	2,875	2,116	2,116	2,523	2,523	2,468	2,468
R-squared	0.361	0.364	0.270	0.271	0.426	0.431	0.338	0.340	0.407	0.405

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. The sample is restricted to 2008 which is the pre-policy period. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table A2: Robustness: Alternative Clustering

VARIABLES	Low Educ+High Int (1) ΔScience	Low Educ+Low Int (2) ΔScience	High Educ+High Int (3) ΔScience	High Educ+Low Int (4) ΔScience	Low Educ+High Int (5) ΔScience	Low Educ+Low Int (6) ΔScience	High Educ+High Int (7) ΔScience	High Educ+Low Int (8) ΔScience
Louisiana × After	-0.608***	-0.284	-0.165	0.112	-0.608**	-0.284	-0.165	0.112
	(0.153)	(0.211)	(0.110)	(0.234)	(0.238)	(0.262)	(0.148)	(0.300)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,972	2,151	2,821	2,180	2,972	2,151	2,821	2,180
R-squared	0.213	0.311	0.267	0.363	0.213	0.311	0.267	0.363

Robust standard errors in parentheses clustered by city in columns (1)-(4) and by state × year in columns (5)-(8). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the change in science test scores. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table A3: Science Test Scores: Income Split and Extended Demographics

VARIABLES	High Income (1) ΔScience	Low Income (2) ΔScience	Low Income+High Int (3) ΔScience	Low Income+Low Int (4) ΔScience	High Internet (5) ΔScience	Low Internet (6) ΔScience	Low Educ+High Int (7) ΔScience	Low Educ+Low Int (8) ΔScience
Louisiana × After	-0.0300	-0.0856	-0.055	-0.037	-0.281***	-0.124	-0.654***	-0.224
(0.0876)	(0.1599)	(0.225)	(0.181)	(0.0858)	(0.085)	(0.142)	(0.189)	(0.198)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extended Demographics	No	No	No	No	Yes	Yes	Yes	Yes
Observations	5,195	4,929	1,498	3,431	5,792	4,310	2,972	2,141
R-squared	0.192	0.337	0.339	0.363	0.228	0.334	0.217	0.313

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures, school district test scores in other subjects while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Extended Demographics include controls for total population, metropolitan area classification, population density and commuting times which are all interacted with After since they are time invariant. Only time varying controls are interacted with number of internet providers to capture heterogeneity.

Table A4: Science Scores with Alternative Control Group: Mississippi

VARIABLES	Low Educ+High Int (1) ΔScience	Low Educ+Low Int (2) ΔScience	High Educ+High Int (3) ΔScience	High Educ+Low Int (4) ΔScience
Louisiana × After	-0.320**	0.00357	0.199	0.522
	(0.137)	(0.269)	(0.197)	(0.516)
Year FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	619	477	721	226
R-squared	0.083	0.128	0.114	0.115

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is a school's average science test score in first differences. Controls include the number of Internet providers (interacted with After) and child population. Due to the noise in the school level data, matching with other observables makes the dataset noisier and unsuitable for further analysis.

Table A5: Additional Robustness Checks

VARIABLES	Low Educ+High Int (1) $\frac{Science}{Total}$	Low Educ+High Int (2) Log(Science)	Low Educ+High Int (3) $\Delta(\frac{Science}{Math})$	Low Educ+High Int (4) ΔScience	Low Educ+High Int (5) ΔScience	Full Sample (6) ΔScience	High Int (7) ΔScience	Low Int (8) ΔScience	Low Educ+High Int (9) ΔScience	Low Educ+Low Int (10) ΔScience	High Educ+High Int (11) ΔScience	High Educ+Low Int (12) ΔScience
Louisiana × After	-0.00280**	-0.0143**	-0.0162**	-0.601***	-0.591***	-0.359***	-0.228**	-0.0709	-0.534***	-0.281	-0.231	0.191
	(0.00121)	(0.00707)	(0.00801)	(0.203)	(0.212)	(0.117)	(0.0989)	(0.149)	(0.174)	(0.203)	(0.157)	(0.244)
Louisiana × After × High Int × Low Educ						-0.385**						
						(0.194)						
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	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
Controls × Internet	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,293	3,293	2,972	2,684	2,540	10,124	3,477	4,222	1,933	2,106	1,543	2,116
R-squared	0.510	0.872	0.059	0.222	0.226	0.283	0.229	0.326	0.225	0.306	0.275	0.353

Robust standard errors in parentheses clustered by school district. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the science score as a proportion of the total in column (1), the logarithm of science in column (2), the change in science relative to math in column (3) while the dependent variable is a school's average science test score in first differences in columns (4)-(12). Column (4) ((5)) drops schools in which the number of students taking the ACT is less (greater) than the 10th (90th) percentile. Columns (7) - (12) analyze the robustness of the results with different internet high-low thresholds and outliers with low internet areas being those which have 4-8 ISPs while high internet areas are those with 10-13 providers. Controls include the number of Internet providers interacted with After; Time varying controls include school district revenue, child population, school district expenditures while time invariant controls include proportion of African-Americans, proportion of Whites, average household income, proportion with less than high school degree, proportion with some degree, average ratio of poor income to the average income, are all interacted with After. Only time varying controls are interacted with number of internet providers to capture heterogeneity. We include the district level test scores in other subjects as a control in columns (4)-(12) while we control for the district level reading scores (in logarithm) in column (3). We also include all other baseline interactions in column (6).

Table A6: Google Trends Descriptive Statistics

Search Word	N	Mean	Std. Dev.	Min.	Max
Creationism	242	6.68	9.75	0	100
Intelligent Design	242	4.16	9.77	0	100
Young Earth Creationism	242	5.75	15.14	0	100
Bible	242	14.42	17.05	0	100
God	242	10.32	11.94	0	100
Christianity	242	7.93	11.97	0	100
Catholic Church	242	7.44	12.90	0	100
Flat Earth	242	3	8.01	0	100
Book of Genesis	242	5.60	14.65	0	100
Dinosaur	242	38.82	12.32	13	100
Darwinism	242	6.11	11.20	11	100
Human Evolution	242	4.90	4.01	0	100
DNA	242	37.62	14.76	0	100
Adaptation	242	20.39	16.12	0	100
Darwin	242	20.84	15.85	0	100
Homo Sapiens	242	30.48	12.27	0	100
Hunter-Gatherer	242	2.61	11.53	0	100
Genetics	242	28.60	16.69	0	100
Sexual Selection	242	2.96	10.85	0	100