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Marie Hull
University of North Carolina at Greensboro and IZA

Katherine Duch
One Minus Beta Analytics

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

One-To-One Technology and Student Outcomes*

New technologies offer many promises to improve student learning, but efforts to bring them to the classroom often fail to produce improvements to student outcomes. A notable exception to this pattern is one-to-one laptop programs. While early evaluations of these programs have been encouraging, they are costly to implement, and no study has investigated the impact of a one-to-one technology program implemented on a large scale over a multiyear period. With administrative school data, this paper uses a differences-in-differences strategy to evaluate the impact of a one-to-one laptop program implemented in a midsize school district. We find that while short-term impacts of the program were modest, math scores improved by 0.15–0.17 standard deviations in the medium term (4–5 years post-implementation). We also investigate heterogeneity in impacts on test scores and the impact of the program on several measures of student behavior.

JEL Classification: I21, J24, O33

Keywords: technology, computers, education, achievement, test score, time use

Corresponding author:
Marie Hull
Department of Economics
University of North Carolina at Greensboro
PO Box 26170
Greensboro, NC 27402-6170
USA
E-mail: mchull2@uncg.edu

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NON-TECHNICAL SUMMARY

We analyze a one-to-one laptop program implemented in the Mooresville Graded School District in North Carolina, USA. One-to-one (or 1:1) technology programs provide one laptop or tablet to every student. The switch to 1:1 technology was accompanied by changes in teaching practices to take advantage of the technology, and students were permitted to take the devices home. We find that math and reading test scores in Mooresville rose with introduction of the program, and that increases were larger in the medium-term than the short-term. This evidence suggests that technology interventions may need longer evaluation periods to produce changes in student outcomes. Furthermore, Mooresville was able to shift costs such that their operating budget covers 98% of the cost of the program. If the test score gains in Mooresville can be reproduced in other districts, 1:1 laptop programs should more than pass cost-benefit tests.

1 Introduction

Educational technology offers many opportunities to innovate in and out of the classroom. Special software gives teachers more scope to customize lesson plans—challenging the advanced students while still offering extra help to students who are struggling.[1] Encountering new technology at school can be especially important for poor and minority students, who often lag behind in computer and internet access at home. Moreover, we know that the workforce of the future must be able to adapt easily to new technologies for our economy to grow. Realizing the potential benefits, many schools and districts have sought to improve the technology they offer to teachers and students. When incorporating educational technology, the goal is not only to improve student learning in core subjects but also to increase computer skills.

In spite of its promise, the evaluations of programs to increase the presence of computer technology in schools and at home yield mixed evidence, with many null effects.[2] Theoretically speaking, the predicted effect of increased computer use is ambiguous because the prediction depends on the activities that computer use replaces.[3] Students may use the internet and computer software to conduct research and complete assignments more efficiently. They also might benefit from computer-adaptive instruction, which customizes lessons based on individual student needs. On the other hand, computers may distract students from educational pursuits through social networking, games, and other entertainment. We still know relatively little about which interventions are effective, and even for interventions that are potentially effective, fidelity of implementation can be a barrier (Chatterji, 2017).

Of the existing studies, most focus on improving access to technology at school or at home, but an increasingly popular intervention seeks to do both. One-to-one (or 1:1) technology programs traditionally provide one laptop or tablet to each student for use at home

[1] Chatterji (2017) overviews the mechanisms by which technology may improve student performance.

[2] Bulman and Fairlie (2016) offer a comprehensive review of technology in education.

[3] Several papers formally model the sources of this ambiguous prediction (see, for example, Fairlie et al., 2010; Belo et al., 2014).

and at school. These programs are more intensive and more costly than most technology interventions, but initial evaluations show promise. Beginning in 2002, the Maine Learning Technology Initiative provided all 7th and 8th grade students in the state and their teachers with laptop computers. Schools were equipped with wireless internet infrastructure, and teachers received professional development on incorporating laptop technology into the curriculum. Comparing 8th grade writing achievement before the initiative and two years after, an evaluation team found that scores increased by one-third of a standard deviation (Silvernail et al., 2011). Suhr et al. (2010) examine students in a suburban southern California school district who participated in a one-to-one program in 4th and 5th grades. While reading scores declined for 4th graders in the first year of the program, the authors find positive effects for reading for both grades in the second year. However, the treatment sample size of 54 students is very small. Shapley et al. (2010) evaluate the Texas Technology Immersion Pilot (TIP), which provided students in grades 6-8 at 21 treatment schools with laptops. Shapley et al. (2009) find that the program had no effect on reading test scores but that the program increased math test scores by 0.16 standard deviations at the end of two years and by 0.20 standard deviations at the end of three years. However, the program was not consistently implemented across treatment schools. Some schools restricted out-of-school laptop use, which goes against the goal of most one-to-one programs to increase access at home and at school. From the developing country context, Cristia et al. (2017) find no evidence that the One Laptop per Child Program in rural Peru increased math or language scores, but their evaluation data was collected only 15 months after implementation. An accompanying qualitative analysis indicates that pedagogical practices did not meaningfully change with the introduction of the laptops. While the literature on one-to-one technology is growing, it is still small relative to the body of research on computer and instructional technology in general. To our knowledge, this study is the first to investigate the impact on test scores of a one-to-one program implemented on a large scale over a period of more than three years.

Our study is also related to research that investigates heterogeneity in the impacts of technology exposure or the impact of technology on intermediate inputs. In their review article, Bulman and Fairlie (2016) note that few studies highlight heterogeneity in treatment effects for school-based interventions and speculate that this omission comes from the large number of studies finding no effect overall. In studies of home computer use, most find no differential impact by gender (Fairlie et al., 2010; Malamud and Pop-Eleches, 2011; Fairlie and Robinson, 2013; Fairlie, 2016).[4] The same is generally true of impacts by race or ethnicity (Fairlie et al., 2010; Fairlie and Robinson, 2013; Fairlie, 2016).[5] Finally, Fairlie and Robinson (2013) and Fairlie (2016) find no evidence that treatment effects vary by prior achievement. To our knowledge, the main evidence on the effect of technology interventions on intermediate outcomes comes from Fairlie and Robinson (2013).[6] Along with a null impact on academic outcomes, they also report that the intervention had no effect on time spent on homework or unexcused absences.

In this paper, we evaluate the impact of the Digital Conversion Initiative in the Mooresville Graded School District on student outcomes. Beginning in 2008, the Mooresville district distributed laptop computers to every student 4th grade and up. At the same time, the district trained its teachers in ways to take advantage of this new technology in their lesson plans. Several thousand students were exposed to the program over the course of five years.

With administrative school data from North Carolina, we use a difference-in-differences strategy to determine whether student outcomes improved with the implementation of the laptop program in Mooresville. We focus on math and reading achievement for students in grades 4-8, but we also examine the impact of the program on student behavior, such as time use and absences. While the short-term impacts of the program were modest, we find that math scores in Mooresville improved by 0.15-0.17 standard deviations in the medium term (4-

[4]Attewell and Battle (1999), which finds a larger impact for boys, and Fiorini (2010), which finds a larger impact for girls, are exceptions.

[5]Exceptions are Attewell and Battle (1999), which finds larger impacts for whites, and Fairlie (2012), which finds larger impacts for minority students in a community college setting.

[6]Vigdor et al. (2014) find some evidence that increased evidence to broadband internet increases time spent on homework. Cristia et al. (2017) find no impact of the One Laptop per Child program on attendance, homework time, or free reading, but the context is quite different from studies based in the U.S.

5 years post-implementation) relative to the trend in neighboring school districts. Reading scores also rose significantly, but by a smaller margin. The difference between the short-term and medium-term impacts highlights the importance of long evaluation periods for technology programs. We also present evidence indicating that the initiative had no impact on the poorest students, despite the positive overall impact, and no differential impact by gender. The evidence on whether students benefitted differentially by ethnicity or ability is mixed, if not inconclusive. In our analysis of student behavior, we find that students exposed to the laptop program did not spend more time overall on homework, but they did spend more of their homework time using a computer. Time spent free reading decreased, and there is some evidence that absences decreased as well.

The results in the paper provide new evidence that one-to-one technology programs are an effective means to raise student achievement. However, there may be a short-term adjustment period before gains are realized. While one-to-one programs are expensive, the effect sizes estimated here are large enough that they may still pass a cost-benefit test. The Mooresville school district was able to cover almost all of the program cost with its operating budget, suggesting that other districts can implement similar interventions without the need to seek outside funding. A caveat to our analysis is that it only represents the experience of one school district; however, the positive results found here illustrate the potential for one-to-one programs to have a substantial impact on student learning.

The remainder of the paper proceeds as follows. The next section provides a summary of Mooresville's Digital Conversion Initiative. Section 3 describes the North Carolina education data and the difference-in-differences methodology. We present results on the impact of the initiative on test scores and student behavior in Section 4. Section 5 concludes.

2 Mooresville's Digital Conversion Initiative

The Mooresville Graded School District is one of a handful of city school districts in the state of North Carolina. The city of Mooresville is located about 25 miles north of Charlotte and is part of the state's Piedmont region. Roughly 5,800 students were enrolled in the district in the 2012-13 academic year across the district's seven schools: three elementary schools serving grades K-3, two intermediate schools serving grades 4-6, one middle school serving grades 7-8, and one high school serving grades 9-12.

The Mooresville district began preparing for the Digital Conversion Initiative in 2007.[7] In advance of the program launch, the district tested network capacity, let teachers test laptops, and provided professional development opportunities to help teachers integrate laptops into their classrooms.

The Digital Conversion Initiative had a staggered rollout. In August 2008, laptops were distributed to all students at the high school and one of the intermediate schools. In August 2009, the program was expanded to the middle school and at the other intermediate school. Thus, by the start of the 2009-10 school year every Mooresville student in grades 4-12 received a laptop.[8] Students are required to return their laptops at the end of each academic year, and so their exposure is restricted to the school year and does not include summer vacation. The district has official policies regarding laptop care,[9] responsible use,[10] and privacy.[11] The district charges parents an annual technology usage fee of $50 per child, though the fee is

[7]Information on the Digital Conversion Initiative comes from several sources. District administrators, most prominently the superintendent, have published pieces on the initiative (Edwards et al., 2012; Edwards, 2013). We also draw information from reporting in the popular press and in education trade publications (Farrell, 2013; Quillen, 2011; Schwarz, 2012; Helms, 2013). Finally, one of the authors toured the district in October 2012.

[8]Beginning in August 2011, all 3rd grade students received a laptop for in-school use only. This study only considers the impact of the program on students in grades 4-8.

[9]Students receive a laptop case and backpack, and they are required to attend a training class with their parents. Families are required to pay for damaged laptops. If a family cannot afford to repair or replace a laptop, the student may borrow a laptop for in-school use only.

[10]The district prohibits use of laptops for amusement or entertainment, commercial activities, or political purposes. Many social media and gaming websites are blocked. The Responsible Use Policy can be found at http://nela2.weebly.com/uploads/3/0/7/9/30795203/mgsd_rup.pdf.

[11]Students have no right of privacy, and administrators may monitor students' computer screens in real time when they are at school.

waived for roughly 18% families that qualify as low-income.

The district worked to ensure that wireless internet access would not be a barrier for students. Students have access to wireless internet on school grounds. With the Digital Conversion Initiative, the Mooresville district extended school building hours and collaborated with town officials to offer free wireless connections in libraries, community centers, municipal buildings, and parks. The district also partnered with a local broadband service provider to offer reduced rates on internet service for low-income families in the district. Still, the district asks teachers not to assign homework that requires the internet so that students without home access to the internet are not disadvantaged.

With the transition to the Digital Conversion Initiative, teachers changed their teaching practices and gained access to a different set of teaching materials. Many teachers adopted a "flipped classroom." In a traditional classroom, teachers lecture during class time, and students complete assignments at home. In a flipped classroom, students might be assigned video lectures to watch at home so that class time can be used to work through assignments, individually or in groups. Flipped classrooms offer students the opportunity to work at their own pace, and teachers may have more time for one-on-one interactions with students.

New technological tools had the potential to change the experience of teachers, administrators, students, and parents. With the laptops, students and teachers gained access to a great number of digital resources, including games, simulations, and activities. Teachers could more easily use technology to design lesson plans, administer tests, and tailor lessons to individual students. Communication is often facilitated with technological tools: Students can collaborate on group projects, and teachers can host review sessions and communicate with students via discussion boards after school hours. With grading tools, teachers save time, students receive immediate feedback, and parents can monitor their child's performance. Administrators may also identify students near the threshold of passing key assessments.

In this study, we do not analyze the role that each of these factors play in the success of the Digital Conversion Initiative. Mainly, this is due to a lack of information on the extent to which individual teachers, students, etc., took advantage of new technological tools. Regardless, the goal of this study is to assess whether a particular one-to-one technology program improved student outcomes. Any one-to-one program likely includes many of the elements mentioned above. Thus, we believe that this study provides valuable information to policymakers and other researchers on the potential impact of one-to-one programs. However, we acknowledge that this lack of information limits our ability to identify the key features that made Mooresville's program successful.

To make room for new tools, the district decreased spending on print textbooks, calculators, encyclopedias, globes, and maps. The district also saved money by closing computer labs. The Digital Conversion Initiative costs about $1 per student per day, exclusive of infrastructure. In other words, this amount includes expenses for software programs, hardware, digital subscriptions, maintenance, and training. The district's operating budget covers 98% of the cost of the program, but it has also relied on funding from community sources. Most notably, Lowe's Home Improvement, which is headquartered in Mooresville, provided $250,000 in the early stages on the conversion.

2.1 The control group

The interpretation of our difference-in-differences estimates hinges on the counterfactual technology environment. Specifically, the impact of Mooresville's Digital Conversion Initiative is estimated relative to the trends in educational technology in other school districts. We conceive of the counterfactual technology environment as "business as usual," where business as usual likely includes some degree of innovation, whether that be updating computer hardware or initiatives to incorporate technology into classroom activities.

Although we have data from all school districts in North Carolina, our preferred control group consists of a set of school districts from neighboring counties. Several considerations motivate this choice. First, as we show with our descriptive results in Section 4.1, the students in neighboring counties are more similar to Mooresville in their observed characteristics. It is also more likely that Mooresville and neighboring counties are similar in terms of unobserved characteristics and trends in unobserved characteristics. Second, we were able to confirm that no school district in our sample of neighboring counties had a major, district-wide technology intervention during the period of our study.[12] Given that North Carolina has over 100 school districts and this study considers a period of 8 years, it was not feasible to document to the technology environment in every district across all years in the sample.[13] We still report results using the rest of the state as a control group and note that these impact estimates are likely lower bounds. An advantage of using the rest of the state as a control group is that we obtain smaller standard errors. This precision is useful in our heterogeneity analysis. We also show empirical evidence that Mooresville Graded School District was truly an outlier in technology use relative to the rest of the state in Section 4.1.

3 Data and methodology

3.1 Data

The data for this study come from the North Carolina Education Research Data Center (NCERDC), which compiles files from the North Carolina Department of Public Instruction (NCDPI). This data set is well-suited for the purposes on this study because it covers the universe of students attending public school in North Carolina for several years before and after the Digital Conversion Initiative. With these records, we can contrast outcomes for the students in Mooresville Graded School District with other students in the state, both before

[12]By major technology intervention, we mean a one-to-one program or a "bring your own device" (BYOD) program.

[13]To our knowledge, there were some school districts outside of the districts in the neighboring county control group that implemented more ambitious technology interventions. We considered eliminating the school districts and schools that we knew to have a major technology initiative during the 2006-2013 period. However, we were not confident that we had identified every one. To this end, we contacted the North Carolina Department of Public Instruction for historical information about technology initiatives. Reports on initiatives by district are only available beginning in 2015, after our sample period ends.

and after the initiative.

For our main test score results and our heterogeneity analysis, we report two sets of results for two different control groups: neighboring counties and the rest of North Carolina. The neighboring county control group includes students attending schools in the counties surrounding Iredell County, except Mecklenburg County,[14] plus the students attending schools in Iredell County that are not part of the Mooresville Graded School District.[15] We do not include Mecklenburg County because it includes the major urban area of Charlotte.

Many of the variables in our analysis, including achievement test scores, come from the End of Grade (EOG) files. Every North Carolina student in grades 3-8 takes standardized math and reading tests at the end of the school year, usually in May. Since the Digital Conversion Initiative targets students in grades 4 and up, we focus on grades 4-8. The files for 2006, 2007, and 2008 offer three years of pre-initiative data; the 2008-09 academic year is a transition year when the initiative was partially implemented; and the files from 2010-2013 provide post-initiative data. This data set includes over 4 million student-year observations, of which about 2,000 come from Mooresville in each year. We standardize test scores to be mean zero, standard deviation one, by subject, grade, and year, using the sample of all students in the state. The EOG files also contain information on student ethnicity, gender, and age rounded to the nearest month. The Masterbuild files contain variables on economic disadvantage, specifically whether a student qualifies for reduced lunch or free lunch. Economic disadvantage is missing for all students in 2008. When this variable is missing, we impute its value from an adjacent year. Each student is assigned a unique master identifier, and thus it is possible to follow students across time, even as they change schools. We pull time-varying school characteristics from the Public School Universe files and create a variable for the percentage of students eligible for subsidized lunch and a set of school ethnic composition variables.[16]

[14]These counties are Alexander, Catawba, Lincoln, Cabarrus, Rowan, and Davie.
[15]Iredell County also contains the Iredell-Statesville Schools district.
[16]These files contain a pupil-teacher ratio variable, but it was discontinued in the 2010-11 school year.

We make use of various NCERDC files to investigate the impact of the Digital Conversion Initiative on student behavioral outcomes. Time allocation is an important mechanism through which the laptop program could affect achievement. Up to 2011, students reported their time use for the following activities at testing time: using a computer at home for schoolwork, working on homework, and free reading. In the raw data, the response options for time use are given in ranges. We convert these ranges to a continuous scale using the midpoint of the range when relevant; for the top option, we use a value close to the lower bound. For computer use, the student had the option to indicate that he does not have a computer at home, and for homework time, he could report that his teacher does not assign homework. In both of these instances, we convert the response to zero. In Appendix Table A.1, we report the original categorical responses, their frequencies, and the conversion scale. Another potential mechanism is a reduction in absenteeism. The variable for days absent is available until 2012.

3.2 Methodology

We use a difference-in-differences strategy to identify the impact of the Digital Conversion Initiative on student outcomes. Because we consider a span of eight years and have a large sample in each year, we allow for differential program impacts in each year. This more flexible specification lets us differentiate short-term and medium-term effects of the program, and it lets us determine whether our conclusions are sensitive to the choice of the base year. We specify the production of outcome y for student i attending school s in year t as:

$$y_{ist} = \sum_{t \neq 2008} \alpha_t * Mooresville_{it} * 1[year = t] + x_{it}\beta + z_{st}\delta + \gamma_s + \varepsilon_{ist} \qquad (1)$$

where the vector α contains the parameters of interest. We use the 2007-08 academic year as the omitted time period since it was the last year before the Digital Conversion Initiative was implemented. Thus, 2006 and 2007 give us two snapshots of how Mooresville fared relative to the control group before implementation. In 2009, the first year of implementation, only students in one of the middle schools had laptops, but by 2010, all Mooresville students in our sample had laptops. Each of the coefficients from 2010 on tell how Mooresville students diverge from the control group trend. We also control for student characteristics x_{it}, which include ethnicity, gender, age-for-grade, and economic disadvantage, and for time-varying school characteristics z_{st}, namely the ethnic composition of the school and the proportion of students eligible for subsidized lunch. The school fixed effect γ_s captures persistent differences across schools in the outcome. Unlike the textbook set-up for difference-in-differences, we do not give the Mooresville district an indicator because the school fixed effects subsume any district-level effects. For test score regressions, we do not include year or grade indicators since we standardize these outcomes by grade and year; we do include them when the outcome is not standardized. Standard errors are clustered at the school district level.

The parameter α_t is interpreted as the impact of attending school in the Mooresville school district $|t - 2008|$ years before or after the initial implementation of the one-to-one laptop program, relative to other school districts in the control group. When the control group is the rest of North Carolina, the estimate of α_t likely understates the true impact of exposure to the Digital Conversion Initiative because some school districts in the control group had their own major technology initiatives. For this analysis, we have opted for a contemporaneous specification over a value-added specification.[17] While the value-added specification offers the advantage of including a proxy for endowed ability and past inputs, the interpretation of the parameters of interest becomes less straightforward. Many students were exposed to the program for a series of years, and for these students, lagged outcomes include the impact of the laptop program in previous years. Instead of thinking of the policy intervention as occurring on the student level, we emphasize the districtwide implementation of the program. In this paper, we ask whether the one-to-one laptop program caused outcomes to change for a school district relative to what we would expect from the control group

[17] See Todd and Wolpin (2003).

trend and the district-level baseline. This formulation is most relevant for policymakers who are considering implementing a one-to-one laptop program on a large scale, such as districtwide or statewide. For completeness, we present estimates for alternative specifications, such as value-added, in our discussion of robustness in Section 4.2.2.

4 Results

4.1 Descriptive results

A simple comparison of mean test scores offers the first evidence of the effectiveness of the laptop program. In Table 1, we report sample means for Mooresville and the control group of neighboring counties at three time intervals: pre-initiative (2006 and 2007), short-term post-initiative (2010 and 2011), and medium-term post-initiative (2012 and 2013). Mean test scores for the neighboring counties are slightly above zero for each time period. Recall that test scores are normalized using the sample of all students in the state, so students in the sample of neighboring counties on average scored slightly above students in the rest of the state. In the pre-initiative period, Mooresville students scored 0.2 standard deviations higher in both subjects compared to the rest of the state. Because students in the treatment and control groups performed better on achievement tests before implementation, it is important to difference out their respective baseline achievement. With this differencing from the short-term post-initiative test scores, we find that Mooresville students scored 0.10 standard deviations higher in math but did not show any significant change in reading. In the medium term, they outperformed the neighboring county trend by 0.18 standard deviations in math and 0.07 standard deviations in reading. From this descriptive evidence, the Digital Conversion Initiative is associated with significant improvements in math, which become larger over time. In reading, only the medium-term effect is statistically significant relative to the baseline.

For the behavioral outcomes, only baseline and short-term means are reported due to data limitations. In the neighboring county sample, computer use increased by about 1 day per month, from 2.5 to 3.4 days. In Mooresville, computer use increased by almost 12 days per month, making the mean difference-in-difference almost 11 days per month. These results indicate that the Digital Conversion Initiative worked in the most basic sense of increasing students' exposure to computers. For the three other behavioral outcomes, Mooresville and its neighboring counties have similar baselines: Students spend 2.5 hours per week on homework, 0.8 hours per day reading in their free time, and are absent from school about 7 days per year. The mean difference-in-differences estimates suggest that the laptop program had a small, positive impact on homework time and negative impacts on free reading and days absent. All of mean difference-in-differences for behavioral outcomes are statistically significant.

The next set of rows contains sample means for demographic characteristics. These variables are important to include as covariates in our difference-in-differences analysis if they are trending differentially. In the pre-initiative period, 72% of students in neighboring counties were white, 14% were black, 9% were Hispanic, and the rest were another ethnicity or multiracial. About half of this sample is female and the average age is about 12.5 years. Eight percent qualify for reduced lunch, and 31% qualify for free lunch. In contrast, Mooresville students in the pre-initiative period were slightly less diverse: 77% were white, 15% were black, and 4% were Hispanic, with the remainder marking some other ethnicity. They were also less economically disadvantaged, with 22% qualifying for free lunch. Comparing the pre-initiative columns to the short- and medium-term columns for Mooresville and the rest of the state, we see that the demographic variables carry some time trend. The last two columns test whether they are trending differentially. Most notably, the proportion of economically disadvantaged students in the neighboring counties is growing faster in the neighboring counties than in Mooresville. Overall, we do not have strong evidence that the student background variables are trending differentially; however, we still include them in the regression results for precision.

In the remainder of the descriptive results section, we discuss the appropriateness of our choice of control group. First, we show evidence that Mooresville is truly an outlier in terms of its students' computer use after the implementation of the Digital Conversion Initiative. In Figure 1, we present histograms of district-year averages of computer use for the periods 2006-2008 (before implementation of the laptop program) and 2009-2011 (after implementation). For this analysis, we include all districts in North Carolina, except charter schools and entities that that are not under the North Carolina Department of Public Instruction.[18] In the panel for 2006-2008, there are no visible outliers.[19] In the panel for 2009-2011, the observations from Mooresville in 2010 and 2011 are clearly extreme outliers. Even excluding these two observations, the distribution appears to skew right, and so we examine the other district-year observations in the upper tail of the histogram. Of these districts, we know that some had major technology initiatives in 2010 or 2011.[20] However, other districts with reported one-to-one programs during this period do not appear in the tail.[21] This evidence suggests that Mooresville as a district is truly an outlier in the extent to which its students use computers. Furthermore, the presence of a one-to-one initiative does not guarantee that a district's students will report higher computer use. A caveat to this analysis is that the computer use variable is only available until 2011, and some major technology initiatives may have started in 2012 or 2013.

Ultimately, we prefer the neighboring county sample as the control group. After researching the school districts in this sample, we found no indication that any district had a major technology initiative in the period 2006-2013. Thus, we have reason to believe that the

[18]Examples of non-DPI education entities are Fort Bragg and the Department of Juvenile Justice.

[19]The observations with the highest z-scores in this period all come from the Chapel Hill-Carrboro school district, which includes the University of North Carolina at Chapel Hill and has been recognized as one of the top-performing school districts in the nation. Thus, it is not surprising that this district has higher-than-average computer use.

[20]For example, Rutherford County Schools launched an initiative in the 2010-11 academic year. Union County Public Schools piloted a one-to-one program in 2008 and later rolled out the program to middle schoolers.

[21]For example, Lee County Schools started a one-to-one laptop initiative in the 2009-10 school year for middle school students and then expanded the program to several elementary schools the next year. Still, the mean computer use among Lee County students was within one standard deviation of the mean in 2010 and 2011.

interpretation of the difference-in-differences estimates as the effect of Mooresville's Digital Conversion Initiative relative to a school district with "business as usual" is the correct one. Furthermore, descriptive results indicate that the "parallel paths" assumption for difference-in-differences analysis is more likely satisfied with the neighboring county sample. Appendix Table A.2 reports descriptive statistics where Mooresville is compared to the rest of the state. Here, we see that Mooresville is less diverse and more economically advantaged compared to the rest of the state at baseline. More importantly, there is evidence that Mooresville and the rest of North Carolina are trending differentially on observed characteristics, which raises the concern that these groups have different trends on unobserved characteristics. In contrast, the observed characteristics for Mooresville and the neighboring counties are on similar paths, and the geographic proximity of these counties suggests that the parallel trends assumption holds.

Still, we report results that use the rest of North Carolina as the control group. If this control group is indeed contaminated with school districts with successful technology initiatives, then we would expect the corresponding impact estimates to be biased toward zero. The larger sample size offers a higher level of precision, which is especially helpful in the heterogeneity analyses.

4.2 Regression results for test scores

Our regression results show the potential for one-to-one technology programs to increase achievement in the short term but more so in the medium term. In Table 2, we present these results for achievement scores, which come from estimating Equation (1). We show results using neighboring counties as the control group as well as the rest of North Carolina. The regressions control for student demographics, time-varying school characteristics, and persistent differences across schools via school fixed effects, and they allow the laptop program to have a separate impact for each year. Standard errors are clustered at the school district level. The year 2008 is omitted, so program impacts are relative to 2008. Recall that 2008 is the last year before the Digital Conversion Initiative was implemented, though it was only partially implemented in 2009. In Figure 2, we plot the coefficients for Mooresville interacted with year using the estimates from the neighboring county control group; the dashed lines indicate the 95% confidence intervals for each estimate. The discussion below focuses on the estimates using the neighboring county control group. Note that the estimates using all observations from the state are generally similar but that the impacts of the laptop program are slightly smaller. This attenuation is consistent with some contamination in the control group.

Math scores in Mooresville are relatively similar in the period 2006-2009. While many of these coefficients are statistically different from each other due to the large sample size, the magnitudes of the coeffients are relatively small. However, Mooresville students trend away from nearby districts by 0.09 standard deviations in 2010, 0.10 standard deviations in 2011, 0.15 standard deviations in 2012, and 0.17 standard deviations in 2013. While the Digital Conversion Initiative had an economically significant impact on math scores in the short term, we find more substantial impacts in the medium term. As a point of comparison, the medium-term impacts are about one-third the size of the black-white test score gap. The medium-term impact is also similar to the effect of assigning every student in the district a teacher that is one standard deviation higher in quality (Chetty et al., 2014).

On reading tests, we find that Mooresville students score similarly, practically-speaking, in the years before the program was fully implemented. The program impacts are 0.07 standard deviations in 2010, 0.09 standard deviations in 2011, and 0.14 standard deviations in 2012. While these effect sizes are meaningful, they are not quite as large as the math impacts in the same years. In 2013 the impact of the laptop program fell to 0.07 standard deviations; the reason behind this reversal is not clear. The remaining coefficients in Table 2 have the expected signs and magnitudes.

A potential concern for the test score results is that there may be selection into which students are administered the standardized test. The data files provided by the NCERDC include records for students who have a missing EOG test score. Three percent of student-year records have a missing or invalid math score, while 3.6% have a missing or invalid reading score. We estimate Equation (1) with missing math score and missing reading score as the outcome and present the results in the first two columns of Appendix Table B.1. Some of the interactions of Mooresville with year are found to be statistically significant using neighboring counties as the control group. Relative to the base year 2008 and the neighboring county trend, Mooresville students are more likely to be missing a test score in 2010 and 2011 but equally likely in 2012 and 2013. Given the small rate of missing test scores overall and the mixed findings on missing test scores for Mooresville in Appendix Table B.1, we conclude that selection into taking the standardized tests is not a major concern for our interpretation of the results.

4.2.1 Heterogeneity

We explore whether the test score results vary by gender, ethnicity, economic disadvantage, prior performance, and grade. Most of the previous literature on technology interventions at home and at school finds no evidence of heterogeneity in impacts. Since the strongest effects of Mooresville's laptop program were found for math scores, we focus on this outcome. We plot estimates and 95% confidence intervals from triple-difference models in the various panels of Figure 3. While the plotted estimates come from models using neighboring counties as the control group, results from both control groups are reported in the tables in Appendix C.

Program impacts may vary by student characteristics, like gender and ethnicity. We estimate models that include triple interactions of Mooresville, year, and a selected student characteristic. In Panel A of Figure 3, we show that students do not benefit from the laptop program differentially by gender. The triple interactions of female, Mooresville, and year, are not statistically significant in 2011, 2012, or 2013. This evidence is consistent with other studies that find that home computers do not impact boys and girls differently, such as Fairlie (2016). However, in this instance, we find that both boys and girls experience positive impacts, but that one gender does not benefit more than the other.

We illustrate trends in impacts for the three largest ethnic groups (white, black, and Hispanic) in Panel B of Figure 3. The evidence on differential impacts by ethnicity is largely inconclusive. By the point estimates alone, we find larger program impacts for black and Hispanic students relative to whites in the years 2011 and 2012, and in 2013 only for Hispanics. However, the 95% confidence intervals mostly rule out statistically significant differences. When the rest of North Carolina is used as the control group, these differential impacts become significant. Another issue is the choice of base year: The triple interactions for the pre-initiative years of 2006 and 2007 suggest that the program impacts are sensitive to this choice and thus should be interpreted with further caution.

Next, we analyze heterogeneity in program impacts by student economic disadvantage. We have two rough indicators of this trait through the student's eligibility for free or reduced price lunch. A student is eligible for free lunch if his family's income is less than 130% of the poverty line, which was $30,615 for a family of four in 2013. A student is eligible for reduced lunch if his family's income is between 130% and 185% of the poverty line, or between $30,615 and $43,568 for a family of four in 2013. We present triple-difference results using these two measures of economic disadvantage in Panel C of Figure 3. Interestingly, these results indicate that there are nonlinearities by income in the impact of the laptop program. The impact for students eligible free lunch is not statistically different from zero in both the short and medium term. However, program impacts in the medium term are significantly higher for students eligible for reduced lunch. Without more information, it is difficult to speculate about the reasons behind this finding.

Finally, we examine whether program impacts vary by baseline student performance. One of the advertised benefits of technology programs that incorporate computer-adaptive technology is that lessons are more easily customizable. While teachers in traditional classrooms may be forced to teach to the middle of the ability distribution, technology may help students work at their own pace. If this is the case, we would expect the Digital Conversion

Initiative to benefit the upper and lower tails of the ability distribution more than the middle 50% of the distribution. In our analysis, we divide students by their place in Mooresville's math score distribution in the previous year. We present these triple-difference results in Panel D of Figure 3. The evidence on whether high- or low-ability students benefit more from the laptop program is mixed, and conclusions are sensitive to the choice of base year. Even if a technology intervention offers more in-class benefits to students at the ends of the distribution, it is possible that these students experience more distraction from technology at home so that the school and home impacts cancel each other out. Still, this evidence casts doubt on the claim that technology helps teachers teach to the entire distribution of students. Furthermore, if teachers or administrators are using technology to target students on the margin of passing state assessments, this evidence indicates that the targeting was not effective.[22]

Not shown in Figure 3 is heterogeneity results by grade, though estimates are given in Appendix Table C.5. While there are reasons to believe that program impacts might vary by grade,[23] we do not find a clear pattern by grade. This analysis is problematic because it is difficult to separate grade, year, and exposure length. For example, most 8th graders in Mooresville in 2013 had had laptops for 4-5 years, depending on their intermediate school. However, 5th graders had at most two years of laptop exposure, regardless of year, though their teachers may have had more exposure. Thus, the results are difficult to interpret.

4.2.2 Robustness

In this section, we explore whether the test score results are robust to a series of sample restrictions and alternative specifications. Again, we focus on math scores for these robustness checks.

[22] This targeting behavior would be consistent with Neal and Schanzenbach (2010).

[23] Students in certain grades, or their teachers, may be more effective at utilizing laptops for human capital enhancing activities. For example, customizable lessons may be more advantageous for students in lower grades, before they are traditionally tracked into math classes by ability. Alternatively, older students may be more likely to be distracted by social media and gaming sites.

Given that we study medium-term impacts, there is concern that the test and control groups may be contaminated through transferring behavior. For example, high-achieving families may be more attracted to the laptop program. If this is the case, we would expect that high-achieving families are (i) less likely to move if they already reside in Mooresville, (ii) more likely to move to Mooresville relative to surrounding areas, or (iii) seek to transfer into Mooresville if they live in a nearby district. The first group of families is difficult to identify; however, we can study the flow of students into Mooresville Graded School District and contrast it to districts in the neighboring county sample. The student identifier created by the NCERDC follows students as they change schools and school districts. Thus, we can analyze the proportion of students that are new to the North Carolina public school system and the proportion of students transferring from other districts within the state system. We present the results of this analysis in Appendix Table A.3.

We find that overall transfers into the Mooresville Graded School District declined with the introduction of the Digital Conversion Initiative. In 2006-2007, 12% of Mooresville students were new to the North Carolina public school system, and 7% were transfers from another NC school district. Shortly after implementation, 6% were new to NC schools, and 4% were new to Mooresville from another NC district. However, transfers into neighboring districts also declined in the same time period. Families may have been less mobile in the aftermath of the 2008 financial crisis. We compare the mean difference-in-differences of these transfer indicators and find that transferring behavior declined more in Mooresville than in neighboring counties. We also examine transfers between Mooresville and districts in the neighboring county sample. In each time period, a very small proportion of students in neighboring counties (less than 0.2%) had transferred from Mooresville. Before the implementation of the laptop program, 4.2% of Mooresville students were transfers from nearby districts. Shortly after implementation, a lower proportion were transfers from these districts, but the proportions returned to near their normal levels in the medium term. One potential explanation is that families are wary of moving to a district that is making significant structural changes. Once the Digital Conversion Initiative was advertised as a success, families' concerns were smoothed over.

Even though we find that the proportion of transfer students decreased in Mooresville, we still examine whether our regression results are robust to dropping transfer students. Table 3 contains our robustness results. The first column of estimates gives results for our baseline specification and sample (i.e., Equation (1) and neighboring county control group) as a point of comparison. The next two columns report estimates from samples that drop sets of transfer students. First, we condition on students attending the same school district in the previous year, or two years total. Second, we condition on attending the same school district for the past three years. Since the EOG files start at 3rd grade, 4th grade students are automatically dropped from this second sample. In both cases, results are very similar to estimation with the full sample. Thus, transferring behavior does not seem to contaminate our results.

Next, we check whether excluding charter schools affects our impact estimates. Equation (1) includes school fixed effects, so each charter school is allowed to have its own intercept. However, one of the purported benefits of charter schools is that they have the freedom to innovate, and if this is the case, charter schools may be on different trajectories than non-charter schools. Charter schools would not be appropriate to include in the control group because their environment is not "business as usual." In Table 3, we report results where the sample is restricted to non-charter schools. The results are almost identical to the baseline results, so charter schools do not contaminate the control group.

We also explore several alternative specifications to Equation (1) and report results for these in Table 3. In the first, we collapse the base year category to include 2006, 2007, and 2008, or all of the pre-intervention years. The estimates of the laptop program's impacts are very similar to the baseline estimates, which is not surprising since the interactions of Mooresville with pre-intervention years were close to zero.

Next, we include the student's math score from the previous year in the model. This addition alters the interpretation of the coefficients for Mooresville interacted with year. The interactions now tell us how much Mooresville students deviated from their expected test score growth in each year, relative to the base year and the control group. The interactions in the post-implementation years are all negative, though they are generally small in magnitude. Note that the interaction with Mooresville and 2007 is -0.078, so if 2007 was used as the base year, all of the other interactions would be positive. Specifically, the interactions for the years 2010-2013, when the program was fully implemented, would range from 0.048 to 0.064. While the value-added estimates in Table 3 may seem to contradict our conclusion that the laptop program caused test scores to rise at first glance, these results are sensitive to the choice of base year. We prefer the estimates without the lagged test score because the interpretation is more straightforward and policy relevant. Lagged test scores help the econometrician control for past inputs, but in our case, past inputs include past exposure to the laptop program. As a policy, a one-to-one laptop program is more likely to be implemented on a large scale, such as districtwide. In this light, it is more useful to know how outcomes changed in levels for the Mooresville school district relative to the counterfactual. Our preferred specification offers this interpretation.

Last, we show results that for a specification that includes student fixed effects and thus controls for unobserved, time-constant variation across students. Relative to the baseline specification, this one suggests larger program impacts: In the medium-term, the laptop program improves test scores by about a quarter of a standard deviation once we control for unobserved, student-level heterogeneity. However, the interactions of Mooresville with the pre-implementation years reveal that the parallel trends assumption is likely violated. Mooresville appears to be on an upward trajectory before the implementation of the laptop program. Thus, we interpret results from this specification with caution.

4.3 Results for behavioral outcomes

Information on student behavior may provide clues about the mechanisms through which the laptop program improved student achievement in the district. The NCERDC files contain data on student time use and days absent. The coverage of years for these behavioral variables is less comprehensive than the test score data. Specifically, time use data were not collected after 2011, and days absent was not reported after 2012. In contrast, the achievement results combine test score data for the years 2006-2013.

We first analyze whether the laptop program brought about changes to time spent using a computer at home for school work, time spent on homework, and time spent free reading. We present these results in Panels A, B, and C of Figure 4, and we report point estimates in Appendix Table D.1. We find that the laptop program did indeed increase the amount of time students spent using a computer at home for schoolwork. Relative to the base year of 2008, computer use increased by 9.4 days per month in the first full year of implementation. The mean reported computer use in the pre-implementation period (2006-2008) was 2.6 days per month, so computer use for students exposed to the laptop program increased more than threefold relative to neighboring districts. In the second year of full implementation, computer use further increased by 3.2 days. This additional increase could be due to teachers more fully integrating the laptops into their lesson plans and assigned homework. We take these results as evidence that the laptop program worked in the most basic sense of increasing student exposure to computers.

In contrast, there is little evidence that exposure to the laptop program substantially changed the time students spent working on homework or reading for leisure. Relative to the base year of 2008, there is no evidence that time spent on homework changed. Furthermore, these estimates are precise; the 95% confidence intervals rule out increases larger than 11 minutes per week. With a base year of 2007, we would find small, positive impacts on homework time, but with 2006, our conclusions would be the same. These results suggest that Mooresville students kept their total amount of homework time constant but substituted that time towards computer-based activities. For time spent free reading, we find that the laptop program caused a slight decrease of about 5 minutes per day in the short term. Relative to the mean from the pre-implementation period, this change represents a decrease of about one-tenth of a standard deviation. To the extent that free reading is more beneficial for reading achievement than math achievement, the decrease in time spent free reading could partly explain why the laptop program had a smaller impact on exposed students' reading scores.

While the time use survey was offered at the time of testing in all years, there is a higher rate of nonresponse in 2009-2011. We analyze whether Mooresville students had differentially higher nonresponse rates. Since nonresponse across time use outcomes is highly correlated (above 0.98 in all pairwise correlations), we define a single dummy variable that equals one when all time use outcomes are missing. We then estimate Equation (1) with this variable as the outcome and report the results in the last column of Appendix Table B.1. For the neighboring counties, time use survey nonresponse was 14-18 percentage points higher in 2009-2011 relative to 2008. Mooresville students also had higher nonresponse rates in those years relative to 2008, but by 2-6 percentage points less than the students in neighboring counties. The generally elevated level of time use nonresponse is concerning, but it is less of an issue in Mooresville. Still, we interpret the time use results with some caution.

Another mechanism through which the laptop program could improve student achievement is through a reduction in absenteeism. If students are more engaged at school, they are less likely to miss class. Aucejo and Romano (2016) show that reductions in absences lead to test score gains. The final column of Table D.1 reports point estimates for the impact of the laptop program on absences; Panel D of Figure 4 displays them graphically. We find some evidence that the laptop program reduced student absenteeism. The biggest reduction in absences occurred in 2009 and 2010, shortly after initial implementation; students on average missed 1.7 and 1.3 fewer days in those years, respectively. Absences in Mooresville were also lower in 2011 and 2012, but by a smaller margin. Using 2007 as a base years yields similar conclusions, but using 2006 does not. The initial reduction in absences could be due to temporary excitement at the prospect of using laptops at school. When laptops became a normalized part of school and district culture, students may have reverted to their usual patterns of absenteeism.

5 Conclusion

One-to-one computing is a promising avenue to bring about positive changes in student outcomes. However, the desired effects may not appear until a few years after the transition. This paper highlights the need to follow outcomes into the medium and long term. Of potential concern is the finding that test scores of the poorest students stayed the same; the benefits of the program went to students whose family incomes were more than 185% of the poverty line. Although technology programs are sometimes advertised as benefitting the tails of the ability distribution through customized lesson plans, we do not find conclusive evidence that students with prior scores in the upper and lower quartiles benefit more than the middle 50% of the distribution.

We find some evidence that the laptop program impacted student behavior. Although the total time spent on homework stayed the same, students exposed to the laptop program spent more of their homework time using a computer. The results also suggest that absences decreased with the laptop rollout. However, student reported spending less time reading for pleasure. This evidence suggests that changes to student behavior played a role in the laptop program's success. It is important to stress that this paper only analyzed a few of the many mechanisms through which the laptop program may have impacted test scores. Most notably, we did not consider a role for changes in teacher and parent behavior.[24] We leave these mechanisms for future work.

[24] Fairlie and Robinson (2013) analyze whether program impacts vary by several measures of parental involvement and supervision, and find no evidence that they do. However, they also find a null treatment effect overall. Taylor (2015) finds that the variance in teacher productivity, as measured by student test scores, decreases with the introduction of computer-aided instruction software, and that this reduction comes in part from changes in teacher effort level and decisions about how to allocate class time.

Also not considered here is a role for change in the district's culture. In writing about the Digital Conversion Initiative, the Mooresville Superintendent Mark Edwards emphasizes the concept of "second-order change." The first-order change was giving every student a laptop. Second-order change involves a deeper level of transformation. Edwards writes that digital conversion "supports second-order change by enabling a fundamental shift across all aspects of daily life in our schools. It affects instruction, pedagogy, professional development, student and teacher motivation, student-teacher roles, learning experiences, and relationships" (Edwards, 2013, p. 4). If culture change is indeed a key mechanism, the external validity of this study is called into question. We could not give a laptop to every student in a school district and expect the same changes in outcomes without also focusing on changing district culture.

Future work is also needed to determine whether one-to-one technology interventions are effective for students outside of grades 4-8. For example, students in lower grades may need to focus on skills not easily taught through computers. There may be more scope for a reduction in absenteeism in later grades. Compounding effects of the laptop program could increase high school graduation rates. If the laptop program sparks greater interest in technology, another effect could be greater interest in STEM majors and careers, such as computer engineering. Early computer-based interventions could help eliminate gaps in female and minority interest in STEM majors.

A final, important consideration is whether one-to-one technology programs can pass cost-benefit tests. While the test score increases found here are quite large, providing every student with a laptop or tablet is costly. The Mooresville Graded School District has received some outside funding, and a local broadband provider offers discounted rates to low-income families. The community also bears some costs by extending hours for city buildings so that students can have Wi-Fi access after school hours. Parents are charged a technology usage fee, although many school districts charge fees for textbooks and activities. However, the Mooresville district emphasizes that its operating budget covers 98% of the cost of the program. While some expenses increased, others, such as print textbooks and computer labs, were reduced or eliminated. Mooresville's experience suggests that other school districts can also adopt one-to-one technology programs by shifting spending. If these programs can be implemented in a cost-neutral way, even small test score gains provide enough benefit to justify the transition.

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Figure 1: Distribution of computer use

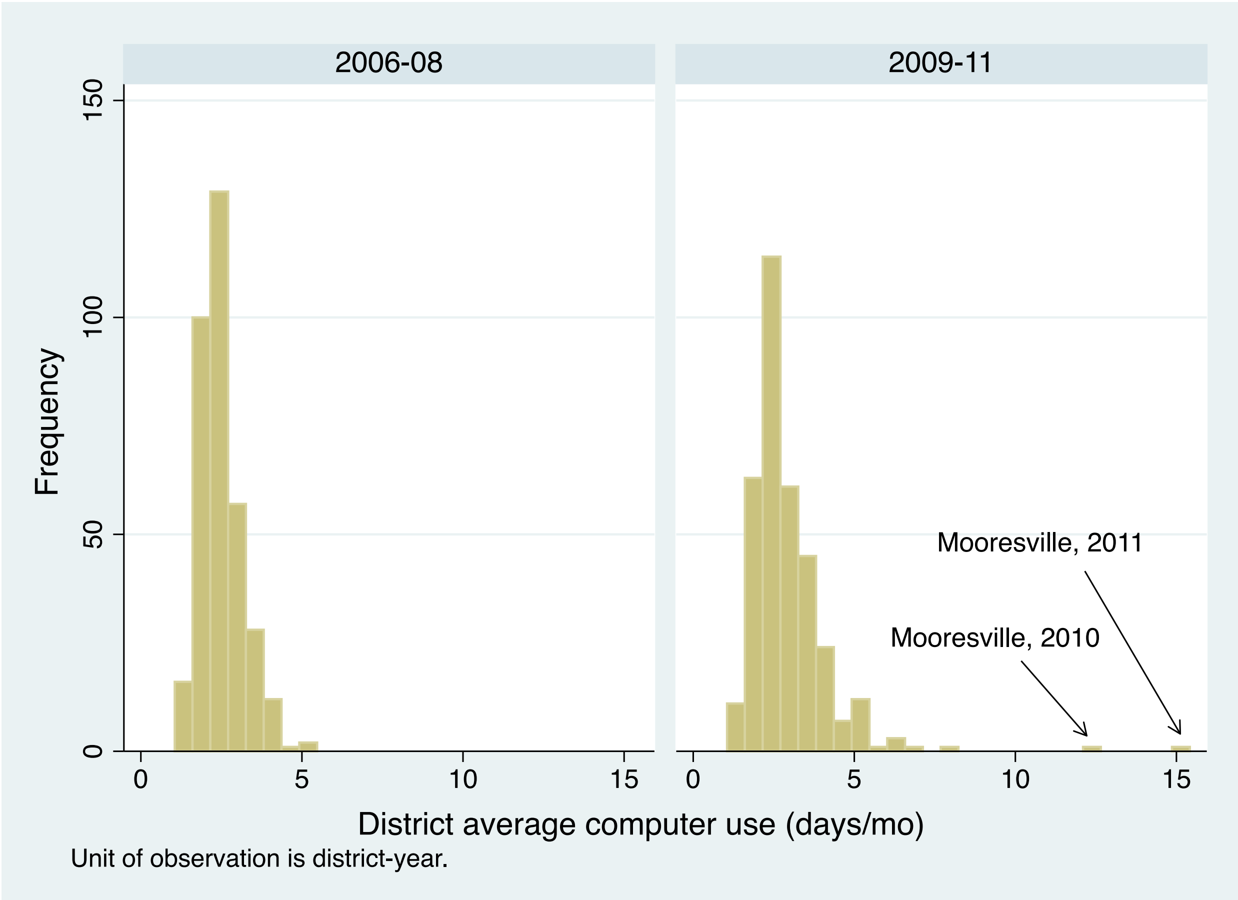
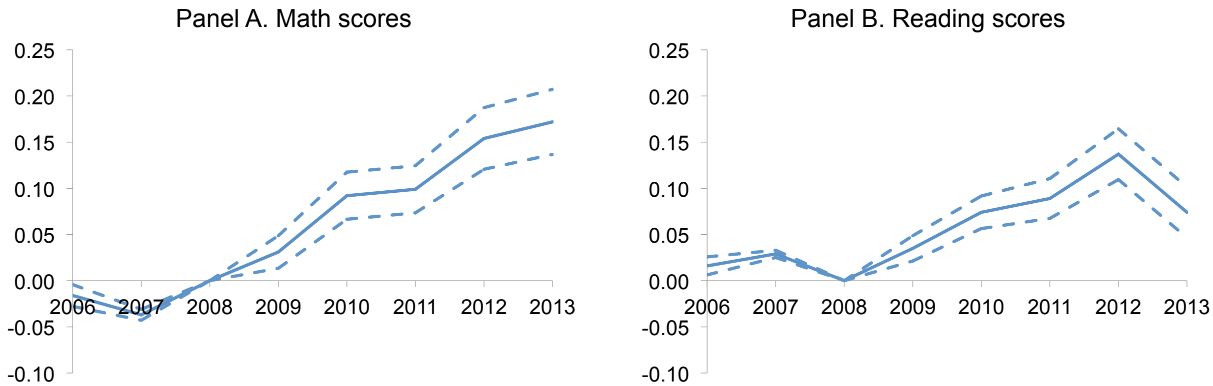
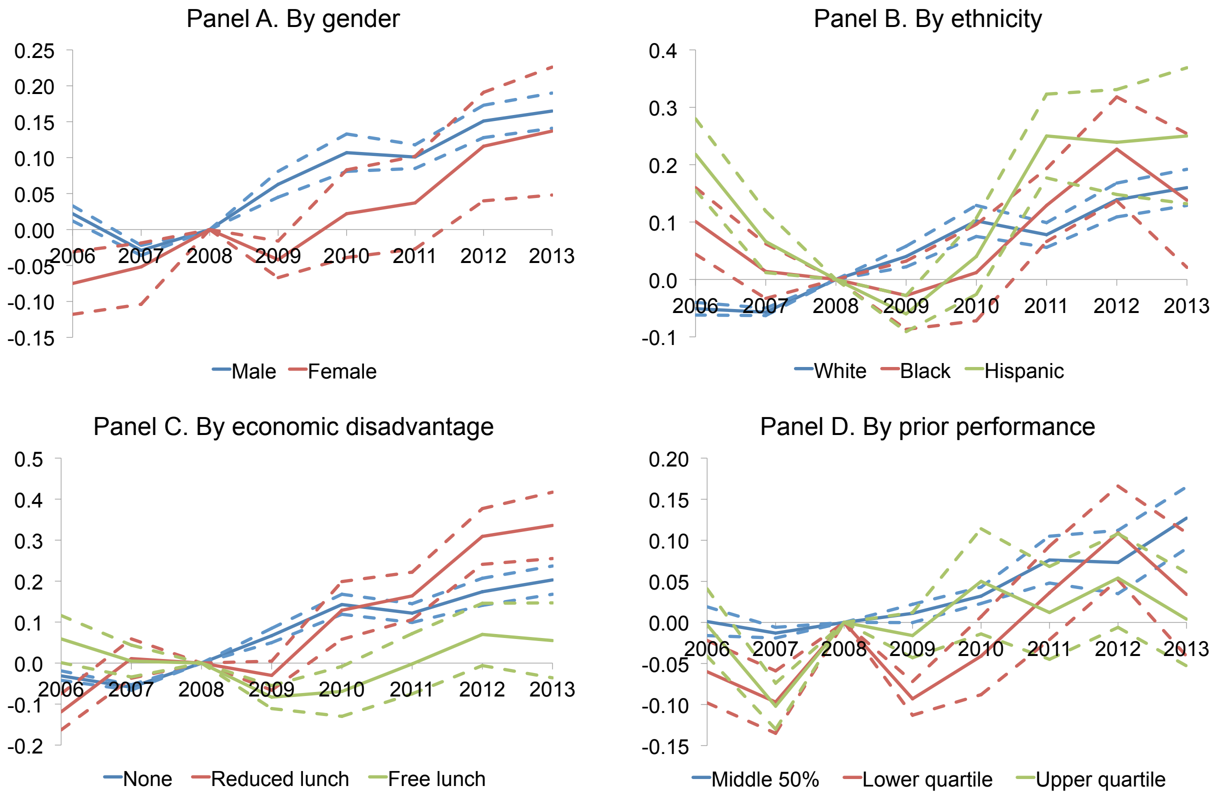


Figure 2: Difference-in-differences estimates for test scores



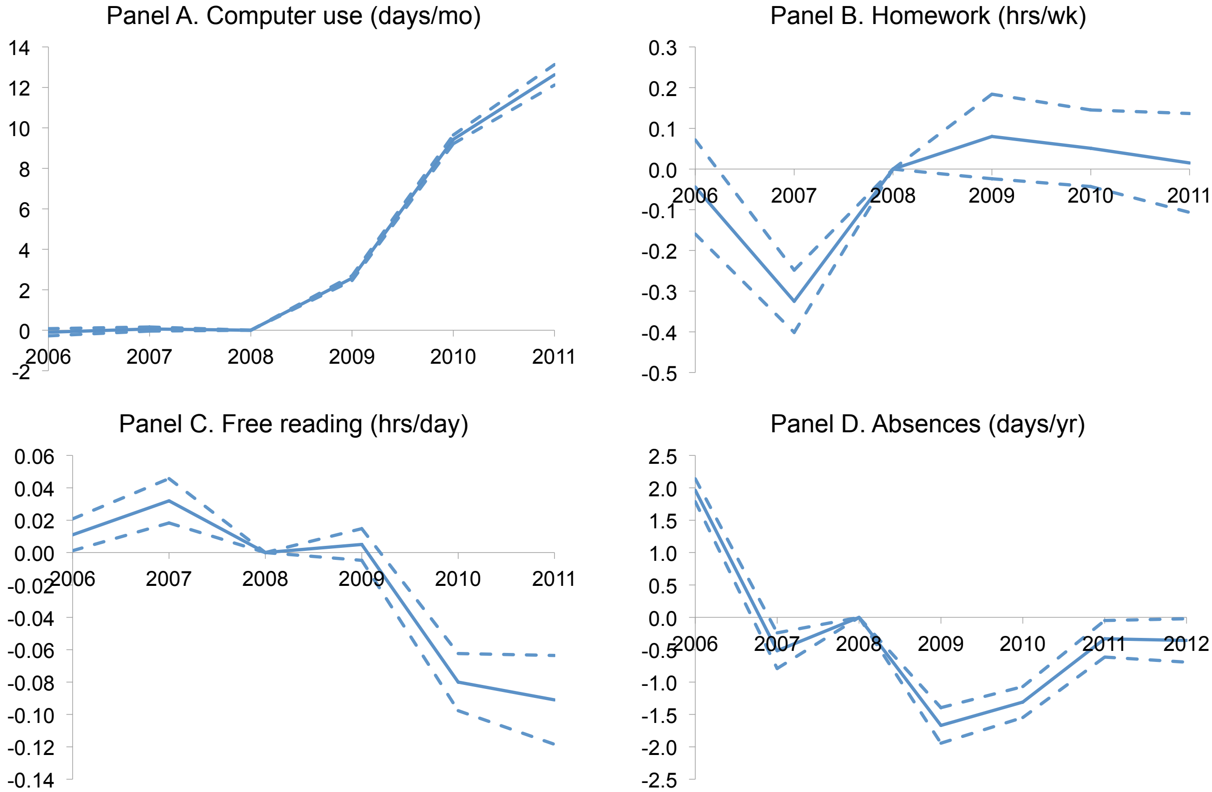
Notes: Estimates in Table 2. Sample of neighboring counties. Dashed lines give 95% confidence intervals.

Figure 3: Heterogeneity in difference-in-differences estimates for math scores



Notes: Estimates in Appendix Tables C.1, C.2, C.3, and C.4. Sample of neighboring counties. Dashed lines give 95% confidence intervals.

Figure 4: Difference-in-differences estimates for student behavior



Notes: Estimates in Appendix Table D.1. Dashed lines give 95% confidence intervals.

Table 1: Mean student characteristics

	Neighboring counties			Mooresville			Diff-in-diff	
	Pre	Short	Medium	Pre	Short	Medium	Short	Medium
Math score	0.062	0.050	0.032	0.210	0.293	0.356	0.095***	0.176***
	(0.003)	(0.003)	(0.003)	(0.015)	(0.015)	(0.014)	(0.022)	(0.021)
Reading score	0.051	0.049	0.023	0.195	0.216	0.234	0.021	0.066***
	(0.003)	(0.003)	(0.003)	(0.015)	(0.015)	(0.014)	(0.021)	(0.021)
Computer use (days/mo)	2.53	3.38		2.28	13.87		10.74***	
	(0.02)	(0.02)		(0.08)	(0.18)		(0.20)	
Homework time (hrs/wk)	2.46	2.60		2.38	2.66		0.14**	
	(0.01)	(0.01)		(0.04)	(0.04)		(0.06)	
Free reading (hrs/day)	0.762	0.797		0.768	0.692		-0.112***	
	(0.002)	(0.002)		(0.010)	(0.010)		(0.15)	
Days absent (days/yr)	6.91	6.55		7.55	5.74		-1.44***	
	(0.02)	(0.02)		(0.12)	(0.09)		(0.15)	
White	0.715	0.689	0.673	0.771	0.745	0.731	0.000	0.002
Black	0.139	0.136	0.137	0.153	0.144	0.145	-0.006	-0.006
Hispanic	0.087	0.111	0.128	0.038	0.059	0.075	-0.002	-0.004
Multiracial	0.030	0.036	0.033	0.020	0.032	0.033	0.006*	0.010***
Asian	0.027	0.026	0.026	0.017	0.017	0.014	0.001	-0.002
Am. Ind. or Pac. Is.	0.002	0.003	0.003	0.001	0.002	0.002	0.001	0.001
Female	0.485	0.489	0.487	0.499	0.479	0.493	-0.023**	-0.007
Age (mos)	150.2	149.2	149.4	150.2	148.6	149.1	-0.7*	-0.3
	(0.1)	(0.1)	(0.1)	(0.3)	(0.3)	(0.3)	(0.4)	(0.4)
Reduced lunch	0.077	0.074	0.070	0.078	0.067	0.073	-0.009	0.001
Free lunch	0.305	0.397	0.426	0.228	0.337	0.318	0.016	-0.031***
Max observations	93,095	98,779	100,190	3,799	4,272	4,631		

Notes: Standard errors for the mean in parentheses. Baseline years are 2006 and 2007; short term is 2010 and 2011; and medium term is 2012 and 2013. Difference-in-differences are relative to the base years.

Key: Difference-in-difference relative to base years significant at the * 10% level, ** 5% level, or *** 1% level.

Table 2: Difference-in-differences estimates for test scores

Outcome:	Math score		Reading score	
Control group:	Neighbor	Rest of NC	Neighbor	Rest of NC
Mooresville X 2006	-0.016	-0.018	0.016	0.015
	(0.006)	(0.002)	(0.005)	(0.002)
Mooresville X 2007	-0.037	-0.040	0.029	0.025
	(0.003)	(0.001)	(0.002)	(0.001)
Mooresville X 2009	0.031	0.019	0.035	0.026
	(0.009)	(0.002)	(0.007)	(0.002)
Mooresville X 2010	0.092	0.071	0.074	0.060
	(0.013)	(0.003)	(0.009)	(0.003)
Mooresville X 2011	0.099	0.098	0.089	0.087
	(0.013)	(0.004)	(0.011)	(0.004)
Mooresville X 2012	0.154	0.145	0.137	0.127
	(0.017)	(0.005)	(0.014)	(0.004)
Mooresville X 2013	0.172	0.157	0.074	0.058
	(0.018)	(0.005)	(0.014)	(0.005)
Black	-0.486	-0.504	-0.459	-0.492
	(0.014)	(0.021)	(0.014)	(0.016)
Hispanic	-0.202	-0.231	-0.335	-0.373
	(0.028)	(0.026)	(0.026)	(0.022)
Multiracial	-0.199	-0.179	-0.153	-0.150
	(0.014)	(0.011)	(0.016)	(0.009)
Asian	0.156	0.252	-0.194	-0.070
	(0.055)	(0.029)	(0.061)	(0.031)
Am. Ind. or Pac. Is.	-0.101	-0.292	-0.050	-0.305
	(0.026)	(0.022)	(0.031)	(0.027)
Missing ethnicity	-0.727	-0.207	-0.756	-0.211
	(0.019)	(0.035)	(0.044)	(0.035)
Female	-0.029	-0.015	0.109	0.110
	(0.008)	(0.004)	(0.005)	(0.004)
Age-for-grade (mos)	-0.029	-0.028	-0.025	-0.025
	(0.001)	(0.001)	(0.002)	(0.001)
Reduced lunch	-0.286	-0.253	-0.281	-0.261
	(0.013)	(0.015)	(0.011)	(0.014)
Free lunch	-0.452	-0.419	-0.443	-0.434
	(0.016)	(0.018)	(0.016)	(0.017)
Missing economic disadvantage	-0.422	-0.380	-0.291	-0.299
	(0.044)	(0.025)	(0.030)	(0.019)
% subsidized lunch	-0.025	0.023	0.012	0.040
	(0.034)	(0.013)	(0.028)	(0.011)
% black	-0.659	-0.369	-0.402	-0.274
	(0.334)	(0.078)	(0.239)	(0.063)

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Table 2 – continued from previous page

Outcome:	Math score		Reading score	
Control group:	Neighbor	Rest of NC	Neighbor	Rest of NC
% Hispanic	-0.558	-0.200	-0.602	-0.178
	(0.316)	(0.118)	(0.164)	(0.126)
% Asian	2.973	0.345	2.695	0.319
	(0.883)	(0.187)	(0.606)	(0.153)
% Am. Ind. of Pac. Is.	-0.197	-0.267	-1.130	0.016
	(2.326)	(0.245)	(1.348)	(0.263)
% two or more races	-0.034	-0.080	0.129	0.115
	(0.438)	(0.133)	(0.382)	(0.101)
Missing school characteristics	-0.135	-0.215	-0.214	-0.137
	(0.216)	(0.064)	(0.118)	(0.067)
Constant	0.420	0.472	0.313	0.391
	(0.089)	(0.043)	(0.045)	(0.040)
Observations	393,341	4,317,242	391,157	4,292,051

Notes: Standard errors are in parentheses and clustered at the school district level. Regressions also control for school fixed effects.

Table 3: Difference-in-differences estimates for math scores, alternative samples and specifications

Sample restriction:		Same district, 2 years	Same district, 3 years	No charters			
Specification:					More base years	Lagged score	Student FE
Mooresville X 2006	-0.016	-0.007	0.017	-0.017		0.008	-0.165
	(0.006)	(0.006)	(0.007)	(0.006)		(0.003)	(0.019)
Mooresville X 2007	-0.037	-0.023	-0.024	-0.037		-0.078	-0.122
	(0.003)	(0.003)	(0.003)	(0.003)		(0.001)	(0.010)
Mooresville X 2009	0.031	0.037	0.013	0.031	0.049	-0.055	0.042
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.005)	(0.009)
Mooresville X 2010	0.092	0.106	0.107	0.092	0.109	-0.014	0.098
	(0.013)	(0.014)	(0.014)	(0.013)	(0.013)	(0.006)	(0.015)
Mooresville X 2011	0.099	0.125	0.116	0.100	0.117	-0.030	0.148
	(0.013)	(0.013)	(0.012)	(0.013)	(0.012)	(0.005)	(0.021)
Mooresville X 2012	0.154	0.171	0.160	0.155	0.171	-0.024	0.228
	(0.017)	(0.017)	(0.015)	(0.017)	(0.016)	(0.007)	(0.028)
Mooresville X 2013	0.172	0.187	0.167	0.173	0.190	-0.025	0.254
	(0.018)	(0.018)	(0.016)	(0.018)	(0.017)	(0.008)	(0.033)
Observations	393,341	354,623	259,024	382,491	393,341	393,341	393,341

Notes: Standard errors are in parentheses and clustered at the school district level. Sample of neighboring counties. Unless otherwise noted, regressions also control for gender, ethnicity, age-for-grade, economic disadvantage, school ethnic composition, school proportion subsidized lunch, and school fixed effects.

A Extra descriptive tables

Table A.1: Conversion of time use categories to continuous values

Response[a]	Relative frequency	Assigned value
Student uses computer at home		*days/mo*
I use a computer at home for school work almost every day	0.061	24
Once or twice a week	0.187	6
Once or twice a month	0.172	1.5
Hardly ever	0.354	0.5
Never, even though there is a computer at home	0.148	0
There is no computer at home	0.078	0
Time on homework		*hrs/wk*
No homework is ever assigned	0.013	0
Less than one hour each week	0.311	0.5
Between 1 and 3 hours	0.399	2
More than 3 but less than 5 hours	0.159	4
Between 5 and 10 hours	0.093	7.5
More than 10 hours	0.016	12
Has homework, but does not do it	0.001	0
Amount of time spent free reading		*hrs/day*
None	0.138	0
About 30 minutes	0.491	0.5
About 1 hour	0.194	1
Between 1 and 2 hours	0.108	1.5
More than 2 hours	0.070	2.5

Notes: Sample of neighboring counties.
[a] Variable titles and labels as they appear in the codebook.

Table A.2: Mean student characteristics, state sample

	Rest of NC			Mooresville			Diff-in-diff	
	Pre	Short	Medium	Pre	Short	Medium	Short	Medium
Math score	-0.001	-0.001	-0.001	0.210	0.293	0.356	0.083***	0.147***
	(0.001)	(0.001)	(0.001)	(0.015)	(0.015)	(0.014)	(0.021)	(0.021)
Reading score	-0.001	-0.001	-0.001	0.195	0.216	0.234	0.021	0.039*
	(0.001)	(0.001)	(0.001)	(0.015)	(0.015)	(0.014)	(0.021)	(0.020)
Computer use (days/mo)	2.80	3.63		2.28	13.87		10.77***	
	(0.01)	(0.01)		(0.08)	(0.18)		(0.19)	
Homework time (hrs/wk)	2.35	2.43		2.38	2.66		0.20***	
	(0.00)	(0.00)		(0.04)	(0.04)		(0.06)	
Free reading (hrs/day)	0.776	0.816		0.768	0.692		-0.118***	
	(0.001)	(0.001)		(0.010)	(0.010)		(0.14)	
Days absent (days/yr)	7.61	6.96		7.55	5.74		-1.16***	
	(0.01)	(0.01)		(0.12)	(0.09)		(0.15)	
White	0.561	0.542	0.525	0.771	0.745	0.731	-0.006	-0.004
Black	0.288	0.256	0.261	0.153	0.144	0.145	0.024***	0.020**
Hispanic	0.085	0.126	0.136	0.038	0.059	0.075	-0.020***	-0.015***
Multiracial	0.029	0.038	0.037	0.020	0.032	0.033	0.003	0.006
Asian	0.022	0.022	0.026	0.017	0.017	0.014	0.000	-0.007**
Am. Ind. or Pac. Is.	0.015	0.017	0.016	0.001	0.002	0.002	-0.001	-0.000
Female	0.483	0.490	0.490	0.499	0.479	0.493	-0.027**	-0.013
Age (mos)	150.5	149.5	149.6	150.2	148.6	149.1	-0.6	-0.2
	(0.0)	(0.0)	(0.0)	(0.3)	(0.3)	(0.3)	(0.4)	(0.4)
Reduced lunch	0.083	0.071	0.068	0.078	0.067	0.073	0.001	0.010*
Free lunch	0.358	0.438	0.480	0.228	0.337	0.318	0.028***	-0.032**
Max observations	1,067,183	1,125,194	1,142,247	3,799	4,273	4,631		

Notes: Standard errors for the mean in parentheses. Baseline years are 2006 and 2007; short term is 2010 and 2011; and medium term is 2012 and 2013. Difference-in-differences are relative to the base years.

Key: Difference-in-difference relative to base years significant at the * 10% level, ** 5% level, or *** 1% level.

Table A.3: Mean student transfer behavior

	Neighboring counties			Mooresville			Diff-in-diff	
	Pre	Short	Medium	Pre	Short	Medium	Short	Medium
Transfer from outside NC public schools	0.071	0.051	0.038	0.115	0.061	0.061	-0.034***	-0.020***
	(0.001)	(0.001)	(0.001)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)
Transfer from inside NC public schools	0.053	0.041	0.046	0.072	0.040	0.062	-0.021***	-0.003
	(0.001)	(0.001)	(0.001)	(0.004)	(0.003)	(0.004)	(0.005)	(0.006)
Transfer from neighboring county				0.042	0.031	0.040		
				(0.003)	(0.003)	(0.003)		
Transfer from Mooresville	0.0017	0.0018	0.0014					
	(0.0001)	(0.0001)	(0.0001)					
Max observations	93,095	98,779	100,190	3,799	4,272	4,631		

Notes: Standard errors for the mean in parentheses. Baseline years are 2006 and 2007; short term is 2010 and 2011; and medium term is 2012 and 2013. Difference-in-differences are relative to the base years.

Key: Difference-in-difference relative to base years significant at the * 10% level, ** 5% level, or *** 1% level.

B Results for missing outcomes

Table B.1: Difference-in-differences estimates for missing outcomes

Outcome	Math score missing	Reading score missing	Time use missing
Mean	0.031	0.036	0.130
Mooresville X 2006	-0.006	-0.015	-0.002
	(0.003)	(0.003)	(0.009)
Mooresville X 2007	0.005	0.001	-0.013
	(0.002)	(0.002)	(0.006)
Mooresville X 2009	0.002	0.002	-0.036
	(0.003)	(0.003)	(0.010)
Mooresville X 2010	0.016	0.013	-0.056
	(0.004)	(0.004)	(0.014)
Mooresville X 2011	0.026	0.029	-0.023
	(0.004)	(0.004)	(0.020)
Mooresville X 2012	0.008	0.009	
	(0.004)	(0.004)	
Mooresville X 2013	-0.000	-0.003	
	(0.005)	(0.005)	
Black	0.012	0.011	0.058
	(0.004)	(0.003)	(0.004)
Hispanic	0.003	0.013	0.037
	(0.003)	(0.005)	(0.005)
Multiracial	-0.004	-0.004	0.020
	(0.003)	(0.003)	(0.004)
Asian	-0.008	0.001	0.005
	(0.004)	(0.004)	(0.006)
Am. Ind. or Pac. Is.	-0.006	-0.003	0.004
	(0.005)	(0.009)	(0.011)
Missing ethnicity	-0.009	-0.010	0.624
	(0.008)	(0.007)	(0.100)
Female	-0.010	-0.014	-0.016
	(0.001)	(0.002)	(0.002)
Age-for-grade (mos)	0.004	0.005	0.006
	(0.001)	(0.001)	(0.001)
Reduced lunch	0.011	0.012	0.038
	(0.002)	(0.002)	(0.003)
Free lunch	0.021	0.024	0.072
	(0.004)	(0.005)	(0.004)
Missing econ. disadvantage	0.077	0.071	0.088
	(0.010)	(0.011)	(0.012)

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Table B.1 – continued from previous page

Outcome	Math score missing	Reading score missing	Time use missing
% subsidized lunch	0.004	0.002	-0.044
	(0.007)	(0.007)	(0.030)
% black	-0.021	-0.027	-0.182
	(0.041)	(0.042)	(0.121)
% Hispanic	0.123	0.115	0.355
	(0.041)	(0.046)	(0.103)
% Asian	-0.119	-0.129	-0.709
	(0.068)	(0.066)	(0.468)
% Am. Ind. of Pac. Is.	-0.002	0.127	-0.485
	(0.139)	(0.167)	(1.201)
% two or more races	0.004	0.012	-0.043
	(0.022)	(0.022)	(0.136)
Missing school characteristics	0.044	0.032	0.000
	(0.018)	(0.020)	(.)
Grade 5	0.003	0.002	0.018
	(0.001)	(0.001)	(0.002)
Grade 6	0.007	0.003	-0.019
	(0.002)	(0.002)	(0.006)
Grade 7	0.008	0.004	-0.003
	(0.002)	(0.002)	(0.008)
Grade 8	0.007	0.003	-0.003
	(0.002)	(0.002)	(0.007)
2006	-0.033	-0.031	-0.038
	(0.003)	(0.003)	(0.010)
2007	-0.001	-0.000	-0.023
	(0.002)	(0.002)	(0.005)
2009	0.005	0.006	0.183
	(0.004)	(0.004)	(0.013)
2010	-0.008	-0.003	0.166
	(0.004)	(0.004)	(0.018)
2011	-0.012	-0.008	0.143
	(0.004)	(0.004)	(0.024)
2012	-0.014	-0.009	
	(0.004)	(0.004)	
2013	-0.015	-0.010	
	(0.004)	(0.004)	
Constant	0.023	0.030	0.056
	(0.008)	(0.008)	(0.020)
Observations	405,801	405,801	301,024

Notes: Standard errors in parentheses and clustered at the school district level. Sample of neighboring counties. Regressions also control for school fixed effects.

C Regression results tables for heterogeneity analysis

Table C.1: Triple-difference estimates for math scores by gender

Control group:	Neighbor		Rest of NC	
Mooresville X 2006	0.022	(0.005)	0.020	(0.002)
Mooresville X 2007	-0.029	(0.003)	-0.033	(0.001)
Mooresville X 2009	0.063	(0.009)	0.046	(0.002)
Mooresville X 2010	0.107	(0.012)	0.080	(0.003)
Mooresville X 2011	0.101	(0.008)	0.096	(0.004)
Mooresville X 2012	0.151	(0.011)	0.137	(0.005)
Mooresville X 2013	0.165	(0.012)	0.145	(0.005)
Mooresville X female	0.085	(0.019)	0.040	(0.006)
Mooresville X 2006 X female	-0.097	(0.021)	-0.096	(0.010)
Mooresville X 2007 X female	-0.024	(0.016)	-0.011	(0.005)
Mooresville X 2009 X female	-0.105	(0.007)	-0.058	(0.006)
Mooresville X 2010 X female	-0.085	(0.020)	-0.033	(0.011)
Mooresville X 2011 X female	-0.064	(0.025)	-0.005	(0.010)
Mooresville X 2012 X female	-0.035	(0.027)	-0.009	(0.007)
Mooresville X 2013 X female	-0.028	(0.034)	0.012	(0.010)
Observations	393,341		4,317,242	

Notes: Standard errors in parentheses and clustered at the school district level. Regressions also control for ethnicity, age-for-grade, economic disadvantage, school ethnic composition, school proportion subsidized lunch, and school fixed effects. Not reported are the double interactions of gender with year.

Table C.2: Triple-difference estimates for math scores by ethnicity

Control group:	Neighbor		Rest of NC	
Mooresville X 2006	-0.051	(0.005)	-0.055	(0.002)
Mooresville X 2007	-0.057	(0.003)	-0.062	(0.001)
Mooresville X 2009	0.040	(0.008)	0.025	(0.002)
Mooresville X 2010	0.102	(0.013)	0.079	(0.003)
Mooresville X 2011	0.078	(0.010)	0.084	(0.003)
Mooresville X 2012	0.139	(0.014)	0.137	(0.004)
Mooresville X 2013	0.160	(0.015)	0.151	(0.004)
Mooresville X black	-0.051	(0.031)	-0.055	(0.033)
Mooresville X 2006 X black	0.153	(0.029)	0.130	(0.014)
Mooresville X 2007 X black	0.071	(0.021)	0.066	(0.007)
Mooresville X 2009 X black	-0.068	(0.022)	-0.008	(0.008)
Mooresville X 2010 X black	-0.090	(0.029)	-0.069	(0.016)
Mooresville X 2011 X black	0.052	(0.024)	0.048	(0.021)
Mooresville X 2012 X black	0.089	(0.032)	0.057	(0.013)
Mooresville X 2013 X black	-0.023	(0.046)	-0.009	(0.015)
Mooresville X Hispanic	-0.115	(0.050)	-0.113	(0.038)
Mooresville X 2006 X Hispanic	0.268	(0.031)	0.299	(0.016)
Mooresville X 2007 X Hispanic	0.123	(0.024)	0.157	(0.009)
Mooresville X 2009 X Hispanic	-0.100	(0.010)	-0.067	(0.012)
Mooresville X 2010 X Hispanic	-0.062	(0.025)	-0.029	(0.019)
Mooresville X 2011 X Hispanic	0.172	(0.029)	0.172	(0.016)
Mooresville X 2012 X Hispanic	0.101	(0.033)	0.075	(0.017)
Mooresville X 2013 X Hispanic	0.090	(0.047)	0.091	(0.017)
Mooresville X multiracial	0.023	(0.023)	-0.022	(0.021)
Mooresville X 2006 X multiracial	-0.184	(0.037)	-0.178	(0.015)
Mooresville X 2007 X multiracial	0.091	(0.043)	0.099	(0.008)
Mooresville X 2009 X multiracial	-0.118	(0.022)	-0.075	(0.008)
Mooresville X 2010 X multiracial	-0.130	(0.018)	-0.087	(0.013)
Mooresville X 2011 X multiracial	-0.118	(0.030)	-0.103	(0.017)
Mooresville X 2012 X multiracial	-0.132	(0.023)	-0.135	(0.014)
Mooresville X 2013 X multiracial	-0.153	(0.032)	-0.149	(0.016)
Mooresville X Asian	0.120	(0.062)	0.004	(0.030)
Mooresville X 2006 X Asian	0.123	(0.028)	0.129	(0.027)
Mooresville X 2007 X Asian	0.026	(0.023)	0.051	(0.012)
Mooresville X 2009 X Asian	-0.103	(0.019)	-0.054	(0.012)
Mooresville X 2010 X Asian	0.007	(0.039)	0.055	(0.016)
Mooresville X 2011 X Asian	-0.097	(0.032)	-0.032	(0.034)
Mooresville X 2012 X Asian	-0.102	(0.051)	-0.114	(0.020)
Mooresville X 2013 X Asian	0.291	(0.068)	0.252	(0.027)
Observations	393,341		4,317,242	

Notes: Standard errors in parentheses and clustered at the school district level. Regressions also control for gender, age-for-grade, economic disadvantage, school ethnic composition, school proportion subsidized lunch, and school fixed effects. Not reported are the double interactions of ethnicity with year and all sets of interactions for the group American Indian or Pacific Islander.

Table C.3: Triple-difference estimates for math scores by economic disadvantage

Control group:	Neighbor		Rest of NC	
Mooresville X 2006	-0.031	(0.005)	-0.035	(0.002)
Mooresville X 2007	-0.059	(0.003)	-0.064	(0.001)
Mooresville X 2009	0.067	(0.008)	0.052	(0.002)
Mooresville X 2010	0.143	(0.012)	0.121	(0.002)
Mooresville X 2011	0.122	(0.011)	0.119	(0.005)
Mooresville X 2012	0.174	(0.015)	0.162	(0.006)
Mooresville X 2013	0.203	(0.016)	0.183	(0.006)
Mooresville X reduced lunch	0.014	(0.021)	-0.047	(0.019)
Mooresville X 2006 X reduced lunch	-0.089	(0.021)	-0.092	(0.012)
Mooresville X 2007 X reduced lunch	0.070	(0.021)	0.092	(0.006)
Mooresville X 2009 X reduced lunch	-0.098	(0.018)	-0.035	(0.008)
Mooresville X 2010 X reduced lunch	-0.014	(0.027)	0.033	(0.013)
Mooresville X 2011 X reduced lunch	0.042	(0.021)	0.111	(0.020)
Mooresville X 2012 X reduced lunch	0.135	(0.021)	0.163	(0.015)
Mooresville X 2013 X reduced lunch	0.133	(0.034)	0.150	(0.014)
Mooresville X free lunch	0.039	(0.030)	-0.013	(0.025)
Mooresville X 2006 X free lunch	0.089	(0.026)	0.099	(0.009)
Mooresville X 2007 X free lunch	0.064	(0.017)	0.080	(0.005)
Mooresville X 2009 X free lunch	-0.150	(0.008)	-0.112	(0.007)
Mooresville X 2010 X free lunch	-0.212	(0.019)	-0.167	(0.011)
Mooresville X 2011 X free lunch	-0.124	(0.027)	-0.064	(0.016)
Mooresville X 2012 X free lunch	-0.104	(0.022)	-0.096	(0.009)
Mooresville X 2013 X free lunch	-0.147	(0.033)	-0.118	(0.011)
Observations	393,341		4,317,242	

Notes: Standard errors in parentheses and clustered at the school district level. Regressions also control for gender, ethnicity, age-for-grade, school ethnic composition, school proportion subsidized lunch, and school fixed effects. Not reported are the double interactions of economic disadvantage with year.

Table C.4: Triple-difference estimates for math scores by prior performance

Control group:	Neighbor		Rest of NC	
Mooresville X 2006	0.001	(0.008)	-0.003	(0.002)
Mooresville X 2007	-0.013	(0.003)	-0.016	(0.001)
Mooresville X 2009	0.011	(0.005)	0.003	(0.001)
Mooresville X 2010	0.033	(0.005)	0.022	(0.002)
Mooresville X 2011	0.076	(0.013)	0.070	(0.004)
Mooresville X 2012	0.073	(0.018)	0.060	(0.005)
Mooresville X 2013	0.127	(0.018)	0.110	(0.005)
Mooresville X 2006 X low prior score	-0.061	(0.015)	-0.066	(0.010)
Mooresville X 2007 X low prior score	-0.085	(0.016)	-0.073	(0.006)
Mooresville X 2009 X low prior score	-0.104	(0.011)	-0.055	(0.007)
Mooresville X 2010 X low prior score	-0.074	(0.020)	-0.037	(0.010)
Mooresville X 2011 X low prior score	-0.040	(0.018)	0.006	(0.010)
Mooresville X 2012 X low prior score	0.036	(0.013)	0.037	(0.011)
Mooresville X 2013 X low prior score	-0.093	(0.025)	-0.082	(0.012)
Mooresville X low prior score	0.097	(0.019)	0.074	(0.011)
Mooresville X 2006 X high prior score	-0.004	(0.016)	0.014	(0.009)
Mooresville X 2007 X high prior score	-0.089	(0.013)	-0.085	(0.004)
Mooresville X 2009 X high prior score	-0.027	(0.012)	-0.004	(0.005)
Mooresville X 2010 X high prior score	0.017	(0.026)	0.037	(0.007)
Mooresville X 2011 X high prior score	-0.065	(0.017)	-0.060	(0.013)
Mooresville X 2012 X high prior score	-0.019	(0.013)	0.010	(0.009)
Mooresville X 2013 X high prior score	-0.123	(0.018)	-0.084	(0.008)
Mooresville X high prior score	0.051	(0.016)	0.046	(0.007)
Observations	393,341		4,317,242	

Notes: Standard errors in parentheses and clustered at the school district level. Regressions also control for gender, ethnicity, age-for-grade, economic disadvantage, school ethnic composition, school proportion subsidized lunch, and school fixed effects. Not reported are the double interactions of prior performance with year.

Table C.5: Difference-in-differences estimates for math scores by grade

Grade:	4th	5th	6th	7th	8th
Mooresville X 2006	-0.098	-0.125	-0.080	0.081	0.088
	(0.007)	(0.007)	(0.009)	(0.009)	(0.014)
Mooresville X 2007	-0.146	-0.023	-0.143	-0.018	0.118
	(0.004)	(0.007)	(0.006)	(0.007)	(0.007)
Mooresville X 2009	0.030	0.122	-0.048	0.019	-0.029
	(0.011)	(0.013)	(0.020)	(0.011)	(0.007)
Mooresville X 2010	-0.003	0.167	0.106	0.056	0.060
	(0.014)	(0.016)	(0.026)	(0.015)	(0.009)
Mooresville X 2011	0.083	0.215	0.228	-0.019	-0.095
	(0.020)	(0.015)	(0.021)	(0.021)	(0.020)
Mooresville X 2012	0.147	0.211	0.236	0.099	0.036
	(0.027)	(0.018)	(0.024)	(0.018)	(0.018)
Mooresville X 2013	0.168	0.064	0.177	0.163	0.251
	(0.027)	(0.019)	(0.025)	(0.017)	(0.015)
Observations	78,727	78,419	78,783	78,688	78,724

Notes: Standard errors are in parentheses and clustered at the school district level. Sample of neighboring counties. Regressions also control for ethnicity, gender, age-for-grade, economic disadvantage, school ethnic composition, school proportion subsidized lunch, and school fixed effects.

D Regression results table for student behavior

Table D.1: Difference-in-differences estimates for time use and absences

Outcome	Computer use (days/mo)	Homework (hrs/wk)	Free reading (hrs/day)	Days absent (days/yr)
Mooresville X 2006	-0.099	-0.044	0.011	1.963
	(0.091)	(0.059)	(0.005)	(0.090)
Mooresville X 2007	0.062	-0.325	0.032	-0.515
	(0.051)	(0.039)	(0.007)	(0.140)
Mooresville X 2009	2.574	0.080	0.005	-1.669
	(0.060)	(0.053)	(0.005)	(0.140)
Mooresville X 2010	9.431	0.051	-0.080	-1.308
	(0.110)	(0.048)	(0.009)	(0.122)
Mooresville X 2011	12.625	0.015	-0.091	-0.331
	(0.258)	(0.062)	(0.014)	(0.144)
Mooresville X 2012				-0.357
				(0.171)
Observations	261,494	261,789	261,345	351,282

Notes: Standard errors are in parentheses and clustered at the school district level. Sample of neighboring counties. Regressions also control for ethnicity, gender, age-for-grade, economic disadvantage, school ethnic composition, school proportion subsidized lunch, grade fixed effects, year fixed effects, and school fixed effects.