



Technology in the classroom: Personal computers and learning outcomes in primary school

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ABSTRACT

The shift to remote teaching during the COVID-19 pandemic accelerated the use of digital technology in education. Many schools today provide personal computers not only to older students, but also in primary school. There is little credible evidence of the effects of one-to-one (1:1) computer programs among younger students. We investigate how 1:1 technology impacts student performance in primary school in Sweden. Using an event study design, and data from an expansion that took place before the pandemic, we examine effects of 1:1 technology on national standardized test results in math, Swedish, and English in grade 6. We find no important effects on student performance on average.

1. Introduction

During the COVID-19 pandemic, most governments closed schools and initiated remote teaching to counter the spread of the virus. Students of different ages faced a new form of teaching where information and communication technology (ICT) became important. In economically advanced societies, most schools had already integrated ICT in education to some degree, but the pandemic accelerated this development: digital technology became used to a greater extent, partly in a new way, and in the teaching of younger children. One-to-one programs (or 1:1 programs), where each student receives a personal computer from their school, became increasingly prevalent, also among younger pupils.¹ The pandemic may well prove to be a turning point that changes the way and the extent to which computers are used in teaching. Given this development, research on how the use of digital technology impacts learning among younger pupils is urgent: Should schools continue the path of providing personal computers to pupils in lower grades, or would children's learning benefit if technology played a smaller role in teaching after the pandemic?

In theory, the effects of schools' investments in digital technology on

student performance are ambiguous (Bulman and Fairlie, 2016). On the one hand, digital technology may imply opportunities to innovate teaching. For instance, specific computer software can make it easier to tailor teaching to individual students' needs and increase their motivation. Computers can also be used to access information from internet sources, complete assignments more efficiently, and they facilitate communication and feedback. On the other hand, using financial resources to invest in technology will come at the expense of other inputs that can also affect learning. For instance, it is not clear whether investing in computers is more beneficial than employing more teachers or teacher assistants. It is also not clear if teaching methods based on digital technology generally enhance learning compared to more traditional teaching methods, and there are downsides associated with ICT in education to consider, such as increased elements of distraction (e.g., through games, videos, and social media).²

There is a small but growing literature that provides evidence on the causal impacts of 1:1 initiatives on educational performance (de Melo et al., 2014; Hull and Duch, 2019; Hall et al., 2021a; Mora et al., 2018; Yanguas, 2020). The findings are mixed, but most analyses find no effect on learning outcomes.³ Few studies so far focus specifically on primary

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¹ In a survey with a representative sample of school district leaders in the U.S., 42 % reported that the schools in their district supplied one device per student in elementary school at the onset of the pandemic. One year later, this share was 84 % (EdWeek Research Center, 2021).

² Extensive discussions of benefits and downsides of digital technology in education are provided in Bulman and Fairlie (2016), Haelermans (2017), Hall et al. (2021a), Islam and Grönlund (2016), Hull and Duch (2019), Zheng et al. (2016).

³ There are also studies based on less credible strategies to isolate causal effects; see Islam and Grönlund (2016) and Zheng et al. (2016) for a discussion.

school students. An exception is de Melo et al. (2014) investigating the impact of a national implementation of 1:1 in Uruguay, finding no effects on student performance in math or reading.⁴⁵

Worldwide, schools have generally introduced 1:1 technology to a greater extent in higher grades than in lower grades. However, the question of whether 1:1 technology works better for older or younger pupils has not been examined. Teaching a 10-year-old pupil is different from teaching a 15-year-old pupil, and the impact of teaching methods and learning strategies may vary depending on student age (Brod, 2021). Previous research has shown that digital literacy increases with age (Jin et al., 2020; Lazonder et al., 2020). With increasing age and more complex tasks, technology also has the potential of making more of a difference. For example, when students work with longer and more complicated texts, word processing software and access to information on the internet is likely to be more useful. On the other hand, computer assisted learning could also be valuable for repetitive tasks, which may be especially important in lower grades (e.g., practicing name geography, spelling, or multiplication). In addition, it might be easier for teachers in lower grades to make sure that the computers are used for the intended purpose. Ultimately, it is an empirical question whether the effect of 1:1 technology differs for younger compared to older students.

In the present article, we study how 1:1 programs impact student performance in primary school (grades 4–6) in Sweden, using data from the period before the pandemic. We examine how students who are given a personal laptop or tablet, in comparison to having more limited computer access, are affected in terms of performance on standardized tests in language and math in 6th grade. Note that the question posed is not about using vs. not using computers in education, but rather about more intensive use of digital technology compared to more restricted use. We have surveyed all primary schools in 26 municipalities regarding their implementation of 1:1 technology during 2009–2020 and linked this information to administrative data on students' characteristics and performance on standardized tests. To identify a causal relationship, we exploit the staggered implementation of 1:1 programs and the fact that many schools never launched such an initiative during our study period. We compare how student performance changes across student cohorts in schools that introduce 1:1 programs to changes for schools that do not launch such programs in an event study analysis, and we follow the recent methodological literature (e.g., Cengiz et al., 2019) that removes earlier treated units as controls.

Hall et al. (2021a) examine the effects of 1:1 technology in lower secondary school on test scores in 9th grade in the same Swedish municipalities, during a similar period, and using a similar methodology as we do in this article. This enables a comparison of impacts between primary school and lower secondary school. Our study's findings echo those of Hall et al. (2021a): we find no evidence suggesting that 1:1

⁴ Cristia et al. (2017) also examine the impacts of 1:1 in primary school. However, the context (poor parts of Peru) is very different from the context of most economically advanced countries today and almost none of the schools included in the study had access to the internet. Crista et al. find no impact on test scores in math and language, but positive effects on pupils' general cognitive skills.

⁵ Besides the literature on 1:1 technology, there are studies on the effects of other types of ICT initiatives. For instance, the impact of free internet access, computer-assisted homework, subsidies for ICT investments in schools or certain educational computer softwares; see, e.g., Bettinger et al. (2022), Malamud et al. (2019), Goolsbee and Guryan (2006), Banerjee et al. (2007) and Roschelle et al. (2016). Haelermans (2017) and Escueta et al. (2020) provide overviews. They conclude that the findings are mixed, but that investments in ICT that merely improves access to technology without a distinct educational purpose often have limited effects on learning. Interventions where technology is integrated in teaching in a more structured way with a clear aim have yielded more promising results. However, it is uncertain whether the results from this literature can be generalized to 1:1 technology as the initiatives studied generally implies a much less extensive use of ICT in the classroom.

technology, on average, affects student performance in math, Swedish or English.

We also undertake an exploratory analysis to assess the heterogeneity of effects along two key dimensions: Firstly, it is often argued that investments in digital technology may foster greater equality in student performance across socio-economic backgrounds, by ensuring that all students have access to computers (e.g. Swedish Government, 2017; Zheng et al., 2016). However, many of the benefits of using technology in teaching are potentially valuable for all students. Consequently, it is not immediately apparent that 1:1 investments should reduce the educational performance gap related to socio-economic background. Secondly, the impact of 1:1 programs can be expected to be more positive if schools receive extra financial resources for investing in 1:1, since they then do not have to cut down on other expenses to the same extent (Bulman and Fairlie, 2016). While acknowledging the limitations of our data for studying heterogeneity along these two dimensions, especially concerning funding, our analysis yields some initial insights: Our results do not indicate that investments in 1:1 technology reduce the performance gap between students from different socio-economic backgrounds. Moreover, we find no clear evidence of more positive outcomes if schools receive financial support when implementing 1:1 programs.

2. The Swedish case

In Sweden, children begin school (grade 0) in the fall semester of the year they turn six, and schooling is compulsory throughout 9th grade. All schools adhere to a national curriculum, but the organization of schools is decentralized to the municipal level. Traditionally, compulsory school was divided into three stages: grades 0–3, grades 4–6 and grades 7–9, and schools were often organized as primary schools (grades 0–6) and lower secondary schools (grades 7–9). Today, other grade configurations are relatively common: schools are sometimes organized as grade 0–3 and grade 4–9 schools, or as grade 0–5 and grade 6–9 schools. There are also schools that offer all ten grades. After 9th grade, nearly all students continue to upper secondary education.

There are both public and 'independent' (but publicly funded) schools. Around 85 % of the children in compulsory school attended a public school in the school year 2016/2017 (NAE, 2017). Families can choose any school for their children, but since the admission rules to public schools are based on proximity, the vast majority attends the nearest public school (Böhlmark et al., 2016). Independent schools can also base admission on a first-come-first-served basis, but not on ability or other personal characteristics. They are also not allowed to charge a tuition fee if they want to receive public funding.

Local income taxes and central government grants constitute the schools' main sources of finance. Each school has its own budget, and the decision to invest in 1:1 technology is therefore often made at the school level. However, municipal initiatives where resources are earmarked for 1:1 programs in some or all public schools in the municipality are also common (NAE, 2020).

Sweden belongs to the group of countries in the world with the highest level of digitalization of the school system (European Commission, 2019; OECD, 2021). The share of pupils in grades 4–6 receiving a personal computer from school was around 50 % in 2018 (Hall et al., 2021b), and all schools have the duty of integrating ICT into their teaching to strengthen students' digital competence (NAE, 2018). There is good access to student computers even in schools without 1:1 programs (NAE, 2019), and almost all children have access to computers and the internet at home (Swedish Media Council, 2015). Hence, it is important to underscore that 1:1 technology is not a question of using or not using computers in education in the Swedish context. However, various studies have shown that schools with 1:1 technology use ICT in teaching much more than schools without 1:1 (Hall et al., 2021b);

Lindqvist, 2015; NAE, 2016).

The Swedish National Agency for Education (NAE) conducts surveys with representative samples of pupils every three years.⁶ In the online appendix, we use these data to illustrate differences between schools with and without 1:1 technology. Figure A2 and Figure A3 show that pupils in 1:1 schools use computers for schoolwork much more than pupils in other schools. Figure A3 also illustrates that computers are used for a variety of tasks, but the most common activity is working with texts (in Swedish). Searching for information on the internet, preparing presentations and practicing skills through educational computer games are other common tasks.⁷

3. Data

We have collected data on the presence of 1:1 technology from all schools with grades 4–6 in 26 (out of 290) Swedish municipalities. In the online appendix, we describe in detail how we sampled municipalities and collected data. A variety of municipalities, in terms of geographic location, population size, and average education level, were included in the sample. We contacted all schools in these municipalities by email. Non-responders were reminded by email and phone calls. A first contact was made in 2016. At this point, schools were asked about investments in 1:1 between 2009 and 2016. In 2020, we conducted a follow-up survey and asked the same questions for the period 2016–2020; only schools that responded the first time were approached this second time. The initial number of schools contacted was 410. In 2016, 293 of these schools responded. In 2020, 196 schools responded. This means that we can follow around 70 % of the schools in the selected municipalities from 2009 to 2016, and approximately 50 % for the whole period 2009–2020.

Due to our sampling method, our data cannot be generalized to depict the prevalence of 1:1 programs across Sweden over time. However, note that this limitation does not affect the internal validity of our effect estimates. Moreover, our analysis sample of students is rather similar to the full population of same-age students in terms of background variables (see Table A2).

We asked the schools about the presence of 1:1 technology and which grades were included in the initiative at different points in time. In the follow-up survey in 2020, we also asked if the schools had received any additional funding, on top of the school's ordinary budget, earmarked for implementing 1:1 technology. Note that we, due to higher non-response on this item (around 50 %), have information on funding for a more limited sample of schools. Furthermore, the funding measure does not provide a high level of detail, e.g., we cannot observe exact amounts or confirm that the funding was used for the intended purpose. During the survey development process, it became evident that schools had challenges in providing information regarding the funding, especially when reflecting on past periods. Thus, we decided to gather less detailed information.

Fig. 1 shows the share of schools using 1:1 technology in our sample. In 2009, almost no school provided personal computers to their students. Over time there is a steady increase of 1:1 programs. Around 2016, the increase gained extra momentum. In the spring of 2020, more than 60 % of the schools in our sample use 1:1 technology. It is generally

⁶ The response rates in these surveys are around 80 %. The number of respondents vary between 2000 and 4000 students.

⁷ Figure A2 and Figure A3 show raw differences. The conclusions are robust to controlling for student characteristics (sex, age, born in Sweden/abroad, and parents born in Sweden/abroad), class size, and school variables (public/private school, number of pupils, share of girls, share of pupils born abroad, share of pupils that have immigrated to Sweden during the last four years, share of pupils with both parents born abroad, education level of pupils' parents, number of teachers, share of teachers with a formal teaching license, and teachers' average experience and age).

more common to use 1:1 in grade 6 than in grades 4 and 5. Figure A3 illustrates the extent to which schools have opted for laptops or tablets, respectively. Laptops are used much more often than tablets, especially from 2016 and onwards.

We have merged the school level data on 1:1 technology with individual level register data on school enrollment and performance on national standardized tests in math, Swedish and English that all pupils take in 6th grade. The students' grades on these tests are used as three separate outcome variables in the analysis. To account for potential changes in grading standards and content of the tests over time, we standardize the test results within cohort to have mean 0 and standard deviation 1.⁸ For many students, we also have access to results on national standardized tests taken in 3rd grade in math and Swedish, which we include as control variables in a robustness test.⁹ Moreover, various background variables, retrieved from national registers, have been linked to each student. These include information on age, sex, and immigrant background, as well as the parents' education, earnings, and immigrant background.

4. Empirical design

To capture effects of 1:1 technology on student learning, we compare how results on standardized tests develop over time for schools that introduce 1:1 technology and schools that do not launch such an initiative in an event study analysis.¹⁰ Our sample consists of pupils who enrolled in 4th grade in the schools for which we have obtained 1:1 data during 2009–2016. We follow these pupils until the year they are expected to finish 6th grade (i.e., until 2012–2019).^{11,12} A factor that makes the identification of causal effects complicated, is that a school's decision to introduce 1:1 technology also may affect the selection of students to the school. To mitigate this problem, we exclude all pupils who were given a laptop or tablet already from the first semester of grade 4 from the sample.¹³ This means that all pupils in our sample enrolled in 4th grade in a school that, at the time, had not introduced 1:1 technology in this grade. This sampling procedure results in a sample of 55,972 students enrolled in 269 different schools.¹⁴

⁸ The standardized tests given to the first cohort of students included in the analysis (those finishing grade 6 in 2012), differed from the tests given to the other cohorts. The standardized tests are generally designed to constitute the basis for the students' final grades, but this year the purpose was merely to assess whether the student had reached the lowest acceptable level of knowledge in each subject. For this cohort, we have access to data on test scores rather than the students' overall grades on the test. Our results do not change depending on if this cohort is included in the analyses or not.

⁹ These results are not available for the first cohort of students. For the other cohorts, we can observe the students' test scores on some of the sub-tests in Swedish and mathematics for most students.

¹⁰ In a few cases, 2 % of the observations, schools that have introduced 1:1 technology later abolishes this initiative. In these cases, we exclude the cohorts that are no longer part of the program from the analysis.

¹¹ Grade repetition during compulsory school is rare in Sweden.

¹² Our survey data also include information about 1:1 programs in 2020. However, due to the COVID-19 pandemic, all national standardized tests were cancelled this year.

¹³ This restriction also alleviates the concern that children in treated schools may have had greater access to laptops/tablets already before grade 4 compared to children in untreated schools. Around 8 % of the students are dropped due to this restriction. Imposing this restriction has the drawback that we exclude the students that were exposed to 1:1 technology for the longest period. However, we get similar results if we include these students in the sample.

¹⁴ The number of schools included in the analysis is somewhat lower than the number of schools that responded to our survey. The main reason for this discrepancy is that some schools that were registered as grade 4–6 schools in the School Registry did not have pupils registered in all these grades during the period in question.

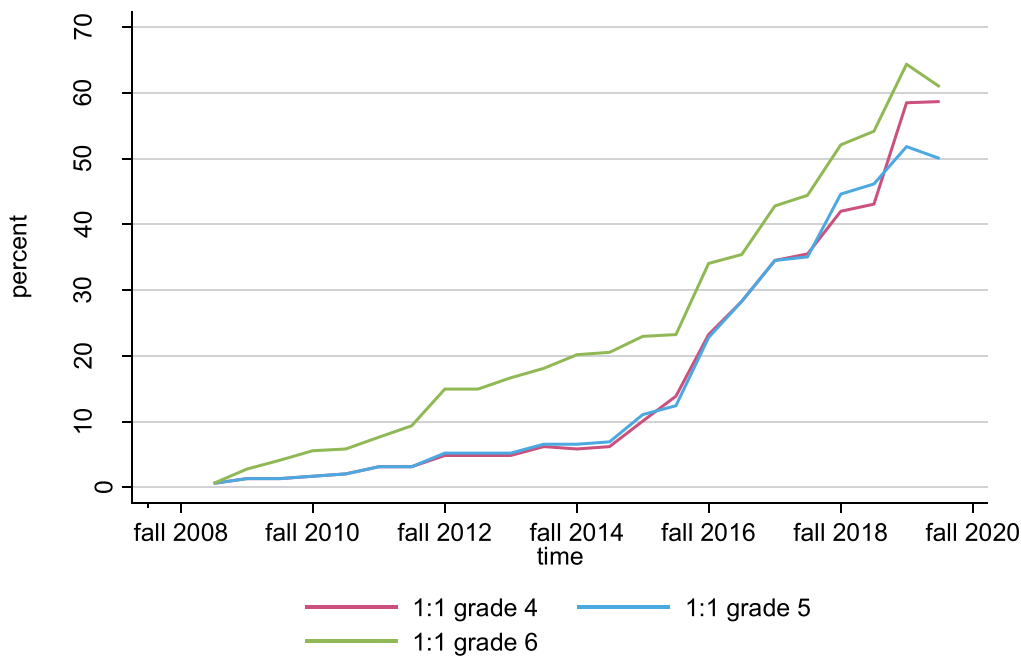


Fig. 1. Percent schools using 1:1 technology in different grades among the schools that responded to the survey.

Notes: Numbers for 2009–2016 are calculated among the 293 schools that responded to the first survey; numbers for 2016–2020 are calculated among the 196 schools that also responded to the follow-up survey. The pattern is almost identical if the sample is limited to schools that responded to both surveys .

Table A3 presents descriptive statistics. It is rare that schools introduced 1:1 technology in such a way that it affected students already in grade 4: only 1.6 % of the students had been provided with a computer already during the second semester of 4th grade. This share rose to 6 % a year later and reached 22 % by the spring semester of 6th grade, with laptops (16 %) being more common than tablets (6 %). On average, the students who received a laptop or tablet had access to their device for 2.5 semesters.

Table A4 compares background characteristics among students who in 2009 (i.e., the first cohort) enrolled in schools that later introduced 1:1 technology, and students from the same municipality who enrolled in schools that did not launch such an initiative during our study period. The two groups are balanced in terms of background characteristics, apart from a somewhat lower probability (around 5 percentage points) of attending a school that later introduces 1:1 among foreign-born students. Based on an F-test, we cannot reject the hypothesis that all the coefficients on the individual covariates are jointly zero (p-value 0.174).

To estimate causal impacts, we use the following event study model for student *i*, enrolled in school *s*, and belonging to enrollment cohort *c*:

$$y_{isc} = \alpha + \sum_{j \neq -1} \lambda_j (ICTprogram_{sc}) + \gamma_s + \delta_c + \theta X_{isc} + e_{isc} \quad (1)$$

where y_{isc} is the outcome of interest, i.e., the individual’s result on the national standardized test in mathematics, Swedish or English, which students take towards the end of 6th grade. $ICTprogram_{sc}$ is an indicator that takes the value 1 for students who enroll in schools that introduce 1:1 technology, otherwise it is 0. Event time, *j*, refers to time in relation to when the school implements 1:1 technology: $j=0$ refers to the first student cohort that was part of the program, $j=1$ the second cohort, and so on. Hence, the λ_j coefficients for $j \geq 0$ capture how student performance is affected by exposure to 1:1 technology year by year since the program was first introduced, while the coefficients for $j \leq -2$ capture relative trends in the period before these programs were launched. $j=-1$ is the reference year, which means that all λ_j estimates are relative to the year prior the introduction of 1:1. X_{isc} is a vector of individual background characteristics (sex, immigrant background, each parents’ education and earnings, and in some analyses the students’ previous test

scores; see Table A3 for a complete list), and γ_s and δ_c represent school and enrollment cohort fixed effects, respectively. e_{isc} is an error term.

Exposure to 1:1 is measured based on which school the student attended in the beginning of grade 4, which implies that λ_j (for $j \geq 0$) should be interpreted as intention-to-treat (ITT) estimates of 1:1 technology. However, around 80 % of the students stayed enrolled in the same school until 6th grade. A causal interpretation of λ_j (for $j \geq 0$) relies on the assumption that, in the absence of 1:1 initiatives, trends in student performance would not differ systematically between schools that introduce 1:1 technology and schools that do not launch such an initiative. This assumption is fundamentally untestable, but an inspection of the estimated leads (i.e., λ_j for $j \leq -2$) allows us to assess whether it seems credible.

De Chaisemartin and D’Haultfœuille (2020) and Goodman-Bacon (2021) show that two-way fixed effects models sometimes fail to identify meaningful treatment effects when treatment effects are heterogeneous. The underlying problem is that already treated units (in our case schools) to some extent also will be used as controls in the analysis. To circumvent this problem, we use the stacked event study estimator discussed in, e.g., Cengiz et al. (2019) and Baker et al. (2022), which removes earlier treated schools from the control group. More precisely, for each implementation year, we keep schools that introduced 1:1 technology during this year and schools that never introduced 1:1. Effects are identified within each such dataset (or stack) by comparing changes in outcomes in treated schools to the corresponding changes in never treated schools. These stacks are then combined, and the regression model includes school by stack fixed effects as well as cohort by stack fixed effects.¹⁵ The standard errors are clustered on school by stack.

¹⁵ The method is implemented using the statistical package *stackedev* in Stata; see Bleiberg (2021).

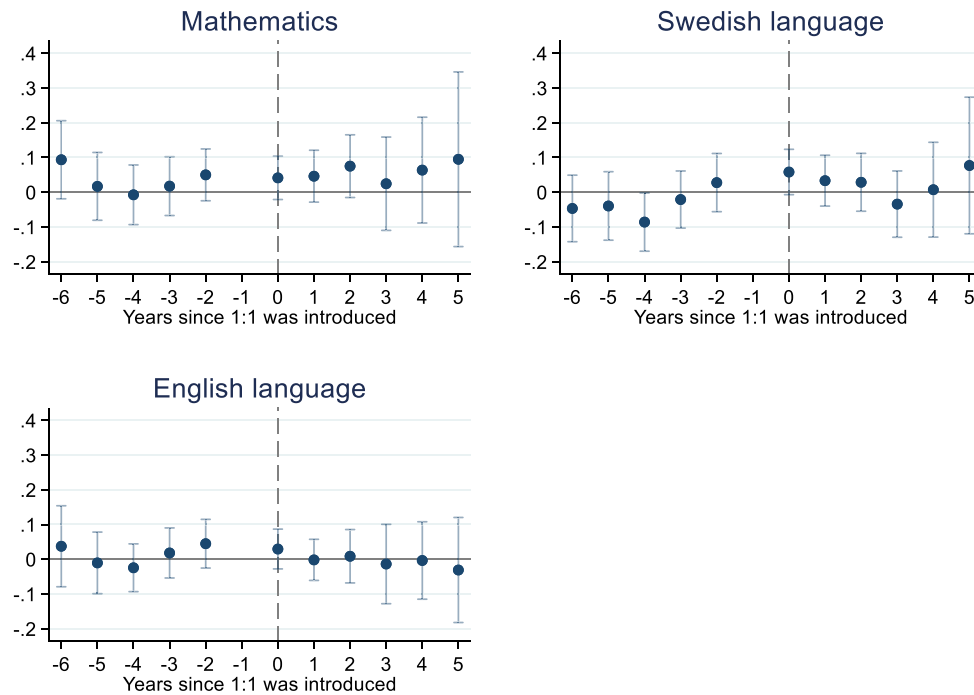


Fig. 2. Estimated effects of 1:1 technology on student performance on standardized tests.

Note: The figure shows estimated annual leads and lag effects of the introduction of 1:1 technology. Students' test results are standardized each year to have mean 0 and a standard deviation 1. The vertical line marks the year 1:1 technology was introduced. The reference year is -1 (i.e., all estimates are relative to the year prior to treatment). All regressions control for school and cohort fixed effects, sex, age, foreign-born, foreign-born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings as well as missing data on parental earnings and/or education. Leads >6 and lags >5 are included in the model but not shown as they are based on very few observations. The 95 % confidence intervals are based on cluster-robust standard errors clustered on school by stack.

5. Results

5.1. Main results

Fig. 2 displays results for the full sample of students. The results are based on a specification that includes the full set of (child and parental) background variables (except prior test scores). When interpreting the results, it is important to recognize that the estimates for each event time (j) are based on a somewhat different sample of treated schools, due to the fact that we have data from a fixed number of years and schools implementing 1:1 at different points in time. We can follow most of the treated schools four years before they introduced 1:1 (i.e., until $j=-4$) and two years after (i.e. until $j=1$). Leads and lags further away from the implementation year are based on progressively fewer schools, which is reflected by larger confidence intervals. Table A5 gives a detailed picture of the number and share of treated schools that can be followed for different lengths of time.¹⁶ Since higher leads and lags may be less representative, we put most weight in estimates closer to the implementation year when we interpret the results.

We can first note that the estimated leads give no clear sign of divergent trends in student performance between treated and control schools in the period before 1:1 was introduced, which is reassuring. The estimate for $j=-4$ is marginally statistically significant for Swedish but as this is relatively far back in time given the context, we do not think it should cause much concern.

The estimates for $j \geq 0$ capture how the impact of 1:1 technology evolves over time since the program was first introduced at the schools, with $j=0$ showing effects for the first student cohort that was part of the program. Length of exposure to 1:1 varies somewhat for the different treated cohorts included in the figure, ranging from 1.9 to 3.2 semesters; with the first treated cohort ($j=0$) having the shortest exposure and the second and third cohorts ($j=1$ and 2) the longest (see Table A5 for details).

The estimates for mathematics and Swedish, while generally positive, are not statistically significant and generally modest in size for the first four years (<5 % of a standard deviation).¹⁷ The estimates for English hover around zero throughout the study period. Hence, we find no evidence that 1:1 technology on average has an important impact on student performance.

5.2. Robustness checks

In Figure A4 in the online appendix we show results from the same event study model as in Fig. 2, but where we exclude all individual controls. In Figure A5 we show results when we on top of the covariates included in Fig. 2 also control for the child's results on the national standardized tests in Swedish or mathematics taken in 3rd grade, i.e., a measure of academic performance before potential exposure to 1:1

¹⁶ Leads and lags that are based on 10 % or less of the treated schools are included in the model but not shown in the figures.

¹⁷ Kraft (2020) proposes benchmarks for effect sizes of educational interventions based on estimates from 747 RCTs evaluating educational interventions on standardized test scores. By these standards, effects smaller than 0.05 of a standard deviation are considered small; 0.05–0.20 represent medium effects; and effects larger than 0.2 are considered large.

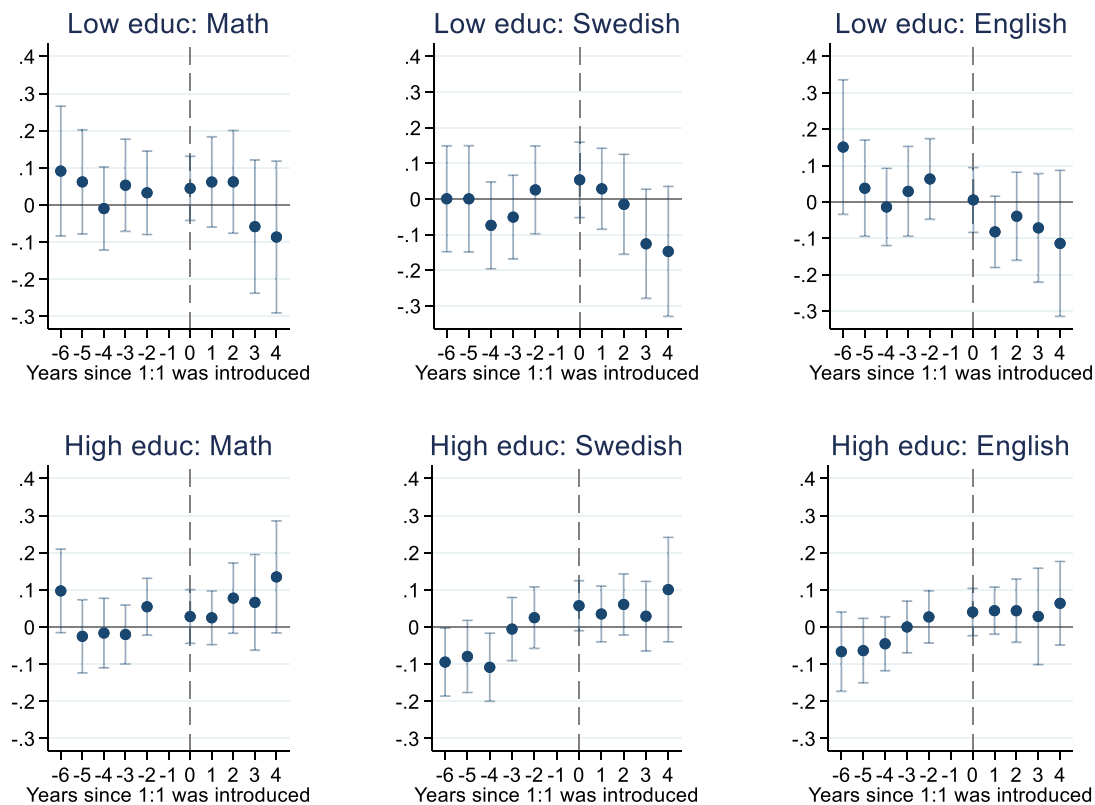


Fig. 3. Estimated effects of 1:1 technology, by parents' level of education.

Note: The figure shows estimated annual leads and lag effects of the introduction of 1:1 technology. Students' test results are standardized each year to have mean 0 and a standard deviation 1. The vertical line marks the year 1:1 technology was introduced. The reference year is -1 (i.e., all estimates are relative to the year prior to treatment). 'High education' is defined as at least one of the parents having post-secondary education, and 'low education' as none of the parents having post-secondary education. All regressions control for school and cohort fixed effects, sex, age, foreign-born, foreign-born parents, father's education, mother's education, father's earnings, mother's earnings, and missing data on parental earnings and/or education. Lead 7 and lags 5–7 are included in the model but not shown in the figures. The 95 % confidence intervals are based on cluster-robust standard errors clustered on school by stack.

technology.¹⁸ These test results are available for all but the first enrollment cohort.¹⁹ The pattern of results in both figures is similar to the pattern presented in Fig. 2, indicating that the results are not sensitive to whether we control for potential compositional changes in student background characteristics.

We also examine directly if there are differential compositional changes in student characteristics among schools that implement 1:1 technology and schools that do not launch such an initiative during our study period. To do this, we perform balance tests where we regress pre-determined variables that are strongly correlated with student performance – parental education and prior test scores – on the treatment variable, controlling for school and cohort fixed effects. In other words, we estimate model (1) without individual background controls, using

¹⁸ When the outcome variable is performance in math, we control for the student's result on the standardized test in math in 3rd grade; when the outcome variable is performance in Swedish or English, we control for the student's result on the test in Swedish in 3rd grade since tests in English are not given in 3rd grade. In both cases, we sum up the student's test scores for all subtests that are available. We then standardize the test score variables within cohort to have mean 0 and standard deviation 1. The tests in grade 3 are designed to help the teacher assess whether the student has reached the lowest acceptable level of knowledge. This means that many pupils get high scores, and that these control variables are better at capturing differences in student ability further down compared to higher up in the ability distribution.

¹⁹ The national tests in grade 6 were also differently constructed for the first student cohort compared to the other cohorts. For this reason, it is also interesting to examine if the results change depending on if this cohort is included in the analysis.

pre-determined characteristics as the outcome. Figure A6 shows that all estimates for the first four years after the introduction of 1:1 are statistically insignificant and close to zero, indicating that differential changes in student composition should not pose problems for our analyses during this period. For the higher lags, we find two significant estimates (prior test scores in math for $j=5$, and father's education for $j=4$), suggesting that the results for the highest lags should be interpreted with somewhat more caution.

We also investigate if our results are sensitive to certain sample restrictions. Figure A7 and Figure A8 show that we arrive at similar results if the sample also includes pupils who were given a computer already in the beginning of 4th grade, and if we restrict the sample to only include schools that answered both of our surveys (i.e., a more balanced sample of schools over time).²⁰

5.3. Heterogenous effects

Average effects may hide variations among different subgroups of students or schools. In this section, we conduct an exploratory analysis to investigate whether the impacts of 1:1 technology differ (i) based on student socio-economic background or (ii) depending on whether the school has received additional funding for 1:1 technology investments.

It is often anticipated that schools' investments in digital technology

²⁰ As an additional robustness check, we have estimated impacts where we exclude schools that have opted for tablets. In some cases, the estimates become slightly more positive. However, precision decreases and confidence intervals widen, maintaining conclusions largely unchanged.

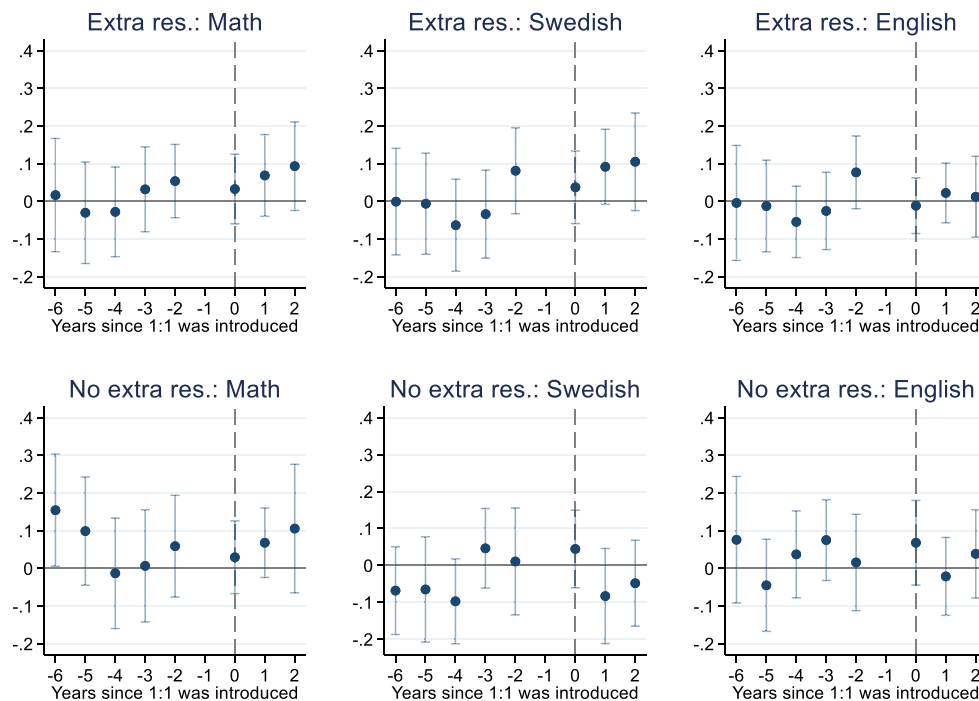


Fig. 4. Estimated effects of 1:1 technology. Separate analyses for treated schools with and without additional financial resources for 1:1 initiatives.

Note: The figure shows estimated annual leads and lag effects of the introduction of 1:1 technology. The analysis is conducted separately for treated schools that have answered “yes” or “no” to our survey question on additional financial resources for 1:1 initiatives. Students’ test results are standardized each year to have mean 0 and a standard deviation 1. The vertical line marks the year 1:1 technology was introduced. The reference year is -1 (i.e., all estimates are relative to the year prior to treatment). All regressions control for school and cohort fixed effects, sex, age, foreign-born, foreign-born parents, father’s education, mother’s education, father’s earnings, mother’s earnings, and missing data on parental earnings and/or education. Lead 7 and lags >3 are included in the model but not shown in the figures as they are based on very few treated schools. The 95 % confidence intervals are based on cluster-robust standard errors clustered on school by stack.

will contribute to greater equality between students of different socio-economic background by ensuring that all students have access to computers (e.g., Zheng et al., 2016). Although this argument often focuses on ICT skills and access to technology *per se*, policy-makers sometimes express hope that more technology in education will also lead to greater equality in educational outcomes (e.g., Swedish Government, 2017). However, it is not clear what effects we should expect on equality in school performance. Many of the benefits that exist with more technology in education (e.g., individualized learning and increased motivation) have the potential of benefitting all students. Moreover, if students from lower socio-economic background, on average, are more easily distracted by computers in the classroom, or if they are less able to take advantage of the technology due to less experience and help from parents, it is possible that 1:1 instead leads to increased inequality in educational performance.²¹ The results in Hall et al. (2021a) for Swedish secondary schools point in this direction.

Fig. 3 presents estimated effects of 1:1 technology on student performance in 6th grade separately by parents’ level of education. The upper panel displays results for children of parents who both lack post-secondary education, while the lower panel shows results for those where at least one parent has attained post-secondary education.²² All effects are imprecisely estimated. Nevertheless, we find no indication

that the provision of personal computers would lead to reduced inequality in performance by socio-economic background. If anything, the point estimates would suggest the reverse: the estimates of 1:1 exposure for children with highly educated parents are generally positive, whereas for children of less educated parents, they exhibit more variability and are consistently zero or negative for performance in English.

Fig. 4 illustrates estimated impacts, contingent upon whether the treated schools have received supplementary funding for investments in 1:1 technology. It is reasonable to expect more favorable effects when 1:1 initiatives are accompanied by additional funding, as opposed to instances where schools must reallocate resources from other areas. Our data on supplemental funding designated for personal student computers have inherent limitations, and we have information from a more limited number of schools (see Section 3 and Table A6 for details), which further decreases precision when we divide the sample into sub-groups of schools. Caution is therefore warranted when interpreting these findings.²³ In terms of math and English, our analysis reveals no evidence suggesting more positive impacts of 1:1 technology when additional resources are provided. In Swedish, the point estimates are more positive in the group of schools that have received additional funding. Nevertheless, firm conclusions cannot be drawn at this stage. The emerging picture suggests that future research should incorporate information on extent of additional funding when assessing the impacts of 1:1 technology on student outcomes.

²¹ Findings in Beland and Murphy (2016) suggest that mobile phones in the classroom implies a larger distraction for low-performing than for high-performing pupils. Bergdahl et al. (2020) show that academically weaker pupils find it more difficult to concentrate using digital tools and are more likely to use social media or streaming media to escape when lessons are boring, compared to academically stronger students.

²² Note that we exclude visualizing effect estimates for $j=5$. The confidence intervals for this event-time are extremely large, necessitating a re-scaling of the y-axis that would compromise the figures’ readability.

²³ We do not show estimates for event-times that are based on less than 10 treated schools in either of the two groups.

6. Conclusion

Few would argue that computers should not be utilized in education in a modern society, but the extent, the way and for whom technology is best used can be debated. In this article, we contribute to this discussion by providing evidence on how 1:1 computer programs in Sweden affect learning outcomes in primary school (grades 4–6) in terms of performance on standardized tests in mathematics and language. As a fore-runner when it comes to computer access and incorporating digital technology in education (European Commission, 2019), Sweden offers valuable insights for other nations.

In general, there is less knowledge on how 1:1 technology and the integration of ICT in teaching affects learning among younger compared to older students. Hall et al. (2021a) examine the impact of 1:1 programs on learning outcomes in lower secondary school (grade 7–9) within the same Swedish municipalities, during a similar period, and using a similar empirical design as we do. Thus, we can compare the results at the end of primary school with the results for students who are three years older. Among the younger children focused upon in this article, we find no evidence suggesting that 1:1 technology, in comparison to more limited computer use, have an impact on average performance in language and mathematics.²⁴ These findings are in line with the findings for lower secondary school in the previous study. The findings are also in line with most of the few prior studies on the causal impact of 1:1 on school performance that exist (de Melo et al., 2014; Mora et al., 2018; Hull and Duch, 2019; Yanguas, 2020), although these studies usually focus on older students. Furthermore, there is a general discussion in the literature on the effects of ICT investments on student performance. Review articles conclude that investments in ICT that merely improve access to technology without having a distinct educational purpose have limited effects on learning outcomes (Escueta et al., 2020; Haelermans, 2017). Our results align with this literature as well.

Declaration of competing interest

The authors have no conflict of interest.

Data availability

Data will be made available on request.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at [doi:10.1016/j.econedurev.2024.102536](https://doi.org/10.1016/j.econedurev.2024.102536).

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²⁴ Of course, 1:1 technology may have benefits that are not captured by our outcome variables; above all, students' computer skills are likely to be enhanced, and we cannot rule out that there is important age heterogeneity in such impacts.

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