WORKING PAPER 2023:3

Teaching, technology and test scores

The impact of personal computers on student performance in primary school

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IFAU INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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ISSN 1651-1166

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The impact of personal computers on student performance in primary school[a]

by

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February 10, 2023

Abstract

The closing of schools and shift to remote teaching during the COVID-19 pandemic has 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 on how one-to-one (1:1) computer programs affect learning outcomes among younger pupils. We investigate how 1:1 computer technology impacts student performance in primary school in Sweden, using data from an expansion of 1:1 programs that took place before the pandemic. Using a staggered difference-in-differences design, we examine impacts on student performance on standardized tests in language and math in 6th grade. We find no important effects on these learning outcomes on average, but a positive effect on test scores in Swedish and English among students with highly educated parents. Moreover, the results indicate a positive effect in Swedish in schools that received additional financial support for implementing 1:1 technology. Nevertheless, all positive impacts in subgroups appear to be rather small, amounting to 0.01–0.03 SD per semester of 1:1 exposure.

Keywords: Technology, computers, one-to-one programs, student performance
JEL-codes: I21; I24

[a] We are grateful to Sara Roman and seminar participants at IFAU for valuable comments. We also want to thank the National Agency for Education for sharing survey data and Kristina Sibbmark, Tove Mörtlund and Emma Eliasson Åström for assistance in the data collection.
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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) received a key role. In economically advanced societies, most schools had already integrated ICT in education, but the pandemic accelerated this development: digital technology became used to a much greater extent, partly in a new way, and also in the teaching of younger children. The pandemic may well prove to be a turning point that changes the way and the extent to which computers are used in teaching. In the aftermath of the pandemic, school leaders and teachers need to decide to what extent ICT should remain integrated in daily school activities. One important policy decision concerns one-to-one computer programs (or 1:1 programs); that is, whether schools should invest in personal laptops or tablets (with online connection) for each student to be used at school (and often also at home). Such a strategy is likely to be beneficial in a situation of remote teaching, as lack of technology may otherwise limit some students' access to education, but what are the impacts on student learning under more normal circumstances?

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 would be more beneficial than employing more teachers or teacher assistants. It is also not clear if teaching methods based on digital technology generally enhances learning compared to more traditional teaching methods, and there are downsides associated with ICT in education to consider, such as increased elements of distraction (through games, videos, and social media).

1:1 programs are common in most developed countries (Yanguas 2020), especially in higher grades. In Sweden (National Agency for Education 2019) and Norway (Fjørtoft, Thun, and Buvik 2019), over 90 percent of upper secondary school students receive a personal laptop or tablet from their school. 1:1 technology has also become more common among younger pupils, and the COVID-19 pandemic has accentuated this development. In a survey with a representative sample of school district leaders in the U.S., 42 percent reported that the schools in their district supplied one device per student in elementary school (grades 0–5) at the onset of the pandemic. One year later, this share was 84 percent. In middle school (grades 6–8), the corresponding increase was from 65 percent to 90 percent (EdWeek Research Center 2021). 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? There is a small but growing literature that provides reliable evidence on the causal impacts of 1:1 initiatives on educational performance (de Melo, Machado, and Miranda 2014; Mora, Escardíbul, and Di Pietro 2018; Hull and Duch 2019; Yanguas 2020; Hall, Lundin, and Sibbmark 2021)[1], but few studies so far focus specifically on primary school students. An exception is de Melo, Machado, and Miranda (2014) who investigate the impact of a national implementation of 1:1 in Uruguay, finding no effects on student performance in math or reading.[2]

In the present study, we investigate 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 Swedish municipalities regarding their implementation of 1:1 technology during 2009–2020 and linked this information to administrative data on students' characteristics and their performance on standardized tests (after as well as before they were exposed to 1:1 technology). To identify a causal relationship, we compare how student performance changes across cohorts in schools that introduce 1:1 programs, to changes for schools that have not yet launched such programs, in a staggered difference-in-differences design.

We find no evidence suggesting that 1:1 technology affects student performance in math or English on average. There are some indications of a positive average effect in Swedish, but the estimate is small and in some model specifications not statistically significant at conventional levels. Thus, we conclude that 1:1 technology, on average, has no important impact on the outcomes we study. When examining heterogeneity in impact across subgroups of schools and pupils, we find a positive effect on test scores in Swedish and English among students with highly educated parents, while there are no clear effects for students with less educated parents.

[1] There is also a rather large number of studies that have used less credible strategies to isolate causal effects. For a discussion of this literature, see, e.g., Islam and Grönlund (2016), Zheng et al. (2016), and Hull and Duch (2019).

[2] Another study that focuses on primary school is Cristia et al. (2017), who examine the impacts of 1:1 in poor regions in Peru. However, the context in this study is very different from the context of most economically advanced countries today. For instance, 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 some evidence of positive effects on the pupils' general cognitive skills. We review the previous literature in more detail in Section 2.

Furthermore, the results indicate a positive effect in Swedish in schools that have received extra financial resources for implementing 1:1 technology.

Our study makes at least four contributions to the understanding of the impact of digital technology in education: First, we add to a small but growing literature on the impacts of 1:1 programs on educational performance by confirming the main conclusion from the few causal studies that exist: providing a personal computer to students will on average not improve performance on standardized tests in language and math to any important extent. Second, we can address the question of whether 1:1 technology works better for younger or older pupils. This has not been examined in detail before. Hall, Lundin, and Sibbmark (2021) examine the effects in grades 7–9 (age 13–15) in the same Swedish municipalities, during a similar period, and using the same methodology as we do in this article. Thus, we can make a direct comparison of the findings for grades 4–6 in the present study with the prior findings for pupils in grades 7–9. Overall, the findings are similar. Third, 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). This is the first study that we are aware of to examine this issue. The few positive impacts that we can observe are concentrated to schools that have received additional financial support. Fourth, an unsolved issue in the literature is whether 1:1 decreases or amplifies differences in performance between groups of students. Our results suggest that students with a comparably strong socioeconomic (SES) background may improve their performance to some extent in a 1:1 environment; hence, contributing to somewhat increased differences in school results by socioeconomic background.

The rest of the paper is outlined as follows: We start by reviewing the related literature in Section 2. In Section 3, we describe the Swedish school system and the role of ICT in education in Sweden. In Section 4, we present the data, and in Section 5 we discuss the empirical strategy. The results are presented in Section 6, and Section 7 concludes.

2 Literature review

The main purpose of 1:1 technology programs is to improve students' ICT skills and prepare them for a future society where computers have a central role. In addition, these programs are usually intended to enhance learning in general. There are several mechanisms through which ICT may improve student performance (see, e.g., Bulman and Fairlie 2016; Haelermans 2017; Hall, Lundin, and Sibbmark 2021; Islam and Grönlund 2016; Hull and Duch 2019; Zheng et al. 2016): Teaching can more easily be individualized to suit students' strengths and weaknesses; for example, through computer software programs based on self-paced instructions. Using software in teaching may also imply other educational gains. For instance, word processing programs can be used by pupils to work with texts, and other software can be used for practice, rehearsal and to consolidate knowledge. Assignments may be completed in a better and faster way if students have access to computers and the internet. Moreover, interactive teaching methods and educational computer games may make learning more enjoyable and thereby increase student motivation. ICT also provides opportunities to access more and newer information through the internet, which may stimulate learning. Additionally, the possibilities for communication and feedback may be improved, for example, between students and teachers, between teachers and parents, as well as among students.

Few would argue that computers should be disregarded in education, but there are potential downsides that must be acknowledged (see, e.g., Bulman and Fairlie 2016; Haelermans 2017; Hall, Lundin, and Sibbmark 2021; Islam and Grönlund 2016; Hull and Duch 2019; Zheng et al. 2016). Most notably, 1:1 programs require considerable investments in computers, infrastructure, teacher training, and technical support. These financial resources could have been used for something else that might have been more beneficial for students' learning outcomes. Implementation problems and technical challenges are also often present with 1:1 technology. Furthermore, there is always a risk that computers distract pupils from educational activities through games, videos, and social networking. The use of ICT in the classroom may also imply that students work a lot on their own, which may or may not be superior to other ways of learning. In the end, it is an empirical question if investments in 1:1 technology is an effective strategy to enhance learning, or if it is better to use the financial resources and time in school for other things.

There is a quite large literature on the role of 1:1 technology in education; see Islam and Grönlund (2016) and Zheng et al. (2016) for overviews. However, only a limited number of studies provide evidence on the causal impact of 1:1 programs on student performance using experimental or quasi-experimental approaches.[3] The evidence from these studies is mixed, with the majority finding no impact on learning outcomes: de Melo, Machado, and Miranda (2014) and Yanguas (2020) examine the impact of a nation-wide implementation of a 1:1 program in Uruguay. The former study finds no effect of the program on reading or math performance in primary school (grades 3–6), and the latter finds no impact of program exposure (in primary or middle school) on educational attainment in adulthood. Mora et al. (2018) analyze the effects of the implementation of a 1:1 program in secondary school (grades 7–10) in Catalonia, and find that the program had negative effects on student performance in language and math, particularly among boys. Another study is Hull and Duch (2019) analyzing a 1:1 initiative in seven schools

[3] The findings in the studies that do not use experiments or quasi-experimental methods are mixed. Many studies find a positive association between having a 1:1 program and student performance, but there are also several studies indicating no association and a couple of studies suggesting negative associations. This literature is summarized in Islam and Grönlund (2016) and Zheng et al. (2016).

in North Carolina (grades 4–8). They find no short-term impact on math and language scores, but positive effects on math scores in the medium term. Last, Hall, Lundin, and Sibbmark (2021) investigate how 1:1 initiatives affect student performance in secondary school (grades 7–9) in Sweden, finding no effect on average test scores in math and language, nor on admittance to high school. However, they find that 1:1 programs increase inequality in school performance by worsening test scores among pupils from homes with lower SES background.

Besides the literature on 1:1 technology, there are several empirical studies on the effects of other types of ICT initiatives. For instance, Beuermann et al. (2015) study the impact of home computers, Malamud et al. (2019) analyze the effects of free internet access, Goolsbee and Guryan (2006) examine subsidies for internet and communication investments in schools, and Banerjee et al. (2007) and Roschelle et al. (2016) estimate the impact of certain educational computer software. Haelermans (2017) and Escueta et al. (2020) provide extensive overviews of prior research. They conclude that the findings in the literature are mixed, but it seems that investments in ICT that merely improves access to technology without a distinct educational purpose often have limited effects on learning outcomes. 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 to what extent the results from this strand of literature can be generalized to 1:1 technology as the initiatives studied generally implies a much less extensive use of ICT in the classroom. Bettinger et al. (2022) shows, in the context of homework, that there is diminishing marginal return to computer-assisted learning: while moving from zero to a low level has a positive effect on learning outcomes, the estimated impact turns to zero or negative when the use of educational technology is increased to a high level.

There are several interesting sub-questions to the overall question of how 1:1 technology affects student performance. In the present study, we address three of them. First, 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 (cf. Brod 2021). In general, there is much less evidence on the impact of ICT on educational outcomes among younger pupils (Escueta et al. 2020). Worldwide, schools have generally introduced 1:1 technology to a greater extent and earlier in higher than in lower grades. This is most certainly based on the idea that 1:1 programs are more suitable and likely to produce better outcomes as students become older, which might be a reasonable assumption; digital literacy increases with age (Jin et al. 2020; Lazonder et al. 2020). Basic skills can probably be taught well without computers, and as children grow older, they are likely to be more able to take care of their device and use it appropriately. With increasing age and more complex tasks, technology may have 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 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. If the impact of 1:1 technology differs for younger compared to older students is ultimately an empirical question.

Second, some schools use their existing budget to finance 1:1 initiatives, while others receive extra funding to cover some (or all) of the expenses. Schools receiving additional financial resources do not need to cut down on other spending to the same extent as schools that finance these initiatives from their ordinary budget. This means that we should expect the effects of 1:1 technology to differ depending on if the initiative has been coupled with additional funding or not. Prior empirical research has largely neglected this issue (Bulman and Fairlie 2016).

Third, it is possible that 1:1 technology is more beneficial, or detrimental, for certain groups of students. 1:1 initiatives may, for instance, provide students from a low-SES background with resources they otherwise have less access to in comparison to high-SES students. This could potentially lead to greater equality in educational outcomes. However, many of the potential benefits associated with 1:1, such as more individualized teaching, may benefit all students. Furthermore, if low-SES students on average are more easily distracted by computers in the classroom[4], or if they are less able to take advantage of the technology due to less experience, 1:1 technology might instead lead to greater inequality in learning outcomes. The question of how 1:1 initiatives affect the digital divide have been recognized in the literature, but empirical evidence is still scarce (see Hall, Lundin, and Sibbmark (2021) for an exception).

In the empirical analysis that follows, we examine the impact of 1:1 technology on student performance on standardized tests in math and language at the end of primary school (grade 6). We analyze whether effects differ depending on how 1:1 programs are financed, and take a closer look on how students with different SES-background are affected. In the final discussion, we also compare our findings with previous estimates for Swedish secondary school students.

3 The Swedish case

We study the impact of 1:1 technology in a Swedish context. Sweden has for a long time had nine years of compulsory schooling, starting in the fall semester of the year a child turned seven. From

[4] The results in Beland and Murphy (2016) suggests 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.

2018, one year of preschool-class/kindergarten (grade 0) is also mandatory, adding an additional year to the compulsory school system. There is a national curriculum that all schools must adhere to, but the organization of schools is decentralized to the municipal level. Grade configurations vary across schools. 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 also 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, which consists of various college-preparatory and vocational tracks.

There are both public and 'independent' (but publicly funded) schools. Around 85 percent of the children in compulsory school attended a public school in the school year 2016/2017 (National Agency for Education 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, Holmlund, and Lindahl 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.[5]

Local income taxes and central government grants constitute the schools' main sources of finance. Each school has its own budget, and the principal decides how to use the money. A decision to invest in a 1:1 program can therefore sometimes be 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 relatively common (National Agency for Education 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 that receive a personal computer from their school has increased steadily since the first 1:1 program was introduced around 2007/2008. The share of pupils in grades 4–6 that received a personal laptop or tablet from their school was around 20 percent in 2012, 30 percent in 2015, and 50 percent in 2018 (Hall et al. 2021).[6]

All schools have the duty of integrating ICT into their teaching, at least to some extent, to strengthen the students' digital competence (National Agency for Education 2018). There is usually good access to student computers even in schools without 1:1 programs (National Agency for Education 2019), and almost all children have access to computers and the internet at home

[5] There are very few fully private schools (with a tuition fee) in Sweden.

[6] In 2018, about 90 percent of the students in upper secondary school (grades 10–12) were provided with a personal laptop or tablet from their school. In grades 7–9, the corresponding figure was 75 percent (National Agency for Education 2019).

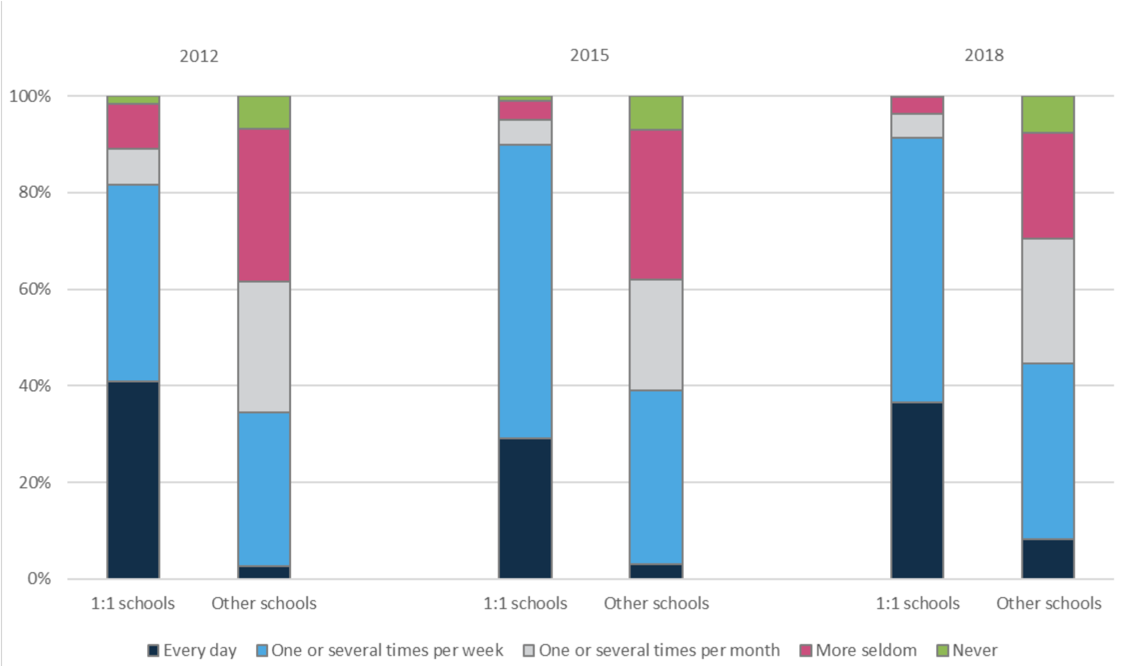
(Swedish Media Council 2015). Hence, it is important to underscore that 1:1 technology is not a question of using or not using computers in education in the Swedish context. However, various studies, focusing on Sweden and on the same time period as we do in this article, have shown that schools with 1:1 technology use ICT in teaching to a much larger extent than schools without 1:1 (Hall et al. 2021; see also National Agency for Education 2016, Lindqvist 2015 and Hallerström and Tallvid 2008).

The Swedish National Agency for Education conducts surveys with representative samples of pupils every three year.[7] We use these data for pupils in grades 4–6 to illustrate differences between schools with and without 1:1 technology.[8] Figure 1 shows that pupils in 1:1 schools use laptops/tablets for schoolwork much more than pupils in schools that do not offer personal computers. Figure 2 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. From Figure 2 it is also clear that students with access to personal computers use computers for all these various tasks to a greater extent than other students.

[7] The response rates in these surveys are high, around 80 percent. The number of respondents vary between 2 000 and 4 000 students. See Hall et al. (2021) for additional information.

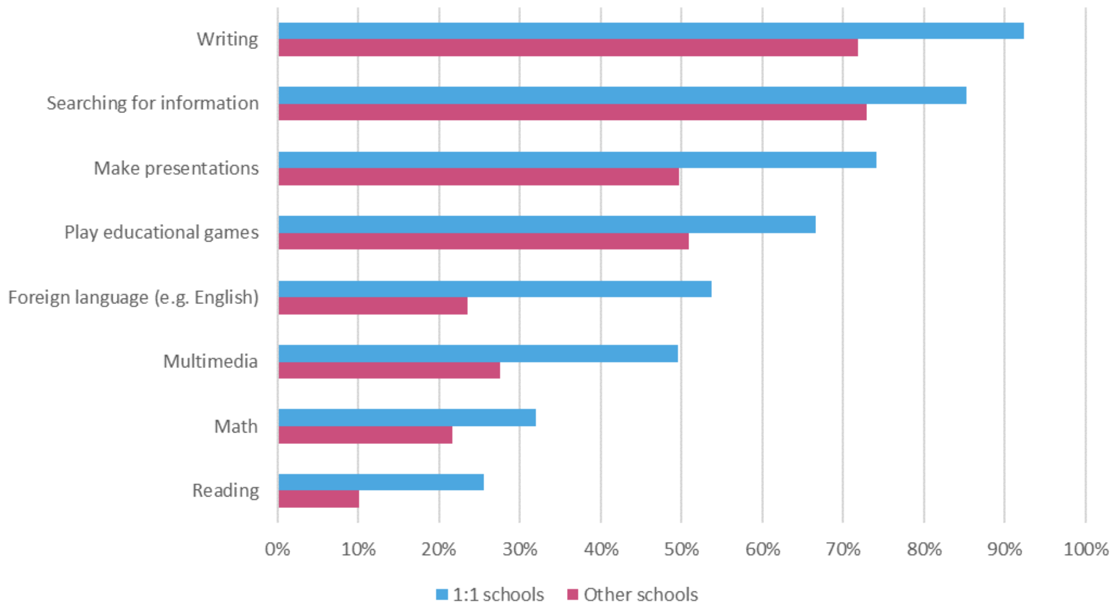
[8] Figure 1 and Figure 2 illustrate raw differences. The conclusions remain the same if we analyze data within a regression framework and control for the following background characteristics: *Student variables:* sex, age, born in Sweden/abroad, and parents born in Sweden/abroad. *Class variable:* class size. *School level 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.

Figure 1 The extent to which laptops/tablets are used in schoolwork by Swedish pupils in grades 4–6



Notes: Own calculations based on survey data collected by the Swedish National Agency for Education. Number of respondents: 2 276 (2012), 3 704 (2015) and 2 385 (2018).

Figure 2 How laptops/tablets are used in schoolwork by Swedish pupils in grades 4–6 (2018)



Notes: Own calculations based on survey data collected by the Swedish National Agency for Education. Number of respondents: 2 475.

4 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.[9] A variety of municipalities, in terms of geographic location, population size, and average education level, were included in the sample. To ensure that the sample would include a sufficient number of schools that had introduced 1:1 technology, we selected half of the municipalities based on prior information[10] indicating a comparatively extensive use of 1:1 programs. We contacted the schools by e-mail and non-responders were reminded by e-mail 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.[11] In 2016, we received a response from 293 of these schools. In 2020, 193 schools responded. This means that we can follow around 70 percent of the schools in the selected municipalities from 2009 to 2016, and approximately 50 percent for the whole period 2009–2020.

We asked the schools about the presence of 1:1 technology during 2009–2020. If present, we also wanted to know which grades were included in the initiative at different points in time, and whether the school used laptops or tablets. 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.

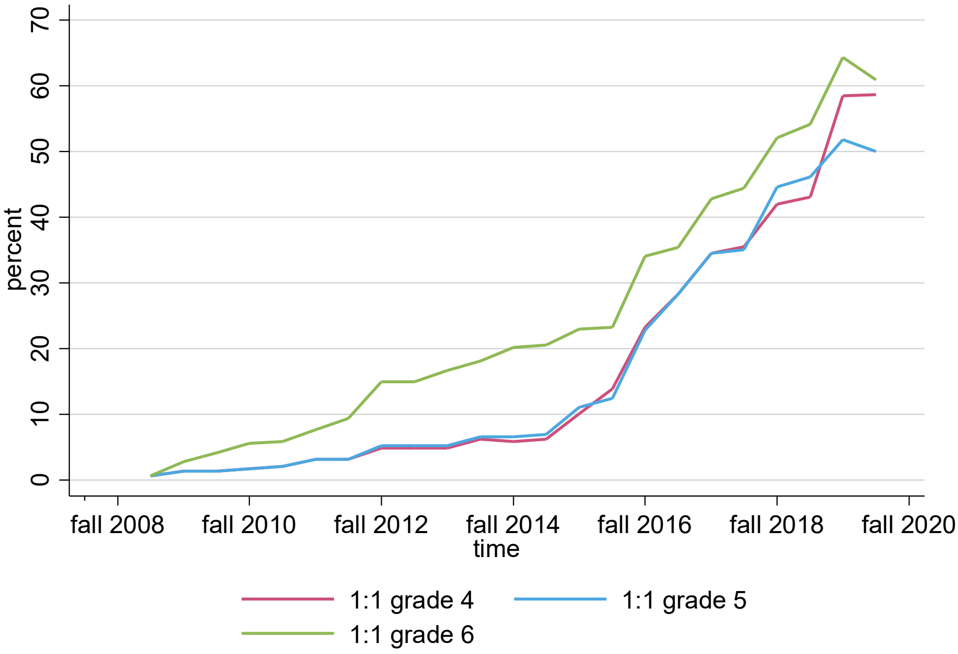
Figure 3 shows the share of schools using 1:1 technology in our sample, 2009–2020. In 2009, almost no school provided personal computers to their students. Over time there is a steady increase of 1:1 programs. Around 2016, the increase gained extra momentum. At the last measurement point, in the spring of 2020, more than 60 percent of the schools in our sample use 1:1 technology. It is generally more common to use 1:1 in grade 6 than in grades 4 and 5. Figure A 1 in the appendix 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.

[9] We only contacted schools that offered all these grades according to the National School Registry.

[10] We consulted local newspapers and internet homepages of municipalities and schools to get an indication of 1:1 coverage in the municipalities.

[11] We refrained from contacting a few schools that had a very small number of students; some of these were schools in rural areas with only a few students in the relevant grades, others were schools for children with special needs (e.g., Autism).

Figure 3 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 responed 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 .

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 as well as content of the tests over time, we standardize the test results within cohort to have mean 0 and standard deviation 1.[12] For most student cohorts, 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 some analyzes.[13] 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.

[12] In particular, the standardized tests given to the first cohort of students included in the analysis (those finishing grade 6 in 2012), differed substantially 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; see column (2) and (3) in Table 2**.**

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

5 Empirical design

To capture effects of 1:1 technology on student learning, we compare how results on national standardized tests change across student cohorts for schools that introduce 1:1 technology, to changes over the same time period for schools that have not yet introduced 1:1, in a staggered difference-in-differences design.[14] 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 finish 6th grade (i.e., until 2012–2019).[15] [16] A factor that makes the identification of causal effects of 1:1 more complicated, is that a school's decision to provide personal laptops (or tablets) 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.[17] 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 56,862 students enrolled in 270 different schools.[18]

Table 1 presents descriptive statistics. It is relatively uncommon that the schools introduce 1:1 technology in such a way that it affects the students already in 4th grade: only 1.6 percent of the students had been provided with a personal laptop/tablet already the following spring. A year later, this share amounts to 6 percent. By the spring semester of 6th grade, 22 percent of the students had either received a personal laptop (16 percent) or tablet (6 percent) from their school. On average, the students who received a laptop or tablet had access to their device for 2.5 semesters (not shown in the table).[19]

[14] The same design has previously been used in Hall, Lundin, and Sibbmark (2021) to study the impact of 1:1 technology in secondary school.
[15] Grade repetition during compulsory school is rare in Sweden.
[16] 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.
[17] 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, since children often attend the same school also in earlier grades. Around 8 percent 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. In Section 6.2, we show that we get similar results if we include these students in the sample.
[18] 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.
[19] Given that they remained in the same school up until the end of 6th grade.

Table 1 Descriptive statistics for the sample

	Average	Standard deviation
Exposure to 1:1 technology		
1:1 technology, spring semester grade 4	0.016	0.124
1:1 technology, spring semester grade 5	0.060	0.238
1:1 technology, spring semester grade 6	0.221	0.415
Personal laptop, spring semester grade 4	0.012	0.110
Personal laptop, spring semester grade 5	0.048	0.214
Personal laptop, spring semester grade 6	0.163	0.369
Personal tablet, spring semester grade 4	0.003	0.058
Personal tablet, spring semester grade 5	0.013	0.112
Personal tablet, spring semester grade 6	0.059	0.235
Background characteristics		
Female	0.491	0.500
Born abroad	0.093	0.290
Both parents born abroad	0.234	0.423
One year younger than classmates	0.014	0.118
One year older than classmates	0.026	0.159
Two years older than classmates	0.001	0.024
Mother has (at most) upper secondary education	0.397	0.489
Mother has post-secondary education	0.449	0.497
Data on mother's education is missing	0.045	0.208
Father has (at most) upper secondary education	0.447	0.497
Father has post-secondary education	0.359	0.480
Data on father's education is missing	0.070	0.256
Yearly earnings, father	316,955	291,201
Yearly earnings, mother	218,657	180,605
Missing data on father's earnings	0.061	0.239
Missing data on mother's earnings	0.033	0.177
Result on 3rd grade standardized tests in math[a]	-0.060	1.043
Result on 3rd grade standardized tests in Swedish[a]	-0.044	1.037
Number of observations	56,862	

Notes: Results on 3rd grade standardized tests are missing for the cohort that began 4th grade in 2009.[a] The test scores have been standardized within cohort (nationally) to have mean 0 and standard deviation 1.

Table A 1 in the appendix 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 of students are balanced in terms of background characteristics, apart from a somewhat lower probability (4.5 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 0.333).

In the empirical analysis we estimate the following regression model:

$$y_{isc} = \alpha_0 + \beta_1 ICTsemesters_{sc} + \beta_2 X_{isc} + \theta_c + \delta_s + e_{isc} \qquad (1)$$

where i indexes individual, s the school the individual attends in the beginning of 4th grade, and c the year the individual begins 4th grade (which we refer to as "student cohort"). y_{isc} is the individual's results on the national standardized test in mathematics, Swedish or English, which students take towards the end of 6th grade. $ICTsemesters_{sc}$ counts the number of semesters the individual would be exposed to 1:1 if staying enrolled in the same school up until the end of 6th grade.[20] X_{isc} is a vector of individual background characteristics (sex, age, immigrant background, each parents' education and earnings; see Table 1 for a complete list), and θ_c and δ_s represent cohort and school fixed effects, respectively. e_{isc} is an error term. The parameter of interest, β_1, gives us an estimate of how student performance is affected per semester of exposure to 1:1 technology. Since exposure to 1:1 is measured based on which school the pupil attends in the beginning of grade 4, β_1 should be interpreted as an intention-to-treat (ITT) estimate of 1:1 technology. However, note that the vast majority – around 80 percent – stay enrolled in the same school until 6th grade. The standard errors are clustered at the school level to account for correlation between students who attend the same school.

By including school fixed effects, our model accounts for unobserved differences between schools that remain constant over time. However, a causal interpretation of β_1 will rely on the assumption that trends in student performance would not differ systematically between schools that introduce 1:1 technology and schools that have not yet introduced 1:1, in the absence of these initiatives. This assumption is fundamentally untestable, but by examining pre-treatment trends we can assess whether or not it seems credible. To do this, we perform placebo-test by estimating the same model but (artificially) define 1:1 technology to have been introduced one, two, three, and four years before the actual start date. We also perform balance tests where we regress several pre-determined characteristics on the treatment variable, controlling for school and cohort fixed effects. The latter analysis sheds light on the presence of differential compositional changes in observed characteristics in 1:1 schools relative to other schools during our study period. Moreover, we test the robustness of our results to a weakness of this type of staggered difference-in-differences model in the presence of heterogenous treatment effects, which is discussed in the recent methodological literature (e.g., de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2021). These analyses are all presented in Section 6.2 along with some additional robustness checks.

[20] This is our preferred treatment variable as it explicitly accounts for the length of exposure to 1:1 technology. In Section 6.2, we also show results for a binary treatment variable that measures whether the student was ever exposed to 1:1 during grades 4–6.

6 Results

This section presents the results from the empirical analysis. We first show results for the full sample of students (Section 6.1). These results are followed by a number of placebo-analyses and robustness checks (Section 6.2). Thereafter, we investigate if there are heterogenous effects for different sub-groups of students (Section 6.3), and whether effects differ depending on if the schools have received supplemental financial resources for their investment in 1:1 technology (Section 6.4).

6.1 Main results

Table 2 displays estimates for the regression model discussed in Section 5 for the full sample of pupils. For each outcome variable, we estimate four different specifications of the model: Column (1) only includes controls for school and cohort fixed effects. Column (2) also controls for the full set of (child and parental) demographic background variables. In column (4) we additionally control for the child's results on the national standardized test in Swedish or mathematics taken in 3rd grade, that is, a measure of the child's academic performance *before* he/she began 4th grade.[21] Since 3rd grade test scores are not available for the first student cohort (those that began 4th grade in 2009), column (3) replicates the results in column (2) but for the same cohorts of students that are included in the last column.[22]

For mathematics and English, there is nothing to suggest that 1:1 technology would impact student performance: all estimates are close to zero and statistically insignificant. For Swedish, there are some indications of a positive effect, albeit small in magnitude.[23] However, when we in column (4) control for previous test scores, which should be considered the most reliable specification, the effect is no longer statistically significantly different from zero. Hence, taken together, we find no clear evidence that 1:1 technology impacted student performance on average.

[21] 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 standardized test in Swedish in 3rd grade since standardized test 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 in our data material. We then standardize the test score variables within cohort to have mean 0 and standard deviation 1. The standardized national 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.

[22] The standardized national tests in grade 6 were also differently constructed for the first student cohort compared to the other cohorts; see Section 4. For this reason, it is also interesting to examine if the results change depending on if this cohort is included in the analysis or not.

[23] Kraft (2020) proposes new 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 can be considered small: 0.05–0.20 represent medium effects; and effects lager than 0.2 can be considered large.

Table 2 Effects of 1:1 technology on student performance on standardized test

	(1)	(2)	(3)	(4)
A. Mathematics				
No. of semesters with 1:1 program	0.005	0.005	0.004	0.004
	(0.008)	(0.008)	(0.008)	(0.009)
Observations	52,918	52,260	45,518	43,969
R-squared	0.118	0.211	0.212	0.445
B. Swedish				
No. of semesters with 1:1 program	0.016*	0.018**	0.016*	0.015
	(0.009)	(0.008)	(0.009)	(0.009)
Observations	52,858	52,216	45,397	44,003
R-squared	0.118	0.249	0.252	0.398
C. English				
No. of semesters with 1:1 program	0.001	0.003	0.003	0.002
	(0.008)	(0.007)	(0.008)	(0.008)
Observations	52,710	52,034	45,232	43,840
R-squared	0.087	0.155	0.152	0.310
Demographic controls	No	Yes	Yes	Yes
Control for prior test scores	No	No	No	Yes
Included cohorts	2009–2016	2009–2016	2010–2016	2010–2016

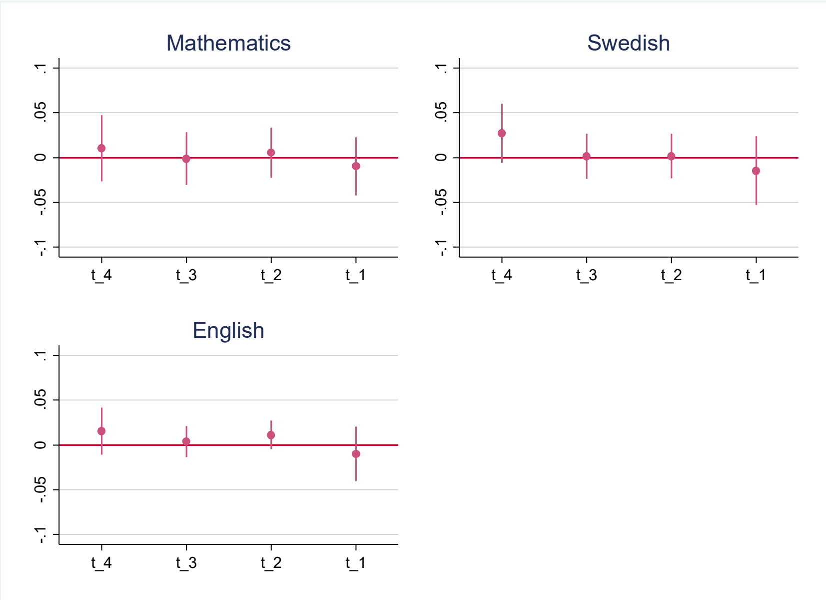
Notes: Students' test results are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects. Col. (2)–(4) additionally control for sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings as well as missing data on parental earnings and/or education. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

6.2 Placebo analyses and robustness checks

Our empirical strategy relies on the assumption that trends in student performance would not differ systematically between schools that introduce 1:1 technology and schools that have not yet introduced 1:1, in the absence of these initiatives. This means that we need to assume that schools generally do not choose to adopt 1:1 technology in response to improvements or deteriorations of student performance. To assess the credibility of this assumption, we investigate if there are differences in trends between schools that launch 1:1 programs, and schools that have not yet launched such a program, *before* the programs started. We do this by performing placebo tests: We estimate our preferred model specification (Table 2, col. 4), but (artificially) set the start date of the program to one, two, three, and four years before the actual start date. To make sure that the placebo-estimates do not pick up effects of actual 1:1 initiatives, all students that were affected by the actual initiatives are excluded from these regressions.

Figure 4, which shows the results from this exercise, gives us no reason to suspect that schools would choose to introduce 1:1 technology as a response to changes in student performance. All placebo estimates for the three years preceding the initiation of 1:1 are small in magnitude and far from being statistically significant. For year t-4, the estimate for Swedish is somewhat larger and just marginally insignificant (p-value: 0.104). However, as this is relatively far back in time given the context, we do not think this should cause much concern.

Figure 4 Placebo estimates



Notes: Placebo estimates with 95% confidence intervals. The model estimated is the same as in Table 2, col. 4, but where the treatment is (artificially) defined to have taken place one year (t_1), two years (t_2), three years (t_3) and four years (t_4) before actual program start. Students' test results are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects as well as sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings, missing data on parental earnings or education, and 3rd grade test scores. Standard errors are robust and clustered on schools.

To examine if there are differential compositional changes in observed characteristics among schools that implement 1:1 technology and schools that have not yet launched such an initiative, we perform balance tests where we regress several pre-determined characteristics (each parent's education and earnings, immigrant background, age, sex, and 3rd grade test scores) on the treatment variable, controlling for school and cohort fixed effects. The overall impression from these results, which are presented in Table 3, is that differential changes in student composition should not pose any major problem for our analyses. Most estimates are small in size and not statistically significantly different from zero. For two variables, we find statistically significant relationships: the father's earnings and having two foreign-born parents. But in terms of magnitude these relationships are negligible. The estimates imply that one additional semester of 1:1 is associated with a decrease of the father's earnings by 1 percent (-3,518/316,955) and a 2 percent increase in the probability of having two foreign born parents (0.005/0.234). One could also note that the point estimates in the main table (Table 2) stay very similar when individual background controls are included in the model, which is reassuring.

Table 3 Balance tests

	(1) Father, post-sec. education	(2) Mother, post-sec. education	(3) Father, earnings	(4) Mother, earnings	(5) Born abroad	(6) Parents born abroad
Number of semesters with 1:1 program	0.002 (0.003)	-0.002 (0.003)	-3 517.880** (1 555.558)	-55.672 (1 255.589)	0.002 (0.002)	0.005*** (0.002)
Observations	56,862	56,862	56,862	56,862	56,320	55,773
R-squared	0.113	0.128	0.147	0.167	0.083	0.377
Outcome mean	0.359	0.449	316 954.5	218 656.5	0.092	0.234

	(7) Female	(8) Older than classmates	(9) Younger than classmates	(10) 3rd grade test scores math	(11) 3rd grade test scores Swedish
Number of semesters with 1:1 program	-0.001 (0.003)	-0.001 (0.001)	-0.000 (0.001)	0.002 (0.013)	0.005 (0.012)
Observations	56,862	56,320	56,320	46,960	47,145
R-squared	0.010	0.026	0.030	0.132	0.110
Outcome mean	0.491	0.027	0.014	-0.060	-0.044

Notes: All regressions control for school and cohort fixed effects. Robust standard errors in parentheses, clustered on schools.*** p<0.01, ** p<0.05, * p<0.1.

We have also investigated to what extent our results are affected by certain sample restrictions. In Table A 2, column (2), we show that we get similar results if the sample also includes pupils who were given a personal laptop or tablet already in the beginning of 4th grade. The estimate for Swedish is slightly smaller for this sample, while the estimates for math and English remain close to zero. Column (3) of Table A 2 shows that the results also remain similar 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).

Last, we have performed sensitivity analyses to address a concern recently raised in the methodological literature regarding this type of staggered difference-in-differences models, sometimes referred to as two-way fixed effects models (Goodman-Bacon 2018; de Chaisemartin and D'Haultfœuille 2020). de Chaisemartin and D'Haultfœuille (2020) show that this type of model sometimes fails to identify meaningful treatment effects when treatment effects are heterogenous. The underlying problem is that already treated units (here schools) to some extent also will be used as controls in the analysis. This does not necessarily constitute an important problem in our application as our sample includes a relatively large proportion (52 percent) of never-treated schools (Baker, Larcker, and Wang 2022). To investigate if this issue poses a problem, we have performed analyses using an alternative difference-in-differences technique developed by Callaway and Sant'Anna (2021). This alternative estimator (henceforth referred to as the 'CS estimator') circumvents the problem of inappropriate controls by ensuring that only never-treated or not-yet-treated units are used as controls.[24]

Table 4 reports the results from this exercise. Since the CS estimator is developed for binary treatment variables and is most straightforward to implement based on panel data, we begin by showing results based on our original estimation strategy when data is aggregated to a panel of schools and for both our preferred treatment variable (i.e. number of semesters exposed to a 1:1 program) and a binary treatment variable indicating if the students were ever exposed to a 1:1 program.[25] The overall conclusions from our main table (Table 2) remain the same also for these model specifications; see columns (1) and (2) of Table 4. The last two columns show results based on the CS estimator. Column (3) uses only never-treated schools as controls, while all not-yet-treated schools are used as controls in column (4). The point estimate for Swedish is similar in size for these specifications, while the estimates for math and English are larger; hence, suggesting that the inclusion of already treated schools may bias these estimates downwards. However, all estimated treatment effects remain statistically insignificant. Thus, again we find no evidence suggesting that 1:1 technology impacted students' learning outcomes on average.

[24] The method first estimates cohort-time-specific treatment effects, allowing treatment effects to be heterogenous. These treatment effects are then aggregated to produce an estimate of the average effect of the treatment for the treated units; see Callaway and Sant'Anna (2021) for details. The method is implemented using the Stata package *csdid*; see Rios-Avila, Sant'Anna, and Callaway (2021).

[25] Moreover, since the CS estimator assumes that units that have been treated at some point always remain treated, we have also removed observations where the schools stopped providing individual computers to the pupils from these analyses. This only concerns 2 percent of the observations, and the results do not change based on whether these observations are included or not.

Table 4 Results based on Callaway and Sant'Anna's (2021) difference-in-differences estimator

	(1) Original model (TWFE)	(2) Original model (TWFE)	(3) CS estimator Controls: never-treated	(4) CS estimator Controls: all not-yet-treated
Mathematics				
No of semesters with 1:1 program	0.006			
	(0.010)			
Ever exposed to 1:1 program		0.007	0.053	0.051
		(0.031)	(0.038)	(0.038)
Observations	1,653	1,653	1,533	1,533
Swedish				
No of semesters with 1:1 program	0.019*			
	(0.011)			
Ever exposed to 1:1 program		0.031	0.026	0.022
		(0.032)	(0.036)	(0.036)
Observations	1,653	1,653	1,533	1,533
English				
No of semesters with 1:1 program	0.009			
	(0.011)			
Ever exposed to 1:1 program		0.010	0.041	0.040
		(0.033)	(0.041)	(0.041)
Observations	1,652	1,652	1,532	1,532
Covariates included	No	No	No	No

Notes: Column (1) and (2) show results from our original estimation strategy, i.e. a two-way fixed effects model (TWFE) that includes time- as well as school fixed effects. Column (3) and (4) show results from the difference-in-difference estimator developed by Callaway and Sant'Anna (2021). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

6.3 Heterogenous effects

Average effects may of course hide important heterogeneity between subgroups of students. It is often anticipated that schools' investments in digital technology will contribute to greater equality between students of different socio-economic background by ensuring that all students have access to computers (e.g. Swedish Government 2017; 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 in general (see, e.g., Swedish Government 2017). Theoretically, however, it is not clear what effects we should expect on equality in school performance. Many of the benefits that potentially exist with more technology in education – such as more individualized learning and increased motivation – have the potential of benefitting all students. If low-SES students 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. The results in Hall, Lundin, and Sibbmark (2021) for Swedish secondary schools point in this direction.

In Table 5 we estimate effects of 1:1 technology on student performance in 6th grade separately by parents' level of education (which we regard as an indicator of socio-economic background). We use our preferred model specification, which includes the full set of control variables (i.e., the same as in Table 2, column 4).

Table 5 Effects of 1:1 technology on student performance, by parents' level of education

	(1) Mathematics	(2) Swedish	(3) English
A. Parents have high level of education			
No of semesters with 1:1 program	0.005	0.015*	0.012*
	(0.008)	(0.009)	(0.007)
Number of observations	25,680	25,615	25,558
R-squared	0.416	0.361	0.269
B. Parents have low level of education			
No of semesters with 1:1 program	0.003	0.014	-0.013
	(0.015)	(0.015)	(0.014)
Number of observations	18,289	18,388	18,282
R-squared	0.377	0.360	0.272

Notes: 'High level of education' is defined as at least one of the parents having post-secondary education, and 'low level of education' as none of the parents having post-secondary education. Students' results on the national tests are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects as well as sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings, missing data on parental earnings or education, and 3rd grade test scores in mathematics/Swedish. Robust standard errors in parentheses, clustered on schools. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

The results in Panel A of Table 5 suggest positive effects on performance in Swedish and English among students with highly educated parents (defined as at least one of the parents having post-secondary education). These effects, which are statistically significant at the 10 percent level, are rather small in magnitude (cf. Kraft 2020), corresponding to increases by 0.012–0.015 of a standard deviation per semester of 1:1. However, it is not a negligible impact if we recognize that students on average had their computer (or tablet) for 2.5 semesters. For students whose parents have a low level of education (defined as none of the parents having post-secondary education), the estimates point in different directions and are never statistically significant (see Panel B). Hence, we find no evidence that 1:1 technology impacts educational performance among low-SES students. In Table A 3 in Appendix, we show estimates using the alternative difference-in-difference estimator developed by Callaway and Sant'Anna (2021). The results are in line with those presented in Table 5: For students with highly educated parents, the point estimates are positive and almost always statistically significant. For students whose parents have a low level of education, the point estimates are either close to zero or negative, and never statistically significant. Hence, overall, our results suggest that 1:1 technology to some extent contributes to increased inequality in student performance by socio-economic background, by primarily benefitting high-SES students.

We have also investigated if the effects differ by gender or depending on if the school chooses to use laptops or tablets. The estimates are largely similar for boys and girls; see Table A 4 in Appendix. In line with the results in Hall, Lundin, and Sibbmark (2021) we find some indications of more positive effects for laptops compared to tablets; see Table A 5 in Appendix.[26]

6.4 The importance of supplemental financial resources

Last, we investigate if the effects differ depending on if the schools have received additional funding for investments in 1:1 technology or if they have financed these initiatives from their ordinary budget, for example, by reducing the teacher/student ratio or the spending on traditional teaching materials. We should expect more positive effects when 1:1 initiatives come with additional funding, compared to when schools have to cut back on something else.

Around half of the schools (129 schools) that responded to our survey answered the question about supplemental funding earmarked for student computers. 83 of these schools (64 percent) answered that they had, at least at some point, received additional funding for this purpose and, out of these, 60 schools (72 percent) had introduced 1:1 technology during our study period. Table 6 displays effects of 1:1 separately for schools that had received supplemental resources (Panel A) and for schools responding “no” to this question (Panel B). The point estimates are consistently positive for schools receiving additional funding, and negative for schools that fully funded their 1:1 initiative out of their ordinary budget. While this pattern is in line with expectations, most effects are imprecisely estimated and not statistically significant. For Swedish, however, there is a statistically significant positive effect for schools receiving supplemental funding (p-value: 0.055), corresponding to 0.029 of a standard deviation. Taking into account that students on average had their computer for 2.5 semester, this can be considered a small-medium size effect (cf. Kraft 2020). Hence, if accompanied with additional financial resources, 1:1 technology seems to produce some positive effects. However, this does not mean that an alternative use of these extra financial resources could not have generated even more favorable outcomes.[27]

[26] We have also performed similar heterogeneity analyses using the CS estimator. The tendency of a slightly larger positive effect for girls in Swedish is not visible in these analyses. The positive estimates for laptops get stronger if we use the CS estimator. These results are available from the authors. When it comes to tablets, there are too few school level observations to be able to carry out a similar analysis.

[27] It was not possible to validate the differential patterns for schools with and without additional resources using the CS estimator (due to too few school level observations in the group without additional financial resources). Hence, some caution should be applied in the interpretation of the results presented in this section.

A further division into sub-groups shows that the positive impact in schools receiving additional funding is driven by high-SES students (see Table A 6). For low-SES students there is no significant impact. Hence, despite the additional funds, 1:1 technology does not seem to have benefited student with socio-economically weaker backgrounds.

Table 6 Effects of 1:1 technology on student performance. Separate effects depending on if the school has received additional financial resources

	(1) Mathematics	(2) Swedish	(3) English
A. Schools with supplemental resources			
No of semesters with 1:1 program	0.017	0.029*	0.005
	(0.015)	(0.015)	(0.014)
Number of observations	14,627	14,677	14,622
R-squared	0.449	0.405	0.303
B. Schools without supplemental resources			
No of semesters with 1:1 program	-0.012	-0.011	-0.023
	(0.018)	(0.019)	(0.017)
Number of observations	9,026	9,046	8,979
R-squared	0.430	0.355	0.292

Notes: Students' results on the national tests are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects as well as sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings, missing data on parental earnings or education, and 3rd grade test scores in mathematics/Swedish. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

7 Concluding discussion

Children's learning environments are becoming increasingly technologically advanced. Worldwide, a growing number of schools provide each student with a personal computer to facilitate and increase the use of technology in teaching. The school closures and shifts to remote instruction during the COVID-19 pandemic accelerated this development, and 1:1 computer programs are now common not only among older pupils, but also in primary school. Few would argue that computers should not at all be utilized in education in a modern society, but the extent, the way and for whom technology is best used can be debated. In the wake of the pandemic, an urgent task for policymakers and school leaders is to decide whether it is a good idea to keep investing in 1:1 technology. In this article, we contribute to this discussion by providing evidence on how 1:1 computer programs in primary school (grades 4–6) in Sweden affect learning outcomes in terms of performance on standardized tests in mathematics, Swedish and English. By surveying all schools in 26 municipalities regarding the implementation of 1:1-programs during a 12-year-period (2009–2020) and linking these data to administrative records, we estimate effects on student performance using a staggered difference-in-differences design.

We find no evidence suggesting that 1:1 technology, in comparison to more limited computer use, have an important impact on student performance in language and mathematics on average. These findings are in line with the results in most of the few prior studies that provide credible evidence on the impact of 1:1 on school performance, although most of these studies focus on older students. There is also a more general discussion in the literature on the effects of ICT investments on student performance. Recent review articles conclude that investments in ICT that merely improve access to technology without having a distinct educational purpose often seem to have limited effects on learning outcomes (Escueta et al. 2020; Haelermans 2017). Our results can be interpreted as aligning with this literature as well.

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 pupils (Escueta et al. 2020). Hall, Lundin, and Sibbmark (2021) examine how 1:1 technology affects learning outcomes in lower secondary school (age 13–15) in the same Swedish municipalities, during a similar time period, and using the same empirical design as we do. It is thus possible to make a rather direct comparison of our results for primary school students (age 10–12) with the results for somewhat older students. None of the studies find any clear indication that 1:1 technology affects student performance on average, suggesting that impacts are similar for these age groups. Of course, increased use of ICT in education may have other 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.

Hall, Lundin, and Sibbmark (2021) identify some negative effects of 1:1 programs on performance among students with less educated parents. This suggests that inequality in school performance in the socioeconomic dimension is amplified by 1:1 technology. In the present study of younger pupils, we also find evidence of heterogeneity in impacts by socioeconomic background. For the younger students, however, there are positive effects on performance in language (Swedish and English) among students with highly educated parents. Hence, the findings can be interpreted in a more positive way, but precisely as in Hall, Lundin, and Sibbmark (2021) at the cost of increased differences in educational performance by socioeconomic background.

Effects of 1:1 initiatives can be expected to differ depending on if the schools have received additional funding for these initiatives or not, an issue that has often been neglected in the literature (Bulman and Fairlie 2016). Although we do not have detailed information on how the 1:1 programs included in our sample have been financed, our data includes an indicator of whether the schools have received additional funding on top of their ordinary budget for these investments. Our results confirm that this type of heterogeneity is crucial to consider: In line with expectations, we find more positive effects when 1:1 initiatives have been accompanied with supplementary financial resources. Most notably, there is a positive and statistically significant effect on performance in the Swedish language in these cases. For schools were the 1:1 initiatives have been fully financed by cuts in other school spendings, there are no statistically significant effects. This finding emphasizes that a further understanding of the impact of technology in teaching, requires that we do not only address how the technology is used, but also the extent to which investments in technology crowds out other types of costs.

A last thing to note is that the few positive and significant estimates that we find in subgroups tend to be related to how students perform in language, most notably in Swedish. This is also the subject where computers are used the most, according to survey results (see Section 3). It is hard to know whether this means that 1:1 technology is more useful in language studies or if the findings can be explained by the fact that computers are used less often in mathematics. This is something for future studies to explore.

Sweden is one of the forerunners when it comes to computer access and incorporating digital technology in education (European Commission 2019) from which other countries can learn. This is valuable in the aftermath of the COVID-19 pandemic when schools across the world have returned to in-person instruction, but with increased computer access. The main message from our research is that 1:1 technology is unlikely to have a large positive or negative impact on learning for primary school children, but if coupled with additional funding some positive effects may exist, at least for high-SES students.

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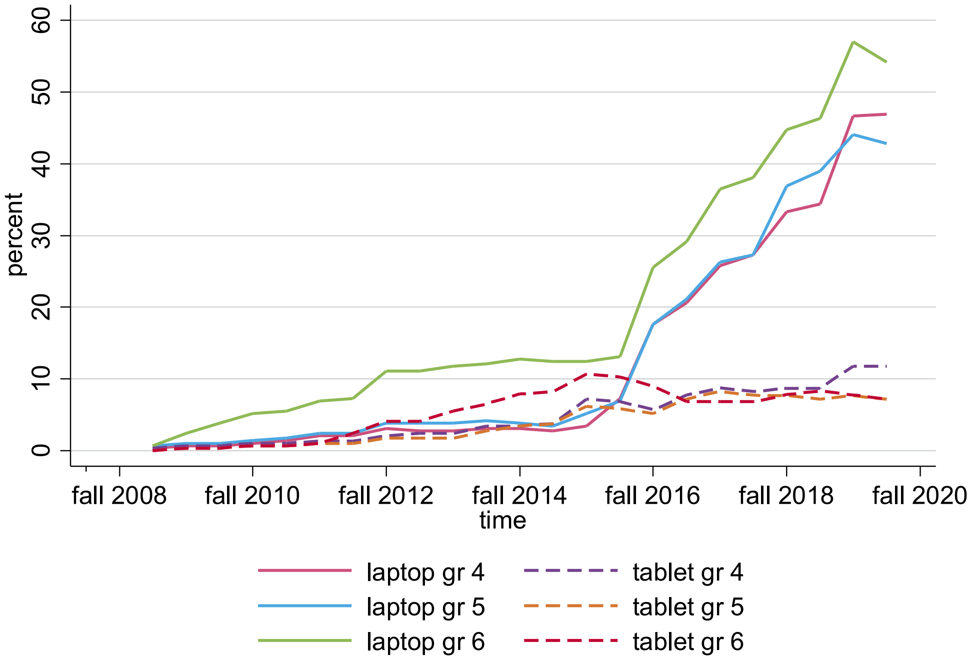
Appendix: Additional figures and tables

Figure A 1 Percent schools providing personal laptops vs. tablets among the schools that responded to the survey



Notes: Numbers for 2009–2016 are calculated among the 293 schools that responed 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 A 1 Comparison of students that attend schools that later introduce 1:1 technology and schools that do not. Comparison of grade 4 students in 2009.

	Attends a school that later introduces 1:1 technology
Female	0.002
	(0.010)
Both parents are born abroad	0.021
	(0.039)
Born abroad	-0.045*
	(0.023)
At least one parent has post-secondary education	-0.001
	(0.017)
One year younger than classmates	0.070
	(0.059)
One year (or more) older than classmates	-0.044
	(0.066)
Enrolled in an independent school	-0.052
	(0.113)
Number of observations	7,471
R^2	0.436

Notes: OLS estimates. The regression controls for municipality fixed effects. Robust standard errors in parentheses. Standard errors are clustered on schools. *** p<0.01, ** p<0.05, * p<0.1

Table A 2 Effects of 1:1 technology on student performance on standardized test. Robustness checks

	(1) Main results (Table 2, col. 4)	(2) Incl. pupils who were treated in the beginning of 4th grade	(3) Only incl. schools that answered both surveys
A. Mathematics			
No. of semesters with 1:1 program	0.004	0.002	0.004
	(0.009)	(0.008)	(0.009)
Observations	43 969	48 387	35 883
R-squared	0.445	0.452	0.445
B. Swedish			
No. of semesters with 1:1 program	0.015	0.011	0.013
	(0.009)	(0.007)	(0.009)
Observations	44 003	48 419	35 934
R-squared	0.398	0.404	0.397
C. English			
No. of semesters with 1:1 program	0.002	0.004	0.003
	(0.008)	(0.007)	(0.008)
Observations	43 840	48 243	35 761
R-squared	0.310	0.316	0.307
Demographic controls	Yes	Yes	Yes
Control for prior test scores	Yes	Yes	Yes

Notes: Students' test results are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects as well as the following covariates: sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings, missing data on parental earnings and/or education and results on 3rd grade national standardized tests in math or language. Robust standard errors in parentheses, clustered on schools. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A 3 Effects of 1:1 technology by parents' level of education. Results based on Callaway and Sant'Anna's (2021) difference-in-differences estimator.

	(1) Original model (TWFE)	(2) Original model (TWFE)	(3) CS estimator Controls: never-treated	(4) CS estimator Controls: all not-yet-treated
A. Parents have high level of education				
Outcome: Mathematics				
No of semesters with 1:1 program	0.021*			
	(0.012)			
Ever exposed to 1:1 program		0.057	0.117**	0.112**
		(0.036)	(0.049)	(0.049)
Outcome: Swedish				
No of semesters with 1:1 program	0.033**			
	(0.014)			
Ever exposed to 1:1 program		0.099***	0.102**	0.096*
		(0.038)	(0.050)	(0.050)
Outcome: English				
No of semesters with 1:1 program	0.021*			
	(0.013)			
Ever exposed to 1:1 program		0.061	0.081*	0.066
		(0.039)	(0.045)	(0.044)
Number of observations	1,637–1,641	1,637–1,641	1,515–1,521	1,515–1,521
B. Parents have low level of education				
Outcome: Mathematics				
No of semesters with 1:1 program	-0.000			
	(0.016)			
Ever exposed to 1:1 program		-0.031	0.004	0.008
		(0.044)	(0.059)	(0.060)
Outcome: Swedish				
No of semesters with 1:1 program	0.009			
	(0.016)			
Ever exposed to 1:1 program		-0.016	-0.038	-0.046
		(0.047)	(0.070)	(0.071)
Outcome: English				
No of semesters with 1:1 program	-0.003			
	(0.016)			
Ever exposed to 1:1 program		-0.045	-0.007	-0.001
		(0.047)	(0.061)	(0.063)
Number of observations	1,640–1,641	1,640–1,641	1,521–1,522	1,521–1,522
Covariates included	No	No	No	No

Notes: 'High level of education' is defined as at least one of the parents having post-secondary education, and 'low level of education' as none of the parents having post-secondary education. Students' test results are standardized within cohort to have mean 0 and a standard deviation 1. All models are estimated based on aggregated school level data. Column (1) and (2) show results from our original estimation strategy, i.e., a two-way fixed effects model (TWFE) that includes time- as well as school fixed effects. Column (3) and (4) show results from the difference-in-difference estimator developed by Callaway and Sant'Anna (2021). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A 4 Effects of 1:1 technology on student performance, by gender

	(1) Mathematics	(2) Swedish	(3) English
A. Girls			
No of semesters with 1:1 program	0.005	0.018*	0.000
	(0.011)	(0.011)	(0.010)
Number of observations	21,780	21,895	21,706
R-squared	0.458	0.390	0.322
B. Boys			
No of semesters with 1:1 program	0.005	0.013	0.004
	(0.010)	(0.010)	(0.009)
Number of observations	22,189	22,108	22,134
R-squared	0.443	0.347	0.316

Notes: Students' results on the national tests are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects as well as sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings, missing data on parental earnings or education, and 3rd grade test scores. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Table A 5 Effects of 1:1 laptop vs. tablet programs on student performance

	(1) Mathematics	(2) Swedish	(3) English
No of semesters with personal laptop	0.002	0.016*	0.006
	(0.010)	(0.009)	(0.009)
No of semesters with personal tablet	0.014	0.010	-0.016
	(0.016)	(0.024)	(0.014)
Number of observations	43 969	44 003	43 840
R-squared	0.445	0.398	0.311

Notes: Students' results on the national tests are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects as well as sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings, missing data on parental earnings or education, and 3rd grade test scores in mathematics/Swedish. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Table A 6 Effects of 1:1 technology on student performance, by parents' level of education. Separate analyses depending on if the school has received additional financial resources

	(1) Mathematics	(2) Swedish	(3) English
A. Schools with supplemental resources			
A1. Parents have high level of education			
No of semesters with 1:1 program	0.029**	0.036**	0.018
	(0.014)	(0.015)	(0.013)
Number of observations	8,062	8,050	8,036
R-squared	0.417	0.381	0.263
A2. Parents have low level of education			
No of semesters with 1:1 program	0.005	0.022	-0.008
	(0.021)	(0.020)	(0.019)
Number of observations	6,565	6,627	6,586
R-squared	0.378	0.357	0.264
B. Schools without supplemental resources			
B1. Parents have high level of education			
No of semesters with 1:1 program	-0.015	-0.014	-0.021
	(0.017)	(0.022)	(0.015)
Number of observations	5,817	5,786	5,743
R-squared	0.398	0.323	0.266
B2. Parents have low level of education			
No of semesters with 1:1 program	-0.003	-0.005	-0.035
	(0.039)	(0.029)	(0.032)
Number of observations	3,209	3,260	3,236
R-squared	0.354	0.321	0.248

Notes: 'High level of education' is defined as at least one of the parents having post-secondary education, and 'low level of education' as none of the parents having post-secondary education. Students' results on the national tests are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects as well as sex, age, foreign born, foreign born parents, father's education (3 categories), mother's education (3 categories), father's earnings, mother's earnings, missing data on parental earnings or education, and 3rd grade test scores in mathematics/Swedish. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.