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A laptop for every child?

The impact of ICT on educational outcomes

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IFAU INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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A laptop for every child? The impact of ICT on educational outcomes[a]

by

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Abstract

Classrooms all over the world are becoming increasingly technologically advanced. Many schools today provide a personal laptop or tablet to each pupil for use both in the classroom and at home. The intent of these 1:1 programs is that information and communication technology (ICT) should be extensively involved in the teaching of all subjects. We investigate how pupils who are given a personal laptop or tablet, rather than having more limited computer access, are affected in terms of educational performance. By surveying schools in 26 Swedish municipalities regarding the implementation of 1:1 programs and combining this information with administrative data, we estimate the impact on educational outcomes using a difference-in-differences design. We find no significant impact on standardized tests in mathematics or language on average, nor do we find an impact on the probability of being admitted to upper secondary school or the students' choice of educational track. However, our results indicate that 1:1 initiatives may increase inequality in education by worsening math skills and decreasing enrollment in college-preparatory programs in upper secondary school among students with lower educated parents.

Keywords: technology, computers, one-to-one, student performance
JEL-codes: I21

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1 Introduction

Classrooms around the world are becoming more and more technologically advanced. A growing trend is for schools to provide a personal laptop or tablet to each pupil for use both in the classroom and at home. The idea behind these one-to-one computer programs (or 1:1 programs) is that information and communication technology (ICT) should be extensively involved in the teaching of all subjects. The aim is partly to improve ICT skills, but the ultimate goal is that these initiatives will enhance learning in general (Islam and Grönlund 2016). Despite their increasing importance, there is little credible evidence on the causal impact of these programs on students' educational outcomes, especially from high income countries.

We investigate how pupils who are given a personal laptop or tablet, rather than having more limited computer access, are affected in terms of performance on standardized tests in mathematics and language at the end of compulsory school. In addition, we examine how 1:1 programs affect students' progression to a higher level of education. By surveying all lower secondary schools in 26 Swedish municipalities regarding the implementation of 1:1 programs and combining this information with administrative data, we estimate the impact on educational outcomes using a difference-in-differences design. We compare how educational outcomes change across cohorts for schools that launch 1:1 programs, to changes for schools that have not yet introduced such programs.

The theoretical implications of schools' investments in ICT on student performance are ambiguous: The expenditure a school devotes to ICT will unavoidably come at the expense of other inputs that are likely to affect learning (e.g. the number of teachers or books), and which may be more or less efficient. Similarly, the time that students devote to using technology may come at the expense of other educational activities, which again may be more or less efficient for learning (Bulman and Fairlie 2016).

Several previous studies have estimated the effects of investments in ICT on student achievement using credible empirical strategies (e.g. Angrist and Lavy 2002; Leuven et al. 2007; Banerjee et al. 2007; Machin, McNally and Silva 2007), with very mixed findings. We review the previous literature in Section 2. However, it is uncertain to what extent the findings from this strand of the literature can be generalized to 1:1 programs, as 1:1 initiatives most likely imply a much more intensive use of ICT in the classroom (as well as at home) compared to the initiatives studied in these papers (Hull and Duch 2018).[5]

Some studies have attempted to estimate effects of 1:1 programs on student outcomes, but the majority are plagued with methodological shortcomings (Hull and Duch 2018; Islam and Grönlund 2016; Zheng et al. 2016). Hitherto, two papers provide more reliable evidence on the impact of 1:1 initiatives. Cristia et al. (2012) study a randomized experiment with a 1:1 program in poor regions of rural Peru and find positive effects on students' general cognitive skills, but no significant impact on test scores in mathematics or language. It is hard to know whether the findings from this context are relevant for schools in developed countries. For instance, very few of the schools in the sample had access to the internet, which puts major constraints on how the technology could be used. De Melo, Machado and Miranda (2014) provide evidence from a setting that bears somewhat more similarities with ours. They study the national implementation of a 1:1 program in primary schools in Uruguay, exploiting the gradual introduction of the program in order to identify its effects. They also do not find any effects on math or reading scores. However, they also show that the computers were not used that frequently in the classroom. In contrast, survey evidence from the National Agency for Education shows that computers and tablets are highly involved in the teaching in Swedish 1:1 programs (see Section 3).

We contribute to the literature by providing one of the first pieces of evidence on the impact of 1:1 programs from a high income country. Compared to De Melo, Machado and Miranda (2014) we study outcomes at higher ages (lower secondary rather than primary school). While earlier 1:1 studies have focused on short-term impacts (1–2 years) on tests scores, we can follow the students somewhat longer and also examine impacts on their progression to a higher level of education. Specifically, we investigate if exposure to 1:1 programs affects whether students are admitted to upper secondary school and what type of track they enroll in (academic or vocational). This allows us to also capture effects on a broader set of skills compared to merely examining test scores.

Moreover, it is proposed in the literature that 1:1 programs may reduce social inequity by providing computers to pupils with low socio-economic status (SES) (e.g. Zheng et al.

[5] For instance, Banerjee et al. (2007) study a specific computer-assisted learning program offered to students two hours per week, and the targeted student-computer ratio in the intervention studied by Angrist and Lavy (2002) was 10:1.

2016). But, to our knowledge, there is no rigorous empirical support for this claim. Access to data on parental background, allows us to directly examine whether 1:1 programs reduce the gap in educational performance between students with different SES backgrounds.

Furthermore, the rich data we have at hand allow us to perform a number of additional analyses to shed light on some potential mechanisms behind our findings: We examine if the costs associated with introducing 1:1 programs have led to larger classes (a proxy for higher student-teacher ratios), and if these programs have affected the sorting of teachers across schools. We also examine if the effects on student performance differ depending on the type of technology used (laptops vs. tablets).

We find no evidence suggesting that 1:1 programs impact average student performance on the standardized tests, the probability of being admitted to upper secondary school, or the choice of educational track. We show that absence of positive impacts is unlikely to be explained by lower teacher-student ratios or changes in the composition of teachers employed. However, our analysis suggests that 1:1 initiatives may increase inequality in education by worsening math skills and decreasing enrollment college-preparatory programs in upper secondary school among students with lower educated parents. We also find some indication that the impact of tablet programs is worse than the impact of laptop programs.

The rest of the paper is outlined as follows. We start by reviewing the literature on the effects of ICT in education in Section 2. In the subsequent section we describe the Swedish education system and the role of ICT in Swedish schools. Section 4 presents the data, and Section 5 discusses the empirical strategy. In Section 6 we present our results. Section 7 concludes.

2 Previous literature

A good theoretical starting point for an analysis of how students are affected by 1:1 computer initiatives is a standard model of educational production (e.g. Hanushek 1986). A student’s academic achievement is assumed to be a function of individual characteristics, home environment and earlier achievement, as well as expenditures and time allocated to different teaching methods. Within the limitations of the budget and available instructional time, schools determine the scope for ICT investments (such as 1:1 programs) relative other uses of resources and methods of teaching.

Providing students with a personal laptop or tablet could be a strategy to improve learning processes, and there are several mechanisms through which 1:1 programs may enhance student achievement (e.g. Bulman and Ferlie 2016; Haelermans 2017; Islam and Grönlund 2016; Zheng et al. 2016). First, learning may become more individualized to suit the strengths and weaknesses of the individual pupil. For instance, computer software can provide self-paced instruction that can be hard to achieve in traditional learning environments. Second, use of ICT could increase student motivation. This can potentially be achieved through interactive teaching methods, which are easier to use in a 1:1 environment. Third, computers and the internet provide an opportunity for students to access more and better information which can stimulate learning. Lastly, the possibilities for communication and coordination are likely to be improved, for example, among students, between students and teachers, and between teachers and parents.

At the same time, investments in ICT will come at the expense of investments in other key factors, such as the number of teachers hired[6] and time devoted to traditional methods of instruction (Bulman and Ferlie 2016). It is well-known that 1:1 programs require large investments in infrastructure, support and training (Grönlund 2014). Studies have also shown that there are implementation challenges associated with these initiatives (Chatterji 2018). For instance, it is difficult to change the educational paradigm and secure teachers' commitment to the new technology (Haelermans 2017; Islam and Grönlund 2016). Moreover, there are possible negative side effects associated with technology in the classroom. Laptops and tablets might, for example, be used for other purposes than what the teacher intended, such as for playing games or using social media. Empirical studies demonstrate that computers in the classroom can imply an element of distraction decreasing student performance (Beland and Murphy 2016; Carter, Greenberg and Walker 2017; Sana, Tina and Cepeda 2013). In addition, experimental evidence suggests that students using pen and paper for taking notes perform better than pupils using laptops. A potential interpretation of these results is that writing by hand implies that students have to process information and condense the content of the lecture rather than just

[6] Research shows that larger classes decreases student performance and the impact tends to be largest among children with lower socioeconomic status (see e.g. Angrist and Lavy 1999; Krueger and Whitmore 2001; Fredriksson, Oosterbeck and Öckert 2013, 2016).

transcribing lectures verbatim. The pen and paper approach could therefore mean a cognitive encoding of the content leading to enhanced learning (Mueller and Oppenheimer 2014). To conclude, it is an empirical question whether 1:1 programs are superior to other ways of using financial resources and time in school.

The trend towards more technology in education has generated quite a lot of empirical research on the impact of ICT on student performance. The findings are mixed: For instance, Goolsbee and Guryan (2006) and Beuremann et al. (2015) find no impact of increased use of ICT on student achievement. Mainly negative effects are identified by Angrist and Lavy (2002) and Leuven et al. (2007). On the other hand, Banerjee et al. (2007), Machin, McNally and Silva (2007) find positive effects. Haelermans (2017) summarizes the economic literature and concludes that general investments in ICT without a distinct purpose and plan rarely provide positive results.[7] If technology is used with a clear aim, the findings can be more positive: there are examples of studies finding positive effects of specific digital learning tools on test scores in mathematics and language.[8] It is important to point out that the literature often estimates effects of supplemental resources earmarked for ICT and/or increased instructional time. Hence, the estimates tend to reflect whether ICT can have a positive impact on student performance in the absence of constraints in these dimensions (Bulman and Fairlie 2016).

The empirical literature specifically on effects of 1:1 programs is mainly dominated by other scholars than economists. The methods used in order to isolate causal effects are usually not that reliable.[9] In fact, Islam and Grönlund (2016) argue in a recent overview that "overall there is not much research into causal relations between interventions [in the form of 1:1 programs] and effects" (p. 193–194).[10] Many studies within this literature indicate a positive correlation between 1:1 programs and student performance. But there are also a number of studies showing mixed findings, no correlation or occasionally negative associations.[11]

As discussed in the introduction, few studies provide credible evidence on the causal effects of 1:1 programs using experimental or quasi-experimental designs. The two most

[7] See also the overview in Bulman and Fairlie (2016).
[8] The literature on computer-assisted-learning programs is also surveyed in Escueta et al. (2017).
[9] Within this literature, there are attempts to construct comparison groups and control for selection by including covariates and/or by using some kind of basic difference-in-differences approach. It is fair to say that any causal statement based on these studies relies on strong assumptions.
[10] For a similar way of reasoning, see Zheng et al. (2016, p. 25).
[11] For overviews, see Islam and Grönlund (2016) and Zheng et al. (2016).

reliable studies so far are Crista et al. (2012) who study a randomized experiment with a 1:1 program in poor regions of rural Peru, and De Melo, Machado and Miranda (2014) who study the implementation of a 1:1 program in primary schools in Uruguay. Neither of these studies find any significant impact on test scores in mathematics or language. However, Crista et al. find a positive and sizable effect on a test of students' general cognitive skills. Another recent study worth mentioning is Hull and Duch (2018) examining a laptop program in one school district (only seven schools) in North Carolina using a difference-in-differences strategy. The analysis suggests no short-term impact on student achievement. In the medium term, however, there are indications of a positive impact on math scores.

Our study is one of the first to provide credible evidence on the impact of 1:1 programs from a high-income country. While the analysis by De Melo, Machado and Miranda (2014) and Hull and Duch (2018) bear some similarities with ours, Crista et al. (2012) study a very different context and it is hard to know to what extent their findings are relevant for schools in developed countries. For instance, hardly any of the schools in their sample had access to the internet, which severely limits how the technology could be used but also the potential sources of distractions in the classroom. Our study also extends the literature in various ways. For example, we investigate if progression to higher levels of education is affected, and if effects differ depending on the type of technology used.

An additional issue discussed in the literature is whether technology in education can reduce educational and social inequity (e.g. Zheng et al. 2016). It is sometimes suggested that 1:1 programs give low-SES students access to resources they do not have to the same extent as high-SES students. Increased use of technology in teaching may also imply more individualized learning and/or increase student motivation, which may be particularly important for pupils with a more disadvantaged background. On the other hand, these students may find it more difficult to take advantage of the technology due to less experience and help from their parents. We contribute to this discussion by investigating if the impact of 1:1 programs differ for low- and high-SES students.

3 The Swedish education system and the role of ICT in schools

Sweden has nine years of compulsory schooling, starting in the fall of the year the child turns seven. Traditionally, compulsory schooling has been divided into three stages (grades 1–3, 4–6 and 7–9), and schools were often organized as primary schools (grade 1–6) and lower secondary schools (grade 7–9). As a result of compulsory schooling being decentralized to the municipal level, the organization of schools has become more flexible in recent years, making also other grade configurations common. For instance, schools today are sometimes organized as grade 1–5 and grade 6–9 schools, or as grade 1–3 and grade 4–9 schools. A single school may also comprise all nine grades.

After nine years of schooling, almost all pupils move on to upper secondary education. While compulsory education has a comprehensive curriculum, upper secondary school consists of several different educational programs (both college preparatory and vocational[12]) to which individuals apply based on their 9th grade GPA. Pupils that have not attained eligibility for a regular upper secondary school program, have the possibility of enrolling in an introductory program where they can qualify for a regular program.

Formally, Sweden has rather far-reaching school choice: Families may choose any public or 'independent' (but publicly funded) school for their children.[13] However, since the admission rules to public schools for grades 1–9 are based on proximity to the school, it is still most common that pupils attend the nearest public school.[14] Independent schools may, on top of proximity, also base admission on a first-come-first-served basis, but they are not allowed to select pupils based on ability or other personal characteristics. In order to receive public funding, they are also not allowed to charge a tuition fee.[15]

The municipality is the responsible administrative body for organizing compulsory education, and local income taxes as well as central government grants constitute the main sources of finance. Each school has its own budget and the decision to invest in ICT, such as providing each pupil with an individual laptop or tablet, is usually made by the principal. However, general municipal initiatives, where financial resources are

[12] Students who enroll in vocational programs can also attain college eligibility by choosing certain optional courses.
[13] There are very few fully private schools in Sweden.
[14] Böhlmark, Holmlund and Lindahl (2016) approximate that just above 30 percent of the pupils in 2009 opted out from their assigned public school to either an independent school or a public school outside their catchment area.
[15] In the school year 2016/17, around 15 percent of the pupils in compulsory school attended an independent school (Swedish National Agency for Education 2017).

earmarked for 1:1 programs in some or all public schools in the municipality, exist as well.

ICT has become an increasingly important part of teaching in Sweden. For example, the number of students per computer decreased from 5.9 in 2008 to 1.8 in 2015 (Swedish National Agency for Education 2016a). Small-scale 1:1 programs were introduced for the first time around 2007/2008 (Tallvid 2015). In 2015, almost 30 percent of the students in compulsory school had access to a personal laptop or tablet provided by the school. This share was about the same in public and independent schools (Swedish National Agency for Education 2016a).

There is a lively debate in Sweden regarding the increased use of computers in education (see e.g. Lindgren 2011; Danielsson 2015; Thurfjell 2017). Some debaters refer to ICT in very favorable terms: 1:1 is almost regarded as the ultimate solution to the key challenges faced by the education system. Others have a much more skeptical view, highly emphasizing the downsides of increased use of ICT in the classroom. However, it is obvious that increased access to individual laptops or tablets is a rising trend in Swedish schools. The Swedish National Agency for Education recommends that all pupils, from first grade and upwards, be provided with a personal laptop or tablet (Swedish National Agency for Education 2016b). Moreover, the Swedish government has in a recent strategy declared the importance of securing good access to ICT for all pupils (Swedish Government 2017).

There are no studies of the causal impact of 1:1 programs on student achievement from Sweden. However, there are studies focusing on other questions. These studies are usually based on questionnaires or interviews, and they tend to show that pupils and (for the most time) teachers in 1:1 schools are positive toward the initiatives (Tallvid 2015). But there are also studies suggesting that practices in the classrooms are not altered that much (Molin and Lantz-Andersson 2016), that distraction increases (Hattaka, Andersson and Grönlund 2013), and that the use of computers and tablets varies quite a lot from school to school (Grönlund, Andersson and Wiklund 2014).

It is important to note that laptops and tablets are standard tools in essentially all Swedish schools today. However, a study by the National Agency for Education (2016a) shows that schools with 1:1 programs tend to incorporate ICT into their teaching to a much greater extent than other schools. Table 1 demonstrates that students with access to an individual laptop or tablet report a more frequent use of ICT in all subjects in comparison to other students (panel A). In addition, these pupils also report that they use computers for various school tasks to a greater extent (panel B).

Table 1 The use of ICT in Swedish schools in grade 7–9, 2015

	Pupils in 1:1 schools	Other pupils
A: Share (%) of pupils using computers/tablets in "all/almost all" or "most" classes, different subjects		
Swedish	66	28
Social science	63	27
English	53	21
Science	49	16
Mathematics	25	11
Art, Music, Needlework/Woodwork	20	11
P.E.	5	1
B: Share (%) of pupils using computers "always" or "often" for different tasks		
Search for information	91	71
Papers/assignments	87	60
Presentations	88	64
Work with pictures, sound, music and movies	56	35
Communication with teachers (outside class)	45	33
Cooperation with other pupils	51	38
Calculations, statistics, create graphs	33	21
Communication with people outside the school	59	53

Note: The table reports questionnaire responses from a survey with pupils. The selection of pupils was based on a random stratified cluster sample with around 2,600 students (58 % response rate).

Source: National Agency for Education (2016a).

4 Data and descriptive statistics

We have collected data on 1:1 programs from lower secondary schools (grades 7–9) in 26 Swedish municipalities, for the period 2008–2016. Our sample covers a variety of municipalities in terms of population size, average education level, and geographic location. In order to make sure our sample would include a significant proportion of schools that had implemented 1:1 programs, half of the municipalities were selected based on prior information indicating a relatively wide use of 1:1 programs; for example, due to municipality-wide initiatives. We describe the selection of municipalities in greater detail in Appendix A.

A short questionnaire was sent by email to all schools (299) with grades 7–9 in the selected municipalities, followed by phone calls to non-respondents. About 73 percent of the schools that were contacted responded to our questions, resulting in a sample of 219 schools.[16]

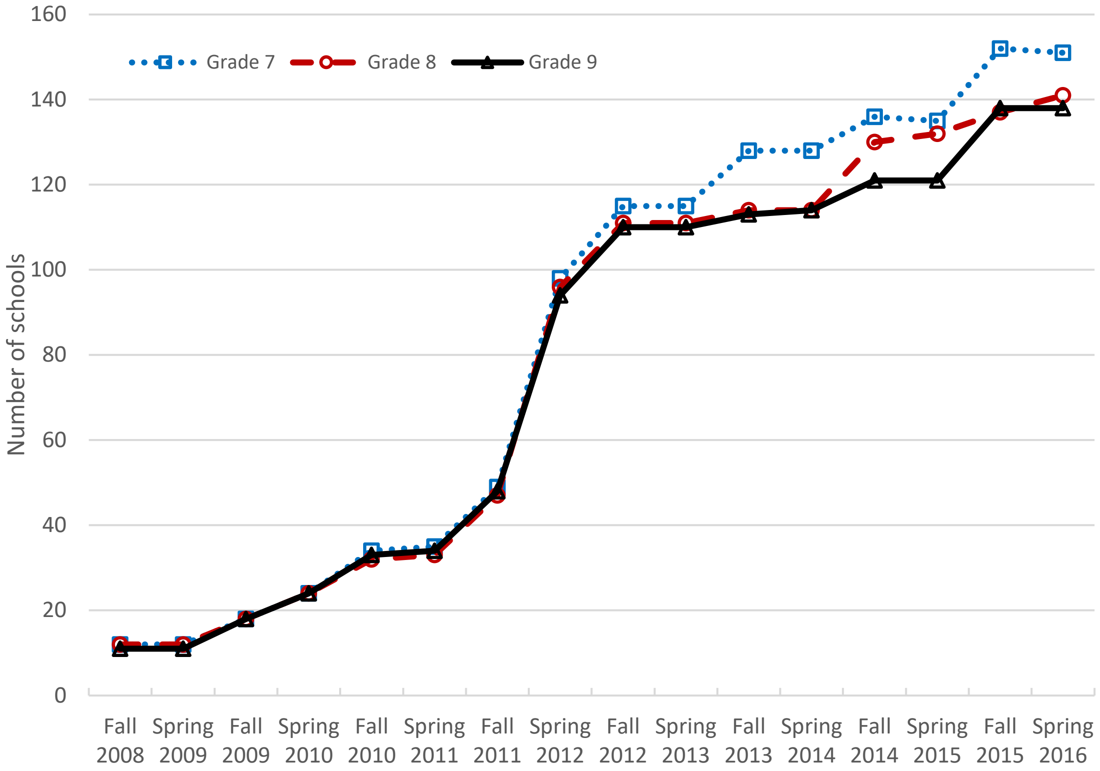
The schools were asked about the existence of a 1:1 program and, if present, when it was implemented and which grades were included at different points in time. They were also asked whether the pupils were provided with a laptop or a tablet, whether the school had any documented strategy for how ICT should be incorporated into the teaching at the school, and whether they had any documented strategy for teacher training in relation to increased use of technology in teaching.

Figure 1 shows the presence of 1:1 programs in our sample of schools. These types of programs were very unusual in 2008, but increase substantially in importance over the sampling period. We see a particularly large shift between 2011 and 2012. In 2016, 151 of the schools had implemented a 1:1 program in grade 7. This corresponds to almost 70 percent of the schools (with grade 7) in our sample (see Table A2).[17] Similar figures are reported for grades 8 and 9.

It is more common to use laptops than tablets. The increase in 1:1 programs after 2012 is, however, largely driven by an increase in programs using tablets; see Figures A1 and A2. Essentially no 1:1 tablet programs existed before 2012.

[16] Grade 9 existed in 209 of these schools.
[17] Note that this figure is not representative for all Swedish schools; half of the municipalities included in our study were selected because we knew in advance that 1:1 programs were especially common here. According to a survey by the National Agency for Education (2016a), around 50 percent of Swedish students in grade 7–9 participated in a 1:1 program in 2015.

Figure 1 Presence of 1:1 programs in different grades among the schools in the sample



We have linked the school level data on 1:1 programs to individual level register data on school attendance in grade 7 (available since 2008), and students' school performance, measured by their performance on national standardized tests in Swedish, English and mathematics which are given in grade 6 (or in some cases grade 5) and 9 (available since 2009 and 2005, respectively).[18] We use the results from these tests as three separate dependent variables in the analysis (rather than constructing an index). Studying mathematics and language separately is standard in the literature on ICT and educational outcomes. Technology is used in different ways, and to varying extents, in different subjects (see e.g. Table 1) and there is thus no reason to believe that the effect would be the same. We have also added information on whether the students enrolled in upper secondary school and what type of track they enrolled in (college-preparatory, vocational, or introductory program). This gives us the opportunity to capture effects on a broader set of skills and on outcomes that potentially have more long-term relevance.

[18] The registers that contain test scores from compulsory school also include information on which school the student attended in grade 9. Hence, we can observe which school the student attended in grade 9 (but not other grades) already from 2003.

Our dataset also includes several background variables for the students (e.g., age, immigrant background and parents' educational background[19]) and the schools (e.g. class size and number of students) as well as information about the teachers and principals employed (e.g. education and years of teaching experience).[20] The register data come from Statistics Sweden.

Municipality-wide 1:1 initiatives may come with additional funding for the schools. To find out to what extent schools in municipalities with a wide use of 1:1 programs have received additional funding for such programs, we have contacted the school administration in these municipalities. Many of them did not respond, and others were unable to provide reliable information. In the somewhat small group of municipalities where we received trustworthy information, most replied that additional funding from the municipality had been provided, to either fully or partially finance 1:1 programs, but there are also cases where this was not the case. Due to the low response rate, we cannot use the information from this mini-survey in the empirical analysis, but it to some extent helps us to interpret the results (we will return to this question in Section 6 as well as in the conclusions).

4.1 What kind of schools implement 1:1 programs?

Before discussing how we go about to identify the impact of 1:1 programs, we look into what type of schools in our sample that choose to implement these programs and whether the selection of schools to these programs has changed over time. Table 2 shows how the probability that a school has launched a 1:1 program in grade 7 is related to various characteristics of the schools in the same municipality. We show these relationships for the fall semester of 2009, 2011, 2013 and 2015, respectively. To circumvent the problem that the introduction of a 1:1 program could influence what types of students (and teachers) that are attracted to the school, as well as their performance, the school characteristics are here measured during the academic year 2007/08 – that is, prior to the introduction of 1:1 programs for the vast majority of schools.[21]

[19] Students are linked to register data for their parents using the Multi Generation Register, which contains information on ties between children and parents for all residents.

[20] Our dataset links teachers to schools but not to the specific students taught. Hence, average teacher characteristics refer to the whole school although our sample only consists of upper secondary school students.

[21] Out of the 209 lower secondary schools (grades 7–9) in our sample, 162 can be linked back to the school year 2007/08. Among these schools, only 6 percent reported that they had implemented a 1:1 program during the first semester for which we have collected data, i.e. the fall semester of 2008.

Table 2 Relationship between the presence of a 1:1 program in grade 7 and earlier school characteristics

	(1) Fall 2009	(2) Fall 2011	(3) Fall 2013	(4) Fall 2015
Student characteristics				
Share female pupils	-0.512*	-0.731*	-0.717	0.228
	(0.308)	(0.369)	(0.532)	(0.593)
Average test result[a]	0.108	-0.081	-0.023	-0.118
	(0.118)	(0.169)	(0.178)	(0.185)
Share foreign born pupils	0.145	0.178	-0.776	-0.222
	(0.339)	(0.739)	(0.580)	(0.707)
Share pupils with foreign born parents	0.026	0.029	0.183	0.393
	(0.184)	(0.404)	(0.343)	(0.396)
Share mothers with post-sec educ	0.258	0.056	-0.215	-0.290
	(0.308)	(0.503)	(0.487)	(0.523)
Share fathers with post-sec educ	0.063	-0.280	0.151	-0.064
	(0.237)	(0.376)	(0.484)	(0.546)
Average wage earnings, father	-0.001**	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Average wage earnings, mother	0.001	0.002	0.003*	0.003**
	(0.001)	(0.002)	(0.001)	(0.001)
School, teacher and principal characteristics				
Public school	0.008	-0.098	-0.006	-0.058
	(0.091)	(0.128)	(0.147)	(0.156)
No of students	0.001	0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Average teacher experience (years)[b]	-0.006	0.018*	-0.007	-0.000
	(0.006)	(0.010)	(0.012)	(0.016)
Share female teachers	-0.534*	0.114	-0.069	-0.398
	(0.321)	(0.502)	(0.454)	(0.513)
Share teachers with teaching degree	0.260	0.333	0.641*	0.506
	(0.308)	(0.375)	(0.350)	(0.380)
Average principal experience (years)[b]	-0.000	-0.003	-0.010**	-0.011**
	(0.003)	(0.004)	(0.004)	(0.005)
Share female principals	-0.046	-0.031	-0.094	-0.066
	(0.040)	(0.071)	(0.074)	(0.076)
Share principals with teaching degree	-0.031	-0.003	0.097	0.120
	(0.077)	(0.122)	(0.127)	(0.127)
Observations	157	157	157	157
R-squared	0.536	0.555	0.657	0.607
Mean of outcome	0.074	0.210	0.426	0.506

Notes: School characteristics are measured among students who finished grade 9 in 2008, and among teachers and principals employed during the school year 2007/2008. [a]Average test result refers to students' average grade on the standardized tests in Mathematics, English and Swedish. [b]Teacher and principal experience is defined as number of years employed at any school in Sweden. The regressions control for municipality fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The results do not give the impression that schools that implement 1:1 programs are largely different from schools that do not; very few estimates are statistically significant.[22] There seems to be somewhat of a tendency for early implementers to have a larger share of male students. The size of the estimates implies that an increase in the share of female

[22] The high R-squared is mainly explained by the municipality fixed effects. If we estimate the models without municipality fixed effects, the R-squared decreases to 0.09–0.15.

students by one standard deviation (0.09) is associated with a 5-7 percentage point decrease in the probability that the school has launched a 1:1 program in 2009 and 2011. There is also a tendency for fathers to have lower earnings in 1:1 schools in the beginning of the time period, whereas the pattern is the reverse for mothers, especially towards the end of the time period. It is noteworthy that there is no significant relationship between the probability that a school has launched a 1:1 program and the students' performance on the standardized tests.

The table does not indicate that the likelihood of introducing 1:1 programs consistently differs depending on teacher characteristics such as sex, education and experience. When it comes to principal characteristics, we find a significant correlation with years of experience (in teaching and/or school leadership) for the last two years.[23] Hence, more experienced principals seem less likely to introduce these programs at their schools.

5 Estimating the impact of 1:1 programs

To capture effects of 1:1 programs, we compare how educational outcomes change across cohorts for schools that introduce 1:1 programs, to changes for schools that have not yet introduced such programs, in a difference-in-differences design.

Our main sample consists of pupils that enrolled in grade 7 in any of the schools for which we have obtained data during 2008–2013. This means that they graduated from compulsory school during 2011–2016.[24] Our empirical design requires at least one untreated cohort per school in order for the school to contribute to identification; schools that introduced 1:1 program early enough to also affect the 2008 cohort are therefore excluded from the sample. This sampling procedure results in a sample of 168 schools, out of which 78 schools (46 percent) launched 1:1 programs at some point before the summer of 2016. A factor that will complicate the identification of causal effects of 1:1 is that a school's decision to provide laptops (or tablets) to the students also may affect the selection of students to the school. In order to mitigate this problem, we exclude all pupils that were given a laptop or tablet already from the first semester of grade 7.[25]

[23] Teacher and principal experience are defined as number of years employed at any school in Sweden and thus includes both experience in teaching and school leadership.
[24] Grade repetition during compulsory school is rare in Sweden.
[25] This restriction also alleviates the concern that children in treated schools may have had greater access to laptops/tablets already before grade 7 compared to children in untreated schools, since students sometimes attend the same school also in earlier grades.

Hence, all pupils that are included in the sample enrolled in schools without 1:1 programs when they began lower secondary school.

Our final sample consists of 49,937 pupils. Table B1 in the appendix shows descriptive statistics for their background characteristics as well as for their exposure to 1:1 programs. Very few students in the sample were provided with a laptop/tablet already in the spring semester of grade 7. A year later this share is about 8 percent, and by the spring of 9^{th} grade about 15 percent of the students had received either a laptop (13 percent) or a tablet (2 percent) from their school. The pupils that were included in a 1:1 program during their last semester of grade 9 had on average had their laptop or tablet for 2.9 semesters.

In the previous section, we saw that the schools that implement 1:1 programs are generally rather similar to the rest of the schools in terms of observable characteristics. In Table 3 we examine whether there are any differences in observables in the sample which we will use for estimating the impact on students. The table compares the background characteristics among students that in 2008 attended schools that later introduced 1:1 programs, and students that attended schools that did not launch such a program during our sampling period. We can see that the two groups of students are balanced in terms of observable background characteristics; the only statistically significant difference between the groups is the mother's earnings whose coefficient is very close to zero.[26] Based on an F-test, we cannot reject the hypothesis that all the coefficients on the individual covariates are jointly zero (p-value 0.259).

[26] The size of the estimate (-0.00000495) implies that an increase in mother's earnings by one standard deviation (1,879) is associated with a 0.9 percentage point decrease in the probability that the school launches a 1:1 program within the next eight years.

Table 3 Comparison of students that attend schools that later introduce 1:1 programs and schools that do not. Comparison of grade 7 students in 2008

	Attends a school that introduces 1:1
Female	-0.006
	(0.007)
Foreign born parents	0.024
	(0.035)
Foreign born	-0.014
	(0.023)
Mother has upper secondary education	-0.019
	(0.021)
Father has upper secondary education	-0.011
	(0.009)
Mother has post-secondary education	-0.026
	(0.021)
Father has post-secondary education	-0.024
	(0.016)
Missing data on mother's education	-0.006
	(0.043)
Missing data on father's education	0.074
	(0.069)
Wage earnings, mother	-0.000**
	(0.000)
Wage earnings, father	-0.000
	(0.000)
Missing data on father's earnings	-0.084
	(0.068)
Missing data on mother's earnings	0.031
	(0.051)
One year younger	0.085
	(0.052)
One year older	0.019
	(0.021)
Two years older	-0.018
	(0.063)
Observations	9,707
R-squared	0.559

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

The first regression model we estimate is the following:

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

where i indexes individual, s the school the individual attends in the beginning of 7th grade, and c lower secondary school starting year (which we refer to as "cohort"). y_{isc} is the individuals' grade on the national standardized test in mathematics, Swedish or English, which the students take at the end of 9th grade, or an indicator for admittance to upper secondary school. To account for changes in grading standards over time, students' test results are standardized within cohort to have mean zero and standard deviation one.[27] $ICTprogram_{sc}$ is an indicator that takes the value one if the individual, in the beginning of grade 7, attends a school that (later) introduces a 1:1 program and belongs to a cohort that will be treated by the program; otherwise it is zero. X_{isc} is a vector of individual background characteristics (those listed in Table 3), and θ_c and δ_p represent cohort and school fixed effects, respectively. e_{isc} is an error term. The parameter of interest, β_1, is the difference-in-differences estimate of exposure to a 1:1 program (of any length) during lower secondary school. Since exposure to 1:1 is measured based on which school the individual attends in the beginning of grade 7, β_1 should be interpreted as an intention-to-treat (ITT) estimate of 1:1 programs.

In order to account for the length of exposure to 1:1 programs, we also estimate a regression model where the variable $ICTprogram_{sc}$ is replaced by $ICTsemesters_{sc}$, which counts the number of semesters the pupil would be exposed to 1:1 if staying enrolled in the same school up until the end of grade 9. This is our preferred model specification.

Table 3 showed that students in (later) treated and untreated schools were similar in terms of the background characteristics we can observe in our data. By incorporating school fixed effects, our models also account 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 outcomes would not differ systematically between schools that launched 1:1 programs at different points in time in the absence of these programs. This assumption is fundamentally untestable, but by examining pre-treatment trends we can make an assessment of whether it seems credible. To do this, we perform placebo-test by estimating our preferred model specification but (artificially) define the program to have been launched one as well as two years before the actual program start (see Section 6.1.1).

[27] An alternative way to standardize test scores is to percentile rank students (based on their test results) within cohort. Results from regression models based on percentile ranked test results are displayed in appendix in Tables B6–B8. All results are robust. If anything, the overall pattern becomes even clearer.

6 Results

This section presents the results from the empirical analysis. We first show results for the full sample of lower secondary school students, followed by robustness checks (Section 6.1). Thereafter, we examine a number of extensions: We investigate if 1:1 programs affect the achievement gap between low- and high-SES students (Section 6.2); if effects vary depending on if laptops or tablets are used (Section 6.3); or depending on if the schools have strategies for teacher training in relation to the new technology (Section 6.4). Last, we examine if the introduction of 1:1 programs have impacted class size (Section 6.5) or the composition of teachers at the schools (Section 6.6).

6.1 Main effects of 1:1 programs

Table 4 displays estimates for the two regression models discussed in Section 5 for the full sample of students. The first two columns show the estimated effects of being exposed to a 1:1 program (of any length) during lower secondary school, while the last two show estimates for our preferred model specification which also takes into account the number of semesters of (potential) exposure to 1:1.

The results give no indication that 1:1 programs would enhance student performance in neither language nor mathematics: The estimated coefficients for the 1:1 indicators are negative, small and statistically insignificant for all three outcomes, independently of which model we use and independently of whether the models include controls for student background characteristics or not. One could also note that the point estimates stay very similar when individual background controls are included in the model, which is reassuring. In appendix, we present results from alternative regression models where we instead have standardized the outcome variables using percentile ranked test results. The results are very robust (see Table B6).

Table 4 Effects of 1:1 programs on standardized test results in 9th grade

	(1) No background controls	(2) All controls	(3) No background controls	(4) All controls
A: Mathematics				
ICT program	-0.013	-0.017		
	(0.034)	(0.031)		
No. of semesters with ICT program			-0.008	-0.007
			(0.010)	(0.009)
Number of observations	44,920	44,479	44,920	44,479
Number of schools	161	161	161	161
R-squared	0.080	0.191	0.080	0.191
B: Language: Swedish				
ICT program	-0.007	-0.008		
	(0.027)	(0.025)		
No. of semesters with ICT program			-0.007	-0.005
			(0.008)	(0.008)
Number of observations	46,217	45,747	46,217	45,747
Number of schools	161	161	161	161
R-squared	0.074	0.227	0.074	0.227
C: Language: English				
ICT program	-0.005	-0.005		
	(0.026)	(0.024)		
No. of semesters with ICT program			-0.002	0.001
			(0.007)	(0.007)
Number of observations	45,764	45,308	45,764	45,308
Number of schools	161	161	161	161
R-squared	0.071	0.178	0.071	0.178

Notes: Students' test results (grades) are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects. Col. (2) and (4) additionally control for all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Extensive involvement of ICT in teaching can of course benefit (or harm) students in a number of ways although not reflected in higher (or lower) test scores in mathematics or language; for example, by affecting their motivation or performance in other subjects. In order to try to capture effects on a broader set of skills, as well as on outcomes that potentially have even more long-term relevance, we also examine the impact on the probability of continuing to a regular upper secondary school program directly after 9th grade. As described in Section 3, almost all pupils continue to upper secondary school, but not all are qualified to enter a regular upper secondary school program and instead need to enroll in an introductory program to obtain additional qualifications before they can enter a regular program.

Table 5 Effects of 1:1 programs on admittance to upper secondary education

	(1) No background controls	(2) All controls	(3) No background controls	(4) All controls
A: Admitted to a regular program (college-preparatory or vocational)				
ICT program	-0.005	-0.006		
	(0.009)	(0.009)		
No. of semesters with ICT program			-0.002	-0.002
			(0.003)	(0.003)
Number of observations	49,889	49,190	49,889	49,190
Number of schools	161	161	161	161
R-squared	0.055	0.122	0.055	0.122
Outcome mean	0.854	0.854	0.854	0.854
B: Admitted to a college-preparatory program				
ICT program	-0.000	-0.003		
	(0.011)	(0.010)		
No. of semesters with ICT program			-0.000	-0.000
			(0.003)	(0.003)
Number of observations	49,889	49,190	49,889	49,190
Number of schools	161	161	161	161
R-squared	0.076	0.184	0.076	0.184
Outcome mean	0.617	0.617	0.617	0.617

Notes: All regressions control for school and cohort fixed effects. Col. (2) and (4) additionally control for all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

The first panel of Table 5 displays estimates from the same models as in Table 4, but where the outcome is an indicator for being admitted to a regular upper secondary school program, either college-preparatory or vocational, three years after beginning 7th grade. In the second panel the outcome is instead an indicator for being admitted to a college-preparatory program. We can see that all estimates are statistically insignificant and close to zero also for these outcomes.

In sum, we find no evidence suggesting that 1:1 programs in lower secondary school have impacted students' educational outcomes on average.

6.1.1 Robustness analyses

Our identification strategy relies on the assumption that trends in student outcomes would not differ systematically between schools that launch 1:1 programs at different points in time in the absence of these programs. To assess whether this seems to be a credible assumption, we investigate if there are differences in trends for schools that launch 1:1 programs at different points in time already before the start of these programs. We do this by performing placebo tests: We estimate our preferred model specification (Table 4, col. 4), but (artificially) set the start date of the program to one as well as two years before the actual start date. To make sure that the placebo-estimates do not pick up effects of actual 1:1 programs, all students that were affected by the actual programs are excluded from these analyses.

The school enrollment data, which we use to construct our main sample, is not available before 2008. Therefore, we here define (placebo) treatment status based on school attendance during the last semester of grade 9 (which is available from the graduation records further back in time). Table 6 displays the results from these analyses. The sample is here based on students graduating from compulsory school during 2005–2015 (Panel A) and 2005–2014 (Panel B); for earlier cohorts we do not have comparable information on students' performance on the standardized tests.[28]

Table 6 Placebo estimates

	(1) Math	(2) Swedish	(3) English	(4) Reg.program	(5) College-prep.
A. Placebo analysis, t-1					
Placebo estimate	-0.007	0.003	0.010	0.002	0.001
	(0.010)	(0.009)	(0.009)	(0.002)	(0.004)
Observations	95,211	97,217	96,652	104,453	104,453
R-squared	0.177	0.211	0.174	0.123	0.190
B. Placebo analysis, t-2					
Placebo estimate	-0.005	-0.006	-0.001	-0.002	0.001
	(0.007)	(0.007)	(0.006)	(0.002)	(0.004)
Observations	89,148	91,032	90,561	97,888	97,888
R-squared	0.176	0.210	0.173	0.122	0.188

Notes: The model estimated is the same as in Table 4, col. 4, but where the treatment is (artificially) defined to have taken place one year (panel A) or two years (panel B) before actual program start. Students' test results (grades) in col. 1-3 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 all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Reassuringly, the results from the placebo regressions do not indicate that schools that implement 1:1 program differ in trends in student performance during the two years preceding the launch of these programs. The placebo estimates are statistically insignificant for all five outcomes both one and two years before the start date of the programs.

[28] Since new schools start every year and others close, we inevitably lose some schools from the sample as we move the start date of the intervention back in time. Out of the 168 schools included in our main analysis, 147 are included in the placebo regressions and 143 can be followed all the way back to 2005.

For the last four cohorts of the sample we have information on results on standardized national tests (in the same subjects) from an earlier point in time – before the students enrolled in 7th grade. Those who enrolled in 7th grade in 2010 and 2011 took standardized tests in mathematics, Swedish and English at the end of 5th grade, and those who enrolled in 2012 and 2013 took tests in the same subjects at the end of 6th grade. Adding controls for previous performance to our baseline model can be seen as an additional check of whether our results may be biased due to unobserved heterogeneity. However, our data on prior test results only contain information on whether or not the student received a passing grade on each of the sub-tests within each subject. Prior test results are also lacking for a part of the sample (6–9 percent of the students). Together, the sample restrictions imply that only around half of the original sample can be included in these regressions. Nonetheless, it is reassuring that our conclusions hold if we replicate the analysis for the sample with available prior test results and add controls for prior performance. Table 7 displays regression results when we have added controls for whether or not the student passed all sub-tests in math, Swedish and English, respectively, to our baseline model.[29]

Table 7 Robustness: controlling for results on earlier standardized test

	(1) Math	(2) Math	(3) Swedish	(4) Swedish	(5) English	(6) English
No. of semesters with ICT program	0.004	-0.005	-0.030	-0.023	-0.010	-0.004
	(0.019)	(0.021)	(0.018)	(0.018)	(0.014)	(0.012)
Pass on earlier math test		0.672***				
		(0.025)				
Pass on earlier Swedish test				0.685***		
				(0.021)		
Pass on earlier English test						1.147***
						(0.025)
Number of observations	25,617	25,617	25,312	25,312	25,542	25,542
R-squared	0.202	0.263	0.237	0.314	0.177	0.349

Notes: Students' test results (grades) 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 all covariates presented in Table 3. The sample consists of students who enrolled in grade 7 in 2010–2013 only. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Our preferred specification assumes that the effect of 1:1 programs on student performance is linear in the number of semesters of exposure. Given that previous studies

[29] The standardized tests performed in grade 5 or 6 differ over time, also when it comes to the number of sub-tests included and how the tests are graded. The results are similar if we instead rank the students, among those taking the test the same year, based on the number of sub-tests they passed, and instead control for this rank.

find that there are several implementation challenges associated with the launch of 1:1 initiatives (Haelermans 2017; Islam and Grönlund 2016) this may be an unrealistic assumption. In Table 8 we relax this assumption by including two treatment dummies in the model: $ICTprogram_{sc}$ takes the value one if the pupil belongs to a cohort that was exposed to a 1:1 program for any length of time (same definition as before); and $ICTprogram\_long_{sc}$ takes the value one for cohorts that were exposed for more than two semesters. The second treatment indicator is thus intended to capture if the effect on student performance improves (or deteriorates) with longer exposure. We find no evidence that this is the case; the estimate for the second indicator is statistically insignificant for all five outcome variables. Based on this analysis, we find no reason to reject the linear specification.[30]

Table 8 Robustness: specifications with nonlinear effects

	(1)	(2)	(3)	(4)	(5)
	Grades on standardized tests:			*Upper sec. school admittance:*	
	Math	Swedish	English	Reg. program	College-prep.
ICT program	-0.013	-0.012	-0.022	-0.004	-0.006
	(0.034)	(0.027)	(0.030)	(0.009)	(0.011)
ICT program long	-0.008	0.008	0.039	-0.004	0.007
(> 2 semesters)	(0.035)	(0.030)	(0.030)	(0.008)	(0.012)
Observations	44,479	45,747	45,308	49,190	49,190
R-squared	0.191	0.227	0.178	0.122	0.184

Notes: Students' test results (grades) are standardized within cohort to have mean 0 and a standard deviation 1. The regressions control for school and cohort fixed effects as well as all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

6.2 The digital divide – is the performance gap between low and high SES students affected by 1:1 programs?

Our results do not indicate that the adoption of 1:1 programs in lower secondary school impacts students' educational performance on average. However, it is still possible that these programs are beneficial (or detrimental) for certain groups of students. As we have discussed above, 1:1 initiatives are sometimes regarded as a means to decrease the performance gap between students from different socioeconomic backgrounds. In Table 9 we investigate if the impact of 1:1 differs depending on the parents' level of education (which we regard as an indicator of SES background). The table displays results from our

[30] It should be noted that none of the schools included in our sample have had 1:1 programs for more than five semesters. We can therefore not detect possible improvements (or deteriorations) several years into the programs.

preferred model specification but where we have added an interaction term between the ICT treatment indicator ($ICTsemesters_{sc}$) and an indicator for the parents having a low level of education, defined as none one of the parents having post-secondary education.

Table 9 Effects of 1:1 programs by parental education

A: Grades on standardized test	(1) Math	(2) Swedish	(3) English
No. of semesters with ICT program	0.000	0.002	0.004
	(0.010)	(0.008)	(0.007)
No. of semesters with ICT program* low educated parents	-0.016*	-0.014	-0.005
	(0.009)	(0.009)	(0.009)
Observations	44,479	45,747	45,308
R-squared	0.191	0.227	0.178

B: Admitted to upper secondary school	(4) Any regular program	(5) College-prep. program
No. of semesters with ICT program	0.003	0.004
	(0.003)	(0.004)
No. of semesters with ICT program* low educated parents	-0.010***	-0.008*
	(0.003)	(0.004)
Observations	49,190	49,190
R-squared	0.123	0.185

Notes: Low level of parental education is defined as no parent with post-secondary education. Students' test results (grades) 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 all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools.*** p<0.01, ** p<0.05, * p<0.1.

The estimated impact of 1:1 consistently shows the opposite sign for students with higher vs. lower educated parents, indicating that these programs may in fact increase inequality in educational outcomes along the socio-economic dimension. When it comes to results on the standardized tests, we get a statistically significant interaction term for math, suggesting that the adoption of 1:1 computer programs increases the gap in math performance between students with higher and lower educated parents by 0.016 of a standard deviation per semester. This corresponds to 10 percent of a standard deviation (1.6×6=9.6) if students are exposed to 1:1 during all semesters of lower secondary school.[31] In relation to the raw gap in test results between the groups (0.594), the estimate corresponds to a 16 percent increase (0.096/0.594). We interpret this impact as quite small, but not negligible.

[31] In economics of education it is common to follow the rule of thumb that less than 10 percent of a standard deviation is a small effect, 10–25 percent represents an encouraging effect, and effects above 25 percent are large (e.g. Escueta et al. 2017).

The estimated effect is similar in size for Swedish language, but marginally insignificant (p-value 0.105). If we instead use percentile ranked outcome variables, the impact on both math and Swedish is more pronounced; see Table B7. For English, on the other hand, there is no indication of a differential impact depending on parental background.

The patterns for test results are very similar if we instead estimate separate regressions for students whose parents have a low vs. high level of education, but splitting the sample further reduces the precision of the estimates (see Table B2).

The results presented in panel B suggest that the increased inequality in school performance also may be carried over to upper secondary school: The estimate for the interaction term is negative and statistically significant for both admittance to upper secondary school and for admittance to a college-preparatory program. While the estimated impact on the former gap gets smaller if we instead estimate separate regressions for the two sub-samples of students, the estimate for admittance to a college-preparatory program remains very similar in size (see Table B2). In relation to the raw gap in admission to college-preparatory programs between the groups, the estimate is of similar size as the estimates for math and Swedish language.[32] [33]

6.3 Do effects vary depending on the type of technology used?

Whether a school chooses to utilize laptops or tablet computers in its 1:1 program is likely to have consequences for how 1:1 affects the teaching at the school. Computers' built-in keyboard makes them a better tool for writing, and they generally have more powerful processors which allow them to handle more complex software. Tablets may on the other hand offer other advantages; for instance, in terms of being easier for students to carry around and having longer battery life. What specific digital learning tools that exist on the market for laptops vs. tablets is also likely to differ as well as how familiar teachers are with incorporating these tools into their instruction.

[32] Placebo-analyses in line with those presented in Table 6, give no indication that the achievement gap between low and high SES students was increasing at 1:1 schools, compared to other schools, already before the adoption of these programs. Such a pattern would have indicated that we risk overestimating this effect. Rather, a couple of placebo estimates suggest that the gap was closing a few years back, while most estimates are statistically insignificant; see Table B3.

[33] We have also examined if the impact of 1:1 programs differs for boys and girls. There is some tendency of a more negative impact on test results in math for girls, but overall we do not find clear evidence of important differences between the sexes.

Table 10 Effects of 1:1 laptop vs. tablet programs

	(1)	(2)	(3)	(4)	(5)
	Grades on standardized tests:			*Upper sec. school admittance:*	
	Math	Swedish	English	Reg. program	College-prep.
No. of semesters with laptop program	-0.006 (0.010)	-0.000 (0.008)	0.003 (0.008)	-0.002 (0.003)	0.001 (0.003)
No. of semesters with tablet program	-0.014 (0.019)	-0.034*** (0.011)	-0.014 (0.009)	-0.001 (0.004)	-0.008 (0.007)
Observations	44,479	45,747	45,308	49,190	49,190
R-squared	0.191	0.227	0.178	0.122	0.184

Notes: Students' test results (grades) 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 all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

In the survey we asked schools to specify whether the pupils were provided with laptops or tablets. As was apparent in Table B1, most of the lower secondary schools use laptops. Of the 7,560 pupils in our sample that have participated in 1:1 programs, only 15 percent were given a tablet computer. Table 10 shows results from estimating our preferred model specification (Table 4, col. 4) but with separate treatment variables for laptop and tablet programs. The results suggest that the type of technology used may in fact be crucial for how educational outcomes are affected. While the estimated coefficient for the number of semesters with a 1:1 laptop program is always small and statistically insignificant, we get a negative and highly significant estimate for the impact of 1:1 tablet programs on students' test results in Swedish language. The effect corresponds to a reduction of 0.034 of a standard deviation per semester of tablet program. This implies a fairly large reduction of 0.204 of a standard deviation if tablets are used throughout all six semesters of lower secondary school. The estimated effect for English language is also negative but not statistically significant at conventional levels (p-value 0.129). However, if we percentile rank the outcome variables, we get a negative and statistically significant estimate also for English (see Table B8). Although these results do not inform us about why the results are worse for tablet programs, they suggest that an important key to understanding the impact on students lays in understanding how 1:1 programs alter the teaching practices used in the classroom.[34]

[34] We have also performed placebo-analyses in line with those in Table 6 for this model; see Table B4. The results give no indication of differential trends in language performance in 1:1 laptop or tablet schools compared other schools before the introduction of these programs.

6.4 Are ICT strategies and teacher training important?

Previous studies have highlighted the importance of teachers having sufficient training in how to incorporate the new technology into teaching in order for 1:1 programs to be successful (see e.g. Haelermans 2017). To get a rough idea about the presence of this type of training at the schools, our survey included questions on whether the school had any documented strategy for how ICT should be incorporated into the instruction, and whether they had any documented strategy for teacher training in relation to increased use of technology in the classroom. Around half of the schools that had initiated 1:1 programs answered "yes" to one or both of these questions.[35] However, estimating interactions models that allow the effect of 1:1 to differ for schools with and without strategies for ICT and teacher training, provide no evidence in support of more successful student outcomes in schools with such documented strategies; see Table B5.

6.5 Impact on class size

To the extent that the schools have not received additional funding to fully cover their 1:1 initiatives, ICT expenditure will come at the expense of something else. Schools may, for instance, fund their 1:1 programs by reducing the number of teachers hired, and there are previous studies finding that larger classes tend to worsen student performance (e.g. Angrist and Lavy 1999; Krueger and Whitmore 2001; Fredriksson, Oosterbeck and Öckert 2013, 2016). The responses from our mini-survey among municipalities show that all 1:1 programs were not fully financed with additional funds even though municipalities often seem to have put in extra money (see Section 4).[36] Hence, it is possible that the absence of positive effects on average student performance could be explained by increases in the student-teacher ratio.

The dataset that provides information on which school a student attends each school year, also contains information on the school's division of students into classes. Using these data, we can examine if 1:1 initiatives seem to have led to larger classes.

However, it is important to point out that the schools' registered class division is unlikely to constitute a perfect measure of actual class size at all times; e.g. schools may

[35] 46 percent of the 1:1 schools answered that they had an ICT-strategy, 38 percent answered that they had a plan for teacher training, and 35 percent answered "yes" to both of these questions. 13 percent of the 1:1 schools did not answer these questions. In the analysis we treat missing values as a "no". Note that we only have information of whether documented strategies existed at the time when data were collected.

[36] Due to the low response rate, as well as incomplete answers, we are not able to further examine whether effects differ depending on if the schools have been provided with additional funding or not.

divide pupils into smaller (or larger) groups in certain subjects, and some schools do not divide the cohorts into classes at all in the register but instead record the whole cohort as the same class. Hence, for this analysis it is critical to examine if the results are sensitive to the inclusion/exclusion of unreasonably large (and small) classes.

Table 11 Effects on class size

	(1)	(2)	(3)	(4)	(5)
ICT program	-0.408	-0.415	-0.402	-0.456	-0.259
	(0.424)	(0.417)	(0.420)	(0.417)	(0.473)
Observations	44,809	45,077	44,541	45,048	38,922
R-squared	0.386	0.384	0.392	0.376	0.382
Sample restriction:					
Included class sizes	5-40	5-45	5-35	3-40	5-40
Excl. potentially affected controls	no	no	no	no	yes

Notes: All regressions control for school and cohort fixed effects as well as all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Table 11 shows the estimated effect of exposure to a 1:1 program (of any length) on class size, measured in grade 9. In the first column we include all pupils who attend classes with at least 5, but no more than 40, pupils.[37] 1:1 initiatives do not seem to have resulted in larger classes: the estimated impact is negative and statistically insignificant. Columns (2)–(4) show that the results are very similar if we change the sample restriction to also include somewhat larger or smaller classes, or if we are even more restrictive and only include classes with up to 35 pupils. In the last column we have excluded schools where it is possible that pupils in the control group also would be affected by an increased class size, although they were not part of the 1:1 program (this would be the case if a given school has both treated and untreated pupils enrolled at the same point in time). Imposing this restriction also does not alter our findings. Hence, we find no indications that the schools in our sample have financed their 1:1 programs by increasing the number of students per class. To the extent that the schools have not received extra funds from the municipality for their 1:1 programs, the initiatives seem to have been financed by other means, e.g. by cutting spending on other types of teaching materials.

[37] The median class size in our sample is 26 pupils. 5 percent of the students are recorded as attending classes with more than 40 students, and 1 percent in classes with 5 pupils or less.

6.6 Impact on teacher sorting

Another channel through which 1:1 programs may impact student performance is through affecting the sorting of teachers across schools. Efforts to intensify the use of technology in teaching sometimes meets resistance among the teachers; see e.g. discussion in Haelermans (2017). If 1:1 initiatives result in a flight of qualified and experienced teachers from the schools, this may counteract any potential positive effects of technology on student performance, and could thereby explain why we find zero effects on average.

To examine if the introduction of 1:1 programs has impacted the sorting of teachers, we relate the presence of 1:1 programs to average teacher characteristics (experience, degree in teaching, and sex), controlling for year and school fixed effects. Since our dataset only links teachers to schools, and not to the specific students taught, average teacher characteristics refer to the whole school, and a school is considered treated as soon as any of the grades (7–9) is involved in a 1:1 program. We conduct this analysis at the school level and weight the regressions with the number of teachers employed to account for the fact that the schools differ greatly in size.[38]

The results from this exercise, shown in Table 12, do not indicate that the introduction of 1:1 initiatives have altered the composition of teachers: all estimates are insignificant both in statistical and economic terms.

Table 12 Impact on teacher sorting

	(1) Average teacher experience (years)	(2) Share of teachers with teaching degree	(3) Share of female teachers
ICT program	-0.050 (0.262)	-0.006 (0.008)	-0.002 (0.007)
Observations (with weights)	44,240	44,240	44,240
Unique observations	1,434	1,434	1,434
R-squared	0.798	0.699	0.666
Outcome mean (with weights)	13.237	0.822	0.711

Notes: Experience is defined as number of years employed at any school in Sweden. All regressions control for school and year fixed effects. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

[38] Unweighted regressions (not shown) produce very similar results.

7 Conclusion

One-to-one computer programs are becoming increasingly popular in schools all over the world. By combining survey data on the implementation of such programs between 2008 and 2016 in a large number of Swedish schools with rich administrative data, we estimate the impact of 1:1 initiatives on students' educational performance using a difference-in-differences design. Essentially all Swedish schools use ICT in their teaching today. Thus, the comparison we make is between intensive use of ICT in the form of 1:1 and more standard use of computers (e.g. pupils borrowing laptops or tablets for certain tasks). Survey evidence from the National Agency for Education (2016a) confirms that 1:1 schools tend to involve ICT in their teaching to a much greater extent than other schools.

On average, we find no significant impact of 1:1 programs on student performance, as measured by their results on national standardized tests in mathematics and language at the end of lower secondary school. These findings are in line with the results in Crista et al. (2012) and De Melo, Machado and Miranda (2014), although at least the former studies a vastly different context (schools in poor regions of rural Peru). We extend the literature by examining effects on the probability of transitioning to upper secondary school and the students' choice of educational track. We find no effects on average in these respects either. We also examine if the impact differs depending on if the schools use laptops or tablets. These results indicate that tablets may bring about some negative effects on student performance. The estimates for 1:1 laptop programs are, on the other hand, always small and statistically insignificant.

It is sometimes proposed that 1:1 programs may reduce inequality in educational outcomes as they provide low-SES students with resources they might otherwise lack (the so called 'digital divide'). We find no support for this claim. If anything, our results suggest the reverse. The performance gap in mathematics between students with low- and high-SES background seems to increase to some extent, and so does the gap in the probability of being admitted to a college-preparatory program in upper secondary school.

The interpretation of a zero effect on average will, of course, differ depending on how 1:1 initiatives are financed. The lack of positive effects on student outcomes, and perhaps also negative effects in some dimensions, should be considered a larger problem if the schools have received additional resources to launch these programs; an alternative use of the resources (e.g. hiring more teachers) could then have generated better outcomes.

If the schools have financed the programs from their own budgets, a zero effect is less problematic. It is, after all, likely that the increased use of ICT in the classroom have taught students other things (such as general ICT skills), which are not captured by the outcomes we examine.

Unfortunately, we have no systematic data on how the 1:1 programs examined have been financed. We have contacted municipal school administrations to get a sense of whether municipalities often put in extra money to finance these initiatives. Due to a low response rate it is not possible to generalize from this mini-survey, but it seems like municipalities rather often (but not always) have provided additional resources to either fully or partially finance 1:1 programs.

For methodological reasons, we have focused the empirical analysis on the time period when 1:1 initiatives are first introduced at the schools and the subsequent 1–5 semesters. This gives us the best opportunities to identify causal effects. The downside is that the effects may change when the schools have used 1:1 for a while. It is reasonable to assume that it takes some time to find out how to best use the technology. Over time, initial problems can perhaps be solved, appropriate software can be identified, teachers will accumulate experience and perhaps receive adequate training. Thus, it is possible that effects will become more positive over time. One should note, however, that we do not find that students who have been exposed to a 1:1 program for more than two semesters outperform those with less exposure.

All in all, the absence of positive impacts on student performance should not necessarily be interpreted as a clear signal that it is a bad strategy for schools to invest in 1:1. At the same time, our findings point at some potential pitfalls, and it is obvious that providing students with personal computers is unlikely to be a quick fix that improves educational outcomes in general.

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Appendix A: Description of the data collection

To identify schools with and without 1:1 programs, we collected information through a short survey with all lower secondary schools in 26 Swedish municipalities. The survey took place during the period June 2016 to March 2017.

To be able to analyze effects of 1:1, we needed a relatively large number of schools that had implemented 1:1 programs. We therefore consulted local newspapers and various internet sources (e.g. municipality homepages) to identify municipalities where 1:1 was especially common. We selected 13 such municipalities located in different parts of Sweden. We also made sure to get a good spread in terms of population size and educational level. For each of the 13 municipalities selected in the first step, we selected another municipality with similar observable characteristics but with no prior information (from internet sources) of 1:1 programs being more common than elsewhere. Table A1 shows that the two groups of municipalities are fairly similar. In comparison to the Swedish average, the municipalities included in the sample have more inhabitants and a somewhat more well-educated population. Note that the sampling method implies that our data cannot be used to give a reliable description of the existence of 1:1 programs in Sweden over time.

We collected e-mail addresses and telephone numbers to all existing schools in the 26 municipalities from internet homepages. Both public schools and independent schools were included in the sample. A very small number of schools were excluded at this stage, for instance, schools in rural areas having only a couple of pupils in grade 7–9 and schools for children with diagnoses like Autism or Asperger syndrome. Note that our data cover schools that existed in 2016. This means that schools that have been closed down between 2008 and 2015 are not included in the sample; collecting information from these schools would basically have been an impossible task.

Table A1 Selection of municipalities

Municipality	Indication in advance of frequent existence of 1:1	Part of Sweden	Population (2008)	Average grades in grade 9 (2008)	Municipal educational level, inhabitants aged 25–64 (2008)	
					Elementary education or less (%)	Post-secondary education (%)
			Small (less than 25,000 inhabitants)			
Haparanda	Yes	Norrland	10,112	224	18.8	20.1
Pajala	No	Norrland	6,429	224	13.0	22.6
Nykvarn	Yes	Svealand	9,035	213	16.5	29.3
Vaxholm	No	Svealand	10,747	213	9.8	48.3
Gagnef	Yes	Svealand	10,107	212	15.2	26.8
Leksand	No	Svealand	15,288	214	13.9	31.4
Stenungsund	Yes	Götaland	23,657	217	14.5	34.6
Höganäs	No	Götaland	24,248	212	13.2	38.7
			Middle size (25,000–99,999 inhabitants)			
Ljungby	Yes	Götaland	27,430	207	17.4	26.5
Oskarshamn	No	Götaland	26,309	200	18.3	26.6
Hudiksvall	Yes	Norrland	36,905	213	17.8	27.6
Söderhamn	No	Norrland	25,987	210	18.6	22.4
Falkenberg	Yes	Götaland	40,451	205	20.0	25.6
Trelleborg	No	Götaland	41,558	205	18.2	27.1
Sollentuna	Yes	Svealand	62,097	221	10.1	51.2
Solna	No	Svealand	65,289	211	9.6	53.3
Skellefteå	Yes	Norrland	71,862	209	11.9	32.6
Östersund	No	Norrland	58,914	215	11.2	40.9
Botkyrka	Yes	Svealand	80,055	205	23.0	28.9
Haninge	No	Svealand	74,968	200	19.3	26.9
Växjö	Yes	Götaland	81,074	214	12.3	42.5
Karlstad	No	Götaland	83,994	211	11.4	43.1
			Large (at least 100,000 inhabitants)			
Malmö	Yes	Götaland	286,535	201	14.6	43.0
Uppsala	No	Svealand	190,668	213	10.7	52.9
Västerås	Yes	Svealand	134,468	209	13.8	39.5
Norrköping	No	Götaland	128,060	207	17.5	32.1
Averages:						
Indication of 1:1			67,241	212	15.8	32.9
No indication of 1:1			57,881	210	14.3	35.9
All Swedish municipalities			31,918	209	17.8	28.2

Notes: Data on population, grades and educational level have been retrieved from *Kolada* (www.kolada.se). *Kolada* is a database with official statistics on Swedish regions and municipalities. Part of Sweden refers to a traditional division of three Swedish geographical regions: Norrland (North), Svealand (Middle) and Götaland (South). These regions have no administrative function.

A short questionnaire was sent by e-mail to the schools. They were asked about the existence of 1:1: Did the school have a 1:1 program? What grades were affected at different points in time between the fall semester of 2008 and the spring semester of 2016? Were pupils provided with a laptop or a tablet? This information made it possible for us to construct a panel dataset, based on schools, grades and semesters 2008–2016. We also asked whether the school had a documented plan for how teachers should integrate 1:1 computers in their daily work in the classroom, and whether the school had a documented plan for how teachers should be trained in how to use the new technology. However, we only have information about the presence of these plans at the time when the survey took place.

The initial e-mail was followed up by two e-mail reminders. If the school did not reply within 14 days, we instead contacted them by phone. In the end, we received data from 219 of 299 schools. This implies a good overall responses rate of 73 percent. In 2016, grade 7 existed in 216 schools, grade 8 in 211 schools and grade 9 in 209 schools. For various reasons, 168 schools are used in our empirical analysis (see Section 4).

Table A2 shows that laptop programs existed in around half of the schools in our sample in grade 7–9 in 2016. Tablets were used by 15 percent of the schools in grade 7, 17 percent in grade 8 and 19 percent in grade 9. The development over time is displayed in Figure A1 (laptops) and Figure A2 (tablets). Laptops are more common than tablets all years, but the increase in 1:1 programs after 2012 is to a large extent driven by schools providing tablets to their pupils.

Table A2 Schools in the sample with 1:1 programs in 2016

Grade	Number of schools	Schools with laptop programs		Schools with tablet programs	
		Number	Percent	Number	Percent
7	216	109	50	42	19
8	211	105	50	36	17
9	209	106	51	32	15

Figure A 1 Presence of 1:1 laptop programs in different grades among the schools in the sample

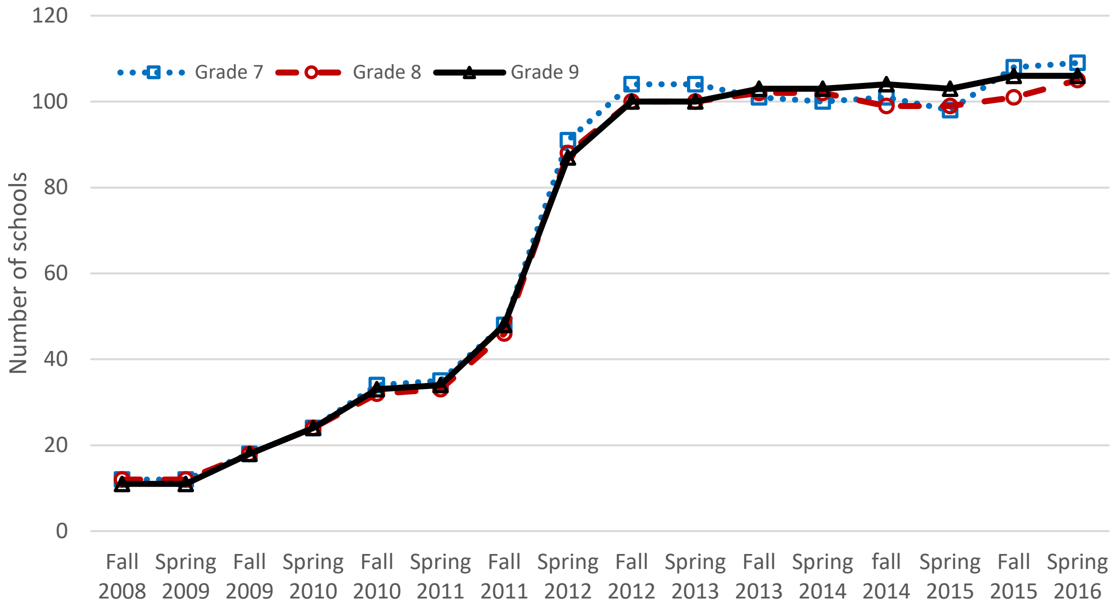
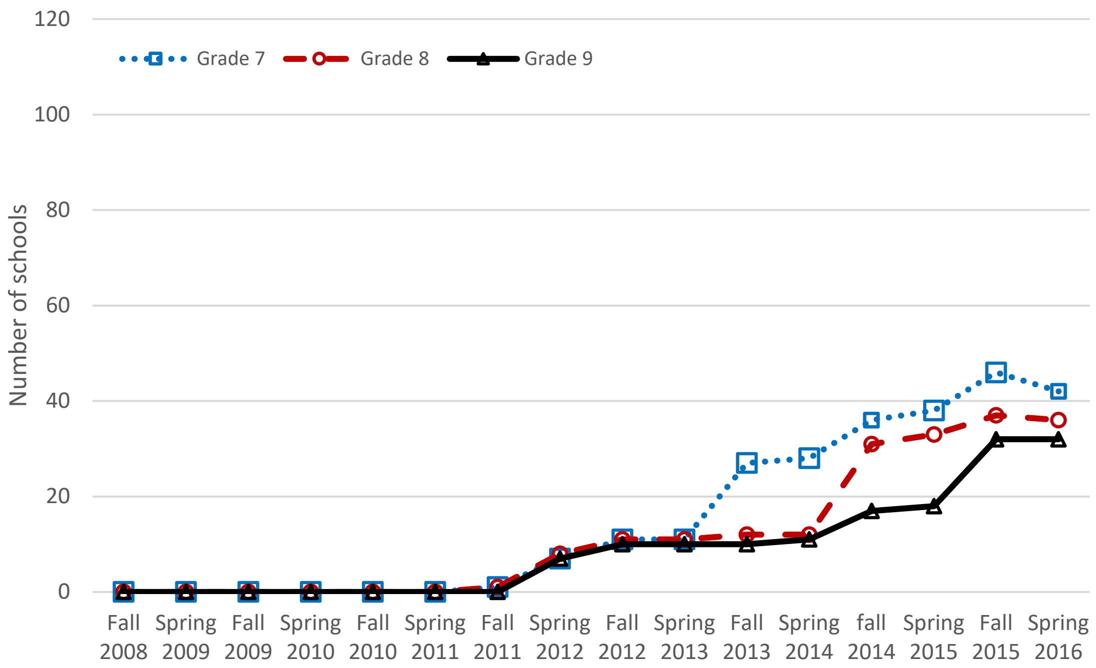


Figure A 2 Presence of 1:1 tablet programs in different grades among schools in the sample



Appendix B: Additional tables

Table B 1 Descriptive statistics

	(1) mean	(2) sd
Exposure to 1:1 programs		
1:1 program, spring gr 7	0.0208	0.143
1:1 program, spring gr 8	0.0830	0.276
1:1 program, spring gr 9	0.149	0.356
Laptop, spring gr 7	0.0208	0.143
Laptop, spring gr 8	0.0680	0.252
Laptop, spring gr 9	0.132	0.338
Tablet, spring gr 7	0	0
Tablet, spring gr 8	0.0150	0.121
Tablet, spring gr 9	0.0174	0.131
Background variables		
Female	0.487	0.500
Foreign born parents	0.224	0.417
Foreign born	0.0973	0.296
Mother has upper secondary education	0.449	0.497
Mother has post-secondary education	0.405	0.491
Missing data on mother's education	0.0325	0.177
Father has upper secondary education	0.478	0.500
Father has post-secondary education	0.324	0.468
Missing data on father's education	0.0672	0.250
Wage earnings, mother	2,313	1,841
Wage earnings, father	3,157	2,974
Missing data on father's earnings	0.0622	0.242
Missing data on mother's earnings	0.0229	0.150
One year younger than classmates	0.0157	0.124
One year older than classmates	0.0471	0.212
Two years older than classmates	0.00153	0.0390
Number of individuals: 49,937		

Notes: The pupils are linked to the schools they attended in grade 7. Hence, the 1:1 variables show the share of individuals that would have been exposed to 1:1 programs if they stayed enrolled in the same school until the spring of grade 7, 8 and 9, respectively. Missing data on parents' income are replaced with zeros.

Table B 2 Effects of 1:1 programs by parental education. Separate regressions for students' whose parents have a high vs. low level of education

	(1) High level of education	(2) Low level of education
A: Mathematics		
No. of semesters with ICT program	0.006	-0.020
	(0.011)	(0.013)
Observations	23,772	20,707
R-squared	0.149	0.093
B: Swedish		
No. of semesters with ICT program	0.006	-0.014
	(0.009)	(0.011)
Observations	24,262	21,485
R-squared	0.197	0.159
C: English		
No. of semesters with ICT program	0.011	-0.009
	(0.008)	(0.010)
Observations	24,196	21,112
R-squared	0.133	0.100
D: Admitted to upper secondary school (any regular program)		
No. of semesters with ICT program	0.002	-0.004
	(0.003)	(0.004)
Observations	25,523	23,667
R-squared	0.075	0.126
E: Admitted to college-preparatory program		
No. of semesters with ICT program	0.004	-0.004
	(0.005)	(0.004)
Observations	25,523	23,667
R-squared	0.112	0.115

Notes: High level of education is defined as at least one parent with post-secondary education; low level of education is defined as no parent with post-secondary education. Students' test results (grades) 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 all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools.*** p<0.01, ** p<0.05, * p<0.1.

Table B 3 Placebo estimates by parental education

	(1) Math	(2) Swedish	(3) English	(4) Reg.program	(5) College-prep.
A. Placebo analysis, t-1					
Placebo estimate	-0.012	-0.001	0.008	0.001	-0.001
	(0.010)	(0.010)	(0.008)	(0.002)	(0.004)
Placebo estimate*	0.012	0.008	0.004	0.001	0.006
low educated parents	(0.012)	(0.012)	(0.009)	(0.003)	(0.005)
Observations	95,211	97,217	96,652	104,453	104,453
R-squared	0.177	0.211	0.174	0.123	0.190
B. Placebo analysis, t-2					
Placebo estimate	-0.013*	-0.012	-0.005	-0.002	-0.000
	(0.007)	(0.008)	(0.006)	(0.002)	(0.004)
Placebo estimate*	0.017**	0.013*	0.007	0.000	0.002
low educated parents	(0.008)	(0.008)	(0.006)	(0.002)	(0.003)
Observations	89,148	91,032	90,561	97,888	97,888
R-squared	0.176	0.210	0.173	0.122	0.188

Notes: The model estimated is the same as in Table 9, but where the treatment is (artificially) defined to have taken place one year (panel A) or two years (panel B) before actual program start. Students' test results (grades) in col. 1-3 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 all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Table B 4 Placebo estimates: laptops vs. tablet programs

	(1) Math	(2) Swedish	(3) English	(4) Reg.program	(5) College-prep.
A. Placebo analysis, t-1					
Placebo estimate	0.003	0.006	0.011	0.001	0.005
laptop program	(0.012)	(0.010)	(0.010)	(0.003)	(0.005)
Placebo estimate	-0.029	-0.005	0.007	0.003	-0.006
tablet program	(0.023)	(0.017)	(0.016)	(0.003)	(0.006)
Observations	95,211	97,217	96,652	104,453	104,453
R-squared	0.177	0.211	0.174	0.123	0.190
B. Placebo analysis, t-2					
Placebo estimate	-0.003	-0.006	0.000	-0.000	0.001
laptop program	(0.008)	(0.008)	(0.006)	(0.003)	(0.005)
Placebo estimate	-0.010	-0.006	-0.007	-0.008**	-0.002
tablet program	(0.013)	(0.016)	(0.007)	(0.004)	(0.004)
Observations	89,159	91,044	90,573	97,900	97,900
R-squared	0.176	0.210	0.173	0.122	0.188

Notes: The model estimated is the same as in Table 10, but where the treatment is (artificially) defined to have taken place one year (panel A) or two years (panel B) before actual program start. Students' test results (grades) in col. 1-3 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 all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Table B 5 Differential impact depending on presence of ICT-strategy and strategy for teacher training at the school

	(1)	(2)	(3)	(4)	(5)
	Grades on standardized tests:			*Admitted upper sec. school:*	
	Math	Swedish	English	Reg. program	College-prep. program
A: Presence of ICT strategy					
No. of semesters with 1:1 program	-0.002	-0.008	0.003	-0.003	-0.005
	(0.015)	(0.013)	(0.010)	(0.004)	(0.004)
No. of semesters with 1:1 program*ICT strategy	-0.009	0.006	-0.003	0.003	0.008
	(0.017)	(0.014)	(0.013)	(0.004)	(0.005)
Observations	44,479	45,747	45,308	49,190	49,190
R-squared	0.191	0.227	0.178	0.122	0.184
B: Presence of strategy for teacher training					
No. of semesters with 1:1 program	-0.004	0.003	0.002	-0.001	-0.003
	(0.014)	(0.012)	(0.009)	(0.003)	(0.004)
No. of semesters with 1:1 pr.*training strategy	-0.007	-0.016	-0.001	-0.001	0.006
	(0.016)	(0.014)	(0.012)	(0.004)	(0.005)
Observations	44,479	45,747	45,308	49,190	49,190
R-squared	0.191	0.227	0.178	0.122	0.184

Notes: Students' test results (grades) are standardized within cohort to have mean 0 and a standard deviation 1. All regressions control for school and cohort fixed effects, presence of ICT/training plan as well as all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Table B 6 Effects of 1:1 programs on percentile ranked test results in 9th grade

	(1) No background controls	(2) All controls	(3) No background controls	(4) All controls
A: Mathematics				
ICT program	-0.378	-0.512		
	(0.883)	(0.788)		
No. of semesters with ICT program			-0.211	-0.192
			(0.265)	(0.239)
Number of observations	44,920	44,479	44,920	44,479
Number of schools	161	161	161	161
R-squared	0.081	0.203	0.081	0.203
Outcome mean	49.776	49.776	49.776	49.776
B: Language: Swedish				
ICT program	-0.258	-0.321		
	(0.765)	(0.701)		
No. of semesters with ICT program			-0.250	-0.199
			(0.231)	(0.220)
Number of observations	46,217	45,747	46,217	45,747
Number of schools	161	161	161	161
R-squared	0.075	0.246	0.075	0.246
Outcome mean	50.486	50.486	50.486	50.486
C: Language: English				
ICT program	-0.168	-0.268		
	(0.728)	(0.693)		
No. of semesters with ICT program			-0.090	-0.034
			(0.212)	(0.195)
Number of observations	45,764	45,308	45,764	45,308
Number of schools	161	161	161	161
R-squared	0.078	0.184	0.078	0.184
Outcome mean	50.948	50.948	50.948	50.948

Notes: Students' test results (grades) are percentile ranked within cohort. All regressions control for school and cohort fixed effects. Col. (2) and (4) additionally control for all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

Table B 7 Effects of 1:1 programs on percentile ranked test results by parental education

A: Grades on standardized test	(1) Math	(2) Swedish	(3) English
No. of semesters with ICT program	0.059	-0.008	0.005
	(0.273)	(0.264)	(0.222)
No. of semesters with ICT program* low educated parents	-0.515**	-0.387*	-0.082
	(0.241)	(0.226)	(0.229)
Observations	44,479	45,747	45,308
R-squared	0.203	0.246	0.184
Outcome mean	49.776	50.486	50.948

Notes: Low level of parental education is defined as no parent with post-secondary education. Students' test results (grades) are percentile ranked within cohort. All regressions control for school and cohort fixed effects as well as all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools.*** p<0.01, ** p<0.05, * p<0.1.

Table B 8 Effects of 1:1 laptop vs. tablet programs on percentile ranked test results

	(1) Math	(2) Swedish	(3) English
No. of semesters with laptop program	-0.120	-0.045	0.063
	(0.250)	(0.239)	(0.213)
No. of semesters with tablet program	-0.650	-1.178***	-0.669**
	(0.506)	(0.344)	(0.322)
Observations	44,479	45,747	45,308
R-squared	0.203	0.246	0.184
Outcome mean	49.769	50.492	50.948

Notes: Students' test results (grades) are percentile ranked within cohort. All regressions control for school and cohort fixed effects as well as all covariates presented in Table 3. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.