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

by

Caroline Hall[*], Martin Lundin[#] and Kristina Sibbmark[Δ]

September 12, 2018

Preliminary draft - please do not cite!

Abstract

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 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 performance on standardized tests at the end of compulsory school. 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 student performance on average, nor do we find an impact on the probability of transitioning to upper secondary school or the students' choice of educational track. However, heterogeneity analyzes suggest that 1:1 initiatives may increase inequality in education by worsening math and language skills among students with low educated parents.

Keywords: technology, student performance
JEL-codes: I21

♦ We are grateful to seminar participants at IFAU and Vancouver School of Economics at University of British Columbia for valuable comments. We also want to thank Emma Eliasson Åström for assistance with the data collection.

* Institute for Evaluation of Labor Market and Education Policy (IFAU) and Uppsala Center for Labor Studies (UCLS); email: caroline.hall@ifau.uu.se.
# IFAU; email: martin.lundin@ifau.uu.se.
Δ IFAU; email: kristina.sibbmark@ifau.uu.se.

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 computing programs (or 1:1 programs) is that information and communication technology (ICT) should permeate all areas of the curriculum. 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 pupils' progression to higher levels of education. By surveying all 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 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.[1]

Two recent papers provide 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 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.

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 somewhat higher ages (lower secondary rather than primary school).[2] While the 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 enroll in 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. Future versions of the paper will also try to disentangle different mechanisms at work. For instance, access to register data for all teachers employed will allow us to determine to what extent investments in ICT come at the expense of hiring more teachers, and whether ICT initiatives affect teacher turnover and the sorting of teachers across schools.

We find no significant impact of 1:1 programs on average student performance, the probability of enrolling in upper secondary school, or choice of educational track.

[1] 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.

[2] In later versions of this paper, we will also exploit the fact that our data allow us to examine if the impact of 1:1 program differ for different stages of the education system (primary versus lower secondary school).

However, our heterogeneity analyzes suggest that 1:1 initiatives may increase inequality in education by significantly worsening math and language skills among students with low educated parents.

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, starting with the baseline results, which are followed by a number of robustness checks. We then turn to effects by background characteristics, and finally we try to try to disentangle some of the mechanisms at work. 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 investments 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-pace instructions 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 more easy to use in a 1:1 environment. Third, computers and the Internet provide an opportunity for students to search for and get more and better information that 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[3] 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 showed that there are several implementation challenges associated with these initiatives. 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 1:1. Laptops and tablets might, for example, be used for other reasons than what they primarily are intended to be used for, such as for games and 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 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 ICT in education has generated empirical research within several academic disciplines. Hitherto, the most credible strategies to identify causal effects of investments in ICT on student performance are to be found within economics. The findings are mixed: Goolsbee and Guryan (2006) and Beuremann et al. (2015) find no impact of increased use of ICT on student achievements. 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) and Haelermans (2017) find positive effects.

Haelermans (2017) summarizes the prior international economics literature. She concludes that general investments in ICT without a distinct purpose and plan rarely provide positive results. If technology is used with a clear aim, the findings are more

[3] Research shows that more students per teacher decreases student performance, especially among children with a background of lower socioeconomic status (e.g. Fredriksson, Oosterbeck and Öckert 2016).

positive. Moreover, she notes from the literature that specific digital learning tools seem to generate positive effects in mathematics (but probably not in other subjects) in western countries. In developing countries, positive impacts are found both in language and mathematics.[4]

The empirical literature specifically on the effects of 1:1 programs is mainly dominated by other scholars than economists. The methods used in this literature in order to isolate the effects of computer programs on student achievement are usually not that reliable.[5] In fact, Islam and Grönlund (2016) argue in a recent overview that "overall there is not much research into causal relations between interventions [in form of 1:1 programs] and effects" (p. 193–194).[6] 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.[7] In sum, it is hard to really know if, how and under what conditions providing students with individual laptops or tablets improves student performance.

As discussed in the introduction, there are two recent papers which provide credible evidence on effects of 1:1 programs using experimental or quasi-experimental designs: 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.

We contribute to the literature in several ways: First, 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) bears 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

---

[4] Haelermans (2017) own research, conducted in form of experiments in the Netherlands, suggest positive impacts of certain ICT tools both in mathematics and language.

[5] Within this literature, there are attempts to construct comparison groups and control for selection effects by including covariates and/or by using some kind of basic difference in differences approach. This research provides valuable information, but it is fair to say that any causal statement based on these studies rely on strong assumptions.

[6] For a similar way of reasoning, see Zheng et al. (2016, p. 25).

[7] For overviews, see Islam and Grönlund (2016) and Zheng et al. (2016).

limits how the technology could be used but also the potential sources of distractions in the classroom. While the 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. Future versions of the paper will also try to disentangle different mechanisms at work by exploiting the rich register data we have at hand.

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[8]) 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.[9] 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.[10] 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

[8] Students who enroll in vocational programs can also attain college eligibility by choosing certain optional courses.
[9] There are very few fully private schools in Sweden.
[10] Böhlmark, Holmlund and Lindahl (2015, p. 45) 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.

personal characteristics. In order to receive public funding, they are also not allowed to charge a tuition fee. In the school year 2016/17, around 15 percent of the pupils in compulsory school attended an independent school (Skolverket 2017). Most of them have a general profile, but there are also religious schools and schools with a special pedagogical orientation.[11]

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. When a student enrolls in an independent school, his or her home municipality must provide that school with a grant, which should be based on the same principle as the municipality uses for its own schools.

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

ICT has become an increasingly important part of teaching in compulsory school in Sweden. For example, the share of teachers with access to an own computer has increased from 27 percent in 2008 to 96 percent in 2015. During the same time period, the number of students per computer decreased from 5.9 to 1.8. There are basically no differences between public schools and independent schools in these regards (Skolverket 2016a).

Small-scale 1:1 programs were introduced for the first time in Sweden around 2007 (Tallvid 2015). In 2015, almost 30 percent of the students in compulsory school had access to an own computer or laptop provided by the school. This share was about the same in public and independent schools (Skolverket 2016a).

There is a lively debate in Sweden regarding the increased use of ICT 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 educational system. Others have a much more skeptical view, highly emphasizing the downsides of increased use of ICT in Swedish classrooms. However, it is obvious that improved access to individual laptops or tablets for students is a rising trend in Sweden right now. The Swedish National Agency for Education

[11] There are no restrictions regarding the ownership of independent schools in order to be eligible for public funding and as such there are both non-profit and for profit independent schools.

recommends that all pupils, from first grade and upwards, be provided with a personal laptop or tablet (Skolverket 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 very positive toward the initiatives – they think that teaching and learning are improved (Tallvid 2015).[12] But there are also studies suggesting that practices in 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. Thus, certain schools without 1:1 integrate computers in education to a greater extent than certain 1:1 schools. However, a study by the National Agency of Education shows that ICT, on average, is in practice used much more in schools where pupils are provided with a personal laptop or tablet than in other schools (Skolverket 2016). For example, 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).

[12] Findings in Grönlund (2014) indicate, however, that similar positive attitudes exist at schools without 1:1 computer programs as well.

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	53	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: Skolverket (2016).

4 Data and descriptive statistics

We have collected data on 1:1 programs from primary and lower secondary schools (grades 4–9) during 2008–2016, across 26 Swedish municipalities. In order to make sure our sample would include a significant proportion of schools that had implemented 1:1 programs, half on these municipalities were selected based on prior information indicating a relatively wide use of 1:1 programs; for example, due to municipality-wide initiatives. In the selection of these municipalities we also made sure to get a spread of municipalities in terms of population size, average education level and geographic location. Each of the selected municipalities was then matched with a municipality with similar characteristics, but without any municipality-wide 1:1 initiative, for which we also collected data. We describe the selection of municipalities in greater detail in Appendix A.

A short questionnaire was sent by email to all schools (671) in the selected municipalities, followed by phone calls to non-respondents. Almost 75 percent of the schools that were contacted responded to our questions, resulting in a sample of 503 schools. 209 of these schools are lower secondary schools (grades 7–9) and 347 of the schools encompass all grades in upper primary school (grades 4–6). In the main analysis, we restrict the analysis to 173 lower secondary schools; see Section 5. Future versions of the paper will also provide results for a sample of primary schools.

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 grades 6–9. In 2016, 151 of the schools had implemented a 1:1 programs in grade 7. This corresponds to almost 70 percent of the schools (with grade 7) in our sample (see Table A2).[13] Similar figures are reported for grades 6, 8 and 9. In grades 4–5, computer programs are less common. In 2016, 52 schools had a 1:1 program in grade 4 and 41 schools in grade 5. This corresponds to around 10 percent of the schools included in our sample (see Table A2).

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.

We have linked the school level data on 1:1 programs to individual level register data on school attendance in different grades (available since 2008), and on students' school performance, measured by their performance on standardized tests in Swedish, English and Mathematics which are given in grade 6 (or 5) and 9 (available since 2009 and

[13] 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, around 50 percent of Swedish students in grade 7–9 participated in a 1:1 program in 2015 (Skolverket 2016a).

2005, respectively).[14] 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). Our dataset also includes background variables for the students (e.g., age, immigrant background, parents' educational background and parents' income[15]) and the schools (e.g. class size) as well as information about the teachers employed (e.g. education level and years of experience). The register data come from Statistics Sweden.

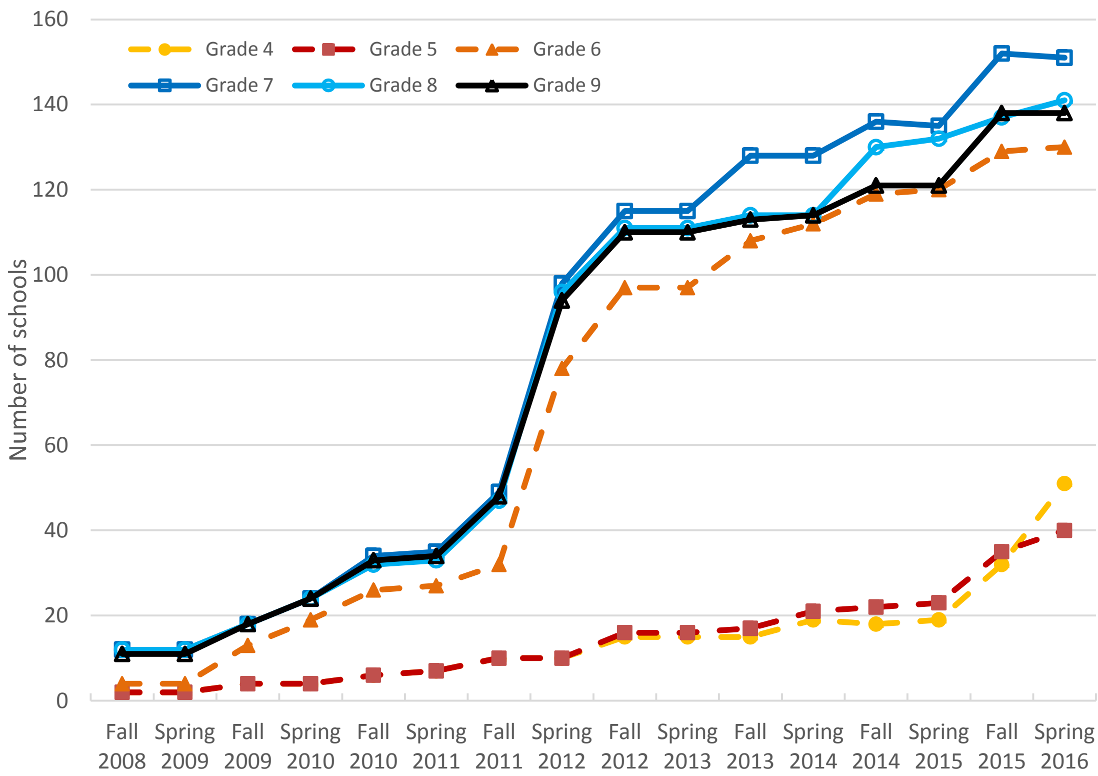

Figure 1. Presence of 1:1 programs in different grades among schools in the sample

Note: The sample consists of 503 schools offering some (or in some cases all) of the grades 4–9.

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 that choose to implement these programs and whether the selection of schools to these programs have changed over time. Table 2 shows how the

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

[15] 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.

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 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.[16]

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. [17] There seems to be somewhat of a tendency for early implementers to have a larger share of male students, but the estimate is only statistically significant for 2011. The size of the estimate implies that an increase in the share of female students by one standard deviation (0.09) is associated with a 5 percentage point increase in the probability that the school has launched a 1:1 program. 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.

[16] 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.

[17] 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 0.05–0.07.

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
Share female students	-0.609	-0.671*	-0.607	0.175
	(0.370)	(0.379)	(0.518)	(0.557)
Average test result (percentile ranked)	0.005	-0.001	-0.002	-0.005
	(0.004)	(0.005)	(0.006)	(0.006)
Share foreign born students	0.295	0.053	-0.255	0.074
	(0.322)	(0.666)	(0.566)	(0.649)
Share with foreign born parents	-0.018	0.074	-0.034	0.356
	(0.152)	(0.360)	(0.311)	(0.335)
Share mothers with post-sec education	0.163	0.140	-0.308	-0.393
	(0.300)	(0.467)	(0.486)	(0.524)
Share fathers with post-sec education	0.005	-0.256	0.021	-0.165
	(0.221)	(0.332)	(0.428)	(0.485)
Average wage earnings, father	-0.001**	-0.000	-0.001	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
Average wage earnings, mother	0.000	0.002	0.002*	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Public school	-0.004	0.029	0.072	-0.027
	(0.060)	(0.100)	(0.120)	(0.127)
No of students	0.001*	0.000	0.000	-0.000
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	162	162	162	162
R-squared	0.512	0.536	0.620	0.573
Mean of outcome	0.074	0.210	0.426	0.506

Notes: School characteristics are measured among students who finished grade 9 in 2008. Average test result refers to students' average grade on the standardized tests in Mathematics, English and Swedish. The regressions control for municipality fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

5 Estimating the impact of 1:1 programs

To capture effects of 1:1 programs, we will 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.[18] 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 173

[18] Grade repetition during compulsory school is rare in Sweden.

schools, out of which 75 schools (43 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.[19] 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 9th 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 generally are 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.[20] 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).

[19] 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.

[20] 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-sec educ	-0.026
	(0.021)
Father has post-sec educ	-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 ("cohort"). y_{isc} is the (percentile ranked) grade on the standardized test in Mathematics, Swedish or English, which the students take at the end of 9th grade.[21] $ICTprogram_{sc}$ 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.[22]

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 6.2.

[21] The individuals are ranked among all pupils in the country who took the test the same semester.
[22] In Section 6.2 we relax the assumption that the effect is linear in the number of semesters of exposure.

6 Results

This section presents the results from the empirical analysis. We begin by presenting results for the full sample of lower secondary school students (Section 6.1). This presentation is followed by a number of robustness checks (Section 6.2). Thereafter, we examine if the impact of 1:1 programs differ for various subgroups of students (Section 6.3). Last, we try to shed light on some of the potential mechanisms behind our findings (Section 6.4).

6.1 Effects of 1:1 programs

Table 4 displays estimates for the two regression models discussed above 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 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 relative effects are not altered that much by the inclusion of individual background controls, which is reassuring. To the extent that unobserved components of skills are positively correlated with observed components, this suggests that unobserved heterogeneity does not significantly bias these findings.

Table 4: Effects of 1:1 programs on (percentile ranked) grades on standardized tests, gr 9

	(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)
Observations	44,920	44,479	44,920	44,479
R-squared	0.081	0.203	0.081	0.203
Outcome mean	49.769	49.769	49.769	49.769
Relative effect	*-0.008*	*-0.010*	*-0.005*	*-0.004*
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)
Observations	46,217	45,747	46,217	45,747
R-squared	0.075	0.246	0.075	0.246
Outcome mean	50.492	50.492	50.492	50.492
Relative effect	*-0.005*	*-0.006*	*-0.005*	*-0.004*
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)
Observations	45,764	45,308	45,764	45,308
R-squared	0.078	0.184	0.078	0.184
Outcome mean	50.948	50.948	50.948	50.948
Relative effect	*-0.003*	*-0.005*	*-0.002*	*-0.000*

Note: 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 relative effect is obtained by relating the estimate to the mean of the dependent variable.

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; e.g. 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 9^{th} 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.

The first panel of Table 5 displays estimates from the same models as in Table 4, but where the outcome is an indicator for whether the student was admitted to a regular upper secondary school program, either college-preparatory or vocational, three years after he or she began 7th grade. In the second panel the outcome is instead an indicator for whether the student was admitted to a college-preparatory program. We can see that all estimates are statistically insignificant 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.

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)
Observations	49,889	49,190	49,889	49,190
R-squared	0.055	0.122	0.055	0.122
Outcome mean	0.854	0.854	0.854	0.854
Relative effect	*-0.006*	*-0.007*	*-0.002*	*-0.002*
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)
Observations	49,889	49,190	49,889	49,190
R-squared	0.076	0.184	0.076	0.184
Outcome mean	0.617	0.617	0.617	0.617
Relative effect	*-0.000*	*-0.005*	*-0.000*	*-0.000*

Note: 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 relative effect is obtained by relating the estimate to the mean of the dependent variable.

6.2 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.[23]

Reassuringly, the results from the placebo regressions do not indicate that school that implement 1:1 program differ in trends in student performance two years preceding the launch of these programs, at least not as measured by results on the standardized tests in mathematics, Swedish or English. The placebo estimates are statistically insignificant for all three outcomes both one and two years before the start date of the programs.

[23] 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 173 schools included in our main analysis, 147 are included in the placebo regressions and 137 can be followed all the way back to 2005.

Table 6: Placebo estimates

	(1) Math	(2) Swedish	(3) English
A. Placebo analysis, t-1			
Placebo estimate	-0.223	0.159	0.315
	(0.265)	(0.279)	(0.269)
Observations	95,211	97,217	96,652
R-squared	0.184	0.222	0.176
Outcome mean	49.708	49.648	50.858
Relative effect	*-0.004*	*0.003*	*0.006*
B. Placebo analysis, t-2			
Placebo estimate	-0.148	-0.133	-0.055
	(0.182)	(0.203)	(0.148)
Observations	89,148	91,032	90,561
R-squared	0.182	0.220	0.174
Outcome mean	49.693	49.551	50.812
Relative effect	*-0.003*	*-0.003*	*-0.001*

Note: 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. The grades on the standardized tests in Mathematics, Swedish and English are percentile ranked by 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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The relative effect is obtained by relating the estimate to the mean of the dependent variable.

For the last four cohorts of the sample we have information on results on standardized tests (in the same subjects) from an earlier point in time – before the students enrolled in 7^{th} grade. Those who enrolled in 7^{th} grade in 2010 and 2011 took standardized tests in math, Swedish and English at the end of 5^{th} grade, and those who enrolled in 2012 and 2013 took tests in the same subjects at the end of 6^{th} 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, which means that we only have a rather crude measure of previous performance. 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 the student passed all sub-tests in math, Swedish and English, respectively, to our baseline model.[24] The estimated effects of exposure to 1:1 programs remain statistically insignificant for all three outcomes.

Table 7: Robustness to controlling for results on earlier standardized test

	(1) Math	(2) Swedish	(3) English
No. of semesters with ICT program	0.062	-0.409	-0.145
	(0.790)	(0.575)	(0.495)
Pass on earlier math test	18.287***		
	(0.659)		
Pass on earlier Swedish test		19.935***	
		(0.538)	
Pass on earlier English test			30.602***
			(0.572)
Observations	21,955	21,780	22,012
R-squared	0.271	0.331	0.317
Outcome mean	50.674	52.152	52.060
Relative effect	0.001	-0.008	-0.003

Note: All regressions control for school and cohort fixed effects as well as all covariate 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. The relative effect is obtained by relating the estimate to the mean of the dependent variable.

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 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 three outcome variables. Base on this analysis, we find no reason to reject the linear specification.[25]

[24] 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.

[25] It should be noted that none of the schools 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.

Table 8: Specification with nonlinear effects

	(1) Math	(2) Swedish	(3) English
ICT program	-0.325	-0.251	-0.686
	(0.857)	(0.754)	(0.805)
ICT program long (> 2 semesters)	-0.430	-0.161	0.963
	(0.936)	(0.941)	(0.806)
Observations	44,479	45,747	45,308
R-squared	0.203	0.246	0.184
Outcome mean	49.769	50.492	50.948

Note: The regressions control for school and cohort fixed effects as well as all covariates presented in Table 3. The grades on the standardized tests in Mathematics, Swedish and English are percentile ranked by cohort. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.

6.3 Heterogeneous effects

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. In this section we investigate whether the impact of 1:1 programs differs for boys and girls as well as depending on the parents' level of education. We also replicate the analysis for the sample of primary schools that responded to our survey, in order to examine if the impact of 1:1 programs differs for younger vs. older pupils.

6.3.1 Effects by parental education and gender

Table 9 displays results from our 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 high level of education. A high level of parental education is defined as at least one of the parents having post-secondary education. The estimated coefficient for $ICTsemesters_{sc}$ thus gives us the estimated effect (per semester) for pupils with low-educated parents, while we get the estimated effect for pupils with highly educated parents by adding up the two estimates displayed in the table.

The results in panel A suggest that 1:1 programs have a negative impact on performance in mathematics and Swedish among students with low-educated parents. Both of these estimates are statistically significant at the 10 percent level. If we relate the estimates to the means of the dependent variables for the same group of pupils (40.6 for math and 42.5 for Swedish; see Table B2), they correspond to reductions by 1.1 percent per semester for mathematics, and 0.9 percent for Swedish. These are not negligible effects for students who take part of these programs for several semesters. The estimates are similar if we instead estimate separate regressions for students whose parents have a low vs. high level of education (see Table B2), but splitting the sample reduces precision and the estimate for Swedish is no longer statistically significant. For English, which is most students' second language, the estimated effect displayed in Table 9 is very close to zero and statistically insignificant.

We can also see that the estimates for all three standardized tests sum up to effects close to zero for pupils whose parents have a high level of education. Our results thus indicate that extensive use of ICT in teaching may increase inequality in education by worsening math and language skills among students with low-educated parents.

The results presented in panel B suggest that the increased inequality in school performance also may be carried over to upper secondary school. The estimates indicate that 1:1 programs result in a lower probability of being admitted to a regular upper secondary school program among pupils with low-educated parents, but not among pupils whose parents have a higher level of education. That worse performance in math and Swedish influences upper secondary school admittance would not be a surprising finding since a passing grade in both of these subjects is required for eligibility.[26] However, the patterns of results for upper secondary school admittance are less clear if we instead estimate separate regressions for the two sub-samples of students; see Table B2. Hence, these results should be interpreted with some caution.

[26] To be eligible for a vocational upper secondary school program, the student needs a passing grade in math, Swedish and English. Eligibility for college-preparatory programs additionally requires passing grades in an additional nine subjects.

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	-0.456*	-0.395*	-0.076
	(0.259)	(0.226)	(0.230)
No. of semesters with ICT program*	0.515**	0.387*	0.082
Parents have high level of education	(0.241)	(0.226)	(0.229)
Observations	44,479	45,747	45,308
R-squared	0.203	0.246	0.184
B: Admitted to upper secondary school	Admitted (any reg. program)	Admitted to college-prep. program	
No. of semesters with ICT program	-0.006**	-0.004	
	(0.003)	(0.003)	
No. of semesters with ICT program*	0.010***	0.008*	
Parents have high level of education	(0.003)	(0.004)	
Observations	49,190	49,190	
R-squared	0.123	0.185	

Note: High level of parental education is defined as at least one parent with post-secondary education. The grades on the standardized tests are percentile ranked by 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.

In Table 10 we have instead interacted the treatment indicator with a dummy variable for being female. All in all, we find no clear evidence of important differences between the genders. The estimate for math points to a more negative impact of 1:1 for girls than for boys, but the estimate is not precise enough for us to be able to reject the null hypothesis of no gender difference (p-value 0.115).[27] When it comes to Swedish, English, and the probability of being admitted to a regular upper secondary school program, there are no indications of differential impacts between boys and girls. For the last outcome examined, the probability of being admitted to a college-preparatory program, the results point to a negative (but statistically insignificant) effect for boys, but not for girls – the estimated coefficient for the interaction term is positive and statistically significant. However, if we instead estimate separate models for boys and girls there are no clear differences between the genders; see Table B3.

[27] Algan and Fortin (2017) find a negative girl-specific effect of intense computer gaming on math test scores, while the effect for boys is positive.

Table 10: Effects of 1:1 programs by gender

A: Grades on standardized test	(1) Math	(2) Swedish	(3) English
No. of semesters with ICT program	0.023	-0.287	-0.086
	(0.242)	(0.242)	(0.228)
No. of semesters with ICT program* Girls	-0.434 (0.273)	0.179 (0.257)	0.105 (0.236)
Observations	44,479	45,747	45,308
R-squared	0.203	0.246	0.184
B: Admitted to upper secondary school	Admitted (any reg. program)	Admitted to college-prep. program	
No. of semesters with ICT program	-0.003	-0.007	
	(0.003)	(0.004)	
No. of semesters with ICT program* Girls	0.003 (0.003)	0.014*** (0.005)	
Observations	49,190	49,190	
R-squared	0.122	0.184	

Note: The grades on the standardized tests are percentile ranked by 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.

6.4 Understanding the role of some potential mechanisms

Future versions of the paper will examine if ICT investments tend to crowd out spending on teachers, and whether 1:1 programs impacts teacher sorting.

6.4.1 Do effects vary depending on the type of technology used or teachers' training?

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.

In the survey we asked schools to specify whether the pupils were provided with laptops or tablets computers. 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 11 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 sizeable negative estimates for the impact of 1:1 tablet programs on students' test results. For both Swedish and English the estimated effects are statistically significant and correspond to reductions by 2.3 and 1.3 percent, respectively, per semester. 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 may lay in understanding how 1:1 programs alter the teaching practices used in the classroom.

Table 11: 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	-0.120	-0.045	0.063	-0.002	0.001
	(0.250)	(0.239)	(0.213)	(0.003)	(0.003)
No. of semesters with tablet program	-0.650	-1.178***	-0.669**	-0.001	-0.008
	(0.506)	(0.344)	(0.322)	(0.004)	(0.007)
Observations	44,479	45,747	45,308	49,190	49,190
R-squared	0.203	0.246	0.184	0.122	0.184
Outcome mean	49.769	50.492	50.948	0.854	0.617
Rel. effect laptops	*-0.002*	*-0.000*	*0.001*	*-0.002*	*0.002*
Rel. effect tablets	*-0.013*	*-0.023*	*-0.013*	*-0.001*	*-0.013*

Note: All regressions control for school and cohort fixed effects as well as all covariates presented in Table 3. The grades on the standardized tests are percentile ranked by cohort. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1. The relative effects are obtained by relating the estimates to the mean of the dependent variable.

Previous studies have highlighted the importance of teachers having sufficient training in how to incorporate the new technology into their 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 teaching, and whether they had any documented strategy for teacher training in relation to increased use of technology in teaching. Around half of the schools that had initiated 1:1 programs answered "yes" to one or both of these questions.[28] 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 B4.

7 Concluding remarks

One-to-one computing programs are becoming increasingly popular in schools all over the world. By combining survey data on the implementation of such programs from a large number of Swedish schools with rich administrative data, we estimate the impact of 1:1 programs on students' educational performance using a difference in differences design. We find no significant impact on student performance on average, as measured by their results on standardized tests in Mathematics and language at the end of lower secondary school. This finding is in line with the results of Crista et al. (2012) and De Melo et al. (2014), who find no overall impact on test scores in Mathematics and language from 1:1 programs in Uruguay and rural Peru. However, splitting our sample according to parental education suggests that 1:1 initiatives may increase inequality in education by significantly worsening math and language skills among students with low educated parents.

[28] 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".

References

Algan, Y. and N. Fortin (2017), ”Computer Gaming and the Gender Math Gap: Cross-Country Evidence among Teenagers”, unpublished manuscript (October 2017).

Angrist, J. and V. Lavy (2002), “New evidence on classroom computers and pupil learning”, *The Economic Journal* 112(482): 735–765.

Banerjee, A., S. Cole, E. Duflo and L. Linden (2005), “Remedying education: Evidence from two randomized experiments in India”, *Quarterly Journal of Economics* 122(3): 1235–1264.

Beland, L. P. and R. Murphy (2016), “Ill communicaton: Technology, distraction and student performance”, *Labour Economics* 41(1): 61–76.

Beuermann, D. W., J. P. Cristia, S. Cueto, O. Malamud and Y. Cruz-Aguayo (2015), “One laptop per child at home: Short-term impacts from a randomized experiment in Peru”, *American Economic Journal: Applied Economics* 7(2): 53–80.

Bulman, G. and R. W. Fairlie (2016), “Technology and education: Computers, software, and the internet” in Hanushek, E. A., S. Machin and L. Woessman (eds.), *Handbook of the Economics of Education*, Elsevier, Amsterdam.

Böhlmark, A., H. Holmlund and M. Lindahl (2015), ”School choice and segregation: evidence from Sweden”, Working Paper 2015:8, IFAU, Uppsala.

Carter, S. P., K. Greenberg and M. Walker (2017), “The impact of computer usage on academic performance: Evidence from a randomized trial at the United States Military Academy”, *Economics of Education Review* 56: 118–132.

Cristia, J. P., P. Ibarraran, S. Cueto, A. Santiago and E. Severin (2012), “Technology and child development: Evidence from the one laptop per child program”, IDB Working Paper Series No. IDB-WP-304, Inter-American Development Bank.

de Melo, G., A. Machado and A. Miranda (2014), “The Impact of a One Laptop per Child Program on Learning: Evidence from Uruguay”, IZA Discussion paper No. 8489.

Danielsson, H. (2015), ”Förbättra resultaten i skolan genom att kasta ut datorerna från lektionerna”, DN Debatt, *Dagens Nyheter*, 17 April. Available at:

http://www.dn.se/debatt/forbattra-resultaten-i-skolan-genom-att-kasta-ut-datorerna-fran-lektionerna/ (accessed 8 March 2017).

Fredriksson, P., H. Oosterbeek and B. Öckert (2016), ”Long-term effects of class size”; *Quarterly Journal of Economics* 128: 249–85.

Goolsbee, A. and J. Guryan (2006), “The impact of internet subsidies in public schools”, *Review of Economics and Statistics* 88(2): 336–347.

Grönlund, Å. (2014), *Att förändra med teknik: Bortom ”en dator per elev”*, Örebro University, Örebro.

Grönlund, Å., A. Andersson and M. Wiklund (2014), ”Unos uno årsrapport 2013”, Örebro University, Örebro.

Haelermans, C. (2017), *Digital tools in education. On usage, effect and the role of the teacher*, SNS Förlag, Stockholm.

Hanushek, E. A. (1986), “The economics of schooling: Production and efficiency in public schools”, *Journal of Economic Literature* 24(3): 1141–1177.

Hatakka, M., A. Andersson & and Å. Grönlund (2013), ”Students’ use of one to one laptops: a capability approach analysis”, *Information Technology & People* 26(1): 94–112.

Islam, M. S. and Å. Grönlund (2016), ”An international literature review of 1:1 computing in schools”, *Journal of Educational Change* 17: 191–222.

Leuven, E., M. Lindahl, H. Oosterbeek and D. Webbink (2007), “The effect of extra funding for disadvantaged pupils on achievement”, *Review of Economics and Statistics* 89(4): 721–736.

Lindgren, K. (2011), ”Datorn – fälla eller frälsare?”, *Lärarnas Tidning*, 1 December. Available at: http://lararnastidning.se/datorn-falla-eller-fralsare/ (accessed 8 March 2017).

Machin, S., S. McNally and O. Silva (2007), “New technology in schools: Is there a payoff?”, *Economic Journal* 117: 1145–1167.

Mueller, P. A. and D. M. Oppenheimer (2014), ”The pen is mightier than the keyboard. Advantages of longhand over laptop note taking”, *Psychological Science* 25(6): 1159–1168.

Molin, L. and A. Lantz-Andersson (2016), “Significant structuring resources in the reading practices of a digital classroom”, *Journal of Information Technology Education: Research* 15: 131–156.

Sana, F., W. Tina and N. J. Cepeda (2013), “Laptop multitasking hinders classroom learning for both users and nearby peers”, *Computers & Education* 62: 24–31.

Skolverket (2016a), *IT-användning och IT-kompetens i skolan. Skolverkets IT-uppföljning 2015*, Skolverket (National Agency for Education), Stockholm.

Skolverket (2016b), *Redovisning av uppdraget om att föreslå nationella IT-strategier för skolväsendet*, Skolverket (National Agency for Education), Stockholm.

Skolverket (2017), ”Skolor och elever i grundskolan läsåret 2016/17”, The Internet homepage of the National Agency for Education, https://www.skolverket.se/statistik-och-utvardering/statistik-i-tabeller/ grundskola/skolor-och-elever/skolor-och-elever-i-grundskolan-lasaret-2016-17-1.258586

Swedish Government (2017), “Nationell digitaliseringsstrategi för skolväsendet”, Bilaga till regeringsbeslut I:1, 2017-10-19, Utbildningsdepartementet, Stockholm.

Tallvid, M. (2015), *1:1 i klassrummet – analyser av en pedagogisk praktik i förändring*, University of Gothenburg, Gothenburg.

Thurfjell, K. (2017), “Brittisk expert: Barn lär sig sämre med digitala hjälpmedel”, *Svenska Dagbladet*, 1 March. Available at: https://www.svd.se/brittisk-expert-barn-lar-sig-inte-med-digitala-hjalpmedel/om/sverige (accessed 8 March 2017).

Zheng, B., M. Warschauer, C. Lin and C. Chang (2016), ”Learning in one-to-one laptop environments: A meta-analysis and research synthesis”, *Review of Educational Research* 86(4): 1052–1084.

Appendix A: Data collection

To identify schools with and without 1:1 computer programs, information was collected through a questionnaire with all primary and lower secondary schools (grades 4–9) in 26 Swedish municipalities. The survey took place during the period June 2016 to March 2017.

To be able to analyze the effects of 1:1, we needed a relatively large number of schools that had implemented 1:1 programs. Thus, we consulted local newspapers and various internet sources (e.g. municipal 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 characteristics but with no prior indication 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 4–9 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 between 2008 and 2015 are not included in the sample; collecting information from these schools would basically have been an impossible task.

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 written plan for how teachers should integrate 1:1 computers in their daily work in the classroom, and whether the school had a written 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. In addition, internal missing data for these variables is quite common.

The initial e-mail was followed up by two e-mail remainders. If the school did not reply within 14 days, we started to make telephone calls. In the end, we received data from 503 of 671 schools.[29] This implies a good overall responses rate of 75 percent. Table A1 shows that the responses rates varied among municipalities from 54 percent (Haninge) to 100 percent (Skellefteå, Falkenberg and Stenugnsund).

Table A2 shows that laptop programs existed in around half of the schools in our sample in grade 7–9, in around 25 percent in grade 6 and in around 5 percent in grade 4–5 in 2016. Tablets were used by 13–19 percent of the schools in grade 6–9 and in 5 percent of the schools in grade 4–5. 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.

We believe that these data are of good quality; it should be quite easy to report whether the school has a 1:1 program or not and what grades are affected. However, some errors are likely to exist. In Figure A2, we know there is a small error: Three schools have reported that a 1:1 tablet program started in their schools in grade 4 before the spring semester of 2011. Two of them also had tablet programs in grade 5 and 6. However, no tablets were on the market at that point in time. Thus, it is not possible that tablets were used in these schools. It is possible that the schools have not reported the correct start date. Another possibility is that laptops rather than tablets were used. We have excluded these few schools from the analysis.

[29] In 2016, grade 4 existed in 417 of the 503 schools included in our sample. The corresponding figures for grade 5–9 were the following: 414 (gr 5), 347 (gr 6), 216 (gr 7), 211 (gr 8) and 209 (gr 9).

Table A1. Selection of municipalities

Municipality (response rate)	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 (88 %)	Yes	Norrland	10,112	224	18.8	20.1
Pajala (83 %)	No	Norrland	6,429	224	13.0	22.6
Nykvarn (67 %)	Yes	Svealand	9,035	213	16.5	29.3
Vaxholm (67 %)	No	Svealand	10,747	213	9.8	48.3
Gagnef (80 %)	Yes	Svealand	10,107	212	15.2	26.8
Leksand (78 %)	No	Svealand	15,288	214	13.9	31.4
Stenungsund (100 %)	Yes	Götaland	23,657	217	14.5	34.6
Höganäs (67 %)	No	Götaland	24,248	212	13.2	38.7
			Middle size (25,000–100,000 inhabitants)			
Ljungby (65 %)	Yes	Götaland	27,430	207	17.4	26.5
Oskarshamn (83 %)	No	Götaland	26,309	200	18.3	26.6
Hudiksvall (60 %)	Yes	Norrland	36,905	213	17.8	27.6
Söderhamn (62 %)	No	Norrland	25,987	210	18.6	22.4
Falkenberg (100 %)	Yes	Götaland	40,451	205	20.0	25.6
Trelleborg (94 %)	No	Götaland	41,558	205	18.2	27.1
Sollentuna (60 %)	Yes	Svealand	62,097	221	10.1	51.2
Solna (64 %)	No	Svealand	65,289	211	9.6	53.3
Skellefteå (100 %)	Yes	Norrland	71,862	209	11.9	32.6
Östersund (77 %)	No	Norrland	58,914	215	11.2	40.9
Botkyrka (85 %)	Yes	Svealand	80,055	205	23.0	28.9
Haninge (54 %)	No	Svealand	74,968	200	19.3	26.9
Växjö (69 %)	Yes	Götaland	81,074	214	12.3	42.5
Karlstad (63 %)	No	Götaland	83,994	211	11.4	43.1
			Large (at least 100,000 inhabitants)			
Malmö (65 %)	Yes	Götaland	286,535	201	14.6	43.0
Uppsala (93 %)	No	Svealand	190,668	213	10.7	52.9
Västerås (69 %)	Yes	Svealand	134,468	209	13.8	39.5
Norrköping (62 %)	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. Responses rates in our web survey are presented within parentheses after municipality names. 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.

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
4	417	31	7	21	5
5	414	22	5	19	5
6	347	86	25	45	13
7	216	109	50	42	19
8	211	105	50	36	17
9	209	106	51	32	15

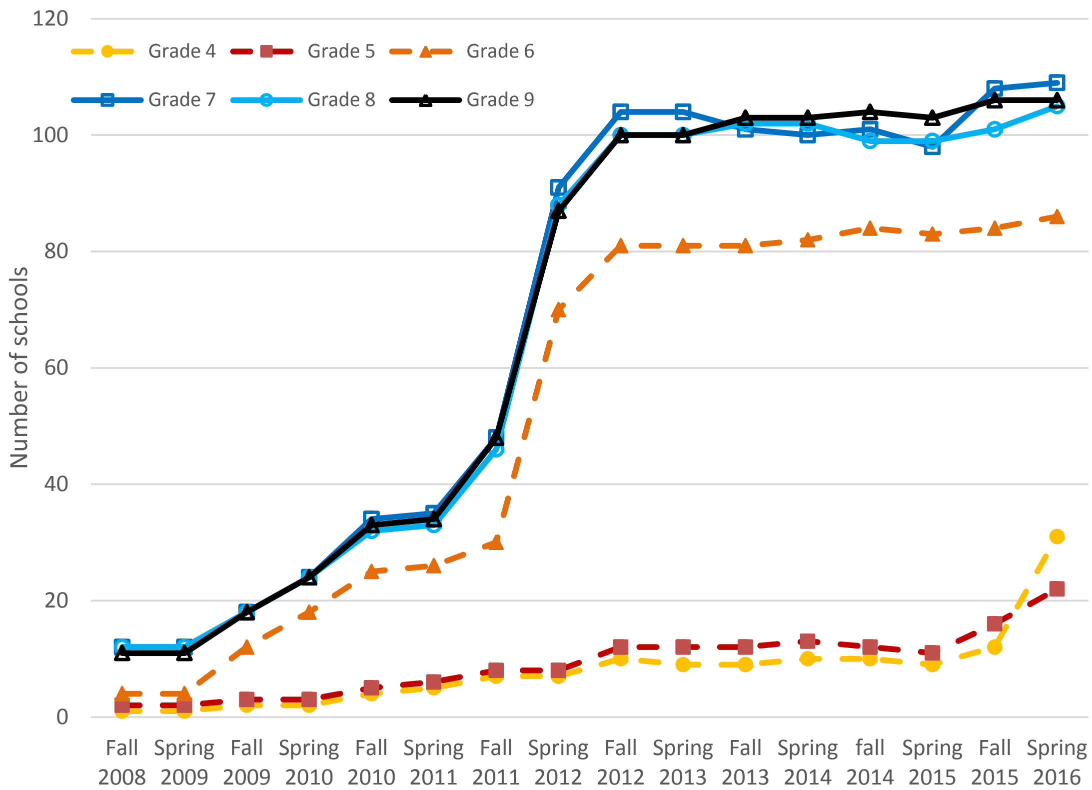

Figure A1. Presence of 1:1 laptop programs in different grades among the schools in the sample

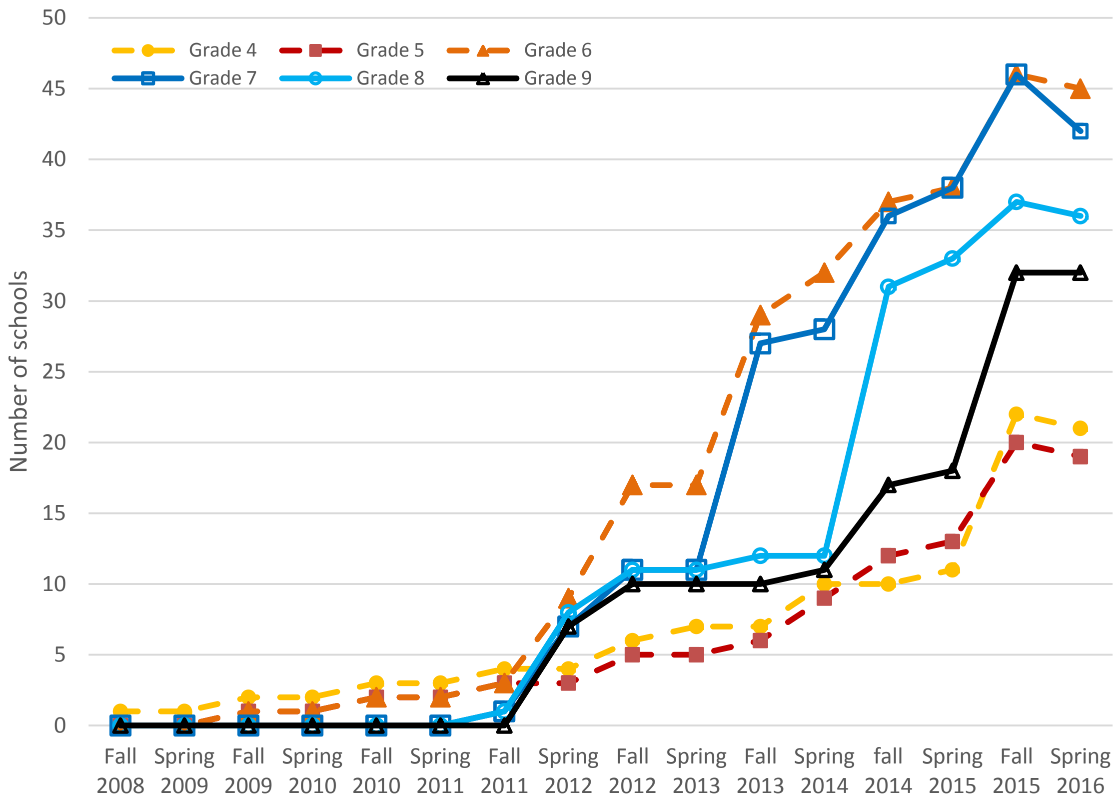

Figure A2. Presence of 1:1 tablet programs in different grades among schools in the sample

Appendix B: Additional tables

Table B1: 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		

Note: 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 B2: 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	0.167	-0.524*
	(0.301)	(0.298)
Observations	23,772	20,707
R-squared	0.152	0.087
Outcome mean	57.937	40.589
B: Swedish		
No. of semesters with ICT program	0.029	-0.363
	(0.283)	(0.255)
Observations	24,262	21,485
R-squared	0.208	0.166
Outcome mean	57.757	42.463
C: English		
No. of semesters with ICT program	0.178	-0.233
	(0.254)	(0.256)
Observations	24,196	21,112
R-squared	0.132	0.094
Outcome mean	58.509	42.465
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
Outcome mean	0.913	0.793
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
Outcome mean	0.769	0.467

Note: 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. The grades on the standardized tests are percentile ranked by 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 B3: Effects of 1:1 programs by gender. Separate regressions for boys and girls.

	(1) Boys	(2) Girls
A: Mathematics		
No. of semesters with ICT program	0.040	-0.412
	(0.255)	(0.353)
Observations	22,753	21,726
R-squared	0.208	0.207
Outcome mean	49.738	49.802
B: Swedish		
No. of semesters with ICT program	-0.166	-0.208
	(0.268)	(0.309)
Observations	23,290	22,457
R-squared	0.190	0.206
Outcome mean	43.434	57.808
C: English		
No. of semesters with ICT program	0.186	-0.258
	(0.256)	(0.258)
Observations	23,108	22,200
R-squared	0.190	0.189
Outcome mean	50.419	51.498
D: Admitted to upper secondary school (any regular program)		
No. of semesters with ICT program	-0.002	-0.002
	(0.003)	(0.004)
Observations	25,215	23,975
R-squared	0.128	0.126
Outcome mean	0.843	0.866
E: Admitted to college-preparatory program		
No. of semesters with ICT program	-0.003	0.002
	(0.005)	(0.004)
Observations	25,215	23,975
R-squared	0.204	0.160
Outcome mean	0.571	0.665

Note: The grades on the standardized tests are percentile ranked by 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 B4: 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	-0.126 (0.409)	-0.169 (0.369)	0.052 (0.297)	-0.003 (0.004)	-0.005 (0.004)
No. of semesters with 1:1 program*ICT strategy	-0.112 (0.441)	-0.051 (0.412)	-0.149 (0.351)	0.003 (0.004)	0.008 (0.005)
Observations	44,479	45,747	45,308	49,190	49,190
R-squared	0.203	0.246	0.184	0.122	0.184
A: Presence of strategy for teacher training					
No. of semesters with 1:1 program	-0.169 (0.361)	0.043 (0.338)	-0.055 (0.280)	-0.001 (0.003)	-0.003 (0.004)
No. of semesters with 1:1 pr.*training strategy	-0.045 (0.410)	-0.475 (0.391)	0.040 (0.345)	-0.001 (0.004)	0.006 (0.005)
Observations	44,479	45,747	45,308	49,190	49,190
R-squared	0.203	0.246	0.184	0.122	0.184

Note: All regressions control for school and cohort fixed effects, presence of ICT/training plan as well as all covariates presented in Table 3. The grades on the standardized tests are percentile ranked by cohort. Robust standard errors in parentheses, clustered on schools. *** p<0.01, ** p<0.05, * p<0.1.