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Just the right amount of caution?

Remote instruction and student performance in Sweden during the COVID-19 pandemic

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Remote instruction and student performance in Sweden during the COVID-19 pandemic[a]

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Abstract

This study examines the impact of distance learning on educational outcomes for lower secondary school students in Sweden during the COVID-19 pandemic. We leverage variation in the implementation of remote instruction across schools and compare pre-pandemic and pandemic-affected cohorts using a difference-in-differences design with entropy balancing weights. We examine effects on grade 9 students' test scores on standardized tests and their transition to upper secondary school. Our findings suggest that students in schools that adopted remote instruction performed similarly to those in schools that maintained in-person teaching throughout the pandemic. Moreover, progression to upper secondary school was not negatively affected. In some cases, we even find evidence of positive effects of remote instruction. We find some support for the interpretation that these positive effects may be due to remote instruction enabling more teaching hours during a period with high teacher and student absence.

Keywords: Remote instruction, distance learning, school performance, COVID-19
JEL-codes: I21; I28

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1 Introduction

The COVID-19 pandemic resulted in a global large-scale shift to distance (or remote) learning as governments around the world closed schools to reduce the spread of the virus. Many studies have tried to gauge the learning impacts of pandemic induced school closures, with analyses generally finding a significant decline in student performance (e.g., Engzell, Frey, and Verhagen 2021; Maldonado and De Witte 2022; Skar, Graham, and Huebner 2021). There is now widespread consensus that the pandemic – and the policy responses implemented to the reduce the spread of the virus – led to decreased human capital among children and adolescents as well as widened educational inequality between students from different socioeconomic backgrounds. Several meta-studies and systematic reviews arrive at this conclusion (see Donnelly and Patrinos 2022; König and Frey 2022; Di Pietro 2023; Betthäuser, Bach-Mortensen, and Engzell 2023).

We complement this body of research by examining the impact of remote instruction on educational outcomes among lower secondary school students in Sweden during the pandemic. A common limitation in existing studies is the difficulty to isolate the effects of distance learning from other concurrent pandemic-related factors (Gillitzer and Prasad 2024). With schools nationwide often closing simultaneously, it is generally not possible to distinguish the impacts of distance learning from factors such as general stay-at-home orders, restrictions on leisure activities, anxiety, and economic uncertainty. Unlike most other countries, Sweden did not implement government-mandated school closures for lower secondary schools (grades 7–9). Instead, the extent of remote instruction varied across locations and schools. This gives us better opportunities to isolate the effects of distance learning, as we can exploit variation in its adoption across schools and over time. Only a few prior studies have used within-country variation to capture the effects of school closures during the pandemic on student performance. A couple find negative effects (Goldhaber et al. 2023; Lichand et al. 2022), while others report no significant impact (Gillitzer and Prasad 2024; Riudavets-Barcons and Uusitalo 2024).

Test scores in reading and mathematics have commonly been used as outcomes in prior research. However, test-taking often decreased during the pandemic, introducing potential selection bias as certain student groups tend to be more under-represented than others (Betthäuser, Bach-Mortensen, and Engzell 2023; and Werner and Woessmann 2023). The test scores we use for students affected by distance learning stem from the period after all COVID-19 restrictions were lifted and, hence, we do not face the problem of low participation in test taking. Furthermore, our study examines effects on tests scores in multiple subjects, not only reading and mathematics.

Importantly, we also assess how remote instruction influenced students' progression to upper secondary school.[1]

The nature of distance learning during the pandemic has often been characterized as 'uncontrolled', or as 'emergency remote teaching' (Bozkurt and Sharma 2020; Hodges et al. 2020). That is, the switch to distance learning was sudden, and, at least initially, there was often a lack of technology and limited experience of using information and communication technology (ICT) in teaching.[2] Remote instruction was also often used for a rather long time, for all classes, and with few or no exceptions for students with special needs. As a result, there remains uncertainty about the effects of distance learning when implemented in a more 'controlled' manner and less extensively.

Sweden offers a context characterized by a comparably high level of digital preparedness.[3] In the age groups we study, distance learning was used to a limited extent, the transition was less abrupt, exceptions were made for students with special needs, and remote instruction was typically complemented with in-person teaching on school premises. Accordingly, we provide evidence from a context that differs significantly from those previously examined. It is important to underscore, however, that throughout the pandemic, students and teachers were instructed to stay home if they exhibited any symptoms of COVID-19, and absenteeism increased substantially during this period (Öckert 2021; National Agency for Education, NAE, 2022).

We base our analysis on a nationwide, large-scale survey of Swedish lower secondary schools' use of distance learning during the pandemic. The survey provides detailed data on the extent of remote instruction, but also information on how it was implemented. This allows us to examine the role of distance learning during the pandemic in greater detail. The survey is combined with register data on students' performance on national standardized tests, their continuation to upper secondary education, and various background variables.

We identify the impact of remote instruction using a difference-in-differences design, comparing how educational outcomes developed across cohorts (pre-pandemic vs. pandemic-affected) in schools that introduced distance learning to those that continued to rely solely on in-person instruction. Prior studies have usually not been able to use a non-treated comparison group

[1] This type of longer-term outcome is rarely studied. Aalto, Müller, and Tilley (2023) examine the impacts of the pandemic on upper secondary school program choice in Sweden, and van de Werfhorst (2024) study track placement after grade 6 in a Dutch context. However, none of these studies investigate if distance learning mattered for these outcomes.

[2] In many settings, school closures might be more accurately characterized as an interruption or absence of schooling, rather than a functional shift to remote instruction. For instance, UNICEF (2020) reports that two-thirds of the world's school-age children lack internet access at home. Even in high-income countries such as the UK and the US, insufficient access to the internet and necessary devices presented significant barriers for many students (see, e.g., Stelitano et al. (2020).

[3] In international comparisons, Sweden belongs to the group of countries with the highest level of digitalization of the school system before the pandemic; see European Commission (2019) and OECD (2021a).

during the pandemic. Since schools with and without remote instruction differ in characteristics, we add a pre-estimation matching stage and balance the treatment and control group using entropy balancing weights (see Hainmueller 2012), thereby increasing the credibility of the parallel trends assumption.

We find no evidence of negative impacts of distance learning on educational outcomes on average. Students attending schools that used remote instruction generally performed as well as those in schools that continued with teaching in-person throughout the pandemic, and progression to upper secondary school was not negatively affected. In some cases, we even find evidence of positive effects of distance learning. While we are unable to directly study teacher and student absence, suggestive evidence indicates that this may be a mechanism behind the positive effects observed. Schools that used more distance learning were better able to offer remote teaching solutions when teachers or students had to stay home due to illness, also when teaching was planned to take place on-site. Thus, remote instruction may have contributed to an increase in total teaching hours.

These findings contribute valuable insights to the ongoing discussion about how the COVID-19 pandemic affected children and youth. Like most OECD countries, Sweden experienced a significant decline in student performance in mathematics and reading on the PISA assessment between 2018 and 2022 – a decline commonly attributed to the pandemic (OECD 2023). However, our results suggest that remote instruction was not the primary driver in the Swedish case. This indicates important heterogeneity in the effects of distance learning and highlights Sweden as a context where the particular implementation of online instruction may have prevented the negative learning outcomes observed elsewhere. Our findings thus add nuance to the prevailing conclusion in the literature that pandemic-induced school closures and distance learning had universally negative effects on student achievement.

Besides contributing to our understanding of how COVID-19 impacted society, we also add to the discussion on distance learning in education. This form of teaching was already gaining traction before the pandemic and will most likely continue to play a role in education in the future. A body of research examines the causal impact of distance learning on student performance; see Escueta et al. (2020) for an overview. Most of these studies focus on students at American universities, and the results tend to show that distance learning leads to poorer results compared to in-person instruction, although the average differences in educational performance are often quite small. While these studies provide reliable evidence, research from additional contexts is valuable. Our findings do not imply that distance learning is generally equivalent to in-person instruction, but they do suggest that there are situations when elements of distance learning do not affect students' educational achievements negatively.

2 Distance learning – prior research

In a contemporary setting, distance learning refers to an educational approach where teachers and students engage in classes and communicate via the internet, communication platforms, email, and other digital means instead of meeting in person. Distance learning can take two primary forms: synchronous and asynchronous. Synchronous distance learning involves real-time online classes, enabling students to interact with the teacher and their peers. Asynchronous distance learning, on the other hand, implies that students and teachers are physically separated both in space and time. This means that students participate in self-paced learning activities independently, allowing for greater flexibility in learning schedules. Elements of synchronous and asynchronous distance learning can, of course, be combined.

Distance learning has been studied extensively from various perspectives, employing diverse theoretical frameworks and a range of qualitative and quantitative methods (for overviews, see, e.g., Simonson, Zvacek, and Smaldino 2019; Carrillo and Flores 2020). The present study is situated in the literature aimed at estimating the causal impacts of distance learning on students' educational outcomes (for an overview, see Escueta et al. 2020).

In theory, distance learning encompasses aspects that can be both positive and negative for learning (Escueta et al. 2020). On the positive side, remote instruction provides greater flexibility, particularly in asynchronous formats. Students have the freedom to access course materials, including pre-recorded lectures, at their convenience, enabling them to allocate more time to content they find challenging. Moreover, remote instruction allows lectures to proceed even if teachers or students are required to stay home due to, for example, minor illness or quarantine. This is likely to have been an especially attractive feature during the pandemic, potentially resulting in increased hours of instruction compared to a situation with teaching in-person.[4]

On the negative side, distance learning places much greater demands on students in terms of planning and self-discipline. It becomes more challenging for teachers to monitor student progress and ensure comprehension. Furthermore, distance learning diminishes opportunities for natural interaction that occur in a traditional classroom setting. These interactions play a crucial role in the development of various interpersonal and communication skills. Not meeting teachers and classmates in-person can also reduce motivation, which may negatively affect learning and well-being.[5]

[4] Distance learning can also potentially provide students with access to a broader range of courses than are available locally. However, this is unlikely in the context we study, as the study content is determined by a national curriculum, and the remote instruction occurred during courses that were already in progress. Distance learning is sometimes also perceived as attractive from a cost-saving perspective; see, e.g., Deming et al. (2015).

[5] Note, however, that Björkegren, Svaleryd, and Vlachos (2024) find that remote instruction in upper secondary schools during the pandemic in a Swedish context led to a decrease in mental health care use, suggesting potential mental health benefits from distance learning.

Empirical research on the causal impact of distance learning conducted before COVID-19 primarily focused on higher education. Compelling evidence from the US is provided by studies such as Figlio, Rush, and Yin (2013), Alpert, Couch, and Harmon (2016) and Bettinger et al. (2017), all of which show that student learning outcomes are worse in an online setting compared to face-to-face classroom instruction. These negative impacts tend to be stronger for low-ability students, while for high-ability students, the effects are usually close to zero. Heppen et al. (2017) analyze remote instruction for high school students, also in a US context. Their findings indicate that students who participate in online courses perform worse than those who take part in traditional classes. Reliable causal evidence on younger students is scarce.[6] An interesting conclusion in the overview by Escueta et al. (2020), of potential relevance in our setting, is that the negative effects of distance learning do not appear to exist when in-person and online instruction is blended.

The sudden transition to distance learning during the COVID-19 pandemic generated a lot of research aimed at understanding the impacts of pandemic induced school closures on student performance. An early study is Engzell, Frey, and Verhagen (2021), who study school closures in the Netherlands and their impact on learning among children aged 8–11. They estimate that an 8-week school closure resulted in an average learning loss in mathematics, spelling, and reading equivalent to around one-fifth of what students normally learn in a school year. Since the schools were closed for about one-fifth of the school year, the authors conclude that pupils made virtually no progress during the lockdown.

Many studies have echoed the negative findings from the Netherlands.[7] Meta-studies and systematic reviews covering studies from several countries have arrived at similar findings (Donnelly and Patrinos 2022; König and Frey 2022; Di Pietro 2023; Betthäuser, Bach-Mortensen, and Engzell 2023). Bätthauser, Bach-Mortensen and Engzell (2023) conclude that the pandemic generated a large learning deficit, which is particularly large among children from low socio-economic backgrounds. The negative effects are more pronounced in mathematics than in reading and are greater in middle-income countries compared to high-income countries. Notably, there is no evidence of systematic variation in effects across grade levels.

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[6] Fitzpatrick et al. (2020) study children in grades 5–8 in the US and find that switching from a traditional school to a charter school where all teaching takes place online is associated with a decline in performance in mathematics and English. However, it is challenging to determine whether these impacts are due to the remote nature of the courses or other differences between the two types of schools.

[7] For example, Haelermans et al. (2021) confirm the findings of Engzell, Frey, and Verhagen (2021) for the Netherlands, but for a later time period during the pandemic. Furthermore, mathematics skills declined among primary school children in Italy (Contini et al. 2022), writing skills worsened among grade 1 pupils in Norway (Skar, Graham, and Huebner 2021), and primary school students in Belgium experienced learning losses in several subjects due to the pandemic (Maldonado and De Witte 2022). Arenas and Gortazar (2024), studying primary and secondary school students in the Basque county of Spain, found no effect in Spanish language, but worsened performance in mathematics and Basque language. For the US, Pier et al. (2021) found decreased learning in English and mathematics in grades 4–8 in California as a consequence of COVID-19. The list of studies can be made much longer.

Although the overall picture is well-established, it is worth noting that some studies indicate no effect, mixed effects, or even positive effects of the pandemic on student performance. For instance, Birkelund and Karlson (2023) analyzed results on a national reading test in Denmark, 14 months into the pandemic, after 8 to 22 weeks of school closures. They found that 8^{th} grade students, who faced the longest closures, showed a reading performance decrease equivalent to about 7 weeks of lost learning. However, students in grades 2 and 4 exhibited a learning gain. They also found little evidence of widening socioeconomic learning gaps. The authors suggest that Denmark's national measures, such as additional teacher resources, and a focus on reading upon returning to school, might explain the lack of reading loss among younger students. In Sweden, Hallin et al. (2022) found no impact of the pandemic on reading among pupils in grades 1–3. However, the schools were never closed for young children in Sweden. Thus, this study is not informative when it comes to the impact of distance learning.

The studies discussed above capture the pandemic's overall impact on student performance. Although remote instruction was a significant part of children's experiences during this period, the extent to which this form of teaching explains the observed patterns remains unclear. A few studies exploit variation in school closures within-country and over time, providing a closer estimate of the impact of distance learning. The results from these studies are mixed: Gilitizer and Prasad (2024) examine student performance in grades 3, 5, 7, and 9 in Australia, finding no clear evidence of increased learning loss in English or mathematics among schools with extended closures. Moreover, they observe no widening of the socioeconomic gap. Riudavets-Barcons and Uusitalo (2024) analyze test scores in Finnish and mathematics for students in grades 8 and 9 in Finland, and find that students who studied online for longer periods performed as well as those with shorter school closures. Their findings also indicate that these effects did not vary by socioeconomic background. In contrast, Lichand et al. (2022) find large declines in test scores and increased dropout rates due to remote learning among secondary students in Brazil. Similarly, Goldhaber et al. (2023) examine performance gaps among U.S. students in grades 3–8, focusing on differences by race and school poverty levels. Their findings indicate that remote instruction played a significant role in widening these gaps.[8]

The context of Swedish lower secondary schools during the pandemic, discussed in the following section, provides an opportunity to build on these studies to deepen our understanding of how distance learning affected students' educational performance and progression within the school system.

[8] There are also some studies on the impact of remote learning in higher education among adult students. For example, Kofoed et al. (2024) find that online instruction negatively affected student performance at a US Military Academy.

3 The case of Sweden

3.1 The education system

Children in Sweden typically begin school during the fall of the year they turn six and schooling is compulsory for ten years. While there is a national curriculum, the organization of schools falls under municipal jurisdiction. Traditionally, schools have followed a three-stage structure (grades 0–3, grades 4–6, and grades 7–9), but alternative grade configurations are now also utilized. Schools can be both public and 'independent' (but publicly funded); approximately 85% of all compulsory school pupils attend a public school (NAE 2020). Families have the freedom to choose any school for their children, but the vast majority opt for their closest public school since admission rules are primarily based on proximity (Böhlmark, Holmlund, and Lindahl 2016). Independent schools may employ a first-come-first-served rule for admissions, but they cannot base admission on ability or other personal characteristics. Moreover, they are not permitted to charge tuition fees if they want to receive public funding.

Upon completing 9th grade, nearly all students continue to upper secondary education, which offers a variety of 3-year college-preparatory and vocational tracks. Admission to upper secondary education is based on compulsory school GPA. Students who have not attained eligibility for a regular upper secondary school program (due to not passing certain courses), have the possibility of enrolling in an introductory program where they can qualify for a regular program.

3.2 Remote instruction during the COVID-19 pandemic

According to the Swedish School Law (2010:800), remote instruction is generally not allowed and can only be used under special circumstances.[9] As the COVID-19 pandemic spread in the spring of 2020, remote instruction became an integral component of many countries' official strategies to combat the virus. Sweden employed a less interventionist approach compared to most other advanced economies. The Swedish strategy was rooted in the belief that children were not significant vectors of the virus; they displayed milder symptoms compared to adults and were less likely to transmit the virus. Additionally, there was widespread concern that closing schools would have detrimental effects on children's learning and mental health. Consequently, Sweden never implemented a complete lockdown of its education system, nor a lockdown of society at large. Nevertheless, Swedish students and teachers were already from the onset of the pandemic

[9] Asynchronous remote instruction can be considered if a specific student has difficulties participating in traditional schooling. An individual assessment must then be made to determine if this is a suitable solution. Synchronous remote instruction on school premises with a mentor present may be used for a group of students if the student base is very small and/or there is a severe lack of qualified teachers. No more than 25% of the students' instructional hours can be organized as synchronous remote instruction in such cases. Permission to organize distance learning is granted by the Swedish Schools Inspectorate and decisions to allow remote instruction are made for a maximum period of one year.

instructed to stay at home if they exhibited any symptoms of COVID-19, and absenteeism increased substantially during the pandemic (Öckert 2021). Remote instruction was used during certain periods, for certain grades, and in certain schools (Hall, Hardoy, and Lundin 2022; NAE 2022).

In upper secondary schools, remote instruction was utilized quite frequently, with an extent and character similar to other Nordic (Hall, Hardoy, and Lundin 2022) and European (OECD 2021b) countries. In compulsory school, the general rule during the entire pandemic was in-person instruction at school premises. Hence, there was no sudden and general switch to distance learning. However, as time progressed the government increasingly granted local school organizers the authority to decide when to implement distance learning.

In grades 0–6, distance learning was intended to be avoided throughout the entire pandemic and, in practice, was rarely used.[10] However, in grades 7–9, which is the focus of this paper, the situation differed. The possibility of utilizing remote instruction was greater than for younger children, particularly from 2021 onwards. During the spring semester of 2020, about 10% of students in grades 7–9 had remote instruction for, on average, 2.5 net weeks. The corresponding figures for the fall of the same year were 30% with an average duration of 1 net week. Remote instruction peaked during the spring of 2021, with 80% participating for an average of 5 net weeks. In the fall of 2021 and the spring of 2022, the equivalent figures dropped to approximately 10% with an average duration of 1.5 weeks (Statistics Sweden 2023). This is substantially less remote instruction than in most other countries, as illustrated in Figure A1 in the appendix which shows reported number of days during the pandemic in which schools were closed for the majority of students (as reported in PISA 2022). In Section 4.2, we demonstrate that there was considerable variation in the extent to which students in grades 7–9 experienced distance learning.

When distance learning was used in grades 7–9, nearly all schools made exceptions for certain students. Commonly exempted were students receiving special support, those with limited proficiency in the Swedish language, and students who experienced difficulties participating in remote instruction from home due to other circumstances. Regarding the organization of distance learning, around 70% of the students who participated in distance learning attended schools where all or nearly all remote instruction was synchronous, meaning teachers and pupils were not separated in time. Moreover, even the schools that used asynchronous distance learning at some point usually conducted classes synchronously for the most part. It was quite common to alternate between in-person and remote learning, either on a daily or weekly basis; approximately 60% of

[10] Statistics Sweden (2023) report that less than 5% of pupils in grades 4–6 participated in remote instruction during the school year 2021/2022, with an average duration of less than 1 week. The corresponding figures for younger children were even lower.

the pupils who participated in remote instruction attended schools that organized distance learning in this manner (Statistics Sweden 2022; 2023). [11]

A prerequisite for remote instruction is digital technology, such as computers and internet access. While some challenges related to the digital infrastructure undoubtedly existed, it is not portrayed as a significant issue in the comprehensive report by the National Agency for Education detailing education in Sweden during the COVID-19 pandemic (NAE 2022). Given the circumstances, it appears to have been a minor obstacle, which is perhaps not surprising. In a comparative perspective, Sweden belonged to the group of countries with the highest level of digitalization of the school system before the pandemic (European Commission 2019; OECD 2021a). Schools were already before the pandemic obliged to integrate ICT in education to strengthen students' digital competence (NAE 2018) and many pupils receive a personal laptop or tablet from their school. For example, 75% of the pupils in grades 7–9 received a personal laptop from their school already in 2018 (NAE 2019).[12] There is also good access to computers in schools that do not provide a personal device to each student (NAE 2019), and almost all children have access to computers and the internet at home (Swedish Media Council 2015).

To sum up, the conditions for distance learning in grades 7–9 in Swedish schools during the pandemic appear to have been relatively favorable: there was no sudden shift to remote instruction in the spring of 2020, internet and computer access were generally adequate, remote instruction was used to a limited extent and often combined with in-person instruction, and students with special needs were generally exempted from remote instruction. Yet, it is crucial to underscore that across various descriptive and qualitative studies discussing schooling in Sweden during COVID-19, there is consensus that (i) the overall quality of education did not match pre-pandemic standards, and (ii) distance learning fell short compared to traditional in-person instruction (see, e.g., NAE 2022). Nevertheless, it remains an empirical question whether students who were more exposed to distance learning experienced a learning loss compared to other students in the Swedish context.

4 Data and descriptive statistics

4.1 Data

Our study is based on a large-scale survey on schools' use of distance learning during the pandemic, carried out by Statistics Sweden, which is combined with individual-level

[11] For example, one third of students could have in-person teaching and two thirds distance learning in any given day, according to an alternating schedule, thereby decreasing crowding in schools.
[12] One-to-one computer programs, where each student receives a personal computer from their school, have over the years become increasingly common in Sweden. See Hall, Lundin, and Sibbmark (2021) and Hall and Lundin (2024) for descriptions on how 1:1 is used in Sweden and causal estimates of these programs on student performance.

administrative data on student background characteristics and school performance. The survey on distance learning was conducted among principals at all lower secondary schools (grades 7–9) and covers the spring 2020 to the spring 2022. It was carried out in two waves, with a response rate of around 50% in the first wave (covering spring 2020 to spring 2021) and around 30% in the second (covering fall 2021 to spring 2022); see Statistics Sweden (2022; 2023).[13]

The survey provides data on the extent of distance learning used each semester for each grade. Our measure reflects the *net number of weeks of remote instruction*, capturing not only its occurrence but also its actual duration. However, it is important to note that we do not measure the amount of remote instruction at the student level, and exceptions to the general rule were made for students with specific needs in almost all schools (see Section 3.2). The survey also provides additional data on how remote instruction was implemented, such as the use of synchronous vs. asynchronous distance learning.

The survey data on distance learning are linked to individual-level administrative data on school enrollment, results from national standardized tests, subject grades, records of potential special education support, and demographic characteristics (e.g., immigrant background, sex, and parental education and income[14]). We also use administrative data on the teachers employed at the schools. All data are obtained from Statistics Sweden.

We rely on results on national standardized tests, rather than subject grades, to measure impacts on learning.[15] This is crucial in the context of the pandemic, as several reports conclude that the use of distance learning affected grading practices; teachers seem to have applied more lenient grading standards than usual (e.g., Svaleryd and Vlachos 2021; NAE 2021). Lower secondary school students generally take standardized tests in mathematics, Swedish, English, science, and social science towards the end of 9th grade. The tests in mathematics and language are the same for all students, while the tests in science and social science vary across schools: all students in each school take a test in a randomly chosen social science subject (geography, history, religion or civics) as well as a randomly chosen science subject (biology, physics or chemistry).

To account for potential changes in grading standards and difficulty of the tests over time, we standardize students' results in each subject test within test cohort to have mean zero and standard deviation one. As our main outcome, we use the students' average result on the tests taken, but

[13] Analyses of the non-response give no clear indication that schools with more or less distance learning were more likely to respond to the survey; see Statistics Sweden (2022; 2023).

[14] We measure parental education and income the year the student enrolled in lower secondary school, except for the last enrollment cohort – those who enrolled in 2020 – for which we use data from 2019 to avoid influences from the pandemic.

[15] An exception is math performance in 2018, one of the pre-pandemic years. This year, the 9th grade national test in mathematics leaked beforehand. Many schools instead used replacement tests, but the results on these were not collected, resulting in missing test scores for the majority of students. We therefore use the subject grade in mathematics instead of the test score for this cohort. To make sure this does not alter our findings, we also show results excluding this cohort; see Section 6.3.

we also show results for each of the five subjects separately. Due to the pandemic, the national standardized tests were cancelled in the spring of 2020 and 2021, which has implications for which cohorts we can include in the analysis of test scores (see Section 4.2). This also means that all test scores for students affected by remote instruction are from the period after all COVID-19 restrictions were lifted, and, hence, we do not face the problem of low participation in test taking which is a limitation in many previous studies (see, e.g., discussion in Werner and Woessmann 2023).

In addition to test scores, we study impacts on students' progression in the education system in terms of enrollment in upper secondary school at the expected time and whether they enrolled in an academic program. Effects on upper secondary school enrollment may arise from the potential learning impacts of remote instruction, as well as from changes in teachers' grading standards, which could affect students' GPA and subsequently impact which programs they are admitted to. Effects could also stem from possible impacts on students' motivation and preferences for various upper secondary school programs.

4.2 Sample and descriptive statistics

Our sample consists of students who began 7th grade in schools for which we have survey data during the period 2013–2020. We define exposure to distance learning based on the school the student attended in the beginning of grade 7 assuming they adhered to normal study pace.[16] Since the response rate to the survey was much lower in the second wave, we rely on information from the first survey wave only in our main analysis. This means that the distance learning measure we use is based on the first three semesters of the pandemic (i.e. spring 2020–spring 2021). However, it should be noted that most distance learning took place during this time (see Section 3.2) and that the correlation between the distance learning measure based on the first wave and both waves is very high (0.95) for schools that have responded to both surveys. In the robustness analysis (Section 6.3), we show that our results are similar if we instead use data from both waves.

The sampled students would normally take 9th-grade national standardized test in the spring of 2016–2023 and begin upper secondary school in the fall of the same year.[17] The fact that all national tests were cancelled during the spring of 2020 and 2021 means that students who enrolled

[16] Grade repetition is generally rare in Sweden. However, some students change school between 7th and 9th grade. To change school seems to be both more common for the COVID-19 affected cohorts, and to be systematically related to the use of remote instruction. While around 8% of students changed school between 7th and 9th grade in the pre-pandemic cohorts, this number increased to 10.8% for the cohort expected to graduate in 2022 and to 13.8% for the cohort expected to graduate in 2023. Students who started grade 7 in a school that used remote instruction changed school more often than students in schools that did not; see Table A 7. In Section 6.3, we examine whether this affects our results on student performance.

[17] We measure results on national tests in the spring three years after the student enrolled in 7th grade, that is, assuming that they maintained a normal study pace. To not take the test this year, which could be an indication of grade repetition, is used as a separate outcome variable in the analysis.

in 7th grade in the spring of 2017 and 2018 cannot be included in the analyses of test scores. Our sampling strategy provides us with four untreated cohorts of students, who attended grades 7–9 before the pandemic, and two treated cohorts that were exposed to different amounts of distance learning, which can be used to study impacts on both tests scores and enrollment in upper secondary school (see Table 1).

Table 1. Cohorts affected by distance learning due to the COVID-19 pandemic in our sample

Year enrolled 7th grade	COVID-19 during grade 7-9?	Grade 7		Grade 8		Grade 9		Expected graduation 9th grade	National tests in grade 9?
		Fall	Spring	Fall	Spring	Fall	Spring		
Fall-13	No							Spring-16	Yes
Fall-14	No							Spring-17	Yes
Fall-15	No							Spring-18	Yes[a]
Fall-16	No							Spring-19	Yes
Fall-17	Yes							Spring-20	No
Fall-18	Yes							Spring-21	No
Fall-19	Yes				*Potential distance learning*			Spring-22	Yes
Fall-20	Yes							Spring-23	Yes

Notes: [a] National test results are missing for mathematics. The lighter blue color refers to the time period covered by the first survey wave, and the darker blue to the period covered by the second survey wave.

Table 2 presents summary statistics on the amount of distance learning experienced by each enrollment cohort. Students who began 7th grade in 2017 and graduated from 9th grade at the onset of the pandemic experienced minimal remote instruction (only 1 day on average). The other cohorts affected by COVID-19 received on average 4–5.5 weeks of remote instruction, with the number of weeks ranging from 0 to 31.[18] Our main sample consists of the cohorts for which we have data on both test scores and upper secondary school enrollment, that is, those expected to graduate in 2016–2019 and 2022–2023. This sample comprises 290,901 students from 819 different schools; see Table A1 for descriptive statistics. We also provide results for those expected to graduate in 2021 in terms of upper secondary school enrollment.

[18] Table 2 is based on the first survey wave; hence, it only covers the first three semesters of the pandemic. However, the averages are rather similar if we instead use data from both surveys for the smaller sample of schools that responded to both; see Table A2.

Table 2. Summary statistics for number of net weeks of distance learning, by enrollment cohort

Enrollment year	Exp. grad. year	Mean	Sd	Min	Max
2013	2016	0	0	0	0
2014	2017	0	0	0	0
2015	2018	0	0	0	0
2016	2019	0	0	0	0
2017	2020	0.18	0.90	0	12
2018	2021	4.01	3.93	0	31
2019	2022	5.79	4.39	0	26.7
2020	2023	5.43	4.34	0	24.1

Notes: The table is based on data from the first survey wave; hence covering the first three semesters of the pandemic. The averages are rather similar if we instead use data from the smaller sample of schools that responded to both surveys; see Table A2.

4.3 What types of schools relied more on distance learning?

Table 3 shows descriptive statistics for schools with varying levels of distance learning (0 weeks; 1–5 net weeks; and at least 6 net weeks), focusing on students who enrolled in 7th grade in 2019, that is, the cohort most affected by remote instruction (see Table 2).

The schools that relied more on distance learning are positively selected in terms of students' earlier academic performance and socioeconomic background. For instance, parents at schools implementing distance learning, especially those with at least 6 net weeks of such instruction, tend to have a higher level of education compared to those at schools without distance learning. Students at schools that did not use remote instruction at all are much more likely to receive special education support and had on average lower grades across all academic subjects prior to entering 7th grade. Hence, it seems that schools with academically weaker students made greater efforts to maintain teaching in-person.

There are also differences by municipality type. Among schools in the group with the highest amount of distance learning, nearly half (45%) are located in large cities, while only 16% of the schools that did not use distance learning at all are in large cities. These schools are instead most commonly located in larger towns. The share of schools in small towns, on the other hand, does not vary much depending on the amount of distance learning used. The differences depending on municipality size may be related to a desire to organize teaching in such a way that both teachers and students would be less exposed to transmission risks, particularly due to commuting in large cities.[19] In line with this, we also observe that schools utilizing more distance learning tend to be larger. In the next section, we describe the empirical strategy we use to account for differences in characteristics between students exposed to varying amounts of remote instruction.

[19] More densely populated regions tended to face a higher risk of virus transmission. For instance, the Swedish Corona Commission reports that Stockholm county experience significantly more COVID-related deaths than any other county, accounting for the age composition of the population; see SOU 2022:10, p. 230.

Table 3. Descriptive statistics for schools with different amounts of distance learning, based on students who enrolled in 7th grade in 2019

	(1) 0 weeks mean	(2) 0 weeks sd	(3) 1–5 weeks mean	(4) 1–5 weeks sd	(5) ≥6 weeks mean	(6) ≥6 weeks sd
Student characteristics						
Female	0.420	0.186	0.476	0.106	0.488	0.089
Born abroad	0.150	0.171	0.139	0.122	0.143	0.131
Parents born abroad	0.232	0.250	0.237	0.225	0.249	0.228
Mother upper secondary educ.[a]	0.393	0.212	0.361	0.132	0.337	0.119
Mother post-secondary educ.[a]	0.433	0.217	0.493	0.195	0.513	0.192
Father upper secondary educ.[a]	0.453	0.228	0.431	0.155	0.410	0.138
Father post-secondary educ.[a]	0.290	0.185	0.365	0.190	0.387	0.182
Mother social assistance[a]	0.085	0.161	0.055	0.069	0.059	0.078
Father social assistance[a]	0.052	0.120	0.040	0.054	0.044	0.059
Students with special support[b]	0.294	0.385	0.096	0.127	0.079	0.084
6th grade std grade, math[c]	-0.352	0.669	-0.039	0.432	-0.022	0.370
6th grade std grade, English[c]	-0.328	0.594	-0.035	0.393	-0.008	0.358
6th grade std grade, Swedish[c]	-0.394	0.737	-0.017	0.402	-0.009	0.385
6th grade std grade, soc. science[c]	-0.414	0.831	-0.034	0.431	-0.016	0.429
6th grade std grade, science[c]	-0.404	0.825	-0.006	0.434	-0.025	0.442
School characteristics						
Independent school	0.356	0.481	0.324	0.469	0.301	0.459
Number of students in 7th grade	50.86	48.16	73.74	50.64	82.01	47.63
School located in small town	0.280	0.451	0.297	0.458	0.254	0.436
School located in larger town	0.559	0.499	0.340	0.474	0.295	0.457
School located in large city	0.161	0.369	0.363	0.482	0.451	0.498
Number of schools	118		306		319	

Notes: The table is based on the schools for which we have (first wave) survey data on distance learning. [a] We measure parental background variables the year the student enrolled in 7th grade. [b] Special support indicates if at least one of the following measures are relevant for the student: individualized educational plan (*åtgärdsprogram*); adapted study path (*anpassad studiegång*); special education group (*särskild undervisningsgrupp*); individual instruction (*enskild undervisning*); mother tongue study support (*studiehandledning på modersmål*). [c] Grades in different subjects have been standardized within test cohort nationally to have mean 0 and standard deviation 1.

5 Empirical design

We identify the impact of distance learning on student performance and continuation to upper secondary school using a difference-in-differences design. The recent methodological literature has shown that the standard continuous two-way fixed effects estimator does not identify any meaningful causal parameter (De Chaisemartin et al. 2024; Callaway, Goodman-Bacon, and Sant'Anna 2024). In the main analysis, we therefore categorize net weeks of remote instruction into two levels of treatment, one for schools that used a "small" amount of remote instruction (1–5 net weeks) and one for schools that used a "large" amount (≥6 net weeks), and a control group that did not use any remote instruction. The cut-off between the two treated groups was set to 6 net weeks since it is approximately the median among schools that used some amount of distance learning.

We estimate average level effects of distance learning for the two treatment groups using a standard binary difference-in-differences estimator where students in treated schools are compared to students in schools that did not use any distance learning. We thereby only need to assume standard parallel trends, that is, the outcomes of students in schools that used distance learning would have followed the trajectory of students in schools that did not, had it not been for the use of distance learning (Callaway, Goodman-Bacon, and Sant'Anna 2024). The existence of a pure control group that did not use any remote instruction in the Swedish context is an important advantage compared to other settings. The identification of causal effects in contexts where all schools used distance learning, but to different degrees, relies on a substantially stronger parallel trends assumption (Callaway, Goodman-Bacon, and Sant'Anna 2024). More specifically, they need to assume that students in schools that used a higher amount of remote instruction would have followed the same path as students in schools that used a lower amount, if their schools had used the lower amount.[20] Note that we cannot investigate the credibility of this stronger parallel trends assumption using pre-treatment data.

We estimate model (1), presented below, separately for the two treatment groups[21] and for each cohort exposed to remote instruction. Analyzing the cohorts separately is primarily motivated by their differing timing of exposure to the pandemic during lower secondary school (see Figure 1).[22] Students in the cohort expected to graduate in 2023 experienced distance learning at a time when many schools already had prior experience with this form of instruction. Additionally, they participated in distance learning earlier in their lower secondary education, giving schools more time to compensate for any potential learning loss before students took the national tests in grade 9. For these reasons, we expect the cohort expected to graduate in 2023 to be better off.

[20] Or put differently, that treatment effects are homogenous, or that, at least, there is no selection of the amount of distance learning that is correlated with treatment effects.

[21] In Table A9, we further investigate the continuous variation in distance learning by estimating separate effects for treatment intensity deciles. De Chaisemartin et al. (2024) show how similar pairwise difference-in-difference estimates can be combined into average slope parameters without requiring a stronger parallel trends assumption. However, as it is clear from treatment intensity decile estimates that there is no systematic relationship between treatment intensity and the size of the treatment effect, we find little value in estimating a parameter measuring the effect per week. Similarly, it seems unnecessary to estimate the dose-response estimator (relying on stronger parallel trends assumptions) in Callaway, Goodman-Bacon, and Sant'Anna (2024).

[22] Moreover, schools do not necessarily belong to the same treatment/control group (i.e., 0, 1–5 or ≥6 weeks) for all cohorts potentially exposed to remote instruction. This provides an additional rationale for conducting separate analyses for the cohorts expected to graduate in 2022 and 2023, respectively, comparing them to the cohorts in the same schools that graduated before the pandemic. When we study enrollment in upper secondary school, we also conduct analyses for those expected to graduate in 2021.

$$Y_{ist} = \alpha + \beta Distance\_learning_{st} + \gamma_s + \delta_t + e_{ist} \qquad (1)$$

Y_{ist} is the outcome for student i, who enrolled in school s, and is expected to graduate in year t. $Distance\_learning_{st}$ is a dichotomous variable that takes the value 1 if the student enrolled in a school that used distance learning for his/her enrollment cohort, and 0 otherwise. The parameter of interest, β, thus gives us the estimated impact of exposure to distance learning. Since exposure is measured based on the school the student attended at the beginning of 7th grade, β should be interpreted as an intention-to-treat (ITT) effect of exposure to remote instruction.[23] Since we measure student outcomes some time after exposure to distance learning (particularly for the cohort expected to graduate in 2023), β may also capture the effects of potential compensatory behaviors from schools. γ_s and δ_t represent school and (expected) graduation year fixed effects, respectively, and e_{ist} is the error term. Standard errors are clustered at the school level since both the sampling and the treatment is at this level (cf. Abadie et al. 2023).

As we saw in the previous section, there are clear and systematic differences between treated schools and schools that used no distance learning when it comes to pre-treatment outcomes and student and school characteristics. This does not need to be a problem if they are on parallel trends. However, to increase the credibility of the parallel trends assumption, we estimate models with matched treated and control groups. By adding a pre-estimation matching stage, we only have to assume parallel trends conditional on matching covariates (Abadie 2005). To balance the treated and control samples, we weight control group observations using entropy balancing weights (see Hainmueller 2012). Entropy balancing directly estimates control sample weights to achieve perfect balance between the treated and the control sample with regard to specified sample moments of the matching covariates (to the extent that this is possible). The intuition is that even if it is not possible to find exact matches when using several matching characteristics, we can reweight control units such that sample moments are the same as in the treated group (Hainmueller 2012; Zhao and Percival 2017).[24]

To create a balanced control group for each treated group, we use the following individual-level covariates: sex, foreign background, prior performance (in math, English, Swedish, science and social science), each parent's level of education, foreign background and receipt of social assistance; as well as the following school-level covariates: independent school status and size of

[23] Note also that our school-level measure of remote instruction captures the school's general policy for the cohort, which may not directly align with the specific amount received by all students as schools generally made exceptions for certain disadvantaged students (see Section 3.2).
[24] We use three sample moments. Hence, we create a weighted control group which is balanced with the treated group with regard to the mean, the variance and the skewness of covariates.

the school measured by number of students (see Table 3).[25] We balance observations from each cohort separately. Figure A2 shows covariate balance with and without entropy weights for the last pre-COVID cohort, that is, students taking the 9th grade national tests in 2019. The covariate balance improves substantially, in particular for independent school status, but also for parents' education, the student's prior subject grades, and school size. We report standard errors obtained via bootstrapping to account for the estimation of entropy balance weights.[26]

To assess the validity of the parallel trends assumption, we complement model (1) with an event study model, which provides estimates of how the impact of attending a school belonging to each of the treatment groups evolves over time during the years prior to the pandemic. More specifically, for the cohort expected to graduate in year *T*, we estimate the following model for each treatment group:

$$Y_{ist} = \alpha + \sum_{\substack{t=2016 \\ t\neq 2019,2020\ldots T-1}}^{T} [\lambda_t(Distance\_learning_s)] + \gamma_s + \delta_t + e_{ist} \qquad (2)$$

where Y_{ist} is the outcome for student i, who enrolled in school s, and is expected to graduate in year t. $Distance\_learning_s$ takes the value 1 if the student enrolled in a school that belongs to the treatment group, and 0 if it belongs to the control group. The λ_t coefficients for the pre-treatment period (λ_{2016} to λ_{2018}) trace out relative trends during the years preceding the pandemic, while λ_T provides an estimate of the effect of exposure to distance learning for cohort T. The reference year is 2019, which means that all λ_t estimates are relative to the last year before the start of the pandemic. γ_s and δ_t represent school and (expected) graduation year fixed effects, respectively, and e_{ist} is the error term.

The control group observations are again weighted using entropy balancing weights, and the standard errors are clustered at the school level. While the pre-pandemic period serves as an important test of the parallel trends assumption, it is possible that the pandemic's unique context could have amplified the importance of certain family or school characteristics not equally salient before. Our use of entropy balancing based on a rich set of covariates alleviates this concern by ensuring that we compare groups with similar observable pre-existing conditions.

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[25] We use all individual-level variables included in Table 3, except for special support, as this information is unavailable for the first two cohorts. Municipality type has also been excluded from the school level variables, as its inclusion worsened rather than improved the pre-trends (figures available upon request). In addition to the variables listed in Table 3, we include indicators for missing data on parental education and prior school performance, as well as an indicator for whether the grade in Swedish refers to Swedish as a second language.

[26] More specifically, we use a cluster bootstrap procedure with replacement and 1,000 bootstrap replications. In practice, this choice has little importance for our results; all conclusions remain unchanged when using conventional cluster-robust standard errors.

6 Results

We begin by presenting results from the event study model (Section 6.1), followed by our main results for the full estimation sample (Section 6.2). Next, we conduct a number of robustness checks (Section 6.3). Section 6.4 explores heterogenous effects across subjects, student characteristics, and type of distance learning. Last, in Section 6.5, we examine some potential mechanisms underlying our findings.

6.1 Assessing the assumption of parallel trends

Figure 1 presents the results from the event study model (model 2), showing no indication of divergent trends in students' educational outcomes between the groups of schools compared during the period before the pandemic. The trends also appear largely parallel in the unweighted version of the model (see Figure A3), but the pre-period estimates are more volatile. For this reason, and since schools that did not use distance learning clearly differ in important ways from schools that did, we consider the weighted regressions to be the most reliable.

Additionally, Figure 1 shows no indications of negative impacts from exposure to remote instruction for the treated cohorts. The point estimates are positive and, in some cases, statistically significant at the ten percent level, suggesting that remote instruction – the way it was implemented in the Swedish context – may sometimes even have been beneficial for students' academic performance.

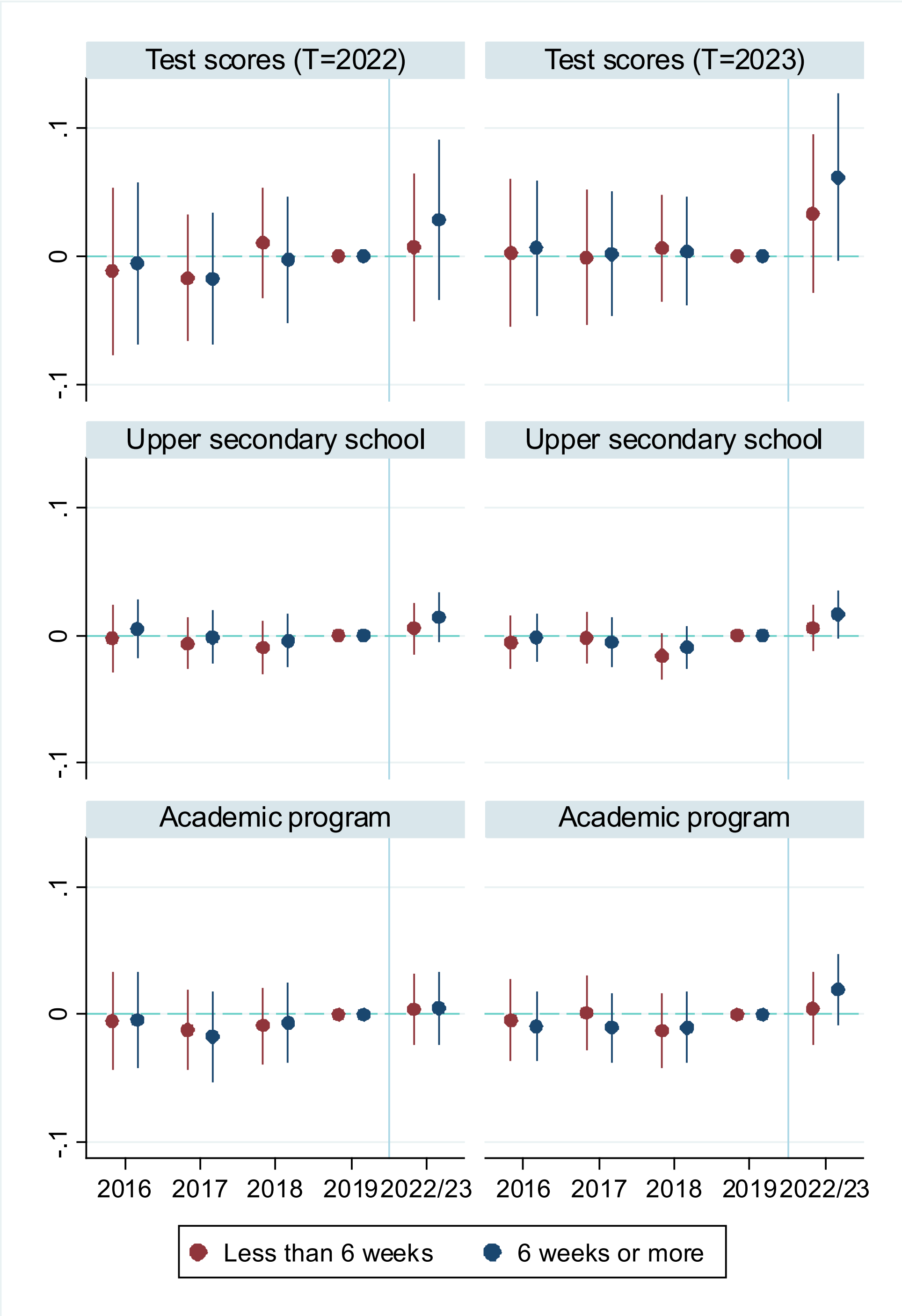

Figure 1. Time-varying effects of attending a school belonging to each of the two treatment groups, weighted regressions (entropy balance)

Note: The figure shows estimates of model (2), separately by (expected) graduation cohort. The vertical line marks the start of the pandemic. 2019, the last cohort to graduate before the pandemic, is the reference cohort. Students belonging to graduation cohort 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.

6.2 Main results

Table 4 presents our main results for the full estimation sample. In line with Figure 1, there are no indications of negative impacts from remote instruction on any of the outcomes examined. For the students exposed to less than 6 net weeks of remote instruction, the estimates are generally close to zero and never statistically significant. For those exposed to 6 weeks or more, the estimates are more positive, and for the 2023 graduation cohort, they are statistically significant for all three outcomes.[27] The point estimates suggest that distance learning led to an increase in average test scores for this cohort by nearly 6% of a standard deviation, a 2.2% increase in on-time enrollment in a regular upper secondary school program (0.02/0.888), and a 4.3% increase in enrollment in an academic program (0.027/0.622).

Using Kraft's (2020) benchmarks for educational interventions, the estimated test score impact for the 2023 cohort can be categorized as a small-to-medium-sized effect.[28] The consistency of these positive findings across test scores and upper secondary school progression (enrollment and track choice) suggests that the latter effects may indeed be driven by learning gains, though, as previously noted, factors such as changes in teachers' grading standards or student preferences could also contribute.

[27] For the 2021 graduation cohort, for whom national tests were cancelled, the estimates for upper secondary school enrollment are nearly identical to those for the 2022 cohort (see Table A3).

[28] Kraft (2020) proposes benchmarks for interpreting effect sizes in educational interventions, based on estimates from 747 RCTs evaluating educational interventions. According to these benchmarks, effects below 0.05 standard deviations are considered small, effects between 0.05 and 0.2 are classified as medium, and those exceeding 0.2 are regarded as large.

Table 4. Effects of distance learning, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
A. Average test score				
Distance learning	0.010	0.035	0.031	0.059**
	(0.028)	(0.032)	(0.030)	(0.029)
Observations	104,378	117,669	109,988	119,956
R-squared	0.118	0.113	0.126	0.117
Outcome mean (untreated obs.[a])	0.013	0.031	0.029	0.014
B. Enrolled in upper secondary school				
Distance learning	0.010	0.015*	0.012	0.020**
	(0.008)	(0.008)	(0.008)	(0.008)
Observations	109,242	123,189	115,029	125,500
R-squared	0.057	0.062	0.057	0.059
Outcome mean (untreated obs.[a])	0.885	0.891	0.888	0.888
C. Enrolled in academic program				
Distance learning	0.010	0.012	0.009	0.027**
	(0.012)	(0.012)	(0.013)	(0.013)
Observations	109,242	123,189	115,029	125,500
R-squared	0.100	0.097	0.110	0.095
Outcome mean (untreated obs.[a])	0.608	0.629	0.614	0.622

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. [a] Untreated obs. refers to all observations from the pre-pandemic cohorts as well as observations from the control group during the pandemic.

An important question is whether the improved test scores observed could be driven by remote instruction affecting test-taking. For instance, if distance learning led to fewer students taking the tests, those who did might be more positively selected, potentially explaining the positive impact observed on test scores for the 2023 graduation cohort. Table 5 presents results using an indicator for having test results as the outcome, and the findings suggest the opposite. While there is no impact on test-taking for the 2022 cohort or for the 2023 students exposed to less than 6 weeks of remote instruction, those exposed to a higher amount were actually somewhat more likely to take the tests. This means that the positive impact observed on test scores is unlikely to be driven by a more positively selected group of test-takers.

Table 5. Effects of distance learning on the probability of having results from at least one national test the expected year, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
Distance learning	-0.004	0.005	0.004	0.016***
	(0.011)	(0.008)	(0.010)	(0.005)
Observations	109,242	123,189	115,029	125,500
R-squared	0.074	0.086	0.076	0.071
Outcome mean (untreated obs.)	0.959	0.957	0.960	0.958

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. Outcome means are calculated for all untreated observations.

6.3 Robustness checks

Table A4 compares our main results with those obtained when estimating model (1) without entropy weights and with those obtained using alternative entropy balancing weights, which also balance treated and control units based on municipality characteristics – the population size and density. These weights should at least partially adjust for the fact that COVID-19 transmission rates were higher in larger cities (see SOU 2022:10). Directly controlling for transmission rates would, however, be problematic, as the extent of COVID-19 transmission may itself have been influenced by whether schools remained open or closed (see, e.g., Vlachos, Hertegård, and Svaleryd 2021). Most estimates in Table A4 are statistically insignificant across all specifications, with point estimates being almost exclusively positive. For the 2023 cohort, where we observe positive impacts of exposure to at least 6 net weeks of distance learning, the results remain similar under the alternative weighting scheme, while the unweighted regressions show no significant effects.

In Table A5, we replicate our main analysis using the smaller sample of schools (around 40% of the original sample) that responded to both surveys. This gives us a more accurate measure of the amount of distance learning used, as it covers the full duration of the pandemic. However, as noted previously (see Section 4.2), only a small amount of distance learning took place during the period covered by the second survey, and the correlation between the two distance learning measures is very high. The results remain largely similar when both surveys are utilized, showing no evidence of negative impacts of remote instruction. For the 2023 graduation cohort, there are again indications of a positive effect from exposure to at least 6 weeks of distance learning. However, the precision of the estimates is considerably lower for this smaller sample.

Table A6 shows that our results are not affected by the use of subject grades rather than test results in mathematics for the 2018 graduation cohort, when the national test leaked (see

Section 4.1). The estimates for the average score remain almost identical when this cohort is excluded from the analyses. Moreover, Table A7 suggests that the patterns we find are unlikely to be driven by the higher frequency of school changes among students who attended schools that used more distance learning. When we restrict the sample to students who remained in the same school throughout lower secondary school in Table A8, the results are similar to those in our main analysis. It is important to note, however, that this analysis conditions on an outcome, and the results should therefore be interpreted with caution.

Table A9 brings us closer to an analysis of the continuous variation in net weeks of distance learning. We estimate separate difference-in-difference estimates for each treatment intensity decile for the treated cohorts. The comparison group is still schools that did not use any remote instruction. There appears to be no monotonic relationship between the amount of distance learning and estimated effects. Since estimated effects clearly do not indicate any dose-response effects, we do not proceed with estimations comparing students in schools that used different amounts of remote instruction.

6.4 Heterogenous effects

6.4.1 Heterogeneity across subjects

In our main analysis, we pooled test scores in mathematics, Swedish, English, science, and social science. However, remote instruction may affect student performance in these subjects differently. Some subjects may be more challenging to teach remotely – for example, conducting science labs remotely is often impossible – and prior research have found more pronounced negative effects of distance learning in mathematics than in reading (see Section 2). It is important to note, however, that the survey does not provide information on the extent to which each subject was taught remotely. Since many schools alternated between on-site and distance learning, they may have prioritized activities more suited to remote instruction during remote teaching while reserving other activities for in-person instruction. As a result, it is difficult to form clear hypotheses regarding what to expect in terms of differential impacts across subjects.

Table 6, which presents results separately for each subject, suggests that the positive impacts observed for the 2023 graduation cohort exposed to 6 weeks or more of distance learning are primarily driven by improved test scores in social science and mathematics. For mathematics, there is also a marginally significant positive impact for the 2022 cohort exposed to this amount of remote instruction.[29]

[29] Figure A4 presents results from the event study model by subject. In line with the main outcomes, there are no clear indications of divergent trends between the groups of schools compared during the pre-pandemic period. However, the pre-pandemic estimates are more volatile when estimated separately by subject.

Table 6. Effects of distance learning on test scores in different subjects, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
A. Mathematics				
Distance learning	0.045	0.066*	0.049	0.063**
	(0.036)	(0.038)	(0.031)	(0.032)
Observations	99,420	111,834	105,021	114,310
R-squared	0.085	0.082	0.087	0.081
Outcome mean (untreated obs.)	0.037	0.056	0.054	0.040
B. Swedish				
Distance learning	-0.043	-0.008	0.004	0.053
	(0.042)	(0.048)	(0.036)	(0.033)
Observations	95,293	105,718	100,176	107,945
R-squared	0.078	0.074	0.084	0.080
Outcome mean (untreated obs.)	0.074	0.073	0.083	0.059
C. English				
Distance learning	0.001	0.015	0.002	0.022
	(0.028)	(0.030)	(0.030)	(0.030)
Observations	95,316	107,893	100,934	110,008
R-squared	0.096	0.091	0.101	0.091
Outcome mean (untreated obs.)	0.039	0.066	0.054	0.054
D. Science				
Distance learning	0.011	0.037	0.028	0.035
	(0.049)	(0.053)	(0.049)	(0.044)
Observations	95,592	107,201	101,592	110,147
R-squared	0.106	0.097	0.106	0.098
Outcome mean (untreated obs.)	0.045	0.059	0.064	0.043
E. Social science				
Distance learning	-0.008	0.039	0.050	0.103**
	(0.031)	(0.030)	(0.049)	(0.046)
Observations	98,172	109,910	103,699	112,428
R-squared	0.104	0.101	0.115	0.110
Outcome mean (untreated obs.)	0.056	0.062	0.068	0.043

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. Outcome means are calculated for all untreated observations.

6.4.2 Heterogeneity by student characteristics

We continue by exploring potential heterogeneity across various subgroups of students. As discussed previously, distance learning tends to require greater self-discipline and provides less direct contact with teachers, which can be expected to make it particularly challenging for weaker students. Home environments also vary in their conduciveness to learning, including differences in access to a quiet space, technology, and parental support. Consequently, students from weaker socioeconomic backgrounds are likely to face greater challenges. This is also a common finding in the existing literature (see, e.g., Betthäuser, Bach-Mortensen, and Engzell 2023). However, in the Swedish context, nearly all schools that used distance learning made exceptions for certain students, for instance, those receiving special support and those who had difficulties participating in remote instruction from home due to other circumstances (Statistics Sweden 2022; 2023). As a result, differences in impacts across subgroups may be less pronounced than in many other countries.

Figure 2 presents results for average test scores, broken down by parents' educational background (at least one parent with tertiary education vs. no parent with tertiary education), primary school grades (average grade in math, English, Swedish, science, and social science above vs. below the cohort-specific median), and gender.[30] The figure focuses on the effects of being exposed to at least 6 weeks of distance learning, as the estimates for shorter exposure are almost always statistically insignificant. Nearly all estimates are positive. For the 2022 graduation cohort, the estimates vary in size and are generally not statistically significant, the exception being for students with low grade 6 grades. For the 2023 cohort, the estimates are rather similar in size across all subgroups but often imprecisely estimated and statistically significant only for boys and students with above-median primary school grades.

The patterns for on-time enrollment in a regular upper secondary school program and enrollment in an academic program, shown in Figure A5 and Figure A6, are broadly similar to those for test scores. However, for these outcomes, we also find statistically significant positive effects for students whose parents have a higher education level, and in terms of enrollment in a regular upper secondary school program, also for girls. The positive and significant effects found are generally not robust to estimation without entropy balance weights (not shown).

In summary, we do not find any clear differences in effects across subgroups, and we find no evidence of negative impacts of attending a school using remote learning on 'weak' student groups.

[30] We have also specifically examined students who received some form of special support when starting grade 7. While there is no indication of negative impacts from attending a school that used remote instruction, the estimates (not reported) are very imprecise for this relatively small groups of students.

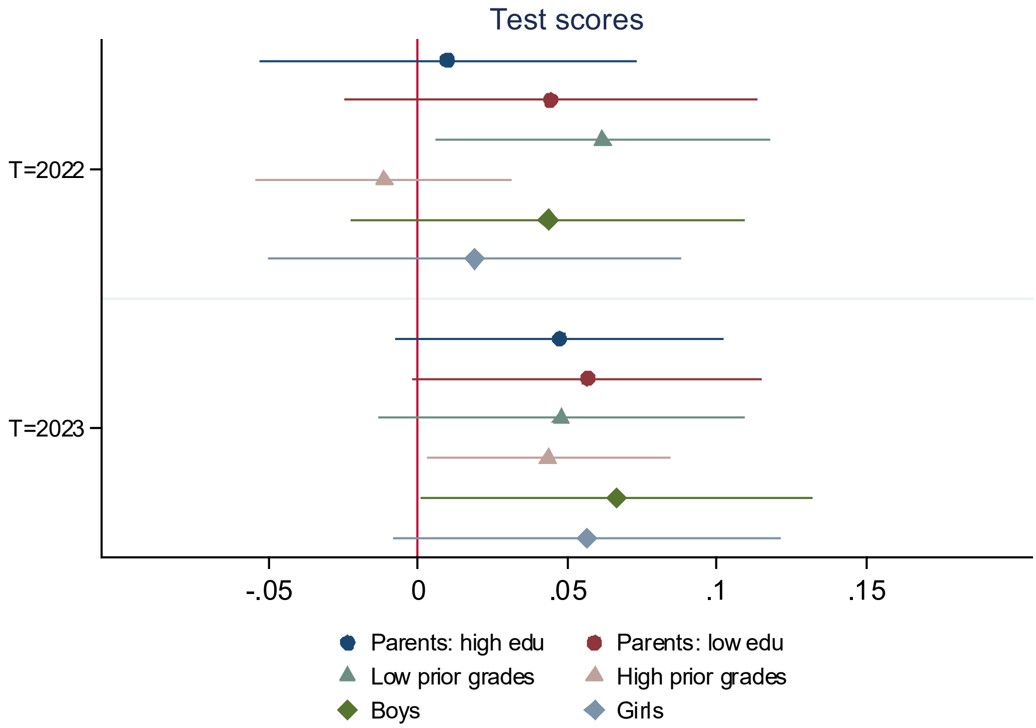

Figure 2. Effects of distance learning (6+ weeks) on test scores for various sub-groups of students, weighted regressions (entropy balance)

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.

6.4.3 Heterogeneity by type of distance learning

Most students (around 70%) who participated in distance learning attended schools where all or nearly all such instruction was synchronous, meaning that classes were held online in real time. Figure 3 presents results for test scores separately for students who attended schools that almost exclusively used synchronous distance learning and those who attended schools that incorporated asynchronous elements either partially or exclusively. However, it was rare for schools to rely solely on asynchronous distance learning.

As in the previous section, we focus on displaying effects for at least 6 weeks of distance learning, as the estimates for shorter exposure are consistently statistically insignificant. Effect sizes are similar across the two groups of schools, but it is only for the 2023 graduation cohort in schools that relied exclusively on real time instruction that the effect is statistically significant. Figure A7 and Figure A8 show similar patterns for on-time enrollment in upper secondary school and enrollment in an academic program. Again, the significant effects found are not robust to estimation without entropy balance weights (not shown).

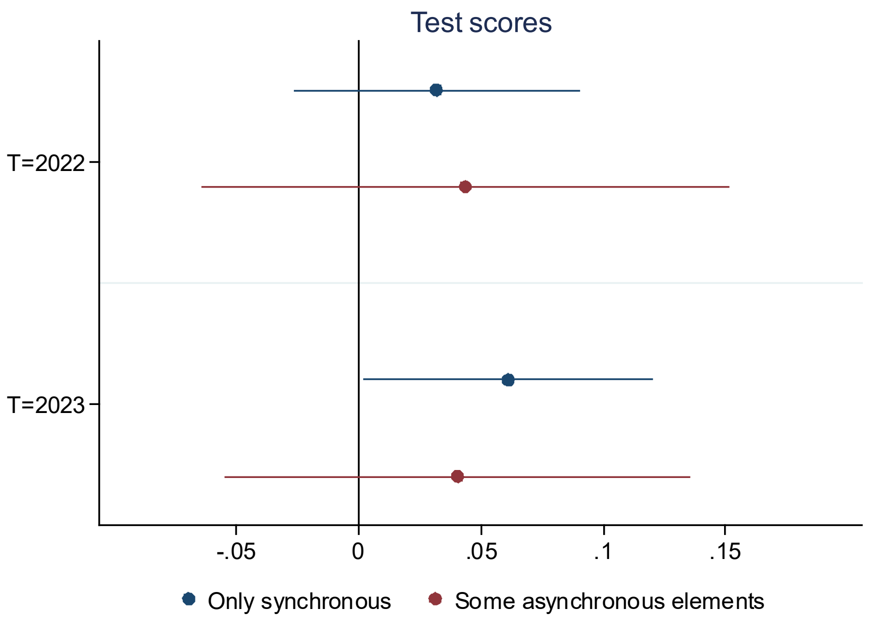

Figure 3. Effects of distance learning (6+ weeks) on test scores, by type of distance learning, weighted regressions (entropy balance)

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.

6.5 Mechanisms

6.5.1 Did schools with remote instruction compensate students afterwards?

Since we measure student outcomes some time after exposure to distance learning, our estimates will also capture potential compensatory behaviors by schools. A possible explanation for the absence of negative impacts and, in some cases, even positive impacts could therefore be that schools compensated students afterwards for potential learning losses caused by remote instruction. This might then explain why it is primarily the 2023 graduation cohort that was positively affected, as schools had more time to implement compensatory measures before these students took the national tests in grade 9.

Our data allow us to examine two types of compensatory behaviors: whether students are more likely to receive special education support, and the student-teacher ratio.[31] We measure both outcomes in the year the student is expected to attend 9th grade. Special education support includes the following measures: an individualized education plan, adapted study path, placement in a special education group, individual instruction, and mother tongue study support. The student-teacher ratio is observed at the school level, which makes it a somewhat crude proxy, particularly

[31] There are, of course, other ways in which schools could have compensated, such as by changing teaching practices or reallocating resources within schools, which we cannot observe in our data.

for students attending schools that also include lower educational stages (i.e., below grade 7), which applies to roughly half of the sample.[32]

Table A10, which shows the results when these two measures are used as outcome variables in our baseline specification, gives no indication that schools compensated in these ways: all estimates are statistically insignificant, and the estimates either have the opposite sign (special education support) or are of negligible size (student-teacher ratio). Hence, our data provide no support for the idea that schools that used distance learning implemented more compensatory measures afterwards.

6.5.2 Did remote instruction increase teaching hours?

A potentially important advantage of distance learning, particularly during a pandemic, is that it allows teaching to continue even when teachers or students must stay home due to minor illness or quarantine. Additionally, schools that relied more on distance learning may have been better equipped to enable sick students to participate remotely even at times when teaching took place on-site. These factors may lead to increased hours of instruction, which could be a potential explanation for the lack of negative effects of distance learning and the positive effects observed for some students.[33]

The survey we use includes questions that allow us to, to some extent, examine these mechanisms. For each semester, schools were asked: (i) whether they allowed teachers with mild symptoms or in quarantine to teach remotely even when the teaching was scheduled to take place on-site, and (ii) whether, when teaching took place on-site, they offered students who had to stay home due to illness or quarantine the option to participate in some or all classes remotely. We sum the number of semesters for which schools reported having these practices and use these counts as outcome variables in our baseline specification. Table 7 reports the results.

[32] In some rare cases, the number of students per teacher appears unreasonably low. However, the results remain almost identical when we exclude observations with fewer than 5 students per teacher (0.24% of the sample).

[33] A report by Statistics Sweden (2024) on teacher absence during the pandemic shows less absence among lower secondary teachers during semesters with more remote instruction compared to semesters with less such instruction, while the same pattern over time is not observed among teachers in lower grades, where remote teaching was rarely used.

Table 7. Effects of distance learning on opportunities to teach or learn remotely when sick, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
A. Semesters teachers could work remotely with symptoms				
Distance learning	-0.035	0.178	0.129	0.330**
	(0.258)	(0.251)	(0.180)	(0.165)
Observations	107,952	122,209	114,576	125,213
R-squared	0.649	0.694	0.678	0.719
Outcome mean	0.083	0.074	0.067	0.061
B. Semesters with remote learning option for sick students				
Distance learning	-0.028	0.179	0.112	0.260*
	(0.223)	(0.218)	(0.167)	(0.142)
Observations	107,785	121,087	113,706	124,263
R-squared	0.733	0.764	0.763	0.787
Outcome mean	0.103	0.092	0.082	0.075

Note: The table shows estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. Outcome means are calculated for all untreated observations.

The results provide some support for the hypothesis that remote instruction, when implemented during a pandemic with high sickness absence rates, can lead to more teaching hours. For the 2023 graduation cohort, for which we find positive effects on educational outcomes from being exposed to 6 or more weeks of remote instruction, teachers had greater opportunities to teach from home when experiencing symptoms and students seem to similarly have had more opportunities to participate remotely when sick. Thus, a potential explanation for the positive impact of remote instruction observed for this cohort is that it allowed for more teaching hours.

7 Conclusions

We have examined the impact of remote instruction on the educational outcomes of lower secondary school students in Sweden during the COVID-19 pandemic. Our results show that students in schools that implemented remote instruction performed at least as well on national standardized tests, on average, as those in schools that continued with in-person teaching throughout the pandemic. Furthermore, these students continued to regular programs in upper secondary school and enrolled in academic programs at least to the same extent. These conclusions are robust across different specifications and consistent across various student groups. We find no evidence of negative effects. In fact, for the later cohort studied – students expected to graduate in 2023 – our results indicate positive, albeit perhaps not that large, impacts for students in schools that used most distance learning (6 net weeks or more).

How should we interpret these findings, given the strong prior evidence that the pandemic negatively affected learning, as well as research indicating that remote instruction is generally less effective than in-person teaching? While we cannot provide definitive answers to this question, we can discuss some possible explanations.

First, it is important to recognize that previous research on learning outcomes during the pandemic has primarily captured the overall impact of the pandemic. The few studies that have utilized within-country variation and data over time to examine the effects of school closures – offering a closer reflection of the impact of remote instruction – have produced mixed results. Relatedly, our findings do not contradict the widely accepted view that the pandemic had a negative impact on student learning. International PISA assessments have consistently shown a global decline in student performance coinciding with the pandemic, a trend also observed among Swedish students. However, our results suggest that the use of remote instruction in Sweden was not a key factor driving this decline.

As argued in this paper, Swedish lower secondary schools represent a case with high levels of digital preparedness, relatively short periods of remote instruction, and a more structured approach to such instruction, where exceptions were made for certain students and online teaching was typically supplemented with teaching in-person. Prior research has shown that a lack of digital infrastructure posed significant challenges in many countries during COVID-19. It has also shown that students with poor prior academic performance or disadvantaged socioeconomic backgrounds were disproportionately negatively affected by the pandemic. Furthermore, earlier research has indicated that negative effects of distance learning tend to be larger when remote instruction is not complemented by face-to-face teaching. In Sweden, all of these challenges were less pronounced due to the way distance learning was implemented.

Another aspect to highlight is that remote instruction may have enabled students and teachers to continue participating in education despite experiencing COVID-19 symptoms. The Swedish strategy was clear: stay home if you have any symptoms. Consequently, the use of distance learning could have increased both teaching opportunities and instructional time. Notably, our findings indicate that for the 2023 graduation cohort – where we observe positive effects on educational outcomes – teachers and students at schools that relied most on remote instruction had greater opportunities to participate in classes when experiencing symptoms also when remote instruction was not in place.

It is also important to recognize that the outcomes in our analysis are measured at the end of, and after, lower secondary school. This means that schools had some time to implement compensatory measures following the period of remote instruction, before students participated in national tests and progressed within the education system. However, when examining student-teacher ratios and schools' use of special education support, we find no sign of compensatory behavior.

Lastly, it is crucial not to interpret our findings as evidence that remote instruction is generally comparable to, or even superior to, traditional in-person teaching. Our analysis has focused on a limited set of outcome measures and the specific approach taken in Sweden during the COVID-19 pandemic. Our results suggest that when implemented appropriately, and perhaps not too extensively, remote instruction can serve as a valuable tool, that can increase hours of instruction, during, for example, disease outbreaks.

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Appendix: Additional tables and figures

Table A 1. Descriptive statistics for the full sample of students

	(1) mean	(2) sd
Female	0.486	0.500
Born abroad	0.119	0.324
Parents born abroad	0.206	0.404
Mother at most upper secondary education[a]	0.378	0.485
Mother post-secondary education[a]	0.480	0.500
Missing data on mother's education	0.040	0.197
Father at most upper secondary education[a]	0.443	0.497
Father post-secondary education[a]	0.366	0.482
Missing data on father's education	0.076	0.265
Father received social assistance[a]	0.060	0.237
Mother received social assistance[a]	0.045	0.207
Special support[b]	0.053	0.224
6^{th} grade standardized grade in math[c]	0.028	0.982
6^{th} grade standardized grade in English[c]	0.025	0.982
6^{th} grade standardized grade in Swedish[c]	0.033	0.978
6^{th} grade standardized grade in social science[c]	0.032	0.980
6^{th} grade standardized grade in science[c]	0.033	0.977
Independent school	0.203	0.402
School located in small town	0.388	0.487
School located in larger town	0.371	0.483
School located in large city	0.241	0.428
Number of obs.	290,901	

Notes: The sample consists of students who began 7^{th} grade in 2013–2016 and 2019–2020, in the schools for which we have (first wave) survey data on distance learning. [a] We measure parental background variables the year the student enrolled in 7^{th} grade, except for the last enrollment cohort, for which we instead use data from 2019 to avoid influences from the pandemic.[b] Special support indicates if at least one of the following measures are relevant for the student: individualized educational plan (*åtgärdsprogram*); adapted study path (*anpassad studiegång*); special education group (*särskild undervisningsgrupp*); individual instruction (*enskild undervisning*); mother tongue study support (*studiehandledning på modersmål*). [c] Grades in different subjects have been standardized within test cohort nationally to have mean 0 and standard deviation 1.

Table A 2. Summary statistics for number of weeks of distance learning, by enrollment cohort, based on schools that responded to both surveys

Cohort	Mean	Sd	Min	Max
2013	0	0	0	0
2014	0	0	0	0
2015	0	0	0	0
2016	0	0	0	0
2017	0.13	0.67	0	11
2018	3.88	3.36	0	20.6
2019	5.78	4.24	0	26
2020	5.64	4.69	0	28.1

Table A 3. Effects of distance learning for students expected to graduate in 2021, weighted regressions (entropy balance)

	(1) <6 weeks T2021	(2) 6+ weeks T2021
A. Enrolled in upper secondary school		
Distance learning	0.010*	0.015**
	(0.005)	(0.006)
Observations	161,656	117,818
R-squared	0.051	0.056
Outcome mean (untreated obs.)	0.887	0.888
B. Enrolled in academic program		
Distance learning	0.010	0.009
	(0.008)	(0.008)
Observations	161,656	117,818
R-squared	0.112	0.110
Outcome mean (untreated obs.)	0.616	0.616

Note: The table shows estimates of model (1). In line with all other analyses, we exclude students belonging to graduation cohort 2020. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. Outcome means are calculated for all untreated observations.

Table A 4. Robustness of main results to using alternative entropy balancing weights

	(1) Unweighted regressions	(2) Weighted, main spec.	(3) Weighted, incl. municipality characteristics[a]
A. Average test score			
<6 weeks, T2022	0.027	0.010	-0.031
	(0.026)	(0.028)	(0.042)
6+ weeks, T2022	0.031	0.035	0.011
	(0.025)	(0.032)	(0.038)
<6 weeks, T2023	0.034	0.031	0.026
	(0.025)	(0.030)	(0.031)
6+ weeks, T2023	0.032	0.059**	0.053*
	(0.026)	(0.029)	(0.030)
B. Upper secondary school			
<6 weeks, T2022	0.016*	0.010	-0.015
	(0.009)	(0.008)	(0.021)
6+ weeks, T2022	0.016*	0.015*	0.003
	(0.008)	(0.008)	(0.013)
<6 weeks, T2023	0.008	0.012	0.011
	(0.009)	(0.008)	(0.012)
6+ weeks, T2023	0.010	0.020**	0.020**
	(0.009)	(0.008)	(0.009)
C. Academic program			
<6 weeks, T2022	0.009	0.010	0.000
	(0.011)	(0.012)	(0.019)
6+ weeks, T2022	0.003	0.012	0.004
	(0.011)	(0.012)	(0.015)
<6 weeks, T2023	0.003	0.009	0.005
	(0.011)	(0.013)	(0.021)
6+ weeks, T2023	0.011	0.027**	0.024*
	(0.011)	(0.013)	(0.014)

Note: The table shows estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Column 1 uses robust standard errors clustered on schools (in parenthesis). Column 2 and 3 use bootstrapped standard errors clustered on schools (in parentheses). *** p<0.01, ** p<0.05, * p<0. [a] Column 3 includes population size and density (measured in 2019), on top of the variables included in col. (2), in the estimation of sample weights.

Table A 5. Effects of distance learning based on schools that responded to both surveys, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
A. Average test score				
Distance learning	0.002	0.068	-0.006	0.080
	(0.046)	(0.044)	(0.069)	(0.056)
Observations	41,296	48,458	42,739	51,210
R-squared	0.119	0.112	0.120	0.109
Outcome mean[a]	0.0311	0.0434	0.0581	0.0210
B. Enrolled in upper secondary school				
Distance learning	-0.005	0.004	0.012	0.024*
	(0.015)	(0.012)	(0.013)	(0.014)
Observations	43,148	50,848	44,551	53,743
R-squared	0.067	0.060	0.056	0.055
Outcome mean[a]	0.885	0.895	0.890	0.891
C. Enrolled in academic program				
Distance learning	-0.013	-0.009	-0.002	0.030
	(0.033)	(0.028)	(0.025)	(0.024)
Observations	43,148	50,848	44,551	53,743
R-squared	0.103	0.094	0.102	0.086
Outcome mean[a]	0.615	0.645	0.629	0.633

Note: The table shows estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.
[a] Outcome means are calculated for all untreated observations.

Table A 6. Robustness of results to excluding cohort 2018 (when national tests leaked)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
A. Average score				
Distance learning	0.015	0.036	0.033	0.058*
	(0.028)	(0.034)	(0.032)	(0.030)
Observations	83,260	94,349	88,016	96,070
R-squared	0.121	0.115	0.128	0.120
Outcome mean[a]	0.0125	0.0271	0.0264	0.00973
B. Mathematics				
Distance learning	0.057	0.075*	0.060*	0.069**
	(0.039)	(0.044)	(0.034)	(0.034)
Observations	78,370	88,551	83,115	90,469
R-squared	0.092	0.088	0.095	0.088
Outcome mean[a]	0.0284	0.0407	0.0437	0.0249
C. Swedish				
Distance learning	-0.032	-0.004	0.012	0.054
	(0.040)	(0.046)	(0.036)	(0.034)
Observations	77,024	86,870	81,661	88,081
R-squared	0.083	0.079	0.089	0.085
Outcome mean[a]	0.0710	0.0717	0.0802	0.0544
D. English				
Distance learning	-0.000	0.012	-0.003	0.021
	(0.027)	(0.028)	(0.031)	(0.031)
Observations	78,017	88,469	82,757	90,090
R-squared	0.098	0.091	0.102	0.092
Outcome mean[a]	0.0392	0.0622	0.0537	0.0491

Note: The table shows estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Regressions are weighted using entropy balancing weights. Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. [a] Outcome means are calculated for all untreated observations.

Table A 7. Effects of distance learning on the probability of attending the same school in spring of 9th grade as at the start of lower secondary school, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
Distance learning	-0.028**	-0.025*	-0.040*	-0.054***
	(0.014)	(0.014)	(0.021)	(0.020)
Observations	106,777	119,704	112,350	122,107
R-squared	0.065	0.071	0.086	0.091
Outcome mean[a]	0.930	0.925	0.929	0.924

Note: The table shows estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. [a]Outcome means are calculated for all untreated observations.

Table A 8. Effects of distance learning excluding students who changed school during lower secondary school, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
A. Average test score				
Distance learning	0.006	0.027	0.039	0.058*
	(0.029)	(0.033)	(0.032)	0.031
Observations	96,558	107,968	101,103	108,964
R-squared	0.122	0.115	0.129	0.119
Outcome mean[a]	0.0343	0.0486	0.0506	0.0319
B. Enrolled in upper secondary school				
Distance learning	0.009	0.012	0.007	0.016**
	(0.007)	(0.008)	(0.007)	0.008
Observations	98,454	109,966	102,945	111,021
R-squared	0.052	0.054	0.053	0.051
Outcome mean[a]	0.907	0.914	0.911	0.910
C. Enrolled in academic program				
Distance learning	0.006	0.006	0.008	0.016
	(0.012)	(0.013)	(0.013)	(0.013)
Observations	98,454	109,966	102,945	111,021
R-squared	0.105	0.099	0.116	0.099
Outcome mean[a]	0.625	0.645	0.632	0.638

Note: The table shows estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. [a] Outcome means are calculated for all untreated observations.

Table A 9. Effects of distance teaching for each treatment intensity decile, students expected to graduate 2022 and 2023

	(1)	(2)	(3)	(4)	(5)	(6)
	Average test score		Enrolled in upper secondary school		Enrolled in academic program	
	T2022	T2023	T2022	T2023	T2022	T2023
0.33-2	0.014	0.008	0.010	0.013	-0.000	0.009
	(0.028)	(0.040)	(0.010)	(0.011)	(0.013)	(0.018)
N	51,803	44,289	54,113	46,308	54,113	46,308
2.01-2.64	0.092^{*}	0.065^{*}	0.031^{**}	0.016	0.050^{**}	0.025
	(0.050)	(0.039)	(0.015)	(0.011)	(0.022)	(0.017)
N	30,434	45,798	31,750	47,765	31,750	47,765
2.65-3.66	-0.004	0.063^{*}	0.015	0.017	0.013	0.013
	(0.035)	(0.034)	(0.010)	(0.011)	(0.017)	(0.016)
N	40,436	47,845	42,445	50,115	42,445	50,115
3.66-5	-0.007	0.020	-0.003	-0.002	0.011	0.003
	(0.038)	(0.044)	(0.012)	(0.012)	(0.017)	(0.018)
N	44,373	43,123	46,406	44,956	46,406	44,956
5.29-6	-0.028	-0.011	0.011	0.009	-0.002	0.002
	(0.037)	(0.032)	(0.011)	(0.012)	(0.016)	(0.017)
N	40,045	51,466	41,746	53,672	41,746	53,672
6.02-7	0.065	0.087^{**}	0.025^{*}	0.019*	0.017	0.035^{*}
	(0.057)	(0.043)	(0.013)	(0.010)	(0.023)	(0.020)
N	37,956	44,997	39,751	47,017	39,751	47,017
7.27-8	0.042	0.128^{**}	0.014	0.042^{***}	0.018	0.068^{***}
	(0.043)	(0.052)	(0.012)	(0.017)	(0.018)	(0.023)
N	41,069	43,383	42,864	45,224	42,864	45,224
8.04-10	0.048	0.053	0.017^{*}	0.019	0.013	0.021
	(0.037)	(0.042)	(0.009)	(0.012)	(0.015)	(0.019)
N	47,310	43,667	49,420	45,677	49,420	45,677
10.05-12	0.021	0.053	-0.001	0.018	0.010	0.018
	(0.037)	(0.042)	(0.009)	(0.012)	(0.015)	(0.019)
N	36,320	47,944	38,026	50,272	38,026	50,272
12.06-26.72	0.025	0.012	0.015	0.011	0.001	-0.004
	(0.036)	(0.042)	(0.011)	(0.013)	(0.016)	(0.019)
N	38,125	41,936	39,958	43,707	39,958	43,707

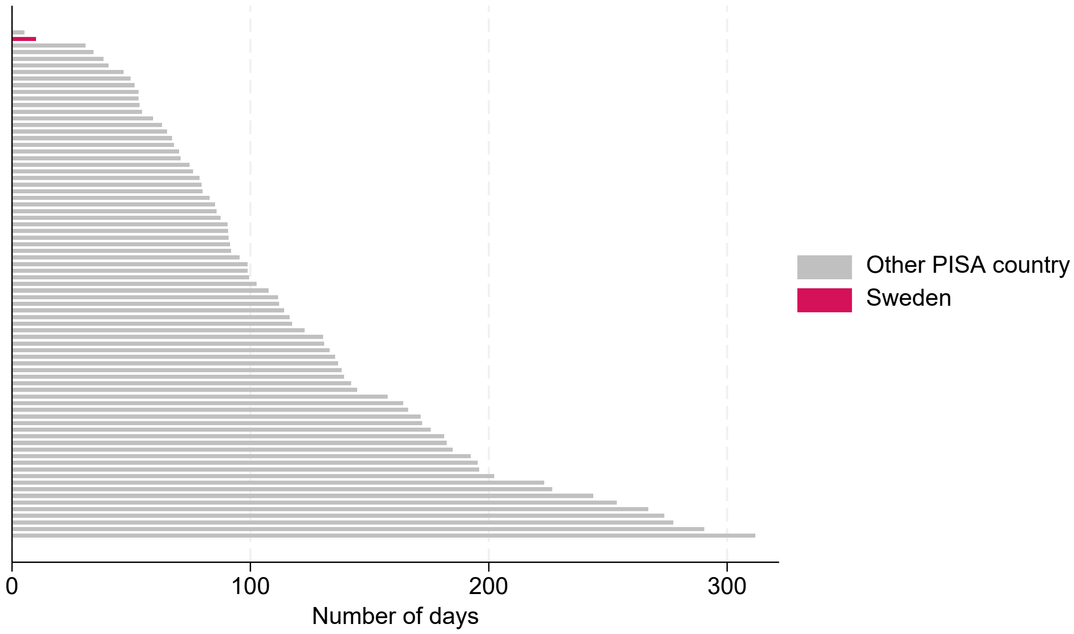
Note: The table shows estimates of model (1), separately for each net weeks of distance teaching decile for the (expected) graduation cohort T2022 and T2023. Regressions are weighted using entropy balancing weights Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0.01

Table A 10. Effects of distance learning on schools' use of compensatory measures, weighted regressions (entropy balance)

	(1) <6 weeks T2022	(2) 6+ weeks T2022	(3) <6 weeks T2023	(4) 6+ weeks T2023
A. Received special education support[a]				
Distance learning	-0.021	-0.019	-0.002	-0.010
	(0.014)	(0.015)	(0.011)	(0.010)
Observations	108,114	121,734	113,765	124,016
R-squared	0.078	0.085	0.080	0.079
Outcome mean[c]	0.0956	0.0866	0.0926	0.0904
B. Number of students per teacher[b]				
Distance learning	-0.058	-0.065	-0.058	0.101
	(0.233)	(0.232)	(0.187)	(0.207)
Observations	108,589	121,819	113,353	123,804
R-squared	0.825	0.780	0.835	0.769
Outcome mean[c]	12.55	12.70	12.57	12.69

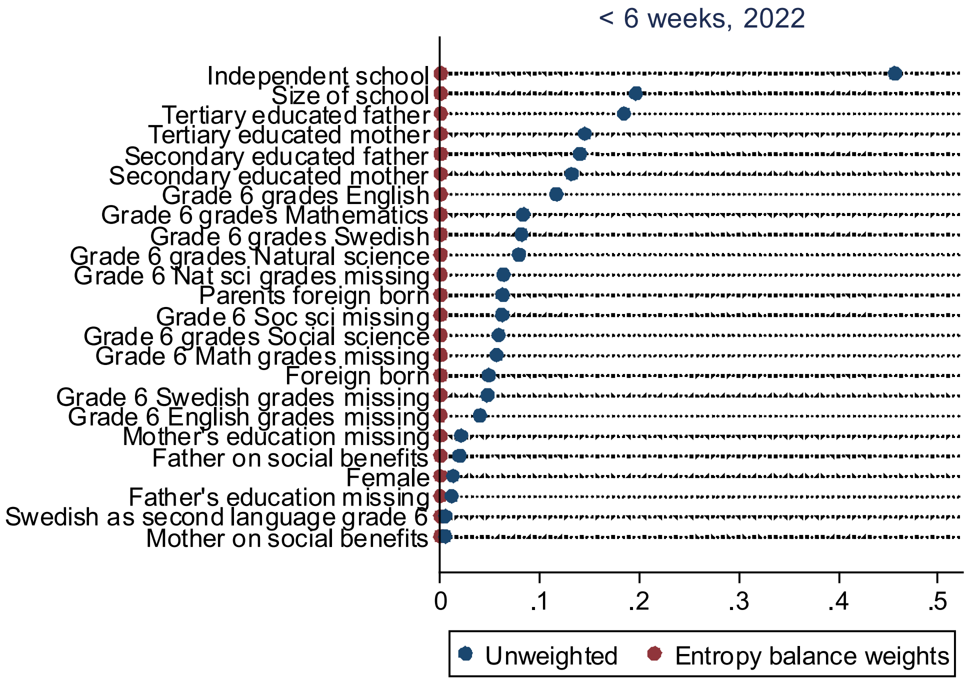
Note: The table shows estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded. Bootstrapped standard errors clustered on schools are presented in parentheses. *** p<0.01, ** p<0.05, * p<0. [a]Special education support includes: an individualized education plan (*åtgärdsprogram*); adapted study path (*anpassad studiegång*); special education group (*särskild undervisningsgrupp*); individual instruction (*enskild undervisning*); mother tongue study support (*studiehandledning på modersmål*). [b]Number of students per teacher is measured at the school level. [c]Outcome means are calculated for all untreated observations.

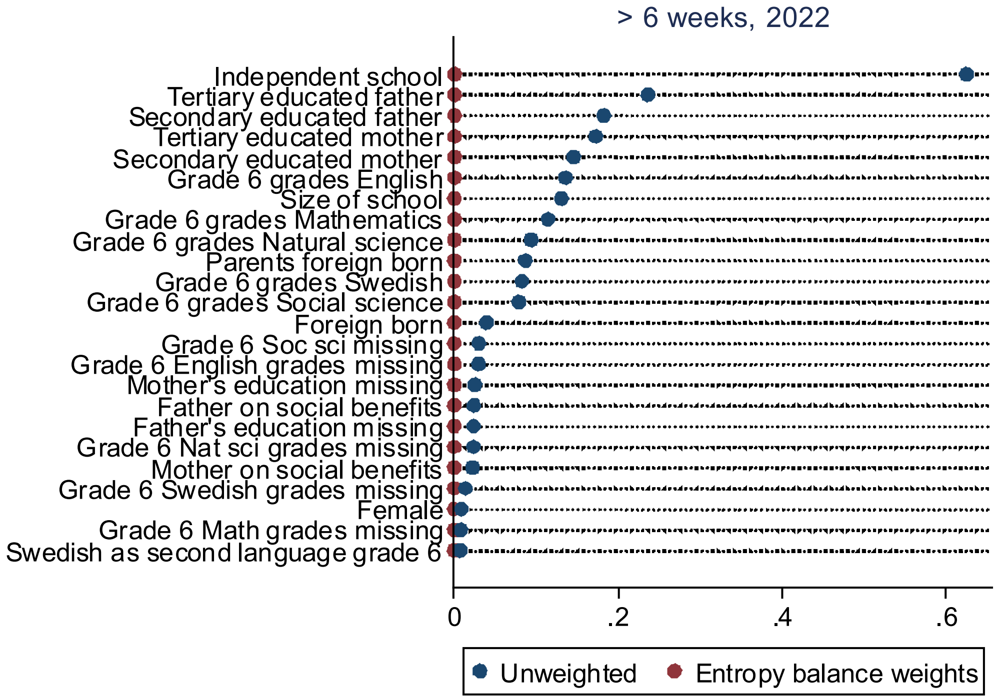
Figure A 1. Number of days in which school premises were closed for the majority of students

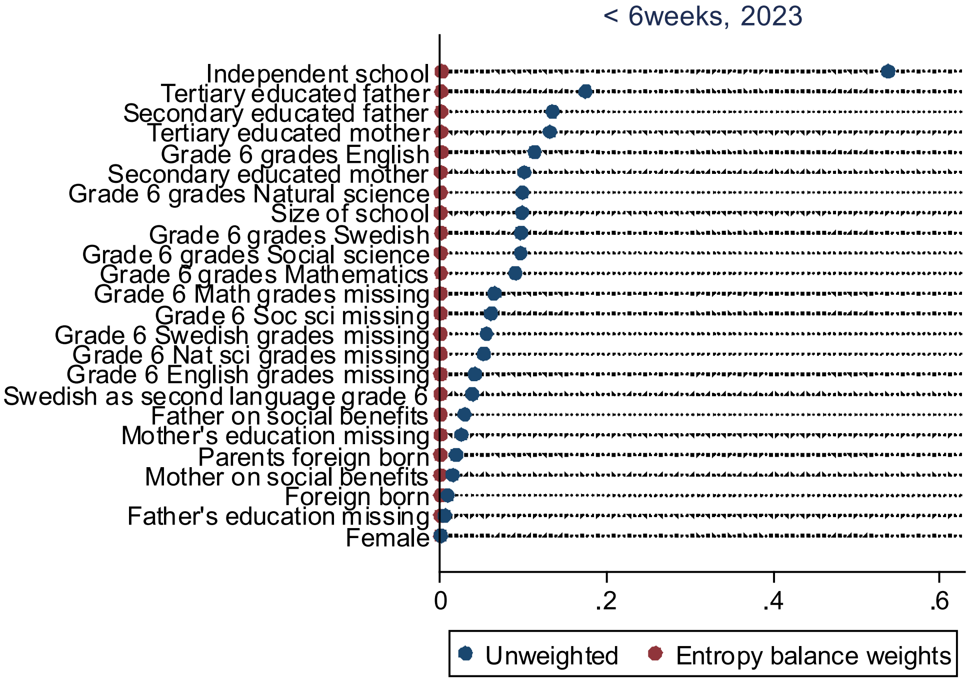


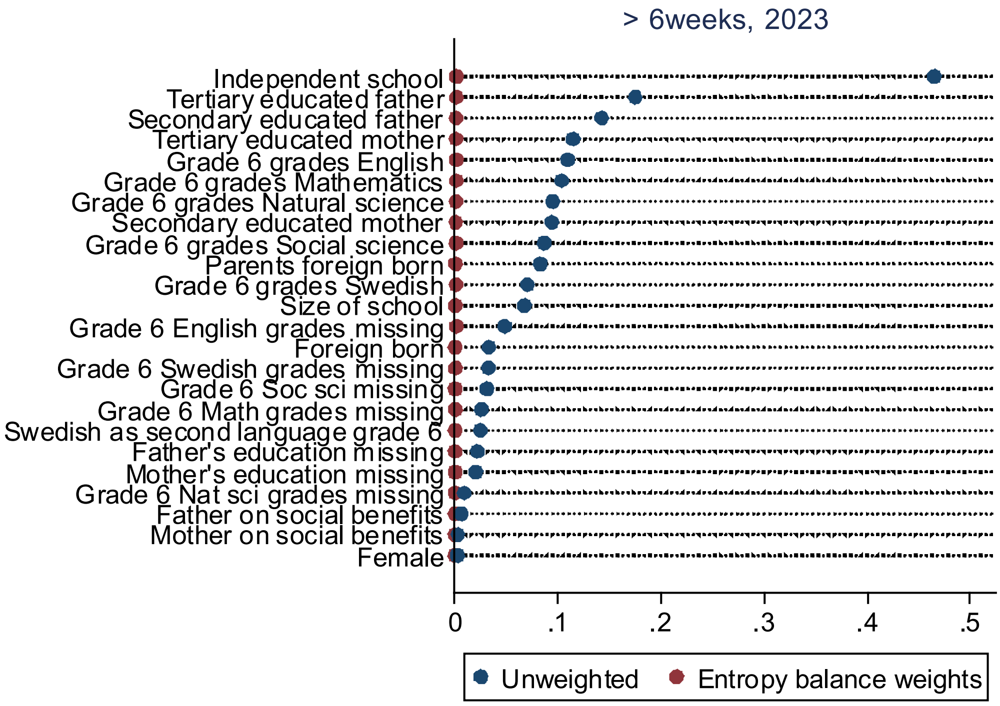
Source: Own calculations using OECD:s PISA 2022 Database

Figure A 2. Covariate balance in treated and control groups (absolute mean standardized differences), for the cohort expected to graduate in 2019 (i.e., the last pre-COVID cohort)






< 6weeks, 2023
Independent school
Tertiary educated father
Secondary educated father
Tertiary educated mother
Grade 6 grades English
Secondary educated mother
Grade 6 grades Natural science
Size of school
Grade 6 grades Swedish
Grade 6 grades Social science
Grade 6 grades Mathematics
Grade 6 Math grades missing
Grade 6 Soc sci missing
Grade 6 Swedish grades missing
Grade 6 Nat sci grades missing
Grade 6 English grades missing
Swedish as second language grade 6
Father on social benefits
Mother's education missing
Parents foreign born
Mother on social benefits
Foreign born
Father's education missing
Female
0
.2
.4
.6
Unweighted
Entropy balance weights


> 6weeks, 2023
Independent school
Tertiary educated father
Secondary educated father
Tertiary educated mother
Grade 6 grades English
Grade 6 grades Mathematics
Grade 6 grades Natural science
Secondary educated mother
Grade 6 grades Social science
Parents foreign born
Grade 6 grades Swedish
Size of school
Grade 6 English grades missing
Foreign born
Grade 6 Swedish grades missing
Grade 6 Soc sci missing
Grade 6 Math grades missing
Swedish as second language grade 6
Father's education missing
Mother's education missing
Grade 6 Nat sci grades missing
Father on social benefits
Mother on social benefits
Female
0
.1
.2
.3
.4
.5
Unweighted
Entropy balance weights

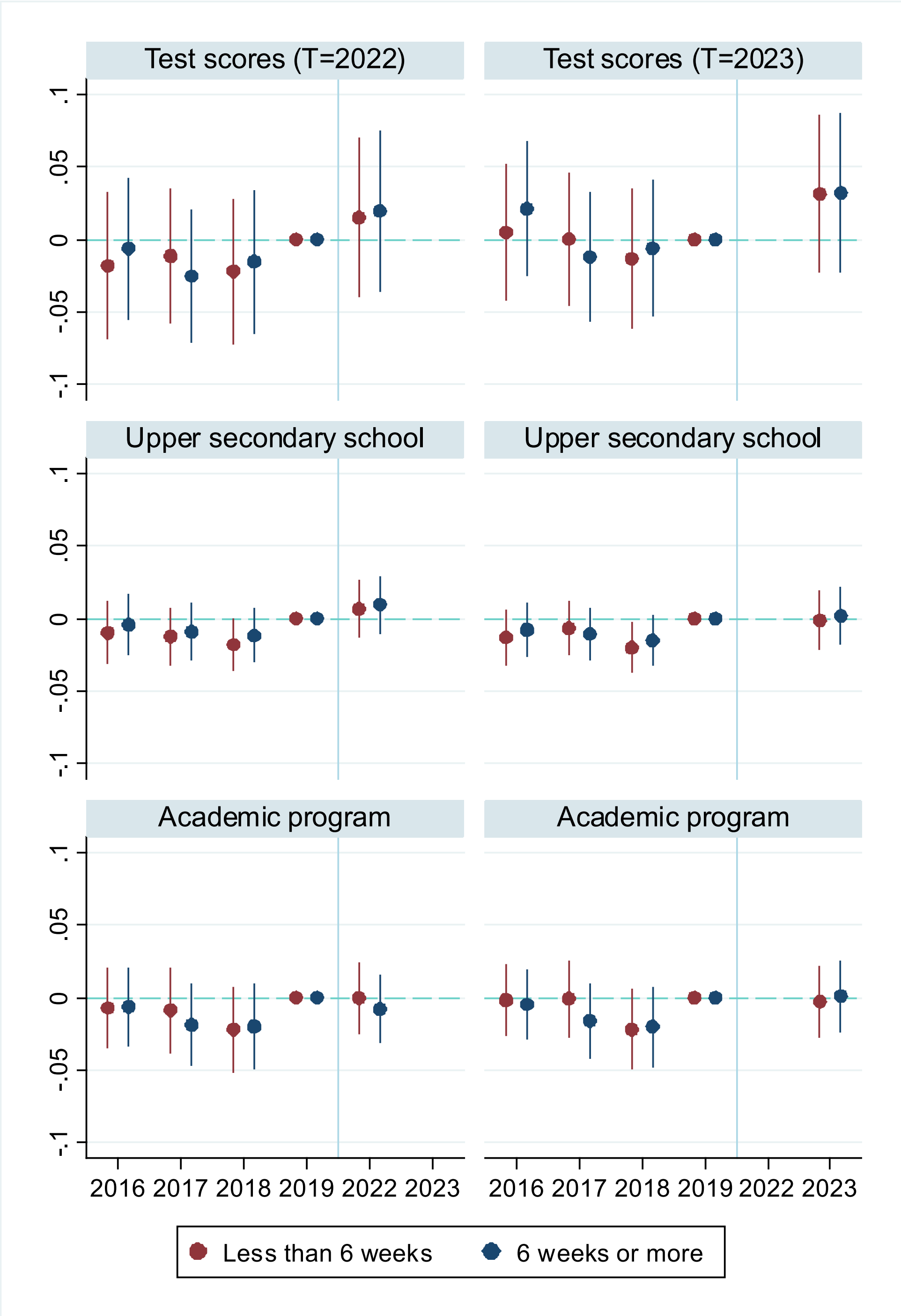


Figure A 3. Time-varying effects of attending a school belonging to each of the two treatment groups, unweighted regressions

Note: The figure shows estimates of model (2), separately by (expected) graduation cohort. The vertical line marks the start of the pandemic. 2019, the last cohort to graduate before the pandemic, is the reference cohort. Students belonging to graduation cohort 2020 and 2021 are excluded as the national tests were cancelled during the pandemic.

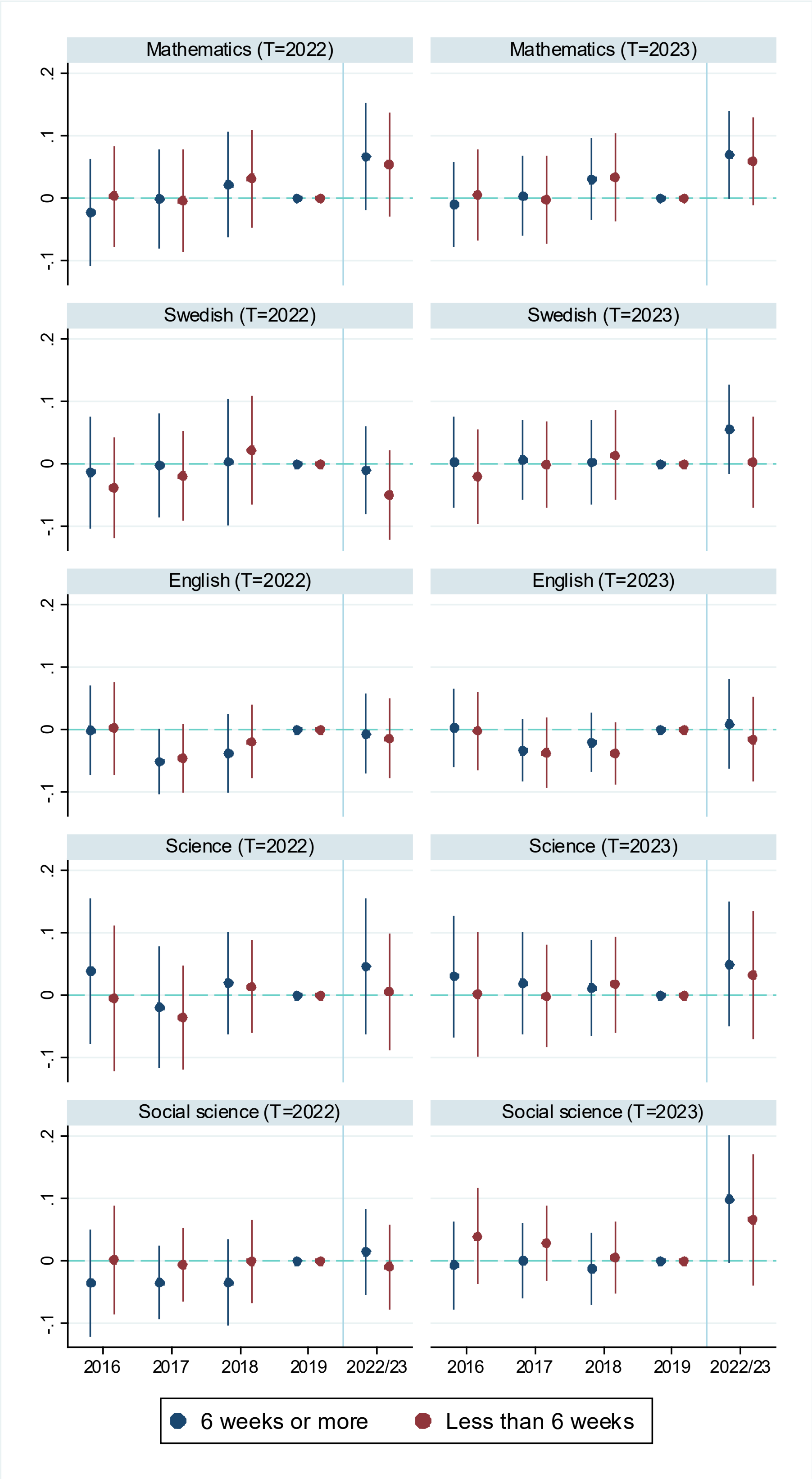


Figure A 4. Time-varying effects of attending a school belonging to each of the two treatment groups, across subjects, weighted regressions (entropy balance)

Note: The figure shows estimates of model (2), separately by (expected) graduation cohort. The vertical line marks the start of the pandemic. 2019, the last cohort to graduate before the pandemic, is the reference cohort. Students belonging to graduation cohort 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.

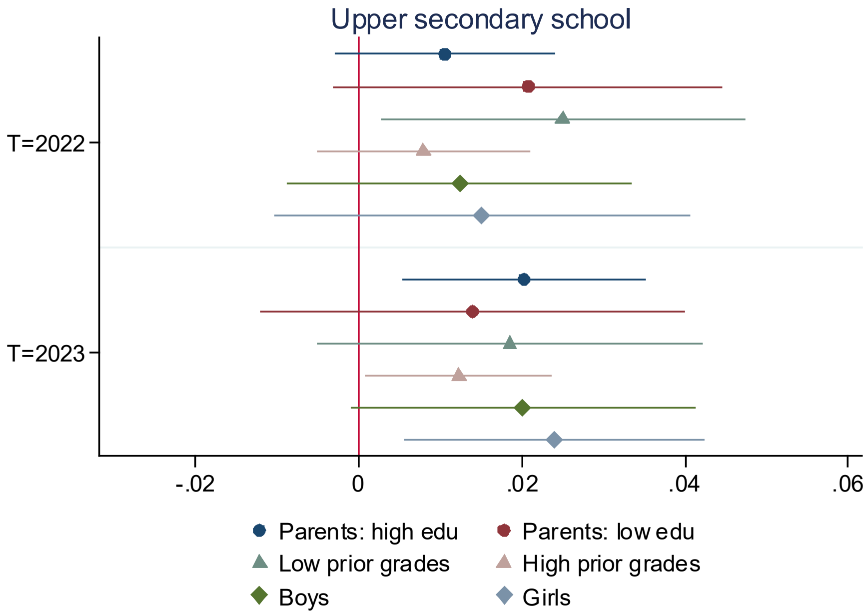


Figure A 5. Effects of distance learning (6+ weeks) on enrollment in a regular upper secondary school program for various sub-groups of students, weighted regressions (entropy balance)

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.

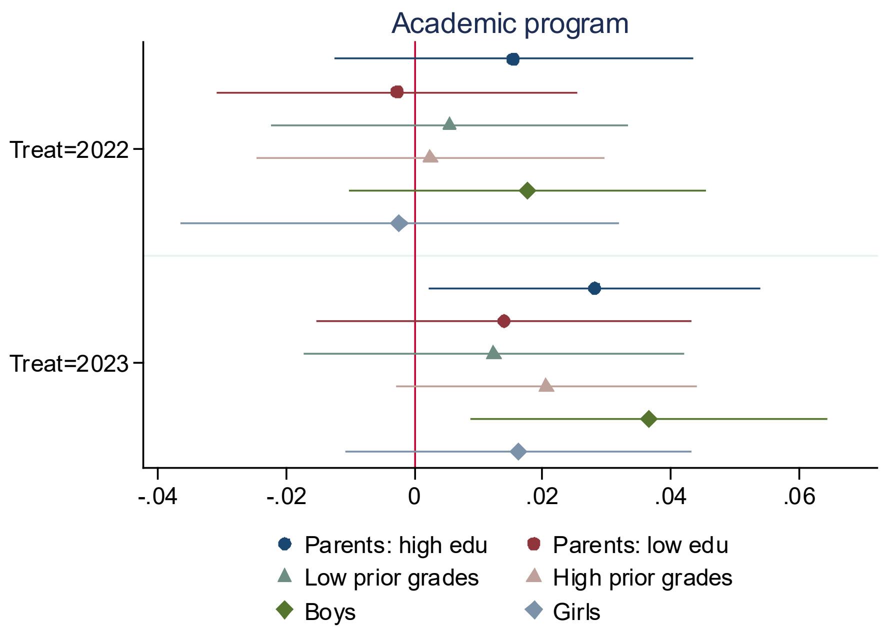


Figure A 6. Effects of distance learning (6+ weeks) on enrollment in an academic program for various sub-groups of students, weighted regressions (entropy balance)

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.

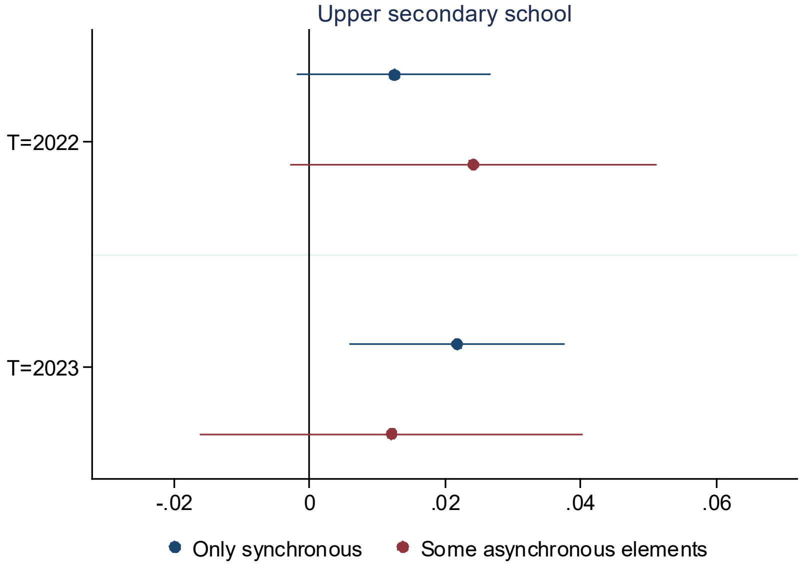


Figure A 7. Effects of distance learning (6+ weeks) on enrollment in a regular upper secondary school program, by type of distance learning, weighted regressions (entropy balance)

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.

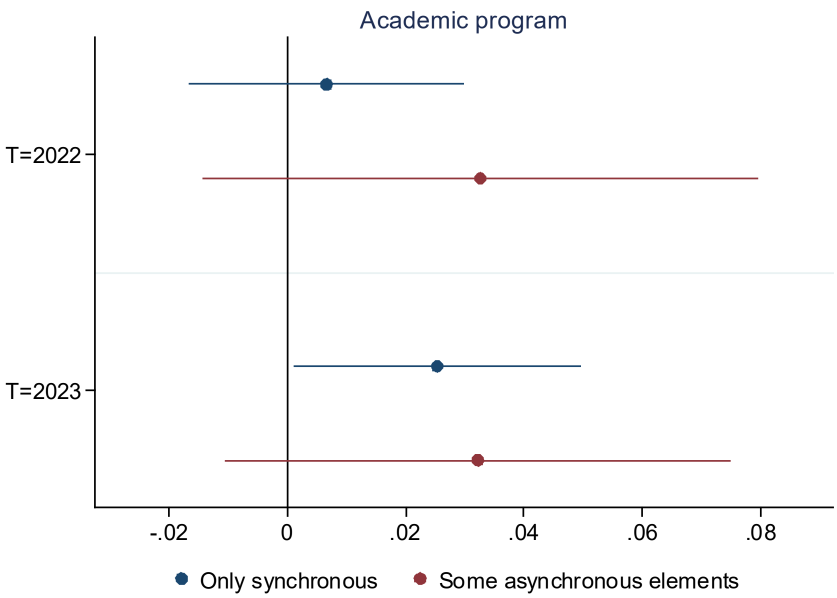


Figure A 8. Effects of distance learning (6+ weeks) on enrollment in an academic program, by type of distance learning, weighted regressions (entropy balance)

Note: Estimates of model (1), separately by (expected) graduation cohort (T2022; T2023). Students belonging to graduation cohorts 2020 and 2021 are excluded as the national tests were cancelled during the pandemic. 95% confidence intervals are based on bootstrapped standard errors clustered on schools.