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Essays on Educational Interventions for Disadvantaged Children

PhD dissertation

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PREFACE

This dissertation was written from September 2018 to December 2021 at the Department of Economics and Business Economics at Aarhus University. I would like to thank the department and TrygFonden's Centre for Child Research for providing a truly excellent research environment and for their generous financial support.

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Søren Albeck Nielsen
Aarhus, December 2021

UPDATED PREFACE

The predefence took place on January 12, 2022. The assessment committee consists of Associate Professor Maria Knoth Humlum (Aarhus University), Professor Mette Ejrnæs (University of Copenhagen), and Associate Professor Joshua Goodman (Boston University). I am grateful for their careful reading of my dissertation and their valuable comments and suggestions. Some of their suggestions for improvement have already been implemented while others remain for future research.

Søren Albeck Nielsen
Aarhus, February 2022

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BRIEF SUMMARY

The aim of this thesis is to investigate educational remedial programs targeting disadvantaged children in lower secondary education. There exists large discrepancies within the OECD countries in the allocation of resources to the school systems with Denmark ranking at the very top. Despite this, Denmark, as well as many other OECD countries, struggles with a large tail of underperforming pupils who do not obtain the necessary level of pro ciency in reading and math.

The dissertation comprises three self-contained chapters within economics of education. Each chapter empirically studies different educational interventions targeting disadvantaged children. The rst two chapters make use of a eld experiment as well as quasi-experimental methods combined with Danish administrative records with the purpose of identifying causal effects of intensive learning camps for grade 8 pupils at-risk of not being able to complete upper secondary education. The third chapter exploits observational methods to evaluate a dyslexia intervention.

The rst chapter investigates an intensive learning camp implemented in the regular school hours and shows positive short-run effects on test scores in math, but no short-run effects on test scores in Danish language. Further, we nd indications of positive long-run effects on the school-leaving exam in math and on enrollment in post-compulsory education. Finally, the analysis nds no evidence that training non-cognitive skills affects academic outcomes.

The second chapter exploits data from a two-week Danish summer camp with a one-year follow-up program. Using difference-in-differences and triple-differencing methods, I nd that the summer camp has positive effects on academic and personal competencies. Furthermore, I nd that changing the follow-up program from individual to group mentoring improves personal and social competencies.

The nal and third chapter studies a 10-week learning program with 18 months follow-up for children with severe dyslexia using difference-in-differences techniques. Surprisingly, there is limited evidence on the effect of dyslexia learning programs in general, although 8% of the Danish population suffer from dyslexia. I nd positive and persistent effects on reading abilities, personality traits, and school well-being, such that participating in the intervention reduces the gap to similar non-dyslexics with up to 80%.

KORT RESUMÉ

Formålet med denne afhandling er at undersøge uddannelsesinterventioner rettet mod udfordrede børn og unge i udskolingen. Der er store forskelle mellem OECD-landene i tildelingen af ressourcer til folkeskolen med Danmark i toppen af fordelingen. På trods af dette kæmper Danmark, såvel som mange andre OECD-lande, med en stor gruppe af underpræsterende elever, der ikke opnår det nødvendige færdighedsniveau i læsning og matematik.

Denne afhandling omfatter tre selvstændige kapitler inden for uddannelsesøkonomi. Hvert kapitel undersøger, ved hjælp af empiriske metoder, forskellige uddannelsesinterventioner rettet mod udfordrede børn og unge. De to første kapitler anvender et lodtrækningsstudie og kvasi-eksperimentelle metoder kombineret med danske administrative registre med det formål at identi cere årsagseffekter af intensive læringsforløb for 8. klasses elever, der er i risiko for ikke at kunne gennemføre en ungdomsuddannelse. Det tredje kapitel anvender observationelle metoder til at evaluere en ordblindeindsats.

Første kapitel undersøger effekten af intensiv læringsforløb implementeret i den almindelige skoletid. Analysen af lodtrækningsforsøget viser positive kortsigtede effekter i matematik, men ingen kortsigtede effekter i dansk. Ydermere nder vi indikationer på positive langsigtede effekter på afgangseksamen i matematik og på optag på ungdomsuddannelse. Endelig nder analysen ingen evidens for, at træning af ikke-kognitive færdigheder påvirker akademiske resultater.

Det andet kapitel udnytter data fra en to-ugers sommerskole med et 12 måneders opfølgningsprogram. Ved at bruge en difference-in-difference strategi, der sammenligner sommerskole drenge med den resterende population af drenge i perioden fra 8.klasse til 9.klasse, og triple differencing nder jeg, at sommerskolen har positive effekter på deltagernes faglige og personlige kompetencer. Ydermere viser jeg, at en ændring af opfølgningsprogrammet fra individuel til gruppevejledning forbedrer de personlige og sociale kompe-

tencer.

Det sidste og tredje kapitel undersøger et 10-ugers læringsprogram med 18 måneders opfølgning for børn med svær ordblindhed. Metodisk anvender jeg en difference-in-difference strategi til at sammenligne ordblinde elever der modtager indsatsen med andre ordblinde over tid. Overraskende er der begrænset evidens for effekten af læringsprogrammer rettet mod ordblinde, selvom 8% af den danske befolkning lider af ordblindhed. Jeg nder positive og vedvarende effekter på læseevner, personlighedstræk og skoletrivsel. Deltagelse i interventionen reducerer således afstanden til sammenlignelige ikke-ordblinde med op til 80%.

SUMMARY

There exists large discrepancies within the OECD countries in the allocation of resources to the school systems, with Denmark ranking at the very top. Despite this, Denmark, as well as many other OECD countries, struggles with a large tail of underperforming pupils who do not obtain the necessary level of pro ciency in reading and math (OECD, 2019). This is a problem for at least two reasons. First, it signals de ciencies in the educational system, since it does not live up to its purpose. Second, low achieving pupils often come from a disadvantaged background, which exacerbating intergenerational mobility and leads to persistent educational inequality of opportunity.

Recent literature shows that low levels of academic abilities are associated with negative school behavior and low school well-being as well as a range of negative long-run outcomes, such as low educational attainment and reduced earnings, employment, and health (Heckman and Mosso, 2014). Thus, academic low performers are a problem, not only for the children themselves, but also for society. High-quality educational remedial interventions are acknowledged as important tools for decreasing social inequality and ensuring a highly quali ed labor force, which is the very foundation for future economic growth and welfare. This dissertation investigates educational remedial programs targeting disadvantaged children in lower secondary education.

The dissertation comprises three self-contained chapters within economics of education. Each chapter empirically studies different educational interventions targeting speci c groups of disadvantaged children. It contributes to the literature, initiated by the 2021 Nobel Prize winners Joshua Angrist, Guido Imbens and David Card on causal impact analysis of policy initiatives, especially within educational policies targeting disadvantaged children (Angrist and Keueger, 1991; Card and Krueger, 1994; Imbens and Angrist, 1994). There is an urgent need for evidence-based policy initiatives aimed at disadvantaged children because it is tremendously expensive to

enhance their human capital (Heckman, 2006). The rst two chapters make use of a eld experiment and methods for observational data combined with Danish administrative records with the purpose of identifying causal effects of intensive learning camps for grade 8 pupils at-risk of not being able to complete upper secondary education. The third chapter employs a difference-in-differences framework to investigate a 10-week learning program with 18 months follow-up for children with severe dyslexia.

The rst chapter is titled "An intensive, school-based learning camp targeting academic and non-cognitive skills evaluated in a randomized trial" (co-authored with Charlotte Hvidman, Alexander Koch, Julia Nafziger, and Michael Rosholm) and evaluates school-based intensive learning camps for pupils assessed "not ready" for upper secondary education using a randomized controlled trial involving 264 schools in Denmark. Despite a great number of studies on intensive learning camps, there is little causal evidence of the impacts of this particular type of camp; only few studies rely on experiments or use appropriate methods for causal inference and none of the interventions studied take place in the school. We investigate two camp variants, with the main variant targeting Danish language, math, and non-cognitive skills. The alternative variant drops the non-cognitive skills training for additional lectures of Danish language and math. Both camps consist of two weeks of learning camp and additional eight weeks of short follow-up sessions with the primary teacher. We nd positive short-run effects in math but no short-run effects in Danish in which only one of two targeted areas improves. We nd weak evidence of positive long-run effects on the school-leaving exam in math and on enrollment in upper secondary education. Finally, we nd no evidence that targeting non-cognitive skills in this camp setting improves academic performance. Our results thus provide a new perspective on recent evidence regarding the effects of training non-cognitive skills – by running an intervention with older pupils and in a comparatively high-resource school system.

The second chapter, "Boys Left Behind: The Effects of Summer Camp and Follow-up Strategies on Academic, Personal, and Social Competencies", investigates the consequences of summer camp participation for disadvantaged boys in grade 8. I also study how mentoring strategies in the follow-up program affects outcomes. Previous studies show weak evidence of summer camps effect on academic outcomes for pupils in the transition from lower to upper secondary education (Mariano and Martorell, 2013; Jacob and Lefgren,

2004; Battistin and Schizzerotto, 2019). Yet, there is not much knowledge about the effect on non-academic outcomes. The analysis is based on individual level panel data obtained from the detailed administrative data covering all children in compulsory school. Using a difference-in-differences strategy, I nd positive effects on academic and personal competencies. Furthermore, I exploit a structural change in the follow-up program to evaluate how changing from individual to group mentoring in the one-year follow-up program affects outcomes. Using a triple differences strategy, I nd that group mentoring dramatically improves personal and social competencies. The results suggest that the camp itself affects academic competencies whereas the format of the follow-up program is crucial for the impacts on non-academic competencies.

In the third and nal chapter, called "How to Cope with Dyslexia: The Effects of Special Education on Academic Performance, Personality Traits, and Well-being", I use Danish administrative data to study the effects of a special education intervention for pupils with severe dyslexia. The intervention consists of a 10-week learning program with 18 months follow-up for pupils in grade 4 to 8. Dyslexia is a learning disorder that affects 3-10% of the population (Snowling, 2013). It affects the ability to read and write and has potential long-run consequences through low school grades, poor educational attainment, and behavior problems (Undheim, 2009; Epnion, 2018; Einar et al., 2001). Yet, there exists no causal evidence for programs speci cally targeting pupils with dyslexia. I use Danish administrative data combined with the membership list from the Danish Library and Expertise Center for people with print disabilities, which enables me to identify pupils diagnosed with dyslexia who have not participated in the intervention under study. My empirical approach exploits individual level panel data that allow me to track pupils before and after the initiation of treatment. Using a difference-in-differences strategy, I nd positive and persistent effects on reading abilities, personality traits, and school well-being. The intervention increases pupils' outcomes well beyond other dyslexics and signi cantly reduces the gap to non-dyslexics. The results show that the intervention enables pupils with severe dyslexia to participate in age-appropriate learning with their peers.

This dissertation compounds three self-contained intervention studies that all aim to improve disadvantaged children in lower secondary education life trajectories. Table 1 shows the mean of key standardized variables for each chapters' intervention group prior to intervention. It is evident from the table that the participating children are signi cantly disadvantaged. Across

Table 1: Mean for the intervention group across chapters

	Chapter 1	Chapter 2	Chapter 3
Language Comprehension (std.)	-0.522	-0.546	-0.823
Decoding (std.)	-0.609	-1.095	-1.475
Text Comprehension (std.)	-0.637	-0.927	-0.862
Conscientiousness (std.)	-0.515	-0.616	-0.387
Agreeableness (std.)	-0.214	-0.527	-0.22
Emotional Stability (std.)	-0.149	-0.103	-0.111

Notes: The table shows mean of key variables for the main intervention group across the three chapters. All variables are standardized to mean zero and standard deviation of zero using the full population. They are measured in the time-period up to treatment.

all variables, they are below the population average of zero with the largest differences for the academic measures. Their reading abilities are between 0.5 and 1.5 standard deviations below, indicating a signi cant academic backlog.

A common theme in all three chapters concerns the impacts of interventions on non-cognitive skills, which are known to have important effects on later life outcomes of the child (Durlak et al., 2010; Kautz et al., 2014). This is possible due to the existence of high-quality data from compulsory national well-being tests in public schools in Denmark (Andersen et al., 2020). Furthermore, each chapter aims to reduce the inequalities by examining not only the effects of the intervention under study but also exploiting small design variations within the interventions to provide better guidance for policy-makers, i.e. additional training of cognitive skills vs. non-cognitive skills and mentoring strategies in the follow-up program.

The rst two chapters of this dissertation investigates how intensive learning camps implemented at different stages with different follow-up programs impact both academic and non-academic outcomes. This is a crucial and important piece of information for policy-makers in their policy decisions, i.e. deciding on a strategy to enhance the abilities of disadvantaged pupils, especially when considered in relation to the large discrepancies in the costs of different interventions. Chapter 1, then, nds that additional hours of non-cognitive skills training relatively to cognitive skills training, do not change the effect of a school-based intensive learning camp in a high-resource lower secondary school system. Chapter 2, in contrast, focuses on the follow-up mentoring strategies and nds that policy-makers must carefully select the follow-up program in order to enhance personal and social competencies. Chapter 3, supports this nding by showing that a 18 months individual

follow-up program to a 10-weeks learning program leads to persistent learning effects and in some cases increasing effects on personality traits and school well-being.

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CHAPTER 1

AN INTENSIVE, SCHOOL-BASED LEARNING CAMP TARGETING ACADEMIC AND NON-COGNITIVE SKILLS EVALUATED IN A RANDOMIZED TRIAL

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Abstract

We evaluate school-based, intensive learning camps for pupils assessed `not ready' for post-compulsory education, using a strati ed cluster randomized trial involving 15,559 pupils in 264 schools in Denmark. Next to Danish and mathematics, the main variant targets non-cognitive skills. The alternative variant uses this time for more training in Danish and math. We nd positive short-run effects in the standardized test score in math (effect sizes 0.06 – 0.2) but not in Danish, where only one of two targeted academic areas improves. We nd weak evidence of positive long-run effects on the nal exams in math in grade 9 and enrollment in post-compulsory education 2.5 years post-intervention. We nd no evidence that training of non-cognitive skills affects academic outcomes. Our results provide a perspective on recent evidence regarding the effects of training non-cognitive skills – by running an intervention with older pupils and in a comparatively high-resource school system.

Keywords: Randomized trial, remedial education program, non-cognitive skills

JEL Codes:I21, C21, D91, I28

1.1 Introduction

All over the world, education systems struggle with a large tail of underperforming pupils. In a recent PISA study (OECD, 2019), 23 percent of pupils were below the minimum level of pro ciency in reading that all children should have acquired by the end of lower secondary education. In mathematics, the share scoring below this threshold was 24 percent. Such outcomes are a problem for at least two reasons. First, it is a signal that the education system does not live-up to its purpose. Second, low achieving pupils often come from a disadvantaged background, thus exacerbating low intergenerational mobility and persistent educational inequality of opportunity.

Numerous public and private remedial education programs exist that target low-achieving pupils. Interventions that can be delivered at scale at a relatively low cost per pupil are of particular interest to policy makers, which explains the popularity of intensive learning camps where pupils train one or a few subjects intensively during a limited time period (typically 1-4 weeks). Speci cally, the disruption to education caused by the Covid-19 pandemic has increased the need for effective remedial education programs that can help a large number of pupils to recover learning losses (Di Pietro et al., 2020).

The aim of this study is to evaluate, using a large strati ed cluster randomized trial involving 15,559 pupils in 264 schools in Denmark, the impact of an intensive learning camp that includes novel elements compared to traditional intensive learning camps in terms of (i) being run at the pupils' school by the school's teaching staff during regular school hours and (ii) targeting both academic and non-cognitive skills.

There is reason to believe that combining these elements can strengthen the impact of intensive learning camps. First, running such a camp at a pupil's own school during regular school hours can remove potential barriers to participation. For example, Lee et al. (2006) document that many potential participants face barriers to participation in after-school or summer school programs targeting at risk pupils, most notably getting to and from the program and participation in other activities. Second, being school-based permits teachers to follow-up on the learning camp, both in the associated follow-up program and during regular teaching, and ensures longer-lasting engagement by the pupils. Third, it is widely recognized that non-cognitive skills play an important role for academic performance. [1] Recent evidence points to such non-cognitive skills being malleable over a short time frame: Alan and Ertac (2018) and Alan et al. (2019) use randomized trials to document large and lasting effects – also on academic outcomes – of training elementary school pupils in Turkey

[1] For example, Duckworth et al. (2007) demonstrates the importance of grit for academic achievement. Duckworth and Seligman (2005); Duckworth et al. (2012) shows that self-control is as important as IQ in predicting academic performance and Eskreis-Winkler et al. (2014) observe that grit predicts completing school, among other outcomes. Heckman et al. (2006) and Almlund et al. (2011) examine the predictive power of various personality measures (vs. IQ).

in non-cognitive skills related to patience (the former study) and perseverance (the latter study). Thus, the many and diverse reasons for which pupils fall behind in their learning can potentially better be addressed by complementing the commonly applied `more of the same' training on academic skills with training on non-cognitive skills.[2]

The target group of the two-week camps were grade 8 pupils assessed as "not ready for further education". [3] The camps were conducted at the school of a participating pupil during the regular teaching hours – replacing the lessons that these pupils would otherwise have received. After the camp, pupils participated in a follow-up program where they met with a camp instructor for 1.5 hours once a week for eight weeks. The camp costs the school approx. 750$ per pupil.

We evaluate two variants of the camp. In addition to math and Danish, the main learning camp variant devoted roughly 30 percent of the time to strengthen the non-cognitive skills of pupils. For example, pupils (and teachers) learned about self-regulation strategies – such as goal setting (Locke and Latham, 1990) and mental contrasting with implementation intentions (Gollwitzer et al., 2011); and they were introduced to the concept of “growth mindsets" (Dweck, 2006), which re ects a view that ability is malleable and that success is driven by effort.

In the alternative variant of the camp, the time spent on training non-cognitive skills in the main variant was instead used for extra training in math and Danish. The motivation for testing two variants of the camp stems from the uncertainty as to whether non-cognitive skills can be manipulated during an intervention as short as the one studied here and which is aimed at 8 th graders rather than younger pupils. The two variants allow us to explore if the time spent training non-cognitive skills makes a difference relative to using it for additional training of academic skills.

An advantage of our study is that we can exploit Danish register data to evaluate the effects of the camps using standardized national tests. In doing so, we can circumvent issues that other studies face when relying on teachers' evaluations of pupils' skills (the treatment may affect teachers' evaluations rather than pupils' actual academic level) or tests designed by the researchers (the problem of `teaching to the test'). The national tests were conducted brie y after the interventions and thus provide short-run effects of the interventions. In each subject, these tests measure

[2] First, there are intellectual disabilities, dyslexia/dyscalculia, and other medical/neurological causes. Second, psychological causes comprise, for example, math anxiety, low self-ef cacy, and low levels of conscientiousness/grit (e.g., OECD, 2013). Third, sociological causes include primarily the learning environment and the fact that some children grow up in less advantaged circumstances than others, and this affects their ability to learn. Lastly, didactic factors re ect inadequate teaching methodologies and variations in teacher quality (e.g., Hanushek and Rivkin, 2006; Hanushek, 2011).

[3] Between a quarter and a third of a school cohort are considered to be 'not ready for further education' by the end of lower secondary education (Undervisningsministeriet, 2017a,b). Denmark has 10 years of compulsory schooling and starts counting with grade 0. So grade 8 corresponds to grade 9 in other countries.

the competencies of the pupil in different areas – some of which were targeted in the camp (such as numbers and algebra in math or text and language comprehension in Danish) and some of which were not (such as decoding in Danish or geometry and statistics and probability in math). This allows us to test both for direct effects of the camp on learning and for indirect spillover effects to other academic areas. Further, we measure the long-run effects of the camp using grades in the nal exams in grade 9, which were given one year after the intervention and involved external examiners, and by looking at post-compulsory education enrollment.

We observe positive short-run effects on the targeted area in mathematics (numbers and algebra) with effect sizes ranging from 0.07 to 0.19, depending on whether we focus on the entire grade 8 cohort among which some pupils were offered the camp or the pupils actually receiving treatment. The effect sizes for overall performance in the math standardized test range from 0.06 to 0.2. In Danish, the evidence for positive effects is weaker as only one out of the two targeted areas (namely, text comprehension) is positively affected by the camp with effect sizes ranging from 0.07 to 0.18. In particular, there is no signi cant impact on the overall performance in the Danish standardized test. In terms of long-run effects, we do not nd any effects that are consistently statistically signi cant at conventional levels. There is some weak indication of the camp having a positive impact on math performance in the nal exams in grade 9 (effect sizes range from 0.06 to 0.18) and on being enrolled in post-compulsory education 2.5 years after the camp (0 to 8 percentage points, depending on the estimate, which should be compared with the baseline of 19 percent not being enrolled in post-compulsory education in the control group).

We also examine the effects of the camp on non-cognitive skills. Next to self-administered pre- and post-surveys that included several psychological scales, we observe in the register data how school counsellors evaluated, according to schemes provided by the ministry, a pupil's readiness for further education, and use here speci cally the evaluation of the personal and social skills. We do not observe any positive effects on any of these outcome measures for the main learning camp that trained non-cognitive skills.

The measurement of non-cognitive skills with surveys and the evaluation of the pupil's readiness for further education has some disadvantages. However, there is some evidence that suggests that the lacking effects on non-cognitive skills are not merely a measurement problem. Firstly, we do not observe any spillover effects to non-targeted academic areas in the standardized national tests. If the intervention at the camps affected non-cognitive skills such as self-control, one could expect the improved non-cognitive skills to broadly enhance academic performance both in targeted and non-targeted areas. This, however, is not the case. Secondly, when comparing the two camps, we primarily nd non-signi cant differences in outcomes between the two, once again suggesting the lacking effect of non-cognitive skills. Overall, the results thus suggest that the main camp did not affect non-cognitive skills.

The latter results are in contrast to the studies by Alan and Ertac (2018) and Alan et al. (2019). Their interventions trained non-cognitive skills for a total duration of 24 hours spread over a course of 12 weeks in Turkish primary schools. When compared to our main camp, a similar amount of time was dedicated to training non-cognitive skills, with 16 hours devoted solely for this purpose during the two-week camp and additional time for revisiting these skills during weekly sessions in the eight-week follow-up program. Thus, our intervention had a similar dosage to that of Alan and Ertac (2018) and Alan et al. (2019). One possible reason for the diverging ndings across studies could stem from the different age pro les – the pupils in our study are 15-16 years old, while those in Alan and Ertac (2018) and Alan et al. (2019) are 9-10 years old. Non-cognitive skills may be more malleable in these younger kids than for the pupils in our study (cf. Kautz et al., 2014, for a discussion of differential plasticity of different skills by age). Other differences that we discuss in section 1.5 are that their interventions focused on a more narrow set of non-cognitive skills than our camp and that they implemented their interventions in a school system with fewer resources[4] compared to the Danish system.

The paper is structured as follows. Next, we discuss the related literature. Section 1.2 describes the background and design of the study, such as the teaching materials and randomization procedure. We describe the data in section 1.3 and the results in section 1.4. Section 1.5 provides a discussion of possible caveats and interpretations of our ndings. The last section contains conclusions.

1.1.1 Related literature

Our study provides two main contributions. First, we evaluate, using a large randomized trial, a camp that is conducted in school, during school time by the school's regular teaching staff and is, in doing so, distinguished from the prevalent summer camps. Clean evidence for such school-based camps is limited in the literature. Related evidence, however, exists regarding learning camps during summer, small and medium group instruction in schools, and increased instruction time in school, all of which constitute elements in the school-based intensive learning camp in our study. Yet, the existing evidence is quite mixed, so it is hard to have any a priori expectations regarding the effectiveness of such a camp. Second, we contribute to the literature by including non-cognitive skills in the curriculum of an intensive learning camp. As most existing studies examining non-cognitive skills focus on longer duration programs and/or on younger pupils, the question arises whether the non-cognitive skills of middle-school pupils can be affected during an intensive learning camp.

[4] One dimension of measuring the resources used in a school system are monetary expenditures per pupil. Denmark has higher expenditures per pupils than the OECD average, Turkey lower expenditures.

Intensive learning camps. Recent meta-analyses of intensive learning camps, which are primarily summer camps taking place outside the school system, are Lauer et al. (2006), Cooper et al. (2000), and Kim and Quinn (2013).They all report statistically signi cant effects on academic outcomes (math and/or reading), albeit with small effect sizes.

Despite a great number of studies on intensive learning camps, there is little causal evidence. Only few studies rely on experiments (based on randomization) or use quasi-experimental methods. Among the most convincing existing evaluations of intensive learning camps are studies relying on quasi-experimental methods using a regression discontinuity design (RDD) and data from standardized tests. Mariano and Martorell (2013) exploit test score cutoffs in the assignment to a summer camp for 5th-7th graders in New York. They can track the grades of the pupils up to 2-3 years after the intervention. They nd some effects of the camp on English language performance, but little effect on math performance. Matsudaira (2008) study an intensive learning camp for pupils in or above grade 3 in a large urban school district in the U.S. He nds positive effects on math and reading performance around one year after the camp. The RDD of Jacob and Lefgren (2004) is based on the Chicago Social Promotion Policy. They nd positive effects on math and reading performance in the short and long run for grade 3 pupils but not for grade 6 pupils. Battistin and Schizzerotto (2019) exploit geographic variations in the implementation of mandatory summer courses for at-risk pupils in Italy. They nd negative short term effects on academic performance (the marks given by teachers, nal examination and a test based on the PISA tests) in vocational schools and no effects in academic schools. Using a difference-in-differences framework, Schueler et al. (2017) nd positive effects of week-long vacation academies in math taken by 1,800 pupils.

The few studies that rely on randomization mostly have small sample sizes. [5] In a study with 573 observations, Somers et al. (2015) evaluate a summer camp for middle school pupils called the Building Educated Leaders for Life program. They nd some positive effects on math performance, but little impact on reading. In a study with 263 observations, Lynch and Kim (2017) study a math summer camp for 3rd-9th graders from low-income households in the US. Only the condition where pupils participate in the camp and receive a laptop has an effect on academic performance. In a study with 435 observations, Gorard et al. (2015) study a summer camp for grade 5 and 6 pupils on English and math. They nd a short-run effect in English, but no effect in math.

Schueler (2020) evaluates an RCT with 1,187 struggling sixth and seventh graders nominated for one week vacation academies in math offered as part of turnaround reforms in nine low-performing Massachusetts middle schools. She nds no signi - cant improvement in standardized math test scores, but evidence of lower exposure

[5] In the following, we only review those studies that have at least 100 observations (which is still on the low side in terms of ensuring suf cient power).

to disciplinary actions. Compliance however was an issue with only 44 percent of the pupils assigned to treatment attending the academy and 18 percent of control pupils ending up getting treated.

Overall, there is weak evidence on the effect of intensive summer camps on academic outcomes in the transition from lower to upper secondary education. Evidence on school-based intensive learning camps and on non-academic outcomes is lacking in the literature.

School-based interventions. Dietrichson et al. (2020b) and Dietrichson et al. (2020a) provide systematic reviews and meta-analyses of the large literature on school-based interventions for low achieving pupils in grades k-6 and 7-12, respectively. Taken together, these two studies cover close to 300 interventions, however, none of which could be classi ed as an intensive learning camp.

Most closely related to our study are programs that increase instruction time in certain subjects, or that analyze the effects of teaching smaller groups. Regarding the latter, Dietrichson et al. (2020b) and Dietrichson et al. (2020a) nd that peer-assisted instruction and small-group instruction (1-5 pupils per teacher) has the largest positive effects in comparison to other school-based interventions. Medium-group instruction (6-20 pupils per teacher) also shows signi cant positive effects, but this category only exists for the review of interventions aimed at grades k-6. Our intervention contributes here with evidence on the possible effects of such medium-group instructions for older pupils.

Regarding the effects of an increase in instruction time, Lavy and Schlosser (2005) exploit the gradual phasing in of schools to identify the causal effect of a program targeting underperforming pupils in Israel in grades 10-12. They nd that participating pupils were more likely to receive matriculation certi cates. Yet, the program was found to be less cost-effective than alternative interventions. Cortes et al. (2015) use an RDD to study the effects of doubling the instruction time in math for low-skilled 9th graders and nd positive effects both in the short and long run.

Non-cognitive skills. Given the importance of non-cognitive skills for academic outcomes, a range of educational programs try to target these skills. Durlak et al. (2010) provide a meta-analysis of after-school programs that have the aim to enhance non-cognitive skills. They report positive effects not only on the non-cognitive skills targeted, but also on academic outcomes. Kautz et al. (2014) summarize the literature on interventions targeting cognitive and non-cognitive skills in children and adolescents. They emphasize the importance of not only considering cognitive skills (IQ and test scores) when evaluating interventions, but also non-cognitive skills. Further, they point out how interventions for younger children typically have a larger impact than interventions targeted at adolescents or young adults. While the former often impacts non-cognitive skills, the latter often treat problem behavior. They note

that the most successful interventions for adolescents are those that target (also) non-cognitive skills.

Unlike intensive learning camps, programs targeting non-cognitive skills typically run over extended periods of time and use or combine mentoring or training for parents and teachers (see, e.g., McCord, 1978; Tierney et al., 1995; Kemple and Willner, 2008; Durlak et al., 2010; Rodriguez-Planas, 2012; Holmlund and Silva, 2014; Martins, 2017; Kosse et al., 2020, for programs targeting a similar age group as our study). Most studies, with the exception of McCord (1978) and Rodriguez-Planas (2012), nd positive effects on outcomes. Our study is most closely related to the studies by Alan and Ertac (2018) and Alan et al. (2019) discussed above, as these studies are also school-based and run over a comparable time frame.

1.2 Study design

1.2.1 Background

The camps are funded by a special grant by the Ministry of Children and Education to investigate intensive learning camps as tools for improving readiness for post-compulsory school education in grade 8. All pupils in Danish public schools undergo an Education Readiness Assessment (ERA henceforth) during the Fall term in grades 8 and 9, and from 2018/2019 also during the Spring term in grade 9. The evaluation is done by the pupil's school following detailed guidelines from the Ministry of Children and Education [6] and encompasses academic outcomes (grades) as well as personal and social skills. The purpose with the ERA is to identify pupils who are not ready for their desired upper secondary education and initiate discussion on suitable educational choice and implement interventions to make them ready. In a given year, between a quarter and a third of the pupils are assessed to lack the academic, social or personal skills required for a post-compulsory school education (Undervisningsministeriet, 2017a,b). The intensive learning camps in this study are targeted at such “non-ready"-pupils (NR-pupils henceforth).

1.2.2 Structure of the camp

We evaluate three rounds of intensive learning camps that took place in 2017-2019. There are two variants of the camp: Camp+ and Camp, which vary in terms of whether part of the time is devoted to training non-cognitive skills (Camp+) or to additional training in Danish and math (Camp). The other key components of the camps are lower teacher-student ratio, teaching materials developed to low-performing pupils, school-based main camp, and 8-weeks follow-up program with one of the camp

[6] Guidelines (in Danish): https://www .uvm.dk/vejledning-og-stoettemuligheder/vejledning/ungdomsuddannelse/uddannelsesparathed/om-vurderingen .

teachers. Each camp is taught by the school's own teachers and pedagogues as a remedial education program. [7] The camps run during the regular school hours and replace the normal instruction that participating pupils otherwise would receive.

In each school, approximately 10-14 pupils participate in the camp. Two teachers (or a teacher and a pedagogue) teach these pupils. In case more than 15 pupils participate (a camp may take up to 20 pupils), 3 teachers are present. Prior to the camp, the teachers participate in a two-day work shop during which they learn about the teaching materials, key ideas behind the camp, and the rules and procedures of the camp. Materials include detailed teacher guides for both the camp and the 8-week follow-up. [8]

A camp lasts 2 weeks with 35 teaching hours per week (including around one hour of breaks each day). Each day is structured in the same way except for the rst and last day of the camp. The consistent structure is supposed to help pupils create learning habits and routines, which in turn facilitate self-control (cf., e.g., the arguments in Galla and Duckworth, 2015). The rst day has a longer introduction to the topics in math and Danish and includes brief tests in these subjects; the last day includes summaries for the different subjects and the camp in general. The daily structure is clearly communicated to participants, for example, by posting the timetable of the day on the wall. [9]

Each day starts with welcoming pupils and presenting the program of the day. Each day, pupils have two 2-hour blocks of math and Danish, one subject in the morning and one in the afternoon. In Camp+ they have two 45-minute blocks covering non-cognitive skills around lunch (one block before lunch and one after). In variant Camp, the blocks on non-cognitive skills are substituted by additional blocks of math and Danish, so that pupils in Camp have 2.75 hours of math and 2.75 hours of Danish each day. The additional time in Camp is mainly used for practicing exercises. Some time also is devoted to explaining different strategies for solving speci c problems in math and Danish.

After the two weeks of the learning camp, pupils in Camp+ and Camp continue for 8 weeks in a follow-up program that builds on the material from the camp. Each week during this 8-week period, a teacher (typically one of the camp teachers) meets with 5-7 pupils - during regular school hours - for 1.5 hours to repeat, practice, and deepen the understanding of the material from the camp and help the pupils to apply the material in their regular school work. In treatment Camp+, pupils hence face material covering math, Danish, and non-cognitive skills; while in treatment Camp, they only cover math and Danish.

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[7] The regular classroom would in the two weeks the teacher is conducting the camp have a substitute teacher.

[8] The materials (in Danish) can be accessed here: https://emu .dk/grundskole/overgange/uddannelsesparathed?b Æ15-t28 .

[9] We surveyed the schools after the camp and 91% of the schools state that the camp was conducted after the plan to a high degree and 8% in some degree.

1.2.3 Teaching materials

1.2.3.1 Math and Danish

Both camps train pupils in the most important areas in the math and Danish curricula to enable them to catch-up in both subjects and, in the longer run, pass the nal exams in grade 9 and commence on an educational trajectory. Due to its intensive nature, the camp does not cover all topics of the curriculum in the respective subject. In math, geometry is not covered and statistics and probability is only covered cursorily. In Danish, decoding, a focus area of the national tests (see below), is not practiced. [10]

Speci cally, in math, pupils work with decimal numbers, fractions, percentages, mathematical formulas, as well as general problem solving competencies. They revisit the concepts and perform calculations by hand and with a calculator. Some of the exercises are formulated in a similar way as the ones posed in the compulsory school leaving exam in grade 9.

In Danish, pupils work with topics that are tested in the school leaving exams: language comprehension, spelling (exercises targeted at expanding the vocabulary with the aim to foster a better understanding of texts and writing correctly), and text comprehension (including writing of texts and interpretation and discussion of literary texts). Pupils are confronted with different text genres. Next to literary texts, they read, for example, newspaper articles from different sections (such as news, opinion, science).

Pupils take short math and Danish tests at the rst day of camp and at the end of camp. These tests were designed together with the teaching materials. The test results are available to the camp teachers as well as the pupils Danish and math teachers. They give the teachers an indication of the academic strengths and weaknesses of the pupil and inform conversations with the pupil on individual focus areas for the camp.[11]

1.2.3.2 Non-cognitive skills (variant Camp+)

Variant Camp+ includes teaching modules on non-cognitive skills. The teaching materials contain various exercises and six short videos. Like Alan et al. (2019), we aim not only to teach the pupils certain concepts, but also to teach the teachers how to apply them. Accordingly, the teacher manual and the 2-day teacher training course emphasize, for example, how to create a growth mindset, how to build good working habits by practicing routines and providing structure, how to increase the self-control and attention of pupils, and how to give feedback.

[10] The decoding part of the national test, for example, asks pupils to separate three words, that were collapsed (like tabooallegiancetyphoon) into individual words.

[11] Since these tests were not administered to the control groups, and since their purpose explicitly was to inform the teachers about their pupils' progression during the camp, they are not used in the evaluations. Moreover, they also did not cover the 8-week follow-up period.

The teaching material on non-cognitive skills includes several components that other studies successfully implemented before. Self-regulation strategies are the rst main focus area of the teaching material. Pupils learn about goal setting (Locke and Latham, 1990) and set goals for math and Danish. [12] As goal attainment is enhanced if used in combination with implementation intentions (Gollwitzer and Sheeran, 2006) and mental contrasting (Duckworth et al., 2011; Gollwitzer et al., 2011; Duckworth et al., 2013), the teaching material covers these methods and pupils apply them in concrete situations (such as doing homework). Under mental contrasting, pupils imagine the positive aspects of reaching a goal and then re ect on obstacles that could prevent them from reaching the goal. Implementation intentions are if-then plans, such as “if I try to do my homework, but am tempted to glimpse at my smartphone, I give the smartphone to my mother". Along these lines, pupils also learn about the self-control strategy of situation selection and modi cation (Duckworth et al., 2016b,a).

Mindsets (Dweck, 2006) are the second main focus of the teaching material. The aim of these modules is to in uence the mindset of the pupils in different learning situations, and to help them develop a growth mindset. People with a growth mindset believe that ability is not xed and initially given, but that effort can enhance ability. Such a view can help pupils not to attribute failure to a lack of ability and give up, but instead to persevere. Thus, pupils learn how to handle academic challenges and stay motivated to provide effort.

Identifying and evaluating own strengths and dif culties is the third main focus of the teaching material. Based on the VIA (Value in Action) Classi cation of Character Strengths (Seligman et al., 2004), pupils learn about the main categories, which are wisdom and knowledge (e.g., creativity and curiosity), courage (e.g., perseverance and honesty), humanity (e.g., kindness), justice (e.g., teamwork and fairness), temperance (e.g., forgiveness and prudence), and transcendence (e.g., hope). Pupils get inspiration and time to work on how to build their strengths, and they learn how to give feedback to other pupils about strengths in a constructive manner.

In addition, during the welcome session in the morning, pupils get an introduction to the “personal or social skill of the day". The teacher encourages the pupils to re ect upon how they want to work on enhancing this skill. During the day, smaller exercises address the skill. The list of the skills of the day correspond to the skills that are assessed in the ERA (motivation, independence, taking responsibilities, tolerance, reliability, respect, preparedness, working together).

Finally, the material includes smaller exercises on various topics, such as learning zones and healthy habits (sleeping enough and healthy eating). To provide small, productive breaks, pupils also work on some fun concentration tasks (for example, painting a mandala) and fun visual illusion tasks.

[12] The popular writing (for the education context, see, e.g. Conzemius and O'Neill, 2009) has translated the scienti c insights on goal setting into the concept of SMART (Speci c, Measurable, Achievable, Realistic, and Timely) goals that we also used in the teaching materials.

During the last four sessions, the pupils re ect upon the advice that they would give to another pupil based on what they have learned during the camp and they record this in a video.

1.2.4 Randomization procedure

Figure 1.1 provides an overview of the randomization procedure. The randomization was done at the school level and all public schools were eligible for participation in the trials. [13] In rounds 1 and 3, we randomized the schools that signed-up for participation into the treatment variant Camp+ and a control group. In round 2, we randomized between the two treatment variants Camp+ and Camp.[14] Schools were allowed to collaborate with each other in setting up a camp in order to have a suf cient number of participating pupils. Such schools were treated as one unit in the randomization.

We strati ed schools to ensure that similar schools did not all end-up in the same group. First, schools were divided into strata based on how many camp-classes a school had registered for. Second, we strati ed the schools on the basis of their share of NR-pupils in grade 8 in the previous year. After we ranked the schools, we divided them into clusters of six schools. From each cluster, half of the schools were randomly assigned to treatment Camp+ and half to the control group (round 1 and 3) or Camp (round 2). [15] The six school clusters is included in the econometric models as dummies variables because the randomization is performed within those.

As the schools needed to plan in advance – e.g., reserving teachers' time for the camp – it was necessary to communicate the results of the randomization to the schools already in the fall, while the camp ran in the following spring term. The study involves a total of 15,559 pupils at 264 schools. In December 6,094 were assessed as NR-pupils at the ERA and in January 3,600 of the NR-pupils where selected to participate in the camp. In particular, schools in rounds 1 and 3 knew whether they were assigned to the treatment or control group when conducting the ERA and subsequently selecting pupils for the camp. [16] This is less problematic in round 2,

[13] Special needs schools were not eligible for participation because the intervention was design for regular pupils.

[14] This sequencing and design was a consequence of several factors. First, the design of Camp was not ready for round 1, and second, a power issue prevented us from having two treatment arms and a control arm in round 2. Moreover, since there could be differences in selection at sign-up, it would be dif cult to compare two variants tested in different rounds against each other. After round 2 found no remarkable differences between the two camps, we decided to test Camp+ again in round 3 to obtain as much power as possible.

[15] In some clusters, there were fewer than six schools, due to the randomization procedure. Thus, there is not necessarily an equal number of schools in the two groups.

[16] Both treatment and control schools designated which pupils would participate in the camp/function as the corresponding control group. In addition, participating pupils and their parents were asked for consent.

Figure 1.1: Overview of the randomization procedure

because all schools are treated (either with Camp or Camp+). We will return to this issue in section 1.3 when discussing balance tests and in section 1.4.2 when outlining the analysis groups.

1.2.5 Power

Power calculations indicated that 44 schools were needed in the treatment and 44 schools in the control group to detect a minimum effect size of 0.25 of a standard deviation for the main outcome variables. Pooling rounds 1 and 3 in the analysis yielded 83 treatment schools and 77 control schools. In round 2, we compared two different variants, Camp and Camp+, which share around 70 percent of the teaching materials. Ex-ante, the effect size difference was expected to be below 0.25. Thus, even though a total of 104 schools participated in round 2, the comparison of the two variants is under-powered, and the results should be interpreted with caution.

1.2.6 Division of responsibilities

The Ministry of Children and Education funded the intervention. A Steering group governed the entire project. The latter consisted of representatives from the ministry, the team developing the course materials (University College Copenhagen and VIA University College – two major Danish teacher training colleges), the implementation team (a private consultancy rm, Rambøll Management Consulting), and the research team (the authors of this paper).

The research team directed the design of the intervention. It proposed the intervention and evaluation design. The Steering group approved both without con ict. Randomization was conducted by one of the researchers using STATA's built-in runiform() random number generator. The development team developed the teaching materials for the camp and the follow-up program in consultation with the research team. The research team also provided the input for the material covering non-cognitive skills. [17] The implementation team governed the implementation of the intervention, except for the two-day courses for the teachers, which the development team held. Thus, the research team was only partially involved in the development of the intervention and not at all involved in its implementation. Nevertheless, the PI was responsible for the entire project vis-à-vis the Ministry of Children and Education.

1.3 Data

The analysis is based on registry data from Statistics Denmark, the Danish Agency for IT and Learning, as well as surveys that we carried out among all grade 8 pupils of participating schools. Table 1.1 provides an overview of the outcome variables, which we also describe in the following. The outcome variables are standardized [18], with the exception of post-compulsory education choices and the grade 9 ERA (Education Readiness Assessment, see Section 1.2.1).

Short-run academic performance. To measure the short-run academic effects of the camp, we rely on the national tests in math and Danish, which take place in the spring of grade 8 (for a description of the national tests, see Beuchert et al., 2018). As the name suggests, these are nationally administered, standardized and computerized tests. The tests are on average administered 36 days after the camp and hence measure the short-run effects of the camp. In math, the grade 8 national tests were only introduced from 2018 on, i.e., we cannot use this outcome measure for round 1 (which took place in the spring of 2017). With the follow-up program being 56 days the short-run academic test are measured midway though the follow-up period. There is a risk that this leads to conservative estimated effects if one expects the follow-up period to be important for academic performance.

In Danish, the tests measure the academic skills of the pupil within the following three areas: language comprehension, decoding and text comprehension. As mentioned above, decoding is not practiced during the camp, while language and text comprehension are. Thus, we expect effects of the camp on the targeted areas language and text comprehension (primary outcome variables). We also test whether

[17] We thank Kamilla Trille Gumede for her competent assistance in developing this material.

[18] Standardized to mean zero and standard derivation of one using the full population of pupils in Danish public schooling. Only exception is the self-collected survey data, which is standardized using all pupils included in this study.

Table 1.1: Outcome variables

Short-run academic performance		
National test in Danish in grade 8 †	Language comprehension	(primary)
	Text comprehension	(primary)
	Decoding	(secondary)
National test in math in grade 8 †,a	Numbers and algebra	(primary)
	Geometry	(secondary)
	Statistics and probability	(secondary)
Long-run academic performance		
Final exam grades in grade 9 †,b	Danish	(primary)
	Math	(primary)
Post-compulsory education c		(primary)
Non-cognitive skills		
Social and personal skills evaluation in grade 9 (ERA) d		
Psychological scales administered in the post-survey †		

Notes. † Standardized variable. a Only from 2018, i.e. not available for round 1.
b Not available for round 3, because nal exams were not held due to the Covid-19 lockdown.
c Dummy =1 if no further education 2.5 years after the camp and =0 otherwise. Not yet available for round 3.
d Evaluated ready/not ready for a particular post-compulsory education.

there is a spillover effect on decoding (secondary outcome variable). Such a spillover effect can occur, for example, because the pupil, as a result of the camp, gains better self-regulation skills or a growth mindset, because she/he is more motivated, or because the overall Danish skills that she/he acquired during the camp also help with decoding.

In math, the pro le areas are numbers and algebra, geometry, and statistics and probability. The main focus of the camp is on numbers and algebra. Thus, we expect a positive treatment effect in this area (primary outcome variable). In addition, as for decoding in Danish, we test whether there are spillover effects on the non-targeted areas geometry and statistics and probability (secondary outcome variables).

Long-run academic performance. To measure the long-run effects of the camp, we use the nal exam grades in math and Danish in grade 9. These grades are given slightly more than a year after the camp. The tests are either computerized or an external censor takes part in these tests. For round 3, nal exams were not carried out because of the Covid-19 lockdown, and hence these measures are not available.

In Danish, the grades measure the competencies of the pupil in reading, spelling, writing, and speaking. In math, they measure how well the pupil can solve problems without aids and how well she/he can apply formulas with aids (such as computer programs, or calculators). The exams in Danish have both a written and an oral part. In math, all pupils take written tests, and a random draw determines whether

a pupil is orally examined in math or instead in another science subject. For math and Danish, we use the respective overall grade as the main outcome variable, but we also report separately results for the written and oral parts in the appendix.

Moreover, we observe whether pupils enroll in any post-compulsory education (e.g., vocational training, high school, taking the voluntary 10th grade) or take no further education. For round 3, this measure is not yet available. For round 1, we use the status 2.5 years after the camp as an outcome variable.

Non-cognitive skills. We use register data and self-administered surveys to evaluate the effects of the camp on non-cognitive skills. Each of the outcome variables described in the following has certain disadvantages. For this reason, we do not classify outcomes variables as primary or secondary outcome variables. Instead, rather than focusing on the statistical signi cance of any single measure, we will only conclude that the camp has an effect on non-cognitive skills if different measures point in the same direction.

From the register data, we use the evaluation of the social and personal skills of the pupil in the ERA in grade 9. A potential disadvantage with this measure is that the teachers who conduct the ERA know whether a pupil participated in the camp or not which could potentially in uence the evaluation.

From self-administered pre- and post-surveys, we draw on several validated psychological scales (for a detailed description of the included items, see section A.1.1 in the appendix): the 8-item Grit Scale (Duckworth and Quinn, 2009), the Domain-Speci c Impulsivity Scale for Children (Tsukayama et al., 2013), the Core Self-Evaluations Scale (Judge et al., 2003), and the Strengths and Dif culties Questionnaire for adolescents (Goodman et al., 1998). We measure beliefs about the malleability of abilities (mindset) with 4 items based on Dweck (2006). Further, we include the 1-item risk aversion question (Dohmen et al., 2011), the 1-item patience question (Vischer et al., 2013) and two questions on time-preferences from the GSOEP survey. [19] A disadvantage of the survey-based measures is the fact that they are designed by the research team in accordance with the teaching materials. Thus, pupils might give the "desired" answer. Further, the camp might make pupils (and teachers) more aware of certain skills rather than changing them – a difference the survey measures cannot capture.

The schools know when they are taking the pre-survey whether they are treatment or control schools. This might explain that we observe 12% more attrition from the control schools in the pre-survey. This number increases to 19% in the post-survey, which potentially affect our estimates.

[19] Rounds 2 and 3 included four additional, domain-speci c risk questions about taking desirable risks (raising your hand or volunteering to present in class) and risky behavior (drinking, smoking, and illegal actions).

Control variables and balance tests. The register data allow us to include a range of school-related control variables, such as previous test scores, previous school absence, and previous responses in the national school well-being survey, as well as background characteristics of the pupils such as gender, ethnicity, family situation and variables related to the socioeconomic status of the parents. The balance test tables provide the complete list of control variables (cf. tables A.1-A.3 in the appendix). Section 1.4.1 describes how we use LASSO to exogenously select the covariates for each regression.

The balance tables demonstrate that in all three rounds there is balance between treatment and control over a range of observables when we look at all pupils in grade 8, as expected from the randomization process and selection of pupils into the camp (cf. section 1.2.4). When testing 130 covariates in a balances table, we would expect to nd 7 signi cant variables at a 5% level simply due to chance. In table A.1 11 covariates are signi cant, which is close to our expectation. However, the important pre-math tests are signi cant, which would effect our treatment estimates and must be addressed in the econometric models. It appears that schools in Camp+ more carefully classi ed pupils in the ERA than schools in the control group: In rounds 1 and 3, we observe from balance tests that the NR-pupils, as well as the pupils selected for the camp are not balanced between Camp+ and control. Speci cally, treatment schools select fewer, more poorly academic performing pupils with better personal competencies for the camp and to be NR in the ERA. In particular, we observe that Camp+ NR-pupils are on average approximately 0 .1 standard derivation below the control NR-pupils in reading and math pre-tests and less likely to be assessed academic ready at the grade 8 ERA. For pupils selected for camps the academic differences increases by additional 0.05 standard derivations. In general, the academic differences seems to be driven by pupils being dyslexics and not parental characteristics. Thus, we need to address this selection issue in the econometric models that we use. We turn to this issue in the next section.

1.4 Analysis

1.4.1 Estimation strategy

To test the null hypothesis that the camp has no effect on an outcome variable, we estimate the following equation:

$$y_i \text{ Æ®Å} \pm D_i \text{ Å°} S_i \text{ Å}^{-} X_i \text{ Å}^{2}{}_i , \tag{1.1}$$

where subscript i refers to each pupil. Thus, y_i is the outcome variable and $^{2}{}_i$ is the error term. The coef cient of interest $\pm$ measures the effect of the treatment captured by dummy D_i, as explained below. S_i is a indicator variable for each pupil's school randomization-stratum, and X_i is a vector of covariates. Standard errors are clustered

at the school level. We apply the Double-Lasso (DL) variable selection method suggested by Belloni et al. (2014) to select covariates. We include covariates to account for the imbalance showed previously and to improve precision of the estimated intervention effects. The DL method is calibrated to not over-select spurious covariates, and Urminsky et al. (2016) concludes that this method is particularly useful for imperfect randomized experiments (and for under-powered analyses in a perfect randomized experiment, by increasing statistical power). It comprises three steps:

1. Predict the dependent variable using a LASSO regression with the complete list of control variables and save the variables with non-zero coef cients.

2. Predict the treatment indicator using a LASSO regression with the complete list of control variables and save the variables with non-zero coef cients. If the treatment is perfectly randomized, then no covariates should be selected in this step.

3. Combine the covariates selected in the rst two steps and include them in equation (1).

In the LASSO regressions, we apply 10-fold cross-validation to estimate the optimal shrinkage parameter.

1.4.2 Analysis groups

To address the aforementioned imbalances, we report results for different sets of pupils as explained in the following.

Camp+ vs. Control. If we look at all grade 8 pupils, by design randomization should be perfect, which is con rmed by the aforementioned balance tests for rounds 1 and 3 where schools were randomized into Camp+ or control. In our main speci- cation, we therefore include all pupils who were in a treatment or control school in grade 8 (henceforth ITT group for Intention To Treat). We set D_i equal to one if in grade 8 a pupil was in a school that was selected for treatment and zero if she/he was in a control school. Thus, the estimate $\hat{\pm}$ corresponds to the ITT effect. That is, it measures the effect of the school being assigned to the treatment, but not necessarily of being treated.

The intention to treat estimate of the main speci cation likely constitutes an extreme lower bound on the true effect (see also section 1.5.5 for further caveats). We therefore also report two additional estimates relying on different groups of pupils. While these estimates help gauge the true effect size, they have to be interpreted with caution because schools carried out the ERA and selected pupils for the camp after learning their treatment status, as explained in section 1.2.4. This leads to some imbalances, as documented in table A.2.

The rst additional estimate is the intention to treat estimate based only on the sample of pupils evaluated NR in grade 8, to which we refer as ITT-NR (Intention To Treat on the NR-pupils) in the following. The ITT-NR group is a subset of the ITT group.

With the second additional estimate, we estimate the effect of the camp on the pupils who actually participated in the camp with the local average treatment effect (LATE, Imbens and Angrist, 1994) for the ITT-NR group. An underlying assumption of the LATE estimate is that there are no spillover effects on non-treated pupils. Thus, if the schools use the teaching material beyond the camp, we risk having the LATE effect overestimating the effect of the camp. Thus, the LATE estimate likely constitutes an upper bound for the true effect.

Camp+ vs. Camp. In round 2, both groups receive an intervention and by design randomization should be perfect. Indeed, we observe balance between the two groups (cf. table A.3 in the appendix). We therefore restrict the sample to only those pupils who actually were selected for the camp in grade 8. The estimate $\hat{\pm}$ thus corresponds to the average treatment effect on the treated (ATT) of Camp+ versus Camp. The ATT group is a subset of the ITT-NR group.

1.4.3 Short-run effects on academic outcomes

We rst test for the short-run effects of Camp+ vs. control using the national tests in math and Danish conducted in grade 8. We report the results in tables 1.2 and 1.3. Our preferred speci cations include covariates, since we observe some imbalances despite the randomization, especially for the ITT-NR group (cf. section 1.4.2). But we also report speci cations without covariates.

National test in Danish. For all estimates (ITT, ITT-NR, and LATE) we nd positive and signi cant effects of Camp+ on one of the primary outcomes, text comprehension. The effect sizes range from 0.07-0.18, with the LATE estimate being more than twice as large as the ITT estimate. Yet, contrary to our expectations we do not nd a positive effect on the other primary outcome language comprehension as our main estimate, the ITT estimate, shows an insigni cant negative effect. Given these inconclusive results and given that we perform multiple tests (cf. section 1.5), some caution should be taken in interpreting the positive effect on text comprehension.

Further, we do not nd an effect on the secondary outcome variable decoding. If we aggregate the three outcomes (for comparison with longer term outcomes from grade 9 exams), there is no signi cant effect on the overall performance.

National test in math. This test was introduced in 2018 and is therefore not available for round 1. With our sample restricted to round 3, for all estimates, we nd

Table 1.2: Short-run effects on the national grade 8 test in Danish, Camp+ vs. control

	Language Comprehension		Decoding		Text Comprehension		Overall	
ITT	-0.075	-0.033	-0.016	0.017	0.047	0.065***	-0.018	0.017
	(0.052)	(0.043)	(0.037)	(0.024)	(0.035)	(0.021)	(0.041)	(0.028)
Mean outcome, Control	.024	.024	.000	.000	-.057	-.057	-.013	-.013
R-squared	.015	.186	.009	.510	.011	.520	.013	.549
Observations	8,953	8,953	8,953	8,953	8,953	8,953	8,953	8,953
ITT-NR	-0.113**	-0.046	-0.059	0.014	0.055	0.090***	-0.048	0.024
	(0.052)	(0.045)	(0.038)	(0.028)	(0.035)	(0.029)	(0.042)	(0.033)
LATE	-0.231**	-0.094	-0.121	0.028	0.113	0.184***	-0.098	0.050
	(0.107)	(0.092)	(0.078)	(0.058)	(0.073)	(0.059)	(0.087)	(0.067)
Mean outcome, Control	-.233	-.233	-.459	-.459	-.574	-.574	-.519	-.519
R-squared	.024	.213	.009	.431	.013	.342	.014	.442
Observations	3,418	3,418	3,418	3,418	3,418	3,418	3,418	3,418
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

evidence for positive effects of Camp+ on the primary outcome numbers and algebra. Effect sizes range from 0.07-0.20, with the LATE estimate being almost three times as large as the ITT estimate.

The main estimate (ITT) does not show signi cant effects of the treatment on the secondary outcome variables, i.e., areas in math that were not targeted in the camp. Yet, for the ITT-NR and LATE estimates, we nd some suggestive evidence for positive effects on geometry (effect sizes 0.09-0.19) and statistics and probability (effect sizes 0.12-0.24). If we aggregate the three areas, there is a positive effect on the overall performance (effect sizes 0.06-0.20). Comparing these effect sizes to the mean outcome in the control group, it is evident that the intervention closes between 10-30% of the gap in math ability that these pupils have accumulated during their time in school.

Camp+ vs. Camp. We do not nd any signi cant differences between the two variants of the camp. We report the results of Camp+ vs. Camp on the short-run academic outcomes in tables A.4 and A.5. We will return to these results when we discuss the impact of Camp+ on non-cognitive skills in section 1.4.5.

Table 1.3: Short-run effects on the national grade 8 test in math, Camp+ vs. control

	Numbers and Algebra		Geometry		Statistics and Probability		Overall	
ITT	0.025	0.068**	0.000	0.043	0.023	0.063	0.017	0.059*
	(0.060)	(0.032)	(0.063)	(0.035)	(0.065)	(0.039)	(0.065)	(0.034)
Mean outcome, Control	-.064	-.064	-.042	-.042	-.033	-.033	-.050	-.050
R-squared	.018	.640	.020	.607	.019	.601	.021	.706
Observations	4,283	4,283	4,283	4,283	4,283	4,283	4,283	4,283
ITT-NR	0.004	0.094**	-0.001	0.091*	0.017	0.117*	0.007	0.100**
	(0.051)	(0.047)	(0.051)	(0.046)	(0.069)	(0.062)	(0.057)	(0.050)
LATE	0.008	0.192**	-0.002	0.185*	0.035	0.240*	0.015	0.204**
	(0.104)	(0.096)	(0.103)	(0.093)	(0.141)	(0.128)	(0.116)	(0.102)
Mean outcome, Control	-.645	-.645	-.625	-.625	-.619	-.619	-.678	-.678
R-squared	.010	.452	.011	.452	.012	.460	.011	.550
Observations	1,581	1,581	1,581	1,581	1,581	1,581	1,581	1,581
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes:***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome for math only exists for round 3. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

1.4.4 Long-run effects on academic outcomes

To measure the long-run effects of Camp+ vs. control, we consider the nal exam grades in grade 9 and whether pupils enrolled in any post-compulsory education 2.5 years after the camp, reported in table 1.4. Note that these outcomes are not available for round 3 yet (cf. table 1.1).

Final exams in Danish and math. We do not nd a signi cant effect of Camp+ on the nal exams in Danish. We nd suggestive evidence that the short-run effect of Camp+ in math translates into a positive long-run effect on the performance in the math nal tests in grade 9. Effect sizes for the ITT-NR and LATE range from 0.06-0.18, but neither are they signi cant at the 5% level, nor does the ITT estimate show a signi cant effect. Additional analyses suggest that the effect may stem from the better performance in the written part of the math exams (cf. table A.6 in the appendix).[20]

Post-compulsory education. There is some suggestive evidence that Camp+ has a positive effect on pupils being enrolled in education 2.5 years after the camp. While our main estimate, the ITT, is not signi cant and also very close to zero, according to

[20] All pupils take the written tests. Oral examination occurs only in math or another science subject, depending on a random draw.

Table 1.4: Long-run effects of Camp+ vs. control on grade 9 exams and on the fraction not enrolled in education 2.5 years after the camp

	Danish score		Math score		No education	
ITT	-0.055	-0.020	0.023	0.062	0.002	-0.004
	(0.053)	(0.034)	(0.061)	(0.048)	(0.012)	(0.009)
Mean outcome, Control	-.055	-.055	-.101	-.101	.095	.095
R-squared	.014	.684	.016	.660	.005	.152
Observations	4,761	4,761	4,744	4,744	4,820	4,820
ITT-NR	-0.042	-0.005	0.026	0.088*	-0.027	-0.037**
	(0.052)	(0.041)	(0.058)	(0.050)	(0.021)	(0.018)
LATE	-0.086	-0.011	0.053	0.180*	-0.055	-0.076**
	(0.106)	(0.085)	(0.118)	(0.103)	(0.043)	(0.038)
Mean outcome, Control	-.754	-.754	-.779	-.779	.193	.193
R-squared	.017	.520	.013	.477	.012	.163
Observations	1,880	1,880	1,864	1,864	1,931	1,931
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome only exists for round 1. Educational enrollment is measured September 31th three years after the camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

the ITT-NR estimate, NR-pupils in a school that offered the camp are more likely to be enrolled in some post-compulsory education than NR-pupils in the control group. The ITT-NR and LATE estimates show that uptake of post-compulsory education increases by 4-8 percentage points, which should be compared with the baseline of 19 percent of pupils not being enrolled in post-compulsory education in the control group. Additional analyses suggest that the effect may stem from treated pupils being more likely to take-up vocational training (cf. table A.7 in the appendix).

Camp+ vs. Camp. We report the long-run effects of Camp+ vs. Camp in table A.8 in the appendix. As for the short-run academic outcomes, we nd no signi cant differences between the two camp variants in the long-run exam grades or on the fraction enrolled in post-compulsory education.

1.4.5 Effects on non-cognitive skills

Overall, we nd no evidence of an effect of Camp+ vs. control on non-cognitive skills as measured by the ERA and the psychological scales in the post-survey (see tables

A.9a, A.9b, and A.10 in the appendix). [21]

Further, comparing non-cognitive skills between Camp+ (which included around 16 hours of teaching on non-cognitive skills and revisiting them during the 8-week follow-up program) and Camp (which does not teach about non-cognitive skills), we nd an inconsistent picture. Given that we did not classify any outcome measure as primary or secondary, we do not draw a conclusion in one or the other direction from this inconsistent picture, but only summarize the effects in the next paragraph.

There is a tendency that pupils who participated in Camp+ are more likely to be assessed NR-pupils compared to Camp, which shows speci cally in a worse evaluation of the personal skills (see tables A.14 and A.15 in the appendix). [22] Further, we do not nd any difference in most of the non-cognitive skills targeted in Camp+ (self-control, grit, and mindsets). Both the negative and the null result are against what we expected ex-ante. Yet, the 1-item question by Vischer et al. (2013) indicates an increase in patience relative to Camp (see table A.17a and A.17b in the appendix). Camp+ also is associated with pupils being more likely to take risks – both positive risks (like saying something in class) and negative risks (like drinking alcohol or smoking).

Given the inconsistent picture, the comparison of academic skills can provide additional indication of whether Camp+ enhanced non-cognitive skills. As discussed in the previous section, we nd no difference in academic skills between Camp versus Camp+. On the one hand, Camp devotes more time to training in math and Danish. But on the other hand, if Camp+ had a strong positive impact on non-cognitive skills and if these non-cognitive skills impacted academic performance, then pupils who participated in Camp+ rather than Camp should perform better in school – at least in academic areas that were not directly targeted in the camp. Thus, taking all these results together suggests the interpretation that the time spent on non-cognitive skills in Camp+ does not meaningfully affect non-cognitive skills.

1.4.6 Exploratory analysis: subgroups

While our study is not strictly powered to encompass subgroup analyses, it might nevertheless be enlightening to consider some exploratory results. Of course, given their exploratory nature, they should be interpreted with some caution. We focus here on the effects of Camp+ vs. control in the national tests. The results are summarized in tables A.18-A.25 in the appendix and include also comparisons of Camp+ vs. Camp.

[21] In the appendix, in tables A.11 and A.12, we further split-up the results of the ERA into the evaluation of the personal and social skills of pupil. No effects are found either. The ERA evaluates whether pupils are assessed to be ready for their preferred post-secondary educations. Table A.13 in the appendix rules out that the treatment affects what educations pupils prefer.

[22] The preferred education of the pupils is not affected by the treatments, as shown in table A.16 in the appendix.

Gender. As many education interventions have a stronger effect on girls than on boys, a natural question is whether the effects of the camp vary by gender. When we look at the national test in Danish, we observe for boys a positive effect on text comprehension (effect sizes 0.08-0.17). For girls, we do not see a corresponding ITT effect.[23] In contrast, for the national test in math, we observe for girls a positive effect on probability and statistics (effect sizes 0.09-0.35), as well as a positive ITT effect on numbers and algebra (effect size 0.09). These results suggest that the camp speci cally pushes pupils in areas where – according to gender-stereotypical beliefs – they would expect not to do well.

Socioeconomic background. We use the education level of the mother as an indicator for the socioeconomic background of the pupil. Speci cally, we classify pupils as having a low-socioeconomic background if their mothers have 12 years of education or less (i.e., they have at most completed high school). In the national tests in Danish, in the area of text comprehension we observe a positive signi cant treatment impact on pupils with low-socioeconomic background (effect sizes range from 0.14-0.28). Yet, in the national tests in math, we do not observe a positive ITT effect in the targeted area. [24]

Academic low performers. When we look at the worst performing pupils according to the previous national test in grade 6 (lowest 10% of all grade 8 pupils), we observe positive signi cant effects on text comprehension (effect sizes 0.09-0.20). Further, the camp has positive effects in math not only on the targeted area of numbers and algebra (effect sizes 0.12-0.25), but also on the non-targeted area geometry (effect sizes 0.14-0.28).[25] The effect sizes suggest that the treatment has a larger impact on the worst performing pupils compared to all pupils.

1.5 Discussion

1.5.1 Multiple hypotheses

We have reported a fairly large number of results for each treatment variant, even when limiting ourselves to the primary outcome variables. Thus, some of them might be signi cant due to statistical chance. We have tried to accommodate this problem by highlighting primarily results where all estimates point in the same direction. But

[23] There is some evidence of an effect from the ITT-NR and LATE estimates (effect sizes 0.10-0.21).

[24] There are some positive results for the ITT-NR and LATE estimates in the non-targeted area.

[25] There is even suggestive evidence for positive effects on the non-targeted area probability and statistics (0.15-0.31).

of course, we should further caution against interpreting results too strictly, especially when looking at subgroups and at secondary outcome variables.

1.5.2 Economic signi cance

There are only few studies relying on randomized experiments or quasi-experimental methods for assessing the impact of intensive learning camps and they provide a mixed picture (see the literature review in section 1.1). In addition, education studies with over 2,000 participants tend to have far smaller effect sizes than studies with fewer participants and RCTs tend to have much smaller effect sizes than quasi-experimental designs (Cheung and Slavin, 2016; Kraft, 2020).

Hence, it is interesting to place our results in a wider context. The short-run effect sizes that we nd for overall performance in the math standardized test range from 0.06 to 0.2. Comparing them to the empirical distributions of effect sizes from randomized control trials of education interventions with standardized achievement outcomes in Kraft (2020), they lie roughly in the range of 50th to 80th percentile for interventions targeting math. For example, our results are comparable to the effect on math scores of increasing teacher quality in a term by one standard deviation (here effect sizes range from 0.11 to 0.13; cf. Aaronson et al., 2007; Rivkin et al., 2005).

We nd weaker evidence of short-run effects in Danish, as only one out of the two targeted areas in the standardized test (namely, text comprehension) is positively affected. The effect sizes range from 0.07 to 0.18, which is comparable to the effect sizes found in other interventions in Danish primary schools conducted in earlier grades, such as mother tongue teaching (Andersen and Knoth Humlum, 2021) and the `two-teacher in the classroom' program (Andersen et al., 2020). The former estimates, in the Danish national tests, an effect size of 0.15 for language comprehension and 0.17 for decoding. The latter study reports effects sizes on grades in Danish of 0.06 to 0.10.

When it comes to relevance for practice, effect sizes should be seen in relation to the costs and scalability of an intervention, which are key factors for policy makers (Harris, 2009; Cheung and Slavin, 2016; Kraft, 2020). The main costs of the camps stem from the one-time development of the teaching materials. Running the camp itself costs approximately DKK 5000 ($750) per pupil (mainly to pay for additional work hours of teaching staff). Camps are easy to set up because they take place at the school of the pupils during regular teaching hours, are of short duration (two weeks + 8 weeks of follow-up), and they draw on existing teaching staff who require little additional training due the detailed teaching materials and guides. In terms of cost-effectiveness ratio, our intervention thus can be classi ed as easy to scale with a medium effect size and moderate cost (Kraft, 2020).

Further, in contrast to, for example, summer camps, the camp is not in addition to the regular teaching, but substitutes for it. That is, while the pupils receive more intensive teaching in math and Danish than usual, they miss out on their regular

classes. Any effect thus indicates that the camp is more effective than regular classes.

Another important consideration is political feasibility of an intervention. Running camps during regular school hours has clear advantages over programs after school or during school vacation. For example, in several countries it was discussed whether learning losses due to Covid-19 lockdowns and restrictions in schools could be addressed by lengthening school days or shortening the school vacation. This met strong opposition from teachers and parents (e.g., Goldstein and Taylor, 2021; YouGov, 2021; SWR, 2021).

1.5.3 Results on social and personal skills

Contrary to our a priori expectations, Camp+ has no effect on the non-cognitive skills of the pupils. These results are in contrast to the closely related studies by Alan and Ertac (2018) and Alan et al. (2019), who nd large and persistent effects of their intervention on non-cognitive and academic skills.

There might be several reasons behind these different ndings. One reason could lie in the composition of pupils and/or schools. Our pupils are older than the ones in the studies of Alan and Ertac (2018) and Alan et al. (2019), and it may be more dif cult to change the non-cognitive skills of older pupils (cf. Kautz et al., 2014).

Further, our teaching material on non-cognitive skills do not focus on one particular non-cognitive skill (like grit or mindset), but cover a range of non-cognitive skills. The rationale for our approach stems from the evaluations that schools carry out in grade 8 (ERA), where both personal and social skills are evaluated in addition to academic skills. However, we do have some focus topics, such as self-regulation strategies and mindsets. Yet, the total hours on a particular focus topic (3-5 hours) is still less than the total number of hours spent on the speci c topics in the intervention studied by Alan and Ertac (2018) and Alan et al. (2019). Their respective interventions focused on one speci c topic at a time and used around 2 hours per week for 8-12 weeks. Thus, one reason for us not nding large effects on academic skills and essentially zero effects on non-cognitive skills could be that focusing on many topics is less effective than focusing on one topic.

Lastly, in Denmark, a lot of interventions take place in school (see subsection 1.5.2 and 1.5.5.2 for some examples), and there is generally a culture of implementing new teaching and learning concepts. In contrast, Alan et al. (2019) state that their program took place in under-resourced public schools in Turkey which are mainly attended by pupils from low-socioeconomic backgrounds. Denmark also scores higher in the PISA 2018 tests than Turkey (Schleicher, 2018). Thus, the small effects on academic skills and null results on non-cognitive skills that we nd, in comparison to the large effects of Alan and Ertac (2018) and Alan et al. (2019), might be due to the fact that they stem from a setting with lower educational resources and fewer alternatives to the intervention than in our setting.

Thus, overall, caution should be taken in generalizing from our results to con-

clusions such as “non-cognitive skills cannot be changed in older children and thus the main focus should be on training academic skills". Our results are speci c to, for example, the used teaching materials or to country speci c characteristics. Future research may examine in more detail the role of age when trying to impact non-cognitive skills.

1.5.4 Organization of the camp

Our camp takes place during regular teaching hours in the school of the pupils. This form has some advantages over traditional summer camps. First, a school-based camp may remove potential barriers to participation. No special search effort or application is needed in order to participate in the camp. Further, the camp does not take away leisure time as the pupil has to attend school otherwise. Finally, the pupil knows the teachers, other participants and the location – decreasing potential psychological barriers. Second, a school-based camp permits to follow-up on the outcomes of the learning camp during the regular teaching. Either a teacher of the pupil or a close colleague (i.e., a teacher teaching the same cohort of pupils) is involved in the teaching of the camp. Colleagues within a school can easily exchange information and discuss how to follow-up on the outcomes of the camp.

Yet, as the camp takes places during regular teaching hours, pupils miss-out on the regular teaching. This might be less of a problem for Danish and math, where pupils receive more intensive training than normal. Yet, it might be a problem for other subjects (like natural sciences or foreign languages) that are not targeted in the camp. That is, while the camp, on the one hand, gives pupils something (more math and Danish lessons) it also takes away something from them (lessons in other subjects).

1.5.5 Caveats

1.5.5.1 Non-adherence in the control group

Of the 36 control schools in round 3, 12 had participated in the camp either in round 1 or 2. This creates scope for control schools in round 3 to use the teaching materials from either Camp+ or Camp. In addition, some schools indicated beforehand that they planned to conduct a camp themselves should they not be selected as a treatment school. Thus, in round 3 the management of all schools was asked to sign a legally binding statement declaring whether or not they conducted in grade 8 a camp using our teaching materials. Four control schools indicated that they did use the materials from either Camp+ or Camp. We take this into account in the calculation of the LATE estimate. Yet, for the ITT estimates, these schools will continue to be included in the analyses as control schools, supporting the point that the ITT is a conservative estimate of the true effect of the camp.

1.5.5.2 Treatment as usual

Control schools may provide other educational support for NR-pupils. In particular, if a pupil is assessed NR, the school is required by law to offer additional support. This could, for example, consist of enrolling the pupil in a camp that is offered by the relevant municipality (see the next paragraph), providing individual supervision, or offering activities outside the school. For this reason, we have to assume that all NR-pupils receive some kind of treatment, which probably also affects outcomes. In the statements from school management collected in round 3, we further asked whether the school implemented some type of camp in grade 8 (not using our teaching material). About half of the control schools indicated that they completed some type of camp themselves.

Further, there exist many opportunities for attending intensive learning camps in Denmark. Examples are a summer camp for boys in grade 8 (“Drengeakademiet" supported by Løkkefonden, cf. Andersen and Nissen, 2014; Andersen, 2015) or the camp “Plan T", which targets children in a speci c municipality who are dyslexic. In addition, many municipalities offer similar types of intensive learning camps for grade 8 and 9 pupils with academic or social problems (e.g., the 1-week “MOVE" camp in the city of Aarhus).

We treat participation in all such intensive learning camps and initiatives as “treatment as usual". Yet, the presence of such camps and related initiatives implies that we are intervening at a high level of existing support for the NR-pupils. The small effects hence may be due to the fact that the “treatment as usual" has almost as much effect as the camp. Indeed, this might be an explanation for the effect sizes being larger in other countries.

1.6 Conclusion

Using a large randomized trial, we evaluate an intensive learning camp for grade 8 pupils that are assessed `not ready' for further education. The main variant of the camp does not only train pupils in math and Danish, but also aims to strengthen their non-cognitive skills. In the short-run, we nd positive effects on targeted-areas in math and some suggestive evidence for positive effects in Danish. In the long-run, there is some weak indication of the camp having a positive impact on math performance in the nal exams in grade 9 and on being enrolled in post-compulsory education 2.5 years after the camp. We do not nd any evidence that the camp impacts non-cognitive skills. Finally, when comparing the effects of the two types of camps it is evident that teaching non-cognitive skills is no worse than teaching Danish or math.

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A.1 Appendix

A.1.1 Survey measures for non-cognitive skills

In our analysis, we draw on the following measures collected in the post-camp survey. We provide the English translations of the questions that were posed in Danish.

- Grit: Grit Scale (Duckworth and Quinn, 2009)

  1 = Strongly agree, 2 = Agree, 3 = Neither agree nor disagree, 4 = Disagree, 5 = Strongly disagree

  1. New ideas and projects sometimes distract me from previous ones.
  2. Setbacks don't discourage me.(R)
  3. I have been obsessed with a certain idea or project for a short time but later lost interest.
  4. I am a hard worker.(R)
  5. I often set a goal but later choose to pursue a different one.
  6. I have dif culty maintaining my focus on projects that take more than a few months to complete.
  7. I nish whatever I begin.(R)
  8. I am diligent.

- Self-control: Domain-Speci c Impulsivity Scale for Children (Tsukayama et al., 2013)

  1 = At least once a day; 2 = Approx. once a week, 3 = Approx. 2-3 times a month, 4 = Approx. once a month, 5 = Almost never

  1. I forgot something I needed for school.
  2. I interrupted other pupils while they were talking.
  3. I said something rude.
  4. I couldn't nd something because my table, closet, or bedroom was messy.
  5. I got really mad at home or at school.
  6. I couldn't remember what my teacher had asked me to do.
  7. I thought of something else while I should have listened.
  8. I talked back to my teacher or parent because I was angry or upset.

- Self-concept: Core Self-Evaluations Scale (Judge et al., 2003)

1 = Strongly agree, 2 = Agree, 3 = Neither agree nor disagree, 4 = Disagree, 5 = Strongly disagree

1. I am con dent I get the success I deserve in life.(R)
2. Sometimes I feel depressed.
3. When I try, I generally succeed.(R)
4. Sometimes when I fail I feel worthless.
5. I complete tasks successfully.(R)
6. Sometimes, I do not feel in control of my work.
7. Overall, I am satis ed with myself.(R)
8. I am lled with doubts about my competence.
9. I determine what will happen in my life.(R)
10. I do not feel in control of my success in school.
11. I am capable of coping with most of my problems.(R)
12. There are times when things look pretty bleak and hopeless to me.

- SDQ: Strengths and Dif culties Questionnaire for adolescents (Goodman et al., 1998)

1=Not true, 2=Somewhat true, 3=Certainly true

1. I am restless, I cannot stay still for long.
2. I get a lot of headaches, stomach-aches or sickness.
3. I get very angry and often lose my temper.
4. I would rather be alone than with people of my age.
5. I usually do as I am told.(R)
6. I worry a lot.
7. I am constantly dgeting or squirming.
8. I have one good friend or more.(R)
9. I ght a lot. I can make other people do what I want.
10. I am often unhappy, depressed or tearful.
11. Other people my age generally like me.(R)
12. I am easily distracted, I nd it dif cult to concentrate.
13. I am nervous in new situations. I easily lose con dence.
14. I am often accused of lying or cheating.

15. Other children or young people pick on me or bully me.
16. I think before I do things.(R)
17. I take things that are not mine from home, school or elsewhere.
18. I get along better with adults than with people my own age.
19. I have many fears, I am easily scared.
20. I nish the work I'm doing. My attention is good.(R)

    Prosocial scale (not included in the Total dif culties score):

21. I try to be nice to other people. I care about their feelings.
22. I usually share with others, for example CD's, games, food.
23. I am helpful if someone is hurt, upset or feeling ill.
24. I am kind to younger children.
25. I often offer to help others (parents, teachers, children).

- Mindset: Malleability of ability to learn (based on Dweck, 2006)

  1 = Strongly agree, 2 = Agree, 3 = Neither agree nor disagree, 4 = Disagree, 5 = Strongly disagree

  1. I can always improve my ability to learn no matter how old I am.(R)
  2. My ability to learn will never change.
  3. I am above the age, where it is possible to signi cantly improve my ability to learn.
  4. After a certain time during my childhood, I will no longer be able to improve my ability to learn.

- Patience 1: 2 items from the GSOEP

  1 = Strongly agree, 2 = Agree, 3 = Neither agree nor disagree, 4 = Disagree, 5 = Strongly disagree

  1. I do without today to be able to afford more tomorrow.(R)
  2. I prefer to have fun today and don't think about tomorrow.

- Patience 2: 1-item patience question (Vischer et al., 2013)

  Likert scale: 1= very impatient, 10= very patient

  Are you generally an impatient person, or someone who always shows great patience?

- General risk preferences (Dohmen et al., 2011)

Likert scale: 1=not at all willing to take risks, 10=very willing to take risks

Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?

- Positive risk preferences

  1 = Several times a day, 2 = Once a day, 3 = Several times a week, 4 = Once a week, 5 = Once a month, 6 = Never

  1. How often have you raised your hand in class during the last month?(R)
  2. How often have you volunteered to present something in class during the last month?(R)

- Negative risk preferences

  1 = Several times a day, 2 = Once a day, 3 = Several times a week, 4 = Once a week, 5 = Once a month, 6 = Never

  1. How often have you been drinking alcohol and / or smoking cigarettes during the last month?
  2. How often have you done something illegal (for example, jaywalked, stole something) during the last month?

A.2 Additional tables

Table A.1: Balance tests - all pupils in grade 8 (ITT)

	Round 1 and 3				Round 2			
	Camp+	Control	Diff.		Camp+	Camp	Diff.	
Language Comprehension								
- Grade 6	-0.004	0.028	-0.035		0.004	0.025	-0.028	
- Grade 4	-0.055	0.012	-0.063		0.038	0.040	-0.004	
- Grade 2	-0.001	-0.003	0.014		-0.014	0.007	-0.019	
Decoding								
- Grade 6	-0.015	0.032	-0.057		0.020	0.001	0.014	
- Grade 4	-0.026	0.018	-0.048		0.002	-0.025	0.026	
- Grade 2	-0.009	0.018	-0.018		-0.032	-0.002	-0.022	
Text Comprehension								
- Grade 6	-0.034	-0.011	-0.029		0.009	0.001	0.009	
- Grade 4	-0.046	-0.007	-0.039		-0.006	-0.015	0.013	
- Grade 2	-0.012	0.015	-0.021		-0.039	-0.003	-0.029	
Overall Reading								
- Grade 6	-0.020	0.019	-0.047		0.013	0.010	-0.002	
- Grade 4	-0.048	0.009	-0.057		0.013	0.000	0.013	
- Grade 2	-0.008	0.011	-0.009		-0.032	0.001	-0.027	
Numbers and Algebra								
- Grade 6	-0.077	0.033	-0.114	***	0.033	0.017	0.019	
- Grade 3	0.004	0.034	-0.027		0.022	-0.004	0.025	
Geometry								
- Grade 6	-0.051	0.028	-0.080	**	0.038	0.011	0.033	
- Grade 3	-0.007	-0.015	0.012		-0.023	0.021	-0.034	
Statistics and Probability								
- Grade 6	-0.042	0.020	-0.067		0.022	0.009	0.024	
- Grade 3	0.002	0.017	-0.008		0.003	0.009	-0.003	
Overall Math								
- Grade 6	-0.064	0.030	-0.098	**	0.035	0.014	0.029	
- Grade 3	0.000	0.014	-0.008		0.001	0.010	-0.004	
Educational interest at 8th grade assessment								
- 3-year High School	0.739	0.731	0.007		0.705	0.720	-0.012	
- Vocational training	0.526	0.523	-0.000		0.468	0.456	0.014	
- 2-year High School	0.229	0.193	0.024		0.102	0.088	0.014	
- Other	0.018	0.014	0.004		0.021	0.019	-0.001	
Personal ready at 8th grade assessment								
- 3-year High School	0.809	0.793	0.012		0.822	0.794	0.033	
- Vocational training	0.727	0.719	0.010		0.715	0.654	0.062	**
- 2-year High School	0.723	0.698	-0.015		0.717	0.674	0.047	
Social ready at 8th grade assessment								
- 3-year High School	0.863	0.862	0.003		0.882	0.867	0.018	

- Vocational training	0.805	0.804	0.011	0.799	0.765	0.029
- 2-year High School	0.811	0.804	0.016	0.832	0.781	0.039
Academic ready at 8th grade assessment						
- 3-year High School	0.587	0.607	-0.023	0.625	0.610	0.020
- Vocational training	0.753	0.777	-0.027 *	0.786	0.755	0.037 **
- 2-year High School	0.754	0.778	-0.028 *	0.786	0.756	0.037 **
- GPA (std.)	-0.192	-0.168	-0.028	-0.137	-0.159	0.032
Overall ready at 8th grade assessment						
- 3-year High School	0.655	0.661	-0.010	0.708	0.691	0.022
- Vocational training	0.585	0.602	-0.017	0.582	0.535	0.049 *
- 2-year High School	0.639	0.637	-0.042	0.591	0.584	0.060
Personality Traits						
- Grit	-0.002	-0.004	-0.009	0.009	-0.005	0.015
- Self-control	0.014	-0.007	0.003	0.000	0.005	-0.006
- Self-concept	0.000	0.011	0.002	0.012	-0.009	0.010
- Mindset	0.004	0.007	0.003	-0.016	0.020	-0.036
- General risk preferences	-0.020	0.021	-0.028	-0.009	0.014	-0.027
- Positive risk preferences	-0.029	0.051	-0.067	0.001	-0.001	0.005
- Negative risk preferences	0.040	-0.049	0.081	0.036	-0.028	0.065
- Patience1	0.009	-0.015	0.019	-0.009	0.012	-0.014
- Patience2	0.004	-0.005	0.005	0.009	-0.006	0.020
- Self-control awareness	0.029	-0.043	0.055	-0.005	0.003	-0.003
- Academic self-perception	-0.010	0.014	-0.026	0.033	-0.031	0.054
- Academic self-concept	-0.014	0.020	-0.039	0.032	-0.027	0.053
Strengths and Dif culties Questionnaire						
- Prosocial behaviour	-0.003	-0.001	-0.022	0.001	0.001	0.000
- Emotional symptoms	0.013	-0.021	0.033	-0.007	0.002	0.000
- Conduct problems	0.000	-0.004	0.025	0.016	-0.023	0.039
- Hyperactivity/inattention	0.013	-0.014	0.046	-0.008	0.007	-0.016
- Peer relationship problems	0.013	-0.030	0.055	-0.004	-0.008	0.006
- Total dif culties score	0.015	-0.025	0.056	-0.003	-0.005	0.006
Conscientiousness						
- Grade 7	-0.073	-0.026	-0.058 *	-0.001	-0.031	0.025
- Grade 6	-0.005	0.013	-0.034	0.059	0.069	-0.023
Agreeableness						
- Grade 7	-0.002	0.000	0.001	0.035	-0.014	0.055
- Grade 6	0.011	0.055	-0.047 *	0.076	0.023	0.041

Neuroticism								
- Grade 7	0.036	0.009	0.036		-0.073	0.029	-0.097	**
- Grade 6	-0.014	-0.056	0.054	**	-0.110	-0.069	-0.021	
Academic self-perception								
- Grade 7	-0.134	-0.103	-0.042		-0.108	-0.149	0.042	
- Grade 6	0.004	0.047	-0.067	**	0.044	0.019	0.019	
Academic well-being								
- Grade 7	-0.105	-0.058	-0.056	*	-0.037	-0.080	0.039	
- Grade 6	0.004	0.035	-0.053	*	0.062	0.057	-0.008	
Social well-being								
- Grade 7	-0.031	-0.009	-0.028		0.074	-0.011	0.079	*
- Grade 6	0.032	0.073	-0.056	*	0.122	0.085	0.018	
Order and quietness								
- Grade 7	-0.084	-0.061	-0.034		0.010	0.001	0.005	
- Grade 6	-0.092	-0.047	-0.051		0.023	0.027	-0.022	
Support and inspiration								
- Grade 7	-0.183	-0.217	0.027		-0.124	-0.210	0.078	
- Grade 6	-0.027	-0.033	-0.017		0.061	-0.062	0.107	**
Sick absence								
- Grade 7	3.132	3.518	-0.405	**	3.241	3.448	-0.179	
- Grade 6	3.180	3.126	0.057		3.118	3.231	-0.176	
Illegal absence								
- Grade 7	1.305	1.127	0.189		1.232	1.185	0.007	
- Grade 6	0.763	0.748	-0.002		0.526	0.685	-0.151	
Legal absence								
- Grade 7	1.644	1.501	0.103		1.680	1.618	0.051	
- Grade 6	1.308	1.250	0.053		1.269	1.296	-0.041	
Dyslexic information								
- Dyslexic	0.071	0.062	0.010		0.074	0.081	-0.006	
- Uncertain phonological	0.030	0.029	-0.001		0.025	0.026	-0.001	
- Not dyslexic	0.014	0.014	-0.001		0.012	0.013	-0.001	
- Not tested	0.885	0.895	-0.008		0.889	0.881	0.008	
- NOTA membership	0.092	0.083	0.007		0.095	0.114	-0.020	*
Schooling information								
- School starting age	6.213	6.203	0.009		6.220	6.197	0.016	
- Number of classes retaken	0.067	0.063	0.004		0.059	0.077	-0.011	
- Number of school changes	0.729	0.808	-0.069		0.732	0.740	0.005	
Child diagnosis								
- ADHD	0.013	0.011	0.001		0.003	0.002	0.001	
- Autisme	0.008	0.008	-0.001		0.000	0.001	-0.001	*
- OCD and anxiety	0.021	0.017	0.002		0.007	0.006	0.001	
- Other behavioral disorder	0.017	0.015	0.000		0.011	0.008	0.003	

Place of residence								
- Living with both parents	0.637	0.639	-0.003		0.629	0.636	-0.004	
- Living with one parents	0.351	0.349	0.002		0.360	0.352	0.005	
- Living with no parents	0.013	0.012	0.001		0.011	0.012	-0.002	
Etnicity								
- Danish	0.892	0.893	0.004		0.904	0.907	-0.003	
- Non-western	0.091	0.091	-0.003		0.080	0.077	0.002	
- Western	0.017	0.017	-0.000		0.016	0.015	0.001	
Age at birth								
- Mother	29.698	29.806	-0.114		29.784	30.059	-0.310	*
- Father	32.541	32.422	0.099		32.580	32.763	-0.222	
Income (1,000DKK)								
- Mother	278.126	290.945	-13.424	*	286.255	296.473	-10.506	
- Father	379.541	410.002	-31.230	***	400.040	407.164	-5.454	
Etnicity - Mother								
- Danish	0.863	0.851	0.017		0.873	0.878	-0.004	
- Non-western	0.109	0.117	-0.013		0.097	0.096	-0.000	
- Western	0.028	0.032	-0.003		0.030	0.025	0.004	
Etnicity - Father								
- Danish	0.872	0.864	0.011		0.881	0.890	-0.007	
- Non-western	0.101	0.107	-0.010		0.091	0.084	0.005	
- Western	0.028	0.029	-0.001		0.028	0.026	0.002	
Employment status - Mother								
- No bene ts	0.721	0.750	-0.028	**	0.754	0.759	-0.004	
- ALMP	0.158	0.146	0.012		0.139	0.142	-0.004	
- SU	0.025	0.021	0.003		0.017	0.024	-0.007	*
- Pension/leave	0.096	0.083	0.013	**	0.090	0.075	0.015	*
Employment status - Father								
- No bene ts	0.813	0.829	-0.015		0.825	0.823	0.005	
- ALMP	0.091	0.088	0.003		0.083	0.080	0.002	
- SU	0.005	0.006	-0.001		0.004	0.005	-0.002	
- Pension/leave	0.091	0.076	0.013	*	0.089	0.092	-0.005	
Education - Mother								
- No education	0.006	0.007	-0.002		0.005	0.010	-0.006	***
- Primary School	0.160	0.151	0.011		0.140	0.153	-0.011	
- High School	0.057	0.053	0.002		0.052	0.060	-0.006	
- Voccational	0.405	0.402	0.008		0.416	0.399	0.014	
- Short University Degree	0.059	0.053	0.002		0.048	0.047	0.001	
- Medium University Degree	0.258	0.253	0.004		0.269	0.259	0.013	
- Long University Degree	0.055	0.080	-0.026	**	0.069	0.073	-0.005	

Education - Father							
- No education	0.014	0.013	0.001		0.006	0.012	-0.006 **
- Primary School	0.202	0.188	0.013		0.191	0.199	-0.008
- High School	0.037	0.043	-0.007	*	0.039	0.046	-0.008
- Voccational	0.499	0.471	0.033	**	0.493	0.451	0.043 **
- Short University Degree	0.072	0.079	-0.009		0.084	0.079	0.005
- Medium University Degree	0.107	0.116	-0.009		0.115	0.121	-0.005
- Long University Degree	0.068	0.090	-0.022	**	0.072	0.093	-0.020
Observations	4,573	4,851	9,424		3,144	2,991	6,135

Note: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Column 1-2 and 4-5 report the mean for each treatment group. Column 3 and 6 report the differences between the two treatment groups by regressing each baseline covariate on the treatment indicator, including randomization strata xed effect and clustering at the school level. Baseline data are obtained from register data collected before the beginning of the intervention. Pupils are allowed to have more than one educational interest at the 8th grade risk assessment. They are only evaluated on personal, social and overall educational readiness for educations in which they indicate interest. The table is based on non-missing data.

Table A.2: Balance tests - all NR pupils in grade 8 (ITT-NR)

	Round 1 and 3				Round 2		
	Camp+	Control	Diff.		Camp+	Camp	Diff.
Language Comprehension							
- Grade 6	-0.406	-0.335	-0.086		-0.404	-0.375	-0.033
- Grade 4	-0.438	-0.352	-0.089	*	-0.366	-0.342	-0.015
- Grade 2	-0.322	-0.309	-0.008		-0.345	-0.299	-0.054
Decoding							
- Grade 6	-0.482	-0.393	-0.098	**	-0.444	-0.459	0.002
- Grade 4	-0.469	-0.387	-0.080	**	-0.415	-0.469	0.047
- Grade 2	-0.439	-0.412	-0.024		-0.487	-0.432	-0.064
Text Comprehension							
- Grade 6	-0.514	-0.493	-0.039		-0.492	-0.531	0.029
- Grade 4	-0.484	-0.481	-0.006		-0.458	-0.491	0.030
- Grade 2	-0.435	-0.407	-0.018		-0.488	-0.436	-0.062
Overall Reading							
- Grade 6	-0.544	-0.473	-0.087	*	-0.518	-0.528	-0.001
- Grade 4	-0.526	-0.461	-0.066		-0.471	-0.496	0.024
- Grade 2	-0.455	-0.429	-0.019		-0.505	-0.446	-0.069

Numbers and Algebra								
- Grade 6	-0.532	-0.407	-0.131	***	-0.386	-0.445	0.061	
- Grade 3	-0.399	-0.322	-0.076	*	-0.344	-0.410	0.055	
Geometry								
- Grade 6	-0.474	-0.397	-0.086	**	-0.408	-0.489	0.085	*
- Grade 3	-0.390	-0.357	-0.025		-0.373	-0.369	0.002	
Statistics and Probability								
- Grade 6	-0.512	-0.439	-0.083	**	-0.423	-0.496	0.077	
- Grade 3	-0.445	-0.373	-0.066		-0.384	-0.430	0.033	
Overall Math								
- Grade 6	-0.569	-0.466	-0.113	***	-0.455	-0.534	0.083	*
- Grade 3	-0.464	-0.396	-0.063		-0.413	-0.453	0.034	
Educational interest at 8th grade assessment								
- 3-year High School	0.627	0.627	0.004		0.551	0.573	-0.014	
- Vocational training	0.636	0.640	-0.005		0.606	0.602	0.009	
- 2-year High School	0.266	0.229	0.040		0.131	0.107	0.019	
- Other	0.045	0.036	0.011		0.057	0.050	-0.000	
Personal ready at 8th grade assessment								
- 3-year High School	0.444	0.390	0.042		0.393	0.334	0.071	*
- Vocational training	0.439	0.420	0.016		0.412	0.325	0.084	**
- 2-year High School	0.391	0.347	-0.011		0.354	0.276	0.056	
Social ready at 8th grade assessment								
- 3-year High School	0.602	0.594	0.011		0.597	0.570	0.021	
- Vocational training	0.601	0.596	0.022		0.586	0.542	0.029	
- 2-year High School	0.584	0.577	0.040		0.616	0.514	0.054	
Academic ready at 8th grade assessment								
- 3-year High School	0.085	0.108	-0.031	**	0.120	0.096	0.019	
- Vocational training	0.382	0.428	-0.051	**	0.420	0.366	0.062	**
- 2-year High School	0.383	0.430	-0.052	**	0.421	0.367	0.061	**
- GPA (std.)	-1.062	-1.008	-0.061	**	-1.037	-1.100	0.068	
Overall ready at 8th grade assessment								
- 3-year High School	0.000	0.000	0.000		0.002	0.000	0.002	
- Vocational training	0.153	0.178	-0.024		0.138	0.093	0.041	*
- 2-year High School	0.200	0.211	-0.062		0.149	0.121	0.022	
Personality Traits								
- Grit	-0.411	-0.364	-0.061		-0.428	-0.445	0.009	
- Self-control	-0.161	-0.201	0.034		-0.237	-0.217	-0.041	
- Self-concept	-0.003	0.010	0.028		-0.283	-0.330	0.038	
- Mindset	0.001	0.005	0.002		-0.278	-0.220	-0.044	
- General risk preferences	-0.023	0.070	-0.095	**	-0.012	0.009	-0.024	
- Positive risk preferences	-0.450	-0.298	-0.146	**	-0.385	-0.373	-0.005	
- Negative risk preferences	-0.131	-0.290	0.149	*	-0.183	-0.264	0.073	

- Patience1	-0.006	-0.020	0.016		-0.175	-0.172	0.007	
- Patience2	-0.085	-0.148	0.084		-0.135	-0.169	0.036	
- Self-control awareness	0.233	0.145	0.069		0.197	0.202	0.009	
- Academic self-perception	-0.320	-0.347	0.047		-0.349	-0.409	0.035	
- Academic self-concept	-0.527	-0.545	0.035		-0.521	-0.605	0.071	
Strengths and Dif culties Questionnaire								
- Prosocial behaviour	-0.151	-0.225	0.031		-0.188	-0.164	-0.013	
- Emotional symptoms	0.104	0.115	-0.023		0.077	0.096	-0.007	
- Conduct problems	0.363	0.366	0.016		0.355	0.337	0.022	
- Hyperactivity/inattention	0.338	0.372	-0.044		0.361	0.391	-0.033	
- Peer relationship problems	0.318	0.238	0.096		0.294	0.259	0.043	
- Total dif culties score	0.381	0.377	0.006		0.365	0.370	0.001	
Conscientiousness								
- Grade 7	-0.442	-0.365	-0.087	**	-0.380	-0.435	0.046	
- Grade 6	-0.333	-0.344	-0.006		-0.285	-0.308	-0.004	
Agreeableness								
- Grade 7	-0.215	-0.243	0.034		-0.220	-0.299	0.086	*
- Grade 6	-0.169	-0.122	-0.044		-0.164	-0.251	0.081	*
Neuroticism								
- Grade 7	0.131	0.147	0.004		0.088	0.227	-0.135	**
- Grade 6	0.139	0.096	0.057		0.055	0.110	-0.033	
Academic self-perception								
- Grade 7	-0.550	-0.530	-0.040		-0.539	-0.573	0.016	
- Grade 6	-0.327	-0.308	-0.048		-0.328	-0.333	-0.008	
Academic well-being								
- Grade 7	-0.537	-0.483	-0.066		-0.497	-0.544	0.033	
- Grade 6	-0.377	-0.370	-0.031		-0.348	-0.366	-0.004	
Social well-being								
- Grade 7	-0.140	-0.191	0.036		-0.110	-0.233	0.120	**
- Grade 6	-0.120	-0.109	-0.034		-0.076	-0.120	0.027	
Order and quietness								
- Grade 7	-0.191	-0.190	-0.005		-0.114	-0.147	0.028	
- Grade 6	-0.163	-0.201	0.039		-0.078	-0.106	0.005	
Support and inspiration								
- Grade 7	-0.300	-0.386	0.077		-0.334	-0.383	0.037	
- Grade 6	-0.137	-0.184	0.041		-0.085	-0.201	0.097	
Sick absence								
- Grade 7	3.924	4.386	-0.476	*	4.178	4.402	-0.178	
- Grade 6	4.026	3.869	0.181		4.239	4.256	-0.076	
Illegal absence								

- Grade 7	2.260	1.845	0.373		2.193	2.071	0.054
- Grade 6	1.281	1.170	0.051		0.963	1.178	-0.180
Legal absence							
- Grade 7	1.826	1.648	0.114		1.834	1.729	0.071
- Grade 6	1.398	1.309	0.094		1.495	1.315	0.137
Dyslexic information							
- Dyslexic	0.133	0.106	0.028	**	0.133	0.144	-0.008
- Uncertain phonological	0.048	0.045	0.000		0.034	0.046	-0.009
- Not dyslexic	0.023	0.023	-0.001		0.020	0.025	-0.004
- Not tested	0.796	0.826	-0.028		0.813	0.786	0.022
- NOTA membership	0.167	0.141	0.022		0.171	0.205	-0.037 *
Schooling information							
- School starting age	6.250	6.253	-0.003		6.271	6.245	0.018
- Number of classes retaken	0.113	0.101	0.004		0.112	0.116	-0.001
- Number of school changes	0.902	0.965	-0.053		0.903	0.951	-0.009
Child diagnosis							
- ADHD	0.023	0.022	-0.002		0.003	0.004	-0.001
- Autisme	0.014	0.013	-0.001		0.000	0.001	-0.001
- OCD and anxiety	0.026	0.021	0.003		0.012	0.006	0.007 *
- Other behavioral disorder	0.025	0.022	0.001		0.017	0.012	0.005
Place of residence							
- Living with both parents	0.561	0.559	0.006		0.514	0.547	-0.022
- Living with one parents	0.418	0.419	-0.006		0.468	0.433	0.024
- Living with no parents	0.021	0.022	0.000		0.019	0.020	-0.002
Etnicity							
- Danish	0.849	0.847	0.007		0.854	0.865	-0.013
- Non-western	0.126	0.130	-0.008		0.124	0.111	0.016
- Western	0.025	0.023	0.001		0.022	0.024	-0.002
Age at birth							
- Mother	29.094	29.077	0.030		28.916	29.061	-0.162
- Father	32.248	31.991	0.247		31.915	32.218	-0.348
Income (1,000DKK)							
- Mother	231.962	243.491	-12.425		243.162	241.087	1.635
- Father	331.748	346.997	-15.889		336.289	347.120	-10.610
Etnicity - Mother							
- Danish	0.814	0.798	0.024		0.826	0.829	-0.005
- Non-western	0.147	0.161	-0.021		0.140	0.136	0.008
- Western	0.039	0.041	-0.002		0.034	0.034	-0.002
Etnicity - Father							

- Danish	0.831	0.823	0.014		0.843	0.863	-0.025	
- Non-western	0.134	0.146	-0.018		0.131	0.111	0.024	
- Western	0.034	0.031	0.003		0.026	0.026	0.001	
Employment status - Mother								
- No bene ts	0.642	0.661	-0.014		0.668	0.652	0.016	
- ALMP	0.216	0.203	0.008		0.194	0.208	-0.018	
- SU	0.031	0.027	0.003		0.024	0.035	-0.012	
- Pension/leave	0.111	0.109	0.003		0.115	0.104	0.014	
Employment status - Father								
- No bene ts	0.750	0.767	-0.018		0.766	0.735	0.039	*
- ALMP	0.133	0.127	0.007		0.112	0.125	-0.015	
- SU	0.003	0.009	-0.006	**	0.005	0.006	-0.001	
- Pension/leave	0.113	0.097	0.017		0.117	0.134	-0.023	*
Education - Mother								
- No education	0.008	0.011	-0.002		0.009	0.014	-0.006	
- Primary School	0.245	0.232	0.015		0.221	0.252	-0.026	
- High School	0.057	0.050	0.006		0.049	0.054	-0.005	
- Voccational	0.433	0.449	-0.014		0.462	0.447	0.017	
- Short University Degree	0.045	0.037	0.009		0.043	0.036	0.004	
- Medium University Degree	0.181	0.185	-0.005		0.174	0.167	0.004	
- Long University Degree	0.030	0.037	-0.009		0.043	0.029	0.013	*
Education - Father								
- No education	0.017	0.016	0.000		0.008	0.014	-0.005	
- Primary School	0.287	0.268	0.015		0.275	0.312	-0.034	
- High School	0.032	0.043	-0.013	*	0.039	0.032	0.005	
- Voccational	0.511	0.510	0.012		0.522	0.461	0.060	***
- Short University Degree	0.051	0.052	-0.003		0.067	0.063	0.001	
- Medium University Degree	0.068	0.067	0.000		0.058	0.072	-0.012	
- Long University Degree	0.033	0.043	-0.012	*	0.031	0.046	-0.014	
Observations	1,841	1,918	3,759		1,174	1,161	2,335	

Note: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Column 1-2 and 4-5 report the mean for each treatment group. Column 3 and 6 report the differences between the two treatment groups by regressing each baseline covariate on the treatment indicator, including randomization strata xed effect and clustering at the school level. Baseline data are obtained from register data collected before the beginning of the intervention. Pupils are allowed to have more than one educational interest at the 8th grade risk assessment. They are only evaluated on personal, social and overall educational readiness for educations in which they indicate interest. The table is based on non-missing data.

Table A.3: Balance tests - camp selected pupils (ATT)

	Round 1 and 3				Round 2			
	Camp+	Control	Diff.		Camp+	Camp	Diff.	
Language Comprehension								
- Grade 6	-0.522	-0.391	-0.148	**	-0.530	-0.461	-0.064	
- Grade 4	-0.572	-0.408	-0.160	***	-0.516	-0.460	-0.044	
- Grade 2	-0.379	-0.349	-0.017		-0.446	-0.373	-0.079	
Decoding								
- Grade 6	-0.609	-0.474	-0.146	***	-0.595	-0.557	-0.047	
- Grade 4	-0.575	-0.463	-0.104	**	-0.593	-0.597	-0.000	
- Grade 2	-0.565	-0.460	-0.093	*	-0.647	-0.523	-0.136	**
Text Comprehension								
- Grade 6	-0.637	-0.554	-0.101	**	-0.640	-0.627	-0.018	
- Grade 4	-0.584	-0.561	-0.023		-0.588	-0.601	0.010	
- Grade 2	-0.553	-0.455	-0.094	*	-0.663	-0.531	-0.148	**
Overall Reading								
- Grade 6	-0.686	-0.550	-0.153	***	-0.682	-0.636	-0.050	
- Grade 4	-0.654	-0.541	-0.108	**	-0.646	-0.631	-0.013	
- Grade 2	-0.570	-0.481	-0.078		-0.671	-0.545	-0.139	**
Numbers and Algebra								
- Grade 6	-0.650	-0.461	-0.196	***	-0.486	-0.509	0.024	
- Grade 3	-0.495	-0.411	-0.083		-0.422	-0.531	0.095	*
Geometry								
- Grade 6	-0.557	-0.440	-0.121	***	-0.493	-0.522	0.032	
- Grade 3	-0.482	-0.420	-0.054		-0.453	-0.481	0.023	
Statistics and Probability								
- Grade 6	-0.634	-0.508	-0.133	***	-0.515	-0.572	0.058	
- Grade 3	-0.552	-0.452	-0.095	*	-0.508	-0.524	0.002	
Overall Math								
- Grade 6	-0.690	-0.528	-0.168	***	-0.558	-0.599	0.043	
- Grade 3	-0.575	-0.482	-0.087	*	-0.519	-0.576	0.045	
Educational interest at 8th grade assessment								
- High School	0.557	0.572	-0.019		0.488	0.548	-0.056	
- Vocational traning	0.690	0.675	0.015		0.694	0.652	0.043	
- 2-year High School	0.247	0.198	0.045		0.136	0.110	0.023	
- Other	0.026	0.012	0.016		0.017	0.021	-0.007	
Personal ready at 8th grade assessment								
- 3-year High School	0.323	0.203	0.109	***	0.286	0.224	0.055	
- Vocational traning	0.411	0.306	0.090	**	0.381	0.283	0.088	**
- 2-year High School	0.412	0.260	0.122	*	0.277	0.241	-0.028	
Social ready at 8th grade assessment								
- 2-year High School	0.525	0.480	0.034		0.542	0.517	0.016	

- Vocational training	0.595	0.540	0.060		0.568	0.512	0.039
- 2-year High School	0.596	0.510	0.134		0.607	0.497	0.051
Academic ready at 8th grade assessment							
- 3-year High School	0.048	0.099	-0.052	***	0.073	0.072	-0.001
- Vocational training	0.246	0.311	-0.075	**	0.298	0.295	0.009
- 2-year High School	0.246	0.312	-0.076	**	0.298	0.295	0.009
- GPA (std.)	-1.185	-1.109	-0.085	**	-1.153	-1.170	0.026
Overall ready at 8th grade assessment							
- 3-year High School	0.000	0.000	0.000		0.003	0.000	0.004
- Vocational training	0.083	0.066	0.005		0.073	0.068	0.002
- 2-year High School	0.177	0.115	0.047		0.058	0.110	-0.034
Personality Traits							
- Grit	-0.508	-0.432	-0.096	**	-0.477	-0.509	0.026
- Self-control	-0.151	-0.235	0.073		-0.325	-0.247	-0.075
- Self-concept	0.049	0.011	0.062		-0.312	-0.369	0.052
- Mindset	0.045	0.023	0.015		-0.316	-0.266	-0.039
- General risk preferences	-0.044	0.109	-0.142	***	0.030	-0.006	0.035
- Positive risk preferences	-0.520	-0.392	-0.133		-0.458	-0.400	-0.061
- Negative risk preferences	-0.046	-0.304	0.237	**	-0.228	-0.253	0.031
- Patience1	-0.009	-0.027	0.023		-0.200	-0.174	-0.010
- Patience2	-0.081	-0.113	0.030		-0.118	-0.210	0.092
- Self-control awareness	0.311	0.182	0.126	*	0.252	0.231	0.006
- Academic self-perception	-0.382	-0.313	-0.074		-0.384	-0.439	0.047
- Academic self-concept	-0.655	-0.597	-0.068		-0.602	-0.695	0.087
Strengths and Dif culties Questionnaire							
- Prosocial behaviour	-0.158	-0.246	0.073		-0.262	-0.193	-0.071
- Emotional symptoms	0.077	0.083	-0.018		0.055	0.100	-0.041
- Conduct problems	0.351	0.441	-0.077		0.418	0.389	0.024
- Hyperactivity/inattention	0.389	0.396	-0.000		0.426	0.452	-0.033
- Peer relationship problems	0.348	0.294	0.055		0.332	0.319	0.013
- Total dif culties score	0.396	0.411	-0.013		0.412	0.429	-0.020
Conscientiousness							
- Grade 7	-0.515	-0.387	-0.148	***	-0.434	-0.503	0.063
- Grade 6	-0.383	-0.383	-0.016		-0.344	-0.366	0.010
Agreeableness							
- Grade 7	-0.214	-0.320	0.112	**	-0.285	-0.343	0.071
- Grade 6	-0.153	-0.228	0.080		-0.211	-0.316	0.101 *

Neuroticism								
- Grade 7	0.149	0.174	-0.004		0.126	0.225	-0.090	
- Grade 6	0.110	0.101	0.025		0.148	0.110	0.044	
Academic self-perception								
- Grade 7	-0.696	-0.604	-0.119	**	-0.611	-0.658	0.039	
- Grade 6	-0.402	-0.365	-0.068		-0.412	-0.437	0.026	
Academic well-being								
- Grade 7	-0.652	-0.534	-0.139	***	-0.576	-0.617	0.036	
- Grade 6	-0.436	-0.432	-0.023		-0.436	-0.440	-0.003	
Social well-being								
- Grade 7	-0.161	-0.205	0.027		-0.107	-0.221	0.104	
- Grade 6	-0.105	-0.116	-0.010		-0.139	-0.133	-0.011	
Order and quietness								
- Grade 7	-0.179	-0.204	0.019		-0.135	-0.163	0.034	
- Grade 6	-0.118	-0.229	0.086		-0.106	-0.096	-0.008	
Support and inspiration								
- Grade 7	-0.269	-0.418	0.134	**	-0.319	-0.370	0.047	
- Grade 6	-0.096	-0.215	0.107	*	-0.093	-0.195	0.107	*
Sick absence								
- Grade 7	3.740	4.285	-0.536	*	4.172	4.015	0.214	
- Grade 6	3.790	3.926	-0.102		4.109	3.898	0.112	
Illegal absence								
- Grade 7	2.182	2.087	0.040		1.716	1.791	-0.109	
- Grade 6	1.354	1.204	0.120		0.826	1.060	-0.244	
Legal absence								
- Grade 7	1.699	1.702	-0.032		1.698	1.643	0.103	
- Grade 6	1.330	1.329	0.016		1.346	1.248	0.077	
Dyslexic information								
- Dyslexic	0.176	0.113	0.063	***	0.165	0.168	-0.003	
- Uncertain phonological	0.051	0.044	0.008		0.041	0.060	-0.015	
- Not dyslexic	0.028	0.024	0.003		0.022	0.028	-0.006	
- Not tested	0.745	0.819	-0.074	***	0.772	0.743	0.023	
- NOTA membership	0.200	0.146	0.052	**	0.201	0.243	-0.042	
Schooling information								
- School starting age	6.252	6.259	-0.008		6.273	6.257	0.007	
- Number of classes retaken	0.113	0.103	0.005		0.113	0.129	-0.011	
- Number of school changes	0.901	0.923	-0.008		0.921	0.925	0.006	
Child diagnosis								
- ADHD	0.018	0.018	-0.002		0.000	0.005	-0.005	**
- Autisme	0.006	0.004	0.003		0.000	0.001	-0.001	
- OCD and anxiety	0.012	0.018	-0.007		0.008	0.005	0.003	
- Other behavioral disorder	0.017	0.025	-0.009		0.025	0.016	0.011	*

Place of residence									
- Living with both parents	0.553	0.527	0.026		0.504	0.542	-0.033		
- Living with one parents	0.426	0.447	-0.020		0.482	0.443	0.035		
- Living with no parents	0.021	0.026	-0.005		0.014	0.015	-0.002		
Etnicity									
- Danish	0.842	0.855	-0.002		0.868	0.861	0.007		
- Non-western	0.133	0.119	0.004		0.110	0.114	-0.003		
- Western	0.025	0.026	-0.002		0.022	0.025	-0.004		
Age at birth									
- Mother	28.949	28.904	0.008		28.823	28.962	-0.126		
- Father	31.910	31.843	0.029		31.779	31.965	-0.205		
Income (1,000DKK)									
- Mother	224.790	241.804	-12.816		235.731	234.917	0.339		
- Father	313.529	357.524	-42.531	***	332.146	337.463	-3.578		
Etnicity - Mother									
- Danish	0.809	0.804	0.019		0.842	0.825	0.016		
- Non-western	0.153	0.148	-0.008		0.124	0.142	-0.015		
- Western	0.038	0.048	-0.011		0.034	0.033	-0.001		
Etnicity - Father									
- Danish	0.821	0.840	-0.008		0.863	0.864	-0.001		
- Non-western	0.143	0.129	0.003		0.109	0.110	-0.001		
- Western	0.036	0.031	0.005		0.028	0.026	0.002		
Employment status - Mother									
- No bene ts	0.638	0.650	-0.004		0.663	0.657	0.010		
- ALMP	0.221	0.213	-0.000		0.190	0.212	-0.028		
- SU	0.040	0.026	0.013	*	0.023	0.041	-0.019	**	
- Pension/leave	0.102	0.111	-0.010		0.125	0.090	0.037	**	
Employment status - Father									
- No bene ts	0.724	0.776	-0.053	**	0.757	0.738	0.027		
- ALMP	0.152	0.118	0.034	*	0.114	0.123	-0.013		
- SU	0.004	0.008	-0.005		0.008	0.006	0.002		
- Pension/leave	0.120	0.097	0.024		0.120	0.134	-0.016		
Education - Mother									
- No education	0.010	0.012	-0.002		0.003	0.017	-0.013	**	
- Primary School	0.266	0.223	0.041	**	0.228	0.245	-0.017		
- High School	0.050	0.052	-0.004		0.051	0.056	-0.006		
- Voccational	0.452	0.469	-0.010		0.500	0.466	0.035		
- Short University Degree	0.040	0.032	0.008		0.036	0.039	-0.003		
- Medium University Degree	0.155	0.182	-0.030	*	0.149	0.158	-0.011		
- Long University Degree	0.028	0.030	-0.002		0.033	0.019	0.014	*	

Education - Father							
- No education	0.020	0.020	-0.001		0.007	0.013	-0.007	
- Primary School	0.304	0.262	0.036		0.294	0.295	-0.002	
- High School	0.032	0.037	-0.008		0.036	0.028	0.006	
- Voccational	0.504	0.518	-0.001		0.548	0.491	0.059	**
- Short University Degree	0.053	0.045	0.006		0.051	0.060	-0.010	
- Medium University Degree	0.060	0.075	-0.016		0.043	0.073	-0.027	**
- Long University Degree	0.028	0.042	-0.016	**	0.022	0.042	-0.019	*
Observations	955	1,103	2,058		631	748	1,379	

Note: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Column 1-2 and 4-5 report the mean for each treatment group. Column 3 and 6 report the differences between the two treatment groups by regressing each baseline covariate on the treatment indicator, including randomization strata xed effect and clustering at the school level. Baseline data are obtained from register data collected before the beginning of the intervention. Pupils are allowed to have more than one educational interest at the 8th grade risk assessment. They are only evaluated on personal, social and overall educational readiness for educations in which they indicate interest. The table is based on non-missing data.

Table A.4: Short-run effects of Camp+ vs. Camp: performance in the national reading test in grade 8

	Language Comprehension		Decoding		Text Comprehension		Overall	
ATT	0.047	0.081	0.029	0.046	0.070	0.071	0.060	0.078
	(0.081)	(0.076)	(0.051)	(0.047)	(0.060)	(0.048)	(0.061)	(0.051)
Mean outcome, Camp	-.373	-.373	-.622	-.622	-.704	-.704	-.700	-.700
R-squared	.014	.211	.018	.426	.025	.376	.015	.432
Observations	1,316	1,316	1,316	1,316	1,316	1,316	1,316	1,316
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.5: Short-run effects of Camp+ vs. Camp: performance in the national math test in grade 8

	Numbers and Algebra		Geometry		Statistics and Probability		Overall	
ATT	-0.019	-0.030	-0.020	-0.040	-0.012	-0.021	-0.018	-0.037
	(0.057)	(0.042)	(0.063)	(0.047)	(0.066)	(0.047)	(0.063)	(0.046)
Mean outcome, Camp	-.710	-.710	-.707	-.707	-.669	-.669	-.751	-.751
R-squared	.015	.437	.016	.414	.012	.428	.015	.512
Observations	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.6: Effects of the camp on the performance in the nal exams in grade 9, split-up in the oral and written parts

	Danish				Math			
	Written		Oral		Written		Oral	
ITT	-0.043	-0.007	-0.070	-0.040	0.020	0.064	0.013	0.108
	(0.055)	(0.035)	(0.047)	(0.037)	(0.061)	(0.047)	(0.100)	(0.087)
Mean outcome, Control	-.059	-.059	-.038	-.038	-.102	-.102	-.100	-.100
R-squared	.015	.677	.011	.388	.015	.664	.053	.465
Observations	4,759	4,759	4,722	4,722	4,743	4,743	875	875
ITT-NR	-0.017	0.011	-0.090*	-0.052	0.028	0.096*	0.027	0.085
	(0.051)	(0.039)	(0.052)	(0.047)	(0.058)	(0.050)	(0.082)	(0.112)
LATE	-0.036	0.023	-0.183*	-0.105	0.057	0.197*	0.055	0.174
	(0.104)	(0.080)	(0.107)	(0.097)	(0.119)	(0.101)	(0.167)	(0.229)
Mean outcome, Control	-.732	-.732	-.581	-.581	-.778	-.778	-.673	-.673
R-squared	.019	.529	.018	.268	.013	.482	.056	.295
Observations	1,879	1,879	1,847	1,847	1,864	1,864	349	349
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome only exists for round 1. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Oral math exam is an extract exam. Standard errors in parentheses are clustered at the school level.

Table A.7: Effects of the camp on the education choice three years after the camp

	3-year High School		Vocational training		2-year High School		No educ.	
ITT	-0.036	-0.016	0.038**	0.027**	-0.006	-0.009	0.002	-0.004
	(0.028)	(0.016)	(0.017)	(0.013)	(0.010)	(0.009)	(0.012)	(0.009)
Mean outcome, Control	.607	.607	.201	.201	.091	.091	.095	.095
R-squared	.016	.420	.008	.238	.003	.063	.005	.152
Observations	4,820	4,820	4,820	4,820	4,820	4,820	4,820	4,820
ITT-NR	-0.023	0.001	0.062***	0.049***	-0.010	-0.008	-0.027	-0.037**
	(0.032)	(0.023)	(0.022)	(0.019)	(0.016)	(0.016)	(0.021)	(0.018)
LATE	-0.048	0.003	0.127***	0.101***	-0.020	-0.017	-0.055	-0.076**
	(0.066)	(0.047)	(0.046)	(0.038)	(0.032)	(0.034)	(0.043)	(0.038)
Mean outcome, Control	.342	.342	.320	.320	.130	.130	.193	.193
R-squared	.033	.333	.020	.185	.008	.057	.012	.163
Observations	1,931	1,931	1,931	1,931	1,931	1,931	1,931	1,931
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome only exists for round 1. Educational enrollment is measured September31th three years after the camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.8: Long-run effects of Camp+ vs. Camp: Performance in the nal tests in grade 9 and likelihood of being enrolled in no education 1.5 years after the camp

	Danish score		Math score		No education	
ATT	0.008	0.021	-0.022	-0.055	-0.004	0.002
	(0.050)	(0.042)	(0.056)	(0.041)	(0.013)	(0.013)
Mean outcome, Camp	-.955	-.955	-.835	-.835	.066	.066
R-squared	.028	.489	.034	.483	.015	.141
Observations	1,317	1,317	1,310	1,310	1,344	1,344
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. Educational enrollment is measured September 31th one year after the camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.9a: Effects of the camp on non-cognitive skills, measured by psychological scales

	Grit		Self-control		Self-concept		Mindset		Total dif cult score	
ITT	-0.001	0.009	0.016	0.027	0.013	0.005	0.029	0.045	0.010	-0.010
	(0.034)	(0.021)	(0.030)	(0.022)	(0.030)	(0.020)	(0.044)	(0.046)	(0.037)	(0.030)
Mean outcome, Control	-.010	-.010	-.018	-.018	.001	.001	-.025	-.025	.006	.006
R-squared	.006	.490	.008	.414	.006	.487	.009	.221	.016	.403
Observations	6,993	6,993	6,976	6,976	6,957	6,957	3,533	3,533	6,876	6,876
ITT-NR	-0.017	0.005	0.012	0.007	0.009	-0.023	0.075	0.099	-0.002	-0.015
	(0.042)	(0.032)	(0.041)	(0.034)	(0.036)	(0.030)	(0.058)	(0.064)	(0.046)	(0.043)
LATE	-0.036	0.011	0.025	0.014	0.018	-0.047	0.154	0.202	-0.004	-0.032
	(0.086)	(0.065)	(0.084)	(0.070)	(0.073)	(0.062)	(0.119)	(0.131)	(0.095)	(0.087)
Mean outcome, Control	-.374	-.374	-.161	-.161	.040	.040	-.321	-.321	.346	.346
R-squared	.012	.410	.015	.411	.081	.485	.023	.187	.017	.350
Observations	2,550	2,550	2,539	2,539	2,529	2,529	1,249	1,249	2,481	2,481
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Mindset, Positive and Negative risk preference and Patience1 are only measured in round 3. Standard errors in parentheses are clustered at the school level.

Table A.9b: Effects of the camp on non-cognitive skills, measured by psychological scales - cont'd

	General risk preferences		Positive risk preferences		Negative risk preferences		Patience2		Patience1	
ITT	0.033	0.030	-0.021	0.000	0.052	0.016	-0.012	-0.006	0.027	0.001
	(0.025)	(0.023)	(0.051)	(0.037)	(0.063)	(0.043)	(0.026)	(0.022)	(0.035)	(0.032)
Mean outcome, Control	-.018	-.018	.014	.014	-.057	-.057	.002	.002	-.018	-.018
R-squared	.003	.220	.004	.404	.009	.336	.003	.145	.008	.212
Observations	6,903	6,903	3,528	3,528	3,528	3,528	6,902	6,902	3,515	3,515
ITT-NR	0.007	0.038	-0.070	-0.062	0.074	0.029	-0.041	-0.045	0.039	0.017
	(0.040)	(0.039)	(0.074)	(0.060)	(0.073)	(0.063)	(0.042)	(0.040)	(0.054)	(0.053)
LATE	0.015	0.078	-0.144	-0.127	0.152	0.059	-0.084	-0.092	0.079	0.035
	(0.082)	(0.079)	(0.153)	(0.123)	(0.148)	(0.129)	(0.087)	(0.081)	(0.110)	(0.109)
Mean outcome, Control	-.019	-.019	-.308	-.308	-.212	-.212	-.103	-.103	-.176	-.176
R-squared	.013	.190	.009	.341	.011	.332	.010	.127	.008	.160
Observations	2,492	2,492	1,248	1,248	1,248	1,248	2,491	2,491	1,242	1,242
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Mindset, Positive and Negative risk preference and Patience1 are only measured in round 3. Standard errors in parentheses are clustered at the school level.

Table A.10: Effects of the camp on the ERA

	December			
	High School		Vocational training	
ITT	-0.002	0.013	0.000	0.020
	(0.018)	(0.011)	(0.022)	(0.015)
Mean outcome, Control	.760	.760	.790	.790
R-squared	.011	.515	.008	.416
Observations	3,806	3,806	2,677	2,677
ITT-NR	-0.008	0.002	-0.012	0.036
	(0.030)	(0.028)	(0.034)	(0.028)
LATE	-0.017	0.004	-0.024	0.074
	(0.061)	(0.058)	(0.069)	(0.058)
Mean outcome, Control	.383	.383	.613	.613
R-squared	.010	.313	.008	.322
Observations	1,247	1,247	1,364	1,364
Strata	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome only exists for round 1. Pupils are not able to apply separately for 2 and 3 years High School in 2017/2018 and the June assessment is introduced in 2018/2019. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.11: Effects of the camp on the personal skills assessed in the ERA

	December			
	High School		Vocational training	
ITT	0.017	0.017	0.001	0.021
	(0.014)	(0.012)	(0.023)	(0.016)
Mean outcome, Control	.861	.861	.813	.813
R-squared	.010	.463	.011	.401
Observations	3,710	3,710	2,606	2,606
ITT-NR	0.061*	0.043	-0.010	0.034
	(0.034)	(0.029)	(0.038)	(0.029)
LATE	0.124*	0.088	-0.019	0.069
	(0.069)	(0.060)	(0.077)	(0.059)
Mean outcome, Control	.607	.607	.655	.655
R-squared	.032	.397	.016	.329
Observations	1,197	1,197	1,337	1,337
Strata	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome only exists for round 1. Pupils are not able to apply separately for 2 and 3 years High School in 2017/2018 and the June assessment is introduced in 2018/2019. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.12: Effects of the camp on the social skills assessed in the ERA

	December			
	High School		Vocational training	
ITT	0.005	0.001	-0.007	0.009
	(0.012)	(0.009)	(0.021)	(0.013)
Mean outcome, Control	.913	.913	.879	.879
R-squared	.008	.461	.015	.418
Observations	3,710	3,710	2,606	2,606
ITT-NR	0.024	-0.002	-0.017	0.010
	(0.032)	(0.024)	(0.036)	(0.027)
LATE	0.050	-0.005	-0.034	0.020
	(0.065)	(0.049)	(0.073)	(0.054)
Mean outcome, Control	.748	.748	.773	.773
R-squared	.033	.441	.024	.351
Observations	1,197	1,197	1,337	1,337
Strata	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome only exists for round 1. Pupils are not able to apply separately for 2 and 3 years High School in 2017/2018 and the June assessment is introduced in 2018/2019. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.13: Effects of the camp on the preferred education choice in the ERA

			December			
	High School		Vocational training		10th grade	
ITT	0.001	0.004	-0.027	-0.026	0.004	-0.001
	(0.023)	(0.014)	(0.042)	(0.021)	(0.005)	(0.005)
Mean outcome, Control	.785	.785	.572	.572	.009	.009
R-squared	.010	.493	.015	.515	.006	.200
Observations	4,874	4,874	4,874	4,874	4,874	4,874
ITT-NR	0.002	0.018	-0.044	-0.032	0.002	-0.008
	(0.034)	(0.026)	(0.036)	(0.023)	(0.008)	(0.009)
LATE	0.004	0.037	-0.090	-0.066	0.004	-0.016
	(0.070)	(0.054)	(0.074)	(0.047)	(0.016)	(0.018)
Mean outcome, Control	.634	.634	.723	.723	.023	.023
R-squared	.027	.425	.035	.429	.009	.225
Observations	1,977	1,977	1,977	1,977	1,977	1,977
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome only exists for round 1. Pupils are not able to apply for 2-year High School in 2017/2018 and the June assessment is introduced in 2018/2019. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.14: Effects of Camp+ vs. Camp on the ERA

	December						June					
	3-year High School		Vocational training		2-year High School		3-year High School		Vocational training		2-year High School	
ATT	-0.032	-0.057*	-0.016	-0.026	-0.053	-0.137***	-0.119***	-0.149***	0.008	-0.017	-0.109*	-0.084
	(0.041)	(0.033)	(0.038)	(0.031)	(0.066)	(0.050)	(0.040)	(0.033)	(0.037)	(0.035)	(0.056)	(0.067)
Mean outcome, Camp	.363	.363	.602	.602	.473	.473	.488	.488	.661	.661	.582	.582
R-squared	.041	.317	.020	.270	.077	.470	.062	.340	.017	.293	.101	.455
Observations	633	633	978	978	300	300	614	614	992	992	299	299
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.15: Effects of Camp+ vs. Camp on the personal skills assessed in the ERA

	December						June					
	3-year High School		Vocational training		2-year High School		3-year High School		Vocational training		2-year High School	
ATT	-0.068	-0.091**	-0.029	-0.045	-0.142**	-0.151**	-0.087**	-0.102***	-0.002	-0.018	-0.135**	-0.105
	(0.042)	(0.035)	(0.037)	(0.032)	(0.068)	(0.063)	(0.038)	(0.033)	(0.035)	(0.032)	(0.052)	(0.070)
Mean outcome, Camp	.619	.619	.673	.673	.652	.652	.685	.685	.715	.715	.707	.707
R-squared	.023	.293	.018	.265	.079	.314	.031	.334	.016	.250	.120	.401
Observations	609	609	956	956	265	265	608	608	986	986	286	286
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.16: Effects of Camp+ vs. Camp on the preferred education choice in the ERA

	December							
	3-year High School		Vocational training		10th grade		2-year High School	
ATT	-0.038	-0.009	0.022	0.009	-0.001	0.000	-0.018	-0.006
	(0.042)	(0.025)	(0.033)	(0.019)	(0.003)	(0.004)	(0.035)	(0.024)
Mean outcome, Camp	.497	.497	.732	.732	.006	.006	.238	.238
R-squared	.024	.455	.030	.476	.009	.062	.029	.328
Observations	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
	June							
	3-year High School		Vocational training		10th grade		2-year High School	
ATT	-0.028	-0.008	0.056*	0.038*	-0.004	-0.002	0.014	0.028
	(0.041)	(0.026)	(0.033)	(0.022)	(0.005)	(0.006)	(0.037)	(0.029)
Mean outcome, Camp	.479	.479	.728	.728	.010	.010	.221	.221
R-squared	.025	.443	.021	.422	.014	.054	.034	.290
Observations	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.17a: Effects of Camp+ vs. Camp on non-cognitive skills, measured by psychological scales

	Grit		Self-control		Self-concept		Mindset		Total dif cult score	
ATT	-0.008	-0.030	-0.035	-0.040	-0.028	-0.044	-0.027	-0.038	0.066	0.048
	(0.059)	(0.045)	(0.076)	(0.053)	(0.066)	(0.047)	(0.059)	(0.047)	(0.064)	(0.046)
Mean outcome, Camp	-.474	-.474	-.198	-.198	-.331	-.331	-.290	-.290	.396	.396
R-squared	.014	.421	.015	.403	.020	.476	.011	.222	.022	.449
Observations	1,159	1,159	1,156	1,156	1,150	1,150	1,150	1,150	1,137	1,137
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.17b: Effects of Camp+ vs. Camp on non-cognitive skills, measured by psychological scales - con'd

	General risk preferences		Positive risk preferences		Negative risk preferences		Patience2		Patience1	
ATT	0.067	0.073	0.086	0.109**	-0.145*	-0.150**	0.165***	0.191***	0.008	0.001
	(0.062)	(0.050)	(0.069)	(0.050)	(0.087)	(0.064)	(0.060)	(0.055)	(0.057)	(0.056)
Mean outcome, Camp	-.051	-.051	-.419	-.419	-.163	-.163	-.228	-.228	-.163	-.163
R-squared	.010	.248	.011	.396	.020	.298	.017	.182	.005	.162
Observations	1,148	1,148	1,150	1,150	1,150	1,150	1,146	1,146	1,144	1,144
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.18: Effects of camp by gender: Performance in the national Danish test in grade 8

	Language Comprehension			Decoding			Text Comprehension			Overall		
	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl
ITT	-0.033	-0.046	-0.025	0.017	0.035	0.003	0.065***	0.094***	0.038	0.017	0.033	0.006
	(0.043)	(0.044)	(0.050)	(0.024)	(0.027)	(0.029)	(0.021)	(0.025)	(0.029)	(0.028)	(0.031)	(0.031)
Mean outcome, Control	.024	.044	.003	.000	-.095	.100	-.057	-.158	.048	-.013	-.086	.062
R-squared	.186	.211	.190	.510	.512	.509	.520	.493	.555	.549	.538	.568
Observations	8,953	4,594	4,359	8,953	4,594	4,359	8,953	4,594	4,359	8,953	4,594	4,359
ITT-NR	-0.046	-0.070	-0.023	0.014	0.028	-0.005	0.090***	0.084**	0.103**	0.024	0.022	0.037
	(0.045)	(0.054)	(0.063)	(0.028)	(0.034)	(0.045)	(0.029)	(0.037)	(0.039)	(0.033)	(0.041)	(0.040)
LATE	-0.094	-0.142	-0.046	0.028	0.057	-0.010	0.184***	0.172**	0.210**	0.050	0.046	0.075
	(0.092)	(0.110)	(0.129)	(0.058)	(0.070)	(0.092)	(0.059)	(0.077)	(0.081)	(0.067)	(0.084)	(0.082)
Mean outcome, Control	-.233	-.183	-.305	-.459	-.483	-.425	-.574	-.585	-.559	-.519	-.512	-.528
R-squared	.213	.246	.210	.431	.454	.427	.342	.347	.381	.442	.461	.460
Observations	3,418	2,028	1,390	3,418	2,028	1,390	3,418	2,028	1,390	3,418	2,028	1,390
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.19: Effects of camp by gender: Performance in the national math test in grade 8

	Numbers and Algebra			Geometry			Statistics and Probability			Overall		
	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl
ITT	0.068**	0.037	0.089**	0.043	0.045	0.038	0.063	0.042	0.090**	0.059*	0.045	0.076*
	(0.032)	(0.036)	(0.040)	(0.035)	(0.043)	(0.040)	(0.039)	(0.047)	(0.042)	(0.034)	(0.040)	(0.039)
Mean outcome, Control	-.064	-.043	-.086	-.042	-.020	-.065	-.033	-.039	-.028	-.050	-.036	-.064
R-squared	.640	.669	.614	.607	.629	.587	.601	.620	.587	.706	.729	.691
Observations	4,283	2,197	2,086	4,283	2,197	2,086	4,283	2,197	2,086	4,283	2,197	2,086
ITT-NR	0.094**	0.087*	0.078	0.091*	0.109**	0.067	0.117*	0.087	0.173***	0.100**	0.106*	0.123**
	(0.047)	(0.051)	(0.065)	(0.046)	(0.054)	(0.057)	(0.062)	(0.068)	(0.064)	(0.050)	(0.057)	(0.060)
LATE	0.192**	0.178*	0.160	0.185*	0.223**	0.138	0.240*	0.178	0.354***	0.204**	0.216*	0.251**
	(0.096)	(0.104)	(0.133)	(0.093)	(0.111)	(0.118)	(0.128)	(0.138)	(0.130)	(0.102)	(0.117)	(0.122)
Mean outcome, Control	-.645	-.596	-.719	-.625	-.569	-.708	-.619	-.585	-.672	-.678	-.628	-.754
R-squared	.452	.496	.410	.452	.517	.357	.460	.485	.472	.550	.586	.479
Observations	1,581	952	629	1,581	952	629	1,581	952	629	1,581	952	629
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes:***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome for math only exists for round 3. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.20: Effects of camp by SES and academic performance: Performance in the national Danish test in grade 8

	Language Comprehension			Decoding			Text Comprehension			Overall		
	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.
ITT	-0.033	-0.077	-0.080	0.017	0.006	-0.037	0.065***	0.132***	0.100**	0.017	0.016	-0.002
	(0.043)	(0.066)	(0.057)	(0.024)	(0.036)	(0.041)	(0.021)	(0.035)	(0.045)	(0.028)	(0.043)	(0.045)
Mean outcome, Control	.024	-.044	-.459	.000	-.169	-.720	-.057	-.354	-.840	-.013	-.232	-.827
R-squared	.186	.241	.228	.510	.487	.370	.520	.480	.253	.549	.522	.372
Observations	8,953	1,861	1,664	8,953	1,861	1,664	8,953	1,861	1,664	8,953	1,861	1,664
ITT-NR	-0.046	-0.061	-0.080	0.014	0.034	-0.037	0.090***	0.133**	0.100**	0.024	0.056	-0.002
	(0.045)	(0.077)	(0.057)	(0.028)	(0.048)	(0.041)	(0.029)	(0.052)	(0.045)	(0.033)	(0.054)	(0.045)
LATE	-0.094	-0.124	-0.164	0.028	0.071	-0.076	0.184***	0.273**	0.205**	0.050	0.114	-0.004
	(0.092)	(0.158)	(0.116)	(0.058)	(0.098)	(0.084)	(0.059)	(0.106)	(0.093)	(0.067)	(0.111)	(0.092)
Mean outcome, Control	-.233	-.286	-.459	-.459	-.544	-.720	-.574	-.697	-.840	-.519	-.625	-.827
R-squared	.213	.286	.228	.431	.460	.370	.342	.386	.253	.442	.471	.372
Observations	3,418	978	1,664	3,418	978	1,664	3,418	978	1,664	3,418	978	1,664
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.21: Effects of camp by SES and academic performance: Performance in the national math test in grade 8

	Numbers and Algebra			Geometry			Statistics and Probability			Overall		
	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.
ITT	0.068**	0.058	0.125**	0.043	0.061	0.135**	0.063	0.094	0.154*	0.059*	0.081*	0.151**
	(0.032)	(0.047)	(0.055)	(0.035)	(0.047)	(0.059)	(0.039)	(0.060)	(0.082)	(0.034)	(0.048)	(0.064)
Mean outcome, Control	-.064	-.313	-.917	-.042	-.342	-.910	-.033	-.352	-.927	-.050	-.362	-.988
R-squared	.640	.572	.383	.607	.603	.364	.601	.616	.430	.706	.676	.466
Observations	4,283	787	818	4,283	787	818	4,283	787	818	4,283	787	818
ITT-NR	0.094**	0.136	0.125**	0.091*	0.195***	0.135**	0.117*	0.230**	0.154*	0.100**	0.197**	0.151**
	(0.047)	(0.082)	(0.055)	(0.046)	(0.066)	(0.059)	(0.062)	(0.088)	(0.082)	(0.050)	(0.077)	(0.064)
LATE	0.192**	0.278	0.256**	0.185*	0.399***	0.277**	0.240*	0.471**	0.316*	0.204**	0.403**	0.309**
	(0.096)	(0.168)	(0.113)	(0.093)	(0.136)	(0.121)	(0.128)	(0.180)	(0.167)	(0.102)	(0.158)	(0.130)
Mean outcome, Control	-.645	-.734	-.917	-.625	-.756	-.910	-.619	-.819	-.927	-.678	-.829	-.988
R-squared	.452	.471	.383	.452	.508	.364	.460	.583	.430	.550	.591	.466
Observations	1,581	399	818	1,581	399	818	1,581	399	818	1,581	399	818
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes:***pÇ0.01; **pÇ0.05; *pÇ0.1. Outcome for math only exists for round 3. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.22: Effects of Camp+ vs. Camp by gender: Performance in the national Danish test in grade 8

	Language Comprehension			Decoding			Text Comprehension			Overall		
	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl
ATT	0.081	0.120	-0.068	0.046	0.073	-0.060	0.071	0.012	0.071	0.078	0.088	-0.022
	(0.076)	(0.089)	(0.101)	(0.047)	(0.062)	(0.062)	(0.048)	(0.056)	(0.060)	(0.051)	(0.066)	(0.067)
Mean outcome, Camp	-.373	-.407	-.311	-.622	-.679	-.518	-.704	-.753	-.615	-.700	-.758	-.595
R-squared	.211	.253	.325	.426	.435	.449	.376	.450	.365	.432	.459	.498
Observations	1,316	851	465	1,316	851	465	1,316	851	465	1,316	851	465
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.23: Effects of Camp+ vs. Camp by gender: Performance in the national math test in grade 8

	Numbers and Algebra			Geometry			Statistics and Probability			Overall		
	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl	All	Boy	Girl
ATT	-0.030	-0.083	0.012	-0.040	-0.062	-0.046	-0.021	-0.080	0.050	-0.037	-0.087*	-0.003
	(0.042)	(0.053)	(0.062)	(0.047)	(0.053)	(0.066)	(0.047)	(0.052)	(0.079)	(0.046)	(0.052)	(0.059)
Mean outcome, Camp	-.710	-.619	-.876	-.707	-.603	-.897	-.669	-.593	-.806	-.751	-.653	-.928
R-squared	.437	.451	.501	.414	.409	.539	.428	.472	.432	.512	.512	.595
Observations	1,321	856	465	1,321	856	465	1,321	856	465	1,321	856	465
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.24: Effects of Camp+ vs. Camp by SES and academic performance: Performance in the national Danish test in grade 8

	Language Comprehension			Decoding			Text Comprehension			Overall		
	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.
ATT	0.081	0.031	-0.013	0.046	-0.065	0.038	0.071	0.028	0.081	0.078	-0.028	0.039
	(0.076)	(0.125)	(0.084)	(0.047)	(0.076)	(0.049)	(0.048)	(0.080)	(0.067)	(0.051)	(0.074)	(0.064)
Mean outcome, Camp	-.373	-.419	-.491	-.622	-.625	-.804	-.704	-.751	-.901	-.700	-.740	-.905
R-squared	.211	.280	.189	.426	.481	.420	.376	.379	.340	.432	.483	.385
Observations	1,316	374	834	1,316	374	834	1,316	374	834	1,316	374	834
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

Table A.25: Effects of Camp+ vs. Camp by SES and academic performance: Performance in the national math test in grade 8

	Numbers and Algebra			Geometry			Statistics and Probability			Overall		
	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.	All	Low SES	Low perf.
ATT	-0.030	-0.055	-0.123**	-0.040	-0.127*	-0.135**	-0.021	-0.043	-0.090	-0.037	-0.079	-0.130**
	(0.042)	(0.065)	(0.050)	(0.047)	(0.065)	(0.054)	(0.047)	(0.071)	(0.054)	(0.046)	(0.060)	(0.051)
Mean outcome, Camp	-.710	-.841	-.889	-.707	-.836	-.835	-.669	-.814	-.843	-.751	-.896	-.923
R-squared	.437	.504	.349	.414	.500	.332	.428	.459	.356	.512	.597	.408
Observations	1,321	378	836	1,321	378	836	1,321	378	836	1,321	378	836
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***pÇ0.01; **pÇ0.05; *pÇ0.1. Baseline treatment category is Camp. The covariates is imputed with the value zero and a missing-indicator equal to one is added to the conditioning set if data on the covariates is missing. Standard errors in parentheses are clustered at the school level.

CHAPTER 2

BOYS LEFT BEHIND: THE EFFECTS OF SUMMER CAMP AND FOLLOW-UP STRATEGIES ON ACADEMIC, PERSONAL, AND SOCIAL COMPETENCIES

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Abstract

I use Danish administrative data to investigate the consequences of summer camp participation for disadvantaged boys on academic, personal, and social competencies. My identi cation strategy relies on individual level panel data that enables me to observe outcomes before and after summer camp participation. Using a difference-in-differences strategy, I nd overall positive effects on academic and personal competencies that reduce the gap to a matched group of boys with similar background characteristics by 40 to 80 percent. Further, I exploit a structural change in the follow-up program to evaluate how different mentoring strategies affect outcomes. In 2017, the follow-up program was changed from individual mentoring to group mentoring, which allows me to compare camp effects before and after the structural change. Using a triple differences strategy, I nd that group mentoring in the follow-up program dramatically improves personal and social competencies, indicating that the camp itself affects academic abilities whereas the format of the follow-up program is crucial for effects on personal and social competencies.

Keywords: Intensive learning camps, Follow-up, Mentoring, Non-cognitive competencies, Difference-in-differences

JEL Codes:I21, I24, I28, C23

2.1 Introduction

This paper studies the consequences of summer camp participation and the structure of the follow-up program for disadvantaged boys in lower secondary education. During the last decades, there has been a rapid and substantial reversal of the gender gap in educational attainment in much of the developed world (Murnane, 2013). The OECD (2013) report shows that, for 28 of 34 OECD counties, females have surpassed males in higher education among adults aged 25 to 34. Autor et al. (2019) conclude that boys in disadvantaged household have higher rates of disciplinary problems, lower academic performance, and lower rates of high school completion, than girls with similar backgrounds. Similarly, boys from low socioeconomic background have been hit the hardest by the disruption to education caused by the Covid-19 pandemic (Di Pietro et al., 2020). Thus, there is a need for effective remedial education programs that can eliminate this learning gap for disadvantaged boys.

Intensive learning programs are widely used as a policy tool to increase educational attainment and often implemented using summer camps (Kim and Quinn, 2013; Lauer et al., 2006; Cooper et al., 2000). The three meta-analyses report positive short-run effects on math and reading, but with very small effect sizes. Despite the overwhelming use of summer camps in the society, there is little causal evidence. Only a handful of studies rely on experiments or quasi-experiments and often with low statistical power. The most convincing evidence on summer camps exploit regression discontinuity designs (henceforth RDD) and standardized tests. Mariano and Martorell (2013) nd modest effects on language and little effect on math for low performing pupils. The RDD of Jacob and Lefgren (2004) nds positive effects on both reading and math but only for grade 3 pupils and not grade 6 pupils. Recent evidence from Battistin and Schizzerotto (2019) nds negative effects on academic performance of mandatory summer camp for at-risk pupils in Italy. Overall, there is weak evidence on summer camps effect for pupils in the transition from lower to upper secondary education and no evidence on summer camps effect on non-academic outcomes in general.

I use population-level Danish register data covering all grade 8 boys in the period 2015-2019 to study the effect of a two-week summer camp for academically disadvantaged boys with a one-year follow-up mentoring program. In particular, I exploit grade 8 and 9 individual level panel data to implement a difference-in-differences strategy. I ask, rst, what are the effects of participating in a summer camp on academic performance and readiness [1] for upper secondary education? Secondly, what are the consequences of changing the follow-up program from individual mentoring with an adult to a group-mentoring program of 10 to 12 boys?

The analyses deliver a set of noteworthy answers to the research questions: First, I show that participating in a two-week summer camp during the holiday between

[1] Indicator variable taking the value one if ready and zero otherwise. See section 2.3 for the description of the readiness assessment.

grades 8 and 9 has positive impacts on academic performance and readiness for upper secondary education. I observe an increase in the average assessment mark of around 15% of a standard deviation, suggesting that overall academic abilities have improved on average. Additionally, participation in the summer camp increases readiness for upper secondary education by 18 percentage points at the educational readiness assessment in December grade 9. The overall readiness assessment consists of three sub-categories, in which the pupils must qualify in order to be ready for upper secondary education. Interestingly, academic readiness is the main reason for the overall effect with an increase of 22 percentage points, whereas the summer camp increases the personal readiness assessment by 10 percentage points, and the effect on social readiness is not statistically signi cant. Critically, this estimation strategy combined with the introduction of a group-mentoring follow-up program in 2017 allows me to identify the effect of moving from individual mentoring with one adult to a group-mentoring follow-up program. Using a triple differences strategy, I nd that substituting individual mentoring to group mentoring increases both the personal and social readiness assessment by 15 and 16 percentage points and has no impact on academic readiness. This indicates that participating boys bene t both personal and socially from staying connected with equal peers and sharing experiences and solutions. Intuitively, it makes sense that mentoring these boys together should bene t their behavior but one might also fear that connecting challenged boys on a regular basic might increase poor behavior. Petrosino et al. (2013) show exactly this. They nd that crime preventive camps for children at risk of becoming delinquent do not work and in fact increases poor behavior.

Thus, my analysis shows that at-risk boys improve their academic, personal, and social competencies from participating in a two-week intensive learning camp during the summer holiday between grade 8 and 9. Importantly, I nd that the improvement in non-academic competencies is a result of shifting the follow-up program from individual mentoring to group mentoring. This nding is particular important, as it indicates that learning programs aimed at at-risk boys should focus on group mentoring in the follow-up program to boost the effects of the program.

I contribute to the literature on consequences of intensive learning camps. First, often the effect is measured shortly after the camp is completed. In this study, however, I exploit administrative data to investigate the effect up to one year after camp participation. Secondly, the educational readiness assessment allows me to supplement the estimated effects on academic outcomes with effects on measures of personal and social competencies directly related to future educational attainment. These outcomes are under-investigated in the previous literature, which is problematic when considering the importance of non-academic skills for future achievements (Almlund et al., 2011). Finally, the change in the follow-up strategy allows me to add novel evidence on how follow-up programs should be structured with the attempt to maximize the effect on all competencies that are considered relevant for future educational outcomes - and not focus exclusively on the academic competencies.

The paper is organized as follows: Section 2 provides background information on the structure of the summer camp. Section 3 describes the data and descriptive statistics and section 4 explains the empirical strategy. In section 5, I show the effect of the summer camp, section 6 investigates the effect of changing follow-up strategy, and nally, section 7 concludes.

2.2 The two weeks summer camp

The summer camp is a two-week intensive learning camp that takes place during the summer holiday between grades 8 and 9. It targets boys who are at risk of not becoming ready for upper secondary education at the end of compulsory school and, thus, not able to choose their desired education. This group comprises boys with a substantial academic backlog, low self-con dence, and low motivation for going to school. The main objective of the summer camp is to help these academically weak pupils to catch up such that they become prepared to continue in the education system. In order to achieve this object, the program focuses on academic abilities by training reading, writing, spelling, and math, but it also aims to develop socio emotional skills.

2.2.1 Content and organization

The recruitment into the summer camp occur by the boy in collaboration with their parent apply for the camp independently from their local school. The boys can apply from January and application deadline is medio March. The boys are noti ed of acceptance within a month from the deadline where all application is evaluated simultaneously. If the program is over-subscribed the boys are selected based on academic, personal and social problems.

The summer camp consists of an intro camp and a main camp. Approximately two weeks prior to the start of the main camp, pupils attend the intro camp during a weekend. The purpose of the intro camp is to introduce the teachers, structure, settings, and rules at the main camp as well as creating relationships to their peers in order to start the social connections and decrease some of the anxiety before the main camp. Additionally, there are individual conversations between the teachers and the boys for the teachers to get a greater insight into the boys' dif culties and potential. Finally, there is academic tests of abilities in order to organize the main camp learning plan according to the pupils' individual levels and needs (Andersen et al., 2019). The total cost for the summer camp, including the follow-up program, is approximately 5,000 USD per pupil.

The main camp takes place in the rst two weeks of the summer holidays. The boys are divided into teams of approximately 15 pupils, where two teams receive instruction together. The two-week camp consists of a full schedule from morning

to evening with morning wake-up call at 6.30 AM and bedtime at 10.00 PM. This ensures a xed structure that is repeated every day. The day consists of four 90-minute modules, which include the subjects Danish, mathematics, as well as modules focusing on socio emotional skills and plenty of physical activity. See Figure A.1 and A.2 for the schedules for the rst and second week of the main camp. The camp is located at a boarding school with teaching facilities, kitchen, and sleeping rooms. Thus, there is no need to leave the camp during the two weeks. The teachers and peers are different from the participant's local learning environment, mobile phones are only allowed one hour per day, and the camp has a zero-sugar policy. Importantly, the boys do not miss any teaching at their home school, because the intro camp is held during a weekend and the main camp during the summer holiday.

The pedagogical aim of the summer camp is to offer boys teaching methods that, to a greater extent than at their home-school, are tailored the boys' individual needs and strengths. The intervention builds on inspiration from the literature on Visible Learning and Self-Determination Theory (Hattie and Yates, 2013; Ryan and Deci, 2000). The teaching itself is highly structured around Flipped classrooms, where blackboard teaching is replaced by e.g. short videos. The boys watch the videos individually to free up the teacher's time for more student-activating teaching. An additional advantage with this method is that this form of teaching also means that the boys can revisit the material after the camp. The teachers at the camp attempt to turn around the negative school experiences by testing the boys midway through the camp and at the nal day. Thus, they visually illustrate the boys' academic progress to recreate a positive self-narrative as well as the motivation to learn. A clear pedagogical tool of the learning camp is to believe in the boys by praise and acknowledge them for their progress and to support them during the dif cult periods.

2.2.2 The follow-up strategy

Receipt of educational activities in a new environment, away from the home-school peers and teachers, is a key element of the summer camp. The intention is to break bad habits and make new social connections, exploiting that all boys are on common ground by not knowing each other beforehand. This is an obvious strength of the learning program but at the same time, it also constitutes one of the biggest challenges. Andersen et al. (2019) show that many pupils nd it dif cult to maintain the good working habits, the positive academic development, and the joy of learning when they return to their ordinary classroom after the camp. The summer camp is perceived as being detached from everyday school life and, thus, returning to the local environment risks a setback when the boys discover that nothing has changed in how their peers and local teachers perceive them. Therefore, the summer camp has a great focus on follow-up after the camp. The primary follow-up program is a mandatory one-year mentoring scheme with a built-in parenting effort. Additionally, the camp teachers construct an “Exit package” for all boys, which is a written handover to the

boys' local schoolteachers. However, only 50% of the local teachers have heard about the exit package and fewer have used it (Rambøll, 2019). There is no additional formal contact between the camp teachers and the local teachers.

In 2015 and 2016, the mentoring scheme was an individual mentor-mentee program, where the boy and the adult mentor can train academic as well as non-academic skills during meetings twice a month. Additionally, the mentors can help the boys set new learning goals and maintain their positive development. In 2017, the mentoring scheme was changed to mentor groups of 10-12 boys, who meet twice a month, outside regular school hours, at mentor centers geographically spread across Denmark. At the centers, the boys receive academic and personal counseling and guidance from adults who have attended the summer camp and volunteering role models. Furthermore, the content of the summer is repeated at each meeting, they share experiences related to returning to the local learning environment, and they receive homework help and educational guidance.

2.3 Data and descriptive statistics

To investigate the effect of the summer camp and how changes in the follow-up strategies affect the outcomes of participants, I leverage Danish administrative register data available through Statistics Denmark covering the full population of pupils in the Danish school system. Focal to this study is the Danish Student Register comprising all educational choices in Denmark. This register is a unique longitudinal dataset that allows me to follow schooling information such as private vs public schooling, school and classroom movements, and special needs teaching from 2008/2009 to 2019/2020. Crucially, this data is informative about what grade a boy attends, enabling me to observe the boys' academic development though their average assessment mark and their readiness assessment for upper secondary education at grades 8 and 9 obtained from the Ministry of Education. I augment this data with information on dyslexia, psychiatric diagnoses, results from national tests in reading and math, school absence, personality traits, and school well-being. Furthermore, I exploit socio economic information describing demographics, employment, income and educational level of the parents. The sample consist of 158,231 boys of which 241 participated at the summer camp between 2015 and 2019[2]. I study two groups of outcomes that both characterize key information regarding future life-trajectory. Speci cally, I include 1) the average assessment mark and 2) the readiness assessment for upper secondary education.

[2]The 241 boys are those who applied and showed up at the summer camp. The dropout rate is approximately 8%.

2.3.1 average assessment mark

The average assessment mark is an expression of the pupil's academic level averaged across all subjects the pupil attends. [3] The teachers grade the pupil in relation to the academic goals that apply for the subject. Thus, teachers base their assessment on the extent to which the pupil ful lls the subject's competence, skills and knowledge goals. Importantly, the pupil's work effort and/or behavior in the classroom is, generally, not included in the grading. Only if the subject has goals that include these abilities, are they included in the grading. The grading occur in December and June of grades 8 and 9.[4]

To measure the pupils' overall abilities, I standardize each continuous assessment mark within each subject and timing of the assessment to mean zero and standard deviation of one. Then, I calculate the average assessment mark and standardize within the timing of the assessment to mean zero and standard deviation of one. The latter standardization allows us to readily interpret regression coef cients in standard deviations units and, thus, render results comparable to effect sizes of other studies. The camp boys are on average 1.2 standard deviations below the non-camp boys at both grade 8 assessments, indicating that camp boys have a signi cant academic backlog.

2.3.2 Readiness assessment for upper secondary education

Assessing pupils' readiness in relation to choosing and completing upper secondary education is a process that starts in grade 8. The purpose of the assessment is to ensure that non-ready pupils receive school interventions and individual guidance in the process towards the end of grade 9 in order for them to make the best secondary education choice and be prepared for the education chosen. The readiness assessment includes all pupils in public and private schooling and takes place in December. The pupil's primary teachers assess the academic, personal, and social competencies of the pupil. All three criteria must be met for the pupils to be assessed as ready for upper secondary education. On average, 62% of the boys are assessed to be ready for upper secondary education, but for the camp participants in grade 8 only 12% are assessed to be ready for upper secondary education. This 50%-points difference clearly indicates that the camp participants need additional assistance in order to continue in the educational system.

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[3] In Danish, there are four grades (Reading, Spelling, Written, and Oral). In math, there are three grades (Math with aids, Math without aids, and Oral). In foreign languages there are two grades (Oral and Written). For the following mandatory subjects there are only one grade (Physics/chemistry, Biology, Geography, History, Social studies, Religion, and Gym). Finally, the pupils are graded in one of the following four electives (Crafts and design, Food knowledge, Music, and Art).

[4] The June grade 8 grading is initiated in 2018 and do not exists for the previous years.

In order to be academically ready in grade 8, the pupil must have an average assessment mark of at least 4.0[5] for going to vocational training and two-year high school and 5.0 for a three-year high school. In grade 9, the requirements for vocational training drops to an average of 2.0 in Danish and math. In grade 8, 75% are academically ready for their desired educational choice, whereas this fraction is only 19% for the camp participants.

For the personal readiness assessment, the teachers assess whether the pupil has the necessary personal competencies to begin upper secondary education after grade 9. The teachers have ve key areas to guide them through the assessment: 1) Motivation, 2) Independence, 3) Responsibility, 4) Meeting stability, and 5) Choice readiness. Motivation is about having a desire for education and learning, and contributing actively in teaching. Independence is about being able to act on your own, i.e. the pupil is able to take initiatives and to ask for help when needed. Responsibility is whether the pupil shows up prepared for classes and whether the pupil is able to keep appointments that he or she makes with teachers or fellow pupils. Meeting stability is whether the pupil attends school every day and does so timely. Finally, choice readiness is about whether the pupil can make decisions and whether the pupil is able to make a positive and active choice in the educational selection process. Often a pupil will not be able to make the educational choice in grade 8, but they must be able to re ect on it. In grade 8, 74% of all boys are assessed personally ready, while the fraction is 40% for the camp participants. The camp boys are thus still behind the average boy but the difference is not as great as for the academic readiness assessment.

The social readiness assessment focuses on whether the pupil has the social prerequisites needed to be able to start and complete upper secondary education. In order to assess this, the teachers have three focus points to guide them: 1) Collaboration ability, 2) Respect, and 3) Tolerance. Collaboration ability is about being able to solve tasks together with others, to keep common agreements and contribute positively to the community. Respect is about the pupil being able to show consideration for other pupils and teachers. Tolerance is the ability or willingness to accept what is unknown. Thus, tolerance is about being able to understand and accept other people's opinions, behavior, culture, religion, etc. On average, 82% of all boys are assessed to be socially ready, while 56% of the camp boys are socially ready.

Hence, clearly, the large difference in the overall readiness assessment is driven by academic performance. A large fraction of the camp participants does also have personal and social problems but not in the same magnitude as the academic backlog.

[5] Academic grading in Denmark is on a 7-point scale with the following grades from the best to the worst: 12(A), 10(B), 7(C), 4(D), 02(E), 00(FX), -3(F).

2.3.3 Characteristics of camp vs non-camp boys

Leveraging Danish administrative data allows me to characterize in detail the differences between camp and non-camp boys. In column 1 to 5 of table A.1, I present summary statistics for a rich set of covariates, recorded prior to camp participation. The descriptive statistics show signi cant differences between the two groups. There is an under-representation of boys with non-western ethnicity in the camp group. They are more likely to live in broken families, have additional relocations, and thus an increased number of school changes. The camp boys have received on average half a year more special needs teaching, they are over-represented in terms of psychiatric diagnoses, and 35% of them have been diagnosed with dyslexia, whereas only 10% of the non-camp group are dyslexics. They grow up in households with lower socioeconomic status, with both parents having approximately half a year less of education. Academically, the camp group perform signi cantly worse in the national test in reading and math across all grades and pro le areas with gaps to non-camp boys between 0.5 and 1.0 standard derivations. The camp participants on average rate their school well-being lower, especially when assessing their learning self-ef cacy. I observe similar ndings across all three measured personality traits, the participants having lower levels with conscientiousness showing the largest difference of 0.5 standard derivation.

2.4 Empirical strategy

2.4.1 Identifying the consequences of summer camp participation

The rst goal of this paper is to estimate the consequences of summer camp participation for all camp boys [6] on pupil-level outcomes. The key challenge in any program evaluation is to estimate the counterfactual, in the present case the outcomes in the absence of summer camp participation. A natural worry is that selection bias challenges the identi cation, i.e. summer camp participating boys comprise a different population compared to the remaining population of boys who do not participate in the summer camp.

I address this concern with a difference-in-differences strategy using individual level panel data similar to an individual xed effect analysis. This strategy compares the change in participating pupil's outcomes from grade 8 to grade 9 to the similar change for non-participating pupils. This strategy implicitly controls for unobserved time-invariant individual school performance. However, it is likely that school performance develops differently based on the underlying distribution of covariates. To account for this, I combine the difference-in-differences strategy with Entropy Balancing along the lines of Freier et al. (2015). The basic idea is to reduce bias due

[6] I pool together all grade 8 boys in the school years 2014/2015 to 2018/2019 when estimating the overall consequences of summer camp participation.

to different distributions of covariates in the camp vs non-camp groups by matching the non-camp group such that it is identical to the camp group with respect to included characteristics (Heckman et al., 1997; Blundell et al., 2004; Abadie, 2005). Entropy Balancing is a data processing method introduced by Hainmueller (2012) that ensures perfect covariate balance with a binary treatment variable. See Hainmueller and Xu (2013) and Hainmueller (2012) for a detailed description of Entropy Balancing. Columns 6 to 8 in table A.1 show descriptive statistics of the observed covariates for the Entropy Balancing adjusted non-camp group. Column 8 shows that the means in the camp and non-camp groups are perfectly balanced across the full set of covariates.

I start the analyses with the following difference-in-differences equation:

$$y_{it} \text{ÆÆ} u_i \text{Å}^{-}{}_0 \text{Å}^{-}{}_1 \text{camp}_i \text{Å}^{-}{}_2 \text{Time}_i \text{Å}\pm_j (\text{Camp}_i \text{¢Time}_i) \text{Å}^{2}{}_{it} \tag{2.1}$$

where y_{it} is the outcome of interest, camp_i is a binary variable indicating summer camp participation, and Time_i is a time-period categorical variable containing two periods before and after camp participation for the average assessment mark and one for the readiness for upper secondary education assessment. $\pm_j$ are the effects of summer camp participation by time-periods relatively to December in grade 8 and are the parameters of interest, i.e. the average treatment effect of the treated (ATT henceforth). u_i is individual level xed effect and $^{2}{}_{it}$ is the error term. Standard errors are clustered at the individual level as outlined in Bertrand et al. (2004).

The key identifying assumption in the difference-in-differences strategy is that there can be no differential trends between the camp and non-camp groups in the absence of summer camp participation. To investigate the validity of this assumption, I rst observe pre-camp trends for the average assessment mark and then test the robustness of my ndings to different model speci cations, alternative comparison groups, alternative balancing speci cations, and by performing placebo analyses prior to camp participation and on a randomly selected, synthetic, “camp” group. Signi cant and large placebo effects would imply that the identi cation strategy does not capture systematically different trends in pre-camp school performance between camp and non-camp boys.

2.4.2 Detecting the consequences of transforming the follow-up program

The second goal of this paper is to estimate the impact of transforming the follow-up program - the 2017 structural change in the mentoring scheme - on pupil-level outcomes. Individual mentoring is substituted with group mentoring in the one-year mandatory follow-up program. The effect of this change can be estimated using a triple differences estimator [7]. This is equivalent to the difference between the 2015 to

[7] Individual xed effect estimates is no longer possible because none of the boys have participated more than once in the summer camp.

2016 and 2017 to 2019 difference-in-differences estimates and is estimated using the following equation:

$$y_i = \beta_0 + \beta_1 Camp_i + \beta_2 Post_t + \beta_3 Group_i + \beta_4 Camp_i \times Post_t + \beta_5 Camp_i \times Group_i + \beta_6 Post_t \times Group_i + \beta_7 Camp_i \times Post_t \times Group_i + \beta_8 X_i + \varepsilon_i \quad (2.2)$$

where y_i is the outcome of interest, $camp_i$ is a binary variable indicating summer camp participation, $Post_t$ is an indicator variable with the value one (zero) after (before) summer camp participation, $Group_i$ takes the value one for boys participating in the group mentoring scheme and zero for boys in the individual mentoring scheme, X_i is a matrix containing covariates measured prior to camp participation, and ε_i is the error term. β_7 is the effect of summer camp participation with the group mentoring follow-up program relatively to individual mentoring and is the parameter of interest.

Despite that the triple differences estimator can be computed as the difference between two difference-in-differences estimators, Gruber (1994) states that the identifying assumptions are weaker. Olden and Møen (2020) formally show this by proving that the triple differences estimator does not require two parallel trends assumptions in order to estimate causal effects. It requires only one parallel trends assumption to hold to provide causal interpretation. Thus, the difference between two biased difference-in-differences estimators will not be biased if the bias is the same in both estimators because the bias will be removed with triple differencing.

2.5 Results: Effect of camp participation on school performance outcomes

I start with a graphical analysis of the effects on the average assessment mark measured four times across grade 8 and 9. Here I use camp participation for all the years 2015-2019. Figure 2.1 shows the event study representation of summer camp participation on the average assessment mark with December grade 8 as reference point. The solid line presents the unadjusted event study (raw difference-in-differences estimates) and the dashed line presents the entropy balancing adjusted event study. Critically, there is no difference in effect between December and June in grade 8, consistent with parallel trend prior to summer camp participation. Additionally, the adjusted and unadjusted treatment effects are very similar across time, indicating that the different distribution of covariates does not affect the estimated effect of camp participation. Overall, I find effects of 15% of a standard deviation both 6 and 12 months after summer camp participation, suggesting that the effects on academic grades are persistent up to a year after the intervention. Participating in the two-week summer camp reduces the learning gap to the full population of boys by 14% and by 40% when compared to boys with similar characteristics.

A worry might be that schoolteachers who are grading the boys in the average assessment mark might be in uenced by the knowledge of who has participated in the summer camp. Therefore, I exploit data from the grade 9 school leaving exam in June that is graded by external censors to investigate if this changes the results. Figure A.3 shows the Entropy Balancing adjusted event study representation, where the average assessment mark in June of grade 9 is substituted with the exam performance. This worry does not seem to be valid since using exam performance does not change the ndings. To investigate this further I use the written exams because these are completely independent of the teachers. Figure A.4 supports the conclusion that the teachers' believes of the camp do not in uences the effects.

For simplicity and similarity in presentation with the readiness assessment for upper secondary education, which is only registered once in grade 8 and 9, I supplement my event study model with a simple difference-in-differences model that ignores the time to treatment aspect, i.e. it does not allow effects to vary with time distance to summer camp. I also conduct robustness checks using this simpler difference-in-differences model. This is not critical for my ndings, conducting the robustness checks on the main event study model yields similar ndings. Table A.2 shows in column 1 to 3 the unadjusted difference-in-differences estimations and in column 4 to 6 the Entropy Balancing adjusted estimations. I present the raw estimations without covariates in Columns 1 and 4, in columns 2 and 5 I include covariates, and in column 3 and 6 I exploit the panel structure of the data and conduct an individual xed effects analysis. The table shows that the ndings are robust to using a gradually richer speci cation.

Although the estimates are statistically signi cant it is important in a policy perspective to discuss if they are are economically signi cant. Kraft (2020) discusses how to interpret effect sizes of educational interventions using 750 randomized trials. He argues that effects of 15% of a standard deviation are of medium size when benchmarked against a large set of educational interventions conducted in social science. However, when taking into consideration the age of the boys in this study, the effect on average assessment mark is around the 70 percentile of the distribution of effect sizes included in Kraft (2020). In a Danish context, this summer camp is also very competitive when compared to other lower secondary school interventions (Rosholm et al., 2021).

Table 2.1 explores the impact of summer camp participation on the readiness assessment for upper secondary education. The results show that participation in the summer camp leads to a considerable increase in readiness for desired upper secondary education. Participating boys increase their readiness by 18 percentage points relatively to non-camp boys. Remarkably, the summer camp reduces the gap to the full population of boys by 35% and by 80% to the entropy balance adjusted boys with similar characteristics. Columns 1 to 3 and 4 to 6 show that the ndings are robust to using a gradually richer speci cation and combination with entropy balancing.

Figure 2.1: Summer camp and average assessment mark

Notes: This gure shows the main event study representation of the effect of summer camp participation on average assessment mark. The solid line presents the unadjusted event study and the dashed line presents the Entropy Balancing adjusted event study. Each point represents the assessment difference in average assessment mark between camp and non-camp boys with 95% con dence intervals. I use the assessment in December grade 8 as reference time-point.

This result is consistent with the increase in the average assessment mark, which begs the question, if the increased readiness for upper secondary education is purely driven by an increase in academic abilities. Tables A.3 to A.5 show the effect of summer camp participation on the three criteria used in the readiness assessment. Clearly, the summer camp has the largest effect on academic competencies with an increase of 22 percentage points. For personal competencies the effect is borderline signi cant and show an increase by 11 percentage points in the teacher assessed personal readiness. Finally, the effect on social competencies is positive, however, not statistically signi cant. However, as described in section 3 the gap prior to summer camp is also largest for the academic competencies. Thus, there is more room for improvement and maybe a larger focus on academic problems at the summer camp.

An important worry for the educational readiness assessment is that camp boys are able to change their desired upper secondary education between grade 8 and 9, and that this may cause the positive effects. I.e. if the boys change from a 3-year high school to a vocational school, the requirements for being assessed ready are reduced. This does not seem to be a problem, as illustrated in table A.6; there are no effects for

Table 2.1: Effects of summer camp on overall readiness assessment

	Difference-in-difference			DiD & Entropy balance		
	(1)	(2)	(3)	(4)	(5)	(6)
Camp	-0.504	-0.251		-0.220	-0.221	
	(0.022)	(0.023)		(0.022)	(0.019)	
Post	0.147	0.139	0.139	0.206	0.190	0.193
	(0.001)	(0.001)	(0.002)	(0.004)	(0.005)	(0.006)
Camp X Post	0.223	0.214	0.230	0.165	0.172	0.176
	(0.038)	(0.038)	(0.060)	(0.038)	(0.037)	(0.060)
Observations	277,043	277,043	277,043	277,043	277,043	277,043
R-squared	0.026	0.352	0.820	0.119	0.359	0.763
Mean outcome, grade 9	0.776	0.776	0.776	0.522	0.522	0.522
Pupil background chars	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes

Notes: This table presents the results for six separate difference-in-difference speci - cations comparing camp participants to non-participants. Columns 1 to 3 display the simple difference-in-difference speci cations, and columns 4 to 6 show the results from the difference-in-difference combined with entropy balancing. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

the desired types of upper secondary education.

I also investigate the robustness of my ndings to alternative comparison groups and balancing strategies. Using alternative comparison groups' works as further tests of the difference-in-differences combined with Entropy Balancing. I exploit that boys must apply for summer camp participation themselves and this is potentially related to local school interventions. Thus, if the local school does not have suf cient programs for the boys, they and their parents might be more inclined to apply for the summer camp in order to adjust for their local schools limitations. Therefore, I test the sensitivity of the results by using only boys from school that have camp participants enrolled, as well as schools that never sends pupils to the summer camp. The estimates in table A.7 indicate that the ndings do not change. Comparing camp participants to boys from their local schools or from different schools yields estimates with the same sign and magnitude as in the main speci cation. Additionally, I perform robustness analyses of the re-weighting approach in table A.8 to investigate if the

ndings are robust to alternative balancing methods. The rst column of the table presents results from the main speci cation and balance on the rst moment, column 2 balances on the rst and second moments (mean and variance), and column 3 balances on three moments (mean, variance, and skewness). Columns 4 to 6 use logistic regression to perform propensity score matching with 3, 5, and 10 non-camp boys, respectively. Overall, the estimates have similar magnitudes and signs as the main speci cation, indicating that the analysis is robust to alternative balancing methods.

Finally, I investigate the parallel trends assumption using placebo tests. First, I perform a placebo test using a fake treatment group on the main outcomes. The fake treatment group is a random subset of boys not affected by the program. Thus, estimates different from zero indicate a violation of the identi cation strategy. Table A.9 shows that all estimates are essential zero. Another falsi cation test implements a fake camp date in the summer holiday between grades 5 and 6 for the boys participating in the camp between grades 8 and 9. To do so, I exploit the national tests in reading and math as additional outcomes. This fake camp date occurs prior to the real summer camp, and therefore the difference-in-differences strategy should nd insigni cant estimates close to zero in order to reject any underlying difference in trends between the camp and non-camp groups. Tables A.10 to A.12 show small and insigni cant estimates using national tests in reading, grade 6 and 8, and math grade 6, indicating that the camp and non-camp group do not behave differently prior to the summer holiday between grade 8 and 9, at least not academically. An important worry is that the camp boys are more motivated to change their behavior than those who do not participate. Table A.11 show the effects on the national reading score in grade 8, which is measured after the boys have applied for the summer camp but prior to the summer camp. Thus, if motivation is driving the main effects we would expect to see signi cant results in this table. However, all estimates are insigni cant and close to zero. To further investigate this change in behavior I construct similar placebo tests using absence, personality traits, and school well-being. Tables A.13-A.15 analysis if there is any differences from grade 7 to grade 8. This is particular interesting because behavior changes prior to summer camp could bias the ATT estimates. Absence data is measured though out the whole schoolyear whereas personality traits and school well-being is measured in the spring and similar to the national reading test this survey is conducted in the end or after the application deadline but prior to noti cation of enrollment at the summer camp. The tables show no evidence of increased motivation with the individual xed effect estimates being statistically insigni cant. This indicates no systematic change between the two groups leading up to the camp, which supports the main results.

2.6 Results: Individual vs group mentoring

In this section, I estimate the effect of the change from individual to a group mentoring strategy in the follow-up program. As a rst indication, I present a visual illustration of the difference-in-differences results from the two school years with an individual mentoring against the later three years with group mentoring. Overall, gure 2.2 shows larger effects under the group mentoring follow-up program across all outcomes. The effect of camp participation on continuous assessment mark is positive and signi cant in both mentoring regimes. However, the effect nearly doubles from implementation of group mentoring. Interestingly, during the years of the individual mentoring program there are no effects on the personal and social readiness assessment whereas the introduction of group mentoring substantially increases these effects to 19 and 15 percentage points, respectively. For academic readiness, the difference-in-differences estimates are signi cantly different from zero under both the individual and group mentoring follow-up program with the latter being slightly larger. These differences in readiness assessments in different dimensions are also detectable in the overall readiness assessment, in which the difference-in-difference results increase from 12 percentage points to 22 percentage points from changing the individual mentoring program to a group mentoring program during the follow-up year.

The obvious question is whether these effects are statistically signi cant from each other. Therefore, I investigate the consequences of the follow-up mentoring strategy using the triple differences model described in equation (2.2). Table 2.2 presents the triple differences estimates, i.e. the effect of substituting the individual mentoring program with a group mentoring program. The rst column shows that the effect of camp participation on continuous assessment mark is 8% of a standard deviation larger when the follow-up program consist of group mentoring. The difference is, however, not signi cantly different. For the educational readiness assessment, the table shows overall positive results with the effects on the personal and social readiness assessment being statistically signi cant. Substitution from the individual mentoring follow-up program to a group mentoring follow-up program increases the fraction of boys who are assessed personally ready by 15 percentage points and socially ready by 16 percentage points. These results imply that, in terms of personal and social readiness, camp boys reduce the gap to the average boy by up to 60%. This is an important nding because a major problem with intensive learning camps is the transition back into the local school environment. When returning to the local school and classroom, many boys perceive that “nothing has changed” and therefore, their school behavior is likely to revert to how it was before camp participation. However, the correct mentoring strategy in the follow-up program seems able to reduce this problem and avoid the complete fade-out in teacher assessed personal and social competencies. This is crucial if we want to hope for long-run effects of educational learning programs on lifetime success (Kautz et al., 2014).

Figure 2.2: Difference-in-difference estimates by follow-up program

Notes: This gure presents the difference-in-difference effects of participating in the summer camp on outcomes separated by individual (2015-2016) and group (2017-2019) mentoring with 95% con dence intervals. GPA is the average assessment mark and is measured as standardized effect sizes. The remaining outcomes stems from the readiness assessment and can be interpreted as percentage points by multiplying with 100.

The critical assumptions for the triple differences estimates to be causal is the parallel trends between the two difference-in-differences estimates. Entropy balancing ensures perfect balance across all covariates and the large set of robustness analyses conducted in the previous section show no indication of violation of the difference-in-differences parallel trend assumption. Data restrictions - educational readiness assessment only performed once in grade 8 and 9 and the continuous assessment mark for June in grade 8 is rst registered in 2018 - makes visualization of pre-trends impossible. However, as in the above section, I perform placebo test with fake camp group and fake camp date. Table A.16 presents the triple differences results for the fake camp group, which are all insigni cant. Tables A.17 and A.18 investigate academic pre-trends exploiting the national reading and math tests and a fake camp date in the summer holiday between grade 5 and 6. All triple differences estimates are insigni cant, indicating similar pre-trends between the two difference-in-differences models. Additionally, the selection process to the summer camp has not changed as illustrated in table A.19. This all indicates that the changing effects on personal and social competencies are caused by the change in the follow-up program.

Table 2.2: Triple differences: Effects of group mentoring instead of individual mentoring

		Educational readiness assessment			
	GPA	Overall	Personal	Social	Academic
	(1)	(2)	(3)	(4)	(5)
DiDiD	0.082	0.097	0.154	0.155	0.062
	(0.065)	(0.076)	(0.076)	(0.063)	(0.081)
Observations	479,540	277,043	263,480	263,153	265,354
R-squared	0.565	0.362	0.268	0.261	0.418
Pupil background chars	Yes	Yes	Yes	Yes	Yes
Parental background chars	Yes	Yes	Yes	Yes	Yes
Reading abilities	Yes	Yes	Yes	Yes	Yes
Math abilities	Yes	Yes	Yes	Yes	Yes
Absence information	Yes	Yes	Yes	Yes	Yes
School well-being	Yes	Yes	Yes	Yes	Yes
Personality traits	Yes	Yes	Yes	Yes	Yes

Notes: This table shows triple difference (DiDiD) estimates by comparing camp participants attending the individual mentoring program with those attending group mentor centers in the year following the camp. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

2.7 Conclusion and discussion

I studied how summer camps targeted academically disadvantaged boys with lack of school motivation affect school performance and readiness for upper secondary education in Denmark. I did so by utilizing how boys' outcomes evolved from grade 8 to grade 9. Using a difference-in-differences strategy, I compared outcomes among those who participated in the two-week summer camp to those who did not. Additionally, I investigated – exploiting a 2017 structural change in the one-year follow-up program – how substituting individual mentoring with group mentoring affected outcomes. Using a triple differences strategy, I compared the difference-in-differences estimates before and after the change in the follow-up program.

I found large, positive effects of summer camp participation on the average assessment mark and on readiness for upper secondary education. Summer camp participation increased the average assessment mark by 15% of a standard deviation and overall readiness for upper secondary education by 18 percentage points. Thus, the summer camp reduced the gap to the average boy with up to 35% and to boys with similar characteristics with up to 80%. I found that the positive effect on the overall readiness assessment was mainly driven by increased academic competencies (22 percentage points), but the camp also increased personal competencies

(11 percentage points). Further, I saw an increase in the effects of summer camp participation on all outcomes when using group mentoring in the follow-up program instead of individual mentoring. In fact, substitution from the individual mentoring follow-up program to a group mentoring follow-up program signi cantly increased the fraction of boys who were assessed personally ready by 15 percentage points and socially ready by 16 percentage points. Thus, the change in follow-up strategy led to a dramatic increase in the effectiveness of the summer camp on non-academic competencies.

This study thus offers two key ndings; 1) summer camps are effective in improving adolescent boys' school outcomes and 2) implementing a group mentoring scheme in the follow-up program increases the effects. Interestingly, the results suggest that the two weeks of camp primarily increases academic competencies while the follow-up program supports personal and social competencies. Intuitively, it makes sense that group mentoring, where such competencies automatically are in play, is more effective than individual mentoring in improving social and personal competencies. Additionally, group mentoring has the advantage of being cheaper because of the lower adult to boy ratio. These ndings raise the question if a reduction in camp length and additional follow-up sessions with the mentor group, which would reduce the total cost of the intervention, is more cost-effective. A new similar one-week camp with similar follow-up program was, in the autumn holiday of 2021, commenced by the same organization that arrange the summer camp under study. Future research exploiting both the summer and autumn camp may be able to shed light on this question.

Kraft (2020) argues that, from a policy perspective, the effect sizes are important, but more important is the effect relative to program costs. Furthermore, he argues that learning programs are not as relevant if they cannot be taken to scale with high delity. This summer camp cost approximately 5,000USD per participant, which Kraft (2020) de nes as being in the lower end of a high-cost intervention. However, when accounting for the degree of disadvantage facing this group of adolescent boys, which the literature has shown is dif cult to affect, the effects of this summer camp appear quite impressive. There is naturally a limited number of participating boys at each camp, but with the comprehensive curriculum and xed structure, it would seem fairly straightforward to scale up without affecting the gains by conducting several camps simultaneously.

Overall, the ndings in this study are important for policymakers because they show that summer camp can be a relevant and effective tool for improving academic performance for disadvantaged boys who have fallen behind academically. Furthermore, with the large impact on readiness for upper secondary education, the summer camp has the potential to become a key instrument in making pupils ready for upper secondary education. Naturally, the cost of 5,000USD per participant has to be assessed against the bene ts of camp participation. For example, the reduction in cost to society of boys' not attaining secondary education or delaying it. Finally, the

many disruptions to education caused by e.g. the Covid-19 pandemic attens the learning curve and increases the socioeconomic learning gap. Di Pietro et al. (2020) show that these disruptions to a greater extent affect disadvantaged boys by stopping their learning process and in fact decrease their cognitive abilities. Using summer camps as a remedial educational program would have the potential to eliminate parts of this learning gap.

An important limitation with this study is the gender selection, i.e. the summer camp targets boys and the analysis provide no insight into its effect on girls. Thus, future research is needed on the effects of summer camp participation for disadvantaged girls. Another limitation to this study is methodological. The analysis builds on a difference-in-differences strategy using panel data. This requires parallel trends and removes the possibility to investigate long-run effects, such as educational attainment, future income, employment history, etc. Therefore, the next step is to study the summer camp using a randomized trial, which would enable long-term follow-up and require much less restrictive identifying assumptions.

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A.1 Appendix

Table A.1: Entropy Balancing Statistics

	Camp		Non-Camp					
			Unadjusted			Adjusted		
	Mean	SD	Mean	SD	Diff.	Mean	SD	Diff.
Pupil:								
- Age (years)	15.299	(0.459)	15.227	(0.419)	0.072	15.299	(0.458)	0.000
- Non-western etnicity (1/0)	0.046	(0.210)	0.090	(0.287)	-0.045	0.046	(0.209)	0.000
- Living with both parents (1/0)	0.556	(0.498)	0.646	(0.478)	-0.090	0.556	(0.497)	0.000
- Number of relocations	2.095	(1.723)	1.865	(1.616)	0.230	2.095	(1.760)	0.000
- Years in private schooling	0.817	(1.906)	1.073	(2.253)	-0.255	0.817	(1.969)	0.000
- Number of school changes	0.834	(0.835)	0.630	(0.773)	0.204	0.834	(0.885)	0.000
- Years in special needs teaching	0.734	(1.253)	0.293	(0.929)	0.441	0.734	(1.281)	0.000
- Total hours of special needs teaching	6.421	(18.835)	4.563	(21.237)	1.859	6.422	(21.010)	0.000
- Dyslexics (1/0)	0.353	(0.479)	0.101	(0.301)	0.252	0.353	(0.478)	0.000
- OCD (1/0)	0.071	(0.257)	0.025	(0.155)	0.046	0.071	(0.256)	0.000
- ADHD (1/0)	0.095	(0.294)	0.038	(0.191)	0.058	0.095	(0.294)	0.000
- Any psychological diagnosis (1/0)	0.207	(0.406)	0.100	(0.299)	0.108	0.208	(0.406)	0.000
Mother:								
- Age at birth (years)	29.737	(4.932)	30.306	(4.764)	-0.570	29.737	(4.917)	0.000
- Wage income (log)	5.607	(1.078)	5.682	(1.017)	-0.075	5.607	(0.987)	0.000
- Years of education	14.125	(2.668)	14.512	(2.732)	-0.388	14.125	(2.376)	0.000
- Highschool or less (1/0)	0.191	(0.394)	0.200	(0.400)	-0.009	0.191	(0.393)	0.000
- Employed in November (1/0)	0.722	(0.449)	0.763	(0.425)	-0.041	0.722	(0.448)	0.000
Father:								
- Age at birth (years)	32.456	(5.774)	32.952	(5.620)	-0.497	32.456	(5.662)	0.000
- Wage income (log)	5.821	(1.089)	5.958	(1.150)	-0.137	5.821	(1.165)	0.000
- Years of education	13.859	(2.437)	14.414	(2.615)	-0.555	13.859	(2.457)	0.000
- Highschool or less (1/0)	0.266	(0.443)	0.249	(0.433)	0.016	0.266	(0.442)	0.000
- Employed in November (1/0)	0.787	(0.410)	0.827	(0.378)	-0.039	0.788	(0.409)	0.000
Language comprehension (std.):								
- Grade 2	-0.509	(0.933)	-0.026	(0.990)	-0.483	-0.509	(1.069)	0.000
- Grade 4	-0.680	(1.008)	0.001	(0.995)	-0.680	-0.680	(1.207)	0.000
- Grade 6	-0.639	(0.894)	0.002	(0.991)	-0.640	-0.639	(1.083)	0.000
- Grade 8	-0.546	(1.113)	0.036	(1.028)	-0.583	-0.546	(1.186)	0.000
Decoding (std.):								
- Grade 2	-0.982	(0.829)	-0.064	(1.006)	-0.918	-0.982	(0.990)	0.000

- Grade 4	-0.973	(1.059)	-0.026	(1.008)	-0.947	-0.973	(1.182)	0.000
- Grade 6	-0.956	(0.841)	-0.068	(0.987)	-0.888	-0.956	(1.139)	0.000
- Grade 8	-1.095	(0.914)	-0.071	(1.010)	-1.024	-1.095	(1.306)	0.000
Text comprehension (std.):								
- Grade 2	-0.942	(0.957)	-0.090	(1.020)	-0.851	-0.942	(1.080)	0.000
- Grade 4	-0.906	(0.975)	-0.065	(0.996)	-0.841	-0.906	(1.116)	0.000
- Grade 6	-0.821	(0.847)	-0.049	(0.992)	-0.772	-0.821	(1.066)	0.000
- Grade 8	-0.927	(0.982)	-0.061	(0.998)	-0.866	-0.927	(1.158)	0.000
Numbers and algrebra (std.):								
- Grade 3	-0.699	(0.962)	0.035	(0.999)	-0.734	-0.699	(1.116)	0.000
- Grade 6	-0.667	(0.911)	0.058	(0.985)	-0.725	-0.667	(1.032)	0.000
- Grade 8	-0.833	(0.766)	0.051	(1.001)	-0.884	-0.833	(1.021)	0.000
Geometry (std.):								
- Grade 3	-0.606	(0.779)	0.023	(1.028)	-0.629	-0.606	(1.053)	0.000
- Grade 6	-0.625	(0.826)	0.011	(0.995)	-0.636	-0.625	(0.976)	0.000
- Grade 8	-0.834	(0.667)	0.063	(0.984)	-0.897	-0.834	(0.935)	0.000
Statistics and probability (std.):								
- Grade 3	-0.724	(0.954)	0.080	(1.008)	-0.805	-0.724	(1.166)	0.000
- Grade 6	-0.733	(0.904)	0.081	(1.008)	-0.814	-0.733	(1.075)	0.000
- Grade 8	-0.814	(0.777)	0.035	(0.987)	-0.849	-0.814	(1.072)	0.000
Sick absence (percent):								
- Grade 2	2.301	(2.244)	2.804	(3.183)	-0.503	2.301	(2.417)	0.000
- Grade 3	2.733	(2.867)	2.822	(3.298)	-0.089	2.733	(3.112)	0.000
- Grade 4	3.172	(3.461)	3.020	(3.591)	0.152	3.172	(3.830)	0.000
- Grade 5	3.270	(3.761)	3.196	(3.921)	0.074	3.270	(4.031)	0.000
- Grade 6	3.230	(3.878)	3.288	(4.163)	-0.058	3.229	(3.834)	0.000
- Grade 7	3.374	(4.424)	3.355	(4.394)	0.018	3.374	(4.283)	0.000
- Grade 8	3.172	(4.098)	3.413	(4.745)	-0.241	3.172	(3.733)	0.000
Illegal absence (percent):								
- Grade 2	0.587	(1.536)	0.432	(1.821)	0.155	0.587	(3.173)	0.000
- Grade 3	0.317	(1.176)	0.460	(1.908)	-0.143	0.317	(1.108)	0.000
- Grade 4	0.383	(0.936)	0.532	(2.011)	-0.148	0.383	(1.192)	0.000
- Grade 5	0.629	(2.340)	0.622	(2.279)	0.007	0.629	(2.301)	0.000
- Grade 6	0.754	(2.052)	0.790	(2.755)	-0.036	0.754	(2.293)	0.000
- Grade 7	1.616	(3.936)	1.280	(3.580)	0.336	1.616	(4.563)	0.000
- Grade 8	2.332	(5.236)	1.753	(4.973)	0.578	2.332	(5.946)	0.000
Legal absence (percent):								
- Grade 2	2.057	(2.757)	1.546	(2.384)	0.510	2.056	(5.982)	0.000
- Grade 3	1.476	(2.179)	1.484	(2.316)	-0.008	1.476	(2.346)	0.000
- Grade 4	1.625	(2.630)	1.420	(2.316)	0.205	1.625	(3.136)	0.000
- Grade 5	1.359	(1.905)	1.409	(2.439)	-0.050	1.359	(2.386)	0.000
- Grade 6	1.268	(1.702)	1.383	(2.475)	-0.115	1.268	(2.060)	0.000
- Grade 7	1.701	(2.264)	1.462	(2.502)	0.239	1.701	(3.613)	0.000
- Grade 8	2.443	(7.064)	1.724	(2.941)	0.719	2.443	(5.367)	0.000
Conscientiousness (std.):								

- Grade 4	-0.621	(0.964)	0.040	(1.038)	-0.661	-0.621	(1.115)	0.000
- Grade 5	-0.591	(0.901)	0.074	(0.999)	-0.666	-0.591	(1.088)	0.000
- Grade 6	-0.420	(0.839)	0.092	(0.956)	-0.512	-0.420	(1.045)	0.000
- Grade 7	-0.433	(0.890)	0.054	(0.943)	-0.487	-0.433	(1.035)	0.000
- Grade 8	-0.616	(0.932)	0.031	(0.947)	-0.647	-0.616	(1.082)	0.000
Agreeableness (std.):								
- Grade 4	-0.061	(0.983)	-0.028	(1.025)	-0.033	-0.061	(1.084)	0.000
- Grade 5	-0.575	(1.168)	-0.088	(1.007)	-0.487	-0.575	(1.168)	0.000
- Grade 6	-0.378	(0.892)	-0.136	(0.987)	-0.242	-0.378	(1.081)	0.000
- Grade 7	-0.435	(0.941)	-0.184	(0.996)	-0.252	-0.435	(1.085)	0.000
- Grade 8	-0.527	(1.051)	-0.172	(0.998)	-0.355	-0.527	(1.128)	0.000
Emotional stability (std.):								
- Grade 4	0.097	(0.827)	0.131	(0.981)	-0.034	0.097	(1.023)	0.000
- Grade 5	-0.276	(1.198)	0.179	(0.953)	-0.455	-0.277	(1.217)	0.000
- Grade 6	-0.042	(1.012)	0.198	(0.933)	-0.240	-0.042	(1.075)	0.000
- Grade 7	-0.008	(0.982)	0.165	(0.910)	-0.173	-0.008	(1.020)	0.000
- Grade 8	-0.103	(0.980)	0.153	(0.904)	-0.255	-0.103	(1.053)	0.000
School connectedness (std.):								
- Grade 4	0.158	(0.869)	0.173	(0.959)	-0.015	0.158	(0.964)	0.000
- Grade 5	-0.310	(1.181)	0.186	(0.942)	-0.496	-0.310	(1.199)	0.000
- Grade 6	-0.096	(0.975)	0.158	(0.938)	-0.254	-0.096	(1.082)	0.000
- Grade 7	-0.048	(0.930)	0.094	(0.916)	-0.142	-0.048	(0.971)	0.000
- Grade 8	-0.209	(0.933)	0.046	(0.919)	-0.255	-0.209	(1.051)	0.000
Learning self-ef cacy (std.):								
- Grade 4	-0.378	(0.782)	0.120	(1.014)	-0.498	-0.378	(1.051)	0.000
- Grade 5	-0.615	(0.978)	0.120	(0.988)	-0.735	-0.615	(1.110)	0.000
- Grade 6	-0.563	(0.778)	0.100	(0.966)	-0.664	-0.563	(1.063)	0.000
- Grade 7	-0.620	(0.935)	0.036	(0.965)	-0.656	-0.620	(1.048)	0.000
- Grade 8	-0.962	(0.992)	-0.029	(0.982)	-0.932	-0.962	(1.157)	0.000
Learning environment (std.):								
- Grade 4	0.370	(0.840)	0.374	(0.997)	-0.003	0.370	(1.080)	0.000
- Grade 5	-0.038	(1.045)	0.189	(0.986)	-0.227	-0.038	(1.114)	0.000
- Grade 6	0.044	(0.913)	0.002	(0.984)	0.042	0.044	(1.000)	0.000
- Grade 7	-0.240	(1.020)	-0.177	(0.980)	-0.063	-0.240	(1.004)	0.000
- Grade 8	-0.420	(0.978)	-0.298	(0.980)	-0.122	-0.420	(1.020)	0.000
Classroom management (std.):								
- Grade 4	0.018	(0.959)	0.023	(1.020)	-0.004	0.018	(1.093)	0.000
- Grade 5	-0.306	(1.133)	0.021	(0.993)	-0.327	-0.307	(1.128)	0.000
- Grade 6	0.006	(0.981)	0.036	(0.989)	-0.029	0.006	(1.026)	0.000
- Grade 7	-0.226	(1.029)	0.014	(0.995)	-0.240	-0.226	(1.077)	0.000
- Grade 8	-0.132	(1.158)	0.059	(0.997)	-0.191	-0.132	(1.093)	0.000
Cohort:								
- 2015	0.274	(0.447)	0.201	(0.400)	0.073	0.274	(0.446)	0.000
- 2016	0.195	(0.397)	0.198	(0.399)	-0.003	0.195	(0.396)	0.000
- 2017	0.203	(0.403)	0.194	(0.395)	0.009	0.203	(0.402)	0.000

- 2018	0.141	(0.349)	0.203	(0.402)	-0.062	0.141	(0.348)	0.000
- 2019	0.187	(0.390)	0.204	(0.403)	-0.017	0.187	(0.390)	0.000
Number of pupils	241		157,990					

Notes: The table shows descriptive statistics of covariates used in the analysis. The mean, standard derivation and difference in means are reported for the camp group and for the non-camp group before and after adjustment. The non-camp group are reweighted using entropy balancing such that it mimics the camp group. Pupil and parent background characteristics are measured at grade 8. The table is based on non-missing data. Bold (italic) indicates signi cance at the 5% (10%) level.

Figure A.1: Example of the main camp schedule for the rst week

Figure A.2: Example of the main camp schedule for the second week

Figure A.3: Summer camp and Exam

Notes: This gure shows the entropy balancing adjusted event study representation of the effect of summer camp participation. The average assessment mark in June grade 9 is substituted with the exam performance. The solid line presents the effects on the average assessment mark and the dashed line presents the effects in which the June grade 9 grading have been substituted with the exam performance. Each point represents the assessment difference in outcome between camp and non-camp boys with 95% con dence intervals. I use the assessment in December grade 8 as reference time-point.

Figure A.4: Summer camp and written exam

Notes: This gure shows the entropy balancing adjusted event study representation of the effect of summer camp participation. The average assessment mark in June grade 9 is substituted with the written exam performance. The solid line presents the effects on the average assessment mark and the dashed line presents the effects in which the June grade 9 grading have been substituted with the exam performance. Each point represents the assessment difference in outcome between camp and non-camp boys with 95% con dence intervals. I use the assessment in December grade 8 as reference time-point.

Table A.2: Effects of summer camp on average assessment mark

	Difference-in-difference			DiD & Entropy balance		
	(1)	(2)	(3)	(4)	(5)	(6)
Camp	-1.179	-0.314		-0.390	-0.392	
	(0.038)	(0.041)		(0.039)	(0.028)	
Post	0.025	0.018	0.020	0.073	0.020	0.014
	(0.002)	(0.001)	(0.001)	(0.007)	(0.007)	(0.005)
Camp X Post	0.167	0.125	0.152	0.120	0.154	0.158
	(0.035)	(0.036)	(0.036)	(0.036)	(0.031)	(0.037)
Observations	479,540	479,540	479,540	479,540	479,540	479,540
R-squared	0.002	0.624	0.944	0.049	0.564	0.910
Mean outcome, grade 9	-0.088	-0.088	-0.088	-0.966	-0.966	-0.966
Pupil background chars	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes

Notes: This table presents the results for six separate difference-in-difference speci - cations comparing camp participants to non-participants. Columns 1 to 3 display the simple difference-in-difference speci cations, and columns 4 to 6 show the results from the difference-in-difference combined with entropy balancing. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. The mean outcome in grade 9 is below zero since the standardization of outcome also include girls. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.3: Effects of summer camp on personal readiness assessment

	Difference-in-difference			DiD & Entropy balance		
	(1)	(2)	(3)	(4)	(5)	(6)
Camp	-0.348	-0.141		-0.132	-0.126	
	(0.033)	(0.035)		(0.034)	(0.027)	
Post	0.067	0.069	0.072	0.092	0.097	0.090
	(0.001)	(0.001)	(0.002)	(0.004)	(0.007)	(0.005)
Camp X Post	0.121	0.114	0.137	0.097	0.096	0.119
	(0.041)	(0.040)	(0.065)	(0.041)	(0.038)	(0.065)
Observations	263,480	263,480	263,480	263,480	263,480	263,480
R-squared	0.007	0.252	0.834	0.029	0.259	0.802
Mean outcome, grade 9	0.807	0.807	0.807	0.598	0.598	0.598
Pupil background chars	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes

Notes: This table presents the results for six separate difference-in-difference speci - cations comparing camp participants to non-participants. Columns 1 to 3 display the simple difference-in-difference speci cations, and columns 4 to 6 show the results from the difference-in-difference combined with entropy balancing. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.4: Effects of summer camp on social readiness assessment

	Difference-in-difference			DiD & Entropy balance		
	(1)	(2)	(3)	(4)	(5)	(6)
Camp	-0.267	-0.120		-0.109	-0.106	
	(0.034)	(0.034)		(0.034)	(0.027)	
Post	0.048	0.048	0.050	0.070	0.078	0.065
	(0.001)	(0.001)	(0.001)	(0.004)	(0.007)	(0.005)
Camp X Post	0.072	0.071	0.097	0.050	0.049	0.083
	(0.035)	(0.034)	(0.052)	(0.035)	(0.033)	(0.052)
Observations	263,153	263,153	263,153	263,153	263,153	263,153
R-squared	0.005	0.203	0.837	0.019	0.251	0.846
Mean outcome, grade 9	0.871	0.871	0.871	0.705	0.705	0.705
Pupil background chars	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes

Notes: This table presents the results for six separate difference-in-difference speci - cations comparing camp participants to non-participants. Columns 1 to 3 display the simple difference-in-difference speci cations, and columns 4 to 6 show the results from the difference-in-difference combined with entropy balancing. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.5: Effects of summer camp on academic readiness assessment

	Difference-in-difference			DiD & Entropy balance		
	(1)	(2)	(3)	(4)	(5)	(6)
Camp	-0.567	-0.321		-0.269	-0.256	
	(0.027)	(0.026)		(0.027)	(0.023)	
Post	0.080	0.065	0.063	0.212	0.186	0.183
	(0.001)	(0.001)	(0.002)	(0.005)	(0.006)	(0.006)
Camp X Post	0.355	0.322	0.335	0.223	0.218	0.216
	(0.040)	(0.040)	(0.067)	(0.040)	(0.039)	(0.067)
Observations	265,354	265,354	265,354	265,354	265,354	265,354
R-squared	0.011	0.353	0.804	0.145	0.417	0.767
Mean outcome, grade 9	0.834	0.834	0.834	0.645	0.645	0.645
Pupil background chars	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes

Notes: This table presents the results for six separate difference-in-difference speci - cations comparing camp participants to non-participants. Columns 1 to 3 display the simple difference-in-difference speci cations, and columns 4 to 6 show the results from the difference-in-difference combined with entropy balancing. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.6: Effects of summer camp on desire for upper secondary education

	3-year HS	Voc. training	2-year HS	Other
	(1)	(2)	(3)	(4)
Camp				
Post	0.022	0.058	0.014	-0.028
	(0.005)	(0.005)	(0.002)	(0.004)
Camp X Post	-0.048	0.033	0.002	0.001
	(0.056)	(0.048)	(0.024)	(0.031)
Observations	280,656	280,656	280,656	280,656
R-squared	0.819	0.814	0.775	0.766
Mean outcome, grade 9	0.456	0.759	0.034	0.063
Pupil background chars	-	-	-	-
Parental background chars	-	-	-	-
Reading abilities	-	-	-	-
Math abilities	-	-	-	-
Absence information	-	-	-	-
School well-being	-	-	-	-
Personality traits	-	-	-	-
Individual FE	Yes	Yes	Yes	Yes

Notes: This table shows difference-in-difference combined with entropy balancing results for secondary educational wishes at the ERA. It is possible for a pupil to wish more than one education and thus be assessed for all of them. Column 1 shows the results for 3-year high school, column 2 for vocational training, column 3 for 2-year high school, and column 4 for other types of education. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.7: Effects of summer camp participation - Alternative comparison groups

	Overall ERA			GPA		
	All	Camp schools	Non-camp schools	All	Camp schools	Non-camp schools
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.193	0.192	0.193	0.014	0.028	0.010
	(0.006)	(0.015)	(0.006)	(0.005)	(0.013)	(0.005)
Camp X Post	0.176	0.177	0.176	0.158	0.144	0.162
	(0.060)	(0.062)	(0.060)	(0.037)	(0.039)	(0.037)
Observations	277,043	54,313	223,155	479,540	95,353	384,901
R-squared	0.763	0.720	0.758	0.910	0.868	0.908
Mean outcome, grade 9	0.522	0.494	0.363	-0.966	-1.076	-1.061
Pupil background chars	-	-	-	-	-	-
Parental background chars	-	-	-	-	-	-
Reading abilities	-	-	-	-	-	-
Math abilities	-	-	-	-	-	-
Absence information	-	-	-	-	-	-
School well-being	-	-	-	-	-	-
Personality traits	-	-	-	-	-	-
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows difference-in-difference combined with entropy balancing speci cations for overall ERA (Columns 1 to 3) and GPA (columns 4 to 6). Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.8: Effects of summer camp participation - Robustness of balancing speci cation

	Entropy-I	Entropy-II	Entropy-III	NN-3	NN-5	NN-10
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall ERA						
Post	0.193	0.210	0.210	0.195	0.202	0.210
	(0.006)	(0.007)	(0.007)	(0.033)	(0.025)	(0.018)
Camp X Post	0.176	0.160	0.160	0.174	0.167	0.159
	(0.060)	(0.060)	(0.061)	(0.069)	(0.065)	(0.063)
Observations	277,043	277,043	277,042	1,679	2,512	4,507
R-squared	0.763	0.753	0.755	0.752	0.754	0.752
Panel B: GPA						
Post	0.014	0.011	0.009	0.029	0.032	0.013
	(0.005)	(0.006)	(0.007)	(0.021)	(0.016)	(0.012)
Camp X Post	0.158	0.162	0.163	0.143	0.140	0.160
	(0.037)	(0.037)	(0.037)	(0.043)	(0.040)	(0.038)
Observations	479,540	479,540	479,537	2,765	4,122	7,389
R-squared	0.910	0.896	0.894	0.905	0.904	0.904
Pupil background	-	-	-	-	-	-
Parental background	-	-	-	-	-	-
Reading abilities	-	-	-	-	-	-
Math abilities	-	-	-	-	-	-
Absence information	-	-	-	-	-	-
School well-being	-	-	-	-	-	-
Personality traits	-	-	-	-	-	-
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows difference-in-difference combined with different balancing speci cations for overall ERA (panel A) and GPA (panel B). Column 1 presents the main speci cation, column 2 (3) exploits entropy balance to ensure balance up to the second (third) moment. Column 4 to 6 exploits propensity score matching with respectively 3,5, and 10 nearest neighbour. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.9: Effect of being in placebo treatment group on main outcomes

	Difference-in-difference			DiD & Entropy balance		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Overall ERA						
Camp	-0.001	-0.001	0.000	0.001	-0.001	0.000
	(0.002)	(0.002)	(.)	(0.002)	(0.002)	(.)
Post	0.148	0.140	0.140	0.149	0.140	0.140
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Camp X Post	-0.003	-0.002	-0.001	-0.003	-0.002	-0.001
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)
Observations	276,618	276,618	276,618	276,618	276,618	276,618
R-squared	0.025	0.352	0.820	0.025	0.352	0.820
Panel B: GPA						
Camp	-0.002	-0.000	0.000	0.003	-0.000	0.000
	(0.005)	(0.003)	(.)	(0.005)	(0.003)	(.)
Post	0.027	0.019	0.020	0.028	0.019	0.020
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Camp X Post	-0.004	-0.002	0.001	-0.004	-0.002	0.001
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Observations	478,826	478,826	478,826	478,826	478,826	478,826
R-squared	0.000	0.623	0.944	0.000	0.623	0.944
Pupil background chars	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes

Notes: This table shows the main results for a randomly selected placebo treatment group. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.10: Placebo test - Effect of "camp" on national reading test in grade 6

	Language Comprehension			Decoding			Text Comprehension		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Difference-in-difference					
Camp	-0.680	-0.472		-0.947	-0.616		-0.841	-0.596	
	(0.070)	(0.072)		(0.074)	(0.070)		(0.068)	(0.072)	
Post	0.001	0.011	0.017	-0.042	-0.031	-0.024	0.016	0.027	0.033
	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Camp X Post	0.040	0.028	0.052	0.059	0.043	0.071	0.069	0.056	0.078
	(0.076)	(0.075)	(0.111)	(0.072)	(0.071)	(0.101)	(0.080)	(0.080)	(0.116)
Observations	255,424	255,424	255,424	255,424	255,424	255,424	255,424	255,424	255,424
R-squared	0.001	0.164	0.779	0.002	0.221	0.865	0.001	0.186	0.833
				Difference-in-difference & Entropy balance					
Camp	-0.473	-0.463		-0.642	-0.621		-0.626	-0.614	
	(0.071)	(0.066)		(0.074)	(0.065)		(0.068)	(0.064)	
Post	0.040	0.049	0.051	-0.021	-0.010	-0.004	0.017	0.024	0.029
	(0.006)	(0.007)	(0.009)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.008)
Camp X Post	0.000	-0.002	0.018	0.037	0.025	0.051	0.068	0.066	0.083
	(0.076)	(0.075)	(0.111)	(0.072)	(0.070)	(0.101)	(0.081)	(0.079)	(0.116)
Observations	255,424	255,424	255,424	255,424	255,424	255,424	255,424	255,424	255,424
R-squared	0.053	0.152	0.761	0.084	0.272	0.841	0.085	0.191	0.769
Covariates	No	Yes	-	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table shows placebo test on national tests in reading using camp vs non-camp pupils and the placebo camp date to be in the summer between grade 5 and 6. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.11: Placebo test - Effect of "camp" on national reading test in grade 8

	Language Comprehension			Decoding			Text Comprehension		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Difference-in-difference					
Camp	-0.680	-0.469		-0.947	-0.614		-0.841	-0.594	
	(0.070)	(0.071)		(0.074)	(0.070)		(0.068)	(0.072)	
Post	0.035	0.054	0.057	-0.046	-0.021	-0.017	0.004	0.031	0.035
	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Camp X Post	0.098	0.088	0.146	-0.077	-0.084	-0.030	-0.025	-0.035	0.017
	(0.098)	(0.100)	(0.147)	(0.076)	(0.076)	(0.101)	(0.089)	(0.090)	(0.132)
Observations	250,235	250,235	250,235	250,235	250,235	250,235	250,235	250,235	250,235
R-squared	0.001	0.114	0.703	0.002	0.213	0.838	0.001	0.189	0.819
				Difference-in-difference & Entropy balance					
Camp	-0.473	-0.465		-0.642	-0.626		-0.626	-0.619	
	(0.071)	(0.067)		(0.074)	(0.066)		(0.068)	(0.064)	
Post	0.064	0.076	0.077	-0.029	-0.006	-0.008	-0.004	0.020	0.013
	(0.007)	(0.010)	(0.010)	(0.006)	(0.008)	(0.008)	(0.007)	(0.008)	(0.009)
Camp X Post	0.069	0.078	0.125	-0.093	-0.081	-0.039	-0.017	0.007	0.039
	(0.099)	(0.097)	(0.147)	(0.077)	(0.072)	(0.102)	(0.089)	(0.086)	(0.132)
Observations	250,235	250,235	250,235	250,235	250,235	250,235	250,235	250,235	250,235
R-squared	0.043	0.116	0.703	0.097	0.294	0.849	0.089	0.195	0.765
Covariates	No	Yes	-	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table shows placebo test on national tests in reading using camp vs non-camp pupils and the placebo camp date to be in the summer between grade 5 and 6. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.12: Placebo test - Effect of "camp" on national math test in grade 6

	Numbers and algebra			Geometry			Statistics and probability		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Difference-in-difference				
Camp	-0.734	-0.533		-0.629	-0.449		-0.805	-0.585	
	(0.069)	(0.070)		(0.056)	(0.058)		(0.068)	(0.070)	
Post	0.024	0.032	0.040	-0.012	-0.003	0.005	0.001	0.011	0.018
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Camp X Post	0.009	0.011	0.018	-0.007	-0.007	-0.019	-0.009	-0.009	0.017
	(0.081)	(0.082)	(0.119)	(0.071)	(0.073)	(0.104)	(0.080)	(0.082)	(0.115)
Observations	253,000	253,000	253,000	253,000	253,000	253,000	253,000	253,000	253,000
R-squared	0.001	0.144	0.771	0.001	0.136	0.758	0.001	0.160	0.787
					Difference-in-difference & Entropy balance				
Camp	-0.558	-0.543		-0.473	-0.466		-0.604	-0.592	
	(0.069)	(0.065)		(0.056)	(0.054)		(0.068)	(0.064)	
Post	-0.001	0.010	0.011	-0.033	-0.022	-0.022	-0.017	-0.002	-0.004
	(0.006)	(0.008)	(0.009)	(0.006)	(0.007)	(0.008)	(0.006)	(0.008)	(0.009)
Camp X Post	0.034	0.025	0.046	0.014	0.010	0.008	0.009	-0.001	0.039
	(0.081)	(0.081)	(0.119)	(0.071)	(0.072)	(0.104)	(0.080)	(0.080)	(0.115)
Observations	253,000	253,000	253,000	253,000	253,000	253,000	253,000	253,000	253,000
R-squared	0.071	0.181	0.748	0.061	0.155	0.737	0.083	0.194	0.769
Covariates	No	Yes	-	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table shows placebo test on national tests in math using camp vs non-camp pupils and the placebo camp date to be in the summer between grade 5 and 6 with test conducted in grade 3 and 6. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.13: Placebo test - Effect of "camp" on absence between grade 7 and 8

	Sick			Illegal			Legal		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Difference-in-difference					
Camp	0.018	-0.176		0.336	0.260		0.239	0.163	
	(0.310)	(0.278)		(0.276)	(0.253)		(0.159)	(0.157)	
Post	0.058	0.052	0.069	0.473	0.459	0.470	0.261	0.260	0.281
	(0.013)	(0.013)	(0.018)	(0.013)	(0.012)	(0.017)	(0.010)	(0.010)	(0.014)
Camp X Post	-0.259	-0.278	-0.251	0.243	0.236	0.231	0.481	0.482	0.310
	(0.268)	(0.267)	(0.385)	(0.353)	(0.354)	(0.523)	(0.509)	(0.510)	(0.726)
Observations	251,655	251,655	251,655	251,655	251,655	251,655	251,655	251,655	251,655
R-squared	0.000	0.156	0.786	0.003	0.132	0.776	0.002	0.044	0.650 [b]
				Difference-in-difference & Entropy balance					
Camp	-0.164	-0.162		0.310	0.375		0.183	0.194	
	(0.311)	(0.253)		(0.276)	(0.228)		(0.159)	(0.164)	
Post	0.098	0.094	0.117	0.582	0.578	0.580	0.345	0.342	0.357
	(0.025)	(0.025)	(0.036)	(0.030)	(0.030)	(0.043)	(0.021)	(0.021)	(0.029)
Camp X Post	-0.299	-0.315	-0.300	0.133	0.118	0.120	0.397	0.371	0.234
	(0.269)	(0.271)	(0.386)	(0.354)	(0.352)	(0.524)	(0.510)	(0.493)	(0.727)
Observations	251,655	251,655	251,655	251,655	251,655	251,655	251,655	251,655	251,655
R-squared	0.002	0.209	0.792	0.007	0.186	0.737	0.007	0.058	0.587
Pupil background chars	No	Yes	-	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table tests for motivation effect by investigating behavior change leading up to camp participation. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) indicates signi cance at the 5% (10%) level.

Table A.14: Placebo test - Effect of "camp" on personality traits between grade 7 and 8

	Conscientiousness			Agreeableness			Emotional Stability		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Difference-in-difference					
Camp	-0.487	-0.281		-0.252	-0.098		-0.173	-0.069	
	(0.076)	(0.072)		(0.081)	(0.078)		(0.085)	(0.085)	
Post	-0.023	-0.019	-0.024	0.012	0.036	0.043	-0.013	-0.009	-0.021
	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.005)
Camp X Post	-0.160	-0.194	-0.141	-0.104	-0.122	-0.230	-0.082	-0.105	-0.128
	(0.084)	(0.082)	(0.133)	(0.096)	(0.094)	(0.154)	(0.100)	(0.100)	(0.173)
Observations	194,129	194,129	194,129	194,344	194,344	194,344	190,467	190,467	190,467
R-squared	0.001	0.114	0.829	0.000	0.069	0.792	0.000	0.071	0.820
				Difference-in-difference & Entropy balance					
Camp	-0.286	-0.288		-0.094	-0.099		-0.061	-0.099	
	(0.076)	(0.064)		(0.081)	(0.070)		(0.085)	(0.081)	
Post	0.009	0.004	-0.010	0.042	0.000	0.054	0.012	0.006	-0.012
	(0.007)	(0.018)	(0.010)	(0.008)	(0.020)	(0.012)	(0.007)	(0.021)	(0.011)
Camp X Post	-0.191	-0.186	-0.155	-0.134	-0.125	-0.240	-0.106	-0.102	-0.137
	(0.084)	(0.078)	(0.133)	(0.096)	(0.091)	(0.155)	(0.100)	(0.100)	(0.174)
Observations	194,129	194,129	194,129	194,344	194,344	194,344	190,467	190,467	190,467
R-squared	0.044	0.191	0.852	0.008	0.128	0.826	0.005	0.103	0.818
Pupil background chars	No	Yes	-	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table tests for motivation effect by investigating behavior change leading up to camp participation. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) indicates signi cance at the 5% (10%) level.

Table A.15: Placebo test - Effect of "camp" on well-being between grade 7 and 8

	School Connectedness			Learning Self-ef cacy			Learning Environment			Classroom Management		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
						Difference-in-difference						
Camp	-0.142	-0.032		-0.656	-0.411		-0.063	0.005		-0.240	-0.155	
	(0.083)	(0.080)		(0.085)	(0.082)		(0.091)	(0.087)		(0.088)	(0.086)	
Post	-0.047	-0.045	-0.058	-0.066	-0.057	-0.056	-0.121	-0.127	-0.142	0.046	0.048	0.041
	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)
Camp X Post	-0.113	-0.136	-0.174	-0.276	-0.295	-0.194	-0.059	-0.091	-0.051	0.049	0.026	-0.011
	(0.096)	(0.095)	(0.154)	(0.099)	(0.098)	(0.166)	(0.098)	(0.097)	(0.131)	(0.109)	(0.109)	(0.184)
Observations	186,750	186,750	186,750	177,224	177,224	177,224	185,973	185,973	185,973	194,204	194,204	194,204
R-squared	0.001	0.077	0.837	0.002	0.149	0.874	0.004	0.072	0.832	0.001	0.044	0.785
						Difference-in-difference & Entropy balance						
Camp	-0.026	-0.050		-0.423	-0.439		-0.006	0.001		-0.153	-0.160	
	(0.083)	(0.075)		(0.086)	(0.073)		(0.091)	(0.074)		(0.088)	(0.078)	
Post	-0.024	-0.027	-0.050	-0.036	-0.016	-0.046	-0.105	-0.095	-0.133	0.063	0.032	0.052
	(0.007)	(0.021)	(0.010)	(0.007)	(0.022)	(0.010)	(0.007)	(0.020)	(0.011)	(0.008)	(0.023)	(0.012)
Camp X Post	-0.136	-0.151	-0.182	-0.306	-0.287	-0.204	-0.075	-0.103	-0.060	0.031	0.035	-0.022
	(0.096)	(0.093)	(0.155)	(0.099)	(0.092)	(0.167)	(0.098)	(0.090)	(0.131)	(0.109)	(0.104)	(0.184)
Observations	186,750	186,750	186,750	177,224	177,224	177,224	185,973	185,973	185,973	194,204	194,204	194,204
R-squared	0.006	0.113	0.846	0.092	0.233	0.891	0.006	0.128	0.874	0.006	0.090	0.804
Pupil background chars	No	Yes	-	No	Yes	-	No	Yes	-	No	Yes	-
Parental background chars	No	Yes	-	No	Yes	-	No	Yes	-	No	Yes	-
Reading abilities	No	Yes	-	No	Yes	-	No	Yes	-	No	Yes	-
Math abilities	No	Yes	-	No	Yes	-	No	Yes	-	No	Yes	-
Absence information	No	Yes	-	No	Yes	-	No	Yes	-	No	Yes	-
School well-being	No	Yes	-	No	Yes	-	No	Yes	-	No	Yes	-
Personality traits	No	Yes	-	No	Yes	-	No	Yes	-	No	Yes	-
Individual FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table tests for motivation effect by investigating behavior change leading up to camp participation. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) indicates signi cance at the 5% (10%) level.

Table A.16: Triple differences on main outcomes - Placebo treatment group

		Educational readiness assessment			
	GPA	Overall	Personal	Social	Academic
	(1)	(2)	(3)	(4)	(5)
DiDiD	0.004	-0.003	-0.002	-0.004	0.002
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Observations	478,826	276,618	263,087	262,760	264,960
R-squared	0.623	0.353	0.252	0.203	0.354
Pupil background chars	Yes	Yes	Yes	Yes	Yes
Parental background chars	Yes	Yes	Yes	Yes	Yes
Reading abilities	Yes	Yes	Yes	Yes	Yes
Math abilities	Yes	Yes	Yes	Yes	Yes
Absence information	Yes	Yes	Yes	Yes	Yes
School well-being	Yes	Yes	Yes	Yes	Yes
Personality traits	Yes	Yes	Yes	Yes	Yes

Notes: This table shows triple difference (DiDiD) estimates by comparing placebo camp participants attending the individual mentoring program with those attending group mentor centers in the year following the camp. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.17: Placebo test - Triple differences estimates of "camp" between grade 5 and 6 on national reading tests

	Language comprehension		Decoding		Text comprehension	
	Grade 6	Grade 8	Grade 6	Grade 8	Grade 6	Grade 8
	(1)	(2)	(3)	(4)	(5)	(6)
DiDiD	0.112	0.056	0.082	-0.196	0.017	-0.074
	(0.151)	(0.198)	(0.140)	(0.149)	(0.160)	(0.175)
Observations	255,424	250,235	255,424	250,235	255,424	250,235
R-squared	0.152	0.116	0.274	0.295	0.193	0.196
Pupil background chars	Yes	Yes	Yes	Yes	Yes	Yes
Parental background chars	Yes	Yes	Yes	Yes	Yes	Yes
Reading abilities	Yes	Yes	Yes	Yes	Yes	Yes
Math abilities	Yes	Yes	Yes	Yes	Yes	Yes
Absence information	Yes	Yes	Yes	Yes	Yes	Yes
School well-being	Yes	Yes	Yes	Yes	Yes	Yes
Personality traits	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows triple difference (DiDiD) estimates by comparing camp participants attending the individual mentoring program with those attending group mentor centers using a fake camp date between grade 5 and 6. I use the national test in reading grade 4 as pre-test and the national test in reading grade 6 and 8 as post-tests. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.18: Placebo test - Triple differences estimates of "camp" between grade 5 and 6 on national math tests

	Numbers and algebra	Geometry	Statistics and probability
	(1)	(2)	(3)
DiDiD	0.039	-0.181	-0.070
	(0.166)	(0.144)	(0.162)
Observations	253,000	253,000	253,000
R-squared	0.181	0.156	0.194
Pupil background chars	Yes	Yes	Yes
Parental background chars	Yes	Yes	Yes
Reading abilities	Yes	Yes	Yes
Math abilities	Yes	Yes	Yes
Absence information	Yes	Yes	Yes
School well-being	Yes	Yes	Yes
Personality traits	Yes	Yes	Yes

Notes: This table shows triple difference (DiDiD) estimates by comparing camp participants attending the individual mentoring program with those attending group mentor centers using a fake camp date between grade 5 and 6. I use the national test in math grade 3 as pre-test and the national test in reading grade 6 as post-test. Standard errors are clustered on the individual level. Missing values are imputed with the value zero and a binary indicator is added to the conditioning set. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

Table A.19: Descriptive statistics of camp boys by follow-up strategy

	Camp		
	2015-2016	2017-2019	Diff.
	(1)	(2)	(3)
Pupil:			
- Age (years)	15.204	15.383	0.179
- Non-western ethnicity (1/0)	0.054	0.039	-0.015
- Living with both parents (1/0)	0.584	0.531	-0.053
- Number of relocations	2.000	2.180	0.180
- Years in private schooling	0.832	0.805	-0.027
- Number of school changes	0.858	0.813	-0.046
- Years in special needs teaching	1.159	0.359	-0.800
- Total hours of special needs teaching	7.938	5.082	-2.856
- Dyslexics (1/0)	0.327	0.375	0.048
- OCD (1/0)	0.080	0.063	-0.017
- ADHD (1/0)	0.062	0.125	0.063
- Any psychological diagnosis (1/0)	0.186	0.227	0.041
Mother:			
- Age at birth (years)	29.633	29.828	0.195
- Wage income (log)	5.615	5.600	-0.015
- Years of education	14.030	14.208	0.178
- High school or less (1/0)	0.204	0.180	-0.024
- Employed in November (1/0)	0.735	0.711	-0.024
Father:			
- Age at birth (years)	32.308	32.586	0.278
- Wage income (log)	5.877	5.773	-0.104
- Years of education	13.800	13.913	0.113
- High school or less (1/0)	0.257	0.273	0.017
- Employed in November (1/0)	0.805	0.772	-0.034
Language comprehension (std.):			
- Grade 2	-0.522	-0.504	0.018
- Grade 4	-0.654	-0.702	-0.048
- Grade 6	-0.656	-0.624	0.032
- Grade 8	-0.579	-0.518	0.061
Decoding (std.):			
- Grade 2	-1.011	-0.973	0.038
- Grade 4	-1.034	-0.920	0.114
- Grade 6	-1.073	-0.857	0.216
- Grade 8	-1.072	-1.115	-0.043
Text comprehension (std.):			
- Grade 2	-1.026	-0.914	0.112
- Grade 4	-0.982	-0.840	0.141
- Grade 6	-0.902	-0.752	0.150
- Grade 8	-0.955	-0.902	0.054
Numbers and algebra (std.):			

- Grade 3	-0.664	-0.727	-0.063
- Grade 6	-0.675	-0.660	0.015
Geometry (std.):			
- Grade 3	-0.637	-0.582	0.055
- Grade 6	-0.563	-0.677	-0.114
Statistics and probability (std.):			
- Grade 3	-0.711	-0.735	-0.024
- Grade 6	-0.722	-0.742	-0.020
Sick absence (percent):			
- Grade 4	2.771	3.328	0.557
- Grade 5	3.459	3.099	-0.359
- Grade 6	3.263	3.201	-0.062
- Grade 7	3.209	3.516	0.307
- Grade 8	3.236	3.116	-0.120
Illegal absence (percent):			
- Grade 4	0.259	0.432	0.173
- Grade 5	0.787	0.486	-0.300
- Grade 6	1.002	0.539	-0.463
- Grade 7	1.422	1.783	0.361
- Grade 8	2.197	2.450	0.253
Legal absence (percent):			
- Grade 4	1.757	1.573	-0.184
- Grade 5	1.253	1.454	0.201
- Grade 6	1.277	1.260	-0.016
- Grade 7	1.493	1.881	0.388
- Grade 8	1.816	2.997	1.181
Conscientiousness (std.):			
- Grade 7	-0.364	-0.457	-0.093
- Grade 8	-0.563	-0.661	-0.098
Agreeableness (std.):			
- Grade 7	-0.496	-0.414	0.083
- Grade 8	-0.313	-0.696	-0.383
Emotional stability (std.):			
- Grade 7	0.104	-0.048	-0.153
- Grade 8	-0.024	-0.172	-0.147
School connectedness (std.):			
- Grade 7	0.064	-0.090	-0.154
- Grade 8	-0.110	-0.292	-0.182
Learning self-ef cacy (std.):			
- Grade 7	-0.558	-0.641	-0.083
- Grade 8	-0.991	-0.934	0.058
Learning environment (std.):			
- Grade 7	-0.197	-0.256	-0.058
- Grade 8	-0.478	-0.369	0.108
Classroom management (std.):			

- Grade 7	-0.188	-0.239	-0.051
- Grade 8	-0.032	-0.219	-0.187
Number of pupils	113	128	241

Notes: The table shows descriptive statistics of covariates used in the analysis. The mean and difference in means are reported for before and after the introduction of group mentoring (in 2017) for the Camp boys. The table is based on non-missing data. Bold (italic) numbers indicate signi cance at the 5% (10%) level.

CHAPTER 3

HOW TO COPE WITH DYSLEXIA: THE EFFECTS OF SPECIAL EDUCATION ON ACADEMIC PERFORMANCE, PERSONALITY TRAITS, AND WELL-BEING

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Abstract

I use Danish administrative data to study the effects of a special education intervention for pupils with severe dyslexia. My empirical approach exploits individual level panel data that allow me to track pupils before and after the initiation of treatment. Using a difference-in-difference strategy in combination with entropy balancing, I nd positive and persistent effects of the intervention on pupils' reading scores, personality traits, and school well-being. The intervention reduces the reading gap to non-dyslexics by 33 percent and the well-being gap by 80 percent.

Keywords: Dyslexia, Special education, Assistive technology, Personality traits, Difference-in-differences

JEL Codes: C21, H52, I21, I28

3.1 Introduction

This paper studies a specialized intervention targeted children with dyslexia. Dyslexia is a genetic disorder that affects 3% to 10% of the population (Snowling, 2013). It affects the ability to spell, read, and write (Hebert et al., 2018) and may even have far-reaching consequences for children's lives such as low school grades, poor educational attainment, and behavior problems (Undheim, 2009; Epnion, 2018; Einar et al., 2001). It is possible, however, that this academic underperformance and negative behavior are a sign of inef ciencies in the educational system since the existing literature nds no relationship between dyslexia and intelligence (Snowling et al., 2020).

I provide the most extensive evidence yet, regarding the impact of education programs for pupils with dyslexia. In particular, I use population-level administrative Danish individual level panel data covering the period 2010–2019 to study the effect of a 10 consecutive weeks dyslexia learning program with one and a half years follow up. I investigate, rst, how the learning program affects the pupils reading abilities as well as personality traits and school well-being. Secondly, I ask whether the observed effects are persistent over multiple time-periods.

The learning program - Reading Competency Center for Dyslexics - is a specialized dyslexia program for pupils with severe dyslexia in public schools grade 4 to 8. The objective is to enhance academic abilities by focusing on quali cations in general use of assistive technology [1] and training conventional reading and writing. The 10 consecutive weeks camp consist of three key components 1) Small group instruction, 2) Non-cognitive skills training, and 3) Training the use of assistive technology. The one and a half years follow up program consists of one-to-one meetings with the pupil, parents, teachers and the management team at the local school to ensure continued progression.

The analysis delivers notable results across all outcome groups. I nd positive effects on two out of three areas of reading (language and text comprehension) with effect sizes of 21% to 24% of a standard deviation. Thus, the program participants increase their reading abilities well beyond the level of the population of dyslexics. In fact, the intervention reduces the reading gap to non-dyslexics with 22% to 33%. Likewise, I nd positive effects on one out of the three personality traits investigated in this study with effect size of 15% of a standard deviation for conscientiousness. Moreover, the intervention increases the participants' school well-being by 14% to 17% of a standard deviation (school connectedness, learning self-ef cacy and classroom management). In fact, participants increase their level of personality traits and school well-being beyond the level of the population of dyslexics. Compared to non-dyslexics, the program signi cantly reduces the gap by 80%. Secondly, I show

[1] Assistive technology is any device, piece of equipment, or software program that increase, maintain, or improve the functional capabilities of persons with reading disabilities. E.g. text-to-speech software for smartphones or computers.

that the effects on reading scores are persistent over the four post-treatment school years I am able to observe. For learning self-ef cacy, the effect increases throughout the post-intervention time-periods with effect sizes of 40% of a standard deviation.

A large and growing number of studies estimate the causal effect of school-based policies and reforms targeting low-achieving pupils. However, there is limited evidence for the effect of special education policies. Ballis and Heath (2021) exploits a sharp reduction in special education from a state policy. This reduction generated signi cant reduction in educational attainment, suggesting long-run bene ts from special education. Schwartz et al. (2021) conclude that general special education in New York City improves academic performance (effect sizes around 0.11 standard deviations) for pupils with learning disabilities, which among others include pupils diagnosed with dyslexia. The impact is largest when entering special education in the earlier grades. These results are in line with previously published studies involving general special education and its effect on academic abilities (Hurwitz et al., 2020; Morgan et al., 2010; Hanushek et al., 2002; Reynolds and Wolfe, 1999). Toffalini et al. (2021) nds in a recent meta-study that causal evidence on program speci cally targeting pupils with dyslexia is limited. They nd 40 randomized control trails related to dyslexia and reading disorder. However, all studies suffers from small sample bias with on average only 20 treated children per study. Additionally, the previous studies only investigate the impact on spelling and not general reading abilities and well-being.

This study relies on population-wide register-based data for Danish pupils and their parents, which provides me with longitudinal information about relevant scholastic and well-being outcomes. From the national reading test that occurs every two years during primary schooling, I am able to measure the following three key aspects of reading: 1) language comprehension, 2) decoding, and 3) text comprehension. Additionally, I use the yearly national well-being survey to construct validated psychometric measures for personality traits and school well-being. I combine these data with a long range of socio-economic background characteristics and the membership list from the Danish Library and Expertise Center for people with print disabilities (henceforth NOTA). The national NOTA register allows me to identify pupils diagnosed with dyslexia and who have not participated in the intervention.

I use a difference-in-difference research design, where I exploit that pupils in practice receive the intervention between grade 4 and 8. Thus, I observe outcomes before and after the intervention, which enables me to estimate treatment effects using a xed-effects analysis. Importantly, the difference-in-difference approach implicitly controls for selection on time-invariant unobserved characteristics such as genetics and intelligence. Access to the NOTA membership list allows me to construct a comparison group that is similar in terms of reading disadvantage but not exposed to the special education intervention.

A key assumption behind the difference-in-difference design is that there can be no differential trends between the treatment and comparison group in the absence of

treatment. To address this, I combine my standard individual difference-in-difference strategy with matching techniques. In practice, I use entropy balancing that relies on a maximum entropy reweighting scheme that calibrates individual weights such that the reweighted group satisfy a set of pre-speci ed balance conditions (Hainmueller, 2012). Incorporating entropy balancing in the econometric framework ensures exact balancing between the NOTA and intervention group, not only concerning the mean but also on higher moments of the large set of observed covariates. Additionally, I use event study graphs to investigate how the effect evolves over time and to test for similar pre-trends. In the sensitivity checks, I use other speci cations for being dyslexic and alternative balancing strategies in order to explore the credibility of the estimated treatment effects. The results are robust to these sensitivity checks.

I thus provide evidence on learning programs targeting dyslexics and complement the existing literature on assistive technology. The present study contributes to the literature by being the rst to estimate causal effects of a learning program speci cally targeting pupils with dyslexia on reading scores, personality traits, and school well-being. Especially the ability to investigate the effects on personality traits - an important prerequisite for future academic achievement – is new in the general special education literature.

The remainder of the paper is structured as follows: Section 3.2 describes the background and institutional settings, such as the key components of the intervention studied, the selection of a valid comparison group, and the Danish school system. I describe the data in section 3.3 and the empirical strategy in section 3.4. Section 3.5 presents the results and robustness tests. Section 3.6 provides a discussion and interpretations of the ndings. The last section concludes.

3.2 Institutional Settings

In this section, I present the structure, components and objective of the intervention under study. Secondly, I provide a description of the comparison group and discuss treatment as usual. Finally, I brie y introduce the Danish compulsory school system.

3.2.1 Reading Competency Center for Dyslexics

The intervention under study - Reading Competency Center for Dyslexics (RCCD henceforth) - is a specialized learning program targeted dyslexics at public schools grade 4 to 8. It takes place in the municipality of Aarhus, the 2nd largest municipality in Denmark. The intervention last for approximately one and a half years with a total (annual) cost of 18,000USD (12,000USD) per pupil. The cost of the program seems high but with an annual cost of 29,500USD for special needs teaching 9 hours per week (Nørgaard et al., 2018) and a total cost of 3,600USD for 10 weeks of regular school teaching the program has potential to be cost-effective. Also in the short-run,

if the program improve academic abilities to a level where special needs teaching is no longer necessary.

The objective of RCCD is to enhance dyslexic pupils academic skills by focusing on quali cations in general use of assistive technology and at the same time practice conventional reading and writing. Through this, the intention is to enhance the pupil's abilities to allow for participation in age-appropriate teaching in their local classroom, and thereby acquire age-appropriate knowledge. Additionally, the intervention has a broader pedagogical aim. The pupils should not only develop their academic skills but also regains faith in own abilities, getting good independent work habits and achieve greater self-esteem in relation to academic learning such that the pupils become self-reliant in relation to continuing education and training.

The structure of the RCCD program consist of four steps. First, the pupil is assigned an RCCD consultant who is responsible for the pupil's learning throughout the program. The consultant meets with the local teachers and forms an overview of the opportunities at the pupil's local learning environment in order to design an individual action plan. Secondly, the pupil attends a 10 consecutive weeks learning camp at RCCD's location. The teaching takes place in groups with approximately ve pupils and consists of 30 weekly lectures in all compulsory school subjects. During the 10 weeks, the pupil trains proper use of assistive technology combined with additional focus on non-cognitive skills such as mindset and self-awareness. Thirdly, after the 10 weeks learning camp, the RCCD consultant facilitates the transfer of the pupil's new acquired learning strategies and methods of learning to the pupil's local learning environment. Finally, the consultant regularly follows up and provides advice and guidance for the pupil, parents, teachers and the management team at the local school to ensure that the pupil continues to make progress.

The 10 weeks learning camp consist of three key components, 1) small group instructions, 2) Non-cognitive skills training, and 3) practice the use of assistive technology, which combined have the potential to enhance academic performance, personality traits, and school well-being for this disadvantaged group of pupils.

Small group instructions. Dietrichson et al. (2020a,b) conduct two systematic reviews that combine different types of instructional methods [2] for pupils at risk of academic dif culties in grade 0-6 and grade 7-12 that are evaluated using standardized tests in reading and mathematics. They conclude that small group instruction (1-5 pupils per teacher) has large positive effects on test scores. In fact, the effect of small group instruction is nearly double the effect of the second best instructional component. Few of the included studies investigate the effect more than three months after the intervention, and therefore, little evidence for the persistence of

[2] Instructional methods include among others Coaching of personnel, Incentives, Computer-assisted instruction, Peer-assisted, Progress monitoring and Small and Medium group instruction.

these effects exists.

Non-cognitive skills training. Non-cognitive skills are widely recognized to play an important role for academic performance (Andersen et al., 2020). Duckworth et al. (2007), for example, show no correlation between grit [3] and intelligence. Nonetheless, the authors demonstrate the importance of grit for future academic performance. Their ndings suggest that achievement of academic targets not only involves genes and socio-economic background but also personality traits.

Importantly, non-cognitive skills are malleable. A large review by Kautz et al. (2014) summarizes the literature on interventions targeting cognitive and non-cognitive skills. The authors nd interventions targeting both cognitive and non-cognitive skills to be superior and argue that it is important to consider non-cognitive in addition to cognitive skills when evaluating interventions. For example, recent studies by Alan and Ertac (2018) and Alan et al. (2019) show, using randomized controlled trials, large lasting effects on both cognitive and non-cognitive outcomes of training non-cognitive skills related to patience (the former study) and perseverance (the latter study) for 12 weeks on grade 2 pupils in Turkey.

Training the use of assistive technology Assistive technologies are alternatives to traditional reading and writing for individuals with dyslexia. Currently, dyslexics have permanent access to assistive technology apps through their devices, which enable them to better understand text and, thus, ease their participation in regular teaching on equal terms (Lindeblad et al., 2017).

One of the issues that assistive technology does not solve yet is to ensure that the pupil does not become a passive listener who just makes the technology read words or text aloud. The pupils must still be able to understand the individual words and understand the context of the text. Thus, there are still important didactic challenges in the application of assistive technology (Svendsen, 2017). The RCCD intervention teaches the use as well as the pros and cons of each assistive technology tool such that the tools become a natural part of the dyslexic's everyday life selection. Thus, the dyslexics must be able to identify the problem and then select the best assistive tool.

SBV (2014) and Perelmutter et al. (2017) conclude in their systematic literature reviews on assistive technology that causal evidence is limited. The existing studies rely on few observations or low quality methods, and thus, there is not enough evidence to conclude that assistive technology in general affects academic performance and well-being. New studies should exploit more comprehensive, systematic, longitudinal, and in-depth investigation methods (Perelmutter et al., 2017; Haßler et al., 2016).

[3] Grit is a positive non-cognitive trait on passion and perseverance for long-term goals. Individuals high in grit are able to maintain their motivation and determination over long periods despite failures and adversities (Duckworth et al., 2007).

3.2.2 Danish Library and Expertise Center for people with print disabilities

There exists no national register for dyslexia prior to the implementation of the national dyslexia test in 2015 by the Ministry of Education. Instead, I exploit the membership list from NOTA to construct a valid comparison group. NOTA is an institution under the Danish Ministry of Culture that produce, buy and sell audio books and e-books and develop synthetic speech for individuals with reading disabilities. Consequently, the NOTA membership list is the best Danish database of pupils with dyslexia during the RCCD intervention period.

Membership of NOTA does not occur automatic after a dyslexia diagnose. To become a member, the pupil or the parents must document that the pupil cannot read ordinary printed text. Thus, members of NOTA are by themselves or with the help of their parents actively seeking help to cope with their disability. In practice, either the school principal or a professional with the competence to test for dyslexia must sign the registration form as evidence for the dyslexia diagnose.

This application process causes a potential selection problem because not all dyslexics become NOTA members. This selection, however, does not affect the internal validity of the study since every RCCD participant is also a member of NOTA. A NOTA membership provides access to learning materials that is only relevant for pupils who are using assistive technology. Thus, NOTA members have access to and uses assistive technology but do not receive the same extensive training in its application and potential. Therefore, I am estimating the effects of RCCD relatively to the average intervention for NOTA members. A recent report concludes using survey data that interventions targeted dyslexics are widespread used throughout the Danish municipalities. However, the structure of the interventions varies across municipalities from providing assistive technology tools to comprehensive learning programs such as RCCD (NOTA, 2019). In section 3.6, I discuss the implication of treatment as usual in further details.

3.2.3 Primary and lower secondary education in Denmark

The Danish school system consists of 10 mandatory grades and an optional grade 10. The typical school starting age is the year when the child turns six in which the child enrolls in grade 0. Grade 0 is a transition year taking place at the school, where pupils learn to go to school, but there is no explicit instruction in any academic subjects. Grade 1 to 9 consist of nine years of primary and lower secondary education. Grade 10 is optional and designed for pupils in need of one additional year to be ready for upper secondary education.

Dyslexia is dif cult to diagnose with certainty at a young age and there exists no test for kindergarten children. However, signs of dyslexia are if a child begins to speak at a late age, have problems with rhyming, and slowly learn new words. In primary

and lower secondary school, the possibilities for detecting dyslexia and implementing interventions are easier. Therefore, the Ministry of Education introduced in 2015 the risk of dyslexia test (grade 0 to 1) and the national dyslexia test (grade 3 to 9). The risk of dyslexia test is a test for early identi cation of pupils at risk of developing severe decoding dif culties, including dyslexia. It indicates risk of dyslexia but not necessarily dyslexia such that the teachers can implement a preventive intervention. If there is still suspicion of dyslexia at the end of grade 3 the teachers test using the national dyslexia test and make a de nitive diagnosis.

3.3 Data and descriptive statistics

The sample consists of 46 , 348 NOTA pupils in public schooling across all 98 Danish municipalities, of which 513 pupils were enrolled in RCCD at some point in between 2010/2011 and 2018/2019. The low treatment ratio occur because only pupils in the municipality of Aarhus are subjects for treatment. To investigate how RCCD affects pupil outcomes, I leverage Danish administrative register data that covers the entire population of children in elementary schooling and is available through Statistics Denmark. Focal to this study is the NOTA membership list that is the best national-wide register for individuals with dyslexia and the Danish Student Register, which is a unique longitudinal dataset. This enables me to follow dyslexics schooling information such as school and classroom movements, type of school as well as special needs teaching from the school year 2009/2010 to 2018/2019. I augment this data with rich socio-economic information describing demographics, ethnicity, marital status, and education, just as I exploit information regarding reading abilities from the Danish Ministry of Education. Finally, for the period 2015-2019, I construct measures of personality traits and school well-being using the Danish Ministry of Educations national well-being survey.

On average, 57 pupils participate in the RCCD program each school year, half of them starting in September and the other half in February. Prior to being considered a candidate for RCCD, the pupils must have been through prolonged or repeated special educational interventions at their local schools such as reading courses, group or one-to-one tuition, additional classroom support, etc. without any or minor positive effects. Around half of the RCCD participants enrolled in the program at grade 5 and 6. This re ects the fact that dyslexia is usually determined at grade 4 due to the timing of the national dyslexic test.

Next, I provide an overview of the outcome variables. The outcome variables cover three areas: Reading abilities, personality traits, and well-being. Table 3.1 provides an illustration of the timeline for the outcomes. Finally, I characterize the RCCD pupils against NOTA members and non-dyslexics.

Table 3.1: Timeline of outcome variables

	Grades	Years
Reading abilities	2nd, 4th, 6th, and 8th	2010-2019
Personality traits	4th-9th	2015-2019
School Well-being	4th-9th	2015-2019

Notes: All variables are measured in the spring semester of the year.

3.3.1 Reading Abilities

Reading performance variables originate from the mandatory national test in reading that takes place in the spring of grade 2, 4, 6 and 8. The national reading test was introduced at a national scale in 2009/2010. It simultaneously tests three cognitive domains of reading, called pro le areas: 1) Language Comprehension, 2) Decoding, and 3) Text Comprehension. A clear advantage of the national test is that it is IT-based, self-scoring and adaptive. Thus, the computer does the scoring automatically such that it re ects objective reading abilities and not in uenced by teacher opinions such as classroom misbehavior. Instead of giving all pupils the same questions, the national test program re-estimates a new ability level after each question and adjusts the dif culty level of the next question. Therefore, the nal ability measure for each pro le area is a function of the dif culty level of the questions and the ability of the pupil. The nal scores are measured on a continuous logit scale distributed from -7 to 7. See Beuchert and Nandrup (2017) for a thorough description of the Danish national tests. On the individual level, national test scores explain 48% to 51% of the variation in average Danish and math exam performances. Across all subjects, a 1 SD increase in test scores is associated with approximately 2 grade points in the GPAs, a 19% higher probability of enrolling in upper secondary education, and a 16% higher probability of completing general upper secondary education (Beuchert and Nandrup, 2017).

I standardize the reading ability measures on the full population within school years and pro le areas to mean zero and standard deviation of one to render the results comparable to effect sizes of other studies.

Table 3.2 shows summary statistics of RCCD participants, NOTA and non-NOTA members. The reading abilities are measure one or two school years prior to RCCD participation. It is evident that RCCD participants reading abilities are below other dyslexics and especially non-dyslexics. Particularly for decoding abilities, where they perform 0 .35 SD below other dyslexics and 1 .6 SD below non-dyslexics. This is in line with the literature as dyslexia involves problems identifying speech sounds and learning how they relate to letters and words, i.e. dyslexia makes the decoding process dif cult, regardless of the pupil's level of academic skills. This lack of decoding abilities in uences also language comprehension and text comprehension, where RCCD scores approximately 0 .1 and 0.9 SD below respectively other dyslexic and non-dyslexics.

Thus, if pupils cannot decode words, they cannot understand what the text means, even if they understand the meaning of words in conversations.

Table 3.2: Descriptive Statistics

	(1) RCCD		(2) NOTA		(3) Non-NOTA		(4)	(5)
	Mean	SD	Mean	SD	Mean.	SD.	(1)-(2)	(1)-(3)
Demographics								
- Boy (1/0)	0.600	(0.490)	0.561	(0.496)	0.505	(0.500)	0.040	0.095
- 1st or 2nd generation immigrant (1/0)	0.103	(0.305)	0.060	(0.237)	0.110	(0.313)	0.044	-0.007
- Preventive personalized interventions (no.)	0.035	(0.239)	0.053	(0.293)	0.033	(0.236)	-0.017	0.002
- Siblings (no.)	1.380	(0.979)	1.328	(0.928)	1.318	(0.913)	0.052	0.062
- Living with both parents (1/0)	0.661	(0.474)	0.647	(0.478)	0.671	(0.470)	0.014	-0.010
- School changes (no.)	0.232	(0.537)	0.431	(0.695)	0.328	(0.611)	-0.199	-0.097
- Class changes (no.)	0.055	(0.236)	0.092	(0.311)	0.038	(0.203)	-0.038	0.017
- Relocations (no.)	1.175	(1.382)	1.735	(1.575)	1.629	(1.472)	-0.560	-0.454
- Special needs teaching (1/0)	0.056	(0.229)	0.106	(0.308)	0.033	(0.178)	-0.051	0.023
- Mother's age at birth (years)	30.907	(4.864)	29.715	(4.870)	30.163	(4.825)	1.193	0.744
- Father's age at birth (years)	33.254	(5.320)	32.379	(5.615)	32.845	(5.675)	0.875	0.409
Mother's marital status								
- Cohabiting couple (1/0)	0.107	(0.310)	0.098	(0.297)	0.082	(0.275)	0.010	0.025
- Divorced (1/0)	0.181	(0.386)	0.161	(0.367)	0.152	(0.359)	0.021	0.029
- Married (1/0)	0.620	(0.486)	0.658	(0.474)	0.686	(0.464)	-0.038	-0.066
- Single (1/0)	0.080	(0.271)	0.072	(0.259)	0.067	(0.250)	0.008	0.013
Father's marital status								
- Cohabiting couple (1/0)	0.105	(0.307)	0.095	(0.293)	0.081	(0.273)	0.010	0.024
- Divorced (1/0)	0.136	(0.344)	0.152	(0.359)	0.140	(0.347)	-0.015	-0.004
- Married (1/0)	0.643	(0.480)	0.651	(0.477)	0.681	(0.466)	-0.008	-0.038
- Single (1/0)	0.078	(0.268)	0.066	(0.248)	0.057	(0.231)	0.012	0.021
Mother's highest educational degree								
- No degree or primary school (1/0)	0.170	(0.376)	0.216	(0.412)	0.166	(0.372)	-0.047	0.004
- High School (1/0)	0.060	(0.239)	0.045	(0.206)	0.061	(0.239)	0.016	0.000
- Vocational traning (1/0)	0.343	(0.475)	0.443	(0.497)	0.349	(0.477)	-0.100	-0.006
- Academy higher education (1/0)	0.060	(0.239)	0.047	(0.211)	0.051	(0.220)	0.014	0.010
- College (1/0)	0.240	(0.427)	0.196	(0.397)	0.249	(0.432)	0.044	-0.009
- University (1/0)	0.127	(0.333)	0.053	(0.225)	0.125	(0.330)	0.073	0.002
Father's highest educational degree								
- No degree or primary school (1/0)	0.226	(0.419)	0.278	(0.448)	0.219	(0.414)	-0.052	0.007
- High School (1/0)	0.047	(0.211)	0.029	(0.168)	0.054	(0.226)	0.018	-0.007

- Vocational traning (1/0)	0.378 (0.485)	0.497 (0.500)	0.395 (0.489)	-0.119	-0.016
- Academy higher education (1/0)	0.109 (0.312)	0.063 (0.244)	0.076 (0.264)	0.046	0.034
- College (1/0)	0.127 (0.333)	0.077 (0.266)	0.123 (0.329)	0.050	0.003
- University (1/0)	0.113 (0.317)	0.056 (0.230)	0.133 (0.340)	0.057	-0.020
National reading test					
- Language Comprehension (std.)	-0.823 (1.175)	-0.697 (1.125)	0.091 (0.935)	-0.126	-0.914
- Text Comprehension (std.)	-0.862 (1.053)	-0.770 (0.984)	0.101 (0.944)	-0.093	-0.963
- Decoding (std.)	-1.475 (0.894)	-1.127 (0.917)	0.143 (0.902)	-0.348	-1.618
School well-being					
- School Connectedness (std.)	-0.115 (0.931)	-0.100 (1.040)	0.089 (0.975)	-0.015	-0.204
- Learning Self-Ef cacy (std.)	-0.494 (0.936)	-0.429 (1.013)	0.101 (0.975)	-0.066	-0.595
- Learning Enviroment (std.)	-0.077 (1.007)	0.067 (1.012)	0.133 (0.982)	-0.143	-0.210
- Classroom Management (std.)	-0.246 (1.048)	-0.155 (1.059)	-0.007 (0.988)	-0.091	-0.239
Personality traits					
- Conscientiousness (std.)	-0.387 (0.960)	-0.367 (1.052)	0.073 (0.990)	-0.020	-0.461
- Agreeableness (std.)	-0.220 (1.101)	-0.223 (1.076)	0.035 (0.977)	0.003	-0.255
- Emotional Stability (std.)	-0.111 (0.912)	-0.148 (1.076)	0.052 (0.986)	0.037	-0.163
Absence					
- Sick (percent)	2.899 (3.397)	3.275 (3.886)	3.093 (3.838)	-0.376	-0.194
- Illegal (percent)	0.719 (1.948)	0.721 (2.436)	0.725 (2.517)	-0.002	-0.006
- Legal (percent)	1.263 (1.799)	1.446 (2.477)	1.473 (2.327)	-0.183	-0.211
Number of individuals	513	45,839	431,009		

Notes: The table shows the descriptive statistics for RCCD participants against NOTA and non-NOTA members. Column 4 (5) shows difference between RCCD and NOTA (non-NOTA) from a regressing each covariate on the RCCD indicator. All variables are measured the school year prior to treatment. Except National tests that is measured up to two school years prior. The table is based on non-missing data.

3.3.2 School Well-being and Personality traits

The Danish Ministry of Education implemented a 40-item well-being questionnaire in 2014/2015, which is a yearly national survey for all public school pupils in grade 4 to 9. The Danish Well-being Survey is electronically distributed to all pupils. The survey is part of the regular teaching, and all pupils in the class must respond to the survey during the same lesson. Teachers are to tell the pupils that they should respond honestly and stress that results are anonymous to their parents, teachers, or other employees at the school. The teachers are encouraged to read questions aloud if a pupil has dif culties understanding them. Additionally, the teachers have the authority to exempt pupils from the survey if they do not feel well answering the survey or if they are not capable of answering the questionnaire. This could

for example be special needs pupils (Andersen et al., 2020; Niclasen et al., 2018). Even though the survey is a mandatory part of the general curriculum, there are no consequences if the pupil is not answering due to sickness or other types of absences. Table A.1 shows the average response rate for RCCD participants, NOTA, and Non-NOTA members. It is clear from the table that the RCCD group is a special group where the average response rate is 76%, which is 4.6% (7.5%) lower than the NOTA (non-NOTA) group.

One way of exploiting the national well-being survey is to construct three personality trait scales from the Big Five Model (Andersen et al., 2020). Agreeableness relates to how pupils tend to treat relationships with others. Conscientiousness describes pupils' ability to be responsible and work carefully to get things done. Emotional Stability measures absences of characteristics such as anxiety, insecurity and self-pitying. Importantly, the authors nd a high correlation between conscientiousness and future academic performance.

Another way of exploiting the information in the national well-being survey is the one recommended by Niclasen et al. (2018). They propose a four-factor model that includes 27 of the 40 items from the national well-being survey. The four-factor model measures School Connectedness, Learning Self-Ef cacy, Learning Environment and Classroom Management. According to the authors, these four scales measure central aspects of school well-being. School Connectedness is the belief held by the pupil that teachers and peers in the school care about their well-being and learning. Learning Self-Ef cacy describe pupils beliefs and attitudes toward their capabilities to achieve academic success. Learning Environment deals with pupils' experience of motivation and co-determination, as well as the help and support of teachers and the surroundings in order to complement future learning. Classroom Management measures pupils' experience of the classroom as well as classroom management by the teachers.

Some of the 40 items from the national well-being survey are used in both the personality trait and the school well-being scales. Table A.2 shows the items used to construct each scale. The scales are standardized, rst by standardizing each item, then calculating the average across all standardized items in each scale and nally standardizing the overall scales.

The school well-being as well as personality traits of the RCCD are at the same level as other dyslexics across all scales. However, when comparing RCCD pupils with non-dyslexics it is evident that they are far less satis ed with their schooling and their personality traits are signi cantly below the average population (see table 3.2). Especially, for Learning self-ef cacy and Conscientiousnessin which RCCD participants scores between 0.45 to 0.6 SD below the non-dyslexics. Furthermore, School Connectedness, Classroom Management, Learning Environment , Agreeableness and Emotional stability all show a difference of approximately 0 .2 standard deviations. This indicates that dyslexics do not thrive in school.

3.3.3 Covariates

Table 3.2 also presents a summary statistics for rich set of socioeconomic variables, recorded the school year prior to RCCD enrollment. The descriptive statistics show that there is an overrepresentation of boys participating in the RCCD program with 4% fewer boys in the NOTA register and 10% fewer in the non-NOTA (non-dyslexic) group. There are 4% fewer immigrants in the NOTA group compared to both the RCCD and non-NOTA groups, indicating that not all dyslexic immigrants are member of NOTA. Additionally, the RCCD pupils have relocated approximately one time during their life whereas the NOTA and non-NOTA group is closer to two times. This is also visible in the number of school movements where the RCCD pupils have signi cantly fewer. Interestingly, I observe that parents of the RCCD pupils are higher educated than the NOTA group parents and in line with the non-NOTA parents, i.e. RCCD pupils are inclined to live in a household of high socioeconomic status (SES henceforth) compared to other dyslexics. For the mothers (fathers) 43% (36%) have acquired an academy higher education, College or University degree where these numbers are 30% (20%) for the NOTA group and 43% (33%) for the non-NOTA group. Related to the educational level, the parents of the RCCD pupils are on average one year older when their child is born.

3.4 Empirical Strategy

The overarching goal of this paper is to estimate the causal effect of RCCD on reading performance, personality traits, and school well-being. Clearly, the key challenge in such an analysis is to estimate outcomes in the absence of RCCD participation. One might worry that non-random selection into RCCD participation challenge the identi cation. Thus, pupils participating in RCCD comprise a different population than pupils who do not participate.

In order to address this potential endogeneity problem, I use a difference-in-difference strategy corresponding to a xed effect analysis. My strategy rst compares one pupil's outcomes after RCCD participation with the same pupil's outcomes before RCCD participation. This rst difference accounts for time-invariant individual outcomes. However, it is unlikely that individual level outcomes do not change over time. To account for this, I exploit information not only for the RCCD pupils, but also for other dyslexics (NOTA members) and the ability to follow their outcomes over multiple pre and post treatment time-periods. The comparison group of NOTA pupils is assigned a synthetic (random) participation grade for practical reasons.

The municipality centrally handles visitation to RCCD based on a recommendation from the local school and the municipalities reading consultants in agreement with the pupil's parents. An obvious worry is that municipality select the pupils most severely affected by dyslexia conditional on other special education activities having failed. Exploiting only dyslexics within the municipality of Aarhus as comparison

group generates a selection problem leading to a downward bias of the treatment effects. Therefore, I utilize information for all of the Danish municipalities to account for this selection problem within the municipality of Aarhus. Access to administrative data on, especially, school performance, legal absences and special needs teaching allows me to identify struggling untreated pupils in other municipalities with similar unresponsive behavior to general special education. Table 3.2 show that parents in uence the likelihood of being treated, i.e. pupils of high educated parents is more likely to participated. Falling to account for parents abilities positively correlates with RCCD participation would likely create a upward bias of the treatment effects.

The analyses begin with the following event study equation:

$$y_{it} \text{ Æ} u_i \text{ Å} \sum_{j \text{ Æ} i 5, j 6 \text{ Æ}}^{X4} \pm_j \text{ ¢} 1(j \text{ Æ} t)_{it} \text{ ¢} 1(RCCD \text{ Æ} 1)_i \text{ Å} \sum_{j \text{ Æ} i 5, j 6 \text{ Æ}}^{X4} \circ_j \text{ ¢} 1(j \text{ Æ} t)_{it} \text{ Å} \text{²}_{it} \quad (3.1)$$

where y is the outcome of interest, 1(RCCD) indicates RCCD participation and (j Æt) are time indicators relatively to participation. $\pm_j$ are the effects of RCCD participation by school year relative to the school year prior to participation (j Æ0). $\circ_j$ are the effects for the NOTA pupils relative to their participation school year. u_i is individual level xed effect and ²_{it} is the error term. Thus, the coef cient of ± measures the average treatment effect of the treated (ATT henceforth) and is the parameters of interest. Standard errors are clustered at the individual level as outlined in Bertrand et al. (2004).

Ideally, I would observe the counterfactual, i.e. what would have happened to the RCCD group in absence of treatment. However, the counterfactual is unobservable, and instead I exploit the panel structure of the data that enables within pupils estimation with a comparison group, which for identi cation rely solely on the parallel trends assumption. Thus, the xed effect estimator produces causal effects if and only if the RCCD and NOTA groups would have had the same trends in the post-periods in absence of treatment. The parallel trend assumption is a much weaker identifying assumption than models based on the selection on observables assumption. Unobserved individual time- xed heterogeneity will not bias the estimations. Only differing time-trends in the treatment and comparison groups will bias the xed effect estimation. In the robustness section 3.5 below, I also report the results when using alternative comparison groups and discuss the parallel trend assumption.

3.4.1 Difference-in-Difference Strategy with Entropy Balance

Since the parallel trends assumption is critical for identi cation, I extend the xed effect framework to account for any observed difference between the RCCD and the NOTA groups prior to treatment. I combine the xed effect estimator with Entropy Balancing. The reason for combining xed effect with a weighting strategy is to reduce bias due to different distributions of covariates in the RCCD and NOTA groups (Heckman et al., 1997; Blundell et al., 2004; Abadie, 2005). Entropy Balancing is

a data processing method that obtain covariate balance with a binary treatment variable (Hainmueller, 2012). Hainmueller and Xu (2013) explain entropy balance as a generalization of the propensity score adjustment method suggested by Rosenbaum and Rubin (1983) that addresses its limitations. The propensity score is typically calculated using a logistic regression, and the resulting balance is assessed to see if the individual assigned weights equalizes the covariates between the two groups. Whereas, Entropy Balance directly calculates weights by integrating covariate balance directly into the weights. See Hainmueller and Xu (2013) and Hainmueller (2012) for a thorough description of the entropy balance method.

Table A.3 shows descriptive statistics for pupils and parents background characteristics measured in the school year prior to RCCD such that they are not affected by the intervention. The RCCD participation grade is likely to affect the selection process and thus entropy balancing is conducted within each grade. Column 2 to 5 reports the unadjusted mean, standard deviation and difference in means between the RCCD and NOTA groups. It is evident that pupils of high SES parents have a higher propensity to participate in the RCCD program. Thus, pupils of mothers (fathers) with either a college or a university degree are 12% (11%) more likely to be in the RCCD group. Column 5 in table A.3 also shows that pupils in the RCCD group have fewer school, classroom, and place of residence movements as well as older parents. Additionally, 5% fewer are receiving additional hours of special needs teaching and 4% more are 1st or 2nd generation immigrants.

The ex ante differences on observed characteristics discussed above are a threat to the parallel trend assumption. For example, if we believe that additional hours of special needs teaching or low SES attens the pupils learning curve this will violate the parallel trends assumption simply because the RCCD pupils will accumulate skills faster, and the xed effect estimate is upwards biased. Entropy Balance deals with this threat to identi cation by ensuring perfect balance between the RCCD and NOTA groups on all observed covariates prior to treatment. Ryan et al. (2018) illustrate using simulations that a combination of a balancing strategy and the difference-in-difference framework does well at dealing with non-parallel trends in a context of health care policy interventions.

Table A.3 shows the covariates used in the balancing speci cation where the rst moment of the covariates are balanced. [4] In section 3.5.3 I test alternative specii cations of the balancing strategy to ensure the credibility of the results. The last column in table A.3 shows that the entropy balance ensures no difference in the means between the RCCD and NOTA groups prior to treatment on observed factors. Thus, Entropy Balancing ensures that the NOTA group is on average similar to the RCCD group.

[4] Balancing on the pupils basic demographics such as gender, age, ethnicity, number of siblings, living arrangement, school and classroom changes and special needs teaching, as well as parents marital status, parents educational level and one period pre-trend in reading performance, personality traits, and school well-being.

3.5 Results

This section presents the results. First, I show the event study representation of the effects on reading abilities, personality traits, and school well-being. Additionally, I supplement the event-study model with a minimalist difference-in-difference setup that do not allow effects to vary with distance to RCCD participation. Then, I perform a set of robustness checks to ensure the validity of the estimates and nally, conduct a set of heterogeneity analyses.

3.5.1 Event study representation

I start by analyzing the effects of RCCD using a event study representation. Figure 3.1 presents the ATT at each time-period, with the time-periods leading up to RCCD (t=0) as baseline for the three pro le areas of the national reading test. The national reading test takes place every second school year starting at grade 2 and ending at grade 8 as illustrated in table 3.1. Thus, it is important to notice that only pupils treated in grade 4 or 5 are included in periods 3 and 4 simply because the older pupils would attend grade 9 or have left lower secondary schooling at that time-point. Critically, I observe, for all three graphs, no differential pre-treatment trends, i.e. the differences are not signi cantly different from zero. Indicating similar development in reading performance prior to RCCD participation across the two groups. Both the language comprehension (a) and text comprehension (b) graphs show large immediate treatment effects of 24% and 20% of a standard deviation already in the period after the RCCD camp. For language comprehension there is a small decease in the effect size in the later periods whereas there is a similar increase for text comprehension. These changes are small and not signi cantly different from the previous period, and thus, the effect on reading shows signs of being persistent over time. In terms of the effect on decoding (c), the line is close to being at indicating that RCCD has no impact on future decoding abilities. This is not surprising since RCCD concentrates on providing the pupils with the necessary tools to participate in normal classroom teaching. Thus, RCCD teaches pupils the use of assistive technology and reading strategies such that they are able to read and write text at an age-appropriate level and not focusing on their decoding issues caused by their diagnose. Therefore, decoding works as a placebo test.

Figure 3.2 and 3.3 show similar graphs for the effect on personality traits and school well-being. All scales originate from the national well-being survey, which takes place once a year from grade 4 and started in the spring of 2015 as explained in section 3.3 and table 3.1. Thus, fewer pupils are included in these analyses and hence, the standard errors are larger and pre-RCCD investigations are only possible for the pupils treated in grade 6 to 8.

In gure 3.2 (a) the RCCD pupils have a higher level of conscientiousness prior to RCCD. However, the difference is not signi cantly different from zero and their