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Studies of Danish Higher Education

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EMPIRICAL ESSAYS ON TECHNOLOGY SUPPORTED LEARNING – STUDIES OF DANISH HIGHER EDUCATION

PhD Series 02.2023

Mette Suder Franck

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STUDIES OF DANISH HIGHER EDUCATION

CBS PhD School Department of Economics

PhD Series 02.2023

CBS COPENHAGEN BUSINESS SCHOOL
HANDELSHØJSKOLEN

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Empirical Essays on Technology Supported Learning

Studies of Danish Higher Education

Mette Suder Franck

Supervisors: Lisbeth la Cour, Annemette Kjærgaard, Julie Buhl-Wiggers

Copenhagen, September 2023

CBS

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Til min mor fordi du har måtte lægge øre til alle mine frustrationer, tvivl og udfordringer.

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I am thankful that I got the chance to be part of CBS's COVID-19 evaluation team and for the people that I got to work with as a result. Had it not been for Annemette facilitating the establishment of this team, I would not have exchanged research ideas with Michael Møller Nielsen, whose interest in educational inequality among students at CBS greatly overlapped with my own and whose knowledge of the subject inspired the hypotheses in my single-authored paper. Without this team I also would not have worked with Sine Zambach, who, as Michael, possesses great institutional knowledge of CBS and additionally have helped me collect (and make sense of) the unique data set on CBS students that forms the basis of the third chapter of this thesis.

During my time at CBS, I have had the pleasure of working with two extremely talented student assistants, Sarah Koelemij Andersen and Sarah Nørby, who have assisted mine and my co-authors' work with meticulous and elegant data work. I am greatly appreciative of the assistance they have provided.

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I am happy that I, despite having a great part of my PhD studies disrupted by the COVID-19 pandemic, still got the opportunity to do a research stay abroad and even happier that this entailed visiting Douglas McKee and George Orlov at Cornell University. In addition to being great researchers, they are both kind and friendly human beings that I absolutely enjoyed getting the chance to participate in weekly research meetings with. I am also truly grateful for George taking the time to discuss my research with me and for Julie suggesting that we reached out to Doug and George in the first place. My visit at Cornell definitely boosted my enthusiasm for educational economics and I returned to CBS feeling recharged and motivated for the final stage of my PhD.

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Summary

This thesis consists of three chapters that empirically investigates how the educational outcomes of students in higher education have been affected by technology supported learning.

The first chapter, Chapter 1, is co-authored with Julie Buhl-Wiggers, Lisbeth la Cour, and Annemette Kjærgaard and investigates the implementation of a blended flipped classroom format in higher education. This pedagogical format often relies on technology to deliver content that has traditionally been delivered in-class out of the classroom and hereby facilitating active learning during face-to-face instruction. Though the popularity of flipped classroom has been increasing in recent years, there is still limited formal quantitative evidence on its effectiveness. In the study presented in Chapter 1, we analyze two iterations of a randomized field experiment that introduced flipped classroom to freshmen students in an undergraduate macroeconomics course. We complement recent literature by considering a large-scale flipped classroom intervention that affected 933 students and 11 teachers. Moreover, we present new knowledge by examining heterogeneous treatment effects according to two classroom-level factors, namely teachers and peer ability composition. Our analyses show a positive but insignificant average effect of flipped classroom on students' performance in the final exam and that there was no differential effect for students exposed to different means of their peers' ability levels. We do, however, find substantial shifts in teachers' relative ranks in terms of their ability to benefit student performance when comparing traditional and flipped classroom conditions, which suggests that the best teacher in a traditional teaching environment is not necessarily the best teacher in a flipped classroom environment.

Chapter 2 is co-authored with Julie Buhl-Wiggers and considers the effects of a predominantly online remedial math course. Math skills have been found to be highly important for economics students in general and

for their success in introductory microeconomics in particular (Allgood et al., 2015, Ballard and Johnson, 2004, Schuhmann et al., 2005). Unfortunately, many freshmen students in higher education do not possess the necessary math skills upon enrollment and thus might struggle academically when they encounter math-based courses (Bettinger and Long, 2009; Büchele, 2020). Many institutions of higher education have therefore attempted to help underprepared students by offering remedial math courses. However, there are only few studies indicating that they benefit student outcomes, of which the time and monetary cost have often been highlighted as substantial drawbacks. In Chapter 2, we analyze how offering an online remedial math course to freshmen students right at the beginning of their studies affected their performance in a subsequent microeconomics course. More specifically, the remedial offer consisted of a single face-to-face workshop and a self-paced online module with tutorial videos and associated exercises with automated feedback. To investigate the effect of the remedial course, we relied on a fuzzy regression discontinuity design and assigned students to treatment, i.e. enrollment in the online module of the remedial offer, based on whether they performed above or below a certain threshold in a mathematical assessment. 58% of the freshmen students completed the assessment and based on their performances we enrolled 806 of them in the online module. The self-paced format of the online module placed a responsibility on students to take control of their own learning, since participation in the math course was fully voluntary. Our data indicates that students might have struggled to take on this responsibility, as we see limited compliance with assignment to treatment with the online module. As a consequence, it is not too surprising that we find do not find a statistically significant effect of the online module on grades in microeconomics. This suggests that incorporating student preferences in the design of online remediation is highly important.

In Chapter 3, I investigate whether the effect of COVID-19 on student outcomes varied according to student characteristics. The pandemic had extensive effects on students on all educational levels and involved an unexpected shift towards online instruction. I cannot disentangle the effect of this change from other concurrent events that might have affected student outcomes and as a consequence, the notion of technology supported learning appears only implicitly in this chapter. Previous lit-

erature has indicated that the effects of COVID-19 on student outcomes were disproportionately felt by students with lower socioeconomic backgrounds (Agostinelli et al., 2022; Hansen et al., 2021). This have been argued to relate to differences in how the pandemic affected the economic outcomes and health across socioeconomic groups, which suggests that social welfare policies might play an important role for whether we might observe a differential effect of COVID-19 on student outcomes (Aucejo et al., 2020). The paper presented in Chapter 3 adds to the knowledge of the differential effect of COVID-19 on student outcomes in higher education, by analyzing how the pandemic affected students with different socioeconomic backgrounds in a generous welfare state. The empirical investigation is based on an estimation strategy inspired by Difference-in-Differences and a dataset combining university-level administrative data sources with national-level register data. The findings suggest that, even within the context of generous welfare state, students with low-income parents and students who themselves had a low income, were relatively adversely affected by the pandemic. Empirically assessing the mechanisms underlying these differential effects constitutes an important path for future research to inform policy makers on how they might mitigate the effects of future events that causes similar disruptions to the educational system.

Resumé

I denne afhandling belyser jeg i tre separate kapitler, hvordan forskellige uddannelsesmål for studerende på længere videregående uddannelser påvirkes af teknologisk understøttet læring. Uanset om implementeringen af denne type læringsform har været intentionel eller ej.

Det første kapitel er udarbejdet i samarbejde med Julie Buhl-Wiggers, Lisbeth la Cour og Annemette Kjærgaard og undersøger effekten af at introducere "flipped classroom" i undervsiningen af studerende på længere videregående uddannelser. Dette pædagogiske format bliver ofte implementeret som et blandet ("blended") format, der både inkluderer online og traditionelle klasseværelsesaktiviteter. For eksempel ved i stedet at præsentere indhold, der traditionelt har foregået i klasseværelset, online for på denne måde at give plads til, at tiden i klasseværelset kan bruges på aktiviteter, der understøtter aktiv læring. Populariteten af flipped classroom er steget betydeligt i de senere år, men dette til trods er der stadig begrænset empirisk baseret viden om, hvorvidt dette pædagogiske format er gavnligt for studerendes læring. Særligt er der mangel på viden, der indikerer, om flipped classroom varierer på tværs af lærere og i forhold til den akademiske formåen blandt en studerendes medstuderende.

Med studiet i Kapitel 2, undersøger vi disse spørgsmål ved hjælp af et randomiseret feltstudie, hvor holdtimerne for halvdelen af en gruppe førsteårsstuderende i et introducerende makroøkonomisk kursus blev omdannet til flipped classroom. I vores analyse evaluerer vi effekten for to iterationer af den pædagogiske intervention og har i alt 933 studerende og 11 lærere i vores datasæt. Vores resultater indikerer, at flipped classroom har en positiv, om end insignifikant, effekt på de studerendes gennemsnitlige beståelsesprocent og karakter i det makroøkonomiske fags afsluttende eksamen. Ligeledes finder vi ikke nogle signifikante heterogene effekter på disse i forhold til medstuderendes gymnasiegennemsnit. Derimod observerer vi flere bemærkelsesværdige skift i læreres relative rangerering mellem flipped classroom og den traditionelle undervisningsform, hvilket indikerer, at de lærerkomptencer, der sikrer gode resultater for studerende i det ene format, ikke nødvendigvis er de samme som dem, der sikrer det i det andet.

Kapitel 2 er skrevet sammen med Julie Buhl-Wiggers og undersøger effekten af et tilbyde førsteårsstuderende et, primært online, forberedende matematikkursus. Matematikkundskaber er essentielle for studiet af økonomi, men desværre oplever videregående uddannelsesinstitutioner i både Danmark og resten af verden, at mange studerende starter på universitet uden at besidde tilstrækkelige matematikfærdigheder. Derfor er flere institutioner begyndt at tilbyde et forberedende matematikkursus til førsteårsstuderende, dog uden at der foreligger formel empirisk evidens, der understøtter, at et sådan tilbud er gavnligt for de studerende. Med dette studie belyser vi effekten af et online forberedende matematikkursus, der, i modsætning til de kurser som undersøges i eksisterende studier, både er forbundet med lave omkostninger og tilbyder studerende en stor grad af fleksibilitet i forhold til hvornår og hvor meget, de har lyst til at benytte kurset. Vores empiriske udgangspunkt er et fuzzy regression discontinuity design, hvor vi på baggrund af studerendes besvarelser af en matematisk placeringstest selektivt indrullerede dem i den online del af det forberedende matematikkursus, hvis deres besvarelser ikke oversteg en fastsat tærskel. 58% af de studerende besvarede testen og på baggrund af deres besvarelser indrullerede vi 806 af dem i den online del af kurset. Da deltagelse i kurset var et tilbud til de studerende og dermed ikke obligatorisk, forudsætter en effekt af kurset, at studerende aktivt tager ansvar for egen læring og tilvælger selvstudie af det online kursus. Vores data indikerer desværre, at dette langtfra var tilfældet. Derfor er det heller ikke så overraskende, at vi ikke finder nogen signifikant effekt af det forberedende matematikkursus. Det lave engagement blandt de studerende indikerer, at det er særlig vigtigt at tage højde for studerendes præferencer og incitamenter i forbindelse med design af online kurser.

I afhandlingens sidste kapitel, Kapitel 3, undersøger jeg om Covid-19 pandemien havde forskellige effekter på studerende alt efter deres socioøkonomiske baggrund. Selvom pandemien betød at meget undervisning måtte flyttes online, er det desværre ikke muligt for mig at adskille effekten af online læring fra alle de andre faktorer, som var relateret til Covid-19 og også påvirkede de studerende. Derfor indgår teknologisk understøttet læring kun implicit i dette kapitel. Studiet er motiveret af tidligere litteratur, som viser, at effekten af pandemien på studerendes resultater var ulige fordelt mellem studerende med forskellige baggrunde. Dette er blevet foreslået at hænge sammen med forskelle i, hvordan Covid-19 påvirkede økonomi og sundhed på tværs af disse grupper, i hvilket tilfælde velfærdspolitik og uddannelsesrammer kan have været afgørende for de forskellige effekter. Derfor er mit bidrag til litteraturen om effekten af Covid-19 på studerende i videregående uddannelse at undersøge, om der var differentielle effekter af pandemien blandt studerende i en kontekst af en omfangsrig velfærdsstat. Mine analyser indikerer, at der var en differentiel effekt af pandemien på studerende med lav-indkomstforældre og som selv havde en lav indkomst, sådan at de, sammenlignet med deres mere velstillede medstuderende, blev relativt negativt påvirket. Jeg diskuterer en række mulige mekanismer, der kan undderlægge disse rersultater, men kan på baggrund af mit tilgængelige datasæt hverken empirisk be- eller afkræfte de foreslåede kanaler. Derfor er videre undersøgelser af potentielle mekanismer et vigtigt emne for fremtidige studier, hvis vi skal kunne informere beslutningstagere i højere uddannelse om, hvordan de kan modvirke effekten af lignende forstyrrelser til undervisningen i fremtiden.

Contents

List of Figures

List of Tables

Introduction

Today most institutions of higher education rely on some form of technology supported learning (Müller & Mildenberger, 2021). Although the prevalence of technology in the teaching and learning practices was accelerated by the COVID-19 pandemic, technology adoption is by no means a new phenomena in higher education where it has been continuously growing since the 1990s (Kirkwood and Price, 2014).

A key motivation for technology supported learning can be found in the related term "technology-enhanced learning", which implies an inherent optimism with respect to the potential of technology to *enhance* student learning (Kirkwood and Price, 2014). For example by using technology to facilitate active learning, which has been found to increase student outcomes (e.g. by Freeman et al., 2014), by moving content that has traditionally been delivered in-class out of the classroom as in the flipped classroom format (Lai et al., 2021). Other examples of hypothesized benefits of technological supported learning include those related to the possibilities it offers for supporting more individualized learning and to students in terms of flexibility (Li et al., 2022).

In addition to such pedagogically founded motivations, the use of technology in education has also been argued to constitute a way to increase its productivity and reduce costs. Even though the productivity of education is inherently difficult to measure, there are indications that it is lagging significantly behind that of the private sector (Hanushek & Ettema, 2017). A possible explanation can be found in the notion of the "Baumol's Disease", which states that the costs of education have increased at a rate surpassing that of the associated productivity, due to a limited scope for productivity gains. However, recently optimism with respect to this question has risen, as researchers, such as Bowen et al. (2014), have suggested that the acceleration in technological advancements presents an opportunity to overcome the problem of the Baumol's Disease. This has spurred an interest in investigating the scope for the use of technology to improve educational outcomes and led to a number of studies on both purely online courses and on blended formats that integrates online elements with traditional face-to-face instruction.

Already in 2004, a survey by Marquis reported that 90% of university instructors believed that the use of technology supported learning in the form of blended learning was superior to traditional classroom instruction. When looking at the empirical evidence, one gets a more ambiguous picture of its effectiveness, as in the review by Zhao and Breslow (2013), who reported that less than half of the reviewed studies showed outcomes favouring blended learning. The mixed findings can be a consequence of the fact that the empirical research on online and blended learning ranges from primary to higher education, as well as across different types of learning formats. As a consequence, the potential of technology adoption in education may be difficult to assess at a very general level, especially since the online learning component, as highlighted by Bowen et al. (2014) *"is hardly one thing"* (p.7). Therefore, the effectiveness of technology supported learning may be better understood by evaluating its potential in the specific contexts in which it would be put to use and by making more conscious distinctions between the different technology supported pedagogical formats.

When focusing on the educational context of higher education, a meta-analysis by Bernard et al. (2014) indicates that there are positive, albeit numerically modest, significant effects of technology integration, while Arbaugh et al. (2010) review studies of blended learning in management-oriented disciplines and similarly find indications of positive outcomes. Though these meta-studies inspire faith in the use of technology supported learning in higher education in general, they offer little guidance for educational decision makers on how to chose between different technology enhanced pedagogical formats. To provide a well-informed basis for such decisions, more context-specific knowledge on the effects of different formats is needed.

With this thesis, I add to the literature on technology supported learning in higher education with formal empirical evidence that investigates how the - more or less intentional - introduction of technology in teaching and learning affected students' study outcomes. Chapter 1 and 2 assess the effects of carefully planned implementations of a blended and an online learning format, respectively, while Chapter 3 examines student outcomes in the light of the shift to online teaching and learning induced by the COVID-19 pandemic. Though all chapters are related to technology supported learning, they constitute distinct contributions to different branches of the literature and can thus be read independently of one another.

The thesis' first two chapters assess the effect of the introduction of two separate pedagogical formats on the educational outcomes of first-year university students. Both interventions were meticulously planned and utilized technological tools as part of the design in the hopes of benefiting student outcomes. Chapter 3 examines whether the effect of the COVID-19 pandemic on student outcomes differed among different types of students. Though the pandemic led to an external shock in teaching practices towards implementing online elements, I cannot disentangle the effect of online instruction from other COVID-19 related effects. Consequently, Chapter 3 assesses the effect of the pandemic as a whole, which, as argued by Bacher-Hicks and Goodman (2021), it is possible to plausibly do. Therefore, the chapter involves an implicit notion of technologically supported learning by considering the joint COVID-19 effect comprised of the shift to online instruction, as well as other pandemic-related factors such as financial and socio-emotional stress, which might have affected student outcomes.

All three of the thesis' chapters report the findings of quantitative data analyses of the outcomes of students at the Copenhagen Business School (CBS). CBS is one of the largest institutions of higher education in Denmark and offers a range of study programs related to economics, social sciences, and business studies. Compared to students at the other Danish universities, the share of CBS students with a high high school GPA has for several years exhibited an upwards trend, while the share of students with a low high school GPAs has been consistently low (Danmarks Evalueringsinstitut, 2015). The empirical analyses in all three chapters are based on CBS's own administrative data. Chapter 3 additionally combines this information with Danish register data on students' socioeconomic background.

The research presented in the thesis was conducted as part of the Research in Blended Learning (RiBL) project. The project was funded by the CANDYS Foundation and implemented as a 6-year cross-disciplinary and cross-institutional strategic initiative at CBS. Before the onset of the

RiBL project, the teaching and learning practices at CBS were for the most part characterized by traditional face-to-face formats. One of the principal goals of the project was to facilitate an increased use of blended learning, which was motivated by the underlying intention of improving students' educational outcomes. As such, the present thesis contributes to the RiBL project's overall aim of enabling teaching and learning practices to support "*[...]students and graduates in developing the skills, knowledge and dispositions to function productively in the workplace both within the conventional economy and the emergent digital platform economy*" ("RiBL Goals – RiBL," 2022).

Outline of the Chapters

Chapter 1 is co-authored with Julie Buhl-Wiggers, Lisbeth la Cour, and Annemette Kjærgaard and examines the effect of flipped classroom on student outcomes in higher education. The use of this pedagogical format has been increasing in recent years and often involves using technology to free up time for active learning during face-to-face instruction by moving content that has traditionally been delivered in-class out of the classroom. In spite of the rise in the popularity of the format, formal quantitative evidence of its effects on student outcomes is limited. In particular, the strand of the flipped classroom literature concerned with heterogeneous effects has dedicated very little attention towards heterogeneity according to factors that vary at the classroom level. With the study presented in Chapter 1, we add to the literature on flipped classroom by exploring how two classroom-level variables, teachers and peer ability composition, affects the potential of the pedagogical format to benefit student outcomes.

We empirically explore the question of heterogeneity by utilizing two iterations of a randomized field experiment involving 933 students and 11 teachers. More specifically, we assess the effect of introducing flipped classroom in the tutorial classes of an introductory macroeconomics course for freshmen students at the biggest study program at CBS. We find a positive but insignificant average effect of flipped classroom on both the students' pass rate and on their final exam grades. Similarly, we find no indications of a differential effect of flipped classroom depending on variations in the means of the ability levels of a students' peers. However, we observe several instances of notable shifts in the ranking of the participating teachers' effectiveness when comparing traditional and flipped classroom conditions. This suggests that the best teacher in a traditional teaching environment is not necessarily the best teacher in a flipped classroom environment.

While Chapter 1 investigates the scope of a blended technology supported learning format, Chapter 2 considers the effect of a (mostly) online pedagogical initiative. To be more precise, I in Chapter 2 together with Julie Buhl-Wiggers investigate the use of online remediation to increase the math skills of underprepared freshmen students. Math skills have both been found to be key for the study of economics and to be the most important determinant of success in introductory microeconomics (Allgood et al., 2015, Ballard and Johnson, 2004, Schuhmann et al., 2005). However, unfortunately many freshmen students in higher education do not master the math skills expected at the university-level and so might struggle academically when they encounter math-based courses (Bettinger and Long, 2009; Büchele, 2020). Therefore, many institutions of higher education have sought to help underprepared students improve their math skills by offering remedial math courses. Despite the prevalence of remediation, empirical evidence of its effectiveness is still scarce.

In the study presented in Chapter 2, we therefore set out to empirically investigate how offering an online remedial math course to freshmen students at CBS affected their performance in a mandatory microeconomics course that is heavily reliant on math-based problem solving. The remedial offer consisted of a face-to-face workshop and a self-paced online module comprised of tutorial videos and accompanying exercises with automated feedback. We assess the effect of the course by means of a fuzzy regression discontinuity design. In particular, we invited the freshmen students at CBS in 2021 to complete a math assessment specifically tailored to economics students and then enrolled them in the online module of the remedial math course if they performed below a certain threshold. 58% of the students completed the assessment and after applying the threshold, we assigned 806 students to "treatment" in the form of enrollment in the online module. Because participation was fully optional we have a *fuzzy* regression discontinuity design, in which compliance among the students assigned to treatment is key. Due to the partially online format, the remedial course offered the students a lot of flexibility in terms of how and how much they wanted to engage with the course content. However, since participation in the math course was fully voluntary this flexibility for the same reasons placed a great deal of responsibility on students to take control of their own learning. Our analyses suggests that students might not have been ready to take on this responsibility, as activity data shows that only a minority of the students complied with assignment to treatment with the online module. We find no statistically significant effect of the online module on neither grades nor pass rates in microeconomics, which is arguably related the low degree of student participation in the course. This indicates a need for incorporating student preferences in the design of online remediation.

In Chapter 3, the notion of technology supported learning enters only implicitly, as I consider how the COVID-19 pandemic, and consequently also the associated change to online teaching and learning, affected the outcomes of students at CBS. Because I cannot disentangle the effect of the change to online instruction from other concurrent pandemic related events, I only attempt to assess the "joint effect" of COVID-19 on student outcomes. More specifically, I investigate the differential effect of the pandemic between students with different socioeconomic backgrounds.

My analyses in Chapter 3 are motivated by previous studies on the effect of COVID-19, which have indicated that the effects of the pandemic on student outcomes were unequally distributed between students with different socioeconomic backgrounds (Agostinelli et al., 2022; Hansen et al., 2021). This finding have been argued to relate to differences in how the pandemic affected the economic and health outcomes across socioeconomic groups, in which case social welfare policies and the specific educational context might play an important role for moderating the effect of COVID-19 on educational outcomes (Aucejo et al., 2020). With the study in Chapter 3, I contribute to the literature on the effect of COVID-19 on the outcomes of students in higher education, by investigating how the pandemic affected students with different socioeconomic backgrounds in a generous welfare state. The empirical analyses are based on a Difference-in-Differences inspired estimation strategy and a rich dataset combining a number of CBS' administrative data sources with national-level register data. The analyses provide some suggestive evidence indicating that, even within this context of a generous welfare state, students with low-income parents and students who themselves had a low income, were comparatively worse off during the pandemic. I discuss potential mechanisms underlying these effects but are given the data currently available unfortunately not able to empirically assess the validity of these hypothesized channels. Therefore, I suggest a more formal exploration of the potential mechanisms as an important topic for future research in the hopes that it might help inform educational policy makers on how to counteract (unequal) effects of future events that are suspected to cause similar disruptions to the educational system.

All three chapters of this thesis offer insights on the scope for introducing technology enhanced learning in higher education and at the same time point towards important subjects for future research to explore. Of these, I want to highlight two important topics for which I expect that further exploration could offer valuable contributions to the literature. The first is related to student engagement and the second to students' attitudes towards technology enhanced learning. Both of these have been found to be crucial for student outcomes in the context of online instruction (Farrell and Brunton, 2020; Muilenburg and Berge, 2005). Therefore, a closer investigation of their relation to the specific formats considered in the present thesis might provide important knowledge on how to best implement these so as to benefit student outcomes in the future.

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Chapters

Chapter 1

Julie Buhl-Wiggers, Lisbeth la Cour, Mette Suder Franck and Annemette Kjærgaard

Investigating Effects of Teachers and Peers in Flipped Classroom

An RCT study of Classroom-level Heterogeneity

CHAPTER 1

Investigating Effects of Teachers and Peers in Flipped Classroom: An RCT study of Classroom-level Heterogeneity

Abstract

Increased use of flipped classroom in higher education calls for more rigorous research into effects on student learning. In this study, we utilize two iterations of a randomized field experiment to asses the effects of a flipped classroom on student outcomes. In particular, we complement recent literature by analyzing a large-scale flipped classroom intervention and investigating heterogeneous treatment effects across two classroom-level factors: teachers and peer ability composition. The empirical setting is an undergraduate macroeconomics course with 933 students and 11 teachers. Our findings show a positive yet insignificant average effect of the flipped classroom on both pass rate and final exam grades. Similarly, we find no differential effect for students exposed to different means of their peers' ability levels. However, we do find substantial shifts in the ranking of the participating teachers' effectiveness when comparing traditional and flipped classroom conditions, which suggests that the best teacher in a traditional teaching environment is not necessarily the best teacher in a flipped classroom environment.

1.1 Introduction

The knowledge about teaching and learning in a flipped classroom setting has grown substantially during the last decades, as educational scholars have mirrored the rising interest in the pedagogical format displayed by teachers with an interest in supporting active learning (H.-M. Lai et al., 2021; Sun et al., 2018). Arguably, a significant reason for the increased interest in flipped classroom is the recent technological developments that has enabled a shift of content traditionally delivered in-class to an online out-of-class setting (H.-M. Lai et al., 2021). This has freed up in-class time for more student-centered activities (Bergmann and Sams, 2012; McLaughlin et al., 2014; O'Flaherty and Phillips, 2015), such as increased student interaction with peers and teachers (van Alten et al., 2019).

The increased popularity of flipped classroom has been reflected in the number of empirical studies aimed at assessing the potential of the format to benefit student outcomes, such as test scores and exam grades. In general, this body of literature is somewhat inconclusive with respect to the question of the effectiveness of flipped classroom on student outcomes compared to traditional teaching formats (see for example Chen Hsieh et al., 2017; Love et al., 2014; Nielsen et al., 2018; Mingorance Estrada et al., 2019). One potential explanation of the ambiguous impression made by these studies is the presence of underlying heterogeneity. A number of studies on flipped classroom considers this possibility by assessing whether the effect of the format varies according to student-level characteristics (Nouri, 2016; Ryan and Reid, 2016; Ficano, 2019), while only limited attention has been dedicated towards investigating heterogeneity caused by classroom-level variables.

The change in in-class activities within flipped classroom suggests that classroom-level factors, such as teachers and peers, may have a different influence on student performance compared to traditional classrooms (Kim et al., 2014; Brewer and Movahedazarhouligh, 2018). Indeed, research has indicated that successful teaching using this pedagogical format involves substantially different skill sets than those demanded in traditional classrooms, while scholars have emphasized that the student-centered learning requires teachers to reconsider their role and rethink their way of teaching when they engage in a flipped classroom (see for example Akçayır and Akçayır, 2018; C.-L. Lai and Hwang, 2016; Sun et al., 2018). Since flipped classroom frees up in-class time for peer interaction and collaborative learning, peers also play an important role in this pedagogical format (van Alten et al., 2019). Therefore, in addition to teachers' mastering of a flipped classroom approach, student engagement and peer interaction have also been highlighted as important prerequisites for successful implementation of a flipped classroom. For example because a flipped classroom typically makes use of in-class time for student collaboration and group work, which suggests that the role of peers is likely to be more important in flipped than in traditional classrooms.

Despite acknowledgement of the changing roles of teachers and peers in a flipped classroom setting, to our knowledge, the influence of these factors has not been the main focus of any quantitative investigations. This indicates a notable gap in the knowledge of classroom-level heterogeneity in flipped classroom and that is what we address in the present paper, by explicitly exploring how the effect of the format varies across teachers and peer characteristics. Our efforts are guided by the following research questions:

1. Does the effect of a flipped classroom on student performance vary between teachers?

2. Does composition of peer ability affect how a flipped classroom influences student performance?

To answer these questions, we study the effect of a flipped classroom intervention that was designed as a randomized business school. The intervention was first implemented in 2018 and then repeated for the new student cohort in the following year. Since only two teachers taught the course in both years, we pool together the two iterations of the randomized control trial to increase the number teachers and students considered. This leaves us with an analytical data set of 11 teachers and 933 students.

Our findings show notable variability in the success of the flipped classroom, in terms of increasing student performance, across teachers. In particular, we observe several cases of relative rank reversals among teachers between traditional and flipped classrooms. This provides some quantitative empirical evidence corroborating the notion from previous qualitative research (e.g. Akçayır and Akçayır, 2018) stating that the teacher skill set aiding student performance in a flipped classroom is distinct from that conducive for student outcomes in traditional classrooms. Regarding the question of peer composition, our analyses show no substantial role of peer effects on student performance in neither the traditional nor the flipped classroom.

With this paper, we contribute to the literature concerned with the potential of a flipped classroom to increase student outcomes in general and when considering classroom-level heterogeneity more specifically. In this way, we add to the knowledge of flipped classroom by exploring if the presence of heterogeneous effects can help shed some light on the ambiguous findings on the effectiveness of flipped classroom presented in previous studies.

We begin by outlining related literature on flipped classroom and teacher and peer effects in Section 1.2, before describing the study design, data, and empirical strategy in Section 1.3, 1.4 and 1.5, respectively. In Section 1.6 we first present our estimates of the average treatment effect and then turn our attention towards answering our two research questions by investigating heterogeneity in the effect of the flipped classroom intervention across teachers and peer ability composition. Next, we discuss our findings in Section 1.7, before finally Section 1.8 summarizes and concludes the study with suggestions for further research.

1.2 Literature Review

Studies concerned with assessing the potential of the flipped classroom to increase student outcomes in higher education have generally reported mixed results. In a recent meta-analysis, Strelan et al. (2020) find an average effect size for student performance of 0.48 SD for higher education. However, the effect varies significantly with discipline - for example, Lo and Hew (2019) found positive effects in a meta-analysis of engineering education, while no significant effect was found in a systematic review of medical education (Chen Hsieh et al., 2017). Similarly, the strand of research sharing the same focus on the field of economics as the present study, reports marked differences in their estimates of the average effect of the flipped classroom. While findings by Calimeris and Sauer (2015) show that flipped classroom increases students' average performance on the final exam by 0.64 standard deviations, other studies find no statistically significant effect on the final exam (Setren et al., 2019; Wozny et al., 2018).

To explore these differences, research has focused on students' experiences with and preferences for flipped classroom compared to traditional teaching formats. Some studies suggest that students are differentially predisposed to be somewhat suited to a flipped teaching environment (McNally et al., 2017, p. 283). The question of the students' attitudes towards the format is, however, less clear. Though some studies find that they generally tend to have a preference for flipped classroom (Bachnak and Maldonado, 2014; Bates and Galloway, 2012; Clark et al., 2014; Tague and Baker, 2014) others report student resistance against the pedagogical format (Amresh et al., 2013; Hagen and Fratta, 2014; Gannod et al., 2008). Moreover, while some research suggest that low-achieving students find flipped classroom more difficult and demanding (Enfield, 2013) it has in other cases been found to be particularly beneficial for lower-achieving students (Nouri, 2016; Ryan and Reid, 2016). Others again find that the flipped classroom format benefits higher achieving students, as in a study of an undergraduate microeconomics course where flipped classroom was found to support students with stronger math skills and non-minority students while the opposite was true for minority students and students with lower math skills (Ficano, 2019).

In short, the findings from these studies, although promising, provide no clear evidence on the effects of flipped classroom in higher education. In addition, while the existing literature suggests some explanations for differences in the effect of flipped classroom, the scope for investigating such heterogeneity has predominantly been limited to the characteristics of students. Therefore, we next present studies arguing why teachers and peers may be a source of heterogeneity in the effect of flipped classroom on student performance.

1.2.1 Teacher Effects in Flipped Classroom

At the general level, the teacher is widely acknowledged among educational economists as being central for students' academic success (for example noted by Hanushek and Rivkin, 2006). Studies assessing the effect of observable teacher characteristics, such as education and certification, on student achievement at lower levels of education report mixed results (Carrell and West, 2010). However, several studies computing a measure of teacher value-added that captures total teacher effects, i.e. both observed and unobserved factors, find that teacher quality has notable effects on students' test scores (Kane and Staiger, 2008; Rockoff, 2004; Rivkin et al., 2005). For postsecondary education Carrell and West (2010) find statistically and economically meaningful differences in achievements of students taught by different professors in both contemporaneous introductory courses and in subsequent courses building on top of these.

While teachers are frequently mentioned as being important in discussions on flipped classroom more broadly, they are rarely the primary focus of papers. One example of a study where teachers do appear as part of the paper's explicit objective of identifying factors conducive for successful implementation of flipped classroom, is in the qualitative study by Kim et al. (2014). The study combines a range of empirical data such as student surveys, interviews, and instructor reflections to outline what aspects of flipped classroom are especially beneficial for teaching and learning. Based on their analyses, the authors formulate a number of design principles including a strong emphasis on the teacher's role as a facilitator to ensure student engagement. The importance of "Teacher Presence" is evident in students' wish for well-structured and clearly defined guidance not only for the concrete assignments at hand, but also for supporting student interactions and facilitating collaborative learning (Kim et al., 2014). This study's explicit focus on teachers is, however, a rarity in the flipped classroom literature, where the subject of teachers' effects on student performance has not yet been the sole focus of any quantitative studies.

More often when the teacher's role is addressed in the flipped classroom literature it is for example in relation to increased workload due to changing the format of courses (Karabulut-Ilgu et al., 2018). Other papers' notion of teachers within flipped classroom is more closely related to their pedagogical impact. For example in arguing that the role of the teacher in flipped classroom is distinct from traditional classrooms (see for example Akçayır and Akçayır, 2018; DeLozier and Rhodes, 2017), that specific teaching beliefs are a prerequisite for successful flipped teaching

(Hwang et al., 2015) or that there is an increased need for teachers to provide individualized student instruction and scaffolding during in-class activities (Ghadiri, 2014). Similarly, some authors note that the shift towards more student-centered learning in the flipped classroom changes the role of teachers towards facilitation of learning rather than transmission of knowledge and moves part of the responsibility for learning from teachers to students (Zou et al., 2020). This suggests that teachers' implementation of the format is pivotal for its ability to benefit student learning outcomes (DeLozier and Rhodes, 2017). Nevertheless, in spite of such seeming consensus acknowledging teachers' importance in the flipped classroom, the literature is remarkably void of quantitative empirical studies on teacher heterogeneity.

1.2.2 Peer Effects in Flipped Classroom

The effect of peers on individual academic outcomes has over the years received extensive attention within the educational economics literature (Sacerdote, 2011). Here there is wide agreement that peers matter for individual outcomes and while several mechanisms have been suggested to underlie this relationship, most of them focus on how the ability level of peers affects a student's own outcomes. For example, a spillover effect might arise if average peer ability has a direct effect on individual outcomes and students learn directly from higher ability peers or if having higher ability peers motivates a student to work harder (Sacerdote, 2011).

Both the size and even the sign of peer effects vary quite substantially within different levels of education, with findings from studies focusing on university-level education generally suggesting no or only modest effects (see Paloyo, 2020 for a recent review of empirical findings). Feld and Zölitz (2017) argue that a possible explanation for this might be that too little attention has been dedicated towards understanding the different mechanisms underlying peer effects. They present some suggestive evidence that the main channel for their own finding of positive peer effects (for especially the low-achieving university students) is improved within-group interaction. Carrell et al. (2013) similarly highlight the importance of actual within-group peer interactions as a peer effects channel. Finally, evidence reported in Skibsted et al. (2016), who considers the same setting as the present study, suggests that though peer ability level did not have a significant effect on first-year GPA when considering the entire student population, it did have a statistically significant and positive effect on the educational performance of low-ability women. This points to the importance of considering heterogeneity in how peer ability affects educational outcomes.

In the flipped classroom literature, several studies comment on the importance of peer interactions among students in flipped classrooms. According to Brewer and Movahedazarhouligh (2018) flipped class pedagogy has enabled students "to make gains at twice the rate of their peers in non-flipped classes" (Brewer and Movahedazarhouligh, 2018, p. 412) resulting in better overall course grades and preference for team work. Other studies find peer learning in flipped classrooms to be the most effective aspect of flipped learning (Bond et al., 2020; H.-M. Lai, 2021; Strayer, 2012) in particular regarding student engagement. Focusing on the effectiveness of a group-based flipped classroom, H.-M. Lai (2021) finds a positive association between group peer interaction and students' behavioral engagement. In addition, several studies explore how flipped classroom may increase peer interaction and dialogue based on the assumption that this is beneficial for students' learning (see for example Chen Hsieh et al., 2017; Zarrinabadi and Ebrahimi, 2019) with Karabulut-Ilgu et al. (2018) concluding that students enjoy working with their peers in flipped classrooms. Also peer accountability has been highlighted as an important factor for motivating students to come prepared for class (Sherrow et al., 2016).

As for the literature on teacher's importance in flipped classrooms, these studies all point to working with peers as beneficial for student learning. Yet there are, to the best of our knowledge, no studies that have directly assessed the relationship between peer interaction and student performance in a flipped classroom within a formal empirical setup. In the following sections, we examine the roles of peers and teachers in flipped classroom by means of our experimental setup. We begin by outlining the details of our setting and RCT.

1.3 Setting and Experimental Design

The flipped classroom investigated in the present study, was first implemented in a second semester introductory macroeconomics course at the largest study program[1] at Copenhagen Business School (CBS) in 2018 and then again in 2019. The macroeconomics course consisted of two separate formats: Large-class lectures[2] and tutorials in 14 (2018) or 15 (2019) smaller classes of approximately 45 students in each. Both lectures and tutorials were scheduled to 90 minutes a week and participation was voluntary as is national standard regulation for university education. In the traditional framework, students were expected to work with assigned exercises before attending the tutorial classes and the intention was that the tutorial classes should provide space for students to ask clarifying questions. However, students often come to class un(der)prepared making the tutorials highly teacher-centered and more like "mini-lectures". It was therefore decided to make the activities of these classes more student-centered, which was implemented in the form of flipped classroom.

1.3.1 Intervention Design

Our setup was motivated by the flipped classroom idea of increasing in-class activity in the tutorial classes, while the lectures proceeded as usual. In this respect, our flipped classroom set-up deviated from a standard flipped-classroom setting where lectures are often provided online before in-class tutorials. More specifically, the overall aim of the intervention was to rely on the flipped classroom philosophy of freeing up time for more student-centered learning in the tutorials. Half of the tutorial classes were changed to a new, more active format (treatment), while the other half continued the business-as-usual style of teaching and learning (control). The intervention was introduced to students through an information e-mail and an in-class presentation in the weeks prior to the beginning of the semester. Students had the opportunity to opt out of the research by

[1] The BSc of Economics and Business Administration

[2] As the cohorts consisted of more than 700 students, they were split into two lecture classes of 300-400 students due to room capacity issues. The lectures were given by the same teacher.

withdrawing consent to the use of their data and the research project was approved by the institutional ethical review board.

The treatment group engaged in collaborative group work on a weekly assigned problem set. Instructors facilitated the group work and supported students during problem solving exercises by scaffolding. To ensure that misconceptions could be corrected, the treatment group had access to video solutions to the assigned problem set after class. In the control group, students were intended to engage with solving the problem set out-of-class, while the teacher explained the solutions in-class. These students did not have access to the video-solutions. Finally, and of particular importance for the teacher focus of this paper, the teachers were carefully prepared on the new format by a couple of workshops before the start of the semester. Members of CBS's pedagogical unit were engaged in these preparatory workshops.

1.3.2 Randomization Procedure

When students at CBS are enrolled in a specific study program, they are stratified by gender and nationality and randomly assigned to tutorial classes. One exception is that the older students are placed in the same tutorial classes[3]. In both intervention years, we made use of randomization to measure the impact of the intervention, however, the level of randomization differed between the two years. In 2018 we randomized at the student-level, thus randomly placing each individual student in either a treatment or a control group and subsequently divided the treatment and control group into 7 tutorial classes. In the 2019 iteration, we decided not to break up the pre-assigned tutorial classes and therefore randomized at the tutorial class level instead.

In both years, students in the treatment group were assigned to tutorial classes but not to specific study groups within the classroom. This meant that students themselves selected into study groups without any interference by the teacher, unless one or more students did not have any peers to collaborate with, in which case the teacher would facilitate allocation of these students to study groups. Due to changes in students' attendance, the study groups could change from week to week.

[3]This is usually the case for 2 out of the classes in a cohort.

To ensure that our results were not affected by potential differences in teachers' competences, we stratified the treatment assignment by teacher in both years, such that each teacher taught both a treatment and a control class. To address potential time-of-day effects, all classes were scheduled for the same day. Because each teacher taught two classes, not all classes could be placed at the same time slot. Therefore, we placed them back-to-back and switched the classes' time slots halfway through the course. To ensure that only students assigned to the treatment classes gained access to the classroom, a research assistant monitored access at the entrance. Likewise, through the learning management system, we limited online access to the treatment group only[4].

1.4 Data

The students' performances in the macroeconomics course is assessed only once at a final closed-book exam. Grading is based on an absolute grading system, blinded, and performed by an internal teacher, who randomly receives a subset of exams from all of the different tutorial classes. To assess the effect of the flipped classroom approach, we consider two main outcomes: 1) The grade from the final exam, which was standardized by the mean and standard deviation of the control group in each year, and 2) a binary pass/fail measure, where fail include both failing grades and students who did not show up for the exam. CBS's own administrative data provides information on the two outcomes, as well as on a number of student-level variables that are included as controls in the analyses; age, gender, enrollment year, and whether they participated in the retake exam in the fall course in microeconomics. Age is measured in years, while the three other variables are defined as dummy variables. We include information on the students' potential participation in the retake of the

[4]Despite our efforts to reduce access for the students in the control group, some of them managed to gain access to the online material. To avoid bias stemming from this contamination of our control group, we utilize the fact that we can identify the students assigned to the traditional classrooms who viewed the material exclusively designed for the flipped classroom students, and eliminate them from our sample. Appendix Table 1.6 shows tests of balances in the control variables between the students excluded due to concerns of potential spill-over effects, and indicates that this group of students did not significantly differ on these observables compared to neither the control group nor to the full estimation sample.

microeconomics exam because the timing of this exam coincided with the beginning of the macroeconomics course. Therefore, students who participated in this retake exam might have had a more challenging start to the macroeconomics course than those who did not.

The administrative data also provides us with information that allows us to control for two separate ability measures, namely high school GPA and an ECTS weighted GPA from the fall semester immediately before the intervention took place[5]. Both of these measures are included as controls because we expect them to capture distinct abilities. High school GPA reflects academic capability in a range of diverse subjects and for this reason also provides an indication of motivation and diligence. On the other hand, the GPA from the fall semester constitutes a quantitative measure of the students' performances in economics-specific courses, as well as their adaption to the teaching and exam formats at the university.

From our full sample of 1215 students and 13 teachers, we obtain our analytical sample in the following way. First, we restrict our sample to only include students who participated in at least one exam during the first semester and did not drop out during the second semester where the intervention took place. Second, we identify and exclude students in the control group who got access to the online video solutions and remove them from our sample. We do this to address potential spillover effects. Then we drop two of the teachers from our analytical sample. One teacher only taught one class and thus does not allow us to control for teacher fixed effects. The other teacher taught the two classes in 2019 that were exempted from the randomization and contained older students. Finally, we only include students in our estimation samples for whom we have information on their high school GPA. When imposing all of these restrictions our analytical sample comprise of 11 teachers and 933 students (509 in 2018 and 424 in 2019) of which 763 (415 in 2018 and 348 in 2019) participated in the final exam in Macroeconomics. Appendix Figure 1.6 summarizes the process of data cleaning.

[5]Because the intervention took place in a second semester course, the fall GPA contains all the grades the student had received at CBS before the intervention.

1.4.1 Measuring Teacher Effects

Since each teacher in our estimation sample taught at least one treatment and one control tutorial class, we can control for a teacher's average "teacher effect" by including teacher fixed effects in our regressions. This way, we reduce the risk of confusing treatment and teacher effects. In practice, we achieve this by including a dummy for all but one teacher. In our setting, a teacher dummy is always (and naturally) a variable at the classroom level.

1.4.2 Measuring Peer Effects

We define peers at the tutorial class level because this is where most of the academic and social interaction among the students typically takes place and thus the most likely level for peers to affect one another.

Estimating the effect of peers on individual outcomes is notoriously difficult (Manski, 1993). The empirical challenges can be divided into three areas (Blume et al., 2011). The first econometric issue is that of simultaneity, in the peer effects literature referred to as the reflection problem, which arises because peers in the same group affect each other's behaviors and outcomes. The second challenge is related to the potential presence of group-level unobserved characteristics and the third to worries about endogenous sorting. We follow the well-known strategy in the peer effects literature and argue that the latter issue of self-selection is not critical in our case due to the randomization into tutorial classes (either at the time of the RCT in 2018 or at the time of admission in 2019). To address the two remaining concerns we use the common method of relying on pre-determined variables and use high school GPA as our measure of peer ability. We use the leave-self-out mean rather than the total tutorial class GPA to avoid the "tautological issue of y on y bar regression" (Angrist, 2014, p. 4).

1.4.3 Balance and Descriptive Statistics

Table 1.1 presents balance on pre-treatment observable characteristics between the students in the treatment and control group for both the full analytical sample and the sample including grades on the final exam[6]. In addition to the student-level controls outlined above, the table also reports our measure of peer ability. The table shows no substantial issues of imbalance for the student-level variables. For the classroom-level variable, i.e. the leave-self-out mean of high school GPA, we do see a statistically significant difference between the treatment and control group in the sample with exam grade. However, this difference is modest in absolute terms and is moreover equivalent to that of the overall mean of high school GPA. In that case, the difference in means is insignificant because there is more variation in the raw mean than in the leave-self-out mean that effectively is a "mean of means" and therefore has a smaller standard deviation.

Table 1.1. Balance of pre-treatment covariates between treatment and control group

Sample	**Full (N=933)**			**Exam Grade (N=763)**		
	Control N=457	**Treatment N=476**	**p-value**	**Control N=367**	**Treatment N=396**	**p-value**
Female	0.337	0.319	0.543	0.357	0.311	0.154
	(0.022)	(0.021)		(0.025)	(0.023)	
Age	21.306	21.265	0.579	21.191	21.205	0.929
	(0.090)	(0.062)		(0.088)	(0.065)	
High School GPA	8.987	8.996	0.908	9.017	9.071	0.551
	(0.062)	(0.058)		(0.070)	(0.061)	
GPA Fall	5.698	5.801	0.556	6.123	6.219	0.605
	(0.126)	(0.126)		(0.131)	(0.1319	
Retake microeconomics exam	0.302	0.326	0.462	0.232	0.245	0.715
	(0.021)	(0.022)		(0.0229	(0.022)	
Mean of peer high school GPA	8.987	8.993	0.627	9.017	9.071	0.003***
	(0.009)	(0.010)		(0.014)	(0.013)	

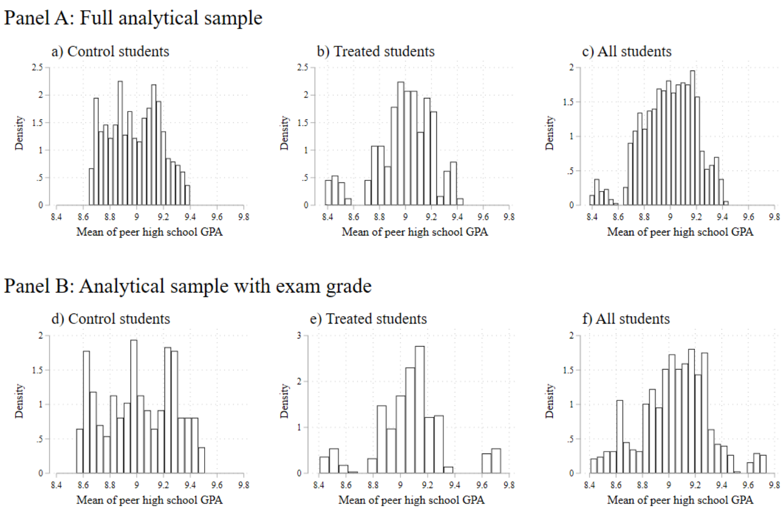
Note: Displays means with standard deviation in parentheses. P-values indicate the significance levels from a test of difference in means. *** p<0.01, ** p<0.05, * p<0.1.

Though the randomization into tutorial classes eases concerns of endogenous sorting in the estimation of peer effects, it may introduce another challenge with respect to the external validity of the results, if there is only limited naturally occurring variation within peer groups. This is because limited variation can result in support problems, which in turn means that it is difficult to generalize based on such estimation results, as this will then have to heavily rely on functional form assumptions (Booij

[6] Because the analyses with the binary pass variable as the outcome also includes students who did not show up for the exam, this analytical sample contains more students than the one with the exam grade as the outcome.

et al., 2017). We follow the approach of Skibsted et al. (2016) and display the variation in the peer measure in Figure 1.1 to argue that we do indeed have sufficient variation in peer ability composition in both of our estimation samples and across treatment status to cover a relevant range of high school peer ability compositions. Panel A of the figure shows the density of mean peer high school GPA for the students in the control and treatment group, as well as for all students in the full analytical sample in subplot a), b), and c), respectively. Analogously, Panel B shows the densities for the analytical sample with exam grade. The range of the peer measure for the treated students are in all cases wider than for the control group. In general, the mean peer high school GPA covers a fair range across samples and treatment status.

Figure 1.1. Density of mean peer high school GPA



Note: Histograms of leave-self-out high school GPA.

Table 1.2 presents descriptive statistics for both analytical samples. Overall, there are no unexpected differences in the descriptive statistics between the two samples. GPA from prior semester is lower and the share participating in the microeconomics retake exam higher for the full analytical sample. This is not surprising, as students who did not participate in the macroeconomics exam are arguably also more likely not to have participated in previous exams than the students who did. For the sample with grades on the final exam, we see that the students in this sample on average received a grade of 6.11 in macroeconomics, which is very close to the sample average of the weighted prior semester GPA of 6.17. Table 1.2 further shows that the mean age is 21.2 years and that the study program has a majority of male students.

Table 1.2. Descriptive statistics

Sample	**Full**		**Exam Grade**	
	N=933		**N=763**	
	Mean	**SD**	**Mean**	**SD**
Outcome				
Macroeconomics grade	.	.	6.11	3.85
Pass rate	0.72	0.45	0.89	0.32
Controls				
Age	21.29	1.65	21.20	1.49
Female	0.33	0.47	0.33	0.47
High School GPA	8.99	1.29	9.05	1.27
GPA Fall	5.75	2.72	6.17	2.56
Retake microeconomics exam	0.31	0.46	0.24	0.43
Mean of peer high school GPA	8.99	0.21	9.05	0.26

Note: Controls for age, gender, high school and pre-pandemic university GPA. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

1.5 Empirical Strategy

To assess the overall effect of the flipped classroom intervention on student outcomes, we begin our analysis by looking at the average treatment effect, which we estimate by a pooled OLS regression:

$$Y_{icky} = \beta_0 + \beta_1 T_i + \epsilon_{icky} \tag{1.1}$$

Where i denotes the individual student, c her classroom, k her teacher, and y the year of her participation in the macroeconomics course. Y_{icky} is either the pass rate, in which case Equation (1.1) is estimated as a linear probability model, or the standardized grade from the final exam. T_i is a dummy variable taking the value one if the student was treated (enrolled in a flipped classroom tutorial) and zero otherwise. Because we have a randomized control trial with balance across treatment and control group for all measures but the mean peer high school GPA, in which case the difference is modest, the need to add additional regressors in order to obtain an unbiased estimate of the average treatment effect might not be dire. However, doing so will tend to increase the efficiency of the estimate. Therefore, we estimate an augmented version of Equation (1.1) that includes a number of covariates. In addition to the peer measure, we in this model include a year dummy, $D19$, which takes the value of one if the student was enrolled in the course in 2019 and zero if she was enrolled in 2018. This allows for differences in the effect of the intervention across the two iterations of the RCT. We also add the vector of student-level controls outlined in Section 1.4, where both of the two GPA variables, fall GPA and high school GPA, are demeaned[7]. Lastly, we include teacher fixed effects. As mentioned earlier, the addition of teacher fixed effects effectively means that we take out the average effect of each teacher and thus avoid bias stemming from differences in individual teacher quality across treatment and control group.

In all of our analyses, we consider it likely that there might be intra-class correlation of the outcomes within tutorial groups or of students taught by the same teacher, as they are exposed to the same learning environment. The presence of such correlation means that we ought not rely on the default methods for computing the standard errors of the estimates. Consequently, the regression tables in this paper report p-values based on a wild cluster bootstrap procedure for inference, which is a common approach to addressing intra-class correlation in empirical settings with few clusters (**colin_cameron_practitioners_2015**). In our case, the choice of clustering level is not clear-cut, as we might observe clustering at both the classroom and at the teacher level. Therefore, we follow the suggestion by MacKinnon et al. (2022) and cluster at the level of randomization in the second iteration of the intervention, i.e. at the tutorial class level[8].

[7]Both in this and all other regression models presented in this paper.

[8]For a more elaborate discussion of the choice of standard errors see Technical Appendix I.1

1.5.1 Estimating Teacher Heterogeneity

After the analysis of the average treatment effect, we turn towards answering our first research question concerned with classroom-level treatment heterogeneity and examine if and how the effect of the flipped classroom varies among the 11 teachers. To do so, we estimate a model including all covariates from the full estimation model of the average treatment effect and additionally include treatment – teacher interaction terms:

$$\begin{aligned} Y_{icky} = \beta_0 + \beta_1 T_i + \beta_2 D19_{iy} + \mathbf{X_i'}\rho + \delta_1 Teacher_1 + ...+ \\ +\delta_{10} Teacher_{10} + \gamma_1 T_i \times Teacher_1 + ... + \gamma_{10} T_i \times Teacher_{10} + \epsilon_{icky} \end{aligned} \tag{1.2}$$

Where $\mathbf{X_i'}$ is the vector of student-level controls outlined previously and where our main interest lies in assessing the coefficients, γ_k, on the interactions between treatment status and each teacher. The coefficient estimates of these interactions inform us about whether the average outcome of students in the teacher's treatment class(es) is different from that of the teacher's control class(es). In this case, the estimate of the coefficient on the treatment variable will indicate the treatment effect for students in the base teacher's (Teacher 11) classes, while the estimates on the interaction terms identify the effect of a given teacher compared to the base teacher. To obtain direct estimates for each of the teacher-specific treatment effects, we estimate the model for teacher treatment heterogeneity 11 times – once with each teacher as the base teacher.

Because estimation of Equation (1.2) provides us with estimates of the difference in outcomes between a teacher's treatment and control class(es), it does not allow us to assess a teacher's effect on average student outcomes in each setting. Therefore, to gain further insights on the relationship between teachers and the effectiveness of the flipped classroom, we follow a procedure suggested by McCaffrey et al. (2012) to obtain separate mean corrected estimates of the average grades and pass rates of the students in the control and treatment classes for each of the teachers. More specifically, we calculate the teacher effects in each classroom setting as the mean outcome of a teacher's students (after correcting for the effect of other regressors) minus the overall corrected mean for all students. We then use these measures as the basis for computing the teachers' relative teacher effect ranks separately for the flipped and traditional classrooms[9].

In these two analyses of teacher heterogeneity, we cannot rely on WCB standard errors at the class level to guide inference on the teacher fixed effects and the interactions of these with treatment status. This is because we for each teacher at most observe four classes, but for 9 out 11 only two classes, which provides us with an insufficient number of clusters to compute cluster-robust WCB for the teacher fixed effects. Consequently, we instead rely on heteroscedasticity robust standard errors.

1.5.2 Estimating Peer Effect Heterogeneity

Our analysis of the flipped classroom intervention continues with a focus on assessing our second research question. To explore whether peer composition matters for the effect of the flipped classroom, we interact the peer measure with treatment and once again estimate an augmented version of the model in Equation (1.1):

$$\begin{aligned} Y_{icky} = \beta_0 + \beta_1 T_i + \beta_2 D19_{iy} + \beta_3 \overline{HSGPA}_{-ic} + \mathbf{X_i'}\rho + \\ + \delta_1 Teacher_1 + ... + \delta_{10} Teacher_{10} + \beta_4 T_i \times \widetilde{HSGPA}_{-ic} + \epsilon_{icky} \end{aligned} \tag{1.3}$$

Where $\widetilde{HSGPA}_{-ic} \equiv \overline{HSGPA}_{-ic} - \overline{HSGPA}_i$. This demeaning of the peer measure in the interaction term is done to ensure comparability of the estimate of the main term, β_3, to that obtained from estimation of Equation (1.1). Our main interest when estimating this equation is the estimate of β_4. This indicates if the mean of the ability level of a student's peers had a differential effect in the flipped classroom compared to the traditional teaching format used in the control group and is thus the relevant parameter when addressing research question 2.

We do find it likely that the effects of teachers and peers on individual outcomes are related and affect one another. Still, we consider them separately in our empirical analyses of treatment heterogeneity due to worries of potential overfitting and leave the task of estimating a fuller model including both peer and teacher effects for future studies.

[9]For a more formal outline see Technical Appendix I.2.

1.6 Results

1.6.1 Average Treatment Effects

Table 1.3 presents our results from estimating Equation (1.1) for our two outcomes of interest: the pass rate and the final exam grade in the macroeconomics course. Column (1) and (4) show the raw average treatment effect for the pass rate and exam grade, respectively. Column (2) and (5) add controls for increased precision, while Column (3) and (6) additionally includes teacher fixed effects.

Although the coefficient estimates on the treatment dummy suggest a positive treatment effect, the estimated effect of the flipped classroom intervention is insignificant across all model specifications. This is largely consistent with previous studies of the average treatment effect of flipped classroom in teaching and learning within the field of economics (as e.g. reported by Setren et al., 2019 and Wozny et al., 2018).

Table 1.3. Average treatment effects

	Pass rate			**Exam Grade**		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.035	0.035	0.037	0.100	0.060	0.057
	[0.322]	[0.280]	[0.276]	[0.192]	[0.428]	[0.376]
Experiment year		0.041	0.026		-0.022	-0.173**
		[0.236]	[0.460]		[0.742]	[0.046]
Fall GPA		0.146***	0.146***		0.640***	0.646***
		[0.000]	[0.000]		[0.000]	[0.000]
High School GPA		0.001	0.002		0.096***	0.092***
		[0.996]	[0.864]		[0.002]	[0.002]
Female		0.002	0.000		-0.147**	-0.152**
		[0.998]	[0.932]		[0.014]	[0.012]
Age		-0.008	-0.009		0.012	0.015
		[0.420]	[0.306]		[0.418]	[0.300]
Retake microeconomics exam		-0.239***	-0.238***		-0.053	-0.045
		[0.000]	[0.000]		[0.458]	[0.492]
Mean of peer high school GPA		0.006	0.098		0.118	0.042
		[0.980]	[0.406]		[0.494]	[0.730]
Observations	933	933	933	763	763	763
R-squared	0.002	0.274	0.283	0.003	0.502	0.516
Teacher Fixed Effects	No	No	Yes	No	No	Yes

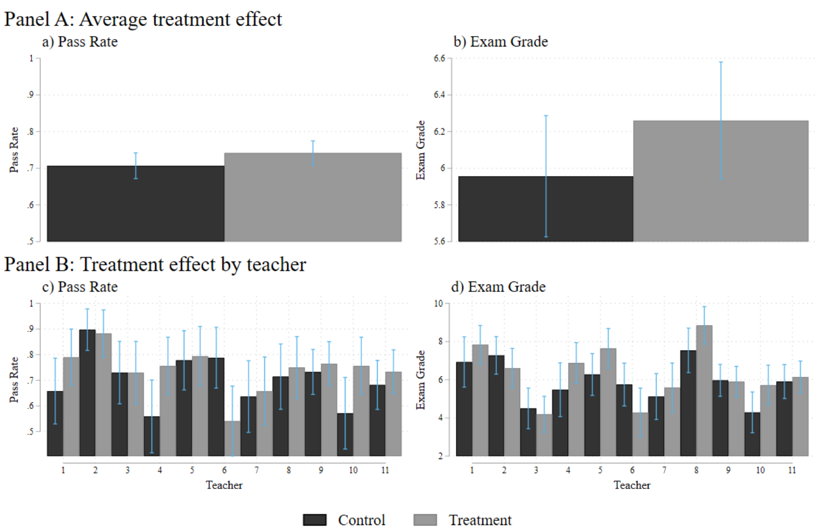
Note: Note: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses wild cluster bootstrap standard errors. Clustered at the class level with B=1000.

For experiment year, age, high school GPA, and gender we see no significance for the pass outcome. However, for the exam grade the coefficient of the experiment year is significantly negative, when we control for teacher fixed effects, as is the coefficients on gender regardless of inclusion of these fixed effects. Unsurprisingly, in all regressions the student's GPA from the fall semester is estimated to be a positive and significant predictor of performance in the macroeconomics exam. Moreover, for the pass rate, our results indicates that students who participated in the retake exam in microeconomics are significantly less likely to pass the macroeconomics exam. For the exam grade itself, we see no significance for this variable. Finally, for the exam grade, we also find significant and positive effects for high school GPA though the magnitude of this effect is notably smaller than for the fall GPA. This suggests that a student's performance in higher education economics-specific courses is a better predictor of their grade in the macroeconomics exam than the broader measure of previous academic achievements and diligence that we attempt to capture by the high school GPA.

We also considered the possibility that the effect of the intervention might have differed in the two years, e.g. due to differences in randomization procedure, and estimated Equation (1.1) separately for each year. The results are displayed in Appendix Table 1.7 and indicate no significant differences of flipped classroom across year nor compared the pooled estimations displayed in Table 1.3.

Because teachers, as mentioned in the literature review, are widely acknowledged as being central to students' educational outcomes, variation in the effect of flipped classroom across teachers might explain why we do not find a significant average treatment effect. Figure 1.2 plots the average pass rate and exam grades for students by treatment status (subplot a) and b) of Panel A) and by both treatment status and teacher (subplot c) and d) in Panel B). This figure offers some explorative insights on whether our finding of no significant effect of the flipped classroom intervention could be due to classroom-level heterogeneity according to teachers. Panel A shows the modest differences in the raw treatment effects, while Panel B indicates marked differences in students' average performances in their macroeconomics exam between students taught in traditional classrooms and flipped classroom, when making within-teacher comparisons. For the pass rate outcome displayed in subplot c), the within-teacher difference is most clearly pronounced for Teacher 1, 4, and 10, where the average pass rate of students in the control group is considerably lower than in the treatment group. However, for Teacher 6, the average pass rate of students in the control group greatly exceeds that of the students in the flipped classroom setting. Similarly, the within-teacher comparisons of the average exam grade displayed in subplot d) also suggest some cases of notable differences, namely for Teacher 4, 5, 8, and 10.

Figure 1.2. Average treatment effects and teacher heterogeneity



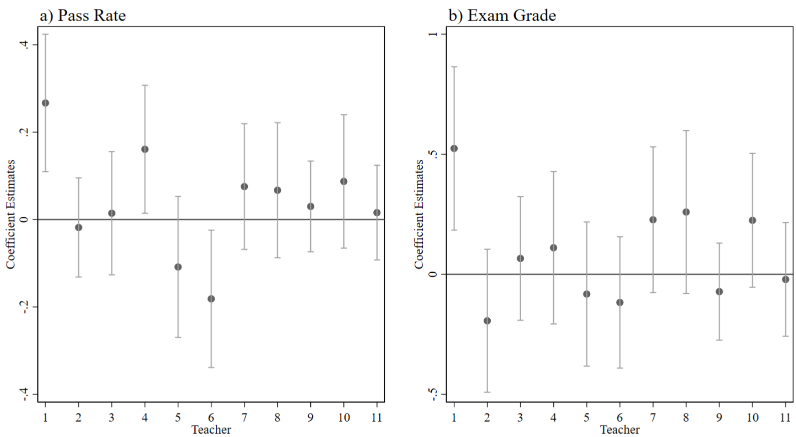
Note: Bars indicate 90% confidence intervals based on heteroscedasticity robust standard errors. Displays raw differences without inclusion of controls.

Overall, Figure 1.2 provides some informal indications that teacher heterogeneity might constitute a source of heterogeneity in the effect of our flipped classroom intervention. This motivates our formal exploration of teacher heterogeneity, which we turn to next.

1.6.2 Heterogeneity across Teachers

To investigate our first research question, we present the estimates of the interaction terms of the model in Equation (1.2) and their associated 90% heteroscedasticity robust confidence intervals visually in Figure 1.3. The figure indicates that there is substantial variation in the treatment effect between teachers with the treatment effects varying from -0.19 SDs to 0.52 SDs (exam grade) and -18.2 to 26.7 percentage points (pass). When evaluating significance at a 10 percent level, two of the eleven teachers in our sample have positive treatment effects, one have negative treatment effects and the rest have insignificant treatment effects in the regressions with the students' pass rate as the outcome. For the exam grades, only Teacher 1 had a significant and positive treatment effect, while the treatment effect for all other teachers was too imprecisely measured for it to be statistically distinguished from zero.

Figure 1.3. Estimates of teacher specific treatment effects

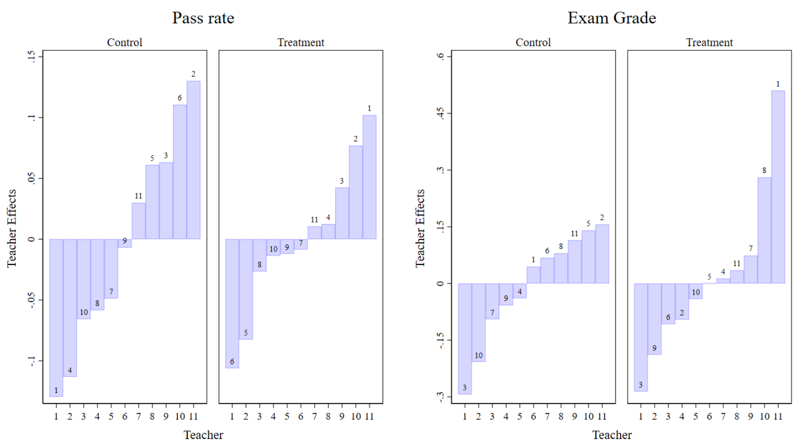


Note: Bars display 90% confidence intervals based on heteroscedasticity robust standard errors. Based on estimation of Equation 1.2.

To further explore the variations across teachers, we calculate the teacher effects separately by each treatment group based on the approach of mean correcting suggested by McCaffrey et al. (2012). This method, which is more thoroughly outlined in the Technical Appendix I.2, is intuitively appealing because it allows us obtain separate measures for the average outcomes of the students in each teacher's treatment and control classes. The method provides a prediction-corrected estimate of a teacher's effect on student outcomes by treatment status that is defined as the difference between the teacher and student residuals and calculated based on the coefficient estimates from Equation (1.2).

These mean-corrected teacher effects are displayed in Figure 1.4, where the teachers are sorted according to their relative rank by treatment status. Several interesting insights arise from this figure. Perhaps the most striking one is that we observe some notable switches across treatment status, when looking at the ranking of teachers. There are two particularly interesting examples for the pass rate. First, observe that for Teacher 1 the change is from the position of being the relatively poorest teacher in the control group to the relatively best one in the treatment group. Second, for Teacher 6 the opposite is observed, as this teacher moves from being the second best teacher in the traditional classroom to being the relatively worst in the flipped classroom.

Figure 1.4. Ranks of within-treatment teacher effects by control and treatment group



Note: Based on method described in McCaffrey et al. (2012). The approach is outlined in Technical Appendix I.2.

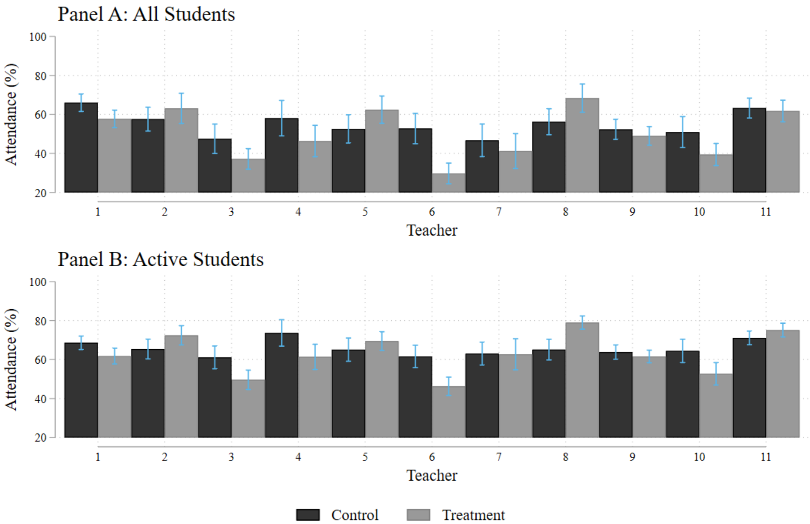
The pattern of rank reversal is only evident for some teachers, as Teacher 7, 9, and 11 are consistently at the middle of the teacher rank distribution. When we look at the graphs with exam grade as the outcome, we again observe changes in the relative teacher ranks, although none of the switches are as extreme as when we consider the pass rate outcomes. For example, Figure 1.4 shows that Teacher 2, who is ranked as the best teacher in the control setting, is part of the low- to middle-ranked teachers in the flipped classroom setting. Moreover, the plot shows that while Teacher 1 by far has the highest teacher effect in flipped classroom, he ranks in the middle of the distribution of teachers' effects on students' average exam grades in the control setting.

Given that class attendance is voluntary, one might wonder if the reason why we observe these switches in relative teacher ranks is due to selective tutorial class attendance among students: If students' attendance on average differs between flipped and traditional classrooms, this could explain the differences in teacher effects across the two formats. Recall that the intervention was designed such that the time slots of the classes were flipped halfway through the semester. Therefore, we are not too concerned that any potential patterns in selective attendance is due to teachers leveraging their experiences with teaching the first class – whether it be the traditional or flipped classroom – to deliver a higher quality of teaching in the second class.

To get some descriptive insights on attendance, Figure 1.5 shows average tutorial class attendance by teacher for all students in the full analytical sample (Panel A) and for the subset of students who participated in at least one third of all tutorial classes (Panel B). Class attendance for a given student is calculated as the share of tutorial classes in which this student showed up. We look at both of these averages, because we want to see if students who never show up drive the overall mean attendance or if it is a general pattern for all students taught by the same teacher.

Figure 1.5 indicates that, on average, there is a higher attendance among the untreated students in traditional classrooms for both student populations. This tendency is particularly pronounced for some teachers, namely Teacher 3, 6, and 10. However, whereas Teacher 6 is one of the prominent examples of rank reversals, Teacher 3 and 10 do not exhibit the same pattern. Moreover, Teacher 1, who changes rank from bottom to top between the two pedagogical formats when considering the pass

Figure 1.5. Tutorial class attendance



Note: Bars indicate 90% confidence intervals based on heteroscedasticity robust standard errors. Displays raw differences without inclusion of controls.

outcome, only has a small difference in attendance between the two different formats. When looking at the confidence bounds of these attendance averages we see that they in most cases overlap, hereby suggesting that the differences might not be significantly different. The perhaps most important takeaway from Figure 1.4 is that selective tutorial class attendance does not appear to be a main factor driving the observed teacher rank changes[10].

Overall, even though we only find few significant estimates of the interactions between teachers and treatment status, the rank analysis in this section does indicate that there still might be important teacher heterogeneity present. More specifically, the notable rank changes in Figure 1.4 suggest that there is great variability in teachers' ability to reap the

[10] It does, however, suggest that attendance might be correlated with treatment, which could affect our overall estimates of the flipped classroom in Table 1.3. To investigate this hypothesis, we estimate Equation (1.1) with attendance as outcome. The results are displayed in Appendix Table 1.8 and show no significant effect of treatment on attendance.

benefits of each of the traditional and flipped classroom format.

Though the effect of teachers is the most widely investigated classroom-level variable affecting student outcomes, the effect of peers have become another factor receiving considerable attention from educational economists. Therefore, we now turn towards investigating whether variation in peer ability composition within tutorial classes might be an important source of heterogeneity in the effect of flipped classroom.

1.6.3 Peer Treatment Effects

To guide the answer to our second research question, Table 1.4 includes the estimates from the analysis of the relationship between peer ability composition and the effectiveness of the flipped classroom. The results are based on estimation of Equation (1.3).

Table 1.4. Peer treatment effects

	Pass rate	Exam grade
	(1)	(2)
Treatment	0.036	0.058
	[0.276]	[0.390]
High School GPA	0.003	0.091***
	[0.820]	[0.002]
Mean of peer high school GPA	0.284	-0.044
	[0.248]	[0.760]
TreatmentX Mean of peer high school GPA	-0.338	0.173
	[0.348]	[0.500]
Observations	933	763
R-squared	0.284	0.516
Baseline Controls	Yes	Yes
Fixed Effects	Yes	Yes

Note: Note: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses wild cluster bootstrap standard errors. Clustered at the class level with B=1000.

The results indicate that in our flipped classroom intervention there was no significant heterogeneity in peer treatment effects. The coefficient estimates point in different directions across the two outcomes, with the estimate being negative for the pass rate outcome and positive when considering the exam grade. As we have no reason to expect the direction of any potential heterogeneity in peer effects to differ for the two outcomes, this is a little puzzling. To check if these differences might be due to differences in samples rather than in outcomes, we re-estimate the models where we use the analytical sample with exam grade for both outcomes.

The estimates are reported in Appendix Table 1.9 and though this exercise decreases the magnitude of the point estimate of the effect of the interaction term between treatment and mean of peer high school GPA on the pass rate outcome, it remains negative. However, as the estimates of this term are insignificant for both outcomes in Table 1.4 as well as in Appendix Table 1.9, we cannot reject that either effect is in fact zero.

The absence of any significant peer related heterogeneous effects is somewhat surprising, given that flipped classroom allow for more interaction among students and therefore potentially increase the possibility of learning from your peers. This expectation is in line with Feld and Zölitz (2017), who suggest peer interaction as an important mechanism for peer effects.

One potential explanation why we do not observe any heterogeneity is if the flipped classroom in fact did not involve an actual increase in peer-to-peer interaction with higher-skilled peers. This explanation mirrors the one proposed by Carrell et al.'s 2013 study of peer effects based on selective peer group formation.

Another potential explanation is related to the discussion of selective attendance in the previous section. Because not all students show up for the tutorial classes, our peer measures may be inaccurate, as students are unlikely to be affected by peers who never or rarely attend the tutorial classes. Therefore, we now turn our focus towards an alternative way of constructing the peer measures based on class attendance. We define effective peers as fellow students in the same tutorial class that showed up for at least one third of the tutorial classes and compute the leave-self-out mean based on these smaller "effective" tutorial classes. For students who attend less than one third of the tutorial classes we set the peer measure to zero based on the assumption that they do not interact with their tutorial class peers and therefore are unaffected by them.

Table 1.5 displays the results for estimations based on Equation (1.3) using the effective peer measures. It shows that using this alternative definition of peers also does not indicate the presence of any heterogeneous treatment effects according to peer composition. Because our decision to apply a threshold in our definition of effective peers as students with attendance in at least one third of the tutorial classes is admittedly an arbitrary choice, we also tested an alternative threshold of attendance in half of the tutorial classes. The estimation results based on this cutoff

Table 1.5. Effective peer treatment effects

	Pass rate		Exam Grade	
	(1)	(2)	(3)	(4)
Treatment	0.035	0.045	0.058	0.075
	[0.320]	[0.288]	[0.354]	[0.332]
High School GPA	0.002	0.002	0.094***	0.094***
	[0.898]	[0.914]	[0.002]	[0.002]
Mean of effective peer high school GPA	0.181	0.248	0.111	0.225
	[0.144]	[0.244]	[0.462]	[0.422]
Treatment×Mean effective peer high school GPA		-0.163		-0.281
		[0.668]		[0.618]
Observations	933	933	763	763
R-squared	0.285	0.286	0.516	0.516
Baseline Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Note: Note: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses wild cluster bootstrap standard errors. Clustered at the class level with B=1000. Effective peers defined as the fellow students in a tutorial class that showed up for at least 1/3 of the teaching.

are displayed in Appendix Table 1.10 and does not indicate any notable changes in heterogeneity according to the peer ability of effective peers compared to Table 1.5.

The analyses in this section suggest that despite theoretical arguments highlighting peer interaction as particularly important in flipped classroom, this did not appear to be the case in our setting, where the mean of peers' ability level did not have a significant impact on students' macroeconomics exam grade nor on their probability of passing the exam. Importantly, however, it should be noted that as effective peers is a post-treatment variable it can be subject to selection and therefore be a bad control.

1.7 Discussion

Our results point to some heterogeneities in the effectiveness of flipped classroom across teachers and that the relative ranks of teachers varies notably across the two different teaching formats. Our analyses do not suggest that peer composition significantly affects the effectiveness of flipped classroom and as such, it appears that teachers were the most important classroom-level factor in our flipped classroom intervention. We find the number of notable teacher rank changes quite striking, as the estimates are obtained from a very controlled setting where the teachers had explicit instruction on how to teach the flipped classroom condition. Moreover, all teachers are similar on basic observable characteristics; all except one are male, most have extensive experience, they are all part time teachers and are roughly around the same age. This could suggest that the observed changes in teacher ranks are more likely to stem from unobservable characteristics such as personality, teaching style, or attitudes towards new teaching formats. Our results are limited by the fact that we only have eleven teachers, which means that going one step further and correlating the teacher effects with observed characteristics or attempting to estimate teacher value-added in each format is out of the scope for this paper. Instead, we suggest this as a potential subject for future research.

The absence of any significant peer effects in our flipped classroom setting echoes the finding in a recent meta study of no or only modest peer effects in university-level education (Paloyo, 2020). Given the fact that students selected their own study group peers, it could, however, also be due to endogenous sorting into sub-tutorial class study groups based on ability level as described in Carrell et al. (2013). Such non-random study group formation would imply that our classroom-level peer measure is inaccurate, but as we unfortunately do not observed the study groups we cannot compute alternative peer measures on this level to test the hypothesis empirically.

Due to worries of overfitting at the classroom-level, we leave the task of investigating a model that allows for peers and teachers to affect one another for future research. From a theoretical point of view, such integration can be motivated by the observation that students in the same class are not just influencing one another, but also the teacher, who in turn influences the students through overall teaching style and their particular implementation of the flipped classroom (H.-M. Lai et al., 2021). Teachers might also indirectly affect individual outcomes by, at least partially, adopting teaching strategies based on student composition, and basing their teaching style and pace on the class's average ability level, or by devoting more time to more demanding peers (Duflo et al., 2011; Sacerdote, 2011). These channels may of course also be at play in a flipped classroom setting. Especially since teachers in this setting can interact more closely with the students (van Alten et al., 2019) and therefore have a better basis for assessing the ability level of the tutorial class.

The findings of this study have implications for practice. First, the increasing use of technology-supported teaching and learning formats places responsibility for managing the educational change process on teachers and institutions as mentioned by Bruggeman et al. (2021). Teachers are central to this process and as our findings show, their ability to transfer their teaching competencies between traditional classroom teaching and flipped classroom (and vice versa) varies substantially across teachers. To generate the positive effect that flipped classroom has the potential of providing to student learning (see for example Strelan et al., 2020), teachers' attributes and skills are critical and should be identified and developed. The expert interviews by Bruggeman et al. (2021) provide relevant knowledge on attributes for (mal)adaptation of blended learning more broadly and future studies should build on this to systematically investigate and test different teacher attributes to generate knowledge about faculty development activities that can facilitate the changes to flipped classroom. This, in turn, could support teachers as well as institutions in the ongoing organizational change process to implement flipped classroom in higher education.

Second, our lack of clear findings in relation to peer effects indicate that designing and organizing learning activities to make the most of enhancing peer effects is challenging, but also that it might be informed by collecting data on actual within-tutorial class interactions. In particular, it would be interesting to see how students engaged with each other within the classroom. If interaction was limited, or limited to only take place between students of similar ability levels, then that might explain why we do not observe any significant effects cf. Feld and Zölitz (2017) and Carrell et al. (2013) who both highlight the importance of actual peer group interaction.

1.8 Conclusion

This study complements recent literature on the effects of flipped classroom by investigating heterogeneous treatment effects across teachers and peer composition. Utilizing two iterations of a randomized flipped classroom intervention, we estimate the average treatment effect of flipped classroom and explore heterogeneities across peer ability and teachers. Our findings show a positive yet insignificant effect of flipped classroom on both pass rate and final exam grades. Similarly, we find no evidence indicating that a student's outcome is differentially affected by flipped classroom if they are in a tutorial class characterized by relatively high- or low-ability peers. Turning to the effect of different teachers, we see few cases of significant teacher-treatment heterogeneity. However, we find substantial shifts in the ranks of teacher effectiveness between the traditional and flipped classroom classes, suggesting that the best teacher in a traditional teaching environment is not necessarily the best teacher in a flipped classroom environment. These results show that even in a highly controlled environment, such as a field experiment, teachers play a role for the effectiveness of flipped classroom. Accordingly, more research is needed on what constitutes a good teacher in a flipped classroom environment, as this appears to differ from a traditional setting.

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I Technical Appendix

I.1 Clustered Data

In this appendix, we motivate our choice of basing inference on wild clustered bootstrap standard errors.

When working with a data set that has a group structure, econometricians will almost certainly worry about the plausibility of the independence of observations assumption. In our case, we consider cohorts of students who all selected to apply and subsequently enroll in a specific study program at a specific school. We therefore consider it likely that they share some unobservable characteristics of for instance motivation, interests, and ambition. Moreover, since we consider individual outcomes but assign treatment at a more aggregate level[11] and have a particular focus on peer and teacher effects that both varies and potentially exert an influence at the classroom-level, we do not expect the assumption of independence of observations to hold for our data.

In other words, our data is likely to face a clustering problem. By not addressing this problem, the analyses could be subject to considerable bias in the estimated standard errors, i.e. in incorrect measures of the analyses' precision, which, in turn, might lead us to draw wrong conclusions concerning the effects of our flipped classroom intervention.

To outline the clustering issue we consider an OLS model, where the outcome of interest is regressed on a number of regressors. In that case, the model can in matrix notation be expressed as:

$$\mathbf{y} = \mathbf{X}\beta + \mathbf{u} \tag{1.4}$$

Where the outcome $\mathbf{y}$ is an $n \times 1$ vector, $\mathbf{X}$ an $n \times k$ matrix of controls, and ϵ an idiosyncratic error term.

Again using matrix notation, the OLS estimate of β is then given by:

[11] In 2018 based on a stratification over age, sex, and geography and in 2019 on the tutorial class level.

$$\hat{\beta} = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}\mathbf{y} \tag{1.5}$$

Using that $\mathbf{y} = \mathbf{X}\beta + \mathbf{u}$, we can rewrite 1.5 to get an expression of the variance-covarance matrix:

$$\hat{\beta} = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}(\mathbf{X}\beta + \mathbf{u}) \Leftrightarrow \hat{\beta} - \beta = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}\mathbf{u}$$

Then an estimator of the variance matrix of β can be derived as:

$$\widehat{Var}(\hat{\beta}) = E\left[(\hat{\beta} - \beta)(\hat{\beta} - \beta)'\right] = E\left[(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{u}\mathbf{u}'\mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}\right]$$

When we condition on X, the variance is given by:

$$\widehat{Var}(\hat{\beta}) = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'E[\mathbf{u}\mathbf{u}'|\mathbf{X}]\mathbf{X}(\mathbf{X}'\mathbf{X})^{-1} \tag{1.6}$$

Which we can recognize as a sandwich formula, where $V = \mathbf{X}'E[\mathbf{u}\mathbf{u}'|\mathbf{X}]\mathbf{X}$ is the filling.

In the case of clustered data with G clusters the covariance-variance matrix of the error terms is given by the $N \times N$ block-diagonal matrix Ω:

$$\Omega = E[\mathbf{u}\mathbf{u}'|\mathbf{X}] = \begin{bmatrix} \Omega_1 & 0 & \ldots & 0 \\ 0 & \Omega_2 & \ldots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \ldots & \Omega_G \end{bmatrix}$$

Where we have assumed that we have independence across clusters and where each diagonal entry, Ω_g, expresses the variance of the g'th cluster and is defined as:

$$\Omega_g = E[\mathbf{u_g}\mathbf{u_g}'|\mathbf{x_g}] \equiv \begin{bmatrix} \sigma^2_{g11} & \sigma^2_{g12} & \cdots & \sigma^2_{g1N_g} \\ \sigma^2_{g21} & \sigma^2_{g22} & \cdots & \sigma^2_{g2N_g} \\ \vdots & \vdots & \ddots & \vdots \\ \sigma^2_{gN_g1} & \sigma^2_{gN_g2} & \cdots & \sigma^2_{gN_gN_g} \end{bmatrix} \tag{1.7}$$

We can now use this expression to find an expression of the variance-covariance matrix for $\hat{\beta}$ in the case of clustered data, by plugging Equation (1.7) into Equation (1.6):

$$\widehat{Var}(\hat{\beta}) = (\mathbf{X}'\mathbf{X})^{-1} \left(\sum_{g=1}^{G} \mathbf{x}' E[\mathbf{u_g}\mathbf{u_g}'|\mathbf{x_g}]\mathbf{x_g} \right) (\mathbf{X}'\mathbf{X})^{-1} \tag{1.8}$$

In order to estimate Equation (1.8) we typically use the sample moments and thus approximate the $N_g \times N_g$ matrix $E[\mathbf{u_g}\mathbf{u_g}'|\mathbf{x_g}]$ by the $N_g \times N_g$ matrix of residuals $\hat{u}_g \hat{u}_g'$.

In that case, we end up with the cluster-robust estimate of the variance matrix (CRVE), which as in Equation 1.6, is expressed as a sandwich formula:

$$\widehat{Var}(\hat{\beta}) = (\mathbf{X}'\mathbf{X})^{-1} \left(\sum_{g=1}^{G} \mathbf{x}' \hat{u}_g \hat{u}_g' \mathbf{x_g} \right) (\mathbf{X}'\mathbf{X})^{-1} \tag{1.9}$$

Where the filling now takes the form of $V_{clu} = \sum_{g=1}^{G} \mathbf{x_g}' \hat{u}_g \hat{u}_g' \mathbf{x_g}$.

I.1.1 Limitations of CRVE

Estimating the variance of the error terms by the residuals is likely to be a poor approximation for each individual cluster, g, as this does not involve any averaging allowing us to apply the Law of Large Numbers. However, because we average over the clusters, G, the CRVE in Equation (1.9) is asymptotically consistent for $G \to \infty$.

In finite samples with a relatively small numbers of clusters[12] there might be a considerable downward bias in the CRVE, because OLS estimates of the residuals are systematically smaller than the true value of the error terms they are meant to estimate.

One reason is that the estimator fails to take into account the fact that the error term is estimated. Even though several finite sample scale factors have been suggested in an attempt to amend this lack of degrees of freedom correction, unfortunately, neither of them are able to fully eliminate this bias. As a consequence, researchers relying on the CRVE in cases with few clusters run the risk of over-rejecting their null hypotheses (**colin_cameron_practitioners_2015**).

[12] One rule of thumb suggests that the CRVE is applicable for $G < 50$.

I.1.2 Wild Cluster Bootstrap

To avoid the issue due to few clusters, empirical researchers often base inference on p-values computed using the wild cluster bootstrap (WCB) procedure proposed by Cameron et al. (2008).

When implementing the WCB method, one first estimates the model of interest, while imposing the null hypothesis in question to obtain restricted residuals, $\tilde{u}_{ig}$, and coefficient estimates, $\tilde{\boldsymbol{\beta}}_{H_0}$. In our case, in which we wish to obtain WCB standard errors, the null hypothesis would be that of significance of our estimated coefficients, thus that $H_0 : \beta_k = 0$. In practice, this amounts to estimating the model with all regressors except the one that is equal to zero under the null and calculate $\tilde{u}_{ig} = y_i - \mathbf{x}'_{\mathbf{ig}}\tilde{\boldsymbol{\beta}}_{\mathbf{H_0}}$, based on the restricted model.

The next step is to get B resamples of step 1. The resamples $b = 1, 2, ..., B$ are obtained by using the Rademacher distribution to randomly assign all observations in each cluster with a weight, a_g, of either -1 or 1 with equal probability. Using these weights, pseudo-residuals can be computed as $u^*_{ig} = w_g \times \tilde{u}_{ig}$, which in turn can be used to define new outcome variables $y^*_{ig} = \mathbf{x}'_{\mathbf{ig}}\tilde{\boldsymbol{\beta}}_{\mathbf{H_0}} + \tilde{u}_{ig}$. Then regress the new outcome on all k regressors, i.e. without imposing the restriction, and calculate the Wald t-statistics corresponding to the null hypothesis as $w^*_b = \frac{\hat{\beta}^*_b - \hat{\beta}}{s_{\hat{\beta}^*_b}}$, where $\hat{\beta}^*_b$ is the coefficient estimate from the b^{th} resample, $s_{\hat{\beta}^*_b}$ the corresponding CRVE standard error, and $\hat{\beta}$ the coefficient estimate from estimation of the unrestricted model.

These w^*_b cannot be directly used to obtain critical values and confidence intervals **colin_cameron_practitioners_2015**. Instead, we rely on p-values for hypothesis testing. To get these p-values for a symmetric test of the null hypothesis, one has to compute the proportion of times that the absolute value of the Wald t-statistics for the original sample exceeds that from the b^{th} resample, i.e. where $|w| > |w^*_b|$.

For all of our OLS estimations, we report WCB standard errors based on $B = 1000$ replications.

I.2 Estimation of Teacher Effects

Here we provide a formal presentation of the method used to rank all 11 teachers for both traditional and flipped classrooms in Figure 3. These estimates are based on the method underlying Stata's areg procedure as outlined in McCaffrey et al., 2012 and provides a way to compute a mean corrected effect of each teacher on student pass rates and exam grades.

We obtain an expression for the effect of each teacher k=1,..., 11, by:

$$TE_k = \left(\bar{y}_k - \bar{x}'_k\hat{\beta}\right) - \left(\tilde{y}. - \tilde{x}'\hat{\beta}\right) \tag{1.10}$$

Where $\bar{y}_k$ denotes the mean outcome (i.e. exam grade or pass rate) for students taught by teacher k, $\bar{x}'_k$ is a vector containing the teacher-level mean values of the student specific controls included in our regressions and $\hat{\beta}$ is the estimated coefficients from our preferred regression model. $\tilde{y}.$ is the mean of the individual values of the outcome and $\tilde{x}'.$ is the individual mean of the student specific and peer controls. In words, our mean corrected measures of the teacher effects based on Equation (1.10) are calculated as the difference between the teacher and student level residuals. To get an impression of whether a teacher's effect on the students' performances varies according to pedagogical format, i.e. between flipped classrooms and traditional tutorial classes, we construct separate measures of the teacher effects by treatment and control group as the student level mean outcome within each format.

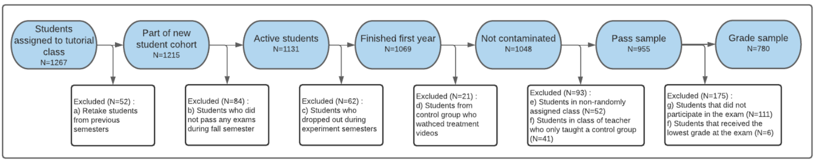
II Appendix Tables and Figures

Appendix Table 1.6. Balance of pre-treatment covariates between students excluded due to spill-over effects and control group and full sample

Sample	Full (N=933)			Full (N=933)		
	Control excl. spill-over	Spill-over		Full sample	Spill-over	
	N=457	N=19	p-value	N=933	N=19	p-value
Female	0.337	0.319	0.543	0.357	0.311	0.154
	(0.022)	(0.021)		(0.025)	(0.023)	
Age	21.306	21.265	0.579	21.191	21.205	0.929
	(0.090)	(0.062)		(0.088)	(0.065)	
High School GPA	8.987	8.996	0.908	9.017	9.071	0.551
	(0.062)	(0.058)		(0.070)	(0.061)	
GPA Fall	5.698	5.801	0.556	6.123	6.219	0.605
	(0.126)	(0.126)		(0.131)	(0.1319	
Retake microeconomics exam	0.302	0.326	0.462	0.232	0.245	0.715
	(0.021)	(0.022)		(0.0229	(0.022)	
Mean of peer high school GPA	8.987	8.993	0.627	9.017	9.071	0.003***
	(0.009)	(0.010)		(0.014)	(0.013)	

Note: Displays means with standard deviation in parenthesis. P-values indicate the significance levels from a test of difference in means. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figure 1.6. Data cleaning



Appendix Table 1.7. Average treatment effects by year

	(1)	(2)	(3)	(4)	(5)	(6)
		Pass rate			Exam Grade	
			2018			
Treatment	0.054	0.039	0.049	0.093	0.073	0.066
	[0.184]	[0.192]	[0.112]	[0.410]	[0.344]	[0.208]
Fall GPA		0.198***	0.195***		0.669***	0.674***
		[0.000]	[0.000]		[0.000]	[0.000]
High School GPA		-0.017	-0.014		0.109***	0.108***
		[0.390]	[0.462]		[0.008]	[0.010]
Female		0.022	0.020		-0.159**	-0.158**
		[0.474]	[0.476]		[0.046]	[0.048]
Age		-0.008	-0.009		0.015	0.018
		[0.472]	[0.424]		[0.458]	[0.340]
Retake microeconomics exam		-0.230***	-0.235***		-0.077	-0.065
		[0.000]	[0.000]		[0.226]	[0.272]
Mean of peer high school GPA		0.114	0.204		0.117**	0.073
		[0.584]	[0.480]		[0.048]	[0.504]
Observations	509	509	509	415	415	415
R-squared	0.003	0.349	0.355	0.002	0.548	0.558
Fixed Effects	No	No	Yes	No	No	Yes
			2019			
Treatment	0.017	0.035	0.041	0.109	0.042	0.057
	[0.732]	[0.508]	[0.614]	[0.312]	[0.730]	[0.758]
Fall GPA		0.084**	0.086**		0.610***	0.619***
		[0.018]	[0.018]		[0.000]	[0.000]
High School GPA		0.027	0.023		0.079**	0.069*
		[0.184]	[0.296]		[0.046]	[0.076]
Female		-0.022	-0.025		-0.125	-0.141
		[0.662]	[0.604]		[0.264]	[0.186]
Age		0.002	-0.003		0.013	0.013
		[0.994]	[0.840]		[0.730]	[0.722]
Retake microeconomics exam		-0.237***	-0.233***		0.011	0.011
		[0.000]	[0.000]		[0.910]	[0.908]
Mean of peer high school GPA		-0.120**	-0.207		0.154	-0.008
		[0.042]	[0.488]		[0.712]	[0.858]
Observations	424	424	424	348	348	348
R-squared	0.000	0.181	0.198	0.003	0.449	0.465
Fixed Effects	No	No	Yes	No	No	Yes

Note: Note: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses wild cluster bootstrap standard errors. Clustered at the class level with B=1000.

Appendix Table 1.8. Average treatment effects with attendance as outcome

	Attendance (%)
Treatment	-4.256
	[0.120]
High School GPA	0.628
	[0.636]
Fall GPA	7.787***
	[0.000]
Female	0.980
	[0.658]
Age	0.234
	[0.816]
Retake microeconomics exam	-12.054***
	[0.000]
Mean of peer high school GPA	7.194
	[0.406]
Observations	933
R-squared	0.288
Baseline Controls	Yes
Fixed Effects	Yes

Note: Note: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses wild cluster bootstrap standard errors. Clustered at the class level with B=1000.

Appendix Table 1.9. Peer treatment effects with same estimation sample

	Pass rate	Exam Grade
	(1)	(2)
Treatment	0.011	0.058
	[0.770]	[0.390]
High School GPA	0.026**	0.091***
	[0.030]	[0.002]
Mean of peer high school GPA	0.036	-0.044
	[0.786]	[0.760]
Treatment×Mean of peer high school GPA	-0.128	0.173
	[0.554]	[0.500]
Observations	763	763
R-squared	0.223	0.516
Baseline Controls	Yes	Yes
Fixed Effects	Yes	Yes

Note: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses wild cluster bootstrap standard errors. Clustered at the class level with B=1000. Both estimations are performed on the analytical sample with information on exam grade

Appendix Table 1.10. Effective peer treatment effects with alternative definition

	Pass rate		Exam Grade	
	(1)	(2)	(3)	(4)
Treatment	0.029	0.013	0.050	0.052
	[0.478]	[0.824]	[0.426]	[0.528]
High School GPA	0.002	0.002	0.094***	0.094***
	[0.902]	[0.874]	[0.002]	[0.002]
Mean of effective peer high school GPA	0.328**	0.253	0.276	0.283
	[0.032]	[0.172]	[0.148]	[0.304]
Treatment×Mean effective peer high school GPA		0.193		-0.018
		[0.368]		[0.986]
Observations	933	933	763	763
R-squared	0.295	0.296	0.517	0.517
Baseline Controls	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes

Note: Note: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses wild cluster bootstrap standard errors. Clustered at the class level with B=1000. Effective peers defined as the fellow students in a tutorial class that showed up for at least 1/2 of the teaching.

Chapter 2

Julie Buhl-Wiggers and Mette Suder Franck

Do the Math

Impact of an Online Remedial Math Course

CHAPTER 2

Do the Math: Impact of an Online Remedial Math Course

Abstract

Math skills are essential to the study of economics and are often found to be the most important determinant of success in introductory microeconomics (Allgood et al., 2015, Ballard and Johnson, 2004, Schuhmann et al., 2005). Unfortunately, at the time when students begin their studies in higher education many of them do not master the math skills expected at the university-level and so might struggle to achieve academic success in math-based courses (Bettinger and Long, 2009; Büchele, 2020). This implies that there is a scope for helping underprepared freshmen students improve their math skills by offering remedial math courses. Despite this potential, little is known about the effectiveness of remedial math courses. In this study, we investigate how offering an online remedial math course to freshmen students right at the beginning of their studies affected their performance in a subsequent microeconomics course. The math course consisted of one face-to-face workshop followed by a self-paced online module comprised of tutorial videos and accompanying exercises with automated feedback. To assess the effect of the course, we invited freshmen students at a large Danish business school to take the Math for Economics Skills Assessment, which 58% of the students did, and then used their performance in this assessment as the sole mechanism for enrolling them in the online module of the remedial math course. More specifically, we set a threshold for assignment to treatment and automatically enrolled all students, 806 students in total, below this threshold in the online module, which allows us to use a fuzzy regression discontinuity design to assess the effectiveness of the course. Due to the partially online format, the remedial course on the one hand offered a lot of flexibility to students in terms

of how and how much they wanted to engage with the course compared to the norm for such courses. However, for the same reasons the course also placed more responsibility on students to take control of their own learning, since participation in the math course was fully voluntary. Our data suggests that students might not have been ready to take on this responsibility, as activity data shows that only few students complied with assignment to treatment with the online module. We find no statistically significant effect of the online module on grades in microeconomics, which is arguably related the low degree of student participation in the course. This indicates a need for incorporating student preferences in the design of online remediation.

2.1 Introduction

Possessing math skills is key to the study of economics (Allgood et al., 2015; Ballard & Johnson, 2004; Schuhmann et al., 2005). However, a substantial number of students are entering tertiary education without the math skills necessary for academic success. A tendency that have been found in several countries including United States (Aud et al., 2011), Germany (Büchele, 2020), and Denmark (Hansen, 2020; Klitgaard, 2020).

A popular way for universities to address these shortcomings have been to offer remedial courses (Boatman & Long, 2018). Despite their prevalence, the effectiveness of such remedial courses, in terms of their ability to increase student outcomes, is still unclear as most empirical studies report no or only modest effects of math remediation on academic outcomes. One notable exception found statistically and economically significant effects of remediation on academic outcomes among Italian freshmen students[1] (De Paola and Scoppa, 2014). However, this particular course was associated with sizable time (160 hours in total) and monetary costs (€1000/student), which even made the authors themselves hesitant to recommend the course to educational policymakers in spite of their promising findings. The costs of offering remedial courses have been the subject of a separate debate in the remediation literature. Mainly because of the often considerable costs for universities associated with offering

[1] This course covered both math and language skills.

the courses but also because remedial courses may include both direct (e.g. tuition) and indirect (e.g. loss of lifetime earnings due to delayed graduation) costs for the students assigned to attend them[2].

In the wake of the COVID-19 pandemic, students and teachers have gained extensive experience with engaging with online courses and teaching materials, hereby providing a scope for offering remedial courses online. This could help alleviate the time constraint associated with previous remedial courses by allowing students the flexibility to choose when and to which degree to use the course. Moreover, once created the marginal cost of admitting an additional student - or even entire student cohorts - to the course, is virtually zero and therefore addresses the issue of costs faced by the institution offering the course.

In the present study, we assess whether a voluntary online remedial math course offered at a large Danish business school helped increase students' math skills and hereby ultimately also their performance in the exam of an introductory microeconomics course. We look at the outcome in the microeconomics exam because this course is mandatory for all students and, more importantly, also heavily reliant on mathematical problem-solving. Therefore, it serves as a measure of the students' ability to apply mathematical skills to economics-related questions, which is central for their future academic path as students at a business school.

To empirically investigate the effectiveness of the online remedial math course, we use a fuzzy Regression Discontinuity Design (RDD). The application of this methodology is possible because student performance in an initial math assessment was used as the single mechanism for enrolling them in the remedial math course. More specifically, we assigned all students with an assessment score below a cutoff to "treatment" with the online course. Because compliance with assignment to treatment, i.e. engagement with the online module, was fully optional we rely on a *fuzzy* RDD.

In most model specifications, we find a negative, yet statistically insignificant, effect on performance in the microeconomics exam when comparing outcomes for students just below and above the cutoff for enrollment in the course. A result that we suspect is strongly related to partial compliance among the students assigned to treatment. When estimating

[2]See Jimenez et al. (2016) for estimates of these costs in an American context.

the effect of treatment for the compliers by OLS we instead find a positive and in some cases also significant correlation between course participation and the students' performances in the microeconomics exam, though we do not make any claims of causality for these results due to potential self-selection into treatment. Our findings suggest that a relevant question for future research is how to increase student compliance in the context of online remediation. Moreover, since compliance appears to be particularly low for academically weak students there is an important scope for engaging student participation in remediation for this particular group of students.

The rest of the paper first provides an overview of existing studies of remediation in Section 2.2 and then a description of the setting and data in Section 2.3. We proceed by outlining our identification strategy in Section 2.4 and presenting the results in Section 2.5 and Section 2.6. Lastly, we discuss our findings in Section 2.7, before we summarize the paper's main findings and contributions in Section 2.8 .

2.2 Literature review

The use of remediation have often been considered controversial in the United States (Bahr, 2008), while the attitude in Europe has generally been more positive (Büchele, 2020). One reason why remedial courses might be a source of controversy is that their effect on student outcomes is ambiguous. On the one hand, the central idea behind offering remedial courses is that it helps students attain skills that will benefit their educational path in higher education, e.g. by increasing their academic knowledge and confidence (Duchini, 2017). On the other hand, opponents have noted that remedial courses might inadvertently end up having the exact opposite effect because they often do not count toward the final degree (gives no credits) and thus pose a large additional burden on students who are already struggling academically (Duchini, 2017). Other critics object to the use of remediation because they fear that admitting ill-prepared students to higher education will decrease the overall quality of academics (Büchele, 2020). However, the perhaps most common critique of remediation is that being assigned to a remedial course can negatively affect a student's perception of their own skills and hereby discourage their pursuit of a higher education (Boatman and Long, 2018), just as the potential of social stigmatization have been hypothesized to exert a similar effect (Büchele, 2020).

Given the a priori ambiguity of remedial courses and the often non-negligible cost of offering them, there has been an increasing number of studies trying to assess their effects on student outcomes in higher education (Boatman and Long, 2018). Such empirical investigations of the effectiveness of remediation is complicated by the fact that participation in remediation is usually non-random. Consequently, researchers have had to be creative in order to estimate the treatment effect of remedial courses. In most studies, researchers have aimed at estimating the local average treatment by exploiting discontinues in the mechanisms for assignment to remediation by means of either a fuzzy (Martorell and McFarlin, 2011; Scott-Clayton and Rodriguez, 2015; De Paola and Scoppa, 2014) or sharp RDD (Duchini, 2017), but the selection problem have also been addressed with an instrumental variable approach (Bettinger and Long, 2009). Within the latter setup, a study found that students who completed remediation courses were more likely to complete their education programs, though the size of this effect was modest (Bettinger and Long, 2009).

Most RDD based studies investigating the effects of remediation in American institutions of higher education generally find little evidence of it having having a meaningful effect on student outcomes. Despite considering both short (education) and long term (labor market) outcomes, as well as potential subgroup heterogeneities, Martorell and McFarlin (2011), at best, find only quantitatively small and statistically insignificant effects of remediation, while some of their analyses suggest that it might even be counterproductive for student outcomes. Scott-Clayton and Rodriguez (2015) find that although participation in a remedial math course did indicate a positive local treatment effect on math abilities for students near the cutoff, this did not seem to carry over into any other educational outcomes.

There are still only few studies examining remedial courses outside of the United States. One of the most notable ones is De Paola and Scoppa (2014), who estimate the local average treatment effect of a 160 hour remedial class covering both math and language skills at a public Italian university. They find a positive effect of participating in the course on the number of credits obtained after two years of study and a both statistically significant and economically meaningful reduction in the probability of dropout of 7-8 %-points, which they suggest might indicate that remedial courses are specifically helpful for low-ability students at risk of dropping out. De Paola and Scoppa hypothesize that the contrast between the modest or insignificant effects found in American settings and the more uplifting ones found in their own study might be explained by differences in the student compositions. More specifically, they note that while remediation in the studies from the United States mainly affects students with low socioeconomic backgrounds, it is in their setting oriented towards bridging the gap between different secondary educational programs (professional and more academically oriented schools). A difference, they argue, which applies to comparisons between the US and many European countries.

Despite their positive findings, the authors were hesitant to make clear policy recommendations based on their study due to considerations about cost-effectiveness: The remedial course they considered involved a substantial time investment from students of 160 hours and a very considerable monetary one for the course providers of 1000 Euros per student (De Paola & Scoppa, 2014). Duchini (2017) also recognizes the importance of considering the costs of remediation and further emphasizes the need for understanding which contexts such courses are effective in. She analyzed a much shorter remedial course of only 21 hours among Italian students, which, she argued, should be too short to effectively help students[3]. Instead, she contends that any possible effects of the course can more credibly be attributed to the fact that students failing to pass the course, which was mandatory for students scoring below the threshold, had to repeat their first year of study. Using a sharp RDD, she finds no significant effects on student outcomes and argued that this was most likely because the method only allows for assessment of a local treatment effect on students who are just below the cutoff for assignment to remediation and for whom the threat of re-enrollment in the first year is not credible, given that the probability of failing the exam of the remedial course is very low for these students. Ultimately, she suggests that as the

[3]This amount of teaching corresponds to a third of standard college-level courses in this educational context.

time and cost intensive remedial course analyzed in De Paola and Scoppa (2014) is the only one resulting in positive local treatment effects on student outcomes, making such investments might be necessary to increase educational outcomes for this group of students.

With this study, we set out to determine if offering online math remediation can provide a way to offer students an effective course that is both low-cost for educational institutions to offer and time-efficient for students to participate in. As previously mentioned, the cost-effectiveness of the online remedial math course analyzed in the present paper is due to the online format that besides a modest fixed cost of creating the course entails hardly any marginal costs. The time-efficiency aspect of the course emerges, as it is designed to allow students to tailor the course to their own individual needs. In this way, students have a great deal of discretion and flexibility in terms of deciding their own time investment, i.e. when, how, and how much to use the remedial course. Moreover, because the self-paced nature of the course allows for students to engage in more targeted learning efforts it might still have a potential for benefiting student outcomes while requiring a lower time investment to be made by students, than what has previously been argued to be necessary for achieving significant effects of remediation (e.g. by Duchini, 2017). In other words, we will expect the course to have the potential to increase students' math skills even if involving a - relative to the 160 hour course considered by De Paola and Scoppa (2014) - more modest student time expenditure, because students only has to study subjects that they find useful and not those that they are already comfortable with.

2.3 Setting and Data Description

The present study analyzes the effect of online remediation on outcomes of first-year students at a large Danish institution of higher education, Copenhagen Business School (CBS), that offers a range of business-related three-year bachelor programs. For a number of years, CBS has provided a remedial math course to incoming students in most study programs within their first few weeks of enrollment. It has traditionally been organized in a way so that students would first receive an invitation to an online math assessment and then to participate in a subsequent one-day face-to-face course. In this on-day course they could then choose to participate in a number of workshops covering different math topics, such as algebra and calculus. Even though this remediation course has always been fully voluntary, it has generally had a high uptake among students, which we interpret as a sign that math remediation does not seem to be subject to any notable social stigmatization among CBS students.

2.3.1 The Online Remedial Math Course

Despite the high participation in the previous remediation offer, many students still struggled in courses with math-based exams. Therefore, the course was redesigned in the fall of 2021 to provide the students with access to math remediation for a longer period of time, hereby creating opportunities for the students to engage with the course while encountering math-based problems in their microeconomics courses. More specifically, the existing one-day face-to-face course was supplemented with a self-paced online module consisting of videos and accompanying exercises with automated feedback. The online module was, as the face-to-face course, optional, available for eight weeks[4], and covered the topics of 1) mathematical foundations, 2) algebra, 3) graphs, and 4) calculus. It was designed as a fully self-paced course in CBS's learning management system Canvas and consisted of short videos and associated quizzes that the students could jump between based on their own individual needs. For example, if the students felt they had sufficient knowledge on mathematical foundations they did not have to engage with the videos and quizzes in this section before looking at any of the other topics or vice versa. In this way, the module allowed students great flexibility in terms of how and how much they wanted to use the course compared to CBS's previous remedial math course offer.

2.3.2 Assessment of Students' Initial Math Skills

To asses the students' initial math skills in fall 2021 we used a new and shorter multiple choice test, the intermediate Math for Economics Skills Assessment (MESA), developed by Orlov et al. (2021). The MESA consist

[4]The duration of eight weeks was chosen to avoid interfering with the students' exam period.

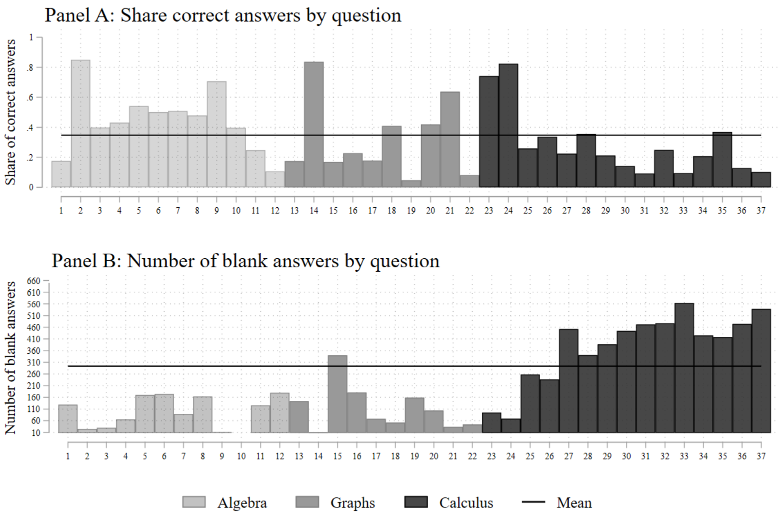
of 37 questions in total divided between three main topics relevant for the study of economics, namely algebra (12 questions), graphs (10 questions), and calculus (15 questions), which also constitutes a substantial part of the content of the online remedial module cf. above. We administered the MESA online one to two weeks before the date of the face-to-face course. The assessment was in English and so we allowed the students the aid of translation tools, but was otherwise intended as a closed-book test. Because it was administered online we have no way of validating if the students complied and refrained from using math books or CAS tools. In our communication, we made it clear that the students should view the MESA as a help to gain insights on their own math skills and not as a test that would have any direct consequences for their studies. Therefore, the incentive to cheat on the assessment was low and given the results of the assessment (see Figure 2.1), we do not believe that there is any reason to be concerned that this might be a significant source of measurement error.

Because of space limitations, the timing of invitations varied, as students were divided into six one-day face-to-face courses, which were scheduled in three consecutive weekends (on either Saturday or Sunday) at the beginning of the semester. Consequently, invitations informing students about the purpose and practicalities surrounding the assessment were sent in three separate rounds to their official CBS email. We used the same channel to provide them with overall feedback a few days before the face-to-face course that indicated whether we would advise them to participate in the remedial math course or not based on their MESA score.

On average students had 13 (or 12.81) correct answers out of the 37 total question, which reveals that the students generally struggled with the MESA test. Figure 2.1 shows the share of correct responses and blank answers by question and topic. From Panel A of Figure 2.1, it is clear that the students were most challenged by the calculus questions. Panel B of Figure 2.1 suggests that this was not because they answered the questions incorrectly, but rather that students submitted many of the answers in this subject blank. This is what we had instructed them to do in case they did not know how to solve a problem, as we wanted to minimize the risk of noise in the test scores due to students picking the correct answers in the multiple choice test by chance.

Admission to the study programs considered here only requires high

Figure 2.1. Performance by question



school math at the intermediate level that does not include topics such as integrals and composite functions. Thus, the number of questions that we would expect all students to be able to answer is 32. That being said, given that the mean number of correct answers is well below 32 and that many students probably did complete high level math and thus should be able to answer more than 32 correctly, it is striking to discover that the students answered only 34.62% of the questions correctly and that this average result is representative of the vast majority of students. The voluntary nature of the assessment and the lack of study related repercussions from under-performing might explain the poor average performance but inspection of the time spent on the assessment reveals no half-hearted tendencies among the students: On average students spent 45 of the 55 minutes available on completing the assessment.

The explanation might be related to self-selection. It could be the case that only students who felt like they could use some math remediation chose to do the assessment, whereas the ones confident in their math skills chose not to. Again the data does not support the hypothesis, as the mean high school GPA among students participating and not participating in the assessment are virtually similar, if anything it is slightly higher among those who did the MESA[5]. Lastly, the assessment was in English and though Danish students generally a have good knowledge of the English language this might be the first time many of them encounter more technical math related terms. Inspection of MESA performances by program language does suggest that students in programs exclusively taught in English performed better than those enrolled in programs taught in Danish. However, this might just reflect that these programs are generally characterized by students with a higher high school GPA[6]. Regardless of the potential underpinnings, the students' performances in the MESA suggest a notable scope for improving the math skills and hereby potentially also other study related outcomes among students at CBS.

2.3.3 Treatment Assignment

All students were offered to participate in the same face-to-face course as in previous years regardless of their performance in the assessment, but only students with a MESA test score below a certain threshold were offered, and automatically enrolled in, the self-paced online module[7]. Completing the MESA was voluntary but strongly encouraged and though there were some variation across study programs, we ended up with an overall response rate of 58% corresponding to a total of 1,060 students. Of those, 1,001 consented to having their data used for research purposes. We observe that a few students spent very little time on the MESA and had very few correct answers. To avoid any bias stemming from these outliers that we suspect might be students, who opened but did not make an honest attempt at completing the assessment, we exclude students that spent less than half the time available and had less than five correct answers. Additionally, we drop students who left more than 27 of 37

[5]Based on a simple t-test comparing the mean high school grades of students who did and did not do the MESA, we get a p-value of 0.27 and therefore cannot reject that the means are similar.

[6]The tabulation by main program language can be found in Appendix Table 2.2

[7]Because we did not want students to skip the face-to-face course, they were not enrolled in the online module before the Monday following the face-to-face course.

questions blank. When applying these two restrictions we lose 11 and 4 observations, respectively, which leaves us with a sample of 986 students.

The upper threshold for invitation to the online module was set at 17 correct answers out of the 37 total questions and was based on the performance of the students in the first round of assessments. In this round of distributions the response rate was 63% and consisted of the students enrolled in the Bachelor in Economics and Business Administration. Because this is by far the largest program at CBS[8] we consider their performance a valid basis for choosing the threshold. Indeed this seems to be the case, given that the threshold of 17 correct answers corresponds to approximately the 80^{th} percentile in both the first round and overall test score distribution.

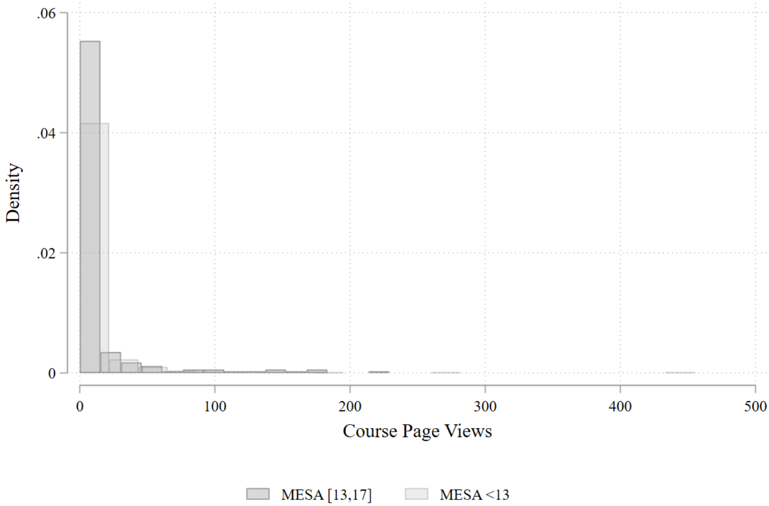
We were committed to allow all students above the threshold who wanted to participate in the online module access but received no such requests. Thus, the threshold is binding from above as no students with more than 17 correct answers participated in the online module. We use the cutoff for enrollment in the course to define a binary indicator for assignment to treatment, "Assigned Treatment", in the form of enrollment in the online remedial course. Using this definition we have 806 students assigned to treatment and 181 students in the control group.

Based on the Canvas data, we see that compliance among the treated was far from perfect. In fact, our activity data from Canvas indicate of the 806 students assigned to treatment only 481 ever visited the course page and of these only 388 students viewed the course content more than once. Even among these students the active use of the course was limited cf. Figure 2.2 that shows the density of course page views by MESA score and indicates that these were left skewed for all students regardless of performance in the MESA test.

We therefore define an additional treatment variable, "Effective Treatment", that takes the value of one if a student viewed the course at least five times. We chose this limit for two reasons. Firstly, because the design of the online course in Canvas consisted of five separate pages - the front page and one for each of the four topics. Students with less than five course page views can therefore not possibly have seen all of the ma-

[8] In fall 2021 760 students were admitted to this program, which is more than four times as many as the second largest program.

Figure 2.2. Course page views



terial. Of course, we cannot be sure that students with five or more page views did navigate through all of the five pages but this way we exclude a number of students who are unlikely to have actively engaged with the course material. Secondly, the choice of keeping students with at least five page views corresponds to the median number of page views among the students who ever viewed the course.

Because course page views might simply reflect that a student browsed through the material without actually engaging with the course content, we define another alternative dummy variable for effective treatment to capture students who actively engaged with the online module. The variable "Active Effective Treatment" is equal to one if a student viewed the course more than five times *and* watched at least one video or attempted to answer one quiz. Using these alternative treatment measures, we have 239 and 98 students with "Effective Treatment" and "Active Effective Treatment" equal to one, respectively.

As a robustness check, we also consider defining the effective treatment as a continuous variable that is equal to the number of page views in the Canvas course room to measure treatment intensity and show the results in Appendix Table 2.3. These estimates do not suggest that our results are sensitive to the choice of using a binary rather than a continuous measure of effective treatment.

2.3.4 Data and Analytical Samples

Our analyses rely on a dataset comprised of data from four separate data sources: MESA data, data from Canvas, the online platform used at the face-to-face course, and data from CBS's administrative records. In addition to assessing the students' math skills, we use the MESA data to see how much time the students spent on the assessment. The Canvas data informs us on the students' engagement with the course. In particular, we have an overall measure of each student's number of page views, viewed videos, and attempts of solving the quizzes. The online platform used at the face-to-face course was mainly a tool for administering mathematical problems to students throughout the duration of the one-day course, but was also available for students after the end of this course. We use student activity data from the platform to investigate if there is any patterns of correlation in students' use of this and the online module offered in Canvas that might indicate if these two components of CBS's overall remedial offer was used as substitutes or complements by the students.

From the administrative data, we get information on the students' performances in their final microeconomics exam and the student background characteristics; study program, sex, high school GPA, and graduation year, which we include as controls in our analyses. In the case of the students' high school graduation year, we use the information to construct a binary measure indicating if a student had at least one gap year between high school graduation and enrollment at CBS. The students in our sample are enrolled in 11 different study programs of which 5 are exclusively taught in English.

2.3.4.1 Outcome Measures

To assess the effect of the online module we consider students' performances in their final microeconomics exams as our outcome, since we expect this to heavily rely on students' math skills. The fact that students are enrolled in different study programs means that they participate in separate microeconomics courses[9] that have their own exams. Because of the different exams and because grading in Denmark is always absolute, the grade level varies across study programs[10]. To address this, we standardize each student's exam grade by the mean and standard deviation of the microeconomics exam grade for all students enrolled in their program. This means that we can interpret the size of our coefficient estimates in terms of standard deviations in the grades of a student's immediate peers. In addition to considering the intensive margin of the students' performances in the microeconomics exam, we also investigate the extensive margin by considering a binary pass/fail indicator as an outcome. When defining this measure, we also include students who did not show up for the exam, which is why we have more observations when considering this outcome.

2.3.4.2 Analytical Samples and Balance

Because we do not have information on control variables for all of the 986 students who completed the MESA[11] and because our two outcomes as explained above differ in terms of which students are included, our effective estimation sample contains less than 986 observations and varies in size across the two outcomes considered. In the subsequent analyses, we apply an additional sample delimitation to each of the two outcome samples such that we end up with four different estimation samples. In the "Full Samples" we include all students who took the MESA test and for whom we have information on controls. We then have 897 students who either passed or failed the exam and 757 students who sat down to the ordinary exam. In the "Restricted Samples", we only include students just around the cutoff for treatment assignment. More specifically, we

[9]Not all programs have a course called "microeconomics" but all have courses that cover microeconomics topics and are fairly similar in their structure and where the final exams in all but one case rely exclusively on blind grading. Importantly, all include a high degree of math-based problem solving. For brevity, we refer to all of these courses and associated grades as microeconomics.

[10]See Appendix Table 2.1 for descriptive statistics by study program.

[11]In particular, for many of the foreign students we do not have information on high school GPA.

consider students with MESA scores in the interval between 13 and 21. In this case, we have 339 and 295 observations in our estimations of pass and exam grade, respectively. This enables us to have a more comparable sample below and above the treatment threshold. We elaborate on our reasoning for doing this in Section 2.4.1.

Table 2.1 provides an overview of the shares of effectively treated students in the different samples for each of the two definitions of effective treatment and echoes the point of limited compliance among the students made in Section 2.3.3. It further shows that when introducing the additional condition of active engagement with the course material, the shares of treated students decrease with between 12.2 and 14.6 percentage points depending on the specific effective treatment definition and sample considered.

Table 2.1. Share of treated students by estimation sample and treatment definition

Panel A	**Pass Outcome**	
	Full Sample (N=897)	**Restricted Sample** (N=339)
Effective Treatment	24.5%	22.4%
Active Effective Treatment	9.9%	9.4%
Panel B	**Exam Grade Outcome**	
	Full Sample (N=757)	**Restricted Sample** (N=295)
Effective Treatment	24.6%	22.4%
Active Effective Treatment	10.4%	10.2%

Note: Restricted Samples include students with a MESA score $\in [13, 21]$.

Table 2.2 shows the balance of control and effective treatment variables for both the Full and Restricted Samples. Panel A shows balances for the pass outcome and Panel B for the exam grade outcome. While Table 2.2 indicates significant imbalance of all variables across treatment assignment status in the Full Samples, the significance disappears in all but one case when we restrict the sample to only include students who had a MESA score in the proximity of the threshold for assignment to treatment. The exception is among students with and without at least one gap year when considering the exam grade outcome. Here Table 2.2

shows that the students assigned to treatment were more likely to have had a gap year than those who were not. Since students who had a gap year are also more likely[12] to have had a longer period of time elapse since their last math studies than those who enrolled at CBS right after high school graduation, it is not too surprising that they are more likely to be assigned to treatment. Still, overall Table 2.2 supports the argument that comparability of students increases as we consider a more narrow bandwidth around the cutoff. The table also shows that the significant difference in outcomes and MESA scores persists in all other cases than for the Pass variable in the Restricted Sample. Because the MESA scores are assumed to be a strong predictor of student performance in microeconomics this significance was to be expected. The significant differences in the outcomes might suggest that there is a positive treatment effect, though we based on these simple raw correlations of course cannot rule out alternative explanations.

2.4 Empirical strategy

We wish to estimate the effect of the online remedial course on students' performances in their microeconomics exams conditional on controls:

$$Y_{is} = \alpha_0 + \alpha_1 \text{Effective T}_i + \alpha_2 \text{MESA}_i + \alpha_3 \mathbf{X}_i + \delta_s + \varepsilon_{is} \tag{2.1}$$

Where Y_{is} is either the standardized grade in the microeconomics exam or a dummy for passing the exam for student i in study program s. MESA_i is the student's score on the MESA test. $\mathbf{X}_i$ is a vector of control variables including the MESA score, high school GPA, gender, and a dummy indicating if a student had a gap year. Effective T_i indicates whether the student participated in the online remedial course and thus α_1 is the coefficient of interest. δ_s is a study program fixed effect, which we include to control for factors, such as lecturers, curriculum, exams, and examin-

[12]Because some students do supplementary math courses in their gap year, while others might have finished their math classes in their penultimate high school year, the students' high school graduation year is not a completely accurate measure of when they did their last math studies.

Table 2.2. Balance tables

Panel A	**Pass Outcome**					
	Full Sample			**Restricted Sample**		
	Not Assigned Treatment (N=153)	Assigned Treatment (N=744)	t-test p-value	Not Assigned Treatment (N=93)	Assigned Treatment (N=246)	t-test p-value
MESA Test Score	21.307	10.958	10.349***	19.280	14.825	4.454***
	[0.250]	[0.123]		[0.114]	[0.085]	
Female	0.366	0.488	-0.122***	0.398	0.439	-0.041
	[0.039]	[0.018]		[0.051]	[0.032]	
High School GPA	9.950	9.041	0.909***	9.635	9.416	0.220
	[0.119]	[0.055]		[0.156]	[0.085]	
Gap Year	0.739	0.878	-0.1391***	0.796	0.854	-0.058
	[0.036]	[0.012]		[0.042]	[0.023]	
Panel B	**Exam Grade Outcome**					
	Full Sample			**Restricted Sample**		
	Not Assigned Treatment (N=136)	Assigned Treatment (N=621)	t-test p-value	Not Assigned Treatment (N=83)	Assigned Treatment (N=212)	t-test p-value
MESA Test Score	21.307	11.016	10.359***	19.373	14.897	4.567***
	[0.265]	[0.134]		[0.121]	[0.091]	
Female	0.346	0.464	-0.118***	0.361	0.410	-0.049
	[0.041]	[0.020]		[0.053]	[0.034]	
High School GPA	9.914	9.034	0.909***	9.570	9.361	0.209
	[0.132]	[0.060]		[0.172]	[0.094]	
Gap Year	0.721	0.882	-0.162***	0.783	0.863	-0.080*
	[0.039]	[0.013]		[0.046]	[0.024]	

Note: Restricted Samples include students with a MESA score $\in [13, 21]$. Standard deviation in squared parentheses. The t-tests test null hypothesis of no differences in means across groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

ers, that vary across but not within study programs and might affect a student's performance in their final exam.

Because participation in the remedial course was voluntary for students in the treatment group we would expect that students who actually used the online module to differ in terms of for example motivation and effort compared to their non-compliant peers. Since these unobservables are likely correlated with their performance in the microeconomics exam as well, we would expect that $\hat{\alpha}_1$ tends to be upward biased.

We address this selection problem by utilizing the fact that the probability of treatment jumps around the cutoff. Formally, we can express this as:

$$Pr[\text{Assigned T}_i = 1|\text{MESA}_i] = \begin{cases} h_0(\text{MESA}_i) & if \quad \text{MESA}_i > 17 \\ h_1(\text{MESA}_i) & if \quad \text{MESA}_i \leq 17 \end{cases} \tag{2.2}$$

We can exploit this discontinuity to estimate the local average treatment effect from the reduced form corresponding to an intention-to-treat estimate:

$$\text{Y}_{is} = \alpha_0 + \alpha_1 \text{Assigned T}_i + \alpha_2 \mathbf{X}_i + \delta_s + \varepsilon_{is} \tag{2.3}$$

Where the parameter α_1 now captures the effect of being assigned to the remedial math course. However, due to partial compliance this estimate is likely downward biased, as some treated students never used the online math module. To address this issue, we apply a fuzzy RDD where we use the conditional exogeneity of the treatment assignment mechanism as an instrument for effective treatment. Specifically, we estimate the effect of the remedial math module by two stage least squares (2SLS), where the first stage is expressed as the following:

$$\text{Effective T}_i = \pi_0 + \pi_1 \text{Assigned T}_i + \pi_2 f(\text{MESA}_i) + \pi_3 \mathbf{X}_i + \delta_s + \mu_{is} \tag{2.4}$$

and the second stage as:

$$Y_{is} = \beta_0 + \beta_1 \text{Effective T}_i + \beta_2 g(\text{MESA}_{is}) + \beta_3 \mathbf{X}_i + \delta_s + \epsilon_i s \tag{2.5}$$

In both stages we follow the common practice within the literature of including functions of the MESA score, $f(\cdot)$ and $g(\cdot)$, rather than only the raw measure itself, to avoid imposing strong functional form assumptions. This is particularly important for RDD estimations because the lack of overlap between treated and untreated observations means that it will always rely on some extrapolation and that correct specification of the functional form as a consequence is crucial in order to avoid specification bias in the estimates. By including functions of the MESA score through $f(\cdot)$ and $g(\cdot)$, we reduce the the risk of *extreme* extrapolation (De Paola & Scoppa, 2014).

Though failure of the empirical model implied by Equation 2.4 and 2.5 to correctly capture the underlying conditional expectation function

(CEF) implies that the estimate of the treatment effect will usually be subject to specification bias, the size of this bias will be negligible relative to its standard deviation if one considers a close interval around the cutoff (Kolesár and Rothe, 2018). Therefore, we estimate and subsequently compare the estimated effects of the online remedial module for both the full sample of students and for a restricted sample of students with a MESA score close to the cutoff, namely those with MESA$\in$ $[13, 21]$. As described in Section 2.3 and displayed in Table 2.2, we cannot reject that the students falling below and above the cutoff in the restricted sample have similar observable characteristics except in terms of whether they had a gap year after finishing high school or not. For robustness, we consider alternative intervals, in the RDD literature referred to as bandwidths, for our main specification with local polynomials and show the results in the appendix. More specifically, we conduct two analyses to test the sensitivity of our results to the choice of bandwidth. In the first one, the results of which are reported in Appendix Table 2.5, we consider both a narrower ($[15-19]$) and a wider ($[11-23]$) interval of the MESA score but detect no changes in the significance of our estimates. The results of the second bandwidth sensitivity analysis are displayed in Appendix Table 2.6. In this case, we not only change the bandwidths but also apply a triangular kernel function to put more weight on observations closer to the cutoff for assignment to treatment. Also in this case, we do not observe any significant changes.

It is important to note that within an RDD setting, treatment estimates should always be considered as estimates of the marginal effect among compliers and that one therefore should be cautious of generalizing the effects to other parts of the student population. In our case, the interpretation of the estimate of Effective Treatment is that it expresses the effect of the online remedial math course of the student with a MESA score on the margin of being treated, i.e. with a score equal to the cutoff value of 17 correct answers. The more likely it is that these students are similar to other students, the more likely it is that the results can be generalized to other parts of the student population.

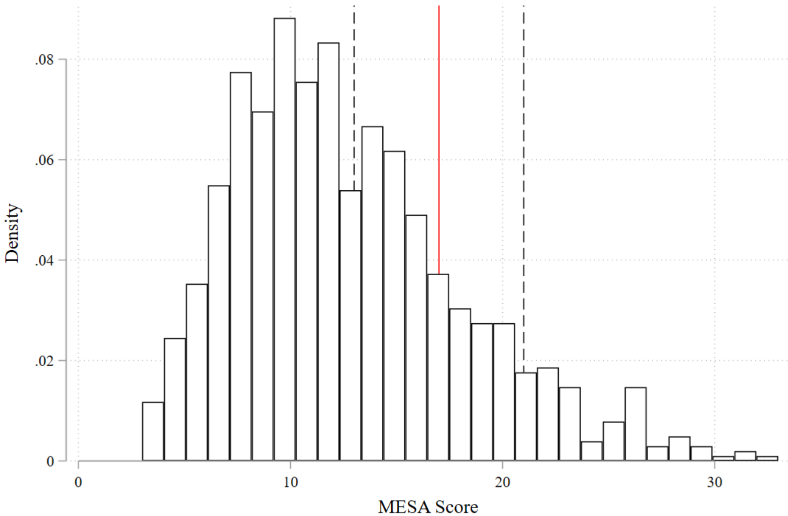
2.4.1 Validity of Fuzzy RDD

Two important assumptions for correct identification of the treatment effect in the RDD setup are 1) that the outcome variable is continuous in the running variable and 2) that there is no manipulation of the running variable, i.e. that the density of the running variable is continuous around the cutoff. In reality, the requirements for the former are less strict. It is only necessary for the relationship to be continuous in the cutoff value. Moreover, it is also acceptable that the running variable is observed as discrete, as long as the underlying relationship is continuous (Cameron and Trivedi, 2005). This will be the case in our setting, where one can think of the MESA score as a continuous variable of the students' mathematical abilities that is, however, only observed as discrete numbers of correct answers.

It is not possible to formally assess the validity of 1), which will therefore have to rely on intuitive argumentation. We further discuss the plausibility of this assumption in Section 2.6.3. To verify 2), one can inspect the density of the running variable around the cutoff and, if there are enough observations in the narrow interval around this point, it is possible to do a t-test to formally compare the observations on either side, as we did in Table 2.2. In general, it is preferable to base treatment assignment on a running variable that cannot be manipulated. As the students did not know the cutoff for assignment to treatment in the online remedial module this is effectively the case in our setting.

Inspection of the density of the test scores in Figure 2.3 confirms our assumption of no jumps around the cutoff. In other words, we do not detect any visual signs of manipulation of the MESA scores, which supports our conclusion of no alarming differences in observable characteristics in the proximity of the cutoff based on Table 2.2. Intuitively, this provides some reassurance that even though performance in the MESA and in the final microeconomics exam are correlated, then, given randomness in the measure of the MESA scores in the interval around the cutoff, students at and right below 17 correct answers will be academically comparable to those just above the threshold. The credibility of this assumption is what allows us to attribute any jump in performance on the microeconomics exam to the online remedial module. Of course, as we move further away from the cutoff the plausibility of this assumption decreases. This is

Figure 2.3. Density of MESA test scores

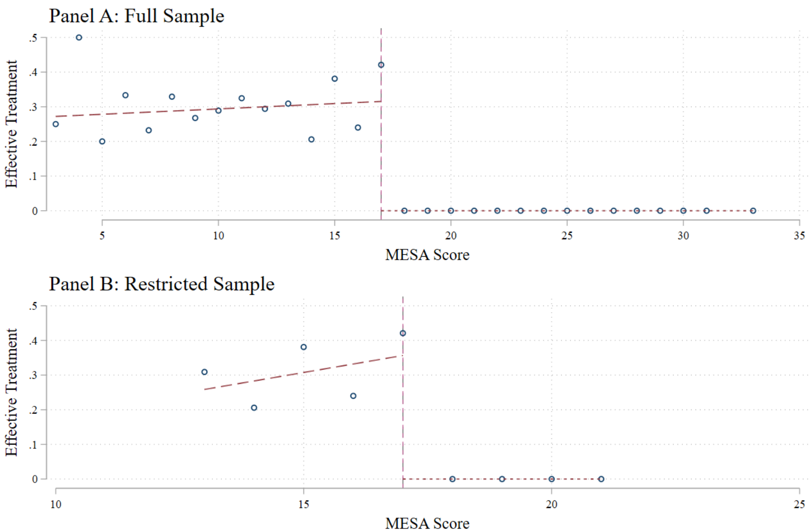


the reason why the conservative interpretation of the RDD estimate only applies for the marginal students assigned to treatment, while the willingness to assume that these are similar to students on aspects relevant for performance in the microeconomics exam in the proximity of the cutoff, will enable us to consider it a valid measure of the treatment effect for students with MESA scores in this interval.

Because of the limited compliance among the students and the fact that we have a discrete running variable, we are limited in our ability to estimate the treatment effect in an interval arbitrarily close to the cutoff, which increases the risk of having a large specification bias relative to the standard deviation. Limitations on the choice set of bandwidths due to having a discrete running variable would persist even if one had access to an infinite sample size (Lee and Card, 2008). In the case of no or only few observations close to the cutoff, having a discrete running variable further implies that the causal treatment effect is not identified without imposing a parametric functional form on the relationship between outcomes and treatment (Lee and Card, 2008). This further emphasizes the importance of choosing the correct functional form.

In addition to the plausibility of 1) and 2), the validity of fuzzy RDD also depends on compliance among those assigned to treatment. This is the case because fuzzy RDD is a type of instrumental variable estimation, where compliance determines the existence and strength of the first stage. Figure 2.4 shows binned scatter plots between the MESA score, which constitutes the basis for treatment assignment, and Effective Treatment to allow for visual inspection of this relationship.

Figure 2.4. First stage relationship



Note: Binned scatter plots based on local linear fits of the first stage relationship between the MESA test score and effective treatment. Panel A depicts the test score distribution for the Full Sample and Panel B for the Restricted Sample.

Panel A depicts the test score distribution for the Full Sample, while Panel B focuses on the Restricted Sample and thus zooms in on the interval around the cutoff. In both cases, we show a local linear fit of the relationship between the MESA test score and effective treatment. Despite the limited compliance among those assigned to treatment discussed earlier, Figure 2.4 does indicate a jump in the effective treatment around the cutoff of 17 correct answers. This is reassuring for our identification strategy and is to a large extent attributable to the fact that no students with more than 17 correct answers was assigned to treatment.

2.5 Compliance, Intention-to-Treat, and OLS Estimations

We begin our empirical investigation by examining the correlation between the MESA score and the outcome variables. In addition to allowing us to assess the existence of these relationships and thus whether the MESA score is a good measure to use for treatment assignment, this also provides us with a first indication of what functional form might provide a good fit of our data. As outlined in the previous section, the task of correctly specifying the functional form is pivotal in RDD based empirical studies. Though some theoretical papers offer more formal guidelines for choosing the functional form, the approach to making this choice has in applied research often amounted to visual inspection of the correlation between the running variable and outcome and to comparisons of different functional forms and intervals around the cutoff (Pei et al., 2022)[13]. Because our data does not allow for applying more formal methods for choosing the functional form, we rely on visual inspection to get an impression of the relationship between the MESA score and the outcome variables.

Figure 2.5 and 2.6 depict binned scatter plots showing the relationship between MESA scores and the standardized microeconomics grades (Figure 2.5) or the pass rate (Figure 2.6), when considering different functional forms of the relationship. Each subplot shows the correlations between outcome and MESA scores in either the Full (Panel A) or Restricted (Panel B) Sample. The first column depicts local linear fits and the second local quadratic fits.

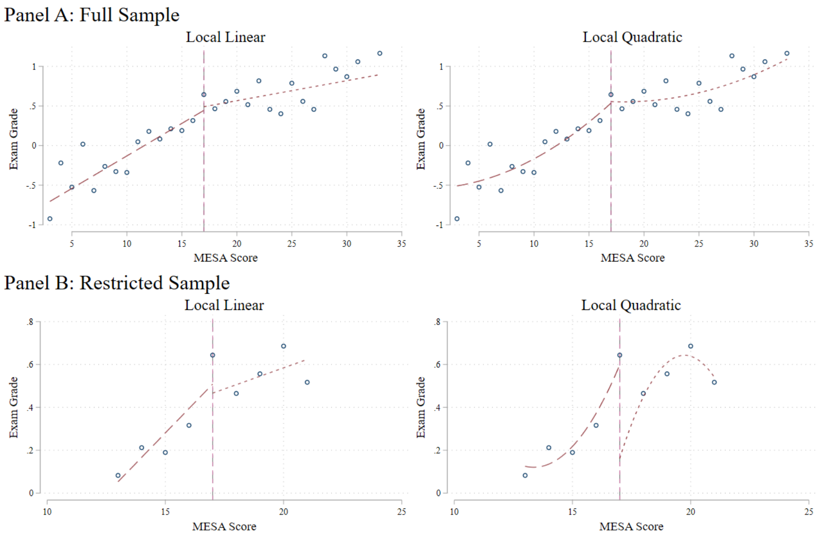
[13] While Pei et al. (2022) develop a data-driven approach to selecting an appropriate polynomial order, their method is unfortunately not applicable for our data. Similarly, the data-driven approach for choosing the optimal bandwidth presented in Calonico et al. (2014) is ill-suited for our discrete data with limited coverage around the cutoff.

When considering the Full Sample correlations, the graphs suggest a positive relationship between the MESA score and performance in the microeconomics exam, which provides some support for using the MESA score as a measure for assignment to treatment. At the same time, the subplots in Panel A in most cases do not display any notable discontinuities and thus, do not suggest the existence of any treatment effects. While neither functional form specification appears to be superior in terms of capturing the relationships for the Full Sample, the local quadratic specifications of the MESA test score looks to perhaps be overfitting the relationship for students close to the cutoff when considering the exam grade as the outcome. Moreover, the differences between the discontinuities implied by the subplots in the Panel B of Figure 2.5 highlight the importance of choosing the correct functional form. Here the local linear fit does not indicate any notable treatment effect, while the local quadratic fit shows a marked jump in the level of the microeconomics exam grade around the cutoff for assignment to treatment, hereby suggesting that there might be a positive treatment effect, as students right below the cutoff on average seem to perform better than those right above. For the pass outcome, Panel B of Figure 2.6 suggests the presence of a discontinuity for both specifications but do not indicate that there is great difference between the local linear and local quadratic fit.

Overall, the visual impression left by Figure 2.5 and 2.6 does not suggest any clear tendency of either functional form to outperform the other. Therefore, due to concerns of potentially overfitting the data we estimate functional forms with linear and local linear fits of the MESA score in our main regressions but do not consider local polynomials of the second degree even though this is has been suggested as good practice (Gelman and Imbens, 2019). Instead we consider this and other alternative specifications in Appendix Table 2.4 as a robustness check. The estimates in this table are generally consistent with those based on our main specification except for the case with the local polynomials of the second degree, which confirms our suspicion of that particular functional form being a poor choice for our data.

In practice, we achieve local linear regressions by including the MESA score, as well as the interaction of Assigned Treatment and the MESA score in both $f(\cdot)$ and $g(\cdot)$. It is the addition of the latter that results in a *local* linear regression, by allowing the slopes to vary on either side of

Figure 2.5. Exam grade



Note: Binned scatter plots of the relationship between the MESA test score and the final exam grade in the students' microeconomics course. Panel A depicts the test score distribution for the Full Sample and Panel B panel for the Restricted Sample. The graphs in the first column show local linear fits and the second shows local quadratic fits.

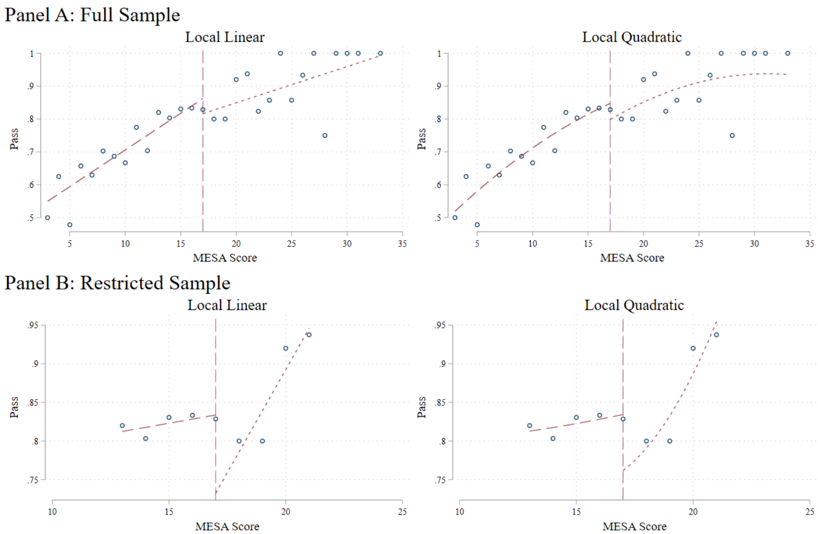
the cutoff.

Having now explored the relationship between our running variable and the outcomes, as well as decided on a baseline specification of the functional form, we next turn our attention towards getting a sense of the extent and effect of compliance among the students by presenting the OLS and reduced form estimates of Effective Treatment. The former exercise allows us to get some suggestive insights on the effect of the online remedial course among the students who actually used the course, while the latter is instructive for guiding policy recommendations.

2.5.1 Reduced Form Estimates

The reduced form estimates based on the regression of the outcomes on Assigned Treatment and covariates, as expressed in Equation (2.3), are

Figure 2.6. Pass rate



Note: Binned scatter plots of the relationship between the MESA test score and the students' probability of passing the exam in the microeconomics course. Panel A depicts the test score distribution for the Full Sample and Panel B panel for the Restricted Sample. The graphs in the first column show local linear fits and the second shows local quadratic fits.

displayed in Table 2.3. Because the reduced form estimates express the effect of being assigned rather than effectively treated with the online remedial math module they capture the intention-to-treat (ITT) effect. The ITT effect is relevant for policy purposes if policy makers are mainly interested in the effect of offering the course to students in general, while putting a lower weight on its effect on the students who actually use the course. This also means that the ITT effect is highly sensitive to the degree of compliance and we expect the estimates to be biased towards zero compared to the ones based on fuzzy RDD, given that we observe limited compliance among the students.

The sign of the point estimates in 2.3 vary across functional form specifications, with the estimates for both outcomes being positive when considering a linear fit but negative when allowing for the slopes to vary on either side of the cutoff. However, in all cases the estimates are associated with large uncertainty compared to the magnitude of the point estimate, which means that we cannot reject that they are equal to zero.

Table 2.3. Reduced form estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pass				**Exam Grade**			
Assigned Treatment	0.045	-0.030	0.004	-0.019	0.072	-0.106	0.100	-0.100
	(0.041)	(0.044)	(0.062)	(0.073)	(0.130)	(0.141)	(0.208)	(0.258)
High School GPA	0.040***	0.040***	0.021	0.021	0.152***	0.152***	0.165***	0.170***
	(0.010)	(0.010)	(0.015)	(0.015)	(0.025)	(0.025)	(0.046)	(0.046)
Female	-0.020	-0.020	-0.031	-0.032	-0.100	-0.099	-0.046	-0.053
	(0.027)	(0.027)	(0.035)	(0.035)	(0.071)	(0.071)	(0.115)	(0.115)
Gap Year	0.097**	0.090**	0.064	0.066	0.514***	0.497***	0.384***	0.400***
	(0.038)	(0.038)	(0.047)	(0.047)	(0.091)	(0.090)	(0.138)	(0.138)
MESA Score	0.017***	-0.006	0.012	-0.001	0.073***	0.018	0.092**	-0.018
	(0.004)	(0.008)	(0.013)	(0.026)	(0.010)	(0.019)	(0.041)	(0.090)
Assigned Treatment×MESA Score		0.027***		0.016		0.064***		0.138
		(0.009)		(0.029)		(0.021)		(0.101)
Observations	897	897	339	339	757	757	295	295
R-squared	0.318	0.323	0.458	0.458	0.205	0.211	0.121	0.126
Sample	Full	Full	Restricted	Restricted	Full	Full	Restricted	Restricted
Program Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

2.5.2 OLS Estimates

Table 2.3 shows the OLS estimates of Effective Treatment (Panel A) and Active Effective Treatment (Panel B). Like the ITT estimates, the OLS estimates displayed in Table 2.4 are sensitive to partial compliance. In the OLS case, the sensitivity arises if the compliant sub-population of students who self-selects into Effective Treatment are different from the full population of students on unobservables that are correlated with their performances in the microeconomics exam. As explained in Section 2.4, we consider this to be highly likely, given that the compliant students arguably had Effective Treatment exactly because they are characterized by differential non-cognitive abilities such as motivation, conscientiousness, and/or diligence compared to the full student population.

The estimated treatment effects in Table 2.3 are in almost all cases positive for both treatment definitions, which indicate that the students who used the online remedial math module on average performed better in their microeconomics exams than those who did not. The OLS estimates on Active Effective Treatment are always numerically larger than for Effective Treatment and significantly different from zero, except in the estimations of the effect of the probability of passing the exam in the the Restricted Sample. This could suggest that students engaging more, or more actively, with the online module experienced the biggest benefits in terms of performance in the microeconomics exam but could also just reflect different unobservables between the groups of students selecting into the two different types of treatments. It should here once again be emphasized that we, as outlined in Section 2.4, should be wary of interpreting this as a causal effect of the remedial math module. Due to the issue of self-selection into (Active) Effective Treatment, these OLS estimates could merely reflect the presence of systematic variation in for example non-cognitive abilities relevant for performance in the microeconomics exam between the full student population and the compliant sub-group.

Though we are not able to assess if students selecting into treatment differ on unobservable student characteristics, we can investigate if they differ on observables.

In Table 2.5 we display the correlations between observable student characteristics and the two effective treatment measures for each of our estimation samples. The estimates show the results from regressing Effective Treatment and Active Effective Treatment on sex, high school GPA, the MESA score, and whether a student had a gap year.

Four interesting insights emerge from this exercise. First, Active Effective Treatment with the online module was more likely to be correlated with observable student characteristics than Effective Treatment, which might suggest that the unobservable differences also vary depending on the definition of effective treatment, as we have previously hypothesized. The results also indicate that the male students were more likely to use the online module, though the indication of such tendency is most clear for Active Effective Treatment. Thirdly, the module was used more by students who had at least one gap year between high school graduation and enrollment at CBS than those who started in the same year as they graduated.

Finally, Table 2.5 shows that there is a positive correlation between high school GPA and Active Effective Treatment across all estimation samples. The table therefore suggests that there might be a particular scope for improving the benefits of the online module by increasing the en-

Table 2.4. OLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pass				Exam Grade			
Panel A								
Effective Treatment	0.032	0.024	-0.019	-0.023	0.148*	0.131*	0.088	0.054
	(0.030)	(0.030)	(0.041)	(0.041)	(0.078)	(0.079)	(0.132)	(0.134)
High School GPA	0.039***	0.039***	0.022	0.022	0.150***	0.150***	0.164***	0.166***
	(0.010)	(0.010)	(0.015)	(0.015)	(0.025)	(0.025)	(0.046)	(0.045)
Female	-0.020	-0.019	-0.033	-0.033	-0.099	-0.095	-0.043	-0.047
	(0.027)	(0.027)	(0.035)	(0.035)	(0.071)	(0.071)	(0.116)	(0.116)
Gap Year	0.096**	0.090**	0.066	0.068	0.504***	0.490***	0.374***	0.391***
	(0.038)	(0.038)	(0.047)	(0.047)	(0.091)	(0.090)	(0.138)	(0.138)
MESA Score	0.015***	-0.001	0.010	0.002	0.071***	0.036***	0.079***	0.016
	(0.003)	(0.006)	(0.007)	(0.015)	(0.006)	(0.013)	(0.023)	(0.052)
Assigned Treatment×MESA Score		0.023***		0.015		0.051**		0.110
		(0.008)		(0.025)		(0.020)		(0.082)
Observations	897	897	339	339	757	757	295	295
R-squared	0.319	0.324	0.458	0.459	0.209	0.214	0.121	0.126
Panel B								
Active Effective Treatment	0.118***	0.111***	0.016	0.014	0.356***	0.339***	0.285*	0.261*
	(0.033)	(0.034)	(0.046)	(0.048)	(0.094)	(0.094)	(0.149)	(0.151)
High School GPA	0.038***	0.038***	0.020	0.021	0.145***	0.145***	0.158***	0.159***
	(0.010)	(0.010)	(0.015)	(0.015)	(0.025)	(0.025)	(0.046)	(0.046)
Female	-0.016	-0.015	-0.030	-0.030	-0.091	-0.088	-0.025	-0.028
	(0.027)	(0.027)	(0.035)	(0.035)	(0.071)	(0.071)	(0.117)	(0.117)
Gap Year	0.091**	0.085**	0.063	0.065	0.495***	0.480***	0.360***	0.375***
	(0.038)	(0.038)	(0.047)	(0.047)	(0.091)	(0.090)	(0.138)	(0.138)
MESA Score	0.015***	-0.001	0.012	0.005	0.071***	0.037***	0.081***	0.023
	(0.003)	(0.006)	(0.007)	(0.016)	(0.006)	(0.013)	(0.023)	(0.051)
Assigned Treatment×MESA Score		0.023***		0.011		0.049**		0.104
		(0.008)		(0.025)		(0.020)		(0.081)
Observations	897	897	339	339	757	757	295	295
R-squared	0.324	0.329	0.458	0.458	0.217	0.221	0.128	0.133
Sample	Full	Full	Restricted	Restricted	Full	Full	Restricted	Restricted
Program Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

gagement of academically weak students. At the same time, it indicates that the positive OLS estimates might be due to differences across the students who select into treatment and those who do not. Both because high school GPA provides a proxy for academic ability and because this measure is likely to also reflect differences in non-cognitive abilities that are correlated with performance in the microeconomics exam. In particular, it may be the case that achieving a high high school GPA requires not only academic capability but also motivation and diligence to perform over a continued period of time and in a range of different subjects that might not overlap with one's specific interests.

Table 2.5. Correlations between observables and effective treatment

	(1)	(2)	(3)	(4)
	Effective Treatment			
Panel A				
High School GPA	0.009	0.032	0.014	0.028
	(0.012)	(0.024)	(0.013)	(0.026)
Female	-0.066**	-0.092	-0.042	-0.056
	(0.034)	(0.058)	(0.037)	(0.064)
MESA Score	0.002	0.026	0.002	0.038
	(0.005)	(0.023)	(0.006)	(0.025)
Gap Year	0.049	0.152*	0.078	0.174*
	(0.052)	(0.084)	(0.057)	(0.095)
Observations	744	246	621	212
R-squared	0.007	0.030	0.006	0.032
	Active Effective Treatment			
Panel B				
High School GPA	0.016*	0.046***	0.023**	0.049***
	(0.009)	(0.017)	(0.010)	(0.019)
Female	-0.052**	-0.106***	-0.039	-0.086*
	(0.023)	(0.040)	(0.026)	(0.045)
MESA Score	0.001	-0.001	0.002	0.009
	(0.004)	(0.016)	(0.004)	(0.018)
Gap Year	0.080**	0.135***	0.080**	0.144**
	(0.032)	(0.051)	(0.038)	(0.063)
Observations	744	246	621	212
R-squared	0.016	0.061	0.017	0.056
Sample	Full	Restricted	Full	Restricted
Outcome Sample	Pass	Pass	Exam Grade	Exam Grade

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Overall, Table 2.5 strengthens our suspicion that the OLS estimates in Table 2.4 may be subject to a positive selection bias.

2.6 The Effect of Online Remedial Math on Microeconomics Performance

In this section, we formally assess the potential of our online remedial math module to benefit students' performances in math-based microeconomics classes. As described in Section 2.2, the effect of remediation is ambiguous from a theoretical point of view. However, given the voluntary nature of the online remedial math module considered in this study, we expect the potential mechanisms resulting in positive effects to be larger than those who might lead to negative student outcomes.

We begin with describing our analysis of the effect of Effective Treatment and then present the results of the estimations of the Active Effective Treatment. Recall that the difference between these two treatment dummy variables is that, while the former takes the value of one if a student had more than five page views in the online module, the latter only considered a student as treated if they also exhibited signs of active engagement in the form of either watching at least one video or attempting to answer one quiz question. Because the latter measure is a stronger indicator for whether students actually engaged with the online material and because previous studies have found that active learning increases students outcomes (Freeman et al., 2014) even when students' own perceptions suggest the opposite (Deslauriers et al., 2019), we would expect that this treatment effect is larger in absolute terms.

2.6.1 Effective Treatment

Panel A of Table 2.6 shows the second stage fuzzy RDD estimates of Effective Treatment on the students' performance in the microeconomics exam based on Equation 2.5 for the Full and Restricted Samples across different functional form specifications. The first four columns show the estimated effect on the students' probability of passing the exam and the last four the effect on their standardized exam grades.

In all columns, the standard errors are large relative to the size of the point estimate and, as a consequence, the estimates of the Effective Treatment are not significantly different from zero for either of the two outcomes regardless of the functional form specifications and samples considered. Ideally, we would like the estimates for each outcome to be relatively consistent across samples and functional form specifications to inspire faith in the estimates' ability to provide an unbiased measure of the effect of Effective Treatment of performance in the microeconomics exam. This is generally not the case here and though all of the estimated treatment effects are insignificant, this emphasizes the point about the importance of choosing the proper functional form that we made in Section

Table 2.6. Fuzzy RDD estimates of effective treatment with online remedial math on student performance in microeconomics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A	**Second Stage**							
	Pass				**Exam Grade**			
Effective Treatment	0.138	-0.096	0.010	-0.060	0.223	-0.334	0.238	-0.298
	(0.128)	(0.140)	(0.160)	(0.228)	(0.402)	(0.442)	(0.490)	(0.751)
High School GPA	0.038***	0.041***	0.021	0.023	0.149***	0.156***	0.161***	0.176***
	(0.010)	(0.010)	(0.016)	(0.018)	(0.026)	(0.025)	(0.047)	(0.049)
Female	-0.016	-0.023	-0.031	-0.036	-0.097	-0.103	-0.034	-0.070
	(0.028)	(0.028)	(0.039)	(0.042)	(0.071)	(0.072)	(0.115)	(0.124)
Gap Year	0.091**	0.095**	0.063	0.072	0.499***	0.519***	0.358**	0.434**
	(0.039)	(0.039)	(0.050)	(0.055)	(0.094)	(0.094)	(0.150)	(0.169)
MESA Score	0.017***	-0.006	0.012	-0.002	0.073***	0.018	0.086***	-0.024
	(0.004)	(0.008)	(0.011)	(0.029)	(0.010)	(0.019)	(0.030)	(0.101)
Assigned Treatment×MESA Score		0.027***		0.019		0.065***		0.156
		(0.009)		(0.035)		(0.022)		(0.130)
Panel B	**First Stage**							
	Effective Treatment							
Assigned Treatment	0.324***	0.313***	0.379***	0.312***	0.322***	0.317***	0.419***	0.334***
	(0.048)	(0.037)	(0.088)	(0.066)	(0.053)	(0.041)	(0.096)	(0.072)
High School GPA	0.012	0.012	0.027	0.028	0.014	0.014	0.019	0.021
	(0.011)	(0.011)	(0.018)	(0.018)	(0.011)	(0.011)	(0.018)	(0.018)
Female	-0.031	-0.031	-0.073	-0.075	-0.012	-0.012	-0.053	-0.056
	(0.031)	(0.031)	(0.046)	(0.046)	(0.033)	(0.033)	(0.049)	(0.049)
Gap Year	0.047	0.046	0.097*	0.101*	0.067*	0.066	0.107*	0.114**
	(0.038)	(0.039)	(0.054)	(0.055)	(0.040)	(0.041)	(0.057)	(0.057)
MESA Score	-0.000	-0.003	0.017	-0.021	-0.001	-0.002	0.026	-0.021
	(0.005)	(0.003)	(0.019)	(0.013)	(0.005)	(0.003)	(0.020)	(0.014)
Assigned Treatment×MESA Score		0.004		0.048*		0.002		0.059**
		(0.006)		(0.026)		(0.006)		(0.028)
Observations	897	897	339	339	757	757	295	295
R-squared	0.091	0.091	0.166	0.169	0.097	0.097	0.172	0.178
Program Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	Restricted	Restricted	Full	Full	Restricted	Restricted
Effective F statistics†	44.77	71.39	18.43	22.40	36.85	60.62	19.01	21.61

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

†: Based on Olea and Pflueger (2013). Corresponding worst case bias for different levels of τ. τ= 5%: 37.42. τ=10%: 23.11. τ= 20%: 15.06

2.4.

Table 2.6 further indicates that there is a significant positive relationship between the MESA score and the performance in the microeconomics exam in all simple linear regression specifications except when estimating the probability of passing the exam for students in the Restricted Sample. This relationship might mainly be driven by students with a MESA score below the cutoff, as indicated by the fact that only the interactions between the MESA score and Assigned Treatment and not the MESA score itself are significant in Column (2) and (6). It also shows that except for the estimations in the restricted samples with pass as the outcome, high school GPA and having had a gap year are significantly correlated with better exam performance and that females tend to perform worse at the exam than their male peers, albeit never significantly so.

As expected, the absolute values of the estimates are always numerically larger than the corresponding estimates of the ITT in Table 2.3, though we, as previously mentioned, in neither case reject that the effects are equal to zero. Moreover, comparison with Table 2.4 suggest that the OLS estimates indeed seem to have an upwards bias compared to the corresponding fuzzy RDD estimates.

Panel B of Table 2.6 displays the corresponding first stage estimates resulting from estimation of Equation 2.4 and indicate that Assigned Treatment is estimated to be a significant predictor of Effective Treatment in all samples and for all functional form specifications. To get an impression of whether we might have a weak instruments problem, in which case our estimates would tend to be biased towards the OLS estimates, the table also includes the effective F-statistics and worst case biases for different threshold levels of uncertainty based on the method described in Olea and Pflueger (2013). As mentioned in Section 2.4.1, the issue of weak instruments arises in a fuzzy RDD setting when there is only limited compliance among those assigned to treatment. Feir et al. (2016) show that the often invoked approach of comparing the first stage F-statistics with a “critical value” of 10, might lead to serious under-reporting of weak instruments, as this rule-of-thumb threshold is significantly lower than what would be needed to a provide credible indication that estimations does not suffer from issues associated with weak identification in a fuzzy RDD setting.

The effective F-statistics suggested by Olea and Pflueger (2013) is a scaled version of the non-robust first-stage F-statistics that allows for heteroskedastic errors and provides an alternative way to test of the null hypothesis of weak instruments. More specifically, the test considers the null that the 2SLS bias is large relative to a so-called “worst-case” bias of level τ that the researcher is willing to accept due to weak instruments[14]. As their baseline implementation, Olea and Pflueger test the

[14]They consider the specification of the bias presented in Nagar (1959).

null hypothesis that the weak instrument bias is bigger than τ=10% of the worst-case bias with a size of 5%. In this case, the relevant "critical value" to compare to the effective F-statistics is 23.1, which is more than twice the size of the standard rule-of-thumb level for the normal first stage model F-statistics of 10. Consequently, Olea and Pflueger suggest their own simple asymptotically valid rule-of-thumb of evaluating whether a model's effective F-statistics exceeds 23.1, in which case researchers can reject that the null of the bias exceeds 10% of the worst-case bias with a size of 5%.

Table 2.6 shows that the effective F-statistics is largest in the full samples where it always exceeds 23.1. This is as expected given that weak instruments bias is a finite sample problem. The table further indicates that the effective F-statistics increase when the functional form assumptions are relaxed by allowing for different slopes around the cutoff for assignment to treatment. The critical values of the effective F-statistics associated with uncertainty levels of $\tau = 5, 10, 20$ indicate that we might have some bias in the restricted samples, as the effective F-statistics does not exceed the threshold values for neither τ= 5% of 37.42 nor τ=10% of 23.11 (the rule-of-thumb level), though the specification with a local linear fit for both outcomes are close to the 10% threshold.

Overall, the effective F-statistics suggest that Assigned Treatment is a reasonably strong instrument for Effective Treatment but also that we might have some weak instrument bias in the restricted samples if we do not allow for the slope to vary around the cutoff.

2.6.2 Active Effective Treatment

We now turn towards estimating the effect of Active Effective Treatment on the students' performance in the microeconomics exam. As previously noted, we expect that this measure is a better indicator of active learning and therefore also associated with a bigger effect on student performance.

The results based on Equation (2.5) and (2.4) using Active Effective Treatment instead of Effective Treatment are reported in Table 2.7. Similar to Table 2.6, the columns in the table vary according to the functional forms and estimation samples considered. As expected, Panel A of the table shows that the estimated effect of the online remedial math module is greater in absolute terms, when we use Active Effective Treatment as our binary treatment indicator. In fact, the point estimates are in all cases at least twice as large compared to the corresponding estimates in Table 2.6. Given that the standard errors of these estimates are similarly larger, it comes as no surprise that none of the estimated treatment effects can be statistically distinguished from zero.

Table 2.7. Fuzzy RDD estimates of active effective treatment with online remedial math on student performance in microeconomics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A	**Second Stage**							
	Pass				**Exam Grade**			
Active Effective Treatment	0.331	-0.233	0.033	-0.216	0.462	-0.725	0.626	-0.835
	(0.308)	(0.345)	(0.527)	(0.838)	(0.834)	(0.972)	(1.302)	(2.115)
High School GPA	0.035***	0.043***	0.020	0.029	0.143***	0.165***	0.146**	0.195**
	(0.011)	(0.011)	(0.025)	(0.036)	(0.029)	(0.031)	(0.063)	(0.083)
Female	-0.009	-0.028	-0.029	-0.049	-0.087	-0.119	0.003	-0.120
	(0.030)	(0.031)	(0.061)	(0.083)	(0.074)	(0.078)	(0.152)	(0.211)
Gap Year	0.078*	0.104**	0.061	0.085	0.489***	0.535***	0.333*	0.470**
	(0.042)	(0.043)	(0.069)	(0.095)	(0.100)	(0.105)	(0.179)	(0.238)
MESA Score	0.017***	-0.006	0.012	-0.005	0.072***	0.017	0.088***	-0.031
	(0.004)	(0.008)	(0.014)	(0.039)	(0.009)	(0.019)	(0.034)	(0.116)
Assigned Treatment×MESA Score		0.027***		0.021		0.067***		0.162
		(0.009)		(0.040)		(0.023)		(0.140)
Panel B	**First Stage**							
	Active Effective Treatment							
Assigned Treatment	0.135***	0.129***	0.115*	0.086**	0.155***	0.146***	0.159**	0.119**
	(0.034)	(0.025)	(0.061)	(0.043)	(0.038)	(0.029)	(0.069)	(0.049)
High School GPA	0.014	0.014	0.034***	0.035***	0.019**	0.019**	0.030**	0.031**
	(0.009)	(0.009)	(0.013)	(0.013)	(0.009)	(0.009)	(0.013)	(0.013)
Female	-0.034*	-0.034*	-0.079***	-0.080***	-0.027	-0.027	-0.080**	-0.081**
	(0.021)	(0.021)	(0.030)	(0.030)	(0.023)	(0.023)	(0.034)	(0.034)
Gap Year	0.057**	0.057**	0.088**	0.089**	0.054*	0.053*	0.081**	0.084**
	(0.024)	(0.024)	(0.034)	(0.035)	(0.028)	(0.028)	(0.039)	(0.040)
MESA Score	0.001	-0.001	-0.002	-0.018*	0.002	-0.001	0.006	-0.016
	(0.003)	(0.002)	(0.013)	(0.010)	(0.004)	(0.002)	(0.014)	(0.010)
Assigned Treatment×MESA Score		0.002		0.021		0.003		0.028
		(0.004)		(0.019)		(0.005)		(0.021)
Observations	897	897	339	339	757	757	295	295
R-squared	0.039	0.039	0.103	0.104	0.041	0.041	0.095	0.097
Program Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Sample	Full	Full	Restricted	Restricted	Full	Full	Restricted	Restricted
Effective F statistics†	16.05	25.79	3.608	3.965	16.33	25.25	5.296	5.848

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

†: Based on Olea and Pflueger (2013). Corresponding worst case bias for different levels of τ. τ= 5%: 37.42. τ=10%: 23.11. τ= 20%: 15.06

The estimations in Panel B show that being assigned to treatment, i.e. having a MESA score at or below 17 correct answers, is across all functional form specifications a significant predictor of Active Effective Treatment at, at least, a 5% significance level. Importantly, however, the effective F-statistics indicate that the estimates might be subject to a weak instrument bias that exceeds 20% of the worst-case bias in all estimations apart from the ones with local linear fits in the Full Samples. Consequently, we should be cautious of drawing any conclusions on Active Effective Treatment based on the estimations in Table 2.7, as they may be subject to a non-negligible weak instruments bias.

2.6.3 Sensitivity Checks

To test the sensitivity of our results to the choices of having a binary treatment indicator, considering specific functional forms, and a particular bandwidth of MESA scores in the Restricted Sample, we also estimated fuzzy RDD based on alternative choices. The results are displayed in Appendix Table 2.3, 2.4, and 2.5, respectively. Appendix Table 2.4 displays the estimation using polynomials that are not allowed to vary on either side of the cutoff for treatment assignment and generally have higher effective F-statistics than Table 2.6. However, neither this nor the two other appendix tables indicate significant treatment effects. We therefore do not consider the choices used in the main specification to be the reason why we do not find a significant effect of the online remedial module on students' performances in their microeconomics exams.

With respect to our identification strategy, we might worry if the outcome variables are continuous in the running variable, i.e. about the validity of assumption 1) described in Section 2.4.1. In most cases, we do consider it highly unlikely that any other important things that could potentially affect the final exam grade in microeconomics also changes for students who have a MESA score around 17, as compared to those who just barely do not. However, one potential exception would be if the compliant group of students were also more likely to have participated in the face-to-face course. Since the compliers self-selected into Effective Treatment it would not be surprising if they also self-selected into the face-to-face remedial offer. By using the activity data from the online platform used at the face-to-face course we can see that while students who were effectively treated on average spent two hours and 44 minutes on the platform, the remaining students spent only two hours and 23 minutes logged on to this online platform. Though the difference between these two groups is only twenty minutes, a simple t-test does indicate that it is statistically significant and therefore that our estimations might not only capture the effect of online remediation but of CBS's full remedial offer as a whole.

A related point is that is fairly common among first-year CBS students to buy and participate in private exam prep courses, which could also affect our results if it meant that students substituted or complimented the online module with such offers. Unfortunately we have no data allowing us to investigate this hypothesis.

In general, the analyses presented in this section have highlighted the importance of how the partial compliance among students assigned to treatment affects our ability to estimate the effect of the online remedial math module. At the same time, it is worth noting that the effective F-statistics only indicate dire problems of weak instrument bias due to low compliance when we attempt to estimate the effect of Active Effective Treatment.

2.7 Discussion

With the absence of a significant relationship between our online module and students' performance in their microeconomics exam, we corroborate the findings of the majority of studies on the effectiveness of remedial courses for student outcomes in higher education.

There are several potential explanations why we do not find any treatment effects. Firstly, we might not find any significant detectable effect because our identification strategy fails to deliver unbiased estimates of the effect of the online module. As argued in Section 2.5 and 2.6, the lack of any detectable treatment effect is arguably related to the partial compliance among those assigned to treatment. Though our main specification did not indicate invalidating problems with weak identification, it is not too surprising that we do not find any significant effects, given that only the minority of assigned students engaged with the course content and even fewer actively so. This suggests that the self-paced nature of the online module and associated responsibility for taking control of one's own learning might have been too big of a task for the newly-enrolled students.

Besides affecting our ability to identify any effect of the online remedial math course the limited compliance also points back towards the design of the course, as one could argue that this issue might be a consequence of not properly accounting for students' preferences and incentives in the course's structure and setup. The question of compliance is therefore a subject worth investigating before we offer guidance to educational decision makers on whether to introduce the remedial course to future cohorts of students.

Based on her findings, Duchini (2017) emphasize that any remedial participation strategies using "the stick" as a motivational factor has to rely on credible threats if it is to have any effect on student outcomes. In a Danish educational context, there is a limited scope for both forcing students to participate in the course, as well as for offering them any "carrots" in terms of for example extra credits or points for the final exam.

Consequently, initiatives aimed at incentivizing the students to participate in the online module appear to have to rely on optimizing information and communication. This emphasizes the need for adjusting the course, and perhaps more importantly the communication to the students about the course, in order to incentivize them to engage with the online module, if we want to see it significantly benefit their academic outcomes.

In terms of informing such initiatives, our existing quantitative data are unfortunately of limited use. Instead, an exploration of the possibilities of collecting and analysing more qualitative insights from students, e.g. in the form of individual or focus groups interviews with first-year students, might provide a path forward for future research.

If deciding to undertake such task of gathering qualitative data, it would be a good idea to keep in mind the insights emerging from Table 2.5 on who used the course. Namely, that there appears to be a low use of the course among academically weak students, as measured by high school GPA, and therefore an important scope from a policy maker perspective for increasing compliance among this group of students.

In the study by De Paola and Scoppa (2014) highlighted in Section 2.2 for being one of the only studies finding sizeable and significant effects of remediation, the authors also did not find an effect on students' grades, but only on the number of credits obtained after two years of studying and on their probability of dropping out. Perhaps any potential effects of our remedial offer are similarly longer termed than what we are able to evaluate at present.

It could also be the case that the remedial module involves both positive and negative effects that cancels each other out, e.g. due to the course increasing the academic confidence of some students while decreasing that of others, such that the treatment effect on average is indistinguishable from zero.

Martorell and McFarlin (2011) showed that math remediation generally had lower effects on student outcomes than those focusing on language skills but also base their finding on a more heterogeneous student population compared to our relatively homogeneous group of business school students for whom we expect math skills to be essential for their microeconomics exam performance. We therefore do not consider the overall subject of our remedial course to have lower potential for increasing the outcomes of our specific student population than others.

The absence of a treatment effect could be because the fuzzy RDD only allows for credible estimation of treatment effects for students who are close to the threshold for assignment to treatment. It is easy to imagine that students who are further away from the cutoff are the ones who have the largest potential gains from remediation, which our empirical setup unfortunately does not allow us to credibly identify. Consequently, we could end up with the finding of no significant effect of remediation in our setting if the students in the proximity of the cutoff were not or only modestly affected by engaging with the online module, even if students with poor initial MESA test scores had great learning gains.

Lastly, it might of course be the case that the pedagogical content of the course did not help the students increase their math skills, in which case it would be naïve to expect that it would have a positive effect on their outcomes in the microeconomics exam.

Neither of the potential explanations for the absence of any significant effect of the online remedial course exclude one another, it is, however, based on the data presently available not possible to assess which of them are more or less likely to be correct.

2.8 Conclusion

In this paper, we suggested offering a cost-effective and flexible online remedial math course to help underprepared students in a Danish business school increase their academic outcomes. We evaluated the course's local average treatment effect within a fuzzy RDD setup but found no significant effect on neither the students' microeconomics exam grades or probability of passing the exam. The absence of any effect of the course is similar to the findings of most empirical studies of remediation. We discuss several potential factors that might explain why we do not find any significant effect and highlight partial compliance among students assigned to treatment as particularly relevant. This makes us emphasize the importance of properly incentivizing students to use and engage with the course. Something that might be more relevant in a self-paced online module such as ours. The extend of partial compliance also points towards the relevance of investigating the issue further and perhaps redesigning the online module to more closely align with student's preferences and needs. This is an important task, if we are to be able to provide solid advice to educational decision makers on the potential of this type of remediation to increase students' math skills. Because most remedial offers are voluntary this is an interesting path for future research in general that has the potential to add important knowledge to the literature on remediation.

Though we cannot conclude anything about causality based on the OLS estimates of intensity of treatment, it is interesting that we observe significant and positive correlations between effective treatment and the students' performances in their microeconomics exams in the compliant sub-group. This underscores our point about looking more closely into the question of who and how students use the course to gain insights on how to engage more of the non-compliant students.

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I Appendix Tables and Figures

Appendix Table 2.1. Descriptives by study program

	Exam Grade	Pass	MESA Total	Effective Treatment	Active Effective Treatment	Assigned Treatment	Female	High School GPA	Gap Year
BSc ALMEN	5.95	0.75	12.80	0.28	0.11	0.82	0.32	8.81	0.85
BSc BLC	6.98	0.67	11.96	0.26	0.11	0.85	0.63	8.97	0.80
BSc FIL	6.19	0.83	12.18	0.33	0.09	0.88	0.58	8.54	0.97
BSc IB	8.08	0.88	16.27	0.15	0.06	0.58	0.35	10.57	0.78
BSc ISH	8.00	0.50	17.57	0.14	0.00	0.29	0.43	10.33	0.29
BSc IT	5.73	0.75	13.30	0.24	0.11	0.78	0.29	8.52	0.88
BSc KOM	4.88	0.92	10.59	0.19	0.09	0.97	0.75	9.04	0.93
BSc POL	9.30	1.00	18.16	0.32	0.12	0.60	0.52	11.09	0.75
BSc PRO	6.91	0.91	12.76	0.22	0.14	0.85	0.50	9.96	0.87
BSc PSY	.	0.00	12.35	0.19	0.05	0.86	0.79	9.59	0.90
BSc SOC	7.19	1.00	19.00	0.18	0.00	0.53	0.82	10.03	0.57
Total	6.25	0.75	12.96	0.24	0.10	0.82	0.46	9.18	0.86

Note: Descriptives for students who completed the MESA. Programs in bold font are exclusively taught in English.
BScPSY only receives binary pass/fail assessments in their first year of studies.

Appendix Table 2.2. MESA performance by main study program language

	Response rate	MESA total	MESA algrebra	MESA graphs	MESA calculus	Calculus (%)	Graphs(%)	Algebra (%)
Danish	0.64	12.76	6.36	3.82	2.48	20.66	38.24	42.38
English	0.36	14.82	8.06	3.80	2.79	23.26	38.02	53.75
Total	0.61	12.96	6.52	3.82	2.51	20.91	38.22	43.49

Appendix Table 2.3. Fuzzy RDD estimates of continuous effective treatment with online remedial math on student performance in microeconomics

Panel A	**Second Stage**							
	Pass				**Exam Grade**			
Continuous Effective Treatment	0.004	-0.003	0.000	-0.001	0.006	-0.009	0.004	-0.006
	(0.004)	(0.005)	(0.003)	(0.005)	(0.011)	(0.013)	(0.009)	(0.015)
High School GPA	0.037***	0.041***	0.021	0.024	0.146***	0.160***	0.158***	0.180***
	(0.010)	(0.010)	(0.016)	(0.020)	(0.026)	(0.028)	(0.048)	(0.055)
Female	-0.010	-0.027	-0.030	-0.038	-0.086	-0.122	-0.027	-0.082
	(0.030)	(0.031)	(0.040)	(0.047)	(0.075)	(0.081)	(0.119)	(0.139)
Gap Year	0.079*	0.103**	0.063	0.075	0.485***	0.542***	0.356**	0.442**
	(0.042)	(0.043)	(0.052)	(0.063)	(0.104)	(0.107)	(0.152)	(0.182)
MESA Score	0.016***	-0.006	0.011	-0.002	0.071***	0.018	0.080***	-0.024
	(0.004)	(0.008)	(0.008)	(0.030)	(0.008)	(0.019)	(0.024)	(0.100)
Assigned Treatment×MESA Score		0.027***		0.022		0.067***		0.167
		(0.009)		(0.043)		(0.024)		(0.151)
Panel B	**First Stage**							
	Effective Treatment							
Assigned Treatment	10.290***	9.669***	19.691***	13.889***	12.177***	11.299***	23.158***	16.420***
	(3.437)	(2.435)	(6.175)	(4.091)	(4.029)	(2.892)	(7.201)	(4.861)
High School GPA	0.494	0.493	1.620**	1.746**	0.896	0.896	1.601**	1.763**
	(0.868)	(0.868)	(0.741)	(0.755)	(0.908)	(0.908)	(0.802)	(0.817)
Female	-2.376	-2.376	-4.481	-4.651	-2.382	-2.380	-4.560	-4.776
	(1.928)	(1.929)	(3.000)	(3.011)	(2.224)	(2.227)	(3.416)	(3.429)
Gap Year	4.191***	4.134***	6.415**	6.751**	4.929***	4.844***	6.413**	6.958**
	(1.401)	(1.364)	(2.608)	(2.675)	(1.585)	(1.551)	(2.844)	(2.934)
MESA Score	0.110	-0.080	2.235*	-1.081*	0.212	-0.057	2.742**	-0.961
	(0.324)	(0.145)	(1.169)	(0.645)	(0.385)	(0.152)	(1.331)	(0.703)
Assigned Treatment×MESA Score		0.223		4.153**		0.318		4.681**
		(0.415)		(1.778)		(0.476)		(1.961)
Observations	897	897	339	339	757	757	295	295
R-squared	0.025	0.025	0.083	0.090	0.026	0.027	0.084	0.092
Program Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Effective F statistics†	8.965	15.77	10.17	11.53	9.134	15.27	10.34	11.41

Note: Controls for study program fixed effects. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

†: Based on Olea and Pflueger (2013). Corresponding worst case bias for different levels of τ. τ= 5%: 37.42. τ=10%: 23.11. τ= 20%: 15.06

Appendix Table 2.4. Fuzzy RDD estimates of effective treatment with online remedial math on student performance in microeconomics with alternative functional forms

Panel A	**Second Stage**							
	Pass				**Exam Grade**			
Effective Treatment	-0.039	-0.085	-0.045	0.056	-0.084	-0.496	-0.103	1.034
	(0.129)	(0.179)	(0.198)	(0.365)	(0.410)	(0.561)	(0.660)	(1.364)
High School GPA	0.040***	0.041***	0.023	0.020	0.152***	0.161***	0.171***	0.149***
	(0.010)	(0.010)	(0.017)	(0.019)	(0.025)	(0.027)	(0.049)	(0.056)
Female	-0.020	-0.024	-0.035	-0.027	-0.099	-0.112	-0.056	0.006
	(0.028)	(0.028)	(0.041)	(0.046)	(0.071)	(0.073)	(0.120)	(0.136)
Gap Year	0.093**	0.095**	0.071	0.060	0.508***	0.536***	0.414**	0.264
	(0.039)	(0.040)	(0.053)	(0.064)	(0.093)	(0.101)	(0.164)	(0.226)
MESA Score	0.010**	-0.014	0.008	0.042	0.060***	-0.073	0.065	0.388
	(0.004)	(0.021)	(0.013)	(0.111)	(0.010)	(0.061)	(0.040)	(0.426)
MESA Score^2	-0.001**	0.001	-0.002	-0.009	-0.001**	0.007*	-0.013	-0.079
	(0.000)	(0.001)	(0.003)	(0.021)	(0.001)	(0.004)	(0.013)	(0.083)
Assigned Treatment×MESA Score		0.044*		-0.035		0.220***		-0.187
		(0.026)		(0.139)		(0.077)		(0.504)
Assigned Treatment×MESA Score^2		0.000		0.007		-0.002		0.108
		(0.002)		(0.021)		(0.004)		(0.092)
Panel B	**First Stage**							
	Effective Treatment							
Asigned Treatment	0.326***	0.348***	0.335***	0.399***	0.331***	0.363***	0.353***	0.388***
	(0.041)	(0.053)	(0.073)	(0.097)	(0.045)	(0.058)	(0.079)	(0.106)
High School GPA	0.012	0.012	0.028	0.028	0.014	0.014	0.021	0.021
	(0.011)	(0.011)	(0.018)	(0.018)	(0.011)	(0.011)	(0.018)	(0.018)
Female	-0.031	-0.033	-0.074	-0.075	-0.012	-0.013	-0.055	-0.056
	(0.031)	(0.031)	(0.046)	(0.046)	(0.033)	(0.033)	(0.049)	(0.049)
Gap Year	0.047	0.046	0.101*	0.097*	0.068*	0.066	0.116**	0.114**
	(0.039)	(0.039)	(0.055)	(0.055)	(0.041)	(0.041)	(0.057)	(0.058)
MESA Score	0.000	-0.003	0.007	0.049	0.001	0.000	0.012	0.037
	(0.004)	(0.008)	(0.015)	(0.058)	(0.004)	(0.008)	(0.016)	(0.064)
MESA Score^2	0.000	-0.000	-0.004	-0.015	0.000	-0.000	-0.006**	-0.012
	(0.000)	(0.001)	(0.003)	(0.012)	(0.000)	(0.001)	(0.003)	(0.013)
Assigned Treatment×MESA Score		0.020		0.013		0.020		-0.002
		(0.020)		(0.101)		(0.022)		(0.110)
Assigned Treatment×MESA Score^2		0.001		0.023		0.002		0.011
		(0.002)		(0.022)		(0.002)		(0.023)
Observations	897	897	339	339	757	757	295	295
R-squared	0.091	0.092	0.168	0.170	0.097	0.099	0.177	0.178
Program Fixed Effects†	YES	YES	YES	YES	YES	YES	YES	YES
Effective F statistics	63.49	43.10	21.04	17.03	54.64	39.37	20.11	13.27

Note: Controls for study program fixed effects. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

†: Based on Olea and Pflueger (2013). Corresponding worst case bias for different levels of τ. τ= 5%: 37.42. τ=10%: 23.11. τ= 20%: 15.06

Appendix Table 2.5. Fuzzy RDD estimates of effective treatment with online remedial math on student performance in microeconomics with alternative bandwidths

Panel A	**Second Stage**					
	Pass			**Exam Grade**		
Bandwidth	15-19	13-21	11-23	15-19	13-21	11-23
Effective Treatment	0.050	-0.063	-0.052	0.863	-0.207	-0.610
	(0.354)	(0.225)	(0.174)	(1.301)	(0.752)	(0.604)
High School GPA	-0.000	0.018	0.030**	0.068	0.144***	0.165***
	(0.021)	(0.017)	(0.013)	(0.071)	(0.045)	(0.038)
Female	-0.053	-0.039	-0.048	-0.289**	-0.081	-0.164
	(0.051)	(0.043)	(0.032)	(0.145)	(0.128)	(0.101)
MESA Score	0.018	-0.002	-0.008	0.124	-0.011	-0.033
	(0.066)	(0.029)	(0.015)	(0.266)	(0.102)	(0.056)
Assigned Treatment×MESA Score	0.008	0.016	0.030*	0.143	0.127	0.108*
	(0.073)	(0.034)	(0.018)	(0.278)	(0.129)	(0.065)
Panel B	**First Stage**					
	Effective Treatment					
Asigned Treatment	0.307***	0.311***	0.315***	0.317***	0.342***	0.338***
	(0.092)	(0.065)	(0.051)	(0.104)	(0.071)	(0.055)
High School GPA	0.029	0.021	0.022	0.029	0.013	0.020
	(0.022)	(0.017)	(0.013)	(0.022)	(0.017)	(0.013)
Female	-0.060	-0.078*	-0.048	-0.040	-0.061	-0.044
	(0.065)	(0.046)	(0.038)	(0.074)	(0.049)	(0.041)
MESA Score	-0.009	-0.021	-0.011	-0.007	-0.018	-0.006
	(0.034)	(0.013)	(0.007)	(0.041)	(0.013)	(0.006)
Assigned Treatment×MESA Score	0.012	0.044*	0.017	-0.002	0.053*	0.017
	(0.065)	(0.026)	(0.014)	(0.074)	(0.027)	(0.015)
Observations	191	339	519	161	295	442
Bandwidth	[15, 19]	[15, 19]	[13, 21]	[13, 21]	[13, 23]	[13, 23]
Program Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Effective F statistics†	11.04	22.51	38.20	9.351	23.04	37.40

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †: Based on Olea and Pflueger (2013). Corresponding worst case bias for different levels of τ. τ= 5%: 37.42. τ=10%: 23.11. τ= 20%: 15.06. The bandwidth [13, 21] is the one used in the main analyses.

Appendix Table 2.6. Fuzzy RDD estimates of effective treatment with online remedial math on student performance in microeconomics with alternative bandwidths and triangular kernel weights

Panel A	**Second Stage**					
	Pass			**Exam Grade**		
Bandwidth	2-33	7-27	12-22	2-33	7-27	12-22
Effective Treatment	-0.089	-0.055	-0.048	-0.418	-0.463	-0.006
	(0.143)	(0.161)	(0.233)	(0.473)	(0.538)	(0.794)
High School GPA	0.037***	0.031***	0.015	0.165***	0.175***	0.159***
	(0.010)	(0.011)	(0.016)	(0.028)	(0.034)	(0.054)
Female	-0.028	-0.037	-0.051	-0.114	-0.131	-0.143
	(0.027)	(0.029)	(0.041)	(0.076)	(0.087)	(0.118)
Gap Year	0.084**	0.074*	0.060	0.492***	0.473***	0.380*
	(0.037)	(0.041)	(0.062)	(0.102)	(0.120)	(0.195)
MESA Score	-0.008	-0.007	-0.000	-0.005	-0.026	0.015
	(0.009)	(0.013)	(0.031)	(0.027)	(0.041)	(0.108)
Assigned Treatment×MESA Score	0.031***	0.032**	0.018	0.093***	0.123***	0.120
	(0.010)	(0.014)	(0.039)	(0.030)	(0.046)	(0.137)
Panel B	**First Stage**					
	Effective Treatment					
Asigned Treatment	0.318***	0.311***	0.315***	0.325***	0.324***	0.319***
	(0.039)	(0.045)	(0.070)	(0.043)	(0.049)	(0.075)
High School GPA	0.015	0.022*	0.030	0.016	0.021*	0.032*
	(0.011)	(0.012)	(0.019)	(0.011)	(0.012)	(0.019)
Female	-0.037	-0.044	-0.055	-0.021	-0.030	-0.039
	(0.031)	(0.035)	(0.052)	(0.034)	(0.038)	(0.056)
Gap Year	0.056	0.072*	0.132**	0.071*	0.083*	0.157**
	(0.039)	(0.043)	(0.062)	(0.041)	(0.045)	(0.062)
MESA Score	-0.004	-0.010*	-0.019	-0.002	-0.008	-0.023
	(0.003)	(0.005)	(0.015)	(0.004)	(0.005)	(0.016)
Assigned Treatment ×MESA Score	0.007	0.016*	0.050*	0.005	0.016	0.058*
	(0.007)	(0.010)	(0.030)	(0.007)	(0.011)	(0.032)
Observations	896	765	339	756	651	295
R-squared	0.109	0.131	0.180	0.115	0.138	0.193
Program Fixed Effects	YES	YES	YES	YES	YES	YES
Effective F statistics†	41	24.59	11.47	35.84	22.58	9.921

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †: Based on Olea and Pflueger (2013). Corresponding worst case bias for different levels of τ. τ= 5%: 37.42. τ=10%: 23.11. τ= 20%: 15.06. Triangular kernel weights calculated as $w = max(0, 1 - |\frac{MESA-17}{bw}|)$, where bw is the bandwidth.

Chapter 3

Mette Suder Franck

The Effect of COVID-19 on Student Outcomes

An Empirical Investigation of the Importance of Parental and Student Characteristics

CHAPTER 3

The Effect of COVID-19 on Student Outcomes: An Empirical Investigation of the Importance of Parental and Student Characteristics

Abstract

The COVID-19 pandemic had extensive effects on students on all educational levels. Previous literature has indicated that these effects were unequally distributed between students with different socioeconomic backgrounds (Agostinelli et al., 2022; Hansen et al., 2021). This has been argued to relate to differences in how the pandemic affected the economic and health outcomes across socioeconomic groups, which suggests that social welfare policies might play an important role for moderating the effect of COVID-19 on educational outcomes (Aucejo et al., 2020). The present paper adds to the knowledge of the differential effect of COVID-19 on student outcomes in higher education by investigating how the pandemic affected students with different socioeconomic backgrounds in a generous welfare state. The empirical investigation is based on an estimation strategy inspired by Difference-in-Differences and a dataset combining university-level administrative data sources with national-level register data. I find suggestive evidence indicating that, even within this context, students with low-income parents and students who themselves had a low income, were comparatively worse off during the pandemic than their more affluent peers. To be more precise, in the first wave of the pandemic in spring 2020 students with low-income parents passed signif-

icantly fewer exams compared to their fellow students with high-income parents, while low-income students gained fewer ECTS relative to their high-income peers. More research is needed to empirically assess the mechanisms underlying these differential effects, if we are to inform policy makers on how to mitigate potential unequal effects of future events that might cause similar disruptions to the educational system.

3.1 Introduction

The COVID-19 pandemic affected the lives of billions of people around the globe. The effects were multifaceted and ranged from affecting economic outcomes as employment and spending (Chetty et al., 2020), over mental health indicators as well-being and life-satisfaction (Schmidtke et al., 2021), to student outcomes in primary (Hanushek and Woessmann, 2020), secondary (Agostinelli et al., 2022), and tertiary education (Orlov, McKee, Berry, et al., 2021).

Already during the first wave of the pandemic, researchers voiced a concern that the effect on student outcomes would likely be unequally distributed with the more vulnerable students - in terms of socioeconomic background such as parental income and education - being more adversely affected than their peers (Kuhfeld et al., 2020; Aucejo et al., 2020). If this is the case, we might very well expect to observe effects on inequality in the future since educational attainments are important determinants of labor market outcomes.

Despite the fact that only a limited amount of time has elapsed since the onset of the COVID-19 pandemic, there has already been produced an impressive number of studies investigating its potential differential effects by parental and student characteristics on a range of educational outcomes. Of these, many focus on the effects in lower levels of education, where some researchers have argued that the pandemic had greater negative effects on the outcomes of students with poorer socioeconomic backgrounds (Agostinelli et al., 2022). The notion of a significant gradient in the effect of the pandemic does not appear to be universal across educational settings. For example, a recent study by Birkelund and Karlson (2022) find no evidence of a widening of learning gaps among Danish primary school students during a period where teaching was conducted online due to a national lockdown.

Several studies have directed attention towards assessing whether the pandemic had a differential effect on students from disadvantaged backgrounds in higher education. Shortly after the conclusion of the spring 2020 semester, Aucejo et al. (2020) used survey data from a large public American university to empirically assess potential heterogeneity of the impact of the first wave of COVID-19. Their findings indicate notable differences in student outcomes according to parental characteristics. More specifically, the authors report that students with parents whose income was below the median were 55% more likely to have delayed graduation than students with parents who had an income above the median and that first-generation students were 50% more likely to delay graduation. Hansen et al. (2021) similarly find a positive social gradient for students who had at least one parent with a college degree, as these performed better in a microeconomics course that was affected by the pandemic compared to students whose parents did not have a college degree.

Empirical studies has, however, far from unambiguously confirmed this finding of a differential negative effect of COVID-19 on the academic outcomes in higher education of students with disadvantaged backgrounds. Orlov, McKee, Berry, et al. (2021) find that although students on average performed worse during the pandemic, this did not seem to be driven by any specific type of students. Rodríguez-Planas (2022a) reports that low-income students actually achieved a significantly higher GPA and dropped less credits during the first wave of the pandemic than their high-income peers, though this effect seemed to be explained by students' differential use of a flexible grading option.

Perhaps the varying findings on COVID-19 related heterogeneity in student outcomes in higher education is due to the fact that they assess the effect in very different educational settings: Public universities (Aucejo et al., 2020; Rodríguez-Planas, 2022a), R1 institutions (Orlov, McKee, Berry, et al., 2021) and German higher education in which tuition is free (Hansen et al., 2021).

Rodríguez-Planas (2022a) notes that the behaviour and outcomes of low-income students during the pandemic was affected by worries associated with losing financial aid. Aucejo et al. (2020) argue that their finding of a significant differential negative effect of COVID-19 between students with low- and high-income parents was related to differences in how the pandemic affected the economic outcomes and health across these groups of students. Together this suggests that the features of the educational settings in particular and of welfare policies in general, would play a role for whether we observe heterogeneity in the outcomes of students with different socioeconomic backgrounds. This is in line with a strand of the literature on intergenerational mobility that highlights the importance of societal characteristics for the strength of transmission of social inheritance between parents and children (e.g. Corak, 2013; Esping-Andersen and Wagner, 2012; Harding and Munk, 2020). Such societal characteristics could potentially be even more important for whether student outcomes are differentially affected by a crisis such as the COVID-19 pandemic.

With this study, I complement the research on the effect of COVID-19 on student outcomes by empirically investigating if the pandemic appeared to unequally affect students with certain backgrounds in an educational system characterized by a number of features that explicitly aims at reducing inequality and promoting mobility (Harding and Munk, 2020). In these efforts, I focus on two main research questions that are both motivated by the concern formulated in previous research that students with disadvantaged backgrounds might have been disproportionately affected by the pandemic. The first research question is related to intergenerational mobility and assesses if there was a *differential* effect of the pandemic on student outcomes in higher education according to parental characteristics. More specifically, I investigate heterogeneity among first generation students and students with low-income parents compared to their more affluent peers. The second research question is similarly concerned with examining differential effects but focuses on students' own characteristics, namely their pre-pandemic income.

To assess the effect of the COVID-19 pandemic on student outcomes, I consider the specific context of Copenhagen Business School (CBS), which is one of the largest institutions of higher education in Denmark. In general, international comparisons indicate that Denmark is characterized by having high social mobility and low inequality when evaluated with respect to parental income (OECD, 2018). This implies that the effect of parental income on their children's educational outcomes should be relatively limited and has popularly been attributed to the generous welfare system (e.g. by Sanders, 2013). Given the extensive Danish welfare policy scheme, I would expect to find no or only modest differential effects of the pandemic between students whose parents differ in terms of education and income levels. If the welfare mechanisms indeed succeed at weakening the link between parents' backgrounds and student outcomes, we might be more likely to see heterogeneity according to students' own income. Many, if not the vast majority of, Danish students in higher education fully support themselves while studying, and are in most cases able to do so by supplementing the government funded study grant by working a part-time job. Therefore, any pandemic induced changes in students' job situations might be a significant financial stressor that can lead to differential student outcomes if the economic shock is unequally distributed between students. On the other hand, employment shocks might have improved student outcomes by decreasing the outside option for studying.

Contrary to previous research, I have the possibility to assess the question at different stages of the pandemic. Although I mainly focus on the effect of the first wave of the pandemic in spring 2020, I also consider student outcomes at longer horizons. Therefore, I add to the literature on student outcomes during COVID-19 with knowledge on the effect of the pandemic on long-term student outcomes. Moreover, the study also contributes to the more general strand of literature concerned with assessing the effect of crises on student outcomes, as e.g. investigated in Sacerdote (2012) and Brück et al. (2019).

I use a Difference-in-Differences inspired estimation strategy to analyze a rich dataset combining a number of administrative data sources from Copenhagen Business School with national-level register data. The analyses suggest that the COVID-19 pandemic had a negative differential effect on the pass rate of bachelor students with low-income parents compared to those whose parents had a high income. In addition, students who themselves had a low income passed significantly fewer ECTS in spring 2020 compared to their more affluent peers. Though the finding of a social gradient in the effect of the pandemic on student outcomes within the context of an extensive welfare may be a little surprising, it did not appear to carry over into any differences in students' propensities to graduate their bachelor studies within the three-year norm period.

The paper proceeds as follows. First, Section 3.2 outlines the institutional setting, data, and descriptive statistics of key variables. The empirical strategy is then explained in Section 3.3, before Section 3.4 describes the average effect of the pandemic on student outcomes. The analyses of heterogeneity in the effect of COVID-19 on the short- and long-term student outcomes according to parental and student characteristics are presented in Section 3.5 and Section 3.6, respectively. Finally, Section 3.7 discusses the results before 3.8 summarizes the paper's main findings.

3.2 Setting and Data Description

The Danish welfare system ensures that tuition in higher education is free and that students receive a monthly government funded study grant, which in the spring of 2020 was approximately €840 before taxes[1]. Moreover, in addition to having some of the most comprehensive educational, health and child care, and tax policies in the world, unemployment and other social benefits in Denmark are also well above average in international comparisons (Harding and Munk, 2020). The social welfare policies implemented in Denmark during the COVID-19 pandemic were characteristically generous. Already during the first wave of the pandemic in spring 2020, the Danish government introduced a number of initiatives aimed at supporting businesses and their employees. In particular, in March 2020 they announced a wage compensation scheme that offered to partially fund the wages of private employees in sectors affected by the lockdown, to help employers avoid laying off employees.

Having now outlined the more general features of the Danish welfare scheme, I next turn towards describing the specific educational setting considered in the present study, before presenting the dataset and descriptive statistics.

[1]The study grant is available for all Danish students and for foreign students meeting certain requirements. The rate referenced here was for students not living with their parents, which is true for the vast majority of students in higher education in Denmark.

3.2.1 Institutional Setting

CBS offers a range of study programs related to economics, social sciences, and business studies. In 2020, there was a total of 15,598 students at CBS of which 7,940 were enrolled in a three-year bachelor program.

Full-time students at CBS are expected to earn 30 ECTS points per semester. In the majority of the bachelor programs, the students will obtain this by enrolling in (and passing) four courses per semester and in most of these courses, a student's performance is assessed only once at a final exam. That being said, there is a considerable amount of courses in which the final exam grade is a weighted average of two or more exam activities. Although some exams are simply graded as either passed or failed, most are graded on the Danish grading scale where they, as all other exams in Denmark, are subject to absolute grading since relative grading is prohibited by law. Students who fail or choose not to complete an exam have the possibility to do a retake exam that is ideally scheduled to take place before the start of the next semester. In total, a student has three attempts to pass an exam. If a student is unable to do so, she is no longer allowed to continue her studies unless she is granted dispensation and gets (and passes) an additional attempt[2].

The effect of COVID-19 on study-related activities at CBS have changed over the course of the pandemic. It first affected the business school in March 2020, when the administration immediately following a press conference hosted by the Danish government and health authorities on the 11th of March 2020 closed down the campus for, what would later turn out to be, the remainder of the semester. Consequently, all study-related activities during the latter part of the spring semester was conducted exclusively online. This meant that students and teachers had to quickly adapt to online teaching and learning and prepare themselves for engaging with new exam formats.

Before the beginning of the fall 2020 semester, CBS decided that 50% of the teaching activities had to be conducted online and that class sizes had to be limited. Due to a rise in the number of infections towards the end of the semester most exam activities had to be done online, which once again meant that students and teachers in many cases had to adapt

[2]If a student is sick at the exam date and has an official certificate from their doctor to corroborate this, they do not use one of the three attempts by being absent at the exam.

to new exam formats, albeit to a lesser extent, as some teachers had prepared for this possibility. The rise in the number of official COVID-19 cases in late 2020 continued to affect CBS for the full duration of the spring 2021 semester, such that all activities were conducted exclusively online.

In my analyses of the effect of COVID-19 on student outcomes, I mainly focus on the spring 2020 semester because I am interested in the effect of the unexpected disruptions of student life that it involved. I do, however, also utilize that I have data on student outcomes up until the end of the spring 2021 semester to consider some longer term effects.

3.2.2 Data

For my empirical analyses, I use two primary data sources. Namely, administrative data from CBS and Danish register data. For all study related data on students, I rely on a combination of CBS's administrative records. The data comprise of overall enrollment data, such as enrollment and matriculation (or unenrollment) date, as well as detailed information on all of the exam grades the student has received while studying at CBS. In particular, the grading data includes the weight of each exam expressed in terms of ECTS points, the date that the exam was graded, whether it was partial or final, if it was an ordinary or a retake exam, and if the associated course was mandatory or not.

I use this information to delimit my sample, so that I only consider the study outcomes in mandatory courses for the cohorts of bachelor students who started their studies at CBS between 2015 and 2019[3]. Some students who drop out before completing their bachelor studies later gets re-enrolled in another bachelor program at CBS. Because the study outcomes of these students might be different from those of other students, e.g. due to credit transfers that potentially decrease their study burden in a given semester, I exclude them from my estimation sample.

[3]I consider students from all but two of the bachelor programs that existed throughout the full period considered. I exclude the students from the BSc in Business Administration and Psychology because they do not receive any numerical grades in their first year of studies and those enrolled in the BSc in International Shipping and Trade, as inspection of the data reveals that many of the fourth semester students were not enrolled in any courses at CBS in spring 2020.

I further limit my investigation to focus on the outcomes of 1^{st} to 4^{th} semester bachelor students and thus exclude observations from students' 5^{th} and 6^{th} semesters. I impose this restriction, because the majority of students in pre-pandemic years chose to spend their 5^{th} semester abroad, which, during the first waves of the pandemic, was unfortunately not an option for the students. Because only the (self-)selected sample of students from the pre-pandemic period who did not go abroad have a CBS study transcript of their 5^{th} semester studies, they constitute a poor counterfactual for the outcomes of the full cohort of students, who had to spend their 5^{th} semester at CBS due to the pandemic.

The decision to exclude the 6^{th} semester students was made because bachelor students in their final semester spend half their study time writing a bachelor project that does not involve any teaching. Therefore, students who were on their 6^{th} semester in spring 2020 or spring 2021 arguably experienced a very different disruption to their studies than the majority of the bachelor students. Similarly, I suspect that the students' performance in internships are distinct from those in regular exams. I have information from CBS's online course catalogue on course titles, which allows me to identify - and subsequently exclude - these from my estimation sample.

For information on students' socioeconomic backgrounds, I rely on Danish register data. The first of my research questions focuses on parental income and I therefore drop the students that does not have information on the income of at least one of their parents. This leads to an exclusion of virtually all foreign students from the sample. After imposing this and the other restrictions outlined above, I end up with my main estimation sample that consists of an unbalanced panel dataset of 8,091 unique students[4].

3.2.3 Outcome Measures

To investigate the extent to which the COVID-19 pandemic had differential effects on student outcomes according to parental characteristics, I consider several student outcomes. In the analyses of the short-term

[4]For overview of sample selection see Appendix Table 3.12 and for number of students by enrollment cohort see Appendix Table 3.13.

effects, I focus on outcomes that allows for comparing a students' pre- and post-pandemic performance: 1) grades, 2) pass rates, 3) number of ECTS points gained in ordinary exams, and 4) total number of ECTS gained. In all cases, I compute the semester-level average outcome of all partial and final exams that a student was signed up for in a given semester[5]. For 1) to 3) I exclude retake exams and only consider ordinary exams, while the latter also includes the students' outcomes in the retake exams. For the grade outcome, the average is based on all passing grades, weighted by the number of ECTS points, and standardized according to the pre-pandemic cohort mean and standard deviation. The choice to only include passing grades is based on the assumption that students will mainly care about the passing grades, as these are the ones that will be of greatest importance for their future academic opportunities, such as acceptance to certain competitive Master's degree programs and the most selective exchange study programs.

Apart from being a measure of the extent to which students succeeded at passing an exam, the pass rate might also reflect patterns of student behavior. On the one hand, it could be a proxy for student well-being if it expresses that a student feels unable to successfully complete an exam (for reasons that they are not able to get an official excuse not to attend), in which case they will be granted a failing grade. On the other hand, it can also reflect that students engage in a type of "strategical behavior" where they decide not to participate in an exam, because they worry not about being able to pass the exam but about not getting a good grade. As mentioned previously, because some of their future academic prospects hinges on their GPA, a student might choose to engage in such behavior and postpone some of the study burden to the retake exam period if they feel like this will increase their chances of receiving a better grade. Similarly, any difference between the two ECTS measures could indicate that students struggle to pass the ordinary exams or that they choose to postpone some of their study burden to the retake exams. However, contrary to the pass rate, the ECTS measures also reflect the "size" of the exams, in terms of ECTS, and so differences between the two measures could indicate that students prioritize smaller or larger exams in their

[5]Some of the study programs have a quarter structure. For students in these programs, I also consider semester-based measures.

ordinary or retake exams.

Because some study programs have a quarterly structure and just about all of them at least some partial exams during the semester, most students have exam observations in many months throughout the study year. This makes it difficult to correctly define exam observations as belonging to either the spring or fall semester. I define an exam as belonging to either the spring or fall semester if it was graded between March and August or September and February, respectively. Though I cannot rule out that this definition leads to some exams being incorrectly labeled, my knowledge of the exam structure at CBS leads me to be believe that it will be correct for the vast majority of exams.

A potential long term effect of COVID-19 on student outcomes could be on students' propensity to finish their bachelor studies within the three-year norm period. To get an indication of whether the pandemic had an effect on students' probability of delaying their studies, I construct a dummy that is equal to 1 if a student finished their studies on time, i.e. within the study norm of three years. For this analysis, I consider the 2015-2018 cohorts and exclude the most recent cohort for which I do not observe whether they finished their bachelor studies within three years in my data.

3.2.4 Income and Education Measures

For my parental income measure, I first calculate the sum of the parents' average personal income between 2015 and 2019 to reduce the influence of any big income fluctuations in a given year. The personal income reflects all types of income including government transfers but excluding wealth income. I use the joint five-year average income to create a set of dummy variables indicating whether the parents' income during the five-year period is low, average, or high relative to that of couples aged 40 to 65 years in the Danish population during the same period. I define the parental income of a student as being low, middle, or high if it is below the 25^{th} percentile, between the 25^{th} and the 75^{th}, or above the 75^{th} percentile, respectively[6].

[6] As a test of the sensitivity of the analyses to using these cutoffs, I have also considered an alternative definition of the parental income ranks based on the 10^{th} and 90^{th} percentile. The

The choice to define the parental income measure relative to a comparable Danish sub-population rather than to the distribution among the CBS students in the estimation sample, was made because the parental income of CBS students is considerably higher than among the Danish population in general. Therefore, a definition based on the relative parental income among CBS students would categorize some students as having low-income parents even though this would hardly be true in a comparison with the joint income of Danish couples. As a robustness check, I also consider defining the categories relative to the income distribution of the parents of the students in the estimation sample.

Given that many of the parents have a business background and are self-employed, their income might not fully reflect their financial situation. If this is true for many parents, wealth could perhaps provide a better indicator of parents' financial means and therefore also be more likely to involve a social gradient in student outcomes during the pandemic. To test whether this appears to be the case, I also investigate if the pandemic had a differential effect according to parental wealth. In this analysis, I once again rely on a relative measure of the rank of parents' joint wealth compared to the Danish population aged 40-65. Because wealth is less volatile than income, I consider the rank of parental wealth in only one year, namely 2018, which is the most recent year for which I have information of wealth.

With respect to parental education, I create a binary indicator for whether one or both parents have completed a higher education. I base this measure on the International Standard Classification of Education (ISCED), where the levels from 5 and up are classified as higher education. In a Danish context, this includes university degrees and degrees from university colleges that, among other professions, educate nurses and primary school teachers and which have a structure that is different from that in universities. To assess if there was a differential effect of COVID-19 on students of parents with and without specific institutional knowledge of educations similar to their children, I further define a more granular binary educational measure that indicates whether one or both parents completed a university-level education[7].

results based on these alternative cutoffs are largely consistent with those using the 25$^{\text{th}}$ and the 75$^{\text{th}}$ percentile and are available upon request.

[7] I also considered an educational measure indicating if a parent completed a business education.

The Danish register data also provides me with information on students' own personal income that I use to construct a measure of student income ranks. These are defined analogously to those for the main measure of parental income category, i.e. as the ranks relative to a comparable sub-sample of the Danish population. Contrary to the measure for parental income, I only use the students' personal income from one year and not a five-year average, so that the measure is based on a period during which the students were enrolled at CBS as full-time students. For each cohort of students, I use the personal income in the year following the enrollment and define the students' rank relative to the Danish population of 20-25-year-olds[8]. To be more specific, this means that when e.g. considering the 2016 cohort, I measure their income by their personal income in 2017. Unfortunately, I only have income data up until 2019. For the latest cohort in the estimation sample, 2019, I therefore use the most recent measure of their personal income, i.e. their income in 2019.

Lastly, I also the use the registers to get detailed employment data for both students and parents. This includes data on monthly wages and work hours, as well as occupational industry up until June 2020. I use these data to get some insights on the employment of students during the first wave of COVID-19 in spring 2020.

3.2.5 Descriptive Statistics

Table 3.1 shows descriptive statistics for CBS students and their parents. The first three columns show means and standard deviations of student and parent characteristics by parental income rank, while the fourth shows the same statistics for the full estimation sample. The last three columns tests for balance of variables across the three different parental income groups.

For reference, among the Danish population who were between the ages of 40 and 65 in 2020, 36.4% had completed a college degree, while couples in the same age interval had a joint average yearly income over the five-year period from 2015-2019 of 870.8 (in 1000 DKK). The table shows that compared to this subsample of the Danish population, the parents of

The results based on this definition are consistent with the other definitions and are available upon request.

[8] Using the 25^{th} and the 75^{th} percentiles as cutoffs.

the students at CBS are more likely to have completed a degree in higher education and have a notably higher average income. This is indicated by both the mean income of the parents of all the students in the sample of 1312.84 (1000 DKK), as well as the number of observations in each of the three income rank categories. These show that the majority of the students (4,403 out of 8,091) had parents whose average yearly income between 2015 and 2019 placed them among the top 25% of the distribution of the comparable Danish sub-population.

Table 3.1. Descriptive statistics for students and parents by parental income rank

	Parental Income Rank				**Difference**		
	Low	Middle	High	All	Middle-Low	High-Low	High-Middle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Student variables							
High School GPA	8.55	8.90	9.02	8.92	0.35***	0.48***	0.12***
	(1.55)	(1.52)	(1.53)	(1.54)	[0.00]	[0.00]	[0.00]
Female	0.48	0.46	0.44	0.45	-0.02	-0.04**	-0.02*
	(0.50)	(0.50)	(0.50)	(0.4)	[0.31]	[0.01]	[0.07]
Age at Enrollment	23.27	23.09	22.67	22.89	-0.18	-0.59***	-0.41***
	(3.42)	(2.38)	(1.80)	(2.30)	[0.10]	[0.00]	[0.00]
Parents' mean income 2015-2019 (1000kr.)	368.80	811.52	1852.21	1312.84	442.71***	1483.40***	1040.69***
	(305.54)	(108.62)	(2003.46)	(1602.58)	[0.00]	[0.00]	[0.00]
Wages (1000kr.)	28.37	31.65	29.72	30.13	3.29**	1.35	-1.94**
	(19.41)	(21.33)	(18.80)	(19.70)	[0.01]	[0.24]	[0.03]
Work Hours	175.85	195.23	187.63	188.39	19.38**	11.78	-7.60
	(120.57)	(120.80)	(117.89)	(119.23)	[0.01]	[0.10]	[0.15]
Employed	0.81	0.90	0.90	0.89	0.09***	0.09***	0.00
	(0.39)	(0.31)	(0.30)	(0.32)	[0.00]	[0.00]	[0.73]
Lived with Parent(s) in Spring 2020	0.21	0.12	0.16	0.16	-0.09***	-0.05**	0.03**
	(0.41)	(0.33)	(0.36)	(0.36)	[0.00]	[0.01]	[0.01]
Lived alone in spring 2020	0.75	0.73	0.74	0.74	-0.02	-0.01	0.01
	(0.44)	(0.44)	(0.44)	(0.44)	[0.48]	[0.76]	[0.56]
Pre-pandemic outcome variables							
GPA	7.38	7.70	7.95	7.79	0.31***	0.57***	0.26***
	(1.85)	(1.77)	(1.76)	(1.79)	[0.00]	[0.00]	[0.00]
Pass Rate	0.83	0.87	0.89	0.88	0.05***	0.06***	0.02***
	(0.23)	(0.19)	(0.18)	(0.19)	[0.00]	[0.00]	[0.00]
ECTS in Ordinary Exams	24.03	25.69	26.06	26.65	1.67***	2.03***	0.37**
	(7.49)	(6.27)	(6.01)	(6.36)	[0.00]	[0.00]	[0.02]
ECTS in All Exams	26.33	27.47	27.76	27.46	1.14***	1.43***	0.30**
	(7.09)	(5.75)	(5.55)	(5.88)	[0.00]	[0.00]	[0.04]
Parental variables							
Mother Employed	0.52	0.83	0.89	0.81	0.31***	0.37***	0.06***
	(0.50)	(0.38)	(0.31)	(0.39)	[0.00]	[0.00]	[0.00]
Father Employed	0.30	0.80	0.87	0.77	0.50***	0.57***	0.07***
	(0.46)	(0.40)	(0.34)	(0.42)	[0.00]	[0.00]	[0.00]
Parent with College Degree	0.38	0.51	0.74	0.62	0.13***	0.36***	0.23***
	(0.49)	(0.50)	(0.44)	(0.49)	[0.00]	[0.00]	[0.00]
Mother with College Degree	0.32	0.41	0.58	0.49	0.09***	0.26***	0.16***
	(0.47)	(0.49)	(0.49)	(0.50)	[0.00]	[0.00]	[0.00]
Father with College Degree	0.21	0.28	0.57	0.44	0.07***	0.36***	0.29***
	(0.41)	(0.45)	(0.49)	(0.50)	[0.00]	[0.00]	[0.00]
Observations	1,188	2,500	4,403	8,091	3,688	5,591	6,903

Note: Mean values with SD in parentheses and p-values in squared parentheses. The last three columns displays balance tests across parental income ranks. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ indicate significance of difference in means. All employment variables refer to fall 2019 and the mean values are computed based on the sub-sample of students who were enrolled as 1^{st} to 4^{th} semester bachelor students at that time. Similarly, the indicators for living situation in spring 2020 only includes students from the 2018-2019 cohorts.

Table 3.1 further shows that the majority of students in the estimation sample are male and does not live with a parent, but that the share of female students and students living with at least one parent is higher among students with low-income parents. It also indicates that student performance is increasing in parental income rank. The tendency of a positive correlation applies both at the high school and at the university level.

The pattern of occupation in fall 2019 across the three different income groups, shows that the majority of students that were enrolled in a bachelor program at CBS in fall 2019 had a job and that employment was lowest among students with low-income parents and highest among those with middle- and high-income parents. For parental employment, the differences in employment are more pronounced than among the students. This may, however, be partly attributed to the fact that I only observe employment in Denmark, such that parents working abroad would be classified as unemployed.

The last three columns of Table 3.1 show that these patterns across parental income groups in most cases reflect significant differences in means. Especially when considering the differences between students with low-income parents and those with middle-or high-income parents.

Table 3.2 shows the same set of descriptive statistics as Table 3.1 but now by student income categories. The table indicates that the majority of students in the estimation sample, 5,785 out of 8,091, fall within the middle-income group.

The low-income students are more likely to be male and 27% points more likely to have lived with at least one parent during spring 2020 than students with a high income. Moreover, the comparison of parental income across the same two groups reveals that the parents of low-income students have a significantly higher average income. This might be because high-income parents are likely to have better possibilities of providing their children with financial support while studying, hereby reducing their need to work a part-time job and therefore also their personal income, which does not reflect private transfers.

Given the virtually universal nature of the government study grant, the extent of part-time work is expected to be the main source of differences in student incomes. This expectation is confirmed by the statistics in Table 3.2: Though 89% of all the students in the estimation sample was employed at one point during fall 2019, the employment rate varies quite

Table 3.2. Descriptive statistics for students and parents by student income rank

	Student Income Rank				**Difference**		
	Low	Middle	High	All	Middle-Low	High-Low	High-Middle
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Student variables							
High School GPA	8.96	8.93	8.76	8.92	-0.03	-0.20***	-0.17***
	(1.50)	(1.53)	(1.68)	(1.54)	[0.51]	[0.01]	[0.01]
Female	0.35	0.48	0.44	0.45	0.12***	0.08***	-0.04**
	(0.48)	(0.50)	(0.50)	(0.50)	[0.00]	[0.00]	[0.03]
Age at Enrollment	22.12	22.98	23.58	22.89	0.86***	1.46***	0.60***
	(2.49)	(2.08)	(2.97)	(2.30)	[0.00]	[0.00]	[0.00]
Parents' mean income 2015-2019 (1000kr.)	1363.84	1312.79	1221.50	1312.84	-51.05	-142.34**	-91.29*
	(1691.75)	(1627.72)	(1209.23)	(1602.58)	[0.30]	[0.02]	[0.05]
Wages (1000kr.)	20.53	29.22	45.98	30.13	8.69***	25.45***	16.76***
	(15.23)	(16.70)	(28.19)	(19.70)	[0.00]	[0.00]	[0.00]
Work Hours	134.83	185.97	262.80	188.39	51.14***	127.96***	76.82***
	(110.37)	(106.70)	(151.51)	(119.23)	[0.00]	[0.00]	[0.00]
Employed	0.59	0.97	0.98	0.89	0.38***	0.39***	0.01
	(0.49)	(0.16)	(0.13)	(0.32)	[0.00]	[0.00]	[0.32]
Lived with Parent(s) in Spring 2020	0.33	0.11	0.06	0.16	-0.22***	-0.27***	-0.05***
	(0.47)	(0.31)	(0.24)	(0.36)	[0.00]	[0.00]	[0.00]
Lived alone in spring 2020	0.76	0.73	0.73	0.74	-0.03	-0.03	-0.01
	(0.43)	(0.44)	(0.45)	(0.44)	[0.12]	[0.21]	[0.80]
Pre-pandemic outcome variables							
GPA	7.70	7.83	7.69	7.79	0.13**	-0.01	-0.14*
	(1.84)	(1.76)	(1.90)	(1.79)	[0.01]	[0.95]	[0.05]
Pass Rate	0.86	0.88	0.85	0.88	0.03***	-0.01	-0.04***
	(0.20)	(0.18)	(0.23)	(0.19)	[0.00]	[0.19]	[0.00]
ECTS in Ordinary Exams	25.17	25.94	24.48	25.65	0.76***	-0.69**	-1.45***
	(6.70)	(6.09)	(7.37)	(6.36)	[0.00]	[0.03]	[0.00]
ECTS in All Exams	27.19	27.70	26.26	27.46	0.51***	-0.94***	-1.45***
	(6.23)	(5.54)	(7.22)	(5.88)	[0.00]	[0.00]	[0.00]
Parental variables							
Mother Employed	0.76	0.83	0.83	0.82	0.07***	0.07***	-0.01
	(0.43)	(0.37)	(0.38)	(0.39)	[0.00]	[0.01]	[0.76]
Father Employed	0.74	0.77	0.77	0.77	0.03	0.03	-0.00
	(0.44)	(0.42)	(0.42)	(0.42)	[0.11]	[0.29]	[0.98]
Parent with College Degree	0.64	0.63	0.54	0.62	-0.02	-0.10***	-0.09***
	(0.48)	(0.48)	(0.50)	(0.49)	[0.27]	[0.00]	[0.00]
Mother with College Degree	0.49	0.50	0.41	0.49	0.02	-0.07***	-0.09***
	(0.50)	(0.50)	(0.49)	(0.50)	[0.30]	[0.00]	[0.00]
Father with College Degree	0.50	0.44	0.36	0.44	-0.06***	-0.14***	-0.09***
	(0.50)	(0.50)	(0.48)	(0.50)	[0.00]	[0.00]	[0.00]
Observations	1,482	5,785	824	8,091	7,267	2,306	6,609

Note: Mean values with SD in parentheses and p-values in squared parentheses. The last three columns displays balance tests across parental income ranks. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ indicate significance of difference in means. All employment variables refer to fall 2019 and the mean values are computed based on the sub-sample of students who were enrolled as 1st to 4th semester bachelor students at that time. Similarly, the indicators for living situation in spring 2020 only includes students from the 2018-2019 cohorts.

a bit over student income categories. While the employment rate among the middle- and high-income students were 97% and 98%, respectively, it was only 59% among the low-income students.

Due to the fundamentally limited time endowment, students' time spend working is likely to be inversely related to their study outcomes, because it decreases their time available for studying. However, if students who select into spending (relative many) hours working a part-time job, do so because they are more ambitious, motivated, or higher-skilled than their peers, they may need to apply a lower study effort to achieve good study outcomes. In that case, we might perhaps observe the opposite relationship between a student's income and study outcomes. Based on the pre-pandemic averages of the outcome variables across student income categories displayed in Table 3.2, both of these hypothesis might be plausible. For the two ECTS measures, Column (6) indicates that high-income students on average earned fewer ECTS than low-income students. Though this finding is consistent with the time constraint argument, the magnitude of the differences are for both measures limited and in neither case exceed one ECTS, i.e. $\frac{1}{30}$ of the semester norm. When instead considering the differences in the pass rate and GPA, the balance tests in the same column do not suggest any significant differences between low- and high-income students. This lends some support to the argument of self-selection into part-time work according to unobservables that is likely to also be positively correlated with student outcomes. Interestingly, low-income students have a significantly higher high school GPA, which might on the other hand speak against the validity of this hypothesis.

Both Table 3.1 and 3.2 suggested that there might be some dependency between the income of students and that of their parents. To investigate this relationship between parental and student income, Table 3.3 displays a type of intergenerational mobility matrix that shows the cross-tabulation of student and parental income rank. The table indicates that there is an overrepresentation of students with low-income parents that are low-income themselves. As a consequence, students in this parental income group are underrepresented in the middle-income category compared to students with middle- and high-income parents. With more than 52% of the low-income students having high-income parents the same conclusion of underrepresentation does not apply for this parental income category. Together with Table 3.2 this gives an impression of the low-income students as a heterogeneous group in terms of parental income that both contains students with relatively low- and high-income parents.

Given the insights from the descriptive statistics, in particular those in Table 3.1, it is perhaps not too surprising that we observe a social gradient in student achievements cf. Figure 3.1 that depicts the students' rank in the GPA distribution as a function of their parents' rank in the income distribution before the onset of he pandemic. The figure shows that despite controlling for high school GPA and parental education, there is a significant positive correlation between parental income and student

Table 3.3. Intergenerational mobility matrix

	Student income rank			
Parent income rank	**Low**	**Middle**	**High**	**Students**
Low	19.10%	16.88%	15.41%	1,188
Middle	28.48%	31.24%	32.89%	2,500
High	52.43%	55.32%	51.70%	4,403
Students	1,482	5,785	824	8,091

Note: Displays column percentages.

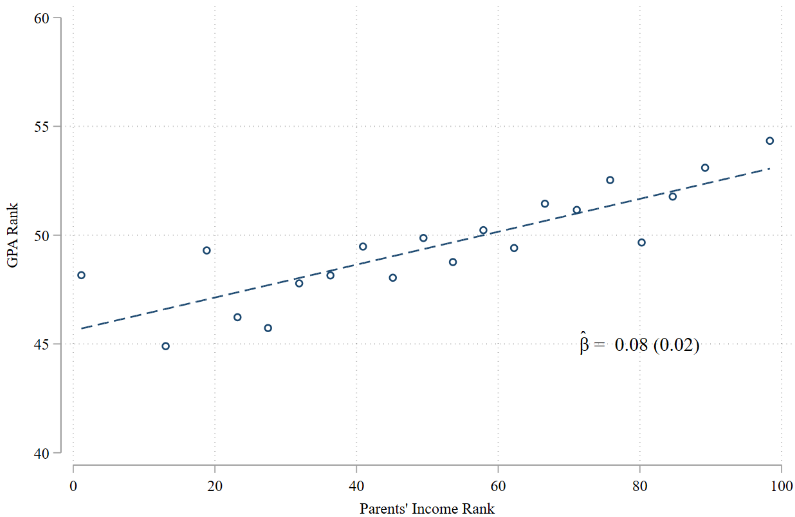
performance. This suggests that even in the context of Danish education where there are several policies aimed at reducing the relationship between parental income at their children's educational attainments, there is still a degree of dependency between the two.

3.3 Empirical Strategy

To estimate whether the COVID-19 pandemic involved any changes in the differences between students with different background characteristics, I follow Rodríguez-Planas (2022a) and use an empirical strategy inspired by Differences-in-Differences (DiD) estimation, in which I control for individual fixed effects. Intuitively, this means that I compare a student's outcomes in the spring of 2020 to their own performance in pre-pandemic semesters. Given that their own average outcomes in previous semesters are valid counterfactuals to those in the spring of 2020 if there had not been a pandemic, any deviations from the student's pre-pandemic performances can be attributed to the COVID-19 pandemic. I refer to the methodological approach as DiD *inspired* to draw attention to the fact that the present setting is conceptually different from the standard DiD setup, since there are no untreated observations as all students in the same cohort were affected by the pandemic. Therefore, I aim at estimating the *differential* effect of the pandemic between the students who were all affected by COVID-19 but who differed in terms of socioeconomic backgrounds.

Formally, I estimate the differential effect of the pandemic on outcome Y_{ist} for student i in year t and semester s according to the student characteristic of interest, C, by:

Figure 3.1. Social gradient in student GPA rank



Note: A student's GPA rank is defined as the rank of their cumulative pre-pandemic GPA in the fall 2019 GPA distribution among the bachelor students present in the estimation sample. Standard error in parenthesis. Parents' income rank is defined relative to the parents of the students in the same sample and based on their joint mean personal income in the period from 2015 to 2019. The estimation controls for high school GPA and parental education level.

$$Y_{ist} = \beta_0 + \beta_1 DS20_s + \sum_{c \neq b} \alpha_c (C_{ci} \times DS20_s) + \gamma Spring_s + \phi_i + \delta_t + \epsilon_{ist} \tag{3.1}$$

Y_{ist} can be either of the four outcomes described in Section 3.2 and the model in Equation (3.1) is estimated with OLS regardless of whether the outcome is continuous or bounded between 0 and 1 (pass rate). C either refers to parental income rank or education, or to students' own income rank, as outlined in Section 3.2. $DS20_s$ is a dummy for the spring 2020 semester and $Spring_s$ is a dummy indicating if the observation is from a spring semester. The latter is included to control for semester-specific effects on student outcomes[9]. δ_t are year fixed effects and 2019 is the base year. ϕ_i are the student fixed effects that absorb all time-invariant student characteristics including the level of the considered student categories, C, and the students' previous academic achievements. Lastly, ϵ_{ist} is an idiosyncratic error term that is assumed to be uncorrelated with the student fixed effects and the explanatory variables in all semesters.

In all cases, the excluded category, $c = b$, constitutes the base category. When considering parental education as the student characteristic of interest $c_N = 2$ and C is therefore a dummy variable. For all other C there are three categories. The β_1 parameter expresses the effect of COVID-19 on students in the base category compared to their own average outcomes before the pandemic. The coefficients on the interaction terms, α_c, inform about the differential effect of the pandemic for students with the characteristic of interest $c \neq b$ compared to the reference group constituted by the base category. In other words, the α_c coefficient estimates are the parameters of interest when it comes to answering the two main research questions of assessing the existence of a differential COVID-19 effect.

It is important to emphasize that Equation (3.1) will only inform me about a "joint COVID-19 effect", as I am not able to disentangle all of the pandemic's potential effects on Y_{ist} from one another. Therefore, β_1 and α_c will capture both direct and indirect effects of the pandemic on student outcomes, such as shift to online teaching and emotional and economic stress. This is a point made in Bacher-Hicks and Goodman (2021), who argues that though the pandemic cannot be used as a credible instrument to estimate the effect of online teaching it *is* possible to plausibly estimate the effect of the pandemic as a whole, which is exactly what the term joint COVID-19 effect is meant to reflect.

Identification of β_1 and α_c in Equation (3.1) relies on the credibility of comparing students' pre- and post-pandemic outcomes. To be more precise, the critical identifying assumption is that of parallel trends in the outcomes across the groups of students with different characteristics, C, i.e. that any changes in the differences in student outcomes across the different groups in spring 2020 can be credibly attributed to COVID-19 and not any other concurrent factors or random variation over time.

[9]For GPA in particular, we see that there is a tendency for higher grades in the spring semesters.

There is no perfect way to formally assess the validity of this assumption but a comprehensive synthesis of the recent developments in the DiD literature by Roth et al. (2022) suggests to include a visual inspection of so-called "event-study plots". These plots depict the development in outcomes across treated and untreated observations to see if there are any visual indications of violations of the assumption in the pre-treatment periods.

To create such event-study plots, I once again follow Rodríguez-Planas (2022a) and estimate:

$$Y_{is} = \beta_0 + \sum_{s \neq F19} \gamma_s D_s + \sum_{s \neq F19} \sum_{c \neq b} \mu_{sc}(C_{ci} \times D_s) + \phi_i + \epsilon_{is} \tag{3.2}$$

In this equation, D_s is a dummy equal to 1 if an observation is from a given semester, s. The semester immediately prior to the pandemic, fall 2019, is the base semester.

After estimation, I plot the coefficient estimates of μ_{sc} to see if there are visual signs of the estimates diverging before the onset of the pandemic, i.e. if the $\hat{\mu}_{sc}$ are significant for $s < S20$. If I observe any statistically significant deviations from parallel trends in the pre-periods, it will be difficult to make a convincing argument that any changes in the outcome gaps between students with different characteristics, C, in spring 2020 are due to COVID-19 and not unobserved confounders or random fluctuations.

Roth et al. (2022) caution against relying too heavily on tests of pre-existing trends. This is partly because of the fact that even if there are no apparent violations of the pre-trends, it does not rule out the possibility that the assumption is invalid (Kahn-Lang and Lang, 2020). It is also due to the fact that the test suffers from low power, because it reverses the traditional roles of type I and II errors and considers parallel trends as the null hypothesis to be rejected (Bilinski and Hatfield, 2018). As a consequence, the test only rejects the parallel trends assumption if there is strong evidence against it. Therefore, it should be emphasized that inspection of the event-study plots does not allow me to validate the parallel trends assumption if I observe that the pre-trends are jointly insignificant. However, it may provide an indication of whether I should be cautious of placing too much confidence in the findings, if I find notable visual indications of the opposite.

Roth et al. (2022) further encourage empirical researchers to use context-specific knowledge to discuss the plausibility of assuming parallel trends. In the present context, a violation could occur if students' outcomes across student categories diverge over the course of their studies due to selection into different types of courses with different perspectives for their study outcomes. For example, if either group is more likely to choose electives with pass/fail rather than numerical grading. It is to avoid any bias stemming from such behavior that I only include outcomes from mandatory courses in my semester-wise measures of student outcomes.

Due to the panel data structure of my estimation data, I use standard errors that are robust to clustering at the individual level in all of the model estimations based on Equation (3.1).

3.3.1 On-Time Graduation

To assess whether COVID-19 had a differential effect on students' decisions to delay graduation according to student characteristics, I cannot use the same type of estimation strategy with individual fixed effects as described above. Instead, I estimate:

$$y_{ik} = \beta_0 + \sum_{k>2016} \phi_k D_k + \sum_{c \neq b} \kappa_c C_{ic} + \sum_{c \neq b} \omega_{kc}(C_{ic} \times D_k) + \mathbf{X}_\mathbf{i}'\theta + \epsilon_{ik} \quad (3.3)$$

where y_{ik} is a binary indicator that is equal to one if student i in cohort $k \in [2015, 2018]$ delayed graduation. D_k is a dummy equal to one if a student is part of cohort k and where the two cohorts, 2015 and 2016, that according to the study norm should have graduated in the two years immediately prior to the pandemic, are treated as one. C_{ic} are the levels of the student characteristic of interest, where $c = b$ is the excluded category. $\mathbf{X}_\mathbf{i}'$ is a vector of student-level controls, namely sex, age at enrollment, and high school and pre-pandemic university-level GPA.

In Equation (3.3), the parameters of interest are the coefficient estimates on the interactions between the cohort indicators and the categorical variable of interest, ω_{kc}. These capture whether there was a differential effect on students' propensity to finish within three years of studying according to these student characteristics in cohorts that were affected by the pandemic.

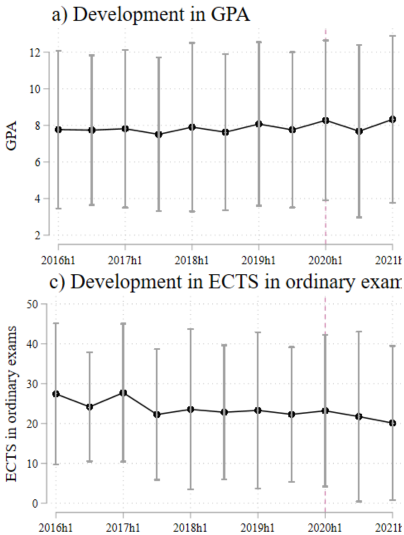
In these estimations, identification of the coefficients of interest hinges on the validity of assuming that pre-pandemic student cohorts can be used as a counterfactual for on-time graduation of post-pandemic cohorts. This implies that these academic outcomes of students in semesters affected by the pandemic are comparable to the pre-pandemic ones in all aspects apart from those attributable to a joint COVID-19 effect. Though y_{ik} is binary, I estimate Equation (3.3) as a linear probability model using OLS. For inference, I rely on heteroskedasticity robust standard errors.

3.4 The Effect of COVID-19 on Average Student Outcomes

To get an overall impression of how the pandemic affected students at CBS, Figure 3.2 and 3.3 display the simple means of different student outcomes. Figure 3.2 shows the development in the average of each of the four short-term outcome variables between 2016 and 2021. Neither of the graphs indicates any marked differences in the average outcomes of students in spring 2020 compared to those in spring 2019, while the 95 % confidence intervals suggest that had there been any notable differences they would likely have been insignificant.

Panel A in Figure 3.3 shows the development in dropout by cohort and semester. For the three cohorts that were affected by the pandemic, 2017-2019, the mean dropout is lower than for the two cohorts that were not affected, i.e. the students who were enrolled in 2015 and 2016. However, since this is the case in all semesters the graph does not suggest that the pandemic might have led to changes in the dropout rates among CBS students. The second graph in Figure 3.3, displayed in Panel B, shows the development in the share of students who completed their bachelor studies within the three-year study norm period. It indicates that average on-time completion is higher for the cohorts of students who were affected by the pandemic compared to the two cohorts that would have graduated

Figure 3.2. Development in mean student outcomes between spring 2016 and spring 2021



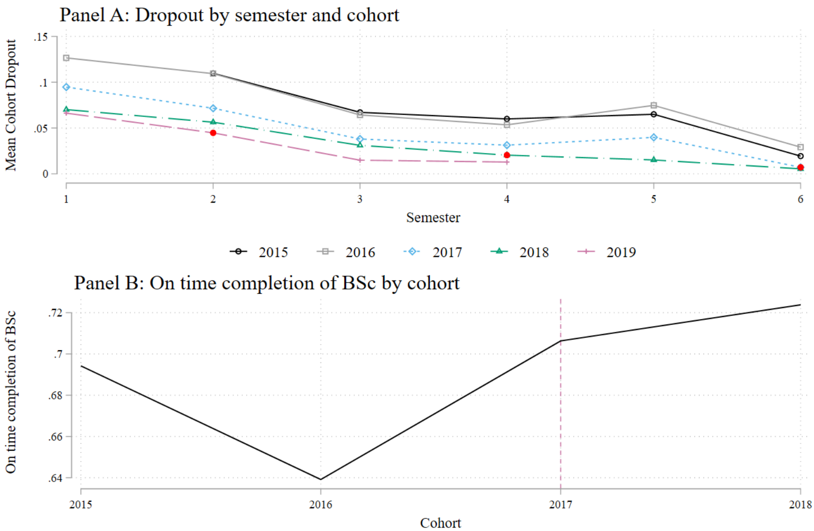
Note: Bars show 95% confidence intervals. Red vertical lines indicate the onset of the COVID-19 pandemic.

in the years immediately before the pandemic, had they graduated on time[10].

Hansen et al. (2021) find that though having a student job is normally associated with poorer student outcomes, it was in spring 2020 during the first lockdown a positive, albeit insignificant, predictor of student performance among students at a German university. Because the majority of the students in the estimation sample is employed in a part-time job, cf. Table 3.1 and 3.2, any changes in their employment might affect their study outcomes. To get an impression of whether this could be the case, Figure 3.4 shows the development in students' work hours, wage income, and employment rate in spring 2020 according to students' income ranks. The figure shows marked differences in the levels of these employment

[10]The confidence intervals are for both graphs very wide and therefore not included, as they make it difficult to properly display the developments over time. The span of the confidence intervals indicate that any differences are unlikely to be significant.

Figure 3.3. Development in mean student dropout and on-time completion of BSc

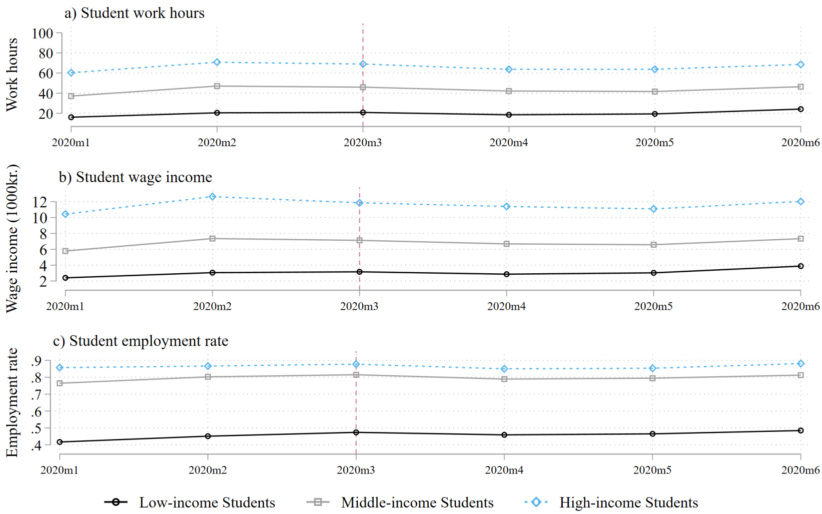


Note: Red dots in Panel A indicate if and when a cohort was affected by the pandemic. The red vertical line in Panel B indicates the onset of the COVID-19 pandemic.

variables but no divergence from these after the onset of the pandemic and the lockdown that was imposed in March 2020 and which forced several industries to temporarily shut down. This could indicate that the policies implemented to counteract the effect of the COVID-19 pandemic on the labor market worked as intended.

The descriptive analysis based on Figure 3.2, 3.3 and 3.4 does not suggest that COVID-19 had much effect on the average student outcomes for cohorts and semesters affected by the pandemic but does not rule out the existence of heterogeneous effects. In the next sections, I present the results of formal analyses of heterogeneity in the effect of the pandemic on student outcomes. I begin by considering whether there was a differential effect according to parental income and education.

Figure 3.4. Development in student employment in spring 2020



Note: Red vertical lines indicate the onset of the COVID-19 pandemic.

3.5 Heterogeneous Effects of COVID-19 on Student Outcomes According to Parental Characteristics

Much of the existing literature on potential differential effects of the pandemic on student outcomes examines heterogeneity according to the parental characteristics of income and educational background (Aucejo et al., 2020; Hansen et al., 2021; Orlov, McKee, Foster, et al., 2021). In this section, I similarly investigate whether parental income and education level was associated with any differential effects of the COVID-19 pandemic on student outcomes. I first estimate the differential effects based on the two main indicators for parental income and education outlined in Section 3.2, before testing the robustness of these estimates to alternative measures of parents' financial means and a more granular definition of their educational background.

3.5.1 Parental Income

To empirically investigate heterogeneous effects of COVID-19 by parental income, Table 3.4 shows the results from estimation of Equation (3.1) with parental income rank as the categorical variable of interest. Column (1) indicates that the first wave of the pandemic did not affect the GPA of students with high-income parents differently than that of their peers with middle- and low-income parents. However, Column (2) shows that in spring 2020 students with low-income parents passed a significantly smaller share of their ordinary exams, 2.3 percentage points less to be precise, compared to their fellow students with high-income parents. This corresponds to a relative change of 38.33% compared to the pre-pandemic gap[11]. Still, in absolute terms the effect is limited.

Table 3.4. Parental income and student performance

	(1) GPA	(2) Pass Rate	(3) ECTS in Ordinary Exams	(4) ECTS in All Exams
$DS20$	0.155***	0.016**	-2.195***	-2.194***
	(0.033)	(0.007)	(0.305)	(0.290)
Middle-income Parents×$DS20$	-0.015	-0.009	-0.542	-0.532
	(0.037)	(0.009)	(0.348)	(0.325)
Low-income Parents×$DS20$	-0.014	-0.023**	-0.640	-0.195
	(0.051)	(0.011)	(0.435)	(0.407)
Observations	28,129	28,129	28,129	28,129
R-squared	0.647	0.630	0.565	0.533
Number of students	8,091	8,091	8,091	8,091

Note: Controls for whether an observation is from a spring semester, as well as for year and student fixed effects. Individual-level cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Column (3) and (4) do not indicate that students with low-income parents on average gained neither significantly fewer nor more ECTS-points in the ordinary exams (Column (3)) or all exams (Column (4)) during spring 2020 than their peers with high-income parents. The coefficient estimates on the interactions between middle-income parents and

11 $\frac{-0.023}{-0.06} \times 100 = 38.33\%$, where 0.06 is the size of the pre-pandemic gap between students with high- and low-income parents cf. Column (6) of Table 3.1.

the spring 2020 dummy in all cases point in the same direction as for those for low-income parents but are insignificant for all of the considered outcome variables. There is therefore no evidence in Table 3.4 indicating that there was a differential effect of the pandemic on student outcomes between these two groups of students.

For each outcome, the coefficient estimate on $DS20$ reflects the effect of the pandemic on students with high-income parents compared to their own pre-pandemic average outcome. For all the outcomes, the pandemic appears to have significantly affected their performance compared to their own pre-pandemic benchmark. More specifically, in spring 2020 students with high-income parents achieved a GPA that was 0.155 SD's higher than the pre-pandemic cohort standard and passed more exams, as indicated by Column (1) and (2), respectively. Interestingly, they at the same time gained significantly fewer ECTS in total and in the ordinary exams. This can be due to these students prioritizing to focus on the exams that grant relatively fewer ECTS points, e.g. on midterms rather than final exams, compared to their pre-pandemic exam strategies. The fact that there is no notable difference in the coefficient estimates of the two ECTS measures could indicate that their exam behaviour did not involve postponing ordinary exams.

In general, the variation in the R^2 measures[12] across the different outcomes indicates that the model is better at explaining the variation in the students' GPA and pass rate than in the number of ECTS points they obtained in all or only their ordinary exams. For the first two outcomes the model explains 64.7% and 63.0% of the variation, respectively, while it for the two ECTS outcomes explains 56.5% for the ordinary exams and 53.3% when considering all exams.

Figure 3.5 shows the coefficients on the interactions between semesters and the indicator for having low-income parents resulting from estimation of Equation (3.2)[13]. Before taking a closer look at the graphs, it should be noted that the coefficient estimates on the interaction terms in Table 3.4 do not correspond to the ones displayed in the graphs and that the significance levels therefore also differ as a consequence. This is due to how the estimations control for individual fixed effects across the two

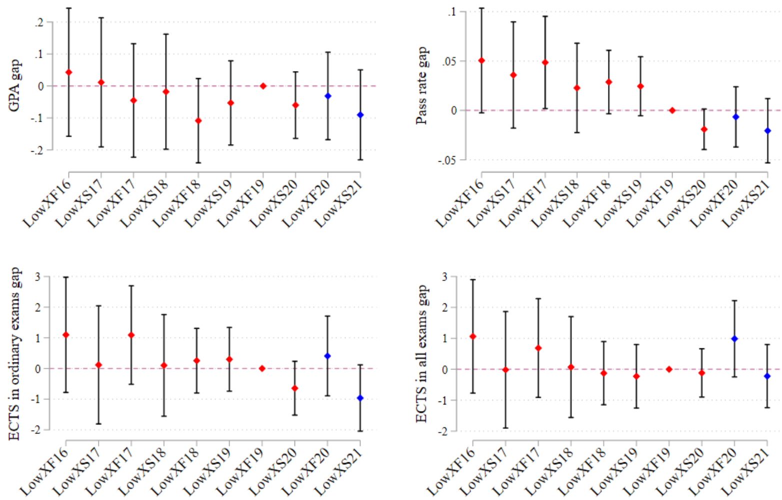
[12] R^2 includes student fixed effects in all models based on Equation (3.1).

[13] The corresponding graphs for students with middle-income parents are included as Appendix Figure 3.10.

regression models. Because of the high number of individual fixed effects and the fact that I am merely interested in controlling for these and not their point estimates, I use an estimation approach that allows me to do just that while being less computationally demanding. For this I rely on Stata's areg command, in which the outcome variable and regressors are first calculated to have a mean of zero within each absorbed category, i.e. within each student in this particular case. Then the mean across all students are added back in and the outcome variable is regressed on the independent variables. Since the initial exercise of demeaning the regressor and regressors depends on which variables are included, the regression estimates across the two models based on Equation (3.1) and (3.2) will also vary because they include different regressors.

Now, when turning the attention towards the subplots in Figure 3.5, visual inspection of the graphs suggests that while there is no apparent indication of a violation of the parallel trends assumption for the GPA and two ECTS outcome gaps, one of the pre-pandemic gaps between students with high- and low-income parents is significantly different from zero. That being said, in that particular period it is only borderline significant at the 5% significance level. Therefore, it might not be unreasonable to assume parallel trends in the pass rate gap between students with high- and low-income parents and thus that the estimate on the interaction between low-income parents and $DS20$ reflects a differential effect of the pandemic. Figure 3.5 additionally suggests that the effect of COVID-19 on the pass rate gap between students with low- and high-income parents in later stages of the pandemic points in the same direction as in the spring 2020 semester. However, these effects appear to be insignificant and are moreover not directly comparable to the previous gaps, as they only include one cohort of students whereas all other gaps include two. This is because the 2020 cohort is excluded from the estimation sample. The choice to exclude these students is based on the assumption that they might not be comparable to previous cohorts. This is because the COVID-19 induced lockdowns in Denmark and many other countries reduced students' outside option by e.g. limiting their possibility to study abroad or take a gap year to work or travel before pursuing a university degree. The anticipation of such effect on students' outside option made the Danish government increase the intake of freshmen students in 2020. As a consequence, the students who started at CBS in 2020 are likely to

Figure 3.5. Event-study plots of low-income vs. high-income parents and short-term student outcome gaps



Note: Plots the development in the gaps in study outcomes between students with low- and high-income parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2). The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1$^{\text{st}}$ to 4$^{\text{th}}$ semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3$^{\text{rd}}$ and 4$^{\text{th}}$ semester, respectively.

be different from previous cohorts.

3.5.1.1 Alternative Parental Income Measures

The finding of a differential COVID-19 effect on students' pass rate in spring 2020 suggested by Table 3.4 and Figure 3.5 relies on a rank measure that is defined relative to the entire Danish population aged 40-65 years. In this section, I consider whether using an alternative measure where the rank of parents' income is defined relative to the income distribution of the parents of the students in the estimation sample affects the results. In other words, I examine if the effect of COVID-19 varied according to using parents' rank relative to the Danish population rather than to that of the parents of a students' immediate peers within their university. This comparison provides some insights as to whether parents' "absolute" or "relative" income rank matters (the most) for the differential student outcomes.

I further investigate, how the estimates are affected when using parents' wealth instead of their income as the basis for determining the ranks of parental financial means.

The results of the first analysis are displayed in Appendix Table 3.14 and the associated event-study plots of the outcome gaps between students with low- and middle-income parents relative to those with high-income parents are depicted in Appendix Figure 3.11 and 3.12, respectively. All of the coefficient estimates in Appendix Table 3.14 point in the same direction as the corresponding estimates in Table 3.4, but are smaller in magnitudes and consequently neither of these estimates are statistically significant. This absence of significant differential effects might suggest that the "absolute" parental income rank is more important for student outcomes than the rank relative to a student's immediate university peers.

As argued in Section 3.2, many parents are self-employed and might therefore have a wealth that is notably higher than their income. To investigate if measuring the rank of parents' financial means by their wealth rank indicates a social gradient in the effect of COVID-19 on student outcomes, I once again estimate the model in Equation (3.1) with indicators for parental wealth instead of income. Appendix Table 3.15, and Appendix Figure 3.13 and 3.14 shows the results of this robustness check and are largely similar to the results in the previous analysis. Both in terms of the magnitude of the point estimates and in that they do not indicate the presence of any significant differential COVID-19 effect across parental wealth groups.

Together the analyses of a potential differential effects on student outcomes according to different measures of parental financial means suggest that it is parents' absolute income that appeared to be most important for affecting the gaps in student outcomes during the first wave of the COVID-19 pandemic.

3.5.2 Parental Education

The question of whether first generation students fared differently through the pandemic compared to their peers who had at least one parent with a higher-level education is the focus of this section. Table 3.5 shows the estimates based on Equation (3.1) where the coefficient on the interaction between having a parent with a college-level degree and spring 2020 is the parameter of interest. For neither of the four outcome variables, did having a parent with a college-level degree appear to involve any differential student outcomes during the pandemic.

Table 3.5. Parental college education and student performance

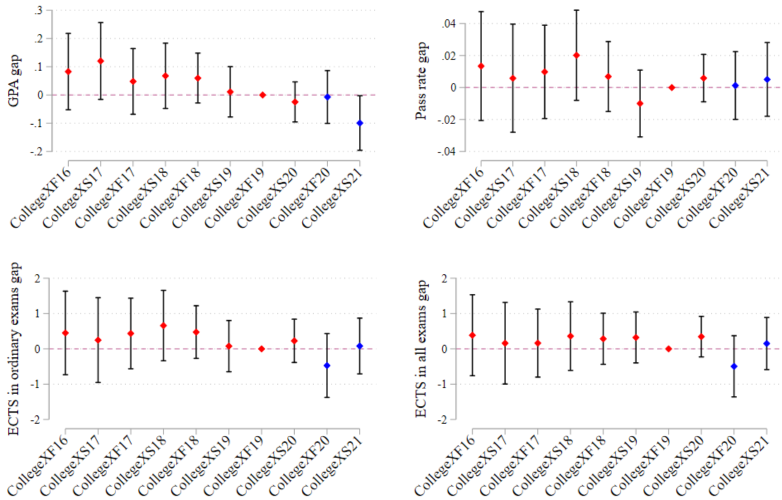
	(1) GPA	(2) Pass Rate	(3) ECTS in Ordinary Exams	(4) ECTS in All Exams
$DS20$	0.164***	0.006	-2.599***	-2.593***
	(0.037)	(0.008)	(0.349)	(0.333)
Parent University× $DS20$	-0.019	0.005	0.200	0.304
	(0.035)	(0.008)	(0.313)	(0.294)
Observations	27,703	27,703	27,703	27,703
R-squared	0.646	0.629	0.564	0.531
Number of students	7,966	7,966	7,966	7,966

Note: Controls for whether an observation is from a spring semester, as well as for year and student fixed effects. Individual-level cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The associated event-study plots based on Equation (3.2) are depicted in Figure 3.6 and do not indicate any issues of violations of the parallel trends assumption.

Though there is little evidence of a *differential* COVID-19 effect according to parental education, Table 3.5 does show that for the two ECTS measures the performance of students who did not have a parent with at least a college-level degree was adversely affected by the pandemic compared to their own pre-pandemic level, as indicated by the estimates on $DS20$, while their GPA were positively affected.

Figure 3.6. Event-study plots of parental education and short-term student outcome gaps



Note: Plots the development in the gaps in study outcomes between students with and without parents with a college-level degree corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2). The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1st to 4th semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3rd and 4th semester, respectively.

3.5.2.1 Alternative Parental Education Measures

The findings in the previous analysis suggested that parental education - as measured by whether a parent had completed at least a college degree - was not significantly associated with differential student outcomes in spring 2020. This could indicate that parents did lot leverage their own experiences with higher education to assist their children with navigating the system during the first wave of the pandemic. It could, however, also be due to the fact that only a minority of the parents with at least a college-level education studied at a university and therefore did not have specific institutional knowledge from a university setting. Table 3.6 shows that there are marked differences in the number of students who had at least one parent who completed a college degree and one parent that completed a university-level degree.

Table 3.6. Highest level of completed education for at least one parent

	(1) College	(2) University
No	38.07%	65.62%
Yes	61.93%	34.38%
Observations	7,966	7,966

To investigate if having parents with an educational background that more closely resembles that of their children's educational setting at CBS involved a differential effect on student outcomes during the pandemic, I estimate Equation (3.1) with an alternative binary measures of parental education. More specifically, in Appendix Table 3.16 I look at whether there was a differential effect of COVID-19 among students who had at least one parent that completed a university degree, while Appendix Figure 3.15 shows the related event-study plots.

This analysis does not indicate any differential effect of COVID-19 according to this alternative educational measure. Together the estimations of the differential effects by parental education provide some suggestive evidence against the hypothesis that parents with higher levels of education leveraged their educational experiences to help their children better navigate the educational system during the pandemic.

3.5.3 Heterogeneity in Delayed Graduation by Parental Characteristics

I now turn towards investigating, whether any differential COVID-19 effects by parental characteristics manifested themselves after a longer period of time. More specifically, I compare the outcomes of student cohorts affected by the pandemic to those who graduated in the years just before, to see if there are any changes in the tendencies in terms of delayed graduation. Table 3.7 includes the relevant results based on estimation of Equation (3.3).

Table 3.7. Parental income and study completion

	On-time
2017 Cohort	0.039**
	(0.019)
2018 Cohort	0.027
	(0.021)
Low-income ParentsXPre-pandemic Cohorts	-0.022
	(0.026)
Low-income ParentsX2017 Cohort	-0.073*
	(0.039)
Low-income ParentsX2018 Cohort	-0.020
	(0.033)
Middle-income ParentsXPre-pandemic Cohorts	-0.014
	(0.019)
Middle-income ParentsX2017 Cohort	-0.017
	(0.025)
Middle-income ParentsX2018 Cohort	-0.044*
	(0.024)
Constant	0.821***
	(0.129)
Observations	5,813
R-squared	0.050

Note: Controls for age, gender, high school and pre-pandemic university GPA. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

For the two affected cohorts, only the students with low-income parents who began their studies at CBS in 2017, and thus were to graduate at the end of the spring 2020 semester, have a significantly different probability of graduating on time than students with high-income parents from a pre-pandemic cohort. However, since the point estimate in this case is close to that for the pre-pandemic cohorts with low-income parents, this effect is likely to not reflect an effect of pandemic.

Table 3.8 show the corresponding estimates with a focus on assessing the potential differential COVID-19 effect by parental education. Analogously to Table 3.7 there is little indication of differential effects for pre- and post-pandemic cohorts. The only differential effect is between students with parents with and without a college degree in the pre-pandmeic cohorts in which case the former group, somewhat surprisingly, had a higher risk of not graduating on time.

Table 3.8. Parental education and study completion

	On-time
2017 Cohort	0.038
	(0.024)
2018 Cohort	0.019
	(0.025)
Parent UniversityXPre-pandemic Cohorts	-0.015
	(0.017)
Parent UniversityX2017 Cohort	-0.027
	(0.023)
Parent UniversityX2018 Cohort	-0.018
	(0.022)
Constant	0.840***
	(0.131)
Observations	5,776
R-squared	0.049

Note: Controls for age, gender, high school and pre-pandemic university GPA. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The main take away from the analyses in this section is that there did not seem to be any differential effects of the pandemic on students' probability of graduating their bachelor studies within the three-year study norm. Moreover, the notably low R^2s indicate that the models only explain a very limited amount of variation in both outcomes.

3.6 Heterogeneous Effects of COVID-19 on Student Outcomes According to Student Income

In addition to investigating if the effect of COVID-19 varied according to parental characteristics, the literature on potential heterogeneous effects of the pandemic on student outcomes has also examined the question of heterogeneity by focusing on student characteristics. This is the topic of the present section, in which I focus on answering my second research question and thus investigate, if there was a differential effect of the COVID-19 pandemic according to student income. I begin my analysis by considering the effect of students' income separately, before analyzing the effect in a model that also indicators for both student and parental income.

3.6.1 Student Income and Student Performance in Spring 2020

Table 3.9 shows the estimation of Equation (3.1), when considering students' own income rank as the categorical student characteristic of interest. The table shows that low-income students earned significantly fewer ECTS than high-income students in spring 2020, both when only considering the ordinary exams and when including retake exams. Importantly, the associated event-study plots in Figure 3.7 indicate that the parallel trends assumption might be violated for both of the ECTS outcomes. Therefore, the significant coefficient estimates on the interaction terms in Table 3.9 could just reflect random variation in the ECTS gaps between low- and high-income students and not a differential effect of the pandemic. However, this risk arguably mainly poses an imminent threat for identification of the ECTS measure including retake exams, as the one focusing on students' performance in the ordinary exams only has one pre-period gap that is borderline significant.

The descriptive statistics presented in Section 3.2 revealed some apparent patterns in the relationship between the income and employment of students and parental income. In particular, the intergenerational mobility matrix in Table 3.3 showed an overrepresentation of students with low-income parents who themselves were classified as having a low income. This could indicate that including income indicators for both the income of students and that of their parents might be important in investigations of differential effects.

Table 3.10 shows the estimation of Equation (3.1) when including measures of both student and parental income rank. In this estimation, the base group is high-income students with high-income parents. Compared to this group, low- and middle-income students earned significantly fewer ECTS in their ordinary exams in spring 2020 with the associated event

Table 3.9. Student income and performance

	(1) GPA	(2) Pass Rate	(3) ECTS in Ordinary Exams	(4)) ECTS in All Exams
$DS20$	0.101*	0.024*	-1.601***	-1.526***
	(0.052)	(0.013)	(0.521)	(0.533)
Middle-income students×$DS20$	0.060	-0.013	-0.919*	-0.928*
	(0.051)	(0.012)	(0.505)	(0.512)
Low-income students×$DS20$	0.039	-0.021	-1.066*	-1.069*
	(0.059)	(0.014)	(0.574)	(0.568)
Observations	28,129	28,129	28,129	28,129
R-squared	0.647	0.630	0.565	0.533
Number of students	8,091	8,091	8,091	8,091

Note: Controls for whether an observation is from a spring semester, as well as for year and student fixed effects. Individual-level cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

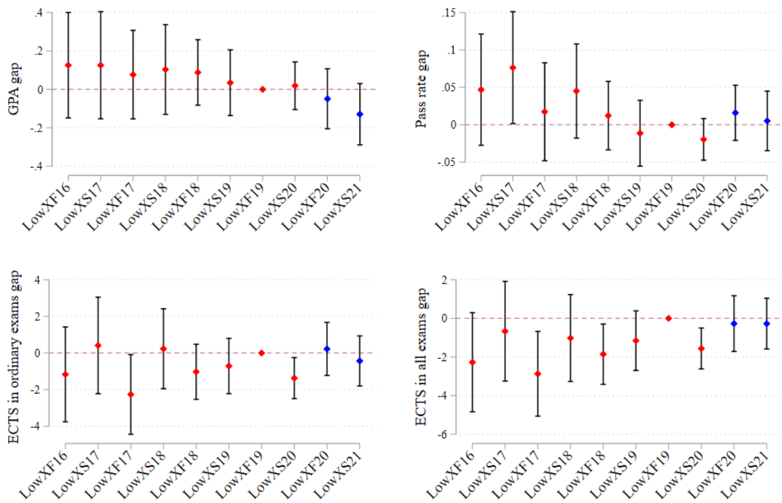
Table 3.10. Parental income and student performance when controlling for student income

	(1) GPA	(2) Pass Rate	(3) ECTS in Ordinary Exams	(4)) ECTS in All Exams
$DS20$	0.108*	0.030**	-1.312**	-1.311**
	(0.055)	(0.013)	(0.543)	(0.552)
Middle-income Parent×$DS20$	-0.014	-0.010	-0.565	-0.556*
	(0.037)	(0.009)	(0.349)	(0.326)
Low-income Parents×$DS20$	-0.011	-0.023**	-0.650	-0.204
	(0.051)	(0.011)	(0.435)	(0.407)
Middle-income Students×$DS20$	0.059	-0.014	-0.951*	-0.949*
	(0.051)	(0.012)	(0.506)	(0.513)
Low-income Students×$DS20$	0.038	-0.021	-1.087*	-1.098*
	(0.059)	(0.014)	(0.575)	(0.570)
Observations	28,129	28,129	28,129	28,129
R-squared	0.647	0.630	0.565	0.533
Number of students	8,091	8,091	8,091	8,091

Note: Controls for whether an observation is from a spring semester, as well as for year and student fixed effects. Individual-level cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

study plots (Figure 3.8 and Appendix Figure 3.17) indicating that the parallel trends assumption is most plausible for the ordinary ECTS outcome. Table 3.10 further indicates that the finding of a differential effect on pass rate for students with low-income parents reported in Table 3.4

Figure 3.7. Event-study plots of low-income vs. high-income students and short-term student outcome gaps

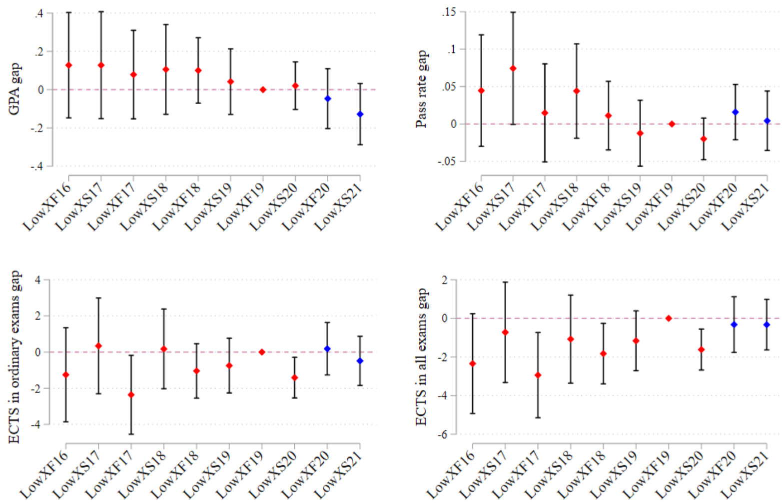


Note: Plots the development in the gaps in study outcomes between low- and high-income students corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2) when controlling for interactions of semesters and parent income rank. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3$^{\text{rd}}$ and 4$^{\text{th}}$ semester, respectively.

is robust to controlling for a student's own income. The estimate of the interaction between having a low-income parent and $DS20$ is still significantly different from zero and of a similar magnitude, just as there is no apparent indication that the parallel trends assumption is violated cf. Figure 3.9.

Overall, the analysis in Table 3.10 suggests that students with low-income parents who themselves had a low income, was the group of students whose short-term study outcomes were relatively hardest hit by the pandemic.

Figure 3.8. Event-study plots of low-income vs. high-income students and short-term student outcome gaps when controlling for parent income

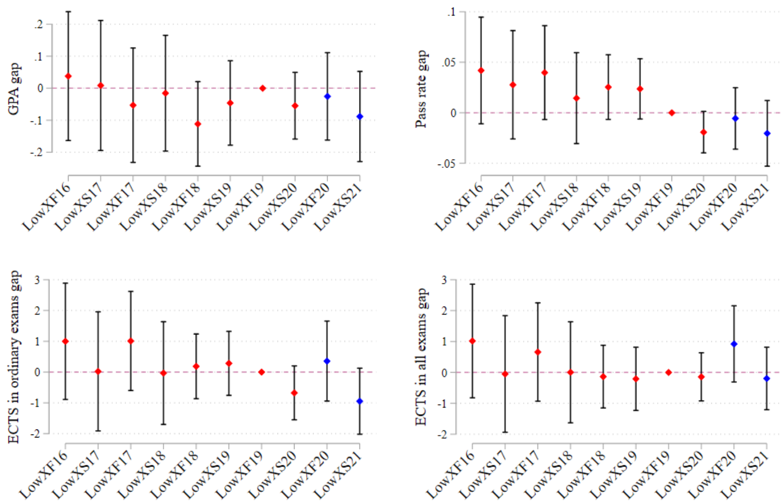


Note: Plots the development in the gaps in study outcomes between low- and high-income students corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2) when controlling for interactions of semesters and parent income rank. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3$^{\text{rd}}$ and 4$^{\text{th}}$ semester, respectively.

3.6.2 Heterogeneity in Delayed Graduation by Student Income

As a last exploration of potential heterogeneity in the pandemic's effect on student outcomes, I investigate if the effect of student's income rank on students' probability of graduating their bachelor degrees within three years differed for cohorts who were and were not affected by the pandemic. The results of this analysis are based on estimation of Equation (3.3) and displayed in Table 3.11. As for the analogous estimation focusing on parental income displayed in Table 3.7, the estimates do not suggest any significant differences in students' propensity to graduate on time

Figure 3.9. Event-study plots of low-income vs. high-income parents and short-term student outcome gaps when controlling for student income



Note: Plots the development in the gaps in study outcomes between students with low- and high-income parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2) when controlling for interactions of semesters and student income rank. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3$^{\text{rd}}$ and 4$^{\text{th}}$ semester, respectively.

according to their income rank and whether they belonged to a cohort that was affected by the pandemic.

3.7 Discussion

The general tendency of significance of the $DS20$ dummies across models indicate that students' average study outcomes were significantly different in spring 2020. One potential reason why we observe different study outcomes in spring 2020 are changes in student well-being.

International survey evidence from Germany (Hansen et al., 2021) and

Table 3.11. Student income and study completion

	On-time
2017 Cohort	0.041
	(0.049)
2018 Cohort	0.027
	(0.046)
Low-income Students	-0.003
	(0.044)
Low-income Students×Pre-pandemic Cohorts	-0.068
	(0.045)
Low-income Students×2017 Cohort	-0.067
	(0.066)
Low-income Students×2018 Cohort	-0.088
	(0.062)
Middle-income Students	0.031
	(0.033)
Low-income Students×Pre-pandemic Cohorts	-0.053***
	(0.020)
Low-income Students×2017 Cohort	-0.037
	(0.052)
Low-income Students×2018 Cohort	-0.036
	(0.047)
Constant	0.886***
	(0.145)
Observations	5,827
R-squared	0.052

Note: Controls for age, gender, high school and pre-pandemic university GPA. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

America (Rodríguez-Planas, 2022b), as well as local insights from CBS (Møller Nielsen, 2020), indicate a decrease in student well-being during the initial wave of the pandemic in spring 2020. Together these reports suggest that student outcomes might have been adversely affected by the pandemic. However, in most cases, we observe a significant positive effect on GPA and pass rate, although the estimates on the ECTS measures are consistently negative.

A potential explanation of the positive effects on the two former measures relates to changes in students' time spend studying. This might have changed in spring 2020 if the restrictions imposed by the government decreased students' outside option for study effort and therefore resulted in an increase in their time spend studying.

The positive effects on GPA and pass rate could also be related to the many changes in exam formats, which might have lead to a more lenient grading. Or perhaps to an increase in the amount of cheating, as the many online exams without student supervision meant that it was easier for students to work together on individual exams. Importantly, such mechanisms do not affect the estimates of the differential effect of the pandemic that are the main focus of the present study, unless they are correlated with the student and parental income indicators. As many exams are blind graded and moreover graded by persons who do not know the students, it does not seem likely that there should be any tendency in leniency that varies according to student or parental income. Similarly, there is no reason to expect that the disposition to cheat is systematically different across these groups of students.

The analyses in Section 3.5 and 3.6 provide some suggestive evidence that the effect of the COVID-19 pandemic on student outcomes were unequally distributed among students with different economic backgrounds. This is on the one hand a little surprising given the number of Danish welfare policies aimed at reducing the social gradient in educational outcomes, while on the other hand perhaps what was to be expected, given that the descriptive statistics in Section 3.2 that showed a difference in student outcomes according to income even before the onset of the pandemic.

When relating the results to the descriptive statistics on pre-pandemic differences in the outcome variables across parental and student income ranks in Table 3.1 and 3.2, an interesting insight emerges. While the differential effect on pass rate according to parental income tends to exacerbate the gap in this outcome between students with high- and low-income parents, the differential effect on the ECTS-measures between students with high- and low-income appear to have closed some of the existing gap. This suggests that different mechanisms may be underlying the observed differential effects by student and parental income.

The theoretical literature offers several channels through which parents may influence their children's educational outcomes. In the context of differences in the effect of the COVID-19 pandemic on student outcomes in higher education, Aucejo et al. (2020) argue that the presence of a social gradient in the effect of the pandemic on student outcomes might be related to differences in how the pandemic affected the economic outcomes and health across students with low- and high-income parents. This could be the case, if parental income counteracted some of the potential emotional and financial stress associated with the pandemic. For example, we might observe a differential effect of the pandemic according to parental income rank, because high-income parents have better possibilities of offering their children financial support in the form of monetary transfers. It could also be due to students with high-income parents being more easily able to move in with their parents during the pandemic, assuming that higher income is associated with superior housing conditions. This, in turn, might help alleviate any feelings of social isolation or stressors associated with poor internet connections during online instruction or exams. Unfortunately, the data does not inform me on neither monetary transfers nor on whether students decided to move in with their parents during the pandemic and as a consequence, I am unable to empirically test if these hypothesized mechanisms attributed to the observed differential effects of the COVID-19 pandemic.

With respect to the effect of students' own income, Figure 3.4 showed that there was no apparent changes in neither wages, work income, nor employment rates across students with different income ranks during spring 2020. This could indicate that the Danish welfare policies in general and the COVID-19 wage compensating scheme in particular, limited the effect of the pandemic on student income. In that case, the observed differential effects according to student income might be more closely related to systematic variation in other characteristics, such as for example motivation and diligence, across these groups of students, than to the differences in their financial means. In terms of my ability to more formally investigate this mechanism, I am sadly once again limited by the data presently available and therefore not able to empirically assess the validity of the hypothesis.

It is important to note that the negative estimates of the interactions of students who have a low income or low-income parents and the spring 2020 dummy does not necessarily imply that these students did worse during this semester compared to pre-pandemic semesters. It only indicates that they performed worse *relative* to the students in the base category, i.e. to those with high-income (parents). As an example, consider the analysis in Table 3.4 where parents' income rank is the characteristic of interest. In this case, the difference between the spring 2020 semester and the baseline semester (fall 2019) for students with low-income parents is given by the sum of the coefficient estimates on the $DS20$ dummy and the interaction between this and the indicator for having low-income parents: $0.016+(-0.023) = -0.007$, which is not significantly different from zero[14]. In other words, students with high-income parents passed significantly more exams during spring 2020 compared to fall 2019, while the pass rate for students with low-income parents was not significantly different in these two semesters.

As outlined in Section 3.2, a student's GPA is important for their future educational paths, as it is the basis for admission to selective Master's degree programs and for the most competitive student exchange programs. Additionally, some of the most high-paying jobs for graduates, e.g. in consultancies, are partly based on students' GPAs. Therefore, the finding of no differential effect on this short-term outcome is perhaps the best indicator for whether we might see any differential effect of the pandemic on later labor market outcomes and could reflect a deliberate optimizing strategy for students.

On another note related to students' exam strategies, it is interesting that though there are instances of significant differential effects on the pass rate, the coefficient estimates on the two ECTS outcomes are remarkably similar in all of the models. This might be due to students opting to prioritize some exams over others, while it is not consistent with any differential patterns in terms of applying a strategy that smoothen the study burden by postponing some exams to the retake period.

3.8 Conclusion

This paper investigated whether the COVID-19 pandemic had a differential effect on the performance of students in Danish higher education. More specifically, I considered two research questions concerned with potential heterogeneous effects of the pandemic and focused on heterogeneity for students 1) with parents of different income and education levels and 2) who themselves differed in terms of income.

I find suggestive evidence that the pandemic had a differential effect between students with different economic backgrounds. In particular, the analyses indicate that students with low-income parents passed signif-

[14]The associated p-value is 0.566.

icantly fewer of their exams during the first semester affected by the pandemic compared to the students with high-income parents. Moreover, students who themselves had a low income obtained significantly fewer ECTS in spring 2020 than their high-income peers. Together this indicates that the students with the most disadvantaged economic backgrounds were the ones, who fared comparatively worst through the first wave of the pandemic. At the same time, the findings of a relatively more adverse COVID-19 effect on student outcomes for the more economically vulnerable students during spring 2020 did not appear to carry over in to longer term student outcomes. At least not in terms of their propensity to finishing their bachelor degrees within the three-year norm period.

The differential effect on short-term outcomes according to student and parental income pointed in opposite direction compared to the pre-pandemic gaps, with the gaps in pass rate between students with high- and low-income parents widening and the gap between low- and high-income students in ECTS gained narrowing. This might be interpreted as a sign that different mechanisms underlie the differential effects of the pandemic. I discuss potential channels for the differential effects but am given the data presently available unfortunately not able to empirically assess their validity. Instead, I suggest as a subject for further research.

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I Appendix Tables and Figures

Appendix Table 3.12. Data cleaning

	Students
Raw sample	12,929
Students with pre-pandemic GPA	10,656
First time CBS students	10,073
Excl. BSc Psyk & BSc Ship	9,391
Information on parental income	8,091

Appendix Table 3.13. Students by semester

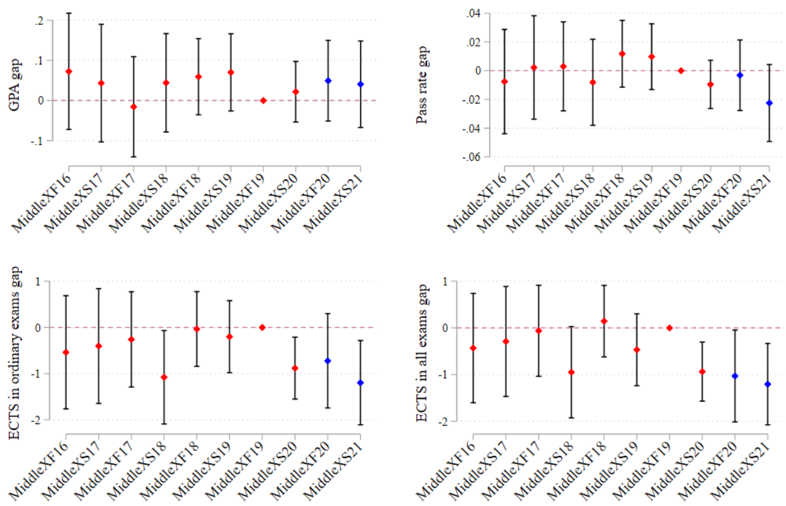
Cohort	**Students**
2015	1,716
2016	1,713
2017	1,688
2018	1,698
2019	1,480
Total	8,091

Appendix Table 3.14. Within-university parental income rank and short-term student performance

	(1) GPA	(2) Pass Rate	(3) ECTS in Ordinary Exams	(4) ECTS in All Exams
$DS20$	0.163***	0.014*	-2.291***	-2.218***
	(0.039)	(0.009)	(0.361)	(0.348)
Relative Middle-income Parents×$DS20$	-0.032	-0.002	-0.104	-0.170
	(0.038)	(0.008)	(0.347)	(0.327)
Relative Low-income Parents×$DS20$	0.006	-0.013	-0.433	-0.319
	(0.047	(0.010)	(0.414)	(0.392)
Observations	28,129	28,129	28,129	28,129
R-squared	0.647	0.630	0.565	0.533
Number of students	8,091	8,091	8,091	8,091

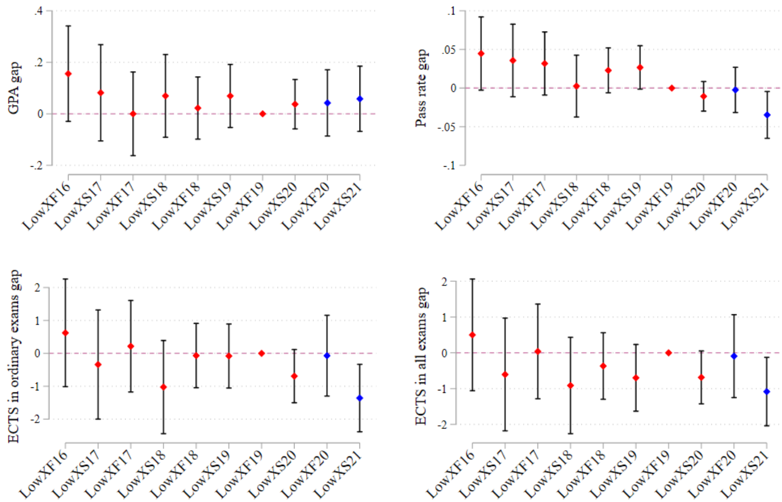
Note: Controls for whether an observation is from a spring semester, as well as for year and student fixed effects. Individual-level cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figure 3.10. Event-study plots of middle-income vs. high-income parents and short-term student outcome gaps



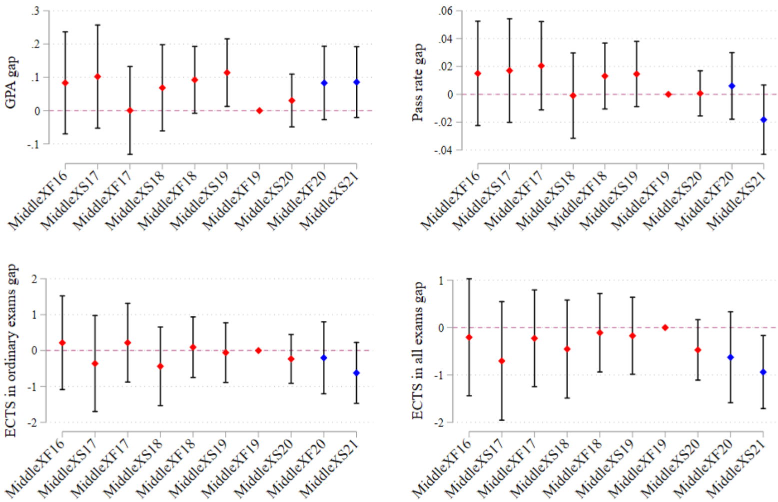
Note: Plots the development in the gaps in study outcomes between students with middle- and high-income parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2). The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1^{st} to 4^{th} semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3^{rd} and 4^{th} semester, respectively.

Appendix Figure 3.11. Event-study plots of within-university low-income vs. high-income parents and short-term student outcome gaps



Note: Plots the development in the gaps in study outcomes between students with low- and high-income parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2) when defining income rank relative to parents of the estimation sample. The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1st to 4th semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3rd and 4th semester, respectively.

Appendix Figure 3.12. Event-study plots of within-cohort middle-income vs. high-income parents and short-term student outcome gaps



Note: Plots the development in the gaps in study outcomes between students with middle- and high-income parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2) when defining income rank relative to parents of the estimation sample. The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1^{st} to 4^{th} semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3^{rd} and 4^{th} semester, respectively.

Appendix Table 3.15. Parental wealth rank and short-term student performance

	(1) GPA	(2) Pass Rate	(3) ECTS in Ordinary Exams	(4) ECTS in All Exams
$DS20$	0.143***	0.013*	-2.268***	-2.197***
	(0.034)	(0.008)	(0.314)	(0.299)
Middle-wealth Parents×$DS20$	0.023	-0.009	-0.370	-0.370
	(0.036)	(0.008)	(0.336)	(0.314)
Low-wealth Parents×$DS20$	-0.001	-0.012	-0.709	-0.646
	(0.051)	(0.011)	(0.446)	(0.422)
Observations	27,624	27,624	27,624	27,624
R-squared	0.646	0.629	0.564	0.531
Number of students	7,944	7,944	7,944	7,944

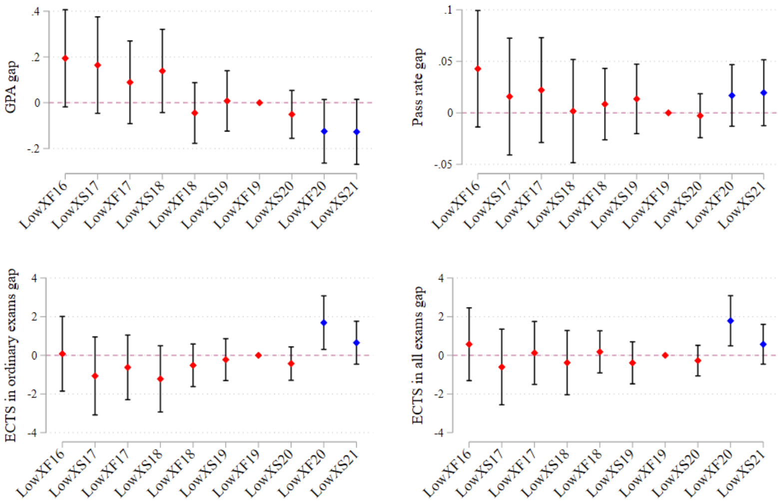
Note: Controls for whether an observation is from a spring semester, as well as for year and student fixed effects. Individual-level cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 3.16. Parental university education and student performance

	(1) GPA	(2) Pass Rate	(3) ECTS in Ordinary Exams	(4) ECTS in All Exams
$DS20$	0.145***	0.008	-2.587***	-2.521***
	(0.033)	(0.007)	(0.306)	(0.296)
Parent university×$DS20$	0.020	0.003	0.321	0.335
	(0.034)	(0.007)	(0.309)	(0.284)
Observations	27,703	27,703	27,703	27,703
R-squared	0.646	0.629	0.564	0.531
Number of students	7,966	7,966	7,966	7,966

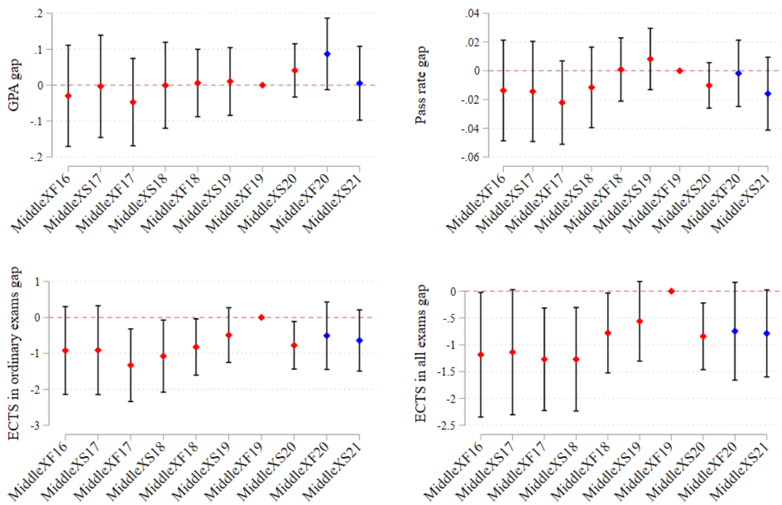
Note: Controls for whether an observation is from a spring semester, as well as for year and student fixed effects. Cluster robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figure 3.13. Event-study plots of middle-wealth vs. high-wealth parents and short-term student outcome gaps



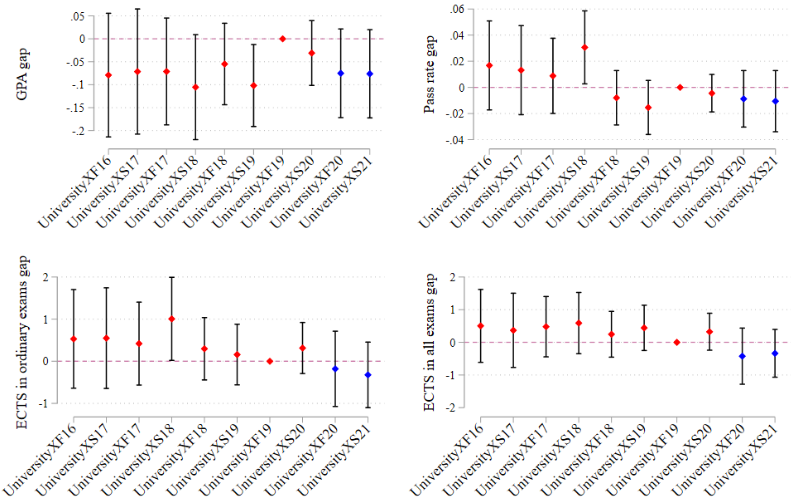
Note: Plots the development in the gaps in study outcomes between students with low- and high-wealth parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2). The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1st to 4th semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3rd and 4th semester, respectively.

Appendix Figure 3.14. Event-study plots of low-wealth vs. high-wealth parents and short-term student outcome gaps



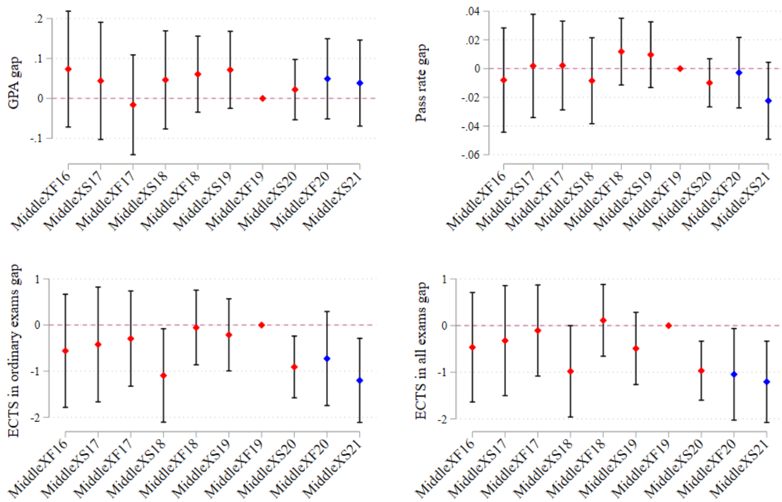
Note: Plots the development in the gaps in study outcomes between students with middle- and high-wealth parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2). The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1st to 4th semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3rd and 4th semester, respectively.

Appendix Figure 3.15. Event-study plots of parental university education and short-term student outcome gaps



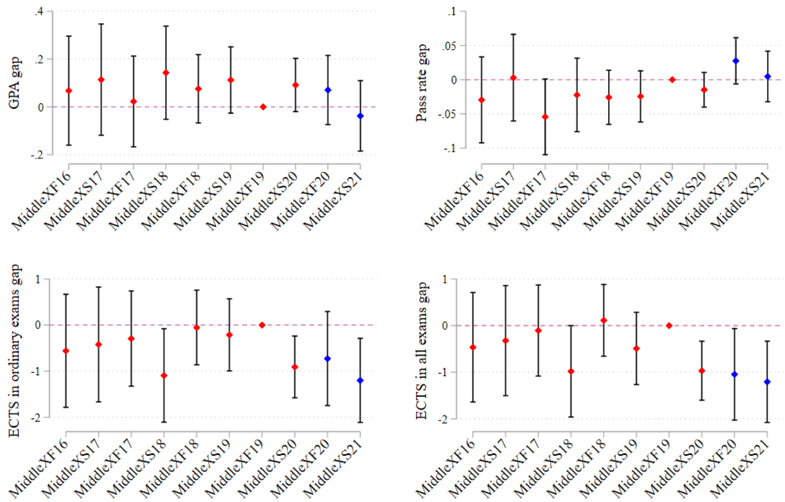
Note: Plots the development in the gaps in study outcomes between students who had at least one parent that had completed a university-level degree and students who did not, corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2). The point estimates in the two last periods are marked with blue to indicate that there is a change in the population considered, as I always only include students on their 1^{st} to 4^{th} semester. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3^{rd} and 4^{th} semester, respectively.

Appendix Figure 3.16. Event-study plots of middle-income vs. high-income parents and short-term student outcome gaps when controlling for student income



Note: Plots the development in the gaps in study outcomes between students with middle- and high-income parents corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2) when controlling for student income rank. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3rd and 4th semester, respectively.

Appendix Figure 3.17. Event-study plots of middle-income vs. high-income students and short-term student outcome gaps when controlling for parent income



Note: Plots the development in the gaps in study outcomes between middle- and high-income students corresponding to the coefficient estimates of μ_{sc} and the associated standard errors resulting from estimation of Equation (3.2) when controlling for parent income rank. Because I exclude the 2020 cohort, the two last point estimates only include the 2019 cohort who in fall 2020 and spring 2021 were on their 3^{rd} and 4^{th} semester, respectively.

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