Empirical Essays in Economics of Education and Labor

Skibsted, Marie Kruse

COPENHAGEN BUSINESS SCHOOL
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EMPIRICAL ESSAYS IN ECONOMICS OF EDUCATION AND LABOR

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HANDELSHØJSKOLEN

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Marie Kruse Skibsted

Supervisors: Anders Sørensen and Battista Severgnini

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"The Doctoral School of Economics and Management is an active national and international research environment at CBS for research degree students who deal with economics and management at business, industry and country level in a theoretical and empirical manner".

Preface

This thesis is the final product of my doctoral studies at the Department of Economics at Copenhagen Business School. I am grateful for the funding provided by Copenhagen Business School, which has allowed me to work on this project for three years and to travel to conferences and participate in workshops. There are numerous people that I would like to thank who have, in very different ways, contributed to my work in the last few years.

First and foremost, I want to express my gratitude to Anders Sørensen, my principal supervisor, for our collaboration, for countless comments and suggestions, and for making my visit to the Stanford Graduate School of Business possible. Also, a special thanks to my co-supervisor, Battista Severgnini, who contributed with valuable suggestions, ideas, and encouragement whenever needed.

In 2013, I got the opportunity to visit the Stanford Graduate School of Business. The months there were filled with academic discussions and seminars, which provided me with so much valuable experience and so many new ideas. I am grateful to Professor Edward Lazear for inviting me and creating this opportunity. Without the funding from several funds, including the Otto Mønsted Fond, Knud Højgaards Fond, Fabrikejer, ingeniør Valdemar Selmer Trane og hustru, Elisa Tranes Fond, and the Augustinus Fonden, this trip would not have been possible.

I am sincerely grateful for the careful reading of my papers and the detailed comments and feedback that I received from my pre-defense committee, consisting of Professor Lisbeth la Cour and Professor H.C. Kongsted, chairman of my Ph.D. committee. Also, thanks to my colleagues from the Department of Economics at Copenhagen Business School for providing an inspiring research environment. A special thanks to Fane Groos for helpful comments and mental support, to Moira Daly for always being ready to help me understand all of the econometrics, and to Anette Boom for being a supportive and helpful Ph.D. coordinator. The entire secretariat at the Department of Economics, the CBS Graduate Administration, and the Administrative Planning Unit for the Business Economics undergraduate program at CBS were also very helpful when I had questions related to the structure of the programs at CBS.

Also, I want to thank Christian Møller Dahl for our collaboration on my second chapter and for many helpful comments on other parts of my work. I also owe Benedikte Bjerg, with whom I have written my fourth chapter, a large thank you. Thanks for your cooperation and for all of the encouragement. Finally, I have spent my years as a Ph.D. student with the best Ph.D. colleagues possible. Thanks for innumerable discussions about endogeneity, for being the best office-mates possible, and for all the great laughs we shared along the way.

I also need to thank all of my friends and my family for all the encouragement and for always being there when I needed a reason to smiley. Specially, I want to thank my father for talking sense into me, my mother for being a steady rock, and my grandmother for always reminding me that there is so much more to life than a Ph.D. I also need to thank my brothers for, among other things, creating the soundtrack of my Ph.D. Last, but not least, I need to thank my boyfriend Simon, without whom I would not have managed to finish. Thanks for so much loving support, endless patience and for always being there for me even when I was not able to be there for myself.

As my time as a Ph.D. student comes to an end, I have come to realize how little I knew when I started. In many aspects, I now feel ready to write my papers properly since I now know what I should have done and particularly what I would like to do in further research. I guess that is the nature of finishing a Ph.D., how ironic it might be.

Summary (English)

This thesis focuses on individuals' educational achievements and labor market outcomes in a Danish context. Particularly, the thesis aims at determining the returns to specific tertiary educational decisions and understanding the mechanisms underlying such decisions. These related objectives are addressed using econometric methods applied on Danish micro data. All four chapters are empirical studies and combine data from different sources. The main source of data is an administrative data set obtained from Copenhagen Business School (CBS) that contains detailed educational information on students enrolled at CBS. I combine this data with register data obtained from Statistics Denmark. The educational data is the core of Chapter 2, Chapter 3, and Chapter 4 and defines the sample in these chapters. Chapter 1 relies exclusively on data from Statistics Denmark.

Chapter 1 (a joint work with Anders Sørensen from Copenhagen Business School) estimates the wage premium of those with a master's degree in business economics and management when compared to the wages of those with master's degrees in other fields in the social sciences. By means of an Instrumental Variable (IV) approach, we identify the returns to a business education by addressing the endogenous selection of master's programs. Using season of birth as an exogenous determinant of master's degree choice, we find that a master's degree in business economics and management results in a wage premium of around 12% compared to other master's degrees in the social sciences. Moreover, we find that the probability of private sector employment is significantly larger for individuals with a master's degree in business economics and management. Finally, in contrast to the literature that finds significant reductions in the gender wage gap when controlling for educational fields, controlling for a master's degree in business economics and management does not affect the large and robust gender wage gap prevalent in our sample.

Chapter 2 (a joint work with Anders Sørensen from Copenhagen Business School and Christian Møller Dahl from University of Southern Denmark) documents how variation in choice of electives and educational diversification within a master's program corresponds to variation in labor market outcomes across individuals. Chapter 2 uses information on individuals who enrolled in the same master's programs at CBS but took different elective courses in order to estimate the association between detailed educational decisions and both wage outcomes and the probability of obtaining leadership positions. The findings in Chapter 2 indicate, among other things, that elective management courses and educational diversification within classical business school topics are associated with a higher probability of obtaining leadership positions. By contrast, we find that educational diversification

outside classical business school courses is insignificant in our model. The latter provides more insight into the widespread idea that top managers have broad knowledge and benefit from general abilities by contrast to firm specific skills.

Both Chapter 3 (single authored) and Chapter 4 (a joint work with Benedikte Bjerg from University of Copenhagen) use data on students enrolled in the largest undergraduate program at CBS. Relying on information about randomly assigned peer groups, Chapters 3 estimates the impact from peers on the probability of dropping out during the first year of study and Chapter 4 estimates the impact from peers on the choice of master's degree program. Both chapters address the econometric problems of self-selection into peer groups by using randomly assigned groups as a measure of peer groups.

Chapter 3 addresses the two-way causality that arises from the interdependence of individual and peer group behavior by using pre-determined measures of abilities to create measures of peer group quality and finds that women's probability of dropping out increases with the ability level of their peers. By contrast, men's probability of dropping out is unaffected by peers' abilities. Chapter 3 also shows how women's peer group rank is a stronger determinant of the drop out probability than women's high school GPA. One interpretation of my findings is that women compare themselves with their peers and create wrong ideas about their own ability level, which could potentially distort the expectations of cost and benefits of education and make women under-invest in their own education.

Concerning peer effect in master's degree choice, Chapter 4 uses the nontraditional method of dyadic regression to document how pairs of students that were assigned to the same peer group when enrolled in the bachelor's program are more likely to choose the same master's program after bachelor's graduation. In the context of our model, this can be thought of as positive assortative matching on peers. Importantly, we find that positive assortative matching among peers is stronger for individuals with similar abilities measured by first year GPA. Finally, we see no strong adverse effect of following peers on educational performance.

Resume (Danish)

Denne afhandling fokuserer på individers uddannelsesmæssige og arbejdsmarkedsrelaterede resultater i en dansk kontekst. Konkret har afhandlingen til formål at bestemme individers økonomiske afkastet knyttet til forskellige aspekter af videregående uddannelser samt at forstå mekanismerne bag forskellige beslutninger relateret til en videregående uddannelse. Disse emner behandles ved hjælp af økonometriske metoder, der anvendes på det danske mikro data. Alle fire kapitler er empiriske undersøgelser og kombinere data fra forskellige kilder. Den vigtigste datakilde er et administrativ datasæt fra Copenhagen Business School (CBS), der indeholder detaljeret uddannelsesinformation på studerende på CBS. Jeg kombinerer disse data med det danske registerdata fra Danmarks Statistik. Uddannelsesdataet fra CBS er kernen i kapitel 2, kapitel 3, og kapitel 4 og definerer stikprøven i disse kapitler. Analyserne i Kapitel 1 beror udelukkende på data fra Danmark Statistik.

Kapitel 1 (udarbejdet med Anders Sørensen fra Copenhagen Business School) benytter det danske registerdata til at estimere en lønpræmie for dem med en kandidatgrad i erhvervsøkonomi og ledelse set i forhold til dem med kandidatgrader indenfor andre områder af samfundsvidenskab. Vi identificerer afkastet til en uddannelse i erhvervsøkonomi ved hjælp af Instrument Variable (IV) teknikker. Helt konkret forklarer vi valg af uddannelsesretning med graden af uddannelsesmæssig modenhed, et karakteristika vi antager er eksogent i forhold til arbejdsmarkedsrelaterede resultater og vi måler denne uddannelsesmæssige modenhed med fødselstidspunktet på året. Ved hjælp af denne metode finder vi, at en kandidatgrad i erhvervsøkonomi og ledelse resulterer i en lønpræmie på omkring 12 % set i forhold til andre kandidatuddannelser indenfor samfundsvidenskab. Derudover finder vi, at sandsynligheden for at blive ansat i den private sektor er signifikant større for personer med en kandidatgrad i erhvervsøkonomi og ledelse. Endelig, i modsætning til flere studier i litteraturen der finder at lønforskellen mellem kønnene reduceres betydeligt når der kontrolleres for uddannelsesvalg, finder vi ingen reduktion i lønforskellen mellem kønnene selv efter vi har kontrolleret for en kandidatgrad i erhvervsøkonomi og ledelse.

Kapitel 2 (udarbejdet med Anders Sørensen fra Copenhagen Business School og Christian Møller Dahl fra University of Southern Danmark) dokumenterer hvordan variation i valg af valgfag og uddannelsesmæssige diversificering bliver reflekteret i resultater på arbejdsmarked. I Kapitel 2 benytter vi data på studerende, der alle har studeret den samme kandidatgrad på CBS, men som har valgt forskellige valgfag. Ved hjælp af dette data estimerer vi sammenhængen mellem detaljerede uddannelsesmæssige beslutninger og både løn og sandsynligheden for at opnå lederstillinger. Resultaterne

præsenteret i kapitel 2 viser, blandt andet, at kurser i ledelse og uddannelsesmæssig diversificering indenfor klassiske business school kurser er associeret med en højere sandsynlighed for at opnå lederstillinger. I modsætning til dette finder vi også, at uddannelsesmæssig diversificering udenfor de klassiske business school kurser er insignifikante i vores model. Sidstnævnte resultat giver mere indsigt i den udbredte idé om, at topchefer har bred viden og drager fordel af generelle færdigheder i modsætning til at have virksomhedsspecifikke færdigheder.

Både kapitel 3 og kapitel 4 (sidstnævnte udarbejdet med Benedikte Bjerg fra Københavns Universitet) bruger data på studerende der har været indskrevet på den største bacheloruddannelse på CBS. Dette data indeholder information om tilfældigt inddelte grupper som de studerende inddeles i ved uddannelsesstart, hvilket er en essentiel information for begge kapitler fordi de begge løser det økonometriske problem med selektion af peers ved at bruge de tilfældigt inddelte grupper som et mål for peer-grupper. Kapitel 3 estimerer effekten fra peers på sandsynligheden for at droppe ud i løbet af det første studieår, mens kapitel 4 estimerer effekten fra peers på valg af kandidatretning.

Et kendt problem når man estimerer peer-effekter er den tovejs kausaliteten der opstår, fordi individets og peer-gruppens adfærd er indbyrdes afhængig. I Kapitel 3 benytter jeg de studerendes gymnasiegennemsnit som et mål for deres evner og niveau til at udregne et mål for "peer-kvalitet", der er forudbestemt i modellen og derfor ikke endogent. Resultater i Kapitel 3 viser, at kvinders sandsynlighed for at droppe ud stiger med niveauet i deres peer-gruppe. I modsætning til det viser det sig, at mænds sandsynlighed for at droppe ud er upåvirket af niveauet i deres peer-grupper. Kapitel 3 viser også, at kvinders rang indenfor deres peer-gruppe har en stærkere indflydelse på kvinders sandsynlighed for at droppe ud end kvindernes eget gymnasiegennemsnit. Jeg tolker disse resultater som, at kvinder sammenligner sig med deres peers og skabe forvredne forestillinger om deres eget niveau, hvilket kan medføre at de dropper ud. Sådanne forkerte ideer om eget niveau kan ligeledes resultere i forkerte forventninger til omkostningerne og fordelene ved at tage en uddannelse, hvilket kan gøre, at kvinder underinvestere i deres egen uddannelse.

Kapitel 4 estimerer peer-effekter i valg af kandidatretning ved at bruge dyadic regression. Dyadic regression er i uddannelseslitteraturen en utraditionel økonometrisk metode, men den er oplagt at bruge til at estimere hvorvidt par af studerende, der ved uddannelsesstart tilfældigt blev sat i samme gruppen, er mere tilbøjelige til at vælge den samme kandidatuddannelse efter bachelor-eksamen. I rammerne af vores model kan dette tolkes som positiv assortative matching på peers. Vores resultater viser, at studerendes valg af kandidatgrad er påvirket af deres peers og at denne positive assortative matching på peers er stærkere for personer med samme karaktergennemsnit fra første år. Endelig observerer vi ingen effekt af at følge peers i valg af kandidatretning på hverken uddannelsesmæssige resultater eller resultater på arbejdsmarkedet.

Contents

Introduction

This thesis consists of an introduction followed by four numbered chapters and ends with a short conclusion. All chapters are based on empirical research papers, are self-contained, and can be read independently. However, all four chapters fall under the heading of *Economics of Education and Labor* and are concerned with aspects of the associations between tertiary educational decisions and labor market outcomes and mechanisms underlying these educational decisions.[1] My Ph.D. was funded by Copenhagen Business School (CBS) and was initiated because of the need to better understand how students, firms, and society in general can benefit from a business school as CBS. Thus, many questions were raised in advance of the research. Chapters 1 and 2 initiate from and expand on these questions, whereas the ideas for Chapter 3 and Chapter 4 were formed during the Ph.D. Chapter 1 estimates the return to a master's degree in business economics and management and Chapter 2 estimates the association between labor market outcomes and students' choice of elective master's courses. Chapters 3 and 4 focus on peer effects on educational decisions related to a tertiary education. Particularly, Chapter 3 investigates the impact of peers' abilities on individuals' probability of dropping out of university and Chapter 4 estimates peer effects in master's program choice. While each chapter has its own topic, the chapters are linked through the use of Danish micro data, the empirical focus and econometric techniques, and an overlap in background literature.

The topics of this thesis are motivated by both the micro and macro literature. It is broadly recognized that educational investment decisions are related to welfare and growth both at the country and individual levels. Existing empirical research at the micro level provides evidence of field-of-study choices being strong predictors and causal determinants of labor market outcomes, with the literature on this topic growing in recent years (e.g., Arcidiacono, 2004; Altonji et al., 2012; Kirkebøen et al., 2014; Joensen and Nielsen, 2015; Altonji et al., 2015). Moreover, at the macro level, findings suggest that growth and cognitive skills (in contrast to years of education) are positively associated (e.g., Hanushek and Kimko, 2000; Hanushek and Woessmann, 2012). These findings suggest that students

[1] In Appendix A, page 258, I describe the Danish education system in more detail. For people unfamiliar with the Danish education system it might be beneficial to consult Appendix A before reading the papers in this thesis.

should be guided in their educational choices in order to improve both individual-level outcomes and the economic growth. However, the causal influence from the field of education is still debated, and knowledge about the impact of detailed educational choices is limited. For policymakers to be able to design policies aimed at improving micro-and macro-level outcomes through changed educational behavior, gaining a better understanding of how educational choices are reflected in the labor market is crucial. Moreover, to design policies that are adequate in changing educational behavior, knowledge concerning the drivers of these choices is essential. The former justify the research focus in Chapters 1 and 2— the returns to specific educational decisions—while the latter motivate the focus of Chapters 3 and 4 on the determinants of educational decisions. Importantly, my findings also feed into the ongoing debate in Denmark focusing on productivity differences across educational fields and how to construct a more efficient education system.

Almost all empirical work on the returns to and choice of education relies on the theoretical and empirical work of Becker (1964), Mincer (1958, 1974), and earlier studies. Among other things, Becker contributed to the economic literature by expanding on the ideas that individuals could decide on their education and that such decisions could be considered investments in *human capital*, which would contribute to productivity. In the framework of human capital theory, individuals choose their education based on the cost and benefits associated with such decisions.[2] Mincer (1974) introduced the Mincer log wage equation where he related earnings to not only years of schooling but also experience or on-the-job training. The Mincer log wage equation has since been used as the foundation for countless studies on the return to education. Following these studies, the empirical literature investigated the causal impact of years of education on earnings by means of Instrumental Variable (IV) approaches (e.g., Angrist and Krueger, 1991; Card, 1999).

More recently, the empirical literature has been concerned with the differences in labor market outcomes caused by differences in field-of-study choices (e.g., James et al., 1989; Blundell et al., 2000; Arcidiacono, 2004; Altonji et al., 2012; Kirkebøen et al., 2014; Altonji et al., 2015). This thesis is inspired by such studies, but suggests alternative ways of addressing the selection into education and also considers the impact of self-selected curricular differences within a specific field of study. Building on the literature that has established a significant connection between educational decisions and labor market outcomes, researchers have realized the importance of understanding the mechanism underlying such educational decisions (e.g., Berger, 1988; Montmarquette et al., 2002; Arcidiacono, 2004; De Giorgi

[2]Moreover, Becker was the first to distinguish between general and firm specific skills. This distinction can be related to Chapter 2, which investigates the importance of managerial skills. Managerial skills are often though of as being the opposite to firm specific skills.

et al., 2010; Zafar, 2013). Following this literature, this thesis provides empirical studies of students' decisions of dropping out of university as well as their choice of master's program.

In the process of writing my Ph.D. thesis, the results obtained from one chapter inspired me to initiate and continue working on the other chapters, which again inspired me to take up the previous chapters for revision. However, the order of the chapters still reflects an important learning process in two ways. First, Chapter 1 was simply the first chapter that I wrote while Chapter 4 was the last chapter I initiated. Second, the order of the chapters reflects a learning process at a more substantial level. Chapter 1 estimates the return to a master's in business economics and management and Chapter 2 investigates the labor market consequences of detailed educational choices, such as type of elective master's courses and the extent of educational diversification within a specific master's program. Both Chapter 1 and Chapter 2 establish a significant association between educational choices and labor market outcomes and emphasize the need to understand what drives individuals to make these educational choices. Motivated by these results, Chapters 3 and 4 focus on determinants of educational decisions related to a tertiary education. In particular, Chapters 3 and 4 focus on understanding peer effects in the decision to drop out of university and in choice of master's program. Chapter 4 is the least traditional of the chapters, as it applies an estimation method that is not very familiar within the economics of education literature.

The four chapters share a key feature, namely the use of Danish micro-level data. All chapters use the Danish register data that covers the entire Danish population and is maintained by Statistics Denmark. Besides the Danish register data, a key data source is a unique data set obtained from the administration at CBS. This data contains detailed educational information on individuals that were enrolled at CBS between the 1980s and 2011. I use this data in Chapters 2, 3, and 4. The large amount of information in this data enabled me to shed light on questions that the literature often only answers using survey data. This data also contains information on randomly assigned peer groups, which I exploit in Chapter 3 and Chapter 4 to overcome the standard self-selection problem when estimating peer effects (Manski, 1993). Finally, Chapter 2 also uses data from the Danish Business Authority about individuals on the executive boards of all joint stock companies that already existed or were established during the 2000–2010 period.

Chapter Overview

A large body of research investigates the labor market returns to education, and studies have analyzed both the return to an extra year of education (the quantity of education) and the return to specific fields of education (the quality of education) (e.g., Angrist and Krueger, 1991; Card, 1999; James et al., 1989; Blundell et al., 2000; Arcidiacono, 2004; Altonji et al., 2012; Kirkebøen et al., 2014). Particularly, the causal impact from education continues to be a topic of interest for policymakers and is therefore still debated (for reviews and discussions about this see Altonji et al., 2012, 2015). Using Danish register data, Chapter 1, *The Returns to a Business Education - Evidence from Danish Administrative Register Data* (a joint work with Anders Sørensen from Copenhagen Business School), builds on the traditions of this literature and establishes a causal relationship between a master's degree in business economics and management and labor market outcomes, measured both as hourly wage and the probability of employment in the private sector.[3] Chapter 1 compares the labor market outcomes of students who graduated with a master's degree in business economics and management to those of students who graduated with a master's degree in other social science fields.

By contrast to other studies that measure wages at one point in time, the Danish register data allows us to measure hourly wages every year after graduation up and until the 10$^{\text{th}}$ year. Estimating a potential business education wage premium every year after graduation helps us to understand how this business education wage premium works together with enhanced labor market experience. To estimate a causal relationship, we apply an Instrumental Variable (IV) approach. Determinants of educational choices that are exogenous to labor market outcomes is scarce (Altonji et al., 2015). We complement the literature by suggesting educational maturity as an instrument for master's degree choice. We define educational maturity as a student's certainty about field-of-study and career choices. The intuition behind this idea is that the more educationally mature students are, the more certain they are about their field-of-study choice. Because a master's program at a business school is much broader than a master's program in other fields of the social sciences, we hypothesize that students with a low level of educational maturity are more likely to choose a master's degree in Business Economics. We measure educational maturity by quarter of birth and argue that, because we consider the case of Denmark, quarter of birth is exogenous to labor market performance.

Using our suggested instrument, we find a causal and positive impact of a master's degree in business on a set of measures of labor market outcomes. Specially, we show that a master's degree in

[3] The term "a master's degree in business economics and management" is composed of master's degrees obtained from different Danish business schools.

business economics and management is associated with a wage premium of 12-17%. Moreover, the estimated business wage premium increases in years after graduation, suggesting that the acquired skills become more valuable when integrated with labor market experience. Estimating the wage regression for each year after graduation, we also find an increasing gender wage gap. The wage gap increase from 7% to 23% in the period from the first year to the 10$^{\text{th}}$ year after graduation. By contrast to the literature that finds significant reductions in the gender wage gap when controlling for educational fields, controlling for a master's degree in business economics and management does not narrow the gender wage gap prevalent in our sample. Finally, we find that a master's degree in Business Economics increases the probability of private sector employment significantly.

Chapter 2, *Choice of Electives and Future Leadership - Evidence from Business School Students* (a joint work with Anders Sørensen from Copenhagen Business School, and Christian Møller Dahl from University of Southern Denmark), uses data from CBS, the Danish register, and the Danish Business Authority to examine how the extent of educational diversification, i.e., the number of different types of elective courses a student took during the master's program, and the choice of certain elective master's courses are reflected in both the hourly wages and the probability of getting a position in the executive board of a firm (a C-level position). Building on existing findings in the literature, we expect that individuals who are educated in management would have a higher probability of getting a C-level position. Moreover, we expect individuals with diversified knowledge from their education to be more likely to obtain a C-level position.

A growing body of literature is concerned with determining what specific skills are required for leaders. Within this literature, both theoretical and empirical studies have been concerned with the influence of managerial abilities and diversified knowledge on CEO appointments and payments (e.g., Murphy and Zabojnik, 2004; Lazear, 2012; Custódio et al., 2013; Falato et al., 2015). Specifically, empirical studies have shown that being educationally diversified and having diversified labor market experiences are positively reflected both in the probability of becoming a leader and in the wages of CEOs (e.g., Lazear, 2012; Custódio et al., 2013; Falato et al., 2015). Alongside this literature, a body of research investigates the influence of specific educational decisions and skills on labor market outcomes, holding the level and field of education constant. Within this literature, studies have found that skills related to mathematics and finance particularly improve wage outcomes (e.g., Joensen and Nielsen, 2009; Bertrand et al., 2010; Lazear, 2012; Joensen and Nielsen, 2015). Finally, findings in a slightly different branch of the literature suggest that management practices and choice of CEO can explain part of the differences in performance across otherwise equal firms (e.g., Bloom and Van Reenen, 2007; Bennedsen et al., 2006; Bloom et al., 2013; Falato et al., 2015). Considering these parts of the literature together helped us to form our expectations.

Chapter 2 contributes to the literature by asking how detailed curriculum characteristics of an individual's master's program predict both leadership and wage outcomes. The empirical analysis is meant to provide a detailed description of the people in leadership positions and the variables associated with their characteristics. The results should therefore not be interpreted causally. However, as the literature on the association between detailed educational choices and leadership is limited, a study of correlations can still be valuable because it can uncover patterns and lay ground for further research. We find that management courses and educational diversification within classical business topics are strong predictors of leadership, whereas broader diversification is insignificant in determining leadership. Moreover, we find that courses in finance and accounting are positively associated with wage outcomes. Such findings confirmed our expectations and are in line with previous research (e.g., Bertrand et al., 2010; Lazear, 2012; Bloom et al., 2013; Joensen and Nielsen, 2009).

Chapter 3, *Dropping Out of University: Estimating Peer Effects Using Randomly Assigned Groups*, also uses the detailed educational data obtained from CBS together with the Danish register data and investigates peers' impact on both students' probability of dropping out during the first year of an undergraduate program at CBS and on their first-year GPAs. While the literature on peer effects in education is extensive, most studies are concerned with peer effects in educational performance, and fewer papers investigate peer effects in tertiary education (e.g., Sacerdote, 2001; Carrell et al., 2009; Ammermueller and Pischke, 2009; Lavy et al., 2012; Carrell et al., 2013; Murphy and Weinhardt, 2014; Elsner and Isphording, 2015). Likewise, only few studies investigate the impact from peers on the probability of dropping out (Johnes and McNabb, 2004; Booij et al., 2015). I contribute to this literature by an investigation of peer effects on students' dropout decisions in tertiary education.

Because education is a significant determinant of labor market outcomes (as also seen in Chapters 1 and 2), it is important to understand the mechanism behind educational performance and decisions, which explains why peer effects in educational outcomes continue to be debated in the literature. Besides the importance for labor market outcomes, university dropouts are associated with inefficient use of time and resources for students, universities, and the broader society. For instance, when students do not complete their studies, they have spent time gaining human capital that they are likely not to use. Also, either they delayed their labor market entrance by postponing a potential graduation or they enter the labor market with a lower level of education, which is likely to be reflected in their lifetime earnings. Finally, they have taken up a space at the university that could have been used by another student. Thus, obtaining a better understanding of peer influence on the decision to drop out is important.

The main contribution of Chapter 3 is an analysis of how the ability level of a peer group is associated with individuals' probability of dropping out of university. Moreover, inspired by recent literature that investigates the influence of ability ranking on educational performance (Murphy and Weinhardt, 2014; Elsner and Isphording, 2015), Chapter 3 is also concerned with how and if ability ranking in peer groups in tertiary education affects the decision to drop out. To the best of my knowledge, no other papers have investigated this relationship. The detailed educational data from CBS provided me with information on randomly assigned peer groups and knowledge about individuals' pre-university abilities, which enabled me to overcome the econometric problems of reflection (two-way causality) and endogenous selection of peers, as described by Manski (1993).

Chapter 3 finds that women's probability of dropping out increase with peers' abilities , whereas men's are unaffected by their peers. This is especially true for women in the lower end of the ability distribution. A potential explanation for this result is the so-called "big fish, little pond" effect (BFLPE) found in the psychology literature (e.g., Marsh and Parker, 1984; Marsh and Hau, 2003). The BFLPE appears when students form their own concepts of self by comparing their academic abilities to those of their peers. Having high-ability peers could potentially make students underestimate their own abilities, which might make them feel like they are falling behind. The finding that it is women who are adversely impacted by the ability level of their peers might be explained by a tendency among women to underestimate themselves and to shy away from competition (e.g., Gneezy et al., 2003; Niederle and Vesterlund, 2007, 2010). My results also show that women who rank high in their peer groups are less likely to drop out, whereas their own ability level (measured by high school GPA) becomes insignificant in determining the probability of dropping out when the peer group rank measure is included. The latter indicates that peer group rank is a stronger determinant of dropout than own ability level, which could distort the expectations of the trade-off between cost and benefit associated with education and might make women under-invest in their own education. Overall, my results suggest that, for women, the influence from peers is significant in determining the important educational decision of dropping out, suggesting that educational institutions should take that into considerations when designing education programs.

Finally, Chapter 4, *Do Peers Matter? - Impacts of Peers on Master's Choice and Labor Market Outcomes* (a joint work with Benedikte Bjerg from University of Copenhagen), complements Chapter 3 and investigates peer impacts on master's degree choice using the same data. Because individuals' choice of master's program is reflected in both labor market outcomes and the skill composition of the labor force—and thereby in growth and productivity—it is essential to understand the mechanisms driving it. This has already been recognized in the literature (e.g., Berger, 1988; Montmarquette et al., 2002; Arcidiacono, 2004; De Giorgi et al., 2010; Zafar, 2013). However, likely because of the econometric problems related to estimating peer effects (see Manski, 1993), only a few studies have been concerned with peer effects in post-undergraduate decisions (Lyle, 2007; De Giorgi et al., 2010; Ost, 2010; Poldin et al., 2015).

Chapter 4 contributes to the literature by estimating an association between individuals' choice of master's program and peers' choice of master's program using the application of an econometric methodology normally applied in the study of social network formation in development economics; it is referred to as dyadic regression (e.g. Fafchamps and Gubert, 2007). The method of dyadic regression can be compared to the Gravity model from the international trade literature, which is commonly used to model bilateral aggregate trade flows between pairs of countries (dyads) (e.g., Bergstrand and Egger, 2011; Mayer, 2014). By means of dyadic regression together with data on randomly assigned peer groups, we overcome both the reflection and peer-selection problems (Manski, 1993). Particularly we investigate the presence of positive and negative assortative matching along multiple dimensions using dyadic regressions, including matching on peers. In the context of our study, positive assortative matching means that two students who are more similar are more likely to enroll in the same master's program and vice versa for negative assortative matching.

Among other findings, our results show indications of positive assortative matching on peers: Students randomly assigned to the same group the first year of undergraduate studies are more likely to enroll in the same master's program three years later. Our results vary across years and the effect is, however, only significant at the 10 percent level for our main year of interest. Importantly, the results from Chapter 4 also show that positive assortative matching among peers is stronger for individuals with similar abilities. Inspired by De Giorgi et al. (2010), Chapter 4 also investigates how educational performance and labor market outcomes are associated with being impacted by peers when choosing master's programs. We find no effect of following peers on educational performance and a 10 percent significant negative effect on labor market outcomes. The lack of an effect on educational performance is explained by our previous finding: namely, that positive assortative matching is much stronger among students who are similar in terms of abilities. We interpret this as the fact that following peers with a similar level of abilities might actually result in positive effects from, for instance, improved collaboration, which would cancel out the negative effects that stem from following peers while ignoring one's own abilities.

Collectively, the four chapters focus on the determinants and consequences of educational decisions at the tertiary level. I find, among other things, that educational decisions such as choice of master's program and choice of elective master's courses are influential in terms of labor market outcomes. Moreover, I find that peer effects are present in educational decisions, such as the decision to drop out during the first year of undergraduate studies and the choice of master's program. From a policy perspective, my findings can help inform policymakers about which educational fields that provide the highest return and how young people are influenced in their educational decisions.

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Chapter 1

The Returns to a Business Education

- Evidence from Danish Administrative Register Data

The Returns to a Business Education -

Evidence from Danish Administrative Register Data *

Marie Skibsted†

Anders Sørensen‡

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Abstract

Using Danish administrative register data, we estimate the labor market returns to master's degrees in business economics and management by comparing students from business economics with students from other fields in the social sciences. We address selection into fields of study using an Instrumental Variable (IV) approach, through which we introduce a novel application of an existing instrument. We hypothesize that educational maturity is important for educational decisions and use season of birth as an exogenous predictor of master's degree choice. Our results show that individuals with a master's degree in business economics and management, on average, have a wage premium of approximately 12-17% and a significantly higher probability of private sector employment. Comparing IV and Ordinary Least Squares (OLS) estimates shows that our OLS estimates are downward biased, which indicates negative selection into a business education. Finally, our results show that controlling for a master's degree in business economics does not reduce the observed gender wage gap in our data.

Keywords: return to education, business education, labor market outcomes, wage regression
JEL classifications: I21, I26, J24, J31

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†Marie Skibsted, mks.eco@cbs.dk, Department of Economics, Copenhagen Business School, Porcelaenshaven 16 A, 1st floor, DK-2000 Frederiksberg, Denmark.

‡Anders Sørensen, as.eco@cbs.dk, Department of Economics, Copenhagen Business School, Porcelaenshaven 16 A, 1st floor, DK-2000 Frederiksberg, Denmark.

1 Introduction

The idea that education is fundamental for individual productivity is well established in the literature and most economists now agree that education is increasingly important for labor market success. The labor market continues to reward highly educated employees perhaps because of the increasingly globalized economy and the corresponding high competition. Because the labor force continues to be more educated and the returns to many fields is now equivalent to the college wage premium (Kirkebøen et al., 2014), educational decisions are no longer only about the amount of education; they are actually more about the type and field of study. However, which type of skills that results in higher returns is still not completely clear, even though the literature on the returns to education continues to grow. This paper complements the literature by estimating a causal relationship between a master's degree in business economics and management and labor market outcomes, which are measured by hourly wages and the probability of private sector employment.[1] Using Danish administrative register data, we compare the labor market outcomes of individuals who obtain a master's degree in business economics and management to those of individuals who receive master's degrees in other fields in the social sciences.

Ample research examines the returns to education and covers both the returns to quantity (years) and quality (field) of education, including pioneering studies that have evaluated the impact of additional years of schooling (e.g., Angrist and Krueger, 1991; Ashenfelter and Krueger, 1994; Card, 1999). By contrast, this paper is concerned with the returns to a specific field of study (e.g., James et al., 1989; Blundell et al., 2000; Arcidiacono, 2004; Hamermesh and Donald, 2008; Buonanno and Pozzoli, 2009; Dalgaard et al., 2009; Altonji et al., 2012; Hastings et al., 2013; Kirkebøen et al., 2014). Much attention has already been directed towards understanding the relationship between wages and education, and the literature on the returns to various fields of education has been growing rapidly in recent years. To complement the existing literature, we establish a causal business education wage premium and subsequently re-estimate this wage premium every year for 10 years after graduation. We do the latter to better understand how the business education wage premium works. Moreover, we estimate the causal relationship between a business education and the probability of private sector employment. To identify the effect of a business education, we suggest a novel application of an existing instrument, which enables us to address self-selection into a master's program in business economics and management. By contrast to other studies, we limit our sample to consists of individuals that have

[1] The term "a master's degree in business economics and management" is composed of master's degrees obtained from different Danish business schools.

graduated with either a master's degree in business economics and management or a master's degree in other fields in the social sciences. This means that our estimated business wage premium is relative to having a master's degree in other fields of the social sciences.

Policymakers continue to ponder whether the association between educational choices and labor market success is only caused by self-selection or if certain types of education provide students with more productive human capital. At the micro level, research has shown that educational choices are a strong predictor of labor market outcomes (e.g., Blundell et al., 2000; Arcidiacono, 2004; Hamermesh and Donald, 2008; Lazear, 2012; Altonji et al., 2012; Joensen and Nielsen, 2015); at the macro level, cognitive skills (unlike years of education) have been shown to be an important determinant of economic growth (e.g., Hanushek and Kimko, 2000; Hanushek and Woessmann, 2012). Policymakers should thus consider these factors when designing educational policies. However, to make informed and useful policy recommendations, one needs to fully understand how educational choices are reflected in labor market performance. In particular, we need to understand whether students should be encouraged to pursue certain fields of study or if a positive association only exists for students with specific pre-determined abilities and preferences.

Educational choices are widely recognized as endogenous, which means that a causal interpretation of the estimated effect of education is difficult. Despite the vast literature on the returns to education, the question of how to determine causality is still debated, and structural estimation, selection on observables as a guide for selection on unobservables, and the exogenous variation of an instrument or in admission criteria have been among the proposed solutions (e.g., Berger, 1988; Arcidiacono, 2004; Dalgaard et al., 2009; Webber, 2014; Kirkebøen et al., 2014; Altonji et al., 2015). Acknowledging the non-random self-selection into fields of study, we address endogeneity using an Instrumental Variable (IV) approach, whereby we hypothesize that educational maturity is an important determinant of educational choices and is exogenous to labor market outcomes. Measuring educational maturity by season of birth, we use quarter-of-birth dummies as our exogenous instrument.

The problem of endogenous selection emerges if, for instance, an individual chooses a field of study that corresponds to his or her unobserved abilities. If these unobserved abilities also have an impact on future labor market outcomes, the Ordinary Least Squares (OLS) estimates are biased. If someone performs better in, for instance, the natural sciences, then he or she may be more likely to choose an education in which he or she can benefit more from these skills. Such abilities are also likely to influence a person's wage outcome and employment opportunities, which causes the OLS estimates to be upward biased. Endogenous selection can also introduce biased estimates if individuals observe wage differences across types of master's degrees and account for these differences in their educational choices. Likewise, if less-able students compensate for their shortcomings by choosing fields of study that offer higher wages or more productive human capital, such action will bias the OLS estimates downward. To address the issues that arise from self-selection into fields of study, we need an instrument that is both relevant and exogenous.

To identify the impact of business education on labor market outcomes, we start by studying the mechanisms behind these educational choices. That is, we try to understand tertiary education choices. We posit that educational maturity is an important factor in field-of-study choice in tertiary education. We follow Naylor and Sanford (1980) and define educational maturity based on the student's certainty about his or her university field and career choices. Based on this definition, we hypothesize that, if educational maturity is low, a prospective student will tend to choose a field of study with more general characteristics than a prospective student with high educational maturity. We argue that a master's degree in business economics and management at a business school is broader and has more general characteristics than similar master's programs within the social sciences at the university. Thus, we expect that students with low educational maturity will be more likely to choose a master's program in a business school. Based on this expectation, we model field-of-study choice as a function of a student's background characteristics, high school performance, and educational maturity. We expect high school performance and other background characteristics to have an impact on labor market outcomes, but educational maturity should be exogenous to future labor market outcomes, making it a potential instrument.

We assume that educational maturity is related to age. In other words, we assume that, within the same birth year, individuals who are born later in the year display, on average, less educational maturity than individuals who are born earlier in the year. Students in Denmark choose tertiary education at the same time each year, which means that students born in the same year differ in age and educational maturity when they choose their fields of study. We measure educational maturity by season of birth, and our instrument is quarter-of-birth dummies. In our estimations, we find that the later a person is born in the year, the more likely he or she is to enroll in a master's degree in business economics and management. To the best of our knowledge, this is the first paper to address the endogeneity of master's program choice using season of birth as an instrument.

We need to discuss why season of birth is exogenous to labor market outcomes. Several studies have attempted to document the negative implications of starting school relatively young in terms of both long-term and short-term outcomes (e.g., Bedard and Dhuey, 2006; McEwan and Shapiro, 2008). However, more recent studies have concluded that school starting age has no long-term effect on labor market performance and level of education (e.g., Dobkin and Ferreira, 2010; Black et al., 2011; Fredriksson and Öckert, 2013; Rockwool-Foundation, 2015).[2] In contrast with their findings for almost all other countries, Bedard and Dhuey (2006) show that season of birth does not have an impact on Danish pupils' educational performance, perhaps because Danish pupils are not tracked based on abilities until they have finished lower secondary school at 15 or 16 years of age. Additionally, a November newsletter from the Danish Rockwool Foundation Research Unit concluded that, for Danish students, starting school young does not influence the final years of their obtained education (Rockwool-Foundation, 2015). These findings are especially important for this study, as they substantiate our assumption that season of birth is exogenous to labor market outcomes in a Danish context.

Our baseline OLS results show that a master's degree in business is, on average, associated with a wage premium of approximately 6% when compared with other master's degrees in the social sciences. Applying our IV strategy, we observe an average business wage premium of approximately 12-17%, which suggests that the OLS estimates are downward biased. Additionally, our IV results show that the business wage premium increases with the years after graduation. The literature in general finds that having an education in the natural sciences, engineering, or business yields a higher return compared with other tertiary fields of study (e.g., James et al., 1989; Blundell et al., 2000; Arcidiacono, 2004; Altonji et al., 2012; Webber, 2014; Kirkebøen et al., 2014), which is in line with our results. In a slightly different branch of the literature, Bloom and Van Reenen (2007) and Bloom et al. (2013) show that differences in management practices correspond with differences in firms' performances. Because business schools often teach management to their students, one might expect that individuals with master's degrees in business economics and management have managerial skills that might help them contribute to improved management practices and, in turn, improved firm performance. If individuals contribute positively to firm performance, we expect to see their contributions reflected in higher wages, which may partly explain the observed business wage premium.

Continuing to our employment sector model, our OLS estimates show a 34 percentage-point increase in the probability of private sector employment if an individual holds a master's degree in business economics and management. Compared with our IV strategy, we see a slight increase in the estimated impact of business education (36-38%) on the probability of private sector employment. Because business universities generally teach skills that are primarily in demand in the private sector, this

[2] This question has recently been discussed in a Danish context (Dee and Sievertsen, 2015; Landersø et al., Forthcoming; Rockwool-Foundation, 2015), including recent reports in both Danish and international newspapers (e.g., Dee, 2015; Landersø and Sievertsen, 2015). We return to these studies later.

result is in line with our expectations.

We also observe a gender wage gap of approximately 20% in our sample. Our results show that the gender wage gap increases in the years following graduation, particularly when estimating for a sample of individuals with at least one child. In our estimations for the entire sample, we observe a gender wage gap of approximately 7% and 23% 1 and 10 years after graduation, respectively. Comparing men and women without children, the gender wage gap is less pronounced and is nearly constant across the years after graduation. Thus, our results suggest that the main explanation for the observed gender wage gap is having at least one child. Finally, in contrast to the literature that finds significant reductions in the gender wage gap when controlling for educational fields, we do not find that having a master's degree in business economics and management narrows the gender wage gap.

The rest of the paper is structured as follows: Section 2 introduces selected parts of the existing literature with more detailed descriptions of the findings of a few chosen papers. Section 3 presents the empirical framework and the identification strategy and Section 4 describes the data. Section 5 explains the rationale behind our instrument. Sections 6 and 7 report and discuss our results, and Section 8 presents robustness checks. Finally, Section 9 concludes the paper.

2 Related Literature

The first papers on the returns to education initially estimated the returns to additional years of schooling. The findings in the literature suggest that an extra year of schooling increases the wage outcomes by approximately 10% (e.g., Angrist and Krueger, 1991; Ashenfelter and Krueger, 1994; Card, 1999; Leigh and Ryan, 2008). More recently, returns to the quality/field of study has received the most attention, and the literature continues to increase (e.g., James et al., 1989; Blundell et al., 2000; Arcidiacono, 2004; Hamermesh and Donald, 2008; Buonanno and Pozzoli, 2009; Walker and Zhu, 2011; Altonji et al., 2012; Hastings et al., 2013; Webber, 2014; Kirkebøen et al., 2014). Overall, significant differences are found across degrees, and the findings most commonly show that degrees in engineering, business, law and natural sciences are the strongest determinants of higher wages. However, some of this literature focuses on individuals who graduated 20-30 years ago, and some of these studies fail to address the endogeneity of educational choices.[3]

Some studies concerned with the returns to specific skills use data from the US (e.g., James et al., 1989; Arcidiacono, 2004; Hamermesh and Donald, 2008). For instance, using data on male college

[3]For great reviews of the literature and discussions about causality and determinants of educational choices see Altonji et al. (2012, 2015).

graduates in the US, James et al. (1989) show that wage outcomes differ significantly across college majors but that institutional differences are not strong determinants of the variation in wages. Because of this finding, James et al. (1989) are often cited for concluding that majoring in engineering at a local college is a better private investment than enrolling in Harvard.

Hamermesh and Donald (2008) use survey data on students at the University of Texas to model the impact of college degrees on earnings; they account for non-response bias and selection into employment. They show that, compared with a major in education, majors in hard and soft business are associated with wage premiums of 48.7% and 37.8%, respectively, while a major in social sciences results in a wage premium of 27.9%. These results show a 20% log point difference between the earnings of a (hard) business major and the earnings a social science major.

Moreover, several papers use data from the United Kingdom, all of which document earnings differences across fields of study (e.g., Blundell et al., 2000; Bratti and Mancini, 2003; Walker and Zhu, 2011; Chevalier, 2011). For instance, Blundell et al. (2000) estimate the returns to higher education and to field of study. They find that having a higher degree or an undergraduate degree is generally associated with a significant wage premium compared to not going into higher education.[4] With regard to the wage differences across fields of study, they find the strongest effect for women, where degrees in education, economics, accounting and law, or "other social sciences" are associated with higher wages.

Altonji et al. (2012, 2015) conduct extensive reviews of the empirical literature on the returns to fields of study, the determinants of educational choices, and the potential ways of handling self-selection. Comparing results in the literature, they conclude that engineering consistently yields a high wage premium, usually followed by business and science, while humanities, social sciences, and education are further behind. Altonji et al. (2015) also highlight the endogenous selection into field of study and discuss selection on observables as a guide for selection on unobservables (e.g., Webber, 2014), structural estimation (e.g., Arcidiacono, 2004), and variation in access to fields (e.g., Hastings et al., 2013; Kirkebøen et al., 2014) as ways of handling this selection. Finally, when discussing the control function approach, they also note the difficulty in finding a variable that can function as an instrument (i.e., influence major choices but not wages).

Arcidiacono (2004) addresses selection differently than this paper does and estimates a dynamic choice model that accounts for college choices and major choices. He finds that students who major in math generally perform better in the labor market and that the wage premium—relative to that

[4] Similar to our study, Blundell et al. (2000) restrict their sample to individuals who obtained at least one A-level qualification. Thus. their reference group is individuals that had at least one A-level qualification, and thus the prospect of going into higher education, but who did not continue into higher education.

of those without a college education—is highest in the natural sciences and engineering, followed by business. Moreover, Arcidiacono (2004) documents a college selection process based on individual preferences for specific workplaces and majors.

More recently, studies have used a regression discontinuity approach to address the selection issue and have also documented large earnings effects across fields of study (Hastings et al., 2013; Kirkebøen et al., 2014). Using data from countries with centralized admission requirements for all university programs and with certain master's programs only accepting students with a high school GPA above a specific threshold, a regression discontinuity approach can be used to estimate the causal impact of field of study.

Using data on Norwegian students and central admission data, Kirkebøen et al. (2014) estimate the payoff of a chosen field of study compared with that of a specific next-best alternative. In particular, Kirkebøen et al. (2014) formulate a regression model with multiple treatments (multiple fields), and knowledge about students' rankings of alternative fields allows them to relax some of the strong assumptions normally required when performing IV estimation. Kirkebøen et al. (2014) find significant differences in payoffs across fields of study, with business education producing higher payoffs when compared with all other fields, except engineering and law.

Finally, at a more detailed level, a part of the literature has considered the influences of curriculum and course choices, holding degrees or educational level constant. This body of research has shown that, in particular, skills related to mathematics are positively associated with wage outcomes and that the lack of such skills might explain the gender wage gap (e.g., James et al., 1989; Hamermesh and Donald, 2008; Joensen and Nielsen, 2009; Bertrand et al., 2010; Lazear, 2012; Joensen and Nielsen, 2015).

As documented above, the literature suggests that educational choices are important contributors to the differences in labor market outcomes across individuals. Because schooling choices are significantly associated with earnings, numerous studies have aimed to explain field-of-study choice. These studies generally find that the important determinants of field-of-study choice is gender, ability, expected future earnings, peers, and individual preferences (e.g., Berger, 1988; Montmarquette et al., 2002; Arcidiacono, 2004; Ost, 2010; De Giorgi et al., 2010; Arcidiacono et al., 2012; Zafar, 2013). These findings show that field-of-study choice is non-random, which indicates that master's program choice may also be driven by unobservables. The latter underlines the need to address the corresponding endogeneity in educational choices when estimating the returns to fields of study.

3 Empirical Framework and Identification Strategy

3.1 Econometric Model

In this paper, we aim to identify the labor market return to a master's degree in business economics and management, ρ, relative to a master's degree in other fields in the social sciences. We start by specifying a wage estimation equation that includes the individual choice of field of study, which is measured by a "business dummy", D_i^{BE}. Unlike in other studies, we do not simultaneously estimate the returns to several fields (multiple treatments/fields). Our empirical specification is shown in Equation (1):

$$y_i = \beta_0 + \beta X_i + \rho D_i^{BE} + \phi_t + \alpha_l + \theta_b + \varepsilon_i \tag{1}$$

i represents individuals and y_i is either the logarithm of the hourly wage of individual i or is a dummy variable that is equal to one if individual i was employed in the private sector in 2008. X_i includes individual specific characteristics that are expected to impact wage outcomes such as gender, labor market experience, parental characteristics, age when finished high school, high school GPA etc. ϕ_t is graduation year fixed effects and are included to control for macroeconomic characteristics that might impact starting wages and, potentially, also earnings in a longer perspective. α_l are location fixed effects and are included to control for differences in job possibilities across regions in Denmark and θ_b is birth year fixed effects.[5] Finally, D_i^{BE} is a dummy variable that is equal to one if individual i graduated with a master's degree in business economics and management and is the primary variable of interest.

As discussed, if master's program choices are endogenous, including D_i^{BE} in the model introduces endogeneity and our OLS estimates will be biased. However, we start by estimating Equation (1) using standard OLS, and we present the estimates as our baseline results. We will ultimately treat D_i^{BE} as endogenous, and apply an IV procedure to identify the returns to a master's degree in business economics and management. Because our endogenous variable is a dummy, we apply an extended version of a standard IV approach and use the following two-step IV procedure, which is described in detail on page 939 of Chapter 21 in Wooldridge (2010) and is used by, for instance, Doerr et al. (2013). This two-step procedure is primarily used because it produces more efficient estimates than a standard

[5] Due to multicollinearity between birth year, age when finished high school and age in 2008, we do not include age in 2008 in the regressions. In standard wage regressions age and age^2 are considered proxies for labor experience. Because we also include a measure of labor market experience obtained from Statistic Denmark, it is not crucial to also control for age.

two-stage least square (2SLS) approach. To test robustness, we also report results from standard 2SLS estimation in Appendix B.4.[6]

The first step in this involves estimating the probability of enrolling in a master's program in a business school in Denmark. We generate these estimates using a binary choice model provided by Equation (2) and Equation (3):

$$P(D_i^{BE} = 1|X_i, Z_i) = G(X_i, Z_i; \gamma, \phi, \alpha, \theta) \tag{2}$$

$$D_i^{BE} = \begin{cases} 1 & \text{if } \gamma^0 + \gamma^x X_i + \gamma^z Z_i + \phi_t + \alpha_l + \theta_b + u_i \geq 0 \\ 0 & \text{Otherwise} \end{cases} \tag{3}$$

D_i^{BE}, X_i, ϕ_t, α_l, θ_b are defined as in Equation (1) and Z_i is a vector of our suggested instrument(s). We assume that u_i are independent and identically distributed (i.i.d.) standard normal, meaning that we use a probit model to model the probability of selecting into a master's degree in business.[7] After estimating Equation (2), we predict the fitted probabilities, which we denote $\hat{G}_i$. The second step involves using these fitted values as our instrument in the standard 2SLS framework described in Equations (4) and (5).

$$\text{1. stage: } D_i^{BE} = \lambda_0 + \lambda X_i + \nu \hat{G}_i + \phi_t + \alpha_l + \theta_b + \epsilon_i \tag{4}$$

$$\text{2. stage: } y_i = \beta_0 + \beta X_i + \rho_{iv} \hat{D}_i^{BE} + \phi_t + \alpha_l + \theta_b + \varepsilon_i \tag{5}$$

As in standard IV estimations, we need our instrument to meet two required conditions. First, the instrument should be significantly correlated with the endogenous variable it seeks to explain—i.e., the instrument should be relevant. Second, the instrument should not be related to our measures of labor market performance given the set of observable determinants that are already included—i.e., the instrument should be exogenous.[8] Because we are interested in the returns to a business education, we suggest an instrument that is a significant and exogenous determinant of the selection into a master's program in business economics and management. Relevance is testable, whereas exogeneity is a matter

[6] As is normal when performing standard IV estimations, though in contrast with the literature on heterogeneous treatment effects/local average treatment effects (LATE), we will assume that the returns to a master's degree in business economics and management is constant across all individuals in our sample. Because we have a sample of individuals who are similar in terms of educational choices (they all graduated with a master's degree in the social sciences), we believe that this assumption is plausible.

[7] Even if u_i are not i.i.d. standard normal, the estimates obtained from Equation (5) are still consistent. For more on the assumptions, see ASSUMPTION ATEIV.1' on page 939 of Wooldridge (2010).

[8] If one is willing to rely on the non-linearity of $G(\cdot)$ it is possible to identify ρ even without an exogenous instrument. However, this is *not* recommended, as discussed on page 940 of Wooldridge (2010). Moreover, in order to compare the results from the 2-step procedure to the results from standard 2SLS estimations, we need a reliable instrument.

of beliefs and intuition. In Section 5, we discuss the instrument in greater detail.

4 Data

This paper uses Danish register data. These data are maintained and administered by Statistics Denmark and cover the entire Danish population. They convey very detailed individual information, such as detailed labor market information, and information on individuals' educational backgrounds, parents, and other socioeconomic characteristics.

4.1 Sampling of Data

We restrict our sample so it is very homogeneous in terms of individuals' educational attainment. In particular, we restrict our data to include individuals with either a master's degree in business economics and management from a Danish business school or a master's degree in political science, law, sociology, anthropology, administration or economics from a Danish university. These master's programs fall under the umbrella of the social sciences.[9] By restricting our sample to individuals who have chosen similar fields of study, we have generated a sample of individuals who have similar occupational opportunities in the labor market.[10]

Furthermore, we restrict our sample to individuals who have graduated from a general high school in Denmark. When students leave primary school in Denmark, the institutional setting allows them to choose between a vocational education, a business high school, a technical vocational high school, or a more general high school (or nothing).[11] However, to be admitted to tertiary education in Denmark, students need to graduate from one of the high schools. Thus, only students who have graduated from high school are included in the sample. However, only students from the general high school are registered with high school GPAs in our data.[12] Therefore, to control for high school GPA in the regressions, we only keep these students in our sample. The exclusion of individuals with a business/technical high school education and other individuals with missing high school GPA forces us to disregard a relatively large share of the sample. However, observing high school GPAs across fields of study not only allows us to include high school GPA in the regressions but also indicates whether we

[9]Even though Statistic Denmark's official definition of social sciences also includes psychology and musical sciences, we have excluded these fields of study, as they seem less comparable with business economics.

[10]This method of sampling can be compared with the sampling done by Blundell et al. (2000) and share similarities with the methods of matching.

[11]Students can also choose a 2-year high school program that is mainly for students that are a little older and has taken the optional 10. grade. This type of high school is also considered a general high school. For an introduction to the Danish education system see the Main Appendix A of this thesis

[12]GPA from other types of high school are not registered in the data before 2000.

see positive or negative selection into business education. Finally, as one of our dependent variables is the hourly wage rate in November 2008, we only include individuals who have graduated from tertiary education before 2007. We also limited our sample to individuals that graduated before 1984.

Because one of our dependent variables is the hourly wage measured in November 2008, the individuals in our sample were wage-employed in 2008, which means that we exclude the self-employed and individuals who are outside the labor force. Additionally, we exclude individuals who had annual earnings below 200,000 Danish kroner (DKK) in 2008.[13] The exclusions ensure that we do not have individuals in our sample who were wage-employed in November 2008 but were outside the labor force the rest of the year. Robustness tests where these individuals are included back in the sample reveals, in fact, no qualitative changes to the results.[14] Finally, to limit measurement errors, we follow recommendations from Statistic Denmark and disregard observations where the hourly wage rate is unobserved or is measured imprecisely. This leaves us with a sample of 30,418 individuals who have obtained either a master's degree from a Danish business school or one of the aforementioned master's degrees in the social sciences from a Danish university and was wage employed in November 2008.

4.2 Summary Statistics

Table 1 presents summary statistics on the relevant variables for this study. In 2008, the average hourly wage was DKK 383 for individuals with a business education and DKK 343 for individuals with a degree in the social sciences from the university. As expected, we see a major difference in the earnings of men and women in 2008, with men earning, on average, an hourly wage of DKK 407 and women earning, on average, an hourly wage of DKK 300. The individuals in our sample have, on average, 12 years of labor market experience, and 86% of the individuals with a business education were hired in the private sector in 2008. By contrast, only 50% of the individuals with a master's degree in the social sciences were hired in the private sector in 2008. In our sample, 45% of the women and 28% of the men were hired in the public sector in 2008.

Forty percent of our sample graduated with a master's degree in business economics and management. Forty-four percent of the men and 34% of the women obtained a business education. Individuals were, on average, 19 years old when they finished high school. Twenty-nine percent of the men and 23% of the women took an additional non-compulsory year in the 10th grade of lower secondary school (in Denmark, only the first 9 grades of schooling are mandatory—more about this requirement later).

[13] Annual earnings covers the total wages in current year as well as tax-free wages. 1 US dollar is equal to approximately 6.53 DKK.

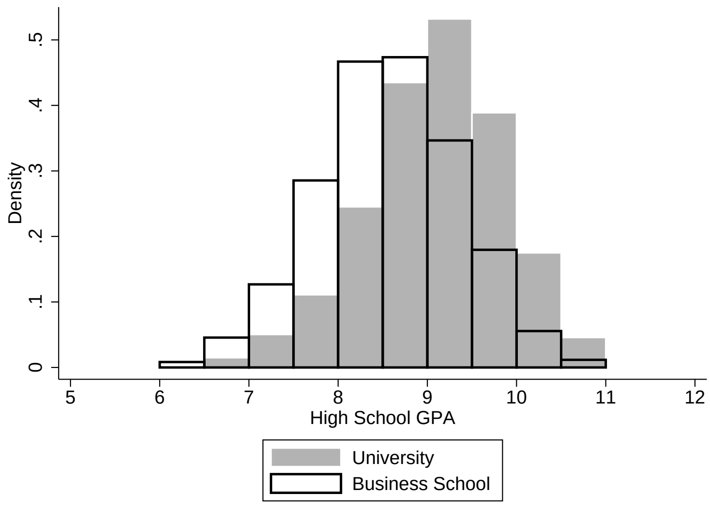
[14] In a robustness test we include these observations in the sample again, see Tables B.3 and Table B.4. The OLS results are the same and the IV results are slightly weaker in terms of significance, but offer the same conclusions.

Table 1: Summary Statistics

	All	Private Sector	Public Sector	Business educated	University educated	Men	Women
Personal characteristics:							
Dane (=1)	0.99	0.99	0.99	0.99	0.99	0.99	0.99
	(0.08)	(0.09)	(0.08)	(0.09)	(0.08)	(0.08)	(0.08)
High school GPA	8.83	8.77	8.95	8.52	9.04	8.75	8.93
	(0.83)	(0.82)	(0.82)	(0.79)	(0.78)	(0.86)	(0.78)
Standardized high school GPA	0.00	-0.08	0.14	-0.38	0.25	-0.10	0.12
	(1.00)	(0.99)	(1.00)	(0.96)	(0.95)	(1.04)	(0.94)
Age when finished high school	19.25	19.22	19.30	19.23	19.26	19.32	19.16
	(0.88)	(0.78)	(1.03)	(0.76)	(0.95)	(0.89)	(0.85)
Continued into 10. grade	0.26	0.25	0.28	0.27	0.26	0.29	0.23
	(0.44)	(0.44)	(0.45)	(0.45)	(0.44)	(0.45)	(0.42)
Gender (Male=1)	0.55	0.61	0.43	0.61	0.51		
	(0.50)	(0.49)	(0.50)	(0.49)	(0.50)		
2008: Children<18 in the family (=1)	0.65	0.63	0.68	0.63	0.66	0.63	0.66
	(0.48)	(0.48)	(0.47)	(0.48)	(0.48)	(0.48)	(0.47)
Business educated	0.40	0.53	0.15			0.44	0.34
	(0.49)	(0.50)	(0.36)			(0.50)	(0.47)
Law	0.24	0.21	0.30		0.40	0.18	0.31
	(0.43)	(0.41)	(0.46)		(0.49)	(0.38)	(0.46)
Political Science	0.10	0.05	0.19		0.17	0.10	0.10
	(0.30)	(0.22)	(0.39)		(0.38)	(0.30)	(0.30)
Economics	0.14	0.14	0.14		0.23	0.18	0.09
	(0.35)	(0.34)	(0.35)		(0.42)	(0.38)	(0.29)
Social science unknown	0.01	0.01	0.02		0.02	0.00	0.02
	(0.11)	(0.08)	(0.15)		(0.14)	(0.07)	(0.15)
Administration	0.08	0.04	0.14		0.13	0.07	0.09
	(0.27)	(0.21)	(0.34)		(0.33)	(0.25)	(0.28)
Anthropology	0.02	0.01	0.03		0.03	0.01	0.03
	(0.12)	(0.10)	(0.16)		(0.16)	(0.08)	(0.16)
Labor market characteristics:							
2008: Experience	12.03	12.12	11.86	12.33	11.83	12.59	11.35
	(5.95)	(5.99)	(5.86)	(5.95)	(5.93)	(6.08)	(5.71)
Age per 1. January 2009	38.84	38.54	39.37	38.68	38.94	39.26	38.32
	(5.90)	(5.88)	(5.89)	(5.78)	(5.97)	(5.92)	(5.83)
Hourly wage in 2008	358.48	395.87	291.01	382.58	342.74	406.93	299.66
	(268.07)	(319.95)	(98.70)	(306.83)	(238.08)	(336.33)	(124.34)
Hired in private sector	0.64			0.86	0.50	0.72	0.55
	(0.48)			(0.34)	(0.50)	(0.45)	(0.50)
Location:							
Copenhagen	0.39	0.41	0.34	0.42	0.36	0.38	0.39
	(0.49)	(0.49)	(0.47)	(0.49)	(0.48)	(0.49)	(0.49)
Zealand	0.14	0.14	0.14	0.13	0.14	0.13	0.15
	(0.34)	(0.35)	(0.34)	(0.34)	(0.35)	(0.33)	(0.36)
South Denmark	0.16	0.15	0.17	0.15	0.17	0.17	0.16
	(0.37)	(0.36)	(0.38)	(0.36)	(0.37)	(0.37)	(0.36)
Central Jutland	0.19	0.19	0.19	0.18	0.19	0.20	0.18
	(0.39)	(0.39)	(0.39)	(0.38)	(0.40)	(0.40)	(0.38)
North Jutland	0.13	0.11	0.16	0.12	0.13	0.13	0.12
	(0.33)	(0.31)	(0.36)	(0.32)	(0.34)	(0.34)	(0.33)
Parents' characteristics:							
Father's year of edu.	13.82	13.84	13.79	13.61	13.98	13.91	13.71
	(3.02)	(2.96)	(3.15)	(2.93)	(3.08)	(2.96)	(3.09)
Mother's year of edu.	13.06	13.06	13.06	12.80	13.26	13.07	13.04
	(2.96)	(2.90)	(3.08)	(2.90)	(2.99)	(2.96)	(2.97)
N	30418	19571	10847	12019	18399	16681	13737

Note: Means and standard deviations (in parentheses) are reported. 26.485 and 27.456 have information on father's and mother's years of education, respectively. In the regressions, I control for this using dummies.

Figure 1: Histograms of High School GPA Across Type of Educational Fields*



*In order to comply with the discretion rules of Statistic Denmark, we have excluded observations with high school GPA below 6 and above 11.

Until 2007, Denmark used the "13" grading scale when assigning grades to students.[15] Thus, the reported high school GPA is also computed based on this scale. On this scale, the lowest passing grade is 6, and the highest grade is 13. However, the scale does not make use of the value 12, skipping from 11 to 13, and students are almost never awarded 13. To ease the interpretation of the estimated coefficients in the regression models, we use a standardized measure of high school GPA. Particularly, we include a measure of high school GPA that has a mean of zero and a standard deviation of one in the full sample (this measure is also reported in Table 1). Table 1 shows that the average high school GPA is significantly lower for individuals with a master's degree in business economics and management compared with individuals with another university degree in the social sciences. In addition to the average, the distributions of high school GPA across fields of study differ, as seen in Figure 1. The averages reported in Table 1 and the distribution presented in Figure 1 indicate some sort of negative selection into business studies.

5 The Instrumental Variable

As discussed above, if master's program choice is endogenous, our OLS estimates will be biased. Different sources of bias have an impact on the naïve OLS estimates. For instance, if individuals select a master's program that corresponds with their unobserved abilities, then the OLS estimate of the

[15]See the Main Appendix A of this thesis for a translation of the Danish grading scale.

effect of a specific master's degree is upward biased. Likewise, if students that want to earn higher wages choose a master's program with expected higher earnings, then the OLS estimate is also biased upwards. We think of this type of selection as positive selection. By contrast, if less able individuals compensate by completing master's programs that are associated with higher wages and very productive human capital, the OLS estimates are biased downward. We think of this type of selection as negative selection. Thus, a priori, predicting the direction of the bias is difficult. To address the problem of self-selection, we treat D_i^{BE} as endogenous and employ the IV approach described above, which is why we need a valid instrument.

We use educational maturity as our instrument for master's program choice and follow Naylor and Sanford (1980) in defining educational maturity based on students' certainty about field-of-study and career choices. Our IV strategy is based on the idea that (1) individuals' educational maturity is an important determinant of field of study; (2) students with low levels of educational maturity are more prone to enroll at a business university; and (3) educational maturity is uncorrelated with unobserved factors that also influence labor market outcomes (educational maturity is uncorrelated with ε_i in Equation (1)). We assume that educational maturity can be measured by season of birth, and we create our instruments as quarter-of-birth dummy variables.

Season of birth has been used as an instrument in several other papers, though for slightly different purposes (e.g., Angrist and Krueger, 1991; Lee and Orazem, 2010). Most important is the influential paper by Angrist and Krueger (1991), who use season of birth as an instrument for years of schooling, as students in the US are eligible to drop out of school at age 16. However, the exogeneity of season of birth to labor market outcomes has since been questioned (e.g., Bound and Jaeger, 2000; Plug, 2001; Buckles and Hungerman, 2013). Thus, further discussion is needed to understand why we believe that season of birth is a plausible instrument in a Danish context. In the following section, we explain why a low level of educational maturity is associated with selection into a business university and why season of birth can be used as an exogenous measure of educational maturity to further elaborate this issue.

5.1 The Relevance of the Instrument

We hypothesize that, because a prospective student with a high level of educational maturity is more likely to know the type of career that he or she wants to pursue after completing a master's programs, such a student is also more likely to choose a more specialized education. By contrast, if prospective students have low levels of educational maturity and are very insecure about their career paths, they will generally choose an educational type with more general characteristics, as this general type of education offers more broad employment opportunities.

We argue that a master's degree in business economics and management is more general compared with those of other fields in the social sciences, which we argue are more specialized—or are at least perceived as such. For example, at Copenhagen Business School (CBS)—the largest business school in Denmark—most students are enrolled in a very broad bachelor's program that then gives them access to various master's programs. After completing a bachelor degree at CBS, students can choose a master's program in economics, finance and accounting, organization and innovation, marketing, and global business. In each field, students can specialize in an additional 3-4 tracks, which allows them to choose among 13 diverse tracks at CBS.[16] Thus, the choice of a bachelor's program at CBS does not naturally lead into a specific and specialized master's program. By contrast, choosing a bachelor's degree in the social sciences will naturally lead into the one corresponding master's program. Given these different structures, enrolling in a business school is likely more attractive to individuals who are educationally immature. Because most students enrolled in a bachelor's program in business economics proceed to a master's program in business economics, we can also model master's program choice as dependent on educational maturity.[17]

We assume that educational maturity is positively related to age, and we argue that, conditional on birth year, individuals born later in the year will be less educationally mature than individuals born earlier in the year. Comparing individuals with the same birth year, the difference in age can be almost an entire year, which we expect to manifest itself in different levels of educational maturity. The university application system in Denmark is centralized, which means that Danish prospective students, irrespective of when they are born, choose their tertiary education at the same time of the year. In Denmark, children start school in August of the year in which they turn 7, and they are likely to continue through the educational system at the same speed.[18] As such, students born in the same year are likely to differ in age and educational maturity when they choose their fields of study for tertiary education.

To measure educational maturity, we create our instrument as three binary variables that indicate quarter of birth. Because we control for birth year in our regressions, we only compare the educational maturity of individuals who were born in the same year. Table 2 shows the distribution of individuals

[16] The number of tracks depends on the year of enrollment.

[17] In Bjerge and Skibsted (2016), Chapter 4 of this thesis, we observe that more than 90% of the students who finish a bachelor's program at CBS will select into a master's program there.

[18] In 2009, the rule change to having mandatory school start at age 6. This means that from 2009, students begin school in grade 0 the year they turn 6.

into business schools and universities across seasons of birth. The share of individuals born in November and December who enrolled in a master's program at a business school is significantly higher compared with the rest of the sample.

Table 2: Business Educated Across Quarter of Birth

		Quarter of Birth				
		1	2	3	4	Total
Business educated (in %)	No	61.25	60.01	60.85	59.80	60.49
	Yes	38.75	39.99	39.15	40.20	39.51

		Month of Birth						
		January-February	March-April	May-June	July-August	September-October	November-December	Total
Business educated (in %)	No	60.66	60.96	60.18	60.80	61.48	58.58	60.49
	Yes	39.34	39.04	39.82	39.20	38.52	41.42	39.51

Even if they are born in the same year, some Danish students do not follow the same time path as their peers for a couple of reasons. First, in Denmark, some students might stay an additional year in the primary/lower secondary school system. This extra year is non-compulsory and is intended to benefit students who are not ready to proceed to high school. If students born late in the year are more likely to spend an extra year in the lower secondary school system, they will also be more likely to be a year older when they choose their tertiary education, which may weaken our instrument. To handle this potential snag, we include a dummy for students who spend an extra year in secondary school (10^{th} grade).

Second, the school cut-off rules are not strictly followed in Denmark. Some parents tend to delay their children's school entries if they think that their children are not ready for school. If students born later in the year are more likely to postpone entry into primary school, they will be a year older when they choose their tertiary education, which may weaken our instrument. Unfortunately, we do not have information in our data to control for delayed school entry. As a second-best alternative, we control for students' ages when they complete high school.

Finally, students may choose not to continue directly from high school into tertiary education. Again, if students born later in the year are less likely to directly enroll in tertiary education, our instrument will be weakened. The latter issue is unlikely to be of great concern, whereas the postponed school entry could present a problem. However, despite these issues, our instrument is still a strong predictor of master's program choice, as we will see in the following section.

5.2 Season of Birth as a Predictor of Master's Program Choice

To more stringently and analytically assess the relevance of season of birth as a determinant of educational choices, we present the results from our estimations of the probability of selecting a master's degree in business, i.e., the results from the estimations of the probit model captured in Equations (2) and (3):

$$P(D_i^{BE} = 1|X_i, Z_i) = G(X_i, Z_i; \gamma, \phi, \alpha, \theta) \tag{2}$$

$$D_i^{BE} = \begin{cases} 1 & \text{if} \quad \gamma^x X_i + \gamma^z Z_i + \phi_t + \alpha_l + \theta_b + u_i \geq 0 \\ 0 & \text{Otherwise} \end{cases} \tag{3}$$

Table 3 shows the results and the χ^2 test statistics from testing H_0, where $H_0 : \gamma^z_{Q2} = 0, \gamma^z_{Q3} = 0, \gamma^z_{Q4} = 0$. In this paper, +, *, and ** denote significance at the 10%, 5%, and 1% level, respectively. Across all the specifications, the three quarter-of-birth dummies are jointly significant at the 1% level. Moreover, one by one, the instrument dummies are also significant at the 1% or 5% level. Thus, educational maturity measured by quarter of birth is significant in determining the field of tertiary education. As an alternative instrument, we use a variable that counts the days between January 1 and the student's birthday. Thus, this variable ranges in value from zero and 365. When estimating Equation (2) with Z_i =*Days Between January 1 and Birthday (divided by 100)*, the instrument is an equally strong predictor of master's program choice, as can be seen in Table 3.

The sign of the estimated coefficients also confirms our hypothesis that individuals born later in the year are more likely to choose a master's degree in business compared with individuals born in the first three months of the year.[19] In addition to the significance level and the sign of γ^z, we notice that men are more likely than women to enroll in a business school; parents' years of education negatively affect the enrollment in a business program; and high school GPA and enrollment in a business program are negatively correlated. That high school GPA enters with a negative sign indicates negative selection into a business program.

Finally, the inclusion of post-treatment characteristics in columns (2)-(5) seemingly does not have an impact our instruments' prediction power, and the estimated coefficients for the quarter-of-birth dummies remain practically unchanged across the 5 specifications. In other words, the inclusion of post-treatment controls does not change the magnitude of the estimated coefficients for the three

[19] The reported coefficients are not marginal effects, but the sign of the marginal effects is the same as the sign of the estimated coefficient in the probit model.

quarter-of-birth dummies. This lack of change suggests that quarter of birth is uncorrelated with the variables that we expect to predict wage outcomes, which supports the assumption of exogeneity.

Table 3: The Selection into a Business Education

	Probit Estimation							
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Born 2. quarter	0.069**	0.068**	0.071**	0.071**				
	(0.022)	(0.022)	(0.022)	(0.022)				
Born 3. quarter	0.058**	0.057*	0.062**	0.062**				
	(0.022)	(0.023)	(0.023)	(0.023)				
Born 4. quarter	0.094**	0.091**	0.098**	0.098**				
	(0.023)	(0.024)	(0.024)	(0.024)				
Days Between 1. January and Birthday (divided by 100)					0.032**	0.031**	0.034**	0.034**
					(0.008)	(0.008)	(0.008)	(0.008)
Age when finished High School	0.016	0.025*	0.033**	0.033**	0.018	0.027*	0.035**	0.035**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)
Continued into 10. grade	0.039+	0.052*	0.056**	0.055*	0.036+	0.050*	0.054*	0.052*
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Standardized High School GPA	-0.533**	-0.542**	-0.542**	-0.542**	-0.533**	-0.542**	-0.542**	-0.542**
	(0.009)	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)
Dane (=1)	-0.324**	-0.233*	-0.239*	-0.240*	-0.323**	-0.232*	-0.237*	-0.239*
	(0.098)	(0.098)	(0.098)	(0.098)	(0.098)	(0.098)	(0.098)	(0.098)
Gender (Male=1)	0.186**	0.151**	0.182**	0.147**	0.185**	0.151**	0.182**	0.147**
	(0.016)	(0.027)	(0.016)	(0.027)	(0.016)	(0.027)	(0.016)	(0.027)
2008: Children<18 in the family (=1)		-0.103**		-0.107**		-0.102**		-0.107**
		(0.027)		(0.027)		(0.027)		(0.027)
Gender (Male=1) * Children (=1)		0.059+		0.047		0.059+		0.047
		(0.033)		(0.033)		(0.033)		(0.033)
2008: Experience			0.020**	0.021**			0.020**	0.021**
			(0.003)	(0.003)			(0.003)	(0.003)
Father's year of edu.	-0.009**	-0.013**	-0.012**	-0.012**	-0.009**	-0.013**	-0.012**	-0.011**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mother's year of edu.	-0.021**	-0.024**	-0.024**	-0.024**	-0.021**	-0.024**	-0.024**	-0.024**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2	17.907	16.971	19.366	19.236	16.329	15.503	18.084	17.887
p	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is a dummy that is equal to 1 if an individual enrolled in a master's in business economics and management. χ^2 and p comes from testing the (joint) significance of the instruments. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Table 4 shows the results from estimating the selection equation with a Linear Probability Model (LPM) and reports the F-statistics from testing the joint significance of the instruments.[20] These results and F-statistics help us assess the strength of our instruments in a more traditional way by comparing the F-statistic of the joint test to the rule-of-thumb threshold of 10. Testing $H_0 : \gamma^z_{Q2} = 0, \gamma^z_{Q3} = 0, \gamma^z_{Q4} = 0$ reveals an F-statistic of approximately 5.5, which we would prefer to be at least 10. However, using the instrument Z_i =*Days Between January 1 and Birthday (divided by 100)* reveals F-statistics above 10 and indicates that season of birth measured by Z_{Days} is a stronger instrument. Because the different definitions of our instrument provide us with the same main results—when applying the described two-step method (see Tables B.7 and B.8) and when performing standard 2SLS estimations (see Tables B.9 and B.11)—we feel confident about the strength of our instrument. Moreover, when we use $\hat{G}_i$ as our direct instrument, we observe F-statistics that are well above 10 for both measures of educational maturity.

Table 4: The Selection into a Business Education

	Linear Probability Model							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Born 2. quarter	0.021**	0.020**	0.021**	0.021**				
	(0.007)	(0.007)	(0.007)	(0.007)				
Born 3. quarter	0.017*	0.016*	0.018*	0.018*				
	(0.007)	(0.007)	(0.007)	(0.007)				
Born 4. quarter	0.029**	0.027**	0.030**	0.029**				
	(0.007)	(0.007)	(0.007)	(0.007)				
Days Between 1. January and Birthday (divided by 100)					0.010**	0.009**	0.010**	0.010**
					(0.003)	(0.003)	(0.003)	(0.003)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F	5.498	5.066	5.817	5.790	15.258	14.139	16.474	16.430
p	0.001	0.002	0.001	0.001	0.000	0.000	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is a dummy that is equal to 1 if an individual enrolled in a master's in business economics and management. F and p comes from testing the (joint) significance of the instruments. Controls as in Table 3 are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

[20]Table A.2 in addition also reports the results on all the controls included.

5.3 The Exogeneity of the Instrument

The assumption that season of birth is uncorrelated with the error term in the main equation can be challenged. That is, if season of birth is correlated with unobserved abilities that are also important for labor market outcomes, the exogeneity assumption fails. We cannot test whether our instrument is appropriately exogenous; thus, we need to be sufficiently convinced of its exogeneity. First, the assumption is supported because our data allows us to control for a rich set of specific characteristics that determine labor market outcomes, including high school GPA and parents' years of education. In what follows, we argue that, because season of birth is uncorrelated with observed outcomes (e.g., school performance, final educational level, and later labor market outcomes) in a Danish context, one can reasonably assume that season of birth is also uncorrelated with unobservables that explain labor market outcomes.

Some findings suggest that individuals born later in the year perform, on average, worse in primary school and that they are more likely to commit crimes and have mental health problems, which does not support our exogeneity assumption (e.g., Bedard and Dhuey, 2006; McEwan and Shapiro, 2008; Black et al., 2011; Elder, 2010; Dee and Sievertsen, 2015; Landersø et al., Forthcoming).[21] However, the results regarding the implications of school starting ages for individuals' educational performances, labor market outcomes, and mental health continues to be discussed in the literature (e.g., Bedard and Dhuey, 2006; Black et al., 2011; Fredriksson and Öckert, 2013; Dee and Sievertsen, 2015). In contrast with studies that show that starting school relatively older leads to substantially better performance on school tests, more recent studies have shown that the measured benefits from relatively older school starting ages only emerge because students' ages at the time of the test differ and are not caused by benefits directly related to an older school starting age (e.g., Black et al., 2011; Rockwool-Foundation, 2015). In fact, using Norwegian data, Black et al. (2011) find a small significant negative effect of school starting age on an IQ test taken at age 18 but a strong positive effect of age at test date.

In an analysis of fourth and eighth graders across OECD countries, Bedard and Dhuey (2006) show that the youngest students score substantially lower than their oldest counterparts in both the fourth and the eighth grade. However, Bedard and Dhuey (2006) find no evidence of relative age effects on test scores in eighth grade in Denmark and Finland. They argue that relative age has no effect on

[21] The potential reasons that starting age influences in-school performance, including the advantages of being relatively and absolutely mature, are discussed in the literature. The advantages of students' relative maturity is the potential benefits of simply being older, as such students are more developed than their younger classmates. The advantages of absolute maturity refer to students who benefit from being older because the educational system is better suited for older children (Dee and Sievertsen, 2015). If skill accumulation at an early age is positively associated with learning later in life, a student's relative age at the beginning of his or her educational career might have long lasting effects on his or her performance.

performance in Denmark because students in Denmark are not differentiated based on abilities and grades until they have finished lower secondary school at 15 or 16 years of age. Fredriksson and Öckert (2013) use Swedish administrative data to estimate the effect of school starting age on educational attainment and long-run labor market outcomes. Along the same lines of Bedard and Dhuey (2006), Fredriksson and Öckert (2013) find only a small effect of a child's school starting age on educational attainment when tracking is delayed until age 16.

Moreover, in November 2015, the monthly newsletter from the Danish Rockwool Foundation Research Unit reported that starting primary school at a young age does not influence Danes' final years of education (Rockwool-Foundation, 2015). The findings of Bedard and Dhuey (2006), Fredriksson and Öckert (2013), and Rockwool-Foundation (2015) suggest that season of birth is uncorrelated with observable individual-level educational outcomes in a Danish context, which supports the assumption that season of birth does not explain labor market outcomes through unobserved abilities that are acquired through pre-university education.

The potential associations between school starting age and both mental health and the propensity to commit crime could pose a problem for our exogeneity assumption.[22] Dee and Sievertsen (2015) find that a one-year delay in school entry significantly reduces the probability of observing inattention/hyperactivity in 7- and 11-year-old children. However, they do not find strong evidence of its effect on any other measures of mental health. Additionally, Black et al. (2011) find that boys who start school later are less likely to have poor mental health at age 18, but the magnitude of this effect is very small.[23] By contrast, using Danish data, Dalsgaard et al. (2012) find no support for the claim that children who are relatively old for their grade are less likely to be diagnosed with attention deficit hyperactivity disorder (ADHD).[24] Because we only consider students that successfully graduated from both high school and university, instances of poor mental health and criminal behavior are likely to be limited in our data. Moreover, studies have shown that the adverse consequences of relatively early school entry, if they exist, do not persist into later labor market outcomes (e.g., Dobkin and Ferreira, 2010; Black et al., 2011; Fredriksson and Öckert, 2013).

Dobkin and Ferreira (2010) find no effect of early school entry on adult outcomes, such as employ-

[22] Also the assosiation between school starting age and criminal behavior could be an issue for our identification strategy. For instance, using Danish data, Landersø et al. (Forthcoming) show that school starting age has an effect on criminal behavior in the late teens and early 20s. They suggest that primary school helps girls avoid criminal behavior and that high school keeps boys from committing crime. Thus, the effect of school starting age is seemingly caused by incapacitation rather than a developmental effect, which, despite the significant results, favors our exogeneity assumption.

[23] Black et al. (2011) measure mental health by a psychologist's assessment of a patients' suitability for military service at the age 18.

[24] The results of Dalsgaard et al. (2012) indicate that when diagnosing in some countries is performed by non-specialists, they are more prone to make relative diagnoses by comparing children in the same classes rather than by making objective diagnoses, which explains the documented relationship between ADHD diagnoses and early school entry.

ment rates, wages, and home ownership. In fact, Black et al. (2011) find that starting school at an older age has a negative short-run effect on earnings, which is consistent with the claim that starting school later reduces labor market experience. However, Black et al. (2011) find that this negative effect of later school entry only persists until age 30. Fredriksson and Öckert (2013) define the prime age as falling between 25 and 54 and show that, on average, prime age earnings are unaffected over the life cycle, except for those whose parents have lower levels of education and, to some extent, for women.

Finally, season of birth has also been suggested to possibly be correlated with unobserved characteristics in the mother (or father) that may also have an impact on labor market outcomes. Buckles and Hungerman (2013) find that women who give birth during the winter differ from other women because they are younger, less educated, and less likely to be married. If unobserved characteristics in the mother (or the father) determine season of pregnancy/birth and also play an important role in the child's labor market outcomes, season of birth is not exogenous. However, as Buckles and Hungerman (2013) conduct their study using data from the US—a country that is very different from Denmark in terms of inequality and socioeconomic context—the same pattern will likely not emerge in Denmark. Additionally, Table 5 shows the differences in parents' years of education across the child's quarter of birth. The t-test results show no statistically significant difference-in-means across the quarter-of-birth groups. Finally, as our data allow us to control for parents' years of education, the potential problem suggested by Buckles and Hungerman (2013) should be mitigated.

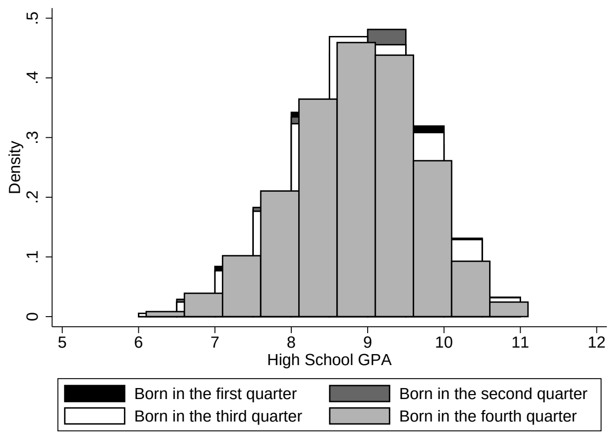
Table 5: Summary Statistics Across Quarter of Birth

Difference between:	**Q1 and rest**		**Q2 and rest**		**Q3 and rest**		**Q4 and rest**	
	Diff_1	p	Diff_2	p	Diff_3	p	Diff_4	p
High School GPA	-0.004	0.696	-0.002	0.873	-0.010	0.343	0.018	0.118
Hourly wage in 2008	2.287	0.518	4.056	0.239	1.044	0.768	-8.252	0.025
Father's year of edu.	0.042	0.479	-0.048	0.411	-0.122	0.041	0.140	0.024
Mother's year of edu.	0.013	0.786	0.106	0.024	-0.121	0.012	-0.005	0.913

Note: $\text{Diff}_i = \mu_{sample} - \mu_{Qi}$ where $i = 1, 2, 3, 4$. p is the p-value from testing the hypothesis of no mean-difference. *** p<0.01, ** p<0.05, + p<0.1.

In addition to the evidence in the literature, our data offer additional support for the exogeneity assumption. Table 5 reveals no significant difference across quarters of birth in the average hourly wage in 2008 or in high school GPA. Figure 2 shows histograms of high school GPA across the sample of students born in the first, second, third and fourth quarters. No noticeable difference is observed in the distributions. Performing a two-sample Kolmogorov-Smirnov equality-of-distributions test between the first quarter and the remaining quarters, the second quarter and the remaining quarters, the third quarter and the remaining quarters, and the fourth quarter and the remaining quarters, the test fails to reject the hypothesis that the two distributions are equal. Finally, controlling for birth year and year of high school graduation, we find no evidence of a significant impact of season of birth on high school GPA, as seen in Table A.3 in the Appendix.

Figure 2: Histograms of High School GPA Across Quarter of Birth



*In order to comply with the discretion rules of Statistic Denmark, we have excluded observations with high school GPA below 6 and above 11.

Thus, both the presented results and the findings in our data suggest that Danish students' season of birth does not significantly correlate with their parents' years of education, hourly wages, and educational performance; therefore, we feel confident in assuming that our instrument does not correlate with labor market outcomes through unobservables.

6 Results: The Business Education Wage Premium

Table 6 shows results from estimating Equation (1) with the OLS (columns (1)-(4)) and Equation (5) with the IV (columns (5)-(8)) approaches. Each column includes different control variables that we expect to have an impact on wage outcomes. The OLS estimates of Equation (1) show that a master's degree in business economics and management is, on average, associated with a wage premium of approximately 6% compared with the wages associated with a master's degree in other areas in the social sciences. As discussed, the OLS estimates are biased, and the IV estimates presented in column (5)-(8) of Table 6 try to address that. However, despite the endogeneity, the OLS results are interesting for comparative purposes and offer an opportunity to assess the direction of potential selection bias.

Table 6: The Return to a Business Education - Wage Estimations

	OLS estimation				2-step IV estimation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.066**	0.062**	0.055**	0.055**	0.154**	0.172**	0.147**	0.155**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.027)	(0.027)	(0.027)	(0.027)
Age when finished High School	-0.019**	-0.018**	-0.012**	-0.013**	-0.019**	-0.019**	-0.013**	-0.014**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	-0.043**	-0.041**	-0.039**	-0.039**	-0.044**	-0.043**	-0.040**	-0.040**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Standardized High School GPA	0.041**	0.040**	0.039**	0.040**	0.056**	0.059**	0.055**	0.057**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)	(0.005)
Dane (=1)	-0.014	-0.008	-0.010	-0.014	-0.005	-0.000	-0.004	-0.007
	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.023)	(0.023)	(0.023)
Gender (Male=1)	0.207**	0.105**	0.200**	0.103**	0.202**	0.099**	0.194**	0.098**
	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)
2008: Children<18 in the family (=1)		-0.053**		-0.056**		-0.049**		-0.053**
		(0.005)		(0.005)		(0.006)		(0.005)
Gender (Male=1) * Children (=1)		0.163**		0.153**		0.161**		0.152**
		(0.008)		(0.008)		(0.008)		(0.008)
2008: Experience			0.017**	0.016**			0.016**	0.015**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.002*	0.001+	0.002**	0.002**	0.002**	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.002**	0.002*	0.002**	0.002**	0.003**	0.003**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2					17.907	16.971	19.366	19.236
p					0.000	0.001	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is the logarithm of the hourly wage measured in November of 2008. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. χ^2 and p come from testing H_0, where $H_0 : \gamma_{Q2} = 0, \gamma_{Q3} = 0, \gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

The IV estimates show that a master's degree in business economics and management leads to an hourly wage premium of approximately 12-17% when compared with wage outcomes of graduates in the social sciences. The estimated business education effect is significant at the 1% level using both the OLS and IV estimation techniques. Most papers that examine the returns to majors use different reference groups, different methodological approaches, and different classifications; therefore, an exact comparison of the results is impossible (as also noted by Hamermesh and Donald (2008) and Altonji et al. (2012)). However, to validate our results, we do make a few comparisons.

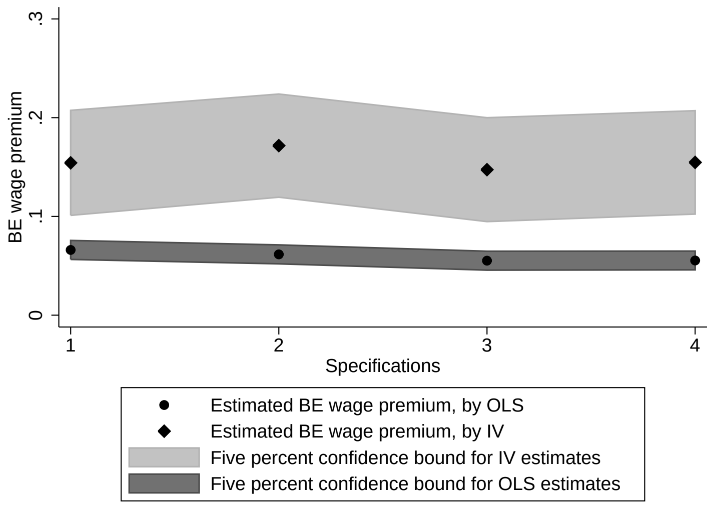
When comparing education majors with hard business majors, soft business majors and social science majors, Hamermesh and Donald (2008) find wage premiums of 48.7%, 37.8%, and 27.9%, respectively. Thus, the results of Hamermesh and Donald (2008) find a 20% log-point difference between the earnings of (hard) business majors and a social science majors. Comparing these results to the results in Table 6, a wage difference of approximately 15% does not appear overly large. Hamermesh and Donald (2008) do not account for the endogeneity of major choice and expect positive selection into majors, which means that their estimates are likely upward biased. The lack of a control for the self-selection into majors and the anticipated positive selection might explain why Hamermesh and Donald (2008) find a larger wage gap between social science and business majors than we do.

Using data from the American Community Survey Altonji et al. (2012) present simple OLS estimates of the returns to various majors. Controlling for occupation type, Altonji et al. (2012) find that men with a business education receive, on average, 14% higher wages when compared with men with a general education. Without the occupation controls, the business wage premium is as large as 33.9%. An accounting major presents an even higher wage premium. In general, Altonji et al. (2012) find that the same majors offer lower wage premiums for women than for men.

As discussed in Section 5, determining the direction of the bias a priori is difficult, as the OLS estimates may be upward biased because of positive selection and downward biased because of negative selection. A stringent comparison of coefficients across un-nested models is impossible, which makes comparing the IV and OLS estimates a complicated task. Therefore, comparing confidence intervals and coefficient estimates is our best option when assessing the direction of the bias. Figure 3 graphically presents the estimated business wage premiums, along with the corresponding confidence intervals, across models and estimation methods. The OLS estimates underestimate the impact of a business education on wage outcomes, as seen in Figure 3. This finding indicates negative selection into business schools, where less able individuals may compensate by selecting into business master's programs, which will cause the OLS to underestimate the wage premium.

Figure 1, Table 1, and Table 3 also indicate negative selection into business education. Table 1 shows that, on average, individuals who enroll in a master's program in business have lower high school GPAs compared with those who enroll in other master's programs in the social sciences. Table 3 shows a negative and significant correlation between high school GPA and selection into business school. Finally, Figure 1 shows the distribution of high school GPA across business and social science students and offers the same conclusion.

Figure 3: Estimated Business Wage Premium Across Model Specifications



Our results also show a gender wage gap of 20%, which decreases to 10% when controlling for children younger than 18 in the household. Moreover, we observe a positive and significant wage premium effect for men with children in the household. Similar results have also been documented in the literature. Finally, increasing high school GPA by one percentage point is associated with a 4% increase in the hourly wage, whereas students who spend an extra year in lower secondary education (10^{th} grade) receive significantly lower wages than their counterparts. As expected, our measure of experience enters the model with a significant and positive coefficient.

6.1 Wage Premium and Years After Graduation

In this section, we estimate the business education wage premium across years after graduation to better understand how the wage premium works. In particular, we measure an individual's hourly wages from 1 to 10 years after graduation. In doing so, we allow all individuals to have been available on the labor market for the same amount of time, thereby indirectly controlling for any post-treatment variables that are related to the labor market without including them directly in the equation.[25]

[25]To make wages comparable across years, we inflation-adjust all wages with 2000 as the baseline year. Figure A.3 shows the average hourly wage across years after graduation.

Table 7: The Return to a Business Education - Wage Estimations

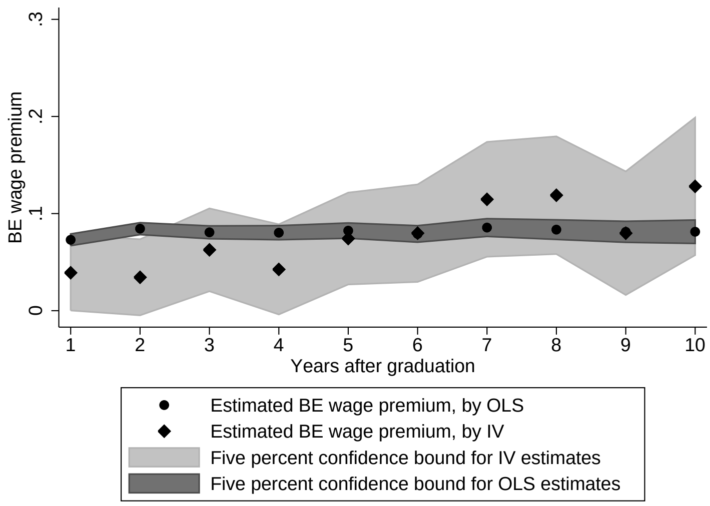
	OLS estimation									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Wages measured year after graduation	1 year	2 year	3 year	4 year	5 year	6 year	7 year	8 year	9 year	10 year
Business educated	0.073**	0.084**	0.081**	0.080**	0.082**	0.079**	0.086**	0.083**	0.081**	0.081**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Age when finished High School	-0.005**	-0.008**	-0.009**	-0.009**	-0.012**	-0.013**	-0.014**	-0.013**	-0.017**	-0.017**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
Continued into 10. grade	-0.011**	-0.016**	-0.029**	-0.035**	-0.042**	-0.040**	-0.044**	-0.046**	-0.045**	-0.049**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)
Standardized High School GPA	0.008**	0.016**	0.020**	0.025**	0.029**	0.031**	0.036**	0.041**	0.044**	0.042**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Dane (=1)	0.008	-0.009	0.005	-0.022	-0.018	0.000	0.008	0.020	0.009	0.016
	(0.019)	(0.018)	(0.019)	(0.022)	(0.024)	(0.026)	(0.029)	(0.034)	(0.040)	(0.042)
Gender (Male=1)	0.068**	0.092**	0.116**	0.133**	0.151**	0.169**	0.186**	0.202**	0.217**	0.228**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Father's year of edu.	0.002**	0.002**	0.003**	0.002**	0.002**	0.003**	0.003**	0.003**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	-0.001	0.000	0.001	0.001+	0.002**	0.003**	0.002*	0.003**	0.004**	0.005**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	2-step IV estimation									
Business educated	0.039*	0.034+	0.063**	0.043+	0.074**	0.080**	0.115**	0.119**	0.080*	0.128**
	(0.020)	(0.020)	(0.022)	(0.024)	(0.024)	(0.026)	(0.030)	(0.031)	(0.032)	(0.036)
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs	25663	26786	26999	25467	24050	22581	21168	19931	18628	17171

Note: The dependent variable is the logarithm of the hourly wage measured in November 1-10 years after graduation. All controls are also included in the IV estimations. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. In all estimations, we use predicted probabilities obtained by estimating a specification of the selection equation that is equal to the specification in column (1) of Table 3. Thus, χ^2 and p values from testing H_0, where $H_0 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$, can be seen in Table 3. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Table 7 shows the estimation results, and Figure 4 shows the estimated business wage premium, with the corresponding confidence intervals across years after graduation. Our OLS results show a constant and statistically significant business education wage premium of approximately 8% across all years after graduation. The IV estimations show slightly different results. The wage premium from a master's degree in business is increasing with the years after graduation. The estimated business wage premium is approximately 4% one year after graduation and is only statistically significant at the 10% level. By contrast, the business wage premium is 13% 10 years after graduation and statistically significant at the 1% level.

The estimated business education wage premium might increase because a master's degree in business economics provides an individual with more employment opportunities and, in turn, work through enhanced experience. In addition, the value of the skills learned in business school perhaps increases with labor market experience, as individuals learn how to better use their acquired skills in a labor market context. The estimated business wage premium observed in Table 6 is approximately 15%, which corresponds well with this explanation because the results in Table 6 rely on a sample that includes individuals with, on average, more than 10 years of labor market experience (see summary statistics in Table 1).

Figure 4: Estimated Business Wage Premium Across Years After Graduation



If hourly wage outcomes are a measure of productivity, the findings in Tables 6 and 7 suggest that a business education provides students with more productive human capital than other fields in the social sciences do and that the returns to these skills increase with the years on the labor market. The studies of Bloom and Van Reenen (2007) and Bennedsen et al. (2006), among others, show that the CEO and the differences in management practices can explain differences in firm performance. Particularly with regard to business programs, this type of education might provide the degree holder with certain managerial abilities that manifest themselves in improved firm productivity and, in turn, higher wages.

However, business school graduates are also more likely to obtain private sector employment, where wages, on average, are higher, which may also explain the observed wage premium. Table 1 shows that, in 2008, the average hourly wages in the private sector and in the public sector were DKK 395.87 and DKK 291.01, respectively. In Table 1, we can also observe how business school graduates are much more likely to work in the private sector (86%, i.e., 10,388 individuals, in the private sector and 14%, i.e., 1,631 individuals, in the public sector). This finding indicates that the high probability of private sector employment is likely to explain the estimated business wage premium. Additionally, Altonji et al. (2012) show a major decrease in the impact of any major when occupation controls are included in the model. We do not include a private sector dummy in the model because it introduces additional endogeneity (selection into private sector employment). However, to better understand the estimated business education wage premium, Section 7 models the probability of private sector employment as dependent on education type.

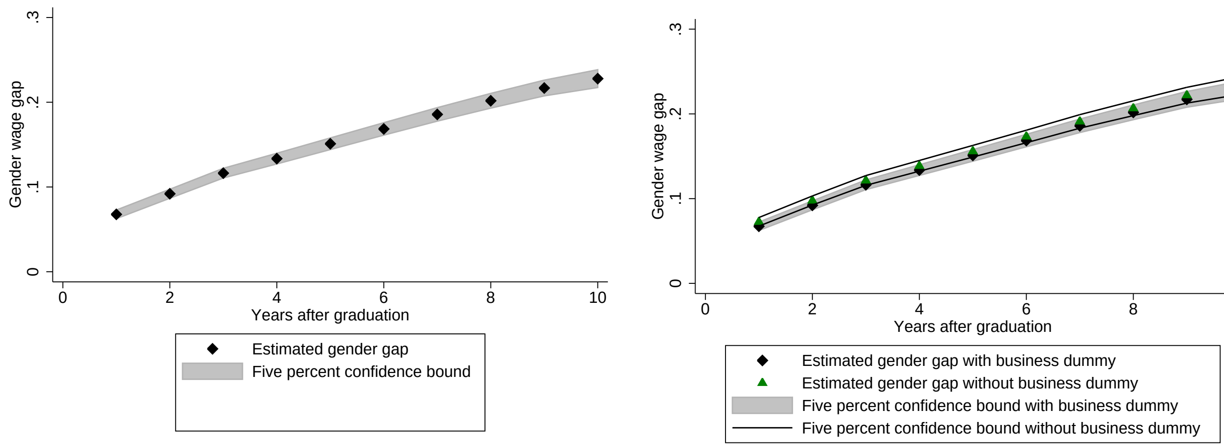
6.2 The Gender Wage Gap

In addition to the estimated wage premium, the results in Table 7 also reveal a gender wage gap. In particular, we observe an estimated gender wage gap of 7% in the first year after graduation, 15% in the fifth year after graduation, and as high as 23% in the tenth year after graduation. Thus, the gender wage gap more than doubles in the first five years after graduation. Figure 5 presents these findings graphically.

The gender wage gap is not an unknown phenomenon and has been documented many times, with a growing body of literature that shows that the gender wage gap can, to some extent, be explained by differences in educational attainment (e.g., Bertrand et al., 2010; Joensen and Nielsen, 2015). For instance, using a sample of MBAs who graduated from the Booth School of Business (at the University of Chicago) between 1990 and 2006, Bertrand et al. (2010) show that, when controlling for differences in business school courses and grades, differences in career interruption, and differences in weekly hours worked, the gender wage gap disappears. Given these results, the estimated gender wage gap in Table 7 is somewhat surprising, as we consider a very homogeneous sample with individuals who have received the same level of education.

Figure 5: Estimated Gender Wage Gap Across Years After Graduation

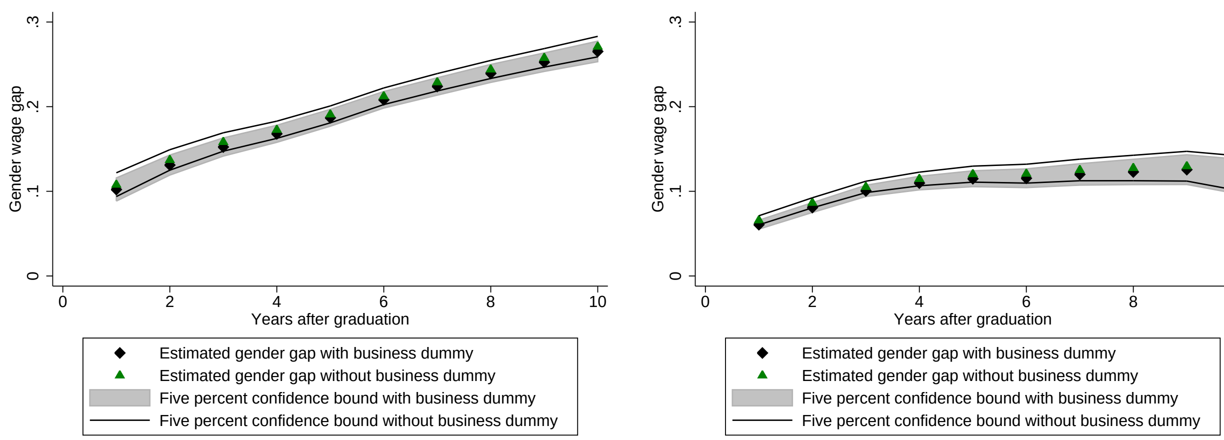
(a) Estimated Gender Wage Gap **(b) Estimated Gender Wage Gap - With and Without D^{BE} Included**



To test the impacts of field of study on the gender wage gap, we estimate the model with and without D^{BE}. In Figure 5b, we show the estimated gender wage gap across models with and without the business education dummy. The figure shows that the gender wage gap only decreases slightly after controlling for the type of master's degree (including D^{BE} in the regression).

Figure 6: Estimated Gender Wage Gap Across Years After Graduation - Comparing Individuals With and Without Children

(a) Comparing Individuals With Children **(b) Comparing Individuals Without Children**



In addition to field of study, Bertrand et al. (2010) show that differences in career interruptions, such as maternity leave, explain a large part of the gender wage gap. To understand the impact of children on the estimated gender wage gap, we re-estimate the wage equation with two samples, namely, individuals with and without children "x" years after graduation.[26] Figure 6 presents the results. Figures 6a and 6b clearly show that children explain a large part of both the initial gender wage gap and its subsequent increase, even though individuals in our sample are very similar in terms of education. This result also corresponds well with the results presented in Table 6, which reports a gender wage gap of 10-20%, with men receiving a wage premium associated with children in the household and women receiving a wage penalty for having children in the household. Because we do not include any post-treatment variable, we have not controlled for weekly hours worked or job sector. Thus, these factors may also partly explain the gender wage gap and the observed increase across years after graduation, as seen in Bertrand et al. (2010).

Using Danish data, Nielsen et al. (2004) show that women in the private sector are punished much more for birth-related leave compared with women in the public sector; they argue that seeking employment in the public sector might in fact be a rational choice for women. Thus, it might be that some women who expect to have children in the near future deliberately self-select into the public sector, where they experience more family-friendly policies and a much smaller birth-related wage penalty. This could potentially explain the gender wage gap that appears one year after graduation as this self-selection into the public sector could introduces a lower starting wage and a corresponding wage gap between recent graduates.

7 Results: Private Sector Employment

Eighty-seven percent of the business graduates in our samples were hired in the private sector in 2008 (see Table 1 on page 29), which could explain the estimated wage gap between individuals with master's degrees in business economics and management and individuals with master's degrees in the social sciences. Unsurprisingly, business schools supply more employees to the private sector than universities, but different factors may contribute to this increased probability of private sector employment. The profile and curriculum at a business school may be better suited to the private sector, which makes individuals with a master's degree in business more attractive to private sector employers, or students may already know that they prefer to be employed in the private sector before enrolling in a business school. Again, the question concerns causality and understanding whether students decided to seek private employment before enrolling in a business school or become interested in private sector

[26] Figure A.1 in the appendix show the share of individuals with children across years after graduation.

employment after enrolling is important. As the Danish labor market is very segregated in terms of gender, with more women employed in the public sector and more men employed in the private sector, knowledge about the relationship between business schools and employment sectors may also be important in understanding how to influence choices based on gender.

Table 8: Probability of Private Sector Employment

	Linear Probability Model							
	OLS estimation				2-step IV estimation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.340**	0.332**	0.334**	0.332**	0.380**	0.362**	0.371**	0.372**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.038)	(0.037)	(0.038)	(0.038)
Age when finished High School	-0.018**	-0.017**	-0.017**	-0.017**	-0.006+	-0.005	-0.002	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	-0.025**	-0.023**	-0.023**	-0.023**	-0.019**	-0.016*	-0.014*	-0.015*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Standardized High School GPA	-0.001	-0.003	-0.003	-0.003	-0.002	-0.006	-0.004	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.007)	(0.007)	(0.007)	(0.007)
Dane (=1)	-0.007	0.012	0.012	0.012	0.014	0.031	0.030	0.029
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Gender (Male=1)	0.138**	0.084**	0.140**	0.084**	0.135**	0.086**	0.133**	0.085**
	(0.005)	(0.008)	(0.005)	(0.008)	(0.006)	(0.009)	(0.006)	(0.009)
2008: Children<18 in the family (=1)		-0.077**		-0.077**		-0.058**		-0.059**
		(0.009)		(0.009)		(0.009)		(0.009)
Gender (Male=1) * Children (=1)		0.083**		0.083**		0.078**		0.073**
		(0.011)		(0.011)		(0.011)		(0.010)
2008: Experience			0.001	0.001+			0.007**	0.007**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.002+	0.001	0.001	0.001	0.002+	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.002+	0.001	0.001	0.001	0.001	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2					17.907	16.971	19.366	19.236
p					0.000	0.001	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

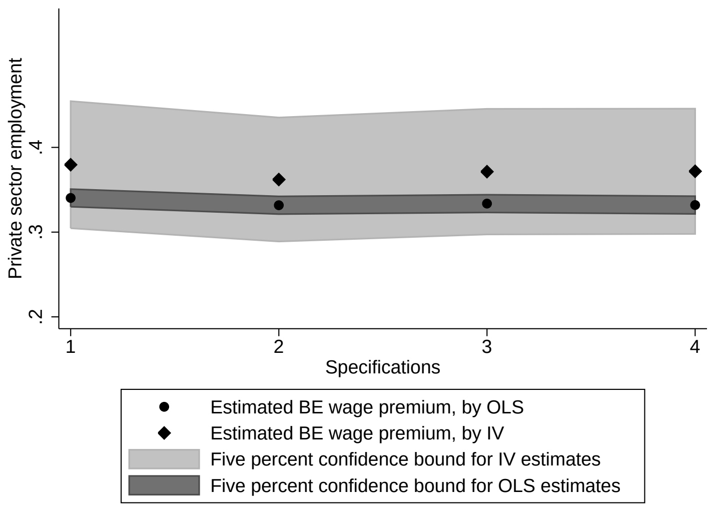
Note: The dependent variable is a dummy that is equal to 1 if an individual was employed in the private sector in 2008. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. χ^2 and p come from testing H_0, where $H_0 : \gamma_{Q2} = 0, \gamma_{Q3} = 0, \gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

In this section, we estimate the probability of private sector employment. As in the previous section, we use an IV approach to overcome the selection problem associated with master's program choice.

Table 8 shows the results from the estimations of Equation (1) (OLS) and Equation (5) (IV), with the dependent variable as a dummy that takes a value one if an individual was hired in the private sector in 2008. Figure 7 graphically presents the differences between the OLS and IV estimates of the probability of obtaining private sector employment.

As expected, the OLS results presented in column (1)-(4) of Table 8 reveal a positive association between business education and private sector employment and show an increased probability (approximately 34 percentage points) of private sector employment for those with a master's degree in business. The IV estimate in Table 8 shows a statistically significant business education premium of approximately 36-38 percentage points. Again, a comparison of the OLS results and the IV results, as shown in Figure 7, suggests that the OLS estimates are downward biased. However, the OLS and IV estimates are not significantly different from one another.

Figure 7: Estimated Business Education Effect on Private Sector Employment Across Model Specifications



The IV estimates in column (5)-(8) of Table 8 suggest that the probability of private sector employment is 36-38 percentage points higher for individuals with a master's degree in business economics and management compared with individuals with a master's degree in the social sciences. Thus, enrolling in a master's program at a business school increases the probability of private sector employment significantly.

In addition to the aforementioned results on the business education effect, we also observe that men are 14 percentage points more likely to be employed in the private sector and that having children younger than 18 in the household significantly decreases the probability of private sector employment, but this decrease only applies to women with children. In addition, we find that parents' education is not significant in the model.

Most individuals with a business education are ultimately employed in the private sector, which is not terribly surprising given the nature of business school curricula. However, the labor market in Denmark is relatively segregated both in terms of gender and educational background, which requires further consideration. Thus, from a policy perspective, the results may be helpful when considering different aspects of the current labor market and the skill supply. Firstly, encouraging women to enroll at business schools might make them more likely to be employed in the private sector, which potentially could reduce the gender wage gap. Moreover, if less-able students are more likely to select into a business education and if a business education increases the probability of private sector employment, the supply of initial human capital might be lower in the private sector compared with the supply in the public sector. Additionally, highly skilled individuals who graduate with a master's degree from a university might have opportunities in the private sector that they do not realize because they are not encouraged to seek private sector employment.

8 Robustness

To validate our results, we conduct a series of robustness estimations. The results from these estimations are presented in Appendix B. Robustness is tested along two different dimensions. We test whether our results are sensitive to alternative model specifications, to the inclusion of individuals who had annual earnings below 200,000 Danish kroner (DKK) in 2008, and the exclusion of outliers. Moreover, we apply an alternative definition of our instrument and estimate using a standard 2SLS approach.

We first test the robustness of our results to the wage measure. Thus, we perform estimations of Equation (1) and Equation (5), where the dependent variable is the annual earnings in 2008. Table B.2 reports the results. The results remain practically unchanged, and we observe a significant business wage premium.

One might also worry that the results are primarily driven by outliers. Thus, as a robustness test, we exclude wage observations that lie above or below the 99th and 1st percentile and re-estimate the model. The results are presented in Table B.1. The estimated coefficients remain virtually unchanged compared with the results show in Table 6.

The hourly wage measured in November 2008 might not be a perfect measure of an individual's productivity, as an individual might have only been employed for a short period that year and this period then included the last months of 2008. In the paper, we account for this possibility by excluding individuals with annual earnings below DKK 200,000 in 2008. To test whether our results are robust to this sampling, we re-estimate our models with an extended sample, including individuals with annual earnings below DKK 200,000 in 2008. The hourly wage results are presented in Table B.3 and Table B.4, and the annual earning results are presented in Table B.5. Both Table B.3 and Table B.4 reveal results that are qualitatively the same as those reported in Section 6, but the estimates in Table B.4 are not as significant. By contrast, the results in Table B.5 are more ambiguous and show an insignificant business wage premium. However, examine the data more carefully reveals that the results in Table B.5 are sensitive to the inclusion of annual earnings observations below 10,000 DKK (approximately 1500 US dollars) and below 50,000 DKK (approximately 7400 US dollars). Individuals with annual earnings below 10.000 DKK and below 50,000 DKK could reasonable be considered outside the labor market, which makes the results in Table B.5 difficult to interpret.

We estimate the probability of private sector employment with an LPM. The LPM is sometimes problematic, as it is not limited to the unit interval and thus can predict probabilities outside this interval. However, if the explanatory variables are also bounded or are mostly dummies, as in our case, the LPM often constitutes an acceptable alternative. To test whether our results are robust to model choice, we re-estimate Equation (1) with a probit model, and the results are presented in Table B.6. Table B.6 shows that the average marginal effects obtained after probit estimations are almost identical to the estimated marginal effects obtained from the LPM.

To test the sensitivity of the results to our preferred instrument, we re-estimate our models using a different definition of the instrument. Table B.7 and Table B.8 show the results of our estimations, where the instrument is a continuous variable that counts the days between January 1 and the student's birthday. Both Table B.7 and Table B.8 show almost unchanged results, with only slightly different χ^2-statistics from the test of the instruments' significance in the selection equation.

Finally, we also perform standard 2SLS estimations with the two different instruments. The results from the wage regression are presented in Table B.9 and Table B.10, and the results from estimating the probability of private sector employment are presented in Table B.11. As expected, the results from Table B.9 and Table B.10 show less precise IV estimates and thus much less significant results. However, Table B.9 still shows a positive business wage premium, and the estimates are not significantly different from the ones presented in Table 6. The results in Table B.10 are a bit more ambiguous and shows mostly a insignificant business education effect. The sign of the business education effect is negative in the first year after graduation and turns positive from the 6^{th} year after graduation. However, the coefficients are not significantly different from zero and the standard errors are large; thus, the estimates are actually not significantly different from the results presented in Table 7. Estimating the probability of private sector employment using 2SLS reveals larger point estimates than those presented in Table 8. Again, the 2SLS estimates in Table B.11 have large standard errors, meaning they are less precise and have corresponding broad confidence intervals. This in fact means that the estimates are not significantly different from the ones presented in Table 8. However, the magnitude of the point estimates gives reason to some concern. Finally, B.9 and B.11 shows how the 2SLS results are robust across the two different instruments, despite differences in the reported F-statistics.

9 Conclusion

Using Danish register data, this paper conducts an analysis of the consequences and advantages of graduating with a master's degree in business economics and management compared with graduating with a master's degree in other social sciences. To do so, we estimate a general wage equation and model the probability of private sector employment conditional on educational attainment using Danish register data. To address the endogeneity of educational selection, we apply an IV approach, in which we use educational maturity as our exogenous determinant of master's program choice.

We claim that educational maturity is an important determinant of fields of study, and we argue that individuals who are less educationally mature are more likely to self-select into business education, as this type of field of study is more general and allows for more diversity in the curriculum. We measure educational maturity by season of birth, and we claim that season of birth is exogenous to future labor market outcomes in the case of Denmark. Moreover, season of birth is a significant determinant of field of study in all our estimations, which makes it a relevant instrument.

Our results show that individuals who complete a master's program in business economics and management obtain, on average, a wage premium of approximately 12-17%. This business education wage premium is smaller when measured close to graduation and increases with the years after graduation. The latter finding indicates that the returns to a master's degree in business economics and management are enhanced with labor market experience. Comparing our IV and OLS results shows that the OLS estimates are downward biased, which indicates negative selection into business education. In terms of gender, our results surprisingly show that controlling for having a master's degree in business economics and management does not reduce the observed gender wage gap. Parenthood actually increases the gender wage gap, as it is positively associated with wage outcomes for men and negatively associated with wage outcomes for women.

When modeling the probability of private sector employment, our results show that individuals with a master's degree in business economics and management are, on average, 36-38 percentage points more likely to be employed in the private sector. Because wages are generally higher in the private sector, this increase in the probability of private sector employment is likely a main explanation for the observed wage gap between a master's degree in business and a master's degree in the social sciences. Although individuals with a master's degree in business unsurprisingly seek private sector employment, our results pave the way for a broader discussion about how to ensure diverse skills and competences across the public and private sectors in Denmark.

Finally, in 2008, 72% of the men in our sample were hired in the private sector, whereas only 55% of the women were hired in the private sector. This imbalance might partially explain the wide gender wage gap that we observe. Although the gender-segregated labor market in Denmark is primarily caused by the differences in family-friendly policies across sectors, policymakers may still offer some ideas on how to address the gender wage gap through education.

Before coming to any definitive conclusions about the impact of a master's degree in business economics and management, more research in this area is still recommended and such research should consider alternative methods to address the endogeneity of master's program choice. If the results are consistent across different methods, they will be further validated, particularly because IV estimation hinges on the exogeneity assumption, which cannot be tested. In addition, before offering further conclusions about the observed gender wage gap, more research is needed. For instance, we must perform analyses that include more explanatory variables, such as course-specific variables and measures of weekly hours worked.

First and foremost, this paper shows that field-of-study choice plays an important role in eventual labor market outcomes. In addition to offering more insights into the impact and consequences of master's program choices, from a policy perspective, this paper also provides insights into the selection process of fields of study. As our results show that individuals born later in the year (and with lower levels of educational maturity) are more likely to choose business education, policymakers should perhaps consider how students are guided when deciding on their tertiary education. This insight will be especially useful if policymakers intend to have an impact on students' educational choices.

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Appendix A Additional Statistics

Figure A.1: Share of Individuals With Child(ren) Across Years After Graduation

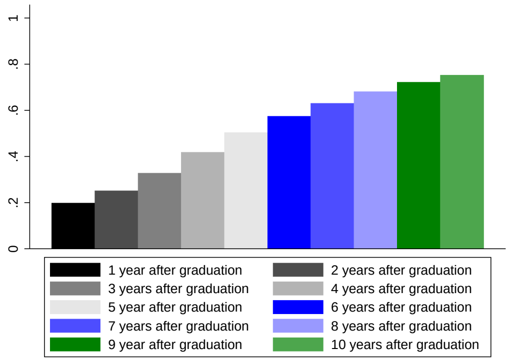
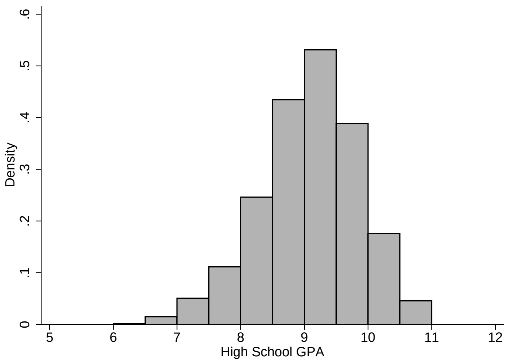
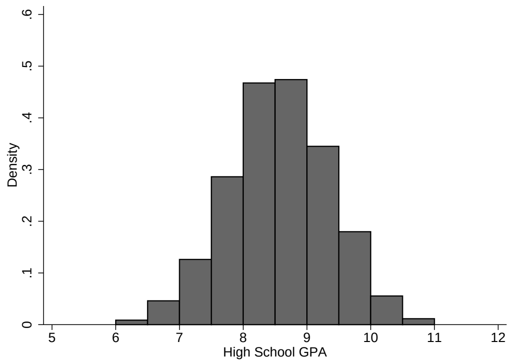


Figure A.2: Histograms of High School GPA*

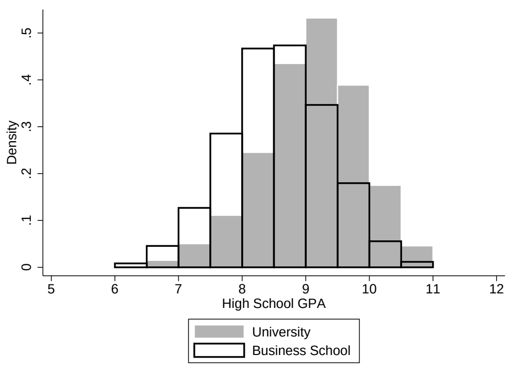
(a) University Students



(b) Business Students



(c) University and Business Students



* In order to comply with the discretion rules of Statistic Denmark, we have excluded observations with high school GPA above 11 or below 6.

Table A.1: Summary Statistics Across Quarter of Birth

	Quarter of birth				
	1	2	3	4	Total
Full sample:					
High School GPA	8.84	8.83	8.84	8.82	8.83
	(0.83)	(0.82)	(0.82)	(0.84)	(0.83)
Age when finished High School	19.38	19.33	19.18	19.08	19.25
	(0.94)	(0.80)	(0.85)	(0.89)	(0.88)
Hourly wage in 2008	356.77	355.54	357.70	364.89	358.48
	(244.91)	(241.61)	(306.89)	(277.02)	(268.07)
Father's year of edu.	13.01	13.08	13.13	12.93	13.04
	(4.34)	(4.30)	(4.26)	(4.45)	(4.33)
Mother's year of edu.	12.77	12.70	12.87	12.78	12.78
	(3.52)	(3.53)	(3.42)	(3.51)	(3.50)
Business educated:					
High School GPA	8.52	8.52	8.53	8.50	8.52
	(0.80)	(0.78)	(0.79)	(0.80)	(0.79)
Age when finished High School	19.36	19.32	19.15	19.07	19.23
	(0.83)	(0.68)	(0.70)	(0.80)	(0.76)
Hourly wage in 2008	384.33	379.24	387.08	379.83	382.58
	(285.88)	(263.72)	(413.28)	(232.58)	(306.83)
Father's year of edu.	12.91	12.97	12.99	12.86	12.94
	(4.08)	(4.06)	(4.11)	(4.16)	(4.10)
Mother's year of edu.	12.53	12.47	12.65	12.47	12.53
	(3.37)	(3.51)	(3.28)	(3.44)	(3.40)
University educated:					
High School GPA	9.04	9.04	9.04	9.03	9.04
	(0.79)	(0.78)	(0.77)	(0.79)	(0.78)
Age when finished High School	19.38	19.35	19.20	19.08	19.26
	(1.01)	(0.88)	(0.93)	(0.94)	(0.95)
Hourly wage in 2008	339.34	339.74	338.80	354.84	342.74
	(213.19)	(224.31)	(209.75)	(302.87)	(238.08)
Father's year of edu.	13.08	13.16	13.24	12.99	13.12
	(4.52)	(4.48)	(4.36)	(4.66)	(4.50)
Mother's year of edu.	12.94	12.88	13.03	13.02	12.96
	(3.63)	(3.54)	(3.51)	(3.54)	(3.55)

Table A.2: The Selection into a Business Education

	Linear Probability Model							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Born 2. quarter	0.021**	0.020**	0.021**	0.021**				
	(0.007)	(0.007)	(0.007)	(0.007)				
Born 3. quarter	0.017*	0.016*	0.018*	0.018*				
	(0.007)	(0.007)	(0.007)	(0.007)				
Born 4. quarter	0.029**	0.027**	0.030**	0.029**				
	(0.007)	(0.007)	(0.007)	(0.007)				
Days Between 1. January and Birthday (divided by 100)					0.010**	0.009**	0.010**	0.010**
					(0.003)	(0.003)	(0.003)	(0.003)
Age when finished High School	0.008*	0.010**	0.012**	0.012**	0.008*	0.011**	0.013**	0.013**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	0.008	0.013+	0.014*	0.014*	0.008	0.012+	0.014*	0.013*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Standardized High School GPA	-0.172**	-0.174**	-0.173**	-0.173**	-0.173**	-0.174**	-0.173**	-0.173**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dane (=1)	-0.099**	-0.069*	-0.071*	-0.071*	-0.099**	-0.069*	-0.071*	-0.071*
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Gender (Male=1)	0.061**	0.051**	0.059**	0.050**	0.061**	0.051**	0.059**	0.050**
	(0.005)	(0.009)	(0.005)	(0.009)	(0.005)	(0.009)	(0.005)	(0.009)
2008: Children<18 in the family (=1)		-0.030**		-0.031**		-0.030**		-0.031**
		(0.008)		(0.008)		(0.008)		(0.008)
Gender (Male=1) * Children (=1)		0.015		0.011		0.015		0.011
		(0.011)		(0.011)		(0.011)		(0.011)
2008: Experience			0.006**	0.006**			0.006**	0.006**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	-0.003**	-0.004**	-0.004**	-0.004**	-0.003**	-0.004**	-0.004**	-0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	-0.007**	-0.008**	-0.008**	-0.008**	-0.007**	-0.008**	-0.008**	-0.008**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F	5.498	5.066	5.817	5.790	15.258	14.139	16.474	16.430
p	0.001	0.002	0.001	0.001	0.000	0.000	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

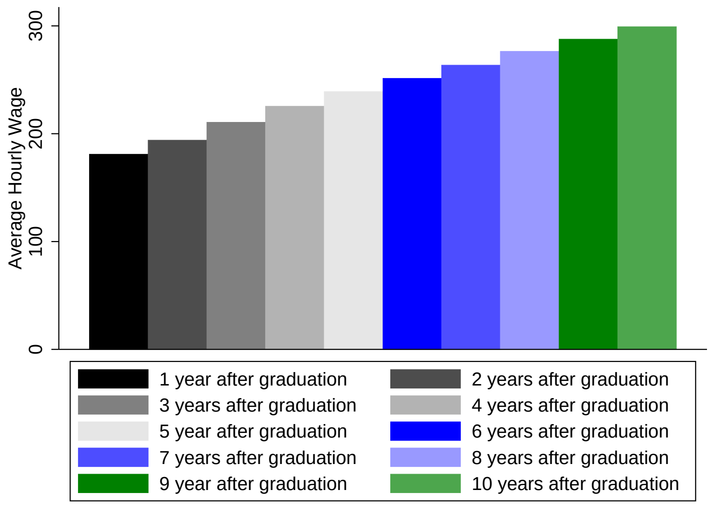
Note: The dependent variable is a dummy that is equal to 1 if an individual enrolled in a master's in business economics and management. F and p comes from testing the (joint) significance of the instruments. Robust standard errors are computed and reported in parentheses. *** $p<0.01$, ** $p<0.05$, + $p<0.1$. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Table A.3: High School GPA Regressed on Quarter of Birth

	OLS estimation		
	(1)	(2)	(3)
	Full Sample	Men	Women
Born 2. quarter	0.005	0.016	-0.007
	(0.013)	(0.018)	(0.018)
Born 3. quarter	0.012	0.002	0.022
	(0.013)	(0.018)	(0.019)
Born 4. quarter	0.002	-0.004	0.009
	(0.014)	(0.019)	(0.020)
Father's year of edu.	0.015**	0.013**	0.016**
	(0.002)	(0.003)	(0.003)
Mother's year of edu.	0.025**	0.027**	0.023**
	(0.002)	(0.003)	(0.003)
Gender (Male=1)	-0.163**		
	(0.009)		
Birth Year fixed Effects	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes
p	0.819	0.720	0.401
No. of obs	30418	16681	13737

Note: The dependent variable is High School GPA. p come from testing H_0, where $H_0 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Figure A.3: Average Hourly Wage Across Years After Graduation*



* The hourly wage has been inflation adjusted with 2000 as the basis year.

Appendix B Robustness

Table B.1: Robustness: The Return to a Business Education - Wage Estimations Sample Excluding Wage Observations Below and Above the 99 and 1 Percentile

	OLS estimation				2-step IV estimation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.062**	0.058**	0.052**	0.052**	0.142**	0.157**	0.137**	0.145**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.025)	(0.024)	(0.024)	(0.024)
Age when finished High School	-0.017**	-0.016**	-0.010**	-0.010**	-0.017**	-0.017**	-0.011**	-0.011**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Continued into 10. grade	-0.041**	-0.040**	-0.037**	-0.037**	-0.042**	-0.041**	-0.038**	-0.039**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Standardized High School GPA	0.035**	0.034**	0.033**	0.034**	0.049**	0.051**	0.048**	0.050**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.004)	(0.004)
Dane (=1)	-0.027	-0.020	-0.024	-0.026	-0.019	-0.013	-0.018	-0.020
	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)
Gender (Male=1)	0.183**	0.103**	0.176**	0.101**	0.178**	0.098**	0.171**	0.097**
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)	(0.004)	(0.005)
2008: Children<18 in the family (=1)		-0.044**		-0.047**		-0.041**		-0.044**
		(0.005)		(0.005)		(0.005)		(0.005)
Gender (Male=1) * Children (=1)		0.127**		0.117**		0.126**		0.116**
		(0.007)		(0.007)		(0.007)		(0.007)
2008: Experience			0.017**	0.016**			0.016**	0.016**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.002**	0.002*	0.002**	0.002**	0.002**	0.002**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.001+	0.001	0.002*	0.002*	0.002*	0.002*	0.002**	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2					18.204	17.213	19.732	19.582
p					0.000	0.001	0.000	0.000
No. of obs	29823	29823	29823	29823	29823	29823	29823	29823

Note: The dependent variable is the logarithm of the hourly wage measured in November of 2008. Wage observations below and above the 99 and 1 percentile are excluded. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. χ^2 and p come from testing H_0, where $H_0 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Table B.2: Robustness: The Return to a Business education - Wage Estimations Dependent Variable is the Logarithm of Annual Earnings

	OLS estimation				2-step IV estimation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.067**	0.063**	0.055**	0.055**	0.153**	0.181**	0.156**	0.166**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.028)	(0.028)	(0.028)	(0.028)
Age when finished High School	-0.020**	-0.019**	-0.012**	-0.013**	-0.020**	-0.020**	-0.013**	-0.014**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	-0.046**	-0.045**	-0.041**	-0.041**	-0.047**	-0.046**	-0.042**	-0.043**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Standardized High School GPA	0.043**	0.042**	0.041**	0.042**	0.058**	0.062**	0.059**	0.061**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)	(0.005)
Dane (=1)	-0.012	-0.008	-0.012	-0.016	-0.004	0.000	-0.005	-0.009
	(0.024)	(0.024)	(0.023)	(0.023)	(0.024)	(0.024)	(0.023)	(0.023)
Gender (Male=1)	0.251**	0.124**	0.241**	0.121**	0.245**	0.118**	0.235**	0.116**
	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)
2008: Children<18 in the family (=1)		-0.088**		-0.092**		-0.085**		-0.089**
		(0.006)		(0.006)		(0.006)		(0.006)
Gender (Male=1) * Children (=1)		0.199**		0.186**		0.197**		0.185**
		(0.008)		(0.008)		(0.008)		(0.008)
2008: Experience			0.021**	0.021**			0.021**	0.020**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.003**	0.002*	0.003**	0.003**	0.003**	0.003**	0.004**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.003**	0.003**	0.003**	0.003**	0.003**	0.003**	0.004**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2					17.907	16.971	19.366	19.236
p					0.000	0.001	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is the logarithm of the annual earnings (including tax-free earnings) measured in 2008. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. χ^2 and p come from testing H_0, where $H_0 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

B.1 Estimations on Expanded Sample

Table B.3: The Return to a Business Education - Wage Estimations

	OLS estimation				2-step IV estimation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.058**	0.055**	0.049**	0.050**	0.122**	0.163**	0.120**	0.147**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.035)	(0.033)	(0.033)	(0.033)
Age when finished High School	-0.019**	-0.018**	-0.012**	-0.013**	-0.019**	-0.019**	-0.013**	-0.014**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	-0.043**	-0.040**	-0.038**	-0.038**	-0.043**	-0.042**	-0.039**	-0.039**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Standardized High School GPA	0.039**	0.038**	0.038**	0.038**	0.050**	0.056**	0.050**	0.055**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.006)	(0.006)	(0.006)
Dane (=1)	-0.013	-0.005	-0.009	-0.012	-0.007	0.002	-0.005	-0.005
	(0.023)	(0.023)	(0.022)	(0.022)	(0.024)	(0.024)	(0.023)	(0.023)
Gender (Male=1)	0.208**	0.114**	0.201**	0.111**	0.205**	0.110**	0.197**	0.108**
	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)
2008: Children<18 in the family (=1)		-0.031**		-0.038**		-0.025**		-0.032**
		(0.006)		(0.005)		(0.006)		(0.006)
Gender (Male=1) * Children (=1)		0.153**		0.144**		0.150**		0.141**
		(0.008)		(0.008)		(0.008)		(0.008)
2008: Experience			0.017**	0.016**			0.017**	0.016**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.002*	0.001+	0.002**	0.002**	0.002*	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.002*	0.002+	0.002**	0.002**	0.003**	0.003**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2					16.607	15.681	17.519	17.330
p					0.001	0.001	0.001	0.001
No. of obs	30884	30884	30884	30884	30884	30884	30884	30884

Note: The dependent variable is log of the hourly wage measured in November of 2008. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. χ^2 and p come from testing H_0, where $H_0 : \gamma_{Q2} = 0, \gamma_{Q3} = 0, \gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample also includes individuals with annual earnings below DKK 200,000 in 2008.

Table B.4: The Return to a Business Education - Wage Estimations

	OLS estimation									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Wages measured year after graduation	1 year	2 year	3 year	4 year	5 year	6 year	7 year	8 year	9 year	10 year
Business educated	0.071**	0.083**	0.079**	0.078**	0.080**	0.076**	0.083**	0.080**	0.078**	0.078**
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Age when finished High School	-0.005*	-0.008**	-0.009**	-0.010**	-0.011**	-0.014**	-0.014**	-0.013**	-0.017**	-0.018**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
Continued into 10. grade	-0.012**	-0.016**	-0.028**	-0.035**	-0.042**	-0.040**	-0.044**	-0.045**	-0.044**	-0.049**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)
Standardized High School GPA	0.008**	0.016**	0.020**	0.025**	0.029**	0.030**	0.036**	0.040**	0.043**	0.042**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Dane (=1)	0.008	-0.008	0.003	-0.022	-0.016	-0.002	0.005	0.018	0.006	0.009
	(0.018)	(0.018)	(0.019)	(0.021)	(0.024)	(0.026)	(0.028)	(0.033)	(0.039)	(0.041)
Gender (Male=1)	0.068**	0.093**	0.117**	0.135**	0.153**	0.170**	0.187**	0.203**	0.218**	0.228**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Father's year of edu.	0.002**	0.002**	0.003**	0.002**	0.002**	0.003**	0.003**	0.003**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	-0.001	0.000	0.001	0.001*	0.002**	0.003**	0.002**	0.003**	0.004**	0.005**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	2-step IV estimation									
Business educated	0.022	0.018	0.053*	0.035	0.068**	0.053+	0.104**	0.085*	0.040	0.100+
	(0.021)	(0.021)	(0.022)	(0.025)	(0.026)	(0.029)	(0.032)	(0.040)	(0.046)	(0.051)
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs	25978	27117	27318	25714	24264	22782	21350	20093	18763	17288

Note: The dependent variable is the logarithm of the hourly wage measured in November 1-10 years after graduation. All controls are also included in the IV estimations. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. In all estimations, we use predicted probabilities obtained by estimating a specification of the selection equation that is equal to the specification in column (1) of Table 3. Thus, χ^2 and p values from testing H_0, where $H_0 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$, can be seen in Table 3. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample also includes individuals with annual earnings below DKK 200,000 in 2008.

Table B.5: Robustness: The Return to Business Education - Wage Estimations
Dependent Variable is the Logarithm of Annual Earnings

	OLS estimation				**2-step IV estimation**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.037**	0.034**	0.026**	0.026**	0.018	0.114**	-0.011	0.068
	(0.005)	(0.005)	(0.005)	(0.005)	(0.045)	(0.042)	(0.043)	(0.042)
Age when finished High School	-0.018**	-0.017**	-0.009**	-0.009**	-0.018**	-0.018**	-0.008**	-0.009**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	-0.042**	-0.040**	-0.036**	-0.036**	-0.042**	-0.041**	-0.036**	-0.037**
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)
Standardized High School GPA	0.035**	0.034**	0.034**	0.034**	0.032**	0.047**	0.027**	0.041**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.009)	(0.008)	(0.008)	(0.008)
Dane (=1)	-0.007	-0.001	-0.011	-0.011	-0.009	0.004	-0.011	-0.008
	(0.028)	(0.028)	(0.026)	(0.026)	(0.028)	(0.028)	(0.026)	(0.027)
Gender (Male=1)	0.257**	0.155**	0.150**	0.150**	0.258**	0.152**	0.246**	0.149**
	(0.004)	(0.007)	(0.007)	(0.007)	(0.005)	(0.008)	(0.005)	(0.007)
2008: Children<18 in the family (=1)		-0.015*	-0.025**	-0.025**		-0.011		-0.022**
		(0.007)	(0.007)	(0.007)		(0.007)		(0.007)
Gender (Male=1) * Children (=1)		0.167**	0.153**	0.153**		0.164**		0.152**
		(0.009)	(0.009)	(0.009)		(0.009)		(0.009)
2008: Experience			0.025**	0.025**			0.026**	0.025**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.003**	0.002*	0.003**	0.003**	0.003**	0.002**	0.003**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.003**	0.002**	0.003**	0.003**	0.003**	0.003**	0.003**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2					16.627	15.723	17.630	17.436
p					0.001	0.001	0.001	0.001
No. of obs	30873	30873	30873	30873	30873	30873	30873	30873

Note: The dependent variable is the logarithm of the annual earnings (including tax-free income) measured in 2008. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. χ^2 and p come from testing H_0, where $H_0 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample also includes individuals with annual earnings below DKK 200,000 in 2008.

B.2 Probit and LPM Estimation of Private Sector Employment

Table B.6: Robustness: Probability of Private Sector Employment
Probit and LPM Estimation

	LPM estimation				Probit estimation - AME			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated=1	0.340**	0.332**	0.334**	0.332**	0.338**	0.329**	0.331**	0.330**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Age when finished High School	-0.018**	-0.017**	-0.017**	-0.017**	-0.017**	-0.016**	-0.016**	-0.016**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade=1	-0.025**	-0.023**	-0.023**	-0.023**	-0.027**	-0.025**	-0.024**	-0.025**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
Standardized High School GPA	-0.001	-0.003	-0.003	-0.003	-0.001	-0.003	-0.002	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Dane (=1)=1	-0.007	0.012	0.012	0.012	-0.001	0.019	0.019	0.018
	(0.030)	(0.030)	(0.030)	(0.030)	(0.031)	(0.031)	(0.031)	(0.031)
Man	0.138**	0.084**	0.140**	0.084**	0.134**	0.134**	0.136**	0.133**
	(0.005)	(0.008)	(0.005)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)
2008: Children<18 in the family (=1)=1		-0.077**		-0.077**		-0.030**		-0.032**
		(0.009)		(0.009)		(0.006)		(0.006)
Gender (Male=1) * Children (=1)=1		0.083**		0.083**				
		(0.011)		(0.011)				
2008: Experience			0.001	0.001+			0.001	0.001+
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.002+	0.001	0.001	0.001	0.002+	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.002+	0.001	0.001	0.001	0.002+	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is a dummy that is equal to 1 if an individual was employed in the private sector in 2008. Columns (5)-(8) report average marginal effects (AME) computed after the probit estimation. Robust standard errors are computed and reported in parentheses. *** $p<0.01$, ** $p<0.05$, + $p<0.1$. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

B.3 Estimation Using Alternative Instrument

Table B.7: The Return to a Business Education - Wage Estimations

	2-step IV estimation							
	Instrument 1				**Instrument 2**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.154**	0.172**	0.147**	0.155**	0.153**	0.171**	0.146**	0.154**
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Age when finished High School	-0.019**	-0.019**	-0.013**	-0.014**	-0.019**	-0.019**	-0.013**	-0.014**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	-0.044**	-0.043**	-0.040**	-0.040**	-0.044**	-0.043**	-0.040**	-0.040**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Standardized High School GPA	0.056**	0.059**	0.055**	0.057**	0.056**	0.059**	0.055**	0.057**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Dane (=1)	-0.005	-0.000	-0.004	-0.007	-0.005	-0.000	-0.004	-0.007
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Gender (Male=1)	0.202**	0.099**	0.194**	0.098**	0.202**	0.099**	0.194**	0.098**
	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)	(0.006)
2008: Children<18 in the family (=1)		-0.049**		-0.053**		-0.049**		-0.053**
		(0.006)		(0.005)		(0.006)		(0.005)
Gender (Male=1) * Children (=1)		0.161**		0.152**		0.161**		0.152**
		(0.008)		(0.008)		(0.008)		(0.008)
2008: Experience			0.016**	0.015**			0.016**	0.015**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.002**	0.002*	0.003**	0.003**	0.002**	0.002*	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.003**	0.003**	0.003**	0.003**	0.003**	0.003**	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Instrument	D_Q	D_Q	D_Q	D_Q	*Days*	*Days*	*Days*	*Days*
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2	17.907	16.971	19.366	19.236	16.329	15.503	18.084	17.887
p	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is the logarithm of the hourly wage measured in November of 2008. The instruments in columns (1)-(4) are quarter-of-birth dummies and the instrument in columns(5)-(8) is a continuous variable that counts the days between individual i's birthday and January 1. χ^2 and p comes from testing H_0^j $j = 1, 2$, where $H_0^1 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$ and $H_0^2 : \gamma_{Days} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Table B.8: Probability of Private Sector Employment

	Linear Probability Model							
	2-step IV estimation							
	Instrument 1				Instrument 2			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.380**	0.362**	0.371**	0.372**	0.381**	0.363**	0.373**	0.373**
	(0.038)	(0.037)	(0.038)	(0.038)	(0.038)	(0.037)	(0.038)	(0.038)
Age when finished High School	-0.006+	-0.005	-0.002	-0.003	-0.006+	-0.005	-0.002	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Continued into 10. grade	-0.019**	-0.016*	-0.014*	-0.015*	-0.019**	-0.016*	-0.014*	-0.015*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Standardized High School GPA	-0.002	-0.006	-0.004	-0.004	-0.002	-0.006	-0.004	-0.004
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Dane (=1)	0.014	0.031	0.030	0.029	0.014	0.031	0.030	0.029
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Gender (Male=1)	0.135**	0.086**	0.133**	0.085**	0.135**	0.086**	0.133**	0.084**
	(0.006)	(0.009)	(0.006)	(0.009)	(0.006)	(0.009)	(0.006)	(0.009)
2008: Children<18 in the family (=1)		-0.058**		-0.059**		-0.058**		-0.059**
		(0.009)		(0.009)		(0.009)		(0.009)
Gender (Male=1) * Children (=1)		0.078**		0.073**		0.077**		0.073**
		(0.011)		(0.010)		(0.011)		(0.010)
2008: Experience			0.007**	0.007**			0.007**	0.007**
			(0.001)	(0.001)			(0.001)	(0.001)
Father's year of edu.	0.002+	0.001	0.001	0.001	0.002+	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.001	0.000	0.000	0.000	0.001	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Instrument	D_Q	D_Q	D_Q	D_Q	*Days*	*Days*	*Days*	*Days*
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
χ^2	17.907	16.971	19.366	19.236	16.329	15.503	18.084	17.887
p	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.000
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is a dummy that is equal to 1 if an individual was employed in the private sector in 2008. The instruments in columns (1)-(4) are quarter-of-birth dummies and the instrument in columns(5)-(8) is a continuous variable that counts the days between individual i's birthday and January 1. χ^2 and p comes from testing H_0^j $j = 1, 2$, where $H_0^1 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$ and $H_0^2 : \gamma_{Days} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

B.4 Standard 2SLS

Table B.9: The Return to a Business Education - Wage Estimations
2 Stage Least Square

	2SLS IV estimation							
	Instrument 1				Instrument 2			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.209	0.183	0.352+	0.333	0.199	0.172	0.362+	0.342
	(0.204)	(0.210)	(0.206)	(0.204)	(0.213)	(0.219)	(0.214)	(0.211)
Age when finished High School	-0.020**	-0.019**	-0.015**	-0.016**	-0.020**	-0.019**	-0.015**	-0.016**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Continued into 10. grade	-0.045**	-0.043**	-0.043**	-0.043**	-0.045**	-0.043**	-0.043**	-0.043**
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
Standardized High School GPA	0.066+	0.061+	0.091*	0.088*	0.064+	0.059	0.093*	0.089*
	(0.035)	(0.037)	(0.036)	(0.035)	(0.037)	(0.038)	(0.037)	(0.037)
Dane (=1)	0.000	0.001	0.011	0.006	-0.001	0.000	0.011	0.006
	(0.031)	(0.027)	(0.029)	(0.028)	(0.032)	(0.028)	(0.029)	(0.029)
Gender (Male=1)	0.198**	0.099**	0.182**	0.089**	0.199**	0.099**	0.182**	0.088**
	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)
2008: Children<18 in the family (=1)		-0.049**		-0.047**		-0.049**		-0.047**
		(0.008)		(0.009)		(0.009)		(0.009)
Gender (Male=1) * Children (=1)		0.161**		0.150**		0.161**		0.150**
		(0.009)		(0.009)		(0.009)		(0.009)
2008: Experience			0.015**	0.014**			0.015**	0.014**
			(0.002)	(0.002)			(0.002)	(0.002)
Father's year of edu.	0.002*	0.002	0.003**	0.003**	0.002*	0.002	0.003**	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	0.003+	0.003	0.005*	0.005*	0.003+	0.003	0.005*	0.005*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Instrument	D_Q	D_Q	D_Q	D_Q	*Days*	*Days*	*Days*	*Days*
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	5.498	5.066	5.817	5.790	15.258	14.139	16.474	16.430
p^J	0.427	0.408	0.429	0.440				
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is the logarithm of the hourly wage measured in November of 2008. The instruments in columns (1)-(4) are quarter-of-birth dummies and the instrument in columns(5)-(8) is a continuous variable that counts the days between individual i's birthday and January 1. The F-statistic comes from testing H_0^j $j = 1, 2$, where $H_0^1 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$ and $H_0^2 : \gamma_{Days} = 0$. p^J is the p-value from the Sargan-Hansen test, which tests the null hypothesis that instruments are uncorrelated with the error term. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Table B.10: The Return to a Business Education - Wage Estimations
2 Stage Least Square

	2SLS IV estimation									
Wages measured year after graduation	1 year	2 year	3 year	4 year	5 year	6 year	7 year	8 year	9 year	10 year
Business educated	-0.399*	-0.239	-0.148	-0.294	-0.035	0.268	0.185	0.587*	0.256	0.345
	(0.156)	(0.146)	(0.144)	(0.215)	(0.199)	(0.225)	(0.224)	(0.272)	(0.224)	(0.229)
Age when finished High School	-0.016**	-0.016**	-0.014**	-0.012**	-0.013**	-0.015**	-0.013**	-0.014**	-0.014**	-0.012**
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Continued into 10. grade	-0.011+	-0.017**	-0.030**	-0.034**	-0.042**	-0.043**	-0.044**	-0.052**	-0.046**	-0.052**
	(0.006)	(0.005)	(0.004)	(0.006)	(0.005)	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)
Standardized High School GPA	-0.067*	-0.034	-0.015	-0.037	0.010	0.065	0.053	0.127**	0.072+	0.084*
	(0.028)	(0.026)	(0.025)	(0.037)	(0.035)	(0.040)	(0.039)	(0.048)	(0.039)	(0.040)
Dane (=1)	-0.039	-0.042+	-0.022	-0.050+	-0.027	0.026	0.021	0.054	0.028	0.034
	(0.027)	(0.023)	(0.025)	(0.030)	(0.029)	(0.038)	(0.036)	(0.047)	(0.045)	(0.049)
Gender (Male=1)	0.096**	0.112**	0.130**	0.156**	0.158**	0.157**	0.179**	0.171**	0.206**	0.209**
	(0.010)	(0.009)	(0.009)	(0.013)	(0.012)	(0.014)	(0.015)	(0.018)	(0.015)	(0.017)
Father's year of edu.	-0.000	0.001+	0.002*	0.001	0.002*	0.004**	0.004**	0.005**	0.004**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother's year of edu.	-0.003*	-0.001	-0.001	-0.001	0.001	0.004*	0.003	0.006**	0.004*	0.006**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Instrument	D_Q	D_Q	D_Q	D_Q	D_Q	D_Q	D_Q	D_Q	D_Q	D_Q
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	6.074	5.518	5.534	3.449	3.287	2.947	3.090	3.447	3.840	4.457
No. of obs	25663	26786	26999	25467	24050	22581	21168	19931	18628	17171

Note: The dependent variable is the logarithm of the hourly wage measured in November 1-10 years after graduation. The instruments are quarter-of-birth dummies, and the reference group includes individuals born in the first quarter. The F-statistic comes from testing $H_0^1 : \gamma_{Q2} = 0, \gamma_{Q3} = 0, and \gamma_{Q4} = 0$. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Table B.11: Probability of Private Sector Employment
2 Stage Least Square

	Linear Probability Model							
	2SLS IV estimation							
	Instrument 1				Instrument 2			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business educated	0.864**	0.840**	0.890**	0.900**	0.966**	0.949**	0.991**	1.001**
	(0.278)	(0.288)	(0.275)	(0.275)	(0.302)	(0.313)	(0.296)	(0.295)
Age when finished High School	-0.009*	-0.009+	-0.008	-0.008	-0.010*	-0.009*	-0.009+	-0.009+
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Continued into 10. grade	-0.024**	-0.023**	-0.023*	-0.023**	-0.025**	-0.025**	-0.024**	-0.025**
	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
Standardized High School GPA	0.081+	0.077	0.085+	0.087+	0.099+	0.095+	0.103*	0.105*
	(0.048)	(0.050)	(0.048)	(0.048)	(0.052)	(0.054)	(0.051)	(0.051)
Dane (=1)	0.062	0.064	0.067	0.066	0.072	0.071	0.075+	0.074+
	(0.045)	(0.041)	(0.041)	(0.042)	(0.048)	(0.043)	(0.044)	(0.044)
Gender (Male=1)	0.105**	0.062**	0.102**	0.058**	0.099**	0.056**	0.096**	0.053**
	(0.018)	(0.018)	(0.017)	(0.017)	(0.019)	(0.019)	(0.019)	(0.018)
2008: Children<18 in the family (=1)		-0.044**		-0.043**		-0.041**		-0.040**
		(0.013)		(0.013)		(0.014)		(0.014)
Gender (Male=1) * Children (=1)		0.070**		0.067**		0.068**		0.066**
		(0.013)		(0.013)		(0.013)		(0.013)
2008: Experience			0.004+	0.003+			0.003	0.003
			(0.002)	(0.002)			(0.002)	(0.002)
Father's year of edu.	0.003*	0.003+	0.003*	0.003*	0.004*	0.003+	0.004*	0.004*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Mother's year of edu.	0.004+	0.004	0.004+	0.005+	0.005*	0.005+	0.005+	0.005*
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Instrument	D_Q	D_Q	D_Q	D_Q	*Days*	*Days*	*Days*	*Days*
Location Fixed Effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Birth Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Graduation Year fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	5.498	5.066	5.817	5.790	15.258	14.139	16.474	16.430
p^J	0.878	0.897	0.866	0.830				
No. of obs	30418	30418	30418	30418	30418	30418	30418	30418

Note: The dependent variable is a dummy that is equal to 1 if an individual was employed in the private sector in 2008. The instruments in columns (1)-(4) are quarter-of-birth dummies and the instrument in columns(5)-(8) is a continuous variable that counts the days between individual i's birthday and January 1. The F-statistic comes from testing H_0^j $j = 1, 2$, where $H_0^1 : \gamma_{Q2} = 0$, $\gamma_{Q3} = 0$, $\gamma_{Q4} = 0$ and $H_0^2 : \gamma_{Days} = 0$. p^J is the p-value from the Sargan-Hansen test, which tests the null hypothesis that instruments are uncorrelated with the error term. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, + p<0.1. The sample is restricted to individuals with annual earnings above DKK 200,000 in 2008.

Chapter 2

Choice of Electives and Future Leadership

- Evidence from Business School Students

Choice of Electives and Future Leadership

- Evidence from Business School Students*

Christian Møller Dahl†

Marie Skibsted‡

Anders Sørensen§

Abstract

Using detailed educational data on graduates from Copenhagen Business School (CBS), this paper uses within-master's program variation in course selection and performance to model the relationship between detailed educational characteristics and labor market outcomes, which we measure by both the probability of attaining a leadership role and hourly wages. We find that choosing courses in management is a significant predictor of leadership and that individuals who have diversified curricula with many different types of classical business school courses are more likely to attain leadership roles. By contrast, we find that educational diversification outside classical business school courses is insignificant in our model. Consistent with previous findings, we also observe that particularly finance and accounting courses are significantly associated with higher wage outcomes. Finally, we observe a strong gender effect when modeling the selection of course types.

Keywords: Education, leadership, labor market, wage regression
JEL classifications: I21, I26, J24, M12

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†Christian Møller Dahl, Department of Business and Economics, University of Southern Denmark, Campusvej 55, 5230 Odense M, Denmark

‡Marie Skibsted, Department of Economics, Copenhagen Business School, Porcelaenshaven 16 A, 1st floor, DK-2000 Frederiksberg, Denmark.

§Anders Sørensen, Department of Economics, Copenhagen Business School, Porcelaenshaven 16 A, 1st floor, DK-2000 Frederiksberg, Denmark.

1 Introduction

Firms are important contributors to growth, which might explain why the determinants of firm productivity have received increasing attention in the aftermath of the financial crisis. Although many findings in the literature indicate that high-quality management is important for firms' performance (e.g, Bennedsen et al., 2006; Bloom and Van Reenen, 2010; Lazear, 2012; Bloom et al., 2013) and that chief executive officers (CEOs) with general managerial abilities are rewarded with higher wages (e.g., Murphy and Zabojnik, 2004; Custódio et al., 2013), we still know little about the type of educational skills that are, in fact, demanded in the market for leaders.[1] Using a unique dataset containing detailed educational information about individuals who enrolled to pursue a Master of Science (MSc) in Economics and Business Administration at Copenhagen Business School (CBS) between 1984 and 1991, we estimate the relationship between educational choices, such as the type of master's elective courses taken and overall educational profiles, and labor market outcomes, which we measure by the attainment of leadership positions and wage outcomes. The results of this analysis help us understand the educational skills that are, in fact, valued in leadership positions.

Theorists suggest that the most able leaders are generalists (Lazear, 2012) and that the increased importance of general managerial skills has resulted in increased wages for CEOs and in more external hires than internal promotions (Murphy and Zabojnik, 2004, 2007). Evidence from the empirical literature suggests that variation in management practices corresponds with variation in firm productivity and corporate decisions (e.g., Bertrand and Schoar, 2003; Bloom and Van Reenen, 2010; Bloom et al., 2013). The empirical literature also finds that the CEO play an important role in firm performance (e.g., Bennedsen et al., 2006, 2007), that high-quality management enhances worker productivity (e.g., Lazear et al., 2012) and that CEOs with general managerial skills receive significantly higher wages (e.g., Custódio et al., 2013). Such findings suggest that hiring leaders with managerial skills and a breadth of knowledge is one way of improving firm performance.

Managerial skills are general skills that can be transferred across firms and industries, and these skills are distinct from firm- or sector-specific human capital. Managerial skills can be thought of as (1) the knowledge and experience that an individual gains by working in different positions or as a manager (i.e., managerial skills from the labor market) or (2) the managerial and general knowledge that an individual gains through tertiary education (i.e., through a management education). This paper considers the latter type of managerial skills, namely, managerial skills that are acquired through

[1] Lazear (2012) is one of the few researchers to provide insights into this topic by using data on MBA graduates from Stanford to model the relationship between educational characteristics and the probability of attaining a leadership role.

education, and estimates its associations with labor market outcomes. The literature has primarily measured managerial skills with different types of labor market experiences and has estimated their impact on wages (e.g., Custódio et al., 2013; Falato et al., 2015). By contrast, we measure individuals' managerial skills based on their management education and the extent of their educational diversification, and we model the relationship between managerial skills and the probability of attaining a C-level position; a C-level position is defined as employment on a firm's executive board.[2] As in previous studies, we also consider wages as an outcome variable.

The empirical literature that investigates the influence of detailed education choices on leadership is limited, with Lazear (2012) serving as a prominent exception. Using data on MBA students from Stanford, Lazear (2012) shows that individuals with diversified curricula and those who take economics courses are more likely to become leaders.[3] Inspired by the work of Lazear (2012), we contribute to the literature by estimating the potential relationship between management education and the probability of attaining a C-level position. Assuming that firms will take advantage of the gains achieved through improved management practices, we expect that individuals with managerial skills are more likely to attain C-level positions. Our access to very detailed educational data on CBS graduates allows us to empirically model this relationship.

Lazear (2012) also tests whether wide-ranging labor market experience is associated with leadership. Using data on Stanford MBA students, Lazear (2012) shows that individuals who have played many different roles in the labor market are also more likely to secure leadership positions. Along the same lines, Custódio et al. (2013) show that CEOs with general knowledge due to diverse positions in the labor market are the highest paid. Building on these results, we expect that individuals who choose to diversify their curricula through educational choices are more likely to attain C-level positions. Using detailed educational data from CBS, we test this theory by creating empirical measures of individuals' educational diversification and estimate the relationship between these measures and the probability of attaining leadership roles.

This paper primarily focuses on the determinants of leadership. However, we also estimate the relationship between individuals' detailed educational information and wage outcomes. By doing that, we get a better understanding of if and how curricular differences within a specific field of study

[2]The self-employed and entrepreneurs are not considered C-level individuals.

[3]Lazear (2012) introduces a theoretical model of leadership that implies that the most able leaders are generalists, not specialists. Using data on MBA students from Stanford who were surveyed about their labor market experiences, Lazear (2012) tests and confirms his theoretical predictions. Similarly, Falato et al. (2015) estimate a significant premium of different credentials/skills in relation to a CEO's pay. Falato et al. (2015) measure credentials based on an indicator of individuals' reputations, their career track records, and the quality/competitiveness of their undergraduate institutions (the indicator of education is not a measure of field of study).

explain differences in wage outcomes. Thus, in contrast to the ample research assessing the return to particular fields of study, we evaluate the “return” to education on a more detailed level. Studies have already shown that fields of study such as business and engineering are associated with a wage premium (e.g., James et al., 1989; Blundell et al., 2000; Arcidiacono, 2004; Hamermesh and Donald, 2008; Altonji et al., 2012) and that math-related skills have an important impact on wage outcomes (e.g., Joensen and Nielsen, 2009; Bertrand et al., 2010; Joensen and Nielsen, 2015). If certain skills are associated with leadership and if leadership is associated with higher wages, we expect that this relationship will be reflected in wage outcomes. Moreover, based on the findings in the literature, we expect math-related skills to be positively related to wages.

Because our results show that certain course types are positively related to both leadership and wages, we examine the mechanism underlying students’ course choices within a specific master’s program. The literature has almost exclusively focused on the drivers of major choices, and the findings reveal that gender, abilities, expected future earnings, peer effects, and individual preferences are the main determinants of post-undergraduate decisions (e.g., Montmarquette et al., 2002; Arcidiacono, 2004; De Giorgi et al., 2010; Zafar, 2013). Combining the detailed educational data from CBS with the data from Statistics Denmark allows us to identify the individual characteristics that drive the choice of specific course types and the construction of a certain educational profile.

Our access to a detailed dataset with information on individuals who enrolled in the MSc in Economics and Business Administration program at CBS between 1984 and 1991 and graduated between 1986 and 1996 enables us to conduct our empirical studies. These data allow us to compare individuals who graduated from the same master’s program but who displayed differences in terms of course selection and performance. For all individuals, we are able to observe what we call an “educational portfolio”, which we define as a vector containing the following information: (i) the course types taken during the master’s program, (ii) the master’s GPA, and (iii) the degree of educational diversification. Because students who pursue an MSc in Economics and Business Administration take almost entirely elective courses, we observe high variation in the educational portfolios across individuals. This variation in educational choices allows us to estimate the potential relationship between detailed educational characteristics and labor market outcomes.

Due to the structure of the MSc in Economics and Business Administration, students almost exclusively take elective courses, which means that the course choices are endogenous in a regression model. Thus, without any exogenous determinants of course selection, which would allow us to perform Instrumental Variables (IV) estimations, we cannot conclude anything about the causal impact of such educational choices. However, an empirical analysis that is meant to provide a detailed description of the people in leadership positions and their educational characteristics are still valuable. Particularly because the literature on detailed educational decisions and leadership is limited, a study like ours can still be informative and uncover correlations whose causal mechanics can be explored in future research.

We combine the educational data from CBS with the Danish register data and data from the Danish Business Authority, thereby creating a matched employer-employee dataset. With the Danish register data, we gain access to detailed background information on the individuals in our sample, and the Danish Business Authority data provide information on individual who sat on the executive boards of firms that already existed or were founded during the 2000–2010 period. Combining these three data sources enables us to identify individuals who had a C-level position as their main occupation in a Danish joint stock company during the 2000–2010 period and to match this information with educational history, other labor market measures and additional background information.

Overall, this paper contributes to the literature in several ways. We provide insights into the mechanisms underlying selection into specific courses and the labor market consequences of these course selections. Our results suggest a significant and positive association between management courses and the probability of leadership, which complements the results of Bloom and Van Reenen (2010) and Bloom et al. (2013). Furthermore, our results indicate that individuals with a diversified curriculum from their master's program are more likely to attain a C-level position, which is in line with the findings of Lazear (2012). However, our results on educational diversification are ambiguous. In particular, our results show that the impact and significance of educational diversification depends on the set of courses underlying the measure of educational diversification. We find that a diversified curriculum of classical business courses, such as management, marketing, finance, organization, and accounting, is positively associated with subsequent leadership roles. By contrast, being more broadly diversified is insignificant in our estimations. Moreover, we show that certain course combinations are stronger predictors of leadership than other course combinations. Overall, our results suggest that diversification among subjects that complement each other is a good predictor of leadership, whereas overly broad diversification is not.

Turning to the wage estimations, our results show that courses in management, marketing, finance and accounting show a positive and significant coefficient in our model, which is in line with previous findings (Bertrand et al., 2010; Lazear, 2012; Joensen and Nielsen, 2009). Controlling for C-level positions in the wage equation indicates that the marketing wage premium is partly driven by the increased probability of attaining a leadership position.

When estimating the selection into specific course types, we observe a strong gender effect, as women are less likely to choose courses in finance and accounting and more likely to choose courses in marketing and organization. Such results reveal that women are less likely to choose the course types that we find to be associated with an increased probability of attaining a C-level position and with higher wages. Finally, we also see that course supply is significant in the course selection equation, indicating that universities can have an impact on students' course choices through their course catalog offerings.

The rest of the paper is organized as follows: Section 2 outlines the background for the analysis and presents related research. Section 3 introduces the analytical framework, which is based on the background described in Section 2. Section 4 describes the institutional details of the studied master's program at CBS, and Section 5 presents the data from three different datasets. Section 6 and 7 discuss the results, and Section 8 tests the robustness of these results. Finally, Section 9 concludes the paper.

2 Background and Related Literature

Despite the vast amount of research on the determinants of firm performance and productivity, fewer studies have been concerned with the impact and importance of managers and management practices on firm performance (e.g., Bennedsen et al., 2006; Bloom and Van Reenen, 2007, 2010; Bloom et al., 2013; Falato et al., 2015). Bloom and Van Reenen (2010) show that variation in management practices potentially explains the persistent and otherwise unexplained differences in firm productivity. They show that firms with better management practices are larger, perform better and are more likely to survive. Bloom and Van Reenen (2007) measure the differences in management practices across firms and countries and show that market competition and family firms are the two main factors that explain differences in management practices. Firms in highly competitive markets tend to be better managed, whereas firms that pass on leadership to the eldest son are poorly managed. Finally, Bloom et al. (2013) conduct an analysis on a sample of Indian firms; a random sub-sample of these firms was offered assistance with their management practices (the treatment effect). Bloom et al. (2013) compare the productivity of these firms with the productivity of the firms in the control group and find that good management practices improved firm productivity by up to 17%.

Along the same lines as Bloom and Van Reenen (2007, 2010), Bennedsen et al. (2006, 2007) investigate the value of CEOs and show that firm performance varies depending on the CEO. Bennedsen et al. (2006) hypothesize that family deaths increases the time that a CEO spends with his or her family and, using Danish firm level data, find that both the deaths of top managers or members of their immediate family negatively affects firm performance. Bennedsen et al. (2007) show that passing the CEO position on to a family member instead of an external CEO has a considerable negative causal impact on firm performance; therefore, they suggest that professional, non-family CEOs are more valuable to firms than family CEOs.

Falato et al. (2015) formulate two hypotheses, stating (1) that CEOs with better credentials will receive higher wages and (2) that CEOs with better credentials are more likely to improve firm performance and, in turn, benefit shareholders. CEOs' credentials are measured by their reputations, their career track records, and the quality of their educational background, and Falato et al. (2015) find that all measures of credentials are positively associated with pay. Moreover, they find that CEOs with better credentials have a positive impact on the performance of medium-sized and large firms.

Similar to Falato et al. (2015), Custódio et al. (2013) evaluate CEO skills based on their impact on wages. Custódio et al. (2013) create a measure of managerial skills based on managerial experience from the labor market and find that CEOs with general managerial skills receive, on average, significantly higher wages. Finally, Murphy and Zabojnik (2004) and Murphy and Zabojnik (2007) present a theoretical model that predicts that an increase in the importance of more general managerial skills results in an increase in CEO wages and in external hiring (compared with internal promotions).

Lazear (2012) presents a theoretical model of leadership and shows that, within the frame of this model, good leaders, given a certain ability level, have skills and knowledge in many different areas. In short, leaders are generalists as opposed to specialists. Lazear (2012) argues that leaders must be able to make quick decisions within many different areas and thus require a broad skill set. To test his theoretical predictions, Lazear (2012) uses data on students from the MBA program at Stanford Graduate School of Business (GSB). He finds that students with a narrow curriculum at Stanford are less likely to become leaders and that diversified experience in the labor market increases the probability of attaining leadership positions. These results are in line with the expectation that leaders are generalists. Furthermore, he finds that taking economics courses is positively associated with leadership, whereas finance courses are insignificant when estimating the probability of attaining leadership positions. By contrast, finance courses show a significant and positive impact in an estimated wage equation.

As we are interested in the relationship between detailed educational choices and labor market outcomes, we also draw from the literature on the returns to education. Although this literature is vast and still growing (e.g., Angrist and Krueger, 1991; Card, 1999; Blundell et al., 2000; Arcidiacono, 2004; Altonji et al., 2012; Kirkebøen et al., 2014; Altonji et al., 2015), only a few studies have focused on determining the association between specific course choices at the tertiary level and labor market outcomes (e.g., Athey et al., 2007; Bertrand et al., 2010; Lazear, 2012). Similarly, Joensen and Nielsen (2009, 2015) seek to uncover the impact of high school course choices on labor market performance.

Joensen and Nielsen (2015) and Bertrand et al. (2010) are mainly concerned with the influence of course choices in reducing gender differences in the labor market. Using a pilot scheme that exogenously makes acquiring advanced high school mathematics more attractive to and less costly for high-performing girls, Joensen and Nielsen (2015) show that high school mathematics have a positive and causal impact on girls' eventual labor market performances. Bertrand et al. (2010) study the careers of MBAs who graduated from the Booth School of Business at the University of Chicago between 1990 and 2006. They find that the gender wage gap between these MBAs can be explained by differences in business school courses and grades, differences in career interruption (such as maternity leave), and differences in weekly hours worked. In particular, Bertrand et al. (2010) find that the share of finance courses is positively and significant correlated with wage outcomes and accounts for a large part of the gender wage gap. In general, math-related skills are often found to be positively related to labor market outcomes (e.g., James et al., 1989; Hamermesh and Donald, 2008; Joensen and Nielsen, 2009; Bertrand et al., 2010; Joensen and Nielsen, 2015).

3 Conceptual Framework

This paper aims to determine the relationship between specific educational characteristics and both the probability of leadership and wage outcomes. We consider an individual to occupy a leadership position if he or she is on a firm's executive board. Throughout this paper, we will refer to individuals on a firm's executive board as those with C-level positions.

Building on the theoretical and empirical findings presented in the previous section, we assume that managerial skills are important for firm productivity, and we expect that firms hire individuals with such abilities. As such, we expect that individuals with a management education (and the assumed high-quality managerial skills) are more likely to attain C-level positions. Moreover, inspired by the theoretical predictions and empirical findings of, for instance, Lazear (2012) and Custódio et al. (2013), we expect that a good leader has a broad knowledge base and general skills. Thus, we expect that individuals who diversify their curricula via course selection will also more likely to attain C-level positions. These two things are not mutually exclusive. In fact, high-quality leaders presumably, to a certain degree, do both. Finally, based on the findings in the literature (e.g., Joensen and Nielsen, 2009; Bertrand et al., 2010; Lazear, 2012; Joensen and Nielsen, 2015), we expect math-related courses to be positively associated with wage outcomes.

To test which types of detailed educational characteristics that are actually associated with leadership and wages, we specify two regression equations: Equation (1) and Equation (2). Equation (1) is estimated by a probit model, and Equation (2) is estimated by standard Ordinary Least Squares (OLS).

$$P(y_i = 1|X_i, Z_i) = \Phi\Big(\beta_0 + \beta' Z_i + \alpha' X_i + \theta_t\Big) \tag{1}$$

$$y_i = \beta_0 + \beta' Z_i + \alpha' X_i + \theta_t + \varepsilon_i \tag{2}$$

y_i is either a dummy variable that is equal to 1 if the individual held a C-level position (Equation (1)) or the logarithm of the hourly wage (Equation (2)). Z_i is a $r \times 1$ vector of r variables that captures the individual's educational characteristics and choices. Accordingly, β is a $r \times 1$ vector of parameter estimates. We will refer to Z_i as the educational portfolio, which we will define and describe more carefully in Section 5.2. θ_t represents graduation-year fixed effects, where t indicates the graduation year. Graduation-year fixed effects are included to control for macroeconomic conditions and corresponding labor market fluctuations in the graduation year, as they can have an impact on the "first job opportunity" and have longer-term implications. Finally, X_i is a vector of personal characteristics.

4 Institutional Details

The analyses in this paper are based on a dataset with detailed educational information on individuals who enrolled to pursue a MSc in Economics and Business Administration at CBS from 1984-1991. By detailed educational information, we mean information about individuals' mandatory and elective courses and their grades in all courses. Because we use data on individuals who graduated from the same master's program but took different electives, we are able to model the relationship between detailed educational characteristics and labor market outcomes.

Between 1984 and 1991, CBS only offered one master's program in economics and business, namely, an MSc in Economics and Business Administration. The educational structure was reformed in 1992, and, instead of a very general master's program with many electives, CBS introduced a number of different master's programs. To compare the impact of individuals' specific course choices—in contrast to the impact of different master's programs—on labor market outcomes, we only consider individuals who enrolled before 1992. After the reform, CBS decided that 1996 would be the last year in which individuals from the old scheme could graduate. Thus, we have also limited our sample to individuals who graduated before 1996. In so doing, we have a sample of individuals from the same master's program who show high variation in the types of courses selected.

In the 1984-1991 period, the MSc in Economics and Business Administration at CBS was structured as follows: the program included a 2-year full-time curriculum, and students were expected to complete 1,800 working hours (WH) each year. Entering the program, students had only one mandatory course/project, apart from the mandatory master's thesis. In 1984 and 1985, the mandatory course was general economics, whereas the mandatory course from 1986-1991 was called "Advanced Project Work". Both courses required the completion of a report, which was mostly performed in groups. The syllabus was more or less the same across these two differently titled courses, and only the evaluation format changed between 1985 and 1986. The remainder of the program consisted of electives and a master's thesis.[4]

For the mandatory course, the master's thesis, and the electives, the workload was distributed as 900 WH, 900 WH and 1,800 WH, respectively. This structure means that students who pursue an MSc in Economics and Business Administration at CBS generally decide how they will put together their curriculum and whether they want to be specialists or generalists.[5] For the elective part of the program, students may choose to enroll in, for instance, four 400-WH courses and one 200-WH course or three 400-WH courses and three 200-WH courses. As such, the total number of courses can vary a bit across individuals.

The described structure of the master's program means that we cannot avoid talking about self-selection. By design, individuals self-select into electives. Thus, when estimating the relationship between course choices and labor market outcomes, we cannot distinguish between the selection effect and the pure course effect (more about this later). However, this structure ensures enough variation in course choices across individuals to compare the labor market outcomes of individuals who graduated with the same master's degree, though with different course choices and curricula.

[4]A complete list of the departments that offered electives is presented alongside the share of our sample who took at least one course in the given department in Table A.1 on page 111.

[5]Some guidelines recommend certain courses be taken together, but, overall, students are free to choose courses.

5 Data

Our access to three different data sources allows us to create a matched employer-employee dataset, which enables us to estimate Equations (1) and (2). The core of our analysis utilizes data with detailed educational information on individuals who graduated with an MSc in Economics and Business Administration from CBS between 1986 and 1996. These individuals make up our sample and are the foundation for our analysis. Combining these data with data from Statistics Denmark and the Danish Business Authority provides us with information on labor market achievements and background characteristics of these individuals.

5.1 Labor Market and Background Data

When creating our estimation data, the first step involves merging the educational data from CBS with the administrative register data from Statistics Denmark. In so doing, we obtain information on individuals' backgrounds and eventual labor market outcomes. From Statistics Denmark, we obtain labor market information from the Integrated Database for Labor Market Research (IDA). The IDA covers the total population of workers in Denmark and allows us to identify and match workers and firms consistently over time. The latter is important because it enables us to identify the firms in which individuals had their primary occupation.

As we are interested in modeling the probability of attaining leadership roles, we need to identify the individuals who constitute a firm's executive board (C-level individuals). As the data from Statistics Denmark only enable us to determine whether an individual is considered a "manager", we use data from the Danish Business Authority because these data have information on individuals on the executive boards of all joint stock companies that already existed or were founded during the 2000–2010 period. By combining the data from Statistics Denmark and the Danish Business Authority, we can identify individuals who were wage employed and had a registered C-level position as their main occupation in November of each year from 2000–2010.[6] As such, we do not regard the self-employed or entrepreneurs as holding a C-level position.[7]

Although the labor market information is based on the 2000–2010 period, we have not created a

[6]We only use information obtained in November of each year. Thus, if individuals hold C-level positions for instances in the first 6 months of the year but not in November, these individuals are not registered as holding C-level positions in that year.

[7]The data from the Danish Business Authority contains information about individuals on executive boards and boards of directors. This information is extremely important for a study like ours. However, the data are messy, and some individuals have missing or inconsistent information with regard to the variable of interest. We exclude these observations. To ensure that we do not incorrectly classify someone as a C-level individual, only individuals who are reported to sit on a firm's executive board in the Danish Business Authority data and to hold a top position (or listed as wage-employed without a label) in Statistics Denmark are considered C-level individuals.

panel. Because we want to capture the potential association between detailed educational information (time invariant) and labor market outcomes, the panel dimension does not offer additional information. Instead, we create a cross-sectional dataset with individual-level means over the considered period. When computing the individual-level means over the 2000–2010 period, we exclude years in which an individual is not observed in the Statistics Denmark database. If individuals live outside Denmark, they are not found in the data, and we cannot know whether they are active in the labor market outside Denmark.

Creating cross-sectional data means that we formulate our main dependent variable as a binary variable that is equal to one if an individual had a C-level position at least once during the 2000–2010 period and zero otherwise. For our wage regressions, we create a dependent variable that is the mean hourly wage across the ten-year period. We discard observations in which the hourly wage rate is unobserved or is measured with inadequate precision according to Statistics Denmark, and we also exclude wage observations for self-employed individuals. We categorize missing wage observations as "non-C-level", which means that we perform our wage and C-level estimations on two different samples.

From Statistics Denmark we also obtain specific information on individuals, including information about the children in the household and civil status (married or single). We rely on information obtained in 2000—the first year from which we have labor market information. Excluding non-Danes who studied at CBS, individuals with missing background information and those who did not appear in the data during the 2000–2010 period, we have a sample of 1,835 individuals who graduated with an MSc in Economics and Business Administration from CBS between 1986 and 1996. Table 1 presents summary statistics on all the non-educational variables.

Table 1 shows that 12% of our sample held a C-level position at least once during the 2000–2010 period. The average number of years that these individuals held a C-level position was 3.90 years.[8] Women account for 33% of our sample, but only 4% of the women held a C-level position at least once. By contrast, 16% of the men in the sample held a C-level position once during the 2000–2010 period. Sixty-seven percent of the C-level sample was married in 2000, and 67% had children in 2000. By contrast, only 60% of the non-C-level individuals were married in 2000, and only 59% had children. As expected, we see a significant wage difference between C-level and non-C-level individuals and between men and women.[9]

Table 1 also shows that C-level individuals had been on the labor market longer than non-C-level individuals in 2000 (the time between their graduation and 2000). The inclusion of graduation-year

[8]Figure A.1 presents the distribution if the years as a C-level individual are included.
[9]Note that 1 US dollar is equal to approximately 6.53 DKK.

fixed effects in our regression models ensures that we control for this variation in years on the labor market across individuals.

Table 1: Summary Statistics

	All	Not C-level	C-level	Difference	Men	Women	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender, female=1	0.33	0.37	0.11	0.26***			
	(0.47)	(0.48)	(0.31)				
Father with manager position	0.10	0.091	0.18	-0.088***	0.10	0.096	0.0085
	(0.30)	(0.29)	(0.38)		(0.31)	(0.30)	
Time since graduation	8.02	7.92	8.73	-0.80***	8.17	7.74	0.43***
	(2.47)	(2.42)	(2.71)		(2.57)	(2.22)	
Children in 2000	0.60	0.59	0.67	-0.080**	0.58	0.65	-0.067***
	(0.49)	(0.49)	(0.47)		(0.49)	(0.48)	
Married in 2000	0.57	0.55	0.67	-0.11***	0.55	0.59	-0.039
	(0.50)	(0.50)	(0.47)		(0.50)	(0.49)	
Age in 2000	35.1	35.0	35.4	-0.40**	35.3	34.6	0.73***
	(2.64)	(2.66)	(2.48)		(2.63)	(2.60)	
Average hourly wage in DKK	362.4	331.8	580.3	-248.5***	403.0	281.2	121.8***
	(239.6)	(193.4)	(382.6)		(262.0)	(158.8)	
C-level position	0.12				0.16	0.039	0.12***
	(0.33)				(0.37)	(0.19)	
Number of years as CEO			3.90				
			(2.84)				
No. of obs	1835	1612	223	1835	1222	613	1835

Note: Means and standard errors in parenthesis are reported in Columns (1), (2), (3), (5), and (6). Column (4) reports the mean difference between the sample of not C-level individuals and the sample of C-level individuals and Column (7) reports the mean difference between men and women. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Educational Data

All the individuals in our sample enrolled in the MSc in Economics and Business Administration program at CBS between 1984 and 1991 and graduated between 1986 and 1996. Excluding non-Danes and individuals with missing labor market or background information, we have very detailed educational information on a sample of 1,835 individuals.[10] Using the educational data from CBS, we create what we will refer to as an *educational portfolio* (Z_i). For each individual, we create an educational portfolio that contains information on educational achievements at CBS, specific course types taken, and the extent of educational specialization/diversification. Table 2 shows the variables included in the educational portfolio and summary statistics.

[10] In our sample, we keep students who either have a bachelor's degree from somewhere outside CBS or have graduated with a Bachelor of Science in Economics and Business Administration. This criterion means that we exclude approximately 50 students with a different bachelor's degree from CBS.

The educational portfolio includes five dummy variables, each indicating whether a student took courses in a specific department. The five departments considered are **Business Economics and Leadership**, **Accounting**, **Marketing**, **Finance**, and **Organization and Labor Market Sociology**, which we consider the five classical business school departments at CBS. We focus on these departments because the course types that they offer correspond with the most common types of C-level positions in firms, namely, the CEO, the chief operations officer (COO), the chief financial officer (CFO), and the chief marketing officer (CMO); therefore, these five departments ex ante can be expected to be relevant for leadership. Moreover, they all offer courses that are considered classical courses for a business education.[11]

Table 2: Educational Portfolio

Variable	Definition	All (1)	Not C-level (2)	C-level (3)	Diff. (4)
Man_D	Equal 1 if at least one course was taken at the Dep. of Business Economics and Leadership	0.70	0.69	0.82	-0.13***
		(0.46)	(0.46)	(0.39)	
Acc_D	Equal 1 if at least one course was taken at the Dep. of Accounting	0.17	0.16	0.22	-0.060**
		(0.38)	(0.37)	(0.42)	
Mar_D	Equal 1 if at least one course was taken at the Dep. of Marketing	0.61	0.61	0.62	-0.012
		(0.49)	(0.49)	(0.49)	
Fin_D	Equal 1 if at least one courses was taken at taken at the Dep. of Finance	0.23	0.22	0.30	-0.081**
		(0.42)	(0.41)	(0.46)	
Org_D	Equal 1 if at least one course was taken at the Dep. of Organization and Labor Market Sociology	0.38	0.39	0.33	0.062*
		(0.49)	(0.49)	(0.47)	
Diversification 1	Total number of different departments/courses in the educational portfolio	2.91	2.90	3.00	-0.098
		(0.88)	(0.88)	(0.83)	
Diversification 2	Total number of different classical business departments/courses (**C**) in the educational portfolio	2.09	2.07	2.29	-0.22***
		(0.70)	(0.71)	(0.63)	
Master GPA	Grade point average from the Master program. Calculated based on the Danish 13-grading scale.	8.50	8.50	8.52	-0.026
		(0.83)	(0.83)	(0.82)	
Entry GPA	Measure of ability when entering the master program. Average over high school and bachelor GPA.	7.70	7.67	7.86	-0.18***
		(0.70)	(0.68)	(0.80)	
Starting age	Age when starting at the master program	23.8	23.9	23.6	0.26**
		(1.73)	(1.76)	(1.48)	
Number of students		1835	1612	223	

Note: Means Standard errors (in parenthesis) are reported in Columns (1)-(3). Column (4) reports the mean difference between the sample of not C-level individuals and the sample of C-level individuals. *** p<0.01, ** p<0.05, * p<0.1.

[11] Individuals pursuing an MSc in Economics and Business Administration at CBS had the option of taking electives in the departments listed in Table A.1. The departments that offered classical business courses are labeled with **C** (classical), and the remaining departments are labeled with **O** (other). Except for **International Economy and Management**, the departments labeled with **C** are those in which most students took courses. International Economy and Management is not included in the pool of considered departments because it does not offer what we consider to be classical business school courses; it instead offers a very broad supply of courses, ranging from development economics to international management.

To test whether educational diversification increases the probability of leadership, the educational portfolio also includes measures of educational diversity. CBS is a large institution and offers a wide variety of courses in its departments. As such, diversification across all the departments at CBS alone might not be positively associated with leadership. The study guidelines from CBS support this assumption, as they suggest certain course combinations for different careers, indicating that not all courses are well suited for all types of labor market participation. To test how the type and the extent of educational diversification is associated with leadership, we create two different measures of educational diversification. Our first measure, *Diversification 1*, counts the total number of departments in which a student took courses, and our second measure, *Diversification 2*, counts the number of classical departments in which a student took courses.

Table 2 reveals that the share of individuals with management courses differs across C-level individuals and non-C-level individuals. When pooling all the years, the share of C-level individuals and non-C-level individuals who took management courses is 0.82 and 0.68, respectively, which reveals significantly different shares. Figure 1 reflects similar findings across enrollment years.[12]

Figure 1: Share of Students With Management Courses Across Enrollment Year and C-level Status

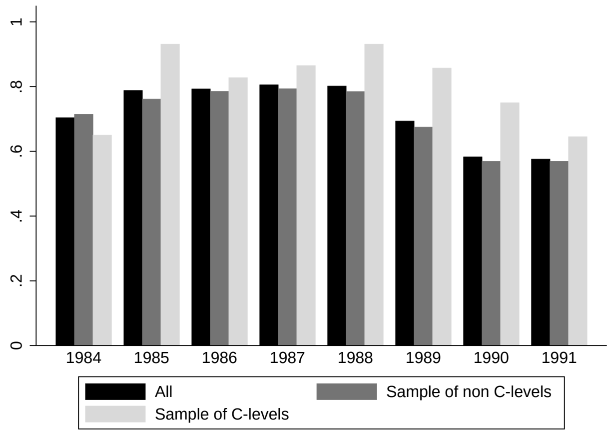
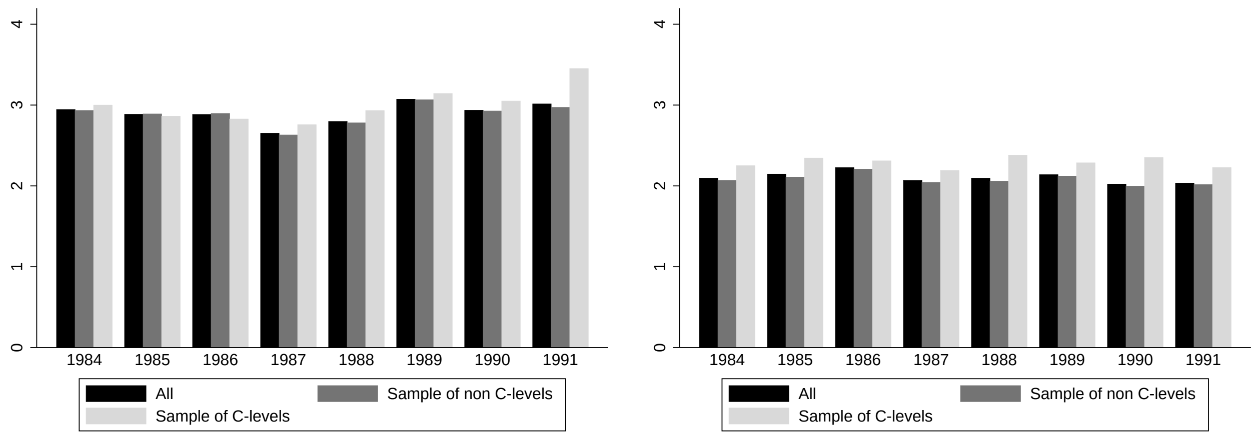


Table 2 and Figure 2 show the degree of educational diversification across enrollment years and C-level/non-C-level individuals. Table 2 shows a significant difference between C-level and non-C-level

[12]Figure 1 shows a relatively sharp decline in the share of individuals taking management courses from 1989 to 1990. This drop can likely be explained by a measurement error in the data due to the 1992 reform of the master's program in question. The reform is reflected in the data; courses taken under the umbrella of the new system are not labeled by department. As such, some courses taken after 1992 (mostly students enrolled in 1990 and 1991) might be wrongly categorized as "other", which means that we might underestimate the share of students who took management courses in 1990 and 1991. We thus risk comparing students with management courses to students with and without management courses. As such a comparison would only make our results weaker, we do not considered it a crucial threat to our results.

individuals in the second measure of diversification, *Diversification 2*, but no significant difference in the first diversification measure, *Diversification 1*. Figure 2 separately indicates the same pattern across enrollment years.

Figure 2: Diversification Across Enrollment Years and C-level Status



(a) Average of Diversification 1 **(b) Average of Diversification 2**

The master's GPA is also included in the educational portfolio. Until 2007, grades in Denmark were rewarded according to the Danish "13" grading scale. On this grading scale, the highest grade is 13, which only an exceptionally independent and excellent performance merits, and the lowest passing grade is 6. The "13" grading scale never awards a value of 12, meaning that it jumps from 11 to 13, and students almost never receive a 13. An average of 8 indicates that a student is just above the average.[13] To ease the interpretation of the estimated coefficients when GPA is included in the regression models, we use a standardized measure of master's GPA. This means that we include a standardized measure of the master's GPA computed as $GPA^s = \frac{GPA_i - \overline{GPA}}{sq(GPA)}$, such that it has mean zero and standard deviation one.

6 Results

6.1 Educational Characteristics and Labor Market Outcomes

Table 3 reports the results from the estimations of Equations (1) and (2). Columns (1)-(6) present the results from estimating the probability of attaining a C-level position, and columns (7)-(12) present the results from the estimated wage equation. Columns (1)-(4) show that management course(s) are a

[13] See the Main Appendix A of this thesis for a translation of the Danish grading scale.

very strong predictor of leadership and that this result is robust to the inclusion of additional course dummies. Across all specification, we observe a strong correlation between management courses and the probability of leadership positions, which confirms our expectations. This positive relationship might result from management education improving managerial skills, which improves firm performance and, in turn, increases the probability of being hired for a C-level position. However, the estimated positive relationship may result from individuals with leadership skills being more likely to sort into management courses; thus, the estimated effect may be due to self-selection. We will discuss self-selection further later in this section.

Adding additional course-specific dummies to the regression reveals that management, marketing, and accounting are the course types that significantly predict leadership. However, the marketing dummy only enters the model significantly when we also control for finance (see columns (3) and (4)). Thus, the significant and positive coefficient on the marketing dummy is sensitive to the reference group. By contrast, organization and finance are insignificant in the C-level regressions. The latter finding is in line with the results of Lazear (2012), who also finds that finance courses are insignificant in predicting leadership.

The results in columns (2)-(4) of Table 3 also suggest that having a combination of management, accounting and marketing in the educational portfolio is associated with a higher probability of attaining a C-level position compared with only having one or two of these course types in the portfolio. Moreover, we observe that the included course dummies are jointly significant ($p < 0.01$). These results indicate that diversification is positively associated with the probability of attaining a C-level position. To investigate the importance of educational diversification further, we include our two measures of educational diversification, namely, *Diversification 1* and *Diversification 2*. *Diversification 1* counts the total number of different departments/courses that are represented in the educational portfolio, and *Diversification 2* counts the number of classical departments/courses (**C**) that are represented in the educational portfolio.

Columns (5) and (6) present the results from the estimations that include the diversification measures. Because the diversification measures are created based on the course dummies, we do not include the diversification measures together with the course dummies in the regressions, as doing so would lead to multicollinearity. Interestingly, columns (5) and (6) show that diversification between a limited and relevant pool of courses is a good predictor of leadership, whereas broader diversification is insignificant in the estimations (*Diversification 2* enters significant and *Diversification 1* enters insignificant). These results provide a deeper and more intuitive understanding of the association between leadership

Table 3: Baseline Results - The Impact of Course Choice

	C-level Probit Regression						Wage OLS Regression					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Man_D =1	0.047***	0.051***	0.052***	0.054***			0.054**	0.067***	0.077***	0.074***		
	(0.015)	(0.015)	(0.015)	(0.015)			(0.022)	(0.022)	(0.021)	(0.021)		
Acc_D =1		0.063***	0.057**	0.062**				0.159***	0.114***	0.110***		
		(0.024)	(0.025)	(0.025)				(0.031)	(0.031)	(0.032)		
Mar_D =1		0.022	0.028*	0.033**				0.013	0.058***	0.053**		
		(0.016)	(0.016)	(0.017)				(0.021)	(0.021)	(0.022)		
Fin_D =1			0.021	0.027					0.177***	0.171***		
			(0.020)	(0.021)					(0.029)	(0.029)		
Org_D =1				0.018						-0.016		
				(0.017)						(0.021)		
Diversification 1					0.011						0.022**	
					(0.008)						(0.011)	
Diversification 2						0.040***						0.074***
						(0.010)						(0.013)
Standardized master GPA	0.009	0.011	0.012	0.012	0.008	0.011	0.045***	0.048***	0.057***	0.057***	0.044***	0.049***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Gender, female=1	-0.138***	-0.134***	-0.131***	-0.132***	-0.139***	-0.136***	-0.300***	-0.284***	-0.264***	-0.263***	-0.299***	-0.296***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.019)
Starting age	-0.005	-0.005	-0.005	-0.005	-0.006	-0.005	-0.006	-0.005	-0.002	-0.002	-0.006	-0.005
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Father with manager position=1	0.087***	0.088***	0.086***	0.086***	0.089***	0.085***	0.101***	0.103***	0.098***	0.099***	0.101***	0.097***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.037)	(0.037)	(0.036)	(0.036)	(0.037)	(0.037)
Children in 2000	0.022	0.022	0.022	0.022	0.022	0.023	0.044*	0.045**	0.040*	0.040*	0.045*	0.043*
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.023)
Married in 2000	0.033*	0.034*	0.034*	0.033*	0.035**	0.033*	0.101***	0.105***	0.105***	0.106***	0.103***	0.103***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.022)
Age in 2000	0.067	0.069	0.070	0.071	0.083	0.078	0.115	0.139*	0.148*	0.148*	0.130*	0.122
	(0.064)	(0.064)	(0.064)	(0.064)	(0.066)	(0.065)	(0.078)	(0.078)	(0.076)	(0.076)	(0.078)	(0.078)
Age in 2000 squared	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002*	-0.002**	-0.002**	-0.002**	-0.002**	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
p		0.001	0.001	0.001				0.000	0.000	0.000		
No. of obs	1835	1835	1835	1835	1835	1835	1795	1795	1795	1795	1795	1795

Note: Through columns (1)-(6) the dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the 2000–2010 period. Through columns (7)-(12) the dependent variable is the logarithm of the average hourly wage during the 2000–2010 period. Columns (1)-(6) report average marginal effects (AME). When computing AMEs for dummy variables, we report the effect from the discrete change from 0 to 1. p is the p-value from testing the hypothesis that all included course dummies are jointly significant. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

and diversification. Broad diversification or diversification outside the classical business school topics does not predict leadership, whereas diversified knowledge within a relevant pool of courses is a good predictor of future leadership.

Using data on MBAs from Stanford, Lazear (2012) finds that having a specialized education has a negative impact on the probability of attaining leadership. We find that only diversification within a pool of classical business school course types is a significant predictor of leadership. Because the course catalog at Stanford GSB is probably not as broad as the course catalog at CBS, *Diversification 2* likely functions much like the measure of education generalization used by Lazear (2012). Thus, our finding might be well in line with the finding of Lazear (2012).

Turning to the results from estimating the wage equation (columns (7)-(12)), unsurprisingly, we observe that particularly finance and accounting enter the wage equation with a positive and significant coefficient. Although we are unable to determine causality, it is worth noting that the coefficient on the finance dummy is very large. The findings that both accounting and finance are important for wage outcomes complements the literature that shows that mathematics is positively correlated (even causal) with wage outcomes (e.g., Joensen and Nielsen, 2009, 2015). Finance enters the model significantly and with a positive sign, which also confirms the findings of Bertrand et al. (2010) and Lazear (2012).

Additionally, management is significant in the wage equation with a positive, though relatively small, coefficient. Given the results from the C-level regressions, this finding is not overly surprising. However, the question becomes whether management in itself is associated with a positive wage premium or the relationship works through the increased probability of attaining a C-level position. Section 6.3 discusses this question further.

Moreover, after controlling for finance courses, marketing enters the wage equation positively and significantly. Thus, marketing does not offer a wage premium when compared with individuals with finance courses. However, when controlling for finance courses, we observe a positive and significant correlation between wage outcomes and marketing courses.

The master's GPA is insignificant in the C-level regression, though positively and significantly correlated with wage outcomes. A one-unit increase in the master's GPA increases wages outcomes by 4.5%. Having a father who once held a manager position is also positively and significant associated with both leadership and wages.[14] Finally, being married in 2000 enters with a positive and significant coefficient in both the C-level and wage regressions. Having children in 2000 also positively influences the wage regression. As being married and having children are found to have positive effects for men

[14]We obtain data on parents from the Danish Register Data, and these "management positions" could also be lower-level managers.

and negative effects for women, these results are most likely driven by men.

Section 4 described the structure of the master's program and self-selection into electives. Due to self-selection into course types, we cannot distinguish between the *selection effect* and the pure *course type effect* in our estimations. Thus, one explanation for the positive association between attaining a C-level position and taking management courses (or any other course type) could be that individuals who are already aiming to attain a C-level position during their studies are focused and, in turn, choose management courses. Another explanation for this positive relationship could be that management courses provide students with skills that make them well suited to leadership, which will make them more likely to be hired for a C-level position. Thus, the relationship between management courses and leadership could result from either a pure course effect or a pure selection effect.

In other words, we cannot determine whether a management education makes students better leaders and, in turn, increases their probability of attaining a C-level position or if students are pre-determined to be leaders before choosing management courses. Their upbringing and the characteristics of their parents are important in developing leadership ambitions later in life. However, the ability to lead a firm in a globalized economy in which conditions constantly change might not be pre-determined. Thus, today's leaders have likely engaged in an educational process at some point that enables them to lead under such conditions (see also Custódio et al., 2013). This means that our results are likely driven by both a management education effect and a selection effect. The distinction between these two effects and the identification of a "management education effect" is something that should be investigated in future research.

6.2 Complementarity or Diversification?

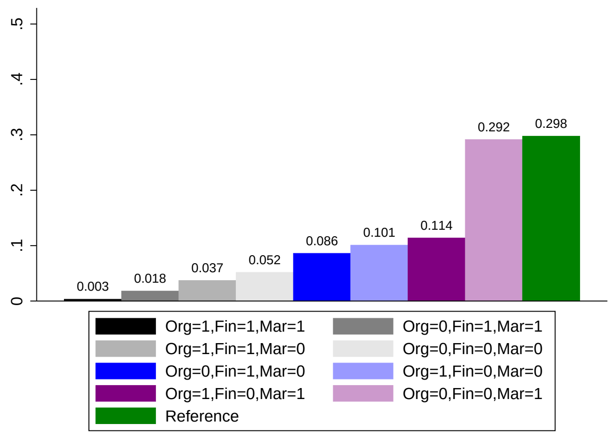
The results in Table 3 suggest that educational diversification among a pool of relevant courses is a good predictor of future leadership, whereas broad diversification is not necessarily valued for leadership. Furthermore, the results suggest that management, marketing, and accounting are the important courses for leadership positions. In this section, we further investigate the mechanisms that drive the diversification results.

Figure 3 shows the share of students with certain course combinations who took at least one management course. Thus, Figure 3 provides an indication of how students combine course types.[15] We see that a large share of the students combine management and marketing. Given that 61% of the students have marketing in their educational portfolios and only 23% and 17% have finance and

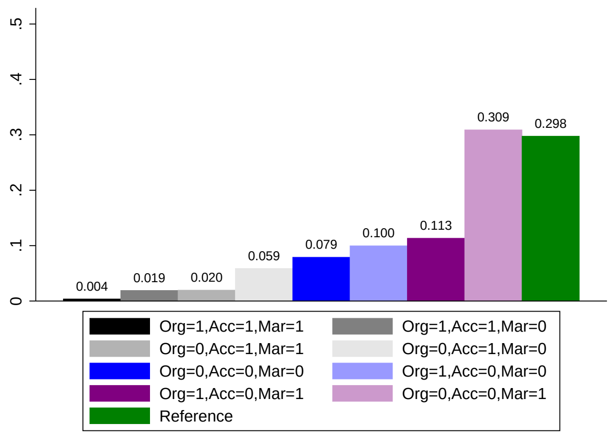
[15] Table A.2 shows the corresponding numbers, and Table A.3 shows additional summary statistics across course types.

accounting, respectively, in their educational portfolios, the large share of individuals with marketing and management is perhaps not surprising. However, in Figure 3c, we see that the share of students that combined finance and accounting is slightly larger (4.6%) than both the share of students that combined finance and marketing (3.8%) and the share of students that combined accounting and marketing (2.1%), even though many more students took marketing courses compared with finance and accounting courses. Additionally, Figure 3a shows that the share of students with organization and marketing is larger than the share of students with marketing and finance and the share of students with organization and finance. Figure 3b shows a similar picture to that in Figure 3a, though with accounting rather than finance. Overall, the figures indicate that students are more willing to combine courses that are similar in terms of demanded and taught skills.

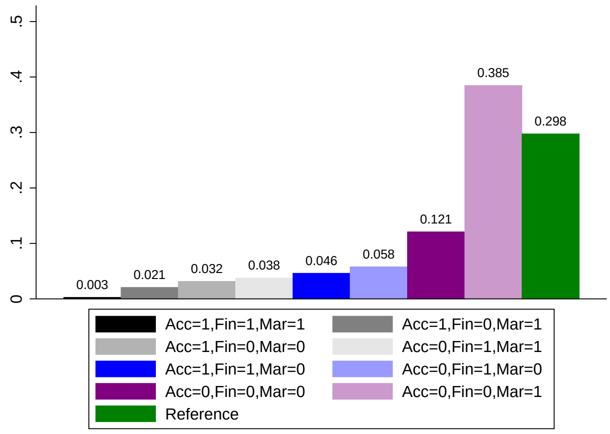
Figure 3: Course Combinations Given Management=1



(a) Combinations of Organization, Marketing, and Finance given Management=1



(b) Combinations of Organization, Marketing, and Accounting given Management=1



(c) Combinations of Accounting, Marketing, and Finance given Management=1

To understand whether a complementarity effect exists between different course types, we include complementarity dummies that correspond to the 8 groups depicted in Figures 3a-3c in our model and estimate the probability of attaining a C-level position using a linear probability model. We estimate using the linear probability model to ease the interpretation of the marginal effects of our complementarity dummies. This means that we specify Equation (3) as follows and estimate it using OLS:

$$y_i = \beta_0 + \beta' Z_i + \alpha' X_i + \theta_t + \mu_i \tag{3}$$

y_i is the C-level dummy, and both X_i and θ_t are defined as in Equation (1). For each regression, the educational portfolio, Z_i, now contains 8 complementarity dummies. Thus, we investigate whether, given at least one management course in the educational portfolio, some course combinations are stronger predictors of C-level positions than others. To examine the impact of management courses on the course combinations, we also estimate the model with dummies that represent the same combinations and with $\text{Man}_D = 0$. Table 4 presents the results from the estimations of Equation (3).

Table 4 shows that management in combination with either finance, accounting or marketing is a good predictor of future leadership. Furthermore, combinations of marketing and organization (column (1) and (3)) or finance and accounting (column (5)), conditioned on having at least one management course, enters the regression significantly and positively. The results from column (5) also show that the combination of finance and accounting (together with management) predicts leadership more strongly than the combination of management and finance or of management and accounting. Moreover, we observe that combinations of, for instance, management, finance and organization; management, accounting and organization; or management, finance and marketing are insignificant in the regressions.

Comparing the results in columns (1), (3) and (5) to the results in columns (2), (4) and (6), we observe that the effect of diversification is conditioned on having a management course in the educational portfolio. Diversification without management in the educational portfolio can actually be negatively associated with leadership. For instance, combinations of marketing and organization courses without a management course enters the model with a significant and negative sign.

The results from columns (1), (3) and (5) indicate that, conditioned on having at least one management course, diversification within course types that somehow complement one another is a strong predictor of leadership. For instance, combining marketing and organization or finance and accounting is a significant predictor of leadership, whereas combining finance and organization is not. We interpret

Table 4: Complementarity or Diversification

	Linear Probability Model					
	Man_D=1	Man_D=0	Man_D=1	Man_D=0	Man_D=1	Man_D=0
	(1)	(2)	(3)	(4)	(5)	(6)
$Org_D = 1, Fin_D = 1, Mar_D = 1$	0.114	0.190				
	(0.143)	(0.187)				
$Org_D = 0, Fin_D = 1, Mar_D = 1$	0.015	-0.032				
	(0.044)	(0.054)				
$Org_D = 1, Fin_D = 1, Mar_D = 0$	0.032	-0.016				
	(0.059)	(0.083)				
$Org_D = 0, Fin_D = 0, Mar_D = 0$	0.002	-0.118***				
	(0.034)	(0.038)				
$Org_D = 0, Fin_D = 1, Mar_D = 0$	0.102***	-0.047				
	(0.034)	(0.033)				
$Org_D = 1, Fin_D = 0, Mar_D = 0$	0.009	-0.036				
	(0.022)	(0.032)				
$Org_D = 1, Fin_D = 0, Mar_D = 1$	0.067**	-0.053**				
	(0.027)	(0.021)				
$Org_D = 0, Fin_D = 0, Mar_D = 1$	0.051***	-0.053**				
	(0.019)	(0.022)				
$Org_D = 1, Acc_D = 1, Mar_D = 1$			0.034	-0.107***		
			(0.119)	(0.029)		
$Org_D = 1, Acc_D = 1, Mar_D = 0$			0.038	0.051		
			(0.054)	(0.085)		
$Org_D = 0, Acc_D = 1, Mar_D = 1$			0.105	-0.044		
			(0.066)	(0.054)		
$Org_D = 0, Acc_D = 1, Mar_D = 0$			0.103***	-0.024		
			(0.040)	(0.039)		
$Org_D = 0, Acc_D = 0, Mar_D = 0$			0.034	-0.142***		
			(0.032)	(0.016)		
$Org_D = 1, Acc_D = 0, Mar_D = 0$			0.008	-0.053*		
			(0.023)	(0.030)		
$Org_D = 1, Acc_D = 0, Mar_D = 1$			0.069**	-0.041*		
			(0.027)	(0.022)		
$Org_D = 0, Acc_D = 0, Mar_D = 1$			0.042**	-0.050**		
			(0.019)	(0.023)		
$Acc_D = 1, Fin_D = 1, Mar_D = 1$					-0.074**	-0.081***
					(0.037)	(0.030)
$Acc_D = 1, Fin_D = 0, Mar_D = 1$					0.115*	-0.052
					(0.066)	(0.054)
$Acc_D = 1, Fin_D = 0, Mar_D = 0$					0.043	0.021
					(0.043)	(0.074)
$Acc_D = 0, Fin_D = 1, Mar_D = 1$					0.028	0.022
					(0.044)	(0.066)
$Acc_D = 1, Fin_D = 1, Mar_D = 0$					0.118**	-0.017
					(0.046)	(0.040)
$Acc_D = 0, Fin_D = 1, Mar_D = 0$					0.067*	-0.104***
					(0.039)	(0.033)
$Acc_D = 0, Fin_D = 0, Mar_D = 0$					-0.003	-0.075***
					(0.021)	(0.027)
$Acc_D = 0, Fin_D = 0, Mar_D = 1$					0.051***	-0.054***
					(0.017)	(0.017)
Individual course effects included	No	No	No	No	No	No
No. of obs.	1835	1835	1835	1835	1835	1835

Note: The dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the 2000–2010 period. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

this finding as a complementarity effect, whereby diversification within similar areas is beneficial for leadership.

Many factors may explain this complementarity effect. First, although individuals should diversify their educational portfolios and skill sets, different types of abilities are preferred for different C-level positions within a firm. For instance, the combination of finance and accounting may be beneficial for a CFO, whereas marketing may be more important for a CMO. Second, individuals who know their strengths and weaknesses understand how to take advantage of their specific abilities and, in turn, seek diversification in an area in which they perform best. These individuals might also be better at recognizing the strengths of others (e.g., employees), which is likely a valuable quality in a leader.

Overall, the results point towards the same conclusions as in Section 6.1. Diversification within a narrow area is a significant and positive predictor of leadership, whereas broad educational diversification is not significantly associated with leadership.

6.3 Direct or Indirect Wage Relationship

Table 3 presents our baseline results, which show a positive association between wage outcomes and management, marketing, accounting and finance courses, although the magnitude of these estimates differs significantly across the different course types. Despite that we cannot determine causality, an understanding of the relationship between course types and wage outcomes might still help us better understand the positive relationship between business education and wage outcomes, which has been documented in the literature (e.g. James et al., 1989; Blundell et al., 2000; Arcidiacono, 2004; Hamermesh and Donald, 2008; Altonji et al., 2012).

To investigate whether the course wage premiums of management, marketing, and accounting result from the increased probability of attaining a C-level position or work through a direct channel, we include a C-level dummy in all the regressions in addition to the course-specific explanatory variables. This C-level dummy takes a value one if the individual had a C-level position at least once during the 2000–2010 period. We want to investigate whether we capture a positive relationship between management courses and wage outcomes because management predicts leadership and leaders earn more or, instead, a more direct relationship exists between management courses and wage rates. If the management dummy loses its prediction power when the C-level dummy is included, a more indirect relationship is implied (similar for marketing and accounting dummies).

Table 5 presents the results from these estimations. Unsurprisingly, the C-level dummy enters the wage equation with a significant and positive sign. This result holds true across all specifications.

Moreover, all the course dummies stay significant in the wage regression. However, the marketing dummy is sensitive to the inclusion of the C-level dummy and is only significant at the 10% level in column (3). As such, part of the marketing wage premium might be driven by the increased probability of attaining a C-level position. For all the course dummies, we observe that the magnitude of the estimated coefficients decreases slightly compared with the baseline results. However, the differences between the estimated coefficients are not significant. Overall, the results indicate that course types within management, accounting, and finance are all directly related to wage outcomes.

Table 5: Wage Estimation - Direct or Indirect Relationship

	OLS estimation			
	(1)	(2)	(3)	(4)
C-level position			0.387***	0.392***
			(0.034)	(0.034)
Diversification 2		0.074***		0.057***
		(0.013)		(0.013)
Man_D =1	0.074***		0.051**	
	(0.021)		(0.020)	
Acc_D =1	0.110***		0.087***	
	(0.032)		(0.031)	
Mar_D =1	0.053**		0.037*	
	(0.022)		(0.021)	
Fin_D =1	0.171***		0.159***	
	(0.029)		(0.028)	
Org_D =1	-0.016		-0.024	
	(0.021)		(0.020)	
No. of obs	1795	1795	1795	1795

Note: The dependent variable is the logarithm of the average hourly wage during the 2000–2010 period. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

7 What Determines Course Selection

The results from the previous section show that the selection of course types and the extent of specialization/diversification within a master's program is important for labor market outcomes. Because educational choices predict labor market outcomes, understanding course selection mechanisms is essential. To better understand the determinants of course choices, we estimate selection into the five main course types: management, marketing, finance, accounting, and organization.

The literature has shown that field-of-study choice is fundamental for labor market outcomes (e.g., James et al., 1989; Arcidiacono, 2004; Hamermesh and Donald, 2008; Altonji et al., 2012; Hastings et al., 2013; Webber, 2014; Altonji et al., 2015). Because of this relationship, selection into majors has received ample attention. Studies have found that gender, abilities, expected future earnings, peer effects and individual preferences are important determinants of field-of-study selection (e.g. Montmarquette et al., 2002; Arcidiacono, 2004; Ost, 2010; De Giorgi et al., 2010; Arcidiacono et al., 2012; Zafar, 2013). However, the selection into specific types of electives has not been carefully investigated. Despite the importance of determining the mechanisms underlying course selection, this paper does not offer an in-depth analysis of this topic. As in the previous section, our results are meant to uncover interesting patterns and to guide further research.

7.1 Empirical Framework

To examine selection into course types, we estimate a regression equation (Equation 4).

$$P(y_i^c = 1|X_i) = \Phi\Big(\alpha_0 + \alpha' X_i + \eta_t\Big) \tag{4}$$

where y_i^c is a dummy that indicates whether an individual i has a particular course type c in his or her educational portfolio. X_i is a vector of individual characteristics, such as age at enrollment, gender, entry GPA, and a dummy that indicates whether the father once held a managerial position. η_t refers to enrollment-year fixed effects. We formulate entry GPA as the average of the bachelor's GPA and the high school GPA. As such, we do not have to exclude too many observations due to missing values either in the bachelor's or the high school GPA. If the bachelor's GPA or the high school GPA is missing, we replace entry GPA with the GPA that is not missing. In two extended regressions, we also include (1) the supply of each specific course type during the enrollment period and (2) the share of individuals with managerial positions in the municipality of residence in the four years prior to entering CBS. Table 6 presents summary statistics on the explanatory variables.

The course supply cannot be obtained directly from the data, but it can be created based on available information. We create a measure of the number of different courses to which an individual is exposed during enrollment, which we consider a proxy for course supply. For each pair of enrollment and graduation years, we create this proxy by counting all the different courses in each department, except for the courses listed for individual i. We exclude individual i when counting course names to ensure that we do not create a measure that is correlated with the outcome by design (Angrist, 2014). To obtain a measure that is comparable across all periods, we consider the course supply to be the number of one course type relative to the total number of courses—i.e., the share of a certain course type. Table 7 presents the mean statistics across enrollment years.

In a standard setting of supply and demand, we would assume that supply and demand are determined simultaneously. However, in this setting, the course catalog is created before students are able to choose courses. The demand in one year is likely to impact the supply in the next, but the determination of supply and demand in the former does not happen at the same time.

Table 6: Summary Statistics

	All	$Man_D = 1$	$Mar_D = 1$	$Acc_D = 1$	$Fin_D = 1$	$Org_D = 1$
	(1)	(2)	(3)	(4)	(5)	(6)
Father with manager position	0.10	0.11	0.11	0.089	0.11	0.11
	(0.30)	(0.31)	(0.31)	(0.29)	(0.31)	(0.31)
Entry GPA	7.70	7.67	7.64	7.94	7.93	7.61
	(0.70)	(0.70)	(0.68)	(0.70)	(0.77)	(0.66)
Gender, female=1	0.33	0.31	0.36	0.21	0.17	0.42
	(0.47)	(0.46)	(0.48)	(0.41)	(0.38)	(0.49)
Starting age	23.8	23.8	23.9	23.8	23.7	24.0
	(1.73)	(1.72)	(1.69)	(1.92)	(1.66)	(1.76)
Municipality share of manager level individuals	0.012	0.012	0.012	0.011	0.011	0.012
	(0.0091)	(0.0096)	(0.0094)	(0.0081)	(0.0090)	(0.0091)
No. of obs	1835	1289	1124	315	413	700

Note: Means are reported and standard errors in parentheses.

Table 7: Supply of Courses Across Enrollment Year

	1984	1985	1986	1987	1988	1989	1990	1991
Supply of management courses	0.20	0.21	0.21	0.16	0.14	0.12	0.13	0.11
	(0.04)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)	(0.02)	(0.02)
Supply of accounting courses	0.13	0.11	0.09	0.06	0.07	0.09	0.07	0.06
	(0.04)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Supply of marketing courses	0.16	0.18	0.19	0.24	0.22	0.24	0.20	0.18
	(0.04)	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)
Supply of finance courses	0.07	0.08	0.06	0.09	0.07	0.06	0.09	0.18
	(0.02)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)
Supply of organization courses	0.15	0.13	0.15	0.13	0.19	0.17	0.16	0.17
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.01)	(0.02)	(0.01)
No. of obs	125	184	164	221	252	274	266	349

Note: The supply of a course type are calculated as the share relatively to all courses offered. Means are reported and computed as the mean across all individuals within each enrollment year. Standard errors in parentheses.

7.2 Results

Table 8 presents the results from the estimations of Equation 4. Across all the specifications, we observe a significant gender effect. Our results show that women are less likely to choose management (only 10% significance), finance and accounting, whereas they are more likely to choose marketing and organization. These results are important, as they show that women are less likely to choose the course types that we find to be associated with higher wages and the increased probability of attaining a C-level position.

Furthermore, pre-determined ability—measured as entry GPA—is significant in most of the course selection equations. Accounting and finance attract students with high entry GPAs, whereas the opposite is true for marketing and organization. Because entry GPA plays a role in course selection, it might impact the interpretation of the estimated relationship between course types and labor market outcomes (see Table 3). If highly capable individuals are more likely to choose finance and accounting, the observed wage premium for finance and accounting might be caused by these individuals' ability levels and not so much by a course-specific effect. However, as we control for master's GPA when estimating the association with labor market outcomes, the problem is likely to be negligible.

Panel A also shows that the labor market history of the father does not play a significant role in the course selection equation. By contrast, the share of individuals with manager positions in a municipality shows a positive and significant association with the selection of management and marketing and a negative association with the selection of accounting. The municipality share reflects the environment in which the students lived. This measure is clearly correlated with family background and perhaps merely reflects the characteristics of the parents, as they are likely responsible for students' place of residence prior to enrollment. Thus, this measure might only be a proxy for background characteristics. However, it could also capture spillover or peer effects from the surrounding environment, whereby individuals are more likely to choose courses that are related to leadership because they observe those around them who occupy leadership positions.

Table 8: Course Choices

	Probit estimation									
Dependent Variable:	Management	Accounting	Marketing	Finance	Organization	Management	Accounting	Marketing	Finance	Organization
PANEL A:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Standardized Entry GPA	-0.017	0.061***	-0.056***	0.080***	-0.054***	-0.017	0.061***	-0.056***	0.079***	-0.054***
	(0.011)	(0.009)	(0.012)	(0.010)	(0.012)	(0.011)	(0.009)	(0.012)	(0.010)	(0.012)
Starting age	-0.000	0.003	0.002	-0.002	0.005	0.002	0.001	0.003	-0.003	0.006
	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)
Gender, female=1	-0.045*	-0.109***	0.108***	-0.162***	0.126***	-0.047**	-0.108***	0.107***	-0.162***	0.126***
	(0.023)	(0.018)	(0.025)	(0.019)	(0.025)	(0.023)	(0.018)	(0.025)	(0.019)	(0.025)
Father with manager position=1	0.032	-0.040	0.055	-0.008	0.038	0.020	-0.035	0.048	-0.003	0.034
	(0.034)	(0.027)	(0.037)	(0.032)	(0.038)	(0.034)	(0.027)	(0.038)	(0.032)	(0.038)
Municipality share of manager level individuals						4.456***	-2.193**	2.630**	-1.663	1.212
						(1.191)	(1.015)	(1.291)	(1.102)	(1.241)
PANEL B:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Standardized Entry GPA	-0.021*	0.062***	-0.056***	0.079***	-0.054***	-0.021*	0.064***	-0.057***	0.081***	-0.056***
	(0.012)	(0.009)	(0.012)	(0.010)	(0.012)	(0.012)	(0.009)	(0.012)	(0.010)	(0.012)
Starting age	-0.000	0.002	0.002	-0.002	0.005	-0.000	0.002	0.002	-0.002	0.006
	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)
Gender, female=1	-0.045*	-0.109***	0.108***	-0.162***	0.126***	-0.044*	-0.110***	0.108***	-0.162***	0.126***
	(0.023)	(0.018)	(0.025)	(0.019)	(0.025)	(0.023)	(0.018)	(0.025)	(0.019)	(0.025)
Father with manager position=1	0.030	-0.040	0.055	-0.007	0.038	0.029	-0.039	0.054	-0.005	0.038
	(0.033)	(0.027)	(0.037)	(0.032)	(0.038)	(0.033)	(0.027)	(0.037)	(0.032)	(0.038)
Supply of management courses	1.496***					1.769***	-0.965*	0.420	-0.951*	0.744
	(0.454)					(0.558)	(0.503)	(0.640)	(0.530)	(0.614)
Supply of accounting courses		0.824				0.707	0.125	-0.095	-0.876	1.226
		(0.634)				(0.882)	(0.819)	(0.996)	(0.804)	(0.950)
Supply of marketing courses			0.106			0.614	0.271	0.211	0.350	0.213
			(0.568)			(0.632)	(0.508)	(0.681)	(0.540)	(0.677)
Supply of finance courses				-0.410		0.503	0.527	0.315	-0.489	-0.164
				(0.584)		(0.653)	(0.538)	(0.710)	(0.606)	(0.693)
Supply of organization courses					0.108	-0.284	0.856**	0.346	0.792*	0.205
					(0.530)	(0.540)	(0.433)	(0.610)	(0.466)	(0.598)
No. of obs	1679	1679	1679	1679	1679	1679	1679	1679	1679	1679

Note: The dependent variable is a dummy that is equal to 1 if the individual took at least one course at the Department of Management, Accounting, Marketing, Finance, or Organization, respectively. Average Marginal Effects (AME) are reported. When computing AMEs for dummy variables, we report the effect from the discrete change from 0 to 1. Enrollment-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Finally, in Panel B, we observe a positive effect of the management course supply on the probability of choosing management courses. Additionally, the management course supply enters the selection equation for accounting and finance negatively, which might reflect an increase in the supply and, in turn, the probability of selection into management courses, thus decreasing the probability of selection into accounting or finance courses. Thus, an increase in the management course supply will lead students to substitute accounting and finance courses for management courses. The results from Panel B indicate some sort of substitution effect among course types, but they also suggest that educational institutions can have an impact on their students' course selection.

8 Robustness

To test the sensitivity of our results, we perform a battery of robustness checks. The results of these estimations are presented in Appendix B.

One may worry that the results are driven by a correlation between those who have been on the labor market longest and their propensity to take management courses. Obviously, students who graduated in 1986 are more likely to be leaders in 2010 than students who graduated in 1996 due to the former's extra years on the labor market. If students who graduated in 1986 took more management courses (or other course types) than students who graduated in 1996, the results might be driven by this difference. To test if our results are robust to potential scenario, we create another dependent variable, namely a dummy that equals 1 if the individual had a C-level position at least once in the 19 years since his or her CBS enrollment (D^C_{19}). As such, we only count C-level positions until 2005 for individuals who enrolled in 1984, and we count C-level positions until 2010 for individuals who enrolled in 1991.[16] In so doing, we ensure that individuals have been on the labor market for a comparable time period. Table B.4 reports the estimation of Equation (1) with D^C_{19} as our dependent variable. The results are qualitatively the same as the main results, but the coefficients on Mar_D and Acc_D are slightly weaker in terms of significance. Overall, this finding indicates that a correlation between the enrollment year and management education are not driving the results.

Because the data attained from the Danish Business Authority contain inconsistent and missing observations, we exclude some of the information in this data. To test the robustness of our results to the data source, we perform our estimations by exclusively relying on the data from Statistics Denmark. In so doing, we define our dependent variable as a dummy variable that is equal to one whenever an

[16] We use enrollment year instead of graduation year because we are unable to account for work experience during the master's program. In Denmark, students often work and pursue degrees simultaneously, which often means that they prolong the duration of their studies.

individual held a managerial position according to Statistics Denmark. As such, we likely also consider lower-level managerial positions compared with our original measure of C-level individuals. The results are presented in Table B.5, which shows that the results on both management and marketing courses are robust to this specification of the dependent variable. Moreover, we find that our second measure of educational diversification remains significant at the 10% level.

When estimating the model with the inclusion of the "complementarity dummies", we have not included dummies to control for a single-course effect (see Table 4). The exclusion of the single-course dummies might cause the estimates to suffer from an omitted variable bias. Thus, as a robustness test, we re-estimate the model and include these single-course dummies. Table B.6 shows the results from estimations in which 5 single-course dummies are included, and Table B.7 shows the results from estimations in which we include all the complementarity dummies together (meaning both where $Man_D = 0$ and $Man_D = 1$). The results from these estimations are qualitatively the same as the ones presented in Table 4. In fact, the single-course dummies are insignificant in the model presented in Table B.6 (they are not reported).

Having established that narrow educational diversification is positively associated with leadership, we expect to observe "diminishing marginal return to course choices".[17] This expectation is natural given the observed effect of educational diversity—as every student has a limited number of courses in their educational portfolios, an increase in the number of courses in one department will necessarily lead to a decrease in the number of courses in another department and, in turn, a potential decrease in diversification. Thus, to test the robustness of our diversification result, the share ($share$) of a course type and the share squared ($share^2$) are included, and the model is estimated using a linear probability model.[18] The hypothesis of diminishing marginal returns to course type choices leads us to expect that $\beta_{share} > 0$ and $\beta_{share^2} < 0$. Table B.8 shows the results. As expected, for both management and marketing, β_{share} is significant and positive, and β_{share^2} is negative. For management, marketing, and organization, β_{share} and β_{share^2} are jointly significant in all the specifications. The remaining course shares, except for accounting, show the expected and economically meaningful signs, but they are not consistently significant across all the specifications.

Finally, to examine the sensitivity of our results to the model chosen, we estimate a Tobit model with the fraction of years holding a C-level position on the left-hand side. Table B.10 presents the

[17] As we cannot establish causality, referring to the estimated coefficient on the course dummies as the return to a specific course type might seem misleading. However, even if the effect is caused by either self-selection or skill accumulation, the positive correlation can somehow be thought of as a return, and we will refer to it as such.

[18] In Table B.3, we only include the shares. The results are qualitatively equivalent to those in Table B.8. Table B.9 shows the estimations from a probit model.

results and confirms the findings in Table 3—management, marketing and accounting remain significant predictors of leadership.

To ensure that our results from the wage estimations are not driven by outliers, we exclude the wage observations above and below the 99th and 1st percentiles, respectively, and re-estimate the wage equation. Table B.11 present the results from these estimations. The results are qualitatively the same as those in Table 3.

9 Conclusion

This paper estimates the relationship between educational profiles and labor market outcomes. Based on the theoretical and empirical results in the literature, we expect that students who take management courses and students with diversified educational portfolios are more likely to attain C-level positions. Moreover, we expect that math-related courses are positively associated with wage outcomes.

Our access to detailed educational data on students enrolled in the same master's program at CBS and data from Statistics Denmark and the Danish Business Authority allows us to conduct our analyses. Because of the structure of the MSc in Economics and Business Administration at CBS, we can compare individuals who graduated from the same master's program but who show considerable variation in their selection of electives and the extent of their educational diversification.

Our empirical results confirm our expectations and are consistent with other results in the literature and several other underlying explanations (e.g., Bloom and Van Reenen, 2007, 2010; Lazear, 2012; Bloom et al., 2013). We show that having a diversified educational portfolio within classical business school disciplines is associated with an increased probability of attaining a C-level position. Furthermore, we show that taking management courses is a strong predictor of leadership. Our results also show that certain course combinations are stronger predictors of leadership than others. In particular, we show that combinations of courses that are similar in terms of required and obtained skills are stronger predictors of leadership than combinations of dissimilar courses.

To explain our results we suggest some out of many potential reasons. Perhaps firms benefit from C-level individuals with diversified knowledge among classical business school topics and managerial abilities and are thus more likely to hire such people. However, the estimated effect could also be driven by self-selection. Most likely, our results are caused by both things.

Estimating a wage equation, we show that finance, accounting, marketing and management courses are positively correlated with wage outcomes. The wage premium associated with marketing is sensitive to the inclusion of a C-level dummy in the model, which indicates that the marketing wage premium might be caused to some extent by the increased probability of attaining a leadership position.

Summarizing the results, we show that not only the master's degree but also the ways in which a student puts together his or her curriculum in that master's program are important for labor market outcomes. The results summarized above motivated us to also investigate the mechanisms underlying elective course selection and we, therefore, estimated 5 course selection equations. Our results show that high school GPA, gender, course supply, and the characteristics of the previous area of residence are significant in the course selection equations.

Despite the lack of a causal interpretation, our results can still contribute to policy considerations, as they open a discussion regarding the importance of education type for leadership. If firms hire individuals with a management education because they contribute positively to productivity, policy-makers might want to encourage pre-university students to consider this aspect when choosing their education. However, we do not identify the channels or reasons for the estimated positive association between management education and leadership, and our results could just as well result from self-selection. Thus, more research that is capable of uncovering the causal mechanism is needed before explicit policy recommendations can be offered.

Therefore, extensions and further research are worth considering. First, establishing a causal relationship between educational profiles and leadership potential is an obvious next step. However, the identification of a specific course effect requires, for one, a valid instrument. Moreover, investigating whether C-level individuals with a management education in fact have a positive influence on firm productivity would also be interesting. Such analyses could be conducted by adding firm performance to our data and modeling firm performance as dependent on the educational characteristics of C-level individuals. However, to identify a potential causal impact of improved management on firm performance, one would need exogenous variation in the change/turnover of C-level individuals, which probably is the largest obstacle for such a study.

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Appendix A Descriptive Statistic

Table A.1: Departments at CBS

	Name of institute	Share of sample that took at least one course
	Marketing	0.61
	Organization and Labor Market Sociology	0.38
C	Business Economics and Leadership (Management)	0.70
	Finance	0.23
	Accounting	0.17
	Information technology and Financial Management	0.07
	Applied Computer Science	0.03
	Educational research	0.01
	Applied statistic	0.03
	European Trade Law	0.04
O	Macro Economics	0.05
	Social Sciences	0.07
	Traffic, Tourist and Regional Economics	0.13
	International Economy and Management	0.29

Note: **C** refers to the set of *Classical* courses for business education and **O** refers to *other* courses.

Table A.2: Diversification or Complementarity - Summary Statistics

	Mean (std.er)		Mean (std.er)		Mean (std.er)
Org=1,Fin=1,Mar=1	0.003	Org=1,Acc=1,Mar=1	0.004	Acc=1,Fin=1,Mar=1	0.003
	(0.057)		(0.062)		(0.052)
Org=0,Fin=1,Mar=1	0.037	Org=1,Acc=1,Mar=0	0.019	Acc=1,Fin=0,Mar=1	0.021
	(0.189)		(0.137)		(0.142)
Org=1,Fin=1,Mar=0	0.018	Org=0,Acc=1,Mar=1	0.020	Acc=1,Fin=0,Mar=0	0.032
	(0.133)		(0.139)		(0.175)
Org=0,Fin=0,Mar=0	0.052	Org=0,Acc=1,Mar=0	0.059	Acc=0,Fin=1,Mar=1	0.038
	(0.222)		(0.235)		(0.190)
Org=0,Fin=1,Mar=0	0.086	Org=0,Acc=0,Mar=0	0.079	Acc=1,Fin=1,Mar=0	0.046
	(0.281)		(0.270)		(0.210)
Org=1,Fin=0,Mar=0	0.101	Org=1,Acc=0,Mar=0	0.100	Acc=0,Fin=1,Mar=0	0.058
	(0.301)		(0.300)		(0.233)
Org=1,Fin=0,Mar=1	0.114	Org=1,Acc=0,Mar=1	0.113	Acc=0,Fin=0,Mar=0	0.121
	(0.318)		(0.317)		(0.326)
Org=0,Fin=0,Mar=1	0.292	Org=0,Acc=0,Mar=1	0.309	Acc=0,Fin=0,Mar=1	0.385
	(0.455)		(0.462)		(0.487)
Reference group	0.298	Reference group	0.298	Reference group	0.298
	(0.457)		(0.457)		(0.457)
Obs.	1835		1835		1835

The numbers are based on individuals with $Man_D = 1$ in their educational portfolio.

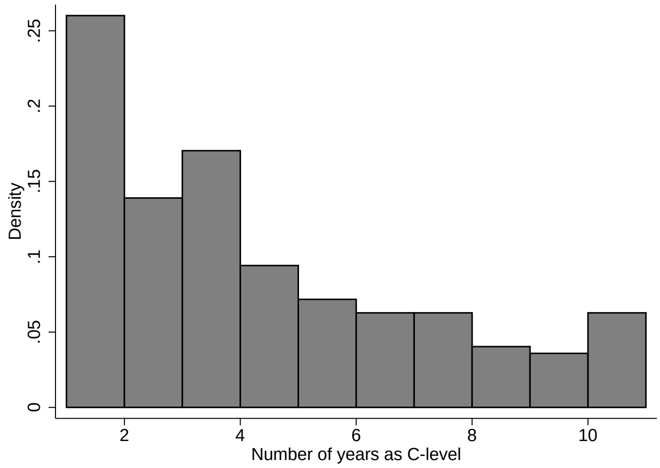
Table A.3: Summary Statistics Across Courses

	Across Man_D			
	All	$Man_D = 0$	$Man_D = 1$	Difference
	(1)	(2)	(3)	(4)
Mar_D	0.61	0.56	0.63	-0.074***
	(0.49)	(0.50)	(0.48)	
Acc_D	0.17	0.24	0.14	0.092***
	(0.38)	(0.43)	(0.35)	
Fin_D	0.23	0.27	0.21	0.065***
	(0.42)	(0.44)	(0.40)	
Org_D	0.38	0.49	0.34	0.15***
	(0.49)	(0.50)	(0.47)	
Diversification 1	2.91	2.68	3.01	-0.33***
	(0.88)	(0.94)	(0.83)	
Diversification 2	2.09	1.56	2.32	-0.76***
	(0.70)	(0.63)	(0.60)	
No. of obs	1835	546	1289	1835
	Across Mar_D			
	All	$Mar_D = 0$	$Mar_D = 1$	Difference
	(1)	(2)	(3)	(4)
Man_D	0.70	0.66	0.73	-0.065***
	(0.46)	(0.47)	(0.45)	
Acc_D	0.17	0.35	0.059	0.29***
	(0.38)	(0.48)	(0.24)	
Fin_D	0.23	0.43	0.097	0.33***
	(0.42)	(0.50)	(0.30)	
Org_D	0.38	0.47	0.33	0.14***
	(0.49)	(0.50)	(0.47)	
Diversification 1	2.91	3.01	2.84	0.17***
	(0.88)	(0.92)	(0.84)	
Diversification 2	2.09	1.91	2.21	-0.31***
	(0.70)	(0.77)	(0.63)	
No. of obs	1835	711	1124	1835
	Across Acc_D			
	All	$Acc_D = 0$	$Acc_D = 1$	Difference
	(1)	(2)	(3)	(4)
Man_D	0.70	0.73	0.59	0.14***
	(0.46)	(0.45)	(0.49)	
Mar_D	0.61	0.70	0.21	0.49***
	(0.49)	(0.46)	(0.41)	
Fin_D	0.23	0.16	0.56	-0.40***
	(0.42)	(0.36)	(0.50)	
Org_D	0.38	0.42	0.22	0.20***
	(0.49)	(0.49)	(0.41)	
Diversification 1	2.91	2.80	3.45	-0.65***
	(0.88)	(0.83)	(0.89)	
Diversification 2	2.09	1.99	2.57	-0.58***
	(0.70)	(0.66)	(0.69)	
No. of obs	1835	1520	315	1835

Table A.4: Summary Statistics Across Courses

	Across Fin_D			
	All	$Fin_D = 0$	$Fin_D = 1$	Difference
	(1)	(2)	(3)	(4)
Man_D	0.70	0.72	0.64	0.078***
	(0.46)	(0.45)	(0.48)	
Mar_D	0.61	0.71	0.26	0.45***
	(0.49)	(0.45)	(0.44)	
Acc_D	0.17	0.098	0.42	-0.33***
	(0.38)	(0.30)	(0.49)	
Org_D	0.38	0.45	0.15	0.30***
	(0.49)	(0.50)	(0.36)	
Diversification 1	2.91	2.78	3.36	-0.58***
	(0.88)	(0.83)	(0.89)	
Diversification 2	2.09	1.98	2.48	-0.50***
	(0.70)	(0.66)	(0.70)	
No. of obs	1835	1422	413	1835
	Across Org_D			
	All	$Org_D = 0$	$Org_D = 1$	Difference
	(1)	(2)	(3)	(4)
Man_D	0.70	0.75	0.62	0.14***
	(0.46)	(0.43)	(0.49)	
Mar_D	0.61	0.67	0.53	0.14***
	(0.49)	(0.47)	(0.50)	
Acc_D	0.17	0.22	0.099	0.12***
	(0.38)	(0.41)	(0.30)	
Fin_D	0.23	0.31	0.087	0.22***
	(0.42)	(0.46)	(0.28)	
Diversification 1	2.91	2.80	3.08	-0.28***
	(0.88)	(0.85)	(0.89)	
Diversification 2	2.09	1.95	2.33	-0.39***
	(0.70)	(0.64)	(0.73)	
No. of obs	1835	1135	700	1835

Figure A.1: Distribution of Years as C-level - Sample of C-level Individuals



Appendix B Robustness

Table B.1: The Impact of Course Choice - C-level Regression

	Probit estimation									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Man_D =1	0.048***					0.052***	0.047***	0.050***	0.047***	0.055***
	(0.015)					(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Acc_D =1		0.040*				0.046**				0.055**
		(0.022)				(0.022)				(0.024)
Mar_D =1			0.012				0.009			0.037**
			(0.015)				(0.015)			(0.017)
Fin_D =1				0.022				0.027		0.032
				(0.020)				(0.020)		(0.023)
Org_D =1					-0.007				-0.002	0.017
					(0.015)				(0.015)	(0.017)
No. of obs	1835	1835	1835	1835	1835	1835	1835	1835	1835	1835

Note: The dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the 2000–2010 period. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, we report the effect from the discrete change from 0 to 1. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B.2: The Impact of Course Choice - Wage Regression

	OLS estimation									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Man_D =1	0.054**					0.067***	0.057***	0.072***	0.046**	0.074***
	(0.022)					(0.022)	(0.022)	(0.021)	(0.022)	(0.021)
Acc_D =1		0.145***				0.153***				0.110***
		(0.030)				(0.030)				(0.032)
Mar_D =1			-0.028				-0.031			0.053**
			(0.020)				(0.020)			(0.022)
Fin_D =1				0.178***				0.186***		0.171***
				(0.026)				(0.026)		(0.029)
Org_D =1					-0.080***				-0.075***	-0.016
					(0.020)				(0.020)	(0.021)
No. of obs	1795	1795	1795	1795	1795	1795	1795	1795	1795	1795

Note: The dependent variable is the logarithm of the average hourly wage during the 2000–2010 period. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3: The Impact of Course Choice - C-level Regression Shares as Regressors

	C-level regression (Probit)				Wage regression (OLS)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Man_S	0.105***	0.144***	0.151***	0.147***	0.077	0.131**	0.210***	0.163***
	(0.039)	(0.043)	(0.045)	(0.047)	(0.056)	(0.057)	(0.056)	(0.060)
Acc_S		0.170**	0.164**	0.159**		0.456***	0.382***	0.328***
		(0.070)	(0.072)	(0.075)		(0.098)	(0.100)	(0.106)
Mar_S		0.038	0.046*	0.041		0.020	0.111***	0.063
		(0.027)	(0.027)	(0.034)		(0.035)	(0.036)	(0.044)
Fin_S			0.043	0.037			0.474***	0.413***
			(0.056)	(0.061)			(0.080)	(0.085)
Org_S				-0.011				-0.117**
				(0.038)				(0.052)
No. of obs	1835	1835	1835	1835	1795	1795	1795	1795

Note: Through columns (1)-(4) the dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the 2000–2010 period. Through columns (5)-(8) the dependent variable is the logarithm of the average hourly wage during the 2000–2010 period. Columns (1)-(4) report average marginal effects (AME). When computing AMEs for dummy variables, we report the effect from the discrete change from 0 to 1. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.4: The impact of Course Choice - C-level Regression
Re-definition of the Depend Variable

	Probit estimation					
	(1)	(2)	(3)	(4)	(5)	(6)
Man_D =1	0.032**	0.034**	0.035**	0.038***		
	(0.014)	(0.014)	(0.014)	(0.014)		
Acc_D =1		0.037*	0.033	0.040*		
		(0.021)	(0.021)	(0.022)		
Mar_D =1		0.017	0.020	0.028*		
		(0.014)	(0.015)	(0.015)		
Fin_D =1			0.013	0.023		
			(0.018)	(0.019)		
Org_D =1				0.028*		
				(0.016)		
Diversification 1					0.005	
					(0.007)	
Diversification 2						0.032***
						(0.009)
Standardized master GPA	0.005	0.006	0.007	0.006	0.004	0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Gender, female=1	-0.125***	-0.123***	-0.121***	-0.123***	-0.126***	-0.123***
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Starting age	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Father with manager position=1	0.082***	0.082***	0.081***	0.081***	0.083***	0.080***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Children in 2000	0.003	0.003	0.003	0.004	0.003	0.003
	(0.016)	(0.016)	(0.016)	(0.017)	(0.016)	(0.017)
Married in 2000	0.042***	0.042***	0.042***	0.041**	0.043***	0.041**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Age in 2000	0.065	0.065	0.066	0.069	0.075	0.073
	(0.059)	(0.059)	(0.059)	(0.059)	(0.060)	(0.059)
Age in 2000 squared	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
No. of obs	1835	1835	1835	1835	1835	1835

Note: The dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the first 19 years since the enrollment year (D^C_{19}). Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, we report the effect from the discrete change from 0 to 1. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.5: The impact of Course Choice - C-level Regression
Re-definition of the Depend Variable

	Probit estimation					
	(1)	(2)	(3)	(4)	(5)	(6)
Man_D =1	0.061**	0.060**	0.060**	0.058**		
	(0.025)	(0.025)	(0.025)	(0.025)		
Acc_D =1		0.030	0.028	0.025		
		(0.033)	(0.033)	(0.034)		
Mar_D =1		0.060**	0.063**	0.059**		
		(0.025)	(0.026)	(0.027)		
Fin_D =1			0.010	0.006		
			(0.031)	(0.032)		
Org_D =1				-0.012		
				(0.026)		
Diversification 1					-0.016	
					(0.013)	
Diversification 2						0.030*
						(0.016)
Standardized master GPA	0.049***	0.051***	0.052***	0.052***	0.047***	0.050***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Gender, female=1	-0.089***	-0.091***	-0.090***	-0.089***	-0.095***	-0.089***
	(0.024)	(0.024)	(0.025)	(0.025)	(0.024)	(0.024)
Starting age	0.002	0.002	0.002	0.002	0.002	0.002
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Father with manager position=1	0.048	0.046	0.045	0.046	0.051	0.047
	(0.037)	(0.037)	(0.038)	(0.038)	(0.037)	(0.038)
Children in 2000	0.047*	0.045*	0.045	0.045	0.046*	0.047*
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Married in 2000	0.059**	0.062**	0.062**	0.062**	0.060**	0.060**
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Age in 2000	0.142	0.134	0.135	0.134	0.153	0.155*
	(0.095)	(0.093)	(0.093)	(0.093)	(0.094)	(0.094)
Age in 2000 squared	-0.002	-0.002	-0.002	-0.002	-0.002*	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
No. of obs	1835	1835	1835	1835	1835	1835

Note: The dependent variable is a dummy that is equal to 1 if the individual held a manager position according to Statistics Denmark at least once during the 2000–2010 period. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, we report the effect from the discrete change from 0 to 1. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.6: Complementarity or Diversification

	Linear Probability Model					
	Man_D=1	Man_D=0	Man_D=1	Man_D=0	Man_D=1	Man_D=0
	(1)	(2)	(3)	(4)	(5)	(6)
$Org_D = 1, Fin_D = 1, Mar_D = 1$	0.053	0.151				
	(0.150)	(0.190)				
$Org_D = 0, Fin_D = 1, Mar_D = 1$	-0.013	-0.073				
	(0.053)	(0.059)				
$Org_D = 1, Fin_D = 1, Mar_D = 0$	-0.009	-0.032				
	(0.068)	(0.087)				
$Org_D = 0, Fin_D = 0, Mar_D = 0$	0.042	-0.095**				
	(0.043)	(0.043)				
$Org_D = 0, Fin_D = 1, Mar_D = 0$	0.094**	-0.063				
	(0.042)	(0.040)				
$Org_D = 1, Fin_D = 0, Mar_D = 0$	0.016	-0.013				
	(0.031)	(0.035)				
$Org_D = 1, Fin_D = 0, Mar_D = 1$	0.055*	-0.055**				
	(0.030)	(0.027)				
$Org_D = 0, Fin_D = 0, Mar_D = 1$	0.072***	-0.055**				
	(0.024)	(0.024)				
$Org_D = 1, Acc_D = 1, Mar_D = 1$			-0.046	-0.166***		
			(0.124)	(0.046)		
$Org_D = 1, Acc_D = 1, Mar_D = 0$			-0.015	0.019		
			(0.064)	(0.090)		
$Org_D = 0, Acc_D = 1, Mar_D = 1$			0.059	-0.106*		
			(0.072)	(0.062)		
$Org_D = 0, Acc_D = 1, Mar_D = 0$			0.084*	-0.058		
			(0.049)	(0.047)		
$Org_D = 0, Acc_D = 0, Mar_D = 0$			0.080**	-0.116***		
			(0.036)	(0.025)		
$Org_D = 1, Acc_D = 0, Mar_D = 0$			0.021	-0.027		
			(0.030)	(0.033)		
$Org_D = 1, Acc_D = 0, Mar_D = 1$			0.056*	-0.041		
			(0.030)	(0.028)		
$Org_D = 0, Acc_D = 0, Mar_D = 1$			0.063***	-0.051**		
			(0.023)	(0.024)		
$Acc_D = 1, Fin_D = 1, Mar_D = 1$					-0.128**	-0.164***
					(0.050)	(0.045)
$Acc_D = 1, Fin_D = 0, Mar_D = 1$					0.073	-0.107*
					(0.074)	(0.062)
$Acc_D = 1, Fin_D = 0, Mar_D = 0$					0.025	0.004
					(0.058)	(0.079)
$Acc_D = 0, Fin_D = 1, Mar_D = 1$					0.019	-0.008
					(0.057)	(0.071)
$Acc_D = 1, Fin_D = 1, Mar_D = 0$					0.086	-0.066
					(0.055)	(0.050)
$Acc_D = 0, Fin_D = 1, Mar_D = 0$					0.082*	-0.098**
					(0.048)	(0.042)
$Acc_D = 0, Fin_D = 0, Mar_D = 0$					0.025	-0.039
					(0.028)	(0.030)
$Acc_D = 0, Fin_D = 0, Mar_D = 1$					0.056***	-0.053***
					(0.019)	(0.019)
Individual course effects included	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	1835	1835	1835	1835	1835	1835

Note: The dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the 2000–2010 period. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.7: Complementarity or Diversification

	Linear Probability Model				
	(1)		(2)		(3)
$Man_D = 1, Org_D = 1, Fin_D = 1, Mar_D = 1$	0.120	$Man_D = 1, Org_D = 1, Acc_D = 1, Mar_D = 1$	0.036	$Man_D = 1, Acc_D = 1, Fin_D = 1, Mar_D = 1$	-0.065*
	(0.144)		(0.121)		(0.037)
$Man_D = 1, Org_D = 0, Fin_D = 1, Mar_D = 1$	0.021	$Man_D = 1, Org_D = 1, Acc_D = 1, Mar_D = 0$	0.040	$Man_D = 1, Acc_D = 1, Fin_D = 0, Mar_D = 1$	0.123*
	(0.046)		(0.056)		(0.066)
$Man_D = 1, Org_D = 1, Fin_D = 1, Mar_D = 0$	0.039	$Man_D = 1, Org_D = 0, Acc_D = 1, Mar_D = 1$	0.107	$Man_D = 1, Acc_D = 1, Fin_D = 0, Mar_D = 0$	0.050
	(0.062)		(0.068)		(0.044)
$Man_D = 1, Org_D = 0, Fin_D = 0, Mar_D = 0$	0.008	$Man_D = 1, Org_D = 0, Acc_D = 1, Mar_D = 0$	0.106**	$Man_D = 1, Acc_D = 0, Fin_D = 1, Mar_D = 1$	0.036
	(0.038)		(0.043)		(0.045)
$Man_D = 1, Org_D = 0, Fin_D = 0, Mar_D = 0$	0.109***	$Man_D = 1, Org_D = 0, Acc_D = 0, Mar_D = 1$	0.036	$Man_D = 1, Acc_D = 1, Fin_D = 1, Mar_D = 0$	0.126***
	(0.038)		(0.036)		(0.047)
$Man_D = 1, Org_D = 1, Fin_D = 1, Mar_D = 0$	0.015	$Man_D = 1, Org_D = 1, Acc_D = 0, Mar_D = 0$	0.010	$Man_D = 1, Acc_D = 0, Fin_D = 1, Mar_D = 0$	0.075*
	(0.028)		(0.029)		(0.040)
$Man_D = 1, Org_D = 1, Fin_D = 0, Mar_D = 1$	0.073**	$Man_D = 1, Org_D = 1, Acc_D = 0, Mar_D = 1$	0.071**	$Man_D = 1, Acc_D = 0, Fin_D = 0, Mar_D = 0$	0.004
	(0.032)		(0.032)		(0.023)
$Man_D = 1, Org_D = 0, Fin_D = 0, Mar_D = 1$	0.057**	$Man_D = 1, Org_D = 0, Acc_D = 0, Mar_D = 1$	0.045*	$Man_D = 1, Acc_D = 0, Fin_D = 0, Mar_D = 1$	0.058***
	(0.025)		(0.025)		(0.019)
$Man_D = 0, Org_D = 1, Fin_D = 1, Mar_D = 1$	0.247	$Man_D = 0, Org_D = 1, Acc_D = 1, Mar_D = 1$	-0.055*	$Man_D = 0, Acc_D = 1, Fin_D = 1, Mar_D = 1$	-0.025
	(0.188)		(0.033)		(0.031)
$Man_D = 0, Org_D = 0, Fin_D = 1, Mar_D = 1$	0.023	$Man_D = 0, Org_D = 1, Acc_D = 1, Mar_D = 0$	0.102	$Man_D = 0, Acc_D = 1, Fin_D = 0, Mar_D = 1$	0.001
	(0.057)		(0.087)		(0.055)
$Man_D = 0, Org_D = 1, Fin_D = 1, Mar_D = 0$	0.039	$Man_D = 0, Org_D = 0, Acc_D = 1, Mar_D = 1$	0.006	$Man_D = 0, Acc_D = 1, Fin_D = 0, Mar_D = 0$	0.077
	(0.085)		(0.057)		(0.075)
$Man_D = 0, Org_D = 0, Fin_D = 0, Mar_D = 0$	-0.065	$Man_D = 0, Org_D = 0, Acc_D = 1, Mar_D = 0$	0.027	$Man_D = 0, Acc_D = 0, Fin_D = 1, Mar_D = 1$	0.078
	(0.042)		(0.043)		(0.067)
$Man_D = 0, Org_D = 0, Fin_D = 1, Mar_D = 0$	0.008	$Man_D = 0, Org_D = 0, Acc_D = 0, Mar_D = 0$	-0.092***	$Man_D = 0, Acc_D = 1, Fin_D = 1, Mar_D = 0$	0.040
	(0.038)		(0.025)		(0.042)
$Man_D = 0, Org_D = 1, Fin_D = 0, Mar_D = 0$	0.017	$Man_D = 0, Org_D = 1, Acc_D = 0, Mar_D = 0$	-0.004	$Man_D = 0, Acc_D = 0, Fin_D = 1, Mar_D = 0$	-0.048
	(0.036)		(0.035)		(0.035)
$Man_D = 0, Org_D = 1, Fin_D = 0, Mar_D = 1$	0.001	$Man_D = 0, Org_D = 1, Acc_D = 0, Mar_D = 1$	0.009	$Man_D = 0, Acc_D = 0, Fin_D = 0, Mar_D = 0$	-0.021
	(0.027)		(0.029)		(0.028)
Observations	1835		1835		1835

Note: The dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the 2000–2010 period. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The reference group in Column (1) is $Man_D = 0, Org_D = 0, Fin_D = 0, Mar_D = 1$

The reference group in Column (2) is $Man_D = 0, Org_D = 0, Acc_D = 0, Mar_D = 1$

The reference group in Column (3) is $Man_D = 0, Acc_D = 0, Fin_D = 0, Mar_D = 1$

Table B.8: The Impact of Course Choice - C-level Regression Diminishing Return to Specialization

	Linear Probability Model						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Man_S	0.184*					0.175*	0.170
	(0.104)					(0.105)	(0.106)
Man_S^2	-0.124					-0.085	-0.063
	(0.197)					(0.197)	(0.198)
Acc_S		0.047				0.131	0.091
		(0.181)				(0.185)	(0.189)
Acc_S^2		0.140				0.114	0.154
		(0.390)				(0.393)	(0.393)
Mar_S			0.172**			0.193**	0.212**
			(0.077)			(0.079)	(0.083)
Mar_S^2			-0.245***			-0.224**	-0.232**
			(0.091)			(0.093)	(0.093)
Fin_S				0.180			0.193
				(0.128)			(0.144)
Fin_S^2				-0.308			-0.239
				(0.204)			(0.213)
Org_S					0.028		0.085
					(0.085)		(0.091)
Org_S^2					-0.159		-0.141
					(0.110)		(0.111)
p_1	0.010					0.007	0.010
p_2		0.244				0.032	0.100
p_3			0.014			0.048	0.036
p_4				0.320			0.398
p_5					0.001		0.372
No. of obs	1835	1835	1835	1835	1835	1835	1835

Note: Linear Probability Model. The dependent variable is a dummy that is equal to 1 if the individual held a C-level position at least once during the 2000–2010 period.. p_i, $i = (1, 2, 3, 4, 5)$, is the p-value form testing the hypothesis $H : \beta_{share_i} = 0, \beta_{share_i^2} = 0$. For instance, p_1 corresponds to the test that $Man_S = 0$ and $Man_S^2 = 0$. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.9: The Impact of Course Choice - C-level Regression
Diminishing Return to Specialization

	Probit estimation - Diminishing return						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Man_S	0.117***					0.129***	0.130***
	(0.040)					(0.042)	(0.047)
Acc_S		0.085				0.169	0.135
		(0.108)				(0.113)	(0.116)
Mar_S			0.041			0.074**	0.082**
			(0.029)			(0.033)	(0.040)
Fin_S				0.107			0.122
				(0.085)			(0.099)
Org_S					0.047		0.103
					(0.073)		(0.079)
No. of obs	1835	1835	1835	1835	1835	1835	1835

Note: The dependent variable is a dummy equal to 1 if an individual had a C-level position at least once in the period from 2000–2010. Average Marginal Effects (AME) are reported. We included *share* and $share^2$ in the regressions and due to the functional form of the Probit, the AME is the overall effect from these two components. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.10: The Impact of Course Choice - C-level Tobit Regression
Dependent Variable is Share of Years as C-level

	Tobit regression					
	(1)	(2)	(3)	(4)	(5)	(6)
Man_D =1	0.157**	0.170***	0.174***	0.186***		
	(0.062)	(0.061)	(0.061)	(0.061)		
Acc_D =1		0.188***	0.171**	0.186***		
		(0.066)	(0.068)	(0.069)		
Mar_D =1		0.080	0.104*	0.123**		
		(0.055)	(0.059)	(0.061)		
Fin_D =1			0.077	0.101		
			(0.064)	(0.067)		
Org_D =1				0.071		
				(0.058)		
Diversification 1					0.037	
					(0.027)	
Diversification 2						0.136***
						(0.034)
No. of obs	1835	1835	1835	1835	1835	1835

Note: Tobit estimation. The dependent variable is the fraction of years an individual held a C-level position at least once during the 2000–2010 period. In all estimations, we have included the same controls as in Table 3. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.11: The Impact of Course Choice - Wage Regression Extreme Wage Observations are Excluded

	OLS estimation					
	(1)	(2)	(3)	(4)	(5)	(6)
Man_D =1	0.064***	0.076***	0.083***	0.082***		
	(0.019)	(0.019)	(0.018)	(0.018)		
Acc_D =1		0.146***	0.108***	0.107***		
		(0.026)	(0.027)	(0.028)		
Mar_D =1		0.015	0.052***	0.050**		
		(0.019)	(0.019)	(0.020)		
Fin_D =1			0.146***	0.143***		
			(0.025)	(0.026)		
Org_D =1				-0.008		
				(0.020)		
Diversification 1					0.019*	
					(0.010)	
Diversification 2						0.073***
						(0.012)
Standardized master GPA	0.046***	0.048***	0.056***	0.056***	0.044***	0.049***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Gender, female=1	-0.289***	-0.275***	-0.258***	-0.258***	-0.289***	-0.285***
	(0.018)	(0.018)	(0.017)	(0.018)	(0.018)	(0.017)
Starting age	-0.011	-0.010	-0.008	-0.008	-0.011	-0.010
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Father with manager position=1	0.069**	0.071**	0.068**	0.068**	0.070**	0.066**
	(0.030)	(0.029)	(0.029)	(0.029)	(0.030)	(0.030)
Children in 2000	0.038*	0.039*	0.035*	0.035*	0.039*	0.037*
	(0.021)	(0.021)	(0.020)	(0.020)	(0.021)	(0.021)
Married in 2000	0.091***	0.094***	0.095***	0.095***	0.093***	0.093***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Age in 2000	0.071	0.092	0.100	0.100	0.087	0.080
	(0.069)	(0.068)	(0.068)	(0.068)	(0.069)	(0.069)
Age in 2000 squared	-0.001	-0.002*	-0.002*	-0.002*	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
No. of obs	1787	1787	1787	1787	1787	1787

Note: The dependent variable is the logarithm of the average hourly wage during the 2000–2010 period. Wage observations below or above the top or bottom one percentiles have been excluded. Graduation-year fixed effects are included. Robust standard errors are computed and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

Dropping Out of University:

Estimating Peer Effects Using Randomly Assigned Groups

Dropping Out of University:

Estimating Peer Effects Using Randomly Assigned Groups *

Marie Skibsted†

June 7, 2016

Abstract

In this paper I analyze the impact of peers on achievements in tertiary education. Using unique educational data on students who were randomly assigned to peer groups, I investigate the impact of peers on students' decisions to drop out and on their first-year GPAs. Within-school and across-peer-group variations in peer quality allow me to estimate a peer effect. My main finding is that women in peer groups with high ability levels are more likely to drop out during the first year. This is particularly true for women in the lower half of the ability distribution. By contrast, men are unaffected by their peers. Including a measure of peer group rank in my model shows that for women, peer group rank is a stronger determinant of the probability of dropping out than is own high school GPA. Finally, my results show a positive peer effect on the educational performance only of women in the lower half of the ability distribution.

Keywords: peer effects, human capital, tertiary education, social interaction

JEL classifications: I20, I21, J24

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†Marie Skibsted, Department of Economics, Copenhagen Business School, Porcelaenshaven 16 A, 1st floor, DK-2000 Frederiksberg, Denmark, E-mail: mks.eco@eco.dk

1 Introduction

Most people have experienced peer effects in their lives. Following your high school friend into high-level math courses might lead you to become a university mathematician and being inspired and helped by your peers in school is likely to improve your educational performance. In this paper, using unique data on students at the largest bachelor's program at Copenhagen Business School (CBS), who were randomly assigned into peer groups, I estimate a relationship between peer group ability level and individual academic achievements, as measured by the probability of dropping out during the first year and by first-year GPA.

During the past 15 years, peer effects in education have received increased attention in the literature (e.g., Sacerdote, 2001; Carrell et al., 2009; Ammermueller and Pischke, 2009; Duflo et al., 2011; Lavy et al., 2012; Carrell et al., 2013). The determinants of educational achievement are worth understanding because education is a significant determinant of labor market success and is influential on other individual-level outcomes, such as crime, health status, and family formation (e.g., Angrist and Krueger, 1991; Arcidiacono, 2004; Altonji et al., 2012; Hjalmarsson et al., 2011). However educational institutions can also benefit from insights into peer effects when they want to reduce dropout rates or improve students' performances. Despite the vast literature on peer effects, the results remain ambiguous, and most studies are concerned with the effects in primary and secondary education. Moreover, few researchers have investigated the relationship between university dropout rates and the ability levels of peers (Johnes and McNabb, 2004; Booij et al., 2015). The main contribution of this paper is thus the identification of an effect of peer group ability level on individuals' probability of dropping out during the first year of university. I also estimate potential peer effects on university performance as measured by first-year GPA.

There are several reasons it is worth understanding the determinants of the decision to drop out. For instance, students who decide not to complete their studies will probably not use the human capital they gained in the courses they have already taken. They have also taken up spaces at their university that could have been filled by other students. Furthermore, either they delay their labor market entrance by postponing a potential graduation or they enter the labor market with a lower level of education, which both are likely to be important for their lifetime earnings. This is an inefficient use of time and resources by students, universities, and society. If universities are to minimize this waste of resources, they will need to understand the mechanisms behind their students' decisions to drop out. Of course, if a student has enrolled in a field he or she is not suited to, quitting may be the best decision. But dropping out because of other academic problems or social discomfort may not be the optimal solution for the student. Greater knowledge of students' reasons for dropping out thus opens up the possibility of reducing dropout rates and improving both the efficiency of universities and the conditions for their students.

The main challenge in estimating a peer effect is handling the econometric problems of self-selection and reflection (Manski, 1993). Analyses made without addressing these problems will result in biased estimates. Self-selection occurs when a student selects his or her own peers. Reflection occurs when the behavior of the peer group and the behavior of the individual happen simultaneously (two-way causality).[1] A unique data set describing students enrolled in the largest bachelor's program in business economics at Copenhagen Business School (CBS) between 1996 and 2004 allows me to overcome these two problems. In addition to detailed educational information about students' performances, the data set contains information on smaller groups that students were randomly assigned to when they enrolled. Students were assigned to exercise classes on the basis of these groups and I use these tutorial groups as my measure of peer groups and thereby avoid the problem of endogenous group formation. The data also allow me to circumvent the problem of reflection with their detailed information on pre-university performance. Using high school GPA as a proxy for quality, I create pre-determined and exogenous measures of peer quality.

Despite the unique features of the data, this paper faces potential threats to identification. Unofficial sorting in and out of peer groups (endogenous subgroup formation) might make my results weaker and neglected heterogeneity in the probit model (which I use to estimate the probability of dropping out) might cause the estimates to suffer from attenuation bias. I discuss these issues in detail and argue that they are not crucial threats to identification.

To get a grip on the channels through which peer effects work, I estimate alternative specifications of a standard linear-in-means model. For instance, students affect each other in several ways, and some peers might be more important to an individual's academic performance than others. To better understand how the potential peer effect works, I allow for heterogeneous peer effects. I also estimate a potential peer effect across multiple subgroups, as this makes it possible to determine which students are the most responsive to peer influence.

[1] The classic example of this is in educational performance. If the performance of an individual is affected by the performance of his or her peer group, the opposite holds true as well. It is less obvious that this problem occurs in the relationship between the decision to drop out and the peer group's performance. But peer group performance is likely to influence an individual's decision to drop out, and both the consideration of and the decision to drop out are likely to affect the individual's academic performance, which in turn will affect that of the peer group. Thus even though the relationship between the individual's decision to drop out and the peer group's performance is not a classic example of what Manski (1993) calls reflection, one still needs to be aware of two-way causality there.

Moreover, I follow recent literature on the affects of rank on educational performance (e.g., Murphy and Weinhardt, 2014; Elsner and Isphording, 2015). For instance, Elsner and Isphording (2015) show that ability rank in a high school cohort is associated with a student's probability of finishing high school, attending college and completing a college degree. I complement this literature by investigating whether ability rankings in peer groups also play a role at the university level—specifically, whether and how they affect the decision to drop out. If individuals have rank concerns regarding their closest peers, it could affect the students' development of self-confidence and academic self-concepts, which might affect their probability of dropping out. Particularly if students' closest peers do not reflect the larger population, the developed academic self-concept could be misleading. I compare the effects of peer group rank and absolute ability level (as measured by high school GPA), by estimating a model with and without high school GPA as a determinant. The results of these estimations can help us understand whether it is an individual's absolute ability level (high school GPA) or a comparison effect (peer group ranking) that is more important in a case of dropping out.

My main findings are that women's probability of dropping out is increased by peers' ability level and men's is unaffected. This means that women's chances of remaining in university are adversely affected by the ability levels of their peers; this is especially true for women in the lower half of the ability distribution. In addition, peer quality has a positive and significant effect on educational performance only for women in the lower half of the ability distribution. Interestingly, female students who are not pushed out by the ability levels of their peers are in fact the ones who benefit from higher peer quality.

When a measure of peer-group ability rank is included, my results show that women who rank highly in their peer groups are less likely to drop out. Furthermore, for women, peer-group ranking is a stronger predictor of dropout probability than is high school GPA. By contrast, men's probability of dropping out is unaffected by ability ranking but still significantly decreasing in high school GPA. Thus women's relative ability among close peers is statistically important to the dropout decision, but their absolute ability level is not. This means that whereas women are more affected by peer group rank than ability level, the opposite is true for men.

It is puzzling that women with high-quality peers should be more likely to drop out. In the psychology literature, similar behavior is explained by the big-fish-little-pond effect (BFLPE; e.g., Marsh and Parker, 1984; Marsh and Hau, 2003). This is the hypothesis that students compare their own academic abilities with those of their peers to form their academic self-concepts (Marsh and Hau, 2003). One implication of this is that students might be better off with low-ability peers, as this would not have a negative impact on their self-concepts. In the present case, it might be that students with high-quality peers tend to underestimate their own abilities, and this low self-evaluation might be the reason they leave their programs. The literature showing that women are less willing to compete than men of the same ability levels is related to this too (e.g., Gneezy et al., 2003; Niederle and Vesterlund, 2007, 2010). If groups with high average ability levels also have high levels of internal competition, it might explain why women with high-quality peer groups are more likely to drop out.

The fact that peer group rank is important for women's decisions to drop out but high school GPA is not could indicate that women compare themselves to their close peers, and this comparison affects their decision to drop out, while their true ability levels are not taken into account. If the peer group comparison leads to a lower academic self-concept, and this in turn leads to dropping out, my results indicate that non-cognitive traits such as academic self-confidence and academic self-esteem are also important for the decision to drop out. This result is in line with previous studies finding that non-cognitive traits are important for both educational and other outcomes (e.g., Heckman and Rubinstein, 2001; Valentine et al., 2004; Murphy and Weinhardt, 2014; Mendolia and Walker, 2014). If probability of dropping out is affected by low academic self-esteem or a wrong self-concept, it is a problem of imperfect information. Thus, from a policy point of view, educational institutions might be able to reduce dropout rates by running campaigns to inform students, particularly women, of their potential.

Unless peer effects are non-linear across types of students, there is nothing to be gained by sorting students into peer groups. This was also pointed out by Carrell et al. (2009). For instance, moving a student from one peer group into another would make one peer group gain the same amount of ability as the other lost. Only if peer impacts were non-linear could reallocation of students result in overall social gains. Because my results do reveal non-linear peer effects (on low-ability students and women) they open the door for policy interventions. However, as Carrell et al. (2013) also show, any such interventions should be considered carefully in advance.[2] As I mentioned, my results and my interpretations of them also open the way for other kinds of intervention. Because my results indicate connections among peer ability level, academic self-concept, and the decision to drop out, not only should interventions that reallocate students be considered, but so should interventions based on providing information about students' real ability levels, perhaps more strongly. Overall, my results show that universities could realize benefits by paying more attention to peer group mechanisms and

[2] Carrell et al. (2013) conduct an experiment in which they assign students to peer groups in a way they expect to be optimal. In contrast to their expectations, they observe that the students they intended to help were actually harmed by this intervention.

formations.

The rest of this paper proceeds as follows: Section 2 introduces the background and the literature. The peer effect literature is extensive, and Section 2 discusses only the most relevant studies in detail. Section 3 describes the institutional setting of CBS and the structure of the bachelor's program. Section 4 introduces the econometric model, and Section 5 describes the data and tests the identifying assumption of randomly assigned peer groups. Sections 6 and 7 report and discuss the results, and Section 8 presents robustness checks. Finally, Section 9 concludes the paper.

2 Background and Literature

2.1 Econometric Challenges

Manski (1993) defines three kinds of effects that can make individuals in the same group more likely to behave in similar ways. He distinguishes between (1) endogenous peer effects, (2) exogenous (contextual) peer effects, and (3) correlation effects. Exogenous effects occur when an individual's behavior is influenced by "spillover" effects of the socio-economics characteristics of the peer group. Endogenous effects occur when an individual's behavior varies with that of the peer group. The endogenous and exogenous effects are often considered together as social peer effects. Correlation effects are group-specific effects on the behavior of both the peer group and the individual.

Manski (1993) outlines the complex econometric problems involved in identifying these peer effects. First, there is the problem of self-selection, in which individuals sort themselves into the groups that are most beneficial to them. If self-selection is not addressed, the estimation of peer effects will be biased. Second, there is the reflection problem. Manski (1993) uses the term "reflection" because the difficulty resembles that of interpreting the almost-simultaneous movements of a person and his reflection in a mirror. Econometrically speaking, the reflection problem corresponds to the problem of two-way causality that arises from the interdependence of individual and peer group behavior. Self-selection into peer groups and two-way causality of individual and peer group behavior, makes it difficult to separating identify the different kinds of peer effects described above. Moreover, if the correlation effect is not independent of the peer effects in question, the results can suffer from an omitted variable bias.

Thus when one is estimating peer effects, the above mentioned challenges are present. This paper addresses the self-selection problem, the problem of two-way causality, and argues that the correlation effect is independent of it's measure of peer group ability, but it does not attempt to distinguish endogenous and exogenous peer effects. Very few studies have managed to do that (e.g., Bramoullé et al., 2009; De Giorgi et al., 2010); most have simply estimated what they refer to as a "social peer effect". Because peer effects on university dropouts are largely overlooked in the literature, any results will add to our knowledge of them. Thus, this paper contributes to the literature even though it does not separately identify the different peer effects.

More recently, Angrist (2014) has outlined problems of biased estimates and spurious correlations when estimating peer effects, underlining the difficulties in estimating and interpreting a peer effect. In order to understand the impact of such issues on my study, I perform (pseudo) placebo estimations where artificial peer groups are created. If my placebo results are insignificant, it indicates that my results are in fact a peer effect and not caused by a mechanical relationship, measurement errors, or spurious correlations. In most of the cases, my placebo peer groups have no significant impact on individuals' outcomes. However, I do sometimes observe a significant placebo peer effect. This could, however, be generated by the way I construct my artificial peer measures. I return to this in Section 8.

One of the things that Angrist (2014) shows is how measurement errors in behavior/performance at both the individual and peer group level can lead to an overestimation of a peer effect. This is opposite of what is commonly believed when measurement error is considered.[3] However, building on the results in Angrist (2014), Feld and Zölitz (2015) show, analytic and using Monte Carlo simulations, that with random assignment of peer groups, the estimates of a peer effect will suffer only from attenuation bias due to measurement errors. Relying on randomly assigned peer groups and pre-determined measure of peer group performance, Feld and Zölitz (2015) continues with a similar strategy as presented in this paper.

2.2 Previous Literature

Although there is extensive literature on peer effects at all educational levels, the vast majority of the studies concentrate on peer effects on educational performance at primary, secondary, and high school levels (e.g., Ammermueller and Pischke, 2009; Duflo et al., 2011; Lavy et al., 2012; Burke and Sass, 2013; Vardardottir, 2013; Murphy and Weinhardt, 2014).[4] Some papers have examined effects on

[3] Angrist (2014) relates the estimated peer effects to the difference in an IV estimator and a OLS estimator, where the IV estimate comes from a regression that uses group dummies as instruments for individual i's behavior and the OLS estimate comes from regression i's outcome on the group behavior. By relating the estimated peer effect to the difference in these two estimated coefficients, Angrist (2014) show how measurement error in peer group and individual behavior can result in an overestimation of a peer effect. This result is also deduced in Feld and Zölitz (2015).

[4] See also Sacerdote (2011) and Epple and Romano (2011) for very useful overviews of the methods and empirical findings in education.

performance at the tertiary level (e.g., Sacerdote, 2001; Zimmerman, 2003; Arcidiacono and Nicholson, 2005; Carrell et al., 2009; Han and Li, 2009; Carrell et al., 2013; Hasan and Bagde, 2013; Thiemanny, 2013), but only a few have looked at peer effects on university dropout rates (Johnes and McNabb, 2004; Booij et al., 2015). More papers have focused on establishing other determinants of the decision to drop out of university (e.g., Smith and Naylor, 2001; Becker, 2001; Montmarquette et al., 2001; Arulampalam et al., 2005; Stinebrickner and Stinebrickner, 2009). Peer effects on other behaviors, such as alcohol consumption, exercise habits, and choice of major, have also been investigated (e.g., Sacerdote, 2001; De Giorgi et al., 2010; Ost, 2010; Carrell et al., 2011).

Different estimation strategies have been adopted to address the econometric difficulties pointed out by Manski (1993). Some studies have used school- and pupil-fixed effects to address endogeneity due to self-selection (e.g., Ammermueller and Pischke, 2009; Arcidiacono and Nicholson, 2005; Burke and Sass, 2013). Others have used random assignment of roommates (e.g., Sacerdote, 2001; Han and Li, 2009; Hasan and Bagde, 2013), classes, or peer groups (e.g., Carrell et al., 2009; Thiemanny, 2013; Booij et al., 2015). Finally, some have used the method of discontinuity design to identify peer effects (e.g., Duflo et al., 2011; Vardardottir, 2013).

When creating measures of peer quality, it is common to use pre-determined ability level as an exogenous measure, because this lets one overcome the reflection problem (e.g., Ammermueller and Pischke, 2009; Carrell et al., 2009; Lavy et al., 2012; Thiemanny, 2013; Burke and Sass, 2013). Only a few studies have separately identified endogenous peer effects: Bramoullé et al. (2009) use students' social network interactions to distinguish endogenous and exogenous effects, and De Giorgi et al. (2010) use overlapping peer groups to do so.

Dropouts

The relationship between peer group ability and the probability of dropping out of university has not been adequately explored in the literature. Few similar studies to this one have been carried out by Johnes and McNabb (2004) and Booij et al. (2015), and related ones by Arulampalam et al. (2005) and Smith and Naylor (2001).

Arulampalam et al. (2005) use data from the UK to investigate the impact of in-class variation and rank on the probability of dropping out during the first year of university. They find that ranking higher (or lower) decreases (or increases) the probability of dropping out by 1 percentage point for men. They observe the same results for low-ability women but find no effect for high-ability women. However, their sample consisted of 56 universities and 19 broad subject areas, and they used around a thousand groups with an average of 100 students each. Thus despite the similarities between our studies, the captured effects might have different causes, and the results cannot be compared directly.

Smith and Naylor (2001) examine data on the cohorts of students enrolling in for three- and four-year degree programs at UK universities in 1989 and 1990. They find that prior academic preparedness and social integration at the university are important to probability of completion. They also find that non-UK European students are significantly more likely to drop out than are UK students, and that students who live off-campus are more likely to drop out. This impact of lack of social integration can be interpreted as a sort of peer effect.

Using data on English university students, Johnes and McNabb (2004) investigate peer effects on dropouts. They differentiate among three outcomes: completion of degree, academic failure (involuntary dropout), and dropout (voluntary) and therefore estimate a multinomial logit model. In addition to individual-specific effects on the probability of dropping out, they also find that students who are above the ability levels of their peers are more likely to quit voluntarily.

In a recent, related paper, Booij et al. (2015) examine data on first-year students in the undergraduate program in economics and business at the University of Amsterdam in 2009–10, 2010–11, and 2011–12. They use tutorial groups as their definition of peer groups and create variation in the peer-ability measures by assigning students randomly into peer groups on the basis of their pre-university grades. In contrast with the present paper, they find no significant effect of average peer ability level on the likelihood of dropping out. However, their results from simulation do suggest that switching from ability-mixing to groups with three-way tracking can reduce dropout rates among low-ability students by as much as 17 percentage points. Almost all studies of the determinants of dropping out find that higher levels of academic aptitude and pre-determined abilities decrease the probability of dropping out.

Peer Effects on Educational Performance

Researchers have recently started investigating the impact of ability ranking on educational performance (Murphy and Weinhardt, 2014; Elsner and Isphording, 2015). These studies are, among other things, inspired by the literature on the effects of workplace rankings on job satisfaction (e.g., Brown et al., 2008; Card et al., 2012). Using English administrative data, Murphy and Weinhardt (2014) find a significant rank effect on educational performance. They show that students with higher academic ranks in a subject in primary school perform better in that subject in subsequent years, even among new peers. On the basis of survey data, they argue that increased confidence is the most likely cause of this. Thus, their results indicate that non-cognitive traits such as confidence can have an impact on educational performance. Along the same lines, Elsner and Isphording (2015) show that students with higher ranks in high school are significantly more likely to finish high school and to attend and finish college. They also show using survey data that students with higher ranks are more optimistic, have higher perceived intelligence, and are helped more by their teachers, which could be potential mechanisms for the positive association between cohort rank and educational achievement.

Sacerdote (2001), Han and Li (2009), and Hasan and Bagde (2013) use random assignments of roommates to investigate peer effects in college. Sacerdote (2001) uses data from Dartmouth College to estimate the impacts of roommates and dorm-mates on several outcomes, including GPA, choice of major, and choice of fraternity. He finds a positive roommate effect on GPA and both roommate and dorm-mate effects on the decision to join a fraternity. Using data on roommates in China, Han and Li (2009) find that weak females benefit from stronger female peers and that strong females are not harmed by weaker female peers. They also find that males do not respond to the academic levels of their peers. Hasan and Bagde (2013) examine data from an engineering college in India at which students are randomly assigned roommates. They too find a positive and significant roommate effect on first-year performance. Moreover, they find that roommate effects persist through the first two years only when the roommate is a high-performing student. The latter result indicates that students become more selective of their peers the longer they are in college.

Carrell et al. (2009) study the random assignment of freshmen to squadrons in the US Air Force Academy. Using a pre-determined measures of peer ability levels, they find significant peer effects. Their results suggest that the lowest-ability students benefit the most from having high-ability peers. However, Carrell et al. (2013), after implementing an optimal distribution of peers on the basis of their own earlier results in Carrell et al. (2009), observe that the students they intended to help were actually harmed by their intervention. This happened because students formed smaller subgroups with others of the same ability levels, which had an adverse impact on the performance of low-ability students.

Arcidiacono and Nicholson (2005) investigate peer effects on academic achievement and choice of specialization among students who graduated from a US medical school between 1996 and 1998. After controlling for school-fixed effects, they find a significant peer effect only for female students. Moreover, this effect persists only in educational performance (board exams), not in specialization preferences.

In a study related to my analyses of both performance and dropout rates, Thiemanny (2013) measures student performance as a binary variable. Unlike this paper, however, her study models the probability of passing the first year as dependent on peer-group characteristics and abilities. She uses a unique data set from the University of St. Gallen and finds positive effects of peer quality on the academic performance of individuals who fall below the median of the distribution of peer quality. In particular, she finds that for men, the probability of passing the first year increases with peer quality.

3 Bachelor's Program Structure and Assignment of Peers

This paper uses a data set obtained from CBS containing information on students enrolled in the three-year bachelor's program in business economics at CBS between 1996 and 2004. This is the largest bachelor's program at CBS. It is structured as follows: During their three undergraduate years, students mostly take mandatory courses taught by professors in large lecture rooms. They often have exercise classes, run by teaching assistants, connected with these courses. When they enroll in the bachelor's program, students are randomly sorted into small groups and are assigned to the exercise classes on the basis of these groups.[5] In the exercise classes, the students solve problems together or have the teaching assistant go through assignments on the blackboard. Students are likely interacting much more in these exercise classes than in the lectures. I use the tutorial groups as my definition of peer groups, which is a way of defining peer groups already applied in the literature (e.g., De Giorgi et al., 2012; Feld and Zölitz, 2015; Booij et al., 2015).

My definition is based on the assumption that students attend the exercise classes and that peer effects are thus fostered through interactions in these groups. The exercise classes are known to be popular, and it is not uncommon for students to prefer them to the lectures, because they offer a better opportunity for talking and asking questions. This means that the expectation that students attend the exercise classes and spend the majority of their time at CBS with other students from their peer groups is a plausible one. Importantly, because these peer groups are assigned randomly, I do not face the standard econometric problem of self-selection of peer groups.

It is intended that students stay in their peer groups throughout the bachelor's program. Under certain circumstances, however, individuals can change groups and get new peers in their second or third year. For example, if one group becomes too small, it may be merged with another. A student might also actively choose to change groups. This is allowed, under very limited circumstances, in the second and third years of study. If a student wants to change groups he or she must apply for dispensation and find someone in the preferred group who is willing to switch. Because I use first-year GPA and first-year dropout decisions as my dependent variables, the impact of peers (and other

[5] Sometimes one exercise class includes students from two of these groups. However, this is mostly in the second or third year, when students can also choose electives.

factors) is measured only through the first year of study, and later changes in peer groups do not create a problem for my estimates.

Few stratification rules are implemented by CBS. The program's administrators assign individuals into groups on the basis of gender, nationality, and age. Students are distributed so that the proportions of men and of Norwegians are approximately the same in all peer groups in a given year.[6] The administration also creates one or two "older" groups. These groups have a higher average enrollment age and are likely to contain more students who have spent a couple of years working or studying other subjects. Given that their members are older and are likely to have more experience, the impact of peers in these groups might be either more or less important. I therefore control for the average age of the group when estimating potential peer effects.[7] The fact that the distribution into groups is based on these three characteristics does not pose a difficulty for avoiding the problem of self-selection. In fact, it guarantees very homogeneous peer groups in terms of these characteristics, which ensures that it is not a group-composition effect that is captured in the estimations.

Despite the unique features of the data, one issue still requires consideration. Some students might sort themselves into other exercise classes than the ones they were assigned to and thereby make unofficial changes to peer groups. Unfortunately, the data do not allow me to identify this type of behavior. However, such mistakes in the measurement of peers would only weaken my results as I would risk consider some individuals as i's peers even if they are not. The most serious consequences of this is that if, for instance, men are more likely than women to make unofficial changes to their peer groups, this could explain why I find no significant peer effect for men. Thus, the main consequences is that I risk finding no peer-effect when there is in fact one. However, because such unofficial changes are more likely to occur in the second and third years, after the students are familiar with the systems and regulations of CBS, the problem is likely to be small.

4 Econometric Framework

Formally, my data on peer groups can be summarized by:

- Enrollment year, t, with $t = 1996, ..., 2004$
- $\forall$ t: H_t initial peer groups, where h_t is a particular peer group in year t and $h_t = [1, \ldots, H_t]$.

[6] CBS has a large number of Norwegians enrolling every year. In order to avoid a "Norwegian" group, Norwegians are distributed equally among groups.

[7] I account for the age differences by including the average starting age of one's peer group and the average starting age of the peer group squared (leave-out-mean) in all regressions. I have also done a battery of robustness estimations with various ways of controlling for the average starting age of the peer group. The results remain across all specifications.

- $\forall\ t$, h_t: N_{h_t} students and for each student belonging to h_t we have $N_{h_t} - 1$ peers.

In order to estimate a peer effect, I start out by specifying a reduced form linear-in-means model (e.g., Manski, 1993; Carrell et al., 2009).

$$y_{iht} = \beta_0 + \gamma \bar{P}_{-i} + \beta X_i + \alpha Z_{-i} + \theta_t + \varepsilon_{ih_t} \tag{1}$$

$$\varepsilon_{ih_t} = c_{h_t} + \epsilon_i \tag{2}$$

y_{iht} is the outcome variable of individual i in peer group h with enrollment year t. $\bar{P}_{-i}$ is a measure of peer quality, and accordingly γ is the peer effect. Z_{-i} is a vector of peer group characteristics such as class size, male share, and average age; X_i is a vector of individual-specific characteristics, including type of high school, high school GPA, gender, enrollment age, place of residence five years before entering CBS, and parental characteristics; and θ_t is a cohort-fixed effect. Finally, c_{h_t} is an unobserved peer-group-fixed effect, also referred to as the correlation effect. Because c_{h_t} introduces error correlation across individuals in the same peer group, I cluster all standard errors by peer group. If c_{h_t} is correlated with the explanatory variables, the model will suffer from omitted-variable bias. In Section 4.1, I explain how c_{h_t} is independent of $\bar{P}_{-i}$, X_i, and Z_{-i}, which means that the estimated peer effects are not contaminated by omitted variable bias.

When the behavior of an individual, y_{iht}, and of the peer group, $\bar{P}_{-i}$, are determined simultaneously, the model presented by Equations (1) and (2) suffers from the reflection problem described by Manski (1993). To handle this, I create a measure of peer quality based on pre-determined characteristics of the individuals in the peer group—specifically, on peers' high school GPAs. I return to this in Section 5.1. Moreover, because peer groups are assigned randomly, the problem of endogenous selection into peer groups is circumvented by construction.

When y_{iht} is a dummy equal to one if individual i drops out during the first year, I model the probability of dropping out using a probit specification captured by Equation (3).

$$P(y_{iht} = 1) = \Phi(\beta_0 + \gamma \bar{P}_{-i} + \beta X_i + \alpha Z_{-i} + \theta_t) \tag{3}$$

Where the underlying assumptions of the probit model are:

$$y_{iht} = \mathbf{1}[y^*_{iht} \geq 0]$$
$$y^*_{iht} = \beta_0 + \gamma \bar{P}_{-i} + \beta X_i + \alpha Z_{-i} + \theta_t + \varepsilon_{ih_t}$$
$$\varepsilon_{ih_t} \sim N(0,1)$$

If the assumption made in the probit model, namely that $\varepsilon_{ih_t} = c_{h_t} + \epsilon_i \sim N(0,1)$, fails because $c_{h_t} + \epsilon_i$ is distributed differently and c_{h_t} has an impact on the outcome, it will introduce a problem of neglected heterogeneity and cause the estimates to suffer from attenuation bias (but the sign of the peer effect remains) (see section 15.7 in Wooldridge, 2010). By contrast with standard Ordinary Least Square (OLS) methods, this is true even if c_{h_t} is independent of all the explanatory variables. The main consequence of this is that the estimated peer effect must be considered a lower bound (in absolute terms). To test the consequences of potential neglected heterogeneity, I perform a robustness test in which I estimate the probability of dropping out with a Linear Probability Model (LPM) and compare the results to the ones obtained from probit estimation. The LPM and probit models give almost identical results, which indicates that the neglected heterogeneity problem is minor or nonexistent.

Because students are assigned to peer groups randomly, it means that individual characteristics are supposed to be uncorrelated with peer quality. Thus, the inclusion of X_i in the regressions should not affect the estimated peer effects. X_i is included anyway because it provides more efficient estimates. Moreover, the results on the individual-specific variables are also interesting for comparison purposes and provide additional knowledge about dropout decisions. The estimates made with and without individual characteristics are shown in Appendix Table B.2.

4.1 The Correlation Effect

One of the main problems in identifying a peer effect is that the behavior of the peer group can be affected by unobserved group factors that might also affect individual behavior. Such group-specific effects arise when the group is subjected to a common influence or shock that affects both individual outcomes and group outcomes but is not modeled directly in the regression. This is what Manski (1993) refers to as the correlation effect. If a study uses data from multiple schools, for instance, the quality of the schools will be captured in the correlation effect. Because this paper uses data only on students from CBS, the problem of confounding effects related to schools is not present (see also Ammermueller and Pischke, 2009). This does not, however, completely eliminate the problem of bias due to the correlation effect.

Formally, the correlation effect enters the model through the error term, as shown in Equation (1) and Equation (2). Here, the error term consist of two components: a group-specific effect, c_{h_t}, and an individual-specific effect, ϵ_i. The former, c_{h_t}, might measure, for instance, the effect of a very gifted teacher who raises or lowers the educational level of each individual and of the entire group simultaneously. It might also capture various characteristics of the classrooms, such as its being too dark or cold, or differences in time schedules among groups. If the group-specific effect is correlated at the same time with the behavior of the group and the behavior of each individual, the estimated peer-effect coefficients will be biased. However, because the measure of peer quality used in this study, $\bar{P}_{-i}$, is pre-determined, neither classroom conditions nor teacher quality is correlated with it, which ensures that this bias is not present.

Finally, if the groups are discriminated, positively or negatively, on the basis of pre-determined characteristics, the group-specific effect could be correlated with my measure of peer quality. This would be the case, for instance, if CBS matched teachers with peer groups so that better teachers were consistently assigned to higher-level groups. But because this is not a policy of CBS, the predetermined measures of peer quality are not correlated with the peer-group-specific effect.

In summary, there are two reasons that c_{h_t} is not correlated with any of the measures of peer quality. First, the measure of peer quality is based on pre-university characteristics and is thus predetermined in the model. A common shock or a teacher effect that might affect the behavior of an individual will not affect the measure of peers' abilities. Furthermore, and importantly, CBS does not treat any groups differently on the basis of high school GPA or any other factor. Thus all measures of $\bar{P}_{-i}$ based on pre-determined characteristics of the peers are independent of c_{h_t}.

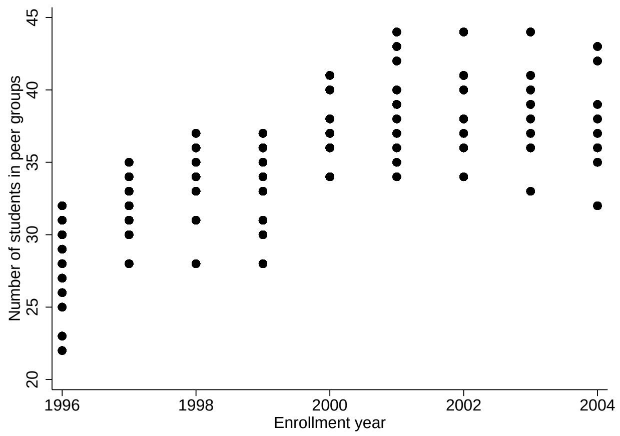
5 Data

This paper uses data on students who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004. The data include detailed educational information, such as course-specific grades, dropout information, and first-year and high school GPAs. I combine these data with Danish register data containing socio-economic information on the entire Danish population. This lets me combine unique and detailed educational data with background characteristics that might also explain educational choices and performances.

5.1 Measuring Peer Quality

When individuals enroll in the business economics bachelor's program at CBS each year, they are randomly divided into small (peer) groups. The number of groups varies from 14 to 18. The number of students in each group also varies, but has increased over time, as can be seen in Figure 1. This development also shows the importance of including cohort-fixed effects when estimating on a pooled sample.

Figure 1: Number of Students in Peer Groups across Enrollment Years*



* Some peer groups have the same number of students which, for instance, explain why there is only 8 points for 2004.

In this paper, I measure peer quality by the ability level of the peer group. I use students' high school GPAs to create different measures of peer quality. The key feature of these measures is that they are based on predetermined achievements and are therefore exogenous in the model: they have not been determined simultaneously with individual behavior.[8] The first measure of peer quality I use is the average ability level of peers, as given by Equation (4):

$$\bar{A}_{-i} = \frac{\sum_{j=1, j \neq i}^{N_h} GPA_j^{HS}}{N_h - 1} \tag{4}$$

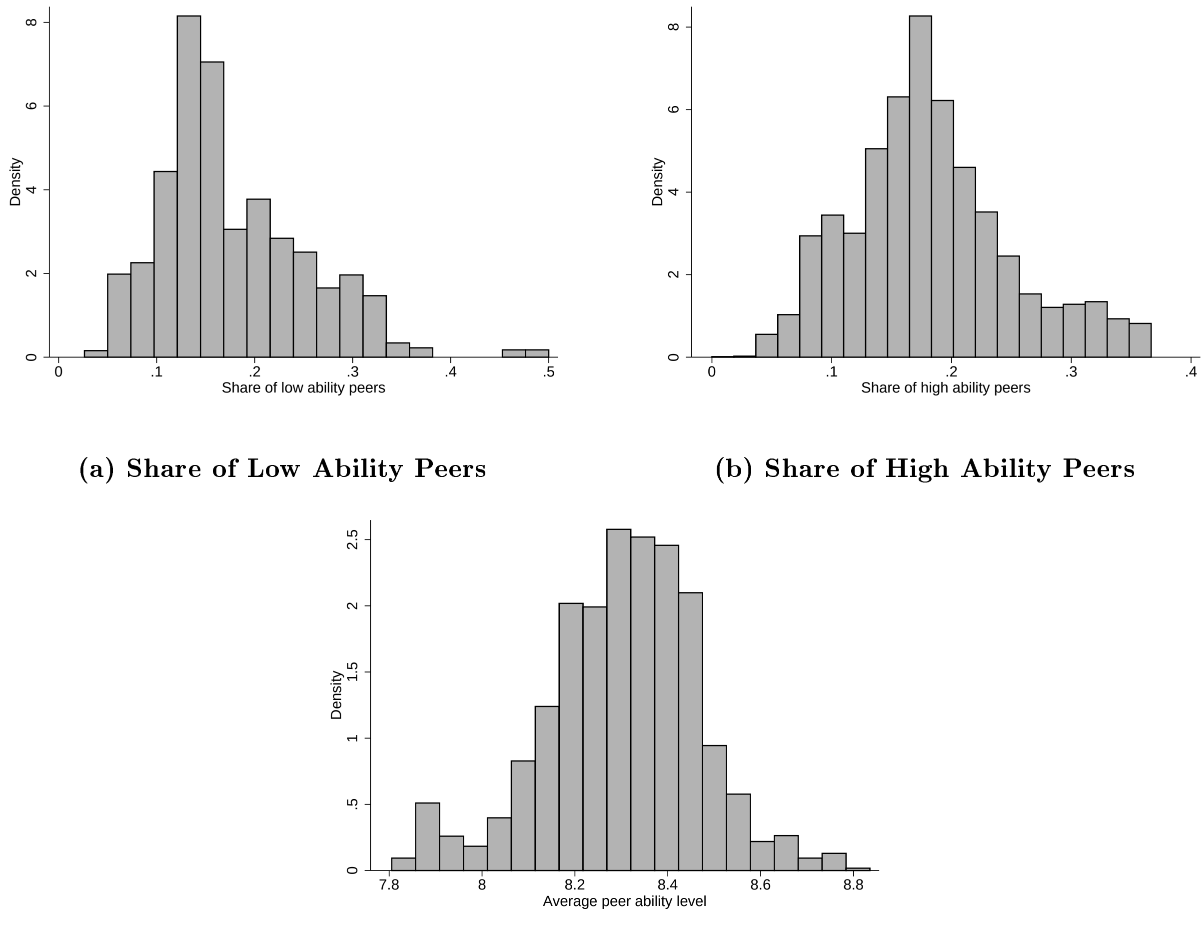
Here, GPA_j^{HS} is the high school GPA of individual j. I also measure peer quality by the shares of peers with high school GPAs above and below the 80$^{\text{th}}$ and 20$^{\text{th}}$ percentile of the cohort's GPA distribution. I label these variables *Share of high-ability peers* and *Share of low-ability peers*, respectively. When

[8] This way of creating an exogenous measure of average peer ability is standard in the peer effect literature (e.g., Carrell et al., 2009; Lavy et al., 2012). An example is the work of Lavy et al. (2012), who conduct their analysis on pupils in secondary schools (14 years) and use performance at age 11 as their prior-achievement measure.

computing these shares, I consider the cohort high school GPA distribution excluding individual i's high school GPA. I do this for the same reason as I leave out individual i in the measure of $\bar{A}_{-i}$.

Because the peer groups are created on the basis of gender, nationality, and age, one might worry that there is not enough variation in peer quality measures between groups. However, the program is not an elite one and admits students of many ability levels.[9] The variation in ability levels and the random allocation into peer groups ensure sufficient variation in peer-group quality. This variation is depicted in Figure 2.

Figure 2: Histogram of Peer-Quality Measures, All Years Pooled



(a) Share of Low Ability Peers

(b) Share of High Ability Peers

(c) $\bar{A}_{-i}$

Finally, to measure the impact of the standard deviation in peer quality, I compute the standard deviation of high school GPAs within peer groups. Inclusion of the standard deviation is not common in the literature, but is done by some (e.g., Booij et al., 2015). When creating all measures of peer quality,

[9]Fields like international business economics, medicine, and political science are extremely popular in Denmark, which means that only students with very high GPAs from high school are allowed into these types of fields.

I leave out i in order to ensure that the measure is not correlated with the outcome by construction.

5.2 Summary Statistics and Background Variables

Access to the Danish register data allows me to combine educational information with background characteristics of the students. This requires me to limit the study sample to Danes because it lets me include background characteristics that are likely to be important for educational behavior. Moreover, the background characteristics allow me to test the assumption of random assignment into peer groups. I also exclude individuals with inconsistent or missing information (e.g., missing high school GPAs) in the educational raw data.[10] In total, I exclude approximately 10 percent of my original sample, which leaves me with a sample of 4,340 students.

Table 1 provides summary statistics on all the relevant variables across different sub-samples. The average first-year dropout rate across all years is 19%. Sixteen percent drop out of CBS without supplying information on what they do afterward, and 3% transfer to other programs at CBS or other business schools. Thiemanny (2013) uses data on first-year students at the University of St. Gallen and shows that 7% of the sample drops out each semester and only 66% passes the first year. Compared to this, a dropout rate of 19% does not seem unreasonable. For comparison, Carrieri et al. (2015) reports a dropout rate of around 35% among first-year students in economics at the University of Salerno between 2005 and 2010.

On average, students are 21 years old when they enter CBS and have a high school GPA just above 8. All GPAs are computed using the Danish "13" grading scale, on which the lowest passing grade is 6 and the highest grade is 13. A high school GPA of 8 indicates that a student is slightly above average.[11] I find no major difference in performance or pre-determined ability between men and women. Most of the students lived in Zealand for five years prior to enrollment, but around 15% of the sample came from other parts of Denmark, mostly Jutland.

[10] Most individuals with missing high school GPAs are non-Danes. The administration at CBS translates the GPA from foreign students into a Danish GPA whenever it is possible. In creating the measures of peer quality, $\bar{P}_{-i}$, I relied on information from the entire sample of students with available information on high school GPAs. Relying only on Danes does not change my results. Ammermueller and Pischke (2009) show how peer effects can be biased due to measurement error in peer group composition. However, in their example of within school peer effects, they show that the estimated peer effect suffer only from attenuation bias.

[11] See the Main Appendix A of this thesis for a translation of the Danish grading scale.

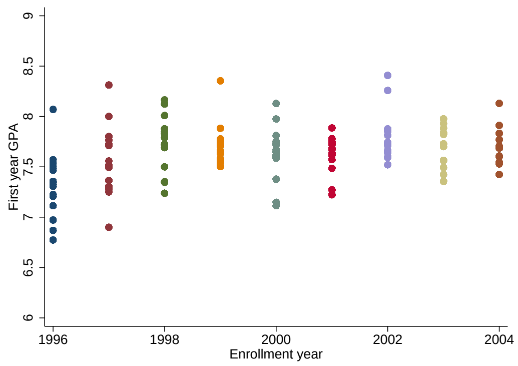
Table 1: Summary Statistic

	All		Without dropouts		Men		Women	
	Mean	Std.	Mean	Std.	Mean	Std.	Mean	Std.
In-university characteristics:								
Regular dropout	0.16	(0.36)			0.15	(0.35)	0.17	(0.38)
Change of study within CBS	0.03	(0.18)			0.03	(0.17)	0.04	(0.19)
First Year GPA			7.63	(0.99)				
$\bar{A}_{-i}$	8.30	(0.17)	8.30	(0.16)	8.30	(0.17)	8.30	(0.16)
Initial class male share	0.69	(0.06)	0.69	(0.06)	0.68	(0.05)	0.70	(0.06)
Initial class size	35.58	(4.43)	35.60	(4.37)	35.52	(4.46)	35.73	(4.35)
Share of high ability peers	0.18	(0.07)	0.18	(0.07)	0.18	(0.07)	0.18	(0.07)
Share of low ability peers	0.18	(0.08)	0.18	(0.08)	0.18	(0.08)	0.18	(0.08)
Pre-university characteristics^{A}:								
GPA^{HS}	8.29	(0.80)	8.35	(0.79)	8.27	(0.82)	8.35	(0.74)
Starting age	21.47	(1.86)	21.41	(1.70)	21.51	(1.86)	21.38	(1.84)
Woman	0.31	(0.46)	0.30	(0.46)	0.00	(0.00)	1.00	(0.00)
General high school	0.72	(0.45)	0.73	(0.45)	0.70	(0.46)	0.75	(0.43)
Jutland	0.09	(0.28)	0.09	(0.28)	0.08	(0.27)	0.10	(0.30)
Fyn and Bornholm	0.04	(0.19)	0.04	(0.20)	0.04	(0.20)	0.03	(0.18)
Copenhagen	0.11	(0.32)	0.11	(0.31)	0.11	(0.32)	0.11	(0.31)
Greater Copenhagen	0.32	(0.47)	0.32	(0.47)	0.32	(0.47)	0.32	(0.47)
Frederiksborg	0.23	(0.42)	0.23	(0.42)	0.24	(0.43)	0.20	(0.40)
Father's year of education*:								
Missing	0.12	(0.32)	0.11	(0.32)	0.12	(0.32)	0.12	(0.32)
Mandatory edu.	0.13	(0.34)	0.12	(0.33)	0.13	(0.33)	0.13	(0.34)
General High School	0.04	(0.19)	0.04	(0.20)	0.04	(0.20)	0.04	(0.18)
Business High School	0.02	(0.13)	0.02	(0.13)	0.02	(0.12)	0.02	(0.14)
Professional Qualifications	0.28	(0.45)	0.29	(0.45)	0.27	(0.44)	0.31	(0.46)
Short Tertiary	0.04	(0.19)	0.04	(0.19)	0.04	(0.19)	0.04	(0.19)
Medium tertiary	0.17	(0.38)	0.17	(0.38)	0.18	(0.38)	0.16	(0.36)
Bachelor	0.02	(0.14)	0.02	(0.15)	0.02	(0.14)	0.02	(0.14)
Master's or above	0.18	(0.39)	0.19	(0.39)	0.19	(0.39)	0.17	(0.37)
Mother's year of education*:								
Missing	0.05	(0.21)	0.04	(0.19)	0.05	(0.21)	0.04	(0.20)
Mandatory edu.	0.17	(0.37)	0.16	(0.37)	0.16	(0.36)	0.19	(0.39)
General High School	0.05	(0.21)	0.05	(0.21)	0.05	(0.21)	0.04	(0.20)
Business High School	0.01	(0.09)	0.01	(0.10)	0.01	(0.10)	0.01	(0.09)
Professional Qualifications	0.30	(0.46)	0.31	(0.46)	0.30	(0.46)	0.32	(0.47)
Short Tertiary	0.05	(0.22)	0.05	(0.22)	0.04	(0.21)	0.07	(0.25)
Medium Tertiary	0.28	(0.45)	0.29	(0.45)	0.30	(0.46)	0.24	(0.43)
Bachelor	0.01	(0.09)	0.01	(0.09)	0.01	(0.08)	0.01	(0.12)
Master's or above	0.09	(0.28)	0.09	(0.28)	0.09	(0.29)	0.07	(0.26)
No. of obs	4340		3562		2998		1342	

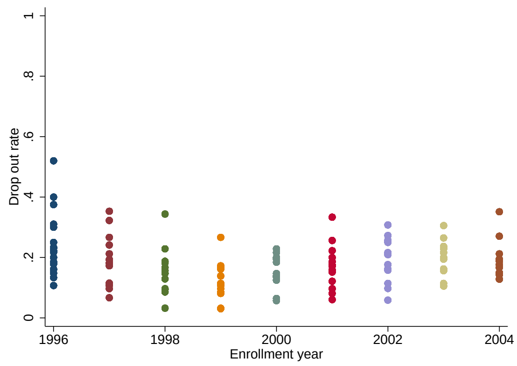
Note: The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004. ^{A}: The excluded location group is the rest of Zealand. *I handle the missing parental information by including dummies in the regressions with the group with missing as my reference. Business and General High School are consider one group in the regressions.

Figure 3a shows average first-year GPAs by peer group and enrollment year, and Figure 3b shows dropout shares by peer group and enrollment year. The figures show variation among peer groups in first-year GPAs and in dropout rates, which indicates that some kind of group effect is probably occurring, but whether this is caused by variation in peer quality or in other group characteristics cannot be determined from the graphs alone. This is why an econometric model is needed. By estimating an empirical model that allows educational behavior to depend on peer quality, I can investigate whether individual educational behavior during the first year of study can be explained by variation in peer quality or must stem from other factors. Figures 3a and 3b also show variation in group behavior across enrollment years, which again underlines the importance of including cohort-fixed effects when estimating on a pooled sample. Finally, Figure 3c shows average high school GPAs by peer group and enrollment year. Variation can be observed across both enrollment years and peer groups, but less than in Figures 3a and 3b.

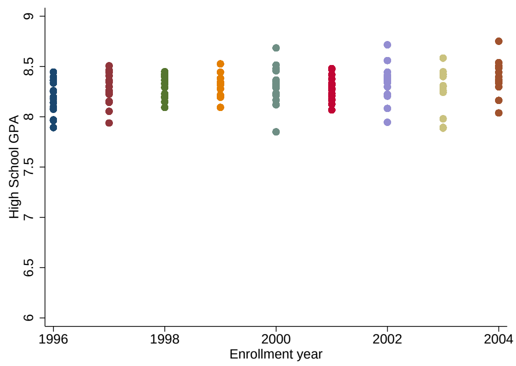
Figure 3: Educational Behavior Across Peer Groups and Enrollment Years*



(a) First year GPA



(b) Dropout rates



(c) High School GPA

* Note: Figure 3a only consider the sample of students that did not drop out during the first year.

5.3 Identifying Assumption: Random Assignment of Peer Groups

In order to consistently estimate a peer effect, the model assumes that individuals were assigned to their peer groups randomly and that there is therefore no self-selection. If individuals selected their own peer groups, variations in peer group quality would be caused by this selection. For instance, if high-ability individuals formed groups together to benefit from high-level peers, the estimated peer effect would be upward biased.[12] The absence of self-selection is thus crucial for identifying the estimated peer effect.

Because of the random assignment, peer quality and individual characteristics, both observed and unobserved, should be uncorrelated by construction. To test whether peer group assignment has the properties that one would expect under random assignment, I test whether individual-specific observables are in fact uncorrelated with one of my measures of peer ability, $\bar{A}_{-i}$, by regressing $\bar{A}_{-i}$ on all controls. If the assignment is truly random, there should be no significant correlation between the background variables and $\bar{A}_{-i}$. Table B.1 shows the results of the estimations. Unsurprisingly, it reveals no significant correlation between $\bar{A}_{-i}$ and almost any individual-specific controls. Enrollment age enters the regressions as significant. This is not surprising, as individuals are assigned to groups on the basis of enrollment age. Few educational characteristics of the father are also significant when estimating on the sample of women. However, all education dummies are jointly insignificant as can be seen from the p-values reported in the bottom of Table B.1. These results support the assumption of random assignment of peers.

Table B.2 shows estimations made with and without individual characteristics included as controls. The fact that the estimated peer effect does not change significantly in magnitude between specifications means that none of the additional explanatory variables takes any power from the peer quality measures, which they would do if they were correlated with the peer quality measure. This also supports the assumption of random assignment of peers.

6 Results: Dropouts

6.1 Baseline Results

Table 2 reports the average marginal effects from a probit estimation of Equation (3), with y_{iht} as a binary variable equal to one if the student dropped out during the first year. The results of the estimations on the full sample reveal a significant peer effect on the probability of dropping out.

[12] Lavy et al. (2012) formulate it it this way: *"...the identifying assumption is that the variation of peer quality over time or across classes is idiosyncratic and uncorrelated with students' potential outcomes and background."*

Estimating by gender reveals that this result is driven by women. Specifically, women's probability of dropping out increases with peer quality ($\bar{A}_{-i}$ and *Share of high ability peers*), whereas men's probability of dropping out is unaffected by peer quality. Thus, for women, a high-quality peer group increases the probability of dropping out during the first year. Moreover, the dropout probability for women is decreasing in the share of males in the peer group, which is consistent with the findings of Johnes and McNabb (2004), who show that women are less likely to drop out if they are grouped with a high proportion of males. The finding that women are more affected by their peers is in line with the literature showing that females are more affected than males by school interventions, peer interactions, and educational inputs (e.g., Arcidiacono and Nicholson, 2005; Angrist et al., 2009; Han and Li, 2009; Ost, 2010; Lavy et al., 2012). The standard deviation of the peer group ability distribution is insignificant across all estimations (columns 7–9).

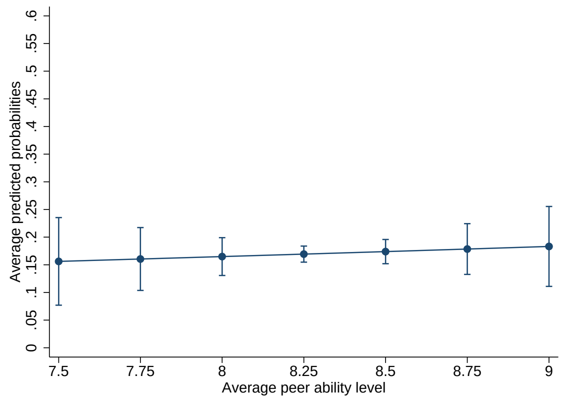
Table 2: Probability of Dropping Out - Probit Estimation

	All			Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peer effects:									
Share of high ability peers	0.217**			0.106			0.517***		
	(0.092)			(0.097)			(0.169)		
Share of low ability peers	0.101			0.105			0.086		
	(0.095)			(0.106)			(0.157)		
$\bar{A}_{-i}$		0.076			0.019			0.222***	
		(0.048)			(0.051)			(0.082)	
SD_{-i}			0.050			0.050			0.069
			(0.070)			(0.072)			(0.115)
Initial class male share	-0.193	-0.218*	-0.213*	-0.111	-0.123	-0.121	-0.384*	-0.465**	-0.433**
	(0.118)	(0.122)	(0.120)	(0.124)	(0.128)	(0.126)	(0.213)	(0.217)	(0.221)
Initial class size	0.004*	0.003	0.003	0.001	0.001	0.001	0.011***	0.010***	0.009**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
Personal characteristics:									
GPA^{HS}	-0.071***	-0.071***	-0.071***	-0.078***	-0.078***	-0.078***	-0.048***	-0.048***	-0.049***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)
Starting age	0.004	0.004	0.004	0.005	0.006	0.005	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)
General high school=1	-0.034**	-0.034**	-0.034**	-0.037**	-0.037**	-0.037**	-0.027	-0.027	-0.026
	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)	(0.018)	(0.027)	(0.027)	(0.027)
Woman=1	0.053***	0.054***	0.053***						
	(0.013)	(0.013)	(0.013)						
Jutland=1	-0.030*	-0.031*	-0.031*	-0.027	-0.027	-0.027	-0.035	-0.038	-0.038
	(0.018)	(0.018)	(0.018)	(0.023)	(0.023)	(0.023)	(0.033)	(0.032)	(0.033)
Fyn and Bornholm=1	-0.051*	-0.051*	-0.050*	-0.049	-0.049	-0.049	-0.057	-0.061	-0.056
	(0.029)	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(0.059)	(0.057)	(0.060)
Copenhagen=1	-0.016	-0.015	-0.015	-0.018	-0.018	-0.018	-0.012	-0.009	-0.010
	(0.020)	(0.020)	(0.020)	(0.024)	(0.024)	(0.024)	(0.037)	(0.037)	(0.037)
Greater Copenhagen=1	-0.029*	-0.030*	-0.029*	-0.027	-0.028	-0.028	-0.036	-0.037	-0.035
	(0.015)	(0.015)	(0.015)	(0.019)	(0.019)	(0.019)	(0.025)	(0.024)	(0.024)
Frederiksborg=1	-0.011	-0.011	-0.012	-0.009	-0.009	-0.009	-0.015	-0.015	-0.018
	(0.017)	(0.017)	(0.017)	(0.019)	(0.019)	(0.019)	(0.032)	(0.032)	(0.032)
Observations	4340	4340	4340	2998	2998	2998	1342	1342	1342

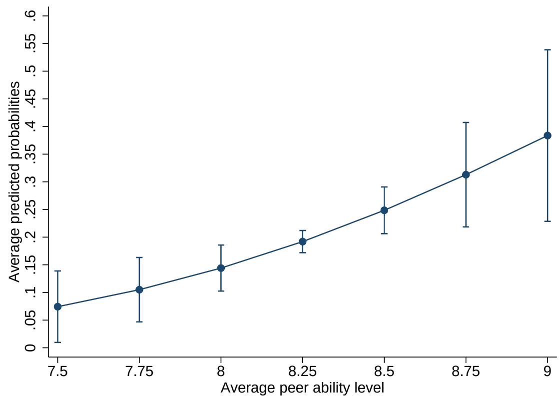
Note: Probit estimation. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. I include dummies in all regressions to control for educational length of the parents, cohort-fixed effects, and the average age and average age squared of the peer group, leave-out-mean. Zealand is the excluded location group. Standard errors are clustered by peer group and reported in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

The magnitude of the peer effect for women is in fact sizable. With a standard deviation of $\bar{A}_{-i}$ equal to 0.17 and an estimated average marginal effect from $\bar{A}_{-i}$ of 0.222, a one-standard-deviation increase in $\bar{A}_{-i}$ corresponds to an increase of approximately 4 percentage points in women's probability of dropping out. A 5-percent increase in Share of high-ability peers corresponds to an increase of approximately 2.5 percentage points in women's probability of dropping out. By comparison, a one-standard-deviation increase in high school GPA decreases women's probability of dropping out by approximately 4 percentage points. Thus, for women, the impact of peer quality is comparable to the impact of the student's own abilities (as measured by high school GPA). An estimated peer effect of 4 percentage points is comparable in size to the peer effects found in other studies of peer effects on discrete outcomes (e.g., Lyle, 2007; Ost, 2010).[13] Figure 4 shows the predicted probabilities with confidence intervals as dependent on the average level of peer quality separately for men and women.

Figure 4: Average Predicted Probabilities for Different Values of $\bar{A}_{-i}$



(a) Men



(b) Women

Table 2 also shows that high school GPA has a significant and negative effect on the probability of dropping out. This holds for both men and women and agrees with findings in the literature that academic aptitude is an importing determinant of the propensity to drop out of university (e.g., Smith and Naylor, 2001; Johnes and McNabb, 2004; Arulampalam et al., 2005). It is likely that individuals who do well in high school are more mature and can better handle the academic demands of university,

[13]Ost (2010) models the impact of peers and grades on major persistence in the life and physical sciences. He predicts peers' propensity for persisting through the major and includes this in his regression. He finds that a 10 percentage point increase in the propensity of one's peers to persist leads to a 2.08 percentage point increase in one's probability of persisting through a major in the physical sciences. Lyle (2007) models individual choices to enroll in different majors or continue in the army as dependent on peers and role models. He finds that a 10 percentage point increase in the fraction of role models who intend to study engineering corresponds to a 1.5 percentage point increase in the probability that a plebe will choose to major in engineering.

which could make them less prone to dropping out. I also observe that women are more likely to drop out than men, a result contrasting that of Johnes and McNabb (2004), who find that men are more likely to drop out.

The estimated positive peer effect on women's dropout probability seems puzzling, as one might expect women to benefit from high-level peers and be less likely to drop out. One potential explanation in the literature is the finding that women are less willing to enter competitions, and likely to perform worse in competitive environments, than men of equal abilities (e.g., Gneezy et al., 2003; Niederle and Vesterlund, 2007, 2010). If we assume that the level of competitiveness in a group corresponds to the group's ability level, then high-ability groups will have high levels of competitiveness, and this increased competition could be a reason for the positive relationship between peer quality and dropout rates among women.

Another potential explanation is offered by the psychology literature, the so-called big-fish-little-pond effect (BFLPE) introduced by Marsh and Parker (1984). This is the hypothesis that students compare their own academic abilities with those of their peers and use this comparison to form their academic self-concepts (see also Marsh and Hau, 2003); the name refers to the idea that one feels smarter as the brightest member of a small group (a big fish in a small pond) than as a member of a large group with many brighter peers. Individuals of average ability in the general population thus might view themselves as low-ability when they are put in high-ability peer groups. Applied to the results in Table 2, this could mean that women with high-ability peers tend to perceive themselves as having lower academic abilities than they actually do, which could lead them to exit the program more often than students with peers of lower quality. These suggested interpretations of the estimated peer effect are obviously only few out of many potential explanations.

6.2 Peer Effects Across the Ability Distribution

In order to learn which students respond the most to peer impacts, I estimate alternative specifications of the model described in the previous section. Specifically, I estimate peer effects across the ability distribution. Table 3 presents the results of estimations across two ability-dependent sub-samples. Columns (1) to (3) show the results of estimating on the sample of individuals in the upper part of the high school ability distribution, and columns (4) to (6) show the results for those in the lower part of the distribution. By estimating in this way, I allow low- and high-ability students to react differently to their peers.

My results show that low-ability students' probability of dropping out is significantly increasing in peer quality. In particular, $\bar{A}_{-i}$ and the *Share of high ability peers* enter the regression significantly. Table 3 shows that this effect is driven by women. My results also show that high-ability women are affected by the average ability levels of their peer groups. Both high- and low-ability men are unaffected. That low-ability students are the most responsive to their peers is consistent with previous findings (e.g., Carrell et al., 2009; Ost, 2010).

Table 3: Probability of Dropping Out - Probit Estimation

	High ability students			Low ability students		
	Al	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Share of high ability peers	0.060	-0.036	0.332	0.375***	0.238	0.765***
	(0.139)	(0.149)	(0.233)	(0.132)	(0.148)	(0.234)
Share of low ability peers	0.094	0.088	0.122	0.107	0.140	0.031
	(0.125)	(0.140)	(0.215)	(0.126)	(0.150)	(0.187)
Initial class male share	-0.378**	-0.319*	-0.496*	-0.042	0.076	-0.373
	(0.181)	(0.186)	(0.295)	(0.164)	(0.172)	(0.365)
Woman=1	0.064***			0.043**		
	(0.018)			(0.020)		
$\bar{A}_{-i}$	0.003	-0.068	0.185*	0.150**	0.098	0.286**
	(0.062)	(0.067)	(0.106)	(0.068)	(0.071)	(0.113)
Initial class male share	-0.392**	-0.324*	-0.560*	-0.076	0.062	-0.472
	(0.186)	(0.187)	(0.303)	(0.163)	(0.174)	(0.366)
Woman=1	0.064***			0.045**		
	(0.018)			(0.020)		
Observations	2061	1389	672	2279	1609	670

Note: Probit estimation. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. In all estimations I have included the same controls as in Table 2. High-ability students are defined as students with high school GPAs above the cohort median, and low-ability students as students with high school GPAs below or equal to the cohort median. Standard errors are clustered by peer group and reported in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

6.3 Heterogeneous Gender Effects

In this section, I examine gender-specific measures of peer quality to see which peers are the most influential. Table 4 shows the results of estimations made with gender-specific peer measures, where $\bar{A}^F_{-i}$ is the average ability level of i's female peers and $\bar{A}^M_{-i}$ is the average ability level of i's male peers.

Interestingly, my results show that women are adversely affected primarily by their female peers. Women's probability of dropping out is significantly increasing in the *Share of high ability female/male peers* and $\bar{A}^F_{-i}$, but there is no significant effect from $\bar{A}^M_{-i}$. As I mentioned, the literature has shown that women are less likely on average to enter competitions. However, it also shows that women are more willing to enter competitions facing other women as their main competitors (Gneezy et al., 2003; Niederle and Vesterlund, 2007, 2010). Thus one could imagine that women are more likely to compete with and compare themselves with other women than with men. This may be why the adverse impact on women comes from their female peers. These results are also consistent with my previous finding that the dropout probability for women is decreasing in the share of males in the peer group.

Table 4: Probability of Dropping Out - Probit Estimation

	All			Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share of high ability male peers	0.123		0.126*	0.077		0.078	0.263*		0.268*
	(0.075)		(0.075)	(0.080)		(0.080)	(0.152)		(0.151)
Share of low ability male peers	0.096		0.115	0.110		0.112	0.066		0.109
	(0.072)		(0.072)	(0.078)		(0.079)	(0.126)		(0.116)
Share of high ability female peers		0.076*	0.088**		0.002	0.016		0.242***	0.249***
		(0.042)	(0.043)		(0.051)	(0.050)		(0.072)	(0.072)
Share of low ability female peers		-0.035	-0.031		-0.047	-0.042		-0.020	-0.021
		(0.054)	(0.053)		(0.055)	(0.054)		(0.100)	(0.099)
$\bar{A}^M_{-i}$	0.014		0.016	-0.017		-0.016	0.090		0.102
	(0.039)		(0.038)	(0.041)		(0.041)	(0.066)		(0.064)
$\bar{A}^F_{-i}$		0.075***	0.075***		0.041	0.041		0.156***	0.160***
		(0.024)	(0.024)		(0.026)	(0.026)		(0.044)	(0.043)
Observations	4340	4340	4340	2998	2998	2998	1342	1342	1342

Note: Probit estimation. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. In all estimations I have included the same controls as in Table 2. Standard errors are clustered by peer group and reported in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

6.4 Peer Group Ability Rank

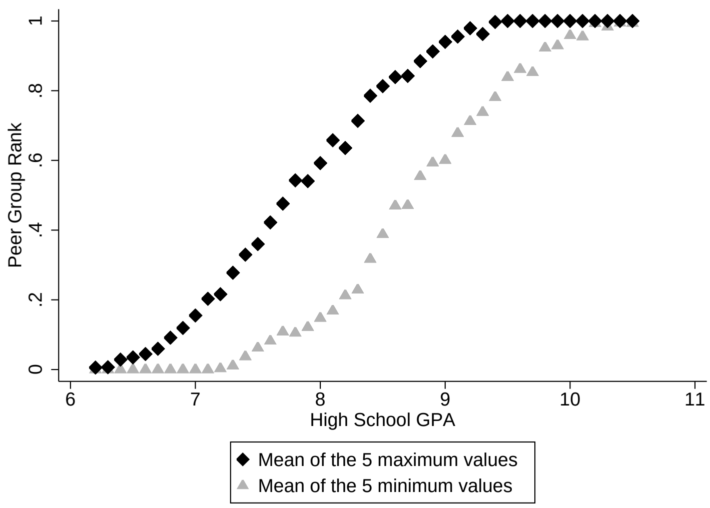
Following more recent literature that shows that relative rank is important for educational performance, job satisfaction, and the formation of a self-concept or self-image (e.g., Brown et al., 2008; Card et al., 2012; Murphy and Weinhardt, 2014; Elsner and Isphording, 2015), I investigate whether ranking within one's peer group is important for the decision to drop out. Peer group rank can affect students in several ways. Students of the same absolute ability levels (as measured by high school GPA) can be ranked differently in their own peer groups, and this might lead them to develop more or less self-confidence. If students form their self-concepts on the basis of their peer group ranks rather than their absolute ability levels, they are relying on incomplete information and risk forming misleading pictures of their own ability levels. A misleading picture of this sort could give a student the wrong expectations about the trade-off between the costs and benefits of education, which could affect the decision to drop out. If a student ranks low in his peer group, he might see himself as a low-ability student in absolute terms, even if he is not, and this could lead him to drop out of university. By contrast, a student with a high peer group rank might see himself as a high-ability student and gain a correspondingly higher probability of finishing university. Another view is that students are motivated by competition, and low peer group ranks encourage them to put more effort into their education and improve their ranks. Because students are not exposed to all the members of their cohorts the way they are to their peer groups, I would not expect them to create academic self-concepts based on their cohort ranks, but in this way peer group rank can have an impact on dropout probability.

To determine how important peer group rank is, I create a measure of it, R_i^P, based on high school GPA (see Equation (5)) and include this measure in the estimation equation. Because R_i^P is based on high school GPA, its inclusion in the estimation equations does not introduce a problem of reflection, or two-way causality. R_i^P is created as a comparable rank measure across peer groups. Specifically, I transform the rank position of each individual, as given by Equations (5). Doing it this way ensures that R_i^P is a measure of relative rank among one's closest peers but is also comparable across cohorts and peer groups.

$$R_i^P = \frac{n_{h,t}^i - 1}{N_{h,t} - 1} \tag{5}$$
$$R_i^P \in [0, 1]$$

$n_{h,t}^i$ is the high school GPA-rank of individual i in peer group h and enrollment year t and $N_{h,t}$ is the numbers of students in the peer group. The reason this approach works is that peer group rank varies among students with the same high school GPA because of the variations in ability level among peer groups. Students of the same absolute ability levels can be ranked differently in their peer groups. This is shown graphically in Figure 5.

Figure 5: Variation in Peer Group Rank Across High School GPA*



* Note: Information is pooled across all years. All points in the figures are calculated averages based on at least 5 observations. This is done in order to comply with the guidelines of Statistic Denmark.

To compare the influence of peer group rank with that of absolute ability level on dropping out, I estimate two models, excluding high school GPA from the first and including it in the second. Table 5 presents the results of the estimations in which R_i^P is included. When high school GPA is excluded from the regression, R_i^P significantly decreases the probability of dropping out across all sub-samples. This is unsurprising because R_i^P is correlated with individuals' abilities, which are not controlled for when high school GPA is excluded. The inclusion of high school GPA yields interesting results; see Columns (2), (4), and (6) of Table 5. Column (6) shows that women's probability of dropping out is also decreasing in R_i^P when high school GPA is controlled for. When both R_i^P and high school GPA are included, GPA becomes insignificant for women, but the opposite is true for men. Moreover, Column (4) shows that when GPA is controlled for, men are unaffected by R_i^P. These findings suggest that women are affected by peer group rank and that comparison to peers matters more than personal ability level. Again, the opposite seems to be the case for men. This is an important finding. One consequence of it is that women might under-invest in their human capital relative to the optimal situation given their absolute abilities. Given the results in the previous sections, this finding is not too surprising and supports my suggested interpretations.

Table 5: Probability of Dropping Out - Probit Estimation

	All		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Ordinal rank in peer group	-0.186***	-0.049	-0.195***	0.052	-0.153***	-0.315**
	(0.021)	(0.071)	(0.025)	(0.078)	(0.037)	(0.124)
Initial class male share	-0.223*	-0.216*	-0.113	-0.124	-0.459**	-0.463**
	(0.125)	(0.121)	(0.135)	(0.127)	(0.223)	(0.218)
GPA^{HS}		-0.054**		-0.097***		0.066
		(0.026)		(0.029)		(0.046)
Woman=1	0.053***	0.054***				
	(0.013)	(0.013)				
Observations	4340	4340	2998	2998	1342	1342

Note: Probit estimation. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. In all estimations I have included the same controls as in Table 2. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

My results correspond to those of Elsner and Isphording (2015), who find that when two students with the same ability level but different ranks in their high school cohorts are compared, the one who ranks higher is significantly more likely to finish high school, attend college, and complete a 4-year degree. In addition, Arulampalam et al. (2005) find a significant effect of students' in-class ranks. They divided students into three ranking groups (high, middle, low) and included dummies corresponding to these in their regressions. They find that for male students, being ranked higher (or lower) is associated with an approximately one-percentage-point lower (higher) probability of dropping out. Among women, they also observe a significant and positive effect of about one percentage point from being ranked low in the class.

Unlike the measures of peer quality, the measure of peer group rank, R_i^P, is not completely exogenous in the model. Because R_i^P is computed from pre-university educational performance, its inclusion does not introduce the reflection problem. On the other hand, students' unobserved abilities are likely to affect the decision to drop out and are also likely to be correlated with peer group rank. The inclusion of high school GPA should account for a large part of these unobserved abilities. Nonetheless, the positive correlation between R_i^P and unobserved ability levels, and the expected negative influence of unobserved ability levels on y_{iht}, might cause the estimated effect from R_i^P to be down-biased.

7 Results: Educational Performance

7.1 Baseline Results

Table 6 presents the baseline results of estimations of a peer effect on educational performance. Interestingly, I find no significant peer effect in any of the regressions. This is in contrast to other studies that have found a significant peer effect on educational performance (e.g., Sacerdote, 2001; Carrell et al., 2009; Ammermueller and Pischke, 2009; Vardardottir, 2013). However, many of these studies looked at different educational levels, estimated the impact of smaller units, such as roommates, or found a peer effect only in certain sub-samples. Furthermore, Carrell et al. (2013) show that their expected peer effect disappeared after they designed what they thought would be optimal peer groups. Thus, the existence of a peer effect on educational performance remains uncertain.

The share of males in a peer group does not have an impact on academic performance. This result contrasts with that of Lavy and Schlosser (2011), who show that the proportion of girls in the classroom has a significant and positive impact. They find that a higher share of girls improves academic performance by way of, among other things, lower levels of classroom disruption. However, they use data on Israeli primary, middle, and high schools students to estimate on a sample of younger students than those looked at in this paper. Moreover, the students in our sample have completed high school and have continued by choice into tertiary education, and can be expected to make fewer interruptions in class.

Table 6: Educational Performance - OLS Estimation

	All			Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peer effects:									
Share of high ability peers	-0.000			-0.040			0.222		
	(0.302)			(0.308)			(0.468)		
Share of low ability peers	-0.436			-0.291			-0.801		
	(0.318)			(0.355)			(0.499)		
$\bar{A}_{-i}$		0.159			0.085			0.377	
		(0.146)			(0.149)			(0.249)	
Std in peer ability			-0.116			-0.105			-0.069
			(0.215)			(0.229)			(0.343)
Initial class male share	-0.134	-0.099	-0.093	-0.156	-0.133	-0.134	-0.045	0.013	0.072
	(0.448)	(0.449)	(0.448)	(0.465)	(0.464)	(0.464)	(0.624)	(0.623)	(0.626)
Initial class size	0.004	0.004	0.003	0.001	0.002	0.001	0.011	0.011	0.010
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.011)	(0.011)	(0.011)
Personal characteristics:									
GPA^{HS}	0.669***	0.670***	0.669***	0.677***	0.677***	0.677***	0.644***	0.646***	0.644***
	(0.019)	(0.019)	(0.019)	(0.022)	(0.022)	(0.022)	(0.032)	(0.032)	(0.032)
Starting age	0.058***	0.058***	0.058***	0.054***	0.054***	0.054***	0.062***	0.062***	0.061***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)	(0.020)	(0.020)	(0.019)
General high school=1	0.257***	0.255***	0.256***	0.225***	0.225***	0.226***	0.349***	0.342***	0.346***
	(0.030)	(0.030)	(0.030)	(0.035)	(0.035)	(0.035)	(0.062)	(0.061)	(0.061)
Woman=1	-0.052	-0.052	-0.053						
	(0.032)	(0.032)	(0.032)						
Jutland=1	0.198***	0.201***	0.198***	0.282***	0.284***	0.282***	0.043	0.044	0.042
	(0.056)	(0.056)	(0.056)	(0.073)	(0.073)	(0.073)	(0.095)	(0.096)	(0.096)
Fyn and Bornholm=1	0.242***	0.243***	0.243***	0.256***	0.256***	0.257***	0.258*	0.260*	0.254*
	(0.073)	(0.073)	(0.073)	(0.079)	(0.079)	(0.079)	(0.133)	(0.134)	(0.134)
Copenhagen=1	-0.107**	-0.105**	-0.107**	-0.017	-0.015	-0.017	-0.309***	-0.310***	-0.316***
	(0.051)	(0.051)	(0.051)	(0.063)	(0.063)	(0.062)	(0.080)	(0.080)	(0.081)
Greater Copenhagen=1	0.016	0.017	0.016	0.065	0.066	0.064	-0.079	-0.076	-0.074
	(0.040)	(0.040)	(0.040)	(0.043)	(0.043)	(0.044)	(0.078)	(0.079)	(0.079)
Frederiksborg=1	0.090**	0.091**	0.088**	0.132***	0.132***	0.130***	-0.011	-0.011	-0.017
	(0.041)	(0.041)	(0.041)	(0.045)	(0.045)	(0.045)	(0.073)	(0.074)	(0.074)
Adj. R-squared	0.322	0.322	0.321	0.330	0.330	0.330	0.302	0.301	0.299
Observations	3562	3562	3562	2493	2493	2493	1069	1069	1069

Note: OLS estimation. The dependent variable is first-year GPA. I have included dummies in all regressions to control for educational length of the parents, cohort-fixed effects, and the average age and average age squared of the peer group, leave-out-mean. Zealand is the excluded location group. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Table 6 also shows that high school GPA enters the model with a positive and significant coefficient. Because high school GPA is a proxy for pre-university ability level, it is not surprising that it translates into high performance in university. The results also show that students who lived in Frederiksborg (part of Zealand), Jutland, Fyn, or Bornholm five years before enrolling at CBS perform, on average, better than students who lived in the part of Zealand not controlled for. As CBS is located in Zealand, it might be that students coming from Jutland and Fyn chose CBS for a specific reason, and maybe for a specific master's program afterward, which could result in their being more focused and performing better.

7.2 Peer Effects Across the Ability Distribution

Table 7 shows the results of estimating a peer effect in educational performance for students in the upper and lower halves of the ability distribution. My results show a positive effect of average peer ability level on low-ability women. The former is in accordance with the results of Carrell et al. (2009). Thus women who do not drop out during the first year see a positive effect from their peers. However, an increase of one standard deviation of $\bar{A}_{-i}$ results in an improvement in first-year GPA of only 0.12 (0.16 * 0.705). The standard deviation of first-year GPA is 0.99, so this corresponds to only around one-tenth of a standard deviation. This effect is small compared to the results of Vardardottir (2013), who finds that a one-standard-deviation increase in academic ability of peers correspond to approximately a 0.85 and 0.58 standard deviation increase in the spring exam results and year-end grades of Icelandic high school students. Finally, Table 7 shows no significant peer effect from the share of high-ability peers and a 10 percent significant negative effect from the share of low-ability peers. Overall, I do not see very strong peer effects on educational performance.

Table 7: Educational Performance - OLS Estimation

	High ability students			Low ability students		
	Al	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Share of high ability peers	-0.370	-0.402	-0.068	0.373	0.343	0.321
	(0.371)	(0.392)	(0.657)	(0.400)	(0.397)	(0.679)
Share of low ability peers	-0.627*	-0.649	-0.689	-0.278	0.042	-1.106*
	(0.343)	(0.408)	(0.610)	(0.456)	(0.481)	(0.645)
Initial class male share	0.178	-0.113	0.829	-0.397	-0.026	-1.230
	(0.523)	(0.551)	(0.834)	(0.562)	(0.579)	(0.846)
Woman=1	-0.101**			0.013		
	(0.040)			(0.047)		
$\bar{A}_{-i}$	0.085	0.113	0.136	0.257	0.040	0.705**
	(0.142)	(0.163)	(0.319)	(0.219)	(0.225)	(0.332)
Initial class male share	0.215	-0.097	0.908	-0.392	-0.055	-1.188
	(0.526)	(0.552)	(0.835)	(0.557)	(0.572)	(0.825)
Woman=1	-0.100**			0.012		
	(0.041)			(0.047)		
Observations	1795	1231	564	1767	1262	505

Note: OLS estimation. The dependent variable is first-year GPA. In all estimations I have included the same controls as in Table 6. High ability students are defined by students with a high school GPA above the cohort median and low ability students are defined as students with a high school GPA below or equal to the cohort median. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Taking together my results from Sections 6 and 7, I observe that the educational performance of women in the lower half of the ability distribution is positively affected by the average ability level of the peer groups and that women's probability of dropping out is increasing in the ability level of the peer group. It is interesting to note that low-ability women who do not drop out during the first year are in fact helped by their higher-ability peers. This underlines the need to understand and address the drop-out decisions of women.

8 Robustness

I have done a battery of robustness checks to validate my results. To ensure that the estimated effect is driven by peers, I create placebo measures of peer quality and estimate the models, including these measures as explanatory variables. I also test the robustness of my results to the exclusion of potential outliers, different model specifications, and alternative measures of peer quality. Finally, I discuss a potential problem of sample selection.

8.1 Placebo Estimation

In order to verify that the estimated effect is in fact a peer effect, I perform (pseudo) placebo estimations. When conducting these I take two approaches. First, I assign students into placebo peer groups on the basis of random draws from the uniform distribution. Relying on these peer groups, I create a placebo measure of peer quality, $\bar{A}_i^P$, computed as $\bar{A}_i$ but with artificial peers. This measure is labeled $\bar{A}_i^P$. Table B.3 and Table B.4 present the results of 15 placebo estimations each, including the first placebo peer quality measure.

Second, I randomly match all students and peer quality measures $\bar{A}_i$ based on random draws from the uniform distribution. Table B.5 shows the results of 15 estimations, including the second placebo peer quality measure. However, with this approach I risk matching a student with a peer quality measure from his or her original peer group. Because the peer quality measure from the original group is correlated with student i's outcome by construction (unless student i is matched exactly with his or her own peer quality measure), I drop all students who are matched with a peer quality measure from their original peer groups. This is why the results in Table B.5 are based on different samples.

As expected, the placebo measure of peer quality is insignificant in almost all the regressions and my placebo results suggest that on average the placebo peer quality measure is insignificant, which indicates that my results are in fact capturing a true peer effect. However, few times the placebo peer measure is significant in predicting both dropout and performance. Having the points of Angrist (2014) in mind, such results are not ensuring of an actual peer effect in my main results. However, both of the above methods of constructing the placebo peer quality measure have flaws and should perhaps be considered pseudo-placebo estimations. For further work on this paper, I should consider other methods of constructing a placebo peer quality measure. These methods could include simulations of the distribution of the peer quality measure and subsequent random draws from this simulated distribution. Also methods of averaging over coefficients from repeated placebo estimations and bootstrapping the standard errors is a potential way forward

8.2 Re-estimation and Alternative Measure of Peer Quality

In order to test whether my results are sensitive to the measure of peer quality, I re-estimate the model using an alternative measure. This measure is the 75$^{\text{th}}$ percentile of the high school GPA distribution of the individual's peer group, given by $q_{-i}^{75} = Q_{75}(GPA_{-ih_t}^{HS})$. Table B.6 shows the results of estimations with this measure included. The overall picture is the same as in Tables 2 and 6: only women are affected by their peers, and women's dropout probability increases with peer quality. As in the main estimations, I find no significant peer effects on performance.

To test the sensitivity of the results to model specifications, and to investigate whether neglected heterogeneity is a problem in the probit estimations, I estimate the dropout equation using a Linear Probability Model (LPM). Table B.7 shows the results. The estimated coefficients of the LPM are almost identical to the average marginal effects obtained from the main probit estimations. This indicates that my results do not suffer from problems of neglected heterogeneity or from sensitivity to model specifications.

To test whether my results are driven by an inaccurate definition of "dropping out", I re-estimate the models on a sub-sample that excludes students who dropped out because they transferred to other business programs. If students moved to programs that were otherwise comparable but had more prestige or, in general, higher-ability students, it could explain why the probability of dropping out increases with peer quality. The results of the estimations on this sub-sample are shown in Table B.8. Once again, the results are similar to the main results.

To determine whether my results are driven by older students, who might have dropped out of university before and thus be more likely to do so again, I exclude the peer groups with average ages above 22.5. Estimating on just the "young" peer groups does not change the results (not reported).

Because the exclusion of non-Danes might have affected the results, I re-estimate both the dropout and performance equations on the full sample. Tables B.9 and B.10 show the results; no significant changes are observed.

Finally, to ensure that my results on educational performance do not depend on outliers in performance, I re-estimate the model on a sub-sample from which students with first-year GPAs above or below the 95 and 5 percentiles have been excluded. This too does not change the results for educational performance (not reported).

8.3 Sample Selection and Heckman Approach

One real concern regarding my results on peer effects in educational performance is the possibility of sample-selection bias. If selection into the sample (the choice of not dropping out) is caused by unobserved individual characteristics that are also correlated with educational performance, my results will be biased. Even variables that a priori are considered exogenous, such as the peer quality measure, might turn out to be correlated with the error term in the selected sample. Formally, if the error term in the selection equation is correlated with the error term in the performance equation, the OLS estimates will suffer from an omitted variable bias. Obviously the problem of sample selection is an issue only in the performance estimations.

In order to determine whether my results change when sample selection is corrected for, I run a Heckman sample selection (HSS) model. This works by modeling the selection into the sample by a probit selection equation and modeling the main (performance) equation by OLS while correcting for selection into the sample. The HSS model can be estimated either by maximum likelihood or by a two-step procedure. The two-step procedure requires an exclusion restriction in the first stage, which makes it difficult to apply. Because I do not have a valid exclusion restriction, I perform only a maximum likelihood estimation of the Heckman model.[14] A disadvantage of the maximum likelihood procedure is that it demands strong assumptions of the simultaneous distribution of the errors in the selection and performance equations. Despite this, the results may serve for the purposes of comparison. The results of the maximum likelihood estimation are presented in Table B.11. Overall, the results are the same as those reported in Table 6, and I observe no significant peer effects. Table B.12 shows the results of estimations from an HSS model across the ability distribution. The results are not qualitatively different from those in Table 7 and show in fact a larger and stronger peer effect for low-ability women. In addition they show a stronger negative effect from the *Share of low ability peers* on women's first-year GPA.

9 Conclusion

In this paper I estimate the relationship between peer quality and the propensity to drop out during the first year of a bachelor's program. I also consider peer effects on educational performance. The relationship between educational performance and peer quality has been covered extensively in the literature, though with ambiguous results. By contrast, the relationship between peer quality and the

[14] I have experimented with location prior to enrollment and with different measures of performance of siblings at CBS as potential exclusion restrictions. None of them was applicable.

decision to drop out is less well-documented. I also investigate how the peer group rank is associated with the probability of dropping out, which is not addressed in other papers in this area.

Using data on students who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004 allows me to identify the impact of peer quality on educational outcomes. Students in this program were randomly assigned to smaller groups when they enrolled, and I use these groups as my peer groups. Because of the random assignment, my estimates do not suffer from self-selection bias. Moreover, by using data on pre-university performance, I can create exogenous measures of peer group quality that are not contaminated by two-way causality.

Regarding probability of dropping out during the first year, I observe that women are adversely impacted by the ability levels of their peers: women in peer groups with high ability levels are more likely to drop out. One interpretation of this result is that women tend to avoid competition and thus to leave groups with high ability levels and corresponding high levels of competition. Another is the BFLPE hypothesis found in the psychology literature. This is the suggestion that students compare their own academic abilities with those of their peers and use these comparisons to form their academic self-concepts. A consequence of this would be that students are more likely to form negative pictures of their abilities if they are in peer groups with high-ability students. This diminished self-concept may lead them to drop out.

When I estimate the impact of peer group rank on the probability of dropping out during the first year, I find that women's peer group ranks have a significant effect on their probability of dropping out. Being ranked highly in a peer group reduces women's probability of dropping out, whereas it does not affect men's dropout probability. Moreover, high school GPA becomes insignificant for women when my measure of peer group rank is included, whereas the opposite is true for men. This suggests that women are guided by their relative ability levels and men are guided by their absolute ability levels. These results are well in line my other findings and also support the BFLPE interpretation. The interpretations of my results, however, are obviously only suggestions from among a number of possible explanations.

First and foremost, this paper adds to our knowledge of the nexus between peer group quality and the propensity to dropping out of university. Overall, my results raise the question of whether universities, in order to improve performance and reduce dropout rates, should focus more on group formation and social interaction among students. My results also open the way for a discussion of how to handle the fact that women's probability of dropping out increases with the academic quality of their peers. In particular, my results suggest that greater awareness of women's academic potential might reduce dropouts. However, more information is needed before we can fully understand the implications and drivers of these results. The results presented in this paper should be treated as informative and as an encouragement to further investigation of why students drop out. In particular, the interpretations offered in this paper should be investigated in depth by means of, for instance, survey analysis and experimental approaches.

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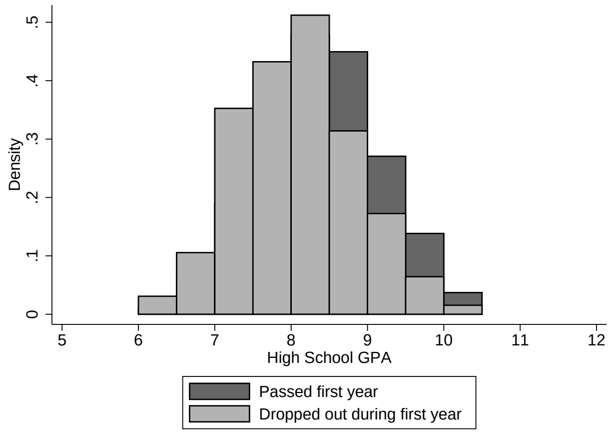
Appendix A Figures and Summary Statistics

Figure A.1: Histogram of High School GPA Across First Year Pass and Fail Students - Sample Pooled over Enrollment Years



Note: In order to comply with the discretion rules of Statistic Denmark, I have excluded observations with high school GPA above 10.5.

Table A.1: Summary Statistic Across Enrollment Years

	1996	1997	1998	1999	2000	2001	2002	2003	2004
First year dropout	0.24	0.20	0.15	0.12	0.15	0.18	0.19	0.20	0.19
	(0.43)	(0.40)	(0.36)	(0.32)	(0.36)	(0.38)	(0.39)	(0.40)	(0.39)
$\bar{A}_{-i}$	8.20	8.27	8.32	8.30	8.33	8.30	8.34	8.26	8.40
	(0.17)	(0.16)	(0.11)	(0.10)	(0.20)	(0.11)	(0.16)	(0.20)	(0.14)
Initial class size	28.10	31.72	33.99	33.66	37.53	39.14	38.42	38.53	37.92
	(2.55)	(2.03)	(2.19)	(2.74)	(2.21)	(3.00)	(2.83)	(2.52)	(2.96)
GPA^{HS}	8.18	8.27	8.31	8.30	8.31	8.28	8.33	8.26	8.39
	(0.79)	(0.84)	(0.85)	(0.77)	(0.80)	(0.76)	(0.80)	(0.81)	(0.77)
Woman	0.28	0.27	0.32	0.33	0.31	0.35	0.35	0.30	0.28
	(0.45)	(0.45)	(0.47)	(0.47)	(0.46)	(0.48)	(0.48)	(0.46)	(0.45)
N	486	424	455	458	497	511	508	502	499

Note: The sample includes Danes that enrolled in bachelor's program in business economics at CBS between 1996 and 2004. Means are reported, standard deviation in parenthesis

Appendix B Additional Estimations

Table B.1: Regressing $\bar{A}_{-i}$ on Controls - OLS Estimation
Identifying Assumption - Random Assignment

	All	Men	Women
	(1)	(2)	(3)
GPA^{HS}	-0.001	0.002	-0.010
	(0.004)	(0.004)	(0.007)
Starting age	-0.018***	-0.018***	-0.020***
	(0.004)	(0.004)	(0.004)
Woman=1	-0.002		
	(0.003)		
General high school=1	0.000	-0.000	0.005
Mother's education:			
	(0.006)	(0.007)	(0.012)
Mandatory edu.	-0.012	-0.008	-0.019
	(0.013)	(0.015)	(0.023)
General High School	-0.014	-0.010	-0.024
	(0.014)	(0.019)	(0.024)
Business High School	-0.006	0.011	-0.048
	(0.028)	(0.037)	(0.037)
Professional Qualifications	-0.022*	-0.022	-0.023
	(0.013)	(0.015)	(0.023)
Short Tertiary	-0.012	-0.006	-0.019
	(0.017)	(0.019)	(0.026)
Medium tertiary	-0.010	-0.012	-0.002
	(0.013)	(0.015)	(0.022)
Bachelor	-0.070**	-0.062	-0.072
	(0.031)	(0.045)	(0.046)
Master's or above	-0.019	-0.022	-0.010
	(0.015)	(0.020)	(0.024)
Father's education:			
Mandatory edu.	0.014	0.003	0.037**
	(0.011)	(0.013)	(0.017)
General High School	0.017	0.003	0.045*
	(0.011)	(0.013)	(0.024)
Business High School	0.012	0.002	0.023
	(0.019)	(0.025)	(0.024)
Professional Qualifications	0.007	0.003	0.018
	(0.008)	(0.010)	(0.015)
Short Tertiary	-0.005	0.001	-0.022
	(0.014)	(0.017)	(0.021)
Medium tertiary	0.006	-0.003	0.023
	(0.010)	(0.011)	(0.016)
Bachelor	0.001	-0.010	0.026
	(0.016)	(0.021)	(0.032)
Master's or above	0.011	0.007	0.017
	(0.011)	(0.012)	(0.017)
Location:			
Jutland=1	-0.014	-0.020	-0.005
	(0.011)	(0.013)	(0.019)
Fyn and Bornholm=1	0.008	0.008	0.001
	(0.015)	(0.018)	(0.025)
Copenhagen=1	-0.002	-0.001	-0.007
	(0.008)	(0.010)	(0.015)
Greater Copenhagen=1	-0.004	-0.014*	0.015
	(0.007)	(0.008)	(0.013)
Frederiksborg=1	-0.012	-0.011	-0.018
	(0.007)	(0.009)	(0.014)
Adj. R-squared	0.144	0.148	0.132
p_1	0.809	0.990	0.103
p_2	0.452	0.520	0.378
No. obs.	4340	2998	1342

Note: Dependent variable is average peer ability level, $\bar{A}_{-i}$. In all the regressions I have included cohort-fixed effects. Parental reference groups is the ones with missing information. p_1 and p_2 are the p-values from the test of joint significance of the father's and mother's educational characteristics, respectively. Reference group for parental education is the group with missing values. The excluded location group is the rest of Zealand. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

B.1 Estimations With and Without Controls

Table B.2: Probability of Dropping Out - Probit Estimation
Estimations With and Without Controls

	(1)	(2)	(3)	(4)	(5)	(6)
Full sample:						
Share of high ability peers	0.215**	0.217**				
	(0.092)	(0.092)				
Share of low ability peers	0.105	0.101				
	(0.094)	(0.095)				
$\bar{A}_{-i}$			0.088*	0.076		
			(0.048)	(0.048)		
SD_{-i}					0.062	0.050
					(0.066)	(0.070)
Initial class male share	-0.116	-0.193	-0.138	-0.218*	-0.133	-0.213*
	(0.117)	(0.118)	(0.125)	(0.122)	(0.119)	(0.120)
Initial class size	0.004*	0.004*	0.004*	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Controls included:	No	Yes	No	Yes	No	Yes
Observations	4340	4340	4340	4340	4340	4340
Only men:						
Share of high ability peers	0.091	0.106				
	(0.094)	(0.097)				
Share of low ability peers	0.111	0.105				
	(0.106)	(0.106)				
$\bar{A}_{-i}$			0.031	0.019		
			(0.054)	(0.051)		
SD_{-i}					0.055	0.050
					(0.069)	(0.072)
Initial class male share	-0.088	-0.111	-0.099	-0.123	-0.098	-0.121
	(0.130)	(0.124)	(0.136)	(0.128)	(0.132)	(0.126)
Initial class size	0.001	0.001	0.001	0.001	0.001	0.001
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Controls included:	No	Yes	No	Yes	No	Yes
Observations	2998	2998	2998	2998	2998	2998
Only Women:						
Share of high ability peers	0.507***	0.517***				
	(0.164)	(0.169)				
Share of low ability peers	0.071	0.086				
	(0.150)	(0.157)				
$\bar{A}_{-i}$			0.226***	0.222***		
			(0.078)	(0.082)		
SD_{-i}					0.092	0.069
					(0.106)	(0.115)
Initial class male share	-0.441**	-0.384*	-0.517**	-0.465**	-0.481**	-0.433**
	(0.209)	(0.213)	(0.215)	(0.217)	(0.217)	(0.221)
Initial class size	0.011***	0.011***	0.010***	0.010***	0.010**	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Controls included:	No	Yes	No	Yes	No	Yes
Observations	1342	1342	1342	1342	1342	1342

Note: Probit estimation. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. In columns (2), (4), and (6) I have included the same controls as in Table 2. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

B.2 Robustness - Additional Estimations

Table B.3: Robustness: Placebo Estimation

	Probit - Drop out equation			OLS - Performance equation		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
$\bar{A}^P_{-i}$	-0.086	0.037	-0.293	-0.096	-0.121	-0.013
	(0.167)	(0.204)	(0.297)	(0.101)	(0.122)	(0.182)
$\bar{A}^P_{-i}$	-0.069	0.088	-0.302	0.037	0.100	-0.077
	(0.165)	(0.200)	(0.292)	(0.099)	(0.119)	(0.175)
$\bar{A}^P_{-i}$	-0.382**	-0.528***	-0.126	-0.238**	-0.201	-0.308*
	(0.162)	(0.203)	(0.274)	(0.100)	(0.123)	(0.176)
$\bar{A}^P_{-i}$	0.032	0.196	-0.225	-0.100	-0.120	-0.015
	(0.158)	(0.201)	(0.255)	(0.094)	(0.115)	(0.165)
$\bar{A}^P_{-i}$	-0.314*	-0.283	-0.447	-0.030	0.057	-0.200
	(0.171)	(0.210)	(0.305)	(0.101)	(0.120)	(0.185)
$\bar{A}^P_{-i}$	-0.136	0.058	-0.530*	-0.099	-0.010	-0.251
	(0.170)	(0.211)	(0.296)	(0.102)	(0.122)	(0.190)
$\bar{A}^P_{-i}$	-0.316*	-0.097	-0.792**	-0.031	0.097	-0.380*
	(0.184)	(0.224)	(0.329)	(0.115)	(0.137)	(0.216)
$\bar{A}^P_{-i}$	0.131	0.208	-0.034	-0.133	-0.128	-0.115
	(0.181)	(0.219)	(0.317)	(0.112)	(0.134)	(0.206)
$\bar{A}^P_{-i}$	-0.068	0.009	-0.230	-0.041	-0.087	0.083
	(0.176)	(0.213)	(0.315)	(0.107)	(0.130)	(0.186)
$\bar{A}^P_{-i}$	-0.094	-0.176	0.098	-0.075	-0.117	0.020
	(0.177)	(0.214)	(0.318)	(0.110)	(0.131)	(0.210)
$\bar{A}^P_{-i}$	0.042	0.253	-0.336	-0.099	-0.199	0.130
	(0.171)	(0.212)	(0.297)	(0.106)	(0.127)	(0.192)
$\bar{A}^P_{-i}$	0.047	0.146	-0.059	-0.141	-0.045	-0.391**
	(0.165)	(0.201)	(0.296)	(0.099)	(0.118)	(0.185)
$\bar{A}^P_{-i}$	0.275	0.392*	0.113	-0.030	-0.038	-0.034
	(0.183)	(0.229)	(0.311)	(0.110)	(0.136)	(0.190)
$\bar{A}^P_{-i}$	-0.124	-0.110	-0.148	-0.036	-0.009	-0.106
	(0.153)	(0.186)	(0.275)	(0.092)	(0.110)	(0.167)
$\bar{A}^P_{-i}$	-0.189	-0.199	-0.108	0.091	0.015	0.309*
	(0.155)	(0.190)	(0.268)	(0.094)	(0.113)	(0.170)
	4340	2998	1342	3562	2493	1069

Note: Probit and OLS estimation. In columns (1)-(3) the dependent variable is a dummy that is equal to 1 if the student dropped out during the first year and in columns (4)-(6) the dependent variable is first-year GPA. Main coefficients and not AME are reported in columns (1)-(3). In all estimations I have included the same controls as in Table 2. Robust standard errors are reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004. My artificial peer groups consist of 35 students.

Table B.4: Robustness: Placebo Estimation

	Probit - Drop out equation			OLS - Performance equation		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
$\bar{A}^P_{-i}$	-0.224	-0.380*	0.077	0.030	0.046	-0.039
	(0.163)	(0.202)	(0.278)	(0.099)	(0.120)	(0.176)
$\bar{A}^P_{-i}$	0.206	0.233	0.076	-0.019	-0.030	0.016
	(0.154)	(0.188)	(0.276)	(0.090)	(0.106)	(0.173)
$\bar{A}^P_{-i}$	-0.229	-0.282	-0.039	0.017	0.044	-0.079
	(0.177)	(0.219)	(0.301)	(0.106)	(0.130)	(0.188)
$\bar{A}^P_{-i}$	0.074	0.153	-0.095	-0.104	-0.022	-0.306*
	(0.145)	(0.177)	(0.253)	(0.085)	(0.101)	(0.159)
$\bar{A}^P_{-i}$	-0.151	-0.050	-0.337	0.047	0.041	0.057
	(0.157)	(0.192)	(0.279)	(0.093)	(0.114)	(0.164)
$\bar{A}^P_{-i}$	0.030	-0.188	0.488*	-0.030	-0.106	0.173
	(0.165)	(0.204)	(0.288)	(0.100)	(0.120)	(0.185)
$\bar{A}^P_{-i}$	0.094	0.139	-0.077	0.126	0.104	0.227
	(0.163)	(0.199)	(0.289)	(0.099)	(0.119)	(0.182)
$\bar{A}^P_{-i}$	0.166	0.230	0.014	-0.047	-0.082	-0.000
	(0.168)	(0.208)	(0.290)	(0.101)	(0.124)	(0.178)
$\bar{A}^P_{-i}$	-0.062	-0.056	-0.149	-0.130	-0.199*	0.018
	(0.139)	(0.173)	(0.240)	(0.084)	(0.102)	(0.152)
$\bar{A}^P_{-i}$	-0.170	-0.007	-0.523	-0.082	-0.129	-0.003
	(0.175)	(0.210)	(0.318)	(0.106)	(0.125)	(0.203)
$\bar{A}^P_{-i}$	0.154	0.167	0.162	-0.101	-0.168	0.069
	(0.175)	(0.211)	(0.309)	(0.109)	(0.134)	(0.187)
$\bar{A}^P_{-i}$	-0.030	0.080	-0.217	0.058	0.067	-0.012
	(0.175)	(0.213)	(0.314)	(0.106)	(0.127)	(0.194)
$\bar{A}^P_{-i}$	0.496***	0.474**	0.554*	0.032	0.005	0.168
	(0.182)	(0.224)	(0.323)	(0.112)	(0.134)	(0.213)
$\bar{A}^P_{-i}$	0.273*	0.423**	0.025	-0.046	0.065	-0.352**
	(0.151)	(0.185)	(0.265)	(0.088)	(0.106)	(0.160)
$\bar{A}^P_{-i}$	0.006	-0.018	-0.003	0.179*	0.158	0.231
	(0.178)	(0.220)	(0.306)	(0.105)	(0.127)	(0.191)
	4340	2998	1342	3562	2493	1069

Note: Probit and OLS estimation. In columns (1)-(3) the dependent variable is a dummy that is equal to 1 if the student dropped out during the first year and in columns (4)-(6) the dependent variable is first-year GPA. Main coefficients and not AME are reported in columns (1)-(3). In all estimations I have included the same controls as in Table 2. Robust standard errors are reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004. My artificial peer groups consist of 35 students.

Table B.5: Robustness: Placebo Estimation

	Probit - Drop out equation			OLS - Performance equation		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
$\bar{A}^P_{-i}$	-0.192	-0.170	-0.281	-0.032	0.027	-0.161
	(0.148)	(0.184)	(0.261)	(0.086)	(0.104)	(0.154)
Observations	3703	2520	1153	3068	2136	932
$\bar{A}^P_{-i}$	-0.103	-0.229	0.034	-0.026	-0.010	-0.113
	(0.151)	(0.187)	(0.262)	(0.091)	(0.110)	(0.163)
Observations	3701	2521	1150	3065	2136	929
$\bar{A}^P_{-i}$	0.184	0.124	0.308	-0.026	-0.109	0.138
	(0.149)	(0.190)	(0.245)	(0.088)	(0.106)	(0.160)
Observations	3698	2517	1150	3066	2136	930
$\bar{A}^P_{-i}$	-0.013	-0.101	0.179	-0.087	-0.180	0.112
	(0.152)	(0.190)	(0.259)	(0.090)	(0.110)	(0.164)
Observations	3697	2513	1153	3062	2129	933
$\bar{A}^P_{-i}$	0.002	-0.147	0.276	-0.060	-0.189*	0.237
	(0.153)	(0.193)	(0.259)	(0.089)	(0.107)	(0.155)
Observations	3697	2518	1150	3068	2138	930
$\bar{A}^P_{-i}$	0.096	-0.047	0.448*	-0.155*	-0.142	-0.184
	(0.146)	(0.180)	(0.255)	(0.085)	(0.103)	(0.153)
Observations	3691	2512	1148	3060	2132	928
$\bar{A}^P_{-i}$	0.154	0.135	0.163	0.030	-0.053	0.218
	(0.156)	(0.193)	(0.266)	(0.090)	(0.109)	(0.164)
Observations	3692	2517	1144	3062	2134	928
$\bar{A}^P_{-i}$	0.183	0.374**	-0.175	-0.046	-0.047	-0.021
	(0.149)	(0.186)	(0.252)	(0.086)	(0.104)	(0.161)
Observations	3703	2523	1149	3067	2138	929
$\bar{A}^P_{-i}$	-0.273*	-0.227	-0.390	0.049	-0.010	0.143
	(0.150)	(0.189)	(0.251)	(0.086)	(0.105)	(0.149)
Observations	3700	2519	1150	3066	2134	932
$\bar{A}^P_{-i}$	-0.013	-0.153	0.344	0.072	0.034	0.170
	(0.154)	(0.189)	(0.264)	(0.086)	(0.101)	(0.162)
Observations	3705	2526	1148	3071	2141	930
$\bar{A}^P_{-i}$	0.186	0.240	0.165	0.074	0.067	0.118
	(0.152)	(0.184)	(0.278)	(0.087)	(0.102)	(0.166)
Observations	3692	2516	1145	3059	2132	927
$\bar{A}^P_{-i}$	0.103	0.131	0.005	0.034	0.001	0.111
	(0.150)	(0.187)	(0.266)	(0.087)	(0.105)	(0.158)
Observations	3694	2515	1148	3063	2135	928
$\bar{A}^P_{-i}$	0.146	0.221	-0.055	-0.020	-0.027	-0.065
	(0.148)	(0.182)	(0.263)	(0.089)	(0.110)	(0.156)
Observations	3698	2518	1149	3063	2134	929
$\bar{A}^P_{-i}$	-0.122	-0.140	-0.119	0.050	0.056	0.049
	(0.147)	(0.182)	(0.246)	(0.088)	(0.106)	(0.161)
Observations	3687	2512	1144	3057	2131	926
$\bar{A}^P_{-i}$	-0.132	-0.030	-0.378	-0.050	-0.113	0.039
	(0.150)	(0.182)	(0.272)	(0.087)	(0.103)	(0.168)
Observations	3698	2523	1144	3063	2137	926

Note: Probit and OLS estimation. In columns (1)-(3) the dependent variable is a dummy that is equal to 1 if the student dropped out during the first year and in columns (4)-(6) the dependent variable is first-year GPA. Main coefficients and not AME are reported in columns (1)-(3). In all estimations I have included the same controls as in Table 2. Robust standard errors are reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Table B.6: Robustness: Alternative Measure of Peer Quality

	Probit - Drop out probability			OLS - Education performance		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Peer effects:						
$q^{hT}_{75,-i}$	0.076**	0.036	0.182***	0.096	0.053	0.253
	(0.030)	(0.032)	(0.054)	(0.098)	(0.100)	(0.158)
Initial class male share	-0.197	-0.115	-0.383*	-0.075	-0.123	0.100
	(0.121)	(0.128)	(0.210)	(0.435)	(0.455)	(0.594)
Initial class size	0.003	0.001	0.010***	0.004	0.002	0.012
	(0.002)	(0.002)	(0.004)	(0.007)	(0.007)	(0.011)
Personal characteristics:						
GPA^{HS}	-0.071***	-0.078***	-0.047***	0.669***	0.677***	0.647***
	(0.008)	(0.009)	(0.014)	(0.019)	(0.022)	(0.032)
Starting age	0.004	0.006	0.003	0.058***	0.054***	0.063***
	(0.004)	(0.004)	(0.007)	(0.009)	(0.011)	(0.019)
General high school=1	-0.034**	-0.037**	-0.031	0.255***	0.225***	0.338***
	(0.016)	(0.018)	(0.027)	(0.030)	(0.035)	(0.061)
Woman=1	0.053***			-0.053*		
	(0.013)			(0.032)		
Jutland=1	-0.031*	-0.027	-0.039	0.201***	0.285***	0.043
	(0.018)	(0.023)	(0.032)	(0.056)	(0.073)	(0.096)
Fyn and Bornholm=1	-0.051*	-0.049*	-0.062	0.244***	0.257***	0.264*
	(0.029)	(0.030)	(0.057)	(0.073)	(0.079)	(0.135)
Copenhagen=1	-0.016	-0.018	-0.011	-0.105**	-0.015	-0.312***
	(0.020)	(0.024)	(0.036)	(0.051)	(0.063)	(0.080)
Greater Copenhagen=1	-0.030**	-0.028	-0.039	0.018	0.066	-0.075
	(0.015)	(0.019)	(0.025)	(0.040)	(0.043)	(0.079)
Frederiksborg=1	-0.011	-0.009	-0.014	0.090**	0.132***	-0.009
	(0.017)	(0.019)	(0.032)	(0.041)	(0.045)	(0.073)
Observations	4340	2998	1342	3562	2493	1069

Note: Probit and OLS estimation. Through columns (1)-(3) the dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Through columns (4)-(6) the dependent variable is first-year GPA. Average marginal effects. (AMEs) are reported in columns (1)-(3). When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. I include dummies in all regressions to control for educational length of the parents, cohort-fixed effects, and the average age and average age squared of the peer group, leave-out-mean. The excluded location group is the rest of Zealand. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Table B.7: Robustness: Probability of Dropping Out - Linear Probability Model

	All			Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(4)	(5)	(6)
Peer effects:									
Share of high ability peers	0.216**			0.101			0.512***		
	(0.091)			(0.096)			(0.172)		
Share of low ability peers	0.099			0.104			0.074		
	(0.097)			(0.110)			(0.153)		
$\bar{A}_{-i}$		0.081			0.024			0.223***	
		(0.049)			(0.053)			(0.083)	
SD_{-i}			0.048			0.044			0.067
			(0.071)			(0.074)			(0.113)
Initial class male share	-0.181	-0.203*	-0.197*	-0.091	-0.102	-0.101	-0.393*	-0.456**	-0.418*
	(0.113)	(0.117)	(0.115)	(0.122)	(0.125)	(0.123)	(0.206)	(0.206)	(0.212)
Initial class size	0.004	0.003	0.003	0.001	0.000	0.000	0.011***	0.009**	0.009**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
Personal characteristics:									
GPA^{HS}	-0.071***	-0.070***	-0.070***	-0.078***	-0.078***	-0.078***	-0.048***	-0.047***	-0.048***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.014)
Starting age	0.005	0.005	0.005	0.007	0.007	0.007	0.003	0.003	0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.008)	(0.008)
General high school=1	-0.037**	-0.037**	-0.037**	-0.041**	-0.041**	-0.041**	-0.031	-0.031	-0.030
	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)	(0.018)	(0.028)	(0.028)	(0.028)
Woman=1	0.050***	0.051***	0.051***						
	(0.013)	(0.013)	(0.013)						
Jutland=1	-0.033	-0.034*	-0.034*	-0.033	-0.033	-0.033	-0.036	-0.039	-0.039
	(0.020)	(0.020)	(0.020)	(0.025)	(0.025)	(0.025)	(0.036)	(0.035)	(0.036)
Fyn and Bornholm=1	-0.054*	-0.054*	-0.053*	-0.053	-0.053	-0.053	-0.056	-0.060	-0.058
	(0.032)	(0.032)	(0.032)	(0.033)	(0.033)	(0.033)	(0.066)	(0.066)	(0.067)
Copenhagen=1	-0.015	-0.015	-0.015	-0.018	-0.018	-0.018	-0.010	-0.006	-0.009
	(0.023)	(0.023)	(0.023)	(0.027)	(0.027)	(0.027)	(0.042)	(0.042)	(0.042)
Greater Copenhagen=1	-0.030*	-0.030*	-0.030*	-0.029	-0.030	-0.029	-0.037	-0.038	-0.037
	(0.016)	(0.016)	(0.016)	(0.021)	(0.021)	(0.021)	(0.027)	(0.027)	(0.027)
Frederiksborg=1	-0.013	-0.013	-0.014	-0.012	-0.012	-0.012	-0.014	-0.014	-0.018
	(0.018)	(0.018)	(0.018)	(0.020)	(0.020)	(0.020)	(0.035)	(0.034)	(0.034)
Observations	4340	4340	4340	2998	2998	2998	1342	1342	1342

Note: Linear Probability Model. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. I include dummies in all regressions to control for educational length of the parents, cohort-fixed effects, and the average age and average age squared of the peer group, leave-out-mean. The excluded location group is the rest of Zealand. Standard errors are clustered by peer group and reported in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Table B.8: Probability of Dropping Out - Probit Estimation
Outliers Excluded

	All	Men	Women	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(4)	(5)	(6)
Peer effects:									
Share of high ability peers	0.229***			0.129			0.527***		
	(0.083)			(0.094)			(0.159)		
Share of low ability peers	0.073			0.076			0.083		
	(0.087)			(0.103)			(0.146)		
$\bar{A}_{-i}$		0.092**			0.042			0.222***	
		(0.046)			(0.051)			(0.080)	
SD_{-i}			0.049			0.032			0.127
			(0.065)			(0.073)			(0.100)
Initial class male share	-0.209*	-0.231**	-0.227**	-0.104	-0.113	-0.114	-0.427**	-0.511**	-0.474**
	(0.114)	(0.114)	(0.113)	(0.119)	(0.120)	(0.119)	(0.215)	(0.210)	(0.218)
Initial class size	0.002	0.002	0.001	0.000	0.000	-0.000	0.007*	0.006	0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
Personal characteristics:									
GPA^{HS}	-0.070***	-0.069***	-0.070***	-0.074***	-0.074***	-0.074***	-0.055***	-0.054***	-0.055***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.014)	(0.014)	(0.015)
Starting age	0.004	0.004	0.004	0.005	0.005	0.005	0.006	0.006	0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)
General high school=1	-0.031**	-0.031**	-0.030**	-0.033*	-0.033*	-0.033*	-0.028	-0.028	-0.028
	(0.015)	(0.015)	(0.015)	(0.018)	(0.018)	(0.018)	(0.026)	(0.026)	(0.026)
Woman=1	0.045***	0.046***	0.046***						
	(0.013)	(0.013)	(0.013)						
Jutland=1	-0.033*	-0.033*	-0.034*	-0.026	-0.026	-0.027	-0.046	-0.047	-0.049
	(0.018)	(0.018)	(0.018)	(0.023)	(0.023)	(0.023)	(0.031)	(0.031)	(0.031)
Fyn and Bornholm=1	-0.044	-0.046	-0.044	-0.046	-0.047	-0.046	-0.035	-0.039	-0.034
	(0.029)	(0.028)	(0.029)	(0.029)	(0.029)	(0.029)	(0.058)	(0.057)	(0.059)
Copenhagen=1	-0.011	-0.010	-0.010	-0.007	-0.007	-0.007	-0.016	-0.012	-0.015
	(0.019)	(0.019)	(0.019)	(0.024)	(0.023)	(0.023)	(0.035)	(0.035)	(0.035)
Greater Copenhagen=1	-0.023	-0.023	-0.023	-0.016	-0.017	-0.017	-0.038	-0.039*	-0.037
	(0.015)	(0.015)	(0.015)	(0.019)	(0.019)	(0.019)	(0.023)	(0.023)	(0.023)
Frederiksborg=1	-0.014	-0.014	-0.015	-0.006	-0.006	-0.006	-0.028	-0.028	-0.031
	(0.016)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)	(0.031)	(0.030)	(0.030)
Observations	4201	4201	4201	2909	2909	2909	1292	1292	1292

Note: Probit estimation. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. I include dummies in all regressions to control for educational length of the parents, cohort-fixed effects, and the average age and average age squared of the peer group, leave-out-mean. The excluded location group is the rest of Zealand. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004. I have excluded students who dropped out because they transferred to other business programs.

Table B.9: Robustness: Probability of Dropping Out - Probit estimation Full Sample

	All	Men	Women	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(4)	(5)	(6)
Peer effects:									
Share of high ability peers	0.193**			0.096			0.420***		
	(0.093)			(0.098)			(0.156)		
Share of low ability peers	0.097			0.096			0.110		
	(0.090)			(0.102)			(0.142)		
$\bar{A}_{-i}$		0.079*			0.033			0.182**	
		(0.047)			(0.050)			(0.075)	
Std in peer ability			0.058			0.054			0.083
			(0.069)			(0.075)			(0.099)
Initial class male share	-0.195*	-0.219*	-0.214*	-0.128	-0.139	-0.138	-0.341*	-0.420**	-0.387*
	(0.117)	(0.120)	(0.118)	(0.130)	(0.133)	(0.130)	(0.201)	(0.200)	(0.207)
Initial class size	0.004*	0.003	0.003	0.001	0.001	0.001	0.010***	0.009***	0.009**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)	(0.004)
Personal characteristics:									
GPA^{HS}	-0.071***	-0.071***	-0.071***	-0.077***	-0.077***	-0.077***	-0.051***	-0.051***	-0.052***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.013)	(0.013)	(0.013)
Starting age	0.004	0.004	0.004	0.005	0.005	0.005	0.004	0.004	0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)
General high school=1	-0.031**	-0.030**	-0.030**	-0.032**	-0.032**	-0.032**	-0.025	-0.024	-0.024
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.025)	(0.025)	(0.025)
Woman=1	0.046***	0.047***	0.047***						
	(0.012)	(0.012)	(0.012)						
Jutland=1	-0.028	-0.029	-0.029	-0.022	-0.022	-0.022	-0.043	-0.045	-0.045
	(0.019)	(0.019)	(0.019)	(0.025)	(0.025)	(0.025)	(0.032)	(0.031)	(0.032)
Fyn and Bornholm=1	-0.049	-0.050*	-0.048	-0.046	-0.047	-0.046	-0.061	-0.065	-0.060
	(0.030)	(0.030)	(0.030)	(0.031)	(0.031)	(0.031)	(0.058)	(0.057)	(0.059)
Copenhagen=1	-0.000	-0.000	0.000	-0.000	-0.001	-0.000	-0.000	0.002	0.001
	(0.021)	(0.021)	(0.021)	(0.025)	(0.025)	(0.025)	(0.036)	(0.036)	(0.036)
Greater Copenhagen=1	-0.022	-0.023	-0.022	-0.019	-0.019	-0.019	-0.040	-0.041*	-0.039
	(0.016)	(0.016)	(0.016)	(0.020)	(0.020)	(0.020)	(0.025)	(0.025)	(0.025)
Frederiksborg=1	-0.011	-0.010	-0.010	-0.005	-0.005	-0.005	-0.023	-0.022	-0.025
	(0.017)	(0.017)	(0.017)	(0.019)	(0.019)	(0.019)	(0.031)	(0.030)	(0.030)
Danish=1	0.010	0.009	0.008	-0.027	-0.027	-0.028	0.069*	0.065*	0.065*
	(0.029)	(0.028)	(0.028)	(0.039)	(0.039)	(0.039)	(0.038)	(0.039)	(0.038)
Observations	4555	4555	4555	3124	3124	3124	1431	1431	1431

Note: Probit estimation. The dependent variable is a dummy that is equal to 1 if the student dropped out during the first year. Average marginal effects (AMEs) are reported. When computing AMEs for dummy variables, I report the effect from the discrete change from 0 to 1. In all regressions I include cohort-fixed effects and the average age and average age squared of the peer group, leave-out-mean. The excluded location group is the rest of Zealand. Standard errors are clustered by peer group and reported in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The sample includes Danes and non-Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Table B.10: Robustness: Educational Performance - OLS Estimation
Full Sample

	All			Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peer effects:									
Share of high ability peers	-0.095			-0.152			0.045		
	(0.300)			(0.297)			(0.471)		
Share of low ability peers	-0.430			-0.335			-0.618		
	(0.332)			(0.348)			(0.531)		
$\bar{A}_{-i}$		0.104			0.045			0.224	
		(0.151)			(0.149)			(0.256)	
Std in peer ability			-0.153			-0.192			-0.035
			(0.216)			(0.219)			(0.334)
Initial class male share	-0.188	-0.149	-0.148	-0.280	-0.251	-0.253	0.042	0.100	0.134
	(0.443)	(0.442)	(0.442)	(0.455)	(0.453)	(0.454)	(0.604)	(0.601)	(0.600)
Initial class size	0.003	0.003	0.003	0.001	0.001	0.001	0.008	0.009	0.008
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.011)	(0.011)	(0.011)
Personal characteristics:									
	ols1	ols11	ols111	ols2	ols22	ols222	ols3	ols33	ols333
GPA^{HS}	0.655***	0.655***	0.655***	0.677***	0.676***	0.677***	0.599***	0.600***	0.599***
	(0.019)	(0.020)	(0.020)	(0.021)	(0.021)	(0.021)	(0.037)	(0.037)	(0.038)
Starting age	0.045***	0.045***	0.045***	0.046***	0.047***	0.046***	0.038*	0.038*	0.037*
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.021)	(0.021)	(0.021)
General high school=1	0.270***	0.269***	0.269***	0.251***	0.250***	0.251***	0.329***	0.325***	0.327***
	(0.029)	(0.029)	(0.029)	(0.031)	(0.031)	(0.031)	(0.060)	(0.060)	(0.060)
Woman=1	-0.054*	-0.055*	-0.055*						
	(0.032)	(0.032)	(0.032)						
Jutland=1	0.223***	0.226***	0.223***	0.294***	0.297***	0.293***	0.090	0.092	0.091
	(0.056)	(0.056)	(0.057)	(0.073)	(0.073)	(0.073)	(0.096)	(0.097)	(0.097)
Fyn and Bornholm=1	0.259***	0.260***	0.259***	0.256***	0.257***	0.256***	0.298**	0.301**	0.299**
	(0.073)	(0.073)	(0.073)	(0.079)	(0.079)	(0.079)	(0.133)	(0.134)	(0.133)
Copenhagen=1	-0.117**	-0.116**	-0.117**	-0.031	-0.029	-0.031	-0.324***	-0.325***	-0.329***
	(0.048)	(0.048)	(0.047)	(0.060)	(0.060)	(0.060)	(0.083)	(0.082)	(0.083)
Greater Copenhagen=1	0.015	0.017	0.015	0.062	0.063	0.060	-0.074	-0.071	-0.070
	(0.038)	(0.038)	(0.038)	(0.043)	(0.042)	(0.042)	(0.078)	(0.078)	(0.078)
Frederiksborg=1	0.100**	0.101**	0.098**	0.142***	0.142***	0.139***	0.008	0.008	0.004
	(0.041)	(0.041)	(0.041)	(0.045)	(0.045)	(0.045)	(0.073)	(0.073)	(0.073)
Danish=1	0.340***	0.343***	0.340***	0.290***	0.292***	0.289***	0.402***	0.408***	0.406***
	(0.080)	(0.081)	(0.080)	(0.089)	(0.090)	(0.090)	(0.131)	(0.132)	(0.131)
Adj. R-squared	0.304	0.304	0.304	0.322	0.322	0.322	0.263	0.263	0.262
Observations	3738	3738	3738	2594	2594	2594	1144	1144	1144

Note: OLS estimation. The dependent variable is first-year GPA. In all regressions I include cohort-fixed effects and the average age and average age squared of the peer group, leave-out-mean. The excluded location group is the rest of Zealand. Standard errors are clustered by peer group and reported in parentheses. ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.1. The sample includes Danes and non-Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

B.3 Robustness: Heckman Sample Selection

Table B.11: Robustness: Educational Performance
Maximum likelihood Heckman Sample Selection Estimation

	Maximum likelihood Heckman Sample Selection Estimation								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peer effects:									
Share of high ability peers	-0.159			-0.123			0.743		
	(0.293)			(0.294)			(0.525)		
Share of low ability peers	-0.517*			-0.402			-0.753		
	(0.296)			(0.316)			(0.529)		
$\bar{A}_{-i}$		0.104			0.077			0.344	
		(0.142)			(0.144)			(0.239)	
Std in peer ability			-0.154			-0.153			-0.086
			(0.193)			(0.210)			(0.330)
Initial class male share	-0.002	0.051	0.052	-0.071	-0.035	-0.040	-0.422	0.081	0.174
	(0.455)	(0.454)	(0.454)	(0.486)	(0.487)	(0.487)	(0.693)	(0.621)	(0.684)
Initial class size	0.001	0.001	0.001	0.001	0.001	0.000	0.022*	0.010	0.008
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.011)	(0.011)	(0.012)
Personal characteristics:									
GPA^{HS}	0.721***	0.722***	0.721***	0.746***	0.746***	0.744***	0.599***	0.654***	0.656***
	(0.025)	(0.024)	(0.025)	(0.028)	(0.027)	(0.028)	(0.037)	(0.034)	(0.045)
Starting age	0.054***	0.054***	0.053***	0.047***	0.048***	0.047***	0.069***	0.062***	0.060***
	(0.010)	(0.010)	(0.010)	(0.012)	(0.012)	(0.012)	(0.020)	(0.019)	(0.019)
General high school=1	0.282***	0.280***	0.281***	0.257***	0.257***	0.257***	0.328***	0.346***	0.353***
	(0.035)	(0.035)	(0.035)	(0.040)	(0.040)	(0.040)	(0.063)	(0.060)	(0.060)
Woman=1	-0.090**	-0.092**	-0.092**						
	(0.038)	(0.038)	(0.038)						
Jutland=1	0.224***	0.227***	0.225***	0.311***	0.313***	0.310***	0.006	0.050	0.052
	(0.059)	(0.059)	(0.059)	(0.075)	(0.075)	(0.076)	(0.098)	(0.095)	(0.101)
Fyn and Bornholm=1	0.283***	0.285***	0.284***	0.305***	0.306***	0.305***	0.199	0.269**	0.269**
	(0.082)	(0.081)	(0.082)	(0.086)	(0.085)	(0.085)	(0.140)	(0.132)	(0.136)
Copenhagen=1	-0.094*	-0.092*	-0.094*	0.002	0.004	0.002	-0.324***	-0.308***	-0.313***
	(0.054)	(0.054)	(0.054)	(0.066)	(0.066)	(0.066)	(0.090)	(0.079)	(0.080)
Greater Copenhagen=1	0.038	0.040	0.038	0.092*	0.093*	0.090*	-0.115	-0.070	-0.065
	(0.046)	(0.046)	(0.046)	(0.050)	(0.050)	(0.050)	(0.080)	(0.080)	(0.085)
Frederiksborg=1	0.100**	0.100**	0.098**	0.143***	0.143***	0.140***	-0.031	-0.008	-0.013
	(0.044)	(0.044)	(0.044)	(0.049)	(0.049)	(0.049)	(0.080)	(0.073)	(0.076)
Observations	4340	4340	4340	2998	2998	2998	1342	1342	1342

Note: Maximum likelihood estimation of a Heckman sample selection model. The dependent variable is first-year GPA. I have included dummies in all regressions to control for educational length of the parents, cohort-fixed effects, and the average age and average age squared of the peer group, leave-out-mean. The excluded location group is the rest of Zealand. Standard errors are clustered by peer group and reported in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Table B.12: Robustness: Educational Performance
Maximum likelihood Heckman Sample Selection Estimation

	Maximum likelihood Heckman Sample Selection Estimation					
	High ability students			Low ability students		
	Al	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Share of high ability peers	-0.382	-0.396	0.385	0.493	0.045	1.121
	(0.365)	(0.383)	(0.692)	(0.414)	(0.437)	(0.788)
Share of low ability peers	-0.647*	-0.660*	-0.648	-0.247	-0.182	-1.099*
	(0.335)	(0.396)	(0.677)	(0.448)	(0.488)	(0.655)
Initial class male share	0.254	-0.073	0.241	-0.410	-0.203	-1.637*
	(0.523)	(0.543)	(0.885)	(0.561)	(0.627)	(0.939)
Woman=1	-0.114***			0.026		
	(0.043)			(0.048)		
$\bar{A}_{-i}$	0.084	0.121	0.424	0.076	-0.063	1.036***
	(0.139)	(0.162)	(0.366)	(0.241)	(0.245)	(0.385)
Initial class male share	0.293	-0.064	0.248	-0.322	-0.188	-1.690*
	(0.530)	(0.547)	(0.895)	(0.580)	(0.621)	(0.922)
Woman=1	-0.113***			-0.040		
	(0.044)			(0.052)		
Observations	2061	1389	672	2279	1609	670

Note: Maximum likelihood estimation of a Heckman sample selection model. The dependent variable is first-year GPA. In all estimations I have included the same controls as in Table 6. High-ability students are defined as students with high school GPAs above the cohort median, and low-ability students as students with high school GPAs below or equal to the cohort median. Standard errors are clustered by peer group and reported in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The sample includes Danes who enrolled in the bachelor's program in business economics at CBS between 1996 and 2004.

Appendix C Illustration of The Reflection Problem

Given random assignment the educational production function that depends on peers' performance look like this

$$Y_i^g = \theta_0 + \theta_1 X_i + \theta_2 \bar{Y}_j^g + \theta_3 \bar{X}_j^g + \epsilon_i \tag{1}$$

To simplify I consider the system for two individuals, i=1,2. Thus, each peer group is only one person. To see the problem of reflection consider equation 2 and 3.

$$Y_1^g = \theta_0 + \theta_1 X_1 + \theta_2 Y_2^g + \theta_3 X_2^g + \epsilon_1 \tag{2}$$

$$Y_2^g = \theta_0 + \theta_1 X_2 + \theta_2 Y_1^g + \theta_3 X_1^g + \epsilon_2 \tag{3}$$

Reducing the system by plugging Equation 3 into Equation 2 it becomes

$$(1 - \theta_2^2) Y_1^g = \theta_0(1 + \theta_2) + \theta_1\theta_2 X_2 + \theta_2\theta_3 X_1 + \theta_2\epsilon_2 + \theta_3 X_2 + \epsilon_1 \tag{4}$$

$$\Leftrightarrow \tag{5}$$

$$Y_1^g = \frac{\theta_0(1 + \theta_2)}{(1 - \theta_2^2)} + \frac{(\theta_1 + \theta_2\theta_3) X_1}{(1 - \theta_2^2)} + \frac{(\theta_3 + \theta_2\theta_1) X_2}{(1 - \theta_2^2)} + \frac{\theta_2\epsilon_2 + \epsilon_1}{(1 - \theta_2^2)} \tag{6}$$

Even if a reduced form of this equation is estimated, the underlying peer effects is not identified because there is only 3 reduced form parameters to be estimated and 4 underlying peer effect parameters.

Chapter 4

Do Peers Matter?

- Impacts of Peers on Master's Choice and Labor Market Outcomes

Do Peers Matter?
- Impacts of Peers on Master's Choice and Labor Market Outcomes*

Benedikte Bjerge
University of Copenhagen†

Marie Skibsted
Copenhagen Business School‡

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Abstract

This paper uses an unusually rich dataset to measure peer influence on the choice of a master's degree. Among the undergraduate students who are randomly assigned to exercise classes in the largest business school in Denmark, we find indications of peers being more likely to specialize in the same degree. The effect is, however, heterogeneous and stronger among students with similar revealed abilities in undergraduate mandatory courses. Our finding of heterogeneous effects by gender and age is ambiguous. Finally, the decision to follow peers has limited adverse consequences for academic performance and students' starting wages, and no impact on dropout rates.

Keywords: higher education; peer effects; heterogeneous impacts; labor market outcomes
JEL classifications: I21, I23, J24

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†bab@ifro.ku.dk. Department of Food and Resource Economics, Rolighedsvej 25, 1958 Frederiksberg C, Denmark.

‡mks.eco@cbs.dk. Department of Economics, Copenhagen Business School, Porcelænshaven 16A, 2000 Frederiksberg, Denmark

1 Introduction

The importance of school peers is widely recognized (Sacerdote, 2011; Epple and Romano, 2011), and many avenues have been discussed by which individual behavior is influenced by peers (e.g., Sacerdote, 2001; Kremer and Levy, 2008; De Giorgi et al., 2010; Carrell et al., 2011; Ali and Dwyer, 2011; Dahl et al., 2014). The extensive literature on peer effects in education has been most successful in documenting the role of peers for school performance and early schooling choices; however, only few studies are concerned with peer effects in higher education decisions (Lyle, 2007; De Giorgi et al., 2010; Ost, 2010; Poldin et al., 2015). If peer effects are also important in postgraduate decisions, then these decisions may have adverse consequences for educational performance and labor market outcomes if they are not in line with students' relative abilities. Peer effects in schooling choices may therefore be an important explanation for why such decisions are sometimes found to be inefficient (Rochat and Demeulemeester, 2001; Robst, 2007).

In this paper, we use a unique administrative dataset of students enrolled at the largest business school in Denmark, Copenhagen Business School (CBS), to cast light on peer influence in postgraduate choices and the subsequent consequences. The data track 10 cohorts of students from their time of enrollment in the same undergraduate program until they finish their two-year master's degree approximately five years later. Unique to the Danish schooling system, undergraduate students are guaranteed enrollment in one of the master's degree programs offered at the institution in which they hold their bachelor's degree. More than 90 percent of the undergraduate students at CBS choose to continue in one of the offered two-year master's degree programs. During the bachelor's program, students attend mandatory lectures and smaller exercise classes in randomly assigned groups. Students are assigned to exercise classes based on institutional features at the start of the first year. Similar to other studies in the literature, we consider these classes as our measure of peer groups (e.g., De Giorgi et al., 2012; Booij et al., 2015; Feld and Zölitz, 2015). Because the allocation into these groups is random, the standard problem of endogeneity in peer-group formation is overcome by construction. We combine these data with high quality Danish register data to obtain information on parental characteristics and students' subsequent performance in the labor market.

The empirical analysis is divided into two parts. First we look for evidence of positive and negative assortative matching along multiple dimensions, including matching on peers, using dyadic regressions. An estimating equation is said to be dyadic if each observation corresponds to a pair of students. This is in contrast to standard methods, where each observation corresponds to a single student. Positive assortative matching means that two students who are more similar are more likely to enroll in the same master's program and vice versa for negative assortative matching. Using information about all possible pairs of students, we investigate whether pairs of students enroll in the same master's degree program based on peer effects or because they resemble each other along other dimensions such as gender and revealed abilities.[1] We also test whether there are differing impacts of peers depending on students' observable characteristics. Essentially, we test whether peer influence is stronger between more similar students.

Second, since we have information about students' postgraduate performance and first job after graduation; we investigate whether peers can divert students away from a specific master's degree in which they have a relative ability advantage with adverse consequences for educational performance and labor market outcomes. In order to do that, we create different measures indicating whether students followed their peers, their potential (measured by revealed abilities during first year), both peers and potential, or none of the three.

We find evidence of positive assortative matching: Students randomly assigned to the same group the first year of undergraduate studies are more likely to enroll in the same master's program three years later. However, the significance of the effect differ across years and is significant at the 10 percent level for our main year of interest. This result is stronger for different definitions of peers and holds for placebo peer groups. In particular, we find that assortative matching among peers is significantly stronger between individuals with similar revealed abilities in mandatory courses in the first year of undergraduate studies. Contrary to studies analyzing peer influence on performance, we find no consistent heterogeneous effects by gender or age prior to enrollment.

In the second part of the analysis, we find that 21 percent of the students on average followed their peers into a master's program not in line with their relative abilities and 17 percent neither followed their peers nor their potential. However, we find only limited evidence of adverse consequences from not following one's potential on the probability to drop out, students' final GPA, and subsequent starting wage. We conclude that following peers are not necessarily inefficient, as peers are likely to help overcome the negative effects of not following ones revealed abilities. Also, as we show that students are more likely to follow peers with whom they share similar characteristics, this might damper the potential adverse effect from following peers.

This paper contributes to the literature in two ways. First, we introduce an econometric methodology, dyadic regression, normally used to study social network formation in development economics

[1] We measure revealed abilities using students officially reported high school grade point average (GPA), first-year GPA or/and bachelor's GPA.

(e.g., Fafchamps and Gubert, 2007; Aker, 2010; Beck et al., 2015). This method of dyadic regression can be compared to the gravity model used in the international trade literature to model trade flows between pairs of countries (dyads).[2] Typically, when estimating peer effects, studies are interested in determining the influence from the average performance (or behavior) of the peer group on the performance of an individual. The endogeneity that occurs, also called the reflection problem, is often handled by replacing the measure of group behavior with a predetermined measure of behavior. For instance, Lavy et al. (2012) use prior educational performance, Kremer and Levy (2008) use students drinking habits prior to college, while Carrell et al. (2011) apply students' fitness score prior to enrollment as proxy for current behavior of the group. The standard approach is not applicable when studying master's degree choice due to the absence of a reliable predetermined proxy for the peer group's choice of master's degree. Rather, the dyadic regression model enables us to identify whether randomly assigned university peers assort into the same master's degree programs.

Second, this paper contributes to the literature that examines differences in sensitivity to peer influence across groups. This literature has mostly focused on gender and race in peer influence on performance. Some have found that male students are more sensitive to peer ability (Goethals, 2001; Sacerdote, 2001; Zimmerman, 2003; Griffith and Rask, 2014), that low-ability students are helped most by higher-ability roommates (e.g., Griffith and Rask, 2014) and that peer effects are stronger intra-race (Hoxby, 2000). Others disagree and find evidence that female students are more affected than male students by both school interventions and peer effects (Arcidiacono and Nicholson, 2005; Anderson, 2008; Angrist et al., 2009; Lavy et al., 2012). We complement the existing literature by looking for evidence of heterogeneous peer influence in postgraduate studies across a larger range of individual characteristics and in particular across revealed abilities.

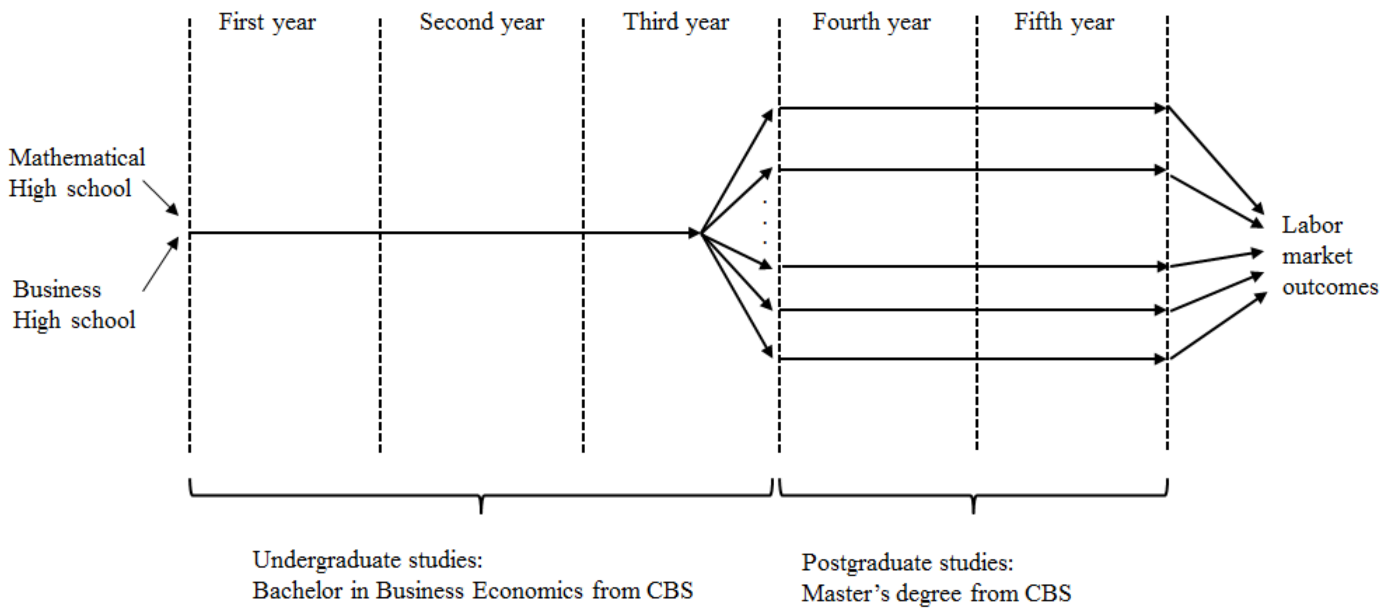
This paper is organized as follows: In Section 2, we describe the institutional structure of the Copenhagen Business School and the allocation of students into peer groups. In Section 3 we develop our empirical estimation strategy and discuss identification before the data are described in Section 4, together with a test of our identification strategy. Section 5 presents the results of the analysis on assortative matching in master's degree choice, while Section 6 analyzes the adverse consequences of following one's peers on students' drop-out probability, final master's GPA, and subsequent labor market outcomes. Section 7 concludes this paper.

[2] Other examples of the applicability of this method can be found in the broader economics literature as well as in political science (e.g., Mayer and Puller, 2008; Lindgren et al., 2009; Comola and Mendola, 2015). The dyadic regression approach was also applied to investigate choice of specialization in higher education in Russia (Poldin et al., 2015). However, among other things, we use the method differently by considering positive and negative assortative matching and by solving the endogeneity problem concerning peer selection.

2 Institutional Details

The educational focus in this paper is on higher education (sometimes also referred to as tertiary, post-secondary, or third-level education) offered by Copenhagen Business School (CBS), which is the largest Danish public institution that specializes in business and economics. Higher education is taken to include undergraduate and postgraduate education. We focus exclusively on one particular undergraduate degree, namely the largest bachelor's program offered by CBS called Business Economics.[3] This degree takes three years to complete and is awarded with a Bachelor of Science (B.Sc.) in Business Economics. After successful completion of the Business Economic bachelor's program, students are eligible for different master's degrees (i.e. postgraduate education) offered at CBS and elsewhere. All postgraduate degrees in Denmark take two years to complete. We follow the students through their entire higher education study lifecycle from enrollment in Business Economics to the completion of their master's degree approximately five years later. The complete higher educational program offered at CBS is illustrated in Figure 1. We follow 10 cohorts of students who were enrolled in the Business Economics bachelor's program in the period from 1995 to 2004. Unless otherwise stated, we distinguish between cohorts by *enrollment year* in undergraduate studies.

Figure 1: Higher Education Structure at Copenhagen Business School



The bachelor's program in Business Economics primarily consists of mandatory courses. The

[3]A total of 25 undergraduate programs was offered at CBS in 2015, and 25 percent of the undergraduate students was enrolled in the Business Economics bachelor's program.

program combines lectures and exercise classes. Lectures are a teaching session where an instructor, typically a senior faculty member, presents the material of the course. Lectures normally take place in auditoriums large enough to fit a full cohort of students.[4] Exercise classes are practical sessions where a teaching assistant solves problem sets and applied exercises with the students. The exercise classes take place in smaller (peer) groups of the same students but the assignments to be solved and the curriculum are the same across groups. In these exercises, the students are exposed to the curriculum in more concrete ways and are likely to interact with other group members more intensely. Students are assigned randomly into these smaller groups and have all their exercise classes with the same group members throughout the three years of the bachelor's program. Similar to other papers (e.g., De Giorgi et al., 2012; Feld and Zölitz, 2015; Booij et al., 2015), we use these exercise groups as our preferred measure of peer groups.

Class attendance and number of hours heavily influences with whom students spend most of their time. In the first year of the bachelor's program, the students have eight mandatory classes, corresponding to four courses each semester. A semester is 12 weeks long. In the first semester, each course on average consists of two lecture hours and 2 hours of exercise classes per week. Exercise classes start one week later than the lectures as exercise classes solve assignments related to the topic addressed in the lecture the preceding week. The number of lecture weeks as well as the number of exercise weeks depends on the specific course. On average, each course is taught for 11 weeks and has 10 weeks of exercise classes. Taken together, this means that students who finish the first year of the bachelor's in Business Economics within the prescribed period spend as much as 16 hours per week in the same room.[5] However, as for many higher educational programs, there is no attendance checking, meaning that students are not forced to attend all classes. There is no official reporting on class attendance at CBS, but it is the general perception that exercise classes are very popular as the students get to interact and work with the curriculum in smaller forums.

It is custom in Denmark for undergraduate students to continue their postgraduate studies at the same institution. Students who successfully complete the Business Economics bachelor's program can choose to progress and enroll in one of the offered master's programs. In fact, students who finished

[4] During the first year some lectures are offered twice because of the large amount of students that does not fit into one auditorium. While it is voluntary which of the two lectures the students attend, we expect students from the same exercise class to be more likely to attend the same lectures as they have similar course schedules.

[5] These numbers are based on the course schedule for Business Economics the first semester in 2015. It was not possible to obtain the course schedule for the cohort that enrolled in Business Economics in preceding years. However, according to the student administration at CBS, the number of teaching hours has not changed substantially the last 20 years. If anything, the budgets for teaching have followed a downward trend and thus we consider the estimate to be conservative. The courses taught the first semester include Managerial Economics, Microeconomics, Organization and Statistics.

their bachelor's degree before 2008 were, by law, automatically guaranteed enrollment in one of the programs.[6] In the considered period between 1995 and 2004, more than 90 percent of all students who graduated from the B.Sc. Business Economics program enrolled in one of the master's degrees offered by CBS. Students who did not continue into postgraduate studies at CBS either did not continue studying or enrolled in a master's degree program at another university in Denmark or abroad.

Over the period considered, the number of master's degrees offered at CBS has varied. For instance, the cohort enrolled in 1997 was free to choose between 13 different master's programs—three years later—whereas the 2001 cohort was offered 15 different master's programs. Table A.1 in the appendix lists the different master's degree programs throughout the period 1998-2007 (matched with cohorts enrolled in Business Economics from 1995 to 2004), the average bachelor's GPA and the distribution of students across these master's programs.[7]

Students who continued to postgraduate education at CBS were assigned to different degree programs based on a ranking list of their top four preferred degrees. If the number of students with the same first priority exceeded the number of available seats in that particular program, the selection was based on a lottery approach. The number of students not assigned to their first priority is around 20-30 students per cohort. However, since the assignment into the remaining master's degree programs was done by lottery, there is no systematic sorting on the second best choice. This approach rules out the possibility that students not assigned to their first priority will select another degree based on where their peers were assigned. If the assignment to a master's degree different from the students' top priority is random, the results can be seen as lower bounds on the true absolute effect size.

2.1 Definition of Peer Groups

The definition of peers is based on students attending exercise classes throughout their bachelor studies. Hence, the peer-group measure is meant to capture the network in which students interact academically and socially. Since exercise classes take place among a smaller number of students (on average 24 students) and focus on solving assignments related to the curriculum discussed in the lectures, the assumption that students interact with each other in class is plausible. Independently, the underlying assumption, similar to other papers studying the importance of peer groups (for a list of studies see

[6]After 2007, students no longer had this legal right to be accepted into a master's degree at CBS following their bachelor's in Business Economics. Student who enrolled in postgraduate studies after 2007 are excluded from the analysis.

[7]Some of the master's degrees offered at CBS specifically target students that finish the Business Economic bachelor's program whereas other master's degrees are intended for other bachelor's programs offered at CBS. Less than 1.25 percent of the students in our sample are registered with a master's degree program not ment for students with a B.Sc. in Business Economic. We exclude these students from our sample. As a robustness test, we investigate the sensitivity of our results by using the entire sample.

Epple and Romano, 2011), is that these interactions are fostered by class attendance.

The assignment criteria underlying the allocation of students into groups is based on gender, nationality, and age. The objective of CBS is to make homogeneous groups by gender and nationality, but heterogeneous groups in terms of students' starting age. Older students are assigned to the same group(s). These "older" groups have a higher average enrollment age and most likely contain students who have spent time working or studying other subjects. Given that older students have more experience, the impact from peers in these groups might be either more or less important. In the empirical analysis we therefore account for average age of the peer group by including peer-group fixed effects. Though the assignment of students into peer groups is based on the aforementioned criteria, it does not pose a problem in terms of self-selection.[8] Rather, the assignment rule helps safeguard that peer groups are homogeneous, which in turn ensures that we are not capturing a group-composition effect in the subsequent empirical analysis.[9]

In order to investigate whether student's choice of a master's degree is driven by peer effects, we assume that the institutionally constructed peer groups at enrollment contain the relevant set of peers. Fortunately, the majority of students stay in the same group throughout their bachelor studies. Under certain circumstances, however, individuals can be assigned to a new group. One reason for reassignment is that the core group becomes too small as students drop out. In this case, the smaller group is merged with an existing core group. This would result in an expansion of the existing peer group, but would not force individuals to change their existing peers. Another reason for a change in the peer group is that individual i chooses to change core group during the bachelor studies. Only under very limited circumstances are students allowed to change core group. They can do that only if they (i) apply for an exemption to change peer group, and (ii) identify a member of the preferred peer group willing to swap group with them. Changes in group composition can take place only in the second and third year of the bachelor's program.[10]

[8]The problem of self-selection emerges when students self-select into groups with peers that match their own ability level (Manski, 1993).

[9]The supplementary material (Appendix B) to this paper provide descriptive statistics for individual groups across a number of characteristics including individual and academic outcomes for students enrolled in undergraduate studies for each study cohort. As expected, the peer groups are only systematically different when it comes to age.

[10]Some students might attend exercise classes in groups to which they are not officially assigned. Unfortunately, we do not observe such unofficial changes in peer groups. However, we expect that these unofficial changes only happen in the second or third year of the bachelor's program, why it does not influence our measure of peer groups. We discuss this further in Section 5.3.

3 Choice of Master Program: Estimation Strategy

The empirical analysis of master's degree choice is divided into two steps. We first investigate characteristics associated with a higher probability of enrollment in a specific master's degree (enrollment regression) before turning to collocation analysis identifying presence of assortative matching (dyadic regression). We begin with a standard but naive OLS estimation in order to build a bridge to our preferred dyadic approach. If the naive results point toward some sort of peer effect, it adds additional reasons to investigate this effect in more depth with a dyadic regression approach.

3.1 Enrollment Regressions

In this part of the analysis, the main variable of interest is the share of peer group members— i.e. students assigned to the same first-year exercise classes—who enroll in the same master's degree program. Let $m_{i,k} = 1$ if student i is enrolled in master's degree k, and 0 otherwise. We model the probability of degree choice, by Equation (1) and estimate it with a Linear Probability Model:

$$m_{i,k} = \beta_o + \eta g_{-i,hk} + \beta X_i + \theta_c + \epsilon_i \tag{1}$$

Here, $g_{-i,hk}$ is the share of i's peers from exercise class h who enrolled in the same degree k, excluding individual i. X_i contains individual and parental characteristics that are likely to influence the choice of a master's degree and θ_c is a cohort fixed effect. Estimation of Equation (1) tells us whether student characteristics differ systematically between students enrolled in degree k compared to students enrolled in other master's programs, and whether assigned first-year classmates are more likely to enroll in the same master's program.

The main drawback of estimating Equation (1) is that the peer-effect measure ($g_{-i,hk}$) suffers from the classical problem of reflection (or two-way causality) where individual choice is determined together with the master's degree choice of the larger peer group (Manski, 1993).[11] Since there exists no reliable predetermined measure of degree choice, it is not possible to solve this problem using the standard estimation approach. To overcome the problem and come closer to determining and identifying a potential peer effect in choice of specialization, we allow for interdependency between students using a dyadic regression approach in the next section. This method also introduces the opportunity to investigate presence of assortative matching in a master's degree choice.[12]

[11] The reflection problem refers to the general problem of simultaneity of individual performance/behavior and group performance/behavior. This problem is well know in the peer effect literature and is normally addressed by using a predetermined measure of peer group behavior (e.g., Manski, 1993; Carrell et al., 2011; Lavy et al., 2012).

[12] In Equation (1), the choice of degree k is modeled as a binary choice variable between degree k or no degree, despite

3.2 Dyadic Regressions

With the dyadic regression approach we examine the data for evidence that students enrolled in the same master's degree program share similar characteristics or belonged to the same exercise (i.e. peer) group. Recently, Poldin et al. (2015) has also used the dyadic regression approach to investigate choice of specialization in higher education in Russia. Both their idea and approach are very similar to ours, however, among other things, we use the method differently by considering positive and negative assortative matching and particularly we solve the endogeneity problem concerning peer selection. The basic idea is that students may have preferences regarding with whom to collocate. In that case, the choice of degree depends not only on the students' own characteristics, but also on the characteristics of other students. For instance, students may choose their degree of specialization based on the choice of their peers or with students with similar observable characteristics—e.g., students of the same gender or with similar abilities. In the literature, this is commonly referred to as homophily (the tendency to love similar others) (McPherson et al., 2001). Given the lack of information about student preferences, it is not possible to investigate presence of homophily; however, in equilibrium, the formation process of master's degree programs may result in assortative matching: Students with similar (dissimilar) characteristics are more (less) likely to choose to specialize in the same degree. Positive assortative matching means that two students who are more similar are more likely to choose the same degree. Contrarily, negative assortative matching means that students belonging to the same degree program are less similar to each other compared to the rest of the population.

Empirical work on assortative matching has generally been hindered by the fact that assortative criteria are often correlated, which makes inference difficult (Arcand and Fafchamps, 2012). For instance, suppose that in Equation (1) we find that students in the same master's degree program have the same gender and are more likely to belong to the same peer group. If gender and group membership are correlated, we cannot distinguish whether students in the same degree share the same gender because they follow their peers, or whether they come from the same peer group because they sort on gender. To conduct a multivariate analysis on assortative matching we therefore use dyadic regressions following Arcand and Fafchamps (2012). Using information about all possible pairs of students, we investigate whether pairs of students enroll in the same master's degree based on peer effects or because they resemble each other along different dimensions such as revealed abilities or gender.

Let $m_{ij,k} = 1$ if both student i and j enroll in the same degree k, and 0 otherwise. We estimate

that this does not reflect the reality where students are free to select between several master's degrees. One possibility is to model this by a Multinomial Logit. However, the estimations would still suffer from the reflection problem.

the following dyadic regression model with a Linear Probability Model:

$$m_{ij,k} = \beta_0 + \beta g_{ij,h} + \varphi w_{ij} + \gamma_h + \varepsilon_{ij} \tag{2}$$

where $g_{ij,h}$ is a link-specific characteristic equal to one if students i and j were randomly assigned to the same exercise class (i.e. peer group) h. We also refer to this variable as the peer effect variable. Because students are randomly assigned to these groups at enrollment, this peer measure is predetermined in terms of students' master's degree choice. Thus, in Equation (2) we do not have to be concerned with the reflection problem as in Equation (1). A positive and significant coefficient (i.e. $\beta > 0$) means that students belonging to the same peer group are more likely to enroll in the same master's degree program. w_{ij} is a vector of regressors and γ_h is a peer-group fixed effect.

Since the dependent variable $m_{ij,k}$ in our study is symmetric by construction (i.e. $m_{ij,k} = m_{ji,k}$), regressors w_{ij} must be constructed in such a way that $w_{ij} = w_{ji}$. Especially, because the outcome $m_{ij,k}$ is equal to the outcome $m_{ji,k}$, the impact from w_{ij} needs to be the same as the impact from w_{ji} and thus w_{ij} needs to equal w_{ji}. To achieve this, we follow Fafchamps and Gubert (2007) and construct regressors of the form $|w_i - w_j|$ and $(w_i + w_j)$, where w_i and w_j are characteristics of i and j. Combining these types of regressors enables a distinction between the cases where students who belong to the same master's degree program have a higher (lower) w than students belonging to different degrees from cases where same-degree students have similar (dissimilar) w than students not belonging to the same degree. The coefficient on $|w_i - w_j|$, identifies negative and positive assortative matching. A negative coefficient on $|w_i - w_j|$ indicates positive assortative matching, while a positive $|w_i - w_j|$ indicates negative assortative matching. The coefficient on $(w_i + w_j)$, captures the propensity for two students to select the same master's degree program conditional on w. This means that the interpretation is similar to that of standard linear regression estimates: A positive coefficient on $(w_i + w_j)$ indicates that this characteristic is associated with same degree choice in the larger group. However, as pointed out by Fafchamps and Gubert (2007), the identification of coefficients on $(w_i + w_j)$ is difficult and one should mostly include regressors of this type as controls and results should be interpreted with caution.

The method of dyadic regression shares similarities with the gravity model, which is commonly used in the international trade literature to model bilateral aggregate trade flows between pairs of countries (dyads) (for a review and explanations, see Bergstrand and Egger, 2011; Mayer, 2014). The starting point for an analysis using the gravity model is to model trade flows between countries i and j by the following equation: $ln(PX_{ij}) = ln(\beta_0) + \beta_1 ln(GDP_i) + \beta_2 ln(GDP_j) + \beta_3 ln(DIST_{ij}) + \varepsilon_{ij}$, where PX_{ij} is the merchandise trade flow from exporter i to importer j and $DIST_{ij}$ is the distance between countries i and j. The later literature has added additional country-specific and country pair-specific variables to the model, such as dummies for common languages, common land boarders, or the absence of a Free Trade Agreement (FTA). It is normally expected that $\beta_1 > 0$, $\beta_2 > 0$, and $\beta_3 < 0$, which corresponds to saying that the distance between countries i and j has a negative impact on the trade flows between the two countries and that GDP in general is positively associated with trade flows. Relating this to our study, we also expect the "distance" in characteristics, measured by $w_{ij} = |w_i - w_j|$, to have a significant impact on the probability that students i and j choose the same degree. The gravity model also has been used to model the probability that countries i and j form a Free Trade Agreement (FTA) (e.g., Baier and Bergstrand, 2004). Similar to our study, modeling a FTA between countries i and country j, the dependent variable is an undirectional dummy equal to one if countries i and j have a FTA, and zero otherwise.

In order to investigate assortative matching along more dimensions than the peer-group effect, we include a number of regressors (w_{ij}) such as high school GPA, first-year GPA, gender, and age. A substantial literature documents the importance of family background, why we also account for parents' occupational type prior to first-year enrollment, place of residence while in primary school, and parents' educational level. In this study, however, we expect background effects to be smaller as everyone in the postgraduate sample was enrolled in the same bachelor's program at the same institution.

Since the networks of students are non-overlapping between cohorts, the dyadic approach uses the cross-sectional dimension of the data.[13] This means that Equation (2) is estimated for each cohort of students resulting in 10 different estimations. Results for the main variables of interest (β in Equation (2)) are reported for all cohorts, while more detailed estimation output is shown for a randomly selected cohort of students. Given that the dependent and independent variables are defined for every pair of students (ij), estimations are based on $n \times (n-1)/2$ observations, where n denotes the number of students.[14] Descriptive statistics at the dyad level by cohort has been relegated to the appendix, Table A.2.

Dyadic observations are generally not independent, as residuals containing the same student are correlated (Fafchamps and Gubert, 2007; Cameron et al., 2011; Cameron and Miller, 2014). To ensure that standard errors are robust to correlation in the error terms across students, we compute dyadic

[13]The network of students are non-overlapping between cohorts as assignment into first-year peer groups are based exclusively on students enrolled in the same academic year. Only if courses were not mandatory and students progressiveness was unimportant for the choice of courses would we end up in a situation where the network of students would cut across cohorts.

[14]For example, the empirical analysis for the 2001 cohort is performed on 57,630 unique student pairs (340 students).

standard errors as suggested by Fafchamps and Gubert (2007) throughout the empirical analysis.

Heterogeneous Effects

Understanding heterogeneous effects in peer influence is important for at least two reasons. First, if peer effects are homogeneous, changing peer groups to improve the outcomes of some students will necessarily harm other students. Thus, if peer effects are homogeneous, policy implications are limited. Second, as illustrated by Carrell et al. (2013), failure to understand the underlying peer mechanisms can result in educational policies that end up hurting students instead of helping them.[15]

To assess the underlying mechanisms driving peer-group influence on degree choice, we examine the heterogeneous impact from observable student characteristics. Specifically, we interact the peer-effect variable ($g_{ij,h}$) with differences in individual characteristics ($w_{ij} = |w_i - w_j|$). A regression of the following form is estimated by a Linear Probability Model

$$m_{ij,k} = \beta_0 + \beta g_{ij,h} + \varphi w_{ij} + \delta w_{ij} \cdot g_{ij,h} + \gamma_h + \varepsilon_{ij} \tag{3}$$

This specification allows the effect from being in the same peer group to depend on the differences in individual characteristics such as GPA, age, and gender. The partial effect from being in the same peer group simplifies to $\frac{\partial m}{\partial g} = \beta + \delta|w_i - w_j|$ and can thus be separated into a main effect, $\hat{\beta}$, and an additional effect, $\delta|w_i - w_j|$. If β and δ are jointly significant, it implies that being in the same peer group has a significant effect on the probability of $m_{ij,k} = 1$. If δ is statistically significant, it implies that the effect from being in the same peer group is significantly different across the distribution of $|w_i - w_j|$. Finally, $\hat{\beta}$ can be interpreted as the effect from being in the same peer group given no difference in $|w_i - w_j|$.

Identification

The identification of the peer effect (i.e. β in Equations (2) and (3)) has been the topic of several papers (e.g., Manski, 1993; De Giorgi et al., 2010; Angrist, 2014) due to issues related to self-selection and contamination of the peer effect.[16]

[15] Carrell et al. (2013) implement what they expect to be an optimal allocation of peers in a controlled setting. However, in contrast to their expectations, this does not lead to improved outcomes. In fact, because students of similar ability cluster in smaller subgroups within the assigned peer groups, some students (mostly low-ability students) are harmed by this intervention.

[16] In terms of identification, the literature also has been concerned with what is known as the reflection problem (Manski, 1993). Since our preferred peer group measure in the dyadic regression is predetermined to the choice of a master's degree we are not concerned with feedback effects from the choice of degree on peers. As discussed previously, this however does not apply in the case of Equation (1).

The self-selection problem arises when individuals choose their peers endogenously (see for instance Poldin et al., 2015). To account for self-selection into peer groups we use the randomly assigned first-year groups (for more details see Section 2). Since the individual students have no influence on the composition of the groups, we overcome the problem of self-selection. We test the validity of the assumption of randomly assigned peer groups in Section 4.

Second, in order to identify a peer effect it is important to separate out the correlation effect. The correlation effect is a group-specific effect that arises when the group is subject to influences that have an impact on the entire group performance and at the same time influence individual behavior. One such factor is teacher quality, which is intrinsically indivisible: Different teachers are assigned to different classes (i.e. groups of students). Classes may be homogeneous in mean student ability, but student achievement may be non-monotone in teacher quality across all classes. To address the correlation problem we exploit the social network dimension of the data and include peer-group fixed effects (γ_h).

Another potential threat to our identification strategy can arise if students at CBS are assigned to groups based on their own abilities or if teachers are assigned to groups based on the ability composition of the group and the teacher's experience or knowledge. In short, if CBS practices some kind of tracking or non-random teacher assignment, we have an endogeneity problem. According to the administration at CBS, teachers are randomly assigned to exercise classes and students are not assigned based on observed high school GPA. For this reason, we are less concerned that our peer-group measure (g_{ij}) is correlated with the fixed effects (γ_h) or students unobserved individual-specific ability level contained in the error term.

Finally, apart from controlling for students' innate ability measured by their high school GPA, we use students' first-year GPA to investigate how differences in students' revealed educational abilities affect their choice of a master's degree. Since first-year GPA is based exclusively on mandatory courses common to the entire cohort, it is comparable between students. As a robustness check, we also use students' bachelor's GPA to measure students' revealed abilities. Bachelor's GPA is a more noisy measure of revealed abilities because it is calculated based on both mandatory and elective courses. It might be that some students choose electives that are known to be "easy", leading to higher grades, whereas other students choose more difficult electives that are likely to result in lower grades. In this case, the commitment and time spent on the electives are not reflected in the bachelor's GPA and we thus believe that first-year GPA is a more reliable measure of students revealed abilities.

4 Data

In order to implement the empirical strategy outlined in the previous section, we rely on a unique dataset from Copenhagen Business School (CBS) and combine it with high quality Danish register panel data compiled by Statistics Denmark. The educational administrative dataset contains information about 10 cohorts of students from the time they enroll in the Business Economics bachelor's program until they finish their postgraduate degree. The cohorts considered enrolled in Business Economics in the years from 1995 to 2004.

For each student, the educational part of the data includes information on names of both mandatory and elective courses, grades obtained in individual courses followed, type of master's degree, starting year of the undergraduate and master's degree programs, and time spent at university. In addition, information about student's ECTS-weighted grade point average (GPA) from the first year, from the entire bachelor's, and from their postgraduate studies is contained in the data. Importantly, the educational part of the data also includes information about students' assigned first-year exercise group and high school GPA. The Danish register data cover the entire population and thus the full sample of students enrolled at CBS. We use this data to obtain information about students' labor market outcomes in their first job after graduation, as well as parental background, which has been found to heavily influence educational decisions, at least in the early educational trajectory (Ermisch and Francesconi, 2001; Dustmann, 2004).

We exclude students who did not complete the bachelor's program and students who did not continue into a master's degree program at CBS.[17] We also drop non-Danish students in order to control for a larger number of background variables. For the same reason, we restrict the sample to students for whom we have parental information.[18] The sample we use for the empirical analysis consists of 3,132 students. Out of these, 2,633 students graduated from the master's degree program in which they were enrolled after finishing their undergraduate studies in Business Economics.

[17] Around 10 percent of the undergraduate students who graduated from the bachelor's program did not continue into a master's degree program at CBS. In the robustness section, we expand the sample to include these individuals. Moreover, more than 19 percent of the original sample dropped out of the bachelor's program. This means that our measure of peer groups contains fewer peers than the original number of peers.

[18] In total we drop 158 non-Danish students from the sample. Results including non-Danish students are discussed in the robustness section and estimation output is provided in the supplementary material (Appendix B) to this paper.

Table 1: Descriptive Statistics

	All cohorts		2001 cohort	
	Mean	Std.dev.	Mean	Std.dev.
Individual student characteristics:				
Starting age (years)	21.29	1.61	21.46	1.76
Gender (female=1)	0.30	0.46	0.33	0.47
Business high school (=1)	0.25	0.43	0.22	0.41
General (mat) high school (=1)	0.60	0.49	0.66	0.47
High school GPA	6.92	1.95	6.77	1.84
First-year GPA	5.29	2.24	5.20	2.05
Bachelor's GPA	6.42	2.10	6.37	1.82
Dropped out of master's program (=1)	0.13	0.34	0.10	0.30
Master's GPA^A	7.03	1.96	6.95	1.79
Hourly wage, first job (DKK)^B	184.66	37.93	189.56	37.48
Parents characteristics:				
Father self-employed (=1)	0.11	0.31	0.11	0.32
Mother self-employed (=1)	0.05	0.22	0.05	0.22
Father's education:				
High school or less	0.18	0.38	0.20	0.40
Professional qualifications	0.29	0.45	0.31	0.46
Short or Medium tertiary	0.24	0.43	0.21	0.41
Long tertiary	0.19	0.39	0.21	0.41
Mother's education:				
High school or less	0.21	0.41	0.21	0.41
Professional qualifications	0.32	0.47	0.31	0.46
Short or Medium tertiary	0.35	0.48	0.34	0.48
Long tertiary	0.09	0.28	0.09	0.29
Place of residence 10 years prior to enrollment:				
Region of Copenhagen	0.44	0.50	0.45	0.50
Zealand without Copenhagen	0.41	0.49	0.39	0.49
Jytland	0.09	0.29	0.10	0.30
Fynen	0.03	0.17	0.03	0.18
Other	0.02	0.15	0.03	0.16
Sector occupation in first job:^{B}				
Agriculture, fishing and quarrying	*		-	
Industries and utilities	0.09	0.29	0.07	0.26
Construction	*		*	
Trade, transport, info. and communication	0.22	0.41	0.25	0.43
Finance and business services	0.64	0.48	0.63	0.48
Public and personal services	0.05	0.22	*	
Observations	3,132		343	

Note: "*" means that we are not able to disclose the information due to discretion roles set by Statistics Denmark. For the same reasons minimum and maximum are not reported. "-" indicates that there are no observations in that group.
^{A}Means reported for 2,633 students on final master's degree GPA (288 observations for the 2001 cohort).
^{B}Means reported for 1,854 observations on sector and hourly wages (253 observations for the 2001 cohort). Wages are inflation adjusted with 2000 as the basis year.
For the full sample we have that father's education is missing in 10 percent of the cases and that mother's education is missing in 3 percent of the cases. We consider the missing information as a category by itself.

Descriptive statistics across all 10 cohorts and separately for the cohort enrolled in 2001 are presented in Table 1. In the subsequent empirical analysis, we first report detailed estimation results for the 2001 cohort as an illustrative example before considering the entire sample of cohorts. The average student is 21 years old upon enrollment in Business Economics at CBS. The majority of enrolled students are male (70 percent). Some 60 percent of the students went to a regular high school with focus on mathematics while 25 percent went to a business high school. The remaining 15 percent of the students completed other types of university-preparation corresponding to Danish high school level.

We measure a student's *revealed* abilities by first-year GPA, which is based exclusively on mandatory courses. The GPA is reported using the Danish "7"-step grading scale where the lowest passing grade is 2 and the maximum grade is 12.[19] A comparison of the Danish grading scale to the ECTS and the American grading scale are shown in the supplementary material (Appendix B) to this paper. Across all cohorts, the average high school GPA is 6.92, the average first-year GPA is 5.29, while the average bachelor's GPA is 6.42. The average master's GPA is slightly higher with an average of 7.03. The higher master's GPA suggests that we have some level of self-selection out of the master's degree programs—concerning students who decided not to take a postgraduate degree and students who dropped out of the program in which they were enrolled.

Information about parental background includes place of residence, occupational type prior to first-year enrollment, and parents' educational level measured by different categories. Parental educational information is reported at the aggregated level. In the actual estimations we include it at a more detailed level.[20] Eleven percent of fathers and 5 percent of mothers are self-employed. Place of residence is measured 10 years prior to enrollment at CBS in order to capture the region in which the student grew up and went to primary school. With this variable we account for different social contextual effects and traditions across the country, which is likely to impact how students make educational decisions (Akerlof, 1997). Place of residence in Table 1 is reported at the aggregated level. In the actual estimations, place of residence 10 years prior to enrollment is based on the 12 counties in Denmark. The vast majority of students grew up on Zealand (85 percent), which is the island on which the capital city Copenhagen is located.

[19] The Danish grading system changed in 2007. Thus, GPA observed for students who graduated before 2007 is based on the old grading scale. In order to ensure comparability between academic outcomes across cohorts we convert students' GPAs reported before 2007 to the new "7"-step grading scale using the official converting table provided by the Danish Ministry for Children, Education, and Gender Equality (see `http://eng.uvm.dk/Education/General/7-point-grading-scale?allowCookies=on`).

[20] An obvious thing to include is the income level of the parents. This should be done in future versions of this paper. The reason for not including it here is that it need more investigation of the reason for some students' mission information on parents' income. In fact, the inclusion of parental income and the exclusion of students' with this information missing reveals stronger results.

Information about labor market outcomes is used to analyze the subsequent effect of following one's peers in students' choice of master's degree (see Section 6). For this analysis, the sample of students is restricted to students who graduated from a master's degree program at CBS, have non-missing information about final master's GPA and about their first job after graduation. To avoid measurement errors, we also exclude students with wage observations that are measured with low precision according to Statistics Denmark. We have complete information about sector occupation and hourly wages for 1,854 graduates—all observed to be employed in Denmark between the day of graduation and three years after.[21] Students' average wage in their first job is 184.66 DKK per hour, corresponding to approximately 25 USD.[22] More than 60 percent of the students were hired in the financial and business sector upon graduation, while 22 percent were hired in the sectors of transportation, trade, information, and communication. Given the nature of the postgraduate educational programs offered as CBS, the occupational division across sectors is hardly surprising.

4.1 Descriptive Statistics by Peer Groups

Table 2 reports descriptive statistics on the average number of groups and class size across all cohorts and separately for the cohort enrolled in 2001. Students were, on average, assigned to 15 exercise classes with an average number of 21 students in each class. Through the second and third year of the bachelor's program, the number of groups decreased to 14 in the second year and further to 13 in the third year (on average).[23] The drop in the number of exercise classes increased the average number of students to 23 and 24 in the second and third years, respectively.

Considering the cohort enrolled in 2001, the increase in class size is substantially higher. However, the rise in the number of students per exercise class was primarily driven by administrative decisions to combine small exercise classes. In fact, only 12 and 13 students changed exercise class prior to the second and third years, respectively.[24] The small number of students who choose to change exercise class suggests that the problem with self-selection is limited.

The marginal change in peer groups during undergraduate studies in our case may, however, cast doubt on the possibility of capturing the relevant peer interactions by looking at assigned first-year classmates. To address this, we also investigate the importance of peer groups based on group com-

[21] The hourly wage is measured in November each year. If students finish their master's degree after November, the hourly wage in their first job is not measured before November the following year.

[22] The average exchange rate for July 2015 was used. Wages are inflation adjusted with 2000 as the basis year.

[23] Additional information on group changes across years is provided in the supplementary material (Appendix B) to this paper.

[24] Unfortunately, we are not allowed to report the actual transition matrix due to the low number of switchers which introduces the risk of disclosing confidential information.

Table 2: Descriptive Statistic Across Peer Groups

	All cohorts				2001 cohort		
	Mean	Std.dev.	No. of groups	Average no. of groups	Mean	Std.dev.	No. of groups
Group size (no. of students):							
First-year peer group	21.31	4.87	147	14.7	24.5	2.71	14
Second-year peer group	22.86	7.29	137	13.7	28.58	3.12	12
Third-year peer group	23.91	10.68	132	13.1	31.18	10.70	11
First-year peer group characteristics:							
Share of males	0.70	0.07	147	14.7	0.67	0.06	14
Age	21.31	0.73	147	14.7	21.46	0.41	14
High school GPA	8.39	0.20	147	14.7	8.33	0.20	14
First-year GPA	7.67	0.33	147	14.7	7.67	0.22	14
Bachelor's GPA	8.16	0.32	147	14.7	8.17	0.11	14

position in the second and third years. Considering these alternative peer groups, however, comes with the disadvantage of introducing self-selection as some of the individuals change groups based on a personal choice/request and not due to an administrative decision. Due to the self-selection problem, we expect the peer effect to be larger compared to the effect when the first-year peer-group measure is used.

4.2 Test of Random Assignment into Peer Groups

The assumption that allows us to estimate and identify a peer effect is random assignment into first-year groups. Even though this is formally the case, we check the validity of our preferred definition to ensure that there are no systematic differences across peer groups. To do this, we investigate whether peer-group assignment is correlated with individual socio-economic and pre-educational characteristics. To be exact, we estimate a Linear Probability Model (LPM) of the form $h_{i,gt} = \beta_o + \beta X_i + \epsilon_i$, where $h_{i,gt} = 1$ if individual i was assigned to peer group g in enrollment year t and X_i is a vector of background variables. We undertake this exercise for each group in each cohort (i.e. 147 estimations) and test for joint significance of all the explanatory variables except from enrollment age which is used as class assignment criteria by CBS.

The estimation output is summarized here and detailed further in Tables 6 and 7 in the supplementary material (Appendix B) to this paper. As expected, given the randomization, the variables are not jointly significant in the vast majority of cases. For the cohort enrolled in 2001 we find that the variables are not jointly significant predictors of peer groups in all cases. Besides starting age, we find that it is mostly the characteristics of the parents that enter the regressions significantly. To mitigate the potential problem with correlation between background characteristics and peer-group assignment, we control for the difference in enrollment age and parents' characteristics, and include peer-group fixed effects in all estimations. That starting age enters the regression significantly is not overly surprising given the allocation criteria used by CBS.

The test of random assignment is based on the sample of students that completed the bachelor's program in Business Economics. More than 19 percent on average drop out of the Business Economics program either in the first, second or third year. While we have information about the students that dropped out, we have not included them in the analysis. We recognize that this may lead to out-of-sample bias due to the self-selection nature of the decision to drop out or stay in the sample. If the students that dropped out during the first three years are also the ones with less strong peer attachments, our estimates would thus lead to a higher peer effect.

However, Skibsted (2016), Chapter 3 of this thesis, shows how women's probability of dropping out is increasing in the peers' ability level. This result is interpreted as women being more prone to compare themselves with their peers and to create wrong self-images based on these comparisons, which make them drop out. If this is true, it could just as well be that the students that drop out of the sample (the bachelor's program) are the ones that in fact react most to their peers. This would mean that our sample of students that finished their bachelor's program consists of students that react the least to peers' impact and thus our estimates of a peer effect would be lower. This explanation corresponds well with the finding that controlling for sample selection, Skibsted (2016) finds stronger and larger peer effects on educational performance compared with standard OLS peer effect estimates. In fact, even if our estimated peer effect is higher because the students that stay in the sample are also the ones with strongest peer attachments, our estimates can still be thought of as a true peer effect. This is so because the effect is simply just generated by a two-step decision process where students decide to stay in the sample and then decide on their master's degree program, where both decisions were impacted by the same *randomly* assigned peers.

Given the arguments above, we feel confident that our estimates will reflect a peer effect and that our results can contribute with important knowledge about how students decide on their master's degree program. Nevertheless, the issue of sample selection is worth considerations and for further research a sample selection model of Equation (2) should be considered. To ensure that our assumption of random assignment into peer groups is still plausible despite the issue with self-selection out of the sample, we perform the test of random assignment on the smaller group of students that did not drop out in order to ensure that the remaining students are randomly assigned to groups.

5 Results

5.1 Enrollment Regressions

We begin by estimating Equation (1), focusing exclusively on the effect from the share of peer-group members enrolled in the same master's degree program. We wish to investigate whether assigned first-year classmates are more likely to enroll in the same degree three years later. Estimation results for the 15 most popular degrees available to the cohorts enrolled in the period 1995-2004 are presented in Table 3. Each column corresponds to a separate estimation for a specific master's degree k.

Results show a positive association between enrollment in master's degree k and the share of peers enrolled in degree k: Students are more likely to enroll in a master's degree program the larger the share of peers enrolled in the same degree. For about half of the cases, the association is statistically significant at least at the 5-percent level. A possible explanation for the insignificant peer effect is the large share of foreign students enrolled in these master's programs.[25] This is consistent with the findings that social interaction and peer impacts are less important in an environment or class with more foreign students (Hoxby, 2000).

Table 3: Enrollment Regressions: Share of Peers in Same Master's Degree (all cohorts)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Degree k:	M1	M2	M3	M4	M5	M6	M7	M8
$g_{-i,hk}$	0.299***	0.079	0.225**	0.182**	0.157*	0.311***	0.078	0.019
	(0.075)	(0.075)	(0.089)	(0.073)	(0.091)	(0.066)	(0.126)	(0.096)
Years	1995-2004	1995-2004	1995-2001	1995-2004	1995-2002	1995-2004	1999-2004	1995-2004
Observations	3132	3132	2194	3132	2534	3132	1990	3132
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	
Degree k:	M9	M10	M11	M12	M13	M14	M15	
$g_{-i,hk}$	0.153	0.246***	0.186	0.005	0.205**	0.222***	0.168	
	(0.110)	(0.083)	(0.117)	(0.105)	(0.097)	(0.077)	(0.110)	
Years	1995-2004	1995-2004	2000-2004	1999-2004	1995-1999	1995-2004	1995-2001	
Observations	3,132	3,132	1,637	1,990	1,495	3,132	2,194	

Note: Linear Probability Model. Dependent variable is equal to 1 if students are enrolled in master's degree k, and zero otherwise. Standard errors reported in parenthesis. Each column correspond to a master's degree. The k=(M1,...,M15) master's degree programs corresponds to a degree with the same number in Table A.1 shown in the appendix. All estimations include cohort fixed effects and control variables (student's bachelor's GPA, a high school dummy, age at enrollment, gender and family characteristics). *** p<0.01, ** p<0.05, * p<0.1

[25] Between 5-20 percent of the students enrolled in the master's degree programs named M2, M7, M9, M11, M15 are identified as non-Danish students. This compares with an average of 4 percent in the master's programs where the share of peers are found to have a positive and significant effect.

The results support presence of peer effects in choice of master's degree among a homogeneous group of bachelor's students at the largest Danish business school in Denmark. While this result is encouraging, it may suffer from two-way causality (reflection). Furthermore, Equation (1) models only the choice of degree as a function of own characteristics. For this reason it is not possible to identify positive and negative assortative matching, which may lead us to conclude that some master's degrees are only for higher achieving students when in fact the data are better explained by assortative matching.

5.2 Dyadic Regressions

Results: 2001 Cohort

We first estimate the dyadic regression for the cohort of students enrolled in Business Economics in the year 2001. Estimation results are shown in Table 4. We find that the measure of first-year peer group is significant at the 10 percent level for students' choice of specialization (column 1): students randomly assigned to the same exercise class upon enrollment at the bachelor's level are 1.3 percentage points more likely to study for the same master's degree. This effect size does not change when student and parental characteristics are included in columns 2-4. This supports the previous evidence that students are randomly assigned into peer groups.

The negative sign on the estimated coefficient to the difference in first-year GPA indicates that students with similar revealed abilities in mandatory courses are more likely to choose the same master's degree program (i.e. positive assortative matching). To investigate whether the estimated peer effect depends on the measure of revealed abilities used in the model, column 4 replaces first-year GPA with students' final bachelor's GPA. The absolute difference in students' bachelor's GPA is statistically significant at the 5-percent level, but the size of the coefficient estimate falls slightly compared to the estimate using first-year GPA.

We observe no significant effect from differences in high school GPA, age, or being of different gender. A negative and significant effect from the sum of high school GPA is observed. This indicates that a pair of students who together have a high sum of high school GPA are less likely to choose the same degree compared to a pair of students with a low sum of high school GPA. Thus, our results show that students of similar and higher abilities are less likely to choose the same master's degree compared to pairs of students with similar and lower abilities.

Columns 5 and 6 replicate the result reported in column 3 by substituting only the first-year-peer group measure with the second- and third-year peer-group measure, respectively. The peer effect is statistically significant at the 5 percent level and the magnitude of the effect increases slightly in size: Students from the same peer group are 1.3-1.4 percentage points more likely to choose the same master's degree.

Table 4: Dyadic regressions: Peer effects in the choice of master's degree

	(1)	(2)	(3)	(4)	(5)	(6)
First-year peer group	0.013*	0.013*	0.013*	0.013*		
	(0.007)	(0.007)	(0.007)	(0.007)		
Second-year peer group					0.013**	
					(0.006)	
Third-year peer group						0.014**
						(0.006)
Same gender (=0)		-0.007	-0.007	-0.006	-0.007	-0.007
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Same location (=0)		0.001	0.002	0.002	0.002	0.002
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Fathers both self-employed (=0)		-0.029**	-0.027**	-0.027**	-0.027**	-0.027**
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Mothers both self-employed (=0)		-0.013	-0.014	-0.013	-0.014	-0.014
		(0.016)	(0.015)	(0.015)	(0.015)	(0.015)
Fathers with same education (=1)		0.004	0.004	0.004	0.005	0.004
		(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mothers with same education (=1)		-0.005	-0.005	-0.006	-0.005	-0.005
		(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Absolute difference in:						
First-year GPA		-0.008***	-0.007***		-0.007***	-0.007***
		(0.003)	(0.003)		(0.003)	(0.003)
Bachelor's GPA				-0.006**		
				(0.003)		
High school GPA		-0.002	-0.001	-0.002	-0.001	-0.001
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age		0.003	0.003	0.003	0.003	0.003
		(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Sum of:						
First-year GPA			0.002		0.002	0.002
			(0.003)		(0.003)	(0.003)
Bachelor's GPA				0.001		
				(0.003)		
High school GPA			-0.006**	-0.006**	-0.006**	-0.006**
			(0.002)	(0.002)	(0.002)	(0.002)
Age			-0.001	-0.001	-0.001	-0.001
			(0.003)	(0.003)	(0.003)	(0.003)
Observations	58,653	58,653	58,653	58,653	58,653	58,653

Note: Linear Probability Model. Dependent variable is equal to 1 if i and j are enrolled in the same master's degree. Standard errors reported in parenthesis. Estimation sample includes Danish students who enrolled in undergraduate studies at CBS in 2001. All estimations include cohort and peer-group fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Even though our estimated peer effect is modest in magnitude and only significant at the 10 percent level, the size of the effect is similar to previous findings in the literature (Arcidiacono and Nicholson, 2005; Lyle, 2007; Ost, 2010; De Giorgi et al., 2010; Poldin et al., 2015). Using a somewhat similar approach to our enrollment Equation (1), Lyle (2007) finds a significant role-model effect and an insignificant peer effect in major choice using data on plebes from a U.S. military academy. Particularly, he finds that a 10-percent increase in the fraction of role models that intended to study engineering results in a 1.5-percentage-point increase in the probability that a cadet will choose to major in engineering. Using overlapping peer groups and IV estimations to identify a peer effect in major choice, De Giorgi et al. (2010) finds that adding one average peer with a major in economics to an average student's peer group increases the probability that the average student will choose to major in economics by 7.4 percentage points. Compared to these studies investigating peer effects in major choice, our estimated effect of 1.3 percentage points is in the lower end, yet comparable to Lyle (2007).

Heterogeneous Effects: 2001 Cohort

We proceed to investigate the underlying mechanisms driving assortative matching by peer groups. Estimation results of Equation (3) for the cohort enrolled in 2001 are reported in Table 5.

We include interactions between the peer-effect variable and the absolute difference in first-year GPA, bachelor's GPA, high school GPA, age, and gender to examine whether matching on peer groups is homogeneous across observable student characteristics. Considering students' first-year GPA (column 2), the total peer effect now depends on the difference in GPA between pairs of students. We find that students belonging to the same peer group are more likely to choose the same master's degree if they are similar in terms of revealed abilities measured as first-year GPA. The effect is even stronger in column 3 when we account for the effect from students' bachelor's GPA. In comparison to the result without interactions, the likelihood of choosing the same program increases from 1.3 percentage points to 3.4-4.0 percentage points (columns 2 and 3 in Table 5) from being in the same peer group given no difference in first-year GPA or bachelor's GPA, respectively. Moreover, the peer effect is significant at the 1 percent level. Furthermore, the interaction term in columns 2 and 3 ($g_{ij,h}$ interacted with first-year GPA and bachelor's GPA, respectively) is statistically significant at the 5 and 1 percent level, respectively. This suggests that the peer effect is different across students' ability distribution.

The estimated peer effect disappears when we include the interaction with the absolute difference in high school GPA (column 4). This is in line with the finding presented in Table 4: If a pair of students with a high sum of high school GPA are less likely to choose the same master's degree compared with a pair of students with a low sum of high school GPA, then this effect cancels out the effect from the difference in high school GPA. This is the case because two students with a high sum of high school GPA will also have a small absolute difference in high school GPA.

Table 5: Heterogeneous effects: Peer effects in the choice of master's degree

	(1)	(2)	(3)	(4)	(5)	(6)
First-year peer group	0.013*	0.034***	0.040***	0.004	0.015	0.024***
	(0.007)	(0.013)	(0.013)	(0.009)	(0.009)	(0.009)
Abs. diff. in first-year GPA * First-year peer group		-0.009**				
		(0.004)				
Abs. diff. in bachelor's GPA * First-year peer group			-0.013***			
			(0.004)			
Abs. diff. in high school GPA * First-year peer group				0.004		
				(0.003)		
Same gender (=0) * First-year peer group					-0.004	
					(0.012)	
Abs. diff. in age * First-year peer group						-0.006*
						(0.003)
Absolute difference in:						
Difference in first year GPA	-0.007***	-0.007***		-0.007***	-0.007***	-0.007***
	(0.003)	(0.003)		(0.003)	(0.003)	(0.003)
Difference in GPA			-0.005*			
			(0.003)			
Difference in high school GPA	-0.001	-0.001	-0.002	-0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Same gender (=0)	-0.007	-0.007	-0.006	-0.007	-0.006	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Difference in age	0.003	0.003	0.003	0.003	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mothers with same education (=1)	-0.005	-0.005	-0.006	-0.005	-0.005	-0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Fathers with same education (=1)	0.004	0.004	0.004	0.004	0.004	0.004
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
p_1-value from joint test		0.025	0.006	0.065	0.167	0.021
p_2-value from joint test		0.001	0.003	0.092	0.212	0.040
Observations:	58,653	58,653	58,653	58,653	58,653	58,653

Note: Linear Probability Model. Dependent variable is equal to 1 if i and j are enrolled in the same master's degree. Standard errors reported in parenthesis. $H_1 : \beta_{g_{ij,h}} = 0$, $\delta_{interaction} = 0$ and $H_2 : \beta_{g_{ij,h}} = 0$, $\delta_{interaction} = 0$, $\varphi_{characteristics} = 0$. The estimation sample includes Danish students who enrolled in undergraduate studies at CBS in 2001. All estimations include control variables similar to Table 4. *** p<0.01, ** p<0.05, * p<0.1

We also find that the effect from peers increases and stays significant when we include the interaction with the absolute difference in starting age (column 5). This result suggests that students from the same peer group with no age difference are more likely to choose the same degree compared to students in the same peer group, but with a larger age difference. The partial effect from being in the same peer group increases to 2.4 percentages points when peer-group members assort based on no age difference. The interaction between peer group and absolute age difference ($\delta_{interaction}$) is not significant in itself, but the main peer-group effect and the interaction effect are jointly significant. In addition, all three effects—the main peer-group effect, the interaction effect, and the absolute age difference effect—are jointly significant. Finally, we find only weak presence of heterogeneous peer effects based on students' gender (column 6).

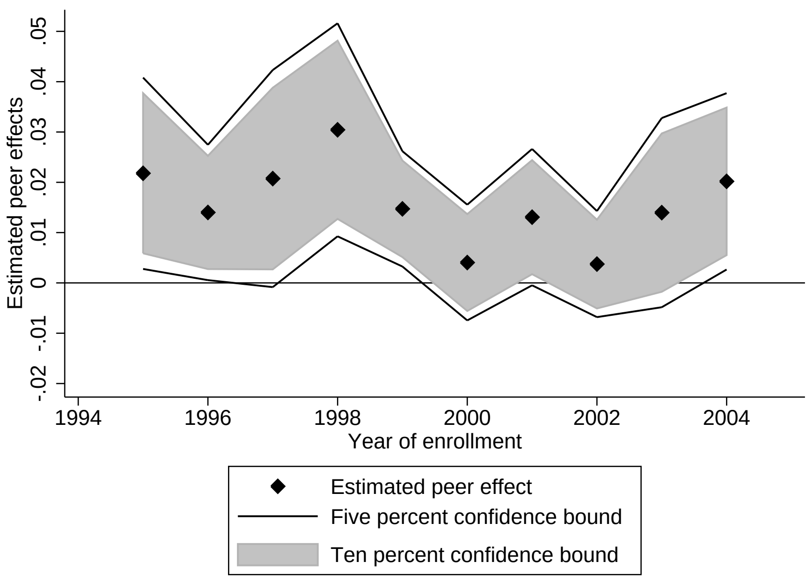
In summary, students follow the peers with whom they share similar characteristics in terms of revealed abilities and age. This result is in line with Carrell et al. (2013) who show that within randomly assigned peer groups individuals form endogenous subgroups of peers with similar ability levels. Consistent with their findings, our findings indicate that students tend to form more homogeneous sub-peer groups in terms of revealed abilities and age. Accounting for heterogeneous peer effects, the magnitude of the effect increases to 3.4-4.0 percentage points, which is in line with the broader peer-effect literature.

Dyadic Results and Heterogeneous Effects: All Cohorts

Figure 2 summarizes the key results of the dyadic regressions with respect to peer effects across all cohorts. The graph shows the estimated $\hat{\beta}$ from Equation (2) across the cohorts enrolled between 1995-2004, as well as the 5 and 10 percent confidence bounds. The dependent variable is equal to one if students i and j enroll in the same master's degree program, and zero otherwise. All estimations include peer-group fixed effects and control variables similar to the ones included in Table 4.

For the majority of years considered, we find evidence to support that students randomly assigned to the same first-year group during undergraduate studies are more likely to enroll in the same master's degree three years later. Our results are stronger for cohorts enrolled in the early years of the period. This is most likely explained by the fact that exercise groups used to be smaller, leading to stronger peer influence and/or access to the master's programs easier. Looking at the partial effects, it is also evident that the effect size found in 2001 is one of the smallest significant effects (10 percent level). Not accounting for heterogeneous peer effects, students from the same peer group are on average 1.3-3.0 percentage points more likely to specialize in the same degree.

Figure 2: Dyadic Regressions: Peer Effects in the Choice of Master's Degree (all cohorts)



Note: Linear Probability Model. Coefficient estimates for first-year peer group reported. All estimations include control variables similar to the ones included in Table 4. The estimation samples include Danish students who enrolled in undergraduate studies at CBS in the period 1995-2004.

Estimation results of Equation (3) for all 10 cohorts are represented in Tables 6-8. For the years where we find a strong peer effect (Figure 2), we also find a strong positive main effect from belonging to the same peer group when interactions with difference in first-year GPA or difference in bachelor's GPA are included. Generally, across all cohorts the results including interactions are very similar to the results found for the cohort enrolled in 2001, with even stronger results for some cohorts. For instance, we observe a positive and strongly significant gender effect for the cohort enrolled in the period between 1995-2000. This is likely to be explained by the more unequal gender distribution at CBS in the 1990s, leading to more positive assortative matching along gender dimensions.

Table 6: Heterogeneous effects: Peer effects in the choice of master's degree (all cohorts)

	1995				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.039***	0.032**	0.043**	0.025**	0.025**
	(0.013)	(0.014)	(0.017)	(0.012)	(0.012)
Abs. diff. in first-year GPA * First-year peer group	-0.007**				
	(0.003)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.005			
		(0.005)			
Abs. diff. in high school GPA * First-year peer group			-0.009*		
			(0.005)		
Same gender (=0) * First-year peer group				-0.008	
				(0.017)	
Abs. diff. in age * First-year peer group					-0.002
					(0.003)
p-value from joint test	0.009	0.052	0.037	0.075	0.079
No. of nodes	38226	38226	38226	38226	38226
	1996				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.030**	0.038***	0.010	0.030***	0.019
	(0.012)	(0.013)	(0.012)	(0.010)	(0.012)
Abs. diff. in first-year GPA * First-year peer group	-0.007				
	(0.004)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.011**			
		(0.005)			
Abs. diff. in high school GPA * First-year peer group			0.002		
			(0.004)		
Same gender (=0) * First-year peer group				-0.037**	
				(0.017)	
Abs. diff. in age * First-year peer group					-0.003
					(0.006)
p-value from joint test	0.032	0.015	0.115	0.017	0.122
No. of nodes	40186	40186	40186	40186	40186
	1997				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.042**	0.046***	0.031**	0.041***	0.027**
	(0.018)	(0.016)	(0.014)	(0.013)	(0.013)
Abs. diff. in first-year GPA * First-year peer group	-0.009*				
	(0.005)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.012**			
		(0.005)			
Abs. diff. in high school GPA * First-year peer group			-0.004		
			(0.005)		
Same gender (=0) * First-year peer group				-0.046**	
				(0.018)	
Abs. diff. in age * First-year peer group					-0.004
					(0.004)
p-value from joint test	0.069	0.017	0.075	0.007	0.134
No. of nodes	33411	33411	33411	33411	33411
	1998				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.029**	0.038***	0.026*	0.036***	0.036**
	(0.013)	(0.014)	(0.015)	(0.011)	(0.015)
Abs. diff. in first-year GPA * First-year peer group	0.001				
	(0.004)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.003			
		(0.004)			
Abs. diff. in high school GPA * First-year peer group			0.002		
			(0.004)		
Same gender (=0) * First-year peer group				-0.013	
				(0.014)	
Abs. diff. in age * First-year peer group					-0.003
					(0.005)
p-value from joint test	0.018	0.013	0.017	0.005	0.019
No. of nodes	52326	52326	52326	52326	52326

Note: Linear Probability Model. All estimations include control variables similar to the ones included in Table 4. The estimation samples include Danish students who enrolled in undergraduate studies at CBS in the period 1995-2004. The reported p-value is from the test of joint significance of the peer effect and the interaction effect, H_1 where $H_1 : \beta_{g_{ij,h}} = 0, \delta_{interaction} = 0$. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Heterogeneous effects: Peer effects in the choice of master's degree (all cohorts)

	1999				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.015*	0.023**	0.018*	0.026***	0.019**
	(0.009)	(0.010)	(0.010)	(0.010)	(0.008)
Abs. diff. in first-year GPA * First-year peer group	-0.000				
	(0.002)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.004			
		(0.003)			
Abs. diff. in high school GPA * First-year peer group			-0.002		
			(0.003)		
Same gender (=0) * First-year peer group				-0.026*	
				(0.015)	
Abs. diff. in age * First-year peer group					-0.002
					(0.004)
p-value from joint test	0.035	0.032	0.042	0.022	0.025
No. of nodes	62128	62128	62128	62128	62128
	2000				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.012	0.012	0.011	0.013*	0.005
	(0.010)	(0.010)	(0.010)	(0.008)	(0.008)
Abs. diff. in first-year GPA * First-year peer group	-0.003				
	(0.003)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.004			
		(0.003)			
Abs. diff. in high school GPA * First-year peer group			-0.003		
			(0.003)		
Same gender (=0) * First-year peer group				-0.021*	
				(0.012)	
Abs. diff. in age * First-year peer group					-0.000
					(0.005)
p-value from joint test	0.459	0.394	0.565	0.174	0.758
No. of nodes	63190	63190	63190	63190	63190
	2001				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.034***	0.040***	0.004	0.015	0.024***
	(0.013)	(0.013)	(0.009)	(0.009)	(0.009)
Abs. diff. in first-year GPA * First-year peer group	-0.009**				
	(0.004)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.013***			
		(0.004)			
Abs. diff. in high school GPA * First-year peer group			0.004		
			(0.003)		
Same gender (=0) * First-year peer group				-0.004	
				(0.012)	
Abs. diff. in age * First-year peer group					-0.006*
					(0.003)
p-value from joint test	0.025	0.006	0.065	0.167	0.021
No. of nodes	58653	58653	58653	58653	58653
	2002				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.005	0.005	-0.003	0.001	0.003
	(0.008)	(0.008)	(0.006)	(0.008)	(0.011)
Abs. diff. in first-year GPA * First-year peer group	-0.000				
	(0.003)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.000			
		(0.003)			
Abs. diff. in high school GPA * First-year peer group			0.003		
			(0.003)		
Same gender (=0) * First-year peer group				0.005	
				(0.010)	
Abs. diff. in age * First-year peer group					0.000
					(0.005)
p-value from joint test	0.765	0.742	0.456	0.619	0.746
No. of nodes	57630	57630	57630	57630	57630

Note: Linear Probability Model. All estimations include control variables similar to the ones included in Table 4. The estimation samples include Danish students who enrolled in undergraduate studies at CBS in the period 1995-2004. The reported p-value is from the test of joint significance of the peer effect and the interaction effect, H_1 where $H_1 : \beta_{g_{ij,h}} = 0$, $\delta_{interaction} = 0$. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Heterogeneous effects: Peer effects in the choice of master's degree (all cohorts)

	2003				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.017	0.026	0.030*	0.009	0.031*
	(0.016)	(0.017)	(0.017)	(0.011)	(0.017)
Abs. diff. in first-year GPA * First-year peer group	-0.001				
	(0.004)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.005			
		(0.005)			
Abs. diff. in high school GPA * First-year peer group			-0.007		
			(0.006)		
Same gender (=0) * First-year peer group				0.013	
				(0.011)	
Abs. diff. in age * First-year peer group					-0.011*
					(0.006)
p-value from joint test	0.346	0.249	0.178	0.144	0.182
No. of nodes	45150	45150	45150	45150	45150
	2004				
	(1)	(2)	(3)	(4)	(5)
First-year peer group	0.036***	0.049***	0.018	0.018	0.009
	(0.013)	(0.015)	(0.011)	(0.011)	(0.011)
Abs. diff. in first-year GPA * First-year peer group	-0.006*				
	(0.003)				
Abs. diff. in bachelor's GPA * First-year peer group		-0.013***			
		(0.004)			
Abs. diff. in high school GPA * First-year peer group			0.001		
			(0.004)		
Same gender (=0) * First-year peer group				0.006	
				(0.013)	
Abs. diff. in age * First-year peer group					0.009
					(0.007)
p-value from joint test	0.020	0.003	0.075	0.059	0.049
No. of nodes	43956	43956	43956	43956	43956

Note: Linear Probability Model. All estimations include control variables similar to the ones included in Table 4. The estimation samples include Danish students who enrolled in undergraduate studies at CBS in the period 1995-2004. The reported p-value is from the test of joint significance of the peer effect and the interaction effect, H_1 where $H_1 : \beta_{g_{ij,h}} = 0$, $\delta_{interaction} = 0$. *** p<0.01, ** p<0.05, * p<0.1

5.3 Robustness

In this section, we present a series of robustness checks to test the sensitivity of our results as well as to give a sense of the assumptions on which the results rely.

We purposefully excluded non-Danish students in the analysis due to a lack of background information such as parents' educational level and place of residence 10 years prior to enrollment. In the supplementary material (Appendix B) to this paper we re-estimate Equation (2) using the full sample of students. We find that students randomly assigned to the same exercise class upon enrollment at the bachelor's level are more likely to study for the same degree, however the estimates are slightly weaker in significance and smaller in magnitude. A possible explanation is that foreign students studying in Denmark are likely to have chosen CBS for a specific reason or master's degree, and thus are less likely to be prone to peer influence.

Second, to ensure that our results are not driven by a selective sample of students who took their undergraduate and postgraduate degrees at CBS, we expand the sample to include (i) students who did not continue into a master's program at CBS, and (ii) students who enrolled in one of the "non-traditional" master's degrees listed in Table A.1. Results are shown for all cohorts in Figure A.1. Across all cohorts, our baseline results remain and we find indications of that students belonging to the same first-year peer group are more likely to enroll in the same master's degree three years later.

Third, to validate our results we construct placebo peer groups by randomly assigning students to hypothetical classes and re-estimate Equation (2) with the placebo peer groups as our main explanatory variable. We expect to find no significant effect from the placebo group variable in the presence of a true peer effect from our original first-year peer effect variable. The estimated peer effects across all cohorts from 6 different placebo estimations are shown in Figure A.2. Across almost all estimations there is no significant effect from being assigned to artificial peer groups. The results indicate that, on average across estimations, we would see no significant placebo peer effect. In fact, the magnitude of the results is much closer to zero than the baseline results reported in Figure 2. The significance of the control variables does not change as expected. This suggests that our finding is an actual peer-group effect. However, with this way of constructing artificial peer groups, we risk combining students in placebo peer groups that are in fact from the same original peer group, which could drive a significant result. Thus, for further research, alternative ways of creating placebo groups should be considered.

While the randomly assigned first-year classes are meant to capture the network in which students interact academically and socially, studies have shown that the peer effects are working through smaller sub-units such as study partnerships (Carrell et al., 2009) and roommates (Sacerdote, 2001). Common to these findings is that students interact more regularly with their study partners and their roommates as compared to the larger social group in which they are placed. To investigate the hypothesis that peer effects are stronger among students who interact more we experiment with additional measures of peer groups. We construct two alternative, and possibly stronger, peer group measures for the cohort enrolled in 2001. These measures are based on the elective courses taken throughout the second and third year of the bachelor's studies.

The first and least restrictive measure requires that classmates have attended at least one common elective course. More formally, individual i's peer group ($\tilde{g}_{ij,h}$) includes all individuals j who were randomly assigned to the same class h as individual i in the first year, and have attended at least one common elective course during the second or third year. The second measure is more restrictive and requires that classmates in the first year attend at least two elective courses together.[26] If students

[26]The maximim number of elective courses in the Business Economics program is five, and thus in order to ensure at least one sub-group within each first-year peer group we do not allow for a measure based on three common elective

interact more intensively with students they spend more time with and/or share similar interest with, then we expect the peer effect to be stronger. However, given the voluntary nature of the elective courses on which we base the sub-peer group measures, we face the problem of self-selection causing the estimates to be upward biased. It is important to note that the selection is limited to within first-year assigned peer groups.

Estimation results including our preferred first-year peer-group measure and additional control variables for the 2001 cohort are reported in Table A.3. We find that both the smaller peer-group measures are positive and statistically significant. In other words, we find an additional effect from taking at least one elective course together beyond the observed effect from belonging to the same first-year peer group. Particularly, we find that students who had at least one elective course together at some point during their undergraduate studies are 6.4 percentages point more likely to enroll in the same master's degree program. Compared to the partial effects estimated from Equations (2) and (3), this is a large yet comparable peer effect to the effect found by De Giorgi et al. (2010).

The last central threat to the definition of our peer-group measure is measurement error. Sojourner (2013) and Ammermueller and Pischke (2009) discuss identification of peer effects in the presence of measurement error generated by misresponses and/or missing values leading to incomplete peer groups. The first type of measurement error is less of a concern in this paper as we exclusively use data from the Danish register and the university administration database. The second type of measurement error is more relevant as our measure of peer groups may not include the relevant set of peers. For instance, if the relevant peer group includes friends outside the assigned first-year group.

Another possibility is that students unofficially change group or follow different exercise classes in some of the subjects. This might occur if some teaching assistants are better or due to incompatibility between the course schedule for exercise classes in the students' first-year group and their part-time job. We expect this problem to be less pronounced in the first year or at least the first semester when the institutional framework is still new to the students, which we expect makes students less likely to unofficially shop between exercise classes. If the measurement error is random in any of the two cases, which is not unlikely, then the OLS underestimate the true effect (i.e. attenuation bias). Thus, presence of a random measurement error in our peer-group measure means that the true coefficient is larger in absolute value. The estimated effect of 1.3-3.0 percentages points should therefore be regarded as a lower bound estimate of the peer effect in degree choice.

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courses.

6 Does Schooling Choice Lead to Inefficiencies?

We now proceed to analyze students' educational achievements in their master's studies and their subsequent labor market outcome based on how they chose their degree. We distinguish between two decision modes, namely whether they choose a specific master's degree based on their own revealed abilities or based on their peers' behavior.

We follow a similar approach to De Giorgi et al. (2010) and create indicators of whether students follow their peers, their potential, both, or neither in the choice of degree. In contrast to De Giorgi et al. (2010), we consider a larger number of groups based on a larger number of master's degrees and measure abilities relative to the peer group rather than the entire cohort. The number of groups is determined by the number of master's degree programs offered each year. Concretely, we first construct a measure of "peer impact", f_i, and then a measure of "relative ability," q_i. Combining these measures we divide students into four different decision modes.

The measure of peer impact, f_i, measures the relative fraction of peers who choose the same degree program. If student i chose the master's degree k, then f_i captures the fraction of i's peers who also choose degree k relative to the entire cohort. Formally, f_i is defined as follows:

$$f_i = \frac{N_h^{-1} \sum 1_{[m_{j,h}=k]}}{N^{-1} \sum 1_{[m_j=k]}} \text{ if } m_i = k \tag{4}$$

where k is an indicator of the master's degree, m_i is the indicator of individual i's degree choice, while h captures the first-year peer group. Thus, if $f_i > 1$, it means that in i's peer group the share of individuals who choose degree k is higher than the average share in the cohort. Hence, $f_i > 1$ is taken as suggested evidence that individual i followed his/her peers in choice of specialization.

The measure of relative ability, q_i, measures individual i's relative educational achievement in mandatory courses relevant for individual i's chosen master's degree. If student i chose the master's degree k, then q_i is computed as the ratio between i's average grade in the mandatory courses relevant for degree k and i's average grade in the other mandatory courses. We normalize this measure by the relative performance of i's peer group. We choose the reference group as i's classmates the first year of the undergraduate studies, i.e. our preferred peer-group measure, as these individuals are more likely to be the group of students with whom i compares him/herself.[27] Formally, q_i is defined as follows:

$$q_i = \frac{GPA_i^{R_k}}{GPA_i^{O_k}} \times \frac{\sum GPA_j^{O_k}}{\sum GPA_j^{R_k}} \text{ if } m_i = k,\ \forall\ i,j\ i \neq j\ \&\ g_{ij,h} = 1 \tag{5}$$

[27]The subsequent results are not sentitive to the choice of reference group.

where R_k is the set of relevant mandatory undergraduate courses for master's k, O_k is the set of other mandatory undergraduate courses, and $g_{ij,h} = 1$ indicate that i and j belong to the same peer group h. If $q_i > 1$, it means that individual i performed better in courses relevant for the chosen degree compared to i's peers.[28] Thus, if $q_i < 1$, we interpret it as evidence that individual i did not follow his/her potential.

We constructed the sets R_k and O_k based on answers about the most important mandatory courses from the responsible professor of each of the master's programs offered at CBS. For four master's programs, it was either not possible to obtain the list of the most important mandatory courses or the degree were no longer offered, in which case the responsible person could not be identified. In these cases, we used the online available enrollment requirements to create the set of relevant courses. Moreover, we consider only students who chose one of the 15 most commonly chosen degrees (see Table A.1). More details about the mandatory courses in Business Economics and the master's degree programs offered can be found in the supplementary material.

Based on the indicators q_i and f_i we create four groups similar to De Giorgi et al. (2010). For each of the groups we create a dummy variable taking the value 1 if the student belongs to the decision mode, and zero otherwise. The different decision modes are as follows:

- Following one's peers and potential: $D^{both} = 1$ if $f_i > 1 \wedge q_i > 1$
- Following one's potential but not peers: $D^{potential} = 1$ if $f_i < 1 \wedge q_i > 1$
- Following one's peers but not potential: $D^{peers} = 1$ if $f_i > 1 \wedge q_i < 1$
- Following neither one's potential nor peers: $D^{none} = 1$ if $f_i < 1 \wedge q_i < 1$

The distribution of students into the different decision modes are reported in Table 9. Most students belonging to $D^{potential}$ (32 percent) and fewest students neither follow their peers nor their potential D^{none} (17 percent). Some 21 percent follow their peers and 30 percent follow both their abilities and their peers.

[28] Student i performed better in courses relevant for the chosen degree relative to i's peers whenever $\frac{GPA_i^{R_k}}{GPA_i^{O_k}} > \frac{\sum GPA_j^{R_k}}{\sum GPA_j^{O_k}} \Rightarrow q_i > 1$

Table 9: Grouping by decision mode

	Followed peers: $f_i > 1$	Did not follow peers: $f_i < 1$	Total
Followed potential: $g_i > 1$	943	1003	1,946
	(30.1)	(32.02)	
Did not follow potential: $g_i < 1$	644	542	1,186
	(20.56)	(17.31)	
Total	1,587	1,545	3,132

Note: Frequency. Percentages reported in parenthesis. $g_i < 1$ indicate that i did not follow its potential and $g_i > 1$ indicate that i followed its potential. The sample includes cohorts of Danish students enrolled between 1995-2004.

We use these groups to estimate the association between these decision modes and different outcomes y_i. Formally, we estimate the following equation using standard OLS estimation procedures:

$$y_i = \alpha_0 + \alpha_1 D_i^{none} + \alpha_2 D_i^{peer} + \alpha_3 D_i^{both} + \gamma X_i + \delta_m + \eta_l + \theta_t + \mu_i \tag{6}$$

where the reference group is the students who follow their potential, i.e. $D^{potential}$. The vector X_i contains individual specific characteristics including a gender dummy, students' enrollment age, bachelor's GPA, high school GPA, and a dummy for whether they went to a business high school.[29] We also include master's degree fixed effects (δ_m), as well as location fixed effects defined as the place of residence 10 years prior to enrollment (η_l) and enrollment year fixed effects (θ_t). The different outcomes (y_i) considered include educational achievement measured in terms of students' final master's GPA, dropout rates, and labor market performance measured as the hourly wage in the first job after graduation. When we consider the relationship between starting wages and decision mode about a degree, we also include sector occupation dummies.

6.1 Results

Estimation results are presented in Table 10. Columns 1, 3, and 5 include only the dummies for the different decision modes, while columns 2, 4, and 6 extend the specification with control variables.

The coefficient estimate on D^{peers} is negative across all estimations. In contrast to the findings by De Giorgi et al. (2010), the effect is not statistically significant for educational performance and dropout. This result is likely to be explained by our previous findings. Here, we find that peer effects are stronger among students who share similar abilities. If the students who follow their peers benefit from efficient study partnerships with their peers, then the consequences from following peers might

[29] When included in Equation (6), bachelor's and high school GPA are not transformed to the "7"-step scale.

not be an adverse impact on performance. This could potentially cause students to perform even better despite the fact that they did not choose a master's degree in which they had a relative ability advantage.

Table 10: Academic and labor market outcomes on decision modes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Master's degree GPA		Drop out		Starting wage (log)	
D^{none}	-0.198*	-0.115	-0.006	-0.008	-0.008	-0.006
	(0.105)	(0.084)	(0.018)	(0.018)	(0.014)	(0.014)
D^{peer}	-0.153	-0.106	-0.009	-0.009	-0.029*	-0.027*
	(0.098)	(0.082)	(0.017)	(0.017)	(0.016)	(0.016)
D^{both}	-0.036	0.011	-0.003	-0.000	-0.012	-0.009
	(0.089)	(0.072)	(0.015)	(0.015)	(0.012)	(0.012)
Starting age		-0.043**		0.011**		0.006*
		(0.020)		(0.004)		(0.003)
GPA from master's degree						0.010***
						(0.003)
GPA from bachelor's degree		1.146***		-0.077***		
		(0.043)		(0.010)		
GPA from high school		0.353***		0.004		0.013*
		(0.046)		(0.010)		(0.007)
Business high school dummy		-0.196***		0.028*		-0.005
		(0.073)		(0.016)		(0.012)
Gender, female=1		-0.038		-0.038***		-0.056***
		(0.063)		(0.013)		(0.011)
Mother self-employed		0.151		0.023		0.024
		(0.135)		(0.030)		(0.021)
Father self-employed		0.142		-0.003		-0.014
		(0.088)		(0.019)		(0.017)
Master's degree fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Location fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Starting year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Parental education fixed effect	Yes	Yes	Yes	Yes	No	No
Sector fixed effect	No	No	No	No	Yes	Yes
Observations	2633	2633	3132	3132	1854	1854

Note: OLS. Standard errors reported in parenthesis. Estimation sample includes cohorts of Danish students enrolled in the period 1995-2004. The reference group is ability-driven individuals ($D^{potential}$). In the wage regression wage observations below and above the 1 % and 99 % percentile, respectively, are dropped. *** p<0.01, ** p<0.05, * p<0.1

The coefficient estimate on D^{peers} is negative and significant at the 10 percent level when starting wages are model. Thus, even though following peers is not negatively associated with educational performance, it is negatively reflected in starting wages. If students follow peers into less profitable master's programs, this might explain this results.

Finally, we do observe a borderline significant and negative effect from neither following peers nor potential (D^{none}) on master's GPA, indicating an adverse impact relative to following one's potential.

However, this effect does not stay significant when individual specific controls are included.

We test the sensitivity of the results in two ways. First, the measure of relative abilities (q_i) might be measured with error for the master's programs for which we have used the enrollment criteria specified online. To test the sensitivity of the results, we exclude students enrolled in programs where the relevant set of courses is not based on answers from the responsible professor. The estimation results are found to be slightly stronger, but does not change qualitatively as shown in Table A.4. Second, the consequences from following one's peers might differ for non-Danish students. Estimation results on the full sample including non-Danish students are shown in the supplementary material to this paper. Again, we find that our main results hold.

7 Conclusion

In this paper we have examined whether students' choices of a master's degree are influenced by the behavior of peers. It is widely recognized that peers influence individual outcomes such as student performance (e.g., Sacerdote, 2011; Lavy et al., 2012; Carrell et al., 2013) while the role of peers in students' educational decision about specialization is less well-documented. Previous studies have shown that roommate peer effects are not important in determining choice of major (Sacerdote, 2001), while classroom peers are important among undergraduates when choosing their major (De Giorgi et al., 2010). While the later study documents the importance of peers in major choice, it does not consider heterogeneous effects within peer-group influence.

To our knowledge, this paper constitutes the first attempt to use a novel testing strategy—while accounting for endogeneity of peers—to distinguish assortative criteria in order to analyze whether peer influence or individual characteristics determine students' choice of master's degree. We subsequently examine whether peer influence is heterogeneous and the consequences for future educational achievement and labor market outcomes. The data requirements for testing the importance of peers in degree choice and impact on labor market outcomes are quite daunting; however, we solve this by using administrative university data that follow 10 cohorts of students through their entire study lifecycle (approximately five years) combined with Danish register data.

At the start of the first year of undergraduate studies, students are randomly assigned to exercise classes in which the vast majority of students stay throughout their entire bachelor's studies. Using first-year exercise classes as our preferred peer-group measure, we find that peers are important for students' postgraduate choice. However, the effect is particularly strong among students who share similar abilities. We find that students revealed abilities measured as first-year GPA and bachelor's GPA as well as age are most important. Contrary to expectations, we do not find that similarities in other individual or parental characteristics explain degree choice. In the second part of the analysis, we find that the decision to follow peers has only limited impact on academic achievement and no impact on dropout rates and students' starting wage.

Overall, the result demonstrates that even within a group of highly selected university students, peer effects are important to understand not only regarding students' outcomes, but also regarding central educational decisions such as the choice of a master's degree. We conclude that peer influence in degree choice is not a likely explanation for why such decisions are sometimes found to be inefficient (e.g., Rochat and Demeulemeester, 2001; Robst, 2007). One explanation supported by the evidence in this paper is that students are more likely to follow peers with similar revealed abilities, limiting the adverse consequences from their master's degree choice on educational performance and starting wage after graduation. Another potential explanation is that following peers into postgraduate studies make it easier to form study partnerships, which might have a positive impact on students' outcome.

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Appendix A Additional Estimations and Descriptive Statistics

Table A.1: List of Master's Programs Offered by Students Characteristics

Degree (k)	Name of master's degree program	All cohorts			2001 cohort		
		Frequency	Percent	Bachelor's GPA	Frequency	Percent	Bachelor's GPA
M1	M.Sc. in Business Administration and Auditing (CMA)	405	12.93	6.15	45	13.12	5.97
M2	M.Sc. in Applied Economics & Finance (CMAEF)	189	6.03	7.91	24	7.00	7.15
M3	M.Sc. in Design and Communication Management (CMDCM)	170	5.43	5.43	*		
M4	M.Sc. in Economic Marketing (CMEMF)	644	20.56	5.81	69	20.12	5.93
M5	M.Sc. in Management Accounting & Control (CMEST)	156	4.98	6.42	15	4.37	6.09
M6	M.Sc. in Finance and Accounting (CMFR)	574	18.33	7.40	72	20.99	6.69
M7	M.Sc. in Finance & Strategic Management (CMFSM)	113	3.61	7.63	19	5.54	8.17
M8	M.Sc. in Human Resource Management (CMHRM)	118	3.77	5.84	*		
M9	M.Sc. in International Business (CMIBS)	68	2.17	5.92	8	2.33	6.55
M10	M.Sc. in International Marketing and Management (CMIMM)	196	6.44	8.19	13	3.79	6.45
M11	M.Sc. in Marketing Communications Management (CMMCM)	36	1.15	6.28	10	2.92	6.04
M12	M.Sc. in Management of Innovation & Business Development (CMMIB)	57	1.82	6.03	15	4.37	5.88
M13	M.Sc. in Management of Technology (CMMOT)	43	1.37	6.41	*		
M14	M.Sc. in Supply Chain Management (CMSCM)	229	7.31	5.87	36	10.50	6.2
M15	M.Sc. in Business Administration and Organization (CMSOL)	134	6.04	8.03	5	1.46	7.82
		3,132			343		

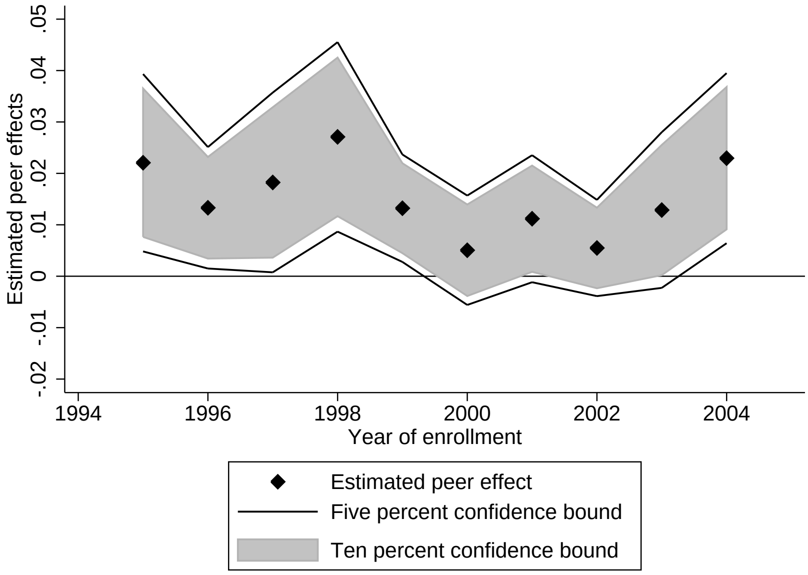
Note: The name of master's degree program in parenthesis refer to the Danish abbreviation of the degree. "*" means that we are not able to disclose the information due to discretion roles set by Statistics Denmark. // Less than 1.25-percent of the students are registered with one of the following master's programs: M.Sc. in Strategic Market Creation, M.Sc. in International/Industrial Marketing and Purchasing, M.Sc. in Strategy, Organization and Leadership, M.Sc. in Business Economics and Computer Science, M.Sc. in Economics of International Strategy and Governance, M.Sc. in Business Economics and Philosophy, M.Sc. in Business Administration and Modern Languages, M.Sc. in Business Economics and Business Law, and M.Sc. in Creative Business Processes. For this reason they are excluded from the main analyses. As a robustness check we include these master's programs - see Figure A.1.

Table A.2: Descriptive Statistics: Dyad Level

Enrollment year (i.e. cohort):	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004
Individual characteristics:										
Starting age (years)	21.00	21.06	21.14	21.33	21.16	21.47	21.37	21.34	21.38	21.09
	(1.85)	(1.34)	(1.55)	(1.51)	(1.41)	(1.58)	(1.62)	(1.48)	(1.85)	(1.35)
Gender, female=1	0.27	0.28	0.28	0.33	0.32	0.31	0.35	0.36	0.27	0.25
	(0.44)	(0.45)	(0.45)	(0.47)	(0.47)	(0.46)	(0.48)	(0.48)	(0.44)	(0.44)
Academic characteristics:										
GPA from high school	6.85	6.77	7.04	7.11	6.79	6.93	6.89	6.99	6.97	7.20
	(2.11)	(1.96)	(1.89)	(2.11)	(1.90)	(2.00)	(1.85)	(1.89)	(1.89)	(1.96)
GPA from master's degree	4.12	4.69	5.24	5.70	5.56	5.52	5.19	5.67	5.57	5.44
	(2.28)	(2.13)	(2.15)	(2.04)	(2.18)	(2.25)	(2.04)	(2.30)	(2.30)	(2.17)
GPA from bachelor's degree	5.29	6.05	5.80	6.78	6.99	6.53	6.40	6.59	6.80	6.86
	(1.84)	(2.10)	(1.92)	(2.31)	(2.10)	(1.99)	(1.81)	(2.05)	(2.13)	(2.06)
Business high school (=1)	0.35	0.38	0.33	0.31	0.24	0.26	0.21	0.19	0.15	0.17
	(0.48)	(0.49)	(0.47)	(0.46)	(0.43)	(0.44)	(0.41)	(0.39)	(0.35)	(0.38)
Normal (mat) high school (=1)	0.59	0.52	0.59	0.53	0.66	0.22	0.66	0.72	0.78	0.73
	(0.49)	(0.50)	(0.49)	(0.50)	(0.48)	(0.41)	(0.47)	(0.45)	(0.41)	(0.44)
No. of nodes	38226	40186	33411	52326	62128	63190	58653	57630	45150	43956

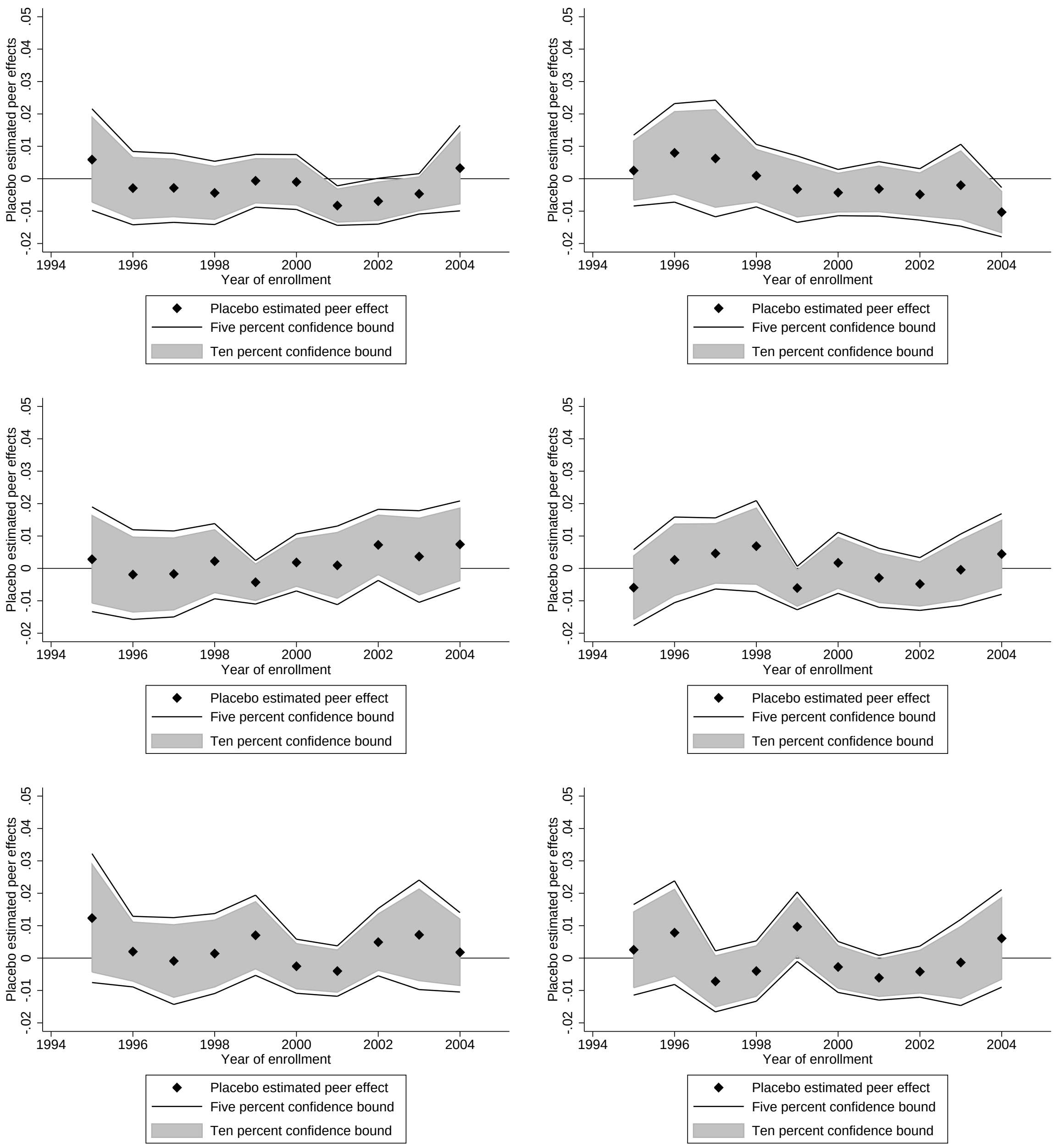
Note: Means. Standard deviations reported in parenthesis. The samples include cohorts Danish students who enrolled in undergraduate studies at CBS.

Figure A.1: Robustness: Peer Effects in the Choice of Master's Degree



Note: Linear Probability Model. Coefficient estimates for first-year peer group reported. All estimations include control variables similar to the ones included in Table 4. The estimation samples include students that did not continue with a master's degree at CBS or graduated from one of the master's degree program reported in footnote to Table A.1.

Figure A.2: Robustness: Placebo peer effects in the choice of master's degree



Note: Linear Probability Model. Coefficient estimates for first-year peer group reported. All estimations include control variables similar to the ones included in Table 4. The estimation samples include Danish students who enrolled in undergraduate studies at CBS in the period 1995-2004.

Table A.3: Robustness: Alternative peer-group measures in choice of master's degree

	(1)	(2)	(3)	(4)	(5)	(6)
At least one common elective	0.064***	0.064***	0.063***			
	(0.012)	(0.012)	(0.012)			
At least two common electives				0.132***	0.131***	0.126***
				(0.026)	(0.026)	(0.026)
First-year peer group		0.010	0.010		0.011	0.011*
		(0.007)	(0.007)		(0.007)	(0.007)
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	No	Yes	No	No	Yes
Observations	58,653	58,653	58,653	58,653	58,653	58,653

Note: Linear Probability Model. Dependent variable is equal to 1 if i and j are enrolled in the same master's program. Standard errors reported in parenthesis. Estimation sample includes Danish students enrolled in undergraduate studies at CBS in 2001. All estimations include control variables similar to Table 4. *** p$<$0.01, ** p$<$0.05, * p$<$0.1

Table A.4: Robustness: Academic and labor market outcomes on decision modes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Master's degree GPA		Drop out		Starting wage (log)	
D^{none}	-0.249**	-0.152*	-0.017	-0.018	-0.007	-0.004
	(0.112)	(0.089)	(0.019)	(0.019)	(0.014)	(0.014)
D^{peer}	-0.179*	-0.120	-0.014	-0.014	-0.033*	-0.031*
	(0.106)	(0.088)	(0.018)	(0.018)	(0.017)	(0.017)
D^{both}	-0.061	0.013	0.006	0.007	-0.011	-0.008
	(0.096)	(0.077)	(0.016)	(0.016)	(0.012)	(0.012)
Starting age		-0.053**		0.011**		0.006*
		(0.022)		(0.005)		(0.003)
GPA from master's						0.009**
						(0.004)
GPA from bachelor's		1.182***		-0.077***		
		(0.046)		(0.010)		
GPA from high school		0.357***		0.009		0.016**
		(0.049)		(0.010)		(0.008)
Business high school dummy		-0.182**		0.021		-0.003
		(0.079)		(0.017)		(0.012)
Gender, female=1		-0.080		-0.038***		-0.050***
		(0.068)		(0.014)		(0.011)
Mother as self-employed		0.211		0.038		0.023
		(0.150)		(0.033)		(0.021)
Father as self-employed		0.200**		-0.016		-0.015
		(0.091)		(0.020)		(0.018)
Master's degree fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Location fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Starting year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Parental education fixed effect	Yes	Yes	Yes	Yes	No	No
Sector fixed effect	No	No	No	No	Yes	Yes
Observations	2292	2292	2717	2717	1641	1641

Note: OLS. Standard errors reported in parenthesis. Sample includes cohorts of Danish students enrolled in undergraduate studies at CBS in the period 1995-2004. The reference group is ability driven individuals ($D^{potential}$). In the wage regression wage observations below and above the 1 % and 99 % percentile, respectively, are dropped. We exclude students enrolled in programs where the relevant set of courses is not based on answers from the responsible professor. We exclude students that enrolled in the master's programs M3, M9, M13, and M15 due to the low share that graduated - for details of the programs see Table A.1. *** p<0.01, ** p<0.05, * p<0.1.

Supplementary Material (online) for: Do Peers Matter? - Impacts of Peers on Master's Choice and Labor Market Outcomes

Benedikte Bjerge
University of Copenhagen*

Marie Skibsted
Copenhagen Business School†

June 7, 2016

Abstract

This appendix meant for online access only contains supplementary material to accompany the paper "Do Peers Matter? - Impacts of Peers on Master's Choice and Labor Market Outcomes". The appendix is organized as follows. The first section describe the institutional details, provide a list of mandatory courses and the different master programs offered at Copenhagen Business School (CBS), the largest business school in Denmark. Section B.2 discusses peer group composition and transition. Estimation results including non-Danish students are presented and summarized in Section B.3, while Section B.4 compares the Danish grading system to the ECTS scale and the American grading system.

*bab@ifro.ku.dk. Department of Food and Resource Economics, Rolighedsvej 25, 1958 Frederiksberg C, Denmark.

†mks.eco@cbs.dk. Department of Economics, Copenhagen Business School, Porcelænshaven 16A, 2000 Frederiksberg, Denmark

Appendix B Supplementary Material

B.1 Institutional Details and Master's Programs Offered

Copenhagen Business School (CBS) is the largest business school in Denmark. The largest undergraduate program at CBS is Business Economics, and the focus of this paper. It takes three years to complete the undergraduate program, after which students can enroll in one of the master's degrees offered at CBS, at a different institution, or apply for a job. Contrary to higher-education traditions in many parts of the world, the vast majority of bachelor's students also enroll in a two-year master's program. Part of the explanation is that it is not commonly accepted that a bachelor's degree is sufficient in order to find a relevant job with a Danish employer. More than 90 percent of the undergraduate students who enrolled in and subsequently graduated from the Business Economics bachelor's program at CBS in the period 1995-2004 choose to continue in one of the two-year master's programs offered by CBS.

During the bachelor's program (i.e. first three years), students primarily have mandatory courses. Table B.1 provides a list of mandatory courses and specifies the cohorts for which the course was mandatory. For instance, Financing was a mandatory course for all cohorts enrolled in Business Economics in the academic years between 1995-2004, while Information Technology was mandatory only for cohorts enrolled after 1996.

In the second part of the analysis in the paper, we investigate whether choice of master's program leads to inefficiencies in terms of educational achievements and subsequent labor market outcomes. We construct a measure of revealed ability (q_i) measured as peers' relative educational achievement in relevant mandatory courses. To identify the relevant mandatory courses we first drop the master's programs that are not specifically targeted on students at the Business Economics bachelor's program. Students in these master's programs constitute less than 1.29 percent of the sample. For the remaining master's programs we ask the responsible professor for each master's program to select up to four courses from a roster list of mandatory courses (listed in Table B.1). In four cases we were not able to obtain information from the responsible professor. For these master's programs we identify the set of relevant mandatory courses based on the enrollment requirements for each of the programs specified online. Table B.2 lists the relevant courses by master's program and specify whether the information is obtained from descriptions of enrollment requirements. Also, it specifies the years for which the master's program was offered at CBS. We test the sensitivity of the results by excluding students enrolled in master's programs where the relevant set of courses was not based on answers from the responsible professor.

Table B.1: List of All Mandatory Courses in Business Economics

	Mandatory courses in 1995	Mandatory courses in 1996-2000	Mandatory courses in 2001-2004
1	Marketing and strategy of the firm	Marketing	Marketing
2	Method	Empirical economics and methodology	Empirical economics and methodology
3	Financial accounting	Financial accounting	Financial accounting
4	Business law	Business law	Business law
5	Financing	Financing	Financing
6		Information technology	Information technology
7		New Economic and contextual theory about firms	Contextual theory about firms
8	Macroeconomics	Macroeconomics	Macroeconomics
9	Business economics	Microeconomics	Microeconomics
10	Organization	Organization	Organizational methods (methodological part)
11			Organizational methods (operational part)
12	Statistics	Statistics	Statistics
13		Company's decision analysis	Company's decision analysis
14	Management accounting	Management accounting	Management accounting

Note: For the 1995 cohort "Business and sociography" and "Data processing" were also mandatory. For the 1996 and 1997 cohort "Business economics" was also mandatory.

Table B.2: List of Master's Programs Matched with Relevant Mandatory Courses

Degree (k)	Name of master's degree program	Relevant mandatory courses	Enrollment requirements	Offered
M1	M.Sc. in Business Administration and Auditing (CMA)	Financial accounting, Business Law, Management accounting		1995-2004
M2	M.Sc. in Applied Economics & Finance (CMAEF)	Financing, Macroeconomics, Microeconomics, Statistics		1995-2004
M3	M.Sc. in Design and Communication Management (CMDCM)	-	Yes	1995-2001
M4	M.Sc. in Economic Marketing (CMEMF)	Marketing, Organizational methods (operational part)		1995-2004
M5	M.Sc. in Management Accounting & Control (CMEST)	Contextual theory about firms, Organizational methods (operational part), Company's decision analysis, Management accounting		1995-2002
M6	M.Sc. in Finance and Accounting (CMFR)	Financial accounting, Business law, Financing, Statistics		1995-2004
M7	M.Sc. in Finance & Strategic Management (CMFSM)	Empirical economics and methodology, Financing, Contextual theory about firms, Statistics		1999-2004
M8	M.Sc. in Human Resource Management (CMHRM)	Contextual theory about firms, Organizational methods (methodological part), Organizational methords(operational part), Company's decision analysis		1995-2004
M9	M.Sc. in International Business (CMIBS)	-	Yes	1995-2004
M10	M.Sc. in International Marketing and Management (CMIMM)	Contextual theory about firms, Marketing, Organizational methods (methodological part), Company's decision analysis		1995-2004
M11	M.Sc. in Marketing Communications Management (CMMCM)	Marketing, Empirical economics and methodology, Company's decision analysis		2000-2004
M12	M.Sc. in Management of Innovation & Business Development (CMMIB)	Information technology, Contextual theory about firms, Organizational methods (operational part), Company's decision analysis		1999-2004
M13	M.Sc. in Management of Technology (CMMOT)	-	Yes	1995-1999
M14	M.Sc. in Supply Chain Management (CMSCM)	Empirical economics and methodology, Microeconomics, Company's decision analysis		1995-2004
M15	M.Sc. in Business Administration and Organization (CMSOL)	-	Yes	1995-2001

Note: The name of master's degree program in parenthesis refer to the Danish abbreviation of the degree. Relevant mandatory courses reported by the responsible professor of each of the master's degrees offered. For master's degrees where the responsible professor was not located, we use online reported enrollment requirements. "Offered" refer to the period in which the different degrees were offered to graduate students at CBS. Less than 1.29-percent of the students are registered with one of the following master's programs: M.Sc. in Strategic Market Creation, M.Sc. in International/Industrial Marketing and Purchasing, M.Sc. in Strategy, Organization and Leadership, M.Sc. in Business Economics and Computer Science, M.Sc. in Economics of International Strategy and Governance, M.Sc. in Business Economics and Philosophy, M.Sc. in Business Administration and Modern Languages, M.Sc. in Business Economics and Business Law, M.SC. in Business Economics and Language, and M.Sc. in Creative Business Processes. For this reason they are excluded from the main analyses. As a robustness check we include these master's programs - see Figure A.1 in the Appendix to the paper.

B.2 Descriptive Statistics: Peer Groups

Table B.3 reports descriptive statistics for the individual group across a number of characteristics including individual and academic outcomes for students enrolled in 2001. Overall, the groups are not systematically different from each other in terms of number of students, gender, or academic performance.[30] Table B.4 shows similar descriptive statistics across all cohorts. The picture that emerges is similar to the one for the cohort enrolled in 2001.

[30]Despite that the share of males looks slightly larger in group number 9 compared to the other groups, this difference is not statistically significant.

Table B.3: Descriptive Statistics Across First-year Peer Groups: 2001 Cohort

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Share of males	0.64	0.70	0.68	0.72	0.73	0.65	0.60	0.62	0.83	0.63	0.62	0.64	0.67	0.68
	(0.49)	(0.47)	(0.48)	(0.46)	(0.46)	(0.49)	(0.50)	(0.50)	(0.38)	(0.50)	(0.50)	(0.49)	(0.48)	(0.48)
Starting age	21.83	21.91	21.01	21.08	20.84	21.22	21.31	21.12	21.55	22.13	22.10	21.40	21.69	21.25
	(1.48)	(2.68)	(1.43)	(1.12)	(1.28)	(1.14)	(1.33)	(1.19)	(1.81)	(2.30)	(2.56)	(2.51)	(1.40)	(1.31)
High school GPA	6.08	6.70	7.28	7.29	7.29	7.07	7.36	6.22	6.65	5.88	6.80	6.90	6.40	6.88
	(1.95)	(1.88)	(2.38)	(1.98)	(1.49)	(2.01)	(2.15)	(1.32)	(1.66)	(2.20)	(1.60)	(1.65)	(1.45)	(1.63)
First-year GPA	4.92	5.42	5.59	5.64	4.81	5.10	5.71	4.05	5.73	5.57	5.23	5.46	4.39	5.47
	(1.97)	(2.59)	(2.30)	(1.84)	(1.86)	(1.90)	(2.29)	(1.77)	(2.03)	(2.62)	(1.87)	(1.84)	(1.57)	(2.04)
Bachelor's GPA	5.99	6.41	6.63	6.50	6.35	6.20	6.70	5.81	6.40	6.13	6.38	6.31	6.29	6.94
	(1.82)	(2.22)	(1.94)	(1.56)	(1.55)	(1.46)	(2.24)	(1.64)	(1.55)	(2.31)	(1.56)	(1.43)	(1.97)	(2.12)
Students	25	23	22	25	22	23	25	26	24	19	26	25	30	28

Note: Means are reported for the cohort of Danish students enrolled in the bachelor's program in 2001. Standard deviations reported in parenthesis. The column numbers refer to the relevant first-year peer group (i.e. exercise classmates).

Table B.4: Descriptive Statistics Across First-year Peer Groups: All Cohorts

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Share of males	0.67	0.74	0.68	0.68	0.73	0.72	0.69	0.67	0.72	0.72	0.65	0.71	0.66	0.71	0.64	0.73	0.71	0.71
	(0.47)	(0.44)	(0.47)	(0.47)	(0.44)	(0.45)	(0.47)	(0.47)	(0.45)	(0.45)	(0.48)	(0.45)	(0.48)	(0.46)	(0.49)	(0.45)	(0.46)	(0.47)
Starting age	21.44	21.02	21.06	21.15	21.10	21.47	21.29	21.74	21.27	21.74	21.38	21.38	21.03	21.22	20.75	21.10	21.45	20.80
	(1.66)	(1.46)	(1.23)	(1.37)	(1.33)	(1.78)	(1.42)	(2.12)	(1.68)	(1.90)	(1.60)	(1.76)	(1.41)	(1.69)	(1.05)	(1.02)	(1.40)	(1.04)
High school GPA	6.98	7.08	7.23	7.09	6.83	6.95	7.06	6.65	6.84	6.70	6.97	6.73	7.01	6.80	6.31	7.15	6.80	6.69
	(2.03)	(1.89)	(2.04)	(1.96)	(1.95)	(1.91)	(2.03)	(1.95)	(1.98)	(1.96)	(1.81)	(1.94)	(1.87)	(1.95)	(2.08)	(2.11)	(2.26)	(1.76)
First-year GPA	5.57	5.51	5.60	5.51	4.95	5.11	5.22	4.87	5.13	5.22	5.61	5.13	5.41	5.43	4.36	5.22	5.15	3.12
	(2.20)	(2.28)	(2.37)	(2.23)	(2.27)	(2.21)	(2.10)	(2.08)	(2.31)	(2.33)	(2.27)	(2.10)	(2.35)	(2.07)	(2.32)	(2.53)	(2.40)	(0.92)
Bachelor's GPA	6.51	6.61	6.60	6.80	6.47	6.24	6.28	6.10	6.27	6.02	6.55	6.41	6.61	6.59	5.48	5.93	6.63	4.86
	(2.16)	(2.18)	(2.17)	(2.14)	(2.15)	(2.05)	(2.10)	(1.83)	(2.18)	(2.17)	(2.08)	(1.84)	(2.16)	(2.08)	(2.08)	(2.06)	(1.87)	(1.69)
Students	209	231	215	224	222	208	213	224	214	192	218	207	224	235	22	26	31	17

Note: Means are reported for the cohort of Danish students enrolled in the bachelor's program in the period 1995-2004. Standard deviations reported in parenthesis. The column numbers refer to the relevant first-year peer group (i.e. exercise classmates).

Table B.5 reports changes in exercise groups across the three years in Business Economics (undergraduate studies). Columns 2-4 show average changes across all 10 cohorts, while columns 5-7 show changes separately for the cohort enrolled in 2001. For the 2001 cohort, groups number 7 and 10 were merged with other exercise classes in the second year. The number of groups fell further to 11 groups in the third year as group number 3 was closed down and merged with other exercise classes. The abolishment of some exercise classes increased the average number of students per exercise class in the second and third year.

Table B.5: Group Changes Across Years

Group	All cohorts			2001 cohort		
	Number of individuals in each group					
	First year	Second year	Third year	First year	Second year	Third year
1	20.90	22.30	21.70	25.00	32.00	33.00
2	23.10	27.22	28.25	23.00	31.00	36.00
3	21.50	23.20	23.78	22.00	22.00	*
4	22.40	25.90	27.00	25.00	29.00	33.00
5	21.20	25.56	25.13	22.00	27.00	39.00
6	20.80	22.88	23.20	23.00	29.00	35.00
7	21.30	23.38	25.43	25.00	NA	NA
8	22.40	22.70	27.00	26.00	29.00	*
9	21.40	21.67	20.38	24.00	24.00	*
10	19.20	20.67	22.13	19.00	NA	NA
11	21.80	21.80	23.22	26.00	27.00	26.00
12	20.70	22.88	21.33	25.00	30.00	28.00
13	22.40	23.11	26.22	30.00	32.00	32.00
14	23.50	24.30	28.5	28.00	31.00	37.00
15	11.0	11.00	11.00			
16	13.00	12.00	12.50			
17	15.50	14.00	14.00			
18	17.00	16.00	14.00			

Note: Means by peer group (i.e. exercise class). Groups number 15-18 only exist for 1995 and 1996. "*" means that we are not able to disclose the information due to discretion rules set by Statistics Denmark. "NA" implies that the groups (i.e. exercise classes) no longer exist because the class was merged with other classes.

Test of Random Assignment into Peer Groups

The assumption that allows us to estimate and identify a peer effect is random assignment into first-year groups. To do this we investigate whether peer-group assignment is correlated with individual socio-economic and pre-educational characteristics. Tables B.6 and B.7 examine this by reporting estimation results and F-statistics of the joint significance of all the explanatory variables (except enrollment age) from a linear probability regression of the probability of being assigned into each group on individual characteristics and pre-educational characteristics. Results are further discussed in the main text of the paper.

Table B.6: Test of random assignment into first-year peer groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Starting age	0.003	0.012	-0.004	-0.004	-0.012*	-0.004	-0.000	-0.013*	0.001	0.006	0.017	-0.002	0.002	-0.001
	(0.007)	(0.012)	(0.005)	(0.005)	(0.007)	(0.006)	(0.007)	(0.007)	(0.008)	(0.010)	(0.011)	(0.012)	(0.008)	(0.007)
Gender, female=1	0.010	-0.023	-0.003	-0.012	-0.024	0.006	0.022	0.013	-0.038	0.020	0.026	0.018	-0.004	-0.011
	(0.030)	(0.030)	(0.031)	(0.030)	(0.030)	(0.030)	(0.031)	(0.031)	(0.027)	(0.027)	(0.036)	(0.032)	(0.034)	(0.034)
GPA from high school	-0.032	0.005	0.020	0.007	0.019	0.001	0.035	-0.031*	-0.004	-0.036*	0.019	0.004	-0.012	0.005
	(0.024)	(0.020)	(0.024)	(0.019)	(0.018)	(0.020)	(0.026)	(0.019)	(0.017)	(0.021)	(0.019)	(0.021)	(0.021)	(0.020)
Business high school dummy	-0.040	-0.028	0.034	0.071	-0.044	0.015	-0.017	-0.047	-0.000	0.039	0.035	0.095*	-0.094	-0.017
	(0.046)	(0.067)	(0.051)	(0.052)	(0.058)	(0.054)	(0.044)	(0.049)	(0.038)	(0.046)	(0.059)	(0.050)	(0.066)	(0.051)
Normal (mat) high school (=1)	-0.021	-0.088	0.015	-0.003	-0.055	-0.006	0.037	0.010	0.073*	0.005	0.015	0.066*	-0.057	0.008
	(0.046)	(0.057)	(0.042)	(0.037)	(0.050)	(0.046)	(0.038)	(0.043)	(0.038)	(0.036)	(0.045)	(0.036)	(0.061)	(0.046)
Mother self-employed	0.056	-0.034	-0.003	-0.053	-0.075**	-0.060**	0.043	-0.008	0.055	0.122	0.054	-0.053*	0.016	-0.060**
	(0.082)	(0.060)	(0.059)	(0.061)	(0.031)	(0.027)	(0.073)	(0.063)	(0.081)	(0.096)	(0.073)	(0.029)	(0.084)	(0.030)
Father self-employed	-0.044	-0.051	0.046	0.052	0.051	0.025	0.007	0.013	-0.042*	-0.005	-0.041	-0.054*	-0.016	0.059
	(0.036)	(0.040)	(0.056)	(0.056)	(0.057)	(0.045)	(0.051)	(0.051)	(0.025)	(0.037)	(0.046)	(0.033)	(0.044)	(0.056)
P-value	0.264	0.662	0.567	0.434	0.508	0.400	0.362	0.372	0.296	0.580	0.320	0.493	0.139	0.208
No. of obs	343	343	343	343	343	343	343	343	343	343	343	343	343	343

Note: Linear Probability Model. The dependent variable is equal to 1 if assigned to the same first-year peer group, and zero otherwise. Standard errors reported in parenthesis. The column numbers refer to the relevant first-year peer group (i.e. exercise classmates). Numbers are reported for the cohort of Danish students that enrolled in undergraduate studies in 2001. Fixed effects for parental educational characteristics are included. The p-value from joint test refers to the test of joint significance of all the variables reported together with dummies for parents' educational level. *** p<0.01, ** p<0.05, * p<0.1

Table B.7: Test of random assignment into first-year peer groups

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1995	P-value	0.917	0.581	0.845	0.824	0.817	0.459	0.679	0.482	0.668	0.625	0.976	0.860	0.632	0.859	0.915	0.955	0.839	
	No. of obs	277	277	277	277	277	277	277	277	277	277	277	277	277	277	277	277	277	
1996	P-value	0.811	0.662	0.464	0.553	0.555	0.776	0.970	0.737	0.827	0.993	0.841	0.907	0.734	0.756	0.940	0.830	0.710	0.669
	No. of obs	284	284	284	284	284	284	284	284	284	284	284	284	284	284	284	284	284	284
1997	P-value	0.593	0.685	0.680	0.449	0.807	0.512	0.021	0.586	0.714	0.827	0.506	0.644	0.378	0.656				
	No. of obs	259	259	259	259	259	259	259	259	259	259	259	259	259	259				
1998	P-value	0.548	0.343	0.394	0.236	0.385	0.675	0.439	0.432	0.741	0.277	0.360	0.379	0.670	0.466				
	No. of obs	324	324	324	324	324	324	324	324	324	324	324	324	324	324				
1999	P-value	0.610	0.129	0.191	0.361	0.363	0.324	0.000	0.171	0.195	0.232	0.234	0.852	0.077	0.086				
	No. of obs	353	353	353	353	353	353	353	353	353	353	353	353	353	353				
2000	P-value	0.181	0.282	0.316	0.371	0.164	0.489	0.339	0.147	0.731	0.438	0.615	0.585	0.446	0.183				
	No. of obs	356	356	356	356	356	356	356	356	356	356	356	356	356	356				
2001	P-value	0.264	0.662	0.567	0.434	0.508	0.400	0.362	0.372	0.296	0.580	0.320	0.493	0.139	0.208				
	No. of obs	343	343	343	343	343	343	343	343	343	343	343	343	343	343				
2002	P-value	0.256	0.363	0.395	0.375	0.287	0.220	0.242	0.338	0.051	0.895	0.391	0.368	0.555	0.066				
	No. of obs	340	340	340	340	340	340	340	340	340	340	340	340	340	340				
2003	P-value	0.466	0.523	0.578	0.514	0.391	0.347	0.000	0.111	0.376	0.612	0.389	0.345	0.620	0.201				
	No. of obs	301	301	301	301	301	301	301	301	301	301	301	301	301	301				
2004	P-value	0.000	0.315	0.399	0.227	0.498	0.472	0.582	0.212	0.248	0.358	0.288	0.173	0.800	0.686				
	No. of obs	297	297	297	297	297	297	297	297	297	297	297	297	297	297				

Note: Linear Probability Model. Each reported p-value corresponds to a separate estimation. The dependent variable is equal to 1 if assigned to the same first-year peer group, and zero otherwise. Standard errors reported in parenthesis. The column numbers refer to the relevant first-year peer group (i.e. exercise classmates). All estimations include cohort fixed effects and control variables (student's bachelor's GPA, a high school dummy, age at enrollment, gender and family characteristics). The reported p-value refers to the test of joint significance of all the included variables.

B.3 Full Sample Including Non-Danes

Summary Statistics

Tables B.8 and B.9 show summary statistics based on the full sample including non-Danes. We have missing information for non-Danish students if they did not go to high school where the GPA could be translated into the Danish scale by CBS. Furthermore, we have missing information on their wages if they did not take a job with a Danish employer after graduation. Moreover, we are not able to account for parental education or labor market background for non-Danes.

Table B.8: Descriptive Statistics: All Students

	All cohorts		2001 cohort	
	Mean	Std.dev.	Mean	Std.dev.
Individual student characteristics:				
Starting age (years)	21.32	1.66	21.47	1.80
Gender (female=1)	0.31	0.46	0.34	0.47
Business high school (=1)	0.25	0.43	0.20	0.40
General (mat) high school (=1)	0.59	0.49	0.64	0.48
High school GPA	6.93	1.96	6.83	1.85
First-year GPA	5.26	2.24	5.20	2.04
Bachelor's GPA	6.40	2.10	6.39	1.82
Dropped out of master's program (=1)	0.14	0.34	0.10	0.30
Master's GPA ^{A}	7.05	1.96	6.97	1.79
Hourly wage (DKK)^{B}	184.49	37.81	188.65	38.07
Pleace of residence teen year prior to enrollment:				
Region of Copenhagen	0.42	0.49	0.43	0.50
Zealand without Copenhagen	0.39	0.49	0.36	0.48
Jytland	0.09	0.29	0.09	0.29
Fyn	0.03	0.16	0.03	0.17
Other	0.02	0.15	0.02	0.15
Outside Denmark (non-Danish)	0.05	0.21	0.07	0.25
Sector occupation in first job^{B}:				
Agriculture, fishing and quarrying	*	*	NA	NA
Industries and utilities	0.092	0.29	*	*
Construction	*	*	*	*
Trade, transport, info. and communication	0.216	0.41	0.24	0.43
Finance and business services	0.638	0.48	0.63	0.48
Public and personal services	0.05	0.22	0.04	0.19
Observations	3,290		367	

Note: "*" means that we are not able to disclose the information due to discretion roles set by Statistics Denmark. For the same reasons minimum and maximum are not reported. "NA" indicates that there are no observations in that group. ^{A}Means reported for 2,601 students on final master's degree GPA (286 observations for the 2001 cohort). ^{B}Means reported for 1,810 observations on sector and hourly wages (250 observations for the 2001 cohort).

Table B.9: Descriptive Statistic Across Peer Groups

	All cohorts				2001 cohort		
	Mean	Std.dev.	No. of groups	Average no. of groups	Mean	Std.dev.	No. of groups
Group size (no. of students):							
First-year peer group	22.37	5.16	147	14.7	26.21	2.78	14
Second-year peer group	24.00	7.81	137	13.7	30.58	3.58	12
Third-year peer group	25.09	11.59	131	13.1	33.36	11.53	11
First-year peer group characteristics:							
Share of males	0.69	0.07	147	14.7	0.66	0.07	14
Age	21.34	0.78	147	14.7	21.46	0.42	14
High school GPA	8.39	0.19	147	14.7	8.36	0.18	14
First-year GPA	7.65	0.32	147	14.7	7.67	0.20	14
Bachelor's GPA	8.15	0.32	147	14.7	8.18	0.11	14

Choice of Master's Program

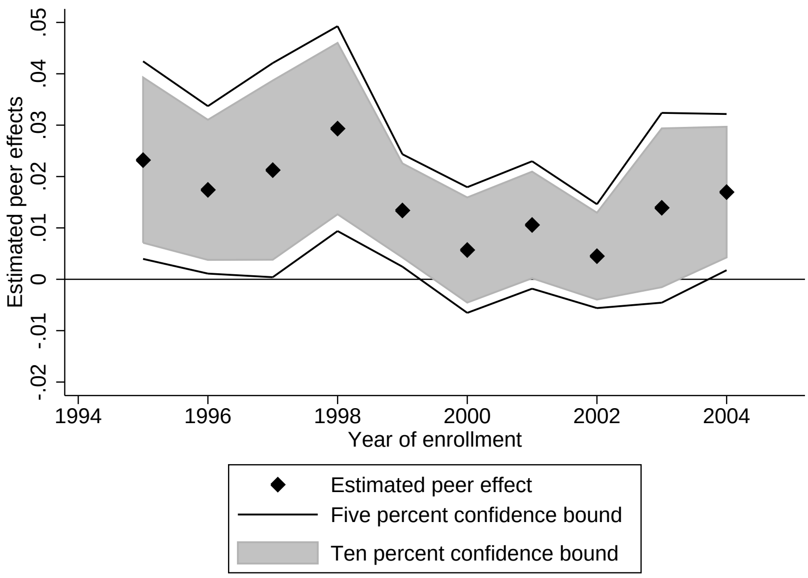
Table B.10: Robustness: Peer Effects in the Choice of Master's Degree (all students)

	(1)	(2)	(3)	(4)	(5)	(6)
First-year peer group	0.010	0.010	0.010*	0.011*		
	(0.006)	(0.006)	(0.006)	(0.006)		
Second-year peer group					0.011*	
					(0.006)	
Third-year peer group						0.011**
						(0.005)
Same gender (=0)		-0.007	-0.007	-0.006	-0.007	-0.007
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Same location prior to enrollment (=0)		-0.000	0.001	0.001	0.001	0.001
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Absolute difference in:						
First-year GPA		-0.008***	-0.008***		-0.008***	-0.008***
		(0.003)	(0.003)		(0.003)	(0.003)
Bachelor's GPA				-0.007**		
				(0.003)		
High school GPA		-0.002	-0.002	-0.002	-0.002	-0.002
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age		0.004*	0.004	0.004	0.004	0.004
		(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Sum of:						
First-year GPA			0.001		0.001	0.001
			(0.003)		(0.003)	(0.003)
Bachelor's GPA				0.000		
				(0.003)		
High school GPA			-0.006**	-0.006**	-0.006**	-0.006**
			(0.002)	(0.002)	(0.002)	(0.002)
Age			-0.001	-0.001	-0.001	-0.001
			(0.003)	(0.003)	(0.003)	(0.003)
Observations	67,161	67,161	67,161	67,161	67,161	67,161

Note: Linear Probability Model. Dependent variable is equal to 1 if i and j are enrolled in the same master's degree. Standard errors reported in parenthesis. Estimation sample includes all students (i.e. Danish and non-Danish) who enrolled in undergraduate studies at CBS in 2001. All estimations include cohort and peer-group fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table B.10 shows estimation results using the full sample of postgraduate students including non-Danish students. Again, we find that students randomly assigned to the same exercise class upon enrollment at the bachelor's level are more likely to study for the same degree; however, the estimate is at best statistically significant at the 10-percent level when the full sample is used. Since foreign students studying in Denmark are likely to have chosen Copenhagen Business School for a specific reason and maybe for a specific master's program, we would also expect them to be less prone to peer influence, explaining the insignificant and lower coefficient estimate reported in column 2. Figure B.1 presents the estimation results on our main variable of interest, first-year peer group, across all cohorts. In line with the results for 2001 detailed above, we find that peer influence is less strong in terms of significance in master's choice when the entire sample of students is considered.

Figure B.1: Robustness: Peer Effects in the Choice of Master's Degree (all students)



Note: Linear Probability Model. Coefficient estimates for first-year peer group reported. All estimations include control variables similar to the ones included in Table B.10. The estimation samples include all students (i.e. Danish and non-Danish) who enrolled in undergraduate studies at CBS in the period 1995-2004.

Does Schooling Choice Lead to Inefficiencies?

The distribution of students into the different decision modes based on the full sample (i.e. Danish and non-Danish students) is reported in Table B.11.[31] Twenty-nine percent of the students followed both peer and potential, meaning that $D^{both} = 1$. Seventeen percent followed neither peer nor potential, 21 percent followed their peer without following their potential at the same time, and, 32 percent of the students followed only their potential. The distribution is almost identical to the distribution observed in the sample including only Danish students.

Table B.11: Grouping by Decision Mode (all students)

	Followed peers: $f_i > 1$	Did not follow peers: $f_i < 1$	Total
Followed abilities: $g_i > 1$	985	1,043	2,028
	(29.94)	(31.70)	
Did not follow abilities: $g_i < 1$	679	583	1,262
	(20.64)	(17.72)	
Total	1,664	1,626	3,290

Note: Frequency. Percentages reported in parenthesis. $g_i < 1$ indicates "did not follow potential" and $g_i > 1$ indicates followed potential. The sample includes all students (Danish and non-Danish) enrolled in undergraduate studies at CBS in the period 1995-2004.

Estimation results using the full sample are presented in Table B.12. Columns 1, 3, and 5 include only the dummies for the different decision modes, while columns 2, 4, and 6 extend the specification with control variables.

Our main interest is the comparison between the students who follow their abilities ($D^{potential}$ - reference group) and students who follow their peers (D^{peers}). The coefficient estimate on D^{peers} is negative across all estimations but not statistically significant when control variables are included. These results are almost identical to the results reported in the main text. We therefore conclude that the result obtained in the main text is not driven by exclusion of non-Danish students.

[31] The different decision modes are as follows: (i) Following one's peers and potential: $D^{both} = 1$ if $f_i > 1 \wedge q_i > 1$, (ii) following one's potential but not peers: $D^{potential} = 1$ if $f_i < 1 \wedge q_i > 1$, (iii) following one's peers but not potential: $D^{peers} = 1$ if $f_i > 1 \wedge q_i < 1$, and (iv) following neither one's potential nor peers: $D^{none} = 1$ if $f_i < 1 \wedge q_i < 1$. Here f_i measures the relative fraction of peers who choose the same degree program, while q_i measures individual i's relative educational achievement in mandatory courses relevant for individual i's chosen master's degree.

Table B.12: Academic and Labor Market Outcomes on Decision Modes

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Master GPA		Drop out		Starting wage (log)	
D^{none}	-0.189*	-0.133	-0.004	-0.007	-0.005	-0.004
	(0.101)	(0.081)	(0.018)	(0.018)	(0.014)	(0.014)
D^{peer}	-0.177*	-0.118	-0.004	-0.008	-0.028*	-0.027*
	(0.095)	(0.080)	(0.017)	(0.017)	(0.016)	(0.015)
D^{both}	-0.077	-0.019	-0.007	-0.007	-0.015	-0.012
	(0.087)	(0.070)	(0.015)	(0.015)	(0.012)	(0.012)
Starting age		-0.042**		0.012***		0.005
		(0.018)		(0.004)		(0.003)
GPA from master's degree						0.010***
						(0.003)
GPA from bachelor's degree		1.133***		-0.078***		
		(0.043)		(0.009)		
GPA from high school		0.343***		0.002		0.010
		(0.043)		(0.010)		(0.007)
Business high school dummy		-0.255***		0.030**		-0.004
		(0.070)		(0.015)		(0.012)
Gender (female=1)		-0.071		-0.036***		-0.053***
		(0.061)		(0.013)		(0.011)
Master fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Location fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Starting year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Sector fixed effect					Yes	Yes
Observations	2,757	2,757	3,290	3,290	1,918	1,918

Note: OLS. Standard errors reported in parenthesis. Estimation sample includes all students (Danish and non-Danish) enrolled between 1995 and 2004. The reference group is ability-driven individuals ($D^{potential}$). In the wage regression wage observations below and above the 1% and 99% percentile, respectively, are dropped. *** p<0.01, ** p<0.05, * p<0.1

B.4 Comparison of Grading Systems

Table B.13 compares the Danish grading scale (both the "13" and the "7" scales) to the ECTS and the American grading scale, respectively. Thus, an average degree of 7 on the Danish "7" scale corresponds to a C on the ECTS scale and a B+ on the American scale (4.5).

Table B.13: Academic Grading Scale Comparison

Definition	Excellent		Very good	Good		Satisfactory	Passed	Failed		
Danish "13" scale (old scale)	13	11	10	9	8	7	6	5	03	00
Danish "7" scale (new scale)	12	12	10	7	7	4	02	00	00	-3
ECTS scale	A	A	B	C	C	D	E	Fx	Fx	F
American scale (4.0)	A	A-	B+	B	B-	C+	C	D	F	F
American scale (4.3)	A+	A	A-	B+	B	B-	C	D	F	F
American scale (4.5)	A+	A+	A	B+	B+	B	C+	D	F	F

Note: At higher education institutions, the Danish "13" scale was replaced by a new "7" scale in 2007.
Source: World Education Services.

Conclusion

This thesis is concerned with the influence of educational decisions on labor market outcomes and the mechanisms explaining these different educational decisions. The research questions asked all fall under the heading of economics of education and labor and are addressed empirically by means of Danish micro data. The topics in this thesis are motivated by existing research that debates how different educational decisions of individuals can explain variation in welfare, both at the individual and at the country level. However, there is still a need for better knowledge of these topics. Chapter 1 and Chapter 2 complement the literature by providing new empirical evidence on the returns to educational decision. Chapter 1 investigates the returns to a master's degree in business economics and management and Chapter 2 looks at how the choice of elective courses within a specific master's program is reflected in both the probability of obtaining a leadership position and wage outcomes. Inspired by the findings in the first two chapters, Chapter 3 and Chapter 4 cast light on determinants of educational choices and in particular on peer effects on tertiary education decisions. Chapter 3 investigates peer effects on the decision of dropping out of university and Chapter 4 examines peer effects in master's degree choice.

Chapter 1 compares labor market outcomes from graduates with master's degrees in business economics and management to labor market outcomes from graduates with master's degrees from other fields in the social sciences. Using an Instrumental Variables (IV) approach to address selection into educational field, the results from Chapter 1 show that a master's degree in business economics and management results in significantly higher wage outcomes and a higher probability of obtaining employment in the private sector. By contrast to the literature that finds that the gender wage gap can be explained by differences in type of educational field, we find that controlling for a master's degree in business economics and management does not affect the gender wage gap prevalent in our sample.

Chapter 2 examines how self-selected differences in the curriculum within the same master's program correspond to differences in labor market outcomes. We show in this chapter how elective courses in management are associated with an increased probability of obtaining leadership. Moreover, and in line with previous research, we show how math-related courses such as courses in finance and accounting are positively associated with wage outcomes. Finally, our results show how being educationally diversified among classical business courses is reflected positively in the probability of obtaining a leadership position whereas being educationally diversified outside the classical business school type courses has no significant effect.

Chapters 3 and 4 estimate peer effects and exploit data on students who were randomly assigned into groups at enrollment in the largest bachelor's program at CBS to overcome the problem of endogenous selection of peers. Similar to other papers in the literature, these randomly assigned groups work as our definition of peer groups.

Chapter 3 shows how women are more likely to drop out of the first year of university if they have high ability peers. Chapter 3 also show that women's probabilities of dropping out are decreasing with their peer group ranks. Moreover, when including peer group rank in my model, high school GPA becomes insignificant in predicting the drop out probability for women. By contrast, men are unaffected by both the ability level of their peers and their peer group rank. However, absolute ability, measured by high school GPA, is still a significant predictor of men's probability of dropping out even after controlling for peer group rank. These results underline how men and women react differently to their peers and suggests that the comparison to peers is more important for women's decision to drop out than their absolute ability level. The opposite seems to be true for men.

Chapter 4 shows how the choice of a master's degree is impacted by peers. We find that pairs of students assigned to the same exercise group when enrolled in a bachelor's program are more likely to choose the same master's degree program. Particularly, we also find that students' with similar first-year GPA from the bachelor's program experience a stronger peer effect indicating that peer effects are heterogeneous across individuals with different ability levels. Finally, we see no adverse impact of following your peers into a master's degree program on master's degree GPA or the probability of dropping out of the master's program.

This thesis studies individual educational decisions, their determinants, and their consequences. Overall, I find that educational decisions are significantly related to differences in labor market outcomes. Moreover, I find peer effects on both the decision to drop out of university and on the choice of master's program. From a policy perspective, my findings can help inform politicians about how different study fields and curriculum characteristics are reflected in labor market outcomes and I can provide information about how prospective and current students decide on their educational investments.

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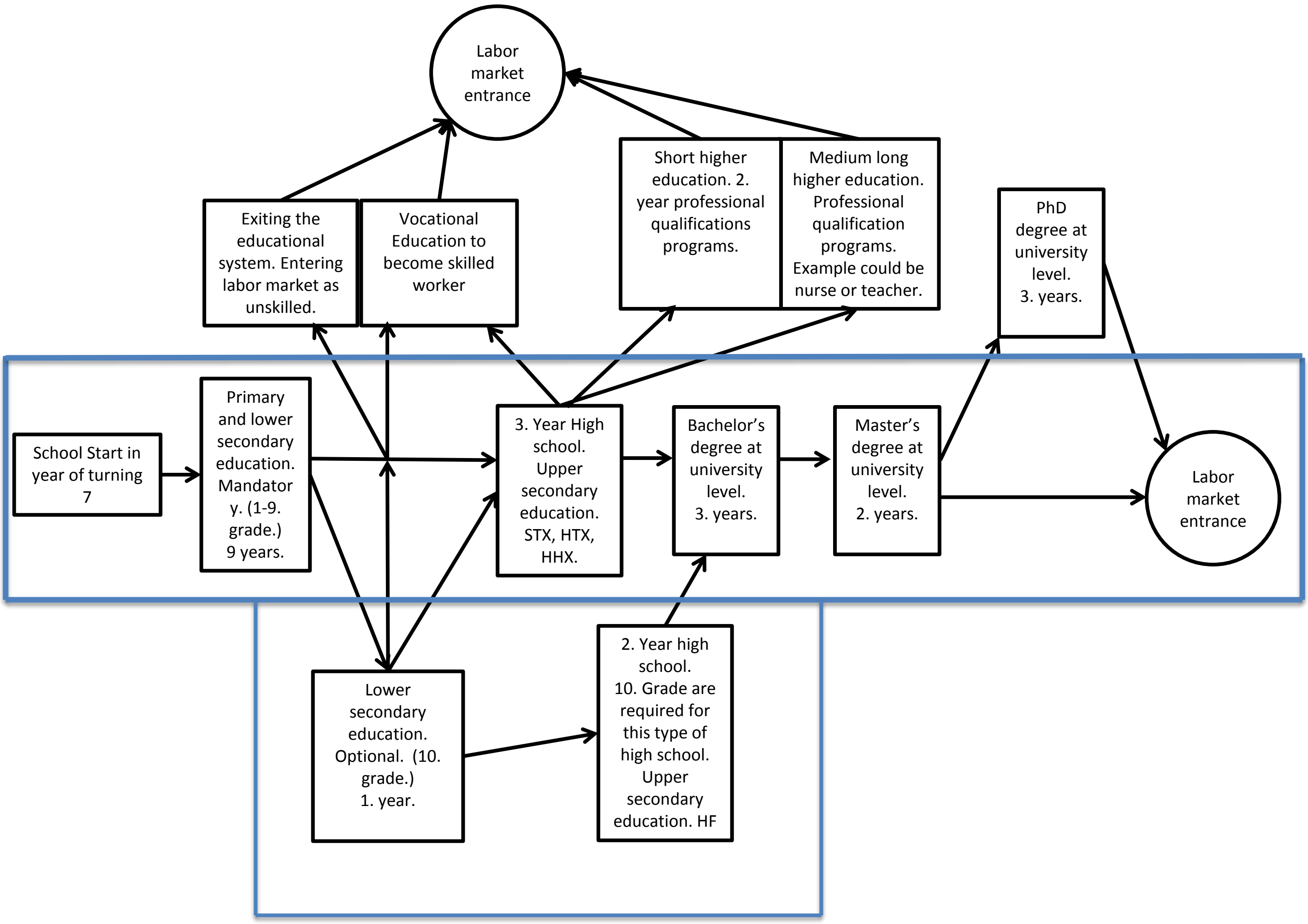
Main Appendix

Appendix A The Danish Education System

Figure A.1: Pathways of the Danish Education System



The thesis is concerned mostly with the returns to study fields at the tertiary level and with the mechanisms underlying tertiary educational investment decisions and address these topics using Danish micro data. Thus, before reading this thesis, it might be beneficial to have an introduction to the Danish education system. Danish students must fulfill certain requirements to proceed to tertiary education and the Danish education system also has a somewhat different structure from those of other countries. For example, in Denmark almost everyone who graduates with a bachelor's degree continues into a master's program, because a bachelor's degree is not valued highly in the Danish labor market. But, in contrast with other countries such as the U.S., very few students with master's degrees continue into doctoral programs. The majority of students with master's degrees is ready for and demanded by the labor market. A sketch of the different pathways through the Danish education system is shown in Figure A.1.

Before 2007, Danish students were graded on the the Danish "13" scale. This scale is named for its highest grade, 13, which can only be achieved with an exceptionally independent and excellent performance. The lowest passing grade is 6. This scale does not use the value 12, skipping directly from 11 to 13, and 13 is almost never awarded. A 7-step grading scale replaced the "13" system in 2007, with the objective that grading should be absolute in order to measure students' achievements relative to a pre-defined fixed standard. This scheme uses 7 values. The lowest passing grade is 2, the maximum grade is 12, and the remaining passing grades are 4, 7, 10, and 12. To clarify this, Table A.1 compares both Danish grading scales (the "13" and the "7" scales) with the ECTS and the American grading scale. This table is similar to that in Appendix B of Chapter 4. As can be seen, a grade of 7 on the Danish "7" scale corresponds to a C on the ECTS scale and a B+ on the American scale (4.5).

Table A.1: Academic Grading Scale Comparison

Definition	Excellent		Very good	Good		Satisfactory	Passed	Failed		
Danish "13" scale (old scale)	13	11	10	9	8	7	6	5	03	00
Danish "7" scale (new scale)	12	12	10	7	7	4	02	00	00	-3
ECTS scale	A	A	B	C	C	D	E	Fx	Fx	F
American scale (4.0)	A	A-	B+	B	B-	C+	C	D	F	F
American scale (4.3)	A+	A	A-	B+	B	B-	C	D	F	F
American scale (4.5)	A+	A+	A	B+	B+	B	C+	D	F	F

Note: At higher education institutions, the Danish "13" scale was replaced by a new "7" scale in 2007.
Source: World Education Services.

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