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Empirical Essays on Economics of Education

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Preface

This thesis was written in the period from January 2010 to December 2012 while I was enrolled as a PhD student at the Department of Economics and Business at Aarhus University. I am grateful to the Centre for Strategic Educational Research for its financial support, and to the Department of Economics and Business at Aarhus University for providing an excellent research environment.

There are a number of people I wish to thank. First and foremost, I would like to thank my main supervisor, Helena Skyt Nielsen, for encouraging me to enroll as a PhD student, giving me guidance whenever needed, for her always competent and constructive comments along the way, and for being a unique inspiring mentor. Also, I would like to thank my secondary supervisor, Nina Smith, for her eager and inspirational guidance, and ideas both as secondary supervisor and as co-author on one of my projects. She also deserves special thanks for bringing cake, almonds and chocolate, keeping me productive in the late hours. I have also benefitted greatly from working with Maria K. Humlum and Rune M. Vejlin on the first project. It has truly been inspiring, and it has been a great learning process for me and given me valuable experience. Also, the faculty members and the PhD students deserve thanks for comments at various stages on my three projects.

During my studies, I enjoyed a visit at Tepper School of Business at Carnegie Mellon University in Pittsburgh. I would like to thank the department for its hospitality, and especially Dennis Epple for the invitation and for constructive comments during my stay.

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I owe my parents many thanks for always encouraging a curious, yet critical mind. You have always shown interest in my work. Thank you for e-mailing me with the current debate, keeping me up-to-date with the real world outside the yellow (and now red) bricks. Thanks to my in-laws for always being there for me and keeping my feet on the ground.

Finally and most importantly, thanks to my loving husband and colleague, Mark, for listening to me during the past three years whenever I needed it. Thanks for all your support, guidance and belief in me – even when I sometimes failed to believe in myself. I love you with all my heart, and I am forever grateful.

Jannie H.G. Kristoffersen
Aarhus, December 2012

Updated preface

The predefence took place on February 7, 2013. I would like to thank the assessment committee, Martin Salm, Anna Sjögren, and Marianne Simonsen (chair), for their careful reading of the dissertation and their constructive comments and suggestions for my work. Some of the suggestions have already been incorporated, and more will follow in the near future.

Jannie H.G. Kristoffersen
Copenhagen, May 2013

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Summary

This dissertation consists of three self-contained chapters that all empirically investigate issues related to the educational system. Understanding why individuals attain a certain educational level and how behavior affects school outcomes are important in the society we live in today. In recent decades, the industrialized countries have argued that future growth and welfare depend on a higher educational level in the population. Thus, investigating how a higher level of education can be obtained is important to acquire knowledge on where new initiatives are needed and how to design the optimal policies.

The three chapters consider individuals at different educational levels. While the first chapter focuses on individuals after completing high school and their timing of enrollment in college and decisions concerning family formation, the last two chapters investigate how individuals in compulsory school perform in the exit exams when they complete compulsory schooling. The second chapter investigates whether there are gender differences in the relationship between behavioral problems and school performance, and the third chapter examines whether a student's school outcomes are affected by having a peer group with a relatively high proportion of vulnerable children.

In the first chapter in this dissertation, "Timing of College Enrollment and Family Formation Decisions" (co-authored with Maria Knoth Humlum and Rune Majlund Vejlin), the emphasis is on the effect of college enrollment on later family formation decisions. Several empirical studies suggest that there is a link between the increase in women's educational attainment over the last decades and the low fertility rates observed in many developed countries. For example, compulsory schooling laws have been shown to affect a wide range of outcomes, including fertility, marriage, and health, see e.g. Black, Devereux, and Salvanes (2008) and Oreopoulos (2007). A potential determinant of the timing of family formation is educational attainment or the timing hereof. In this chapter, the effect of enrolling in college in the year of application is estimated on later family formation decisions such as the probability of being a parent at a certain age. Administrative data is merged with data from the Coordinated Enrollment System, making it possible to identify individuals who are just above or just below the grade requirement for their preferred college program. Based on this idea, a fuzzy regression discontinuity design is used, and we find that delays in college enrollment postpone family formation decisions. For

example, we find that the effect of enrolling in college on the probability of being a parent at age 27 is about 9 percentage points, corresponding to an increase of about 70 percent.

In the second chapter, "Differences in the Effects of Behavioral Problems on School Outcomes" (co-authored with Nina Smith), the focus is turned to students in compulsory school and how behavioral problems affect boys and girls differently in their school outcomes. In many countries an emerging gender gap in the educational level is seen, and this is, among others, documented by Goldin, Katz, and Kuziemko (2006) and Fortin, Oreopoulos, and Phipps (2012). Recently, there has been an increasing emphasis on the importance of how behavior affects this gender gap. Even after controlling for school grades, behavioral problems are found to matter for educational outcomes in a number of studies, see for instance Heckman and Rubinstein (2001), Jacob (2002), Heckman, Stixrud, and Urzua (2006), Bertrand and Pan (2011), and the survey in Heckman (2008). This analysis is based on administrative data merged with data on behavior for about 6,000 students born in 1990-1992 in a large region in Denmark. We find significant and large negative coefficients of the externalizing behavioral indicators. The effects tend to be larger when based on parents' SDQ scores compared to teachers' SDQ scores. According to our estimations, the school outcomes for girls with abnormal externalizing behavior are not significantly different from those of boys with the same behavioral problems. A decomposition of the estimates indicates that most of the gender differences in Reading and Math cannot be related to gender differences in behavioral problems. The large overall gender gap in Reading seems mainly to be the result of gender differences between children without behavioral problems living in 'normal families', i.e., families which are not categorized as low-resource families.

In the third chapter, "Vulnerable Children and Peer Effects", I investigate whether the proportion of vulnerable peers at the cohort level affects school performance of the individual student. The vulnerable children might be more prone to attract teacher attention, either due to behavioral issues or due to learning disabilities, and thereby reducing the overall performance of all the students. As seen in Epple and Romano (2011) and Sacerdote (2011), peer effects in behavior on school outcomes have recently received great attention. Based on Carrell and Hoekstra (2010) and Black et al. (2008), I use the variation across cohorts within schools in the proportion of vulnerable children to estimate whether peer effects are present in the Reading and Math test scores in the 9th grade exit exam. The vulnerability measures include whether the child experienced i) a deceased parent, ii) divorced parents, iii) a mentally diagnosed parent, or iv) a criminal parent. The results show that the proportion of vulnerable children affects the Math test score negatively by about .1 standard deviation. However, no effects seem to be present for Reading test scores. Performing a range of robustness checks suggest that it is important to take selection to schools before school entry into account. Even though the events these vulnerable children are exposed to cannot easily be altered, it seems like early initiatives increasing the focus on vulnerable children could potentially mitigate the adverse effects both on the student and on the student's peers.

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Dansk resumé (Danish summary)

Denne afhandling består af tre selvstændige kapitler, der alle omhandler emner relateret til det danske uddannelsessystem. De seneste årtier har der været stor debat i de industrialiserede lande om, hvordan befolkningerne kan opnå højere uddannelser for at sikre fremtidig vækst og velstand. Det er derfor af stor betydning, at man er i stand til at identificere, hvor der er brug for nye tiltag, og hvordan disse designes optimalt.

Kapitlerne i denne afhandling omhandler hver især empiriske analyser af individer i forskellige dele af uddannelsessystemet. Mens der i det første kapitel fokuseres på individer, som har bestået en studentereksamen, og deres timing af start på videregående udannelser samt deres beslutninger omkring tidspunktet for at stifte familie, undersøges det i de to sidste kapitler, hvordan individer i folkeskolen klarer sig til afgangseksamen. Det andet kapitel undersøger, om der er kønsforskelle i forholdet mellem adfærdsproblemer og skolepræstationer, og det tredje kapitel undersøger, om elever bliver påvirket af deres skolekammerater, hvis disse i højere grad er udsatte børn.

I første kapitel af afhandlingen, "Timing of College Enrollment and Family Formation Decisions"(skrevet med Maria Knoth Humlum og Rune Majlund Vejlin), undersøges effekten af at starte på en videregående uddannelse på beslutninger omkring tidpunktet for at stifte familie. En del studier viser en sammenhæng mellem de sidste årtiers stigning i kvinders uddannelsesniveau og de lave fertilitetsrater, som der ses i mange af de industrialiserede lande. For eksempel kan det nævnes, at lovmæssige ændringer i folkeskolen har vist sig at påvirke blandt andet fertilitet, ægteskab og sundhed, se f.eks. Black, Devereux og Salvanes (2008) og Oreopoulos (2007). En afgørende faktor for timingen af familiestiftelse er uddannelsesniveauet og timingen af dette. I dette kapitel undersøges effekten af at blive optaget på en videregående uddannelse i det år, man ansøger, på senere familiestiftelse, såsom sandsynligheden for at være forælder på et givet tidspunkt. Administrative data kobles med data fra Den Koordinerede Tilmelding (KOT), hvilket gør det muligt at identificere individer lige over og lige under optagelseskravet på deres ønskede uddannelse. Dette betyder, at individer med en karaktergennemsnit fra gymnasiet, som ligger over optagelseskravet på den ønskede uddanelse, har en større sandsynlighed for at blive optaget på en videregående uddannelse. Netop denne egenskab udnyttes i et såkaldt "fuzzy regression discontinuity design". Resultaterne viser, at senere studiestart forsinker familiestiftelse. Som eksempel kan nævnes, at det er 9 procentpoint mere sandsynligt, at man er forælder som 27-årig, hvis man startede på en videregående uddannelse i det år, hvor man ansøgte. Dette svarer til en stigning på 70 procent.

I det andet kapitel, "Gender Differences in the Effects of Behavioral Problems on School Outcomes"(skrevet med Nina Smith), fokuseres der på elever i folkeskolen, og hvordan adfærdsproblemer påvirker drenge og piger forskelligt i deres skolepræstationer. Blandt andet Goldin, Katz og Kuziemko (2006) samt Fortin, Oreopoulos og Phipps (2012) dokumenterer en voksende kønsforskel i uddannelsenniveauet i mange lande. Der er i de seneste år kommet stor opmærksomhed på betydningen af, hvordan adfærd påvirker denne kønsforskel. Selv efter man har taget højde for karakterer i skolen, finder mange studier, at adfærdsproblemer har betydning for, hvordan man klarer sig i uddannelsessystemet, se f.eks. Heckman og Rubinstein (2001), Jacob (2002), Heckman, Stixrud og Urzua (2006), Bertrand og Pan (2011) samt Heckman (2008). I denne analyse bruges administrative data, som kobles med adfærdsdata for omkring 6.000 elever, som er født i årene 1990-1992 i et stort område i Danmark. Resultaterne viser, at der er en negativ sammenhæng mellem udadreagerende adfærd og skolepræstationer. Sammenhængen virker stærkere, når man baserer analysen på forældrenes vurderinger frem for lærernes vurderinger. Dog er der ikke tegn på, at sammenhængen mellem adfærdsproblemer og skolepræstationer er forskellig for piger og drenge. En dekomponering af resultaterne viser også, at kønsforskellene i skolepræstationer ikke kan forklares af kønsforskelle i adfærdsproblemer. Kigger man på elevernes eksamenskarakterer i mundtlig dansk, viser det sig, at kønsforskellene primært er til stede for "normale" børn og altså ikke i så høj grad for udsatte børn, der kommer fra familier med relativt få ressourcer.

I det tredje kapitel, "Vulnerable Children and Peer Effects", undersøges det, om andelen af udsatte børn på samme årgang på en skole har betydning for skolepræstationer. Udsatte børn får måske mere opmærksomhed fra læreren, enten som følge af adfærdsproblemer eller indlæringsproblemer, hvilket kan reducere de andre elevers skolepræstationer. Som det ses i blandt andet Epple og Romano (2011) samt Sacerdote (2011), har kammeratskabseffekter fået stor opmærksomhed. Baseret på Carrell og Hoekstra samt Black et al. (2008) udnytter jeg variationen inden for skolerne på tværs af årgange i andelen af udsatte børn til at estimere kammeratskabseffekter på afgangseksamen i 9. klasse i dansk og matematik. Et barn defineres som udsat, hvis barnet har i) mistet en forælder, ii) skilte forældre, iii) en forælder med en psykisk diagnose, eller iv) en kriminel forælder. Resultaterne viser, at en højere andel af udsatte børn på årgangen påvirker eksamenskarakteren i matematik negativt. Dog findes der ikke tilsvarende effekt for eksamenskarakteren i dansk. Ved at udføre en række robusthedtjek viser det sig, at det er vigtigt at tage højde for selektion til skolerne før skolestart. Selvom man ikke kan ændre på at nogle børn er udsatte, så kan man foretage tidlige tiltag, som kan modarbejde den negative effekt på barnets egne skolepræstationer såvel som for de andre elever på årgangen.

Litteratur

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Chapter 1

Timing of College Enrollment and Family Formation Decisions

Timing of College Enrollment and Family Formation Decisions*

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Abstract

The stage of one's educational or labor market career is a potentially important factor for family formation decisions. We consider a specific type of career interruption and its consequences for relationship formation and fertility decisions. Specifically, we estimate the effect of enrolling in college in the year of application on later family formation decisions. Using college admission data, we find that individuals with a GPA above the admission requirement for their preferred college program are more likely to enroll in college in a given year. Employing an IV strategy based on this idea, we find that delayed college enrollment implies delayed family formation. For example, we find that enrolling in college in the year of application doubles the probability of being a parent five years later.

JEL Classification: I2, J12, J13

Keywords: fertility, education policy, career interruptions, delayed college enrollement

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1.1 Introduction

Low fertility rates in almost all OECD countries have caused a large demographic shift in recent years. Fertility rates in Italy, Spain, Germany, and Japan are as low as 1.3 children per woman, whereas only the United States, New Zealand, Ireland, Iceland, and France have fertility rates above 1.9 children per woman, see Feyrer, Sacerdote, & Stern (2008). Understanding why we observe lower fertility rates is essential in order to design policies that can help alleviate the problem. The drop in fertility has to some extent coincided with an increase in women's educational attainment, see Goldin & Katz (2002), and there is considerable empirical evidence linking education and fertility decisions, e.g., Currie & Moretti (2003) and Black, Devereux, & Salvanes (2008). We contribute to this literature by presenting evidence that delayed college enrollment has substantial effects on family formation. We use grade requirements for college programs to instrument enrollment essentially comparing those just below and above the grade requirement.

From a theoretical perspective, there are a number of potential explanations why delayed college enrollment would affect family formation decisions. Education is a time-consuming activity, and as such, it may crowd out time spent on other activities including searching for a mate and caring for a child. Becker (1965) proposes a labor supply model in which education increases earnings and thus increases women's opportunity cost of having children. However, as higher education leads to higher permanent income, this could also lead to higher fertility. The literature on the quantity-quality trade-off regarding children generally makes the prediction that fertility is decreasing in education, Becker & Lewis (1973) and Becker & Tomes (1976). Happel, Hill, & Low (1984) consider a model of the timing of the first birth. Among other things they suggest that prospective parents have an economic incentive to align the costs of having a child with a period of their lives where their income is relatively high if they wish to smooth consumption over their life-cycle and capital markets are not perfect. This is what Happel et al. (1984) term the "consumption-smoothing motive in child-timing decisions". Since household income will tend to increase substantially when one or both wage earners complete their education and enter the labor market, the presence of this consumption-smoothing motive implies that the timing of college enrollment may affect the timing of family formation. Also, if there is uncertainty about getting a job after completing college, or uncertainty about the wage in the potential job, then a risk-averse prospective parent may want to postpone family formation until having procured the first job. It may also be that women prefer to delay childbirth until they have reached certain career-related milestones due to social norms. This mechanism would probably be especially important for career-oriented and highly-educated women, who are over-represented in our sample since we consider college enrollment.

Delayed enrollment has become increasingly widespread in recent years in many countries. Cammelli, Antonelli, di Francia, Gasperoni, & Sgarzi (2011) report that prior to 2001, 11 percent of Italian high school graduates delayed enrollment, while in 2008 this was more than doubled. Holmlund, Liu, & Skans (2008) find that 27 percent of Swedish college entrants in 2000 had delayed enrollment by two to four years. Furthermore, over the period from 1977 to 2002 the median number of years between high school graduation and college enrollment was 3. Based on the National Educational Longitudinal Study, Bozick & DeLuca (2005) establish that 16 percent of high school graduates in the US postpone college enrollment by more than seven months. In addition, Horn, Cataldi, & Sikora (2005) report that the proportion of undergraduates who delay enrollment has increased over the past decade in the US.[1] Delayed enrollment thus appears to be an increasingly important type of career interruption, not only in continental Europe, but also in the US.

Our study is closely related to the literature on the relationship between educational attainment and family formation. This literature has documented a strong link between the two. In order to establish whether this is indeed a causal link, some studies have used exogenous variation in educational attainment generated by rules regarding age at school entry and compulsory schooling laws. For school entry rules, the existing evidence is somewhat mixed. McCrary & Royer (2011) find little effect of mother's education on fertility outcomes using natality data from Texas and California. However, using a similar approach, Skirbekk, Kohler, & Prskawetz (2004) find positive effects on the age at first and second birth and age at first marriage for Swedish women, while Black, Devereux, & Salvanes (2011) using Norwegian data find little impact of school starting age on educational attainment, but some impact on teenage pregnancies.

Compulsory schooling laws have been used to instrument educational attainment to study the effects of education on various outcomes. Focusing on family formation outcomes, there is a substantial amount of evidence suggesting that educational attainment affects fertility and marriage choices. Whether or not the effects on family formation decisions are temporary, e.g. affecting only the age at first birth, or permanent, e.g. affecting completed fertility, appears to remain an open question though. While Monstad, Propper, & Salvanes (2008) use Norwegian data and find little effect on completed fertility, Fort, Schneeweis, & Winter-Ebmer (2011) use data from eight European countries and find that more education actually increases completed fertility. Also, increasing the length of compulsory schooling reduces the incidence of teenage pregnancies, see Black et al. (2008) for Norway and the U.S. or Silles (2011) for Great Britain and Northern Ireland. Devereux & Tripathi (2009) find that increasing the length of compulsory schooling increases age at first marriage.

That exogenous variation changes education length at higher levels in the educational system is much more rare. Obviously, the mechanisms and potential effects of parents' education are

[1]In the 1999–2000 school year, 46 percent of undergraduate students did not start their postsecondary schooling the same calendar year that they completed high school according to Horn et al. (2005). Their sample is based on all students aged 18–56.

likely to be different. Currie & Moretti (2003) use college openings as an instrument for mother's education and find that education reduces completed fertility. Overall, there appears to be a consensus that educational decisions and thereby educational policy affect many other important life decisions including family formation decisions.

We contribute to the current literature on several dimensions. First, we use a very different source of exogenous variation than existing studies, namely college admission requirements. Thus, we use exogenous variation that affects individuals at a much later stage in life than almost all of the studies mentioned above. This type of variation is interesting to study from a policy perspective, since to some extent it is a policy choice whether or not to encourage immediate enrollment in college. Secondly, we will show that our instrument does not affect the obtained level of education, so this this is not the channel behind our result. This is opposed to many of the studies using school starting age as an instrument. The main point in these studies is exactly that years of education is an important determinant of fertility. We argue that there may be an additional mechanism through which school starting age affects fertility, namely the timing of educational progression. We show that individuals who are affected by the admission requirements are induced to postpone their college entry, but not alter their college-going decision as such. Therefore, we interpret our estimates as reflecting primarily a timing effect and not a human capital effect. We find that later college entry leads to delays in family formation and possibly to changes in completed fertility. Compared to the literature using school starting age, we find much larger effects on family formation outcomes.

The main contribution of this paper is to estimate the causal effect of college enrollment in a given year on later family formation decisions. As such, we estimate the effect of a particular type of career interruption on the timing of fertility and relationship formation. In order to deal with the likely endogeneity of the decision of when to enroll in college, we use variation in enrollment based on the college admission system in Denmark.[2] Danish colleges have capacity constraints, and a centralized admission system ensures that the most able applicants in terms of high school GPA are allocated to their preferred college programs, resulting in an effective grade requirement for each college program each year. Applicants with a GPA above the requirement are admitted, and applicants with a GPA below the requirement are rejected.[3] Since the grade requirements are unknown to the applicants when they apply, the admission system potentially generates exogenous variation in enrollment.

In order to interpret the estimates of the effect of college enrollment on fertility and marriage, we also consider how college completion and earnings are affected. At best we find small effects on the level of college completed and conclude that the primary effect of the admission system is to delay educational attainment. Similarly, earnings in the years following application are

[2]See Öckert (2010) for an example of a study using a college admission system to identify causal effects of education on earnings.

[3]In reality the admission system is more complex. Additional details are given in section 1.2.

relatively unaffected. Especially, in the long run, we see no indication that the timing of enrollment affects earnings. We find that the timing of family formation is affected by the timing of college enrollment, e.g., enrollment in the year of application increases the probability of being a parent five years later by about 6 percentage points — corresponding to an increase of about 100 percent. We find effects of similar size on the probability of being married, but the estimates tend to be more precise for parenthood than for marriage. The pattern of the estimates is consistent with a setting in which the level of progression in the educational system affects individuals' choices regarding family formation and especially childbearing.

Even though our estimates suggest relatively large effects of delayed enrollment, the macro-level effects might be even larger. Feyrer et al. (2008) argue that if large peer effects exist in fertility choices, even individual-level effects might have large effects on the macro level. Ciliberto, Miller, Nielsen, & Simonsen (2010) offer evidence that peer effects in fertility exist at the workplace, while Kuziemko (2006) presents evidence on sibling peer effects in fertility choices. In the presence of peer effects, our individual-level estimates represent a lower bound of the macro-level effect.

The paper is organized as follows. In section 1.2 the institutional settings and the Coordinated Enrollment System for colleges in Denmark are presented. In section 1.3 we describe our empirical approach. Section 1.4 describes the data used, and in section 1.5 we present our estimation results. Finally, section 1.6 concludes.

1.2 Institutional Settings

In Denmark compulsory schooling has a duration of 9 years, and children usually start school at the age of seven.[4] After compulsory school further education can be obtained by attending either a vocational education and training program or a high school program. In high school, the students can choose between a business track, a technical track, or an academic track. High school programs and to a smaller extent vocational programs serve as qualifying educations for entering college programs. The costs of going to college are negligible in Denmark compared to, for example, the US, as the college programs are free and publicly provided, and the government provides very generous student grants.[5] It is also possible to take up additional student loans at favorable terms. In general, the student grant is set to cover living expenses.

[4] In 2009 this was changed to 10 years of compulsory schooling starting at the age of six.

[5] In 2010 the student grant for a student not living at home was DKK 5,384 corresponding to approximately USD 900 per month.

1.2.1 The Coordinated Enrollment System

All applications to college programs in Denmark are handled by a centralized admission system, the Coordinated Enrollment System (CES). The applicant can apply to up to eight different college programs in the same application. For a large number of college programs there are more applicants than available slots, which implies that college applicants are potentially constrained in their choice of college program.

CES allocates applicants to educations such that the best applicants are allocated to their preferred college programs. The system is complex, but the majority of applicants are assessed exclusively on their high school GPA. In addition, each college program has some basic requirements that mainly consist of high school course requirements. Each college reports to CES how many slots they have in each program. The applicants are ranked according to their high school GPA. Higher ranking applicants are admitted, and the admission requirement for a particular college program is set at the GPA of the marginal applicant. Applicants with a GPA above the admission requirement are offered slots, whereas applicants with a GPA below the admission requirement are rejected. In this way, the admission requirement for each college program is determined each year after the application deadline. Even though the number of slots in each program varies little from year to year, the number of applicants varies, which implies that the admission requirement varies. Thus, applicants do not know the admission requirement at the time they apply, and at least for individuals with a GPA close to the admission requirement, it will not be possible to predict whether the admission requirement of a given college program will be above or below their GPA.[6]

Many programs use a simple admission rule for applicants with a GPA equal to the admission requirement: They simply offer admission to all of these applicants. Another common rule is an age tie-breaking rule. The rule implies that when two applicants are tied with respect to GPA, the oldest applicant is admitted. In addition, some college programs use other types of admission rules for these marginal applicants. In the empirical analysis, we focus on programs that offer admission to all applicants with a GPA equal to the admission requirement.

As previously mentioned, some applicants are assessed on characteristics besides their high school GPA. One can apply based on a point system which gives points partly for the GPA from the qualifying education and partly for other activities such as relevant work experience, stays abroad etc. The slots in a given year allocated to such applicants constitute a minor part of the total number of slots.

The system also allows applicants to apply for standby slots, i.e., the applicant is guaranteed a slot in the next academic year at the latest. These slots are allocated to applicants who have a GPA slightly below the admission requirement. For college programs that offer standby slots, a standby requirement will be determined along with the admission requirement. The existence

[6]We document the time variation in the admission requirements in section 1.4.

of the standby slots implies that individuals who are just below the admission requirement are likely to enroll in their preferred college program within a year of their original application, if they have applied for a standby slot.

1.3 Empirical Approach

The empirical approach is inspired by the institutional settings described above. The basic idea is to use the fact that applicants at the time of application cannot perfectly predict what the admission requirement is going to be in a given college program. The underlying assumption behind the identification strategy is that applicants who end up being just above or just below the admission requirement are essentially the same ex ante. If the admission requirement for an applicant's preferred program turns out to be above the applicant's GPA, there is a much lower chance of being admitted to that program than if the admission requirement turns out to be below the applicant's GPA. Thus, one can think of this as a fuzzy regression discontinuity design, if one considers each college program in each year separately.

1.3.1 Instrumental Variables

The goal of this paper is to estimate the causal relationship between an outcome, say, being a parent, y_i, and enrollment *in the year of application*,[7] e_i, where i indexes individuals.

$$y_i = X_i\beta_0 + \delta e_i + \varepsilon_i.$$

Estimating this relationship assumes that e_i and the error term ε_i are independent, which is unlikely to be a valid assumption given that those who choose to enroll at a given point in time constitute a selected group of people. Following, among others, Angrist & Lavy (1999), who use an RD inspired IV method, we use the RD design described above to instrument enrollment.

Let d_i measure the distance from the admission requirement of individual i's preferred college program to individuals i's high school GPA.

$$d_i = GPA_i - GR_i \qquad \begin{cases} < 0 \Rightarrow t_i = 0 \\ \geq 0 \Rightarrow t_i = 1 \end{cases}$$

where GPA_i is the grade point average for individual i, and GR_i is the grade requirement at individual i's preferred program. Under the assumption that applicants cannot perfectly predict the grade requirement, t_i will be uncorrelated with later outcomes except through its effect on the enrollment decision for d_i sufficiently close to zero. Thus, for applicants with distances sufficiently close to zero, we can estimate the causal effect of enrollment on later

[7] Specifically, e_i is an indicator variable that is equal to 1 if individual i enrolls in the year of application. If $e_i = 0$, individual i may enroll later or not at all.

family formation outcomes by instrumenting enrollment with the indicator for being below or above the grade requirement, t_i. The IV strategy is based on the assumption that t_i affects family formation outcomes only through its effect on enrollment. For example, if t_i affects the probability of ever enrolling in college, then family formation may also be affected by t_i through this channel, and the IV estimate of δ will not be consistent. In section 1.5, we will present evidence that t_i affects the timing of enrollment but not the enrollment decision per se.

The first stage is estimated by

$$e_i = \alpha_1 + f_1(d_i) + \gamma_1 t_i + h_1(GPA_i) + X_i\beta_1 + \varepsilon_{1i}$$

where f_1 is a function such as splines or polynomials approximating the underlying variables. h_1 is a function of the GPA. For a single program in a single year, GPA and distance would be perfectly correlated, but since we consider several programs in several years, the effects of GPA and distance can be separately identified. Thus, we are able to include a measure for ability (GPA) even though the admission system is in fact based on admitting the most able. This is important since the forcing variable, d_i, to some extent reflects GPA and therefore may be correlated with the outcome. X_i is a vector of control variables such as age, gender, parental background etc.

A central question in relation to the empirical implementation of this strategy is when the distance is sufficiently close to zero. A wider window—in terms of distance—will increase precision of the estimates, but it can introduce bias. By looking at how the admission requirements actually change over time in section 1.4.3, some rough guidelines can be deduced. In the end, we also perform some sensitivity analysis on this dimension to check the robustness of our results.

1.3.2 Interpretation of Estimates

The estimated effects are based on IV estimation, and assuming that treatment effects are homogenous, we are estimating average treatment effects (ATEs). However, in the case of heterogeneous treatment effects, the interpretation of the estimated effect is that of a Local Average Treatment Effect (LATE). In this case, we can think of the estimated effect as being the average treatment effect of enrollment on later family formation outcomes for those applicants who choose to enroll because they were above the admission requirement but who would not have enrolled otherwise. This parameter is of interest to policy makers, since it is the equivalent of increasing capacity at the universities, e.g., giving marginal students in each field access to their desired program. This is exactly one of the policy handles that policy makers have at their disposal when colleges are public and admission is centralized.

We estimate the effect of enrollment in the year of application on later family formation decisions. Hence, the year of application (henceforth we will use YOA instead of year of application) is the obvious reference point, and we consider marriage and parenthood x years after application. In order to be more specific about the ages at which we are looking, we also consider marriage and parenthood at specific ages. Since there is some variation in the age of the applicants in the YOA, the resulting estimates based on outcomes at specific ages are blurred to some extent. Especially, if family formation decisions are indeed based on the stage of career that the individual is at.

1.4 Data

The data used for the empirical analyses are administrative data hosted by Statistics Denmark that covers the entire Danish population. These are linked to data from the Coordinated Enrollment System (CES) for college programs in Denmark. The combined data set contains detailed information on young individuals, their college-related choices and preferences, and their educational and family background.

The CES data contains information on all applications to college programs in Denmark in the period 1996–2006. Thus, the data provides information on all individuals who *intended* to enroll in college during this period. Since a college application to CES includes a prioritized list of college programs, we can distinguish between the applicants' actual college choices and their stated college preferences.

1.4.1 Description of the Sample

To obtain a suitable sample for the analyses described in the empirical section, we introduce a number of restrictions on the data. Table 1.1 provides an overview of the sample selection. The data contains information on about $600,000$ applications to college programs in the period 1996–2006.

We focus on the first application[8] where the preferred program is a long-cycle college program.[9] By using only the first application, we are ensuring that the sample is more homogeneous and avoid giving more weight to individuals who apply repeatedly. It is a requirement for our empirical strategy that the preferred program has a grade requirement, i.e., that the admission requirement is a grade requirement, and, in addition, that the preferred program has a simple enrollment rule for the marginal applicants, cf. section 1.2. Whether or not a particular college program has a grade requirement can vary from year to year. To be included in the sample, the preferred program has to have a grade requirement in the year of application. From Table 1.1,

[8]Since we only have application data from 1996 onwards, we cannot be sure that we can identify the first application. We therefore impose the restriction that only individuals who graduate high school in 1995 or later are included in the sample.

[9]Long-cycle college programs are programs at university level.

it is clear that the vast majority of individuals apply to programs with a grade requirement. However, many programs do not have a simple enrollment rule for marginal applicants. The estimation sample cannot be considered a random sample of college applicants. It consists mainly of applicants for relatively prestigious college programs at the two largest universities in Denmark.

In order to compute the distance to the admission requirement for each individual applicant, information about the high school grade point average (GPA) is required. GPA is missing for a large number of applicants which is mainly explained by the presence of applicants with a foreign high school degree or types of qualifying education where GPA was not registered.

In order to obtain a relatively homogenous sample in terms of age—which is arguably one very important factor in fertility decisions—we focus on applicants who were 20 years old or younger when they applied. Younger applicants are more likely to be affected by the admission system since they are less likely to have formed relationships and families. Later, we will compare the results for young applicants with the results for older applicants.

After imposing all of the above restrictions on the sample, 19,327 observations remain. These include all applications who satisfy the above criteria, but as argued above, the empirical strategy is only valid for applicants whose GPA is in the vicinity of the admission requirement of their preferred program in the year of application. In the main analyses, a window of 0.3 is considered, i.e., 6,313 observations.[10] Figure 1.1 shows the distribution of the distance from the admission requirement to high school GPA before imposing the window restriction. The distribution is centered slightly to the left of 0 and resembles a normal distribution. We highlight two features of this graph. First, there is a lot of variation around 0, so the window of 0.3 does not seem to be unreasonable. Secondly, there is no sign that individuals are bunching just to the right of 0. This indicates that individuals cannot perfectly predict the admission requirement. We will return to these points later.

1.4.2 Descriptive Statistics

Table 1.2 shows the sample means for the main covariates, four selected outcomes, and four enrollment indicators separated by whether an individual is above or below the admission requirement. The means are shown for a window of 0.3. In addition, the two columns to the right show the difference in means and whether this difference is statistically significant for a window of 0.3 and 0.1, respectively. The identification strategy requires the groups above and below the admission requirement to be relatively similar. However, Table 1.2 reveals a few differences—even for the smaller window. Specifically, the GPA is significantly higher for individuals above the admission requirement than for individuals below the admission requirement, as is expected

[10]See section 1.4.3 for further discussion of the choice of the size of the window and section 1.5.5 for some robustness checks.

since distance is based on GPA. Individuals above the admission requirement are more likely to prefer Humanities and less likely to prefer Health Science. Overall, the differences between the two groups are small, except for GPA. Since we can use variation in the GPA requirement over years to control for GPA in the estimations, these slight differences should not be of major importance for the empirical strategy. Considering the selected outcomes, there is some initial indication that individuals above the admission requirement are moving faster into parenthood. For enrollment, we see that individuals above the admission requirement are much more likely to enroll in the YOA as expected.

We consider the following two family formation outcomes measured at a specific age or a specific number of years after application: the probability of being a parent and the probability of being married. In Denmark premarital cohabitation is widespread and therefore marriage is not the best indicator for family formation, but we include this outcome in lack of a better measure. Figure 1.2 shows how the probabilities of these outcomes evolve over time. Generally, the probability of being a parent and the probability of being married are close to zero for very young individuals or around the YOA. The probabilities are increasing and at age 30, 40 percent are parents and 30 percent are married. The picture is roughly the same 10 years after YOA. Clearly, the time period covered constitutes a stage in these individuals' lives where family formation is important.

1.4.3 Variation in Grade Requirements Over Time

If applicants are able to predict the grade requirement of their preferred program in the year of application, applicants who are above and below the grade requirement are likely to be systematically different. Table 1.3 provides a rough picture of how much the grade requirements vary from year to year. The table reports the mean absolute change and the standard deviation of the absolute change in grade requirement for each program for each year. The weights used are based on the number of applicants in the estimation sample that applies to a given program in a given year. Thus, the weighted mean is the more relevant measure in terms of individuals in the estimation sample.

The unweighted measure shows an average absolute change in grade requirements of about 0.33, whereas the weighted mean is considerably lower at about 0.17, corresponding to a yearly change of approximately 2–4 percent, which implies that potential applicants will not be able to perfectly predict the grade requirement in the year they apply. The standard deviations are also quite high, which again makes it harder for potential applicants to forecast the cutoff. Presently, these measures should be considered relatively conservative, as changes in grade requirements that reflect a change from a non-binding capacity constraint to a binding capacity constraint are not included. Such changes in grade requirements would tend to be larger. The presented results will be based on applicants whose distance from the admission requirement of their preferred education is −0.3 to 0.2. Based on the statistics in Table 1.3, we consider this a reasonable window. However, as this is a crucial assumption in the analysis, robustness of results with respect to this assumption will also be investigated.

1.5 Results

For the purpose of comparison, we will begin by presenting OLS estimates of the outcome equation, i.e., ignoring the potential endogeneity of enrollment. Then, in order to motivate our IV strategy, we will show how enrollment is affected by the admission system. Subsequently, we will present IV estimates—and corresponding robustness checks—of the effect of enrollment on family formation using an indicator for being below or above the admission requirement as an instrument for enrollment.[11]

1.5.1 OLS Estimates

As a starting point, we estimate the outcome equation without using an instrument. We estimate the effect of enrollment in the YOA on being a parent or being married at specific ages or years since application. We use two different samples for the OLS estimations. The first is labeled the 'full sample', which is the original sample of applicants after having dropped observations that are not related to the first application (195,576 observations) and observations where the applicant is older than 20, cf. Table 1.1. The second sample is the estimation sample. Figure 1.3 shows OLS estimates for the four different outcomes for each of the samples. These estimates are of interest to us since they provide a relevant benchmark for the IV estimates. The OLS estimates are all positive, but not always significant. For the probability of being a parent, the estimates are slowly increasing from zero to around two percentage points for the estimation sample and less for the full sample. For marriage the estimates increase sharply from around zero to around two to three percentage points around age 26 or about 7 years after YOA. The OLS estimates based on the estimation sample are consistently larger than the ones based on the full sample. This indicates that individuals in the estimation sample are slightly more affected by enrollment than individuals in the full sample, or alternatively, that there are varying degrees of selection bias in the two samples. We will compare these estimates with estimates obtained by IV, but one should keep in mind that there is no basis for comparison, unless we assume that treatment effects are homogeneous. With heterogeneous responses, the IV estimates are ATEs for a specific subgroup namelig the compliers. Thus, the OLS and IV estimates are not directly comparable in this case.

[11] Unless otherwise stated, the presented results are for a window of 0.3, i.e., for distances to the admission requirement of −0.3 to 0.2.

1.5.2 The Discontinuity in Enrollment

Whether or not a prospective student chooses to enroll in college in a given year is likely to be an endogenous variable. As described earlier, we therefore instrument college enrollment by whether or not individuals are above or below the admission requirement of their preferred program in the YOA. Figure 1.4 illustrates the suitability of this instrument. There are four graphs in the figure representing different enrollment measures. The top left graph shows the probability of enrolling in college in the YOA by distance from the admission requirement of one's preferred college program. This graph shows a discontinuous jump in enrollment. The top right graph instead shows the probability of *ever* enrolling in college. While the probability of college enrollment appears to have a kink at the cutoff, there is no indication that the propensity to enroll in college jumps discontinuously. This suggests that the admission system does not discourage these individuals from attending college, but it does affect the timing of college enrollment. The two bottom graphs in Figure 1.4 instead show the probability of enrolling in one's preferred college program. Again we see a substantial jump in the probability of enrolling in the year of application, but for the probability of ever enrolling in one's preferred college program, we do not see such a jump. While these graphs suggest that the admission system primarily affects the timing of enrollment, of course, we cannot completely rule out that other dimensions such as the choice of college program are affected as well.

The vertical discontinuity in the probability of enrolling in college in the YOA is almost 45 percentage points, i.e., being above the admission requirement increases the probability of college enrollment by about 45 percentage points. Individuals who are not admitted to their preferred program may still be admitted to another college program. The probability of ever enrolling in college tends to be increasing in the distance from the admission requirement. This is consistent with more able individuals (i.e., with higher GPAs) being more likely to enroll. Even for individuals below the admission requirement, the college enrollment rate is above 95 percent. Individuals below the admission requirement have a positive probability of enrolling in their preferred college program nonetheless. This is due to the fact that individuals who are below the admission requirement still can be admitted to their preferred program given that they meet a number of other criteria as mentioned briefly in section 1.2. Also, individuals above the admission requirement have a probability of enrolling in their preferred college program of less than 0.8. Again, there are a couple of potential explanations for this. First, individuals may decide not to enroll after all, even if offered a slot. Second, individuals may fail to meet some of the other official requirements, e.g., the high school course requirements.

Why is it that we do not see a discontinuity in the probability of ever enrolling in the preferred college program? If we imposed a more flexible fit on the data instead of a linear fit, we would see a roughly continuous but kinked curve. There are two particular features of the admission system that we think could generate this type of pattern. The first is that individuals can gain admission based on other criteria than the GPA, but the GPA may still carry some weight in the admission decision. This would generate the upward slope in the probability of enrolling in one's preferred college program to the left of the cutoff. It cannot explain the polynomial shape in the probability of ever enrolling in one's preferred college program. However, the presence of standby slots for individuals just below the admission requirement would generate exactly this pattern. Individuals just below the admission requirement are then guaranteed a slot in their preferred program in the following year. If everyone who were just below the admission requirement were offered standby slots, we could think of compliers as individuals who would have enrolled if they were above the admission requirement, but who would have been given a standby slot, if they were below the admission requirement and therefore delayed enrollment by one year.

1.5.3 IV Results

In this section we first analyze in depth the effect on our two outcomes at two specific points in time, namely at age 26 and 5 years after YOA. We then proceed by estimating the IV regression for all ages and years after YOA for each outcome. It would of course be nice to be able to present both graphical evidence and detailed results from the IV regressions for all outcomes. However, due to spacial considerations we proceed in this manner.[12] We are not considering the points in time where the estimated effects are largest, as will be evident later. However, we have chosen points in time where something appears to be going on, cf. Figure 1.2. A priori, we would expect many individuals to start making family formation decisions around these points in time. At age 26, individuals are still young, but relatively mature adults. 5 years after YOA, many individuals will have graduated from college.

Figure 1.5 provides a graphical illustration of how the IV strategy works for the four selected outcomes. The figure shows a graph of each outcome by distance from the admission requirement. For being a parent at age 26 and 5 years after YOA, the graphs are consistent with an RD setup. For marriage the graphs are not quite as convincing. This suggests that these estimates might be slightly more sensitive to changes in the size of the estimation window.

Table 1.4 shows the IV estimates for the four selected outcomes. The table also includes the coefficient of the instrument, the t-statistic, and the r-squares from the first-stage regressions. The t-statistics are generally high, about 21–23, suggesting that the instrument is very strong. The estimated effects of enrollment are relatively large for parenthood and statistically significant at the 1 percent level. Enrolling in the year of application increases the probability of being a parent by 9.4 and 6.0 percentage points at age 26 and 5 years after YOA, respectively. Given that the average probability of being a parent at age 26 is about 10 percent at age 26

[12] Detailed results are of course available upon request.

and 5 percent 5 years after YOA, enrolling in the YOA just about doubles the probability of being a parent at these specific points in time.

The estimated effects of enrollment on the probability of being married at age 26 and 5 years after YOA are 3.2 and 2.1 percentage points, respectively. However, these estimates are insignificant. Gender is an important determinant of all four family formation outcomes. Women are generally more likely than men to have begun some family formation at the specific points in time considered here. The age of the applicant's parents at birth appears to be the most important (of the parental background variables) determinants of the family formation outcomes. Overall, the pattern is consistent with some form of intergenerational transmission of family capital or preferences for family formation, since individuals whose parents were younger when having children are also more likely to have achieved some degree of family formation themselves by the specific points in time considered here. Especially, the mother's age at birth is an important predictor of the timing of the child's family formation.

Generally, the coefficients on the indicators for the preferred field of the applicant are relatively small and mostly insignificant. However, it seems to be a pattern that individuals who prefer college programs in Natural Science are less likely than individuals who prefer college programs in Social Science to have started family formation at age 26 or 5 years after YOA. The family formation status in the YOA, i.e., being a parent in YOA and cohabiting in YOA, is an important determinant of later family formation outcomes. As expected, the coefficients on these two variables are positive and highly significant for most outcomes. It is interesting though that being a parent in YOA is negatively associated with the probability of being married later on. Since the individuals are relatively young, being a parent in the YOA more or less implies that you became a parent as a teenager, and so the negative association seems reasonable.

In order to illustrate how sensitive the IV estimates of the effect of enrollment on the selected outcomes are, Table 1.5 shows how the IV estimates change when different groups of covariates are gradually included. The size of the estimated coefficients and standard errors are not particularly sensitive to the inclusion of different covariates. Based on the results in this table, i.e., the fact that inclusion of covariates matter relatively little, the identification strategy appears to be relatively sound. Column (7) in Table 1.5 represents the results from Table 1.4. This provides suggestive evidence that the few differences in observed means between the treatment and control group in Table 1.2 do not seem to be overly important for the outcomes of interest.

The detailed IV estimation above was carried out for two specific points in time, i.e., at age 26 and 5 years after YOA. In order to get an overview of our main results, Figure 1.6 shows corresponding IV estimates of the effect of enrollment on fertility and marriage plotted against either years after application or age. Already four years after YOA we find a significant effect on the probability of being a parent. This positive effect continues to increase until 10 years after YOA. A similar pattern is seen for ages 24 to 30. However, here the effect seems to stabilize after age 27. There is little indication of catching-up in either of the outcomes regarding parenthood. The coefficient estimates are almost as large as the population averages, as shown in Figure 1.2. This indicates that enrollment is a very important channel for family formation and especially for parenthood. Turning to marriage, results are less pronounced, but the overall pattern is fairly similar.

Figure 1.7 shows IV estimates separated by gender. There seems to be a tendency for the estimates on parenthood to be larger for women than for men, although the difference is never statistically significant for any one year. There seems to be no real differences in the estimated effects on being married for men and women. In our main estimations, we have disregarded old applicants. Figure 1.8 shows how the IV estimates vary by age of the applicant, i.e., for young and old applicants. For old applicants, the estimated effects are never statistically different from zero and the confidence intervals are very wide. They constitute a more heterogeneous group and are less likely to be affected by the admission system since they are more likely to have already made their family formation decisions.

A general weakness of the analysis is that we are unable to consider completed fertility as an outcome, since the individuals in our sample are simply too young. That being said, our results do not suggest that the effect on parenthood is temporary. Often the decisions about the timing of the first child and completed fertility are considered linked. D'Addio & d'Ercole (2005) remark that delays in family formation lead to decreases in fertility rates as well as increased health risks for mothers and their children and increases in the extent of childlessness. Thus, delayed family formation is likely to be associated with both economic and psychological costs.

1.5.4 Why Does the Admission System Affect Family Formation?

The estimated effects presented above are average treatment effects of enrollment on later family formation outcomes for those applicants who chose to enroll in the YOA because they were above the admission requirement but who would not have enrolled otherwise. We now discuss a couple of potential threats to the internal validity of the estimates. The main question is why the admission system affects family formation. Does it induce differences in the timing of enrollment for individuals above or below the threshold? Does it induce differences in the type of college program attended? Or both?

First of all, we have already documented, in Figure 1.4, that the probability of enrolling in college does not jump discontinuously at the admission requirement. Figure 1.9 shows that enrolling in the YOA does not affect the probability of having completed at least a three-year college program in the long term.[13] In the short term (3-4 years after YOA) there is a substantial positive effect of enrolling in the YOA as we would expect. The graph is consistent with the notion that the main effect of the admission system is to delay some individuals in their educational career and not to change their career path as such.

Second, we also documented in Figure 1.4 that the probability of enrolling in one's preferred college program does not jump discontinuously at the admission requirement. This is a major concern for the interpretation and validity of the IV estimates. If being below or above the grade requirement affects the choice of college program, then this difference in attended college programs could be driving the differences in the timing of family formation. An example of this could be if individuals below the grade requirement are more likely to enroll in college programs within Natural Science and individuals who study Natural Science tend to delay family formation. Differences in the timing of family formation across college programs could arise due to differing sex ratios or social norms. To explore this issue further, we include in our baseline IV estimations a detailed set of indicators for the actual college program that the individual enrolls in. The results are shown in column (1) of Table 1.6. Of course, the included indicators for actual college program are endogenous in the sense that they are determined after the admission requirement is determined. Put differently, being above or below the admission requirement can potentially affect the choice of actual college program. Including the indicators for actual college program has little effect on the IV estimates. This suggests that the differences in family formation that we document are not driven by differences in the college programs attended.

Finally, we consider another potential outcome that could be affected by the admission system, namely earnings. If individuals rank college programs according to earnings potential and the college program realization differs for individuals below and above the admission requirement, we would expect individuals above the admission requirement to end up with higher earnings, ceteris paribus. Higher earnings is usually also considered to increase one's value on the marriage market. As such, the differences in timing of family formation could be caused by differences in expected match quality of individuals below and above the admission requirement. To explore these issues, we include in our baseline IV estimations measures of earnings 5 and 10 years after YOA, cf. Table 1.6. Similar to the inclusion of actual college program, this has little effect on the estimates. This suggests that the differences in family formation that we observe are not driven by differences in earnings which could arise due to differences in college programs attended. Earnings may also be an additional channel through which the enrollment in the YOA affects family formation. An increase in earnings can lead to both a

[13] This measure is chosen in order to have a comparable measure across college programs and to minimize problems with right censoring. Most long-cycle programs actually consist of two parts: A three-year degree and a two-year degree on top of that. The vast majority of college graduates from the long-cycle programs complete both degrees.

substitution effect – it becomes more costly to spend time on children – and an income effect. Figure 1.9 shows the IV estimates of the effect of enrollment in the YOA on earnings. There is a naturally negative effect in the first year after YOA, since those who do not enroll typically 'take a year off' to work implying that they have high earnings compared to individuals who are studying. In addition, there are positive and significant effects of enrollment in the YOA on earnings 6-7 years after YOA. Around this time, individuals who enroll in the YOA are more likely to have left college and entered the labor market. Thus, the result that delayed college enrollment delays family formation can to some extent be driven by differences in the earnings trajectories of individuals who delay enrollment and individuals who do not. In this case earnings have a positive effect on the demand for children. In the long-run there is no significant effect of enrollment in the YOA on earnings. This is consistent with the very low returns to college found by Öckert (2010). He uses a similar identification strategy to estimate the effect of being admitted to college on earnings.

Our main concern is that individuals below the admission requirement are forced to enroll in different types of college programs and that this generates the observed differences in family formation outcomes for individuals above and below the admission requirement. However, given that we have shown that the two groups differ very little in terms of enrollment, completion and earnings, it seems implausible that differences in attended college programs are generating these very large differences in family formation. Thus, neither differences in actual enrollment, educational level, nor later earnings seem to be the main channel. The estimates presented in Figure 1.6 suggest that the effect on parenthood starts 4 years after YOA. This roughly coincides with the time where the shortest of the long-cycle programs that we consider ends for those who have enrolled in the YOA. We take this as suggestive evidence that the main channel is actually the timing of labor market entry. One reason for this could be that individuals want to postpone having children until they have established a labor market career. This would be consistent with Del Bono, Weber, & Winter-Ebmer (2012) who find that career oriented women who are laid off after a plant closure delay having children. This is supported by the findings in Figure 1.7, where the effect on parenthood appears to be higher for women.

1.5.5 Window Size

The choice of the window size is potentially very important. We therefore perform a robustness check on our four outcomes. Figure 1.10 shows how sensitive the estimates are to smaller changes in the size of the window. The full lines correspond to the estimates in Figure 1.6 with a window of 0.3. The results are very robust regarding different window sizes. All estimates using different window sizes are well within the 95 percent confidence interval of the main estimates. As expected the standard errors increase as the window is reduced and the number of observations becomes smaller.

1.5.6 Placebo Estimates

We also perform a placebo analysis. We pretend that there is a cut-off at 0.6 above the real cuf-off and then use the same empirical strategy as in the main analyses. Figure 1.11 shows the regular estimates as reported in Figure 1.6 and the placebo estimates. The placebo estimates are much closer to zero and none of them are significant at a 5 percent level.

1.6 Conclusion

We analyze a problem that has not been paid much attention in the literature on educational choices, namely how delays in the educational career affect the timing of family formation and especially fertility decisions. We argue that the identification strategy pursued allows us to identify the causal effect of delayed college enrollment on later family formation decisions. Other studies have sought to estimate effects of education on family formation, but they have focused on earlier educational interventions such as rules regarding school starting age and compulsory schooling laws that may have both timing and human capital effects.

We find strong evidence that delayed college enrollment affects the timing of later family formation decisions. The estimated effects tend to be substantial. We find that enrolling in the year of application doubles the probability of being a parent five years later. The effect on parenthood at a certain age is generally increasing, although there is some indication that the effect stabilizes at later ages. The OLS estimates greatly underestimate the effect. We find somewhat smaller and more imprecise effects on marriage. Clearly, this suggests that the timing of college enrollment—and as a consequence college completion—is an important determinant of later family formation decisions. Strictly speaking, we cannot say whether delays in the educational career also lead to a reduction in completed fertility, but we find no signs that the effect on fertility is temporary.

Theoretically, our results are consistent with several underlying explanations. First, potential parents may wish to smooth consumption and therefore postpone parenthood until they reach a certain income level. Secondly, career-oriented individuals may wish to gain a foothold on the labor market before starting up a family.

From a policy perspective, this highlights another potential cost of delays in the educational career, namely delayed family formation and childbearing. Many educational policies potentially affect not only the human capital formation of children and youth, but also the timing of that human capital, e.g., school entry rules, compulsory schooling laws etc. Our study highlights the importance of considering other consequences of educational policies than just the 'pure' human capital effects. Also, if increasing fertility rates is a policy objective, our results suggest that providing incentives for individuals to progress faster in the educational system might be desirable. In the specific context of delayed college enrollment, policy makers may want to consider how to increase incentives to enroll earlier, e.g., by increasing the quality of guidance counseling in high school.

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Appendices

1.A Figures

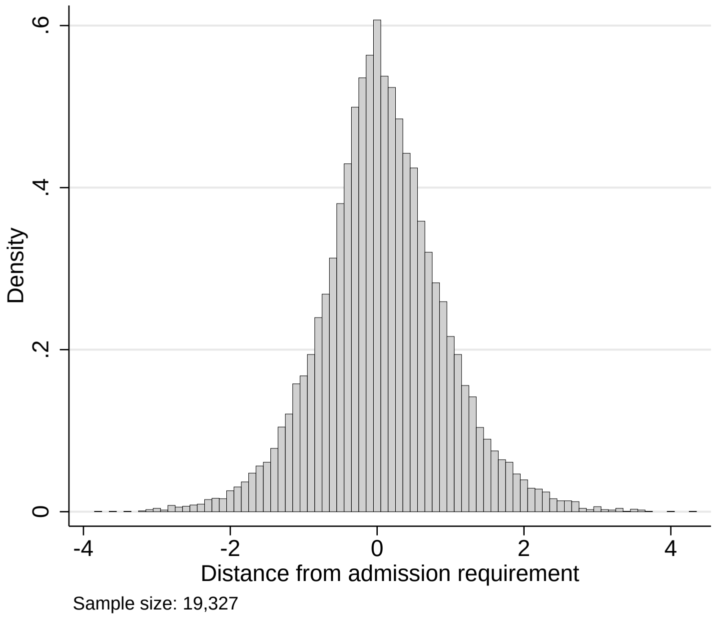

Figure 1.1: Distribution of distance from admission requirement.

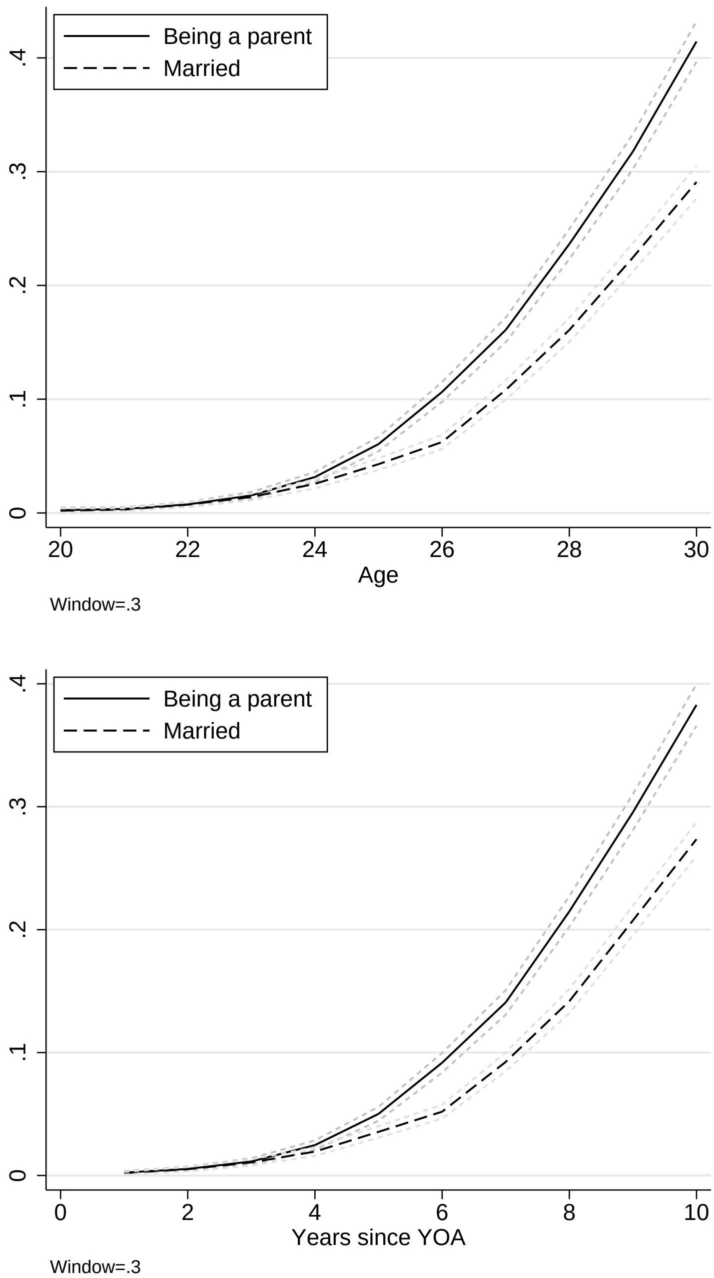

Figure 1.2: Probability of being a parent or being married by age and YOA. 95 percent confidence bands.

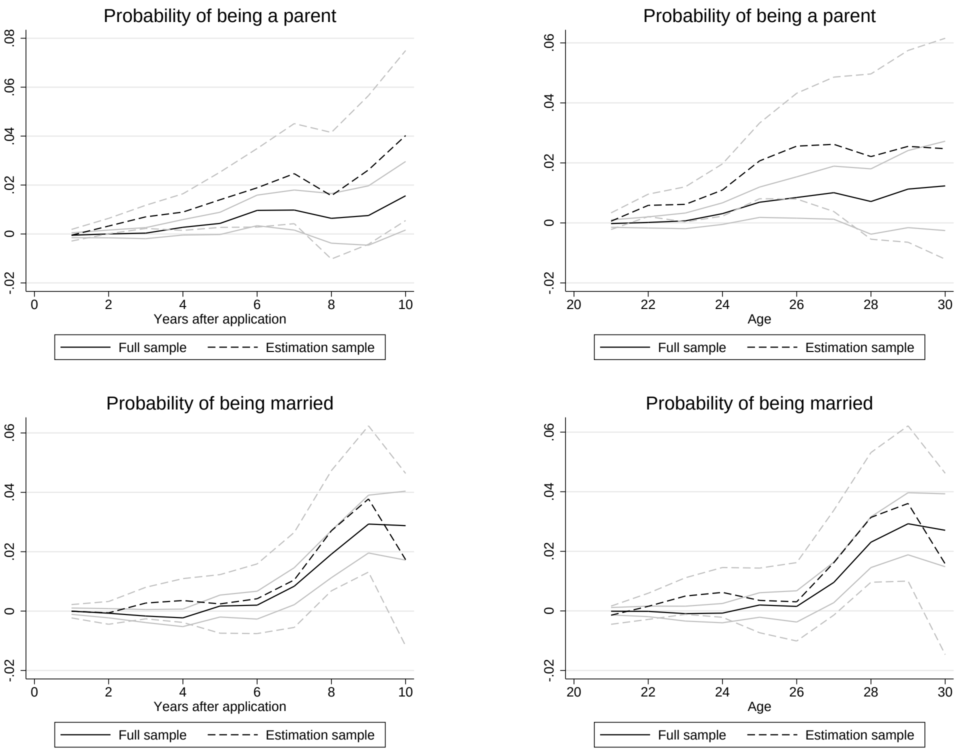

Figure 1.3: OLS estimates of the effect of enrollment on main outcomes and 95 percent confidence bands for the full sample and the estimation sample. Includes all covariates except distance. Standard errors are heteroscedasticity robust.

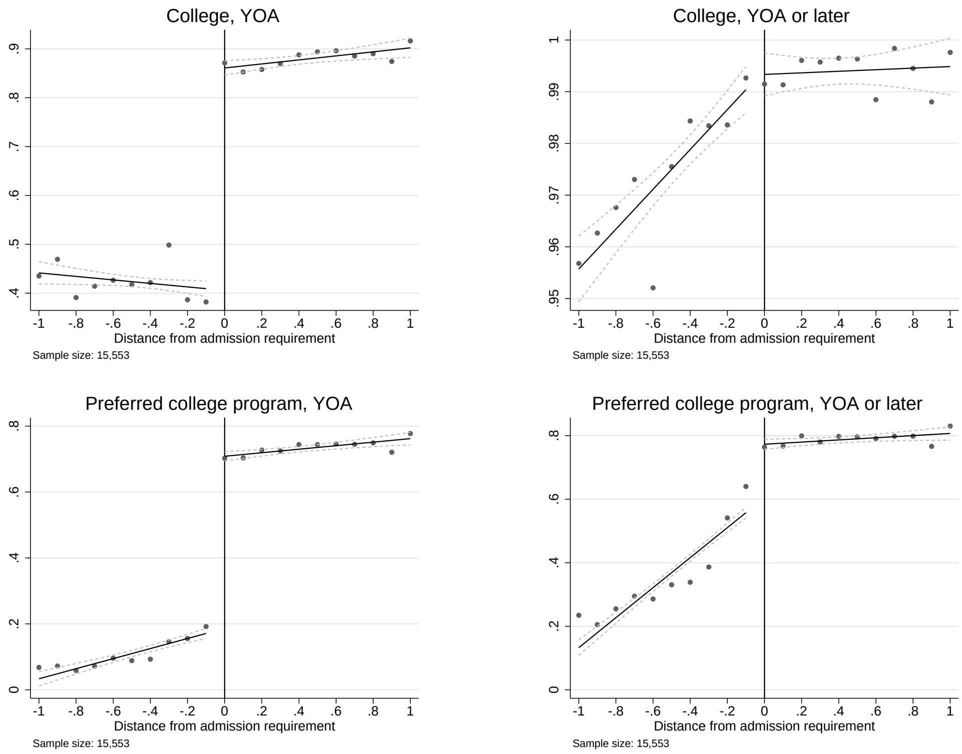

Figure 1.4: Discontinuities in enrollment by distance from admission requirement. Scatter plots are overlaid with fitted values and 95 percent confidence bands from linear regressions on distance, an indicator for being above the grade requirement, and an interaction of the two.

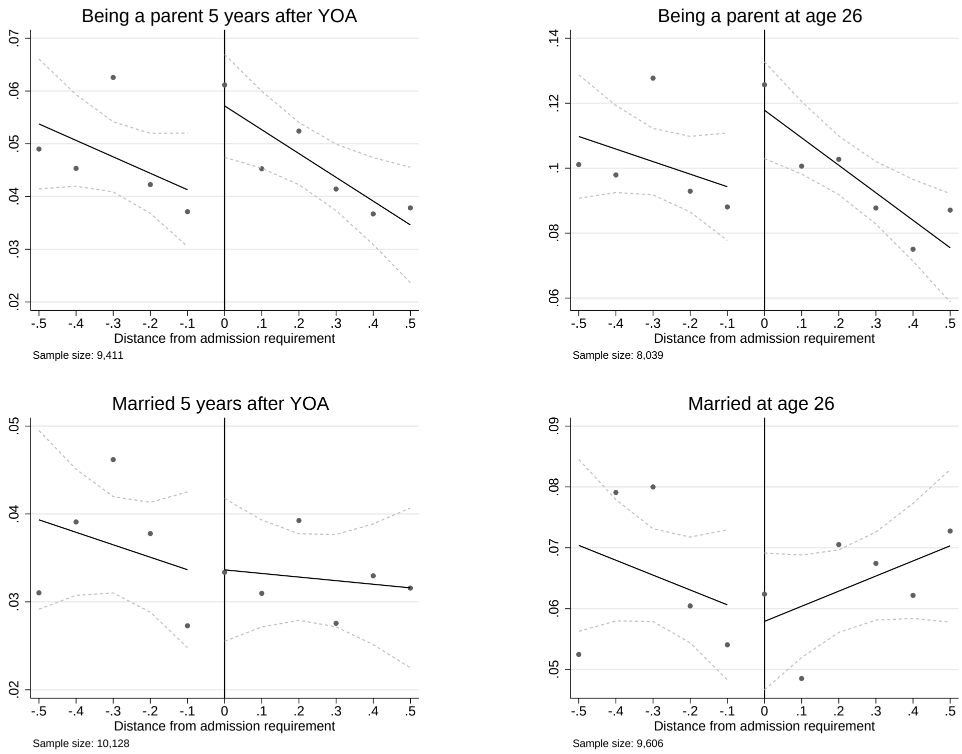

Figure 1.5: Selected outcomes by distance from admission requirement. Scatter plot is overlaid with fitted values and 95 percent confidence bands from a linear regression on distance, an indicator for being above the grade requirement, and an interaction of the two.

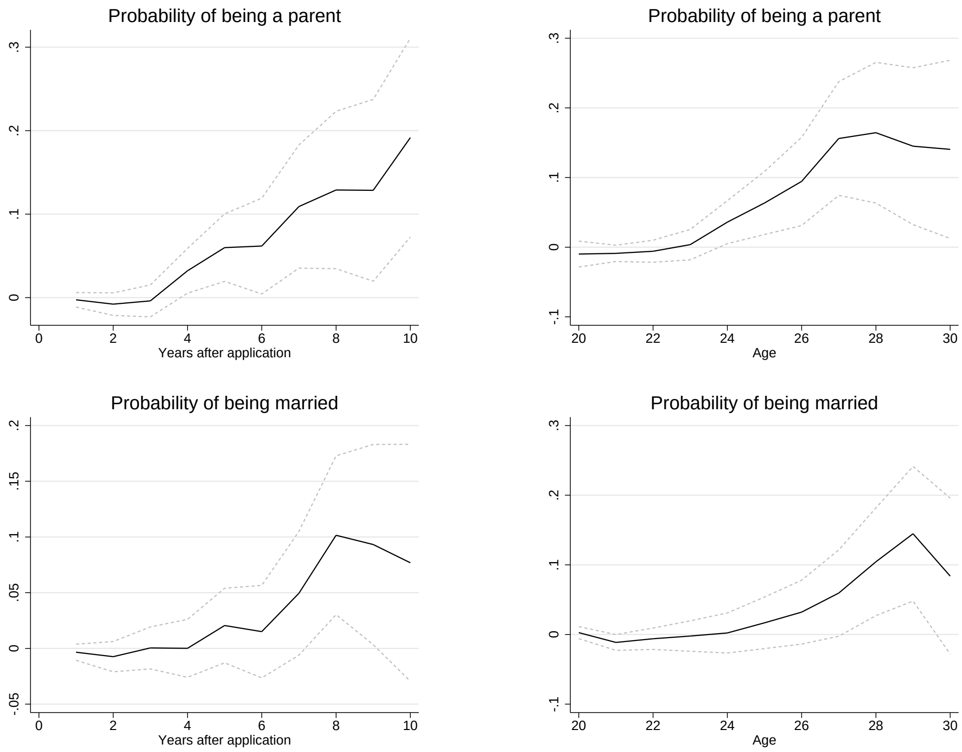

Figure 1.6: IV estimates of the effect of enrollment on main outcomes and 95 percent confidence bands. Includes all covariates. Standard errors are heteroscedasticity robust.

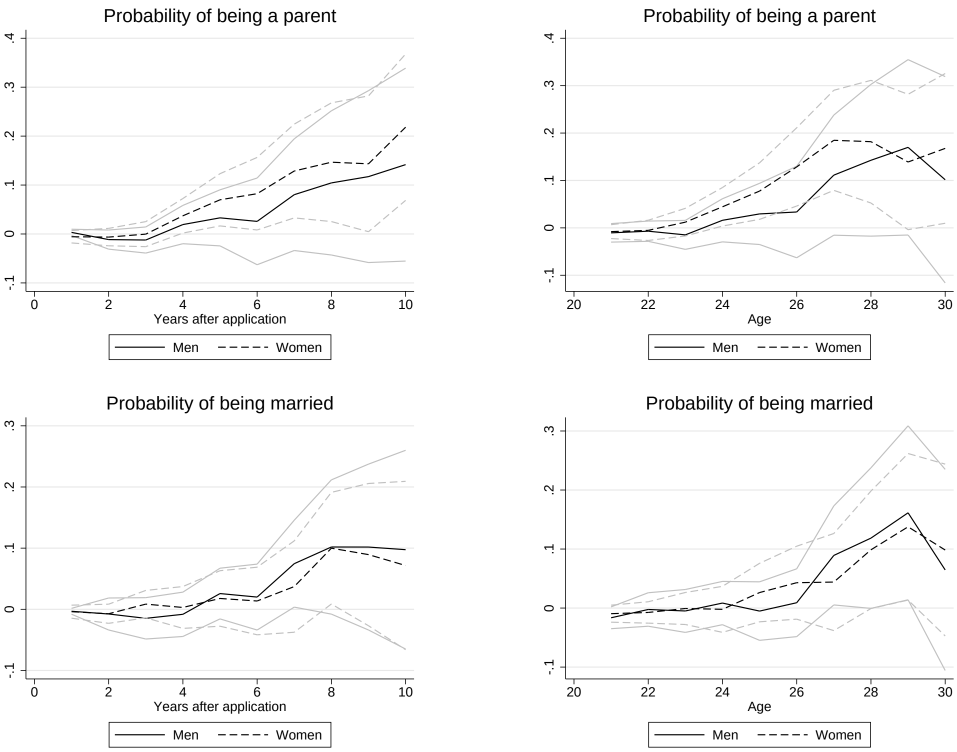

Figure 1.7: IV estimates of the effect of enrollment on main outcomes and 95 percent confidence bands by gender. Includes all covariates. Standard errors are heteroscedasticity robust.

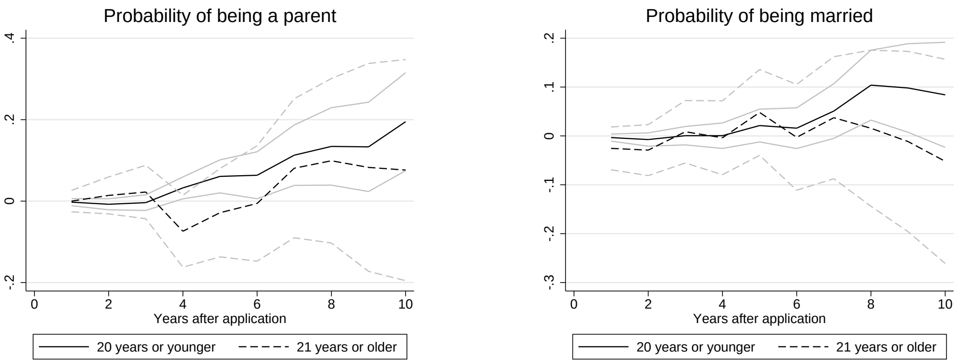

Figure 1.8: IV estimates of the effect of enrollment on main outcomes and 95 percent confidence bands by age in YOA. Includes all covariates. Standard errors are heteroscedasticity robust.

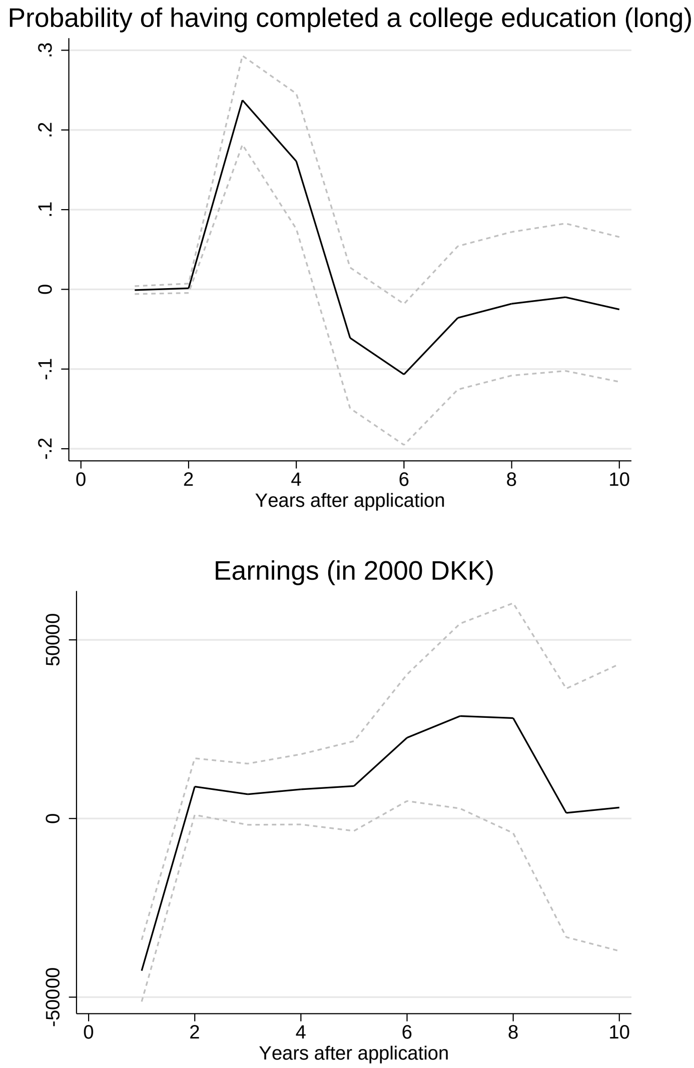

Figure 1.9: IV estimates of the effect of enrollment on college completion and earnings and 95 percent confidence bands. Includes all covariates. Standard errors are heteroscedasticity robust.

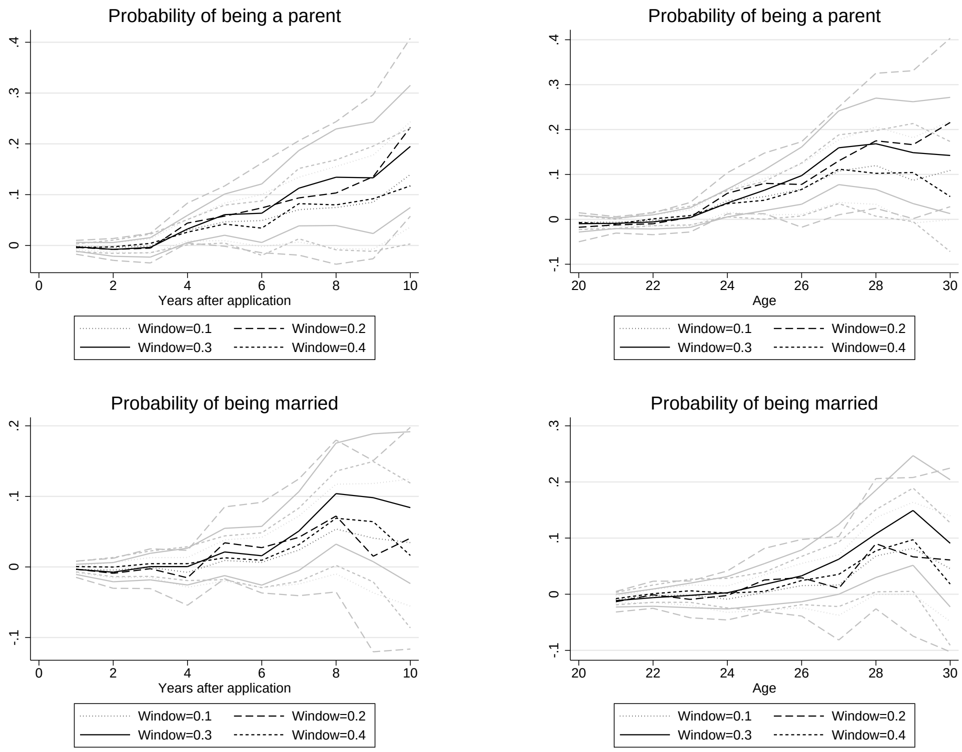

Figure 1.10: IV estimates of the effect of enrollment on main outcomes and 95 percent confidence bands for varying window widths. Includes all covariates. Standard errors are heteroscedasticity robust.

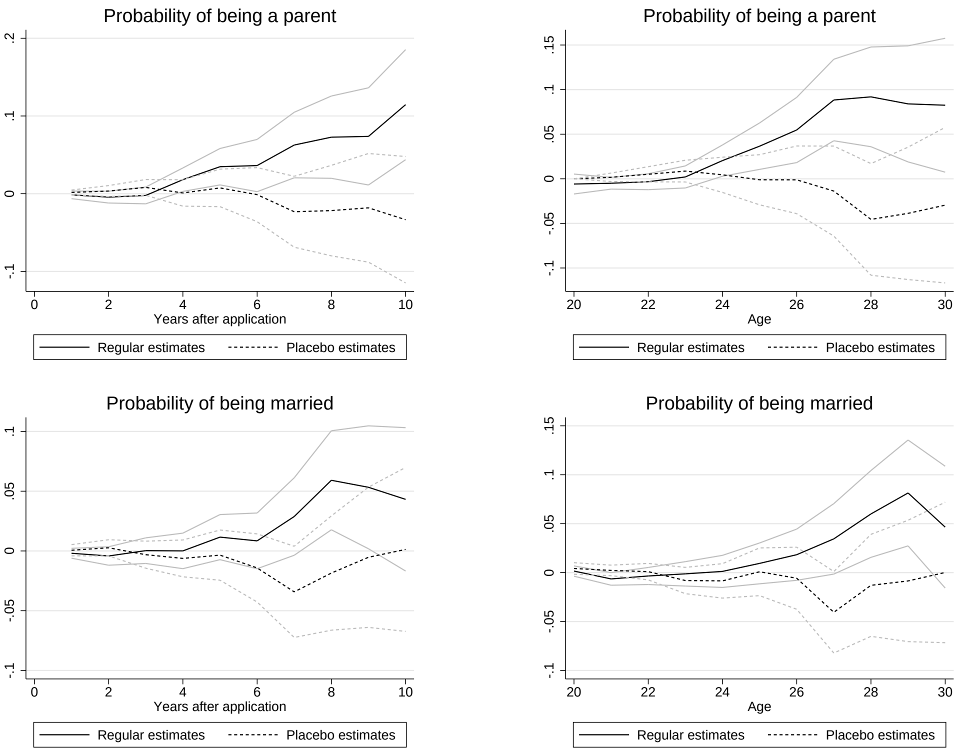

Figure 1.11: Placebo IV estimates of the effect of being above the admission requirement on main outcomes and 95 percent confidence bands. Placebo distance is defined as distance from the admission requirement minus 0.6. Includes all covariates.

1.B Tables

Table 1.1: Sample Selection

Description	Number of observations	Percentage of total
Applications to college educations 1996-2006	621,695	100.0
Applications to college (long)	288,531	46.4
First application	195,576	31.5
The preferred education has a grade requirement	133,735	21.5
The preferred education has a simple enrollment rule for those who exactly meet the grade requirement	66,871	10.8
Non-missing GPA	44,945	7.2
Young applicants	19,327	3.1
Window=0.3	6,313	1.0

Table 1.2: Descriptive Statistics

	Below admission requirement		Above admission requirement		Equality of means	
					Window=0.3	Window=0.1
	Mean	Std.dev	Mean	Std.dev.	Difference	Difference
Female	0.673		0.657		-0.015	0.008
Age in YOA	19.643	0.538	19.651	0.544	0.008	0.014
High school GPA	8.945	0.554	9.180	0.545	0.236***	0.071***
Preferred field						
Humanities	0.246		0.283		0.037***	0.055***
Social science	0.404		0.391		-0.013	-0.027
Natural science	0.076		0.081		0.005	0.000
Health science	0.274		0.245		-0.029***	-0.029
Mother's age at birth						
Less than 25	0.161		0.167		0.007	0.004
25 to 34	0.718		0.697		-0.021*	-0.028
More than 34	0.109		0.124		0.015*	0.018
Missing	0.013		0.012		0.000	0.006
Father's age at birth						
Less than 25	0.065		0.060		-0.005	0.003
25 to 34	0.659		0.659		0.000	0.000
More than 34	0.247		0.243		-0.004	-0.013
Missing	0.029		0.038		0.009*	0.010
Mother's education						
Basic	0.178		0.185		0.007	0.002
Vocational	0.228		0.220		-0.008	0.000
College	0.547		0.555		0.008	0.004
Missing	0.047		0.040		-0.008	-0.006
Father's education						
Basic	0.156		0.165		0.009	0.010
Vocational	0.227		0.225		-0.001	0.014
College	0.531		0.518		-0.013	-0.034
Missing	0.086		0.092		0.005	0.010
Mother missing	0.013		0.012		0.000	0.006
Father missing	0.029		0.038		0.009*	0.010
Mother's log earnings at age 18	10.670	4.276	10.733	4.193	0.063	-0.097
Mother's earnings missing	0.031		0.027		-0.005	-0.006
Father's log earnings at age 18	12.643	1.021	12.681	0.937	0.038	0.104**
Father's earnings missing	0.213		0.225		0.012	0.021
Being a parent in YOA	0.000		0.001		0.001	0.000
Cohabiting in YOA	0.025		0.025		-0.001	-0.004
- Missing	0.026		0.024		-0.002	-0.015**
Prior enrollments	0.008		0.010		0.001	0.002
Selected outcomes						
Being a parent 5 years after YOA	0.047		0.053		0.007	0.024**
Married 5 years after YOA	0.037		0.034		-0.002	-0.003
Being a parent at age 26	0.102		0.110		0.008	0.038**
Married at age 26	0.064		0.061		-0.004	0.008
Enrollment						
College, YOA	0.420		0.861		0.441***	0.489***
College, YOA or later	0.987		0.993		0.006**	-0.001
Preferred college program, YOA	0.165		0.711		0.545***	0.511***
Preferred college program, YOA or later	0.528		0.776		0.248***	0.124***

Notes:
a) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
b) In addition to the variables listed in this table, the following variables are also included in the analyses in section 1.5: indicators for age in YOA instead of continuous variable and indicators for YOA and parents' age at birth.
c) The numbers of observations for the selected outcomes generally differ from the size of the estimation sample.

Table 1.3: Variation in Grade Requirements

Absolute change in grade requirement	Mean	Std.dev.
Unweighted	0.327	0.492
Weighted	0.167	0.269

Table 1.4: IV Results

	Being a parent 5 years after YOA	Married 5 years after YOA	Being a parent at age 26	Married at age 26
	Coef./Std.err.	Coef./Std.err.	Coef./Std.err.	Coef./Std.err.
First-stage				
Above grade requirement	0.580***	0.563***	0.580***	0.570***
	(0.026)	(0.025)	(0.028)	(0.025)
t-statistic of instrument	22.555	22.808	20.802	22.390
R-squared	0.241	0.240	0.240	0.240
Second-stage				
Enrollment	0.060***	0.021	0.094***	0.032
	(0.021)	(0.017)	(0.032)	(0.023)
Distance to grade req.	-0.088**	-0.070**	-0.113*	-0.105**
	(0.042)	(0.035)	(0.065)	(0.048)
(Above grade requirement)Xdistance	0.060	0.117**	0.033	0.156**
	(0.070)	(0.057)	(0.106)	(0.077)
Female	0.029***	0.023***	0.056***	0.042***
	(0.006)	(0.004)	(0.009)	(0.006)
Age in YOA (ref: 20)				
19 or below	-0.009	0.007	0.024**	0.047***
	(0.006)	(0.005)	(0.010)	(0.008)
High school GPA	-0.003	-0.010*	-0.014	-0.006
	(0.006)	(0.005)	(0.010)	(0.007)
Parental education (ref: basic)				
Mother - vocational	0.004	0.000	-0.007	-0.017
	(0.010)	(0.008)	(0.015)	(0.011)
Mother - college	-0.005	-0.006	-0.002	-0.020*
	(0.009)	(0.007)	(0.013)	(0.010)
Mother - missing	-0.024	-0.010	-0.020	-0.006
	(0.015)	(0.021)	(0.034)	(0.027)
Father - vocational	0.011	-0.003	0.030**	-0.011
	(0.010)	(0.009)	(0.015)	(0.011)
Father - college	0.008	-0.010	0.015	-0.016
	(0.008)	(0.008)	(0.013)	(0.010)
Father - missing	-0.001	0.003	-0.011	-0.017
	(0.014)	(0.014)	(0.021)	(0.017)
Father's log earnings	0.000	-0.001*	-0.003**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Mother's log earnings	-0.003	-0.006*	-0.006	-0.005
	(0.003)	(0.003)	(0.005)	(0.004)
Father - missing earnings	0.030	-0.012	-0.001	-0.024
	(0.027)	(0.027)	(0.045)	(0.033)
Mother - missing earnings	-0.037	-0.076*	-0.077	-0.065
	(0.041)	(0.042)	(0.067)	(0.052)
Mother's age at birth (ref: 25-34)				
Below 25	0.017*	0.019**	0.035**	0.011
	(0.010)	(0.009)	(0.015)	(0.011)
Above 34	-0.001	-0.005	-0.015	-0.015
	(0.009)	(0.007)	(0.014)	(0.010)
Father's age at birth (ref: 25-34)				
Below 25	0.030*	-0.009	0.035	0.005
	(0.017)	(0.012)	(0.023)	(0.017)

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Table 1.4 – continued from previous page.

	Being a parent 5 years after YOA	Married 5 years after YOA	Being a parent at age 26	Married at age 26
	Coef./Std.err.	Coef./Std.err.	Coef./Std.err.	Coef./Std.err.
Above 34	-0.007	-0.007	-0.018	-0.004
	(0.007)	(0.006)	(0.011)	(0.009)
Mother missing	-0.011	0.021	-0.141***	-0.078***
	(0.039)	(0.038)	(0.043)	(0.028)
Father missing	0.003	0.004	-0.025	-0.009
	(0.022)	(0.023)	(0.033)	(0.027)
Preferred field (ref: Social Science)				
Humanities	0.004	0.006	-0.001	0.000
	(0.007)	(0.007)	(0.011)	(0.008)
Natural Science	-0.008	-0.025***	-0.009	-0.031***
	(0.011)	(0.007)	(0.017)	(0.010)
Health Science	-0.001	-0.004	0.017	-0.001
	(0.007)	(0.006)	(0.012)	(0.009)
Prior enrollment	0.015	0.002	0.021	-0.013
	(0.033)	(0.026)	(0.043)	(0.026)
Being a parent in YOA	0.934***	-0.060***	0.897***	-0.082***
	(0.022)	(0.023)	(0.033)	(0.028)
Cohabiting in YOA	0.061**	0.074***	0.053	0.074**
	(0.029)	(0.026)	(0.036)	(0.029)
Cohabiting in YOA (missing)	-0.025	-0.025**	-0.062***	-0.044***
	(0.015)	(0.010)	(0.019)	(0.011)
Constant	0.041	0.185***	0.210*	0.155*
	(0.074)	(0.068)	(0.114)	(0.085)
R-squared	0.023	0.020	0.024	0.027
Number of observations	5,682	6,108	4,858	5,789

Notes:
a) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors in parentheses. Standard errors are heteroscedasticity robust.
b) Estimations also include indicators for year of application.

Table 1.5: IV Estimates of the Effect of Enrollment on Selected Family Formation Outcomes

	(1) Coef./Std.Err.	(2) Coef./Std.Err.	(3) Coef./Std.Err.	(4) Coef./Std.Err.	(5) Coef./Std.Err.	(6) Coef./Std.Err.	(7) Coef./Std.Err.
Being a parent	0.061***	0.060***	0.059***	0.059***	0.060***	0.060***	0.060***
5 years after YOA	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Married	0.024	0.024	0.021	0.020	0.020	0.020	0.021
5 years after YOA	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Being a parent	0.100***	0.097***	0.095***	0.095***	0.095***	0.095***	0.094***
at age 26	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.032)
Married	0.031	0.033	0.032	0.031	0.032	0.032	0.032
at age 26	(0.024)	(0.023)	(0.024)	(0.024)	(0.023)	(0.023)	(0.023)
Basic controls		+	+	+	+	+	+
Parental background			+	+	+	+	+
Preferred field				+	+	+	+
Year of application (YOA)					+	+	+
Prior enrollments						+	+
Family formation in YOA							+

Notes:
a) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively.
Standard errors in parentheses. Standard errors are heteroscedasticity robust.
b) Basic controls: gender, age group, and high school GPA. Parental background: level of education, earnings, age, missing.
Indicators for preferred field, year of application, and prior enrollments. Family formation in YOA: cohabition status, missing, marital status.

Table 1.6: Robustness of IV Estimates to Inclusion of Actual Enrollment and Earnings

	Coef./Std.Err.	Coef./Std.Err.
Being a parent	0.062***	0.060***
5 years after YOA	(0.020)	(0.021)
Married	0.024	0.020
5 years after YOA	(0.017)	(0.017)
Being a parent	0.095***	0.094***
at age 26	(0.032)	(0.032)
Married	0.034	0.031
at age 26	(0.023)	(0.023)
Actual college program (detailed)	+	
Earnings 5 and 10 years after YOA		+

Notes:
a) '***', '**', and '*' indicate statistical significance at the 1, 5, and 10 percent levels, respectively. Standard errors in parentheses. Standard errors are heteroscedasticity robust.
b) All regressions include the controls listed in Table 1.4.

Chapter 2

Gender Differences in the Effects of Behavioral Problems on School Outcomes

Gender Differences in the Effects of Behavioral Problems on School Outcomes*

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Abstract

Behavioral problems are important determinants of school outcomes and later success in the labor market. We analyze whether behavioral problems affect girls and boys differently with respect to school outcomes. The study is based on teacher and parent Strengths and Difficulties Questionnaire (SDQ)-evaluations of about 6,000 children born in 1990-92 in a large region in Denmark. The sample is merged with register information on parents and students observed until the age of 19. We find significant and large negative coefficients of the externalizing behavioral indicators. The effects tend to be larger when based on parents' SDQ scores compared to teachers' SDQ scores. According to our estimations, the school outcomes for girls with abnormal externalizing behavior are not significantly different from those of boys with the same behavioral problems. A decomposition of the estimates indicates that most of the gender differences in Reading and Math cannot be related to gender differences in behavioral problems. The large overall gender gap in Reading seems mainly to be the result of gender differences between children without behavioral problems living in 'normal families', i.e., families which are not categorized as low-resource families.

JEL Classification: J16, I29, I19

Keywords: Gender differences, education, behavior

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2.1 Introduction

During the latest decades there has been a growing focus on why girls seem to perform better in primary schools than boys in most dimensions, only with the exception of math grades where boys are as good as or even outperform girls. Since student achievement in primary school is an important determinant of enrollment into high school and a determinant of success in the educational system, this may be one of the explanations of the emerging gender gap in the educational level which is found in many countries, see for instance Goldin, Katz, and Kuziemko (2006) and Fortin, Oreopoulos, and Phipps (2012). This is also the case for Denmark where girls are much more successful in the educational system with respect to completing high school and obtaining a qualifying education, i.e., a degree from college or university or a vocational education.

The number of Danish children placed in special needs classes is steeply increasing. A significant proportion of the children in special needs classes have behavioral problems and the majority of them are boys, see Mehlbye (2008). From an economic policy perspective, this development is important and costly. The school outcomes and long term perspectives for employment of children in special needs classes are less positive than for other children, and 20-33 percent of all expenditures allocated to compulsory schools in Denmark are now spent on students with special needs, see Ministry of Finance (2010).

According to PISA 2009, see e.g. OECD (2010), Danish students have a problematically high score regarding some aspects of behavioral problems. For instance a relatively large proportion of the teachers in the Danish PISA 2009 study claim that in most lessons 'there is noise and disorder' in the classroom.[1] These classroom problems may clearly have negative effects on the learning outcomes of students. The goal of this paper is to investigate the gender differences in the relation between behavioral problems and school outcomes, and how these effects depend on the family background.

In this study, we analyze gender differences in the relation between behavioral problems and student outcomes based on a large sample of Danish children born in 1990-1992. Student outcomes are measured as whether or not they took the 9th grade exit exam (i.e., took at least one of the course exams) and as the performance in terms of grades in Math and Reading, and enrollment into high school (including vocational programs). We focus on whether the effects from behavioral skills follow the same pattern for girls and boys, and which factors such as family background influence this pattern. Our general hypothesis is that the relation between school outcome and behavioral skills is more sensitive to family and school environment for boys than for girls. For instance we want to test whether boys with disruptive behavior are more negatively affected than girls coming from, what we define as, a low-resource family. Further, we analyze whether teachers' and parents' views on behavioral problems differ systematically

[1] See PISA 2009 Database (ST36Q02).

between boys and girls and whether this potential difference in perception matters for the gender-specific link between behavior and school outcome.

Our results show that about 11 percent of the boys and 6 percent of the girls in our sample have abnormal or borderline externalizing behavioral problems. For internalizing problems, girls and boys have about the same average scores, and 14 percent of the children are categorized as having abnormal or borderline internalizing problems. These measures are based on Strengths and Difficulties Questionnaire (SDQ) scores assessed by the parents. Teachers tend to categorize more children as having abnormal problems.

We find significant and large negative coefficients of the externalizing behavioral indicators. The effects tend to be larger when based on parents' SDQ scores compared to teachers' SDQ scores. According to our estimations, the school outcomes for girls with abnormal externalizing behavior are not significantly different from those of boys with the same behavioral problems. For borderline externalizing problems and for internalizing problems, the estimated main coefficients are numerically smaller but still significantly negative in most cases, and for these behavioral categories, girls tend to have less negative coefficients.

Children from low-resource families have lower school outcomes. The gender gap in Reading is observed to be largest in the sub-group of 'normal families', i.e., families which are not categorized as low-resource families and smallest in the sub-group of families with a young mother. A decomposition of the estimations indicates that most of the gender differences in Reading and Math cannot be related to gender differences in behavioral problems, neither differences in endowments nor gender differences in estimated coefficients. The overall gender gap in Reading seems mainly to be the result of gender differences between children without behavioral problems living in 'normal families', i.e., families which are not categorized as low-resource families. For Math, the higher overall grades for boys compared to girls seem mainly to be the result of very low Math grades for girls without behavioral problems from low-income families.

2.2 Background

An increasing number of studies have focussed on the importance of behavioral problems or behavioral skills of school children. Even after controlling for school grades, behavioral problems are found to matter for educational outcomes in a number of studies, see for instance Heckman and Rubinstein (2001), Jacob (2002), Heckman, Stixrud, and Urzua (2006), Bertrand and Pan (2011), and the survey in Heckman (2008). Behavioral skills may be measured in a number of dimensions, such as externalizing behavior, hyper activity, self-control, approaches to learning, interpersonal skills and internalizing problems such as loneliness and low self-esteem. Behavioral problems may stem from both nature and nurture but the most important determinant of behavioral skills seems to be gender: Girls tend to have much less behavioral problems at school age than their male peers, see Bertrand and Pan (2011). A large strand of research stresses the biological ('nature') reasons for behavioral problems, i.e., the development of female brains is different from male brains and this may have consequences at early ages for the observed gender gap in behavior. But 'nurture' also seems to matter. Gender differences in for instance child rearing inputs may affect the way behavioral skills are produced, see the survey in Heckman (2008). Since the focus here is on the latter aspect, we restrict our discussion to 'nurture' explanations of the gender gap, recognizing that biological factors may also play an important role.

The study by Bertrand and Pan (2011) is based on the Early Childhood Longitudinal Study (ECLS) which is a US sample of around 20,000 children who were followed from kindergarten until 8th grade. The gender gap in non-cognitive skills exists already in kindergarten but the gender gap evolves and increases steadily during childhood up until 5th grade. However, when looking at separate groups, the growing gender gap shows to be the result of boys living in single mother families, boys from the lowest social economic status families, and boys born by mothers who had their first child before the age of 24. For other boys and for girls from all social groups the disruptive behavior is stable during childhood ages. The same pattern is found in Carneiro and Heckman (2003). Their study strongly stresses the importance of the family resources which are devoted to boys. Single mothers tend to devote less time resources to their sons compared to their daughters, and boys living in single-parent families (or teenage-mother families or the lowest social economic status families) tend to receive less parental resources compared to other boys and girls. The results in Bertrand and Pan indicate that teacher effects, peer effects and school environment are less important: 'Overall, our findings strongly suggest that boys' deficit in non-cognitive skills is not purely biological but instead subject to very strong environmental influences, particularly from the home' (Bertrand and Pan (2011, p. 7)).

In a study on UK children, Ermisch (2008) also finds that the gender gap in non-cognitive skills exists early in life, already at the age of 3. His study is based on the Millenium Cohort Study and includes data from the 'Strengths and Difficulties Questionnaire' (SDQ) which is also applied in our study on Danish students but for older children. Ermisch (2008) finds large effects of parental background and effects of parents' activities with their children (e.g. reading to the child on a regular basis). When controlling for these variables, Ermisch (2008) finds that an indicator for 'girl' turns insignificant in his estimation of determinants of behavioral problems.

Jacob (2002) poses an empirical link between non-cognitive skills and educational attainment in a US setting. Based on data from National Education Longitudinal Study (NELS), a representative sample of 8th graders who were followed from 1988 to 1994, he finds an overall gender gap in college enrollment of 5 percentage points (7 points if the sample is restricted to bottom 3 quartiles of socioeconomic groups). About 40 percent of this observed gender gap in college attendance is related to differences in non-cognitive skills between girls and boys. The non-cognitive skills considered in Jacob (2002) are middle-school grades and the number of hours spent on homework per week in 8th grade. In a later study by Heckman, Malofeeva, Pinto, Savelyev, and Yavitz (2008), it is found that early treatment of children in the Perry program in Michigan in the 1960s only had temporary effects on the IQ of the treated children but still there was a long-run effect on labor market outcomes of the treatment which was mainly due to non-cognitive effects of the program treatment. Thus, behavioral skills seem to be extremely important for later outcomes in life, and especially for educational outcomes.

2.3 Data: Selection, SDQ scores, and School Outcomes

The study is based on a panel survey of 10,907 children born at Aarhus University Hospital, Denmark, between January 1990 and March 1992, denoted the Aarhus Birth Cohort (ABC) data. Aarhus University Hospital is one of the largest maternity wards in Denmark, and it covers the entire region. This implies that the children born here are also likely to enter the same school classes as long as their families also live in Aarhus County. The ABC data are survey data and contain extensive information on child health, behavioral variables (SDQ variables) given by parents as well as school teachers. For a sub-sample of children we have information on the gender of the teacher as well. The ABC data are merged to administrative data hosted by Statistics Denmark and they contain information on parents' socioeconomic status, income, education and marital status during the period. Since the children were born in the early 1990s, we are able to observe their school outcome up until the age of 15-18. School outcomes are also based on administrative data from Statistics Denmark who collects grades in Math and Reading for all Danish pupils exiting from 9th grade in public and private schools in Denmark. Further, the administrative registers contain information on enrollment into vocational programs and high school after compulsory school.

2.3.1 Description of the Sample

In the late prenatal stages, the mothers who were expected to give birth at Aarhus University Hospital during the period January 1990 – March 1992 were asked to participate in a survey concerning the health of their expected child after child birth. Among these soon-to-be mothers, 98 percent accepted to participate. An illustration of the timing of the set-up can be seen in Figure 2.1. The first survey was collected when the child was 3.5 years old. This survey included a number of questions relating to health of the child and parents' time allocation.[2] In

[2]Data from the first survey are only available for the younger cohort because the idea of giving a survey when the child was around 3.5 years old came halfway through the period, implying that half of the children

2001, a second survey was collected among the parents. The children were in 2001 in the range of 9 to 12 years old. The 2001 survey covers some health measures for the child along with a range of parental and family measures and early childhood measures. The following year, 2002, all parents, regardless of earlier participation, received a follow-up survey, in which they answered the Strengths and Difficulties Questionnaire (SDQ). This survey was relatively short and easy to answer, giving a response rate of 61.7%. At the same time, the children's teachers also received the SDQ survey, and for 46% of the sample we have SDQ data reported by both parents and teachers. Thus, each child's behavior is evaluated by both the parents (in 2001 and 2002) and subsequently by the teacher (in 2002).

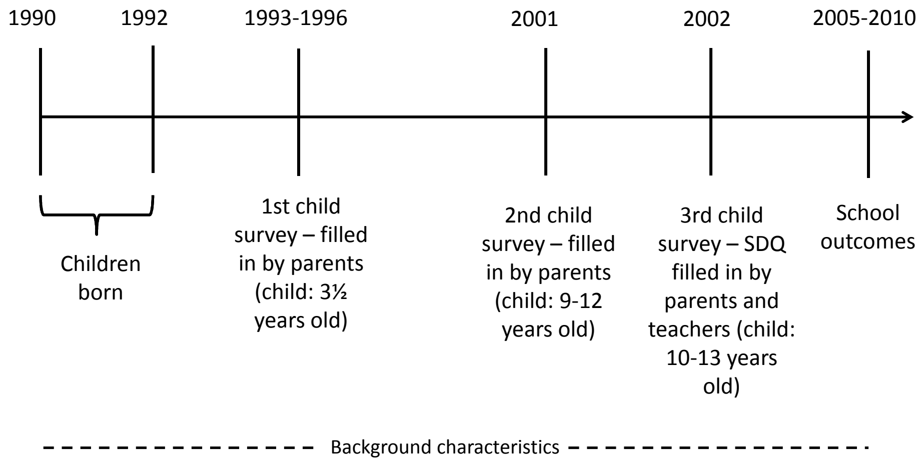

Figure 2.1: Timing of the set-up

Table 2.1 shows the sample selection. The sample is restricted to observations where parental evaluation of the child is non-missing. Thus, there may be a problem of systematic sample attrition since low-resource parents may have lower response rates.

Table 2.1: Sample selection

	No. obs.	**%**
Full sample	10,907	100.00
Parental SDQ	6,729	61.69
Teacher SDQ	5,053	46.33
Gender of the teacher	1,662	15.24

were already older.

2.3.2 The SDQ Scores

The Strengths and Difficulties Questionnaire (SDQ) was first developed by Goodman (1997), and it includes 25 questions,[3] which are answered by both parents and teachers concerning the child's mental health. Following Goodman et al. (2010), we use 20 of the 25 questions to construct two groups of behavioral problems; externalizing problems (SDQ1) and internalizing problems (SDQ2), see Table 2.14 in Appendix 2.A for a detailed list of the questions. According to Goodman et al. (2010), this categorization is appropriate for an analysis of a low risk sample, i.e., ordinary school classes and not special needs classes. What externalizing and internalizing behavior captures, can be seen in Table 2.14 in Appendix 2.A. Externalizing behavior captures 'acting out' behavior. For example, one of the questions in the questionnaire concerns whether the child often has temper tantrums or hot tempers. Internalizing behavior captures internal problems. For example, one of the questions concerns whether the child would rather be solitary and whether the child tends to play alone. So these two behavioral measures capture different aspects of being behaviorally challenged.

Each of the questions can be marked 'not true', 'somewhat true' or 'certainly true', and they are given scores from 0 to 2.

The total score of the 10 (10) measures of SDQ1 (SDQ2) is then found by summing the questions in each category, giving a score ranging from 0 to 20. The scores are given such that the higher the score, the more problematic behavior.

In our main analysis, we use the parental assessment of the child's behavior in order to analyze to which extent there are gender differences in the relation between behavior and school outcomes. We do this as we believe that the parents are more informed on any problems with the child's behavior in different environments than what we would expect from the teachers. Even though the teachers are aware of what happens in school, they might not be aware of what happens in the child's home or with friends, etc. Thus, they may have less focus on internalizing problems than the parents. Teachers may also be more likely than parents to (unintentionally) let current academic performance in the class affect the student's SDQ assessment. However, parents' assessments may suffer from a problem of a 'common standard' for the students since they typically only observe their own child. A recent study by Datta Gupta, Lausten, and Pozzoli (2012) shows that the SDQ assessment is sensitive to whether it is the father or the mother who answers the survey questions. In order to test the robustness of our results and also to test if parents and teachers evaluate the children differently, we make the same analysis and estimations using teacher assessments in Section 2.5, where this pattern is analyzed in more detail.

The distribution of SDQ1 and SDQ2 measures based on parents' evaluation are shown in

[3] SDQ is a well-documented questionnaire investigating children's behavioral skills, see Goodman (1997), Goodman, Lamping, and Ploubidis (2010) and `http://www.sdqinfo.org/a0.html`

Figure 2.2.

As found in other studies, Figure 2.2 shows that boys tend to score higher values on the externalizing problems (SDQ1) compared to girls, while the distributions are much more alike for internalizing problems (SDQ2).

When estimating the effects of behavioral skills, we focus on problematic behavior. Therefore, the SDQ1 and SDQ2 variables are split into three categories according to Goodman et al. (2010): 'Normal behavior', 'borderline behavior', and 'abnormal behavior'. As shown in Figure 2.2, externalizing behavior is categorized as normal if the SDQ score is below 8, borderline behavior is between 8 and 9, and abnormal externalizing behavior is a SDQ score above 9. Internalizing behavior is likewise categorized as normal, when the SDQ score is below 6, borderline behavior when the score is between 6 and 7, and abnormal behavior when the SDQ score is above 7. In our empirical analysis, normal behavior is the reference category.

In the psychology literature, it is argued that the thresholds should vary between boys and girls, such that the limits are lowered for girls. However, to our knowledge, no previous study neither use different thresholds nor provide evidence on teachers and parents having different standards in evaluating behaviors of boys and girls. We use the same threshold values for boys and girls, recognizing that this might be a conservative estimate for girls. Since we include a girl-indicator in the estimated regression models, we expect this variable to capture a potential difference between boys and girls with respect to the thresholds.

The distributions of the categorizations of behavioral problems are shown in Table 2.2 for boys and girls. About 11% of the boys have abnormal or borderline externalizing behavioral problems while the figure is 6% for girls. For internalizing behavioral problems the figure is 14% for boys and slightly higher for girls (15%).

Table 2.2: Frequency of SDQ categories by gender

	Boys		Girls	
Externalizing (SDQ1)	N	%	N	%
Normal	3032	88.8	3113	94.0
Borderline	211	6.2	118	3.6
Abnormal	173	5.1	82	2.5
Total	3416	100	3313	100
Internalizing (SDQ2)	N	%	N	%
Normal	2931	86.0	2818	85.1
Borderline	205	6.0	249	7.5
Abnormal	274	8.0	244	7.4
Total	3410	100	3311	100

2.3.3 School Outcome Variables

Compulsory schooling in Denmark typically starts at the age of seven,[4] and ends with an exit exam after 9th grade. In 9th grade all students receive annual marks in Reading and Math. At

[4]In 2009 this was changed to a school starting age of six.

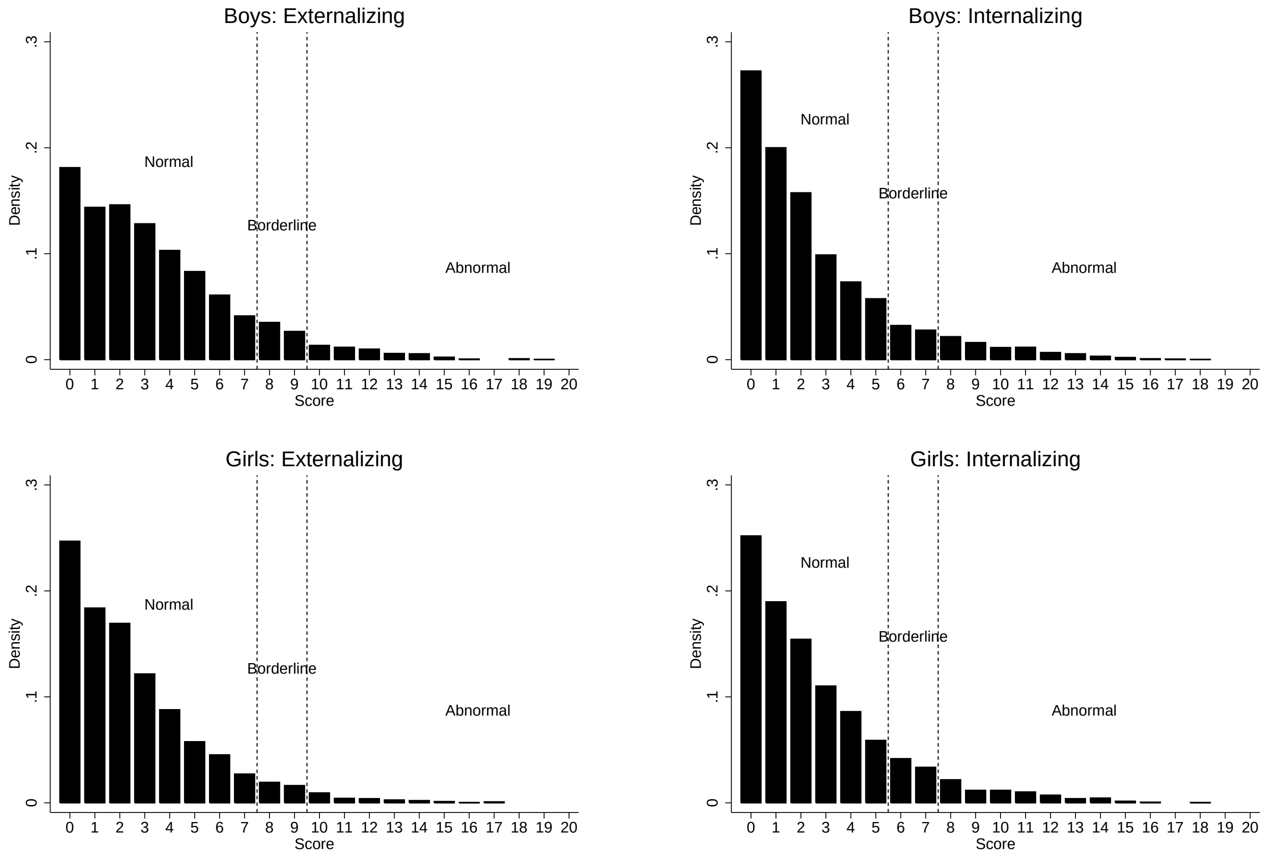

Figure 2.2: SDQ scores for boys and girls

the end of the school year the students take the exit exam, where they receive marks in Reading and Math. After compulsory schooling individuals can choose to either leave the educational system, enroll in high school, or enroll in vocational training programs.

The outcome variables stem from administrative registers on school grades and enrollment into high school (including vocational education). We use four outcome variables: Whether the student took the 9th grade exit exam, the 9th grade exit exam results in Math and Reading, and enrollment in high school or vocational training before the age of 19. This means that we observe the outcome variables several years (5-8 years) after the SDQ questionnaire was filled by the parents. 9th grade is typically passed at the age of 15 (depending on age at school start and class repetition).

In 2005-2006, when the exit exam results are observed for the students in our sample, the Danish grading scale had 10 values, ranging from 0 to 13.[5] As part of the grading, teachers in compulsory school also give term marks. The term marks are expected to show the outcomes of the students during the semester contrary to the exam which is based on a 'point estimate'.

[5]The grading scale was: 00, 03, 5, 6, 7, 8, 9, 10, 11 and 13. The lowest passing grade was 6, and the average grade was 8.

However, the exam result is based on the teacher's as well as an external reviewer's evaluation. We expect the exam results to be more 'neutral' than the term marks with respect to behavioral problems which might bias the term marks. Therefore, we prefer to use the exam results.[6] We standardize the scale to an outcome measure with zero mean and a standard deviation of 1 (for the two grading outcomes). The standardization is based on the full sample of students for whom we have register information.

Table 2.3: School outcomes by gender

	Girls			Boys			
Outcomes	N	Mean	Std.	N	Mean	Std	t-value
9th grade exit exam	3185	0.9758	0.1536	3220	0.9776	0.1479	-0.48
Reading (exit exam)	3142	0.4378	0.9235	3167	0.0892	0.9577	14.72
Math (exit exam)	3125	0.2572	0.8949	3183	0.3833	0.8752	-5.66
High school enrollment	3313	0.9185	0.2736	3416	0.9013	0.2982	2.46

Table 2.3 shows the standardized average outcome variables for girls and boys in the sample.[7,8] As found in other studies, girls get higher grades in Reading and boys get higher grades in Math. The gender differences are less significant with respect to taking the 9th grade exit exam. However, girls who are categorized as having a borderline externalizing behavior have a very high proportion (30 percent) who do not take the 9th grade exit exam (significantly higher than boys). In all sub-samples, girls tend to enroll in high school at a marginally (though significantly) higher rate than boys. Since the mean grades are (significantly) positive, this indicates that the sample of students in Table 2.3 for which we have SDQ information is positively selected in the grade distribution from the full sample of students.

Table 2.4 shows the average values of the four outcome variables by gender and by category of externalizing and internalizing behavioral problems. The last column gives the t-test for differences between girls and boys. The overall picture is that the differences between boys and girls with respect to Reading where girls perform better than boys and Math where boys perform better than girls are mainly due to differences between children in the 'normal' category, i.e., children who are not evaluated as having behavioral problems. For the 'abnormal' and borderline' groups the t-statistics is numerically much smaller and most often insignificant. (Part of the insignificance is of course due to fewer observations, but also the absolute size of

[6]Boys get higher grades at exam compared to the term mark, and opposite for girls, while the enrollment rate at high school is significantly higher for girls than for boys. The difference between exit exam grades and term marks may indicate that girls and boys perform differently during their daily school activities and when they are tested at exam. See e.g. Niederle and Vesterlund (2010), who argue that the gender gap reflects that boys are more competitive and perform better in competitive environments than girls.

[7]The Danish compulsory school conducts exams in oral Danish, written Danish and written Math. The students must write an essay at the exam in written Danish and this is combined with a spelling grade. In this analysis, we are interested in the students ability to read and understand, which is the content of the oral exam. For this reason, we focus on the oral exam in Danish and the written exam in Math.

[8]Note that children in the sample in general perform better compared to the national average. This is a result of selection in who participated in the SDQ survey, where as an example immigrant students are underrepresented.

the gender difference is numerically smaller for the abnormal and borderline groups than for the normal groups in most cases).

Table 2.4: SDQ and school outcomes by gender

	Girls			Boys			
	N	Mean	Std.	N	Mean	Std.	t-value
Externalizing - Abnormal							
9th grade exit exam	59	0.9322	0.2536	128	0.9141	0.2814	0.44
Reading	58	-0.4131	0.9484	120	-0.4617	0.9631	0.32
Math	56	-0.8022	0.9778	120	-0.3775	1.0316	-2.64
HS Enrollment	82	0.6463	0.4810	173	0.6821	0.4670	-0.56
Internalizing - Abnormal							
9th grade exit exam	204	0.9559	0.2059	211	0.9194	0.2728	1.54
Reading	202	0.0129	0.9879	199	-0.1633	1.0448	1.74
Math	195	-0.0761	0.9487	205	0.0145	0.9573	-0.95
HS Enrollment	244	0.8074	0.3952	274	0.7336	0.4429	2.00
Externalizing - Borderline							
9th grade exit exam	102	0.8824	0.3238	165	0.9515	0.2154	-1.91
Reading	96	0.0459	0.8028	160	-0.3327	0.9303	3.44
Math	92	-0.1960	1.0001	161	-0.1929	0.8792	-0.02
HS Enrollment	118	0.7034	0.4587	211	0.7393	0.4400	-0.69
Internalizing - Borderline							
9th grade exit exam	230	0.9739	0.1597	173	0.9422	0.2340	1.53
Reading	224	0.2619	0.9089	165	-0.1605	0.9257	4.48
Math	227	0.0360	0.9880	163	0.1564	0.8692	-1.27
HS Enrollment	250	0.8560	0.3518	205	0.7756	0.4182	2.19
Externalizing - Normal							
9th grade exit exam	3024	0.9798	0.1406	2927	0.9819	0.1334	-0.58
Reading	2988	0.4670	0.9161	2887	0.1354	0.9464	13.64
Math	2977	0.2911	0.8739	2902	0.4467	0.8411	-6.96
HS Enrollment	3113	0.9338	0.2486	3032	0.9251	0.2632	1.33
Internalizing - Normal							
9th grade exit exam	2752	0.9771	0.1496	2834	0.9841	0.1250	-1.90
Reading	2716	0.4844	0.9103	2801	0.1210	0.9486	14.52
Math	2703	0.3007	0.8747	2813	0.4228	0.8617	-5.22
HS Enrollment	2820	0.9333	0.2495	2935	0.9257	0.2623	1.13

2.3.4 Covariates

We use an extensive list of covariates relating to family composition, health, and school information in our conditioning set in order to control for as much unobserved variation in relevant background factors as possible. A complete variable description with a list of included covariates and outcomes can be found in Table 2.12 in Appendix 2.A. All covariates included are measured before the school start (when the child is 0-6 years old) except for the behavioral measures (SDQ) which are measured at the age of 10-12, i.e., before school outcomes are measured at the age of 15-19. This is, of course important, as we want to make sure that the effect does not work the other way around.

2.3.4.1 Basic characteristics

First of all, we include indicators for the gender of the child, and whether the child is a native Dane, whether the child has any younger and older siblings, and how many younger and older siblings. The measure of siblings is determined when the child is six years old, which makes sure that no effects can go through having more siblings after starting in school.

Since day care is widely used in Denmark, it is natural to include variables for the type of day care the child is in. We determine the type of day care when the child is four years old. Parents with problematic children may move between municipalities more often than other parents. For that reason, we include an indicator for whether or not the family has moved between municipalities, and a variable for how many times the family has moved from one municipality to another. We measure this just before the child is 7 years old.

We also control for age at completing compulsory schooling. We do this for several reasons. First of all, the children in this sample are born in three consecutive years, which might influence the given grades. Also, as girls tend to start earlier in school than boys, it is important to take the year effects into account.

Further, we include health information from the mother's pregnancy and the birth of the child, which include the length of the pregnancy, whether the child was born prematurely, extremely prematurely, whether there was any complications at birth, and the birth weight. We also include a dummy for the birth year.

Mental health and medicine

The register data allow us to control for whether the child has been diagnosed with a mental or behavioral disorder. We construct the variables such that a diagnosis should occur before the child is 7 years old. One could argue that examining behavioral problems would only capture the effect for those children who are diagnosed with a behavioral disorder, but this is not the case in this study, as we are able to control for that as well.

A child with depression or cardiovascular problems might have different behavior and school outcomes than other children. Therefore, we also include information on whether the child receives cardiovascular and antidepressant medicine before school age.

2.3.4.2 Parental characteristics

Not only a child's own characteristics may be important for behavioral problems and school outcomes. Therefore, we include a range of parental characteristics as well. Parental employment might be an important factor, and we include employment along different dimensions to capture different aspects of the influence on the child. We include the degree of employment when the child is 4, 5 and 6 years old. This captures to what extent the parents have been employed during the year in the years up until the child enters school. The sector of employment is also included, i.e., private versus public, and whether the parents are full-time or part-time employed. Furthermore, the occupation is included, e.g. a manager versus an unskilled worker.

Besides employment measures, the highest obtained education for the parents might matter in the formation of skills for the child, which is why this is also included in our conditioning set.

As for each child, we include three specific health measures regarding the mental health. That is, we include an indicator for whether the parents have been diagnosed with a mental or behavioral disorder, an indicator for being prescribed antidepressant medicine, and an indicator for cardiovascular medicine. These health measures for the parents are determined before the child is 6 years old. In addition, we have information from the crime registers, enabling us to identify parents with criminal behavior before the child turns 7 years old. All of the parental characteristics are included for both the mother and the father.

Family background

Since family background characteristics may be important for the child's development, we include a number of family characteristics. First, the mother's age at birth might matter for the ability and skill formation of the child, which might suggest a quantity-quality trade-off, see e.g. Miller (2009). We further include the age difference between the father and the mother. We include three variables which may proxy family resources: (1) Having a young mother, defined by an indicator being 1 if the mother was 23 years old or younger at time of birth, 0 else. (2) An indicator for having a non-intact family defined by an indicator taking the value of 1 if the biological parents are cohabiting up until the child is six years old, 0 else. (3) An indicator for whether it is a low-income family, measured as 50% of the median income.

2.3.4.3 School fixed effects and class information

School and class data are obtained for all children who were born in the region of Aarhus, and who at one point in time entered compulsory school in that region. This means that we do not include children who were born in the region of Aarhus but never went to school in the Aarhus area. Even though the majority of our sample has information on school and class, there are still some missing data. Individuals with missing school and class data are kept in the sample and grouped in a 'missing group'. (An indicator for missing data is included).

In order to capture variation in class characteristics, we include the fraction of boys in class, the fraction of boys in class squared, and a variable telling how many times the child changed classes before 2002, i.e., before behavior is measured. Because the class environment may affect behavior and vice versa, children with problematic behavior might be more prone to enroll in another school and class.

2.3.5 Descriptive Statistics

Table 2.13 in Appendix 2.A shows the sample means for all included covariates, the behavioral measures and the outcomes. As found in other studies, see for instance Bertrand and Pan (2011), there are no significant differences between boys and girls regarding the parental background characteristics for the majority of covariates. However, for some variables differences for boys and girls are present. The birth weight of boys are larger than for girls, which is a matter of physiological differences. Also, boys are more likely to have a psychiatric diagnosis, which is also found in Gaub and Carlson (1997). Besides that, girls finish 9th grade sooner than boys, which corresponds to boys enrolling later than girls.

2.4 Estimation Results

The estimated model of school outcomes and behavior is based on the following specification with ordinary least squares

$$\begin{aligned} Outcome_{is} &= \alpha + Girl_{is}\alpha^{g} + \sum_{j=1}^{4}\left(SDQ_{j,is}\gamma_j + SDQ_{j,is}Girl_{is}\gamma_j^{g}\right) \\ &\quad + X_{is}\beta + \eta_s + \varepsilon_{is}, \end{aligned} \tag{2.1}$$

Outcome is the standardized exit exam outcome or enrollment indicator for individual i in school s, *Girl* is an indicator for being a girl which is interacted with all SDQ variables in order to allow the behavioral parameters in the model to be gender-specific. SDQ_j denotes SDQ measure j (i.e., abnormal and borderline externalizing and internalizing behavior), X is a vector of other covariates, including child, parental, family background characteristics, and class characteristics, etc., and η_s is school fixed effects. The parameters of interest are γ_j^g which indicates whether there are gender differences in the effects of behavioral skills on school outcomes.

In order to identify causal effects from behavior to school outcomes, we either have to have valid instruments of behavior, experimental data or be able to identify all relevant covariates, and all covariates related to behavior other than the variables of interest have to be predetermined, see Angrist and Pischke (2009). We do have a large battery of background characteristics and child information on the children from their birth up until the age of 18-20. Behavior is measured at the age of 10-12 years, school outcomes are measured at the age of 15-18, and social background control variables when the child is aged 0-6. But since we do not have any valid instruments at hand nor do we have experimental data, we have to rely on the conditional independence assumption which we cannot test. Thus, though we sometimes use the term 'effect' in the following sections, we do not claim to present causal effects.

2.4.1 Main Model

The results from the main model are presented in Table 2.5 for the four school outcomes. Most of the SDQ externalizing variables have significantly negative coefficients. In column 1, the results for the probability of taking the 9th grade exit exam are shown. Students with abnormal externalizing behavior are estimated to have a 3.6 percentage points lower chance of taking the 9th grade exit exam, their score in Reading is 0.36 lower, in Math 0,57 lower and their high school enrollment chance 4.1 percentage point lower than children without behavioral problems. For children with borderline externalizing behavior the estimated coefficients are of the same sign, but numerically smaller in most cases.

The coefficients of the interaction effect between the girl indicator and the indicators for externalizing behavior are surprising. The estimations indicate the girls with abnormal externalizing behavior tend to have the same school outcome as boys in this category. But girls with borderline externalizing behavior seem to have a significantly lower chance of taking the 9th grade exit exam compared to boys (8 percentage points lower), while girls with borderline externalizing behavior seem to get significantly higher grades in Math and Reading than boys with borderline externalizing behavior.

For children with a high SDQ score on internalizing behavior, the story seems to be different. The coefficients of the main effect variables for internalizing behavior in rows 3-4 are numerically small and in most cases insignificant. The girl interaction effects also tend to be insignificant, except for girls with abnormal internalizing behavior where the coefficient is significantly positive for taking the 9th grade exit exam and high school enrollment.

2.4.2 The Importance of the Conditioning Set

In order to evaluate the significance of a large conditioning set, Table 2.6 shows the estimates of behavioral problems on Math (exit exam), where additional controls are gradually added. Column (5) in Table 2.6 represents the results shown in Table 2.5.[9]

Column (1) shows the estimates of behavioral problems on Math (exit exam), where no additional controls are added. Including the basic characteristics of the child (such as day care and health measures) in column (2), does not change the estimates much. Including the parental characteristics in column (3) lowers the estimates in magnitude. It is worth noting that the inclusion of conditioning variables changes the explanatory power of the model considerably. In column (1) where no controls are added, the R-squared is 5 percent, while including family and parental characteristics increases this figure to 23 percent, including school fixed effects further increases the R-squared to 26 percent, but adding class variables does not change the R-

[9]We only present the estimates of gradually including covariates for the Math outcome. The pattern is the same for the Reading outcome, except that the level effect is of opposite sign. For the outcome of attending high school, there is no gender difference in levels either. The estimations are available upon request.

Table 2.5: Estimates of behavioral effects (Parents' SDQ) on school outcomes

	9th grade exit exam	Reading (exit exam)	Math (exit exam)	HS Enrollment
Abnormal Externalizing Behavior (SDQ1)	-0.0363*	-0.3554***	-0.5716***	-0.0406
	(0.0190)	(0.1090)	(0.0807)	(0.0263)
Borderline Externalizing Behavior (SDQ1)	-0.0180	-0.3122***	-0.4837***	-0.0574***
	(0.0156)	(0.0666)	(0.0648)	(0.0219)
Abnormal Internalizing Behavior (SDQ2)	-0.0452***	0.0053	-0.0159	-0.0265
	(0.0167)	(0.0793)	(0.0536)	(0.0204)
Borderline Internalizing Behavior (SDQ2)	-0.0297	-0.1277*	-0.0632	-0.0419**
	(0.0195)	(0.0766)	(0.0770)	(0.0201)
Girl	-0.0080***	0.3637***	-0.1275***	-0.0033
	(0.0030)	(0.0282)	(0.0209)	(0.0048)
Abnormal Externalizing Behavior*Girl (SDQ1)	0.0070	-0.0059	-0.1305	-0.0518
	(0.0267)	(0.1438)	(0.1609)	(0.0416)
Borderline Externalizing Behavior*Girl (SDQ1)	-0.0841*	0.1710*	0.2556**	-0.0526
	(0.0494)	(0.1013)	(0.1061)	(0.0428)
Abnormal Internalizing Behavior*Girl (SDQ2)	0.0430**	-0.2027	0.0251	0.0450*
	(0.0205)	(0.1225)	(0.0700)	(0.0234)
Borderline Internalizing Behavior*Girl (SDQ2)	0.0391	0.0461	0.0202	0.0344
	(0.0236)	(0.0728)	(0.0806)	(0.0278)
N	6122	6031	6031	6122
R^2	0.087	0.201	0.257	0.166

Standard errors in parentheses and clustered at the school level.
Conditioning variables: Basic, parental, School FE, and class variables.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

squared much. During this process of expanding the conditioning set of variables, the estimated coefficients of the SDQ variables are fairly stable. Thus, the estimates in column (5) might be considered conservative and fairly robust.[10]

2.5 Are Teachers' SDQ Different from Parents' SDQ?

In this section, we investigate whether parent and teacher evaluations are different in the assessment of child behavior. Parents and teachers might have different views of what defines bad behavior. Further, they are assessing the child in different settings, which might also generate different answers. Parents may be more prone to exaggerate the positive parts of their child's behavior and underreport problematic behavior, whereas teachers might evaluate the child relative to the other children in the class, giving different results. Further, since the teacher is the responsible agent in the class room, it may change the relationship between SDQ measures and student outcomes which was observed in the previous section because teachers may 'compensate' in different ways when he or she is aware of behavioral problems. Or the opposite: If the teacher has daily conflicts with some students with (externalizing) behavioral

[10]The corresponding tables with 9th grade exit exam, Reading and high school enrollment as outcomes show the same pattern. These tables are available on request from the authors.

Table 2.6: Estimates of behavioral effects (Parents' SDQ) on Math (exit exam)

	(1) No controls	(2) (1) + Basic	(3) (2) + Parental	(4) (3) + School FE	(5) (4) + Class Var.
Abnormal Externalizing Behavior (SDQ1)	-0.7493***	-0.7009***	-0.5598***	-0.5693***	-0.5716***
	(0.0831)	(0.0825)	(0.0811)	(0.0808)	(0.0807)
Borderline Externalizing Behavior (SDQ1)	-0.5934***	-0.5565***	-0.4754***	-0.4801***	-0.4837***
	(0.0823)	(0.0833)	(0.0616)	(0.0651)	(0.0648)
Abnormal Internalizing Behavior (SDQ2)	-0.2047***	-0.1499***	-0.0301	-0.0168	-0.0159
	(0.0559)	(0.0498)	(0.0531)	(0.0539)	(0.0536)
Borderline Internalizing Behavior (SDQ2)	-0.1330	-0.1026	-0.0305	-0.0688	-0.0632
	(0.0806)	(0.0663)	(0.0822)	(0.0756)	(0.0770)
Girl	-0.1469***	-0.1509***	-0.1378***	-0.1363***	-0.1275***
	(0.0219)	(0.0214)	(0.0205)	(0.0207)	(0.0209)
Abnormal Externalizing Behavior*Girl (SDQ1)	-0.2350	-0.1889	-0.1139	-0.1398	-0.1305
	(0.1595)	(0.1607)	(0.1465)	(0.1605)	(0.1609)
Borderline Externalizing Behavior*Girl (SDQ1)	0.1760	0.1652	0.2781***	0.2527**	0.2556**
	(0.1172)	(0.1191)	(0.1023)	(0.1078)	(0.1061)
Abnormal Internalizing Behavior*Girl (SDQ2)	0.0061	-0.0015	0.0262	0.0264	0.0251
	(0.0759)	(0.0707)	(0.0711)	(0.0705)	(0.0700)
Borderline Internalizing Behavior*Girl (SDQ2)	-0.0760	-0.0762	-0.0305	0.0255	0.0202
	(0.1100)	(0.0890)	(0.0849)	(0.0795)	(0.0806)
N	6308	6258	6031	6031	6031
R^2	0.054	0.093	0.225	0.256	0.257

Standard errors in parentheses and clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Basic chracteristics are for instance type of day care, health, native, siblings, etc.
For the full list of included covariates, see Section 2.3.4

problems, this may have effects on both teacher SDQ scores and school outcomes. Parents do not have easy access to a comparison to their child or a standard for normal behavior. In contrast to this, teachers actually do have a comparison group. Further, there may be gender differences in these assessment patterns. According to Ullebø, Posserud, Heiervang, Obel, and Gillberg (2012) who analyze children with externalizing behavior (ADHD diagnosis), parents more often tend to identify girls with these symptoms than teachers do. Therefore, we look into how the teacher evaluations of behavior affect the school outcomes for boys and girls in order to evaluate whether teachers and parents tend to agree on behavioral problems, and whether it matters for the results if they do not agree.

Figure 2.3 shows how the students are evaluated by both parents and teachers. The picture is clear: Teachers are more prone to assess 'extreme' behavior, i.e., an SDQ score of 0 or above 13, whereas the parental evaluations are more evenly distributed.

In the same spirit, Table 2.7 shows the categorization of students in abnormal, borderline and normal behavior by parent and teacher evaluations. The picture is the same: teachers categorize more children as normal and as abnormal, whereas parents report more borderline behavior. This might either reflect that parents have a more nuanced view of their child's behavior, or that teachers are more aware of, what is normal and not normal behavior.

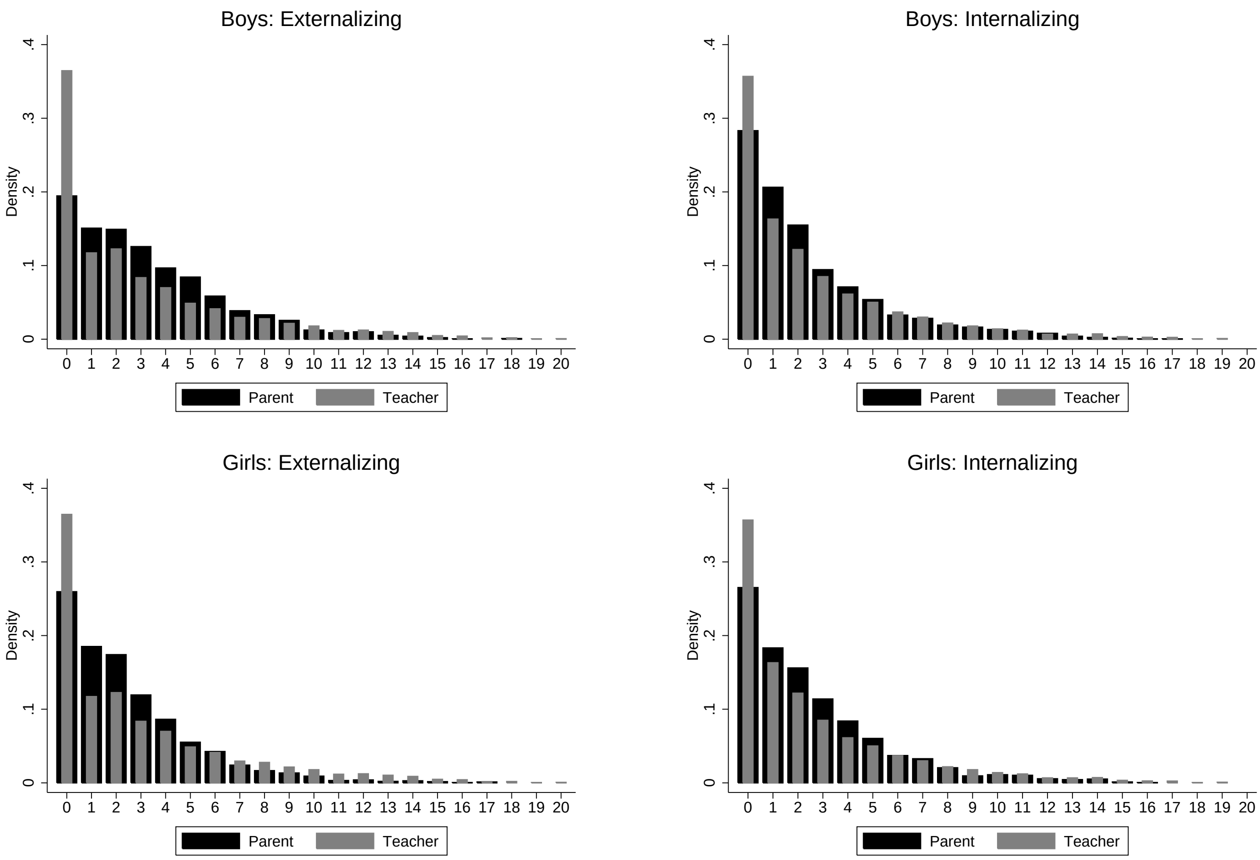

Figure 2.3: SDQ scores for boys and girls evaluated by parents and teachers

Table 2.7: Behavioral assessment of parents and teachers

	Teacher evaluation					
Externalizing	**Girls**			**Boys**		
Parental evaluation	Abnormal	Borderline	Normal	Abnormal	Borderline	Normal
Abnormal	23	5	33	59	16	34
Borderline	6	3	67	43	21	83
Normal	49	46	2,307	194	157	1,907
	Teacher evaluation					
Internalizing	**Girls**			**Boys**		
Parental evaluation	Abnormal	Borderline	Normal	Abnormal	Borderline	Normal
Abnormal	75	19	78	82	31	77
Borderline	38	19	120	28	26	99
Normal	132	132	1,925	128	110	1,930

Table 2.8 shows the estimates of the main model for the four school outcomes using teacher evaluations. The model estimated in Table 2.8 is fully comparable to the model on the parental SDQ scores in Table 2.5, except that there are fewer observations included in Table 2.8.[11]

Table 2.8: Estimates of behavioral effects (Teachers' SDQ) on school outcomes

	9th grade exit exam	Reading (exit exam)	Math (exit exam)	HS Enrollment
Abnormal Externalizing Behavior (SDQ1)	0.0048	-0.1914***	-0.5269***	-0.0290*
	(0.0096)	(0.0522)	(0.0459)	(0.0169)
Borderline Externalizing Behavior (SDQ1)	-0.0006	-0.3678***	-0.3892***	-0.0366
	(0.0105)	(0.0745)	(0.0709)	(0.0222)
Abnormal Internalizing Behavior (SDQ2)	0.0012	-0.1316**	-0.0699	-0.0428**
	(0.0162)	(0.0537)	(0.0659)	(0.0206)
Borderline Internalizing Behavior (SDQ2)	-0.0204	-0.0814	-0.0211	-0.0332
	(0.0157)	(0.0727)	(0.0619)	(0.0221)
Girl	-0.0012	0.3347***	-0.1565***	-0.0052
	(0.0041)	(0.0308)	(0.0273)	(0.0062)
Abnormal Externalizing Behavior*Girl (SDQ1)	-0.0179	-0.1271	-0.2166	-0.0098
	(0.0348)	(0.1445)	(0.1315)	(0.0486)
Borderline Externalizing Behavior*Girl (SDQ1)	-0.0452	0.1516	-0.1433	-0.0791
	(0.0522)	(0.2027)	(0.1092)	(0.0562)
Abnormal Internalizing Behavior*Girl (SDQ2)	-0.0218	-0.0625	0.0960	0.0138
	(0.0172)	(0.0858)	(0.0966)	(0.0286)
Borderline Internalizing Behavior*Girl (SDQ2)	0.0322*	-0.0223	-0.0816	-0.0007
	(0.0184)	(0.1045)	(0.0824)	(0.0348)
N	4625	4565	4562	4625
R^2	0.086	0.203	0.269	0.165

Standard errors in parentheses
Conditioning variables: Basic, parental, School FE, and class variables.
Standard errors are clustered at the school level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Most of the results in the upper part of Table 2.8 based on teachers' SDQ scores are similar to the results found in Table 2.5 using the parents' SDQ scores, implying that no matter who evaluates the student, behavioral problems tend to have a negative impact on school outcomes. In the lower part of Table 2.8 we find only insignificant coefficients, except for one (girls with borderline internalizing behavior in taking the 9th grade exit exam). Thus, our results indicate that it matters whether the SDQ scores are given by the parents or the teachers with respect to the relation to student outcomes. The gender-specific coefficients tend to disappear when we base our behavioral measures on teachers' evaluation. Based on Table 2.8 we are not able to explain which mechanisms drive this result. It may be that teachers are more gender-neutral in evaluating the behavior of boys and girls, or it may be that teachers tend to moderate or

[11] In alternative estimations, not shown here, we have restricted the estimations based on parents' SDQ scores to the students for whom we also have the teacher's SDQ scores shown in Table 2.8. The results do not change in any notable way when restricting the sample in this way. Thus, we conclude that the differences in coefficients between teachers' and parents' SDQ scores on student outcomes are not due to fewer observations. The results are available upon request from the authors.

compensate for the potential negative effects of bad behavior when they are aware of these behavioral problems. The results may also reflect that parental SDQ scores (and also teachers' SDQ scores) are related to their own social background, and this relationship may be different for boys and girls if parents from different social backgrounds have different gender-specific expectations regarding the behavior of their children. We look closer at this question in Section 2.6.

For 1,662 students we also observe the gender of the teacher. It might be that the gender of the teacher could have an effect on the different school outcomes. In the study by Datta Gupta et al. (2012) it is found that mothers and fathers have significantly different SDQ assessment of their children. Thus, a hypothesis may be that the SDQ scores depend on the gender of the person who make the assessment.[12] Alternatively, the gender of the teacher may have effects on the behavior of the children. Therefore, we have estimated an alternative model (not shown here, but the results are available on request from the authors) where we include as additional right-hand-side variables an indicator for the teacher being male, and interactions with this variable with SDQ-score, Girl, and SDQ-score*Girl. In general we do not find many significant effects relating to the gender of the teacher. One obvious reason may be that our sample is too small when restricted to only 1,662 observations to document significant effects. When significant effects appear, our results indicate that male teachers improve the student outcomes for children with externalizing behavioral problems and opposite for children with internalizing problems. We do not find notable gender effects for this relation.

2.6 Social Background, Behavior and Student Outcome

It is a well known fact that student outcome is highly sensitive to social background. Bertrand and Pan (2011) show that especially boys from low-resource families are worse off when they have behavioral problems, i.e., the gender-specific impact of behavioral problems on later school outcomes interacted with social background. In this section we look at this interaction between gender, social background and behavior and the impact on student outcome. Our a priori expectation is – in line with Bertrand and Pan (2011) – that boys from low-resource families are relatively more vulnerable than boys from other families and more vulnerable than girls from low-resource families. Thus, we expect that the observed girl-SDQ interaction coefficient is more negative for children born in low-resource families than in other families.

Table 2.9 shows the descriptive statistics for means of outcomes by three categories of low-resource families (low-income families, with a young mother, and non-intact families, i.e., the biological parents stopped living together before the child enters school) separately for boys and

[12]Unfortunately, we do not have information of the gender of the parent who filled the parents' survey.

girls.[13] We merge the categories 'abnormal' and 'borderline' in Table 2.9 and the regression analyses. We also restrict the analysis to Reading and Math outcomes.

As expected, students from low-resource families have lower outcomes than 'other students'. This pattern is mainly evident for grades in Math where children (girls as well as boys) with young mothers have the lowest mean grades, children in non-intact families have higher grades than children with young mothers, and children in low-income families have higher grades than children from non-intact families and lower grades than 'other families', i.e., families which are not low-resource families. (The differences in mean grades between the four sub-groups are all significant). Looking at the gender gap in Math grades, boys score significantly higher mean grades in all four sub-groups. But the lowest gap is found for the group 'other families'.

For Reading grades the picture is slightly different. In all four sub-groups, girls obtain higher mean grades in Reading than boys. But children from low-income families obtain almost as high grades as 'other children', and boys obtain even higher mean grades. The gender gap in Reading is fairly stable across sub-groups, except for children in families with a young mother where the gap is insignificant, i.e., girls obtain almost as low grades as boys from this family type. This result is the opposite of what was expected, i.e., that boys in low-resource families experience larger difficulties in the school system than girls.

Table 2.9: Descriptives: Outcomes and SDQ by gender for low resource families

	All	**Other**	**Low income**	**Non-intact**	**Young mother**
			SDQ		
Proportion with externalizing behavior					
Girls	0.0604	0.0418	0.0931	0.1231	0.1399
Boys	0.1123	0.0895	0.1372	0.1888	0.1742
Proportion with internalizing behavior					
Girls	0.1489	0.1218	0.1861	0.2334	0.2937
Boys	0.1405	0.1154	0.1722	0.2126	0.2516
			Outcomes		
Reading					
Girls	0.4379	0.4890	0.4177	0.2236	-0.2337
Boys	0.0889	0.1118	0.0993	-0.0379	-0.1587
Diff: Girls-Boys	**0.3490**	**0.3772**	**0.3184**	**0.2615**	**-0.0749**
Math					
Girls	0.2578	0.3640	0.1000	-0.0949	-0.3516
Boys	0.3829	0.4424	0.3024	0.1510	-0.0058
Diff: Girls-Boys	**-0.1251**	**-0.0784**	**-0.2024**	**-0.2459**	**-0.3458**

In order to test the relationship between the observed pattern in SDQ scores and Math and Reading outcomes, we replicate the regressions above for each sub-sample of students. The results for the SDQ variables and the girl indicator are shown in Table 2.10 for the full sample (column 1) and each sub-group (columns 2-5). As found above, for the full sample there is a strong and significantly negative coefficient of the indicator for externalizing behavioral prob-

[13]The three categories may overlap in the sense that some students may show up in more of the low resource family categories.

Table 2.10: Estimates of behavioral effects (Parents' SDQ) on Reading (exit exam) and Math (exit exam) for subgroups

	All	Other	Low income	Non-intact	Young mom
		Reading			
Externalizing Behavior (SDQ1)	-0.5058***	-0.5083***	-0.5173***	-0.4514***	-0.5900**
	(0.0599)	(0.1238)	(0.1482)	(0.1364)	(0.2322)
Internalizing Behavior (SDQ2)	-0.2326***	-0.2204***	-0.2466	-0.1814	-0.0505
	(0.0487)	(0.0729)	(0.1498)	(0.1094)	(0.2725)
Girl	0.3851***	0.6335***	-0.0112	0.2493	0.0889
	(0.1039)	(0.1478)	(0.2201)	(0.1813)	(0.3814)
Externalizing Behavior*Girl (SDQ1)	0.0220	0.1539	-0.1226	-0.0225	0.3167
	(0.1186)	(0.1802)	(0.2121)	(0.1481)	(0.2694)
Internalizing Behavior*Girl (SDQ2)	0.0306	0.1462*	-0.2450	-0.0251	-0.3101
	(0.0830)	(0.0826)	(0.2410)	(0.1626)	(0.3828)
N	4773	3157	870	930	286
R^2	0.061	0.057	0.090	0.059	0.042
		Math			
Externalizing Behavior (SDQ1)	-0.6804***	-0.6511***	-0.5610***	-0.7377***	-0.4739***
	(0.0729)	(0.0877)	(0.1417)	(0.1572)	(0.1665)
Internalizing Behavior (SDQ2)	-0.1797***	-0.1447**	-0.2665*	-0.1479	-0.1360
	(0.0448)	(0.0657)	(0.1475)	(0.1098)	(0.1454)
Girl	-0.0243	0.1529	-0.4498**	0.0099	-0.0640
	(0.1622)	(0.2126)	(0.1932)	(0.2007)	(0.2699)
Externalizing Behavior*Girl (SDQ1)	0.0900	0.2003	-0.2786	0.2671	-0.0875
	(0.1254)	(0.1716)	(0.1784)	(0.1944)	(0.3033)
Internalizing Behavior*Girl (SDQ2)	0.0333	0.0733	0.0080	0.0065	0.3052
	(0.0813)	(0.0977)	(0.1927)	(0.1535)	(0.2628)
N	4769	3157	868	930	287
R^2	0.046	0.030	0.078	0.070	0.054

Standard errors in parentheses
Standard errors are clustered at the school level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

lems on both Reading and Math (the estimated coefficients are -0.51 and -0.68, i.e., more than one half standard deviation of the scores in Reading and Math). For internalizing behavioral problems, the coefficients are numerically smaller, but still negative and significant (-0.18 to -0.23). When splitting the full sample into sub-groups of low-resource families, an interesting pattern emerges: The significantly negative coefficients of the internalizing problem indicators disappear, except for one coefficient (Math, Low income). Further, there is no clear indication that externalizing behavioral problems are more negatively related to low grades in Math and Reading in low-resource families compared to families which are not observed as low-resource families. Also, we do not find, that the coefficients of the girl*SDQ measures are positive (i.e., that boys with behavioral problems should be more vulnerable than comparable girls in low resource families). All interaction coefficients are insignificant! The only positively significant interaction is found for the sub-group of 'other families' (Reading, Internalizing behavior). Of course, a part of the explanation is that the sample size is reduced, when we look at these sub-groups. Actually, we observe that many of the point estimates are the same for the low-resource samples.

Table 2.10 further indicates that the large positive coefficient of the girl indicator in Reading in full sample (+0.39) stems from the observations in the group of 'other families' (+0.63), but we do not find this result in low-resource families where all coefficients of the girl indicator are numerically smaller and insignificant. Thus, our results indicate that the large and positive gap in Reading grades between girls and boys is mainly due to a 'girl-gap' in normal families, i.e., families which are not categorized as low-resource families. For Math, the results are different. The girl indicator is significantly negative in the sub-group of low-income families, but it is insignificant and sometimes even positive in all other groups.[14]

In order to sum up our results, we perform an Oaxaca decomposition[15] of the gender gap in Reading and Math scores based on the estimation in Table 2.10. The mean outcome gap between girls and boys is decomposed into three components, an 'endowment component' (differences in characteristics), a 'coefficient component', and an interaction component (which implies that the three components add up to the total raw gap in school outcome). We use boys' coefficient and endowments as the reference point (for simplicity we drop the subscripts):[16]

$$\overline{Outcome}_{girls-boys} = \overline{Z}^g\widehat{\lambda}^g - \overline{Z}^b\widehat{\lambda}^b = (\overline{Z}^g - \overline{Z}^b)\widehat{\lambda}^b + \overline{Z}^b(\widehat{\lambda}^g - \widehat{\lambda}^b) + (\overline{Z}^g - \overline{Z}^b)(\widehat{\lambda}^g - \widehat{\lambda}^b)$$

where superscripts g and b refer to girls and boys, respectively, and Z and λ are characteristics (endowments) and parameters in the estimated model. The results relating to the SDQ behavioral variables are shown in Table 2.11.[17]

Table 2.11 indicates that the gender gap in Reading and Math cannot be ascribed to gender differences in behavioral problems, either the endowment component or the coefficient component.[18] Though the previous tables show that behavioral problems are clearly related to school outcomes, we do not find many significant components in Table 2.11, and in most cases where some of the components are significant, their absolute size is marginal compared to the observed gender gap. Especially for the Reading gap, the SDQ components only account for a few percentages of the observed reading gap (for instance for the subgroup 'other families' 4 percent of the gender gap in Reading is estimated to be due to the endowment component. For young mothers, the estimated percentage is higher, but here the component is insignificant). For Math, differences in behavioral problems are estimated to be relatively larger. The sign of the estimated components are positive for the SDQ components while the observed gaps in Math are negative, i.e., the differences in 'endowment of behavioral problems' are in favor of girls' grades in Math. But since the observed Math gap is negative, the unexplained gap is actually larger than the observed gap, according to the estimations.

[14]The analysis is also done for high school enrollment (or vocational education) and the results show no indication of a pattern in the gender gap in behavioral problems on further education. The results are available upon request.

[15]Introduced in Oaxaca (1973).

[16]The decomposition is performed by a STATA program, see Jann (2008).

[17]The results from the estimations, the full decomposition, and the two other school outcome variables are available upon request from the authors.

[18]In Appendix 2.A, Table 2.15 shows the results of a decomposition relating behavioral problems to taking any further education (i.e. either high school or vocational education). The results show no gender gap in behavioral problems on further education.

Table 2.11: Oaxaca Decomposition for samples of students from low-resource families: Reading and Math

	All	Other	Low income	Non-intact	Young mother
			Reading		
Difference: Girls-Boys	0.3536***	0.3730***	0.3112***	0.2919***	0.0630
	(0.0251)	(0.0303)	(0.0684)	(0.0678)	(0.0919)
Percentage	*100.00*	*100.00*	*100.00*	*100.00*	*100.00*
Endowments					
SDQ Externalizing	0.0148***	0.0167***	0.0086	0.0098	0.0236
	(0.0031)	(0.0060)	(0.0081)	(0.0068)	(0.0270)
Percentage	*4.19*	*4.48*	*2.76*	*3.36*	*37.4*
SDQ Internalizing	-0.0017	-0.0002	-0.0019	-0.0047	0.0047
	(0.0015)	(0.0013)	(0.0046)	(0.0052)	(0.0181)
Percentage	*-0.48*	*-0.05*	*-0.61*	*-1.61*	*7.46*
Coefficients					
SDQ Externalizing	0.0114	0.0216	-0.0088	-0.0091	0.0658
	(0.0094)	(0.0134)	(0.0197)	(0.0235)	(0.0425)
Percentage	*3.22*	*5.79*	*-2.83*	*-3.12*	*104.44*
SDQ Internalizing	0.0032	0.0099	-0.0218	-0.0182	-0.0544
	(0.0119)	(0.0101)	(0.0255)	(0.0293)	(0.0468)
Percentage	*0.90*	*2.65*	*-7.01*	*-6.24*	*-86.35*
N	4691	3132	846	881	278
			Math		
Difference: Girls-Boys	-0.1165***	-0.0815**	-0.1945***	-0.2334***	-0.2242**
	(0.0279)	(0.0331)	(0.0521)	(0.0657)	(0.0972)
Percentage	*100.00*	*100.00*	*100.00*	*100.00*	**100.00**
Endowments					
SDQ Externalizing	0.0214***	0.0227***	0.0093	0.0252*	0.0109
	(0.0038)	(0.0047)	(0.0078)	(0.0132)	(0.0155)
Percentage	*-18.37*	*-27.85*	*-4.78*	*-10.80*	*-4.86*
SDQ Internalizing	-0.0002	0.0004	-0.0037	0.0057	-0.0139
	(0.0012)	(0.0008)	(0.0048)	(0.0055)	(0.0135)
Percentage	*0.17*	*-0.49*	*1.90*	*-2.44*	*6.20*
Coefficients					
SDQ Externalizing	0.0150*	0.0208**	-0.0276	0.0293	-0.0189
	(0.0089)	(0.0090)	(0.0189)	(0.0290)	(0.0464)
Percentage	*-12.88*	*-25.52*	*14.19*	*-12.55*	*8.43*
SDQ Internalizing	0.0074	0.0034	0.0189	0.0029	0.0889**
	(0.0091)	(0.0102)	(0.0207)	(0.0246)	(0.0396)
Percentage	*-6.35*	*-4.17*	*-9.72*	*-1.24*	*-39.65*
N	4688	3132	845	882	279

Standard errors in parentheses.
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In most cases, the SDQ components are insignificant for the low-resource families (and the absolute size of the coefficients are also relatively small, i.e., the insignificance is generally not because of fewer observations in these sub-groups).

Thus, the overall conclusion from this section is that only a minor proportion of the 'girl gap' in Reading can be related to differences in abnormal or borderline behavioral problems between boys and girls, and there is no tendency that behavioral problems account for a larger proportion of the gender gap in Reading in low-resource families. The large and positive gap in Reading grades between girls and boys is mainly due to a 'girl-gap' in normal families. For Math, our results indicate that the SDQ endowment components slightly favor girls' grades, mostly in 'normal families'. For low-income families, we find a very negative coefficient of the girl indicator, i.e., girls in low-income families seem to get much lower grades than boys from low-income families, irrespective of behavioral endowments.

2.7 Conclusion

The results in this study document that behavioral problems are significantly related to lower exit exam grades in Reading and Math and a lower probability of taking 9th grade exit exam and being enrolled at high school. About 11 percent of the boys and 6 percent of the girls in our sample have abnormal or borderline externalizing behavioral problems. For internalizing problems, girls and boys have about the same average scores, and 14 percent of the children are categorized as having abnormal or borderline internalizing problems.

The estimations show significantly and large negative coefficients of the externalizing behavioral indicators. Boys with abnormal externalizing behavior are estimated to have 3.6 percentage points lower chance of taking the 9th grade exit exam, and their score in Reading is estimated to be 0.36 standard deviations lower than children without behavioral problems. For Math this figure is 0.57 of a standard deviation. According to our estimations, the school outcomes for girls with abnormal externalizing behavior are not significantly different from those of boys with the same behavioral problems. For borderline externalizing problems and for internalizing problems, the estimated main coefficients are numerically smaller but still significantly negative in most cases, and for these behavioral categories girls tend to have less negative coefficients.

We document that measurement of behavioral problems depends on whether it is the parents or the teachers who report the problems and this evidence has an effect on the estimated relationship between gender, behavioral problems and school outcomes. The negative outcome effects of behavioral problems are numerically smaller, and the gender differences less significant when teachers' Strengths and Difficulties Questionnaire (SDQ) scores are applied in the estimation. This may reflect that teachers who are aware of behavioral problems try to compensate for these problems.

Splitting the sample into subgroups of low social resource families, we find that children from low-resource families have lower school outcomes. The gender gap in Reading is observed to be largest in the subgroup of families who are not categorized as low-resource families and smallest in the subgroup of families with a young mother. Thus, we do not find that boys with behavioral problems from low-resource families seem to be more vulnerable than girls coming from low-resource families.

A decomposition of the estimations indicates that most of the gender differences in Reading and Math cannot be related to gender differences in behavioral problems. The overall gender gap in Reading seems mainly to be the result of gender differences between children without behavioral problems living in 'normal families', i.e., families which are not categorized as low-resource families. For Math, the higher overall grades for boys compared to girls seem mainly to be the result of very low Math grades for girls without behavioral problems from low-income families.

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Appendices

2.A Tables

Table 2.12: Description of variables

Variable name	Description	Source
Basic characteristics		
Boy	Boy (0/1)	Registers
Native	Native Dane (0/1)	Registers
Birth weight (g)	The child's birth weight in grams	Registers
Length of pregnancy	Length of pregnancy in weeks	Registers
Born prematurely	The child is born before the 37th gestational week (0/1)	Registers
Born extremely prematurely	The child is born before the 28th gestational week (0/1)	Registers
Complications at delivery	Complications during delivery of the child based on an APGAR score of 7 or above. (0/1)	Registers
Year of birth: 1990	The child is born in 1990 (0/1)	Registers
Year of birth: 1991	The child is born in 1991 (0/1)	Registers
Year of birth: 1992	The child is born in 1992 (0/1)	Registers
Number of younger siblings	Number of younger siblings before the child is 7 years old, including half-siblings.	Registers
Number of older siblings	Number of older siblings before the child is 7 years old, including half-siblings.	Registers
Younger siblings	The child has younger siblings before the age of 7 (0/1)	Registers
Older siblings	The child has older siblings measured before the age of 7 (0/1)	Registers
Psychological diagnosis	The child is diagnosed with a mental or behavioral disorder before the age of 7. (0/1)	Registers
Cardiovascular medicine	The child is prescribed cardiovascular medicine before turning 7 years old. (0/1)	Registers
Antidepressant medicine	The child is prescribed antidepressant medicine before turning 7 years old. (0/1)	Registers
Finished 9th grade in 2005	Completion of 9th grade in 2005 (0/1)	Registers
Finished 9th grade in 2006	Completion of 9th grade in 2006 (0/1)	Registers
Finished 9th grade in 2007	Completion of 9th grade in 2007 (0/1)	Registers
Finished 9th grade in 2008	Completion of 9th grade in 2008 (0/1)	Registers
Finished 9th grade in 2009	Completion of 9th grade in 2009 (0/1)	Registers
Finished 9th grade in 2010	Completion of 9th grade in 2010 (0/1)	Registers
Moved to another municipality	Moved to another municipality before the age of 7 (0/1)	Registers
Number of moves between municipalities	Number of moves between municipalities before the age of 7.	Registers
Not registered day care	Day care is not registered (0/1)	Registers
Centralized day care	The child is enrolled in centerbased day care at the age of 4 (0/1)	Registers
No outside home day care	The child is taken care of in the home by either parents or grandparents at the age of 4 (0/1)	Registers
Private day care	The child is enrolled in private family day care at the age of 4	Registers

Table 2.12: Description of variables – continued from previous page

Variable name	Description	Source
Mother's characteristics		
Committed crime	Crime committed before the child is 7 years old(0/1).	Registers
Mother's degree of employment		
Degree of employment, 4 years old	Degree of year employed when the child is 4 years old. = 1000 if unemployed the whole year.	Registers
Degree of employment, 5 years old	Degree of year employed when the child is 4 years old. = 1000 if unemployed the whole year.	Registers
Degree of employment, 6 years old	Degree of year employed when the child is 4 years old. = 1000 if unemployed the whole year.	Registers
Mother's employment sector		
Private sector	Works in the private sector (0/1)	Registers
City authorities	Works in the city authorities (0/1)	Registers
Public sector	Works in the public sector (0/1)	Registers
Mother's employment		
Full-time employed	Is full-time employed (>25 h/week) (0/1)	Registers
Part-time employed	Is part-time employed (<25 h/week) (0/1)	Registers
Other	Is in other form of employment (0/1)	Registers
Mother's occupation		
Employer	Is employer when the child is 6 years old (0/1)	Registers
Manager	Is employed at management level when the child is 6 years old (0/1)	Registers
White-collar worker	Is employed at medium level as a white-collar worker when the child is 6 years old (0/1)	Registers
Unskilled worker	Is employed as an unskilled worker at lower level when the child is 6 years old (0/1)	Registers
Not in the labor force	Not in the labor force	Registers
Mother's highest education		
No education	Compulsory school, corresponding to 9 years of education when the child is 6 years old (0/1)	Registers
High school education	High school, corresponding to 12 years of education when the child is 6 years old (0/1)	Registers
Vocational education	Vocational education, corresponding to 12 years of education when the child is 6 years old (0/1)	Registers
Short cycle education	Short cycle education, corresponding to 14 years of education when the child is 6 years old (0/1)	Registers
Long cycle education	Long cycle education, corresponding to 17 years of education when the child is 6 years old (0/1)	Registers
On-going education	On-going education. If enrolled in education program when the child is 6 years old (0/1)	Registers

Table 2.12: Description of variables – continued from previous page

Variable name	Description	Source
Mother's health		
Psychological diagnosis	Diagnosed with a mental or behavioral disorder before the child is 6 years old (0/1)	Registers
Cardiovascular medicine	Prescribed cardiovascular medicine before the child is 6 years old (0/1)	Registers
Antidepressant medicine	Prescribed antidepressant medicine before the child is 6 years old (0/1)	Registers
Father's characteristics		
Committed crime	Crime committed before the child is 7 years old(0/1).	Registers
Father's degree of employment		
Degree of employment, 4 years old	Degree of year employed when the child is 4 years old. = 1000 if unemployed the whole year.	Registers
Degree of employment, 5 years old	Degree of year employed when the child is 4 years old. = 1000 if unemployed the whole year.	Registers
Degree of employment, 6 years old	Degree of year employed when the child is 4 years old. = 1000 if unemployed the whole year.	Registers
Father's employment sector		
Private sector	Works in the private sector (0/1)	Registers
City authorities	Works in the city authorities (0/1)	Registers
Public sector	Works in the public sector (0/1)	Registers
Father's employment		
Full-time employed	Is full-time employed (>25 h/week) (0/1)	Registers
Part-time employed	Is part-time employed (<25 h/week) (0/1)	Registers
Other	Is in other form of employment (0/1)	Registers
Father's occupation		
Employer	Is employer when the child is 6 years old (0/1)	Registers
Manager	Is employed at management level when the child is 6 years old (0/1)	Registers
White-collar worker	Is employed at medium level as a white-collar worker when the child is 6 years old (0/1)	Registers
Unskilled worker	Is employed as an unskilled worker at lower level when the child is 6 years old (0/1)	Registers
Not in the labor force	Not in the labor force	Registers

Table 2.12: Description of variables – continued from previous page

Variable name	Description	Source
Father's highest education		
No education	Compulsory school, corresponding to 9 years of education when the child is 6 years old (0/1)	Registers
High school education	High school, corresponding to 12 years of education when the child is 6 years old (0/1)	Registers
Vocational education	Vocational education, corresponding to 12 years of education when the child is 6 years old (0/1)	Registers
Short cycle education	Short cycle education, corresponding to 14 years of education when the child is 6 years old (0/1)	Registers
Long cycle education	Long cycle education, corresponding to 17 years of education when the child is 6 years old (0/1)	Registers
On-going education	On-going education. If enrolled in education program when the child is 6 years old (0/1)	Registers
Father's health		
Psychological diagnosis	Diagnosed with a mental or behavioral disorder before the child is 6 years old (0/1)	Registers
Cardiovascular medicine	Prescribed cardiovascular medicine before the child is 6 years old (0/1)	Registers
Antidepressant medicine	Prescribed antidepressant medicine before the child is 6 years old (0/1)	Registers
Family background		
Mother's age at birth	The mother's age when she gave birth to the child	Registers
Age difference between father and mother	The age difference between the father and the mother at the time of birth	Registers
Young mother (age $<$ 24)	The mother is younger than 24 years old (0/1)	Registers
Non-intact family	The biological parents are not cohabiting up until the child is 6 years old (0/1)	Registers
Average household income	Average household income measured at time of birth.	Registers
Low-income household ($<$ 50% of the median)	Low-income household, having an average income below 50% of the median income (0/1)	Registers
Behaviour		
Abnormal Externalizing behavior	Showing behavioral problems in terms of abnormal externalizing behavior based on the SDQ score (0/1)	2002 Questionnaire
Borderline Externalizing behavior	Showing behavioral problems in terms of borderline externalizing behavior based on the SDQ score (0/1)	2002 Questionnaire
Abnormal Internalizing behavior	Showing behavioral problems in terms of abnormal internalizing behavior based on the SDQ score (0/1)	2002 Questionnaire
Borderline Internalizing behavior	Showing behavioral problems in terms of borderline internalizing behavior based on the SDQ score (0/1)	2002 Questionnaire
Outcomes		
Taking 9th grade exit exam	Taking the 9th grade exit exam (0/1)	Registers
Reading (exit exam)	Reading exam standardized test score	Registers
Math (exit exam)	Math exam standardized test score	Registers
High school enrollment	Enrolled in high school before turning 19 years old	Registers

Table 2.13: Descriptive statistics of covariates, behavior and school outcomes

	Girls		Boys		
Variable	**Mean**	**Std.dev.**	**Mean**	**Std.dev.**	**Diff**
Native	0.97	0.16	0.98	0.14	
Birthweight	3426.66	573.08	3548.66	599.09	***
Length of pregnancy	39.53	1.82	39.51	1.88	
Born prematurely	0.09	0.29	0.10	0.30	
Born pxtremely prematurely	0.00	0.03	0.00	0.04	
Complications at delivery	0.01	0.10	0.01	0.11	
Year: 1990	0.40	0.49	0.40	0.49	
Year: 1991	0.39	0.49	0.41	0.49	
Year: 1992	0.20	0.40	0.19	0.39	
No. older siblings	0.85	1.01	0.86	1.00	
No. younger siblings	0.63	0.68	0.64	0.68	
Older siblings	0.55	0.50	0.56	0.50	
Younger siblings	0.53	0.50	0.54	0.50	
Psychological diagnosis	0.00	0.06	0.01	0.10	***
Cardiovascular medicine	0.00	0.06	0.00	0.06	
Antidepressant medicine	0.01	0.11	0.01	0.10	
Finished 9th grade in 2005	0.01	0.11	0.00	0.05	***
Finished 9th grade in 2006	0.36	0.48	0.33	0.47	**
Finished 9th grade in 2007	0.39	0.49	0.40	0.49	
Finished 9th grade in 2008	0.23	0.42	0.25	0.43	
Finished 9th grade in 2009	0.00	0.05	0.01	0.10	***
Finished 9th grade in 2010	0.00	0.02	0.00	0.02	
Moved to another municipality	0.23	0.42	0.23	0.42	
No. of moves between municipalities	0.30	0.60	0.29	0.58	
Not registered day care	0.02	0.16	0.02	0.15	
Centralized day care	0.90	0.30	0.90	0.29	
No outside home day care	0.04	0.19	0.03	0.18	
Private day care	0.04	0.19	0.04	0.19	
Mother's age at birth	29.76	4.58	29.73	4.47	
Age difference between father and mother	2.41	4.23	2.37	4.35	
Young mother (age $<$ 24)	0.07	0.25	0.07	0.25	
Non-intact family	0.21	0.41	0.21	0.41	
Average household income	178741.05	98256.22	179826.73	96787.37	
Low-income household ($<$ 50% of median)	0.19	0.39	0.18	0.39	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13: Descriptive statistics of covariates, behavior and school outcomes – continued from previous page

	Girls		Boys		
Variable	**Mean**	**Std.dev.**	**Mean**	**Std.dev.**	**Diff**
Mother degree of year employed (4 years old)	101.42	219.15	100.45	218.52	
Mother degree of year employed (5 years old)	95.46	209.61	87.42	203.11	
Mother degree of year employed (6 years old)	88.18	201.70	79.91	193.37	*
Mother employed in private sector	0.29	0.46	0.30	0.46	
Mother employed in the city authorities	0.65	0.48	0.63	0.48	*
Mother employed in public sector	0.06	0.24	0.07	0.26	**
Mother full-time employed	0.83	0.37	0.84	0.37	
Mother part-time employed	0.03	0.18	0.04	0.19	
Mother other type of employment	0.14	0.34	0.13	0.33	
Mother employer	0.01	0.12	0.01	0.11	
Mother manager	0.30	0.46	0.29	0.45	
Mother white-collar worker	0.31	0.46	0.32	0.47	
Mother unskilled worker	0.15	0.36	0.15	0.36	
Mother unemployed	0.11	0.31	0.11	0.32	
Mother not in labor force	0.11	0.31	0.10	0.30	
Mother no education	0.19	0.39	0.18	0.39	
Mother long cycle education	0.08	0.28	0.08	0.27	
Mother high school education	0.15	0.36	0.14	0.35	
Mother vocational education	0.28	0.45	0.29	0.45	
Mother short cycle education	0.30	0.46	0.31	0.46	
Mother on-going education	0.10	0.30	0.09	0.28	
Mother psychological diagnosis	0.02	0.14	0.03	0.16	
Mother cardiovascular medicine	0.14	0.35	0.15	0.36	
Mother antidepressant medicine	0.23	0.42	0.23	0.42	
Father degree of year employed (4 years old)	65.84	191.11	63.11	189.70	
Father degree of year employed (5 years old)	55.65	175.23	49.78	166.62	
Father degree of year employed (6 years old)	46.83	156.90	44.86	153.68	
Father employed in private sector	0.56	0.50	0.55	0.50	
Father employed in the city authorities	0.36	0.48	0.35	0.48	
Father employed in public sector	0.09	0.28	0.10	0.29	
Father full-time employed	0.86	0.35	0.87	0.34	
Father part-time employed	0.01	0.11	0.01	0.11	
Father other type of employment	0.13	0.33	0.12	0.33	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13: Descriptive statistics of covariates, behavior and school outcomes – continued from previous page

	Girls		Boys		
Variable	**Mean**	**Std.dev.**	**Mean**	**Std.dev.**	**Diff**
Father employer	0.05	0.22	0.05	0.23	
Father manager	0.34	0.47	0.35	0.48	
Father white-collar worker	0.14	0.35	0.14	0.35	
Father unskilled worker	0.17	0.38	0.16	0.37	
Father unemployed	0.07	0.25	0.06	0.24	
Father not in labor force	0.08	0.27	0.08	0.27	
Father no education	0.19	0.39	0.17	0.38	
Father long cycle education	0.16	0.36	0.16	0.37	
Father high school education	0.12	0.33	0.11	0.31	*
Father vocational education	0.34	0.47	0.35	0.48	
Father short education	0.19	0.40	0.20	0.40	
Father on-going education	0.11	0.31	0.10	0.30	
Father psychological diagnosis	0.03	0.16	0.02	0.15	
Father cardiovascular medicine	0.09	0.29	0.08	0.28	
Father antidepressant medicine	0.16	0.36	0.16	0.37	
Fraction of boys in class	0.29	0.29	0.33	0.28	***
Fraction of boys in class squared	0.14	0.14	0.19	0.18	***
Fraction of boys in class missing	0.41	0.41	0.39	0.49	*
Class moves before 2002	0.40	0.40	0.37	0.91	
Abnormal Externalizing Behavior (Parent eval.)	0.02	0.16	0.05	0.22	***
Borderline Externalizing Behavior (Parent eval.)	0.04	0.19	0.06	0.24	***
Abnormal Internalizing Behavior (Parent eval.)	0.07	0.26	0.08	0.27	
Borderline Internalizing Behavior (Parent eval.)	0.08	0.26	0.06	0.24	**
Abnormal Externalizing Behavior (Teacher eval.)	0.02	0.15	0.09	0.28	***
Borderline Externalizing Behavior (Teacher eval.)	0.02	0.13	0.06	0.23	***
Abnormal Internalizing Behavior (Teacher eval.)	0.07	0.26	0.07	0.25	
Borderline Internalizing Behavior (Teacher eval.)	0.05	0.22	0.05	0.22	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14: Strengths and Difficulties Questionnaire (SDQ)

Measures	**Description**
Externalizing problems	Measures acting out behavior.
	10 questions (or statements) rated as ”certainly true”, ”somewhat true” or ”not true”.
	The questions include:
	- Restless, overactive, cannot stay still for long
	- Often has temper tantrums or hot tempers
	- Generally obedient, usually does what adults request
	- Constantly fidgeting or squirming
	- Often fights with other children or bullies them
	- Easily distracted, concentration wanders
	- Often lies or cheats
	- Thinks things out before acting
	- Steals from home, school or elsewhere
	- Sees tasks through to the end, good attention span
Internalizing problems	Measures the apparent internal problems.
	10 questions (or statements) rated as ”certainly true”, ”somewhat true” or ”not true”.
	The questions include:
	- Often complains of headaches, stomach-aches or sickness
	- Rather solitary, tends to play alone
	- Many worries, often seems worried
	- Has at least one good friend
	- Often unhappy, down-hearted or tearful
	- Generally liked by other children
	- Nervous or clingy in new situations, easily loses confidence
	- Picked on or bullied by other children
	- Gets on better with adults than with other children
	- Many fears, easily scared

Table 2.15: Oaxaca Decomposition for samples of students from low-resource families: Further education (High school and vocational education)

	All	Other	Low income	Non-intact	Young mother
		Further education			
Difference: Girls-Boys	0.0013	0.0095	-0.0311	-0.0311	-0.0053
	(0.0065)	(0.0077)	(0.0239)	(0.0197)	(0.0314)
Percentage	*100.00*	*100.00*	*100.00*	*100.00*	*100.00*
Endowments					
SDQ Externalizing	0.0014	0.0020*	-0.0000	0.0002	0.0014
	(0.0011)	(0.0012)	(0.0017)	(0.0031)	(0.0050)
Percentage	*107.69*	*21.05*	*0.00*	*-0.64*	*-26.42*
SDQ Internalizing	-0.0002	0.0000	0.0013	-0.0001	-0.0030
	(0.0004)	(0.0004)	(0.0017)	(0.0023)	(0.0058)
Percentage	*-15.38*	*0.00*	*-4.18*	*0.32*	*56.60*
Coefficients					
SDQ Externalizing	-0.0050	0.0033	-0.0269**	-0.0190*	-0.0149
	(0.0032)	(0.0027)	(0.0125)	(0.0110)	(0.0176)
Percentage	*-384.62*	*34.74*	*86.50*	*61.09*	*281.13*
SDQ Internalizing	0.0045	0.0037	-0.0016	0.0068	0.0046
	(0.0028)	(0.0035)	(0.0086)	(0.0136)	(0.0108)
Percentage	*346.15*	*38.95*	*5.14*	*-21.86*	*-86.79*
N	4753	3168	858	901	281

Standard errors in parentheses.
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3

Vulnerable Children and Peer Effects

Vulnerable Children and Peer Effects*

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Abstract

Peer effects in school outcomes have been of great concern over the last decades. In this paper I examine whether the proportion of vulnerable peers at the cohort level affects school performance with the use of administrative data from Denmark and estimate the magnitude of the negative external effects of children who experience adverse life events. Vulnerable children are defined along four dimensions, i.e., children with criminal parents, divorced parents, one or more deceased parent, and children with parents diagnosed with a mental disorder. Peer effects are estimated using the exogenous variation within schools but across cohorts in the proportion of vulnerable peers. Controlling for a wide range of background characteristics and selection to schools, the results show that the proportion of vulnerable children affects the Math test score negatively by about .1 standard deviation. However, no effects seem to be present for Reading test scores. The overall effects mask some interesting heterogenous effects across gender. The results are driven by girls, since they are harmed by the adverse life events of the peers, whereas boys are unaffected. Performing a range of robustness checks suggests that it is important to take selection to schools before school entry into account. Also, using predicted school starting age as an instrument for actual school starting age shows estimates of higher magnitude, indicating that high resource families are more capable in postponing school entry of their children if they observe a relatively unfavorable cohort in the year their child was supposed to enroll.

JEL Classification: I21, J12

Keywords: peer effects, vulnerable children, education

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3.1 Introduction

Over the last decades peer effects have received a great deal of attention. Especially effects of having low ability or bad behaving peers have been investigated, both of which have shown to have negative effects on school performance.

The negative effect on classroom learning may come from one of three channels. First, having a disruptive classmate could cause commotion in the classroom, thereby distracting the teacher and other students. Second, a disruptive student could encourage additional disruptive behavior among other students. And finally, it could be that the child experiences difficulties in learning, such that one student requiring extra teacher attention leaves less teacher attention for the remaining students.

While little is known about how children from dysfunctional homes affect classroom learning, understanding which children cause the problems are important for several reasons. First, the composition of students influences student performance, and designing the optimal education policies that change the composition of peers is important to improve those outcomes. Second, in production function considerations, human capital development uses schools, peers and parents as input. The question is how important the peer effects are. Third, if children exposed to specific family events are extraordinary prone to affect other students negatively, different initiatives could be accommodated, lowering the negative spillovers.

In this paper I investigate peer effects in compulsory school where they potentially have the most pronounced effect, by examining whether the proportion of vulnerable peers at the cohort level affects school performance. The paper contributes to the literature by estimating the magnitude of the negative external effects of children who experience adverse life events. Clearly, an adverse life event may have a negative impact on the child who experiences the event, but the negative effect may go beyond the direct effect and also affect the child's peers. In order to examine how vulnerable students affect other students, I utilize the Danish register data, which is combined with school data from the most recent cohorts in Denmark. It is therefore possible to identify the students exposed to vulnerable peers at the cohort level. Also, former analyses have focused on the effect of having peers of low ability, where this paper uses indicators for vulnerable children in order to capture the potentially problematic behavior or learning disabilities caused by the children who experience such adverse life events. The broad definition of vulnerable children is ment to capture effects that make these children more likely to affect their peers negatively. In this sense, this paper examines whether there exist "bad apples" that affect the learning outcomes of other students.

Using the variation across cohorts within schools, the results show that the proportion of vulnerable children affects the Math test score negatively by about .1 standard deviation. However, no effects seem to be present for Reading test scores. The overall effects mask some interesting heterogenous effects across gender. The results are driven by girls, since they are harmed by the adverse life events of the peers, whereas boys are unaffected. By conducting a range of robustness checks, the results suggest that it is important to take selection to schools before school entry into account Also, using predicted school starting age as an instrument for actual school starting age shows estimates of higher magnitude. One possible explanation could be that high resource familes are more capable in postponing school entry of their children if they observe a relatively unfavorable cohort in the year their child was supposed to enroll.

The rest of the paper is structured as follows. Section 3.2 presents the background literature, and Section 3.3 describes the institutional settings. The data is presented in Section 3.4, whereafter the empirical analysis is carried out in Section 3.5. A wide range of robustness checks are investigated in Section 3.6, and the paper is concluded in Section 3.7.

3.2 Background

Peer effects have been studied with great and increasing interest over the last decades, and several studies have estimated peer effects in schools. Depending on their available data and the settings they study, different estimation strategies have been used to estimate peer effects. Some have used experimental designs such as random assignment (of students to dorms, see e.g. Sacerdote (2001); of freshmen to squadrons, see Carrell, Fullerton, and West (2009); of students to classrooms, see Whitmore (2005)), instrumental variables (see e.g. Angrist and Lang (2004) using Maimonides rule to instrument actual class size by predicted class size in the busing program, *Metco*), variation in student mobility (see e.g. Hanushek, Kain, Markman, and Rivkin (2003) and Zabel (2008)), variation in school closures (see. e.g. Engberg, Gill, Zamarro, and Zimmer (2012)), exogenous variation in peer quality using e.g. hurricane Kathrina as a natural shock to the peer group (see e.g. Imberman, Kugler, and Sacerdote (2012)), measurement error (e.g. Ammermueller and Pischke (2009)), and peer composition in the form of variation across cohorts (e.g. Hoxby (2000)).

As noted in Sacerdote (2011), a determinant of equal importance as class size and parental involvement is that of peer composition. A number of studies have focused on different aspects of peer compositions. Hoxby (2000) examines how students in the classroom are affected by the achievement of their peers using exogenous variation in the gender and racial peer composition across adjacent cohorts. She uses data from 3rd to 6th grade from the public schools in Texas to estimate the effect of changes in the percent of female peers. She finds that a 1 point increase in peer achievement increases a student's own test score by .10 to .55 depending on the specification. She notes that some of the peer effects do not operate through peers' achievement, as both males and females do better in math in classrooms where the fraction of females is higher even though females do not perform better in math than males.

Ammermueller and Pischke (2009) estimate peer effects for 4th graders in six European countries (France, Germany, Iceland, the Netherlands, Norway and Sweden) using data from the Progress in International Reading Literacy Study. They use variation across classes within schools, as they argue that the assignment to classrooms within schools are as good as random. Besides controlling for selection to schools, they also correct for possible measurement error in their survey data. Their results suggest that measurement error is indeed important, and they find on average, across the six countries, that a one standard deviation change in the peer composition leads to a .17 standard deviation change in the reading test scores. They further conclude that the selection to schools seems to be of second order importance once the measurement error is taken into account.

Lavy, Silva, and Weinhardt (2012) investigate peer effects in educational outcomes in secondary schools. They examine both the size of the peer effect and which parts of the ability distribution that drive the impact of peer quality on school performance. This implies that they focus on the exceptionally low and high achievers and how they affect the outcomes of other students. They exploit the variation in achievement across three compulsory subjects with repeated tests within each student. Their results show negative peer effects stemming from low achieveing peers, and they find little evidence that average and high achievers matter. Their results are heterogenous in gender, as girls benefit from high achieving peers whereas boys do not.

While the literature has looked at peer effects at all educational levels,[1] the combination of educational attainment and poor peer behavior, especially in compulsory school, has only recently received attention.

Among the few, Carrell and Hoekstra (2010) investigate the spillovers from children exposed to domestic violence on school performance and disciplinary infractions committed. They use the exogenous variation in the proportion of peers exposed to domestic violence within school and grade over time to identify peer effects. Their data have been collected in Alachua County in Florida and consist of student-level data linked to public courthouse records on domestic violence. They find that adding one troubled child to a classroom of 20 students decreases the achievement test scores (a composite score of math and reading) of the peers by .69 percentage points, though the effect is only significant at the 10% level. When they consider the behavioral outcomes, they find that adding one troubled child to a classroom of 20 students increases each child's disciplinary infractions by .093 percentage points, corresponding to an increase of 17% and significant at the 1% level. They also look at differentiated effects, and they find that especially low-income children and boys from troubled families affect peer performance and behavior, and that especially math and reading outcomes of boys and children from high-income families are affected by troubled children.

[1]Two comprehensive and excellent overviews of the peer effects literature are given in Epple and Romano (2011) and Sacerdote (2011).

Figlio (2007) examines the relationship between the proportion of disruptive peers in the classroom and the students' outcomes and behavior. He proposes an instrument based on the proportion of boys in the peer group with feminine names[2] to instrument for misbehavior in sixth grade. He utilizes a data set from a large district in Florida, and as a proxy for misbehavior he uses student suspension data. He finds that boys with feminine names tend to misbehave disproportionately in sixth grade. In addition, the results show that adding one additional disruptive student to a classroom of 30 students reduces peer math test scores by 2.2 percentage points and increases the likelihood of a peer him- or herself getting suspended at least once for five or more days by 3.2 percentage points. He uses different specifications but the results do not change.

Imberman et al. (2012) use the exogenous inflow of students from the entire ability distribution to examine the peer effects of the intake of evacuees from two areas hit by hurricanes, the Houston Independent School District and the school districts in Louisiana. They look at both performance and behavioral outcomes, i.e., math and language test scores, disciplinary infractions and absenteeism. Their results are all based on "within grade and school"-estimates, and they show that the inflow of evacuees had, on average, little effect on students in the receiving schools in Louisiana. They do not find any mean effects on achievement in Houston, but they do find effects on lower attendance rates and worse behavior. Their results also show that all students benefit from higher ability peers and are hurt by lower ability peers. They conclude that their results suggest that the effects work through students learning from one another or from teachers reacting on the change in the student distribution.

Black, Devereux, and Salvanes (forthcoming) consider the effect of peer composition in 9th grade on a battery of longer-run outcomes such as adult labor market status, earnings and teenage childbearing. They use the idiosyncratic variation in cohort composition within schools to estimate the peer effects. They utilize data from Norwegian administrative data, and they focus on cohorts born between 1959 and 1973. Their results show that their outcomes are affected by the proportion of females, and that the effects are heterogenous across gender, i.e., that a higher proportion of girls is good for the longer run outcomes for girls but bad for boys. They also find that average age and mother's education in general only have little impact on teenagers. They therefore conclude that peer sex composition appears to have more complicated effects when the longer run effects are taken into account as well.

One of the main findings in the peer literature is that peer effects both in achievement and behavior seem to exist and that the effects are important both in the short and longer run.

[2]Figlio (2007) defines feminine names as names empirically given more frequently to girls than to boys.

3.3 Institutional Settings

The Danish compulsory school starts in the year the child turns 7 and goes from 1st grade to 9th grade, or consists of nine years of primary and lower secondary education.[3] A school reform in 2005 introduced the free school choice option, implying that parents could enroll their child in a public school in any municipality of their own choice given that the chosen school has room for one more student.

In general, the distribution of students within schools within a given cohort is not random. This is because two principles are used in the construction of classes. First, classes are intended to be of equal sizes, and second, boys and girls are intended to be equally distributed.[4] For this reason, identifying peer effects in the classroom does not seem possible, and as described in Section 3.5, I will focus on identifying peer effects at the cohort level, i.e., for students entering 9th grade at the same schools.

The Danish schooling system is free of charge as it is financed by tax payers. About 88% of a cohort attend the public schools from entry to kindergarten, whereas 12% attend private schools.[5] Private schools are partially funded through a voucher system which is financed by tax payments. In 7th grade about 82% of a cohort attend public schools and 16% attend private schools.[6] Therefore, some mobility between private and public schools is observed, but the majority of students attend the same school with the same classmates throughout their compulsory education.

At the end of 9th grade, students take the 9th grade exit exam and combined with annual marks this gives the grade point average from compulsory schooling. The annual grades are given by the teacher in the specific subject, and the written exams are marked centrally, whereas the oral exams are marked by one local teacher and one teacher appointed by the Ministry of Education.

3.4 Data

The empirical analysis is based on administrative data hosted by Statistics Denmark and covers the entire Danish population. The school data consist of all Danish students enrolled in 9th grade in the years from 2007/2008 to 2010/2011, and which schools they attend. As a result, I

[3] The obligation to go to school starts August 1st in the calender year where the child turns 7, and stops July 31st after the child has received teaching on a regular basis for 9 years. Teaching in kindergarten does not count, Folkeskoleloven (2005).

[4] Some minor principles are also used, and they are based on equally distributing students with special needs, and thus creating a mixture of strong and weak students.

[5] From the Ministry of Children and Education: "Student numbers for compulsory schooling 2011/2012".

[6] The remaining 2% attend special needs schools.

know which school the student attended in 9th grade.[7] These are linked to exam grades at the end of 9th grade and register data on background characteristics based on yearly information from 1980 to 2010, such as parental employment, education and wage income.

It also contains information collected at birth, such as mother's (and father's) age at birth, birth weight, the length of the pregnancy, whether the child was born prematurely and extemely prematurely, if the mother smoked during pregnancy, if there were any complications at birth, gender and ethnicity. The data also contain information on the type of day care the child attended.

In combination with these data, four separate registers are linked to the students in order to examine whether they were exposed to a particular adverse life event making them extraordinary vulnerable or not. First, the data are linked to a register of deaths, which enables me to observe which children have at least one deceased parent, and at what age this event took place. Second, the Danish crime register is used to observe which parents show criminal behavior. The crime register consists of all charges, but in this analysis I focus on parents who are convicted of a criminal activity.[8] Third, from the health registers, information on mental diagnosis is collected and linked to each child and parent. The fourth and final register used is the cohabitation register, which contains information on the family structure. In this way information regarding parental cohabitation is linked to each child. For a complete list of variables, see Table 3.8 in Appendix 3.A.

Table 3.1: Sample Selection

Description	**No. of obs**	%
All students attending 9th grade in 2007-2010	212,864	100.00
Students with school outcomes in 2008-2011	211,443	99.33
Students with background characteristics	197,095	92.59
Subsample of siblings	46,356	21.78
Subsample of siblings attending the same school	35,050	16.47

An overview of the sample selection can be seen in Table 3.1. The data contain information on about 212,000 students attending 9th grade from 2007/2008 to 2010/2011. Of those, students where no school outcomes are observed are excluded. They are however not excluded from the peer group, as especially these students might influence other students. In addition, I exclude individuals where the background characteristics are missing. This leaves a total sample of 197,095 students.

[7]Unfortunately, the data do not contain information about Kindergarten enrollment or any other annual enrollments before 9th grade. The anaysis conducted in this paper is therefore limited to focus on enrollment in 9th grade.

[8]All traffic offences are excluded in order for the crime register to be internationally comparable. Around 90% of all convictions in the crime register are speeding or parking tickets, and these offences should not per se affect the child's behavior or be a serious family event affecting the child's behavior or learning ability.

In order to analyze peer effects for siblings, a subsample of siblings is used, and it contains 46,356 individuals. 35,050 attend the same school and 11,306 attend different schools.

A potential concern is if students change schools in response to the peer quality. But since the data only consist of students attending 9th grade this specific issue cannot be analyzed. Another data source is, however, available that makes it possible to investigate the pattern of how many students change schools in the period of 2007-2011. The data set consists of annual records of all individuals enrolled in the Danish compulsory school from 2007 and onwards. The data show that of the 304.265 children enrolled in kindergarten in the period 2007-2011, 35.421 children changed school at some point, yielding a total of 11,7% of students changing schools after entering kindergarten.[9]

3.4.1 Outcome variables

The outcomes considered are the normalized 9th grade exit exams in math and reading. A typical student starts in Kindergarten at age 6 and starts 1st grade at age 7, and compulsory schooling ends after 9th grade.[10] At the end of compulsory school, it is mandatory to attend the exit exams. However, a minor fraction of students do not take the exams, and for those the school outcomes are not observed. The reading outcome is based on the student being able to read and understand, and the math outcome is based on general math skills and problem solving.[11] These are of course short term outcomes, but they are nevertheless important for the students' later education and future carreers.[12]

It would be interesting to investigate the peer effects on late outcomes as well, but because the school data in Denmark is only available from 2007 and onwards, it is not feasible (yet) to undertake an investigation of such peer effects on longer-term school outcomes.

3.4.2 Own and peer characteristics

In order to estimate peer effects of specific peer variables, it is important to try to eliminate peer effects stemming from other factors. For that reason, I try to control for a wide range of background characteristics both for the child and for the peers. Tables 3.2, 3.3 and 3.4 show the summary statistics for the background characteristics for the child, parents and peers, respectively.

[9]This is a lower bound of the number of students changing schools. If instead the number of students who changed schools up until 9th grade was counted, the number would be 17.9%.

[10]In 2009, compulsory schooling changed from 9 to 10 years, now including Kindergarten, and changing the school starting age from 7 to 6. Folkeskoleloven (2009)

[11]In the Danish compulsory school system, grades are given both annually and at the exams. The annual grades are given by the teacher, whereas the exam test scores are an average of the teacher's assessment and an external examiner's assessment. For that reason, I focus on the exam grades as these are supposed to be more objective measures of the student's ability.

[12]See e.g. Joensen and Nielsen (2007; 2012).

Table 3.2: Descriptive statistics - Child characteristics

Description	**Mean**	**Std.dev.**
Child characteristics		
Boy	0.50	0.50
Native	0.94	0.23
No. of siblings	1.81	1.66
No. of younger siblings	0.89	1.32
Complications at birth	0.01	0.11
Born prematurely	0.10	0.29
Born extremely prematurely	0.00	0.03
Mental diagnosis	0.01	0.08
Birth weight, grams	3312.55	932.96
Day care		
Private day care	0.01	0.10
Centralized day care	0.79	0.41
No day care	0.09	0.29
Not registered	0.11	0.31
Own vulnerability measures at age 5		
Criminal parent	0.08	0.27
Deceased parent	0.01	0.07
Parent with mental diagnosis	0.07	0.25
Divorced parents	0.13	0.33
Own vulnerability measures, 9th grade		
Criminal parent	0.11	0.31
Deceased parent	0.02	0.16
Parent with mental diagnosis	0.14	0.35
Divorced parents	0.56	0.84
Outcomes (normalized)		
Reading	0.03	1.00
Math	0.05	0.93
GPA	0.03	0.80
Number of observations	197,095	

The variables are predetermined as they are measured before the school outcomes. The parental background characteristics are all determined at school entry. The same is the case for the child and peer characteristics, except for the vulnerability measures which are measured at entry in 9th grade.

In this analysis, peers are defined as the group of students who attend the same school and are in your grade at the beginning of the school year in 9th grade. In general, peers in 9th grade would also be peers from the beginning of Kindergarten except for those changing schools and repeating grades. The effects measured at entry to 9th grade will therefore be a cumulative effect of peers throughout compulsory school. Also, the peer characteristics are defined *before* excluding observations with missing information on school outcomes or parental background characteristics.

Table 3.5 shows the descriptives for the analysis on the endogenous school starting age. The majority of children attend school in the year they are expected to, but some variation in school starting age is nevertheless present, suggesting that the instrumental variables strategy proposed in Section 3.5.1 could potentially solve an endogeneity problem in school starting age.

Table 3.3: Descriptive statistics - Parental characteristics

	Mean	**Std.dev.**
Mother's characteristics		
Native	0.87	0.33
Mental dignosis	0.03	0.18
Log wage income	9.36	4.94
On leave	0.03	0.16
Self-employed	0.03	0.17
Manager	0.29	0.45
Regular employment	0.42	0.49
Unemployed	0.05	0.23
Receiving unemployment benefits	0.06	0.24
Unfit for work	0.11	0.31
Age at child's birth	29.02	4.70
Basic education	0.23	0.42
High school	0.08	0.28
Vocational	0.37	0.48
Short-cycle	0.04	0.20
Medium cycle	0.18	0.38
Long-cycle	0.07	0.25
Missing info	0.03	0.17
Father's characteristics		
Native	0.87	0.34
Mental dignosis	0.04	0.18
Log wage income	10.07	4.88
On leave	0.00	0.06
Self-employed	0.09	0.28
Manager	0.30	0.46
Regular employment	0.47	0.50
Unemployed	0.03	0.17
Receiving unemployment benefits	0.02	0.14
Unfit for work	0.05	0.22
Age at child's birth	31.81	5.68
Basic education	0.21	0.41
High school	0.06	0.23
Vocational	0.40	0.49
Short-cycle	0.07	0.25
Medium cycle	0.10	0.31
Long-cycle	0.10	0.30
Missing info	0.06	0.23

Which way the mechanisms work in peer effects are not clear, and for that reason, this is an attempt to analyze a particular aspect of the mechanisms working through vulnerable children, defined as children exposed to an event that might change the home environment in a drastic manner.

3.4.3 Vulnerable Peers

The four vulnerability measures considered in this analysis are meant to be examples of events in the home environment that could possibly affect either a student's behavior in school or create temporary or permanent learning disabilities for the child. In this sense, peers would be affected either through more commotion in school or less teacher attention. These four events are not meant to be exhaustive of events which could possibly create a "bad apple", but merely an attempt to capture events that *might* affect a child and thereby peers in an adverse way.

Table 3.4: Descriptive statistics - peer characteristics

	Mean	**Std.dev.**
Average peer vulnerability measures at age 5		
Criminal parent	0.08	0.06
Deceased parent	0.01	0.01
Parent with mental diagnosis	0.07	0.05
Divorced parents	0.13	0.07
Average peer vulnerability measures, 9th grade		
Criminal parent	0.11	0.06
Deceased parent	0.02	0.03
Parent with mental diagnosis	0.03	0.03
Divorced parents	0.39	0.11
Average peer Characteristics		
Boy	0.50	0.10
Non-native	0.32	0.28
No. of siblings	0.67	0.26
No. of younger siblings	0.41	0.18
Complications at birth	0.01	0.02
Born prematurely	0.06	0.05
Born extremely prematurely	0.00	0.00
Day care		
Private day care	0.01	0.02
Centralized day care	0.57	0.31
No day care	0.07	0.13
Not registered	0.08	0.20

Table 3.5: Descriptive statistics - School starting age

	Mean	**Std.dev.**
School starting age	6.21	0.37
Expected school starting age	6.09	0.28
Fraction of students starting on time	0.89	0.32
Number of schools	702	
Number of observations	197,095	

The first vulnerability measure (or adverse event) is the fraction of peers where one of the parents has committed any form of crime up until the child's entry in 9th grade. The second measure is the fraction of peers who have experienced the loss of a parent up until their entry in 9th grade. The third measure is the fraction of peers where one of the parents is diagnosed with a mental disorder. The fourth and final measure is the fraction of peers where the parents have been divorced (or not cohabiting at some point in time) before the child's entry in 9th grade. For all the vulnerability measures it applies that they in some way might affect the child's behavior or learning abilities for a period in time, which then again could translate into a negative externality on the peers.

The timing of the vulnerability may be important as it could show how the length of exposure matters for the negative effects. The analysis focuses on vulnerability measured up until 9th grade. On one hand, one could argue that recent events ought to have less impact on school outcomes when aquiring knowledge is considered a cumulative process. On the other hand, recent events might have a higher impact on learning and school outcomes when they happen later on. Whether the effects are larger when the adverse life events happen earlier or later in life is considered in more detail in Section 3.6.4, where vulnerability is defined before school entry.

3.4.4 Variation across cohorts

In order to indirectly test the identification strategy of using *some* variation across cohorts within schools in the proposed vulnerability measures, I regress each of the measures on the list of covariates including school fixed effects and cohort indicators. The four resulting R^2 give an idea of whether the variation in the vulnerability measures is sufficient when controlling for a range of covariates.

The total variation in the four measures (or events) that can be explained by the remaining control variables indicate whether there is some variation left, implying that the identification strategy cannot on this basis be rejected. As the R^2 ranges from 3.86% (for average peer deceased parents) to 10.20% (for average peer divorced parents), 18.09% (for peer criminal parents) and 20.74% (for average peer mentally diagnosed parents), the variation explained by the included controls still leaves room for using the remaining variation for identification.

3.5 Empirical Analysis

3.5.1 Method

I estimate peer effects by the following estimation equation:

$$y_{isc} = \alpha + \beta X_{isc} + \gamma \frac{\sum_{i \neq k} T_{ksc}}{n_{sc} - 1} + \lambda_s + \sum_{j=2007}^{2010} \delta_j D_j + \varepsilon_{isc} \tag{3.1}$$

where y measures the 9th grade outcomes for individual i, at school s, and in cohort c. X_{isc} is a vector of covariates measured in the year the child turns 5. $\frac{\sum_{i \neq k} T_{ksc}}{n_{sc}-1}$ is the proportion of peers who are considered vulnerable. λ_s is a school fixed effect and D_j is cohort dummies. Thus, the parameter of interest is γ. A cohort is defined as all children who start in 9th grade in the same year.

The vulnerability measure, T, is an indicator for some form of problematic home environment which makes the child exposed to the treatment more vulnerable than the remaining children in class. Four measures are considered separately, namely, whether or not the child has i) divorced parents, ii) at least one dead parent, iii) at least one parent diagnosed with a mental illness, or iv) at least one parent who committed a crime. The proportion of vulnerable peers is measured at the time of entering 9th grade.

The observable characteristics, X, include own vulnerability status both at age 5 and at entry to 9th grade. Also, a wide range of child and parental background characteristics are included, which can be seen in Table 3.8 in Appendix 3.A.

The optimal estimation strategy would focus on peer effects in the classroom. This strategy is however only valid in case of random assignment to classrooms within school and within cohort. Since this does not seem to be the case, this strategy will in general yield biased estimates. For this reason, the regression is estimated within school at the cohort level, essentially assuming random assignment within schools but between cohorts. If this is the case, eq. (3.1) generates unbiased estimates of the peer effect, γ.

It is difficult to determine if a vulnerable child causes the classmates to misbehave or if it is the other way around. However, as long as a student's peers do not cause the vulnerability, i.e., i) the parents being divorced, ii) a dead parent, iii) mentally diagnosed parent and iv) a criminal parent, this strategy circumvents the reflection problem. This is exactly a key advantage of the vulnerable children measures, as the events in the child's family are not likely to be caused by a child's school peers, and if the vulnerable child's family events are in fact exogenous, the indicators overcome the reflection problem. In general, this seems to be the case, but there might be a problem with the fraction of divorced parents in the peer group, as the parents in the peer group might influence the child's own parents.

Another problem is the tendency of vulnerable children to self-select into the same schools as other vulnerable children. This implies that the proportion of vulnerable peers varies across schools and therefore varies with the characteristics of the population who attend the schools. Thus, the variability of the proportion of vulnerable peers across cohorts would essentially be correlated with the characteristics of the children who attend the schools. This could potentially bias the estimates. Therefore, it is important to consider the variation *within* schools to control for the selection into schools. So the problem of self-selection is solved by exploiting the natural variation in cohort composition across time within schools, which implies using school fixed effects.

An important issue is to control for the number of students in the same school in the same grade to disentangle the effect stemming from school-grade size and peer variation, as these are closely related. Therefore, the number of students in the same cohort in the same school is included in the conditioning set.

Some potential threats to the identification strategy might arise. First, if some schools (e.g. high resource schools) are better at avoiding enrollment of more vulnerable children than other schools, the estimates would be biased. This could imply falsely concluding that vulnerable children harm the school outcomes of the remaining class, even though the effect is purely driven by systematic differences in the administrative personnel of the schools. Another possible threat is if some parents (e.g. high resource parents) are more likely to delay or hurry enrollment into schools for their child in case of a particular cohort having a high proportion of vulnerable children. Thus, parents must not observe the specific variation in the proportion of treated individuals before choosing which cohort to enroll their child into. Otherwise, the estimates would also be biased. These potential threats will be dealt with as part of the robustness checks, see Section 3.6.

Parameterization of School Effects

One could of course discuss an alternative parameterization of school effects that does not include school fixed effects, but instead includes school averages across cohorts of the control variables in order to capture the selection of students living in the same neighborhood and going to the same schools. Thus, if the area is characterized by, say, high unemployment, this will be captured by this alternative specification, as the estimates would take the school average of parental unemployment into account. This parameterization is also used in Black et al. (forthcoming).

Sibling Fixed Effects

In the former analysis, a potential concern is that the effect captures differences in the familiy resources devoted to the children. Thus, some correlation between the peers and a student's family characteristics might still be present. To circumvent this issue, it is natural to look at the subgroup of siblings, where the family background is the same. One problem arises if the children with relatively higher ability are systematically placed in higher ability peer groups. This can happen either if parents were to strategically affect the school starting age or if siblings are placed in different schools. These issues are further investigated below.

In order to examine the peer effects within families, the sample is restricted to siblings. To examine the effects for siblings where the same school resources are devoted to the students, the sample is further restricted to siblings attending the same schools. The exogenous variation used in this subsample is the variation in the proportion of vulnerable peers across cohorts within schools within families. However, as families might react to differences in peer group quality by moving one child to another school, it might be the case that siblings attending the same school are those, where the parents regarded the impact of the peers as unimportant. Thus, in order to test if this is the case, the analysis of siblings is also carried out for siblings attending different schools.

Timing of Adverse Family Events

Up until now, the adverse family events in the peer group are only considered by entry into 9th grade. But if selection into schools occurs, including these adverse family events before school entry, say at age 5, would capture the within school variation before starting in school. Thus, if selection happens before entering school, including the peer adverse family events at age 5, i.e., before starting in school, would capture this effect.

One could also argue that the adverse family events would create the most vulnerable peers, when the events happen during the school years and not when the child is younger than 5 years old. For example, it could be that a parental divorce affects the child, and thereby the peer group, more in the first years after the divorce.

However, the opposite could also be the case. For example, it could be that a deceased parent affects the child, and thereby the peer group, more in the 9th grade test scores if learning is a cumulative process. Therefore, a way to investigate whether there are effects in the longer-run of these family events is to include the family events in the peer group at age 5 as well as in 9th grade. This implies that the interpretation of the estimates would be in changes from age 5 to 9th grade, and therefore the estimates are not directly comparable to the estimates presented in the former analysis.

Predicted School Starting Age

If the school starting age is endogenous, then the proportion of vulnerable peers is biased. In order to investigate whether vulnerables peers affect school outcomes, I construct an instrument based on an enrollment rule in the Danish compulsory schooling system. I use the rule that children should enroll in compulsory school in the summer in the year the child turns 6.[13] In this way I instrument the actual proportion of vulnerable peers with the predicted proportion of vulnerable peers (i.e., the proportion of peers given that all children enrolled according to the rule).

A potential concern is if the school choice is endogenous in response to the peer quality. In order to investigate this issue, the municipality of residence at time of entry into 9th grade is used, and thereby using the variation in municipality to identify the peer effect.

3.5.2 Results

The first outcome is the exit exam in 9th grade Reading, and the results are presented in Table 3.6. Looking at the peer variables of interest, namely the vulnerability measures, none of them are significant for the Reading test score in 9th grade. On the other hand, looking at the individual level vulnerability measures at the entry to 9th grade, they show as expected a negative relationship between experiencing this family event and Reading. These estimates represent the change in own vulnerability status from the age of 5 to the beginning of 9th grade. All the 9th grade vulnerability measures are, in general, significantly negative, except for the

[13] A similar instrument is used in Black et al. (forthcoming).

measure of a deceased parent. Not surprisingly, coming from a divorced family, a family with a mentally diagnosed parent, or a family with a criminal parent lowers the Reading test score by .08, .05, and .10 standard deviations, respectively. Thus, it is what happens between the age of 5 and the beginning of 9th grade that matters for the student's own Reading test score, and not what happens in the period before entering school.

Turning to the exit exam in 9th grade Math, presented in Table 3.7, the results show that, taking the student's vulnerability into account at age 5, the individual level vulnerability at the beginning of 9th grade affect the Math test score negatively by .11, .09, .02 and .11 standard deviations for divorced parents, having a mentally diagnosed parent, having a deceased parent and having a criminal parent, respectively, but the estimate for a deceased parent is insignificant. Again, these effects are not surprising. Now, looking at the peer variables, a pattern emerges. For the fraction of peers with a deceased parent and with a mentaly diagnosed parent, there are no significant peer effects. However, the two remaining peer vulnerability variables, i.e., the fraction of peers with divorced parents and the fraction of peers with a criminal parent, influence the Math test score negatively with estimated effects of -.09 and -.14 standard deviations, respectively. To get a sense of the magnitude, in a school with 50 students in the same grade the students will on average experience a drop of .014 standard deviations in the math test score, when the number of peers with a criminal parent increases from, say, 5 to 10.

Thus, peer effects are present in Math but not in Reading,[14] which could be because Math is a course demanding more study effort than Reading, because you need specific knowledge to solve mathematical problems, whereas Reading is relatively easier.

Although these adverse family events cannot easily be altered, it seems like having more focus on students who experiences these family events, could potentially mitigate the adverse effects both on the student and on the student's peers.

3.6 Robustness Checks

3.6.1 Inclusion of controls

An important issue is to analyze how much the inclusion of covariates matters for the effects. For this reason, estimates of the four family events on the test score outcomes where the covariates have been included gradually are shown in Tables 3.10, 3.11, 3.12, and 3.13 for Reading and 3.14, 3.15, 3.16, and 3.17 for Math in Appendix 3.A, respectively.

[14] Obviously, it could be the case that the vulnerability measures are mutually dependent, and that only one of them in reality drives the results. Therefore, I have also included all four vulnerability measures simultaneously in estimations for Reading and Math. The results are shown in Table 3.9 in Appendix 3.A, and they are consistent with the main results presented, i.e., no significant effects in Reading and peers with divorced or criminal parents affect the Math test score negatively, and therefore, it is not likely that only one of the vulnerability measures drives the results.

Table 3.6: Estimates of Vulnerability on Reading (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0110	0.0110	0.0110	0.0109
	(0.0151)	(0.0151)	(0.0151)	(0.0151)
Deceased parent, age 5	0.0006	0.0006	0.0005	0.0006
	(0.0396)	(0.0396)	(0.0396)	(0.0396)
Mentally Diagnosed parent, age 5	0.0267	0.0267	0.0270	0.0267
	(0.0425)	(0.0425)	(0.0425)	(0.0425)
Divorced parents, age 5	0.0188**	0.0188**	0.0188**	0.0188**
	(0.0076)	(0.0076)	(0.0076)	(0.0076)
Criminal parent, 9th grade	-0.1052***	-0.1053***	-0.1053***	-0.1047***
	(0.0135)	(0.0135)	(0.0135)	(0.0135)
Deceased parent, 9th grade	-0.0196	-0.0196	-0.0209	-0.0195
	(0.0158)	(0.0158)	(0.0159)	(0.0158)
Mentally Diagnosed parent, 9th grade	-0.0509***	-0.0510***	-0.0509***	-0.0509***
	(0.0091)	(0.0091)	(0.0091)	(0.0091)
Divorced parents, 9th grade	-0.0832***	-0.0831***	-0.0831***	-0.0831***
	(0.0054)	(0.0054)	(0.0054)	(0.0054)
Peer divorced parents	-0.0064			
	(0.0396)			
Peer mentally diagnosed parent		-0.0071		
		(0.0551)		
Peer deceased parent			-0.1436	
			(0.1186)	
Peer criminal parent				0.0496
				(0.0611)
N	190179	190179	190179	190179
R^2	0.138	0.138	0.138	0.138

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results for both Reading and Math show that especially the inclusion of the child's basic charcteristics and parental controls before school entry lowers the estimates in magnitude. However, in most cases it is the inclusion of school fixed effects that turn the estimates insignificant. This is not due to higher standard errors but due to the estimates being of lower magnitude. Thus, it seems like selection is present before school entry and that this selection can be handled partly by including covariates of peer variables before school entry and partly by using school fixed effects.

3.6.2 Results by Gender

An interesting issue is whether boys and girls are affected differently by the presence of vulnerable peers. This subsection therefore presents the results based on separate analyses made for boys and girls. The results showing separate effects for girls and boys are presented in Tables 3.18 and 3.19 for Reading and 3.20 and 3.21 for Math in Appendix 3.A, respectively. The results for Reading show the same pattern as in the full sample for both boys and girls: No significant peer effects are present. However, turning to the results in Math, the picture is changed. They show that the overall effects mask a surprising gender difference. For boys, the

Table 3.7: Estimates of Vulnerability on Math (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0080	0.0080	0.0079	0.0081
	(0.0140)	(0.0140)	(0.0140)	(0.0140)
Deceased parent, age 5	0.0138	0.0138	0.0137	0.0139
	(0.0371)	(0.0370)	(0.0370)	(0.0370)
Mentally Diagnosed parent, age 5	0.0745*	0.0745*	0.0746*	0.0742*
	(0.0384)	(0.0384)	(0.0384)	(0.0384)
Divorced parents, age 5	-0.0109*	-0.0109*	-0.0109*	-0.0109*
	(0.0065)	(0.0065)	(0.0065)	(0.0065)
Criminal parent, 9th grade	-0.1108***	-0.1108***	-0.1107***	-0.1122***
	(0.0130)	(0.0130)	(0.0130)	(0.0130)
Deceased parent, 9th grade	-0.0176	-0.0174	-0.0189	-0.0174
	(0.0146)	(0.0146)	(0.0147)	(0.0146)
Mentally Diagnosed parent, 9th grade	-0.0942***	-0.0946***	-0.0942***	-0.0942***
	(0.0079)	(0.0080)	(0.0079)	(0.0079)
Divorced parents, 9th grade	-0.1068***	-0.1062***	-0.1062***	-0.1062***
	(0.0047)	(0.0047)	(0.0047)	(0.0047)
Peer divorced parents	-0.0882**			
	(0.0347)			
Peer mentally diagnosed parent		-0.0456		
		(0.0492)		
Peer deceased parent			-0.1655	
			(0.1060)	
Peer criminal parent				-0.1431***
				(0.0508)
N	190372	190373	190373	190373
R^2	0.158	0.158	0.158	0.158

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

effects are insignificant, whereas for girls, the negative peer effects in the fraction of children in the peer group having divorced families and having criminal parents correspond to the joint effect found above. Thus, the joint effects are driven by girls who are negatively affected by the fraction of peers from divorced families and the fraction of peers with criminal parents. It seems like girls are more easily affected by their vulnerable peers compared to boys.

3.6.3 Sibling Fixed Effect

If family resources are important for the learning outcomes of students and for the sensitivity to low quality peers, it would be important to perform the analysis on a subsample of students who are siblings, since this would eliminate the effects coming from families with different resources.

The analysis is first performed for siblings attending the same schools, since school resources might also influence the students. The results of the sibling fixed effects model for siblings attending the same schools are presented in Tables 3.22 and 3.23 for Reading and Math in Appendix 3.A, respectively. As this analysis requires at least two children from the same family as well as for these siblings to attend the same school, the sample size is remarkably reduced, leading to much higher standard errors. In Reading, no effects are present as was the case using the full sample. In Math the results turn insignificant, and in some cases the effects (though insignificant) even turn signs. One reason is that the standard errors are higher, but the estimates are also of larger magnitude. Another reason why these results do not match what is found in the main analysis could be that parents react to differences in the peer group quality by moving one child to a different school. It could be that parents who have their children attend the same school correctly regard the impact of the peers as unimportant.

For this reason, the analysis is also performed for siblings attending different schools. The results are shown in Tables 3.24 and 3.25 in Appendix 3.A for Reading and Math, respectively. Overall, the results are in accordance with the main results. For Reading the results are negative but insignificant, but for Math the fraction of divorced parents and deceased parents in the peer group both affect the test score negatively. The fact that the fraction of divorced parents in the peer group negatively affects the Math outcome is also found in the main analysis, indicating that the main results seem reasonable. The analysis of siblings shows that parents do react to the possibility of a low quality peer group.

3.6.4 Timing of the family events

The timing of the adverse family events might be important for later school outcomes. As mentioned above, the adverse family events might have immediate impact on school outcomes, or the effect might be larger in magnitude as the negative effects accumulate over time.

The results are presented in Tables 3.26 and 3.27 for Reading and Math in Appendix 3.A, respectively, and they take selection before school start into account. The result shows that there still are no significant effects on the Reading test score. However, the picture is different looking at the Math test score. Here we see that controlling for divorced parents in the peer group at age 5 does not change the fact that peers are negatively affected by a higher fraction of parents divorced in the peer group, and the estimated coefficient is not changed significantly from the results presented in Table 3.7. Thus, at least for the event of a divorce, it is not selection into schools that drives the result. It it rather what happens during the school years that affect the Math test score. But looking at the fraction of peers exposed to criminal parents, it does seem like selection into schools is the driving force. Now, the former negative effect from Table 3.7 is no longer present, and it seems like the main negative coefficient (although insignificant) is the coefficient on the peer group composition at age 5.

Thus, if for the most part, parents are divorced when the child is in the school age, we would see this result. But if this is indeed the case, then it could be that a student's own parents are affected by the fraction of divorced parents in the peer group. For this reason the results for the fraction of divorced parents in the peer group should be interpreted carefully.

3.6.5 Instrumenting school starting age

As mentioned above a potential concern is that parents manipulate their child's school starting age in response to observing low peer quality in the predicted peer group. Using predicted school starting age as an instrument for actual school starting age this issue is considered in more detail.

However, in order to be able to perform the instrumental variables analysis, it is neccesary to model the school specific effects as school averages of the control variables and include those instead of school fixed effects. It is simply not feasible to include about 700 school specific dummies when the possible endogeneity of the school starting age is considered. Thus, Tables 3.28 and 3.29 in Appendix 3.A show OLS estmates without school fixed effects (odd columns) and estimates including school averages (even columns) for reading and math test scores, respectively.

The estimates show that controlling for school averages lowers the estimates of the peer variables of interest, however, not to the same extent as the school fixed effects do, cf. Table 3.6. Thus, it seems as if including school averages of the control variables do indeed capture a fraction of the selection into schools and neighborhoods, but they do not do as good a job as the school fixed effects.

Despite the fact that the school averages do not capture as much of the school specific effects as the school fixed effects, this parametarization is used in the instrumental variables approach below.

In order for a student to enroll in a cohort with a relative low proportion of vulnerable peers, parents may choose to enroll their child later than what the Danish administrative school rules say. The instumental variables approach therefore uses predicted school starting age as an instrument for actual school starting age. As seen in Table 3.5, 89% of the students enroll when they were supposed to, indicating that the instrument used is strong. In addition to the predicted school starting age, this strategy also uses birth year as instrument for enrollment year, predicted peer quality as instrument for actual peer quality. Predicted peer quality is here defined as the parental background and vulnerability of the peers a child were to have, had the child enrolled in the year he was supposed to. As an example, the t-statistic in the first stage for predicted school starting age on actual school starting age for criminal parents on Math is 146.01. In fact, the F-statistics of the joint significance in the first stages ranges from 8.43 to 14,310.47, which shows that the instruments are quite strong.

The results using an instrument for school starting age are shown in Tables 3.30 and 3.31 in Appendix 3.A for Reading and Math, respectively. The estimates are comparable to those presented in Tables 3.28 and 3.29 for Reading and Math, respectively.

The results for Reading show that the proportion of divorced parents in the peer group lowers the Reading test score by .16 standard deviations, whereas the estimate was .07 standard deviations in the results using school averages of the control variables but no instrument. The estimate of the proportion of peers with a deceased parent on the Reading test score shows no significant change using the instrument, which corresponds to the accident of losing a parent being an unexpected event not foreseeable by the parents of the other students. However, the remaining estimates of the proportion of peers exposed to a family event, namely mentally diagnosed parents and criminal parents, both exhibit significantly lower estimates. The proportion of criminal peers in the peer group is still insignificant, and the proportion of mentally diagnosed parents in the peer group turns less significant (now only significant at the 10%-level).

The results for Math show the same pattern as above, but here the estimates keep being significant at the 1%-level, except for the proportion of peers with a deceased parent, where the estimate is less negative and turns insignificant.

Thus, in most cases the OLS estimates (with school averages) are less negative than the IV estimates, which indicates that there are positive selection effects. One explanation could be that high resource families postpone or move forward their child's school start in order to mitigate (some of) the adverse peer effects.

3.6.6 Municipality level analysis

Since there might be identification issues using the variation between cohorts within schools, this section defines the peers at the municipality level instead. Table 3.32 and 3.33 show the result of the analysis at the municipality level for Reading and Math, respectively. The results are comparable to the main results presented in Table 3.6 and 3.7, respectively. The results show that there are no significant peer effects on the Math test score, whereas there is a very large (but imprecisely estimated) negative effect from having peers with deceased parents on the Reading test score. Furthermore, the effect of having peers with divorced parents on Reading has changed sign compared to the main results.

Thus, it is not the peers at the municipality level that determines how well you do in school. It is rather the peers in the school. It therefore seems reasonable that the negative peer effects found in the main analysis vanish, when the peers are defined at the municipality level.

3.7 Conclusion

In this paper, I have investigated how vulnerable peers seem to affect the students at their cohort by using the variation across cohorts within schools in the proportion of vulnerable peers. Four vulnerability measures of peers have been used, and they include the proportion of peers with a deceased parent, the proportion of peers with a parent diagnosed with a mental disorder, the proportion of peers with a criminal parent, and the proportion of peers with divorced parents.

Controlling for a wide range of background characteristics and selection to schools, the main results show that the proportion of vulnerable children affects the Math test score negatively by about .1 standard deviation. However, no effects seem to be present for Reading test scores.

The overall effects show some interesting heterogenous effects across gender. The results are driven by girls, since they are harmed by the adverse life events of the peers, whereas boys are unaffected.

By conducting a range of robustness checks, the results suggest that it is important to take selection to schools before school entry into account. Also, using predicted school starting age as an instrument for actual school starting age yields estimates of higher magnitude, indicating that high resource familes are more capable in postponing school entry of their children if they observe a relatively unfavorable cohort in they year their child was supposed to enroll.

Thus, even though the events these vulnerable children are exposed to cannot easily be altered, it seems like early initiatives increasing the focus on vulnerable children could potentially mitigate the adverse effects both on the student and on the student's peers.

The outcomes considered in this analysis are all short term outcomes. Thus, there is scope for investigating whether the small effects of vulnerable peers on Math test scores found in this analysis tramsmit into long-run outcomes, or whether the adverse effects vanish after compulsory schooling.

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Appendices

3.A Tables

Table 3.8: List of included covariates

Variable name	Description
Basic characteristics	
Boy	Boy (0/1)
Native	Native Dane (0/1)
Birth weight (g)	The child's birth weight in grams
Length of pregnancy	Length of pregnancy in weeks
Born prematurely	The child is born before the 37th gestational week (0/1)
Born extremely prematurely	The child is born before the 28th gestational week (0/1)
Complications at delivery	Complications during delivery of the child based on an APGAR score of 7 or above. (0/1)
Number of younger siblings	Number of younger siblings when the child is 5 years old, including half-siblings.
Number of siblings	Number of siblings when the child is 5 years old, including half-siblings.
Mental diagnosis	The child is diagnosed with a mental or behavioral disorder at the age of 5. (0/1)
Indicator for the year entering 9th grade	Entering 9th grade in the specific year (0/1)
Day care	
Not registered day care	Day care is not registered (0/1)
Centralized day care	The child is enrolled in centerbased day care at the age of 4 (0/1)
No outside home day care	The child is taken care of in the home by either aprents or grandparents (0/1)
Private day care	The child is enrolled in private family day care (0/1)
School starting age	The age of the child when starting in school
Number of student in a cohort	number of students entering 9th grade at the same school in the same year

Table 3.8: List of included covariates – continued from previous page

Variable name	Description
Mother and Father characteristics	
Age at birth	The age when they had their child
Native	Native Dane (0/1)
Mental diagnosis	Diagnosed with a mental disorder
Log wage income	Wage income in logs
Employment status	
On leave	On leave from job (0/1)
Self-employed	Self-employed (0/1)
Manager	Manager (0/1)
Regular employment	Regular employment (0/1)
Unemployed	Unemployed (0/1)
Receiving unemployment benefits	Receiving unemployment benefits (0/1)
Unfit for work	Unfit for work (0/1)
Highest attained education when the child is 5 years old	
Basic education	Compulsory schooling (0/1)
High school	High school (0/1)
Vocational	Vocational (0/1)
Short-cycle	Short-cycle (0/1)
Medium cycle	Medium cycle (0/1)
Long-cycle	Long-cycle (0/1)
Missing info	Missing info (0/1)
Vulnerability measures	
Criminal parent	The child has at least one parent who ever committed a crime (0/1)
Deceased parent	The child has at least one deceased parent (0/1)
Parent with mental diagnosis	The child has at least one parent (0/1) with a mental diagnosis
Divorced parents	The parents are divorced (0/1)
Outcomes	
Reading (exit exam)	Reading exam standardized test score
Math (exit exam)	Math exam standardized test score

All basic and parental characteristics are included as peer variables as well.

Table 3.9: Estimates of All Vulnerability Measures on School Outcomes

	(1) Reading	(2) Math
Criminal parent, age 5	0.0109	0.0081
	(0.0151)	(0.0140)
Deceased parent, age 5	0.0004	0.0138
	(0.0396)	(0.0370)
Mentally Diagnosed parent, age 5	0.0270	0.0748*
	(0.0425)	(0.0384)
Divorced parents, age 5	0.0188**	-0.0109*
	(0.0076)	(0.0065)
Criminal parent, 9th grade	-0.1047***	-0.1121***
	(0.0135)	(0.0130)
Deceased parent, 9th grade	-0.0209	-0.0189
	(0.0159)	(0.0147)
Mentally Diagnosed parent, 9th grade	-0.0510***	-0.0943***
	(0.0091)	(0.0080)
Divorced parents, 9th grade	-0.0832***	-0.1068***
	(0.0054)	(0.0047)
Peer divorced parents	-0.0062	-0.0727**
	(0.0412)	(0.0356)
Peer criminal parent	0.0559	-0.1240**
	(0.0619)	(0.0515)
Peer mentally diagnosed parent	-0.0074	-0.0064
	(0.0562)	(0.0500)
Peer deceased parent	-0.1459	-0.1332
	(0.1201)	(0.1071)
N	190179	190372
R^2	0.138	0.158

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.10: Estimates of Divorced parents on Reading (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer divorced parents	-0.3270***	-0.1506***	-0.0744**	-0.0584*	0.0025	-0.0064
	(0.0427)	(0.0381)	(0.0313)	(0.0323)	(0.0397)	(0.0396)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0013	0.0703	0.1559	0.1565	0.1378	0.1381
N	193520	192775	190184	190179	190179	190179

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.11: Estimates of Mentally Diagnosed parents on Reading (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer mentally diagnosed parents	-0.9189***	-0.5898***	-0.1221***	-0.1110**	-0.0256	-0.0071
	(0.0586)	(0.0538)	(0.0460)	(0.0476)	(0.0546)	(0.0551)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0046	0.0718	0.1559	0.1565	0.1378	0.1381
N	193520	192775	190184	190179	190179	190179

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12: Estimates of Deceased parents on Reading (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer deceased parents	-0.8367***	-0.5273***	-0.1997*	-0.1585	-0.1411	-0.1436
	(0.1269)	(0.1170)	(0.1058)	(0.1075)	(0.1190)	(0.1186)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0006	0.0702	0.1559	0.1565	0.1378	0.1381
N	193520	192775	190184	190179	190179	190179

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.13: Estimates of Criminal parents on Reading (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer criminal parents	-0.9520***	-0.7132***	-0.0992*	-0.0425	0.0489	0.0496
	(0.0663)	(0.0606)	(0.0516)	(0.0528)	(0.0610)	(0.0611)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0037	0.0720	0.1559	0.1564	0.1378	0.1381
N	193520	192775	190184	190179	190179	190179

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.14: Estimates of Divorced parents on Math (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer divorced parents	-1.0212***	-0.7354***	-0.6245***	-0.4738***	-0.0808**	-0.0882**
	(0.0565)	(0.0475)	(0.0377)	(0.0370)	(0.0347)	(0.0347)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0144	0.0964	0.2077	0.2117	0.1575	0.1578
N	193720	192969	190377	190372	190372	190372

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.15: Estimates of Mentally Diagnosed parents on Math (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer mentally diagnosed parents	-1.7829***	-1.2495***	-0.7171***	-0.3784***	-0.0591	-0.0456
	(0.0774)	(0.0641)	(0.0527)	(0.0523)	(0.0493)	(0.0492)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0198	0.0984	0.2055	0.2099	0.1574	0.1577
N	193721	192970	190378	190373	190373	190373

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.16: Estimates of Deceased parents on Math (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer deceased parents	-1.5369***	-0.9975***	-0.6148***	-0.2929**	-0.1636	-0.1655
	(0.1758)	(0.1462)	(0.1247)	(0.1201)	(0.1061)	(0.1060)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0022	0.0901	0.2029	0.2093	0.1574	0.1577
N	193721	192970	190378	190373	190373	190373

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.17: Estimates of Criminal parents on Math (exit exam) – gradually including covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Peer criminal parents	-1.6309***	-1.2511***	-0.6451***	-0.4117***	-0.1410***	-0.1431***
	(0.0911)	(0.0753)	(0.0620)	(0.0579)	(0.0509)	(0.0508)
Basic controls	No	Yes	Yes	Yes	Yes	Yes
Parental controls	No	No	Yes	Yes	Yes	Yes
Peer variables at age 5	No	No	No	Yes	Yes	Yes
School FE	No	No	No	No	Yes	Yes
Cohort dummies	No	No	No	No	No	Yes
R^2	0.0126	0.0963	0.2044	0.2098	0.1575	0.1578
N	193721	192970	190378	190373	190373	190373

Standard errors in parentheses
Standard errors are clustered at the school level
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.18: Estimates of Vulnerability on Reading (exit exam) for Girls

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0111	0.0111	0.0111	0.0111
	(0.0217)	(0.0217)	(0.0217)	(0.0217)
Deceased parent, age 5	0.0297	0.0298	0.0298	0.0298
	(0.0550)	(0.0550)	(0.0550)	(0.0550)
Mentally Diagnosed parent, age 5	-0.0028	-0.0029	-0.0029	-0.0030
	(0.0563)	(0.0563)	(0.0563)	(0.0563)
Divorced parents, age 5	0.0288***	0.0288***	0.0288***	0.0288***
	(0.0108)	(0.0108)	(0.0108)	(0.0108)
Criminal parent, 9th grade	-0.1062***	-0.1062***	-0.1062***	-0.1063***
	(0.0193)	(0.0193)	(0.0193)	(0.0193)
Deceased parent, 9th grade	-0.0248	-0.0247	-0.0248	-0.0247
	(0.0217)	(0.0218)	(0.0218)	(0.0217)
Mentally Diagnosed parent, 9th grade	-0.0734***	-0.0735***	-0.0734***	-0.0734***
	(0.0125)	(0.0126)	(0.0126)	(0.0126)
Divorced parents, 9th grade	-0.0846***	-0.0844***	-0.0844***	-0.0844***
	(0.0076)	(0.0076)	(0.0076)	(0.0076)
Peer divorced parents	-0.0210			
	(0.0500)			
Peer mentally diagnosed parent		-0.0073		
		(0.0670)		
Peer deceased parent			-0.0118	
			(0.1495)	
Peer criminal parent				-0.0104
				(0.0775)
N	94939	94939	94939	94939
R^2	0.113	0.113	0.113	0.113

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.19: Estimates of Vulnerability on Reading (exit exam) for Boys

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0180	0.0180	0.0180	0.0176
	(0.0216)	(0.0216)	(0.0216)	(0.0216)
Deceased parent, age 5	-0.0279	-0.0279	-0.0286	-0.0282
	(0.0577)	(0.0577)	(0.0577)	(0.0577)
Mentally Diagnosed parent, age 5	0.0634	0.0634	0.0637	0.0626
	(0.0651)	(0.0651)	(0.0651)	(0.0651)
Divorced parents, age 5	0.0098	0.0098	0.0098	0.0098
	(0.0110)	(0.0110)	(0.0110)	(0.0110)
Criminal parent, 9th grade	-0.1091***	-0.1092***	-0.1092***	-0.1077***
	(0.0195)	(0.0195)	(0.0195)	(0.0195)
Deceased parent, 9th grade	-0.0166	-0.0166	-0.0187	-0.0167
	(0.0229)	(0.0229)	(0.0230)	(0.0229)
Mentally Diagnosed parent, 9th grade	-0.0270**	-0.0272**	-0.0271**	-0.0270**
	(0.0127)	(0.0127)	(0.0127)	(0.0127)
Divorced parents, 9th grade	-0.0820***	-0.0820***	-0.0820***	-0.0820***
	(0.0077)	(0.0076)	(0.0076)	(0.0076)
Peer divorced parents	0.0037			
	(0.0538)			
Peer mentally diagnosed parent		-0.0188		
		(0.0766)		
Peer deceased parent			-0.2353	
			(0.1611)	
Peer criminal parent				0.1362
				(0.0849)
N	95240	95240	95240	95240
R^2	0.105	0.105	0.105	0.105

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.20: Estimates of Vulnerability on Math (exit exam) for Girls

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0113	0.0112	0.0112	0.0114
	(0.0186)	(0.0186)	(0.0186)	(0.0186)
Deceased parent, age 5	0.0746	0.0748	0.0748	0.0746
	(0.0504)	(0.0504)	(0.0504)	(0.0504)
Mentally Diagnosed parent, age 5	0.0679	0.0673	0.0675	0.0665
	(0.0524)	(0.0524)	(0.0524)	(0.0524)
Divorced parents, age 5	-0.0117	-0.0118	-0.0118	-0.0118
	(0.0094)	(0.0094)	(0.0094)	(0.0094)
Criminal parent, 9th grade	-0.1197***	-0.1197***	-0.1196***	-0.1213***
	(0.0174)	(0.0174)	(0.0174)	(0.0174)
Deceased parent, 9th grade	-0.0016	-0.0013	-0.0027	-0.0015
	(0.0200)	(0.0200)	(0.0201)	(0.0200)
Mentally Diagnosed parent, 9th grade	-0.1072***	-0.1076***	-0.1073***	-0.1073***
	(0.0113)	(0.0113)	(0.0113)	(0.0113)
Divorced parents, 9th grade	-0.1171***	-0.1162***	-0.1162***	-0.1163***
	(0.0068)	(0.0068)	(0.0068)	(0.0068)
Peer divorced parents	-0.1259***			
	(0.0478)			
Peer mentally diagnosed parent		-0.0283		
		(0.0627)		
Peer deceased parent			-0.1435	
			(0.1402)	
Peer criminal parent				-0.1656**
				(0.0665)
N	94780	94780	94780	94780
R^2	0.162	0.162	0.162	0.162

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.21: Estimates of Vulnerability on Math (exit exam) for Boys

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0039	0.0040	0.0039	0.0041
	(0.0204)	(0.0204)	(0.0204)	(0.0204)
Deceased parent, age 5	-0.0527	-0.0529	-0.0531	-0.0525
	(0.0504)	(0.0504)	(0.0504)	(0.0504)
Mentally Diagnosed parent, age 5	0.0846	0.0850	0.0847	0.0852
	(0.0558)	(0.0559)	(0.0558)	(0.0558)
Divorced parents, age 5	-0.0092	-0.0092	-0.0092	-0.0092
	(0.0097)	(0.0097)	(0.0097)	(0.0097)
Criminal parent, 9th grade	-0.0976***	-0.0977***	-0.0976***	-0.0990***
	(0.0189)	(0.0189)	(0.0189)	(0.0190)
Deceased parent, 9th grade	-0.0327	-0.0324	-0.0338*	-0.0323
	(0.0204)	(0.0204)	(0.0204)	(0.0204)
Mentally Diagnosed parent, 9th grade	-0.0799***	-0.0805***	-0.0799***	-0.0799***
	(0.0113)	(0.0113)	(0.0113)	(0.0113)
Divorced parents, 9th grade	-0.0970***	-0.0965***	-0.0965***	-0.0965***
	(0.0065)	(0.0065)	(0.0065)	(0.0065)
Peer divorced parents	-0.0607			
	(0.0450)			
Peer mentally diagnosed parent		-0.0722		
		(0.0652)		
Peer deceased parent			-0.1576	
			(0.1415)	
Peer criminal parent				-0.1314*
				(0.0706)
N	95592	95593	95593	95593
R^2	0.147	0.147	0.147	0.147

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.22: Estimates of Vulnerability on Reading (exit exam) for the subgroup of siblings attending the same school

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.1123	0.1123	0.1122	0.1122
	(0.1180)	(0.1180)	(0.1180)	(0.1180)
Deceased parent, age 5	0.4193	0.4205	0.4212	0.4198
	(0.2790)	(0.2789)	(0.2787)	(0.2789)
Mentally Diagnosed parent, age 5	-0.1823	-0.1827	-0.1789	-0.1821
	(0.4011)	(0.4015)	(0.4016)	(0.4012)
Divorced parents, age 5	0.0169	0.0168	0.0170	0.0169
	(0.0407)	(0.0407)	(0.0407)	(0.0407)
Criminal parent, 9th grade	-0.3786***	-0.3785***	-0.3795***	-0.3787***
	(0.1150)	(0.1150)	(0.1149)	(0.1150)
Deceased parent, 9th grade	-0.0049	-0.0048	-0.0011	-0.0051
	(0.1449)	(0.1449)	(0.1452)	(0.1449)
Mentally Diagnosed parent, 9th grade	-0.0757	-0.0758	-0.0768	-0.0758
	(0.0739)	(0.0739)	(0.0740)	(0.0739)
Divorced parents, 9th grade	-0.0426	-0.0424	-0.0415	-0.0426
	(0.0907)	(0.0907)	(0.0907)	(0.0907)
Peer divorced parents	0.0132			
	(0.0901)			
Peer mentally diagnosed parent		-0.0257		
		(0.1267)		
Peer deceased parent			-0.4189	
			(0.2771)	
Peer criminal parent				-0.0114
				(0.1466)
N	34343	34343	34343	34343
R^2	0.057	0.057	0.057	0.057

Standard errors in parentheses
Standard errors are clustered at the family level.
Estimations include basic, parental, peer and cohort variables, and family fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.23: Estimates of Vulnerability on Math (exit exam) for the subgroup of siblings attending the same school

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	-0.0045	-0.0047	-0.0054	-0.0052
	(0.0865)	(0.0864)	(0.0863)	(0.0864)
Deceased parent, age 5	0.2235	0.2289	0.2272	0.2261
	(0.2492)	(0.2488)	(0.2492)	(0.2491)
Mentally Diagnosed parent, age 5	-0.8186*	-0.8200*	-0.8146*	-0.8177*
	(0.4965)	(0.4974)	(0.4949)	(0.4957)
Divorced parents, age 5	0.0245	0.0241	0.0243	0.0246
	(0.0330)	(0.0330)	(0.0330)	(0.0330)
Criminal parent, 9th grade	-0.0793	-0.0786	-0.0798	-0.0798
	(0.0980)	(0.0980)	(0.0979)	(0.0981)
Deceased parent, 9th grade	-0.0871	-0.0881	-0.0861	-0.0888
	(0.1281)	(0.1278)	(0.1280)	(0.1279)
Mentally Diagnosed parent, 9th grade	-0.1170*	-0.1177*	-0.1185*	-0.1175*
	(0.0619)	(0.0619)	(0.0619)	(0.0619)
Divorced parents, 9th grade	0.0323	0.0333	0.0334	0.0326
	(0.0766)	(0.0765)	(0.0766)	(0.0766)
Peer divorced parents	0.0732			
	(0.0721)			
Peer mentally diagnosed parent		-0.1120		
		(0.1025)		
Peer deceased parent			-0.3617	
			(0.2313)	
Peer criminal parent				-0.1057
				(0.1181)
N	34366	34367	34367	34367
R^2	0.025	0.025	0.026	0.025

Standard errors in parentheses
Standard errors are clustered at the family level.
Estimations include basic, parental, peer and cohort variables, and family fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.24: Estimates of Vulnerability on Reading (exit exam) for the subgroup of siblings attending different schools

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	-0.3666*	-0.3664*	-0.3650*	-0.3655*
	(0.1926)	(0.1925)	(0.1929)	(0.1926)
Deceased parent, age 5	0.4507	0.4477	0.4587	0.4510
	(0.4612)	(0.4629)	(0.4627)	(0.4612)
Mentally Diagnosed parent, age 5	0.6962*	0.6907*	0.6907*	0.6918*
	(0.3870)	(0.3880)	(0.3855)	(0.3869)
Divorced parents, age 5	0.0826	0.0830	0.0823	0.0832
	(0.0570)	(0.0570)	(0.0571)	(0.0570)
Criminal parent, 9th grade	0.1128	0.1142	0.1171	0.1142
	(0.1815)	(0.1817)	(0.1819)	(0.1817)
Deceased parent, 9th grade	0.0370	0.0366	0.0370	0.0342
	(0.2260)	(0.2263)	(0.2254)	(0.2261)
Mentally Diagnosed parent, 9th grade	-0.0306	-0.0269	-0.0331	-0.0297
	(0.1294)	(0.1293)	(0.1290)	(0.1294)
Divorced parents, 9th grade	-0.3059**	-0.3043**	-0.3038**	-0.3051**
	(0.1347)	(0.1345)	(0.1347)	(0.1345)
Peer divorced parents	-0.0608			
	(0.1159)			
Peer mentally diagnosed parent		-0.1602		
		(0.1684)		
Peer deceased parent			-0.5632	
			(0.3546)	
Peer criminal parent				-0.0409
				(0.1892)
N	10761	10761	10761	10761
R^2	0.077	0.077	0.078	0.077

Standard errors in parentheses
Standard errors are clustered at the family level.
Estimations include basic, parental, peer and cohort variables, and family fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.25: Estimates of Vulnerability on Math (exit exam) for the subgroup of siblings attending different schools

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	-0.2103	-0.2082	-0.2051	-0.2059
	(0.1437)	(0.1445)	(0.1454)	(0.1448)
Deceased parent, age 5	0.6465*	0.6385*	0.6504*	0.6405*
	(0.3510)	(0.3510)	(0.3509)	(0.3500)
Mentally Diagnosed parent, age 5	-0.3266	-0.3592	-0.3530	-0.3588
	(0.3332)	(0.3321)	(0.3286)	(0.3312)
Divorced parents, age 5	0.0417	0.0445	0.0439	0.0448
	(0.0483)	(0.0484)	(0.0484)	(0.0484)
Criminal parent, 9th grade	0.0806	0.0914	0.0925	0.0901
	(0.1476)	(0.1499)	(0.1505)	(0.1500)
Deceased parent, 9th grade	-0.1057	-0.1222	-0.1204	-0.1239
	(0.1725)	(0.1759)	(0.1736)	(0.1752)
Mentally Diagnosed parent, 9th grade	0.0944	0.1026	0.0947	0.1000
	(0.0913)	(0.0914)	(0.0911)	(0.0914)
Divorced parents, 9th grade	-0.3961***	-0.3920***	-0.3908***	-0.3922***
	(0.1255)	(0.1248)	(0.1256)	(0.1249)
Peer divorced parents	-0.3691***			
	(0.0969)			
Peer mentally diagnosed parent		-0.1512		
		(0.1392)		
Peer deceased parent			-0.7604**	
			(0.3688)	
Peer criminal parent				-0.0676
				(0.1644)
N	10812	10812	10812	10812
R^2	0.055	0.052	0.053	0.052

Standard errors in parentheses
Standard errors are clustered at the family level.
Estimations include basic, parental, peer and cohort variables, and family fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.26: Estimates of Decomposition of Vulnerability on Reading (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0111	0.0111	0.0110	0.0096
	(0.0151)	(0.0151)	(0.0151)	(0.0152)
Deceased parent, age 5	0.0006	0.0007	-0.0027	0.0006
	(0.0396)	(0.0396)	(0.0400)	(0.0396)
Mentally Diagnosed parent, age 5	0.0266	0.0260	0.0270	0.0269
	(0.0425)	(0.0425)	(0.0425)	(0.0425)
Divorced parents, age 5	0.0192**	0.0189**	0.0188**	0.0187**
	(0.0077)	(0.0076)	(0.0076)	(0.0076)
Criminal parent, 9th grade	-0.1053***	-0.1053***	-0.1053***	-0.1037***
	(0.0135)	(0.0135)	(0.0135)	(0.0136)
Deceased parent, 9th grade	-0.0195	-0.0195	-0.0203	-0.0195
	(0.0158)	(0.0158)	(0.0159)	(0.0158)
Mentally Diagnosed parent, 9th grade	-0.0509***	-0.0505***	-0.0509***	-0.0509***
	(0.0091)	(0.0091)	(0.0091)	(0.0091)
Divorced parents, 9th grade	-0.0833***	-0.0832***	-0.0832***	-0.0831***
	(0.0054)	(0.0054)	(0.0054)	(0.0054)
Peer divorced parents, 9th grade	-0.0196			
	(0.0445)			
Peer divorced parents, age 5	0.0408			
	(0.0657)			
Peer mentally diagnosed parent, 9th grade		0.0386		
		(0.0732)		
Peer mentally diagnosed parent, age 5		-0.0928		
		(0.0973)		
Peer deceased parent, 9th grade			-0.0753	
			(0.1340)	
Peer deceased parent, age 5			-0.3116	
			(0.2731)	
Peer criminal parent, 9th grade				0.1641
				(0.1167)
Peer criminal parent, age 5				-0.1504
				(0.1324)
N	190179	190179	190179	190179
R^2	0.138	0.138	0.138	0.138

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.27: Estimates of Decomposition of Vulnerability on Math (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0079	0.0080	0.0079	0.0066
	(0.0140)	(0.0140)	(0.0140)	(0.0141)
Deceased parent, age 5	0.0138	0.0138	0.0139	0.0139
	(0.0371)	(0.0370)	(0.0373)	(0.0370)
Mentally Diagnosed parent, age 5	0.0746*	0.0742*	0.0746*	0.0743*
	(0.0384)	(0.0384)	(0.0384)	(0.0384)
Divorced parents, age 5	-0.0111*	-0.0109*	-0.0109*	-0.0110*
	(0.0065)	(0.0065)	(0.0065)	(0.0065)
Criminal parent, 9th grade	-0.1107***	-0.1108***	-0.1107***	-0.1111***
	(0.0130)	(0.0130)	(0.0130)	(0.0131)
Deceased parent, 9th grade	-0.0177	-0.0174	-0.0190	-0.0174
	(0.0146)	(0.0146)	(0.0147)	(0.0146)
Mentally Diagnosed parent, 9th grade	-0.0942***	-0.0944***	-0.0942***	-0.0942***
	(0.0079)	(0.0080)	(0.0079)	(0.0079)
Divorced parents, 9th grade	-0.1068***	-0.1062***	-0.1062***	-0.1062***
	(0.0047)	(0.0047)	(0.0047)	(0.0047)
Peer divorced parents, 9th grade	-0.0811**			
	(0.0391)			
Peer divorced parents, age 5	-0.0218			
	(0.0572)			
Peer mentally diagnosed parent, 9th grade		-0.0241		
		(0.0642)		
Peer mentally diagnosed parent, age 5		-0.0438		
		(0.0842)		
Peer deceased parent, 9th grade			-0.1685	
			(0.1167)	
Peer deceased parent, age 5			0.0147	
			(0.2529)	
Peer criminal parent, 9th grade				-0.0174
				(0.0943)
Peer criminal parent, age 5				-0.1649
				(0.1090)
N	190372	190373	190373	190373
R^2	0.158	0.158	0.158	0.158

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.28: Estimates of Vulnerability on Reading (exit exam)

	(1) Divorce no school FE	(2) Divorce incl. school avg.	(3) Mental Diagnosis no school FE	(4) Mental Diagnosis incl. school avg.	(5) Deceased Parent no school FE	(6) Deceased Parent incl. school avg.	(7) Crime no school FE	(8) Crime incl. school avg.
Criminal parent, age 5	0.0111	0.0127	0.0112	0.0128	0.0113	0.0128	0.0113	0.0128
	(0.0152)	(0.0152)	(0.0152)	(0.0152)	(0.0152)	(0.0152)	(0.0152)	(0.0152)
Deceased parent, age 5	-0.0099	-0.0133	-0.0098	-0.0134	-0.0099	-0.0134	-0.0098	-0.0134
	(0.0397)	(0.0398)	(0.0397)	(0.0398)	(0.0397)	(0.0398)	(0.0397)	(0.0398)
Mentally Diagnosed parent, age 5	0.0195	0.0210	0.0196	0.0211	0.0199	0.0215	0.0193	0.0209
	(0.0417)	(0.0417)	(0.0417)	(0.0417)	(0.0418)	(0.0418)	(0.0418)	(0.0418)
Divorced parents, age 5	0.0164**	0.0159**	0.0161**	0.0156**	0.0159**	0.0155**	0.0160**	0.0155**
	(0.0076)	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0077)	(0.0076)	(0.0077)
Criminal parent, 9th grade	-0.1056***	-0.1063***	-0.1058***	-0.1065***	-0.1059***	-0.1065***	-0.1058***	-0.1065***
	(0.0135)	(0.0135)	(0.0135)	(0.0135)	(0.0135)	(0.0135)	(0.0135)	(0.0135)
Deceased parent, 9th grade	-0.0173	-0.0166	-0.0170	-0.0163	-0.0170	-0.0163	-0.0170	-0.0163
	(0.0159)	(0.0159)	(0.0159)	(0.0159)	(0.0159)	(0.0159)	(0.0159)	(0.0159)
Mentally Diagnosed parent, 9th grade	-0.0532***	-0.0533***	-0.0531***	-0.0532***	-0.0534***	-0.0535***	-0.0534***	-0.0535***
	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)
Divorced parents, 9th grade	-0.0835***	-0.0839***	-0.0841***	-0.0844***	-0.0843***	-0.0846***	-0.0843***	-0.0846***
	(0.0054)	(0.0054)	(0.0054)	(0.0054)	(0.0054)	(0.0054)	(0.0054)	(0.0054)
Peer divorced parents	-0.0625*	-0.0665**						
	(0.0323)	(0.0327)						
Peer mentally diagnosed parent			-0.1010**	-0.1089**				
			(0.0480)	(0.0488)				
Peer deceased parent					-0.1582	-0.1552		
					(0.1072)	(0.1070)		
Peer criminal parent							-0.0427	-0.0319
							(0.0529)	(0.0539)
N	190179	189982	190179	189982	190179	189982	190179	189982
R^2	0.157	0.157	0.157	0.157	0.157	0.157	0.157	0.157

Standard errors in parentheses
Standard errors are clustered at the school level. Estimations include basic, parental, peer and cohort variables.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.29: Estimates of Vulnerability on Math (exit exam)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Divorce	Divorce	Mental Diagnosis	Mental Diagnosis	Deceased Parent	Deceased Parent	Crime	Crime
	no school FE	incl. school avg.	no school FE	incl. school avg.	no school FE	incl. school avg.	no school FE	incl. school avg.
Criminal parent, age 5	0.0168	0.0151	0.0172	0.0153	0.0173	0.0152	0.0173	0.0154
	(0.0140)	(0.0140)	(0.0141)	(0.0140)	(0.0141)	(0.0141)	(0.0140)	(0.0140)
Deceased parent, age 5	0.0086	0.0044	0.0089	0.0034	0.0087	0.0033	0.0087	0.0035
	(0.0373)	(0.0373)	(0.0373)	(0.0373)	(0.0373)	(0.0373)	(0.0372)	(0.0373)
Mentally Diagnosed parent, age 5	0.0829**	0.0853**	0.0834**	0.0859**	0.0835**	0.0863**	0.0810**	0.0841**
	(0.0387)	(0.0387)	(0.0388)	(0.0388)	(0.0388)	(0.0388)	(0.0389)	(0.0389)
Divorced parents, age 5	-0.0162**	-0.0148**	-0.0191***	-0.0170**	-0.0196***	-0.0173***	-0.0191***	-0.0168**
	(0.0066)	(0.0066)	(0.0066)	(0.0066)	(0.0067)	(0.0067)	(0.0067)	(0.0067)
Criminal parent, 9th grade	-0.1207***	-0.1188***	-0.1224***	-0.1201***	-0.1225***	-0.1200***	-0.1217***	-0.1195***
	(0.0131)	(0.0131)	(0.0131)	(0.0131)	(0.0131)	(0.0131)	(0.0131)	(0.0131)
Deceased parent, 9th grade	-0.0160	-0.0148	-0.0140	-0.0129	-0.0140	-0.0128	-0.0143	-0.0131
	(0.0148)	(0.0147)	(0.0148)	(0.0147)	(0.0148)	(0.0147)	(0.0148)	(0.0147)
Mentally Diagnosed parent, 9th grade	-0.0990***	-0.0978***	-0.0992***	-0.0981***	-0.1004***	-0.0988***	-0.1002***	-0.0987***
	(0.0080)	(0.0080)	(0.0080)	(0.0079)	(0.0080)	(0.0080)	(0.0080)	(0.0080)
Divorced parents, 9th grade	-0.1139***	-0.1139***	-0.1191***	-0.1179***	-0.1198***	-0.1183***	-0.1192***	-0.1177***
	(0.0048)	(0.0048)	(0.0049)	(0.0049)	(0.0049)	(0.0049)	(0.0049)	(0.0049)
Peer divorced parents	-0.4738***	-0.4196***						
	(0.0369)	(0.0362)						
Peer mentally diagnosed parent			-0.3705***	-0.2832***				
			(0.0526)	(0.0525)				
Peer deceased parent					-0.2841**	-0.2079*		
					(0.1202)	(0.1182)		
Peer criminal parent							-0.4119***	-0.3616***
							(0.0578)	(0.0567)
N	190372	190179	190373	190179	190373	190179	190373	190179
R^2	0.213	0.215	0.211	0.213	0.210	0.213	0.211	0.213

Standard errors in parentheses
Standard errors are clustered at the school level. Estimations include basic, parental, peer and cohort variables.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.30: Instrumental Variables Estimates of Vulnerability on Reading (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0088	0.0095	0.0096	0.0095
	(0.0175)	(0.0172)	(0.0171)	(0.0172)
Deceased parent, age 5	0.0069	0.0040	0.0037	0.0041
	(0.0452)	(0.0442)	(0.0442)	(0.0443)
Mentally Diagnosed parent, age 5	-0.0083	-0.0040	-0.0032	-0.0046
	(0.0509)	(0.0495)	(0.0495)	(0.0497)
Divorced parents, age 5	0.0135	0.0132	0.0130	0.0131
	(0.0086)	(0.0085)	(0.0085)	(0.0085)
Criminal parent, 9th grade	-0.1098***	-0.1098***	-0.1098***	-0.1098***
	(0.0152)	(0.0149)	(0.0149)	(0.0149)
Deceased parent, 9th grade	-0.0201	-0.0189	-0.0187	-0.0189
	(0.0175)	(0.0172)	(0.0172)	(0.0172)
Mentally Diagnosed parent, 9th grade	-0.0511***	-0.0512***	-0.0516***	-0.0516***
	(0.0103)	(0.0101)	(0.0101)	(0.0101)
Divorced parents, 9th grade	-0.0849***	-0.0861***	-0.0863***	-0.0862***
	(0.0058)	(0.0058)	(0.0058)	(0.0058)
Peer divorced parents	-0.1649**			
	(0.0724)			
Peer mentally diagnosed parent		-0.1741*		
		(0.0967)		
Peer deceased parent			-0.1432	
			(0.1808)	
Peer criminal parent				-0.1156
				(0.1167)
N	189502	189502	189502	189502

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school averages.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.31: Instrumental Variables Estimates of Vulnerability on Math (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0090	0.0097	0.0097	0.0101
	(0.0150)	(0.0151)	(0.0151)	(0.0150)
Deceased parent, age 5	0.0241	0.0149	0.0150	0.0145
	(0.0415)	(0.0413)	(0.0414)	(0.0413)
Mentally Diagnosed parent, age 5	0.0717	0.0846*	0.0849*	0.0836*
	(0.0458)	(0.0455)	(0.0456)	(0.0456)
Divorced parents, age 5	-0.0121	-0.0131*	-0.0134*	-0.0130*
	(0.0073)	(0.0073)	(0.0074)	(0.0073)
Criminal parent, 9th grade	-0.1122***	-0.1116***	-0.1117***	-0.1113***
	(0.0141)	(0.0142)	(0.0142)	(0.0141)
Deceased parent, 9th grade	-0.0067	-0.0031	-0.0030	-0.0035
	(0.0158)	(0.0158)	(0.0158)	(0.0158)
Mentally Diagnosed parent, 9th grade	-0.0875***	-0.0887***	-0.0892***	-0.0894***
	(0.0089)	(0.0089)	(0.0089)	(0.0088)
Divorced parents, 9th grade	-0.1127***	-0.1166***	-0.1170***	-0.1164***
	(0.0051)	(0.0052)	(0.0052)	(0.0052)
Peer divorced parents	-0.4826***			
	(0.0674)			
Peer mentally diagnosed parent		-0.2888***		
		(0.0935)		
Peer deceased parent			-0.0787	
			(0.1801)	
Peer criminal parent				-0.4177***
				(0.1127)
N	189694	189694	189694	189694

Standard errors in parentheses
Standard errors are clustered at the school level.
Estimations include basic, parental, peer and cohort variables, and school averages.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.32: Estimates at the Municipality Level of Vulnerability on Reading (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	-0.0035	-0.0034	-0.0033	-0.0036
	(0.0152)	(0.0151)	(0.0151)	(0.0152)
Deceased parent, age 5	-0.0440	-0.0438	-0.0446	-0.0437
	(0.0492)	(0.0492)	(0.0491)	(0.0492)
Mentally Diagnosed parent, age 5	0.0257	0.0256	0.0258	0.0254
	(0.0443)	(0.0443)	(0.0444)	(0.0443)
Divorced parents, age 5	0.0228***	0.0228***	0.0229***	0.0228***
	(0.0077)	(0.0077)	(0.0077)	(0.0077)
Criminal parent, 9th grade	-0.0924***	-0.0925***	-0.0925***	-0.0922***
	(0.0160)	(0.0160)	(0.0160)	(0.0160)
Deceased parent, 9th grade	0.0047	0.0046	0.0031	0.0045
	(0.0186)	(0.0186)	(0.0186)	(0.0186)
Mentally Diagnosed parent, 9th grade	-0.0561***	-0.0561***	-0.0560***	-0.0560***
	(0.0097)	(0.0097)	(0.0097)	(0.0097)
Divorced parents, 9th grade	-0.0902***	-0.0903***	-0.0904***	-0.0903***
	(0.0059)	(0.0058)	(0.0059)	(0.0058)
Peer divorced parents	0.2306*			
	(0.1332)			
Peer mentally diagnosed parent		-0.1899		
		(0.2577)		
Peer deceased parent			-1.6503***	
			(0.5712)	
Peer criminal parent				0.2509
				(0.2143)
N	165872	165872	165872	165872
R^2	0.150	0.150	0.150	0.150

Standard errors in parentheses
Standard errors are clustered at the municipality level.
Estimations include basic, parental, peer and cohort variables, and municipality fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.33: Estimates at the Municipality Level of Vulnerability on Math (exit exam)

	(1) Divorce	(2) Mental Diagnosis	(3) Deceased Parent	(4) Crime
Criminal parent, age 5	0.0053	0.0053	0.0054	0.0053
	(0.0149)	(0.0149)	(0.0148)	(0.0148)
Deceased parent, age 5	0.0026	0.0026	0.0023	0.0027
	(0.0468)	(0.0468)	(0.0468)	(0.0468)
Mentally Diagnosed parent, age 5	0.1095***	0.1095***	0.1096***	0.1094***
	(0.0394)	(0.0394)	(0.0394)	(0.0394)
Divorced parents, age 5	-0.0123	-0.0123	-0.0123	-0.0123
	(0.0077)	(0.0077)	(0.0077)	(0.0077)
Criminal parent, 9th grade	-0.1114***	-0.1114***	-0.1114***	-0.1113***
	(0.0154)	(0.0154)	(0.0154)	(0.0154)
Deceased parent, 9th grade	0.0094	0.0094	0.0088	0.0094
	(0.0162)	(0.0162)	(0.0162)	(0.0162)
Mentally Diagnosed parent, 9th grade	-0.1072***	-0.1071***	-0.1072***	-0.1072***
	(0.0086)	(0.0086)	(0.0086)	(0.0086)
Divorced parents, 9th grade	-0.1151***	-0.1151***	-0.1151***	-0.1151***
	(0.0055)	(0.0056)	(0.0056)	(0.0056)
Peer divorced parents	0.0018			
	(0.1733)			
Peer mentally diagnosed parent		0.1022		
		(0.2045)		
Peer deceased parent			-0.6347	
			(0.6044)	
Peer criminal parent				0.1072
				(0.2061)
N	166024	166024	166024	166024
R^2	0.188	0.188	0.188	0.188

Standard errors in parentheses
Standard errors are clustered at the municipality level.
Estimations include basic, parental, peer and cohort variables, and municipality fixed effects.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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