FACULTY OF SOCIAL SCIENCES
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PhD Dissertation

Hans Henrik Sievertsen

From Birth to Graduation

Essays on the Economics of Education and Early Interventions

Supervisors: Paul Bingley, Mette Ejrnæs & Søren Leth-Petersen

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Hans Henrik Sievertsen

University of Copenhagen, Department of Economics
SFI - The Danish National Centre for Social Research

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Hans Henrik Sievertsen, Copenhagen, June 2014.

SUMMARY

Governments spend many resources on job-market programs, health care, and crime prevention. One way to reduce these public costs is to target the public interventions earlier in life. If the unskilled unemployed had received more education in earlier years, he would have better chances of finding a new job. Better nutrition in childhood reduces the individual's need for health care later in life. More education and better job opportunities would also increase the opportunity costs of committing crime, and reduce the the individual's propensity to participate in risky behavior. But early life investments are harder to target than later life investments. It is difficult to identify the child who will become the unemployed, unhealthy or criminal. However, we can create public policy in such a way, that factors which affect everyone maximize the individuals' opportunities to unfold their potential and reduce their need for public support in the future.

This PhD dissertation consists of three self-contained essays which revolve around the question of how external factors affect the individual's health and educational attainment. While each essay is a result of independent work, the three essays as a whole cover three important events in the individual's life: birth, school enrollment and the transmission from school to the labor market.

The first essay, "Before Midnight: Discharge on the Day of Birth, Parental Responses and Health and Schooling Outcomes", is joint work with my colleague at SFI, Miriam Wüst. We analyze how mandated discharge on the day of birth affects the well-being of the child and the mother in terms of health and educational attainment. Five Danish counties introduced mandated discharge on the day of birth policies over the period 1990 to 2003. We exploit the staggered introduction of these policies using Danish administrative data and find that being discharged on the day of birth increases the probability of child hospital readmission within the first 28 days, but not in the longer run. While this result suggests that parents substitute postpartum hospital stays with readmissions, we also find longer-run negative consequences of being discharged on the day of birth: Children and mothers have more general practitioner contacts in the first three years after birth. Moreover the children do worse in primary school both in terms of second and ninth grade test scores. Longer-run health effects are strongest for the least healthy infants and schooling effects are driven by children of at-risk mothers. We use complementary survey data from the Danish National Birth Cohort to show that these at-risk mothers decrease their investments – breastfeeding and well-baby visits to the general practitioner – in response to being discharged on the day of birth and thereby reinforce the effect of reduced health treatment. Privileged mothers in contrast increase their investments if they were discharged on the day of birth, and they thereby compensate for the reduced health treatment. Thus, while being discharged on the day of birth may not cause a lasting damage to the health of a general population of mothers

and infants, at-risk children are affected in the longer run, both in terms of health and schooling outcomes.

The second essay "School Starting Age and Non-Cognitive Skills" is joint work with Thomas S. Dee from Stanford University. While evidence shows that older school starting age is linked to better outcomes in terms of educational attainment and a reduced propensity to participate in risky behavior, recent evidence has cast a doubt about the direct effect on the child's skills. The positive effect of starting school later may driven by two effects not related to the individual's skills: (1) Children who enroll later perform better in tests simply because they are older when they are tested. (2) Children that are older when they enroll in school have less time to participate in risky activities as teenagers, because they also are older when they leave school. Using Danish survey and register data we estimate the causal effect of school starting age on a direct measure of non-cognitive skills. We obtain exogenous variation in school starting age by using information on exact date of birth and exploiting that children typically enroll in school in the calendar year they turn six. Non-cognitive skills are measured independently of grade, when all children are seven years old, by means of the Strength and Difficulties Questionnaire (SDQ). Results show that older school starting age causes a better SDQ score. The aggregated effect is driven by the hyperactivity dimension of the SDQ, while the emotional symptoms, the peer problems, the conduct and the pro-social scales are unaffected by school starting age. The effect is primarily identified for girls, for whom the effect is largest for those with a low level of latent ability, as the marginal treatment effects analysis reveals. Our findings show that school starting age also has a direct effect on the individual's skills.

The third essay is entitled "The Long-Run Effects of Local Unemployment on Educational Attainment" and deals with the individual's transmission from upper secondary schooling to higher education or the labor market. Using Danish administrative data on all high school graduates in Denmark from 1984 to 1999, I show that local unemployment has both a short- and a long-run effect on school enrollment and completion. The short-run effect causes students to enroll and consequently complete additional schooling earlier. The long-run effect causes students who would otherwise never have enrolled to enroll and complete schooling. Local unemployment is measured in terms of the unskilled youth unemployment in the individual's home municipality one month before graduating high school. The effects are strongest for sons of parents with no university degree. These results imply that inter-generational educational mobility is weakest in economic good times. The conclusion that individuals react to fluctuations in local labor markets in their educational decision indicates that making youth employment less attractive could increase the demand for higher education.

Resumé

Den offentlige sektor bruger mange ressourcer på arbejdsmarkedsprogrammer, sundhedssektoren og bekæmpelse af kriminalitet. Disse udgifter kunne reduceres, hvis man investerede mere i individet tidligere i livet. Den ufaglærte arbejdsløse ville sandsynligvis have lettere ved at finde beskæftigelse, hvis han havde gennemført en uddannelse. Samtidig er faren for at individet begår kriminalitet lav, hvis individet har en uddannelse og et godt job, idet omkostningen, i form af et potentielt job og indtægtstab, er høj. Endelig gør en sund barndom det også mindre sandsynligt at individet har brug for sundhedsydelser senere i livet. Investeringer senere i livet har dog den fordel, at det er langt lettere at afgøre, hvem der har brug for den ekstra indsats, når skaden er sket. Det er svært at forudsige, hvilke børn der senere bliver arbejdsløse, kriminelle og får brug for ekstra sundhedsinvesteringer. Vi kan dog indrette den offentlige sektor på en måde, så de institutionelle rammer optimerer individets muligheder for at udfolde sit potentiale og derved minimerer behovet for offentlig støtte senere i livet.

Denne ph.d.-afhandling består af tre selvstændige artikler, der alle handler om, hvordan eksterne faktorer påvirker individets udvikling. Artiklerne kan læses uafhængigt af hinanden, men i sammenhæng dækker de tre vigtige begivenheder i de fleste menneskers liv: fødsel, skolestart og overgangen fra skole til arbejdsmarkedet.

Det første kapitel er skrevet i samarbejde med min kollega ved SFI, Miriam Wüst, og har titlen "Before Midnight: Discharge on the Day of Birth, Parental Responses and Health and Schooling Outcomes". I dette kapitel undersøger vi, hvordan en ambulant fødsel, dvs. en udskrivning fra hospitalet samme dag som fødslen, påvirker barnets og morens helbred og velvære på sigt. For at analysere dette spørgsmål udnytter vi, at fem danske amter indførte tvungen ambulant fødsel i perioden 1990 til 2003. Vi anvender registerdata fra Danmarks Statistik og finder, at børn som blev udskrevet fra hospitalet samme dag de blev født, har højere sandsynlighed for at blive genindlagt indenfor 28 dage, men der er ingen effekter på genindlæggelsessandynligheden på længere sigt. Vi finder dog, at både moren og barnet har flere besøg hos den almene læge i de første tre år efter fødsel, og at barnet klarer sig dårligere i grundskolen. Bag gennemsnitseffekterne ligger en stor grad af heterogenitet. Det er primært børn, som enten har få initiale ressourcer eller ressourcesvage forældre, der oplever disse langsigtseffekter. Ved at kombinere registerdata med data fra spørgeskemaundersøgelsen "Bedre Sundhed for Mor og Barn" finder vi, at disse heterogene effekter potentielt kan forklares ved at mødre i risikogruppen, som oplever en ambulant fødsel, ammer barnet mindre og er mindre tilbøjelige til at gå til de planlagte besøg hos den almene læge. Ressourcestærke mødre derimod øger investeringerne i deres børn, hvis de har oplevet en ambulant fødsel, for eksempel ved at amme mere og udskyde barnets skolestart. Tvungen ambulant fødsel kan derfor have utilsigtede langsigtskonsekvenser for børn og mødre i

risikogruppen.

Det andet kapitel, som er skrevet i samarbejde med Thomas S. Dee fra Stanford University, har titlen "School Starting Age and Non-Cognitive Skills". Det danske skolesystem er ligesom de fleste udenlandske systemer indrettet således, at alle børn i en årgang starter i skole på samme dag. Det medfører en aldersforskel ved skolestart på op til et år, hvilket kan have store konsekvenser for barnets udvikling. I den eksisterende litteratur har man fundet, at en senere skolestart medfører bedre karakterer, en længere uddannelse, en lavere sandsynlighed for at begå kriminalitet og en lavere sandsynlighed for blive gravid som teenager. Der er dog ingen resultater, som entydigt konkluderer, at senere skolestart har en direkte effekt på barnets kognitive eller nonkognitive evner. De bedre karakterer kan skyldes, at barnet er ældre, når det bliver evalueret. Endvidere, så peger forskningen på, at effekterne på kriminalitet og graviditet skyldes, at børn der starter senere, er ældre når de forlader skolen og derfor har mindre tid til at udvise risikoadfærd som teenagere. Vi bidrager til denne litteratur ved at undersøge effekten på et direkte mål af barnets nonkognitive færdigheder, såsom sociale færdigheder, koncentrationsevne og hyperaktivitet. Vi undersøger dette ved at sammenligne børn, som er født kort før nytår med børn, som er født kort efter nytår, fordi børn skal starte i børnehaveklassen det kalenderår, de fylder seks. Vi udnytter altså, at den forventede skolestartsalder ændres markant, afhængigt af om barnet er født kort før eller kort efter første januar. Denne variation i skolestartsalder kombinerer vi med dansk survey- og registerdata og finder at børn, som er ældre ved skolestart, har signifikant bedre nonkognitive færdigheder i form af mindre hyperaktivitet, men vi finder ingen effekter på andre mål af nonkognitive evner, der omfatter relationer til jævnaldrende, sociale færdigheder, adfærdsproblemer og emotionelle problemer. Effekten er primært identificeret for piger, og det er sandsynligvis piger med svage forudsætninger, som har gavn af en senere skolestart.

I det tredje kapitel, "The Long-Run Effects of Local Unemployment on Educational Attainment", undersøger jeg, hvordan lokal arbejdsløshed ved afslutning af gymnasiet påvirker den unges sandsynlighed for at fortsætte i uddannelse. Jeg analyserer uddannelsesmønstret for alle gymnasiumstudenter i perioden 1985 til 1999 og undersøger hvordan det er påvirket af arbejdsløsheden i deres bopælskommune måneden før, at de afslutter gymnasiet. Effekterne kan opdeles i en kort- og en langsigtseffekt: Kortsigtseffekten består i, at forhøjet lokal arbejdsløshed får unge til at fortsætte i uddannelse hurtigere, end de ellers ville gøre. I stedet for at holde et til to års pause mellem uddannelserne, fortsætter de unge med det samme, hvis jobmulighederne i lokalområdet er ringe. Langsigtseffekten består i, at højere arbejdsløshed i bopælskommunen får unge, som ellers aldrig ville fortsætte i uddannelse, til at påbegynde og afslutte en uddannelse. Effekterne er størst for børn af lavtuddannede forældre, hvilket indikerer, at den sociale arv, med hensyn til uddannelse, er størst i højkonjunkturer. Resultatet, at unge reagerer på udsving i lokal arbejdsløshed, tyder på, at vi kan øge tilgangen til uddannelser ved at gøre ungdomsbeskæftigelse mindre attraktivt.

CHAPTER 1

BEFORE MIDNIGHT: DISCHARGE ON THE DAY OF BIRTH, PARENTAL RESPONSES AND HEALTH AND SCHOOLING OUTCOMES[†]

Hans Henrik Sievertsen
The Danish National Centre for Social Research (SFI) & The Department of Economics, University of Copenhagen

Miriam Wüst
The Danish National Centre for Social Research (SFI) & Aarhus University, AU RECEIV

Abstract

We exploit the staggered introduction of mandated early discharge – discharge on the day of birth – to estimate the effects of early hospital discharge on child and mother well-being. Using Danish administrative data, we find that discharge on the day of birth increases the probability of child hospital readmission only within the first 28 days. While this result suggests that parents substitute postpartum hospital stays with readmissions, we also find longer-run negative consequences of being discharged on the day of birth, in terms of the number of general practitioner contacts within the first three years for both mothers and children, and children's primary school GPA. These longer-run health and schooling effects are driven by at-risk mothers and children. We use complementary survey data to show that while advantaged mothers compensate for being discharged on the day of birth by increasing their investments in the child in terms of breastfeeding behavior and well-baby visits to the general practitioner, disadvantaged mothers reinforce the effect of the reduced health treatment by lowering these investments. Thus, while an early discharge policy may not cause a lasting damage to the health of a general population of mothers and infants, it impacts health and school outcomes in the longer-run for at-risk children, both through the direct effect on children's health and by reducing parental investments.

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1. Introduction

Childbirth is expensive for health systems. Childbirth-related patients accounted for 23 percent of all discharged patients from U.S. hospitals in 2005 (Sakala and Corry, 2008). To contain costs, 26 out of 34 OECD countries reduced the average length of postpartum hospital stay (i.e., hospital stay after birth) in the period 2000-2009 (see Figure 1.1). However, a great degree of heterogeneity in postpartum hospitalization length remains. Not only across but also within countries, postpartum hospitalization length of mothers and children varies greatly. Factors explaining this variation include mothers' and children's underlying health, insurance status, and variation in administrative procedures across hospitals or regions.

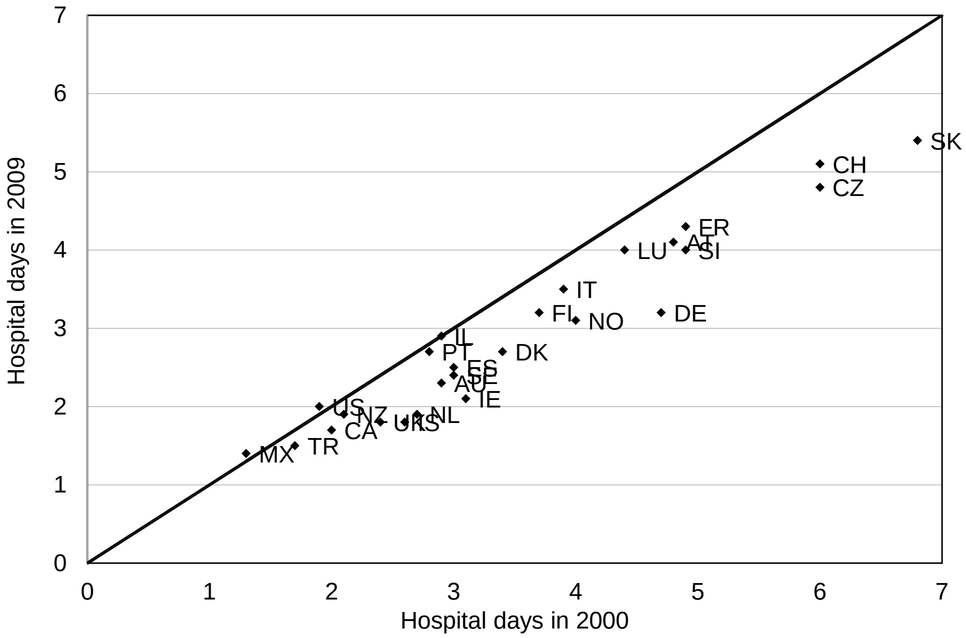


Figure 1.1: Average length of postpartum hospital stay (days) for mothers with a spontaneous delivery in 2000 and 2009. Source: OECD (2012)

Important for policy, we still lack evidence that links the variation in the duration of postpartum hospital stay to differences in health outcomes for mothers and children. For example, post-birth hospital stay and related medical services do not explain much of the variation in infant health outcomes between Austria, Finland and the U.S. (Chen et al., 2013). Results from the U.S. indicate that discharging mothers after two (or one) nights instead of three (or two) nights does not impact short-run health outcomes for an average population of mothers and infants (Almond and Doyle, 2011). At the same time, this finding appears to conceal potential positive health effects of longer hospital stays for mothers and infants who experience complications (Evans and Garthwaite, 2012). These findings taken together point to the importance of 1) the margin at which we evaluate the effect of a change in the length of postpartum hospital stay and 2) the population of mothers and infants that we consider.

Denmark makes an excellent case to study these two factors. First, 5 of 16 Danish counties, together accounting for 34 percent of all births, have introduced mandated discharge on the day of birth for all uncomplicated births by multiparous mothers (i.e., non-first time mothers) in the period 1990-2003. We exploit these policy changes in a difference-in-differences framework to evaluate the short- and medium-run effects of the shortest possible hospital stay after birth on children and mothers. As previous studies exploit exogenous variation in post-birth hospitalizations of at least one or two nights, their analyses may not be at the relevant margin – in terms of its impacts on child and mother outcomes.[1] Second, by analyzing effects on a general and on at-risk populations of mothers and children, we are the first to study the potential heterogeneous effects of a population-wide policy that mandated discharge on the day of birth. Our results can inform policies in Denmark and other countries about the potential costs related to such large-scale early-discharge policies.

A growing number of studies highlights the importance of early life health and interventions targeting early life health for long-run health and economic outcomes (Black et al., 2007; Chay et al., 2009; Almond and Currie, 2011; Bharadwaj et al., 2013).[2] Our study contributes to the evidence on the impact of shortening postpartum hospital stay, a policy that is relevant in many developed countries. Given that previous studies on postpartum hospital stay have exclusively focused on short-run child outcomes, we still know very little about its longer-run effects. By using data on the universe of Danish births between 1985 and 2006, this paper considers longer-run impacts of postpartum hospitalization on child and mother hospital readmission and children's school achievement. Moreover, while the studies most similar to ours have focused on extreme measures such as readmission and mortality, we add more detailed and less severe measures of child and mother health, namely contacts to general practitioners (GP).

A final main contribution of our paper is our focus on the importance and nature of parental response. We use complementary survey data from the Danish National Birth Cohort (DNBC) and thus extend earlier work by studying potentially important mechanisms for the long-run effects of medical investments early in life. The sparse, earlier work on the impact of parental response to early-life health interventions has not found indication for the importance of parental investments as a mediating factor (Bharadwaj et al., 2013).

Our results show that same-day discharge rates for multiparous mothers increased sharply and significantly in treated counties after the introduction of mandated discharge on the day of birth policies. In treated counties, the percentage of mothers experiencing a same-day discharge increased by up to 300 percent. Exploiting the introduction of these policies as an instrument, we find that a discharge on the day of birth increases the probability of child hospital readmission

[1] Almond and Doyle (2011) consider a rule that changed hospitalization length from three to two nights (and two to one nights). They conclude that an, on average, 0.25 day difference in duration has no effect on readmissions within 28 days or on 28-day mortality. Evans and Garthwaite (2012) exploit variation in hospitalization length caused by the same legislative changes and identify heterogeneous effects of being discharged within 48 hours. They estimate a probit model on the probability of being discharged on the day of birth before the legislative changes. The effect of being discharged on the day of birth is largest for those with the lowest propensity score.

[2] Particularly relevant for our study are papers demonstrating the impact of medical treatment early in life: Bharadwaj et al. (2013) find that low birth weight infants treated with intensive medical care at birth do better than their untreated counterparts in terms of educational achievement. Chay et al. (2009) demonstrate that improved access to medical care for black children narrowed the black-white achievement gap observed in the U.S.

in the first 28 days. This result may indicate that parents substitute postpartum hospital stays with readmissions. However, we also find longer-run consequences of early discharge after birth: Same-day discharged infants and mothers have more general practitioner contacts in the first three years of life. Examining schooling outcomes, we find that being discharged on the day of birth leads to a significantly lower test score in grade two and a lower GPA in grade nine.

While we find no heterogeneity of effects for hospital readmissions, mothers and children who have a low a-priori probability of being discharged on the day of birth, have the strongest longer-run effects. Specifically, we show that the negative effects on child health outcomes are strongest for children in poor initial health (as measured by their size for gestational age at birth). Children of at-risk mothers (defined by their age, educational status and income) drive the negative effect of being discharged on the day of birth on schooling outcomes at age 15.

Highlighting the importance of parental response to initial child health as a mechanism for these findings, we show that at-risk mothers who are discharged on the day of birth are less likely to breastfeed exclusively at 4 months of their child's life compared to at-risk mothers who were not discharged on the day of birth. Their children are less likely to receive well-baby visits to health professionals before age 18 months (measured as participation in the vaccination program). Finally, these mothers are more likely to report that their child is in worse health than the average child at age seven. These responses may result from a lack of adequate postnatal care for early-discharged at-risk mothers who as a consequence lack either knowledge, skills or confidence in parenting. At the same time, well-off parents appear to compensate for the reduced medical treatment at birth by increasing their investments in response to being discharged on the day of birth. Thus, the longer-run effects of early discharge appear to be driven by both a direct health effect and by parental responses.

This paper is organized as follows. Section 2 describes relevant features of the Danish health care system and the development of discharge on the day of birth policies over time. Section 3 presents our empirical strategy. Section 4 describes the data used in our analysis. Section 5 presents the results and robustness checks. Section 6 concludes.

2. Background: Births and postpartum care in Denmark

2.1 Relevant features of the Danish health care system

The Danish health care system consists of a municipal primary sector encompassing general practitioners (GP), pharmacies, nursing homes, and the home visiting nurses for infants and new mothers; and a secondary sector covering public hospitals[3] under the responsibility of the Danish counties.

Standard prenatal care consists of three GP examinations throughout pregnancy and four to seven examinations by a midwife. All examinations are free of charge. During the first trimester, GPs refer mothers to a public hospital where the midwife consultations and ultrasound exami-

[3]Today, Denmark has approximately 60 public hospitals. This number is the result of many mergers and the closure of small hospitals over the past 30 years.

nations by trained nurses take place, and where the mother will give birth.[4] While mothers can in principle freely choose among all public hospitals in Denmark, the norm is that mothers are referred to the closest hospital. The mothers' choice of hospital is constrained by the hospitals' capacity. That means that mothers only can choose other than the default hospital if the given (other) hospital has free slots. Only public hospitals provide birth assistance. Most births in Danish hospitals are assisted by trained midwives, while obstetricians and other doctors participate only in the case of complications.

Standard postnatal care consists of a postpartum hospital stay and visits by a municipal home visiting nurse. The home visits start on average within 10 days after birth and end when the child is one to two years old.[5] Moreover, the mother and infant are entitled to scheduled GP examinations. The number of planned examinations within the children's first six years was reduced from eight to seven in 1995 (Sundhedsstyrelsen, 1995). Three of the scheduled examinations are in the first year after birth. These changes are captured by year fixed effects in our empirical model.

2.2 Postpartum hospital stay in Denmark 1985-2006

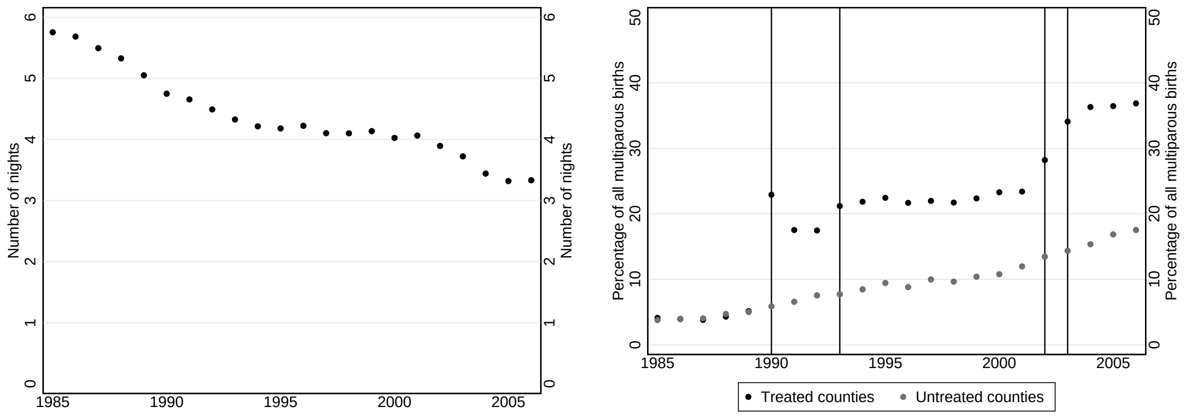
Figure 2.1a shows the average length of postpartum hospital stays for the universe of singleton live births in yearly bins over the 1985-2006 period. Over the period, the average length of hospital stays has decreased by 42 percent from around six nights to around 3.5 nights. While this reduction reflects partly an overall trend towards shorter hospital stays for Danish hospital patients in the period considered, the reduction was also driven by the introduction of mandatory discharge on the day of birth in a set of counties.

Figure 2.1b shows that *voluntary* discharge on the day of birth accounted for less than five percent of all multiparous births in 1985. This figure supports a number of small-scale studies reporting that the demand for discharge on the day of birth was very low (at around 5% of all births) in the 1980s (Fabrin and Olsen, 1987; Fyns Amtskommune, 1987). Both types of counties – the ones that later introduced mandatory discharge on the day of birth and the ones that did not – displayed this very low percentage of mothers who chose to leave hospital on the day of birth. As the vertical lines in Figure 2.1b show, the counties that introduced mandatory discharge on the day of birth in the period we consider show considerable jumps around the timing of introduction of these policies. These jumps departed from the overall trend towards more discharge on the day of birth from the early 1990s onwards.

Table 2.1 gives an overview of the policy changes in 5 out of 16 Danish counties that we use to identify the effect of early discharge on child and mother outcomes. The policies introduced mandated "outpatient birth", i.e. a hospital discharge within 24 hours, which we will refer to as "discharge on the day of birth". All policies were introduced at a centralized level (i.e., by elected county governments), were motivated by the aim of cost containment, and were implemented

[4]Mothers in the 1980s and 1990s had no legal claim to ultrasound examinations. This claim was introduced in 2004. By that time, the majority of pregnant women received two ultrasound examinations during pregnancy (Jørgensen, 2003). From 2004 onwards, all women have been entitled to two ultrasound examinations, around week 12 and 20 of the pregnancy.

[5]The number of visits and whether any visits after the first year of life are provided depends on the municipal service level and has also changed considerably over time.



(a) Average number of postpartum hospital nights for all live births at Danish hospitals

(b) Percentage discharged on the day of birth among multiparous mothers. The vertical lines indicate policy change years.

Figure 2.1: (a) Average length of postpartum hospital stay (all live births at Danish hospitals) and (b) percentage of children discharged on the day of birth (all multiparous live births at Danish hospitals). Source: Own calculations based on hospital records.

without additional changes in other services at the county level. As illustrated in Appendix Figure B.1, two counties in central Jutland (Aarhus and Ringkøbing county) were the first to introduce mandated early discharge for all uncomplicated multiparous mothers in 1990 (Kierkegaard, 1991; Kierkegaard et al., 1992; Lange, 1992; Kierkegaard and Hansen, 1993). Both counties left other hospital services for new mothers and their children unchanged. However, midwives provided a home visit to early-discharged mothers after birth and a few municipalities in the two counties provided additional home visits by home visiting nurses for early-discharged mothers (Kierkegaard et al., 1992). The county of Viborg introduced a mandatory discharge on the day of birth policy in 1993. Only one of 16 municipalities in Viborg county increased the resources for the home visiting program as a reaction to this policy change (Sundhedsplejerskegruppen, 1995).

In a second wave, the counties of Vejle and Ribe introduced mandatory early-discharge policies in 2002 and 2003, respectively (Drevs, 2012; Jensen, 2013). From January 2002, the hospitals in Vejle were to assign a discharge on the day of birth to 80 percent of multiparous mothers with uncomplicated births and no other hospital resources were adjusted (Drevs, 2012). While other hospitals and counties have also seen increases in discharge on the day of birth rates, non of them introduced policies that led to sharp increases in the probability of experiencing a discharge on the day of birth similar to the five treatment counties.

Figure 2.2 illustrates the impact of the introduction of discharge on the day of birth policies on the distribution of mothers' postpartum hospital stays in the treated counties. Comparing the two distributions of hospital stay lengths one year pre- and post-policy, we find a clear shift towards a larger percentage of mothers experiencing very short stays (from one to four nights to zero to one midnights).

Table 2.1: Policy variation: Introduction of discharge on the day of birth policies in Danish counties.

County	Date	Parity	Prepa-ration	Moti-vation	Primary source
Aarhus	1.1.1990	>1	4m	Cost containment	(Aarhus Amtskommune, 1990)
Ringkøbing	1.1.1990	>1	17m		(Kierkegaard, 1991)
Viborg	1.1.1993	>1			(Sundhedsplejerskegruppen, 1995)
Vejle	1.1.2002	>1	2m		(Drevs, 2012)
Ribe	1.1.2003	>1			(Jensen, 2013)

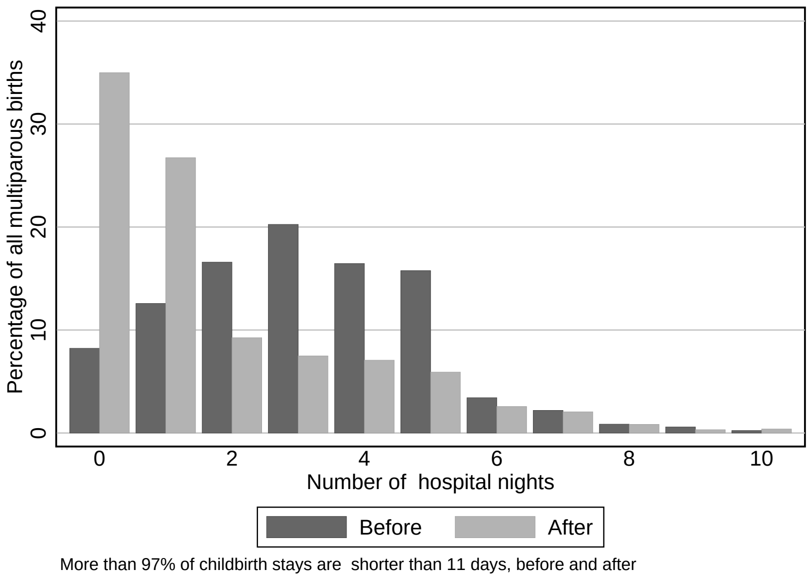


Figure 2.2: Pre- and post-policy distribution of postpartum hospital stay length for multiparous mothers (measured in terms of the number hospital nights). The histogram includes data for births in a one year window around the policy implementation in the five treated counties.

2.3 How reducing postpartum hospital stay may affect the child and the mother

Postpartum hospital care has two objectives: First, health professionals observe the well-being of the child and the mother, and identify the potential need for medical treatment. Second, health professionals provide guidance for the parents to improve their confidence in parenting. In reaction to being discharged on the day of birth, mothers and their children may substitute the reduced care in hospital with general practitioner consultations and hospital readmissions. This substitution effect may also be supply side driven (i.e. by the general practitioner and the hospital. Any short-run effect of a same-day discharge may thus be homogenous with respect to mother and child characteristics.

Even if treated individuals substitute postpartum hospital care with general practitioner contacts there may still be longer-run health effects, because the care and observation period is considerably shorter at the general practitioner than at the hospital. In this case we would expect to see an increase in general practitioner contacts in the years following birth, especially for the mother, because the child already has a number of scheduled contacts in the first two years. The persistent health effect may vary with individual background, if parents compensate or reinforce for the reduced medical treatment by changing their investments in the children, while socioeconomic disadvantaged parents potentially lack resources to compensate for the reduced hospital care (Bernardi, 2014).

The lack of postpartum hospital guidance in parenting may affect the mothers' behavior directly, for example the propensity to breastfeed the child. As for the longer-run health effect, the lack of guidance may have a very heterogeneous effect depending on the parental background. While some mothers may be able to substitute the lack of guidance by seeking advice and support elsewhere, other parents may react by giving up exclusive breastfeeding and introducing suboptimal nutrition for the child.

Taken together, we may expect that the lack of supervision and guidance through postpartum hospital stays has a homogeneous short-run effect where parents, hospitals, or general practitioners react by increasing the number of contacts. The longer-run effect may be heterogeneous, because some parents reinforce the effect of shortened hospital stays by lowering their investments, while other parents compensate by increasing their investments in the child. Longer-run effects on schooling outcomes could therefore be strongest for disadvantaged children who experienced health shocks that were not compensated by increased parental investments.

3. Empirical strategy

To estimate the effect of being discharged on the day of birth on the outcome y for individual i born at time t in hospital h located in county c, we may estimate a model such as:

$$y_{itch} = \alpha_0 + \alpha_1 DBD_i + \alpha_2 \boldsymbol{X}_{itch} + \lambda_h + \theta_t + f(timetrend) + \omega_h \times f(timetrend) + e_{itch} \tag{1.1}$$

where $\boldsymbol{X}_{itch}$ is a vector of child and mother covariates, ϵ_{itch} is a random error, and $DBD = 1$ if the child was discharged on the day of birth. As hospitals may vary systematically in both their quality, their population of mothers, and the duration of postpartum stay that they assign to mothers and children, we include λ_h, a set of hospital indicators accounting for time-invariant differences across hospitals. θ_t is a set of year indicators, accounting for macro shocks. As mothers' and children's outcomes may develop differentially across hospitals (e.g., due to the faster adoption of new technologies in some hospitals), we include $f(timetrend)$ and $\omega_h \times f(timetrend)$, which account for hospital-specific time trends.

However, a comparison of outcomes for mothers and children who experience a discharge on the day of birth to those who do not is most likely biased. On an individual level, the duration of hospital stay is not randomly assigned. Obstetricians and midwives decide on the length of postpartum hospitalization based on both observed mother and child characteristics such as gestational length, birth weight and mother's age and characteristics unobserved by the researcher. Since mothers and children with more favorable unobserved characteristics may on average have more favorable health outcomes and be more likely to experience a discharge on the day of birth, a comparison of same-day discharged births with all other births may lead us to underestimate the impact of a being discharged on the day of birth. Additionally, given that all policy changes were implemented on January 1, seasonality in both fertility and birth outcomes may confound our analysis (see, e.g., Currie and Schwandt, 2013).

To overcome this problem, we exploit variation in administrative rules over time and across Danish counties in a difference in differences framework (DiD). The sudden increase in the percentage of discharge on the day of birth in five counties gives rise to exogenous variation in the probability of being discharged from hospital on the day of childbirth. Thus we compare the differences in outcomes of multiparous mothers who give birth before and after the implementation of new administrative policies in treated counties to the differences in outcomes of multiparous mothers who give birth in non-implementing counties. Our reduced form relationship is:

$$\begin{aligned} y_{itch} = & \gamma_0 + \gamma_1 Post_t \times County_c + \gamma_2 \boldsymbol{X}_{itch} + \lambda_h + \theta_t \\ & + f(timetrend) + \omega_h \times f(timetrend) + \epsilon_{itch} \end{aligned} \tag{1.2}$$

Where $Post_t \times County_c$ indicates a birth in a treated county after the introduction of discharge on the day of birth in that county and, consequently, γ_1 measures the impact of the policy change on child and mother outcomes.

To estimate the effect of discharge on the day of birth on the mothers who change status due to the policy change, we exploit the administrative changes as an instrumental variable. Thus, in a first stage regression we estimate:

$$\begin{aligned} DBD_{itch} = & \beta_0 + \beta_1 Post_t \times County_c + \beta_2 \boldsymbol{X}_{itch} \\ & + \lambda_h + \theta_t + f(timetrend) + \omega_h \times f(timetrend) + \epsilon_{itch} \end{aligned} \tag{1.3}$$

β_1 measures the increased probability of being discharged on the day of birth that is due to the policy change. We allow for arbitrary correlation in errors within hospital times year-cells.[6]

Given that we use across-county differences in administrative rules, we assume that the differences in the outcomes of multiparous mothers and their infants, for example, in Aarhus county and the remaining counties would have remained constant over the period in the absence of the treatment. This assumption may be violated if, for example, other policies were implemented in Aarhus or the composition of the Aarhus county's population changed differentially (and if the

[6]The conclusions are robust to using standard errors clustered on the hospital level.

time trends that we specify fail to account for this change).

We assess the robustness of our preferred DiD estimates in several ways as also detailed in section 5.5. To examine the impact of a potential violation of the common trend assumption, we exploit information on children's parity. As the discharge on the day of birth policies only impacted multiparous mothers, we compare differences in outcomes for primi- and multiparous mothers before and after the policy change in treated counties to the same differences in differences in untreated counties (difference-in-differences-in-differences). While our setting has several attractive features that may encourage a regression discontinuity design (e.g., clear cut-offs in the forcing variable, administrative and daily data for a population of births), we lack power to locally exploit the policy changes.[7]

4. Data and summary statistics

4.1 Data sources and sample construction

To construct our main sample we use information on all registered live births in Denmark from 1985 to 2006 (see Appendix Table A.1 for details on the sample construction). We restrict these data to multiparous births. We exclude 47,794 multiple births (3 percent of the gross sample) and another 24,204 observations with missing hospital information (home births or births outside Denmark, 1.7 percent of the gross sample). These restrictions result in a main dataset of 730,178 births. We also create a subsample of uncomplicated births as defined in Fonnest and Thranov (1998), i.e. we consider only singleton births with a child of at least 2,500 grams, born after at least 37 weeks of gestation, and born by mothers of at least 18 years and without any birth complications (such as cesarean section).

While we have data on hospital admissions for the period 1985-2006, other data sources are available only for subsets of this period. Table 4.1 provides an overview of the available data. We have GP and diagnoses data (for diagnoses given at hospitals) for all years from 1997 onwards.[8] Data on the ninth grade GPA is available for cohorts who completed ninth grade in the period 2002-2012 (which corresponds roughly to the birth cohorts 1987-1997). We use data on the second grade test in Danish covering all children who attended second grade in the period 2009-2012 (which roughly corresponds to birth cohorts 2001 to 2004).

To assess potential mechanisms for long-run effects, we use survey data from the Danish National Birth Cohort (DNBC) (for details on this survey see, e.g., Olsen et al., 2001). This survey is linked to the administrative data and contains initially a sample of around 100,000 births from

[7]We have estimated RDD models that use one year of pre- and post-treatment data for the treated counties. While results are qualitatively similar, given that we only use data from the five treated counties, our estimates are less precise. We also experimented with combining the RDD design with a DiD approach, but naturally we here face similar constraints. Estimates are available on request.

[8]Until 1997 children's contacts to GPs were registered as a parent visit (i.e., the visit was registered with the parent's personal identifier). Thus we can only distinguish between mother and child GP contacts for children of the cohorts from 1997 onwards. To create a consistent sample for our analyses, we use only information on diagnoses during pregnancy, birth and after birth for births after 1997 (the diagnoses data is available in the ICD10 scheme from 1994 onwards). We add indicators for missing information for all births before that year in regressions that control for pregnancy and birth complications.

the period 1997-2003. For the DNBC data collection, pregnant women were invited to participate in two pre-birth and up to four post-birth surveys (at 6 and 18 months, 7 years and 11 years). The survey waves collected a broad set of measures, among them information on maternal health behaviors, maternal investments in children's health and development, and mother-reported child health. We use data from the 18 months and seven year interviews for all mothers and their children. While the DNBC like most other surveys suffers from some attrition between waves and thus is not a representative sample of Danish mothers and children, at the last interview (currently at age 7) around 60 percent of mothers still participated (Jacobsen et al., 2010).

Table 4.1: Analysis samples, outcomes and data sources.

Counties (Policy change year)	Outcome period	Outcomes	Data source
Aarhus (1990), Ringkøbing (1990), Viborg (1993)	1985-2006	Hospital readmission, ninth grade GPA	Admin.
Vejle (2002), Ribe (2003)	1997-2006	Hospital readmission, GP contacts, Post birth complications (mother), diagnoses for readmission (children)	Admin.
Vejle (2002), Ribe (2003)	2001-2004	Second grade test scores	Admin.
Vejle (2002), Ribe (2003)	1997-2003	Breastfeeding duration, child development at age 18 months	Survey

4.2 Variable definitions

We use hospital records from the Danish Inpatient Register to compute the length of hospital stay at birth. Since we have daily (and not hourly) information on hospital admissions and discharges, we define a discharge on the day of birth as a birth where the child is discharged from hospital on the calendar day of birth, although the policy changes stated targets for the share of children discharged within 24 hours of birth. The medical birth registry contains a variable for the length at stay at birth from 1990 onwards. For the period after 1997, our definition of length of stay and the birth registry definition are consistent for 98% of the cases. Results based on the measure from the birth registry are provided in the robustness section.[9]

[9]For the period 1991 to 1996 we have data on the exact number of hours from birth to discharge. Figure B.2 in the Appendix provides a comparison of the length of hospital stay from hospital records with the length of stay data from the detailed birth records for Viborg county.

To account for children's direct admission to another hospital ward, we define all hospital stays with a same-day readmission as one single hospital spell. Children who get moved to another hospital ward get assigned a new admission date that in most cases is the same date as the discharge date from the previous ward. We regard these admissions as a single hospital spell, as it is common to move children from the maternity ward to other wards in case of health issues. Furthermore, we use the hospital data to construct measures for mother and child hospital readmissions within the first 28 days and within the first 365 days after birth.

To measure maternal and child health outcomes in greater detail, we use administrative data on mother and child post birth diagnoses. We construct a measure of mother post-birth complications and operations within three months after birth.[10] Similarly, we construct indicators for diagnoses indicating problems in infants that may be related to poorer postnatal care and lead to readmissions: we consider diagnoses related to problems with infant nutrition and jaundice within 28 days after birth. We also construct an indicator for any of these problems being present.[11]

As existing studies have found very small effects of length of postpartum hospital stay on readmissions (Almond and Doyle, 2011; Evans and Garthwaite, 2012), we also evaluate less severe outcomes. Thus we examine the number of mother and child GP contacts within the first month and the first, second, and third year of the child's life. We use data on GP contacts for the period 1997 to 2006. GP information is available from 1997 and in principle we could include, e.g., children born in 1996 in our analysis for GP visits in the second year of life. Thus our analysis would be based on different data windows for each single GP outcome (no. of visits at a given age). To avoid these "rolling" samples, we constrain the analysis to GP visits in the first three years of life, where we have data for all children's GP contacts at all ages between 0-3. These records include one observation per GP "contact", each of which may contain several services. Given that the data are on GP reimbursements, we lack a precise measure of the timing of the contacts. Thus, to measure the timing of the GP contact, we use information on the week the GP claimed a reimbursement in the public payment system. We assume that this week is shortly after service provision.

To measure child school achievement, we use data on scores from the national test in second grade and primary school test data – when children are approximately 15 years old – from the Danish Ministry of Education. We create an unweighted and standardized (by school year) GPA based on all grades given in ninth grade (all subjects, tests, and teacher evaluations), as well as the average grade in mathematics and Danish (average over tests and teacher evaluations).

Additionally, we compute children's age at school start (in Kindergarten/ grade 0) using information on when they took their first national test in second grade. While children are supposed to start in Kindergarten the year they turn 6, some parents do not comply with this norm. Non-

[10] This measure includes frequent post-birth maternal complications (Danish National Board of Health, 2005). We use the ICD 10 diagnoses codes DO85, DO860, DO861C, DO862A, DK556H, DO871, DO882D, DO702, DF53, DO990A, and operation codes KMWA, KMWB, KMWC, KKCH00, KJFA70, KJFA80, KLCD00, KMBA, KMBB, KMBC00, KTAB30.

[11] This measure includes the ICD 10 diagnoses: Dehydration (DE869A, DE871A), child well-being (DR628A), jaundice (DP59), nutrition problems (DP92, DF982), and breastfeeding problems (DP925).

Table 4.2: Summary Statistics for multiparous births. Means, standard deviations, and sample sizes.

	Mean	SD	N	–Yearly means– 1985	1998	2006
Discharged on the day of birth	0.14	0.34	730,524	0.04	0.14	0.24
Number of hospital nights	3.46	6.17	730,524	5.29	3.11	2.26
Child readmitted 1 < day < 4	0.01	0.07	730,524	0.00	0.01	0.01
Child readmitted day ≤ 28	0.04	0.20	730,524	0.03	0.04	0.06
Child readmitted day ≤365	0.21	0.40	730,524	0.18	0.20	0.24
Mother readmitted 1 < day < 4	0.00	0.05	728,995	0.00	0.00	0.00
Mother readmitted day ≤28	0.02	0.14	728,995	0.01	0.04	0.03
Mother readmitted day ≤365	0.10	0.29	728,995	0.10	0.10	0.08
Child GP visits year 1	8.91	7.42	347,111		8.28	9.33
Child GP visits year 2	8.97	7.19	347,111		8.61	9.02
Mother GP visits year 1	8.73	7.29	346,149		8.65	9.14
Mother GP visits year 2	8.03	7.54	346,149		7.69	8.52
Male child	0.51	0.50	730,524	0.51	0.51	0.51
Birthweight	3,570	557.73	728,097	3,479	3,605	3,611
Low birth weight	0.03	0.17	728,097	0.04	0.03	0.02
Gestation length in weeks	39.73	1.69	726,108	39.67	39.95	39.77
Preterm birth	0.04	0.19	726,108	0.04	0.04	0.04
APGAR cont at 5 min	9.84	0.90	311,270		9.82	9.86
C-section	0.15	0.36	311,270		0.11	0.19
Uncomplicated birth	0.73	0.45	311,270		0.75	0.70
Mother's age	30.85	4.47	722,905	29.57	31.13	32.15
Taxable income (1,000 DKK)	122.90	70.64	722,883	77.05	131.08	167.72
Mother was married	0.63	0.48	722,905	0.66	0.64	0.64
Mother was unemployed	0.13	0.34	722,905	0.16	0.13	0.11
Mother was self-employed	0.03	0.16	722,905	0.03	0.02	0.02
Mother has a higher education	0.28	0.45	722,905	0.23	0.28	0.41
Mother was in education	0.02	0.14	722,905	0.02	0.01	0.03
Mother was self-employed	0.03	0.16	722,905	0.03	0.02	0.02
Mother is unknown	0.00	0.05	730,524	0.00	0.00	0.00

Notes: The sample selection is described in Table A.1. The means in the second column and the standard deviations in the third column are for the full sample period, for which the variable is available.

compliance may be a sign of parental response to poor child health or other factors determining school readiness.[12]

Using survey data from the DNBC, we add additional measures for parental investments and mother-reported measures for child health to the analysis. From the 18 month DNBC survey wave, we create an indicator for maternal breastfeeding of the child for at least four months. As another proxy for parental health investments, we create an indicator for the child having received all scheduled vaccines at age 18 months. Furthermore, from the seven year interview we add mother-reported measures of child health to the analysis: indicators for whether the child

[12]While attendance became obligatory in 2008, around 98% of all children attended Kindergarten (grade 0) also before 2008.

has been diagnosed by a doctor with or treated for food allergies or asthma, and an indicator for the child being in poorer health than average children of the same age.

To account in detail for observed differences between mothers and children, we use administrative data on maternal background characteristics including employment status (indicator variables for whether the mother is in education, retired, self-employed or unemployed, all defined in November), gross income, education (an indicator variable for whether the mother has completed an educational level higher than high school), and civil status. We measure all socio-economic background characteristics with a two year lag for births in the first six months of the year and with a one year lag for births in the last six months.[13] Furthermore, we control for mother's age at birth. For the child, we control for indicators for birth weight and prematurity (gestational age at birth <37 weeks). Finally, for all analyses on post-1996 data, we further control for maternal pregnancy-related health (measured as a set of diagnoses), birth mode (an indicator for caesarean section), and the child's 5 minute APGAR score.

Table 4.2 provides summary statistics for selected outcome and control variables in our main samples, and sample means for selected years in the period considered (only administrative data). The table indicates that child readmission rates and the number of GP contacts have increased over the period considered, while the average number of postpartum hospital nights has decreased. Similarly, changes in the means for the percentage of caesarean section births and the percentage of uncomplicated births indicate the importance to control for trends in our analyses.

5. Results

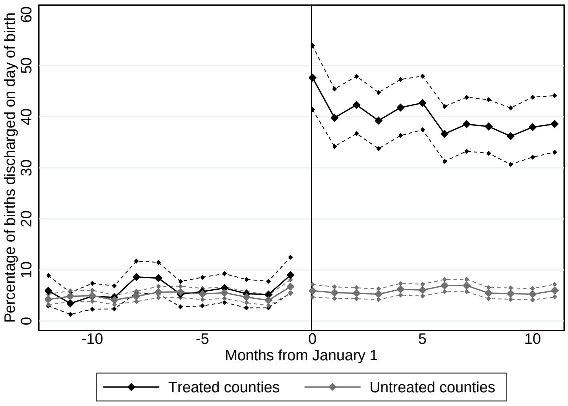
5.1 Graphical evidence

Figure 5.1 presents graphically the variation that we use to identify the effect of a discharge on the day of birth. It shows the monthly share of same-day discharged births by multiparous mothers in treated and untreated counties before and after the introduction of the five policies (the between county variation used in the DiD estimation).[14] The figure shows 1) clear jumps in discharge on the day of birth rates around the introduction of the policies in treated counties and 2) flat and parallel pre- and post-treatment trends across treatment and control counties.

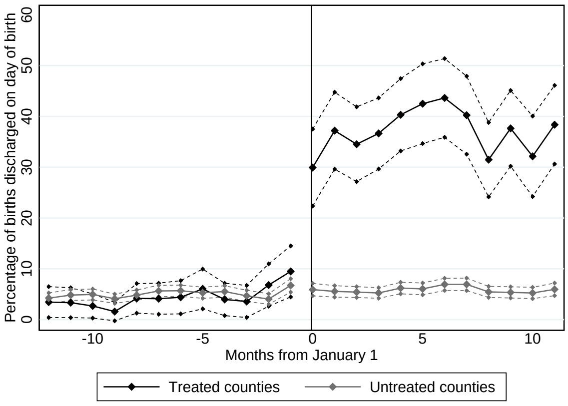
Appendix Figure B.3 shows the monthly discharge on the day of birth rates for first-time mothers in treated and untreated counties. To test the robustness of our results we use these mothers as an additional control group in a triple-difference regression (DiDiD) that accounts for factors such as county-specific parallel policies that may confound our DiD analysis. Appendix Figure B.3 indicates that there was no change in first-time mothers' discharge on the day of birth rates in either treatment or control counties in the period we consider.

[13] For example, for births in December 1989 or January 1990 the background characteristics are measured in 1988. This six months shift ensures comparability in background characteristics around the timing of the policy change.

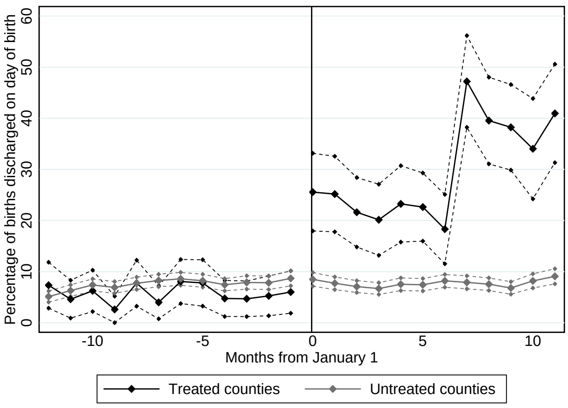
[14] The untreated counties are all other counties, including those that are treated at a different point in time. In the Figure for the 1990 interventions we exclude the other county that experienced a policy change at the same time.



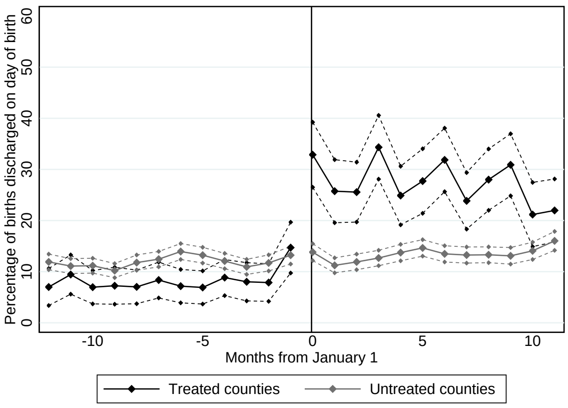
(a) Aarhus county 1990 (Ringkøbing county is excluded)



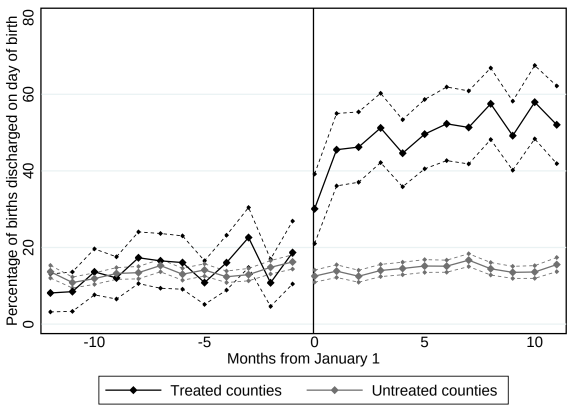
(b) Ringkøbing county 1990 (Aarhus county is excluded)



(c) Viborg county 1993



(d) Vejle county 2002



(e) Ribe county 2002

Figure 5.1: Discharge on the day of birth rates for multiparous mothers in treated and control counties, monthly bins. The control counties include all other counties, including the counties that were treated at a different point in time.

Figures 5.2a to 5.2f show event graphs for selected outcomes of multiparous mothers and their children in treated counties. We collapse the outcome data in years-to-treatment cells. Event graphs should show an impact of the introduction of the new policies only after time $t = -1$. The graphs indicate a small jump in readmission rates in the first month for the child, and a clear jump in ninth grade GPA in year 0. For mother outcomes, we find weaker indication of increased readmission rates the first month, but clear jumps in the number of GP visits in the first month and in the second year of the child's life.

5.2 Main results

Given the graphical evidence, Table 7.4 presents our main results for the effect of a discharge on the day of birth on mother and child health outcomes and child school achievement. Column (1) presents our first stage estimates (FS) and column (2) presents reduced form estimates (RF) for regressions of our outcome measures on an indicator for a post-treatment birth (a birth in a county with a mandated discharge on the day of birth policy in action). Column (3) presents two-stage least squares estimates (2SLS). Each cell presents estimates and standard errors from a separate regression. All specifications account for our set of controls, hospital and year fixed effects, as well as hospital-specific quadratic trends in birth year.[15]

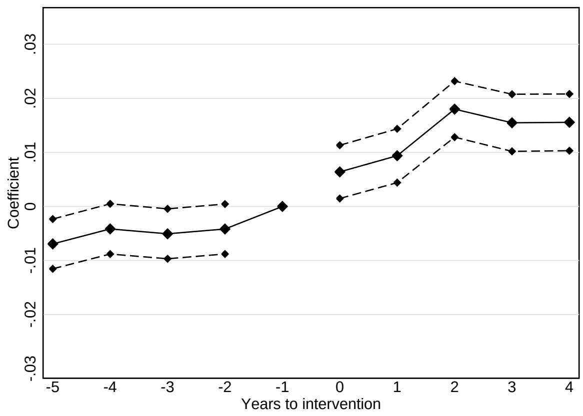
As the samples vary across outcomes due to data availability, we present first stage regressions for all samples in the first row of panels A-D. In line with Figure 5.1, across samples we find a large and consistent jump of 24-25 percentage points in the probability of discharge on the day of birth after the introduction of mandatory discharge on the day of birth policies for multiparous mothers.

For maternal and infant hospital readmission both reduced form and 2SLS estimates show that discharge on the day of birth increases the probability of child readmission within the first 28 days after birth. Column (3) shows that early discharged children experience a three percentage points increase in early readmissions. At a sample mean of around four percent this estimate implies a 75 percent increase in infant hospital readmissions for marginal children. While we examine a set of medical diagnoses given at readmission of infants during the first four days of life – diagnoses related to nutrition problems and jaundice – we lack power to have enough confidence in the results for these rare outcomes (see Appendix Table B.2).[16] As our estimates do not detect an increase in these relevant diagnoses that may indicate a lack of proper postnatal care, the increase in very early readmission may indicate that parents (or hospitals) substitute postpartum hospital stays with readmissions to hospitals. Turning to hospitalizations after the first 28 days of life and up to the first year of the child's life, we do not find a persistent effect of discharge on the day of birth. For mothers the estimate for readmission in the first 28 days is only close to significance at the 10% level. Thus our estimates for readmission indicate no strong effects for mothers.

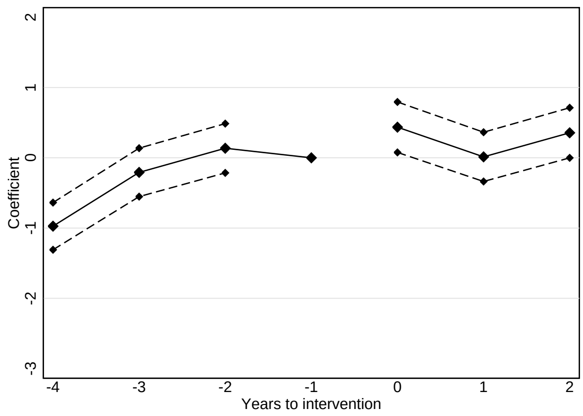
If discharge on the day of birth leads to lasting health problems for mothers or children, we should not only consider readmissions but we may also expect mothers and children to demand

[15] We assess the sensitivity of our results to the model specification in Table 5.6.

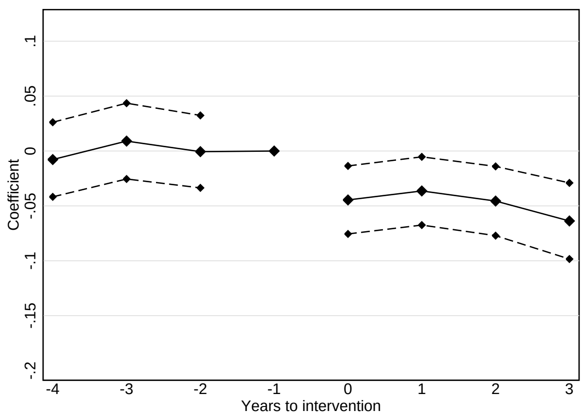
[16] Only around 2-4 percent of readmitted infants are registered in the Inpatient Register with these diagnoses in the period we consider. Given that the registration of diagnoses at hospitals is changing over time, this finding suggests that the respective ICD codes were not routinely used. It may or may not reflect that the conditions were present.



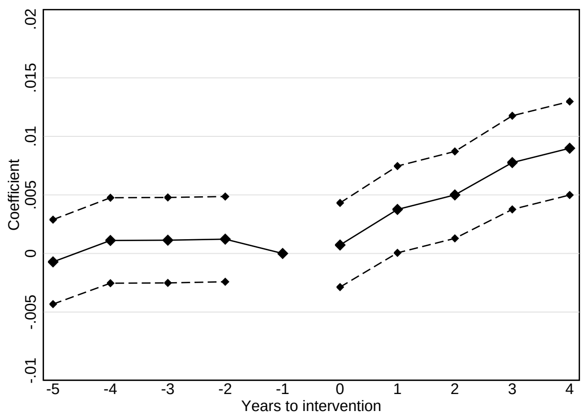
(a) Child readmitted day ≤ 28



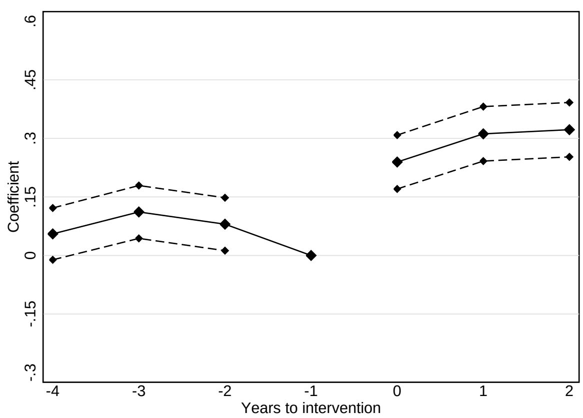
(b) Child GP visits first year



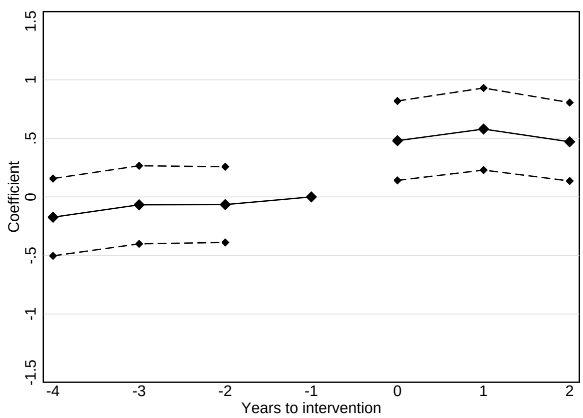
(c) 9th grade GPA



(d) Mother readmitted day ≤ 28



(e) Mother GP visits first month



(f) Mother GP visits second year

Figure 5.2: Event study graphs. Each graph is obtained by regressing the outcome variables on the full set of controls, and a set of indicators for the time to treatment for the treated counties only. The indicator for one year to intervention ($t = -1$) is the reference category.

Table 5.1: The effect of discharge on the day of birth on health and schooling outcomes

	FS (1)	RF (2)	2SLS (3)	Mean
A. Hospital Readmission Outcomes - Sample: 1985-2006; N: 730,524				
Post × County	0.24*** (0.02)			
Child readmitted day ≤ 28		0.01*** (0.00)	0.03*** (0.01)	0.04
Child readmitted day ≤365		-0.01 (0.01)	-0.02 (0.02)	0.21
Mother readmitted day ≤28		0.00 (0.00)	0.02 (0.01)	0.02
Mother readmitted day ≤365		-0.01 (0.01)	-0.03 (0.02)	0.10
B. General Practitioner Outcomes- Sample: 1997-2006; N: 347,111				
Post × County	0.24*** (0.02)			
Child GP visits first month		0.09*** (0.03)	0.39*** (0.15)	0.38
Child GP visits year 1		0.15 (0.23)	0.62 (0.98)	8.91
Child GP visits year 2		0.28 (0.18)	1.17 (0.77)	8.97
Child GP visits year 3		0.32** (0.14)	1.34** (0.58)	5.53
Mother GP visits first month		0.23*** (0.04)	0.96*** (0.15)	1.15
Mother GP visits year 1		0.67*** (0.17)	2.86*** (0.66)	8.73
Mother GP visits year 2		0.44** (0.21)	1.88** (0.86)	8.03
Mother GP visits year 3		0.22 (0.15)	0.95 (0.64)	7.92
C. National Test Score Outcomes - Sample: 2000-2003; N: 108,162				
Post × County	0.25*** (0.01)			
2nd grade Danish (standard scores)		-0.04* (0.02)	-0.16* (0.09)	-0.07
D. 9th grade GPA Outcomes - Sample: 1985-1995; N: 316,146				
Post × County	0.25*** (0.03)			
9th grade GPA (standard score)		-0.03** (0.01)	-0.12** (0.06)	-0.10
9th grade Math (standard score)		-0.02 (0.01)	-0.07 (0.05)	-0.08
9th grade Danish (standard score)		-0.03** (0.02)	-0.13** (0.06)	-0.08

Notes: Each cell shows point estimates from a separate regression. All models are with covariates, year and hospital fixed effects, and hospital-specific quadratic trends. Column (1) provides the first stage coefficient from regressing an indicator for discharge on the day of birth on an indicator for a birth in a treated county after the introduction of discharge on the day of birth policies. Column (2) provides the reduced form regression results from regressing the dependent variables on an indicator for a birth in a treated county after the introduction of discharge on the day of birth policies. Column (3) shows the 2SLS estimates from regressing the dependent variables on an indicator for discharge on the day of birth. The included covariates are indicators for low birth weight and preterm birth, mother's age at birth, employment status (indicators for being unemployed, self-employed, in education), taxable income and education level. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

more contacts to other health professionals. As GPs are the primary access to the Danish health care system, the middle panel of Table 7.4 examines the number of GP contact for children and mothers.[17] Indeed, same-day discharged infants have 0.39 more contacts to the GP in their first month of life. However, as discharge on the day of birth infants from 1995 onwards were granted an additional GP visit in the first month of life (Sundhedsstyrelsen, 1995), our result for short-run increases in GP contacts is partly driven by this change.

While estimates for GP visits in year one, two and three of the child's life are also positive and indicative for around one additional visit in response to being discharged on the day of birth, only the estimate for visits in year three is precise.[18] As postnatal care consists of scheduled GP contacts in the first years as well as the home visiting nurse program, the lack of an effect in the first two years may be because the institutionalized care is sufficient to capture health effects. This is also in line with the large number of GP contacts in the first two years. In both year one and year two the average number of visits at the GP is nine, but in year three it drops to six.[19]

There is no institutionalized GP consultation program for the mothers, which might explain why we find that being discharged on the day of birth increases their number of GP contacts considerably in year one and two after birth. We find a one visit increase of mother GP visits in the first month of the child's life and an increase of around three and two visits in the first and second year of the child's life. The persistent effect for mothers may partly be driven by some mothers' subsequent fertility and related pregnancy checks at GPs.

Taking the results for child health as a point of departure, panels C and D of Table 7.4 examine the impact of discharge on the day of birth on school achievement in the subsamples of children with available data. We find negative and large effects of discharge on the day of birth on school attainment in grade two and nine (interpretable as changes in standard deviations), most of which are significant at the five or 10 percent level. Discharge on the day of birth leads to a decrease in 2nd grade test scores (of around -0.16 standard deviations in our instrumental variable regression) and to a decrease in 9th grade GPA of around -0.12 standard deviations. Note that the results for second grade and ninth grade achievement are estimated using data for different cohorts. Thus our results suggest that discharge on the day of birth has not only impacted school achievement of the oldest cohorts but also impacts the school achievement for the younger cohorts in our data. The result for grade nine achievement is driven by a negative impact of discharge on the day of birth on the score in Danish of about -0.13 of a standard deviation. Thus we see not only evidence for early discharge affecting health care usage of children in the time directly after birth, but also that discharge on the day of birth impacts children's longer-run school achievement negatively. In subsection 5.3 we explore the heterogeneity of these effects across subgroups in the population. Subsection 5.4 turns to potential mechanisms for these observed

[17] Recall that because of data constraints these regressions are based on the policy changes in Vejle county (2002) and Ribe county (2003).

[18] Given that we account flexibly for year fixed effects and trends, we can rule out that country-wide increases in e.g. the offered number of well-baby visits to GPs impacts this result. The number of scheduled GP visits is centrally determined by the Danish national board of health.

[19] As GP data is only available for 1997 to 2010, were are not able to compute longer-run GP outcomes for the sample of children born 1997 to 2006. To obtain information on longer-run health outcomes, we complement the GP measure with a measure of subjective health at age seven in section 5.4.

effects.

5.3 Heterogeneity of the effects of discharge on the day of birth

The effect of being discharged very soon after birth may vary considerably among mothers and children of different characteristics. Following Evans and Garthwaite (2012), this section examines the heterogeneity of the effects of discharge on the day of birth for subgroups of mothers and children. We proceed in two steps: First, we obtain an estimate for the likelihood of experiencing a discharge on the day of birth for all mothers who give birth in treated counties in the pre-policy period. We predict this probability based on the child and mother characteristics.[20] We assign mothers in the post-treatment period a propensity score for experiencing a discharge on the day of birth. Second, we estimate our main specification on 3 subsamples of mothers/children defined by the propensity of being discharged on the day of birth.

Table 5.2: Covariate means for the propensity score samples.

	(1)	(2)	(3)
Propensity score sample	0-33	34-66	67-100
Low birth weight	0.05	0.02	0.01
Preterm birth	0.07	0.03	0.01
Birthweight	3,499.09	3,611.53	3,604.41
Gestation length in weeks	38.97	39.94	40.28
Mother's age	29.50	31.02	32.05
Mother is married	0.60	0.63	0.65
Mother has a higher education	0.11	0.30	0.43

Notes: The table presents means for the given variables for the three samples that are defined by the propensity score for experiencing a discharge on the day of birth. For details consult section 5.3.

The first subset contains the observations with the 33 percent lowest propensity score for each year and parity group. This group has the lowest likelihood of experiencing a discharge on the day of birth based on pre-implementation data. In contrast, the group with the 33 percent largest propensity scores has highest likelihood of experiencing an early discharge.

To illustrate the composition and differences in observable characteristics of mothers and children in the propensity score groups, Table 5.2 shows covariate means for mother and child characteristics in the three subsamples. As expected, in the low-propensity score group children are least healthy and mothers are (on average) younger and less educated than the mothers in the other groups (and vice versa). These differences indicate that the propensity score divides our sample across relevant dimensions (with respect to mother and child characteristics that likely to matter for early discharge decisions). Furthermore, as Figure 5.3 shows, pre- and post-

[20] Specifically, we estimate the following probit model on all births in the treated counties *prior* to the introduction of mandatory discharge on the day of birth:

$$Prob(DBDitch = 1) = P_{itch} = \Phi(\alpha + \boldsymbol{\eta}\boldsymbol{X}_{itch})$$

where X_{itch} contains indicator variables for whether the mother was married, unemployed, in education, has a higher education and for 20 percent quantiles of birth weight, gestational length, and mother's age.

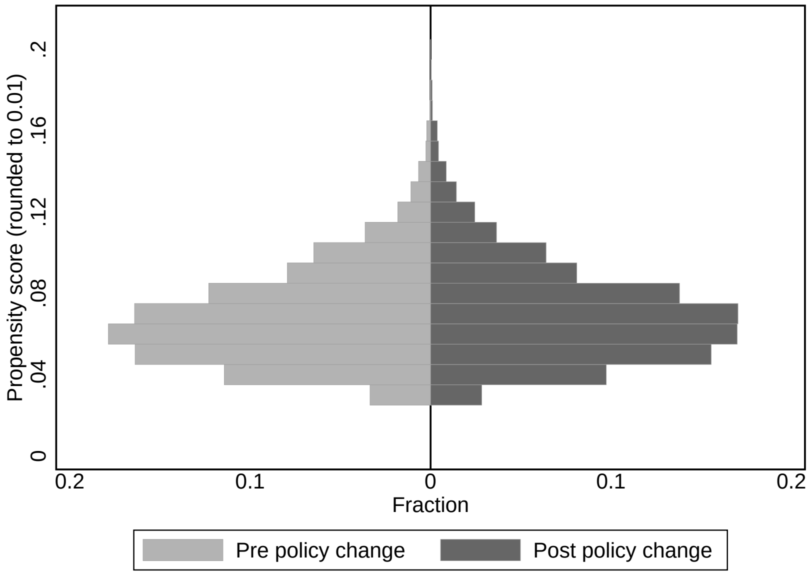


Figure 5.3: Common support, pre (control) and post (treated) policy change for multiparous mothers in Aarhus, Ringkøbing, Viborg, Vejle, and Ribe county.

policy change mothers are very similar in their distribution of the propensity scores. The figure suggests that the samples of mothers that we construct for the pre- and post-policy change are well balanced in terms of their observable characteristics (i.e., there is a common support for the propensity scores in the treated counties before and after the introduction of mandatory discharge on the day of birth).

Table 5.3 presents our first stage and 2SLS results for the three subsamples defined by their propensity score. As expected, the first stage is strongest for the high propensity group in all four subsamples. Overall we see no large heterogeneity of the very early effects (on readmission and first month GP contacts).

However, we find considerable heterogeneity in the effects of discharge on the day of birth on GP outcomes for both mothers and children in the first three years after birth. Especially the effects on school attainment is largest in absolute size and only significant for children in the lowest propensity score sample. For this group a discharge on the day of birth reduces the ninth grade GPA by 0.21 standard deviation (from a lower mean than the other two groups). Moreover, effects on both Danish and math scores are significantly larger and Danish is only significantly estimated for the low propensity score group, i.e., the group of children that prior to policy introduction had the lowest probability of being discharged on the day of birth, based on their characteristics.

Table 5.3: Heterogeneity of health and school achievement results.

Propensity score sample	0-33		34-66		67-100	
	FS	2SLS	FS	2SLS	FS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
A. Hospital Readmission Outcomes - Sample: 1985-2006						
Post × County	0.23***		0.26***		0.25***	
	(0.02)		(0.02)		(0.02)	
Child readmitted day ≤ 28		0.03**		0.04***		0.03*
		(0.01)		(0.01)		(0.02)
		[0.05]		[0.04]		[0.04]
Child readmitted day ≤365		-0.02		-0.02		-0.02
		(0.04)		(0.03)		(0.04)
		[0.22]		[0.20]		[0.19]
Mother readmitted day ≤28		0.01		0.04***		0.02
		(0.01)		(0.01)		(0.02)
		[0.02]		[0.02]		[0.02]
Mother readmitted day ≤365		-0.03		0.00		-0.02
		(0.03)		(0.03)		(0.03)
		[0.09]		[0.09]		[0.10]
B. General Practitioner Outcomes- Sample: 1997-2006						
Post × County	0.18***		0.26***		0.26***	
	(0.02)		(0.02)		(0.02)	
Child GP visits first month		0.51**		0.25*		0.46***
		(0.23)		(0.13)		(0.17)
		[0.39]		[0.38]		[0.37]
Child GP visits year 1		1.30		0.39		0.97
		(1.42)		(1.13)		(1.33)
		[9.73]		[8.84]		[8.22]
Child GP visits year 2		1.98		1.17		0.70
		(1.31)		(1.12)		(1.18)
		[9.69]		[8.95]		[8.34]
Child GP visits year 3		2.57**		0.56		1.54*
		(1.20)		(0.85)		(0.83)
		[5.89]		[5.47]		[5.25]
Mother GP visits first month		1.13***		1.08***		0.64***
		(0.28)		(0.20)		(0.22)
		[1.22]		[1.12]		[1.11]
Mother GP visits year 1		4.39***		1.95**		2.52***
		(1.17)		(0.95)		(0.84)
		[9.06]		[8.56]		[8.58]
Mother GP visits year 2		3.94***		0.53		1.15
		(1.14)		(0.94)		(1.16)
		[8.33]		[7.86]		[7.92]
Mother GP visits year 3		2.61*		-0.75		1.04
		(1.42)		(0.85)		(0.74)
		[8.22]		[7.72]		[7.83]
C. National Test Score Outcomes - Sample: 2000-2003						
Post × County	0.21***		0.28***		0.29***	
	(0.02)		(0.02)		(0.03)	
2nd grade Danish (st. score)		-0.31		-0.01		-0.06
		(0.24)		(0.15)		(0.17)
		[-0.12]		[-0.05]		[-0.04]
D. 9th grade GPA Outcomes - Sample: 1985-1995						
Post × County	0.26***		0.27***		0.27***	
	(0.03)		(0.03)		(0.03)	
9th grade GPA (st. score)		-0.21**		-0.07		0.02
		(0.10)		(0.09)		(0.09)
		[-0.21]		[-0.06]		[-0.02]
9th grade Math (st. score)		-0.19*		-0.03		0.09
		(0.10)		(0.09)		(0.10)
		[-0.16]		[-0.04]		[-0.04]
9th grade Danish (st. score)		-0.24**		-0.06		0.00
		(0.10)		(0.10)		(0.09)
		[-0.19]		[-0.04]		[-0.00]

Notes: Each cell presents estimates from separate regressions based on the three propensity score groups. Subsample means in square brackets. All models are with covariates, year and hospital fixed effects, and hospital specific quadratic trends. For details on model specification and table layout see the notes for Table 7.4. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

One concern with the analysis based on the propensity score is that we pool mother and child characteristics to determine the probability of being discharged on the day of birth. Thus we cannot speak to the question as to whether mother *or* child characteristics are important with respect to the heterogeneity that we observe. On the one hand, as many studies have illustrated the path dependency of health in childhood (see e.g., Case et al., 2005), children with poor initial health status may experience larger effects of discharge on the day of birth. On the other hand, recent studies emphasize the impact of parental responses to initial health conditions, thereby highlighting that parental background may be the most important factor accounting for heterogeneity of effects (see e.g., Almond and Currie, 2011; Cunha et al., 2013; Bernardi, 2014; Heckman and Mosso, 2014).

To distinguish between the impact of mother and child characteristics, we proceed by dividing our main sample into four groups defined by two dimensions: first, a propensity score as described above but now only calculated by incorporating maternal characteristics (education, income and age). We then restrict our attention to the top and bottom 33% of mothers as defined by this propensity score. Second, as a measure of child health at birth, we use an indicator for being small for gestational age (SGA). This measure captures infants' maturity at birth and is therefore a better proxy for initial health than measures such as birth weight.[21] Table B.1 in the Appendix shows the results of an analysis of the effects of being discharged on the day of birth in four subsamples defined by combinations of the mothers' propensity score and children's SGA status. Figure 5.4 illustrate this result graphically for child GPA.

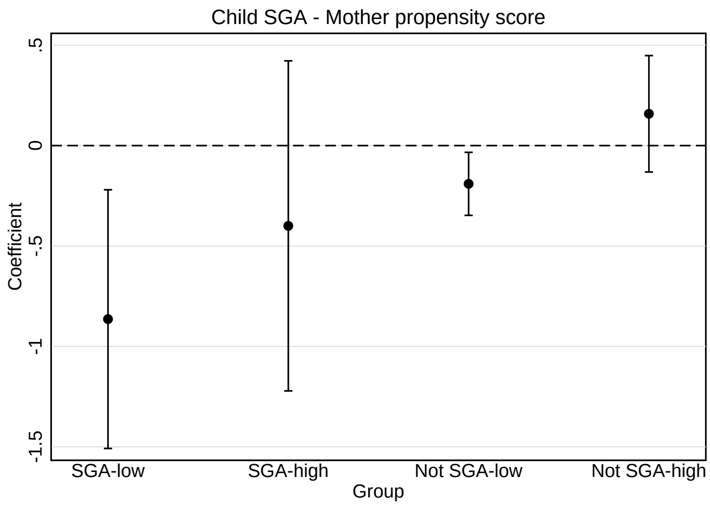


Figure 5.4: Heterogeneity of effects. Outcome: Child GPA.

Similar to our findings in the "pooled" heterogeneity analysis (with one combined maternal and child characteristics propensity score), we find no heterogeneity for child and mother read-

[21]We define an indicator for being SGA according to the sex-specific formula presented in (Marsal et al., 1996). Around 10 percent of children in our sample are classified as SGA.

missions when dividing our sample according to children's SGA status and maternal propensity score for experiencing a discharge on the day of birth. However, children who are SGA drive the GP results, i.e. children with an initial health disadvantage are the ones who experience lasting health effects. Moreover, mothers with a low propensity score (for experiencing a discharge on the day of birth) have larger GP effects irrespective of the SGA status of their child.

Note that the four groups in Appendix Table B.1 are not of equal size. Given that around 10 % of all infants are classified as SGA, confidence intervals for the groups defined as SGA are much wider. Nevertheless, there is evidence (illustrated in Figure 5.4) for mothers' characteristics rather than children's SGA status impacting the effect of discharge on the day of birth on children's longer-run school achievement. For these outcomes, only the estimates for children of low propensity score mothers are significantly estimated, large and negative. In other words, children of normal size at birth only experience a negative effect of discharge on the day of birth on school achievement, if their mothers have a low predicted probability of being discharged on the day of birth. We take this finding as a sign of the importance of parental response to policies impacting early life health for longer-run child schooling outcomes.

5.4 Mechanisms: Parental response

The analysis of the heterogeneous effects of a same-day discharge points to both the impact of infants' initial health status and the impact of parental background and potentially their responses for the effects of discharge on the day of birth on children's well-being. This section further explores mechanisms that may account for the longer-run effects of discharge on the day of birth that we find.

Prior evidence from small-scale studies on the Danish early discharge policies from the early 1990s may inform us on potential mechanisms for the effects that we have shown in the main analysis. These studies show that women who experienced mandated discharge on the day of birth perceived the breastfeeding support as inadequate to a higher degree than hospitalized mothers (Kierkegaard et al., 1992; Kierkegaard, 1993; Brinkmann, 2011). While these studies do not find differences in median breastfeeding duration, one study shows that early-discharged mothers were significantly less likely to breastfeed at four weeks after birth (Kierkegaard, 1993). Finally, early-discharged mothers appeared to have more telephone contacts and personal contacts to other health professionals than their controls. While all these previous studies face small sample sizes and only use control mothers from inside the same county, they indicate that parental self-confidence and parental early investments may have been impacted by mandated early discharge.

Table 5.4 shows results from an analysis of complementary survey data on mothers who gave birth between 1997-2003 and participated in the DNBC.[22] We use the full sample of mothers and children and complement this analysis with a heterogeneity analysis using a propensity score based on mother and child characteristics as in section 5.3 (Table 5.5). Given the smaller sample size in the DNBC data we do not divide the sample along child and mother characteristics sepa-

[22] Given the timing of the survey, we can only use the 2002 and 2003 policy changes for these analyses. Furthermore, earlier analyses indicate that the survey represents a somewhat positively selected sample of mothers (increasingly so for the post-birth survey waves) (Jacobsen et al., 2010).

rately. We account for the timing of the survey interviews by controlling for child's age in months (at interview scheduled for 18 month) and years (at interview scheduled for 7 years).

Table 5.4: Mechanisms: The effect of discharge on the day of birth on parental investments

	FS (1)	RF (2)	2SLS (3)	Mean
A. Danish National Birth Cohort Survey - Sample: 1997-2003; N: 40,414				
Post × County	0.24*** (0.02)			
At least four months exclusive breastfeeding		-0.04* (0.02)	-0.17* (0.10)	0.72
Received all scheduled 7 vaccines, 18 months		-0.00 (0.02)	-0.02 (0.06)	0.86
Food allergy, age 7		0.00 (0.01)	0.01 (0.02)	0.01
Asthma age 7, mother reported		-0.00 (0.02)	-0.01 (0.09)	0.13
Mother reported poor health, age 7		0.03*** (0.01)	0.14*** (0.03)	0.03
B. National Test Data - Sample: 2000-2003; N: 108,162				
Post × County	0.25*** (0.01)			
School starting age (Kindergarten)		0.03*** (0.01)	0.14*** (0.04)	6.26

Notes: Each cell shows point estimates from a separate regression. All models are with covariates, year and hospital fixed effects, and hospital-specific linear trends (except for panel B that includes quadratic hospital-specific trends). Column (1) provides the first stage coefficient from regressing an indicator for being discharged on the day of birth on an indicator for a birth in a treated county after the introduction of discharge on the day of birth policies. Column (2) provides the reduced form regression results from regressing the dependent variables on an indicator for a birth in a treated county after the introduction of same-day discharge policies. Column (3) shows the 2SLS estimates from regressing the dependent variables on an indicator for discharge on the day of birth. The included covariates are indicators for age at interview, low birth weight and preterm birth, mother's age at birth, employment status (indicators for being unemployed, self-employed, in education), taxable income and education level. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

In line with mothers' report of lack of breastfeeding support in earlier studies, we find that mothers that are discharged on the day of birth are less likely to breastfeed exclusively for at least four months.[23] At the mean of the dependent variable, early discharge decreases the probability of breastfeeding exclusively until month four by around 24 percent. Although the heterogeneity results for our survey outcomes should be interpreted with more caution due to the size and characteristics of our survey sample, Table 5.5 suggests that the negative effects are driven by the low propensity score group. Children from this group are less likely to be breastfed for four months. While this finding suggests that disadvantaged mothers reinforce the effect of lower health treatment at birth, parents from the high-propensity score group who experienced a same-day discharge, are actually more likely to breastfeed for four months than mothers in the same group who stayed in hospital longer. This finding –that suggests compensating behavior of those parents – may indicate that parents respond differentially to early health shocks.

[23] We chose this margin as the official recommendations in Denmark suggest a four months period of exclusive breastfeeding. From around four months, Danish visiting nurses encourage mothers to introduce solid food.

While discharge on the day of birth does not impact the probability of having received all scheduled vaccines at age 18 months in the analysis based on the full sample, Table 5.5 indicates that at-risk children are significantly less likely to have received all scheduled vaccines. Thus the increase in GP visits that we find in the main analysis (which is especially strong for poor health children) is most likely not driven by an increased number of planned well-baby visits, but reflects actually underlying health problems. Also for our vaccination measure, we find early-discharged mothers at the top of the propensity score distribution to be more likely to engage in these investments. Again this finding highlights important differences in parental responses to large-scale policies that decrease the access to default hospital postnatal care.

Finally, Tables 5.4 and 5.5 present the results from an analysis of administrative data for children's school starting age and data from the 7 year interview of the DNBC. We find no effects of discharge on the day of birth on indicators for diagnosed asthma or allergy, but we see some indication for heterogeneity in the prevalence of these conditions as well. Moreover, low propensity score mothers report a significantly lower level of child health in this interview.

Table 5.5: Heterogeneity of results for parental investments

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	0-33	0-33	34-66	34-66	67-100	67-100
A. Danish National Birth Cohort Survey - Sample: 1997-2003; N: 40,414						
Post × County	0.18^{***} (0.02)		0.25^{***} (0.02)		0.25^{***} (0.03)	
At least four months exclusive breastfeeding		-0.49^{*} (0.25) [0.67]		-0.29 (0.18) [0.73]		0.33^{*} (0.17) [0.77]
Received all scheduled 7 vaccines, 18 months		-0.37^{***} (0.13) [0.87]		0.21^{**} (0.09) [0.87]		0.31^{**} (0.13) [0.84]
Food allergy, age 7		-0.02 (0.05) [0.01]		0.06^{**} (0.03) [0.01]		-0.01 (0.04) [0.01]
Asthma age 7, mother reported		-0.29 (0.24) [0.15]		0.11 (0.10) [0.12]		-0.04 (0.12) [0.12]
Mother reported poor health, age 7		0.26^{***} (0.10) [0.03]		0.03 (0.05) [0.02]		0.02 (0.05) [0.02]
B. National Test Data - Sample: 2000-2003; N: 108,162						
Post × County	0.21^{***} (0.02)		0.28^{***} (0.02)		0.29^{***} (0.03)	
School starting age (Kindergarten)		0.10 (0.08) [6.27]		0.16^{***} (0.06) [6.26]		0.15^{**} (0.06) [6.26]

Notes: Each cell presents estimates from separate regressions based on the three propensity score groups. Subsample means in square brackets. All models are with covariates, year and hospital fixed effects and hospital specific linear trends (except for panel B that includes quadratic hospital-specific trends). For details on model specification and table layout see the notes for Tables 5.3 and 5.4. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

In line with other research from Denmark, we find that parents with higher socio-economic status may actively use their discretion with respect to their children's age at school start: early-discharged children from the high propensity score groups are older when they enter Kindergarten. This finding may account for the lack of a long-run effect of being discharged on the day of birth on test scores in the sample of advantaged children.

These findings taken together suggest that some parents actively compensate while others appear to reinforce early life health shocks. Parental investments in the low propensity score group appear to decrease as a response to a discharge on the day of birth, but the same is not true in the high propensity score group. These results suggest that a lack of postnatal hospital care for disadvantaged parents decreases their subsequent investments. This decrease could be due to a lack of knowledge, skills or self-confidence.

5.5 Additional robustness tests

Table 5.6 presents the results of robustness tests that assess the impact of the different samples and the functional form chosen for the results of our main analyses. All cells present estimates from separate 2SLS regressions. In general, we find rather stable estimates for the impact of mandated discharge on the day of birth on early child readmission, mother and child GP visits in the first month and year, and child school achievement measured as GPA at age 15. In contrast to our main results, some of our findings indicate that discharge on the day of birth also increases mothers' probability of readmission in the first 28 days.

To test whether the early or late policy changes have differential impact on child and mother outcomes, we perform our analysis on data only around the early and late wave of introduction of early discharge policies (columns (1) and (2)). Given that we use few years of data, we only include linear trends (interacted with birth hospital) in these regressions. Overall the conclusions are very similar to the main results.

Column (3) in Table 5.6 shows that excluding mother and child control variables has negligible effect on the results.[24] Assessing the robustness of our main findings to different polynomials in birth year (linear and cubic trends), we show that our results are in general robust to the specification of trends (columns (4) and (5) of Table 5.6). Additionally, our results are very similar when we use the policy changes as five separate instruments (column 6).

To rule out that county-specific additional policy changes or shocks account for our findings, we next use primiparous mothers as an additional control group in a triple-differences specification (column 7). This specification compares differences in outcomes between primi- and multiparous mothers before and after the policy changes across treated and control counties. Across outcomes we find very similar results to the ones obtained by the simpler differences in differences version of our model. This finding indicates that differential trends across treated and untreated counties due to factors such as other parallel policies or changes in the counties' population of mothers do not appear to play an important role.[25]

Column (8) in Table 5.6 defines the length of the post-birth hospital stay using mother (and not child) records from the Inpatient Register. This change to the variable definition does not significantly change our main conclusions. In column (9), we constrain our analysis samples to uncomplicated multiparous singleton births as defined in section 4. We expect this group to be

[24] Moreover, Table B.3 in the Appendix confirms that the policy changes in the given counties do not impact mother and child characteristics. Using several mother and child observables as outcomes, we do not find significant effects of the introduction of mandated discharge on the day of birth.

[25] Appendix table B.4 shows first stage and reduced form results for first time mothers (placebo). We see no first stage and very few significant reduced form results, as expected.

Table 5.6: Robustness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Early	Late	No controls	Linear	Cubic	5 instr	DIDID	Mother	Uncomp	2nd born	Birth reg	Nights
Child readmitted day ≤ 28	0.02***	0.07***	0.04***	0.04***	0.04***	0.03***	0.03***	0.04***	0.03***	0.04***	0.02***	-0.01***
	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Child readmitted day ≤365	0.02	0.11	-0.02	-0.03**	-0.01	-0.03	0.03	-0.03	-0.03	-0.03	-0.02	0.00
	(0.02)	(0.07)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.00)
Mother readmitted day ≤28	0.01	0.04**	0.02*	0.01	0.02*	0.02	0.04***	0.03	0.03*	0.02	0.01	-0.00
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)
Mother readmitted day ≤365	0.05**	0.01	-0.03	-0.06***	-0.03	-0.03	0.01	-0.04	-0.01	-0.03	-0.02	0.01
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.00)
Child GP visits first month		0.82***	0.40***	0.27*	0.38***	0.33**	0.40***	0.47***	0.48***	0.39**	0.39***	-0.08***
		(0.16)	(0.15)	(0.15)	(0.15)	(0.13)	(0.13)	(0.18)	(0.13)	(0.15)	(0.14)	(0.03)
Child GP visits year 1		6.68***	0.93	-0.74	0.45	1.03	3.43***	0.75	0.62	0.03	0.61	-0.13
		(0.79)	(1.09)	(1.04)	(0.99)	(0.97)	(1.09)	(1.18)	(0.93)	(0.99)	(0.96)	(0.22)
Child GP visits year 2		1.97***	1.37	-0.13	1.06	1.65**	-0.40	1.41	0.75	0.91	1.16	-0.25
		(0.75)	(0.84)	(0.95)	(0.77)	(0.81)	(1.07)	(0.93)	(0.73)	(0.80)	(0.76)	(0.17)
Child GP visits year 3		1.75***	1.47*	0.28	1.25**	1.63**	-0.94	1.62**	0.66	1.43**	1.32**	-0.28**
		(0.50)	(0.82)	(0.74)	(0.56)	(0.65)	(0.75)	(0.70)	(0.61)	(0.72)	(0.58)	(0.13)
Mother GP visits first month		0.44***	1.07***	0.94***	0.95***	0.89***	0.70***	1.15***	0.89***	1.08***	0.95***	-0.20***
		(0.16)	(0.18)	(0.14)	(0.15)	(0.14)	(0.18)	(0.19)	(0.19)	(0.18)	(0.15)	(0.04)
Mother GP visits year 1		2.73***	3.07***	3.48***	2.85***	3.07***	3.42***	3.45***	1.79***	2.53***	2.83***	-0.61***
		(0.90)	(0.66)	(0.68)	(0.67)	(0.64)	(0.84)	(0.81)	(0.62)	(0.66)	(0.66)	(0.18)
Mother GP visits year 2		0.71	1.96**	1.74**	1.88**	2.27**	2.17**	2.27**	1.21	2.12*	1.86**	-0.40**
		(0.51)	(0.87)	(0.72)	(0.86)	(0.90)	(0.85)	(1.04)	(0.80)	(1.12)	(0.85)	(0.19)
Mother GP visits year 3		-0.62	1.27*	0.14	0.85	0.86	1.55**	1.15	0.50	0.86	0.94	-0.20
		(0.55)	(0.70)	(0.76)	(0.64)	(0.59)	(0.79)	(0.77)	(0.60)	(0.93)	(0.63)	(0.13)
2nd grade Danish (standard scores)		-0.19	-0.25***	-0.06	-0.17*	-0.15*	-0.21*	-0.19*	-0.08	-0.09	-0.16*	0.04*
		(0.16)	(0.07)	(0.12)	(0.09)	(0.09)	(0.12)	(0.11)	(0.08)	(0.08)	(0.09)	(0.03)
9th grade GPA (standard score)	-0.10*		-0.12**	-0.05	-0.13**	-0.11**	-0.15*	-0.15**	-0.11*	-0.16**	-0.08*	0.02**
	(0.05)		(0.06)	(0.06)	(0.06)	(0.05)	(0.09)	(0.07)	(0.06)	(0.06)	(0.04)	(0.01)
9th grade Math (standard score)	-0.13***		-0.08	-0.04	-0.09*	-0.04	-0.08	-0.10	-0.06	-0.12*	-0.05	0.01
	(0.05)		(0.06)	(0.06)	(0.05)	(0.05)	(0.09)	(0.07)	(0.06)	(0.07)	(0.04)	(0.01)
9th grade Danish (standard score)	-0.12**		-0.13**	-0.07	-0.14**	-0.13**	-0.16*	-0.17**	-0.13**	-0.18***	-0.09**	0.02**
	(0.06)		(0.06)	(0.06)	(0.06)	(0.06)	(0.08)	(0.08)	(0.06)	(0.06)	(0.04)	(0.01)
Sample	89-93	01-03	Full	Full	Full	Full	Full	Full	Full	Full	Full	Full
Covariates	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Hospital fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Hospital-specific linear trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Hospital-specific quadratic trends			✓		✓	✓	✓	✓	✓	✓	✓	✓
Hospital-specific cubic trends					✓							
Trends and fixed effects × parity							✓					

Notes: Each cell presents the estimates from a separate regression. In Model (8) discharge on the day of birth is defined by the duration of the mother's stay. Model (9) only includes uncomplicated births. Uncomplicated birth is defined as: birth weight ≥ 2,500g; gestation length ≥ 37weeks; mother's age ≥ 18years; only singletons; no birth complications. Model (10) shows results using a dataset that only includes second born children. Model (11) shows results using hospital stay at birth from birth registers (where direct readmissions are counted as separate hospital spells). Model (12) shows the results using number of nights as the treatment (instead of an indicator for whether number of nights is zero). Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

the one closest to a group of compliers with the new policy. Also in this sample we confirm our main findings, although we see slightly less precise results for the schooling outcomes (as our heterogeneity analyses would suggest). Another strategy to make the sample more homogenous is to only consider second born children. Column (10) shows that this sample restriction has little impact on the main conclusions.

As noted in the data section hospital stays from birth records are available for the post 1990 period. Using information on postpartum hospital stay from birth registers instead of information from Inpatient Registers makes some of the results more precise, as column (11) reveals. Finally in column (12) we show regression results from a specification where the treatment is number of postpartum hospital nights (instead of a binary indicator for zero nights as in all other specifications). The conclusion is in line with the main specification: Longer postpartum hospital stays causes less GP contacts and better primary school GPA.

A final concern in our analysis is that multiparous mothers in treated counties may have reacted to the introduction of mandated discharge on the day of birth. As described in section 2, Danish mothers can choose their hospital of birth. If mothers choose to give birth in other counties as a reaction to the introduction of discharge on the day of birth, our estimates may be biased. Additionally, we may see effects on the number of mothers who choose a home birth and thus do not appear in our data. Appendix Figures B.4 and B.5 do not support these suggestions. Thus we conclude that these factors do not impact our conclusions.

6. Conclusion

In this paper we exploit the county-by-county introduction of the shortest possible postpartum hospital stay – mandatory discharge on the day of birth – for multiparous mothers to estimate its causal effect on mother and child health and well-being. We find significant effects of discharge on the day of birth on child hospital readmission in the first 28 days as well as on the number of child and mother general practitioner examinations in the first years of life. While the short-run readmission results may indicate that the health effects merely reflect substitution of default postpartum hospital care with other postpartum care, we find that especially at-risk populations of children experience lasting health effects – measured as general practitioner contacts and mother-reported health at age seven– and negative effects on test scores at age 15.

Examining data on parental investments and examining the heterogeneity of effects across children of different initial health conditions, we show that both path dependency of health status and parental responses account for these longer-run effects. The finding that the infants in worst initial health have the strongest effects for measures of health care usage in childhood supports the idea that early life health impacts health during childhood. The finding that maternal characteristics matter for the strength of the effect on school achievement points to the crucial role of parental responses to early life health in explaining longer-run consequences for child well-being. We find that parental responses (such as breastfeeding, well-baby visits to GPs, and delaying school enrollment) may explain some of the longer-run effects that we find. Thus while our findings are in line with recent research documenting effects of early life health interventions on longer-run educational outcomes (Bharadwaj et al., 2013), we point to the importance of parental responses, that appear to matter as mechanisms for these effects.

Mandated early discharge policies for large parts of the population may come at significant costs if they are not combined with targeted interventions towards at-risk children and mothers. Thus when evaluating the cost effectiveness of postpartum care, researchers and policy makers should consider longer-run consequences. Health interventions have the potential to significantly impact parental investments, such as breastfeeding. Depending on whether this impact is due to their effect on parental knowledge, parenting skills or self-confidence, it gives rise to rather different policy implications. Thus, future research should consider in greater detail the impact of postpartum care on parents' behaviors.

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Appendices

A. Data sources and data structure

Table A.1: Data selection

		Observations
All births 1985-2006		1,407,272
Not singletons	-47,794	
Primiparous	-605,096	
Not born at a hospital	-23,858	
(1) Full sample		730,524
(2) GP and diagnoses sample (born 98-06)		347,111
(3) GPA sample (completed 9th grade 02-12)		316,146
(4) 2nd grade test score sample (attended 2nd grade 09-12)		108,162
(5) DNBC survey (Born 1997-2003)		40,414

B. Additional figures and tables

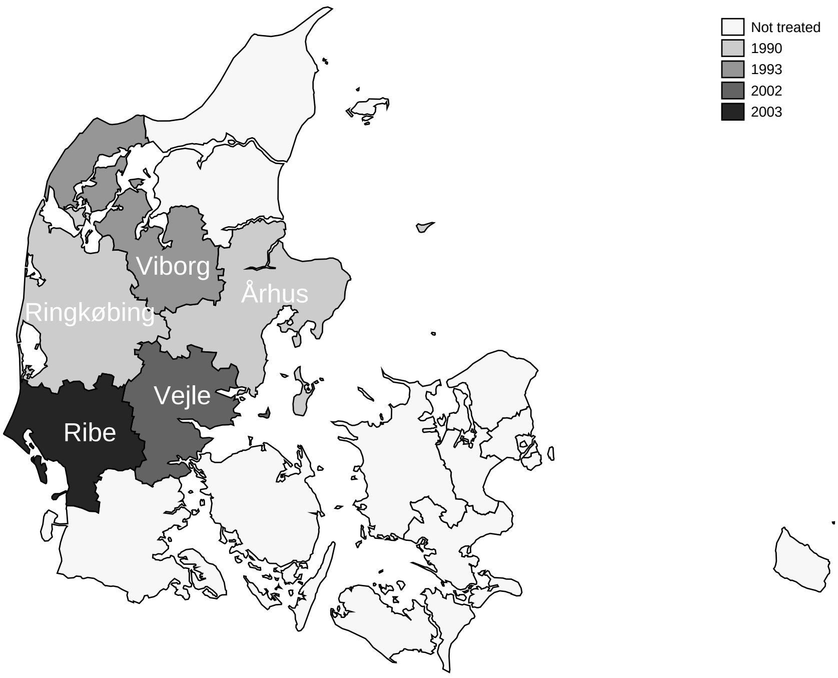


Figure B.1: Introduction of discharge on the day of birth policies across Danish counties.

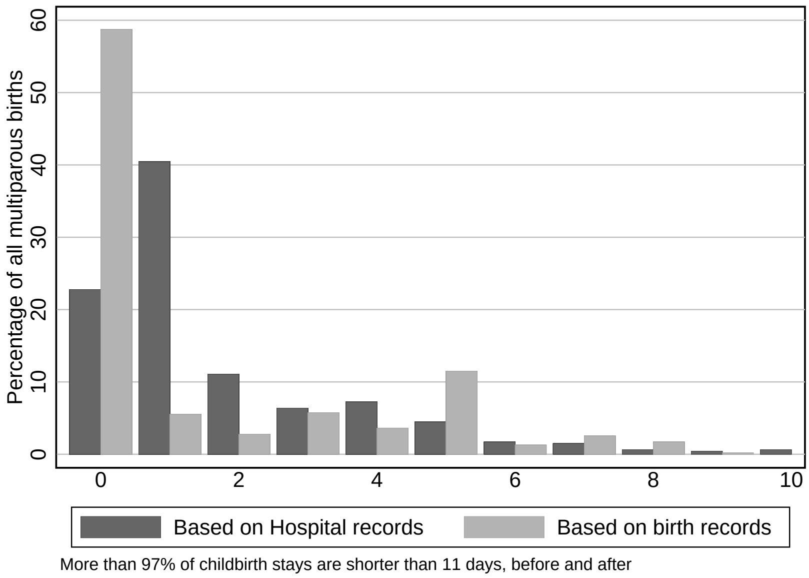
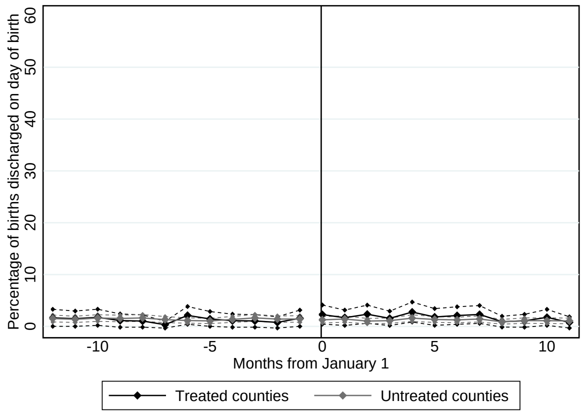
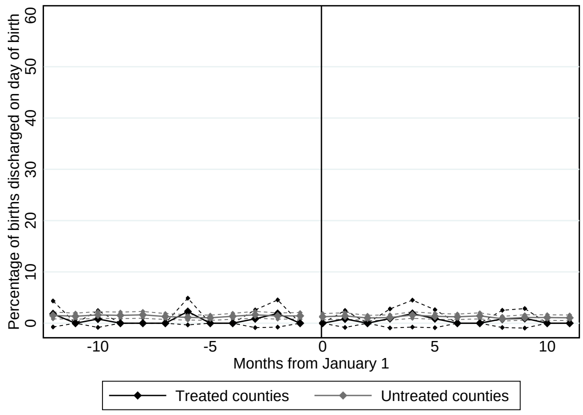


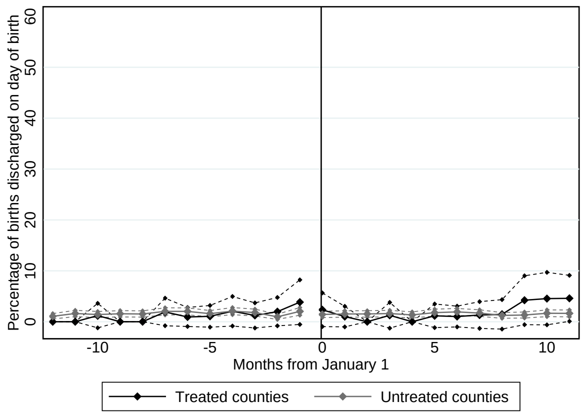
Figure B.2: Post-policy distribution of postpartum hospital stay length for multiparous mothers in Viborg county. The histogram includes data for births in the first year after the policy change.



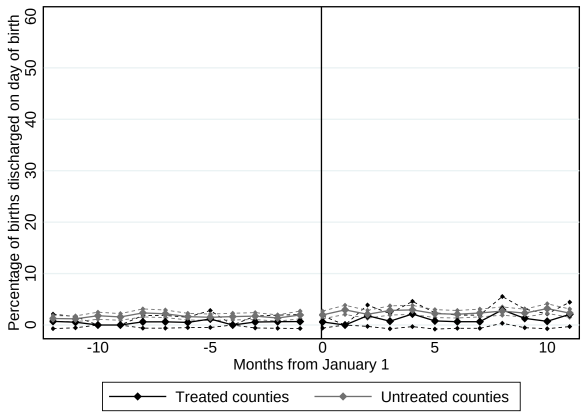
(a) Aarhus county 1990 (Ringkøbing county is excluded)



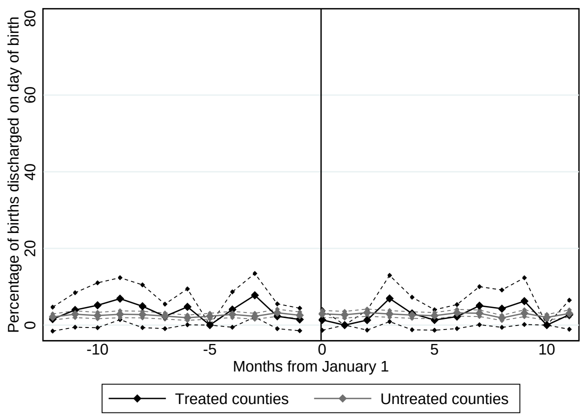
(b) Ringkøbing county 1990 (Aarhus county is excluded)



(c) Viborg county 1993

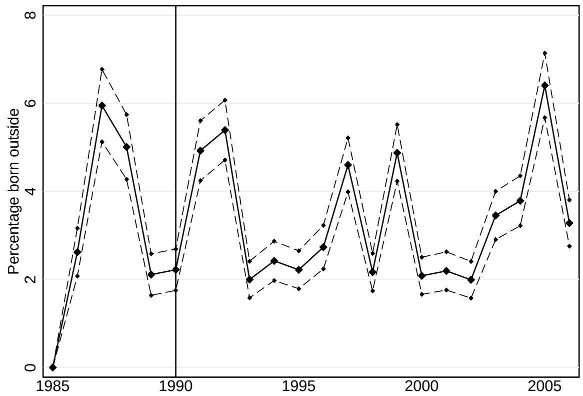


(d) Vejle county 2002

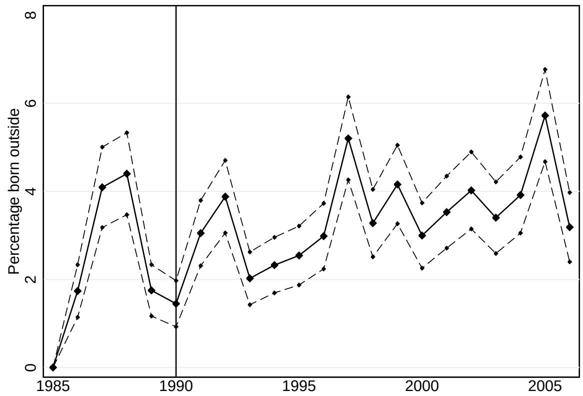


(e) Ribe county 2003

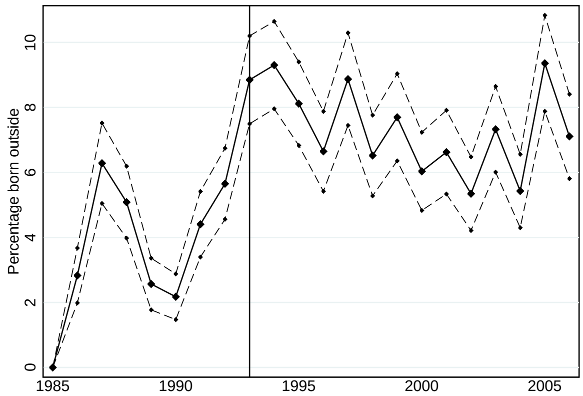
Figure B.3: Discharge on the day of birth rates for first-time mothers in treated and control counties, monthly bins. The control counties include all other counties, including the counties that were treated at a different point in time.



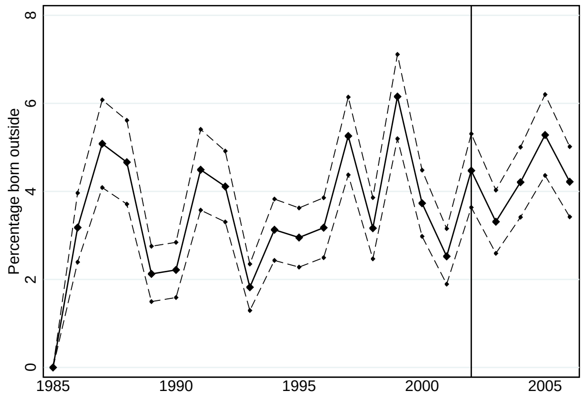
(a) Aarhus county 1990



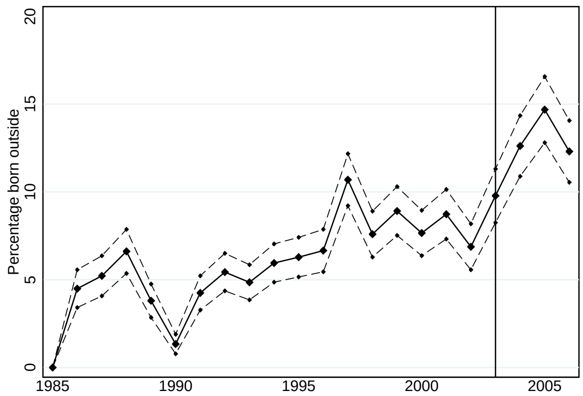
(b) Ringkøbing county 1990



(c) Viborg county 1993

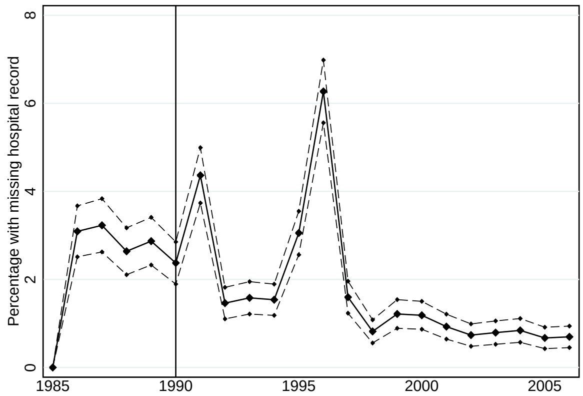


(d) Vejle county 2002



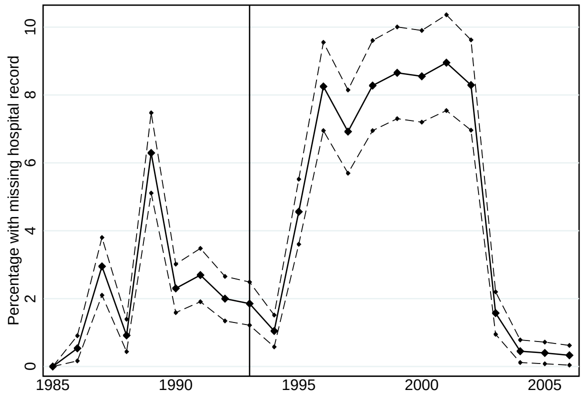
(e) Ribe county 2003

Figure B.4: Born outside home county. Each diamond shows the share of children born by multiparous mothers, who were born in a hospital outside the home county.

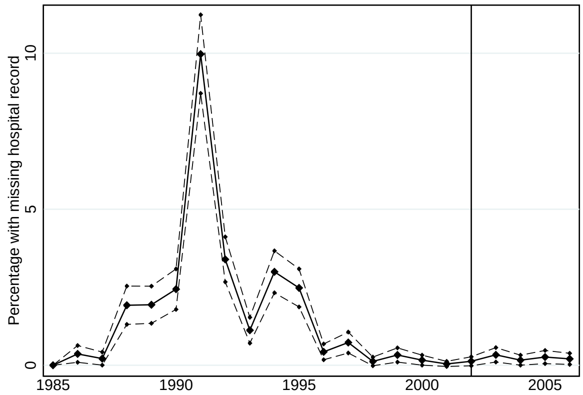


(a) Aarhus county 1990

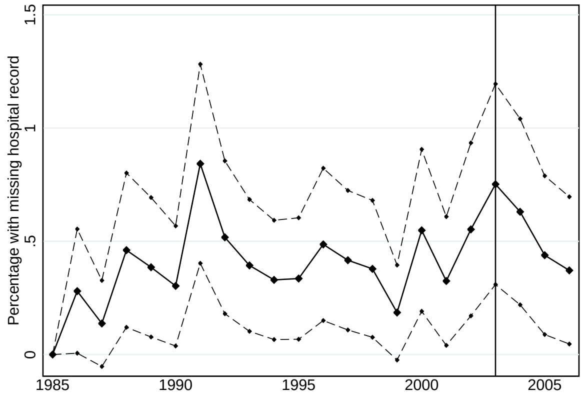
(b) Ringkøbing county 1990



(c) Viborg county 1993



(d) Vejle county 2002



(e) Ribe county 2003

Figure B.5: Missing birth hospital. Each diamond shows the share of children born by multiparous mothers, with missing birth records (usually due to home births).

Table B.1: Heterogeneity of health and school achievement results: SGA measure and mother propensity score groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SGA	Yes	Yes	Yes	Yes	No	No	No	No
Mother SES	Low	Low	High	High	Low	Low	High	High
A. Hospital Readmission Outcomes - Sample: 1985-2006								
Post × county	0.19***		0.19***		0.27***		0.22***	
	(0.02)		(0.02)		(0.02)		(0.02)	
Child readmitted day ≤ 28		0.03		-0.00		0.01		0.04**
		(0.06)		(0.08)		(0.01)		(0.02)
		[0.04]		[0.05]		[0.04]		[0.05]
Child readmitted day ≤365		0.06		-0.23		0.00		-0.02
		(0.13)		(0.15)		(0.03)		(0.04)
		[0.25]		[0.27]		[0.20]		[0.21]
Mother readmitted day ≤28		-0.08*		0.04		0.02		0.02
		(0.05)		(0.06)		(0.01)		(0.02)
		[0.02]		[0.03]		[0.02]		[0.02]
Mother readmitted day ≤365		-0.07		-0.02		-0.00		-0.04
		(0.10)		(0.11)		(0.03)		(0.04)
		[0.10]		[0.12]		[0.09]		[0.10]
Observations	23,312		23,152		207,846		207,846	
B. General Practitioner Outcomes- Sample: 1997-2006								
Post × county	0.17***		0.14***		0.26***		0.22***	
	(0.05)		(0.04)		(0.02)		(0.02)	
Child GP visits first month		2.62***		1.16		0.10		0.52**
		(0.87)		(0.90)		(0.14)		(0.20)
		[0.35]		[0.31]		[0.41]		[0.37]
Child GP visits year 1		2.84		3.55		0.40		-0.09
		(4.16)		(7.50)		(1.25)		(1.55)
		[9.88]		[8.68]		[9.73]		[8.29]
Child GP visits year 2		8.40*		14.13**		1.36		0.85
		(4.68)		(6.21)		(1.06)		(1.58)
		[9.94]		[8.84]		[9.64]		[8.34]
Child GP visits year 3		6.33		6.35		0.68		1.89*
		(3.94)		(5.73)		(0.86)		(1.07)
		[6.07]		[5.82]		[5.86]		[5.30]
Mother GP visits first month		1.03		1.47		1.00***		0.74**
		(1.04)		(1.24)		(0.22)		(0.29)
		[1.10]		[1.11]		[1.17]		[1.16]
Mother GP visits year 1		4.65		-5.39		3.38***		2.79**
		(5.41)		(8.74)		(0.74)		(1.42)
		[8.98]		[9.72]		[8.90]		[9.03]
Mother GP visits year 2		13.75**		-14.92		1.21		1.90
		(6.64)		(10.04)		(0.85)		(2.19)
		[8.60]		[9.42]		[8.27]		[8.35]
Mother GP visits year 3		8.34		-6.64		1.46*		0.67
		(7.04)		(9.36)		(0.76)		(1.42)
		[8.42]		[9.26]		[8.13]		[8.27]
Observations	10,092		10,774		100,214		100,214	
C. National Test Score Outcomes - Sample: 2000-2003								
Post × county	0.26***		0.22***		0.26***		0.31***	
	(0.04)		(0.05)		(0.02)		(0.02)	
2nd grade Danish (st. score)		-0.41		-0.07		0.10		0.18
		(0.56)		(0.55)		(0.18)		(0.22)
		[-0.33]		[-0.37]		[-0.17]		[-0.07]
Observations	3,272		3,039		30,359		30,359	
D. 9th grade GPA Outcomes - Sample: 1985-1995								
Post × county	0.21***		0.20***		0.30***		0.24***	
	(0.02)		(0.03)		(0.03)		(0.03)	
9th grade GPA (st. score)		-0.86***		-0.40		-0.19**		0.16
		(0.33)		(0.42)		(0.08)		(0.15)
		[-0.40]		[-0.33]		[-0.21]		[-0.02]
9th grade Math (st. score)		-0.29		-0.23		-0.14*		0.11
		(0.34)		(0.42)		(0.08)		(0.17)
		[-0.39]		[-0.39]		[-0.15]		[-0.04]
9th grade Danish (st. score)		-1.04***		-0.30		-0.20**		0.13
		(0.34)		(0.42)		(0.08)		(0.15)
		[-0.33]		[-0.27]		[-0.19]		[-0.01]
Observations	10,533		8,968		86,428		86,428	

Notes: Each cell shows the estimates from a separate regression. All models are with covariates, year and hospital fixed effects, and hospital-specific quadratic trends. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

Table B.2: Regression results - readmission by diagnoses and post birth complications

	FS (1)	RF (2)	2SLS (3)	Mean
Post × County	0.24^{***} (0.02)			
Mother post birth complications		0.01 (0.01)	0.06 (0.04)	0.10
Child: Dehydration/Hypernatremia day ≤ 28		-0.00 (0.00)	-0.00 (0.00)	0.00
Child: Well-being, day ≤ 28		0.00 (0.00)	0.00 (0.00)	0.00
Child: Jaundice day ≤ 28		-0.00 (0.00)	-0.02 (0.02)	0.03
Child: Breastfeeding, day ≤ 28		-0.00 (0.00)	-0.00 (0.00)	0.00
Child: Nutrition, day ≤ 28		-0.00 (0.00)	-0.00 (0.01)	0.01
Child: Nutrition-related diagnoses, hospital adm. day ≤ 28		-0.00 (0.01)	-0.02 (0.02)	0.03

Notes: Each cell shows the estimates from a separate regression. All models are with covariates, year and hosptial fixed effects and hospital specific linear trends. Column (1) provides the first stage coefficient from regressing an indicator variable for outpatient birth on an indicator variable for giving birth in a treated county after the county introduced mandated outpatient birth. Column (2) provides the reduced form regression results from regressing the dependent variables on an indicator variable for outpatient birth on an indicator variable for giving birth in a treated county after the county introduced mandated outpatient birth. Column (3) gives the 2SLS estimates from regressing the dependent variables on an indicator for outpatient birth. The included covariates are indicators for low birth weight and preterm birth, mother's age at birth, employment status (indicators for being unemployed, self-employed, in education), taxable income and education level. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

Table B.3: Robustness test: Placebo outcome regressions.

	(1)	(2)	(3)
Low birth weight	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)
Preterm birth	-0.00	0.00	-0.00
	(0.00)	(0.01)	(0.00)
Mother has a higher education	-0.01	-0.02**	-0.00
	(0.00)	(0.01)	(0.01)
Mother's age <18	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)
Mother was married	-0.00	0.01	-0.01
	(0.01)	(0.01)	(0.01)
Mother was unemployed	-0.00	0.01	0.00
	(0.00)	(0.01)	(0.01)
Mother was self-employed	-0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)
Mother was in education	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)
Taxable income (1,000 DKK)	0.91	-0.73	1.63
	(0.79)	(1.20)	(1.19)
C-section		0.01*	
		(0.01)	
Breech		0.00	
		(0.00)	
APGAR cont at 5 min		0.02	
		(0.02)	
Sample	Full	GP	GPA

Notes: Each cell presents the estimates from a separate regression. All models are with covariates, year and hospital fixed effects and hospital-specific quadratic trends. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

Table B.4: The effect of discharge on the day of birth on health and schooling outcomes - First-time mothers

	FS (1)	RF (2)	2SLS (3)	Mean
A. Hospital Readmission Outcomes - Sample: 1985-2006; N: 589,338				
Post × County	-0.00 (0.00)			
Child readmitted day ≤ 28		0.00 (0.00)	-0.38 (0.89)	0.04
Child readmitted day ≤365		-0.01** (0.00)	3.58 (3.39)	0.19
Mother readmitted day ≤28		-0.00 (0.00)	0.97 (1.13)	0.02
Mother readmitted day ≤365		-0.01* (0.00)	2.75 (2.47)	0.08
B. General Practitioner Outcomes- Sample: 1997-2006; N: 265,897				
Post × County	-0.01* (0.00)			
Child GP visits first month		0.01 (0.03)	-0.85 (4.60)	0.37
Child GP visits year 1		-0.69** (0.30)	91.67 (78.66)	9.64
Child GP visits year 2		0.35 (0.28)	-46.17 (44.72)	11.59
Child GP visits year 3		0.59*** (0.16)	-78.80 (54.76)	6.76
Mother GP visits first month		0.07 (0.05)	-9.67 (6.95)	1.10
Mother GP visits year 1		-0.12 (0.15)	15.66 (24.32)	8.57
Mother GP visits year 2		-0.11 (0.19)	15.18 (25.26)	8.66
Mother GP visits year 3		-0.17 (0.17)	22.15 (25.59)	9.05
C. National Test Score Outcomes - Sample: 2000-2003; N: 84,353				
Post × County	-0.01* (0.01)			
2nd grade Danish (standard scores)		-0.02 (0.03)	2.05 (3.82)	0.09
D. 9th grade GPA Outcomes - Sample: 1985-1995; N: 269,303				
Post × County	0.00 (0.00)			
9th grade GPA (standard score)		0.01 (0.02)	2.61 (4.30)	0.08
9th grade Math (standard score)		0.01 (0.02)	1.41 (4.82)	0.08
9th grade Danish (standard score)		0.01 (0.01)	1.78 (3.91)	0.08

Notes: Each cell shows the estimates from a separate regression. All models are with covariates, year and hospital fixed effects, and hospital-specific quadratic trends. Column (1) provides the first stage coefficient from regressing an indicator for discharge on the day of birth on an indicator for a birth in a treated county after the introduction of mandated discharge on the day of birth. Column (2) provides the reduced form regression results from regressing the dependent variables on an indicator for discharge on the day of birth on an indicator for a birth in a treated county after introduction of mandated discharge on the day of birth. Column (3) shows the 2SLS estimates from regressing the dependent variables on an indicator for discharge on the day of birth. The included covariates are indicators for low birth weight and preterm birth, mother's age at birth, employment status (indicators for being unemployed, self-employed, in education), taxable income and education level. Standard errors clustered on the hospital times year level in parenthesis. * p<0.1, ** p<0.05, *** p<0.01.

CHAPTER 2

SCHOOL STARTING AGE AND NON-COGNITIVE SKILLS†

Thomas S. Dee
Graduate School of Education, Stanford University & NBER

Hans Henrik Sievertsen
The Danish National Centre for Social Research (SFI) & The Department of Economics, University of Copenhagen

Abstract

An increasing number of parents, particularly those with socioeconomic advantages, have chosen to delay the school starting age of their children (i.e., academic "red-shirting"). However, a growing body of carefully identified empirical evidence provides mixed evidence on the conjectured human-capital benefits of such delays with much of the seeming benefits due merely to a delayed age at which tests are taken or to the incapacitation of older youth from risky behaviors. This study presents new evidence on whether school starting age influences student outcomes by relying on linked Danish survey and register data that include several distinct measures of non-cognitive skills measured contemporaneously, with regard to age, for young students who may be in different grades. This study identifies the causal effects of delayed school starting ages using a regression discontinuity design based on exact dates of birth and the fact that children typically enroll in school during the calendar year in which they turn six. We find that a delayed school start dramatically reduces hyperactivity, a measure with strong negative links to student achievement. However, the estimated effects on non-cognitive dimensions with weaker links to student achievement (emotion, conduct, peer relations, and social skills) are small and statistically insignificant. The effects are driven by girls with a low level of latent ability.

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1. Introduction

Delaying school entry of their children – also known a academic "red-shirting" – is becoming increasingly prevalent, particularly among socioeconomic advantaged parents.[1] According to the U.S. National Center for Education Statistics six percent of all school entrants in fall 2010 were delayed, and data from Statistics Denmark reveals that in Denmark one out of five boys and one out of ten girls have a delayed school start.[2] However, the gains of postponing school enrollment to boost the child's development have recently been questioned both in research and in the public debate (The New Yorker, 2013). While older school starting age lowers the propensity to commit crime (Landersø et al., 2013) and the risk of teenage pregnancy (Black et al., 2011), both effects are driven by incapacitation rather than human capital effects. Moreover, the positive effects of school starting age on tests in primary school (Bedard and Dhuey, 2006) seem to be dominated by age-at-test effects, as Black et al. (2011) show using post schooling test scores. Research on the causal evidence on a direct effect of delaying school entry on children's skills suffers from the fact that the link between school starting age and primary school test scores is "fundamentally unanswerable" (Black et al., 2011), as it is empirically impossible to control for the fact that children who are older at enrollment also are older when tested within school.

Using Danish data we provide evidence on the causal effect of school starting age on children's human capital, by using out-of-school measures of non-cognitive skills and holding age constant.[3] We focus on non-cognitive skills because these skills (1) are less dependent on grade (which has to vary, when age is held constant), (2) are important for the children's ability to acquire cognitive skills, and (3) are important for later life outcomes in terms of crime, health, and the labor market (Cunha et al., 2006). The study most similar to ours is by Elder and Lubotsky (2009), who show that older school starting age causes a reduced likelihood of an ADHD diagnosis, but these effects may be driven by a relative age effect.

Denmark provides an excellent case for evaluating the direct effects of school starting age for three reasons: One, the universal day care system and the centrally specified school program constitute homogeneous control and treatment environments.[4] Two, the existence of a discontinuity in expected school starting age created by the rule that Danish children are supposed to enter school the calendar year they turn six. Three, the availability of register-based data linked

[1]According to the U.S. National Center for Education Statistics 14% of the children who delayed school entrance in 2010 were children of parents in the lowest 20% group according to socioeconomic status, while 24% were children of parents in the highest 20% socioeconomic status group. Socioeconomic status is measured based on parental education, occupation, and household income at the time of data collection.

[2]See Appendix Figure A.1 for the development of red-shirting in Denmark. Throughout this paper we refer to school enrollment as the year the child enters kindergarten, which in Danish is referred to as grade zero or "Børnehaveklasse".

[3]We are only aware of one other study attempting to exploit out-of-school measures of skills, holding age constant: Mühlenweg et al. (2012) use a sample of 360 children from the German Rhine-Neckar region to show that later school starting age is related to being less hyperactive at age eight. They exploit the panel structure of their data to compensate for the issue that variation in school starting age is only obtained through variation in birth date as they have no variation in cutoff dates.

[4]According to Statistics Denmark more than 95 percent of a cohort is in daycare. In the US, in contrast, 27 percent of the delayed school entrants in 2010 were not in a non-parental arrangement according to the US National Center for Education Statistics.

to a large survey on 55,000 children's skills measured in terms of the Strength and Difficulties Questionnaire (SDQ) (Goodman, 1997). The SDQ provides information on children's non-cognitive skills on five dimensions: conduct, emotional symptoms, hyperactivity, peer problems, and pro-social behavior. Linking the SDQ scores to the children's later performance in tests of cognitive skills we find that especially the hyperactivity scale is closely related to the test performance in mathematics and Danish.

Using a fuzzy regression discontinuity design we find that older school starting age causes better non-cognitive skills at age seven measured by the aggregated SDQ score. The effects are driven by better outcomes on the hyperactivity scale of the SDQ, while the other dimensions are unaffected. The results thus show that school starting age affects a dimension of non-cognitive skills that is important for the performance in tests of cognitive skills. Assessing the heterogeneity in the treatment effects by means of marginal treatment effects reveals that the average effect is driven by girls with a low level of latent ability.

This paper proceeds as follows: Section two gives a brief summary of the related literature. Section three describes the Danish institutional setting. Section four describes the Strength and Difficulties Questionnaire, which is the outcome variable in this study. Section five presents the empirical strategy. Section six describes the applied data. Section seven presents the results. Section eight concludes this paper.

2. A brief review of the related literature

Bedard and Dhuey (2006) consider a sample of twenty countries and find that being older at school enrollment causes better fourth and eight grade test scores in mathematics and science, with the exception of children in Denmark and Finland. Their identification is based on predicted school starting age using country specific cutoff dates and individual birth dates. To check for potential endogeneity due to to season of birth effects they also run a pooled regression using variation between countries.

The study by Black et al. (2011) refine the identification and the interpretation of the outcome. Firstly, using Norwegian data they use a regression discontinuity design and thereby reduce season-of-birth issues in the identification strategy. Secondly, they exploit post school tests and can therefore control for both school starting age and age-at-test. They find that the effects of being older at school enrollment on test-scores primarily is driven by the age-at-test effect. In other words, as children who start later will always be older when they are tested within school, the effect of school starting age on test scores may be due to the age at the time of the test, and not because of a direct effect of school starting age. They find that starting school later has a positive effect on mental health at age 18 for boys and a negative effect on the likelihood of teenage pregnancy for girls. However, they also find that older school starting age increases the likelihood of pregnancy within 12 years after school enrollment. The negative effects on teenage pregnancy are therefore driven by the fact that children who start later also are older when they leave school, leaving them less time to participate in risky behavior as teenagers.

Fredriksson and Öckert (2013) use Swedish data on birth cohorts from 1935 to 1955 in a regression discontinuity framework and show that being older at school enrollment increases educational attainment. Their sample period includes a school reform which postponed tracking, and their results show that in the period with postponed tracking the effects of school starting age are smaller. While they find that the effects on discounted life-time earnings on average is very small to negative, they also find positive earnings effects of school starting age for individuals with low-educated parents.

Landersø et al. (2013) use exact day of birth - in contrast to Black et al. (2011) and Fredriksson and Öckert (2013) who use month of birth - and compare a sample of the Danish population born in January and December. Exploiting the January 1st school starting age cutoff date in Denmark, they find that being older at school enrollment lowers the propensity to commit crime. The result is however, mainly driven by an incapacitation - and not a human capital - effect.

There are very few studies on the importance of school starting age for children's behavior in the short run. Elder and Lubotsky (2009) exploit variation in school starting age cutoff dates across U.S. States and find that being one year older at school entry reduces the probability of a Attention Deficit/Hyperactivity Disorder (ADD/ADHD) diagnosis between kindergarten and fifth grade. Exploiting variation in school entry cutoff dates in a pooled sample of 17 countries, Mühlenweg (2010) shows that older school starting age is linked to a reduced likelihood of being "victimized" in school. The effects are driven by a school starting age effect or age-at-test effects.

This present paper makes three contributions to the literature: One, we employ a standardized measure of non-cognitive skills in terms of the SDQ. Two, our outcomes are measured out of school, which reduces the problem that measured differences in non-cognitive skills may be caused by relative age effects.[5] Three, our outcome measures are independent of grade, holding age constant, allowing us to rule out age-at-test effects.

3. Institutional setting

3.1 The years before schooling

Daycare in Denmark is almost exclusively publicly provided and organized by the municipality. Child care consists of center based nurseries for children aged up to two and daycare for children aged three to six. In addition to the center based nurseries, municipalities provide family day care. Requirements to center based day care staff are high compared to other OECD countries (Datta Gupta and Simonsen, 2010). For example, there is a high staff-child ratio and all permanent day care staff must have a pedagogical education. Requirements to family day care are lower.

3.2 The education system

Compulsory schooling begins in "grade zero" (also called kindergarten class) in August the year the child turns six. Until 2009 grade zero was not mandatory, but 98% of a cohort attended grade

[5] Elder and Lubotsky (2009) for instance argue that the identified effects on ADHD may be driven by the facts that older children may be under-diagnosed, because they are compared to their younger classmates.

zero (Browning and Heinesen, 2007). Compulsory schooling ends after ten years of schooling or in August of the year the child turns 17. Figure 3.1 summarizes the timing of events in childhood. The children typically do not change institution or class after they enrolled in grade zero, i.e. Most children stay in the same class within the same school from grade zero until grade nine. After leaving compulsory education, the individual can choose between three-year upper secondary school (high school), vocational training (apprenticeship), or the labor market. Completing high school allows access higher education.

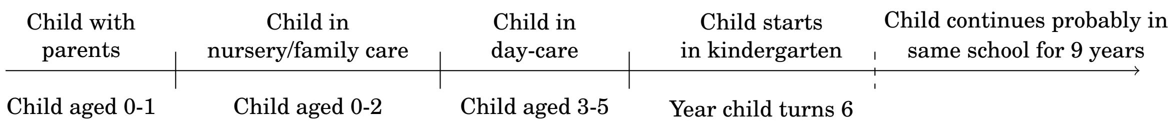


Figure 3.1: Timing of childhood

As children are supposed to enroll in school the year they turn six, school starting age increases discontinuously from December 31 to January 1. To illustrate this we compare the events in Figure 1 for a child born December 31 to a child born January 1 in Table 3.1. Children who comply to the rules will be one year older at school enrollment if they are born on January 1 compared to if they are born one day earlier. It is possible to postpone enrollment in school. This requires considerable effort of the parents and involves meeting representatives from the future school. Based on individual evaluations children may enroll in grade zero one year earlier, if their birthday is before October 1.

Table 3.1: Timing of childhood for a child born December 31 and a child born January 1

Born	December 31st	January 1st
With parents	Months 0-12	Months 0-12
In nursery	Months 13-36	Months 13-36
In day-care	Months 37-65	Months 37-77
Enroll in grade zero	Month 66	Month 78

3.3 What is Kindergarten class/grade zero in Denmark?

Kindergarten class is part of the primary school and free of charge in the public schools. The kindergarten class year starts with an obligatory assessment of the child's verbal communication skills and the outlining of an individual teaching plan (in Danish: Elevplan).[6] The formation of the class is based on either pedagogical or practical considerations, and the principle is the same

[6]The individual plans were introduced in 2006.

as for grades one to seven. Kindergarten class has a formally specified curriculum by the Ministry of Education. The curriculum includes topics such as verbal and non-verbal communication, as well as science and nature (The Danish Ministry of Education, 2009). The Ministry of Education also specifies a minimum number of 600 teaching hours per school year (approximately 3 hours per school day).

As almost all children attend daycare before they enroll in school, the control environment is very homogeneous. Identified effects of school starting age are therefore not a result of a different pre-treatment environments. Also, the kindergarten class is centrally defined and constitutes a homogeneous treatment environment.

4. The Strengths and Difficulties Questionnaire (SDQ)

4.1 Background and validity

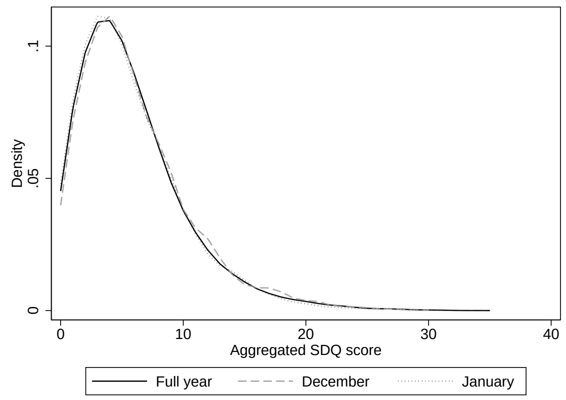
The SDQ is a questionnaire developed by the English child psychiatrist Robert N. Goodman in the mid 1990s. The objective of the SDQ is to provide a tool to describe behavior of children aged 4 to 16 (Goodman, 1997). Compared to two established screening devices, the Rutter questionnaire and the Child Behavior Checklist (CBCL), the SDQ is distinctive in that it is shorter (The SDQ has 25 questions in contrast to 120 in the school-age CBCL), it is uniform, and it assesses both children's strengths and difficulties.

The questionnaire is filled out by a parent in our case, but can also be filled out by a teacher. For each question there are three possible answers: Not True, Somewhat True, Certainly True. An example of a question is that the child is "Restless, overactive, cannot stay still for long". The SDQ scores are computed according to a standardized aggregation procedure.[7] The questionnaire consists of 25 questions covering five dimensions of children's behavior related to non-cognitive skills:

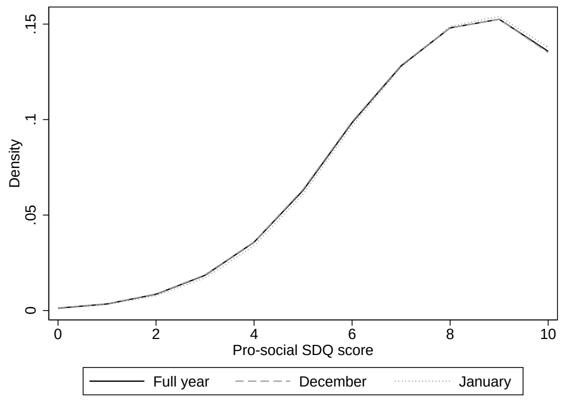
1. Emotional Symptoms Scale
2. Conduct Problems Scale
3. Hyperactivity Scale
4. Peer Problems Scale
5. Pro-social Scale

Higher values means worse skills, with the exception of the pro-social scale. The aggregated measure (Total difficulties score) includes all dimensions except for the pro-social scale. Figure 4.1 shows the distribution of SDQ scores in our sample. For the first four dimensions, scores between 0 and 13 are regarded as normal, while scores 14-16 are borderline and scores above 16 are regarded as abnormal. For the pro-social scale 6-10 is normal, 5 is borderline, and 0-4 is abnormal.

[7] The aggregation procedure is described on the website www.sdqinfo.com. We compared the outcome of the standardized aggregation procedure to a principal component analysis (PCA) using our data. The PCA revealed the same five dimensions as the standardized procedure.



(a) The Total Difficulties Score (includes conduct, emotional, hyperactivity, and peer problems scales. Larger values implies worse skills.)



(b) The Pro-social scale. Larger values implies better skills.

Figure 4.1: The distribution of SDQ scores in our sample. The full sample uses data on all children, the January sample only includes data on the children born in January, the December sample only includes data on children born in December. The SDQ is mother-reported.

Goodman (1997) finds a close correlation between the SDQ and the Rutter questionnaire using a sample of about 400 English children aged four to 16. Goodman and Scott (1999) compare the SDQ to the Child Behavior Checklist (CBCL) on about 130 English children aged four to seven from both a high and a low risk sample. The measures are highly correlated and are both able to identify the two underlying samples. In addition, interview-based evaluations seem to be stronger correlated to the SDQ than to the CBCL.

4.2 Use of the SDQ in research

Using the British Cohort Study, Meschi et al. (2008) test the importance of parents numeracy and literacy skills for the child's cognitive and non-cognitive skills, aged three to six. In the unconditional regressions there is a strong correlation between parental skills and both cognitive and non-cognitive child skills. Once they control for socio-economic background and child characteristics the correlation between parents skills and the SDQ measure of non-cognitive skills becomes insignificant.

In a Danish context Datta Gupta and Simonsen (2010) use SDQ scores computed from the Danish Longitudinal Survey of Children (DALSC) to assess the effects of center based child care versus family day care for children aged three on non-cognitive skills. They find a negative effect of being in family day care for boys of lower educated mothers. Using the same data and identification strategy, Datta Gupta and Simonsen (2012) evaluate SDQ outcomes for the children aged 11, but find no effects.

4.3 SDQ and primary school test scores

While non-cognitive skills are important in themselves, they might also be important for the development of cognitive skills. Research on the impact of school starting age on cognitive skills has been motivated by two potential channels (Bedard and Dhuey, 2006): (1) Age-at-test effects, because children that start later are older when evaluated. (2) A maturity effect, because children who start later are more mature, and therefore more able to comprehend the human capital inputs they receive in school. If the latter is the case, we should expect to find a correlation between SDQ scores and children's performance in tests of cognitive skills.

Table 4.1: Test scores in Danish and mathematics and the five dimensions of the SDQ.

Subject	- - - Danish - - -			- - - Math - - -	
Grade	2	4	6	3	6
Emotional Symptoms Scale	0.03**	0.04**	0.03**	0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Conduct Problems Scale	-0.05**	-0.06**	-0.05**	-0.05**	-0.06**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Hyperactivity Scale	-0.16**	-0.16**	-0.15**	-0.15**	-0.11**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Problems Scale	-0.01	-0.01	0.00	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pro-social Scale	-0.05**	-0.05**	-0.04**	-0.03**	-0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	20,383	37,221	24,028	31,221	23,994

Standard errors clustered on the school level in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$. Each column shows results from one regression with test scores as the dependent variable, the five SDQ dimensions as independent variables and a set of covariates. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, mother's civil status, age at test monthly indicators (both for SDQ and the mathematics/Danish tests), test year, school, and birth year fixed effects. Both the five SDQ scores and the test scores are standardized.

To assess the link between cognitive and non-cognitive skills we use data on the mother reported SDQ for children aged seven and data from the mandatory tests in primary school.[8] We regress the performance in three tests in Danish and two tests in mathematics on the five dimensions of non-cognitive skills from the SDQ. In each regression we include school fixed effects to handle the fact that schools decide how the tests are carried out. We also control for age at test, both for the SDQ scores and the in school tests, by means of monthly indicators.[9] Table 4.1 shows the coefficients on each of the five dimensions for the five separate regressions.

The sample consists of children born between 1998 and 2003 who attended a test in the years 2010 to 2013. The samples differ across columns in Table 4.1. The first column consists of the sample for which second grade test results are available, which roughly covers birth cohorts 2001 to 2003. The fifth column includes children for whom sixth grade test results are available, which

[8] The data is described in section 6.

[9] While the tests are standardized and computer based, the school decides whether the tests are open book or not.

corresponds to the cohorts 1998 to 2000. Considering that the samples, grades and subjects differ across the five columns the correlations between the SDQ scores and the test scores are remarkably constant. A one standard deviation better (i.e. lower) conduct score is associated with a 0.05-0.06 standard deviation higher test score in Danish and Math in grades two to six. The correlation between the hyperactivity scale and the test results is almost three times the coefficients on the conduct scale: A one standard deviation better hyperactivity score is linked to 0.11-0.16 standard deviation better test scores. Peer problems are almost unrelated to cognitive skills, while better pro-social and emotional skills are related to worse test performances. The correlations are almost identical across gender as Tables A.2 and A.3 in the appendix show. For girls, however, there is also a weak correlation between the peer problems and test scores in Danish in early grades.

Using data from the Danish Longitudinal Survey of Children (DALSC), Table A.4 in the Appendix shows the correlation between SDQ scores and the brief version of the Big Five Personality Traits (Rammstedt and John, 2007) and a brief version of Raven's Progressive Matrices. The DALSC is a survey of about 5,000 children born in the fall of 1995. The parents and the children have been surveyed several times. The data used here is from the interviews conducted in 2011. The columns (1) to (6) show that the mother reported SDQ is closely related to the Big Five and the Raven score. Interestingly the SDQ's hyperactivity dimension is closest related to the Raven score, a measure of the individuals "reasoning ability".

Intuitively it makes sense that the non-cognitive skills that are most closely linked to cognitive skills are hyperactivity and conduct. Hyperactivity probably gives a good indication for whether the child can sit still and concentrate in class, while conduct gives an indication of whether the child can behave and follow instructions by a teacher. It is therefore of special interest whether school starting age affects these two dimensions, as this could give us some indication of whether the effect of school starting age on cognitive skills also is a human capital effect, or whether the entire effect is driven by age-at-test effects. The finding that correlations are very constant across samples, subjects, and grades indicates that the SDQ measures capture important aspects of children's non-cognitive skills.

5. Empirical framework

5.1 Identifying strategy

Formally we assume that the relationship between school starting age (SSA) and non-cognitive skills (Y) for individual i with covariates $\boldsymbol{X_i}$ can be represented by the following linear relationship:

$$Y_i = \beta_0 + \beta_1 SSA_i + \boldsymbol{\phi X_i} + e_i \tag{2.1}$$

Identifying the causal effect of school starting age on non-cognitive skills is challenging because children with less developed non-cognitive skills are more likely to enroll later. The ordinary least squares (OLS) estimates of β_1 in equation (2.1) are therefore potentially downward biased. For example if children have difficulties concentrating and focusing, one might decide that starting school is too early. Consequently, children with older school starting age will have worse non-cognitive skills than children who start when they are younger. However, the causal effect of school starting age on non-cognitive skills may still be positive.

To identify the causal effect we need an instrument Z, which is unrelated to unobserved characteristics of the child that affect the outcome and is related to school starting age. In our case we exploit that in Denmark children are supposed to enroll in school the year they turn six. We create an instrument that takes the value of one if the child is born January 1st or later for a year running from July 1st to June 30th, in a fuzzy regression discontinuity design. The fuzziness is created by the fact that parents and their children not necessarily comply with the January 1st cutoff rule.

While evidence shows that season of birth is not random with respect to parental characteristics (Buckles and Hungerman, 2013), it is unlikely that the exact date of birth is related to individual observed and unobserved characteristics. In practice two approaches can be used to handle this issue. One, by considering only the local sample around the January 1st cutoff in a non-parametric approach, or two, by considering the full sample and specifying the relationship between season of birth and age at school enrollment parametrically. In both cases the first stage regression is specified as follows:

$$SSA_i = \gamma_0 + \gamma_1 after_i + g(days_i) + after_i \times g(days_i) + \boldsymbol{\rho X_i} + \epsilon_i \tag{2.2}$$

Where *after* is an indicator for whether the individual was born between January 1st and June 30th. We center the forcing variable, birthday, to January 1, so that the year runs from July to June. The variable *days* is the number of days from January 1, $\boldsymbol{X}$ is a vector of controls including parents income and education, and ε is random noise. The function $g(\cdot)$ is a polynomial function of day of birth, which is included to control for trending behavior that is continuous. The predicted exogenous variation in school starting age (*SSA*) from this first stage regression is then included in the second stage regression of the following equation:

$$outcome_i = \beta_0 + \beta_1 \widehat{SSA}_i + g(days_i) + after_i \times g(days_i) + \boldsymbol{\phi X_i} + e_i \tag{2.3}$$

Where the coefficient β_1 identifies the causal effect of school starting age on the outcome variable. For the local specification we consider a 30 day bandwidth with linear trends interacted with the cutoff date as in Landersø et al. (2013). We assess the robustness of our results to the choice of bandwidth. For the full sample analysis, using a July to June sample, we select the polynomial function based on a graphical judgment and by comparing the Akaike Information Criteria (AIC) for various specifications as suggested by Lee and Lemieux (2010).

5.2 The LATE interpretation

The coefficient β_1 captures the constant effect of starting school later for the population if the treatment effect is homogeneous. As children may differ substantially in their school readiness it is likely that the treatment effect is not constant across the population. The identified treatment effect is therefore only an average treatment effect. Because we exploit exogenous variation in school starting age caused by the rule that children should start school the year they turn six, we only have exogenous variation in treatment for those who comply with these rules. In this case the identified treatment effect is a local average treatment effect (LATE). Formally the treatment parameter can be expressed in terms of the Wald estimator (Angrist and Pischke, 2008):

$$\beta_1^{LATE} = \frac{E[Y|Jan] - E[Y|Dec]}{P(SSO|Jan) - P(SSO|Dec)} \tag{2.4}$$

Where the numerator is the reduced form relationship between date of birth and the outcome Y and the denominator the first stage relationship between date of birth and being older at school enrollment. Note that for always takers (those who always start school late) and never takers (those who never start school late) the expression is not defined as the denominator is zero. β_1 can be interpreted as the LATE effect if the instrument is valid and it increases the propensity of being older at school enrollment monotonically. In other words, we interpret our estimates as LATE effects by the assumption of having no defiers, that is children where parents always choose the opposite of what the school starting age rules recommend. It seems unlikely that parents plan to enroll their child one year too early if it is born in January, but plan to postpone enrollment one year if it is born in December.

5.3 Characterizing the compliers

While being born in January increases the expected school starting age, it is important to stress that there are some children who are unaffected by this rule. For simplicity consider school starting age as binary variable that takes the value of one if the child is older than six years and six months at school enrollment, $SSO = 1(SSA > 6.5)$. Children with very low school readiness will probably never be treated, whether they are born in December or January. Likewise, children who are very school ready will probably always be treated, whether they are born in December or January. The treatment effect is therefore only identified for the subgroup of the population that complies to the rule, i.e. changes behavior when born after December 31st.

While it is empirically impossible to identify the subpopulation who complies to the school starting rule, it is possible to characterize their observable characteristics. Formally we can calculate the probability that a complier has a characteristic $x_i = a$ using the following expression (Angrist and Pischke, 2008):

$$P[x_i = a|P(SSO|Jan) > P(SSO|Dec)] = \frac{P_i(SSO|Jan, x_i = a) - P_i(SSO|Dec, x_i = a)}{P_i(SSO|Jan) - P(SSO|Dec)} \tag{2.5}$$

That is, the probability that a complier has a characteristic a is given by the ratio of the first stage coefficient for individuals with this characteristic to the overall first stage coefficient.

5.4 Identifying the marginal treatment effect

If we consider school starting age as a binary variable as above, it is possible to identify the marginal treatment effect (MTE) which is the treatment effect for children at the margin of being old at school enrollment:

$$\beta_1^{MTE} = E[Y|\epsilon = \epsilon^*, \boldsymbol{X} = \boldsymbol{X}^*] \tag{2.6}$$

Where ϵ_i^* is the error term in equation (2.2) moving the individual to the margin of treatment. Individuals with a very large observed propensity of treatment will need a numerically very large error term to make them "indifferent" between treatment and non-treatment. To calculate the MTE we follow the parametric approach described by Heckman et al. (2006):

1. Estimate a probit for the propensity of treatment
2. Specify the area of common support, that is observed propensities of treatment where we observe both treated and untreated.
3. Compute the MTE as a function of u, where $\epsilon = F_u(u)$

6. Data

6.1 The data structure

We create our analysis sample by combining Danish administrative registers from Statistics Denmark with data from the Danish Ministry of Education on the compulsory tests in grades two to eight, and data from the Danish National Birth Cohort Survey (DNBC) (Olsen et al., 2001). The registers provide information on the child's birthday (the forcing variable), the test data is used to impute school starting age (the treatment), and the survey data provides information on the non-cognitive skills in terms of the parent reported SDQ scores (the outcomes). The three data sources are described below. As the registers and tests cover the full population, the sample is limited by the survey data which covers 57,280 children born in the period 1998 to 2003.

6.2 The administrative registers

The administrative data consists of several individual registers including the birth records, the income registers, the annual register based labor force statistics (RAS), and the education registers. All datasets are hosted by Statistics Denmark and linked by the unique personal identifier.

For the children we use information from the registers on birthday, birth weight, 5 minute APGAR score[10], and the gestational age. For the parents we use information on gross annual income, the labor market attachment in November, educational attainment, civil status, origin and age. We also record the number of siblings (living in the household) when the child is two years old using register data.

[10] The APGAR score is an evaluation of the infants' health measured on a 0-10 scale (where 10 is the best).

Before we link the children to their parents and siblings we adjust the birth year to run from July to June instead of January to December. For example all children born in the period July 2000 to June 2001 are merged to parents' characteristics for the calendar year January to December 1999.

6.3 The National Test data

The National Test is an obligatory assessment of the school children's cognitive skills in grades two to eight. The tests were introduced in 2010. The tests are used as a tool for the teacher to assess the child's development, and they have no direct impact on the child's continued schooling (except that the teacher can use test results when grading the pupils). The test data provided by the Ministry of Education provides information on the exact testing time, the grade, and the subject. We use this information to impute the school starting age. For example if the child took the second grade test in spring 2010 the child must have enrolled in school in August 2007, as the school year runs from August to June.

It may be the case that children who enroll early also are more likely to retake kindergarten class, which would cause a non-classical measurement error in school starting age. We assess this issue by also running regressions using parent reported school starting age.

6.4 The Danish National Birth Cohort (DNBC)

The DNBC is a Danish nation-wide cohort study in which 101.042 pregnant women were sampled over the period 1997 to 2003. The mother was interviewed twice during pregnancy, when the child was six months old, when the child was 18 months old, and when then child was seven years old.[11] All interviews contain questions on the parents' behavior (smoking, drinking, sport activities, and employment), the mother's health and the child's health, cognitive, and non-cognitive skills (at age 18 months and seven years). 92,892 mothers replied to the first interview, 66,764 replied to the fourth interview (child aged 18 months), and 57,280 replied to the fifth interview (child aged seven years).

Attrition from the sample was not random. Mothers who were not in the labor force constitute a share in the survey which is about 60% lower than in the population data, single women are underrepresented by about 30%, and low educated women by about 40% (Jacobsen et al., 2010). Table A.1 in the appendix provides a comparison between the survey sample and the full population of children born in the period 1998 to 2003 for the covariates used in this paper. With the exception of gender and APGAR score all variables have significantly different means. Children in the survey have a higher birth weight, and have parents who have a higher income and completed more schooling. As academic red-shirting is especially prevalent among socioeconomic advantaged parents, we may therefore find that parents in the survey data comply less to the school starting age rule than the parents in the population. We discuss the implications for the external validity of our results in section 8.

[11] A sixth survey when the children are 11 years old will be completed in 2014.

In this paper we only use data from the fifth interview of the DNBC, when the children are approximately seven years and two months old. In this interview the parents answered the 25 questions of the SDQ, which we use to create the six outcome variables (the five dimensions of the SDQ and the aggregated score). The fifth interview also contains mother reported school starting age which we use to test the validity of our school starting age measure. In a robustness check we use information from the fourth interview on cognitive and non-cognitive skills before school enrollment, as a placebo test.

6.5 Data selection and descriptive statistics

The sample selection takes point of departure in the 57,280 children surveyed in the fifth interview of the DNBC. We exclude 2,012 children with incomplete SDQ measures, and 210 children for whom the school starting age is missing. The final sample consists of 55,058 children born between 1997 and 2003 as shown in Figure A.2 in the appendix. If covariates are missing, the value is set to zero and a missing covariate indicator, which takes the value of one if this covariate is missing, is included.

Table 6.1: Variable descriptives

	30 day bandwidth			Full sample		
	Mean	SD	N	Mean	SD	N
School starting age	6.23	0.56	7,718	6.10	0.51	55,058
School starting age>6.5	0.68	0.47	7,718	0.18	0.39	55,058
Birth weight	3530.30	601.78	7,675	3557.53	596.93	54,796
Girl	0.49	0.50	7,718	0.49	0.50	55,058
5min APGAR	9.90	1.86	7,640	9.91	1.79	54,593
Mother's age at childbirth	30.71	4.31	7,695	30.64	4.21	54,883
Mother's educational length	14.31	2.22	7,695	14.38	2.20	54,883
Mother is nonwestern	0.01	0.08	7,695	0.01	0.07	54,883
Mother is single	0.13	0.34	7,695	0.12	0.33	54,883
Mother's gross income	269.15	130.75	7,718	268.08	118.20	55,058
Mother is employed	0.85	0.36	7,695	0.85	0.36	54,883
Father's age at childbirth	33.01	5.27	7,490	32.88	5.12	53,940
Father's educational length	14.13	2.42	7,490	14.23	2.41	53,940
Father is nonwestern	0.01	0.12	7,490	0.01	0.11	53,940
Father's gross income	373.00	318.75	7,718	371.10	264.79	55,058
Father is employed	0.88	0.33	7,490	0.87	0.33	53,940

The 30 day bandwidth includes all children born within 30 days of January 1st. Birth weight is measured in grams. Educational length is measured in years. Parents are defined as non-western if they are immigrants to Denmark from a non-western country according to the classification by Statistics Denmark. The mother's single status is one if the child is living with the mother, and the mother is not married or cohabiting. The gross income is measured in 1,000 DKK and adjusted to the 2010 level using the consumer price index. The parents' employment is for November in the lagged year.

As we present results using both a local 30 day bandwidth and the full sample, Table 6.1 provides sample means, standard deviations and the number of observations for the covariates and dependent variables in these two samples. Average school starting age is 6.23 in the local sample, which is slightly higher than in the full sample. Both the covariates and the dependent variables are very similar across these groups.

6.6 Age at test

A key advantage of our data is that the outcome variables are independent of grade, so that all children have the same age when they are measured, independently of their enrollment age. This is confirmed by Figure 6.1, which shows the average age at SDQ measurement in three day bins, as well as the median and the 10th and 90th percentiles. There seems to be small seasonality effects, but there is no sign of a jump around January 1st. Also note that 80 percent of all children were between 7 and 7.3 years old at the measurement of non-cognitive skills.

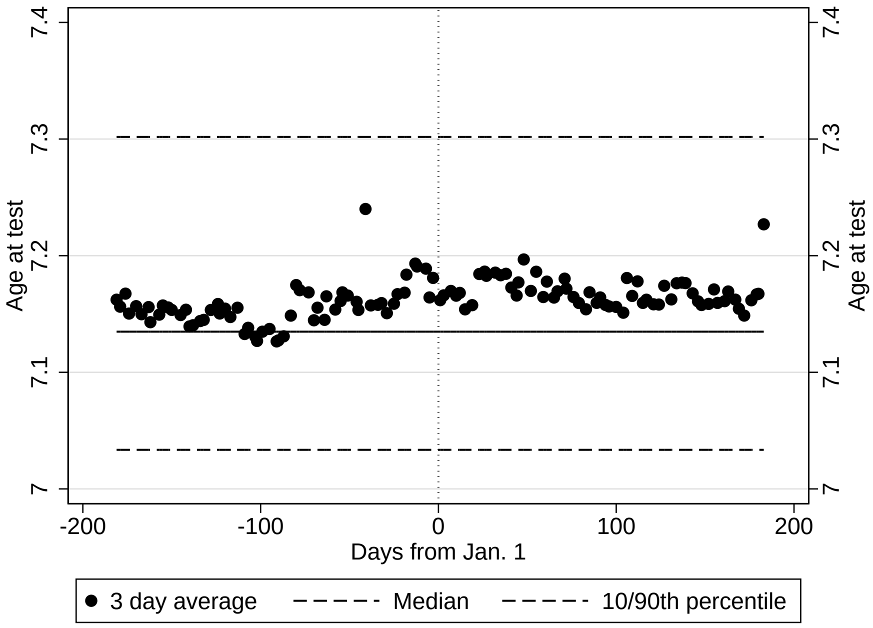


Figure 6.1: The child's age in years at the time the parent answers to the 25 elements of the SDQ in the DNBC questionnaire. Each dot shows the mean age for a three-day bin.

It is important that children on average are 7.1 and not exactly 7, because this implies that parents to children born around January 1st are interviewed in February. If the interview was exactly seven years after birth, jumps in the SDQ could be due to Christmas.

7. Results

7.1 Validity of the RD design

Before we assess the results of our analysis we first carry out a number of tests of whether the setting provides a valid RD design. Our instrument, the indicator variable for whether the child was born after January 1st, has to be uncorrelated with unobserved characteristics affecting the child's skills. Parents that consider the impact of date of birth on school enrollment six years later, are probably not a random sample of parents. Rather, such parents are a selected subsample of parents who are more prepared and potentially offer more support for their child. If parents can manipulate the date of birth perfectly the children born just after January 1st will probably have systematically different unobserved characteristics to those born just before January 1st. In this section we assess whether this is the case in three ways: (1) We evaluate the distribution of births over the cutoff. We would expect an increase in birth just after January 1st, if parents have perfect control over the forcing variable. (2) We compare the mean values for observable characteristics just before and after the cutoff, to assess whether children born after the cutoff have better observed characteristics than would be expected if the parents manipulate the day of birth. (3) We graphically as well as in a regression framework test for any jumps in observed characteristics over the cutoff date.

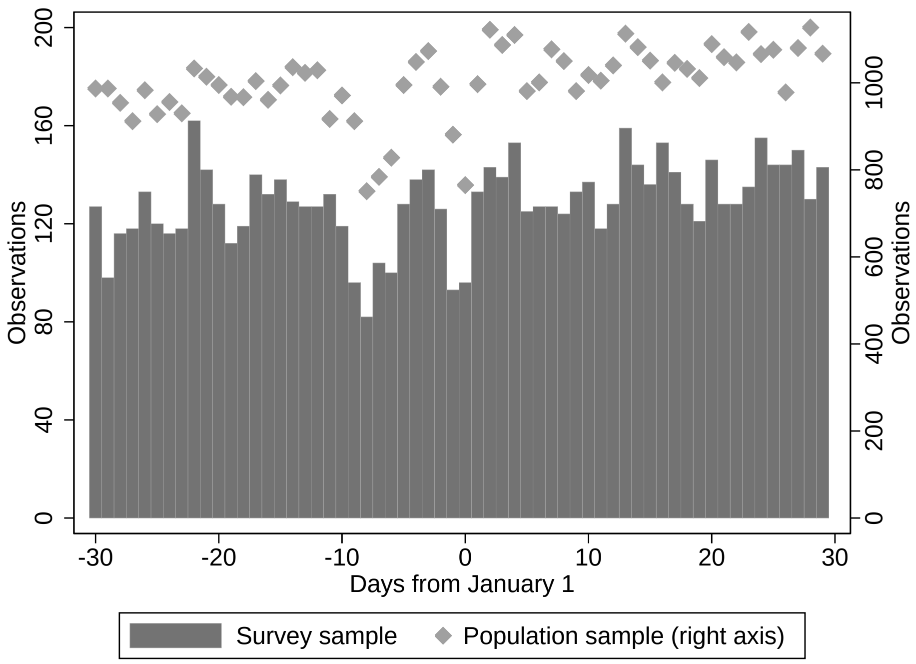


Figure 7.1: Observations by date of birth, survey data and population data. The survey data is the data used in our analysis, and the population includes all children born in Denmark in the period 1998-2003.

Figure 7.1 shows the distribution of date of birth in our sample. We observe no spike around January 1st. There is a small drop in births at December 31st and January 1st, but the low level is not different from the drop one week earlier. The pattern is probably driven by a Christmas and New Years effect, and not that parents manipulate the date of birth. This effect may also be problematic, if a certain group of the population is affected by these events, in which case we would expect to find differences in observable characteristics across the cutoff.

Table 7.1 shows the sample means for the covariates included in the regression, 30 days before and after January 1st. The rightmost column presents the p-value from a t-test on the equality of the means in the two groups. The first row shows that school starting age is significantly higher for children born in January. Also, as shown in the second row, 80 percent of all children born in January are at least 6 years and 6 months at school enrollment, while this proportion is only 60 percent for December children. None of the child and parent characteristics are statistically different in January compared to December.

Table 7.1: Comparison of means, Sample: 30 days before and after January 1st

	30 days before			30 days after			P-val
	Mean	SD	N	Mean	SD	N	
School starting age	6.13	0.60	3,663	6.32	0.51	4,055	0.00
School starting age>6.5	0.56	0.50	3,663	0.78	0.41	4,055	0.00
Birth weight	3,519	621.72	3,644	3,540	583.06	4,031	0.12
Girl	0.49	0.50	3,663	0.49	0.50	4,055	0.81
5min APGAR	9.92	2.18	3,626	9.89	1.52	4,014	0.61
Mother's age at childbirth	30.67	4.35	3,650	30.74	4.28	4,045	0.48
Mother's educational length	14.29	2.24	3,650	14.33	2.20	4,045	0.44
Mother is nonwestern	0.01	0.08	3,650	0.00	0.07	4,045	0.27
Mother is single	0.13	0.34	3,650	0.13	0.33	4,045	0.49
Mother's gross income	271.22	150.57	3,663	267.28	109.80	4,055	0.19
Mother is employed	0.85	0.36	3,650	0.85	0.36	4,045	0.99
Father's age at childbirth	32.95	5.22	3,494	33.07	5.31	3,996	0.32
Father's educational length	14.10	2.39	3,494	14.16	2.46	3,996	0.28
Father is nonwestern	0.01	0.12	3,494	0.01	0.12	3,996	0.85
Father's gross income	369.93	390.95	3,663	375.78	235.20	4,055	0.42
Father is employed	0.88	0.32	3,494	0.87	0.33	3,996	0.39

Notes: Birth weight is measured in grams. Educational length is measured in years. Parents are defined as non-western if they are immigrants to Denmark from a non-western country according to the classification by Statistics Denmark. The mother's single status is one if the child is living with the mother, and the mother is not married or cohabiting. The gross income is measured in 1,000 DKK and adjusted to the 2010 level using the consumer price index. The parents employment is for November in the lagged year. The rightmost column presents the p-value from a t-test on the equality of the means in the two groups, assuming equal variances.

Sample means may conceal jumps in covariates at the cutoff, as any trending behavior is ignored. We therefore use the same specification as in the first stage regressions (equation (2.2) without covariates) and include each covariate as a dependent variable. Table A.5 in the appendix shows the coefficient on the indicator for being born after January 1st for each of the covariates. None of the the covariates show signs of jumps at the cutoffs in neither the 30 day local specification nor in the full sample parametric specification. An alternative testing strategy is to regress the outcome variable on all covariates (without trends) and compute the predicted values (the y-hat). The y-hat then represents the average of the covariates, weighted by their influence on the dependent variable. In Table 7.2 we show the outcome of regressing this weighted average on the cutoff and time trends for for each of the six dependent variables. As for the single-covariate regressions, there is no sign of a jump in any of these specifications.[12]

Figures A.3, A.4, and A.5 in the appendix show the mean values in three day bins for all the covariates over the full sample period. The only graph indicating a small jump at the January 1st cutoff is the father's educational length, which is slightly lower just before January 1st. All in all

[12]Note that both Table A.5 and 7.2 show uncorrected standard errors and significance levels. Any corrections for multiple testing will make the conclusions of no correlation even stronger.

Table 7.2: Auxiliary RD estimates, balancing of the covariates.

	(1)	(2)
Y-hat (Total difficulties score)	-0.01	-0.01
	(0.01)	(0.01)
Y-hat (Emotional symptoms)	-0.02	-0.01
	(0.01)	(0.01)
Y-hat (Conduct problems)	-0.01	-0.01
	(0.01)	(0.00)
Y-hat (Hyperactivity)	-0.01	-0.01
	(0.01)	(0.01)
Y-hat (Peer problems)	-0.01	-0.01
	(0.01)	(0.00)
Y-hat (Pro-social)	-0.01	0.00
	(0.01)	(0.01)
Bandwidth	30 days	Full
Linear trend × cutoff	✓	✓
Quadratic trend × cutoff		✓

Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Each cell represents the point-estimate for being born on January 1st or later, from an individual regression of y-hat on this cutoff and the trends. Y-hat is created by estimating the corresponding depending variable on all covariates. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), and birth year fixed effects.

there is no indication of manipulation of the forcing variable.

7.2 Model selection

We now turn to the selection of the parametric specification. A good starting point for selecting the parametric specification is a graphical inspection of the reduced form relationships between the outcome variables and the forcing variable, the date of birth. Figures A.6a to A.6f in the Appendix show this relationship using a three day bin with a quadratic fit on each side of the cutoff. For all outcomes this quadratic specification captures the development in outcome scores reasonably well.

Table 7.3 provides a more rigorous test of the parametric specification by comparing the Akaike Information Criterion (AIC) across three types of parametric specifications. For all outcomes, except for the total difficulties score and the pro-social behavior scale, the information criterion is minimized using a quadratic trend. For the total difficulties scale the criterion is minimized using a cubic trend (or a higher order polynomial), while for the pro-social behavior scale the criterion is minimized with a linear trend.

Since the quadratic trend is suggested both by the graphical representation and the comparison of the AIC for four out of six outcomes, we apply this specification in the main regressions. [13]

[13] While the parametric specification has almost no impact on the point estimate on the total difficulties scale, using a linear trend makes the coefficient estimates on the pro-social scale smaller and more precise.

Table 7.3: Model selection - Akaike Information Criteria (AIC)

	(1)	(2)	(3)
Total difficulties score	-144.57	-156.10	-156.75
Emotional symptoms	-33.38	-39.07	-38.70
Conduct problems	-10.71	-14.02	-11.44
Hyperactivity	-211.78	-228.64	-226.71
Peer problems	-50.74	-54.71	-52.66
Pro-social	-22.97	-20.42	-18.64
Linear trend × cutoff	✓	✓	✓
Quadratic trend × cutoff		✓	✓
Cubic trend × cutoff			✓

The AIC is calculated by $AIC = N * \ln(RSS/N) + 2 * k$.

For the bandwidth selection we follow the approach taken in the existing evidence (Landersø et al., 2013) and use a 30 day bandwidth for the local regression with a linear trend interacted with cutoff. It is reasonable to assume that parents to some extent can control the childbirth by month. We also ran a regression using all children whose gestational age made it likely that they were born on January 1st. The results are very similar.

Throughout we will show results using both the local specification and the full sample specification with the parametric specified above. In the robustness analysis we show how the results vary by choice of bandwidth and parametric specification.

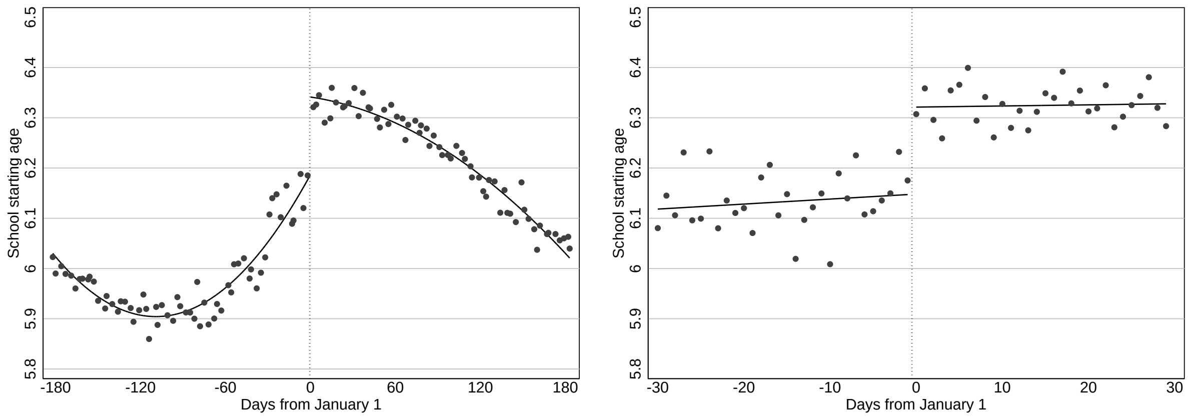
7.3 Main results

The first stage relationship between date of birth and school starting age is shown in Figures 7.2a and 7.2b. For the full-year sample in Figure 7.2a the quadratic trends capture the variation in school starting age reasonably well. The average school starting age jumps from around 6.2 to about 6.35 at January 1st. In the local specification in Figure 7.2b the linear trend seems sufficient to describe the relationship. At January 1st the average school starting age jumps from 6.15 to 6.3.[14]

While Figure A.6 in the Appendix shows the "raw" reduced form relationship by plotting the mean values of the outcome variables using three day bins and fitted quadratic lines, Figure 7.3 shows that a jump in the hyperactivity dimension can be identified even in a less parametric (i.e. no fitted polynomial) version of the reduced form relationship. The graphs show the outcome means for 15 day bins, and their corresponding 95 percent standard error confidence bands. For all outcomes, but the hyperactivity and the total difficulties score, there seems to be no jump in outcomes for children born around January 1st. But for these remaining two SDQ scores a discontinuity is clearly identifiable.

The main regression results are shown in Table 7.4. Columns (1) and (2) show the results from the first stage regression. Both in the local (The upper part: Panel A.) and the parametric specification (The lower part: Panel B.), the point estimate on being born January 1st or later

[14] Figure A.7 in the Appendix shows the first stage by gender.



(a) Full year bandwidth & 3 day bins.

(b) 30 days bandwidth & one day bins.

Figure 7.2: Date of birth and school starting age

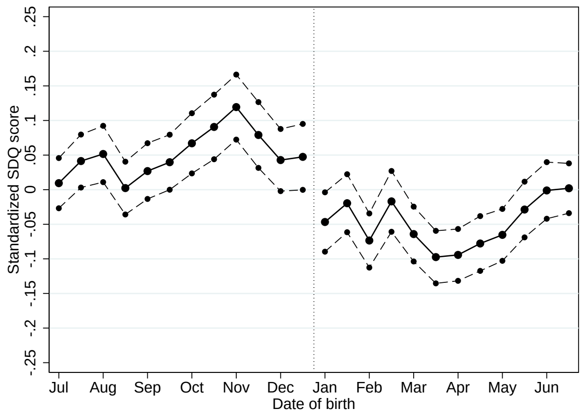
is positive and strongly significant. The size of the jump is slightly smaller in the parametric specification, and adding the full set of covariates makes the jump slightly larger, but also more precisely estimated.

Columns (3) and (4) in Table 7.4 show coefficients from the reduced form regression of the outcome variables on the cutoff-indicator and the trends. Each cell is the point estimate on being born January 1st or later for a year running from July to June. The regressions reveal a jump in the Total Difficulties Score, which is driven by the jump on the hyperactivity scale. While adding covariates has almost no impact on the point estimates, the jumps are considerably smaller in the parametric compared to the local specifications.

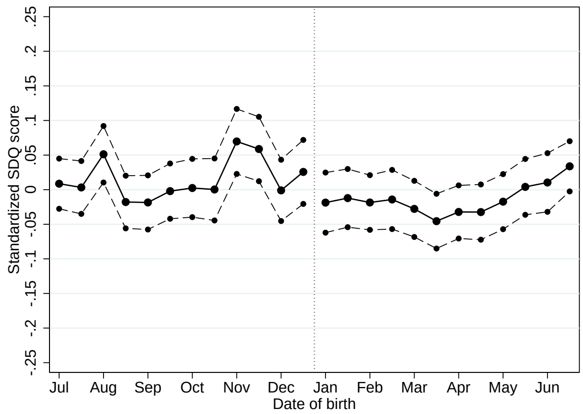
The OLS results in Column (5) of Table 7.4 are mostly positive and significant. The results in Table 7.4 stress the importance of using an instrument to obtain exogenous variation in school starting age. All OLS estimates are considerably smaller and have the reverse sign compared to the 2SLS estimates in column (6). The OLS results show that children who enroll later have worse non-cognitive skills.

Dividing the cells in columns (3) and (4) by the cells in columns (1) and (2) gives the Wald estimate (as specified in equation (2.4)) of the effect of school starting age on the SDQ measures, which are presented in terms of the 2SLS coefficients in columns (6) and (7). The point estimates in the local specifications are up to 50 percent larger than the parametric specifications. The difference is driven by the fact that the reduced form jump is larger with a 30 day bandwidth than with the full sample. Enrolling in school one year later improves the hyperactivity scale by between 0.5 and 0.8 of a standard deviation, while the other dimensions remain unaffected.

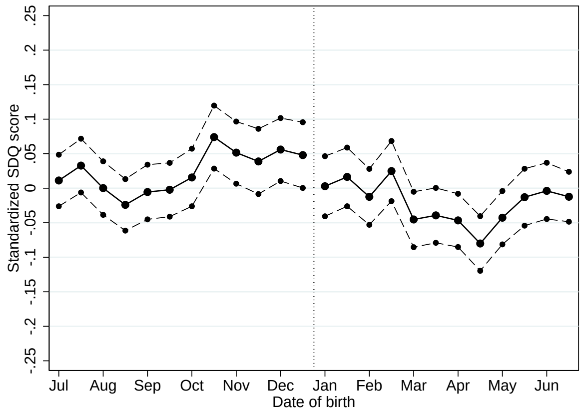
Recall that the effect is only identified for the subgroup of the population that complies to the school starting age rule. Table 7.6 provides a comparison of the first-stage coefficients for subgroups of the population. Compliers are mostly children of low educated parents, girls and children with no older siblings.



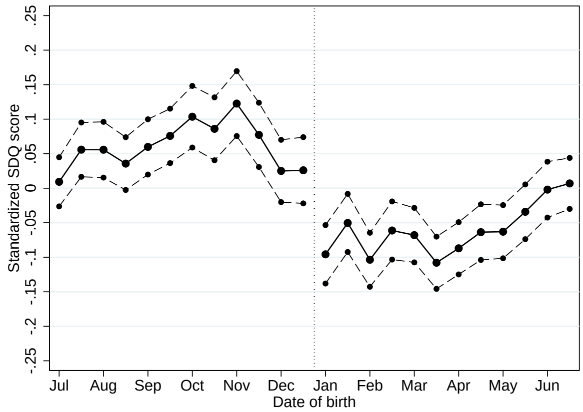
(a) Total difficulties score



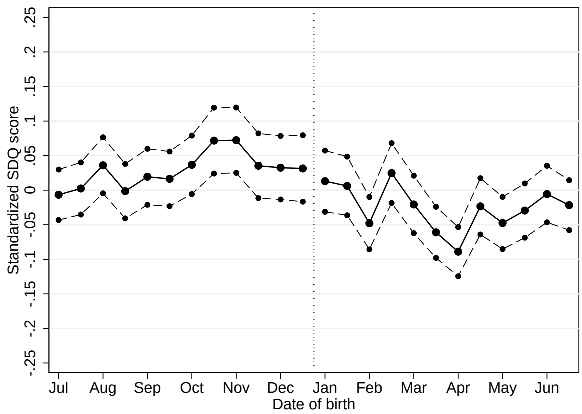
(b) Conduct problems



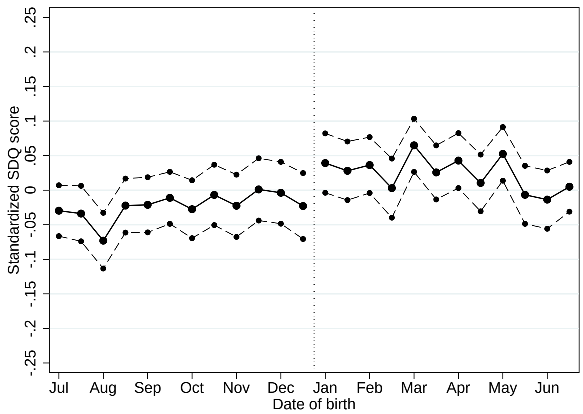
(c) Emotional symptoms



(d) Hyperactivity



(e) Peer problems



(f) Pro-social

Figure 7.3: SDQ outcomes and date of birth. All outcome variables are standardized. 15 day bins.

Table 7.4: The first stage, reduced form, and 2SLS results of the main specification

	First stage		Reduced form		OLS	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Local specification: 30 day bandwidth & linear trends interacted with January 1. cutoff							
Date of birth ≥ Jan. 1	0.17^{**}	0.18^{**}					
	(0.03)	(0.02)					
Total difficulties score			-0.13^{**}	-0.12^{*}	0.13^{**}	-0.75^{*}	-0.65^{*}
			(0.05)	(0.05)	(0.02)	(0.30)	(0.28)
Emotional symptoms			-0.08	-0.06	0.11^{**}	-0.46	-0.36
			(0.05)	(0.05)	(0.02)	(0.29)	(0.27)
Conduct problems			-0.05	-0.05	0.08^{**}	-0.31	-0.26
			(0.05)	(0.05)	(0.02)	(0.27)	(0.26)
Hyperactivity			-0.15^{**}	-0.15^{**}	0.13^{**}	-0.87^{**}	-0.82^{**}
			(0.05)	(0.05)	(0.02)	(0.31)	(0.28)
Peer problems			-0.04	-0.02	0.04	-0.23	-0.14
			(0.05)	(0.05)	(0.02)	(0.28)	(0.27)
Pro-social			0.08	0.09^{*}	-0.10^{**}	0.48	0.53
			(0.05)	(0.05)	(0.02)	(0.28)	(0.27)
Observations	7,718	7,718	7,718	7,718	7,718	7,718	7,718
B. Parametric specification: Full sample & quadratic trends interacted with January 1. cutoff							
Date of birth ≥ Jan. 1	0.15^{**}	0.16^{**}					
	(0.01)	(0.01)					
Total difficulties score			-0.08^{**}	-0.07^{*}	0.21^{**}	-0.53^{**}	-0.43^{*}
			(0.03)	(0.03)	(0.01)	(0.19)	(0.17)
Emotional symptoms			-0.05	-0.04	0.11^{**}	-0.32	-0.25
			(0.03)	(0.03)	(0.01)	(0.18)	(0.17)
Conduct problems			-0.04	-0.04	0.13^{**}	-0.28	-0.24
			(0.03)	(0.03)	(0.01)	(0.18)	(0.17)
Hyperactivity			-0.09^{**}	-0.08^{**}	0.19^{**}	-0.60^{**}	-0.50^{**}
			(0.03)	(0.03)	(0.01)	(0.19)	(0.17)
Peer problems			-0.02	-0.01	0.12^{**}	-0.13	-0.07
			(0.03)	(0.03)	(0.01)	(0.18)	(0.17)
Pro-social			0.04	0.04	-0.11^{**}	0.24	0.28
			(0.03)	(0.03)	(0.01)	(0.18)	(0.17)
Observations	55,021	55,021	55,021	55,021	55,021	55,021	55,021
Covariates		✓		✓			✓

Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Each cell shows the estimate from a single regression. In columns (1) and (2) the dependent variable is school starting age (in years) which is regressed on an indicator for being born after January 1st, trends, and trends interacted with the January 1st cutoff. In columns (3) and (4) the SDQ measure is regressed on the same specification as in (1) and (2). Column (5) shows the results from a simple OLS regression of the dependent variable on school starting age and the time trends. Columns (6) and (7) show the 2SLS results from estimating the SDQ measure on the predicted school starting age, the time trends, and the time trends interacted with the cutoff. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), and birth year fixed effects.

Table 7.5: The first stage, reduced form, and 2SLS results by gender

	First stage		Reduced form		2SLS	
	Boys	Girls	Boys	Girls	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
A. Local specification: 30 day bandwidth & linear trends interacted with January 1. cutoff						
Birthday ≥ Jan. 1	0.15**	0.20**				
	(0.03)	(0.04)				
Total difficulties score			-0.06	-0.17**	-0.38	-0.85*
			(0.07)	(0.06)	(0.46)	(0.34)
Emotional symptoms			0.01	-0.14*	0.07	-0.69
			(0.07)	(0.07)	(0.43)	(0.37)
Conduct problems			-0.05	-0.05	-0.31	-0.22
			(0.07)	(0.06)	(0.44)	(0.31)
Hyperactivity			-0.12	-0.17**	-0.79	-0.84*
			(0.07)	(0.06)	(0.48)	(0.33)
Peer problems			0.03	-0.08	0.21	-0.42
			(0.07)	(0.06)	(0.46)	(0.31)
Pro-social			0.04	0.14*	0.26	0.71*
			(0.07)	(0.06)	(0.45)	(0.33)
Observations	3,912	3,806	3,912	3,806	3,912	3,806
B. Parametric specification: Full sample & quadratic trends interacted with January 1. cutoff						
Birthday ≥ Jan. 1	0.08**	0.24**				
	(0.02)	(0.02)				
Total difficulties score			-0.02	-0.11**	-0.27	-0.48**
			(0.04)	(0.03)	(0.51)	(0.15)
Emotional symptoms			-0.00	-0.08*	-0.03	-0.33*
			(0.04)	(0.04)	(0.48)	(0.17)
Conduct problems			-0.03	-0.05	-0.39	-0.19
			(0.04)	(0.04)	(0.51)	(0.15)
Hyperactivity			-0.04	-0.12**	-0.52	-0.49**
			(0.04)	(0.03)	(0.53)	(0.15)
Peer problems			0.03	-0.05	0.36	-0.20
			(0.04)	(0.04)	(0.53)	(0.15)
Pro-social			0.03	0.06	0.34	0.25
			(0.04)	(0.03)	(0.52)	(0.15)
Observations	28,107	26,914	28,107	26,914	28,107	26,914
Covariates	✓	✓	✓	✓	✓	✓

Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Each cell shows the estimate from a single regression. In columns (1) and (2) the dependent variable is school starting age (in years) which is regressed on an indicator for being born after January 1st, trends, and trends interacted with the January 1st cutoff. In columns (3) and (4) the SDQ measure is regressed on the same specification as in (1) and (2). Columns (5) and (6) show the 2SLS results from estimating the SDQ measure on the predicted school starting age, the time trends, and the time trends interacted with the cutoff. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), and birth year fixed effects.

Table 7.6: Characterizing compliers, comparison of the first stage across subgroups of the sample.

	(1)	(2)	(3)	(4)	(5)	(6)
	Girls	Boys	No siblings	Siblings	Low educ.	Highly educ.
Coefficient	0.19	0.14	0.21	0.14	0.19	0.16
t-value	(5.02)	(4.60)	(5.74)	(3.92)	(5.70)	(4.05)
Rel. to overall first	1.12	0.84	1.22	0.81	1.07	0.95
Observations	3,806	3,912	3,645	4,073	4,365	3,353
Bandwidth	30 d	30 d	30 d	30 d	30 d	30 d
Linear trend × cut	✓	✓	✓	✓	✓	✓

The row "Coefficient" gives the point-estimate on being born on January 1st or later, in a regression of school starting age on this indicator, the trend, the trend interacted with the cutoff, and the covariates. The t-value is the t-statistic on this estimate. The third rows shows the ratio of the subgroup point-estimate to the full sample point-estimate of the first stage. High/low education is measured by more or less that 15 years of education.

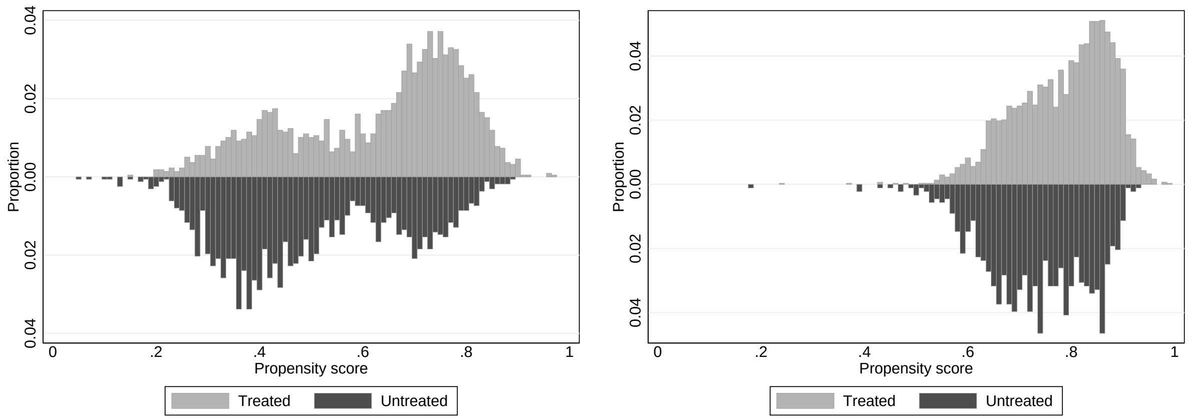
7.4 Heterogeneity

Table 7.5 shows the results divided by gender. As we already saw in Table 7.6 the first stage is much stronger for girls than for boys. The results in the table reveal that the girls also have slightly larger reduced form point estimates, but the difference in the first stage is also large, so that the 2SLS results in general are quite similar, but much less precise for boys. For girls there is also an effect on the pro-social scale in the local specification. Girls who enroll one year later score 0.7 standard deviation better on the pro-social scale.

While subgroup regressions provide a means to compare differences in treatment effects across observables, marginal treatment effects allow us to compare treatment effects along an "unobserved" dimension. To compute the marginal treatment effects we consider the local bandwidth of 30 days and consider a binary treatment variable, SSO, which takes the value of one if the individual is older than 6.5 years at school enrollment. Using this specification we calculate the marginal treatment effects by means of the parametric specification outlined in Heckman et al. (2006) and used by Landersø et al. (2013).

We first estimate a probit model for being treated, and predict each individual's propensity score for being old at school enrollment. This propensity score is then included in a specification for the outcome equation. Intuitively for every observed level of probability for treatment (the propensity score) we compare individuals who were treated to individuals who were not treated. The effect can therefore only be identified for propensity scores where we observe both treated and untreated children. Figure 7.4 shows that for girls there is a common support over the interval 0.2 to 0.9. For boys there is essentially no one with a propensity score below 0.6. Very few boys start early. This is in line with the previous section where we found that compliers are primarily girls.

Children with a very high level of observed probability who were not treated must have had a large error term to not make them treated. This is what Landersø et al. (2013) interpret as



(a) Girls **(b)** Boys

Figure 7.4: Common support. Treatment: School Starting Age > 6.5. 30 days bandwidth. We estimated a probit model where the dependent variables takes the value of one, if the child is older than 6.5 years at enrollment. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), linear time trends, and birth year fixed effects.

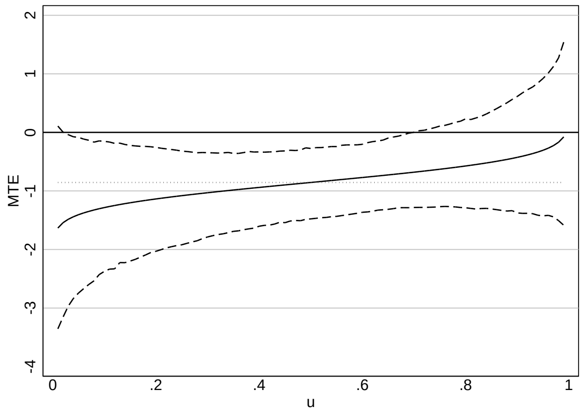
latent utility, u. Figure 7.5 shows the marginal treatment effects for all outcomes for girls plotted against u.

Although the confidence bands are relatively wide, an upward sloping trend is recognizable for most outcomes. The marginal treatment effect is only significant for the lower half of the "latent ability" distribution: Primarily girls with a low level of latent utility benefit from enrolling in school later, measured by the Total Difficulties score and the hyperactivity scale. This finding is somewhat in contrast to the conclusion by Landersø et al. (2013), who find that the marginal treatment effects of school starting age on crime are homogeneous for girls. It makes intuitive sense, that the estimated treatment effect is largest for girls with a low level of latent ability, while for girls with a high level of latent ability starting school later has no benefits.

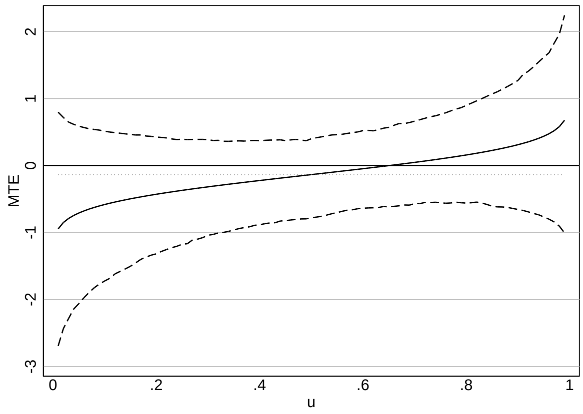
For boys the marginal treatment effects is identified for a much narrower bandwidth covering the propensity scores from 0.6 to 0.9. While for girls most treatment effects were decreasing in latent ability (because lower values means larger effects), Figure 7.6 reveals that for boys the reverse is the case. The confidence bands are also quite wide for boys, but especially for the hyperactivity scale we find signs of a downward sloping relationship. Only boys with a very high level of latent ability benefit from enrolling late in school. This is in line with the finding by Landersø et al. (2013) who conclude that older school starting age reduces crime for the most able boys.

7.5 Robustness

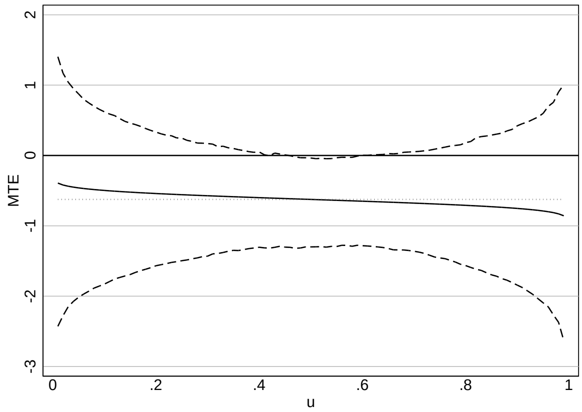
Table 7.7 provides results of separate regressions of each outcome variable on a quadratic trend interacted with the January 1st cutoff and seven birthday cutoffs. Only for the peer problem



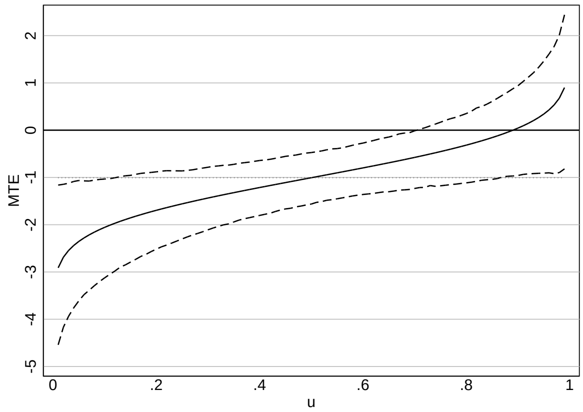
(a) Total Difficulties



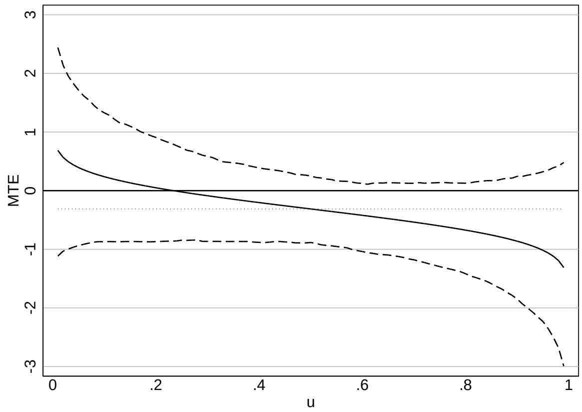
(b) Conduct



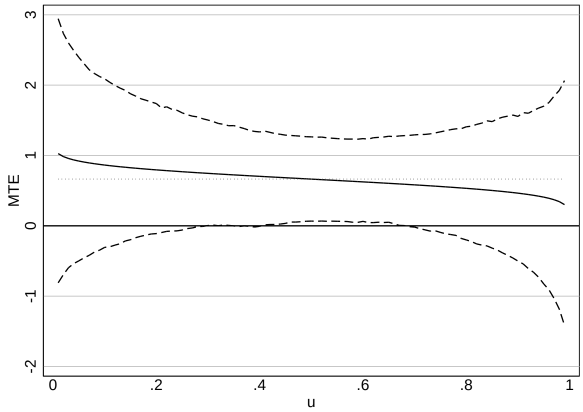
(c) Emotional



(d) Hyperactivity

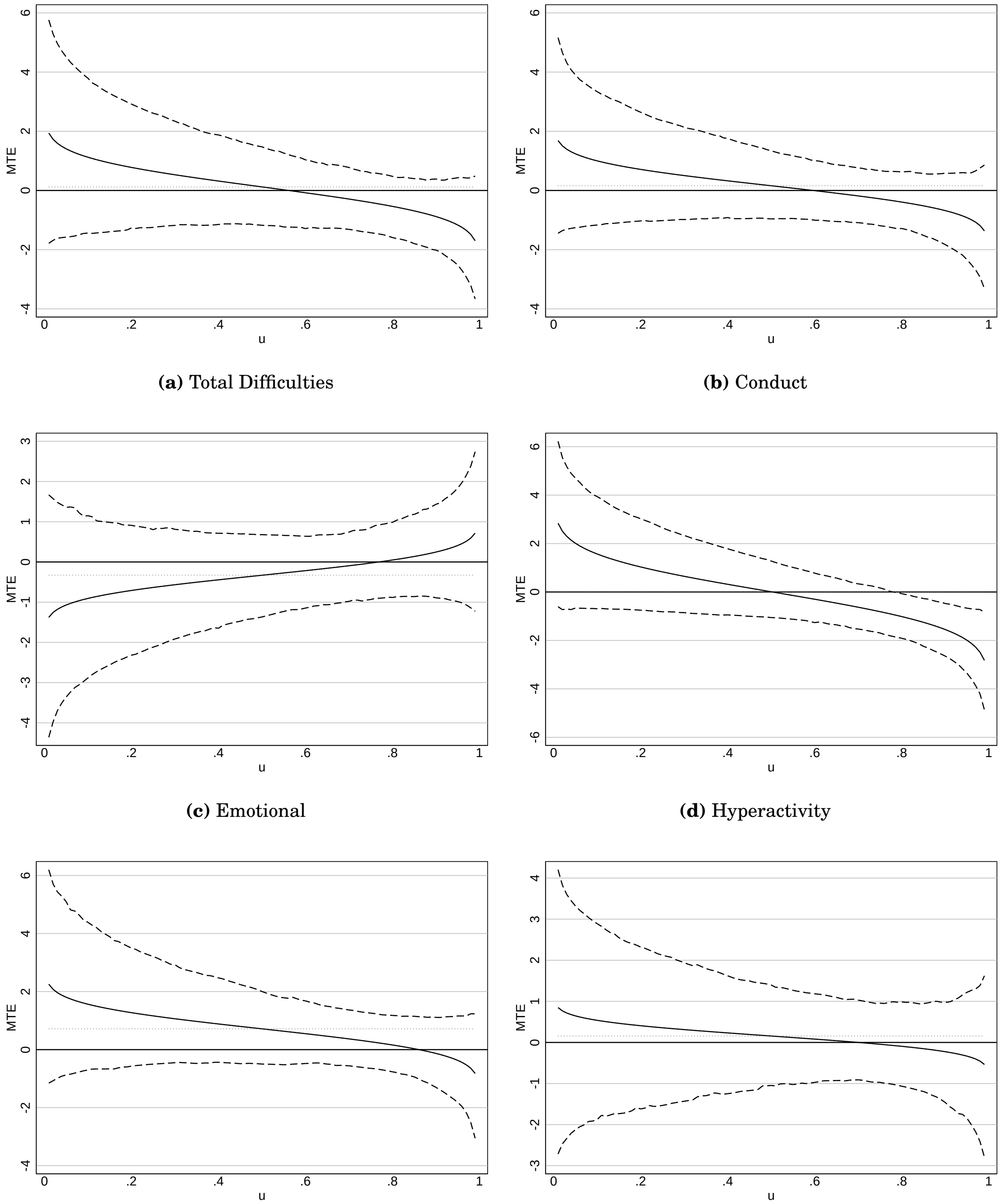


(e) Peer problems



(f) Pro-social

Figure 7.5: Marginal treatment effects for girls. Treatment: School Starting Age > 6.5. Sample: 30 days bandwidth. We use a parametric specification in which we first estimated a probit for the propensity of being old at school enrollment, and then computed the marginal treatment effects according to Heckman et al. (2006). Bootstrapped standard errors are computed using 1,000 replications. The dotted lines indicate the average treatment effect.



(a) Total Difficulties

(b) Conduct

(c) Emotional

(d) Hyperactivity

(e) Peer Problems

(f) Pro-social

Figure 7.6: Marginal treatment effects for boys. Treatment: School Starting Age > 6.5. Sample: 30 days bandwidth. We use a parametric specification in which we first estimated a probit for the propensity of being old at school enrollment, and then computed the marginal treatment effects according to Heckman et al. (2006). Bootstrapped standard errors are computed using 1,000 replications. The dotted lines indicate the average treatment effect.

scales do we find a jump outside the January 1st cutoff. This jump is smaller and considerably less precisely estimated than the jumps at January 1st on the total difficulties score, the hyperactivity scale, and the pro-social scale. All in all the placebo regression in Table 7.7 indicates that for the total difficulties score and the hyperactivity scale, for which we find effects in the main specification, the only jump of importance is the January 1st cutoff.

Table 7.7: Placebo regression

	Total Difficulties	Emotional Symptoms	Conduct Problems	Hyperactivity	Peer Problems	Pro-social
Cutoff 150 days before January 1	-0.01	-0.04	0.05	-0.03	0.03	-0.04
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Cutoff 100 days before January 1	0.04	0.02	0.04	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Cutoff 50 days before January 1	-0.01	-0.03	0.02	-0.01	-0.01	0.05
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)
Cutoff at January 1	-0.08*	-0.06	-0.05	-0.09*	-0.00	0.07*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Cutoff 50 days after January 1	0.00	-0.03	-0.03	0.01	0.06	-0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Cutoff 100 days after January 1	0.00	-0.04	-0.00	0.00	0.06*	-0.00
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Cutoff 150 days after January 1	0.04	0.02	0.01	0.03	0.05	-0.02
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
Observations	55,021	55,021	55,021	55,021	55,021	55,021
Bandwidth	Full	Full	Full	Full	Full	Full
Linear trend × cutoff	✓	✓	✓	✓	✓	✓
Quadratic trend × cutoff	✓	✓	✓	✓	✓	✓

Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$, Each column represents one regression of the dependent variable (the column title) on the seven indicators in the rows and on a quadratic trend on each side of the January 1st cutoff.

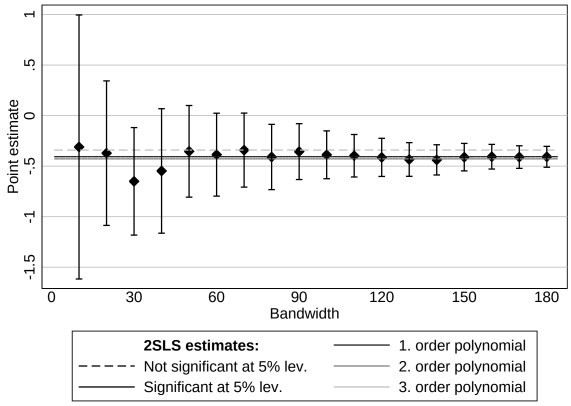
As the outcome variables are parent reported answers to the Strength and Difficulties Questionnaire, the effect may stem from the fact that parents compare their children to the class mates. It could therefore be a pure measurement effect. To assess this we consider the subsample of children who have older siblings. The intuition is that for children who have older siblings the parent should have another reference category than the class mates. If we find no effect for this subgroup, the main effect could be driven by pure measurement. This is not the case as shown by Table 7.8, in contrast the point estimates of the reduced form regression and the 2SLS regressions are all larger for children with older siblings. Also including the average age of the cohort (excluding the individual) has little impact on the results as Table A.8 in the Appendix shows.

In Figure 7.7 we assess the sensitivity to the bandwidth selection, and the parametric specification. Focusing on the hyperactivity scale we find that using a first, second or third order polynomial does not affect the point estimate notably. Also for almost all bandwidths the local specification coefficients are in line with the parametric first and second order polynomial specification.

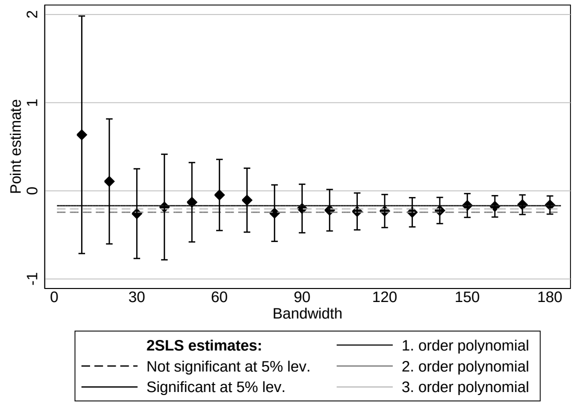
Table 7.8: The first stage, reduced form, and 2SLS results by sibling status

	First stage		Reduced form		2SLS	
Older Siblings	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
A. Local specification: 30 day bandwidth & linear trends interacted with January 1. cutoff						
Birthday ≥ Jan. 1	0.22**	0.14**				
	(0.04)	(0.03)				
Total difficulties score			-0.07	-0.15*	-0.34	-1.07*
			(0.07)	(0.06)	(0.32)	(0.52)
Emotional symptoms			0.01	-0.12	0.03	-0.82
			(0.07)	(0.06)	(0.34)	(0.49)
Conduct problems			-0.02	-0.07	-0.08	-0.53
			(0.07)	(0.06)	(0.31)	(0.47)
Hyperactivity			-0.14*	-0.16*	-0.63	-1.11*
			(0.07)	(0.06)	(0.33)	(0.52)
Peer problems			-0.02	-0.03	-0.08	-0.24
			(0.07)	(0.06)	(0.32)	(0.46)
Pro-social			0.06	0.12	0.30	0.86
			(0.07)	(0.06)	(0.31)	(0.50)
Observations	3,645	4,066	3,645	4,066	3,645	4,066
B. Parametric specification: Full sample & Quadratic trends interacted with January 1. cutoff						
Birthday ≥ Jan. 1	0.16**	0.16**				
	(0.02)	(0.02)				
Total difficulties score			-0.01	-0.11**	-0.08	-0.73**
			(0.04)	(0.04)	(0.25)	(0.25)
Emotional symptoms			0.02	-0.08*	0.11	-0.52*
			(0.04)	(0.03)	(0.27)	(0.23)
Conduct problems			0.00	-0.08*	0.01	-0.50*
			(0.04)	(0.04)	(0.25)	(0.25)
Hyperactivity			-0.05	-0.10**	-0.34	-0.65**
			(0.04)	(0.04)	(0.26)	(0.24)
Peer Problems			0.02	-0.04	0.13	-0.25
			(0.04)	(0.04)	(0.26)	(0.24)
Pro-social			0.02	0.07	0.12	0.43
			(0.04)	(0.04)	(0.25)	(0.25)
Observations	25,128	29,812	25,128	29,812	25,128	29,812
Covariates	✓	✓	✓	✓	✓	✓

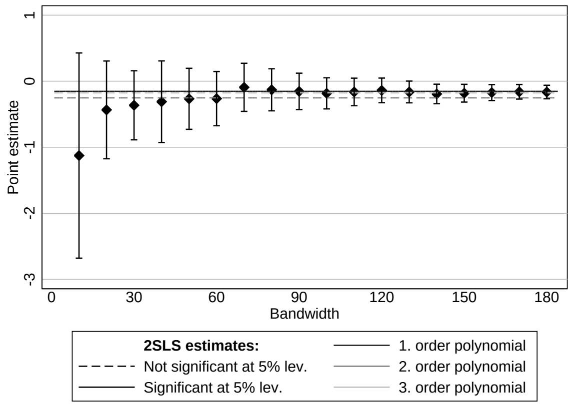
Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Each cell shows the estimate from a single regression. In columns (1) and (2) the dependent variable is school starting age (in years) which is regressed on an indicator for being born after January 1st, trends, and trends interacted with the January 1st cutoff. In columns (3) and (4) the SDQ measure is regressed on the same specification as in (1) and (2). Column (5) shows the results from a simple OLS regression of the dependent on variable on school starting age and the time trends. Columns (5) and (6) show the 2SLS results from estimating the SDQ measure on the predicted school starting age, the time trends, and the time trends interacted with the cutoff. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), and birth year fixed effects.



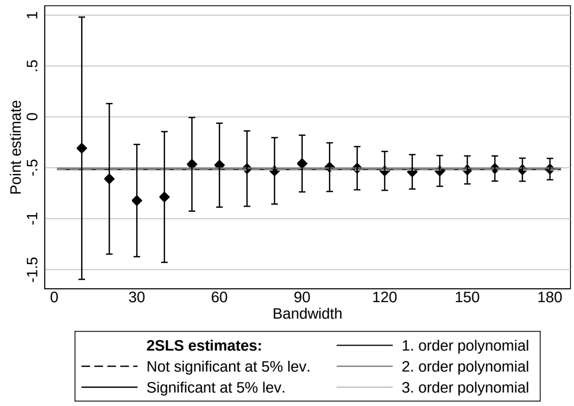
(a) Total difficulties



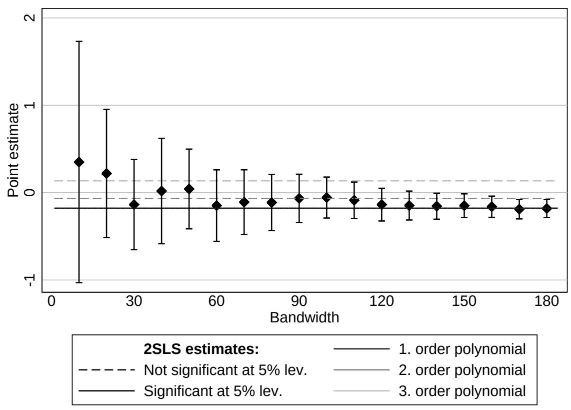
(b) Conduct



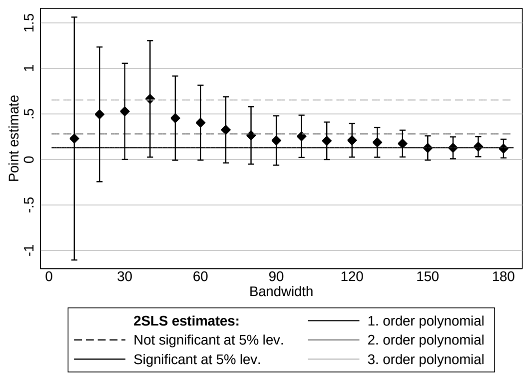
(c) Emotional symptoms



(d) Hyperactivity



(e) Peer problems



(f) Pro-social

Figure 7.7: Bandwidth sensitivity. Each diamond marker is the 2SLS point estimate from a local regression with the bandwidth size denoted on the x-axis. The bandwidth size increases in steps of 10 days. A bandwidth of 10 implies a sample of children born 10 days before and after January 1st. The horizontal lines are the 2SLS point estimate from a regression using the full sample with separate trends on each side of the January 1st cutoff. The lines are solid if the estimate is significant on a five percent level, and dashed if it is not significant on a five percent level.

It could also be the case that other interventions are changed by the January 1st cutoff, for example changes in pre- or postnatal care programs. To rule out that such programs cause the outcomes, Table A.6 in the appendix shows results using outcomes measured at age 18 months. In none of 12 cases do we find significant effects.

As a final robustness check we ran regressions using the survey reported school starting age, to assess the importance of a potential bias due to children retaking the kindergarten class. The results are shown in Table A.7 in the appendix. The results are very similar to the main results using the imputed school starting age based on national test data.

8. Discussion and conclusions

Using data from the Danish National Birth Cohort (DNBC) linked with Danish register based data we estimate the causal effect of school starting age on non-cognitive skills in the short run. We find strong effects of school starting age on the hyperactivity scale in the Strength and Difficulties Questionnaire when the children are seven years old. Being one year older at school enrollment improves the score on the hyperactivity scale by about 0.5-0.8 of a standard deviation, indicating decreased hyperactivity.

The effect is identified for compliers to the school starting rule, which states that children should enroll the calendar year they turn six. We find that compliers are considerably more likely to be girls, and that the effect is driven by girls with a low level of latent ability. For boys we only find a significant effect of school starting age on hyperactivity for the boys with the highest degree of latent ability. We find no clear evidence on the other scales of the Strength and Difficulties Questionnaire: The emotional symptoms scale, the conduct problems scale, the peer problems scale, and the pro-social behavior scale.

All children in our sample are seven years old when we measure the non-cognitive skills. Holding age constant implies that those who were older at enrollment are at the end of kindergarten class/grade zero, while those who were young at enrollment are at the end of grade one. The identified effects could therefore be driven by the fact that children are in different grades for two reasons: (1) The grades affect the non-cognitive skills differently. (2) The reference group is different. Regarding the first threat, especially for non-cognitive skills, holding age constant is likely to be more important than holding grades constant. While kindergarten class includes more element of "play and learn", both grades have class-room teaching, a centrally specified curriculum, and the same amount of minimum teaching hours. Regarding the second threat, results could be driven by the fact that those in grade zero are old compared to their cohort, and those in grade one are young compared to their cohort. However, we have shown results are robust to evaluating the subsample of children with older siblings, and to including a measure of the cohorts average age, both these robustness checks indicate that the findings are not driven by a pure reference group effect. Also, it seems unlikely that a measurement effect only affects one dimension of the SDQ.

The analysis is a based on a non-random survey of children, in which socio-economic advantaged parents are overrepresented. As compliers mainly consists of low-educated parents and effects are driven by girls with a low level of latent ability, it is likely that effects would be even stronger on a random sample of the population.

Compared to existing evidence this study is the first to use standardized measures of non-cognitive skills holding age constant. While existing evidence finds that effects of school starting age on outcomes of non-cognitive skills is driven by incapacitation, rather than human capital effects (i.e. higher human capital), our finding suggests that non-cognitive human capital indeed is affected in the short run. The fact that children who start school later have better hyperactivity outcomes may explain effects of school starting age on cognitive skills.

The survey used in this study will also collect SDQ scores for the same children at age eleven. It will be interesting to assess whether the effects remain four years after the short-run results identified in this paper.

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Appendices

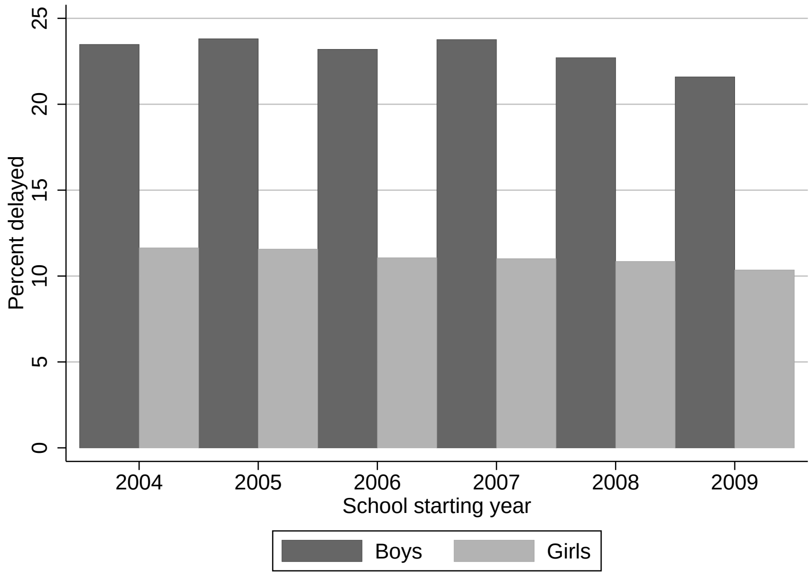


Figure A.1: Share of school entrants that are delayed. Imputed by when they participated in the first National Test.

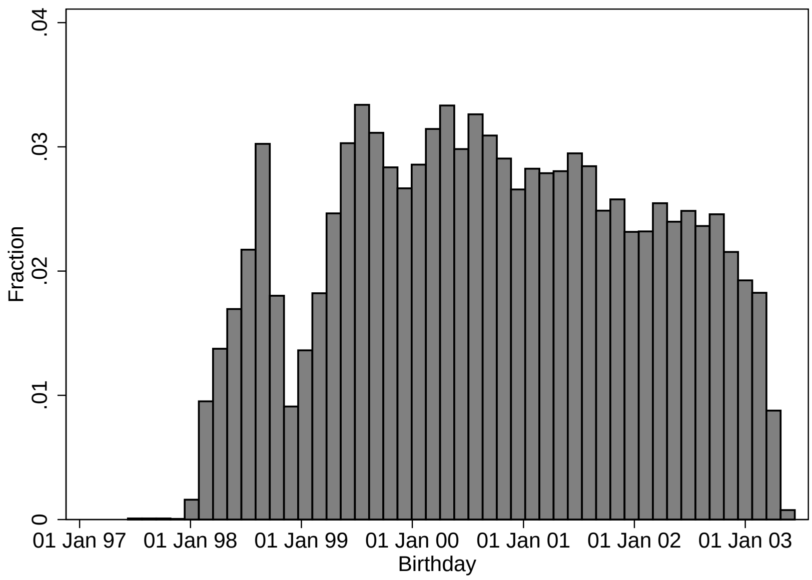


Figure A.2: Distribution of day of birth for the 55,058 in the final analysis sample.

Table A.1: Variable descriptives, Survey sample compared to population data, 30 days before and after the cutoff date.

	Population data			Survey			P-value
	Mean	SD	N	Mean	SD	N	
Birth weight	3475.60	621.63	51,527	3530.14	602.20	7,687	0.00
Girl	0.49	0.50	52,358	0.49	0.50	7,730	0.26
5min APGAR	9.94	2.46	51,315	9.90	1.86	7,652	0.26
Mother's age at childbirth	28.96	7.32	52,358	30.62	4.62	7,730	0.00
Mother's educational length	13.14	2.64	50,528	14.31	2.22	7,707	0.00
Mother is nonwestern	0.11	0.32	50,528	0.01	0.08	7,707	0.00
Mother is single	0.18	0.38	50,528	0.13	0.34	7,707	0.00
Mother's gross income	237.17	148.93	50,508	270.01	130.11	7,707	0.00
Mother is employed	0.70	0.46	50,528	0.85	0.36	7,707	0.00
Father's age at childbirth	30.87	9.64	52,358	32.05	7.63	7,730	0.00
Father's educational length	13.22	2.76	49,243	14.13	2.42	7,502	0.00
Father is nonwestern	0.12	0.33	49,243	0.01	0.12	7,502	0.00
Father's gross income	344.63	476.72	49,241	384.59	316.62	7,502	0.00
Father is employed	0.78	0.41	49,243	0.88	0.33	7,502	0.00

Notes: Birth weight is measured in grams. Educational length is measured in years. Parents are defined as non-western if they are immigrants to Denmark from a non-western country according to the classification by Statistics Denmark. The mother's single status is one if the child is living with the mother, and the mother is not married or cohabiting. The gross income is measured in 1,000 DKK and adjusted to the 2010 level using the consumer price index. The parents' employment is for November in the lagged year.

Table A.2: Test scores in Danish and mathematics and the five dimensions of the SDQ. Girls only

Subject	- - - Danish - - -			- - - Math - - -	
Grade	2	4	6	3	6
Emotional Symptoms Scale	0.02*	0.04**	0.02**	0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Conduct Problems Scale	-0.06**	-0.06**	-0.06**	-0.06**	-0.06**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Hyperactivity Scale	-0.17**	-0.16**	-0.15**	-0.15**	-0.10**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Problems Scale	-0.03**	-0.02*	-0.01	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pro-social Scale	-0.06**	-0.04**	-0.04**	-0.02*	-0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	9,642	18,304	12,023	14,910	11,989

Standard errors clustered on the school level in parenthesis. $^{*}p < 0.05$, $^{**}p < 0.01$. Each column shows results from one regression with test scores as the dependent variable, the five SDQ dimensions as independent variables and a set of covariates. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, mother's civil status, age at test monthly indicators (both for SDQ and the mathematics/Danish tests), school and birth year fixed effects. Both the five SDQ scores and the test scores are standardized.

Table A.3: Test scores in Danish and mathematics and the five dimensions of the SDQ. Boys only

Subject	- - - Danish - - -			- - - Math - - -	
Grade	2	4	6	3	6
Emotional Symptoms Scale	0.03**	0.04**	0.03**	0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Conduct Problems Scale	-0.04**	-0.05**	-0.05**	-0.04**	-0.06**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Hyperactivity Scale	-0.15**	-0.16**	-0.15**	-0.16**	-0.12**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Problems Scale	0.00	-0.00	0.01	0.02	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pro-social Scale	-0.04**	-0.05**	-0.03**	-0.04**	-0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	10,530	18,770	11,835	16,151	11,840

Standard errors clustered on the school level in parenthesis. * $p < 0.05$, ** $p < 0.01$. Each column shows results from one regression with test scores as the dependent variable, the five SDQ dimensions as independent variables and a set of covariates. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, mother's civil status, age at test monthly indicators (both for SDQ and the mathematics/Danish tests), school and birth year fixed effects. Both the five SDQ scores and the test scores are standardized.

Table A.4: SDQ, Big Five, and Raven Score

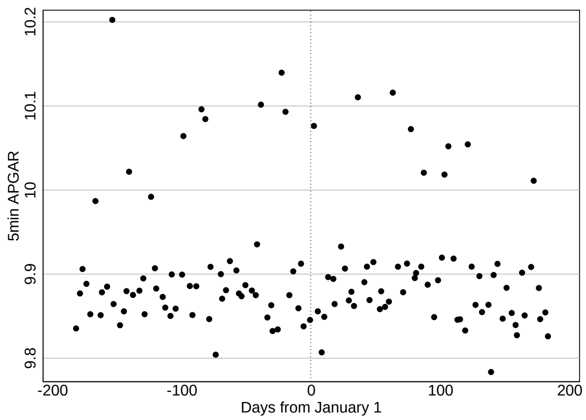
	- - - Big Five - - -					
	Extra-version	Agree-ableness	Conscient-iousness	Neuro-ticism	Openness	Raven
Conduct	-0.03*	0.05*	0.04*	0.01	0.01*	-0.10*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Emotional	0.13**	-0.01*	-0.05**	-0.31**	-0.04*	-0.14**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Hyperactivity	-0.07**	0.06**	0.18**	0.07**	0.02*	-0.24**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Peer problems	0.15**	0.07**	-0.00	-0.01*	-0.04*	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Pro-social	-0.09**	-0.07**	-0.08**	-0.03*	-0.07**	-0.10**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Mean of dep. variable	-0.00	0.00	-0.00	0.00	-0.00	8.25

Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$. Each column shows results from one regression with test scores as the dependent variable, the five SDQ dimensions as independent variables and a set of covariates. Covariates included are birth weight (indicators for each 20th percentile) father gross income (indicators for each tenth percentile), mother gross income (indicators for each tenth percentile), an indicator for whether the father has completed higher education, an indicator for whether the mother has completed higher education. The Big Five is from the 10-item short version of the Big Five Inventory (Rammstedt and John, 2007). The Raven score consists of 12 questions. The data source is the Danish Longitudinal Survey of Children. All children are 15 years old when measured.

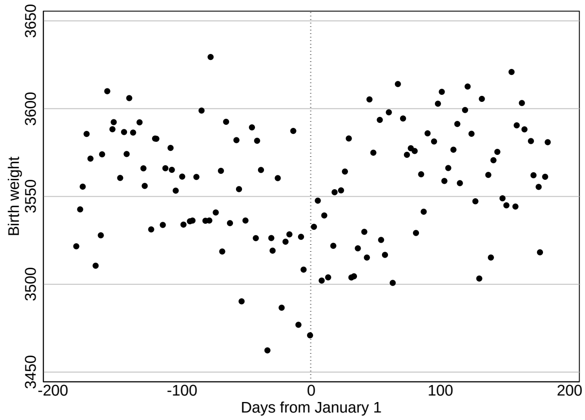
Table A.5: Auxiliary RD estimates, balancing of the covariates.

	(1)	(2)
Birth weight	16.32	17.21
	(28.99)	(16.72)
Girl	-0.02	0.00
	(0.02)	(0.01)
5min APGAR	0.07	-0.01
	(0.09)	(0.05)
Mother's age at childbirth	0.15	0.04
	(0.20)	(0.12)
Mother's educational length	0.16	0.02
	(0.10)	(0.06)
Mother is nonwestern	-0.01	-0.00
	(0.00)	(0.00)
Mother is single	-0.02	-0.01
	(0.02)	(0.01)
Mother's gross income	7.22	-4.35
	(5.83)	(3.35)
Mother is employed	0.02	0.00
	(0.02)	(0.01)
Father's age at childbirth	0.12	0.03
	(0.25)	(0.15)
Father's educational length	0.13	0.05
	(0.11)	(0.07)
Father is nonwestern	0.00	0.00
	(0.01)	(0.00)
Father's gross income	-5.09	1.64
	(19.69)	(9.06)
Father is employed	-0.00	-0.01
	(0.02)	(0.01)
Bandwidth	30 days	Full
Linear trend × cutoff	✓	✓
Quadratic trend × cutoff		✓

Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Regressions of the covariates on the indicator for being born on January 1st or later as well as time trends. Each cell represents a regression and shows the point estimate on the indicator for being born January 1st or later.



(a) 5 min. APGAR



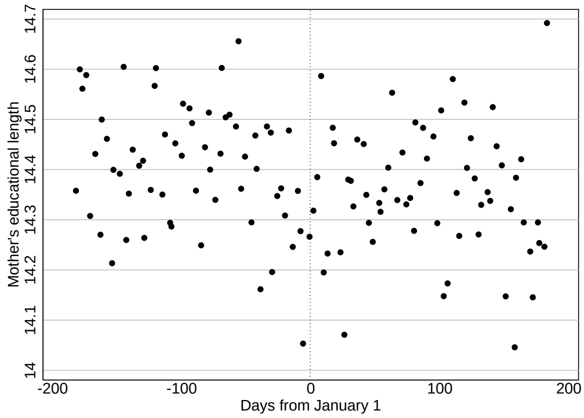
(b) Birth weight (grams)

Figure A.3: Birthday and child covariates. Full year bandwidth & 3 day bins.

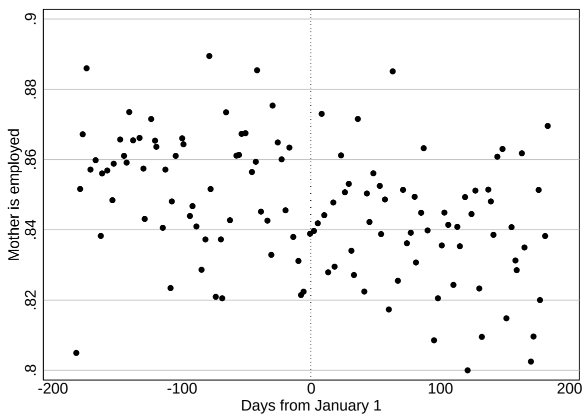
Table A.6: Placebo regressions with pre-treatment outcomes

	(1)	(2)	(3)	(4)
Can keep occupied for 15min aged 18m	-0.02	-0.04	0.03	0.00
	(0.10)	(0.10)	(0.07)	(0.07)
Turns pictures right aged 18m	0.22	0.24	0.10	0.08
	(0.14)	(0.14)	(0.10)	(0.10)
Makes word sounds aged 18m	0.04	0.04	0.02	0.01
	(0.04)	(0.04)	(0.03)	(0.03)
Observations	5,938	5,938	41,810	41,810
Bandwidth	30 days	30 days	Full	Full
Covariates		✓		✓
Linear trend × cutoff	✓	✓	✓	✓
Quadratic trend × cutoff			✓	✓

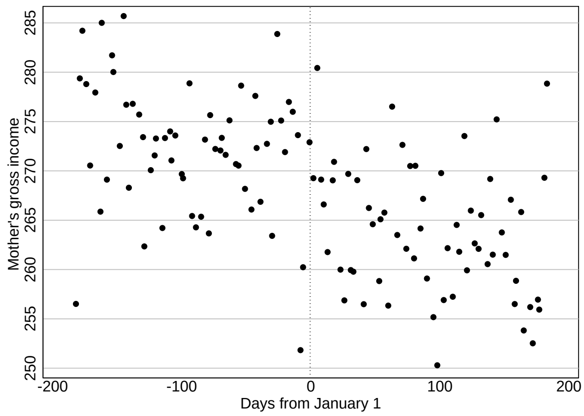
Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), and birth year fixed effects.



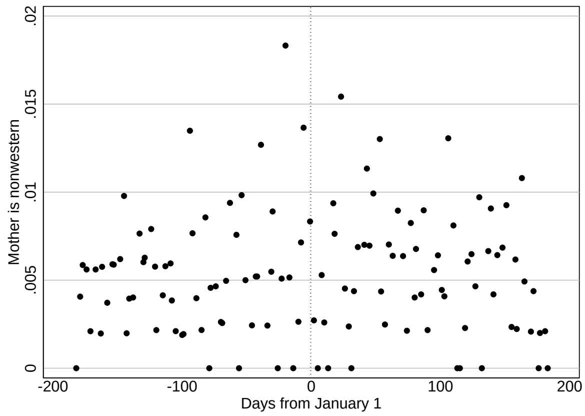
(a) Educational length



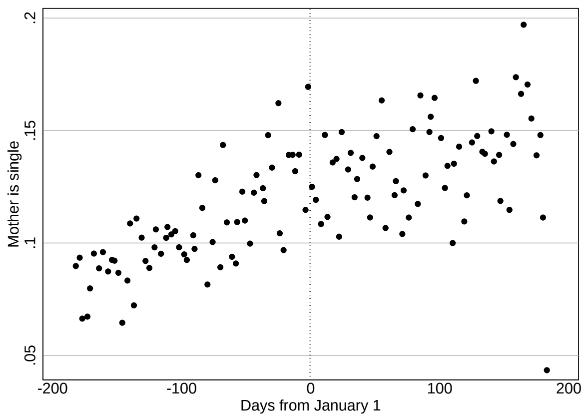
(b) Employed (November, lagged year)



(c) Gross income (lagged year)

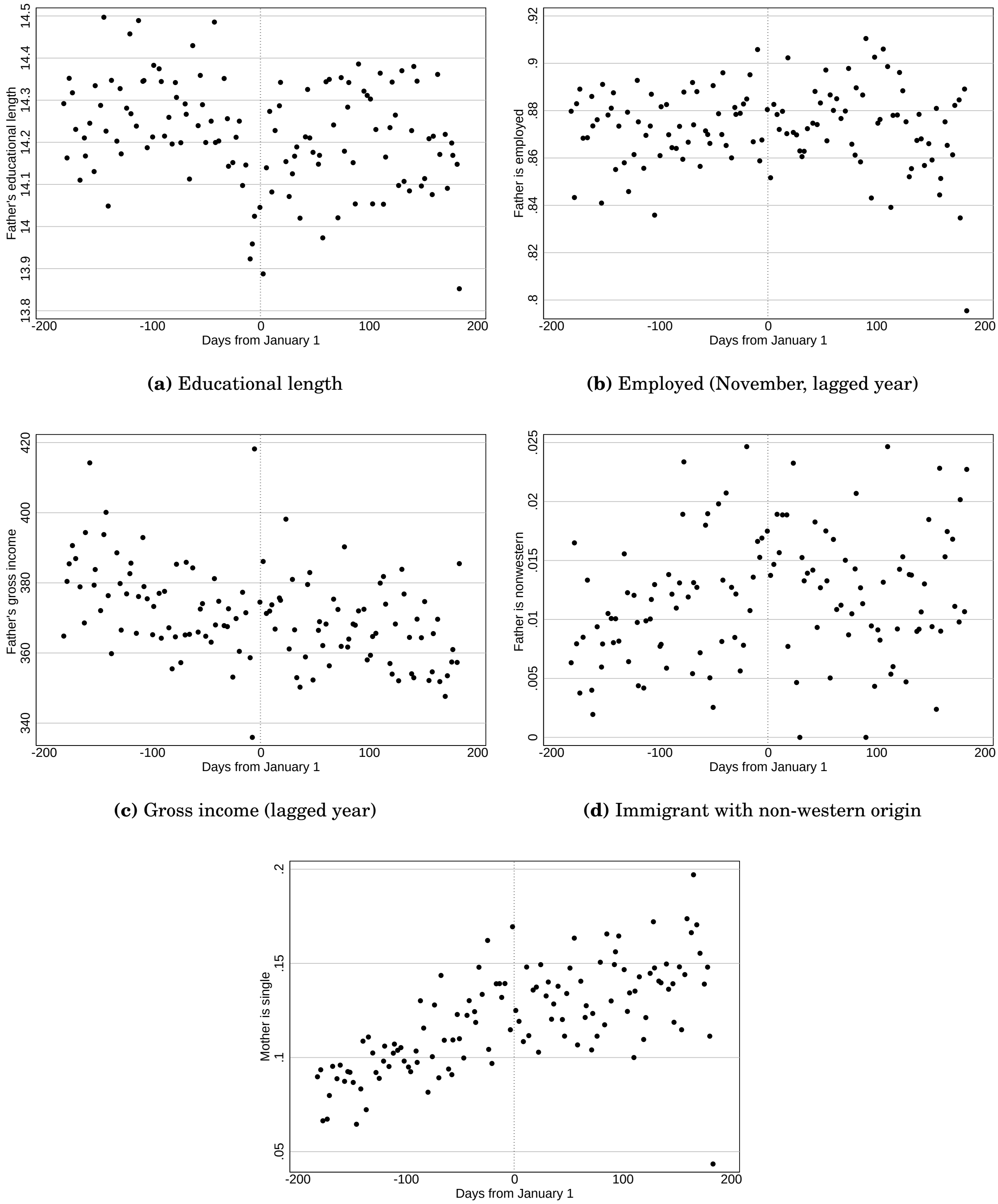


(d) Immigrant with non-western origin



(e) Single

Figure A.4: Birthday and mother covariates. Full year bandwidth & 3 day bins.



(a) Educational length

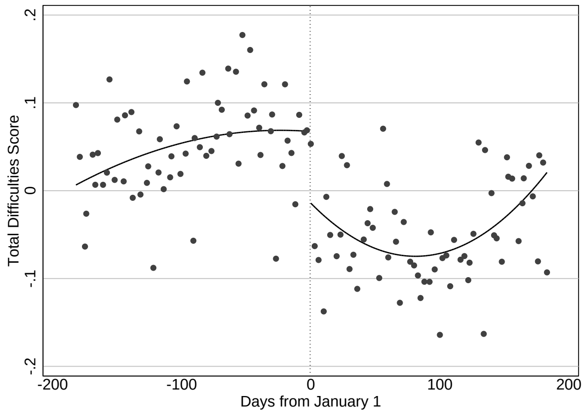
(b) Employed (November, lagged year)

(c) Gross income (lagged year)

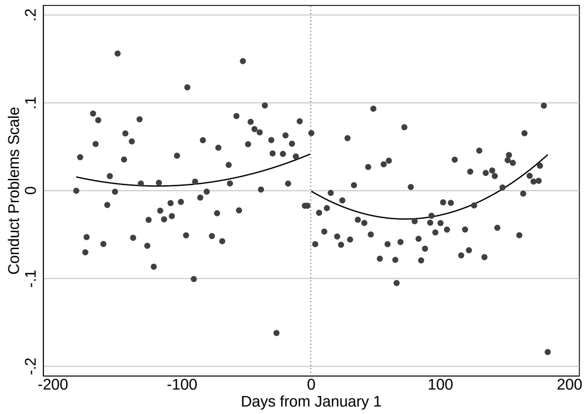
(d) Immigrant with non-western origin

(e) Single

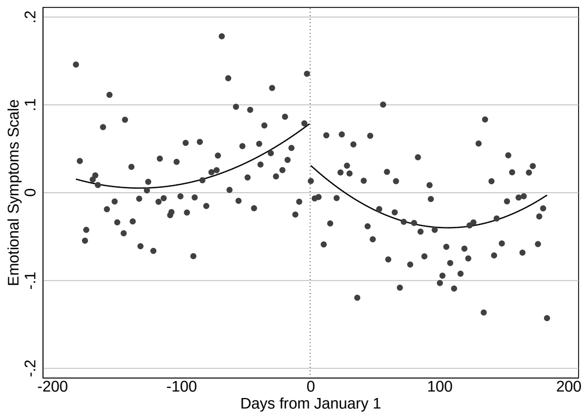
Figure A.5: Birthday and father covariates. Full year bandwidth & 3 day bins.



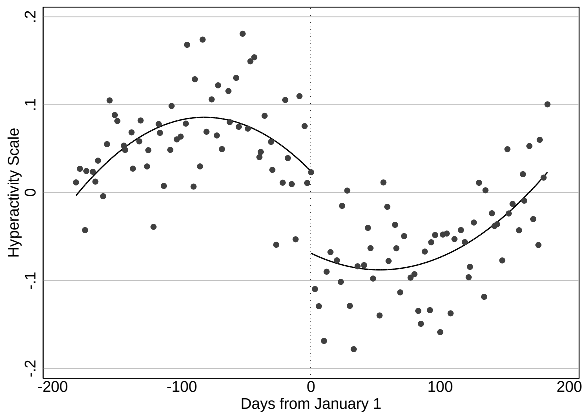
(a) Total difficulties score



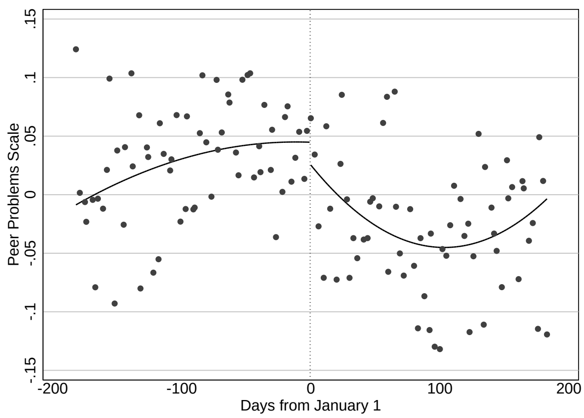
(b) Conduct problems



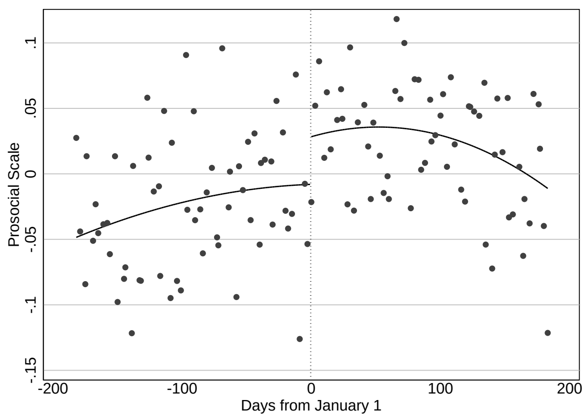
(c) Emotional symptoms



(d) Hyperactivity

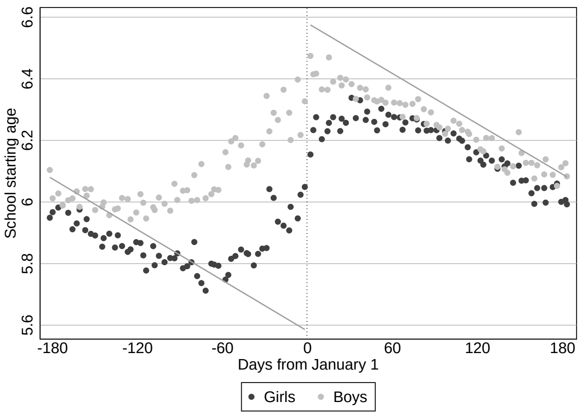


(e) Peer problems

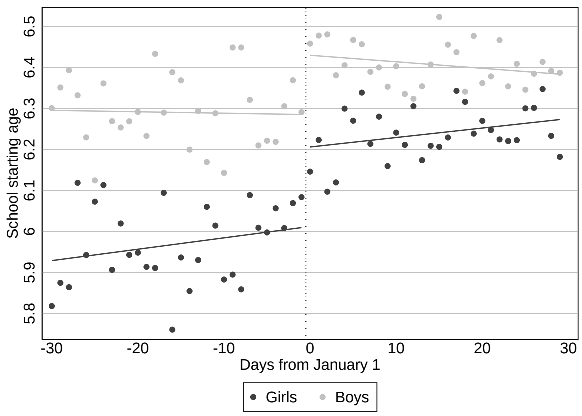


(f) Pro-social

Figure A.6: SDQ outcomes and date of birth. All outcome variables are standardized. 3 day bins with quadratic trends interacted with the January 1st cutoff (the trends are fitted to the raw data, not the bins).



(a) Full year bandwidth & 3 day bins.



(b) 30 days bandwidth & one day bins.

Figure A.7: Date of birth and school starting age, by gender

Table A.7: The first stage, reduced form, and 2SLS results using mother reported school starting age.

	First stage		Reduced form		OLS	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Local specification: 30 day bandwidth & linear trends interacted with January 1. cutoff							
Date of birth ≥ Jan. 1	0.17**	0.20**					
	(0.03)	(0.03)					
Total difficulties score			-0.13**	-0.12*	0.10**	-0.79*	-0.61*
			(0.05)	(0.05)	(0.02)	(0.32)	(0.25)
Emotional symptoms			-0.08	-0.06	0.05**	-0.47	-0.32
			(0.05)	(0.05)	(0.02)	(0.29)	(0.24)
Conduct problems			-0.05	-0.05	0.07**	-0.36	-0.27
			(0.05)	(0.05)	(0.02)	(0.28)	(0.24)
Hyperactivity			-0.15**	-0.15**	0.09**	-0.89**	-0.75**
			(0.05)	(0.05)	(0.02)	(0.32)	(0.26)
Peer problems			-0.04	-0.02	0.06**	-0.25	-0.14
			(0.05)	(0.05)	(0.02)	(0.28)	(0.24)
Pro-social			0.08	0.09*	-0.06**	0.50	0.48*
			(0.05)	(0.05)	(0.02)	(0.29)	(0.24)
Observations	7,652	7,652	7,718	7,718	7,652	7,652	7,652
B. Parametric specification: Full sample & quadratic trends interacted with January 1. cutoff							
Date of birth ≥ Jan. 1	0.13**	0.17**					
	(0.02)	(0.02)					
Total difficulties score			-0.08**	-0.07*	0.13**	-0.65**	-0.41*
			(0.03)	(0.03)	(0.01)	(0.23)	(0.16)
Emotional symptoms			-0.05	-0.04	0.06**	-0.39	-0.24
			(0.03)	(0.03)	(0.01)	(0.21)	(0.16)
Conduct problems			-0.04	-0.04	0.08**	-0.34	-0.23
			(0.03)	(0.03)	(0.01)	(0.21)	(0.16)
Hyperactivity			-0.09**	-0.08**	0.11**	-0.74**	-0.48**
			(0.03)	(0.03)	(0.01)	(0.23)	(0.16)
Peer problems			-0.02	-0.01	0.11**	-0.17	-0.08
			(0.03)	(0.03)	(0.01)	(0.21)	(0.16)
Pro-social			0.04	0.04	-0.07**	0.28	0.25
			(0.03)	(0.03)	(0.01)	(0.21)	(0.16)
Observations	54,298	54,298	55,021	55,021	54,298	54,298	54,298
Covariates		✓		✓			✓

Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Each cell shows the estimate from a single regression. In columns (1) and (2) the dependent variable is school starting age (in years) which is regressed on an indicator for being born after January 1st, trends, and trends interacted with the January 1st cutoff. In columns (3) and (4) the SDQ measure is regressed on the same specification as in (1) and (2). Column (5) shows the results from a simple OLS regression of the dependent on variable on school starting age and the time trends. Columns (6) and (7) show the 2SLS results from estimating the SDQ measure on the predicted school starting age, the time trends, and the time trends interacted with the cutoff. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), and birth year fixed effects.

Table A.8: The first stage, reduced form, and 2SLS results including cohort average age

	First stage		Reduced form		OLS	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Local specification: 30 day bandwidth & linear trends interacted with January 1. cutoff							
Birthday ≥ Jan. 1	0.20**	0.20**					
	(0.02)	(0.02)					
Total difficulties score			-0.12*	-0.11*	0.12**	-0.60*	-0.55*
			(0.05)	(0.05)	(0.03)	(0.25)	(0.24)
Emotional symptoms			-0.06	-0.06	0.13**	-0.31	-0.30
			(0.05)	(0.05)	(0.03)	(0.25)	(0.25)
Conduct problems			-0.07	-0.06	0.08**	-0.34	-0.31
			(0.05)	(0.05)	(0.03)	(0.25)	(0.24)
Hyperactivity			-0.15**	-0.14**	0.06	-0.74**	-0.71**
			(0.05)	(0.05)	(0.03)	(0.25)	(0.25)
Peer problems			-0.02	0.00	0.07*	-0.07	0.00
			(0.05)	(0.05)	(0.03)	(0.24)	(0.24)
Pro-social			0.08	0.08	-0.09**	0.39	0.37
			(0.05)	(0.05)	(0.03)	(0.25)	(0.24)
Observations	6,702	6,648	6,702	6,648	6,648	6,702	6,648
B. Parametric specification: Full sample & quadratic trends interacted with January 1. cutoff							
Birthday ≥ Jan. 1	0.18**	0.18**					
	(0.01)	(0.01)					
Total difficulties score			-0.06*	-0.06*	0.33**	-0.33*	-0.35*
			(0.03)	(0.03)	(0.02)	(0.16)	(0.16)
Emotional symptoms			-0.03	-0.03	0.22**	-0.16	-0.17
			(0.03)	(0.03)	(0.02)	(0.16)	(0.17)
Conduct problems			-0.04	-0.05	0.19**	-0.23	-0.26
			(0.03)	(0.03)	(0.02)	(0.16)	(0.16)
Hyperactivity			-0.07*	-0.08**	0.25**	-0.40*	-0.43**
			(0.03)	(0.03)	(0.02)	(0.16)	(0.16)
Peer problems			-0.01	-0.00	0.24**	-0.04	-0.00
			(0.03)	(0.03)	(0.02)	(0.16)	(0.16)
Pro-social			0.04	0.05	-0.13**	0.25	0.27
			(0.03)	(0.03)	(0.01)	(0.16)	(0.16)
Observations	47,719	47,304	47,719	47,304	47,304	47,719	47,304
Average age of cohort		✓		✓			✓
Covariates	✓	✓	✓	✓	✓	✓	✓

Robust standard errors in parenthesis. $^{**}p < 0.01$, $^{*}p < 0.05$. Each cell shows the estimate from a single regression. In columns (1) and (2) the dependent variable is school starting age (in years) which is regressed on an indicator for being born after January 1st, trends, and trends interacted with the January 1st cutoff. In columns (3) and (4) the SDQ measure is regressed on the same specification as in (1) and (2). Column (5) shows the results from a simple OLS regression of the dependent variable on school starting age and the time trends. Columns (6) and (7) show the 2SLS results from estimating the SDQ measure on the predicted school starting age, the time trends, and the time trends interacted with the cutoff. Covariates included are birth weight, 5 minute APGAR score, parental education, parents' age, parental income, parental employment, age at test (monthly indicators), and birth year fixed effects.

Chapter 3

The Long-Run Effects of Local Unemployment on Educational Attainment[†]

Hans Henrik Sievertsen
The Danish National Centre for Social Research (SFI) & The Department of Economics, University of Copenhagen

Abstract

Using Danish administrative data on all high school graduates from 1984 to 1999, I show that local unemployment has both a short- and a long-run effect on school enrollment and completion. The short-run effect causes students to advance their enrollment, and consequently their completion, of additional schooling. The long-run effect causes students who would otherwise never have enrolled to enroll and complete schooling. The effects are strongest for children of parents with no higher education. These results imply that inter-generational mobility is weakest in economic good times.

1. Introduction

The opportunity costs of education are reduced in recessions because the probability of finding unskilled employment, and therefore the expected foregone earnings, is lower. Consequently, enrollment rates in post-compulsory education increase during recessions and decrease during expansions. Empirical research confirms that demand for education is counter-cyclical (Pissarides, 1981; Fredriksson, 1997; Rice, 1999; Card and Lemieux, 2000; Bedard and Herman, 2008). Despite a vast amount of research, there is no evidence on whether labor market conditions have a long-run effect on educational attainment. The observed counter-cyclicality could be driven by individuals who postpone their school enrollment in economic good times and return to education in bad times.

[†] *I thank Paul Bingley, Martin Browning, Mette Ejrnæs, Niels-Jakob Harbo Hansen, Bo Honoré, Søren Leth-Petersen, and Lukas Wenner for valuable comments and suggestions. This paper also benefited from comments by participants at the annual workshop of the European Doctoral Group in Economics in Munich, September 2012; at the CAM December Workshop 2012 in Copenhagen, December 2012; and at the Workshop on the Economics of Successful Children in Aarhus, January 2013.*

This study contributes to the literature by assessing this long-run impact of local labor market conditions on educational attainment. Using Danish administrative data on all high school graduates from 1984 to 1999, I estimate the effect of local unemployment one month before high school graduation on the probability of continued and completed schooling within ten years. The identifying variation in unemployment comes from local labor market shocks. A ten-year panel for each of the 16 cohorts of graduates allows me to estimate the detailed long-run effects of local unemployment on school enrollment and completion. This paper also contributes to the literature by also assessing whether short- and long-run effects are heterogeneous with respect to parental background.

I analyze the importance of local–not national–labor market conditions for the following two reasons. First, the individual is presumably best informed about the local labor market. Second, using local unemployment and enrollment rates gives variation over time and area, thereby allowing me to identify the causal effect of unemployment on school enrollment and completion. To rule out any reverse causality issues due to students mobility I record the home municipality four years before the individuals graduate from high school.

Using Danish data, I find that a one percentage point increase in local unskilled unemployment increases the probability of continued schooling immediately after high school by 0.17 percentage points, the probability of continued schooling within ten years by 0.07 percentage points, and the probability of completed additional schooling within ten years by 0.07 percentage points. After about five years the effect of unemployment, at the time of high school graduation, on enrollment stabilizes and remains constant, indicating that a ten-year horizon is sufficient for evaluating the long-run effect. I find that both the short- and long-run effects are strongest for children of parents with no higher education. This finding implies that intergenerational educational mobility is weakest in economic good times.

The existing literature has typically included either a measure for general unemployment or a measure for youth unemployment. I show that including both skilled and unskilled unemployment is necessary for obtaining unbiased estimates. Skilled unemployment affects the expected return to education, while unskilled unemployment affects the opportunity costs. Consequently, skilled unemployment affects the enrollment decision in the opposite direction than unskilled unemployment. Results in the literature, based on only one measure for unemployment, may therefore underestimate the counter-cyclicality of school enrollment.

My results show that local business cycles have two effects on educational enrollment. The short-run effect causes students who would normally continue education at some point to continue earlier. The long-run effect causes individuals who would otherwise never again enroll in education to enroll and complete their education. As the results confirm that labor market conditions affect the educational decision, policy makers can therefore increase enrollment, and consequently completion rates, by making youth employment less attractive.

This paper is organized as follows. Section 2 describes the conceptual framework. Section 3 provides a brief review of existing literature, and section 4 describes the empirical strategy and the identifying assumptions. Section 5 explains the data, and section 6 presents the results. Section 7 concludes.

2. The model and institutional background

In this section I formalize the educational choice in a simple model and relate it to the Danish institutional setting. The purpose of the model is to illustrate why both skilled and unskilled unemployment matter for the educational choice. Furthermore, including the tax and unemployment systems in the model allows me to assess the role of two important aspects of the Danish institutional setting.

2.1 A simple model of educational choice

I consider a two-period model where the individual who just left high school can decide to immediately join the labor market or to continue in education and join the labor market in the next period. In the first period the individual consumes c_1, saves s, and either works ($e = 0$) or attends education ($e = 1$). The direct costs of education T_i, which vary by individual, i, include direct pecuniary costs (e.g., fees and books) as well as non-pecuniary costs (e.g., effort and stress). In the second period the individual works, and consumes c_2. The duration of period one and two are identical. Unskilled workers receive a wage w_0 and skilled workers receive a wage w_e. The tax on unskilled wage income is t_0, and the additional skilled wage income, $w_e - w_0$, is taxed at a rate t_e. If $t_e > t_0$ the tax system is progressive. If $t_e = 1$ there are no pecuniary returns to education, because the entire wage premium is taxed.

The unemployment rate for unskilled workers is denoted u_0, while the unemployment rate for skilled workers is u_e. There are unemployment benefits for unskilled workers, b_0, and for skilled workers, b_e. The individual earns an interest r on the savings from period one (or pays interest on debt if $s < 0$). Capital markets are assumed to be perfect, thus ruling out any credit constraints. Utility is representable by $u(c_1, c_2)$, with $u'_{c_1}, u'_{c_2} > 0$, $u''_{c_1c_1}, u''_{c_2c_2} < 0$ and $u''_{c_1c_2} = 0$. Combining period one and two's budget constraints gives the following inter-temporal condition:

$$
\begin{aligned}
c_1 + \frac{c_2}{1+r} \leq & (1-e)[(1-u_0)w_0(1-t_0) + u_0 b_0] - eT_i + \frac{(1-e)[(1-u_0)w_0(1-t_0) + u_0 b_0]}{1+r} \\
& + \frac{e[(1-u_e)\{(w_e - w_0)(1-t_e) + w_0(1-t_0)\} + u_e b_e]}{1+r}
\end{aligned} \tag{3.1}
$$

If the individual joins the labor market in period one, he or she receives either a wage income or unemployment benefits. If the individual attends education there is no income in period one. In period two, the individual is either skilled or unskilled, and receives the corresponding wage or unemployment benefits.

The individual observes unemployment rates, assume that these will stay constant, and decides to choose education or the labor market. This assumption is not required for making demand for education counter-cyclical. It is sufficient that changes in unemployment rates affect the immediate probability of unskilled employment and therefore the opportunity costs of education. The individual chooses the consumption and education levels that maximizes utility subject to the inter-temporal budget constraint (3.1). As education is not included in the objective function, the optimal choice of whether to choose education can be found by maximizing the budget set with respect to e. Let $\overline{T}$ be the level of direct costs that makes an individual indifferent between attending and not attending. The individual will continue in education if $T_i \leq \overline{T}$:

$$T_i \leq \overline{T} \equiv \frac{\overbrace{(1-u_e)\{(w_e-w_0)(1-t_e)+w_0(1-t_0)\}+u_e b_e-(1-u_0)(1-t_0)w_0-u_0 b_0}^{I}}{1+r} - \overbrace{(1-u_0)w_0(1-t_0)-u_0 b_0}^{II} \tag{3.2}$$

Choosing to stay in education is optimal if the direct costs are lower than the discounted return (I) minus the opportunity costs of education (II). The return consists both of the net wage gain $(w_e - w_0)(1-t_e)$ and the higher unemployment benefits b_e. If the unemployment benefits are identical to the net wage incomes (i.e. a 100% replacement rate) unemployment has no impact on the educational decision.

I describe the Danish institutional setting and discuss the role of unemployment and taxation and in the following subsections.

2.2 The role of taxes and unemployment benefits

As $\overline{T}$ increases, more individuals will continue in education because the level of individual direct costs that make individuals indifferent between attending and not attending education increases. The direct costs of the marginal person is increasing in unskilled unemployment. Higher unskilled unemployment makes education more attractive because the opportunity costs are reduced. Consequently, worse labor market conditions for unskilled workers will induce more individuals to enroll in education. The harder it is to find employment, the more likely it is that an individual will have to rely on unemployment benefits b_0. As long as these benefits are lower than the unskilled wage income w_0, the opportunity costs of education will vary over the business cycle. In contrast an increase in skilled unemployment reduces the expected return to education and therefore discourages education.

The relative size of unemployment benefits plays an important role for how much students react to labor market conditions in their educational choice. A replacement rate of 100% for unskilled workers corresponds to setting $b = w(1-t_0)$ in which case unemployment cancels out in (3.2). Therefore, the cyclicality of schooling is smaller when the replacement rate is higher. With a replacement rate of 100%, it plays no role for an individual's income whether he or she is employed or not. Consequently, the educational decision is independent of the labor market conditions.

The more progressive the income tax system is, the smaller is the net wage premium and the less attractive education is. Setting $t_e = 1$ implies that there is no pecuniary return to education, because the entire premium is taxed away. The taxes have the same effect as the unemployment benefits, because individuals care about the net values. The difference between being employed and unemployed is thus reduced, making fluctuations in unemployment less important for the schooling decision.

2.3 The Danish institutional setting

Denmark has two institutional systems for unemployment: unemployment benefits and social assistance. Individuals who are members of an unemployment fund may be eligible for unemployment benefits. Membership of an unemployment fund is voluntary, with eligibility for unemployment benefits depending on previous employment and education. Danish high school graduates rarely satisfy these requirements, because very few belong to an unemployment fund. Moreover, they do not have sufficient work experience and therefore are dependent on social assistance. Social assistance is available for the non-insured unemployed with less wealth than 10,000 DKK (about 1,130 GBP). The monthly gross welfare benefit is between 3,123 DKK (about 350 GBP) and 13,345 DKK (about 1,510 GBP), per month.[1] The lowest rate is for individuals living with their parents; the highest rate, for single parents living on their own.

During the period I study income was taxed in a three-bracket system. The marginal income tax starts at approximately 45% and tops at about 63%.

Compulsory education begins in August of the year that children turn seven and ends after nine years of schooling or in August of the year they turn 17. After leaving compulsory education, an individual can choose between three-year upper secondary school (high school), vocational training (apprenticeship), or the labor market. Completing high school allows access to university education and shorter educational programs. Almost all Danish educations are free, and students older than 18 are eligible for a monthly student grant (with a gross-value of 2,728 to 5,486 DKK, about 310 - 620 GBP). All students in higher education receive the highest grants if they are not living with their parents. An overview of the Danish education is shown in Fig. 2.1.

High unemployment benefits and a high degree of redistribution through the income tax system implies that the income difference between being employed and unemployed is low. The labor market condition will presumably have a very low impact on the educational decision in Denmark, and the estimates found using Danish data may therefore be a lower bound. The educational subsidies in Denmark could make education attractive as a replacement for unemployment benefits. As I study both enrollment and completion effects, I can assess the size of this issue. If the enrollment effect is larger than the completion effect, students might enroll in education only to receive the subsidy.

In sum, the standard human capital model predicts that optimal level of schooling is a function of foregone earnings, direct costs, and expected return. The foregone earnings and the expected return depend on the tax and benefit system, while the direct costs depend on the tuition fee and subsidy policy. The Danish system, with its high unemployment benefits and high income taxes, reduces the income difference between being unemployed and employed.

3. A Brief Review of the Literature

A rich literature exists on the link between school enrollment and both national labor market conditions (Pissarides, 1981; Fredriksson, 1997; McVicar and Rice, 2001; Dellas and Sakellaris, 2003; Ewing et al., 2010) and sub-national labor market conditions (Rivkin, 1995; Rice, 1999; Al-

[1] Wealth includes both liquid and illiquid wealth (e.g., property, cars). All values in this article are for 2011.

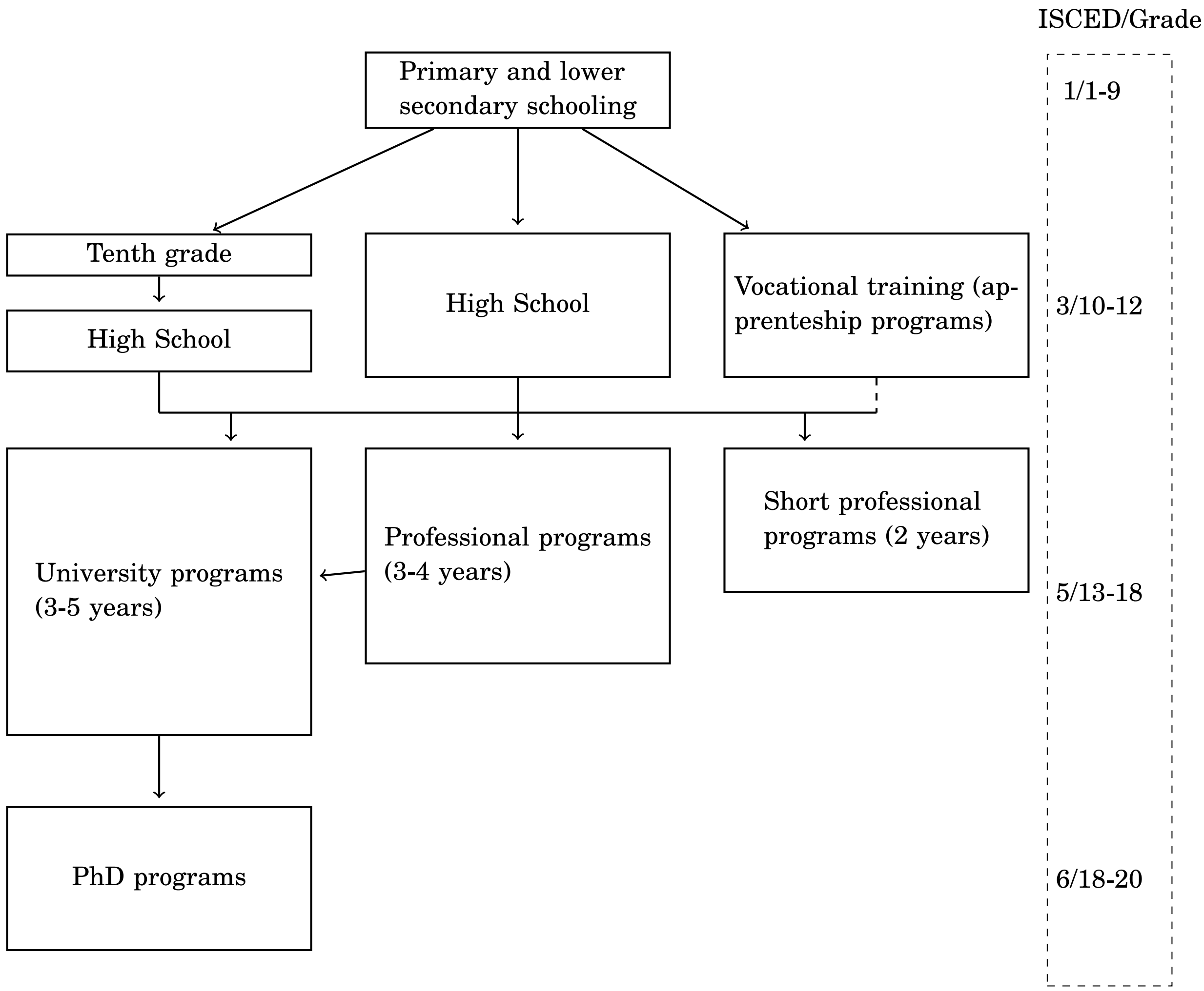


Figure 2.1: The Danish Education System. The ISCED and grade levels are shown to the right. ISCED is the UNESCO 1997 International Standard Classification of Education.

bert, 2000; Card and Lemieux, 2000; Petrongolo and San Segundo, 2002; Dellas and Sakellaris, 2003; Giannelli and Monfardini, 2003; Bedard and Herman, 2008; Flannery and O'donoghue, 2009). Table 3.1 summarizes the main contributions. All studies find that demand for schooling is counter-cyclical, except for Bedard and Herman (2008), who find that men's enrollment in masters programs is pro-cyclical. Evidence covers Canada, Ireland, Italy, Spain, Sweden, the United Kingdom, and the United States, and all levels of post-compulsory schooling. The main measure for the business cycle condition is unemployment, although inflation, earnings, growth rates and employment rates have also been used.

Dellas and Sakellaris (2003) find that the cyclicality is similar across gender but not across ethnicity. In contrast, Pissarides (1981); Card and Lemieux (2000); Petrongolo and San Segundo (2002); Rivkin (1995) and Bedard and Herman (2008) find that men react more to business cycle fluctuations than women. Rice (1999) finds that the cyclicality decreases in ability, while Bedard and Herman (2008) find that the cyclicality of postgraduate schooling among men is driven by those with the highest GPA. Rice (1999) finds that the effects are strongest in recessions, but Dellas and Sakellaris (2003) conclude that the cyclicality is symmetric in economic recessions and expansions. Demand for schooling is counter-cyclical from upper secondary schooling (Rice, 1999) through graduate school (Bedard and Herman, 2008).

Compared to these studies, the contribution of this paper is fourfold. First, Dellas and Sakellaris (2003) observe that the increase in college enrollment in recessions is driven by new high school graduates, indicating the existence of a long-run effect. I explicitly analyze this effect.

Second, this paper is the first to analyze heterogeneity with respect to parental background. Children of well-educated parents are likely to become highly educated. The imperfect intergenerational educational mobility is observed even in Denmark, where education is free and students are subsidized (Holm and Jæger, 2008). I analyze whether the inter-generational educational link varies over the business cycle. The outcome is of policy interest, because if, for example, children of less-educated parents react more to fluctuations in labor market conditions, policies that make youth employment less attractive can improve inter-generational educational mobility.

Third, I show that including both skilled and unskilled labor market conditions is crucial for obtaining unbiased estimates. The only other studies to consider both skilled and unskilled unemployment rate and obtain the expected signs are Pissarides (1981) and Fredriksson (1997), both of whom estimate time-series models using U.K. and Swedish data, respectively. The only micro-level study to include both rates is Albert (2000), who finds a positive effect of both skilled and unskilled unemployment on the demand for schooling.

Fourth, I include fixed effects on a very detailed level. Rice (1999) is among the first to realize that sub-national variation in unemployment rates may be due to unobserved characteristics that are also correlated with the school enrollment decision. Unfortunately, her fixed effects are at the regional level, while the variation in unemployment is at the county level. The first contribution to include fixed effects at the same level as the variation in enrollment and labor market conditions is Card and Lemieux (2000), who use data on 15 Canadian provinces and American regions. They are also the first to remove both year and regional fixed effects. Controlling for both these effects is crucial, because the variation in labor market conditions is on both these dimensions.

The only other two papers to use both area and year fixed effects are Bedard and Herman (2008), who include state and year fixed effects, and Flannery and O'donoghue (2009), who include seven waves and regional dummies. In this paper I use variation over 271 municipalities over 16 years. Doing so has two advantages relative to the existing evidence. First, as the municipalities cover a relatively small area (i.e., about 110 high school students per municipality per year), my fixed effects are therefore very detailed. Second, I am able to cluster my standard errors on the area level because the number of clusters is sufficiently large.

4. Empirical Strategy

The objective of the empirical analysis is to estimate the causal effect of local labor market conditions on individual school enrollment and completion behavior. I can only identify this effect if the

Table 3.1: Empirical findings on the counter-cyclicality of schooling

Study	*Type*	*Data*	*Outcome*	*Cyclicality measure*	*Fixed effects*	*Heterogeneity*	*Is schooling counter-cyclical?*
Pissarides (1981)	Time-series	UK; nat; 1955-1978	Schooling aged 16	Unemployment (youth, graduate, & general)	NA	Gender	Yes^H
Rivkin (1995)	Micro-level (Mult. logit)	US; community; 1980	Post high school	Unemployment, wage return	No	Gender, ethnicity	Yes^H
Fredriksson (1997)	Time-series (W2SLS)	SWE; nat; 1967-1991	Schooling aged 22	Unemployment & wages (skilled & unskilled)	NA	No	Yes
Rice (1999)	Micro-level (Logit)	UK; loc; 1988,1990, 1991	Schooling aged 16-17	Unemployment	Region	Gender; GCSE; up/downturn	Yes^H
Albert (2000)	Micro-level (Logit)	ESP; reg; 1987-1998	Schooling aged 19-24	Unemployment (skilled & unskilled)	Year	No	Yes
Card and Lemieux (2000)	Micro-level (WLS)	US/CAN; reg; 1971, 1981, 1991	Schooling aged 19-24	Employment	Region & Year	Gender	Yes^H
McVicar and Rice (2001)	Time-series (CVAR)	UK; nat; 1954-1994	Schooling aged 16	Unemployment	NA	Gender	Yes ^{H}
Petrongolo and San Segundo (2002)	Micro-level (Logit)	ESP; reg; 1987,1991;1996	Schooling aged 16-17	Employment	Region	Gender	Yes ^{H}
Dellas and Koubi (2003)	Time-series	US; reg; 1950-1990	Schooling aged 16-35	Unemployment, wages, interest rates	NA	Age groups	Yes^H
Dellas and Sakellaris (2003)	Micro-level (probit)	US; reg; 1968-1988	Schooling aged 18-22	Unemployment	State, trends	Up/downturn, gender, ethnicity	Yes ^{H}
Giannelli and Monfardini (2003)	Micro-level (Mult. probit)	ITA; reg; 1995	Schooling aged 18-32	Unemployment & lifetime earnings	Region	Gender	Yes ^{H}
Bedard and Herman (2008)	Micro-level (probit)	US; reg; 1993-2001	Postgraduate schooling	Unemployment	State & year	GPA, gender, major, program	Yes^H
Flannery and O'donoghue (2009)	Micro-level (logit)	IRE; reg; 1994-2001	Schooling aged 17-22	Employment & lifetime earnings	Region & wave	No	Yes
Ewing et al. (2010)	Time-series (VAR)	US; national; 1963-2004	College enrollment	Growth and inflation	NA	Gender	Yes^H

This table by no means cover all contributions but only highlights the main trends and developments.
^{H} indicates that cyclicality is found to be heterogeneous.

local labor market conditions are uncorrelated with the individual's unobserved characteristics. To obtain such exogenous variation, I exploit changes in skilled and unskilled unemployment over time and area. This strategy allows me to remove unobserved time-invariant characteristics at the same level as the variation in unemployment.

Local unemployment rates may reflect the skills composition of the local labor force and the composition of businesses. Both these characteristics might be correlated with unobserved characteristics of the individual, such as abilities and ambitions. I therefore include municipality fixed effects to control for all time-invariant unobserved municipality characteristics.

National education policy might react to business cycles, for example, by increasing the supply of education in recessions. To ensure that the effect of unemployment does not capture any policy effect, I also include annual fixed effects. The remaining variation in unemployment and enrollment therefore stems from differences in these rates over time and area. Such local variation could come from firms that decide to open or close a branch in a municipality.

The remainder of this section describes the short- and long-run estimation strategies and the underlying identifying assumptions.

4.1 The supply of education

The supply of education could vary over area and over the business cycle. Moreover, if a large share of a cohort continues in education, there may be general equilibrium effects. An advantage of evaluating local labor markets is that, doing so minimizes the issue of the supply, because year and area fixed effects are removed. As the local variation in supply of education is fixed over time, the issue of supply of education is minimized.

4.2 Short-run effects

The starting point is a short-run estimation in which the dependent variable takes the value of one if the individual continues in education immediately after graduating from high school. This variable is regressed on the local unemployment rates measured just before high school graduation, a set of controls, as well as year and municipality dummies. A natural strategy would be to apply a non-linear model, for example probit or logit. However, as the maximum likelihood estimations of these models yield almost identical results to those of a linear probability model (LPM), the LPM is preferred for its simplicity. The following equation is estimated by means of ordinary least squares (OLS).

$$en_{imt} = \alpha_0 + \alpha_1 une_{umt} + \alpha_2 une_{smt} + \boldsymbol{\beta}_0 \boldsymbol{X}_{imt} + \boldsymbol{\beta}_1 \boldsymbol{M}_m + \boldsymbol{\beta}_2 \boldsymbol{T}_t + \varepsilon_{imt} \tag{3.3}$$

Where en_{imt} is the enrollment indicator for individual i living in municipality m graduating from high school in June year t. The variable une_{umt} is the unskilled *youth* unemployment rate in municipality m in *May*, year t. T_t and M_m are a set of dummies for year and municipality, respectively. To avoid a potential omitted variable bias problem in the parameter-estimate on unskilled unemployment, I include the unemployment rate for skilled workers, une_{smt}.

$\boldsymbol{X}_{imt}$ is a vector of controls. I include cohort size to control for increased demand for jobs and education, and the labor force size to handle potential feedback effects, as explained in subsection 4.4 Given a rich literature on the importance of parental background for the educational choice (see e.g. Willis and Rosen (1979)), I include a measure of parents education, employment status, wealth, and income (as well as squared wealth and income terms). As siblings may influence the educational choice through information and by affecting the available resources, I add variables for the number of siblings, number of siblings in education, and number of siblings who have completed an education higher than high school. Finally, I include a gender dummy, a variable for age, and a dummy for whether the individual is of non-western descent. As the variation in unemployment is obtained between municipalities across years, I cluster the standard errors by this level.

4.3 Long-run effects

I obtain the long-run enrollment and completion effects of local unemployment by estimating equation (3.3), where the dependent variable equals one if the individual has been enrolled in an education *within* ten years after high school. Ten years may not be a sufficiently long time-horizon for classifying the effect as long-run. To find the number of years needed, I analyze the school continuation and completion behavior for each of the first ten years after high school. If the continuation effect is stabilized within ten years, this time horizon is likely sufficiently long.

For each year j after high school, my dependent variable is a dummy that takes the value of one if the individual has been enrolled *within* j years. If the effect of local unemployment on enrollment is smaller in year $j+1$ than in year j, some individuals must have been induced to enroll within j years instead of $j+1$ years. As the results will show, the coefficient is constant after five years, implying that a ten-year horizon is more than sufficient for assessing the long-run impact of local unemployment. The full model consists of eleven equations, representing the immediate effect combined with each of the first ten years after high school. These equations are estimated by OLS, but as enrollment within $j+1$ years is not independent of enrollment within j years, the standard errors are corrected to account for this interdependence.

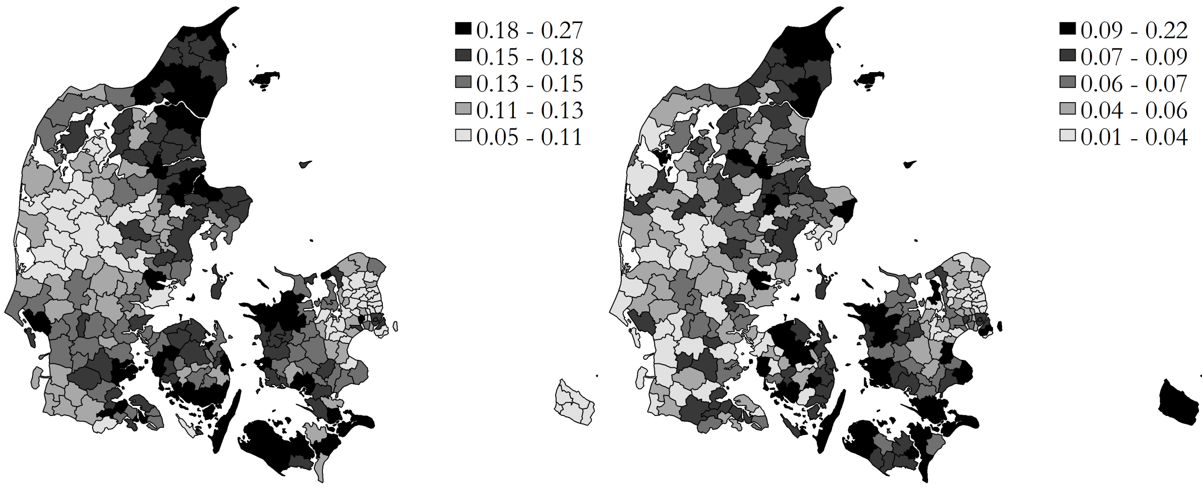
4.4 The identifying assumptions

Underlying my empirical strategy are two identifying assumptions, both of which this section discusses in detail. I also describe the robustness tests I carry out to assess these assumptions.

I: Unemployment is contemporaneous ly exogenous

I assume that there are no time-varying unobserved municipality characteristics correlated with either the unemployment rate or the individuals' enrollment decision.

If municipalities, with more attractive labor market conditions attract high school students who are determined to enter the labor market after high school, that effect would upward bias my estimates. To rule out this effect, I measure the home municipality for each student in the year before the student enters high school.



(a) Unemployment in 1984

(b) Unemployment in 1999

Figure 4.1: Local unemployment in 1984 and 1999 for individuals with at most a high school degree. The source is administrative data from Statistics Denmark.

An unobserved time-varying municipality characteristic could also downward bias my estimates. Consider a municipality that manages to attract investments in a year j. These investments cause an inflow of skilled workers in the same year. The children of these skilled workers have unobserved characteristics that favor continued schooling (because their parents are highly educated). Consequently, investments push down unemployment and increase the percentage of high school graduates that continue in school in year j. Thus the mobility of skilled workers causes a negative relationship between continued schooling and unemployment, thereby downward biasing my estimates.

To investigate this possibility, I estimate a version of equation (3.3) where I include region times year fixed effects. In that model, municipality time-varying unobserved characteristics can violate my identifying assumptions only if they do not affect neighboring municipalities. Given the small size of municipalities and the high degree of unemployment clustering in Figure 4.1, such a scenario seems highly unlikely. The maps show that high unemployment areas are clustered in the outer regions, e.g., in northern Denmark.

To further examine this issue, I estimate the baseline model in a specification with municipality specific linear trends, and a model with municipality specific quadratic trends. The specifications with municipality specific trends gives lower, but still significant, point estimate. Also a placebo regression using high school GPA a the dependent variable, shows that local unemployment is uncorrelated with high school performance, once I control for municipality fixed effects.

II: Unemployment is strictly exogenous

I assume that there are no feedback effects from enrollment in year t to unemployment in year $t+1$. For example, imagine that a shock (e.g., a career guidance counselor visiting the high school) hits a high school cohort in a municipality in year t, causing many students to continue education.

The increased demand for education will reduce the competition for unskilled jobs, presumably causing a lower unemployment rate in year $t+1$. In year $t+1$ we therefore observe a below-average percentage of students continuing schooling, and a below-average unemployment rate. Consequently, the feedback effect would upward bias my estimates.

To examine this potential problem, I estimate a model where unemployment is measured earlier and therefore is closer to previous year's enrollment behavior. Doing so should trigger the feedback effect and increase the point estimate. Likewise, I estimate a model with a definition of unemployment that covers a larger part of the population. Doing so should reduce the potential feedback effect, thereby lowering the point estimates. In both cases, the results are contrary to expectations. In sum, I find no evidence of a feedback effect. Given that I control for labor force size in the regression, this finding is not surprising. Moreover, a cohort of graduates on average constitutes only about two percent of the unskilled labor market.

5. Data Description

The applied data comes from longitudinal administrative registers covering all high school graduates in Denmark from 1984 to 1999. The individuals are followed for eleven years after graduation. As educational enrollment is measured with a one-year lag, I am able to follow each student's school record up to ten years after he or she left high school.

An overview of the sample selection appears in Table 5.1. About 40,000 observations are deleted because they are the individual's second high school degree. In Denmark students can complete academic high school and then obtain a commercial high school degree with one more year of schooling. This degree gives them direct access to traineeships (e.g., in sales). As their home municipality is less obvious, I have to exclude them from the main sample. I only consider individuals for whom the educational level of at least one parent is known. Parental education required for estimating the heterogeneous effects. In the robustness regressions, I show that this selection process has no impact on the conclusions in the main regressions without heterogeneity. In the long-run regressions I lose additional observations because individuals either leave the country or die.

Table 5.1: Sample selection

All Danish high school graduates 1984-1999	518,898
Only consider first high school degree	-39,763
Both parents unknown	-2,004
Both parents education unknown	- 6,153
Estimation sample:	470,978

Until 2007 Denmark was divided into 16 regions (*Amtskommuner*) and 275 municipalities

(*Primærkommune*).[2] The average population of a municipality is about 20,000. Figure 4.1 shows the spatial distribution of unemployment rates and continued schooling in 1991. As the maps show that a substantial degree of clustering, defining these clusters as local markets may be natural. However, as these clusters change slightly over time, when and how one should define an area by means of clustering is not clear.

The results are robust to using regions instead of municipalities (the point estimates are somewhat larger). Optimally, the labor market should be specified for each individual, so that no one lives at the border. This strategy, however, is impossible to combine with an empirical strategy for removing local unobserved time-invariant effects, because it would result in N different local areas.

5.1 Variables

For the baseline estimations, the dependent variable equals one if the individual is enrolled in an education in October after high school graduation in June. For the long-run effects the dependent value takes the value of one if the individual has been enrolled *within* j years. Enrollment and completion is measured annually in October. For the long-run effects on completion, the dependent variable takes the value of one if the individuals has completed *another* education within t years.

The timing of when the variables are recorded is crucial for this analysis. I record unemployment just before the individual makes the educational decision in June, to (1) ensure that the individual is *not* included in the unemployment measure and (2) to ensure that the labor market situation is captured at the time of the individual decision. As explained earlier, residence is measured in December the year before entering high school in August. Figure 5.1 illustrates the timing of measurement.

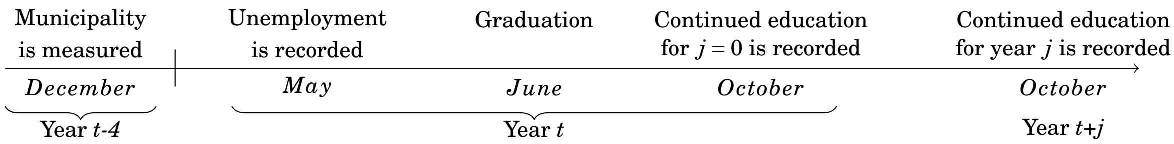


Figure 5.1: Timing of events and measurements

Local unskilled youth unemployment in May is measured by the number of benefit claimants (social assistance and unemployment benefits) aged 16 to 29 with at most a high school degree relative to the total number of individuals with these same characteristics that are not enrolled in education. Skilled unemployment is measured among all 16-to-64-year-olds with higher education (degree 5 or higher in Figure 2.1). The results are robust to the definition of unemployment. I include a dummy for whether parents are self-employed and for whether they were unemployed in May, the latter to rule out business cycle effects driven by parents. I convert parents' wealth

[2] In this study, the five municipalities on the island of Bornholm are merged into one, giving me variation from 271 municipalities over 16 years.

and income to 2010 values using the consumer price index from Statistics Denmark. A description of all variables appears in the appendix.

Table 5.2 gives yearly means for selected variables, while complete summary statistics are in the appendix. The percentage of individuals continuing directly in higher education was at its lowest at the end of the 1990s when only every fourth high school graduate continued in education. At the same time the percentage who enroll within ten years and who complete an education within ten years has been increasing. This reason is that "gap years" between high school and higher education have become increasingly popular.

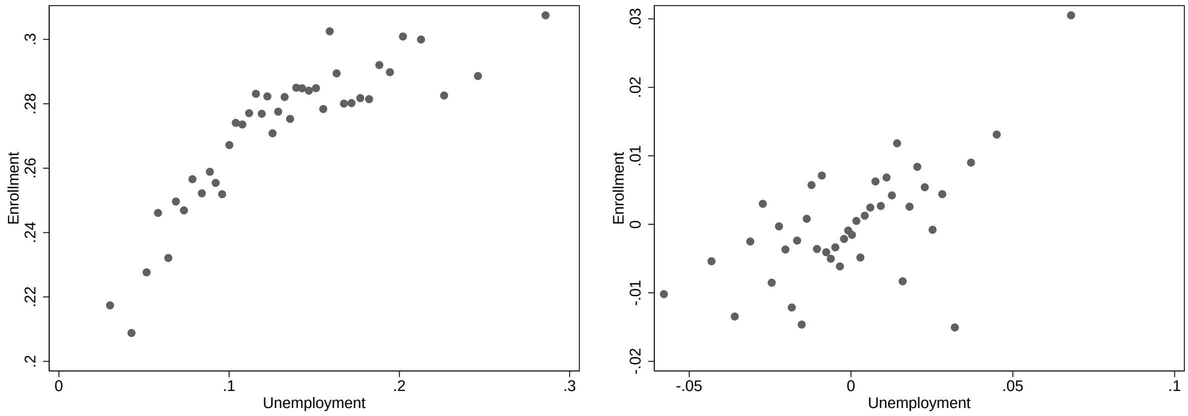
Table 5.2: Descriptive statistics

Year	*HS-leavers*	une_u	*enroll0*	*enroll10*	*complete*	*female*	*origin*
1984	30,110	14.8	31.4	85.3	72.2	58.1	0.6
1985	29,596	14.0	33.6	89.4	75.6	57.8	0.5
1986	28,748	11.9	30.2	87.6	74.5	59.2	0.5
1987	26,683	14.5	28.7	89.5	76.8	57.9	0.5
1988	26,000	16.7	27.3	90.1	77.1	57.8	0.5
1989	27,267	17.0	27.8	90.9	78.8	58.2	0.5
1990	28,599	13.6	27.9	91.9	78.9	57.7	0.6
1991	31,110	13.9	29.2	92.6	79.7	58.0	0.5
1992	30,870	17.8	26.7	93.1	80.9	57.8	0.5
1993	30,502	19.2	25.7	93.0	81.7	57.8	0.6
1994	31,035	19.1	23.7	92.5	81.5	57.4	0.7
1995	31,652	14.5	22.9	94.1	83.0	58.2	0.6
1996	29,974	10.1	22.8	95.1	85.1	58.3	0.6
1997	29,756	9.7	22.5	95.5	86.2	57.1	0.6
1998	30,426	8.0	23.0	95.4	86.4	57.5	0.7
1999	28,650	7.1	22.3	95.5	86.2	58.3	0.7

Notes: *HS-leavers*: Number of high school graduates. une_u: May unskilled youth unemployment rate. *enroll0*: share of HS-leavers immediately enrolling in an education. *enroll10*: share of HS-leavers enrolling in an education within ten years. *complete*: share of HS-leavers completing an education within ten years. *female*: share of female HS-leavers. *origin*: share of HS-leavers with a non-western origin.

6. Results

A graphic evaluation of the relationship between continued education and unskilled youth unemployment is useful for understanding the following estimation results. Figure 6.1 shows scatter plots of local unskilled unemployment and the percentage of a high school cohort that continues in education. The 16 years of data for 271 municipalities gives variation over 4,336 observations. To improve the readability of the graphs, I have constructed 40 equally sized bins. The first bin contains the observations with the 2.5 percent lowest unemployment rate, the second the next 2.5 percent, and so on. In the graphs I plot the mean unemployment against the mean enrollment for each bin. Figure 6.1a reveals a clear positive relationship between the raw unemployment and enrollment rates. A municipality-year combination with a high unemployment rate is associated with a high enrollment rate. In Figure 6.1b, in which municipality and year effects have been removed, the relationship is positive but less clear. The slope of a fitted linear line in Figure



(a) Actual data

(b) Cleaned for municipality & year fixed effects

Figure 6.1: The residuals from regressing unemployment and enrollment on municipality and year dummies. The plots show the means of 40 equally sized bins.

6.1b corresponds to the coefficient on unemployment in the OLS regression of the simple linear probability model.

The regression results appear in Table 6.1. The coefficients in column (1) are results from estimating a simple version of equation (3.3), which includes only unskilled unemployment, municipality fixed effects, and year fixed effects. The point estimate on unskilled youth unemployment, corresponding to the slope in Figure 6.1b, is 0.13. A one percentage point increase in local unskilled youth unemployment increases the probability of continued schooling by 0.13 percentage points. The only difference between model (1) and model (2) is that skilled unemployment is added as an independent variable in the latter. As expected, an omission of skilled unemployment downward biases the coefficient on unskilled youth unemployment. The coefficient increases by 30 percent to 0.17. Although skilled unemployment has the expected negative sign, it is only significant at a ten percent level. Given that only few existing studies have controlled for both unemployment rates, the conclusion that omitting skilled unemployment biases the estimate on unskilled unemployment is an important finding.

Column (3) presents the results from estimating to equation (3.3), in which all control variables are included. A one percentage point increase in local unskilled youth unemployment increases the probability of continued schooling by 0.17 percentage points. The result that the point estimate on unemployment is almost unchanged when the full set of controls is included indicates only a very weak correlation between unemployment rates and other observed characteristics. If it had been very correlated with observed variables, I would expect it to be correlated to unobserved variables as well. Students with more siblings are less likely to continue and complete more schooling, while students who have siblings enrolled in higher education are more likely to enroll and complete additional schooling.

Table 6.1: Estimation results - Linear Probability Model

	- - Immediate enrollment - -			Enrollment $\leq$ 10years	Completion $\leq$ 10years
	(1)	(2)	(3)	(4)	(5)
Unskilled unemp. rate	0.13***	0.17***	0.17***	0.07***	0.07**
	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)
Skilled unemp. rate		-0.26*	-0.27**	-0.00	-0.12
		(0.14)	(0.14)	(0.07)	(0.10)
Age			0.02***	-0.02***	-0.03***
			(0.00)	(0.00)	(0.00)
Female			-0.06***	0.01***	0.05***
			(0.00)	(0.00)	(0.00)
Non-western			0.03***	-0.02***	-0.06***
			(0.01)	(0.01)	(0.01)
Father self-employed			0.00	0.01***	0.03***
			(0.00)	(0.00)	(0.00)
Mother self-employed			0.02***	-0.01***	-0.00
			(0.00)	(0.00)	(0.00)
Father univ. degree			-0.03***	0.03***	0.00
			(0.00)	(0.00)	(0.00)
Mother univ. degree			-0.00	0.02***	0.00
			(0.00)	(0.00)	(0.00)
Parental wealth			0.00*	0.00**	0.00*
			(0.00)	(0.00)	(0.00)
Parental income			0.00***	0.00***	0.00***
			(0.00)	(0.00)	(0.00)
Parents unknown			-0.03***	-0.00	-0.05***
			(0.01)	(0.00)	(0.01)
Father unemployed			0.00	-0.00	-0.02***
			(0.00)	(0.00)	(0.00)
Mother unemployed			0.01***	-0.01***	-0.03***
			(0.00)	(0.00)	(0.00)
Number of siblings			-0.01***	-0.00***	-0.02***
			(0.00)	(0.00)	(0.00)
Sibling w. education			-0.02***	-0.01***	0.00**
			(0.00)	(0.00)	(0.00)
Cohort size			-0.00	-0.00***	0.00
			(0.00)	(0.00)	(0.00)
Labor market size			-0.00	-0.00*	-0.00***
			(0.00)	(0.00)	(0.00)
Observations	470,978	470,978	470,978	454,831	443,181
Year fixed effects	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓

Notes: Columns (1) to (3) show results from regressing an indicator for immediate continued schooling after high school on the unemployment variables and the covariates listed. Column (4) shows the results from regressing an indicator for continued schooling within ten years after high school on the unemployment variables and the covariates listed. Column (5) shows the results from regressing an indicator for completed schooling within ten years after high school on the unemployment variables and the covariates listed. Municipality times year clustered standard errors in parenthesis. *p<0.1, **p<0.05, and ***p<0.01.

While older students are more likely to enroll immediately, they are less likely to have been enrolled within ten years. In other words, they are less likely to take a gap year between educations. The same is the case for students of non-western origin. Female students however, show the opposite pattern. While they take more gap years, they continue education at a higher rate. That parental education is correlated with a lower probability of continued schooling immediately after high school may appear surprising. However, this finding is also in line with the explained tendency to take more gap years, because in the long-run regressions parental education has the expected positive sign. Parental income increases the children's probability of continued and completed schooling for the children but at a decreasing rate. Although the probability of immediate continued schooling is increased if the mother is affected by the unemployment in May, the long-run effect is negative.

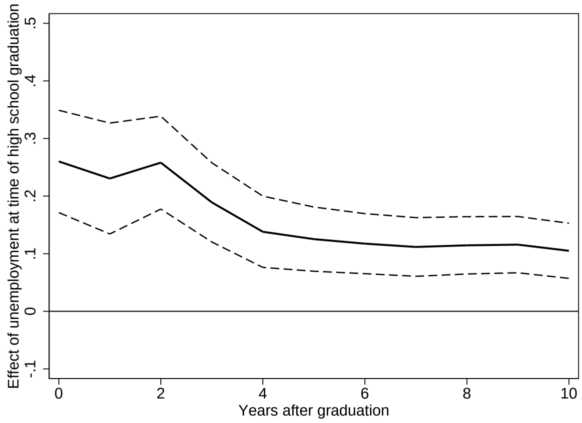
The simple long-run results appear in models (4) and (5) in Table 6.1. Model (4) shows that a one percentage point increase in local unskilled youth unemployment, at the time of high school graduation, increases the probability of continued education within ten years by 0.07 percentage points. The long-run completion effect in model (5) is almost identical to the long-run enrollment effect, indicating that very few students enroll in education in bad times for those purposes. The long-run effects are significantly lower than the short-run effect, and the effects may die out after more than ten years. To assess this possibility, I analyze that year-to-year effect in the next subsection.

6.1 Long-run dynamics

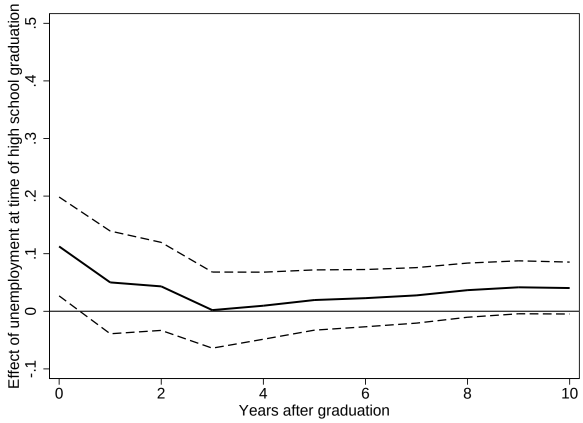
As discussed in section II, research has failed to identify whether students delay or skip education when labor market conditions improve. The regression results in Table 6.1 show an enrollment and completion effect after ten years. However, even though an increase in unskilled unemployment increases the probability of continued education within ten years, those who are affected by the unemployment could have enrolled anyway, at a later point in time. If the effect of unskilled unemployment is monotonically decreasing, the ten-year estimate will not be the long-run effect. But if the coefficient is stabilized on a constant level within the ten years, it appears safe to say that ten years are sufficient.

Figure 6.2 shows the timing effect of local unskilled youth unemployment on enrollment. The vertical axes show the coefficient on unskilled youth unemployment, α_1, estimate for the 11 equations, corresponding to the 11 years. The first Figure, 6.2a, shows the long-run enrollment effect for men, and the second Figure, 6.3b, shows the effects for women. After about four years the coefficient on α_1 is constant in both cases. For men, about half of the short-run effect is a timing effect, because the majority would have continued schooling within about four years. But after four years a significant effect remains constant. For women, in contrast, I only find a significant short-run effect for the first year, indicating that lower unemployment causes women to postpone continued schooling, but it does not keep them away from continued schooling. That the coefficient is constant between the fifth and tenth year indicates that ten years is than sufficient for analyzing the long-run enrolllment effect.

One question remains: Do the students also complete the education in which they are enrolled? As the students who react to business cycle fluctuations must be close to the margin, they might decide to drop out after labor market conditions improve. The two graphs in Figure 6.3



(a) Continued education, only men.



(b) Continued education, only women.

Figure 6.2: The marginal effect of a one-percentage point increase in local unskilled unemployment - at the time of high school graduation - on the probability of continued education within zero to ten years after leaving high-school. The dashed lines mark the 95% confidence intervals, using municipality clustered standard-errors. Results are obtained by estimating 11 separate OLS regressions where the dependent variable takes value of one if the individual continues schooling within 0, 1, ...10 years, respectively. The standard errors are clustered on the municipality times year level and corrected for the interdependence between the 11 equations. Heterogeneity is evaluated by using interaction terms.

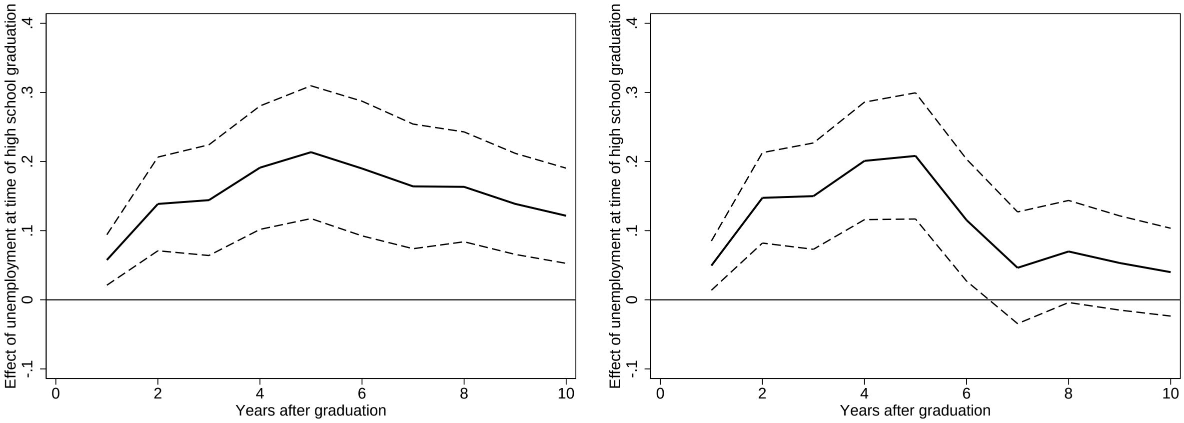
show the long-run completion effect. First, students enroll in relatively short educational programs, as the effect is already significant after one to two years. The hump after four to five years is in line with the timing effect seen in the enrollment graphs. After four years, students who would otherwise have postponed education a few years and completed it after six to seven years complete their education. While this effect after four years is similar across gender, it declines rapidly for women, while for men it stays above 0.10 percentage points. Although there are signs of stabilization for the long-run completion effects after seven years, it is by far not as clear as the enrollment effect. In line with the findings for continued schoolings, a long-run completion effect is only found for boys, while local unemployment only affect girls timing of continued education.

Local labor market conditions affect the educational attainment in both the short- and the long-run. The short-run effect causes students to enroll and complete education earlier. The long-run effect causes students who would otherwise never have enrolled to enroll and complete schooling.

6.2 Heterogeneity

The preceding graphs revealed that, in line with existing evidence, men appear to react more to local labor market conditions than women.[3] An advantage of the Danish administrative data is that it also allows me to consider heterogeneity with respect to parental background, an aspect not yet analyzed in the literature. These effects appear in Table 6.2. All models are with the full

[3]Regression results for subsamples of men and women are shown in the Appendix Table A.3.



(a) Completed education, only men.

(b) Completed education, only women.

Figure 6.3: The marginal effect of a one-percentage point increase in local unskilled unemployment - at the time of high school graduation - on the probability of completed education within zero to ten years after leaving high-school. The dashed lines mark the 95% confidence intervals, using municipality clustered standard-errors. Results are obtained by estimating 11 separate OLS regressions where the dependent variable takes value of one if the individual completes additional schooling within 0, 1, ...10 years, respectively. The standard errors are clustered on the municipality times year level and corrected for the interdependence between the 11 equations. Heterogeneity is evaluated by using interaction terms.

set of controls. The first two models are the short-run enrollment models; the third and fourth, the long-run enrollment; and the fifth and sixth, the long-run completion models.

Heterogeneity is evaluated by dividing the sample into two subsamples. Columns (1),(3), and (5) show regression results for the subsample of children whose parents completed at most high school or vocational training. Columns (2), (4), and (6) show results using the subsample of children where at least one parent completed an educational level higher than high school. Only children of low educated parents react to local labor market conditions. There is no short, nor a long-run effect on children of highly educated parents.

Table A.4 in the Appendix shows some additional results from regressions on specific subsamples. The effects are considerably larger when leaving the four largest cities out of the sample (Aalborg, Aarhus, Copenhagen, and Odense). Also, effects are considerably stronger for high school students from vocational high schools (business or technical degrees), but there is also a significant effect for graduates from academic schools. It is empirically difficult to separate out these effects. Highly educated parents are more likely to live in cities, and their children are probably also more likely to attend academic high schools than vocational high schools. More research is required to identify what drives this heterogeneity.

Table 6.2: Estimation results - Linear Probability Model - Heterogeneity by parental background

	Immediate enrollment		Enrollment $\leq$ 10years		Completion $\leq$ 10years	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Unskilled unemp. rate	0.27^{***}	0.04	0.09^{***}	0.04	0.11^{**}	0.02
	(0.05)	(0.05)	(0.03)	(0.03)	(0.04)	(0.05)
Observations	268,538	202,440	259,788	195,043	254,342	188,839
Year fixed effects	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓

Notes: Columns (1) and (2) show results from regressing an indicator for immediate continued schooling after high school on the unemployment variables and all covariates. Columns (3) and (4) show results from regressing an indicator for continued schooling within ten years after high school on the unemployment variables and all covariates listed. Municipality times year clustered standard errors in parenthesis. Columns (5) and (6) show results from regressing an indicator for completed schooling within ten years after high school on the unemployment variables and all covariates listed. Low is for the subsample of children where the parents have at most a high school degree or completed vocational training. High is for the subsample of children where at least one parent has completed more than high school or vocational training. Municipality times year clustered standard errors in parenthesis. *p<0.1, **p<0.05, and ***p<0.01.

6.3 Robustness

A number of robustness regressions appear in Table 6.3. All models are estimated with the full set of controls. The result for the first model in Table 6.3, with municipality specific linear trends, gives a considerably lower point estimate on unskilled youth unemployment, but the coefficient remains significantly different from zero. Model (2) includes municipality specific quadratic trends which gives a point estimate on unemployment slightly larger than using linear trends, and also significantly different from zero. Model (3) includes regional specific year fixed effects, a specification that also gives a smaller point estimate than in the baseline regression, but again the effect remains significant.

Model (4) is a specification corresponding the baseline model but where the home municipality is measured two years before entering high school. Changing the timing for measuring the home municipality does not alter the conclusions. Recording unemployment in March instead of May reduces the point estimate, as model (5) shows. Recall that by measuring the unemployment closer to the previous year, I trigger the feedback effect. Therefore, if the feedback effect is apparent, I would expect an increase in the coefficient. But this is not the outcome. Using a broader definition of unemployment should minimize the feedback effect, and therefore lower the point estimate. The reason is that the influence of the previous year's students on the entire labor force is smaller than their influence on the labor market conditions among young workers. Model (5) includes the rate of unemployment among all unskilled workers, independent of age. The point estimate is slightly larger than in the baseline regressions. These two results indicate no feedback effect.

Finally, the model (6) shows that the results of running a placebo regression where the de-

Table 6.3: Estimation results - Linear Probability Model - robustness

	- - Immediate enrollment - -					GPA>8
	Linear	Quadratic	County	Lagged	March	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)
Unskilled unemp. rate	0.10^{**}	0.11^{**}	0.13^{***}	0.14^{***}	0.11^{***}	-0.01
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
Skilled unemp. rate	-0.26^{*}	-0.19	-0.22	-0.21	0.02	-0.15
	(0.15)	(0.16)	(0.14)	(0.15)	(0.06)	(0.14)
Observations	470,978	470,978	470,978	414,171	470,978	470,978
Year fixed effects	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓
County × year fixed effects			✓			
Municipality linear trends	✓	✓				
Municipality quad. trends		✓				

Notes: Columns (1) to (5) show results from regressing an indicator for immediate continued schooling after high school on the unemployment variables and all covariates. Column (1) includes municipality specific linear trends. Column (2) includes municipality specific quadratic trends. Column (4) includes county times year fixed effects. Column (4) uses the unemployment rate in the municipality the individual lived in three years prior to high school enrollment. Column (5) uses the March instead of the May unemployment. Column (6) is a placebo regression using an indicator for an above median high school GPA as the dependent variable. Municipality times year clustered standard errors in parenthesis. *p<0.1, **p<0.05, and ***p<0.01.

pendent variable takes the value of one if the individual obtains a high school GPA above the median. There is no sign that local unemployment is related to high school GPA after controlling for municipality and year fixed effects.[4]

Table A.4 in the Appendix shows that using general unskilled unemployment, column (5), instead of unskilled youth unemployment makes the point estimate larger, while using the raw sample without any sample selection, column (6), gives a point-estimate almost identical to the main results.

7. Conclusion

This paper provides four contributions to the literature. First, I show that the cyclicality of school enrollment is both a short- and long-run phenomenon. The short-run effect causes students who in any case would enroll within five years to continue in education immediately. They therefore also complete their additional education earlier. The long-run effect causes students who would otherwise never continue in education to enroll and complete an education.

Second, I show that effects are heterogeneous with respect to parental background. My results show that children of less educated parents react more to business cycles in both the short and the long run. This result indicates that intergenerational education mobility is lower in economic good times.

Third, I show that including both skilled and unskilled unemployment is important for obtain-

[4]Results using the raw GPA as the dependent variable gives the same conclusion. Note that high school GPA is only available for academic high schools.

ing unbiased estimates. Fourth, I apply a more conservative estimation strategy than in existing studies. I include fixed effects on a very detailed level and show that models are robust to a number of alterations.

The analysis was carried out with Danish administrative data on all high school graduates from 1984 to 1999. Each cohort was followed for ten years, thereby allowing me to assess the effects of local unemployment on continued and completed schooling in each of the first ten years following high school. I chose to evaluate high school graduates because they all finish at the same time, and they all apply for continued education at the same time. I can therefore measure their local unemployment rate just before they graduate. Results are robust to adding municipality specific trends, and a placebo regression shows that local unemployment is unrelated to students' high school performance, controlling for municipality fixed effects.

Local business cycle fluctuations affect the timing of continued schooling after high school, as well as the completion rate of continued schooling after high school graduation. Policy makers can therefore increase enrollment and completion rates by making youth employment less attractive, for example, through increased taxation or mandatory savings.

A limitation of this study is that the educational type is ignored. I show that local labor market conditions induce students to continue and complete another education, but I have not identified what type of education they continue in. Future research could identify the educational type and whether there is substitution between types. It is not unlikely that the cyclicality of school enrollment differs by educational type. Another natural extension to this study is to impose more structure on the estimated model. Such a structural approach would identify parameters of the individual's preferences, thereby allowing for policy simulations of counter factual scenarios.

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Appendices

Table A.1: Variable definitions

Unskilled une	Average unemployment rate in May for individuals with at most a high-school degree. The labor force is defined as all individuals aged between 16 and 29, who do not attend education. The unemployment is measured at the municipality level.	*I*
Skilled une	Average unemployment rate in May for individuals with at least a university degree. The labor force is defined as all individuals aged between 16 and 64, who do not attend education. The unemployment is measured at the municipality level.	*I*
Unskilled uneall ages	Average unemployment rate in May for individuals with at most a high-school degree. The labor force is defined as all individuals aged between 16 and 64, who do not attend education. The unemployment is measured at the municipality level.	*I*
Enroll$j$	Equals one if the individual has been enrolled in any education in October year j after leaving high-school.	*D*
Com$j$	Equals one if the individual has completed another education within ten years after leaving high-school (including vocational training).	*D*
Father/mother uni	Equals one if the father/mother has a university degree.	*C*
Female	Equals one if the individual is a women.	*C*
Non-western	Equals one if the individual has a non-western origin (Western: EU, Andorra, Iceland, Liechtenstein, Monaco, Norway, San Marino, Switzerland, Vatican City State, Canada, United States, Australia, New Zealand).	*C*
Cohort size	Number of high school graduates in the municipality.	*C*
Labor market size	Size of the municipality labor force (16-64 year olds, at most high school degree).	*C*
Age	The individuals' age in the year they leaves high school.	*C*
Siblings	Number of siblings.	*C*
Siblings w edu.	Equals one if a sibling has completed higher education.	*C*
Siblings in edu.	Equals one if a sibling is enrolled in higher education.	*C*
Father/mother unemp.	Equals one if the father/mother was unemployed in May.	*C*
Parental wealth	Parents net-wealth, measured in 100,000 DKK (2010 level). Also included with an additional quadratic term.	*C*
Parental income	Parents net-income, measured in 100,000 DKK (2010 level). Also included with an additional quadratic term.	*C*
Father/mother self-employment	Equals one if the father/mother are self-employed.	*C*

I: Variable of interest, D: Dependent variable, C: Control.

Table A.2: Summary statistics

	Mean	SD	Min	Max
Age	18.68	1.24	15.00	46.00
Female	0.58	0.49	0.00	1.00
Non-western	0.01	0.07	0.00	1.00
Father self-employed	0.15	0.36	0.00	1.00
Mother self-employed	0.08	0.28	0.00	1.00
Father univ. degree	0.10	0.31	0.00	1.00
Mother univ. degree	0.03	0.18	0.00	1.00
Parental wealth	6.27	69.46		
Parental income	6.12	5.31		
Parents unknown	0.01	0.12	0.00	1.00
Father unemployed	0.03	0.16	0.00	1.00
Mother unemployed	0.05	0.19	0.00	1.00
Number of siblings	1.54	0.99	0.00	19.00
Sibling w. education	0.78	0.41	0.00	1.00
Cohort size	343.53	409.35	3.00	1715.00
Labor market size	15839.11	25761.08	487.00	130681.00
Unskilled unemp. rate	0.13	0.05	0.01	0.38
Immidiate continued schooling	0.27	0.44	0.00	1.00
Continued schooling within 10 years	0.92	0.27	0.00	1.00
Completed schooling within 10 years	0.80	0.40	0.00	1.00
Observations	443,181			

Table A.3: Estimation results - Linear Probability Model - Heterogeneity by gender

	Immediate enrollment		Enrollment ≤ 10years		Completion ≤ 10years	
	Boys	Girls	Boys	Girls	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
Unskilled unemp. rate	0.16^{***}	0.18^{***}	0.08^{***}	0.05	0.07^{*}	0.08
	(0.05)	(0.06)	(0.03)	(0.03)	(0.04)	(0.05)
Observations	272,870	198,108	263,739	191,092	258,211	184,970
Year fixed effects	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓

Notes: Columns (1) and (2) show results from regressing an indicator for immediate continued schooling after high school on the unemployment variables and all covariates. Columns (3) and (4) show results from regressing an indicator for continued schooling within ten years after high school on the unemployment variables and all covariates listed. Municipality times year clustered standard errors in parenthesis. Columns (5) and (6) show results from regressing an indicator for completed schooling within ten years after high school on the unemployment variables and all covariates listed. Municipality times year clustered standard errors in parenthesis. *p<0.1, **p<0.05, and ***p<0.01.

Table A.4: Estimation results - Linear Probability Model - Additional regressions

	- - Immediate enrollment - -					
	Cities	No cities	Academic	Vocational	Unemp	Select.
	(1)	(2)	(3)	(4)	(5)	(6)
Unskilled unemp. rate	0.06	0.25***	0.09**	0.37***	0.29***	0.18***
	(0.06)	(0.06)	(0.04)	(0.09)	(0.09)	(0.04)
Observations	219,657	251,321	349,745	121,233	470,978	510,741
Year fixed effects	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Covariates	✓	✓	✓	✓	✓	✓

Notes: All columns show results from regressing an indicator for immediate continued schooling after high school on the unemployment variables and all covariates. Column (1) shows the results from using a sample that only includes Aalborg, Aarhus, Odense, and greater Copenhagen area. Column (1) shows results when Aalborg, Aarhus, Odense, and greater Copenhagen area are excluded from the sample. Column (3) shows results using only students on academic high schools. Column (4) shows results using only students on vocational high schools. Column (5) shows results using the general unskilled unemployment rate (instead of the youth unemployment for unskilled workers). Column (6) presents results using the raw sample without sample selection. Municipality times year clustered standard errors in parenthesis. *p<0.1, **p<0.05, and ***p<0.01.