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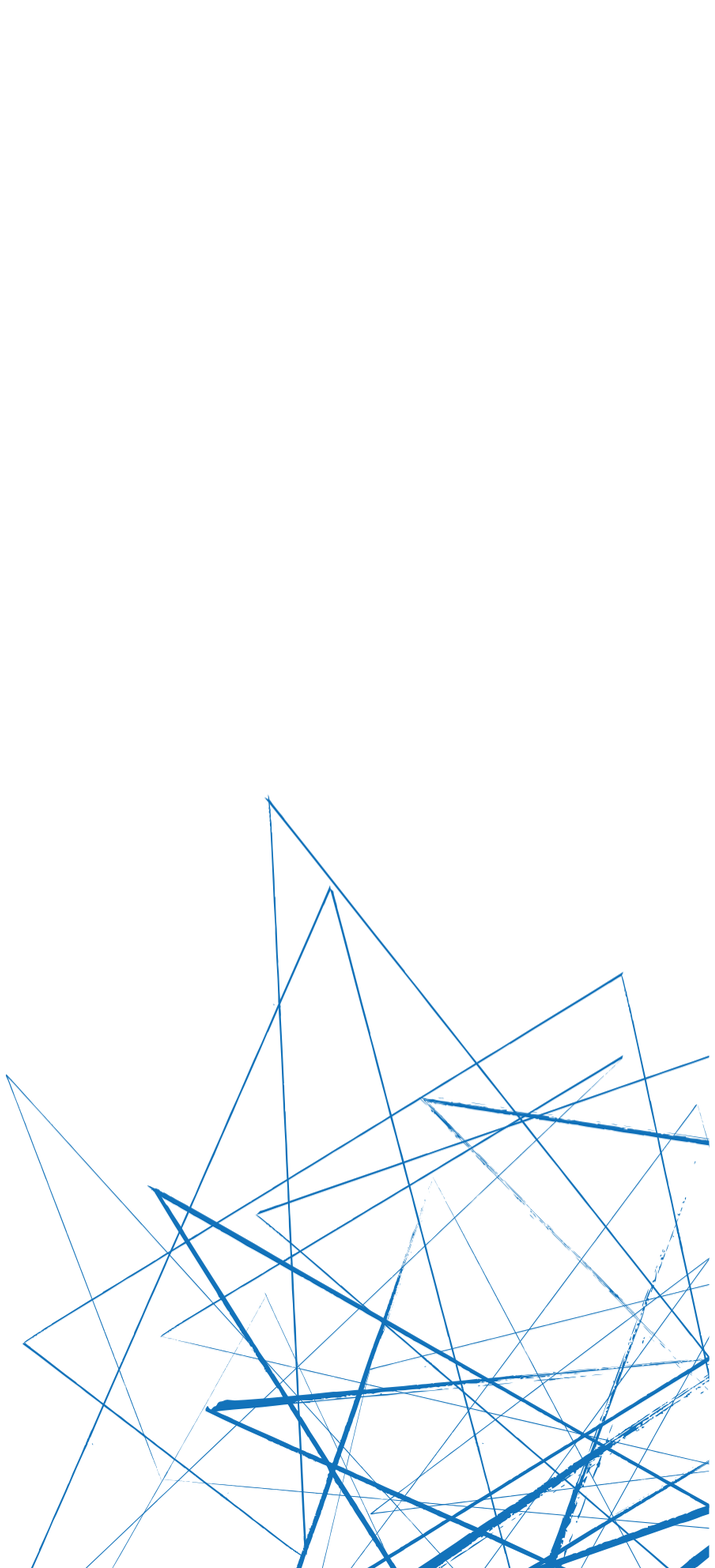
ESSAYS IN EDUCATION, CRIME, AND JOB DISPLACEMENT



PhD Series 18-2016

Patrick Bennett

ESSAYS IN EDUCATION, CRIME, AND JOB DISPLACEMENT

The PhD School of Economics and Management | **PhD Series 18.2016**

CBS COPENHAGEN BUSINESS SCHOOL
HANDELSHØJSKOLEN

Essays in Education, Crime, and Job Displacement

Patrick Bennett

Supervisors: Birthe Larsen and Lisbeth la Cour

PhD School in Economics and Management
Copenhagen Business School

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

Forward

This Ph.D. thesis has been written over the course of my doctoral studies in the Department of Economics at the Copenhagen Business School. I am very grateful for the financial support provided by the Copenhagen Business School throughout my Ph.D. First and foremost, I wish to thank my two supervisors, Birthe Larsen and Lisbeth la Cour, for their numerous comments and suggestions, for our co-authorship, and more generally for always being so supportive and always being available. I wish to thank Amine Ouazad not only for our co-authorship but for providing so much guidance and insight throughout our relationship. During 2014, I was fortunate enough to visit the Swedish Institute for Social Research (SOFI), and I am grateful to Matthew Lindquist not only for the invitation but for all his support and to everyone else who provided feedback and insight during my stay at SOFI. I also wish to acknowledge the financial support from Otto Mønsteds Fond and Christian og Ottilia Brorsons Rejselegat which made my visit to SOFI possible. I am very grateful to Marcus Asplund for his detailed comments and work on the first chapter of my thesis. I also wish to thank all of those who have taught me throughout my academic career both in the UK and Denmark.

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Finally, I wish to thank mother for (quite correctly) teaching me to always print, my father for paving the way for Bennett graduates in Economics, and both of them for all that they have done. Thank you to my grandparents for always looking out for me and their constant support

throughout my academic career. A special thanks goes to all of my high school English teachers who, although they may never even see this, certainly deserve praise and appreciation for their tireless and underpaid educational efforts. Special thanks also go to Fabio Aricò for his words of encouragement, support, and assistance over the years, without which I wouldn't be anywhere close to where I am today. Lastly, but certainly not least, I wish to thank my wife Anna for her endless support and for dealing with the countless late nights, weekends, and trips abroad required throughout my Ph.D. And to anyone who I mistakingly left out, please know I appreciate your efforts and please accept my sincere apologies for the omission.

Abstract

With a limited budget and resources, governments must decide how to allocate funds across a variety of factors which benefit society such as education, crime deterrence, and public safety. Each increase in spending on one area comes with the knowledge that this money cannot be spent on social problems in another area. As such, externalities and unexpected spillover effects impact the costs and benefits of public spending to society and may have large and meaningful implications on how to most effectively allocate resources across a multitude of outcomes. For example, an increase in education corresponds to an increase in the opportunity cost of engaging in criminal activity, decreasing the probability an individual commits crime. Likewise, loss of employment decreases the opportunity cost of engaging in criminal activity, increasing the probability an individual commits crime. Discrimination towards immigrants can impact their employment prospects which, in turn, impacts their decision to further pursue education. Identifying how these individual level factors have an impact on society is key to informing and designing effective public policy.

This Ph.D. thesis, entitled "Essays in Education, Crime, and Job Displacement", analyzes the determinants and social implications of these three factors. While independent, each essay within this thesis examines the impact of factors such as education, in terms of reduced crime, job loss, in terms of increased crime, and discrimination, in terms of its impact on the educational attainment of immigrants, on society.

The first essay of my thesis, "The Heterogeneous Effects of Education on Crime: Evidence from Danish Administrative Twin Data" examines whether education is equally effective in reducing crime for everyone. Education reduces crime, but this paper is the first to directly examine heterogeneous effects, in particular how the crime reducing capabilities of education depend on specific individuals and factors. I make use of twins contained in detailed Danish Register Data to control for characteristics, both observable and unobservable, which are common between twins. I focus on twin data as identification using twins provides many advantages—giving the freedom to explore the impact of many educational qualifications across the entire education distribution and directly estimating the effects of juvenile crime on educational attainment to examine reverse causality. I find that family factors are important, where education reduces crime for males of

low educated families, with less effects seen for individuals coming from highly educated families. Environmental factors are found to be less important—education lowers crime irrespective of the levels of crime in childhood neighborhoods. I find that the completion of high school lowers crime considerably while, contrary to expectations, the completion of vocational education is found to have no crime reducing effects. The role of juvenile crime is examined in great detail, revealing that taking into account participation in juvenile crime is important but that education reduces adult crime above and beyond what is explained through juvenile crime. My results imply that these specific "at risk" individuals obtain education which is less than socially optimal, as, in addition to the private benefits to education, the social benefits of education are large.

The second essay, entitled "Job Displacement and Crime" investigates, together with Amine Ouazad, the individual level impacts of job loss on crime. While unemployment measured at an aggregate level causes crime, the extent to which a transition into unemployment increases crime is less clear, as previous literature has focused on measures of unemployment at the state or county level. We identify the impact of sudden and unexpected job loss on crime, using detailed police, employer, and employee data. Following Jacobson, LaLonde and Sullivan (1993), we examine high tenured workers with strong labor market ties to their firms. For these individuals, job loss occurring in a mass layoff event is likely sudden and unanticipated. We find that displaced individuals are significantly more likely to be convicted in the time following job loss, but importantly, not in the time before job loss. These effects are primarily seen for property crimes and are concentrated for individuals at the lower ends of the educational distribution. These effects are long lasting, particularly for individuals with less than high school education. Individuals educated to the university level or beyond are more resilient, in terms of criminality, to job loss. We examine a possible intergenerational effect where father's job loss may impact the criminality of children, finding some evidence that sons are marginally more likely to commit crime in the short-run following father's displacement. We see sizable, significant, and long lasting effects for individuals who live alone. Our results are robust to changing our mass-layoff criteria including increasing the number of employees who must lose their job to cause a mass-layoff event and altering how we define a mass-layoff event. We argue that neither employers nor employees fully internalize the social costs of job loss, which justifies an active role of policies which incentivize unemployed to transition back into formal employment or additional taxation of employers.

The final essay of my thesis, "Negative Attitudes, Network and Education", investigates, to-

gether with Lisbeth la Cour, Birthe Larsen, and Gisela Waisman, what factors can explain the educational gap that exists between natives and immigrants. We examine, both theoretically and empirically, the importance of two specific factors: negative attitudes towards immigrants and networking amongst immigrants. Theoretically, we formulate a Becker-style taste discrimination model within a search and wage bargaining setting where the educational decision of natives and immigrants is endogenous. We show that the education an immigrant obtains depends on negative attitudes, which directly influence their employment prospects. If all immigrants are equally affected by discrimination, immigrants obtain less education than natives. If only low skilled immigrants are affected by negative attitudes, immigrants obtain more education than natives to improve their employment prospects. We find that more immigration increases the fraction of educated immigrants via networking which also improves immigrant employment prospects. Empirically, we analyze the educational decision of young immigrants, specifically whether they decide to attend high school. We find evidence that negative attitudes against immigrants increase the likelihood male immigrants attend high school, supporting the case when discrimination is against only low skilled immigrants. We find evidence supporting our theoretical findings that networking amongst immigrants, in the form of a higher fraction of own nationality immigrants for females and a higher fraction of own nationality immigrants who are employed for males, increases the likelihood young immigrants attend high school. Due to the fact that immigrant's can selectively locate, we examine how our results change taking into account unobservable factors which could drive these decisions. We show that, under reasonable assumptions regarding the role of unobservables, that while unobservables do explain a portion of our results, they are unable to completely explain the positive impact of negative attitudes on education we see for males. Our results are also robust to excluding those families who have recently relocated and are explained almost entirely by an impact on 1^{st} generation immigrants.

Resumé (Abstract - Danish)

Med et begrænset budget og ressourcer må regeringer beslutte sig for hvordan de vil fordele midler til forskellige faktorer, så som uddannelse, bekæmpelse af kriminalitet og sikkerhed. Enhver stigning i udgifter til et område betyder at disse penge ikke kan bruges på sociale udfordringer på et andet område. På den måde har eksternaliteter og uforudsete spill-over effekter indvirkning på omkostninger og gevinster fra offentlige udgifter for samfundet og kan have længerevarende og betydningsfulde implikationer for hvor effektivt ressourcerne er allokeret. For eksempel, en stigning i uddannelse svarer til en stigning i alternativomkostningerne ved at engagere sig i kriminel aktivitet, hvilket sænker sandsynligheden for at et individ bliver kriminel. På samme måde, sænker et beskæftigelsestab alternativomkostningerne ved at en person begår kriminalitet. Diskrimination mod indvandrere kan påvirke deres beskæftigelses chancer, hvilket videre kan påvirke deres beslutning om at tage videre uddannelse. En identifikation af hvordan disse individuelle faktorer kan påvirke samfundet er nøglen til at informere og designe effektiv offentlig politik.

Denne Ph.D. afhandling, 'Essays in Education, Crime, and Job Displacement' (Essays om uddannelse, kriminalitet og arbejdstab) analyserer determinanter og sociale implikationer af disse tre faktorer. Selvom de er uafhængige, undersøger hvert essay hvorledes faktorer som uddannelse, i form af reduceret kriminalitet, arbejdstab, i form af større kriminalitet, og diskrimination, i form af dens påvirkning af indvandreres uddannelsesniveau, påvirker samfundet.

Det første essay af min afhandling: 'The Heterogeneous Effects of Education and Crime: Evidence from Danish Administrative Twin Data' (Heterogene effekter af uddannelse på kriminalitet: Evidens fra Dansk register tvillingedata) undersøger hvorvidt uddannelse er lige effektivt for alle med henblik på at reducere kriminalitet. Uddannelse begrænser kriminalitet, men dette papir er det første som direkte undersøger heterogene effekter, dvs. hvorvidt uddannelse begrænser kriminalitet afhænger af specifikke individer og faktorer. Jeg benytter mig af tvillingepar i detaljeret dansk register data til at kontrollere for karakteristika, både observerbare og ikke-observerbare, som er fælles for tvillinger. Jeg fokuserer på tvillingedata da identifikation via tvillinger har mange fordele – hvilket giver friheden til at udforske implikationen af forskellige uddannelseskvalifikationer - og estimerer direkte indvirkningen af ungdomskriminalitet på uddannelsesniveau med henblik på

at undersøge om der er omvendt kausalitet. Jeg finder at familiære faktorer er vigtige, hvor uddannelse reducerer kriminalitet for mænd fra lavt-uddannede familier, hvorimod der er mindre effekt for individer fra højtuddannede familier. Samfundsmiljømæssige faktorer findes at have mindre betydning – uddannelse reducerer kriminalitet uafhængig af kriminaliteten i nabolaget, hvor personen voksede op. Jeg finder at en studentereksamen reducerer kriminalitet betydeligt, hvorimod, i modsætning til hvad vi ville forvente, at en uddannelse som faglært ikke sænker kriminalitet. Betydningen af ungdomskriminalitet undersøges detaljeret og det afsløres at ungdomskriminalitet er vigtig men at uddannelse reducerer kriminalitet mere end hvad kan forklares ved ungdomskriminalitet. Mine resultater betyder at disse individer i risikogruppen opnår et uddannelsesniveau som er under det, som ville være samfundsmæssigt optimalt, da udover de individuelle gevinster ved at uddanne sig, er der tillige nogle store samfundsmæssige gevinster.

I det andet essay: 'Job Displacement and Crime' (Arbejdstab og kriminalitet) undersøger jeg sammen med Amine Ouazad, de individuelle effekter af arbejdstab på kriminalitet. Hvor arbejdsløshed på aggregeret plan medfører kriminalitet, er det mere uklart om et job tab og dermed transition til arbejdsløshed hæver kriminalitet på individ niveau. Dette er tilfældet da tidligere litteratur har fokuseret på arbejdsløshed på lande eller regionsplan. Vi identificerer påvirkningen af et pludseligt og uventet arbejdstab på kriminalitet, ved at benytte os af detaljeret kriminalitetsregister, virksomhedsdata og individ og dermed ansættelsesdata. Idet vi følger Jacobson, Lalonde og Sullivan (1993) ser vi på individer med lang arbejdserfaring i den virksomhed de er ansat i. For disse personer er et arbejdstab som følge af en massefyring sandsynligvis pludselig og uventet. Vi finder at disse personer, som mister deres job, med større sandsynlighed bliver dømt for kriminalitet i tiden derefter, og dette selvom de ikke har begået kriminalitet i tiden før de mistede deres arbejde. Disse effekter ses primært for ejendomsforbrydelser og er koncentreret i denne lave ende af uddannelsesfordelingen. Disse effekter er langvarige, især for individer med mindre uddannelse end studentereksamen eller lignende. Personer med længere uddannelse, som pludseligt mister deres arbejde er mindre påvirkede med hensyn kriminalitet. Vi undersøger en mulig generationseffekt, hvor farens beskæftigelsestab kunne tænkes at påvirke børnenes kriminalitet, og vi finder noget evidens for at sønner begår marginalt mere kriminalitet lige efter farens beskæftigelsestab. Vi finder betydelige og langvarige effekter for individer, som bor alene. Vores resultater er robuste overfor at ændre massefyringskriteriet således at flere skal have mistet deres job og ser på en ændring af andelen ansatte, der mister deres job. Vi argumenterer for at hverken arbejdsgiveren eller de

ansatte fuldt ud internaliserer de sociale omkostninger ved beskæftigelsestab, hvilket retfærdiggør en aktiv rolle for politik med henblik på at intensivere transitionen fra arbejdsløshed tilbage i beskæftigelse og/eller en yderligere beskatning af virksomheder.

Det sidste essay: 'Negative Attitudes, Network and Education' (Negative holdninger, netværk og uddannelse) undersøger, sammen med Birthe Larsen, Lisbeth la Cour og Gisela Waisman, hvilke faktorer der kan forklare uddannelsesgabet mellem indfødte og indvandrere. Vi undersøger, både teoretisk og empirisk, vigtigheden af to specifikke faktorer: negative holdninger overfor indvandrere og indvandreres netværk. Teoretisk formulerer vi en Becker smagsdiskriminationsmodel i en søge-matching forhandlingsmodel, hvor uddannelsesbeslutningen for indfødte og indvandrere er endogen. Vi viser at det uddannelsesniveau, som indvandrere opnår afhænger af negative holdninger, da de direkte påvirker forventet beskæftigelse. Hvis alle indvandrere oplever samme diskrimination vil indvandrere opnå mindre uddannelse end indfødte. Hvis derimod kun lavt-uddannede indvandrere påvirkes af negative holdninger, vil indvandrere opnå mere uddannelse end indfødte, da de derved forøger deres beskæftigelseschancer og løn. Vi finder at mere indvandring forøger andelen af uddannede indvandrere gennem netværkseffekter, da det forøger deres beskæftigelseschancer og løn. Empirisk analyserer vi uddannelsesvalget for unge indvandrere med hensyn til om de fortsætter på gymnasiet eller lignende uddannelse. Vi finder at negative holdninger overfor indvandrere øger sandsynligheden for at mandlige indvandrere går i gymnasiet, hvilket stemmer overens med modellen, hvor diskrimination kun finder sted mod lavt-uddannede indvandrere. For kvinderne finder vi dokumentation for modellens forudsigelse at indvandreres netværk, i form af en højere andel af egen nationalitet øger sandsynligheden for at den unge indvandrer går i gymnasiet eller lignende. For de unge indvandrermænd finder vi at en højere andel ansatte af egen nationalitet øger sandsynligheden for at den unge indvandrer går i gymnasiet eller lignende. På grund af indvandreres mobilitet, undersøger vi hvordan vores resultater ændrer sig hvis vi tager hensyn til ikke-observerbare faktorer, der kunne påvirke disse beslutninger. Vi viser, at under rimelige antagelser om den rolle som ikke-observerbare faktorer spiller, kan de ikke fuldt ud forklare den positive indvirkning af negative holdninger på uddannelse, som vi ser for mænd. Vores resultater er også robuste i forhold til at udelade de familier, der er flyttet for nylig og forklares næsten udelukkende af en indvirkning på 1. generations indvandrere.

Contents

Introduction

This Ph.D. thesis is composed of three chapters and ends with a general conclusion for all three chapters. It should be noted that while all three chapters are independent research papers and can be read as such, they all address important topics within the field of Labor Economics. More specifically, all three chapters analyze the determinants and social implications of education, crime, and job displacement. The first chapter finds that education is not equally effective in lowering crime for everyone, and identifies specific individuals for whom we can expect education to have an impact on criminal activity. The second chapter finds that job displacement, sudden and unexpected job loss in a mass-layoff event, increases the probability of committing crime and that the magnitude and longevity of these effects depend on education, household factors, and post-displacement employment outcomes. The third chapter proposes two potential explanations of the educational gap that exists between immigrants and natives, negative attitudes towards immigrants and networking, finding that while networking amongst immigrants can reduce this gap, negative attitudes motivate immigrants to pursue further education and, as such, are unable to explain this gap in education.

All three of these chapters are motivated by and build upon existing literature and make unique and novel contributions within their respective fields. Over the last decade, economists have established that education has a negative and significant impact on an individual's propensity to commit crime. There are numerous studies establishing this fact (Lochner and Moretti 2004; Machin et al. 2011; Åslund et al. 2015; Hjalmarsson et al. 2015), which generally find that education has a negative and causal impact on crime. There are many reasons education is expected to lower crime, for example by increasing the opportunity cost of crime or by impacting individual factors such as instilling social values or increasing the patience of individuals. The motivation behind the first chapter is to recognize that while, overall, education reduces crime, that this may not be true for everyone and important heterogeneity in the crime reducing capabilities of education may exist below this overall effect. Similarly, the second chapter is motivated by the fact that unemployment and crime are related at an aggregate level (Raphael and Winter-Ebmer 2001; Gould et al. 2002; Öster and Agell 2007; Lin 2008; Fougère et al. 2009), but practically nothing

is known about the individual level relationship between becoming unemployed and engaging in crime. Job loss can impact crime by lowering the opportunity cost of crime but also by having a psychological impact on the individual, and there are many reasons to believe the individual level and aggregate impacts may differ, in terms of both types of crime and the longevity of the effects. Finally, the third chapter combines and expands on two strands of existing literature: one which analyzes the impacts of discrimination on employment and wages (Mailath et al. 2000; Lang et al. 2005; Charles and Guryan 2008; Waisman and Larsen 2015) but without the additional analysis of an individual's educational decision and another which analyzes the importance of networking in securing employment (Calvó-Armengol and Jackson 2004; Andersson et al. 2009; Kramarz and Skans 2014). Negative attitudes may lead immigrants to obtain lower education, if immigrants are equally affected by discrimination, or may lead immigrant's to obtain more education, if only low-skilled immigrants are affected by education, while networking will increases education.

Within all of these strands of existing literature, a common underlying point of emphasis is the identification of a causal relationship between the explanatory variable and the outcome of interest. However, the methods used to estimate causal impacts differ across these fields. To examine the crime reducing effects of education, many studies have used changes in compulsory schooling laws; the first chapter of this thesis takes a different approach—controlling for characteristics which are common between twins. The previous literature on unemployment and crime has relied on exogenous changes at the state or county levels which impact unemployment but not crime; the second chapter relies on sudden and unexpected mass-layoffs at the individual level to generate exogenous changes in unemployment. The third chapter focuses on a sample of young immigrants for whom household location is plausibly exogenous to their educational decision and, additionally, provides evidence on the role of unobservable factors in explaining the findings.

All three chapters make use of detailed administrative population data from Denmark. Danish Register Data, which is maintained and provided by Statistics Denmark, is a panel dataset compiled from various administrative sources which completely covers the entire Danish population. The detail and depth of this data is useful for assessing the heterogeneous impacts of education on crime, enables the linking of matched employer-employee data to police data to analyze the impacts of job displacement on crime, and the institutional structure of the educational system of Denmark enables the analysis of the impact of negative attitudes on a non-compulsory decision to attend further education. All three chapters would not be possible using other non-Register based data

sources where either information would not be detailed enough, data would simply not be available, or the timing of a crucial education decision would be either too early or too late.

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Chapter 1 - The Heterogeneous Effects of Education on Crime: Evidence from Danish Administrative Twin Data

The Heterogeneous Effects of Education on Crime: Evidence from Danish Administrative Twin Data

Patrick Bennett[◇]

March 2016

Abstract

Using administrative Danish Register Data to identify all twins aged 18-35 in 2000, this paper identifies heterogeneous effects of education on crime. Controlling for genetic and environmental factors, an additional year of education significantly lowers the probability of conviction for total, property, and violent crimes by 14%, 12%, and 19% for males. Estimation by parental education reveals family factors matter—education overwhelmingly lowers crime for children of low educated parents—while estimation by exposure to crime during childhood reveals environmental factors have less impact on the relationship between education and crime. Examining different educational programs reveals the completion of high school matters for crime reduction of males, while the completion of vocational training or university do not. These results are robust to correcting for the presence of dizygotic twins, directly estimating reverse causality between education and crime, and using data on incarcerations instead of convictions.

JEL classification: *I2, K42*

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1 Introduction

Education reduces crime—this fact has been established in a number of studies. This is true for both years of education as well as the completion of high school. This is also true for multiple crime types—total, property, and, in most instances, violent crimes. But is education equally effective in reducing crime for everyone? Given the vast differences in not only types of crime but also the motivating factors behind these crimes, there is good reason to believe education impacts the criminal behavior of specific types of individuals very differently.

This paper analyzes heterogeneity in the crime reducing capabilities of education using Danish twin data, and makes three important contributions to the literature. First, heterogeneous effects are examined across family factors such as parental education as well as environmental factors such as growing up in neighborhoods with high and low levels of crime. Second, it evaluates the importance of specific educational qualifications and programs for the crime reducing effects of education. Third, it expands upon Webbink et al. (2013) by providing more generalizable results which use administrative twin panel data rather than self reported survey data, analyzing both male and female twin pairs, and examining detailed crime types in addition to investigating heterogeneity along other dimensions.

Previous studies (Lochner and Moretti 2004; Machin et al. 2011; Meghir et al. 2012; Hjalmarsson et al. 2015) typically exploit changes in compulsory schooling laws to provide causal interpretations of the effects of education on crime. This paper takes a different approach—controlling for characteristics which are common between twins. As twins are genetically similar and are usually raised in the same environment as children, many unobservable factors which affect both education and crime are controlled for by comparing the outcomes of one twin to the other. Additionally, twin estimation is capable of estimating effects across the entire educational distribution of the population and not only for those at the lower end of the distribution.

Using within twin fixed effects estimation, this paper confirms the existence of significant negative effects of years of education on the probability of conviction for male twins of total and violent crimes, with marginally significant effects seen for property crimes. An additional year of education lowers the probability a male twin was convicted of any crime committed from 2001-2006 by 14%, of a property crime by 12%, and of a violent crime by 19%. Involvement in juvenile crime significantly increases an individual's probability of being convicted as an adult, but to less of an

extent than has been found previously (Webbink et al. 2013).

Having confirmed education reduces an individual's probability of conviction as an adult, the paper examines the presence of heterogeneous effects of education on crime. Family factors are found to be important—the effects of education on crime are large in magnitude for those from a low educated household and, with the exception of violent crimes, virtually non-existent for those with two highly educated parents. Environmental factors are found to be less important—education lowers crime irrespective of the levels of crime in the neighborhood twins are exposed to during childhood. Examining different educational qualifications reveals that the completion of high school is important in terms of crime reduction for males. However, contrary to expectations, the completion of vocational training geared directly towards professional employment has no effects on a male's propensity to engage in crime. When analyzing every available crime type for male twins, education significantly decreases participation in firearms and alcohol related traffic offenses, with marginally significant effects found for sexual and other crimes.

Isolating heterogeneous effects of education on crime which are causal in nature represents an empirical challenge. This relationship is complicated by the fact that education decisions are endogenous—more crime prone individuals are both less likely to pursue education and more likely to commit crime—and causality flows in both directions—participation in crime as a juvenile can directly affect the level of schooling an individual obtains. In addition to identifying causal effects of education on crime, there are many other advantages to using within twin estimation which enable the examination of heterogeneity in the effects of education on crime. Firstly, effects are identified over the entire population, not from those who comply with compulsory schooling reforms at the lower end of the educational distribution. Secondly, the analysis is not constrained to one specific educational change, giving the freedom to analyze the impact of multiple educational qualifications and time periods. Thirdly, estimation within twins enables the analysis of reverse causality between education and crime.

Despite the prominent use of twin studies within Economics,[1] there are also limitations to using twin data. Specifically, while monozygotic (MZ) twins are virtually genetically identical, differences in unobservable factors which affect both education and, in this paper, crime participation are

[1]Twin studies have long been used in Economics, and particularly in the education literature, to estimate the returns to education (Ashenfelter and Krueger 1994; Ashenfelter and Rouse 1998; Isacsson 1999), the intergenerational transmission of education (Behrman and Rosenzweig 2002; Holmlund et al. 2011; Pronzato 2012; Lundborg et al. 2014), the impact of spousal education on earnings (Huang et al. 2009), and even in the portfolio choice literature (Calvet and Sodini 2014).

determined by more than just genetic factors, and these differences in unobservables could be what drive twins to obtain different education levels (Griliches 1979; Bound and Solon 1999; Sandewall et al. 2014).

While the results presented in this paper are subject to these criticisms, particularly as data on zygosity is unavailable, multiple steps are taken to ensure that unobservable differences between twins are accounted for. Importantly, twins raised in different households during childhood are excluded from the sample, as these twins are not exposed to similar environmental factors. Correcting for the potential bias introduced by the presence of dizygotic (DZ) twins as in Conley et al. (2006) and Holmlund et al. (2008) indicates that if zygosity were observable, the estimated effects of education on crime identified would be of a similar magnitude for MZ twins and, in most instances, remain statistically significant. Examining GPA differences for a subset of twins where GPA data is available reveals no major differences in the academic achievement of twins. Additionally, results are robust to using data on incarcerations instead of convictions, excluding twins with large differences in education, and directly estimating the reverse causality between education and crime.

The next section briefly outlines the reasons why education can affect crime. Section 3 details the existing literature on education and crime. Section 4 describes Danish Register Data, while Section 5 provides summary statistics. Section 6 outlines within twin fixed effects estimation and potential threats to this methodology. The baseline results of the effects of education on crime are reported in Section 7, while Section 8 examines the heterogeneous effects of education on crime. Section 9 details the robustness of the results and Section 10 concludes and provides a brief discussion of the results.

2 Reasons Education Can Affect Crime

2.1 Effects on Employment

Education can lower criminal activity by affecting an individual's labor market prospects, predominantly through increasing wages and increasing an individual's probability of employment.[2] Firstly, education builds human capital which leads to higher wages. Increased wages increases the opportunity cost of crime (foregone wages while incarcerated), thus reducing an individual's

[2]For a theoretical model, see Lochner (2011).

propensity to engage in criminal behavior. Secondly, if employers see educational qualifications as an indicator of potential productivity, education can increase the probability that an individual will be employed. Having a legal job reduces the financial need for illegal wages through crime, also lowering an individual's propensity to engage in crime. Lochner (2011) identifies that effects on employment and wages are the most prominent reasons why education can reduce crime.

2.2 Effects on the Individual

Education can also have direct effects on the individual, shifting individual preferences away from crime. Firstly, Lochner and Moretti (2004) argue that education alters an individual's preferences, leading to increased risk aversion and patience. Risk averse individuals will engage in less criminal activity due to their desire to avoid possible incarceration, while more patient individuals are more willing to invest in the time required to obtain education for higher future wages.

Secondly, Lochner (2011) identifies two additional effects of education on individuals: peer and incapacitation effects. By attending school, students interact with other students. This interaction reinforces the crime reducing effects that education has on the individual, reducing students' future participation in criminal activity. Lochner also explains that peer effects could have a positive effect on crime, as students are all released from school simultaneously, and that their propensity for "group-based delinquency" (Lochner (2011), 9) is increased. Additionally, by attending school, students' opportunities to engage in criminal activity are reduced, as students are unable to commit crime "on the streets" while in school. Also, when students leave school, they are assigned homework and required to study, further reducing free time available for criminal activity. Limiting the criminal opportunities of youth can theoretically have a major impact on crime, as a significant amount of criminals are repeat offenders, and by limiting initial involvement in crime, potential future crimes may be reduced as well.[3]

2.3 Possibility of Positive Effects

While it seems that, theoretically, education will have a negative effect on crime, it is also possible that more education will lead to increases in crime. The motivation, knowledge, and skills required for specific crimes are very different, and no two crimes are alike. Education will be most likely to increase crimes where it leads to increased returns from criminal behavior, either by decreasing an individual's probability of being caught or by increasing the monetary payoff of these crimes.

[3] 15.5% of the US parolee population returned to incarceration in 2007 (Bonczar and Glaze 2009).

Lochner (2011) concludes that this is most probable for white collar crimes, such as embezzlement and fraud, and there is some evidence that this may be the case (Lochner 2004).

3 Existing Literature

Education and crime is a topic increasingly analyzed, with studies building upon Lochner and Moretti's seminal publication. Lochner and Moretti (2004) exploit changes in compulsory schooling laws over time and states in the US to estimate causal effects of education on crime. Combining FBI and Census data, the authors find graduating high school has a significant negative effect on an individual's probability of incarceration and arrest for total, violent, and property crimes. They also find a significant negative effect on these types of crimes for years of education obtained.

Buonanno and Leonida (2009) analyze the effect of education on crime in Italy, using panel data from 1980-1995. Lacking a suitable instrument for education, the authors control for as many factors as possible. Using fixed effects estimation, the authors conclude graduating high school has a significant negative effect on a person's probability to commit property crimes, and an insignificant negative effect on someone's probability to commit total crimes. To account for the inertia of crime, the authors also use GMM system estimation, including a lag of crime rates. When included, both the effect of high school graduation and schooling years significantly reduce property and total crimes. Due to its significance across specifications, the authors conclude accounting for lagged crime rates is quantitatively important.

Machin et al. (2011) study the effects of education on criminal activity in the UK. Following Lochner and Moretti (2004), the authors use changes in compulsory schooling laws to identify causal effects of education on crime, using both IV and also RD estimation. The authors find a significant negative effect of years of education on property and total crimes, and an insignificant negative effect on violent crimes. They also find a significant negative effect of holding an educational qualification on property, violent, and total crimes. Their results are similar to Lochner and Moretti (2004), with the exception that they find a larger crime reducing effect of education for property crimes. Machin et al. (2012) analyze the introduction of the GCSE exam system in the UK,[4] and also find significant negative effects of staying in school after 16 on property, violent, and total crimes.

[4]GCSE (General Certificate of Secondary Education) is a national wide secondary education qualification in England, Northern Ireland, and Wales.

Hjalmarsson et al. (2015) analyze the impact of education on criminal activity in Sweden, making use of Swedish Register Data. Exploiting differences in the timing of changes in compulsory schooling, the authors use difference-in-differences and IV estimation to estimate causal effects of education on crime. The authors find significant negative effects of education on property and total crimes, and insignificant negative effects on violent crimes. The authors also analyze education and crime across age groups, finding greater negative effects of education on crime in younger years.

Table 1 summarizes the findings of the existing literature, and unless indicated otherwise, reports effects for males only. It is worth noting that the findings reported in Table 1 measure crime over a longer duration of an individual's lifespan than used in this paper.

Table 1: Existing Results

Authors	Property Crimes	Violent Crimes	Total Crimes
Additional year of education reduces crime by:			
Lochner and Moretti (2004)	11%	11-12%	16-18%
Hjalmarsson et al. (2015)	14%	10%	7%
Machin et al. (2011)	26-30%	8-15% †	19-21%
10% Increase in High School Graduation/Those with Qualification reduces crime by:			
Lochner and Moretti (2004)	6%	8%	9-10%
Buonanno and Leonida (2009)◇	4-5%	N/A	3% †
Machin et al. (2011)	9-10%	3-5%	7-8%
10% Increase in proportion staying after 16 causes:			
Machin et al. (2012)	18%	13%	11-17% (women-men)

† indicates an effect is insignificant at standard significance levels. ◇ indicates effects reported are for both men and women.

While the relationship between an individual's educational attainment and their criminal propensity as an adult has not been examined in Denmark, Landersø et al. (2015) examine how the age at which children start school affects their criminal propensity, exploiting a discontinuity in the age that children typically first begin primary education. The authors find that children old for their grade are significantly less likely to be charged with a crime before the age of 18, and that this relationship appears to be driven by incapacitation effects, as those who start later also graduate later and are enrolled in school longer than those who start school at a young age.

3.1 Education and Crime Using Twin Data

Most similar in terms of methodology to this paper, Webbink et al. (2013) analyze education and crime using survey data on Australian twins. The authors investigate the reverse causality of education and crime, finding that the effects of early arrests on crime dominate the effects of education on crime; completing high school is associated with a 1.8 percentage point reduction in the probability of arrest,[5] while an early arrest increases the probability of arrest as an adult by around 11 percentage points. When correcting for possible measurement error in self-reported levels of education, the estimated effects of education on crime are larger in magnitude, giving strength to the argument that measurement error could be an issue in the self-reported education data. Due to the strong effects of juvenile crime on education and the dominance of early arrest in explaining adult crime, the authors conclude that while education can reduce crime, limiting involvement in juvenile crime is a more dominant mechanism than education in terms of crime reduction. For the same reasons, the authors also conclude that reverse causality plays a large role in estimating the impact of education on crime. However, one shortcoming of Webbink et al. (2013) is that the concerns over measurement error in the twin survey data as well as the limited variation in the juvenile crime measure,[6] limit the extent to which these strong results on juvenile crime are generalizable.

4 Data

In order to analyze the heterogeneous impacts of education on criminal behavior within Denmark, this paper makes use of detailed Danish Register Data provided by Statistics Denmark. A comprehensive statistical database of Danish residents from 1980-present, Danish Register Data is compiled through a variety of administrative sources to create an individual level panel dataset of every Danish resident. Each individual has a unique identification number, which is used to match individuals across various data sources. The detail and richness of the dataset make it possible to control for a variety of individual level factors which could be unobservable in other datasets. For example, by linking children to parents using their identification numbers, it is possible to include factors such as household characteristics during childhood and parental education. The dataset provides information on income, employment, personal characteristics (factors such as sex, age,

[5] This effect is 6 percentage points when considering only identical twins, and insignificant.
[6] Only 14 identical twin pairs have variation in their juvenile crime measure.

marital status, nationality, etc), education, and detailed criminal history including the exact date and crime type corresponding to specific offenses, convictions, and incarcerations.

4.1 Sample Definition and Restrictions

Twins are defined as individuals who are born on the same day and have both the same mother and father. An issue for twin analysis, which is especially pertinent for analyzing the effects of education on crime as the majority of individuals are not criminals, is having a sufficient number of twin observations. The potential usable twin sample is composed of 30,560 twins (from 15,280 pairs) all of whom are aged 18 or above in 2000, are not enrolled in education in 2000, contain educational information in 2000, and whose twin also contains educational information in 2000.

To ensure that twins are as similar as possible, two additional sample restrictions are imposed. Firstly, twins who were raised, at any point below the age of 16, in different childhood households are excluded from the estimation sample. While there are not many twin pairs raised in different households,[7] focusing only on twins who were raised in the same household excludes twins who experience vastly different environmental factors. It is also worth noting a similar check was performed for the school twins attended, and revealed that all twin pairs attended the same schools from grade 7 and onwards.[8]

Secondly, 2,645 different gendered twin pairs, composing approximately 31% of the number of twin pairs, are excluded. Differences in gender within twins are important for two reasons. Firstly, men are much more likely to commit crimes than women, so accounting for gender differences between twins is quantitatively important. Secondly, gender different twins are non-identical (DZ),[9] and are less genetically similar than identical twins. As data on twin type (MZ or DZ) is unavailable, distinguishing between different gendered twins is the only way to exclude twins who can confidently be classified as non-identical. Estimation throughout Sections 7 & 8 is always split by gender of the twin pair, where both twins are male or both twins are female.

While excluding different gendered twins will reduce the number of non-identical twins in the sample, non-identical twins remain in the sample. Skytthe et al. (2011) show that among twin pairs born from 1968-1982, 2,788 are MZ, 2,887 are same gender DZ, 2,921 are opposite gender DZ, and 1,616 have unknown zygosity. These measures give a very precise idea of the zygosity of the sample of twins analyzed in this paper, as this cohort of twins is 18-32 in 2000, nearly the exact

[7] There are 505 twin pairs in 2000 among the final usable sample of twins.
[8] This data is only available from grade 7 and onwards.
[9] Biologically, they cannot be MZ twins.

age ranges used in the final sample of twins described below. Taking these numbers as correct, eliminating different gendered twins from the estimation sample eliminates approximately half of the DZ twins from the entire twin sample.

In addition to this, it is also necessary to eliminate twins for whom data is unobservable at the ages of 15 and 16 in order to observe juvenile criminality. This imposes two additional restrictions: (1) limiting the sample to twins who are, at a maximum, aged 15 in 1980 when the data begins and (2) eliminating those who are not in the data at the ages of 15 or 16. Imposing these restrictions eliminates 12,314 twins who are either older than 15 in 1980, who are not present in the data as a juvenile, or whose twin is not present in the data as a juvenile from the potential estimation sample that could be used.

While not ideal, this restriction is necessary in order to control for, as in Webbink et al. (2013), whether a twin was convicted of any crime as a juvenile. It is worth noting that this restriction excludes any twin who is 36 or older in 2000, imposing an upper bound on the age of twins by construction. While this may be of concern, individuals aged 18-35 can be thought of as an age range where a large portion of offending takes place, and results which include all twins, produce similar (in terms of sign and significance), but unequivocally smaller effects of education on crime.[10] This smaller magnitude of the effects of education on crime in the unrestricted sample is likely attributable to the use of an older sample, as young individuals are more likely to offend.

As juvenile criminality is a strong predictor of adult criminality, controlling for juvenile criminality is likely to be quantitatively important. Specifically, controlling for juvenile crime history can be thought to capture unobservable characteristics that vary within twin pairs and determine adult criminality.[11] Throughout this paper, juvenile crime is captured through the inclusion of two control variables: whether an individual was convicted of a crime while aged 15 and whether an individual was convicted of a crime while aged 16.[12] Juvenile crime is analyzed separately by age to examine if the timing of juvenile crime matters for the effects of education on crime.

Limiting the variable to crimes committed below the age of 17 increases the plausible exogeneity of this proxy variable, as younger persons are still restricted by compulsory schooling laws and are

[10] Results available upon request.

[11] In addition to being a proxy for unobservables, the inclusion of a juvenile crime dummy could also be interpreted as including a lag dependent variable, which in the use of within twin fixed effects estimation, would produce biased results.

[12] The age of criminal responsibility in Denmark is 15. Due to this, any crimes committed by individuals aged 14 and younger are not observable in the data. Age is recorded as the exact age when an individual is charged by the police with a crime.

legally unable to leave school. Because of this, juvenile crime committed within the compulsory schooling window are less likely to directly affect an individual's educational attainment. While 15 year olds will still be restricted by these laws, some 16 year olds will have completed compulsory schooling during the age of 16. While using only 15 year olds would ensure students are still bound by compulsory schooling laws, including 16 year olds as well provides a more complete picture of juvenile delinquency. It should be noted that specifications excluding the juvenile crime at age 16 control and specifications including the control produce similar results. Because the extent to which a juvenile delinquency dummy can be considered truly exogenous in a regression of crime on education is perhaps uncertain, all baseline results are reported both with and without controls for juvenile crime history.

4.2 Crime and Education Definitions

Given the panel nature of Danish Register Data, a very complete picture of an individual's criminal history is observable. However, as with any administrative individual-level crime data, individuals are only classified as criminals if they are apprehended for the crime committed. Due to this, there could be measurement error in the crime data. In particular, if more skilled or clever criminals are both more educated and also better able to avoid detection, then the estimates of the effects of education on crime will be biased. As alternative measures of criminal activity, such as self-reported crime are unavailable, there is little that can be done to investigate this potential issue. However, as Danish Register Data is linked directly to police records, any individual either charged, convicted, or incarcerated in Denmark can be classified as a criminal.

Using detailed crime codes, it is possible to identify types of offenses (property, violent, etc) as well as specific offenses (assault, motor vehicle theft, etc). The obvious concern with using detailed offenses is that to begin with, each offense does not contain many individual observations, a problem which is only compounded when estimating within twins. As such, broad offense categories, such as total, property, and violent crimes, are used. Total crimes correspond to the Danish classification of offenses are comprised of: sexual, violent, property, alcohol related traffic, narcotics, firearms, tax, unknown, and other crimes, as well as crimes against special legislation.[13] A detailed discussion of crime types, as well as the offenses that make up property and violent crimes, can be found in Appendix A.

Throughout this paper, crime is defined as a binary variable indicating if a twin has been

[13]This excludes traffic violations and citations, accidents, etc which are also recorded in police data.

convicted of a crime which was committed between 2001-2006,[14] while education is measured as the highest education an individual has obtained in 2000. Using only the year 2000 enables the use of within twin fixed effects estimation, while using whether a twin was convicted in the 6 years following 2000 provides a more complete picture of offending than if a single year was used.[15] A summary of Denmark during the time period of the analysis is provided in Appendix B.

A major advantage of using Danish Register Data for twin estimation is that, unlike most twin datasets, data is not obtained through surveys of twins, but through administrative sources. One problem in using twin survey data is that it is subject to measurement error, caused primarily by twin recall errors. In addition, selective response amongst surveyed twins can introduce selection bias if twins who do not respond to the survey differ systematically from twins who do respond. As Danish Register Data is linked directly to administrative sources, within twin estimation conducted in this paper is free from measurement error caused by twin recall errors and selection problems which remain in twin survey data. However, as years of education is calculated based on achieved qualifications, this could introduce measurement error in education length if a twin takes either more or less years to achieve a given qualification. Due to this, education defined in terms of the qualification achieved is also analyzed in addition to years of education.

5 Summary Statistics

Mean values and standard deviations for relevant determinants of an individual's criminal propensity included in the analysis for the twin sample are summarized in Table 2. These values are separated by whether or not an individual was convicted of a crime committed from 2001-2006 in columns (1) and (2) respectively. Two striking differences appear when comparing these two columns, the differences in years of education and whether an individual was convicted of a crime as a juvenile. Those convicted of some crime, on the whole, receive 1.5 years less of education, and are 9 and 10 percentage points more likely to have been convicted of a crime at age 15 and 16 respectively.

The gender difference in criminals, which has long been documented by economists and crimi-

[14] Specifically, whether an individual was convicted of a crime which was committed between January 1, 2001 to December 31, 2006. For a small fraction of crimes, the date of the offense is unobservable. For these crimes, the date of conviction is used instead. Estimation using the date of conviction or only those offenses with a date of offense produce similar results.

[15] While the results do fluctuate slightly between the chosen year, results are relatively stable across years, and are available upon request.

Table 2: Summary Statistics of Sample of Twins by Whether Charged with Crime or Not During 2001-2005

Variable	(1) Not Convicted of Crime Committed 2001-2006	(2) Convicted of Crime Committed 2001-2006	(3) Total	(4) (1)-(2)
Years of Education	12.46	10.97	12.38	1.49**
	(2.18)	(2.11)	(2.20)	[17.3]
Male	0.48	0.86	0.51	-0.38**
	(0.50)	(0.35)	(0.50)	[-19.2]
Age	27.82	27.06	27.77	0.75**
	(4.99)	(5.38)	(5.02)	[3.8]
Juvenile Crime 15	0.01	0.10	0.02	-0.09**
	(0.11)	(0.31)	(0.13)	[-17.9]
Juvenile Crime 16	0.01	0.11	0.02	-0.10**
	(0.12)	(0.32)	(0.14)	[-18.1]
Parents Highly Educated†	0.43	0.35	0.42	0.08**
	(0.49)	(0.48)	(0.49)	[3.6]
High Crime Municipality at 15	0.66	0.71	0.66	-0.05*
	(0.47)	(0.46)	(0.47)	[-2.6]
Number of Twins	11276	670	11946	
Fraction of Twin Sample	94.39%	5.61%	100%	

Mean values for 2000 values. Standard deviations in parentheses, t statistics reported in brackets. **, *, and + correspond to significance at the 1% 5% and 10% levels respectively. †: sample sizes for Both Parents Highly Educated are 9,888, 548, and 10,436 respectively due to missing parental education information.

nologists, is also visible in Table 2, with 86% of twins convicted during 2001-2006 being male. For twins whose parental education information is available, those convicted as an adult also have a lower fraction of parents who are both highly educated, a point which is investigated further in Section 8. The fraction of twins convicted of some crime in 2001-2006, 5.6% of the sample twin population, is also reported in the bottom of Table 2.

Consistent with the descriptive evidence on the links between education and crime, Figure 1 displays the fraction of twins with certain years of education separated by whether a twin was convicted of a crime committed from 2001-2006 or not.[16] Twins who are convicted of any crime are, on the whole, less educated than twins who are not convicted, with more than 60% of those convicted with a crime receiving 10 years or less of education. Conversely, there are less twins who are not convicted of a crime educated only 10 years or less, with a large fraction of twin pairs receiving 12 years or more of education.

[16]In order to comply with Statistics Denmark's data confidentiality criteria, individuals with 17 or more years of education are not presented in Figure 1.

Figure 1: Percentage of Twins with Years of Education Separated by Criminality

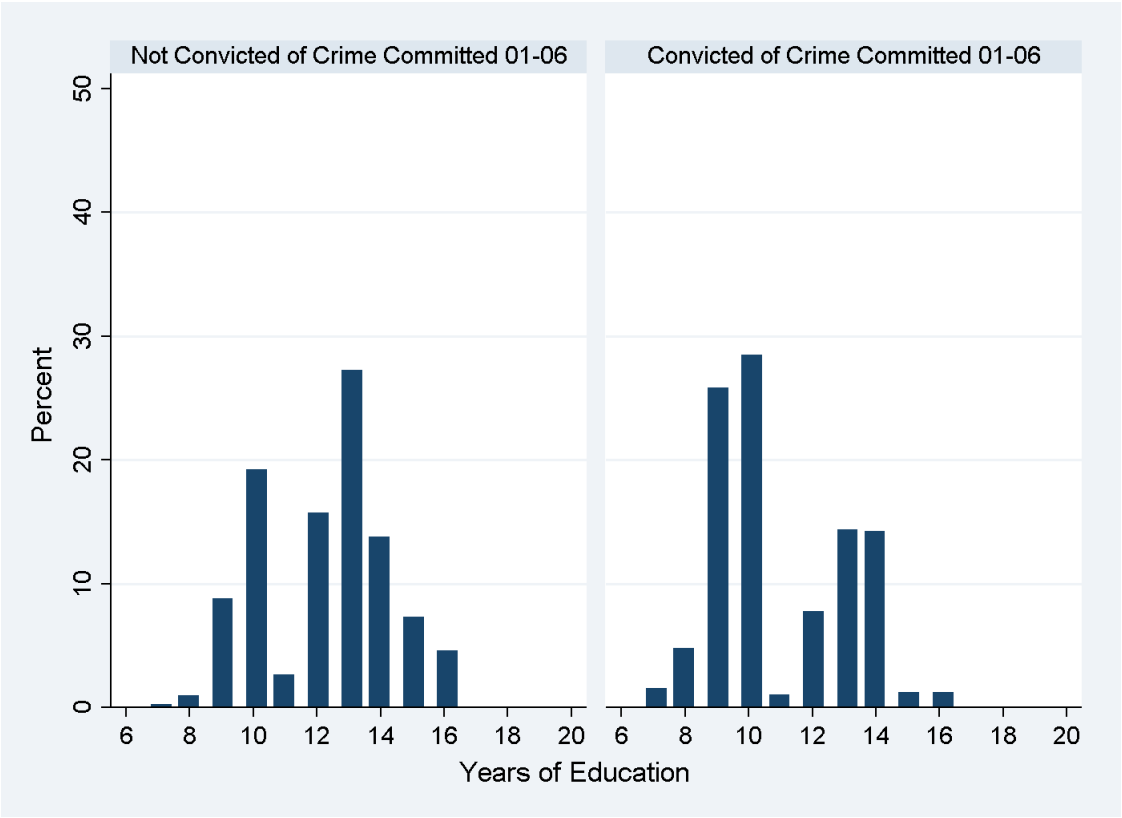


Table 3 displays the distribution of crimes by type for twins convicted from 2001-2006 for both males and females combined. The majority of convictions are alcohol related traffic offenses followed by property crimes. The total number of convictions in Table 3 does not perfectly match the total number of twins convicted with a crime in Table 2 as there are some twins who are convicted of multiple types of crime during this time period.

Table 3: Distribution of Crimes by Type

	(1)	(2)
	Both Male and Female Twin Pairs	
Crime Type	# Twins Convicted	%
Number Twins Convicted Of Any Crime	670	
Sexual	17	1.9%
Violent	129	14.3%
Property	221	24.6%
Other	65	7.2%
Alcohol Related Traffic	225	25.0%
Narcotics	103	11.4%
Firearms	39	4.3%
Special Legislation	101	11.2%
Sum of All Types	900	100%
Number of Twins	11946	

Sum of all convictions is greater than the number of individuals convicted of any crime due to individuals being convicted of multiple crime types from 2001-2006. Percents in column (2) correspond to percentage of sum of all crime types (900) for a given crime. Twins are not separated by gender in order to comply with Statistics Denmark's data confidentiality criteria.

Table 4 displays within twin differences in years of education. In 2000, 55% of twin pairs have different education lengths, with the vast majority of twins having differences of 4 years or less in educational attainment.

6 Empirical Framework

An advantage of using within twin estimation is it is possible to control for factors (both observable and unobservable) which both affect an individual's probability to engage in crime and are constant between twins, including genetic, environmental, and familial factors. By estimating fixed effects

Table 4: Distribution of Education Differences by Twin Pair

Differences in Years	Frequency	Percent	Cumulative Percent
0	2675	44.78	44.78
1	1230	20.59	65.38
2	750	12.56	77.93
3	671	11.23	89.17
4	383	6.41	95.58
5	187	3.13	98.71
6	46	0.77	99.48
7+ years	31	0.52	100.00
Number of Twin Pairs	5973	100.00	

within twin pair, variation in twin education levels enables the estimation of causal effects of education on crime. To obtain causal estimates, the following linear probability[17] fixed effects regression is estimated:

$$C_{ij} \;=\; \beta(S_{ij}) + \phi_1(J_{ij}^{15}) + \phi_2(J_{ij}^{16}) + \gamma(A_{ij}) + \alpha_i + \varepsilon_{ij} \tag{1}$$

C_{ij} represents whether twin j in twin pair i was convicted from 2001-2006 and is explained by: S_{ij}, the years of schooling of twin j in twin pair i; J_{ij}^{15} and J_{ij}^{16}, control variables for whether twin j in twin pair i was convicted of a crime as a juvenile at age 15 or 16; A_{ij}, unobservable factors which vary across both twin pairs and twins; α_i, unobservable factors which are identical to twins but vary across twin pairs; and ε_{ij}, the (criminal) error term.

To further explore the assumptions necessary for within twin fixed effects estimation to identify causal effects, the (criminal) error term, as in Bound and Solon (1999), can be expanded into two separate components: a component which is constant between twins and a component which is random, but only to twin j in twin pair i.

$$\varepsilon_{ij} \;=\; f_i + u_{ij} \tag{2}$$

[17] A linear probability model is used in order to facilitate the comparison of coefficients across specifications to see how the estimated effects change when examining the heterogeneity and robustness of the baseline results. Estimation using logit provide similar results, and are available upon request.

Additionally, an individual's schooling can be explained by:

$$S_{ij} \;=\; \delta_1(A_{ij}) + g_i + w_{ij} \tag{3}$$

Within twin fixed effects estimation is able to account for f_i, the portion of ε_{ij} which is constant within twin pair. Thus, as with any twin study, the identifying assumption is that differences in education are driven by factors which do not also impact the outcome of interest, here crime participation. In other words, it is assumed that differences in schooling levels are caused by random factors (w_{ij}) which are unrelated to an individual's propensity to engage in crime, $cov(u_{ij}, w_{ij}) = 0$, and also $cov(A_{ij}, w_{ij}) = 0$, so that schooling and crime are not correlated through A_{ij} when estimating within twins. Related to this, it is required that $A_{i1} = A_{i2}$, otherwise differences in schooling are driven by differences in unobservable factors which vary between twins. In order for equation (1) to produce unbiased estimates, these assumptions must hold. If differences in factors that affect both education and crime participation drive variation in twin education levels,[18] then within twin fixed effects estimates will be biased.

An additional concern is the critique of Bound and Solon (1999) who, building upon the work of Griliches (1979), state that while MZ twins are nearly genetically identical, differences in unobservable factors[19] which affect both education and, in this paper, crime decisions are determined by more than just genetic factors as twins experience different factors during their development. Twins are exposed to a variety of different environmental factors, particularly in school, and while genetically they may be similar, these environmental differences can contribute to unobservable differences which may be driving both crime and education decisions.

This criticism is related to the assumption of the equality of unobservable factors between twins, $A_{i1} = A_{i2}$. If unobservable factors between twins are different, then within twin estimation will produce biased estimates. To further investigate this criticism, A_{ij} can be broken down into two separate components:

$$A_{ij} \;=\; G_i + E_{ij} + \mu_{ij} \tag{4}$$

where G_i are unobservable factors which are the same between twins (genetic factors) and E_{ij} are unobservable factors which can vary both within and across twin pairs (environmental factors).

[18] For example time discounting or risk aversion.
[19] The classic example is ability

Twins experiencing different environmental factors is equivalent to the case where $E_{i1} \neq E_{i2}$. In this case, unobservable factors (A_{ij}), which affect both schooling and criminality are different and within twin estimation will produce biased results. While it is not straightforward to test the validity of assuming $A_{i1} = A_{i2}$, one important factor that can be exploited, as previously mentioned, is to exclude twins who were raised in separate households as children. In this case, the environmental factors between twins are vastly different, and this identifying assumption for within twin estimation in all likelihood does not hold, as $E_{i1} \neq E_{i2}$, and $A_{i1} \neq A_{i2}$.

One additional concern is that early differences in health, potentially from the time of birth, could drive differences in twin education levels. In particular, competition for nutrients in utero could cause poor health outcomes of one twin and not another leading to differences in, for example, birth weight and these differences could potentially determine the within twin variation in education levels. Using Swedish data, Sandewall et al. (2014) empirically investigate these concerns over the validity of the identifying assumptions of twin studies, finding that the estimated effects of education on wages fall when including measures such as twin birth weight and IQ scores into standard twin earnings regressions. As such, the author's question the validity of the identifying assumptions in twin studies.

There is also a large literature, commonly using twins as an identification strategy, which analyzes the impact of infant health and birth weight on numerous adult outcomes such as employment, education, and mortality. This literature has produced mixed findings in that some papers find that differences in early health outcomes such as birth weight and Apgar infant health scores[20] have no effects on the probability of twin's attending high school at the age of 17 (Oreopoulos et al. 2008), estimate an effect of birth weight on education which is very small in magnitude (Royer 2009), or estimate an impact of twin birth weight on high school completion which is larger in magnitude (Black et al. 2007). While these studies tend to report some variation in the early health outcomes of twins, for example Oreopoulos et al. (2008) report differences in Apgar scores and birth weight, these differences are often not important enough to explain a large portion the differences in education between two twins. While the similarity of twins remains a concern here, Section 9.3 presents differences in GPA for a subset of twins for whom GPA information is available, as birth weight, IQ, or other early health measures are unavailable for the sample of twins used in this paper.

[20] Apgar scores measure newborn health over five criteria, where the scores range from 0-10.

A related concern could be that one twin is born with a congenital birth defect, which could also drive differences in twin educational levels. However, previous studies have reported these defects affecting either twin are very rare,[21] and are unlikely to be of major concern. One final possibility is that behavioral disorders affecting one twin could drive differences in education levels. In the working paper version of Webbink et al. (2013) (Webbink et al. 2008), the authors find that controlling for juvenile crime has a much larger impact on the effects of education on crime than when controlling for conduct disorders. While concerns over behavioral disorders may remain, it seems likely that controlling for juvenile crime will be more quantitatively important in estimating the impact of education on crime.

7 Results

7.1 OLS Estimation on Population Compared with Existing Results

Prior to examining the heterogeneous effects of education on crime, it is first worthwhile to establish whether the Danish population provides a good basis for comparing how the effects of education on crime depend on specific factors and individuals by comparing results for the general population to existing findings in the literature. If population results across countries are similar, it is suggestive that the heterogeneous effects found in Denmark are not due to something particular about Danish data. A natural point of comparison is Hjalmarsson et al. (2015), who use Swedish population data which is Register based and collected in a similar fashion. However, lacking a similar instrument, OLS results are the only results that can be compared.

Wherever possible, this separate population data is constructed to ensure the comparability of the Danish data with the dataset used in Hjalmarsson et al. (2015). For example, the exact same birth cohorts (1943-1954) are used, and the definitions of crime and education are identical. The main difference results from the fact that Danish Register Data only begins in 1980, while the Swedish data can be linked back to 1960. As such, factors such as municipality of residence and educational attainment are recorded at the start of 1980 and not earlier, and crime data is only available from 1980-2007 instead of from 1973-2007 as in the Swedish data.

OLS results on Denmark are presented in columns (1) and (2) of Table 5 and for Sweden in columns (3) and (4). Despite these minor differences in the years available, OLS results using

[21] For example, Black et al. (2007) report excluding only 2.1% of twin pairs from their sample where either twin has a birth defect.

Table 5: OLS Results of Danish Data Compared to Hjalmarsson et al. (2015)

	(1)	(2)	(3)	(4)
	Denmark		Sweden - from Hjalmarsson et al. (2015)	
	Total - Male	Total - Female	Total - Male	Total - Female
Years of Education	-0.026**	-0.008**	-0.026**	-0.006**
	(0.001)	(0.001)	(0.001)	(0.000)
Municipal FEs?	Yes	Yes	Yes	Yes
Birth Cohort FEs?	Yes	Yes	Yes	Yes
	Prison - Male	Prison - Female	Prison - Male	Prison - Female
Years of Education	-0.010**	-0.001**	-0.011**	-0.001**
	(0.001)	(0.000)	(0.001)	(0.000)
Municipal FEs?	Yes	Yes	Yes	Yes
Birth Cohort FEs?	Yes	Yes	Yes	Yes
N	473942	453425	208626	203734

Standard errors clustered at municipal level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively. OLS results from Sweden are taken from Hjalmarsson et al. (2015), Table 5. Total corresponds to a conviction for any crime from 1980-2007 in Denmark and from 1973-2007 in Hjalmarsson et al. (2015). Prison corresponds to a prison sentence from 1980-2007 in Denmark and from 1973-2007 in Hjalmarsson et al (2015).

comparable Danish data are nearly identical to the OLS results in Hjalmarsson et al. (2015).[22] While Denmark and Sweden are institutionally very similar, the consistency of the OLS results of the entire population with findings of previous literature, albeit during a different time period used in the main results, suggests that Danish data provides a good basis for investigating the heterogeneous effects of education on crime. The results that follow examine the sample of Danish twins in order to identify exactly for what types of individuals we can expect education to reduce crime.

7.2 OLS Estimation on Sample of Twins

Having established the comparability of the Danish population as a whole to existing studies in the literature, Tables 6 and 7 report results using OLS estimation on the sample of male and female twins in the year 2000 respectively. Throughout the results section, columns (1) & (2), (3) & (4), and (5) & (6) show results using convictions for total crimes, property crimes, and violent crimes as a dependent variable. Even columns present results controlling for juvenile crime. Across all specifications, standard errors are clustered at the twin level.

[22]The sample in Denmark uses the full population data, where Hjalmarsson et al. (2015) use a representative sample of the Swedish population.

Table 6: OLS Estimates of Sample of Male Twins In Same Childhood Environments

	(1) Total	(2) Total	(3) Property	(4) Property	(5) Violent	(6) Violent
Years of Education	-0.0255**	-0.0221**	-0.0089**	-0.0071**	-0.0067**	-0.0055**
	(0.0018)	(0.0017)	(0.0011)	(0.0010)	(0.0009)	(0.0008)
Juvenile Crime 15		0.2089**		0.1143**		0.1269**
		(0.0348)		(0.0273)		(0.0268)
Juvenile Crime 16		0.2138**		0.1151**		0.0307+
		(0.0332)		(0.0258)		(0.0181)
R^2	0.039	0.076	0.015	0.050	0.012	0.041
Number Individuals	6038	6038	6038	6038	6038	6038

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

For both male and female twins, years of education significantly and negatively affects the probability of conviction across crime types. This effect is most dominant for total crimes, where an additional year of education, holding all other things equal, reduces the probability of conviction for an offense between 2001-2006 by 2.6 percentage points for males and 0.5 percentage points for females. When including juvenile crime controls, the effect of education on the probability of conviction is drastically reduced for males, while only marginally reduced for females. Having committed crime as a juvenile significantly increases the probability of conviction across crime types for males. The results in Tables 6 and 7 fail to deal with the endogenous relationship between education and crime and within twin fixed effects estimation, which identifies causal effects of education on crime, is reported below in Section 7.3.

Table 7: OLS Estimates of sample of Female Twins In Same Childhood Environments

	(1) Total	(2) Total	(3) Property	(4) Property	(5) Violent	(6) Violent
Years of Education	-0.0052**	-0.0051**	-0.0033**	-0.0032**	-0.0010**	-0.0010**
	(0.0009)	(0.0009)	(0.0007)	(0.0007)	(0.0004)	(0.0004)
Juvenile Crime 15		0.0762		0.0185		-0.0037**
		(0.0545)		(0.0328)		(0.0012)
Juvenile Crime 16		0.0021		0.0158		-0.0038**
		(0.0314)		(0.0310)		(0.0012)
R^2	0.008	0.010	0.006	0.006	0.002	0.002
Number Individuals	5908	5908	5908	5908	5908	5908

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

7.3 Within Twin Estimation on Sample of Twins

Tables 8 and 9 analyze separately same gendered male and female twin pairs who were also raised in the same household. As criminals are overwhelmingly male, it is perhaps unsurprising that a greater negative effect of education on the probability of conviction is seen for males compared to females. For male twins who were raised in the same childhood household, a ceteris paribus additional year of education reduces an individual's probability of conviction for an offense committed between 2001-2006 for any crime by 1.3 percentage point (14% of the sample mean). For property crimes, marginally significant effects of education are found, where an additional year of education lowers the probability of conviction by 0.3 percentage points (12%) while for violent crimes, the estimated effect of an additional year of education is 0.4 percentage points (19%). When controlling for participation in juvenile crime, these numbers fall to 14%, 12%, and 18% respectively. On the whole, the estimated effects of education on crime are large for males. For female twins, the estimated effect of years of education is imprecisely estimated, with virtually no effects either positive or negative. Estimating the effects of education on the probability of conviction for female twins leads to the conclusion that education can predominantly reduce crime for male twin pairs.

Table 8: Within Twin Fixed Effect Estimates of Sample of Same Gender Male Twins In Same Childhood Environments

	(1) Total	(2) Total	(3) Property	(4) Property	(5) Violent	(6) Violent
Years of Education	-0.0132**	-0.0129**	-0.0034^{+}	-0.0033^{+}	-0.0036*	-0.0035*
	(0.0031)	(0.0031)	(0.0019)	(0.0019)	(0.0016)	(0.0016)
Juvenile Crime 15		0.0999^{+}		0.0421		0.0965**
		(0.0517)		(0.0361)		(0.0331)
Juvenile Crime 16		0.1071*		0.0431		0.0041
		(0.0447)		(0.0353)		(0.0227)
R^2	0.039	0.076	0.015	0.050	0.012	0.040
Number Individuals	6038	6038	6038	6038	6038	6038
Twin Pairs	3019	3019	3019	3019	3019	3019

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

Comparing even and odd columns reveals the importance of accounting for juvenile crime history. Again, participation in juvenile crime increases the probability of conviction as an adult for males, although the estimated effects are lower in magnitude and less precisely estimated

Table 9: Within Twin Fixed Effect Estimates of Sample of Same Gender Female Twins In Same Childhood Environments

	(1) Total	(2) Total	(3) Property	(4) Property	(5) Violent	(6) Violent
Years of Education	-0.0008	-0.0009	-0.0001	-0.0001	-0.0005	-0.0005
	(0.0014)	(0.0014)	(0.0010)	(0.0010)	(0.0006)	(0.0006)
Juvenile Crime 15		0.1281^{+}		0.0432		0.0015
		(0.0674)		(0.0409)		(0.0020)
Juvenile Crime 16		-0.0766		-0.0373		-0.0361
		(0.0488)		(0.0352)		(0.0352)
R^2	0.008	0.003	0.006	0.000	0.002	0.000
Number Individuals	5908	5908	5908	5908	5908	5908
Twin Pairs	2954	2954	2954	2954	2954	2954

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

than in Table 6. Having been convicted of a crime committed at 15 increases the probability of conviction by 10 percentage points for total crimes, 4.2 percentage points for property crime, and 9.7 percentage points for violent crime, where the effect on property crimes is insignificant. Having been convicted at the age of 16 positively impacts the probability of conviction as an adult, an effect which is significant for total crimes.

While the results of this paper indicate the underlying importance of involvement in juvenile crime on adult crime participation, an additional year of education leads to a 1.3 percentage point reduction in the probability of conviction for males. While this number is indeed smaller than the 10 percentage point effect estimated for juvenile crime at age 15 or 16, there is considerably less gap between the two figures than in Webbink et al. (2013), where the authors find a large effect of juvenile crime on adult crime participation but a much smaller (and sometimes statistically insignificant) effect of completion of high school on adult crime. As such, this paper finds evidence both that accounting for juvenile crime history when estimating the impact of education on adult crime is important and once controlling for this, that education significantly reduces adult participation in total, property, and violent crimes for males by a sizable factor. However, it should be noted that the results presented above are potentially biased as it is only possible to exclude fraternal twins, who only share 50% of their genes, of different genders and not fraternal twins of the same gender. Lacking zygosity information, Section 9 investigates the robustness of these results taking into account the potential bias of DZ twins in the estimation sample, while the next

section highlights the heterogeneous effects of education on crime.

8 Heterogeneous Effects

Having confirmed a link between education and crime in the baseline results, the next section outlines how the crime reducing effects of education differ across four aspects: parental education, growing up in a high or low crime neighborhood, specific educational programs, and detailed crime types. In the interest of brevity and given that estimation including juvenile crime controls produces very similar results to estimation not controlling for juvenile crime, only estimation controlling for juvenile crime is reported in the following sections.

8.1 Parental Education

A variety of studies using various identification methods have established the existence of intergenerational transmission of education (see Holmlund et al. (2011) for a summary) as well as the intergenerational transmission of crime (Hjalmarsson and Lindquist 2012, 2013), so it is natural to investigate how the crime reducing effects of education vary across children with differently educated parents. Similar to this, Meghir et al. (2012) investigate the intergenerational links of parental education and criminal participation of children, finding a national education reform in Sweden not only decreased parental criminality through increased education, but also reduced son's criminality as well. Investigating the exact causes of the intergenerational relationship, the authors find that increases in parental resources and increases in the quality of parenting through better role modeling are the most plausible mechanisms driving this relationship. While the analysis conducted here is similar, the former investigates the impact of an exogenous change in parental education on criminality of children while the analysis below investigates how the effects of education on crime for children vary across a given level of parental education. Given the importance of parental background in both the education and criminality of children, larger effects of education on crime are expected for children of low educated families, with smaller effects expected for high educated families.

Throughout this subsection, parental education is defined as high if a parent has been educated to the high school level or beyond and low if a parent is educated to a level less than high school. Table 10 presents results restricting the estimation sample to two different subsamples of twin pairs: those with one or both parents with low education and those with both parents with high

Table 10: Within Twin Fixed Effect Estimates of Sample of Same Gender Twins In Same Childhood Environments By Educational Attainment of Parents

	(1)	(2)	(3)	(4)	(5)	(6)
	Male Twins			Female Twins		
At Least One Low Educated Parent						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0199**	-0.0041^{+}	-0.0041*	-0.0022	-0.0020	-0.0014
	(0.0040)	(0.0024)	(0.0020)	(0.0022)	(0.0018)	(0.0010)
Number Individuals	3080	3080	3080	2924	2924	2924
Twin Pairs	1540	1540	1540	1462	1462	1462
Both Parents High Educated						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0043	-0.0042	-0.0046^{+}	-0.0008	0.0012	0.0005
	(0.0051)	(0.0034)	(0.0024)	(0.0025)	(0.0014)	(0.0011)
Number Individuals	2200	2200	2200	2232	2232	2232
Twin Pairs	1100	1100	1100	1116	1116	1116

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively. Low educated parent is either a low educated father, low educated mother, or both low educated mother and father. Not reported are controls for juvenile crime.

education.[23] As before males and females are analyzed, as while little effect of education on crime was seen previously for women, it is possible that estimation on the overall sample of women masks substantial heterogeneous effects by parental education. Columns (1)-(3) present results for male twins, while columns (4)-(6) for female twins. It should be noted that the parental educational information of 755 same gender, same household twin pairs is missing, and these twin pairs are excluded from this analysis.[24]

The top panel of Table 10 suggests that for male twins who have at least one low educated parent, the effects of education on crime are larger in magnitude than comparable estimated effects using an unrestricted sample. In contrast to male twins with at least one low educated parent, when twins' parents are both highly educated, there are virtually zero effects of education on crime, with the exception of violent crimes. For males of highly educated families, the estimated effects

[23] 35% of twins with at least one low educated parent also have low education, while 23% of twins with both parents highly educated have low education.

[24] Baseline estimation on the subsample of twins who contain parental information produces similar, but larger estimated effects of education on crime. This is attributable to a younger sample, as older twins, who are less crime prone than their younger counterparts, are more likely to have parents whose educational information is unobservable, making them more likely to be excluded from the parental education sample. Assuming that missing parental education corresponds to low parental education produces results similar to those in the top panel of Table 10.

of years of education on total and property crimes are both small in magnitude and insignificant, where these differences between low and highly educated families are significant for total crimes. For females, there appear to be no significant effects of education on adult crime for either low or highly educated families, although the estimated effect of education on adult crime is negative for low educated families, but far from significant.

Appendix C analyzes the role of parental education in further detail, presenting results of the three parental education subgroups which comprise at least one parent with low education: those with both parents low educated, those with a low educated mother but high educated father, and those with a high educated mother but low educated father. However, due to the small number of twins in each of these groups, the results should be interpreted with some degree of caution. Taking this into account, Table A1 indicates some non-linearity in the effects of education on crime by parental education: male twins with two low educated parents experience the largest effects of education on total crime while those with one low educated parent, either mother or father, and one high educated parent experience less crime reducing effects of education. For female twins, as before, there appear to be no significant effects of education on crime.

The results presented in Tables 10 and A1 lead to the conclusion that parental education matters for the crime reducing effects of education of their children, where for males with at least one low educated parent, there are large crime reducing capabilities of education, but for males of highly educated parents, there are little crime reducing benefits of education. The fact that no effects are seen for females of low educated families reinforces the previous finding that education can reduce crime predominantly only for males. Three separate, but interrelated, explanations of these findings seem plausible. Firstly, Table 10 could simply be capturing the intergenerational transmission of education. If individuals with highly educated parents obtain more education and there is less crime reduction caused by higher education than lower education, then the lack of any negative effect seen for highly educated parents is due to the higher educational attainment of their children. Secondly, education could act as an adjusting mechanism, in that an additional year of education in terms of crime reduction, is very important for children of a low educated family and not very important for children of a high educated family. If, for whatever reason, children of low educated parents are more disposed to commit crime, then an additional year of education would have greater crime reducing benefits for these children. A third explanation could be that parental education captures "good parenting", and good parenting simultaneously leads to both more ed-

ucation and less crime. Lacking a way to disentangle these potential explanations, identifying the exact cause of these differential effects of education on crime across parental education is beyond the scope of the paper. However, given the supportive evidence of the last two mechanisms found in Meghir et al. (2012), it seems likely that parenting or social mobility are fundamental reasons for the heterogeneous effects across parental education.

8.2 Growing Up In High or Low Crime Neighborhood

In addition to family factors, environmental factors could also have differential impacts on the effects of education on crime. Many studies examine the Moving to Opportunity (MTO) experiments, which randomly allocate moving vouchers to low income families, to analyze the effects of moving to a better neighborhood on both education and criminal outcomes. Sanbonmatsu et al. (2006) find that children of families who were offered moving vouchers performed no better in school, while Chetty et al. (2015) find that, in the longer run, children moving to a neighborhood with less poverty at young ages are more likely to attend college and have higher wages. Kling et al. (2005) look at the effects of moving to a lower poverty/crime area on crime, finding that both males and females are less likely to engage in crime in the short run, while for males, participation in property crime actually increases in the long run. Damm and Dustmann (2014) use an exogenous placement scheme of refugees to examine in the impact of living in a high crime neighborhood, finding the share of young individuals convicted in neighborhoods increases the likelihood of conviction for male refugees later in life.

Growing up in a high crime area could also have a strong impact on the effects of education on crime. For instance, those residing in a high crime area could be more likely to engage in juvenile crime and/or less likely to pursue education. At the same time, those residing in higher crime areas could, all else equal, have larger returns to education than those residing in lower crime areas. As the sample of twins constructed all reside in the same household during childhood, and hence same municipality, both twins are exposed similarly to these environmental factors.

Table 11 examines the impact of growing up in a high or low crime area on the effects of education on adult crime for a predetermined neighborhood.[25] High crime neighborhoods are defined as a municipality where the youth conviction rate is higher than the youth conviction rate of the median municipality in that year. Youth conviction rate is defined, as in Damm and

[25]Across all twins, municipality of residence is measured in the year twins are age 15. Results are robust to alternative definitions of municipality. There are 275 municipalities in the time period analyzed in Denmark.

Dustmann (2014), as the fraction of individuals aged 15-25 convicted of a crime in a given year. As before, the estimation controls for juvenile crime, which is especially relevant in this subsection given the amplifying effects that growing up in a high crime neighborhood could have on an individual's propensity to engage in juvenile crime.

Table 11: Within Twin Fixed Effect Estimates of Sample of Same Gender Twins In Same Childhood Environments By Municipal Youth Conviction Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Male Twins			Female Twins		
Raised in a Municipality With Low Youth Conviction Rate						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0144**	-0.0046	-0.0082**	-0.0025	-0.0006	-0.0013
	(0.0049)	(0.0030)	(0.0029)	(0.0029)	(0.0021)	(0.0018)
Number Individuals	2090	2090	2090	1966	1966	1966
Twin Pairs	1045	1045	1045	983	983	983
Raised in a Municipality With High Youth Conviction Rate						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0122**	-0.0026	-0.0010	-0.0002	0.0001	-0.0002
	(0.0039)	(0.0024)	(0.0020)	(0.0016)	(0.0011)	(0.0005)
Number Individuals	3948	3948	3948	3942	3942	3942
Twin Pairs	1974	1974	1974	1971	1971	1971

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1% 5% and 10% levels respectively. High juvenile conviction: residing in a municipality at age 15 where the youth conviction rate is higher than the rate of the median municipality in that year. Low juvenile conviction: residing in a municipality at age 15 where the young conviction rate is equal to or lower than the rate of the median municipality in that year.

For males, the estimated effects of education on total crimes presented in Table 11 are relatively similar irrespective of growing up in a high or low crime municipality. While the effects, in percentage point terms, are greater for those raised in a low crime municipality, they are very similar to those raised in a high crime municipality. Education appears to only reduce violent crime for individual's raised in low crime areas, with negative, but insignificant effects seen for those in a high crime area. For property crimes, negative but insignificant effects of education are seen for both groups. For females, the effects of education on crime are greater for those raised in a low crime municipality, but are never significant at conventional levels for either low or high crime neighborhoods. With the exception of violent crimes, environmental factors, measured as being raised in a high or low crime neighborhood, appear to lead to minimal differences in terms of the crime reducing capabilities of education.

8.3 Differential Margins of Education

As discussed previously, one advantage of using within twin estimation to identify causal effects of education on crime is that, unlike a change in compulsory schooling laws, multiple margins of education can be investigated. Despite the many studies on the effects of education, little emphasis has been placed on how these effects differ across an individual's specific program of education. One exception is Åslund et al. (2015), who find that an expansion of vocational upper secondary education from two to three years lowered property crimes in Sweden. Their findings support the role of incapacitation effects, with the crime reduction largely attributable to the introduction of the additional third year in vocational education. This supports the idea that both vocational and non-vocational education can be expected to reduce criminal propensity.

In this section, qualifications are divided by vocational training and non-vocational education to correspond to the Danish education system. In Denmark during the time period examined, once students complete compulsory schooling of 9 years, they then can either progress directly to high school or attend an optional 10^{th} grade and then proceed to high school. There are three different types of high school students can attend in Denmark: regular, business (Højere Handelseksamen), and technical (Højere Teknisk Eksamen) high school. In addition to this, students can attend vocational training directly after compulsory schooling, which is similar to apprenticeship programs and geared not towards attending higher education, but rather professional employment. For simplicity, education of 9 or 10 years is referred to as lower secondary education, such that individuals can either attend high school or vocational training following lower secondary education.

Table 12 reports the effect of completion of three different types of education on crime: (i) completing any high school from lower secondary education, (ii) completing any university education from any high school, (iii) completing vocational training from lower secondary education. Results reported in Table 12 are constructed in such a way that the sample is restricted to twin pairs where both twins are in one of the two educational groups analyzed. For example, the results in the top row compare the criminal participation of one twin who has only completed lower secondary education to a twin who has completed any high school. While in principle the three types of high school could also be separately analyzed, it is necessary to combine all types of high school into one qualification in order to ensure there are an adequate number of twins in each educational group. It is worth noting that for some qualifications, the effects are being driven by a small number of twins who make up a small percentage of the population.

Table 12: Differential Margins of Education by Type of Education Program

	(1)	(2)	(3)	(4)	(5)	(6)
	Male Twins			Female Twins		
Regular (Non-Vocational) Education						
	Completing HS from Lower Secondary Education					
	Total	Property	Violent	Total	Property	Violent
Completing HS	-0.0993**	-0.0473*	-0.0253+	0.0273+	0.0266+	0.0072
	(0.0331)	(0.0225)	(0.0142)	(0.0144)	(0.0144)	(0.0072)
Number Individuals	1748	1748	1748	1820	1820	1820
Twin Pairs	874	874	874	910	910	910
	Completing Any University from HS					
	Total	Property	Violent	Total	Property	Violent
Completing Any Univ.	-0.0107	-0.0089	-0.0178	0.0056	0.0056	0.0056
	(0.0181)	(0.0089)	(0.0125)	(0.0097)	(0.0056)	(0.0056)
Number Individuals	1144	1144	1144	1608	1608	1608
Twin Pairs	572	572	572	804	804	804
Vocational Training						
	Completing Vocational Training from Lower Secondary Education					
	Total	Property	Violent	Total	Property	Violent
Completing Voc. Training	-0.0263	0.0002	-0.0117	-0.0104	-0.0036	-0.0095+
	(0.0190)	(0.0104)	(0.0106)	(0.0084)	(0.0032)	(0.0055)
Number Individuals	3462	3462	3462	2940	2940	2940
Twin Pairs	1731	1731	1731	1470	1470	1470

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively. Not reported are controls for juvenile crime.

Previous studies on education and crime have found that while the completion of high school (or an equivalent level) produces large crime reducing effects for males, attending university has little to no impact on crime participation (Lochner and Moretti 2004). Consistent with these expectations, for male twins, results on regular (non-vocational) eduction in Table 12 reveal the completion of high school leads to a 9.9 percentage point reduction in the probability of conviction for any crime, while the completion of any university has no significant effects either positive or negative. For female twins, completion of high school actually leads to a 2.7 percentage point increase in total and property crimes, an effect which is significant at the 10% level.

Conversely, results using vocational training, reported in the bottom panel of Table 12, display a different trend. For male twins, additional vocational training past the compulsory schooling level appears to have no crime reducing benefits. For female twins, vocational education lowers the probability of conviction for total and violent crimes, where the effect on total crimes is insignificant. The lack of any crime reducing effect of vocational training for males is striking, especially when compared to the strong crime reducing effects of completing high school seen for males. As the lengths of normal high school and vocational training programs are very similar, the differences seen in Table 12 are not due to incapacitation effects.[26]

These differential effects by type of education could be driven by underlying differences between the students of both types of education, underlying differences between the two types of education, or by underlying differences in employment prospects between the two education groups. If, for example, students who attend vocational training are more prone to commit crime than students who attend regular education, then it could take longer for education to negatively impact an individual's propensity to engage in criminal behavior. If this were the case, this could also be reinforced through peer effects while in school. An alternative explanation could be that the teaching and educational methods differ across the two types of education. If this were the case, student's criminal propensity could develop differently across the two educational programs. Another possible explanation is that the completion of high school increases an individual's probability of employment, but not for vocational training. If this were the case, then the crime reducing effects of employment caused by greater education would affect the two groups of students differently. However, as many vocational training programs are directly geared towards professional

[26]If anything, some vocational training programs can take longer than normal high school. In this case, the incapacitation effects of being involved in education for longer would lead to greater crime reducing effects of vocational training compared to normal high school, and the effects shown in Table 12 would actually underestimate the effects of completing high school compared to vocational training.

employment, this seems a less plausible explanation.

Whatever is driving the underlying differences in reduced criminality between these two types of education, the results displayed in Table 12 demonstrate the importance to crime reduction not only of an individual's educational qualifications, but also of an individual's educational program.

8.4 Detailed Crime Types

Estimation using all available crime categories that comprise total crime for male same household twin pairs is reported in Table A2 in Appendix D. Results are directly comparable to even columns of Table 8, which include juvenile crime controls. For more uncommon crimes, the effects are being driven by a small amount of male twins who make up a small percentage of the population. Taking this into account, estimated effects of education are generally negative across crime types, but with fluctuating significance.

The results of Table A2 reveal large crime reducing effects of education on traffic related alcohol crimes, where a ceteris paribus year increase in education lowers the probability of conviction by 0.7 percentage points (20%). Large negative effects are also seen for firearms and sexual crimes, where a ceteris paribus year increase in education leads to a 0.2 percentage point (32%) and 0.1 percentage point (44%) reduction in the probability of conviction respectively, where the effect for sexual crimes is significant at the 10% level. Education also negatively affects participation in other crimes for male twins, an effect which is significant at the 10% level.[27] Table A2 reveals that, for males, education can not only reduce participation in violent and total crimes, but in most other crime types as well.

9 Robustness Checks

Having detailed heterogeneous effects by parental education and across educational program, the results below not only investigate the robustness of the main specification but also investigate the validity of the identifying assumptions required for within twin fixed effects.

9.1 Using Incarceration as a Dependent Variable

Table 13 uses, as a dependent variable, whether an individual has spent time in jail resulting from an offense committed from 2001-2006. Results in the columns are divided by gender and not crime

[27] The three most common crimes in the definition of other crimes are false accusations, the sale of drugs (those that are not covered by the law on narcotics), and crimes against public authority.

type, where column (1) shows results for only male twin pairs and column (2) shows results for only female twin pairs. While being convicted of a crime corresponds to criminal activity, individuals who are ultimately incarcerated usually commit more severe crimes, and as incarceration is costly to society, it is worthwhile examining both convictions and incarcerations. As such, incarceration data is used to not only determine whether the negative effects seen using conviction data are robust to an alternative measure of criminal activity but also whether education can effectively reduce incarcerations. If the effects of education on crime are robust to changes in the crime measure used, this supports the argument that education truly reduces an individual's propensity to engage in crime.

Table 13: Within Twin Fixed Effect Estimates of Sample of Same Gender Twins In Same Childhood Environments using Incarceration

	(1)	(2)
	Male Twins	Female Twins
	Incarcerated	Incarcerated
Years of Education	-0.0035*	-0.0001
	(0.0014)	(0.0006)
Juvenile Crime 15	0.0517+	0.0000
	(0.0272)	(0.0001)
Juvenile Crime 16	0.0203	-0.0001
	(0.0195)	(0.0004)
Number Individuals	6038	5908
Twin Pairs	3019	2954

Standard errors reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

As before, years of education significantly lowers the probability of being incarcerated for male twin pairs, but not for female twin pairs. For male twin pairs, a ceteris paribus year of education lowers the probability of incarceration by 0.4 percentage points (22%). For female twin pairs, education appears to have no significant negative effects on the probability of incarceration.

As with estimation using convictions, having been convicted of a crime committed as a juvenile increases the probability of incarceration, by roughly 2-5 percentage points depending on the age an individual engaged in juvenile crime, but there still remain significant negative effects of education on incarceration. The results in Table 13 lead to the conclusion that education has a larger, in terms of percentage of mean reduction, impact on an individual's probability of incarceration than

on their probability of conviction, a finding which is also seen in other papers using Scandinavian data (Hjalmarsson et al. 2015). This is indicative that education not only decreases an individual's probability of committing crime, but also has additional effects on reducing the severity of crimes committed, as these crimes usually correspond to jail sentences.

9.2 Accounting for role of DZ Twins in Estimation

Following Holmlund et al. (2008), under certain assumptions and exploiting knowledge of the fraction of same gender DZ twins out of all same gender twins in the population (θ), the potential bias introduced by the presence of DZ twins in the estimation sample can be corrected for, even if zygosity of the individual twins is unknown. Making use of non-twin siblings (with both the same mother and father) to approximate the effect for DZ twins and under certain assumptions, the same gendered twin estimate, a composite of both MZ and DZ twins, can be decomposed into an average of the effect for MZ twins and the effect of DZ twins. This requires assuming that the education and crime equation, equation (1), is the same for same gender DZ twins and non-twin siblings, that DZ twins and non-twin siblings are treated the same, and there is no measurement error in schooling.[28]

In order to make these similarity assumptions more believable and, again, following Holmlund et al. (2008), only same gendered siblings who are born close to each other, defined here as within 2.5 years, are used.[29] Some suggestive evidence of the extent to which these sibling populations are similar is presented in Björklund and Jäntti (2012), who examine sibling correlations for years of education, earnings, IQ, and non-cognitive skills. The authors explore a wide variety of siblings, including twin siblings, and show that DZ twins and closely spaced siblings (which they define as those with less than 4 years of age difference) have similar sibling correlations in education. While the DZ correlations are more imprecise, they also show that the sibling correlations for closely spaced siblings are more similar to DZ twins compared to non-closely spaced siblings (those with 4 or more years of age differences). As siblings with age differences of exactly 2.5 years are used in this paper, the similarity of same gender DZ twins and closely spaced siblings is, presumably, even more believable.

[28] As the quality of Register data is regarded as high, particularly with education, it seems realistic to assume measurement error in schooling is unproblematic.

[29] Closely spaced siblings are those born within exactly 912 days of each other. Results are robust to altering this somewhat arbitrary cutoff.

Under the above assumptions, the MZ twin estimate is calculated as:

$$\hat{\beta}_{MZ} = \frac{1}{\theta}\hat{\beta}_{MZDZ} - \hat{\beta}_{CS}$$

where $\hat{\beta}_{MZDZ}$ is the estimate for both same gendered MZ and DZ twins which has been estimated previously and $\hat{\beta}_{CS}$ is the estimate for close siblings which represents the estimate for same gendered DZ twins had zygosity been observable. θ is taken as 0.509 from Skytthe et al. (2011), who report among twins born from 1968-1982, 2,788 are MZ and 2,887 are same gender DZ. Standard errors are estimated as in Conley et al. (2006); Conley and Strully (2012), and require the additional assumption that the covariance between the same gender twin estimates and the close sibling estimates is zero, $cov(\beta_{MZDZ}, \beta_{CS}) = 0$. This is a fairly strong assumption, which is equivalent to assuming that the error terms for same gendered twins and closely spaced siblings are unrelated.[30] While this assumption is necessary in order to calculate any standard errors, the standard errors resulting from this method should be interpreted with some degree of caution.

Results presented in Table 14 show that if it were possible to identify zygosity, the estimated effects of education on crime would be of a similar magnitude for MZ twins alone than in the same gender twin sample. For males, the effects of education on total convictions for all same gendered, same household males and for those with low parental education remain negative, sizable, and statistically significant. For estimation using incarceration as a dependent variable, while the estimated effects of education on crime become insignificant, the imputed MZ coefficient shows an effect which is similar in terms of magnitude, only less precisely estimated. Table 14 suggests that if zygosity were observable, a MZ twin sample would produce estimated effects of education on crime in a similar range as the effects from the same gendered twin sample used throughout the paper.

9.3 Examining Differences in GPA

As discussed in Section 6, underlying differences in the ability levels of twins will violate the identifying assumptions required for within twin estimation. Sandewall et al. (2014) investigate the possibility of this using IQ scores and birth weight. While both IQ scores and birth weight are unavailable for the sample of twins used in this paper, for a subsample of twins, average GPA

[30] To the extent there could be positive covariance between same gendered twins and closely spaced siblings estimates, if these are biased in the same direction, this would lead to standard errors which are too large. For a further discussion of this method and its shortcomings, see Conley et al. (2006).

Table 14: Within Twin Fixed Effect Estimates of Sample of Same Gender Male Twins In Same Childhood Environments Correcting for Dizygotic Twins

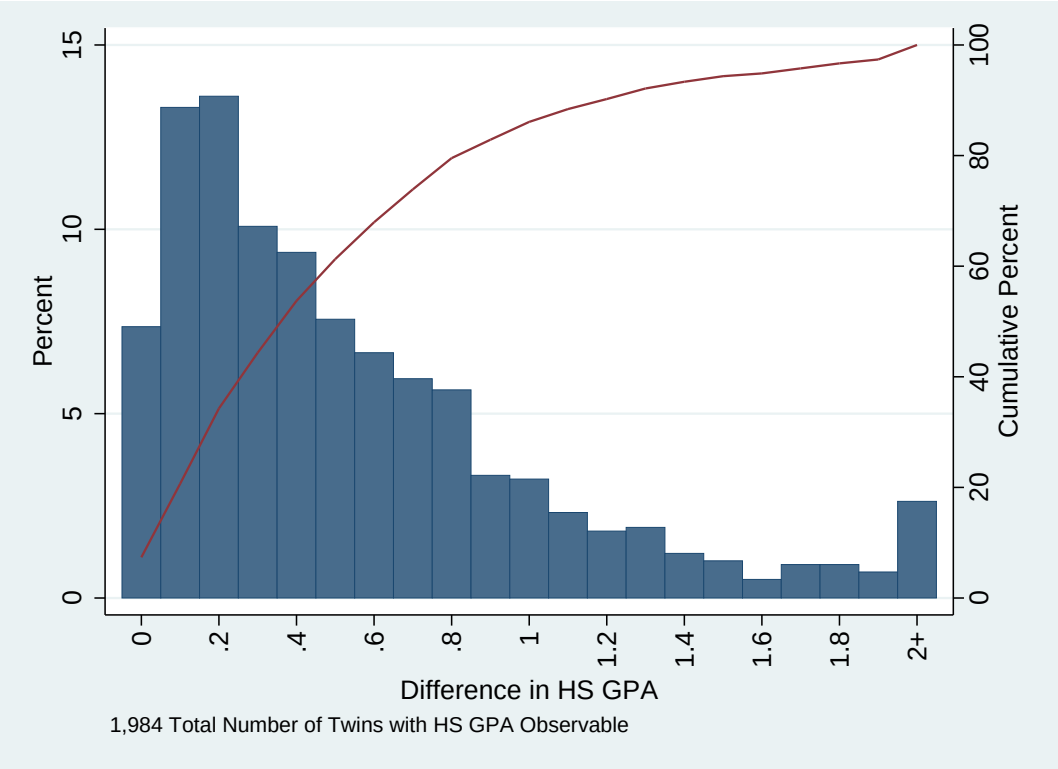
	(1) Twins - $\hat{\beta}_{MZDZ}$	(2) Close Siblings - $\hat{\beta}_{CS}$	(3) Imputed MZ Twins - $\hat{\beta}_{MZ}$
Same Gendered, Same Household Male Twins			
	Total	Total	Total
Years of Education	-0.0129**	-0.0137**	-0.0116+
	(0.0031)	(0.0017)	(0.0064)
	Incarcerated	Incarcerated	Incarcerated
Years of Education	-0.0035*	-0.0035**	-0.0035
	(0.0014)	(0.0008)	(0.0029)
Number Individuals	6124	17794	
Twin/Sibling Pairs	3062	8897	
At Least One Low Educated Parent - Male Twins			
	Total	Total	Total
Years of Education	-0.0199**	-0.0171**	-0.0220*
	(0.0040)	(0.0024)	(0.0085)
Number Individuals	3118	8970	
Twin/Sibling Pairs	1559	4485	

Standard errors clustered at twin/sibling level reported in parentheses. Standard errors in column (3) are calculated as in Conley et al. (2006), as the square root of the variance of the $\hat{\beta}_{MZ}$ where $Var(\hat{\beta}_{MZ}) = Var(\hat{\beta}_{MZDZ})(\frac{1}{(1-\theta)})^2 + Var(\hat{\beta}_{CS})(\frac{\theta}{(1-\theta)})^2$. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively. Column (1) presents results from same gendered twins (MZ & DZ). Column (2) presents results from closely spaced siblings.

during high school is observable. At the time the twins examined in this study attended high school, grades in Denmark were on a 13 point scale, where an individual could receive either a 13, 11, 10, 9, 8, 7, 6, 5, 03, or 00, where grades 5 or below were considered failing. At the time, 11 and above corresponded to an excellent grade, 10 corresponded to very good, 9 and 8 corresponded to good, while 7 was satisfactory and 6 was passing.[31]

Figure 2 shows the differences in average high school GPA, for all same gendered, same household twin pairs where data is available. It is expected that the closer twins' academic achievement is, the more similar they are, and the more likely the identifying assumptions required for within twin estimation will hold. More than 50% of the twins have average GPA differences of 0.4 or less, and on the whole, the majority of twins have very similar GPA. Consistent with this, 86% of all twins have GPA differences of 1 point or less. While 1 point may seem like a large difference, on the 13 point scale, a change of one point is often not enough to change the quality associated with the GPA.

Figure 2: Distribution of High School GPA Differences



[31] Denmark now uses a 7 point scale, which is a similar concept to the 13 point scale.

Ideally, GPA would be included as a control variable throughout the estimation, as this would provide more direct insight into how the estimated effects of education on crime differ when accounting for some measure of ability differences between twins. However, since only high school GPA is available for a subsample of twins, there is insufficient variation in either education defined as a qualification or the length of education for twin pairs where GPA is observable. While the lack of major differences in academic achievement between twins who both completed high school shown in Figure 2 is supportive of the underlying similarity of twins, this is as much as the role of underlying differences between twins can be analyzed in this paper.

9.4 Excluding Twins with Large Differences in Education

Previous twin studies (Amin 2011; Lundborg 2013) have emphasized the importance of outliers, where outlying twin observations have been shown to affect results, sometimes significantly. While a binary dependent variable may limit the potential influence of outlying twins as there can only be two outcomes, the results presented could potentially be driven by twins with large differences in education. This could be true especially given the rarity of criminal convictions, where despite a large sample of twins, a few individuals could have a large impact on the estimation. Additionally, twins who have large educational differences may also be less similar than twins with closer education. Excluding twins with large differences in education serves to not only limit the potential influence of outlying twins but also imposes a sample of twins who are, arguably, most similar.

Table 15 excludes twin pairs who have differences in education of 4 years or more. The top panel presents results on the baseline sample of twins, while the bottom panel presents results for those with at least one low educated parent. Results for total and violent crime remain largely unaffected for the baseline male sample, while the point estimate on property crime remains unchanged but loses the marginal significance seen in Table 8. Results for males from low educated families are very similar to results presented in Table 10. For females, as before, no significant effects of education on crime are seen. Overall, the results presented in Table 15 reveal that twins with large differences in education are not driving the results presented previously.

9.5 Investigating Reverse Causality

Given the importance of juvenile crime in the previous results it is natural, in addition to simply including juvenile crime as a proxy for unobservables, to analyze the reverse causality between education and crime in further detail by directly estimating the effects of participation in juvenile

Table 15: Within Twin Fixed Effect Estimates of Sample of Same Gender Twins In Same Childhood Environments Excluding Twins with Large Education Differences

	(1)	(2)	(3)	(4)	(5)	(6)
	Male Twins			Female Twins		
Entire Sample						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0138**	-0.0040	-0.0052*	0.0018	0.0011	-0.0002
	(0.0049)	(0.0030)	(0.0021)	(0.0019)	(0.0012)	(0.0010)
Number Individuals	5236	5236	5236	5416	5416	5416
Twin Pairs	2618	2618	2618	2708	2708	2708
At Least One Low Educated Parent Sample						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0192**	-0.0076^{+}	-0.0059*	0.0016	0.0005	-0.0011
	(0.0072)	(0.0044)	(0.0028)	(0.0023)	(0.0015)	(0.0011)
Number Individuals	2636	2636	2636	2700	2700	2700
Twin Pairs	1318	1318	1318	1350	1350	1350

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively. Low educated parent is either a low educated father, low educated mother, or both low educated mother and father. Not reported are controls for juvenile crime. Excludes twins with 4 or more years differences in education.

crime on educational attainment. In addition to the findings of Webbink et al. (2013) already mentioned, Hjalmarsson (2008) analyzes the effects of juvenile crime on education in the US, finding significant negative effects of arrest and incarceration before age 17 on high school graduation, but virtually no effect of being charged with a crime as a juvenile on high school graduation. Additionally, Aizer and Doyle (2015) find, using the randomization of case assignment to judges, that juvenile incarceration significantly lowers the probability of graduating high school and that these effects are strongest at the ages of 15 and 16 due to the fact that incarceration will likely end education for these individuals. While Hjalmarsson (2008) uses criminal activity that occurred when aged 16 or younger, Webbink et al. (2013) examine how the age an individual was first arrested impacts educational attainment, finding that arrests at younger ages (13, 14, and 15) are more detrimental to education than arrests at later ages (16 and 17). As criminal behavior prior to the age of 15 is unobservable in Danish data, and criminal behavior after the age of 16 is outside the compulsory schooling window, the estimation below analyzes the effects of a conviction for a crime committed at the age of 15 or 16 on educational attainment.

Three separate measures of educational attainment are used in Tables A3 & A4 in Appendix E

to estimate the reverse causality between education and crime: whether an individual was educated past compulsory schooling, whether an individual completed high school, and how many years of education an individual received. These measures are reported in columns (1), (2), and (3) respectively. Using only male twin pairs, the estimated effects of juvenile crime are generally negative, but mostly insignificant, while for females, large and negative estimated effects of juvenile crime at the age of 16 on education are seen for all education measures.

While the effects of juvenile crime on education are large, they are much smaller than what has previously been found. Webbink et al. (2013) find approximately a 20 percentage point reduction in the probability of being a senior in high school due to juvenile crime,[32] Hjalmarsson (2008) finds an 11 percentage point reduction in the probability of graduating high school, and Aizer and Doyle (2015) find juvenile incarceration leads to a 13 percentage point reduction in the probability of high school graduation. For the purposes of comparison with these existing results, Table A4 in Appendix E defines juvenile crime as being convicted of any crime as a juvenile committed at any point during the ages of 15 and 16, ignoring any effects that juvenile crime age have on educational attainment. As before, the effects of juvenile crime on education are not so large that they dominate the estimated effects of education on crime. For example, Webbink et al. (2013) find that an arrest before the age of 18 reduces education for all twin pairs by 0.7 years,[33] much larger than the results found here.

The results presented in Tables A3 and A4 reveal that crime committed as a juvenile reduces educational attainment and is an important component in the relationship between education and crime. However, the impact of the juvenile crime on education is less than has previously been found, and once accounting for the reverse causality between education and crime, educational attainment still significantly reduces participation in crime as an adult for males. Interestingly, there appear to be larger effects of juvenile crime on education for women. Given that it is less common for women to be involved in crime compared to men, it is possible that engaging in such a rare event as juvenile crime is very detrimental to the educational attainment of women.

[32]For identical twins, this effect is 12 percentage points and insignificant.

[33]For identical twins, this effect is 0.02 years, but when correcting for missing values, this number climbs to 0.32. Both are insignificant.

10 Conclusions

This paper provides new evidence on the heterogeneous effects of education on crime, overcoming the endogeneity problem that exists between education and crime by making use of twin data contained in detailed Danish Register Data. The paper first confirms a causal link between education and crime in Denmark, finding significant negative effects of education on an individual's probability of conviction for total and violent crimes, with marginally significant effects for property crimes. In the preferred baseline specification, an additional year of education significantly reduces the probability of conviction as an adult for any crime by 14%, for a property crime by 12%, and for a violent crime by 19% for males. The effects of education on adult crime are largely unchanged controlling for whether an individual was convicted of a crime as a juvenile.

Heterogeneous effects are then examined. Family factors, measured by parental education, are found to be important for the effects of education on crime for children, with large crime reduction seen for children of low educated families and virtually no effects on crime seen for children with both parents highly educated. Environmental factors, measured as growing up in a high or low crime neighborhood, have much less impact on the effects of education on crime. Examining how the effects of education on crime differ across crime types reveals education can also reduce traffic related alcohol, sexual, and firearms crimes. Consistent with past literature, the completion of high school is found to reduce criminal propensity for all crime types for male twins, while for female twins, the completion of high school is found to significantly increase the probability of conviction for total and property crimes. Contrary to expectations, the completion of vocational training after lower secondary education is found to have no crime reducing effects for males, while, consistent with expectations, additional university education past high school is found to have no significant crime reducing effects both for males and females.

Due to the potential bias introduced by the presence of DZ twins, and following Conley et al. (2006) and Holmlund et al. (2008), closely spaced siblings are used to account for the presence of these DZ twins. Imputed effects for identical twins remain similar, in terms of magnitude, to the effects of education on crime found and, in most cases, statistically significant. Additionally, the environment twins were raised in is explored in great detail, ensuring the environmental component of twins' development is the same. Education is found to also reduce an individual's probability of incarceration, eliminating concerns that the effects of education on crime seen are not due to

reduced criminality but perhaps something particular in the use of conviction data. Similar effects of education on crime are found when excluding twins with large differences in education, limiting concerns that the findings are driven by a few potentially outlying twins. Direct estimation of the reverse causality between education and crime reveals that while juvenile crime does lead to lower educational attainment as a youth, the effects are much smaller than has previously been found, and are not large enough to explain the entire relationship seen between education and adult crime.

While on the whole, the estimated effects of education on crime using Danish twin data are in line with previous research, this paper improves on existing studies, which predominantly examine changes in compulsory schooling laws, by estimating heterogeneous effects of education on crime which are representative of the entire population while, at the same time, netting out inherent common factors between twins which is crucial to identifying causal effects of education on crime. One obvious concern is that the twins analyzed are unique and not representative of the general population. However, OLS results for twins are comparable not only to the non-twin sibling population, but also to the entire non-twin Danish population as well.[34] This is indicative that the effects of education on crime found are representative at least of the Danish population.

From a policy perspective, the findings of this paper reveal not only the overall importance of education in terms of crime reduction, particularly for the completion of high school, but also the importance of family background in the crime reducing capabilities of education. For children of low educated parents, it appears to be extremely beneficial to motivate additional schooling as for these individuals, levels of education obtained can be less than is socially optimal. The high crime reduction from children of low educated families also highlights the importance of the intergenerational transmission of education, as while individuals from low educated families experience large crime reduction from additional education, they may be more likely follow in the educational footsteps of their parents and fail to benefit from the crime reducing capabilities of education. This may be of concern even in Nordic countries, where most intergenerational estimates find that an additional year of parental education increases the education of their children by around 0.1 years (Holmlund et al. 2011). Encouraging individuals from low educated families to remain in education, as well as to avoid engaging in juvenile crime, could offer an effective way to not only reduce longer term educational inequality but also reduce criminality on the whole. The

[34] Results available on request.

heterogeneous effects outlined reveal the importance of accounting for differences across individuals when estimating the crime reducing capabilities of education, and further research is required to identify precise mechanisms behind these differences.

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Appendices

A Data Sources and Explanation

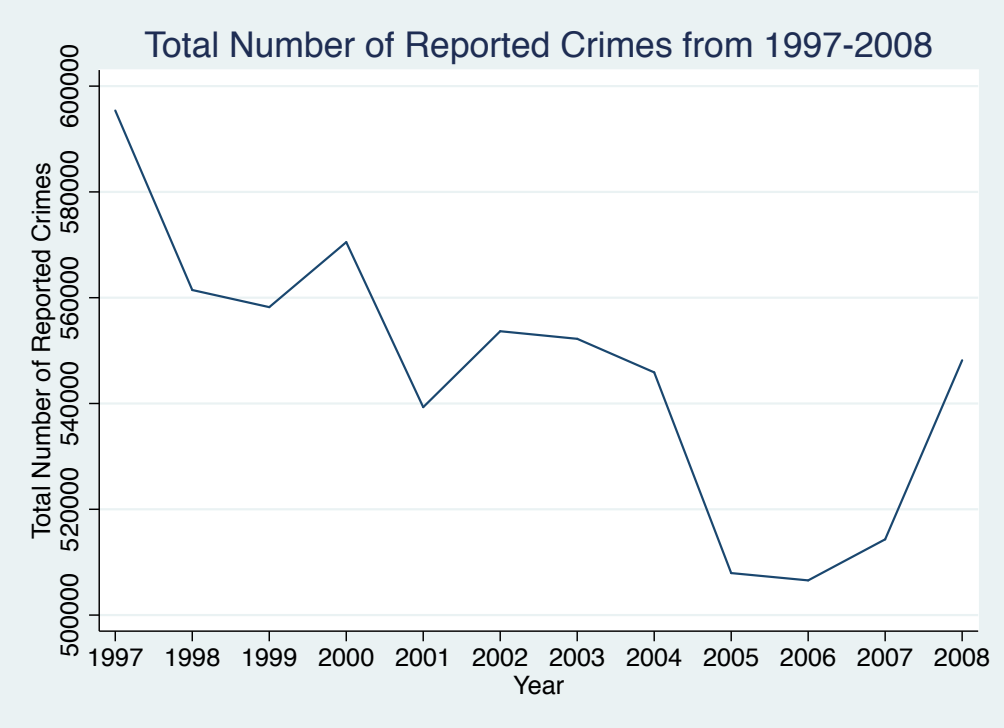
Danish Register Data is composed of various databases through administrative sources, which are interlinked by a consistent and unique personal identifier number. This enables Danish residents to be tracked over time and the various separate databases. There are separate databases for employment data, individual data, family data, education data, income data, and crime data. The crime data used throughout the paper is conviction data which is combined with data on charges to make use of the exact date the offense was committed on.

In Denmark, crimes against penal code are: sexual crimes (comprised of incest, rape, heterosexual offenses, homosexual offenses, public indecency, and prostitution), property crimes (comprised of forgery, check forgery, arson, various degrees of burglary, theft from vehicles, theft of various vehicles, shoplifting, larceny, embezzlement, fraud, blackmail, robbery, handling stolen goods, tax evasion, malicious damage to property, and offenses against property), violent crimes (comprised of violent against public authorities, disturbance of public order, homicide, attempted homicide, varying degrees of assault, intentional bodily harm, offenses against life, and threats), as well as some offenses which are categorized as unknown and other. There are special laws against offenses such as narcotics, firearms, and tax crimes.

B Denmark from 2000-2006

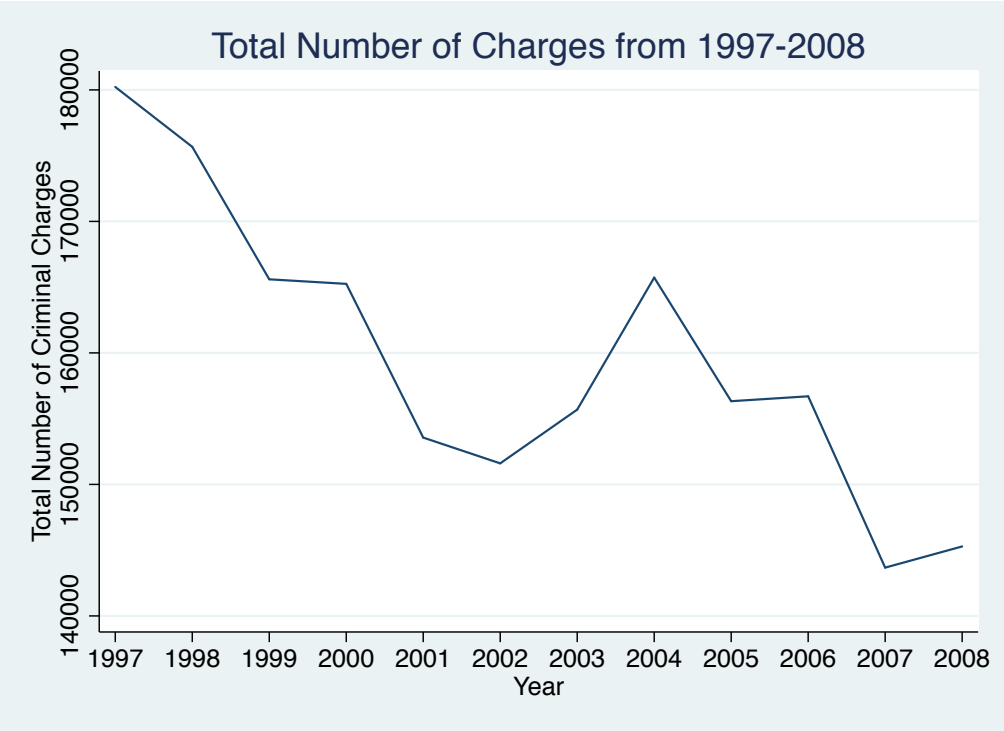
While the time period from 2000-2006 is not a perfectly stable, it is relatively stable both in terms of unemployment and crime. There are no large or sudden increases or decreases during the period for the crime data, and while unemployment does fluctuate from 2002-2004, by 2006, the unemployment rate returns to its 2001 level. Results using other years, while not perfectly identical, give similar coefficients. First two figures are author's own calculation, aggregating individual level data to the national level, while last figure uses labor force survey data available from `http://statistikbanken.dk`.

Figure A1: Total Reported Crimes from 1997-2008



Source: Author's calculation using data provided by Statistics Denmark.

Figure A2: Total Charged Crimes from 1997-2008



Source: Author's calculation using data provided by Statistics Denmark.

Figure A3: Quarterly Unemployment Rate from Labour Force Survey from 1997-2008

Source: `http://statistikbanken.dk`, "AKU123: Seasonaly adjusted labour force status in percentage by employment status".

C Within Twin Estimates by Detailed Parental Education

Table A1: Within Twin Fixed Effect Estimates of Sample of Same Gender Twins In Same Childhood Environments By Educational Attainment of Parents

	(1)	(2)	(3)	(4)	(5)	(6)
	Male Twins			Female Twins		
Both Parents Low Educated						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0243**	-0.0022	-0.0041	-0.0039	-0.0021	-0.0030
	(0.0063)	(0.0041)	(0.0032)	(0.0034)	(0.0021)	(0.0022)
Number Individuals	1322	1322	1322	1304	1304	1304
Twin Pairs	661	661	661	652	652	652
Low Educated Mother, High Educated Father						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0149*	-0.0049	-0.0005	-0.0015	-0.0031	0.0000
	(0.0065)	(0.0034)	(0.0025)	(0.0040)	(0.0038)	(.)
Number Individuals	1188	1188	1188	1072	1072	1072
Twin Pairs	594	594	594	536	536	536
High Educated Mother, Low Educated Father						
	Total	Property	Violent	Total	Property	Violent
Years of Education	-0.0216*	-0.0090	-0.0118*	0.0000	-0.0000	-0.0000
	(0.0083)	(0.0056)	(0.0058)	(.)	(0.0000)	(0.0000)
Number Individuals	570	570	570	548	548	548
Twin Pairs	285	285	285	274	274	274

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively. Not reported are controls for juvenile crime.

D Detailed Crime Types

Results for all available crime categories are reported in Table A2.

Table A2: Within Twin Fixed Effect Estimates of Sample of Same Gender Male Twins In Same Childhood Environments for Detailed Crime Types

	(1) Total	(2) Property	(3) Violent	(4) Sexual	(5) Other	(6) Firearms	(7) Traffic Alcohol	(8) Narcotics	(9) Special Legislation
Years of Education	-0.0129**	-0.0033+	-0.0035*	-0.0012+	-0.0020+	-0.0020*	-0.0066**	-0.0007	0.0019
	(0.0031)	(0.0019)	(0.0016)	(0.0006)	(0.0012)	(0.0010)	(0.0023)	(0.0012)	(0.0014)
Juvenile Crime 15	0.0999+	0.0421	0.0965**	0.0178	-0.0275	0.0168	0.0415	-0.0194	0.0186
	(0.0517)	(0.0361)	(0.0331)	(0.0123)	(0.0195)	(0.0125)	(0.0339)	(0.0306)	(0.0215)
Juvenile Crime 16	0.1071*	0.0431	0.0041	-0.0082	0.0155	0.0141	0.0426	0.0453	-0.0152
	(0.0447)	(0.0353)	(0.0227)	(0.0075)	(0.0149)	(0.0211)	(0.0312)	(0.0276)	(0.0235)
Number Individuals	6038	6038	6038	6038	6038	6038	6038	6038	6038
Twin Pairs	3019	3019	3019	3019	3019	3019	3019	3019	3019

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

E Investigating Reverse Causality

Results showing the estimated effect of juvenile crime on educational attainment are reported in Table A3.

Table A3: Within Twin Fixed Effect Estimates of the Effect of Juvenile Crime on Educational Attainment of Sample of Same Gender Twins In Same Childhood Environments

	(1)	(2)	(3)
Estimates of Sample of Same Gendered, Same Household Male Twins			
	Educ. Past Compulsory	Completion HS	Years of Educ.
Juvenile Crime 15	-0.0510	-0.0253	-0.1899
	(0.0448)	(0.0458)	(0.2027)
Juvenile Crime 16	-0.0582	-0.0366	-0.1361
	(0.0445)	(0.0454)	(0.1968)
R^2	0.019	0.017	0.018
Number Individuals	6038	6038	6038
Twin Pairs	3019	3019	3019
Estimates of Sample of Same Gendered, Same Household Female Twins			
	Educ. Past Compulsory	Completion HS	Years of Educ.
Juvenile Crime 15	-0.1639	0.0104	-0.0581
	(0.1120)	(0.1028)	(0.4374)
Juvenile Crime 16	-0.0656	-0.2504**	-0.6051*
	(0.0842)	(0.0963)	(0.2875)
R^2	0.005	0.005	0.004
Number Individuals	5908	5908	5908
Twin Pairs	2954	2954	2954

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

Table A4: Within Twin Fixed Effect Estimates of the Effect of Juvenile Crime on Educational Attainment of Sample of Same Gender Twins In Same Childhood Environments

	(1)	(2)	(3)
Estimates of Sample of Same Gendered, Same Household Male Twins			
	Educ. Past Compulsory	Completion HS	Years of Educ.
Any Juvenile Crime 15 or 16	-0.0558^{+}	-0.0305	-0.1523
	(0.0331)	(0.0358)	(0.1509)
R^2	0.016	0.016	0.017
Number Individuals	6038	6038	6038
Twin Pairs	3019	3019	3019
Estimates of Sample of Same Gendered, Same Household Female Twins			
	Educ. Past Compulsory	Completion HS	Years of Educ.
Any Juvenile Crime 15 or 16	-0.1200^{+}	-0.1400^{+}	-0.3600
	(0.0672)	(0.0749)	(0.2650)
R^2	0.006	0.008	0.006
Number Individuals	5908	5908	5908
Twin Pairs	2954	2954	2954

Standard errors clustered at twin level reported in parentheses. **, *, and + correspond to significance at the 1%, 5%, and 10% levels respectively.

Chapter 2 - Job Displacement and Crime

Job Displacement and Crime*

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Abstract

We use a detailed employer-employee data set matched with detailed crime information (timing of crime, convictions, crime type) to estimate the impact of job loss on an individual's probability to commit crime. We focus on job losses due to displacement, i.e. job losses in firms losing a substantial share of their workers, for workers with at least three years of tenure. Displaced workers are more likely to commit offenses leading to conviction for total crimes and property crimes in the years following displacement. We find no evidence that displaced workers' propensity to commit crime is higher than non-displaced workers before the displacement event, but it is significantly higher afterwards. We find that the impacts of displacement on crime depend on the education, household factors, and post-displacement employment outcomes of displaced individuals.

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1 Introduction

In the last decade, Europe has experienced a "reversal of misfortunes": as crime rates have reached historic lows in the U.S., Europe on the contrary currently experiences historically high crime rates (Buonanno et al. 2011). Crime, arrests, and convictions generate large social costs, and the determinants of crime have been the focus of existing literature (Benson & Zimmerman 2010, Freeman 1999).

Descriptive statistics suggest that in the United States, the peak of crime of the early 1990s approximately matches to the peak of the U.S. unemployment rate in 1994, and a positive relationship between unemployment and crime also exists for Denmark. Gould et al. (2002) uses trade instruments to estimate that wage trends explain more than 50% of the variation in property crime in the U.S. over their sample period (1979–1997), and that the decline in the unemployment rate of non-college-educated men after 1993 contributed to the decline in crime rates. Lin (2008) uses union membership rates and a state's industrial structure to estimate that a one percentage point increase in the unemployment rate leads to a 4 percent increase in property crime.

Prior literature has indeed uncovered convincing causal estimates of the impact of unemployment on crime in a number of countries including Sweden (Öster & Agell 2007) and France (Fougère et al. 2009). However, prior literature relies on state or municipality level data. In particular, it is hard to pinpoint exactly what individual-level mechanism generates the state- or municipality-level relationship between unemployment and crime rates.

This paper estimates the impact of mass layoffs on the individual probability of committing a criminal offense in Denmark. Focusing on Denmark allows us to use a detailed employer-employee-unemployment matched individual-level data set with crime information taken from police records. The data set includes information on convictions, broken down by crime type—property crime, violent crime, etc—as well as individual earnings, weeks of unemployment, age, marital status, family information, and the area of residence of the individual. We focus on displaced workers, i.e. male individuals that have been in employment for at least 3 years in the same firm and move into unemployment when the firm experiences a mass layoff event, i.e. loses a substantial fraction of its employees compared to peak employment in a five year window prior to the time period of analysis. If events that drive the firm's business cycle are arguably independent of the individual dynamics of the employee's criminal offenses, focusing on displaced workers during mass layoff

events is likely leading to more causal estimates of the impact of job loss on the probability to commit crime.

This paper finds statistically and economically significant impacts of displacement on the probability of an offense leading to a conviction for total and property crimes. We find that although displaced workers are no more likely to commit crime at any point prior to displacement, displaced workers are substantially more likely to commit crime after displacement. Results are robust to the inclusion of individual fixed effects, controls for family factors, and municipality fixed effects. Results are also robust to alternative definitions for mass layoffs: either (i) changing the threshold (30 or 40%) decline in firm size below which a firm is labeled as experiencing a mass layoff, (ii) using mean firm employment as the reference point for the firm-size decline instead of peak employment (Jacobson et al. 1993), or (iii) identifying mass layoffs as large deviations from a firm-specific trend in employment, estimated using prior firm size changes in 1985-1989. Results are also robust to focusing on larger-sized firms, for which a given percentage decline in size is less likely to be driven by temporary changes in firm size.

We examine a variety of individual and family factors to assess the potential mechanisms behind why displacement leads to increases in crime. We assess whether there exists an intergenerational impact of father's job displacement on the criminality of their children. We see a small impact of father's displacement on son's criminality in the short-run which is, at best, marginally different from the criminality of sons of non-displaced fathers. We find effects of displacement on crime are concentrated for individuals with low education—those with less than high school education and, to a lesser extent, those with vocational training education. Those displaced with education to the university level or higher are no more likely to be convicted than non-displaced individuals following displacement. We show that those living without another adult, either those unmarried or living in a single adult household, are more likely to engage in crime than displaced workers who are living with another adult, while displaced individuals are likely to engage in crime irrespective of age or whether they have children. Displaced workers experience substantial short-run and permanent earnings losses and spend longer in unemployment after the displacement event. While earnings losses and unemployment spells may explain part of the impact of displacement on crime, results suggest that our estimates are an effect of displacement on crime over and above what is explained by earnings losses but that time spent in unemployment can explain substantially more of the impact of displacement on crime. Given the generous unemployment system in Denmark

which may in part mitigate the role of earnings losses, this points to the fact that idleness may, at the very least, explain a portion of the impact of job displacement on crime.

This paper makes contributions to two different literatures. First, the paper provides individual-level estimates of the impact of job losses on crime using detailed employer-employee data. Previous literature (Gould et al. 2002, Öster & Agell 2007, Fougère et al. 2009) used regional-level data (such as county-level or state-level data) to estimate such impacts. Although the literature uses credible instrumental variable estimates, individual-level evidence of a mechanism relating unemployment and crime remains to be established. In particular, no U.S. data set matches individuals with their employers and includes crime data. Focusing on Denmark allows such analysis.

Focusing on individual level data for Denmark provides estimates of a different relevance as compared to U.S. aggregate estimates. Individual-level estimates document the individual-level mechanism that may explain the aggregate level results: in particular a discrepancy between individual-level estimates and area-level estimates suggests either that regional level estimates are confounded or that there are social interactions in crime within states or municipalities: as unemployment rises, both individual incentives and social incentives to commit crime increase (Glaeser et al. 1996), and area-level estimates may be larger than individual level estimates. On the other hand, Denmark differs from the U.S. in significant respects. First, unemployment benefits and social benefits are significantly more generous in Denmark than in the U.S. Second, this paper focuses on the impact of job displacement on offenses leading to a conviction. Per capita incarceration rates are significantly lower in Denmark than in the U.S., and given the vast institutional differences in crime between the two countries, results presented in this paper are arguably a lower bound compared to what would be expected if similar data were available in the U.S.

The paper also makes a contribution to the literature on the wider impacts of job displacement, which has documented the impact of job displacement on earnings (Jacobson et al. 1993, Couch & Placzek 2010), health (Eliason & Storrie 2009, Sullivan & von Wachter 2009, Browning & Heinesen 2012, Black et al. 2012), and mobility (Huttunen et al. 2015). Jacobson et al. (1993) documented the short-run and long-run earnings losses of displaced workers using U.S. Social Security data. Sullivan & von Wachter (2009) present evidence that job displacement leads to higher mortality rates. In this paper, we present results defining displacement in a similar way as in Jacobson et al. (1993) and Sullivan & von Wachter (2009), but we also use declines relative to a firm-specific trend in employment to identify large and sudden changes in firm size.

Results should be useful to policymakers: by establishing a link between individual-level displacement and crime, a job separation is likely to impact other parties than the firm and the employee. Job displacement may thus lead to increased policing costs, and overall negative welfare externalities for the municipality. In Blanchard & Tirole (2008) framework, neither employers nor workers may fully internalize the social cost of the job separations, which justifies either additional taxation of employers and/or active labor market policies that incentivize or help unemployed individuals to go back to formal employment.

The paper proceeds as follows. Section 2 presents the rich Danish employer-employee data set. Section 3 presents the identification challenges and the paper's identification strategy. Section 4 describes our empirical findings for convictions by crime types. Section 5 examines the possibility of intergenerational impacts of displacement on children's probability of engaging in crime. Section 6 provides estimates of the impact of displacement by education, while Section 7 examines heterogeneity within our baseline results and identifies which individuals are more likely to be affected by displacement in terms of increased criminality. Section 8 presents results controlling for potential confounding characteristics and estimates what share of the effect of displacement on crime is mediated by the effect of displacement on earnings and unemployment, while Section 9 tests whether results are robust to alternative definitions for mass layoff and displacement. Section 10 concludes.

2 Data Set

To analyze the impact of job displacement on crime, we utilize detailed employer and employee data contained in Danish Register Data made available by Statistics Denmark. Danish Register Data is a database of every individual residing in Denmark from 1980-present which is collected from various governmental and administrative sources. We follow individuals over time and across different data sources via an anonymous personal identification number derived from the central personal register (CPR, Det Centrale Personregister), and are able to match individuals to their employer using a unique firm identification number. We focus solely on males as males are overwhelmingly those who commit crime. We construct an individual level panel of every male residing in Denmark from 1985-2000 by combining five different data registers.

First, the Population Registers include demographic factors such as age, gender, municipality

of residence, and immigrant and marital status. Second, the Danish Student Register contains education data such as an individual's exact educational qualification and educational institution as well as information of any ongoing schooling. Third, the employer-employee data comes from the Integrated Database for Labor Market Research (IDA, Integrerede Database for Arbejdsmarkedsforskning) and contains information on an individual's employment as well as the universe of firms in Denmark in a given year. The employee data set provides information such as an individual's employment status (recorded at the end of November), the number of weeks in the year an individual was unemployed, information on part time or full time status, salary earned in the job , and a workplace (plant) identification number as well as a firm identification number. The employer data contains variables such as the number of employees in a workplace and the number of workplaces in a firm. We use this information to construct a firm level dataset. We consider only an individual's primary job, according to a criteria set by Statistics Denmark which follows the definitions of the International Labour Organization (ILO).[1] All of the employee and employer data contained in IDA is observed annually, as in the French (Abowd et al. 1999) and Pennsylvania (Sullivan & von Wachter 2009) employer-employee data sets.

Fourth, welfare and unemployment insurance payments received are observed at weekly frequency in the Central Register of Labour Market Statistics (CRAM, Det Centrale Register for Arbejdsmarkedsstatistik). This paper's data set thus includes annual measures of an individual's unemployment status by pointing out in which week of the year the individual goes from receiving no benefits to receiving some form of benefits.[2] As we match individuals to their firm, we know exactly when employees transition in and out of employment with that specific firm.

Finally, crime data comes directly from the Central Police Register, and is available for charges (individuals charged by the police with a crime), convictions, as well as incarcerations. After a crime is reported, if the police suspect someone of committing this crime, this individual is formally charged with the crime. The criminal record then includes the date of the offense and the date charges were filed. After this, we observe whether and when the individual was tried for the crime, and the trial's conviction outcome. The outcome can be either incarceration, a suspended sentence, a fine, a settlement, no charge/warning, or another, less frequent decision

[1] See www.dst.dk/kvalitetsdeklaration/848 for an explanation.

[2] In what follows, we use the terms receiving unemployment benefits and weeks of unemployment interchangeably. This could be problematic in that we would misclassify someone as non-displaced if they were displaced, but did not claim any benefits. Past studies have found this to be an unimportant factor, and this is particularly the case for us, as the high tenure individuals we identify are all eligible to receive social assistance or unemployment insurance (if they are a member) following job loss.

(for example a youth program or military punishment). While all of these are possible conviction outcomes, the overwhelming majority of convictions in Denmark result in either probation, a fine, or incarceration. In the estimation that follows we examine all types of convictions.

A unique police case number links the criminal offense across the crime registers and the personal identification number links crimes to the specific offender's in all other registers. The crime register also records the precise day charges were filed, convictions recorded, and incarceration started, which can then be compared to the week unemployment benefits started and the individual's employment spell ended. Across all three crime databases, we also observe a detailed crime code, corresponding to the Danish classification system of offenses. Crimes are comprised of: sexual, violent, property, alcohol related traffic, narcotics, firearms, tax, unknown, and other crimes, as well as crimes against special legislation.[3] Within these large categories, the specific kind of offense is also recorded, i.e. burglary within property crimes, assault within violent crimes.

Table 1 provides summary statistics for the individual level panel of males born from 1945-1960 residing in Denmark from 1985-2000 for our five different data sources.

3 Empirical Strategy

A number of identification challenges make the identification of the effect of the loss of employment on crime difficult. In particular, individuals typically do not leave their firm for exogenous reasons. Individuals may choose to leave the labor force altogether, or choose to leave their current job and start an unemployment spell to look for a better match. Employers may also choose to separate from employees who are more likely to commit crime. If individuals leave their firm because of opportunities in the informal sector, OLS results will suggest a positive correlation between changes in employment status and the probability of crime. Such correlation will not likely reflect a causal relationship between employment loss and crime; indeed, current unobservables would drive both the probability of job loss and the probability of committing crime.

The literature has identified a firm-level cause of job losses that should be arguably independent of the worker's individual dynamics. In this paper, we focus on high-tenure workers whose firm experiences a mass layoff event, as in Jacobson et al. (1993).

We focus on individuals born from 1945-1960 that are continuously employed in full-time work (30 hours a week or more) in the same firm who have at least 3 years of tenure in 1989. We also

[3]Excluding offenses such as traffic fines and accidents, which are also recorded in the police data.

Table 1: Impact of Displacement on Crime

Variable	(1) Mean	(2) SD	(3) Observations
Annual Wage (2000 DKK)	238170	169906	8830448
Weeks Fully Unemployed	2.88	9.06	8830448
Firm size	4124.46	9860.5	7494777
Weeks on social assistance	27.1	17.05	150083
Weeks on UI benefits	16.77	15.02	1271574
Age	39.23	6.56	8830448
Birth Year	1952.27	4.67	8830448
Less than high school	27.23%	0.4452	8830448
High School	4.20%	0.2006	8830448
Vocational	44.33%	0.4968	8830448
University or beyond	22.75%	0.4192	8830448
Missing education	1.49%	0.121	8830448
Household income (2000 DKK)	484396	451135	8830448
Wage as fraction of HH Income	50.47%	29.97	8830448
Household size	2.89	1.35	8830448
Adults in Household	1.89	0.62	8830448
Number of children	1.05	1.14	8830448
Probability of charge	2.27%	14.89%	8830448
Number of charges	1.66	3.34	200391
Probability of conviction	1.91%	13.69%	8830448
Probability of conviction - Property	0.65%	8.06%	8830448
Probability of conviction - Violent	0.13%	3.67%	8830448
Probability of conviction - DUI	0.67%	8.14%	8830448
Number of convictions	2.26	5.89	168517
Probability of conviction to Prison	26.29%	44.02%	168517
Length of prison sentence (days)	2341.89	5844.60	44304

Sample: Danish males born from 1945-1960 who are continuously in Denmark from 1985-2000. Means and standard deviations from relevant variables from five different data sources are reported.

exclude individuals whose firm has fewer than 10 employees immediately prior to the displacement period in 1989 in order to exclude the possibility that small changes in the number of employees constitute a mass layoff event. In addition, we exclude any individual who is enrolled in education during the pre-displacement period, a first or second generation immigrant as residency may be tied to employment, or employed in a firm in 1989 which is publicly owned by the state or municipality as the definition of what a firm is for these employees is less obvious. Finally, we restrict our sample to those who are observable throughout our sample period from 1985-2000.[4]

Individuals identified by our sample construction with high-tenure are more likely to have accumulated firm-specific human capital, more likely to be enjoying a more favorable employer-employee specific match, and are thus more likely to face relatively worse outside options as compared to low-tenure workers. Such workers are also less likely to leave a firm during a mass layoff event.[5] The restrictions we put on the sample of workers ensure that those individuals we classify as displaced are high tenured workers with strong ties to their firms, for whom displacement is likely to be sudden and unexpected. In our detailed employer-employee dataset, an individual loses employment between year t and year $t-1$ if the individual was in employment with their 1989 firm in year $t-1$ and has at least one week of unemployment in year t.[6] In the data weeks of unemployment are identified such that a positive number of weeks of unemployment indicates that the worker either lost his employment or has transitioned back into employment with an unemployment spell in between positions. This paper's data set excludes individuals that leave the workforce as (i) a majority of employees receive either unemployment benefits or social assistance and are counted as unemployed and thus (ii) individuals not in employment in year t are individuals that most likely separated from their firm voluntarily.

A firm experiences a mass layoff event in year t if the firm experiences a decline in employment greater than 30% from that firm's peak of employment from 1985-1989 (before the displacement period).[7] According to our definition of displacement, an individual is displaced only if they transition from employment into unemployment in a firm who experiences a mass layoff event, where displacement occurs somewhere between year $t-1$ and t.

[4]Individuals can exist in the data in one year but not the following year either by emigrating from Denmark or through death. This sample restriction excludes a very small portion of individuals and relaxing this restriction does not alter our results.

[5]By focusing on high-tenure individuals, we are likely to get an underestimate of the impact of job losses on crime given the negative correlation between the probability of committing crime and job tenure.

[6]Appendix A presents a detailed discussion of the unemployment system in Denmark as well as the level and type of benefits available during unemployment.

[7]We also consider two alternative definitions of mass-layoff events in Section 9.

An individual is then displaced if he loses employment (following the above definition) from their 1989 employing firm during a firm-level mass layoff event, and we write $Displaced_{it} = 1$. The sample focuses on mass layoffs that occur in the five years after our pre-displacement period such that an individual can be first displaced in 1990 and last displaced in 1994. We follow workers, both displaced and non-displaced, unconditionally in the post-displacement period from 1990-2000, such that our non-displaced sample is composed of individuals who remain in employment (either with the same firm or another firm), individuals who transition into unemployment in a non-mass layoff firm, individuals who transition into non-employment, and individuals who transition from full-time work into part-time work. This ensures we compare the criminal outcomes of high tenure displaced workers to the high tenure workers identified in our sample construction who are not displaced not only in the short run but also in the long run. Table A1 in Appendix B shows the structure of our final sample as well as the number of displaced individuals in each year of our displacement period, and the number of crimes committed in our final estimation sample.

Within a firm that experiences a 30% or greater reduction in employment, individuals who lose employment may be specific individuals in observable and unobservable dimensions. Specifically, a firm and a set of employees may agree on voluntary layoffs. If such voluntary or selective layoffs affect workers that are more likely to commit crime, results correlating displacement events with criminal outcomes will be upward biased. Workers who are less productive, or whose nominal wage is high compared to the firm's outside options, may be more likely to experience job separations. This is where the availability of a longitudinal dataset of individuals with wage and crime in every time period, with individual identifiers, allows us to control for an individual-specific fixed effect. For instance, childhood experiences, dimensions of educational achievement that are not controlled for, will be absorbed by the worker fixed effect.

Individuals may also experience negative productivity shocks right before the firm's mass layoff event, and thus be more likely to lose employment during that firm's mass layoff event. For instance, the loss of a relative (Bennedsen et al. 2006), changes in marital status (Korenman & Neumark 1991), and other time-varying shocks have been shown to affect either worker pay or worker productivity. Such unobservable time-varying life events in the period from 1990-1994, that are correlated with worker productivity or pay and also with the propensity to commit crime, may confound the estimates of the impact of displacement on crime. To test for such possibility, we observe the criminal outcomes of individuals in all years prior to displacement and all years

after displacement. If displacement is truly exogenous to the individual's prior unobservables – including propensity to commit crime – we should not observe that displaced individuals display a more crime-prone history before displacement than other workers.

Another identification issue is that causality could flow from crime to job displacement if, for example, an individual is convicted of or incarcerated for a crime, an employer may separate from the worker as a result. Indeed, the timing of both the displacement event and criminal activity are crucial for our results to be interpreted as the effects of job displacement on crime. A criminal event – left-hand side of the regressions – is an offense that leads to a conviction. Because the dataset includes the date of the offense and a unique identifier for the charges that is linked to the judicial outcome and the sentencing, we trace each conviction back to the date of the corresponding offense.[8] In that way the data focuses only on severe offenses, i.e. with convictions, but avoids the problem of reverse causality that would occur if we defined a criminal event using the conviction date.[9] For instance, an individual could commit a crime prior to displacement, but, due to lags between when the offense is committed and being charged and convicted, may not ultimately be convicted until after displacement. Table A2 in Appendix C presents the typical time between when an offense is committed, is charged, is convicted, and is incarcerated for the overall population from 1985-2000 as well as our displaced sample. The lag between when the crime is committed and when an individual is finally convicted can be quite substantial. In this paper, $Conviction_{it} = 1$ when an offense committed in year t results in a conviction. Focusing on the offense date alleviates concerns that the conviction itself may be a driver of employment loss, as this would reveal itself in the dummies prior to displacement.

Noting $Conviction_{it}$ such a criminal event (offense leading to a conviction), we estimate the full dynamics of criminal events pre- and post-displacement event. As such the main specification of this paper considers the regression of criminal events $Crime_{i,t}$ on the full set of dummy variables. The propensity to commit crime is modeled by the following equation:

$$Conviction_{it} = \alpha_i + \underbrace{\sum_{k=0}^{k=\overline{K}} \delta_k (Disp.\ in\ t-k)}_{\text{post-displacement}} + \underbrace{\sum_{k=\overline{K}}^{k=1} \delta_{-k} (Disp.\ in\ t+k)}_{\text{pre-displacement}} + \tau_t + x_{it}\beta + \varepsilon_{it} \quad (1)$$

[8] For a small percentage of crimes, date of offense is unobservable. For these crimes, date of conviction is used. Results are robust to dropping these crimes.

[9] While convictions corresponds to established guilt of a crime, estimation using charges instead of convictions produces relatively similar results which, if anything, are slightly larger in magnitude.

where $(Disp.\ in\ t-k)$ is a set of post-displacement dummy variables, with k ranging from 0 to $\bar{K}=7$ years, equal to one if the individual was displaced in year $t-k$ and 0 otherwise. Thus the coefficients δ_k measure the impact of displacement on crime for the 7 years following displacement. In particular, δ_k measures both the short-run and the long-run impact of displacement on crime. δ_0 is the coefficient of the impact of displacement on the probability of crime in the year of displacement.

For values $k<0$, the coefficients δ_k measure pre-displacement propensity to commit crime. If the event of displacement is truly exogenous with respect to individual time-varying unobservables, the timing of criminal offenses leading to conviction will always be such that, for each individual, the probability of being convicted for an offense is significantly higher after displacement than before displacement. As such the propensity to commit crime k years prior to displacement should not be different from the propensity to commit crime in the year right before displacement. Formally, their estimates $\hat{\delta_k},\ k=-5,\ ...,\ -1$ of pre-displacement or so-called placebo dummies δ_k should not be statistically significantly different from 0 if displacement is truly independent of the individual's unobservables prior to displacement. In particular if individuals who experience negative family events are more likely to commit crime in the years prior to displacement and are also more likely to be laid off during a mass layoff event, the placebo coefficients δ_k in specification (1) will be significantly above zero.

τ_t are year dummies that captures national trends in the evolution of crime. Such a control is key, as the fraction of individuals committing offenses declines over the time period of analysis. Unemployment declines too, but the country-level correlation between the decline in crime in Denmark and the unemployment rate may be spurious. Hence the dummy-variables control for country-level trends in crime.

X_{it} is a vector of individual characteristics that capture changes in, for example, the individual's marital status and the change in whether the individual has children. As such, as individuals that transition from unmarried to married are less likely to commit crime (in correlation) and are less likely to be displaced, not including the time-varying marital status X_{it} in the vector of covariates may lead to an overestimation of the impact of displacement on crime. Similarly if an increase in children reveals an individual's unobservable propensity to be displaced in future years, including the control for children in the regression should dampen the estimate δ_k of the impact of displacement on crime.

In specification (1), residual unobservables ε_{it} are assumed robust and clustered by individual, so that the unobservables in the propensity to commit crime can experience some degree of autocorrelation $Cov(\varepsilon_{it}, \varepsilon_{it'}) \neq 0$ for any two periods $t,\ t'$. For examining the impact of a shock on an individual's outcomes over time, controlling for autocorrelation typically leads to substantially wider standard errors.[10]

We estimate the effects of displacement on the probability of conviction for an offense committed in year t using fixed effect estimation where α_i absorbs individual-specific non-time-varying observables and unobservables that affect an individual's propensity to commit crime, as in specification (1).[11] It is advantageous to control for individual specific time-invariant factors, as these are relevant in determining an individual's criminal propensity both prior to and following displacement. Estimation using an individual fixed effect implies multiple cohorts of displacement are pooled, in the sense that the non-displaced control group will contain everyone who is not displaced at that time period: both non-displaced individuals but also individuals who have not yet, but will be in the future, displaced.

Specification 1 is estimated over the period 1985 to 2000. The first displacement events occur in 1990, but the inclusion of data from 1985 ensures the identification of the pre-displacement dummies δ_k for $k = -5$ to $k = -1$ starting with the 1990 displacement cohort. As the sample ends in 2000, those individuals who are displaced later in the 1994 cohort, are only followed for 6 years following displacement.

4 Baseline Empirical Findings

Table 2 presents the results of estimating the impact of displacement on offenses leading to a conviction using fixed effect estimation. Columns (1)-(3) show the impacts of displacement on offenses leading to a conviction using individual fixed effect estimation for total, property, and violent crimes respectively. Authors have suggested, using state- or county-level U.S. data, that unemployment may have a causal impact on property crime as individuals' opportunities in the formal labor market are affected by their job separation (Becker 1974, Raphael & Winter-Ebmer 2001). This paper's identification strategy focuses on job displacement as an arguably exogenous

[10] On the importance of controlling for autocorrelation or clustering of residuals in the estimation of treatment effects, see Bertrand et al. (2002).

[11] The coefficient δ_{-1} is normalized by convention to 0 in individual fixed effect estimation, so that all effects are measured relative to the year just prior to displacement.

source of job separation, as individuals may separate endogenously when they have higher opportunities in the formal sector (Rogerson et al. 2005). Job displacement may also cause other types of crime such as violent crime that have been explained either by economic mechanisms (Fajnzylber et al. 2002) or social and psychological mechanisms (Fergusson et al. 2001). Effects are estimated up to 7 years after displacement and up to 5 years prior to displacement.

The coefficients are percentage point impacts: the coefficient of 0.0052 on the impact of displacement on total crime in the year of displacement indicates the probability of committing an offense leading to a conviction is 0.52 percentage points higher in the year of displacement than it was in the year prior to displacement. Using the mean values reported in the last row of Table 2, the impact of displacement on total crime corresponds to an increase in the probability of conviction of 81% sample mean. Effects for total crime peak immediately after displacement, and vanish almost entirely after 4 years. For property crimes, there are both short-run effects, seen in the first few years after displacement, and long-run effects, seen up to 7 years following displacement, of displacement on crime. This effect is strongest 4 years after displacement, where the probability of a conviction for a displaced worker is 0.56 percentage points higher than a non-displaced worker, relative to the year prior to displacement. In terms of percent of the sample mean, these effects for property crime range from a 100-400% increase in the probability of committing a property crime which leads to a conviction. For violent crimes, there are little effects of job displacement, while the probability of conviction for displaced workers is actually significantly lower 4 years after displacement. However, violent offenses are very rare in our sample of high tenured workers and this significant and negative impact 4 years after displacement is attributable to the fact there are zero displaced workers convicted in this particular year. As there is no strong prior reason to believe that displaced individuals are significantly less likely to commit violent crimes 4 years following displacement, particularly as no effects are seen in the years prior, this significant and negative impact is of little consequence.[12]

Pre-displacement coefficients are an important indicator of the magnitude of potential dynamic selection effects: if individuals that are more likely to be convicted are also more likely to be displaced, we should observe positive and significant effects for the $\hat{\delta_k}$ with $k < 0$. The coefficients are presented in the "Year –5" to "Year –2" rows. In all cases none of these placebo coefficients are statistically different from 0, suggesting that displaced individuals are not significantly different

[12]While it is meaningful that no one is convicted of an offense committed 4 years after displacement, it could be imagined that with a much larger sample of displaced workers this effect may be negative but insignificant.

Table 2: Impact of Job Displacement on Crime

	(1) Total	(2) Property	(3) Violent
Year +7	0.0026	0.0042***	-0.0003
	(0.0025)	(0.0015)	(0.0004)
Year +6	-0.0003	0.0025**	0.0000
	(0.0022)	(0.0012)	(0.0005)
Year +5	0.0005	0.0007	-0.0003
	(0.0023)	(0.0010)	(0.0004)
Year +4	0.0046*	0.0056***	-0.0007***
	(0.0026)	(0.0016)	(0.0002)
Year +3	0.0015	0.0014	0.0010
	(0.0024)	(0.0011)	(0.0008)
Year +2	0.0028	0.0022*	-0.0004
	(0.0025)	(0.0012)	(0.0004)
Year +1	0.0050*	0.0036**	0.0010
	(0.0027)	(0.0014)	(0.0008)
Displacement year	0.0052*	0.0038***	-0.0000
	(0.0027)	(0.0014)	(0.0004)
Year -1		Reference	
Year -2	-0.0000	-0.0001	-0.0003
	(0.0023)	(0.0008)	(0.0004)
Year -3	-0.0009	0.0001	-0.0003
	(0.0023)	(0.0008)	(0.0004)
Year -4	-0.0012	0.0002	-0.0003
	(0.0022)	(0.0008)	(0.0004)
Year -5	-0.0013	-0.0005	-0.0003
	(0.0022)	(0.0006)	(0.0004)
R^2	0.097	0.089	0.074
Observations		1638016	
Individuals		102376	
Sample Means	0.64%	0.14%	0.03%

Columns (1)-(3) report the impact of job displacement on the probability of committing an offense leading to a conviction for total, property, and violent crimes respectively. Standard errors are clustered at the individual level. ***, **, and * correspond to significance at the 1%, 5%, and 10% levels respectively.

from non-displaced individuals in terms of their propensity to commit crime prior to displacement. The coefficients are graphically depicted in Figure A1a for offenses leading to a conviction for total crimes and in Figure A1b for offenses leading to a conviction for property crimes in Appendix D.

Results available on request also suggest that individual fixed effects α_i (see specification 1) are positively and significantly correlated both with the probability of displacement and with the probability of a conviction, indicating that they capture a significant share of the individual unobservables that both affect the likelihood of a job separation and of crime. Year dummies capture the national-level declining trend in crime, as aggregate statistics on crime indicate that all types of crime peak in 1994 before declining all throughout the 1990s.

To obtain a detailed sense of the specific types of property crime committed following displacement, Figure A2 in Appendix E breaks down the offenses making up property crimes committed by displaced individuals. Theft is overwhelmingly the most frequent offense, followed by fraud.

5 Household Dynamics

As our data allows us to link not only families but also parents to children, we are able to examine whether job displacement of a father has not only an impact on their criminal participation but also if any impact exists on the criminal participation of their children. Within the job displacement literature, there are numerous studies examining the intergenerational impact of job displacement on numerous outcomes of children, earnings, employment, education, and health (Oreopoulos et al. 2008, Lindo 2011, Schaller & Zerpa 2015). Most studies tend to find that parental job displacement during childhood adversely impacts the outcomes of their children, and emphasize the importance of displacement as a stress inducing event with long lasting repercussions. In addition, there is a wide literature investigating the intergenerational transmission of crime (Hjalmarsson & Lindquist 2012, Meghir et al. 2012, Hjalmarsson & Lindquist 2013), which generally find that more criminal parents cause, at least in part, more criminal children. The literature on the intergenerational transmission of crime emphasizes a number of potential mechanisms explaining the impact such as bad parenting, fostering a negative family culture, negative role model effects, the poor quality of parental peers, direct or indirect transmission of criminal skills, and abusive behaviors transmitting from one generation to the next. Studies generally emphasize that parenting and role modeling effects can explain a large portion of the intergenerational effects. In particular, as males are the

dominant gender committing crime, there is the possibility that effects flowing from father to son may matter most. Previous studies on the impacts of unemployment on crime (Öster & Agell 2007) have findings consistent with the fact that instability in the lives of parents may spillover negatively to the lives of their children. Given the findings of these separate strands of literature, parental displacement could have an impact on the propensity to commit crime of their children.

Table 3 reports the effects of father's job displacement on the criminal behavior of sons and daughters (columns 1 and 2) and only sons (columns 3 and 4) for total and property crimes respectively.[13] We examine these outcomes for children born after 1960, such that displaced individuals are excluded, and define the family as the family in the pre-displacement period in 1989 such that we follow the criminal outcomes of all individuals who were in a 1989 family whose father experienced displacement in the following years independent of whether the children remain living with the same family or not. This is due to the fact that a child may not commit crime while not at home, but commit crime later on, potentially after having moved out. While estimated effects are generally positive for total and property crimes, these are, at best, only marginally significant in the year following displacement for children. We see that the effect is driven entirely by an impact of father's displacement on son's criminal behavior, and there is little evidence that displacement of the father impacts the crime of the daughter. While we find that children of displaced fathers are marginally more likely to engage in crime compared to children of non-displaced fathers one year after displacement, these effects are short lasting and are suggestive of only a minor and short-run impact of father's displacement on son's criminal activity. In terms of potential mechanisms, we see some evidence (in regressions unreported) suggesting that the stress inducing impact of job displacement at the very least explains a portion of this short-run effect as we see adverse effects of job displacement on measures of family structure such as marital status and adult family size in the years after, but not prior to displacement.

6 Displacement Impacts by Education

For education, results are grouped into separate samples by educational attainment (measured prior to displacement in 1989) where we estimate the impact of displacement on property crime separately for individuals with less than high school education, vocational training, and university (or greater) education. In Denmark during the time period of this paper, education was compul-

[13] In the interest of brevity, only post-displacement dummies are reported.

Table 3: Impact of Job Displacement on Crime

	(1)	(2)	(3)	(4)
	Sons and Daughters		Sons	
	Total	Property	Total	Property
Year +7	0.0025	0.0032	0.0029	0.0040*
	(0.0026)	(0.0022)	(0.0024)	(0.0021)
Year +6	-0.0001	0.0004	-0.0010	0.0000
	(0.0024)	(0.0020)	(0.0021)	(0.0018)
Year +5	-0.0007	-0.0009	-0.0023	-0.0025
	(0.0024)	(0.0019)	(0.0021)	(0.0016)
Year +4	-0.0010	-0.0012	0.0001	-0.0005
	(0.0022)	(0.0018)	(0.0021)	(0.0016)
Year +3	0.0025	0.0019	0.0019	0.0020
	(0.0024)	(0.0020)	(0.0022)	(0.0018)
Year +2	0.0016	0.0003	0.0022	0.0009
	(0.0025)	(0.0020)	(0.0023)	(0.0018)
Year +1	0.0042*	0.0025	0.0046**	0.0032*
	(0.0024)	(0.0020)	(0.0023)	(0.0019)
Displacement year	0.0020	0.0014	0.0014	0.0006
	(0.0023)	(0.0019)	(0.0021)	(0.0017)
Year -1	Reference			
R^2	0.191	0.168	0.200	0.178
Observations	1638016		1638016	
Individuals	102376		102376	

Crime is defined an offense leading to a conviction for children born from 1961 onwards who in 1989 reside in the same household as their father. Columns (1)-(2) use this measure of crime for both sons and daughters, columns (3)-(4) use this measure only for sons. Not reported are pre-displacement variables. Standard errors are clustered at the individual level. ***, **, and * correspond to significance at the 1%, 5%, and 10% levels respectively.

sory to grade 9.[14] After compulsory education, individuals may decide to continue to high school (either directly after compulsory education or via an optional 10^{th} grade) which is geared towards attending university, vocational training which is geared towards professional employment, or terminate education. Given the importance of educational attainment in determining an individual's criminal propensity (Lochner & Moretti 2004, Machin et al. 2011, Hjalmarsson et al. 2015), all else equal, we would expect to see larger effects of displacement on crime for those with less education.

Table 4 reveals that individuals with less than high school education experience sizable, significant, and long lasting effects of displacement on the probability of conviction for property crimes. Those displaced with vocational training education experience significant increases in crime compared to non-displaced individuals, but not as strong as the effects seen for those displaced with less than high school education. For individuals with university education, the estimated effects fluctuate between positive and negative, but never significantly different from zero.

7 Which Individuals Are Most Affected?

In addition to education, we can examine what other factors (if any) predict whether displacement has an impact on crime. Table 5 investigates which types of individuals are most affected by the impact of job displacement on property crimes. While overall, we see large and long lasting effects of job displacement on property crimes, these effects, both in their magnitude and longevity, are likely to differ by family and socioeconomic factors. As with an individual's education, we separate our estimation sample for predetermined levels of four potentially relevant variables in their 1989 values: age, whether displaced individuals have children, marital status, and single adult family. Ages are examined across two birth cohorts within our displaced sample, those born from 1945-1950 and those born from 1951-1960. The effects of age on offending are established to peak at late teens and early 20s, and declining over time. Thus it could be expected that the younger birth cohort may have larger effects of displacement on crime. Those who have any children in 1989 could be more responsible and less likely to offend in order to avoid the possibility of being apprehended and losing time spent with their children. On the other hand, it could be that those with children have a greater need for income in order to maintain their levels of expenditure prior to displacement, increasing the probability of committing crime. Those married and living with at least one other adult in 1989 are, similar to those with children, potentially less likely to offend to avoid the

[14] Recently a grade 0 became compulsory as well.

Table 4: Impact of Job Displacement on Crime by Educational Attainment

	(1)	(2)	(3)
	Less than High School	Vocational Education	University or Beyond
		Property	
Year +7	0.0051*	0.0042**	0.0026
	(0.0029)	(0.0020)	(0.0032)
Year +6	0.0057*	0.0017	0.0020
	(0.0030)	(0.0014)	(0.0030)
Year +5	0.0018	-0.0003	0.0020
	(0.0022)	(0.0009)	(0.0030)
Year +4	0.0051*	0.0044**	0.0075
	(0.0029)	(0.0019)	(0.0049)
Year +3	0.0036	0.0001	0.0047
	(0.0028)	(0.0011)	(0.0040)
Year +2	0.0057*	-0.0000	0.0018
	(0.0032)	(0.0011)	(0.0021)
Year +1	0.0070**	0.0035*	-0.0009
	(0.0035)	(0.0018)	(0.0007)
Displacement year	0.0097**	0.0019	-0.0008
	(0.0039)	(0.0015)	(0.0007)
Year -1		Reference	
R^2	0.078	0.087	0.081
Observations	377024	896672	292256
Individuals	23564	56042	18266

Samples are defined taking an individual's educational qualification in the pre-displacement period in 1989. Property crime is defined as committing a property offense which leads to a conviction. Not reported are pre-displacement variables. ***, **, and * correspond to significance at the 1%, 5%, and 10% levels respectively.

possibility of losing time spent with their spouse or fellow household members. Additionally, there is also the possibility both for those married and those living with another adult that income could be pooled over the household, and the impacts of job displacement on personal income could be less meaningful to financial stability. Estimation using whether a displaced individual lives with another adult takes into account the possibility that two individuals could be living together, but not be married.

The impact of job displacement on property crime is presented in Table 5 by displaced individual's age, whether they have children, whether they are married, and whether they live with other adults in columns (1)-(4) respectively. Within the separate birth cohorts, the estimated effects of job displacement on property crimes are positive and relatively similar irrespective of the displaced individual's birth cohort. This suggests that displacement is such a negative and sizable shock to an employee that it is sufficient to push someone on the margin of committing crime to engaging in crime irrespective of their age. Examining the importance of having children reveals that job displacement impacts crime for both those with and without children in 1989. While it could be argued that having children of a young age, as opposed to children of any age, is what matters, the post-displacement effects on property crime are unchanged if children are defined not of any age, but those below the age of 18. The lack of differences by whether displaced individuals have children suggests that family factors may not necessarily predict the existence of effects of job displacement on crime. Where the estimated effects of displacement on crime differ is whether a displaced individual is married or living with another adult in 1989. There is a strong impact of job displacement on the crime participation of unmarried and those living with no other adults and while some effects are seen for those married or those living with at least one other adult, both the short- and long-run impacts of displacement on crime are much smaller in magnitude and, in most instances, not significantly different from zero. Job displacement appears to have meaningful and large impacts on crime, both in the short- and long-run, for individuals who are residing in a single adult household, suggesting the earnings potential of the household plays a role in mitigating the effects of job displacement on crime.

Table 5: Impact of Job Displacement on Crime for Relevant Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ages		Children		Married		Adult in HH	
	1945-50	1951-60	Children	No Children	Married	Unmarried	Single Adult	2+ Adults
	Property		Property		Property		Property	
Year +7	0.0024	0.0058**	0.0030*	0.0064**	0.0018	0.0075***	0.0105**	0.0022
	(0.0018)	(0.0023)	(0.0016)	(0.0030)	(0.0015)	(0.0029)	(0.0042)	(0.0014)
Year +6	0.0032*	0.0018	0.0024	0.0026	0.0014	0.0041*	0.0090**	0.0004
	(0.0019)	(0.0016)	(0.0015)	(0.0021)	(0.0014)	(0.0022)	(0.0037)	(0.0011)
Year +5	0.0004	0.0009	-0.0003	0.0025	-0.0005	0.0023	0.0031	-0.0001
	(0.0013)	(0.0014)	(0.0010)	(0.0021)	(0.0009)	(0.0019)	(0.0025)	(0.0010)
Year +4	0.0050**	0.0061***	0.0042**	0.0083***	0.0028*	0.0095***	0.0100**	0.0042**
	(0.0022)	(0.0023)	(0.0017)	(0.0032)	(0.0016)	(0.0030)	(0.0040)	(0.0016)
Year +3	0.0013	0.0014	0.0009	0.0023	-0.0002	0.0037	0.0027	0.0009
	(0.0016)	(0.0016)	(0.0013)	(0.0021)	(0.0011)	(0.0022)	(0.0025)	(0.0012)
Year +2	0.0005	0.0039*	0.0018	0.0031	0.0008	0.0043*	0.0038	0.0017
	(0.0014)	(0.0021)	(0.0015)	(0.0023)	(0.0014)	(0.0023)	(0.0029)	(0.0014)
Year +1	0.0046**	0.0026	0.0033*	0.0041*	0.0014	0.0067**	0.0094**	0.0017
	(0.0022)	(0.0018)	(0.0017)	(0.0025)	(0.0014)	(0.0028)	(0.0040)	(0.0014)
Displacement year	0.0027	0.0048**	0.0030*	0.0053**	0.0010	0.0076***	0.0097**	0.0019
	(0.0019)	(0.0022)	(0.0017)	(0.0027)	(0.0014)	(0.0029)	(0.0040)	(0.0014)
Year -1	Reference							
R^2	0.091	0.087	0.091	0.084	0.083	0.094	0.093	0.087
Observations	720704	917312	1213872	424144	1105248	532768	269840	1368176
Individuals	45044	57332	75867	26509	69078	33298	16865	85511

Sample are defined over ages, the presence of children, marital status, and residing with another adult. For columns (3)-(8) these are measured using their values in the pre-displacement period in 1989. Property crime is defined as committing a property offense which leads to a conviction. Not reported are pre-displacement variables. Standard errors are clustered at the individual level. ***, **, and * correspond to significance at the 1%, 5%, and 10% levels respectively.

8 Controlling for Confounder Characteristics

Figure A3 replicates the findings of Jacobson et al. (1993), i.e. estimates the impact of job displacement on annual earnings and on the number of weeks of unemployment. Jacobson et al.'s (1993) finding is that displaced workers will experience permanent earnings losses and long-term impacts on weeks of unemployment. Figure A3 presents results of the estimation of their specification on our Danish data. In particular the specification includes an individual fixed effect, year dummies, and the full set of pre- and post-displacement year-level dummies. Figure A3a is for annual salary in Danish Kroner, while Figure A3b is for weeks of unemployment. Figure A3a is similar to JLS's finding that displaced workers experience permanent long-term earnings losses. In this paper's analysis, the permanent loss 7 years after displacement is about 50,000 DKK, or 7,500 USD per year in 2015 dollars. Given the magnitude of such impacts of displacement on long-term earnings in the formal sector, opportunities in the informal sector—burglary, thefts, pilfering—may be become relatively more attractive; and thus the impact of displacement on earnings may explain the impact of displacement on crime.

Tables 6 present estimates of the impact of displacement on offenses leading to a conviction for property crimes, controlling for a variety of family, geographic, and employment factors which were previously unaccounted for. The first column presents results including municipality fixed effects, while the next two columns present estimates conditional on whether a displaced individual has children and marital status in the current year. For example, as we do see an impact of job displacement on family dissolution, it is possible that job displacement impacts crime only, or partially, through its negative impacts on family structure. Likewise it could be that specific municipal factors could also affect an individual's probability of engaging in crime and accounting for these may be important. However, the results of the three columns are virtually unchanged for property crimes, leading to the conclusion that neither family factors nor time-invariant municipality factors have much impact on the increase in crime due to displacement.

The last two columns of Tables 6 present estimates of the impact of displacement on offenses leading to a conviction for property crimes, conditional on the individual's unemployment weeks in the current year (column 4) and conditional on the individual's annual salary in the current year (column 5). Given that lower earnings lead to increased probability of criminal offenses, and that there is a negative correlation between displacement and earnings, we should expect

Table 6: Controlling for Confounder Controls - Property

	(1)	(2)	(3)	(4)	(5)
	Municipality Effects	Children Dummy	Married Dummy	Weeks Unemployed	Annual Salary
			Property Crime		
Year +7	0.0042***	0.0042***	0.0042***	0.0040***	0.0040***
	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0015)
Year +6	0.0025**	0.0025**	0.0025**	0.0022*	0.0022*
	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Year +5	0.0007	0.0006	0.0006	0.0002	0.0004
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)
Year +4	0.0056***	0.0056***	0.0056***	0.0049***	0.0053***
	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)
Year +3	0.0014	0.0014	0.0013	0.0005	0.0010
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Year +2	0.0022*	0.0022*	0.0022*	0.0008	0.0018
	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Year +1	0.0036**	0.0036**	0.0036**	0.0014	0.0030**
	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
Displacement year	0.0038***	0.0038***	0.0038***	0.0021	0.0034**
	(0.0014)	(0.0014)	(0.0014)	(0.0015)	(0.0014)
Year -1			Reference		
Control		-0.0003*	-0.0004***	0.0001***	-0.00056***
		(0.0002)	(0.0001)	(0.0000)	(0.0000)
Municipality FEs	Yes	No	No	No	No
R^2	0.089	0.089	0.089	0.089	0.089
Observations	1638016	1638016	1638016	1638016	1638016
Individuals	102376	102376	102376	102376	102376

Control corresponds to the estimate of the variable listed in the column heading. Annual salary is reported per 100,000DKK. Property crime is defined as committing a property offense which leads to a conviction. Not reported are pre-displacement variables. Standard errors are clustered at the individual level. ***, **, and * correspond to significance at the 1%, 5%, and 10% levels respectively.

that the coefficient conditional on earnings will be smaller in magnitude. Contrary to this, the results controlling for earnings are relatively similar, although less precisely estimated, to the baseline results, despite the negative and significant effect of wages. Given that the generous unemployment benefits are available in Denmark, these benefits may diminish the role of lost salary in explaining the effects of job displacement on crime, as individuals still receive a base level of income. However, when controlling for weeks spent in unemployment, the effect of displacement on property crimes is reduced, where all post-displacement coefficients decline in magnitude. The positive and significant effect of weeks of unemployment as well as its impact on the effects of displacement on crime indicate that inactivity, more so than individual earnings losses, may play some role in the links between job displacement and crime.

9 Robustness of Baseline Findings

The paper's baseline results present estimates of the impact of job displacement on crime assuming that firms' mass layoff events occur when the firm's size is lower than 30% of its peak employment, measured between 1985 and 1989 in the pre-displacement period (Jacobson et al. 1993). We examine the robustness of our baseline findings re-estimating our displacement effects considering displaced workers from larger firms, an alternative threshold for a mass layoff event, and two alternative definitions for mass layoff events.

9.1 Changes in Firm-Size Threshold

The paper's baseline estimates of Table 2 present results considering firms with 10 or more employees. Firms in Denmark are of a typically smaller size than firms in the United States, and the threshold of 10 corresponds to firms in 72^{nd} percentile of the distribution of firm size in 1989.[15] However, one may argue that with only 10 workers, a mass layoff event may be caused by a temporary changes in firm size. We thus test the robustness of our results to restricting the data set to firms with 20 or more employees (87^{th} percentile), 25 or more employees (90^{th}), and 50 or more employees (95^{th}) in 1989. Results are presented in columns (2)-(4) of Table 7 and overall show very similar effects of displacement on conviction.

However, it is worth noting that marginally larger effects are seen for the samples with larger firms. One potential shortcoming in using displacement as an identification strategy is that while an individual's job loss is unanticipated, their fellow co-workers in their potential network may also experience a similar employment shock. It could be, in the case of very large firms, that these mass layoff events will be sufficiently large to alter an individual's potential network, in which case some of the effects we attribute to job displacement on crime could also be due to the effects of a change in one's network on crime. However, given that these effects are only marginally larger in these robustness specifications, are well within the confidence intervals of the baseline findings, and that (in regressions unreported) excluding individuals in firms in the top 1 percentile of firm size in 1989 produces very similar results to the baseline estimation, this issue seems minor, but is worth taking into consideration.

[15] Excluding those firms which are a self-employed individual.

Table 7: Robustness

	(1)	(2)	(3)	(4)	(5)
	Baseline Estimation	20+ Employees	25+ Employees	50+ Employees	40% JLS Threshold
			Property Crime		
Year +7	0.0042***	0.0049***	0.0054***	0.0061***	0.0025*
	(0.0015)	(0.0017)	(0.0019)	(0.0022)	(0.0014)
Year +6	0.0025**	0.0025*	0.0028**	0.0023	0.0023*
	(0.0012)	(0.0013)	(0.0014)	(0.0015)	(0.0014)
Year +5	0.0007	-0.0001	0.0001	-0.0001	0.0012
	(0.0010)	(0.0009)	(0.0010)	(0.0011)	(0.0012)
Year +4	0.0056***	0.0067***	0.0068***	0.0070***	0.0048***
	(0.0016)	(0.0018)	(0.0019)	(0.0022)	(0.0017)
Year +3	0.0014	0.0017	0.0019	0.0023	0.0005
	(0.0011)	(0.0013)	(0.0014)	(0.0016)	(0.0011)
Year +2	0.0022*	0.0031**	0.0035**	0.0033*	0.0012
	(0.0012)	(0.0015)	(0.0016)	(0.0018)	(0.0013)
Year +1	0.0036**	0.0040**	0.0044**	0.0038**	0.0036**
	(0.0014)	(0.0016)	(0.0017)	(0.0019)	(0.0016)
Displacement year	0.0038***	0.0046***	0.0046***	0.0046**	0.0042**
	(0.0014)	(0.0017)	(0.0017)	(0.0020)	(0.0017)
Year -1			Reference		
Year -2	-0.0001	0.0002	0.0003	0.0007	0.0001
	(0.0008)	(0.0009)	(0.0010)	(0.0012)	(0.0009)
Year -3	0.0001	0.0004	0.0006	0.0004	0.0004
	(0.0008)	(0.0009)	(0.0010)	(0.0011)	(0.0009)
Year -4	0.0002	0.0005	0.0006	0.0005	0.0005
	(0.0008)	(0.0009)	(0.0009)	(0.0011)	(0.0009)
Year -5	-0.0005	-0.0008	-0.0007	-0.0007	-0.0005
	(0.0006)	(0.0006)	(0.0006)	(0.0007)	(0.0006)
R^2	0.089	0.090	0.089	0.087	0.089
Observations	1638016	1472016	1407120	1201344	1638016
Individuals	102376	92001	87945	75084	102376

Column (1) corresponds to baseline estimation for property crime. Property crime is defined as committing a property offense which leads to a conviction. Columns (2)-(4) alter the number of employees in 1989 restriction, column (5) increases the criteria for a mass-layoff event. Standard errors are clustered at the individual level. ***, **, and * correspond to significance at the 1%, 5%, and 10% levels respectively.

9.2 Increasing JLS Criteria

Column (5) of Table 7 examines defining mass layoff events as a year when firm size is 40% lower than its peak from 1985-1989 compared to the baseline of reduction of 30%. Using a threshold of 40% should lead to less frequent mass layoff events and thus to more conservative estimates of the impact of displacement on crime.[16] The estimated effects of displacement on property crime using the 40% threshold are similar in magnitude to the baseline estimation, with slightly less statistical significance in the long-run effects of job displacement on crime.

9.3 Alternative Definitions for Firm-Level Mass Layoff

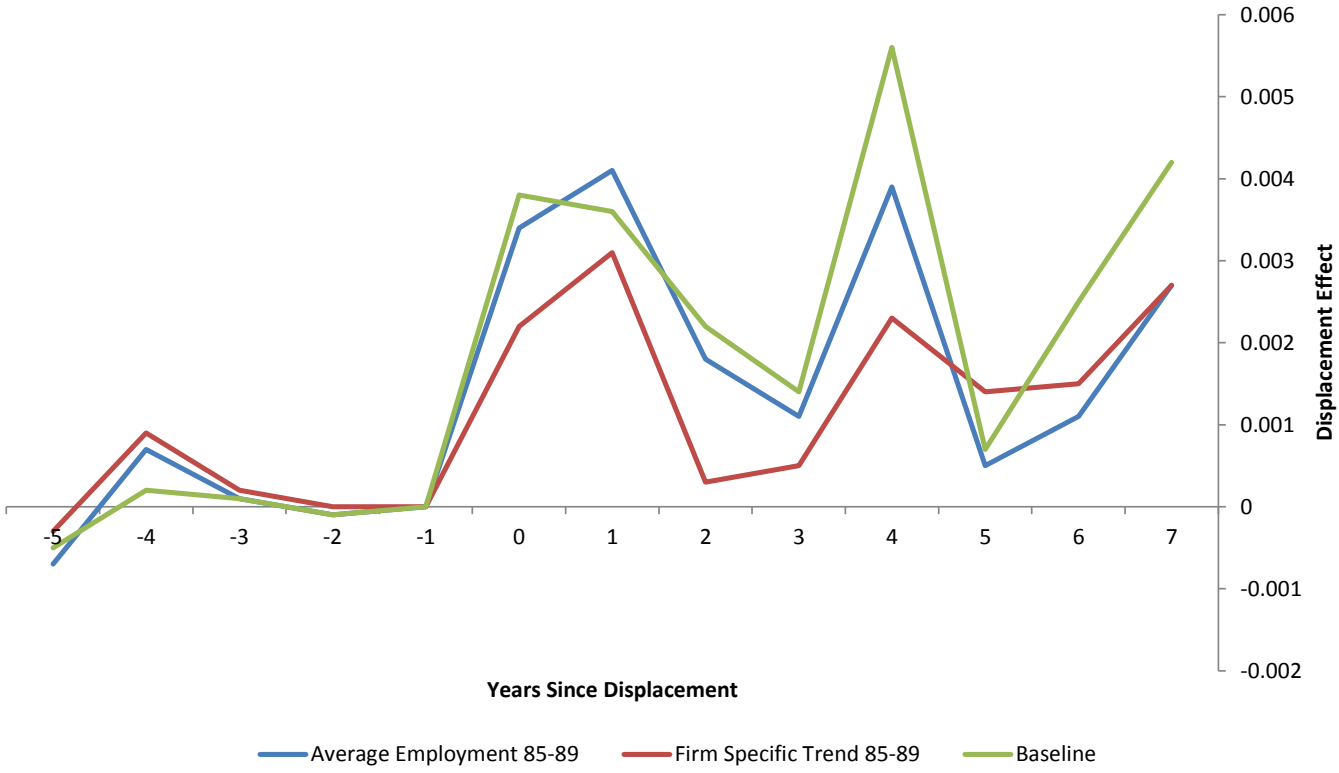
Additionally, Figure 1 examines two alternative definitions of the reference point used to define mass layoff: a year in which firm size is 30% below the firm's average employment in 1985-89 rather than its peak employment and a year in which firm size is 30% below a predicted firm-specific trend in employment. Such a firm-specific trend in employment is constructed as follows. First, for each firm, a linear regression of firm size on years since 1985 is run, for years between 1985 and 1989: $FirmSize_{jt} = Constant_j + Slope_j(Year - 1985)_t$, where j indexes firms. When the firm has increasing size, we set $Slope_j = 0$ and use its average employment to keep only genuine declines in employment, such that the firm-specific trend is only used for firms with declining employment. Such linear trend is then used to predict firm size after 1989. By using this firm-specific trend, we exclude the possibility that a firm whose employment is in slow decline throughout the entire sample period could eventually be classified as a firm with a mass layoff event, if gradual declines in employment persisted over multiple years. Within such a framework, the firm experiences a mass layoff event if its size in year t for $t = 1990, ..., 1994$ is 30% lower than its predicted size according to this linear extrapolation.

These two alternative definitions should lead to more conservative estimates of job displacement on crime as using average levels of employment to calculate a mass layoff will understate mass layoff events for firms with increasing employment trends who then experience a sudden decline in employment during the displacement period. Despite this, Figure 1 shows that for all definitions of firm-level mass layoff events, the effects of job displacement on property crimes are positive and long lasting. While these results are overall similar to the results of the baseline estimation, these alternative definitions produce effects of displacement on property crimes which are smaller

[16] Indeed the total number of displaced individuals decreases by slightly more than 800 individuals.

in magnitude.

Figure 1: Firm-Specific Mass Layoff Event



Baseline corresponds to baseline estimation for property crimes. Blue and red lines define a mass layoff as a 30% decline from the average number of employees in a firm from 1985-1989 and a 30% decline from a firm-specific trend in employment respectively.

10 Conclusion

Using a data set of high-tenure male workers from 1985-2000 in Denmark, we examine the relationship between unemployment and crime at the individual level by estimating the impact of job displacement on an individual's probability to commit a criminal offense leading to a conviction. Following Jacobson et al. (1993), we define as a mass layoff event where firms lose 30% of their employment from their peak employment during a pre-displacement period. We find that displaced workers are significantly more likely to engage in total and property crimes than their non-displaced counterparts following displacement, but importantly, are not significantly different prior to displacement. We find that individuals with less than high school, and slightly less so, vocational education are proportionately more affected by displacement than individuals with university education. The effects are both long lasting and economically significant, lasting up to 7

years following displacement.

We show that there are little effects of father's displacement on the criminality of sons and daughters, where we estimate a marginally significant short-run increase in crime attributable to an impact of father's displacement on sons. We find that individuals residing in a single adult household are more likely to be affected by displacement than those either married or living with another adult, but see impacts of displacement on crime irrespective of an individual's age and whether a displaced individual has children. Estimating the effects of displacement on annual salary and weeks of unemployment, we conclude that these effects of displacement on crime are partly driven by the impact that displacement has on unemployment, and less so by the impact on earnings. We also examine the possibility that a relationship between marital status, number of children, or municipality factors drives our results, and find that results are robust to the inclusion of such controls—even though the probability of being married is lower after displacement as compared to after displacement. Our results are robust to a variety of specification adjustments, including changing our definition of a mass layoff event and increasing the number of employees required to be laid off to cause a mass layoff event. Our results are useful to policymakers and suggest that job separations are likely to have impacts on society which extend beyond employers and employees, which may provide scope for welfare increasing labor market policies.

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A Unemployment in Denmark

When an individual becomes unemployed in Denmark, they can receive either unemployment insurance benefits or social assistance. In order to be eligible for unemployment insurance, an individual must, voluntarily, pay monthly membership into a unemployment insurance fund. As a member of an unemployment insurance fund, an individual receives 90% of their original salary, subject to a maximum and eligibility rules. During the time period in this paper, these eligibility criteria were membership into an unemployment insurance fund for a minimum of one year, and employment for at least half a year out of the previous 3 years before unemployment. If an individual was not a member of an unemployment insurance fund, they still may be eligible for social assistance. Social assistance in Denmark is means-tested, and less clearly defined compared to unemployment benefits. The amount an individual would receive depends on a variety of factors such as household size, marital status, spousal earnings, and household wealth. For example, a single father with kids would receive fairly large welfare payments, while a married father whose spouse earns a high income would receive very little welfare payments. While all these factors go into the level of benefits one would receive, the precise amount received ultimately depends on an assessment determined by a welfare counselor. Membership in an unemployment insurance fund is relatively common in Denmark, where during the 1990s, membership among employed workers was around 85%. Unemployment insurance membership does vary across socio-economic factors and earnings, due to the fact that some individuals would receive similar payments whether they joined a fund or not, while some individuals would receive nearly no social assistance payments but high unemployment insurance payments.

B Structure of the Estimation Sample

Table A1: Structure of the Panel Dataset

Year	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000
	Pre-Displacement Period					Post-Displacement Period										
						Displacement Years										
Observations																
Final panel	102,376					102,376										
Displacement	0	0	0	0	0	647	710	623	660	321	0	0	0	0	0	0
Crime																
Property	122	95	78	86	89	114	155	216	226	216	189	136	157	154	135	86
Violent	39	26	26	30	24	36	28	25	40	43	34	35	30	27	27	23
Total	713	687	674	609	663	723	693	794	767	730	623	575	600	612	591	427

Reported are the final number of individuals in each year of the data, the sums of displacement for the displacement years, and the sums of crimes in each year for the estimation sample over the pre- and post-displacement periods.

C Time Between Charges, Convictions, and Incarcerations

Table A2: Timing of Judicial Process for Overall Sample and Displaced Sample

	Time from Offense to Charges (days)					Time from Charges to Conviction (days)					Time from Conviction to Prison (days)				
	Mean	Median	P25	P75	Charges	Mean	Median	P25	P75	Convictions	Mean	Median	P25	P75	Prison Terms
Sample: all of Denmark with at least one charge over sample period															
Total Crimes	78.1	1	0	44	2759322	136	94	43	180	1172128 (42.5%)	170.6	124	47	229	213246 (18.2%)
Sample: displaced individuals with at least one charge over sample period															
Total Crimes	81	0	0	18	922	129.9	89.6	52	151.4	646 (70.1%)	203.2	166	106	236.8	117 (18.1%)

Top panel reports the time between charges, convictions, and incarcerations for all individuals charged by the police from 1985-2000. Bottom panel reports the same times for individuals who are displaced at any point from 1990-1994 from 1985-2000. For confidentiality reasons, the 25^{th} percentile, the median, and the 75^{th} percentile are calculated using the average observations of the 5 individuals surrounding each statistic.

D Graphical Representation of Baseline Estimates

Figure A1: Impact of Job Displacement on Probability of an Offense Leading to a Conviction

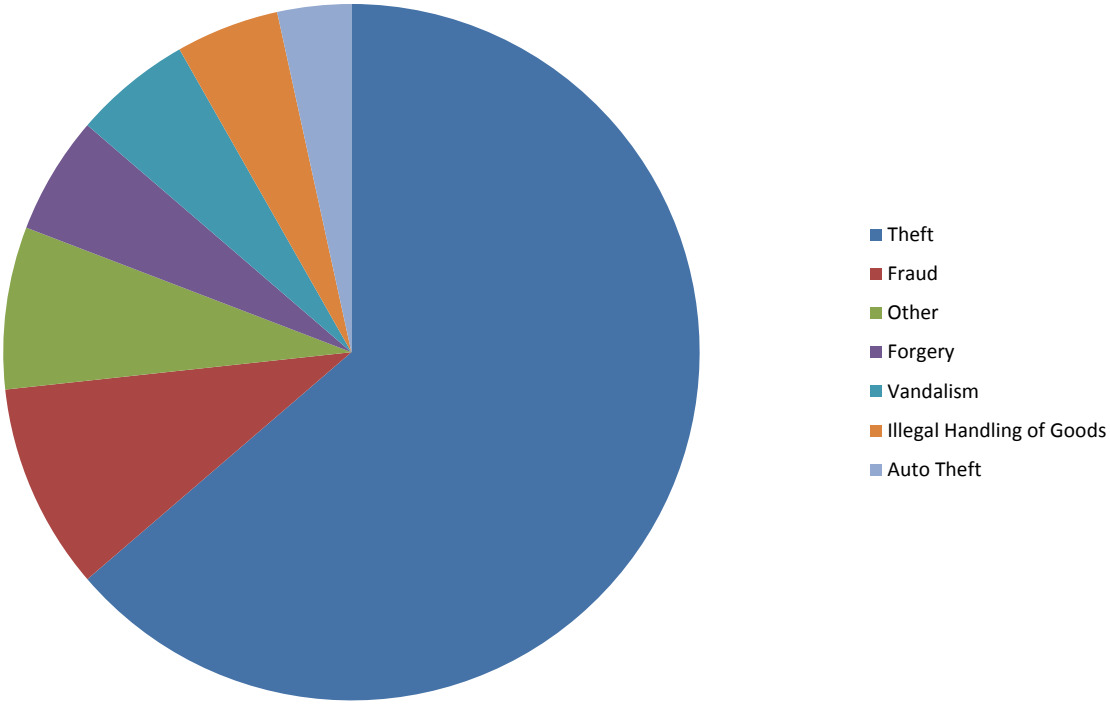
(a) Total Crimes

(b) Property Crimes

Plots correspond to baseline estimates in Table 2 for total and property crimes.

E Detailed Property Crimes

Figure A2: Detailed Property Crimes Committed by Displaced Individuals Post-Displacement



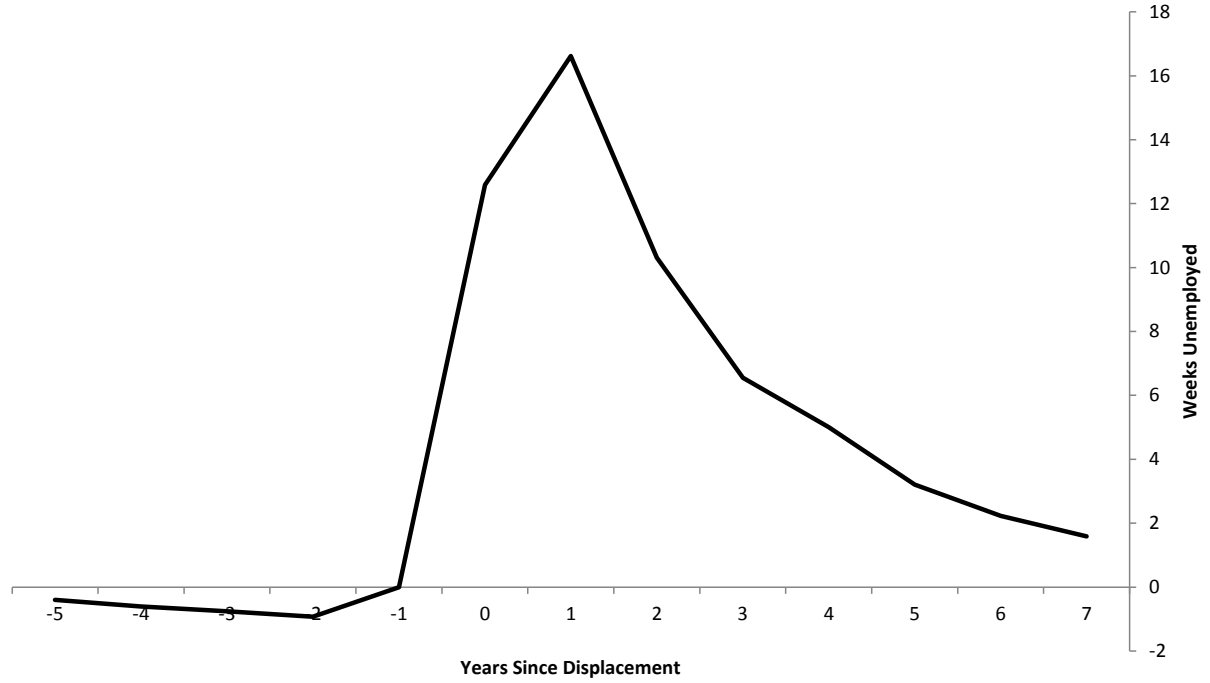
Percentages correspond to the fraction of specific offenses within the property crimes committed by displaced individuals following displacement. "Other" corresponds to arson, burglary, extortion, and embezzlement and is aggregated in order to comply with Statistics Denmark's data confidentiality policies.

F The Impact of Job Displacement on Earnings Losses and Unemployment

Figure A3: Impact of Job Displacement on Employment Outcomes

(a) Impact of Displacement on Annual Salary

(b) Impact of Displacement on Weeks Unemployed



Plotted are coefficients from estimation of pre- and post-displacement dummies on weeks unemployed and annual salary respectively.

Chapter 3 - Negative Attitudes, Network and Education

Negative Attitudes, Network and Education

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Abstract

This paper assesses, both theoretically and empirically, the potential explanations behind the educational gap between young natives and immigrants using two measures, negative attitudes towards immigrants and networking. The paper considers the impact of negative attitudes and networking and that these parameters may influence high and uneducated workers as well as immigrants and natives differently, creating different incentives to acquire education for the two ethnic groups. Theoretically, this paper concludes that if all immigrants are equally affected by discrimination, immigrants obtain less education than natives while if only low-educated immigrants are affected by negative attitudes, immigrants obtain more education than natives to improve their employment prospects. Using rich Danish administrative data, this paper finds evidence consistent with this second case, that greater negative attitudes have a positive impact on male immigrants decision to acquire education and that networking can also increase immigrant education.

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1 Introduction

An OECD report from 2006 reveals that immigrant and immigrant offspring at a very young age express equal or sometimes even higher motivation to learn mathematics than their native counterparts and very positive attitudes towards school and education in general.[1] However, at the age of 15, they under perform compared to the natives. More than a third of the first and second generation immigrant children in Austria, Belgium, Denmark, Germany, Norway and the USA, who have spent all their entire schooling in the host country, perform below the baseline PISA benchmark for mathematics performance, a period at which students begin to demonstrate the kind of skills that enable them to actively use mathematics.[2] Furthermore, when taking their parental background into account, immigrants tend not to perform as well in school as their native peers.[3] This fact may then, in turn, influence their choice of further education, and eventually their labour market outcome and performance.

When explaining the educational gap between immigrants and natives, measures which impact immigrants and natives differently are likely to be important. The aim of this paper is to discover the factors that shift the motivation and performance of immigrants when the decision about education beyond compulsory school is taken. For the educational decision, workers compare the value corresponding to acquiring education to the value of not acquiring education. These values depend on the expected incomes which are influenced by both the employment probability as well as wages. The novelty of this paper is to examine theoretically, as well as empirically, whether negative attitudes towards immigrants and networking could influence immigrant employment chances, as well as immigrant wages, differently for educated workers and uneducated workers compared to the same variables for natives. If this were the case, the value of acquiring education may be impacted differently for natives and immigrants and as such, may explain the educational gap between natives and immigrants.

In particular, we will examine the effect of negative attitudes towards immigrants in a region and potential impact of networking through individuals of the same ethnicity living in a region. Negative attitudes towards immigrants may cause discrimination, implying that workers are fired or decide to quit a job. This lowers the value of employment, through both shorter employment

[1] OECD 2006
[2] ibid.
[3] Nielsen and Rangvid

periods and lower wages, as the bargaining power of immigrants falls which in turn affects the value of acquiring education.

There are some empirical papers on discrimination and employment and wages (see for example Waisman and Larsen 2015, Kofi Charles and Guryan 2008) but, to our knowledge, no papers on the additional impact through these channels on education. Concerning networking, immigrants from the same home country or region may increase the likelihood of getting a job and improve labour market performance. Hence, more well-educated immigrants from the same home country or region may increase the return of education, implying that more immigrants acquire education. This may work in different ways. Social networks may influence employment outcomes: the more employed contacts the individual has, the more likely it is that the individual will learn about new job openings (Calvo-Armengol and Jackson 2004, Hellerstein et al 2009) and networks may influence both wages and employment opportunities (Fontaine 2007, Galeanios 2014, Damm 2014). Similarly, empirical research confirms that (see for example Andersson et al 2009, Solignac and Tô 2015) more immigrants living in areas with a large number of employed neighbours are more likely to have jobs compared to immigrants living in areas with fewer employed neighbours. This could be due to networking and/or social norm effects. Furthermore, Kramarz and Skans (2014) show for Swedish data that family networks are important, in terms of obtaining the first job after graduation, and that this impact is stronger for youth of uneducated parents and immigrants in regions with high unemployment. Hence, networking may increase employment probability, and more networking among immigrants may, to some extent, offset the decrease in employment perspectives and wage modifications due to negative attitudes or discrimination.

We formulate a Becker-style taste discrimination model within a search and wage bargaining setting. Bowlus and Eckstein (2002), Flabbi (2010), Mailath et al. (2000), and Lang et al. (2005) study discrimination in the presence of search frictions but with no educational decision. We assume that potential negative tastes towards immigrants imply that their separation rate from the job is higher than the separation rate of a native worker. This may be due to both the worker deciding to quit and the employer firing the worker. This assumption allows us to assume that neither job searchers nor employers know whether discrimination will take place in a particular firm; all that is known is that immigrants face a higher separation rate than natives. We show that immigrants' potential higher separation rate, ceteris paribus, also implies that their employment chances fall as firms, in turn, supply fewer vacancies. Natives and immigrants decide

whether to educate or not. They are aware of the existence of discrimination in the labour market and of the possibility of influencing their chances of getting employed through networking. In terms of negative attitudes towards immigrants, we consider two different cases. In the first case, all immigrant workers are affected by negative attitudes towards them and in the second, only low-educated workers are affected. The channel through which the educational level is affected by networking and negative attitudes in our model is through the impact on the expected employment perspectives. However, the possibility that negative attitudes also influence the value of being unemployed directly, that is, over and above the impact on wages and employment chances, could easily be included in the theoretical model and is consistent with the empirical analysis which we perform.

Empirically, we analyse the educational gap between immigrants and natives using Danish Register Data at both the municipality and individual level. Due to the excellent quality of the Danish Register Data, we have the whole population, can link to family members, and have information on employment, education, income, etc. More specifically, we analyse the impact of networking and negative attitudes on education by considering how young immigrants' high school decision, which is not obligatory in Denmark, depends on the the number of people of their own nationality and negative attitudes towards immigrants in the area where they live relative to the impact on young natives. We examine this decision to attend high school as it is made at a young age, around 16, and individuals will usually be living at home while attending high school. The advantage of this is that the parents decide where to live and young immigrants and natives then decide whether to continue in high school. We therefore have that the household placement is, plausibly, exogenous for the person making the educational decision; that is the young immigrants and natives are not both deciding where to live and deciding whether to attend high school.

Despite this, there are concerns that unobservable factors could drive parents, either in their emigration or subsequent relocation, to locate in order to give the young immigrant or native a better choice of high school prior to this high school decision. While we control for parental characteristics together with a variety of municipality controls, we address these potential omitted unobservable characteristics in two ways. Firstly, we allow for the possibility that individuals can relocate due to educational considerations by estimating, as a robustness check, the high school decision for only those who have not recently moved. Secondly, we directly examine the importance of unobservable characteristics compared to observables in explaining our estimated effects using

a procedure developed in Oster (2015). While there are reasons to believe that concerns over the importance of unobservables may be mitigated due to the timing of the decision to attend high school, we are able to directly quantify how our estimates change depending on the degree of omitted variable bias due to unobservables.

In the macro-econometric level analysis, we exploit the panel nature of our data to control for unobserved time-invariant factors which affect the fraction of young individuals attending high school. We find positive, but imprecisely estimated, effects of negative attitudes on the fraction of immigrants attending high school. We see little impact of networking on high school attendance, and turn to analysis at the individual level to not only more precisely estimate an individual's potential network but also take into account important family and individual level factors.

At the micro-econometric level we see a positive and significant impact of negative attitudes on male immigrants' probability to attend high school and positive, but less precise effects for females. We see no effects, either positive or negative, for natives. We find that networking matters for immigrants, but indications that the quality of an immigrant's potential network matters for males while only the quantity matters for females. These results on negative attitudes, and to a lesser extent networking, are robust to the exclusion of households who have moved recently, within the past 3 or 6 years. Under reasonable assumptions about the importance of unobservables, we are able to bound the estimated effect for males away from zero; that is we can state with a good deal of confidence that even when accounting for potentially correlated unobservables, negative attitudes have a positive impact on high school attendance for male immigrants. Lastly, we see that the negative attitudes measure matters only for 1st generation immigrants and not for 2nd generation immigrants, who are likely more assimilated and less likely to be adversely impacted by negative attitudes. Overall, our empirical findings are consistent with the second case of the theoretical model, where negative attitudes are prevalent only in the low-skilled sector and more severe negative attitudes increase the incentives of immigrants to acquire education.

The paper is structured as follows. In section 2 the model is setup, then the following sections consider the impact of negative attitudes towards immigrants and the fraction of immigrants. In Section 6 we consider heterogenous networking effects. Sections 7 and 8 provide a macro-econometric and a micro-econometric analysis. Section 9 explores the robustness of the micro-econometric results, and Section 10 concludes.

2 The Model

We consider a search and matching model with natives, N and immigrants, I, which may be educated with productivity y^h or non-educated with productivity, y^l where $y^h > y^l$. The workers search for jobs and firms search for workers and the labour force is normalised at one. For simplicity, we assume that firms may supply vacancies directed towards natives or immigrants. We then include the two features, which may differ for immigrants and natives, influencing their labour market performance differently and thereby their educational decision - namely negative attitudes towards immigrants and networking effects.[4]

Immigrants may be harmed by negative attitudes towards them at their workplace, resulting in separation from the job. The reason may be many-fold: negative attitudes against immigrants may imply that a firm needs to deal with unexpected issues in the firm or with clients, and/or the immigrant voluntary quits. Hence, immigrants face a random negative shock. We therefore assume that the separation rate, s_i^m, $m = h, l$, $i = N, I$, may be increasing in negative attitudes towards immigrants, a^m, $m = h, l$, giving a separation rate for immigrants of $s_I^m = s_N (1 + a^m)$ and a separation rate for natives of $s_N^h = s_N^l = s_N$. Negative attitudes may (among other things) themselves be influenced by the fraction of immigrants in an area, an issue we will return to below.

On the other hand, more immigrants may make it easier to obtain employment through networking. We here follow Fontaine (2007) by assuming that networking, $\lambda_i^m, i = N, I$, $m = h, l$ is increasing in the number of people of the same origin as the individual. We assume that networking for high productivity immigrants and natives is given by: $\lambda_I^h = t^h \frac{I(1-\hat{e_I})}{(N+I)(1-\hat{e_I})} = t^h I$ and $\lambda_N^h = t^h \frac{N(1-\hat{e_N})}{(N+I)(1-\hat{e_N})} = t^h N = t^h (1 - I)$ as $N + I = 1$, and that networking for low productivity immigrants and natives is given by: $\lambda_I^l = t^l \frac{I\hat{e_I}}{(N+I)\hat{e_I}} = t^l I$ and $\lambda_N^l = t^l \frac{N\hat{e_N}}{(N+I)\hat{e_N}} = t^l N = t^l (1 - I)$ as $N + I = 1$, where $0 < t^m < 1$, $m = h, l$, and $\hat{e}_i$, $i = N, I$ is the number of low-educated workers and $1 - \hat{e}_i$, $i = N, I$, is the number of educated workers. One may argue that a very large number of own ethnicity may not be as important as a relative smaller number, a potential network may grow so big that it is not really a usually network in terms of employment perspectives. This could be included in the analysis by changing the functional form of the network variable, so that it is increasing in the number the worker's own nationality but at a decreasing rate. We will return to

[4] In Larsen and Waisman 2012, it is assumed that it is not possible for firms to direct their search to either immigrants or natives. Therefore, any negative impact on immigrants, will through changed vacancy supply also affect natives. As the present paper also include educational choice and networking we, for simplicity, keep this additional channel out of the present set-up.

this issue below.

2.1 Matching

We assume that firms advertise $V_i^m, i = N, I,\ m = h, l$ vacancies. Unemployment rates are given by $u_i^m, i = N, I,\ m = h, l$ and there are $L_i^m, i = N, I,\ m = h, l$ employees. Labour market tightness by the ethnic group is given by $\theta_i^m = (V_i^m + \lambda_i^m L_i^m)/u_i^m$, where the transition rate for an unemployed worker is given by $f(\theta_i^m)$ and for the firm it is $q(\theta_i^m)$. We assume that the worker transition rate is increasing in labour market tightness and at a decreasing rate, $\partial\left(f(\theta_i^m)\right)/\partial\theta_i^m > 0, \partial^2\left(f(\theta_i^m)\right)/\left(\partial\theta_i^m\right)^2 < 0$ and the firm's transition rate is decreasing in labour market tightness at a decreasing rate, $\partial\left(q(\theta_i^m)\right)/\partial\theta_i^m < 0$ and $\partial^2\left(q(\theta_i^m)\right)/\left(\partial\theta_i^m\right)^2 > 0$.

2.2 The Firm

The firm chooses the number of vacancies so as to maximise profits subject to negative attitudes towards immigrants and subject to networking effects. We assume, for simplicity, that firms can direct their search towards natives or immigrants and that each worker produces $y^m,\ m = h, l$ and receives the bargained wage, $w_i^m, i = N, I,\ m = h, l$. We denote the discount rate by ρ and hiring costs are increasing in productivity, $ky^m,\ m = h, l$. A firm chooses the number of vacancies to advertise, $V_i^m, i = N, I,\ m = h, l$ and takes into account that its employees also produce applicants through networking. Each firm hiring natives or immigrants solves the following Bellman equation:

$$\rho\Pi_i(L_i^m) = max\left(y^m L_i^m - w_i^m - ky^m V_i^m + \Pi_i(L_i^m)\right), i = N, I,\ m = h, l, \qquad s.t. \tag{1}$$

$$\dot{L}_N^m = (\lambda_N^m L^m + V_N^m)q(\theta_N^m) - s_N L_N^m,\ m = h, l, \tag{2}$$

$$\dot{L}_I^m = (\lambda_I^m L^m + V_I^m)q(\theta_I^m) - s_I^m L_I^m,\ m = h, l. \tag{3}$$

Firms choose their optimal number of employees, using two methods of search: advertising by the firm or networking, which happens at the rate $\lambda_i^m L_j^m f(\theta_i^m),\ i = N, I$. Separation rates for immigrants, $s_I^m = s_N\left(1 + a^m\right) \geq s_N$, which depend on negative attitudes, $a^m,\ m = h, l$ may differ for low productivity and high productivity workers. Hence, matches between immigrants

and the firm may be dissolved more often than matches for natives and also may differ for high- and low-educated workers. This implies that, for given networking, the expected profitability of a firm employing natives may be different than the expected profitability of employing a high- and/or low-educated immigrant.

With identical firms, using equations (1)-(3) and Kuhn-Tucker conditions, we obtain the non-trivial solution in the steady state determining labour market tightness, $\theta_i^m,\ i = N, I, m = h, l$:

$$\frac{ky^m}{q(\theta_N^m)} = \frac{y^m - w_N^m}{\rho + s_N - \lambda_N^m q(\theta_N^m)}, \frac{ky^m}{q(\theta_I^m)} = \frac{y^m - w_I^m}{\rho + s_N(1 + a^m) - \lambda_I^m q(\theta_I^m)}. \tag{4}$$

The partial equilibrium results are the following: more severe negative attitudes, a higher a^m, will tend to reduce labour market tightness and more networking, a higher λ_i^m, will raise labour market tightness for the firm hiring the specific type, for either immigrants or natives.

2.3 The Worker

Let U_i^m be the value of being an unemployed worker and $E_i^m, m = h, l,\ i = N, I$ be the value of being an employed worker. The values are determined by

$$\rho U_i^m = f(\theta_i^m)(E_i^m - U_i^m) - \Gamma(m)\, c(e_i), i = N, I,\ m = h, l, \tag{5}$$

$$\rho E_I^m = w_I^m + s_I^m(U_I^m - E_I^m) - \Gamma(m)\, c(e_i), m = h, l \tag{6}$$

$$\rho E_N^m = w_N^m + s_N(U_N^m - E_N^m) - \Gamma(m)\, c(e_i), m = h, l. \tag{7}$$

We assume that workers have different abilities, e_i, and therefore different costs of obtaining education, $c(e_i)$. The variable e_i is uniformly distributed, $e_i \in [0, 1]$ where educational costs are decreasing in ability at a decreasing rate, $c'(e_i) < 0, c''(e_i) > 0$. In order to guarantee a non-trivial solution where some, but not all, individuals choose to acquire education, the individual with the highest ability faces a very low cost of education, $c(1) = 0$, and the individual with the lowest ability level face very high costs of education, i.e. $lim_{e_i \to 0} c(e_i) = \infty$. $\Gamma(m), m = h, l$, is an indicator function, taking the value zero if the worker does not acquire education and one, if the worker acquires education. Hence, $\Gamma(h) = 1$ and $\Gamma(l) = 0$.[5]

[5]We assume that the educational cost is a cost to acquire and maintain education or skills. This is a simplifying

2.4 Wages

We assume that wages are determined by Nash bargaining and that the bargaining power is a half, so that $X_i^m = E_i^m - U_i^m$, $i = N, I$, $m = h, l$, where from equation (4) we have that $X_i^m = ky^m / \left(q(\theta_i^m)\right) = \left(y^m - w_i^m\right) / \left(\rho + s_i - \lambda_i^m q(\theta_i^m)\right)$. We assume that the hiring cost parameter, k, is equal across firms, but that productivity and therefore actual hiring costs are higher for firms employing educated workers. This gives that $ky^m = X_i^m q(\theta_i^m)$ and thereby

$$X_i^m = \frac{y^m - w_i^m + \lambda_i^m k y^m}{(\rho + s_i^m)},\ m = h, l. \tag{8}$$

Subtracting equation (5) from equation (6) or (7) and then using $X_i^m = E_i^m - U_i^m$ and (8) give the wage equations

$$w_N^m = 0.5 \cdot y^m \left(1 + \left(\lambda_N^m + \theta_N^m\right) k\right), \tag{9}$$

$$w_I^m = 0.5 \cdot y^m \left(1 + \left(\lambda_I^m + \theta_I^m\right) k\right). \tag{10}$$

We note that wages are increasing in labour market tightness, networking and productivity. Substituting for wages into the equation determining labour market tightness, we obtain the equations for labour market tightness (8) as a function of parameter values and independently of productivity as hiring costs are a function of productivity:

$$k(\rho + s_I^m)2 = \left(1 - \theta_I^m k + \lambda_I^m k\right) q(\theta_I^m), \tag{11}$$

$$k(\rho + s_N)2 = \left(1 - \theta_N^m k + \lambda_N^m k\right) q(\theta_N^m). \tag{12}$$

We note the following. Regarding relative separation rates we have that, if the separation rate of both high and low productivity immigrants is greater than the separation rate of natives $s_I^m > s_N$, then the left hand side of (11) is larger than the left hand side of (12) tending to reduce labour market tightness for firms employing immigrants and thereby the transition rate for immigrants. Considering networking, labour market tightness is increasing in labour networking: $d\theta_i^m / \left(d\lambda_i^m\right) = kq(\theta_i^m)/D_i^m > 0$, $i = N, I$, $m = h, l$, where $D_i^m = -\left(\left(1 - \theta_i^m k + \lambda_i^m k\right) q'(\theta_i^m) - \theta_i^m k q(\theta_i^m)\right) > 0$. If networking is higher for immigrants than natives, $\lambda_I^m > \lambda_N^m$, this tends to increase θ_I^m relatively

assumption and is not important for the results. The assumption enables us to use a model without having workers continuously being born and dying. Such a model would deliver similar qualitative expressions.

to θ_N^m. However, if $s_I^m > s_N$ this tends to increase θ_N^m relatively to θ_I^m. Therefore, if there the separation rate is greater for immigrants than for natives, $s_I^m > s_N$, and networking for natives is greater than or equal to networking for immigrants, $\lambda_I^m \leq \lambda_N^m$, then the labour market tightness for natives is higher than labour market tightness for immigrants, $\theta_I^m < \theta_N^m$. If networking among immigrants is greater than networking among natives, $\lambda_I^m > \lambda_N^m$, and $s_I^m > s_N$ then the relative size of labour market tightness is ambiguous.

For the rest of the theoretical analysis we assume that educated and uneducated workers face the same networking effect, hence $\lambda_i^h = \lambda_i^l = \lambda_i$, $i = N, I$. With this assumption we obtain that labour market tightness is the same for high and low-educated natives, $\theta_N^h = \theta_N^l = \theta_N$ whereas we have two scenarios for immigrants. In the first case, negative attitudes is present for both high and low productivity workers and hence $s_I^h = s_I^l = s_I$ resulting in $\theta_I^h = \theta_I^l = \theta_I$. In the second case, negative attitudes exist for educated workers only and hence $s_N = s_I^h < s_I^l$ resulting in $\theta_I^h > \theta_I^l$. This assumption allows us to consider the impact of a change in attitudes and immigration on labour market tightness, education and unemployment, without making any assumptions about the relative importance of networking for educated or uneducated workers. We will in Section 6 below discuss how the results are modified in the case of heterogeneous networking effects. We have the following result.

Result: In case 1, where negative attitudes are present in both the high and low productivity sector, $a^h = a^l > 0$, and networking of natives is larger than or equal to networking of immigrants, $\lambda_N \geq \lambda_I$ then labour market tightness for natives is higher than labour market tightness for immigrants, $\theta_N > \theta_I$, and natives' wages are thus higher than immigrants' wages, $w_N^m > w_I^m$.

In case 2, when negative attitudes are present in the low productivity sector only, $a^l > a^h = 0$, and networking of natives is larger than or equal to networking of immigrants, $\lambda_N \geq \lambda_I$, then: (i) for low productivity workers, labour market tightness for natives is higher than labour market tightness facing immigrants, $\theta_N^l > \theta_I^l$, and low productivity natives' wages are thus higher than low productivity immigrants' wages, $w_N^l > w_I^l$, and (ii) for high productivity workers, labour market tightness and wages of natives and immigrants are equal, $\theta_N^h = \theta_I^h$ and $w_N^h = w_I^h$.

When networking of natives is less than networking of immigrants, $\lambda_N < \lambda_I$, then the relative sizes of labour market tightness, θ_N^m and θ_I^m, and wages, w_N^m and w_I^m, for natives and immigrants are indeterminate.

Note that given the assumption above that $\lambda_I = tI$ and $\lambda_N = t(1 - I)$, where $0 < t < 1$ we

have that $\lambda_N > \lambda_I$ as long as $I < 0.5$, which is the most realistic case. In case the networking function takes another form, namely if it is increasing in the number of the worker's own ethnicity but at a decreasing rate, for example, $\lambda_I = tI^{1/2}$ and $\lambda_N = t(1-I)^{1/2}$, we will still have that $\lambda_N > \lambda_I$ as long as $I < 0.5$, but the impact of an additional labour force participant is larger for immigrants than natives as long as immigrants are the minority.

2.5 Education

When individuals decide on whether to educate or not, they compare the value of acquiring education to the value of remaining uneducated. That is, at each point in time, as an unemployed worker, they compare the value of being unemployed as a educated worker to the value of being unemployed as an uneducated worker. Workers with high educational costs find it too costly to obtain education, whereas high ability workers and low educational costs individuals find it more than worthwhile to do so. The marginal worker has the ability level, $\hat{e_i}, i = N, I$, which makes the worker just indifferent between acquiring education or remaining uneducated. For simplicity, we assume that natives and immigrants are identical with respect to the distribution of educational costs. We write the condition determining the educational costs of the marginal worker as

$$\rho U_i^h(\hat{e_i}) = \rho U_i^l, \ i = N, I. \tag{13}$$

The higher $\hat{e_i}$ is, the higher is the ability level of the marginal worker acquiring education. Hence, fewer workers acquire education, and a smaller fraction of the workers will be educated. Use equations (5)-(7) and (13), the bargaining condition together with the free entry condition, to obtain the following simplified condition in the first case where $a^h = a^l$ for immigrants and $a = 0$ for natives:

$$\left(y^h - y^l\right)\theta_i k = c(\hat{e_i}), i = N, I. \tag{14}$$

Equation (14) gives $\hat{e_i}, i = N, I$ as a function of the endogenous labour market tightness variables, $\theta_i, i = N, I$. The higher the productivity difference is, the higher are wage differences, and then the more people will acquire higher education. For equal networking rate, labour market tightness facing natives is higher than labour market tightness facing immigrants, which results in that natives acquire more education than immigrants, that is, $\hat{e}_I > \hat{e}_N$.

In the second case, the result changes for immigrants whereas the natives' educational decision

is still given by equation (14), i.e. when $a^h = 0$ and $a^l > 0$ then we obtain:

$$\left(y^h \theta_I^h - y^l \theta_I^l\right) k = c\left(\hat{e_I}\right). \tag{15}$$

In this case, with equal networking rate for all workers, we now obtain that $\hat{e}_N > \hat{e}_I$ as low productivity immigrants are worse of than natives in terms of a lower transition rate into a job, $\theta_I^l < \theta_N^l$ and lower wages and high productivity immigrants have the same wages and employment probability as natives, $\theta_I^h = \theta_N^h$. Hence, due to that the uneducated immigrants are relative worse of than natives, immigrants in this case experience stronger incentives for acquiring education that natives. This is summarised in the following result.

Result: In case 1, where negative attitudes are present in both the high and low productivity sector, $a^h = a^l > 0$, and networking of natives is equal to networking of immigrants, $\lambda_N = \lambda_I$, natives acquire more education than immigrants, that is, $\hat{e}_I > \hat{e}_N$.

In case 2, where negative attitudes are present in the low productivity sector only, $a^l > a^h = 0$, and networking of natives is equal to networking of immigrants, $\lambda_N = \lambda_I$, then immigrants acquire more education than natives $\hat{e}_I < \hat{e}_N$.

Notice here the significance of the networking assumption. In section 6 below we discuss the impact of including heterogeneity and we discussed the nonproportionality of the networking function above.

2.6 Unemployment

In equilibrium, inflows are equal to outflows. The equilibrium flows characterising the labour market for workers are then, $f\left(\theta_i^m\right)\mu_i^m = s_i^m n_i^m, i = N, I$, $m = h, l$, and $n_i^h + \mu_i^h = \left(1 - \hat{e}_i\right) i$, $i = N, I$, $n_i^l + \mu_i^l = \hat{e}_i i$, $i = N, I$, where employment is n_i^m, $i = N, I$, $m = h, l$ and unemployment is $\mu_i^m, i = N, I$, $m = h, l$. The labour force is normalised to one, $N + I = 1$, giving the following expression for natives' unemployment rates: u_N^m, $m = h, l$: $u_N^h = u_N^l = u_N = s_N / \left(f\left(\theta_N\right) + s_N\right)$, as $\theta_N^h = \theta_N^l$. In the first case, we have the separation rate for high and low productivity immigrants is equal, $s_I^h = s_I^l$, and hence labour market tightness is equal, $\theta_i^h = \theta_i^l$, then unemployment rates are the following:

$$u_i^h = u_i^l = u_i = \frac{s_i}{f\left(\theta_i\right) + s_i},\ i = N, I. \tag{16}$$

Unemployment rates for educated workers are equal to unemployment rates of uneducated workers. This results stems from the assumption that hiring costs are proportional to productivity.

In the second case, where the separation rate of low productivity immigrants is higher than that of high productivity immigrants, $s_I^h < s_I^l$ as $a^h = 0$ and $a^l > 0$, then $s_I^l = s_N(1 + a^l) > s_I^h = s_N$ and therefore$f\left(\theta_I^l\right) < f\left(\theta_I^h\right) = f\left(\theta_N\right)$ which results in the following unemployment rates

$$u_N = u_I^h < u_I^l = \frac{s_I}{f\left(\theta_I^l\right) + s_I},\ i = N, I. \tag{17}$$

The result is the following.

Result: In case 1, where negative attitudes are present in both the high and low productivity sector, $a^h = a^l > 0$, and networking of natives is larger than or equal to networking of immigrants, $\lambda_N \geq \lambda_I$, the unemployment rate of natives is smaller than the unemployment rate of immigrants, $u_N < u_I$.

In case 2, where negative attitudes are present in the low productivity sector only, $a^l > a^h = 0$, and networking of natives is larger than or equal to networking of immigrants, $\lambda_N \geq \lambda_I$, then the unemployment rate of low productivity immigrants is larger than that of high productivity immigrants and the unemployment rate of high productivity immigrants is greater than or equal to that of natives, $u_I^l > u_I^h \geq u_N$.

When $\lambda_N < \lambda_I$ then the relative sizes of the unemployment rates facing natives and immigrants, u_N and u_I are indeterminate.

3 Negative Attitudes

In this section, we examine what happens to labour market tightness, wages, education and unemployment when immigrants face more severe negative attitudes. For simplicity, we consider the case where $\lambda_N = \lambda_I$. The impact on labour market tightness, wages and unemployment as well as education will differ dependent on whether negative attitudes towards immigrants exists in both sectors or in the low productivity sector only. We have the following proposition.

Proposition: In case 1 where negative attitudes are present for both high and low productivity workers, $a^h = a^l > 0$, then when negative attitudes increase, immigrants' wages fall and their unemployment rates increase. Education of immigrants is also reduced.

In case 2, when only low productivity workers face negative attitudes, $a^l > a^h = 0$, then wages for low productivity immigrants fall and their unemployment rate increases, whereas high productivity immigrants are not affected, increasing education for immigrants. There is no impact on natives.

Proof: see Appendix A.

In the first case, where $a^h = a^l = a$, an increase in negative attitudes increases the separation of immigrants and therefore makes it less profitable to open a vacancy. The reduction in labour market tightness for immigrants reduces their bargaining power and thereby their wages. Immigrants' transition rate falls which together with their higher separation rate increases their unemployment rate. Concerning educational choice, the impact depends on the impact on employment perspectives for high productivity workers relatively to the impact on low productivity workers. The reduced employment perspectives, through lower employment chances and lower wages, affect both high productivity and low productivity workers. However, due to higher productivity, the reduction in wages is going to be larger for high productivity workers than for low productivity workers and therefore the incentives to acquire education fall. The result is that fewer immigrants acquire education. As negative attitudes have no impact on the separation rate of natives, they are not affected.

For the second case, that is, where $a^l > a^h = 0$, an increase in negative attitudes only increases the separation of low productivity workers and only for the low productivity firms hiring immigrants, there is a reduction in the profitability of opening a vacancy. The resulting reduced labour market tightness for low productivity firms hiring immigrants increases uneducated immigrants' unemployment rate. High productivity immigrants are not affected as their separation rate is not affected. When we turn to educational choice, the result changes compared to in case 1. The employment perspectives for high productivity workers are not affected and as the employment perspectives of low productivity workers worsens, and the incentives to acquire education increase. In this case, we therefore obtain the opposite result compared to in case 1, namely that more immigrants acquire education. Again, as negative attitudes have no impact on the separation rate of natives, they are not affected.

As a caveat, notice, that we could allow for the possibility that negative attitudes affect the value of being unemployed also directly, and not only indirectly through wages and employment chances. In this case, the impact on unemployment will not be affected, but if, in case 1, negative

attitudes directly diminish the value of being unemployment equally for uneducated and educated workers, then there is no impact on education. In case 2, the direct impact will also, as the indirect through employment and wages, tend to increase education.

4 Immigration

In this section, we examine the impact on labour market tightness, wages, education and unemployment from more immigration. Notice that $\lambda_I = tI$ and $\lambda_N = tN = t(1-I)$. The impact on labour market tightness, wages and unemployment as well as education will differ dependent on whether negative attitudes towards immigrants exists in both sectors or in the low productivity sector only. We have the following proposition.

Proposition: When the fraction of immigrants increases, the unemployment rate of immigrants falls and their wages increase. The improved labour market prospects of immigrants raise their level of education in both case 1 and case 2 and the opposite holds for natives.

Proof: see Appendix A.

More immigrants will induce the fraction of immigrants to increase, improving networking and thus labour market tightness for firms hiring immigrants and therefore immigrants' transition rate. Similarly, networking among natives fall, and thereby labour market tightness for natives falls. As networking both directly and indirectly has a positive impact on immigrants' wages, their wages increase whereas natives' wages fall. Furthermore, the increase in immigrant's transition rate reduces their unemployment rate and the corresponding reduction in natives' transition rate raise their unemployment rate. Finally, concerning education for immigrants, improved labour market conditions due to more networking are better for high productivity workers than low productivity workers, wherefore education increases.

As an illustration, consider the situation where $a = 0$ and hence $s_I = s_N$ and initially $N = I$. In this case, labour market tightness facing immigrants is equal to labour market tightness facing natives. The fraction of educated immigrants and natives are also identical, $\hat{e}_I = \hat{e}_N$ and thereby $c'(\hat{e}_I) = c'(\hat{e}_N)$. The increase in educated natives is therefore equal to the fall in the fraction of educated immigrants. However, a more realistic setup is where $N > I$ so that $\theta_N > \theta_I$ and thus $\hat{e}_I > \hat{e_N}$ (the fraction of natives acquiring skills is higher than the fraction of immigrants acquiring skills). In this case, $c(\hat{e}_I) < c(\hat{e}_N)$, and $|c'(\hat{e}_I)| > c'(\hat{e}_N)$, the impact through the lower

educational costs will increase the impact on education. However, substituting from equation (11) and (12) we obtain that the positive impact of networking is smaller for immigrants than the negative impact from networking for the natives, $|d\theta_I/dI| < d\theta_N/dI$. Hence, given $N > I$ initially, the impact from an increase in the number of immigrants on their educational level may be smaller or larger than the negative impact on the educational level facing natives.

5 Immigration and Negative Attitudes

In this section we expand the model by allowing for the possibility that a higher fraction of immigrants aggravates negative attitudes, giving for case 1, $s_I^h = s_I^l = s_N(1 + a(I))$ and for case 2, $s_I^h = s_N$ and $s_I^l = s_N(1 + a(I))$. The idea is that more immigrants around increases the possibility of a multi-ethnic society, which for some people is a negative development. As results now in general becomes ambiguous we consider the special case where the matching function takes the form $X_I^m = \sqrt{v_I^m u_I^m}$ and that $a'(I) = 1$. The impact on natives is identical to the impacts above.

Proposition: Natives are affected as above. For immigrants we have the following. In the first case, differentiating equations (11), including the matching function, $X_I^m = \sqrt{v_I^m u_I^m}$ with respect to I where now $a(I)$ we obtain $\frac{d\theta_I}{dI} \mid_{a(I)} = \frac{-k\left(s_N a'(I)\sqrt{\theta_I} - t\right)}{\left(k(\rho + s_I^m)/\sqrt{\theta_I} + k\right)}$. *Substituting for the solution for labour market tightness we obtain the condition for* $a'(I) = 1$ *that* $\frac{d\theta_I}{dI} \mid_{a(I)} \gtreqless 0 \Leftrightarrow z \gtreqless I$, *where* $z = \frac{s_N(2\rho tk + s_N(1-2tk)) - t^2 k}{s_N^2 tk}$. *This implies that for case 1 we obtain* $\frac{dw_I^m}{dI} \mid_{a(I)} \gtreqless 0$ *and* $\frac{d\hat{e}_I}{dI} \mid_{a(I)} \gtreqless 0 \Leftrightarrow z \gtreqless I$, *and that* $\frac{du_I^m}{dI} \mid_{a(I)} > 0$ *for* $z \geq I$. *In the second case, we obtain where* $da^l/dI = 1$ *that* $\frac{d\theta_I^l}{dI} \mid_{a^l(I)} = \frac{-k\left(s_N\sqrt{\theta_I^l} - t\right)}{k(\rho + s_I^l)/\sqrt{\theta_I^l} + k}$ *and* $\frac{d\theta_I^h}{dI} \mid_{a^l(I)} = \frac{tk}{k(\rho + s_I^l)/\sqrt{\theta_I^l} + k} > 0$. *For wages we have* $\frac{dw_I^l}{dI} \mid_{a^l(I)} \gtreqless 0$ $z \gtreqless I$, $\frac{dw_I^h}{dI} \mid_{a^l(I)} > 0$, *and education increases,* $\frac{d\hat{e}_I}{dI} \mid_{a^l(I)} < 0$ *and unemployment increases if* $\frac{du_I^l}{dI} \mid_{a(I)} > 0$ *for* $z \geq I$, *and* $\frac{du_I^h}{dI} \mid_{a(I)} < 0$.

The impact of immigration on labour market performance for immigrants now becomes ambiguous. The reason is that more immigration improves networking and thereby employment chances and wages, but at the same time, negative attitudes may become more severe which reduce labour market tightness again. In the first case, where $a^h = a^l = a(I) > 0$, the positive impact through networking on labour market tightness is more important than the negative impact through increased negative attitudes if the fraction of immigrants is sufficiently high. The condition for a positive sign for labour market tightness is dependent on the separation rate and the networking

effect, so that in this case, the separation rate has to be low, low s, relatively to the networking effect, high t. In the second case, $a^h = 0$, $a^l = a(I) > 0$, high productivity workers are not affected and low productivity workers are effected as in case 1, implying that education unambiguously increases, as the relative gain of acquiring education increases.

6 Heterogeneous Networking Effects

In this section we allow the networking effects to differ for uneducated and educated workers as well as for natives and immigrants.[6] First, we consider the case where $s_I^m > s_N$, $m = h, l$, which results in the left hand side of (11) being larger than the left hand side of (12) and therefore tends to reduce labour market tightness for firms employing immigrants and thereby the transition rate for immigrants. Therefore, when immigrants face more networking than natives, $\lambda_I^m > \lambda_N^m$, we cannot determine the relative size of θ_I^m and θ_N^m as this networking effect would tend to increase labour market tightness for immigrants relative to natives. For the rest of this section we therefore consider the case where networking for immigrants is lower than networking for natives, $\lambda_N^m > \lambda_I^m$, $m = h, l$.

Regarding education, we now need to consider the more general equation, allowing labour market tightness to differ both for educated and uneducated natives as well as immigrants :

$$\left(y^h\theta_i^h - y^l\theta_i^l\right)k = c\left(\hat{e}_i\right),\ i = N, I. \tag{18}$$

To begin with, we assume that networking is the same for educated and uneducated immigrants and consider first the case where $\lambda_N^h > \lambda_N^l$. In this case, educated natives are more efficient using the network and we obtain that $\theta_N^h > \theta_N^l > \theta_I^h = \theta_I^l = \theta_I$, resulting in higher wages for educated native workers than uneducated native workers, (see equation (9)), who then in turn, as before, receive higher wages than immigrants (see equation (9) relative to (10) inserting for labour market tightness and networking). Furthermore, considering education, using equation (18) we obtain that a higher fraction of natives than immigrants acquire education, $\hat{e}_I > \hat{e}_N$, as $y^h\theta_I^h - y^l\theta_I^l < y^h\theta_N^h - y^l\theta_N^l$ if and only if $y^l\left(\theta_N^l - \theta_I^l\right) < y^h\left(\theta_N^h - \theta_I^h\right)$ as there is a larger gain involved for natives than immigrants acquiring education.

If instead, uneducated native workers are better at networking than educated natives workers,

[6] As we do not allow networking effects to depend on the number of each educational type (as then labour market tightness would be a function of $\hat{e}_i$, $i = N, I$) then this corresponds to assuming that t^m is different for the two different educational types.

$\lambda_N^h < \lambda_N^l$, then there are relative more vacancies supplied towards uneducated native workers than educated native workers and hence, $\theta_N^l > \theta_N^h > \theta_I^h = \theta_I^l = \theta_I$. In this case, the order of educated native wages and uneducated native wages become ambiguous as the higher productivity of educated natives will tend to raise w_N^h relative to w_N^l whereas the higher networking effect for uneducated natives both directly and indirectly through a higher labour market tightness will tend to increase w_N^l relative to w_N^h. In terms of education, we cannot tell whether $\hat{e}_I > \hat{e}_N$ or $\hat{e}_I \leq \hat{e}_N$ as $y^l < y^h$ but $\theta_N^l - \theta_I^l > \theta_N^h - \theta_I^h$ as $\theta_N^l > \theta_N^h$ and $\theta_I^h = \theta_I^l = \theta_I$. This is the case as good networking for uneducated natives means that being uneducated tends to be more attractive for natives, but on the other hand, as networking is still better for educated natives than immigrants, this will tend to increase the number of educated natives.

Next, we allow the networking variable to vary also for immigrants. First, we consider the case where uneducated immigrants are more efficient using their network, that is, $\lambda_I^l > \lambda_I^h$. When $\lambda_N^h = \lambda_N^l$ then $\theta_N^h = \theta_N^l > \theta_I^l > \theta_I^h$, implying that natives are better paid but we cannot tell whether the uneducated immigrants or the educated immigrants earn the most, as the higher networking and labour market tightness for uneducated workers compared to educated workers tends to raise wages for this group but the latter group of immigrants has a higher productivity than the former. In this case, $\hat{e}_I > \hat{e}_N$ as $\theta_N^h = \theta_N^l$ and $\theta_I^l > \theta_I^h$ implying that uneducated immigrants are relative better off than educated immigrants in terms of transition into work. When instead $\lambda_N^h > \lambda_N^l$ then $\theta_N^h > \theta_N^l > \theta_I^l > \theta_I^h$, the conclusion concerning relative wages for natives is as above when we considered the same relative networking effects for natives and indeterminate for immigrants. We also have that $\hat{e}_I > \hat{e}_N$ as the uneducated immigrants are relative better off than educated immigrants in terms of transition into work and the reverse holds for natives, making the impact even stronger. Finally, when $\lambda_N^l > \lambda_N^h$ then $\theta_N^l > \theta_N^h > \theta_I^l > \theta_I^h$. Here the relative sizes of low and educated wages are ambiguous for both natives and immigrants and we cannot determine the relative skill levels for natives and immigrants as we do not know the relative size of $\theta_N^l - \theta_N^h$ and $\theta_I^l - \theta_I^h$.

When there are more educated immigrants than uneducated immigrants, that is, $\lambda_I^h > \lambda_I^l$, then $\theta_I^h > \theta_I^l$, and relative labour market tightness for natives and thereby wages will vary as above dependent on the relative size of λ_N^h and λ_N^l. Here, we know that $w_I^h > w_I^l$ as networking, labour market tightness and productivity move in the same direction. We cannot determine whether a higher fraction of immigrants or natives acquire education as we do not know the relative size of

$\theta_N^l - \theta_N^h$ and $\theta_I^l - \theta_I^h$.

In the second case, when negative attitudes exist for low productivity workers only, that is when, $s_N = s_I^h < s_I^l$, then if networking is the same for uneducated and educated immigrants, but higher for educated natives than uneducated natives, i.e. $\lambda_N^h > \lambda_N^l > \lambda_I^h = \lambda_I^l$, then we obtain $\theta_N^h > \theta_N^l > \theta_I^h > \theta_I^l$ and the wage order is similar and the order of unemployment rates is $u_N^h < u_N^l < u_I^h < u_I^l$. We cannot determine the relative size of $\hat{e}_I$ and $\hat{e}_N$ unless we know the relative size of $\theta_N^h - \theta_I^h$ and $\theta_N^l - \theta_I^l$. If instead, uneducated native workers have a better network than educated natives workers, $\lambda_N^h < \lambda_N^l$, then there are relative more vacancies supplied towards uneducated native workers than educated native workers and hence, $\theta_N^l > \theta_N^h > \theta_I^h > \theta_I^l$. As above with equal separation rates for immigrants, we cannot determine the relative size of immigrant wages and education, as $y^l < y^h$ but $\theta_N^l - \theta_I^l > \theta_N^h - \theta_I^h$.

When uneducated immigrants have a better network than educated, that is, $\lambda_I^l > \lambda_I^h$, then when $\lambda_N^h = \lambda_N^l$ we obtain that $\theta_N^h = \theta_N^l$ but we cannot determine the relative size of θ_I^l and θ_I^h, something which still holds when $\lambda_N^h > \lambda_N^l$ where for natives we now obtain $\theta_N^h > \theta_N^l$ and we can still not determine the relative size of labour market tightness for immigrants. Finally, when $\lambda_N^l > \lambda_N^h$ then $\theta_N^l > \theta_N^h$ and the relative size of θ_I^h and θ_I^l remain ambiguous. Relative wages follow the order of labour market tightness for natives and are indeterminate for immigrants. We cannot determine whether $\hat{e}_I > \hat{e}_N$ or $\hat{e}_I \leq \hat{e}_N$ as we do not know the relative size of θ_I^h and θ_I^l.

When educated immigrants are more efficient using their network than uneducated, that is, $\lambda_I^h > \lambda_I^l$ then we obtain the same relative labour market tightness for educated and uneducated natives as above as well as wages. In none of these situations, we can determine the relative skill levels for natives and immigrants as we do not know the relative size of $\theta_N^l - \theta_N^h$ and $\theta_I^l - \theta_I^h$. See Table A1 in Appendix B for a detailed summary of the results.

7 Empirical Analysis

In this section, we test the model predictions regarding the impact of networking and negative attitudes on education for 1st and 2nd generation immigrants.[7]

[7]In what follows, we use the terms 1st and 2nd generation immigrants and immigrants interchangably.

7.1 Data

We consider data for Denmark in 2002 and examine both the variation between municipalities as well as educational decisions at the individual level. Denmark is chosen for two reasons. Firstly, there is detailed and rich data available both at the municipality and individual level (where we have data for the whole population) which enables the impact of negative attitudes on education to be examined both at the macro and micro level. Secondly, the educational structure of Denmark allows us to examine an individual's decision of whether to attend high school, which is non-compulsory, after the completion of compulsory education at a young age. In Denmark, students' first year in high school will be when they are 15-16 years old and only the first 9 years of schooling are obligatory.[8] As this decision takes place at such a young age, mobility of the student is of little concern, which is discussed in further detail below. While high school is optional, most of the students beginning high school will graduate with a high school degree. Moreover, in this paper we aim to evaluate whether the potential impact of negative attitudes and immigration on relative labour market performance for educated and uneducated workers influence the decision to begin high school.

In 2002, there are 275 municipalities in Denmark. 2002 is chosen as there is a general election in 2001 and it is well prior to 2007 when the 275 municipalities are merged into 98 municipalities. The fundamental idea behind using macro data is to potentially show some correlation between the general prevalence of negative attitudes in a municipality and the expected return to education, through employment perspectives and the fraction of immigrants attending high school. The impact may thus be both direct and indirect, and in this sense we may capture something different than if we were to only consider micro data. Furthermore, the macroeconomic nature of the theoretical model, makes the macro-econometric analysis a natural starting point.

The fundamental idea behind using micro data is we are able to precisely take into account many individual and household factors which may impact an individual's educational decision. In order to examine the relationship between immigrant high school attendance and negative attitudes towards immigrants and networks at the individual level, we use detailed Danish Register Data made available by Statistics Denmark. Danish Register Data is a database containing detailed information on every resident of Denmark from 1980-present. The data is interlinked across various government and administrative sources by an anonymous personal identification number, so indi-

[8] Only recently a grade zero has become obligatory.

viduals are also observed overtime. The data used contains similar, but more detailed information compared to the macro-econometric data and is composed of: education history (information such as where an individual attends school and what qualification they are studying towards and have already achieved); demographic information such as gender, age, and municipality of residence; immigration history (including an individual's nationality, exact date of immigration, and whether an individual is a 1st or 2nd generation immigrant; and household characteristics such as family composition and parental information. As individuals are linked to their parents, it is possible to include factors such as parental education, employment history, and marital status, all of which will likely affect an immigrant's high school decision.

Our main dependent variable is an immigrant's high school attendance in 2002. In the macro-econometric analysis, this is measured by the fraction of immigrants attending high school in the municipality while in the micro-econometric section this is measured by whether or not an individual is attending high school. We measure negative attitudes using the fraction of votes for two political parties, Fremskridtspartiet and Dansk Folkeparti, due to their emphasis on reducing immigration.[9] One potential concern in using voting data is that immigrants may not vote for parties who emphasise reducing immigration. This would be problematic for our measure of negative attitudes as municipalities with higher concentrations of immigrants could actually have few votes for Fremskridtspartiet and Dansk Folkeparti. However, the voting behaviour of immigrants is unable to influence our measure of negative attitudes as only natives are permitted to vote in general elections. As such, the voting data is a good measure of the negative attitudes of natives against immigrants. Furthermore, the 2001 general election campaign had a huge emphasis of immigration, implying a large increase in the votes for Fremskridtspartiet and Dansk Folkeparti.

For the network variable we consider two possible variables: (1) the fraction of immigrants residing in a municipality and (2) the fraction of immigrants residing in a municipality who are educated and/or employed. The first of these measures captures whether the quantity of individuals

[9]Fremskridtpartiet's webpage: http://www.fremskridtspartiet.dk/page9.html (all in Danish: main idea: immigrants may stay for shorter or longer periods).

Dansk Folkeparti's webpage (http://www.danskfolkeparti.dk/ The_Party_Program_of_the_Danish_Peoples_Party): *Denmark is not an immigrant-country and never has been. Thus we will not accept transformation to a multiethnic society.*

Denmark belongs to the Danes and its citizens must be able to live in a secure community founded on the rule of law, which develops along the lines of Danish culture.

It ought to be possible to absorb foreigners into Danish society provided however, that this does not put security and democratic government at risk. To a limited extent and according to special rules and in conformity with the stipulations of the Constitution, foreign nationals should be able to obtain Danish citizenship.

Other Danish parties may also be interested in limiting immigration but not to such an extent that it is on their official webpage.

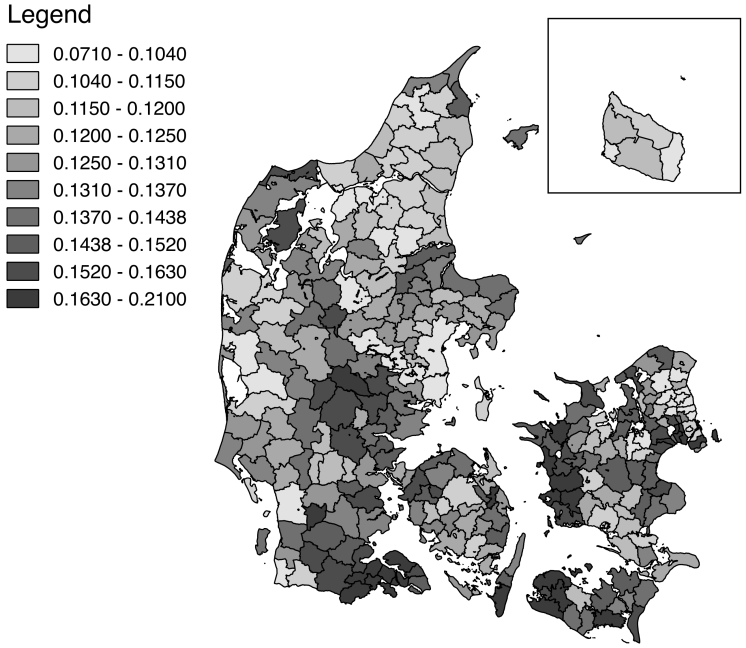
matter for networking while the second captures whether the quality of individuals matter. In the macro-econometric analysis, this second measure is captured by the fraction of Western/non-Western immigrants in employment. Throughout the micro-econometric analysis, we construct exact sums of immigrants from a certain nation residing in every municipality. By interacting these sums with an immigrant's own nationality, we recreate an individual's potential network based on the fraction of the population living in his municipality from his homeland (excluding the immigrant him or herself). Using education and employment data, we are able to construct the education and employment levels of immigrants by nationality for each municipality. From theory it is not evident which measure to choose and while exposure to immigrants may impact an individual's educational decision, it is plausible that exposure to educated or employed immigrants may have a far greater impact on an individual's educational decision. Even though the employment and educated fractions are considered closest to our theoretical model presented above, we include both networking measures for completeness. When the regressions are run for natives, we translate these fractions into the fraction of natives.

Figures 1 and 2 show the dispersion of the fraction of votes for Fremskridtspartiet and Dansk Folkeparti and the fraction of 1^{st} and 2^{nd} generation immigrants residing in the municipality. Votes for the two parties are relatively scattered across Denmark, with a high concentration of municipalities with a large fraction of votes for both parties near the Danish/German border. Immigrants are also scattered across Denmark, with the exceptions that they tend to reside closer to large cities (Aarhus, Odense, and the greater Copenhagen area) as well as near the Danish/German border.

7.2 Identification

In order to disregard mobility issues, we examine the high school decision as a function of immigration and attitudes as well as other explanatory variables. Our focus on the high school decision is important for our identification strategy as the parents decide where to live and young immigrants and natives then decide whether to continue to high school. As they are young when this decision is made, they will usually stay at home when attending high school. As such, the validity of our empirical analysis relies on two assumptions. Firstly, that young children, rather than their parents, are making the decision of whether to attend high school or not. As high school starts later in life than, for example, in the USA, students will be more mature and involved in their own education.

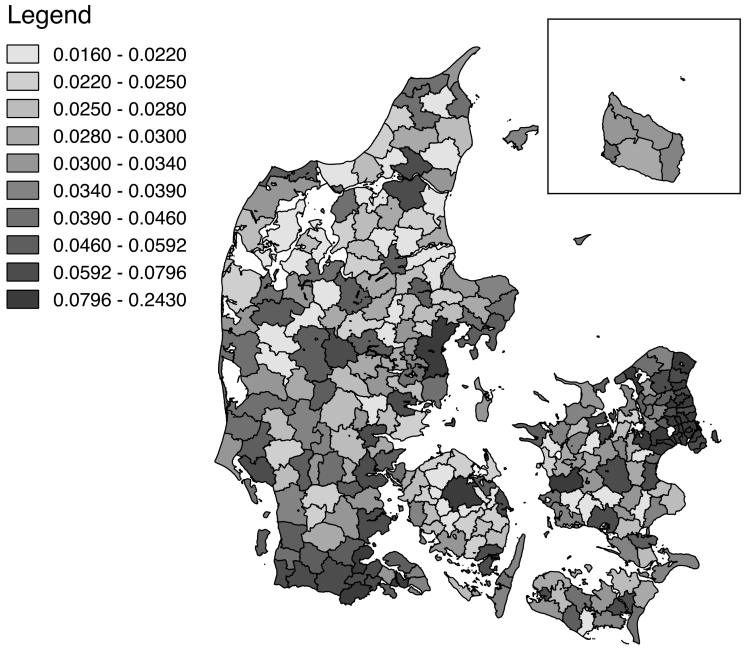
Figure 1: Municipal Fractions of Votes for Fremskridtpartiet and Dansk Folkeparti in 2001 General Election



This is especially true in our case as in Denmark, the young person, rather than parent, is heavily involved with the school in the education process at the time of deciding to attend high school - it is, for example the young person having the code needed in order to access the online application form and fill in the application form and not the parent. Secondly we assume, that parents, both in their emigration and subsequently, do not selectively locate into municipalities. These imply that the household placement is exogenous for the young person making the educational decision.

There are a number of potential threats to these assumptions, particularly the second assumption. Firstly, parents may move in order to place their child in a specific municipality before the high school decision is made. We examine this possibility by, as a robustness check for our micro-econometric analysis, only considering the high school decision for families who have not moved recently. Secondly, high ability and resourceful parents may either move or emigrate to better municipalities, and these parents may also have high ability children. We overcome this problem by controlling for parental characteristics together with a host of municipality level controls. These high ability parents may also move in response to negative attitudes. Lastly, emigration as well as moving away from or towards certain municipalities could be driven by unobserved parental characteristics, and these unobservable characteristics could be correlated with our negative attitudes and networking measures as well as the child's educational decision. In the micro-econometric

Figure 2: Municipal Fractions of Immigrants Residing in Municipality



analysis, the last two concerns is partially addressed by the robustness check excluding those who have recently moved. However, this does not completely solve the unobservables problem, as the decision of staying and locating initially is also endogenous. As such, we investigate the importance of unobservables and examine the sensitivity of our results to the omitted variable bias problem using a procedure developed in Oster (2015) which tests the proportional importance of unobservables in relation to observables. In the macro-econometric analysis below, we address these concerns by exploiting the panel nature of our data and using a municipal fixed effects specification. We employ data from 1999 in order to account for any unobservable time-invariant factors at the municipal level which affect our measure of high school attendance. Additionally, a dynamic panel data specification allows us to comment on the theoretical predictions of Sections 3 and 4, which examine changes in the values of negative attitudes and immigrants.

7.3 Macro-econometric Model and Analysis

Table 1 presents descriptive statistics of the municipal data. While the total number of municipalities in Denmark during the time period is 275, we drop 27 municipalities as there were no 16 year old immigrants residing in them in either 2002 or 1999.[10] This leaves us with 248 municipalities

[10]The municipalities we drop are: Hvalsø, Fuglebjerg, Gørlev, Høng, Holeby, Højreby, Ravnsborg, Rudbjerg, Egebjerg, Haarby, Langeskov, Marstal, Sydlangeland, Tommerup, Tranekær, Aarup, Gram, Blåvandshug, Hinnerup, Nørhald, Samsø, Fjends, Sallingsund, Læsø, Sejlflod, Sindal, Åbybro.

for our regressions. From Table 1, it is seen that the average share of 16 year old immigrants in high school is about 45.8% while it is larger for natives, 74%.

Table 1: Summary Statistics at the Municipal Level

	(1) Mean	(2) Median	(3) Std Dev	(4) Min	(5) Max
Immigrants Aged 16 in Any HS (%)	45.8%	50.0%	27.0	0.0%	100%
Natives Aged 16 in Any HS (%)	74.0%	74.2%	5.9	54.9%	88.2%
Votes Both Parties (%)	13.2%	13.0%	2.3	7.1%	21.0%
Network for Immigrants (% of immigrants)	5.0%	3.9%	3.3	2.0%	25.2%
'Network' for Natives (% of natives)	95.0%	96.1%	3.3	74.8%	98.0%
Western Immigrants Employed (%)	95.0%	95.5%	3.2	81.8%	100.0%
Non Western Immigrants Employed (%)	89.1%	90.9%	9.0	27.3%	100.0%
Natives Employed (%)	96.5%	96.8%	1.3	90.4%	98.5%
Gross Income Per Capita/100000	1.732	1.677	0.195	1.448	2.771
Population Density/1000	0.285	0.073	0.846	0.021	10.413
LF Short Tertiary Education (%)	40.1%	40.2%	3.4	27.1%	50.3%
LF Medium Tertiary Education (%)	10.7%	10.2%	2.8	6.3%	24.2%
LF Long Tertiary Education (%)	3.2%	2.3%	2.9	0.7%	20.4%
Observations			248		

Our negative attitudes measure has an average value of 13.2%. The first network measure for immigrants (fraction of immigrants) is on average 5% and the two elements of the second network measure for immigrants (fraction of employed immigrants from Western and non-Western countries respectively) are on average 95% and 89.1%. It is not surprising that the employment ratio for Western immigrants is larger than for non-Western immigrants. The average (of average) gross income level per capita is 173200 DKK and the average population density is 285 people per square kilometre. Population density is included to account for the degree of urbanisation of a municipality. The average share of the population with a short, medium and long education are respectively 40.1%, 10.7% and 3.2%. When comparing the mean values and the median values of all variables we see that in some cases – especially for variables involving immigrants – the distributions are a bit skewed.

As stated above, we want to disregard mobility taken by the individual due to different labour market conditions or attitudes. We therefore estimate the high school decision at the municipal

level using a fixed effects model:

$$(1 - \hat{e}_{rt}) = \beta_0 + \beta_1 a_{rt} + \beta_2 \lambda_{rt} + \sum_{\eta} \beta_{rt\eta} Controls_{rt\eta} + \gamma_t + c_r + \varepsilon_r, \tag{19}$$

$$r = 1, ..., 266 \; t = 1999, 2002.$$

The left hand side variable, $(1 - \hat{e}_{rt})$, is either the fraction of young 16 years old immigrants attending high school in year t (our main group of interest) or, for control purposes, the same fraction for natives in municipality r. Ideally, we would expect both the attitude and the network variables to be significant for immigrants while being insignificant for natives. We examine whether negative attitudes, a_{rt}, and networking, λ_{rt}, have any impact on the fraction of young immigrants (16 year olds) attending high school. For networking, we consider two potential measures for λ_{rt} to assess whether the quantity of individuals or the quality of individuals matters: (1) the fraction of immigrants residing in a municipality (quantity) and (2) the fraction of immigrants residing in a municipality who are employed (quality). The inclusion of both year fixed effects (γ_t) and municipality fixed effects (c_r) control for differences in the fraction of 16 year olds attending high school over time and for municipality specific time-invariant characteristics. As additional control variables, we include gross income per capita in the municipality, population density, and the percentage of the labour force (LF) with short, medium and long tertiary education.

Table 2 presents estimation results of equation 19, where columns (1) and (2) use the fraction of natives/immigrants as a networking measure for natives and immigrants respectively, while columns (3) and (4) use the fraction of natives/immigrants employed for natives and immigrants respectively. Across all specifications, we find no significant effects of negative attitudes on the school choice for either immigrants or natives. For immigrants, these effects are positive, but never significant, while for natives, these effects are close to zero. This is contrary to the expectations of our theoretical model, which predicts a negative impact of negative attitudes on immigrant education in case 1 and a positive impact in case 2. For networking, the first measure indicates a negative and (marginally) significant effect for immigrants and natives, while using the fractions employed, arguably a better network in relation to our theoretical model, shows a marginally significant positive effect for immigrants and no effect for natives. The overall weak effects found may be due to the fixed effect estimation which uses variation within municipalities. However, as the fixed effect approach is necessary to best handle the potential endogeneity problem, the results

at the macro level fail to provide much insight into the predictions of our theoretical model.

Table 2: High School Participation for Immigrants and Natives at the Municipal Level

	(1) Frac Native Enrolled HS	(2) Frac Imm Enrolled HS	(3) Frac Native Enrolled HS	(4) Frac Imm Enrolled HS
% Votes Both Parties	-0.10	0.88	-0.10	0.74
	(0.22)	(0.87)	(0.22)	(0.87)
% Native	-2.44**			
	(1.20)			
% Immigrants		-14.42*		
		(7.33)		
% Natives Employed			-0.03	
			(0.65)	
% Western Imm Employed				0.57
				(0.93)
% Non Western Imm Employed				0.40*
				(0.23)
Controls?	Yes	Yes	Yes	Yes
R^2	0.234	0.076	0.195	0.082
N	248	248	248	248

Robust standard errors are reported in parentheses. Negative attitudes are measured as the share of votes for the Fremskridtspartiet or Dansk Folkeparti. Not reported are controls for gross income per capita, population density, and the fraction of the labour force with short, medium, and long tertiary education. Vocational education is included in the short tertiary category. Medium education also includes bachelor degrees. An intercept is included in the model but not reported in the table. * p<0.10, ** p<0.05, *** p<0.01.

The natural next step is therefore to further examine the possibility of a positive networking effect for immigrants when using the fraction of immigrants employed and the positive, but insignificant, effect of negative attitudes using micro data. Moving to the individual level enables many individual and family level factors to be taken into account which cannot be at the municipal level. We are also able to more precisely measure an individual's potential network using information about their home country. Hence in the next subsection we model the educational choice of the individual young immigrants, and again natives for comparison, based on information about negative attitudes and networking possibilities in their municipality.

7.4 Micro-Econometric Model and Results

Using individual level data on 16 year old individuals living in Denmark in 2002, Table 3 presents summary statistics of all relevant variables included in the estimation. As before we see that immigrants on average attend high school less than natives. Our negative attitudes measure has a similar average value as the macro section, 12.8%.

Table 3: Summary Statistics at the Individual Level

	(1)
Votes Both Parties (%)	12.74%
	(2.41)
Munc. Unemployment Rate (%)	5.21%
	(1.50)
Munc. Population Density/1000	0.7098
	(1.5072)
Munc. Gross Income Per Cap/100000	1.7592
	(0.20569)
Native (%)	93.99%
	(23.77)
1st Generation Immigrant (%)	3.82%
	(19.18)
2nd Generation Immigrant (%)	2.19%
	(14.63)
Natives Aged 16 in Any HS (%)	74.24%
	(0.00)
Imm./Desc. Aged 16 in Any HS (%)	52.65%
	(0.00)
Male (%)	51.06%
	(49.99)
Mother's Education	12.55
	(2.92)
Father's Education	12.33
	(3.04)
Parents Married (%)	68.53%
	(0.46)
Father Employed (%)	84.85%
	(35.86)
Mother Employed (%)	82.99%
	(37.57)
Household Income/100000	6.2022
	(5.0702)
High School in Municipality (%)	72.41%
	(44.7)
Observations	53256

Mean values shown for 16 year old individuals residing in Denmark in 2002 unless otherwise indicated. Standard deviations in parentheses.

We estimate the following equation separately for natives and immigrants:

$$(1-\hat{e}_i) = \beta_0 + \beta_1 a_r + \beta_2 \lambda_r + \beta_3 ParentEdu_p + \sum_{\mu} \beta_{r\mu} MuncControls_{r\mu} + \sum_{\eta} \beta_{i\eta} HHControls_{i\eta} + Origin_i + \varepsilon_i, \quad (20)$$

where $(1-\hat{e}_i)$ is the educational decision of individual i represented by a dummy variable if an individual is attending any high school or not, which is determined by: a_r, negative attitudes captured by the fraction of votes for both Fremskridtspartiet and Dansk Folkeparti in municipality r; λ_r, potential networking of same nationality individuals residing the same municipality r; $ParentEdu_p$, the years of education of parent p where $p = mother, father$; $MuncControls_r$, municipal factors such as population density and the fraction of immigrants/natives unemployed which may affect an individuals education decision; $HHControls_i$, additional household controls such as parental employment status in December of the previous year, total household income, and parental marital status; $Origin_i$, origin country dummies that capture educational differences across specific immigrant home countries; and ε_i, residual unobservables which are clustered at the municipality level. As in the macro-econometric estimation, we use separate measures of networking in order to assess whether the quantity or quality of an individual's potential network matters where for the former, networking is measured by the fraction of own nationality individuals residing in the same municipality r where for the latter, networking is measured by both the fraction of own nationality individuals residing same municipality r who have (at least) a high school education and the fraction who are employed. As before, in order to identify the effects for immigrants, we separately estimate equations (20) for natives and for immigrants.

In the micro section, we also consider the high school choice of both 1st and 2nd generation immigrants in one measure. We do so in order to explore how the effects of negative attitudes on education depend on an individual's gender, as sample sizes when combining the two groups are sufficiently large.[11] Tables 4 and 5 present results for males and females. Columns (1) and (2) present results for natives and immigrants respectively using only the municipal fraction of own nationality individuals as a measure of networking. Columns (3) and (4) use the detailed networking measures of educated and employed own nationality individuals.

[11] Section 8.3 provides a comparison of the high school choice of 1st and 2nd generation immigrants.

Table 4: High School Participation for Male 16 Year Olds by Immigrant Status

	(1) Native - Any HS Ongoing	(2) Immigrant - Any HS Ongoing	(3) Native - Any HS Ongoing	(4) Immigrant - Any HS Ongoing
% Votes Both Parties	0.063	1.483***	-0.028	1.275**
	(0.178)	(0.568)	(0.156)	(0.551)
Frac. of Own Nat. Educated			-0.104	0.141
			(0.093)	(0.150)
Frac. of Own Nat. Employed			0.087	0.468***
			(0.179)	(0.167)
Frac of Own Nat.	0.092	-1.115		
	(0.150)	(0.724)		
Mother's Education	0.014***	0.010***	0.014***	0.010***
	(0.001)	(0.003)	(0.001)	(0.003)
Father's Education	0.016***	0.006*	0.016***	0.006*
	(0.001)	(0.003)	(0.001)	(0.003)
Parents Married	0.064***	0.113***	0.064***	0.113***
	(0.007)	(0.034)	(0.007)	(0.033)
Father Employed	0.053***	0.081***	0.053***	0.074***
	(0.009)	(0.028)	(0.009)	(0.028)
Mother Employed	0.102***	0.082***	0.102***	0.068**
	(0.009)	(0.025)	(0.009)	(0.026)
Household Income/100000	0.003**	0.014**	0.003**	0.012*
	(0.001)	(0.006)	(0.001)	(0.006)
% Natives Unemployed	-0.304		-0.252	
	(0.277)		(0.396)	
% Western Imm Unemployed		-0.136		-0.050
		(0.636)		(0.625)
% Non Western Imm Unemployed		-0.032		0.094
		(0.217)		(0.211)
Population Density/1000	-0.004	0.009*	-0.004	0.007
	(0.004)	(0.006)	(0.003)	(0.005)
High School in Municipality	0.016*	0.063	0.017**	0.036
	(0.008)	(0.039)	(0.008)	(0.038)
Origin Country Dummies?	No	Yes	No	Yes
R^2	0.064	0.186	0.064	0.192
N	25526	1669	25526	1669

Standard errors reported in parentheses clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). * p<0.10, ** p<0.05, *** p<0.01.

Table 5: High School Participation for Female 16 Year Olds by Immigrant Status

	(1) Native - Any HS Ongoing	(2) Immigrant - Any HS Ongoing	(3) Native - Any HS Ongoing	(4) Immigrant - Any HS Ongoing
% Votes Both Parties	0.021	0.869	-0.373**	1.184**
	(0.226)	(0.590)	(0.173)	(0.576)
Frac. of Own Nat. Educated			-0.368***	-0.101
			(0.115)	(0.174)
Frac. of Own Nat. Employed			0.173	0.175
			(0.207)	(0.193)
Frac of Own Nat.	0.501***	1.824*		
	(0.162)	(1.029)		
Mother's Education	0.014***	0.010***	0.014***	0.010***
	(0.001)	(0.003)	(0.001)	(0.003)
Father's Education	0.014***	0.007**	0.014***	0.007**
	(0.001)	(0.003)	(0.001)	(0.003)
Parents Married	0.067***	0.045	0.066***	0.049
	(0.008)	(0.038)	(0.008)	(0.039)
Father Employed	0.077***	0.062**	0.076***	0.061**
	(0.009)	(0.025)	(0.009)	(0.025)
Mother Employed	0.111***	0.075***	0.111***	0.074***
	(0.008)	(0.026)	(0.008)	(0.027)
Household Income/100000	0.006**	-0.000	0.006**	-0.000
	(0.003)	(0.007)	(0.003)	(0.007)
% Natives Unemployed	-0.839***		-0.829*	
	(0.303)		(0.433)	
% Western Imm Unemployed		-1.351**		-1.376**
		(0.589)		(0.603)
% Non Western Imm Unemployed		0.153		0.198
		(0.221)		(0.222)
Population Density/1000	0.002	-0.000	-0.001	0.000
	(0.003)	(0.004)	(0.003)	(0.005)
High School in Municipality	0.014	0.154***	0.016*	0.159***
	(0.009)	(0.043)	(0.009)	(0.044)
Origin Country Dummies?	No	Yes	No	Yes
R^2	0.081	0.235	0.081	0.233
N	24528	1533	24528	1533

Standard errors reported in parentheses are clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). * p<0.10, ** p<0.05, *** p<0.01.

Examining males in Table 4, networking significantly increases the propensity of an immigrant to attend high school, where a one percentage point increase in the fraction of own nationality immigrants employed residing in the municipality would lead to a 0.47 percentage point increase in the probability of attending any high school. No equivalent significant effects are seen for natives, a finding which is consistent with networking amongst immigrants. The other networking variables, the fraction of own nationality individuals residing in the same municipality and the fraction of own nationality individuals with at least a high school education, are imprecisely estimated for both natives and immigrants. This is consistent with our theoretical model, where employment prospects

are key in determining the level of education an individual obtains, and it is reassuring that networking in terms of employed immigrants matters. The negative attitudes measure significantly increases an immigrant's probability of attending high school, where a 1 percentage point increase in the fraction of votes for either political party significantly increases the probability of attending any high school by 1.3-1.5 percentage points, while no effects are seen for natives. For both natives and immigrants, household controls matter a lot for an individual's propensity to attend high school, with education, employment, and marital status of both parents significantly increasing the probability of attending high school in all specifications.

For females, in Table 5, a different picture is seen. While the fraction of own nationality immigrants leads to a significant and positive increase in the probability of attending high school for immigrants, there are no significant effects, either positive or negative, of the fraction of own nationality immigrants who are educated or employed. For natives, a significant positive effect is seen for the fraction of natives while a significant negative effect is seen for the fraction of natives with education to high school or beyond. For female immigrants, negative attitudes increase high school attendance, but is imprecisely estimated depending on which networking measure is used. For natives, a similar pattern emerges when networking is measured as the fraction of natives residing in a municipality. Similar to the male estimation, parental education, employment, and marital status significantly increase the probability of attending high school for both natives and immigrants.

We find evidence that for males, negative attitudes towards immigrants increase the propensity of an immigrant to attend high school. For females, this effect is positive, but imprecisely estimated. For natives, no significant effects are seen for males nor for females. This is consistent with the macro-econometric analysis, which finds a positive, but insignificant, effect of negative attitudes on immigrant high school attendance. For male immigrants, networking seems to matter most in terms of employed immigrants of the same nationality, while for female immigrants, networking seems to matter in terms of the presence of any immigrants of the same nationality. The positive effect of negative attitudes on male immigrant education supports the second case of the theoretical model, where negative attitudes may have differential effects on immigrants propensity to attend high school depending on their productivity levels. In the case where high productivity workers are comparable to natives, as outlined in Section 3, negative attitudes only affect low productivity workers. This leads to lower employment perspectives for these low productivity workers, lowering

the future wages that young immigrants expect to receive and increasing the incentives of young immigrants to acquire education.

8 Robustness of Micro-econometric Results

We examine the robustness of our micro-econometric analysis in the following subsections. In particular, we examine how the mobility of immigrants within Denmark affect the results obtained in Section 7.4, how sensitive our results on male immigrants are to the presence of unobservables, and how negative attitudes affect 1st generation immigrants compared to 2nd generation immigrants.

8.1 Exploring Mobility of Immigrants

While the results presented in Section 7.4 are supportive of the second case of the theoretical model, it is possible our estimation fails to properly estimate the impact of negative attitudes on high school attendance. In particular, we focus on the educational decision of 16 year old individuals in order to disregard mobility concerns, as students of this age are likely to reside at home in this period. While this may be the case, it could be that parents either selectively locate to certain municipalities or move as a reaction to negative attitudes in a municipality. We examine the possibility that movers are driving the positive effect of negative attitudes we see for males in Appendix C by examining if our results are stable to restricting the sample to individuals who have resided in the same house for 3 or more years and 6 or more years. Similarly, we look at years since immigration for the non-native sample in order to see if recent immigrants, who could have selectively located within Denmark, are driving our results.

On the whole, the results presented in Appendix C are very similar to the main results. For males, the negative attitude variable always increases the propensity to attend high school and is of a similar magnitude for immigrants, while there is no effect seen for natives. This is true for both the 3 or more years restriction as well as the 6 or more years restriction. For females, the effects of negative attitudes for immigrants is insignificant across specifications when imposing the years since moved restriction, which is consistent with the positive but imprecise effects seen in Table 5. The networking variables are less robust where, for males, the fraction of own nationality immigrants educated now matters while the fraction employed no longer does. For females, there are no effects of networking on the propensity to attend high school.

A similar pattern is seen for estimation restricting the time since immigration for immigrants;

the positive estimated effect of negative attitudes on the propensity to attend high school remains for males and is imprecisely estimated for females.[12] The effect of negative attitudes on high school attendance is remarkably robust for male immigrants and seems to not be driven by mobility within Denmark, while the results on networking fluctuate excluding families who have recently moved, indicative of a less stable relationship between networking and high school attendance. For female immigrants, these relationships remain insignificant.

8.2 Unobservables and Observables

In the estimation reported up to this point, unobservables could be problematic in the sense that immigrant location could be driven by unobserved parental characteristics, and these unobservable characteristics could be correlated with the high school attendance of their children. In order to make statements beyond simple associations between negative attitudes and the decision to attend high school, we examine the influence of unobservables biasing our previous results.

Oster (2015) formalises a relationship between the association between selection on observables and unobservables and R-squared values to explain the potential importance of omitted variable bias. In the method, which builds upon the work of Altonji et al. (2005), movements in the coefficients and R-squared values identify the bias resulting from omitted variables. Formally, this assumes (i) a relationship between the covariance of the variable of interest, X, and observables and the covariance of X and unobservables (proportional selection, δ) and (ii) an R-squared from a hypothetical regression of the outcome of interest, Y, on X, observables, and unobservables (R_{max}) in order to quantify the ratio in the movements of the R-squared from a regression of Y on X ($\mathring{R}$), to a regression of Y on X and observables ($\tilde{R}$), to this R_{max} value. Under these two assumptions, the resulting omitted variable bias is a function of δ and R_{max}. When $\delta = 1$, unobservables and observables are equally important. When $\delta = 2$, selection on unobservables is twice as important as selection on observables. Both Altonji et al. (2005) and Oster (2015) suggest equal selection, when $\delta = 1$, as a reasonable bound. When $R_{max} = 1$, Y is fully explained by X and both observable and unobservable controls. As this may not be reasonable to assume,[13] Oster suggests a reasonable choice of $R_{max} = 1.3\tilde{R}$.

Two types of statements on the importance of unobservables result from these assumptions:

[12] 2nd generation immigrants, by definition, have "immigrated" when they are born, so all and 2nd generation immigrants are included in both the years since immigrated tables.

[13] For example, this may overstate the total explanatory power of these potential variables. If $R_{max} = 1$, high school attendance can be completely explained, implying no optimization errors in an individual's optimal schooling and zero measurement error in data.

(i) the degree of selection on unobservables compared to observables, δ, required to completely explain away the estimated effect of X on Y for a given R_{max} and (ii) statements of bounds of the estimated effect of X on Y for a given level of δ and R_{max}. We examine the sensitivity of the positive and significant effect of negative attitudes on high school attendance we see previously for males to unobservables in Table 6. Column (1) reports the raw effect of negative attitudes on immigrant's high school decision from a regression of Y on X. Column (2) is the controlled effect, including the observables, reported in column (4) of Table 4 and acts as an upper bound for the estimated effect of negative attitudes on high school attendance. The reported R-squared value corresponds to $\tilde{R}$.

Table 6: Importance of Unobservables and Omitted Variable Bias Male 16 Year Olds

	(1) Baseline Effect	(2) Controlled Effect	(3) Identified Set	(4) δ for $\beta = 0$	(5) Bias-adjusted β if $\delta = 2$	(6) Bias-adjusted β if $R_{max} = 1$ & $\delta = 1$
% Votes Both Parties	1.454**	1.275**	[1.214,1.275]	13.844	1.150	0.072
	(0.560)	(0.551)				
R^2	0.006	0.192				
N	1669	1669	1669	1669	1669	1669

Standard errors reported in parentheses clustered at municipality level. * p<0.10, ** p<0.05, *** p<0.01. R^2 reported in columns (1) and (2) correspond to $\mathring{R}$ and $\tilde{R}$ respectively in Oster (2015). Column (3) presents bounded set for effect of negative attitudes on high school attendance accounting for role of unobservables assuming $\delta = 1$ and $R_{max} = 1.3\tilde{R}$. Column (4) presents the level of selection on unobservables relative to observables required for estimated effect in column (2) to be zero. Column (5) presents the lower bound estimate of column (3) when $\delta = 2$. Column (6) presents the lower bound estimate of column (3) when $R_{max} = 1$. Calculations done using psacalc developed in Oster (2015).

Column (3) reports the bounded set taking the value from column (2) and the lower bound estimate assuming $\delta = 1$ and $R_{max} = 1.3\tilde{R}$ using the procedure developed by Oster (2015). The resulting bound suggests that, using reasonable values for the necessary assumptions, we can exclude the possibility that negative attitudes decrease high school attendance for male immigrants. Despite these relatively tight bounds, we can further investigate what properties, in terms of unobservables, would have to hold in order to explain away the entire positive effect of negative attitudes on education. This is crucial to examine in further detail as unobservables may have a major role in explaining the effects of negative attitudes on high school attendance seen previously. Column (4) reports the level of δ required for the effect of negative attitudes on high school attendance to be zero, and indicates that selection on unobservables would have to be far greater than selection on observables, nearly 14 times more important, in order for $\beta = 0$. Columns (5) and (6) present two additional results: the lower bound estimate from increasing δ from column

(3) from 1 to 2 and the lower bound estimate from increasing the R_{max} from column (3) to 1. Even if we assume that high school attendance can be fully explained by negative attitudes and both observables and unobservables, the effects of negative attitudes on high school attendance are still non-negative. The results of Table 6 indicate that for males, the effect of negative attitudes on high school attendance is bounded away from zero, and that negative attitudes do not decrease high school attendance. This effectively excludes the first case of our theoretical model, when negative attitudes affect all immigrants and decrease immigrant education, for male immigrants.

8.3 Comparing 1st and 2nd Generation Immigrants

Table A8 analyses the high school decision of 1st and 2nd generation immigrants separately, allowing the effects of negative attitudes on education to affect the two groups differently. While 1st and 2nd generation immigrants can both be impacted by negative attitudes, 2nd generation immigrants have been raised in Denmark and may have assimilated more compared to 1st generation immigrants. As we expect that 2nd generation immigrants may be less adversely impacted by negative attitudes, finding a larger impact of negative attitudes on 1st generation immigrants than on 2nd generation immigrants would support that the voting data captures negative attitudes while finding a larger impact for 2nd generation immigrants would raise concerns about the validity of this measure. In addition, we expect that networking may be more important to 1st generation immigrants, as they may be less assimilated. Due to the few numbers of 2nd generation immigrants in a given municipality, males and females are combined into one sample, and a dummy for male is included as a control variable. For the same reason, the results presented in Table A8 should be interpreted with some caution, as there are some municipalities with very few 2nd generation immigrants residing in them.

Estimation using only 1st generation immigrants with the different networking measures is reported in columns (1) and (3) of Table A8 while estimation on a sample of only 2nd generation immigrants is reported in columns (2) and (4). Comparing these two columns reveals that the positive impact of negative attitudes on high school attendance seen previously is driven by the impact of negative attitudes on 1st generation immigrants, while virtually no effect is seen on the probability of attending high school for 2nd generation immigrants. For networking, there is an effect seen for 1st generation immigrants, in terms of the fraction of own nationality immigrants employed, while there is no effect for 2nd generation immigrants. It is also reassuring that household

and municipality controls, which are not reported, are relatively similar for 1st and 2nd generation immigrants, as while negative attitudes may affect the two groups differently, there is less reason to believe that the two types of immigrants would be differentially affected by other controls.

9 Conclusion

We considered the impact of negative attitudes and immigration on educational choice of immigrants and natives. We did this theoretically and empirically.

Theoretically, we formulated a Becker-style taste discrimination model within a search and wage bargaining setting. We assumed that potential negative tastes towards immigrants implied that their separation rate from the job was higher than the separation rate of a native worker. Furthermore, we allowed for networking effects, which increased the probability of obtaining employment. We included endogenous education, where a higher expected income as educated in terms of both employment chances and wages relative to the expected income as uneducated, increases the number of educated workers. We considered two different cases. In the first case, discrimination existed for all immigrants, while it was only present in the sector employing uneducated workers in the second case. We found that an increase in negative attitudes reduced education for immigrants in the first case, increased education in the second case, where there was no impact on natives in the two cases. We also found that more immigration improved employment perspectives for immigrants and thereby increased the fraction of educated immigrants due to increased networking. Finally, we considered endogenous negative attitudes in the sense that more immigration increased negative attitudes. In this case, the impact of more immigration on the educational level of immigrants was ambiguous.

Empirically, we considered an immigrant's high school attendance as a function of the variables in the theoretical model. Exploiting the panel nature of the data at the macro-econometric level, we found positive, but insignificant, effects of negative attitudes on the fraction of immigrants attending high school. We found little of impact of networking, and turned to estimation at the individual level in order to more precisely measure an individual's potential network but also to account for important household level controls. At the micro-econometric level, we found positive impacts of negative attitudes on young immigrant's high school attendance which were significant for males, but less precisely estimated for females. We estimated positive impacts of networking

on high school attendance for immigrants, where the quality of networks mattered for males while only the quantity of individuals in a potential network mattered for females. Overall, our baseline empirical findings are consistent with the second case of the theoretical model, where negative attitudes are prevalent in the sector hiring low skilled workers and more severe negative attitudes increase the incentives to acquire education.

Considering high school attendance in the institutional context of the Danish education system allowed us to, plausibly, disregard mobility issues for the individual acquiring education. As these individuals usually live at home while attending high school, the parents decide where to live and the young immigrants and natives, placed by their parents in a particular municipality, decide whether to attend high school. We therefore obtain a situation were placement is plausibly exogenous for the person taking the educational decision. Despite this, we addressed concerns that unobservable factors could drive parents, either in their emigration or subsequent relocation, to locate in order to give the young immigrant or native a better choice of high school prior to this decision in two robustness checks. Firstly, we examined the possibility that parents could re-locate within Denmark in order to affect the young person's educational decision by estimating the effects of negative attitudes on high school attendance for young people who had not moved recently, in the last 3 and 6 years. Excluding these movers, we see very similar effects as our baseline individual level findings for the impact of negative attitudes and, to a lesser extent, networking. However, as the decision to stay is also endogenously determined we, secondly, directly assessed the importance of unobservable characteristics compared to observables in explaining our estimated effects of negative attitudes on high school attendance using a procedure developed in Oster (2015). While there are reasons to believe that concerns over the importance of unobservables may be mitigated due to the timing of the decision to attend high school, we were able to directly quantify how our estimates change depending on the degree of omitted variable bias due to unobservables. Doing so, we found that, under reasonable assumptions of the importance of unobservables, we were able to bound the estimated effect for males away from zero. As such, we could confidently assert that even accounting for potentially correlated unobservables that negative attitudes have a positive impact on high school attendance for male immigrants. We also saw that our findings are driven by 1^{st} generation immigrants rather than 2^{nd} generation immigrants, who are likely more assimilated.

If negative attitudes are most prevalent in the uneducated sector, and the second case of the

model is the most realistic one, negative attitudes cannot be the explanation behind the lower fraction of immigrants attending high school. Rather, negative attitudes towards immigrants may induce more immigrants to continue school beyond the 9th grade and thus improve their employment chances, wages, and expected lifetime income significantly. On the other hand, as female immigrants tend to benefit from networking with a quantity of individuals of their own nationality and male immigrants tend to benefit from networking with quality individuals of their own nationality, a low fraction of immigrants and quality immigrants in a municipality seems to partly explain the low fraction of female and male immigrant high school attendance. Hence, our results indicate that potential immigrant high school students have higher incentives to attend high school in areas where many of their own nationality or many of their own nationality who are employed live as it improves their networking chances. While we cannot exclude that variables other than negative attitudes and networking can explain the high school educational gap between immigrant and natives, the main empirical finding of this paper is that, at least for males, discrimination towards immigrants cannot explain the educational gap between immigrants and natives, as male immigrants tend to avoid discrimination by acquiring further education.

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Appendices

A Proofs of Theoretical Propositions

A.1 Proof for Negative Attitudes Proposition in Section 3

First case: Differentiating equations (11), (12), (10) and (9) with respect to $a^h = a^l = a$ *shows that there is a negative impact on labour market tightness and wages facing immigrants but no impact for natives* $\frac{d\theta_I}{da} = \frac{-k2}{D_I}\frac{ds_I}{da} < 0$, $\frac{d\theta_N}{da} = 0$, $\frac{dw_I^m}{da} = 0.5y^m\frac{d\theta_I}{da}k < 0$, $\frac{dw_N}{da} = 0$. *Concerning unemployment and education, we differentiate equations (16) and (14) with respect to a to obtain:*

$\frac{du_I}{da} = \frac{-s_I f'(\theta_I)}{(f(\theta_I)+s_I)^2}\frac{d\theta_I}{da} > 0$, $\frac{du_N}{da} = 0$, $\frac{d\hat{e}_I}{da} = \frac{\left(y^h - y^l\right)\frac{d\theta_I}{da}}{c'(\hat{e}_I)} > 0$, $\frac{d\hat{e}_N}{da} = 0$.

In the second case, the results for immigrants change to $\frac{d\theta_I^l}{da^l} = \frac{-k2}{D_I^l}\frac{ds_I^l}{da^l} < 0$, $\frac{d\theta_I^h}{da^l} = 0$, $\frac{dw_I^l}{da^l} = 0.5y^m\frac{d\theta_I^l}{da^l}k < 0$, $\frac{dw_I^h}{da^l} = \frac{dw_N}{da^l} = 0$. *Next, we differentiate equation (17) and (15) to obtain* $\frac{du_I^l}{da^l} = \frac{-s_I f'\left(\theta_I^l\right)}{\left(f\left(\theta_I^l\right)+s_I^l\right)^2}\frac{d\theta_I^l}{da^l} > 0$, $\frac{du_I^h}{da^l} = 0$, $\frac{d\hat{e}_I}{da^l} = \frac{-y^l\frac{d\theta_I^l}{da^l}}{c'(\hat{e}_I)} < 0$. *Q.E.D.*

A.2 Proof for Immigration Proposition in Section 4

For both cases: Differentiating equations (11), (12), (10) and (9) with respect to I *delivers a positive impact on labour market tightness for immigrants and a negative impact on natives:*

$\left(d\theta_I^m\right)/\left(dI\right) = \left(tkq(\theta_I^m)\right)/D_I^m > 0$, $m = h, l$, $\left(d\theta_N\right)/\left(dI\right) = ktq(\theta_N)/D_N < 0$,

$\left(dw_I^m\right)/\left(dI\right) = 0.5y^m\left(t + \left(d\theta_I^m\right)/\left(dI\right)\right)k > 0$, $\frac{dw_N^m}{dI} = 0.5y^m\left(-t + \left(d\theta_N^m\right)/\left(dI\right)\right)k < 0$.

Again for both case 1 and 2, we differentiate equations (16) and (17) with respect to I to obtain:

$$\frac{du_I}{dI} = \frac{-s_I f'\left(\theta_I\right)}{\left(f\left(\theta_I\right)+s_I\right)^2}\frac{d\theta_I}{dI} < 0,\ \frac{du_I^m}{dI} = \frac{-s_I^m f'\left(\theta_I^m\right)}{\left(f\left(\theta_I^m\right)+s_I^m\right)^2}\frac{d\theta_I^m}{dI} < 0,\ \frac{du_N}{dI} = \frac{-s_N f'\left(\theta_N\right)}{\left(f\left(\theta_N\right)+s_N\right)^2}\frac{d\theta_N}{dI} > 0.$$

Concerning education, for case 1, we differentiate equation (14) with respect to I *to obtain:*

$\left(d\hat{e}_I\right)/\left(dI\right) = \left(y^h - y^l\right)\left(d\theta_I/\left(dI\right)\right)/c'\left(\hat{e}_I\right) < 0$, $\left(d\hat{e}_N\right)/\left(dI\right) = \left(y^h - y^l\right)\left(d\theta_N/\left(dI\right)\right)/c'\left(\hat{e}_N\right) > 0$.

In the second case, the result for education for immigrants is:

$\left(d\hat{e}_I\right)/\left(dI\right) = \left(y^h\left(d\theta_I^h/\left(dI\right)\right) - y^l\left(d\theta_I^l/\left(dI\right)\right)\right)/c'\left(\hat{e}_I\right) < 0$, which is negative as $d\theta_I^h/\left(dI\right) > d\theta_I^l/\left(dI\right)$. *Q.E.D.*

B Summary of Heterogeneous Networking Effects

Table A1: Heterogeneous Networking Effects

Separation	Networking immigrants	Networking natives	Labour Market Tightness	Education
$s_I^l = s_I^h > s_N$	$\lambda_I^h = \lambda_I^l$	$\lambda_N^h > \lambda_N^l$	$\theta_N^h > \theta_N^l > \theta_I^l = \theta_I^h$	$\widehat{e}_I > \widehat{e}_N$
	$\lambda_I^h = \lambda_I^l$	$\lambda_N^h < \lambda_N^l$	$\theta_N^l > \theta_N^h > \theta_I^l = \theta_I^h$	ambiguous
	$\lambda_I^l > \lambda_I^h$	$\lambda_N^h \geq \lambda_N^l$	$\theta_N^h \geq \theta_N^l > \theta_I^l > \theta_I^h$	$\widehat{e}_I > \widehat{e}_N$
	$\lambda_I^l > \lambda_I^h$	$\lambda_N^h < \lambda_N^l$	$\theta_N^l \geq \theta_N^h > \theta_I^l > \theta_I^h$	ambiguous
	$\lambda_I^h > \lambda_I^l$	$\lambda_N^h \lessgtr \lambda_N^l$	$\theta_N^h \lessgtr \theta_N^l > \theta_I^h > \theta_I^l$	ambiguous
$s_I^l > s_I^h = s_N$	$\lambda_I^h = \lambda_I^l$	$\lambda_N^h > \lambda_N^l$	$\theta_N^h > \theta_N^l > \theta_I^h > \theta_I^l$	ambiguous
	$\lambda_I^h = \lambda_I^l$	$\lambda_N^h < \lambda_N^l$	$\theta_N^l > \theta_N^h > \theta_I^h > \theta_I^l$	ambiguous
	$\lambda_I^l > \lambda_I^h$	$\lambda_N^h \geq \lambda_N^l$	$\theta_N^h \geq \theta_N^l > \theta_I^h ? \theta_I^l$	ambiguous
	$\lambda_I^l > \lambda_I^h$	$\lambda_N^h < \lambda_N^l$	$\theta_N^l \geq \theta_N^h > \theta_I^h ? \theta_I^l$	ambiguous
	$\lambda_I^h > \lambda_I^l$	$\lambda_N^h \lessgtr \lambda_N^l$	$\theta_N^h \lessgtr \theta_N^l > \theta_I^h > \theta_I^l$	ambiguous

s_i^m corresponds to the job separation rate for high and low productivity, $m = h, l$, immigrants and natives, $i = N, I$. λ_I^m corresponds to networking for high and low productivity, for immigrants. λ_N^m corresponds to networking for high and low productivity, for natives. θ_i^m corresponds to the labour market tightness for high and low productivity, $m = h, l$, immigrants and natives, $i = N, I$. $\widehat{e}_i$ corresponds to education for immigrants and natives, $i = N, I$.

C Robustness Checks

C.1 3 or More Years Since Moved

Table A2: High School Participation for Male 16 Year Olds by Immigrant Status - 3 or More Years Since Moved

	(1) Native - Any HS Ongoing	(2) Immigrant - Any HS Ongoing	(3) Native - Any HS Ongoing	(4) Immigrant - Any HS Ongoing
% Votes Both Parties	0.115	1.680**	0.039	1.434**
	(0.188)	(0.697)	(0.168)	(0.696)
Frac. of Own Nat. Educated			-0.078	0.503**
			(0.096)	(0.237)
Frac. of Own Nat. Employed			0.038	0.015
			(0.175)	(0.228)
Frac. of Own Nat.	0.079	-1.815**		
	(0.159)	(0.810)		
Parental Education Controls?	Yes	Yes	Yes	Yes
Household Controls?	Yes	Yes	Yes	Yes
Municipality Controls?	Yes	Yes	Yes	Yes
Origin Country Dummies?	No	Yes	No	Yes
R^2	0.052	0.117	0.052	0.118
N	21861	1276	21861	1276

Standard errors reported in parentheses clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). Includes same parental education, household, and municipality controls as Table 4. * p<0.10, ** p<0.05, *** p<0.01.

Table A3: High School Participation for Female 16 Year Olds by Immigrant Status - 3 or More Years Since Moved

	(1) Native - Any HS Ongoing	(2) Immigrant - Any HS Ongoing	(3) Native - Any HS Ongoing	(4) Immigrant - Any HS Ongoing
% Votes Both Parties	0.082	0.612	-0.330*	0.862
	(0.234)	(0.573)	(0.178)	(0.552)
Frac. of Own Nat. Educated			-0.358***	0.019
			(0.118)	(0.210)
Frac. of Own Nat. Employed			0.110	0.120
			(0.201)	(0.218)
Frac. of Own Nat.	0.551***	1.258		
	(0.165)	(0.892)		
Parental Education Controls?	Yes	Yes	Yes	Yes
Household Controls?	Yes	Yes	Yes	Yes
Municipality Controls?	Yes	Yes	Yes	Yes
Origin Country Dummies?	No	Yes	No	Yes
R^2	0.062	0.153	0.062	0.152
N	20795	1171	20795	1171

Standard errors reported in parentheses clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). Includes same parental education, household, and municipality controls as Table 5. * p<0.10, ** p<0.05, *** p<0.01.

C.2 6 or More Years Since Moved

Table A4: High School Participation for Male 16 Year Olds by Immigrant Status - 6 or More Years Since Moved

	(1) Native - Any HS Ongoing	(2) Immigrant - Any HS Ongoing	(3) Native - Any HS Ongoing	(4) Immigrant - Any HS Ongoing
% Votes Both Parties	0.201	2.308***	0.115	2.000**
	(0.179)	(0.848)	(0.159)	(0.804)
Frac. of Own Nat. Educated			-0.042	0.525
			(0.095)	(0.356)
Frac. of Own Nat. Employed			-0.100	0.037
			(0.165)	(0.284)
Frac. of Own Nat.	0.140	-2.075**		
	(0.141)	(1.006)		
Parental Education Controls?	Yes	Yes	Yes	Yes
Household Controls?	Yes	Yes	Yes	Yes
Municipality Controls?	Yes	Yes	Yes	Yes
Origin Country Dummies?	No	Yes	No	Yes
R^2	0.045	0.109	0.045	0.108
N	18259	928	18259	928

Standard errors reported in parentheses clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). Includes same parental education, household, and municipality controls as Table 4. * p<0.10, ** p<0.05, *** p<0.01.

Table A5: High School Participation for Female 16 Year Olds by Immigrant Status - 6 or More Years Since Moved

	(1) Native - Any HS Ongoing	(2) Immigrant - Any HS Ongoing	(3) Native - Any HS Ongoing	(4) Immigrant - Any HS Ongoing
% Votes Both Parties	0.116	0.206	-0.282	0.371
	(0.254)	(0.639)	(0.202)	(0.626)
Frac. of Own Nat. Educated			-0.326**	-0.110
			(0.129)	(0.265)
Frac. of Own Nat. Employed			0.051	0.132
			(0.226)	(0.280)
Frac. of Own Nat.	0.582***	0.895		
	(0.190)	(1.030)		
Parental Education Controls?	Yes	Yes	Yes	Yes
Household Controls?	Yes	Yes	Yes	Yes
Municipality Controls?	Yes	Yes	Yes	Yes
Origin Country Dummies?	No	Yes	No	Yes
R^2	0.054	0.133	0.053	0.132
N	17435	853	17435	853

Standard errors reported in parentheses clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). Includes same parental education, household, and municipality controls as Table 5. * p<0.10, ** p<0.05, *** p<0.01.

C.3 3 or More Years Since Immigrated

Table A6: High School Participation for Immigrant 16 Year Olds - 3 or More Years Since Immigrated

	(1) Male - Any HS Ongoing	(2) Male - Any HS Ongoing	(3) Female - Any HS Ongoing	(4) Female - Any HS Ongoing
% Votes Both Parties	1.603***	1.508**	0.874	1.178**
	(0.601)	(0.598)	(0.561)	(0.554)
Frac. of Own Nat. Educated		0.500**		-0.067
		(0.218)		(0.226)
Frac. of Own Nat. Employed		0.105		-0.040
		(0.204)		(0.211)
Frac. of Own Nat.	-1.207*		1.515	
	(0.724)		(0.924)	
Parental Education Controls?	Yes	Yes	Yes	Yes
Household Controls?	Yes	Yes	Yes	Yes
Municipality Controls?	Yes	Yes	Yes	Yes
Origin Country Dummies?	Yes	Yes	Yes	Yes
R^2	0.132	0.137	0.155	0.154
N	1524	1524	1398	1398

Standard errors reported in parentheses clustered at municipality level. * p<0.10, ** p<0.05, *** p<0.01. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). Includes same parental education, household, and municipality controls as Table 4 and 5.

C.4 6 or More Years Since Immigrated

Table A7: High School Participation for Immigrant 16 Year Olds - 6 or More Years Since Immigrated

	(1) Male - Any HS Ongoing	(2) Male - Any HS Ongoing	(3) Female - Any HS Ongoing	(4) Female - Any HS Ongoing
% Votes Both Parties	1.669***	1.529**	0.643	0.882*
	(0.597)	(0.590)	(0.523)	(0.521)
Frac. of Own Nat. Educated		0.515**		-0.226
		(0.235)		(0.207)
Frac. of Own Nat. Employed		0.040		0.069
		(0.215)		(0.203)
Frac. of Own Nat.	-1.298*		1.324	
	(0.779)		(0.839)	
Parental Education Controls?	Yes	Yes	Yes	Yes
Household Controls?	Yes	Yes	Yes	Yes
Municipality Controls?	Yes	Yes	Yes	Yes
Origin Country Dummies?	Yes	Yes	Yes	Yes
R^2	0.118	0.122	0.135	0.134
N	1393	1393	1274	1274

Standard errors reported in parentheses clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). Includes same parental education, household, and municipality controls as Table 4 and 5. * p<0.10, ** p<0.05, *** p<0.01.

C.5 1st and 2nd Generation Immigrants

Table A8: High School Participation for 1st and 2nd Generation Immigrants

	(1) 1st Gen. - Any HS Ongoing	(2) 2nd Gen. - Any HS Ongoing	(3) 1st Gen. - Any HS Ongoing	(4) 2nd Gen. - Any HS Ongoing
% Votes Both Parties	1.575***	-0.035	1.652***	0.035
	(0.511)	(0.553)	(0.505)	(0.548)
Frac. of Own Nat. Educated			0.065	-0.077
			(0.111)	(0.256)
Frac. of Own Nat. Employed			0.344**	0.309
			(0.134)	(0.241)
Frac. of Own Nat.	0.919	0.474		
	(1.524)	(0.699)		
Male	-0.054**	-0.073**	-0.054**	-0.074**
	(0.024)	(0.029)	(0.023)	(0.029)
Parental Education Controls?	Yes	Yes	Yes	Yes
Household Controls?	Yes	Yes	Yes	Yes
Municipality Controls?	Yes	Yes	Yes	Yes
Origin Country Dummies?	Yes	Yes	Yes	Yes
R^2	0.230	0.096	0.234	0.097
N	2037	1165	2037	1165

Standard errors reported in parentheses clustered at municipality level. Any HS Ongoing corresponds to either enrollment in regular high school, business high school, or vocational training programs (apprenticeships). Includes same parental education, household, and municipality controls as Table 4 and 5. * p<0.10, ** p<0.05, *** p<0.01.

Conclusion

This Ph.D. thesis examined the determinants and social implications of education, crime, and job displacement. This was done using detailed Danish Register Data which provided the detail and comprehensiveness required for such analysis. The first chapter, "The Heterogeneous Effects of Education on Crime: Evidence from Danish Administrative Twin Data", assessed whether education is equally effective in lowering crime for all individuals, and found that education is most effective in lowering crime for males, for those of low educated households, and for the completion of high school. No effects of education on crime were seen for those of highly educated households, and education was found to lower crime irrespective of the level of crime an individual is exposed to during childhood. The results were robust to using data on incarcerations, to imputing the estimated effects of education on crime that would result from using a sample of identical twins, to excluding twins with large differences in education, and to directly estimating the reverse causality that exists between education and crime. The results found in Chapter 1 imply that these "at risk" individuals for whom education lowers crime obtain levels of education which are less than socially optimal, as they fail to consider the additional social benefits of education in terms of reduced crime.

The second chapter, "Job Displacement and Crime", examined the individual level relationship that exists between unemployment and crime, and found that individuals who lost their job in a sudden and unexpected mass-layoff event were significantly more likely than non-displaced individuals to engage in criminal activity after displacement, but not significantly different in the time leading up to displacement. These effects were found to be long lasting, and exist up to 7 years after displacement. These effects were found mainly for property crimes and to be concentrated at the lower end of the educational distribution, for those with less than high school education and for those with vocational education. The potential for an intergenerational effect of displacement on children's crime was examined, and revealed that, at best, father's displacement has a marginal short-run impact on son's crime. Large and long-lasting effects were found for those unmarried or living with no other adults. Job displacement impacted crime over and above what was explained by individual earnings losses, while weeks spent in unemployment explained substantially more of

the impact of displacement on crime. Our results were robust to a variety of specification changes and alterations to mass-layoff criteria, and suggest that both employers and employees fail to take into account the social costs of job separation, creating the potential for active public policy.

The third chapter, "Negative Attitudes, Network and Education", assessed potential explanations behind the educational gap existing between natives and immigrants. This was done both theoretically and empirically using two potential measures, negative attitudes towards immigrants and networking. Theoretically, we determined that negative attitudes can decrease the levels of education obtained by immigrants if negative attitudes affect all types of immigrants, while if they affect only low-skilled immigrants, negative attitudes can increase the levels of education of immigrants who educate in order to improve their employment prospects. Empirically, we analyzed an immigrant's educational choice at the macro- and micro-levels, and found a positive impact of negative attitudes on educational attainment, particularly for male immigrants. We found suggestive evidence of a positive impact of networking on education which differed across genders. We determined that, under reasonable assumptions about the importance of unobservables, while these unobservable factors do explain some of our results, we were able to eliminate the possibility that negative attitudes have a negative impact on immigrant education. As such, negative attitudes are determined to not explain the educational gap between immigrants and natives as they motivate immigrants to obtain more education, while a lack of networking could explain some of this gap.

Overall, this thesis emphasizes that taking into account specific factors in and the social implications of individual-level decisions matter greatly, both in terms of heterogeneity and social spillovers. This thesis has shown that these externalities can have meaningful impacts both on the social costs and benefits of public expenditure. As such, accounting for heterogeneity and externalities is necessary not only for the sake of drawing accurate conclusions about the relevant factors assessed in this thesis but also for public policy to be designed effectively.

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