

Essays on Educational Decisions, Academic Performance, and Inequality of Educational Opportunity

Joan E. Madia
Nuffield College
Department of Sociology
University of Oxford

A thesis presented for the degree of

Doctor of Philosophy in Sociology

Trinity Term 2022

Word count: 54,101

Essays on Educational Decisions, Academic Performance, and Inequality of Educational Opportunity

Joan E. Madia
Nuffield College
Department of Sociology
University of Oxford

A thesis presented for the degree of Doctor of Philosophy in Sociology
Trinity Term 2022

Thesis Abstract

This thesis investigates the determinants of inequality of educational opportunity (IEO) in the comprehensive educational systems of England and Sweden. Reducing IEO remains a pressing problem for industrialized countries with democratic regimes. Despite major governmental efforts for widening participation in post-compulsory education, it is still possible to observe substantial socioeconomic gaps in educational achievements and attainments. Sociologists analysing IEO tend analytically to separate 'performance' from 'choice' effects. Performance effects are all those, independently of being determined by genetic or socio-cultural traits, that are expressed in socio-economic differences in academic performance (such as grades, or school marks). Such indicators of performance have a strong effect on educational transitions. Choice effects are in turn all those that are expressed in actual educational decisions for students at similar levels of academic performance. Over the last three decades, research in this area has shown that performance effects are relatively larger than choice effects and that this difference has widened among younger cohorts of students. Importantly, quantifying the relative importance of these two mechanisms can inform and help policy makers to decide 'when' and 'what' type of interventions should be implemented to reduce inequality. However, the current methodology utilized to quantify the relative importance of performance and choice effects presents both technical and conceptual limitations. First, the standard decomposition methods are likely to yield biased estimates due to confounding and scaling bias. Second, the model of performance and choice effects stresses micro-behavioural mechanisms at the individual level, disregarding the role of school organizational factors. These limitations compromise the possibility of drawing valid inferences and, thus, of informing policy makers.

With these concerns in mind, over the course of three empirical chapters, I firstly explored the weaknesses in the standard decomposition approaches used in this field. Secondly, I estimated the relative importance of performance and choice effects using novel data from Children on Immigrants Longitudinal Survey in Four European Countries (CILS4EU) and causal inference approaches. Finally, I analysed the role of ability grouping, i.e., how the organisation of learning environment, affects students' performance and choices.

The findings presented in this thesis make a number of important advances that arguably have implications for policy. First, I provide a more comprehensive framework and guidelines to quantify the relative importance and performance effects. Second, I show that total IEO has continued to decline in England and Sweden for students born in 1995/6. However, once more robust decomposition methods and sensitivity analysis were employed, I found that choice effects are in fact substantially more important than what has been claimed in past research. Third, employing several identification strategies, I show that ability grouping does not affect students' performance and choices in the context of comprehensive educational system as the ones of England and Sweden. These results indicate that policies aimed at changing the learning environment of schools are not very efficient for tackling IEO. Instead, it would probably be more fruitful to implement policies aimed at reducing inequality in early-life circumstances as well as policies aimed at reducing opportunity costs in access, and guiding students to make more efficient choices.

Contents

Introduction 1
Background 1
The distinction between performance and choice effects and its limitations 2
Contribution 4
Overview of thesis 5
Summary of findings 7
Chapter 1: 9
Abstract 9
1. Introduction 10
2. Potential limitations in the original Boudon formulation 13
Linking theory with empirics 13
3. An empirical example of bias in performance and choice research and solutions for anticipatory decisions 26
Data and variables 26
Description of decomposition methods and empirical strategy 29
Estimation of total and direct effects 33
Methods comparison in performance and choice effects decomposition 35
Sensitivity analysis 42
4. Conclusion and Discussion 44
Chapter 2: 50
Abstract 50
1. Background and research objectives 51
The English and Swedish school systems 54
Performance, choice, and anticipatory decision effects 55
2. Data and Empirical strategy 59
Data 59
Variables 60
Identification of performance and choice effects 63
Estimation 67
3. Results 70
Distribution of cognitive ability and aspirations 70
Decomposition of performance and choice effects 72
Sensitivity analysis 77
Comparing past and current estimates of the relative importance of choice effects 79
4. Conclusion and Discussion 82

Chapter 3: 85
Abstract 85
1. Introduction 86
2. Theory and previous literature 92
The English and Swedish school systems 96
3. Data and Empirical strategy 98
Data and variables 98
Approach and models 100
4. Results 104
Ability homogeneity results 105
Ability grouping results 108
Inequality results 115
5. Conclusion and discussion 117
Conclusions 119
Main contribution 119
Summary of findings and policy implications 123
Areas for future research 128
Appendix - Chapter 1 130
Section A.1.1 Summary statistics and total and direct effects result tables 131
Section A.1.2. Further note on Erikson decomposition and the counterfactual approach 135
Section A.1.3. Studying the variability of the PM in the presence of confounders through simulation 137
Section A.1.4. Further note on the potential endogeneity of the PM 143
Appendix - Chapter 2 148
Section A.2.1. Sample description 149
Section A.2.2. Erikson et al./Buis decomposition results 151
Section A.2.3. LPM and School Fixed effect models 152
Section A.2.4. Analysis using multiple imputation on England survey data 154
Section A.2.5. LPM and School Fixed effect models, ISEI vs ESeC comparison 156
Section A.2.6. Additional sensitivity analysis based on PSM and Mantel and Haenszel bounds 158
Section A.2.7. Description of the decomposition model through causal diagrams 158
Appendix - Chapter 3 161
Section A.3.1. Further homogeneity analysis on math performance, transitions without GPA control, and GPA 162
Section A.3.2. Further ability grouping analysis on GPA and transitions without GPA control 166
Section A.3.3. Percentage distribution of Math and English/Swedish ability grouping 168
Section A.3.4. Inequality analysis – full tables 169
References 175

List of Tables

Chapter 1

Table 1.1: Total and indirect effects across different mediation methods 37
Table 1.2: Total and indirect effects accounting for post-treatment confounders in G-formula..... 39

Chapter 2

Table 2.1: Imai, Keele, and Yamanoto (IKY) decomposition results..... 76
Table 2.2: G-estimation results 76

Chapter 3

Table 3.1: Summary statistics 105
Table 3.2: Inequality analysis, School FE models with interaction effects 116

Appendix – Chapter 1

Table A.1.1: Summary statistics before and after sample selection..... 131
Table A.1.2: GPA outcome – total and direct effects, OLS regression 132
Table A.1.3: Academic track transition outcome - total and direct effects, LPM regression 133
Table A.1.4: Example of data structure in mediation analysis using the Potential Outcome (PO) approach..... 135
Table A.1.5: Profile equivalence between Erikson et al. 2005 approach and the PO framework 135
Table A.1.6: Description of linear effect decomposition..... 138

Appendix – Chapter 2

Table A.2.1: Summary statistics – Survey data 149
Table A.2.2: Erikson et al./Buis decomposition results..... 151
Table A.2.3: Total and direct effects – England 152
Table A.2.4: Total and direct effects – Sweden..... 153
Table A.2.5: Missingness across key variables..... 154
Table A.2.6: Standard method, Erikson et al./Buis decompositions..... 154
Table A.2.7: Causal mediation method, KHB 155
Table A.2.8: Causal mediation method, G-computation..... 155
Table A.2.9: Upper secondary ed. transition and GPA outcomes using ISEI and (simplified) ESeC, England 156
Table A.2.10: Upper secondary ed. transition and GPA outcomes using ISEI and (simplified) ESeC, Sweden..... 157
Table A.2.11: Propensity Score Matching and Mantel and Haenszel bounds estimates for Total Effects 157
Table A.2.12: G-estimation results with alternatives specifications..... 157

Appendix – Chapter 3

Table A.3.1: Robustness check. GPA regressed on Coefficient of Variation, students in mixed ability classes only 165
Table A.3.2: Inequality analysis – Coefficient of Variation#Parental yrs of education, England 169
Table A.3.3: Inequality analysis – Coefficient of Variation#Parental ISEI, England 169
Table A.3.4: Inequality analysis – Coefficient of Variation#Migration status, England..... 170
Table A.3.5: Inequality analysis – Coefficient of Variation#Parental yrs of education, Sweden..... 170
Table A.3.6: Inequality analysis – Coefficient of Variation#Parental ISEI, Sweden 171
Table A.3.7: Inequality analysis – Coefficient of Variation#Migration status, Sweden 171
Table A.3.8: Inequality analysis – Math ability grouping#Parental yrs of education, England 172
Table A.3.9: Inequality analysis – Math ability grouping#Parental ISEI, England..... 172
Table A.3.10: Inequality analysis – Math ability grouping#Migration status, England 173
Table A.3.11: Inequality analysis – Math ability grouping#Parental yrs of education, Sweden 173
Table A.3.12: Inequality analysis – Math ability grouping#Parental ISEI, Sweden 174
Table A.3.13: Inequality analysis – Math ability grouping#Migration status, Sweden..... 174

List of Figures

Chapter 1

Figure 1.1: Mediated effects in the standard performance and choice theory (Panel a) and endogeneity scenarios (Panel b, c, d and e) 21
Figure 1.2: Total and direct effects – OLS/LPM 34
Figure 1.3: Proportion Mediated across different methods 40
Figure 1.4: Sensitivity analysis for mediator-outcome confounder 44

Chapter 2

Figure 2.1: Standard and more advances approaches for studying IEO 64
Figure 2.2: Distribution of students' GPA and Cognitive Ability by Parental ISEI in England and Sweden 71
Figure 2.3: Proportion of students with university aspirations by Parental ISEI 72
Figure 2.4: Robustness checks. School fixed effects (Panel A) and Sensitivity analysis (Panel B) 78
Figure 2.5: Overtime comparison in the relative importance of choice effects 80

Chapter 3

Figure 3.1: Standardized GPA and CV in cognitive ability 106
Figure 3.2: Academic transition in upper secondary education and CV in cognitive ability, controlling for standardized GPA 107
Figure 3.3: Standardized Math grades and ability grouping 109
Figure 3.4: Academic transition in upper secondary education and ability grouping, controlling for standardized GPA 111
Figure 3.5: Placebo test, Math ability grouping on standardized English/Swedish grades 112
Figure 3. 6: Selection into high ability groups, linear probability model 113
Figure 3. 7: Selection into high ability groups, linear probability model 114

Appendix – Chapter 1

Figure A.1.1: Proportion mediated using only GPA, CA (cognitive ability), or Realistic aspirations as mediator 134
Figure A.1.2: Causal diagram with effect decomposition in the presence of confounders 137
Figure A.1.3: Absolute and relative bias in the proportion mediated (PM) across extreme endogeneity scenarios (weak/strong confounding) 140
Figure A.1 4: Absolute and relative bias in the proportion mediated (PM) across extreme endogeneity scenarios (weak/strong confounding) 142

Appendix – Chapter 2

Figure A.2.1: IEO (socioeconomic gaps in upper secondary transitions) across cohorts 150
Figure A.2.2: Decomposition of performance and choice effects with and without cognitive ability and aspirations 150

Appendix – Chapter 3

Figure A.3.1: Ability homogeneity analysis on standardized math grades 162
Figure A.3.2: Ability homogeneity analysis on academic transition without controlling for GPA 163
Figure A.3.3: Ability homogeneity analysis on GPA 163
Figure A.3.4: Ability grouping analysis on standardized GPA 166
Figure A.3.5: Ability grouping analysis on academic transition without controlling for GPA 166
Figure A.3.6: Distribution of propensity scores in England 167
Figure A.3.7: Distribution of propensity scores in Sweden 167
Figure A.3.8: Distribution of Math and English ability groups in England 168
Figure A.3.9: Distribution of Math and Swedish ability groups in Sweden 168

Acknowledgements

This thesis would not have been possible without the support of many people. First and foremost, I am deeply grateful to my supervisor, Prof. Janne Jonsson, whose generous guidance, support, and patience have been invaluable. He went above and beyond his duties, providing me with insightful advice and encouragement throughout this process. I truly appreciate his unwavering dedication.

I would like to extend a special thank you to several individuals within the Oxford Sociology Department. Prof. Christiaan Monden served as my research advisor, offering mentorship and valuable guidance. Patrick Praeg who always contributed by reading and providing constructive feedback on my work. Additionally, Prof. Richard Breen provided invaluable insights during my CoS.

I am also indebted to Carina Mood, who graciously invited me to SOFI, University of Stockholm, Sweden, for a visiting research period. This opportunity allowed me to access Swedish register data for my second chapter, connect with a remarkable group of scholars, and learn from them.

I am deeply appreciative of Nuffield College, particularly the senior tutor, Eleni Kechagia-Ovseiko, and the supportive community of staff members who have been a continuous source of assistance throughout my years of study.

I would also like to express my gratitude to the following individuals, in no particular order: Federico Podesta', Davide Azzolini, Claudio Gianesin, Catia Nicodemo, Francesco Moscone, Nicolo Cavalli, Giacomo Vagni, who are not only wonderful friends but also amazing co-authors. I am also grateful to Giacomo Gabbuti, Marco Ranaldi, Said Hassan, Andrea Tartakowsky, Alejandro Espinosa Herrera, Riley Taiji, John Kenny, Felix Tropf, and Eva Akimova, who made my time in Oxford truly special. Special thanks also go to the members of the FBK-IRVAPP and Oxford-INET institutes. Additionally, I extend my thanks to my brothers from Trento: Emanuele Chiappa, Eugenio Chiappa, and Francesco Tonezzer who supported me during my time in Italy and make me feel at home. I would also like to express my gratitude to the football community of Holloway in North London, who embraced me as part of their family from the very beginning and frequently reminded me of the motto, 'Always forward.

To my partner, Ana, I express my deepest gratitude for your unending support and encouragement throughout this journey.

Finally, I would like to dedicate this thesis to my parents, grandparents (including Pacheco), sisters Sol and Nay, and my dearest friends from Parque Patricios (‘la quema’) and Pompeya: Nahu, Pato, Ale V, Pilu, Fer, Ale G, Mati, Lea, Chipi, Ari, Cesar, Coya, and Carlitos. Despite the physical distance that has separated us for the past 15 years, your constant presence in my life has been immeasurable. For what it’s worth, I dedicate this thesis to you.

Introduction

Background

In industrialized countries mass participation in education is widely considered to be crucial for increasing international economic competitiveness, promotion of social justice, and social mobility (OECD 2014; UNESCO 2015). Consistent with this statement, the active policy intervention of national governments for expanding participation in education over the last five decades has been motivated by the principle of 'individual merit' and the assumption that educational expansion can reduce social inequality in society (Young 1958; Robbins 1963). According to this liberal view (Treiman 1970; Bell 1972, 1973), a meritocratic society, and especially a meritocratic educational system, should reward individual attainments, ability and effort (i.e., achievements) with better opportunities in life (Arrow et al. 2000; Moore 2004). Inequality of opportunity instead prevails when individuals' chances of success are influenced by attributes that are ascribed at birth such as race, sex, ethnicity or socioeconomic background (Breen and Jonsson 2005) but also by contextual factors out of individuals' own control such as institutional aspects that structure opportunity sets or preferences (Allmendinger 1989; Van de Werfhorst 2019).

Generally speaking, the conception that educational expansion automatically produces equalization in the educational system and, at the same time, reduces inequality of opportunity over the life-course is mainly rooted in modernization theory (Parsons 1970; Treiman 1970; Bell 1973; Kerr 1983). Advocates of this theory claimed in the 1970s that the constant development of industrial and modern societies would produce a higher requirement for qualified workers, making people obtain higher education, and producing a more meritocratic allocation to professional occupations in the social structure. However, empirical studies in Europe and USA in the 1980s and 1990s challenged this theory, arguing that this functionalist view was too simplistic and optimistic (Featherman and Hauser 1978; Blossfeld and Shavit

1993; Breen and Goldthorpe 2001). The main critical observation made by social stratification scholars was that in spite of different political efforts, educational expansion has not brought much equalization in post-compulsory education (Goldthorpe 1996). Even if it was possible to observe an absolute increase in the number of people participating in secondary and tertiary education, socioeconomic inequality in educational attainment was still found in many industrialized societies (Boudon 1974; Featherman and Hauser 1978; Halsey et al. 1980; Gambetta 1987; Blossfeld and Shavit 1993; Goldthorpe 1996).

Notably, in response to this modernisation theory, European sociologists have attempted to develop new explanations to the persistence of inequality of educational opportunity (IEO) by moving beyond the description of absolute and relative educational mobility trends and focusing on the mechanisms that lies behind these crude rates (Boudon 1974; Erikson and Jonsson 1996; Breen and Goldthorpe 1997). This was an important change in the inequality research agenda that enabled a large number of studies on the determinants of socioeconomic gaps across different branching points of the students' educational trajectories (Jackson et al. 2007; Erikson and Rudolphi 2010; Rudolphi 2011), brought also new methodologies (Erikson et al. 2005; Morgan 2012), and added significant policy implications to the public debate (Jackson 2013).

The distinction between performance and choice effects and its limitations

Understanding the main determinants of different educational decisions is indeed key for designing policy interventions targeted at reducing IEO (Heckman, Lochner, and Todd 2006; Holmes and Myhew 2016) and improving social mobility in society (Elias and Purcell 2012). Since mid-90s sociologists have studied such type of inequality adopting Boudon's conceptual model of primary and secondary effects (also called performance and choice effects, terms I use hereafter). More precisely, performance effects are all those, whether determined by genetic or socio-cultural factors, that are expressed in the association between students' social

background and their academic performance at school. Choice effects are those expressed in actual educational decisions once the differences in academic performance are taken into account. By decomposing IEO into these two mechanisms, and quantifying their relative importance, it is possible to understand how inequality is generated and perpetuated across generations. Additionally, knowing the magnitude of the relative importance of these two mechanisms can help policy makers to design more efficient policy interventions by indicating whether inequality is generated early in students' educational trajectory (for example, because of poor academic performance and issues in acquiring learning skills) or later at the moment of making educational decisions (for example, due to lack of information or behavioural bias like loss aversion). In a period of limited public economic resources, knowing 'when' and 'what' type of intervention should be adopted could be crucial for reducing inequality.

Although the distinction between performance and choice is well-stablished among sociologists, the methodology for quantifying their relative importance still presents several problems that compromise the possibility of informing policy makers. Three major limitations should be noted. First, previous research that analysed performance and choice effects were based on a simplified model that decomposed odds ratios. As noticed by Holm and Karlson (2011) these models are likely to provide biased estimates due to scaling issues.

Second, these models did not take into account the fact that there are several sources of confounding that should be taken into account when these two mechanisms are decomposed. Namely, bias can take the form of exposure-outcome, exposure-mediator, mediator-outcome, and post-exposure/intermediate confounding. The last one is especially worrisome since it is very likely that students anticipate educational decisions and thus they might underperform at school for this reason. This would produce an endogenous selection because anticipatory decisions would affect their academic performance as well as their choices. Naïvely controlling

for these variables or not controlling at all would induce bias in the estimates of performance and choice effects.

Third, the empirical papers that decomposed the relative importance of performance and choice effects completely omitted the calculation of confidence intervals and the performance of sensitivity analysis. Confidence intervals are especially important for determining whether performance and choice effects differ significantly. Sensitivity analysis is useful for understanding how strong the confounding need to be for changing the direction of our estimates and conclusions. The two most important methodological contributions in this field, i.e., Erikson et al. (2005) and Morgan (2012), briefly discussed the implications and potential risk of exposure-outcome confounding bias but never addressed the issue of anticipatory decisions, nor discussed the importance of sensitivity analysis and confidence intervals. Fourth, contemporary to performance and choice effects research, another set of papers investigated the effect of ability grouping, i.e., how the learning environment is organised within classes and schools, on academic performance. Although closely related to performance and choice effects, these two strands of research never interacted. This is probably because ability grouping reflects institutional aspects of the school and the canonical model of performance and choice effects puts the focus on micro-behavioural mechanisms that are considered to operate independently of the educational context (Goldthorpe 2016). Therefore, it is timely and necessary to understand whether ability grouping is an important factor for students' performance and educational choices.

Contribution

This DPhil thesis, therefore, initially looks at the problem of separating performance and choice effects in educational transitions and how this approach can be more useful for informing policy. Then, adopting a more robust methodology it estimates the relative importance of

performance and choice effects for a cohort born around 1995/96, and further addresses whether ability grouping is an important determinant for students educational outcomes. For this purpose, I use a unique dataset, Children of Immigrants Longitudinal Survey in Four European Countries (CILS4EU), which contains rich information on social stratification variables such as family background, academic performance, and educational transitions. Importantly, CILS4EU data also offer the opportunity to explore the role of other variables that are usually unobserved in this kind of studies, such as cognitive ability, aspirations, and ability grouping. These are all important determinants of academic performance and educational transitions. Moreover, this dataset also offers the possibility of obtaining novel estimates of IEO for younger birth cohorts which have not been studied before. Therefore, it adds another time-point estimate for studying changes across birth cohorts.

It should be noted that I will focus on England and Sweden, two countries with a comprehensive educational systems and very similar branching transition points (although these countries have different practices in terms of ability grouping within schools). They are also the two countries with the longest tradition in performance and choice effects research and therefore they both represent a good case for discussing the current methodology and comparing previous and new estimates of performance and choice effects.

Overview of thesis

Chapter 1 consists of two parts. The first re-evaluates the theory behind Boudon's model of educational transitions from a causal inference perspective and makes the identification assumptions to separate performance and choice effects more transparent. Based on the identification issues discussed in the first part, the second part scrutinizes the methods for decomposing performance and choice effects, showing their advantages and limitations, and proposes a series of robustness checks based on additional models and sensitivity analysis. Then it concludes with series of recommendations to avoid bias in this research and obtain more

robust estimates of the relative importance of choice and performance effects. The main scope of this chapter is to provide a more comprehensive and robust framework for quantifying the relative importance of performance and choice effects.

The second chapter implements the methodology proposed in chapter 1 and shows new estimates of performance and choice effects in Sweden and England for a cohort of individuals born around 1995/6. Bearing in mind some of the difficulties when comparing different studies over time, it also provides a short description of how either total IEO, performance, and choice effects in upper secondary transitions differ across birth cohorts by collecting the majority of the estimates reported in the literature for people born in early 60s till mid-90s in England and Sweden.

The third chapter explores whether homogeneity in terms of cognitive ability is beneficial for school performance and educational choices. In the field of sociology of education there is an ongoing and currently lively debate on whether mixed ability classes are bad or good for student educational outcomes. The evidence so far is mixed and inconclusive. This is partly because researchers usually lack longitudinal data with measures of cognitive ability measured at individual, class, and school levels. Moreover, students are not randomly allocated into schools and classes, making it difficult to separate the effect of sorting from class homogeneity. Lastly, few previous studies in this field have looked at how homogeneity in ability and ability grouping affect not only students' academic performance but also educational choices. Importantly, the literature in the performance and choice effects has hitherto not taken into account the organisation of the learning environment, while studies of ability grouping have not considered moving beyond academic performance outcomes and to analyse educational transitions. With this last chapter, I bring these two strands of research together. In this regard, ability grouping might be considered as another relevant mechanism

in determining performance and choice effects. Also, if there is any significant effect, it could represent a direct channel through which policy makers can intervene to reduce inequality.

Summary of findings

Each of the outlined chapters is bound together by their common interest in understanding differentials in academic performance and educational decisions, moving beyond descriptive methods and adopting a causal inference perspective.

The first chapter discusses the identification issues for estimating performance and choice effects, and compares standard methods used in sociology with those developed in the causal inference. The conclusion is that the former are likely to provide biased estimates, and the chapter advises on sensitivity analysis and better practices for obtaining more robust results.

The second chapter implements new causal decomposition methods and shows that once anticipatory decisions are accounted for, the relative importance of performance and choice effects differ to what was found before. In particular, I found that choice effects are almost equally important to performance effects in England, while in Sweden they are smaller but not negligible.

The last chapter evaluates whether ability grouping is an important factor in determining students' performance and educational choices. Using novel identification strategies based on counterfactual and sensitivity analysis, my results indicate that there is not enough evidence to claim that ability grouping is an important factor for academic performance and educational choices in comprehensive educational systems.

Taken together, these chapters fill methodological and empirical gaps in the literature and shine a light on what future research should focus in order to provide policy makers with better evidence and advice for reducing inequality of educational opportunity. In this regard, two empirical findings are relevant for the policy agenda. First, the fact that choice effects are still sizeable in magnitude indicates that perhaps a more lucid choice structure, and more

information about the choices and their consequences should be provided to students in comprehensive educational systems. These type of interventions could be implemented before students take their major exams rather than at the end of the school year when they only need to decide what to do next. An early intervention on choice effects could have both beneficial effects for school performance and improve efficiency in academic and professional decisions. Second, the fact that ability grouping did not affect students' grades and educational choices suggests that interventions targeted at changing the school organisation level might be less effective than interventions targeted to alleviate early socioeconomic disparities or those targeted to increase efficiency in students' decision-making.

Chapter 1:

On the Quantification of Performance and Choice Effects in Educational Transitions

Abstract

Boudon's model of performance and choice effects was one of the first sociological approaches to analyse in conjunction the mechanisms that generate inequality of educational opportunity (IEO). In recent years, it has been shown that this approach can be also used to quantify the relative importance of these two mechanisms and, in this way, inform policy about how to reduce IEO. However, decomposing performance and choice effects poses different challenges that have not been considered in past research. After discussing a series of theoretical and methodological issues in the decomposition of performance and choice effects, I employ rich longitudinal data from Children on Immigrants Longitudinal Survey in Four European Countries (CILS4EU) to show through real empirical examples how results regarding the relative importance of performance and choice effects vary depending on the technique and model specification employed. In particular, estimates of performance and choice effects in decomposition analysis are likely to be affected by confounding and collider bias since the estimation procedures are based on nested reduced-form models. The results from these exercises show that standard methods are prone to bias and estimates of the relative importance of performance and choice effects should always be interpreted with caution. Given these limitations, the chapter concludes providing a set of good practices for analysing and reporting quantities of the relative importance of performance and choice effects.

1. Introduction

Different attempts to explain the causes of persistent educational inequality in industrialized societies have identified mechanisms that are intrinsically related to students' academic performance and family endowments. Sociologists, in particular, have analytically classified them as primary and secondary effects (Keller and Zavalloni 1964; Boudon 1974; Erikson and Jonsson 1996; Breen and Goldthorpe 1997; Jackson 2013). Namely, primary effects (hereafter performance effects or PE) are all those, whether determined by genetic or socio-cultural traits, that are expressed in socio-economic differences in academic performance (typically at formal examinations or school tests) and affect educational transitions. In turn, given similar levels of performance, secondary effects (hereafter choice effects or SE) are those that are expressed in actual educational choices that students together with parents make in the course of their educational careers (Breen and Goldthorpe 1997; Jackson et al. 2007). Over the last two decades researchers have shown that students from well-off families not only academically outperform those from less advantaged backgrounds but, even when academic performance is held constant, they also make more ambitious choices (e.g., Jackson 2013).

This widely-used analytical approach, initially proposed by Boudon (1974) and extended, then, by Erikson and Jonsson (1996) and Breen and Goldthorpe (1997) have not found, however, a proper empirical implementation yet. Although the issue of how quantifying the relative importance of performance and choice effects have received more attention in the last years (see Erikson et al. 2005; Buis 2010; Karlson and Holm 2011; Morgan 2012; Jackson 2013), this is a question that still remains inconclusive, especially when it comes to the discussion of endogeneity problems in the standard mediation/decomposition techniques (Erikson et al. 2005; Morgan 2012). This matter should not be overlooked since the analysis of these two mechanisms arguably has policy implications (Jackson et al. 2007; Jackson 2013). In particular, the most recent studies collected in the book edited by Jackson (2013) show that

performance effects accounted for almost 75-84% of inequality of educational opportunity (IEO) in transitions to upper secondary education and 66-70% in transition to higher education among countries with low levels of school tracking like England and Sweden. This suggests that public policies in these countries should strive to improve students' performance at early stages of the educational career rather than focusing on late educational choices.

Because the relative importance of choice and performance mechanisms can be crucial for informing policy makers about how to reduce IEO, empirical evidence should not rely on weak designs. In fact, very often the sociological scholarship in inequality research poses implicit advice and recommendations for policy makers neglecting, however, the discussion about causation, or simply avoiding the description of the assumptions behind their conclusions. Given that this approach has the potential to become an important policy tool, greater scrutiny of its methodology is needed.

Methodologically speaking, the two major contributions to this field were produced by Erikson et al. (2005) and Morgan (2012). The first one proposed a new method for decomposing and quantifying the relative importance of performance and choice effects and also noticed that anticipatory decisions (i.e., the fact that students might anticipate their future academic choices and perform poorly at early stages of their scholastic trajectory) could bias these estimates. However, they never provided any solution to this type of bias and thus their decomposition method still remains under the assumption that students do not anticipate decisions. The second contribution discussed the assumptions for identifying performance and choice effects in a more detailed way than Erikson et al. and also highlighted the importance of conceptualizing the model in terms of causal diagrams (also known as directed acyclic graphs), although this was made specifically for the context of university transitions in the USA. In particular, this paper made explicit the fact that we need to condition on several exogenous sociodemographic characteristics for identifying total and directs effects. However,

as before, this second contribution did not provided any solution for the presence of anticipatory decisions, nor discussed the causal methods for decomposing performance and choice effects. Furthermore, none of the past empirical and methodological contributions in this field considered the importance of calculating confidence intervals (i.e., the dispersion) for the estimates of relative importance of performance and choice effects and, given the large number of potential unobserved variables, to conduct sensitivity analysis as a robustness check. Not modelling anticipatory decisions, bad adjustment strategies, and lack of confidence intervals as well as sensitivity analysis can seriously compromise the validity of the estimates of the relative importance of performance and choice effects. This paper looks at addressing these three neglected issues and setting the ground for a robust quantification of performance and choice effects.

Building on previous empirical and methodological research on performance and choice effects as well as new developments in the causal mediation literature, this study provides, therefore, a more comprehensive assessment of the theory and methods for identifying and quantifying performance and choice effects. In particular, it contributes to the educational inequality literature in three respects. First, upon the performance and choice effects theory, it clarifies the relationship between variables involved in the educational stratification process, the assumptions behind standard models of educational decisions, and potential issues for identification and quantification of these effects. Second, it describes the estimation methods available for decomposing direct/indirect effects and compares the standard ones used in the sociology of education literature with those developed in the causal inference literature. Third, it provides empirical examples of the type of bias that can be introduced in the quantification of performance and choice effects due to the omission of relevant variables in educational transitions, the adjustment strategy, and the methods employed. For the empirical analyses I use the Children of Immigrants Longitudinal Survey in Four European Countries (CILS4EU)

(Kalter et al. 2014, 2018; www.cils4.eu), a unique dataset that contains rich information on classic observable variables such as family background, academic performance, and educational transitions. Importantly, these data also include other individual variables that are usually unobserved, but known in the literature to be associated with the mechanisms of interest, such as cognitive ability (hereafter CA) and educational aspirations. The results show that standard methods are prone to bias if there are several sources of confounding and, thus, estimates of the relative importance of performance and choice effects should always be interpreted with caution. Also, sensitivity analysis and confidence intervals should be reported, independently of the decomposition method adopted. I end the paper by suggesting a set of good practices for analyzing and reporting quantities of the relative importance of performance and choice effects.

The remainder of this paper is structured as follows. Section 2 revises the performance and choice theoretical model from a causal identification perspective, describing its limitations and necessary assumptions for identification under conditional exogeneity. Section 3 describes current methods for decomposing performance and choice effects and shows empirically the risk of omitting certain variables in the analyses across different model specifications. In this way the identification is linked with its methodology and statistical implementations. Section 4 concludes with a summary of recommendations to avoid bias and improve transparency in this field of research.

2. Potential limitations in the original Boudon formulation

Linking theory with empirics

Boudon's model of educational stratification provided an alternative explanation, in respect to culturalist (Bourdieu and Passeron 1973), normative, and value-centered (Hyman 1953; Banks 1971; Willis 1981) approaches, to the formation and persistence of educational inequalities in contemporary societies. His main contribution was to partial out two significant mechanisms,

stressing the importance of agent's decision-making processes in terms of cost-benefit analysis. This means that Boundon's theory is not just focused on assessing class gaps in educational attainment but also aims at providing a more comprehensive explanation about the causes of such phenomenon by looking at transition points in the educational structure.

Although Boudon does not sympathize with the method proposed by Blau and Duncan (1967), which is known now as Structural Equation Modeling (SEM), or mediation analysis, but labelled at the time as path analysis (Wright 1921; Blalock 1964), his theoretical model fits into this type of statistical implementation. Indeed, further studies on performance and choice effects followed this line of research becoming a standard approach for quantifying the relative importance of these two mechanisms in educational stratification (Erikson et al. 2005; Jackson et al 2007; Jackson 2013; Karlson 2013).

Basically, mediation refers to the process by which exposure variables transmit an effect onto another through different mediating variables (Pearl 2001; Imai et al. 2011; VanderWeele 2015). The key aspect of this technique is the idea of decomposing the total effect of an exposure variable on the outcome into direct and indirect parts. But what are these effects in performance and choice research? The indirect effect represents the amount of the total effect of an exposure variable on the outcome that is due to the proposed mediator variable. In this case, the indirect effect is the 'initial explanation' to the question of why disadvantaged children are under-represented in higher and more ambitious educational levels. Although this is an important information for understanding IEO, it is not as exhaustive as one can imagine. Here is when the idea of choice effects shed more light into the problem of IEO. That is, choice effects are the remaining effect of the key exposure variable that is not channeled through the proposed mediator and which, according to Boudon (1974), is intrinsically related to class decision processes. Clearly, the presence of choice effects in education challenges the idea that improving learning outcomes of disadvantaged students alone would be enough to eliminate

socioeconomic gaps in educational attainments (Goldthorpe 1996). . As Boudon states, choice effects are the core problem of IEO and the reason why policy changes in the educational system have not reduced the gap between upper- and working-class students (Boudon 1974, p. 31).

Moreover, if Boudon set the analytical basis for decomposing IEO into specific mechanisms, later contributions articulated and enriched the theory, by pointing out the importance of micro processes responsible for choice effects and defining more refined ways to empirically test them. In this regard, and from a more technical perspective, performance and choice theory can be seen and formulated in terms of a structural model of educational decisions that is empirically translated into reduced form equations for statistical testing. Structural models formally define behavioral mechanisms in terms of agent's course of actions, constraints and preferences.[1] A reduced form equation, on the other hand, is a statistical simplification that aims at approximating these unobserved processes using the data available.[2]

Examples of models that impose structural courses of action in this research are the Erikson and Jonsson model (1996); the Breen and Goldthorpe model (1997), which both stipulate the presence of a 'relative risk aversion' mechanism in terms of fear of social demotion (i.e. loss aversion); the Breen and Yaish model (2006) which extends the former models by including the idea of 'expectations' thresholds, and, finally, the Breen, van de Werfhost and Jaeger (2014) model, which considers 'time discounted preferences' in educational decisions, that is, the weight or value that people assign to future rather than immediate return. These represent key contribution for the identification of performance and

[1] Structural models can be also statics or dynamic depending on the assumptions about the sequential behaviour of agents and the inclusion of life course data.

[2]The idea of using structural models have a long tradition in labour economics where utility maximization represents the core element of agent behaviour. For an introduction to Structural models in economics see Low and Meghir (2017).

choice effects. Nonetheless, the crucial aspect in this respect is that regardless of how the theory is formalized and empirically tested, we usually infer a complex theoretical mechanism from an observed parameter in the model assuming certain 'necessary conditions' – that is, elements that are not directly observed in the data or empirically proven but are required for the model to be valid. This is a very important point because, if we want to continue using reduced form models to infer such complex micro-mechanisms, we need to have a better understanding of the assumptions behind our models and the potential limitations related to them.

A common problem in the performance and choice research is the lack of understanding about the role of unobservable and observable variables, especially in terms of confounding and collider bias (also called collider-stratification bias or endogenous selection). The distinction between these two sources of bias can be shown by using Directed Acyclic Graphs (DAGs, also called causal diagrams). This approach is useful for disentangling relationships between random variables and establishing causal effects. Three elements are of particular importance in this technique (Pearl 2009; Elwert and Winship 2014): causal chains (A→B→C; A→C), forks (A←B→C) and inverted forks (A→B←C). These three elements represent a practical way for describing sources of causation, confounding and colliders bias. DAGs are by definition non-parametric in the sense that they do not impose any assumption about the distribution and functional forms of variables. The only restriction placed in this approach is the causal structure of dependencies and independences implied by the directed arrows. DAGs in practice can be seen as a system of non-parametric structural equations that facilitates the identification of causal effects (Winship and Morgan 2014; Breen 2018; Dafoe 2018). Moreover, related to the idea of using DAGs for causal identification is also the concept of blocking and the backdoor criterion which was originally developed by Pearl (2001; 2009) and now is commonly used in the majority of causal inference textbooks (see for example, Winship and Morgan 2014; Hernan and Robins 2020). A common approach in quantitative research for

investigating causal questions is to analyze potential causal relationships within groups defined by one or more variables. The basic idea is examine the data after conditioning on group membership identified by a variable that is believed to be associated with both the exposure variable and the outcome variable. From a graphical perspective, this modeling strategy leads to the creation of simplified subgraphs for each subgroup or stratum, corresponding to different values of the conditioning variable. In essence, this procedure involves disconnecting the conditioning variable from all other variables it influences in the original graph and reconfiguring the graph for each value of the conditioning variable. A researcher aiming to estimate a causal effect often encounters a common concern: the exposure variable X and the outcome variable Y may be mutually dependent on a confounding variable C. In such cases, the observed association between X and Y represents a combination of the genuine causal effect of X on Y and the common dependence of X and Y on C. This confounding is often referred to as the causal effect of X on Y being confounded by C. To consistently estimate the causal effect of X on Y in this scenario, researchers can condition on C. As part of the operational data analysis routine, the effect of X on Y can be estimated through a two-step process: (1) calculating the association between X and Y for each subgroup with C equal to a specific value, and (2) averaging these subgroup-specific associations over the marginal distribution of the values C takes on. This resulting weighted average represents the causal effect of X on Y within the standard regression framework, often referred to as the average treatment effect in counterfactual causality literature. Conditioning on C in this way identifies the causal effect of X on Y in this example.

From a graphical perspective within the Directed Acyclic Graph (DAG) framework, a path refers to any sequence of edges connecting variables. A back-door path is a path between a causally ordered pair of variables that includes a directed edge pointing to the first variable. For the pair of variables X and Y, the path $X \leftarrow C \rightarrow Y$ is a back-door path since it includes a

directed edge pointing to X. In contrast, the path X → Y is not a back-door path as it lacks a directed edge pointing to X. The issue with back-door paths is that they can contribute to the association between X and Y, leading to inconsistent estimation of the causal effect of X on Y. The observed association between X and Y fails to identify the causal effect because it represents an unknown combination of the true causal effect X → Y and the back-door path X ← C → Y. To determine if conditioning on a set of observed variables can identify the causal effect of interest, Pearl's "back-door criterion" offers guidance. If one or more back-door paths connect the exposure variable to the outcome variable, conditioning on a set of variables Z (i.e., variables that close back-doors) identifies the causal effect if and only if all back-door paths are blocked after conditioning on Z.

To put it simply, Winship and Morgan (2014) and Hernan and Robins (2020) following Pearls work, suggest that all back-door paths are effectively blocked by Z if and only if each back-door path satisfies one of the following conditions: (a) it consists of a chain of mediation, A → C → B, where the middle variable C is included in Z; (b) it involves a fork of mutual dependence, A ← C → B, where the middle variable C is part of Z. and c(c) it encompasses an inverted fork of mutual causation, A → C ← B, where the middle variable C and all its descendants are not present in Z.

According to Winship and Morgan, (2014), the back-door criterion provides valuable insights in two key aspects. Firstly, it clarifies that researchers do not need to condition on all omitted direct causes of an outcome variable to address omitted-variable bias. Instead, Pearl's back-door criterion highlights the importance of conditioning on a minimally sufficient set of variables that effectively block all back-door paths. Secondly, the framework offers a clearer understanding of the appropriateness of conditioning on endogenous variables. By systematically examining each back-door path and determining if each endogenous variable

acts as a collider, researchers can consider the complexities of a conditioning strategy more easily than with previous approaches.

However, it's important to note that what Winship and Morgan (2014) defined as a criterion for more transparency and improvement controlling strategies in causal inference. Other scholars suggest that these approaches could be a limitation for sociological research. For example Goldthorpe (2001; 2016) in a review of causal approaches, noticed that most of the path analysis employed by sociologist in the 70s (standard mediation models) could be affected by three limitations. Firstly, the process of modelling itself necessitates a theoretical framework that guides the selection of variables, establishes their causal order, determines the functional relationships between them, and so on. Secondly, it is essential to recognize that if the underlying theory is flawed or inconsistent with the actual social processes that generate the data being utilized, the results of the analysis will be compromised and unreliable. Lastly, it is worth noting that the existing sociological theory may not be robust enough to develop models that possess true "structural" characteristics. By this, we mean models that are adequately parameterized, allowing for coefficients that remain invariant and autonomous, enabling claims about the consequences of changes in the variables considered to be exogenous. Moreover, he also raised some important concerns on the possibility of adopting a potential outcome framework (causality as manipulation) in sociological research. In practice he highlights that observational studies within sociology, particularly studies aiming to determine the effects of interventions or mimic experimental settings, cannot assume that the response of the involved units (such as individuals affected or potentially affected by the intervention) will be comparable to that of units in an experiment conducted in an applied natural science field. These individuals are likely to be aware of the intervention taking place and hold beliefs about its objectives and potential outcomes. They may then interpret the

situation based on their own interests and goals, subsequently acting in a manner that could potentially contradict or undermine the intended effects of the intervention.

Confounding and collider bias in performance and choice research

The relationship between family background (X), academic performance (M) and educational transitions (Y) is graphically depicted in the DAGs of Figure 1.1 panel a, where performance effects are those effects of family background on Y that operate indirectly through academic performance. That is, the path defined by (X→MY). The SE are then the remaining direct effects of family background on Y, that is, the path (X→Y).

The theory recognizes that there are other variables apart from the social class that affect academic performance, and these cannot be always observed. For example, Boudon (1974, p. 29) and Breen and Goldthorpe (1997, p. 277) state that cultural and genetic traits are two important determinants of performance effects. In addition to genetic inheritance, environmental determinants, in the sense of inheritable attributes, mainly explain observed class differential in test scores (or other measures of school performance) through environmental and socialization processes in early childhood. However, such model assumes that these unobserved factors do not directly affect the educational transitions (see Panel a, Figure 1.1). This simplification obviously facilitates the separation of these mechanisms, but it comes at the cost of introducing potential bias from omitted variables (Panel b). Thus, past research has neglected the fact that there is a problem in the way in which the determinants of academic performance enter into the model and how they relate to educational transitions.

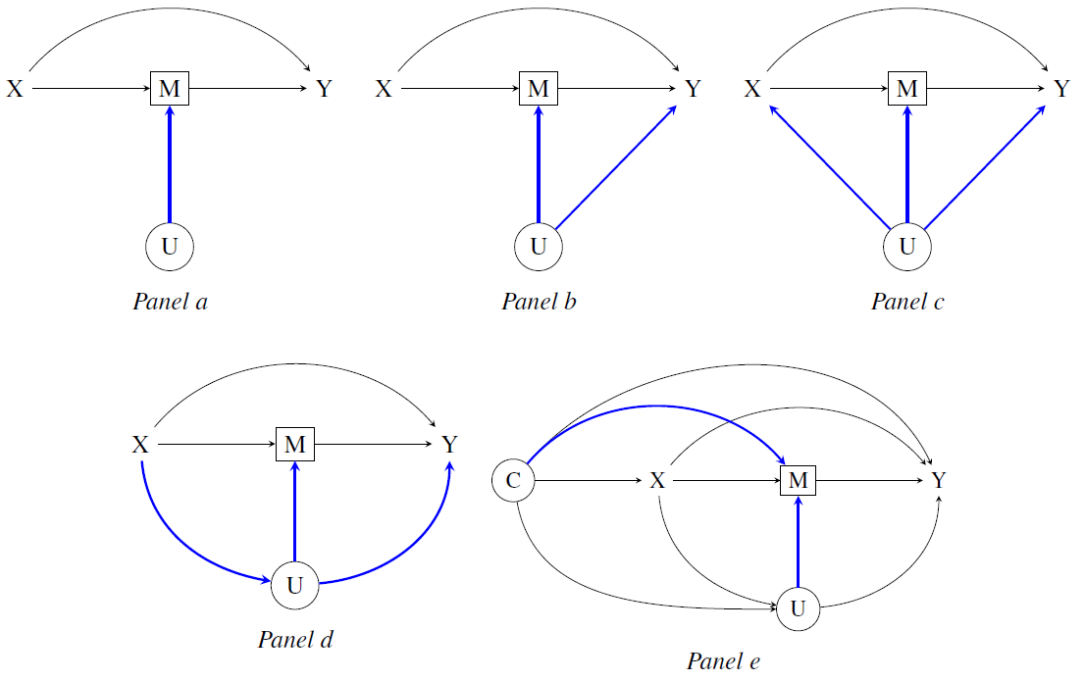


Figure 1.1: Mediated effects in the standard performance and choice theory (Panel a) and endogeneity scenarios (Panel b, c, d and e)
Note: Panel a = scenario according to standard performance and choice theory; Panel b, c and d = scenarios relaxing exclusion restriction assumption. Panel e = collider bias; X = social class; M = academic performance; Y = educational transition; U = unobserved factors. C = control variables. Square represents conditioning on an observable variable while circle represents (unconditioned) unobservable variables.

More precisely, the main assumption in the standard theory is the independence of the unobserved factors U from the educational transition Y once conditioned on academic performance M, (i.e., $[U \perp Y|M, X]$) or in other words that the effect of U on Y is completely mediated by M. However, this *exclusion restriction* assumption will rarely hold. Instead, it would be more realistic to hypothesize that whatever affects academic performance can directly affect choice as well. If we accept that U is not independent from educational choices we will need to consider potential bias from '*mediator-outcome*' confounders as shown in Figure 1.1, panel b. For instance, in higher education transitions in the UK, students need to apply to university before taking their final examinations. In the meantime, universities usually send offers which are confirmed once the students get the required final grades. In this case, the transition outcome is not purely determined by families but also universities and, since

universities have specific grades requirements they can directly affect students' performance. More in general, mediator-outcome confounders could be found in school factors such as school resources and quality, curriculum differentiation, and peer effects.

Further, this issue might be even more complicated if we recognize that U can also adopt other forms such as '*exposure-outcome*' and '*exposure-mediator*' confounders (Figure 1.1, Panel c). For instance, Sullivan et al. (2014) have argued that cultural differences do not exclusively affect performance but these factors have a direct effect on choices as well. Therefore, they propose to include parental education as a further control in the model since this variable also captures differences in students' cultural capital. Other examples of this type of confounders were described by Morgan et al. (2013) as race and area of residence. However, additional important omitted variables can be identified in the demographic literature where it has been documented that families have grown in complexity in the last two decades, and factors such as family composition and family fragility indicators are directly associated with family's resources as well as with educational outcomes (Bernardi and Bortien 2017; Choi et al. 2020). In this regard, a key aspect of the performance and choice theory is that social class or socioeconomic indicators based on occupation are good proxies for capturing differences in decision making and loss-aversion mechanisms (Breen and Goldthorpe 1997). Thus, if we want to specifically test whether this behavioral mechanism is responsible for choice effects, it would be wise to remove variation in educational transition that is due to other family of origin characteristics.

To some extent the non-independence of X, U and Y once M is held constant has been already recognized in the performance and choice effects literature.[3] An example of this

[3] Erikson and Jonsson (1996, p.10-12) and Jackson (2013, p.12) provide very interesting insights into this matter. For example, they claim that six different factors can affect academic performance: (a) genetic, (b) the home environment and its implications for economic, cultural, and social resources, (c) health and nutrition, (d) sibship size, (e) the cultural biases exhibited by schools, and (f) psychological mechanisms interacting with school factors.

problem was discussed by different scholars in terms of 'anticipatory decisions' (AD) (see Erikson and Jonsson 1996; Erikson et al. 2005; Jackson et al. 2007; Erikson and Rudolphi 2010; Morgan 2012; Jackson 2013). For instance, if students anticipate that they are not going to participate in post-compulsory education, they may decide to put less effort into school and consequently achieve lower academic grades (X→U(AD)→M; X→Y). If there is any anticipatory decision of this kind, the model will not properly represent the effect of family background on performance. Thus, the size of the choice effects would be underestimated because absorbed into (X→M→Y).

This kind of mechanism represents a real threat to the implied standard model because it violates the principle that these effects act independently in the creation of class inequality in education. As noticed by Jackson (2013, p. 17), AD are basically '*early choice effects*' that affect performance and are, then, crystallized into observed choices. A few solutions to this issue were proposed by Jackson et al. (2007), Erikson and Rudolphi (2010) and Rudolphi (2011). The first two have suggested replacing GPA with cognitive ability or early performance measures, and the latter controlling for students' educational and occupational aspirations. The rationality behind is to employ direct measures or proxies of potential ability and effort that were less affected by AD processes because they were measured well before the decision moment. However, even if AD has been acknowledged in the literature as an important confounder, past research has assumed that unobservable variables U, like AD, are independent of the family background (X→U(AD)=0).That is, students' early decisions are not affected by parents' actions and context, for example via socialization and early life course processes. In a more realistic scenario, such as the one represented in Figure 1.1, Panel d, we will have *omitted*

Points b, d, e and f, for example, could also affect choice in a direct way making them strong candidates for intermediate and mediator-outcome confounders.

mediated paths[4] (X→U→Y) but also potential *post-treatment confounders* (X→U→M), called also dynamic or intermediate confounder, since U affect M as well. This is an important aspect because the omission of the path that goes from X to U and then to M would also affect the estimation of the relative importance of performance and choice effects. In particular, conditioning naively on U blocks the causal pathways (X→U→Y) which brings bias from over-controlling of intermediate pathways. If we, on the other hand, do not condition on U, we introduce omitted variable bias from the path (X→M←U→Y). This is a serious problem because conventional regression adjustment inevitably leads to bias in the presence of post-treatment variables.

A second issue that has gained prominence in the methodological discussion of mediation analysis is the potential presence of collider variables (Elwert 2013; Elwert and Winship 2014; Winship and Morgan 2014; Breen 2018). This is usually addressed as the bias in the estimate of the association between two variables due to conditioning on a common cause of these two variables, that is, a collider, or a descendant of the collider (Pearl 2001; Greenland 2003). This type of bias can arise due to stratifying or subsetting the sample on the collider or simply by just conducting statistical adjustment (Hernan et al. 2004; Elwert and Winship 2014). In the case of performance and choice research, as represented in Figure 1.1, Panel e, the mediator M can be considered a collider (C→M←U). This is a typical problem in mediation analysis in which there are several types of confounders involved in the mechanism of interest. Since we need to condition on the collider for decomposing direct and indirect effects, we might introduce bias into our estimates (Tchetgen Tchetgen and Vanderweele 2014). For instance, following the standard procedure, one would condition on M in order to estimate the direct effect of X on Y and M on Y but, if we do not manage to control for C and U, this unblocks a

[4] For instance, Morgan (2012) states that there is no such thing as the direct effect of social class on educational transitions but instead other non-observed mediated effects.

non-causal path (X←C→M←U→Y), inducing endogenous selection on the mediator. This is also a problematic scenario because if we control for C, but not for U, this leads again into post-treatment confounding.

Overall, what is important to note in this discussion is that confounding and collider biases are serious sources of concern in mediation analysis. Researchers who aim at quantifying the relative importance of performance and choice effects, would need to put more efforts in identifying potential exposure-outcome; exposure-mediator; mediator-outcome and post-treatment confounders. Mediation method relies on fairly strong assumptions that can easily be (unintentionally) violated. The most important assumptions for decomposing direct and indirect effects can be summarized using the concepts of '*Ignorability*' and '*Sequential Ignorability*' (Imai, Keeley, and Yamanoto 2010; Breen, Karlson, and Holm 2013). Ignorability means that the probability of being exposed to X should be effectively random when conditioned on observable characteristics (C). Sequential Ignorability means that the mediator, M, should be also effectively random after including the proper control variables (C). In other words, the first part of the assumption says that given the observed pre-exposure confounders, the treatment assignment is assumed to be ignorable, that is, statistically independent of potential outcomes and potential mediators. The second part of this assumption, on the other side, states that the mediator is ignorable given the observed exposure and pre-exposure confounders. That is, the second part of the sequential ignorability assumption is made conditional on the observed value of the ignorable expousure and the observed pre-exposure confounders. For example, defining Y as the transition outcome; X as the exposure variable with two potential status x' (1) and x (0), social class, M the mediator variable, GPA, and C a set of observed control variables and the subscript *i* for referring to each student in the sample, we can define sequential ignorability as follows (Imai Keeley and Yamanoto 2010):

$$\{Y_i(x', m), M_i(x)\} \perp\!\!\!\perp X_i \mid C_i = c,$$
$$Y_i(x', m) \perp\!\!\!\perp M_i(x) \mid X_i = x, C_i = c,$$

Basically, sequential ignorability is satisfied if a) there are no unobserved pre-exposure covariates that influence both exposure and GPA or the transition outcome; b) There are no unobserved pre-exposure covariates that influence both GPA and the transition outcome.

Most recent methodological contributions agree that a critical aspect for conducting mediation analysis is the absence of exposure-outcome and mediator-outcome confounders given the set of control variables included in the model (Imai et al. 2011; VanderWeele 2015; Keele et al. 2015). In particular, Sequential Ignorability tends to be a very strong assumption in almost all mediation designs (Sobel 2008) and surprisingly it has not been considered or discussed in prior performance and choice research. This is probably because decomposition methods developed in sociology have followed the logic of the classic Oaxaca-Blinder (Blinder 1973; Oaxaca 1973) decomposition in economics which basically disregards the presence of confounding.

3. An empirical example of bias in performance and choice research and solutions for anticipatory decisions

Data and variables

For this first exercise I used the British and Swedish files of the Children of Immigrants Longitudinal Survey in Four European Countries (CILS4EU) dataset (Kalter et al. 2014, 2018; www.cils4.eu). CILS4EU consists of surveys completed by students (born around 1995/6) at age 14-15 (wave 1), 15-16 (wave 2), 16-17 (wave 3), their parents, and teachers (these last two interviewed only at wave 1) in England, Sweden, the Netherlands and Germany. This dataset has the advantage of providing rich information on socio-demographic background, academic performance, and educational transitions together with students' cognitive ability (CA), and aspirations. CILS4EU represents a unique opportunity to analyse performance and choice

effects because it contains all the standard observable variables recognized in the literature (e.g., family background, performance, and educational transitions) but also many other variables that are usually not observed in this kind of studies (e.g., students' CA and aspirations). I selected only England and Sweden because these two countries have similar education systems. Both countries are characterized by two key branching points (i.e., from lower secondary to upper secondary and from upper secondary to university) and do not have any early tracking differentiation such as Germany and Netherlands. This selection, therefore, allows me to increase the sample size without introducing further institutional heterogeneity (to be sure, such differences in educational systems may in other approaches be a rich source of variance that would aid in analysing institutional effects).

Since the main purpose of this exercise is to understand the risk of omitting certain factors or naively controlling for them in the decomposition of performance and choice effects, analyses were run on the pooled sample controlling for country of origin to optimise the sample size. Both countries have very similar institutional characteristics. In CILS4EU, schools with a high proportion of immigrants were oversampled by using a three-staged disproportional stratified sampling strategy. To account for these two specific characteristics of the sample, I controlled for country and the school stratum of ethnic composition.

As is often the case in panel studies, CILS4EU suffers from attrition and nonresponse. However, descriptive analysis shows that, after accounting for these two issues, the samples still remain representative of the national population of students. After attrition and listwise deletion of cases with missing information among key variables, the analytical sample consists of 4,119 (2,055 cases in England and 2,064 in Sweden). In the Appendix (Table A.2.1), I provide a detailed description of the sample characteristics and sizes before and after attrition.

The outcome in this study is the transition from lower secondary education (compulsory education) to the academic track of upper secondary education (post-compulsory education, grade 10 or ISCED 3). In both countries the academic track is the one which facilitates the access to university. This transition is captured by a dichotomous variable that indicates whether the student is attending the academic track at age 17. Parental ISEI is the main exposure variable and was derived from the higher of the two parents'/guardians' ISEI. This variable was subsequently divided into quartiles leading to four ISEI groups (i.e., high ISEI; high-mid ISEI; mid-low ISEI and low ISEI). For this variable, I took the parents responses as primary information and complemented with students' self-reported information if the occupations reported by the parents were missing. Further, the main mediator in this is study is the GPA variable which corresponds to the final grades in Math and English in GCSE exams for England and the grades in Math, Swedish and English for Sweden in the last year of compulsory education (grade 9, ISCED97 2). In both cases the information was self-reported by students. These GPA variables were z-standardized within countries.

In addition to these three key variables, I included country and school stratum (*survey characteristics*); students' gender and ethnicity, which is composed by natives/white, mixed and migrants categories (*individual demographic factors*), parental education which distinguishes between primary education or less, lower secondary, upper secondary and university (dominance criterion and information primarily taken from parents); an intact family indicator, coded as 1 if children are living with a lone parent or in a stepfamily and 0 if both biological parents are present (*further family background factors*); students' cognitive ability which is measured via a general cognitive test at age 14, and, finally, idealistic and realistic aspirations measured at wave 1, that is, one and two years before students take their final exams and make their transitions. These last three variables are assumed to be already affected by the family background (*endogenous controls*). Aspirations are also assumed to be independent

from the cognitive ability variable (students could not know their scores obtained in this cognitive test).

Description of decomposition methods and empirical strategy

In general terms, the strategies for decomposing performance and choice effects are mostly based on a two steps estimation procedure. This is known as differences in coefficients strategy (Alwin and Hauser 1975), e.g., estimating full and reduced models for the outcome; or product of coefficients strategy (Baron and Kenny 1986), e.g., estimating full models for the outcome and mediator. Both approaches provide the same results under conditional exogeneity and correct model specification. However, they cannot be employed when the outcome variable is not linearly related to predictors.

In educational stratification research, the most used method for non-linear decomposition is the odds-ratios decomposition developed by Erikson et al. (2005) and extended by Buis (2010) and Morgan (2012). The Buis method represents an improvement in the standard mediation methodology since it relaxes the normality assumption in the academic performance distribution and allows for interactions terms and multiple mechanism decomposition in binary-outcome settings. This method is based on estimating a full logit model, i.e., including the key exposure variable (social background), the mediator (academic performance) and controls, and then predicting for each individual the log odds of success on Y and compute within each group of the social background variable the average log-odds between groups. This exercise is then repeated assuming that low social background individuals have the academic performance of the high social background group (a counterfactual scenario). From these factual and counterfactual estimates, it's then possible to reconstruct total, indirect and direct effects. However, this approach can be seriously biased due to skewed distribution of the mediator and outcome, confounding, and scaling bias in the odds-ratios. On the one hand, Karlson and Holm (2011) showed that if the mediator or outcome variables are skewed, this

nonlinear decomposition method will provide biased estimates. These issues can be also magnified in the presence of confounding bias. On the other hand, as discussed by Breen, Karlson, and Holm (2013, 2018) and Mood (2010), the estimation of the full logit models and the prediction of the counterfactual quantities can be compromised due to scaling issues as well. Comparing estimates from logit models is risky due to the fact that effects are identified up to the scale. For example, the scaling issue could attenuate the estimate of the key exposure variable with the introduction of the mediator or other control variables independently of confounding issues or whether these variables are correlated. If this is the case, one could expect to underestimate the indirect effect since the difference between factual and predicted log odds in these groups would become larger. Moreover, the other extension of the Erikson et al. approach developed by Morgan allows to decompose effects in terms of probabilities, following the same logic as the Farlie decomposition in economics which, at the same time, is an extension of the Oaxaca-Blinder decomposition for nonlinear outcomes (Farlie 2013; Contini and Azzolini 2014). Although this is certainly an improvement since probabilities have an easier interpretation than odds ratios and can avoid scaling issues, this approach cannot handle more complex DAGs such as those discussed in Figure 1.1.

Empirically speaking, performance and choice effects in educational transitions have been directly assessed using the decomposition of odds ratio methods (Erikson et al. 2005; Buis 2010) or, indirectly, by estimating direct effect of family background, holding academic performance constant (i.e., without reporting the relative importance of performance and choice effects). None of past studies has attempted to use the methods developed in the causal inference literature yet and, therefore, their results rely on very strong assumptions about confounding. The potential outcome framework in causal mediation analysis represents an advantage in respect to the standard decomposition methods since it provides clearer definitions and assumptions for identifying direct and indirect effects. One of the most

important advantages is that the identification is independent of any estimation method and assumption regarding the distribution of the data.

The logic behind this approach can be briefly described by considering a binary exposure X where $x = 0$ and $x' = 1$ are the two exposure statuses, each individual has an outcome value Y equal to $Y(0)$ or $Y(1)$ depending on their exposure status x or x'. If it would be possible to observe both exposure statuses for the same individual, the causal effect at the individual level would be $Y(1) - Y(0)$. However, due to the 'fundamental problem of causal inference' (Holland, 1986), we are unable to observe the outcome for the same individuals since they are observed only under one exposure regime. The only solution to this problem is to estimate the causal effect of the exposure variable at the average level assuming treatment and control groups are exchangeable and there are no confounders $[Y(1) - Y(0)]$. This idea can be extended to mediation analysis by simply adding the variation produced by the mediator variable, M, among exposed and control group, fixing the mediator to a certain value m, i.e., $E[Y(1, m)]$ and $E[Y(0, m)]$. Similarly, we can also study the outcome holding the mediator constant at the natural value that would have been observed in the two-exposure status $\mathrm{M}(1)$ and $\mathrm{M}(0)$, leading to four potential scenarios in which the effect of the exposure X is mediated through M: $E[Y(1, M(1))]$, $E[Y(1, \mathrm{M}(0))]$, $E[Y(0, \mathrm{M}(1))]$, $E[Y(0, \mathrm{M}(0))]$.

As before, the scenarios $E[Y(1, \mathrm{M}(0))]$ and $E[Y(0, \mathrm{M}(1))]$ are not directly observed at the individual level but can be estimated considering the average effect. From these scenarios is possible to identify[5]; a) the Total Effect (TE) which is equal to $E[Y(1, \mathrm{M}(1)) - Y(0, \mathrm{M}(0))]$; b) the natural(pure) indirect effect (NIE) which is the effect of X on Y through M, when the mediator is held constant at the exposure status $E[Y(1, \mathrm{M}(1)) - Y(1, \mathrm{M}(0))]$; c) the total

[5] Note that Pearl (2001) uses only the term 'natural' direct and indirect effects for any exposure status in X and M, while Robins and Greenland (1992) distinguishes between pure and total direct and indirect effects depending on the exposure status of these variables.

natural indirect effect (TNIE), which is the effect of X on Y mediated through M, when the mediator is held constant at the control-group status value $E[Y(0, \mathrm{M}(1)) - Y(0, \mathrm{M}(0))]$; d) the natural(pure) direct effect (NDE), which is the effect of X on Y, while fixing M to the value that would naturally have been observed had the individual been in the control-group status $E[Y(1, \mathrm{M}(0)) - Y(0, \mathrm{M}(0))]$; e) the total natural direct effect (TNDE), which is the effect of X on Y, while fixing M to the value that would naturally have been observed had the individual been assigned to the exposure status $E[Y(1, \mathrm{M}(1)) - Y(0, \mathrm{M}(1))]$; f) the controlled direct effect (CDE), which is finally the effect of X on Y, while fixing the mediator at a predetermined value m. However, it is important to mention that the CDE would not make sense in performance and choice research because in reality is not possible to fix someone's GPA to a specific value.

The parameters to identify these effects can be obtained following a two-step procedure similar to the product of coefficient (a full model for the outcome and another one for the mediator) or the difference in coefficient approaches (reduced and full models for the outcome) complemented with simulation, standardization, imputation or prediction of the unobserved counterfactuals. When there are no post-treatment confounders, three common methods to identify the effects described above are V&VW (Valeri and VanderWeele 2013), IKY (Imai, Keele, and Yamanoto 2010), and KHB (Karlson, Holm, and Breen 2012).

If there are observed post-treatment confounders (i.e., when U is observed we labelled it as PC), we can employ the G-formula procedure (Daniel et al. 2011). This approach can be summarised in three steps. First, it obtains the appropriate distribution of each variable and models the mediator as function of the exposure and covariates E(M|X,C), the post-treatment confounder as function of the exposure and covariates E(PC|X,C), and the outcome as function of the exposure, mediators and covariates E(Y|X,M,PC,C). Second, it creates N copies of the

original sample and simulate covariates that are marginally independent between them. Then, it simulates the exposure X, which is also independent of the covariates, and each M, PC as well as Y as function of their ancestors using the parameters obtained in the first step. Finally, it regresses each different Y on the exposure to obtain the point estimates of each marginal effect using the pooled sample obtained from all the copies of the data and calculate bootstrapped standard errors. These new estimates account for the potential bias induced by the post-treatment confounder and should approximate the true effects of interest.

In this study, I first use conventional regression models to assess total and directs effects and then compare the Buis decomposition method with those developed in the causal inference literature, referred above. The main aim of this exercise is to understand how much our results change on the basis of the method and specification adopted once a certain causal diagram is assumed to represent the true relationship between variables, and not to infer how much IEO is explained by performance and choice effects in these two countries. The focus is, therefore, on the limitations of the current methodology and the description of the (potentially) best procedures from which to draw valid inference following the identification discussion of section 2.2.

Estimation of total and direct effects

Figure 1.2 displays the coefficients from a series of OLS/Linear Probability Model (LPM) regressions on academic performance and the probability of enrolling into an academic track at upper secondary education. The purpose of this exercise is two-fold. Firstly, to evaluate whether a significant association exists between the key variable, parental ISEI, and the students' GPA and transition outcome, while controlling for other relevant factors independently of the causal order of these variables. Secondly, to replicate the standard regression adjustment procedures commonly performed in the field of sociology of education, aiming to estimate socioeconomic disparities in educational outcomes, and describe how these

results would be interpreted. In this sense, several controls were included in a stepwise fashion and changes in the coefficients of key exposure variables measured. Three model specifications were used: a) the first model controlled for the school stratum of ethnic composition and country (*survey characteristic*), ethnicity and gender (*demographic factors*); b) the second model included the previous controls plus parental education and an indicator of whether student is living in an intact family; and c) the third model naively added GPA (only in the model for academic transition), CA and students' realistic and idealistic aspirations (endogenous factors).

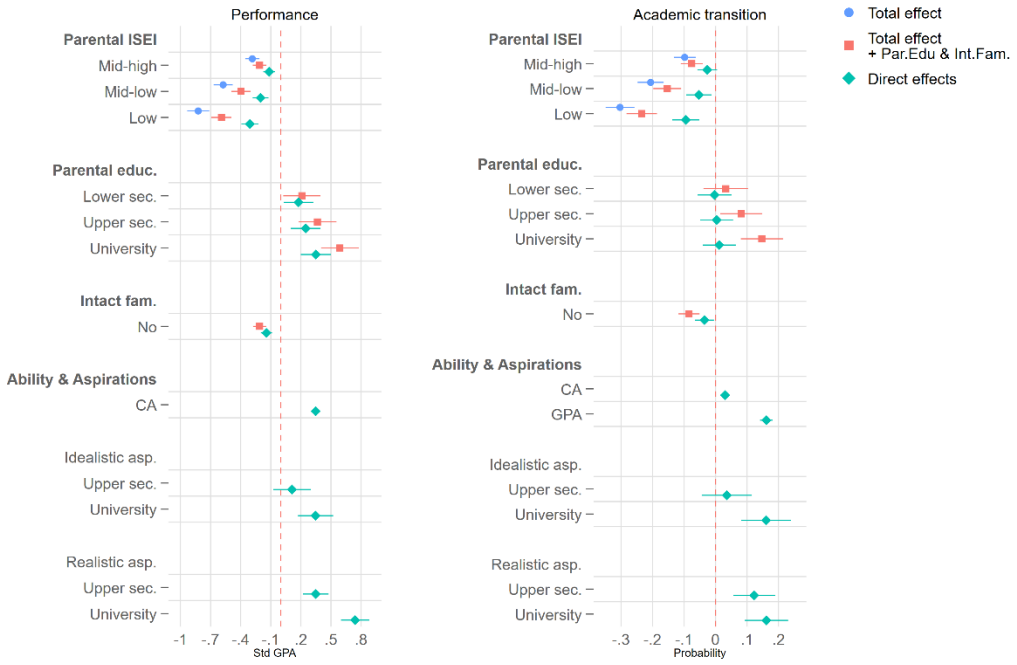


Figure 1.2: Total and direct effects – OLS/LPM
Note: 95% CI; adj. std err. at the school level; Total effect = Parental ISEI + country & school stratum + ethnicity & Sex. Coefficients for school stratum; country; gender and ethnicity are not displayed in the figure. N=4,119.

These sets of regressions show that all the variables under consideration have significant effects on the two outcomes. In particular, besides the standard social stratification variables, we can observe that intact family indicator, CA and aspirations are positively associated with academic performance and transitions to upper secondary education. The effects of the ability and aspiration variables are also substantially large in magnitude. For example, a 1 standard

deviation increase in CA produces an improvement of 0.35 standard deviations in GPA and 3 percentage points in the transition probability. In the case of aspirations, we also observe that those who idealistically and realistically aspire to obtain a university degree have higher GPA performance (0.35 and 0.74, respectively) and are more likely to attend an academic track in upper-secondary education (0.16 and 0.162, respectively). Regarding the main exposure variable, we observe that the effect of parental ISEI decreased slightly with the inclusion of parental education and intact family indicator (red squares, model with exogenous controls) and became much smaller after the inclusion of the endogenous controls (green diamonds). Also, it is interesting to note that the effect of parental education on transition outcome became null once student ability and aspirations were included. This might indicate that most of the parental education effect on educational transition is channelled through the transmission of cognitive skills and aspirations. Controlling for these mediators, in fact, reduces the total effect of parental education on the transition outcome.

With this approach we will conclude that parental ISEI continue to exert significant effects on IEO, although its coefficients become smaller once we account for ability and aspirations. That is, both mechanisms, performance and choice, seem to be important in absolute terms for these birth cohorts. However, we will not be able to say which one is relatively more important. Crucially, the coefficients of the direct effects of parental ISEI on academic performance and upper secondary transition could be seriously biased due to the inclusion of endogenous factors (blue circle coefficients) or the omission of other important exogenous ones (red squares coefficients). The risk of unintentionally introducing bias from over-controlling or conditioning on a collider is very high in this setting.

Methods comparison in performance and choice effects decomposition

Table 1.1 reports the total and indirect effects across Buis, V&VW, IKY and KHB techniques with five different model specifications while Table 2 reports the total and indirect effects for

the G-estimation technique with three model specifications. For the sake of exposition, I report only the results of the contrast between high-ISEI and low-ISEI groups. In Buis and V&VW the decomposition results are represented by odds-ratios while in IKY and KHB by average marginal effects. The rows (1) and (2) represent the DAG of Panel a in Figure 1.1, that is, the standard approach in performance and choice research. Precisely, the indirect path X→M→Y and the direct path X→Y, without account for potential unobserved variables U, e.g., X←U→Y, X←U→M, and M←U→Y. The only difference between specifications (1) and (2) is the inclusion of the variables that take into account the features of the sample (school stratum and country), which has only a marginal effect on the estimates.

Row (3) represents the DAG of Panel c in Figure 1.1. In this case we controlled for those variables that are usually considered as important X-Y and X-M confounders. That is, we estimate the indirect path X→M→Y and the direct path X→Y but this time including some controls variables C (i.e., unobserved variable U is replaced now with C and we have now the path X←C→Y and X←C→M). Subsequently, following the Sullivan et al. (2014) and Morgan et al. (2012) discussion, I included as further exogenous controls parental education, ethnicity, gender, and an intact family indicator which also entered in the set of exogenous controls C for closing other back-doors. Finally, the last two models aim at removing potential bias from anticipatory decisions, AD, by naively controlling for cognitive ability, CA, and aspirations variables. To the set of exogenous control variables described above, I first included CA alone, row (4), and then CA together with the two aspiration variables, row (5). Therefore, the model remains the same of Figure 1.1 panel *e*, but in this setting the potential endogenous mediator CA and aspirations are naïvely controlled for dealing with the potential unobserved U (M←U→Y and X→U→Y). That is to say, in order to estimate X→M→Y and X→Y we include further mediator variables as control variables, supposing that these control will account for anticipatory decisions.

Table 1.1: Total and indirect effects across different mediation methods

Model specifications	Buis		V&VW	
	Total effects	Indirect effects	Total effects	Indirect effects
(1) Without controls	0.25	0.43	0.19	0.37
	(0.027)	(0.029)	(0.022)	(0.028)
(2) With survey controls (Baseline)	0.24	0.41	0.20	0.37
	(0.025)	(0.023)	(0.025)	(0.030)
(3) All exogenous controls	0.25	0.41	0.28	0.51
	(0.032)	(0.025)	(0.044)	(0.038)
(4) All exogenous controls & CA	0.27	0.43	0.36	0.64
	(0.034)	(0.027)	(0.055)	(0.040)
(5) All exogenous controls & CA & Asp.	0.29	0.45	0.43	0.74
	(0.039)	(0.030)	(0.067)	(0.038)
	IKY		**KHB**	
Model specifications	Total effects	Indirect effects	Total effects	Indirect effects
(1) Without controls	-0.29	-0.18	-0.26	-0.16
	(0.018)	(0.011)	(0.015)	(0.011)
(2) With survey controls (Baseline)	-0.28	-0.17	-0.26	-0.15
	(0.020)	(0.013)	(0.016)	(0.011)
(3) All exogenous controls	-0.21	-0.11	-0.19	-0.10
	(0.025)	(0.012)	(0.020)	(0.010)
(4) All exogenous controls & CA	-0.17	-0.07	-0.15	-0.07
	(0.027)	(0.010)	(0.021)	(0.009)
(5) All exogenous controls & CA & Asp.	-0.13	-0.05	-0.12	-0.045
	(0.025)	(0.008)	(0.021)	(0.007)

Note: the estimates reported above are based on the contrast of High ISEI vs Low ISEI (N = 2,090); Buis and V&VW methods report odds-ratios while IKY and KHB Average Marginal Effects; standard errors in parenthesis; bootstrapped standard errors, 1,000 replications. Buis and V&VW reports changes in odd-ratios while IKY and KHB changes in probabilities.

The results displayed in table 1 show, first, substantial variation between the method developed by Buis and those from the causal inference literature (V&VW, IKY, and KHB) and, second, variation across model specifications. While the Buis model shows moderate changes in the odds-ratio of total and indirect effects, V&VW shows a more consistent declining pattern in these effects. These differences might be due to the scaling issues in Buis. However, it is difficult to understand the magnitude of the changes across specifications due the odds-ratio scale. The methods based on probabilities instead show a clearer picture,

providing both almost identical results between them and follow a similar declining pattern as in V&VW. Looking at IKY, we can notice that the inclusion of exogenous controls reduces the total effects from -0.28 to -0.21 and the indirect effects from -0.17 to -0.11. These effects are further reduced by the inclusion of CA and aspirations. The total effects become -0.13 and the indirect effects -0.05.

Generally speaking, if unobservable variables are positively correlated with the key variables, unobserved heterogeneity should upwardly bias the estimates in both the total and direct effects of interest (and the opposite if the correlation is negative), while the scaling issues in the odds-ratio decomposition and over-controlling should attenuate the direct effects. Similarly, Breen (2018) has shown that collider bias should downwardly bias the direct effects of the social class variable as well. Therefore, if there is omitted variable bias and collider bias, they will offset each other, and the total bias will depend on their respective magnitudes.

None of these methods, however, allows to us to tackle the bias from the post-treatment confounders yet. This exercise shows indeed that if we try to remove bias by controlling for the endogenous factors, the choice effects go up drastically as a result of over-controlling on the path (X→ U→ Y, see specification 4 and 5). If we do not control for them, the performance effects would become much larger due to the omitted path (X→ U→ M→ Y, see specification 3).

To demonstrate how much the coefficients change when we employ a method that adjusts for the post-treatment variables, I report below the results from the G-estimation using a stepwise inclusion of endogenous control variables. In this case, first row (specification 6) shows the results when all the exogenous control variables are included (i.e., gender, ethnicity, intact family, country of origin, school stratum, parental education and ISEI) and the endogenous CA is adjusted for. This initial model was further extended in the second row

(specification 7) by adjusting for CA and the aspiration variables in conjunction. Then, the model in the last row (specification 8) adjusts for the three endogenous variables but it removes the control for parental education. In this way, it is possible to observe how the coefficients change when we do not sufficiently control for X-M and X-Y confounders. This induces endogenous selection from the path C→ M←U→Y in the indirect effects and omitted variable bias in the total effect due to X←C→Y.

Table 1.2: Total and indirect effects accounting for post-treatment confounders in G-formula

	G-formula	
Model specifications	TCE	NIE
(6) Only CA	-0.098	-0.050
	(0.011)	(0.007)
(7) CA & realistic and idealistic asp.	-0.098	-0.047
	(0.012)	(0.008)
(8) CA & realistic asp. but without idealistic asp. and par. edu.	-0.119	-0.065
	(0.010)	(0.008)

Note: the estimates reported above are based on the contrast of High ISEI vs Low ISEI (N = 2,090); standard errors in parenthesis; bootstrapped standard errors, 1,000 replications. TCE = Total Causal Effect, NIE = Natural Indirect Effect. Robins and Greenland (1992) identifying assumptions.

Once we adjust for the post-treatment variables (specification 7), the G-formula shows slightly smaller total and indirect effects than the standard causal methods. This method, however, is also highly sensitive to the model specification. If we remove the control for parental education and one of the post-treatment confounders, idealistic aspirations, we observe that both effects become larger. One more time we observe, therefore, that if we do not sufficiently control for all the relevant exogenous factors such as parental education, ethnicity, and the intact family indicator, the decomposition is compromised. On the one hand, the omitted variable bias amplifies the total effects. On the other, the collider bias, if larger than the omitted variables bias, inflates the indirect effects, reducing by symmetry the direct effects.

However, looking only at the coefficients from the effects decomposition might not be enough to understand how much our conclusion can be biased due to the arbitrariness of the

method and model specification. A better measure to grasp all changes in the obtained estimates is the proportion mediated (PM). This is obtained by computing the ratio between the indirect effect and the total effect. The main advantage of the PM is that it facilitates the comparison between methods since this estimate is based on the same scale, i.e., a ratio between two quantities. In addition, the PM is the final estimate of interest from which inference regarding the relative importance of performance and choice effects is usually drawn. That is, it provides a clear quantity of the magnitude of these two mechanisms.

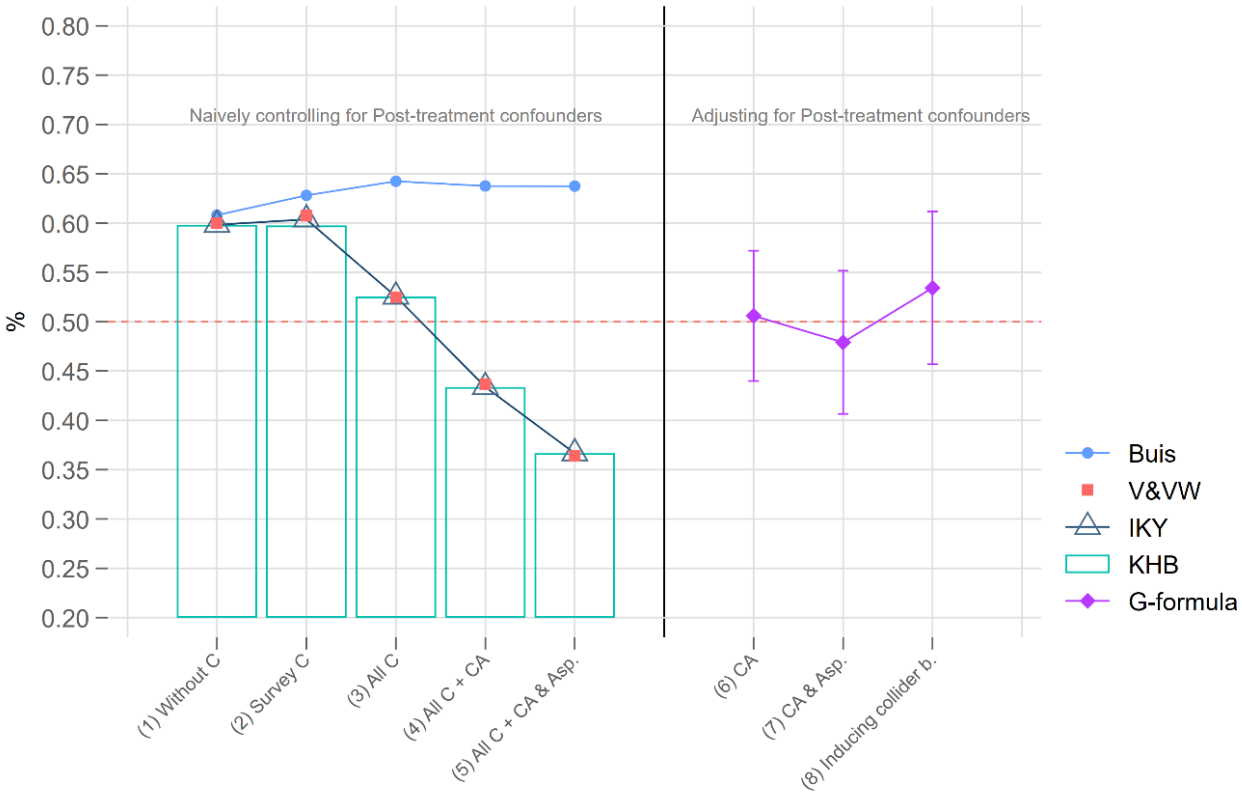


Figure 1.3: Proportion mediated across different methods
Note: PM derived from coefficients in table 2 and 3; G-formula PMs include 95% confidence intervals. The horizontal red line represents a 50% threshold. Values above this threshold indicate that performance effects are relative more important than choice effects and vice versa. The choice effects can be obtained from proportion unmediated (1-PM).

Figure 1.3 displays the PM across the five methods and model specification employed in the decomposition of total and indirect effects (Buis, V&VW, IKY and KHB on the left side

and G-formula of the right side). Without Covariates or simply controlling for the sample characteristics, Buis, V&VW, IKY and KHB show very similar PMs (specifications 1 and 2).[6] However, the PM starts to differ substantially between Buis and the other three methods when students' demographics and family variables are included. In particular, the PM in Buis method increases once the exogenous controls (X←C→M; X←C→Y) are included (4) and slightly decreased (5) with the inclusion of the endogenous controls (X→U→M; M←U→Y). This method shows an average PM of circa 63% across the five model specifications. This means that, if we employ Buis methods, we will find that performance effects are relatively larger than choice effects. In the case of the causal methods, V&VW, IKY and KHB, the inclusion of the exogenous controls decreases the PM (3), remaining yet above 50% in this specification. When controlling for the endogenous factors this is further reduced up to the point in which the relative importance of performance and choice effects is completely reversed (4 and 5). The G-formula shows that once we adjust for the post-treatments confounders, the PM obtained is in-between those of the other methods (6 and 7). This confirms that there is a strong selection process into the mediator that can distort the conclusions regarding the relative importance of performance and choice effects. In other words, the AD mechanism is indeed an important issue in the decomposition of performance and choice effects and the way we try to deal with it seriously affects our results. Additionally to these empirical exercises, in the Appendix, section A.1.3, I report a simulation study that further explores how sensible is the proportion mediated to different combinations and degrees of bias. This simulation provides a further example of the issues discussed in Section 2 and makes clear that already small amounts of bias from exposure-outcome, exposure-mediator and mediator-outcome confounders can change the relative importance of choice and performance effects, making also difficult to

[6] Although V&VW, IKY and KHB are based on different structural equations (i.e., the first two on product of coefficients, the latter on difference in coefficients), their estimates are fairly similar because these methods identify exactly the same quantities in terms of potential mediated outcomes.

determine the direction of the bias. This should make scholars aware of the limitations of their decomposition approaches for quantifying the relative importance of performance and choice effects. Overall, the main conclusion from these analyses is that the standard odds-ratio decomposition methods typically used in this research, the Erikson et al. or Buis, provide biased estimates. They overstate the magnitude of the relative importance of performance effects independently of the model specification, and the bias seems to be quite difficult to detect given the small variation in the estimates. In fact, this method could mistakenly be interpreted as robust due to the stability of its results across the different specifications. The alternative methods developed in the causal mediation literature, on the other side, are also biased, but in the opposite direction. The choice effects in this case are relatively more important than performance effects. The more we naively control for potential mediators of the X-M and X-Y relationship, the more we artificially increase the direct effects due to over-controlling. However, one positive aspect of these methods is that, if the endogenous factors are included in a stepwise manner, they still allow us to see the selection process into the mediator.

Furthermore, it is quite worrisome that past studies have not provided any estimates of the uncertainty around the proportion mediated or unmediated. Since the main scope of this research is to assess whether one component is relatively bigger than the other, confidence intervals (CI) should always be reported. For example, the CI obtained with G-estimation show that it is not possible to claim that one effect is bigger than the other since they overlap with the 50% threshold in all specifications (although each of these effects are still statistically different from zero).

Sensitivity analysis

A final consideration that has not been taken into account in performance and choice research is the possibility of testing the validity of the invoked assumptions for decomposing indirect/direct effects through sensitivity analysis. This technique seeks to assess the impact of

potential omitted variables on the causal parameter of interest. In general, its implementation consists in defining a sensitive parameter, ρ, that represents the degree of violation of the exogeneity assumptions. Then, this parameter is set to different values in order to observe at what point the causal effect would disappear completely or become statistically insignificant (Gangl 2013). More specifically, we ask how substantial the bias from the omitted variables would have to be to change our conclusions. This is the best existent solution to support the validity of mediation analysis in settings of pure conditional exogeneity. However, the type of sensitivity analysis will vary depending on the type of DAG considered and the assumptions included in the model. In the supplementary material, I provide a full description of the techniques available.

In this case, academic performance could be also affected by school factors and these variables are not present in the public data. Unobserved school characteristics, therefore, can be considered as mediator-outcome confounders that could violate the Sequential Ignorability assumption of the models presented above. To test this, I employed the Imai, Keele and Yamanoto (2010) sensitivity analysis which is based on the IKY models employed in Table 1.1 (specification 3).

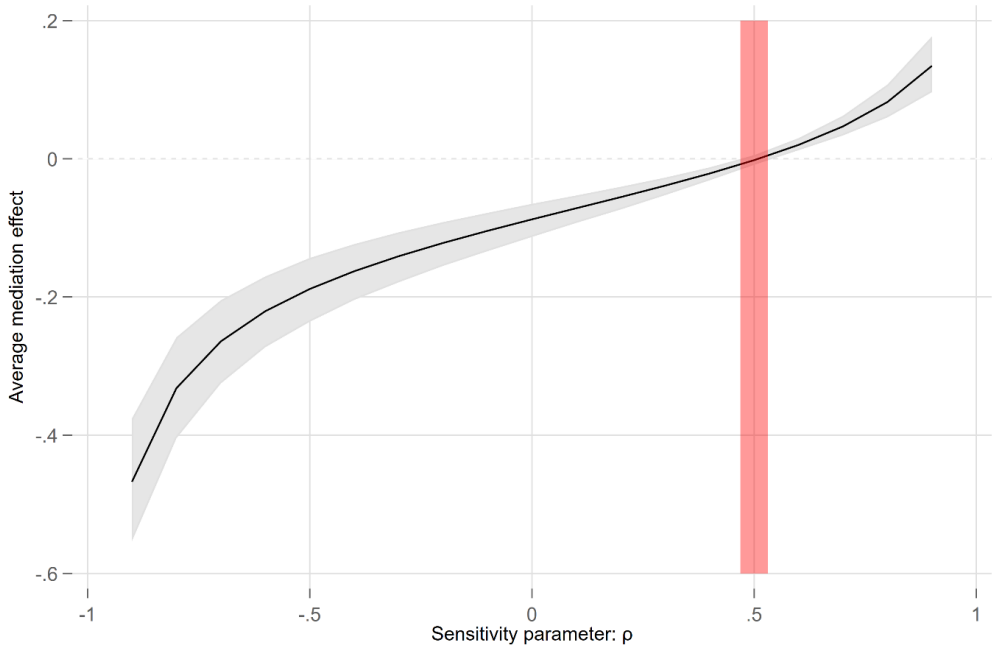


Figure 1.4: Sensitivity analysis for mediator-outcome confounder
Note: 95% confidence intervals.

The Figure 1.4 shows that a potential mediator-outcome confounder should have a correlation with the observed variables of at least .5 to make the mediation effects equal to 0. This indicates that these results are quite strong to mediator-outcome confounders and this should not represent the main source of concern when evaluating the plausibility of the Sequential Ignorability assumption. However, other types of confounding bias are still plausible so these results should be taken with cautions.

4. Conclusion and Discussion

The Boudon's model of performance and choice effects was one of the first approaches to analyze in conjunction the mechanisms that generate IEO. Analyzing performance gaps or inequality in educational transitions separately is useful but not exhaustive enough to fully explain how IEO is generated and perpetuated over time. In fact, if these two mechanisms are not considered in conjunction, we risk misidentifying their relative importance and deliver ill-informed policy advice. In other words, the performance-choice distinction represents a more

comprehensive way to study IEO and has the potential to become useful for informing policy about how to reduce inequality.

Given the data and methodology available at that time, past literature in sociology of education made crucial theoretical and empirical contributions to our understanding of how educational opportunities are determined by social background. From a methodological point of view Erikson et al. (2005) was the first paper in the sociological literature that provided a method for decomposing odds-ratio. Then Morgan (2012) revised the assumption for identification and introduced the importance of conceptualizing the educational transition model, typically used in performance and choice research, in causal terms using diagrams called directed acyclic graphs. However, none of these contributions tried to address the problem of post-treatment confounders in this model and provide solutions and robustness checks to make analyses more robust.

With these contributions as a backdrop, the first objective of this study was to provide a more comprehensive framework for separating and quantifying the relative importance of performance and choice effect in the presence of post-treatment confounders such as anticipatory decisions. The second objective was to introduce the importance of conducting sensitivity analysis and provide confidence intervals for the estimates of the relative importance of performance and choice effects. I did that by discussing identification issues through a causal lens and investigating the performance and sensitivity of past and new mediation/decomposition estimators. Making identification assumption and methodological limitations more transparent as well as complementing results with sensitivity analyses, would improve the quantification of performance and choice effects, enabling the possibility of informing policy makers about when and how to intervene for reducing IEO. For instance, if performance effects are relatively larger in a certain society, policy makers should implement early interventions in the pupils' educational trajectories to reduce performance differentials.

However, if choice effects are larger, it would be better to intervene at later stages of students' career, for example by providing counseling programs to help them to make more efficient choices.

At the moment, *identifying* the relative importance of performance and choice effects presents several difficulties. First of all, there are different types of confounding bias that can be introduced into the analysis when decomposing performance and choice effects. Namely, *exposure-outcome*, *exposure-mediator*, *mediator-outcome* and *post-treatment confounders*. This last type of confounding bias can be more problematic since it cannot be tackled by standard regression adjustment. Controlling for a post-treatment variable or not produces bias in the estimates of interest. Second, conditioning on endogenous variables such as academic performance can also bring endogenous selection since these variables are very likely to be not only mediators but also colliders. Conditioning on a collider or a descendant of a collider also biases estimates and the only way to remove it, if this is needed to perform mediation, is to close all the back doors from this variable.

In this regard, anticipatory decisions, a mechanism already identified by Erikson et al. (2005) as a major threat for the separation of performance and choice effects, can be conceptualized as a post-treatment confounder since it represents '*early choice effects*' that are affected by social class and have a direct effect on school performance and educational transitions as well (Jackson 2013). More precisely, the anticipatory decisions mechanism is basically a process of selection into the mediator and outcome. The literature, however, has not provided clear ways of dealing with it. Some interesting insights into this issue were provided by Jackson et al (2007), Erikson and Rudolphi (2010), and Rudolphi (2011), who proposed to use early measures of ability and aspirations. Yet, their approaches remain suboptimal due to the impossibility of removing the post-treatment confounding bias. Therefore, further research

is still needed on this matter, especially because it is not clear which variables are part of anticipatory decisions and which ones should be considered as separate mechanisms.

From the *quantification* standpoint, the theory in performance and choice effects have become very rich but its methodology still lags behind. Past research has only employed the Erikson et al. 2005 method which is based on the decomposition of odds-ratios. This method presents several limitations in terms of confounding, collider, and scaling bias. Using a dataset that contains rich information on social background, academic performance, cognitive ability, aspirations, and educational transitions, I found that this method tends to overstate the magnitude of performance effects independently of the model specification. Given the vast estimation possibilities that have become available in recent years, new methods should be considered in order to reconciliate the theory with its methodology. In this regard, by using causal mediation approaches we can make the identification assumptions more transparent and refine the way of quantifying the indirect and direct effects. As shown in this study, the bias in one of the estimates (total, indirect, and direct effects) will always compromise the calculation of the proportion mediated or unmediated; that is, the measures of the relative importance of performance and choice effects.

In terms of how to improve the estimation procedure some important advice and conclusions can be drawn from the empirical exercises conducted in this paper. First, estimating the total effects in a proper way is a crucial step because it represents the denominator of the proportion mediated and provides a picture of the IEO in a certain society. Therefore, confounding bias from omitted pre-exposure variables should be the major source of concern when estimating total effects. According to Breen and Goldthorpe (1997), social class or socioeconomic indicators based on parental occupations are good proxies for capturing economic decision-making and loss-aversion mechanisms. Therefore, it is important to remove

the variation in educational transitions that is due to other social background components (e.g., parental education, family composition, and ethnicity).

Second, the decomposition of the total effect into indirect and directs effects is the hardest part because once conditioned on the mediator further confounding and collider bias can be introduced in the estimates. The possibility of conditioning on colliders or post-treatment confounders should always be evaluated before conducting any analyses. If there are post-treatment confounders, the G-computation could be a promising technique to deal with it since it allows to include these variables in the model without introducing over control bias in the estimates. G-estimation is currently the best solution for removing bias from post-treatment variables especially when the outcome is nonlinear. Importantly, as discussed over this chapter, the methods developed in standard and causal mediation as well as effect decomposition literature are always based on the capacity to include all the relevant control and mediator variables. They always have to impose ignorability and sequential ignorability assumptions to make the inference valid. Also, an important part of empirical strategy and identification relies on the capacity of the researcher in designing a proper causal diagram and placing the variables in the right order, for example separating exogenous from endogenous variables, and selecting the best estimator. Different causal diagrams and the inclusion or omission of certain variables might provide different estimates. In this regard, quite important is the utilization of theory and previous evidence for defining testable mechanisms and selecting the most relevant variables available in the data.

Third, the proportion mediated or unmediated should be accompanied by confidence intervals to determine whether there are significant differences between performance and choice effects, and the validity of the imposed assumptions should be tested through sensitivity analysis. Worth mentioning again that mediation techniques requires fairly strong assumptions, such as sequential ignorability, which can be easily violated due to omitted variables.

Unfortunately, these two key aspects have been completely ignored in past research. At the moment, the best solutions to conduct mediation are based on accounting for the most important confounding factors and test through sensitivity analysis and robustness checks how strong the effects of unobservable variables would need to be to negate our conclusions. In this sense, this paper looks at informing social stratification researchers about the caveats of performance and choice decomposition and best procedures to avoid bias and improve transparency in the analyses.

Finally, besides estimation procedures, many of the comments and arguments above point to the importance of getting better data in order to trace all the potential paths/mechanisms. Data improvements will give the possibility to account for all the relevant mechanisms and reduce the risk of introducing measurement error in our key exposure variables. For example, in the absence of survey data on aspirations and cognitive ability, linking survey data to school register data sources could make it possible to get longitudinal measures of academic performance in order to trace when the student starts to perform below their capabilities, and to correct also potential measurement error in the mediator variable. Measurement error is an issue that has not been discussed in this study but it is equally important as confounding and collider bias. If we employ variables with measurement error, the estimates will be attenuated toward the null, affecting again the calculation of the proportion mediated or unmediated.

Chapter 2:

Decomposing Inequality of Educational Opportunity: A new account of performance, choice and anticipatory decision effects in transitions to upper secondary education in England and Sweden

Abstract

Using rich longitudinal data from Children on Immigrants Longitudinal Survey in Four European Countries (CILS4EU), this paper presents new estimates of Inequality of Education Opportunity (IEO) in transitions to upper secondary education for birth cohorts born around 1995/96 in England and Sweden. Further, it is also assessed whether changes in IEO correspond to changes in performance and choice effects taking into account potential endogeneity from anticipatory decisions. Results indicate that IEO has continued to decline among 1995/96 birth cohorts. However, in comparison with previous studies, once anticipatory decisions are properly addressed and confidence intervals are constructed, choice effects are much larger than those previously reported for older cohorts in both countries. In the case of England both mechanisms seem to be equally important while in Sweden performance effects remains relatively higher. The implications of these findings are discussed in the lights of potential policy interventions.

1. Background and research objectives

Understanding the main determinants of different educational decisions is key for designing policy interventions targeted at reducing inequality of educational opportunity (hereafter IEO) (Heckman, Lochner, and Todd 2006; Jackson 2013) and improving social mobility (Breen and Müller 2020). Since mid-60s sociologists have shown that IEO mainly depends on two specific mechanisms (Keller and Zavalloni 1964; Boudon 1974) labelled as primary (performance effects or PE hereafter) and secondary (choice effects or SE hereafter) effects. These mechanisms relied on a theoretical model formally developed by Boudon (1974) and subsequently extended by Erikson and Jonsson (1996) and Breen and Goldthorpe (1997).

Performance effects are the portion of IEO explained by social class differentials in school tests, or grades, while choice effects are the remaining portion of social class disparities in educational transitions, that is, the effects of social class at similar levels of academic performance. In other words, an initial mechanism that explains IEO is that students from the most advantaged backgrounds are more likely to outperform their peers from less advantaged backgrounds at school exams. This then leads into lower transition rates to post compulsory education among working-class students as very often a good academic performance is key for educational success (and sometimes admission) at higher educational levels. On top of this, there is a second mechanism that explains the generation and perpetuation of IEO in advanced societies. Even when students show very similar school performance, students from lower socio-economic background still tend to choose less ambitious educational paths (Boudon 1974; Erikson et al. 2005; Jackson 2013). Differences in choices may be generated partly because of differences in resources, but also because families are principally concerned with avoiding social demotion (Erikson and Jonsson 1996; Breen and Goldthorpe 1997). Families from more advantaged background are thus more motivated to achieve post-compulsory education degrees that secure their offspring a position in the social structure that is as good as

their position of origin. According to the performance and choice theory each educational decision is related to an evaluation of the estimated cost and benefits in the light of the perceived probability of success in achieving the next educational level. Families from less advantaged background are not only less likely to meet the costs of higher education, but are also more loss averse (Kahneman and Tversky 1979) and prefer to enter in safety-educational-nets like those that provide rapid occupational entry and economic returns (Erikson and Jonsson 1996; Breen and Goldthorpe 1997). The presence of performance and choice effects have been empirically proven in several European countries (for a summary see Jackson 2013). Also more recent studies have confirmed some the Erikson and Jonsson (1996) and Breen and Goldthorpe (1997) predictions, showing that cultural capital is associated with schooling outcomes while loss aversion with school ambitions which could be referred as part of the choice effects (Van de Werfhorst and Hofstede 2007).

Recent research suggests that IEO in post-compulsory education decreased across birth cohorts born in late 1970s and 80s in several countries (Jackson 2013;), following the pattern already documented for older birth cohorts (Breen and Jonsson 2005; Breen et al. 2009). The first objective in this study is to analyse whether IEO has continued to decrease in birth cohorts born around mid-90s and to what extent this could be driven by changes in the relative importance of performance and choice effects.

The birth cohorts considered in this study went through the educational system in a period of educational expansion with reduced direct educational costs and more equal circumstances and living standards in childhood. If this is the case, following Erikson and Rudolphi (2010), we would expect decreasing IEO in transitions to upper secondary education. However, net of the absolute increase in living condition in these societies, if there are still differences in parents' investment during childhood, we would expect performance effects to remain larger than choice. For example, this could be the case if parents from lower socioeconomic

background having enough resources (own-generated or available from the public sector) do not properly invest them in their offspring.

Not only predicting, but also quantifying the relative importance of performance and choice effects for this cohort poses, however, several challenges that have not been carefully evaluated in the past. Models of performance and choice effects rely on very strong assumptions regarding confounding and collider bias (Morgan 2012; Morgan, Spiller, and Todd 2013; Breen 2018). Those two issues are likely to distort the estimates of the relative importance of performance and choice effects. Therefore, the second objective in this study is to employ more rigorous decomposition strategies developed in the causal mediation literature, together with better data, to properly identify and quantify the relative importance of performance and choice effects.

Using rich longitudinal data from Swedish and British files of the Children on Immigrants Longitudinal Survey in Four European Countries (CILS4EU), I am able to incorporate some important factors, especially cognitive ability and aspirations, which are usually omitted or unobserved in this type of studies. These are particularly important for reducing endogeneity from anticipatory decisions, which could be seen as early choice effects that affect performance (Erikson et al. 2005; Jackson 2013). In this way, I also assess whether past results might be affected by suboptimal identification strategies. In this sense, if there is a selection into academic performance due to anticipatory decisions, we would expect larger choice effects once our models take these into account. Anticipatory decisions might signal changes in student motivation and effort at school as well as mental health issues and lower well-being.

The results from this study indicate that, first, IEO has been further reduced for 1995/96 birth cohorts in England and Sweden. Second, choice effects seem to be almost as important

as performance effects in England while in Sweden performance effects remain larger than choice effects, although the difference is not as large as previously found in older cohorts. That is, this second finding differs from what has been reported in several previous studies where performance effects were responsible for at least three quarters of the total IEO in birth cohorts born in early 80s and 90s. Part of this contrasting finding can be explained by the fact that the standard Erikson et al. (2005) decomposition method is likely to overestimate performance effects. Once we account for mechanisms that produce selection into the mediator, e.g., anticipatory decisions, performance effects become smaller. However, the variation in findings cannot be fully accounted by methodological differences, suggesting that some generational changes have taken place as well.

I will first shortly describe the educational school system in England and Sweden and discuss the theoretical bases behind the performance-vs.-choice-effects distinction, its limitation, as well as past findings for these two countries. After describing the dataset and the methodological approaches employed in this study, I present the main results together with robustness checks and conclude with a discussion of the main findings and its implication for policy.

The English and Swedish school systems

The transition from lower to upper secondary education is the most important branching point in the British and Swedish educational systems. In both cases, at age 16 students get their final grades. After that, they need to decide whether to stay in full-time post compulsory education or entering into training/employment. If they stay in the educational system, they also need to opt between academic or vocational education, the former being the best assurance of gaining university entrance. This is, therefore, the best transition point to study performance and choice effects since the decision is almost entirely based on parents' and students' preferences and cost-benefit evaluations. Further transitions to tertiary education do not depend entirely on

them. This is particularly the case for England, where students need to apply before taking their A-level exams, and where universities offer places in their degrees on the basis of certain formal requirements and performance at interviews (Dilnot 2018). Also, the criteria for being admitted to a university degree becomes more selective among prestigious universities. In the case of Sweden, there is more room for individual choice in the transition to tertiary education and universities are less stratified in terms of prestige, but admission to the most prestigious programmes at tertiary level is heavily dependent on grades from the academic programme at upper secondary school. Obviously, the configuration of the university system can also affect the decision to continue into upper secondary education or not.

In contrast to past studies, I analyse the educational transitions of a much more recent cohort, students born between 1995 and 1996. These students took their lower secondary exams in the years 2011/12 and made their transition to upper secondary education soon after. In comparison with older birth cohorts, these cohorts benefited from universal secondary education and considerably reduced costs for attending post-compulsory education in both countries. The secondary education enrolment rate in 2012/13 was around 90 percent in England and 93 in Sweden (OECD 2021) while the early leavers from education rate as calculated by Eurostat (2021) remained low (12 in England and 7 in Sweden). This is a period in which mass participation into secondary education has been high and stable.

Performance, choice, and anticipatory decision effects

The performance-vs-choice-effects distinction has been conceptually employed in the majority of European studies on IEO since late 90s, the first empirical paper published in an academic journal that quantified the relative importance of these two mechanisms was Erikson et al.

(2005).[7] They provided two key contributions to the literature. First, they developed a decomposition method, based on the work of Erikson and Jonsson (1996), which was later extended by Buis (2010). Second, they also recognised that performance and choice effects do not act independently of each other in the formation of IEO. This issue is usually manifested in terms of anticipatory decisions. That is, if students anticipate that they are not going to participate in post-compulsory education, they may decide to put less effort at school and consequently achieve lower academic grades (Erikson et al. 2005; Jackson 2013). Anticipatory decisions represent 'early choice effects' that are affected by social class and have a direct effect on school performance and educational transitions as well (Jackson, 2013). Even if the problem was identified, none of the later contribution were able to tackle it, and sometimes it was simply ignored due to data limitations as more information on potential proxies of anticipatory decision is necessary for analysing this issue.

The majority of the studies in England and Sweden have shown that for birth cohorts born in mid-60s until 1990 IEO consistently declined, and performance effects, operationalized as Grade Point Average (GPA), were relatively more important than choice effects in cohorts born around the 80s and 90s (Rudolphi 2013; Jackson 2013). Also, it has been found that these two countries had very similar pattern over time which may be explained by the similarities of their educational systems.

The only studies that evaluated the existence of anticipatory decision effects in these two countries were Jackson et al. (2007) for England, and Erikson and Rudolphi (2010) and Rudolphi (2011) for Sweden. The first two proposed to analyse performance and choice effects using GPA and cognitive ability (CA) as interchangeable mediators and then examining the

[7] Erikson and Jonsson were the first to conduct the decomposition of performance and choice effects finding that these were equally important (50/50). This analysis was published in a Swedish book in 1993 and then briefly commented in their 1996 book.

differences between the estimates of the two decomposition. Both studies found that performance effects were much smaller when cognitive ability was used instead of GPA and the relative importance of these effects was actually reversed. According to Erikson and Rudolphi (2010), cognitive ability tests are more measures of potential than of real performance. Hence, they can be used as measures of initial scholastic ability as these should be less affected by anticipatory decisions. The third paper, Rudolphi (2011), controlled for idealistic and realistic aspirations assuming that these are proxies for anticipatory decisions about future education. Aspirations can be classified into two types. There are idealistic aspirations which represents unconstrained wishes and, then, realistic aspirations which refer to anticipations of future educational attainment taking perceived constraints and opportunities into account. The inclusion of aspiration variables, however, did not change the results regarding the relative importance of performance and choice effects. Yet, naively controlling for aspirations is also problematic because these are mediator variables in the chain between social class and GPA, and between social class and the educational transition, respectively.

In this regard, a recent study conducted by Holtmann, Menze, and Solga (2021) has shown that aspirations and cognitive ability are strong mediators in the association between parents' and children's educational attainment in Germany, suggesting that both measures should be used in this kind of studies and treated as proper mediators instead of exogenous controls. Obviously, where to place aspirations in the causal chain would depend on the moment when these variables were measured and the causal status that the researcher ascribe to them. Therefore, the modelling strategy for GPA, cognitive ability and aspirations should be evaluated very carefully. Not controlling for CA when using GPA as main mediator bring bias from standard confounding since some important noncausal paths remain open. However, naively controlling for CA or aspirations when using GPA as main mediator also brings bias from over-controlling since some important paths are now blocked. Proxies for anticipatory

decisions are post-exposure confounders, that is, variables that affect mediator and outcomes but at the same time are also affected by the exposure.

A more recent example of questionable controlling strategies due to the inclusion of post-treatment confounders in educational inequality literature can be found in economics. Economists so far have never paid attention to the analytical distinction between performance-versus-choice effects developed by sociologists, or, at least, they have never made it explicit in their papers. However, they have also considered the importance of analysing in conjunction the relationship between family background (typically in terms of economic resources), academic performance and educational choices when studying IEO (see Chowdry et al. 2013; Crawford et al. 2016). Using rich register data, these studies analysed the socioeconomic gap in university attendance after controlling, in a stepwise fashion, for all grades available at UK formal examinations over the scholastic trajectory. With this empirical strategy they found no, or a very small socioeconomic gap in university transitions. However, their empirical strategy does not consider that academic performance measures are endogenous to the socioeconomic background. As already mentioned, naively controlling for post-treatment variables increases the risk of introducing over-controlling and collider bias, reducing the effects of socioeconomic background on educational transitions and attainment.

The models that I adopt in this study address the issues discussed above. First, I include several post-exposure variables to reduce potential endogeneity from confounding in the total and indirect/direct effects estimation. Second, I treat cognitive ability and aspirations (proxies for anticipatory decisions) as variables placed in the pathway between parental class and GPA and allow them to also affect choices. In the methods section I provide a graphical representation of the model and further details regarding the identification of total, indirect and direct effects together with the estimation strategy.

2. Data and Empirical strategy

Data

Data for this study are draw from three waves of Children of Immigrants Longitudinal Survey in Four European Countries (CILS4EU) (Kalter et al. 2014, 2018; www.cils4.eu). CILS4EU is a longitudinal survey of adolescents in which the main scope is to study structural, social, and cultural integration of youth of immigrant origin in England, Sweden, Germany and the Netherlands. The samples are based on a two-step cluster design. School represents the first sampling unit. Then, from each school, two classes were randomly selected and all pupils within these classes interviewed. In addition, in wave 1 also at least one parent in the child household was interviewed. Since the focus of CILS4EU is on immigrants, schools with a high proportion of immigrant youth were over-sampled. All analyses were performed taking into account the sample characteristics. In wave 1 a total of 4,315 pupils (107 schools) were interviewed in England and 5,025 pupils (129 schools) in Sweden. As is often the case in panel studies, CILS4EU suffers from attrition and nonresponse. However, descriptive analysis shows that, after accounting for these two issues, the sample still remains representative of the national population of students. At the moment, the sample differs depending on whether I used only the survey data, or a version combined with Swedish register data (more details below).

After attrition and listwise deletion of cases with missing information among key variables, the analytical sample consists of 4,119 (2,055 cases in England and 2,064 in Sweden) when only the survey data is used and 6,636 (4,581 in Sweden) when the Swedish file is complemented with register data. The register data for Sweden allowed to reduce attrition/missingness in the GPA and educational transition variable. Unfortunately, for England it was not possible to complement the survey data with register information. For this reason, as a robustness check, I replicated the main analysis using an imputed dataset of the British files. This was done using multiple imputation with chained equations for the outcome,

mediator and parental education (i.e., the variables with the largest number of missing cases due to attrition and non-response). Results between imputed and not imputed data for England do not change. In the case of Sweden the variables with missing information on grades and academic transitions were replaced with register data and thus the imputation was not necessary. In the Appendix, I provide a detailed description of the sample characteristics and sizes before and after attrition for both countries (Table A.2.1), showing that the composition of the sample does not change.

For this study, I only focused on England and Sweden. These two countries are characterized by low stratification and selectivity and clear vertical branching points in the educational system. In addition, these two countries have a long tradition in this type of research and, therefore, it is also possible to compare the results of these two new birth cohorts (1995/6) with a long series of past results for cohorts born around 1960s till 1990/1.

Variables

The outcome in this study is a dichotomous variable that captures the transition from lower secondary (compulsory education, grade 9 or ISCED97 2) to the academic track of upper secondary education (post-compulsory education, grade 10 or ISCED97 3) at the age of 16/17. As mentioned before, in both countries the academic track is the most common root to university.

The main exposure variable is the socio-economic index of occupational status (ISEI, Ganzeboom, De Graaf, and Treiman 1992). This variable was derived from the highest ISEI among the two parents/guardians. This continuous measure was subsequently divided into quartiles leading to four groups: high ISEI; high-mid ISEI; mid-low ISEI and low ISEI). For this variable, I took the parents responses as primary information and complemented with students self-reported information if the occupations reported by the parents were missing.

Most of the past studies in England and Sweden employed a social class variable based on the traditional EGP/ESeC models with three macro categories. However, social stratification scholars have demonstrated that the correlation between ISEI and these social class models tend to be very high (i.e., 0.90 see Ganzeboom and Treiman 1996).[8] As a sensitivity check I also compared total effects using the ISEI groups and a simplified ESeC variable with three categories derived using ISCO08 2-digits. As before, I reported these additional analyses in the Appendix section (Tables A.2.6a and A.2.6b).

The main mediator in this is study is academic performance (GPA) which was obtained from students' self-reported variables in both countries. In the case of England, I followed Jackson et al. (2007) and Jackson (2013) who derived GPA using the final grades in Math and English at the General Certificate of Secondary Education (GCSE) public examinations. These examinations are usually taken when students turn 16 and defines the end of compulsory education. For Sweden, I used the grades in Math, Swedish and English in the last year of comprehensive school (grade 9) which also correspond to the age of 16 ('meritpoäng'). These are the only three variables available in the public Swedish files of CILS4EU. However, upon access to the register Swedish data, I was able to employ a GPA variable derived from all the teacher-assigned grades (following national guidelines) which comprise 16 different subjects. This GPA measure ('meritpoäng') is given upon finishing comprehensive school and a medium-stake measure that is used for allocation of students to upper secondary school programmes. Finally, to enable comparability, these GPA variables were z-standardized within countries.

Students' cognitive ability is measured via a general cognitive test at age 14 (wave 1). This variable was generated from a grade 8 pattern test (Raven's progressive matrices) and has

[8] Ganzeboom and Treiman (1996) harmonized both ISEI and EGP measures by averaging the ISEI scores over 10 EGP categories (see table 1 in their study).

the advantage that is the most culture-free instrument for measuring 'fluid intelligence' (Weiß 2006). As with GPA, this variable was also z-standardized within countries.

Following Rudolphi (2011), I included measures of idealistic and realistic aspirations. These variables were measured at wave 1, that is, one year before students got their final grades and one-and-a-half year before they made their educational transitions. These two variables were recoded into an ordinal scale with three categories: don't know/no answer, upper secondary education or below, and university education.

Both cognitive ability and aspirations are assumed to be affected by the family background. And, thus, they are endogenous to the ISEI variable and other pre-exposure variables such as demographics and family background. Moreover, aspirations are also assumed to be independent of the cognitive ability (or not directly affected by them). This assumption relies on the fact that students are not aware about their results in the cognitive ability test. If they were allowed to observe their results this could change their aspirations, making these endogenous to cognitive ability. However, in the appendix section 160, I relaxed this assumption and allowed cognitive ability to affect aspirations. Even when a direct effects of cognitive ability on aspirations is included, results remain the same.

In addition to these key variables, I included several pre-exposure control variables. First, the proportion of immigrants within schools, i.e., school stratum, (survey characteristics). Second, individual demographic characteristics measured by four variables: gender; ethnicity, which is operationalized as natives/white, mixed and migrants; year and month of birth. And third, further family background indicators: parental education which distinguishes between primary education or less, lower secondary, upper secondary and university (dominance criterion[9] and information primarily taken from parents); and an 'intact family' indicator, coded

[9] The dominance criterion consists of taking the higher of the parents' educational qualifications.

as 1 if children are living with a lone parent or in a stepfamily and 0 if both biological parents are present in the household.

Identification of performance and choice effects

Figure 2.1 depicts a series of causal diagrams (also known as Directed Acyclic Graphs, DAGs) that describe past approaches in performance and choice research (Panel a and b) and the one that I am implementing in this study (Panel c). In particular, we can observe in Panel a that in the initial model it was naively assumed that the unobserved factors (U), which produce differences in performance effects, are fully independent from family background (X) and these only affect choices (Y) through the academic performance (M), i.e., U→M→Y. Erikson et al. (2005) was the first study to recognize that a potential source of endogeneity was an anticipatory decisions mechanism (AD). However, none of the subsequent contributions were able to tackle this issue since anticipatory decisions are by definition post-exposure confounders (Panel b: X→AD→M→Y; X→ AD →Y) and controlling or not for them will incur bias. Also, past models often overlooked the endogeneity from potential open backdoors path (for example in Panel c: M←C→Y; X←C→Y). That is, closing all back-doors paths (C) from the exposure variable is the first key step to assure that ignorability assumption holds (Morgan et al. 2013). However, in mediation analysis several complications arise since we need to condition on a mediator variable which is usually endogenous to other factors besides the main exposure variable. In addition to ignorability of the exposure, we need to impose sequential ignorability of the mediator assumption (Imai et al. 2011). If both assumptions are violated, we might obtain estimates that are biased by confounding (e.g., X←C→Y) and colliders (e.g., C→M←A), making it impossible to draw valid inferences regarding the relative importance of performance and choice effects.

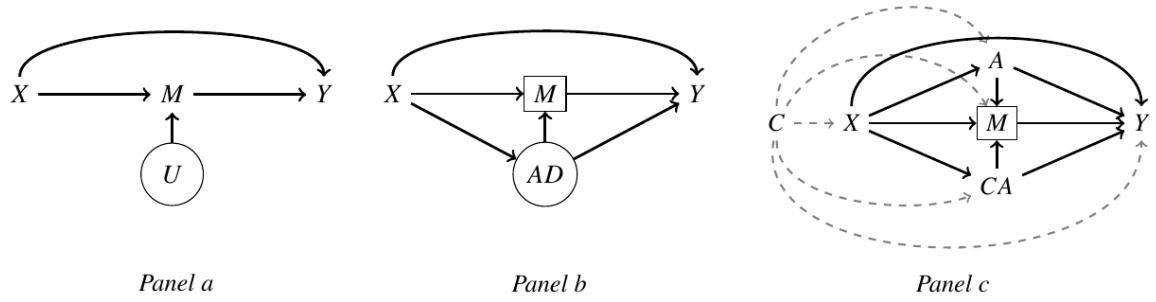


Figure 2.1: Standard and more advances approaches for studying IEO
Note: X = Parental ISEI; M = Academic Performance; Y = Educational transition; U = unobserved component; AD = Anticipatory Decisions; C = Pre-exposures control variables; A = Aspirations; CA = Cognitive ability. Circles represent unobserved variables while squares represent the adjustment on the main mediator.

Given these limitations in the previous models, I propose the model of Panel C which includes two important omitted variables (A and CA) and aims at closing all the backdoors paths (C) by including several pre-treatment variables: school stratum of immigrants' composition, parental education, intact family indicator, gender, ethnicity, month and year of birth. Identification of indirect and direct effects always remains a problem of missing data (Holland 1986) since we are not able to observe the specific variable effects and outcomes for all the students. If we take the example of Erikson et al. (2005) decomposition approach, we can see that their aim is to estimate the transition probabilities (or log odds) if each student would have had experienced four different potential states: i) low-ISEI, low performance; ii) high-ISEI, low performance; iii) low-ISEI, high performance; and iv) high-ISEI, high performance. Since it is only possible to observe one state for each student, they proceed predicting what would be their outcomes in the case these students had belonged to a different socioeconomic group or had the academic performance of this other group. Given that this a pure counterfactual exercise, these states or paths can be also translated into potential outcome notation. Defining $X = x$ as the exposure variable, where $x \in \{0,1\}$, and $M(x)$ the potential mediator state, and $Y(x)$ the potential transition given the exposures and the mediator, the

relationship between these variables can be represented as $Y(x) = Y(x, M(x))$, where the potential outcome is a function of the exposure and mediator variables.

That is, low-ISEI, low performance as observed in the model is equal to Y(0,M(0)); High-ISEI, low performance as predicted by the model is Y(1,M(0)); High-ISEI, high performance, Y(1,M(1)); and d) Low-ISEI, high performance, (Y(0,M(1)). These Potential Outcomes provides the quantities to calculate Total Causal Effect (TCE), Natural Direct Effect (NDE), and Natural Indirect Effect (NIE). Thus, the main difference between decomposition methods is in the estimation part and not in the causal model. Using this notation, it can be shown how the decomposition holds independently of the method adopted. More precisely, we can identify the relative importance of performance and choice effects by the following quantities.

The total causal effect is:

$$TCE = E\{Y(1)\} - E\{Y(0)\} \qquad [1]$$

Natural direct and indirect effects:

$$NDE = E\{Y(1, M(0))\} - E\{Y(0, M(0))\}$$
$$NIE = E\{Y(1, M(1))\} - E\{Y(1, M(0))\} \qquad [2]$$

And, as in Erikson et al. (2005), by changing the reference group we also obtain the alternative decomposition:

$$NDE = E\{Y(1, M(1))\} - E\{Y(0, M(1))\} \qquad [3]$$
$$NIE = E\{Y(1, M(0))\} - E\{Y(0, M(0))\}$$

Given that [1] is the sum of [2] or [3], the relative importance of performance and choice effects can be obtained via the proportion mediated (PM) and unmediated (PUM):

$$TCE = NDE + NIE \qquad [4]$$

$$PM = NIE/(NIE + NDE)$$

$$PUM = 1 - PM$$

However, expressions in [1-4] only identify direct and indirect effects when there is no post-exposure confounding (assuming also conditional ignorability of the exposure and sequential ignorability of the mediator). If this is not the case, we will need to proceed in a slightly different way by defining mediators and outcomes variables as functions of the post-exposure variable as well (Daniels et al. 2011; Shpitser and Tchetgen Tchetgen, 2016; Huber, 2019). Using the DAG of Panel b in Figure 2.1:

$$M(x) = M(x, AD(x)) \; and \; Y(x, M(x)) = Y(x, M(x, AD(x)), AD(x) \qquad [5]$$

Therefore, extending the decomposition in [2], direct and indirect effects are now:

$$\begin{aligned} NDE &= E\{Y(1, M\big(0, AD(0)\big), AD(1))\} - E\{Y(0, M(0, AD(0)), AD(0))\} \\ NIE &= E\{Y(1, M\big(1, AD(1)\big), AD(1))\} - E\{Y(1, M(0, AD(0)), AD(0))\} \end{aligned} \qquad [6]$$

While in case of the NDE, this represents all the effects of X on Y not channelled through M (AD included), NIE represents all the other effects from X to M including the routes through AD. These formulas actually permit a path-specific decomposition by turning on-off the paths of interest (turned off path in x, that is keeping the variables of interest fixed at its potential value for $X = x$). For example, in more general terms, if we look at paths not operating through AD:

$$NIE_M = E\{Y(x, M(1, AD(x)), AD(x))\} - E\{\, Y(x, M(0, AD(x)), AD(x))\} \qquad [7]$$

Or the joint indirect effect going through AD and M:

$$NIE_{M,AD} = E\{Y(1, M(x, AD(x)), AD(x))\} - E\{\, Y(x, M(0, AD(0)), AD(0))\} \qquad [8]$$

Or the direct effect excluding both AD and M variables:

$$NDE_{M,AD} = E(Y(1, M(x, AD(x)), AD(x)) - Y(0, M(x, AD(x)), AD(x)) \quad [9]$$

Estimation

Following past research in performance and choice effects decomposition, I employed first the Erikson et al. (2005) decomposition method which was extended by Buis (2010). This method, however, presents several limitations as discussed by Karlson and Holm (2011) and Breen, Karlson, and Holm (2013). In particular Karlson and Holm, showed that Buis method is distribution sensitive. If mediator variables are skewed this method is likely to provide biased estimates of performance and choice effects. This bias also varies depending on the amount of confounding and the distribution of the outcome. Larger confounding and more skewed outcomes will also increase the bias. Furthermore, this approach can be affected by scaling issues as well. Estimating different logit models and comparing them for constructing counterfactual quantities can be problematic because effects are identified up to the scale. For example, the estimates of a full logit model (including the mediator) and a reduced model (only with main exposure variable) would simply differ because they have different residual standard deviations (Mood 2010; Breen, Karlson, and Holm 2013; 2018). If this is the case, scaling issues would attenuate the estimate of the key exposure variable with the introduction of the mediator or other control variables independently of confounding issues or whether these variables are correlated between them. As a consequence, mediated effects might appear larger than their true size.

Therefore, I used another mediation method developed in the causal inference literature that is not affected by the issues discussed above. The Imai, Keele, and Yamanoto (2010, IKY hereafter) method is particularly useful in cases of nonlinear outcomes and, differently from Buis, it decomposes the estimates of interest using probabilities, facilitating the interpretation

of results. This mediation technique in fact produced very similar results to methods developed by Karlson, Holm, and Breen (2012), KHB, and Valeri and VanderWeele (2013, V&VW). I preferred Imai, Keele, and Yamanoto method over KHB and Valeri and VanderWeele because the former method is accompanied by a package that allows to run sensitivity analysis (Hicks and Tingley 2011). As main results, I present the estimates of the Imai, Keele, and Yamanoto method while, for the sake of comparability, estimates from Buis method are reported in the Appendix (Section A.2.2, Table A.2.2).

One common problem all these methods share is that, at the moment, they cannot handle situations in which there are post-treatment confounders (as depicted in Panel b and c of Figure 2.1). A solution to this problem is the G-computation procedure which is based on a structural model with several reduced form equations (Daniels et al. 2011). Assuming that the model is identifiable upon the structure of Figure 2.1, Panel c, a generalized structural model can be specified with the variables of interest expressed as function of its ancestors:

$$f(Y|X, M, CA, A, \varepsilon_y); f(M|X, CA, A, \varepsilon_m);\ f(CA|X, \varepsilon_{ca}); f(A|X, \varepsilon_a)$$

G-computation formula for mediation employs regression models for each of the (conditional) expectations/probabilities in the identifying equations, estimates their parameters, and then plugs these estimates into the sums/integrals of the equations above to recover the true estimates. When the procedure become too cumbersome to be evaluated analytically, the integration is approximated through Monte Carlo simulation (Daniels et al. 2011). More precisely, this approach can be summarised in three steps. First, it obtains the appropriate distribution of each variable and models the mediator as function of the exposure and covariates, the post-treatment confounder as function of the exposure and covariates, and the outcome as function of the exposure, mediators and covariates. Second, it creates a series of copies of the original sample and simulate covariates that are marginally independent between

them. Then, it simulates the exposure, which is also independent of the covariates, and each mediator, post-exposure confounder as well as the outcome as function of their ancestors using the parameters obtained in the first step. Finally, it regresses each different outcome on the exposure to obtain the point estimates of each marginal effect using the pooled sample obtained from all the copies of the data and calculates bootstrapped standard errors. These new estimates account for the potential bias induced by the post-treatment confounder and should approximate the true effects of interest.

In this case, the regression model for the outcome is a logit; the ones for the academic performance and cognitive ability are linear (OLS); and the ones for the aspirations are ordinal logits. It is important to bear in mind that the estimation rely on the causal structure assumed by the researcher (order of the included in the model variables) and the possibility of including all the relevant variables available in the data. That is, where in the structure the variables enter in the model is important for identification. Different causal diagrams or structures will produce different estimates. Moreover, the model is line with Van de Werfhorst and Hofstede 2007 who shows that cultural capital is mostly correlated with performance and loss aversion with school ambitions since cognitive ability enters in the mediated path of the performance effects and the aspirations goes into a separate path that further decompose the choice effects into a direct effect (which could be interpreted as evidence of loss aversion) and the effects of aspirations allowing for anticipatory decisions (see panel c of Figure 2.1 and the appendix section 2.7).

In addition, two robustness checks were conducted. First, I run a series of Linear Probability Models (LPM) with school fixed effects to grasp whether the omission of school factors could produce bias in the total and direct effects of the key variables. Second, I employed a series of sensitivity analysis to test whether the assumption regarding no mediator-outcome and exposure-outcome confounders are plausible (Imai et al. 2011).

Finally, it is also important to note again that inference from all the models employed in this study remains on the basis of fairly strong assumptions regarding confounders and colliders. If there are further unobserved or omitted exposure-outcome, exposure-mediator and mediator-outcome confounders, the estimates will likely be affected by a combination between confounding and collider bias (Breen 2018). Part of these assumption have been tested through sensitivity analysis and additional regression models. In particular, I suspected that omitted school level variables could affect both performance and choices. However, the checks show that results are robust to these potential mediator-outcome confounders and thus these should not be the main source of concern.[10]

3. Results

Distribution of cognitive ability and aspirations

Figure 2.2 displays the density of GPA and CA distributions by parental ISEI in England and Sweden. In both countries we observe that the curves of GPA and CA for high-ISEI group are more skewed towards the right side of the x-axis than in the other groups. Also, there are no significant differences in the distribution of the ability variables between the two countries. In particular, the CA for the most advantaged group has a higher density and less dispersion in the tails. In other words, this figure suggests that GPA and CA are clearly affected by the family background. The higher the advantage in terms of parental ISEI, the higher the probability of exhibiting better school performance and higher cognitive ability. The correlation between cognitive ability and GPA is 0.51 in England and 0.43 (GPA from survey) and 0.47 (GPA from register data) in Sweden. These correlations are pretty much in line to what have been already reported in the literature (Roth et al. 2015).

[10] In the appendix section 2.6, I added an additional robustness check based on Propensity Score Matching and Mantel and Haenszel bounds estimates for Total Effects which also shows that estimates are robust to exposure-outcome confounder.

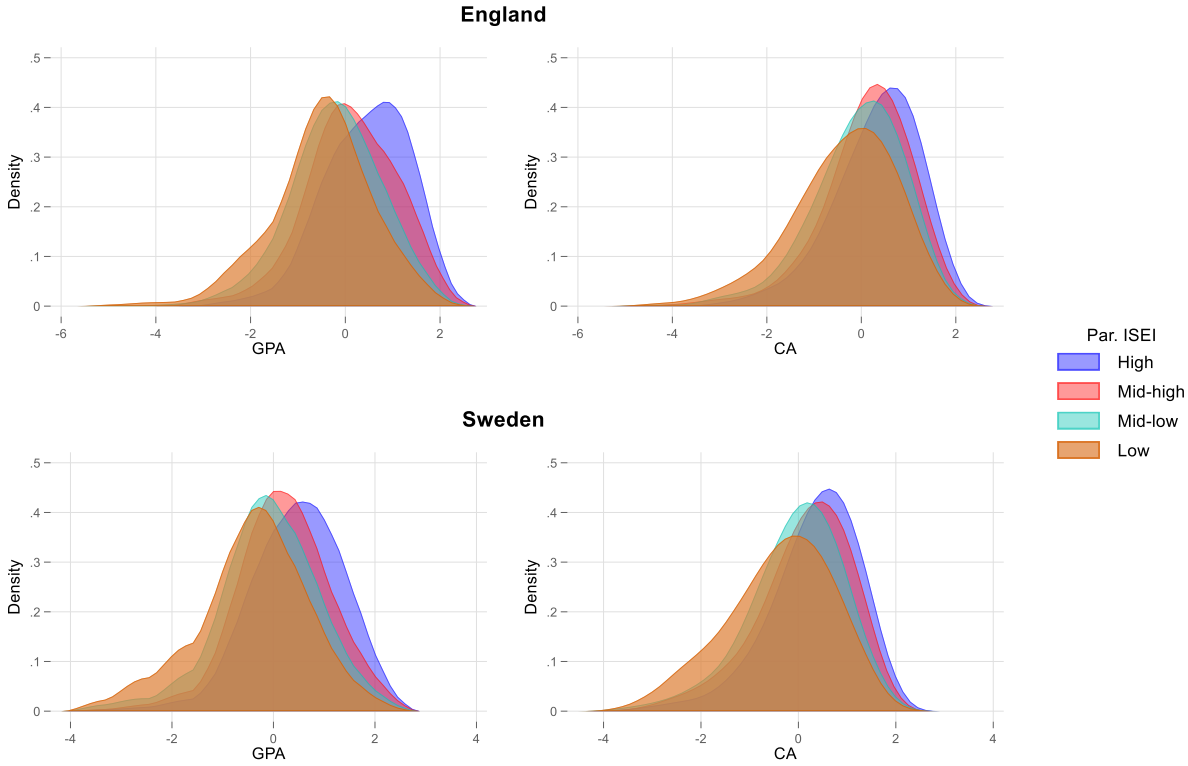


Figure 2.2: Distribution of students' GPA and Cognitive Ability by Parental ISEI in England and Sweden

Not surprisingly, we can also observe a socioeconomic gap in both aspiration measures (Figure 2.3). Students who grew up in high-ISEI families are more likely to report that they would like to achieve a university degree than students in the other groups. The raw gap is substantially large if we compare high-ISEI vs low-ISEI groups (i.e., England = 0.30 and Sweden = 0.20 percentage points for idealistic aspirations and 0.35 and 0.27 percentage points for realistic aspirations, respectively). Further, we can also note that while there is a very similar pattern in idealistic aspirations between the two countries, this is not the case for realistic aspirations. That is, students in Sweden have much higher realistic aspirations than their counterparts in England across all the ISEI groups.

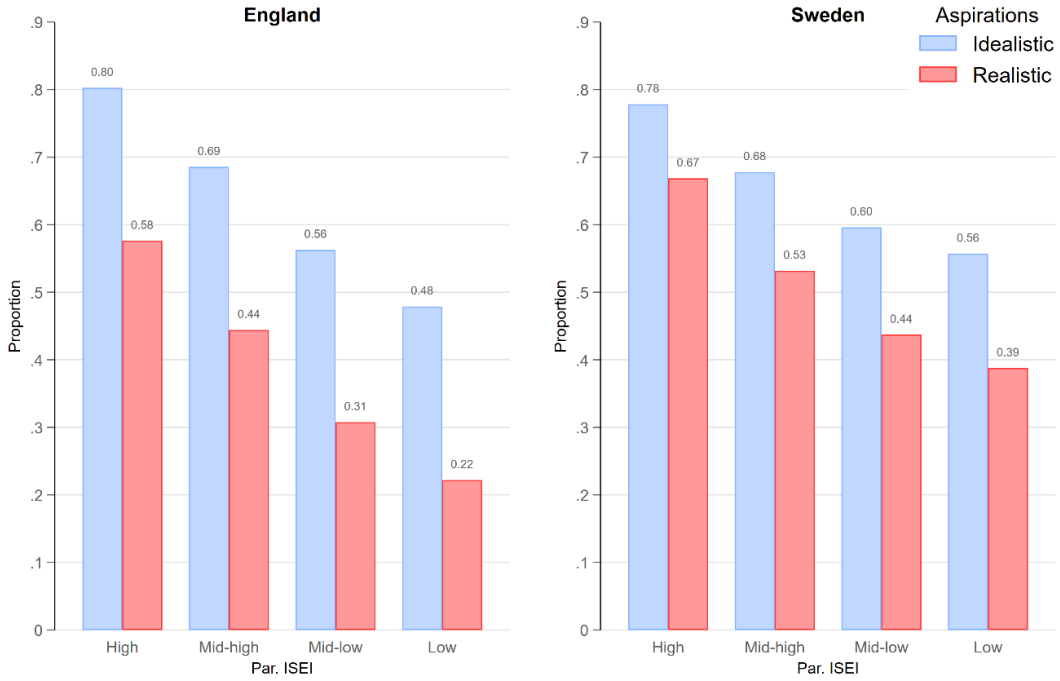


Figure 2.3: Proportion of students with university aspirations by Parental ISEI

Overall Figures 2.2 and 2.3 indicate that students' ability and aspiration measures are socially stratified. Using them merely as controls could be problematic since they are likely to be halfway in the path between ISEI and academic performance and educational choices. Independently of the origin, students with high levels of cognitive ability and aspirations should perform well at school. If this not case, anticipatory decisions might be influencing their performance and choices. These two variables are, thus, key for identifying potential anticipatory decisions and reducing their effects.

Decomposition of performance and choice effects

Table 2.1 reports the coefficients from the performance and choice effects decomposition, Total Causal Effect (TCE), Natural Indirect Effect (NIE), and Proportion Mediated (PM). For each country, I included the estimates of the IKY method with three different model specifications: (1) a baseline model with only the three key variables of interest (same specification as Erikson et al. (2005) and Jackson et al. (2007); (2) a model that also accounts

for the survey characteristics; (3) a full model that includes all the pre-exposure controls (e.g., survey characteristics, individual demographics, and further family factors). For each model, I reported the total effect, the indirect effect and the proportion mediated (i.e., the relative importance of performance effects) together with standard errors and bootstrapped confidence intervals at 95% level of confidence. Further, similarly to Jackson et al. (2007) and Erikson and Rudolphi (2010), I also repeated these decomposition exercises replacing the GPA with the cognitive ability measure. Results from Buis decomposition can be found in the Appendix, section A.2.2. These models are still based on the DAGs of Panel a and b of Figure 2.1 and, thus, they do not account for post-treatment confounder from anticipatory decisions. Table 2.2 instead reports the results of the G-estimation following the DAG discussed in Panel c of Figure 2.1. That is, the full model taking into account also the potential endogeneity from the post-exposure confounders: cognitive ability and aspirations. I reported the contrast between High-ISEI vs Low-ISEI. Typically, in social stratification research the main focus is based on the differences between the most and less advantaged groups.

The upper panel of the Table 2.1 shows the results with GPA as main mediator. Focusing on the total effects, we can see that both countries have very similar levels of IEO (TCE cells). Between specification (2) and (3), the IEO varies around -0.30/-0.22 in England and -0.39/-0.36 in Sweden. In other words, this means that the socioeconomic gap in upper secondary education transition is still substantively large in both countries. However, if we compare these results with those reported in the past, there has been also a reduction of IEO across cohorts. For example, Jackson (2013) reported that the unconditional gap between upper and working classes was approximatively -0.42 and -0.36 for individuals born in 1986 and 1990. In the case of Sweden, the unconditional gap reported by Rudolphi (2013) between high ISEI and low-ISEI families was -0.52 for 1983 cohort; -0.41 for 1989 cohort and -0.39 for 1990 cohort (see Figure A.2.1 in the Appendix). Therefore, IEO continue to decline across cohorts.

In terms of performance effects, we observe that once we include several sociodemographic controls, the NIE is reduced in both countries. While in England we observe an indirect effect of -0.21 for the specification 2 and -0.14 for specification 3, in Sweden the reduction in the indirect effects goes from -0.27 in specification 2 to -0.24 to specification 3. In both countries we found that the relative importance of performance effects (PM) is larger than choice effects. That is, of the total IEO, academic performance explains between 70 (specification 2) and 62 (specification 3) percent in England and 68 (specification 2) and 67 (specification 3) percent in Sweden.

We now move into the results using cognitive ability following the strategy of Jackson et al. (2007) who substituted GPA for cognitive ability. In this case, the picture regarding performance and choice effects completely change if we use the cognitive ability as main mediator. Although we still observe that the inclusion of sociodemographic controls reduces the size of the TCE and NIE, now we observe that performance effects are substantially smaller than those obtained when GPA is used as main mediator. The indirect effects are -0.038 in England and -0.069 in Sweden (for both specification 3). This also corresponds to smaller relative performance effects compared to choice effects (17 percent in England and 16 percent in Sweden). In addition, if we compare the Imai, Keele and Yamanoto with Buis decomposition method, we can observe that the latter method overstates the magnitude of relative importance of performance and choice effects, independently of whether the GPA or the cognitive ability are used as main mediator variables (see Appendix, Table A.2.2). This suggest that past results of the relative importance of performance and choice effects based on the Erikson et al./Buis approach should be interpreted with caution.

Turning back to the main results, we can conclude that the comparison between GPA and cognitive ability suggests that there is a strong process of selection into the academic performance, and this is probably related to anticipatory decisions. None of the methods

previously discussed can properly tackle this issue and, therefore, their estimates are likely to be biased. In fact, the G-estimation formula (Table 2.2) shows lower estimates of the total and indirect effects than the previous method. More precisely, TCE is -0.12 for England and -0.14 for Sweden, still denoting similar levels of IEO between them. Interestingly, the G-formula provides smaller total effects than the previous method, although confidence intervals overlap with the point estimates of the Table 2.1 for England. This is because this method makes exposure and control groups much more similar in terms of observable characteristics by integrating across the distributions (Daniels et al 2011). Moreover, compared to Imai, Keele and Yamanoto (2010) method, once we account for the selection into the mediator, we find lower performance effects in both countries, namely,-0.07 in England and -0.09 in Sweden (i.e., by symmetry larger choice effects). Both countries are also similar when we look at the relative importance of performance and choice effects. In England the relative importance of performance effects is 0.61 (choice effects = 0.39) while in Sweden is 0.65 (choice effects = 0.35). In sum, IKY provides larger performance effects for both countries when we employ GPA as main mediator than when we use cognitive ability, suggesting that anticipatory decision could be an issue in the quantification of performance and choice effects. The G-formula allows to include cognitive ability and aspirations as potential endogenous mediators potentially reducing the effect of anticipatory decisions. Once we account for this issue, performance effects are smaller than those found with IKY method. However, it's important to note that obtained estimates in causal mediation analysis depends on the assumed causal structure (i.e., the order of the variables) and the inclusion of all the relevant factors among the observable variables available. From this point of view, the results obtained from these two models, IKY and G-formula, can be seen as lower and upper bound, respectively, depending on the causal model assumed, how much we unpack the mediated paths, and the assumptions regarding anticipatory decisions.

able 2.1: Imai, Keele, and Yamanoto (IKY) decomposition results

GPA as mediator	England			Sweden		
	TCE	**NIE**	**PM**	**TCE**	**NIE**	**PM**
(1)	-0.310	-0.227	0.73	-0.365	-0.282	0.77
	(0.037)	(0.019)	(0.06)	(0.049)	(0.014)	(0.06)
	[-0.371;-0.251]	[-0.264;0.191]	[0.51;0.75]	[-0.461;-0.27]	[-0.309;-0.255]	[0.65;0.89]
(2)	-0.301	-0.21	0.70	-0.389	-0.266	0.68
	(0.031)	(0.018)	(0.06)	(0.023)	(0.032)	(0.05)
	[-0.368; -0.251]	[-0.247;-0.174]	[0.58;0.85]	[-0.434;-0.344]	[-0.329;-0.203]	[0.58;0.78]
(3)	-0.224	-0.144	0.64	-0.359	-0.240	0.67
	(0.075)	(0.048)	(0.073)	(0.062)	(0.035)	(0.07)
	[-0.277;-0.159]	[-0.161;-0.087]	[0.48;0.88]	[-0.480;-0.237]	[-0.309;-0.171]	[0.53;0.81]
CA as mediator	**England**			**Sweden**		
	TCE	**NIE**	**PM**	**TCE**	**NIE**	**PM**
(1)	-0.306	-0.056	0.18	-0.360	-0.096	0.22
	(0.031)	(0.009)	(0.02)	(0.023)	(0.031)	(0.04)
	[-0.365;-0.247]	[-0.074;-0.038]	[0.14;0.22]	[-0.405;-0.315]	[-0.157;-0.035]	[0.14;0.30]
(2)	-0.316	-0.056	0.18	-0.387	-0.082	0.17
	(0.029)	(0.011)	(0.01)	(0.024)	(0.008)	(0.02)
	[-0.373;-0.259]	[0.078;-0.034]	[0.16;0.20]	[-0.434;-0.340]	[-0.098;-0.066]	[0.13;0.21]
(3)	-0.220	-0.038	0.17	-0.360	-0.069	0.16
	(0.035)	(0.010)	(0.02)	(0.031)	(0.010)	(0.02)
	[-0.289;-0.151]	[-0.058;-0.018]	[0.13;0.21]	[-0.421;-0.299]	[-0.089;-0.049]	[0.12;0.20]

Note: Bootstrapped standard errors in parenthesis; 1000 replications. 95% confidence intervals. High-ISEI vs Low-ISEI, England N = 1042; Sweden N = 2514 (Y and M from register data). Specification (1) = No additional controls; Specification (2) = Survey characteristics; Specification (3) = Survey characteristics; Individual demographics and family background. Total Causal Effect (TCE), Natural Indirect Effect (NIE) and Proportion Mediated (PM).

Table 2.2: G-estimation results

England			Sweden		
TCE	**NIE**	**PM**	**TCE**	**NIE**	**PM**
-0.118	-0.072	0.61	-0.141	-0.091	0.65
(0.022)	(0.015)	(0.08)	(0.012)	(0.010)	(0.027)
[-0.162, -0.075]	[-0.101, -0.043]	[0.45, 0.76]	[-0.164, -0.118]	[-0.111, -0.071]	[0.60, 0.70]

Note: Bootstrapped standard errors in parenthesis; 1000 replications. High-ISEI vs Low-ISEI,
England N = 1042; Sweden N = 2514 (Y and M from register data).
Total Causal Effect (TCE), Natural Indirect Effect (NIE) and Proportion Mediated (PM).

Sensitivity analysis

Two sources of bias are of particular concern in this study. The first one is the possibility of mediator-outcome confounders mainly related to omitted school factors. If variables such as school resources or class climate affect pupils' academic performance and later educational choices, the sequential ignorability assumption would be violated. The second one is the possibility that there are other exposure-outcome confounders not accounted in the model. Unobserved or omitted pre-exposure variables will also violate the standard ignorability assumption. As an initial robustness check for potential school mediator-outcome confounders, I employed linear probability models (LPMs) with school fixed effects to grasp whether there are still significant effects of the key variables on the educational transition and academic performance once time-constant unobserved factors at the school level are removed. Results shows that within schools, the effects of key variable remain quite stable for both the transition and GPA outcomes. This is also in line with recent evidence from Sweden (Jonsson and Treuter 2021) where it was shown that school characteristics do not matter much in explaining educational inequality. More precisely, the authors used a restricted access CILS4EU dataset (linked to register data and included also teacher's variables not available in the public files) and found that school level variables have null or little effect on pupils' outcomes.

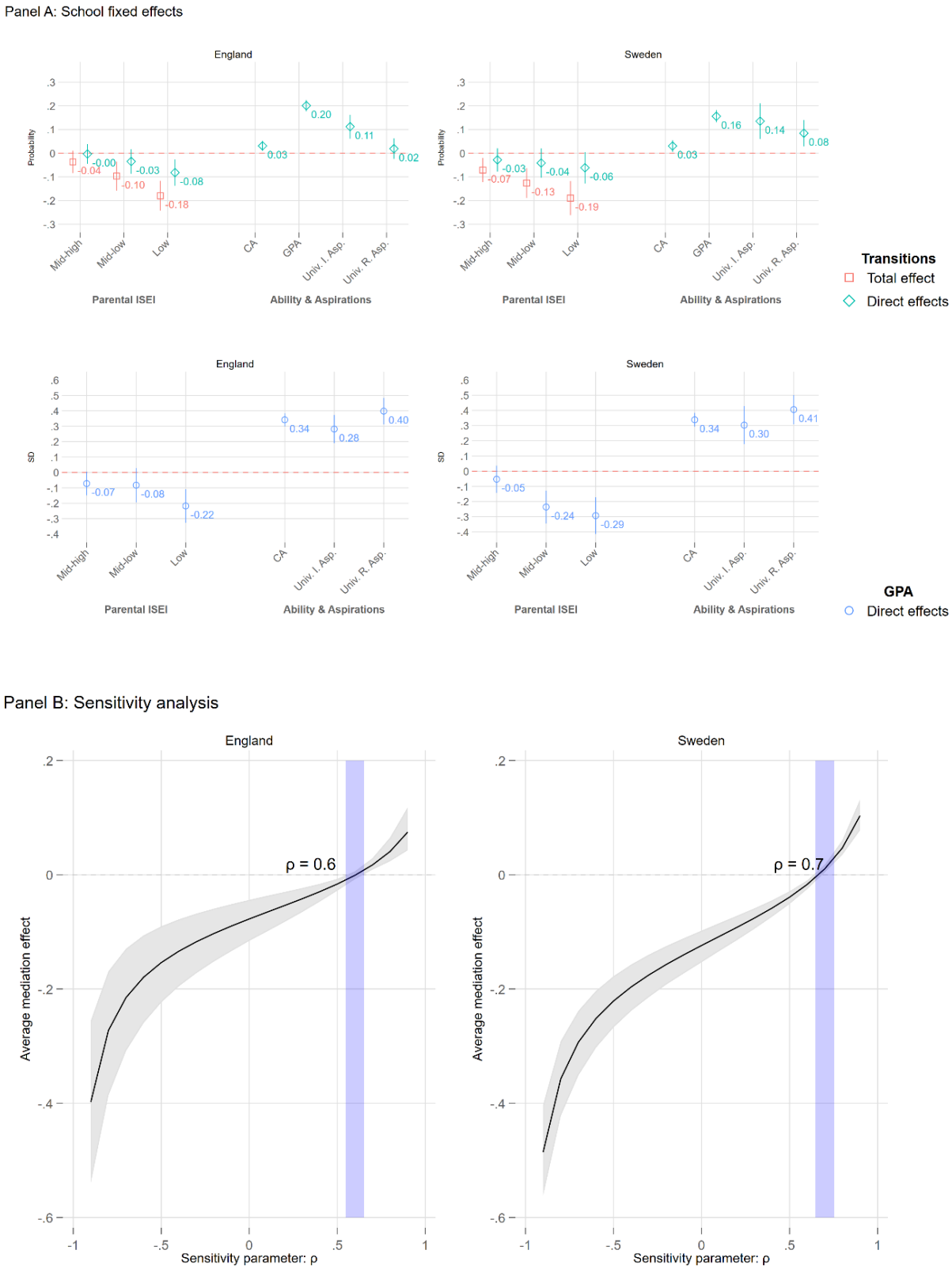


Figure 2.4: Robustness checks. School fixed effects (Panel A) and Sensitivity analysis (Panel B)

I further explored potential mediator-outcome confounders through sensitivity analysis (Imai, Keele, and Yamamoto 2010, Hicks and Tingley 2011). This technique seeks to assess the impact of potential omitted variables on the causal parameter of interest. In general, its implementation consists in defining a sensitive parameter, ρ, that represents the degree of violation of the exogeneity assumptions. Then, this parameter is set to different values in order to observe at what point the causal effect would disappear completely or become statistically insignificant. Results from sensitivity analysis suggest that omitted mediator-outcome confounders should be fairly strongly correlated to wash away the results presented in this study. In the case of England the correlation should be 0.6 while in Sweden 0.7 (see Figure 2.4).

Comparing past and current estimates of the relative importance of choice effects

Moreover, in order to contextualize the results presented in Tables 2.1 and 2.2, I collected all the estimates of the relative importance of choice effects in transition to upper secondary education reported in past studies for both England and Sweden (only those that were numerically available in the papers), displayed in Figure 2.5. These estimates are all based on the same scale (i.e., a percentage) and almost all of them were obtained using Buis method (the only exception is Karlson 2013). For these reasons, as a benchmark, I reported first the results of the Buis decomposition (results available in the Appendix, Table A.2.2), with the standard specification (2), and then those obtained via the G-estimation for the 1995/6 cohorts. This result comparison across cohorts, however, should still be interpreted with caution since there are some slight differences in the operationalisation of variables.

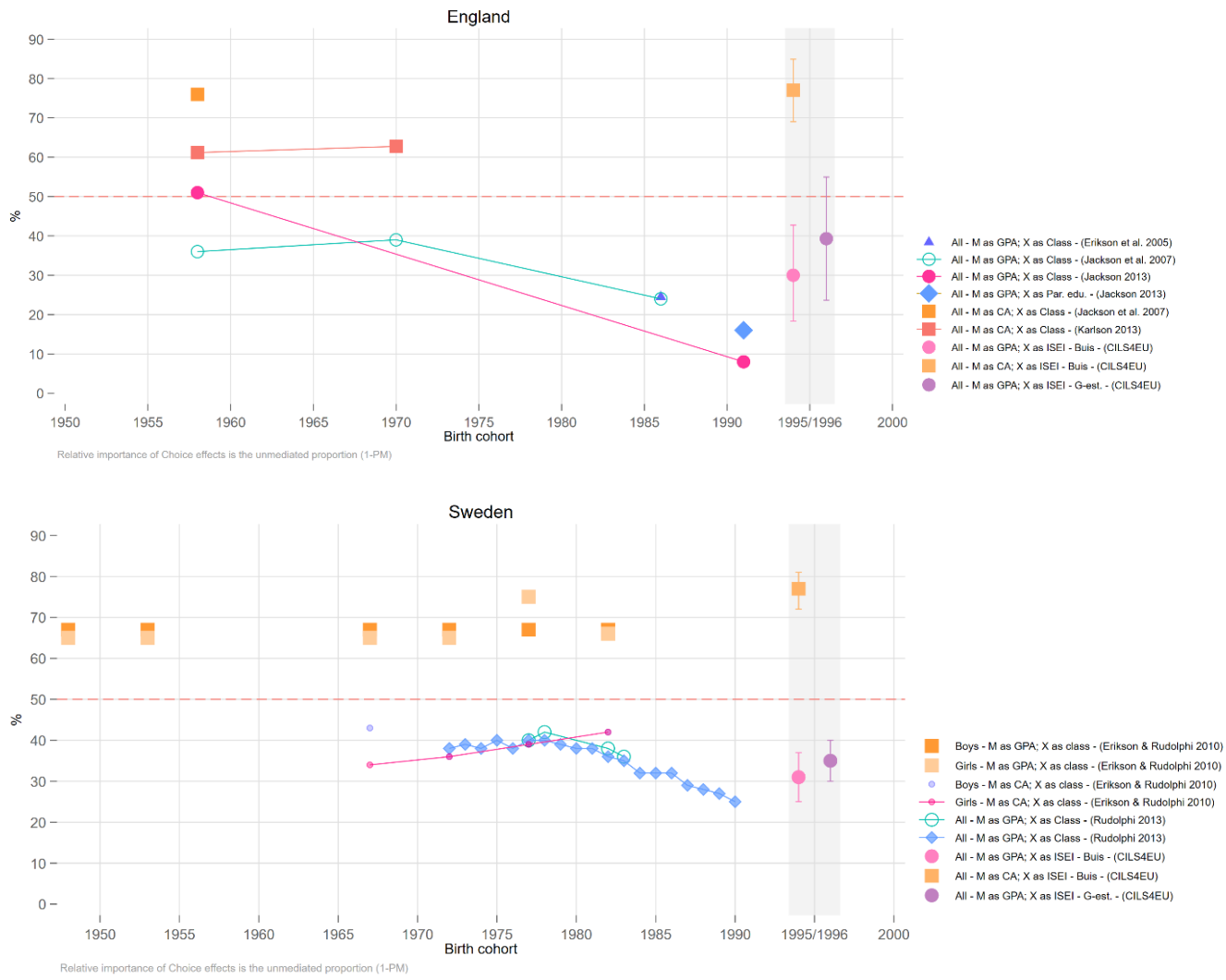


Figure 2.5: Over time comparison in the relative importance of choice effects
Note: Estimates of the relative importance of SE in Karlson 2013 are based on the probability of achieving an upper secondary degree and used an early version of the KHB method. Jackson's (2013) reported estimates of SE for several cohorts but not all of them were numerically available in the text. I excluded the estimates reported by Rudolphi (2011) because they were mostly overlapping with Erikson and Rudolphi (2010). I also excluded Rudolphi and Erikson (2016) because they used parental education as main exposure variable and a different methodology for the decomposition.

What are the main differences between past and new results? The estimates of choice effects obtained using cognitive ability as mediator are much higher than those obtained using GPA in both the current study and the previous ones. The pattern over time is also very stable for the SE

obtained via CA. The difference in estimates using measures of potential ability rather than academic performance indicates that there is a strong process of selection in terms of performance during the scholastic career. Furthermore, if we look at the estimates obtained via GPA as main mediator, we observe a declining pattern in choice effects from the early 70s until 1990/1. The estimates of SE, which I obtained using the same model and very similar specification as previous studies, suggest that there has been a reverse in the declining pattern and now choice effects have become relatively more important than they were in the past. This is also confirmed by the estimates obtained via the G-estimation which reduces the endogeneity induced by post-treatment confounders (i.e., anticipatory decisions). The standard decomposition method employed in performance and choice research produces substantially smaller estimates of SE. This is mainly because the omitted path from the social background to CA and Aspiration and then GPA are likely to inflate the indirect effects. Clearly, the anticipatory decision mechanism is an important component in the educational stratification process. Not taking this into account can distort the conclusions. For example, in the case of England by just looking at the Buis estimates we would conclude that performance effects remain relatively larger (and significantly different from 50%) while estimates from the G-computation formula show no difference between performance and choice effects (i.e., confidence intervals overlap with the 50% threshold).

In terms of cross-country comparison, by looking at past results, we see that both countries followed a similar stable trend between the 60s and early 70s and then a declining pattern took place in choice effects until 1990/1. However, for those born in late 80s and early 90s, choice effects were relatively larger in Sweden than in England. This is not observed for the latest birth cohort, 1995/6 (see pink circles for Buis = 33 percent in England vs 31 percent in Sweden; and

purple circles for G-estimation = 39 percent vs 35 percent, respectively) where Sweden displays slightly lower choice effects, although confidence intervals overlap between the two countries.

4. Conclusion and Discussion

Previous research in educational inequality has indicated that IEO has declined in birth cohorts born around 1970-90s for several countries, and this decline was mainly driven by a reduction of choice effects (Jackson 2013). However, past studies used decomposition methods that were not able to tackle the endogeneity from anticipatory decisions and the majority of them also disregarded the potential confounding bias from pre-exposure variables. Furthermore, none of these studies allowed to evaluate the uncertainty around their estimates of the relative importance of performance and choice effects since they did not report confidence intervals or standard errors, nor conducted sensitivity analysis to test the plausibility of sequential ignorability assumptions in their models.

In this study, I used rich longitudinal data from CILS4EU and more rigorous decomposition methods to assess what is the relative importance of performance and choice effect in transitions to upper secondary education in a cohort of students born around the years 1995/6 in England and Sweden. My results suggest that IEO has continued to decline among younger cohorts of students. However, compared to previous studies conducted on older birth cohorts, I found that there have been some changes in the relative importance of performance and choice effects. Choice effects seem to be equally important as performance effects in these younger cohorts for England (with a lower bound of 0.34 and upper one of 0.55) and remain smaller than performance effects in Sweden (lower bound 0.30 and upper bound 0.40), although the choice effects are still substantial in magnitude and seem to have increased compared to older cohorts in this country. Part of the discrepancy with past results, and the common thinking that choice effects decline across younger

cohorts, could be explained by modelling strategies regarding anticipatory decisions. However, this does not seem to be the only explanation since decomposition exercises performed using same strategies and specifications as those of past studies still provided higher choice effects than those previously reported for older cohorts. Following the Breen and Goldthorpe (1997) prediction, one possible explanation is that given the absolute reduction in total inequality in transitions, for the remaining stock of those students who did not choose academic tracks, their decisions are still driven by loss aversion. However, given the presence of anticipatory decisions, this behavioural mechanism could be already present at early stages of the school trajectory rather than emerging at the transition point as usually hypothesized. In fact, some of the few direct test of loss aversion mechanism in education have shown that while cultural capital is associated with educational outcomes, relative risk aversion is mostly associated with school ambition and educational plans already at early ages (Van de Werfhorst and Hofstede 2007).

Taken together, the analyses suggest that in comprehensive educational systems policy interventions should also be targeted at helping students to make more efficient choices, for example by providing further information regarding the benefits of continuing into higher levels of education and clarifying the potential academic pathways that they can take. Perhaps implementing counselling and informative interventions already before the moment students' take their major exams. In this way they could reduce the negative effects of anticipatory decisions on grades and incentivize the transition into higher levels of education of those who have demonstrated effort and academic potential. To be sure, choice effects may also be the product of differences in resources in the family of origin, affecting the relative costs of making the transition to higher education. Policies that keep direct costs for upper secondary and tertiary-level studies

low and that equalise resources between socioeconomic groups therefore have the potential to further reduce IEO via reducing choice effects.

Chapter 3:

Are mixed ability classes bad for school performance and educational choice? Socioeconomic and ethnic inequality in English and Swedish schools

Chapter co-authored with Jan O. Jonsson[11]

Abstract

A longstanding question in the organization of education has been the one about learning in ability-homogenous groups. Previous research has suggested that tracking or ability grouping, aiming at homogeneity, do not improve learning on average, but may lead to increasing inequality. However, lack of suitable data and methodological difficulties have made results inconclusive. We contribute with a comparative study between England (with institutionalized ability grouping) and Sweden (with occasional use). We use CILS4EU data to study the effect of (a) homogeneity in ability in instructional groups on grade point averages (GPA) and academic transition to upper secondary education; (b) the effect of ability grouping within subjects on grades in these subjects. School fixed effects, Inverse of Probability of Treatment Weighting models, and placebo analyses show little evidence of efficiency gain in homogeneous instruction groups or ability grouping, but also little evidence that such grouping has any effects on inequality; nor do we find any significant differences between England and Sweden.

[11] Nuffield College, University of Oxford.

1. Introduction

A longstanding question in the organization of education has been how to organize the allocation of pupils into classrooms on the basis of ability. Academically, both in sociology and education, a core question has been what consequences different types of organizations have. In the 1940s this issue was at the core of both English and Swedish school reform – England went one way by introducing the 11+ whereby high-ability pupils also from modest backgrounds could go to the exclusive grammar school, thus homogenizing the student body on the basis of ability. Sweden went the other by embarking on the road to mixed-ability classes in earnest. Despite these diverging paths, in the 1960s and 1970s, England followed Sweden (and not countries like Germany and the Netherlands) on the path to the comprehensive school, with a strong focus on mixed-ability classes up until age 16. England has kept a tangible but small private sector, some selective grammar schools, and more subject choice, while Sweden took the mixed ability idea further by reducing ability grouping to a bare minimum (although with a development towards more 'non-public' schools recently). Critics of the mixed ability class policy have claimed that this reduces instructional efficiency, and that it risks leaving low-ability students behind when teachers move through the curriculum at a pace that is too fast for them, and that the brighter students may be held back (Dahllöf 1971; cf. Esser and Seuring 2020). At the centre of this political discussion and academic theorizing is the question of teaching and learning in ability-homogenous groups. Although this appears to be an issue that is straightforward to study, there is no conclusive evidence on whether this promotes or impedes student achievement, one reason being that suitable data are more difficult to come by than one may think; another that ability grouping is just one of several organizational features of schools.

In sociology, the issue of the efficiency of homogenous learning environments has been combined, or even over-shadowed, by the concern that a consequence of ability grouping will be increased socioeconomic and ethnic inequality (e.g., Oakes 1985). Both in England and Sweden equality-minded social scientists and politicians have feared a return to an elite school system where 'white' middle class pupils go to different schools than other pupils, a social segregation that they see as unwanted in itself. More importantly perhaps, many have warned that lower efficiency of instruction and learning in 'low-ability classes' will lead to increased inequality. Pupils from advantaged backgrounds, who both have higher achievements and aspirations, will get better opportunities for learning while the others will find themselves in groups with low ambitions (cf. van Houtte 2016), where teachers have low expectations (Andersen 2018), and where the curriculum at least in practice will be downwardly adjusted in a way that creates less opportunities for learning and makes it difficult to proceed to higher education (Sørensen and Hallinan 1977). A related worry is that 'anti-school' sentiments will emerge among working class kids in such negatively selected groups (cf. Willis 1977); and for racial and ethnic minorities, similar fears have been mounting of 'counter-cultures' against majority norms of school achievement as a valuable goal (e.g., Fordham and Ogbu 1986). In sum, many believe that ability grouping will magnify initial differences in ability and lead to greater inequality in grades and hence educational success.

It is less noted that ability grouping may not only affect grades and the concomitant educational aspirations but also have an effect on educational choices, independently of grades. This is a reasonable hypothesis that may stem from three different sources. First, it could be related to the presumed importance of role models and peer effects on aspirations. Mixed-ability, and thus also mixed-origin classes have, it is often claimed, a positive effect on children of disadvantaged

origins via learning from high-ability school-mates; but this should go for aspirations and educational plans as well. Secondly, lowered teacher expectations may lead to lower academic self-esteem, which could also reduce aspirations even for those whose abilities would be sufficient for higher education. Thirdly, even given peer or teacher effects, the sheer discouragement of being sorted into the low achievement group may instill a feeling that education 'is not for me' – and vice versa, those who so to speak get promoted to the higher ability group may feel encouraged and raise their ambitions. However, the peer-effect, the teacher effect, and the disillusion effect could to some extent be counter-balanced by a social comparison mechanism whereby aspirations depend on one's *relative* position in a school class, or perhaps ability group (Davis 1966), which has also received some support in studies on Swedish comprehensive schools (Jonsson and Mood 2008). In this scenario, those who get discouraged by a demotion to the low-ability group may excel in relation to others in that group, while they would have suffered from being among the weakest students if they instead had ended up in the high-ability group.

The issue of ability homogeneity is closely related to the issue of residential segregation on the basis of socioeconomic and ethnic characteristics, also prominent in studies of educational inequality (e.g., Gamoran and An 2016). Such segregation, in Sweden almost perfectly, in England somewhat less so, determines the social, ethnic, and ability-related composition of schools, because comprehensive school intake is mainly governed by which catchment area a student lives in. This introduces a selection mechanisms into the allocation of pupils to school, based, e.g., on economic resources and choice, which may be driven by parental aspirations for their children. Thus, residential segregation indirectly influences school-level homogeneity, making it difficult for analysts to separate population sorting (selection) into schools from causal effects of school

characteristics, such as peer group composition, on individual outcomes (e.g., Sacerdote 2011; Hanushek et al. 2003).

In this paper, we use nationally representative data on comprehensive school students from England and Sweden, born 1995-96, to study the effect of (a) homogeneity in ability in instructional groups, and (b) ability grouping within subjects. As outcomes we use both school performance (grades) and the transition to academic upper secondary school programmes at given grades – what is known as 'primary' (or performance) and 'secondary' (or choice) effects in the educational stratification literature (Boudon 1974; Erikson and Jonsson 1996; Jackson 2013). We are unusually well equipped to throw light on this fundamental question as we have information on cognitive ability at age 14-15, grades at age 16, and later educational attainment for almost 10,000 students in England and Sweden, nested in over 200 schools and over 400 classrooms. Almost uniquely, we also have information on informal ability grouping (also knows as 'setting'; 'streaming'; and sometimes 'tracking', although in this case the term is different from tracking of school curriculum) within the comprehensive systems in both countries. This includes not only whether the school and school class practice ability grouping, but also which group each pupil was in, and thus we can derive the composition also of ability groups.

Because schools were sampled and all pupils in two school classes were interviewed, unlike many other studies, we are able to (a) elicit reliable data on homogeneity and (b) employ school fixed effect models. Thereby, we can identify a model that strongly reduces the risk of selection bias by using the difference in ability homogeneity between school classes within schools. This means that most, arguably nearly all, selection into schools is accounted for, to the extent that the allocation of pupils to school classes are independent of ability (something we also address). In addition to the school fixed effects, we are able to control for observed (and theoretically relevant)

variables at both the school and classroom level, such as the ethnic and socioeconomic composition. While other studies compare schools, or school classes with and without grouping, we are also able to analyse the school performance of those who actually experienced different ability groups and to relate it to others in that ability group and to those in the other group(s), as well as to pupils in school classes that do not use ability grouping.

We estimate models using the ability homogeneity and grouping to predict grades and educational transitions to academic upper secondary school programmes, controlling for own ability and various exogenous variables, such as gender and parents' education and socioeconomic position. This addresses the *efficiency* question of mixed ability classes in two different ways. Do we find any positive effects of homogeneity on achievement? Are there negative effects of being allocated to a lower ability group in certain subjects?

We then go on to study *inequality*. First, we analyze whether the variance in school performance is greater than the variance in ability in schools that use ability tracking, suggesting a widening of individual differences due to ability grouping. Next, we ask whether (a) socioeconomic and ethnic associations with educational outcomes depend on school class ability homogeneity (a compositional effect), and (b) whether such homogeneity effect differs between pupils of different socioeconomic and ethnic origins (a moderation, or interaction effect). Both these models speak directly to the relation of mixed ability classes to group inequality in educational outcomes: Does the degree of ability homogeneity in school classes impinge on socioeconomic and ethnic inequality in education?

Importantly, we expand on much of the previous literature by also asking whether potential effects of homogeneity play out on grades or on educational choices at given levels of grades – a question that relates to the burgeoning empirical literature on the difference between performance

and choice effects in socioeconomic (e.g., Erikson and Rudolphi 2010; Jackson 2013) and ethnic (e.g., Jonsson and Rudolphi 2011; Heath and Brinbaum 2014) educational inequality.

While our analysis of ability homogeneity addresses the underlying mechanism behind effects of ability grouping and mixed ability classes, our second type of analysis concerns actual ability grouping. We describe the ability and family origin selection into these groups, and then use three kinds of comparisons. First, we compare pupils' outcomes in schools that use ability grouping with those that do not, controlling for observed characteristics of schools (e.g., pupils' aggregate cognitive and verbal abilities; ethnic and socioeconomic composition; teacher and pupil engagement; parents' aspirations). For Sweden, we are then able to use multiple imputation to assign pupils to 'shadow ability groups' in school classes that do not use grouping for a more accurate test of the homogeneity hypothesis. Secondly, we compare school classes within schools that use such grouping with those that do not. Third, we compare the outcomes of students in higher-ability groups with those in lower-ability groups, controlling for individual-level observed characteristics, most importantly own cognitive ability and GPA.

We predict that the homogeneity effects would look rather similar in England and Sweden, partly because it is generic, partly because both countries predominantly organize comprehensive schooling using mixed ability classes. However, because Sweden has 'gone comprehensive' more emphatically than England, where selective schooling and some organizational heterogeneity remain, and ability grouping is more systematically used, we expect the influence of ability grouping, independent on the direction, to be stronger in England; and similarly that such influence will increase inequality more there. Furthermore, we anticipate that this pattern, for the same reasons, will be even more distinct when it comes to making academic upper secondary school choices.

2. Theory and previous literature

The issue of tracking, or mixed ability classes – which are closely related but not identical[12] – is a sociologically interesting example of structural, or systemic characteristics having consequences at the individual level via group-level social interaction. The way schools are organized is a structural phenomenon, typically determined by national educational policy and local school management; it has an impact on individual's school careers and thus life chances, and by aggregation inequality between social groups; and this happens through the dual mechanisms of group-level instruction-and-learning processes and non-linear peer effects, which supposedly lead less able students to learn from more able.

Previous studies relevant to our research questions have analysed the structural level by comparing countries (Hanushek and Wössmann 2006; Pfeffer 2015; van de Werfhorst 2019) or counties (Esser and Seuring 2020; Matthewes 2020); the school or ability group(ing) level (e.g., Gamoran and Mare 1989; Hoffer 1992; Betts and Shkolnik 2000); and the peer-level (Hoxby 2000; Hanushek et al. 2003; Sund 2009). The results are, due to differences in approach and data, mixed, and taking into account difficulties in statistical modelling means that drawing robust conclusions is not straight-forward. If we stick to results using relatively convincing models and data, it would seem that ability homogeneity does not have any, or only weak, effects on efficiency (normally measured as average improvement in school results). This goes for studies from a variety of countries and school organizational characteristics. One type of studies has used test score growth with attempts to deal with the selection into ability groups (Kerckhof 1986; Hoffer 1992; Argys, Rees and Brewer 1996; Figlio and Page 2002); other studies have used school reforms or natural

[12] While ability grouping usually separates students in groups within classes and subjects, tracking separate students between classes or schools for teaching different curriculums (and it's difficult to change or move between tracks).

experiments to identify causal effects (see the review by Slavin 1990; and Meghir and Palme 2005; Guyon, Maurin, and McNally 2012; Kerr, Pekkarinen, and Uusitalo 2013; Sund 2013; Bygren 2016; Roller and Steinberg 2020). A third category of studies have relied on nationally comparable data, asking whether countries with tracking systems see more efficiency in test result growth, and whether inequality is greater (Hanushek and Wössmann 2006; Waldinger 2007; Pfeffer 2015; van de Werfhorst 2019). Additionally, it has been also show that in strong tracking systems like the one of the Netherlands most of the inequality associated with tracking happens through the process of sorting between schools rather than learning (Van de Werfhorst 2021). A relevant summary and review of the effects of tracking on efficiency and inequality has been conducted by Terrin and Triventi (2022) who conducted a meta-analysis of 53 analyses performed in the period from 2000 to 2021. They study concludes that the mean effect size of tracking on efficiency is null, whereas it remains positive and significant for inequality. Contrary to this rather consistent set of results, suggesting almost no average effect of tracking, Figlio and Page (2002) find that homogeneity in ability actually promotes school results, and the same argument is made by Esser (2016) and Esser and Seuring (2020).

Early on, Hallinan (1990) warned that the fact that studies did not find an *average* effect may conceal diverging effects across the ability distribution, so that ability grouping would improve the performance of high-ability students while low-ability students would worsen theirs. Such pattern of increasing gaps has also been found in several studies, again in different countries and using a variety of analytical approaches (Kerckhof 1986; Hoffer 1992; Argys et al. 1996; Hanushek and Wößmann 2006; Waldinger 2007; Brunello and Checchi 2007; Sund 2013; Roller and Steinberg 2020). However, other results have also been reported: Figlio and Page (2002) and Esser and Seuring (2020) maintain that low-ability students benefit from being taught in an ability-

homogeneous environment. The latter study reminds us that there is a difference between ability grouping determined by schools purely on the basis of proven ability and those based on parental choice, a point that has been made to account for the low inequality associated with early but ability-based track selection, primarily as practiced in the Netherlands (Bol et al. 2014; Korthals and Dronkers 2016). This is an example of how socioeconomic inequality could be reduced by deterring self-selection (Erikson and Jonsson 1996) and by letting schools make binding recommendations on track placement (Dollmann 2016). In theory, then, a low degree of stratification in an educational system (e.g., no or late tracking) in combination with strong selection on ability by schools at the point when tracking occurs, would be most effective for equalizing opportunities. However, in reality educational systems tend to be either/or, no doubt partly because of legitimacy problems (Jackson and Jonsson 2013).

The voluminous set of previous studies, many of them impressive and worth reading, are not without problems. Most US studies rely on lagged dependent variables, regressing test results in a subject (usually Math) on initial test results, comparing this average growth in schools (sometimes school classes) with and without ability grouping (this information often coming from teachers). This leaves open the question of why some schools use ability grouping, and others do not, with the risk that selectivity may bias comparisons. When a similar set-up is used for ability grouping within schools, the question of selectivity into ability groups poses a similar problem. Several statistical methods have been suggested to solve this, normally with some sort of selection models to mimic the allocation of students into groups in non-tracked schools, but these methods – for example propensity score matching – have been criticized (see, e.g., Figlio and Page 2002, who themselves use an instrumental variable model, which is far from ideal). Studies based on natural or quasi-experiments, often using differently timed implementation of reforms, offer an elegant

take on the selectivity problem. However, normally they cannot study the question of homogeneity or ability grouping with any precision, as organizational features and reforms often come in bundles (cf. Domina et al. 2019).

A substantial part of the studies in this literature use PISA or similar large-scale comparative data sets to compare the average test results between countries (or school systems) with and without tracking at lower secondary level (age 14-15 mostly), controlling for some initial ability level. Often, inspired by Hanushek and Wössman (2006), this is done via difference-in-difference models that are not based on longitudinal individual-level data, but compare tracking/non-tracking country averages in 'synthetic ability growth' using different samples; although there are studies that follow a cohort's growth in test results too. Also these studies suffer from a lack of precision when it comes to testing the ability homogeneity mechanism, only on a larger scale – many other things may differ between a school system that uses ability grouping and one that does not. In addition, just like most US studies, their outcome variable is theoretically imprecise by typically being a test result that is not curriculum based (it is rather the case that it should not be so constructed, as that would not enable country comparisons).

So, while all these studies are valuable contributions to our understanding of the consequences of ability homogeneity and ability grouping, they have not yet provided a conclusive picture, mostly because of data or methodological problems. This is regrettable when it concerns such fundamental issues as the efficiency and inequality consequences of organizing schools in different ways. We believe that we can contribute to a better understanding of these phenomena, partly by using a recent and suitable data set and adequate methods, partly by extending the analyses (a) into the issue of performance and choice effects, and by (b) analysing socioeconomic as well as ethnic inequality. By studying both England and Sweden, we arguably shed light on the

'core' case of modern Western comprehensive schools, and on country differences in efficiency and inequality that plausibly can be related to differences in the intensity of organizing ability grouping.

The English and Swedish school systems

In both the current English and Swedish educational systems, the large majority of comprehensive level school classes are predominantly mixed ability, with an element of ability grouping that is greater in the English system.

Sweden started the comprehensive journey already in the 1950s, although the reform was implemented gradually, and mostly in the 1960s. However, a formal division into two tracks in Math and English ('demanding'/'easy' course material), based entirely on parental-pupil choices, were used in grades 7-9 (age 14-16) up until 1994. The system was abolished partly because socioeconomic background rather than ability tended to determine the choices, and that those who followed the lower track were not eligible for academic upper secondary studies (Erikson and Jonsson 1993). However, contrary to what many believe, informal ability grouping is in fact still used by many schools (Erikson and Rudolphi 2016). But while around half of our samples of comprehensive schools use such informal ability grouping – most often in Math, and to a lesser extent in Swedish and/or English – there is no strong parental influence on choice of ability group, when these exists. Instead, the allocation of pupils to tracks is now something that schools rather than parents do, and often choose not to; and which ability group a pupil is in has no consequence for the eligibility to upper secondary school programmes as long as a pupil gets a 'pass' in the subject in question. Even in light of a small (but growing) independent school sector, the Swedish comprehensive system is subject- and curriculum-wise almost totally homogenous.

While England went comprehensive in the 1960s and 1970s, in our sample, almost all of the English schools practice ability grouping in Math and English, as well as Science. Ability grouping begins at age 12 and the allocation is done based primarily on tests taken at age 11 (SATS) – the allocation is done by the school with information to pupils. It is possible to move up (or down) the hierarchy of ability groups. In the end, the ability group will affect eligibility to university studies. In addition to ability grouping, there are other, related organizational variations within the comprehensive school system that bring to bear on the issue of ability homogeneity, for example types of ownership (e.g., state schools, public schools, and other types of 'independent' schools), curriculum differences, and variation in the subjects offered by schools (e.g., McMullin and Kulic 2016).

An important difference between the English and Swedish school systems is the 'natural' instructional group. While in Sweden, it is the school class, or classroom, in England it is rather the school cohort (year), as pupils in comprehensive school are in different groups in different subjects to a much larger extent than in Sweden (where this mostly occurs in second foreign languages and some non-core subjects such as physical education). Central for us, when ability groups are in place (as they are systematically in England, less so in Sweden), they constitute the relevant instructional group.

For upper secondary school, there is a predominantly free choice to continue in academic tracks in both countries (in England, A-levels; in Sweden 'studieförberedande program'), although in Sweden around 10% fail to achieve 'general eligibility'. Also in both countries, performance effects are quite sizeable both when it comes to socioeconomic (Jackson 2013) and ethnic (Heath and Brinbaum 2014) background, like in many other countries. Socioeconomic choice effects, to the detriment of the less advantaged groups, are also pertinent in both countries when more

rigorous statistical models are applied (see Chapter 2); and ethnic choice effects are to the advantage of minority groups in both countries, to a similar extent (Jackson et al. 2012). We believe that the ability homogeneity mechanism, if valid, would find fertile ground in both countries, but the more stern commitment to mixed ability classes in Sweden presumably leads to smaller socioeconomic and perhaps greater ethnic differences in the propensity to choose academic upper secondary studies, given proven ability.

3. Data and Empirical strategy

Data and variables

We use the first two waves of the Children on Immigrants Longitudinal Survey in Four European Countries (CILS4EU) (Kalter et al. 2014, 2018; www.cils4.eu). CILS4EU is a longitudinal survey of adolescents in which the main scope is to study structural, social, and cultural integration of youth of immigrant origin in England, Sweden, Germany and the Netherlands. The samples are based on a two-step cluster design. School represents the first sampling unit. Then, from each school, two classes were randomly selected and all pupils within these classes interviewed. In addition, in wave 1 also one parent in the child household was asked to be interviewed. Since the focus of CILS4EU is on immigrants, schools with a high proportion of immigrant youth were over-sampled. We reduced attrition issues by using multiple imputation with chained equations for our main outcome: Math and English/Swedish grades, GPA, and academic transition to upper secondary schools as well as for parental education, one of the few control variables with a substantial number of missing values (in total we used 50 imputed samples).

After imputing our data, the analytical sample comprises 8,816 pupils (4,050 in England and 4,766 in Sweden) clustered into 236 schools and 459 classes (on average 19 pupils per class, although the calculation of the ability averages in classes relies on more pupils). As mentioned

earlier, we focus only England and Sweden and not for example on Germany and the Netherlands (another two members of CILS4EU) because ability grouping and tracking, although related between them, are not strictly the same. The latter two countries are characterized by strong tracking system rather than ability grouping like the former two, England and Sweden.[13]

The first outcome in this study is pupil's grades, which were measured at the age of 16, defining the end of compulsory education in both countries. In the case of England, we use the self-reported grades in Math and English at the General Certificate of Secondary Education (GCSE). In Sweden, we used school records (*Årskurs-9 register*) on grades in Math, Swedish and English in the 9th and final year of comprehensive school. In both cases, grades were z-standardized within countries to enable comparability across countries.

We constructed ability homogeneity in instructional units by calculating the coefficient of variation (i.e., the standard deviation divided by the mean, hereafter also called CV) in pupils' cognitive ability for each school class (Sweden) or school cohort (England), as well as in ability groups, where those existed. Cognitive ability was measured via a general cognitive test at age 14-15 (wave 1), that is, the year before the last in comprehensive school. This variable was generated from a pattern test (Raven's progressive matrices) and has the advantage that is the most culture-free instrument for measuring 'fluid intelligence' (Weiß 2006).

Ability grouping was measured by the question: 'Is there a setting system at your school?' Pupils who answered in the positive were then asked to which ability group they belong in the relevant subjects. In Sweden, three different levels were reported: high, middle or low. In England

[13] In fact, the question regarding ability grouping was not administered in the Netherlands while in Germany a very small of the sample has replied to it. Making also difficult the inclusion of these countries from a technical point of view.

students reported up to 10 different ability groups. In order to improve comparability across countries we collapsed these 10 ability groups into 3 categories similar to the Swedish case.

As individual control variables we include pupil's gender (boy/girl), their cognitive and language ability, generational status (majority, first generation, and second generation), father's and mother's years of education (8 years = primary or less, 12 years = lower secondary, 14 years = upper secondary and >17 years = university/tertiary), parental occupational status (ISEI), aggregated in fourth quantiles where 1st = lowest ISEI; 4th = highest ISEI. At the classroom/school cohort level, we also considered the proportion of pupils from first generation of immigrants, the proportion of fathers and mothers with university education, and the proportion of parents in the fourth highest ISEI quantile. Finally, to account for the survey design, we always include the proportion of immigrants in schools (school stratum in five categories).

Approach and models

Our first set of analyses address the theoretical mechanism directly, namely the link between ability homogeneity and school performance, measured by grades (which is the theoretically relevant outcome, rather than test scores, often used in previous research). Because we have information on homogeneity (CV of cognitive ability in instructional units) and grades in relevant subjects, summarized as grade point averages (GPA), this sounds straight-forward. However, as we have noted, selectivity issues need to be addressed. Two questions have emerged that challenge many of the previous studies: (1) How can we account for the fact that pupils, and their parents, may choose schools depending on whether they practice ability grouping or not (or to which degree they do)? (2) And, in schools that do, how can we account for the process of allocating pupils into instructional groups, so we do not confuse the criteria for doing this with the effect of being in a given group?

For our data, we can almost ignore the problems of (1) because in England, all school classes practice ability grouping, and in Sweden, this practice is informal, often incidental, depending on continuously renewed teachers and collegiate decisions, and therefore difficult for parents to either know or predict. However, in England schools might still differ in resources and teachers quality with disadvantaged students facing larger learning difficulties although they could be assigned to high ability groups. A rather straight-forward, but very useful model for studying the efficiency-generating mechanism is to analyze the effect of ability homogeneity on aggregate school performances, using school fixed effects (variation within schools). We fit such models for both countries but we also run comparable models controlling for cognitive and language ability and background variables, addressing the question whether their unique approach to ability grouping is associated with average outcomes or levels of inequality. In ability homogeneity analysis, therefore, we first estimate an unconditional effect through an OLS model on the standardized GPA (students' performance) and the transition probability to the academic track in upper secondary education (students' choices) to observe the overall association without accounting for selection and sorting effects. Then, we employed school fixed effects (School FE) to reduce such endogeneity, utilizing the fact that we collected data from two school classes from each school in estimating the grouping effect between classes within schools – thereby, we short-circuit the selection into schools, which is a parental/student choice much more than the selection into school classes, which is done by the school, typically randomly. This strategy works best for Sweden, where school classes are the prime organizational unit, but less so in England, where school year is more appropriate. Finally, in our analysis of ability homogeneity, as a sensitivity test, we also run our models only on schools that do not practice ability grouping (this represents less than 10% of the sample in England and circa 50% in Sweden). Since by definition these are mixed ability

classes, and thus more heterogeneous in their ability composition, we should expect a null effect. Indeed, accordingly to this prediction, we do not find any substantive or statistically significant effect of the ability homogeneity variable. This robustness check is available in the Appendix (see Table A.3.1).

After the ability homogeneity analysis, we look at the actual effects of being allocated to a specific math ability group on math performance and educational choices. In this case, our key explanatory variable is the ability grouping variable, distinguishing between mixed ability, low ability, mid ability, and high ability groups. In terms of outcomes we look at the effects of math ability grouping on Math grades and, again, on the academic choice in upper secondary education. For this analysis, we rely on the OLS with individual and contextual controls and the School FE estimators but we also employed an Inverse Probability of Treatment Weighting with Regression Adjustment (IPTW-RA) estimator for categorical treatments (Cattaneo 2010). This approach consist of estimating the probability of receiving the treatment given a series of observed characteristics (first equation) and then reweighing the sample using these estimated propensity scores to restore the balance between groups (second equation). We opted for this approach instead of, for example, matching approaches such as Propensity Score Matching because, first, by reweighting we do not discard any data as in standard matching techniques and, second, it allows to adjust for the observable characteristics in the outcome model, reducing potential selection into the outcome variable as well. Moreover, one important feature of IPTW-RA is its double robustness property: even if one of the models (treatment or outcome) is mis-specified, the estimator is still consistent. Therefore, compared to the OLS and School FE models, the IPTW-RA represents a more flexible way of accounting for potential selection and improving

comparability between the mixed ability group (reference group) and the different treatment status (low, mid, high ability groups).

In addition, we conducted two robustness checks on the ability grouping analysis. First, taking advantage of the fact that our data contain school grades in math and languages subjects, we used a placebo test looking at the effects of math ability grouping on English/Swedish standardized grades. The assumption is that math ability grouping should have an effect on Math grades but not on other unrelated subjects. If we observe similar effects this should indicate that there are other unobserved factors explaining the association between math ability and students' performance and choices. Second, we also develop a counterfactual analysis using the Swedish sample and imputations techniques. In this case, we imputed the potential ("would-be") ability group to those students in mixed classes using as main predictors students' cognitive and language ability and their sociodemographic characteristics. Then within each level of ability we compared the counterfactual group (those with imputed ability group) against the factual group (those with a real ability group) within the same level of ability. The imputation model is based on chained equation and 50 datasets. Each comparison between imputed and factual students within a similar level of ability group was performed using IPTW-RA to account for the sorting into schools. The main assumption behind this approach is that the model for allocating students within ability groups is as good as the teacher decision process. Therefore, as a further check, we also evaluated the selection into ability groups by running a series of linear probability model on the likelihood of attending a high ability group. Once the sorting into schools is accounted for, these models confirm that most of the allocation of students into ability groups is based on their cognitive and language ability.

4. Results

Table 3.1 gives descriptive statistics of key variables in our analyses (weighted to take care of the stratification of the sample). For each variable and country we provide the mean, standard deviation, minimum and maximum values. Overall, there are no large differences in terms of pupils' demographic characteristics between the two countries. We only observe that, compared to England, Sweden has a larger percentage of second generation students (36 percent against 25 percent). Also, Sweden has a lower percentage of highly educated parents than England (25 percent against 35 of parents with 17 years of education).

Table 3.1: Summary statistics

	England				Sweden			
	mean	sd	min	max	mean	sd	min	max
Students variables								
LANG std	0.00	1	-4	2	-0.00	1	-4	2
CA std	0.00	1	-4	2	-0.00	1	-4	2
Girls	0.51	0	0	1	0.50	1	0	1
Natives	0.65	0	0	1	0.56	0	0	1
1st Gen	0.10	0	0	1	0.08	0	0	1
2nd Gen	0.25	0	0	1	0.36	0	0	1
8 years	0.05	0	0	1	0.04	0	0	1
12 years	0.31	0	0	1	0.22	0	0	1
14 years	0.29	0	0	1	0.48	0	0	1
17 years	0.35	0	0	1	0.25	0	0	1
Parental ISEI: High	0.25	0	0	1	0.27	0	0	1
Parental ISEI: Mid-high	0.27	0	0	1	0.26	0	0	1
Parental ISEI: Mid-low	0.23	0	0	1	0.24	0	0	1
Parental ISEI: Low	0.24	0	0	1	0.23	0	0	1
Contextual variables								
Sch. Par. ISEI average	42.79	18	-43	78	45.54	13	-22	72
Sch. Prop. of 1st Gen	0.10	0	0	1	0.08	0	0	1
Sch. Prop. of high educ. fathers	0.25	0	0	1	0.14	0	0	0
Sch. Prop. of high educ. mothers	0.24	0	0	1	0.22	0	0	1
School stratum: 0-10%	0.45	0	0	1	0.31	0	0	1
School stratum: 10-30%	0.23	0	0	1	0.48	0	0	1
School stratum: 30-60%	0.10	0	0	1	0.14	0	0	1
School stratum: 60-100%	0.07	0	0	1	0.07	0	0	1
School stratum: ind. schools	0.15	0	0	1	0.00	0	0	0
Observations	4,050				4,766			

Ability homogeneity results

Figure 3.1 shows the regression line generated by an OLS model with GPA as outcome (Y-axis) and ability homogeneity along the X-axis (higher values indicating more heterogeneity). The left graph is the gross effect (only controlling for the sampling variable), and the right is the net effect, conditional on both individual and aggregate control variables, and using a school fixed effect model. The top part of the figure shows the results for England while the bottom part those for Sweden. Overall we observe that while the gross model shows that more ability heterogeneous school classes have lower GPA, this relation disappears in the conditional model. This, we

conclude, is a rather strong indication that the average effect of ability homogeneity is negligible in both countries.

In terms of country differences, we observe that the unconditional association seems to be much stronger in England than in Sweden, perhaps because there are more homogenously 'high-ability' schools in England. However, as mentioned before, once we employ the full school fixed effects model, the effects on GPA becomes slightly positive and constant across the whole distribution of the classroom homogeneity variable indicating again null effect. In this case, it is not possible to observe any difference in effect sizes (levels) between countries, suggesting that the selection into ability-heterogeneous classes is stronger in England.

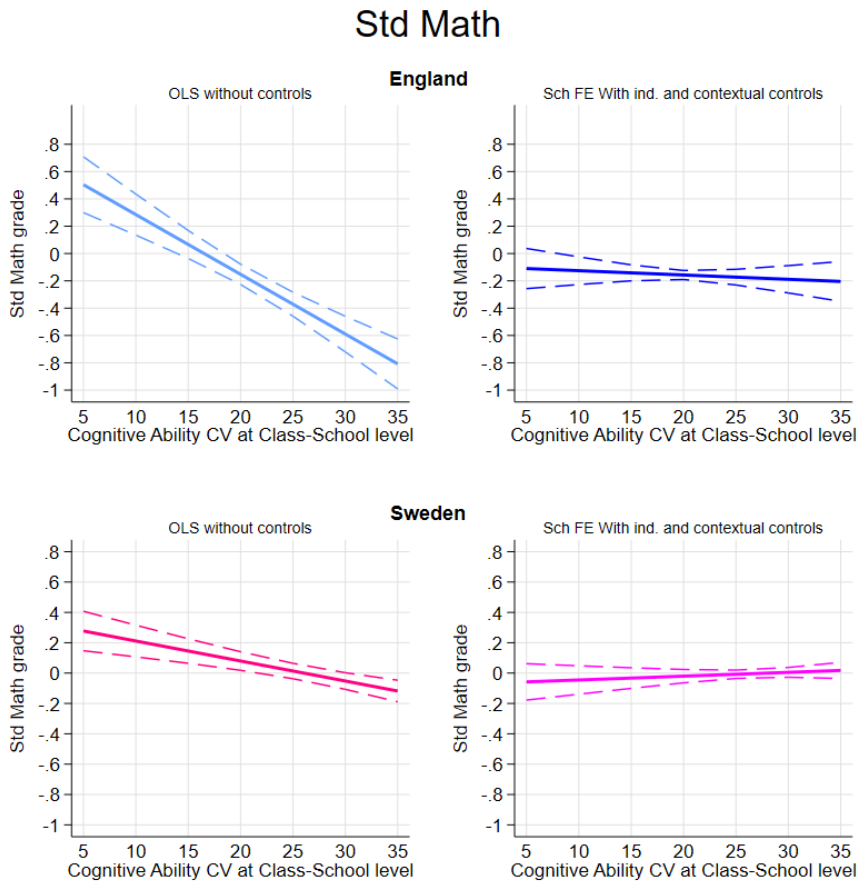


Figure 3.1: Standardized GPA and CV in cognitive ability

Next, we turn to the transition into the academic track of upper secondary education. Figure 3.2 shows the regression line with the transition probability after controlling for students' GPA. In the unconditional models (left-side panels) we observe a slightly negative effect among more heterogeneous schools in both countries. However, these effects are not statistically significant. In the conditional regression models (right-side panels), the slopes slightly change sign, but as before confidence intervals are large enough to indicate no significant difference between schools. In sum, these analysis indicate no differences in school performance and educational choice once conditioned on GPA between low and high heterogeneous ability classes in both countries.

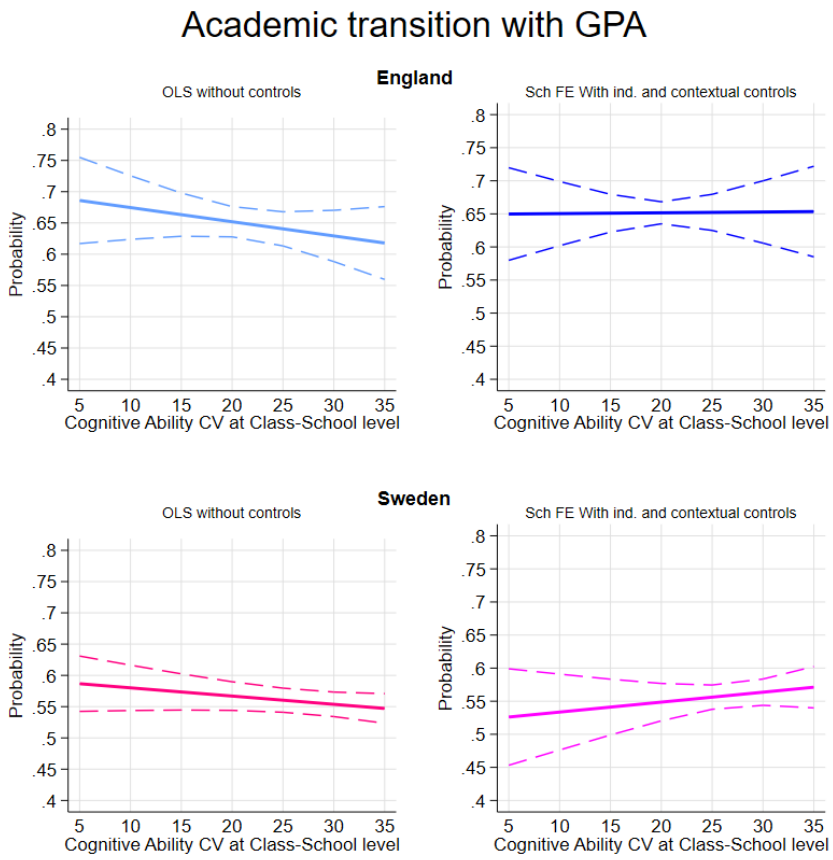


Figure 3.2: Academic transition in upper secondary education and CV in cognitive ability, controlling for standardized GPA

As a sensitivity check, we re-ran the analysis on the GPA outcome on a subsample of students who only attended a mixed ability group, which is a heterogeneous ability class by definition. The regression coefficients of this exercise can be found in Table A.3.1 of the Appendix. As expected, for both countries we found null effects of the variance in cognitive ability on the GPA, reassuring the validity of the previous analysis.

Ability grouping results

Moreover, as a second exercise, we look at the actual effects of being allocated into different Math ability groups on math grades and academic transitions in upper secondary education (controlling for students GPA). In this case, we employ three different types of estimators to account for the potential selection into schools: OLS with individual and contextual controls; school fixed effects and Inverse of Probability of Treatment Weighting with Regression Adjustment (IPTW-RA). The three methods provides very similar results. Therefore, we opted for reporting the estimates of the IPTW-RA as main results here and those from the OLS and school fixed effects in the Appendix, section A.3.2. We also replicated these analysis on the GPA and Academic transition to upper secondary education without controlling for GPA. These analysis are available in the Appendix as well (see section A.3.1).

As before, we start with the analysis of students' performance by looking at their Math grades. Figure 3.3 displays the results of math ability grouping on math grades for England (upper panel) and Sweden (lower panel). For each coefficient we report the confidence intervals at the 90%, 95%, and 99% levels. The reference category are students who do not have any math ability grouping in their school (i.e., referred as mixed classes, or the control group). Each level of ability grouping was compared to this control group. Overall, we find similar pattern between the two

countries with students in high ability groups performing better in both countries. More specifically, in the case of England, we observe that students in low ability classes perform slightly worse than those in mixed classes, although these results are not statistically significant (-0.06 standard deviations, sd). The mid-ability group performs better than the mixed ability group (0.14 sd) as well as the high-ability group who has the higher point estimate (0.39 sd). This pattern is slightly different for the case of Sweden where low-ability (-0.49 sd) and mid-ability groups (-0.10 sd, not significant at conventional levels) perform worse than the mixed group. However, also in this case, the high-ability shows a better performance than the mixed group (0.31 sd).

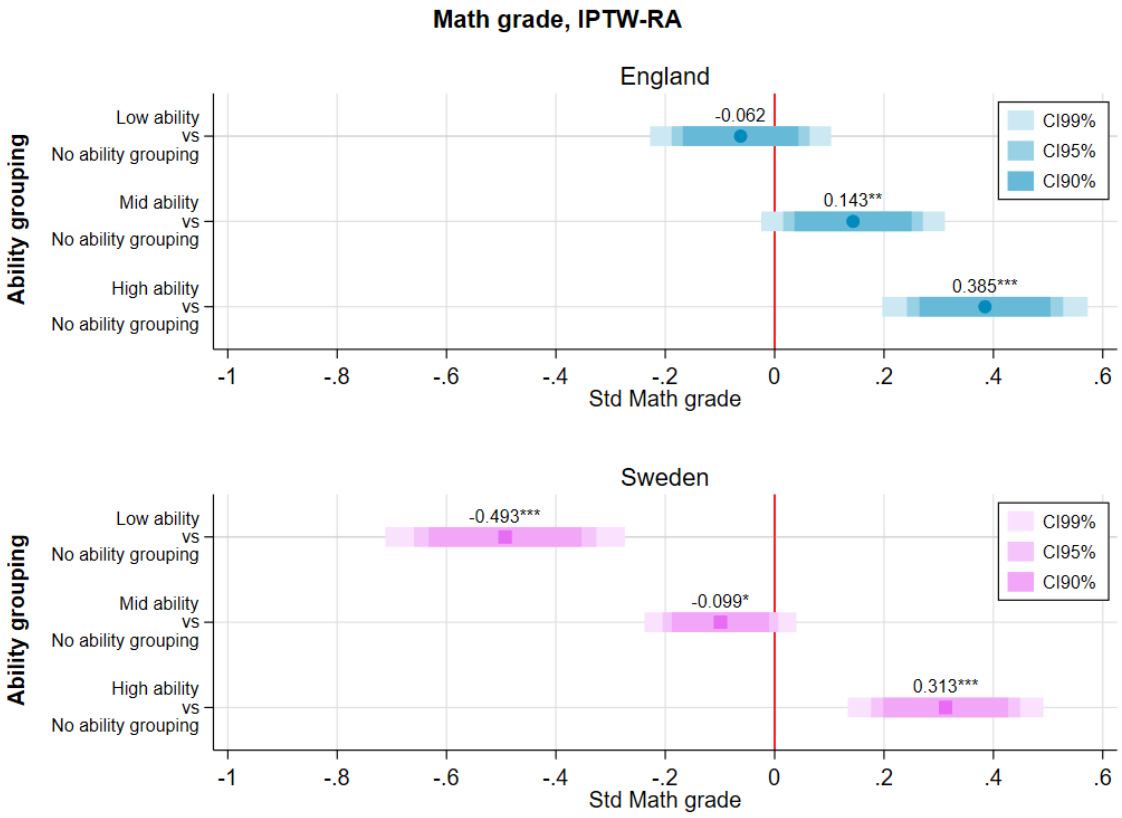


Figure 3.3: Standardized math grades and math ability grouping

In the next step, we analyse whether math ability grouping has an effect on academic transitions at upper secondary education. Figure 3.4 shows these results for England in the upper graph and for Sweden in the lower. These estimates are, again, stable across the three models (IPTW-RA, OLS with controls and school fixed effects) and, in general, we do not find any substantive and significant effects on the transition probability once we account for students' GPA. In England, compared to mixed ability students, the transition probability for low-ability students is 0.02 percentage points (p.p.) higher, while the probability for mid ability is 0.04 p.p., and for high ability 0.06 p.p. (only significant at 90% level), respectively. On the other side, in Sweden, the transition probability for the low ability group is -0.07 p.p. (significant at 90% level), for mid ability -0.01 p.p., and for high ability is 0.01 p.p. We still observe a similar pattern in terms of effects sign: negative sign for low ability and positive sign for mid and high ability groups, but none of these estimates are statistically significant at conventional levels (i.e., 95% and 99%), and remain very small in magnitude.

In sum, the results from the ability grouping analysis suggest that there is a negative effect on math performance for those not attending the highest math ability group as compared to the mixed ability classes. This effect, of around half a standard deviation, is in fact rather sizeable, and gives some credit to concerns that low-ability groups suffer a disadvantage, be it because instruction is of lower quality, or because the learning environment is worse. However, even if our models account for potential sorting into schools there could still be other unobserved factors that make students both more likely to attend (high) ability groups and to perform better. We test this hypothesis through a series of placebo tests that are reported in the following paragraphs. In terms of academic transitions to upper secondary schools, we find similar results to those obtained in the

ability heterogeneity analysis, confirming that ability grouping does not matter much for educational choices once GPA is accounted for.

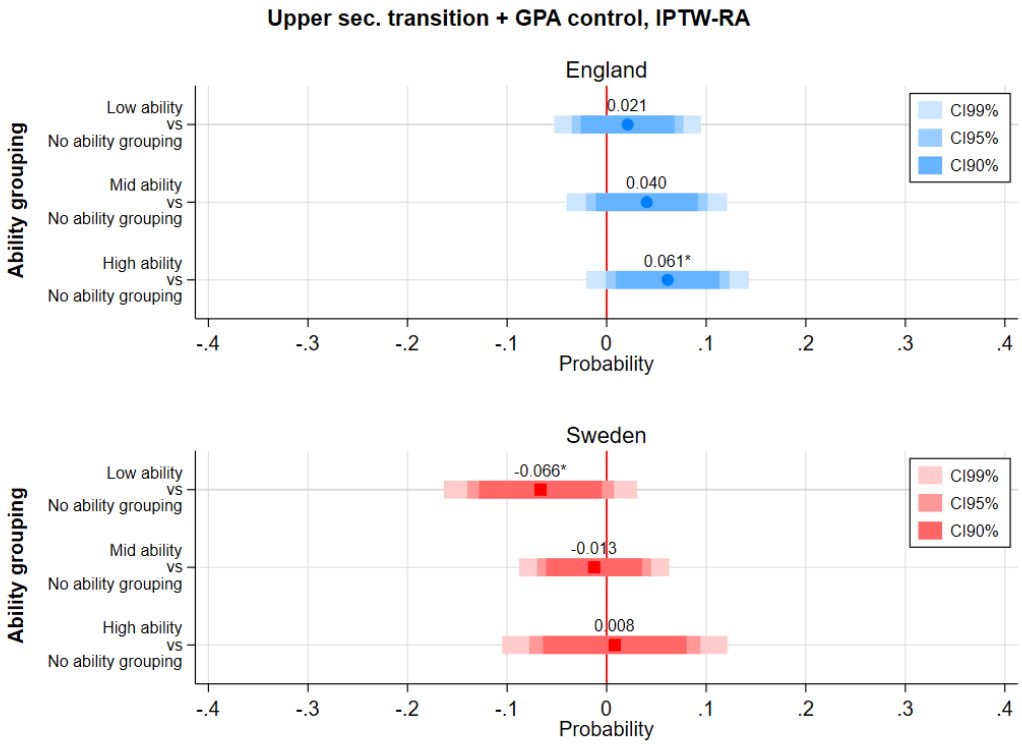
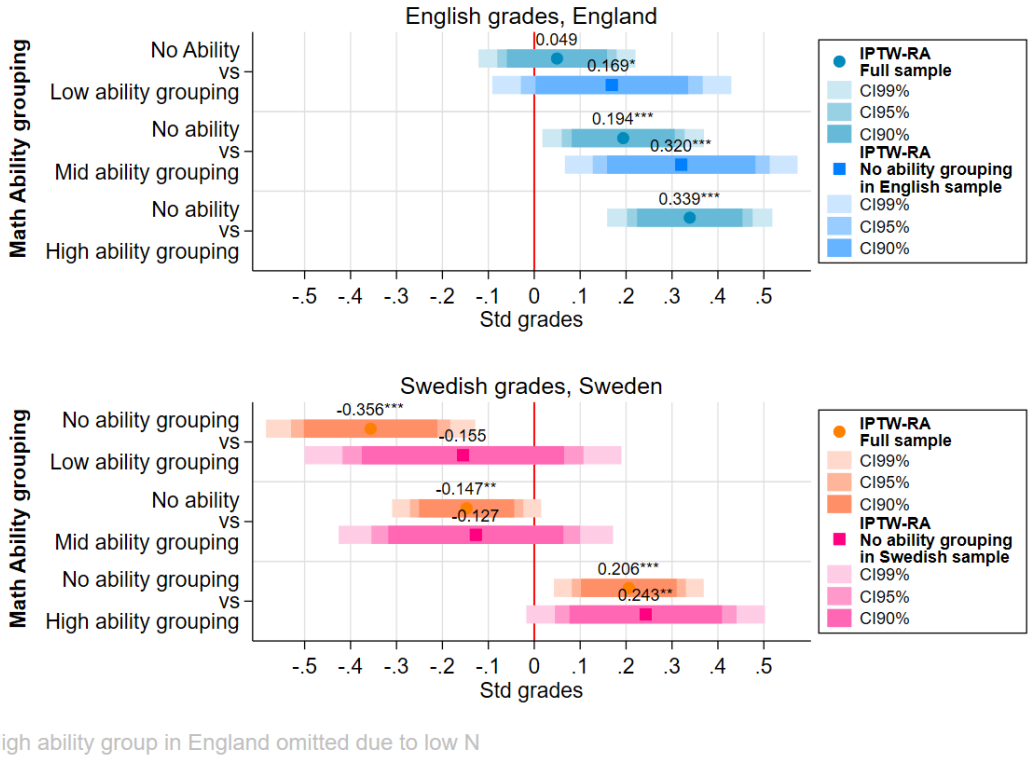


Figure 3.4: Academic transition in upper secondary education and math ability grouping, controlling for standardized GPA

For the placebo exercise, we focus on the English/Swedish grades and look at whether the math ability grouping has some effect on this unrelated outcome using the IPTW-RA models. The logic behind is that math ability grouping should not have the same effects on other subject grades as the one observed in math. If this the case, this should indicate that there are other factors behind that explains the results of ability grouping. We test this, first on the whole sample of students, independently of whether they have ability grouping in English/Swedish, and then on a sample that reported to have ability grouping only in math. The second sample should provide a more precise tests since we exclude students that also have ability grouping on language subjects, and

thereby minimise the risk for some unobserved characteristic of schools to bias our estimates of the ability group effect. For Sweden it is rather common to be in a mixed Swedish ability grouping among those with math ability grouping while in England this seems to be the case only for those in mid and low math ability grouping. That is, in England there is not much variation for the high math ability grouping since only 1% of this group participate in a mixed ability group in the English language subject. Therefore, we suppress the point estimate for the English grade in Figure 3.5. In the Appendix, Figure A.3.8 and A.3.9, we report the percentage distribution of students that have ability grouping in both subjects.



High ability group in England omitted due to low N

Figure 3.5: Placebo test, math ability grouping on standardized English/Swedish grades

As expected, in Figure 3.5, we find a very similar pattern as those of math grades in the effects of math ability grouping on the standardized English/Swedish grades. For England, there are no significant differences between low math ability and mixed ability groups (0.08 sd in the

restricted sample) while there is a positive and significant effect for mid (0.32 sd) and high (0.33 sd only reported for the first sample) groups. For Sweden, we observe instead a negative effect, although not significant for low and mid math ability groups compared to mixed ability (-0.13 sd in the restricted sample), and we see a positive effect for the high ability group (0.24 sd). The placebo test, therefore, casts some doubts on the idea that ability grouping has some causal effects on students' performance and suggests that the observed differences between groups could be completely spurious due to unobserved factors, or because we are still not able to compare similar students with and without ability grouping.

Additionally to the placebo test, we also exploited the fact that the distribution of math ability grouping in Sweden is almost half and half in this sample and conducted a counterfactual analysis as a further robustness check. This counterfactual analysis consists of imputing the potential ability group that would have been assigned to students in mixed classes if their school have had adopted this practice. Then, we compare the differences between the factual (those with real ability groups) and counterfactual (those with the imputed ability groups) treatment status.

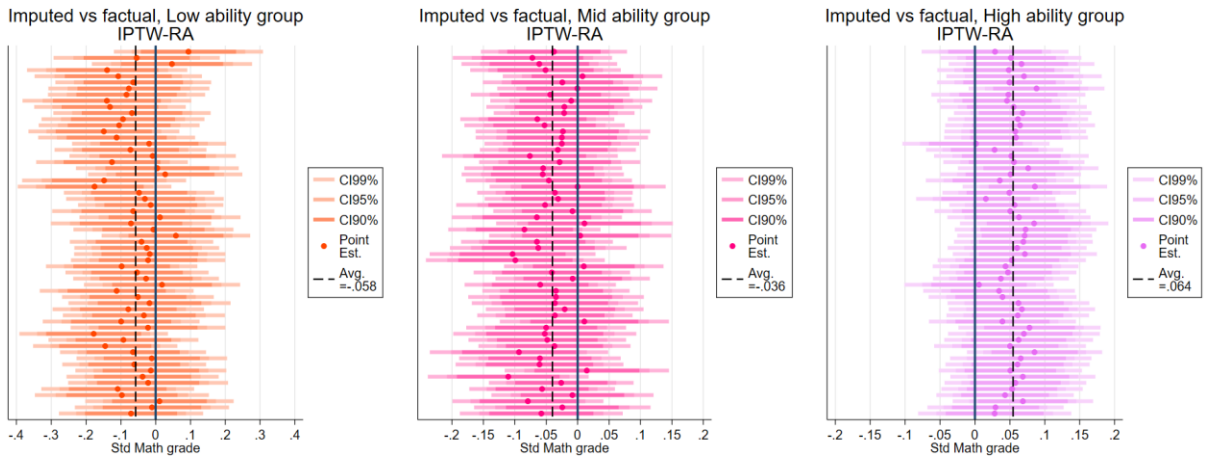


Figure 3. 6: Counterfactual analysis on Sweden, IPTW-RA

Figure 3.6 shows the difference between the imputed and factual groups within each level of ability grouping across the 50 imputed datasets accounting for the potential sorting into schools through the IPTW-RA approach. The left hand side panel is the low ability group, the panel in middle is mid ability group, and the right side is high-ability group. The average effect across all the samples (dash vertical line) is -0.058 sd for the low ability group, -0.036 sd for the mid ability group, and 0.06 sd for the high ability group. As expected, none of these effects are statistically significant. Also, these effects are extremely small in terms of magnitude, remaining very close to 0 (solid black line in the three panels), although in terms of effect sign they follow the pattern observed in the main ability group analysis. In sum, therefore, once we refine the comparison groups, we do not find any effects of ability grouping on math performance.

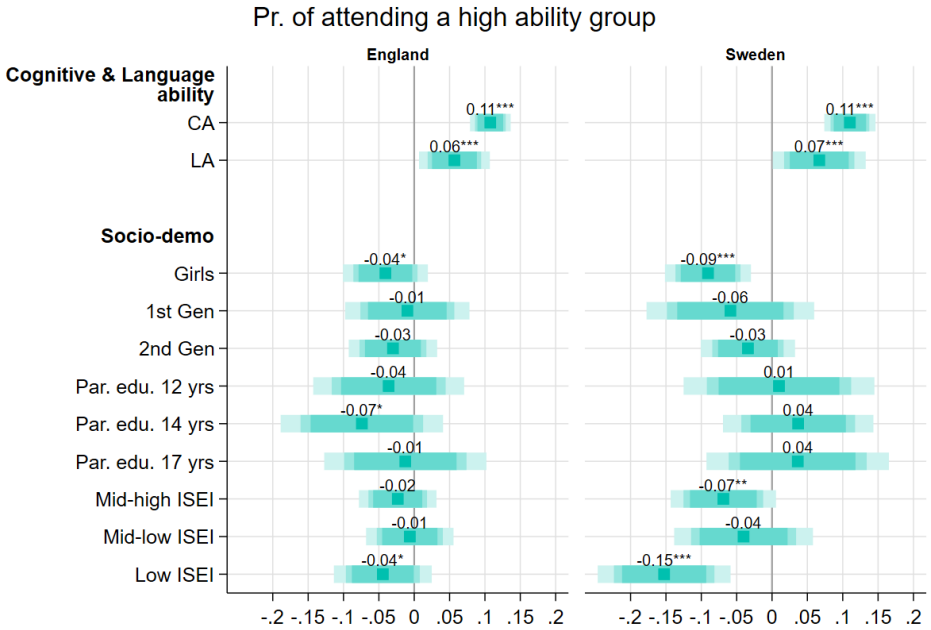


Figure 3. 7: Selection into high math ability groups, linear probability model
Note: confidence intervals at 90%, 95%, 99%.

As a final exercise we look at the predictors of attending a high math ability group. Figure 3.7 displays the regression coefficients with their confidence intervals at 90, 95 and 99 levels. In

both countries the cognitive (0.11 sd for both countries) and language (0.06 sd in England and 0.07 sd in Sweden) ability are significant factors associated with the probability of attending a high ability group. None of the sociodemographic factors seem to matter much in England, but gender and parental ISEI have some effects in Sweden: girls and low-ISEI students are less likely to attend a high ability group in math. However, overall, much of the participation in high ability groups within schools seems to depend on students' ability.

Inequality results

As a final analysis, we also look at whether ability homogeneity and ability grouping interact with parental years of education, parental ISEI, and generation status, generating potential inequalities in math performance and educational choices. For this purpose we employed separate school fixed effects models with the full set of controls variables for each interaction and outcome. In the case of parental years of education and parental ISEI we treated these variables as continuous, facilitating the estimation (higher degrees of freedom) and interpretation (lower number of interaction coefficients). The parental ISEI scale was also standardized. In total, we estimated 12 models per country (circa 48 interaction effects) and for the sake of simplicity we report in Table 3.2 a summary of the main findings, indicating whether there was a null effect (i.e., small effect in magnitude and not statistically significant) or a noteworthy effect (in that case, we report its point estimate, significance level, and standard error). In the Appendix, section A.3.4, we show all the full tables of these regressions.

Table 3.2: Inequality analysis, School FE models with interaction effects

	England		**Sweden**	
Homogeneity analysis	***Std GPA***	***Transition with GPA ctrl***	***Std GPA***	***Transition with GPA ctrl***
CA CV # YPE	Null effect	Null effect	Null effect	Null effect
CA CV # PISEI	Null effect	Null effect	0.005** (0.002)	Null effect
CA CV # Gen status	Null effect	Null effect	Null effect	Null effect
Ability grouping analysis	***Std Math***	***Transition with GPA ctrl***	***Std Math***	***Transition with GPA ctrl***
Abil. grouping # YPE	Null effect	Null effect	Mid ability#YPE -0.039** (0.018)	Null effect
Abil. grouping # PISEI	Null effect	Null effect	Null effect	Null effect
Abil. grouping # Gen status	Null effect	Null effect	Null effect	Null effect

Note: Main results from school fixed effects models with control variables, ** $p < 0.05$, robust standard error in parenthesis; Full regression tables available in the Appendix. YPE = years of parental education; CA CV = cognitive ability - coefficient of variation; PISEI = parental ISEI. For the sake of comparability between CA CV and ability grouping results we adopted the same model specification based on school fixed effects.

The upper part of Table 3.2 shows the results for the ability homogeneity analysis for England (left-side panel) and Sweden (right-side panel) for the GPA and academic transition to upper secondary education after accounting for GPA. The first row is the interaction between ability homogeneity (i.e., the coefficient of variation in cognitive ability, CA CV) interacted with (symbolised by #) parental years of education (YPE). In a similar vein, the second row is the interaction between ability homogeneity and parental ISEI (CA CV#PISEI), and the third row between ability grouping and generational status (CA CV#Gen stauts). The lower part of the table shows the corresponding interaction results replacing the variance in cognitive ability with ability grouping.

The results from the ability homogeneity analyses show no differences across family background and generation status. The only significant estimate is for parental ISEI in Sweden,

where for each standard deviation in the ISEI scale we observe an increase of 0.005 (p-value<0.05) standard deviations in GPA – this is however an extremely small effect. Similarly, we did not find any particularly important heterogeneity in effects for the ability grouping analysis. The only significant results is for the mid-ability group and parental education in Sweden, where we see that for 1 years increase in parents' education there is reduction of the math performance of -0.039 sd (p-value<0.05) which, again, remains relatively small in size. In total, these analyses show that more ability homogeneous/heterogeneous schools or the practice of ability grouping do not seems to produce further inequalities in school performance or educational choices. This could be simply because these instructional practices do not have a causal effect on students' outcomes, at least in the context of more comprehensive educational systems like England and Sweden.

5. Conclusion and discussion

This research set out to examine whether homogeneity of instructional groups in comprehensive schools in terms of cognitive ability and ability grouping, respectively, have an effect on students' performance and educational transition to upper secondary education. In particular, we contribute with a comparative study of England and Sweden, two countries with comprehensive educational systems, but with different ability grouping practices. While England has an institutionalized practice of grouping students in 'sets' on the basis of observed abilities, Sweden only uses ability grouping occasionally and mostly depending on teacher and principal preferences. Equipped with rich data from CILS4EU, we first analyzed the role of ability homogeneity using OLS and school fixed models to reduce potential selection into schools. Once we account for selection, we did not find that higher homogeneity in terms of cognitive ability has any significant effects on academic performance or educational transitions. Second, we then looked directly at the effect of ability grouping using an inverse of probability of treatment weighting with regression adjustment. Our

first estimates suggested that ability grouping in Math has a positive effect on Math grades for high ability groups in both countries and detrimental effect for the low ability group in Sweden only. However, a placebo test based on English grades suggested that observed effects for Math could be due to some unobserved factors and the difficulty of properly compare similar students. In addition, we ran a counterfactual analysis, exploiting the fact that half of the sample in Sweden attend schools with ability grouping and the other half not. In this analysis, we imputed the potential ability group that students would most likely have attended if their school had adopted this practice. Once we managed to compare very similar students, the results indicated that ability grouping does not have any effect on Math grades. Also, we did not find any significant effect of ability grouping on academic transitions once we controlled for students GPA. Finally, we investigated whether there could be some heterogeneous effects with regard to parental education, parental ISEI, and generation status, finding almost no significant differences that would be in line with the hypothesis that ability grouping increase inequality.

In terms of implications our finding suggest that ability homogeneity does not have any effects on efficiency. Similarly, we did not find enough evidence to claim that ability grouping either improves school performance or produces inequality. We rather found that past evidence on the effects of ability grouping on student performance may be explained by unobserved factors and lack of adequate comparison/control groups. These results, it is important to note, concern England and Sweden – the effect of ability grouping may well vary across educational systems. Furthermore, moving beyond academic performance measures, we did not find any substantial effect of ability homogeneity and ability grouping on educational choices once we accounted for students' performance. This is also a novel finding as the previous literature has exclusively focused on performance effects only.

Conclusions

Main contribution

Reducing inequality of educational opportunity (IEO) remains a pressing problem for industrialized countries with democratic regimes. Despite major governmental efforts for widening participation in post-compulsory education, it is still possible to observe substantial socioeconomic gaps in educational achievements and attainments. In order to address this issue, European sociologists have developed a conceptual model of educational decisions that allows to separate and quantify the core mechanisms generating and perpetuating such inequality. These two mechanisms are defined as performance and choice effects (Boudon 1974; Erikson and Jonsson 1996). Performance effects are all those, independently of being determined by genetic or socio-cultural traits, that are expressed in socio-economic differences in academic performance (as measured by, for example, grades or school marks, formal examinations, or school tests). Such indicators of performance has a strong effect on educational transitions. Choice effects are in turn all those that are expressed in actual educational decisions given similar levels of academic performance. That is, even when students from upper and lower socioeconomic background show similar levels of performance, the latter group still makes less ambitious educational choices. Importantly, by quantifying the relative importance of these two mechanisms, this model can be adopted also to inform policy makers about 'when' and 'what' type of interventions should be implemented to reduce inequality (Jackson 2013).

Over the last two decades, empirical studies using this model have shown that performance effects are relatively larger than choice effects and this difference seems to widen among younger birth cohorts (Erikson and Rudolphi 2012; Jackson et al. 2007; Jackson 2013). On the basis of this evidence, it has been claimed that policy makers should focus primarily to improve learning

outcomes at early stages of the educational trajectory rather than, for example, helping students from disadvantaged backgrounds to make more ambitious choices at the moment of the educational transition. In times of limited public resources this kind of policy implications and advice can have tangible repercussions on the opportunities of several younger generations.

Surprisingly, despite a few methodological advances in the decomposition and quantification of performance and choice effects (Erikson et al. 2005; Morgan 2012), there are still important limitations that have not been addressed in the literature, which ultimately casts some doubts about the possibility of this model to inform policy. Partly because of data limitations but also a certain resistance to adopt causal identification approaches, three technical limitations in the literature should be noted. First, the model still relies on strong assumptions regarding confounding bias. Particularly important is the role of post-treatment confounders which in the case of performance and choice effects research usually take the form of anticipatory decisions (Erikson et al. 2005; Jackson 2013). In particular, students might anticipate educational choices at early stages of their educational trajectory and thus underperform at school if they are not interested in continuing education after mandatory schooling. Second, past studies also ignored the implementation of sensitivity analysis for evaluating the robustness of results to unmeasured confounders. Third, and not least important, it the calculation of confidence intervals for the estimates of the relative importance of performance and choice effects has been completely neglected, making it difficult to tell whether these two mechanisms are statistically and significantly different.

In addition, there is also an empirical limitation that has been overlooked when it comes to identifying the most relevant determinants of performance and choice effects. This is how the instructional time is organized within schools and classes, typically known as ability grouping, setting, or streaming. Contemporaneously to the performance and choice effects literature, another

body of research focused on whether ability grouping has any causal effect on students' performance outcomes, finding that this practice usually produce further inequality. Despite of been closely related, the literature on performance and choice effects and the one on ability grouping have never informed each other. Most studies on ability grouping only analyzed students' performance, disregarding the importance for educational choices. Further, many lacked empirical designs to identify causal effects, mostly due to data limitations – typically lacking cognitive ability measures at the student and class levels – and not being able to take selection into ability groups into account. The research on performance and choice effects, on the other hand, never evaluated whether ability grouping could be another important determinant of these two mechanisms. This is probably because the theoretical model of performance and choice effects is conceptualized at the individual level rather than at the institutional level, putting the focus on micro-behavioral mechanisms that should operate in any educational context. It is timely, therefore, to also integrate these two strands of research to improve our understanding of educational inequality.

By addressing all these issues, this thesis makes several contribution to the existing body of knowledge on the mechanisms that explain the persistence of inequality of educational opportunity. These contributions range from methodological to empirical viewpoints. In terms of methodology, following the work of Erikson et al. (2005) and Morgan (2012), I evaluated the identification assumptions of the standard model, surveyed and tested the standard methods for decomposing performance and choice effects, and provided a more comprehensive framework that includes causal mediation methods for dealing with post-treatment confounders and sensitivity analysis. In this regard, I show ways of further unpacking the paths that conform performance and choice mechanisms and make more transparent the limitation of these models. Upon the utilization

of DAGs and the previous theory researchers can decide which variables to include in the model and how to deal with potential confounders. Chapter one, therefore, should guide and help researches in this area to conduct more robust decompositions of performance and choice effects. From the empirical point of view, using novel data from Children of Immigrants Longitudinal Survey in Four European Countries (CIL4EU) and causal mediation methods, I provided new estimates of the relative importance of choice and performance effects in transitions to upper secondary education for younger birth cohorts (i.e., people born in 1995/6) in the context of Sweden and England. These two birth cohorts have not been studied before and thus I add new estimates of performance and choice effects to the literature. I show in chapter 2, I performed different models to show how much the relative importance of performance and choice effects varies depending on the assumed causal structure and the model employed. In this way, by estimates can be also bounded for informing policy.

In addition, I also evaluated the impact of ability grouping and instructional group homogeneity on academic performance and choices (chapter 3). Using several identification strategies that attempt to overcome past limitations in this literature, I show that the common conclusion, that of ability grouping only improving performance among high ability students, thereby increasing inequality, is questionable. Once we manage to compare similar students, I find no such differences, suggesting that there are no effects on both performance and choice outcomes. This chapter brings together the literature focused on the organization of classes and schools with the one on performance and choice effects, and shows that ability grouping might not be relevant factor for explaining students' performance and educational choices in comprehensive educational systems like the ones in Sweden and England. The key findings of this thesis are summarized below.

Summary of findings and policy implications

The first empirical chapter of this thesis initially examined the limitations of the conceptual model originally developed by Boudon (1974) and the standard decomposition methods employed in sociology for quantifying the relative importance of performance and choice effects. The original model was based on measures of parental class or education, students GPA, and educational transitions, completely disregarding the role of potential confounders. When the performance and choice effects are decomposed, it should be considered that exposure-outcome, exposure-mediator, mediator-outcome, and post-treatment confounders (also known as intermediate confounder) could affect the relationship between social origins, GPA, and educational transitions. Particularly worrisome is the role of post-treatment confounders since conditioning or not-conditioning for those will induce bias in the estimates of both performance and choice effects. This bias is likely to be present in this research because students could anticipate decisions, inducing endogenous selection into the GPA variable and educational transitions. That is, those who decide to not continue into higher levels will underperform at schools and thus not reach the level of performance that could secure them a place into higher levels of education. In other words, this mean that choice effects can enter into performance effects. From a more technical point of view, the standard decomposition method, originally developed by Erikson et al. (2005) and extended by Buis (2012), is likely to be biased by confounding but also by scaling issues (Holm and Karlson 2011), making it unsuitable for quantifying the relative importance of choice effects. Methods that overcome the scaling issues are Imai, Keele, and Yamanoto (2010), Karlson, Holm, and Breen (2012), and Valeri and VanderWeele (2013). However, these methods at the moment cannot handle situations in which there are post-treatment confounders like anticipatory decisions. For tackling this issue, this chapter proposes and describes the G-estimation method (Daniels et al.

2011). Through real empirical examples I show that estimates of the relative importance of performance and choice effects change depending on the decomposition method and the variable specification adopted. For this reason, it is important to complement the analysis with sensitivity tests for assessing the role of potential confounding, and also to add measures of dispersion like confidence intervals when reporting the estimates of the relative importance of performance and choice effects.

In the second chapter, the focus was on estimating empirically the relative importance of performance and choice effects in transitions to the academic track of upper secondary education, adopting the methodology proposed in the first chapter, and on comparing these new results with those previously found in the literature. The analysis was conducted on a cohort of students born around 1995/6 in Sweden and England, two countries with comprehensive educational system and very similar transitions points. In particular, compared to previous research, the model of performance and choice effects was enriched by the inclusion of new measures of family background like an intact family indicator, immigration generational status, cognitive ability, and idealistic and realistic educational aspirations. The last three variables represent proxies for anticipatory decisions. In order to tackle the bias from anticipatory decisions (Erikson et al 2005; Jackson 2013), a G-estimation procedure was adopted (Daniel et al 2011). Results from this model indicated that IEO continued to decline for this younger cohort in both countries but performance and choice effects were equally important in England and performance effects remained relatively higher in Sweden. Importantly, previous estimates of the relative importance of performance and choice effects among birth cohorts born between 1960s, 1970s, and 1980s were showing a clear reduction in the total IEO and these changes were mostly accompanied by an increasing importance of performance effects over choice effects, suggesting that equalization primarily was

due to decreasing socioeconomic differences in educational choice. I confirmed that the declining pattern in the total IEO has continued for people born in mid-90s but also noticed that choice effects were considerably bigger than those reported in the past. This support Goldthorpe's (2016) that choice effects and relative risk/loss aversion (Erikson and Jonsson 1996; Breen and Goldthorpe 1997) remains an important mechanism for explaining educational inequality. Part of these discrepancies could be explained by the fact that previous studies did not accounted for anticipatory decisions. However, it is important to not exclude the possibility of some generational changes in the relative importance of performance and choice since also the models that do not accounted for anticipatory decisions were showing substantive choice effects although smaller than performance effects. Importantly, the models presented in this study allowed to further unpack the paths that conform performance and choice effects. The performance mechanism was decomposed into mediated paths through cognitive ability and GPA while the choice mechanism was decomposed into a mediated path through aspirations and the remained direct path between parental ISEI and the transition outcome. This direct path/effect could be interpreted as cleaner evidence of relative-risk/loss aversion mechanism (Erikson and Jonsson 1996; Breen and Goldthorpe 1997) since removes further variation due to aspirations. In terms of country differences, performance and choice effects were equally important in England, while performance effects were a bit higher than choice effects in Sweden - although it should be noted that the latter effect is still sizeable in magnitude. Overall, both countries still exhibit similar levels of IEO which could be attributed to their similar comprehensive education system

Finally, the third chapter set out to examine the impact of ability grouping on academic performance and choice effects in England and Sweden. As mentioned earlier these two countries both have a comprehensive system with similar transitions points. However, they followed

completely different paths when it comes to the organization of the instructional time within schools. England fully implemented the practice of setting students on the basis of their observed ability, Sweden went for a partial implementation, discouraging ability grouping but giving autonomy to schools on this decision. As a results of this, in England we observe that almost all the schools use ability grouping while in Sweden only half of them do so. Identifying the impact of ability grouping on performance and choice effects is a complex task due to the difficulty in separating the effects of sorting into schools, which depends of family background resources and affects the homogeneity of schools, from the true effect of allocating students into different ability groups. For overcoming this issue several novel identification strategies were employed. Equipped with better data than previous studies, it was possible to derive measures of school (or school class) homogeneity in cognitive ability and estimate the effect of this variable using school fixed effects models that account for the sorting into schools. Results from these models show that higher homogeneity neither affects student GPA nor transitions to upper secondary schools. Then it was directly assessed whether ability grouping in Math has an effect on Math grades and upper secondary choices. For this analysis an inverse of probability of treatment weighting with regression adjustment were used. Results suggest that ability grouping in Math has no effect on upper secondary transitions once conditioned on GPA but there are positive effects on Math grades for high ability groups in both England and Sweden, and detrimental effect for the low ability group in Sweden. However, placebo tests on English grades suggested that previous results were spurious. These analyses were complemented with a counterfactual approach for Sweden (i.e., the country were ability is implemented in half of the schools) where an imputation model assigned to students in mixed ability classes the (potential) ability group that 'would have been observed' if

their school had adapted that practice. Results from this counterfactual exercise show null effects of ability grouping on Math performance.

In terms of policy implications, it is worth noting two main findings from the second and third chapters. First, it was found that choice effects remain substantively large suggesting that it is still necessary to implement interventions targeted to students from disadvantaged background with good academic performances, as their conservative educational choices could stem from misinformation regarding the benefits of education. Alternatively, inequality of condition in the families of origin would need to be reduced in order for these students to make the same cost-benefit calculation as students from more fortunate circumstances. Taken together, these results indicate that policy interventions aimed at reducing choice effects are still important in comprehensive systems like the ones of England and Sweden. However, the presence of anticipatory decisions also suggest that it would be convenient to implement these interventions before students take their major exams rather than at the end of the school year when they just have to decide what to do next. For example, early counseling interventions could correct misinformation regarding the costs, opportunities, and benefits of higher education and motive them to perform better at school as well as helping them to make more efficient and informed choices.

The second important finding in this thesis for policy advice refers to the role of ability grouping in students' academic performance and educational choices. After using several identification strategies and robustness checks, we could not identify any effects of ability grouping on matters of efficiency (the overall performance) and inequality (gaps by socioeconomic background and ethnicity). This indicates that schools do not necessarily need to implement the practice of ability grouping and other alternatives for improving the learning environment should

be evaluated. The fact that ability grouping within the realm of comprehensive schooling did not affect neither grades nor choices in our data suggests that policies that focus on school organization within these systems may be less relevant than policies that aim at alleviating early inequalities, or policies that address students' situation at the time of important educational decisions.

Areas for future research

This thesis could be extended in several ways. First, an important aspect for this research is the possibility of tracking changes across birth cohorts. The analysis presented in these chapters were conducted on students born in 1995/96. It would be interesting to extend these analyses to older and younger birth cohorts. In the case of Sweden the school register data should allow to do that. For England it is also possible to use several cohorts studies like the British Cohort Study (BCS, people born in 1970), Youth Cohort Study (YCS, people born in the 1980s), the Longitudinal Survey of Young People in England (LYPSE, people born between 1989/90) and the Millennium Cohort Study (MCS people born in 2000). In this way, it would possible to track changes overtime and confirm whether there are generational changes in the composition of performance and choice effects.

Second, this thesis was mostly focused on vertical transitions to upper secondary education. Future studies should also consider the horizontal dimension of the education system. Students not only choose a specific level of education but also different subjects, tracks, and fields of study (Breen and Jonsson 2000; van de Werfhorst and Luijkx 2010). Even if there is a reduction of inequality in the transition to higher levels of education, students from well-off families might continue choosing more lucrative and prestigious fields of study than their counter parts from disadvantaged backgrounds (Lucas 2001). Moreover, it would be also important to measure loss aversion among students and parents and try to incorporate direct measures or proxies of this

concept (for direct tests and measures or loss aversion see for example, Van de Werfhorst and Hofstede 2007 and Holm and Jaeger 2008). At the moment, the majority of studies that quantified the relative importance of performance and choice effects were unable to do so because of data limitations and the discussion of this mechanism remains on the presence of a direct effect in mediation/decomposition model which could be also composition of other mechanisms.

Third, turning to ability grouping and class homogeneity, it could be analyzed whether ability grouping has any effects on the field of study as well. Lastly, taking advantage of the comparative aspect of CILS4EU, it would be fruitful to extend all the analysis presented in this thesis to other educational context like the one of Germany and the Netherland. This would enable to understand whether performance and choice effects are different for educational systems with early tracking and whether ability grouping might have different effects on both the efficiency in academic performance and choices but also inequality.

Appendix - Chapter 1

On the Quantification of Performance and Choice Effects in Educational Transitions

This appendix section reports summary statistic of the sample, the coefficients from OLS/LPM models shown in Figure 3, and additional results from different decomposition model (not included in the paper).

Section A.1.1	**Summary statistics and total and direct effects result tables**
Section A.1.2.	**Further note on Erikson decomposition and the counterfactual approach for mediation analysis**
Section A.1.3.	**Studying the variability of the PM in the presence of confounders through simulation**
Section A.1.4.	**Further note on the potential endogeneity of the PM**

Section A.1.1 Summary statistics and total and direct effects result tables

Table A.1.1: Summary statistics before and after sample selection

	Wave 1/2: Initial sample				**Wave 3: After attrition**				**Wave 3: After listwise deletion**			
	mean	sd	min	max	mean	sd	min	max	mean	sd	min	max
Academic transition	-	-	-	-	0.65	(0.48)	0	1	0.67	(0.47)	0	1
Std GPA	-0.00	(1.00)	-5	2	0.07	(0.98)	-5	2	0.11	(0.97)	-5	2
Parental ISEI: High	0.25	(0.43)	0	1	0.29	(0.45)	0	1	0.29	(0.45)	0	1
Parental ISEI: Mid-high	0.25	(0.43)	0	1	0.25	(0.44)	0	1	0.26	(0.44)	0	1
Parental ISEI: Mid-low	0.24	(0.43)	0	1	0.23	(0.42)	0	1	0.23	(0.42)	0	1
Parental ISEI: Low	0.26	(0.44)	0	1	0.23	(0.42)	0	1	0.21	(0.41)	0	1
Girls	0.50	(0.50)	0	1	0.54	(0.50)	0	1	0.54	(0.50)	0	1
Ethnicity: White	0.58	(0.49)	0	1	0.59	(0.49)	0	1	0.60	(0.49)	0	1
Ethnicity: Mixed	0.09	(0.29)	0	1	0.08	(0.28)	0	1	0.08	(0.27)	0	1
Ethnicity: Migrants	0.33	(0.47)	0	1	0.33	(0.47)	0	1	0.32	(0.47)	0	1
Parental education: Primary or below	0.07	(0.25)	0	1	0.06	(0.23)	0	1	0.06	(0.23)	0	1
Parental education: Lower sec.	0.26	(0.44)	0	1	0.23	(0.42)	0	1	0.23	(0.42)	0	1
Parental education: Upper sec.	0.40	(0.49)	0	1	0.38	(0.49)	0	1	0.37	(0.48)	0	1
Parental education: University	0.28	(0.45)	0	1	0.33	(0.47)	0	1	0.34	(0.47)	0	1
No intact family	0.35	(0.48)	0	1	0.31	(0.46)	0	1	0.30	(0.46)	0	1
Ability and aspirations (usually unobserved vars)												
Std CA	-0.00	(1.00)	-4	2	0.13	(0.93)	-4	2	0.16	(0.91)	-4	2
Realistic aspirations: No degree/DNK	0.22	(0.42)	0	1	0.20	(0.40)	0	1	0.15	(0.36)	0	1
Realistic aspirations: Below upper sec.	0.10	(0.30)	0	1	0.07	(0.26)	0	1	0.08	(0.27)	0	1
Realistic aspirations: Upper sec.	0.25	(0.43)	0	1	0.25	(0.43)	0	1	0.26	(0.44)	0	1
Realistic aspirations: University	0.43	(0.49)	0	1	0.48	(0.50)	0	1	0.51	(0.50)	0	1
Realistic aspirations: No degree/DNK	0.19	(0.39)	0	1	0.16	(0.37)	0	1	0.12	(0.33)	0	1
Realistic aspirations: Below upper sec.	0.05	(0.22)	0	1	0.03	(0.18)	0	1	0.04	(0.19)	0	1
Realistic aspirations: Upper sec.	0.15	(0.36)	0	1	0.13	(0.34)	0	1	0.13	(0.34)	0	1
Realistic aspirations: University	0.61	(0.49)	0	1	0.67	(0.47)	0	1	0.71	(0.45)	0	1
Sample specific characteristics												
School stratum: 0-10%	0.16	(0.37)	0	1	0.15	(0.35)	0	1	0.16	(0.36)	0	1
School stratum: 10-30%	0.32	(0.47)	0	1	0.32	(0.47)	0	1	0.31	(0.46)	0	1
School stratum: 30-60%	0.25	(0.43)	0	1	0.26	(0.44)	0	1	0.26	(0.44)	0	1
School stratum: 60-100%	0.23	(0.42)	0	1	0.23	(0.42)	0	1	0.22	(0.42)	0	1
School stratum: independent schools	0.04	(0.19)	0	1	0.04	(0.20)	0	1	0.05	(0.22)	0	1
Country: EN	0.45	(0.50)	0	1	0.45	(0.50)	0	1	0.49	(0.50)	0	1
Country: SW	0.55	(0.50)	0	1	0.55	(0.50)	0	1	0.51	(0.50)	0	1
Observations	**9884**				**5052**				**4076**			

Table A.1.2: GPA outcome – total and direct effects, OLS regression

	Survey characteristics		Family factors		Ability and aspirations	
	Coef.	se	Coef.	se	Coef.	se
Parental ISEI (*High*)						
High-mid	-0.284***	(0.036)	-0.211***	(0.035)	-0.116***	(0.030)
Mid-low	-0.575***	(0.048)	-0.398***	(0.048)	-0.201***	(0.040)
Low	-0.823***	(0.056)	-0.592***	(0.051)	-0.308***	(0.043)
Gender (*Boys)*						
Girls	0.091**	(0.039)	0.099**	(0.038)	0.074**	(0.030)
Ethnicity (*white/native*)						
Mixed	0.048	(0.058)	0.073	(0.055)	0.049	(0.047)
Migrants	0.012	(0.044)	0.010	(0.042)	-0.025	(0.034)
School stratum (*0%*)						
10-30% of imm.	0.086	(0.076)	0.067	(0.070)	0.036	(0.052)
30-60% of imm.	0.024	(0.067)	0.011	(0.062)	-0.050	(0.055)
60-100% of imm.	-0.009	(0.086)	-0.008	(0.080)	-0.030	(0.065)
Independent schools	0.625***	(0.165)	0.516***	(0.168)	0.216	(0.140)
Country (*England*)						
Sweden	0.220***	(0.059)	0.216***	(0.056)	0.071	(0.043)
Parental education (*Primary or less*)						
Lower secondary			0.211**	(0.094)	0.176**	(0.075)
Upper secondary			0.366***	(0.096)	0.249***	(0.075)
University			0.589***	(0.095)	0.350***	(0.076)
Intact family (*yes*)						
No			-0.211***	(0.033)	-0.143***	(0.028)
Std CA					0.348***	(0.017)
Idealistic asp. (*lower sec or less*)						
Upper secondary					0.112	(0.095)
University					0.346***	(0.089)
DNK					0.154	(0.096)
Realistic asp. (*lower sec or less*)						
Upper secondary					0.348***	(0.065)
University					0.741***	(0.071)
DNK					0.262***	(0.074)
N	4119		4119		4119	
R^2	0.145		0.179		0.417	
Log lik.	-5392.1		-5307.9		-4603.9	

Note: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.1.3: Academic track transition outcome - total and direct effects, LPM regression

	Survey characteristics		Family factors		Ability and aspirations	
	Coef.	se	Coef.	se	Coef.	se
Parental ISEI (*High*)						
High-mid	-0.098***	(0.018)	-0.076***	(0.018)	-0.026*	(0.016)
Mid-low	-0.206***	(0.021)	-0.153***	(0.022)	-0.053**	(0.020)
Low	-0.303***	(0.023)	-0.234***	(0.025)	-0.094***	(0.022)
Gender (*Boys)*						
Girls	0.065***	(0.018)	0.069***	(0.018)	0.045**	(0.017)
Ethnicity (*white/native*)						
Mixed	-0.019	(0.028)	-0.009	(0.027)	-0.026	(0.025)
Migrants	0.094***	(0.020)	0.090***	(0.019)	0.068***	(0.018)
School stratum (*0%*)						
10-30% of imm.	0.057*	(0.033)	0.050	(0.032)	0.032	(0.027)
30-60% of imm.	0.033	(0.032)	0.029	(0.031)	0.015	(0.031)
60-100% of imm.	-0.004	(0.037)	-0.005	(0.035)	-0.011	(0.029)
Independent schools	0.148***	(0.035)	0.114***	(0.035)	-0.027	(0.040)
Country (*England*)						
Sweden	-0.084***	(0.023)	-0.087***	(0.023)	-0.159***	(0.020)
Parental education (*Primary or less*)			0.000	(.)	0.000	(.)
Lower secondary			0.032	(0.036)	-0.003	(0.027)
Upper secondary			0.081**	(0.034)	0.004	(0.026)
University			0.147***	(0.034)	0.012	(0.027)
Intact family (*yes*)						
No			-0.084***	(0.017)	-0.035**	(0.015)
Std GPA					0.161***	(0.010)
Std CA					0.030***	(0.008)
Idealistic asp. (*lower sec or less*)						
Upper secondary					0.036	(0.040)
University					0.161***	(0.040)
DNK					0.083*	(0.046)
Realistic asp. (*lower sec or less*)						
Upper secondary					0.123***	(0.034)
University					0.162***	(0.035)
DNK					0.142***	(0.033)
N	4119		4119		4119	
R^2	0.083		0.098		0.277	
Log lik.	-2539.1		-2504.9		-2049.9	

Note: Robust standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

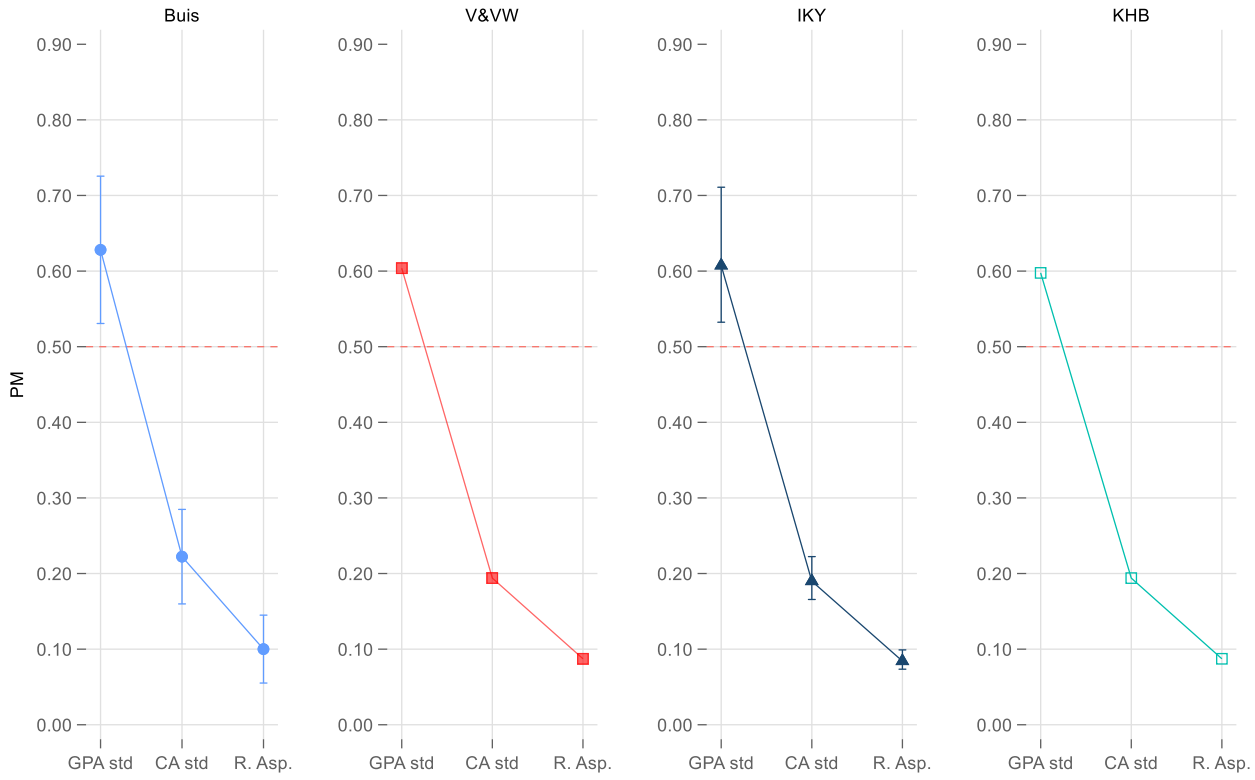


Figure A.1.1: Proportion mediated using only GPA, CA (cognitive ability), or Realistic aspirations as mediator

Note: Standard performance and choice models. The variables employed in these decomposition models were the transition to upper-secondary education outcome; the mediator (GPA, cognitive ability or Aspirations), Parental ISEI and survey characteristics (country and school stratum). Self-calculated 95% CI; IKY shows no symmetric CI (I still need to double-check this) and could not obtain CI for V&VW and KHB yet.

Section A.1.2. Further note on Erikson decomposition and the counterfactual approach for mediation analysis

Coming back to the simplest example of performance/choice effects, when the exposure is binary, this type of decomposition principally identifies four sub-groups of potential outcomes, depending on whether the treatment effect is channeled through the mediator or not. At the individual level, this implies that some effects are by definition non observable across units (Holland, 1986). The way of overcoming what is usually defined as the 'fundamental problem of causal inference' is by predicting or simulating the potential outcomes of interest under different paths decomposition. Table A.1.3 provides a simplified representation of the observable and unobservable potential outcomes in mediation analysis.

Table A.1.4: Example of data structure in mediation analysis using the Potential Outcome (PO) approach

ID	X	M(1)	M(0)	Y(1,M(1))	Y(0,M(1))	Y(1,M(0))	Y(0,M(0))
1	1	M_1		Y_1			
2	1	M_2		Y_2			
3	1	M_3		Y_3			
4	1	M_4		Y_4			
5	0		M_5				Y_5
6	0		M_6				Y_6
7	0		M_7				Y_7
n	0		M_n				Y_n

Note: Grey areas represent missing data points.

In this respect, it is also worth noting that in social stratification research, despite of the intention to differentiate their approach from the potential outcome framework, the Erikson et al. (2005) decomposition follows the same logic of Greenland and Robins (1992) and Pearl (2001) approaches. The table A.4 shows an example of the students' profiles under different potential outcomes, which are also identified for quantifying performance/choice effects in the Erikson et al. (2005) and Buis (2010) odds-ratio decomposition technique.

Table A.1.5: Profile equivalence between Erikson et al. 2005 approach and the PO framework

Profile	**Group**	**Paths**	**PO**
Low-class, low performance	Predicted (factual)	X(x), M(x)	Y(0,M(0))
High-class, low performance	Counterfactual	X(x'), M(x)	Y(1,M(0))
Low-class, high performance	Counterfactual	X(x), M(x')	Y(0,M(1))
High-class, high performance	Predicted (factual)	X(x'), M(x')	Y(1,M(1))

Broadly speaking, their approach consists in estimating a full nonlinear model (Y|X,M).[14] After that, it is predicted the log odds of Y=1 and computed the average predicted probability. Subsequently, these predicted probabilities are transformed into log-odds again. The difference between high-class versus low-class groups is the total effect. Then, it is simply repeated the same procedure but assuming that the class of reference has the performance distribution of the other class. In this way it is obtained different potential outcomes like those of table 2. For example, the group from high-class and high performance (predicted) versus the high-class group with the low-class performance (the counterfactual) expresses the indirect effect. On the other hand, the low-class group with low performance (predicted) versus the high-class group with the performance of the low-class group (the counterfactual) expresses the effect X while controlling for the distribution of M, that is, the direct effect. It becomes evident therefore that the comparison of different student profiles of Erikson et al. follows the same logic used for deriving potential outcomes in causal mediation. The main different between this technique and the causal mediation approach, however, lies in the estimation procedure and the transparency of the assumptions for identification of total, direct and indirect effects.

[14] Usually, the model assumes a logistic distribution.

Section A.1.3. Studying the variability of the PM in the presence of confounders through simulation

In order to isolate and study the problems of having exposure-outcome, exposure-mediator and mediator-outcome confounding and how both, confounding and collider bias, can affect the estimator of the proportion mediated, I set a simulation study using a linear structural equation model. Simulation is useful especially in cases in which we want the properties of an estimator to be tested across different (controlled) scenarios.

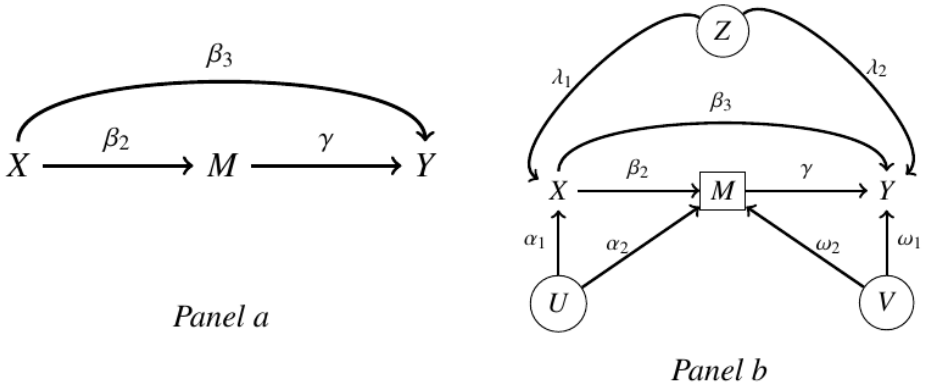


Figure A.1.2: Causal diagram with effect decomposition in the presence of confounders

Panel a in Figure A.1.2 shows the standard decomposition procedure disregarding potential confounding bias from unobserved/omitted variables while Panel b includes three types of confounders: U (exposure-mediator, X-M), V (mediator-outcome confounder, M-Y), and Z (exposure-outcome confounder, X-Y).

$$\textit{Panel a: } \int(X|\varepsilon_x); \int(M|X,\varepsilon_m);\ \int(Y|X,M,\varepsilon_y)$$

$$\textit{Panel b: } \int(X|\varepsilon_x); \int(M|X,U,\varepsilon_m);\ \int(Y|X,M,U,V,Z,\varepsilon_y)$$

When the variables are continous, the relationship between the variables of interest can be parametrically estimated in the following way:

$$\begin{aligned} M_{MX} &= i_1 + \beta_2 X + e_1 \\ Y_{YX|m} &= i_2 + \beta_3 X + \gamma M + hXM + e_2 \end{aligned} \qquad [1]$$

Where $\beta_2 X$ is the direct effect of X on M; $\beta_3 X$ is the direct effect of X on Y; and γM is the direct effect of M on Y. The model can be also augmented for further flexibility by including

the term hXM which is an interaction effect between exposure and mediator. In the absence of U and V confounders, these parameters can be used to calculate the proportion mediated as $PM = \gamma\beta_2/(\gamma\beta_2 + \beta_3)$. Table A.1.5 describes the effects and potential outcomes that can be identified from the model presented above. When there is no XM interaction or the effect of this intereaction is null (i.e., h = 0), the TNDE = PNDE, TNIE = PNIE and CDE(m) = PNDE. The full definition of the effect decomposition is provided in the Supplemental material.

Table A.1.6: Description of linear effect decomposition

Effect decomposition	**Potential outcome**	**Estimator**
TNIE	$E[Y_i(1, M_i(1)) - Y_i(1, M_i(0))]$	$\beta_2\gamma + h\beta_2$
PNIE	$E[Y_i(0, M_i(1)) - Y_i(0, M_i(0))]$	$\beta_2\gamma$
TNDE	$E[Y_i(1, M_i(1)) - Y_i(0, M_i(1))$	$\beta_3 + hi_1 + h\beta_2$
PNDE	$E[Y_i(1, M_i(0)) - Y_i(0, M_i(0))]$	$\beta_3 + hi_1$
CDE(m)	$E[Y_i(1, m) - Y_i(0, m)]$	$\beta_3 + hm$
TE	$E[Y_i(1, M_i(1)) - Y_i(0, M_i(0))]$	$\beta_3 + hi_1 + \beta_2\,\gamma + h\beta_2$

Assuming no interaction, we can see that in the presence of confounding bias (Panel b of Figure D.1) our estimates are represented by:

Total effect

$$\hat{\beta}_{3_{YX}} = \beta_3 + \beta_2\gamma + \lambda_1\lambda_2 + \alpha_1\alpha_2 + \omega_2\omega_1 \qquad [2]$$

Direct effect of X on Y

$$\hat{\beta}_{3_{YX|m}} = \beta_3 + \beta_2\gamma + \lambda_1\lambda_2 - \frac{\beta_2 + \alpha_1\alpha_2\omega_2\omega_1}{1 - {\beta_2}^2} \qquad [3]$$

Direct effect of X on M

$$\hat{\beta}_{2_{MX}} = \beta_2 + \alpha_1\alpha_2 \qquad [4]$$

Direct effect of M on Y

$$\hat{\gamma}_{YM|x} = \gamma + \omega_2\omega_1 \qquad [5]$$

The estimated effects in expression [2, 4 and 5] are biased and the size of this bias depends on the correlation with the unobserved/omitted variables. Moreover, as shown by Breen (2018), expression [3] is basically a combination of confounding and collider bias. When we control for M different non-causal path are opened (see U and V). Depending on the magnitude of these different sources of bias, the sign of the estimated direct effect of X on Y will change.

This brief description already shows how difficult it is to obtain the true effects when there are important unobservable or omitted variables in the model. These issues are further explored in the simulation study for the case of the proportion mediated. For this purpose, the effect of social class was kept constant and relatively high across all the simulation scenarios. The only source of variation is in the effect of the mediator and U and V confounders which ranges between 0.04-0.20 for cases of weak correlations and 0.40-0.70 for strong correlations. I excluded from these simulations the presence of intermediate confounders since this type of bias cannot be ruled out by simply controlling or adjusting in the regression model.

The Figure A.1.3 and A.1.4 represent the absolute (Panel a) and relative (Panel b) bias across different extreme scenarios of potential Z, U and V confounding bias and a combination of them. More precisely, while Panel a shows the direction of the bias, Panel b describes how big this bias can be in relative terms under specific circumstances. In the case of Panel a, when the PM surpasses the dashed line (.50) we have a relatively large performance effect. Contrariwise, when the PM remains below .50, we have a relatively low performance effect (i.e., a large choice effects). On the other hand, if the PM estimates go beyond one or below zero, we will observe the case of an inconsistent mediation. In the first two scenarios the true proportion mediated is larger than the unmediated while in the last two, the proportion unmediated is larger than the proportion mediated. This corresponds to cases in which the performance effects are larger than choice effects and the contrary case, when choice effects are larger than performance effects, respectively.

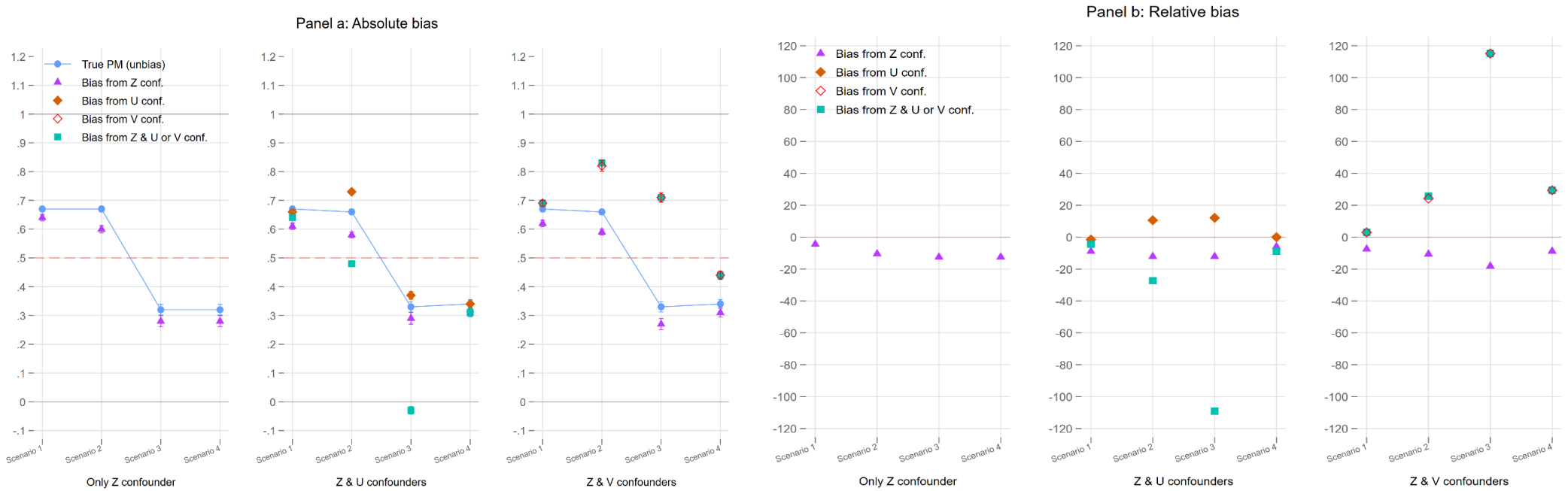


Figure A.1.3: Absolute and relative bias in the proportion mediated (PM) across extreme endogeneity scenarios (weak/strong confounding)
Note:
Scenario 1: Strong indirect effect; Weak confounding; true PM = large performance effect
Scenario 2: Strong indirect effect; Strong confounding; true PM = large performance effect
Scenario 3: Weak indirect effect; Strong confounding; true PM = small performance effect
Scenario 4: Weak indirect effect; Weak confounding; true PM = small performance effect
Solid black line in Panel a represents the limits of the PM (min 0; max 1). Outside that range PM estimate becomes inconsistent.
Dash black line in Panel b = 0.50 which represents the mid-point in the scale.
Bootstrapped standard errors (1,000 replications); confidence intervals at 99% level in Panel a.
Sample size = 10,000 observations.

Under these hypothetical scenarios we can see that the proportion mediated is highly sensitive to confounding bias, and depending on the type of bias, the proportion mediated can move to higher or lower values. Figure A.1.3 displays the results in a scenario in which exposure-outcome (Z), exposure-mediator (U) or mediator-outcome (V) confounder are present. Assuming that there is only confounding from Z, the PM is underestimated (violet triangle). This is mainly due to the fact that we overestimate the total effects (the denominator of the PM). Further, if we include U confounder but manage to control for Z, the proportion mediated goes up (orange diamond). This is because we overestimate the indirect effects (the numerator of the PM). The bias change direction again if we introduce confounding from both Z and U (i.e., we don't control for any confounders). In this case, we underestimate the PM (green squares). This is mainly because we induce bias in the total effect (effects of X are larger) and also collider bias (but confounding prevails). Similarly, we can repeat the exercise but this time with exposure-outcome (Z) and mediator-outcome (V) confounders. If we control for Z but not for V, we overestimate the indirect effects and thus the PM is larger. A similar bias is also observed when we do not control for both Z and V. In this case, we have collider bias (indirect effects are inflated; direct effects of X are attenuated) and this prevails over confounding.

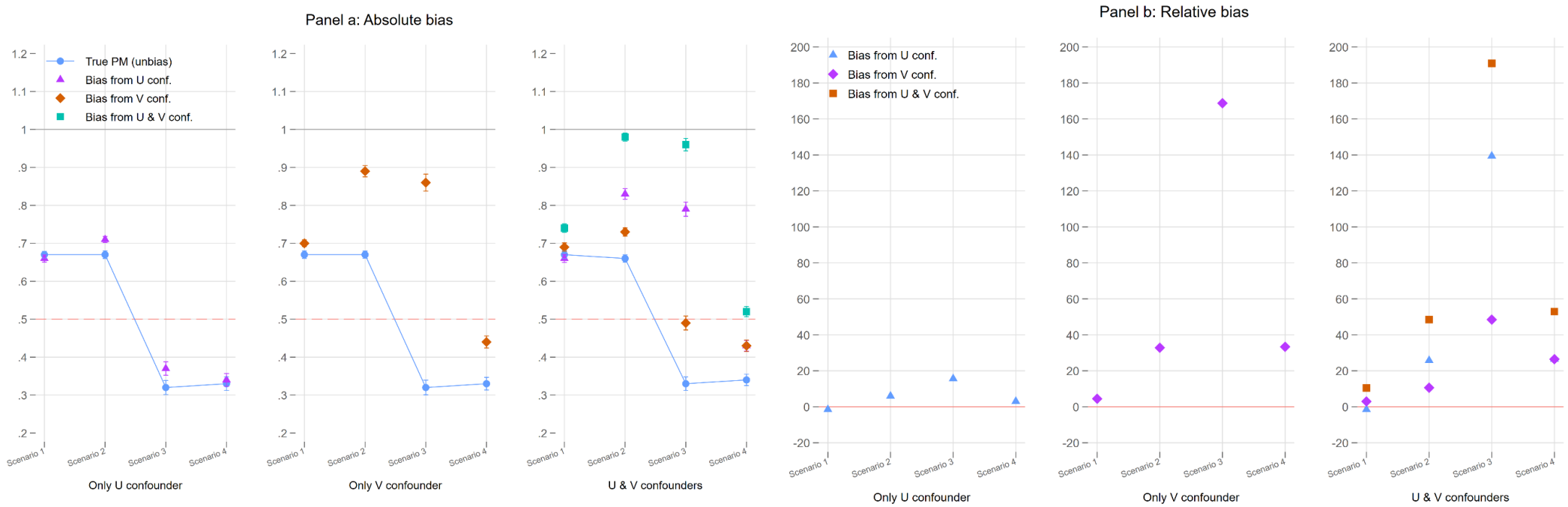


Figure A.1 4: Absolute and relative bias in the proportion mediated (PM) across extreme endogeneity scenarios (weak/strong confounding)
Note:
Scenario 1: Strong indirect effect; Weak confounding; true PM = large performance effects
Scenario 2: Strong indirect effect; Strong confounding; true PM = large performance effects
Scenario 3: Weak indirect effect; Strong confounding; true PM = small performance effects
Scenario 4: Weak indirect effect; Weak confounding; true PM = small performance effects
Solid black line in Panel a represents the limits of the PM (min 0; max 1). Outside that range the estimate becomes inconsistent.
Dash black line in Panel b = 0.50 which represents the mid-point in the scale.
Bootstrapped standard errors (1000 simulations); confidence intervals at 99% level in Panel a.
Sample size = 10.000 observations.

Furthermore, Figure A.1.4 shows that if we only have exposure-mediator or mediator-outcome confounders, we are likely to overestimate the indirect effects and hence PM is always larger than the true one (violet triangle and orange diamond). If neither U nor V are controlled, the bias is magnified (green square). Even when there is a strong indirect effect (scenario 2), the proportion mediated is largely overestimated, compromising the interpretation of the results. These simulation examples show, therefore, how difficult it is to obtain the true proportion mediated in the presence of unobserved/omitted variables. The proportion mediated can be either underestimated or overestimated depending on whether there are omitted/unobserved exposure-outcome, exposure-mediator or mediator-outcome confounders. In other words, this exercise shows that the PM is a poor estimator of the relative importance of the mediated/unmediated effect when there are multiple sources of confounding affecting at the same time total, direct, and indirect effects. For example, accounting for factors related to family complexity (exposure-outcome and exposure-mediator) or school characteristics and peer effects (mediator-outcome) can be crucial for reducing the risk of false positives (e.g., claiming that one effect is larger than the other).

Section A.1.4. Further note on the potential endogeneity of the PM

There are three important points two stress before discussing potential endogeneity scenarios in effects decomposition and its implications for identifying and quantifying choice and performance effects. First, indirect and directs effects are symmetrical, therefore, the bias in one estimate will compromise the other. Interestingly, even if indirect and direct effects are biased, we might still be able to obtain an unbiased estimate of the total effect if there is no measurement error in the exposure variable and potential exposure-outcome confounders are accounted for. Second, independently of the estimator adopted, the strategies are mostly based on a two steps estimation procedure. That is, differences in coefficients (e.g. estimating full and reduced models for the outcome) or product of coefficients strategies (e.g. estimating full models for the outcome and mediator). In this regard, if there are confounders in the analysis, these two strategies are likely to yield equally biased estimates of the proportion mediated. Third, when the bias from omitted variables is very large or the estimation procedure was incorrect, we can end up with an inconsistent proportion mediated. That is, values that goes

beyond one or below zero. For instance, inconsistent mediation was not uncommon in standard linear mediation due to the violation of linearity and additivity assumptions.[15]

Once clarified these points, the following paragraphs aims at setting the basis for answering the following question:

What should happen to overestimate/underestimate choice effects? (and vice versa underestimate/overestimate performance effects)

Overall, we can draw three general settings in which bias can be introduced into the analysis and predict different results within each one. The first one, in which estimates are biased and the proportion mediated is unidentified, is based on standard confounding bias and measurement error; the second is instead based on intermediate confounders and leads into the same expectation as the first one while the last one is a combination of the previous two settings but the proportion mediated is actually identified.

1.a) Confounding bias from X-Y, X-M and M-Y confounders. Estimates are biased, and the proportion mediated remains un-identified; overestimation of choice effects

If we have a positive correlation between the exposure, outcome and X-Y confounder in our decomposition analysis (i.e. the most classic case of spurious correlation discussed in the literature), the total effect will be upwardly biased. As a consequence, the denominator in the proportion mediated is going to be much larger than the numerator, producing a small estimate of the proportion of social class effect on the educational transition that is due to performance effects. This could also happen in the present of other types of bias such as X-M or M-Y confounding. As far as the total effect estimate is upwardly biased, and more biased than the estimates of the direct/indirect, the proportion mediated is going to be smaller than the unmediated proportion.

On the other hand, if the total effect is not biased or if this bias is relatively low, but there are X-M or M-Y confounders and the correlation between the exposure variable, mediator, outcome and the confounder is negative, the indirect effect is going to be downwardly biased, resulting in a smaller numerator and thus a larger proportion of the social class effect due to choice effects.

1.b) Misclassification/measurement error of exposure and mediator variables; overestimation of choice effects

[15] There is also a possibility in mediation analysis that one effect is negative while the other positive. However, this rather unlikely in choice and performance decomposition.

We can end up in a similar situation if we do not suffice to control for concomitant factors in the family background or if we measure these variables with error. For example, if parental education is not properly measured, the effect of social class is likely to be bigger because it will pick up part of the variation due to education. Following the same line of reasoning, we can also overestimate class direct effects if we use academic performance variables, or proxy variables, that were poorly measured. That is, the indirect effect will be attenuated, increasing in this way the relative importance of the unmediated proportion.

1.c) Confounding bias from X-Y, X-M and M-Y confounders. Estimates are biased, and the proportion mediated remains un-identified; underestimation of choice effects

Obviously, the opposite scenarios of those described above can be hypothesized as well in order to predict a larger bias in the proportion mediated and thus an 'underestimation' of choice effects. Namely, a downward bias in the total or direct effect of X; an upward bias in the indirect effects or a disproportional combination of these two biases.

Most importantly, when X-M and M-Y are positively correlated with the observed variables the indirect effects increase. Contrarily, if X-M and M-Y are negatively correlated the indirect effect will decrease and the direct effects will prevail. More complex situations can be hypothesized in the present of X-Y together with X-M or M-Y confounders. In particular, if we manage to remove the bias from X-Y but still have a strong M-Y confounding bias, the direct effect of X on Y will be reduced while the direct effect of M on Y increased. This is the case in which the collider bias, if it is fairly strong, attenuates the direct effect of X (since confounding bias from X-Y is zero after controlling for it) and confounding bias increases the direct effect of M on Y. Both biases in the estimates of direct effects of X and M contribute to inflate the indirect effects. Also, the substantive interpretation of these two coefficients could be seriously misleading. However, it is still possible that the proportion mediated, although biased, might not be that far from the true one.

A similar scenario could happen if we remove the bias from X-Y but the bias from of X-M confounder is not averted. In this case, both numerator and denominator will be inflated but the bias in the indirect effect will prevail, increasing in this way the proportion mediated.

1.d) Misclassification/measurement error in the exposure variables; underestimation of choice effects

Moreover, we can also underestimate the effect of social class if we do not include all the relevant variables of social origins. For example, it has been shown by Mood (2017), using

register data for Sweden, that income and class have different and separate effect and the exclusion of income leads into an underestimation of the social class effect. Similarly, we can also underestimate the effect of class by over-controlling for concomitant and highly correlated variables. For example, Bihagen and Lambert (2018) has recently shown that is rather difficult to empirically capture social status using measures based mainly on occupation information. Variables like these are instead likely to bring multicollinearity issues and create artificial differences in social origins effects mainly due to different functional forms and measurement error. If we control for social status, we especially risk to underestimate the effect of class on the educational transition, producing a smaller denominator in the proportion mediated. This also known as over-controlling. A few examples of this situation in social stratification research have been already shown by Gratz (2019). A pure theoretical selection of variables without empirical or data considerations might bring more issues than a better understanding of the underlying mechanisms.

2) *Confounding bias from intermediate confounders. Choice and performance effects are unidentified in both directions*

The cases presented above, however, are mainly concerned with the present of X-Y, X-M and M-Y confounders. Our estimates can be also affected by intermediate confounders. In this case, if the correlation between the variables of interest and the intermediate confounder is positive, we will likely inflate the indirect effect generating larger performance effects. Contrarily, if the correlation is negative, we will observe smaller performance effects. This is a more complicated case and, as already discussed in section 2, and very likely to happen in decomposition analysis of educational stratification. Therefore, it always convenient to consider and include controls for potential selection into the mediator. This will obviously increase the number of paths to be estimated in the model since new indirect effects from the exposure variable to mediator will be presented. Also, for estimating these multiple paths it will be required more refined assumptions and complicate models (see section 3). Finally, if standard confounding bias is also present and not removed, the total bias will be exacerbated and none of the effects of interest will be identified.

3) *Estimates are biased following 1 and 2 settings, but proportion mediated is identified*

Finally, although rather difficult, there is a third possibility in which biased estimates in the models do not affect the estimate of the proportion mediated. That is, if the bias is proportionally distributed across the three effects of interest (i.e. the total, direct and indirect

effects) and have the same sign, numerator and denominator will both proportionally increase/decrease and therefore no biased would be observed in the proportion mediated. That is, in relative terms, we might still be able to draw useful information from the analysis. However, it's important to stress that any interpretation of total or direct effects from these models might be misleading due to the present of bias in their estimates.

Appendix - Chapter 2

Decomposing Inequality of Educational Opportunity: A new account of performance, choice and anticipatory decision effects in transitions to upper secondary education in England and Sweden

This appendix section reports summary statistic of the sample and additional results not displayed in the main text from different decomposition model, as well as further robustness checks.

Section A.2.1.	**Sample description**
Section A.2.2.	**Erikson et al./Buis decomposition results**
Section A.2.3.	**LPM and School Fixed effect models**
Section A.2.4.	**Analysis using multiple imputation on England survey data**
Section A.2.5.	**LPM and School Fixed effect models, ISEI vs ESeC comparison**
Section A.2.6.	**Additional sensitivity analysis based on propensity score matching and Mantel and Haenszel bounds**
Section A.2.7	**Description of the decomposition model through causal diagrams**

Section A.2.1. Sample description

Table A.2.1: Summary statistics – Survey data

	(1) Initial sample						(2) After attrition						(3) After listwise deletion of missingness					
	England			Swedeb			England			Sweden			England			Sweden		
	mean	min	max	mean	min	max	mean	min	max	mean	min	max	mean	min	max	mean	min	max
Parental High-ISEI	0.25	0	1	0.25	0	1	0.29	0	1	0.29	0	1	0.29	0	1	0.29	0	1
Parental Mid-high ISEI	0.25	0	1	0.25	0	1	0.26	0	1	0.25	0	1	0.27	0	1	0.26	0	1
Parental Mid-low-ISEI	0.24	0	1	0.25	0	1	0.23	0	1	0.23	0	1	0.23	0	1	0.23	0	1
Parental Low ISEI	0.27	0	1	0.26	0	1	0.22	0	1	0.24	0	1	0.21	0	1	0.22	0	1
Primary or below	0.07	0	1	0.06	0	1	0.06	0	1	0.06	0	1	0.05	0	1	0.06	0	1
Lower sec.	0.29	0	1	0.24	0	1	0.26	0	1	0.20	0	1	0.27	0	1	0.19	0	1
Upper sec.	0.28	0	1	0.48	0	1	0.28	0	1	0.47	0	1	0.28	0	1	0.46	0	1
University	0.36	0	1	0.22	0	1	0.39	0	1	0.28	0	1	0.40	0	1	0.29	0	1
Year of birth	1995.65	1993	1997	1995.96	1994	1998	1995.66	1994	1997	1995.97	1994	1997	1995.66	1994	1997	1995.99	1994	1997
Month of birth	6.50	1	12	6.26	1	12	6.53	1	12	6.28	1	12	6.54	1	12	6.24	1	12
Girls	0.49	0	1	0.51	0	1	0.51	0	1	0.57	0	1	0.51	0	1	0.57	0	1
White/natives	0.63	0	1	0.53	0	1	0.64	0	1	0.55	0	1	0.63	0	1	0.56	0	1
Mixed ethnic background	0.07	0	1	0.10	0	1	0.06	0	1	0.10	0	1	0.06	0	1	0.11	0	1
Migrants	0.30	0	1	0.36	0	1	0.31	0	1	0.35	0	1	0.31	0	1	0.34	0	1
Non-intact families	0.37	0	1	0.34	0	1	0.31	0	1	0.31	0	1	0.31	0	1	0.29	0	1
Sch. stratum 0-10%	0.17	0	1	0.16	0	1	0.14	0	1	0.16	0	1	0.14	0	1	0.17	0	1
Sch. stratum 10-30%	0.32	0	1	0.33	0	1	0.33	0	1	0.32	0	1	0.31	0	1	0.31	0	1
Sch. stratum 30-60%	0.22	0	1	0.27	0	1	0.23	0	1	0.28	0	1	0.24	0	1	0.29	0	1
Sch. Stratum 60-100%	0.21	0	1	0.24	0	1	0.22	0	1	0.24	0	1	0.21	0	1	0.23	0	1
Independent schools	0.08	0	1	0.00	0	0	0.09	0	1	0.00	0	0	0.10	0	1	0.00	0	0
Std CA	0.00	-4	2	-0.00	-4	2	0.14	-4	2	0.12	-4	2	0.16	-4	2	0.16	-4	2
Rea. Asp. Upper sec. or less	0.45	0	1	0.27	0	1	0.40	0	1	0.25	0	1	0.41	0	1	0.26	0	1
Rea. Asp. University	0.38	0	1	0.47	0	1	0.43	0	1	0.52	0	1	0.45	0	1	0.57	0	1
Rea. Asp. N/A & DNK	0.17	0	1	0.26	0	1	0.16	0	1	0.23	0	1	0.14	0	1	0.17	0	1
Rea. Asp. Upper sec. or less	0.27	0	1	0.15	0	1	0.21	0	1	0.13	0	1	0.21	0	1	0.12	0	1
Rea. Asp. University	0.62	0	1	0.60	0	1	0.68	0	1	0.67	0	1	0.70	0	1	0.72	0	1
Rea. Asp. N/A & DNK	0.12	0	1	0.25	0	1	0.11	0	1	0.21	0	1	0.08	0	1	0.16	0	1
Observations	4436			5448			2284			4,803			2095			4,581		

Note: Panel (3) corresponds to the analytical sample.

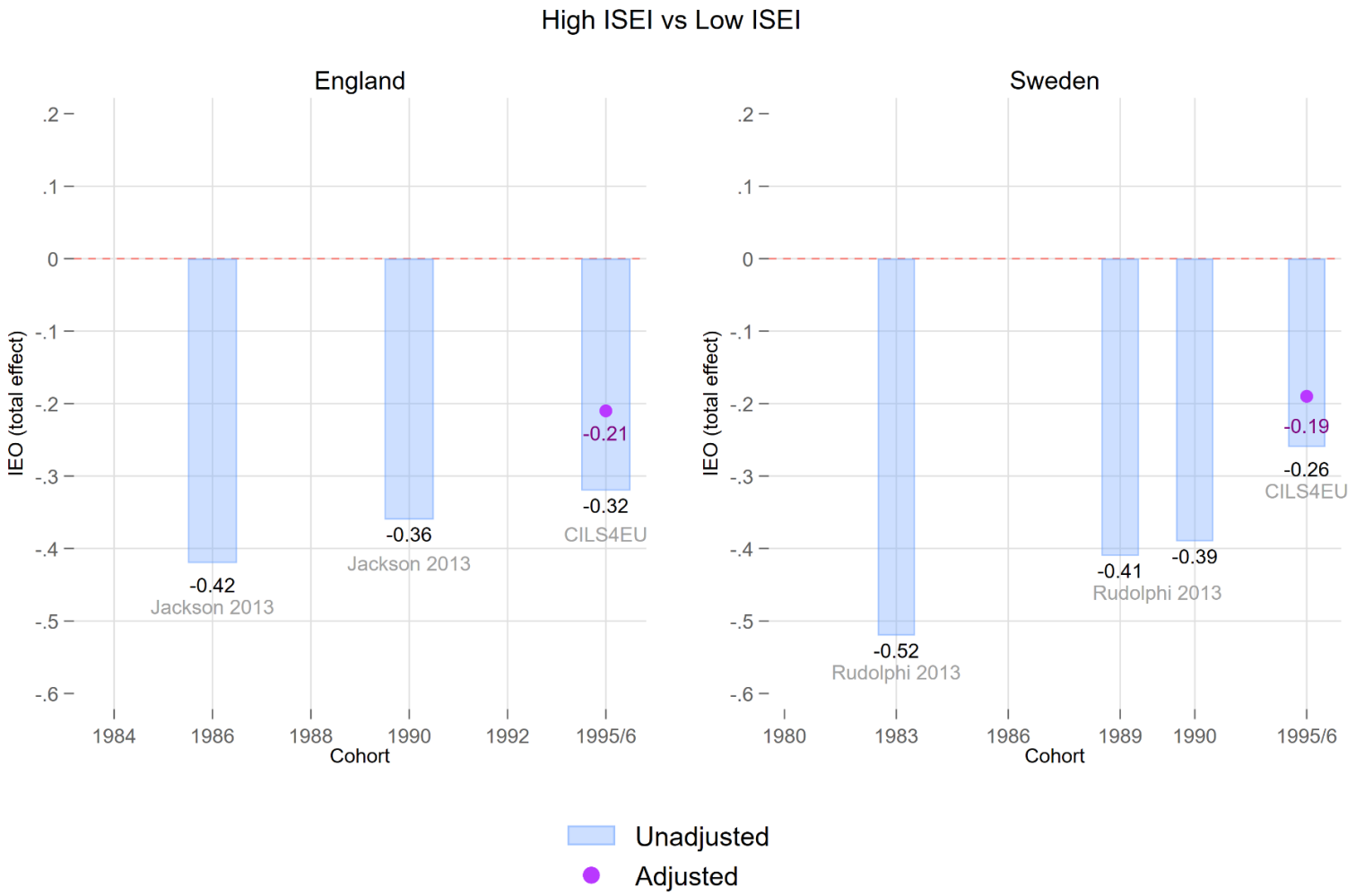


Figure A.2.1: IEO (socioeconomic gaps in upper secondary transitions) across cohorts
Note: IEO estimates were collected from Jackson (2013) and Rudolphi (2013) book chapters.

Section A.2.2. Erikson et al./Buis decomposition results

Table A.2.2: Erikson et al./Buis decomposition results

GPA as mediator	**Erikson et al./Buis method**					
	England			**Sweden**		
	TCE	NIE	PM	TCE	NIE	PM
(1)	0.173	0.296	0.69	0.180	0.267	0.78
	(0.029)	(0.028)	(0.06)	(0.017)	(0.016)	(0.037)
(2)	0.154	0.286	0.67	0.141	0.253	0.69
	(0.028)	(0.029)	(0.06)	(0.014)	(0.016)	(0.032)
(3)	0.172	0.290	0.70	0.146	0.256	0.70
	(0.034)	(0.029)	(0.07)	(0.016)	(0.017)	(0.035)
CA as mediator	**Erikson et al./Buis method**					
	England			**Sweden (Y and M from register)**		
	TCE	NIE	PM	TCE	NIE	PM
(1)	0.181	0.675	0.23	0.180	0.654	0.24
	(0.031)	(0.043)	(0.04)	(0.017)	(0.027)	(0.024)
(2)	0.155	0.647	0.23	0.139	0.618	0.23
	(0.028)	(0.044)	(0.04)	(0.0153)	(0.0270)	(0.021)
(3)	0.226	0.650	0.29	0.147	0.639	0.23
	(0.048)	(0.045)	(0.05)	(0.017)	(0.028)	(0.023)

Note: Bootstrapped standard errors; 1000 replications. High-ISEI vs Low-ISEI, England N = 1042; Sweden N = 2514 (register). Specification (1) = No additional controls; Specification (2) = Survey characteristics; Specification (3) = Survey characteristics; Individual demographics and family background.

Section A.2.3. LPM and School Fixed effect models

Table A.2.3: Total and direct effects – England

	Transition										Performance (GPA)			
	(1) LPM		(2) LPM		(3) LPM		(4) Sch FE		(5) Sch FE		(6) OLS		(7) Sch FE on GPA	
	TE	Std err	TE	Std err	DE	Std err	TE	Std err	DE	Std err	DE	Std err	DE	Std err
Par. ISEI (ref. high)														
High-mid ISEI	-0.080***	(0.023)	-0.061**	(0.024)	-0.005	(0.021)	-0.036	(0.024)	-0.003	(0.021)	-0.131***	(0.042)	-0.072*	(0.039)
Mid-low ISEI	-0.195***	(0.030)	-0.145***	(0.032)	-0.045*	(0.026)	-0.096***	(0.031)	-0.035	(0.026)	-0.147**	(0.062)	-0.083	(0.056)
Low ISEI	-0.314***	(0.033)	-0.244***	(0.033)	-0.102***	(0.027)	-0.180***	(0.032)	-0.082***	(0.028)	-0.293***	(0.065)	-0.218***	(0.055)
Gender (ref. boys)														
Girls	0.071**	(0.028)	0.076***	(0.028)	0.066**	(0.028)	0.047**	(0.021)	0.040**	(0.019)	0.028	(0.047)	0.029	(0.039)
Ethnicity (ref. Natives)														
Mixed	-0.020	(0.048)	-0.005	(0.046)	-0.021	(0.044)	-0.028	(0.044)	-0.039	(0.041)	0.084	(0.078)	0.096	(0.081)
Migrants	0.105***	(0.028)	0.087***	(0.028)	0.059**	(0.024)	0.092***	(0.025)	0.079***	(0.021)	0.035	(0.041)	-0.003	(0.043)
School stratum (ref. 0-10%)														
10-30%	0.105**	(0.050)	0.098**	(0.047)	0.044	(0.035)	-	-	-	-	0.110	(0.086)	-	-
30-60%	0.067	(0.053)	0.059	(0.051)	0.040	(0.053)	-	-	-	-	-0.064	(0.083)	-	-
60-100%	-0.005	(0.056)	-0.006	(0.052)	-0.008	(0.035)	-	-	-	-	-0.046	(0.097)	-	-
Independent schools	0.171***	(0.040)	0.138***	(0.041)	-0.014	(0.043)	-	-	-	-	0.268*	(0.148)	-	-
Parental education (ref. Primary or below)														
Lower sec.			0.079	(0.054)	0.029	(0.037)	0.059	(0.052)	0.010	(0.037)	0.191**	(0.092)	0.177*	(0.093)
Upper sec.			0.120**	(0.051)	0.033	(0.035)	0.082*	(0.049)	0.013	(0.035)	0.245**	(0.095)	0.213**	(0.096)
University			0.168***	(0.048)	0.012	(0.035)	0.113**	(0.046)	-0.010	(0.034)	0.376***	(0.091)	0.309***	(0.093)
Intact family (ref. Yes)														
No			-0.108***	(0.021)	-0.052***	(0.017)	-0.069***	(0.022)	-0.033*	(0.017)	-0.174***	(0.038)	-0.124***	(0.034)
Std GPA					0.189***	(0.016)			0.201***	(0.012)	-	-	-	-
Std CA					0.032***	(0.010)			0.031***	(0.011)	0.379***	(0.023)	0.342***	(0.022)
Idealistic aspirations (ref. Upper sec.)														
University					0.145***	(0.025)			0.112***	(0.025)	0.313***	(0.047)	0.281***	(0.046)
Don't know					0.035	(0.041)			0.013	(0.039)	0.103	(0.082)	0.119	(0.082)
Realistic aspirations (ref. Upper sec.)														
University					0.012	(0.021)			0.019	(0.022)	0.459***	(0.046)	0.398***	(0.044)
Don't know					0.051*	(0.030)			0.058**	(0.028)	-0.083	(0.066)	-0.099	(0.060)
N	2095		2095		2095		2095		2095		2095		2095	
R^2	0.103		0.123		0.341		0.052		0.279		0.463		0.329	
Log lik.	-1171.659		-1147.831		-848.018		-965.676		-679.345		-2304.080		-2105.992	

Note: TE = Total Effect; DE = Direct Effect; standard errors adjusted at school level in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01; models also include controls for month/year of birth

Table A.2.4: Total and direct effects – Sweden

	Transition										Performance (GPA)			
	(1) LPM		(2) LPM		(3) LPM		(4) Sch FE		(5) Sch FE		(6) OLS		(7) Sch FE	
	TE	Std err	TE	Std err	DE	Std err	TE	Std err	DE	Std err	DE	Std err	DE	Std err
Par. ISEI (ref. high)														
High-mid ISEI	-0.106***	(0.026)	-0.080***	(0.025)	-0.033	(0.024)	-0.071***	(0.026)	-0.028	(0.025)	-0.086**	(0.042)	-0.053	(0.045)
Mid-low ISEI	-0.211***	(0.030)	-0.151***	(0.032)	-0.056*	(0.031)	-0.126***	(0.032)	-0.041	(0.031)	-0.260***	(0.050)	-0.236***	(0.055)
Low ISEI	-0.284***	(0.034)	-0.210***	(0.038)	-0.076**	(0.034)	-0.190***	(0.036)	-0.061*	(0.033)	-0.316***	(0.058)	-0.293***	(0.061)
Gender (ref. boys)														
Girls	0.065***	(0.022)	0.067***	(0.022)	0.027	(0.020)	0.059***	(0.023)	0.017	(0.022)	0.126***	(0.037)	0.140***	(0.040)
Ethnicity (ref. Natives)														
Mixed	-0.016	(0.033)	-0.004	(0.032)	-0.019	(0.030)	-0.010	(0.034)	-0.021	(0.033)	0.041	(0.060)	0.018	(0.057)
Migrants	0.076**	(0.029)	0.090***	(0.030)	0.084***	(0.029)	0.092***	(0.031)	0.085***	(0.031)	-0.098*	(0.055)	-0.109**	(0.052)
School stratum (ref. 0-10%)														
10-30%	0.016	(0.042)	0.013	(0.041)	0.030	(0.040)	-	-	-	-	-0.028	(0.062)	-	-
30-60%	0.008	(0.041)	0.008	(0.039)	0.000	(0.039)	-	-	-	-	-0.059	(0.077)	-	-
60-100%	-0.001	(0.049)	0.001	(0.047)	-0.009	(0.044)	-	-	-	-	-0.009	(0.090)	-	-
Parental education (ref. Primary or below)														
Lower sec.			-0.014	(0.047)	-0.042	(0.041)	-0.005	(0.048)	-0.037	(0.042)	0.169	(0.116)	0.166	(0.109)
Upper sec.			0.049	(0.046)	-0.017	(0.042)	0.043	(0.047)	-0.017	(0.044)	0.253**	(0.115)	0.225**	(0.110)
University			0.142***	(0.049)	0.033	(0.043)	0.132***	(0.050)	0.029	(0.045)	0.317***	(0.119)	0.293**	(0.115)
Intact family (ref. Yes)														
No			-0.062**	(0.026)	-0.020	(0.024)	-0.057**	(0.028)	-0.017	(0.026)	-0.125***	(0.041)	-0.104***	(0.039)
Std GPA					0.147***	(0.013)			0.156***	(0.013)	-	-	-	-
Std CA					0.035***	(0.012)			0.030**	(0.012)	0.335***	(0.023)	0.338***	(0.023)
Idealistic aspirations (ref. Upper sec.)														
University					0.158***	(0.037)			0.135***	(0.038)	0.334***	(0.064)	0.303***	(0.064)
Don't know					0.095**	(0.044)			0.082*	(0.043)	0.100	(0.080)	0.078	(0.080)
Realistic aspirations (ref. Upper sec.)														
University					0.088***	(0.028)			0.084***	(0.028)	0.396***	(0.051)	0.405***	(0.049)
Don't know					0.051*	(0.031)			0.049	(0.031)	0.047	(0.064)	0.051	(0.065)
N	4,581		4,581		4,581		4,581		4,581		4,581		4,581	
R^2	0.051		0.065		0.208		0.050		0.191		0.344		0.329	
Log lik.	-1373.214		-1357.754		-1186.603		-1271.660		-1106.190		-2362.542		-2228.676	

Note: TE = Total Effect; DE = Direct Effect; standard errors adjusted at school level in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01; models also include controls for month/year of birth

Section A.2.4. Analysis using multiple imputation on England survey data

Table A.2.5: Missingness across key variables

	England				
Variable	Obs=.	Obs<.	values	Min	Max
Upper sec. transition	2,152	2,284	2	0	1
Parental ISEI	106	4,330	4	1	4
GPA std	2,191	2,245	16	-4.6637	1.768991
CA std	236	4,200	27	-4.46089	1.792695
Parental education	871	3,565	4	1	4
Intact family	71	4,365	2	0	1
Gender	40	4,396	2	0	1
Ethnicity	95	4,341	3	1	3
School stratum	11	4,425	5	1	5

Decomposition models - Imputed results (only for England survey data)

Table A.2.6: Standard method, Erikson et al./Buis decompositions

	England					
	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
Effect decomposition						
Total	-1.648	(0.154)	-10.680	0.000	-1.953	-1.344
Indirect effect 1	-1.139	(0.083)	-13.700	0.000	-1.303	-0.975
Direct effect 1	-0.509	(0.154)	-3.300	0.002	-0.819	-0.200
Indirect effect 2	-1.099	(0.080)	-13.700	0.000	-1.258	-0.940
Direct effect 2	-0.549	(0.163)	-3.370	0.001	-0.875	-0.224
Proportion mediated						
Method1	0.693	(0.071)	9.770	0.000	0.550	0.835
Method2	0.668	(0.074)	9.090	0.000	0.521	0.816
Average	**0.680**	**(0.072)**	**9.440**	**0.000**	**0.536**	**0.825**
Multiple-imputation estimates	Imputations	=	50			
Bootstrap results	Number of obs	=	1,719			
	Average RVI	=	0.1655			
	Largest FMI	=	0.4619			
DF adjustment: Large sample	DF: min	=	46.64			
	avg	=	95.15			
Within VCE type: Bootstrap	max	=	186.59			

Note: higher-ISEI vs low-ISEI. Bootstrapped standard errors.

Table A.2.7: Causal mediation method, KHB

England						
Effect decomposition	Coef.	Std. Err.	t	P>t	[95% Conf.	Interval]
Reduced	-0.209	(0.025)	-8.460	0.000	-0.259	-0.160
Full	-0.093	(0.027)	-3.470	0.001	-0.147	-0.039
Diff	-0.116	.	.	.	.	.
Proportion Mediated	**0.560**					
Multiple-imputation estimates	Imputations	=	50			
Decomposition	Number of obs	=	1,719			
	Average RVI	=	.			
	Largest FMI	=	.			
DF adjustment: Large sample	DF: min	=	51.91			
	avg	=	.			
	max	=	.			

Note: higher-ISEI vs low-ISEI; imputed models do not report std err. for the proportion mediated; I employed the KHB instead of IKY method because the latter does not work with multiple imputations. These two methods are equivalent.

Table A.2.8: Causal mediation method, G-computation

England						
	G-estimate	Std. Err.	z	P>z	[95% Conf.	Interval]
TCE	0.144	(0.026)	-5.55	0.000	-0.19436	-0.09286
NDE	0.057	(0.024)	-2.34	0.019	-0.10534	-0.00937
NIE	0.086	(0.014)	-5.85	0.000	-0.11513	-0.05738
PM	**0.60**					

Note: higher-ISEI vs low-ISEI; Bootstrapped standard errors. Imputed models do not report std err. for the proportion mediated.

Section A.2.5. LPM and School Fixed effect models, ISEI vs ESeC comparison

Table A.2.9: Upper secondary ed. transition and GPA outcomes using ISEI and (simplified) ESeC, England

	Transition to upper secondary education								GPA							
	(1) LPM - ISEI		(2) LPM - ESeC		(3) Sch FE - ISEI		(4) Sch FE - ESeC		(5) LPM - ISEI		(6) LPM - ESeC		(7) Sch FE - ISEI		(8) Sch FE - ESeC	
	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err
Par. ISEI (ref. High ISEI)																
High-mid ISEI	-0.061**	(0.024)	-	-	-0.036	(0.024)	-	-	-0.222***	(0.053)	-	-	-0.125**	(0.048)	-	-
Mid-low ISEI	-0.145***	(0.032)	-	-	-0.096***	(0.031)	-	-	-0.359***	(0.077)	-	-	-0.208***	(0.068)	-	-
Low ISEI	-0.244***	(0.033)	-	-	-0.180***	(0.032)	-	-	-0.577***	(0.078)	-	-	-0.390***	(0.062)	-	-
ESeC (ref. Man and prof.)																
Intermediate class	-	-	-0.019	(0.035)	-	-	-0.005	(0.035)	-	-	-0.144	(0.088)	-	-	-0.060	(0.083)
Working class	-	-	-0.150***	(0.025)	-	-	-0.103***	(0.025)	-	-	-0.380***	(0.063)	-	-	-0.250***	(0.051)
Parental education (ref. Primary or below)																
Lower sec.	0.079	(0.054)	0.089	(0.055)	0.059	(0.052)	0.066	(0.053)	0.248*	(0.126)	0.265**	(0.127)	0.229*	(0.120)	0.240**	(0.119)
Upper sec.	0.120**	(0.051)	0.134**	(0.052)	0.082*	(0.049)	0.092*	(0.049)	0.372***	(0.134)	0.387***	(0.134)	0.291**	(0.128)	0.302**	(0.127)
University	0.168***	(0.048)	0.206***	(0.050)	0.113**	(0.046)	0.139***	(0.047)	0.658***	(0.126)	0.721***	(0.127)	0.500***	(0.122)	0.538***	(0.121)
Intact family (ref. Yes)																
No	-0.108***	(0.021)	-0.113***	(0.022)	-0.069***	(0.022)	-0.072***	(0.022)	-0.243***	(0.047)	-0.249***	(0.046)	-0.154***	(0.041)	-0.157***	(0.042)
Gender (ref. boys)																
Girls	0.076***	(0.028)	0.078***	(0.028)	0.047**	(0.021)	0.048**	(0.021)	0.028	(0.061)	0.035	(0.062)	0.024	(0.048)	0.027	(0.048)
Ethnicity (ref. Natives)																
Mixed	-0.005	(0.046)	-0.007	(0.048)	-0.028	(0.044)	-0.028	(0.046)	0.081	(0.095)	0.074	(0.098)	0.061	(0.096)	0.058	(0.098)
Migrants	0.087***	(0.028)	0.085***	(0.029)	0.092***	(0.025)	0.091***	(0.026)	0.073	(0.056)	0.071	(0.059)	0.016	(0.051)	0.014	(0.051)
School stratum (ref. 0-10%)																
10-30%	0.098**	(0.047)	0.100**	(0.048)	-	-	-	-	0.211*	(0.114)	0.219*	(0.117)	-	-	-	-
30-60%	0.059	(0.051)	0.050	(0.051)	-	-	-	-	0.030	(0.095)	0.015	(0.097)	-	-	-	-
60-100%	-0.006	(0.052)	-0.013	(0.053)	-	-	-	-	-0.023	(0.123)	-0.029	(0.125)	-	-	-	-
Independent schools	0.138***	(0.041)	0.141***	(0.041)	-	-	-	-	0.592***	(0.175)	0.593***	(0.177)	-	-	-	-
N	2095		2095		2095		2095		2095		2095		2095		2095	
R^2	0.123		0.112		0.052		0.043		0.216		0.207		0.076		0.070	
Log lik	-1147.8		-1161.0		-965.7		-975.3		-2700.3		-2712.3		-2441.3		-2448.2	

Note: TE = Total Effect; Standard errors adjusted at school level in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01; models also include controls for month/year of birth.

Table A.2.10: Upper secondary ed. transition and GPA outcomes using ISEI and (simplified) ESeC, Sweden

	Transition to upper secondary education								GPA							
	(1) LPM - ISEI		(2) LPM - ESeC		(3) Sch FE - ISEI		(4) Sch FE - ESeC		(5) LPM - ISEI		(6) LPM - ESeC		(7) Sch FE - ISEI		(8) Sch FE - ESeC	
	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err	TE	Std err
Par. ISEI (ref. High ISEI)																
High-mid ISEI	-0.080***	(0.025)	-	-	-0.071***	(0.026)	-	-	-0.189***	(0.044)	-	-	-0.160***	(0.046)	-	-
Mid-low ISEI	-0.151***	(0.032)	-	-	-0.126***	(0.032)	-	-	-0.422***	(0.059)	-	-	-0.376***	(0.063)	-	-
Low ISEI	-0.210***	(0.038)	-	-	-0.190***	(0.036)	-	-	-0.580***	(0.066)	-	-	-0.553***	(0.068)	-	-
ESeC (ref. Man and prof.)																
Intermediate class	-	-	-0.119***	(0.040)	-	-	-0.091**	(0.041)	-	-	-0.333***	(0.090)	-	-	-0.293***	(0.092)
Working class	-	-	-0.142***	(0.029)	-	-	-0.124***	(0.030)	-	-	-0.387***	(0.054)	-	-	-0.376***	(0.053)
Parental education (ref. Primary or below)																
Lower sec.	-0.014	(0.047)	-0.009	(0.047)	-0.005	(0.048)	-0.001	(0.049)	0.187	(0.137)	0.202	(0.138)	0.191	(0.126)	0.204	(0.127)
Upper sec.	0.049	(0.046)	0.058	(0.046)	0.043	(0.047)	0.051	(0.047)	0.341**	(0.137)	0.371***	(0.137)	0.299**	(0.127)	0.325**	(0.128)
University	0.142***	(0.049)	0.167***	(0.048)	0.132***	(0.050)	0.154***	(0.049)	0.492***	(0.137)	0.569***	(0.139)	0.454***	(0.131)	0.516***	(0.132)
Intact family (ref. Yes)																
No	-0.062**	(0.026)	-0.064**	(0.026)	-0.057**	(0.028)	-0.059**	(0.028)	-0.183***	(0.044)	-0.190***	(0.045)	-0.166***	(0.044)	-0.171***	(0.044)
Gender (ref. boys)																
Girls	0.067***	(0.022)	0.066***	(0.022)	0.059***	(0.023)	0.058**	(0.023)	0.181***	(0.044)	0.179***	(0.045)	0.193***	(0.046)	0.190***	(0.047)
Ethnicity (ref. Natives)																
Mixed	-0.004	(0.032)	-0.007	(0.032)	-0.010	(0.034)	-0.012	(0.034)	0.071	(0.068)	0.060	(0.068)	0.044	(0.067)	0.036	(0.067)
Migrants	0.090***	(0.030)	0.088***	(0.030)	0.092***	(0.031)	0.090***	(0.031)	-0.090	(0.060)	-0.101	(0.062)	-0.088	(0.058)	-0.099*	(0.059)
School stratum (ref. 0-10%)																
10-30%	0.013	(0.041)	0.015	(0.041)	-	-	-	-	-0.073	(0.072)	-0.066	(0.073)	0.000	-	-	-
30-60%	0.008	(0.039)	0.010	(0.040)	-	-	-	-	-0.019	(0.080)	-0.009	(0.082)	0.000	-	-	-
60-100%	0.001	(0.047)	-0.001	(0.047)	-	-	-	-	0.007	(0.103)	0.005	(0.104)	0.000	-	-	-
N	4,581		4,581		4,581		4,581		4,581		4,581		4,581		4,581	
R^2	0.065		0.060		0.050		0.046		0.140		0.130		0.115		0.106	
Log lik	-1357.7		-1362.7		-1271.7		-1276.1		-2641.3		-2653.8		-2514.6		-2525.0	

Note: TE = Total Effect; Standard errors adjusted at school level in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01; models also include controls for month/year of birth.

Section A.2.6. Additional sensitivity analysis based on propensity score matching and Mantel and Haenszel bounds

Table A.2.11: Propensity Score Matching and Mantel and Haenszel bounds estimates for Total Effects

England						Sweden					
TE	**Std. Err.**	**z**	**P>z**	**[95% CI]**		**TE**	**Std. Err.**	**z**	**P>z**	**[95% CI]**	
-0.25	(0.06)	-4.42	0.000	-0.366	-0.141	-0.30	(0.024)	-12.5	0.000	-0.350	-0.255
	Gamma	**Q_mh+**	**Q_mh-**	**p_mh+**	**p_mh-**		**Gamma**	**Q_mh+**	**Q_mh-**	**p_mh+**	**p_mh-**
	1	10.53	10.53	0.00	0.00		1	11.93	11.93	0.00	0.00
	1.5	13.51	7.72	0.00	0.00		1.5	15.65	8.40	0.00	0.00
	2	15.75	5.80	0.00	0.00		2	18.42	5.97	0.00	0.00
	2.5	17.58	4.35	0.00	0.00		2.5	20.65	4.13	0.00	0.00
	3	19.15	3.17	0.00	0.00		3	22.54	2.65	0.00	0.00
	3.5	20.54	2.19	0.00	0.01		3.5	24.17	1.40	0.00	0.08
	4	21.79	1.34	0.00	0.09		4	25.63	0.33	0.00	0.37

Note: Bootstrapped standard errors in parenthesis; 1000 replications. High-ISEI vs Low-ISEI, England N = 1042; Sweden N = 2514 (Y from register data).

Furthermore, to test the presence of potential exposure-outcome confounders, I first estimated the total effects using a propensity score estimator with 1:3 replacement and then computed the Rosembaum bounds to check how large the hidden bias would need to be to negate these total effects. Results from this sensitivity check, displayed in Table A.2.11, show that the log-odds (gamma column) should be at least above 4 for England and 3.5 for Sweden. In the case of Sweden the small negative Mantel and Haenszel statistics (Q_mh-) and its p-values at gamma 3.5 indicate that there is a higher risk of underestimating the total effect.

Section A.2.7. Description of the decomposition model through causal diagrams

The Figure A.2 reported below depicts the different mediated and direct effects decomposed in the last model presented in the chapter 2 which incorporated cognitive ability and aspirations.

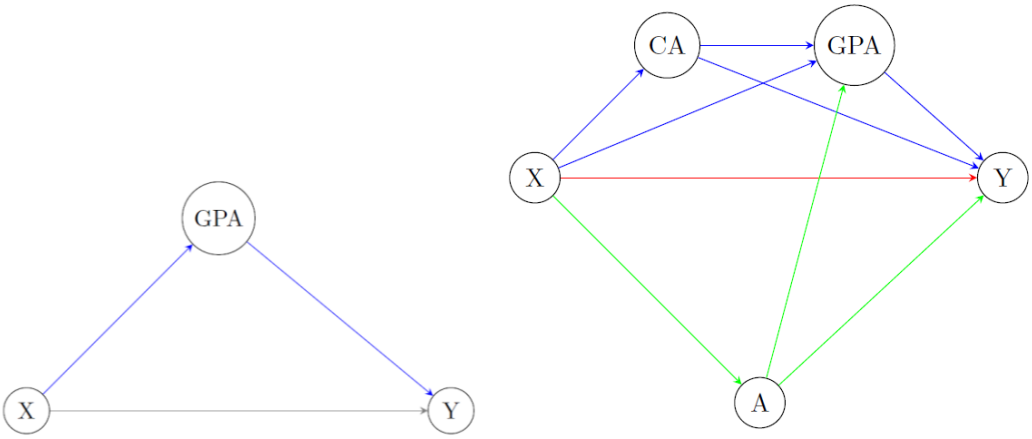


Figure A.2.2: Decomposition of performance and choice effects with and without cognitive ability and aspirations (as formulated in the main results of the thesis)

Note: Left hand side: standard DAG employed in performance and choice research.
Right hand side: DAG incorporating CA and aspirations.
Note: X=Parental ISEI; GPA, CA = cognitive ability; A=Aspirations; Y=Transition outcome. Red arrow direct effect of parental ISEI on academic transition. Blue arrows mediated effects through ability and academic performance. Green arrows mediated effects through aspirations.

The final model (Figure A.2.2) presented in the thesis can be also considered line with the idea that cognitive ability is part of the primary effects since it purely mediates the path that goes through academic performance while aspirations remain in a different mediation path that can still be conceptualized as part of secondary effects. The only difference is that this model further decomposed the choice effects into a direct path (which could be interpreted as loss aversion) and another that goes through aspirations and allows for anticipatory decisions (arrow from aspiration to GPA). More precisely, it can be seen that the blue arrows represented the mediated path through cognitive ability and GPA that capture the part of the variance referred to performance and potential anticipatory decisions (performance effects) while the green arrows and the red ones decompose the variance referred to choice effects into mediated

component through aspirations and an another one, the direct effect, which could be interpreted as loss aversion.

Table A.2.12: G-estimation results with alternatives specifications

England – results without CA and Aspirations		
TCE	**NIE**	**PM**
-0.118	-0.076	0.64
(0.015)	(0.014)	(0.12)
[-0.147, -0.089]	[-0.103, -0.049]	[0.41, 0.87]
England – results with CA only		
TCE	**NIE**	**PM**
-0.118	-0.073	0.62
(0.016)	(0.013)	(0.12)
[-0.149, -0.087]	[-0.098, -0.047]	[0.38, 0.85]
England – results with CA affecting aspirations		
TCE	**NIE**	**PM**
-0.118	-0.071	0.60
(0.0165)	(0.013)	(0.12)
[-0.149, -0.087]	[-0.096, -0.045]	[0.38, 0.85]

Note: Bootstrapped standard errors in parenthesis; 1000 replications. High-ISEI vs Low-ISEI, England N = 1042.Total Causal Effect (TCE), Natural Indirect Effect (NIE) and Proportion Mediated (PM).

Table A.2.12 reports additional model specifications to check whether a) there are changes in the total effects and b) how much indirect and the proportion mediated changes when we assume that CA affects aspirations. These robustness check show that total effects do not change with the inclusion of the cognitive ability and aspirations.

Appendix - Chapter 3

Are mixed ability classes bad for school performance and educational choice? Socioeconomic and ethnic inequality in English and Swedish schools

This appendix section reports summary statistic of the sample and additional results not displayed in the main text from different decomposition model, as well as further robustness checks.

Section A.3.1.	**Further homogeneity analysis on math performance and transitions without GPA control**
Section A.3.2.	**Further ability grouping analysis on GPA performance and transitions without GPA control**
Section A.3.3.	**Percentage distribution of Math and English/Swedish ability grouping**
Section A.3.4.	**Inequality analysis – full tables**

Section A.3.1. Further homogeneity analysis on math performance, transitions without GPA control, and GPA

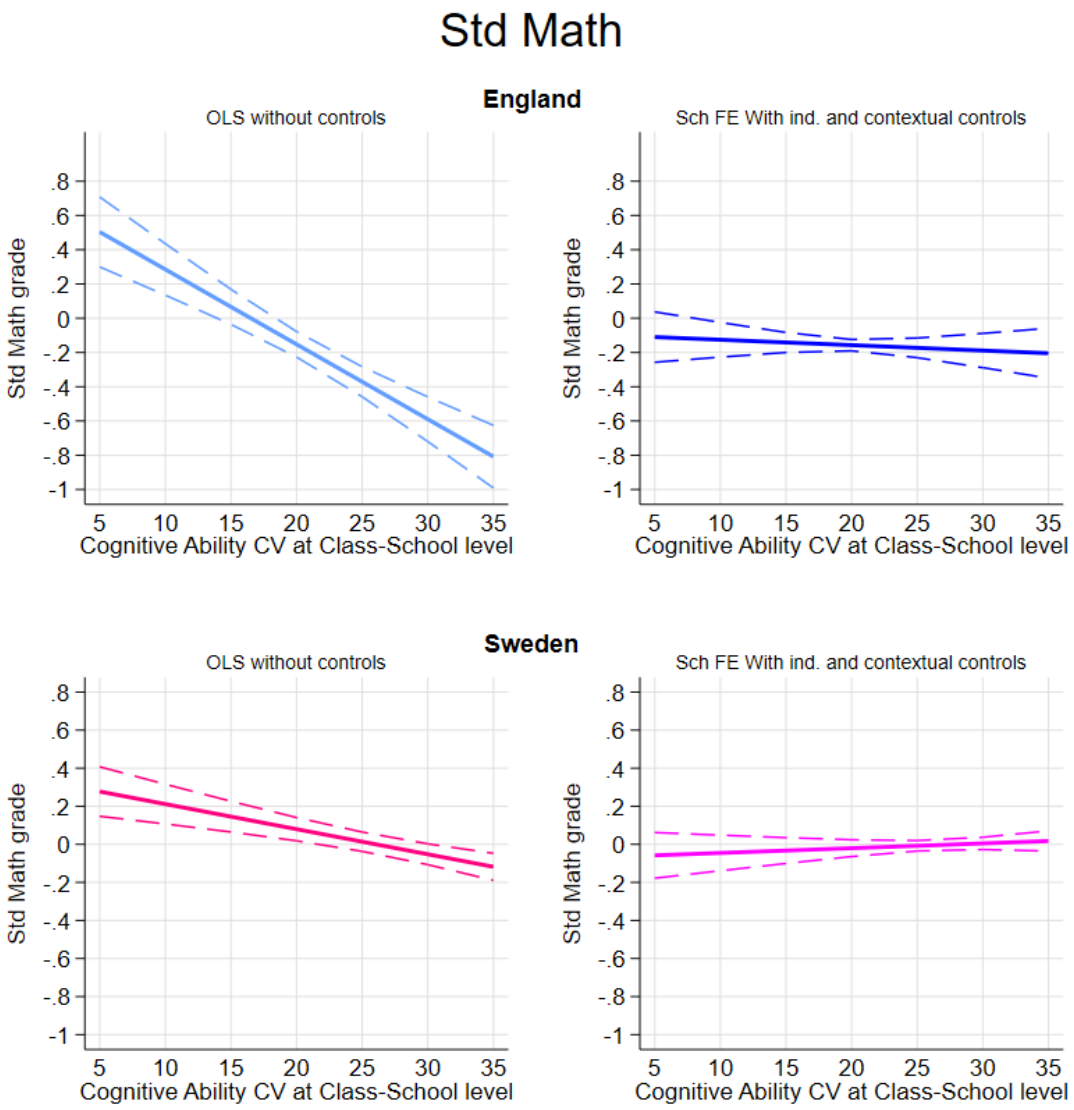


Figure A.3.1: Ability homogeneity analysis on standardized math grades

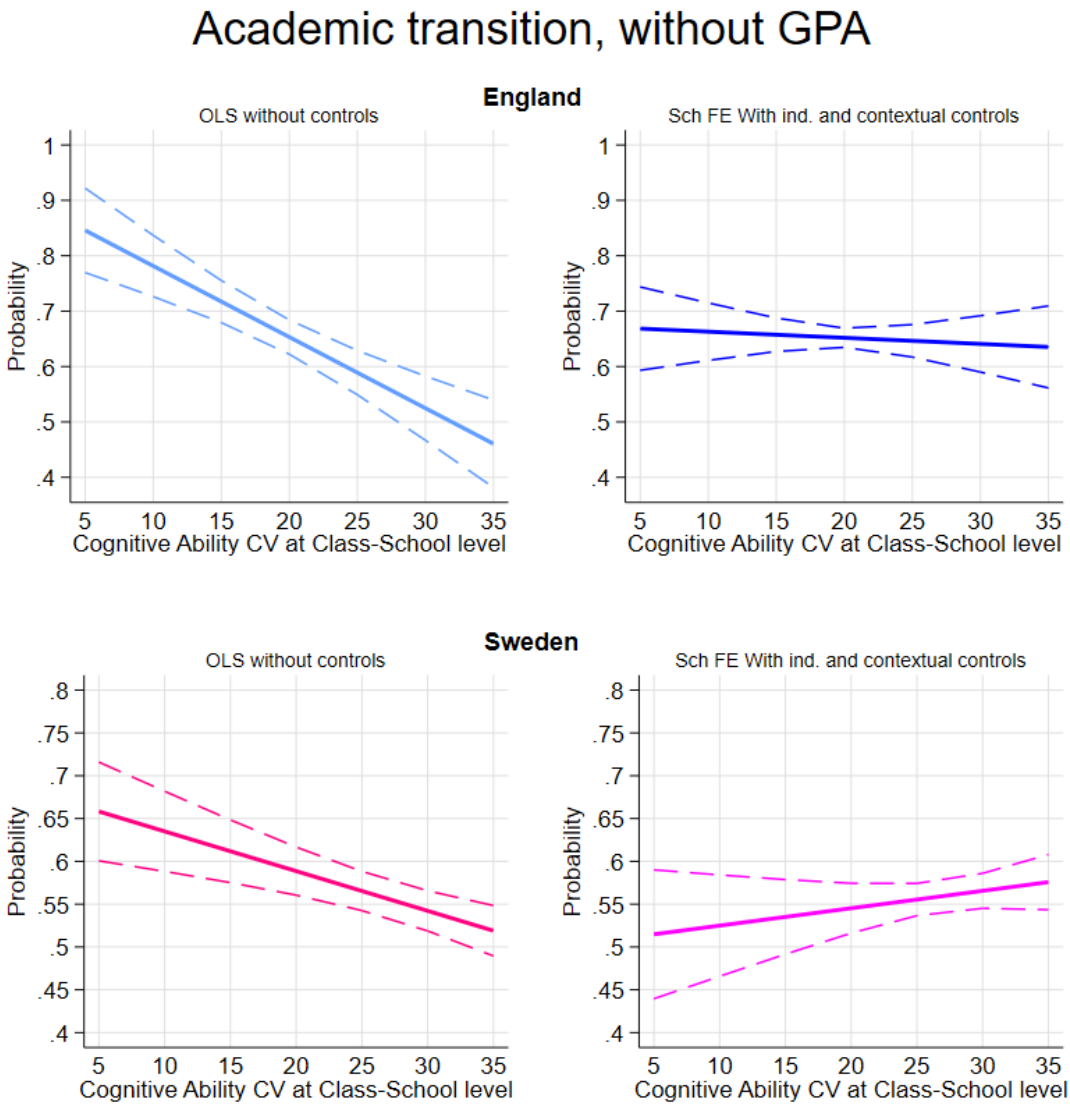


Figure A.3.2: Ability homogeneity analysis on academic transition without controlling for GPA

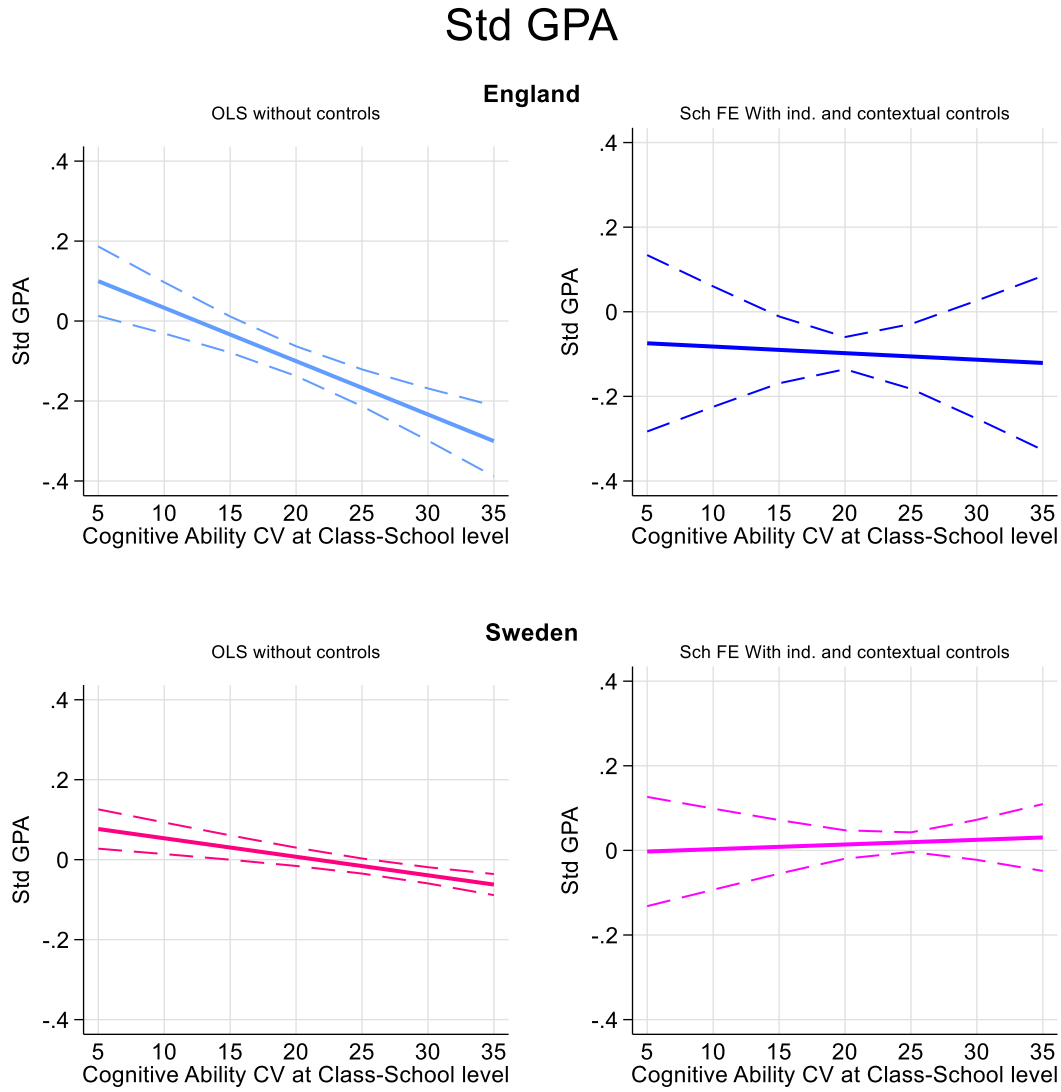


Figure A.3.3: Ability homogeneity analysis on standardized GPA (within class-school)
Note: Robustness check, analysis using GPA standardized within country, school and class rather than GPA standardized within country.

Table A.3.1: Robustness check. GPA regressed on Coefficient of Variation, students in mixed ability classes only

	England		Sweden	
	Coef.	Std.err.	Coef.	Std.err.
CA CV	-0.02	(0.017)	0.00	(0.005)
CA std	0.35***	(0.048)	0.35***	(0.022)
LANG std	0.04	(0.031)	0.12***	(0.021)
Gender (ref. boys)				
Girls	-0.18	(0.112)	-0.01	(0.035)
Generation (ref. natives)				
First gen	0.28*	(0.145)	0.16**	(0.070)
Second gen	-0.08	(0.118)	0.04	(0.039)
Par years of edu. (ref. 8 years)				
12 years	0.30*	(0.162)	0.23***	(0.084)
14 years	0.33*	(0.174)	0.35***	(0.082)
17 years	0.50***	(0.188)	0.55***	(0.089)
Par. ISEI (ref. High)				
Mid-high	-0.16	(0.160)	-0.15***	(0.049)
Mid-low	-0.20	(0.163)	-0.18***	(0.054)
Low	-0.31*	(0.163)	-0.29***	(0.059)
School FE	*yes*		*yes*	
N	635		2695	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Section A.3.2. Further ability grouping analysis on GPA performance and transitions without GPA control

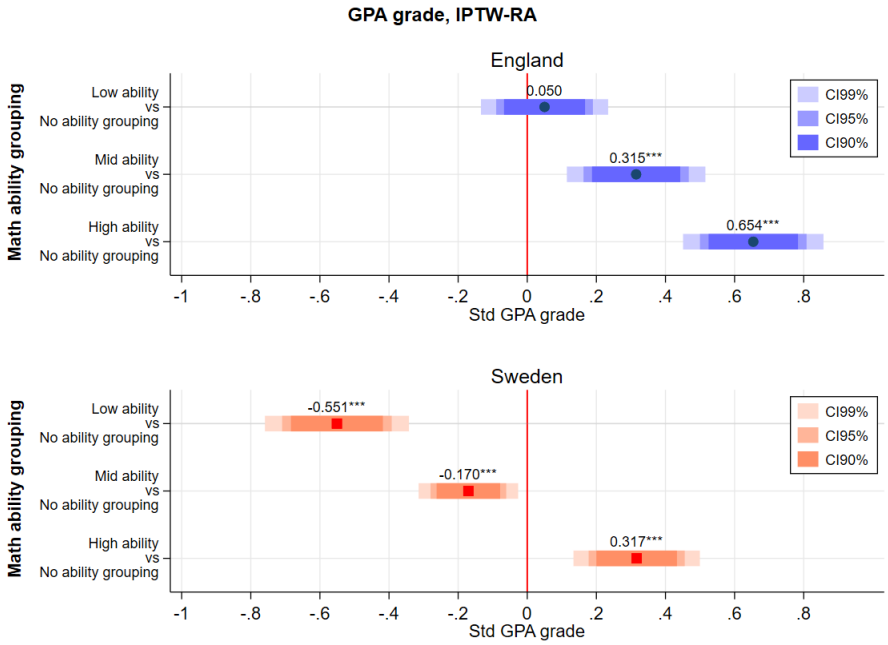


Figure A.3.4: Ability grouping analysis on standardized GPA

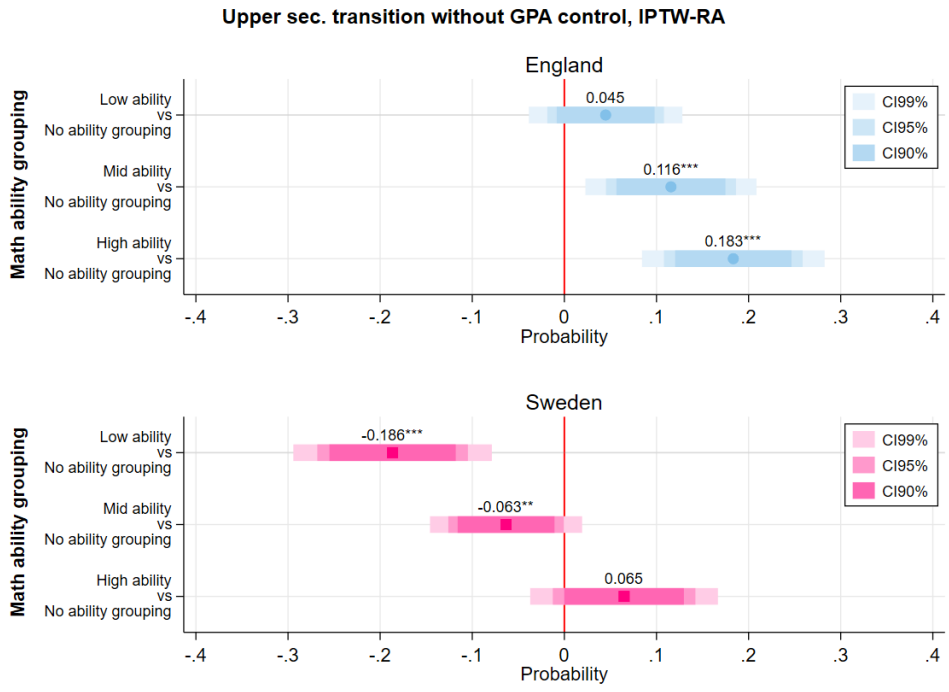


Figure A.3.5: Ability grouping analysis on academic transition without controlling for GPA

Section A.3.3. Distribution of propensity scores in IPTW-RA model

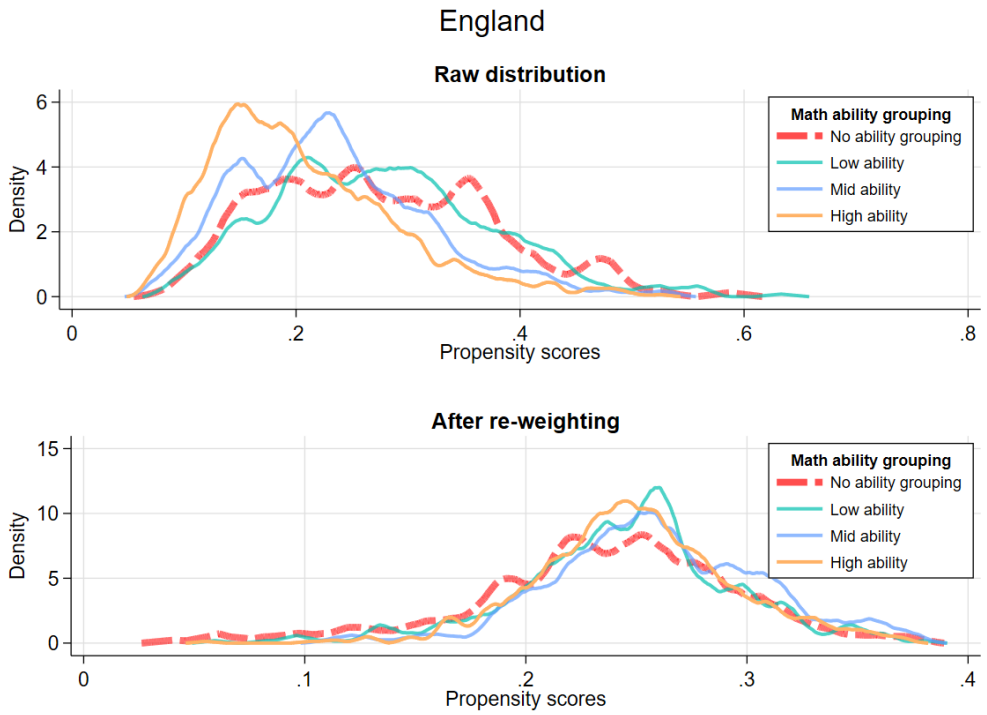


Figure A.3.3: Distribution of propensity scores in England

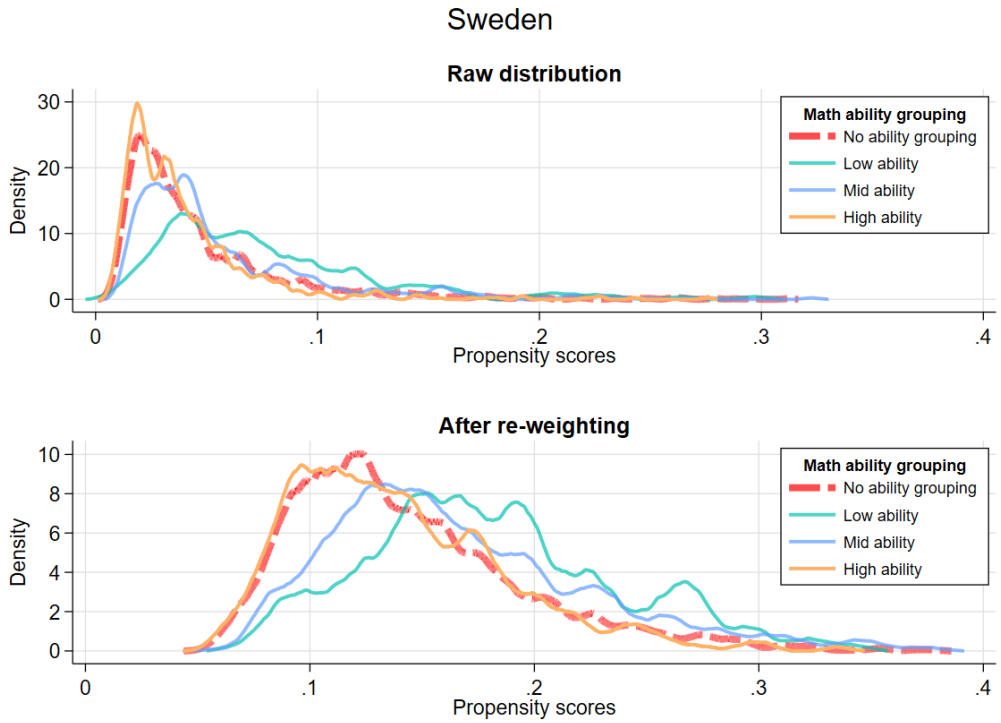


Figure A.3.4: Distribution of propensity scores in Sweden

Section A.3.3. Percentage distribution of Math and English/Swedish ability grouping

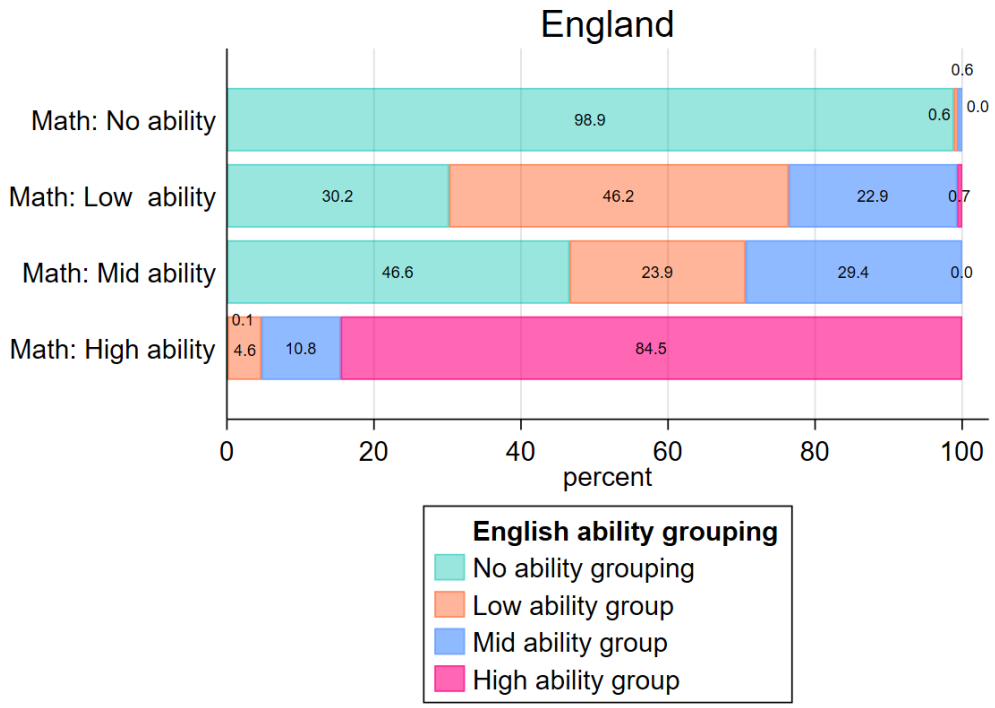


Figure A.3.5: Distribution of Math and English ability groups in England

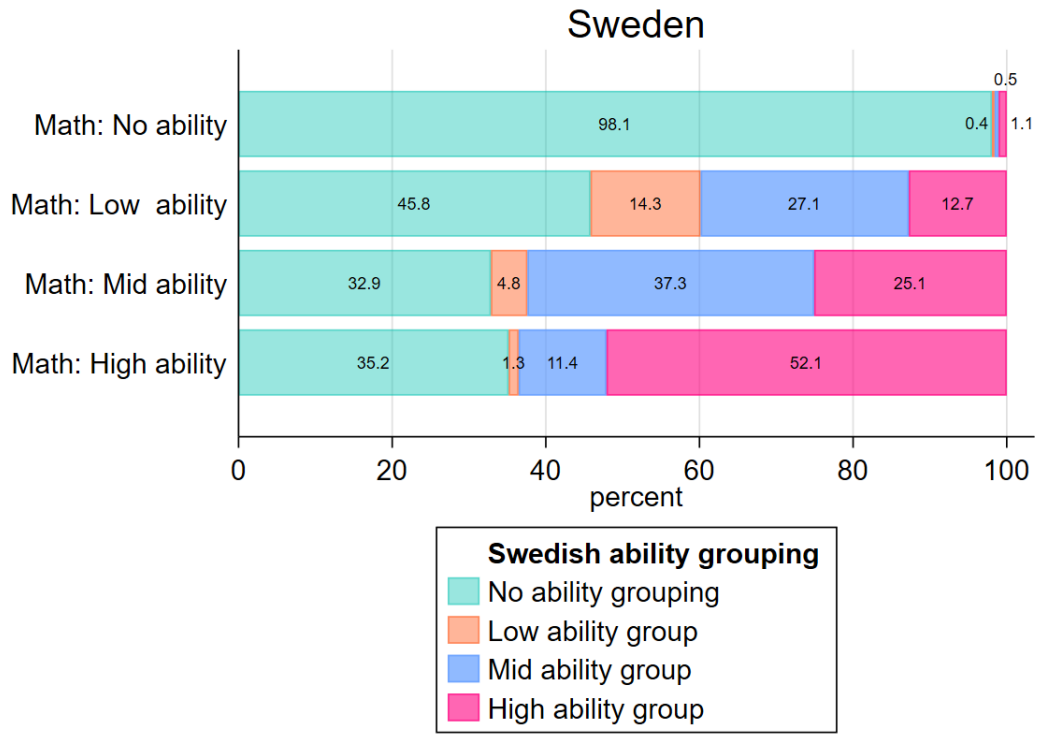


Figure A.3.6: Distribution of Math and Swedish ability groups in Sweden

Section A.3.4. Inequality analysis – full tables

Coefficient of Variation in CA
Tables report interaction Coefficient of variation in cognitive ability and socio-demo characteristics – imputed survey data

Table A.3.2: Inequality analysis – Coefficient of Variation#Parental yrs of education, England

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
CV cognitive ability	0.007	(0.015)	0.003	(0.015)	-0.009	(0.007)	-0.010	(0.006)
Year of parental education (YPE)	0.059***	(0.021)	0.065***	(0.021)	-0.000	(0.010)	-0.013	(0.010)
CV#YPE	-0.001	(0.001)	-0.001	(0.001)	0.001	(0.000)	0.001*	(0.000)
Gen status (ref natives)								
First gen	0.209***	(0.059)	0.168***	(0.058)	0.104***	(0.028)	0.070***	(0.026)
Second gen	0.082**	(0.040)	0.090**	(0.038)	0.050**	(0.021)	0.032	(0.019)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.128***	(0.045)	-0.107**	(0.043)	-0.032	(0.021)	-0.010	(0.021)
Mid-low ISEI	-0.166***	(0.055)	-0.178***	(0.052)	-0.083***	(0.028)	-0.046*	(0.026)
Low ISEI	-0.331***	(0.055)	-0.349***	(0.050)	-0.169***	(0.029)	-0.097***	(0.027)
Std GPA							0.204***	(0.011)
_cons	-0.766**	(0.339)	-0.873**	(0.336)	0.678***	(0.162)	0.856***	(0.150)
N	4030		4030		4030		4030	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.3: Inequality analysis – Coefficient of Variation#Parental ISEI, England

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
CV cognitive ability	-0.002	(0.005)	-0.004	(0.006)	-0.000	(0.003)	0.001	(0.003)
Parental ISEI	-0.066	(0.050)	-0.069	(0.051)	-0.021	(0.026)	-0.007	(0.025)
CV#PISEI	-0.002	(0.002)	-0.003	(0.002)	-0.002*	(0.001)	-0.001	(0.001)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.104	(0.081)	0.183**	(0.080)	0.044	(0.046)	0.007	(0.042)
14 yrs par edu	0.206**	(0.083)	0.295***	(0.081)	0.082*	(0.043)	0.022	(0.041)
17 yrs par edu	0.363***	(0.085)	0.457***	(0.083)	0.111**	(0.044)	0.018	(0.041)
Gen status (ref natives)								
First gen	0.200***	(0.059)	0.163***	(0.058)	0.103***	(0.028)	0.070***	(0.026)
Second gen	0.080**	(0.039)	0.090**	(0.038)	0.050**	(0.021)	0.031	(0.019)
Std GPA							0.204***	(0.011)
_cons	-0.313**	(0.135)	-0.421***	(0.137)	0.531***	(0.071)	0.617***	(0.067)
N	4030		4030		4030		4030	

Note: Robust standard errors clustered at the school level in parentheses;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.4: Inequality analysis – Coefficient of Variation#Migration status, England

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
CV cognitive ability	-0.004	(0.006)	-0.006	(0.006)	-0.001	(0.003)	-0.000	(0.003)
Generation status (natives)								
1st Gen	0.071	(0.165)	0.046	(0.172)	0.017	(0.080)	0.008	(0.075)
2nd Gen	0.050	(0.117)	0.048	(0.116)	0.034	(0.052)	0.024	(0.047)
CV#Generation status								
CV#*1st Gen*	0.006	(0.008)	0.006	(0.008)	0.004	(0.004)	0.003	(0.004)
CV #*2nd Gen*	0.002	(0.006)	0.002	(0.006)	0.001	(0.003)	0.000	(0.002)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.104	(0.081)	0.182**	(0.080)	0.042	(0.046)	0.005	(0.042)
14 yrs par edu	0.207**	(0.083)	0.294***	(0.081)	0.080*	(0.044)	0.020	(0.041)
17 yrs par edu	0.368***	(0.086)	0.461***	(0.084)	0.112**	(0.044)	0.018	(0.041)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.125***	(0.045)	-0.107**	(0.044)	-0.031	(0.022)	-0.010	(0.021)
Mid-low ISEI	-0.160***	(0.055)	-0.177***	(0.052)	-0.080***	(0.029)	-0.044	(0.027)
Low ISEI	-0.325***	(0.055)	-0.348***	(0.050)	-0.168***	(0.029)	-0.097***	(0.027)
Std GPA							0.204***	(0.011)
_cons	-0.126	(0.154)	-0.225	(0.156)	0.623***	(0.083)	0.669***	(0.077)
N	4030		4030		4030		4030	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.5: Inequality analysis – Coefficient of Variation#Parental yrs of education, Sweden

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
CV cognitive ability	0.008	(0.010)	0.009	(0.010)	0.013**	(0.006)	0.011**	(0.005)
Year of parental education (YPE)	0.061***	(0.020)	0.068***	(0.019)	0.036***	(0.012)	0.023*	(0.012)
CV#YPE	-0.000	(0.001)	-0.000	(0.001)	-0.001*	(0.000)	-0.001*	(0.000)
Gen status (ref natives)								
First gen	0.106*	(0.054)	0.012	(0.058)	0.068**	(0.033)	0.066**	(0.031)
Second gen	0.019	(0.028)	0.098***	(0.029)	0.052**	(0.021)	0.034*	(0.021)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.130***	(0.036)	-0.118***	(0.035)	-0.075***	(0.024)	-0.053**	(0.023)
Mid-low ISEI	-0.208***	(0.039)	-0.232***	(0.039)	-0.115***	(0.028)	-0.072**	(0.028)
Low ISEI	-0.355***	(0.051)	-0.386***	(0.048)	-0.175***	(0.027)	-0.105***	(0.027)
Std GPA							0.182***	(0.011)
_cons	-0.723**	(0.294)	-0.925***	(0.291)	0.028	(0.177)	0.198	(0.172)
N	4754		4754		4754		4754	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.6: Inequality analysis – Coefficient of Variation#Parental ISEI, Sweden

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
CV cognitive ability	0.003	(0.003)	0.003	(0.003)	0.002	(0.002)	0.002	(0.002)
Parental ISEI	-0.240***	(0.057)	-0.266***	(0.053)	-0.080***	(0.029)	-0.031	(0.027)
CV#PISEI	0.004*	(0.002)	0.005**	(0.002)	0.001	(0.001)	-0.000	(0.001)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.162**	(0.069)	0.216***	(0.067)	0.016	(0.037)	-0.025	(0.035)
14 yrs par edu	0.284***	(0.071)	0.347***	(0.071)	0.057	(0.036)	-0.008	(0.036)
17 yrs par edu	0.433***	(0.079)	0.488***	(0.075)	0.117***	(0.041)	0.027	(0.040)
Gen status (ref natives)								
First gen	0.101*	(0.054)	0.006	(0.059)	0.068**	(0.033)	0.068**	(0.031)
Second gen	0.021	(0.028)	0.099***	(0.029)	0.052**	(0.021)	0.034	(0.021)
Std GPA							0.183***	(0.011)
_cons	-0.335***	(0.120)	-0.511***	(0.120)	0.373***	(0.062)	0.467***	(0.059)
N	4754		4754		4754		4754	

Note: Robust standard errors clustered at the school level in parentheses;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.7: Inequality analysis – Coefficient of Variation#Migration status, Sweden

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
CV cognitive ability	0.003	(0.003)	0.002	(0.003)	0.002	(0.002)	0.002	(0.002)
Generation status (natives)								
1st Gen	0.043	(0.176)	-0.096	(0.176)	0.077	(0.090)	0.096	(0.085)
2nd Gen	0.047	(0.110)	0.066	(0.107)	0.039	(0.063)	0.025	(0.061)
CV#Generation status								
CV*#1st Gen*	0.002	(0.006)	0.004	(0.006)	-0.000	(0.003)	-0.001	(0.003)
CV *#2nd Gen*	-0.001	(0.004)	0.001	(0.004)	0.000	(0.002)	0.000	(0.002)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.162**	(0.069)	0.216***	(0.068)	0.015	(0.037)	-0.025	(0.035)
14 yrs par edu	0.281***	(0.071)	0.343***	(0.071)	0.056	(0.036)	-0.008	(0.036)
17 yrs par edu	0.440***	(0.079)	0.495***	(0.075)	0.118***	(0.041)	0.026	(0.040)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.129***	(0.036)	-0.121***	(0.035)	-0.074***	(0.024)	-0.052**	(0.024)
Mid-low ISEI	-0.204***	(0.039)	-0.236***	(0.039)	-0.111***	(0.029)	-0.068**	(0.029)
Low ISEI	-0.352***	(0.050)	-0.388***	(0.048)	-0.172***	(0.028)	-0.101***	(0.028)
Std GPA							0.183***	(0.011)
_cons	-0.161	(0.113)	-0.291**	(0.112)	0.470***	(0.070)	0.524***	(0.067)
N	4754		4754		4754		4754	

Note: Robust standard errors clustered at the school level in parentheses;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Math ability grouping

Tables report interaction between levels of ability grouping in math and socio-demo characteristics – imputed survey data

Table A.3.8: Inequality analysis – Math ability grouping#Parental yrs of education, England

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
Math ability grouping (MAB)								
(ref no ability grouping)								
Low ability group	0.453	(0.287)	0.474	(0.311)	-0.123	(0.156)	-0.218	(0.139)
Mid ability group	0.434	(0.275)	0.332	(0.289)	-0.079	(0.142)	-0.146	(0.134)
High ability group	0.670**	(0.280)	0.760**	(0.292)	0.249*	(0.136)	0.098	(0.125)
Year of parental education (YPE)	0.061***	(0.017)	0.069***	(0.018)	0.009	(0.008)	-0.004	(0.008)
MAB # YPE								
low ability group # YPE	-0.038*	(0.020)	-0.038*	(0.022)	0.010	(0.011)	0.017*	(0.010)
mid ability group # YPE	-0.018	(0.019)	-0.011	(0.020)	0.012	(0.010)	0.014	(0.010)
high ability group # YPE	-0.014	(0.020)	-0.020	(0.020)	-0.007	(0.010)	-0.003	(0.009)
Gen status (ref Natives)								
First gen	0.220***	(0.058)	0.180***	(0.057)	0.111***	(0.028)	0.075***	(0.026)
Second gen	0.091**	(0.038)	0.100***	(0.037)	0.052**	(0.021)	0.032*	(0.019)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.108**	(0.043)	-0.090**	(0.042)	-0.027	(0.022)	-0.009	(0.021)
Mid-low ISEI	-0.139***	(0.052)	-0.153***	(0.050)	-0.075***	(0.028)	-0.045*	(0.026)
Low ISEI	-0.279***	(0.055)	-0.300***	(0.049)	-0.156***	(0.029)	-0.096***	(0.027)
Std GPA							0.200***	(0.012)
_cons	-1.061***	(0.246)	-1.251***	(0.256)	0.451***	(0.122)	0.701***	(0.117)
N	4021		4021		4021		4021	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.9: Inequality analysis – Math ability grouping#Parental ISEI, England

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
Math ability grouping (MAB)								
(ref no ability grouping)								
Low ability group	-0.078	(0.063)	-0.060	(0.061)	0.013	(0.031)	0.025	(0.029)
Mid ability group	0.182***	(0.057)	0.187***	(0.058)	0.092***	(0.029)	0.054**	(0.027)
High ability group	0.479***	(0.063)	0.479***	(0.062)	0.151***	(0.030)	0.055*	(0.028)
Parental ISEI (linear)	-0.128***	(0.044)	-0.150***	(0.043)	-0.058**	(0.023)	-0.028	(0.022)
MAB # PISEI								
low ability group # PISEI	0.057	(0.054)	0.060	(0.054)	-0.024	(0.028)	-0.035	(0.026)
mid ability group # PISEI	0.040	(0.050)	0.039	(0.052)	-0.011	(0.026)	-0.018	(0.025)
high ability group # PISEI	0.015	(0.054)	0.045	(0.053)	0.025	(0.027)	0.016	(0.025)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.107	(0.080)	0.187**	(0.078)	0.045	(0.045)	0.008	(0.042)
14 yrs par edu	0.211**	(0.082)	0.299***	(0.079)	0.084*	(0.043)	0.025	(0.041)
17 yrs par edu	0.338***	(0.084)	0.434***	(0.081)	0.107**	(0.043)	0.020	(0.041)
Gen status (ref Natives)								
First gen	0.217***	(0.058)	0.182***	(0.057)	0.111***	(0.028)	0.075***	(0.026)
Second gen	0.092**	(0.038)	0.101***	(0.037)	0.052**	(0.021)	0.032*	(0.019)
Std GPA							0.199***	(0.012)
_cons	-0.559***	(0.089)	-0.722***	(0.090)	0.440***	(0.049)	0.585***	(0.048)
N	4021		4021		4021		4021	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.10: Inequality analysis – Math ability grouping#Migration status, England

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
Math ability grouping (MAB) (ref no ability grouping)								
Low ability group	-0.077	(0.078)	-0.056	(0.078)	0.005	(0.040)	0.016	(0.038)
Mid ability group	0.197**	(0.080)	0.221***	(0.083)	0.109***	(0.040)	0.065*	(0.038)
High ability group	0.499***	(0.077)	0.503***	(0.079)	0.150***	(0.039)	0.050	(0.036)
Generation status (natives)								
1st Gen	0.343***	(0.120)	0.288**	(0.116)	0.140**	(0.059)	0.082	(0.055)
2nd Gen	0.021	(0.103)	0.059	(0.103)	0.049	(0.053)	0.037	(0.048)
MAB#Generation status								
low ability group#*1st Gen*	-0.098	(0.145)	-0.055	(0.140)	0.004	(0.079)	0.015	(0.074)
low ability group #*2nd Gen*	0.091	(0.126)	0.066	(0.124)	0.018	(0.062)	0.005	(0.057)
mid ability group #*1st Gen*	-0.161	(0.145)	-0.150	(0.141)	-0.088	(0.071)	-0.058	(0.067)
mid ability group #*2nd Gen*	0.068	(0.119)	0.020	(0.126)	-0.009	(0.061)	-0.013	(0.055)
high ability group #*1st Gen*	-0.220*	(0.129)	-0.196	(0.131)	-0.032	(0.072)	0.007	(0.068)
high ability group #*2nd Gen*	0.080	(0.119)	0.053	(0.119)	0.003	(0.059)	-0.007	(0.052)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.108	(0.080)	0.188**	(0.078)	0.043	(0.046)	0.006	(0.042)
14 yrs par edu	0.213**	(0.082)	0.300***	(0.080)	0.081*	(0.043)	0.021	(0.041)
17 yrs par edu	0.345***	(0.084)	0.439***	(0.082)	0.106**	(0.043)	0.019	(0.041)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.105**	(0.043)	-0.090**	(0.042)	-0.028	(0.022)	-0.010	(0.021)
Mid-low ISEI	-0.134**	(0.052)	-0.152***	(0.049)	-0.074**	(0.028)	-0.044	(0.027)
Low ISEI	-0.279***	(0.055)	-0.303***	(0.049)	-0.156***	(0.029)	-0.096***	(0.027)
Std GPA							0.199***	(0.012)
_cons	-0.441***	(0.102)	-0.605***	(0.105)	0.504***	(0.053)	0.624***	(0.053)
N	4021		4021		4021		4021	

Note: Robust standard errors clustered at the school level in parentheses;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

able A.3.11: Inequality analysis – Math ability grouping#Parental yrs of education, Sweden

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
Math ability grouping (MAB) (ref no ability grouping)								
Low ability group	-0.121	(0.380)	-0.222	(0.396)	-0.111	(0.208)	-0.074	(0.195)
Mid ability group	0.393	(0.246)	0.308	(0.266)	0.098	(0.137)	0.040	(0.131)
High ability group	0.650***	(0.226)	0.717***	(0.213)	0.230*	(0.132)	0.100	(0.127)
Year of parental education (YPE)	0.061***	(0.011)	0.066***	(0.010)	0.019***	(0.005)	0.007	(0.005)
MAB # YPE								
low ability group # YPE	-0.028	(0.028)	-0.020	(0.028)	-0.002	(0.016)	0.001	(0.015)
mid ability group # YPE	-0.039**	(0.018)	-0.035*	(0.019)	-0.011	(0.010)	-0.005	(0.009)
high ability group # YPE	-0.025	(0.016)	-0.031**	(0.015)	-0.012	(0.009)	-0.006	(0.009)
Gen status (ref Natives)								
First gen	0.102*	(0.054)	0.008	(0.058)	0.066**	(0.033)	0.065**	(0.031)
Second gen	0.007	(0.027)	0.088***	(0.028)	0.049**	(0.021)	0.034	(0.021)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.111***	(0.035)	-0.098***	(0.035)	-0.071***	(0.025)	-0.054**	(0.024)
Mid-low ISEI	-0.187***	(0.038)	-0.210***	(0.039)	-0.110***	(0.028)	-0.072**	(0.028)
Low ISEI	-0.299***	(0.051)	-0.327***	(0.048)	-0.165***	(0.028)	-0.107***	(0.027)
Std GPA							0.178***	(0.011)
_cons	-0.704***	(0.165)	-0.861***	(0.152)	0.318***	(0.082)	0.473***	(0.080)
N	4656		4656		4656		4656	

Note: Robust standard errors clustered at the school level in parentheses;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.12: Inequality analysis – Math ability grouping#Parental ISEI, Sweden

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
Math ability grouping (MAB) (ref no ability grouping)								
Low ability group	-0.501***	(0.079)	-0.504***	(0.079)	-0.147***	(0.047)	-0.058	(0.043)
Mid ability group	-0.151***	(0.049)	-0.165***	(0.052)	-0.055*	(0.029)	-0.026	(0.028)
High ability group	0.308***	(0.046)	0.284***	(0.050)	0.071***	(0.024)	0.020	(0.023)
Parental ISEI (linear)	-0.121***	(0.022)	-0.134***	(0.021)	-0.069***	(0.013)	-0.045***	(0.013)
MAB # PISEI								
low ability group # PISEI	0.004	(0.064)	0.026	(0.064)	0.014	(0.039)	0.009	(0.038)
mid ability group # PISEI	0.055	(0.040)	0.023	(0.041)	0.022	(0.024)	0.018	(0.023)
high ability group # PISEI	0.015	(0.038)	0.026	(0.035)	0.032	(0.020)	0.027	(0.020)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.149**	(0.068)	0.197***	(0.067)	0.010	(0.037)	-0.026	(0.035)
14 yrs par edu	0.251***	(0.071)	0.313***	(0.071)	0.049	(0.037)	-0.008	(0.036)
17 yrs par edu	0.410***	(0.081)	0.463***	(0.076)	0.110**	(0.041)	0.026	(0.040)
Gen status (ref Natives)								
First gen	0.102*	(0.054)	0.008	(0.057)	0.067**	(0.032)	0.066**	(0.031)
Second gen	0.009	(0.027)	0.089***	(0.028)	0.050**	(0.021)	0.034	(0.021)
Std GPA							0.178***	(0.011)
_cons	-0.253***	(0.072)	-0.412***	(0.072)	0.440***	(0.037)	0.515***	(0.036)
N	4656		4656		4656		4656	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3.13: Inequality analysis – Math ability grouping#Migration status, Sweden

	(1) Math std		(2) GPA std		(3) Transition		(4)Transition + GPA cntrl	
	Coef.	se	Coef.	se	Coef.	se	Coef.	se
Math ability grouping (MAB) (ref no ability grouping)								
Low ability group	-0.493***	(0.118)	-0.416***	(0.106)	-0.120*	(0.069)	-0.046	(0.063)
Mid ability group	-0.104	(0.063)	-0.129*	(0.070)	-0.047	(0.044)	-0.024	(0.041)
High ability group	0.335***	(0.052)	0.271***	(0.056)	0.065*	(0.034)	0.017	(0.033)
Generation status (natives)								
1st Gen	0.130*	(0.075)	0.020	(0.077)	0.089**	(0.044)	0.087**	(0.042)
2nd Gen	0.026	(0.036)	0.102***	(0.036)	0.046*	(0.027)	0.028	(0.026)
MAB#Generation status								
low ability group*#1st Gen*	-0.078	(0.219)	-0.313*	(0.184)	-0.112	(0.109)	-0.058	(0.105)
low ability group *#2nd Gen*	0.008	(0.149)	-0.068	(0.129)	-0.015	(0.089)	-0.004	(0.085)
mid ability group *#1st Gen*	-0.035	(0.124)	-0.011	(0.123)	-0.068	(0.072)	-0.068	(0.070)
mid ability group *#2nd Gen*	-0.075	(0.086)	-0.072	(0.082)	0.016	(0.056)	0.029	(0.054)
high ability group *#1st Gen*	-0.084	(0.110)	0.055	(0.104)	0.006	(0.071)	-0.006	(0.071)
high ability group *#2nd Gen*	-0.040	(0.070)	0.005	(0.070)	0.005	(0.045)	0.003	(0.045)
YPE (ref 8 yrs par edu)								
12 yrs par edu	0.146**	(0.068)	0.192***	(0.068)	0.010	(0.037)	-0.025	(0.035)
14 yrs par edu	0.249***	(0.072)	0.309***	(0.071)	0.050	(0.037)	-0.007	(0.036)
17 yrs par edu	0.411***	(0.080)	0.464***	(0.076)	0.112***	(0.041)	0.028	(0.040)
Par ISEI (ref High ISEI)								
Mid-high ISEI	-0.109***	(0.035)	-0.100***	(0.035)	-0.068***	(0.025)	-0.050**	(0.024)
Mid-low ISEI	-0.184***	(0.037)	-0.213***	(0.039)	-0.104***	(0.029)	-0.066**	(0.029)
Low ISEI	-0.297***	(0.050)	-0.328***	(0.047)	-0.159***	(0.029)	-0.100***	(0.028)
Std GPA							0.178***	(0.011)
_cons	-0.113	(0.079)	-0.253***	(0.080)	0.523***	(0.043)	0.569***	(0.042)
N	4656		4656		4656		4656	

Note: Robust standard errors clustered at the school level in parentheses; * $p < 0.05$, ** $p < 0.05$, *** $p < 0.01$.

References

Aassve, Arnstein, Elena Cottini, and Agnese Vitali. 2013. "Youth prospects in a time of economic recession." *Demographic Research* 29(36): 949-962.

Acharya, Avidit, Matthew Blackwell, and Maya Sen. 2016. "Explaining Causal Findings Without Bias: Detecting and Assessing Direct Effects." *American Political Science Review* 110 (3): 512–29. https://doi.org/10.1017/S0003055416000216.

Alwin, Duane F., and Robert M. Hauser. 1975. "The Decomposition of Effects in Path Analysis." *American Sociological Review* 40(1): 37–47. https://doi.org/10.2307/2094445.

Allmendinger, Jutta. 1989. Educational Systems and Labor Market Outcomes. *European Sociological Review* 5: 231–250.

Andersen, Ida Gran. 2018. "Pygmalion in instruction? Tracking, teacher reward structures, and educational inequality." *Social Psychology of Education* 21: 1021-1044.

Angrist, Joshua D. 2014. "The Perils of Peer Effects." *Labour Economics* 30: 98–108.

Argys, Laura M., Daniel I. Rees, and Domenic J. Brewer. 1996. "Detracking America"s schools: equity at zero cost? *Journal of Policy Analysis and Management* 15(4): 623–645.

Arrow, Holly, Joseph E. McGrath, and Jennifer L. Berdahl. 2000. "Small Groups as Complex Systems: Formation, Coordination, Development, and Adaptation." Thousand Oaks, CA: Sage.

Bach, Maximilian, and Mira Fischer. 2020. "Understanding the Response to High-Stakes Incentives in Primary Education." *ZEW-Centre for European Economic Research Discussion Paper:* 20–66.

Banks, Olive. 1971. "The Sociology of Education by Olive Banks: Good Soft Cover (1971) Second Edition. Lazy Letters Books." https://www.abebooks.co.uk/9780713409628/Sociology-Education-Olive-Banks-0713409622/plp.

Baron, Reuben M., and David A. Kenny. 1986. "The Moderator–Mediator Variable Distinction in Social Psychological Research: Conceptual, Strategic, and Statistical Considerations." *Journal of Personality and Social Psychology* 51 (6): 1173–82. https://doi.org/10.1037/0022-3514.51.6.1173.

Bell, Daniel. 1972. "On Meritocracy and Equality." Public Interest, 29:29–68.

Bell, Daniel. 1973. "The Coming of Post–Industrial Society: A Venture in Social Forecasting." New York: Basic Books.

Berkson, Joseph. 1946. "Limitations of the Application of Fourfold Table Analysis to Hospital Data." *International Journal of Epidemiology* 43(2): 511–15. https://doi.org/10.1093/ije/dyu022.

Bernardi, Fabrizio, and Diederik Boertien. 2017. "Non-Intact Families and Diverging Educational Destinies: A Decomposition Analysis for Germany, Italy, the United Kingdom and the United States." *Social Science Research,* 63(March): 181–91. https://doi.org/10.1016/j.ssresearch.2016.09.004.

Betts, Julian R. 2011. "The Economics of Tracking in Education." Pp. 341–81 in *Handbook of the Economics of Education, Vol. 3*, edited by E. A. Hanushek, S. Machin, and L. Woessmann. Amsterdam/San Diego: Elsevier/North-Holland.

Blau, Peter M., and Otis Dudley Duncan. 1967. "The American Occupational Structure." 1967. New York: John Wiley & Sons. https://books.google.co.uk/books/about/The_American_occupational_structure.html?id=14dEAAAAIAAJ&redir_esc=y.

Blalock, Hubert M. 1967. “Causal Inference in Nonexperimental.” Research. North Carolina: The University of North Carolina Press.

Blinder, Alan S. 1973. Wage discrimination: reduced form and structural estimates. *Journal of Human Resources* 8:436–55.

Blossfeld, Hans–Peter, and Yossi Shavit. 1993. “Persisting Barriers: Changes in Educational Opportunities in Thirteen Countries.” In Persistent Inequality: Changing Educational Attainment in Thirteen Countries, edited by Shavit, Y and Blossfeld, H–P. Boulder: Westview Press, pp. 1–23.

Bol, Thijs, Witschge, Jaqueline, van de Werfhorst, Herman G., and Jaap Dronkers. 2013. “Curricular Tracking and Central Examinations: Counterbalancing the Impact of Social Background on Student Achievement in 36 Countries.” *Social Forces* 92(4): 1545-1572.

Bol, Thijs, and Herman G. Van de Werfhorst. 2013. “Educational Systems and the Trade-Off between Labor Market Allocation and Equality of Educational Opportunity.” *Comparative Education Review* 57(2):285–308.

Boudon, Raymond. 1974. “Education, Opportunity, and Social Inequality: Changing Prospects in Western Society.” New York: John Wiley & Sons.

Bourdieu, Pierre, and Jean Claude Passeron. 1977. “Reproduction in Education, Society and Culture.” London, Beverly Hills: Sage Publications. https://catalog.hathitrust.org/Record/000379850.

Breen, Richard. 2018. “Some Methodological Problems in the Study of Multigenerational Mobility.” *European Sociological Review* 34 (6): 603–11. https://doi.org/10.1093/esr/jcy037.

Breen, Richard, and John H. Goldthorpe. 1997. “Explaining Educational Differentials: Towards a Formal Rational Action Theory.” *Rationality and Society* 9 (3): 275–305. https://doi.org/10.1177/104346397009003002.

Breen, Richard, and Jan O. Jonsson. 2000. “Analyzing Educational Careers: A Multinomial Transition Model.” *American Sociological Review*, 65(5):754–72. https://doi.org/10.2307/2657545.

Breen, R., and John H. Goldthorpe. 2001. Class, Mobility and Merit: The Experience of Two British Cohorts. *European Sociological Review* 17(2), pp. 81–101.

Breen, R., and Jan O. Jonsson. 2005. Inequality of Opportunity in Comparative Perspective: Recent Research on Educational Attainment and Social Mobility. *Annual Review of Sociology* 31: 223–43.

Breen, Richard, Kristian Bernt Karlson, and Anders Holm. 2013. “Total, Direct, and Indirect Effects in Logit and Probit Models:” *Sociological Methods & Research* 42(2): 164-191. https://doi.org/10.1177/0049124113494572.

Breen, Richard, Herman G. van de Werfhorst, and Mads Meier Jæger. 2014. “Deciding under Doubt: A Theory of Risk Aversion, Time Discounting Preferences, and Educational Decision-Making.” *European Sociological Review* 30 (2): 258–70. https://doi.org/10.1093/esr/jcu039.

Breen, Richard, and Meir Yaish. 2006. “Testing the Breen-Goldthorpe Model of Educational Decision Making.” Mobility and Inequality: *Frontiers of Sociology and Economics* 232-258.

Breen, Richard, Ruud Luijkx, Walter Müller, and Reinhard Pollak. 2009. “Nonpersistent Inequality in Educational Attainment: Evidence from Eight European Countries.” *American Journal of Sociology* 114 (5): 1475–1521. https://doi.org/10.1086/595951.

Breen, Richard, and Walter Müller. 2020. “Education and Intergenerational Social Mobility in Europe and the United States.” Palo Alto: Stanford University Press. http://www.sup.org/books/title/?id=30634.

Brunello, Giorgio, and Daniele Checchi. 2007. “Does School Tracking Affect Equality of Opportunity? New International Evidence.” *Economic Policy* 22(52):782–861.
Buis, Maarten L. 2010. “Direct and Indirect Effects in a Logit Model.” *The Stata Journal* 10 (1): 11–29.
Cattaneo, Matias. D. 2010. Efficient semiparametric estimation of multi-valued treatment effects under ignorability. *Journal of Econometric*s 155: 138–154.
Choi, Seongsoo, Riley Taiji, Manting Chen, and Christiaan Monden. 2020. “Cohort Trends in the Association Between Sibship Size and Educational Attainment in 26 Low-Fertility Countries.” *Demography* 57 (3): 1035–62. https://doi.org/10.1007/s13524-020-00885-5.
Chowdry, Haroon, Claire Crawford, Lorraine Dearden, Alissa Goodman, and Anna Vignoles. 2013. “Widening Participation in Higher Education: Analysis Using Linked Administrative Data.” *Journal of the Royal Statistical Society: Series A (Statistics in Society)* 176 (2): 431–57. https://doi.org/10.1111/j.1467-985X.2012.01043.x.
Crawford, Claire, Lorraine Dearden, John Micklewright, and Anna Vignoles. 2016. *Family Background and University Success: Differences in Higher Education Access and Outcomes in England*. Oxford, New York: Oxford University Press.
Dafoe, Allan. 2015. “Nonparametric Identification of Causal Effects under Temporal Dependence:” *Sociological Methods & Research*, December. https://doi.org/10.1177/0049124115613784.
Dahllöf, Urban. 1971. *Ability grouping, content validity, and curriculum process analysis*. New York: Teachers College Press.
Daniel, Rhian M, Bianca L. De Stavola, and Simon N. Cousens. 2011. “Gformula: Estimating Causal Effects in the Presence of Time-Varying Confounding or Mediation Using the G-Computation Formula.” *The Stata journal* 11(4): 479-517.
Davis, James A. 1966. “The Campus as a Frog Pond: An Application of the Theory of Relative Deprivation to Career Decisions of College Men.” *American Journal of Sociology* 72(1):17-31.
De Stavola, Bianca L., Rhian M. Daniel, George B. Ploubidis, and Nadia Micali. 2015. “Mediation Analysis With Intermediate Confounding: Structural Equation Modeling Viewed Through the Causal Inference Lens.” *American Journal of Epidemiology* 181 (1): 64–80. https://doi.org/10.1093/aje/kwu239.
Dilnot, Catherine. 2018. “The Relationship between A-Level Subject Choice and League Table Score of University Attended: The “Facilitating”, the “Less Suitable”, and the Counter-Intuitive.” *Oxford Review of Education* 44 (1): 118–37. https://doi.org/10.1080/03054985.2018.1409976.
Dollmann, Jörg. 2016. “Less choice, less inequality? A natural experiment on social and ethnic differences in educational decision-making.” *European Sociological Review* 32(2): 203–215.
Domina, Thurston, Andrew McEachin, Paul Hanselman, Priyanka Agarwal, NaYoung Hwang & Ryan W. Lewis. 2019. “Beyond Tracking and Detracking: The Dimensions of Organizational Differentiation in Schools.” *Sociology of Education* 92: 293–322.
Dronkers, Jaap, Rolf. K. W van der Velden, and Allison Dunne. 2011. “The effects of educational systems, school composition, track-level, parental background and immigrants” origin on the achievement of 15-years old native and immigrant students. A reanalysis of PISA 2006.” METEOR, Maastricht University School of Business and Economics. METEOR Research Memorandum No. 033. https://doi.org/10.26481/umamet.2011033.
Elias, Peter, and Kate Purcell. 2013. “Classifying Graduate Occupations for the Knowledge Society.” Future track Working Paper 5.

Elwert, Felix 2013. “Graphical causal models.” In Handbook of Causal Analysis for Social Research, ed. SL Morgan, pp. 245–73. Dordrecht, Neth.: Springer.
Elwert, Felix, and Christopher Winship. 2014. “Endogenous Selection Bias: The Problem of Conditioning on a Collider Variable.” *Annual Review of Sociology* 40 (1): 31–53. https://doi.org/10.1146/annurev-soc-071913-043455.
Erikson, Robert, and Jan O. Jonsson. 1996. “Can Education Be Equalized.” https://www.nuffield.ox.ac.uk/users/JonssonJ/EriksonJonsson1996CanEducationIntro.pdf.
Erikson, Robert, John H. Goldthorpe, Michelle Jackson, Meir Yaish, and D. R. Cox. 2005. “On Class Differentials in Educational Attainment.” *Proceedings of the National Academy of Sciences* 102 (27): 9730–33. https://doi.org/10.1073/pnas.0502433102.
Erikson, Robert, and Frida Rudolphi. 2010. “Change in Social Selection to Upper Secondary School—Primary and Secondary Effects in Sweden.” *European Sociological Review* 26 (3): 291–305. https://doi.org/10.1093/esr/jcp022.
Esser, Hartmut. 2016. “The Model of Ability Tracking–Theoretical Expectations and Empirical Findings on How Educational Systems Impact on Educational Success and Inequality.” Pp. 25–42 in H.-P. Blossfeld, S. Buchholz, J. Skopek, and M. Triventi (eds.) *Models of Secondary Education and Social Inequality. An International Comparison. 2nd Edition*, Cheltenham: Edward Elgar.
Esser, Hartmut, and Julian Seuring. 2020. “Kognitive Homogenisierung, Schulische Leistungen Und Soziale Bildungsungleichheit.” *Zeitschrift Für Soziologie* 49(5–6):277–301.
EUROSTAT. 2021. “Early leavers from education and training by sex.” Consulted on January 2021: https://ec.europa.eu/eurostat/databrowser/view/sdg_04_10/default/table?lang=en
Featherman, David. L., and Robert M. Hauser. 1978. “Opportunity and change.” New York, NY: Academic Press.
Figlio, David N., and Marianne E. Page. 2002. “School choice and the distributional effects of ability tracking: Does separation increase inequality?” *Journal of Urban Economics* 51(3): 497-514.
Fordham, Signithia, and John U. Ogbu. 1986. “Black students” school success: Coping with the burden of “Acting White.” *The Urban Review* 18: 176–206.
Gambetta, Diego. 1987. “Were They Pushed or Did They Jump? Individual Decision Mechanisms in Education.” Cambridge: Cambridge University Press.
Gamoran, Adam. 2004. “Classroom Organization and Instructional Quality.” In: H.J. Walberg, A.J. Reynolds & M.C. Wang (eds.), *Can Unlike Students Learn Together? Grade Retention, Tracking, and Grouping*, pp. 141–55. Greenwich, CT: Information Age.
Gamoran, Adam, and Robert D. Mare. 1989. “Secondary School Tracking and Educational Inequality: Compensation, Reinforcement, or Neutrality?” *American Journal of Sociology* 94(5): 1146-1183.
Gamoran, Adam, and Brian P. An. 2016. “Effects of school segregation and school resources in a changing policy context.” *Educational Evaluation and Policy Analysis* 38(1): 43–64.
Ganzeboom, Harry B. G., Paul M. De Graaf, and Donald J. Treiman. 1992. “A Standard International Socio-Economic Index of Occupational Status.” *Social Science Research* 21 (1): 1–56. https://doi.org/10.1016/0049-089X(92)90017-B.
Ganzeboom, Harry B.G. and Donald J. Treiman. 1996. “Internationally Comparable Measures of Occupational Status for the 1988 International Standard Classification of Occupations.” *Social Science Research* 25(3): 201-239.

Goldthorpe, John H. 1996. “Class Analysis and the Reorientation of Class Theory: The Case of Persisting Differentials in Educational Attainment.” *The British Journal of Sociology* 47(3): 481–505.
Goldthorpe, John H. 2001. Causation, Statistics, and Sociology, European Sociological Review, 17(1):1–20, https://doi.org/10.1093/esr/17.1.1
Goldthorpe, John H. 2016. “*Sociology as a Population Science*.” Cambridge: Cambridge University Pres
Greenland, Sander. 2003. Quantifying biases in causal models: classical confounding versus collider-stratification bias. Epidemiology 14:300-306.
Guyon, Nina, Eric Maurin, and Sandra McNally. 2012. “The Effect of Tracking Students by Ability into Different Schools: A Natural Experiment.” *Journal of Human Resources* 47(3): 684–721.
Hanushek, Eric, John Kain, Jacob Markman, and Steven Rivkin. 2003. “Does peer ability affect student achievement?” *Journal of Applied Econometrics* 18: 527-544
Hanushek, Eric A., and Ludger Wößmann. 2006. “Does Educational Tracking Affect Performance and Inequality? Differences-in-Differences Evidence across Countries.” *The Economic Journal* 116(510): 63–76.
Heath, Anthony F. and Yaël Brinbaum (Eds.). 2014. “*Unequal Attainments: Ethnic Educational Inequalities in Ten Western Countries*.” Oxford: Oxford University Press.
Heckman, James. J., Lance J. Lochner, and Petra E. Todd. 2003. Fifty Years of Mincer Earnings Regressions. NBER. Working Paper no. 9732.
Heckman, James J., Lance J. Lochner, and Petra E. Todd. 2006. “Chapter 7 Earnings Functions, Rates of Return and Treatment Effects: The Mincer Equation and Beyond.” In *Handbook of the Economics of Education*, edited by E. Hanushek and F. Welch, 1:307–458. Elsevier. https://doi.org/10.1016/S1574-0692(06)01007-5.
Heisig, Jan Paul, Benjamin Elbers, and Heike Solga. 2020. “Cross-National Differences in Social Background Effects on Educational Attainment and Achievement: Absolute vs. Relative Inequalities and the Role of Education Systems.” *Compare: A Journal of Comparative and International Education* 50(2):165–84.
Hernán Miguel A., and James M. Robins. 2020. Causal Inference: What If. Boca Raton: Chapman & Hall/CRC.
Hicks, Raymond and Dustin Tingley. 2011. “Causal Mediation Analysis.” *The Stata Journal* 11(4): 605-619.
Hoffer, Thomas B. 1992. “Middle School Ability Grouping and Student Achievement in Science and Mathematics.” *Educational Evaluation and Policy Analysis* 14(3): 205-27.
Holm Anders, and Mads M. Jæger 2008. Does relative risk aversion explain educational inequality? A dynamic choice approach. *Research in Social Stratification and Mobility* 26: 199–220.
Holmes, Craig, and Ken Mayhew. 2016. The Economics of Higher Education. *Oxford Review of Economic Policy* 32(4): 475–496.
Holland, Paul W. 1986. “Statistics and Causal Inference.” *Journal of the American Statistical Association* 81(396): 945–60. https://doi.org/10.2307/2289064.
Holtmann, Anne Christine, Laura Menze, and Heike Solga. 2021. “Intergenerational Transmission of Educational Attainment: How Important Are Children”s Personality Characteristics?” *American Behavioral Scientist*, March, 0002764221996779. https://doi.org/10.1177/0002764221996779.
Horn, Daniel. 2009. “Age of Selection Counts: A Cross-Country Analysis of Educational Institutions.” *Educational Research and Evaluation* 15(4):343–66.

Horn, Daniel. 2013. “Diverging Performances: the Detrimental Effects of Early Educational Selection on Equality of Opportunity in Hungary.” *Research in Social Stratification and Mobility* 32: 25–43.
Hoxby, Caroline. 2000. “Peer effects in the classroom: Learning from gender and race variation” *NBER Working Paper*, No. 7867.
Huang, M.H. 2009. “Classroom Homogeneity and the Distribution of Student Math Performance: A Country-level Fixed-effects Analysis.” *Social Science Research* 38: 781–791.
Hyman, Herbert M. 1953. “The value systems of different classes: a social psychological contribution to the analysis of stratification.” Pp. 426-442 in R. Bendix and S. Lipset (eds.) Class, Status and Power: A Reader in Social Stratification. Glencoe: Free Press.
Imai, Kosuke, Luke Keele, and Teppei Yamamoto. 2010. “Identification, Inference and Sensitivity Analysis for Causal Mediation Effects” *Statistical Science* 1 (25): 51–71. DOI: 10.1214/10-STS321.
Imai, Kosuke, Luke Keele, and Dustin Tingley. 2010. “A General Approach to Causal Mediation Analysis.” *Psychological Methods* 15 (4): 309–34. https://doi.org/10.1037/a0020761.
Imai, Kosuke, Luke Keele, Dustin Tingley, and Teppei Yamamoto. 2011. “Unpacking the Black Box of Causality: Learning about Causal Mechanisms from Experimental and Observational Studies.” *American Political Science Review* 105 (4): 765–89. https://doi.org/10.1017/S0003055411000414.
Jackson, Michelle. 2013. “Determined to Succeed? Performance versus Choice in Educational Attainment.” Palo Alto: Stanford University Press.
Jackson, Michelle, Robert Erikson, John H. Goldthorpe, and Meir Yaish. 2007. “Primary and Secondary Effects in Class Differentials in Educational Attainment: The Transition to A-Level Courses in England and Wales.” *Acta Sociologica* 50 (3): 211–29.
Johnston, Olivia. and Helen Wildy. 2016. “The effects of Streaming in the Secondary school on Learning Outcomes for Australian Students – a Review of the International Literature.” *Australian Journal of Education* 60: 42–59.
Jonsson, Jan O., and Carina Mood. 2008. “Choice by Contrast in Swedish Schools: How Peers” Achievement Affects Educational Choice.” *Social Forces: International Journal of Social Research* 87 (2): 741–65.
Jonsson, Jan O., and Frida Rudolphi. 2011. “Weak performance—strong determination: school achievement and educational choice among children of immigrants in Sweden.” *European Sociological Review* 27: 487–508.
Jonsson, Jan O., and Georg Treuter. 2021. Do school characteristics matter for ethnic an socioeconomic inequality in education? School resources, climate, and composition in Swedish comprehensive schools. *Mimeo*. Nuffield College and Swedish Institute for Social Research.
Kahneman, Daniel, and Amos Tversky. 1979. “Prospect Theory: An Analysis of Decision under Risk.” *Econometrica* 47 (2): 263–91. https://doi.org/10.2307/1914185.
Kalter, Frank, Jan O. Jonsson, Frank van Tubergen, Anthony Heath (Eds.) *Growing Up in Diverse Societies: The Integration of the Children of Immigrants in England, Germany, the Netherlands and Sweden.* Oxford: Oxford University Press.
Kalter, Frank, Anthony F. Heath, Miles Hewstone, Jan O. Jonsson, Matthijs Kalmijn, Irena Kogan, Frank van Tubergen. 2014. Children of Immigrants Longitudinal Survey in Four European Countries (CILS4EU) - Full version. Data file for on-site use. GESIS Data Archive, Cologne. ZA5353 Data file Version 3.3.0, https://doi.org/10.4232/cils4eu.5353.3.3.0.

Karlson, Kristian Bernt. 2013. “Summarizing primary and secondary effects.” *Research in Social Stratification and Mobility* 33: 72-82. https://doi.org/10.1016/j.rssm.2013.01.001.

Karlson, Kristian Bernt, and Anders Holm. 2011. “Decomposing Primary and Secondary Effects: A New Decomposition Method.” *Research in Social Stratification and Mobility* 29 (2): 221–37. https://doi.org/10.1016/j.rssm.2010.12.005.

Karlson, Kristian Bernt, Anders Holm, and Richard Breen. 2012. “Comparing Regression Coefficients Between Same-Sample Nested Models Using Logit and Probit: A New Method.” *Sociological Methodology* 42 (1): 286–313. https://doi.org/10.1177/0081175012444861.

Keele, Luke, Dustin Tingley, and Teppei Yamamoto. 2015. “Identifying Mechanisms Behind Policy Interventions Via Causal Mediation Analysis: Methods for Policy Analysis.” *Journal of Policy Analysis and Managemen*t 34 (4): 937–63. https://doi.org/10.1002/pam.21853.

Keller, Suzanne, and Marisa Zavalloni. 1964. “Ambition and Social Class: A Respecification.” *Social Forces* 43 (1): 58–70. https://doi.org/10.1093/sf/43.1.58.

Kerckhof, Alan. 1986. “Effects of Ability Grouping in British Secondary Schools.” *American Sociological Review* 51(6): 842-858.

Kerr, Norbert L. 1983. Motivation Losses in Small Groups. A Social Dilemma Analysis. *Journal of Personality and Social Psychology*. 45(4): 819–828.

Korthals, R.A., and J. Dronkers. 2016. “Selection on performance and tracking.” *Applied Economics* 48(30): 2836-2851.

Low, Hamish, and Costas Meghir. 2017. ”The Use of Structural Models in Econometrics.” *Journal of Economic Perspectives* 31 (2): 33-58.

Lucas, Samuel R. 2001.“Effectively Maintained Inequality: Education Transitions, Track Mobility, and Social Background Effects.” *American Journal of Sociology*, 106(6):1642–90. https://doi.org/10.1086/321300.

Manning, Alan, and Jorn-Steffen Pischke. 2006. “Comprehensive Versus Selective Schooling in England & Wales: What Do We Know?” London: Centre for the Economics of Education/ London School of Economics.

Marks, Gary N. 2005. “Cross-National Differences and Accounting for Social Class Inequalities in Education.” *International Sociology* 20(4): 483–505.

Matthewes, Sönke H. 2020. “Better Together? Heterogeneous Effects of Tracking on Student Achievement.” *The Economic Journal* 131(April):1269–1307.

McMullin, Patricia, and Nevena Kulic. 2016. “Onwards or upwards? The role of subject choice and schools in the reproduction of educational inequality in England.” Pp. 213-231 in Blossfeld, H-P., Buchholz, S., Skopek, J., & Triventi, M. (Eds.). *Models of secondary education and social inequality. An international comparison. Cheltenham:* Edward Elgar.

Meghir, Costas, and Mårten Palme. 2005. “Educational Reform, Ability, and Family Background.” *American Economic Review* 95 (1): 414–24.

Morgan, Stephen L. 2012. “Models of College Entry in the United States and the Challenges of Estimating Primary and Secondary Effects.” *Sociological Methods & Research* 41 (1): 17–56.

Morgan, Stephen L., Michael W. Spiller, and Jennifer J. Todd. 2013. *Class Origins, High School Graduation, and College Entry in the United States*. *Determined to Succeed?* Stanford University Press. https://ezproxy-prd.bodleian.ox.ac.uk:2586/view/10.11126/stanford/9780804783026.001.0001/upso-9780804783026-chapter-10.

Mood, Carina. 2010. Logistic Regression: Why We Cannot Do What We Think We Can Do, and What We Can Do About It, European Sociological Review, 26(1),: 67–82, https://doi.org/10.1093/esr/jcp006
Moore, Rob. 2004. Education and Society: Issues and Explanations in the Sociology of Education. Oxford: Oxford University press.
Oakes, Jeannie. 1985. Keeping Track: How Schools Structure Inequality. New Haven: Yale University Press.
Oakes Jeannie. 1992. "Can tracking research inform practice? Technical, normative, and political considerations." *Educational Researcher* 21(4): 12–21.
Oaxaca, Ronald. 1973. "Male-Female Wage Differentials in Urban Labor Markets." *International Economic Review*. 14(3): 693–709.
OECD. 2014. State of Higher Education 2014. http://www.oecd.org/edu/imhe/stateofhighereducation2014.htm
OECD. 2021. Enrolment rate in secondary and tertiary education. Consulted on January 2021: https://data.oecd.org/students/enrolment-rate-in-secondary-and-tertiary-education.htm#indicator-chart
Parson, Talcott. 1970. "Equality and Inequality in Modern Society, or Social Stratification Revisited." *Sociological Inquiry* 40(2):13–72.
Pearl, Judea. 2001. "Direct and Indirect Effects." *Proceedings of the Seventeenth Conference on Uncertainty in Artificial Intelligence*: 411-420.
Pearl, Judea. 2009. "Causality". Cambridge UK: Cambridge University Press.
Pfeffer, Fabian.T. 2008. "Persistent Inequality in Educational Attainment and its Institutional Context." *European Sociological Review* 25(5): 543-565.
Pfeffer, Fabian T. 2015. "Equality and Quality in Education. A Comparative Study of 19 Countries." *Social Science Research* 51:350–68.
Piopiunik, M. 2014. "The Effects of Early Tracking on Student Performance: Evidence from a School Reform in Bavaria." *Economics of Education Review* 42: 12–33.
Raftery, Adrian E., and Michael Hout. 1993. "Maximally Maintained Inequality: Expansion, Reform and Opportunity in Irish Education." *Sociology of Education* 66: 41–62.
Robins, James M., and Sander Greenland. 1992. "Identifiability and Exchangeability for Direct and Indirect Effects." *Epidemiology* 3 (2): 143–55.
Robins, James M., Miguel Ángel Hernán, and Babette Brumback. 2000. "Marginal Structural Models and Causal Inference in Epidemiology:" *Epidemiology* 11 (5): 550–60. https://doi.org/10.1097/00001648-200009000-00011.
Robbins, Charles B. (1963). "Higher Education Report." http://www.educationengland.org.uk/documents/robbins/robbins1963.html
Roller, Marcus, and Daniel Steinberg. 2020. "The Distributional Effects of Early School Stratification-Non-Parametric Evidence from Germany." *European Economic Review* 125:103422.
Roth, Bettina, Nicolas Becker, Sara Romeyke, Sarah Schäfer, Florian Domnick, and Frank M. Spinath. 2015. "Intelligence and School Grades: A Meta-Analysis." *Intelligence* 53 (November): 118–37. https://doi.org/10.1016/j.intell.2015.09.002.
Rudolphi, Frida. 2011. "Inequality in Educational Outcomes : How Aspirations, Performance, and Choice Shape School Careers in Sweden." http://urn.kb.se/resolve?urn=urn:nbn:se:su:diva-63213.
Rudolphi, Frida, and Robert Erikson. 2016. "Social Selection in Formal and Informal Tracking in Sweden." Models of Secondary Education and Social Inequality, November. https://www.elgaronline.com/view/edcoll/9781785367250/9781785367250.00020.xml.

Ruhose, Jens, and Guido Schwerdt. 2016. “Does Early Educational Tracking Increase Migrant-Native Achievement Gaps? Differences-in-Differences Evidence across Countries.” *Economics of Education Review* 52:134–54.
Sacerdote, Bruce. 2011. “Peer effects in education: How might they work, how big are they and how much do we know thus far?” In *Handbook of the Economics of Education*, Vol. 3, edited by Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, 249-277. Amsterdam: North Holland.
Schütz, Gabriela, Heinrich W. Ursprung, and Ludger Wößmann. 2008. “Education Policy and Equality of Opportunity.” *Kyklos* 61(2): 279–308.
Sobel, Michael. E. 2008. Identification of Causal Parameters in Randomized Studies With Mediating Variables. *Journal of Educational and Behavioral Statistics* 33(2): 230–251. https://doi.org/10.3102/1076998607307239
Sørensen, Aage B., and Maureen T. Hallinan. 1977. “A Reconceptualization of School Effects.” *Sociology of Education* 50: 273–289.
Sullivan, Alice, Samantha Parsons, Richard Wiggins, Anthony Heath, and Francis Green. 2014. “Social Origins, School Type and Higher Education Destinations.” *Oxford Review of Education* 40 (6): 739–63. https://doi.org/10.1080/03054985.2014.979015.
Tchetgen Tchetgen, Eric J., and Tyler J. VanderWeele. 2014. “On Identification of Natural Direct Effects When a Confounder of the Mediator Is Directly Affected by Exposure.” *Epidemiology* 25 (2): 282–91. https://doi.org/10.1097/EDE.0000000000000054.
Treiman, Donald J. 1970. “Industrialization and Social Stratification.” *Sociological Inquiry* 40 (2): 207–34. https://doi.org/10.1111/j.1475-682X.1970.tb01009.x.
Terrin, Éder, and Moris Triventi. 2022. “The Effect of School Tracking on Student Achievement and Inequality: A Meta-Analysis.” *Review of Educational Research*, 93(2), 236–274. doi: 10.3102/00346543221100850.
UNESCO. 2015. “Education for All 2000–2015: Achievements and Challenges.” EFA Global Monitoring Report.
Valeri, Linda, and Tyler J. VanderWeele. 2013. “Mediation Analysis Allowing for Exposure–Mediator Interactions and Causal Interpretation: Theoretical Assumptions and Implementation with SAS and SPSS Macros.” *Psychological Methods* 18 (2): 137. https://doi.org/10.1037/a0031034.
VanderWeele, Tyler J. 2015. “Explanation in Causal Inference: Methods for Mediation and Interaction”, Oxford: Oxford University Press.
van de Werfhorst, Herman G., and Saskia Hofstede. 2007. “Cultural Capital or Relative Risk Aversion? Two Mechanisms for Educational Inequality Compared.” The British Journal of Sociology 58(3):391–415. doi: 10.1111/j.1468-4446.2007.00157.x.
van de Werfhorst, Herman. G., and Ruud Luijkx. 2010. “Educational Field of Study and Social Mobility: Disaggregating Social Origin and Education.” *Sociology*, 44(4): 695-715. https://doi.org/10.1177/0038038510369362.
van de Werfhorst, Herman G. and J.J.B. Mijs. 2010. “Achievement inequality and the institutional structure of educational systems: A comparative perspective.” *Annual Review of Sociology* 36: 407–28.
van de Werfhorst, Herman G. 2018. “Early Tracking and Socioeconomic Inequality in Academic Achievement: Studying reforms in Nine Countries.” *Research in Social Stratification and Mobility* 58: 22–32.
van de Werfhorst, Herman G. 2019. “Early Tracking and Social Inequality in Educational Attainment: Educational Reforms in 21 European Countries.” *American Journal of Education* 126(1):65–99.

van de Werfhorst, Herman G., and Saskia Hofstede. 2007. “Cultural Capital or Relative Risk Aversion? Two Mechanisms for Educational Inequality Compared.” The British Journal of Sociology 58(3):391–415. doi: 10.1111/j.1468-4446.2007.00157.x.
van Houtte, Mieke. 2016. “Lower-Track Students” Sense of Academic Futility: Selection or Effect?” *Journal of Sociology* 52 (4): 874–889.
Weiß, Rudolf. 2006. “Grundintelligenztest Skala 2—Revision.” *(CFT 20-R) [Culture Fair Intelligence Test 20-R—Scale 2]*.
Willis, Paul 1981. “Learning to Labor: How Working Class Kids Get Working Class Jobs.” New York, NY: Columbia University Press. ISBN 0-231-05357-6.
Winship, Christopher, and Sanders Korenman. 1997. “Does Staying in School Make You Smarter? The Effect of Education on IQ in The Bell Curve.” In Intelligence, Genes, and Success, edited by Bernie Devlin, Stephen E. Fienberg, Daniel P. Resnick, and Kathryn Roeder, 215–34. New York, NY: Springer New York. https://doi.org/10.1007/978-1-4612-0669-9_10.
Winship, Christopher, and Stephen L. Morgan, eds. 2014. “In Counterfactuals and Causal Inference: Methods and Principles for Social Research, 2nd ed., i–vi.” Analytical Methods for Social Research. Cambridge: Cambridge University Press. https://www.cambridge.org/core/books/counterfactuals-and-causal-inference/frontmatter/E57786B9642F0FB7E0AE31BC9CA31EBD.
Wright, Sewall. 1934. “The Method of Path Coefficients.” *The Annals of Mathematical Statistics* 5 (3): 161–215.
Young, Michael. 1958. “The Rise of the Meritocracy 1870–2033: An essay on education and society.” London: Thames and Hudson.
Zimmer, Ron. 2003. “A new Twist in the Educational Tracking Debate.” *Economics of Education Review* 22: 307–315.