University of Bern
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Inquiring Educational Inequalities: Perspectives on Measuring Social Origin and Mechanisms Beyond Inheritance

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Tobias Ackermann
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Abstract

The four contributions of this thesis centre around the issue of educational inequalities and questions pertinent to measuring the role of social origin and other ascriptive factors. The first two papers target the question surrounding the conceptualisation and the measurement of social origin in contemporary research, while the second two papers address mitigating factors beyond the realm of the social background.

By employing record linkages to two waves of a national Large-Scale Assessment, the results from the first paper indicate that students with lower cognitive abilities, have a higher likelihood of non-response and measurement error regarding questions about their social origin, raising questions about multifaceted measurement error when analysing data from large-scale assessment studies. The second paper utilises administrative data on parental earnings to explain variance in student performance. The results suggest an independent effect of parental earnings on student performance, but only if the selectivity of the sample in complete case analyses is accounted for. In addition, it shows that administrative data holds the advantage of obtaining information on individuals even when they did not participate in the survey, which can be used, for instance, for calculating weights or imputation models.

Using panel data, the third paper tells the story of how educational tracks in lower and upper secondary education in Switzerland are linked to the formation and revision of realistic educational aspirations. While track placement is found to be important for the formation and the revision of aspirations, social origin only accounts for their formation. The last paper investigates the persistence of relative age effects throughout compulsory education in Switzerland. Using a record linkage between the data of mandatory student assessments from the Northwestern part of Switzerland and administrative records, two identification strategies were employed. The results picture a diminishing of relative age effects throughout compulsory education, however, presumably not fast enough since they are still at play at the end of sixth grade when students are allocated to ability-based tracks.

In sum, the works of this thesis show that the conceptualisation and the use of social origin in contemporary educational research is not a close matter and still needs improvement. Furthermore, it highlights the strengths and weaknesses of using additional data sources on social origin, namely parental surveys or administrative data. Lastly, it emphasises considering mechanisms that do not directly relate to the social origin of students and pupils as potential causes for educational inequality.

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Chapter 1

Introduction

Motivation and Background

As a departing point, I want to give the reader some context on the motivations and thoughts behind the individual papers that form the main part of this thesis. In the past three years, I have worked and studied at the Interfaculty Centre for Educational Research (ICER) at the University of Bern. The main mission of the ICER is the realisation of two large-scale assessments in Switzerland, namely the Programme for International Student Assessment (PISA) and the national monitoring of the Verification of the Achievement of Basic Competencies (ÜGK). Despite the pandemic in 2020, when I started my PhD, the planning for the next wave of the ÜGK was already in motion. This particular wave of the ÜGK is the first to assess the basic competencies of eight-year-old children in the second grade of compulsory school (ÜGK H4) in Switzerland. Monitoring the achievement of basic competencies, however, requires not only the testing of the pupils in specific domains but also obtaining valid information on the children's social background. The latter was considered a huge issue because the ÜGK relied on student self-reports to gather information on their social background. Therefore, the search for potential solutions to this problem began. One promising way to get this information was to use data on the children and parents from national registers and surveys. As a first step, I began to establish a record linkage between the administrative data and the ÜGK from 2017 and 2016, which took almost a year until the data was ready. Several questions were addressed in this first project. From a practical point of view, the interest lay in whether the administrative data would contain the key information that could substitute the background questionnaire - e.g., information on parental education, occupational status, or the migration background of the child. From a scientific point of view, it was interesting to investigate which groups of students have more difficulties answering questions about their social origin - which would manifest as measurement error or item non-response and could bias the results from analyses. The outcomes of this investigation are presented in the first paper of this dissertation.

During the cognitive pretesting and while the pilot of the ÜGK H4 was programmed, one major goal was to improve the record linkage process so that it could be used for the main study. During this process, I finally established a linkage to the pilot of the ÜGK H4, which was also accompanied by a background questionnaire for the parents. As the ÜGK does not measure the financial situation of the family as part of the social background, I was intrigued to use the potential of the administrative data from the Central Compensation Office (CCO), which gathers information on earnings that are subject to mandatory contributions to the old age pension in Switzerland. Hence, I was able to test whether parental earnings explain variation in student performance even after controlling for the commonly assessed social background characteristics in the ÜGK. Furthermore, the relatively good coverage of the CCO data made it possible also to evaluate whether the participation in the parental survey accompanying the ÜGK H4 pilot was selective in terms of student performance, migration background, and parental earnings. Both questions are addressed in the second paper of this thesis.

While the two first papers mainly addressed survey-methodological issues concerning the measurement of social origin and the validity of self-reports by pupils, the third paper pursued a (rather) classical research approach. Following a call for papers celebrating the 20th anniversary of the TREE study, a research project was launched to examine educational aspirations from a longitudinal perspective. The interest of this project lies in whether realistic educational aspirations are revisited upon leaving compulsory education and whether the pursued track in upper secondary education can account for these changes, as they form clear and distinct ability signals. The investigation of the influence of educational tracks on the revision of educational aspirations is presented in the third paper of this thesis published in the Swiss Journal of Sociology, 49 (2) 2023, and was presented at the 12th International Conference of Panel Data Users in Switzerland (2023) in Lausanne as well as at the Society for Longitudinal and Lifecourse Studies Annual Conference (2023) in Munich and the congress of the Schweizerische Gesellschaft für Bildungsforschung (2023) in Zurich.

During my PhD, I also participated in and completed the PhD programme from the Leading House VPET-ECON from the University of Zurich. The programme introduced contemporary econometric approaches for research in the field of economics of education as well as up-to-date empirical findings. Inspired by the econometric methods and the experiences from the previous record linkages, a fourth project was started which combines both approaches to answer a research question in the field of sociology of education. This project aimed to examine whether the age at which students enter school affects student performance and, much more interestingly, how persistent this relative age (dis-)advantage is throughout compulsory education in Switzerland. Since 2015, four cantons of the North-Western part of Switzerland have conducted mandatory assessments in the third, fifth/sixth, eighth and ninth grades available for scientific use - the CHECKS BRNW. However, the data can only be used in combination with a record linkage, as not even the exact birth dates of the students are available in the data. Luckily, we were able to link all cohorts since 2015 to administrative records on the students and their parents which created a rich data set to answer the research question. With this data set, two different identification strategies were applied, namely a regression discontinuity design and an instrumental variable approach, to investigate the persistence of the relative age effect. This fourth paper was presented at the European Conference on Educational Research in 2023 in Glasgow.

Structure of the Thesis

After this brief introduction on the motivations and the background in which the four papers that constitute this thesis came into being, the structure of the thesis should be clarified. Chapter 2 discusses conceptual considerations that will form the overarching framework of this thesis. Thus, key concepts are discussed which are later used to stress the motivation and the contribution of each of the four papers presented in this thesis. It introduces the reader to the societal function of education and points toward the unequal distribution of educational opportunities. Chapter 2 also discusses the role of how social origin is measured and represented in contemporary surveys used for educational research.

Chapter 3 then presents each of the individual papers to the reader and embeds them in the framework established in Chapter 2. Chapter 4 consists of a discussion of the findings and will offer an outlook on future work. It critically examines the achievements and contributions of the individual papers while highlighting open questions and challenges.

The last parts of this thesis are embodied by the individual papers which are each preceded by an abstract followed by the main text and the appendix.

Chapter 2

Conceptional Framework on Social Origin and Educational Outcomes

Since Coleman (1966) the relationship between educational outcomes and social origin has been a vivid research interest in the social sciences. Many studies have repeatedly demonstrated that individuals from less fortunate social backgrounds tend to have less favourable educational outcomes - e.g. (OECD, 2019). This is a pressing issue, considering that education is a key component for success in other domains of life. Furthermore, it also highlights that the promise of equality and meritocracy, two core features of the design of modern educational systems, has yet to be realised (Bills, 2019).

These unequal opportunities and outcomes have a long-lasting impact on many domains of life - such as job-market opportunities or health outcomes, which will be discussed in the following section to stress the importance of education in modern societies. The second section takes a deeper look at how social origin and the unequal distribution of educational opportunities can be linked together. It further critically discusses how contemporary research conceptualises dimensions of social origin. Section three looks closer at the current state of available data in educational research and considers its strengths and weaknesses, especially in the way social origin is measured. As three papers of this thesis rely on record linkages, one part of this last section also considers the opportunities and limitations that come with the possibilities granted by additional data sources, followed by a conclusion that clarifies the conceptual framework.

The Importance of Education

Post-industrial societies have all faced similar developments that have contributed to the increased importance of education in recent decades. First, technological change has caused a rise in the demand for skilled labour (P. N. Blossfeld, 2018). The increased demand for skilled labour, however, creates problems for individuals who leave the compulsory education system with poor or no educational credentials or with low competencies (Müller and Jacob, 2008). Hence, acquiring the skills and competencies necessary to fulfil labour market needs during compulsory schooling is an important asset for future life outcomes.

Second, the rapid development in and the now daily use of information and communication technology (ICT) demands that individuals adapt to and learn to apply this technology (Bejaković and Mrnjavac, 2020; Grusky and Hill, 2018). These quick developments, but also the competition for jobs in a globalised world ask for constant improvement and new skills (G. J. Blossfeld, P. N. Blossfeld, and H.-P. Blossfeld, 2019). In turn, self-regulated learning but also the general openness to learning over the life course has become even more essential (Bratsberg, Nyen, and Raaum, 2020; P. N. Blossfeld, 2018). Lastly, these post-industrial societies all face demographic changes as fertility declines and the average lifespan grows, which poses new challenges to the education system as well as the labour market (Grusky and Hill, 2018).

These macro-structural trends demand that the education system but also individuals adapt to the new situation. For example, compulsory schools should be designed so that pupils learn and acquire the much-needed skills demanded by the labour market. In the meantime, individuals should be eager and willing to adapt to new technology and lifelong learning beyond the mandatory part of education. If successful, individuals with a good education tend to have a lower risk of poverty (Hofmarcher, 2021) or unemployment (Lahtinen, Sirniö, and Martikainen, 2020), better health (Zajacova and Lawrence, 2018), and a longer (Mackenbach et al., 2019) and happier life (Ilies et al., 2019; OECD, 2022). All benefits are also desirable from a societal perspective as less poverty relieves the burden on social insurance, or a healthier population does the same for the health care system (OECD, 2022). Hence, from an individual perspective, education can be seen as a gatekeeper to a happy life and, from a social perspective, as a key factor contributing to the stratification of modern societies and the externalities that come with it, which are relevant for policymakers.

The more worrisome it is that studies still find persistent educational inequalities regarding social origin (G. J. Blossfeld, P. N. Blossfeld, and H.-P. Blossfeld, 2019). Although empirical evidence points toward a decline in the educational opportunities that are inherited from generation to generation, at least for Western civilisations in the post-World War II period (Erikson, 2019), the correlation between social origin and educational attainment remains quintessential to educational research. Features that are ascribed thus tend to determine - at least to a considerable extent - educational opportunities (Erikson, 2016) although there is considerable controversy by which mechanisms these inequalities are maintained (Jackson, 2013). Unsurprisingly, one major aim of educational monitoring programmes is to quantify the degree to which social origin contributes to inequalities in the education system. Empirical research, however, goes beyond descriptives and tries to explain different educational outcomes by social origin and unveil the causal mechanisms that link them together.

In sum, education plays a central role in modern societies. Individuals benefit from education as well as society as a whole. In an ever-changing world, the demand for individuals to adapt throughout their lives becomes more immanent, while the cornerstones for this endeavour to succeed are laid early in life (e-g-, Cunha and Heckman, 2007). Different points of departure by social origin can foster disparities and unequal opportunities later in life and even strengthen the stratification of society. Therefore, the next section takes a closer look at how social origin and educational outcomes are related.

Social Origin and the Unequal Distribution of Educational Opportunities

Analysing inequalities of educational opportunities and their persistence by social origin first requires conceptual clarifications. However, although the term social origin frequently is used, it needs some clarification. In general, social origin represents the allocation of an individual in the multidimensional space of social stratification (Erikson, 2019). Examples of these dimensions include parental education, income, or social status (ibid., Willms and

Tramonte, 2019) Put together, these dimensions represent distinct resource patterns available to children which are defined by the inheritance of their parents. In contemporary research, next to using only parental education or t classification of occupational status, one common way to represent social origin is by socioeconomic status (SES), which generally resembles a combined measure considering information on parental education, parental occupational status, and a measure of the financial situation of the family (Avvisati, 2020). However, other authors do not recommend combining the dimensions into one scale (Ensminger and Fothergill, 2003).

But why are these dimensions, education, class and status, and the financial situation, so important for educational outcomes? In short, they all represent resources that can be used to support a child's education. For example, financially more affluent families hold the greater potential to pay for extracurricular activities and support outside of school (Heath et al., 2022). Highly educated parents have more opportunities to support their children in a variety of aspects regarding schooling and might furthermore hold different beliefs and expectations about their child's education (Boneva and Rauh, 2018; Guryan, Hurst, and Kearney, 2008; Spera, Wentzel, and Matto, 2009). Parents with higher (occupational) status can promote their children to elite occupations as they hold privileged resources, e.g., specific social networks, that grant access to otherwise closed social circles (Friedman and Laurison, 2020).

In a controversial statement, Lazarsfeld (1939) claimed that these different dimensions could be interchanged. While the possibility of one latent factor representing the position in the stratified social space cannot entirely be ruled out, there is a lot of evidence pointing towards the opposite direction. For example, Erikson (2016) shows based on a random sample of Swedish school children at the age of 13 combined with registry data that all dimensions of social origin under investigation had independent effects on the level of education as well as cognitive abilities at age 13. Furthermore, Erikson (2016) highlights that a part of the variation in the level of education is transmitted via the cognitive abilities of the children. Hence, looking at social origin in a multidimensional view is a promising way to disentangle the mechanisms by which the dimensions, and the accompanying resources, of social origin affect different educational outcomes (Ensminger and Fothergill, 2003).

If considering the mechanisms by which social origin affects educational outcomes, the work of Erikson (2016) points in two directions that were already described in the seminal work by Boudon (1974). On the one hand, social origin can affect cognitive abilities or at least student performance, which translates into later educational attainment. Boudon (1974) framed these differences in abilities by social background as primary effects of social origin. On the other hand, educational attainment later in life is still tied to social origins beyond the part that can be explained by differences in abilities. Boudon argued that this connection is mainly subject to different choices made during the educational process that can be explained by the social background, which he then defined as secondary effects of social origin. Hence, it is important to understand when and how educational outcomes depend on social origin.

For the primary effects of social origin, one can argue that individuals from less privileged social backgrounds have a different point of departure when starting school. Less privileged children grow up in a less favourable developmental environment where fewer educational skills are transferred. And if skill begets skills (Cunha and Heckman, 2007), these early disparities between pupils from distinct social origins can even grow over time - ultimately resulting in unequal educational attainment stratified by social origin. Many educational programs that target children at an early age have been shown to have superior effectiveness in reducing inequalities by social origin (e.g., the High/Scope Perry Preschool Program; C. R. Belfield et al., 2006).

This mechanism of cumulative advantages postulates that students who are more ready to learn or skilled are also more likely to profit from schooling and thus end up with a steeper learning curve advantaging them in the next educational step (Merton, 1968). However, other mechanisms could be involved as well and operate in parallel. For example, students (from privileged backgrounds) who are better equipped to learn in school due to the more favourable developmental environment in their parental household, might be perceived differently by their teachers and thus get preferential treatment by being more challenged or supported (Rosenthal and Jacobson, 1968). In turn, differential treatment by teachers would also foster educational disparities in addition to the paradigm of skill begetting skill. Furthermore, residential segregation (Boterman et al., 2019) and school choice (Yang Hansen and Gustafsson, 2016) might be another mechanism by which the differences in skills by social origin might be explained as more affluent neighbourhoods tend to have better funding, teachers, and preferential classroom composition.

Educational systems and their institutional representation define the framework in which secondary effects of social origin unfold. This becomes evident considering the pivotal points of transition from one educational stage to another, where parents and students are forced to decide between alternative educational pathways (Erikson, 2019). Students from different origins, while performing equally, tend to make distinct decisions regarding the pursuit of education. In many countries, this transition is marked at the end of compulsory education when students proceed to upper secondary education (ibid.). However, some education systems, such as the one in Switzerland, are characterised by an early transition from primary compulsory school to a tracked lower secondary system where students are allocated by ability (M. Buchmann et al., 2016). While different choices regarding upper secondary education are reported in both types of education systems, education systems where students are allocated to different tracks at an earlier age might enhance the ways by which social origin affects educational outcomes beyond the disparities due to the primary effects of social origin (M. Buchmann et al., 2016; Biewen and Tapalaga, 2017; Maaz et al., 2008).

But how does social origin influence decision-making? Ability sorting and grades represent a signal to parents and students alike (Karlson, 2015). Based on this information, one can expect them to make educational decisions. One approach to conceptualise how these decisions are made is the rational choice theory (RCT), which states that individuals favour the option that holds the most expected subjective utility (Breen and Goldthorpe, 1997; Esser, 1999) and is often used in the sociological literature as paradigm to explain educational choices (Holm, Hjorth-Trolle, and Jæger, 2019). Individuals are thought to evaluate the utility, probability of success and costs for each of the available options and choose one alternative holding the highest subjective expected utility. However, information asymmetries that arise from social origin would potentially account for differences in educational choices (Abbiati and Barone, 2017; Barone et al., 2017; Kretschmer, 2019), despite similar abilities. Furthermore, individuals with scarcer resources might be more attentive to cost and failure and thus form distinct personality traits such as risk aversion (or patience) which partially explain the choice of higher education pathways and thus lead to more years of schooling (Almlund et al., 2011). Choices of the track or whether or not to pursue higher education are subject to more than the ability signals represented by grades and offer ways by which social origin can account for the evident divergence of educational attainment by social background and beyond.

Both primary and secondary effects of social origin, have to be combined when thinking about how social origin affects the unequal educational outcomes by social background. Resources that can be allocated towards education might boost a child's early skills and readiness for learning, which can result in a steeper learning curve and thus performance that translates into grades and recommendations for track placement. Under the assumption of equal ability, students from disadvantaged social backgrounds might hold different beliefs about the return of education (C. Belfield et al., 2020) or have evolved specific personality traits Almlund et al., 2011 that alter the way grades and recommendations, and thus educational options, are evaluated. The combination of both results in distinct patterns of educational attainment by social origin. However, sticking to primary and secondary effects of social origin as determinants for educational inequalities is too short-sighted as other factors affect educational success, as there are multiple social contexts which all have their independent effects (Hillmert, 2019). Namely, these other contexts mean the peers (Sacerdote, 2011), effects resulting from the organisation of the schools and the classrooms as well as the teachers (Moss, Kelcey, and Showers, 2014), or neighbourhood effects (Nieuwenhuis and Hooimeijer, 2016).

Modern societies need to understand by which processes social inequalities are maintained as this would open the way for targeted policy programmes to reduce inequality. Therefore, it is insufficient to solely describe the extent to which social origin determines success in adult life or performance in standardised tests. The theoretical consideration that the different dimensions of social origin, e.g. education, status, and wealth, resemble distinct resources that come into play by different mechanisms for specific educational outcomes highlights the importance of future research, as there are many open questions. At the same time, the question arises of how social origin is measured and which data are used as a foundation to investigate questions about the mechanisms behind the translation of social origin into educational outcomes.

Measuring Social Origin - Achievements and Challenges

As there is a clear link between social origin and educational outcomes, any study investigating the latter is almost forced to have a measure of social origin. However, there is considerable controversy on how social origin is conceptualised in contemporary research (Erikson, 2019). For once, there is still the debate about whether the different dimensions of social origin can be represented by one latent factor or if each of the dimensions represents a unique resource that affects educational outcomes through distinct mechanisms at specific points throughout a child's education (Erikson, 2016; Ensminger and Fothergill, 2003). Both views, however, lead to the question of how and which dimensions of social origin should be measured in surveys, which will be discussed in more detail below.

One of the most well-known research programmes in education, the Programme for International Student Assessment PISA, is a prime example of the (inter-) national monitoring of the education system that also assesses the link between social origin and educational performance at the end of compulsory schooling. Therefore, PISA needs some way of measuring social origin. In the most recent rounds of the PISA study social origin was measured by the index of economic, social and cultural status (ESCS), by combining the highest parental education, highest parental occupational status, and household possessions (OECD, 2019). The way PISA measures social origin in terms of the ESCS often serves as a lighthouse guiding other studies in designing their measures of social origin (Avvisati, 2020). Despite the importance of these three dimensions of social origin (Willms and Tramonte, 2019; Hällsten and Thaning, 2022), many studies on education also favour including different dimensions. The Verification of the Achievement of Basic Competencies (ÜGK), the International Computer and Information Literacy Study (ICILS), or the Educational Standard Survey (BIST-Ü) in Austria, for example, do not include information on the financial situation of a family but assess the number of books as an indicator for cultural capital (Pham, Helbling, Verner, Petrucci, et al., 2016; Pham, Helbling, Verner, and Ambrosetti, 2017).

At the same time, there seems to be somewhat of a consensus - at least from a practical point of view - on measuring parental education and occupational status. Regarding parental education, one could simply ask for the years of education of the parents. However, due to differences between countries and national policy changes, years of education are too volatile to make valid comparisons between countries or cohorts. Thus, parental education is often assessed by using the ISCED scale or similar categorisations of levels of education (UNESCO, 2012). Parental occupational status is generally measured as four-digit ISCO codes, which are then translated into the International Socio-economic Index of Occupational Status (ISEI) (Ganzeboom and Treiman, 2003). While both ISCED and ISEI almost function as a standard for measuring parental education and occupational status, the latter is also subject to critique and needs constant updates (Avvisati, 2020).

For the financial situation of a family, and the other dimensions of the social origin, there is much more variation in how it is measured, if at all. Simply measuring the annual earnings of the parents might be insufficient, as earnings are volatile (Erikson, 2019) and do not entail all financial resources available to a family (e.g., wealth Hällsten and Thaning, 2022). Therefore studies such as PISA, use household possession scales that should reflect the more permanent parts of the financial situation of a family (Marks and O'Connell, 2021). Nonetheless, household possessions change their meaning in terms of available financial resources (ibid.). For example, owning a PC might have been a sign of wealth in the early 1990s but is much less indicative today in Western civilisations. Similarly, the meaning of owning a car or living in a large apartment might be two different things when considering living in metropolitan areas or dense city centres, where average rents are much higher and owning a car is much more expensive. Hence, the cross-national and temporal comparability of household possession scales is very limited.

If there is evident doubt about the quality of the information obtained from household possession scales, why use them anyway? One answer is, that studies often rely on pupils' and students' self-reports on the characteristics of their social origin. This circumstance is challenging as there is considerable uncertainty regarding the validity of student self-reports (Marks and O'Connell, 2021). As students are potentially unaware of the concrete financial situation of a family, it might be most appropriate to ask them whether their household possesses certain items - or receives assistance in the form of lunch checks (Ensminger, Forrest, et al., 2000).

While the quality of self-reports by pupils and students on their family's financial situation is considered particularly problematic, this issue also concerns the statement about the other dimensions of social origin. Studies have repeatedly shown that especially students with lower student performance are also more likely to give erroneous information or are more likely to give no answers (Ensminger, Forrest, et al., 2000; Kreuter et al., 2010; Engzell and Jonsson, 2015). Studies that do not recognise differential measurement error or do not account for the selectivity in complete case analysis thus face the potential of reporting biased estimates on the relationship between the dimensions of social origin and the educational outcomes under investigation (Hovestadt and Schneider, 2021; Carroll et al., 2006).

To overcome this issue of self-reports using or collecting additional data sources would be a promising approach. For example, studies can try to obtain this information directly from the parents or - if available - use registry data. In the first case, the information should be less biased as it is gathered from the parents themselves instead of the students. However, the question arises about which parents participate in such questionnaires and whether this could introduce selectivity, which, in turn, again biases the estimates from analyses. Even if the participation is not selective, the declining response rate in surveys (Luiten, Hox, and Leeuw, 2020) shows that the endeavour of an additional survey for parents could be an inefficient and costly undertaking. In the case of administrative records, the data is already gathered and mostly free of charge when used for scientific projects, hence, they represent an efficient way to gather data for various studies. Furthermore, one could expect to obtain externally valid information. Nonetheless, linking individuals to administrative data on their parents might come with important limitations.

From a practical point of view, record linkages with administrative data are not always possible as not all countries' national registers provide such a service. Further, record linkages might be subject to national regulations that prohibit the possibility of sharing the data, which would jeopardise the reproducibility of the research or the use of the data by other researchers in general. In addition, the potential of administrative data to overcome the issue of self-reports is dependent on the current state of the information in the administrative records. While (Erikson, 2016) was able to get information on four different dimensions of social origin from Swedish register data, one has to mention that this is more or less an exception. For Switzerland, for example, information on education and occupational status can only be obtained via a national survey. This substantially restricts the sample for which this information is available and thus limits the potential of administrative data to substitute survey questions in educational research.

In brief, it can be stated that obtaining a valid measure of social origin is a challenging undertaking. Independent whether a single scale is used as depicted by PISA's ESCS or each dimension of the social origin is included individually in the analysis, researchers should always be aware of the limitations posed by the ways the information is produced. On the one hand, there are practical considerations that promote student and pupil self-reports although they have potential constraints in terms of measurement error and item non-response. On the other hand, linking student data to additional data sources, be it a parental survey or administrative records, has its downsides and restrictions. On the one hand, parental surveys provide information that can mitigate claims about its validity but open the potential for selectivity. On the other hand, administrative data also have potentially higher validity, but the current state does not allow for the assessment of the social origin. Furthermore, the use of administrative data poses a risk to the open science framework as it is often prohibited to share linked data. Lastly, the current state of research shows a variety of ways in which social origin is operationalised for answering research questions. The spectrum reaches from studies that only use one dimension, e.g., parental education, to control for social origin (Sirin, 2005), over studies that use a more multidimensional approach, to research which combines the dimensions to a single indicator. This highlights that the theoretical debate about social origin and its relation to educational outcomes is far from over.

Summary of the Conceptual Framework

This section summarises the conceptual framework of this thesis.

From empirical evidence, it is known that education plays a central role in outcomes in life such as the risk of poverty or good health (cf. Hofmarcher, 2021; Lahtinen, Sirniö, and Martikainen, 2020; Zajacova and Lawrence, 2018; Ilies et al., 2019). Furthermore, it is a well-known fact that education is subject to inequalities that arise from social origin (Erikson, 2019). A major concern, which is also of interest to policymakers, is thus to disentangle the mechanisms by which social origin affects educational opportunities and hence contributes to the persistence of social inequalities.

From a theoretical perspective, social origin means the social position, which is tied to the availability of resources, or the lack thereof. In terms of educational research, the social origin of a child thus defines the resources in a family which potentially can be allocated towards the promotion of the education of the child. Traditionally, social origin consists of parental education, parental occupational status, and the financial situation of a family (Willms and Tramonte, 2019). Although some scholars argue that these dimensions are interchangeable, this thesis follows the argumentation that each of these dimensions has distinct mechanisms by which they affect different educational outcomes (Erikson, 2016). For example, financially better-situated families are much more likely to afford extracurricular education (Heath et al., 2022) or to allocate their children to schools with better funding or classroom composition (Boterman et al., 2019). In contrast, parents with better education might hold different beliefs about the return to education or are more capable of supporting their children regarding schooling (Boneva and Rauh, 2018; Erikson, 2019).

The seminal work of Boudon (1974) opened up this debate on the mechanisms by which social origin affects educational outcomes. On the one hand, his construct of primary effects states that student performance and cognitive abilities are affected by social origin. On the other hand, his conception of secondary effects proclaims that students, and their parents, make distinct educational decisions based on their social background, which can be embedded in the framework of rational choice theory (Breen and Goldthorpe, 1997; Esser, 1999). Educational choices are often made when transitioning from compulsory education to (upper) secondary education, although some education systems know ability-based tracking during primary education, for example Switzerland. However, considering only primary and secondary effects as mechanisms might result in a too simple causal model (S. L. Morgan, Spiller, and Todd, 2013). The strength of this model is thus that it highlights the manifold mediating factors that need to be understood in terms of comprehending the persistence of educational inequalities. Some factors might even fall outside of what is defined by social origin as multiple social contexts affect educational outcomes.

Lastly, it needs to be addressed that measuring social origin is a critical endeavour. Most data that is used for educational research holds information on the social origin of the participants. The information in the data, although professing to measure the same underlying construct, is highly versatile in terms of which and how many dimensions of the social background are available or whether or not they are combined into a single factor (S. L. Morgan, Spiller, and Todd, 2013; Erikson, 2019). Furthermore, the way this data is obtained - mainly by students' and pupils' self-reports - is criticised (Marks and O'Connell, 2021; Kreuter et al., 2010). Hence, there is a trend in using alternatives to measure and collect valid data on social origin, which have to be evaluated in terms of their strengths and weaknesses.

In conclusion, to understand and disentangle the mechanisms by which social origin contributes to educational inequality on a social scale, one needs to consider the multi-dimensionality of the social background. Furthermore, one needs to acknowledge that there are many possibilities for mitigating factors, especially during educational transitions, that can potentially affect educational inequalities beyond the scope of social origin. In addition, it is essential that studies use adequate measures of the social background to obtain valid results on the relationship between social origin and educational outcomes. Due to the issues that come along with self-reports a promising way could mean to use different data which can mitigate this criticism. This thesis tries to contribute to both the understanding of the mechanisms that promote educational inequalities and the improvement of the measurement of social origin. Therefore, the next section will embed the four individual papers in the conceptual framework presented in this section.

Chapter 3

Contributions to the Understanding of Educational Inequalities

This chapter presents the four individual papers which all aim at improving the understanding of educational inequalities. The first two contributions focus more on the methodological part of how social origin is measured and conceptualised in Large-Scale Assessments (LSA) - although the issue of measuring social origin also applies to other types of studies in educational research. Furthermore, they both provide insights into the use of administrative records, which is a novel approach for educational research in Switzerland. The second two papers are directed more towards potential mechanisms that can account for educational inequalities. However, they both consider mechanisms which are not directly linked to the social origin of students and therefore open up the discussion on mitigating factors for the persistence of educational inequality.

Student Self-Reports

Many LSAs rely on student self-reports to obtain information on their social origin. The problem is, that these self-reports are subject to measurement error and item non-response (Engzell and Jonsson, 2015; Kreuter et al., 2010; Ensminger, Forrest, et al., 2000; Hovestadt and Schneider, 2021; Jerrim and Micklewright, 2014). Analyses that do not account for these circumstances are endangered of misreporting the relationship between social origin and student performance (Carroll et al., 2006). Such misreporting can have severe consequences, as LSAs are used to monitor the state of education systems and to derive adequate policy measures (Prenzel and Sälzer, 2019). Therefore, it is important to understand which groups of students are more likely to make erroneous statements or to give no answers to certain questions.

In the first paper, I used data from the Verification of the Achievement of Basic Competencies (ÜGK) from the years 2016 and 2017 (Nidegger, Petrucci, et al., 2019; Nidegger, Roos, et al., 2019), a national LSA in Switzerland, and linked them to administrative records of the children and their parents. This allowed for a comparison of the self-reports of the students to more credible data sources on parental education, parental occupational status, and migration background. The main interest lay in whether previous findings could be reproduced, which show that lower student performance is also associated with a higher likelihood of measurement error and item non-response. Furthermore, as the questions are asked about mothers and fathers separately, the study controls whether or not the absence of the parent in question affects students' answers. Additionally, the hierarchical linear models control for a role model effect and characteristics of social origin, namely migration background, language at home, parental education and occupational status.

In line with previous findings (e.g., Kreuter et al., 2010), the results indicate that higher student performance is associated with a lower likelihood of measurement error as well as a lower likelihood of non-response. This key finding stresses, that self-reports open the possibility for differential measurement error (Carroll et al., 2006) and that samples from complete case analysis are likely to be selective. Furthermore, the models indicate that the absence of the parent in question, for instance, because the parent has deceased or lives in a different household, raises the chances for both dependent variables as is the case when the student does not speak the test language at home.

Nonetheless, there are also clear distinctions between the mechanisms that account for measurement error and item non-response. The models reveal that male students have a higher likelihood of item non-response than female students, while gender does not affect measurement error. Interestingly, the models also suggest that the likelihood of non-response decreases when the student has the same sex as the parent in question, which could be explained by the role model this parent represents for the students. No such effect could be identified for measurement error.

On the one hand, the results of the study highlight that student self-reports bear the potential to bias results from regression analyses because of the relationship between student performance and measurement error. If, for example, students with lower student performance would systematically underestimate their SES the relationship between SES and student performance would be overestimated. On the other hand, the findings demonstrate that household characteristics are essential for obtaining valid information. In line with the argumentation of Tourangeau, Rips, and Rasinski (2000), which states that to answer a question one needs cognitive capabilities and a cognitive representation of the construct in question, the absence of a parent affects both, measurement error and item non-response. From a practical point of view, the results also suggest that the language spoken at home should be considered an important feature which should be given more attention when designing student background questionnaires for LSAs.

Certainly, there are also limitations to this study. First and foremost, one has to mention that the coverage of information on parental education and parental occupational status is poor, which causes many cases to drop out of the analysis regarding measurement error. To overcome this issue, the data source that holds this information was pooled over the five years before the respective ÜGK wave. However, this opens up the potential that the identified measurement errors result from actual changes between jobs, for example, as the information in the administrative records might be outdated.

In sum, the analyses highlight the importance of giving enough attention to the measurement of social origin so that the risk of potential bias can be minimised. It further emphasises the use of administrative data, either to assess measurement error and item non-response or to utilize it to substitute certain questions in the background questionnaire of LSAs. While there is a lack of information on parental education and occupational status in Switzerland's national registries, there is also valid information with almost no missing information on migration background. Furthermore, recent trends in the Federal Statistical Office (FSO) are promising that in the future information on education will be available for almost the entire population. While future studies could benefit from these developments, contemporary research should be aware of the implications of measurement error when analysing data from students' self-reports.

Parental Earnings

Research on educational inequalities has demonstrated a persistent relationship between social origin and educational outcomes (Erikson, 2019) and identified parental education, parental occupational status, and family income as the "big 3" dimensions that constitute social origin (Willms and Tramonte, 2019). This is also visible in the composition of measures of socioeconomic status (SES), for example, the Economic, Social and Cultural Status (ESCS) applied in the Programme for International Student Assessment (PISA).

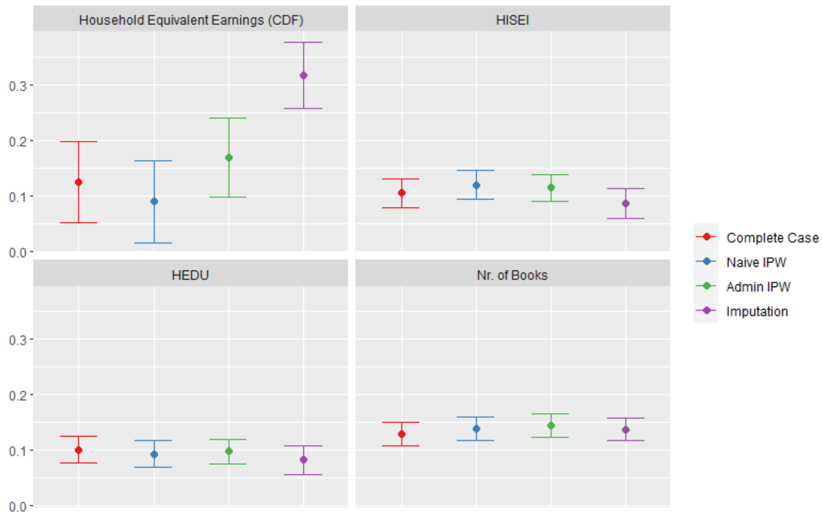
While there seems to be consensus about these core dimensions of social origin, their application in studies is very heterogeneous. Sirin (2005), for example, showed that many analyses rely solely on the dimension of parental education as information on the social origin of students. Furthermore, other LSAs like the ÜGK for Switzerland, the International Computer and Information Literacy Study (ICILS), or the EducationalStandard Survey (BIST-Ü) in Austria do not include a measure for the financial situation of a family (Pham, Helbling, Verner, Petrucci, et al., 2016; Pham, Helbling, Verner, Petrucci, et al., 2016) and thus potentially forgo an important dimension of social origin. In addition, there is much controversy about the way the financial dimension of the social background is measured. Especially considering the challenges posed by the fact that LSAs often rely on student self-reports or that they rely on household possession scales to measure income (Marks and O'Connell, 2021). Hence, it is vital to evaluate alternatives to obtain valid information on the financial dimension of a family for a better measure of social origin.

The second paper addresses this issue by using pooled data on parental earnings from national registry data, which was linked to the pilot of the ÜGK H4 study (EDK, 2024). The information on earnings comes from the Central Compensation Office, which is a federal institute that oversees the pensions and social security accounts in Switzerland and thus keeps track of the individual accounts which hold the information on annual earnings. Furthermore, the ÜGK H4 pilot was accompanied by a parental survey to obtain valid information on the social origin of the pupils, which mitigates the criticism about the validity of self-reports. With this data, it was possible to test whether the financial situation of a family has an impact on the educational performance of the children while controlling for the other dimensions of social origin.

However, as participation in the parental questionnaire was voluntary, the question arose as to whether the response to the survey was selective regarding social origin. Only thanks to a record linkage this hypothesis could be tested as it provides information on the financial situation of a family or the migration background even if the parents did not participate in the survey. Results from logistic regression revealed that the measures for parental earnings, migration status and student performance significantly explained whether the parents participated in the survey.

In the second step, regression models were fit to the data explaining student performance using complete cases. This was done three times, once with no inverse probability weights (IPW), once with naive IPWs, and once with IPWs that also used data from the administrative records to explain the inclusion in the complete case sample. The IPWs should account for the aforementioned selectivity of the parental survey that limits the sample for complete case analyses. However, as weighting only uses the information on incomplete cases to calculate IPWs, the data was also imputed, which allows to use the information of the incomplete cases for the estimation of effects (Little, Carpenter, and K. J. Lee, 2022).

Figure 1: Point Estimates of SES Variables from Different Regression Models



The comparison of the estimates from the different models is depicted in figure 1 and shows considerable differences regarding the point estimate of the variable representing parental earnings, especially in the imputed data. Furthermore, including the variable for parental earnings in the model causes a decline in effect size and significance in the models using the imputed data.

Nonetheless, the CCO data is insufficient to entirely account for the financial dimension of social origin as other financial sources contribute to what is considered permanent family income (Frick and Krell, 2010). Other authors already argue that wealth should be considered one of the ”big 4” dimensions of social origin (Hällsten and Thaning, 2022). Furthermore, this study considered earnings by the parents that were listed in the registry data. However, as family models evolve, it could be more appropriate to aggregate the information on earnings at the household level regardless of whether the adults in the household are the biological parents of the child.

In conclusion, this second paper stresses the utility of administrative records by showing their utility in analysing the selectivity of the parental survey. Furthermore, the pooled data on parental earnings has a high external validity and coverage which other registry data lack. Using data on actual earnings makes cross-country comparisons much more plausible, if one accounts for purchasing power parity, as there is vivid criticism in this direction regarding household possession scales (Marks and O’Connell, 2021). Furthermore, the analysis underlines that the financial situation of a family has an effect on student performance that is independent of other dimensions of social origin, which should be considered in future rounds of national and international LSAs.

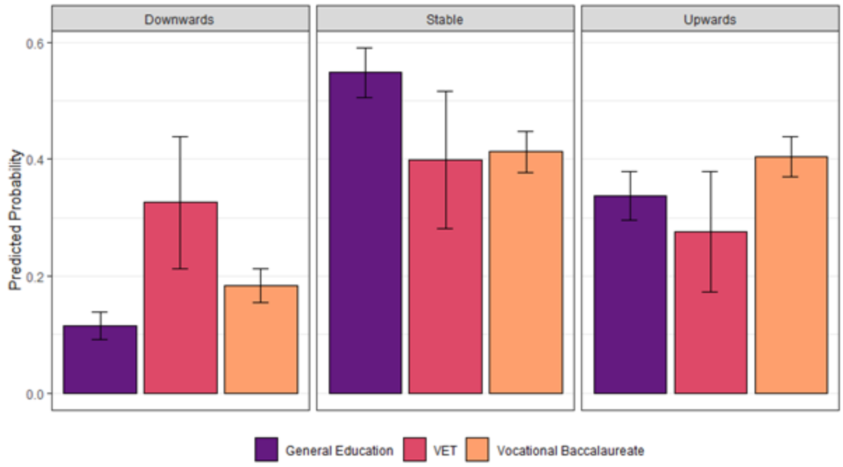
Revisions of Educaitonal Aspirations

In the literature, there is some agreement that educational aspirations predict students' educational attainment (e. g. S. Morgan, 2005; Beal and Crockett, 2010; Guo et al., 2015; Schoon and Burger, 2021. Findings from previous research give reason to believe that the context of the school is highly relevant for students to form and revise their educational aspirations, especially in tracked and stratified education systems (C. Buchmann and Park, 2009; Parker et al., 2016). The ability-based track placement, as is the case for Switzerland, conveys a strong signal about possible educational attainment in the future which students may consider when setting their educational goals (Karlson, 2015; Geven and Forster, 2021). Thus investigating the effect of tracking on educational aspirations is vital and contributes to the existing literature (e.g., Hegna, 2014; Karlson, 2015; Bittmann and Schindler, 2021).

Using the TREE2 study (TREE, 2021), which surveys the educational and occupational pathways of compulsory school-leavers in Switzerland, the third paper analyses the effect of tracking on the formation and adjustment of educational aspirations. The panel data of the study allows to observe temporal change in the dependent variable, which was the realistic educational aspirations (Haller, 1968) stated by the participants at each wave of the survey. The main variables referred to the educational track in lower and upper secondary education. Furthermore, the models controlled for the different dimensions of social origin.

In the first step, the question was whether there is any change in educational aspirations at all after students enter upper secondary education, where students in Switzerland are primarily channelled into either high-ability general education or primarily firm-based vocational education and training (VET) with varying academic requirements. The analysis revealed that a substantial number of students revise their educational aspirations after leaving compulsory school. The second step considered the formation of educational aspirations, for which random-effects ordered logistic regressions were fit to the data. The findings tell that the pursued track in lower and upper secondary education has a clear impact on what level of education is aspired. Furthermore, the results confirm the findings of previous research on the effect of social origin on educational aspirations (e.g., Roth, 2017; Gölz and Wohlkinger, 2019.

Figure 2: Effects of Track Placement on Adjustments of Educational Aspirations



In the last step, a multinomial logistic regression was used to examine whether students' educational aspirations were stable, shifted downwards, or upwards after students left compulsory school. Figure 2 shows the predicted probabilities for each type of aspiration change (stable, upward, and downward) given the track in upper secondary education. The results of the model tell that the most significant predictor for the revision of educational aspirations was the track pursued in upper secondary education, while social origin plays only a marginal role in the revision of educational aspirations.

There are important limitations to this study. First, the period after leaving the compulsory school is a very specific one and does not allow to gain insight on the formation of educational aspirations during compulsory school. Also, the data does not (yet) contain information on whether the aspirations are realised. In addition, neither can the analysis control for educational performance nor the learning environment in the upper secondary track. Lastly, it must be mentioned that the VET programmes are inherently heterogeneous and offer different opportunities, which makes it plausible that specific VET programmes correlate with the adjustment of educational aspirations.

In conclusion, the study highlights that educational aspirations are subject to considerable change over time. It thus contributes to a better understanding of educational trajectories across the life course. Furthermore, the fact that social origin plays a role in the formation of educational aspirations but not in their revision points toward the fact that there are many mitigating factors to consider when analysing educational inequalities and social origin. Lastly, the analysis illustrates that evidence from cross-sectional data should not be considered to be constant over time. Especially as educational decisions are important for the emergence of educational inequalities, cross-sectional considerations fall short of identifying potential change.

Inequalities Beyond Social Origin

Cut-off dates for school entry create systematic age differences as they cause children born right after the cut-off date to be up to a year older than their counterparts born right before the subsequent cut-off date. Due to their more advanced cognitive and psycho-social development, older children find it easier to adapt to the school environment (Black, Devereux, and Salvanes, 2011; Dhuey et al., 2019). The resulting gaps in educational performance, commonly termed as relative age effects (Bedard and Dhuey, 2006), are important as they are persistent over time (Cunha and Heckman, 2007; Skopek and Passaretta, 2021). For example, older children achieve higher test scores (e.g., Smith, 2009), have a lower likelihood of being retained (e.g., Jerrim, Lopez-Agudo, and Marcenaro-Gutierrez, 2022) and are overrepresented in demanding educational programmes at the secondary level (e.g., Ponzo and Scoppa, 2014).

The fourth paper contributes to this strand of literature on relative age effects by investigating the temporal persistence of these effects on student performance in different subjects across different grades of compulsory education in Switzerland. In doing so, this study helps to understand whether and when intervention could address the implications of relative age effects. Therefore, the study relies on test score data from Northwestern Switzerland - the Checks (NWCH, 2021), which covers the period from 2015 to 2020. The annually administered and mandatory tests take place in four cantons of Switzerland (Aargau, Basel-Landschaft, Basel-Stadt and Solothurn) and measure pupils' performance in different subjects in third, fifth/sixth, eighth and ninth grade. The case of Switzerland is interesting for investigating relative age effects as children in Switzerland enter compulsory school after turning four years old, beginning with two years of kindergarten, followed by six years of primary education (grades 1-6) and three years of lower secondary education (grades 7-9), where pupils are allocated to one of several school types that differ by academic requirements. However, the Checks provide almost no information about the pupils. Hence, data on exact birth dates and a variety of sociodemographic and household characteristics (such as sex, migration background, or parental income) had to be obtained via a record linkage to administrative data provided by Switzerland's Federal Statistical Office.

Two complementary identification strategies were applied to the data to analyse relative age effects. The regression discontinuity (RD) design (e.g., D. S. Lee and Lemieux, 2010) exploits the random variation in relative age caused by the arbitrarily cut-off dates resembling a quasi-experiment and compares pupils whose birthday lies before to those whose lies after the cut-off. The sample is limited to students who sustained a linear trajectory and complied with the enrolment regulations for the RD design.

The exclusion of pupils who did not comply with enrolment regulations or did not sustain a linear school career, however, hardly provides an inaccurate picture of the reality in schools. Furthermore, the results of the RD design may be downwardly biased as relatively young pupils who were not able to sustain a linear school trajectory because of their poor performance are excluded. Complementary to the RD design, the instrumental variable (IV) approach allows to consider these observations in the analysis.

However, a solution is needed to account for factors that may confound the observed age at school enrolment and thus the effect of relative age on school performance. For example, pupils could be relatively old because they repeated a grade while others are relatively old because they delayed school entry. Thus, the IV approach uses the assigned relative age, which refers to the age at enrolment children would have in the absence of manipulation (Bedard and Dhuey, 2006).

Figure 3: Estimates of Relative Age at School Enrolment on Test Scores across Grades

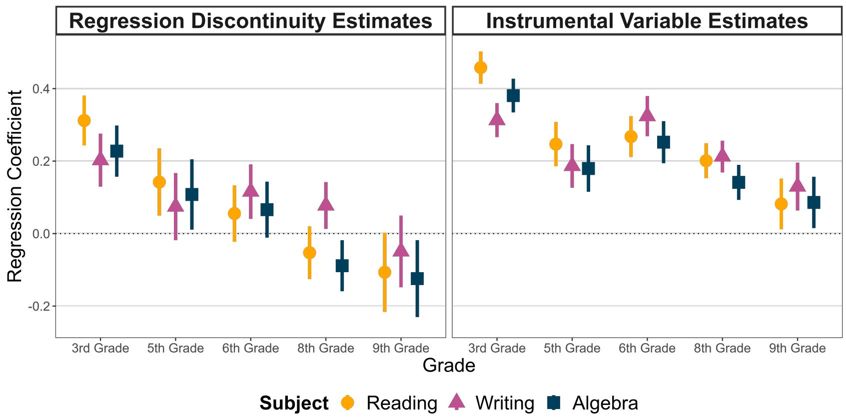


Figure 3 shows the estimates of both identification strategies across grades and subjects. Strikingly, both strategies make compelling cases that the advantages of relatively older pupils diminish over time. While the RD approach finds that children born right before the cut-off even outperform their counterparts in lower secondary education, the IV approach contradicts this finding. On the one hand, the systematic exclusion of observations could explain the exceptional findings in the RD design if the relative disadvantage for young pupils in primary school is large enough so that these pupils might be compelled to repeat a grade and thus fall out of the sample. On the other hand, using assigned relative age as an instrument for relative age is not free of methodological criticism (Barua and Lang, 2016; Fiorini and Stevens, 2021) and potentially overestimates the effect of relative age. An additional limitation of this study is then the inability to create panel-like data to observe individuals' trajectories throughout compulsory education.

Nonetheless, the combination of both identification strategies shows that time works against the relative age effect, but maybe too slowly. As relative age effects are likely still at play when teachers evaluate the pupils' performance to allocate them into ability-based tracks could result in a biased recommendation which has long-lasting effects on the children's educational attainment. Beyond the scope of social origin, institutional factors thus prove to play a crucial role in educational outcomes. In addition, this study showcases the potential of the record linkage with administrative data in the case of Switzerland. The randomness of the cut-off dates that cause systematic differences should also be considered in other educational studies such as LSAs to make the comparison between students fair. Furthermore, the findings lead the way to future research which could investigate relative age effects on track placement or outcomes later in life.

Chapter 4

Discussion

Education is undeniably important for individual lives as well as for society, which explains that it is subject to continuous research. One stream of this literature is focused on educational inequalities and how they persist over time (Erikson, 2019). The description of this relationship is also one of the motivations behind the international and national trend to monitor the education system. However, it is not enough to only describe how social origin is tied to educational outcomes but also to understand how inequality is reproduced. Traditionally, research in this area is concerned with the mechanisms that reproduce unequal educational opportunities and outcomes that are defined by social origin.

The seminal work of Boudon (1974) already points out that social origin has many fold opportunities to affect educational outcomes and thus stresses the importance of analysing a variety of mitigating factors. The ongoing debate on the conceptualisation of social origin and its dimensions furthermore highlights that each dimension of the social background (e.g., parental education, parental occupational status, and income) can all have mechanisms by which they affect educational outcomes at distinct stages and points in time (Erikson, 2016; Willms and Tramonte, 2019).

Each paper of this thesis contributes to the research on educational inequalities as they address contemporary research questions in the literature. Two of these questions concern the measurement of social origin, especially in LSAs (Avvisati, 2020; Marks and O'Connell, 2021). First, how valid is the information on social origin, especially given that most studies rely on student self-reports? Second, which dimensions of social origin are measured and how?

By comparing students' self-reports to administrative data, the first paper of this thesis was able to address the first question, It reproduced the finding of previous studies (e.g., Engzell and Jonsson, 2015; Kreuter et al., 2010; Hovestadt and Schneider, 2021 showing that students with lower cognitive abilities tend to give erroneous answers on their social background. This opens the potential for differential measurement error that could bias estimates in analyses of the relationship between social origin and student performance, (Carroll et al., 2006). Furthermore, the study highlights that the absence of a parent or the language spoken at home has an undeniable influence on whether students can give adequate answers if at all, which has potential implications for how social origin is measured in the future. At the same time, this study evaluated a potential solution to the problems that arise from student self-reports. Namely to obtain information on the social background from administrative records. However, as it became clear - at least in the case of Switzerland - the administrative data lacks much-needed information on core dimensions of social origin, which makes it impossible to use it as a substitute for background questionnaires. Furthermore, administrative data can be outdated when one needs to pool several years of a data source to have sufficient coverage.

The second paper investigated whether administrative data can be used to obtain a valid measure for the income dimension of social origin and whether this measure of parental earnings has an impact on student performance under the control of other social background characteristics. The strength of this paper is that the information on the social origin comes from a parental questionnaire, mitigating the claims about the validity of the information. However, using the administrative data, it was possible to show in the first step that participation in the voluntary survey of the parents was selective. Only due to the record linkage it is possible to obtain otherwise unavailable information on the participants, which is a strength of this approach. The second step of the analysis concerned the explanatory power of the variable on parental earnings for student performance. While the parental earnings seem to have no effect in complete case analyses, the effect becomes stronger when weights are introduced to the models that account for the selectivity of the participation in the parental survey. Furthermore, using imputed data the effect of parental earnings becomes a significant predictor of student performance while the effect sizes and levels of significance of the other variables concerning the social origin shrink. The study emphasises the use of administrative data on earnings, as this information provides some benefits compared to the widely used household possession scales (Marks and O'Connell, 2021). Furthermore, it is a cost-efficient way to obtain valid information on the financial situation of a family. However, parental earnings do not cover the entire aspect of income and financial resources (Frick and Krell, 2010), as is pointed out by others that already proclaim wealth as a fourth dimension of social origin (Hällsten and Thaning, 2022).

The other two papers address a third question, namely what mechanisms contribute to educational inequality that are potentially beyond the scope of social origin. The third paper thus investigates the role of tracking realistic educational aspirations of compulsory school leavers in Switzerland. The analysis of panel data first showed that there is a considerable change in educational aspiration after students proceed to upper secondary education. Furthermore, random-effects ordered logistic regressions find that track placement in lower and upper secondary education has an impact on the level of education aspired alike the dimensions of social origin (e.g., Roth, 2017; Gölz and Wohlkinger, 2019. However, the change in aspiration upon leaving compulsory school was mostly determined by the track pursued in upper secondary education and almost no influence of the social background. While the observed three years are a pivotal moment in life, the study cannot control for the formation of aspirations during compulsory school nor is it able to test whether the changed aspirations are realised. Furthermore, the data does not allow for testing the underlying theoretical assumptions of the rational choice (Breen and Goldthorpe, 1997; Esser, 1999). In sum, the findings highlight that institutional factors of the education system - in this case, tracks in upper secondary education - have a potential impact on educational outcomes.

The last paper also analysed an institutional characteristic - the cut-off dates for school enrolment. While there is some consensus on the existence and magnitude of relative age effects in early childhood, the question that the study mainly addresses is whether these age-driven differences persist throughout compulsory education. Using test data from mandatory assessments comprising the entire school population from North-Western Switzerland that was linked to administrative data, the study deploys two complementary identification strategies. The combined results from both show that the relative age effects diminish over time. However, they are likely still at play when students in Switzerland are allocated to ability-based tracks. This in turn opens the possibility that coincidence - the date of birth - can have long-lasting effects on educational outcomes as tracking in Switzerland is a strong predictor of educational attainment (M. Buchmann et al., 2016). Unfortunately, the data does not allow for the creation of panel-like data to test this hypothesis on individuals' educational trajectories. Furthermore the instrument - although also used in other studies (e.g., Bedard and Dhuey, 2006) - is not free from criticism. Still, the study is an excellent example of the use of record linkage, while the application of two complementary identification strategies makes the insight that relative age effects shrink throughout compulsory education credible.

From the experience and findings of the four papers it is now time to make recommendations for future research. One essential conclusion from the first two papers is that the conceptualisation and the measurement of social origin need more thought in empirical research, given the paradigm of "garbage in - garbage out". Regardless of how elaborate the models are, if bad information is put into the models, the output will be bad information as well. This points not only towards which dimensions of social origin are measured but also how. While researchers should give more attention to these two questions when designing their surveys or analysing secondary data, one could also enrich data with administrative records or test alternatives of measuring social origin entirely. Trends in the composition of families and households should also be given more attention to better reflect the social environment in which children are being brought up.

Another conclusion is that, although promising, administrative data and record linkages are not the holy grail - at least in Switzerland. Administrative data have advantages as demonstrated in the second paper and bear the potential for innovative proxies for social origin (e.g. area per capita), however, they come with a price. The use of administrative data undermines, as for now and for Switzerland, the positive trend of open science. By law, it is impossible to share data which contains information from a record linkage to administrative data in Switzerland. Furthermore, the initial hurdle of the administrative process makes it unlikely that other researchers will make this effort to reproduce an analysis, which runs against the requirements of producing reliable research. Lastly, Large-Scale Assessments, such as the ÜGK, which would like to substitute questions from the costly background survey with already available administrative data are prohibited from doing so - which goes clearly against the idea of efficiency. While the use of administrative data points towards new types of data opportunities, it can also be recommended to use panel data or data from experiments, or the combination of different types of data, to more thoroughly investigate mechanisms that cause different educational outcomes.

In conclusion, this thesis showed that the endeavour of measuring social origin and its utilisation in analyses needs careful consideration. New ways of addressing the issue of obtaining valid information on the social origin were tested by establishing record linkages to administrative data, showing both, the strengths and weaknesses of this approach. Furthermore, the thesis displayed that institutional factors such as tracking and cut-off dates for enrollment play a substantial role in educational outcomes, highlighting the importance of factors beyond the social origin of children. This shows that future research in the field of the sociology of education should consider a variety of influences on educational outcomes in their analyses while improving on the theoretical and empirical valuation of the dimensions of social origin.

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Paper One - Asking Students about their Parents: How Item Non-Response and Measurement Error Depend on Construct Salience and Students' Cognitive Abilities

Abstract: Social stratification research often uses information directly obtained from students to analyze the relationship between household characteristics such as parental education and educational outcomes. Certain groups of students having more difficulties answering these questions or even being incapable of answering could bias estimates and potentially lead to erroneous conclusions about the relationship between the social context of students and their performance. This study examines measurement error and item non-response in students' answers on household characteristics using comprehensive data from two waves of a national large-scale assessment in Switzerland which we link to administrative records of the biological parents. In line with previous work, we find that high-performing students are less prone to measurement error and item non-response. Additionally, if a question is directed towards one parent, e.g., asks about the mother's education, item non-response and measurement error are more likely to occur if this parent does not live in the same household as the student. Comparing the results from regressions on student performance, we find significant differences between models that use student information compared to administrative records. These findings stress the need to better understand what causes measurement error and item non-response in students' answers about household characteristics and demonstrate a possibility to assess the robustness of estimates using student information.[a][b]

Keywords: Measurement Error; Item Non-Response; Large-Scale Assessment; Switzerland; Record Linkage

[a]This paper has been submitted to the Journal of Survey Statistics and Methodology.
[b]OSF repository available at: https://osf.io/k9wsn/

1 INTRODUCTION

Socio-economic status (SES) plays a significant role in explaining educational inequalities. A typical finding regarding SES is a significant difference in educational performance between socio-economically advantaged and disadvantaged students. For example, the Programme for International Student Assessment (PISA) finds that socio-economically advantaged students have significantly higher test scores in reading (OECD, 2019). For Switzerland, this gap in reading performance between the top and the bottom quarter of the SES distribution exceeds 100 points (one-quarter to one-third of a standard deviation), which is roughly equivalent to one school year's learning gains (Woessmann, 2016). Similarly, Switzerland's national large-scale assessment (LSA) in 2016, the "Verification of the Achievement of Basic Competencies (ÜGK)" (ÜGK), found a substantial difference between the proportion of students from the bottom quartile of the SES distribution that meet the educational standards in mathematics (37.7%) and the proportion of students from the top quartile that did so (83.7%) (Konsortium ÜGK, 2019a).

Although the literature generally agrees on the relevance of the social context for student performance, the data quality of the information on the social background in LSAs is challenged (Hovestadt and Schneider, 2021; Nusser and Heydrich, 2016; Engzell and Jonsson, 2015; Jerrim and Micklewright, 2014; Ridolfo and Maitland, 2011; Kreuter et al., 2010, 2006; Andersen et al., 2008; Maaz et al., 2006; Lien et al., 2001; Ensminger et al., 2000). One specific concern is that information, for example, on parents' education or the economic situation of their households, obtained by handing out questionnaires to students, may be affected by differential measurement error. Indeed, Engzell and Jonsson (2015), Kreuter et al. (2010), and Hovestadt and Schneider (2021) show that the relation between social background and student performance is more pronounced when the information is collected directly from the parents compared to information from proxy reports by students.

This study makes two important contributions. First, we integrate the analysis of item non-response which was not systematically included in prior work. Second, we are interested in common effects across a set of proxy variables and include construct salience as well as potential factors that interact with it as proposed by the cognitive model of response behaviour (Tourangeau et al., 2000). Thus, our analysis facilitates an understanding of the mechanisms causing item non-response and measurement error across a set of proxy variables. We use two waves of comprehensive data from the ÜGK (N = 22'423 in 2016, N = 20'177 in 2017) (Nidegger, 2021, 2019). We link the student data to several data sources from the Federal Statistical Office of Switzerland on the biological parents (Federal Statistical Office, 2021b,c,d,e,f,g,h,i,j,k,l,m,n). This linkage gives us the advantage of a cost-efficient way to assess the information from student proxy reports even in the absence of a parental questionnaire.

We investigate whether item non-response and measurement error are subject to the mechanisms proposed by Tourangeau et al. (2000). We use performance tests in mathematics of 11^{th}-grade (mean student age: 15.9 years) and languages of 8^{th}-grade students (mean student age: 12.7 years), respectively, as measures for our main student characteristic, cognitive ability, and additionally investigate the effect of age-related differences in cognitive ability. By analysing several proxy variables with varying degrees of complexity we can investigate differences in construct salience and additionally control for household characteristics that may be related to construct salience, e.g., the absence of a parent.

The structure of the paper is as follows. First, we present an overview of the recent research on proxy reports. Second, we describe the data and the analysis strategy. Third, we report results from the models on item non-response and measurement error. We conclude the paper by critically discussing the findings in a broader context.

2 DETERMINANT AND CONSEQUENCES OF MEASUREMENT ERROR AND ITEM NON-RESPONSE

2.1 Cognitive Abilities

The cognitive model of response behaviour (Tourangeau et al., 2000) argues that answering a question requires cognitive abilities. In other words, the likelihood of item non-response and measurement error should decline with higher cognitive capabilities. Using the PISA 2000 data from Germany, Kreuter et al. (2010) show that measurement error in student reports on parental education correlates with low test scores in mathematics and biases the effect of parental education on students' performance. Other research supports this finding by showing that the likelihood of measurement error in proxy reports is higher within low grades (Wittrock et al., 2017), lower-ability tracks in secondary education (Hovestadt and Schneider, 2021), and special educational needs (SEN) classes (Nusser and Heydrich, 2016). Furthermore, Ensminger et al. (2000), Hovestadt and Schneider (2021) and Jerrim and Micklewright (2014) find that students doing worse in school have a higher likelihood of item non-response. Consult Nusser and Heydrich (2016) for similar trends in SEN classes. Thus, our first hypothesis is that higher cognitive abilities lower the likelihood of item non-response and measurement error.

Further, we expect that older students have a lower likelihood of item non-response and measurement error as cognitive abilities develop throughout adolescence (Breit et al., 2020). Several studies find age effects regarding measurement error on proxy reports (Wittrock et al., 2017; Ridolfo and Maitland, 2011; Kreuter et al., 2010; Ensminger et al., 2000). However, no such effect is found by Lien et al. (2001).

2.2 Construct Salience

The theoretical model identifies the cognitive representation of the construct in question as a second relevant factor for measurement error and item non-response, as it is inherently pertinent for comprehending and retrieving information (Kreuter et al., 2010). Put differently, one can expect a more complex or less salient construct to have poorer cognitive representation and, thus, if asked about it, a higher likelihood of measurement error and item non-response. Therefore, this study aims to analyse several proxy variables with varying degrees of salience that are commonly used to contextualise student performance: Migration status, mother's and father's education, and mother's and father's occupation (Pham et al., 2017, 2016; OECD, 2019). Several studies show that measurement error is less frequent for questions about parental occupation than for questions about parental education (Nusser and Heydrich, 2016; Engzell and Jonsson, 2015; Jerrim and Micklewright, 2014; Maaz et al., 2006). Further, studies find almost no measurement error regarding questions about the country of birth or the language spoken at home (Nusser and Heydrich, 2016; Nordahl et al., 2011; Engzell and Jonsson, 2015).

Regarding item non-response, Jerrim and Micklewright (2014) report for PISA that the question about the number of books at home has fewer missings than questions about education or occupation. Likewise, Nusser and Heydrich (2016) find fewer missing answers among questions about language and country of birth than among questions regarding occupation and education. Therefore, we expect that high construct salience or low complexity leads to a lower likelihood of item non-response and measurement error.

However, Ensminger et al. (2000) present mixed evidence on presumably more practical SES-related indicators, like forms of public assistance and other materialistic representations of the SES. Jerrim and Micklewright (2014) even find that the agreement between parents' and children's reports was poorer for the question about the number of books at home than for questions about parental education and occupation.

2.3 Household Characteristics and other Determinants

Because questions about parental education and occupation are typically asked explicitly for each parent separately, cognitive representation and salience might depend on whether or not a parent lives with the child. The literature presents mixed evidence regarding the absence of a parent: Ensminger et al. (2000) find no effect of absent parents for measurement error and West et al. (2001) only for questions about economic activity but not for social class. However, Ensminger et al. (2000) find that children living with both parents had significantly fewer missing answers than their counterparts in single-parent households.

One argument is that the absence of a parent limits the interaction between parents and children and likely the closeness of their relationship. There is mixed evidence regarding the relationship between parents and children and measurement error: While Kreuter et al. (2010) find that closeness leads to lower measurement error, Hovestadt and Schneider (2021) do not, but associate it with lower item non-response. Hence, we hypothesize that questions explicitly asking about mothers or fathers will have a higher likelihood of item non-response and measurement error if the relevant parent is absent in the household.

Beyond the parent-child relationship, parents might act as gender-specific role models so that the traits of a parent are more salient to a child of the same gender. Thus, we assume that students give more accurate answers and have fewer item non-responses when the parent in question has the same gender as the child.

Several other socio-economic characteristics have been identified to correlate with measurement error and item non-response, most notably the gender of the child, the migration status, parental education, and the family's financial situation. While some researchers report girls giving more reliable answers, others find no meaningful gender differences (Kreuter et al., 2010; Ridolfo and Maitland, 2011; Ensminger et al., 2000). However, Ensminger et al. (2000) report differences in item non-response, where boys are more likely to have missing answers. Regarding migration status, Ridolfo and Maitland (2011) finds differences in the accuracy of answers about parental education and receiving public assistance between ethnic groups in America.

Turning to family-level characteristics, the findings of Ridolfo and Maitland (2011) and Wittrock et al. (2017) contradict each other regarding the effect of parents' highest education on the congruence between answers of parents and their children. However, findings about the effect of the financial situation of the family are more in line with each other: Higher family income or higher financial status is generally associated with better agreement between children's and parents' reports about parental education and occupation (Pu et al., 2011; Ridolfo and Maitland, 2011). Hence, the models should include such characteristics that potentially affect measurement error and item non-response.

2.4 Consequences of Measurement Error and Item Non-Response

The consequences of measurement errors and item non-response depend on whether they are correlated with the dependent variable (Carroll et al., 2006). First, assume that measurement error and item non-response are purely random. Then the consequence of measurement error on the independent variable is that the estimates are biased towards zero (attenuation bias). Uncorrelated measurement errors in the dependent variable would lead to less efficient models, while the estimates should not be biased. Unsystematic item non-response will restrict the sample, as most analyses rely on complete observations. While this should not bias the estimates, statistical power will be reduced.

Differential measurement error and systematic patterns of item non-response yield more problematic consequences. If item non-response follows a systematic pattern, it introduces selectivity bias to the resulting sub-sample used in models that rely on complete observations. Hence, estimates can not be generalised to a broader population and can be severely biased. Likewise, if measurement error is correlated with the dependent variable, the estimates can be under or overestimated or even change the direction, depending on the pattern.

Empirical work has repeatedly shown that various proxy variables on parental characteristics suffer from differential measurement error and that estimates based on such variables tend to underestimate the effects of these characteristics on student performance (Hovestadt and Schneider, 2021; Engzell and Jonsson, 2015; Kreuter et al., 2010). To assess measurement error in children's answers, these studies rely on other sources of information that they see as more valid. However, administrative data or answers from parents are not immune to measurement errors either. Therefore, comparing two data sources should be seen as an assessment of the robustness of the effects.

3 DATA AND METHODS

3.1 Data, Record Linkage and Variables

We draw on a comprehensive dataset from the "Verification of the Achievement of Basic Competencies (ÜGK)", Switzerland's LSA of educational standards, and link the student data to administrative data and additional survey data on their biological parents from the Federal Statistical Office of Switzerland (FSO). Information on replication material and how to obtain the data is provided in the appendix. The ÜGK took place for the first time in 2016, with a sample of 11^{th}-grade students (N = 22'423) and assessed skills in mathematics. In 2017, the ÜGK sampled 8^{th}-grade students (N = 20'177), whose first and second school language competencies were assessed (Verner and Helbling, 2019a,b). In both years, the ÜGK was administered computer-based.

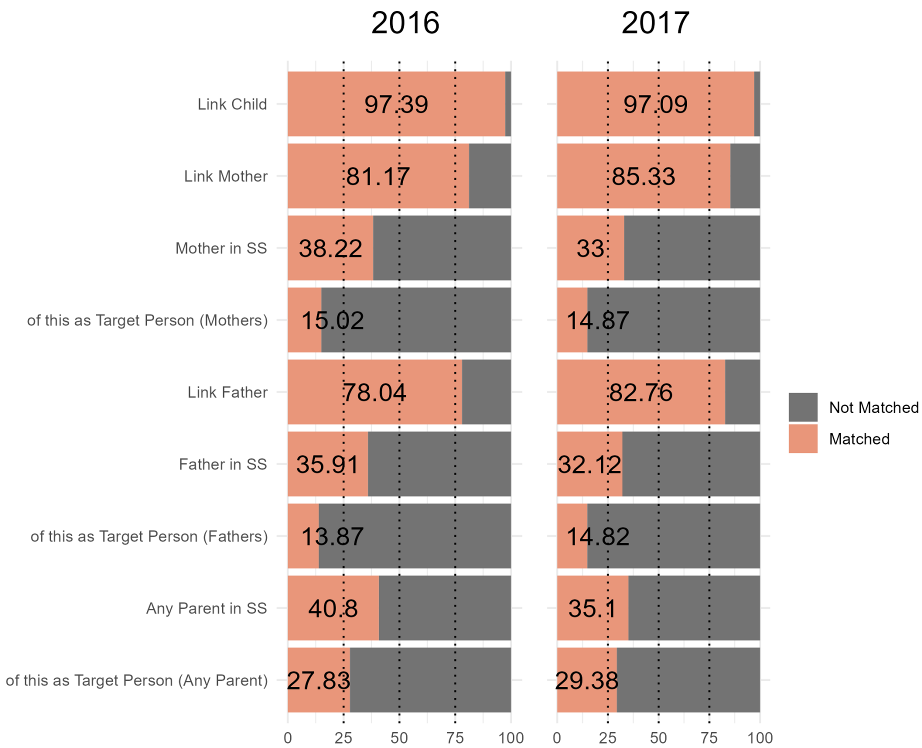
The sampling process in both waves of the ÜGK differed across 29 regions resembling cantons and language regions within cantons: In some regions the entire student population in the targeted grade was observed; in others, all eligible students in sampled schools were assessed, or the sampling occurred at both levels. The response rate of the students varies from 92.5% in 2016 to 96.6% in 2017 due to illnesses, refusal, or technical difficulties (Konsortium ÜGK, 2019b,a). After the sampling, a link to social security numbers (SSN) was established in cooperation with the FSO, which allows us to establish a record linkage.

The two waves of the ÜGK are the departing point for our analyses. We first create the dependent variables for item non-response and measurement error for each proxy variable in our analysis. This results in multiple data sets, one per proxy variable, which are finally appended into one data set for analysis. While we can obtain the dependent variable for item non-response (a dummy variable where 1 indicates missing information) for all the observations and proxy variables, this is not possible for measurement error. The creation of the dependent variable for measurement error relies on non-missing information on the proxy variable in both data sources. Thus, to create a dummy variable, where 1 indicates measurement error, we need information on the mother's education, for example, in the ÜGK and the administrative data. Hence, the samples for measurement error are restricted as we can not identify the dependent variable for all observations.

The missing information in administrative data can be attributed to a) students for which no SSN could be identified in the STATPOP (3%), the annually updated census of Switzerland's permanent and non-permanent inhabitants, and b) children for which no link to the biological parents could be established (which varies between parent and years from 14.7% and 22%). In the case of students, ambiguous matching or a provisional SSN due to refugee status causes missing links. For the biological parents, the link can only be established when it is already recorded in the STATPOP.

While these limitations apply to all variables that rely on identifying the biological parents in administrative records, information on parental education and occupation is even more restricted. This information can only be obtained from the Structural Survey (SS), an annual survey of roughly 200,000 people aged 15 and above living in private households. The survey collects information on the occupation of the target person and information on education for the target person and additional household members. We increase coverage to some extent by pooling several years of the SS (e.g., 2012 to 2016 for the ÜGK in 2016, 2013 to 2017 for the ÜGK in 2017). Figure 1 shows the coverage of the different FSO data relative to the respective ÜGK sample.

Figure 1: Coverage of the FSO Data by ÜGK Year and Matches



As pointed out, we can identify item non-response when a student gives no response in the respective proxy variable. Before creating a dummy variable indicating measurement error, however, we additionally had to operationalize the information in both, the ÜGK and administrative data, in the same way. For migration status, we used information from the STATPOP to create a variable exactly like in the ÜGK: 1 = "Native with at least one parent born in Switzerland", 2 = "2$^{\text{nd}}$ generation" - both parents but the child born abroad, or 3 = "1$^{\text{st}}$ generation" - all born aboard.

From the SS we create variables on the mother's and father's education analogous to the ÜGK: 0 = "Completed compulsory school", 1 = "Completed any form of upper secondary education", and 2 = "Completed any form of tertiary degree". Regarding the variables on mother's and father's occupations, we rely on the International Socio-Economic Index of Occupational Status (ISEI-08) (Ganzeboom, 2010; Ganzeboom et al., 1992). The open format question in the ÜGK was already recoded into four-digit codes resembling the International Standard Classification of Occupations (ISCO-08) (Ganzeboom, 2010; Ganzeboom et al., 1992) and converted to ISEI-08. In the case of the SS, we converted the ISCO classification (in an adapted version for Switzerland (Federal Statistical Office, 2021a)) of the target person to the ISEI-08 classification (Schwitter, 2021). Because PISA and the ÜGK both use an SES-Index to contextualize student performance based on information on the highest parental education (HISCED) and occupation (HISEI), we constructed these variables in both data sources by using the highest available information per household.

To account for the pooling and the possibility of second-hand information in the SS, we prioritized information from the target person over other adult proxy reports (only for the education variables), as well as newer over older information. In contrast to migration status and parental education, where we defined measurement error as any incongruities between the two sources, we apply a bandwidth of ± 5 points to the ISEI scale instead of exact matching, which partially accounts for errors introduced by the manual translation and coding of the question in the ÜGK as well as for the pooling of multiple years regarding the SS data.

Table 1 shows sample statistics regarding the information obtained in the ÜGK. Column 1 refers to the full ÜGK sample while the other columns refer to the samples used in the models. Compared to the original ÜGK sample, we see a higher percentage of students who report being native in the samples used for models 2, 3 and 4 on item non-response and the samples used for the models regarding measurement error. Further, the same samples have a higher percentage of children stating that their mother or father has completed upper secondary or tertiary education. Similarly, the mean for fathers' and mothers' ISEI is higher compared to the original ÜGK sample in these models.

We include several variables in our models corresponding to individual, construct, and household characteristics to test our hypotheses about measurement error and item non-response. Regarding individual characteristics, we use the ÜGK performance assessment in mathematics in 2016 and school languages in 2017, as a measure of cognitive abilities. For each observation, the performance measure is represented by twenty plausible values (PV) (Angelone and Keller, 2019b,a) which we combine to a mean and were then scaled for each wave of the ÜGK data. We use a dummy variable indicating the ÜGK 2016 wave, where children were, on average, 3.2 years older to assess age-related cognitive abilities. As additional individual characteristics, we include information from the STATPOP on gender, the relative age (standardized for each wave), and the migration status of the student.

For the different proxy variables, we create an ordinal variable that reflects the hierarchy of their presumed salience and complexity. Regarding Nusser and Heydrich (2016), Engzell and Jonsson (2015), and Nordahl et al. (2011) we propose that variables about parental occupation are more salient than variables about parental education, while the variable about migration status serves as a baseline. Further, using the STATPOP data, we create two dummy variables that represent household characteristics that interplay with construct salience: The first one addresses the role-model effect and takes on the value 1 when the gender of the parent in question matches the gender of the student. The second dummy indicates the absence of the parent in question, i.e., it takes on the value 1 when the father or mother about whom the question is asked is absent in the household.

Additional household characteristics are the information in the ÜGK data on the test language, where 1 indicates that the test language is not spoken in the household, and the highest parental education and occupational status, which we both obtain from the SS.

Table 1: Comparison between the ÜGK Sample and the Samples used in the Models

			Item Non-Response Models			Measurement Error Models		
		Total ÜGK Observations	Model 1	Model 2 + 3	Model 4	Model 1	Model 2 + 3	Model 4
Nr. of Observations:		42600	213000	163397	47461	93028	89779	52807
Nr. of Students:		42600	42600	32769	9507	33608	32617	9499
Gender	1	49.3%	49.3%	49.6%	50.2%	50.1%	50.2%	50.7%
	2	50.5%	50.5%	50.4%	49.8%	49.8%	49.8%	49.3%
	Missing	0.2%	0.2%	0%	0%	0%	0%	0%
Migration Status	1	68.9%	68.9%	80.9%	83.9%	82.1%	82.9%	84.6%
	2	18.7%	18.7%	15.9%	13.4%	14.3%	14.5%	13%
	3	9.6%	9.6%	2.5%	2.1%	2.4%	2.3%	2%
	Missing	2.8%	2.8%	0.7%	0.6%	1.3%	0.3%	0.4%
Mother's Education	0	19.9%	19.9%	17.4%	15.4%	17%	17%	15.7%
	1	47%	47%	51.2%	51.8%	53.2%	53.4%	53.9%
	2	21.7%	21.7%	21.6%	24.3%	23.9%	23.6%	25.3%
	Missing	11.5%	11.5%	9.7%	8.6%	6%	6%	5.1%
Father's Education	0	18.1%	18.1%	15.9%	14.6%	15.7%	15.7%	15%
	1	41.1%	41.1%	45%	45.3%	46.4%	46.8%	47.2%
	2	27.8%	27.8%	29%	31.2%	31.4%	31.3%	32.6%
	Missing	13%	13%	10.1%	8.8%	6.5%	6.2%	5.2%
HEDU	0	14.1%	14.1%	11.6%	10.3%	11.2%	11.2%	10.4%
	1	42%	42%	44.8%	44.2%	45.7%	45.9%	45.7%
	2	35.7%	35.7%	36.9%	39.7%	39.8%	39.6%	41.3%
	Missing	8.2%	8.2%	6.7%	5.8%	3.3%	3.4%	2.7%
Mother's ISEI	Mean	45.21	45.21	46.51	48.48	47.6	47.57	48.71
	SD	21.3	21.3	20.86	20.69	20.88	20.84	20.71
Father's ISEI	Mean	46.28	46.28	47.25	48.99	48.41	48.31	49.18
	SD	22.2	22.2	22.12	22.1	22.18	22.18	22.1
HISEI	Mean	52.43	52.43	53.93	56.2	55.28	55.22	56.52
	SD	21.56	21.56	21.06	20.48	20.85	20.84	20.42

Notes: Column 1 refers to the combined samples of the ÜGK from 2016 and 2017, whereas columns 2 through 4 refer to the models on item non-response and 5 through 7 to the models on measurement error. The descriptive numbers are calculated using the children's answers on the proxy variables except for relative age which was obtained from the administrative records.

3.2 Analysis Strategy

In both cases, item non-response and measurement error, we first present descriptive results. We display the percentage of missing information and measurement error for each proxy variable per wave of the ÜGK. In the Appendix, we additionally present Cohen's Kappa (Ranganathan et al., 2017; Cohen, 1968, 1960) for measurement error in the ordinal proxy variables and the Intra-Correlation Coefficient (ICC) for metric proxy variables (Ranganathan et al., 2017). We then apply two identical sets of models for item non-response and measurement error, respectively. As mentioned above, we "stack" the samples from each proxy variable on top of each other. Therefore, we have multiple observations of the same student in our final analytical samples, for which we account by applying hierarchical logistic models that control for unobserved heterogeneity at the student level. This approach suits our aim of analysing the common effects of the independent variables on measurement error and item non-response across several proxy variables because it yields estimates that apply homogeneously to all included proxy variables. We report results on the individual proxy variables in the Appendix. The models correspond to the following formula:

$$Logit(Y_{ij}) = \beta_0 + \beta X_{ij} + \beta_{0j} + \varepsilon_{ij}$$

Where:

$\beta_{0j} \rightarrow N(0, \sigma_0^2)$ – random intercept at the student level

$\varepsilon_{ij} \rightarrow N(0, \sigma_\varepsilon^2)$ – random errors

Model 1 in tables 3 and 6 introduces our main predictors (X): student performance, the age dummy, and construct salience, with $i = 1, 2, ...n$ observations and $j = 1, 2, ...m$ students. We run the model without a constant (β_0). Instead, we include all levels of construct salience, such that the coefficients can be interpreted as baseline odds. A baseline odds value of 0.1, for example, means that there are 10 observations with missing values per 100 observations with non-missing values given that all the other covariates are 0. For example, the baseline odds in model 1 in table 3 refer to the odds for a student with an average performance from the wave 2017. We run model 2 with the identical specifications of model 1 and the sample corresponding to model 3. This step lets us investigate, whether changes in the effects are driven by sample differences or the additional variables for student characteristics (relative age, gender, and migration status), construct characteristics (the role-model effect, the absence of the parent in question), and the household characteristic language spoken at home in model 3. Model 4 then uses further household characteristics (the highest parental education and occupational status).

Note that we refrain from including the samples from aggregate proxy variables when analysing non-response because aggregates like the HISEI are inherently dependent on missing information. Lastly, the number of observations per student may vary because of missing information on a proxy variable in the administrative data. Therefore, the analytical samples for the models become unbalanced, which can be handled by the hierarchical logistic models we apply (Maas and Snijders, 2003).

4 RESULTS

4.1 Explaining Item Non-Response

Table 2 reports the percentage of missing information in the ÜGK for each proxy variable per year. Of all the proxy variables, migration status has the fewest missing information (<5%). The other proxy variables have between 10% and 26% missing answers. The variable regarding mothers' ISEI is particularly prone to item non-response for 2017.

Table 2: Percentage Missing

Proxy Variable	Missings 2016 in %	Missings 2017 in %
Migration Status	1.21	4.60
Mothers' ISEI	13.06	25.91
Fathers' ISEI	11.10	15.47
Mothers' Education	15.78	6.68
Fathers' Education	17.71	7.79

Model 1 in table 3 introduces our main predictors, student performance, an indicator for the ÜGK wave, the interaction term for wave and performance, and the levels of construct salience as baseline odds to explain item non-response. We included the interaction term of performance and wave to account for the possible differences in the effect of the performance measures between age groups. While model 2 has the same specifications as model 1, it is restricted to the cases in model 3, which includes additional predictors.

The comparison of models 1 and 2 reveals that the effects for performance, the wave dummy, and the interaction term are robust to the altered sample composition between the models. The change in the baseline odds can be attributed to very few cases with missing information (N = 233) remaining in the category migration status in the sample of model 2. In contrast, the number of missing answers for the other categories hardly changes between the samples. Regarding the additional model specifications, the comparison of models 2 and 3 reveals robust effects for all the above-mentioned variables, although the impact of student performance is reduced slightly.

Model 3, finds that an increase in student performance is associated with a decrease in the likelihood of item non-response (OR = 0.81, $p < 0.001$). Similarly, the - on average - 3.2 years older students from the first wave of the ÜGK had fewer missing answers (OR = 0.83, $p < 0.001$), like students that are older than their average peers in the cohort (OR = 0.97, $p < 0.05$).

However, being non-native or not speaking the test language increases the likelihood of item non-response (2^{nd} generation: OR = 1.59, $p < 0.001$ | 1^{st} generation: OR = 1.78, $p < 0.001$; not speaking the test language: OR = 1.36, $p < 0.001$). Lastly, we see that missing answers are also more likely among male students (1.38, $p < 0.001$). Regarding construct salience, the baseline odds of item non-response increase for presumably less salient constructs. We see that the baseline odds for item non-response are lower for questions about the father (Education: odds = 0.036 | ISEI: 0.036) than

Table 3: Models for Item Non-Response

	Model 1	Model 2	Model 3	Model 4
Construct Salience (ÜGK) - *Intercepts*				
Migration Status	0.010	0.0025	0.0018	0.0020
	(0.0097 - 0.011)	(0.0022 - 0.0029)	(0.0016 - 0.0021)	(0.0013 - 0.0028)
Mothers's ISEI	0.14	0.13	0.095	0.081
	(0.13 - 0.15)	(0.12 - 0.14)	(0.089 - 0.10)	(0.063 - 0.11)
Fathers's ISEI	0.078	0.057	0.036	0.036
	(0.074 - 0.082)	(0.054 - 0.060)	(0.033 - 0.039)	(0.027 - 0.047)
Mothers' Education	0.063	0.054	0.039	0.043
	(0.061 - 0.066)	(0.051 - 0.056)	(0.037 - 0.042)	(0.033 - 0.055)
Fathers' Education	0.076	0.057	0.036	0.041
	(0.073 - 0.080)	(0.054 - 0.060)	(0.034 - 0.039)	(0.031 - 0.053)
Performance (ÜGK)	0.72^{***}	0.74^{***}	0.81^{***}	0.87^{***}
	(0.70 - 0.74)	(0.71 - 0.77)	(0.78 - 0.84)	(0.80 - 0.94)
Wave = 2016 (ÜGK)	0.86^{***}	0.83^{***}	0.83^{***}	0.91
	(0.82 - 0.90)	(0.79 - 0.88)	(0.79 - 0.88)	(0.82 - 1.02)
Wave = 2016 * Performance	0.75^{***}	0.75^{***}	0.74^{***}	0.70^{***}
	(0.71 - 0.78)	(0.71 - 0.79)	(0.70 - 0.78)	(0.62 - 0.78)
Relative Age (FSO)			0.97^{*}	0.96
			(0.94 - 1.00)	(0.90 - 1.01)
Gender = Male (FSO)			1.38^{***}	1.48^{***}
			(1.31 - 1.46)	(1.32 - 1.64)
Role Model Effect (FSO)			0.93^{***}	0.89^{***}
			(0.90 - 0.96)	(0.83 - 0.95)
Parent not at Home (FSO)			2.32^{***}	2.30^{***}
			(2.17 - 2.49)	(1.98 - 2.68)
Migration Status (FSO) - Ref. = *Native*				
2^{nd} Generation			1.59^{***}	1.32^{**}
			(1.46 - 1.72)	(1.11 - 1.58)
1^{st} Generation			1.78^{***}	1.27
			(1.47 - 2.15)	(0.85 - 1.89)
Test language not spoken at Home (ÜGK)			1.36^{***}	1.53^{***}
			(1.26 - 1.48)	(1.30 - 1.80)
HISCED (FSO) - Ref. = *Compulsory Education*				
Upper Secondary Education				0.73^{**}
				(0.58 - 0.91)
Tertiary Education				0.74^{*}
				(0.58 - 0.94)
HISEI (FSO)				0.99
				(0.93 - 1.05)
σ^2	2.27^{***}	2.25^{***}	2.19^{***}	2.38^{***}
	(2.18 - 2.38)	(2.13 - 2.37)	(2.07 - 2.32)	(2.14 - 2.64)
BIC	136977.0	89577.4	88611.4	22868.9
Nr. of Observations	213000	163397	163397	47461
Nr. of Students	42600	32769	32769	9507

Notes: Dependent variable = Item Non-Response. Predictors in Model 1: student performance, wave indicator and the interaction term of performance and wave. Model 2 uses the same predictors while using the same sample as Model 2. Model 3 uses additional predictors: relative age, gender, a dummy for the role model effect, a dummy for an absent parent, migration status, and whether the test language is spoken at home. Model 4 additionally uses the highest parental education and occupation status per household as predictors. All models further include intercepts for all the levels of construct salience. Conditional odds ratios; 95% confidence intervals in parentheses. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

for questions about the mother (Education: odds = 0.039 | ISEI: 0.095), while the difference between the effects of fathers' education and fathers' ISEI is statistically insignificant. Model 3 further identifies that the role model effect lowers the likelihood of missing information (OR = 0.93, $p < 0.001$), i.e., when the parent in question has the same gender as the student, and the absence of the parent in question raises the likelihood significantly (OR = 2.32, $p < 0.001$).

Model 4 reveals that the results are robust to additional specifications regarding household characteristics and an altered sample composition. The main changes apply to the wave dummy, the effect of being 1^{st} generation migrant, and relative age, where the effects become insignificant. It also reveals that missing answers are less likely among students from higher-educated households, while parental occupational status has no effect. Since the sample is highly restricted for model 4, these results should be interpreted cautiously.

Overall, the findings confirm our hypothesis regarding the higher likelihood of item non-response among students with lower cognitive abilities, which is in line with findings from Ensminger et al. (2000), Hovestadt and Schneider (2021), and Jerrim and Micklewright (2014). The age effect, represented by the wave dummy and the relative age variable aligns with our second hypothesis and further corroborates the findings from the above-mentioned authors. Interestingly, the interaction effect of performance informs us that the performance effect is stronger among older students (OR = 0.74, $p < 0.001$). Joint tests reveal that the baseline odds for the levels of construct salience vary significantly regarding item non-response with the only exception being the difference between Fathers' education and occupation. Hence, the results suggest that children are more likely to answer questions about their father rather than their mother regardless of the topic, which is partially contradictory to our expectations based on prior work. We find a role model effect that lowers the chances of missing answers and an effect for absent parents that raises these chances, therefore partially supporting the findings from Ensminger et al. (2000) on the effect of absent parents and confirming our last two hypotheses.

4.2 Explaining Measurement Error

Table 4 displays descriptive statistics for the proxy variables. In the Appendix, we additionally calculated the weighted Cohen's Kappa, a measure commonly used to assess agreement between raters (Ranganathan et al., 2017; Cohen, 1968, 1960) or to assess the overlap of two data sources (Kreuter et al., 2010) for ordinal proxy variables while for continuous proxy variables, we calculated the Intra-Correlation Coefficient (ICC). While measurement errors in the variable migration status are scarce, incongruities for maternal and paternal occupation are more common between the two data sources. The highest percentage of measurement error is found in variables concerning parental education. Regarding the waves, the descriptive results indicate that errors occur more frequently among students from the second wave who are on average 3.2 years younger.

Table 4: Percentage Measurement Error

Proxy Variable	Measurement Error 2016 in %	Measurement Error 2017 in %
Immigration status	1.89	4.31
Mothers' ISEI 08	28.75	41.10
Fathers' ISEI 08	33.56	45.41
HISEI 08	33.52	43.58
Mothers' education	46.48	47.64
Father' education	43.81	50.62
HISCED	62.19	62.75

To analyse measurement error, the models were set up analogous to the models on item non-response. Across the first three models, we find robust effects of cognitive ability, the wave indicator, and the baseline odds for construct salience. The interaction term effects were also stable but not significant. Note, that the baseline odds refer to students with an average performance from the wave of 2017. The baseline odds of about 1 show that we could identify measurement errors for about 50% of the sample with the above-outlined conditions.

Model 3 finds that an increase in performance reduces the likelihood of errors (OR = 0.83, $p < 0.001$). Additionally, the effect of the wave dummy shows that the chance of errors for the on average 3.2 years older students is lower (OR = 0.59, $p < 0.001$) than for younger students. For measurement error, there is no effect of being relatively older than the average peer in the cohort and there is no difference in the effect of performance between the younger and the older cohort. Regarding the levels of construct salience, we again find significant differences between the baseline odds of measurement error except for the question about the occupational status of mothers and fathers.

In model 3, we do not find a role model effect, but if the parent the question is asked about does not live with the child, the likelihood of discrepancies rises (1.26, $p < 0.001$). Additionally, our controls in model 3 reveal that not speaking the test language at home (OR = 1.39, $p < 0.001$) and being 2^{nd} generation migrant (OR = 1.13, $p < 0.05$) increase the chance of measurement errors. The estimate for 1^{st} generation migration status also points in the same direction but remains insignificant. Model 4, which includes the highest parental education and occupational status from administrative data as predictors, reveals that only the estimates for migration status change substantially and become insignificant. It further informs us that students from households with a high HISEI have a lower chance for measurement error, while results on HISCED are inconclusive. Nevertheless, the results should be interpreted cautiously because of the limited sample.

The models highlight the robustness of the effect of cognitive abilities and stress that age plays a substantial role in explaining measurement errors. However, we find no longer a difference between the performance effect of the two waves and no effect of relative age within a cohort. Thus, our findings support previous results on differential measurement errors regarding cognitive abilities (Wittrock et al., 2017; Engzell and Jonsson, 2015; Ridolfo and Maitland, 2011; Kreuter et al., 2010; Ensminger et al., 2000).

Concerning construct salience, we see that the baseline odds are in general significantly different from each other. However, the order of the presumed salience of con-

Table 5: Models for Measurement Errors

	Model 1	Model 2	Model 3	Model 4
Construct Salience (ÜGK) - *Intercepts*				
Migration Stauts	0.016	0.017	0.015	0.017
	(0.015 - 0.018)	(0.015 - 0.018)	(0.013 - 0.016)	(0.014 - 0.022)
Motehrs' ISEI	1.18	1.19	1.09	1.13
	(1.08 - 1.29)	(1.08 - 1.30)	(0.99 - 1.21)	(0.93 - 1.38)
Fathers' ISEI	1.21	1.21	1.08	1.11
	(1.12 - 1.32)	(1.11 - 1.31)	(0.98 - 1.19)	(0.91 - 1.36)
HISEI	2.77	2.75	2.48	2.45
	(2.58 - 2.96)	(2.56 - 2.95)	(2.30 - 2.68)	(2.03 - 2.97)
Mothers' Education	0.57	0.58	0.53	0.57
	(0.54 - 0.61)	(0.55 - 0.62)	(0.49 - 0.57)	(0.47 - 0.70)
Fathers' Education	0.76	0.77	0.69	0.75
	(0.72 - 0.81)	(0.73 - 0.82)	(0.64 - 0.75)	(0.62 - 0.91)
HISCED	0.71	0.71	0.64	0.68
	(0.67 - 0.75)	(0.67 - 0.75)	(0.60 - 0.69)	(0.56 - 0.82)
Performance (ÜGK)	0.81^{***}	0.81^{***}	0.83^{***}	0.89^{***}
	(0.78 - 0.85)	(0.78 - 0.85)	(0.80 - 0.87)	(0.84 - 0.93)
Wave = 2016 (ÜGK)	0.59^{***}	0.58^{***}	0.59^{***}	0.68^{***}
	(0.55 - 0.62)	(0.55 - 0.62)	(0.55 - 0.62)	(0.64 - 0.73)
Wave = 2016 * Performance	0.96	0.97	0.98	0.97
	(0.90 - 1.02)	(0.91 - 1.04)	(0.92 - 1.04)	(0.90 - 1.04)
Relative Age (FSO)			1.00	0.98
			(0.96 - 1.03)	(0.95 - 1.02)
Gender = Male (FSO)			1.05	0.99
			(0.99 - 1.12)	(0.93 - 1.06)
Role Model Effect (FSO)			0.96	0.95
			(0.91 - 1.01)	(0.90 - 1.01)
Parent not at Home (FSO)			1.26^{***}	1.28^{***}
			(1.13 - 1.41)	(1.13 - 1.44)
Migration Status (FSO) - Ref. = *Native*				
2nd Generation			1.13^{*}	0.94
			(1.01 - 1.26)	(0.83 - 1.06)
1st Generation			1.08	0.78
			(0.83 - 1.40)	(0.59 - 1.04)
Test language not spoken at Home (ÜGK)			1.39^{***}	1.15^{*}
			(1.25 - 1.55)	(1.02 - 1.29)
HISCED (FSO) - Ref. = *Compulsory Education*				
Upper Secondary Education				0.80^{*}
				(0.67 - 0.96)
Tertiary Education				1.15
				(0.96 - 1.38)
HISEI (FSO)				0.86^{***}
				(0.83 - 0.89)
σ^2	2.08^{***}	2.03^{***}	2.03^{***}	1.39^{***}
	(1.96 - 2.21)	(1.90 - 2.15)	(1.91 - 2.16)	(1.29 - 1.50)
BIC	83434.6	79515.1	79481.2	56987.4
Nr. of Observations	93028	89779	89779	52807
Nr. of Students	33608	32617	32617	9499

Notes: Conditional odds ratios; 95% confidence intervals in parentheses. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

structs is not supported as it seems that questions about education are generally less prone to measurement error. In line with works that highlight the importance of engagement between children and parents (Ridolfo and Maitland, 2011; Kreuter et al., 2010; Lien et al., 2001), our findings show that the absence of a parent positively affects the likelihood of errors. Consistent with other research (Lien et al., 2001; Ensminger et al., 2000), our model finds no gender effect and does not support our hypothesis about a role model effect.

4.3 Reconsidering the Consequences of Measurement Error

Finally, we reconsider the consequences of measurement error for estimates on student performance: We ran separate regressions for each of the ÜGK waves with student performance as the dependent variable including only one of the proxy variables at a time, while controlling for student gender, the language spoken at home, and the relative age in the cohort. We repeated the regressions in table 6 alternating the source of information for the proxy variable and the sample. Column 1 displays estimates from complete case analyses using student information. The models in column 2 again use student information while being restricted to observations for which we have valid information on the proxy variable in the administrative records. Column 3 refers to models relying on restricted samples for analysis using the information on the proxy variable from administrative data. All coefficients from the OLS models are significant at the $p < 0.001$ level except for the coefficient of being 1st generation migrant in column 3 for 2017.

The comparison of columns 1 and 2 shows that estimates based on student information are not sensitive to the sample restriction. However, estimates based on administrative data are, with two exceptions, significantly different from the estimates using student information. Regarding parental education, for example, the model using student information underestimates the relationship between the social context and

Table 6: Predicted Estimates by Different Sources of Information

	Student information – Complete Cases		Student Information – Restricted Sample		Administrative Information – Restricted Sample	
	Performance 2016		Performance 2016		Performance 2016	
Migration Status – ref: Native						
2nd Generation	-0.401***		-0.383***	-	-0.351***	(a,b)
	(-20.29)		(-15.55)		(-14.04)	
1st Generation	-0.259***		-0.188***	-	-0.0879	(a,b)
	(-10.31)		(-3.51)		(-1.57)	
N:	21549		16913		16913	
Highest Education – ref: Compulsory Education						
Upper Secondary Education	0.399***		0.467***	(a)	0.301***	(a,b)
	(20.37)		(13.79)		(7.27)	
Tertiary Education	0.738***		0.761***	-	0.712***	-
	(36.33)		(22.06)		(17.14)	
N:	20982		5073		5073	
HISEI:						
	0.0123***		0.0120***	-	0.0111***	(a)
	(40.12)		(19.32)		(18.73)	
N:	19511		8235		8235	
	Performance 2017		Performance 2017		Performance 2017	
Migration Status – ref: Native						
2nd Generation	-0.238***		-0.253***	-	-0.285***	(a,b)
	(-11.56)		(-11.00)		(-12.18)	
1st Generation	-0.213***		-0.328***	(a)	0.0334	(a,b)
	(-8.09)		(-7.34)		(0.57)	
N:	18669		15622		15622	
Highest Education – ref: Compulsory Education						
Upper Secondary Education	0.130***		0.106**	-	0.319***	(a,b)
	(6.12)		(2.94)		(7.04)	
Tertiary Education	0.346***		0.299***	-	0.701***	(a,b)
	(16.08)		(8.19)		(15.49)	
N:	17560		4499		4499	
HISEI:						
	0.0130***		0.0127***	-	0.0102***	(a,b)
	(40.35)		(19.63)		(15.76)	
N:	17882		6513		6513	

Notes: Observations in Model 2 and Model 3 are restricted to observations that have non-missing information in both data sources. T statistics in parentheses. a = Significantly different from Model 1; b = Significantly different from Model 2. * p <0.05, ** p <0.01, *** p <0.001

student performance in the case of 2017, which is similar to results reported in Hovestadt and Schneider (2021), Engzell and Jonsson (2015), Jerrim and Micklewright (2014), and Kreuter et al. (2010). This comparison of coefficients from models relying on different data sources highlights that there is a problem with the robustness of the results. However, we do not find that the differences between the data sources support that estimates from regressions using student information generally under or overestimate the effects of the proxy variables on student performance.

5 CONCLUSIONS

Studies have repeatedly shown the importance of the social context for educational outcomes while relying on information from students' answers. However, the literature indicates that measurement errors and item non-response in students' answers correlate with student performance (Hovestadt and Schneider, 2021; Nusser and Heydrich, 2016; Engzell and Jonsson, 2015; Jerrim and Micklewright, 2014; Ridolfo and Maitland, 2011; Kreuter et al., 2010, 2006; Andersen et al., 2008; Maaz et al., 2006; Lien et al., 2001; Ensminger et al., 2000). On the one hand, this correlation can bias estimates of the relationship between student performance and the social context (Engzell and Jonsson, 2015; Kreuter et al., 2010). On the other hand, systematic item non-response potentially biases the sample and limits the generalisability of the findings. Hence, understanding what affects measurement errors and item non-response is crucial for future research on student performance or educational mobility.

This paper uses comprehensive data from the "Verification of the Achievement of Basic Competencies (ÜGK)", Switzerland's national Large Scale Assessment, which we linked to administrative data on the biological parents of the students to examine measurement error and item non-response in questions that students answer about the socio-economic characteristics of their parents. Namely, we look at migration status, parental education and occupation, variables that are commonly used to contextualise student performance in LSAs such as PISA or the ÜGK. Extending on previous research, we not only assess the effect of cognitive abilities and age but also control for construct salience and factors that possibly interact with it and estimate common effects of the independent variables across our set of proxy variables. In addition, we integrate the analysis of item non-response within the same theoretical framework, especially as findings on item non-response regarding student answers are not systematic in the literature.

The results indicate that high student performance is associated with a lower probability of item non-response, and the effect is even larger for students from the first wave of the ÜGK, who are on average 3.2 years older. These older students have fewer missing answers, which can be interpreted as a consequence of higher cognitive abilities due to their advanced adolescence. We find similar effects of being older than the average peers in the same cohort. Further, we see significant differences in item non-response between proxy variables with differing degrees of construct salience. The results also reveal that an absent parent seems to increase the likelihood of missing answers when the question concerns this parent. Furthermore, we find evidence of a role model effect as the likelihood of item non-response decreases if a question concerns the parent that has the same gender as the child, while we find that boys are generally more prone to item non-response. Our findings support the work of Ensminger et al. (2000) and Engzell and Jonsson (2015), two examples of the few studies to analyse item non-response in the context of proxy reports explicitly.

Regarding measurement errors, the results again demonstrate that better performance lowers the chance of errors. However, we find no difference in the performance effect between the waves of the ÜGK, although older students from the first wave are generally less likely to make errors. The results reveal that presumably less salient constructs have significantly higher likelihoods of measurement error, so have questions targeting a parent that is absent in the household. However, we find no indication of a role model effect, nor an effect of the relative age within a cohort. These findings are in line with the works of several authors on differential measurement error regarding student performance (Wittrock et al., 2017; Engzell and Jonsson, 2015; Ridolfo and Maitland, 2011; Kreuter et al., 2010; Ensminger et al., 2000), age (Wittrock et al., 2017; Ridolfo and Maitland, 2011; Kreuter et al., 2010; Ensminger et al., 2000), the absence of parents (Ridolfo and Maitland, 2011; Kreuter et al., 2010; Lien et al., 2001), and gender effects (Lien et al., 2001; Ensminger et al., 2000).

We identify four main limitations of this study: Firstly, due to missing information in the administrative data the sample is restricted. However, the results remain robust across different specifications and samples. Secondly, actual measurement error in the variables for occupational status (ISEI) cannot be distinguished from error due to the manual coding by the ÜGK or the pooling of five waves of the SS. Therefore, interpreting the results on construct salience should be done with caution. Thirdly, the dummy variable for wave, which we interpret as an age dummy, probably contains omitted factors besides developing cognitive abilities, such as more prolonged exposure to the constructs in question. Lastly, as both administrative data and information obtained from students can be affected by measurement error, the comparison between the different data sources is, strictly speaking, not assessing measurement error in one of the data sources but assessing reliability between the two.

Despite these limitations, the results of our analysis demonstrate that the same theoretical mechanisms apply to both, item non-response and measurement error and that there are common effects across a set of proxy variables. Especially, younger students and students with low cognitive abilities are prone to item non-response and measurement error. While we find no common pattern regarding the consequences of measurement error for estimations, estimates using student information and estimates based on administrative information differ significantly and lead to potentially biased estimates or problems in the generalizability of results from LSA such as PISA or the ÜGK. This highlights that the robustness of the estimates relying on student information is challenged. Furthermore, the effect of the absence of a parent highlights the unequal pre-conditions that students face when being asked about their social background. We encourage that subsequent research will continue to investigate causes and possible solutions to the problem that arises from measurement error and item non-response in proxy variables, especially in the light that many ongoing studies - such as PISA - will continue to rely on students making statements about their parents. In that sense, we want to invigorate the use of administrative data: However limited, administrative data present a cost-efficient way to assess measurement error and item non-response while simultaneously holding the potential of creating alternative measures for socio-economic background characteristics or being used in imputation models.

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Appendix - Asking Students about their Parents: How Item Non-Response and Measurement Error Depend on Construct Salience and Students' Cognitive Abilities

Replication - Data and Materials

This is a brief description of how to obtain the data used in this study, which relies on data from the "Verification of the Achievement of Basic Competencies (ÜGK)" (ÜGK) and on administrative records of the Federal Statistical Office (FSO) of Switzerland. The ÜGK scientific use files (SUF) are available at `https://www.swissubase.ch/` (`https://www.swissubase.ch/de/catalogue/studies/13413/19390/overview`; `https://www.swissubase.ch/de/catalogue/studies/12954/19391/overview`). A valid data user agreement is required for downloading the data. See https://www.uegk-schweiz.ch/ for more information on the ÜGK/COFO/VECOF study (website currently available in German, French and Italian).

The record linkage is not available online. For a record linkage, an additional application has to be made to the Swiss Conference of Cantonal Ministers of Education (EDK). With the approval of the application, the data linkage process with the FSO can be started. As this process is rather long and time-intensive, reaching out to the author is recommended, as he can provide valuable insights into the process. For this study, the contract number with the FSO was XXXXXX *(disclosed because of blinding the manuscript)*.

Further, the author made the code available to reproduce the results of this paper in an online repository at OSF *(Link not provided due to blinding the manuscript)*. It contains the files to aggregate and link the FSO data to the SUF files and a script to reproduce the analysis. The analysis was done using R and STATA.

Additional Tables

Table 1: Cohen's Kappa of the Ordinal Proxy Variables

Variable	Year	N	Cohen's Kappa[1]	95%-CI	
Migration Status	2016	16939	0.927	0.919	0.935
Migration Status	2017	15655	0.871	0.861	0.88
Mothers' Education	2016	7294	0.514	0.496	0.532
Mothers' Education	2017	6156	0.342	0.322	0.362
Fathers' Education	2016	6890	0.477	0.46	0.495
Fathers' Education	2017	5999	0.294	0.275	0.314
HISCED	2016	8245	0.456	0.439	0.472
HISCED	2017	6735	0.287	0.268	0.306

[1] We calculated the weighted Cohen's Kappa. The values representing the agreement can be interpreted as: 0.0 - 0.20 "poor", 0.21 - 0.40 "fair", 0.41 - 0.60 "moderate", 0.61 - 0.80 "substantial", and 0.81 - 1.00 "almost perfect"(Pu et al., 2011; Lien et al., 2001) .

Table 2: ICC of the Metric Proxy Variables

	Year	N	ICC	95%-CI	
Mothers ISEI08	2016	2345	0.815	0.799	0.829
Mothers ISEI08	2017	1864	0.834	0.818	0.849
Fathers ISEI08	2016	2680	0.851	0.84	0.862
Fathers ISEI08	2017	2501	0.821	0.807	0.835
HISEI08	2016	5078	0.689	0.671	0.705
HISEI08	2017	4647	0.733	0.717	0.748

An ICC value of 0 indicates no agreement and 1 perfect agreement (Ranganathan et al., 2017).

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Table 3: Average Marginal Effects for Item Non-Response

	Model 1	Model 2	Model 3	Model 4
Construct Salience (ÜGK) - Ref. = *Migration Status*				
Mothers' HISEI	0.16	0.17	0.17	0.13
	(0.16 - 0.17)	(0.16 - 0.17)	(0.17 - 0.18)	(0.12 - 0.14)
Fathers' HISEI	0.10	0.093	0.087	0.071
	(0.10 - 0.11)	(0.090 - 0.097)	(0.084 - 0.091)	(0.065 - 0.076)
Moethers' Education	0.087	0.089	0.093	0.082
	(0.084 - 0.090)	(0.086 - 0.092)	(0.090 - 0.096)	(0.076 - 0.088)
Fathers' Education	0.10	0.094	0.088	0.078
	(0.099 - 0.11)	(0.091 - 0.097)	(0.084 - 0.091)	(0.073 - 0.084)
Performance (ÜGK)	-0.038***	-0.029***	-0.024***	-0.018***
	(-0.040 - -0.036)	(-0.031 - -0.027)	(-0.025 - -0.022)	(-0.021 - -0.015)
Wave = 2016 (ÜGK)	-0.0064***	-0.0098***	-0.0094***	-0.0045
	(-0.0099 - -0.0029)	(-0.013 - -0.0063)	(-0.013 - -0.0059)	(-0.011 - 0.0015)
Relative Age (FSO)			-0.0019*	-0.0024
			(-0.0038 - -0.000053)	(-0.0057 - 0.00080)
Gender = Male (FSO)			0.021***	0.022***
			(0.018 - 0.025)	(0.016 - 0.028)
Role Model Effect (FSO)			-0.0048***	-0.0067***
			(-0.0071 - -0.0025)	(-0.011 - -0.0028)
Parent not at Home (FSO)			0.065***	0.056***
			(0.059 - 0.071)	(0.044 - 0.068)
Migration Status (FSO) - Ref. = *Native*				
2nd Generation			0.032***	0.016**
			(0.026 - 0.039)	(0.0055 - 0.027)
1st Generation			0.042***	0.014
			(0.026 - 0.057)	(-0.011 - 0.038)
Test language not spoken at Home (ÜGK)			0.021***	0.026***
			(0.015 - 0.027)	(0.015 - 0.036)
HISCED (FSO) - Ref. = *Compulsory Education*				
Upper Secondary Education				-0.019**
				(-0.033 - -0.0049)
Tertiary Education				-0.018*
				(-0.034 - -0.0029)
HISEI (FSO)				-0.00077
				(-0.0043 - 0.0028)
BIC	136977.0	89577.4	88611.4	22868.9
Nr. of Observations	213000	163397	163397	47461
Nr. of Students	42600	32769	32769	9507

Notes: Marginal Effects for Item Non-Response; 95% confidence intervals in parentheses. Predictors in Model 1: student performance, wave indicator and the interaction term of performance and wave. Model 2 uses the same predictors while using the same sample as Model 2. Model 3 uses additional predictors: relative age, gender, a dummy for the role model effect, a dummy for an absent parent, migration status, and whether the test language is spoken at home. Model 4 additionally uses the highest parental education and occupation status per household as predictors. All models further include intercepts for all the levels of construct salience. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Marginal Effects for Measurement Errors

	Model 1	Model 2	Model 3	Model 4
Construct Salience (ÜGK) - Ref. = *Migration Status*				
Motehrs' ISEI	0.44	0.44	0.45	0.45
	(0.43 - 0.46)	(0.43 - 0.46)	(0.43 - 0.46)	(0.43 - 0.47)
Fathers' ISEI	0.45	0.45	0.44	0.45
	(0.43 - 0.46)	(0.43 - 0.46)	(0.43 - 0.46)	(0.43 - 0.46)
HISEI	0.59	0.59	0.59	0.60
	(0.58 - 0.60)	(0.58 - 0.60)	(0.58 - 0.60)	(0.59 - 0.61)
Mothers' Education	0.32	0.32	0.32	0.32
	(0.31 - 0.33)	(0.31 - 0.33)	(0.31 - 0.33)	(0.31 - 0.33)
Fathers' Education	0.37	0.37	0.37	0.37
	(0.36 - 0.38)	(0.36 - 0.38)	(0.36 - 0.38)	(0.36 - 0.38)
HISCED	0.35	0.35	0.35	0.35
	(0.35 - 0.36)	(0.35 - 0.36)	(0.34 - 0.36)	(0.34 - 0.36)
Performance (ÜGK)	-0.027	-0.026	-0.023	-0.021
	(-0.031 - -0.023)	(-0.030 - -0.023)	(-0.027 - -0.019)	(-0.027 - -0.015)
Wave = 2016 (ÜGK)	-0.065	-0.065	-0.064	-0.061
	(-0.072 - -0.058)	(-0.072 - -0.058)	(-0.071 - -0.057)	(-0.071 - -0.050)
Relative Age (FSO)			-0.00056	-0.0026
			(-0.0044 - 0.0032)	(-0.0082 - 0.0030)
Gender = Male (FSO)			0.0059	-0.0017
			(-0.0011 - 0.013)	(-0.012 - 0.0085)
Role Model Effect (FSO)			-0.0053	-0.0076
			(-0.012 - 0.0011)	(-0.017 - 0.0016)
Parent not at Home (FSO)			0.028^{***}	0.039^{***}
			(0.014 - 0.041)	(0.019 - 0.058)
Migration Status (FSO) - Ref. = *Native*				
2^{nd} Generation			0.014^{*}	-0.0097
			(0.0012 - 0.027)	(-0.029 - 0.0095)
1^{st} Generation			0.0086	-0.038
			(-0.023 - 0.040)	(-0.082 - 0.0055)
Test language not spoken at Home (ÜGK)			0.040^{***}	0.022^{*}
			(0.027 - 0.053)	(0.0032 - 0.040)
HISCED (FSO) - Ref. = *Compulsory Education*				
Upper Secondary Education				-0.034^{*}
				(-0.062 - -0.0065)
Tertiary Education				0.022
				(-0.0062 - 0.051)
HISEI (FSO)				-0.024^{***}
				(-0.029 - -0.018)
BIC	83434.6	79515.1	79481.2	56987.4
Nr. of Observations	93028	89779	89779	52807
Nr. of Students	33608	32617	32617	9499

Notes: Marginal Effects for Measurement Error; 95% confidence intervals in parentheses. Predictors in Model 1: student performance, wave indicator and the interaction term of performance and wave. Model 2 uses the same predictors while using the same sample as Model 2. Model 3 uses additional predictors: relative age, gender, a dummy for the role model effect, a dummy for an absent parent, migration status, and whether the test language is spoken at home. Model 4 additionally uses the highest parental education and occupation status per household as predictors. All models further include intercepts for all the levels of construct salience. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Logistic Models on Item Non-Response

	Migration Status	Mothers' ISEI	Fathers' ISEI	Mothers' Education	Fathers' Education
Performance (ÜGK)	0.68^{***}	0.88^{***}	0.80^{***}	0.77^{***}	0.77^{***}
	(0.57 - 0.80)	(0.85 - 0.92)	(0.76 - 0.85)	(0.72 - 0.83)	(0.72 - 0.83)
Wave = 2016 (ÜGK)	0.48^{***}	0.37^{***}	0.52^{***}	2.64^{***}	2.41^{***}
	(0.35 - 0.65)	(0.35 - 0.39)	(0.48 - 0.56)	(2.43 - 2.87)	(2.22 - 2.61)
Wave = 2016 * Performance	0.79	0.79^{***}	0.85^{***}	0.86^{***}	0.84^{***}
	(0.60 - 1.04)	(0.74 - 0.84)	(0.78 - 0.92)	(0.79 - 0.93)	(0.78 - 0.91)
Migration Status (FSO) - Ref. = *Native*					
2nd Generation	1.11	1.24^{***}	1.52^{***}	1.58^{***}	1.58^{***}
	(0.71 - 1.73)	(1.13 - 1.36)	(1.35 - 1.70)	(1.40 - 1.77)	(1.41 - 1.77)
1st Generation	2.23^{*}	1.54^{***}	1.71^{***}	1.48^{**}	1.59^{***}
	(1.17 - 4.25)	(1.26 - 1.88)	(1.34 - 2.18)	(1.14 - 1.91)	(1.23 - 2.04)
Test language not spoken at Home (ÜGK)					
	2.76^{***}	1.30^{***}	1.21^{**}	1.25^{***}	1.23^{***}
	(1.81 - 4.21)	(1.19 - 1.42)	(1.08 - 1.36)	(1.11 - 1.40)	(1.10 - 1.38)
Gender = Male (FSO)	2.09^{***}	1.35^{***}	1.22^{***}	1.42^{***}	1.21^{***}
	(1.58 - 2.77)	(1.27 - 1.43)	(1.13 - 1.32)	(1.31 - 1.53)	(1.12 - 1.30)
Relative Age (FSO)	1.07	0.97	0.93^{***}	1.00	1.01
	(0.93 - 1.24)	(0.94 - 1.00)	(0.89 - 0.96)	(0.96 - 1.04)	(0.97 - 1.05)
Parent not at Home (FSO)		1.24^{*}	2.13^{***}	1.85^{***}	1.95^{***}
		(1.04 - 1.49)	(1.96 - 2.31)	(1.53 - 2.23)	(1.79 - 2.12)
Nr. of Observations	32769	32545	32769	32545	32769
BIC	2670.4	28535.5	20375.3	19393.5	20125.8

Notes: Conditional odds ratios; 95% confidence intervals in parentheses. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Marginal Effects for Item Non-Response

	Migration Status	Mothers' ISEI	Fathers' ISEI	Mothers' Education	Fathers' Education
Performance (ÜGK)	-0.0033***	-0.029***	-0.025***	-0.030***	-0.032***
	(-0.0044 - -0.0023)	(-0.033 - -0.024)	(-0.028 - -0.021)	(-0.034 - -0.027)	(-0.036 - -0.029)
Wave = 2016 (ÜGK)	-0.0044***	-0.13***	-0.054***	0.080***	0.076***
	(-0.0064 - -0.0024)	(-0.14 - -0.13)	(-0.061 - -0.048)	(0.074 - 0.086)	(0.070 - 0.083)
Migration Status (FSO) - Ref. = *Native*					
2nd Generation	0.00073	0.030***	0.040***	0.042***	0.043***
	(-0.0024 - 0.0039)	(0.017 - 0.044)	(0.028 - 0.052)	(0.030 - 0.054)	(0.031 - 0.055)
1st Generation	0.0056*	0.065***	0.053***	0.035**	0.044**
	(0.0010 - 0.010)	(0.032 - 0.099)	(0.025 - 0.082)	(0.0087 - 0.061)	(0.016 - 0.072)
Test language not spoken at Home (ÜGK)					
	0.0071***	0.038***	0.017**	0.020***	0.019***
	(0.0040 - 0.010)	(0.024 - 0.051)	(0.0066 - 0.028)	(0.0089 - 0.030)	(0.0084 - 0.030)
Gender = Male (FSO)	0.0052***	0.041***	0.017***	0.029***	0.016***
	(0.0031 - 0.0073)	(0.033 - 0.049)	(0.011 - 0.024)	(0.023 - 0.035)	(0.0099 - 0.023)
Relative Age (FSO)	0.00049	-0.0039	-0.0065***	-0.0000066	0.00045
	(-0.00052 - 0.0015)	(-0.0083 - 0.00039)	(-0.0098 - -0.0031)	(-0.0034 - 0.0033)	(-0.0029 - 0.0038)
Parent not at Home (FSO)		0.032*	0.078***	0.063***	0.067***
		(0.0041 - 0.059)	(0.068 - 0.088)	(0.040 - 0.086)	(0.057 - 0.076)
Nr. of Observations	32769	32545	32769	32545	32769

Notes: Marginal Effects; 95% confidence intervals in parentheses. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Logistic Models on Measurement Error

	Migration Status	Mothers' ISEI	Fathers' ISEI	HISEI	Mothers' Education	Fathers' Education	HISCED
Performance (ÜGK)	0.69^{***}	1.04	1.05	1.01	0.91^{**}	0.88^{***}	0.84^{***}
	(0.63 - 0.75)	(0.94 - 1.15)	(0.96 - 1.14)	(0.94 - 1.07)	(0.87 - 0.97)	(0.83 - 0.93)	(0.80 - 0.89)
Wave = 2016 (ÜGK)	0.44^{***}	0.94	0.76^{***}	0.98	0.58^{***}	0.62^{***}	0.67^{***}
	(0.38 - 0.51)	(0.83 - 1.07)	(0.67 - 0.85)	(0.90 - 1.07)	(0.54 - 0.63)	(0.58 - 0.67)	(0.62 - 0.71)
Wave = 2016 * Performance	0.91	0.98	0.97	0.98	0.92	0.93	0.93
	(0.79 - 1.05)	(0.86 - 1.12)	(0.86 - 1.09)	(0.89 - 1.07)	(0.86 - 1.00)	(0.86 - 1.01)	(0.87 - 1.00)
Migration Status (FSO) - Ref. = *Native*							
2nd Generation	1.88^{***}	0.92	0.98	0.93	1.01	0.91	1.02
	(1.48 - 2.40)	(0.74 - 1.15)	(0.80 - 1.19)	(0.80 - 1.07)	(0.89 - 1.15)	(0.80 - 1.03)	(0.91 - 1.14)
1st Generation	2.19^{***}	1.08	0.97	1.19	0.91	0.73^{*}	0.78
	(1.47 - 3.26)	(0.61 - 1.91)	(0.59 - 1.59)	(0.81 - 1.75)	(0.67 - 1.23)	(0.54 - 0.99)	(0.59 - 1.03)
Test language not spoken at Home (ÜGK)							
	2.48^{***}	0.99	0.98	0.95	1.14^{*}	1.18^{**}	1.16^{**}
	(1.96 - 3.13)	(0.80 - 1.23)	(0.81 - 1.18)	(0.83 - 1.10)	(1.01 - 1.29)	(1.05 - 1.33)	(1.04 - 1.30)
Gender = Male (FSO)	1.24^{**}	0.99	1.00	0.99	1.12^{**}	1.00	0.95
	(1.08 - 1.41)	(0.88 - 1.13)	(0.89 - 1.12)	(0.91 - 1.08)	(1.04 - 1.21)	(0.93 - 1.08)	(0.89 - 1.02)
Relative Age (FSO)	1.11^{**}	1.03	0.97	0.98	0.99	1.00	0.96
	(1.03 - 1.19)	(0.96 - 1.10)	(0.91 - 1.03)	(0.93 - 1.02)	(0.95 - 1.03)	(0.96 - 1.04)	(0.93 - 1.00)
Parent not at Home (FSO)		1.32	1.22^{*}		1.29	1.32^{***}	
		(0.84 - 2.08)	(1.03 - 1.44)		(0.94 - 1.76)	(1.17 - 1.50)	
Nr. of Observations	32535	3953	4973	9238	12641	12382	14057
BIC	8118.9	5542.5	6928.7	12320.0	16059.1	16367.8	18435.7

Notes: Conditional odds ratios; 95% confidence intervals in parentheses. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Marginal Effects for Measurement Error

	Migration Status	Mothers' ISEI	Fathers' ISEI	HISEI	Mothers' Education	Fathers' Education	HISCED
Performance (ÜGK)	-0.011***	0.0078	0.0069	-0.0015	-0.028***	-0.039***	-0.047***
	(-0.014 - -0.0094)	(-0.0091 - 0.025)	(-0.0085 - 0.022)	(-0.012 - 0.0094)	(-0.037 - -0.019)	(-0.048 - -0.030)	(-0.056 - -0.039)
Wave = 2016 (ÜGK)	-0.022***	-0.016	-0.071***	-0.0061	-0.12***	-0.11***	-0.095***
	(-0.026 - -0.018)	(-0.047 - 0.015)	(-0.099 - -0.043)	(-0.026 - 0.014)	(-0.14 - -0.11)	(-0.13 - -0.097)	(-0.11 - -0.080)
Migration Status (FSO) - Ref. = *Native*							
2nd Generation	0.018***	-0.020	-0.0060	-0.018	0.0023	-0.022	0.0040
	(0.011 - 0.025)	(-0.075 - 0.034)	(-0.054 - 0.042)	(-0.052 - 0.016)	(-0.026 - 0.031)	(-0.051 - 0.0064)	(-0.023 - 0.031)
1st Generation	0.022***	0.019	-0.0080	0.041	-0.020	-0.071*	-0.058
	(0.011 - 0.034)	(-0.12 - 0.16)	(-0.13 - 0.11)	(-0.050 - 0.13)	(-0.085 - 0.045)	(-0.14 - -0.0056)	(-0.12 - 0.0067)
Test language not spoken at Home (ÜGK)							
	0.026***	-0.0032	-0.0049	-0.011	0.030*	0.039**	0.035**
	(0.019 - 0.032)	(-0.057 - 0.051)	(-0.052 - 0.042)	(-0.044 - 0.022)	(0.0014 - 0.058)	(0.010 - 0.068)	(0.0094 - 0.061)
Gender = Male (FSO)	0.0060**	-0.0015	0.000085	-0.0020	0.025**	0.00081	-0.012
	(0.0023 - 0.0098)	(-0.033 - 0.030)	(-0.028 - 0.028)	(-0.022 - 0.018)	(0.0081 - 0.041)	(-0.016 - 0.018)	(-0.027 - 0.0045)
Relative Age (FSO	0.0030**	0.0075	-0.0085	-0.0054	-0.0032	-0.00067	-0.0084
	(0.00097 - 0.0051)	(-0.0094 - 0.025)	(-0.024 - 0.0069)	(-0.016 - 0.0055)	(-0.012 - 0.0056)	(-0.0098 - 0.0085)	(-0.017 - 0.00013)
Parent not at Home (FSO)		0.069	0.049*		0.057	0.067***	
		(-0.044 - 0.18)	(0.0076 - 0.090)		(-0.015 - 0.13)	(0.036 - 0.097)	
Nr. of Observations	32535	3953	4973	9238	12641	12382	14057

Notes: Marginal Effects; 95% confidence intervals in parentheses. Parentheses behind the variable names declare the data source of the variable. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Paper Two - Parental Earnings and Student Performance – Making Use of Administrative Data

Abstract: This study proposes the use of administrative data on parental earnings as a feasible way to obtain valid information on the financial situation of a family and to use this information as a predictor for student performance in addition to the commonly used variables on parental education and occupational status. It draws on pilot data from a national Large-Scale Assessment of 2nd-grade pupils (approximately 8-year-old children) in Switzerland (N = 4'333) which was accompanied by a questionnaire for the parents and linked to data on the parents' earnings from administrative records. The analysis shows that the percentage of missing information in the administrative data is lower than in the questionnaire for the parents. Furthermore, participation in the parental questionnaire is systematic regarding migration status, student performance, and parental earnings which could potentially bias estimates from complete case analyses. The comparison of the point estimates of the SES variables parental education, parental occupational status, the number of books at home, and parental earnings reveals differences between weighted and unweighted complete case analyses. Lastly, models using data from multivariate imputations show even larger differences to the point estimates from complete case analysis. The results highlight that parental earnings substantially explain student performance even under the control of other family and student characteristics. Furthermore, they emphasise the benefits of using the information on earnings from a source with high external validity for explaining student performance. [a b]

Keywords: Income; Large-Scale Assessment; Socio-Economic Status; Education; Switzerland; Record Linkage

[a] This paper is a working paper.
[b] OSF repository available at: https://osf.io/ep48d/

Introduction

Prior research has identified family income, parental educational attainment and occupational status to be the "big 3" social background characteristics to determine the socioeconomic status (SES), a characteristic that has been found relevant in a variety of life domains (Willms & Tramonte, 2019). For example, the Programme for International Student Assessment (PISA) repeatedly demonstrated a socioeconomic gap in student performance. Across countries, the average differences in test scores in reading and mathematics between the top and the bottom quarter of the distribution of the Economic, Social and Cultural Status (ESCS), a PISA-specific operationalisation of the SES, correspond to nearly one standard deviation (Blanden et al., 2022). For comparison, learning gains in a test during one school year amount to approximately one-third to one-quarter of a standard deviation (Woessmann, 2016). This shows that the influence of the family background on student performance is non-negligible. However, among these three dimensions, Large-Scale Assessments (LSA) often refrain from measuring the financial dimension. The practical reason for this is that LSAs tend to rely on pupils' self-reports which are prone to measurement error and item non-response (Engzell & Jonsson, 2015; Ensminger et al., 2000; Kreuter et al., 2010), especially as concrete information on the financial situation of a family, such as wages, are potentially unknown to children. In addition, there is an ongoing theoretical debate on how SES should be measured (Avvisati, 2020; Willms & Tramonte, 2019). For example, some scholars argue that eliciting the occupational status of the parents is sufficient as it predominantly determines wages (Willms & Tramonte, 2019). A meta-analysis by Sirin (2005) for the years 1990-2000 even shows that the sample of studies analysing educational outcomes foremost relied on the educational attainment of the parents as the main or even single determinant of the SES. Nonetheless, the literature holds compelling evidence emphasising the importance of the financial dimension of the social background characteristics for educational outcomes in children. A recent meta-analysis (Cooper & Stewart, 2021) of studies that aim for a causal interpretation regarding the impact of family income on educational outcomes finds clear evidence of a positive relationship. For instance, Dahl and Lochner (2012) show in their analysis of low- and middle-income households in the US that an additional 1'000 US$ per year in family income raises test scores by 6 per cent of a standard deviation. Similarly, there is a correlational relationship between family income and educational outcomes. In the US, students from poor households show lower reading and mathematics skills in kindergarten or complete fewer years of schooling (Duncan, Magnuson, & Votruba-Drzal, 2017). Considering the methodological challenges in measuring the financial dimension of the social background characteristics, LSAs incorporate proxy questions that ask about household possessions. On the one hand, such proxies are intended to reflect the permanent components of the financial situation of a family. On the other hand, these questions should be comparably easy for pupils to answer themselves. Yet, the validity of household possessions as proxies is being challenged, in particular in the context of Western countries and from a comparative perspective (Marks & O'Connell, 2021). Another way by which some LSAs try to elicit the financial situation of a family is via a separate questionnaire sent to the parents of the surveyed children, e.g., PISA (OECD, 2019), which can be prone to selection bias. The use of administrative data, as an alternative way to capture the financial situation of a family, is mostly neglected. This paper contributes to the literature as it uses administrative data on parental earnings to obtain valid information on the financial situation of a family and to investigate how this information explains student performance. It relies on the pilot of the Verification of Attainment of Basic Competencies H4 (ÜGK H4 pilot), a national LSA of 8-year-olds in Switzerland (N = 4'333). The ÜGK H4 pilot has the advantage that it was not only possible to link the data to administrative records but that because of the age of the students and the fear of invalid self-reports, it was also accompanied by a questionnaire for the parents. Hence, this study can assess the use of administrative records on the financial situation of a family and its impact on student performance while controlling for information on other social background characteristics that do not rely on pupils' self-reports. The next section presents evidence regarding the effect of a family's financial resources on educational outcomes in children. Thereafter, the study discusses the reasons why LSAs do not elicit the financial situation of a family directly or use instruments to approximate it. Then, the analysis strategy of the study is clarified alongside a description of the data and the operationalization of the variables. In the fourth part, the results are presented, while the last section critically discusses and sums up the study.

Effects of Financial Resources on Educational Outcomes in Children

Previous literature presents a variety of empirical evidence that supports the claim that parents' financial resources are linked to children's educational outcomes. For example, Grätz and Wiborg (2020) show for Germany, Norway and the US, that earnings and wealth have an effect on student performance and that the socioeconomic differences are stronger at the bottom of the performance distribution. Studies even find causal effects regarding shocks in the disposable income on student performance (Black et al., 2014; Duncan et al., 2011; Elstad & Bakken, 2015), and Duncan, Kalil, and Ziol-Guest (2017) make a compelling case that the income gap between high and low-income children accounts for gaps in school completion, college attendance, and college graduation in the US. Pfeffer (2018) complements these findings with similar results for gaps in family wealth. Likewise, Van Bussel and Fecteau (2022) show descriptive findings from Canada where students from high-income families are more likely to pursue post-secondary education and to graduate at a higher and faster rate. All the above highlights the importance of the financial resources of a family for the educational outcomes of their children. Duncan, Magnuson, and Votruba-Drzal (2017) summarise correlational and causal inference for the relationship between the pecuniary dimension of family background characteristics and educational outcomes while discussing theoretical explanations. They point out that low financial resources, especially poverty, are associated with a particular constellation of disadvantageous circumstances. These circumstances, i.e., parents with low education, unemployment, bad health, or living in deprived neighbourhoods, can all influence educational outcomes. However, scarce financial resources are independently affecting educational outcomes. For example, independent of the education of parents or their occupational status, scarce financial resources might prohibit parents from enabling their children to engage in extracurricular courses and training.

One perspective that incorporates these interdependencies originated in the work of Elder (2018). It states that families experience shocks in their everyday lives for which financially disadvantaged families are less capable of compensating, creating high levels of stress within the family. While struggling to make ends meet, psychological well-being can be harmed, interactions between family members can become more hostile, and parenting can be affected. Simultaneously, poorer families tend to live in more adverse neighbourhood environments regarding noise, pollution, crime rates, and traffic contributing to psychological distress that can have disadvantageous effects on student performance. The second perspective, represented by Becker (1991) and his household production theory, highlights that under similar parental investment preferences, children from disadvantaged backgrounds lag behind their advantaged counterparts because of scarcer resources. For one part, this is caused by limited monetary resources which can be put towards educational inputs. Another part refers to the time parents can spend with their children, which likely differs as parents with lower wages are more likely to work more hours or to be engaged in non-standard work compared to their more affluent counterparts. Put together, both perspectives complement each other: financially better-off families should be more likely to provide a child with an enriching, safe, and stressless environment and be able to invest time and money into their children's education. Lastly, cultural theories based on Lewis (1968) further stress that poverty in combination with institutional factors and residential segregation can lead to differences in norms, beliefs, and preferences which shape parenting, the investment in children, as well as behavioural dispositions (Duncan, Magnuson, & Votruba-Drzal, 2017). Furthermore, theoretical and empirical work also emphasizes the timing of scarce financial resources during childhood. If skills are cumulative and the return on investment in the education of a child is larger for children with higher prior levels of skills (Cunha & Heckman, 2007), then early socioeconomic disparities that hinder skill development in the first place should grow larger over time. In sum, financial distress can cause disadvantages regarding educational outcomes. However, it would be short-sighted not to consider the timing alongside coinciding factors. Given the theoretical and empirical evidence, a promotion of a "materialist" view of SES by the "American Psychological Association Task Force on Socioeconomic Status" (APA, 2007) that emphasizes measuring the financial resources of a family is thus not surprising.

The Problem with Self-Reports and Proxy Questions about the Financial Situation of a Family

With the empirical findings and the theoretical argumentation in mind as to why the financial situation of a family matters for their children's educational outcomes, it is now time to ask how LSAs incorporate this dimension of background characteristics in their surveys. The current debate about PISAs ESCS, on which many studies orient their data collection (Avvisati, 2020), illustrates how measures of SES are being challenged. While LSAs often profess that the financial situation of the family is considered in their composite measures of SES, they rather approximate the pecuniary dimension of family background characteristics by using household possession scales. The ESCS is a prime example of such a composite measure as it is based on parental education, occupation, and family income, where the latter is captured by an index of household possessions such as phones or cars (ibid.). While there is critique regarding the measurement of each of the components of the ESCS, e.g., the "International Socio-Economic Index" (Ganzeboom & Treiman, 2019; Ganzeboom et al., 1992) for parental occupation or the International Standard Classification of Education (ISCED, UNESCO, 2012) for parental education, this study focuses solely on the financial dimension. One argument held against the use of household possession scales is that for most Western countries the goods such as cars, cell phones, or computers are not indicative (anymore) of the financial resources of a family. Furthermore, the possession of a car or having multiple bathrooms is less likely in dense city centres even though rents are high compared to the suburbs or rural areas (Marks & O'Connell, 2021). In this sense, the list of possessions would have to be updated from wave to wave to hold specific goods that allow for approximating the financial resources of a family within a country-specific context. However, using simple questions about material possessions that allow approximating the financial situation of a family is eventually still the most promising way given measurement error and item non-response associated with pupils' and students' self-reports (Engzell & Jonsson, 2015; Ensminger et al., 2000; Kreuter et al., 2010). To overcome the issues of these self-reports and the inability of pupils to answer questions about the financial situation of their families, LSAs opt for a separate questionnaire sent to students' parents. Despite a more credible source of information and the possibility of asking detailed questions about the financial situation, the problem of systematic unit and item non-response remains (Turrell, 2000). Furthermore, the advantages of questionnaires for the parents might be outweighed by the resources required in the field and the potentially unsatisfying returns considering the declining response rates in surveys seen in recent years (Luiten et al., 2020). Given all the above, it is not surprising that in some cases the composite measure of SES applied in LSAs does not even include proxies for the financial situation of a family. Such SES-composites can be found in the ÜGK for Switzerland, the International Computer and Information Literacy Study (ICILS), or the Educational Standard Survey (BIST-Ü) in Austria (Pham et al., 2016, 2017). Measures of SES not including the financial situations of the family, however, neglect an important characteristic of the social background as each dimension of the SES has different mechanisms through which educational outcomes are affected (Ensminger & Fothergill, 2003). For example, financially more affluent families spend considerably more on extracurricular learning activities (Schneider et al., 2018) while highly educated parents hold different beliefs and expectations about their child's education (Davis-Kean, 2005; Davis-Kean et al., 2021). Concerning potential measurement error and item non-response of student self-reports, the complex constitution of financial resources, selectivity in participation in questionnaires for the parents, and the insufficiency of proxy questions to capture the financial situation of a family, it appears plausible why LSAs do not simply ask about earnings. At the same time, it seems unreasonable to not include the financial situation of a family, although the past literature has identified the financial situation of a family as one of the "big 3" dimensions of social background characteristics in educational research (Willms & Tramonte, 2019). If not accounted for, these problems (e.g., measurement error and non-response), that concern all SES variables, hold the potential that results using these variables draw an inaccurate picture of the correlations between the social characteristics of the students and their educational performance. Hence, administrative data on parental earnings pose a viable solution to these issues as they have a high external validity and few missings.

This Study

This study aims to demonstrate the use of administrative data on parental earnings to overcome the issues of measuring the financial dimension of the socioeconomic status of children and to use this information to predict student performance. Namely, these issues concern the use of household possession scales to approximate income, the validity of self-reports, and the potential selectivity of participation in questionnaires for the parents. This study showcases a national LSA of Switzerland, the pilot of the Verification of the Achievement of Basic Competencies H4 (ÜGK H4 pilot). The ÜGK H4 pilot assesses basic competencies in mathematics and the first language of second-grade students and is used to set the levels of the basic competencies for the main study. Furthermore, it holds the advantage that the ÜGK H4 pilot can mitigate claims about the validity of self-reports by pupils because an additional questionnaire for the parents was conducted. Hence, this study can test whether the coverage of the administrative data surpasses that of the parental questionnaire, whether participation in the questionnaire for the parents is selective regarding sociodemographic characteristics, and if parental earnings have an independent effect on student performance even under the control of other family background characteristics taken from a more credible source than pupils' self-reports. Complete case analyses are compared to analyses that use weights to account for the selectivity in the parental questionnaire, for which is expected that the effect of parental earnings is larger, if participation in the parental survey is dependent on parental earnings.

Data

The ÜGK H4 pilot in 2022 (EDK, 2024) was designed to be representative of the student population of 2nd-grade pupils of the three largest language regions (German = 36.2%; French 35.1%; Italian 28.7%) of Switzerland and comprises 4'333 students (49% female, 51% male). In Switzerland, the approximate age of the pupils in second grade is between eight and nine years (mean age = 8.43 years) as the official age at which pupils enrol in the mandatory kindergarten for two years is between four and five. The sampling took place at the school level and then students in the second grade were drawn. The computer-based assessments were carried out in the schools. Each student was assessed in mathematics, reading abilities, and listening skills and completed a background questionnaire. At the end of the session, each student was provided with a sheet to take home containing a personalized link which led the parents to the online parent questionnaire (Herzing et al., 2024). This questionnaire asked for information on parental education, occupational status, and the number of books at home, the three components of SES applied in the former waves of the ÜGK. In total, 2'540 parents (58.6%) have completed the survey to more than 80%.

The link to administrative data could only be established if the pupil was identified unanimously, using the name, sex, and birthdate together with the municipality. This is the case for 4'271 pupils or 98.6% of the ÜGK H4 pilot sample. The registry of permanent and non-permanent inhabitants of Switzerland, the STATPOP (FSO, 2022), further contains the link to the parents. For about 6.6% the STATPOP contains no link to any of the parents, for 1.1% it does not hold information on the father and for 0.04% this is the case for the mother. Further, the link to the parents was used to identify them in the registry of the Central Compensation Office (CCO) which holds the information on the parents' earnings (CCO, 2022a, 2022b, 2022c, 2022d, 2022e, 2022f). The CCO is a federal office that oversees the pensions and social security accounts in Switzerland. Because monthly earnings are subject to mandatory contributions to the old age pension in Switzerland, the CCO keeps track of the individual accounts and thus holds the information on annual earnings per person. These accounts can even reckon for multiple employments at the same time. Furthermore, the data is also valid for self-employed. In this case, earnings are reported annually and after taxes. However, the data has two important limitations. First, if a person has no earnings in a year, the CCO assigns a value as earnings that is derived from the savings of a person. Second, the entries in the CCO data are grouped by the employment of a person. Therefore, monthly fluctuations of earnings from the same employment are discarded.

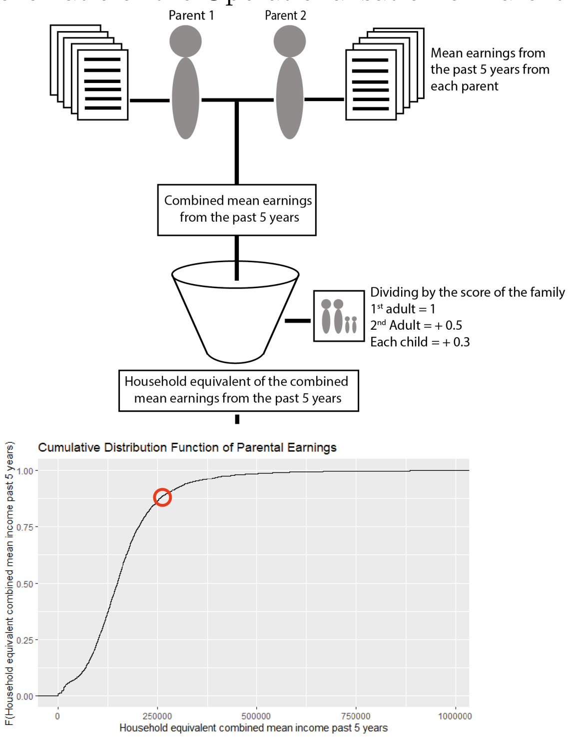
Analysis Strategy

To investigate whether administrative data on parental earnings can be used to resolve shortcomings in the way SES is measured in LSA and explain variation in student performance, several descriptive and analytical approaches are conducted. First, descriptive results present the coverage of the variables concerning the family background characteristics. This is informative as the administrative data should hold fewer missing information than the parental survey. The complete sample is compared to the sample for which the questionnaire for the parents was completed to more than 80%. Large differences in the coverage of the variables from the administrative data between the two samples would indicate a bias in the administrative records. Given that the questionnaire for the parents is voluntary, one could suspect that participation is selective. Therefore, logistic regressions are applied to explain the participation status in the questionnaire for the parents. This is done by conducting several models where the first model only uses predictors that have full coverage (auxiliary variables) for the entire sample. The second model uses the predictors from the first model. At the same time, the sample is held constant between models two and three, which resembles a complete case analysis that introduces additional predictors from the administrative data to the model which allows identifying whether the sample restriction (model 2) or the additional variables (model 3) affect the point estimates. Lastly, the question is whether the information on parental earnings explains variance in student performance after controlling for socioeconomic status and additional student characteristics. Thus, two models are fitted to the data with variables commonly used to explain student performance while only one model includes the variable for parental earnings. To account for the potential selectivity of the parental survey, the second model was repeated using the inverted probability weights (IPW) from logistic regressions that aim to correct this bias. Once using the IPW from a model using only auxiliary variables, (language region, municipality type, and special educational needs status) and once with additional information from the administrative data (student gender, migration status, and parental earnings). The comparison should reveal whether the potential selectivity of the parental survey has consequences for the point estimates of complete case analyses. The same models are fitted to multiply imputed data using the "mice" (multivariate imputation by chained equations) package (Buuren & Groothuis-Oudshoorn, 2011) in R. This is done for two reasons. First, using IPW to correct the bias resulting from missing information that is not at random is inefficient as the information in incomplete cases is only used to calculate the weights. Multiple imputation (MI) is more efficient in this regard, as it preserves the information of the observed values for inference (Little et al., 2022). Second, MI is a common practice when conducting analyses with data from LSAs. The imputations use information on the municipality type, the pupil's SEN status, the region, ISEI, ISCED, the number of books at home, the sex of the pupil, whether the household classifies as single parent household, the area per capita of the household, student performance, and the information on parental earnings. All these models do not include the composite measure of the SES, but the individual variables that the ÜGK uses to calculate the SES. This makes it possible to see which components are affected by the inclusion of parental earnings in the regression models or are affected by applying IPWs/MI.

Variables

Using the CCO data, annual earnings for the five years before the study were calculated for each parent. These were then aggregated to the mean earnings over those years per parent. Then, these were summed up for each pupil in the data. Averaging over multiple years acknowledges that earnings can be versatile (Willms & Tramonte, 2019), especially for young parents, and suits the idea of capturing the permanent financial situation of a family as promoted by economists (Marks & O'Connell, 2021). The operationalization "OECD-modified scale" (OECD, n.d.) was used to calculate household equivalent earnings and should reflect the income relative to the consumption of a household. It assigns a factor of 1 to the head of the household, 0.5 to every additional adult household member, and 0.3 for every child. Lastly, these mean household equivalent earnings were transformed into ranks using the cumulative distribution function as illustrated in figure 1. By doing so, the variable reflects the position in the distribution of earnings in the sample. Thus, regression estimates from this variable refer to a jump from the lowest to the highest earnings in the data. In other words, they resemble the effect of a jump from the last to the first place in the earnings distribution. In the text, this operationalization of earnings is referred to as "household equivalent earnings (CDF)". It must be mentioned that earnings from (self-)employment do not reflect all financial resources available to families which are discussed in more detail as a potential limitation of this study. The next variables concern the items that intend to measure the SES in the ÜGK. Namely, the highest ISEI of the parents, the highest education of the parents and the number of books at home. All variables were scaled. In this analysis, these scaled variables are used in the models, whereas the ÜGK originally utilised the composite measure of the SES, by dividing the sum of the scaled variables by 3 (Pham et al., 2016, 2017). Information on the occupational status was retrieved by an open format question, that was coded to ISEI codes. Parental education was elicited using scales applied in the former waves of the ÜGK ranging from category 1: "first three years of the lower-secondary education" to 8: "university or ETH completed with doctorate". The variable on the number of books at home intended to capture the cultural dimension of SES, and ranges from 0, representing few, to 5, representing many books at home. This question was asked using pictures, which show illustrated bookshelves of different sizes.

Figure 1: Schematic of the Operationalisation of Parental Earnings



Further, information from the sample frame on the language region, referring to German, French, and Italian, as well as the municipality type (urban/rural), and the special educational needs (SEN) status (yes/no) of the child were used. From the registry data, information is obtained on the sex of the child (male/female), the migration status (native/migration background), and whether the household can be classified as a single-parent household or not. The last variables concern the test scores in reading, listening, and mathematics. The test scores are represented by weighted likelihood estimates (WLE), which were scaled by one-parameter partial credit models recognizing the region as a grouping structure. For ease of interpretation, the WLEs were standardized, so they have a mean of zero and a standard deviation of one across the subjects. Additionally, a global test score was calculated, which shows the highest reliability. Because of the reliability, the main results display the effects on this global WLE score.

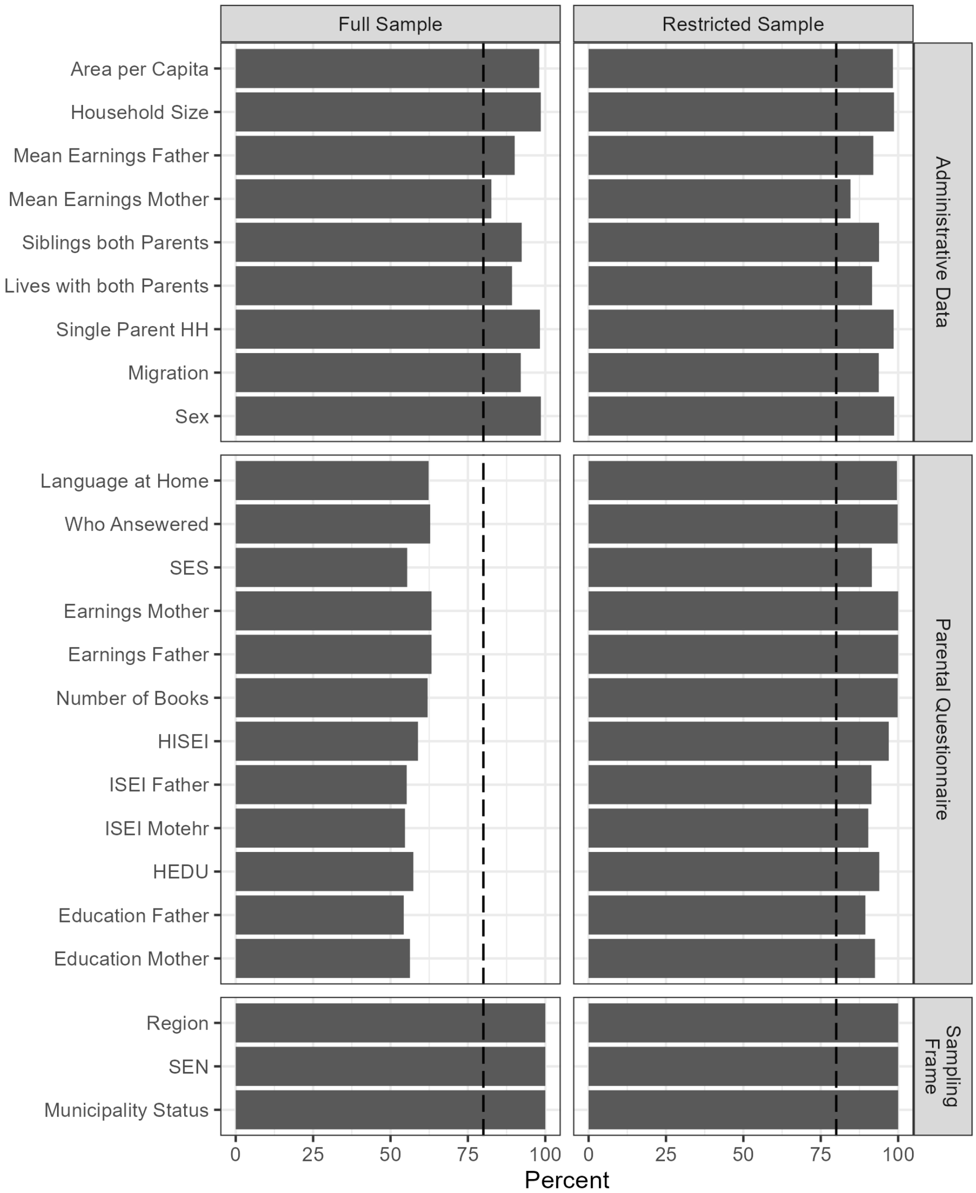
Results

Coverage and Participation in the Questionnaire for the Parents

Descriptive results display the coverage of the data regarding the different variables used throughout this study. Figure 2 reveals that the coverage for the variables from the registry data is over 80% concerning the full sample (N = 4333; upper left panel). Compared to this, the variables from the questionnaire for the parents have coverage between 63% and 54% (middle left panel). All variables from the sampling frame have full coverage (bottom left panel). As a large part of the parents did not participate or complete the questionnaire for the parents, the sample is restricted to observations for which the completion was over 80% (N = 2540; right panels). This restriction naturally increases the coverage of the variables from the questionnaire for the parents to surpass the 80% margin. However, the percentage of missing information in the administrative data hardly changes between the two samples, indicating that missing information is not conditional on participation in the parental survey. Logistic regressions were conducted to answer whether the participation status in the questionnaire for the parents is selective. The first model uses the information from the sample frame to explain the participation status, which was dichotomized into 1 = completed over or equal to 80% and 0 = completed below 80% of the questionnaire for the parents. Model 2 uses the same predictors while relying on the sample from models 3 and 4, which use household equivalent earnings (CDF) as well as the migration status and the sex of the child as additional predictors, thus restricting the sample.

The comparison of model 1 and model 2 from table 1 reveals that using only the auxiliary variables and student performance explains variance in the participation status in the questionnaire for the parents slightly better in the full sample. The direction of the effect and the significance levels are very similar between the models. Models 2 and 3

Figure 2: Coverage of Variables



show larger differences regarding the coefficients from the sample frame variables. Model 4 then reveals that having an SEN status or having a child with a migration background is associated with a lower probability of participation over 80%. On the contrary, coming from a family with high household equivalent earnings or a child with higher student performance correlates with an increased likelihood of participation. These significant coefficients stress the question of whether the realized observations in the questionnaire for the parents are likely to be a selective subpopulation of parents.

Table 1: Logistic Regressions for Participation Status

	Model 1 Full Sample	Model 2 Restricted Sample	Model 3 Parental Earnings	Model 4 Additional Controls
Intercept	0.633 ***	0.634 ***	0.460 ***	0.511 ***
	(0.014)	(0.015)	(0.022)	(0.024)
Household Equivalent Earnings (CDF)			0.322 ***	0.282 ***
			(0.028)	(0.028)
Region - German				
French	-0.054 **	-0.041 *	-0.042 *	-0.024
	(0.017)	(0.019)	(0.018)	(0.018)
Italian	0.025	0.030	0.051 **	0.052 **
	(0.018)	(0.019)	(0.019)	(0.019)
Special Educational Needs - Ref. No SEN				
SEN	-0.123 ***	-0.112 ***	-0.087 ***	-0.067 **
	(0.023)	(0.026)	(0.025)	(0.025)
Municipality Type - Ref. Urban				
Rural	0.016	0.019	0.019	-0.002
	(0.015)	(0.016)	(0.016)	(0.016)
Student Performance (WLE)				
	0.104 ***	0.097 ***	0.073 ***	0.067 ***
	(0.008)	(0.008)	(0.009)	(0.009)
Migration Status - Ref. Native				
Migration Background				-0.129 ***
				(0.018)
Sex - Ref. Female				
Male				0.010
				(0.015)
Num.Obs.	4‘333	3‘835	3‘835	3‘835
Pseudo-R2	0.060	0.049	0.082	0.095

Notes: * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$. Conditional Log Odds, SE robust, Dependent Variable ‘Participation in Parental Questionnaire’ 0 = “completion <80%“; 1 = “completion >= 80%“. Data CCO (2022a, 2022b, 2022c, 2022d, 2022e, 2022f), EDK (2024), FSO (2022), and Herzing et al. (2024): , own calculations.

Explaining Student Performance with Parental Earnings

Further, this study is interested in whether parental earnings can explain variance in student performance after controlling for student characteristics. Thus, two regressions are fitted to the data controlling for the socioeconomic status and student characteristics, while the second model additionally controls for household equivalent earning (CDF). To see which variables are affected by the introduction of parental earnings, the individual components of the SES measure used in the ÜGK were used for the analysis.

In a second step, both models were rerun using IPWs from two logistic regressions (see table 3 in the Appendix) that predict the inclusion of an observation in the complete case analysis using only auxiliary variables (model 1a and 2a). As known from the models explaining participation status, it might be more appropriate to use the information from the administrative records to calculate the IPW. Models 1b and 2b thus use IPWs that consider these variables in predicting being part of the complete case sample.

Table 2: Regressions on Student Performance

Weights	Model 1 -	Model 1a Naive IPW	Model 1b Admin IPW	Model 2 -	Model 2a Naive IPW	Model 2b Admin IPW
Intercept	0.110 **	0.005	0.056	0.038	-0.046	-0.037
	(0.040)	(0.046)	(0.045)	(0.058)	(0.066)	(0.061)
HISEI (scaled)	0.114 ***	0.127 ***	0.127 ***	0.104 ***	0.119 ***	0.114 ***
	(0.025)	(0.027)	(0.027)	(0.026)	(0.028)	(0.028)
HEDU (scaled)	0.109 ***	0.099 ***	0.108 ***	0.101 ***	0.093 ***	0.098 ***
	(0.024)	(0.026)	(0.026)	(0.024)	(0.026)	(0.026)
Books at Home (scaled)	0.129 ***	0.139 ***	0.146 ***	0.129 ***	0.139 ***	0.145 ***
	(0.021)	(0.024)	(0.024)	(0.021)	(0.024)	(0.024)
Household Equivalent Earnings (CDF)				0.125	0.089	0.169 *
				(0.073)	(0.078)	(0.075)
Migration Status – Ref. = Native						
Migration Status	-0.058	-0.021	-0.114 *	-0.054	-0.017	-0.104 *
	(0.045)	(0.050)	(0.049)	(0.045)	(0.050)	(0.048)
Sex – Ref. = Female						
Male	-0.039	-0.050	-0.057	-0.038	-0.049	-0.057
	(0.035)	(0.038)	(0.038)	(0.035)	(0.038)	(0.038)
Region – Ref. = German Part						
French Part	-0.050	-0.102 *	-0.052	-0.055	-0.106 *	-0.058
	(0.044)	(0.047)	(0.048)	(0.044)	(0.047)	(0.048)
Italian Part	-0.232 ***	-0.243 ***	-0.218 ***	-0.223 ***	-0.237 ***	-0.205 ***
	(0.042)	(0.047)	(0.045)	(0.042)	(0.048)	(0.046)
Household Status – Ref. = Other						
Single Parent	0.019	-0.013	0.043	0.004	-0.024	0.024
	(0.072)	(0.096)	(0.078)	(0.073)	(0.096)	(0.078)
Special Educational Needs – Ref. No SEN						
SEN	-0.552 ***	-0.737 ***	-0.588 ***	-0.550 ***	-0.737 ***	-0.583 ***
	(0.075)	(0.082)	(0.089)	(0.075)	(0.082)	(0.089)
Municipality Type – Ref. Urban						
Rural	-0.034	-0.017	-0.060	-0.031	-0.015	-0.058
	(0.036)	(0.040)	(0.041)	(0.036)	(0.040)	(0.041)
N	2212	2212	2212	2212	2212	2212
R	0.149	0.215	0.204	0.150	0.216	0.206

Notes: * p <0.05, ** p <0.01; *** p <0.001. Dependent variable: Student Performance. Models 1 and 2 show estimates with robust standard errors in parentheses from linear regressions using complete cases. Models 1a and 2a show the same models but use IPWs which were calculated using a logistic regression predicting the inclusion of an observation in the complete cases analysis using only auxiliary variables (the municipality type, the pupils' SEN status, student performance, and the region). Models 1b and 2b use IPW from a logistic regression that further includes variables from the administrative data (the sex of the pupil, the migration status of the child, and household equivalent earnings (CDF)). Data: CCO (2022a, 2022b, 2022c, 2022d, 2022e, 2022f), EDK (2024), FSO (2022), and Herzing et al. (2024); own calculations.

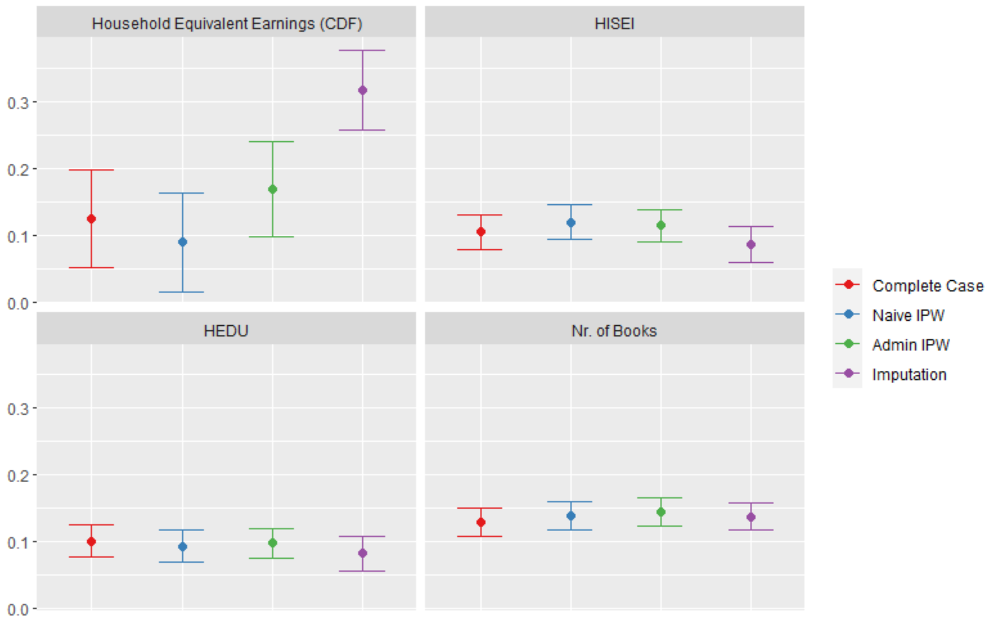
Table 2 shows the results of the different regressions on student performance. The differences between model 1 and model 2 should only be attributed to the inclusion of the household equivalent earnings (CDF). In this complete case sample, the results reveal only minor differences in the point estimates of the variables that occur in both models.

Furthermore, the inclusion of the earnings variable has only a marginal effect on the proportion of variance explained and the estimate of the variable for parental earnings is insignificant. When IPWs from the auxiliary variables are introduced to the models, the comparison reveals differences between models 1 and 1a, especially for the point estimate of the child's SEN status. Likewise, there are discrepancies between models 2 and 2a, however, the point estimate for the parental earnings declines steeply from model 2 to model 2a. Turning to the models that introduce IPWs using administrative records, there are similar differences between models 1 and 1b and models 2 and 2b like when using the naive IPWs. Most notable, however, is that the point estimate for parental earnings becomes significant as it is controlled for in the calculation of the IPWs. This is also true for the variable on the migration status in both models 1b and 2b. The comparison of the complete case models reveals that point estimates are sensitive to the exclusions of certain observations due to selective participation in the questionnaire for the parents. Surprisingly, the variable for migration status only becomes significant after using IPWs that use more information than the auxiliary variables to predict the inclusion in the complete case analysis. Such changes are crucial to the interpretation of the results from the LSAs because it is used for national reports on the state of the educational system. Furthermore, the comparison of the models also reveals that the effect of parental earnings only becomes significant when parental earnings are used to calculate the IPWs. The effect, however, is less significant and comparable to the other SES variables, e.g., the number of books at home.

Results from Multiple Imputation

Furthermore, the same models were fitted to imputed data. This is done because models that use IPWs are inefficient in this sense, that the information in incomplete cases is only used in the weighting but not for the calculation of estimates. MI techniques, however, make use of all the observed values (Little et al., 2022). Additionally, in most applied research with LSAs, the data is imputed to use all observations for the analysis. The imputation that was applied in this study created 100 data sets with 10 iterations each using predictive mean matching as the imputation method (Morris et al., 2014). The calculations were performed with the R package "mice" (Buuren & Groothuis-Oudshoorn, 2011), while no specific model was defined for the imputation. The variables included in the imputation model were the municipality type, the pupils' SEN status, the region, information from the questionnaire for the parents on parental education and occupational status as well as the number of books at home, the sex of the pupil, whether the household classifies as single parent household, student performance as WLEs, and the household equivalent earnings. The models that were applied to the imputed data mimic models 1 and 2 from table 2. The estimates from the multiple imputed data show differences to the complete case analysis as well as the models that use IPWs in table 2. The comparison of the estimates of parental earnings the complete analysis ($b = 0.0125$, $p > 0.05$), the model with the more complex IPWs (model 2b from table 2, $b = 0.169$, $p < 0.05$), and model 2 from the imputed data ($b = 0.317$, $p < 0.001$) shows, that the estimate becomes considerably large in effect size and more significant. Furthermore, the point estimates of the variables on parental education and occupational become smaller in the models with imputed data and have a lower level of significance using the imputed data (Parental education: $b = 0.082$, $p < 0.01$; parental HISEI: $b = 0.086$, $p < 0.01$, see table 4 in the appendix). The comparison of the models is depicted in figure 3, which shows the point estimates of parental earnings, parental education, parental occupational status and the number of books at home from models 2, 2a, and 2b from table 2 and model 2 from the imputed data (see table 4 in the Appendix).

Figure 3: Point Estimates of SES Variables from Different Regression Models



Like the models that predict participation in the questionnaire for the parents, the results from the logistic regression models predicting inclusion in the complete sample analysis (see table 3 in the Appendix) show that higher student performance and parental earnings are both associated with being part of the sample for the complete analysis in table 2. In other words, observations with on average lower student performance and parental earnings are potentially excluded from the complete case analysis. The MI models use these observations and the information they contain and show a strong connection between parental earnings and student performance. The effect size of jumping from the last to the first position in the distribution of earnings in the sample resembles almost 32% of a standard deviation in student performance, which is the second strongest effect after having special educational needs which is equivalent to about 56% of a standard deviation. Furthermore, it is almost two times the effect from the model that uses admin IPW, where the effect amounts to 17% of a standard deviation.

Discussion and Conclusion

This study aims to demonstrate the use of administrative data on parental earnings to overcome the issues of LSAs to obtain valid information on the financial dimension of the socioeconomic status of children and to use this information to explain student performance. Using pilot data from a national large-scale assessment of 8-year-olds in Switzerland, the ÜGK H4 (N = 4'333), this study has the advantage of using information on the social background characteristics reported by the parents rather than pupils' self-reports. First, the study investigates the coverage of the administrative data, as item non-response is a major issue regarding SES variables that rely on self-reports. Furthermore, logistic regressions are conducted to analyse whether participation in the parental survey is subject to selection bias. This would bias estimates from the complete case analysis. The study then focuses on the explanatory power of parental earnings for student performance. This is done once in a complete case analysis, once with naive IPWs that rely only on auxiliary variables from the sampling frame, once with IPWs that use additional administrative data on parental earnings and migration status, and lastly with data that was imputed using the administrative data. The comparison of the regression models should reveal to which extent the selectivity of the parental questionnaire affects the point estimates from the complete case analysis and show what happens if information on the incomplete observations is used in the case of the models that rely on the imputed data. An initial assessment shows, that the coverage of the administrative records regarding parental income is high ($> 80\%$). Furthermore, information from the questionnaire for the parents is likely to be selective, as indicated by the results from logistic regressions predicting participation in the parental questionnaire. The likelihood of participation declines with lower parental earnings, lower student performance, or when the child has a migration background. This implies that research which does not account for this selectivity, especially that student performance is tied to missing information, will come to biased results. This provides evidence that administrative data on parental earnings holds central benefits for contextualizing student performance in Large Scale Assessments. The general coverage of the administrative data is high and has fewer issues regarding its validity compared to pupils' self-reports. Furthermore, the promises of a parental survey to mitigate the problems associated with pupils' self-reports on social background characteristics, e.g., claims about the validity and differential measurement errors (Engzell & Jonsson, 2015; Ensminger et al., 2000; Kreuter et al., 2010), might not be sufficient in the context of the indication that non-response and missing information in the questionnaire for the parents is likely to be selective. Especially, as parental earnings partially explain the participation status. While administrative records hold benefits, there are also downsides to it. Regarding the financial situation of a family, the CCO data is insufficient to address all aspects of income which contribute to the permanent family income (Frick & Krell, 2010). The CCO only captures the realized earnings, with the advantage of using retrospective data, although other sources contribute to a family's financial resources such as savings or inheritance. Second, there are discounts, for example on health insurance or the possibility of paying comparatively less rent due to owning property or living in a housing cooperative, which significantly reduces monthly expenditures and thus defines the available financial resources of a family. Using administrative data on parental earnings to measure the financial situation of a family, however, circumvents the problems of household possession scales and might even improve international comparability. The administrative data holds the potential to incorporate information from the past and to operationalise earnings in different ways. For example, to define empirical and theoretical poverty thresholds. With these benefits in mind, the measure of parental earnings could further benefit from additional data on different sources of family income (e.g. see the work by Pfeffer, 2018). This study used information on the parents of the observed children that were recorded in the administrative data. However, it is possible to think that it would be more appropriate to use information on all adults, living in the same dwelling as the child, e.g. to also represent patchwork families. For this analysis, however, the administrative data indicates that about 90% of the children (for which we observe at least one parent) still live with both. Nonetheless, the administrative data holds the potential to construct the social context of a family in different ways which opens the possibility to investigate diverse research questions. Turning to the effect of parental earnings on student performance, the results reveal that the information on parental earnings does not contribute to the explanation of student performance in the complete case analysis. Using IPWs that should correct for the selectivity of the questionnaire for the parents does not change these results unless administrative information on earnings is included in the logistic model to calculate the IPWs. The results from analyses that use imputed data indicate even a stronger effect of parental earnings on student performance and that the point estimates of parental education and occupational status shrink, while the estimated effect for the number of books at home remains relatively stable. The differences in the point estimates of the parental earnings variable can be attributed to the selectivity of the questionnaire for the parents which results in a subpopulation in the complete case analysis that has, on average, higher student performance and higher parental earnings. When including these observations in the MI models, the correlation between low parental earnings and student performance, which was discarded before, becomes visible. However, it might be that the observations that fall out of the complete case analysis not only have lower parental earnings but also lower levels of parental education and occupational status as these three factors are intertwined (Willms & Tramonte, 2019). Hence, the strong correlation between student performance and earnings might be overestimated. The fact that the estimate of the number of books at home hardly changes between the models, although other variables regarding the SES show considerable differences, highlights that the subdimensions of the social background characteristics are distinct from each other as proposed by Ensminger and Fothergill (2003).

Even under the circumstance that the MI models produce overestimations of the true effect of parental earnings on student performance, the results indicate that the financial dimension has an independent effect on student performance. In conclusion, administrative data on the financial situation of a family is an important addition to the information that is commonly obtained in LSAs. It helps to recognize the financial situation of a person without the issues that come with self-reports or household possession scales. Regarding previous research, it is not surprising that a family's financial situation has, at least in the MI and weighted models, an effect on student performance. Hence, LSAs – or scholars working with such data – should be encouraged to use administrative data on the financial situation of a family to explain student performance where possible. Future research projects analysing student performance with LSA data could even include other sources of income and wealth or focus on the international comparability of measures of the financial situation of a family when using administrative data.

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Appendix

Table 3: Logistic Regressions for Calculating IPWs

	Model 1	Model 2
Intercept	0.564 ***	0.497 ***
	(0.015)	(0.024)
Region – Ref. = German Part		
French Part	-0.080 ***	-0.023
	(0.018)	(0.018)
Italian Part	0.006	0.052 **
	(0.019)	(0.019)
Special Educational Needs – Ref. No SEN		
SEN	-0.168 ***	-0.080 **
	(0.022)	(0.024)
Municipality Type – Ref. Urban		
Rural	0.049 **	-0.011
	(0.015)	(0.016)
Globale WLE	0.100 ***	0.067 ***
	(0.008)	(0.009)
Migration Status – Ref. = Native		
Migration Status		-0.197 ***
		(0.018)
Sex – Ref. = Female		
Male		0.015
		(0.015)
Household Equivalent Earnings (CDF)		0.303 ***
		(0.028)
N	4333	3835
Pseudo R2	0.068	0.130

Notes: * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$. Conditional Log Odds, SE robust in parentheses, Dependent Variable 'Observation in Complete Case Analysis' 0 = "Not in Analysis"; 1 = "In Analysis". Data : CCO (2022a, 2022b, 2022c, 2022d, 2022e, 2022f), EDK (2024), FSO (2022), and Herzing et al. (2024) , own calculations.

Table 4: Replication of the Main Model with Data from Multiple Imputations

	Model 1	Model 2
Intercept	-0.01	-0.181 ***
	(0.032)	(0.045)
HISEI (scaled)	0.109 ***	0.086 **
	(0.027)	(0.028)
HEDU (scaled)	0.101 ***	0.082 **
	(0.026)	(0.026)
Books at Home (scaled)	0.14 ***	0.137 ***
	(0.020)	(0.020)
Household Equivalent Earnings (CDF)		0.317 ***
		(0.059)
Migration Status – Ref. = Native		
Migration Status	-0.148 ***	-0.122 ***
	(0.034)	(0.034)
Sex – Ref. = Female		
Male	-0.029	-0.028
	(0.027)	(0.026)
Region – Ref. = German Part		
French Part	-0.066 *	-0.072 *
	(0.032)	(0.032)
Italian Part	-0.191 ***	-0.171 ***
	(0.032)	(0.032)
Household Status – Ref. = Other		
Single Parent	-0.045	-0.063
	(0.047)	(0.047)
Special Educational Needs – Ref. No SEN		
SEN	-0.573 ***	-0.558 ***
	(0.043)	(0.043)
Municipality Type – Ref. Urban		
Rural	-0.014	-0.012
	(0.027)	(0.027)
N	4333	4333
R2	0.20	0.207

Notes: * $p < 0.05$, ** $p < 0.01$; *** $p < 0.001$. Dependent variable: Student Performance. Models 1 and 2 show OLS estimates with robust standard errors in parentheses from linear regressions using 100 imputed data sets which were combined using Rubin's rule. The imputation models rely on the "PMM" method of the R package mice and use the following variables: Municipality type, SEN status, parental education and occupational status, the number of books at home, student performance, sex, household type, the migration status and the household equivalent earnings (CDF). Data: CCO (2022a, 2022b, 2022c, 2022d, 2022e, 2022f), EDK (2024), FSO (2022), and Herzing et al. (2024); own calculations.

Paper Three - Diverging Educational Aspirations Among Compulsory School-Leavers in Switzerland

Abstract: Educational aspirations play an important role in shaping students' educational trajectories and destinations. Drawing on longitudinal data from the TREE2 study, this paper investigates the effect of tracking on the formation and adjustment of the educational aspirations of Swiss students upon leaving compulsory school. We show that educational aspirations are highly responsive to the educational track attended in upper secondary education. While students in general education tend to stick to their aspirations, their counterparts in vocational programmes exhibit less stable aspirations.[a][b]

Collaboration: This work was developed in collaboration with Robin Benz.

Keywords: Educational Aspirations; Tracking, Upper Secondary Education, Panel Data, Switzerland

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[b] OSF repository available at: https://osf.io/gnfkc/

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1 Introduction

Educational pathways are marked by a series of choices that shape students' development and educational destinations. Educational aspirations play an important mediating role in these processes. The educational goals adolescents consider of value are believed to direct and motivate the effort they apply during their educational careers, thereby increasing their chances of succeeding in the education system (e. g., Bandura 2006; Caprara et al. 2008; Domina et al. 2011). Accordingly, many researchers have demonstrated that educational aspirations predict students' educational attainment (e. g., Morgan 2005; Beal and Crockett 2010; Bozick et al. 2010; Guo et al. 2015; Schoon and Burger 2021). Investigating the dynamics that give rise to educational aspirations thus provides an essential basis for understanding educational mobility.

There is an ongoing debate regarding the factors that contribute to the formation of educational aspirations. Established theoretical frameworks such as the Wisconsin model of status attainment (WM) (Sewell et al. 1969; 1970) or rational choice theory (RCT) (Erikson and Jonsson 1996; Breen and Goldthorpe 1997; Esser 1999) suggest that a variety of contextual conditions shapes educational aspirations. The school context is of particular significance as it provides a frame of reference for students when forming and revising their educational aspirations, especially in tracked and highly stratified education systems (Buchmann and Dalton 2002; Buchmann and Park 2009; Parker et al. 2016). On the one hand, sorting students according to their academic achievement creates distinct learning environments, in which some educational destinations are perceived as more favourable than others (Buchmann and Dalton 2002; Roth 2017; Van den Broeck et al. 2018). On the other hand, track placement conveys a strong signal about academic abilities and prospects, which students may consider when setting their educational goals (Buchmann and Park 2009; Karlson 2015; Geven and Forster 2021).

So far, few studies (e. g., Hegna 2014; Karlson 2015; Bittmann and Schindler 2021) have investigated how tracking relates to educational aspirations. The present study contributes to this strand of literature by examining the temporal dynamics of educational aspirations among students that have completed compulsory school in Switzerland. We aim to show how track allocation is related to a potential revision of educational aspirations, considering the entire spectrum of educational pathways. Using longitudinal data from the second cohort of the Transitions from Education to Employment study (TREE2) and examining both the level of educational goals and the way compulsory school-leavers adjust their educational goals, this study underlines the importance of tracking for educational aspirations. Our results show that the educational pathways adolescents pursue after compulsory school not only determine the educational destinations to which they aspire, but also give rise to a process of divergence with respect to educational goals.

The remainder of this article is structured as follows: The next section establishes a theoretical framework and outlines the state of research on the formation and adjustment of educational aspirations. The third section describes the data and analytical strategy that were pursued. After presenting the results in the fourth section, concluding remarks discuss our findings critically.

2 Theoretical Background

2.1 Educational Aspirations

Educational aspirations have been studied thoroughly over recent decades, across various disciplines. Despite being frequently considered in research, there is no universally accepted definition of educational aspirations (Morgan 2005; Trebbels 2015). We rely on the conceptualisation proposed by Haller (1968). Building on classical aspiration theory (Lewin et al. 1944), Haller (1968, 484) defines the term aspiration as a "cognitive orientational aspect of goal-directed behavior". Hence, aspirations reflect goals individuals set for themselves, given various alternatives. In the case of educational aspirations, the spectrum of alternatives typically follows a hierarchical order, with academically demanding educational degrees (e. g., more time-consuming, requiring specific certificates or performance) on one end of the spectrum, and less demanding on the other (Lewin et al. 1944; Haller 1968).

Haller (1968) further distinguishes between realistic and idealistic aspirations. This distinction acknowledges that the goals individuals wish to achieve may not necessarily coincide with the goals individuals perceive as achievable. Idealistic aspirations thus reflect wishes regarding desired outcomes that are "not limited by constraints on resources" (Hauser and Anderson 1991, 270) and are usually understood as an individual's commitment to achieving a desired goal regardless of the chances of realising this goal (Rojewski 2005; Trebbels 2015). Conversely, realistic aspirations relate to desired outcomes when taking the likelihood of actually achieving this outcome into account, considering constraints and resources (Haller 1968; Stocké 2013; Trebbels 2015). Empirical evidence suggests that students and their parents generally hold higher idealistic than realistic educational aspirations, while both are highly correlated (e. g., Becker and Gresch 2016; Gölz and Wohlkinger 2019; Hadjar and Scharf 2019; Becker et al. 2022). This paper focuses on realistic aspirations as we acknowledge that this type of aspiration is more sensitive to altered circumstances in the social context, transcends mere wishes, and is a more precise reflection of the goals towards which students direct their effort.

In summary, aspirations motivate and channel effort towards desired goals. Educational aspirations are expressed preferences on a spectrum of educational destinations that are typically arranged in order of difficulty. As it has been repeatedly shown that educational aspirations are predictive of future educational attainment (e. g., Beal and Crockett 2010; Bozick et al. 2010; Schoon and Burger 2021), investigating how students adapt their aspirations upon leaving compulsory school is pertinent.

2.2 Theoretical Explanations for Educational Aspirations

RCT and the WM frequently serve as points of departure in the literature when it comes to explaining the formation of educational aspirations. From the perspective of RCT, students are expected to be forward-looking and informed actors who try to maximise individual utility. Accordingly, considering benefits, costs and the probability of success, students are thought to aspire to the educational degree that carries the highest subjective expected utility (Erikson and Jonsson 1996; Breen and Goldthorpe 1997; Esser 1999).

There is ample evidence that students align their educational aspirations in the light of information on their likelihood of succeeding in education. Not only is there a strong correlation between achievement and aspirations (Khattab 2015; Karlson 2019; Bernardi and Valdés 2021). Research also suggests that students tend to stick to their aspirations when they are on track to attain the educational degree to which they aspire (Buchmann and Park 2009; Bittmann and Schindler 2021; Geven and Forster 2021). Furthermore, research provides evidence that students aspire to educational destinations they perceive to be most beneficial for later labour market prospects (Dumont et al. 2017; Salazar et al. 2020; Lievore and Triventi 2021). Recent studies that explicitly model the decisive factors of RCT buttress the assumption that educational aspirations reflect rational cost–benefit calculations (Gölz and Wohlkinger 2019; Jakob and Combet 2020; Zimmermann 2020; Lievore and Triventi 2021).

In contrast, the WM stresses the role of social influence (Sewell et al. 1969; 1970; Haller and Portes 1973). According to the WM, social origin and cognitive skills are linked to educational attainment via educational achievement and the influence of significant others. Significant others are "persons exerting the greatest influence" (Sewell et al. 1970, 1015), commonly specified as parents, friends, classmates and teachers. The mediating role of significant others is based on the idea that, in order to evade cognitive dissonances (Woelfel and Haller 1971), students conform to the pressure exerted by others when forming their educational aspirations. They do so either by imitating their role models' educational aspirations or by aligning their educational aspirations with the expectations of authority figures – their parents in particular (Sewell et al. 1970).

Social influence has proved to be a viable factor in explaining educational aspirations. In particular, the role of parents has been repeatedly stressed: it is suggested that students align their educational aspirations with their parents' expectations (e. g., Marjoribanks 2002; 2003; Augustine 2017; Roth 2017; Forster 2021; Schoon and Burger 2021). While the influence of the family provides a baseline for the initial formation of educational aspirations, it is assumed that peers become an increasingly important source of influence during adolescence (Osterman 2000; Brechwald and Prinstein 2011). The literature provides consistent evidence showing that students adopt their friends' and classmates' educational aspirations (Frost 2007; Roth 2017; Raabe and Wölfer 2019; Lorenz et al. 2020). However, doubts have been raised concerning the robustness of these findings amid potential confounding bias caused by selection effects. For instance, Kretschmer and Roth (2021) demonstrate that selection and peer influence contribute independently to similar aspirations within peer networks. Moreover, some studies show that student–teacher relations mediate the extent of peer influence when forming educational aspirations (Baker et al. 2014; Van den Broeck et al. 2020).

The underlying factors used to test the assumptions of RCT and the WM – most notably social origin and educational achievement – are likely to be linked. Morgan (1998) claims that the WM inherently incorporates processes of rationality, as regards the way that students "adopt the expectations that others have of them and add these to their own expectations formed independently through their own rational self-reflection" (Morgan 1998, 136). The implication that both rational calculus and social influence affect the formation of educational aspirations simultaneously has been given empirical support (Gabay-Egozi et al. 2015; Trebbels 2015; Gölz and Wohlkinger 2019; Zimmermann 2020).

Even though RCT and the WM have proved to be reliable for explaining educational aspirations, the two approaches are not free from criticism. On the one hand, RCT has been criticised for ignoring the role of unobserved early choices and, therefore, the possibility of procedural educational decision-making (Erikson et al. 2005). On the other hand, a major issue of the WM concerns its disregard for institutional constraints imposed by the education system (Kerckhoff 1977; Sewell et al. 2003). In light of this criticism, we agree that one has to consider the altering social and institutional circumstances along educational careers. We therefore argue that educational aspirations should be analysed from a longitudinal perspective, paying particular attention to processes that give rise to altered institutional and social circumstances – such as tracking – to highlight the malleability of aspirations during adolescence.

The literature puts forward other determinants that moderate or go beyond the assumptions of RCT and the WM. Some researchers relate the formation of educational aspirations to psycho-social factors such as self-esteem (e. g., Rothon et al. 2011), school and emotional engagement (e. g., Lazarides et al. 2016), and optimism (e. g., Salmela-Aro and Upadyaya 2017). Furthermore, some research suggests that students adjust their educational aspirations when experiencing economic setbacks (e. g., Taylor and Rampino 2014; Renzulli and Barr 2017; Salazar et al. 2020). While it has been repeatedly shown that female students set more ambitious educational goals than their male peers (e. g., Gil-Flores et al. 2011; Berrington et al. 2016), students with a migration background are found to have higher educational aspirations than native students with comparable academic achievement (e. g., Hadjar and Scharf 2019; Van den Broeck et al. 2020).

2.3 Changes in Educational Aspirations and the Role of Tracking

Considering institutional and social context is pivotal for explaining educational aspirations. So is the focus on educational transitions, as the corresponding changes in context have far-reaching implications – be it a change in the learning environment, the adapted cognitive requirements of differently oriented curricula or a related shift in labour market prospects. The significance of educational transitions is particularly amplified in education systems with early and rigorous tracking (Maaz et al. 2008; Bol and van de Werfhorst 2016; Van de Werfhorst 2019). Sorting students into different tracks creates distinct learning environments as regards students' abilities, interests and social backgrounds. Further, tracking imposes institutional constraints and limits the range of accessible alternatives, while at the same time opening up or consolidating others. Both RCT and the WM implicitly provide additional arguments for why tracking students should affect their aspirations.

From the perspective of RCT, it is assumed that students form their educational aspirations in accordance with what they perceive as maximising utility. When provided with new information, RCT expects that students will revise their educational aspirations (Morgan 1998; Zafar 2011). One of the most relevant pieces of information here is the continuous evaluation of academic abilities (Morgan 2005; Bozick et al. 2010; Khattab 2015). Information about academic abilities, however, transcends mere grades. As Karlson (2015) argues, placement in a specific educational track conveys a strong signal that affects students' beliefs independently of their actual academic abilities, because it involves a process of social labelling (Oakes 2005). Being in a specific track "makes publicly visible the opportunities of achieving success in the educational system" (Karlson 2015, 118). Social labels enter the process of rational calculus by altering students' perceptions of their probability of succeeding. Karlson holds that the behavioural implications of this labelling process depend on the degree of unambiguousness of the signals conveyed by track placement and whether the new information revealed by track placement conforms or conflicts with previous ability signals. Put differently, students are expected to respond more strongly to clear signals as compared to mixed ones, and to consistent signals as compared to inconsistent ones (de Boer et al. 2010; Karlson 2015).

The WM provides a different argument as to why students are likely to revise their educational aspirations upon proceeding to a new educational stage. Sorting students into tracks according to academic achievement creates distinct social contexts for students. Students find themselves in a new learning environment and are confronted with new significant others – be it peers or educators – who may exert social pressure towards specific educational goals (Oakes 2005; Van den Broeck et al. 2018; Kretschmer and Roth 2021). The degree of stratification and the social selectivity of track allocation defines how distinct these new learning environments are from each other. In particular, when tracking starts at an early age, the impact of primary and secondary effects of social origin (Boudon 1974) is found to be exacerbated, reducing the overall socio-economic and achievement-related heterogeneity at later educational stages (e. g., Maaz et al. 2008; Van de Werfhorst and Mijs 2010). In turn, the reduced heterogeneity accentuates the bias regarding the specific educational goals students are influenced to pursue (Buchmann and Dalton 2002; Parker et al. 2016; Van den Broeck et al. 2018). For example, students in the academically most demanding track are likely to be exposed to a learning environment that predominantly promotes pursuing the academically most demanding degrees.

Despite these theoretical arguments about the role of educational transitions in tracked education systems for the formation and adjustment of educational aspirations, this subject has received limited scientific attention. Buchmann and Dalton (2002) investigate the role of tracking for aspirations in differently stratified education systems. They note that a high level of stratification limits the degree to which significant others influence educational aspirations. It appears that in a more stratified education system, "there is little room for interpersonal effects" (Buchmann and Dalton 2002, 99), in such a way that track placement largely pre-empts the educational goals students set for themselves. This argument is in line with research from highly stratified education systems that reveals a systematic pattern of educational aspirations depending on the academic track that students attend. Students attending general academic tracks tend to have higher educational aspirations than those in non-academic tracks (Buchmann and Park 2009; Roth 2017; Van den Broeck et al. 2020; Zimmermann 2020; Bittmann and Schindler 2021; Geven and Forster 2021).

Recent studies report systematic track-related differences in the way students adjust their educational aspirations in light of transitions to the next educational stage. Karlson (2015) demonstrates for the US that students placed in high-ability tracks experience an upward shift in educational expectations, particularly when placement is consistent across different subjects. While those entering a high-ability track from a low-ability track show substantial increases in educational expectations, those moving downward are more likely to decrease their expectations. Similarly, Geven and Forster (2021) provide evidence for the German context suggesting that students are more likely to adjust their educational aspirations upwards if their track placement in lower secondary education exceeds their expectations – and vice versa. Another recent study from Germany indicates that upon entering lower secondary education, high-ability students in non-academic tracks experience a gradual decrease in their aspiration of acquiring a university entry certificate. In contrast, almost all of their counterparts in the academic track stick to their previous aspiration of obtaining a university entry certificate. This relationship is mediated by social origin, which contributes to a process of divergence (Bittmann and Schindler 2021). Evidence from Norway suggests that, compared to those in general education, students in vocational programmes are substantially more likely to redirect their educational aspirations away from tertiary education upon approaching the transition to upper secondary education. After entering upper secondary education, this relationship vanishes, suggesting that tracking plays a more substantial role during the decision-making process preceding the transition than during the transition itself (Hegna 2014). Contrary to earlier findings suggesting that students' aspirations are resilient over time (Grodsky and Riegle-Crumb 2010; Andrew and Hauser 2011), these studies underline that many students revise their educational aspirations during educational transitions.

2.4 The Present Study

This study contributes to the literature on educational aspirations by analysing track allocation as a major driver for the formation and adjustment of educational aspirations. Whether tracking defines opportunities and constraints, sends ability signals or alters the composition of significant others, we expect that transitioning from one educational stage to another incites students to revise their educational aspirations. Further, we expect that this is particularly apparent in highly stratified education systems such as Switzerland's (Buchmann and Dalton 2002; Buchmann and Park 2009; Parker et al. 2016).

In Switzerland, students are sorted into lower secondary school tracks according to their academic record, usually in seventh grade. Track placement at this stage is essential as it sets the course for future educational opportunities (Buchmann et al. 2016; SCCRE 2018; Combet 2019). Compulsory schooling in Switzerland ends with lower secondary education in ninth grade. In upper secondary education, students are primarily channelled into either high-ability general education (baccalaureate schools and upper secondary specialised schools) (about 29 %) or primarily firm-based vocational education and training (VET) with varying academic requirements (about 60 %) (SCCRE 2018; Gomensoro and Meyer 2021; FSO 2021). Students in specific VET programmes can obtain a vocational baccalaureate degree enabling them to enter universities of applied sciences. The strong segmentation of Swiss upper secondary education into several distinctly different tracks or programmes requires an empirical approach that reflects the variety of viable educational pathways after compulsory school. To this end, and unlike previous studies, we go beyond reducing educational aspirations to a dichotomy between tertiary and non-tertiary level educational goals.

While general education primarily prepares students for entry into tertiary education, VET prepares them for entry into the labour market. In contrast, VET programmes that allow obtaining a vocational baccalaureate facilitate tertiary education and labour market entry. Despite the politically claimed permeability of the

Swiss education system, scholars consistently demonstrate that track placement in upper secondary education is predictive of the highest educational attainment (e. g., Buchmann et al. 2016). Furthermore, studies reveal that track allocation at lower and upper secondary levels is characterised by substantial social selectivity (e. g., Becker and Glauser 2018).

Two issues will be investigated in our study: the general impact of tracking on aspirations; and whether track placement is related to distinct patterns of aspirational adjustments. We assume that we will find the highest educational aspirations among students in general education and the lowest among students in VET. While the academically most demanding general education track is geared towards entering tertiary education, students in the least academically demanding VET track are prepared for labour market entry. As institutional constraints limit students' ability to switch tracks, this narrows down the range of feasible educational destinations. At the same time, track placement sends out a strong ability signal. Students in the academically most demanding track are signalled that their academic abilities most likely exceed those of their counterparts in academically less demanding tracks, which encourages them to set high educational goals – and vice versa. In both cases, students entering new learning environments are influenced by significant others, which are now less heterogeneous due to the social sorting that accompanies tracking. This, in turn, contributes to the unambiguousness of the influence of significant others when students evaluate the educational alternatives to which they should aspire. We expect that this consolidates the tendency of students in the academically most demanding track to set high educational goals – and vice versa.

We further propose that track placement systematically affects how students adjust their educational aspirations upon leaving compulsory school. Again, given the institutional constraints limiting the range of feasible educational destinations, the ability signal conveyed through track placement and the distinct influence by significant others, some educational destinations become less or more feasible and desirable. Students in general education are unambiguously geared towards setting high educational goals. Consequently, we expect these students to predominantly adjust their educational aspirations upwards or to stick to their already high initial aspirations. Analogously, we expect students in VET to predominantly adjust their educational aspirations downwards or to stick to their already low initial aspirations. In contrast, for VET programmes that lead to a vocational baccalaureate, we expect the ability signal to be fuzziest and the influence exerted by significant others to be most diverse. Coupled with the variety of educational pathways students can follow upon completing these programmes, we expect to find the most substantial adjustments of educational aspirations – both upwards and downwards.

3 Data and Methods

3.1 Sample

This study draws on longitudinal data from TREE2 (TREE 2022). TREE2 surveys the educational and occupational pathways of compulsory school-leavers in Switzerland. This data comprises a sample of 8'429 students who participated in Switzerland's large-scale assessment study AES (Assessment of the Attainment of Educational Standards; in German: Überprüfung des Erreichens der Grundkompetenzen, ÜGK), in 2016 (Hupka-Brunner et al. 2021). The population covered by TREE2 includes all Swiss ninth-grade students in school year 2015/2016 who did not repeat their ninth grade in the subsequent school year. This article draws on data from the AES baseline survey and the first and third waves of TREE2 from 2017 and 2019, respectively.[1]

The sample is restricted to the 5'850 respondents who participated in all three surveys. Since the research design requires complete information on the dependent variable of realistic educational aspirations in at least the AES baseline and TREE2 third wave, the sample size is reduced to 3'501 respondents. Excluding respondents with missing information for the explanatory variables, the size of the analytical sample amounts to 3'294 individuals that completed compulsory school in 2016. Comparisons of the weighted analytical sample with the original sample weighted for participation in waves 1 and 3 do not indicate any systematic biases.[2] When we describe the variables below, we refer to the weighted descriptives of the baseline survey.

3.2 Measurements

The dependent variable of realistic aspirations is deduced from the question "What do you think will be the highest educational degree that you will attain one day?", with seven ordinal response categories ranging from a two-year VET certificate (EBA) to a tertiary degree from a university. Due to the insignificant number of observations relating to aspiring to obtain a two-year VET certificate, this category is merged with the second category of the three- to four-year VET certificate (EFZ). At the end of compulsory school, students aspire to either an upper secondary-level VET diploma (29.4 %), a vocational baccalaureate (13.9 %), a general baccalaureate (5.7 %), a tertiary-level VET diploma (11.4 %), a university of applied science or teacher education degree (16.5 %), or a university degree (23.1 %). As the wording of this question incorporates an anticipatory perception of the likelihood of suc-

1 The analyses presented in this study rely on provisional pre-published data of TREE2's third wave as of July 2022.

2 Descriptive statistics of the analytical sample across all survey waves are provided by the authors upon request.

cessfully attaining the desired educational degree, the dependent variable reflects realistic educational aspirations (Haller 1968; Hupka-Brunner et al. 2016).

A categorical variable contrasting the educational degree aspired to at the end of compulsory school and three years later, in 2019, is created to measure the adjustment of realistic educational aspirations. We define students as having stable aspirations (45.1 %) if the educational degree to which they aspire does not change over the observed period. Conversely, students adjust their aspirations downwards (16.0 %) or upwards (38.9 %), respectively, if their reported realistic aspiration in 2019 is lower or higher than at the end of compulsory school, in 2016.

The independent variable of interest captures students' educational track in upper secondary education. We categorise the multitude of educational programmes into the following four categories. The category general education (36.1 %) encompasses entirely school-based programmes that allow students to acquire a baccalaureate degree or a specialised school diploma. Students attending two-to four year vocational education and training (EBA and EFZ) are combined under the category VET (42.0 %). The category vocational baccalaureate comprises all programmes that allow students to acquire a vocational baccalaureate (4.5 %). Lastly, we group paid employment, internships, interim solutions, or pursuing a non-certified education within the category NET (not in education or training) (17.4 %). Since previous educational decisions primarily determine track allocation at the upper secondary level, we include a measure capturing the requirement level for the track attended during the last year of compulsory school. This variable distinguishes between high (35.3 %), advanced (39.6 %), and basic requirements (23.0 %), and a separate category for students in integrated schools, alternative programmes, or special education needs classes (2.1 %).

Given the various factors previous studies (e. g., Rothon et al. 2011; Berrington et al. 2016; Hadjar and Scharf 2019; Salazar et al. 2020) have identified as determinants of educational aspirations, and thus as potential confounders, we consider several control variables in the multivariate analyses. We control for educational achievement by calculating the grade point average for first and second school language, mathematics and science in the last year of compulsory school (mean = 0.06, SE = 0.02). A composite measure capturing the perceived parental pressure to achieve (Böhm-Kasper et al. 2000) acts as a control influence exerted by parents (mean = –0.01, SE = 0.02). Concerning socio-demographic characteristics, the regression models include dummy variables for sex (53.3 % females), migration background (25.8 %) and foreign language spoken at home (19.9 %). To capture multiple dimensions of social origin, we further control for highest parental educational attainment (43.2 % with tertiary education, 45.6 % with upper secondary education, and 11.2 % with compulsory schooling only), highest parental ISEI-08 score (Ganzeboom 2010) (mean = 0.13, SE = 0.02), and the number of books at home (Kunter et al. 2002) (mean = 4.41, SE = 0.03).

3.3 Analytical Approach

When investigating the effects of track placement on the educational aspirations of compulsory school-leavers in Switzerland, this study follows a two-step approach. In the first step, we aim to identify factors contributing to the formation of educational aspirations from a longitudinal perspective. To this end, we analyse the educational degree to which students aspire by estimating random-effects ordered logistic models for unbalanced samples. These models allow for individual intercepts, and thus consider that observations from the same individual are correlated. Provided that these random intercepts are uncorrelated with predictor variables in the model, this estimation procedure yields less biased estimates as it accounts for unobserved heterogeneity between individuals (e. g., Wooldridge 2020; Rabe-Hesketh and Skrondal 2022). To account for systematic temporal trends, these models include wave-specific dummy variables. While keeping the number of students in the analytical sample constant, we apply maximum-likelihood estimation and gradually extend the regression models by including additional covariates.

In the second step, we analyse whether track placement is systematically associated with the way students adjust their educational aspirations, namely sticking to the same degree aspired to at the end of compulsory school or adjusting the aspiration downwards or upwards, respectively. In order to estimate the likelihood of exhibiting one of these three patterns simultaneously, we estimate multinomial logistic regression models (e. g., Long and Freese 2014; Greene 2018). The results of the multinomial models are presented in terms of average marginal effects, which facilitates comparing estimates of nested models and reduces bias related to unobserved heterogeneity (Mood 2010).

4 Results

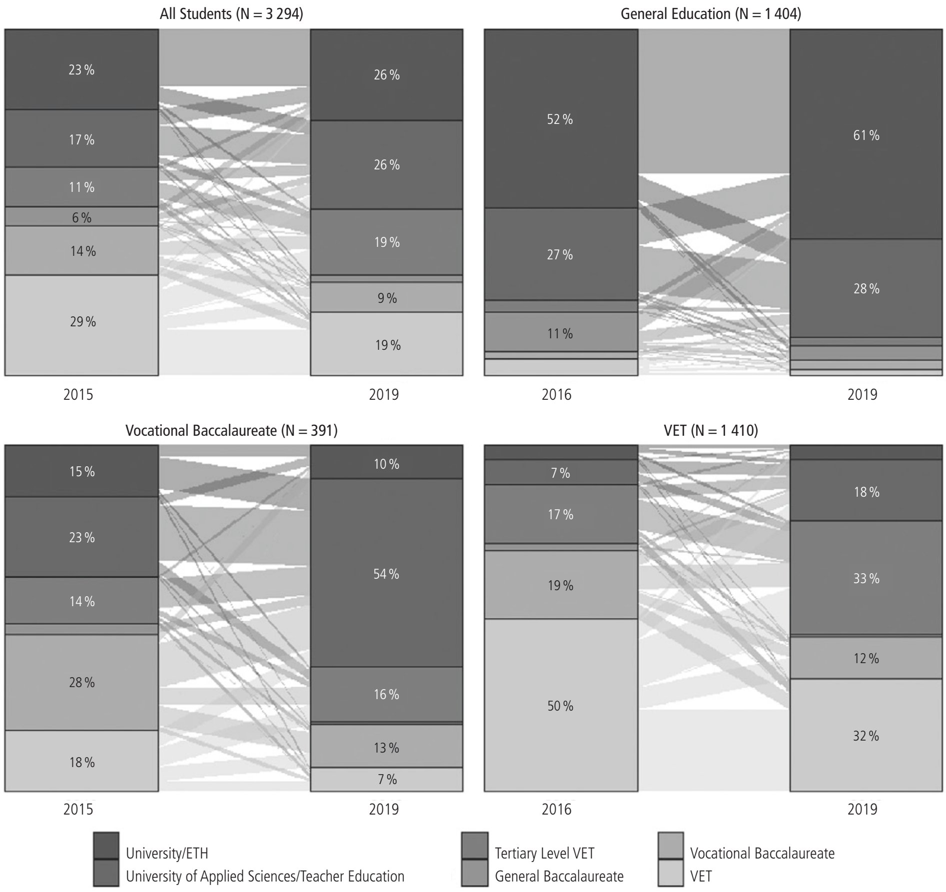
4.1 Educational Aspirations of Compulsory School-Leavers in Switzerland

The educational goals compulsory school-leavers in Switzerland set for themselves cover the entire range of obtainable degrees. Figure 1 depicts realistic educational aspirations over the observed period and illustrates the interrelations between them. Four aspects immediately stand out.

First, some educational degrees are aspired to more frequently than others. Three years into upper secondary education, in 2019, 19 % of the entire analytical sample does not aspire to a degree beyond VET. In contrast, more than half aspire to a degree at a university of applied sciences or teacher education.

Second, the illustrated changes in realistic educational aspirations over time disprove the claim that adolescents only rarely revise the educational goals they set at an earlier age (e. g., Grodsky and Riegle-Crumb 2010; Andrew and Hauser 2011). Over the considered period from 2016 to 2019, 55 % of compulsory school-leavers

Figure 1 Educational Aspirations Over Time



Note: Weighted percentage (N = 3294), students not in education (NET) in 2019 not shown, Data: TREE2, own calculations.

have adjusted their initial educational aspirations. Notably, 8 % of the respondents return to the degree to which they originally aspired but report other aspirations in between. However, the extent to which students adjust their educational aspirations differs across tracks. While six out of ten students in general education exhibit stable educational aspirations over time, only 39 % of VET students and 30 % of students in a programme leading to a vocational baccalaureate have stable aspirations.

Third, a positive trend becomes apparent when comparing the percentages of degrees aspired to from 2016 and 2019. Three years into upper secondary education, the adolescents considered in the analyses set overall higher educational goals than they do at the end of compulsory school. In total, more cases raise their educational aspirations (39 %) than decrease them (16 %). This pattern, again, varies across tracks. While one quarter of students in general education raise their educational aspiration, we observe a substantially higher percentage of upward adjustments among students in VET (45 %) and students in programmes leading to a vocational baccalaureate (50 %).

Lastly, Figure 1 clearly indicates that students aspire to different educational degrees depending on track placement in upper secondary education. A pattern emerges: students in general education predominantly aspire to a university degree, whereas 32 % of students in VET do not aspire to a degree beyond their current training. Less than 5 % of VET students aspire to a university degree, although the overall share of VET students aspiring to a degree at universities of applied sciences or teacher education increases from 2016 to 2019. In programmes leading to a vocational baccalaureate, individuals display a remarkable shift in aspirations towards obtaining a degree from a university of applied sciences or teacher education (54 %).

Overall, descriptive analyses of educational aspirations reveal that a substantial number of students considered in our analyses revise their educational aspirations upon leaving compulsory school. Not only are there indications of specific adjustment patterns over time, there is also compelling evidence that students systematically differ in terms of their educational aspirations depending on the educational track they attend. This assessment leads us to investigate further how the formation of educational aspirations is affected by tracking, and whether changes in educational aspirations depend on track placement at the upper secondary level.

4.2 Formation of Educational Aspirations

In the first step, we investigate the relation between of track placement and realistic educational aspirations by estimating random-effects ordered logistic regressions. Table 1 presents the results of these models in terms of odds ratios for aspiring to a higher educational degree, along with 95 % confidence intervals in parentheses.

Model 1 solely includes the variables of primary interest, Model 2 introduces controls for grades and perceived parental pressure, Model 3 controls for socio-demographic characteristics, and Model 4 contains the full set of predictors. The estimated effects of track placement prove reasonably robust across all four models.

In Model 4, regarding track placement in lower secondary education, we find that the conditional odds of aspiring to a higher educational degree are lower ($OR = 0.237$, $p < 0.001$) for students in the advanced track compared to their counterparts in the high requirement track. Students attending a basic requirement track show an even lower likelihood of setting higher educational goals ($OR = 0.090$, $p < 0.001$).

The negative effects of track placement are even more pronounced in upper secondary education. Adolescents in VET ($OR = 0.109$, $p < 0.001$), programmes leading to a vocational baccalaureate ($OR = 0.196$, $p < 0.001$) or those currently not in education or training ($OR = 0.125$, $p < 0.001$) show a significantly decreased likelihood of aspiring to a higher educational degree than their counterparts in general education. The effects of track placement are in line with the findings of Buchmann and Park (2009), who show that students' aspirations align with the orientation of the track they attend, and that students adapt their educational goals in accordance with the ability signals they receive (Karlson 2015).

Table 1 Random-Effects Ordered Logistic Regression Models on Educational Aspirations. Odds Ratios with 95 % Confidence Intervals

	Model 1 Realistic Aspirations	Model 2 Realistic Aspirations	Model 3 Realistic Aspirations	Model 4 Realistic Aspirations
Lower Secondary Track (Ref. High Requirements)				
Advanced Requirements	0.201***	0.185***	0.263***	0.237***
	(0.154, 0.261)	(0.144, 0.240)	(0.205, 0.338)	(0.186, 0.303)
Basic Requirements	0.064***	0.061***	0.101***	0.090***
	(0.045, 0.091)	(0.043, 0.087)	(0.071, 0.142)	(0.064, 0.127)
Other	0.237***	0.202***	0.258***	0.219***
	(0.112, 0.502)	(0.101, 0.405)	(0.136, 0.486)	(0.120, 0.399)
Upper Secondary Track (Ref. General Education)				
NET	0.082***	0.106***	0.101***	0.125***
	(0.059, 0.113)	(0.077, 0.147)	(0.074, 0.140)	(0.092, 0.172)
VET	0.069***	0.083***	0.096***	0.109***
	(0.053, 0.090)	(0.064, 0.108)	(0.074, 0.124)	(0.084, 0.142)
Vocational Baccalaureate	0.137***	0.156***	0.180***	0.196***
	(0.103, 0.184)	(0.117, 0.207)	(0.136, 0.239)	(0.148, 0.258)
Wave (Ref. 2016)				
2017	1.381***	1.383***	1.397***	1.398***
	(1.203, 1.586)	(1.205, 1.588)	(1.217, 1.604)	(1.218, 1.605)
2019	2.511***	2.572***	2.499***	2.555***
	(2.209, 2.854)	(2.263, 2.924)	(2.200, 2.839)	(2.248, 2.903)
Parental Pressure		1.161**		1.041
		(1.049, 1.286)		(0.940, 1.153)
Average Grade		1.889***		1.722***
		(1.710, 2.085)		(1.566, 1.895)
HISEI 08			1.409***	1.365***
			(1.271, 1.562)	(1.232, 1.511)
Parental Education (Ref. Tertiary Education)				
Compulsory Schooling Only			0.384***	0.409***
			(0.272, 0.543)	(0.291, 0.575)
Upper secondary education			0.474***	0.498***
			(0.390, 0.577)	(0.411, 0.604)
Number of Books at Home			1.218***	1.168***
			(1.136, 1.305)	(1.091, 1.251)
Language Spoken at Home (Ref. Test Language)				
Other			1.306	1.309
			(0.981, 1.738)	(0.990, 1.730)
Immigration Status (Ref. Native)				
Migration Background			2.253***	2.294***
			(1.712, 2.966)	(1.755, 3.000)
Sex (Ref. Male)				
Female			0.909	0.884
			(0.764, 1.082)	(0.743, 1.052)
BIC	149561.3	148002.5	147091.7	145863.0
N of students	3294	3294	3294	3294
Observations	8938	8938	8938	8938

Note: Weighted estimates of random-effects ordered logistic models. Conditional odds ratios (OR), 95 % confidence intervals in parentheses. Cut points and sigma squared have been omitted. Predictors HISEI 08 and Average Grade are z-standardied. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Data: TREE2 (2022), own calculations.

Over the observed three-year period, students set increasingly higher educational goals. Compared to the baseline survey of 2016, the conditional odds of a higher educational aspiration increase by a factor of 1.398 ($p < 0.001$) for the first survey wave of 2017 and more than double for the third survey wave of 2019 ($OR = 2.555$, $p < 0.001$). Our findings suggest that students generally opt for higher aspirations later in upper secondary education.

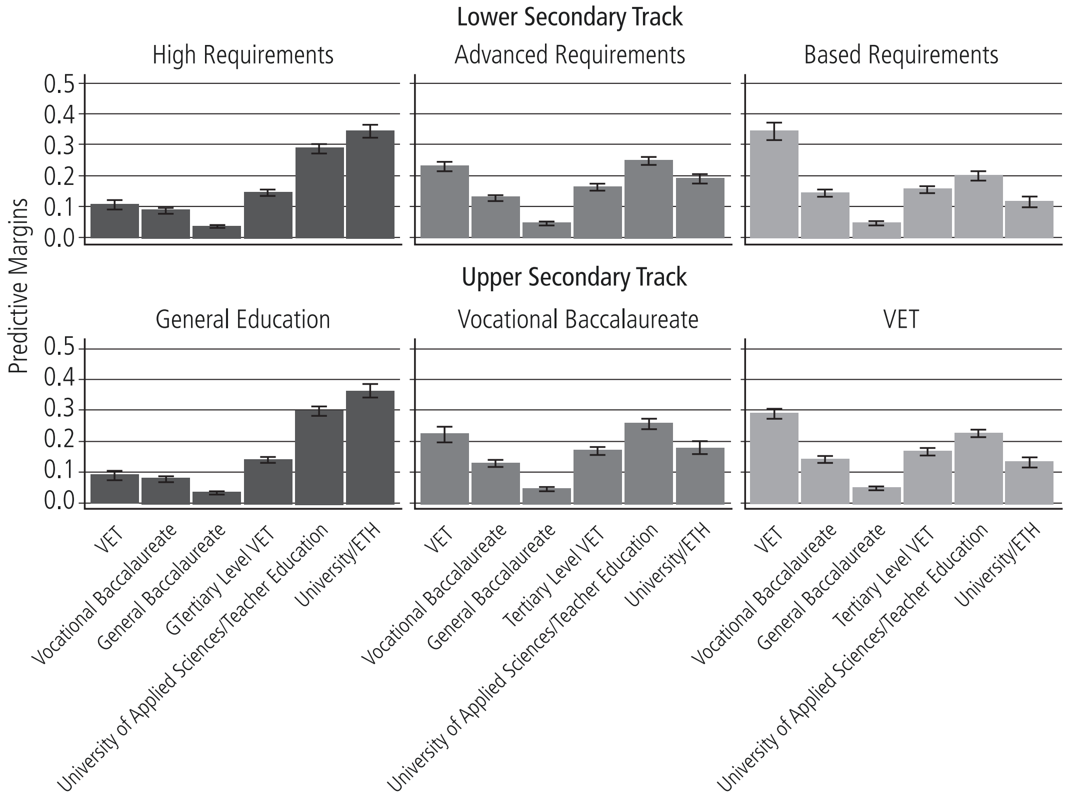
For the first set of controls, we find that perceived parental pressure to achieve is unrelated to educational aspirations ($OR = 1.041$, $p > 0.05$), underlining the notion that peers become a more important source of influence during adolescence, as compared to parents (Osterman 2000; Brechwald and Prinstein 2011). Further, we see that an increase by one standard deviation in grade point average increases the conditional odds of aspiring to a higher degree by a factor of 1.722 ($p < 0.001$). This supports findings on the effect of educational achievement on aspirations, as reported by several other studies (e. g., Khattab 2015; Roth 2017; Karlson 2019; Bernardi and Valdés 2021).

The second set of controls reveals that socio-demographic characteristics are strongly predictive of educational aspirations. An increase by one standard deviation of the highest parental ISEI is related to an increase in the conditional odds of aspiring to the next higher degree ($OR = 1.365$, $p < 0.001$). Adolescents whose parents have not attained tertiary education are predicted to set lower educational goals for themselves (compulsory schooling only: $OR = 0.409$, $p < 0.001$, upper secondary education: $OR = 0.498$, $p < 0.001$). In a similar vein, the number of books at home is significantly positively related to the educational degree aspired to ($OR = 1.168$, $p < 0.001$). These results confirm the crucial role of social origin in the formation of educational aspirations, as illustrated by previous research (e. g., Buchmann and Dalton 2002; Baker et al. 2014; Roth 2017; Gölz and Wohlkinger 2019).

Furthermore, and in line with previous research (e. g., Salikutluk 2016; Hadjar and Scharf 2019; Van den Broeck et al. 2020), we find a positive but statistically insignificant effect for speaking other languages at home ($OR = 1.309$, $p > 0.05$) and a positive significant effect for having a migration background ($OR = 2.294$, $p < 0.001$). Unlike findings from previous studies (e. g., Gil-Flores et al. 2011; Baker et al. 2014; Berrington et al. 2016), our model predicts lower educational aspirations for girls than for boys, although this effect is not statistically significant ($OR = 0.884$, $p > 0.05$).

Summarising the results from these models, we find substantial support for our hypothesis that track placement has a direct effect on the formation of aspirations. To illustrate this effect, Figure 2 depicts predictive margins from Model 4 for each educational goal considered, depending on track placement in lower and upper secondary education. In the upper panels, we see the predicted probabilities by lower secondary track. This reveals that students in high requirement tracks aspire to more demanding degrees than their counterparts in basic requirement tracks, who aim mainly for VET degrees. In advanced tracks, however, students are predicted to aspire

Figure 2 Predicted Educational Aspirations by Track Placement



Note: Predictive margins with 95 % convidence intervals calculated from Model 4 in Table 1. Data: TREE2, own calculations.

in almost equal parts to VET or tertiary education, while the largest share realistically aspires to a university of applied sciences or teacher education. Focusing on the lower panels showing predicted probabilities by track placement in upper secondary education, an almost identical picture emerges. Students in general education aspire to the highest degrees, while VET students are still most likely to aspire to a VET diploma. Students in programmes leading to a vocational baccalaureate are again the most diverse in their predicted aspirations, with the largest share aspiring to a university of applied sciences or teacher education, followed by VET and university.

4.3 Adjustments of Educational Aspirations Upon Leaving Compulsory School

After bringing forward evidence that track placement has an effect on which educational degrees students aspire to, we examine to what extent the transition to upper secondary education is related to how compulsory school-leavers adjust their educational aspirations. In doing so, students' educational aspirations at the end of compulsory school are contrasted with their aspirations three years into upper secondary education. Using multinomial logistic regression, we examine whether students' educational aspirations were stable, shifted downwards or upwards, re-

Table 2 Multinomial Logistic Regression Models on Adjustments of Educational Aspirations from 2016 to 2019. Average Marginal Effects with 95 % Confidence Intervals

	Stable	Downwards	Upwards
Lower Secondary Track (Ref. High Requirements)			
Advanced Requirements	–0.074**	–0.038	0.112***
	(–0.130, –0.018)	(–0.081, 0.006)	(0.057, 0.167)
Basic Requirements	–0.019	–0.048	0.067
	(–0.095, 0.057)	(–0.102, 0.006)	(–0.007, 0.141)
Other	–0.063	–0.041	0.104
	(–0.201, 0.076)	(–0.144, 0.062)	(–0.041, 0.249)
Upper Secondary Track (Ref. General Education)			
NET	–0.150*	0.211***	–0.061
	(–0.278, –0.021)	(0.094, 0.327)	(–0.174, 0.052)
VET	–0.136***	0.069**	0.067*
	(–0.198, –0.073)	(0.026, 0.112)	(0.005, 0.129)
Vocational Baccalaureate	–0.245***	0.084***	0.161***
	(–0.312, –0.179)	(0.034, 0.134)	(0.092, 0.231)
Parental Pressure	0.007	0.012	–0.019
	(–0.017, 0.031)	(–0.006, 0.030)	(–0.044, 0.005)
Average Grade	0.020	0.000	–0.021
	(–0.001, 0.042)	(–0.016, 0.016)	(–0.042, 0.001)
HISEI 08	0.000	–0.012	0.012
	(–0.025, 0.025)	(–0.030, 0.006)	(–0.013, 0.037)
Parental Education (Ref. Tertiary Education)			
Compulsory Schooling Only	–0.029	–0.061*	0.090*
	(–0.110, 0.051)	(–0.114, –0.008)	(0.011, 0.169)
Upper Secondary Education	–0.063**	–0.028	0.090***
	(–0.110, –0.016)	(–0.064, 0.008)	(0.044, 0.137)
Number of Books at Home	–0.003	0.007	–0.004
	(–0.019, 0.013)	(–0.003, –0.018)	(–0.020–0.012)
Language Spoken at Home (Ref. Test Language)			
Other Language	–0.047	–0.001	0.049
	(–0.109, 0.014)	(–0.046, 0.043)	(–0.015, 0.112)
Immigration Status (Ref. Native)			
Migration Background	0.004	0.010	–0.014
	(–0.054,–0.061)	(–0.034, 0.054)	(–0.073, 0.046)
Sex (Ref. Male)			
Female	0.007	0.016	–0.022
	(–0.034, 0.047)	(–0.014, 0.045)	(–0.063, 0.018)
N of students	3294		
BIC	41523.590		
Pseudo R^2 (McFadden)	0.035		

Note: Weighted estimates of multinomial logit regression. Average marginal effects (AME), 95 % confidence intervals in parentheses. Predictors HISEI 08 and Average Grade are z-standardised. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Data: TREE2 (2022), own calculations.

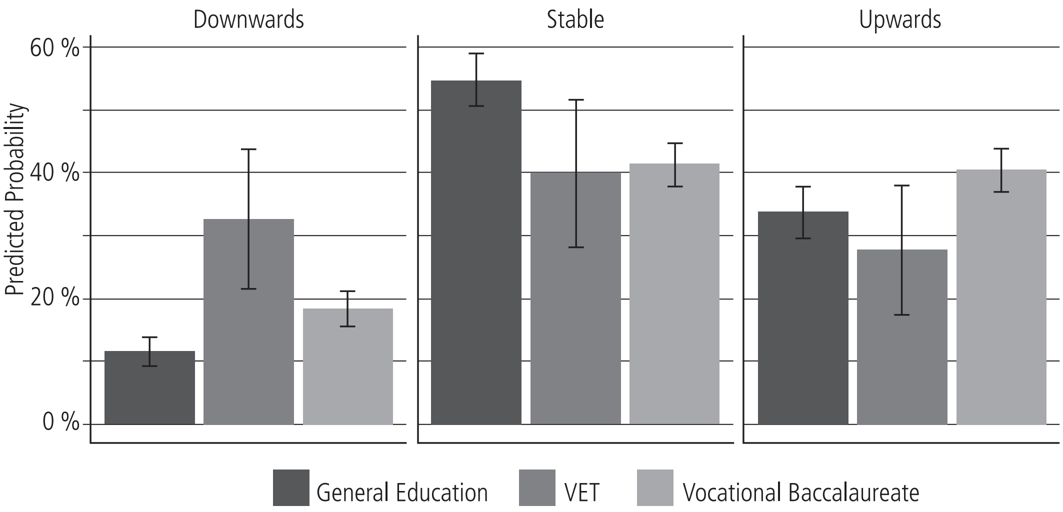
spectively, over this period. Table 2 presents the results in terms of average marginal effects and 95 % confidence intervals in parentheses.

Track placement in upper secondary education is an influential predictor of whether and in which direction students adjust their educational aspirations after leaving compulsory school. Individuals pursuing any other pathway than general education are significantly less likely to stick to the educational aspirations they had at the end of compulsory school. These effects are sizeable, with VET students being 13.6 percentage points (pp.) less likely ($p < 0.001$), and those in programmes leading to a vocational baccalaureate even 24.5 pp. ($p < 0.001$) less likely to stick to their aspirations. In contrast, holding other covariates constant, students in VET are 6.9 pp. ($p < 0.01$) more likely, and those pursuing a vocational baccalaureate 8.4 pp. ($p < 0.001$) more likely to lower their educational aspirations upon leaving compulsory school. Yet students in the aforementioned tracks are also more likely to adjust their educational aspirations upwards (VET: AME = 0.067, $p < 0.05$, vocational baccalaureate: AME = 0.161, $p < 0.001$). Thus, students in these two tracks exhibit a similar pattern of aspirational adjustment when compared to those in general education. In addition to these findings, the track attended at the end of lower secondary education is also statistically related to the way students adjust their aspirations. Compared to their counterparts in the high requirement track, students who attended the track with advanced requirements show a higher likelihood of adjusting their educational aspirations upwards (AME = 0.112, $p < 0.001$). However, those who attended the other two lower secondary tracks considered do not differ from students in the high requirement track regarding their adjustment of their educational aspirations.

In contrast to the results in Table 1 predicting the level of educational aspirations, socio-demographic factors, perceived parental pressure and educational achievement only play a limited role in explaining adjustments of educational aspirations. Although adolescents whose parents have no tertiary degree show a higher propensity to set higher educational goals, neither the highest parental ISEI nor the number of books at home are related to aspirational adjustments.

While treating all other covariates as they were observed, the predicted probabilities in Figure 3 clearly indicate that students in the academically most demanding general education track are least likely to adjust the educational goals they set at the end of compulsory school. The multinomial regression model in Table 2 predicts that 54.8 % (+/– 4.3 pp.) of students in general education will stick to their educational aspirations over the observed period. Conversely, only 11.5 % (+/– 2.3 pp.) of these students lower their educational aspirations. Students in VET (18.3 % +/– 2.9 pp.), and particularly those in programmes leading to a vocational baccalaureate (19.9 % +/– 4.4 pp.), are substantially more likely to adjust their educational goals downwards upon entering the upper secondary level. In contrast, 40.4 % (+/– 3.4 pp.) of VET students and 49.8 (+/– 5.9 pp.) of students in programmes leading to a

Figure 3 Effects of Track Placement on Adjustments of Educational Aspirations



Note: Predicted probabilities with 95% confidence intervals (N = 3294), Data: TREE2, own calculations.

vocational baccalaureate set higher educational goals than they set at the end of compulsory school.

Overall, our results on the adjustment patterns with respect to educational aspirations only partially support our hypotheses and findings from previous research. In line with the mechanisms suggested by RCT and the WM, students placed in the academically most demanding track of upper secondary education are less likely to lower their educational goals. This pattern closely mirrors recent evidence from Germany (Bittmann and Schindler 2021; Geven and Forster 2021), a country whose education system is similarly stratified. Students in the track leading to a vocational baccalaureate degree appear to receive a rather mixed ability signal (Karlson 2015), coupled with a less marked influence of significant others towards aspiring to specific educational goals (Van den Broeck et al. 2020). This is exemplified by the fact that more than two-thirds of students in this track adjust their educational aspirations upwards or downwards. However, students pursuing VET are not dissuaded from setting more ambitious educational goals. On the contrary, an equal share of these students stick to their aspirations or set higher educational goals. This finding contradicts Hegna's (2014) and Bittmann and Schindler's (2021) notion that students in vocationally oriented tracks are increasingly diverted from aspiring to tertiary degrees.

5 Conclusion

Educational aspirations play an important role in shaping students' educational trajectories and destinations. In this study, we examined the formation and dynamics of educational aspirations among compulsory school-leavers in Switzerland, drawing on longitudinal data from the TREE2 study. Theoretical frameworks for explaining educational aspirations, namely RCT and the WM, suggest that proceeding to the next educational stage constitutes a pivotal moment for revising educational aspirations, particularly in highly stratified education systems such as Switzerland's.

Our first analysis of the effect of track placement on the formation of educational aspirations shows that aspirations strongly diverge by track in lower and upper secondary education. Students in academically demanding tracks set substantially higher educational goals than those in the academically least demanding tracks, with those attending intermediary programmes situated in between. This finding proves robust when controlling for various other determinants of educational aspirations identified by previous research.

However, investigating how students adjust their aspirations after leaving compulsory school reveals more nuanced insights. Supporting our hypothesis, we find that students in general education tend to adjust their aspirations upwards or stick to their – generally high – initial aspirations. Further, in line with our expectations, students in programmes leading to a vocational baccalaureate adjust their aspirations the most, either by lowering or by increasing their initial educational goals. Contrary to our expectations, the results suggest that students entering VET are not dissuaded from setting higher educational goals after leaving compulsory school. Students in VET not only stick to or lower their aspirations, they also substantially increase them. This result suggests that students in VET develop aspirations for tertiary education much later than their counterparts in general education. This argument is in line with the fact that many VET graduates enrol in a subsequent vocational baccalaureate programme (e.g., Trede et al. 2020).

The results of the two analyses combined draw an interesting picture. On the one hand, they underline theoretical arguments by showing the unambiguous effects of general education, as this track is strongly oriented towards tertiary education and is accompanied by strong ability signals (Karlson 2015) as well as the influence of significant others towards aspiring to a specific educational goal (Van den Broeck et al. 2018). Similarly, they prove a good fit for intermediary tracks with no clear track orientation, fuzzier ability signals and more diverse influence exerted by significant others. On the other hand, the upward adjustment in the VET track is surprising under the theoretical premises. A similar pattern is observed by Basler and Kriesi (2019) for the occupational aspirations of adolescents in Switzerland.

How can we explain this interesting finding? Like Hegna (2014), we find that social characteristics strongly affect the formation of aspirations, while only barely affecting the way students adjust their aspirations. Empirically, the revision of aspirations is found to be mainly based on track placement and factors that change with it. First, beliefs about costs and benefits strongly mediate the formation of aspirations that coincide with milieu-specific norms, explaining the strong correlation between social characteristics and aspirations in the first place. Second, as track placement limits the spectrum of viable educational options, sends ability signals, and alters the constellation of significant others, there is less space in which milieu-specific norms can unfold. Students will not only assess their opportunities and abilities according to track placement, but also within a track (Bittman and Schindler 2021). When track placement exceeds or is below the students' expectations, they are more likely to revise their aspirations (Geven and Forster 2021). These new evaluations comprise their perceptions of abilities, motivation, and possible opportunities in the future (Heckhausen and Buchmann 2019). Consequently, track placement can shape beliefs about appropriate aspirations for a specific track upon its completion.

Students who complete VET are potentially about to enter the labour market and see that further investment directly affects their prospects. From their perspective, it is reasonable under certain preconditions, or in light of specific beliefs, to set goals for the next stage, as they have already passed a hurdle by obtaining a qualifying certificate. General education tracks do not prepare students to directly enter the labour market as they are oriented towards tertiary education. Given the investment students have already made, it seems most reasonable to follow this orientation and to stick to their aspirations as the hurdle of labour market entry is still ahead.

Despite identifying robust effects across different model specifications, this study has some limitations. The three-year period examined in this study is a specific, though undeniably important, snapshot of a student's educational career. However, the study does not provide insights into the long-term processes behind the formation of educational aspirations, nor does it allow us to evaluate whether and to what extent educational aspirations are realised. Further, the data does not explicitly enable us to model the proposed mechanisms of rational calculus and social influence. Neither can we control for students' educational performance in upper secondary education (which is an undeniably important determinant of educational aspirations; e. g., Khattab 2015; Karlson 2019), or for the learning environment. We further acknowledge the notion of Buchmann and co-authors (2016) that VET programmes are unique and offer different opportunities, and thus may best be treated as a heterogeneous category. Specifically, it is plausible that the requirement levels of different VET programmes correlate with the adjustment of educational aspirations.

The identified track-specific disparities in how students form and adjust their educational aspirations add to an emerging strand of literature and contribute to a deeper understanding of students' educational mobility in Switzerland. Although this cannot be determined here, these mechanisms are presumably more pronounced in the highly stratified Swiss education system than in systems with comprehensive secondary education (Buchmann and Dalton 2002; Parker et al. 2016). Although aspirations do not predetermine educational outcomes, they deserve adequate scientific attention. By demonstrating that educational aspirations are subject to temporal dynamics that are markedly shaped by track placement, we aim to contribute to a better understanding of educational trajectories. On this basis, we encourage researchers to investigate processes of aspirational change further. Specifically, we believe that explicit identification of the underlying mechanisms for, and examining the long-lasting implications of the adjustment of educational aspirations are promising approaches in this regard.

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Paper Four - Disadvantaged by Chance? Examining the Persistence of Relative Age Effects on Educational Achievement

Abstract: Most education systems have arbitrarily chosen annual cut-off dates for school enrolment, which create age differences of up to a year within a cohort of pupils. Prior research has shown that the oldest in a cohort systematically outperform their relatively younger peers. Yet, little is known about the temporal persistence of relative age effects in education. In this article, we investigate how relative age effects on educational achievement evolve over different stages of compulsory education. Drawing on administratively linked test score data comprising entire student cohorts in Northwestern Switzerland, we employ two complementary analytical approaches to examine for how long the advantages of relatively older pupils prevail. The results indicate that relative age effects diminish the more pupils progress in their educational careers. However, effects of relative age at school enrolment are still identifiable beyond sixth grade, which marks the transition into secondary education in Switzerland. Keywords: relative age effect; school starting age; cumulative (dis)advantage; education; Switzerland; data linkage[a b]

Collaboration: This work was developed in collaboration with Robin Benz.

Keywords: Relative Age Effect; School Starting Aage; Cumulative (Dis)Aadvantage; Education; Switzerland; Data Linkage

[a]This paper has been submitted to the journal AERA Open in November 2023.
[b]OSF repository available at: https://osf.io/9ceya/

Introduction

Age-based school entry laws employed in most education systems create systematic age differences by introducing yearly cut-off dates that define an interval of eligible birth dates for a new cohort of pupils to enter school. These arbitrarily set cut-off dates are important as they cause children born right after the cut-off date to be up to a year older than their counterparts born right before the subsequent cut-off date. To put this into perspective, in education systems where children must be five years old to enter school, the age difference created by cut-off dates accounts for up to twenty per cent of the total lifespan of those enrolled. Given the magnitude of these age differences, one would expect that relatively older children find it easier to adapt to the school environment due to their more advanced cognitive and psycho-social development (Black et al., 2011; Dhuey et al., 2019; Duncan et al., 2007). Scholars from various scientific fields – such as sports science, epidemiology, or educational research – have come to demonstrate that relative age within a cohort provides advantages to the relatively older while disadvantaging the relatively younger. The consequential outcomes resulting from age differences within annual age-grouped cohorts are commonly termed as relative age effects (Baker et al., 2010; Bedard & Dhuey, 2006).

Educational research has repeatedly underlined the importance of early childhood in shaping future educational outcomes and pathways. Gaps in educational performance that emerge when children enter school are critical as they prove persistent over time (Cunha & Heckman, 2007; Skopek & Passaretta, 2021). Several studies found that children who enter school relatively old compared to their classmates tend to achieve higher test scores (e.g., Bedard & Dhuey, 2006; Smith, 2009), are less likely to experience grade retention (e.g., Dicks & Lancee, 2018; Jerrim et al., 2022) and are overrepresented in more demanding educational programmes at the secondary level (e.g., Mühlenweg & Puhani, 2010; Ponzo & Scoppa, 2014). However, there are conflicting findings on the longevity of relative age effects in education. While some work indicates that later life outcomes such as earnings (e.g., Solli, 2017) or fertility behaviour (e.g., Peña, 2017) can be traced back to relative age at school enrolment, other studies show that relative age effects diminish the more learners progress on their educational pathways (e.g., Mavilidi et al., 2022; Thoren et al., 2016).

The present study contributes to this strand of literature by investigating the temporal persistence of relative age effects on educational achievement. Specifically, we analyse whether and to what extent pupils' relative age at school enrolment affects their performance in different subjects across different grades of compulsory education. In doing so, this study informs about whether – and no less importantly when – actions should be taken to address the implications of relative age effects. Determining if and when relative age effects diminish is particularly relevant for highly stratified education systems – such as the case of Switzerland portrayed here – since pupils are sorted into tracks with designated educational pathways at a young age.

Identifying the impact of relative age on educational achievement is a complex endeavour, as the effect of relative age is a composite with several channels through which this effect may unfold. In this paper, we employ two complementary identification strategies, namely a regression discontinuity design and an instrumental variable approach, enabling us to rule out specific components of the effect of relative age and allowing for a nuanced investigation of relative age effects on educational achievement. Due to a record linkage, we can analyse data from standardised assessments comprising entire student cohorts in Northwestern Switzerland across different subjects and grades. Our results provide evidence of substantial relative age effects in the early stages of compulsory school that diminish the more pupils progress through compulsory education.

The present study proceeds as follows. The next section introduces an analytical framework of relative age effects in education. Following a mapping of the state of research, we describe the data used and expound on the methodological approach. Subsequently, we present our results and conclude by critically discussing our findings and their implications.

Conceptual Background

Relative age effects refer to differential outcomes resulting from age differences within annual age-grouped cohorts (Baker et al., 2010). These age differences are caused by arbitrarily chosen cut-off dates that determine eligibility for an annual cohort based on the date of birth of individuals in the target group. In most of Switzerland, for instance, the cut-off date for school enrolment is the 31st of July.[1] Every year, children who turn four years old between the 1st of August and the 31st of July make up a cohort of school entrants. If all families comply with the admission rule, children born on the 1st of August are the oldest in a cohort and twelve months older than their counterparts born on the 31st of July. In reality, a non-negligible share of families opts to delay their child's school entry by a year (SCCRE, 2023), a practice that is discussed in the literature as academic red-shirting (e.g., Bassok & Reardon, 2013; Dhuey et al., 2019; Lenard & Peña, 2018).[2]

Conceptually, relative age effects are manifestations in a given outcome, such as performance in school, that can be attributed to initial age differences within a cohort that interact with social mechanisms over time. The link between children's biological age and their physical maturity and socio-emotional development (Eisenberg et al., 2010) grants relatively old children a head start for learning in school. This initial advantage among older children may unleash cumulative processes in a developmental environment where credit and support are allocated among individuals according to their performance.

What Merton (1968) coined as the Matthew effect is one example of a cumulative process leading up to relative age effects. If relatively old students benefit more from schooling early on, they will outperform their younger counterparts. And if skill begets skill, these pupils can follow a steeper learning curve, causing the differences between the oldest and the youngest of a cohort to grow over time. Complementing this view, Hancock and colleagues (2013) argue that gaps due to relative age endure and are propagated through self-fulfilling prophecies. While the Matthew effect identifies initial age-related disparities as the driver of relative age effects, the concept of self-fulfilling prophecies focuses on subsequent relative age (dis)advantages and emphasises the role of expectations and beliefs that arise from them. Self-fulfilling prophecies occur when (false) beliefs lay the ground for a new behaviour that eventually makes previous (false) beliefs come true (Jussim, 1986).

Self-fulfilling prophecies may drive relative age effects when involved actors – namely teachers, parents, and pupils – falsely associate differences in physical maturity and socio-emotional development with actual differences in abilities and talent. According to Rosenthal and Jacobson's (1968) seminal example on Pygmalion effects, teachers may (unconsciously) treat relatively older pupils preferentially if they mistake pupils' relative age for academic aptitude. For example, they might support older children with preferential resources such as more challenging assignments, additional learning opportunities or encouragement while the relatively younger children are denied such treatment. Consequently, pupils with a relative age advantage are better positioned to improve their academic abilities.

Complementary to Pygmalion effects, the notion of Galatea effects postulates that once pupils are aware of the expectations placed upon them, they begin to act in accordance with these expectations (Eden & Kinnar, 1991). Relatively old pupils who, through the confusion between ability and age by their teachers, believe they are more gifted than their younger peers develop higher self-efficacy and are motivated to keep outperforming their younger peers. In a similar vein, Marsh (2016) and Parker and colleagues (2019) proposed the Negative-Year-in-School-Effect as an extension of the Big-Fish-Little-Pond-Effect. This model hypothesises that being relatively young in a given grade negatively affects pupils' academic self-concept since pupils perceive their relative age as a reflection of their academic prowess. This effect endures over time through continuous social comparison with in-grade peers. Over the long run, Marsh (2016) argues that the Negative-Year-in-School-Effect on educational outcomes supersedes the effects of mere age differentials created during school enrolment.

While social mechanisms help explain the emergence and persistence of relative age effects, estimating the consequences of relative age at school enrolment on later educational achievement poses two types of epistemological challenges. The first of these challenges relates to the inseparability of concurring causal links between age and educational outcomes. On the one hand, it cannot be ruled out that children who entered school older relative to their peers simply perform better because they are older when they take the test (e.g., Black et al., 2011). On the other hand, it may not be relative age but rather the absolute age at school enrolment that is predictive of later educational outcomes (e.g., Dhuey et al., 2019). Assuming that all families comply with school enrolment regulations and that all pupils follow a linear educational career, pupils' relative age, absolute age at enrolment, and age at measurement are perfectly collinear, making it impossible to disentangle which effect actually determines educational outcomes. Thus, the estimate of relative age at enrolment is likely to be a composite effect. Nonetheless, this composite effect is integral to the social reality in schools as pupils, their teachers, and parents still act upon the age differences they observe. To align our findings with the established terminology used in previous research, we refer to differential outcomes resulting from age differences within age-grouped cohorts as relative age effects while acknowledging that effects of absolute age and age at measurement are inseparably involved as well.

The second challenge arises from factors that affect a pupil's relative position in the age distribution and may open competing channels through which educational outcomes are affected, thus potentially inducing endogeneity. These factors either stem from non-compliance with school enrolment regulations or non-linear progressions through grades (Sprietsma, 2010). On the one hand, delayed school enrolment, so-called academic red-shirting, or more infrequent early school enrolment, induces age differences that transcend the ones created by cut-off dates. On the other hand, pupils who skip or repeat a grade based on their performance in school experience a sudden shift in their age relative to others in a cohort. As selection happens in both situations – for instance, delayed school enrolment is more common among well-off families (e.g., Bassok & Reardon, 2013; Lenard & Peña, 2018) and low-performing pupils are more likely to suffer from grade retention (e.g., Dicks & Lancee, 2018; Jerrim et al., 2022) – the effect of relative age at school enrolment may be biased for these specific groups of pupils. The same applies to so-called season of birth effects when the season a child is born is related to parents' socio-demographic characteristics or specific developmental risks (e.g., Buckles & Hungerman, 2013). Given the multitude of channels that may be in play when analysing the effects of relative age on educational outcomes, it is vital to follow a methodological approach that limits potential distortions.

Empirical Evidence

Previous empirical work on the relationship between pupils' relative age at school enrolment and academic outcomes generally revealed positive short-term effects of being older relative to the rest of the cohort. Several studies provide evidence that individuals born in the first few months after the cut-off date for school enrolment achieve higher test scores than their younger peers in various subjects (Bjerke et al., 2022; Mavilidi et al., 2022; Peña, 2017; Ponzo & Scoppa, 2014; Smith, 2009; Thoren et al., 2016). As international comparative studies show, this effect is identifiable across different education systems with varying intensity (Bedard & Dhuey, 2006; Sprietsma, 2010). Moreover, scholars have come to demonstrate the positive effect of relative age on academic achievement using different methodological approaches, ranging from common regression frameworks to quasi-experimental designs such as regression discontinuity (e.g., Smith, 2009) or instrumental variables (e.g., Bedard & Dhuey, 2006).

The advantages of relatively old pupils also become apparent concerning educational pathways. Research from Germany (Mühlenweg & Puhani, 2010), Austria (Schneeweis & Zweimüller, 2014) or Italy (Ponzo & Scoppa, 2014) finds that pupils with a relative age disadvantage are less likely to be tracked into academic programmes at the secondary level rather than vocational programmes. Evidence suggests that relative age affects educational pathways even beyond secondary education, with findings suggesting that those with a relative age disadvantage at the time of school enrolment are less likely to attend tertiary education (Peña, 2017; Solli, 2017). Furthermore, recent studies from France (Dicks & Lancee, 2018) and Spain (Jerrim et al., 2022) find that children who were relatively young at school enrolment show a higher likelihood of repeating a grade.

Several studies identify relative age effects within the context of compulsory schooling that transcend mere performance-related outcomes. Using different data sources from the United States, Dhuey and Lipscomb (2008) find that relatively older high school students are 4-11 per cent more likely to become captains in sports teams or presidents in clubs. Instrumental variable estimates from Fumarco and Baert (2019) indicate that pupils with a relative age disadvantage have fewer friends in school and meet with them less often. In line with the notion of Pygmalion effects, results from Dhuey and Lipscomb (2010) indicate that relatively young pupils are over-referred to special educational needs services, with each additional month in relative age decreasing the likelihood of receiving these services by 2-5 per cent.

While persuasive evidence on relative age effects on educational achievement and attainment exists, there are mixed results on how enduring and persistent these effects are. On the one hand, some studies suggest that the effects of relative age at school enrolment persist through their educational careers. Others indicate substantial relative age effects on educational achievement in primary school and that these effects still prevail in secondary education, although the effect sizes slightly decrease (Bedard & Dhuey, 2006; Ponzo & Scoppa, 2014; Smith, 2009). Moreover, scholars provide evidence of modest wage penalties for individuals who entered school relatively young, even when educational achievement and attainment are accounted for (Peña, 2017; Schneeweis & Zweimüller, 2014; Solli, 2017).

On the other hand, some more recent studies fail to underline the persistence of relative age effects by showing that these effects vanish over time. Using longitudinal data, some studies find that while substantial relative age effects on educational achievement can be identified in primary education, these effects consistently diminish in size and vanish completely once pupils reach the end of compulsory education (Bjerke et al., 2022; Mavilidi et al., 2022; Thoren et al., 2016). Nam (2014), for instance, shows for Korea that the effect of relative age at enrolment in school does not persist by the time pupils graduate from upper secondary education. On the contrary, pupils with an initial relative age disadvantage showed higher engagement with academic studies upon entering upper secondary school, thereby compensating for their subpar achievement in lower secondary school. Findings from Bernardi and Grätz (2015) using English data suggest, however, that the negative effects of being relatively young at school enrolment vanish sooner for pupils whose parents are highly educated. Contradicting the findings described above, some studies do not find any indications that the relative age at which children enter school affects their labour market outcomes, such as wages or the probability of employment (Dobkin & Ferreira, 2010; Nam, 2014; Pehkonen et al., 2015).

Our Study

In the present study, we contribute to the literature by investigating the persistence of relative age effects throughout compulsory education. Examining how relative age effects unfold over different stages in pupils' educational careers may offer new insights to untangle conflicting findings on the long-term implications of relative age at school enrolment. Moreover, uncovering the temporal development of how relative age affects educational outcomes informs policymakers and teachers on whether and at which educational stage efforts to mitigate relative age effects should be taken.

This study examines the effect of relative age at school enrolment on test scores for the case of Switzerland. Switzerland's education system is characterised by early tracking, high stratification at the secondary level and marked differences in learning outcomes by the time students leave compulsory school (e.g., Buchmann et al., 2016), making an examination of relative age effects all the more relevant. Usually, children in Switzerland enter compulsory school after turning four years old, beginning with two years of kindergarten, followed by six years of primary education (grades 1-6) and three years of lower secondary education (grades 7-9). In the latter, pupils are allocated to one of several school types that differ by academic requirements. Compulsory education ends with completing ninth grade. At this point, almost all children either continue school in general education (in 2020: 31.3%) or take up vocational training (in 2020: 64.4%) (FSO, 2022a).

Data

The persistence of relative age effects on educational outcomes can best be studied using test scores from standardised performance assessments. This study relies on test score data from Northwestern Switzerland, the so-called Checks (BR NWCH, 2021), covering the period from 2015 to 2020. The Checks are administered annually in four cantons of Switzerland (Aargau, Basel-Landschaft, Basel-Stadt and Solothurn) measuring pupils' competence in various subjects in third, fifth (2018-2020), sixth (2015-2017), eighth and ninth grade. Due to the gradual implementation of the Checks across cantons, there are gaps in data coverage in specific canton-year-grade combinations (see Appendix A). In our analyses, we pool the test score data from different years by grade. As participation in the Checks is generally mandatory, the data covers entire student cohorts in cantons of Northwestern Switzerland. Overall, the region of Northwestern Switzerland comprises approximately one-sixth of all students in Switzerland. Since many employers, particularly host companies in the vocational sector of upper secondary education, request a portfolio of their applicants' results in the Checks from eighth and ninth grade, the Checks can be regarded as high-stakes tests, which likely contributes to the external validity of the data.

The dependent variables are test scores in German reading, German writing, and algebra. The test scores are measured in terms of weighted likelihood estimates (WLE), which were scaled by two-parameter logistic models in the cases of German reading and algebra and multi-facet Rasch models in the case of German writing (König & Berger, 2021). For ease of interpretation, we standardised the WLE, so they have a mean of zero and a standard deviation of one across subjects, years, and grades.

Apart from test scores, the Checks provide minimal information about the test takers. Only due to a record linkage to administrative data provided by Switzerland's Federal Statistical Office (FSO, 2022b, 2022c, 2022d, 2022e, 2022f, 2022g, 2022h, 2022i, 2022j, 2022k, 2022l, 2022m) and the Central Compensation Office (CCO, 2022a, 2022b, 2022c, 2022d, 2022e, 2022f, 2022g, 2022h, 2022i, 2022j), we were able to obtain information on pupil characteristics. Next to their exact birth dates, we gathered information on pupils' sex (male or female), migration background (native or first- or second-generation migrant), parental income (mean taxable income of parents as deciles) and household characteristics, namely the living area per capita (in square metres) in the parental household and whether a pupil lives in a single-parent household. From the Checks, we have information about the canton, grade, the year in which the pupil took the test and foreign language use at home. Table 1 provides an overview of the analytical samples by grade and analytical approach.[3] We obtained information on the cut-off dates by contacting cantonal administrations (see Table A2 in Appendix A).

Table 1: Overview of Sample Sizes

	Number of observations in the original Checks data (2015-2020)	**Number of observations without duplicates and observations with missing birth dates**	**Number of observations without missing enrolment dates that entered school ±1 year around the eligibility window**	**Number of observations with no missing information on all covariates (IV samples)**	**Number of observations born ±60 days around the cut-off date with linear school careers (RD samples)**
3rd Grade	77,006	72,210	50,804	45,495	11,639
5th Grade	27,258	26,964	26,644	23,475	5,858
6th Grade	46,274	42,266	39,943	33,575	8,364
8th Grade	69,057	68,361	60,767	48,934	11,135
9th Grade	33,816	33,538	27,151	21,278	4,585

Three factors limit the analytical samples used in our study. First, since we have duplicates for some children in the data and because we cannot identify a small number of children unambiguously in the administrative records, 4.0 per cent of all observations are excluded from the analyses. Second, we exclude observations for specific cohorts in the cantons of Aargau and Basel-Landschaft where information on the exact cut-off date for school enrolment is unavailable. Yet, the federalist structure of Switzerland's education system offers an analytical benefit. Since the cantons retain extensive jurisdiction over the modalities of school enrolment, there is variation in cut-off dates between cantons and years, which minimises concerns about potential endogeneity due to season of birth effects. Lastly, the number of observations is further restricted by missing information on the variables used in the models, and we limit the observations to pupils who were born one year before or after the legal enrolment dates per canton and year. Pupils that skipped a grade or were retained twice resemble a particular population which we exclude from our analysis. These observations resemble about 0.6 per cent in third grade up to 4.4 per cent in ninth grade.

Empirical Strategy

Given the various channels through which relative age can be affected and influenced (Sprietsma, 2010), it is vital to establish a methodological approach that allows unequivocal inference on relative age effects on educational performance. Bedard and Dhuey (2006) made a convincing case by showing that estimating relative age effects via OLS would yield downwardly biased estimates. In the present study, we opt for two complementary approaches to address these endogeneity concerns.

As the first identification strategy, we exploit random variation in relative age caused by the arbitrarily set cut-off dates as a quasi-experiment. Using a regression discontinuity (RD) design (e.g., Lee & Lemieux, 2010), we compare pupils whose birthday lies right after the cut-off date for school enrolment – the oldest in a cohort – to those born right before the cut-off date. Given that the variation in birthdays is random, a discontinuity in test scores around the cut-off date can be attributed to the difference in relative age. In light that the randomness of birthdays is a compelling assumption, several previous studies have exploited the discontinuity around the cut-off date as an exogenous source of variation for causal inference on relative age effects (e.g., Crawford et al., 2014; Dicks & Lancee, 2018; Smith, 2009).

The absence of manipulation of treatment status is an essential prerequisite in an RD design (Lee & Lemieux, 2010). Delayed or early school entry, as well as grade retention or skipping, likely pose a threat to this identification assumption as these practices – rather than the day of birth in relation to the cut-off date – determine a pupil's relative age, thus introducing endogeneity to the model. This violation is particularly striking when there is self-selection among specific groups into these practices (e.g., Bassok & Reardon, 2013; Dicks & Lancee, 2018; Jerrim et al., 2022; Lenard & Peña, 2018), potentially creating systematic differences in academic outcomes among complying and non-complying individuals. Since our data neither provides information on the year of school enrolment nor on grade retention or skipping, we cannot distinguish pupils who enrolled in school outside the envisaged school year from those who skipped or repeated a grade. In light of these constraints, we opt for a sharp RD design limiting the analytical samples to pupils who – in retrospect – complied with the enrolment regulations and who did not repeat or skip a grade (see Table 1). Hence, our estimate of the relative age effect only applies to individuals born a given number of days around the cut-off date who complied with the enrolment regulations and were able to sustain a linear school career.

By counting the number of days between the birthday of a pupil i and the cut-off date that was in place for a given year and canton ($Birthday$), we define a bandwidth before and after the cut-off date to assign treatment status ($Treatment$), namely being relatively old at school enrolment. To account for the fact that the functional form may differ before and after the cut-off date, we allow for separate slopes by introducing an interaction term, which yields the following equation to be estimated in OLS:

$$Score_i = \alpha + \beta_1 Birthday_i + \beta_2 Treatment_i + \beta_3 Birthday_i \times Treatment_i + \gamma X_i + \varepsilon_i$$

where $Score_i$ represents a pupil's test score in a given subject, β_2 is the causal effect of interest and γX_i denotes the set of control variables.

Guided by optimal bandwidth selectors (Calonico et al., 2020; Imbens & Kalyanaraman, 2012), we find that 60 days on each side of the cut-off date is an appropriate bandwidth to address the bias-variance trade-off (see Table 1). In light of the sensitivity of confidence intervals to the bandwidth and functional form assumptions in an RD design, we complement our results with a non-parametric estimation of the treatment effect (Calonico et al., 2018).

Based on observable characteristics in our data, we find no indication that observations on both sides of the cut-off date are systematically different. The variance in the cut-off dates by canton and year additionally helps to rule out season of birth effects potentially caused by environmental factors and differences in gestational preferences by specific socioeconomic groups. What we do observe is that there are fewer pupils born just before the cut-off in higher grades, particularly in grades eight and nine. We consider the possibility that, over time, grade retention becomes more likely among pupils born before the cut-off date (Dicks & Lancee, 2018; Jerrim et al., 2022). Since we restrict our analytical samples in the RD approach to pupils who sustained a linear school career, the pupils born before the cut-off date who remain the analytical samples in later grades may represent a particularly gifted subpopulation. In this case, we would expect an underestimation of the RD estimates in later grades.

While our estimates using a sharp RD design provide valuable insights into the extent to which relative age differentials induced by the cut-off date for school enrolment manifest in educational performance, the exclusion of pupils that enter school outside the envisaged school year or did not sustain a linear school career might provide an inaccurate reflection of the reality in schools. Furthermore, if treatment status is related to educational performance, the results may be downwardly biased - particularly in later grades - as we potentially exclude relatively young pupils who were not able to sustain a linear school trajectory because of their subpar performance. Therefore, we complement our findings using an instrumental variable (IV) approach, which allows us to consider observations with non-linear school careers and who did not comply with the enrolment regulations.

Once we include pupils whose relative age exceeds the possible range of an enrolment window in the analyses, a solution is needed to overcome the issue that unobservable factors may confound the observed age at school enrolment and thus the effect of relative age on school performance. For instance, a fraction of pupils is relatively old because they repeated a grade, while another fraction is relatively old because they positively selected into delayed school entry. To resolve this problem, we follow an approach introduced by Bedard and Dhuey (2006) and use assigned relative age as an instrument of observed relative age. Assigned relative age refers to the age at enrolment children would have in the absence of early or late enrolment and grade retention or grade skipping, respectively. In practice, assigned relative age is calculated using a child's birthday relative to the cut-off date without considering the birth year. Using assigned relative age as an instrumental variable for observed age is an established approach employed in several previous studies (e.g., Nam, 2014; Peña, 2017; Ponzo & Scoppa, 2014).

More specifically, we estimate the following equations using 2SLS:

$$\text{First stage: } \widehat{ObservedAge_i} = \pi_{10} + \pi_{11} AssignedAge_i + \gamma X_i + \upsilon_i$$

$$\text{Second stage: } Score_i = \pi_{20} + \pi_{21} \widehat{ObservedAge_i} + \gamma X_i + \upsilon_i$$

where π_{11} captures the effect of assigned relative age on children's observed age, adjusting for covariates γX_i, and π_{21} captures the effect of relative age on test scores.

If consistent, the IV approach produces an estimate that resembles an unbiased estimator of the effect of entering school one year older (Bedard & Dhuey, 2006), which is comparable to the estimate of the treatment status in the RD design. However, three conditions must hold for the IV estimate to be interpretable as a local average treatment effect (LATE). First, a sufficient correlation between pupils' actual age at enrolment and their assigned age is needed. Since most observations (75.9%) start school in the envisaged school year and never skip or repeat a grade, this first requirement is satisfied.

Second, assigned relative age is required to be uncorrelated with unobserved covariates of educational achievement in the error term. While this condition cannot be evaluated empirically, there are approaches to corroborate that this condition is satisfied. For instance, Ponzo and Scoppa (2014), which use a similar IV approach, regress all individual controls of the model on assigned age and conduct joint F-tests. Although this test does not validate the exogeneity of the instrument, it makes it more credible that the instrument is uncorrelated with unobservable confounders. In our case, we find no significant F-statistics in any of our analytical samples. Moreover, we find that the main effect is substantially robust to the inclusion of the control variables (see Appendix E3). Furthermore, as outlined above, we are confident to rule out season of birth effects as potential confounders.

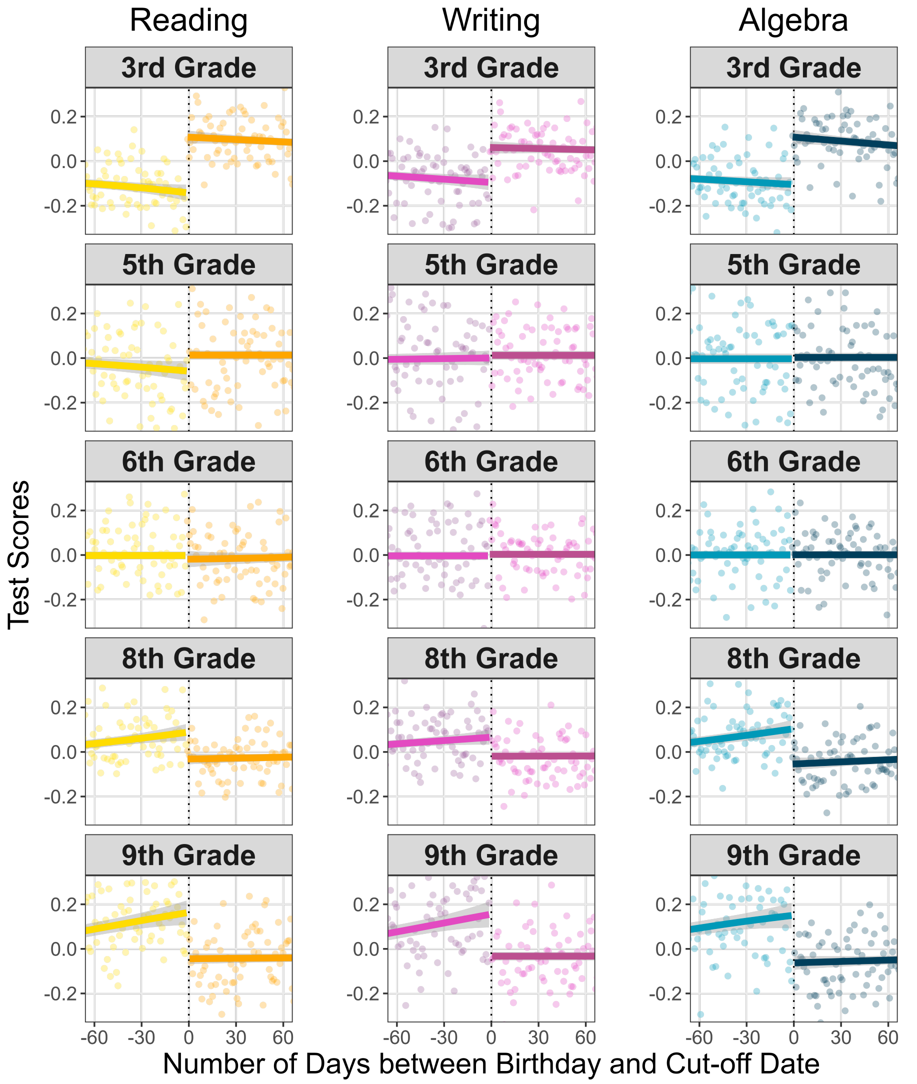
Third, as discussed by Barua and Lang (2016), the IV approach needs to satisfy the monotonicity assumption in relation to essential heterogeneity. Essential heterogeneity refers to the fact that treatment effects can vary across groups (e.g., children with higher academic aptitude could benefit even more from being relatively older) and that there is some degree of sorting based on treatment status (Fiorini & Stevens, 2021). Monotonicity means switching treatment status between two counterfactuals should always affect the treatment in the same direction. We find empirical evidence that pupils in the same grade and born in the same month either enrol late or are retained, enrol early or skip a grade, while most enrol on time. Although these observations have the same assigned relative age, their observed age at enrolment - and thus their treatment status - varies. This circumstance supports that the monotonicity assumption is violated (see Appendix E5 for a detailed discussion). Fiorini and Stevens (2021) analyse the consequences of a violation of the monotonicity assumption regarding the use of assigned age as an instrument for observed age. They conclude that a violation of the monotonicity assumption results in a potential overestimation of the treatment effect, although this effect still fairly reflects a LATE. While the IV approach likely produces upper-bound effects, the estimates still allow for a meaningful interpretation, especially in comparison to estimates from previous research or the RD estimates.

Regression Discontinuity Estimates

Figure 1 displays the bivariate relationship between pupils' birthdays relative to the cut-off date for school enrolment and test scores in the three subjects by grade. In each graph, the dotted vertical line indicates the cut-off date and the horizontal axis shows the number of days between the cut-off date and a pupil's birthday. The points represent binned sample means of test scores, through which a local polynomial model along with a 95 per cent confidence band is fitted.

A clear and substantial discontinuity around the cut-off date is apparent among third graders in all competence domains. Pupils who entered school relatively old achieve considerably higher test scores than their younger counterparts whose birthdays lie before the cut-off date. In fifth grade, the discontinuity around the cut-off date diminished in size and there is considerably more variation in test scores. In sixth grade, the test scores before and after the cut-off date converge and there is no clear evidence of a discontinuity anymore. Visual inspection of test scores around the cut-off date in grades 8 and 9 yields interesting yet unexpected insights. The discontinuity around the cut-off date reappears, but this time inverted. Among eighth and ninth graders, pupils who entered school relatively young outperform their older counterparts across all competence domains. The discontinuity around the cut-off date is more pronounced among ninth graders than among eighth graders.

Figure 1: Discontinuity in Test Scores around the Cut-Off Date by Grade



We estimate parametric and non-parametric RD models to determine whether the observed gaps in test scores depicted in Figure 1 can be attributed to relative age differences created by the cut-off date for school enrolment. Table 2 presents RD estimates across grades for the three competence domains. Each coefficient represents the estimated difference in test scores of pupils born shortly after the cut-off date compared to their younger counterparts whose birthdays lie before the cut-off date. All estimates are adjusted for covariates and apply only to individuals who complied with the enrolment regulations and sustained a linear school trajectory.

In line with the visual evidence presented in Figure 1, the multivariate models estimate a positive and statistically significant effect of being born shortly after the cut-off date on

Table 2: Parametric and Non-Parametric Regression Discontinuity Estimates by Grade

	3rd Grade		5th Grade		6th Grade		8th Grade		9th Grade	
	Parametric	Non-Parametric	Parametric	Non-Parametric	Parametric	Non-Parametric	Parametric	Non-Parametric	Parametric	Non-Parametric
Reading	0.312 ***	0.311 ***	0.142 **	0.165 **	0.055	0.059	-0.053	-0.068	-0.107	-0.109 *
	(0.035)	(0.039)	(0.046)	(0.053)	(0.040)	(0.040)	(0.037)	(0.045)	(0.055)	(0.050)
Observations	11,569		5,771		8,296		9,809		4,516	
R2	0.169	-	0.192	-	0.133	-	0.117	-	0.083	-
Writing	0.203 ***	0.206 ***	0.074	0.053	0.116 **	0.155 ***	0.078 *	0.092 *	-0.049	-0.069
	(0.037)	(0.034)	(0.047)	(0.048)	(0.039)	(0.045)	(0.032)	(0.039)	(0.050)	(0.045)
Observations	10,246		5,732		8,275		11,020		4,544	
R2	0.139	-	0.171	-	0.156	-	0.135	-	0.122	-
Algebra	0.228 ***	0.220 ***	0.108 *	0.118 *	0.066	0.055	-0.089 *	-0.083 *	-0.124 *	-0.061
	(0.036)	(0.037)	(0.049)	(0.049)	(0.039)	(0.042)	(0.036)	(0.035)	(0.055)	(0.065)
Observations	11,580		5,771		8,278		9,818		4,536	
R2	0.106	-	0.123	-	0.096	-	0.102	-	0.067	-

Notes: * p <0.05, ** p <0.01; *** p <0.001.
OLS coefficients for being born after the cut-off date with robust standard errors in parentheses. Controls not shown. Full models are provided in Appendix D1.

test scores among third graders. The effect sizes are marginally higher for reading ($\beta = 0.312, p < 0.001$) than for writing ($\beta = 0.203, p < 0.001$) and algebra ($\beta = 0.228, p < 0.001$). Notably, the estimates for third graders are similar in size to those presented in previous studies employing an RD design. For instance, Smith (2009) presents RD estimates for fourth graders of around 0.25 SD higher test scores in numeracy and reading.

For fifth graders, the estimates decrease in size. In the case of writing competence, neither the parametric ($\beta = 0.074, p > 0.05$) nor the non-parametric ($\beta = 0.053, p > 0.05$) estimates of entering school at a relatively older age are distinguishable from zero. Among pupils in sixth grade, the effect sizes further decrease. Being born shortly after the cut-off date for school enrolment accounts for less than 0.1 SD higher test scores in reading and algebra, with neither estimate being statistically significant at a 95 per cent confidence level. A statistically significant discontinuity around the cut-off date is only found in the case of test scores in writing.

In eighth grade, for test scores in reading ($\beta = -0.053, p > 0.05$) and algebra ($\beta = -0.089, p < 0.05$), the estimated effects even turn negative. In contrast, the models on test scores in writing suggest that relative age effects prevail in favour of relatively old children in eighth grade ($\beta = 0.078, p < 0.05$). Once pupils are in their last year of compulsory school, in ninth grade, the estimated effects on test scores are generally negative, mirroring the unexpected finding based on visual inspection of Figure 1. However, except for test scores in algebra using a parametric estimation ($\beta = -0.124, p < 0.05$) and test scores in reading using a non-parametric estimation ($\beta = -0.109, p < 0.05$), the estimated discontinuity in test scores is statistically insignificant.

Additional analyses generally indicate robustness of the findings presented in Table 2. Parametric models using smaller bandwidths around the cut-off date, namely 30 and 15 days, yield very similar results regarding point estimates and statistical significance (see Appendix D3). We further conducted subgroup analyses separating pupils based on their sex, language spoken at home and parental income. These analyses reveal that the discontinuities in test scores around the cut-off date do not systematically differ between foreign language and German-speaking pupils as well as pupils whose parental income lies in the upper versus lower half of the income distribution. In contrast, we find greater discontinuities in test scores for males in writing and for females in algebra, particularly in eighth and ninth grade (see Appendix D4). Moreover, we find nearly identical estimates when using matching samples created by coarsened exact matching (see Appendix D5).

In the early phases of compulsory school, the RD approach provides evidence of substantial relative age effects in favour of those whose birthdays lie shortly after the cut-off date for school enrolment. Yet, the more pupils proceed in their educational careers, relative age differentials created by cut-off dates diminish. This finding aligns with what some research has previously discovered (e.g., Mavilidi et al., 2022; Thoren et al., 2016). By the time pupils have reached the end of compulsory school, the RD models yield negative coefficients suggesting that pupils who entered school at a relatively younger age outperform their older peers. A study by Nam (2014) using Korean data also finds that relatively young pupils achieve higher test scores than their older peers. However, several of our estimates for eighth and ninth graders lack statistical significance.

The reversal of the discontinuity around the cut-off date towards the end of compulsory school contradicts the theoretical expectations on the persistence of relative age effects. Neither do the results from the RD models support the conjecture that initial age-related achievement disparities induce divergent achievement gains over time, nor do the results provide evidence that subsequent age-related expectations hinder pupils who entered school relatively young from catching up to their older peers.

We can think of three explanations for the unexpected findings regarding test scores of relatively young pupils in lower secondary education. First, schooling effectively counteracts the adverse implications of being relatively young at school enrolment. Just as the age differences become proportionally smaller over time, so do the age-related disadvantages of those who entered school relatively young. Second, it may be plausible that pupils who entered school relatively young develop learning strategies to compensate for their initial relative age disadvantage. Third, the results of the RD models might reflect a statistical artefact due to unobserved processes that systematically induce selectivity around the cut-off date. Since the RD models only consider students who were able to sustain a linear school career, the higher learning outcomes among relatively young students in lower secondary education may be driven by grade retention. Granted that the relatively young tend to perform subpar in school, these pupils may be retained more often, leaving particularly gifted and resilient pupils whose birthdays lie shortly before the cut-off date in the analytical sample. The assumption that relatively young students suffer from grade retention more often finds empirical support in previous studies (Dicks & Lancee, 2018; Jerrim et al., 2022).

Instrumental Variable Estimates

Pursuing an instrumental variable approach allows us to investigate relative age effects in a less confined way since pupils with non-linear educational careers can also be considered. Table 3 depicts the estimates of the IV regressions across grades and subjects. The estimates are adjusted for covariates and represent the effect on test scores of being one year older at school enrolment. For all models in Table 3, F-tests allow rejecting the null hypothesis of weak instruments. Furthermore, all models yield highly significant Wu-Hausman test statistics, indicating that OLS estimates would be inconsistent and 2SLS estimation is preferable.

Similar to the results from the RD design, the estimates from the multivariate IV models find statistically significant positive effects of being one year older on test scores among pupils in third grade in all subjects. The effect is largest in reading ($\pi = 0.458, p < 0.001$), followed by algebra ($\pi = 0.381, p < 0.001$) and writing ($\pi = 0.313, p < 0.001$). Smith (2009) and Peña (2017) report IV estimates of similar magnitude on the same subjects for fourth and third-graders, respectively.

Despite a decrease in size, the effects of age at enrolment remain statistically significant throughout fifth and sixth grade. The estimated effects in sixth grade of being one year

Table 3: Instrumental Variable Estimates by Grade

		3rd Garde	**5th Grade**	**6th Grade**	**8th Grade**	**9th Grade**
Reading	Age at Enrolment	0.458 ***	0.247 ***	0.268 ***	0.201 ***	0.082 *
		(0.023)	(0.031)	(0.029)	(0.025)	(0.036)
	Observations	45,110	23,039	33,215	42,628	20,956
	R2	0.124	0.164	0.112	0.094	0.092
Writing	Age at Enrolment	0.313 ***	0.187 ***	0.324 ***	0.213 ***	0.130 ***
		(0.024)	(0.031)	(0.028)	(0.022)	(0.034)
	Observations	39,729	22,850	33,109	48,386	21,060
	R2	0.116	0.168	0.119	0.107	0.106
Algebra	Age at Enrolment	0.381 ***	0.180 ***	0.252 ***	0.141 ***	0.086 *
		(0.024)	(0.033)	(0.030)	(0.025)	(0.036)
	Observations	45,131	23,039	33,155	42,666	21,020
	R2	0.061	0.090	0.058	0.079	0.068

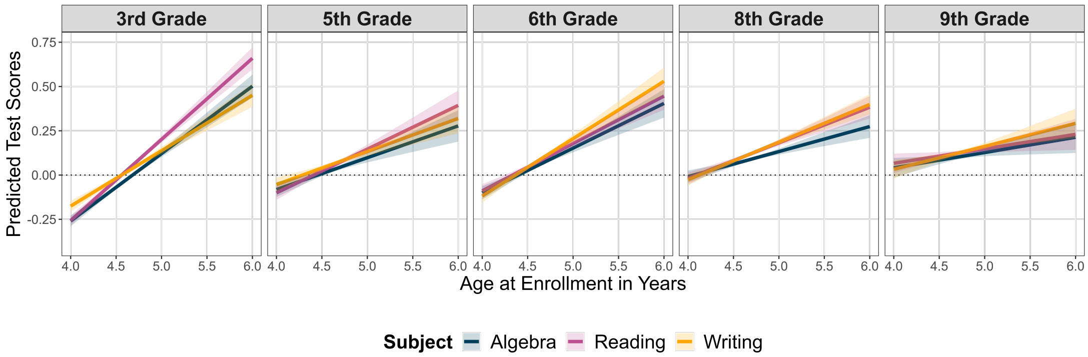
Notes: * p <0.05, ** p <0.01; *** p <0.001.
Estimates from 2SLS regressions with robust errors in parentheses. Controls not shown.
Full models are provided in Appendix E1.

older at the time of school enrolment amount to 0.324 SD ($p < 0.001$) higher test scores in writing and 0.252 SD ($p < 0.001$) higher test scores in algebra. Similar to these results, Ponzo and Scoppa (2014) find an apparent reduction of relative age effects between fourth and eighth graders regarding test scores in mathematics using Italian data.

In contrast to the RD estimates, the models using an IV approach indicate for all subjects that relative age effects in favour of relatively older pupils persist into lower secondary education. In eighth grade, we once more find a reduction in effect sizes across all subjects. Nonetheless, pupils in eighth grade that were one year older at the time of school enrolment have, on average, 0.213 SD ($p < 0.001$) higher test scores in writing, 0.201 SD ($p < 0.001$) higher test scores in reading, and 0.141 SD ($p < 0.001$) higher test scores in algebra. In ninth grade, the estimated effect sizes decrease again and remain statistically significant for the domain of writing ($\pi = 0.130, p < 0.001$), reading ($\pi = 0.082, p < 0.05$), and algebra ($\pi = 0.086, p < 0.05$). This aligns with findings from Smith (2009) who reports a decrease in relative age effects from fourth to tenth grade while the estimates also remain statistically significant. According to their findings, writing is also the subject that shows the smallest decrease in effect size over time.

To illustrate the main results of the IV models, Figure 2 depicts predictive margins of the age at enrolment on test scores across all grades. Mirroring the RD models, the IV models indicate that the advantage of being relatively older at school enrolment decreases

Figure 2: Predicted Test Scores by Age at Enrolment and Grade



throughout the compulsory school. In contrast, however, the IV models suggest that relative age effects persist into lower secondary education and that the effects of relative age at school enrolment do not change direction among eighth and ninth graders.

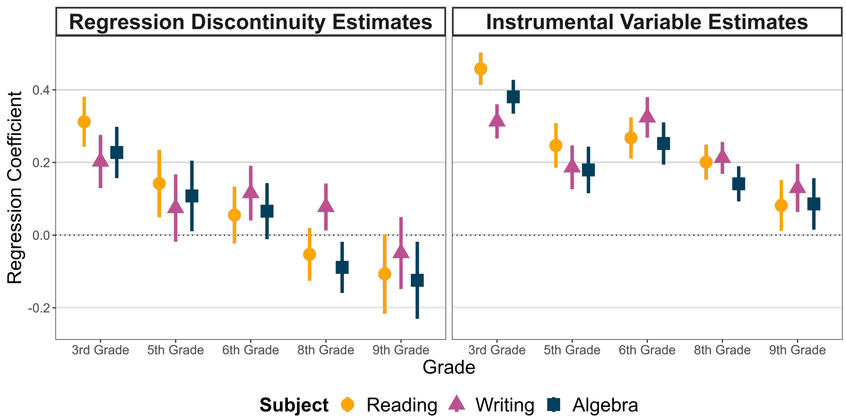
As an additional check for the IV models we ran subgroup analyses, analogous to the RD approach, which indicate overall robustness of our findings (see Appendix E2). Only in ninth grade, we find that the relative age effect is insignificant for males, foreign language-speaking pupils, and pupils from lower-income households in reading. Similarly, in ninth grade, the effect is insignificant for females, foreign language-speaking pupils, and pupils from upper-income households regarding algebra. The relative age effect on writing vanishes only for the sample that speaks a foreign language at home. Further, we compared our IV estimates with estimates from OLS models using the same samples and covariates. The OLS results indicate a consistent negative relationship between being one year older at school enrolment and test scores across all subjects and grades while being highly significant, except for reading in third grade (see Appendix F). This underlines that OLS is unsuitable for identifying relative age effects as they are subject to endogenous factors such as red-shirting or grade retention.

Persistent Relative Age Effects?

In this study, we investigated the temporal persistence of relative age effects in education with two different identification strategies. To compare the estimates of the two analytical approaches, we must clarify and recall what effects they identify. On the one hand, the RD models determine the difference in test scores between pupils born up to 60 days after and those born up to 60 days before the cut-off date for school enrolment for pupils who complied with school enrolment regulations and did not repeat or skip a grade. Thus, the RD models refer to a LATE around the cut-off, which only apply to these pupils. Due to the selectivity among those born before the cut-off date, the RD approach likely underestimates relative age effects, particularly in higher grades. On the other hand, the IV approach allows us to consider pupils with non-linear school trajectories or who enrolled in school early or late as well. However, its estimates only refer to a LATE of being one year older at school enrolment if all conditions of an IV are met. As outlined previously, as our instrument likely violates the monotonicity assumption, the IV approach likely produces upper-bound effects (Fiorini & Stevens, 2021).

When putting the results of both empirical approaches together, we can draw a nuanced picture of the temporal dynamics of relative age effects throughout compulsory education although our estimates only allow for an approximation of the true causal effect. For a graphical overview, Figure 3 depicts the point estimates of the RD and IV models for each subject and grade along with 95 per cent confidence intervals. We find substantial relative age effects for both identification strategies in third grade, which diminish in subsequent grades. Albeit the similarity between the estimates from the RD models and the IV models for grades in primary education, the deflation of effect sizes is more apparent in the RD framework. The RD models' effects for pupils in lower secondary education even contradict the IV estimates and the theoretical implications of relative age effects as they indicate that relatively young pupils outperform older pupils.

Figure 3: Estimates of Relative Age at School Enrolment on Test Scores across Grades



Both identification strategies make compelling cases that the advantages of pupils who entered school relatively old diminish over time. The RD approach finds that - among those who can sustain a linear school career - children born right before the cut-off even outperform their counterparts born right after the cut-off in lower secondary education. The IV approach contradicts this finding, as the relative age effects in favour of those who entered school relatively old remain significant until the end of compulsory education. Considering the potential underestimation in the RD framework and that the IV results resemble upper-bound estimates, we cannot rule out that the effects of relative age at school enrolment are still marked in sixth grade, when pupils are allocated to educational tracks based on their abilities. Hence, it is plausible that relative age affects track placement, as suggested in previous studies (e.g., Mühlenweg & Puhani, 2010; Ponzo & Scoppa, 2014).

If the relative disadvantage for young pupils through primary education is large enough, these pupils might be compelled, via social and institutional mechanisms, to repeat a grade and trade in an additional year of schooling to minimise the externalities of the relative age disadvantages. The systematic exclusion of such observations from the sample could explain the steeper reduction and, in lower secondary education, even the inversion of relative age effects estimated in the RD approach. The imbalance of the samples regarding treatment status supports this conjecture. Such an argumentation, although not testable with our data, is in line with research which shows that grade retention is more frequent among relatively younger pupils (Dicks & Lancee, 2018; Jerrim et al., 2022). Furthermore, alternative explanations for pupils born before the cut-off date to drop out of the sample more frequently than their counterparts who entered school relatively old, namely, to enter a private school or to move outside Northwestern Switzerland, are less compelling.

Discussion

Pupils who did not start learning on the same level as their peers might subsequently fall behind throughout their educational careers. Age-based school entry laws based on cut-off dates may contribute to early gaps in educational performance as they create relative age differences within a cohort of pupils, affecting their school readiness. Previous studies from various countries have come to demonstrate that the youngest in a cohort fall behind their relatively older peers. However, evidence on the longevity of relative age effects remains inconclusive. The present article aims to contribute to this strand of literature by investigating the temporal persistence of relative age effects on educational achievement.

In this study, we used administratively linked test score data encompassing entire student cohorts in Northwestern Switzerland to examine the effects of relative age at school enrolment on test scores at different points of compulsory school. To identify these effects, we employed two complementary empirical strategies, which provide a nuanced picture of relative age effects. On the one hand, estimates from a sharp RD design indicate that the initial advantages of relatively older pupils diminish over time. This is supported by the results from the IV approach, which allows us to consider pupils who entered school outside the envisaged school year or who repeated or skipped a grade. On the other hand, the results differ between the two identification strategies as the RD models suggest that pupils who entered school relatively old achieve lower test scores than their younger peers in lower secondary education, while the IV models indicate a greater temporal persistence of relative age effects. Notably, in the IV models, these effects persist over the transition into lower secondary education.

One convincing explanation for the sooner vanishing and even reversed relative age effects in eighth and ninth grade in the RD design is that the relative age disadvantage to the detriment of relatively young pupils during primary school might be powerful enough that these pupils are more likely to be retained. If this is the case, relatively young pupils who repeated a grade drop out of the RD samples in later grades, leaving only a resilient – and presumably particularly gifted – subpopulation of pupils born before the cut-off date in the samples, which would result in an underestimation of the effect. Consequently, if the educational system would not allow grade retention, we would expect more persistent relative age effects in an RD approach. In contrast, the IV approach still shows noticeable effects of relative age at school enrolment after the transition into lower secondary education. However, these effects should be interpreted as upper-bound estimates. Hence, we argue that the combination of the two results informs us best about the gradations of relative age effects, as both identification strategies imply that relative age effects lessen as pupils progress through compulsory education. However, when interpreting the RD results as lower-bound and the IV results as upper-bound estimates, relative age effects are potentially still at play when students are allocated to performance-based tracks in sixth grade.

One caveat of this study is that the data used does not allow the creation of panel-like data, where individual pupils' learning trajectories could be traced throughout compulsory education. Another limitation stems from the fact that we cannot distinguish between pupils who enrolled in school late or were retained and those who entered school early or skipped a grade, respectively. This would have been very informative to test our argument for more frequent grade retention among relatively younger pupils. Furthermore, despite being widely applied in the literature on relative age effects, the approach of using assigned relative age as an instrument is not free of methodological criticism (Barua & Lang, 2016; Fiorini & Stevens, 2021). The comparison of the two strategies, however, yields valuable insights into the persistence of relative age effects. Like most previous research on relative age effects in education, this study is no exception to the epistemological problem regarding the inseparability of relative age, absolute age at enrolment, and age at test-taking. Similarly, we cannot explicitly model the social mechanisms that give rise to relative age effects. While our results contradict the conjecture of the Matthew effect, it would be very promising for future research to investigate the role of self-fulfilling prophecies in the emergence and temporal development of relative age effects.

Despite these limitations, our study shows that time works against the relative age effect, but likely too slowly. In primary education, the effect is still evident and might cause a biased evaluation of performance by teachers. Further, poor evaluations can motivate parents to reconsider their educational goals for and their investment in their children. This becomes more evident when considering that in Switzerland and other stratified education systems such as Germany or the Netherlands, the transition into performance-based tracks happens at the end of primary education. Parents and teachers might be enticed into considering grade retention to facilitate the allocation into more advanced tracks. However, from a pupil's perspective, grade retention exerts a strong ability signal accompanied by the risk of stigma and decreased self-efficacy (Marsh, 2016; Parker et al., 2019). Furthermore, if track placement is subject to relative age effects, they play a role in determining educational pathways and subsequently affect outcomes later in life. Therefore, particularly during the critical phase of primary education, relative age poses a threat to equity in educational outcomes that should be addressed.

Recognising that relative age effects might partially shape educational pathways, we can draw a line to findings on outcomes later in life. If relative age disadvantages translate into distinct educational pathways where younger students are more likely to face less favourable learning conditions, this can cause diverging outcomes later in life. Additionally, if students compensate for their relative age disadvantages with an additional year of schooling, this will ultimately delay their labour market entry, which can partially explain differences in labour market outcomes.

We acknowledge that cut-off dates for school enrolment are a practical and widely accepted practice to group children into school cohorts. However, the implications of arbitrarily set cut-off dates for pupils' learning outcomes are non-negligible. In view of our results, the adverse effects of age-based school entry laws warrant a policy response to overcome or at least mitigate relative age effects. Webdale and co-authors (2020) recently published an overview of proposed solutions to the relative age effect. One possible approach would be to consider learning gains over time to capture the general aptitude of students rather than performance on a test day. Another approach implies changing the institutional setting by either decreasing the number of months between cut-off dates or clustering pupils with similar birth dates in classes, ultimately reducing relative age differentials. A further – and likely more feasible – approach concerns the social mechanisms that give rise to enduring relative age effects. Teachers should be made more aware that relative age affects their pupils' learning and should adjust their grading practices and means of support accordingly.

Notes

[1]Due to the federalist structure of Switzerland's education system, the subnational units, the cantons, retain extensive jurisdiction over educational policy in compulsory education. Among other things, cantons have autonomy over school entry laws, including cut-off dates for school enrolment. Starting in 2007, the cantons were mandated to gradually adopt the nationwide cut-off date of the 31st of July. For more information, see Appendix A.

[2]The rates of delayed school enrolments in the four cantons that make up Northwestern Switzerland are as follows: Out of all children who reached school eligibility, 14% in Aargau, 14% in Solothurn, 6% in Basel-Landschaft and 6% in Basel-Stadt enter school at least one year late (SCCRE, 2023, 61f.)

[3]More information on the record linkage and the analytical samples is provided in Appendix B. See Appendix C for descriptive statistics of the variables.

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Appendices: *Disadvantaged by Chance? Examining the Persistence of Relative Age Effects on Educational Achievement*[*]

16.10.2023

Contents

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[*]A replication package for this study is available on (link not included for anonymisation).

Appendix A

Table A1 presents the implementation of the Checks by canton, year and grade.

Table A1: Implementation of the Checks by Canton, Year and Grade

	2015	**2016**	**2017**	**2018**	**2019**	**2020**
3rd Grade	AG, BL, **BS**, SO	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, SO	**AG**, **BL**, **BS**, SO
5th Grade				**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**
6th Grade	AG, **BL**, **BS**, SO	**AG**, **BL**, **BS**, SO	**AG**, **BL**, **BS**, SO			
8th Grade	**AG**, **SO**	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**	**AG**, **BL**, **BS**, **SO**
9th Grade	AG, **SO**	**AG**, **SO**	**AG**, **BL**, **SO**	**AG**, **BL**, **SO**		**AG**, **BL**, **SO**

Note: The four cantons of Northwestern Switzerland are Aargau (AG), Basel-Landschaft (BL), Basel-Stadt (BS) and Solothurn(SO). Labels in boldface indicate that participation in the Checks was mandatory for all pupils in the respective year and canton. Missing canton labels indicate that the Checks were not administered in a given year and grade. From 2018 onward, the Checks in sixth grade were replaced with the Checks in fifth grade. The Checks for ninth graders in 2019 were cancelled due to the Covid-19 pandemic. The table illustrates the limited potential for creating a panel structure with multiple observations of pupils across different Checks. For example, third graders who participated in the Checks in 2015 only reappear in the data as eighth graders in 2020, provided that they a) participated in both Checks, b) sustained a linear school career and c) did not move out of Northwestern Switzerland.

Table A2 reports the enrolment dates per year and canton. We collected the dates by contacting the cantonal administrations. For some cantons and years, the enrolment dates could not be recovered.

Table A2: Enrolment Dates by Canton and Year

	SO	**AG**	**BL**	**BS**
2004	01.05.1999	01.05.1999	01.04.1999	01.05.1999
2005	01.05.2000	01.05.2000	01.04.2000	01.05.2000
2006	01.05.2001	01.05.2001	01.04.2001	01.05.2001
2007	01.05.2002	01.05.2002	01.04.2002	01.05.2002
2008	01.05.2003	01.05.2003	- [2]	01.05.2003
2009	01.05.2004	01.05.2004	01.05.2004	01.05.2004
2010	01.05.2005	01.05.2005	01.05.2005	01.05.2005
2011	01.05.2006	01.05.2006	01.05.2006	01.05.2006
2012	01.05.2007	01.05.2007	01.05.2007	01.05.2007
2013	01.06.2008	01.05.2008	16.05.2008	01.06.2008
2014	01.07.2009	01.05.2009	01.06.2009	16.06.2009
2015	01.08.2010	- [1]	16.06.2010	01.07.2010
2016	01.08.2011	- [1]	01.07.2011	16.07.2011
2017	01.08.2012	- [1]	16.07.2012	31.07.2012
2018	01.08.2013	01.08.2013	01.08.2013	01.08.2013
2019	01.08.2014	01.08.2014	01.08.2014	01.08.2014
2020	01.08.2015	01.08.2015	01.08.2015	01.08.2015
2021	01.08.2016	01.08.2016	01.08.2016	01.08.2016

Note: [1] Enrolment dates were specific to each municipality due to cantonal jurisdiction and could not be determined. [2] Data was missing. SO (Solothurn), AG (Aargau), BL (Basel-Landschaft), BS (Basel-Stadt).

Appendix B

The Checks data is available via SWISSUbase (project number: 13889) and requires an approved application. If it is intended to link the data to administrative records from the Federal Statistical Office (FSO), the application must mention this and additional approval is needed by the data owners, the Bildungsraum Nordwestschweiz (BR NWCH). Upon approval, data users contact the FSO to set up a plan for record linkage and a data user agreement. The FSO contract of this study refers to Nr. 220506. For each year of the Checks considered (2015-2020), students are assigned a pseudo-identification number that allows linking students to their social security number in the database of Switzerland's permanent and non-permanent inhabitants (STATPOP). From this source, we obtain information on the pupil's birthday, sex, state of birth, household classification (e.g., single-parent household), and the number of people living in the same home. Further, it contains the ID that links the biological mother and father to the pupil. From their records in the STATPOP, we obtain information on the mother's and father's country of birth. We link this information by year, e.g., the Checks from 2015 to the STATPOP of 2015 and the GWS as of 2015. The latter can be linked via an ID for the building and dwelling, and we can obtain the area of the living area. Lastly, we link data from the CCO (the central compensation office) to obtain information on the parents' taxable income. Because income is subject to temporal dynamics, we pool information from the past five years to calculate the mean income of mothers and fathers, e.g., for the Checks in 2015, the pooled CCO records from 2011 to 2015 were used. Table B1 illustrates the record linkage.

Table B1: Schematic of the Record Linkage

Source	Checks	STATPOP	STAPOP Parent	GWS	CCO Parent
Years	2015	2015	2015	2015	2011 - 2015
	2016	2016	2016	2016	2012 - 2016
	2017	2017	2017	2017	2013 - 2017
	2018	2018	2018	2018	2014 - 2018
	2019	2019	2019	2019	2015 - 2019
	2020	2020	2020	2020	2016 - 2020
Variable	Canton Year Grade Language at Home	Day of Birth Sex State of Birth Household Classification Numbers of Persons Living in the Household	State of Birth	Living Area	Taxable Income
Link ID	Pseudo ID	Pseudo ID ID Motehr ID Father Building ID Dwelling ID	ID Mother ID Father	Building ID Dwelling ID	ID Mother ID Father

Appendix C

C1 Sample Description 3rd Grade

Table C1: Sample Description 3rd Grade

3rd Grade	Checks total	Checks without duplicates and missing birthdates	Checks without invalid enrolment dates			IV Sample			RD Sample	
Observations	77,006	72,210	50,804			45,495			11,639	
Variable		Level	N	%	Missing	N	%	Missing	N	%	Missing
Sex		Female	24,921	49,05	0	22,274	48,96	0	5,821	50,01	0
		Male	25,883	50,95		23,221	51,04		5,818	49,99	
Migration Status		Native	32,024	70,06	5,097	31,941	70,21	0	8,255	70,93	0
		Second Generation	13,163	28,8		13,055	28,7		3,265	28,05	
		First Generation	520	1,14		499	1,1		119	1,02	
Language Spoken at Home		German	32,734	64,43	0	31,152	68,47	0	8,069	69,33	0
		Foreign Language	18,070	35,57		14,343	31,53		3,570	30,67	
Household Composition		Both Parents	45,119	89,11	172	40,504	89,03	0	10,425	89,57	0
		Single Parent	5,513	10,89		4,991	10,97		1,214	10,43	
Canton		AG	16,006	31,51	0	14,467	31,8	0	3,696	31,76	0
		BL	14,474	28,49		13,107	28,81		3,274	28,13	
		BS	8,241	16,22		6,882	15,13		1,790	15,38	
		SO	12,083	23,78		11,039	24,26		2,879	24,74	
Variable			Mean	SD	Missing	Mean	SD	Missing	Mean	SD	Missing
Age at Enrolment			4,63	0,43	0	4,62	0,42	0	4,56	0,43	0
Mean Income Decile			5,46	2,92	3,903	5,5	2,9	0	5,59	2,88	0
Area per Person			30,64	13,64	237	31,13	13,69	0	31,32	13,59	0
WLE Reading			-0,74	1,4	552	-0,69	1,39	385	-0,62	1,39	70
WLE Writing			-0,15	1,31	6,561	-0,1	1,29	5,766	-0,05	1,28	1,393
WLE Algebra			-0,53	1,54	444	-0,51	1,53	364	-0,43	1,51	59

Notes: The numbers reported for the sample used in the IV and the RD framework can slightly vary between models with respect to the dependent variable, as we did not exclude cases with missing information in the variables regarding performance in reading, writing and algebra. The rows that contain a value in the columns "Missing" refer to missing information in the corresponding variable, not the variable's level.

C2 Sample Description 5th Grade

Table C2: Sample Description 5th Grade

5th Grade	Checks total	Checks without duplicates and missing birthdates	Checks without invalid enrolment dates			IV Sample			RD Sample	
Observations	27,258	26,964	26,644			23,475			5,858	
Variable			N	%	Missing	N	%	Missing	N	%	Missing
Sex		Female	13,052	48,99	0	11,523	49,09	0	2,965	50,61	0
		Male	13,592	51,01		11,952	50,91		2,893	49,39	
Migration Status		Native	16,430	69,67	3,063	16,384	69,79	0	4,108	70,13	0
		Second Generation	6,754	28,64		6,711	28,59		1,669	28,49	
		First Generation	397	1,68		380	1,62		81	1,38	
Language Spoken at Home		German	16,680	62,6	0	15,860	67,56	0	3,991	68,13	0
		Foreign Language	9,964	37,4		7,615	32,44		1,867	31,87	
Household Composition		Both Parents	23,284	87,82	131	20,570	87,63	0	5,160	88,08	0
		Single Parent	3,229	12,18		2,905	12,37		698	11,92	
Canton		AG	13,601	51,05	0	12,060	51,37	0	3,019	51,54	0
		BL	5,298	19,88		4,734	20,17		1,157	19,75	
		BS	2,951	11,08		2,395	10,2		618	10,55	
		SO	4,794	17,99		4,286	18,26		1,064	18,16	
Variable			Mean	SD	Missing	Mean	SD	Missing	Mean	SD	Missing
Age at Enrolment			4,64	0,46	0	4,63	0,44	0	4,55	0,43	0
Mean Income Decile			5,51	2,87	2,495	5,54	2,86	0	5,61	2,84	0
Area per Person			31,85	14,06	154	32,45	14,16	0	32,61	13,86	0
WLE Reading			-0,01	1,28	618	0,06	1,26	436	0,12	1,24	87
WLE Writing			0,11	1,56	866	0,2	1,52	625	0,31	1,48	126
WLE Algebra			-0,76	1,4	568	-0,73	1,39	436	-0,64	1,37	87

Notes: The numbers reported for the sample used in the IV and the RD framework can slightly vary between models with respect to the dependent variable, as we did not exclude cases with missing information in the variables regarding performance in reading, writing and algebra. The rows that contain a value in the columns "Missing" refer to missing information in the corresponding variable, not the variable's level.

C3 Sample Description 6th Grade

Table C3: Sample Description 6th Grade

6th Grade	Checks total	Checks without duplicates and missing birthdates	Checks without invalid enrolment dates			IV Sample			RD Sample	
Observations	46,274	42,266	39,943			33,575			8,364	
Variable		N	%	Missing	N	%	Missing	N	%	Missing
Sex	Female	19,622	49,13	0	16,481	49,09	0	4,232	50,6	0
	Male	20,321	50,87		17,094	50,91		4,132	49,4	
Migration Status	Native	24,903	73,89	6,242	24,833	73,96	0	6,290	75,2	0
	Second Generation	8,239	24,45		8,206	24,44		1,966	23,51	
	First Generation	559	1,66		536	1,6		108	1,29	
Language Spoken at Home	German	25,909	64,86	0	24,142	71,9	0	6,154	73,58	0
	Foreign Language	14,034	35,14		9,433	28,1		2,210	26,42	
Household Composition	Both Parents	35,029	87,93	107	29,415	87,61	0	7,356	87,95	0
	Single Parent	4,807	12,07		4,160	12,39		1,008	12,05	
Canton	AG	21,955	54,97	0	18,571	55,31	0	4,519	54,03	0
	BL	6,940	17,37		6,008	17,89		1,505	17,99	
	BS	4,793	12		3,638	10,84		940	11,24	
	SO	6,255	15,66		5,358	15,96		1,400	16,74	
Variable		Mean	SD	Missing	Mean	SD	Missing	Mean	SD	Missing
Age at Enrolment		4,66	0,46	0	4,63	0,44	0	4,54	0,42	0
Mean Income Decile		5,5	2,87	5,072	5,54	2,85	0	5,73	2,84	0
Area per Person		31,76	14,06	156	32,71	14,16	0	33,24	14,5	0
WLE Reading		0,03	1,24	553	0,13	1,22	360	0,23	1,19	68
WLE Writing		-0,28	1,42	720	-0,18	1,39	466	-0,04	1,35	89
WLE Algebra		-0,38	1,41	552	-0,32	1,39	420	-0,19	1,32	86

Notes: The numbers reported for the sample used in the IV and the RD framework can slightly vary between models with respect to the dependent variable, as we did not exclude cases with missing information in the variables regarding performance in reading, writing and algebra. The rows that contain a value in the columns "Missing" refer to missing information in the corresponding variable, not the variable's level.

C4 Sample Description 8th Grade

Table C4: Sample Description 8th Grade

8th Grade	Checks total	Checks without duplicates and missing birthdates	Checks without invalid enrolment dates			IV Sample			RD Sample		
Observations	69,057	68,361	60,767			48,934			11,135		
Variable			N	%	Missing	N	%	Missing	N	%	Missing
Sex		Female	29,683	48,85	0	23,919	48,88	0	5,645	50,7	0
		Male	31,084	51,15		25,015	51,12		5,490	49,3	
Migration Status		Native	38,407	78,17	11,635	38,299	78,27	0	8,819	79,2	0
		Second Generation	9,744	19,83		9,690	19,8		2,141	19,23	
		First Generation	981	2		945	1,93		175	1,57	
Language Spoken at Home		German	43,267	71,2	0	39,379	80,47	0	9,103	81,75	0
		Foreign Language	17,500	28,8		9,555	19,53		2,032	18,25	
Household Composition		Both Parents	53,219	87,87	203	42,790	87,44	0	9,805	88,06	0
		Single Parent	7,345	12,13		6,144	12,56		1,330	11,94	
Canton		AG	36,558	60,16	0	29,674	60,64	0	6,542	58,75	0
		BL	5,177	8,52		4,425	9,04		1,034	9,29	
		BS	5,223	8,6		3,911	7,99		1,019	9,15	
		SO	13,809	22,72		10,924	22,32		2,540	22,81	
Variable			Mean	SD	Missing	Mean	SD	Missing	Mean	SD	Missing
Age at Enrolment			4,74	0,51	0	4,71	0,49	0	4,55	0,42	0
Mean Income Decile			5,52	2,86	10,147	5,55	2,85	0	5,72	2,83	0
Area per Person			33,31	14,7	275	34,7	14,81	0	35,1	14,92	0
WLE Reading			0,32	1,04	8,134	0,42	1,02	6,306	0,52	1	1,326
WLE Writing			0,41	1,26	912	0,54	1,2	548	0,65	1,16	115
WLE Algebra			-0,17	1,16	7,967	-0,09	1,13	6,268	0,01	1,07	1,317

Notes: The numbers reported for the sample used in the IV and the RD framework can slightly vary between models with respect to the dependent variable, as we did not exclude cases with missing information in the variables regarding performance in reading, writing and algebra. The rows that contain a value in the columns "Missing" refer to missing information in the corresponding variable, not the variable's level.

C5 Sample Description 9th Grade

Table C5: Sample Description 9th Grade

9th Grade	Checks total	Checks without duplicates and missing birthdates	Checks without invalid enrolment dates			IV Sample			RD Sample	
Observations	33,816	33,538	27,151			21,278			4,585	
Variable		N	%	Missing	N	%	Missing	N	%	Missing
Sex	Female	13,161	48,47	0	10,343	48,61	0	2,310	50,38	0
	Male	13,990	51,53		10,935	51,39		2,275	49,62	
Migration Status	Native	17,431	81,61	5,791	17,383	81,69	0	3,849	83,95	0
	Second Generation	3,503	16,4		3,488	16,39		661	14,42	
	First Generation	426	1,99		407	1,91		75	1,64	
Language Spoken at Home	German	19,615	72,27	11	17,849	83,88	0	3,941	85,95	0
	Foreign Language	7,525	27,73		3,429	16,12		644	14,05	
Household Composition	Both Parents	24,198	89,47	106	18,958	89,1	0	4,138	90,25	0
	Single Parent	2,847	10,53		2,320	10,9		447	9,75	
Canton	AG	19,972	73,56	0	15,956	74,99	0	3,450	75,25	0
	SO	7,179	26,44		5,322	25,01		1,135	24,75	
Variable		Mean	SD	Missing	Mean	SD	Missing	Mean	SD	Missing
Age at Enrolment		4,8	0,52	0	4,77	0,51	0	4,57	0,41	0
Mean Income Decile		5,52	2,84	5,234	5,55	2,83	0	5,81	2,81	0
Area per Person		34,23	14,96	123	36	15,09	0	36,72	15,16	0
WLE Reading		0,52	1,12	488	0,65	1,1	322	0,78	1,07	69
WLE Writing		0,55	1,26	332	0,7	1,19	218	0,87	1,14	41
WLE Algebra		0,13	1,2	379	0,24	1,17	258	0,41	1,12	49

Notes: The numbers reported for the sample used in the IV and the RD framework can slightly vary between models with respect to the dependent variable, as we did not exclude cases with missing information in the variables regarding performance in reading, writing and algebra. The rows that contain a value in the columns "Missing" refer to missing information in the corresponding variable, not the variable's level.

Appendix D

D1 Full RD Models

Table D1: Full RD Models - Reading

Dependent Variable: Test Scores in Reading

	3rd Grade	5thGrade	6th Grade	8th Grade	9th Grade
Intercept	-0.401 ***	-0.331 ***	-0.098	0.039	0.221 **
	(0.050)	(0.064)	(0.055)	(0.048)	(0.073)
Born after Cut-Off Date	0.312 ***	0.142 **	0.055	-0.053	-0.107
	(0.035)	(0.047)	(0.040)	(0.037)	(0.056)
Days around Cut-Off	-0.001	-0.002	0.001	0.002 *	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sex – ref. = Female					
Male	-0.092 ***	-0.055 *	-0.156 ***	-0.182 ***	-0.234 ***
	(0.017)	(0.023)	(0.020)	(0.018)	(0.027)
Migration Background - ref. = Native					
Second Generation	-0.150 *	-0.012	0.011	0.000	0.011
	(0.060)	(0.071)	(0.064)	(0.053)	(0.079)
First Generation	0.026	0.115 *	0.117 **	0.120 ***	0.154 **
	(0.037)	(0.045)	(0.041)	(0.035)	(0.055)
Language spoken at home – ref. = German					
Foreign Language	-0.462 ***	-0.488 ***	-0.413 ***	-0.394 ***	-0.282 ***
	(0.025)	(0.034)	(0.030)	(0.030)	(0.051)
Mean Income Decile	0.060 ***	0.070 ***	0.059 ***	0.059 ***	0.043 ***
	(0.003)	(0.005)	(0.004)	(0.004)	(0.005)
Household Composition: ref. = Both Parents					
Single Parent	-0.106 ***	-0.122 ***	-0.119 ***	-0.121 ***	-0.075
	(0.028)	(0.037)	(0.031)	(0.029)	(0.048)
Area per Capita	0.004 ***	0.004 ***	0.003 ***	0.002 ***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.057 **	-0.091 **	0.030	-0.081 **	
	(0.022)	(0.031)	(0.027)	(0.031)	
Basel-Stadt	-0.010	-0.050	-0.004	-0.033	
	(0.027)	(0.040)	(0.033)	(0.032)	
Solothurn	-0.130 ***	-0.024	-0.003	-0.073 **	-0.267 ***
	(0.023)	(0.031)	(0.028)	(0.023)	(0.032)
Born after Cut-Off Date × Days around Cut-Off	0.001	0.002	-0.002	-0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Observations	11,569	5,771	8,296	9,809	4,516
R^2 Adj.	0.168	0.191	0.132	0.116	0.081
AIC	30,603.5	14,802.8	21,689.1	25,763.2	12,079.5

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. “Born after Cut-Off Date” refers to the treatment variable, whereas “Days around Cut-Off” is the running variable.

Table D2: Full RD Models - Writing

Dependent Variable: Test Scores in Writing

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-0.165 **	-0.091	0.044	0.060	0.333 ***
	(0.053)	(0.064)	(0.053)	(0.043)	(0.066)
Born after Cut-Off Date	0.203 ***	0.074	0.116 **	0.078 *	-0.049
	(0.037)	(0.047)	(0.038)	(0.033)	(0.050)
Days around Cut-Off	0.000	0.000	0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sex – ref. = Female					
Male	-0.234 ***	-0.449 ***	-0.408 ***	-0.387 ***	-0.417 ***
	(0.018)	(0.023)	(0.019)	(0.016)	(0.025)
Migration Background - ref. = Native					
Second Generation	-0.047	-0.073	-0.028	0.005	0.169 *
	(0.065)	(0.071)	(0.061)	(0.047)	(0.071)
First Generation	0.040	0.024	0.063	0.069 *	0.200 ***
	(0.040)	(0.045)	(0.040)	(0.032)	(0.050)
Language spoken at home – ref. = German					
Foreign Language	-0.366 ***	-0.306 ***	-0.363 ***	-0.363 ***	-0.260 ***
	(0.026)	(0.034)	(0.029)	(0.027)	(0.046)
Mean Income Decile	0.054 ***	0.062 ***	0.052 ***	0.054 ***	0.045 ***
	(0.004)	(0.005)	(0.004)	(0.003)	(0.005)
Household Composition: ref. = Both Parents					
Single Parents	-0.125 ***	-0.107 **	-0.155 ***	-0.095 ***	-0.143 ***
	(0.031)	(0.037)	(0.030)	(0.026)	(0.043)
Area per Capita	0.004 ***	0.004 ***	0.004 ***	0.003 ***	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.118 ***	-0.046	-0.019	-0.008	
	(0.023)	(0.031)	(0.026)	(0.029)	
Basel-Stadt	-0.215 ***	-0.110 **	-0.119 ***	-0.077 **	
	(0.029)	(0.040)	(0.032)	(0.030)	
Solothurn	-0.160 ***	0.003	-0.059 *	-0.037	-0.226 ***
	(0.024)	(0.031)	(0.027)	(0.020)	(0.029)
Born after Cut-Off Date × Days around Cut-Off	0.000	0.001	-0.002	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	10,246	5,732	8,275	11,020	4,544
R2 Adj.	0.138	0.169	0.155	0.134	0.120
AIC	27,123.1	14,624.2	21,015.5	27,635.0	11,267.1

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. “Born after Cut-Off Date” refers to the treatment variable, whereas “Days around Cut-Off” is the running variable.

Table D3: Full RD Models - Algebra

Dependent Variable: Test Scores in Algebra

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-0.370 ***	-0.352 ***	-0.246 ***	-0.097 *	-0.016
	(0.052)	(0.067)	(0.054)	(0.047)	(0.071)
Born after Cut-Off Date	0.228 ***	0.108 *	0.066	-0.089 *	-0.124 *
	(0.036)	(0.050)	(0.039)	(0.036)	(0.054)
Days around Cut-Off	0.000	-0.001	0.000	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sex – ref. = Female					
Male	0.249 ***	0.163 ***	0.188 ***	0.035 *	0.060 *
	(0.017)	(0.024)	(0.019)	(0.018)	(0.027)
Migration Background - ref. = Native					
Second Generation	0.081	0.178 *	0.046	0.067	0.105
	(0.062)	(0.075)	(0.064)	(0.051)	(0.076)
First Generation	0.034	0.116 *	0.050	0.106 **	0.153 **
	(0.038)	(0.047)	(0.041)	(0.034)	(0.054)
Language spoken at home – ref. = German					
Foreign Language	-0.192 ***	-0.189 ***	-0.181 ***	-0.231 ***	-0.186 ***
	(0.025)	(0.035)	(0.030)	(0.029)	(0.050)
Mean Income Decile	0.061 ***	0.074 ***	0.051 ***	0.065 ***	0.053 ***
	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
Household Composition: ref. = Both Parents					
Single Parents	-0.172 ***	-0.152 ***	-0.135 ***	-0.167 ***	-0.137 **
	(0.029)	(0.039)	(0.031)	(0.028)	(0.047)
Area per Capita	0.003 ***	0.004 ***	0.004 ***	0.003 ***	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.231 ***	-0.221 ***	-0.168 ***	-0.124 ***	
	(0.023)	(0.032)	(0.027)	(0.030)	
Basel-Stadt	-0.284 ***	-0.404 ***	-0.333 ***	-0.248 ***	
	(0.028)	(0.042)	(0.033)	(0.031)	
Solothurn	-0.187 ***	-0.064	-0.005	-0.078 ***	-0.167 ***
	(0.023)	(0.033)	(0.027)	(0.022)	(0.031)
Born after Cut-Off Date × Days around Cut-Off	0.000	0.000	-0.001	-0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Observations	11,580	5,771	8,278	9,818	4,536
R2 Adj.	0.105	0.121	0.095	0.101	0.065
AIC	31,324.6	15,320.8	21,478.7	25,245.8	11,890.4

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable, whereas "Days around Cut-Off" is the running variable.

D2 Non-Parametric RD Models

Table D4: Non-Parametric RD Models - Reading

Dependent Variable: Test Scores in Reading

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Conventional	0.296 ***	0.150 ***	0.056	-0.053	-0.096 *
	(0.032)	(0.042)	(0.030)	(0.037)	(0.039)
Bias-Corrected	0.311 ***	0.165 ***	0.058	-0.071	-0.109 **
	(0.032)	(0.042)	(0.030)	(0.037)	(0.039)
Robust	0.311 ***	0.165 **	0.058	-0.071	-0.109 *
	(0.039)	(0.053)	(0.040)	(0.045)	(0.050)
Observations	362,833	18,339	26,864	31,981	14,677

Notes: * p <0.05, ** p <0.01; *** p <0.001. Conventional and bias-corrected local polynomial regression discontinuity point estimates with standard errors in parentheses. "Robust" displays bias-corrected point estimates with robust standard errors in parentheses. Bandwidths around the cut-off were determined for each model separately using a mean squared error-optimal bandwidth selector.

Table D5: Non-Parametric RD Models - Writing

Dependent Variable: Test Scores in Writing

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Conventional	0.194 ***	0.058	0.138 ***	0.076 *	-0.059
	(0.027)	(0.036)	(0.037)	(0.032)	(0.034)
Bias-Corrected	0.206 ***	0.053	0.153 ***	0.090 **	-0.068 *
	(0.027)	(0.036)	(0.037)	(0.032)	(0.034)
Robust	0.206 ***	0.053	0.153 ***	0.090 *	-0.068
	(0.034)	(0.048)	(0.045)	(0.039)	(0.044)
Observations	32,428	18,190	26,790	35,908	14,733

Notes: * p <0.05, ** p <0.01; *** p <0.001. Conventional and bias-corrected local polynomial regression discontinuity point estimates with standard errors in parentheses. "Robust" displays bias-corrected point estimates with robust standard errors in parentheses. Bandwidths around the cut-off were determined for each model separately using a mean squared error-optimal bandwidth selector.

Table D6: Non-Parametric RD Models - Algebra

Dependent Variable: Test Scores in Algebra

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Conventional	0.213 ***	0.097 *	0.054	-0.070 *	-0.080
	(0.029)	(0.040)	(0.032)	(0.028)	(0.052)
Bias-Corrected	0.220 ***	0.119 **	0.055	-0.083 **	-0.062
	(0.029)	(0.040)	(0.032)	(0.028)	(0.052)
Robust	0.220 ***	0.119 *	0.055	-0.083 *	-0.062
	(0.037)	(0.048)	(0.042)	(0.035)	(0.065)
Observations	36,852	18,333	26,823	32,021	14,728

Notes: * p <0.05, ** p <0.01; *** p <0.001. Conventional and bias-corrected local polynomial regression discontinuity point estimates with standard errors in parentheses. "Robust" displays bias-corrected point estimates with robust standard errors in parentheses. Bandwidths around the cut-off were determined for each model separately using a mean squared error-optimal bandwidth selector.

D3 Robustness to Different Bandwidths

Table D7: RD Models with Different Bandwidths - Reading

Dependent Variable: Test Scores in Reading

	3rd Grade: Reading		5th Grade: Reading		6th Grade: Reading		8th Grade: Reading		9th Grade: Reading	
	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days
Born after Cut-Off Date	0.304 ***	0.317 ***	0.226 *	0.180 **	0.064	0.084	-0.020	-0.078	0.008	-0.044
	(0.071)	(0.050)	(0.090)	(0.065)	(0.083)	(0.058)	(0.073)	(0.052)	(0.098)	(0.067)
Observations	2,638	5,408	1,326	2,718	2,012	4,018	2,309	4,574	1,151	2,184
R2	0.198	0.176	0.241	0.212	0.116	0.134	0.184	0.180	0.327	0.294

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Controls not shown.

Table D8: RD Models with Different Bandwidths - Writing

Dependent Variable: Test Scores in Writing

	3rd Grade: Writing		5th Grade: Writing		6th Grade: Writing		8th Grade: Writing		9th Grade: Writing	
	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days
Born after Cut-Off Date	0.139	0.170 **	0.006	-0.037	0.226 **	0.179 **	0.138 *	0.102 *	-0.046	-0.098
	(0.078)	(0.054)	(0.094)	(0.066)	(0.077)	(0.055)	(0.064)	(0.046)	(0.087)	(0.060)
Observations	2,322	4,766	1,314	2,696	2,008	4,007	2,318	4,583	1,158	2,196
R2	0.147	0.144	0.193	0.188	0.133	0.151	0.211	0.204	0.374	0.339

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Controls not shown.

Table D9: RD Models with Different Bandwidths - Algebra

Dependent Variable: Test Scores in Algebra

	3rd Grade: Algebra		5th Grade: Algebra		6th Grade: Algebra		8th Grade: Algebra		9th Grade: Algebra	
	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days	+/- 15 Days	+/-30 Days
Born after Cut-Off Date	0.130	0.178 ***	0.201 *	0.088	0.033	0.045	-0.032	-0.082	0.088	0.023
	(0.071)	(0.051)	(0.099)	(0.069)	(0.076)	(0.055)	(0.067)	(0.048)	(0.092)	(0.064)
Observations	2,643	5,416	1,323	2,716	2,009	4,008	2,315	4,577	1,155	2,184
R2	0.116	0.105	0.150	0.137	0.088	0.095	0.182	0.171	0.367	0.391

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Controls not shown.

D4 Subgroup Analyses for RDD

Table D10: Subgroup Analyses in the RDD Framework - Reading

Dependent Variable: Test Scores in Reading

Subsamples:		Females	Males	German	Foreign Language	Lower Income	Upper Income
3rd Grade	Born after Cut-Off Date	0.359 ***	0.261 ***	0.312 ***	0.307 ***	0.303 ***	0.310 ***
		(0.049)	(0.050)	(0.044)	(0.056)	(0.049)	(0.050)
	Observations	5,784	5,785	8,033	3,536	5,614	5,955
	R^2	0.186	0.152	0.063	0.103	0.149	0.073
5th Grade	Born after Cut-Off Date	0.138 *	0.147 *	0.116 *	0.192 *	0.150 *	0.118
		(0.065)	(0.066)	(0.058)	(0.075)	(0.064)	(0.067)
	Observations	2,924	2,847	3,931	1,840	2,800	2,971
	R^2	0.197	0.190	0.061	0.110	0.157	0.069
6th Grade	Born after Cut-Off Date	-0.019	0.136 *	0.066	0.029	0.070	0.035
		(0.057)	(0.057)	(0.048)	(0.071)	(0.056)	(0.057)
	Observations	4,197	4,099	6,113	2,183	3,831	4,465
	R^2	0.129	0.131	0.048	0.080	0.109	0.053
8th Grade	Born after Cut-Off Date	-0.067	-0.027	-0.023	-0.084	-0.019	-0.062
		(0.049)	(0.053)	(0.041)	(0.072)	(0.053)	(0.049)
	Observations	4,808	4,668	7,660	1,816	4,380	5,096
	R^2	0.187	0.169	0.133	0.149	0.173	0.132
9th Grade	Born after Cut-Off Date	-0.104	-0.051	-0.101	0.043	-0.189 **	-0.006
		(0.062)	(0.075)	(0.053)	(0.113)	(0.070)	(0.067)
	Observations	2,267	2,219	3,859	627	2,033	2,453
	R^2	0.339	0.270	0.281	0.388	0.348	0.245

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Controls not shown.

Table D11: Subgroup Analyses in the RDD Framework - Writing

Dependent Variable: Test Scores in Writing

Soubsamples:		Females	Males	German	Foreign language	Lower Income	Upper Income
3rd Grade	Born after Cut-Off Date	0.235 ***	0.167 **	0.196 ***	0.209 **	0.168 **	0.223 ***
		(0.054)	(0.052)	(0.046)	(0.064)	(0.056)	(0.050)
	Observations	5,119	5,127	7,098	3,148	4,945	5,301
	R^2	0.139	0.117	0.064	0.100	0.113	0.066
5th Grade	Born after Cut-Off Date	0.099	0.053	0.060	0.106	0.029	0.096
		(0.065)	(0.069)	(0.058)	(0.082)	(0.070)	(0.064)
	Observations	2,914	2,818	3,913	1,819	2,772	2,960
	R^2	0.141	0.107	0.118	0.112	0.121	0.111
6th Grade	Born after Cut-Off Date	0.048	0.184 ***	0.098 *	0.171 *	0.153 **	0.077
		(0.056)	(0.055)	(0.046)	(0.075)	(0.057)	(0.053)
	Observations	4,192	4,083	6,096	2,179	3,815	4,460
	R^2	0.118	0.120	0.090	0.110	0.140	0.086
8th Grade	Born after Cut-Off Date	0.029	0.153 **	0.109 **	0.048	0.079	0.094 *
		(0.047)	(0.047)	(0.037)	(0.075)	(0.049)	(0.044)
	Observations	4,803	4,694	7,668	1,829	4,404	5,093
	R^2	0.158	0.183	0.154	0.176	0.201	0.162
9th Grade	Born after Cut-Off Date	-0.001	-0.036	-0.038	0.102	0.007	-0.045
		(0.061)	(0.062)	(0.046)	(0.127)	(0.064)	(0.058)
	Observations	2,285	2,240	3,893	632	2,056	2,469
	R^2	0.312	0.329	0.334	0.378	0.383	0.290

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Controls not shown.

Table D12: Subgroup Analyses in the RDD Framework - Algebra

Dependent Variable: Test Scores in Algebra

Subsamples:		Females	Males	German	Foreign Language	Lower Income	Upper Income
3rd Grade	Born after Cut-Off Date	0.248 ***	0.211 ***	0.241 ***	0.190 **	0.269 ***	0.178 ***
		(0.049)	(0.053)	(0.044)	(0.065)	(0.055)	(0.048)
	Observations	5,794	5,786	8,038	3,542	5,613	5,967
	R^2	0.105	0.079	0.076	0.106	0.075	0.053
5th Grade	Born after Cut-Off Date	0.142 *	0.073	0.045	0.232 **	0.206 **	-0.010
		(0.070)	(0.068)	(0.059)	(0.086)	(0.072)	(0.066)
	Observations	2,922	2,849	3,936	1,835	2,794	2,977
	R^2	0.108	0.130	0.086	0.114	0.077	0.044
6th Grade	Born after Cut-Off Date	0.021	0.114 *	0.053	0.115	0.087	0.039
		(0.058)	(0.054)	(0.045)	(0.080)	(0.060)	(0.053)
	Observations	4,188	4,090	6,096	2,182	3,829	4,449
	R^2	0.098	0.073	0.063	0.092	0.077	0.038
8th Gradae	Born after Cut-Off Date	-0.115 *	-0.068	-0.094 *	-0.002	-0.065	-0.113 *
		(0.047)	(0.051)	(0.038)	(0.083)	(0.052)	(0.046)
	Observations	4,805	4,683	7,666	1,822	4,398	5,090
	R^2	0.177	0.165	0.149	0.129	0.138	0.122
9th Grade	Born after Cut-Off Date	-0.198 ***	0.044	-0.096 *	-0.012	-0.128	-0.062
		(0.057)	(0.069)	(0.049)	(0.113)	(0.066)	(0.061)
	Observations	2,270	2,227	3,871	626	2,040	2,457
	R^2	0.424	0.391	0.383	0.479	0.406	0.368

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. “Born after Cut-Off Date” refers to the treatment variable. Controls not shown.

D5 RD Models using Matching Data

Table D13: RD Models using Matching Data - Reading

Dependent Variable: Test Scores in Reading

Grade:	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-0.382 ***	-0.408 ***	-0.080	0.150	0.261 *
	(0.070)	(0.110)	(0.080)	(0.092)	(0.122)
Born after Cut-Off Date	0.289 ***	0.153 **	0.060	-0.037	-0.118
	(0.037)	(0.054)	(0.044)	(0.044)	(0.063)
Days around Cut-Off	-0.001	-0.002 *	0.001	0.002 *	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sex – ref. = Female					
Male	-0.086 ***	-0.078 **	-0.153 ***	-0.185 ***	-0.251 ***
	(0.018)	(0.026)	(0.022)	(0.022)	(0.032)
Migration Background - ref. = Native					
Second Generation	-0.130	-0.197	0.045	0.117	-0.076
	(0.113)	(0.189)	(0.124)	(0.157)	(0.197)
First Generation	0.020	0.035	0.109	0.163	0.074
	(0.067)	(0.112)	(0.074)	(0.093)	(0.130)
Language spoken at home – ref. = German					
Foreign Language	-0.482 ***	-0.476 ***	-0.438 ***	-0.480 ***	-0.372 ***
	(0.030)	(0.050)	(0.042)	(0.054)	(0.100)
Mean Income Decile	0.064 ***	0.072 ***	0.066 ***	0.058 ***	0.040 ***
	(0.004)	(0.006)	(0.005)	(0.005)	(0.006)
Household Composition: ref. = Both Parents					
Single Parent	-0.115 **	-0.137 *	-0.067	-0.105 *	-0.086
	(0.037)	(0.057)	(0.043)	(0.049)	(0.078)
Area per Capita	0.003 ***	0.003 *	0.002 *	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.061 **	-0.103 **	0.040	-0.052	
	(0.023)	(0.035)	(0.030)	(0.042)	
Basel-Stadt	-0.014	-0.057	-0.020	-0.046	
	(0.030)	(0.052)	(0.039)	(0.043)	
Solothurn	-0.126 ***	-0.034	-0.014	-0.066 *	-0.277 ***
	(0.024)	(0.036)	(0.031)	(0.028)	(0.038)
Born after Cut-Off Date × Days around Cut-Off	0.002	0.003 *	-0.002	-0.002	0.002
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Observations	10,127	4,447	6,849	6,766	3,364
R2 Adj.	0.173	0.200	0.135	0.112	0.075
AIC	26,767.0	11,426.4	17,896.3	17,663.3	8,931.7
F	163.800	86.670	82.907	66.833	25.736

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Samples were matched on the treatment variable using coarsened exact matching.

Table D14: RD Models using Matching Data - Writing

Dependent Variable: Test Scores in Writing

Grade:	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-0.161 *	-0.129	0.038	0.168 *	0.492 ***
	(0.073)	(0.109)	(0.076)	(0.085)	(0.110)
Born after Cut-Off Date	0.191 ***	0.095	0.112 **	0.091 *	-0.043
	(0.040)	(0.053)	(0.042)	(0.040)	(0.057)
Days around Cut-Off	-0.001	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sex – ref. = Female					
Male	-0.229 ***	-0.480 ***	-0.416 ***	-0.409 ***	-0.449 ***
	(0.019)	(0.026)	(0.021)	(0.020)	(0.028)
Migration Background - ref. = Native					
Second Generation	-0.016	-0.267	-0.016	0.032	0.356 *
	(0.118)	(0.186)	(0.117)	(0.145)	(0.178)
First Generation	0.063	-0.037	0.064	0.032	0.225
	(0.070)	(0.110)	(0.070)	(0.086)	(0.117)
Language spoken at home – ref. = German					
Foreign Language	-0.360 ***	-0.269 ***	-0.361 ***	-0.473 ***	-0.363 ***
	(0.032)	(0.050)	(0.040)	(0.050)	(0.089)
Mean Income Decile	0.055 ***	0.060 ***	0.054 ***	0.053 ***	0.042 ***
	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)
Household Composition: ref. = Both Parents					
Single Parent	-0.103 *	-0.130 *	-0.184 ***	-0.158 ***	-0.266 ***
	(0.040)	(0.057)	(0.041)	(0.046)	(0.069)
Area per Capita	0.004 ***	0.004 **	0.004 ***	0.002 *	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.117 ***	-0.055	-0.028	-0.018	
	(0.025)	(0.035)	(0.029)	(0.039)	
Basel-Stadt	-0.210 ***	-0.167 **	-0.139 ***	-0.097 *	
	(0.032)	(0.052)	(0.038)	(0.039)	
Solothurn	-0.161 ***	0.016	-0.077 **	-0.070 **	-0.225 ***
	(0.025)	(0.036)	(0.029)	(0.026)	(0.034)
Born after Cut-Off Date × Days around Cut-Off	0.000	-0.001	-0.001	-0.002	0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Observations	8,982	4,419	6,832	6,773	3,392
R2 Adj.	0.139	0.180	0.158	0.147	0.122
AIC	23,694.0	11,227.6	17,219.9	16,594.3	8,294.7
F	112.892	75.413	99.683	90.731	43.928

Notes: Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Samples were matched on the treatment variable using coarsened exact matching.

Table D15: RD Models using Matching Data - Algebra

Dependent Variable: Test Scores in Algebra

Grade:	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-0.353 ***	-0.543 ***	-0.292 ***	-0.005	0.028
	(0.072)	(0.115)	(0.078)	(0.089)	(0.116)
Born after Cut-Off Date	0.201 ***	0.135 *	0.048	-0.103 *	-0.111
	(0.038)	(0.056)	(0.043)	(0.042)	(0.060)
Days around Cut-Off	0.000	-0.001	0.000	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sex – ref. = Female					
Male	0.248 ***	0.138 ***	0.178 ***	0.032	0.037
	(0.019)	(0.027)	(0.021)	(0.021)	(0.030)
Migration Background - ref. = Native					
Second Generation	0.120	-0.242	-0.021	0.137	0.075
	(0.116)	(0.198)	(0.120)	(0.151)	(0.187)
First Generation	0.036	-0.108	0.025	0.123	0.074
	(0.069)	(0.117)	(0.072)	(0.090)	(0.123)
Language spoken at home – ref. = German					
Foreign Language	-0.213 ***	-0.178 ***	-0.158 ***	-0.347 ***	-0.262 **
	(0.031)	(0.053)	(0.041)	(0.052)	(0.095)
Mean Income Decile	0.064 ***	0.074 ***	0.055 ***	0.059 ***	0.051 ***
	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)
Household Composition: ref. = Both Parents					
Single Parent	-0.186 ***	-0.113	-0.131 **	-0.121 *	-0.161 *
	(0.038)	(0.060)	(0.042)	(0.048)	(0.074)
Area per Capita	0.003 **	0.004 **	0.004 ***	0.003 **	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.219 ***	-0.238 ***	-0.166 ***	-0.113 **	
	(0.024)	(0.037)	(0.030)	(0.040)	
Basel-Stadt	-0.284 ***	-0.457 ***	-0.339 ***	-0.235 ***	
	(0.030)	(0.055)	(0.038)	(0.041)	
Solothurn	-0.177 ***	-0.048	-0.012	-0.068 *	-0.155 ***
	(0.025)	(0.038)	(0.030)	(0.027)	(0.036)
Born after Cut-Off Date × Days around Cut-Off	0.001	0.000	-0.001	-0.001	0.003
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Observations	10,135	4,446	6,835	6,774	3,375
R2 Adj.	0.108	0.121	0.096	0.099	0.054
AIC	27,351.5	11,808.7	17,566.8	17,223.5	8,612.3
F	95.426	47.982	57.094	58.177	18.511

Notes: Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS coefficients with robust standard errors in parentheses. "Born after Cut-Off Date" refers to the treatment variable. Controls not shown. Samples were matched on the treatment variable using coarsened exact matching.

Appendix E

E1 Full IV Models

Table E1: Full IV Models - Reading

Dependent Variable: Test Scores in Reading					
Grade:	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-2.376 ***	-1.384 ***	-1.516 ***	-1.181 ***	-0.452 **
	(0.109)	(0.150)	(0.139)	(0.121)	(0.175)
Age at Enrolment (in years)	0.458 ***	0.247 ***	0.268 ***	0.201 ***	0.082 *
	(0.023)	(0.031)	(0.029)	(0.025)	(0.036)
Sex – ref. = Female					
Male	-0.135 ***	-0.101 ***	-0.186 ***	-0.193 ***	-0.256 ***
	(0.009)	(0.012)	(0.010)	(0.009)	(0.013)
Migration Background - ref. = Native					
Second Generation	-0.068 *	0.039	-0.014	-0.004	-0.003
	(0.030)	(0.035)	(0.030)	(0.024)	(0.035)
First Generation	0.056 **	0.125 ***	0.101 ***	0.092 ***	0.077 **
	(0.019)	(0.022)	(0.019)	(0.016)	(0.024)
Language spoken at home – ref. = German					
Foreign Language	-0.493 ***	-0.532 ***	-0.420 ***	-0.386 ***	-0.366 ***
	(0.013)	(0.018)	(0.016)	(0.015)	(0.023)
Mean Income Decile	0.071 ***	0.079 ***	0.078 ***	0.077 ***	0.060 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Household Composition: ref. = Both Parents					
Single Parent	-0.148 ***	-0.135 ***	-0.143 ***	-0.125 ***	-0.099 ***
	(0.015)	(0.019)	(0.016)	(0.014)	(0.022)
Area per Capita	0.005 ***	0.004 ***	0.004 ***	0.004 ***	0.003 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Canton – ref. = Aargau					
Basel-Land	-0.062 ***	-0.067 ***	-0.008	-0.088 ***	
	(0.011)	(0.016)	(0.014)	(0.015)	
Basel-Stadt	0.071 ***	0.034	0.026	0.002	
	(0.014)	(0.021)	(0.018)	(0.017)	
Solothurn	-0.162 ***	-0.074 ***	0.031 *	-0.050 ***	-0.251 ***
	(0.012)	(0.016)	(0.014)	(0.011)	(0.015)
Observations	45,110	23,039	33,215	42,628	20,956
R2 Adjusted	0.124	0.164	0.112	0.094	0.092
AIC	121,879.9	60,713.6	89,038.4	114,808.8	56,577.3

Notes: * p <0.05, ** p <0.01; *** p <0.001. Reduced form estimates from 2SLS regressions with robust errors in parentheses. The models report the results from IV regression where assigned age was used as an instrument for the observed age.

Table E2: Full IV Models - Writing

Dependent Variable: Test Scores in Writing					
Grade:	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-1.602 ***	-0.934 ***	-1.614 ***	-1.129 ***	-0.587 ***
	(0.115)	(0.148)	(0.137)	(0.110)	(0.166)
Age at Enrolment (in years)	0.313 ***	0.187 ***	0.324 ***	0.213 ***	0.130 ***
	(0.024)	(0.031)	(0.028)	(0.022)	(0.034)
Sex – ref. = Female					
Male	-0.284 ***	-0.490 ***	-0.444 ***	-0.406 ***	-0.420 ***
	(0.009)	(0.012)	(0.010)	(0.008)	(0.012)
Migration Background - ref. = Native					
Second Generation	-0.082 *	-0.062	-0.030	-0.040	-0.017
	(0.033)	(0.034)	(0.029)	(0.022)	(0.033)
First Generation	0.026	0.036	0.064 ***	0.042 **	0.060 **
	(0.020)	(0.022)	(0.019)	(0.015)	(0.023)
Language spoken at home – ref. = German					
Foreign Language	-0.380 ***	-0.366 ***	-0.376 ***	-0.346 ***	-0.321 ***
	(0.014)	(0.017)	(0.015)	(0.014)	(0.022)
Mean Income Decile	0.064 ***	0.070 ***	0.072 ***	0.072 ***	0.058 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Household Composition: ref. = Both Parents					
Single Parent	-0.159 ***	-0.136 ***	-0.155 ***	-0.129 ***	-0.140 ***
	(0.016)	(0.018)	(0.016)	(0.013)	(0.020)
Area per Capita	0.004 ***	0.004 ***	0.004 ***	0.004 ***	0.003 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Canton – ref. = Aargau					
Basel-Land	-0.095 ***	-0.034 *	-0.028 *	-0.006	
	(0.012)	(0.016)	(0.014)	(0.015)	
Basel-Stadt	-0.109 ***	-0.085 ***	-0.055 **	-0.017	
	(0.015)	(0.021)	(0.017)	(0.016)	
Solothurn	-0.156 ***	-0.062 ***	-0.021	-0.048 ***	-0.228 ***
	(0.012)	(0.016)	(0.014)	(0.010)	(0.014)
Observations	39,729	22,850	33,109	48,386	21,060
R2 Adjusted	0.116	0.168	0.119	0.106	0.105
AIC	106,912.5	59,257.2	87,506.1	126,510.1	54,542.0

Notes: * p <0.05, ** p <0.01; *** p <0.001. Reduced form estimates from 2SLS regressions with robust errors in parentheses. The models report the results from IV regression where assigned age was used as an instrument for the observed age.

Table E3: Full IV Models - Algebra

Dependent Variable: Test Scores in Algebra

Grade:	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-2.147 ***	-1.203 ***	-1.579 ***	-1.025 ***	-0.694 ***
	(0.114)	(0.157)	(0.143)	(0.121)	(0.178)
Age at Enrolment (in years)	0.381 ***	0.180 ***	0.252 ***	0.141 ***	0.086 *
	(0.024)	(0.033)	(0.030)	(0.025)	(0.036)
Sex – ref. = Female					
Male	0.203 ***	0.121 ***	0.134 ***	0.028 **	0.037 **
	(0.009)	(0.013)	(0.011)	(0.009)	(0.013)
Migration Background - ref. = Native					
Second Generation	0.039	0.117 **	0.025	0.069 **	0.048
	(0.032)	(0.036)	(0.030)	(0.023)	(0.035)
First Generation	0.013	0.091 ***	0.027	0.061 ***	0.073 **
	(0.020)	(0.023)	(0.020)	(0.016)	(0.025)
Language spoken at home – ref. = German					
Foreign Language	-0.216 ***	-0.238 ***	-0.209 ***	-0.245 ***	-0.251 ***
	(0.013)	(0.018)	(0.016)	(0.015)	(0.024)
Mean Income Decile	0.075 ***	0.082 ***	0.074 ***	0.084 ***	0.070 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Household Composition: ref. = Both Parents					
Single Parent	-0.211 ***	-0.169 ***	-0.206 ***	-0.204 ***	-0.214 ***
	(0.015)	(0.020)	(0.016)	(0.014)	(0.022)
Area per Capita	0.003 ***	0.003 ***	0.004 ***	0.003 ***	0.003 ***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Canton – ref. = Aargau					
Basel-Land	-0.229 ***	-0.184 ***	-0.151 ***	-0.134 ***	
	(0.012)	(0.017)	(0.014)	(0.015)	
Basel-Stadt	-0.174 ***	-0.264 ***	-0.265 ***	-0.162 ***	
	(0.015)	(0.022)	(0.018)	(0.017)	
Solothurn	-0.197 ***	-0.072 ***	0.045 **	-0.067 ***	-0.218 ***
	(0.012)	(0.017)	(0.015)	(0.011)	(0.015)
Observations	45,131	23,039	33,155	42,666	21,020
R2 Adjusted	0.061	0.090	0.057	0.079	0.067
AIC	125,806.4	62,794.4	90,762.9	115,058.6	57,097.5

Notes: * p <0.05, ** p <0.01; *** p <0.001. Reduced form estimates from 2SLS regressions with robust errors in parentheses. The models report the results from IV regression where assigned age was used as an instrument for the observed age.

E2 Subgroup Analyses with IV Approach

Table E4: Subgroup Analyses with IV Approach - Reading

Dependent Variable: Test Scores in Reading

Subsample:		Females	Males	German	Foreign Language	Lower Income	Upper Income
3rd Grade	Age at Enrolment (in years)	0.461 ***	0.453 ***	0.523 ***	0.318 ***	0.401 ***	0.513 ***
		(0.031)	(0.033)	(0.028)	(0.037)	(0.032)	(0.032)
	Observations	22,103	23,007	30,932	14,178	22,494	22,616
	R^2	0.141	0.105	0.009	0.052	0.083	0.024
5th Grade	Age at Enrolment (in years)	0.260 ***	0.232 ***	0.303 ***	0.134 **	0.190 ***	0.286 ***
		(0.042)	(0.045)	(0.039)	(0.050)	(0.042)	(0.045)
	Observations	11,336	11,703	15,579	7,460	11,330	11,709
	R^2	0.170	0.158	0.018	0.083	0.124	0.029
6th Grade	Age at Enrolment (in years)	0.250 ***	0.287 ***	0.282 ***	0.236 ***	0.213 ***	0.318 ***
		(0.039)	(0.042)	(0.035)	(0.050)	(0.040)	(0.042)
	Observations	16,324	16,891	23,917	9,298	16,335	16,880
	R^2	0.123	0.090	0.016	0.048	0.071	0.005
8th Grade	Age at Enrolment (in years)	0.170 ***	0.232 ***	0.215 ***	0.149 **	0.204 ***	0.195 ***
		(0.033)	(0.037)	(0.028)	(0.048)	(0.035)	(0.035)
	Observations	20,905	21,723	33,775	8,853	20,981	21,647
	R^2	0.106	0.069	0.024	0.064	0.044	0.005
9th Grade	Age at Enrolment (in years)	0.101 *	0.060	0.112 **	-0.064	0.062	0.111 *
		(0.047)	(0.054)	(0.040)	(0.079)	(0.051)	(0.051)
	Observations	10,191	10,765	17,589	3,367	10,355	10,601
	R^2	0.090	0.070	0.051	0.086	0.064	0.035

Notes: * p <0.05, ** p <0.01; *** p <0.001. Reduced form estimates from 2SLS regressions with robust errors in parentheses. The models report the results from IV regression where assigned age was used as an instrument for the observed age. The models include controls for canton, spoken language at home, household composition, and living area per capita. The models further control for sex, migration status, and mean income decile, unless the variable is used to create the sample split.

Table E5: Subgroup Analyses with IV Approach - Writing

Dependent Variable: Test Scores in Writing

Subsample:		Females	Males	German	Foreign Language	Lower Income	Upper Income
3rd Grade	Age at Enrolment (in years)	0.357 ***	0.267 ***	0.367 ***	0.192 ***	0.235 ***	0.390 ***
		(0.033)	(0.034)	(0.029)	(0.042)	(0.035)	(0.033)
	Observations	19,477	20,252	27,207	12,522	19,756	19,973
	R^2	0.114	0.090	0.034	0.076	0.085	0.029
5th Grade	Age at Enrolment (in years)	0.179 ***	0.196 ***	0.224 ***	0.110 *	0.098 *	0.259 ***
		(0.042)	(0.045)	(0.037)	(0.055)	(0.045)	(0.042)
	Observations	11,258	11,592	15,483	7,367	11,218	11,632
	R^2	0.136	0.097	0.098	0.101	0.122	0.085
6th Grade	Age at Enrolment (in years)	0.309 ***	0.341 ***	0.315 ***	0.353 ***	0.322 ***	0.325 ***
		(0.039)	(0.041)	(0.033)	(0.055)	(0.042)	(0.039)
	Observations	16,277	16,832	23,849	9,260	16,260	16,849
	R^2	0.090	0.064	0.050	0.046	0.065	0.043
8th Grade	Age at Enrolment (in years)	0.189 ***	0.237 ***	0.220 ***	0.183 ***	0.183 ***	0.236 ***
		(0.030)	(0.033)	(0.025)	(0.050)	(0.032)	(0.031)
	Observations	23,656	24,730	38,931	9,455	23,831	24,555
	R^2	0.079	0.059	0.055	0.074	0.068	0.030
9th Grade	Age at Enrolment (in years)	0.120 *	0.140 **	0.137 ***	0.099	0.157 **	0.115 *
		(0.047)	(0.049)	(0.037)	(0.084)	(0.050)	(0.046)
	Observations	10,245	10,815	17,675	3,385	10,421	10,639
	R^2	0.071	0.055	0.080	0.064	0.066	0.065

Notes: * p <0.05, ** p <0.01; *** p <0.001. Reduced form estimates from 2SLS regressions with robust errors in parentheses. The models report the results from IV regression where assigned age was used as an instrument for the observed age. The models include controls for canton, spoken language at home, household composition, and living area per capita. The models further control for sex, migration status, and mean income decile, unless the variable is used to create the sample split.

Table E6: Subgroup Analyses with IV Approach - Algebra

Dependent variable: Test Scores in Algebra

		Females	Males	German	Foreign Language	Lower Income	Upper Income
3rd Grade	Age at Enrolment (in years)	0.379 ***	0.379 ***	0.417 ***	0.301 ***	0.358 ***	0.402 ***
		(0.032)	(0.035)	(0.029)	(0.043)	(0.035)	(0.032)
	Observations	22,116	23,015	30,936	14,195	22,501	22,630
	R^2	0.066	0.036	0.027	0.055	0.011	0.000
5th Grade	Age at Enrolment (in years)	0.232 ***	0.125 **	0.213 ***	0.115 *	0.117 *	0.220 ***
		(0.045)	(0.046)	(0.039)	(0.058)	(0.047)	(0.044)
	Observations	11,315	11,724	15,586	7,453	11,315	11,724
	R^2	0.077	0.099	0.042	0.070	0.036	-0.001
6th Grade	Age at Enrolment (in years)	0.243 ***	0.262 ***	0.252 ***	0.255 ***	0.229 ***	0.273 ***
		(0.042)	(0.042)	(0.034)	(0.058)	(0.044)	(0.040)
	Observations	16,259	16,896	23,859	9,296	16,314	16,841
	R^2	0.062	0.040	0.023	0.033	0.017	-0.021
8th Grade	Age at Enrolment (in years)	0.114 ***	0.168 ***	0.127 ***	0.197 ***	0.176 ***	0.104 **
		(0.033)	(0.036)	(0.028)	(0.054)	(0.036)	(0.033)
	Observations	20,914	21,752	33,795	8,871	21,008	21,658
	R^2	0.091	0.066	0.047	0.048	0.005	0.002
9th Grade	Age at Enrolment (in years)	0.072	0.100	0.109 **	-0.021	0.119 *	0.065
		(0.049)	(0.054)	(0.040)	(0.087)	(0.054)	(0.049)
	Observations	10,206	10,814	17,643	3,377	10,383	10,637
	R^2	0.079	0.057	0.041	0.070	0.010	0.017

Notes: * p <0.05, ** p <0.01; *** p <0.001. Reduced form estimates from 2SLS regressions with robust errors in parentheses. The models report the results from IV regression where assigned age was used as an instrument for the observed age. The models include controls for canton, spoken language at home, household composition, and living area per capita. The models further control for sex, migration status, and mean income decile, unless the variable is used to create the sample split.

E3 Robustness of the IV Estimation to Alternative Specifications

Table E7: Robustness of the IV Estimates to Alternative Specifications

		Test Scores in Reading			Test Scores in Writing			Test Scores in Algebra		
		Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
3rd Grade	Age at Enrolment (in years)	0.438 ***	0.467 ***	0.458 ***	0.289 ***	0.288 ***	0.313 ***	0.365 ***	0.417 ***	0.381 ***
		(0.024)	(0.036)	(0.023)	(0.025)	(0.037)	(0.024)	(0.024)	(0.036)	(0.024)
	Controls	No	No	Yes	No	No	Yes	No	No	Yes
	Observations	50,252	50,252	45,110	44,243	44,243	39,729	50,360	50,360	45,131
5th Grade	Age at Enrolment (in years)	0.207 ***	0.225 ***	0.247 ***	0.144 ***	0.194 ***	0.187 ***	0.133 ***	0.159 ***	0.180 ***
		(0.033)	(0.047)	(0.031)	(0.033)	(0.047)	(0.031)	(0.033)	(0.047)	(0.033)
	Controls	No	No	Yes	No	No	Yes	No	No	Yes
	Observations	26,026	26,026	23,039	25,778	25,778	22,850	26,076	26,076	23,039
6th Grade	Age at Enrolment (in years)	0.201 ***	0.227 ***	0.268 ***	0.236 ***	0.306 ***	0.324 ***	0.207 ***	0.239 ***	0.252 ***
		(0.030)	(0.044)	(0.029)	(0.030)	(0.045)	(0.028)	(0.030)	(0.044)	(0.030)
	Controls	No	No	Yes	No	No	Yes	No	No	Yes
	Observations	39,390	39,390	33,215	39,223	39,223	33,109	39,391	39,391	33,155
8th Grade	Age at Enrolment (in years)	0.131 ***	0.170 ***	0.201 ***	0.123 ***	0.165 ***	0.213 ***	0.093 ***	0.131 ***	0.141 ***
		(0.025)	(0.034)	(0.025)	(0.023)	(0.031)	(0.022)	(0.025)	(0.034)	(0.025)
	Controls	No	No	Yes	No	No	Yes	No	No	Yes
	Observations	52,633	52,633	42,628	59,855	59,855	48,386	52,800	52,800	42,666
9th Grade	Age at Enrolment (in years)	0.039	0.126 **	0.082 *	0.077 *	0.156 ***	0.130 ***	0.052	0.103 *	0.086 *
		(0.035)	(0.045)	(0.036)	(0.034)	(0.044)	(0.034)	(0.035)	(0.045)	(0.036)
	Controls	No	No	Yes	No	No	Yes	No	No	Yes
	Observations	26,663	26,663	20,956	26,819	26,819	21,060	26,772	26,772	21,020

Notes: * p <0.05, ** p <0.01; *** p <0.001. Reduced form estimates from 2SLS regressions with robust errors in parentheses. The models report the results from IV regression where assigned age was used as an instrument for the observed age. Models 1 is the baseline model with no additional controls. Models 2 include a polynomial for age which is instrumented by the polynomial of the assigned age. Models 3 do not incorporate the polynomial but use controls for canton, sex, migration status, spoken language at home, household composition, mean income decile, and living area per capita.

E4 OLS Models on Assigned Age

Table E8: OLS Models on Assigned Age

Dependent Variable: Assigned Age					
Grade	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	4.530 ***	4.540 ***	4.502 ***	4.508 ***	4.513 ***
	(0.006)	(0.008)	(0.007)	(0.005)	(0.008)
Sex – ref. = Female					
Male	0.002	0.001	0.003	0.004	0.006
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Migration Background - ref. = Native					
Second Generation	0.001	-0.008	-0.010	0.000	0.003
	(0.010)	(0.011)	(0.009)	(0.007)	(0.010)
First Generation	0.001	-0.005	-0.006	-0.002	-0.005
	(0.006)	(0.007)	(0.006)	(0.005)	(0.007)
Language spoken at home – ref. = German					
Foreign Language	-0.005	-0.007	0.000	0.004	0.005
	(0.004)	(0.006)	(0.005)	(0.004)	(0.007)
Mean Income Decile	0.000	-0.001	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Household Composition: ref. = Both Parents					
Single Parent	0.002	-0.011	0.001	0.001	0.004
	(0.005)	(0.006)	(0.005)	(0.004)	(0.006)
Area per Capita	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	45,495	23,475	33,575	48,934	21,278
R2 Adj.	0.000	0.000	0.000	0.000	0.000
AIC	18,880.1	9,783.8	11,838.8	17,001.5	7,456.9
F	0.478	1.05	0.387	0.914	1.111

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS estimates with robust standard errors in parentheses.

E5 Analysis of Monotonicity in the IV-Framework

Table E9 displays the counterfactuals to discuss monotonicity similar to Fiorini and Stevens (2021). We can think of nine combinations of counterfactual decisions that influence the observed age. The sign behind each capital letter in the table informs about the change in observed age at school entry if a child is born after the cut-off rather than before. For example, cell E represents students that would enter school on time in either of the two situations – like most of the observations in our analysis. This behaviour would result in an increase in observed age ($ObsAge_i(before) < ObsAge_i(after)$).

Table E9: Counterfactuals Regarding Assigned Age

	Child born after cut-off		
Child born before cut-off	Early	On Time	Late
Early	A (+)	B (+)	C (+)
On Time	D (-)	E (+)	F (+)
Late	G (-)	H (-)	I (+)

When we think about counterfactuals in combination with constant preferences or beliefs in gains, we can rule out options C and G, as they would violate the assumption of a constant belief or preference regarding relative age. Similarly, we can rule out options A and B as a pattern because we see in the data that children born near before the cut-off enrol early less frequently. Likewise, we see that options F and I are also less prominent among students born just after the cut-off. However, during the time until observation, the children could also be retained, which potentially explains the higher percentage among these cases. Figure E1 is an example of the graphical representation of observed age and assigned age based on the sample of 3rd-grade students in the canton of Aargau in 2015. The blue circles represent the observed age per month of birth, while the size of the circles mirrors the proportion by month of birth. The black dots mark the assigned age. The circles on the dashed line represent students with late school entries (or those who were retained), while the circles on the dotted line represent students with an early school entry.

As mentioned above, type E is the most common case in the data with around 75% of the observations, which is also visible in Figure E1 where the larges circles are found around the black line representing students that enrol on time. However, the concerning parts are types D and H, which we cannot rule out as we also find in the data. For example, about 3% of observations in the 3rd-grade Checks in 2015 in Aargau were born after a cut-off but enrolled early. Similarly, we find students, born before the eligible years that register late. These combinations of counterfactuals would result in a drop in relative age. This can also be seen in Figure E2, where we plot the cumulative density from the sample of 3rd-grade students in Aargau from 2015. For simplicity, we only consider Students born in April (before) and students born in May (after). For children born in May, we see that some enrol early and some late (or that they were retained) alongside children born in April that enrol late.

Figure E1: Assigned Age vs Observed Age Across Months of Birth

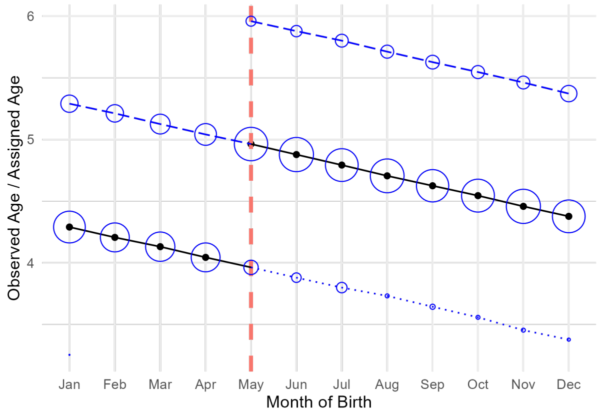
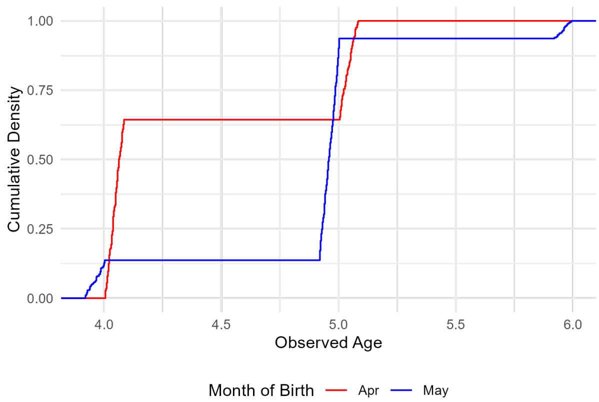


Figure E2: Cumulative Density Plot of 3rd Grade Students Born in April or May in Aargau in 2015



Appendix F

F1 OLS Estimates using the IV Samples

Table F1: OLS Estimates using the IV Samples - Reading

Dependent Variable: Test Scores in Reading

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-0.185 ***	0.753 ***	1.102 ***	1.157 ***	1.246 ***
	(0.054)	(0.069)	(0.059)	(0.048)	(0.067)
Age at Enrolment (in years)	-0.007	-0.205 ***	-0.281 ***	-0.282 ***	-0.268 ***
	(0.010)	(0.013)	(0.011)	(0.009)	(0.013)
Sex – ref. = Female					
Male	-0.107 ***	-0.069 ***	-0.148 ***	-0.163 ***	-0.236 ***
	(0.009)	(0.012)	(0.010)	(0.009)	(0.013)
Migration Background - ref. = Native					
Second Generation	-0.063 *	0.041	-0.007	-0.002	-0.005
	(0.030)	(0.034)	(0.029)	(0.023)	(0.034)
First Generation	0.061 ***	0.129 ***	0.107 ***	0.095 ***	0.072 **
	(0.018)	(0.022)	(0.019)	(0.016)	(0.024)
Language spoken at home – ref. = German					
Foreign Language	-0.477 ***	-0.512 ***	-0.393 ***	-0.363 ***	-0.338 ***
	(0.013)	(0.017)	(0.015)	(0.014)	(0.023)
Mean Income Decile	0.063 ***	0.069 ***	0.064 ***	0.065 ***	0.053 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Household Composition: ref. = Both Parents					
Single Parent	-0.130 ***	-0.123 ***	-0.118 ***	-0.114 ***	-0.077 ***
	(0.014)	(0.018)	(0.015)	(0.014)	(0.021)
Area per Capita	0.004 ***	0.004 ***	0.003 ***	0.003 ***	0.003 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Canton – ref. = Aargau					
Basel-Land	-0.057 ***	-0.055 ***	-0.008	-0.098 ***	
	(0.011)	(0.015)	(0.013)	(0.015)	
Basel-Stadt	0.020	-0.017	-0.053 **	-0.089 ***	
	(0.014)	(0.020)	(0.017)	(0.016)	
Solothurn	-0.137 ***	-0.048 **	0.012	-0.062 ***	-0.244 ***
	(0.012)	(0.016)	(0.014)	(0.011)	(0.015)
Observations	45,110	23,039	33,215	42,628	20,956
R2 Adjusted	0.161	0.203	0.169	0.149	0.124
AIC	119,956.4	59,619.2	86,819.4	112,136.0	55,832.7

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS estimates with robust standard errors in parentheses.

Table F2: OLS Estimates using the IV Samples - Writing

Dependent Variable: Test Scores in Writing

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	0.192 ***	0.847 ***	1.133 ***	1.104 ***	1.254 ***
	(0.057)	(0.069)	(0.058)	(0.043)	(0.063)
Age at Enrolment (in years)	-0.067 ***	-0.190 ***	-0.252 ***	-0.248 ***	-0.249 ***
	(0.011)	(0.013)	(0.011)	(0.008)	(0.012)
Sex – ref. = Female					
Male	-0.260 ***	-0.464 ***	-0.404 ***	-0.376 ***	-0.398 ***
	(0.009)	(0.012)	(0.010)	(0.008)	(0.012)
Migration Background - ref. = Native					
Second Generation	-0.076 *	-0.060	-0.020	-0.040	-0.019
	(0.032)	(0.034)	(0.028)	(0.021)	(0.032)
First Generation	0.029	0.040	0.073 ***	0.043 **	0.055 *
	(0.020)	(0.022)	(0.018)	(0.014)	(0.022)
Language spoken at home – ref. = German					
Foreign Language	-0.367 ***	-0.350 ***	-0.348 ***	-0.326 ***	-0.290 ***
	(0.013)	(0.017)	(0.015)	(0.013)	(0.022)
Mean Income Decile	0.058 ***	0.062 ***	0.058 ***	0.060 ***	0.049 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Household Composition: ref. = Both Parents					
Single Parent	-0.143 ***	-0.125 ***	-0.131 ***	-0.117 ***	-0.115 ***
	(0.015)	(0.018)	(0.015)	(0.012)	(0.020)
Area per Capita	0.004 ***	0.003 ***	0.004 ***	0.003 ***	0.002 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Canton – ref. = Aargau					
Basel-Land	-0.093 ***	-0.023	-0.028 *	-0.020	
	(0.012)	(0.015)	(0.013)	(0.014)	
Basel-Stadt	-0.154 ***	-0.127 ***	-0.138 ***	-0.108 ***	
	(0.015)	(0.020)	(0.016)	(0.015)	
Solothurn	-0.137 ***	-0.040 *	-0.041 **	-0.059 ***	-0.221 ***
	(0.012)	(0.016)	(0.014)	(0.010)	(0.014)
Observations	39,729	22,850	33,109	48,386	21,060
R2 Adjusted	0.141	0.196	0.184	0.161	0.146
AIC	105,762.8	58,475.2	84,972.7	123,470.0	53,553.9

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS estimates with robust standard errors in parentheses.

Table F3: OLS Estimates using the IV Samples - Algebra

Dependent Variable: Test Scores in Algebra	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	0.125 *	1.112 ***	1.318 ***	1.362 ***	1.446 ***
	(0.056)	(0.072)	(0.060)	(0.048)	(0.067)
Age at Enrolment (in years)	-0.101 ***	-0.310 ***	-0.355 ***	-0.352 ***	-0.355 ***
	(0.011)	(0.014)	(0.012)	(0.009)	(0.013)
Sex – ref. = Female					
Male	0.232 ***	0.156 ***	0.177 ***	0.058 ***	0.063 ***
	(0.009)	(0.012)	(0.010)	(0.009)	(0.013)
Migration Background - ref. = Native					
Second Generation	0.045	0.122 ***	0.037	0.071 **	0.047
	(0.031)	(0.035)	(0.029)	(0.023)	(0.034)
First Generation	0.018	0.096 ***	0.036	0.063 ***	0.068 **
	(0.019)	(0.023)	(0.019)	(0.016)	(0.024)
Language spoken at home – ref. = German					
Foreign Language	-0.200 ***	-0.217 ***	-0.180 ***	-0.221 ***	-0.214 ***
	(0.013)	(0.018)	(0.015)	(0.014)	(0.023)
Mean Income Decile	0.067 ***	0.072 ***	0.060 ***	0.072 ***	0.061 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Household Composition: ref. = Both Parents					
Single Parent	-0.192 ***	-0.156 ***	-0.179 ***	-0.193 ***	-0.187 ***
	(0.015)	(0.019)	(0.016)	(0.014)	(0.021)
Area per Capita	0.003 ***	0.003 ***	0.003 ***	0.003 ***	0.003 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Canton – ref. = Aargau					
Basel-Land	-0.224 ***	-0.170 ***	-0.149 ***	-0.144 ***	
	(0.012)	(0.016)	(0.014)	(0.015)	
Basel-Stadt	-0.227 ***	-0.320 ***	-0.351 ***	-0.254 ***	
	(0.014)	(0.021)	(0.017)	(0.016)	
Solothurn	-0.171 ***	-0.043 **	0.023	-0.079 ***	-0.209 ***
	(0.012)	(0.017)	(0.014)	(0.011)	(0.015)
Observations	45,131	23,039	33,155	42,666	21,020
R2 Adjusted	0.099	0.135	0.128	0.137	0.118
AIC	123,909.6	61,620.1	88,192.7	112,271.2	55,920.3

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS estimates with robust standard errors in parentheses.

F2 OLS Results from the Sample with Linear Trajectories (RD)

Table F4: OLS Results from the Sample with Linear Trajectories - Reading

Dependent Variable: Test Scores in Reading

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-1.709 ***	-0.736 ***	-0.505 ***	-0.149	0.354 **
	(0.077)	(0.104)	(0.092)	(0.084)	(0.127)
Age at Enrolment (in years)	0.339 ***	0.131 ***	0.085 ***	0.014	-0.069 *
	(0.016)	(0.022)	(0.019)	(0.018)	(0.027)
Sex – ref. = Female					
Male	-0.109 ***	-0.073 ***	-0.161 ***	-0.174 ***	-0.248 ***
	(0.009)	(0.013)	(0.011)	(0.010)	(0.015)
Sex – ref. = Female					
Second Generation	-0.055	0.042	0.029	0.026	-0.003
	(0.034)	(0.039)	(0.035)	(0.028)	(0.043)
First Generation	0.064 **	0.122 ***	0.142 ***	0.130 ***	0.102 ***
	(0.021)	(0.025)	(0.023)	(0.019)	(0.031)
Language spoken at home – ref. = German					
Foreign Language	-0.480 ***	-0.529 ***	-0.404 ***	-0.373 ***	-0.317 ***
	(0.014)	(0.019)	(0.017)	(0.017)	(0.028)
Mean Income Decile	0.063 ***	0.069 ***	0.065 ***	0.064 ***	0.055 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Household Composition: ref. = Both Parents					
Single Parent	-0.132 ***	-0.136 ***	-0.125 ***	-0.128 ***	-0.071 **
	(0.016)	(0.020)	(0.017)	(0.016)	(0.026)
Area per Capita	0.004 ***	0.004 ***	0.003 ***	0.003 ***	0.002 ***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.075 ***	-0.062 ***	0.002	-0.075 ***	
	(0.012)	(0.017)	(0.015)	(0.017)	
Basel-Stadt	0.005	-0.035	-0.051 **	-0.074 ***	
	(0.015)	(0.022)	(0.018)	(0.018)	
Solothurn	-0.161 ***	-0.049 **	0.013	-0.050 ***	-0.244 ***
	(0.013)	(0.017)	(0.015)	(0.013)	(0.018)
Observations	372,981	18,936	26,932	32,061	14,711
R2 Adjusted	0.166	0.186	0.145	0.123	0.095
AIC	100,871.4	49,015.1	70,440.7	84,074.7	39,096.5

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS estimates with robust standard errors in parentheses.

Table F5: OLS Results from the Sample with Linear Trajectories - Writing

Dependent Variable: Test Scores in Writing

	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-1.059 ***	-0.412 ***	-0.481 ***	-0.126	0.328 **
	(0.082)	(0.102)	(0.089)	(0.076)	(0.117)
Age at Enrolment (in years)	0.219 ***	0.099 ***	0.119 ***	0.033 *	-0.038
	(0.017)	(0.022)	(0.019)	(0.016)	(0.025)
Sex – ref. = Female					
Male	-0.266 ***	-0.478 ***	-0.409 ***	-0.385 ***	-0.405 ***
	(0.010)	(0.013)	(0.011)	(0.009)	(0.014)
Sex – ref. = Female					
Second Generation	-0.050	-0.024	0.014	-0.003	0.046
	(0.036)	(0.038)	(0.034)	(0.026)	(0.040)
First Generation	0.046 *	0.058 *	0.112 ***	0.081 ***	0.113 ***
	(0.023)	(0.025)	(0.022)	(0.017)	(0.028)
Language spoken at home – ref. = German					
Foreign Language	-0.371 ***	-0.352 ***	-0.348 ***	-0.327 ***	-0.254 ***
	(0.015)	(0.019)	(0.016)	(0.015)	(0.026)
Mean Income Decile	0.058 ***	0.061 ***	0.057 ***	0.059 ***	0.049 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Household Composition: ref. = Both Parents					
Single Parent	-0.145 ***	-0.128 ***	-0.135 ***	-0.116 ***	-0.114 ***
	(0.017)	(0.020)	(0.017)	(0.014)	(0.024)
Area per Capita	0.004 ***	0.003 ***	0.003 ***	0.003 ***	0.003 ***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.106 ***	-0.017	-0.023	-0.005	
	(0.013)	(0.017)	(0.015)	(0.016)	
Basel-Stadt	-0.162 ***	-0.150 ***	-0.141 ***	-0.098 ***	
	(0.016)	(0.022)	(0.018)	(0.016)	
Solothurn	-0.158 ***	-0.039 *	-0.038 *	-0.043 ***	-0.195 ***
	(0.013)	(0.017)	(0.015)	(0.011)	(0.016)
Observations	33,437	18,779	262,857	35,995	14,767
R2 Adjusted	0.142	0.184	0.161	0.138	0.117
AIC	88,679.9	47,664.6	68,434.0	90,901.6	36,767.1

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS estimates with robust standard errors in parentheses.

Table F6: OLS Results from the Sample with Linear Trajectories - Algebra

Dependent Variable: Test Scores in Algebra	3rd Grade	5th Grade	6th Grade	8th Grade	9th Grade
Intercept	-1.429 ***	-0.474 ***	-0.489 ***	0.008	0.480 ***
	(0.079)	(0.108)	(0.092)	(0.083)	(0.125)
Age at Enrolment (in years)	0.252 ***	0.050 *	0.055 **	-0.045 *	-0.130 ***
	(0.017)	(0.023)	(0.019)	(0.018)	(0.026)
Sex – ref. = Female					
Male	0.234 ***	0.148 ***	0.167 ***	0.047 ***	0.054 ***
	(0.010)	(0.013)	(0.011)	(0.010)	(0.015)
Sex – ref. = Female					
Second Generation	0.054	0.128 **	0.073 *	0.092 ***	0.082
	(0.035)	(0.040)	(0.035)	(0.028)	(0.043)
First Generation	0.029	0.093 ***	0.065 **	0.101 ***	0.108 ***
	(0.021)	(0.026)	(0.023)	(0.019)	(0.030)
Language spoken at home – ref. = German					
Foreign Language	-0.196 ***	-0.228 ***	-0.196 ***	-0.232 ***	-0.231 ***
	(0.014)	(0.020)	(0.017)	(0.016)	(0.028)
Mean Income Decile	0.068 ***	0.072 ***	0.059 ***	0.068 ***	0.058 ***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Household Composition: ref. = Both Parents					
Single Parent	-0.188 ***	-0.163 ***	-0.176 ***	-0.192 ***	-0.185 ***
	(0.016)	(0.021)	(0.017)	(0.016)	(0.025)
Area per Capita	0.002 ***	0.003 ***	0.004 ***	0.003 ***	0.002 ***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
Canton – ref. = Aargau					
Basel-Land	-0.242 ***	-0.183 ***	-0.136 ***	-0.112 ***	
	(0.013)	(0.018)	(0.015)	(0.017)	
Basel-Stadt	-0.255 ***	-0.352 ***	-0.341 ***	-0.239 ***	
	(0.015)	(0.023)	(0.018)	(0.017)	
Solothurn	-0.193 ***	-0.051 **	0.031 *	-0.053 ***	-0.177 ***
	(0.013)	(0.018)	(0.015)	(0.012)	(0.017)
Observations	37,996	18,926	26,891	32,100	14,762
R2 Adjusted	0.106	0.115	0.102	0.101	0.071
AIC	102,956.4	50,168.7	70,178.9	83,048.0	38,715.3

Notes: * p <0.05, ** p <0.01; *** p <0.001. OLS estimates with robust standard errors in parentheses.

Declaration of Authorship

(Studienreglement WISO; 1. September 2006 Art. 19 and Art. 31)

I hereby declare that I have written this thesis independently and have not used any sources other than those stated. I have labelled as such all passages that were taken literally or analogously from sources. I am aware that otherwise the Senate is entitled to withdraw the title awarded based on this thesis in accordance with Article 36 paragraph 1 letter O of the Law of 5 September 1996 on the University.

Place and Date: Baden; 29.02.2024

Name: ....