

Educational Decisions and Inequality: Studies in the Economics of Education

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Kevin Jan Zuchanek, M. Sc.

aus Lüdenscheid

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井の中の蛙大海を知らず

- The frog in the well knows nothing of the great ocean -

- Japanese Proverb

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List of Abbreviations

ATT	Average Treatment Effect on the Treated
BuT	<i>Bildungs- und Teilhabepaket (Bremen-Pass)</i> (government support programme that provides financial assistance to children to enable them to take part in educational, cultural and leisure activities)
COVID-19	Coronavirus Disease 2019
DA	Deferred Acceptance
DiD	Difference-in-differences
Eq.	Equation
EUR	Euro (€)
GPA	Grade Point Average
IA	Immediate Acceptance
ISCED	International Standard Classification of Education
OCHA	United Nations Office for the Coordination of Humanitarian Affairs
ODP	Operational Data Portal of United Nations High Commissioner for Refugees
OECD	The Organisation for Economic Co-Operation and Development
OLS	Ordinary Least Squares
PISA	Programme for International Student Assessment
ROL	Rank Order List
UNDESA	United Nations Department of Economic and Social Affairs
UNDRR	United Nations Office for Disaster Risk Reduction
UNFPA	United Nations Population Fund
UNHCR	United Nations High Commissioner for Refugees
UNICEF	United Nations International Children's Emergency Fund
VASyR	Vulnerability Assessment of Syrian Refugees in Lebanon
VERA-8	<i>Vergleichsarbeiten in der 8. Jahrgangsstufe</i> (standardised comparative tests in grade 8)
WFP	World Food Programme

1 Introduction

Educational equity, the principle that all individuals, regardless of their socioeconomic background, ethnicity, or migration history, should have equal opportunities to obtain education, is widely upheld as a fundamental societal goal and defined as one of the sustainable development goals of the United Nations. Yet in practice, it remains far from achieved. Social inequalities are frequently perpetuated within education systems, as marginalised children often encounter structural barriers that limit their access to high-quality learning opportunities (OECD, 2018; UN-DESA, 2024; Enchikova et al., 2025). These barriers include the unequal distribution of resources, residential segregation, and information asymmetries that hinder informed decision-making at key educational steps. As a result, affected children face considerable challenges in fully participating in education and in translating their potential into tangible outcomes. Understanding the dual function of education, as both a potential driver of social mobility and, when access is unequal, a mechanism for reproducing disadvantage, is essential to addressing these persistent disparities and designing more equitable systems.

The human capital theory provides a valuable analytical lens through which to examine education and educational inequalities. This foundational concept in the economics of education posits that individuals enhance their productivity and earning potential through investments in education, training, and health. Although the notion dates back to classical economists such as Adam Smith, who touched on aspects of human capital in *An Inquiry into the Nature and Causes of the Wealth of Nations* in 1776, it was significantly formalized and expanded in the 20th century through the works of Jacob Mincer (1958), Theodore Schultz (1961), Gary Becker (1964) and others. Mincer's empirical research, especially the Mincer earnings function, demonstrated how differences in education and work experience account for wage differentials, implicitly illustrating how educational disparities reinforce economic inequalities. Schultz argued that expenditures that were traditionally regarded as consumption, such as education and health, should instead be recognized as investments in human capital. Becker further advanced the theory, emphasizing that individuals make education-related decisions by systematically weighing expected future returns against current costs. However, these decisions are significantly shaped by initial socioeconomic circumstances, often creating substantial barriers for marginalised groups to invest in education. Today, this framework is broadly acknowledged as fundamental to understanding educational trajectories, labour market outcomes, and long-term economic growth.

Underpinning much of the human capital framework is rational choice theory (von Neumann and Morgenstern, 1944; Friedman, 1953), which models individuals as utility-maximizing agents who systematically compare the expected costs and benefits of available options. In educational contexts, this theory suggests individuals weigh the long-term advantages of acquiring education, such as increased earnings potential and improved job security, against immediate costs, including tuition fees, foregone earnings, and personal effort. However, this rational decision-making process frequently presupposes stable preferences and access to sufficient information, conditions that often do not hold true

for marginalised or disadvantaged groups in reality. These groups typically face structural barriers, limited access to reliable information, and fewer available resources, severely constraining their ability to make fully informed educational choices. Consequently, rational choice theory, while valuable, must be contextualized within the broader realities of educational equity and social inequalities. Moreover, rational choice theory is further formalized through the expected utility hypothesis, developed by von Neumann and Morgenstern (1944), which posits that individuals make decisions under conditions of uncertainty by evaluating the expected utility of various outcomes and choosing the option with the highest expected value. In educational decision-making, this framework captures the inherent uncertainties regarding future outcomes such as employment prospects, income, and overall educational success. This uncertainty disproportionately impacts disadvantaged groups, as early educational decisions often set individuals onto trajectories that are difficult, if not impossible, to reverse. Thus, understanding educational choices through the lens of rational choice and expected utility must also account for systemic inequities, informational asymmetries, and socioeconomic constraints that fundamentally shape these decisions and perpetuate educational and social inequalities.

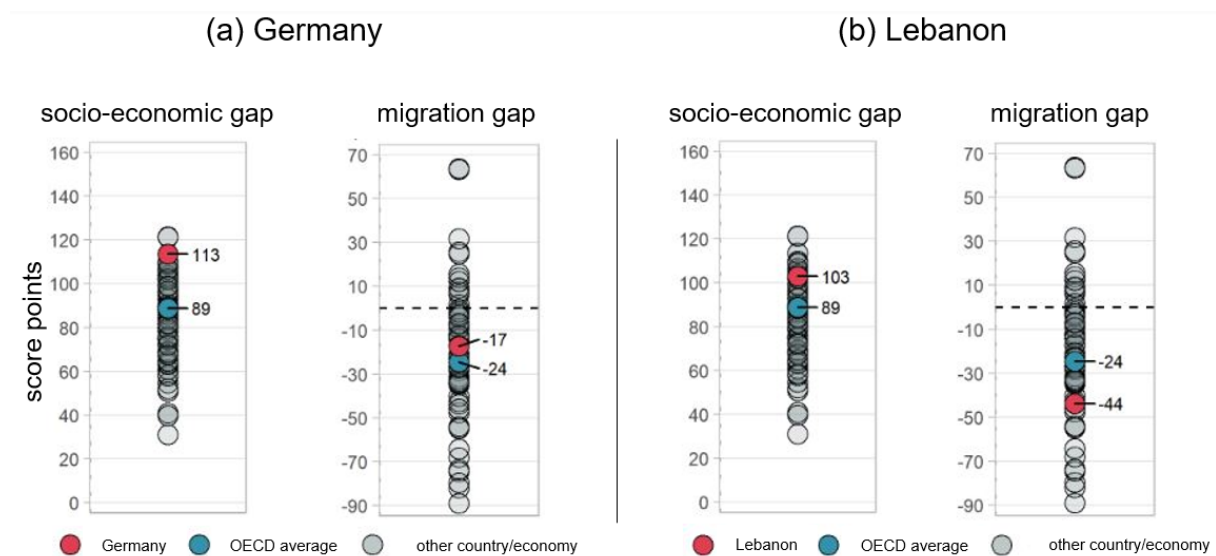
The empirical settings of the studies in this dissertation, the city state Bremen in Germany and Beirut in Lebanon, are places where educational and social inequalities are manifested, albeit in vastly different economic and institutional contexts. Both countries and the specified major cities have experienced significant migration inflows in recent years, especially in the wake of the Syrian civil war, and host large populations with migration backgrounds. In fact, while Germany is among the countries in Europe with the highest number of refugees, Lebanon has one of the largest numbers of refugees per capita in the world. While Germany represents a high-income, developed country with an established social welfare state and education system; Lebanon is classified as a developing country facing economic and political instability. Despite these differences, both locations exhibit different disparities in access to education, particularly for disadvantaged and refugee populations.

These disparities are also evident in international assessments, for example, the 2018 PISA study.¹ **Figure 1-1** shows exemplary PISA test results for reading performance. Socio-economically advantaged students in Germany outperformed their disadvantaged peers by 113 score points. The gap to the OECD average of 89 is exceeded by both countries, Germany and Lebanon. However, the gap is smaller in Lebanon than in Germany. In the latter, advantaged students outperform disadvantaged students by 103 points. Nonetheless, the numbers for both countries underscore the persistent and strong influence of socio-economic status on educational achievement. In general, socio-economic status was also a strong predictor of outcomes in mathematics and science in all other participating PISA countries (OECD, 2019a, b). A similar pattern emerges when comparing immigrant and non-immigrant students.

¹ This year is selected as the point of comparison because it represents the last cycle in which both Germany and Lebanon participated. Moreover, by 2018, the largest refugee movements had largely subsided, allowing for a more stable snapshot of educational systems and outcomes.

In Lebanon, non-immigrant students scored 51 points higher in reading than their immigrant peers; even after controlling for students' and schools' socio-economic profiles, a substantial gap of 44 points remained. In Germany, the initial gap was 63 score points, but it narrowed considerably to 17 points after adjusting for socio-economic background. These findings highlight that educational inequalities based on socio-economic status and migration background are present in both countries, although their extent and persistence are different and shaped by institutional and structural differences. However, this juxtaposition helps illuminate the varying mechanisms through which marginalised groups encounter barriers to education and how these challenges differ in high-income versus low-income contexts.

Figure 1-1 Socio-Economic and Migration-Related Performance Gaps in Reading in Germany and Lebanon, PISA, 2018



Note: The visualisation is compiled from the national reports, OECD (2019a, b): PISA 2018 Country Note – Germany & Lebanon, which is in turn taken from OECD, PISA 2018 Database. The figure cannot be reproduced using publicly available data. Socio-economic gap: advantaged minus disadvantaged students' performance; migration gap: immigrants' minus non-immigrants' performance in reading, after accounting for students' and schools' socio-economic profile.

By examining educational decision-making in these two settings, the dissertation demonstrates that fundamental challenges related to equity and access arise across different economies. These challenges are embedded in systemic structures and are pronounced in varied ways depending on political, institutional, and socioeconomic conditions. Investigating Bremen in Germany and Beirut in Lebanon offers valuable perspectives on how educational choices made under structural disadvantages may influence long-term human capital formation. These critical decisions are particularly important for marginalised groups, as they are often irreversible and may determine whether education becomes a vehicle for overcoming social inequality.

1.1 Study Context for Bremen, Germany

The first two studies contribute to the literature on school choice and centralised assignment mechanisms through analyses of the immediate acceptance mechanism in Bremen. The German schooling system is characterized by an early educational tracking setting around age 10, when children transition from primary to secondary school. Given that early tracking profoundly influences future opportunities, investigating whether these educational choices and assignment mechanisms are fair and efficient becomes particularly important. Centralised admission procedures are generally designed to promote both efficiency and fairness. They aim to prevent preferential treatment of students from advantaged backgrounds in decentralised systems and to avoid cream-skimming practices by schools. Centralized admission systems, on the other hand, seek to eliminate potentially discriminatory factors, such as migration background or socio-economic status, from the allocation process.

In oversubscribed schools, where the number of applicants exceeds the available seats, centralised admission mechanisms typically apply tie-breaking rules to allocate students to the (remaining) schooling places. Under the immediate acceptance (IA) mechanism used in Bremen, students are admitted to the highest-ranked school on their rank-order list (ROL) that still has available capacity (Abdulkadiroğlu and Sönmez, 2003). If demand exceeds supply at that school, priority rules, such as academic performance and the attended primary school, are applied, and any remaining ties are resolved by lottery. This process differs between the two school types in Bremen. For academic schools, students are placed in a priority group if they exceed a certain threshold in German and mathematics at the end of primary school, that is, if they are classified as ‘high-performing’. For comprehensive schools, students from specific affiliated primary schools that are high-performing are prioritised, followed by a second group consisting of all other applicants from those affiliated schools, regardless of academic performance. This makes it essential for families to understand how to rank schools strategically: selecting a highly demanded school as the first choice may lead to participation in an admission lottery and, if the student loses, to potential rejection from their second and third choices as well. Since placements are not revisited in subsequent rounds, available spots may already be filled after the first round. The literature has shown that such mechanisms can lead to strategic mistakes, particularly when parents lack a full understanding of the assignment rules (Pathak and Sönmez, 2008; Hastings et al., 2006; Kapor et al., 2020; Oosterbeek et al., 2021). Especially under IA, where truth-telling is not a dominant strategy, parents may benefit from submitting strategically ordered preferences (Abdulkadiroğlu and Sönmez, 2003), but only if they fully comprehend the admission process and the consequences of their decisions. Therefore, the quality of information available to parents is critical to ensuring that such procedures achieve their intended goals of fairness and efficiency.

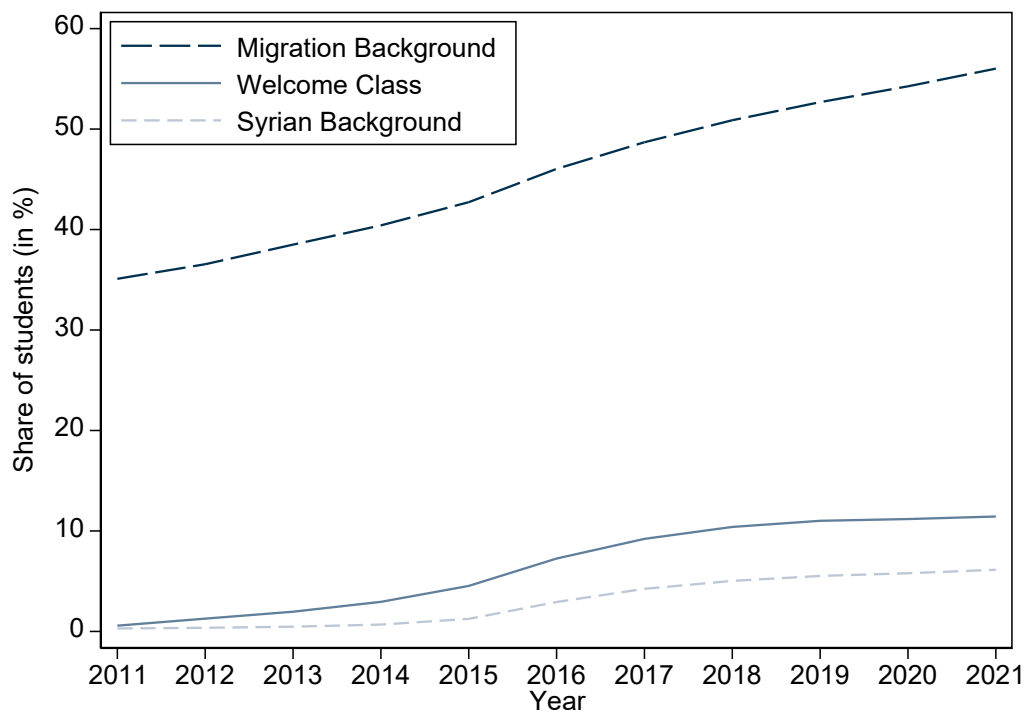
The first Bremen study (**Chapter 2**) investigates this by examining the costs associated with losing the admission lottery. Every year, parents complain when their child does not receive a place at their top-ranked school. And in fact, the allocated secondary school is important for the further

educational career, given that the secondary school is usually the school students attend until the end of compulsory schooling. Underlying these concerns might be the fear that a different peer constellation could negatively affect the child, as well as that being assigned to a lower-ranked or less desired school may reduce their chances of achieving a higher-level school-leaving certificate or hinder their academic development. Therefore, the study aims to quantify the costs of losing the lottery and assess their distribution across different student groups. The findings indicate that the consequences are primarily non-academic and are distributed unevenly across student groups. In short, disadvantaged students are more likely to enter the lottery pool, have limited access to costly opt-out options, and must therefore bear the consequences of their assigned placement. In contrast, native German students incur larger losses when they fail to secure a homogeneous, preferred school but are more able to substitute toward homogeneous private alternatives.

The results from **Chapter 2** highlight disparities that arise when students are assigned to schools other than their top choices, demonstrating that such mismatches are costly on the individual level and can reinforce existing inequalities. These findings also underscore the need to better understand why such disparities persist within a technically fair student assignment mechanism, and whether information asymmetries contribute to them. This question becomes particularly salient in the context of the immediate acceptance mechanism, which is not strategy-proof and thus incentivises strategic behaviour. Although, a prerequisite for strategic behaviour is a sound knowledge of the school system and the allocation mechanism.

Bremen has a long-standing history of educating students with migration backgrounds. **Figure 1-2** provides an overview of the rising number of students with a migration background, with Syrian background and students who initially attended so-called “welcome classes”, where students first acquire German language skills before switching into regular classrooms. In addition to the steady increase over time, an acceleration can be observed beginning in 2015, attributable to the Syrian civil war and the accompanying refugee influx. Participation in a welcome class may also serve as a proxy for limited German language proficiency within the household, suggesting that parents of these students in particular may struggle to understand complex school assignment procedures. This is relevant given existing evidence that matching mechanisms are often difficult to comprehend, for native and non-native speakers alike (Kapor et al., 2020).

Figure 1-2 Share of Students with a Migration and Syrian Background and Those Who Have Attended a ‘Welcome Class’ in Bremen, 2011/12 – 2021/22



Note: Data from the individual student statistics Bremen (2011/12 – 2021/22). A student is classified as having a migration background if at least one of the following criteria applies: a) has another or a second citizenship in addition to German citizenship, b) the place of birth is not Germany or c) mother tongue or the language spoken at home is not German; A student is classified as having a Syrian background if the place of birth is Syria or if they hold Syrian citizenship, either as their sole, first, or second nationality.

It appears that the growing number of students with limited German language proficiency may have contributed to the decision to translate the centralised admission procedure into six different languages in 2017: Arabic, English, French, Persian, Russian, and Turkish. Among students transitioning from primary to secondary school between 2011 (the year in which the centralised admission procedure was introduced) and 2021, the most spoken languages were Turkish and Arabic for non-German native speakers.

The second Bremen study, presented in **Chapter 3**, evaluates this information intervention aimed at reducing informational asymmetries in the school choice process between native and non-native students. The findings indicate that this measure had a meaningful impact on student behaviour: students whose home language matched one of the translated versions were more likely to rank and ultimately attend an academic school rather than a comprehensive school. This suggests that the intervention successfully improved decision-making by enabling better-informed choices. From an educational equity perspective, the intervention contributed to a more level playing field at the outset of

secondary education. It provides evidence that students with a migration background are not necessarily less inclined to pursue academic schools, but rather face informational barriers that prevent them from recognising their actual chances of being accepted. By mitigating these barriers, the intervention helped close opportunity gaps in the transition to secondary education. In the broader context of human capital formation, such targeted information policies can be seen as low-cost, high-leverage tools for improving long-term outcomes. By supporting students in making informed educational decisions early on in Bremen, these interventions help unlock the potential of students from underrepresented backgrounds, thereby contributing to individual upward mobility.

1.2 Study Context for Beirut, Lebanon

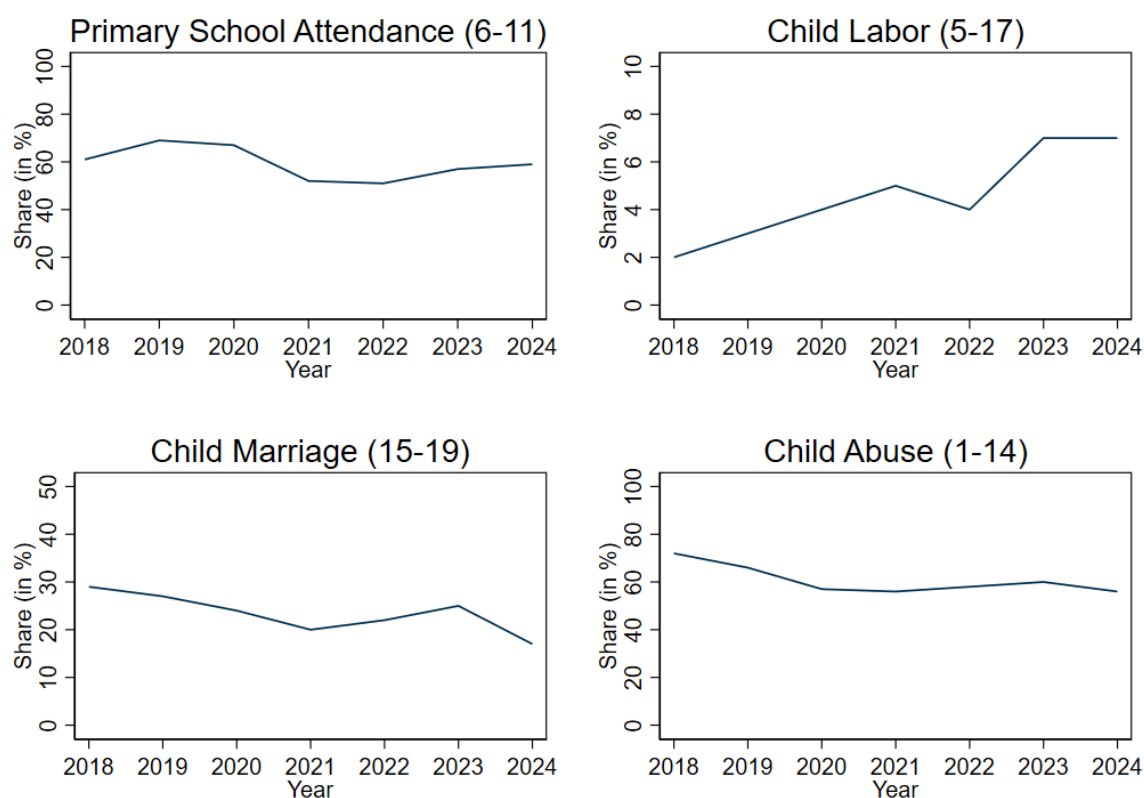
Although situated in a vastly different economic and political context, Beirut likewise faces deep educational inequalities, particularly for students from refugee and low-income backgrounds. Lebanon lacks formal refugee policies and is not a signatory to the 1951 Refugee Convention or its 1967 Protocol Relating to the Status of Refugees, the cornerstone treaties of international refugee law. Consequently, in contrast to Germany, refugees in Lebanon, most of whom are also of Syrian origin, are substantially disadvantaged relative to the host population. Educational and social inequalities are therefore even more pronounced. This includes precarious legal status, limited access to formal employment and education, inadequate housing conditions, and a lack of systematic integration measures. In the education sector specifically, these structural disadvantages translate into fragmented school access. These challenges are particularly acute in early educational stages, where limited institutional support and access to education constrain educational opportunities and long-term human capital development for refugee children, making them particularly vulnerable to exogenous shocks.

The recent numbers from The Vulnerability Assessment of Syrian Refugees in Lebanon (UNHCR, UNICEF and WFP, 2024) reveal that children are among the most severely affected by the precarious living conditions of Syrian refugee households in Lebanon. Approximately 7% of children are engaged in child labour, and nearly one in five girls aged 15 to 19 are already married. Moreover, more than half of all children are exposed to disciplinary practices by their parents that involve psychological aggression or physical punishment. The data also show that only 70% of children over the age of six have ever attended a school, and just 20% of adolescents aged 12 to 17 are enrolled in secondary education. These outcomes are largely driven by economic hardship (with around 90% of Syrian refugees in Lebanon living in extreme poverty), which severely limits the capacity of households to invest in their children's human capital. Under such conditions, even basic educational participation becomes a constrained choice, and long-term development prospects are jeopardised at an early stage.

These alarming figures are not new but are rooted in sustained barriers to integration, and they have remained consistently high since Syrian families sought refuge in Lebanon. In fact, **Figure 1-3**

visualises the primary school attendance rate and key child protection outcomes between 2018 and 2024, underscoring the heightened vulnerability of Syrian refugee households and children. The figure reveals that most indicators have remained constant or even worsened over time. Although reported incidents of child abuse and child marriage show downward trends, they remain at high levels. These developments point to a deepening of educational inequalities and disparities in access to child protection mechanisms, including protection from child labour, early marriage, and food insecurity, posing a serious threat to the healthy cognitive and emotional development of affected children.

Figure 1-3 Trends in School Attendance and Child Protection Outcomes among Syrian Refugees in Lebanon, 2018 – 2024



Note: Data from The Vulnerability Assessment of Syrian Refugees in Lebanon (VASyR, 2018 – 2024). For 2023 and 2024, data is only available on an aggregated level. The values in parentheses indicate the age cohorts to which each indicator refers, as reported in the annual VASyR reports.

The third study, presented in **Chapter 4**, broadens the analytical scope by focusing on this marginalised group of Syrian refugee households in Beirut and examining how crisis conditions shape educational decision-making. Specifically, it investigates the aftermath of the 2020 Beirut port explosion, a large-scale external shock that severely impacted this already vulnerable community. The study explores how

refugee households, faced with increased economic hardship and institutional fragility, make critical decisions regarding their children's schooling. These include whether to keep children enrolled in school or resort to adverse coping strategies such as child labour or early marriage, both of which carry long-term consequences for human capital accumulation. By analysing these responses, the study sheds light on how structural disadvantages and constrained opportunity sets in fragile settings affect the educational trajectories of refugee children.

1.3 Summary Results

Across both settings, Bremen in Germany and Beirut in Lebanon, structurally disadvantaged students face the challenge of navigating unfamiliar and often complex school systems. Whether due to a migration background or socio-economic disadvantages, these students consistently exhibit lower levels of educational participation and attainment, as illustrated in international assessments such as PISA (**Figure 1-1**). Educational inequalities of this kind reinforce broader social disparities and severely constrain opportunities for upward mobility. Even when seemingly fair and impartial mechanisms are implemented to promote equity, **Chapter 2** and **3** show that inequalities persist. While informational interventions can mitigate some barriers, they do not eliminate structural disadvantages entirely. **Chapter 4** further highlights the importance of institutional frameworks in safeguarding equal access to education. In the absence of formal protections, such as refugee conventions or enforceable treaties, there is little incentive to address persistent inequalities, thereby perpetuating the vulnerability of affected populations. More fundamentally, the study shows that families in such fragile settings are often unable to make forward-looking educational investments for their children, as assumed in human capital theory. Instead, exposure to economic precarity and vulnerability to external shocks severely constrains their decision-making capacity and jeopardises long-term development prospects.

A summary of the motivation, contributions to the literature, main findings, and policy implications follows:

Chapter 2 (*joint work with Kerstin Schneider, Anna Makles and Thomas Gößl. Data preparation (45%), analysis (45%) and text (45%).*) investigates the consequences of school allocation outcomes, specifically examining what happens when students lose admission lotteries in Bremen and attend schools other than their top-ranked choice. Each year, this mismatch between stated preferences and actual assignments leads to frustration and complaints from both parents and students, reflecting the strong demand for particular schools. This raises the question of whether attending a non-preferred school results in measurable disadvantages for students, particularly given that tracking in Bremen begins early and secondary school placements typically determine a student's entire compulsory schooling path. In such a context, negative educational effects may accumulate over time and reinforce initial disadvantages.

Several studies have investigated the impact of school admission lottery outcomes on a variety of different academic and non-academic outcomes, but whether these lotteries lead to adverse effects remains contested. Some find that school assignment matters (Angrist et al., 2016; Bibler and Billings, 2020), while others report mixed results (Cullen et al., 2006; Hastings et al., 2006) or negligible effects (Ketel et al., 2023). This chapter contributes to the literature by applying a state-of-the-art methodology developed by Abdulkadiroğlu et al. (2017), comparing students with equal probabilities of admission to oversubscribed schools. The analysis takes place in a setting with unique and rich administrative data, allowing for tracking of every student's educational trajectory over more than a decade in both public and private schools. This allows for the investigation of potential short- and long-term adverse academic and non-academic outcomes. In contrast to previous studies, this study investigates the consequences of very early tracking, accounts for non-compliance, and examines opt-outs to private schools as a potential strategy to circumvent the consequences of losing the admission lottery.

The study uses lottery-induced randomisation, which eliminates concerns about endogenous sorting among students, and finds that students in Bremen who are not assigned to their first-ranked school, e.g. lose the admission lottery, experience significantly longer home-school distances, different school environments (measured by the share of students with a migration background, share of welfare benefit recipients, social index, and academic standard), and reduced access to peer networks. To assess short- and long-term academic outcomes, the study analyses standardised test scores three years after the transition to secondary school and school-leaving certificates five to eight years later. In this regard, the analysis reveals no systematic adverse academic effects. In the expected utility framework, this suggests that while the assignment result may deviate from students' top-ranked school, their realised utility, in terms of academic outcomes, does not necessarily fall below expectations. However, being assigned to a lower-ranked school still incurs substantial non-academic costs. Due to strong preferences for more homogeneous school environments or higher performing schools, native German-speaking students are among the groups exhibiting more pronounced adverse effects in effect size. They are also more likely to deviate from their assigned placement and are frequently observed, often years later, either at their originally top-ranked school or at a private school. This pattern suggests a willingness and ability to pay school fees in exchange for access to a more socio-demographically homogeneous peer environment and a high-performing school. Another group experiencing disproportionately large negative effects are students from households receiving welfare benefits, who are significantly less likely to exercise the opt-out option, either due to financial constraints or a lack of relevant information. As a result, they are more likely to absorb the full cost of a non-preferred placement. By contrast, as a consequence of their school choices, students with a migration background are more likely to end up in the lottery pool and to lose it. Hence, while the effect size of a loss is greater for native German students relative to the lottery winner mean, students with a migration background experience a greater frequency of these costs being realized. These patterns indicate that certain groups face systematically different

constraints and opportunity structures when navigating the school assignment system, thereby reinforcing pre-existing educational inequalities.

This is a challenge that is not unique to Bremen or Germany but common to most countries when students transition between educational stages, such as from primary to secondary school or to higher education. While the immediate acceptance mechanism is widely used for its ability to optimize first-choice matches, it also involves inherent trade-offs. The findings highlight the importance of transparency in the assignment process, as parental dissatisfaction often reflects concerns about real disadvantages associated with non-preferred placements. Ensuring clear communication, transparency, and equal access to information during the transition from primary to secondary school is essential to support informed decision-making and increase acceptance of school assignments, especially in systems where strategic behaviour is rewarded and early tracking may influence long-term educational trajectories.

Chapter 3 (*single-authored. An earlier version of this chapter was published as RWI Working Paper No. 1172*) is motivated by the widespread misperceptions regarding the functioning of centralised admission procedures (Kapor et al., 2020), the previously described frustration of parents from **Chapter 2**, and the surrounding secondary school placement due to imperfect information. Centralised admission procedures are designed to assign students to schools in an efficient and equitable way, without systematically favouring certain individuals or groups. However, even when these mechanisms are structurally fair, understanding how they operate is crucial, especially when they require strategic behaviour, as is the case with the immediate acceptance mechanism in Bremen. According to theory, families are assumed to act as utility-maximising agents who weigh the expected benefits of different school options against the risks and constraints they face. When information is incomplete or misunderstood, these decisions become distorted. In this context, I investigate the impact of providing school assignment information in families' native languages, introduced in 2017, on school choice, admission outcomes, and selectivity for secondary schools. By reducing existing information gaps, the intervention enables more informed choices that converge toward rational decision-making.

A number of studies have conducted field experiments involving information interventions in school choice. These studies provide different groups of students and/or parents with varying degrees of information or test different channels of communication, consistently finding that information asymmetries exist and are particularly pronounced among disadvantaged or non-native speaking students (Hastings and Weinstein, 2008; Lucas and Mbiti, 2012; Corcoran et al., 2018; Cohodes et al., 2025). In contrast, this chapter exploits a natural experiment in the heterogeneous context of Bremen, where more than 50% of students transitioning to secondary schools have a migration background. I examine how an information intervention in families' native languages affects school choice and mitigates informational asymmetries in educational decision-making. To the best of my knowledge, this is the first natural experiment to study the use of language and translation as an information channel in this context.

Leveraging administrative data, I employ a difference-in-differences approach that exploits exogenous variation from students whose language at home was not included in the translated materials. The findings reveal that access to comprehensible information increased the likelihood of students ranking and attending academic schools rather than comprehensive schools, thereby reducing information disparities and barriers to equitable participation. The comparable effect size in ranking and attendance suggests that treated students successfully adapted their school choice behaviour. Additionally, I find evidence of deliberate segregation, as treated students tend to opt for more favourable school environments. The effects are heterogeneous, with the strongest impacts observed among high-performing students, female students, and recipients of welfare benefits.

For policymakers, this study demonstrates that even relatively low-cost interventions targeting informational disparities can have meaningful implications for educational equity and social mobility. Improving access to accurate and understandable information reduces uncertainty and allows families to better align their school choices with their preferences. Given that school choice in Bremen occurs at an early and pivotal stage in students' educational pathways, enhancing access to clear and comprehensible information allows families to make better-informed decisions. Centralised admission procedures offer an efficient way to allocate students to schools under excess demand, but for such systems to function equitably, families must understand both the rules and the potential pitfalls of the assignment mechanism.

Chapter 4 (*joint work with Mireille Kozhaya. Data preparation, analysis, and text were equally distributed between co-authors.*) shifts the focus to Beirut and is motivated by recent natural, military, and man-made disasters in the region, which have displaced large numbers of households and school-aged children. These crises have profound and lasting effects on children's educational trajectories. Evidence shows that interruptions to schooling during times of crisis often result in permanent withdrawal from formal education, thereby severely impeding long-term human capital accumulation (UNICEF, 2025). In the context of the 2020 Beirut explosion, the decision to withdraw a child from school may seem drastic, but for Syrian refugee households facing acute financial hardship, it becomes a rational response to immediate survival needs. Parents are often unable to delay action or wait for long-term benefits, as their utility is driven by pressing short-term constraints. When increasing work hours or securing additional income is not feasible, affected households, particularly the most vulnerable, resort to common coping strategies such as child labour and early marriage. While these strategies vary in form, they serve similar economic functions: child labour generates direct income, whereas marriage either brings in financial resources through dowry payments or reduces the number of household members that need to be supported. These decisions, while aiming to stabilize household welfare in the short run, represent severe trade-offs with potential long-term consequences for children's human capital accumulation and future opportunities, ultimately reproducing cycles of poverty and limiting prospects for upward mobility.

Most studies in the literature so far have focused on a wide variety of different outcomes for the general population experiencing this kind of external shock. For example, extensive research has examined the effects of major natural disasters, such as earthquakes or hurricanes, on household finances and individual earnings (Gallagher and Hartley, 2017; Groen et al., 2020), as well as on behavioural outcomes like patience (Callen, 2015) and risk preferences (Hanaoka et al., 2018). However, while these studies provide important insights into the broader economic and psychological consequences of such crises, they largely overlook the specific vulnerabilities of minority or refugee populations and their school-aged children (Elliott and Pais, 2006; Groen and Polivka, 2010; Doocy et al., 2011). These groups often face compounded risks and are typically excluded from targeted relief efforts, particularly in Lebanon, which is not a signatory of major international refugee conventions. As a result, the decisions for children, such as withdrawing them from school, are often made under severe short-term constraints, driven by immediate utility considerations rather than long-term human capital investment. This study seeks to address this gap by focusing on how refugee households respond to crisis conditions and the resulting decisions made for school-aged children.

In particular, this study investigates the Beirut port explosion in 2020 and employs a difference-in-differences approach, exploiting geographic variation to compare refugee households in Beirut with those in non-affected areas. It first examines economic outcomes such as employment, debt, food insecurity, and damaged shelters – shocks that directly undermine household stability and force families to make difficult decisions for their school-aged children. The analysis reveals significant economic fallout in terms of increased debt, damaged shelters, and food insecurity. To mitigate these adverse effects, families adopt coping strategies to manage the economic shocks, strategies that place a particular burden on children, as the study shows a rise in domestic violence toward children, a decline in school attendance, and an accompanying increase in child labour and early marriages. These responses, while often the only viable means for households to secure additional – or in some cases, any – income, come at the cost of children's education and safety, posing a serious threat to long-term human capital accumulation (León, 2012; Delprato et al., 2015; Sim et al., 2017; Jürges et al., 2022).

This chapter shows that populations living below the extreme poverty line are often forced to make drastic decisions to cope with the compounded vulnerabilities caused by any kind of disaster. Rather than investing in education, as envisioned by human capital theory, these households must prioritize immediate survival, focusing on securing basic needs and making difficult choices that often involve the youngest members of the household. These choices can severely disrupt children's educational trajectories. Therefore, the findings highlight the urgent need for targeted interventions by international aid to support refugee households more broadly, and to protect children's access to education during times of crisis in particular. Such measures are essential to safeguard children's well-being and long-term prospects, especially given the evidence that school dropout is often irreversible even once conditions stabilize.

2 The Costs of Losing Admission Lotteries – The Transition From Primary to Secondary School in Bremen

Abstract: Using individual-level panel data from the German federal state of Bremen, this paper studies the effects of losing secondary school admission lotteries under an immediate acceptance mechanism in a system with early academic tracking. Admission lotteries generate quasi-random variation when demand for schools exceeds available capacity, allowing for causal identification. We find that lottery losers incur substantial non-academic costs. They experience significantly longer home–school distances and are assigned to schools with different peer compositions, including higher shares of migrant students, welfare benefit recipients, and lower social index scores. In contrast, there is no systematic evidence of negative effects on short- or long-term academic outcomes, such as standardized test scores or school-leaving certificates. The burden of lottery losses is heterogeneous: welfare-eligible and native German students experience larger costs in terms of effect size, while students with a migration background are more likely to enter the lottery pool in the first place. Moreover, we document substantial non-compliance: native German students are more likely to opt out of the public system by enrolling in private schools to mitigate the consequences of lottery losses, whereas migrant and welfare-eligible students must bear these adverse effects. Overall, the results indicate that even fair and randomized assignment mechanisms can generate unequal welfare consequences, highlighting the importance of accounting for non-academic outcomes when assessing the efficiency and equity of centralized school assignment systems.

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2.1 Introduction

In the German tracking system, the vast majority of students leave primary school at the age of ten after fourth grade. Parents must make decisions regarding the secondary school type as well as the school itself. In addition to the public education sector, private schools contribute to the diversification of educational options, thereby enhancing the range of choices available to students and their families. The allocation of students to schools is predominantly decentralized at the school level or organized at the municipal level. In the German federal system, the responsibility for education falls under the jurisdiction of the federal states. Consequently, the mechanism for allocating students to schools is also determined at the state level. Nevertheless, student allocation remains a widely discussed issue in debates on how to improve both the quality and fairness of school admissions. This paper utilizes the city state of Bremen to examine the consequences of implementing the immediate acceptance mechanism. Of particular interest are the short- and long-term costs for students who lose the tie-breaking lottery, as these students dominate the public debate.

Bremen, a city state in the northern part of Germany, has used an immediate acceptance mechanism with multiple ties broken by lotteries for its public secondary schools since 2011. Parents are invited to choose up to three schools. Approximately 3,600 students transition from primary to secondary school each year, and the lottery affects a sizable number of students. The percentage of students who are not assigned to their first-ranked school in each year varies between 10 to 13%, while about 2.5 to 6% are not assigned to any of their three ranked schools, causing exasperation and complaints. Some of the complaints are due to strong preferences for schools, but some parents complain because they do not understand why they lose the admission lottery. These issues are well known – in educational policy but also in the literature. Nevertheless, school boards apply matching algorithms with lotteries to place students in schools with excess demand worldwide (e.g. Amsterdam, Barcelona, Boston, Chicago, London, Shanghai), since matching algorithms are perceived to be efficient and fair.

There is an extensive body of research in economics and education examining the impact of school assignment mechanisms on both academic and non-academic student outcomes (Cullen et al., 2006; Hastings et al., 2006; Hastings et al., 2012; Deming, 2014; Angrist et al., 2016; Bibler and Billings, 2020; Ketel et al., 2023). However, there is no clear consensus on whether losing an admission lottery affects students' educational trajectories or leads to other significant costs. This study contributes to the debate by analysing the short and long-term consequences of losing one or multiple admission lotteries in a heterogeneous school environment, using detailed longitudinal administrative data. First, we examine eleven consecutive years without any changes to the school assignment process, allowing for robust identification and mitigating concerns about fluctuations in school demand. This enables us to study a sample of almost 14,000 students who participated in admission lotteries. Moreover, since the risk of losing a lottery occurs across different types of schools, our analysis covers the entire student population rather than focusing on a specific school type. We argue that some of the observed effects

are driven by the heterogeneity in student backgrounds and their different levels of understanding regarding strategy incentives of the applied matching assignment in use. This also means that while the admission lottery itself is random, it is not random who enters the lottery pool. Second, in contrast to previous studies, our data enables us to follow students both in the public and in the private school sector. This allows us to assess whether switching to private schools serves as a strategy to mitigate the costs of losing a lottery, or whether students do not comply to the allocation of schools and are observed in their preferred schools years later. Third, in addition to non-academic outcomes, our dataset permits the analysis of academic outcomes, both short- and long-term. These perspectives are particularly relevant given the early timing of secondary school transitions at the age of ten, as students are exposed to the secondary school for a longer duration. These contributions are possible due to detailed information on all students, including standardised test scores, birthdate, gender, country of birth, language spoken at home, attended school and class, school type, school choice, special educational needs, eligibility for welfare benefits and school leaving certificates. Thus, our unique data is rich and suitable to answer the question of short- and long-term effects of losing admission lotteries.

Comparing lottery winners to lottery losers, we find evidence that losing the admission lottery results in a variety of adverse effects, e.g. home-school distance, peer networks and school characteristics of assigned schools. Losing the lottery in the first round, i.e., not being assigned to the first-ranked school, results in an increase in the home-school distance by roughly 390 meters (17%, compared to the control mean); losing the lottery in the second and third round as well, hence, not being assigned to either the second- or third-ranked school, leads to an increase of up to 28% in home-school distance. In addition, on average, losing the lottery results in a loss of around 2 primary school peers when transitioning to a secondary school that was not the first choice. Other social costs relate to the different social composition of the schools. Losing the first admission lottery results in attending a school with a higher share of students eligible for welfare benefits (8.5 percentage points (p.p.)) and a higher share of migrant students (8.3 p.p.). Students then also attend a school with a worse expected final GPA and a lower socioeconomic status of the school. To analyse academic outcomes, we investigate students' standardised test scores in grade 8 (three years after the transition from primary to secondary school) and, for possible long-term effects, the final school-leaving certificates (five to eight years later). It turns out that there is no systematic effect of losing the admission lottery on standardised test scores in grade 8. We also find no adverse effect on long-term outcomes; lottery losers are overall not less likely to graduate with a certain school-leaving certificate. However, being admitted to a school lower on the rank order or not on the list at all still incurs high non-academic costs. There is also evidence for heterogeneous effects. Some groups of parents understand the applied mechanism and the pitfalls better than others² or

² Kapor et al. (2020) show that only around 10% of parents understand the priority system, 20% the ranking penalty, while only around 5% state that the applied immediate acceptance mechanism is difficult to understand. Zuchanek (2025) shows in Bremen that students with migration background did not consistently have the same access to information about the assignment mechanism.

are more penalized if their strategy fails. For example, migrant students are more likely to enter the lottery pool and compete in highly oversubscribed schools, whereas native German students tend to incur larger costs (in terms of effect size) due to strong preferences for more homogeneous school environments. In general, migrant students are 65% more likely to face costs associated with losing admission lotteries. Finally, we observe that some of the students who lose the lottery do not comply to the assignment. Foremost native German students are either observed in a different school later in their school career or attend a private school with a more homogeneous school environment.

2.2 Student Matching in the Federal State of Bremen

Each federal state in Germany is responsible for its own education policy. The federal state of Bremen is composed of two cities: the city of Bremen (onwards: Bremen) with a population of 577,026 and Bremerhaven with a population of 119,357 (as of 2024). In Bremen, compulsory education starts at the age of six. Around the age of ten, students switch from primary to secondary school, and parents can choose their children's secondary school. The secondary school is the school that children typically attend until they finish schooling. The secondary school system in Bremen, unlike in many other German states, tracks children only into two types of schools: the academic track school (*Gymnasium*) and the comprehensive track school (*Oberschule*). As of 2021, there are eight academic schools, 34 comprehensive schools and ten private schools that offer a secondary school program. Both school types are publicly funded and managed, and no fees are charged. Although there are differences between the two types of public secondary schools, students can obtain the same school-leaving certificates. Students also have the possibility to opt-out to a private school or to leave Bremen's school system to receive education in the neighbouring federal state.³

The Bremen Ministry of Children and Education (onwards: school board) adopted an immediate acceptance mechanism with multiple tie-breaking based on a variation of the Boston mechanism, described in Abdulkadiroğlu and Sönmez (2003). When opting for public secondary schools, parents submit an application, ranking at most three schools (rank order list, onwards: ROL). Once all applications have been submitted, applicants are assigned to the school they have ranked first, provided the number of applications does not exceed the number of seats. However, if the number of applicants is greater than the number of seats for that school, applicants are given priority depending on academic performance. In addition, for admission to comprehensive schools, it is also relevant whether the primary and the secondary school are affiliated (onwards: affiliation). Each primary school has at least one affiliated comprehensive school, typically the schools that are closest to the primary school. Applicants with higher priority are accepted first, and if the school has no seat left afterwards, the remaining students are rejected. If there is excess demand within a priority group, a lottery is conducted within the priority

³ Appendix I-A provides further information on the two different school types and the admission.

group. The multiple tie-breaking rule is used, meaning that ties are broken within each priority group through the random lottery in every school independently (Ashlagi and Nikzad, 2020). In the next round, rejected applicants apply for the remaining seats in the second-ranked school. If there is excess demand, seats are again assigned according to the schools' priority lists and the lottery, as described above. However, if there are no remaining seats left, the third-ranked school is considered. The process is repeated in the third round. If students cannot be assigned to any of the schools on the ROL after the third round, parents will receive a notification from the school board. Following a counselling session, they will be offered another school place (onwards: allocated school). The wait list for schools that have reached their full capacity remains active until the end of the first term of the respective school year.

Parents also have the possibility to opt out of the city-regulated school assignment procedure, either by enrolling their child in a school outside the city state⁴ or by choosing one of the private schools located in Bremen. Families may bypass the assignment procedure entirely and apply directly to a private school (not included in the sample as without submission of a ROL), switch after receiving the assignment results, or transfer later during the child's secondary school career. Private schools follow a decentralized admission process, deciding on a case-by-case basis whether to admit students, for example, according to religious affiliation, suitability for specific pedagogical concepts, or certain performance requirements. For non-compliance of the admission lottery, **Chapter 2.6.3** focuses on cases in which students switch schools after participating in the lottery. During the observation period, ten private schools offered secondary education tracks. Although some of these schools present themselves as academic track institutions, students can generally obtain the same school-leaving certificates across all private schools. The schools are distributed throughout Bremen, and on average, parents are required to pay monthly tuition fees of approximately 340 EUR.⁵

2.3 Effects of Matching Assignments

When students switch from one educational stage to another, different versions of matching assignments are used by school boards worldwide. The assignments usually differ in terms of their mechanism (e.g., immediate or deferred acceptance), priority criteria for oversubscribed schools, the number of seats reserved for priority groups, or the maximum number of schools that can potentially be ranked on the submitted ROL. What they have in common is that once the school is oversubscribed, ties are broken by lottery. In Bremen, annually, about 89% of assignments are resolved in the first round on average. This is in line with results from other cities worldwide despite the variations in the immediate acceptance

⁴ The city of Bremen is an enclave within the federal state of Lower Saxony.

⁵ Median tuition fees for the first child. Data from the 2023/24 and 2024/25 school years, based on private schools with publicly available tuition fees. Since tuition fees may vary across grade levels, we only considered entry-level grades for secondary school and only included private schools that offer secondary school tracks. While most families pay the regular tuition fees, in rare cases where economic hardship applies, families may apply for a fee reduction at some schools, although this remains an exception rather than the rule.

matching assignments (e.g., Calsamiglia and Guell, 2018; Terrier et al., 2026). Therefore, several studies investigate lottery outcomes, but whether or not admission lotteries cause adverse effects remains disputed.

Angrist et al. (2016) investigate admission lotteries in Boston's charter schools and find positive effects on educational performance for lottery winners. In contrast, Hastings et al. (2006) find no noticeable negative impact on reading abilities or math scores from losing the admission lottery. For other outcomes, such as increasing homework time and reducing grade retentions, the effects are minor. However, by investigating subgroups based on preferences for academic quality, the authors conclude that students whose parents have weaker preferences face significant academic losses. For adverse effects in Charlotte-Mecklenburg County, Bibler and Billings (2020) examine residential relocations after switching from kindergarten to primary school and from primary to secondary school. They find that lottery losers are 6 p.p. more likely to opt-out of the public school system, and by doing so, parents are willing to pay a 9% to 11% housing premium to attend a school with higher mean test scores. In the same county, Deming (2014) tests the validity of school value-added models by exploiting the randomness of admission lotteries and concludes that they are good predictors for student's achievement. Hastings et al. (2012) find that the admission notification to the preferred school itself reduces absenteeism rates, particularly for boys, and test scores in general. Cullen et al. (2006) investigate the randomized lottery outcomes for high school admission within public schools in Chicago. They conclude that lottery winners take advantage of various dimensions, such as peer achievement or attainment levels. However, traditional academic outcomes, such as graduation rates, test scores, or school attendance, are not affected.

The most recent and closely related study to ours is Ketel et al. (2023), who analyse the consequences of losing an admission lottery for university track schools as a multidimensional treatment in Amsterdam. They find that lottery losers face disadvantages in terms of increased home-school distance, less favourable teacher and peer characteristics, and overall weaker school profiles. However, for academic outcomes and a range of other variables, some of which are collected via surveys, they find no additional significant effects. The key distinction between their study and ours lies for one thing in the institutional setting: Ketel et al. (2023) examine a deferred acceptance system with first multiple- and then single tie breaking over six years, in which truth-telling is a dominant strategy (Abdulkadiroğlu and Sönmez, 2003). In contrast, our study focuses on an immediate acceptance system with multiple tie breaking over eleven consecutive years with no changes, where families rank up to three schools in a potentially strategic manner. Additionally, while Ketel et al. (2023) restrict their analysis to university-track schools, where capacity constraints are most severe, we include all students across both academic and comprehensive secondary schools in Bremen. This broader scope enables us to capture heterogeneity across the entire student population and in particular also the more disadvantaged groups, thereby enhancing the robustness of our results to fluctuations in school demand. It also offers a possible

explanation for differences in findings, as students entering the university track may be less vulnerable to disruptions in their educational pathways. We also add to the previous study by following students throughout both their primary and secondary school careers, including movements into private schools. Therefore, we analyse private school enrolment as a potential opt-out strategy after a lottery loss and quantify the consequences of such choices. This is particularly relevant in strategy-incentivising mechanisms, where some parents with the financial means for this outside option may be willing to take greater risks in their applications (Akbarpour et al., 2022). Finally, because the school tracking process in Bremen begins earlier, around age of ten, disruptions may have more meaningful and persistent effects, as students are exposed to these conditions in a sensitive phase of development and for a longer duration.

2.4 Data

We use administrative individual-student level data, provided by the school board of Bremen. We use data from 2011 to 2021, covering all periods after the introduction of the school matching procedure. The data contain information on students' characteristics, such as birth date, gender, eligibility for welfare benefits⁶, first and second citizenship, country of birth, mother tongue, language spoken at home, place of residence (city and building block), attended school and class, school type, special educational needs, and other variables.

To evaluate students' performance after the transition from primary to secondary school, we focus on the VERA-8 test, which is the only nationwide standardized performance assessment in Germany (Schult, 2022). This standardised test is conducted in the third year following the transition to secondary school (grade 8). The linkage of this data with the individual-student level data was only implemented in 2019; therefore, we can only use data on VERA-8 from 2019 to 2023, covering students who transitioned from primary to secondary school in $t - 3$, i.e. in the years 2016 to 2020. Hence, we can make use of standardised test scores for five years. We use the VERA-8 tests to assess student achievement in Mathematics and German.⁷ In each subject, different test booklets are administered depending on the school type and performance. We do not distinguish between these booklets in our analysis, as they are standardized, allowing for comparisons across school types. For German, we differentiate between two skill domains: reading and spelling. Depending on the number of correctly answered questions, a level between 1 and 6 is assigned, with 6 representing the highest level.⁸ For a long-term perspective on student performance, we further examine the types of leaving certificates attained by the

⁶ *Bremen-Pass (Bildungs- und Teilhabepaket)*: This pass is for low-income households and is intended to enable families to participate in social and cultural life.

⁷ English test booklets (in Reading and Listening Comprehension) cannot be combined within or across years, since the scaling procedures and the number of booklets differ between cohorts. As a result, no consistent and comparable measure for English test scores over time can be constructed, which is why we exclude this subject from the analysis.

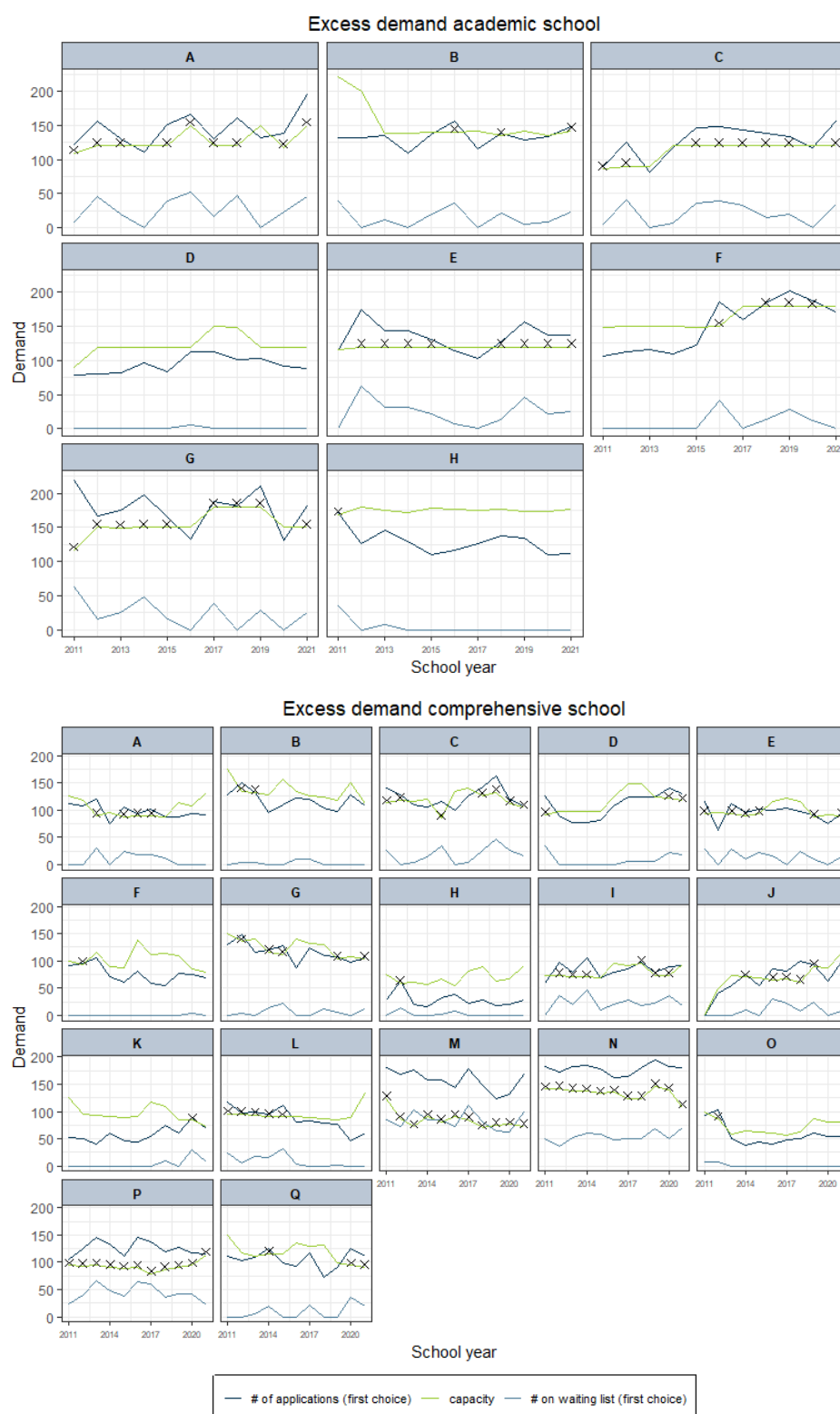
⁸ Descriptive Statistics on the VERA-8 standardised test scores in **Table 2-6-A**.

first four cohorts (2011 to 2014). These include the university entrance certificate (*Abitur*) after seven to eight years after the transition to the secondary school; the intermediate certificate (*Mittlerer Schulabschluss*) after six years, which is awarded after completion of lower secondary education and qualifies students for vocational training or continued education in upper secondary tracks; and the basic certificate (*Einfache Berufsbildungsreife*) after five to six years, which represents the minimum school-leaving qualification and does not entitle students to progress to upper secondary education. Lastly, we investigate whether students leave schools without any school-leaving certificates.⁹

On the school level, we also have information on the average GPA of leaving certificates (central examinations). In Germany, the GPA of leaving certificates can vary from 1.0 (very good) to 4.0 (sufficient) – exactly the opposite of the grading system in Anglo-Saxon countries. A final grade of 5.0 is a failing grade. We observe variations in average GPAs across secondary schools, ranging from 1.97 to 3.38. Other information on school level are the index of social composition (for primary [social index A] and secondary schools [social index B])¹⁰ and the school's address. We calculate the home-school distance for all schools and for each student using the Euclidean distance between the centroid of the students' city block and the exact location of each school. Lastly, we have information on the capacity of each school for each cohort, calculated through the maximum number of students per class.

⁹ According to International Standard Classification of Education (ISCED), *Abitur* is considered to be level 3 and *Mittlerer Schulabschluss* and *Einfache Berufsbildungsreife* are level 2.

¹⁰ This is an index for needs-based/weighted school funding. The higher the index, the more additional funds. The index is ordinal and varies between 1 and 5. Social index A according to exit cohort of the primary school; Social index B according to the secondary school.

Figure 2-1 Excess Demand

Note: The figure shows the number of first-ranked applications, the number of students on the wait list for the first choice and the capacity, which means 30 [25] students per established class minus pre-allocated students with special educational needs across all academic schools [all comprehensive schools with excess demand] over the whole observation period. 17 out of 34 comprehensive schools show excess demand in at least one year. Excess demand is marked with X. We refrain from reporting at the school level using unique school identifiers or the school name.

According to the school board, academic schools in Bremen may admit no more than 30 students per class, while comprehensive schools are limited to a maximum of 25 students per class. **Figure 2-1** displays the number of first-ranked applications, the number of students on the wait list (first-choice), and the total capacity across all academic schools and those comprehensive schools that show excess demand throughout the entire observation period (17 out of 34 comprehensive schools). While the figure shows that a few schools consistently face excess demand, it also reveals non-monotonic year-to-year variation, indicating volatile demand without a clear upward or downward trend. This pattern suggests that parents base their application decisions on information from the previous year(s). Thus, a year of excess demand is often followed by a year of lower demand, possibly reflecting a reaction to rejections at oversubscribed schools in the preceding year(s). Given our eleven-year observation period, the analysis accounts for these fluctuations and does not primarily reflect the effects of exceptional years, such as those influenced by migration influxes, capacity changes, or unusually large or small birth cohorts. In the robustness checks in **Chapter 2.7**, we further explore year-to-year variation and examine whether the estimated effect occurs only in a specific year or extends across multiple years.

Students with special educational needs (8% of all students) are distributed prior to the general student matching assignment.¹¹ Students who do not submit a ROL (about 5% of all students per year) usually either immediately attend a private school, which is not part of the matching mechanism, remain in primary school (grade retention is voluntary) or are allocated by the school board after the centralised admission procedure. These students cannot lose admission lotteries, because they do not participate in the regular student matching assignment and, hence, are excluded from our analysis. We also observe that annually around 40 students make use of the wait list and are ultimately admitted to their preferred, higher ranked school despite initially losing the lottery for that school. These students are also excluded from the analysis, as their inclusion would bias the estimated effects. This ensures that the impact of genuinely losing the admission lottery and not being admitted to the ranked school is captured.

¹¹ Both school types accept up to ten percent so-called hardship cases. These are students a) which have special educational needs, b) who or whose family would be socially harmed by attending another school, or c) with siblings in the chosen secondary school. Note that, according to the school board, such cases are identified under a compelling necessity and are rarely approved after assessment.

Table 2-1 Descriptive Statistics

	All participants (1)	1 st round of the lottery (2)	2 nd round of the lottery (3)	3 rd round of the lottery (4)
a) <u>Student characteristics</u>				
Migrant	0.501	0.515	0.713	0.662
Female	0.493	0.492	0.476	0.482
High performance	0.351	0.377	0.051	0.033
Eligibility for welfare benefits	0.317	0.312	0.433	0.371
Age	10.61	10.61	10.61	10.62
b) <u>Average home-school distance (in meters) to</u>				
Closest comprehensive school	1,007.46	952.79	798.22	904.36
Closest academic school	3,044.65	3,291.20	3,890.18	3,704.95
1 st choice on ROL	1,998.60	2,180.35	-	-
2 nd choice on ROL	2,533.00	2,538.22	2,580.85	-
3 rd choice on ROL	2,930.94	2,880.92	2,862.16	2,301.04
Allocated school	3,007.62	2,895.93	2,454.03	2,791.27
Attending school	2,000.95	2,261.32	2,577.30	2,373.50
c) <u>Assignment results</u>				
1 st choice on ROL	0.887	0.774	-	-
2 nd choice on ROL	0.066	0.146	0.654	-
3 rd choice on ROL	0.021	0.039	0.213	0.880
Allocated school	0.027	0.042	0.134	0.120
d) <u>Lottery winners and losers</u>				
Winners	24,224	10,670	166	242
Losers	-	3,122	88	33
Observations	39,350	13,792	254	275

Note: The table presents descriptive statistics for those 39,350 students who participated in the student matching assignment between 2011 and 2021 in the city of Bremen. In Column (2) to (4), observations are reduced to students who participated in the respective lotteries. In the observation period, 1,334 students are excluded from the first admission lottery due to a low priority group and 1,064 students have not been assigned to any school on the ROL.

Therefore, **Table 2-1** provides descriptive statistics for 39,350 students who handed in a ROL between 2011 and 2021. Column (1) Panel a) shows that around 49% are female and 32% are eligible for welfare benefits. Half of the students are migrants.¹² About 35% perform above a given threshold in Mathematics and German at the end of primary school (onwards: high-performing). In the same Panel a), Columns (2) to (4) present data for students participating in the first, second and third round of the lottery, respectively. While the share of high-performing students decreases, the share of welfare eligible students and migrant students increases in the second admission lottery. This trend continues when comparing the first and the third admission lottery.

Panel b) shows the calculated distance to the different schools. Column (1) indicates that the average distance between students' homes and the closest comprehensive school is about 1 kilometre. The average distance to the closest academic school is 3 kilometres. The distance to the first-ranked

¹² If one of the following criteria applies, we define the student as a migrant: a) has another or a second citizenship in addition to German citizenship, b) the place of birth is not Germany or c) mother tongue or the language spoken at home is not German.

school averages 2 kilometres. Columns (2) to (4) in Panel b) show that home-school distance increases from 2 kilometres (first-ranked school) to a maximum of 3 kilometres (allocated school).

Panel c) provides information on the assignment results.¹³ On average, 88.7% are assigned to the first-ranked school, while only 6.6% and 2.1% are assigned to the second- and third-ranked school, respectively. 2.7% do not get any of the ranked schools on the ROL and are allocated by the school board after the matching assignment. Participating in the admission lottery reduces the chance of being admitted to the first ranked-school to 77.4% and increases all other options accordingly (Column 1, Panel c)). In the last lottery, the student has a chance of 88.0% to get the third-ranked school (Column 4, Panel c)).

Lastly, Panel d) gives information about lottery winners and losers. Out of the 39,350 students, 24,224 rank a safe school¹⁴ as their first choice on the ROL and, thus, are not at risk of participating in any admission lottery. Another 1,334 students are excluded from the first admission lottery due to a low priority group, thus the remaining 13,792 students in Column 2 participate in the first round of the lottery. Of these, 3,122 students lose the lottery. After the first lottery round, most schools reach full capacity, which is why the number of students who are in the second and third admission lottery is small. Column 3 in Panel d) show the participants of the second admission lottery and Column 4 the third and last admission lottery. Only 275 students participate in the last admission lottery, while 1,064 are not admitted to any school on the ROL as their second- or third-ranked school have no more seats left after the prior round(s).

It is important to note that while the lottery itself is random (**Appendix I-D, Table 2-9-A**), access to the lottery is not. That is, some students are more likely to apply to oversubscribed schools and thus enter a lottery, while others are more inclined to choose safe schools and are therefore immediately accepted without facing the risk of lottery rejection. Possible explanations include strong preferences, a greater willingness to take risks or a lack of critical information about the assignment mechanism and its consequences. **Table 2-8-A (Appendix I-D)** presents the results of a t-test comparing students who end up in the lottery pool and those who do not and, therefore, do not participate in an admission lottery. Significant differences emerge, particularly with respect to preference for academic schools, performance and migration background. Academic schools tend to be more competitive and, thus, conduct lotteries more often. High-performing students form in both school types the first priority group and, hence, they compete more often in admission lotteries compared to low-performing students, who are in the second priority group for comprehensive schools and in no priority group in academic

¹³ Further information: about 77% of students rank the maximum possible number of three schools on their ROL, as advised by the school board. Overall, 30% rank academic schools first, and around 26% of all students attend an academic school (comprehensive schools respectively).

¹⁴ A so-called 'safe school' is a school, where admission probability equals one (de Haan et al., 2023). Hence, all students are assigned to this school in the first round of the matching assignment. Those schools do not experience excess demand. Note however, that the safe school status varies over time, depending on demand in a particular year.

schools. That students with migration background are overrepresented in the lottery pool is consistent with previous research, such as Zuchanek (2025), which shows that these students have not had equal opportunities to fully understand the immediate acceptance mechanism in Bremen. Consequently, the costs of the lottery differ not only in effect size (Effect Heterogeneity in **Chapter 2.6.2**), but also in frequency, as, for example, students with migration background are more likely to enter the lottery pool than their German counterpart ($36.67\% > 34.10\%$) – increasing the likelihood that estimated costs arise in the first place.

2.5 Empirical Strategy

Utilizing lottery-induced randomization excludes the issue of endogenous sorting among students. Unlike the entrance into the lottery pool, lottery wins are random and independently from students' characteristics within their priority group, hence, students who win or lose in the admission lottery should be identical in both observable and unobservable characteristics. We analyze the observed differences in student outcomes between lottery winners and losers, and by doing so, we can yield a reliable estimate of the impact of losing the admission lottery.¹⁵ In our model, we (1) compare being assigned to the first-ranked school with not being assigned to the first-ranked school, and then (2) being assigned to the p^{th} -ranked school with not being assigned p^{th} -ranked school, where p is the preference ($p \in (2, 3)$) on the ROL or the allocated school $p > 3$. Thus, due to the nature of the immediate acceptance algorithm, p denotes the rounds of the lottery. In round 1, students are allocated based on their first choice and in round 2 and 3 allocation is based on the second and third choice, respectively. Note, that the number of students who participate in each round decrease in the immediate acceptance algorithm (**Table 2-1**).

To assess the cost of losing the lottery, we first estimate the following linear model for the outcome variables Y_{ist} , for instance home-school distance, of student i for whom school s was the first preference on the ROL in t , where t denotes the admission lottery cohort ($t = 2011, \dots, 2021$):

$$Y_{ist} = \beta D_{ist} + \delta X'_{ist} + \lambda_t + \mu_s + \varepsilon_{ist} \quad (1)$$

Besides home-school distance, the outcome Y_{ist} captures characteristics of the attended secondary school in comparison to the preferred secondary school, i.e. the share of migrant students, the share of students eligible for welfare benefits and the school's social index (social index B) as well as our measures of academic achievement.

D_{ist} is a dummy variable defining the loss of the lottery. It equals one if student i in cohort t is not assigned to her or his first-ranked school s , and zero otherwise. X'_{ist} are individual characteristics, such as age, place of residence, gender, performance, migration background, eligibility for welfare

¹⁵ Note that the probability of participating in a lottery is only conditioned on the demand for a particular school and the priority group of a random student.

benefits and attended primary school. We control for student characteristics, since the outcomes do not only depend on the lottery, but also on other variables, such as the place of residence and, therefore, proximity to certain schools and the demand for the first-ranked school. λ_t are cohort fixed effects and μ_s are first-ranked school fixed effects. ε_{ist} is the error term. While we use first-ranked school fixed effects to account for the magnitude of excess demand – and thus the probability of admission – we also discuss alternative specifications in **Chapter 2.7**.¹⁶

The validity of our empirical strategy depends on the randomness of the lottery, even though access to the lottery itself is unequal. Hence, we checked for the randomness of the lottery draws (**Appendix I-D, Table 2-9-A**) and can conclude that the conditional mean independence assumption holds. Accordingly, β measures the causal effect of not being assigned to the first-ranked school s on the ROL by losing the admission lottery. We restrict the sample to students who participate in at least one admission lottery, which excludes students whose first-ranked school is a safe school that does not reject any students.¹⁷

For $p \in (2, 3)$ on the ROL and the allocated school $p > 3$, we follow the approach of Ketel et al. (2023) based on Abdulkadiroğlu et al. (2017) and categorize not being assigned to the first-ranked school into three groups: i) assigned to the second-ranked school, ii) assigned to the third-ranked school and iii) allocated to a school that is not on the ROL ($p > 3$). To compare equals with equals, we need to account for fixed effects of first-ranked schools (as in in Eq. (1)) and the probabilities of being assigned to the second- or third-ranked or to the allocated school. The probabilities of being assigned to these schools depend only on the priority group and the preference p , which in turn reflects the demand for that school. In line with Abdulkadiroğlu et al. (2017), we compute these probabilities (propensity scores) by executing the immediate acceptance mechanism with multiple tie breaking exactly as it has been implemented in Bremen since 2011. To account for randomness introduced by the lottery, we run the simulation 1,000 times. Then, we average the assignment rates for the second- and third-ranked schools and due to the large number of draws, these average rates converge to the actual scores. With these propensity scores we can now properly compare equals with equals, meaning students with the same probabilities to lose the second- and/or third-ranked admission lottery, and attribute any differences in the outcome variables to the loss of the admission lotteries. Hence, the regression model is:

$$Y_{ipst} = \beta_2 R_{ist}^2 + \beta_3 R_{ist}^3 + \beta_{>3} R_{ist}^{>3} + f(\varphi_i^2, \varphi_i^3) + \delta X'_{ist} + \lambda_t + \mu_s + \varepsilon_{ipst} \quad (2)$$

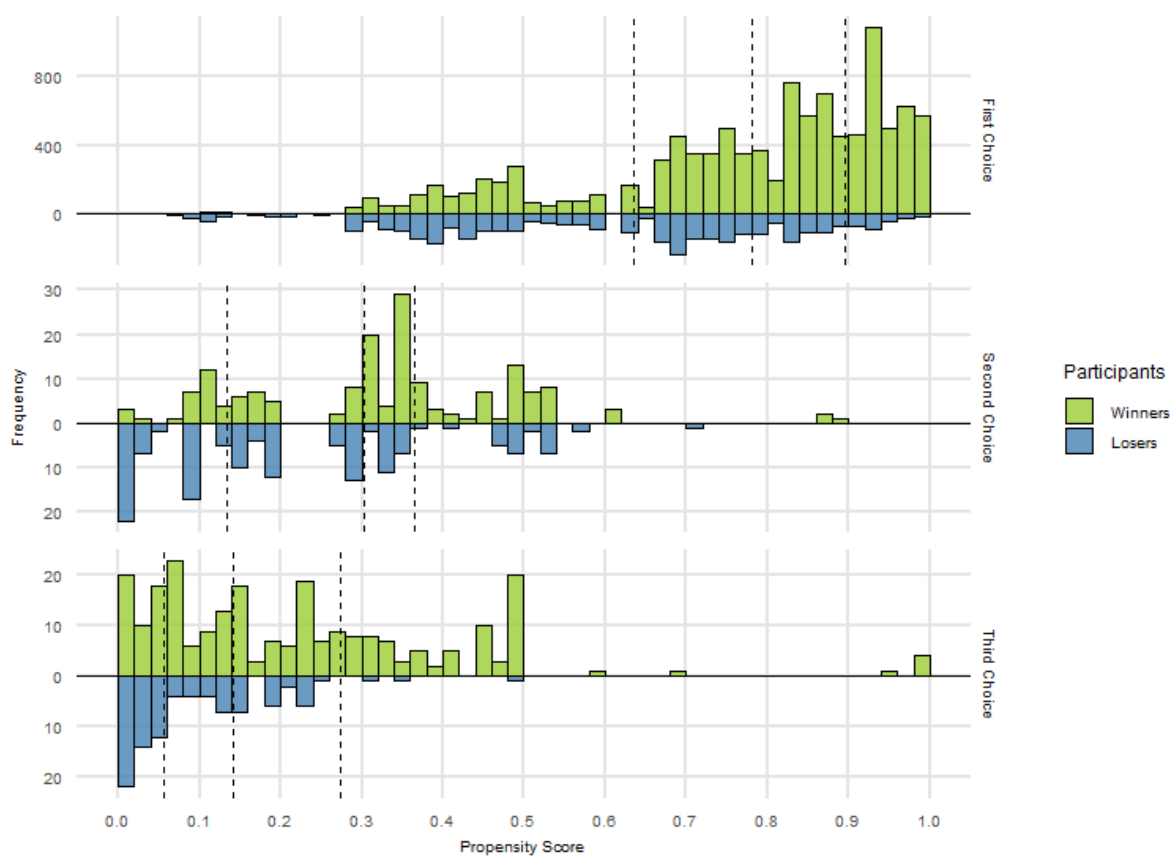
¹⁶ For example, if 101 high-performing students rank the same academic school – offering 100 seats – as their first choice, the probability of admission for each student is approximately 99.01%. In contrast, if 200 high-performing students compete for the same number of seats, the individual probability of admission drops to 50%.

¹⁷ With that we also exclude the very few cases of students that are not participating in any admission lottery. This can happen, when students only rank popular schools for which they are not in the first priority group and which schools have no capacity in the second round, e.g. a low-performing student ranks three popular academic schools or another student ranks only comprehensive schools which are not affiliated with the students' former primary school.

where R_{ist}^p is 1 if student i was assigned to the p^{th} -ranked school s in year t and zero otherwise, φ_i^p represents the propensity score in the form of quantile dummies of student i being assigned to the p^{th} -ranked school. Hence, $f(\varphi_i^2, \varphi_i^3)$ are four dummies for the quantiles of φ_i^2 interacted with four dummies for the quantiles of φ_i^3 . At last, β_p is the causal effect of being assigned to the p^{th} -ranked school.

A drawback of this method in our first-choice maximising immediate acceptance setting is the smaller variation of matches resolved in the second and third round in contrast to the deferred acceptance mechanism as most matches are resolved prior to rounds two and three.¹⁸ However, on the quantile level, β_p yields reliable estimates. In **Figure 2-2**, the distribution of the propensity score for $(p \in (1, 2, 3))$ on the ROL is shown. The distribution of admission probabilities to the respective school on the ROL is depicted for all students that participate in any admission lottery. The bars above the x-axis represent students who are admitted to the respective school; the bars below represent those who are not admitted. The frequencies (y-axis) for the first-ranked schools are higher than for the second- and third-ranked schools due to the implemented immediate acceptance method. For the second- and third-ranked schools, there are less students left and capacities are nearly exhausted. The vertical dotted lines depict the quantiles used in the main results of Eq. (2).

¹⁸ See Abdulkadiroglu et al. (2017) for formal proof that immediate acceptance satisfies the equal treatment of equals property.

Figure 2-2 Admission Probability for Schools on the Rank-Order List

Note: Each plot shows the distribution for the probability to be assigned to the school on the ROL (first, second, and third choice) for students who were assigned to that school (above the x-axis, green) or not (below, blue). Students with assignment probabilities equal to 0 or 1 are excluded. The vertical dotted lines depict the quantiles used in Eq. (2).

2.6 Results

2.6.1 Main Findings

Our results in **Table 2-2** show that losing the admission lottery affects students in a large set of outcomes, while for academic achievement, we find no effect. **Table 2-2** Column (1) shows results based on Eq. (1) for the first-round lottery and Column (2)-(4) present results for being admitted to the second, third or no school on the ROL, according to Eq. (2).

Table 2-2 Effects of Losing Admission Lotteries

Outcome	Lottery winner Mean [S.D.]	Not admitted 1 st ranked (1)	Admission 2 nd ranked (2)	Admission 3 rd ranked (3)	Not admitted to ROL (4)	N
a) Distance to secondary school						
Home-school distance (in meters)	2,254.74 [2,125.23]	391.80*** (44.75)	493.30*** (48.78)	331.50*** (113.50)	637.40*** (136.50)	13,175
b) School characteristics of secondary school						
Social index B	2.445 [1.280]	0.235*** (0.023)	0.078*** (0.026)	0.420*** (0.052)	0.464*** (0.069)	13,175
Share of welfare benefit students	0.276 [0.178]	0.085*** (0.003)	0.055*** (0.004)	0.127*** (0.007)	0.133*** (0.009)	13,175
Share of migrant students	0.416 [0.163]	0.083*** (0.003)	0.064*** (0.004)	0.104*** (0.007)	0.104*** (0.010)	13,175
Average GPA (secondary school)	2.451 [0.181]	0.063*** (0.003)	0.043*** (0.004)	0.071*** (0.008)	0.111*** (0.011)	11,878
c) Potential peers in secondary school						
Potential peers 1 (language)	57.540 [44.928]	-8.757*** (0.674)	-5.501*** (0.793)	-12.320*** (1.566)	-13.910*** (1.593)	13,175
Potential peers 2 (language, class)	12.202 [9.075]	-2.663*** (0.149)	-2.043*** (0.184)	-4.195*** (0.339)	-3.086*** (0.333)	13,175
Potential peers 3 (language, class, sex)	6.634 [4.916]	-1.416*** (0.081)	-1.106*** (0.101)	-2.272*** (0.181)	-1.649*** (0.177)	13,175
d) School entry cohort composition⁺						
Peers A (language)	8.057 [7.210]	-2.379*** (0.140)	-2.245*** (0.182)	-2.465*** (0.294)	-2.997*** (0.303)	13,175
Peers B (classmates)	5.830 [3.941]	-1.825*** (0.081)	-1.773*** (0.102)	-2.014*** (0.182)	-2.016*** (0.183)	13,175
Social index A	2.779 [1.302]	-0.025*** (0.010)	-0.029*** (0.012)	0.012 (0.021)	-0.012 (0.024)	13,175
e) Standardised test scores						
Mathematics	2.925 [1.569]	-0.040 (0.215)	-0.012 (0.221)	0.261 (0.360)	-0.662 (0.472)	1,523
German (Reading)	3.399 [1.522]	0.105 (0.754)	-0.105 (1.077)	-1.590 (1.024)	0.906 (0.968)	444
German (Spelling)	3.203 [1.267]	-0.637 (0.520)	-0.147 (0.740)	-1.584** (0.637)	-0.926 (0.815)	444
f) Long-term outcome						
University entrance certificate	0.481 [0.500]	-0.021 (0.023)	0.003 (0.029)	0.034 (0.051)	-0.057 (0.053)	4,898
Intermediate certificate	0.236 [0.425]	0.023 (0.023)	0.016 (0.030)	-0.027 (0.052)	0.062 (0.052)	4,898
Basic certificate	0.194 [0.388]	-0.030 (0.020)	-0.045* (0.028)	-0.019 (0.048)	-0.045 (0.044)	4,898
No school-leaving certificate	0.079 [0.268]	0.017 (0.012)	0.016 (0.016)	0.013 (0.025)	0.022 (0.030)	4,898

Note: ⁺School entry cohort composition is based on information of primary school exit cohort. Lottery winner mean: students who participate in the first admission lottery and win. Number of observations differs due to missing values: average GPA is not available for the first year of 2011, school-leaving certificates from 2011 to 2014, standardised test scores from 2016 to 2020 (only students for whom the VERA-8 standardised test was compulsory). Full set of covariates and fixed effects. Robust standard errors in parentheses. Standard deviations in brackets. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

As to be expected, losing the lottery in the first round results in a significant increase in the distance to the assigned school (Column 1). Lottery losers are assigned to a school that is on average 392 meters further from their home, relative to the average distance in the control group of 2.2 kilometres, which translates into an increase in the commuting time. Losing the admission lottery in the first round also leads to a significantly different school composition (Panel b)). The social index B of secondary school increases by 0.235 units, there are more students eligible for welfare benefits (8.5 p.p.), and the share of migrant students is higher (8.3 p.p.). The expected GPA of the school-leaving certificate decreases by 0.063, when not attending the first ranked school. Thus, the school attended has a lower academic standard than the first-ranked school.

To analyse effects of peer composition, we define different peer-related variables at two levels: First, potential peers at secondary school in Panel c). These variables include: (i) the number of students speaking the same language in the school, (ii) additionally including only students from the same class, and (iii) further restricted to students of the same sex. We observe between 1 and almost 9 less potential peers, depending on the definition. A similar conclusion is made from the second peer-related level: former peers, based on the primary school exit cohort (same class and same language spoken) in the entry cohort of the secondary school (in Panel d)). Again, we observe that losing the admission lottery leads students to start the secondary school with fewer former peers (around 2 for both definitions).

To estimate the effect of losing the lottery on academic achievement in grade 8, three years after the transition to secondary school, we distinguish between the two subjects (German and Mathematics) assessed in the standardized tests. In Panel e), standardised test scores are estimated separately by subject and skill domain, focusing solely on compulsory tests. Consistent with prior research (Hastings et al., 2006; Angrist et al., 2016; Ketel et al., 2023), the findings suggest that admission lotteries have no negative impact on standardized test performance. Thus, the higher commuting costs or the different social composition do not appear to affect the academic achievement three years after the transition. Finally, in Panel e), we analyse long-term outcomes as we use binary variables indicating whether the students obtain certain or no school-leaving certificates five to eight years after transitioning from primary to secondary school. Note that this reduces the number of observations, because not all cohorts in the sample have graduated yet. We do not find any evidence that losing the admission lottery has a long-term effect in that respect. Losing the lottery does not decrease the likelihood to get a university, intermediate or basic school certificate, nor does the likelihood increase that students leave without a certificate.

The results for Eq. 2 in Columns (2)-(4) in **Table 2-2** show the costs of being assigned to the second- or third-ranked or to a school that was not ranked by the parents. Most variables show that losing the lottery in round one, two and three is associated with strictly increasing costs, while this is not the case for home-school distance. This might be explained by the multidimensionality of school choice, in particular in a school system with different school types. However, the increase in home-

school distance is with 637 meters largest when the student is allocated to a school by the school board. The same is true for the share of welfare benefit students (13.3 p.p.) and the share of migrant students (10.4 p.p.). In addition, students will have less peers from primary school in their class and less potential peers. The cost of losing in terms of overall academic standard is also strictly increasing. Not admitted to any of the ROL schools, decreases the average school GPA by 0.111. We also observe a single negative effect in standardised test scores for German (Spelling) when admitted to the third-ranked school.

2.6.2 Effect Heterogeneity

The main findings show that losing the lottery affects students in numerous ways but not necessarily in students' academic achievement. In the next step, we investigate if specific groups of students face higher costs than others when losing the first admission lottery. Therefore, in Eq. (1), we differentiate groups by income (eligible for welfare benefits or not), gender, country of origin, first-ranked school type (academic or comprehensive school) and students' performance (high-performing or not). Losing the admission lottery indeed affects specific groups of students more than others. As an example, **Table 2-3** shows the results by eligibility for welfare benefits for the first lottery round. Economically disadvantaged students are particularly affected with regard to increased home-to-school distance and changes in peer networks (both former and potential peers), compared to students who are not eligible for welfare benefits. This is in line with previous findings that state that these students are more sensitive to distance (Ajayi et al., 2026) and also to peer networks (Burgess et al., 2015; Walters, 2018).

Table 2-3 Heterogeneous Effects for Students Eligible for Welfare Benefits

Outcome N	No eligibility for welfare benefits (8,960)			Eligibility for welfare benefits (4,215)		
	Lottery winner mean	ATE	% change	Lottery winner mean	ATE	% change
a) Distance to secondary school						
Home-school distance (in meters)	2,340.35 [2,079.38]	383.3*** (58.60)	16.38	1,905.48 [2,041.70]	433.6*** (82.81)	22.76
b) School characteristics of secondary school						
Social index B	2.169 [1.211]	0.243*** (0.031)	11.20	3.178 [1.177]	0.196*** (0.039)	6.17
Share of welfare benefit students	0.232 [0.167]	0.080*** (0.004)	34.48	0.391 [0.153]	0.084*** (0.006)	21.48
Share of migrant students	0.388 [0.156]	0.074*** (0.004)	19.07	0.492 [0.159]	0.091*** (0.005)	18.50
Average GPA (secondary school)	2.379 [0.181]	0.060*** (0.005)	2.52	2.450 [0.164]	0.063*** (0.005)	2.57
c) Potential peers in secondary school						
Potential peers 1 (language)	67.63 [44.98]	-8.609*** (1.066)	12.73	31.86 [33.04]	-9.282*** (0.995)	23.13
Potential peers 2 (language, class)	14.14 [9.04]	-3.165*** (0.239)	22.83	7.19 [6.92]	-2.108*** (0.216)	29.32
Potential peers 3 (language, class, sex)	7.60 [4.96]	-1.642*** (0.131)	21.61	4.15 [3.80]	-2.061*** (0.133)	49.66
d) School entry cohort composition⁺						
Peers A (language)	8.85 [7.34]	-2.673*** (0.219)	30.20	6.41 [6.65]	-2.373*** (0.214)	37.02
Peers B (classmates)	5.71 [3.88]	-1.771*** (0.119)	31.02	6.38 [4.05]	-2.061*** (0.133)	32.30
Social index A (exit cohort)	2.453 [1.246]	-0.033** (0.013)	1.35	3.590 [1.075]	-0.015 (0.016)	-
e) Standardised test scores						
Mathematics	3.241 [1.564]	-0.164 (0.363)	-	1.983 [1.150]	0.106 (0.369)	-
German (Reading)	3.635 [1.509]	-0.137 (1.802)	-	2.900 [1.434]	-0.126 (1.624)	-
German (Spelling)	3.404 [1.278]	-0.305 (0.762)	-	2.776 [1.135]	-0.881 (1.215)	-
f) Long-term outcomes						
University entrance certificate	0.572 [0.495]	0.015 (0.037)	-	0.265 [0.442]	-0.046 (0.041)	-
Intermediate certificate	0.211 [0.408]	-0.010 (0.035)	-	0.294 [0.456]	0.045 (0.047)	-
Basic certificate	0.141 [0.348]	-0.036 (0.029)	-	0.287 [0.453]	-0.015 (0.047)	-
No school-leaving certificate	0.076 [0.264]	0.031 (0.023)	-	0.153 [0.360]	0.016 (0.034)	-

Note: ⁺School entry cohort composition is based on information of primary school exit cohort. Lottery winner mean: students who participate in the first admission lottery and win. Number of observations differs due to missing values. Full set of covariates and fixed effects. Robust standard errors in parentheses. Standard deviations in brackets. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

The complete results of the heterogeneity analysis of the first admission lottery are shown in **Table 2-7-A (Appendix I-C)**.¹⁹ There are no substantial differences between female and male students regarding the consequences of losing the lottery. Adverse effects for low-performing students mirror those of comprehensive school students, while high-performing students experience effects similar to academic school students. This pattern reflects the priority group system, where high-performing students typically target academic schools while low-performing students do not. Interestingly, we find that losing the admission lottery reduces the home–school distance for high-performing students and for those who ranked an academic school first, as they are often admitted to a closer school. This suggests that their initial preferences were for more competitive academic schools located further away. We also find some evidence that low-performing (male) students are less likely to graduate with the basic (university entrance) certificate once they lose the admission lottery.

More nuanced patterns emerge from the heterogeneity analysis by country of origin. Compared to the control group, native German students experience larger changes in school composition across nearly all outcomes, except for home-to-school distance. For example, losing the admission lottery is associated with an approximately 18% higher share of schoolmates with a migration background and a 29% higher share of schoolmates receiving welfare benefits. For students with a migration background, the corresponding increases are 15% and 21%, respectively. German students also tend to experience larger changes in social index scores and in peer network structures. These patterns likely reflect stronger preferences among native German families for particular school compositions, which makes deviations from their first-ranked school choices more consequential for them. However, while German students tend to incur higher costs when losing the lottery (compared to the control mean), the probability of entering a lottery in the first place is disproportionately higher among students with migration background (**Chapter 2.6.4**).

2.6.3 Non-Compliance and Private Schools

Whether or not the adverse effects of admission lotteries are persistent also depends on compliance. Although switching schools in Bremen is not common (e.g., around 85% of students participating in the admission lottery are observed in the same school in $t + 2$)²⁰, we do observe enrolments in schools different from those assigned by the matching algorithm. This is not exclusive to Bremen but is observed in many other places with centralised admission procedures, e.g., Amsterdam (Ketel et al., 2023), Beijing (He, 2017), Chile (Arteaga, 2024), Denver (Abdulkadiroğlu et al., 2017), and New York City (Abdulkadiroğlu et al., 2015).

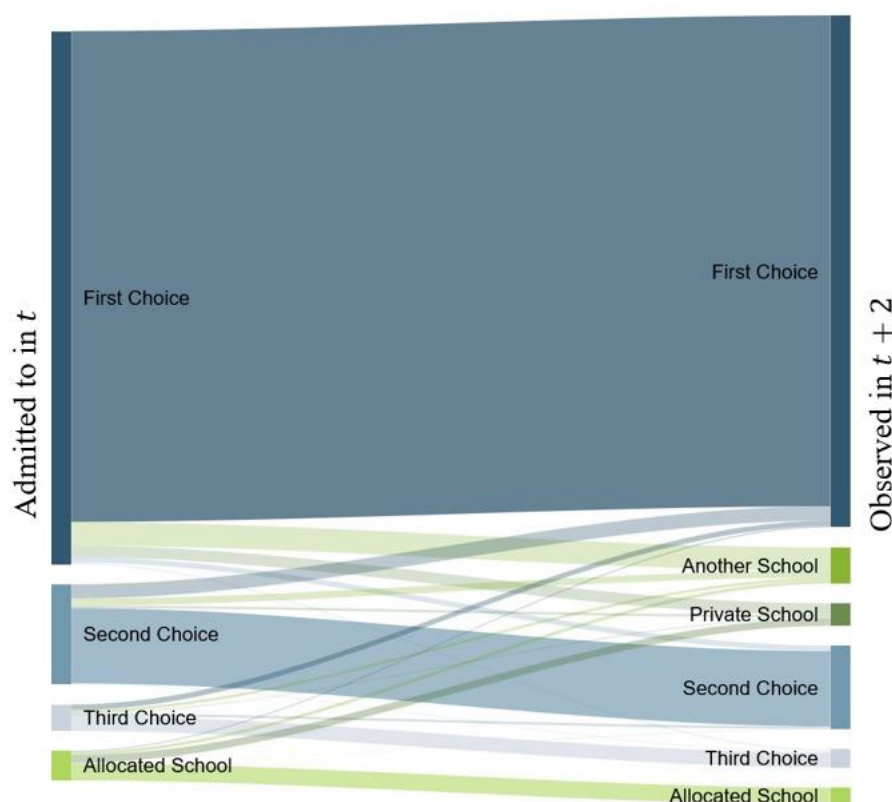
¹⁹ Due to the first-choice maximising immediate acceptance, second- and third-admission lottery observations get naturally more diminutive and more challenging to investigate. That is why we focus on Eq. (1) here. Results available upon request.

²⁰ To increase the number of observations, we focus here on $t + 2$.

Figure 2-3 illustrates the number of (non-)compliant students from the year of assignment t through $t + 2$ for all participants in the first admission lottery in Bremen. The accompanying table below the figure presents both the number and share of students for each year (t , $t + 1$, and $t + 2$). As **Figure 2-3** corresponds to outcomes in $t + 2$, the figure visualizes the cumulative school transfers observed over time. A comparison across the three years reveals that already in grade 5, some students do not remain at the school they were initially assigned to.²¹ These shares increase monotonically over time across all ranked options on the ROL, even among those who initially were admitted to their first-ranked school (almost 8% in $t + 2$). Conversely, the share of students who were not initially assigned to their first-ranked school but are observed there in later years also increases steadily. The share of students transitioning to private schools also rises over time. This pattern is especially pronounced among students who were assigned to lower-ranked schools on the ROL and is highest among those who were allocated to a school outside their ROL by the school board (around 26%). Notably, students who were allocated by the school board are less likely to transition to a school listed on their ROL and more likely to attend a private school. From the figure and accompanying table, we conclude that while some students do switch away from their initially assigned first-ranked school, the overall pattern suggests indeed a strong preference for the first-ranked school, and that despite strategic ranking behaviour, the first-ranked school might often be a preference that remains desirable even after the assignment process. Additionally, the shift toward private schools across all groups indicates that private schools represent a viable and attractive alternative, particularly for students who were placed at lower-ranked or non-preferred public schools, where non-compliance rates are highest.²² Nevertheless, unlike public schools, private schools in Bremen charge tuition fees, with average monthly costs amounting to approximately 340 EUR. As a result, attending a private school is not a feasible alternative for certain student groups, for example, those eligible for social welfare benefits.

²¹ Note, these are not the students who were admitted to a preferred school via the wait list.

²² Around 15% of all non-complying students move to a different residential block, which may partly explain their non-compliance behaviour, as relocating is often associated with a change of school.

Figure 2-3 Non-Compliance

From	To	t		$t + 1$		$t + 2$	
		Obs.	Share (in %)	Obs.	Share (in %)	Obs.	Share (in %)
First Choice	First Choice	10,212	95.76	9,890	93.69	9,604	92.05
	Second Choice	34	0.32	74	0.70	106	1.02
	Third Choice	5	0.05	19	0.18	41	0.39
	Private School	182	1.71	212	2.01	224	2.15
	Another School	231	2.17	361	3.42	458	4.39
Second Choice	First Choice	251	12.52	279	14.03	279	14.23
	Second Choice	1,616	80.64	1,524	76.66	1,473	75.15
	Third Choice	19	0.95	23	1.16	25	1.28
	Private School	43	2.15	48	2.41	48	2.45
	Another School	75	3.74	114	5.73	135	6.89
Third Choice	First Choice	96	18.01	106	20.11	109	21.17
	Second Choice	51	9.57	50	9.49	49	9.51
	Third Choice	334	62.66	309	58.63	289	56.12
	Private School	15	2.81	15	2.85	16	3.11
	Another School	37	6.94	47	8.92	52	10.10
Allocated School	First Choice	2	0.35	20	3.45	24	4.17
	Second Choice	3	0.53	3	0.52	6	1.04
	Third Choice	1	0.18	5	0.86	4	0.69
	Private School	134	23.76	152	26.21	148	25.69
	Another School	34	6.03	53	9.14	65	11.28
	Allocated School	390	69.15	347	59.83	329	57.12

Notes: The Sankey diagram illustrates patterns of non-compliance when transitioning from primary to secondary school. On the left-hand side, students are shown according to the school they were initially admitted to in period (t). On the right-hand side, students are shown according to the school they are actually observed attending two years later ($t + 2$). 'Allocated School' refers to the school assigned by the school board when a student does not receive a placement at any school listed on their ROL. 'Another School' refers to a school that is neither on the ROL, nor the allocated school, nor a private school.

We further compare complying students to those who opt out and enrol in a private school after participating in the first admission lottery. As the immediate acceptance is first-choice maximising, where the majority of matches are resolved in the first round and the number of observations are relatively larger, we focus on students who participate in the first admission lottery and either a) win or b) lose.

Table 2-4 Compliers and Non-Compliers Attending Private Schools

Students Characteristics	Compliers mean	Non-Compliers mean	mean difference	p > t
(a) Win first admission lottery				
Age	10.47	10.49	0.023	0.658
Female	0.448	0.439	0.009	0.849
High performance	0.709	0.717	0.009	0.835
Migrant	0.161	0.166	0.005	0.898
Eligibility for welfare benefits	0.040	0.040	0.000	1.000
Building block	YES	YES	-	-
Propensity score for non-compliance	0.038	0.038		
Std. dev.	0.016	0.016		
N	223	223		
(b) Lose first admission lottery				
Age	10.53	10.56	0.026	0.669
Female	0.613	0.516	0.097	0.086
High performance	0.555	0.555	0.000	1.000
Migrant	0.342	0.303	0.039	0.468
Eligibility for welfare benefits	0.045	0.045	0.000	1.000
Building block	YES	YES	-	-
Propensity score	0.308	0.352		
Std. dev.	0.220	0.223		
N	155	155		

Note: Columns for (a): Compliers, students that participate in the first admission lottery, win, and stay in the first-ranked school, at least two years after the matching assignment; non-compliers, students that participate in the first admission lottery and win, but are observed in a private school two years after the matching assignment. Columns for (b): Compliers, students that participate in the first admission lottery, lose, and stay in the non-first ranked school, at least two years after the matching assignment; non-compliers, students that participate in the first admission lottery and lose, but are observed in a private school two years after the matching assignment.

For a causal effect of non-compliance, particularly, enrolling in a private school, we implement propensity score matching approach based on key observable characteristics, including performance, age, gender, migration background, eligibility for welfare benefits and building block.²³ Specifically, each treated student (non-complier) is matched to a statistical twin from the control group (complier) using 1:1 nearest-neighbour matching without replacement. Matching is restricted to the common support region and applies a narrow calliper of 0.01 to ensure similarity. We assess balance quality using standardized mean differences before and after matching. This allows us to identify statistical twins among the compliers, who remained at their respective schools at $t + 2$, and those non-compliers who attend a private school after being assigned a seat by the lottery, both for winners and losers of the first admission

²³ This approach is different and independent of the propensity score matching with the iterations used in the empirical strategy for Eq. (2).

lottery. As shown in **Table 2-4**, there are almost no differences in the matched sample in these observables between the group of identified statistical twins and the group of non-compliers who attend a private school.²⁴ This is true for lottery winners in (a) and lottery losers in (b). Hence, the results in **Table 2-5** show the cost of attending a private school, after winning or losing the first admission lottery.

Table 2-5 Effects of Attending Private Schools

Outcome	ATT in outcome when switching from public to private school					
	(a) Win first admission lottery			(b) Lose first admission lottery		
	Compliers mean	ATT	T-stat	Compliers mean	ATT	T-stat
a) Distance to secondary school						
Home-school distance (in m)	2,816.4	1,228.8*** (309.18)	3.97	2,345.5	1,057.9*** (214.11)	4.94
b) School characteristics of secondary school						
Share of welfare benefit students	0.175	-0.117*** (0.012)	-9.64	0.323	-0.282*** (0.018)	-16.00
Share of migrant students	0.328	-0.114*** (0.012)	-9.78	0.447	-0.191*** (0.020)	-9.77
c) Potential peers in secondary school						
Potential peers 1 (Language)	86.01	-24.24*** (4.363)	-5.56	57.84	-21.90*** (3.822)	-5.73
Potential peers 2 (Language, class)	18.39	-11.44*** (0.777)	-14.72	12.81	-8.06*** (0.723)	-11.16
Potential peers 3 (language, class, gender)	9.50	-5.44*** (0.431)	-12.63	6.66	-3.79*** (0.390)	-9.72
d) School entry cohort composition⁺						
Peers 1 (language spoken at home)	9.269	4.218*** (1.094)	3.86	7.679	8.397*** (1.334)	6.29
Peers 2 (classmates)	5.245	-3.445*** (0.313)	-11.00	4.733	-3.130*** (0.309)	-10.14
e) Long-term outcomes						
University entrance certificate	0.616	0.027 [0.080]	0.34	0.481	0.296** [0.127]	2.32
Intermediate certificate	0.192	-0.068 [0.060]	-1.13	0.370	-0.222* [0.118]	-1.89
Basic certificate	0.164	0.041 [0.065]	0.64	0.037	0.000 [0.052]	0.00
No school-leaving certificate	0.027	0.000 [0.027]	0.00	0.111	-0.074 [0.072]	-1.03
N	223	223		155	155	

Note: ⁺School entry cohort composition is based on information of primary school exit cohort. Columns for (a): Compliers, students that participate in the first admission lottery, win, and stay in the first-ranked school, at least two years after the matching assignment; non-compliers, students that participate in the first admission lottery and win, but are observed in a private school two years after the matching assignment. Columns for (b): Compliers, students that participate in the first admission lottery, lose, and stay in the non-first ranked school, at least two years after the matching assignment; non-compliers, students that participate in the first admission lottery and lose, but are observed in a private school two years after the matching assignment. ATT: average treatment effect on the treated, e.g. non-compliers effect due to the change to a private school. Standard errors in parentheses. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

²⁴ **Table 2-11-A (Appendix I-F)** presents the table for the unmatched sample. There are significant differences between students who remain in public schools and those who switch to private schools in $t + 2$, e.g. the migration background as most migrant students do not switch to private schools. This highlights the necessity of searching for statistical twins in order to ensure the causal validity of the analysis.

Specifically, for some variables, we see an average treatment effect on the treated (ATT) in **Table 2-5** for first admission lottery winners (a) and losers (b).²⁵ Non-compliers commute between 1,000 and 1,220 additional meters to attend a private school, where they encounter a more homogeneous school composition with a substantial lower share of students eligible for welfare benefits (11 – 28 p.p.) and migration share (11 – 19 p.p.).²⁶ In that respect, it is more profitable to attend a private school when losing the admission lottery in (b) as school composition becomes even more homogeneous in comparison to (a). For peer networks, in both (a) and (b), non-compliers encounter fewer potential peers, as private schools are generally smaller than public schools. The number of former classmates in the private school's entry cohort decreases, while the number of former schoolmates who speak the same language at home increases. These results are primarily driven by German speaking students attending a private school, as 77.5% of non-compliers who attend a private school are German, i.e. students without migration background. While we do observe larger rates of students with the university entrance exam and less students with the intermediate certificate, we argue that opting out by enrolling in a private school is not a panacea and involves both time and monetary costs. On average, parents must pay a monthly tuition fee of around 340 EUR at private schools in Bremen, and their children have to commute a longer distance to school. However, we assume that parents, who choose private schools for their children, prioritize a homogeneous school composition over commuting or monetary costs. This assumption is supported by the results in **Chapter 2.6.2**, which indicates that German students, who represent the largest share of those opting out to private schools, have strong preferences for homogeneity in school composition and are stronger burdened by losing the admission lottery in terms of effect size. On the other hand, this alternative is not generally feasible for all student groups, e.g., students eligible for welfare benefits, or is less frequently used, such as by students with a migration background, who are disproportionately overrepresented in the lottery pool.²⁷

2.6.4 Odds of Experiencing Costs of Lottery Losses

Combining the insights from this paper so far, we argue that the probability of being directly affected by a lottery loss can be expressed as the product of two terms: the probability of entering the lottery pool and the probability of losing the lottery conditional on pool entry. Formally,

$$\Pr(Loss) = \Pr(LP = 1) \times \Pr(L = 1 \mid LP = 1),$$

²⁵ Social index A and B, standardised test scores and average GPA on school level are not available for private schools.

²⁶ Migration background information is not fully available for private school students, as neither mother language nor country of birth are directly recorded there. To construct the migration background indicator, the data set uses information from previous enrolments in public schools and other administrative sources.

²⁷ Reasons why students with migration background are less likely to opt for private schooling after losing the admission lottery might be due to financial constraints, limited information about alternatives, and potential barriers related to application procedures or perceived social exclusion.

where $LP = 1$ denotes entry into the lottery pool by applying to an oversubscribed school, and $L = 1$ indicates losing the lottery once in the pool. It is important to emphasize that the lottery itself is random and unbiased: within a given priority group at a school, winners and losers are determined by chance alone. The formula therefore does not capture a bias in the lottery draws, but rather how often students are exposed to the risk of losing, that is, the likelihood that they will actually bear the costs of a lottery loss. For migrant students, this probability equals 0.1030 while for native German students it is 0.0625. The resulting odds ratio is given by

$$OR = \frac{\Pr(\text{Loss} \mid \text{Migrant})}{\Pr(\text{Loss} \mid \text{German})} = \frac{0.1030}{0.0625} = 1.65.$$

This implies that students with a migration background are about 65% more likely to experience a lottery loss than their native German peers and, hence, are more likely to see the estimated costs materialize. The higher odds are driven by systematic differences in school choice: migrant students apply more often to oversubscribed schools and are disproportionately represented in pools with tighter competition.

This approach shifts the focus from the causal effect of a lottery loss to the ex-ante odds that these costs materialize. In Bremen's immediate acceptance setting, language and information frictions plausibly amplify such exposure differences. Evidence shows that providing native-language assignment information shifts families toward more informed and selective choices, precisely the kinds of behavioural changes that govern who enters competitive lotteries in the first place (Zuchanek, 2025). From a policy perspective, this distinction is crucial: even when the lottery is fair, unequal exposure translates into unequal expected losses. Interventions that reduce information gaps, such as native-language materials that explain priority rules, tie-breaking lotteries, and the strategic incentives of the applied mechanism, can therefore lower group differences in exposure and, in turn, the odds of incurring lottery-related losses, without altering the randomness of the lottery itself.

2.7 Robustness Checks

The results presented are robust across various specification choices. As an alternative to Eq. (1), we replace first-ranked school fixed effects with (a) 50 dummies for every two-percentile of the propensity score for assignment to the first-ranked school and (b) the admission probability for the first-ranked school. We conclude that results are quite similar and remain significant and of similar size. For an alternative specification for Eq. (2), we use (c) deciles instead of quantiles. For (c), we conclude that although the interactions of $f(\varphi_i^2, \varphi_i^3)$ are sparsely populated – since most matches are resolved in the first round – the results are overall consistent with our main findings in **Table 2-2**.²⁸

²⁸ Results for alternative specifications (a)-(c) upon request.

Then, to further test the robustness of our results, we replace time fixed effects with interactions between the treatment variable (losing the admission lottery) and year dummies in Eq. (1). This specification allows us to assess whether the estimated effects are driven by a specific year or emerge only from a certain point in time onward, thereby accounting for potential confounding factors or structural breaks. For all outcome variables, the interaction terms are jointly significant, indicating that treatment effects vary across years. This is not surprising, as fluctuation in demand (**Figure 2-1**), capacity changes and composition of the cohort may vary over time. However, the year-specific coefficients fluctuate without a discernible temporal pattern. Many of them are statistically insignificant, and there is no indication that effects are concentrated in or triggered by a particular year or event. This supports the interpretation that the estimated treatment effects are not driven by single-year anomalies or a confounding factor and remain robust over the full observation period.²⁹

Next, we extend our analysis and include voluntary VERA-8 assessments to increase statistical power and coverage across subjects. In **Table 2-2**, we omitted students who repeated a grade, were diagnosed with special educational needs – specifically dyscalculia or dyslexia – or who have lived in Germany for less than one year. In addition, schools decide whether their students take another test in a subject that is not the compulsory subject that year. Hence, including voluntary assessments also allows us to cover all three subjects tested each year, rather than being limited to a single compulsory test per cohort. In total, we increase the number of observations by 1,466 (around 60%) taken tests. However, voluntary participation of schools may correlate with unobserved characteristics and, hence, the result might be biased in that regard. **Table 2-10-A (Appendix I-E)** presents the results. The estimates remain mostly statistically insignificant, apart from German (Spelling) in Eq. (1), however, only on the 10% significance level. This indicates that incorporating voluntary assessments does not alter the overall conclusion that losing admission lotteries has no systematic adverse effect on students' academic outcomes.

²⁹ Although this is the case, it is important to note that during the COVID-19 pandemic, many of the associated costs did not materialize, as classes were cancelled or held online. Therefore, for example, while students were technically assigned to schools with greater home-school distances, they were not practically affected by this in everyday life.

2.8 Conclusion

School choice mechanisms are intended to promote both efficiency and equity by allowing families to express preferences over schools and to prevent schools from selecting their preferred students in a decentralised manner. However, these systems often assume that families can navigate these often complex procedures impeccably. In reality, information asymmetries and other barriers can distort choices and exacerbate inequalities in access to preferred secondary schools and, at the same time, parents are concerned that their child will bear negative consequences once their child does not attend the school of choice.

Using longitudinal data from Bremen, a German city state, and its centralized secondary school assignment in an early tracking system, we exploit the causal variation introduced by random admission lotteries to study these dynamics. Lottery-induced randomization within priority groups provides a clear-cut solution to the problem of endogenous sorting while transitioning from primary to secondary school, as students who win or lose the lottery are comparable on both observable and unobservable characteristics. Our findings show that students who lose the admission lottery and are therefore not assigned to their first-ranked school bear higher non-academic costs – particularly in terms of social composition of the school and commuting time – than their peers who win the lottery. With regard to student performance, we find no systematic adverse effects on standardized test scores three years after the transition to secondary school, nor on school-leaving certificates awarded at the end of secondary education (five to eight years after transition).

In particular, we find that losing admission lotteries increases the home-school distance by approximately 392 to 637 meters, corresponding to more commuting time. Moreover, students who lose admission lotteries attend secondary schools with a different peer composition. Specifically, these schools have higher shares of students eligible for welfare benefits (an increase between 5.5 and 13.3 p.p.), a higher proportion of migrant students (6.4 to 10.4 p.p.) and lower social index scores and academic standards. Lottery losers are also assigned to schools with fewer former peers from primary school and fewer potential peers in fifth grade, based on language spoken at home and gender on school and class level. We also find evidence that specific groups of students are disproportionately affected by losing admission lotteries, especially students eligible for welfare benefits and native German students in terms of effect size. In response, mostly German students opt out of the public school system and enrol in private schools to access a more homogeneous educational environment, accepting both tuition fees and longer commutes. In contrast, students that are eligible for welfare benefits are unlikely to pursue such a strategy systematically. Given the financial barriers associated with private schooling, opting for a private school to circumvent the consequences of losing the admission lottery does not appear to be a feasible or realistic alternative for them (Akbarpour et al., 2022). Moreover, students with a migration background are disproportionately represented in competitive lottery pools and, hence, lose the lotteries more often, but rarely follow the opt-out strategy. This means that although the effect size

of costs upon losing is heterogeneous, migrant students face them more often because they are more frequently exposed to the lottery.

Our study contributes to a fast-growing literature on admission lotteries and matching assignments. Previous publications cannot rule out any effects of losing the admission lottery and have difficulties pinpointing the driver of their results (e.g., Hastings et al., 2006; Hastings et al., 2007; Bibler and Billings, 2020 and Ketel et al., 2023). We argue that our results are partly driven by the heterogeneity of students and their different knowledge about the pitfalls of the applied student matching algorithm, which rewards strategic play. This implies that although the admission lottery is random by design, the likelihood of entering the lottery pool is unequal across student groups. However, our results do not provide any evidence on why students and their parents make specific (strategic) choices or not, which directly affect admission probabilities to public schools. Hence, we can only investigate the results of choices made in the past, accompanied by the random admission lottery outcome. Nonetheless, these results provide relevant insights for school choice policies and policymakers. When dealing with school enrolment, students and parents should not only consider future academic outcomes, but also social and commuting costs. After all, our analysis shows that social and commuting costs are indeed high while educational attainment is not adversely affected. However, this can explain the parental frustration mentioned at the beginning of the paper about not being assigned to the top-ranked school as research shows that parents prefer schools with high-achieving and high socio-economically advantaged peers (Hastings and Weinstein, 2008; Burgess et al., 2015; Abdulkadiroğlu et al., 2020; Terrier et al., 2026). Therefore, some parents decide to send their children to private schools, where there is a more homogeneous school composition for which parents are willing to pay higher commuting and monetary costs.

This study is politically relevant in Germany, where centralised admission procedures are still uncommon and decentralized decisions do not rule out cream skimming of schools. Therefore, parents in Germany face frustration as their children might fail to be admitted to the preferred school. Clearly, matching mechanisms cannot solve all problems. Only through sufficient information about the applied mechanism can parents make informed decisions, understand the pitfalls of losing admission lotteries and accept the final school assignments. While this issue involves both matching mechanisms and broader supply challenges, studies like this demonstrate the potential of administrative data to provide new insights into school landscapes and school choice. Such research can help identify gaps in access, address areas of excess demand, and inform policymakers on strategies for improving the system. Expanding access to data is crucial to answering these pressing questions and developing solutions that better meet the needs of students, families and schools alike.

2.9 Appendix I

Appendix I-A – Bremen’s Secondary Schools

In Bremen, there are only two types of secondary schools: academic (*Gymnasium*) and comprehensive schools (*Oberschule*)³⁰. While there are differences between the school types, e.g. maximum class size, teaching structure and curriculum, students can obtain the same school-leaving certificates. The school-leaving certificate qualifying for university entrance is obtained after eight years in academic and nine years in comprehensive schools. In 2021, Bremen had eight academic and 34 comprehensive schools. There are ten private schools that offer a secondary school program. Some of them are denominational schools, but are generally open to all students, while others offer alternative pedagogical concepts.

The school types differ in their admission rules. For comprehensive schools, the first priority sorting depends on the affiliated primary school (schools close by) and an above-average academic performance of the student. For these students, one-third of a comprehensive school’s capacity is reserved. The second priority group is defined solely by affiliation of the primary school. Hence, if a high-performing student from an affiliated primary school loses the lottery within the first priority group, the student will be considered again within the second priority group – but competes with low-performing students. This is the only case where students can be considered twice within a preference on the ROL. In case that seats remain unfilled, the remaining seats will be filled by lottery with students from non-affiliated primary schools. Wait list priority for comprehensive schools is 1) students from affiliated primary schools and 2) all other students. For academic schools, priority is only based on performance. Hence, after the first priority group, all other students will be admitted, if seats remain available. General admission rules/priorities:

- a) Both school types accept up to ten percent so-called hardship cases. These are students a) which have special educational needs, b) who or whose family would be socially harmed by attending another school, or c) with siblings in the respective secondary school. Note that, according to the school board, such cases are identified under a compelling necessity and are rarely approved after assessment.
- b) Within each priority group, a lottery takes place if the number of applicants exceeds the number of available seats.
- c) Applications submitted after the application deadline will be given lower priority.
- d) If none of the schools on the ROL could be considered, parents will receive a mail from the school board. Following a counselling session, they will be offered another school place.
- e) The wait list for the over-demanded schools remains active until the end of the first term of the respective academic year.

³⁰ Freie Hansestadt Bremen – Die Senatorin für Kinder und Bildung (2025).

Appendix I-B – Further Descriptive Statistics**Table 2-6-A Descriptive Statistics: Standardised Test Scores**

	2016	For students transitioning in:			
		2017	2018	2019	2020
a) <u>Mathematics</u>					
Mean [S.D.]	2.911 [1.407]	3.119 [1.495]	2.758 [1.294]	2.402 [1.198]	2.439 [1.308]
At academic school	2.600 [1.517]	3.993 [1.423]	3.133 [0.834]	2.500 [1.269]	3.404 [1.296]
At comprehensive school	2.916 [1.407]	2.716 [1.348]	2.747 [1.304]	2.400 [1.197]	2.055 [1.010]
Total No. Obs.	315	2,687	516	470	2,756
of which lottery participant	147	875	120	201	798
of which test compulsory	-	765	-	-	758
b) <u>German (Reading)</u>					
Mean [S.D.]	3.550 [1.311]	2.773 [1.452]	3.292 [1.551]	-	2.042 [1.150]
At academic school	3.400 [1.140]	3.222 [1.394]	4.243 [1.251]	-	2.333 [1.528]
At comprehensive school	3.552 [1.314]	2.762 [1.453]	2.919 [1.498]	-	2.038 [1.148]
Total No. Obs.	367	384	1,734	-	263
of which lottery participant	128	109	564	-	73
of which test compulsory	-	-	449	-	-
c) <u>German (Spelling)</u>					
Mean [S.D.]	2.983 [0.965]	2.406 [1.136]	2.967 [1.286]	-	2.217 [0.892]
At academic school	3.200 [0.837]	3.333 [1.118]	4.028 [1.064]	-	2.667 [1.528]
At comprehensive school	2.980 [0.968]	2.384 [1.129]	2.569 [1.126]	-	2.212 [0.886]
Total No. Obs.	355	384	1,700	-	263
of which lottery participant	125	109	559	-	73
of which test compulsory	-	-	444	-	-

Note: VERA-8 standardised test each year by subject and skill domain. Test is taken three years after transitioning from primary to secondary school. Compulsory Tests: English in 2019 and 2022, Mathematics in 2020 and 2023, German in 2021. Standard deviations in brackets.

Appendix I-C - Heterogeneity

Table 2-7-A Further Heterogeneous Results

Outcome	(1) Sex				(2) Country of origin			
	Male (6,683)		Female (6,492)		German (6,272)		Non-German (6,903)	
N	Lottery winner mean	ATE	Lottery winner mean	ATE	Lottery winner mean	ATE	Lottery winner mean	ATE
<i>a) Distance to secondary school</i>								
Home-school distance (in meters)	2,241.3	340.9*** (67.26)	2,268.6	470.3*** (68.62)	2,314.3	341.1*** (83.22)	2,219.2	420.6*** (58.65)
<i>b) School characteristics of secondary school</i>								
Social index B	2.46	0.237*** (0.036)	2.43	0.196*** (0.036)	2.09	0.265*** (0.038)	2.82	0.199*** (0.032)
Share of welfare benefit students	0.28	0.069*** (0.005)	0.27	0.072*** (0.005)	0.23	0.067*** (0.006)	0.33	0.072*** (0.005)
Share of migration students	0.42	0.076*** (0.005)	0.42	0.069*** (0.005)	0.36	0.065*** (0.005)	0.48	0.072*** (0.004)
Average GPA (secondary school)	2.38	0.056*** (0.006)	2.38	0.068*** (0.059)	2.35	0.047*** (0.008)	2.40	0.072*** (0.005)
<i>c) Potential peers in secondary school</i>								
Potential peers 1 (language)	56.72	-11.05*** (1.089)	58.39	-11.06*** (1.058)	82.55	-16.69*** (1.284)	41.05	-8.14*** (0.867)
Potential peers 2 (language, class)	12.07	-2.91*** (0.233)	12.24	-3.22*** (0.235)	17.23	-5.44*** (0.296)	6.88	-1.83*** (0.179)
Potential peers 3 (language, class, sex)	6.56	-1.48*** (0.127)	6.71	-1.73*** (0.127)	9.17	-2.83*** (0.171)	3.94	-0.99*** (0.095)
<i>d) School entry cohort composition[†]</i>								
Peers A (language)	8.12	-2.81*** (0.214)	7.99	-2.56*** (0.220)	10.57	-3.88*** (0.294)	5.39	-1.92*** (0.158)
Peers B (classmates)	5.86	-1.97*** (0.120)	5.80	-2.00*** (0.127)	5.69	-1.95*** (0.148)	5.98	-1.85*** (0.103)
Social index A (exit cohort)	2.80	-0.021 (0.015)	2.75	-0.031** (0.014)	2.30	-0.025 (0.017)	3.27	-0.023* (0.013)
<i>e) Long-term outcomes</i>								
University entrance certificate	0.44	-0.063* (0.038)	0.52	-0.002 (0.049)	0.54	0.012 (0.044)	0.40	-0.022 (0.037)
Intermediate certificate	0.25	0.057 (0.043)	0.22	0.028 (0.046)	0.22	-0.018 (0.042)	0.26	0.041 (0.038)
Basic certificate	0.21	-0.028 (0.042)	0.16	-0.025 (0.039)	0.15	-0.009 (0.035)	0.24	-0.045 (0.036)
No school-leaving certificate	0.10	-0.034 (0.031)	0.10	-0.002 (0.033)	0.09	0.016 (0.029)	0.10	0.026 (0.026)

Note: Continues on the next page.

Continuing of Table 2-7-A

Outcomes	(3) School Type (first choice)				(4) Performance			
	Comprehensive school (8,540)		Academic school (4,635)		Low-performing (8,340)		High-performing (4,835)	
N	Lottery winner mean	ATE	Lottery winner mean	ATE	Lottery winner mean	ATE	Lottery winner mean	ATE
<i>a) Distance to secondary school</i>								
Home-school distance (in meters)	1,623.9	661.6*** (41.54)	3,176.9	-639.9*** (151.50)	1,863.5	592.6*** (48.52)	3,016.7	-273.4* (144.2)
<i>b) School characteristics of secondary school</i>								
Social index B	3.05	0.086*** (0.025)	1.56	0.575*** (0.064)	3.24	0.142*** (0.026)	1.56	0.393*** (0.068)
Share of welfare benefit students	0.38	0.052*** (0.004)	0.13	0.011*** (0.009)	0.41	0.062*** (0.004)	0.14	0.063*** (0.009)
Share of migration students	0.47	0.070*** (0.003)	0.34	0.050*** (0.009)	0.51	0.070*** (0.003)	0.32	0.044*** (0.009)
Average GPA (secondary school)	2.50	0.036*** (0.003)	2.22	0.189*** (0.013)	2.50	0.045*** (0.003)	2.25	0.120*** (0.014)
<i>c) Potential peers in secondary school</i>								
Potential peers 1 (language)	40.96	-7.21*** (0.629)	81.80	-24.50*** (2.398)	34.9	-7.63*** (0.649)	82.4	-21.11*** (2.257)
Potential peers 2 (language, class)	9.06	-2.08*** (0.144)	16.80	-6.99*** (0.504)	7.54	-2.10*** (0.146)	17.2	-7.08*** (0.540)
Potential peers 3 (language, class, sex)	5.03	-1.10*** (0.078)	8.98	-3.66*** (0.276)	4.26	-1.12*** (0.078)	9.16	-3.70*** (0.304)
<i>d) School entry cohort composition⁺</i>								
Peers 1 (language)	8.83	-3.10*** (0.156)	6.93	-1.51*** (0.389)	7.15	-2.89*** (0.157)	7.61	-2.16*** (0.431)
Peers 2 (classmates)	6.97	-2.36*** (0.091)	4.18	-0.61*** (0.206)	6.05	-2.33*** (0.093)	4.19	-0.81*** (0.203)
Social index A (exit cohort)	3.16	-0.027** (0.011)	2.21	-0.029 (0.022)	3.35	-0.025** (0.012)	2.14	-0.016 (0.022)
<i>e) Long-term outcomes</i>								
University entrance certificate	0.33	-0.015 (0.029)	0.74	-0.027 (0.064)	0.26	0.015 (0.029)	0.80	0.003 (0.061)
Intermediate certificate	0.32	0.024 (0.032)	0.10	0.010 (0.047)	0.34	0.029 (0.033)	0.10	-0.006 (0.035)
Basic certificate	0.23	-0.038 (0.028)	0.10	-0.028 (0.053)	0.27	-0.061** (0.030)	0.05	-0.033 (0.036)
No school-leaving certificate	0.12	0.029 (0.022)	0.06	0.045 (0.046)	0.13	0.017 (0.023)	0.05	0.035 (0.039)

Note: ⁺School entry cohort composition is based on information of primary school exit cohort. Lottery winner mean: students who participate in the first admission lottery and win. Number of observations differs due to missing values. Full set of covariates and fixed effects. Robust standard errors in parentheses. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Appendix I-D – Lottery Participants and Randomness

Table 2-8-A T-Test Lottery Participants

Students Characteristics	Lottery Participant		T-Test Mean diff.
	No Mean [S.D.]	Yes Mean [S.D.]	
Age	10.617 [0.570]	10.607 [0.570]	0.010
Female	0.493 [0.500]	0.491 [0.500]	0.002
High performance	0.338 [0.473]	0.374 [0.484]	-0.035***
Migrant	0.489 [0.500]	0.517 [0.500]	-0.028***
Eligibility for welfare benefits	0.320 [0.467]	0.312 [0.463]	0.008
First choice academic school	0.263 [0.440]	0.360 [0.480]	-0.097***
N	25,427	13,923	

*Note: The table reports mean values for student characteristics by risk of lottery participation. T-tests compare the means of lottery participants and students who listed a safe school first or were excluded from the lottery due to a low priority group and, as a result, were allocated by the school board. Standard deviations in brackets. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.*

Table 2-9-A Randomness Check for First Admission Lottery Win

	Academic School		Comprehensive School		
	(1)	(2)	(3)	(4)	(5)
	Perfor- mance	No-priority group	Performance + affiliation	Affiliation	No-priority group
Eligibility for welfare benefits	-0.005 (0.030)	-0.066 (0.112)	0.054 (0.097)	0.003 (0.015)	-0.137 (0.151)
Migrant	-0.001 (0.035)	0.082 (0.166)	-0.082 (0.144)	0.034* (0.018)	0.038 (0.234)
Female	-0.005 (0.015)	0.019 (0.091)	-0.017 (0.048)	-0.005 (0.012)	-0.077 (0.111)
Age	-0.008 (0.014)	0.055 (0.098)	-0.021 (0.048)	0.008 (0.011)	-0.057 (0.108)
R ²	0.459	0.801	0.721	0.306	0.810
N	4,374	735	755	8,322	747

*Note: Dependent variable is first admission lottery win. Primary school, city block and year fixed effects are included. Lottery losers of (3) participate in (4) again, which is the only case, where students are considered twice (see **Appendix I-A**). Robust standard errors in parentheses. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.*

We use a simple OLS method to check whether or not lottery outcomes are random. We analyse if characteristics like eligibility for welfare benefits, migration status, gender or age affect lottery outcomes. We use primary school, city block and year fixed effects. We also differentiate between priority groups and school types. We find no large effect of different observables on lottery outcomes. Hence, we argue that in the city of Bremen, lottery-induced placement is random, and an exogenous sorting among students takes place.

Appendix I-E – Standardised Test Scores

Table 2-10-A Compulsory and Voluntary Standardised Test Scores

Outcome	Lottery winner mean	Not admitted 1 st ranked	Admission 2 nd ranked	Admission 3 rd ranked	Not admitted to ROL	N
Mathematics	2.852 [1.516]	-0.071 (0.136)	-0.008 (0.148)	0.194 (0.242)	-0.333 (0.239)	2,141
German (Reading)	3.132 [1.520]	-0.064 (0.485)	0.260 (0.632)	-0.586 (0.586)	-0.207 (0.701)	875
German (Spelling)	2.885 [1.202]	-0.526* (0.296)	-0.270 (0.319)	-0.594 (0.466)	-0.881 (0.537)	866

*Note: VERA-8 standardised test (compulsory and voluntary). Lottery winner control mean: students who participate in the first admission lottery and win. Number of observations differs due to missing values. Full set of covariates and fixed effects. Clustered on secondary school class level. Robust standard errors in parentheses. Standard deviations in brackets. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.*

Appendix I-F – Non-Compliance and Private Schools

Table 2-11-A Unmatched Sample: Compliers and Non-Compliers Attending Private Schools

Students Characteristics	Compliers mean	Non-Compliers mean	mean difference	p > t
(a) Winning first admission lottery				
Age	10.59	10.49	0.102	0.007
Female	0.496	0.439	0.057	0.094
High performance	0.444	0.717	0.274	0.000
Migrant	0.489	0.166	0.323	0.000
Eligibility for welfare benefits	0.282	0.040	0.241	0.000
Building block	YES	YES	-	-
Propensity score for non-compliance	0.022	0.038		
Std. dev.	0.019	0.016		
N	9,598	223		
(b) Losing first admission lottery				
Age	10.66	10.50	0.166	0.000
Female	0.481	0.511	0.030	0.435
High performance	0.081	0.637	0.556	0.000
Migrant	0.666	0.300	0.366	0.000
Eligibility for welfare benefits	0.480	0.042	0.438	0.000
Building block	YES	YES	-	-
Propensity score	0.070	0.352		
Std. dev.	0.120	0.223		
N	1,764	190		

Note: Columns for (a): Compliers, students that participate in the first admission lottery, win, and stay in the first-ranked school, at least two years after the matching assignment; non-compliers, students that participate in the first admission lottery and win, but are observed in a private school two years after the matching assignment. Columns for (b): Compliers, students that participate in the first admission lottery, lose, and stay in the non-first ranked school, at least two years after the matching assignment; non-compliers, students that participate in the first admission lottery and lose, but are observed in a private school two years after the matching assignment.

3 Bridging Language Gaps: Native Language School Assignment Information under Immediate Acceptance

Abstract: This paper examines how translating school assignment information into parents' native languages affects school choice outcomes in Bremen, a city state in Germany, where a strategy-incentivizing immediate acceptance mechanism is used. While translated information was made available to non-German-speaking families, the coverage did not extend to all commonly spoken languages. Leveraging administrative data, I employ a difference-in-differences approach that exploits exogenous variation from students whose language spoken at home is not included in these translations. The results show that access to translated information about the assignment mechanism increases the likelihood of ranking and attending academic schools, as well as self-selection to schools with a more favourable composition, suggesting improved comprehension of the admission process. Effects are stronger for high-performing students, females, and students eligible for welfare benefits. This study contributes to the literature by showing that reducing information gaps through native-language provision can shape school choice behaviour and improve equitable access to centralized school admissions.

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3.1 Introduction

School choice aims to optimize student allocation to schools, particularly when demand for certain schools exceeds available seats. However, many parents struggle to navigate these complex systems effectively and make decisions with insufficient information – for example, due to language barriers, which pose a greater challenge for non-native speakers (de Haan et al., 2023). This can lead to costly mistakes. This challenge is even more pronounced in assignment or matching mechanisms that use immediate acceptance (IA), where truth-telling is not a dominant strategy and parents have strong incentives to rank schools strategically (Abdulkadiroğlu and Sönmez, 2003). These difficulties raise important questions about the overall efficiency and equity of centralised admissions, particularly concerning the extent to which language-related information gaps hinder equitable access to the assignment process, and whether targeted information provision can effectively mitigate these disparities.

Given these pressing questions, a growing body of research has addressed related challenges, to which this paper seeks to contribute. First, a broad literature documents that students and their parents often make strategic mistakes and suffer from information gaps in both immediate and deferred acceptance mechanisms (DA), often resulting in costly consequences (Guillen and Hing, 2014; Shorrer and Sóvágó, 2017; Guillen and Hakimov, 2017; Artemov et al., 2020; Terrier et al., 2026). These mistakes stem from misperceptions about admission probabilities (Kapor et al., 2020) as well as individual characteristics such as academic performance, migration background, or gender (Hastings and Weinstein, 2008; Lucas and Mbiti, 2012; de Haan et al., 2023; Gortázar et al., 2023). Second, this paper contributes to research on the value of information, which explores how students and families respond to information, including their willingness to engage in costly search processes (Maxey, 2024) or how they behave when they lack any reliable information (Pais and Pintér, 2008; Pais et al., 2011). Studies such as Chen and He (2021) and Arteaga et al. (2022) highlight that information is particularly valuable under IA, and that disadvantaged families face higher acquisition costs or hold inaccurate beliefs, leading to suboptimal decisions. Finally, this study adds to the literature on information interventions. Field experiments show that better information can significantly alter school choice behaviour (Agte et al., 2024). For example, Ajayi et al. (2026) find that improved access to information increases school selectivity in Ghana. Similarly, Corcoran et al. (2018) and Cohodes et al. (2025) show that students in New York City are less likely to enrol in high schools with low graduation rates when provided with more information. These effects are particularly pronounced among students from non-English-speaking households, suggesting that essential features of the school choice process were previously not well understood.

Building on these existing studies, I examine how translating school assignment information into parents' native languages affects school choice outcomes. Specifically, I investigate whether improved access to information on the transition from primary to secondary school alters the selectivity and characteristics of ranked secondary schools, as well as ultimate admissions. I argue that the supply

of translations leads to information gains, enabling parents to better understand the matching mechanism and evaluate their school options, which in turn affects their decision-making. To identify the effect, I exploit exogenous variation in the availability of translations based on the language spoken at home, using a difference-in-differences approach. In contrast to the studies cited that rely on field experiments, this paper leverages the first natural experiment in a major German city and makes a novel contribution by evaluating the translation of school assignment information as a means to reduce information gaps in school choice. This is particularly relevant in immediate acceptance mechanisms, where understanding the assignment rules is critical for making strategic choices (Dur et al., 2018). To evaluate this, I draw on rich, individual-level panel data covering all students in Bremen over eleven years. This unique dataset includes detailed background characteristics of students and allows me to track each student's educational trajectory.

Bremen, a city state in the northern part of Germany, is one of the few cases in Germany that employs a centralized admission procedure and has used an immediate acceptance mechanism with multiple ties broken by lotteries since 2011. The lottery affects a sizable number of students, as between 3,400 and 3,700 children transition from primary to secondary school each year. This transition takes place at a very early stage – at the end of fourth grade, when students are around 10 years old. The population in Bremen is very heterogeneous, with a high share of migrant students who do not speak German at home (around 35%). Therefore, Bremen presents a case with high linguistic diversity, ensuring sufficient variation in foreign languages. To address this, Bremen's school board decided to translate the information into Arabic, English, French, Persian, Russian, and Turkish prior to the start of the 2017 school year. Before that, it was difficult for non-German-speaking parents to behave optimally and to fully understand the consequences of their school choices as the translated materials explain priority rules, the underlying mechanism, and the consequences of ranking highly demanded schools first, which can lead to lotteries as tiebreakers. This translation covers approximately 21% of the total student population, leaving around 13% of the families without a translation. This ensures enough variation in foreign languages spoken at home for the DiD approach.

The results show that students whose language spoken at home is among the translated languages (treated group) are more likely to rank an academic school first rather than a comprehensive school by 2.9 percentage points (p.p.). The likelihood of including one academic school anywhere on their rank-order list increases by 3.1 p.p. This corresponds roughly to a 10% and 8% increase relative to the pre-treatment control means. Students in the treatment group are also more likely to actually attend an academic school – by 3.1 p.p. (12%) – with an effect size similar to the increase observed in their school choices. This indicates that treated students are more successful in securing schools that align with their ranking. I also find some evidence for deliberate segregation as treated students aim more for schools with a lower share of students who receive welfare benefits and schools with a more favourable

social index.³¹ The intervention effect of attending an academic school shows heterogeneous impacts, with particularly strong effects observed among high-performing students (8.2 p.p.), females (3.9 p.p.), and recipients of welfare benefits (5.7 p.p.).

3.2 Institutional Background

3.2.1 The School System and Student Allocation in the Federal State of Bremen

As a consequence of strict educational federalism, each federal state in Germany is responsible for its education policy. The federal state of Bremen consists of two cities: the city of Bremen (onwards: Bremen) with a population of 577,026 and Bremerhaven with a population of 119,357 (as of 2024). In Bremen, compulsory education starts at the age of six. Around the age of ten, students transit from primary to secondary school, and parents can choose their children's secondary school. Hence, unlike in most countries, tracking starts very early in Bremen and most of Germany and major decisions on educational careers – up to the highest school-leaving certificate – are made after fourth grade, as the secondary school is usually the school that children attend until they finish schooling. The secondary school system in Bremen, in contrast to many other German states, tracks children into two types of schools: the academic track school (*Gymnasium*) and the comprehensive track school (*Oberschule*). As of 2021, there are eight academic schools and 34 comprehensive schools. Both school types are publicly funded and managed, and no fees are charged. Although there are some differences between both types of secondary schools, both school types are generally of equal value and students can obtain the same school-leaving certificates in both school types. However, academic schools are often regarded by parents as more favourable due to their uniform learning pace, which allows for graduation with a university entrance qualification one year earlier than in comprehensive schools, as well as their more homogeneous composition.

In 2011, the Bremen Ministry of Children and Education (onwards: school board) adopted an IA mechanism based on a variation of the Boston mechanism, described in Abdulkadiroğlu and Sönmez (2003).³² For public secondary schools, parents submit an application for their child in February of the year that their child completes the 4th and final grade of primary school, ranking at most three schools (rank order list, onwards: ROL).³³ Once all applications have been submitted, applicants are assigned to the school they have ranked first, provided the number of applications does not exceed the number of seats. However, if the number of applicants is greater than the number of seats for that school, applicants

³¹ This is an index for needs-based/weighted school funding. The higher the index, the more additional funds. The index is ordinal and varies between 1 and 5.

³² The school board decided on a school reform in 2007, which was implemented from 2009 onwards. In 2011, all schools were transformed into either academic or comprehensive schools. Thus, in the school year 2011, parents had to submit a ROL for the first time.

³³ Students can also opt out of Bremen's public school system by attending a private school or another school outside of Bremen.

are given priority depending on academic performance (onwards: high-performing). For admission to comprehensive schools, it is also relevant whether the primary and the secondary school are affiliated (onwards: affiliation). Each primary school has at least one affiliated comprehensive school, typically the schools that are closest to the primary school. Applicants with higher priority are accepted first, and if the school has no seat left afterwards, the remaining students are rejected. If there is excess demand within a priority group, a lottery is conducted within the priority group. A multiple tie-breaking rule is used, meaning that ties are broken within each priority group through the random lottery in every school independently (Ashlagi and Nikzad, 2020). In academic schools, there is only one priority group, which is based on academic performance; in comprehensive schools, the first priority group is determined by both performance and affiliation, while the second priority group is based solely on affiliation. In the next round, rejected applicants apply for a seat in the second-ranked school. If there is excess demand, seats are again assigned according to the schools' priority lists and the lottery, as described above. The process is repeated in the third round. If the student cannot be assigned to any of the schools on the ROL after the third round, parents will receive a notification from the school board. Following a counselling session, they will be offered another school place. The wait list for schools that have reached their full capacity remains active until the end of the first term of the respective school year.³⁴

3.2.2 Translation of School Assignment Information

In order to understand the pitfalls of the applied IA mechanism, parents need to thoroughly understand it. A common mistake, for example, is to list an oversubscribed school as the first choice without belonging to a priority group. If the student is rejected, either due to not meeting the criteria for the priority group or by losing the admission lottery, the second and third choices may already be filled with students who listed those schools as the top preference. Since the mechanism accepts students immediately and does not revisit placements in later rounds (hence the term 'immediate acceptance'), this can result in the student being assigned to a much lower-ranked or administratively allocated school. These outcomes are not the result of unfair treatment, as centralised admission procedures are generally perceived to be fair and efficient. However, if some families lack the necessary information to navigate these procedures effectively, the fairness of access, not the mechanism itself, is compromised, leading to systematically unequal outcomes.

Before 2017, official information on student assignments was only available in German. However, Bremen's school board faced an increasing share of migrant students, also due to the refugee influx since 2015. To visualise this: in 2014 only 2.95% of all students have visited a so-called 'welcome class', where refugee students first learn the German language before they start their regular school career in a regular class. This share was 9.22% in 2017 and peaked at 11.44% in 2021. Most students in welcome

³⁴ Appendix II-A provides further information on the two different school types and the admission.

classes indicate Arabic as the language spoken at home. Given this linguistic heterogeneity in Bremen's schools, for school matching assignments from 2017 onwards, information was translated into six languages: Arabic, English, French, Persian, Russian, and Turkish.³⁵ The information material, in form of a brochure, was made available online and also distributed on parents' consultation days – specifically those held at the end of the school term, when parents receive their child's report and the form for ranking the preferred schools. However, I do not have data on which schools provided the information in printed form and which referred parents to the online version. This newly translated material covers around 21% of the total student population. However, several sizable groups of students whose language spoken at home is not among the six translated languages remain without access to translated materials, e.g., Kurdish, Polish, Albanian, Bulgarian. This group comprises 13% of the total student population and serves as the control group. The remaining students are either German speaking (65%) or have missing information for the language spoken at home and mother tongue (1%).³⁶

The information material on the admission procedure contains the exact same content in all available languages. Among other things, it informs about the admission procedure that a) there are hardship cases and how to apply for them, b) both school types have different priority groups, and that c) a lottery decides if the number of students exceeds the number of seats at the chosen school. In addition, the information material describes requirements that need to be fulfilled to be classified as 'high-performing' which leads to better chances to get to a preferred schooling place. The translation also tells parents that they can freely choose the secondary school for their children and that they should make use of all three available choices, specifically, it says (Freie Hansestadt Bremen – Die Senatorin für Kinder und Bildung, 2025):

“Your first, second, and third choices represent a ranking. Due to the limited number of places, it can happen that your child is not accepted at your first-choice school. They then only participate in the further admission procedure if second and third choices have been given. Otherwise, they will be assigned to another school after consultation with the education authority.”

Lastly, it gives an overview of important dates, such as consultation days, application deadline, and decision outcome date, and whom to contact for help.³⁷

Presumably, the translated material helps parents to understand the student matching assignment, affecting parents' school choices (Vogel and Stock, 2017). However, I cannot assume that all parents read or care about the translated information. Neither can I assume that parents get new insights, as unofficial information, e.g., word-to-mouth, may be exchanged among parents regarding the student

³⁵ Accessible language is also available since 2017.

³⁶ If information on the language spoken at home is missing (564), I consider the mother tongue. That way, I identify 450 more students. I drop the remaining 114 students who do not state either type of language as I cannot assume whether they might be treated or not.

³⁷ **Figure 3-3-A** shows the English version of the brochure as an example. According to the school board of Bremen, further support is only available in German.

assignment, which could be correct, false, or incomplete. Hence, I argue that I can only investigate the intention-to-treat effect. Since this might ultimately affect the student assignment, the assumption is that differences between the treatment and the control group, pre- and post-intervention, can be identified. During the observation period, there were no other changes in the assignment mechanism, school law, or related policies, allowing me to attribute the observed effects solely to the translations.³⁸ German students are omitted from the analysis, as they have always had the opportunity to understand the assignment process and make strategic school choices or not – a pattern that is also reflected in the data.³⁹

3.3 Data

I use administrative individual-student level data, provided by the school board of Bremen, from 2011 to 2021, covering all periods after the introduction of the school matching procedure. The data contain information on students' characteristics, such as birth date, gender, eligibility for welfare benefits, first and second citizenship, country of birth, mother tongue, language spoken at home, attended school and class, school type, special educational needs, and other variables. Basic information on schools, such as location, index of social composition (for secondary schools), the capacity of schools for incoming cohorts, the maximum number of students per class, and the average GPA of leaving certificates for each secondary school, is also available. The latter is publicly available for parents and indicates school performance. In Germany, the GPA of leaving certificates can vary from 1.0 (very good) to 4.0 (sufficient). A final grade of 5.0 is a failing grade. I observe a variation of the average GPAs across secondary schools of 1.97 to 3.38.

Table 3-1 reports descriptive statistics for all 39,904 students who handed in a ROL between 2011 and 2021. About 50% of the students are female. About 32% hold the *Bremen Pass* (*Bildungs- und Teilhabepaket*, BuT), indicating eligibility for welfare benefits. This pass is for low-income households and supports families to participate in social and cultural life. Half of the students are migrants.⁴⁰ About 35% of the students have competencies in Mathematics and German that exceed a given threshold at the end of primary school ('high-performing'). Comparing German students with treated and control students shows that twice as many German students are high-performing than non-German speaking students, while the latter group includes twice as many students that are eligible for welfare benefits. The largest groups of treated students speak Turkish (44%), Arabic (23%), and Russian (23%) at home.

³⁸ In fact, when the last school reform was passed, the major parties in Bremen signed an agreement ('Bremen School Peace'), ensuring the school system would remain unchanged for ten years, regardless of any changes in government. That agreement was extended for another ten years in 2018.

³⁹ Arguably I could also compare German students with migrant students who receive a translation to see how closing information asymmetry might affect school choice and admission. However, I have decided against it because migrant students are more comparable to each other than to German students in terms of observable and unobservable characteristics. Moreover, the parallel trends assumption does not hold, which, however, is a crucial assumption for the difference-in-differences estimation.

⁴⁰ If one of the following criteria applies, I define the student as a migrant: a) has another or a second citizenship in addition to German citizenship, b) the place of birth is not Germany or c) mother tongue or the language spoken at home is not German.

In the control group, students most frequently speak Kurdish (20%), Polish (15%), and Albanian (10%, not shown in **Table 3-1**).

As I am interested in the effects of the translation on school choice behaviour, I focus on a set of related variables, which are also shown in **Table 3-1**. The outcomes are divided into three categories: a) school choice, b) school admission, and c) secondary school composition. School choice outcomes are a.1) first-ranked school on the ROL is an academic school, a.2) at least one academic school on the ROL, a.3) complete ROL with use of three choices, a.4) first-ranked school is a safe school,⁴¹ and a.5) home-school distance to the first-ranked school. To fully capture the effect of the translated information, it is not enough to only look at the choices made, but also if students' admissions differ. Consequently, I also investigate b.1) admission to an academic school, b.2) admission to the first-ranked school, b.3) participation in the admission lottery, b.4) admission to a school outside the ROL, and b.5) home-school distance to admitted school. If families better understand the allocation mechanism, I expect to observe differences in admission. All school choice and admission variables are binary. Finally, I estimate whether or not treated students become more selective for their secondary school in terms of c.1) migration- and c.2) welfare benefit-share, c.3) social index of secondary school, and c.4) schools' average GPA. If parents understand the matching mechanism better and value a certain school environment, I should observe differences in the characteristics of ranked secondary schools as well.

⁴¹ A so-called 'safe school' is a school, where admission probability equals one (de Haan et al., 2023). Hence, all students are assigned to this school in the first round of the matching assignment.

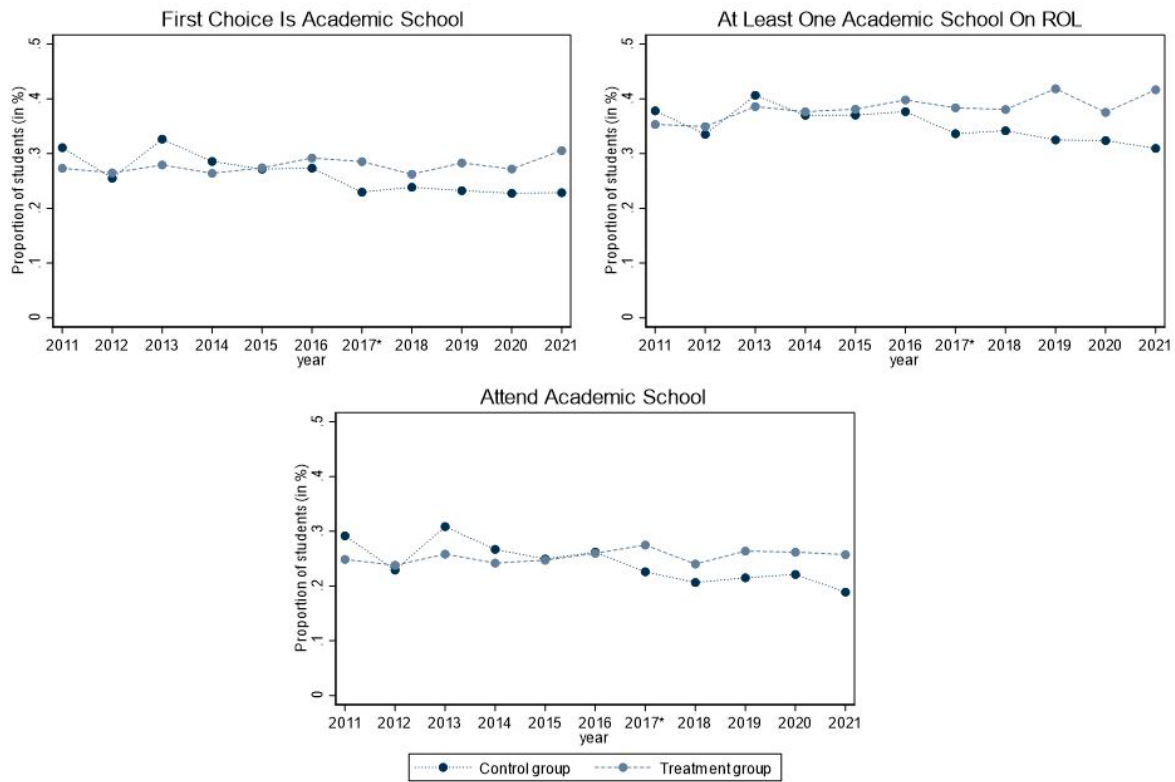
Table 3-1 Descriptive Statistics

	All participants	German speaking	Non-German speaking	
	(1)	(2)	treatment group (3)	control group (4)
<u>Student characteristics</u>				
Female	0.493	0.491	0.495	0.498
Migrant	0.501	0.240	1.000	1.000
High-performing	0.350	0.429	0.206	0.186
Welfare benefit	0.316	0.213	0.501	0.537
<u>Language spoken at home</u>				
German	0.646	1.000	-	-
Arabic	0.048	-	0.227	-
English	0.013	-	0.063	-
French	0.003	-	0.016	-
Persian	0.007	-	0.034	-
Russian	0.046	-	0.225	-
Turkish	0.089	-	0.435	-
<u>School choice</u>				
1 st ranked academic school	0.301	0.317	0.278	0.255
At least one academic school	0.436	0.470	0.384	0.347
ROL = 3	0.769	0.792	0.729	0.723
1 st ranked school is a safe school	0.452	0.432	0.480	0.511
<u>Admission</u>				
Attending academic school	0.281	0.299	0.255	0.235
Attending 1 st ranked school	0.874	0.888	0.844	0.859
Participating in admission lottery	0.355	0.354	0.371	0.338
Attending school on ROL	0.961	0.964	0.955	0.960
<u>Secondary school characteristics</u>				
Migration	0.461	0.420	0.538	0.546
Welfare benefit	0.338	0.304	0.397	0.411
Social index	2.711	2.476	3.132	3.203
GPA	2.450	2.433	2.481	2.481
Observations	39,904	26,090	8,303	5,397

Note: The table presents descriptive statistics for those 39,904 students who participate in the student matching assignment in 2011 to 2021 in the city of Bremen. In Column (2) to (4), observations are reduced to students who (2) speak German at home, (3) treated students who speak Arabic, English, French, Persian, Russian or Turkish and (4) non-treated students who speak other languages. Most spoken language in the non-treated group are Kurdish (20%), Polish (15%), Albanian (10%) and Bulgarian (5%).

With regard to these variables, the descriptive statistics reveal educational disparity as German-speaking students consistently perform better in school choice and admission variables. Also, school characteristics of secondary schools of treated and control groups differ, such as migration- and welfare benefit-share, social index, or schools' average GPA. Again, German-speaking students end up in a more favourable secondary school compared to those students whose language at home is not German. These overall differences between German and non-German speaking students show the importance of measurements to increase educational equity, e.g. by closing information gaps. There are only minor differences between the treatment and control groups, with treated students being slightly better off overall. However, these small differences are descriptive and for a causal interpretation they need to be validated by a DiD approach and by controlling for students' characteristics. This method focuses not on the minor baseline differences, but rather on how the treatment and control groups respond to the translation. Comparing average differences to the pre-treatment situation (**Appendix II-C**) reveals that differences between treatment and control groups become bigger for school choice and admission variables after the translation. In fact, the treatment group has improved in almost all school choice and admission outcome variables, while this apparent trend is not seen in the control group.

This pattern is illustrated in **Figure 3-1**, which displays the unadjusted mean values of three key outcomes: the proportion of students selecting an academic school as their first choice, the proportion of students selecting an academic school somewhere on the ROL and the proportion who ultimately attend an academic school. In the pre-treatment period (2011–2016), the treatment and control groups are relatively similar, with only minor and inconsistent differences in average shares. This suggests an absence of systematic divergence prior to the intervention. Starting in 2017, the treatment year, a pronounced and persistent gap emerges between the two groups, with consistently higher values observed in the treatment group. This divergence is evident in all three outcome measures and may reflect the impact of the translation intervention.

Figure 3-1 Descriptive Trends in School Choice and Admission

Note: The panels display the yearly average shares of students who selected an academic secondary school as their first choice (top panel, left), those who select an academic school somewhere on the ROL (top panel, right) and those who ultimately attended one (bottom panel), separately for treatment and control groups.

3.4 Empirical Strategy

The research design estimates the impact of translating information material into Arabic, English, French, Persian, Russian, and Turkish on school choice outcomes by comparing students whose spoken language is one of these languages to students with another non-German language spoken at home. This results in a classical difference-in-differences setting that exploits exogenous variation from students whose language spoken at home is not included in the translations. I run a standard DiD specification and estimate variants of the following equation:

$$Y_{ilbt} = \alpha + \delta(Treated_i \times Translation_t) + \beta Treated_i + \gamma Translation_t + \lambda X'_{it} + \theta_l + \rho_b + \tau_t + \varepsilon_{ilbt}$$

where the outcome variable Y_{ilbt} denotes a) school choice, b) admission and c) secondary school composition outcomes for student i in school year t from primary school l and building block b .

$Treated_i$ is a dummy variable that takes the value one for migrant students who speak a language at home that is one of the translated languages and zero for migrant students who remain without

a translation. Once again, I estimate the effect of an intention-to-treat as the translation of the information material does not necessarily lead to all parents reading it or gaining new insights from it. Since 2017 is the period when the information was translated, $Translation_t$ is a dummy variable that takes the value one for school year 2017 and onwards, and zero otherwise. δ is the coefficient of interest as it captures the average intention-to-treat effect of the treatment group relative to the control group.

X'_{it} is a matrix of individual characteristics that are likely to be correlated with the outcomes of treated students, such as age, gender, migration background, performance and eligibility for welfare benefits. I also include fixed effects for attended cohort (τ_t), leaving primary school (θ_l), and place of residence (ρ_b). I incorporate attended primary school to account for the circumstance that some primary schools provide the information material in a printed version during consultation days, while others only refer to it online.⁴² I further include place of residence (building blocks) to capture social composition in the neighbourhood, network effects (Ding and Schotter, 2017), the number of schools in the neighbourhood, and student density. ε_{ilbt} is the error term.

The primary identifying assumption of the DiD approach is the common trend assumption, which implies that in the absence of the translation, both groups of non-native speaking students would have behaved identically, e.g., in the submitted ROL or in the final admission to an academic school. To demonstrate that students in the treatment and control group do indeed follow parallel trends, I apply the same reasoning as in the above equation but now I estimate an event study design by interacting the impact of translation by a survey-year indicator for selected variables. The results of this event study are presented in **Figure 3-2**.⁴³ I conclude that there are no significant pre-treatment differences and that, without the treatment, this would not have changed. Thus, δ is a reliable estimate of the causal effect of information.

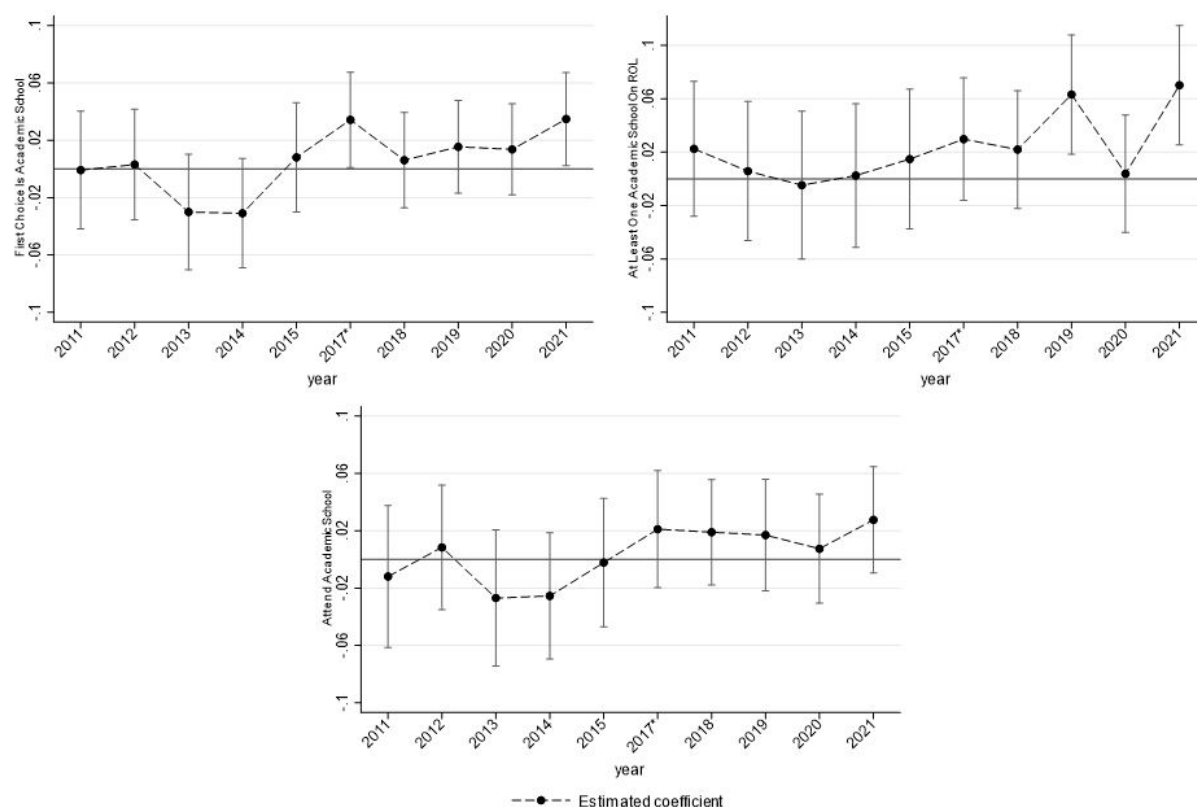
There are two potential threats to the validity of my identification strategy. One threat is treatment contamination. Kurdish-speaking students constitute approximately 20% of the control group, making them the largest language subgroup. Families from this ethnic group predominantly resided in regions of Turkey, Syria, Iraq, and Iran, where they may also speak the official languages of these countries. This could bias the estimates downward, as parents may be able to comprehend the translated informational materials in Arabic, Turkish, or Persian. To address this concern, I conduct various robustness checks (i) I reclassify Kurdish-speaking students as part of the treatment group, (ii) I exclude Kurdish-speaking students entirely from the sample to observe how the results change, and (iii) I treat only Kurdish-speaking students as the treated group while excluding other treated students. Another potential threat arises from confounding factors during the observation period, such as the introduction of the assignment method in 2011, hence the first school year, or the COVID-19 pandemic. To mitigate

⁴² I do not have data on which schools use which method, nor whether this practice changes over time.

⁴³ For variables on secondary school composition, kindly refer to **Appendix II-D**. The common trend assumption for school choice and admission variables also holds when excluding covariates and fixed effects.

this, I restrict the observation period to the years 2012–2019. These potential threats to the identification strategy are further examined in detail in the robustness checks presented in **Chapter 2.3.6**.

Figure 3-2 Common Trends in an Event Study Design – School Choice and Admission



*Note: Each panel shows for the years 2011 till 2021 the relevant school choice (top panels) and admission (bottom panel) outcome variables. Although the effect of the translation of information material cannot be pinpointed to a specific year for the attendance, the regression in **Chapter 2.3.5** shows an overall significant average effect. The regression includes the full set of controls and fixed effects. The confidence interval is set at a 95% level. The year of reference is 2016, which is why this year is omitted. First treatment in 2017. Robust standard errors.*

3.5 Results

3.5.1 Baseline Results

Table 3-2 reports the difference-in-differences regression estimates on school choice and admission outcomes. I estimate three model specifications: (1) an empty model, (2) a model that includes covariates, and (3) a fixed effects model that builds on (2) by additionally controlling for attended cohort (year) and primary school, and building blocks. Results in (3) suggest that treated students are roughly 2.9 p.p. more likely to rank an academic school first. This amounts to an almost 10% increase compared to the pre-treatment control mean. In the panel below, I conclude that not only do treated students aim more for academic schools compared to the control group, but they are also more likely to attend them (3.1.

p.p.). This is an almost 12% increase relative to the pre-treatment mean. Presumably, these treated students either understand priority groups better or they aim for not oversubscribed academic schools, where priority grouping does not apply, and all students get admitted. To investigate this, I perform a heterogeneity analysis in **Chapter 3.5.2**. If treated students understand the relevance of priority groups better through the translation, I should particularly see an effect for high-performing students. If the effect is stronger for low-performing students, it indicates an aim for safe academic schools. For the other outcomes in **Table 3-2**, I find no effects.

Table 3-2 Difference-in-Differences Regression Estimates of Information Intervention on School Choice and Admission

Dependent variables	Treatment effects		
	(1)	(2)	(3)
School Choice			
1 st ranked school is an academic school	0.061*** (0.016)	0.054*** (0.013)	0.029** (0.014)
At least one academic school on ROL	0.067*** (0.017)	0.061*** (0.015)	0.031** (0.015)
All choices on ROL used	0.010 (0.016)	0.010 (0.016)	0.004 (0.016)
1 st ranked school is a safe school	0.019 (0.018)	0.021 (0.017)	-0.003 (0.019)
Home-school distance to the 1 st ranked school	95.74 (74.80)	80.14 (73.02)	55.12 (74.50)
Admission			
Attending academic school	0.066*** (0.015)	0.058*** (0.013)	0.031** (0.013)
Attending 1 st ranked school	0.005 (0.013)	0.002 (0.013)	-0.001 (0.018)
Participating in admission lottery	-0.025 (0.017)	-0.024 (0.017)	0.009 (0.017)
Attending a school not on the ROL	-0.009 (0.007)	-0.008 (0.007)	-0.009 (0.008)
Home-school distance to attending school	72.63 (74.34)	60.87 (72.77)	58.32 (73.56)
Covariates	NO	YES	YES
Fixed Effects	NO	NO	YES
Observations	13,700	13,700	13,700

Estimation method: OLS regression. Covariates include age, gender, eligibility for welfare benefits and performance. Cohort, school and city block fixed effects. Sample period: 2011 – 2021. First treatment in 2017. Robust standard errors in parentheses.

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 3-3 shows difference-in-differences estimates for first-ranked and admitted secondary school composition. Results point to a deliberate segregation as students aim for schools with a more favourable school composition with a lower social index (0.6 units) and welfare benefit share (1.1 p.p.) in the upper panel. This is in line with previous literature, e.g. Ajayi et al. (2026), who conclude that there is increased selectivity when it comes to the characteristics of a school, or Lagos et al. (2025), who document sorting

and segregation in centralised admission lotteries. The lower panel confirms that treated students attend schools with a 1.3 p.p. lower welfare benefit share. I am hesitant to draw conclusions from the GPA estimate due to signs of divergence and a violation of the parallel trend assumption in year 2015 (**Appendix II-D**). Consequently, a causal interpretation of my estimate may not be valid, and the effect cannot be confidently attributed to the translation. Nevertheless, the results would align with the argument that, following the translation, treated students choose and are admitted to schools with an on average lower (better) GPA, which is more desirable for students.

Table 3-3 Difference-in-Differences Regression Estimates of Information Intervention on Secondary School Composition, Ranked and Admitted Schools

	Treatment effects			
Dependent variables	(1)	(2)	(3)	Obs.
School Choice				
Migration share	0.006 (0.006)	0.006 (0.005)	-0.001 (0.005)	13,700
Social index	-0.055 (0.042)	-0.055 (0.040)	-0.063* (0.036)	13,611
Average GPA	-0.004 (0.007)	-0.005 (0.006)	-0.016** (0.006)	11,836
Welfare benefit share	-0.006 (0.006)	-0.006 (0.006)	-0.011** (0.005)	13,700
Admission				
Migration share	0.006 (0.006)	0.006 (0.005)	-0.000 (0.005)	13,685
Social index	-0.040 (0.042)	-0.042 (0.039)	-0.033 (0.035)	13,528
Average GPA	-0.002 (0.006)	-0.003 (0.006)	-0.012* (0.006)	11,766
Welfare benefit share	-0.008 (0.006)	-0.009* (0.005)	-0.013** (0.005)	13,685
Covariates	NO	YES	YES	ALL
Fixed Effects	NO	NO	YES	ALL

*Estimation method: OLS regression. Covariates include age, gender, eligibility for welfare benefits and performance. Cohort, school and city block fixed effects. Number of observations differ as GPA for 2011 is missing and social index for newly opened schools are not available. Number of observations do not change across specifications of the same outcome variable. School choice variables are based on the first-ranked school. Sample period: 2011 – 2021. First treatment in 2017. Robust standard errors in parentheses. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.*

3.5.2 Effect Heterogeneity

It can be argued that the informational intervention was partly motivated by the increase of students in welcome classes and students that do not speak German at home. As previous studies show, subgroups value information differently and, consequently, might not be affected equally by the intention-to-treat, e.g., Lucas and Mbiti (2012) for stronger strategic mistakes for female students, Corcoran et al. (2018) and Cohodes et al. (2025) for the differential effects between native and non-native speakers and

Hastings et al. (2009), Kapor et al. (2020), de Haan et al. (2023) and Ajayi (2024) for other factors such as parental effort or income. To come close to that, I will investigate potential heterogeneous effects with respect to performance, eligibility for welfare benefits, and gender.⁴⁴ Results are shown in **Table 3-4** and **Table 3-5**.

For performance, it is plausible that the take-up of treatment and the resulting treatment effects of the intervention are stronger for high-performing students than for the average student. For example, treated high-performing students might understand that chances are better than expected to attend an academic school; they scrutinise their subjective admission probability and rank schools differently and are more successful. Another potential channel is that high-performing students exhibit stronger treatment effects because teachers are more likely to emphasize their broader range of viable school options. This encouragement may enhance the subgroup's responsiveness to the information provided. **Table 3-4** (3) confirms this as high-performing students are 8.5 p.p. more likely to rank an academic school first and are willing to travel additional 430 meters to school. The lower panel suggests that the admission to an academic school increased by 8.2 p.p. for high-performing students and that the home-school distance increased by 430 meters. In addition, high-performing students aim for and attend schools with a lower welfare benefit share by slightly more than 3.0 p.p. Comparing that with low-performing students, I conclude that the results in **Table 3-2** are primarily driven by high-performing students, though, low-performing students also rank academic schools somewhere on their ROL (2.8 p.p.). Within the observation period, of all low-performing students, 8.5% attend an academic school. In 75% of all academic schools, the chances for low-performing students to be admitted are close to zero. Hence, low-performing students rank other academic schools, which are not in high demand, somewhere on the ROL.

⁴⁴ I also examine heterogeneous effects based on whether the birthplace is Germany or elsewhere, using this as a proxy to investigate family's duration in Germany and potential German language proficiency. However, I do not find evidence of differences between students born in Germany and those born abroad. Results are available upon request.

Table 3-4 Heterogeneity – School Choice and Admission

Dependent variables	(1) Welfare benefit		(2) Gender		(3) Performance	
	no	yes	female	male	low	high
<i>School Choice</i>						
1 st ranked school is an academic school	0.012 (0.022)	0.044** (0.019)	0.037* (0.022)	0.027 (0.022)	0.019 (0.014)	0.085* (0.045)
At least one academic school on ROL	0.027 (0.025)	0.035* (0.021)	0.036 (0.024)	0.031 (0.024)	0.028* (0.016)	0.028 (0.038)
All choices on ROL used	-0.000 (0.027)	0.012 (0.024)	-0.002 (0.026)	-0.009 (0.026)	0.011 (0.018)	-0.052 (0.044)
1 st ranked school is a safe school	-0.037 (0.031)	0.010 (0.027)	-0.018 (0.029)	-0.041 (0.030)	-0.027 (0.020)	-0.014 (0.018)
Home-school distance to the 1 st ranked school	111.9 (123.8)	-22.7 (106.9)	67.75 (114.8)	62.53 (119.1)	-7.49 (75.37)	428.5* (242.7)
<i>Admission</i>						
Attend academic school	0.002 (0.023)	0.057*** (0.018)	0.039* (0.021)	0.028 (0.018)	0.023* (0.013)	0.082* (0.045)
Participate in admission lottery	0.001 (0.031)	0.007 (0.024)	0.006 (0.029)	0.034 (0.029)	0.008 (0.020)	0.017 (0.049)
Attend 1 st ranked school	0.009 (0.025)	0.007 (0.019)	-0.015 (0.023)	-0.001 (0.023)	0.002 (0.017)	0.007 (0.027)
Attend a school outside ROL	0.001 (0.015)	-0.025** (0.010)	-0.007 (0.013)	-0.007 (0.012)	-0.011 (0.010)	0.009 (0.020)
Home-school distance to attending school	97.84 (127.2)	7.58 (105.2)	36.66 (117.3)	149.5 (119.1)	-17.90 (75.86)	424.8* (238.2)
Observations	6,642	7,058	6,798	6,902	10,984	2,716

Estimation method: OLS regression. Covariates include age, gender, eligibility for welfare benefits and performance. Cohort, school and city block fixed effects. Sample period: 2011 – 2021. First treatment in 2017. Robust standard errors in parentheses.

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Comparing students eligible for welfare benefits, I find that welfare benefit students partly drive the effects reported in **Table 3-2**: they are 4.4 p.p. more likely to rank an academic school first and 3.5 p.p. more likely to include one anywhere on their rank-ordered list. They are also 5.7 p.p. more likely to actually attend an academic school. They are also likelier to attend a school with a lower welfare benefit share by 1.6 p.p. (**Table 3-5** (1)). Interestingly, no such effect is observed for students who do not receive welfare benefits. This is noteworthy, as existing research suggests that school choice typically exacerbates disparities in achievement and access to high-performing schools between low- and high-socioeconomic families (Hastings et al., 2009). The translation, however, appears to mitigate this dynamic.

Table 3-4 (2) shows interesting differences between the school choice of female and male students. That female and male students choose schools differently is not disputed (Schneeweis and Zweimüller, 2012; Lucas and Mbiti, 2012). However, the results show that female students are more likely to rank an academic school first (3.7 p.p.) and are also more likely to attend it (3.9 p.p.) as the result of the informational intervention. Hence, this indicates that female students or their parents react differently to information on school choice. **Table 3-5** (2) shows that they also aim for secondary schools with a lower welfare benefit share (1.7 p.p.) and also attend the school with a lower share of welfare benefit students (1.9 p.p.). Interestingly, male students and their parents do not show any effect. In this regard,

Lucas and Mbiti (2012) show that female students are more likely to make more errors in the school choice process. Similar to welfare benefit students, female students benefit from the informational intervention, suggesting the dismantling of faulty admission beliefs and a better understanding of the school choice mechanism after the translation.

Table 3-5 Heterogeneity – Secondary School Composition

Dependent variables	(1) Welfare benefit		(2) Gender		(3) Performance	
	no	yes	female	male	low	high
<i>School Choice</i>						
Migration share	-0.001 (0.008)	0.006 (0.007)	-0.002 (0.007)	0.000 (0.008)	0.003 (0.005)	-0.014 (0.020)
Social Index	-0.047 (0.060)	-0.037 (0.051)	-0.087 (0.056)	-0.031 (0.057)	-0.035 (0.038)	-0.172 (0.141)
Average GPA	-0.013 (0.011)	-0.009 (0.009)	-0.011 (0.010)	-0.022** (0.010)	-0.010 (0.007)	-0.310 (0.027)
Welfare benefit share	-0.011 (0.009)	-0.008 (0.008)	-0.017* (0.009)	-0.006 (0.009)	-0.006 (0.006)	-0.034** (0.017)
<i>Admission</i>						
Migration share	0.004 (0.008)	0.003 (0.007)	-0.001 (0.007)	-0.001 (0.008)	0.002 (0.005)	-0.010 (0.020)
Social Index	0.053 (0.060)	-0.064 (0.049)	-0.053 (0.055)	-0.019 (0.056)	-0.005 (0.036)	-0.163 (0.142)
Average GPA	-0.013 (0.011)	-0.007 (0.008)	-0.007 (0.010)	-0.015 (0.010)	-0.006 (0.006)	-0.027 (0.027)
Welfare benefit share	-0.009 (0.009)	-0.016** (0.008)	-0.019** (0.009)	-0.011 (0.009)	-0.009 (0.006)	-0.035* (0.021)
Observations	6,642	7,058	6,798	6,902	10,984	2,716

*Estimation method: OLS regression. Covariates include age, gender, eligibility for welfare benefits and performance. Cohort, school and city block fixed effects. Number of observations differ as GPA for 2011 is missing and social index for newly opened schools are not available. School choice variables are based on the first-ranked school. Sample period: 2011 – 2021. First treatment in 2017. Robust standard errors in parentheses. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.*

3.6 Robustness Checks

Two potential concerns could affect the robustness of the results presented. First, the possibility of treatment contamination by Kurdish-speaking students, as they may understand the translated information in another language. Second, the influence of confounding effects during the observation period. I address these concerns in **Table 3-6** providing evidence that the main results, school choice and admission to academic schools, remain robust.

Table 3-6 Robustness Checks

	1 st ranked school academic school (1)	Attending academic school (2)
0. Main results from Table 2		
<i>Treated × Translation</i>	0.029** (0.014)	0.031** (0.013)
Observations	13,700	13,700
A. Treatment: include Kurdish-speaking students		
<i>Treated × Translation</i>	0.027** (0.015)	0.028** (0.014)
Observations	13,700	13,700
B. Exclude Kurdish-speaking students		
<i>Treated × Translation</i>	0.030** (0.015)	0.030** (0.014)
Observations	12,619	12,619
C. Treatment: only Kurdish-speaking students		
<i>Treated × Translation</i>	0.016 (0.029)	0.011 (0.027)
Observations	5,469	5,469
D. Shortened time period (2012 – 2019)		
<i>Treated × Translation</i>	0.032* (0.017)	0.037** (0.016)
Observations	9,671	9,671

*Estimation method: OLS regression. Covariates include age, gender, eligibility for welfare benefits and performance. Cohort, school and city block fixed effects. First treatment in 2017. Robust standard errors in parentheses. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.*

Kurdish-speaking students might influence the estimates because their parents may understand the translated informational materials in other languages, such as Arabic, Turkish, or Persian, which are official languages of their countries of origin. Since Kurdish-speaking students constitute the largest language group in the control group, it is important to address this concern. To do so, I conduct a series of robustness checks, as shown in **Table 3-6**. In panel A, I modify the treatment group by identifying Kurdish-speaking students as treated. In panel B, I exclude Kurdish-speaking students entirely from the sample to observe how the results change. Finally, in panel C, I specify only Kurdish-speaking students as treated and exclude all other treated students. The results from panel A and B in **Table 3-6** show similar point estimates and standard errors as compared to the baseline regression. Additionally, when only Kurdish-speaking students are included in the treatment group, as shown in panel C, there is no observed effect on school choice or admission to academic schools. These findings indicate that Kurdish-speaking students do not exhibit an overall strong effect, if any, thereby supporting the continued use of the primary identification strategy based on the language spoken at home. In this regard, the results appear robust. I also examine whether the results are driven by any specific language group and find no significant differences.⁴⁵ This further supports the validity of the treatment specification, as all treated

⁴⁵ Results upon request.

language groups seem to react in a similar pattern. Consequently, the reported results reflect the average intention-to-treat effect.

In panel D of **Table 3-6**, I restrict the sample to a shorter period (2012–2019) for two reasons. First, the centralised admission procedure and the immediate acceptance policy was introduced in the 2011 school year. It is likely that during the initial implementation year, many parents, regardless of language spoken at home, did not fully understand the new assignment system or were unaware of the changes. Migrant families, in particular, who might not have comprehended the informational materials provided exclusively in German, may have relied on informal sources of information, such as word-of-mouth, which, in this case, reflected outdated information from the previous year. To eliminate potential confounding effects associated with the policy's initial implementation, I exclude this first, 'trial' year from the analysis. Second, empirical evidence suggests that the COVID-19 pandemic influenced transition rates to academic schools in Germany, potentially due to disparities in access to technological resources or parents' ability to support their children (Breidenbach et al., 2023). Beyond this, I argue that the pandemic may also have altered access to information. Although information became available online, consultation days in schools were cancelled, and not all parents are equally tech-savvy. Additionally, the imposed contact restrictions reduced informal sources of information, such as interactions among parents. Therefore, I exclude the school years affected by COVID-19, specifically 2020/21 and 2021/22, from the analysis as well. The estimates in **Table 3-6** panel D exhibit slight changes in effect size as the get stronger and remain overall robust.⁴⁶

⁴⁶ As an additional robustness check, I apply a synthetic difference-in-differences estimator (Arkhangelsky et al., 2021), which constructs a weighted control group to more closely match pre-treatment trends, even though the standard parallel trends assumption is not violated in the baseline specification. This approach yields a more data-driven counterfactual and helps mitigate potential bias arising from residual differences in pre-trends or group composition. Because the method requires a balanced panel, language groups with irregular transitions from primary to secondary school (e.g., Thai or Korean) must be excluded; to preserve consistency of the main sample, I therefore do not report these results in the paper. The estimates are comparable in effect size and statistical significance to the baseline findings, if anything slightly larger in effect size.

3.7 Conclusion

To study the effect of an information intervention in the immediate acceptance mechanism in Bremen, a city state in Germany, I employ a difference-in-differences approach that exploits exogenous variation from students whose language spoken at home is not included in the translations. I utilize administrative panel data from Bremen's secondary-school matching assignment. This first-choice maximizing practice was implemented in 2011 and has required a strategic ranking of parents ever since. Evidentially, previous research has shown that parents make mistakes and have mistaken subjective beliefs about their admission probability. One of the most affected groups are non-native speaking families (around 35% in Bremen). I argue that the 2017 translation of school assignment materials into six languages (Arabic, English, French, Persian, Russian, and Turkish) helped reduce information gaps, enabling treated students to adjust their perceived admission probabilities and make more informed school choices. The newly translated information appears to lead to a better understanding of the matching practice, as the change in admission rates is similarly significant to that observed in school choice.

This paper contributes to the research on admission beliefs, value of information and information interventions in school choice. Previous studies show that parents misunderstand school choice mechanisms due to incorrect perceptions of admission probabilities and that intervention in different forms show mostly positive effects, though the value of information differs. This study evaluates a natural experiment in which information was translated into several languages and provided to a specific subgroup of non-native speaking students to improve their understanding of the applied school choice mechanism. To my knowledge, this is the first study to evaluate such translations with a coverage for an entire city and for multiple cohorts of students, offering a new perspective on the issue.

The results show that, on average, treated students are roughly 2.9 p.p. more likely to rank an academic school first and attend it (3.1 p.p.). Notably, there have not been any changes in either the matching assignment or the priority group for any school so that I can attribute these changes fully to the information intervention in the form of the translation of the information material. Additionally, my results show that parents deliberately segregate and choose preferably advantaged schools based on social composition, e.g., they rank and attend schools with a lower share of low-income students. These results vary across different student groups: welfare benefit recipients, high-performing, and female students benefit more than their counterparts from the translation. Above all, high-performing students show double the effect, indicating a better understanding of the school choice mechanism and applied priority groups.

While the paper's primary objective is to examine whether students and their parents respond to the provision of information in their native language, a broader consideration is the potential impact of such interventions on the overall equilibrium, e.g. the allocation of students. Specifically, there is a possibility that students in the control group, and also German-speaking students, may experience unintended consequences as treated students make more informed school choices. Beyond direct behavioural

responses, spillover effects such as increased competition for academic schools, loss of admission lotteries and capacity constraints warrant careful examination. While this disentanglement poses a limitation in this study, previous literature, including Andrabi et al. (2017) and Allende et al. (2019), has explored these dynamics and suggests that information interventions can serve as mechanisms for improving both efficiency and equity in education markets. Understanding these broader implications on education markets is therefore essential for the design and implementation of scalable interventions to optimize overall welfare while mitigating potential adverse effects. However, this requires further research within complex student assignment mechanisms, such as under the immediate acceptance framework.

3.8 Appendix II

Appendix II-A – Bremen’s Secondary School System

In Bremen, there are only two types of secondary schools: academic schools (*Gymnasium*) and comprehensive schools (*Oberschule*).⁴⁷ While there are some differences, e.g. maximum class size, teaching structure and curriculum, students can obtain the same school-leaving certificates. The school-leaving certificate qualifying for university entrance is obtained after eight years in academic schools and nine years in comprehensive schools. In 2021, Bremen had eight academic and 34 comprehensive schools.

The school types differ in their admission rules. For comprehensive schools, the first priority sorting depends on the affiliated primary school and an above-average performance. For these students, one-third of a comprehensive school’s capacity is reserved. The second priority group is defined solely by affiliation. Hence, if a high-performing student from an affiliated primary school loses the lottery within the first priority group, the student will be considered again within the second priority group – but competes with low-performing students. This is the only case where students can be considered twice within a preference on the ROL. If available, the remaining seats will be filled by lottery with students from non-affiliated primary schools. Wait list priority for comprehensive schools is 1) students from affiliated primary schools and 2) all other students. For academic schools, priority is only based on performance. Hence, after the first priority group, all other students will be admitted, if seats remain available. The wait list priorities differ: 1) performance, 2) siblings and 3) all other students.

General admission rules/priorities:

- f) Both school types accept up to ten percent so-called hardship cases. These are students a) which have special educational needs, b) who or whose family would be socially harmed by attending another school, or c) with siblings in the respective secondary school. Note that, according to the school board, such cases are identified under a compelling necessity and are rarely approved after assessment.
- g) Within each priority group, a lottery takes place if the number of applicants exceeds the number of available seats.
- h) Applications submitted after the application deadline will be given lower priority.
- i) If none of the schools on the ROL could be considered, parents will receive a mail from the school board. Following a counselling session, they will be offered another school place.
- j) The wait list for the over-demanded schools remains active until the end of the first term of the respective academic year.

⁴⁷ Freie Hansestadt Bremen – Die Senatorin für Kinder und Bildung (2025).

Appendix II-B – Official Information Flyer

Figure 3-3-A Official Information Flyer on the Centralised School Admission in English

BOOKLETS

Detailed information is provided in the booklets "Lower secondary level in Bremen: an introduction to general and academic secondary schools" and "The transition from Primary School to Year 5". They are available free of charge at primary schools and from the Senator for Children and Education.

ADDRESSES

If you require any further information, contact your child's school or the Senator for Children and Education directly.

Contact persons at the office of the Senator for Children and Education

Ms. Voß	0421 361-6413
Ms. Reinhardt	0421 361-27629
Ms. Tabaka	0421 361-10064
Mr. Wickremetunge	0421 361-27600

Published by
The Senator for Children and Education
Rembertiring 8-12
28195 Bremen

WWW.BILDUNG.BREMEN.DE

FREQUENTLY ASKED QUESTIONS

How does the admission procedure work?
Up to ten percent of places are awarded to hardship cases in the admission procedure. All other places at academic secondary schools and up to one third of places at general secondary schools are preferentially awarded to pupils with achievements above the default standard. The places at general secondary schools are preferentially awarded to pupils from the allocated primary schools. If the number of parents who listed a school as one of their choices exceeds the number of places at the school, places are allocated by lot.

When does the hardship provision apply?
Hardship cases can be recognised under certain conditions:

- Children with physical disabilities,
- Children with unreasonably high social burden or
- Children with siblings at the same school if non-admission would lead to family problems.

You can find more information online and in the booklets. The hardship application must be submitted directly to your chosen secondary school before the application deadline.

Is my child above the default standard?
You can tell how successful your child is from their learning development reports. A score from 1 to 10 is given for several skills per subject to indicate how well your child has learned them. This provides an overview of their strengths and development needs. Performance is considered above the default standard if, after the first half of Year 4, most scores are 9 or 10 for German and Mathematics.

Which foreign languages will my child learn?
English is the first foreign language at both types of school. A second foreign language can be chosen at general secondary schools from Year 6. The second foreign language is mandatory at academic secondary schools.

What does inclusion mean?
Inclusive education is provided for children with learning difficulties or special needs in Bremen. This means that all children learn together in the same class. By learning with and from their peers, everyone can make the most of their potential. In addition, this allows lots of scope for social and content-based learning: teaching something to someone else is an especially good way of learning it.

**From primary school to
SECONDARY SCHOOL**

Information for a successful transition

THE TRANSITION TO GENERAL OR ACADEMIC SECONDARY SCHOOL

When your child is in Year 4, you have to choose a type of secondary school. You have the choice between general secondary schools (Oberschule) and academic secondary schools (Gymnasium). All general education qualifications up to the general university entrance qualification (Abitur) can be obtained at both types of school.

You can freely choose the secondary school for your child
You can freely choose the school location. This also applies to the type of school if you attended the consultation session at the primary school.

Take advantage of all three choices
Be sure to include three different schools. Your first, second, and third choices represent a ranking. Due to the limited number of places, it can happen that your child is not accepted at your first-choice school. They then only participate in the further admission procedure if second and third choices have been given. Otherwise, they will be assigned to another school after consultation with the education authority.

Take advantage of all consultation services
Choosing a secondary school is a responsible decision that schools can help you with. Your primary school offers parent consultation days halfway through the school year. You can also get the application forms here. Information evenings and open days are held in both general and academic secondary schools in December and January.

GENERAL OR ACADEMIC SECONDARY SCHOOL?

General secondary school (Oberschule)	Academic secondary school (Gymnasium)
• The general university entrance qualification (Abitur) is acquired after 13 years • Performance-oriented school for all pupils • Joint learning at different ability levels • All aptitudes are both challenged and encouraged • Class size: 25 • A year team will consistently support the year group from Year 5 to Year 10	• The general university entrance qualification (Abitur) is acquired after 12 years • Increased learning pace for higher-achieving pupils • Joint learning at a single ability level • Greater time demands: up to 35 hours of lessons per week • Class size: 30 • A second foreign language is mandatory from Year 6

OVERVIEW OF DATES

Consultation services
Consultation days at primary schools and information events at general and academic secondary schools.

Application deadline
Please submit the application form to your primary school by 7th February, 2018. If you would like to transfer from a private primary school to a public school, please submit the application form to the Senator for Children and Education.

Admission procedure
If enough places are available at your first-choice school, your child will be accepted there. If not, a decision will be made for each school following the admission procedure.

Decision
You will receive a notification of admission from your new school known as the "Aufnahmebescheid". If none of your wishes could be taken into account, you will receive a letter from the education authority instead. This authority will then offer you a place at another school following a consultation.

Preparing for the new school
The school where you have been offered a place will provide you with information about the next steps. You will have the chance to find out more about the school at a parents' evening for Year 5.

Timeline:

- Nov. to Jan. Consultation services
- 7th February 2018 Application deadline
- Feb. to Mar. Admission procedure
- 7th March 2018 Decision
- Mar. to July Preparing for the new school

Note: The images show the English version of flyer for the 2025/26 school year. The flyers are updated annually, as the application deadline changes each year. The remaining information stays the same. The flyer was taken from the official website of the school board. Parents can either download it online or receive a printed copy from the school.

Appendix II-C – Pre-Treatment Descriptive Statistics

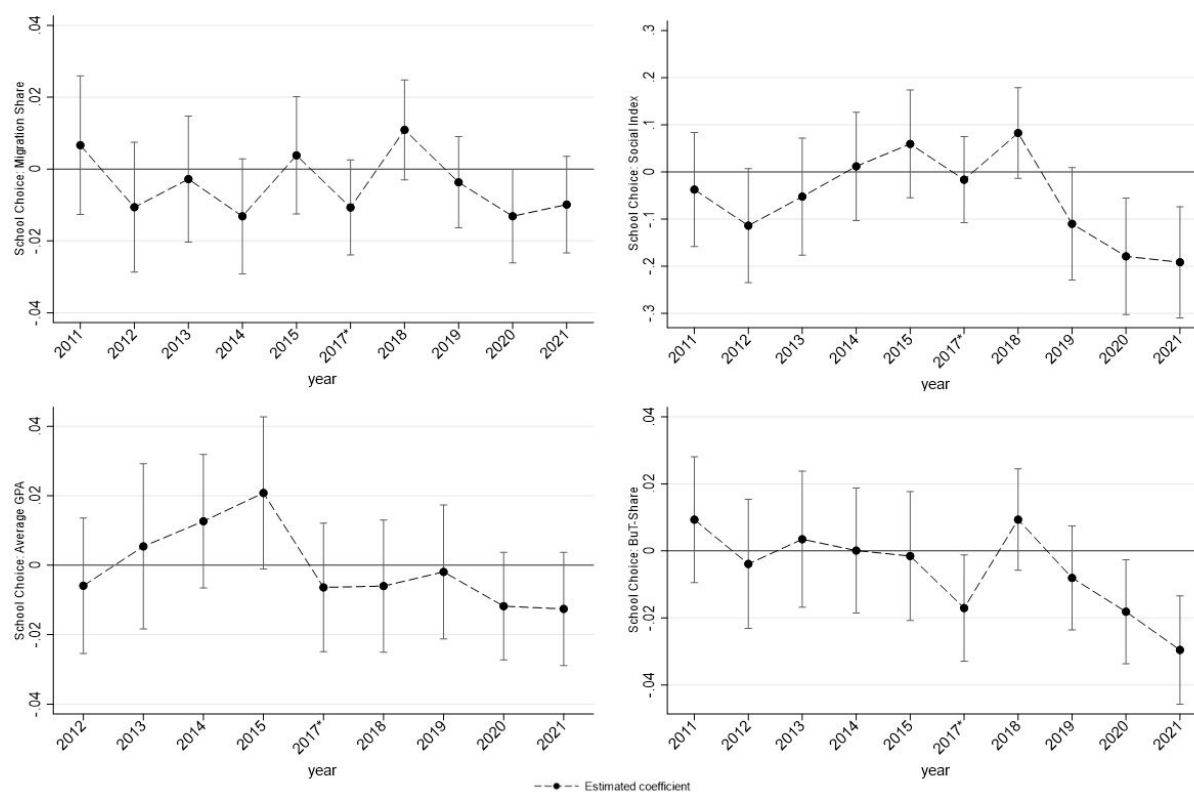
Table 3-7-A Descriptive Statistics, Pre-Treatment

	All participants	German speaking	Non-German speaking	
	(1)	(2)	translation (3)	No translation (4)
<u>Student characteristics</u>				
Female	0.490	0.488	0.491	0.499
Migrant	0.440	0.198	1.000	1.000
High-performing	0.360	0.423	0.220	0.211
Welfare benefit	0.322	0.237	0.502	0.559
<u>Language spoken at home</u>				
German	0.678	1.000	-	-
Arabic	0.032	-	0.164	-
English	0.008	-	0.044	-
French	0.002	-	0.013	-
Persian	0.005	-	0.025	-
Russian	0.047	-	0.249	-
Turkish	0.096	-	0.506	-
<u>School choice</u>				
1 st ranked academic school	0.297	0.304	0.275	0.285
At least one academic school	0.433	0.458	0.373	0.373
All choices on ROL used	0.736	0.763	0.677	0.671
1 st ranked school is a safe school	0.454	0.451	0.450	0.488
<u>Admission</u>				
Attending academic school	0.279	0.289	0.249	0.267
Attending 1 st ranked school	0.873	0.887	0.838	0.855
Participating in admission lottery	0.356	0.346	0.395	0.351
Attending school on ROL	0.966	0.969	0.958	0.968
<u>Secondary school characteristics</u>				
Migration	0.404	0.369	0.484	0.491
Welfare benefit	0.342	0.313	0.407	0.417
Social index	2.670	2.474	3.109	3.157
GPA	2.466	2.454	2.493	2.494
Observations	21,275	14,747	4,084	2,353

Note: The table presents descriptive statistics for those 21,275 students who participate in the student matching assignment in 2011 to 2016 in the city of Bremen. In Column (2) to (4), observations are reduced to students who (2) speak German at home, (3) treated students who speak Arabic, English, French, Persian, Russian or Turkish and (4) non-treated students who speak other languages. Students without indicating a language are dropped (91). Most spoken language in the non-treated group are Kurdish (16%), Polish (15%), Albanian (10%) and Bulgarian (5%).

Appendix II-D – Descriptive Evidence for Common Trends for School Composition Variables

Figure 3-4-A Common Trends in an Event Study Design – Secondary School Composition



Note: The panels show for the years 2011 till 2021 for all secondary school composition outcomes based on the ranked schools. Except for GPA, the pre-treatment periods show no significant divergence in the outcome. Data on GPA is missing for year 2011. The regression includes the full set of controls and cohort, city block and primary school fixed effects. The confidence interval is set at a 95% level. The year of reference is 2016, which is why this year is omitted. First treatment in 2017. Robust standard errors.

4 Disaster, Coping Strategies, and Child Well-Being Among Syrian Refugees: Evidence from the Beirut Port Explosion

Abstract: This paper examines how a large-scale urban disaster affects refugee households in fragile settings by exploiting the Beirut port explosion of August 4, 2020, as a natural experiment. Using five waves (2018–2022) of the nationally representative VASyR survey and a difference-in-differences design that compares Syrian refugees in Beirut with those in unaffected districts, we identify the causal impact of the explosion on household welfare and child outcomes. The explosion directly worsened housing conditions, with the share of households reporting shelter damage increasing by 2.3 percentage points (an 80% increase relative to the pre-treatment control mean), and indirectly intensified financial stress, as borrowing rose by 16.4 percentage points. Food insecurity deteriorated sharply, leading to substantial changes in coping behaviour, including reductions in meal frequency and portion size. Adverse effects on children are pronounced: forms of domestic violence against children increased, school attendance declined by 12.6 percentage points, and child labour rose by 2.6 percentage points. These impacts are strongest among the poorest households, those living in tents, and younger children. Overall, the results highlight the acute vulnerability of refugee households to large shocks and underscore the need for child-centred disaster-response policies in fragile contexts.

Co-authored with Mireille Kozhaya. The authors are grateful to Kerstin Schneider, Anna Makles and Arash Naghavi for helpful comments and suggestions. Raneem Achmar provided excellent data work assistance. All remaining errors are our own.

4.1 Introduction

Large-scale urban disasters pose severe risks to household and child welfare, particularly in fragile settings where social protection is weak and access to formal insurance and labour markets is limited. These risks are especially acute for refugees, who often live in precarious housing, rely on informal employment, and lack legal status or institutional protection. While a growing literature documents the economic consequences of natural and man-made disasters, much less is known about how sudden urban shocks affect displaced populations and how households adjust along margins that are central to child well-being. This gap is increasingly important as disasters, conflict, and forced displacement intersect in many low- and middle-income countries, where prevention and post-crisis management have improved but remain insufficient (UNDRR, 2015).

This paper studies how a large-scale urban disaster shapes economic vulnerability, coping behaviour, and children's outcomes among refugees living in a fragile institutional environment. The setting is Syrian refugees in Lebanon, a population displaced by civil war and facing extreme vulnerability even before 2020. Lebanon hosts one of the largest refugee populations per capita worldwide, yet it is not a signatory to international refugee protection treaties and provides refugees with no formal legal status or comprehensive social protection. As a result, Syrian refugees depend heavily on informal labour markets, humanitarian assistance, and fragile social networks. These structural vulnerabilities were further compounded by Lebanon's political and economic instability. Against this backdrop, the Beirut port explosion of August 4, 2020, constituted an abrupt and highly localized shock to housing, critical infrastructure, and economic activity (World Bank, 2020), disproportionately affecting refugee households often concentrated in poor-quality dwellings.

We exploit the Beirut port explosion as a natural experiment to identify the causal impact of a sudden urban shock on refugee households and children. Using five waves (2018–2022) of the nationally representative Vulnerability Assessment of Syrian Refugees in Lebanon (VASyR), we implement a repeated cross-sectional difference-in-differences design that compares Syrian refugees residing in Beirut with those living in other Lebanese districts that were not directly exposed to the blast. The empirical strategy is complemented by event-study analyses, alternative treatment definitions to assess spillovers, and placebo tests using Tripoli, Lebanon's second-largest port city, to separate the effects of the explosion from concurrent nationwide shocks.

Our results document a clear adjustment pathway following the Beirut port explosion. Beyond its impact on critical urban infrastructure, the shock directly damaged housing, raising the probability of shelter damage by 2.3 p.p. (about 80% relative to the pre-blast mean) and creating immediate liquidity needs among refugee households. These losses are primarily managed through increased borrowing, which rises by 16.4 p.p. (19% relative to pre-explosion levels), while employment and sectoral shifts into secondary and tertiary activities suggest short-run coping rather than sustained income growth. As financial stress intensifies, food insecurity increases sharply: the likelihood of reporting a lack of food

risers by 11.7 p.p. (14% relative to the pre-blast mean), prompting severe food-related coping strategies, including reductions in meal frequency and portion sizes and intra-household reallocation away from adults, particularly women. These pressures spill over to children, with school attendance declining by 12.6 p.p. (18% compared to pre-blast mean), child labour increasing by 2.6 p.p., early marriage among girls rising by 1.1 p.p., and exposure to violence against children becoming more frequent. Together, these findings show how a large urban disaster rapidly propagates from physical destruction to financial stress, food insecurity, and costly child-level coping responses, with potentially long-lasting consequences for human capital accumulation among displaced populations.

This paper makes three main contributions. First, it provides causal evidence on how a large-scale urban disaster affects refugee households in a fragile developing-country context, extending the disaster literature to displaced populations that face severe institutional constraints and limited access to formal infrastructure and labour markets (Elliott and Pais, 2006; Groen and Polivka, 2010; Groen et al., 2020). Second, it documents a coherent sequence of household coping responses, from housing damage and borrowing to food insecurity and child-level adjustments, using rich, nationally representative refugee data that overcomes small sample size limitations and allow us to jointly analyse material deprivation, financial stress, and intra-household behaviour. Third, it contributes to the literature on child welfare in crisis settings by showing how sudden urban shocks exacerbate existing inequalities in schooling, child labour, early marriage, and exposure to violence (Baez et al., 2017; Shah and Steinberg, 2017; Kumala Dewi and Dartanto, 2019; Khanna and Kochhar, 2023), with particularly strong effects among the poorest households and younger children. From a policy perspective, the findings underscore the need for disaster-response frameworks that explicitly account for refugees and prioritize rapid housing assistance, food security, and child protection. In fragile urban contexts, timely interventions such as emergency cash transfers, shelter repair programs, and targeted child-focused support may be critical to preventing short-run coping responses from translating into persistent losses in human capital and long-term welfare.

The remainder of the paper is structured as follows: **Chapter 4.2** reviews the relevant literature. **Chapter 4.3** provides background on Syrian refugees in Lebanon and the Beirut port explosion. **Chapter 4.4** describes the data and **Chapter 4.5** outlines the identification strategy. **Chapter 4.6** presents household-level results and heterogeneous effects. **Chapter 4.7** focuses on children's outcomes, **Chapter 4.8** conducts several robustness checks, and **Chapter 4.9** concludes with policy implications.

4.2 Literature Review

This chapter positions our study within five strands of literature: (i) the economic impacts of disasters, (ii) disasters and marginalized or displaced populations, (iii) labour markets and household finance after

shocks, (iv) domestic violence in crisis settings, and (v) child labour, schooling, and marriage as coping responses, including food-related coping strategies.

A large body of research examines the macro- and micro-level impacts of major disasters such as earthquakes, hurricanes, and floods on household welfare, assets, and individual earnings (Miguel and Roland, 2011; Gignoux and Menédez, 2016; Gallagher and Hartley, 2017; Groen et al., 2020). Others show how disasters affect preferences such as time discounting and risk attitudes (Callen, 2015; Hanaoka et al., 2018), leading to persistent effects on human capital, labour market outcomes, and long-run well-being. However, they focus on general populations in affected regions, rather than on refugees or minorities who may face distinct vulnerabilities and constraints.

Only a few studies analyse the impacts of disasters on displaced or marginalized groups. Research on Hurricane Katrina uses post-disaster surveys and current population survey data to show how existing race and class inequalities shape evacuation, return migration, and recovery trajectories (Elliott and Pais, 2006; Groen and Polivka, 2010). Doocy et al. (2011) use cross-sectional data in Jordan and Syria to provide descriptive evidence on food insecurity and patterns of humanitarian assistance among Iraqi refugees. However, little is known about how sudden, large-scale disasters affect refugees already living in extreme poverty. Our paper fills this gap by exploiting district-level variation in exposure to the Beirut port explosion. For that, we compare refugees in Beirut with those in unaffected districts to estimate the causal impact of the explosion on children's schooling, labour, and early marriage, as well as household employment, food insecurity, shelter damage, borrowing, and exposure to violence.

A third strand of work uses quasi-experimental panel designs that exploit spatial and temporal variation in disaster intensity to study labour markets and household finance. Belasen and Polachek (2009) and Kirchberger (2017) document that natural disasters reallocate labour across sectors and regions, sometimes raising wages in affected areas and depressing them elsewhere, while Gallagher and Hartley (2017) and Keerthiratne and Tol (2017) show that disasters limit credit and increase debt, especially for vulnerable households and small firms. We build on this literature by examining both the level and sector of refugee employment, as well as patterns of borrowing and shelter damage, in the context of an explosion in a developing country where refugees face restricted formal employment and limited access to finance.

Fourth, disasters can intensify intra-household tensions and domestic violence. Economic hardship and displacement undermine social support networks and increase psychological stress, potentially raising violence against intimate partners and children (Anderberg et al., 2015; Lindo et al., 2018; Leslie and Wilson, 2020). Evidence from Sri Lanka and the United States suggests that child abuse reports often rise following hurricanes and earthquakes (Curtis et al., 2000; Catani et al., 2008). But most existing studies rely on small, non-representative samples and limited measures of violence (Murphy et al., 2023). Our analysis adds to this literature by using rich, self-reported data on multiple forms of violence against children, including shaking, spanking, and beating.

Fifth, crises can also increase child labour, reduce schooling, and promote early marriage as coping strategies, especially among poor and credit-constrained households (Beegle et al., 2003; Baez et al., 2017; Shah and Steinberg, 2017; Oppen et al., 2023; UNFPA, 2012; Caruso, 2017; Kumala Dewi and Dartanto, 2019; Khanna and Kochhar, 2023; Save the Children, 2024). For Syrian refugees in Lebanon, early marriage was already widespread before the explosion (Mourtada et al., 2017). Based on this work, our study analyses the impact of an urban disaster on school attendance, labour, and early marriage of refugee children, and examines heterogeneity by age, gender, and household characteristics. Lastly, in low-income countries, households often respond to income and price shocks by adjusting food consumption, reducing portion sizes, skipping meals, or prioritizing some members' intake (Shah and Steinberg, 2017; Sani and Kemaw, 2019; De Haen and Hemrich, 2007). This has long-run consequences for health, cognitive development, and productivity (Alderman et al., 2006; Chen and Zhou, 2007). Existing studies typically rely on small samples and often focus on single coping dimensions. Our rich data set allows us to analyse a broader set of strategies, including meal frequency and portion size, reductions in adult or female consumption, offering a more comprehensive picture of how extremely poor refugee households manage severe shocks.

4.3 Background

4.3.1 Syrian Refugees in Lebanon

During the Syrian civil war, Lebanon has been a key destination for Syrian refugees due to its geographic proximity and shared historical ties. The Syrian refugee crisis is known to be the world's largest displacement crisis (UNICEF, 2022), and by 2015, one in four residents was a Syrian refugee in Lebanon, creating immense pressure on the limited resources of the country and leading to resentment among the local population, as Lebanon faced political instability and economic challenges (Kelley, 2017; Fakhoury, 2017). According to UNHCR (2022), the total refugee population is approximately 815,000, with the majority residing in the eastern districts of Zahle (17.12%) and Baalbek (13.55%) and in the northernmost district of Akkar (12.58%). Although 1.55% of Syrian refugees live in Beirut, neighbouring districts such as Baabda (6.68%) and El Meten (2.47%) have higher concentrations (ODP, 2023). However, most of these refugees live near the Beirut city boundary,⁴⁸ supporting our assumption of potential spillover effects. In **Chapter 4.4.1**, we address this imbalance in refugee distribution to ensure the representativeness of the results.

Despite the large number of refugees, Lebanon lacks formal refugee policies and is not a signatory to the 1951 Refugee Convention or its 1967 Protocol Relating to the Status of Refugees, the cornerstone treaties of international refugee law (Government of Lebanon and the United Nations, 2022).

⁴⁸ For a detailed overview of how refugees are distributed among districts, please check ODP (2023).

Consequently, Syrian refugees seeking protection after their forced migration face a precarious legal status and inadequate accommodation conditions, making them highly vulnerable to exploitation. Furthermore, increased competition for public services, such as healthcare and electricity, as well as for employment opportunities, has sharpened the hardships for both Lebanese citizens and Syrian refugees, even before the compounded crises of late 2019 and 2020 (World Bank, 2015).

4.3.2 Beirut Port Explosion

On August 4, 2020, one of the largest non-nuclear explosions in history occurred in Beirut when 2,750 tons of ammonium nitrate stored at the port exploded. Despite numerous warnings about the danger posed by the chemical, it was improperly stored, leading to widespread public outrage as it became evident that, despite awareness of the problem, no action was taken to remove or secure it. The Beirut port explosion caused devastation in the city, killing hundreds, injuring thousands, and displacing hundreds of thousands. It also caused significant damage to critical infrastructure, including electricity and water networks, food storage facilities in ports, the healthcare and education systems, and numerous other facilities. Physical damage, losses in economic flows, and reconstruction of infrastructure and buildings were shortly afterwards estimated to cost between \$8.5 and \$10.3 billion, intensifying Lebanon's ongoing crises (World Bank, 2020).

The Beirut port explosion occurred during a time of crisis for the country. Lebanon's liquidity crisis became fully apparent a year before, in late 2019, resulting in the devaluation of the currency, high inflation rates, and job losses. Additionally, Lebanon was not spared from the COVID-19 outbreak, which had already put pressure on the healthcare system. Together with the Beirut port explosion, economic conditions deteriorated further, particularly affecting those without savings or capital, as well as vulnerable groups, such as refugees and children.

UNICEF (2022) states that the "survival minimal expenditure basket is unaffordable for almost the entire Syrian refugee population" in Lebanon. Syrian refugees, having fled civil war and endured forced migration, faced new crises in Lebanon, making it necessary to adopt certain coping strategies to address food shortages, damaged shelters, unemployment, and poverty. Vulnerable groups in developing countries often rely on a common set of coping strategies, including changes in eating behaviour (such as reducing portion sizes or skipping meals), early marriage (through dowries or as a means of reducing household dependency), exposure to abuse, and withdrawing children from school and putting them to work, as increases in work hours or additional employment are often insufficient to meet basic needs (Lawson and Kasirye, 2013).

4.4 Data

4.4.1 Vulnerability Assessment of Syrian Refugees in Lebanon

The data used for this study come from the Vulnerability Assessment of Syrian Refugees in Lebanon (VASyR). The study was originally conducted to gain knowledge about the living conditions of Syrian refugees, to inform decision makers, and to redesign processes for humanitarian activities (UNHCR, UNICEF and WFP, 2013). Since 2013, this joint effort by organizations from the United Nations has collected information annually at the individual and household levels. VASyR is a repeated cross-sectional survey: each year a new representative sample of refugee households is drawn. It analyses a representative cross-sectional sample of Syrian refugee households and records rich information on household and individual demographic characteristics (such as education, marital status, age, gender, number of children, household size, and household income). Furthermore, it provides extensive information on food security, health outcomes, safety and security, shelter, economic well-being, child abuse, and coping strategies towards crises, such as child marriage or labour and food procurement (UNHCR, UNICEF and WFP, 2013, 2021, 2023).

The VASyR utilises a two-stage cluster sampling and weighting approach, ensuring that the survey data is representative of the Syrian refugee population in Lebanon. It outlines 30 clusters per district and surveys seven households in each cluster. All 26 districts were sampled, with Beirut and Akkar, both district and governorate, requiring additional strata to ensure coverage (90 clusters each). Next, data weighting is applied to adjust for unequal selection probabilities and to ensure accurate geographic representation. The weight for each district is calculated as follows:

$$W_n = \left(\frac{N_s}{N}\right) / \left(\frac{n_s}{n}\right)$$

where W_n is the normalised weight, N_s is the total sample frame of the district, N is the total national sample frame, n_s is the number of households visited in the district and n is the total of households visited. This ensures that the unequal geographical distribution of Syrian refugees in Lebanon is considered (UNHCR, UNICEF and WFP, 2013, 2021, 2023). All descriptive statistics and regression estimates are weighted using the VASyR household weights.

For our analysis, we use data from 2018 to 2022, accounting for two years before and three years after the 2020 Beirut port explosion.⁴⁹ This dataset includes more than 100,000 individuals from around 20,000 households.⁵⁰ This allows us to account for pre-treatment differences between the treatment and the control group and to estimate the causal impact of the Beirut port explosion on Syrian

⁴⁹ We exclude the years before 2018, as the data is either not publicly available, lacks important socioeconomic variables, such as parental education, or has different policy implications for refugees across district types. The years after 2022 are currently only available on an aggregated level.

⁵⁰ For the main analysis, we exclude districts neighbouring Beirut. We also exclude households with a household head younger than 19 years.

households and individuals. Next, we restrict the individual dataset to children aged 5 to 19, focusing on the sons and daughters of Syrian refugee households, to identify the group of children that suffers the most.⁵¹ For this sample, we observe a total of 19,188 children.

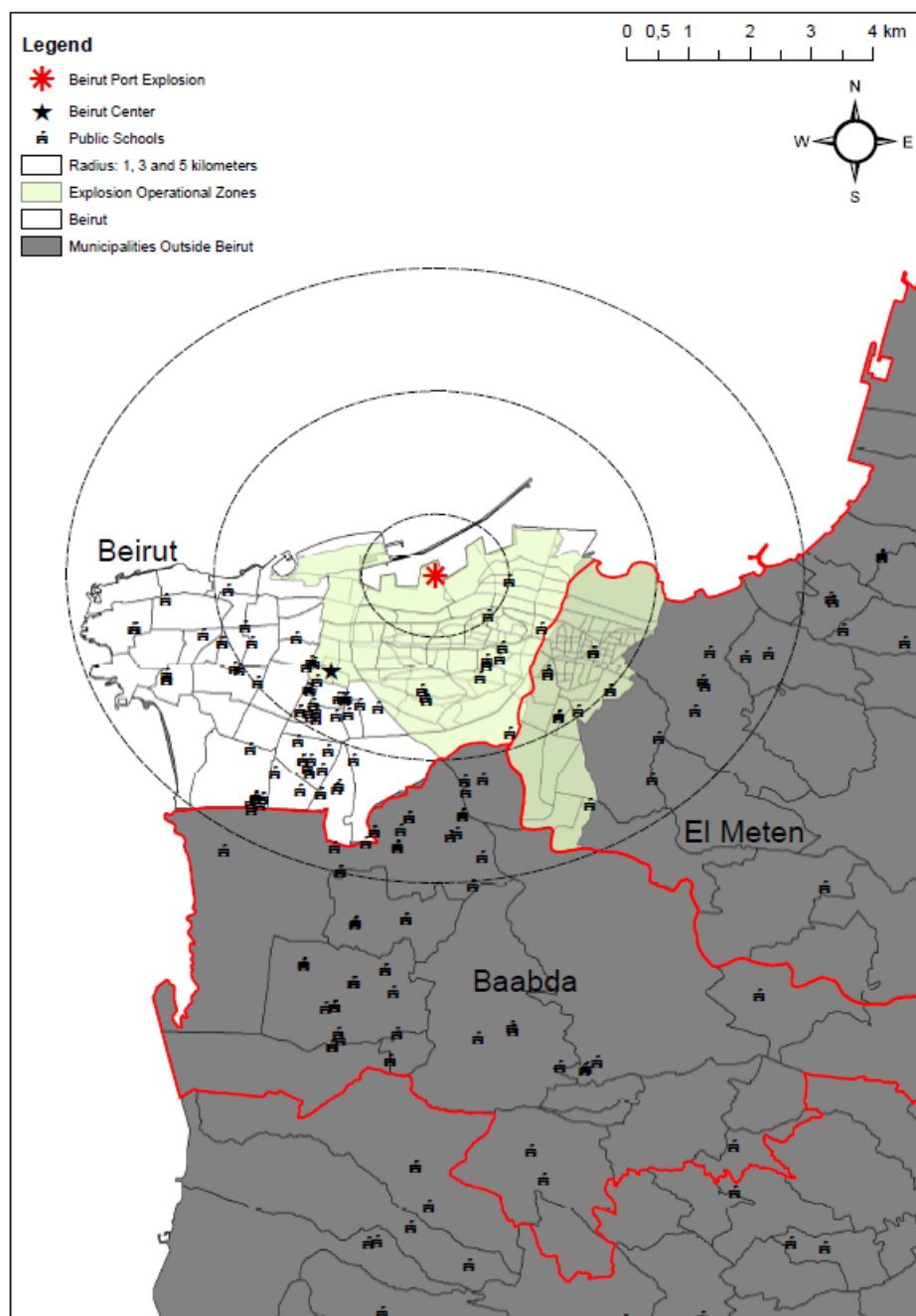
Then we complement this dataset by using the extreme poverty line definition of \$2.5 per day provided by the World Bank (2022) to calculate the poverty level according to the income of the household. It is worthy to mention that the income of the family has been reported in the month of December, which might lead to under-reporting in our results. However, we use this variable in the heterogeneous analysis to help us understand the financial constraints those refugees were facing after the Beirut port explosion.⁵²

4.4.2 Operational Zones of the Beirut Port Explosion

We incorporate two datasets from the United Nations Office for the Coordination of Humanitarian Affairs (OCHA). The first dataset provides a comprehensive list of all public schools in Lebanon (OCHA, 2017), and the second dataset includes detailed information on humanitarian operational zones established after the Beirut explosion (OCHA, 2020). This enables us to georeference the Beirut port explosion, public schools, the designated operational zones, and to visualize the spatial extent and impact of the disaster. This geospatial approach offers an initial perspective on the overlap between areas affected by the port explosion and the locations of educational infrastructure, providing information on the potential disruptions faced by Syrian refugees.

⁵¹ For this analysis, we exclude children who are not sons or daughters of the household and are reported to live with the Syrian refugee household, such as nieces, nephews, or cousins, to avoid biases and endogeneity problems and to better understand how the family itself contributes to the development of its members.

⁵² For the income variable, we provide all calculations in U.S. dollars (\$). For the period 2018–2019, we apply the exchange rate of \$1 = 1,512 Lebanese pounds (LBP). From 2020 onward, we use the following exchange rates: \$1 = 8,500 LBP in 2020, \$1 = 19,000 LBP in 2021, and \$1 = 89,550 LBP in 2022.

Figure 4-1 Beirut Port Explosion and Public Schools

Source: OCHA, authors' georeferenced and analysis. Notes: The figure illustrates the operational zones of the Beirut port explosion (OCHA, 2020). The star represents the centroid of Beirut city, and the asterisk represents the place of the explosion. The three circles show the 1-, 3- and 5-kilometre radii of the explosion. The buildings denote all public schools as of 2017 in Beirut, Baabda and El Meten (OCHA, 2017). Beirut port explosion happened on August 2020.

Consequently, in **Figure 4-1**, we depict Beirut and its centroid (star), neighbouring districts, and the location of the Beirut port explosion (asterisk) with impact radii of 1, 3, and 5 kilometres. Public schools within a 5-kilometer radius, potentially impacted by the explosion, are located not only in Beirut but also in the nearby districts of El Meten and Baabda. To coordinate and collaborate on relief, the OCHA developed operational zones shortly after the Beirut port explosion (OCHA, 2020). The map also highlights these zones in light green, which stretch significantly into El Meten and the 5-kilometer radius, suggesting the likelihood of spillover effects beyond Beirut, that is, on the regions surrounding it. This spatial overlap motivates our decision to exclude Baabda and El Meten from the baseline sample, as refugees there may have been indirectly exposed to the blast and reconstruction activities. Therefore, **Figure 4-1** confirms that the explosion and its operational zones are highly concentrated around Beirut, with limited but non-negligible reach into neighbouring districts.

4.4.3 Descriptive Statistics

Table 4-1 provides descriptive statistics for the treatment and control groups before the Beirut port explosion on August 4, 2020, at the household level. The final column provides the results of a t-test to assess whether the differences in means between the treatment and control groups are statistically significant.⁵³

Looking at **Table 4-1** reveals that approximately 2.9% of Syrian refugees had damaged shelters even before the Beirut port explosion. The table also indicates that 56.1% are employed, mainly in the tertiary sector. Furthermore, more than 80% reported experiencing food shortages and the need to borrow money. To cope with the lack of food, most households reduced the number of meals (57.8%) or portion sizes (54.7%), and more than a third of households reported that adults reduced their own consumption. In terms of housing, 58.6% of households lived in apartments, 22% in tents, and the remaining 19.4% in other forms of accommodations, such as repurposed rooms (e.g., concierges' quarters, engine rooms, agricultural or pump rooms), garages, warehouses, farms, or active construction sites. These numbers underscore the extent of economic hardship faced by Syrian refugee households, suggesting that the shock occurred against an already extremely fragile baseline. Additionally, 98.3% of the household heads in our restricted sample were male, and on average, there were approximately three children per household. More than 40% of parents have completed only primary school, while over 40% have attained a secondary school education or higher (high school, technical school, university).

⁵³ For Post-Beirut Port Explosion descriptive statistics on the household level, please refer to **Table 4-10-A** in the Appendix.

Table 4-1 Pre-Beirut Port Explosion Descriptive Statistics – Household Level

	All		Treatment		Control		t-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean diff.
Dependent variables							
Employment	0.561	0.496	0.646	0.479	0.558	0.497	0.051**
<i>Sector</i>							
Secondary	0.333	0.471	0.245	0.430	0.336	0.473	-0.107***
Tertiary	0.439	0.496	0.668	0.472	0.430	0.495	0.285***
Damaged shelter	0.029	0.168	0.031	0.173	0.029	0.168	-0.001
Borrow money	0.856	0.351	0.663	0.473	0.863	0.343	-0.206***
Lack of food	0.815	0.388	0.657	0.475	0.821	0.383	-0.158***
<i>Coping strategies for lack of food</i>							
Reduced portion size	0.547	0.498	0.517	0.500	0.548	0.498	-0.067***
Adults reduced consumption	0.373	0.484	0.294	0.456	0.376	0.484	-0.106***
Eat elsewhere	0.070	0.255	0.066	0.249	0.070	0.256	0.008
Reduced number of meals	0.578	0.494	0.500	0.500	0.581	0.494	-0.114***
Moved to cheaper place	0.073	0.261	0.085	0.279	0.073	0.260	0.001
Removed child from school	0.094	0.292	0.047	0.213	0.096	0.295	-0.050***
Female reduce consumption	0.102	0.303	0.084	0.278	0.103	0.304	0.003
Beirut	0.036	0.186	1.000	0.000	0.000	0.000	1.000
Control variables							
Male	0.983	0.129	0.975	0.155	0.983	0.128	-0.009*
No. of children	3.112	1.870	3.078	1.861	3.113	1.870	-0.031
No. of adults	2.577	1.196	2.653	1.235	2.575	1.195	0.091*
No. of elderly	0.118	0.382	0.112	0.389	0.118	0.382	0.004
<i>Household age</i>							
15 to 24	0.066	0.248	0.046	0.209	0.067	0.250	-0.022**
25 to 34	0.369	0.483	0.358	0.480	0.369	0.483	-0.015
35 to 44	0.344	0.475	0.403	0.491	0.342	0.474	0.055***
45 to 55	0.147	0.354	0.132	0.339	0.147	0.354	-0.010
> 55	0.074	0.262	0.062	0.241	0.075	0.263	-0.008
<i>Type of housing</i>							
Apartment	0.586	0.493	0.701	0.458	0.582	0.493	0.071***
Tent	0.220	0.414	0.002	0.040	0.228	0.420	-0.147***
Other	0.194	0.395	0.298	0.458	0.190	0.392	0.075***
<i>Household education mother</i>							
No formal education	0.145	0.352	0.128	0.335	0.146	0.353	-0.018
Kindergarten	0.001	0.027	0.005	0.071	0.001	0.024	0.004***
Primary school	0.420	0.494	0.447	0.498	0.419	0.493	0.023
Secondary school	0.286	0.452	0.283	0.451	0.287	0.452	-0.001
High school	0.093	0.291	0.090	0.286	0.093	0.291	-0.005
Technical school	0.023	0.150	0.013	0.113	0.023	0.151	-0.008
University	0.032	0.176	0.034	0.182	0.032	0.176	0.005
<i>Household education father</i>							
No formal education	0.091	0.288	0.072	0.259	0.092	0.289	-0.017
Kindergarten	0.001	0.027	0.003	0.056	0.001	0.026	0.002*
Primary school	0.464	0.499	0.431	0.496	0.465	0.499	-0.036*
Secondary school	0.314	0.464	0.347	0.476	0.313	0.464	0.029
High school	0.067	0.250	0.088	0.284	0.066	0.248	0.022**
Technical school	0.023	0.151	0.018	0.134	0.023	0.151	-0.005
University	0.040	0.195	0.041	0.198	0.040	0.195	0.003
Observations	6,378		612		5,766		

Notes: The data is taken from the VASyR for the years 2018 till 2019 on the household level. Treatment are Syrian refugees living in Beirut vs. Control group those who are living outside Beirut (excluding the near surrounding). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Comparing the pre-treatment means of the treatment and control groups reveals some differences, though most are minor. Regarding the outcome variables, the households in the treatment group, located in Beirut, were slightly more likely to be employed. In the treatment group, both fathers and mothers are more likely to have higher levels of education compared to those in the control group. They are also less likely to borrow money or experience food shortages. Consequently, they use fewer coping strategies to manage food scarcity. Due to urbanization and differences in infrastructure, households in the treatment group were less likely to live in tents. Our specification addresses these minor level differences by employing a difference-in-differences approach, which focuses on differential changes in outcomes rather than on baseline levels. While some pre-treatment means differ between Beirut and other districts, DiD identification relies on parallel trends rather than identical levels; **Chapter 4.5.2** shows that pre-treatment trends are similar.

4.5 Identification Strategy

4.5.1 Difference-in-Differences Design

To quantify the effect of the Beirut port explosion, we estimate a difference-in-differences approach, exploiting the year 2020 as a natural cutoff to define our treatment and control groups. We assign Syrian refugees residing in Beirut to the treatment group, while those residing outside the capital city form the control group,⁵⁴ excluding the neighbouring districts of Baabda and El Meten from the baseline sample to avoid spillover effects.

The Beirut port explosion was a point-source shock whose physical, infrastructural, and humanitarian impacts were overwhelmingly concentrated within the administrative boundaries of Beirut. The maps of OCHA and blast-radius calculations show that most of the destruction and emergency-response activities occurred within the city, with only limited spillover into nearby districts. Syrian refugees in Beirut were therefore disproportionately exposed to the blast, and strict mobility constraints, mainly coming from lack of legal residency, municipal curfews, and frequent checkpoints, made it hard for refugees to relocate or move (Kikano et al., 2021). Hence, we estimate the following model at the household level:

$$Y_{hdt} = \alpha_0 + \beta_0(Treated_h \times BeirutBlast_t) + \theta'X_{hdt} + \delta_d + \gamma_t + \epsilon_{hdt} \quad (1)$$

where Y_{hdt} denotes our outcome variables for households h , in district d , at survey time t , including employment, type of employment sector, debt, shelter damage, food insecurity, and coping strategies, such as changes in food consumption and domestic violence against children. $Treated_h$ is a dummy variable that takes the value one for Syrian refugee households residing in Beirut and zero for those

⁵⁴ In **Figure 4-4-A**, we provide the different treatment and control definitions used in this study.

households outside of Beirut. August 4, 2020, is the day of the Beirut port explosion; therefore, $BeirutBlast_t$ is a dummy variable that takes the value one for the period of 2020 onward and zero otherwise. β_0 captures the differential change in outcomes for refugees in Beirut relative to those in other districts after the explosion.

X_{hdt} is a vector of household characteristics that are likely to affect the outcome variables of Syrian refugee households, such as the gender and age of the household head, type of housing and the number of children, adults, and elderly present in the household. To account for socioeconomic differences, we also include the highest educational attainment of both the mother and the father. We also include district fixed effects (δ_d), absorbing time-invariant geographic and institutional differences (e.g. location, infrastructure, aid environment), and year fixed effects (γ_t), capturing common shocks, such as macroeconomic conditions or nationwide policy changes. To account for the uneven spatial distribution of refugees, we control for time-varying district characteristics using population shares based on the normalised VASyR weights. Finally, ϵ_{hdt} is the error term and the standard errors are clustered at the district level.

Next, to examine impacts of the Beirut port explosion on children, we estimate an analogous model at the individual level:

$$Y_{idt} = \alpha_0 + \beta_0(Treated_i \times BeirutBlast_t) + \theta'X_{idt} + \delta_d + \gamma_t + \epsilon_{idt} \quad (2)$$

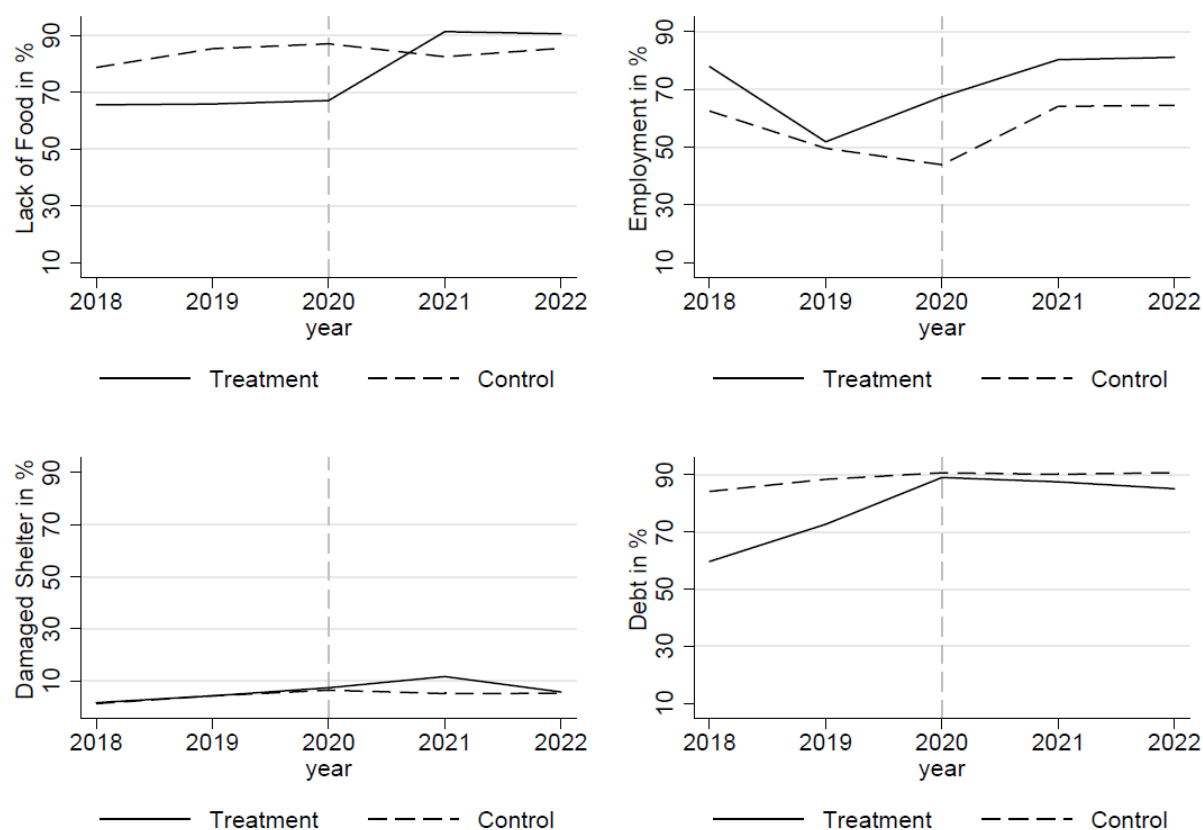
where Y_{idt} denotes individual outcomes, such as school attendance, marital status and labour for children i aged 5 to 19 in household h and district d , at survey time t . $Treated_i$ indicates children living in Beirut, whereas the control group consists of children living in other districts, again excluding Baabda and El Meten as neighbouring districts. X_{idt} includes child- and household-level characteristics (child's gender and age, housing type, number of children and adults, and parental age and education). The fixed effects and clustering structure are the same as in Eq. 1.

The main identifying assumption of DiD is that, in the absence of the explosion, the outcome variables in Beirut and the control districts would have followed parallel trends. We provide three forms of evidence to support this assumption: first, we provide a graphical representation of the pre-trends in **Figure 4-2**, showing the parallel evolution of employment, debt, damaged shelters, and food insecurity before 2020. The figure displays that after the explosion, the treatment group shows an increase in lack of food, damaged shelters, and debt. In particular, damaged shelters for the treated group increase to reach levels even higher than the control group. It is true that we see an increase in debt in 2019; however, after the explosion, the treatment group does not recover and remains at very high levels of debt. Second, event study estimates by survey year in **Figure 4-3** show for most variables no systematic pre-treatment differences between Beirut and the control districts.⁵⁵ Third, we conduct a sensitivity analysis

⁵⁵ Here, we additionally focus on the type of employment sector to determine whether Syrian refugees, who are already employed, experience an increase in job opportunities within those specific sectors.

following Rambachan and Roth (2023) to assess the robustness of our results to deviations from linear parallel trends; the main results remain stable under moderate relaxations of the assumption. Although some pre-treatment means differ between Beirut and other districts (**Table 4-1**), our DiD design relies on similarities in trends rather than identical levels.

Figure 4-2 Outcome Variables by Survey Year – Household Level



Notes: The data is taken from the VASyR for the years 2018 till 2022. This figure illustrates the outcome variables lack of food, employment, damaged shelter, and debt of Syrian refugees in Beirut (treatment) and Syrian refugees outside Beirut (control) without neighboring districts, by controlling for the full set of observable characteristics. The Beirut port explosion happened on August 4, 2020.

In **Chapter 4.8**, we discuss remaining threats to identification that include (i) the limited exposure of households surveyed in 2020, as data collection occurred only 2–3 weeks after the Beirut port explosion (August 19 to September 15), (ii) potential spillover effects into neighbouring districts, and (iii) concurrent shocks such as the COVID-19 pandemic:

- i. We address the concern of limited exposure in 2020 by re-estimating the model after excluding the 2020 survey wave. This removes households that may not yet have fully experienced the

consequences of the explosion. The resulting estimates remain similar or become stronger, particularly for food insecurity outcomes, indicating that limited exposure in 2020 does not drive our main results.

- ii. To assess the sensitivity of our results to the definition of the treatment group and to account for potential spillovers into neighbouring districts, we vary the treatment area by alternately including adjacent districts and excluding Beirut. The pattern of results across these alternative specifications supports the validity of our baseline treatment definition, suggesting that it appropriately captures exposure to the shock.
- iii. Lastly, we address the possibility that concurrent nationwide shocks, most notably the COVID-19 pandemic, confound our estimates by conducting a placebo difference-in-differences analysis using Tripoli. Tripoli is Lebanon's second-largest city and port and thus the most comparable metropolitan area to Beirut in observable and unobservable characteristics.⁵⁶ Because pandemic-related effects were nationwide, any estimated effect in this placebo setting would indicate confounding. We find no significant changes in outcomes for Tripoli or its surrounding districts, strengthening the interpretation that our main effects are attributable to the Beirut port explosion rather than to concurrent shocks.

4.5.2 Event Study Design

For the event study approach, we investigate whether the Beirut port explosion in 2020 had persistent effects over time. The empirical strategy follows the same logic as in Eq. 1, but we estimate the effect by survey year to observe differences between treatment and control groups in the pre-treatment period and differences in the period after the explosion. In **Figure 4-3**, we focus on employment and sector of employment, lack of food, damaged shelters, and debt. The figure reports point estimates and confidence intervals at the 95 percent level by survey year, using 2019 as the reference period. For overall employment and the tertiary sector, the event-study coefficients show a clear pre-trend (with the treatment group already diverging in 2018). Hence, we do not interpret the static DiD estimates for these two results in **Table 4-2** as fully causal and rather suggestive.

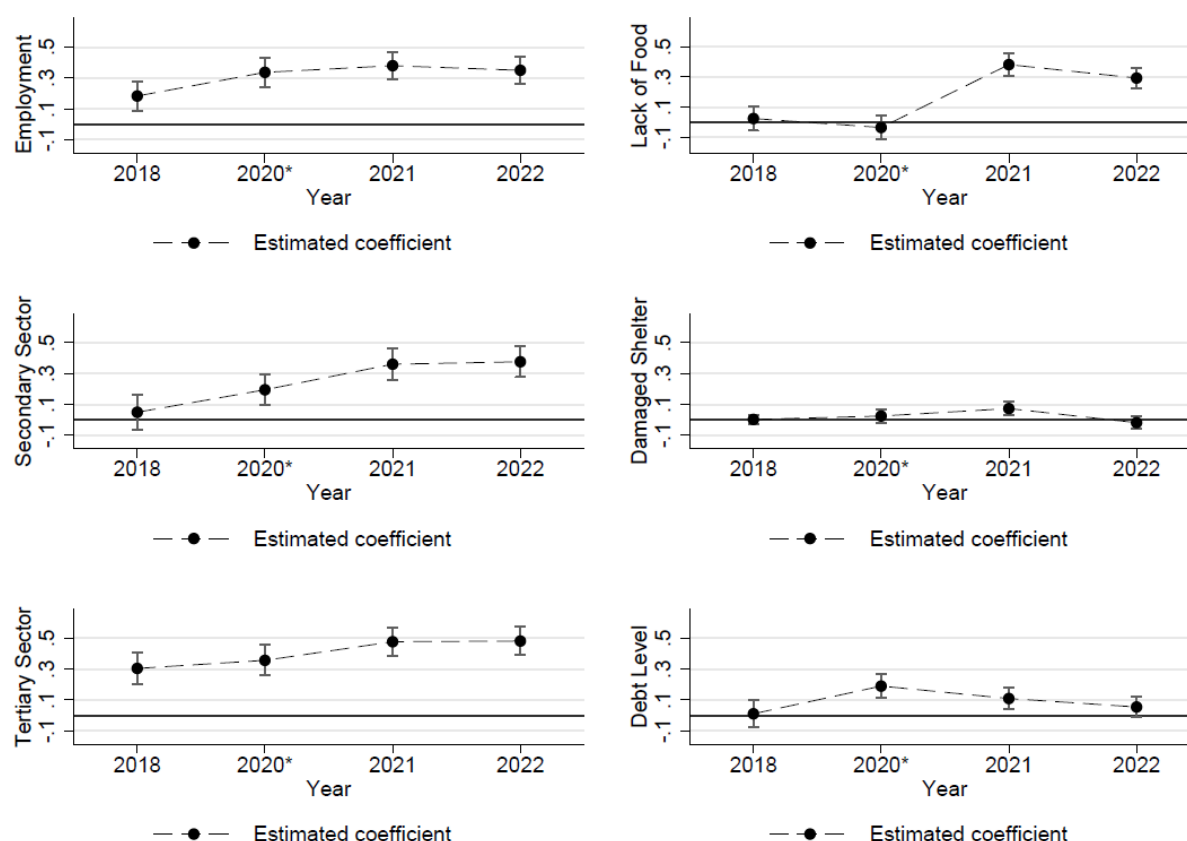
For lack of food, damaged shelters, debt, and the secondary sector, we do not observe pre-trends between treatment and control groups before 2020. After the Beirut port explosion, the gaps widen sharply: the lack of food, damage to shelter, and borrowing all increase in Beirut relative to other districts, and the probability of working in the secondary sector rises, consistent with reconstruction-related

⁵⁶ According to Al Jazeera (2020), Tripoli temporarily served as Lebanon's main port for just over a week following the explosion, before goods flows returned to Beirut once the port's functionality was restored.

jobs. By 2022, we observe some partial recovery, but levels generally remain worse than before the explosion, indicating that the shock had persistent negative effects on Syrian refugee households.

Deviations from parallel pre-trends are most pronounced in 2018, particularly for overall employment and tertiary-sector outcomes. This period coincides with renewed pressures related to the Syrian civil war and increased refugee inflows into Lebanon (Kelley, 2017; Fakhoury, 2017; UNICEF, 2022). Moreover, the event study is normalized to 2019, marking the onset of the Lebanese liquidity and banking crisis, while the post-2020 period overlaps with the COVID-19 pandemic and associated lockdowns. To the extent that these macroeconomic shocks differentially affected refugees in Beirut relative to those in other districts, some of the observed pre-2020 movements may partly capture the influence of concurrent crises rather than true violations of the parallel trends assumption.

To assess the robustness of our conclusions, we complement the event study with the “honest DiD” sensitivity analysis proposed by Rambachan and Roth (2023), reported in **Figure 4-6-A**. The first interval shows the original OLS confidence intervals for β_0 from Eq. 1, which rely on the standard parallel trends assumption. The intervals to the right display robust confidence sets obtained by progressively relaxing this assumption, allowing for increasing deviations from linearity. Apart from the outcome variable “borrowing money,” which appears more sensitive, the confidence intervals remain informative under moderate relaxations of the parallel trends assumption and continue to exclude zero. While this might not entirely eliminate concerns about pre-trend violations, it indicates that conclusions for employment are unlikely to be driven by small to moderate departures from parallel trends and therefore support at least a cautious causal interpretation.

Figure 4-3 Event Study – Impact of Beirut Port Explosion by Survey Year

Notes: The data is taken from the VASyR for the years 2018 till 2022. This figure illustrates the event study design for the treatment and control group. 2020 represents the year when the explosion took place. The reference year is 2019. The regressions include the full set of control variables. Standard errors clustered at district level.

4.6 Results

4.6.1 Damaged Shelter, Debt and Employment

We begin by examining the economic consequences of the Beirut port explosion for Syrian refugee households, focusing first on immediate housing damage and the coping responses it triggers. As shown in **Table 4-2**, column (I), the probability that a household reports a damaged shelter increases by about 2.3 p.p. in Beirut following the explosion (80% increase relative to the pre-treatment control mean). This direct destruction of housing constitutes an immediate negative wealth shock and tightens liquidity constraints for already credit-constrained refugee households.

In response to this housing shock, households may adjust along two main margins to cope with the increased financial stress: borrowing and employment. Column (II) of **Table 4-2** shows that the likelihood of borrowing money rises sharply by approximately 16.4 p.p., around a 19% increase relative to pre-treatment levels of the control group. **Table 4-8-A** in the Appendix indicate that this increase is driven primarily by loans from Lebanese friends and landlords, rather than from non-Lebanese networks

or moneylenders, consistent with refugees relying on nearby informal credit sources in the aftermath of the disaster (Gallagher and Hartley, 2017; Keerthiratne and Tol, 2017).

We further investigate whether some households attempt to smooth the shock by engaging in employment or changing the working sector. **Table 4-2**, column (III), shows that the explosion is associated with a greater probability that at least one household member is in paid work. Conditional on employment, we observe reallocations across sectors. Employment in the secondary sector rises by 15.1 p.p., consistent with increased demand for reconstruction-related activities following the blast (Kameda et al., 2021). At the same time, tertiary-sector employment is associated with an increase by 16.6 p.p., suggesting a shift into more accessible service jobs that often serve as a short-term coping mechanism for refugee households when formal labour market opportunities are limited.

These results that the refugee households respond to this shock primarily by taking on additional debt. The sharp increase in borrowing reflects binding liquidity constraints and the necessity to finance shelter repairs and basic consumption in the absence of formal insurance mechanisms. At the same time, we observe changes in employment and sectoral allocation that are consistent with short-run adjustment behaviour, but should be interpreted more cautiously. While employment and shifts into secondary and tertiary sectors increase following the explosion, these patterns provide suggestive evidence of coping through labour reallocation rather than conclusive causal effects on employment outcomes. In contrast to borrowing, there is no indication of sustained employment growth that would support a durable recovery through income generation. Hence, post-disaster adjustment among Syrian refugee households is driven mainly by increased indebtedness, with labour-market responses playing a secondary and potentially transitory role. For an already highly vulnerable population, this reliance on borrowing deepens financial fragility and increases exposure to future shocks, consistent with evidence from previous crises showing that disaster-induced liquidity constraints can entrench poverty and amplify downstream harms within the household, particularly for children (Baez et al., 2017; Shah and Steinberg, 2017; Khanna and Kochhar, 2023).

Table 4-2 Direct and Indirect Economic Effects of the Beirut Port Explosion

	Damaged shelter I	Borrow money II	Employment III	Secondary IV	Tertiary V
<i>Effect of BeirutBlast</i>	0.023*** (0.008)	0.164*** (0.021)	0.108*** (0.027)	0.151*** (0.032)	0.166*** (0.027)
Observations	14,112	14,112	14,112	8,643	8,643
R^2	0.046	0.037	0.173	0.189	0.171

*Notes: Results are obtained from DiD models, Eq. 1. The data is taken from the VASyR for the years 2018 till 2022. The sample size differs due to missing values reported for the variables. The regressions include the full set of controls, district fixed effects and year fixed effects. Standard errors in parentheses (clustered at district level). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.*

4.6.2 Food Insecurity and Coping Strategies

Rising financial stress follows worsening food insecurity, which, if not addressed swiftly, may translate into persistent adverse effects on household welfare. Therefore, we investigate food insecurity, but also the coping strategies in response to lack of food. **Table 4-3** shows that the probability of reporting lack of food increases by about 11.7 p.p. for Syrian refugee households in Beirut. This is a large effect relative to pre-explosion levels and indicates a substantial deterioration in basic consumption possibilities, over and above the already high baseline level documented in **Table 4-1**. Combined with the evidence on damaged shelters and rising indebtedness in the previous chapter, these results suggest that the explosion deepened the economic vulnerability of refugees in the capital of Lebanon.

Households respond to this worsening situation by adjusting both the quantity and allocation of food. **Table 4-3** indicates that the likelihood of reducing the number of daily meals and portion sizes rises significantly in Beirut relative to other districts. These strategies are consistent with the broader literature on coping in low-income settings, where households often smooth consumption in the short run by cutting back on food rather than on other expenditures (De Haen and Hemrich, 2007; Sani and Kemaw, 2019). We also find that adults, and particularly women, are more likely to reduce their own food intake, suggesting an intra-household reallocation of calories towards children. While this pattern may help protect children from the most immediate nutritional shortfalls, it imposes additional health risks on adults and may not be sustainable in the longer run. In addition, the increase in food insecurity appears to be an important channel through which school attendance declines, as children are taken out of school.

The estimates for food insecurity and coping strategies reinforce the picture of Syrian refugee households in Beirut relying heavily on self-insurance in the face of the shock. The explosion increases the probability of lacking food, and households react by tightening food consumption, prioritizing some members over others, and combining these adjustments with increased borrowing.

Table 4-3 Effect of the Beirut Port Explosion on Lack of Food and Coping Strategies

	Lack of food I	Eat else- where II	Reduce no. of meals III	Move place IV	Child out of school V	Reduce portion size VI	Reduce adult cons. VII	Reduce female cons. VIII
<i>Effect of BeirutBlast</i>	0.117*** (0.018)	0.007 (0.018)	0.186*** (0.032)	0.007 (0.012)	0.081*** (0.016)	0.128*** (0.031)	0.174*** (0.027)	0.074*** (0.013)
Observations	14,112	14,112	14,112	14,112	14,112	14,112	14,112	11,518
R^2	0.064	0.030	0.075	0.017	0.040	0.094	0.075	0.046

Notes: Results are obtained from DiD models, Eq. 1. The data is taken from the VASyR for the years 2018 till 2022. The sample size differs due to missing values reported for the variables. The regressions include the full set of controls, district fixed effects and year fixed effects. Standard errors in parentheses (clustered at district level). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

4.6.3 Heterogeneity

So far, we document the average effects of the Beirut port explosion on Syrian refugee households in Beirut. In this chapter, we examine whether these impacts vary by ex-ante vulnerability, specifically focusing on poverty status and housing conditions of refugees. This allows us to assess whether the shock disproportionately affects households that were already at the bottom of the distribution before 2020.

We first divided households into two categories based on their extreme poverty status in panels A and B of **Table 4-4**, using the US\$2.5 international poverty line (World Bank, 2022). The estimates show that the deterioration in food security, shelter conditions, and indebtedness is concentrated among households below the extreme poverty line. Among these households, the probability of lacking food and of borrowing money increases substantially after the explosion, while point estimates households above the poverty line are smaller in effect size and often not statistically significant. Similarly, the likelihood of reporting a damaged shelter rises mainly for extremely poor households. This pattern suggests that the Beirut port explosion increases existing inequalities among refugees, pushing the poorest households deeper into deprivation. Given that income in VASyR is measured for December of the previous year, any under-reporting of income in late 2019 is likely to attenuate measured differences by poverty status, implying that our heterogeneous effects by poverty may be conservative but do help us further explain the baseline results.

We further examine heterogeneity by type of housing in **Table 4-4**, panel C, distinguishing households living in apartments (baseline) from those in tents or other accommodations. For households living in apartments, the explosion is associated with increases in employment (9.9 p.p.), food insecurity (12 p.p.), borrowing (18.1 p.p.), and shelter damage (4.4 p.p.). For households living in tents, the effects differ markedly: the increase in food insecurity is much larger (31.2 p.p.), as is the increase in shelter damage (88 p.p.). This is consistent with tents offering substantially less physical protection. In contrast, the employment effect is smaller for households living in tents (2.3 p.p.), and the borrowing effect is lower than for households living in apartments, decreasing by 16.2 p.p., suggesting that tent-dwelling households do not increase borrowing in response to the shock to the same extent, potentially reflecting tighter credit constraints or already saturated borrowing before the explosion. Households in other accommodation types also show small increases in employment (13.6 p.p.), similar changes in food insecurity (10.8 p.p.), a smaller increase in borrowing (12.5 p.p.), and less shelter damage (4.1 p.p.) relative to households in apartments. Panel C thus shows that refugees living in tents face a ‘double penalty’: relative to households residing in apartments or other accommodations, they experience substantially larger increases in food insecurity and shelter damage following the explosion, while exhibiting a markedly weaker borrowing response. This pattern is consistent with tighter credit constraints among tent-dwelling households, limiting their ability to smooth the shock through debt despite facing more severe material losses.

Overall, the heterogeneous analysis shows that the Beirut port explosion does not affect all refugees equally. Instead, it deepens pre-existing vulnerabilities by hitting the poorest households and those in the most precarious housing conditions hardest. In the next chapter, we also examine how these layered shocks are reflected in children's outcomes, including schooling, child labour, early marriage, and exposure to violence within the household.

Table 4-4 Heterogeneous Effects of the Beirut Port Explosion on Syrian Refugee Households

	Employment I	Lack of food II	Debt III	Damaged shelter IV
	A. Below poverty line			
<i>Effect of BeirutBlast</i>	0.154*** (0.029)	0.073*** (0.016)	0.115*** (0.017)	0.017** (0.008)
Observations	12,734	12,734	12,734	12,734
	B. Above poverty line			
<i>Effect of BeirutBlast</i>	-0.318** (0.151)	0.196 (0.134)	-0.023 (0.252)	-0.016 (0.023)
Observations	1,378	1,378	1,378	1,378
	C. Type of Housing			
<i>Effect of BeirutBlast</i>	0.099*** (0.026)	0.120*** (0.018)	0.181*** (0.022)	0.044*** (0.008)
<i>Effect of BeirutBlast x Tent</i>	-0.076*** (0.022)	0.192*** (0.024)	-0.343*** (0.016)	0.836*** (0.028)
<i>Effect of BeirutBlast x Other</i>	0.037*** (0.012)	-0.012 (0.010)	-0.056*** (0.009)	-0.085*** (0.011)
Observations	14,112	14,112	14,112	14,112

Notes: Results are obtained from DiD models, Eq. 1. The data is taken from the VASyR for the years 2018 till 2022. The regressions include the full set of controls, district fixed effects and year fixed effects. Standard errors in parentheses (clustered at district level). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

4.7 Child Outcomes

We now turn to the effects of the Beirut port explosion on children. Using the individual-level difference-in-differences model in Eq. 2, we study three key outcomes for children aged 5-19: school attendance, child labour and early marriage, and then examine violence against children within the household, according to Eq. 1. Pre-explosion descriptive statistics on the individual (child) level can be found in **Table 4-9-A**.

4.7.1 School Attendance, Child Labor, and Early Marriage

Table 4-5 reports the estimated effects of the explosion on children's school attendance, child labour, and early marriage. Following the explosion, the probability that a refugee child living in Beirut attends school declines by about 12.6 p.p. relative to children in other districts (column I). Given the already

low baseline attendance documented in the data section, this represents a substantial proportional decline. At the same time, the likelihood that a child engages in paid work increases by approximately 2.6 p.p. (column II), while the average effect on child marriage is not statistically significant.

Turning to heterogeneity, panel B of **Table 4-5** reveals important differences by gender and age. Column III shows an increase in early marriage among girls: the probability that a girl is married rises by about 1.1 p.p. after the explosion in Beirut. Although modest in absolute terms, this effect is sizable relative to the pre-explosion mean and suggests that early marriage may serve as an additional coping mechanism for some households, potentially by generating one-time financial transfers (e.g., dowry payments) or by reducing the number of dependents to be supported within the household. Gender differences are also evident in schooling and labour outcomes: the decline in school attendance is more pronounced for girls, whereas the increase in child labour is stronger for boys. Once children enter paid work, re-entry into formal schooling is unlikely, suggesting that these short-run adjustments may have persistent effects on educational attainment (UNICEF, 2025).

Heterogeneity by age further indicates that younger children (ages 5–9), who were substantially less likely to work prior to the explosion, experience a disproportionately large decline in school attendance and a marked increase in child labour. This pattern suggests that the shock forces some households to adjust margins that are typically protected, underscoring the severity of the economic stress induced by the explosion. These findings mirror the heterogeneous patterns observed at the household level in the previous chapter and highlight how the explosion exacerbates pre-existing inequalities among refugee children.

Together, the results in **Table 4-5** point to a deterioration in children's human capital accumulation: fewer children remain in school, more are pushed into work, and a non-negligible share of girls enter marriage earlier than they otherwise would have. These findings are consistent with previous evidence on household coping responses to severe economic shocks (Skoufias, 2003; Sawada and Takasaki, 2017; UNFPA, 2012; UNICEF, 2025).

Table 4-5 Effect of the Beirut Port Explosion on Children

	School attendance I	Child labour II	Child marriage III
A. Baseline – Individual Effects			
<i>Effect of BeirutBlast</i>	-0.126*** (0.034)	0.026** (0.010)	0.002 (0.002)
Observations	19,188	19,188	19,188
B. Heterogeneous Effects			
I. Gender			
<i>Effect of BeirutBlast</i>	-0.120*** (0.037)	0.006 (0.004)	0.011*** (0.002)
<i>Effect of BeirutBlast x Male</i>	-0.014 (0.013)	0.035** (0.016)	-0.016*** (0.002)
Observations	19,188	19,188	19,188
II. Household Head Gender			
<i>Effect of BeirutBlast</i>	-0.530*** (0.076)	-0.059 (0.049)	0.047 (0.031)
<i>Effect of BeirutBlast x Male</i>	0.409*** (0.068)	0.086* (0.046)	-0.046 (0.031)
Observations	19,188	19,188	19,188
III. Child Age Category			
<i>Effect of BeirutBlast</i>	-0.130*** (0.042)	0.020*** (0.005)	-0.001 (0.002)
<i>Effect of BeirutBlast x 10 to 14</i>	-0.031* (0.017)	0.000 (0.014)	0.004*** (0.001)
<i>Effect of BeirutBlast x 15 to 19</i>	0.025 (0.023)	0.013 (0.013)	0.000 (0.013)
Observations	19,188	19,188	19,188

Notes: Results are obtained from DiD models, Eq. 2. The data is taken from the VASyR for the years 2018 till 2022. Treatment are children of Syrian refugee households aged 5 to 19 years old living in Beirut vs. control group those who are living outside Beirut (excluding the nearby districts to avoid spillover effects). The regressions include the full set of controls, district fixed effects and year fixed effects. Standard errors in parentheses (clustered at district level). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

4.7.2 Violence Against Children

We complement these outcomes with evidence on violence against children within the household. **Table 4-6** presents estimates for several forms of harsh discipline reported by the caregiver, including shaking, spanking, and beating. The probability that a child is subjected to such practices increases significantly for refugees in Beirut after the explosion. For instance, the likelihood of being shaken or hit in the face rises relative to children in other districts (columns I and IV), while milder forms of discipline show smaller changes.

Table 4-6 Effect of the Beirut Port Explosion on Domestic Violence Towards Children

	Shake I	Spank II	Hit with object III	Hit face IV	Hit leg/arm V	Beat VI	Chores against will VII
<i>Effect of BeirutBlast</i>	0.112*** (0.017)	0.034 (0.022)	0.006 (0.007)	0.022** (0.009)	0.005 (0.023)	0.003 (0.003)	0.013* (0.007)
Observations	12,952	12,952	12,952	12,952	12,952	12,952	10,554
R^2	0.052	0.074	0.022	0.034	0.051	0.011	0.027

Notes: Results are obtained from DiD models, Eq. 1. The data is taken from the VASyR for the years 2018 till 2022. The sample size differs due to missing values reported for the variables. The regressions include the full set of controls, district fixed effects and year fixed effects. Standard errors in parentheses (clustered at district level). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Together, the evidence suggests that increased economic stress and uncertainty following the explosion translate into more frequent and severe intra-household conflict, with children bearing a disproportionate share of the burden. Combined with the documented deterioration in housing conditions, food security, and the tightening of food-related coping strategies, the results indicate that the Beirut port explosion not only reduced material well-being but also worsened the psychosocial environment in which refugee children grow up. The shock thus has substantial and multifaceted consequences for children's lives: schooling losses, increased child labour, higher rates of early marriage, and greater exposure to violence all point to a deepening of child poverty and vulnerability among Syrian refugees in Beirut. These patterns are likely to have persistent implications for human capital accumulation and intergenerational mobility (León, 2012; Delprato et al., 2015; Sim et al., 2017; Jürges et al., 2022).

4.8 Robustness Checks

This chapter assesses the robustness of our main results to alternative samples and specifications. **Table 4-7** shows: (i) alternative definitions of the treatment and control areas to account for potential spillover effects, (ii) exclusion of the 2020 survey wave to address timing and concurrent macro shocks, and (iii) placebo exercises based on Tripoli, Lebanon's second-largest port city.

A first concern is that the Beirut port explosion may have affected Syrian refugees living just outside the administrative boundaries of Beirut, particularly in the neighbouring districts of Baabda and El Meten. If households there experienced some of the physical destruction or reconstruction activities, including these districts in the baseline sample might bias our estimates. Therefore, panels A and B of **Table 4-7** redefine the treatment and control groups in two ways. In the first specification, we classify households in Baabda and El Meten as treated together with those in Beirut. Then, we keep only Baabda and El Meten in the treatment group in panel B.

Panel A shows that the estimated effects on employment, food insecurity, debt, and shelter damage remain similar in sign and effect size to our baseline results, and the main coefficients generally remain statistically significant. Columns I and II of panel B suggest that the Beirut port explosion has a positive and significant effects on employment and a negative effect on damaged shelters in the form of

spillover effects for the neighbouring districts (Baabda and El Meten); for the latter, the effect is only significant at the 10% level.

We further present graphical evidence of parallel trends for this specification of panel B in **Figure 4-5-A** in the Appendix. It shows that both the treatment group (Baabda and El Meten) and the control group (all other districts, except Beirut) follow the same trend before and after the Beirut port explosion, again reinforcing the point that Beirut was the most affected directly and indirectly by the damage from the explosion. As argued earlier, employment opportunities may increase after such a disaster to mitigate the damage, and we do see a positive effect for Baabda and El Meten, with an increase in employment for refugee household members of 11.8 p.p. Note that most refugee households in these areas live along the Beirut city boundary and might commute into the capital city for work (ODP, 2023). Therefore, excluding neighbouring districts shows that spillover effects are limited and do not drive our results.

A second concern relates to the timing of the 2020 VASyR data collection. The survey took place only 2–3 weeks after the Beirut port explosion (between August 19 and September 15), implying that some households in Beirut may not yet have fully experienced or reported the consequences of the blast. Moreover, the period around 2019–2020 coincides with the onset of the Lebanese liquidity and banking crisis and the COVID-19 pandemic and associated lockdowns, which may have affected refugees in Beirut and in other districts differently. To address these concerns, panel C of **Table 4-7** re-estimates the baseline models excluding the 2020 survey year. The estimated effects of the explosion on food insecurity, shelter damage, and borrowing remain similar, and in several cases the coefficients are slightly larger, particularly for food-related outcomes. This pattern is consistent with food insecurity emerging with a delay, as households may initially rely on savings, assistance, or short-term coping strategies before reductions in food quantity become fully apparent. In contrast, shelter damage constitutes an immediate and highly salient outcome of the explosion. The larger food-related estimates when excluding 2020 therefore suggest that the baseline results may understate, rather than overstate, the medium-run impact of the shock on household food security. Overall, these findings indicate that our main results are not driven by the specific timing of the 2020 survey or by short-run reporting issues in the immediate aftermath of the blast.

Finally, we examine whether our results could be capturing broader nationwide or port-related shocks rather than the specific impact of the Beirut explosion. Panel D of **Table 4-7** therefore reports placebo DiD estimates in which we use Tripoli, Lebanon's second-largest port city, as a treatment group and its surrounding districts as the control group to assess whether they experienced a similar shock in 2020. Across the same set of outcomes, that is food insecurity, shelter damage, debt, and employment, the placebo coefficients are small and statistically insignificant. We do not observe systematic changes in Tripoli after 2020 that resemble the patterns documented for Beirut. However, we do see significant differences in panel E, that is, between Beirut and Tripoli. This placebo exercise supports the

interpretation that our main estimates capture the local effects of the Beirut port explosion rather than general trends affecting all port areas or all urban centres in Lebanon.

Overall, the robustness checks reinforce the credibility of our findings. Allowing for possible spillovers into neighbouring districts, excluding the 2020 survey wave, and implementing placebo tests around Tripoli all yield results that are consistent with the baseline estimates. While some outcomes, such as overall employment, remain more sensitive to deviations from parallel trends and concurrent macro shocks, the evidence for increased food insecurity, shelter damage, and indebtedness among Syrian refugees in Beirut is robust across a wide range of alternative specifications.

Table 4-7 Robustness Checks on Main Outcome Variables

	Employment I	Lack of food II	Borrow money III	Damaged shelter IV
A. Treatment: Beirut, Baabda, and El Meten				
<i>Effect of BeirutBlast</i>	0.090*** (0.026)	0.107*** (0.019)	0.162*** (0.018)	0.028*** (0.008)
Observations	14,965	14,965	14,965	14,965
B. Treatment: Baabda and El Meten				
<i>Effect of BeirutBlast</i>	0.118*** (0.027)	0.049 (0.078)	0.020 (0.035)	-0.037* (0.022)
Observations	13,559	13,559	13,559	13,559
C. Exclude 2020				
<i>Effect of BeirutBlast</i>	0.092*** (0.028)	0.222*** (0.026)	0.151*** (0.025)	0.031*** (0.009)
Observations	10,942	10,942	10,942	10,942
D. Tripoli vs. surroundings				
<i>Effect of BeirutBlast</i>	0.020 (0.021)	0.014 (0.043)	-0.001 (0.042)	-0.060 (0.053)
Observations	1,617	1,617	1,617	1,617
E. Beirut vs. Tripoli				
<i>Effect of BeirutBlast</i>	0.075** (0.003)	0.138** (0.005)	0.220*** (0.002)	0.023 (0.004)
Observations	1,957	1,957	1,957	1,957

Notes: Results are obtained from DiD models. The data is taken from the VASyR for the years 2018 till 2022. Panel A represents the results where the treatment is defined as Beirut and the two surrounding districts (Baabda and El Meten) vs. the control group (all other districts). In panel B, we redefine treatment as Syrian refugees living in the two surrounding districts, Baabda and El Meten (excluding Beirut) vs. control group (all other districts, excluding Beirut). For panel C, the data is taken from the VASyR for the years 2017 till 2022 excluding the year 2020. Treatment are Syrian refugees living in Beirut (excluding the near surroundings) vs. control group (all other districts, except Baabda and El Meten). In panel D, we run a placebo test in a different region, e.g., Tripoli vs. its two surrounding districts as control (El Koura and Zgharta). In panel E, we define Syrian refugee households in Beirut as the treatment group and those in Tripoli as the control group. The regressions include the full set of controls, district fixed effects and year fixed effects. Standard errors in parentheses (clustered at district level). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

4.9 Conclusion

This paper examines how the Beirut port explosion affected Syrian refugee households in Lebanon. Using district-level variation in exposure to the blast and five waves of the nationally representative VASyR survey, we implement a repeated cross-sectional difference-in-differences design, complemented by event-study estimates and sensitivity analyses. We focus on both household-level outcomes, such as employment, housing conditions, debt, food insecurity and coping strategies, and on child-level outcomes, including schooling, child labour, early marriage, and exposure to violence.

The results show that the Beirut port explosion substantially worsened living conditions for Syrian refugees in Beirut. At the household level, the likelihood of reporting shelter damage, borrowing money, and food insecurity increases significantly after the blast, consistent with prior evidence on the impacts of natural and man-made disasters (Gallagher and Hartley, 2017; Keerthiratne and Tol, 2017). Households respond by reducing meal frequency and portion sizes and by disproportionately cutting adults', and especially women's, consumption, while relying more heavily on debt, particularly from Lebanese friends and landlords (De Haen and Hemrich, 2007; Sani and Kemaw, 2019). Conditional on employment, we observe a shift toward work in the secondary sector, likely reflecting short-run reconstruction activity following the explosion (Miguel and Roland, 2011; Gignoux and Menédez, 2016). However, we find no corresponding increase in overall employment. These patterns suggest that while the shock generated temporary employment opportunities, it did not translate into sustained income gains or a durable exit from extreme poverty. Instead, refugee households appear to finance post-shock recovery primarily through increased borrowing and the involvement of children, rather than through lasting improvements in earnings.

The analysis of child outcomes reveals particularly concerning impacts. School attendance falls sharply, while child labour and early marriage among girls increase, indicating that households adjust by reallocating children from school to work or marriage as coping strategies (Baez et al., 2017; Shah and Steinberg, 2017; Khanna and Kochhar, 2023). At the same time, reports of domestic violence, such as shaking and hitting, become more frequent in Beirut relative to other districts. These findings suggest that the explosion not only reduced children's access to education and increased their economic and social burdens, but also worsened the environment in which they grow up. Given the well-documented long-term consequences of early school dropout, child labour, and exposure to violence for health and human capital (Alderman et al., 2006; Chen and Zhou, 2007), the explosion is likely to have persistent effects on human capital accumulation and intergenerational mobility.

Lastly, heterogeneity analysis shows that extremely poor households and those living in tents experience larger increases in food insecurity, debt, and shelter damage. Children in these households face steeper declines in school attendance and stronger increases in child labour and early marriage, and the effects are often more pronounced for girls and for younger children who were previously less likely to work (UNFPA, 2012; Mourtada et al., 2017; Save the Children, 2024). The Beirut port explosion thus

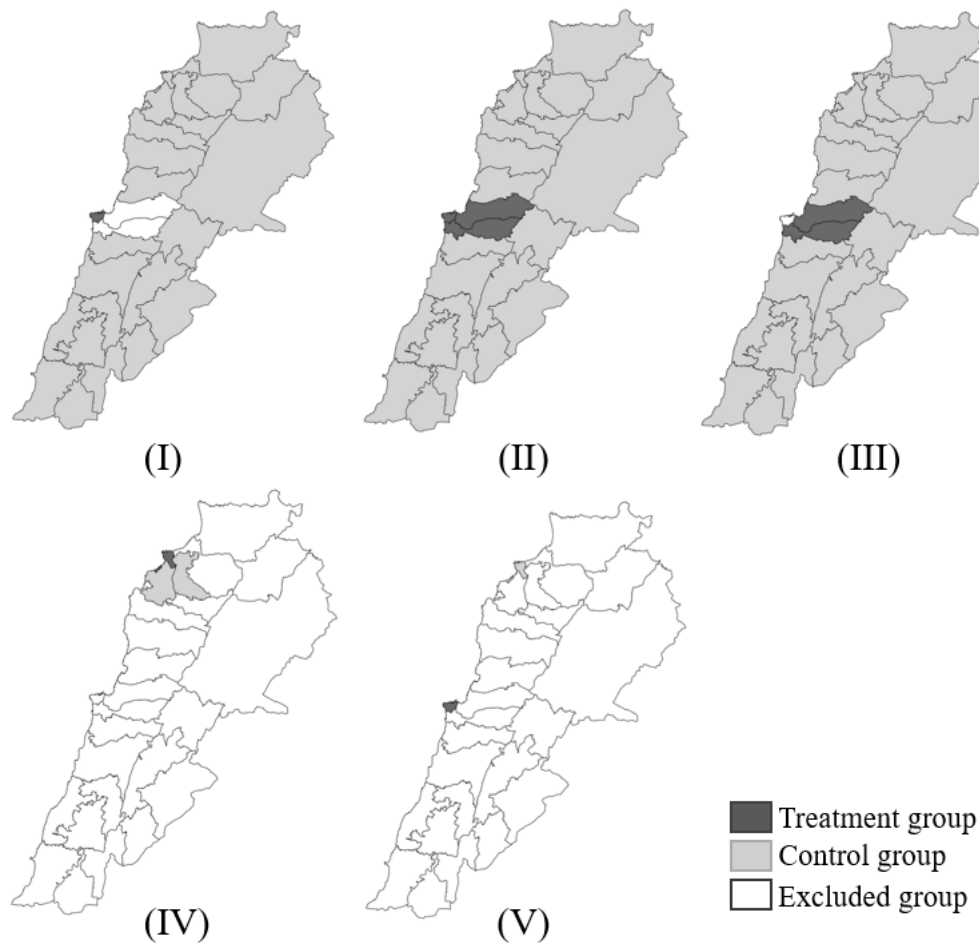
increases pre-existing inequalities among refugees, hitting the most vulnerable households and children the hardest.

These findings carry several policy implications. They underscore the importance of international refugee protection frameworks and, more broadly, the need to systematically prepare for and respond to disasters affecting displaced populations in host countries (UNDRR, 2015). In contexts where legal status is precarious and access to formal social protection is limited, shocks such as the Beirut explosion can rapidly push already vulnerable households into deeper poverty and debt traps (World Bank, 2015). Targeted support for the poorest households and those residing in the most precarious housing conditions, such as emergency cash transfers, rent support, and rapid shelter repairs, is therefore critical to prevent harmful coping strategies, including child labour, early marriage, and severe food insecurity (UNICEF, 2022). Finally, child-centred interventions, including education subsidies, school feeding programs, and strengthened child protection services, are essential to mitigate the long-term human-capital losses associated with large shocks among refugee children (Save the Children, 2024).

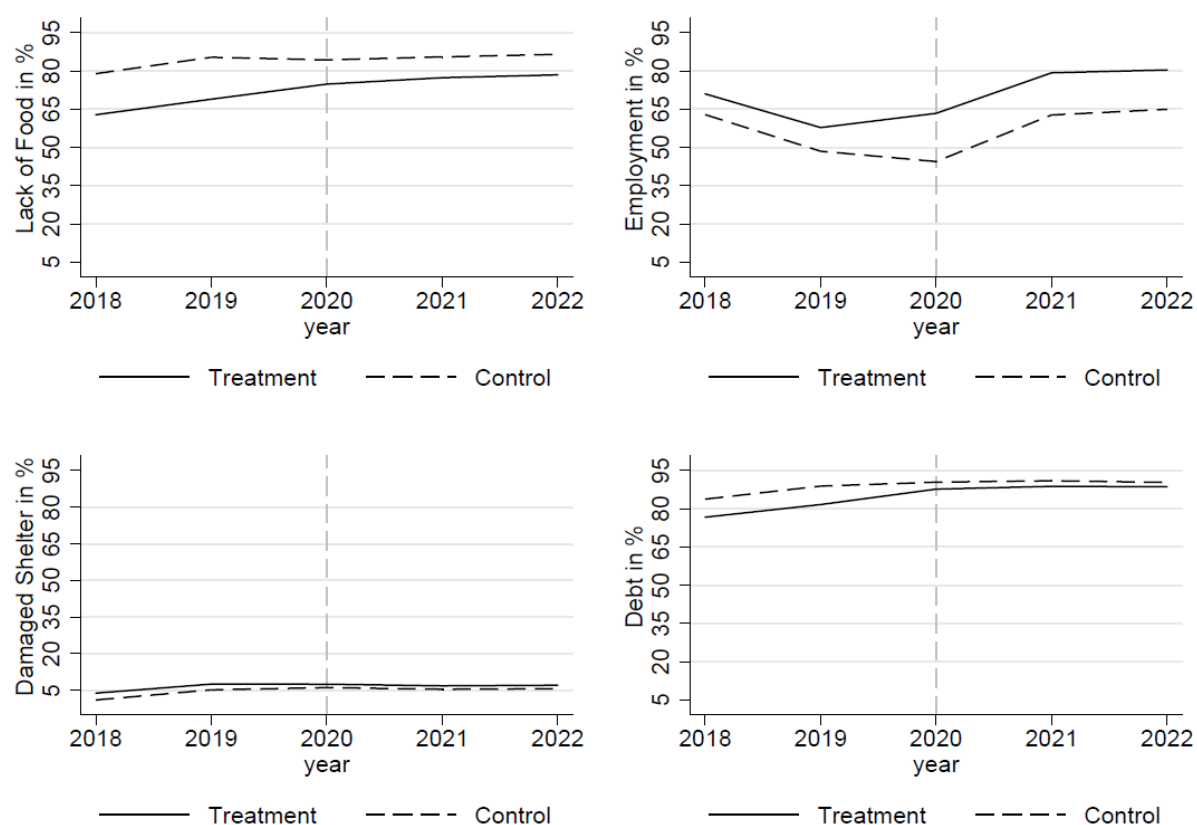
Although it is well understood that disasters disproportionately harm vulnerable populations, evidence on the effect size and underlying transmission channels of such shocks in protracted displacement settings remains limited. This study contributes by quantifying how a sudden urban disaster propagates through constrained refugee households, where access to formal institutions, the labour market, and social safety nets is severely restricted. In such environments, shocks are transmitted through private coping mechanisms, most notably increased debt reliance and the reallocation of children's time away from schooling, rather than being absorbed by public support systems. In the context of ongoing crises across West Asia, where conflict, displacement, and the destruction of infrastructure continue to expose children to repeated shocks, understanding these dynamics is essential for assessing the long-term implications for human capital accumulation.

4.10 Appendix III

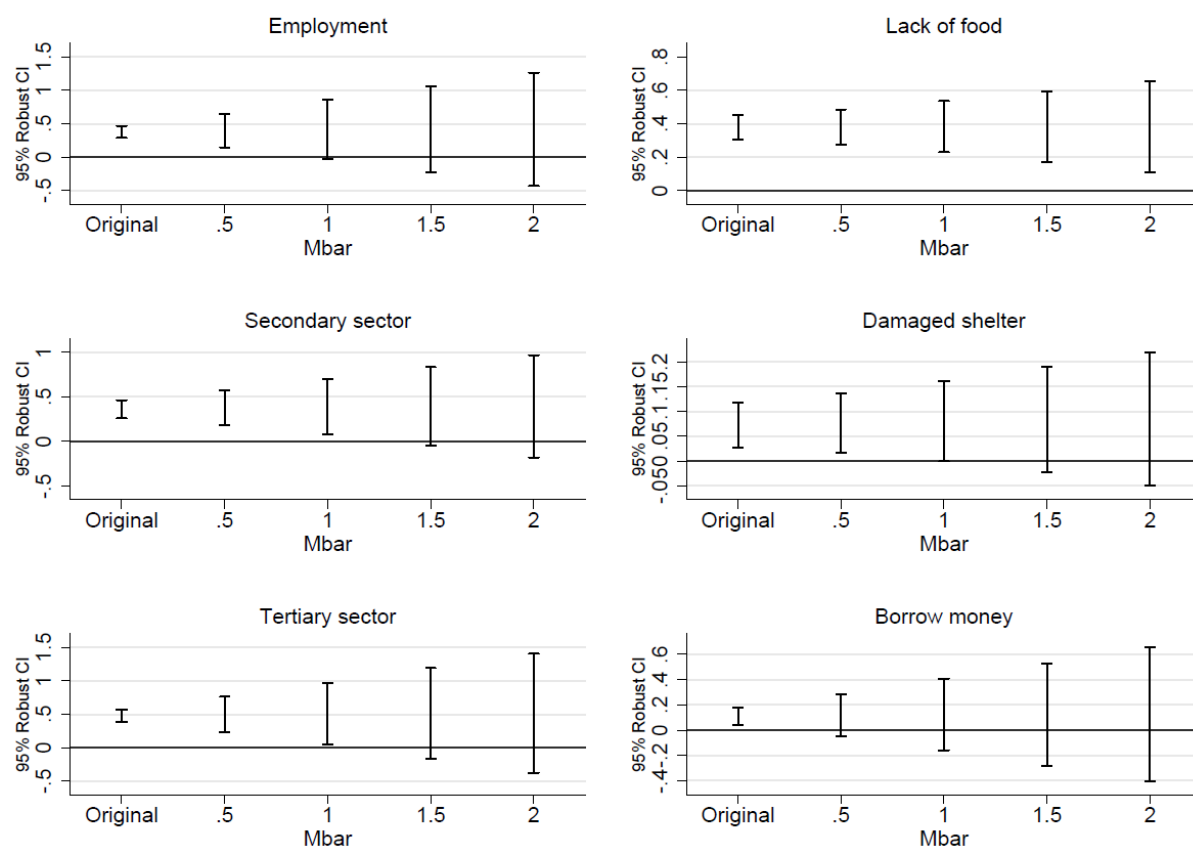
Figure 4-4-A Beirut Port Explosion – Treatment and Control Definitions



Notes: This map represents the districts in Lebanon. For the main analysis, we use the treatment and control definitions provided in (I) of this figure, where we exclude the two neighbouring districts (Baabda and El Meten). In (II), we provide another definition for robustness check, by taking Beirut and the two mentioned neighbouring districts as treated and the other remaining districts as control. In (III), we redefine our treatment to the two neighbouring districts and exclude Beirut from the analysis. In (IV), we define Tripoli as treated and its neighbours, El Koura and Zgharta, as control. Lastly, in (V), we compared Beirut in the treatment group with Tripoli in the control group.

Figure 4-5-A Main Outcome Variables by Survey Year – Household Level

Notes: The data is taken from the VASyR for the years 2018 till 2022. This figure illustrates the predicted shares of lack of food, employment, damaged shelter and debt, based on the full set of observable characteristics of Syrian refugees in Baabda and El Meten (treatment) and remaining Syrian Refugees outside Beirut (control). Beirut is excluded. Beirut port explosion happened on August 4, 2020.

Figure 4-6-A Sensitivity Analysis – Honest DiD Event Study

Notes: The data is taken from the VASyR for the years 2018 till 2022. The figures illustrate the sensitivity analysis conducted according to Rambachan and Roth (2023). The regressions include district and year fixed effects. Standard errors are clustered at the district level.

Table 4-8-A Source of Debt for the Households

	Lebanese friend III	Non-Lebanese friend IV	Lender V	Landlord VI
<i>Effect of BeirutBlast</i>	0.094*** (0.023)	0.005 (0.007)	0.003 (0.003)	0.081*** (0.019)
Observations	14,112	14,112	14,112	14,112
R ²	0.032	0.012	0.007	0.077

Notes: Results are obtained from DiD models. The data is taken from the VASyR for the years 2018 till 2022. The regressions include the full set of controls, district fixed effects and year fixed effects. Standard errors in parentheses (clustered at district level). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 4-9-A Pre-Beirut Port Explosion Descriptive Statistics – Individual Level

	All		Treatment		Control		t-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean diff.
Dependent variables							
Marital status	0.010	0.098	0.013	0.113	0.010	0.097	0.003
School enrolment	0.612	0.487	0.675	0.469	0.610	0.488	0.049***
Labour	0.052	0.221	0.050	0.217	0.052	0.221	-0.006
Control variables							
Male	0.537	0.499	0.546	0.498	0.537	0.499	0.013
No. of children	4.633	1.715	4.574	1.747	4.635	1.713	-0.075
No. of adults	2.262	0.795	2.201	0.721	2.264	0.797	-0.042*
No. of elderly	0.087	0.318	0.105	0.367	0.087	0.316	0.024**
<i>Age category</i>							
05 to 09	0.477	0.500	0.448	0.497	0.479	0.500	-0.032**
10 to 14	0.336	0.472	0.356	0.479	0.335	0.472	0.021
15 to 19	0.186	0.389	0.196	0.397	0.186	0.389	0.012
<i>Household education mother</i>							
No formal education	0.179	0.384	0.171	0.376	0.180	0.384	-0.012
Kindergarten	0.001	0.024	0.001	0.030	0.001	0.023	-0.000
Primary school	0.496	0.500	0.508	0.500	0.496	0.500	0.013
Secondary school	0.235	0.424	0.229	0.420	0.235	0.424	-0.008
High school	0.058	0.235	0.070	0.256	0.058	0.234	0.011
Technical school	0.016	0.127	0.011	0.104	0.017	0.128	-0.003
University	0.014	0.118	0.010	0.100	0.014	0.119	-0.000
<i>Household education father</i>							
No formal education	0.111	0.314	0.079	0.269	0.112	0.315	-0.027***
Kindergarten	0.001	0.030	0.002	0.041	0.001	0.030	0.000
Primary school	0.454	0.498	0.471	0.499	0.454	0.498	0.004
Secondary school	0.333	0.471	0.347	0.476	0.332	0.471	0.020
High school	0.060	0.237	0.074	0.261	0.059	0.236	0.015**
Technical school	0.017	0.128	0.015	0.123	0.017	0.129	-0.002
University	0.025	0.156	0.013	0.111	0.025	0.157	-0.010**
Observations	11,862		1,185		10,677		

Notes: The data is taken from the VASyR for the years 2018 and 2019 on the individual level. Treatment are Syrian refugee children, aged 5 to 19, living in Beirut vs. Control group those who are living outside Beirut (excluding the near surrounding).

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Table 4-10-A Post-Beirut Port Explosion Descriptive Statistics – Household Level

	All		Treatment		Control		t-test
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean diff.
Dependent variables							
Employment	0.566	0.496	0.754	0.431	0.558	0.497	0.151***
<i>Sector</i>							
Secondary	0.333	0.471	0.282	0.450	0.336	0.472	-0.064***
Tertiary	0.409	0.492	0.704	0.457	0.393	0.488	0.385***
Damaged shelter	0.059	0.236	0.082	0.275	0.058	0.234	0.010
Borrow money	0.904	0.294	0.874	0.332	0.905	0.293	-0.020*
Lack of food	0.852	0.355	0.813	0.390	0.853	0.354	0.005
<i>Coping strategies for lack of food</i>							
Reduced portion size	0.698	0.459	0.793	0.405	0.694	0.461	0.088***
Adults reduced consumption	0.358	0.479	0.443	0.497	0.355	0.478	0.072***
Eat elsewhere	0.079	0.269	0.082	0.274	0.079	0.269	0.031***
Reduced number of meals	0.675	0.468	0.778	0.416	0.671	0.470	0.085***
Moved to cheaper place	0.048	0.213	0.067	0.251	0.047	0.211	0.022***
Removed child from school	0.050	0.219	0.079	0.270	0.049	0.216	0.033***
Female reduce consumption	0.082	0.275	0.137	0.344	0.080	0.271	0.092***
Beirut	0.040	0.196	1.000	0.000	0.000	0.000	1.000
Control variables							
Male	0.982	0.134	0.992	0.089	0.981	0.135	0.011**
No. of children	3.109	1.741	2.944	1.717	3.116	1.741	-0.166**
No. of adults	2.652	1.173	2.634	1.170	2.653	1.173	-0.010
No. of elderly	0.092	0.343	0.070	0.312	0.093	0.344	-0.011
<i>Household age</i>							
15 to 24	0.049	0.217	0.060	0.238	0.049	0.216	0.007
25 to 34	0.344	0.475	0.337	0.473	0.345	0.475	-0.004
35 to 44	0.378	0.485	0.385	0.487	0.378	0.485	-0.005
45 to 55	0.158	0.365	0.154	0.361	0.158	0.365	-0.004
> 55	0.070	0.255	0.064	0.246	0.070	0.255	0.006
<i>Type of housing</i>							
Apartment	0.657	0.475	0.725	0.447	0.654	0.476	0.028*
Tent	0.188	0.391	0.003	0.052	0.196	0.397	-0.128***
Other	0.154	0.361	0.273	0.446	0.150	0.357	0.099***
<i>Household education mother</i>							
No formal education	0.060	0.237	0.049	0.216	0.060	0.238	-0.020**
Kindergarten	0.000	0.022	0.000	0.000	0.001	0.023	-0.001
Primary school	0.418	0.493	0.414	0.493	0.419	0.493	-0.018
Secondary school	0.353	0.478	0.341	0.474	0.354	0.478	0.002
High school	0.110	0.313	0.111	0.314	0.110	0.313	0.005
Technical school	0.016	0.126	0.026	0.160	0.016	0.124	0.012***
University	0.042	0.201	0.059	0.236	0.041	0.199	0.019**
<i>Household education father</i>							
No formal education	0.032	0.175	0.021	0.142	0.032	0.177	-0.017**
Kindergarten	0.000	0.019	0.000	0.000	0.000	0.019	-0.000
Primary school	0.456	0.498	0.442	0.497	0.457	0.498	-0.028
Secondary school	0.350	0.477	0.356	0.479	0.350	0.477	0.010
High school	0.090	0.286	0.096	0.295	0.089	0.285	0.013
Technical school	0.018	0.134	0.027	0.163	0.018	0.133	0.011**
University	0.054	0.225	0.058	0.234	0.053	0.255	0.010
Observations	7,734		794		6,940		

Notes: The data is taken from the VASyR for the years 2020 till 2022 on the household level. Treatment are Syrian refugees living in Beirut vs. Control group those who are living outside Beirut (excluding the near surrounding). * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

5 Conclusion

This dissertation has examined how educational decisions, made under conditions of imperfect information, institutional constraints, or socioeconomic pressure, shape students' educational trajectories and, ultimately, human capital formation. Building on the human capital and rational choice theory, the analyses presented here provide empirical evidence on how families and children respond to incentives and constraints when making decisions on schooling, even in the presence of uncertainty and limited resources, and what that means for social mobility.

Chapter 2 investigates the consequences of constrained school allocation, focusing on what happens when students lose the admission lottery and do not receive their top-ranked school place. Despite parental concerns and dissatisfaction, the analysis reveals no adverse effects on short- or long-term academic outcomes. From the perspective of expected utility theory, this suggests that students' realised educational returns may remain stable even when their stated preferences are not met. This finding is important, as tracking in Bremen occurs early and the assigned secondary school typically determines the child's path through the remainder of compulsory schooling. However, the results point to non-academic costs, such as longer travel distances and different peer and school environments, which in some cases lead families to opt out and enrol in private schools with a more homogeneous peer constellation. As access to such opt-outs is socially stratified, the applied centralised admission procedure may unintentionally deepen social inequality despite their formal neutrality.

Chapter 3 shows that reducing informational barriers can substantially improve school choice outcomes during the transition from primary to secondary school. In the context of the immediate acceptance mechanism in Bremen, an information intervention in families' native languages enabled structurally disadvantaged students to make more informed decisions, increasing their likelihood of selecting and attending academic schools and becoming more selective in their ranking with respect to the secondary school composition. These findings suggest that when families are provided with clear and comprehensible information, they are better able to navigate complex allocation systems and make choices more closely aligned with rational decision-making principles. This contributes to reducing systemic informational inequities that would otherwise reinforce educational disadvantage.

Chapter 4 extends the analysis to a context of severe socioeconomic constraint, the 2020 Beirut Port Explosion, demonstrating how households under pressure make rational, albeit difficult, decisions regarding their children's education. Even when these decisions, such as withdrawing children from school or involving them in labour or early marriage, come at the expense of long-term educational prospects, they often represent the only feasible option under binding resource constraints. This underscores the importance of viewing educational choices as economic decisions, particularly when families are forced to prioritise immediate survival over long-term investments in human capital. To prevent this,

aid should be more targeted and effective to help keep children in school, as reattendance becomes increasingly unlikely once children take on other responsibilities within the household.

Clearly, Bremen in Germany and Beirut in Lebanon are contrasting empirical settings to explore different manifestations of inequality across diverse institutional and socioeconomic contexts. Bremen, a city state in a rich country with a formalised school assignment system and a long-standing experience with the migrant population, illustrates how structural inequality can persist even within regulated and ostensibly fair frameworks. Beirut, by contrast, offers insight into how fragile institutions and acute poverty shape educational choices under extreme conditions. Examining both settings contributes to a better and broader understanding of how educational decisions contribute to either the reproduction or the mitigation of social inequality in markedly different environments. As mentioned, international assessments such as PISA have already highlighted the persistent educational inequalities in both countries. These disparities are not limited to Lebanon and Germany but are common in OECD countries and are likely to be present worldwide. Included in the UN's sustainable development goals, it remains a central educational and societal objective that neither family background nor migration history should determine a child's educational opportunities, particularly because education is a powerful driver of social mobility and a prerequisite for a self-determined life.

Thus, education systems need to further develop funding mechanisms that are responsive to need and capable of addressing social disadvantage in a targeted and sustainable manner. This includes allocating resources not only based on student numbers, but also on indicators of socioeconomic vulnerability and migration background. To achieve this, high-quality administrative data and transparent, local-level monitoring are essential. These data must be made accessible to enable researchers to conduct microeconomic analyses, which are crucial for identifying causal relationships and evaluating the effectiveness of educational policies. One strand of future research should therefore focus on the design and assessment of equitable financing models, the implementation of compensatory measures at the school level, and the long-term impacts of early interventions. Only through such sustained and targeted efforts, supported by robust data and rigorous empirical analyses and evaluations, can education systems move closer to delivering on the promise of equal opportunity and mitigating the structural disadvantages that continue to shape educational trajectories.

In sum, this dissertation shows that educational decisions are not only pivotal for individual life courses but also for shaping the distribution of opportunities across societies. When made under structural disadvantage, these decisions can entrench rather than alleviate social inequalities. Although families act rationally within the limits of their resources and knowledge, such decisions often carry long-term costs, especially for marginalised groups. The empirical findings highlight that improving access to clear information empowers disadvantaged families to make better-informed choices, while transparency and equity in allocation mechanisms are essential to maintain trust and fairness. Moreover, crisis conditions reveal the urgency of institutional support systems that protect educational participation

under extreme vulnerability. Ensuring that access to education is not determined by socioeconomic status, migration background, or refugee experience is essential for fostering upward mobility and social cohesion. This dissertation thus provides evidence-based insights for designing more inclusive education policies that reduce inequality and promote long-term human capital development.

6 Bibliography

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Declaration of Academic Integrity

I hereby declare that the work I have submitted has been written independently. In chapters that are co-authored, I have clearly indicated the respective co-authors and specified my contribution to each chapter. All passages that are quoted verbatim or paraphrased from published or unpublished works of others or from my own previous work are clearly marked as such. All sources that I have used for this work are properly cited and listed.

I further declare that artificial intelligence-based tools were used exclusively for language editing purposes, such as checking grammar, spelling, and clarity of expression. No such tools were used to generate content, develop arguments, conduct analyses, or interpret results. All data preparation, statistical analyses, and empirical results were conducted using STATA and R.

Furthermore, I confirm that this work has not been submitted in the same or a substantially similar form to any other examination authority.

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