UNDERSTANDING THE IMPACT OF EDUCATION AND ENVIRONMENTAL POLICY ON HUMAN CAPITAL FORMATION

FOUR ESSAYS IN APPLIED ECONOMICS

INAUGURAL-DISSERTATION

zur Erlangung des akademischen Grades
einer Doktorin der Wirtschaftswissenschaft
doctor rerum politicarum
(Dr. rer. pol.)

am Fachbereich Wirtschaftswissenschaft
der Freien Universität Berlin

vorgelegt von
Laura Schmitz (M.Sc.)
geboren in Boppard

Berlin, März 2023

Erstgutachterin: Prof. Dr. C. Katharina Spieß
Bundesinstitut für Bevölkerungsforschung und bis September 2021 Freie Universität Berlin, seit Oktober 2021 Johannes Gutenberg-Universität Mainz

Zweitgutachter: Prof. Dr. Felix Weinhardt
Europa-Universität Viadrina Frankfurt (Oder) und DIW Berlin

Disputation: 23. Juni 2023

Acknowledgments

First and foremost, I would like to thank my advisors Katharina Spieß and Felix Weinhardt. Thank you Katharina for your continuous encouragement, guidance and very helpful comments on all of my papers. I really appreciate that you were always available – even after you took on your new position and had a million things on your plate. Thanks for reading my drafts during late-night train rides in order to give me swift feedback. Thank you Felix for your methodological guidance and support especially on my single-authored paper. I greatly benefited from your encouragement during Covid times when my motivation was low, progress slow and the offices empty. Thanks also for the always useful advice on all things publishing, academia, and generally "how to PhD".

Further, I would like to thank my co-authors Mara Barschkett, Johannes Brehm, Marcel Helbig, Nico Pestel, Sandra Schaffner, and Sophia Schmitz for the pleasant, engaging and fruitful collaboration. After working mostly on my single-authored paper for the first half of my PhD, it was extremely rewarding to team up with such passionate, smart and kind people to produce papers that I am proud of. Thanks for the great teamwork and stimulating discussions along the way.

I also would like to thank my colleagues from the DIW Graduate Center, especially Martin Kittel, Mara Barschkett, Alex Roth, Laura Pagenhardt, Johannes Seebauer, and Lukas Boer for keeping up my motivation during the course phase, for frequent lunches, stimulating conversations and your friendship. The same holds true for my office mates over the time: Mara Barschkett (again), Annekathrin Schrenker and Elisabeth Asche. I'm further grateful for my supportive and kind colleagues at the former Education and Family Department and the Public Economics Department (which hasgiven me a second home during the last months of my PhD), among others: Jonas Jessen, Josefine Koebe, Ludovica Gambaro, Jan Marcus, Clara Schäper, and Andi Leibing. Thank you Katharina Spieß and Peter Haan for creating a friendly and welcoming atmosphere in your departments.

I gratefully acknowledge funding through the *BRISE* and SAW *ilearn* projects, as well as the DIW Graduate Center. Many thanks also to Juliane Metzner for the organizational support and for the great GC summer workshops. Moreover, I am grateful to the SOEP team for access to the geo-referenced data and support.

Last but not least, I would like to thank my family and friends. Above all, I am deeply grateful to my mother, Mascha, and Johannes for your loving support and for always believing in me.

Berlin, July 11, 2023
Laura Schmitz

Contents

Ko-Autorenschaften und Vorveröffentlichungen

Diese Dissertation besteht aus vier (Arbeits-)Papieren, von denen drei in Zusammenarbeit mit Co-AutorInnen entstanden sind:

Heterogeneous effects of after-school care on child development

- Ko-AutorInnen: keine
- Vorveröffentlichungen:
  - Dies ist eine modifizierte Version des folgenden Arbeitspapiers:
    Schmitz, L. (2022). Heterogeneous effects of after-school care on child development. *DIW Berlin Discussion Paper* No. 2006.
    `http://dx.doi.org/10.2139/ssrn.4159452`
  - Teilweise basierend auf diesem Kapitel ist folgende Transferpublikation erschienen:
    Schmitz, L. (2022). Ganztagsschulen fördern die Entwicklung sozialer Fähigkeiten von Grundschüler*innen. *DIW Wochenbericht* 89(48).
    `https://doi.org/10.18723/diw_wb:2022-48-1`

Sozioökonomische Zusammensetzung der SchülerInnenschaft an Privatschulen: Wie viel erklärt die geografische Verteilung privater Schulangebote?

- Ko-Autor: Marcel Helbig
- Vorveröffentlichungen:
  - Teilweise basierend auf diesem Kapitel ist folgende Transferpublikation erschienen:
    Helbig, M., Schmitz, L., und Weinhardt, F. (2022). Selbst wenn Privatschulen in der Nähe sind: Sozial benachteiligte Schüler* innen sind dort kaum vertreten. *DIW Wochenbericht* 89(51/52).
    `https://doi.org/10.18723/diw_wb:2022-51-1`

From low emission zone to academic track: Environmental policy effects on educational attainment in elementary school

- Ko-AutorInnen: Johannes Brehm, Nico Pestel und Sandra Schaffner
- Vorveröffentlichungen:
  - Dies ist eine modifizierte Version des folgenden Arbeitspapiers:
    Brehm, J., Pestel, N., Schaffner, S., und Schmitz, L. (2022). From low emission zone to academic track: Environmental policy effects on educational achievement in elementary school. *Ruhr Economic Papers* 980.
    `https://doi.org/10.4419/96973145`

Costs and short-term effects of a home-visiting program in *BRISE* – First steps for a cost-effectiveness analysis

- Ko-AutorInnen: Mara Barschkett und Sophia Schmitz
- Vorveröffentlichungen: keine

Rechtliche Erklärung

Erklärung gem. §4 Abs. 2 (Promotionsordnung)

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

Berlin, 11. Juli 2023
Laura Schmitz

Erklärung gem. §10 Abs. 3 (Promotionsordnung)

Hiermit erkläre ich, dass ich für die Dissertation folgende Hilfsmittel und Hilfen verwendet habe. Auf dieser Grundlage und in Zusammenarbeit mit meinen Co-AutorInnen habe ich die Arbeit selbstständig verfasst.

- Software:
    - Stata Versionen 16 und 17
    - Rstudio Versionen 1.2.1335 und 2021.09.0
    - LaTeX mit Overleaf
- Literatur: siehe Literaturverzeichnis

Berlin, 11. Juli 2023
Laura Schmitz

Abstract

This dissertation analyzes different ways in which policymakers can impact the formation of human capital and the emergence of inequalities therein. It comprises four self-contained empirical research papers that contribute to the economic analysis of skill formation during childhood. The chapters explore different dimensions of the multifaceted concept of human capital, including developmental aspects during the first year of a child's life; different measures of student performance in elementary school; socio-emotional development and personality; and aspects of physical and mental health. It analyzes reforms, programs, and developments that affect individuals differently and at various stages in their lives: as toddlers, in primary and secondary school, and as adults when caring for their children. The chapters are preceded by a general introduction of the topic (**Chapter 1**) and followed by a conclusion discussing the policy implications, limitations, and scope for further research (**Chapter 6**).

The first two chapters examine different implications of the expansion of educational offers in the public and private school sectors. **Chapter 2** analyzes the roll-out of after-school care (ASC) programs within German elementary schools due to an extensive subsidy program in 2003. These programs, consisting mainly of homework support and supervised recreational activities, are usually offered voluntarily, although organizational forms with stricter participation requirements also exist. It is often argued that institutionalized ASC can benefit children lacking adequate homework support at home and are likely to foster equality of opportunity. However, despite considerable policy interest, it is unclear whether these afternoon programs benefit child development and whether they are reaching the "right" children. My research interests in this paper are twofold: First, I study which groups of students benefit from ASC. Second, I analyze how the selection into the afternoon programs relates to the treatment effect, i.e., whether a universal offer of ASC attracts the children that benefit from it. I use a unique combination of self-collected school-level data from six Western German federal states between 2003 and 2018 with student-level data from the German Socio-Economic Panel (SOEP). By analyzing the effects on grades in math and German, the Strengths and Difficulties Score, prosocial behavior, and the Big Five personal-

ity traits, I cover critical aspects of cognitive and non-cognitive development. Using a Marginal Treatment Effect framework and regional and temporal variation caused by the subsidy program, I instrument after-school care attendance with the change in distance to the next school offering ASC within one district. My findings suggest that children with a low socio-economic status (SES), who more often select into treatment, tend to have higher ASC premiums. Further, the average treatment effects on the treated's non-cognitive skills are more sizable than those on the untreated, suggesting that selection into ASC is positive and efficient. Overall, a universal voluntary offer of ASC will likely help reduce educational inequality.

Chapter 3 then turns to the expansion of the private school sector in Germany. This paper is motivated by the significant under-representation of socio-economically disadvantaged children in private schools, despite the Basic Law's Article 7(4), which prohibits schools from discriminating based on socio-economic status when selecting students. To comply with this law, private schools must either adjust their fees based on the parents' income, set them low enough to be affordable, or offer allowances for economically disadvantaged households. Although the interpretation of this rule varies across federal states, school fees alone may not be the sole reason for the imbalanced socio-economic composition in private schools. This study investigated the role of the geographical distribution of private schools to better understand why low-SES children attend private schools less frequently. We estimate linear probability models using geo-referenced data from the SOEP and address data for all German schools (public and private) from 2000 to 2019. Our results suggest that high-SES households do not necessarily have shorter distances to private schools but are more "distance-sensitive" when deciding on private school enrollment. Our findings indicate that low-SES students, in addition to school fees, may be deterred from attending private schools because of personal preferences and a lack of information on alternative school forms, and that the spatial distribution of private schools plays a subordinate role in this regard.

Chapter 4 moves away from schooling policies and explores the connections between environmental policies and human capital. Children are particularly susceptible to the adverse health effects of air pollution ranging from respiratory diseases to infant mortality. Recent economic literature has shown that poor air quality may also harm the human brain, affecting individuals' cognitive performance and leading to behavioral problems. Given these findings, it is unsurprising that air quality on a given day can also affect children's test scores and school absence. Substantially less is known about the long-term schooling effects of policies targeting air quality. In this study, we examine the causal effect of implementing Low Emission Zones (LEZs) on the educational achievement of elementary school students in Germany. We focus on the transition rates of children in 4^{th} grade, the last year of primary education, to a *Gymnasium*, the academic track of the secondary school system. LEZs reduce local air pollution by restricting emission-intensive vehicles from accessing designated areas, and have been shown to improve population health. Little is known about the effects of driving restriction policies in other areas of life. Using school-level data from North-Rhine Westphalia (NRW), Germany's most populous federal state, we exploit the staggered adoption of LEZs since 2008 within a difference-in-differences framework. Our results imply that LEZs increased transition rates to the academic track by 0.9-1.6 percentage points in NRW. Our findings on the district level for all of Germany confirm the external validity of these findings. Using geo-referenced data from the German Socio-Economic Panel, we provide suggestive evidence that reducing the prevalence of respiratory infections is a vital channel through which LEZs affect schooling outcomes.

Finally, **Chapter 5** focuses on the home environment during the first year of a child's life. Home-visiting programs targeting families during pregnancy or shortly after birth can be powerful tools to promote child and family well-being, particularly for disadvantaged families. However, there is little evidence on the effectiveness of these programs' (cost-)effectiveness in the European context. In this study, we present novel evidence of the costs and effects of *Pro Kind*, a home-visiting program under the Bremen Initiative to Foster Early Childhood Development (*BRISE*). *BRISE* randomly assigns an information and access treatment on the neighborhood level that nudges families in the treatment group to participate in *Pro Kind*. We exploit this random variation in an instrumental variables (IV) framework combined with entropy balancing to estimate the causal effects of the intervention on several mother and child outcomes during the first seven months of children's lives. In addition, we provide cost estimates based on self-collected cost data. At this early stage of the intervention, and due to data limitations, we cannot deduce meaningful causal effects of *Pro Kind* on child and maternal outcomes. The cost analysis suggests that *Pro Kind* is less costly than most comparable early childhood programs. Our analysis builds the basis for future cost-effectiveness and cost-benefit studies, which are essential tools for making sound policy decisions on allocating scarce resources.

Zusammenfassung

In dieser Dissertation werden verschiedene Möglichkeiten analysiert, wie politische Entscheidungsträger die Bildung von Humankapital und die Entstehung von Ungleichheiten in diesem Bereich beeinflussen können. Sie besteht aus vier in sich abgeschlossenen empirischen Forschungsarbeiten, die jeweils einen Beitrag zur bildungsökonomischen Analyse der Bildung von Humankapital leisten. Unter Berücksichtigung des vielschichtigen Charakters des Konzepts Humankapital werden in den Kapiteln verschiedene Dimensionen untersucht, darunter Entwicklungsaspekte während des ersten Lebensjahres eines Kindes, verschiedene Maßstäbe für die Leistung von SchülerInnen in der Grundschule, die sozio-emotionale Entwicklung und die Persönlichkeit sowie Aspekte der körperlichen und mentalen Gesundheit. Es werden Reformen, Programme und Entwicklungen analysiert, die Individuen zu unterschiedlichen Zeitpunkten im Lebensverlauf betreffen: Als Kleinkinder, in der Grund- und Sekundarschulzeit und als Erwachsene bei der Erziehung der eigenen Kinder. Den Kapiteln geht eine allgemeine Einführung in das Thema voraus (**Kapitel 1**). Die Dissertation schließt mit einer Schlussfolgerung ab, in der politische Implikationen, Limitationen und Möglichkeiten für anknüpfende Forschung diskutiert werden (**Kapitel 6**).

In den ersten beiden Kapiteln werden unterschiedliche Auswirkungen des Ausbaus von Bildungsangeboten im öffentlichen bzw. privaten Schulbereich untersucht. **Kapitel 2** analysiert die heterogenen Effekte des Ausbaus von Ganztagsbetreuung auf die Kindesentwicklung. Es wird häufig argumentiert, dass institutionalisierte Nachmittagsbetreuung insbesondere solchen Kindern zugutekommen kann, die zu Hause keine angemessene Unterstützung bei den Hausaufgaben erhalten und somit die Chancengleichheit fördern kann. Trotz beträchtlichen politischen Interesses ist jedoch unklar, ob diese Nachmittagsprogramme die Entwicklung von Kindern fördern und ob der zugrunde liegende Selektionsmechanismus effizient ist, d. h. ob diejenigen SchülerInnen, die am meisten von den Programmen profitieren würden, sich für die Teilnahme entscheiden. In diesem Beitrag untersuche ich die Auswirkungen von Nachmittagsbetreuung an Ganztagsschulen auf die schulischen Leistungen und die Entwicklung sozio-emotionaler Fähigkeiten von Grundschulkindern. Unter Verwendung der Marginal Treatment Effect

Methode und regionaler sowie zeitlicher Variation im Zugang zu Ganztagsbetreuung im Zuge einer umfassenden Reform in Deutschland instrumentiere ich die Teilnahme an der außerschulischen Betreuung mit der Veränderung der Entfernung zur nächsten Schule mit Ganztagsangebot innerhalb eines Kreises. Meine Ergebnisse deuten darauf hin, dass Kinder aus niedrigeren sozioökonomischen Verhältnissen häufiger Ganztagsangebote nutzen und davon zum Teil auch stärker profitieren. In Bezug auf die sozio-emotionale Entwicklung zeigt sich, dass Kinder mit einer niedrigen "Resistenz", die freiwillig eine Ganztagsschule besuchen, auch häufiger davon profitieren. Dies deutet darauf hin, dass die Selektion in Ganztagsschulen effizient ist, da die Angebote die "richtigen"Kinder anziehen. Insgesamt ist davon auszugehen, dass ein universelles freiwilliges Ganztagsangebot dazu beiträgt, Bildungsungleichheiten zu verringern.

Im Anschluss beschäftigen wir uns in **Kapitel 3** mit der Ausweitung des Privatschulsektors in Deutschland. Der Anlass für dieses Papier ist die erhebliche Unterrepräsentation von sozioökonomisch benachteiligten Kindern an Privatschulen trotz des “Sonderungsverbots” nach Artikel 7(4) im Grundgesetz. Privatschulen dürfen demnach zwar generell ein Schulgeld verlangen, müssen dieses aber entweder nach dem Einkommen der Eltern staffeln, Schulgeldbefreiungen für ökonomisch benachteiligte Haushalte anbieten oder das Schulgeld so niedrig ansetzen, dass es theoretisch von allen Eltern gezahlt werden kann. Obwohl diese Vorschrift in den einzelnen Bundesländern unterschiedlich ausgelegt wird, ist das Schulgeld allein möglicherweise nicht der einzige Grund für die unausgewogene sozioökonomische Zusammensetzung der Privatschulen. Um besser zu verstehen, warum Kinder mit niedrigem sozioökonomischem Status seltener Privatschulen besuchen, wird in dieser Studie die Rolle der geografischen Verteilung von Privatschulen untersucht. Mithilfe von georeferenzierten Daten des Sozio-oekonomischen Panels und Adressdaten aller deutschen Schulen (öffentlich und privat) von 2000 bis 2019 schätzen wir lineare Wahrscheinlichkeitsmodelle. Unsere Ergebnisse deuten darauf hin, dass sozioökonomisch privilegierte Haushalte nicht notwendigerweise näher an Privatschulen wohnen, aber empfindlicher auf die Entfernung reagieren, wenn es um die Entscheidung für eine Privatschule geht. Sozioökonomisch benachteiligte SchülerInnen werden möglicherweise nicht nur aufgrund der Schulgebühren, sondern auch aufgrund persönlicher Präferenzen und mangelnder Informationen zu alternativen Schulangeboten vom Besuch von Privatschulen abgehalten. Die räumliche Verteilung von Privatschulen spielt dabei eine untergeordnete Rolle.

Kapitel 4 wendet sich von der Schulpolitik ab und widmet sich der Untersuchung der Zusammenhänge zwischen Umweltpolitik und Humankapital. Empirische Studien haben gezeigt, dass Kinder aufgrund ihrer erhöhten Anfälligkeit besonders negativ von Luftverschmutzung betroffen sind, die von Atemwegserkrankungen bis hin zur Kindersterblichkeit reicht. Neuere Studien haben zudem gezeigt, dass schlechte Luftqualität das menschliche Gehirn schädigen und die kognitive Leistung beeinträchtigen sowie zu Verhaltensproblemen führen kann. Daher ist es nicht überraschend, dass die Luftqualität auch die Testergebnisse und Fehlzeiten von Kindern in der Schule beeinflussen kann. Allerdings ist bisher wenig darüber bekannt, welche langfristigen Auswirkungen Maßnahmen zur Verbesserung der Luftqualität auf die schulische Bildung haben. In diesem Beitrag wird der kausale Effekt der Einführung von Umweltzonen auf die schulischen Leistungen von GrundschülerInnen in Deutschland untersucht. Umweltzonen reduzieren die lokale Luftverschmutzung, indem sie emissionsintensiven Fahrzeugen den Zutritt zu ausgewiesenen Gebieten verwehren und somit nachweislich die Gesundheit der Bevölkerung verbessern. Wenig ist hingegen darüber bekannt, welche Auswirkungen Fahrverbote wie LEZs auf andere Lebensbereiche haben. Wir nutzen Daten auf Schulebene aus Nordrhein-Westfalen, um die gestaffelte Einführung von Umweltzonen seit 2008 in einem Differenz-in-Differenzen-Rahmen zu analysieren. Die Ergebnisse deuten darauf hin, dass die Umweltzonen die Übergangsraten von Grundschülerinnen und Grundschülern auf Gymnasien in Nordrhein-Westfalen um 0,9-1,6 Prozentpunkte erhöht haben. Diese Ergebnisse werden durch unsere Analysen auf Kreisebene für ganz Deutschland bestätigt, was ihre externe Validität stützt. Zudem liefern wir Hinweise darauf, dass eine Verringerung der Prävalenz von Atemwegsinfektionen ein wichtiger Kanal ist, über den Umweltzonen den Schulerfolg von GrundschülerInnen positiv beeinflussen.

Kapitel 5 befasst sich schließlich mit dem häuslichen Umfeld im ersten Lebensjahr eines Kindes. Hausbesuchsprogramme, die sich an Familien während der Schwangerschaft oder kurz nach der Geburt richten, können ein wirksames Instrument zur Förderung des Wohlergehens von Kindern und Familien darstellen, insbesondere für benachteiligte Familien. Es gibt jedoch nur wenig Evidenz zu der (Kosten-)Effektivität dieser Programme im europäischen Kontext. In dieser Studie präsentieren wir neue Erkenntnisse zu den Kosten und Auswirkungen von *Pro Kind*, einem Hausbesuchsprogramm im Rahmen der Bremer Initiative zur Förderung der frühkindlichen Entwicklung (*BRISE*). Im Rahmen von *BRISE* werden zufällig (randomisiert auf Stadtteilebene) einige Familien über *Pro Kind* informiert und der Zugang zu diesem Programm erleichtert. Somit werden diese Familien angeregt, an *Pro Kind* teilzunehmen. Wir nutzen diese zufällige Variation in einem Instrumentalvariablen (IV)-Ansatz in Kombination mit Entropy Balancing, um die kausalen Effekte der Intervention auf verschiedene Ergebnisse von Müttern und Kindern während der ersten sieben Lebensmonate der Kinder zu schätzen. Darüber hinaus liefern wir Kostenschätzungen auf der Grundlage selbst erhobener Kostendaten. In diesem frühen Stadium der Intervention und aufgrund von

Datenbeschränkungen können wir keine belastbaren kausalen Effekte von *Pro Kind* auf die Ergebnisse bei Kindern und Müttern ableiten. Die Kostenanalyse legt nahe, dass *Pro Kind* weniger kostenintensiv ist als die meisten vergleichbaren (nationalen und internationalen) frühkindlichen Programme. Unsere Analyse bildet die Grundlage für künftige Kosteneffektivitäts- und Kosten-Nutzen-Studien, welche ein wichtiges Instrument sind, um fundierte politische Entscheidungen über die Zuweisung knapper Ressourcen zu treffen.

List of Figures

List of Tables

CHAPTER 1

Introduction

1.1 Motivation

Monetary and non-monetary returns to education

Education is not only recognized as a universal human right by the United Nations[1] but is widely acknowledged as a significant determinant of a person's success in life. Investing in education provides both monetary (see, e.g. Gunderson & Oreopolous, 2020, for an overview) and non-monetary (see, e.g. Becker et al., 2019, for an overview) returns that are invaluable to individuals and society as a whole.

One of the most significant monetary returns to education is the potential to earn a higher income. Studies consistently show that individuals with higher levels of education tend to earn more over their lifetime compared to those with less education (e.g., Angrist & Krueger, 1991; Card, 1994, 1999; Heckman et al., 2018). In addition, individuals with higher levels of education are at a significantly lower risk of unemployment (Mincer, 1991; Riddell & Song, 2011) and have a higher probability of reemployment if unemployed (Kettunen, 1997). These more favorable labor market outcomes translate into higher tax revenues and lower costs for social security systems on a societal level. In addition, the stock of human capital of an economy is regarded as a fundamental driver of economic growth and development (e.g., Barro, 2001; Hanushek & Kimko, 2000).

Education also provides a range of benefits that are not directly financial – albeit also often with fiscal implications – that can significantly enhance an individual's quality of

[1] Article 26 of the Universal Declaration of Human Rights states that everyone has a right to education and that "Education shall be directed to the full development of the human personality and to the strengthening of respect for human rights and fundamental freedoms." (United Nations, 1948)

life. For example, a higher degree of education is associated with better health (e.g., Cutler & Lleras-Muney, 2010; Grossman, 2006) and less risky health behaviors (e.g., Cawley & Ruhm, 2011; De Walque, 2007), increased life expectancy (e.g., Deryugina & Molitor, 2021; Lleras-Muney, 2005), and a higher degree of general life satisfaction (e.g., Oreopoulos & Salvanes, 2011). An individual's level of education is also associated with more civic engagement (e.g., Do, 2004) and political participation (Oreopoulos & Salvanes, 2011; Sondheimer & Green, 2010), an increased willingness to volunteer and donate (Doyle & Skinner, 2017; Son & Wilson, 2012), a more positive attitude towards immigration (e.g., Margaryan et al., 2021), and generally more prosocial behavior (e.g., Bekkers et al., 2005; Heckman et al., 2006b). Further, investments in education can lower crime rates (e.g., Lochner, 2011) and foster innovations (e.g., Becker, 1994).

From Adam Smith to the credibility revolution: Human capital formation in the economics discipline

Throughout the history of economics, education has been recognized as a crucial factor in the formation of human capital. This notion was already present in the works of seminal thinkers from the discipline's early days. The concept of human capital can be traced back to Adam Smith's renowned "The Wealth of Nations", where he highlighted the significance of the division of labor and specialization in promoting economic development. Smith (1937) argued that the productivity of workers could be increased by training and education, which would result in higher wages and economic growth. His emphasis on the importance of education and the development of specialized skills laid the foundation for later human capital theories.

The term human capital was coined by economists Becker (1964) and Schultz (1963) in the 1960s. They considered education and training personal investments that could significantly boost an individual's productivity and earning capacity, ultimately fostering economic growth. The works of Becker and Schultz represented a significant departure from the prevailing neoclassical economic theory, which primarily emphasized the role of physical capital in driving economic progress. Furthermore, their contributions lent credence to the idea that human capital can be cultivated and further enriched by prioritizing investments in education and training over time.

Since its inception, the concept of human capital has gained widespread recognition and has become an essential notion in economics. For many years, the number of years of formal education a person had was the standard measure used to evaluate human capital. However, in the contemporary era, the profession has come to appreciate that human capital is a multifaceted concept that encompasses more dimensions, including health (e.g., Goldin, 2016) and socio-emotional aspects such as personality traits, attitudes, and preferences (Becker, 1994; Heckman & Carneiro, 2003). In a critical review of his earlier research, James Heckman maintained that "...the preoccupation with cognition and academic "smarts" as measured by test scores to the exclusion of social adaptability and motivation causes a serious bias in the evaluation of many human capital interventions..." (Heckman, 2000, p.1). Numerous studies have since confirmed the significance of personality, behavior, and other non-cognitive traits for educational and labor market success (e.g., Heckman, 2006; Heckman & Carneiro, 2003; Heineck & Anger, 2010). At the same time, we know that health, cognitive, and non-cognitive skills are largely complementary (e.g., Heckman, 2006). For example, healthier individuals can attend school on more days, be more productive, and work more over the life cycle. Similarly, a child with strong social skills may be better equipped to develop cognitive skills through interactions with others and to apply these skills in the real world.

The relationship between skill development and economic inputs was conceptualized in James Heckman and coauthors' fundamental work on the technology of skill formation (e.g., Cunha & Heckman, 2007; Heckman, 2006; Heckman & Masterov, 2007). The main features of the technology of skill formation are that skills are shaped over the life cycle through a combination of factors determined by both *nature* (genetic endowments and innate characteristics) and *nurture* (the socio-economic environment, including family upbringing, cultural and social factors, education, and experiences). While both components interact in complex ways to influence human traits and skill formation, *nurture* is the sole component of human capital that policymakers can influence – thereby also enhancing the interaction between nurture and nature.

Cunha and Heckman's dynamic life-cycle model of human capital formation highlights the effectiveness and efficiency of investing in children early on and throughout childhood. As such, it is imperative to note that children fundamentally rely on their parents to make critical investments on their behalf. However, in reality, not all parents possess complete information on the potential returns to education, act in the best interest of their children, and have unrestricted access to credit. In light of these constraints, a compelling argument for efficiency arises in support of public investments in compulsory education systems and early childhood education and care (ECEC) programs. Indeed, a wealth of empirical literature attests to the significant benefits of investing in early childhood education and care yielding substantial returns (see e.g. Currie, 2001; Karoly et al., 2006; Spieß, 2015, for overviews).

Heckman and coauthors' research on skill formation technology also emphasizes the critical role of targeted interventions for socio-economically disadvantaged children. These children often face significant obstacles to acquiring skills due to factors such as poverty, discrimination, and other forms of disadvantage. A child's family's socio-economic status (SES) also plays a significant role in their access to information, credit, and parental attitudes toward education. If the education market were left entirely unregulated, it would result in an unequal distribution of human capital, with disadvantaged children being left behind (e.g., Duncan et al., 1994; Haveman & Wolfe, 1995; Smith et al., 1997). Due to these reasons, primary and secondary education – as well as increasingly ECEC – are funded by most countries as a matter of public policy. Nevertheless, significant socio-economic disparities in education persist. The impact of a child's family background on their academic achievements is evident from the outset, with those from low-SES households tending to have lower birth weights (Currie & Moretti, 2003). By age three, they have significant language gaps (e.g., Levine et al., 2020) resulting in numerous disadvantages in their development and skills by the time they reach school (e.g., Cunha & Heckman, 2007; Feinstein, 2003; Todd & Wolpin, 2007). The disparities tend to expand throughout their education, resulting in those who fall behind in their formative years continuing to lag or even further declining in their academic performance later in life (e.g., Bradbury et al., 2015; Heckman & Mosso, 2014).

These arguments make a compelling case for government investment in human capital, both in terms of promoting efficiency and equity. Recognizing this fact, most countries allocate a significant portion of their gross domestic product to education, with the OECD average reaching 3.1 percent in 2020 (OECD, 2021). However, with limited resources at their disposal, governments must make strategic choices on how to design public policies that best foster human capital development and minimize associated inequalities effectively and efficiently. These decisions must be grounded in sound theory and empirical evidence. With the emergence of the "credibility revolution" in empirical economics (see Angrist & Pischke, 2010), a recent body of research has employed rigorous research designs to establish causal relationships between educational inputs and outcomes. Furthermore, the increasing availability of international large-scale educational assessment studies, such as the OECD's Programme for International Student Assessment (PISA), allows for a more comprehensive evaluation of the effectiveness of educational policies.

This dissertation contributes to this endeavor by feeding into several contemporary strands of the empirical literature that hold relevance for policymaking. In the subsequent section, I briefly overview these strands, highlighting how they inspired my research questions.

Recent advances in the empirical literature

A substantial body of literature in the economics of education is dedicated to investigating the "educational production" within schools, aiming to identify the most effective input factors that inform optimal resource allocation. Recently, one input of educational production, the provision of after-school care (ASC), has garnered more attention in empirical economics. Although ASC is often offered by facilities outside of school, in certain countries, such as France, Finland, and Germany, it is provided as part of the regular school day (OECD, 2017b). Germany started extending their offer of ASC in 2003, with now more than 70 percent of all primary schools offering afternoon programs (KMK, 2021). ASC, usually comprising a combination of homework support and recreational activities, is often said to have an equalizing effect. Since low-SES children may experience lower-quality homework support at home (e.g., Buckingham et al., 2013; van Bergen et al., 2017), they are likely to benefit disproportionately from the afternoon care at school (e.g., Angrist et al., 2010; Blau & Currie, 2006; Levine & Zimmerman, 2010; Plantenga & Remery, 2015). Despite significant political interest, there is a lack of evidence on the effectiveness of ASC on the cognitive and non-cognitive skill formation of school-age children. While studies have evaluated the effectiveness of ASC for fostering human capital formation, most literature in this area focuses either on targeted programs (e.g., Blau & Currie, 2006) or on non-specified adult supervision (e.g., Aizer, 2004).

Besides the overall effectiveness of public investments in educational and care schemes, it is important to examine *who* benefits from them and how they should be designed to reach the "right" individuals, i.e., those who benefit the most from them. A recent stream of the literature has stressed the importance of effect heterogeneity in identifying the causal effects of investments in human capital (e.g., Conti et al., 2010; Florens et al., 2008). Effect heterogeneity is relevant regarding observable characteristics, e.g., sex, achievement, and SES, and unobserved characteristics, i.e., the personal inclination to enroll (Heckman & Vytlacil, 2001). Educational schemes can only impact child development if they are taken up by individuals likely to benefit from them. As the work of Cornelissen et al. (2018) and Felfe & Lalive (2018) demonstrates, a universal offer of educational schemes does not always reach the children that benefit the most from them. Focusing on early daycare expansions in two German regions for children under six and three, respectively, the studies find that children from disadvantaged backgrounds are less likely to attend but have larger positive treatment effects in terms of school readiness (Cornelissen et al., 2018) and socio-emotional skills (Felfe & Lalive, 2018). The opposite is true for non-compulsory higher education (ages 16 and older), where individuals with a higher gain from treatment are more likely to attend (e.g., Carneiro et al. (2011), Kaufmann (2014), Kamhöfer et al. (2019), Westphal et al. (2020) for tertiary education, and Carneiro et al. (2017) for higher secondary education). These findings have relevant implications for essential policy questions, e.g., whether education and care should be provided on a universal, targeted, or "targeted within universal" (i.e., universal but subsidy-targeted) basis (see, e.g. Barnett, 2010; Bartik, 2015; Leseman & Slot, 2020, for discussions on the different concepts). They also touch on broader discussions on the role of the government in the child care market (see, e.g., Blau & Currie, 2006) and the question of how far-reaching compulsory education should be, i.e., whether it should also encompass ECEC and ASC (see, e.g. Woodhead & Moss, 2007, for a discussion). Hence, more evidence is needed on how selection mechanisms correspond to treatment effect heterogeneity at different ages and stages of the educational cycle.

Along with the recognition of human capital formation as a multifaceted concept, economists started incorporating concepts from psychology into economic models to measure non-cognitive skills (e.g., Almlund et al., 2011; Heckman, 2006). Non-cognitive skills are linked to personality, social, and behavioral traits.[2] While psychologists have regarded many of these traits, in particular in the area of personality, as relatively stable for a long time, many more recent studies have stressed their malleability over time, especially during childhood (e.g., Brunello & Schlotter, 2011; Peter, 2016). In line with this notion, many studies research the determinants of non-cognitive skill development during childhood. A large share of these studies examine the role of daycare in early childhood (e.g., Baker et al., 2015; Datta Gupta & Simonsen, 2010; Kuehnle & Oberfichtner, 2017; Peter et al., 2016). At an early age, the effects of policy interventions can vary substantially depending on the policy design. While (good quality) daycare is generally believed to be beneficial for non-cognitive development (e.g., Heckman et al., 2013a), there is no linear or unconditional positive link between publicly provided child care and the development of non-cognitive skills.[3] The effects of

[2]The terminology is not used consistently in the current economic literature. Usually, certain traits or scales are used as non-cognitive measures, such as the Big Five personality traits (McCrae & Costa Jr, 2008), grit (Duckworth et al., 2007) and social skills (Deming, 2017). Chapter 2 uses this term interchangeably with the notion of "socio-emotional development", which comprises the Stenghts-and-Difficulties score (Goodman, 1997) besides the Big Five personality traits.

[3]For example, full-day care at a daycare center in comparison to half-day care can have detrimental effects on children's socio-emotional well-being, with this result being driven by children from disadvantaged families (Felfe & Zierow, 2018). Baker et al. (2019) find adverse long-term effects

different policy designs and care intensities on non-cognitive skill formation are better researched for early daycare than for ASC.

Another fundamental discussion among education economists concerns the role of private schools in increasing school choice for parents and students and the implications of this increased competition on educational inequalities. Compared to public schools, private schools generally receive less public funding and operate more on the basis of private tuition fees, which makes them prone to attract primarily high-SES children, unless there are no other regulations trying to lower such incentives. On the one hand, private schools increase the options available to parents and students, creating positive pressure on both private and public schools to innovate and improve (e.g., Sander, 1999; Woessmann, 2007). Ideally, the increased school choice through private schools leads to higher satisfaction on the part of parents and students, and the increased competition may benefit the overall quality of the education system (e.g., Hoxby, 2003). On the other hand, it is often argued that a greater school choice through private schools can also lead to a "white flight", i.e., large parts of economically privileged students sorting to private schools (Clotfelter, 1976). Suppose the private school sector becomes large enough for a critical share of high-SES students opting for private education. In that case, the education market risks approaching a state close to the unregulated one discussed earlier, with its adverse implications for equal access to quality education. Finding the right balance between freedom of choice and equality of opportunity in education is a significant challenge facing policymakers.

Countries have dealt with this challenge very differently globally, leading to heterogeneous institutional and legal settings and complicating cross-country comparisons and research with validity beyond its regional context. In Germany, Article 7(4) of the Basic law prohibits schools from discriminating based on socio-economic status when selecting students. To comply with this law, private schools must either adjust their fees based on the parents' income or set them low enough to be affordable for everyone. However, the interpretation of this rule varies across federal states (e.g., Helbig & Wrase, 2017), leading to different regulations. With the rise in popularity of private schools and primarily high-SES children selecting into these schools (see, e.g., Görlitz et al., 2018; Helbig et al., 2017b; Jungbauer-Gans et al., 2012; Klemm & Zorn, 2017), it is vital to analyze which factors determine these unbalanced participation patterns. It is often assumed that school fees are the deciding factor driving the unbalanced socio-economic composition of the student body in private schools. Although financial

of universal childcare attendance on non-cognitive child outcomes such as self-reported health and life satisfaction among teens in Quebec, with the adverse effects presumably being driven by quality issues (Currie & Almond, 2011).

barriers certainly play a role, disadvantaged households might also be deterred from choosing a private school due to other structural factors. For example, low-SES students can only attend private schools if they are located within a reasonable distance of their homes. Hence, one of these structural factors could be differences in geographical access to private schools. Distance to the nearest educational facility is generally considered an important determinant of its attendance, as shortened distances reduce the time and financial costs of enrollment (e.g., Card, 1993; Dee, 2004; Do, 2004; Spiess & Wrohlich, 2010). A concentration of private schools in high-SES neighborhoods and hence exclusive access to these schools likely leads to increased segregation and more educational inequality. Hence, analyzing the spatial distribution of private schools adds to our understanding of socio-economic differences in the attendance patterns of private schools in Germany.

Besides evaluating explicit formal education measures in their impact on human capital formation and inequality therein, it is vital also to consider less obvious factors determining the quality of the learning environment. Environmental conditions are an aspect of the *nurture* component that affects human capital formation and adds to unequal opportunities. For example, factors like exposure to pollution and extreme weather can have considerable impacts on children's well-being, health, and capacity to study (e.g., Lavy et al., 2014; Marcotte, 2017; Park, 2017). Air pollution has recently been stressed as a key determinant of child development. Besides causing or exacerbating health problems like respiratory diseases (e.g., Chay & Greenstone, 2003; Coneus & Spiess, 2012; Jayachandran, 2009; Knittel et al., 2016; Luechinger, 2014), exposure to air pollution during critical periods of brain development in childhood can have long-term effects on cognitive development, including lower IQ scores and decreased ability to learn and problem-solve (e.g., Aguilar-Gomez et al., 2022). Low-income and marginalized communities are often disproportionately affected by air pollution since they face greater exposure and have fewer resources to address the harmful effects of pollution on their health and education (e.g., Hajat et al., 2015). Hence, public policies aiming at improvements in air quality can be seen as policies that improve children's learning conditions and are likely to affect human capital formation during childhood. This connection shows that human capital-fostering policies can take different forms. It is vital to assess these policies in their whole range of potential socio-economic impacts to leverage synergies between environmental and social policy considerations.

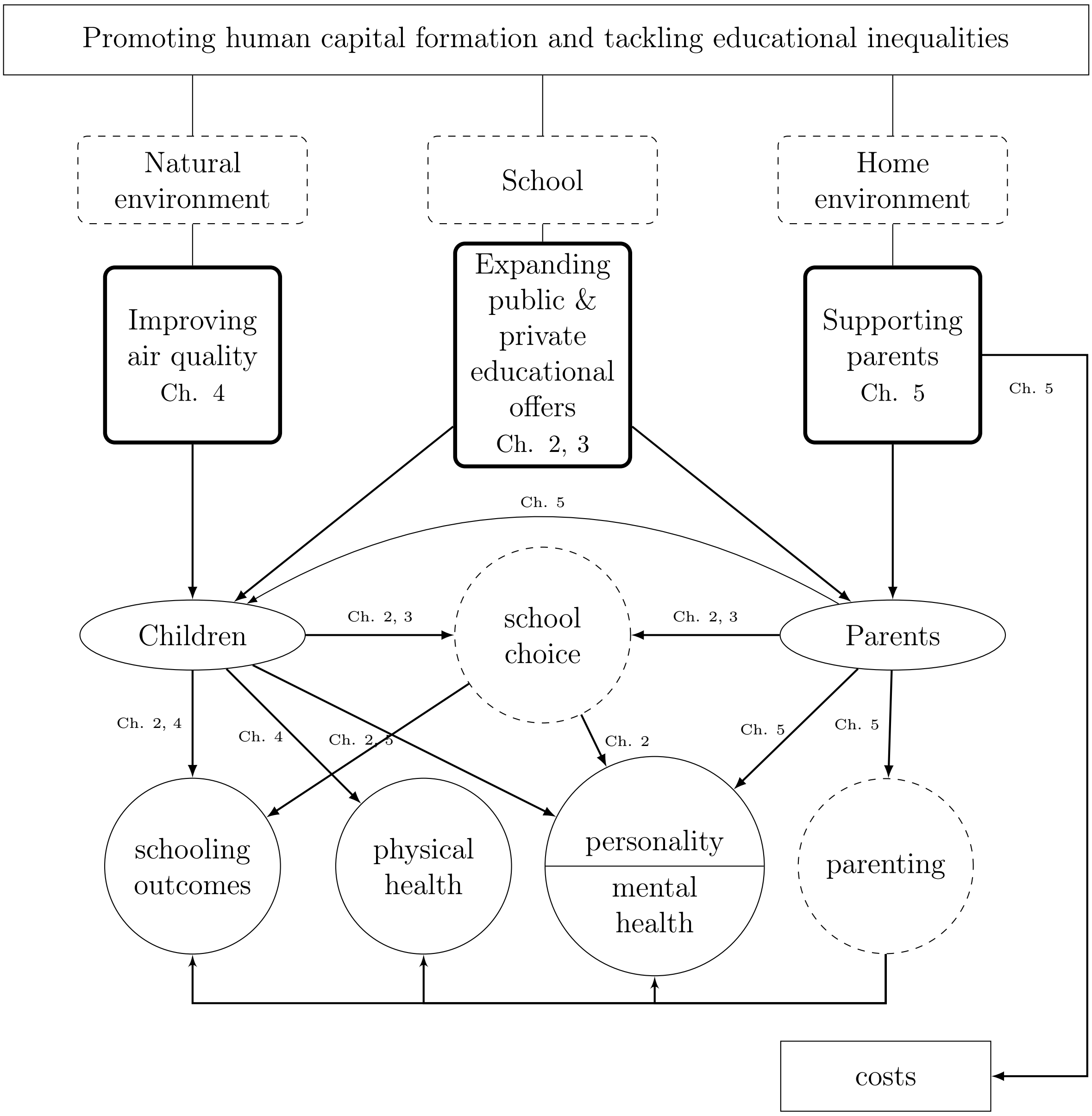
Another way for governments to improve the learning and social environment for infants and children is by offering programs that target parenting skills rather than working directly with children. The home environment is one of the most crucial components of education in the early years. Considering that achievement gaps materialize before children enter formal education or daycare (e.g., Currie & Moretti, 2003; Levine et al., 2020), many traditional policy interventions fail to attack the root cause of socio-economic inequality that forms as early as during pregnancy and the first year of a child's life (e.g., Kalil, 2015). It results from the many differences in parent-child interactions between high- and low-SES parents due to, among other factors, stress, financial constraints, and differences in their own upbringing. Home-visiting programs that aim to support low-SES parents in their role as new parents are one way for governments to level the playing field early on (see, e.g., Cannon et al., 2018). Despite the relevance, evidence on the effectiveness of home-visiting programs beyond the Anglo-American context is scarce, especially in combination with cost analyses. In the face of limited public resources, cost-effectiveness studies are essential tools for policymakers to prioritize among alternative investment options. They can help make investments in particular programs more compelling (e.g., Karoly, 2012; Spieß, 2013). More research is needed on the (cost-)effectiveness of home-visiting schemes in the European and German context (Schmitz et al., 2017).

1.2 Overview and Summary

This dissertation is motivated by and builds upon the strands mentioned above. It comprises four chapters addressing different aspects of human capital formation during childhood and the inequalities therein.

Figure 1.1 illustrates the connection between the different chapters. The dissertation considers three underlying factors that influence the formation of skill development during childhood. They are depicted in the upper part of Figure 1.1: The "natural" environment (i.e., environmental factors like air pollution), the school, and the home environment. The bold-framed squared boxes below depict the specific policy mechanisms under study. The two ellipses *children* and *parents* represent the two generations affected by the policies under study. Finally, the circles on the lower end depict the different outcomes of interest related to human capital formation, schooling outcomes/cognitive development, health, and socio-emotional development, i.e., personality and mental health. The two dashed circles *school choice* and *parenting* represent two intermediary outcomes that in turn affect human capital formation. Finally, besides the outcomes, which represent the *benefits* of the different policy interventions, the dissertation also touches on the *cost* side, depicted in the rectangle on the bottom right side.

Figure 1.1: Connection between chapters



Source: Own illustration.

Key points of each chapter are summarized in Table 1.1, including the research question, main finding, the data sets used, and the methodological approach.

Table 1.1: Overview and summary of the following chapters

	Chapter 2	**Chapter 3**	**Chapter 4**	**Chapter 5**
Title	Heterogeneous effects of after-school care on child development	Socioeconomic composition of the private school student body: the role of the spatial distribution of private schools	From low-emission zone to academic track – Environmental policy effects on educational attainment in elementary school	Costs and short-term effects of a home-visiting program in *BRISE* – first steps for a cost-effectiveness analysis
Research question(s)	What are the heterogeneous effects of attending after-school care (ASC) on elementary school children's schooling outcomes and socio-emotional development?	Can the spatial distribution of private schools in Germany explain the socio-economic composition in these schools?	Do improvements in air quality through low-emission zones (LEZ) affect the schooling outcomes of elementary school children, measured by the transition rates to the academic school track?	What are the short-term costs and effects of a home-visiting program on parental and child outcomes?
Data	SOEP*geo* & administrative school data	SOEP*geo* & administrative school data	Administrative school data, UBA & SOEP*geo*	BRISE
Empirical approach	Marginal treatment effects	Linear probability models, OLS	Two-way fixed effects, stacked-by-event, de Chaisemartin & d'Hautefoeille	Instrumental variable approach combined with entropy balancing
Main finding	Average Treatment effects on the treated's non-cognitive skills are larger than those on the untreated, suggesting that selection into ASC is positive and efficient	Private schools are not systematically located closer to high-SES households but the latter are more "distance sensitive" with regards to choosing a private school	LEZs increase rates of transition to the academic track. Respiratory diseases as one driver through which LEZ improve schooling outcomes.	Statistically insignificant effects across outcomes and generally lower costs compared to other programs.

Notes: SOEP*geo* = geo-referenced data from the German Socio-economic panel (SOEP), UBA = German Environmental Agency (*Umweltbundesamt*), BRISE= Bremen Initiative to Foster Early Childhood Development (*Bremer Initiative zur Stärkung frühkindlicher Entwicklung*).
Source: Own illustration.

In the following, I briefly summarize each Chapter:

The initial two chapters of this paper explore the implications of educational expansions in the public and private school sectors. **Chapter 2** delves into the expansion of after-school care programs in German elementary schools, which arose from a substantial subsidy program in 2003. These programs typically provide homework support and supervised recreational activities and are offered voluntarily, although some may require stricter participation. Despite the widespread belief that institutionalized after-school care can improve opportunities for children who lack adequate homework support at home, it remains unclear whether these programs benefit child development and effectively attract the right children.

My research interests in this paper are twofold: First, I aim to identify which groups of students benefit most from after-school care programs. Second, I examine how the selection process into these programs relates to the treatment effect, that is, whether a universal offer of after-school care attracts the right children. To accomplish these goals, I use a unique combination of school-level data from six Western German federal states between 2003 and 2018, along with student-level data from the German Socio-Economic Panel (SOEP). By analyzing the impact of after-school care attendance on cognitive and non-cognitive aspects of development, such as grades in math and German, the Strengths and Difficulties Score, prosocial behavior, and personality traits, I cover key components of human capital formation. Using a Marginal Treatment Effects framework and taking advantage of regional and temporal variations caused by the subsidy program, I instrument after-school care attendance with the change in the distance to the nearest school offering after-school care within one district. The findings suggest that low-SES children, who are more likely to select into treatment, tend to benefit more from after-school care programs. Furthermore, the average treatment effects on the non-cognitive skills of treated children are greater than those of untreated children, indicating that the selection process for after-school care is effective and efficient. Overall, a universal, voluntary offer of after-school care has the potential to mitigate educational inequalities.

Chapter 3 of this dissertation examines the expansion of the private school sector in Germany. The study is motivated by the marked under-representation of socio-economically disadvantaged children in private schools, despite Article 7(4) of the Basic Law prohibiting discrimination based on socio-economic status in student selection. Private schools must comply with this law by adjusting fees based on parents' income, setting them low enough to be affordable for all, or offering allowances for economically disadvantaged households. Although the interpretation of this rule varies across federal states, school fees alone may not be the sole factor contributing to the imbalanced socio-economic composition in private schools. To gain a more comprehensive understanding of why low-SES children attend private schools less frequently, this study explores the role of the geographical distribution of private schools. We utilize geo-referenced data from the SOEP and address data for all public and private German schools from 2000 to 2019 and estimate linear probability models. The results reveal that high-SES households do not necessarily have a shorter average distance to private schools. However, they are more sensitive to distance when enrolling their children in private schools. Thus, the findings suggest that personal preferences and a lack of information on alternative schools, in addition to school fees, may deter low-SES students from attending private schools. The spatial distribution of private schools plays a secondary role in this regard.

Chapter 4 departs from the school environment and studies the interconnections between environmental policies and human capital. Research has shown that air pollution harms children's health, resulting in respiratory diseases, infant mortality, and impaired cognitive performance leading to behavioral issues. The impact of air quality on school absenteeism and test scores is well documented, but little is known about the long-term educational effects of policies targeting air quality. This paper investigates the causal effect of Low Emission Zones (LEZs) on the academic achievement of elementary school students in Germany. LEZs limit high-emission vehicles from entering designated areas, thereby reducing air pollution and promoting public health. We focus on the transition rates of 4th-grade students to the academic track of the secondary school system, known as the *Gymnasium*, using school-level data from North-Rhine Westphalia, Germany's most populous federal state, and exploit the staggered adoption of LEZs since 2008 in a difference-in-differences framework. Our results suggest that LEZs increased transition rates to the academic track by 0.9-1.6 percentage points. Using geo-referenced data from the SOEP, we also provide suggestive evidence that a decrease in respiratory infections is a crucial channel through which LEZs affect educational outcomes.

Finally, **Chapter 5** delves into the first-year home environment for children, emphasizing the potential of home-visiting programs in promoting child and family well-being, especially for disadvantaged families. However, little evidence exists on the effectiveness and cost-efficiency of such programs in the European context. This study presents new evidence on the costs and effects of the Bremen Initiative to Foster Early Childhood Development's (BRISE) *Pro Kind* home-visiting program. BRISE uses a randomized information and access treatment on a neighborhood level to encourage participation in *Pro Kind*. Through an instrumental variables framework and entropy balancing, we estimate the causal effects of *Pro Kind* on several mother and child outcomes within the first seven months of the children's lives and provide cost estimates based on self-collected cost data. Although we cannot deduce significant causal effects of *Pro Kind* on child and maternal outcomes due to data limitations, our cost analysis indicates that *Pro Kind* is less costly than comparable early childhood programs. Our study provides a foundation for future cost-effectiveness and cost-benefit studies, critical in making well-informed policy decisions regarding resource allocation.

1.3 Common Themes and Contributions

Each chapter in this dissertation makes individual contributions to the economics of education literature, which are discussed in detail in the respective introductions. Yet, some chapters share themes that are prevalent throughout. This section identifies a few noteworthy contributions that are either content-related or methodological in nature.

First, all chapters feed into *topical policy discussions*. Many countries are currently expanding their daycare offers, including ASC. After investing heavily in the expansion of ASC slots between 2003 and 2009, raising the share of primary schools offering ASC to over 70 percent in 2020 (KMK, 2021), the German government announced in 2021 that it would invest a further 3.5 billion to grant a legal entitlement to all-day care for children of primary school age from 2026 onwards (BMBF, 2021). Hence, my findings on the heterogeneous effects of ASC in **Chapter 2** and their implications regarding the best organizational form are directly relevant for current policy discussions in Germany and beyond. **Chapter 3** also caters to current discussions in media and academia around the growing popularity of private schools and its implications for educational inequality in Germany. While it is often assumed that school fees are the deciding factor driving the unbalanced socio-economic composition of the student body in private schools, the paper sheds light on other, often overlooked factors, in particular, the role of the spatial distribution of private schools and behavioral aspects of parents. Following the broad evidence on the effectiveness of interventions in early childhood, many OECD countries have implemented publicly funded daycare schemes for toddlers and preschool children. However, home-visiting programs supporting parents during the first year(s) after childbirth are not yet offered in a comprehensive fashion. Evidence from smaller scale programs like BRISE, subject of **Chapter 5**, can create momentum for the importance of such programs to complement daycare schemes in a trans-regional fashion. Finally, **Chapter 4** contributes to the topical challenge of the social-ecological transformation, i.e., adopting our economy, energy, and traffic policies towards a green transition taking into account social ramifications. In the public discourse, progressive environmental policies are often portrayed as socially unjust. In certain scenarios, the distributive impact of green-transition policies may indeed necessitate the implementation of social policy measures to mitigate potential adverse effects. However, our analysis demonstrates that social and ecological objectives can synergistically reinforce one another in other situations.

My second general contribution is of *methodological* nature. The chapters encompass a wide array of empirical methodologies, from descriptive analyses and linear probability models to quasi-experimental and experimental designs. By showcasing diverse approaches, these chapters illustrate how the distinct research questions and contextual factors require tailored empirical strategies. Three out of four chapters use (quasi)-experimental designs and state-of-the-art econometric methods to identify the causal effects of different policies on human capital formation. **Chapter 2** contributes to the scarce body of literature employing Marginal Treatment Effects (MTE) to identify policy-relevant treatment effect heterogeneity in educational interventions. The MTE literature in the field of education counts notable contributions for both very young children (Cornelissen et al., 2018; Felfe & Lalive, 2018) and young adults (e.g., Carneiro & Ginja, 2016; Carneiro et al., 2017; Kaufmann, 2014), leaving an "age gap" in our understanding of how selection into educational offers relate to treatment effect heterogeneity. The analyses in **Chapter 4** feed into current discussions among applied econometricians (e.g., de Chaisemartin & D'Haultfœuille, 2020a; Goodman-Bacon, 2021; Wooldridge, 2021) on the validity of two-way-fixed-effects (TWFE) estimations in a setting where the treatment implementation is staggered. The main concern is that comparisons of "switchers" and "non-switchers" include potentially problematic comparisons (comparing later treated to earlier treated units), which may lead to negative weights in the weighted average (Goodman-Bacon, 2021). We apply two newly proposed estimators that evade the negative-weights problem; the stacked event-by-event design (Baker et al., 2022; Cengiz et al., 2019; Deshpande & Li, 2019) and the estimator suggested by de Chaisemartin & D'Haultfœuille (2020a). Hence, our analysis is informed by state-of-the-art advances in the econometric literature.

Another notable contribution of this dissertation is its utilization and integration of *various data sources*. The studies draw on survey and administrative panel data at the individual, school, and district levels. Each data source has unique analytical advantages as well as drawbacks. The major strength of survey data sets like the SOEP is that they provide comprehensive information on various aspects of individuals' lives over an extended period, enabling critical heterogeneity analyses. In the case of the SOEP, it is possible to use (anonymized) geo-referenced information on the location of the households and link it to other data sources. **Chapters 2 and 3** use this possibility by geo-matching the SOEP with school address data, thereby creating new variables in the SOEP, like the distance to the nearest primary school offering ASC- and the nearest private primary school. However, due to the high costs involved in collecting and maintaining such rich data, survey data usually covers fewer individuals, which may lead to less precise or less robust estimates of reform effects. This trade-off becomes even more apparent in **Chapter 5**, which employs experimental data from the BRISE project in Bremen. While the BRISE sample analyzed in the paper features only a relatively small number of children (300), the close and frequent monitoring of these children and their families provides a wealth of information exceeding typical surveys. Administrative data, on the other hand, such as the secondary-school transition rates and pollution data used in **Chapter 4**, boasts an extensive sample size, producing more accurate and reliable estimates of reform effects. As administrative bodies typically generate this data, it tends to be of high quality and less susceptible to biases associated with misreporting. However, administrative data usually does not include information beyond the scope of its intended use, like information on socio-economic characteristics. To fully leverage the strengths of individual data sources while mitigating their limitations, this dissertation employs multiple data sources for its analyses.

The fourth contribution of this dissertation is that it includes *concepts from other disciplines* like psychology, sociology, and environmental studies and thus contains inter-disciplinary aspects. Many of the pressing issues facing society today are complex and cannot be effectively addressed through a single disciplinary lens. Inter-disciplinary research brings together expertise from different fields to understand better and tackle these complex problems. **Chapter 3**, coauthored by a sociologist, draws on arguably more straightforward empirical methods than the other chapters but benefits from an extensive institutional knowledge of the German private school system. The BRISE project, subject of **Chapter 5**, is run by a consortium of researchers from different fields spanning psychology, educational sciences, and economics, hence generating a multi-disciplinary data set covering a vast range of developmental parameters. In addition, this dissertation highlights the multifaceted nature of human capital formation by measuring cognitive, non-cognitive as well as health outcomes (Table 1.1), which have partly been developed by other disciplines. The dissertation also spans different policy areas. For example, by analyzing the environmental policy effects on educational achievement in elementary school, **Chapter 4** demonstrates that policies pursuing social and ecological goals are often complementary.

Furthermore, this dissertation emphasizes the significance of accounting for *heterogeneous treatment effects* as well as the underlying mechanisms when assessing the causal effects of a policy on human capital formation. Identifying these heterogeneities can unveil not only which individuals benefit most from policies but also whether selection into treatment is efficient (**Chapter 2**). Understanding these consequences is vital for policymakers to evaluate the overall effectiveness of a program and how it could be altered to reach the intended individuals better. While it may not be methodologically feasible to isolate a single mechanism that drives the results using only one source of exogenous variation (as demonstrated by Frölich & Huber, 2017), **Chapter 4** provides suggestive evidence on potential mechanisms by utilizing additional data sets and outcomes.

Sixth, while touching on several stages of human capital formation, most chapters *focus on (early) childhood* and the elementary school years. According to Heckman and coauthors' work on the technology of skill formation, the first years of a person's life are the most formative because this is when the brain is developing rapidly and is most responsive to policy interventions (e.g., Cunha & Heckman, 2007). **Chapter 5** contributes to the growing literature on the effects of early childhood interventions on human capital formation by evaluating the effects and costs of a German home-visiting program during the first seven months of children's lives. **Chapters 2 and 3** both focus on primary school children. The elementary school years are a crucial time for human capital formation because they provide the foundation for a child's intellectual, social, and emotional development. During these early years, children are introduced to a wide range of academic subjects, including reading, writing, math, science, and social studies. They also learn important social and emotional skills, such as cooperation, empathy, self-regulation, and conflict resolution (e.g., Kosse et al., 2020). Furthermore, besides marking the beginning of a child's school career, performance during this period builds the basis for tracking into different secondary school types after fourth grade in Germany —hence determining a child's educational and professional trajectory in important ways (e.g., Dustmann, 2004; Dustmann et al., 2017). Thus, the students at the center of these chapters are in a critical and highly malleable development phase.

Finally, all chapters of this dissertation focus on the *German context*, albeit addressing questions that should also interest other (industrialized) countries. Germany serves as the site for all policy interventions subject to the different chapters, constituting a valuable departure from the dominance of US-centric literature on human capital formation in economics. Compared to the US, Germany offers a favorable institutional framework with comprehensive social security, education, and health services at reduced private costs. Nevertheless, despite these provisions, a strong correlation persists between a child's human capital and their family background (e.g., Braun & Stuhler, 2018; Waldinger, 2007; Weis et al., 2018). Still recovering from the "PISA shock" in 2000, which famously revealed a significant achievement gap between students from high and low socio-economic backgrounds, inequality in education is rising again in Germany (OECD, 2019; Reiss et al., 2019). The country has experienced significant levels of immigration in recent years, particularly in the wake of the refugee crisis. This has placed additional demands on the education system, as many of the children of immigrants require specialized language support and additional resources. At the same time, Germany is currently facing a shortage of teachers and educators, with many teaching and childcare positions unfilled in many regions of the country. Therefore, evidence on the effectiveness of past policies lends essential insights for the design of future policies that seek to address these challenges. Regarding the contribution to the international literature, gathering new evidence from Germany's distinct institutional environment provides valuable insights. Examining the impact of policies on human capital formation requires collecting reliable empirical data across diverse institutional contexts, which enables the identification of universal patterns that can inform policymaking worldwide.

CHAPTER 2

Heterogeneous Effects of After-School Care on Child Development[1]

2.1 Introduction

After-school care (ASC) programs are a central element in the attempts of many OECD countries to meet the increased demand for institutionalized child care while simultaneously fostering children's cognitive and social development (OECD, 2017b). A particular hope of ASC lies in its potential equalizing effect: Children with low socioeconomic status (SES) tend to experience lower-quality homework support at home (e.g., Buckingham et al., 2013; van Bergen et al., 2017) and hence are likely to benefit the most from spending additional time in school and afternoon care (e.g., Angrist et al., 2010; Blau & Currie, 2006; Levine & Zimmerman, 2010; Plantenga & Remery, 2015). Following these considerations, the German government heavily subsidized the expansion of ASC in elementary schools after 2003 (BMBF, 2009). Despite immense policy interest, evidence on the causal effects of universal afternoon programs on elementary school children is scarce.

This paper studies the heterogeneous effects of ASC on elementary school child development. My main interest is to understand how the selection into afternoon programs relates to the treatment effect and whether a universal offer of ASC reaches

[1]This paper benefited greatly from comments by C. Katharina Spieß, Felix Weinhardt, Matthias Westphal, Sönke Matthewes, Jan Stuhler, Jan Marcus, Mara Barschkett, Jonas Jessen, and Mathias Huebener, as well as participants of the IWAEE Conference 2022, EffEE Conference 2022, UCL SEHO Conference 2022, LEER Conference 2022, CRC Conference 2021, CIDER Conference 2021, the BeNA summer workshop 2021 and the DIW Graduate Center (GC) summer workshop 2021 for valuable comments on drafts and presentations. I gratefully acknowledge financial support from the Leibniz SAW Project on "Improving School Admissions for Diversity and Better Learning Outcomes" (*iLearn*), the DIW GC and BERA through Jan Marcus, as well as technical support by the team of the SOEP infrastructure on using the geo-referenced SOEP data.

the right children. I use a unique combination of self-collected school-level data from six Western German federal states between 2003 and 2018 with student-level data from the German Socio-Economic Panel (SOEP). By analyzing effects on grades in math and German, the Strengths and Difficulties Score (SDQ), prosocial behavior, and the Big Five personality traits, I cover key aspects of cognitive and non-cognitive development.

I employ a Marginal Treatment Effects (MTE) framework (Björklund & Moffitt, 1987; Heckman et al., 2006a; Heckman & Vytlacil, 1999, 2005), which is uniquely suitable for evaluating a policy not only in its efficacy and equity but also in the efficiency of the selection mechanism at play – especially in the presence of unobserved self-selection (e.g., Carneiro et al., 2011; Cornelissen et al., 2018; Felfe & Lalive, 2018). So far, this method has been applied to early and pre-school daycare (Cornelissen et al., 2018; Felfe & Lalive, 2018) and higher education (e.g., Carneiro et al., 2011, 2017; Kamhöfer et al., 2019; Kaufmann, 2014; Redmond, 2014) but not to elementary school children, leaving an "age gap" in our understanding of how selection into educational offers relate to treatment effects.

I make use of arguably exogenous variation in available ASC slots caused by a large reform in Germany that led to an increase in the proportion of primary school children in Germany participating in afternoon care activities at school from roughly ten to close to 70 percent since 2003 (KMK, 2021). Since elementary school children in Germany usually attend their local catchment area school, it is unlikely that they sort into schools based on whether or not they offer ASC. The afternoon care at school consists mostly of homework support and supervised recreational activities; it does not include an increase in instruction time.[2] While elementary school itself is compulsory in Germany, ASC in most cases is organized as non-integrated daycare, where participation is voluntary.[3] The country-wide reform was staggered across the federal states and generated regional and time variation in the availability of publicly available ASC slots, hence creating a promising institutional setting for estimating MTEs. The combination of the SOEP and the administrative school-level data allows me to both observe yearly individual ASC attendance and retrieve the distance to the nearest ASC from the students' home. Controlling for survey year and district fixed effects, the change in the distance over time within the same district builds the continuous instrument needed to estimate MTEs.

[2] In some cases, in the integrated ASC type, instruction time is shifted from the morning to the afternoon. However, this shift does not lead to an increase in the absolute instruction time.

[3] There are different organizational types ranging from compulsory to voluntary programs, with the latter being the most prominent kind. In my sample, 75 percent of ASC programs are non-integrated, 19 percent are partly integrated (participation being mandatory on some days of the week), and six percent are integrated (participation is compulsory on all days).

My study design offers several advantages: First, I use variation in the instrument not only cross-sectionally but using panel data over 16 years. Hence, I use variation in the access to ASC across time and space, which allows me to control for time-constant unobserved district characteristics. In addition, using survey data representative of more than 70 percent of the German elementary school-age population[4] rather than focusing on a narrow geographical area, my results likely have strong external validity and are informative for educational policies in Germany and beyond. Finally, I observe a variety of outcome and control variables, allowing me to draw a comprehensive picture of the effects of afternoon programs on child development. On a negative note, the relatively small sample size limits the precision of my estimates.

I find substantial heterogeneity in returns to ASC with respect to both observed and unobserved characteristics. Low-SES children[5] are more likely to attend ASC and tend to experience higher returns in terms of (non-)cognitive development, which points to a positive selection based on observed characteristics. The selection of unobserved characteristics reinforces this finding since children with lower resistance to attending ASC are more likely to benefit from afternoon care. For most of my outcomes in the area of non-cognitive skills, the MTE curve indicates a higher treatment effect for treated individuals than for non-treated. For these outcomes, the average treatment effect (ATE) is either not statistically different from zero or negative, whereas the average treatment effect on the treated (ATT) is positive in all outcomes and statistically significant for prosociality, the SDQ, openness, extroversion and emotional stability. Hence, while ASC does not benefit everyone, it seems to have beneficial effects on those who select into them, especially in terms of outcomes that broadly categorize as social skills.

My paper contributes to different strands of the literature. First, it adds to our understanding of the effects of afternoon supervision of school-age children (e.g., Aizer, 2004; Blau & Currie, 2006; Felfe & Zierow, 2014; Seidlitz & Zierow, 2020). Most literature in this area focuses either on targeted programs (Blau & Currie, 2006) or on non-specified adult supervision (Aizer, 2004). To my knowledge, Felfe & Zierow (2014) and Seidlitz & Zierow (2020) are the only two economic studies evaluating the impact of

[4]I cover the six Western German federal states of North Rhine-Westphalia, Baden-Wuerttemberg, Bavaria, Hesse, Rhineland Palatinate and Lower Saxony, which together make up 73.8 percent of the German population. Therefore, the 4,002 students in my SOEP sample are roughly representative of 73 percent of the German population of elementary school children

[5]Specifically, I find that children of social transfer receiving and single-parent households more often select into ASC. Since all of these characteristics correlate with lower SES, I broadly characterize selection into ASC this way.

universal after-school center-based care on children,[6] also using variation caused by the German reform. Their results are inconclusive regarding the global and heterogeneous effects of ASC.[7] I add to these findings by revealing important heterogeneity patterns with highly relevant policy implications.

Second, my study adds to our knowledge on personality and non-cognitive skill (NCS) formation (e.g., Brunello & Schlotter, 2011; Deming, 2017; Fletcher, 2013; Heckman et al., 2006b; Kautz et al., 2014) pointing to childhood as the most critical investment period for developing socio-emotional skills (see e.g., Cunha & Heckman, 2007; Kautz et al., 2014; Kosse et al., 2020). In early ages, the effects of policy interventions can vary substantially depending on the policy design. While (good quality) daycare is generally believed to be beneficial for non-cognitive development (e.g., Heckman et al., 2013a)), there is no linear or unconditional positive link. The effects of different policy designs and care intensities on NCS are better researched for early daycare than for school-age daycare.[8] I add to this literature by examining the effects of full-time care during the first years of compulsory schooling on the formation of personality (the Big Five), social skills (prosocial behavior), and socio-emotional development (the SDQ).

Finally, I add to our understanding of how selection into educational offers relates to treatment effects at different educational stages. I contribute to the growing body of literature employing MTEs to identify policy-relevant treatment effect heterogeneity in educational interventions. The MTE literature in the field of education counts notable contributions for both very young children and for young adults, revealing a puzzle on selection patterns into voluntary educational offers: On the one hand, Cornelissen et al. (2018) and Felfe & Lalive (2018), looking at early daycare at the age of three to six and below three, respectively, find that children from disadvantaged backgrounds

[6] So far, economic studies focus mainly on the effects of the expansion of ASC in Germany on maternal labor supply, e.g. Gambaro et al. (2019), Dehos & Paul (2021), Nemitz (2015)). Several studies evaluate the reform in terms of their non-causal effects on students, e.g. the StEG study on the development of all-day schools StEG Konsortium (2016), Lossen et al. (2016) and Sauerwein et al. (2019)).

[7] Felfe & Zierow (2014), using a value-added approach, find no significant effects on average but positive effects on children of less-educated mothers and low-income families in terms of their socio-emotional development. In contrast, Seidlitz & Zierow (2020), employing an IV approach with treatment defined on the school level, find a positive overall effect on language and math skills as well as on the probability of being recommended for the academic track – but no evidence for an equalizing effect of ASC.

[8] For example, full-day care at a daycare center in comparison to half-day care can have detrimental effects on children's socio-emotional well-being, with this result being driven by children from disadvantaged families (Felfe & Zierow, 2018). Baker et al. (2019) find negative long-term effects of universal childcare attendance on non-cognitive child outcomes such as self-reported health and life satisfaction among teens in Quebec, with the negative effects presumably being driven by quality issues (Currie & Almond, 2011). Early entry into childcare seems to exhibit no effects in the short term (Kuehnle & Oberfichtner, 2020) but is shown to increase extroversion in adolescence (Bach et al., 2019).

are less likely to attend but have larger positive treatment effects in terms of school readiness (Cornelissen et al., 2018) and socio-emotional skills (Felfe & Lalive, 2018) – with this pattern of reverse selection being reinforced by selection on unobserved characteristics.[9] In contrast, MTEs estimated for non-compulsory higher education (ages 16 and older) suggest a positive selection pattern – implying that individuals with a higher gain from treatment are more likely to attend (e.g. Carneiro et al. (2011), Redmond (2014), Kaufmann (2014), Kamhöfer et al. (2019), Westphal et al. (2020) for tertiary education, and Carneiro et al. (2017) for higher secondary education).[10] These contrasting findings raise the question of when the negative selection observed for early daycare turns into the positive selection found for higher education. This paper helps to fill this research gap by estimating MTEs for the expansion of ASC in German elementary schools.

The elementary school years are a particularly relevant period for several reasons: The effectiveness of (high quality) early childhood and elementary school interventions on improving later-life outcomes is well documented (e.g., Cunha & Heckman, 2007; Kautz et al., 2014). It is also shown that elementary school is a particularly sensitive period for the development of motivations, beliefs, and behaviors (e.g., Kosse et al., 2020). Furthermore, besides marking the beginning of a child's school career, performance during this period builds the basis for tracking into different secondary school types after fourth grade in Germany – hence determining a child's educational and professional trajectory in important ways (e.g., Dustmann, 2004; Dustmann et al., 2017). Thus, the students at the center of my study are in a critical and highly malleable phase of their development.

My findings have two important policy implications: First, spending more time in school does indeed seem to benefit low-SES students and, hence, the universal offer of ASC slots can serve as a tool to increase equality of opportunity. Second, with different organizational types of ASC currently co-existing in Germany and beyond, these results make a strong case for organizing ASC in a non-integrated way, i.e. offering afternoon slots on a voluntary basis instead of making participation mandatory for all pupils. However, the current dynamic in Germany – primarily low-SES students taking up these offers – bears the risk of increased segregation, where afternoon programs might become increasingly less attractive for high-SES students. Hence, investments in the

[9] The two papers, however, come to different conclusions regarding the effects of child care on motor skills: while Cornelissen et al. (2018) find no statistically significant heterogeneity pattern with respect to this outcome, Felfe & Lalive (2018) finds a positive selection pattern here, i.e. children who more readily select into child care benefit more. This divergence could stem from the differences in the observed age groups and/or regional differences.

[10] Nybom (2017) also finds significant self-selection into college but primarily on observed characteristics.

quality of ASC programs should ensure that they become more attractive for children from different SES backgrounds.

The remainder of this paper is structured as follows. In the next section, I present the institutional setting and characterize the underlying mechanisms. In Section 2.3 , I present the research design. Section 2.4 is dedicated to describing the data and assessing the validity of my instrument. Next, Section 2.5 presents and discusses my results. Section 2.6 concludes.

2.2 Institutional background and mechanisms

2.2.1 Elementary schools in Germany and the IZBB reform

Since education policy in Germany is decentralized and regulated by the federal states, some aspects of the primary education system vary across states. Compulsory primary education starts when children are around six years old and, in most cases, lasts for four years.[11] Hence I focus on children between the age of six/seven and ten/eleven. Based on their performance in fourth grade, children are then divided into three secondary school tracks: basic track (five years), middle track (six years), and higher track (eight to nine years), with only the latter granting access to universities. The recommendation is given by the headteacher but, in most cases, this recommendation is not strictly binding. The rules again differ by the federal states.[12] Once assigned to a track, mobility across tracks is rare, with upward mobility, i.e. moving from the lower to the higher track, being especially difficult (e.g., Bellenberg, 2012; Dustmann, 2004; Dustmann et al., 2017). Hence, performance in primary schools greatly affects the children's future educational and professional paths.

Primary schools in Germany were traditionally designed as half-day schools, i.e., they started at 8 a.m. and ended before 1 p.m. While institutionalized afternoon care existed in the form of *Horts*, their offer and take-up significantly differed across rural and urban areas as well as Eastern and Western Germany. In Western Germany, merely six percent of 6- to 9-year-old children attended after-school care in 2002, compared

[11]This is the case for all six federal states that I focus on in my analysis: North Rhine-Westphalia, Bavaria, Baden-Wuerttemberg, Hesse, Lower Saxony, and Rhineland-Palatinate.

[12]In North Rhine-Westphalia between 2006 and 2010 and in Bavaria for the whole observation period, the teacher's recommendation was binding. Children whose parents disagreed with the recommendation had the opportunity to attend three-day *trial lessons* after which they had to pass exams in German and mathematics with certain grades (Bavarian Ministry for Education and Cultural Affairs, 2010), Ministry of Education North Rhine-Westphalia (2012). While, in most states in my sample, the teacher's recommendation is not binding, it is usually the case that parents follow the teacher's recommendation (Bos, 2003).

to around 60 percent in Eastern German states (DJI, 2008). My analysis shows that 30 percent were cared for by family members or family friends, and two-thirds of the children were taken care of by their parents, typically mothers.[13] The stark differences in afternoon care for school-age children pre-2003 questions a joint analysis of Western and Eastern Germany. Therefore, this paper focuses only on Western German federal states.

Germany began reforming the half-day elementary school system in the early 2000s. The "Future of Education and Care program" (*Investitionsprogramm Zukunft Bildung und Betreuung*, IZBB) was the main reaction to Germany's "PISA Shock" in 2001 and subsidized the expansion of ASC with 4 billion euros between 2003 and 2009.[14] More than half of these funds were spent on elementary schools (BMBF, 2009).[15][16] As a consequence of the IZBB program, the number of primary schools running afternoon programs grew significantly. In the 2018/19 school year, 67.5 percent of all primary schools in Germany offered ASC (KMK, 2018). The majority (65.8 percent) of primary ASC programs are organized in a non-integrated way (*offene Ganztagsschule*), where participation in lunch and afternoon programs is voluntary. The strictest form of ASC is the integrated type (*gebundene Ganztagsschule*), where every student attending the respective elementary school is obliged to participate in these afternoon activities.[17] In my sample, 75 percent of ASC programs are non-integrated, 19 percent are partly integrated, and six percent are integrated. Parents can apply for the afternoon program for their children each year, paying a moderate fee that covers the lunch costs. Including the afternoon program, the median time of supervision amounts to 8.5 hours per weekday (Fischer et al., 2013) compared to the usual average school day of 4.5 hours. Figure A.1 maps the geographical expansion of ASC in my sample.

ASC in Germany consists to a large extent of lunch, homework support, and supervised recreational activities. In some cases, the integrated ASC type includes instruc-

[13]Numbers based on own calculations using the Socio-economic Panel v35.

[14]When Germany participated in the the OECD's Programme for International Student Assessment (PISA) for the first time in 2000, German schools were ranked below the OECD average. Besides the overall disappointing performance, Germany also stood out for a high level of educational inequality measured by a strong link between achievement and family background. As a second objective, the reform pursued a better reconciliation of work and family life (e.g., Dehos & Paul, 2021).

[15]The invested funds, however, were to be spent on construction purposes only. Hence, the states had to cover running costs such as personnel costs, which, varying with the intensity of the program, are estimated to range between 992 and 1981 Euros per child per year (Klemm & Zorn, 2017).

[16]The expansion of the ASC sector continued beyond the end of the IZBB investment program, albeit at a slower pace. In spring 2021, the Federal Government announced it would invest a further 3.5 billion to grant a legal entitlement to all-day care for children of primary school age from 2026 onwards (BMBF, 2021).

[17]A third type is the semi-integrated ASC program (*teilgebundene Ganztagsschule*), where attendance in the afternoon is obligatory either only on certain days of the week or only for certain classes within the school.

tion time in the afternoon. However, in these cases, instruction time is shifted from the morning to the afternoon rather than added to the standard curriculum. Hence students attending ASC do not receive more instruction than their peers. Quality standards are defined by the federal states and are not uniform for all of (West-)Germany. Around three-quarters of the staff in afternoon care have a non-university pedagogical formation over three years or hold a university diploma in social pedagogy. In Bavaria and Lower Saxony, the share of staff with only a short-term formation in child care (less than three years) is relatively high, at 26 and 15 percent, respectively. The average children-per-staff ratio is similar in all six federal states in my sample, ranging between 9 and 10 (Felfe & Zierow, 2014).

2.2.2 Mechanisms

The nexus between selection into ASC and its heterogeneous effects on student's (non-)cognitive development is mainly determined by two factors: (I) Who benefits from the substitution of home care with institutionalized care, and (II) who is more likely to participate in terms of observed and unobserved characteristics. If the first and second group coincide, selection into ASC is positive and efficient. If further, this group consists predominantly of disadvantaged students, ASC can be said to have an equalizing effect.

Regarding (I), the estimated effects of the ASC expansion should be evaluated as the result of substituting home-based care with institutionalized care. The former differs from the latter in three main aspects: spending more time with a relatively large group of peers, the presence of at least one adult with pedagogical training, and a predetermined program of supervised recreational activities (Felfe & Zierow, 2014). Increased peer interactions could be beneficial for children's socio-emotional development.[18] Due to the vital link between non-cognitive and cognitive skills development (e.g., Cunha & Heckman, 2007), a positive effect on schooling outcomes is also plausible. In addition, more peer time could be beneficial for the language skills of children of immigrant ancestry (Washington-Nortey et al., 2020). On the other hand, considering self-control as a scarce resource (Baumeister & Alquist, 2009), adverse effects of ASC attendance on non-cognitive development are possible, especially for children with a predisposition for conduct or attention difficulties. Because of the large group size and the often short training period of the ASC staff, it is unclear whether the second point, the presence of a trained pedagogue, has a significant impact on an average child attending an after-

[18]For example, prosocial behavior is found to be highly malleable at elementary school age, with social interactions being one of the main drivers (Kosse et al., 2020).

noon program, especially on children with special needs. However, for children who do not receive (high-quality) homework support at home, the offered support could make a substantive difference (Felfe & Zierow, 2014). Hence, heterogeneity in the effects of ASC is likely driven by differences in the counterfactual care mode. In the same fashion, the offer of regular supervised sports, cultural events, and other educational activities plausibly makes a larger difference for low-SES children since they have fewer resources and less often take up these offers outside of school (e.g., Hille & Schupp, 2015; Whitaker et al., 2019).

Regarding (II), as shown by Marcus et al. (2016), selection into ASC is complex – albeit, for Western Germany, it is more clearly low-SES children that are selecting into ASC. In addition to selection based on levels, the treatment decision is likely affected by the expected idiosyncratic return to treatment, causing selection based on gains: children who choose treatment because they have a low resistance to it might have different gains than those with high resistance. For example, more motivated children may voluntarily stay in school in the afternoon. This would likely result in a positive selection based on the latent propensity to attend (similar to the MTE literature on higher education). On the other hand, selection based on gains could be induced by persons other than the children themselves, e.g., their parents and teachers. Parents could decide to send their kids to ASC due to their unobserved preference for working full time. Suppose this mechanism is the dominant one at a place. In that case, there could be a reverse selection similar to that identified for early daycare by Cornelissen et al. (2018) and Felfe & Lalive (2018) since full employment is associated with a high SES and these children might benefit less from ASC because of the high-quality support they would otherwise receive at home. Further, it is possible that teachers specifically target low-SES students for ASC by convincing the parents to register because they think that these children would benefit most from the extra homework support. This would add to a positive selection pattern if low-SES students benefit more from the services offered by ASC, as hypothesized in (I).

While ASC quality differs and is not ideal, the environment encountered at ASC is much more homogeneous than the counterfactual environment at home. Hence, ASC likely has an equalizing effect, with differences in effect size driven predominantly by differences in the counterfactual care mode (Felfe & Zierow, 2014). In addition, since low-SES children in Western Germany have a higher probability of selecting into ASC, a positive selection mechanism seems to be in place. Whether this pattern is reinforced by a positive selection based on unobserved characteristics is to be determined.

2.3 Research design

2.3.1 Model

I study the effect of after-school care on elementary school students' skill development. In a simple OLS setting, the model would look like this:

$$Y_i = ASC_i\beta_1 + X_i\beta_2 + \epsilon_i \tag{2.1}$$

where Y_{ji} is the outcome of interest for individual i, ASC_i is the treatment dummy, which takes value one if the individual attended an afternoon program for most of their elementary schooling,[19] X_i is a vector of individual and household covariates, and ϵ_{ij} is the error term.

The selection mechanisms described in the last chapter pose a critical problem for identifying β_1, my parameter of interest since individuals who select themselves into treatment differ from the control group in systematic ways that affect the outcome Y_{ji}. Instrumental variable (IV) techniques can solve selection on levels by estimating local average treatment effects (LATE) for instrument compliers. However, in most settings, selection mechanisms will not solely be based on observed characteristics but also on what the individuals expect to be their gain from treatment, i.e., on a certain degree of "resistance to treatment" unobserved by the researcher (Andresen, 2018; Cornelissen et al., 2016; Zhou & Xie, 2019). The Marginal Treatment Effect (MTE) framework developed by Heckman & Vytlacil (1999, 2005, 2001) allows for studying heterogeneous treatment effects in the presence of self-selection. MTEs identify the average treatment effects (ATE) for people with particular resistance to treatment, allowing to recover economically relevant parameters, like the average treatment effects on the treated (ATT) and the average treatment effect on the untreated (ATUT), given full instrument support (Andresen, 2018).

In the baseline model, let Y_{1i} denote the outcome of student i in the case of treatment, i.e., attending afternoon programs for most of elementary school ($D_i = 1$), Y_{0i} the outcome of student i if he or she attends elementary school only half-day ($D_i = 0$), X_i a vector of observed child and household characteristics, while R_i, T_i and C_i are districts, survey year and birth year fixed effects, respectively:

[19] This variable is derived by: $D_i = mode_i(ASC_{it})$ and is equivalent to at least two years for most students in my sample, and to at least one year when I observe a child for only two years during elementary school, as is the case for a part of my sample in the latest years (2017-2018)

$$Y_{ji} = X_i\beta_j + R_i\alpha + T_i\delta + C_i\gamma + U_{ji}, \quad j = 0, 1 \tag{2.2}$$

I use the following latent index model for selection into treatment D_i:

$$D_i^* = Z_i\beta_d - V_i \tag{2.3}$$

$$D_i = \begin{cases} 1 & \text{if } D_i^* \geq 0, \\ 0 & \text{otherwise.} \end{cases}$$

where Z = (X, R, T, C, Z^*) is equivalent to X in equation (2.2) but additionally contains an instrument Z^*. I rely on spatial and temporal variation in the offer of ASC slots caused by the IZBB reform by using the change in the distance to the next ASC offering school within one district as an instrument for ASC attendance (see section 2.4.2). The term V_i represents the “unobserved resistance” to the treatment of individual i, capturing all unobserved characteristics that lower the probability of attending ASC (Cornelissen et al., 2016).

The treatment effect Y_{1i}-Y_{0i} may vary among students with different observed characteristics X as well as among those with different values of the unobserved components U_1 and U_0 – who may have the same characteristics X. In order to trace the dependence between the treatment effect and the unobserved component of the treatment choice, I rely on the quantiles of the distribution of V, as is common in the MTE literature:

$$Z\beta_d - V \geq 0 \Leftrightarrow Z\beta_d \geq V \Leftrightarrow \Phi(Z\beta_d) \geq \Phi(V) \tag{2.4}$$

with $\Phi(V)$ denoting the c.d.f. of V. $\Phi(Z\beta_d)$=P(Z) is the propensity score of attending ASC based on observed characteristics. The MTE as a function of these quantiles can then be expressed as:

$$MTE(X = x, U_D = u_D = p(Z)) = E(Y_i - Y_0|X = x, U_D = u_D = p(Z)) \tag{2.5}$$

where the MTE is the return to treatment for an individual with observed characteristics $X = x$, who is in the u_Dth quantile of the V distribution, which is equal to their propensity for treatment. The treatment effect at low values of u_D is the effect for students who have a low unobserved resistance, i.e., are eager to attend ASC. A weighted average of the MTEs then yields estimates of the ATE, ATT, and ATUT.[20].

2.3.2 Assumptions

The following assumptions are necessary for identification: First, there needs to be a first stage in which the instrument Z* (the change in the Euclidean straight-line distance to the nearest ASC in km) causes variation in the probability of treatment after controlling for (X, R, T, C). Section 2.4.2 and Table 2.1 present evidence that the distance to the nearest ASC indeed has a strong and significant effect on ASC enrollment after controlling for individual characteristics, district, year, and cohort fixed effects. Second, Z* must be independent of the unobserved component of the outcome and selection equation. That is, $Z^* \perp\!\!\!\perp (U_0, U_1, V)|(X, R, T, C)$. This assumption requires that the instrument is assigned as good as randomly, depending on (X, R, T, C). In addition, this implies the exclusion restriction that the distance to the nearest ASC must not directly affect the outcome conditional on D_i and (X, R, T, C). It further implies that how U_1 and U_0 relate to V (i.e., The MTE curve) must not depend on Z*. These two first assumptions, along with the monotonicity assumption, are virtually equivalent to the standard assumptions necessary to interpret an IV as LATE (Andresen, 2018), Angrist & Imbens (1995). Section 2.4.2 is dedicated to defending these assumptions.

In the ideal case of full support of the propensity score in both treated and untreated samples for all values of X, it is possible to estimate MTEs with no further assumptions (Carneiro et al., 2011; Cornelissen et al., 2016). In practice, however, this is rarely achieved, especially if X is a high-dimensional vector (e.g., Carneiro et al., 2011, 2017; Cornelissen et al., 2018; Felfe & Lalive, 2018). Hence, in the MTE literature, it is common to make a further assumption: $E(U_j|V, X) = E(U_j|V)$. This assumption, called the *separability assumption*, allows for identifying the MTE over the unconditional support of the propensity score, jointly generated by the instrument and the covariates, as opposed to the support of the propensity score conditional on X=x. This assumption has two implications: first, that the shape of the MTE - the manner in which U_0 and U_1 depend on V - is independent of X. Second, that the MTEs are additively separable into an observed component X and unobserved component U_D:

$$Y_{1i} - Y_0 = X_i(\beta_1 - \beta_0) + U_{1i} - U_{0i} \tag{2.6}$$

[20]Consult Cornelissen et al. (2016) for a derivation of the respective weights

Unfortunately, to my knowledge, no testing procedure is available to provide evidence for the additive structure of the MTE (Su et al., 2015). Therefore, I proceed by inspecting how much my data deviates from the ideal case described above and by relying on the separability assumption common in the applied literature.

2.3.3 Estimation

Using p = P(Z), the separability assumption and taking expectations, I deduce the following equation:

$$\begin{aligned}&[Y|X=x,R=r,T=t,C=c,P(Z)=p]\\&=X\beta_0+R\alpha+T\delta+C\gamma+X(\beta_1-\beta_0)p+\underbrace{p(U_1-U_0|U_D\leq p)}_{\text{K(p)}}\end{aligned} \tag{2.7}$$

where K(p) is a nonlinear function of p capturing heterogeneity across U_D. Taking the derivative of this expression with respect to the propensity score p yields the MTE:

$$\frac{\partial[Y|X=x,R=r,T=t,C=c,P(Z)=p]}{\partial p}=X(\beta_1-\beta_0)+\frac{\partial K(p)}{\partial p} \tag{2.8}$$

I start by identifying the selection equation employing a probit model to obtain estimates of the propensity score $\hat{p}=\Phi(Z\beta_d)$. In a second step, I need to assume the unknown shape of $K(p)$ by choosing a polynomial in p of degree k to estimate the outcome equation:

$$Y=X\beta_0+R\alpha+T\tau+C\gamma+X(\beta_1-\beta_0)p+\sum_{k=2}^{K}\alpha_k\hat{p}^k+\epsilon \tag{2.9}$$

I assume a second-order polynomial in $\hat{p}$ in my baseline specification but find similar results for K=3, a joint normal and a semiparametric specification of K(p) (see section 2.5.4).

2.4 Data and instrument

2.4.1 Data set description

The empirical analysis combines geo-referenced data from the German Socio-Economic Panel (SOEP, see Goebel et al., 2019) with a self-collected school data set, as well as information on the district and municipality level.[21]

Student-level data

My estimates use data from the SOEP, a nationally representative survey that started in 1986. Nearly 13,000 households and more than 30,000 individuals are surveyed each year, gathering information about respondents' demographics, household composition, educational outcomes, and labor market characteristics (Goebel et al., 2019). The SOEP is particularly suitable for my research questions because it has data on both attendances of ASC alongside detailed information on the individual and family background of the children. It also comprises a large set of interesting outcome parameters that allow for comprehensively assessing the effects of ASC, covering variables in the domain of (non-)cognitive and social skills. In addition, the SOEP contains several geographically referenced indicators and detailed regional information such as the community type[22] and community size classification, which serve as control variables.[23]

The 2003-2018 SOEP data contains several special surveys, such as the M1 and M2 Migration Sample and the M3 Refugee sample.[24] All estimates include individual weights to avoid the oversampling of these particular groups (Kroh et al., 2017).

School-level data

I complement this student-level data set with self-collected administrative data on the location of elementary schools offering ASC slots between 2003 and 2018 from the six most populous Western German federal states. Specifically, my sample includes school-level data from the following federal states and years, respectively: Bavaria 2003-2018,

[21]I use the *INKAR* database for SES data on the district and municipality level, which mainly serves as control variables and for the balancing test in 2.4.2 Districts correspond to the NUTS 3 definition and there are currently 400 districts in Germany. With currently 11,130 items, municipalities represent an even smaller territorial unit.

[22]the community type groups regions into categories according to the number of inhabitants of the specified socio-economic region, like peripheral regions or agglomerations

[23]I make use of the on-site access at DIW Berlin to obtain street-block geo-coordinates and district and municipality keys. The location of the street-block coordinates allows me to calculate the Euclidean straight-line distance from the respondent household's location to the next ASC. The district keys control for district fixed effects, and the municipality keys control for additional socio-economic municipality characteristics. I thank the team of the SOEP infrastructure for their technical support in the geo-matching process.

[24]See SOEP (2021) for a detailed overview of the different SOEP samples.

North Rhine-Westphalia 2005-2018, Baden-Wuerttemberg 2012-2018, Hesse 2005-2018, Rhineland-Palatinate 2005-2018, and Lower Saxony 2010-2018.[25] All these states have experienced a significant expansion in offered ASC slots during the observed period.[26]

Overall, the combination of these two data sources is suitable to estimate the MTE of ASC in Western Germany for several reasons: First, I observe individual yearly ASC attendance. Second, I can link information on the exact location of the respondents' street block to the address data of ASC provided by the federal statistic offices of the six federal states in my sample. Specifically, I calculate the Euclidean straight-line distance to the nearest school offering ASC and the distance to the nearest elementary school irrespective of ASC offers. The former forms the continuous instrument I need to implement the MTE framework, and the latter serves as a control variable. Third, the data set has strong external validity. There are 4,002 students in my sample for which I have full information, representing 73 percent of the German population and 85 percent of the Western German population of elementary school children.[27] Fourth, using the survey data also means that I observe a rich set of outcome and control variables, which are discussed in the following sections.

2.4.1.1 Outcome variables

As mentioned before, the afternoon programs offered by German ASC consist of care, homework support, and supervised recreational activities, targeting cognitive and non-cognitive skill formation. I explore a variety of outcomes related to these areas, drawing a comprehensive picture of the effects of afternoon care on elementary school-age children's skill formation. I use the mother-child questionnaire surveying the mothers of children aged nine to ten, i.e. when they are still in primary school, for most of the main outcome variables.[28]

Schooling outcomes

I use *grades in German and mathematics* as the primary measure of short-term schooling outcomes. In the German school system, grades range from 1 (best) to 6

[25] I received data from these federal states in response to a request to all Western German federal states posed in spring 2020. I thank the DIW Graduate Center and Jan Marcus for generous funding support.

[26] This area covers almost all of Western German except for the city-states Berlin, Bremen, and Hamburg, as well as the small federal states of Schleswig Holstein and Saarland.

[27] The population in these states amounted to 60.5 million inhabitants in 2020, which corresponded to 72.8 percent of the 83.2 million inhabitants in Germany and to 85.7 percent of 70.6 million in Western Germany the same year (Statistisches Bundesamt, 2021a).

[28] The full questionnaires for each year is found here: `https://www.diw.de/en/diw_02.c.222729.en/questionnaires.html`

(worst), with 4 (sufficient) being the grade at which a class counts as passed. I reverse the scale for better interpretability; thus, 6 is the best possible grade in the outcome variable.

Non-cognitive skills

I observe a number of outcomes broadly classified as non-cognitive skills (NCS),[29] which I divide into two sub-categories: socio-emotional skills and personality. For the first, I use the *Strengths and Difficulties Questionnaire* (*SDQ* Goodman et al., 1998), which captures children's behavior on five scales: hyperactivity, emotional problems, peer problems, conduct problems, and prosocial behavior. The *SDQ* questionnaire has been surveyed since 2010 (Richter et al., 2013). The formation of *prosociality*, which does not form a part of the SDQ Difficulty Scale but rather is assessed separately, is receiving special attention in the economic context for playing a critical role for later life outcomes, including educational success, labor market success, health, well-being, and social capital (Algan et al., 2014; Deming, 2017; Kosse et al., 2020; Peter et al., 2016).[30] I measure personality development using the *Big Five* personality traits (Borghans et al., 2008; Lang et al., 2011; McCrae & Costa Jr, 2008). Table A1 defines these personality traits in more detail. The SOEP adopts a slightly shorter scale that can nevertheless reflect the basic structure of the Big-Five model in a reliable way (Richter et al., 2017). The mother-child survey features two questions per factor, with answers ranging from 1 (not at all) to 10 (applies fully). I standardize all development indicators to have a zero mean and a standard deviation of one. In my results on non-cognitive and social skills, I do not compare children of immigrant ancestry to those who were born and raised in Germany.[31] I further include a rich set of control variables.[32]

[29]For a discussion on the term *non-cognitive skills* and an overview of the commonly used concepts, see Borghans et al. (2008).

[30]The prosocial scale includes five items reading as follows: 'considerate of other people's feelings,' 'shares readily with other children,' 'helpful if someone is hurt, upset, or feeling ill,' 'kind to younger children,' and 'often volunteers to help others.' Responses were given on a seven-point Likert scale ranging from 'does not apply at all' (1) to 'applies completely' (7). In addition, as is common practice, I construct an average score based on the four development dimensions to measure children's socio-emotional development.

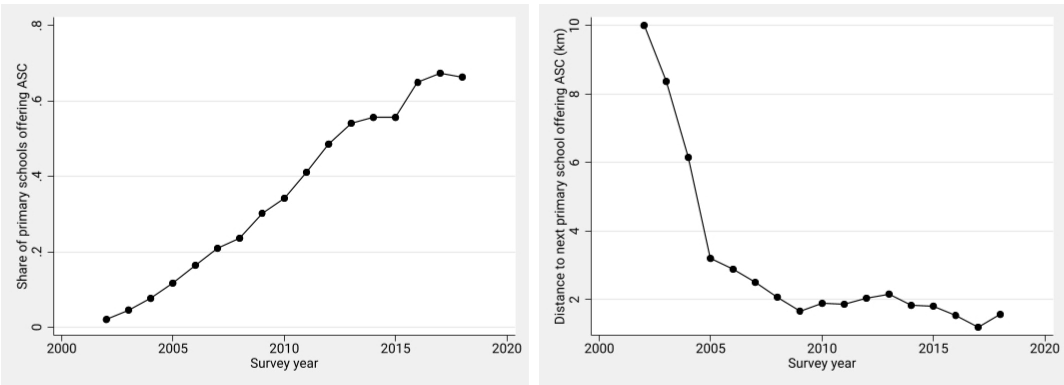
[31]Immigrant parents might be rooted in the culture of their country of origin, which might affect the way they regard socially desirable behavior in children and, hence, how they report about their children (Runge & Soellner, 2019).

[32]These control variables are: age and sex of the child, whether the child resides in a single-parent household, highest parental education (no degree vs. apprenticeship vs. university degree), log household net income adjusted by the OECD modified equivalence scale, the number of younger siblings in the household, a dummy that takes value one if the household receives a social transfer (unemployment benefits I or II, benefits from the educational package, asylum seeker allowance, and/or subsistence allowance), migration background (taking value one when both parents were not born in Germany), as well as the legal status (public vs. private) and type (integrated, semi-integrated, non-integrated) of the nearest ASC. To account for systematic differences in the expan-

2.4.2 Instrument assessment

The expansion of the ASC sector in Western Germany, described in Section 2.4.2, offers a source of variation in ASC attendance that arguably does not depend on child or family characteristics. Proximity to relevant educational institutions is widely used in the economics discipline as an instrument for attending these institutions (e.g., Card, 1993; Neal, 1997; Rouse, 1995)). The basic rationale behind using distance as an instrument for participation in educational offers is intuitive: individuals, weighing their costs and potential benefits, are more willing to take up an educational offer when commuting time and costs are reduced (Chakrabarti & Roy, 2010). This section reviews the validity criteria of this instrument in my setting.

Figure 2.1: Evolution of ASC offer and distance over time



(a) (b)

Notes: Panel (a) shows the evolution of the share of elementary schools offering afternoon programs. Panel (b) displays the evolution of the distance to the nearest elementary school offering ASC by survey year. Source: Own calculations based on the SOEP v35, 2003-2018, KMK (2021).

Relevance

First, the change in the average distance to the nearest ASC within one district must be a strong predictor of ASC attendance. For most federal states in Germany, distance is automatically a predictor of public elementary school attendance since children are usually allocated to schools closest to their homes.[33] 97 percent of the schools in my

sion of the ASC sector, I employ district, year, and municipality size ($\leq$ 500, 500-5000, 5000-50,000, 50,000-500,000, and $\leq$ 500,000 inhabitants) fixed effects.

[33] In most federal states, allocation to elementary schools is organized in catchment areas, with students attending the school closest to their home. North Rhine-Westphalia (NRW) is the only federal state

sample are public; this implies that, in most cases, children attend the school that is closest to their home. Hence, the first stage in my setting is determined by a child starting to attend ASC because their school started offering these programs as a result of the IZBB reform. Figure 2.1 displays the correlation between the ASC extension (panel A) and the change in the distance to the next ASC in kilometers (panel B) for an average student in the six federal states in my sample over the respective observation period. Table 2.1 shows the selection equation results and includes the parameter estimates for the first-stage linear probability model. To allow for a non-linear relationship between distance to the next ASC and afternoon program attendance, e.g., at very low or high values of Z*, I additionally include the square of the distance in km. A reduction in the distance to the nearest ASC facility by one kilometer is associated with a 4.8 percentage point increase in attending afternoon programs, a result that is significant at the 1 percent level and corresponds to a t-value of -3.3. This yields an F-statistic of 10.9, which just exceeds the critical threshold of 10. Hence, the first stage is strong enough to perform an IV/MTE analysis, although a stronger first stage driven by a larger sample size would be more ideal.

Independence assumption

While the relevance of distances as instruments is usually uncontested, its independence has been called into question. The main concern is that, in many cases, educational institutions are not distributed randomly.[34] To address this concern, I residualize out district fixed effects in addition to birth and survey year fixed effects, the municipality size, and the distance to the nearest elementary school to account for urban-rural differences in the geographical density of educational institutions, as well as individual and household characteristics. Therefore, the independence assumption in my case transmutes to the weaker assumption that the change of the average distance within one district conditional on the controls X_{ij} must not co-vary systematically with the child outcomes Y_{ij}.

While this assumption cannot be tested directly, I argue that it is likely to hold. Across federal states, IZBB grants were distributed proportional to the total number of

that abolished this system for elementary schools in 2008, switching to an admission regime based on parental choice. However, even after the reform, incentives for staying within the former catchment area remained since travel costs are reimbursed only when the nearest school is attended (Breuing, 2014). While most elementary school students attend the school closest to their home, there are various ways for parents to avoid catchment areas (e.g., Noreisch, 2007).

[34]For instance, highly educated parents might select into urban areas with a higher density of educational institutions. By including a large set of individual, family, and county-level characteristics, later studies (e.g., Dee, 2004) can plausibly defend the independence assumption of distance to educational institutions.

school students living in the stata in the school year 2001/02. The pace and magnitude of the expansion of the ASC sector in a given district depended on different factors, the most important one being the allocation of IZBB investments. The latter was based on a two-step decision-making process. First, schools had to apply for investments with a school concept developed by the school's director and the school committee. In a second step, schools were selected by the federal states on a first-come, first-served basis. Beforehand, the federal states had declared a particular investment focus. While some states like Hamburg and Saxony-Anhalt focused on schools located in areas with low baseline socio-economic status or a high share of immigrants, the six states in my sample did not state any such priorities (BKJ, 2005). Instead, Bavaria, Baden-Wuerttemberg, and Lower Saxony announced that they would initially focus on secondary schools for the ASC expansion, initially slowing the expansion of all-day primary schools. On the other hand, North Rhine-Westphalia pursued a clear focus on primary schools (BKJ, 2005). Schools that did not apply or did not receive grants could reapply one year later.

Previous studies exploiting the IZBB reform (e.g., Dehos & Paul, 2021; Seidlitz & Zierow, 2020) have made the case that the provision of IZBB funds did not follow any strategic pattern. Seidlitz & Zierow (2020) establish that pre-expansion socio-economic municipality characteristics (birth rate, inward migration, labor market participation rates etc.) do not predict whether a municipality received funding. Dehos & Paul (2021), regressing the average funding intensity of the years 2003 to 2012 on outcomes at the onset of the IZBB program, find that absolute funding measures correlate with the share of foreigners in a county and the county-specific employment ratio of women without a child but not with any other characteristics (GDP, average education level, unemployment rate, share of single parent households etc.). Inportantly, they find no correlation between the average yearly changes in the funding intensity and pre-treatment characteristics.

Similar to Dehos & Paul (2021), my main identifying variation rests on changes in the ASC supply over time – however measured by changes in the average distance to the next ASC providing school rather than the investment intensity. I run two balancing tests to strengthen the independence argument for my empirical setting. First, I test the assumption of the absence a strategic expansion of ASC by regressing the change in the distance to the next ASC offering school on socio-economic district characteristics at the beginning of my observation period.[35] The results of this first balancing test are

[35]Because of data limitations, the observation period differs across the different federal states in my sample and starts in 2003 for Bavaria, 2005 for North Rhine-Westphalia, Hesse, Lower Saxony, and Rhineland-Palatinate, and 2012 for Baden Wuerttemberg.

shown in Table A3. As expected, the change in average distance in a district correlates highly and positively with the initial distance in the same district, which shows that areas with low baseline density of ASC – hence a higher baseline distance – more rapidly expanded the offer of afternoon programs during the observation period. There is also a statistically significant correlation between the pace of ASC expansion with the share of children aged six to nine in the population. This just shows that the ASC expansion prioritized districts with a higher density of elementary schools, which arguably does not threaten my identification. Apart from that, the ASC expansion does not exhibit any correlation with the initial characteristics of the district. Second, I test whether the average change in distance to ASC correlates with individual characteristics in my sample. Table A4 shows that the ASC expansion does not correlate with any individual or household SES variables, strongly supporting the independence argument.

Common support

Full common support implies that for each value of P(Z), I should observe treated and non-treated individuals. To test this assumption, I estimate the propensity score using a probit regression and plot the histogram of common support. Figure A.3 graphs the unconditional support jointly generated by variation of both the instrument and the covariates (X, R, T, C), showing that the first stage generates full common support for the propensity score P(Z), albeit with relatively few observations for the non-treated starting at P(Z)=.9.[36] To account for the scarcity in observations at very high levels of P(Z), I limit my analysis to $0 \geq$ P(Z) $\geq .95$ and $0 \geq$ P(Z) $\geq .9$ as sensitivity checks. As discussed in Section 2.3, I also test how much variation my instrument creates in each covariate cell of X, i.e., conditional on $X_i = x$, to inspect how much my data deviates from the ideal case imposing the minimal assumption of (U_0, U_1, V) being independent of Z given X. Figure A.2 reveals relatively small support of P(Z) for each value of X, as is also the case in other applications (e.g., Carneiro et al., 2011)). However, under the separability assumption, MTEs are identified over the marginal support of P(Z) (Figure A.3).

Monotonicity

With heterogeneous treatment effects, an additional necessary assumption to identify causal effects is monotonicity. This assumption requires that students who attend an

[36]Full common support is a condition rarely achieved in practice in the MTE literature (e.g., Carneiro et al., 2011; Cornelissen et al., 2018), although it is critical for computing the ATT and the ATUT, which heavily weigh individuals at the extremes of the propensity score distribution.

afternoon program would also do so if they lived closer to a school offering them, hence ruling out the existence of defiers in the sample. This assumption is intuitively plausible in my context since it is difficult to think of why students would stop attending afternoon programs once their catchment area school starts offering them. To strengthen this argument, Table A2 shows that the first stage is positive and statistically significant in all subsamples of the data.[37]

2.5 Results

2.5.1 First stage and descriptives

Table 2.1 displays the results of the selection equation, showing that, on the one hand, children from single-parent and social transfer-receiving households are more likely to attend afternoon programs. On the other hand, children whose parents are both gainfully employed are more likely to be found in ASC. Hence, while it is generally low-SES children who take up ASC offers more often, there is also a fraction of high-SES students with a high propensity to attend ASC. Given this selection mechanism, it is not surprising that children who participate in ASC differ in the mean outcome variables from those who did not (Table A5). Children in the treatment group have lower outcomes in all outcome variables but prosociality. On average, children attending ASC have lower grades, have more socio-emotional difficulties measured by the SDQ scale, are less open, and are more introverted, although in most cases, the differences are small.

2.5.2 Treatment Effect Heterogeneity in Observed Student Characteristics

Based on equation 2.7, Table 2.2 reports estimates for the effects of ASC on cognitive skills measured by schooling outcomes and NCS. The results for the German grade (Panel A) points to a slightly equalizing effect of ASC attendance. As can be seen from comparing the coefficient of the non-interacted child characteristics with the coefficient of the characteristics interacted with the propensity score, children from single parent households with lower baseline German grades tend to benefit less from ASC attendance than their peers.

This equalizing pattern is also observed for the effects of ASC attendance on prosociality, and on the Big Five personality traits (Table 2.2, Panel B). Regarding proso-

[37] This is equivalent to testing a weaker form of the monotonicity assumption – average monotonicity – which is sufficient to interpret 2SLS estimates as causal effects (De Chaisemartin, 2017).

Table 2.1: Selection equation

	ASC attendance
Distance to ASC in km	-0.048***
	(0.015)
Distance squared	0.004**
	(0.002)
Female	-0.013
	(0.019)
Social transfer receiving household	0.095**
	(0.038)
Single parent	0.086***
	(0.033)
Migration background	0.016
	(0.027)
At least technical degree	-0.084
	(0.054)
Academic degree	-0.069
	(0.054)
Younger siblings	-0.001
	(0.029)
Low income	-0.016
	(0.030)
Urban area	0.024
	(0.079)
Both parents working	0.045*
	(0.025)
Number of observations	4002

Notes: The reported estimates represent the results of a linear probability model in which the dependent variable is equal to one if the child attends ASC for most of elementary school. The model includes the full set of individual control variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v35 and administrative school data, own calculations.

Table 2.2: Outcome heterogeneity based on observed characteristics

Panel A	Math grade	German grade	Prosociality	SDQ	
Propensity Score (PS)	0.949*	-0.318	0.549	-1.299**	
	(0.573)	(0.508)	(0.662)	(0.649)	
PS squared	-1.184**	-0.680	-0.259	-0.030	
	(0.587)	(0.659)	(0.669)	(0.669)	
Female	-0.132*	0.211**	0.397***	0.087	
	(0.079)	(0.086)	(0.087)	(0.092)	
Migration background	0.095	0.056			
	(0.106)	(0.091)			
Single parent	-0.189*	-0.199*	-0.421***	-0.286**	
	(0.112)	(0.107)	(0.132)	(0.138)	
Academic degree	0.382***	0.427***	-0.210*	0.159*	
	(0.103)	(0.111)	(0.111)	(0.093)	
Low income	-0.135	-0.142	-0.174	-0.095	
	(0.101)	(0.091)	(0.132)	(0.122)	
Female x PS	-0.341	0.075	-0.471*	0.362	
	(0.231)	(0.247)	(0.261)	(0.256)	
Migrant x PS	-0.287	-0.037			
	(0.295)	(0.258)			
Single parent x PS	0.296	0.620**	0.670**	0.476	
	(0.301)	(0.311)	(0.298)	(0.373)	
Academic degree x PS	-0.174	-0.168	0.261	0.046	
	(0.301)	(0.312)	(0.449)	(0.269)	
Low income x PS	-0.387	-0.284	0.163	0.246	
	(0.319)	(0.277)	(0.363)	(0.335)	
Observations	3189	3185	3510	3502	
Panel B	**Openness**	**Conscient.**	**Extroversion**	**Agreeableness**	**Em. stability**
Propensity Score (PS)	-0.218	-0.318	-0.620	-0.539	-1.173*
	(0.751)	(0.765)	(0.646)	(0.732)	(0.704)
PS squared	-0.832	0.066	-0.806	-0.746	-0.033
	(0.835)	(0.851)	(0.833)	(0.778)	(0.834)
Female	-0.015	-0.106	-0.107	-0.093	-0.030
	(0.085)	(0.095)	(0.092)	(0.097)	(0.091)
Single parent	-0.171	-0.214*	-0.016	-0.192	-0.051
	(0.121)	(0.121)	(0.125)	(0.131)	(0.112)
Academic degree	0.179*	0.316***	-0.178	-0.010	-0.022
	(0.099)	(0.086)	(0.115)	(0.090)	(0.102)
Low income	-0.166	0.292**	-0.162	-0.009	-0.042
	(0.125)	(0.120)	(0.117)	(0.132)	(0.109)
Female x PS	0.135	0.158	0.153	0.036	-0.118
	(0.273)	(0.267)	(0.251)	(0.245)	(0.317)
Single parent x PS	0.634*	0.222	0.591*	0.831**	0.366
	(0.324)	(0.334)	(0.333)	(0.337)	(0.282)
Academic degree x PS	0.354	-0.243	0.685**	-0.045	0.207
	(0.328)	(0.227)	(0.309)	(0.247)	(0.290)
Low income x PS	0.122	-0.321	0.233	0.019	0.374
	(0.323)	(0.328)	(0.398)	(0.351)	(0.322)
Observations	3495	3491	3495	3481	3490

Notes: The table displays estimates from the first part of the outcome equation (eq. [9]). The grades and the SDQ are reversed (higher score = better outcome) to ease interpretability. Coefficients of the independent variables not interacted with the propensity score in the first part of the table measure effects on the outcome in the untreated state (i.e., β_0 in eq. [9]), whereas coefficients of the same regressors interacted with the propensity score measure the difference of the effects between the treated and the untreated state ($\beta_0 - \beta_1$ in eq. [9]).* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* SOEP v35 and administrative school data, own calculations.

ciality, openness, extroversion, and agreeableness, it is also children from single parent households who enjoy larger benefits from attending afternoon care. The pattern of observed effect heterogeneity is more complicated for the outcome of extroversion, since besides children from single parent households it is also children from highly educated parents that benefit significantly more than the average student from ASC.[38]

2.5.3 Essential heterogeneity

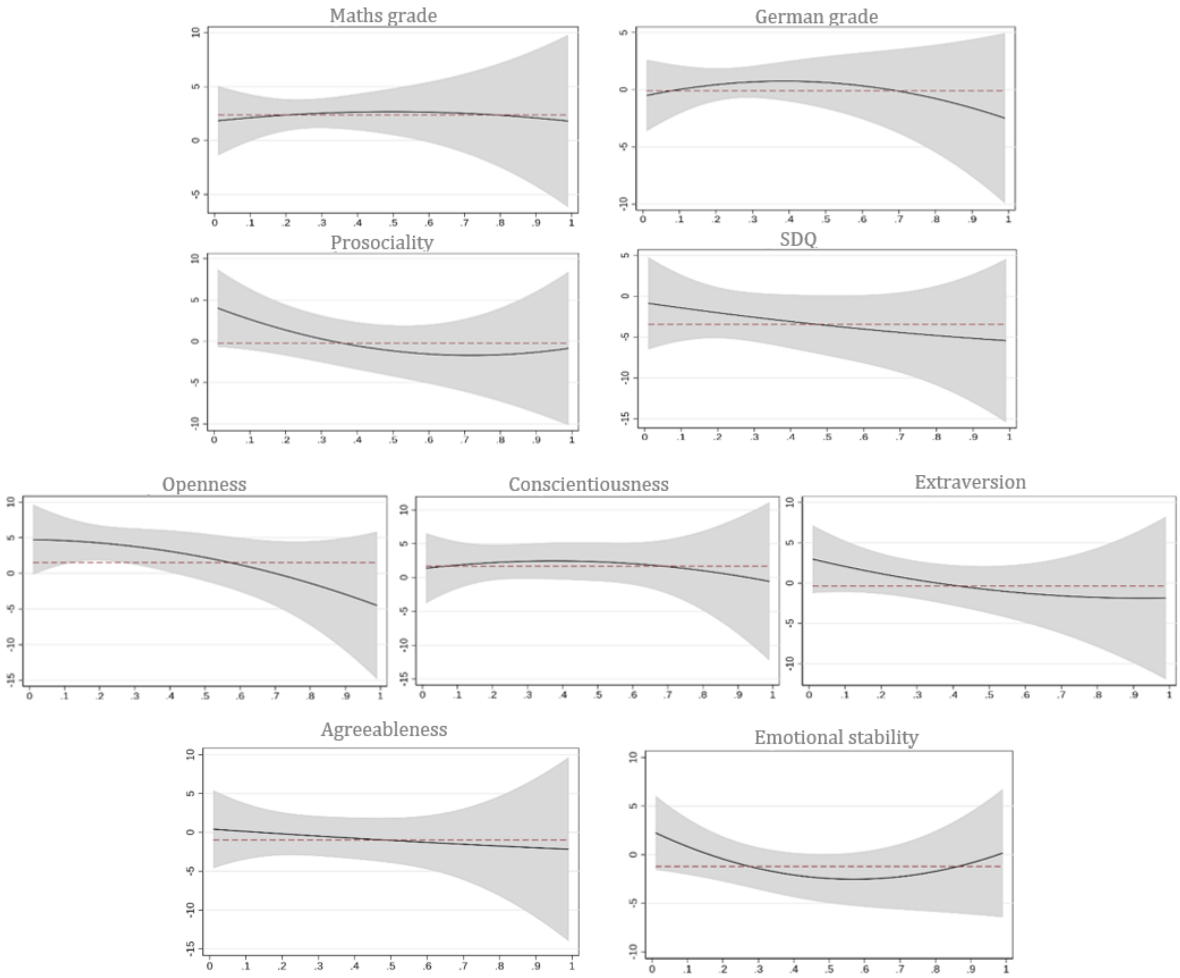
In addition to heterogeneity in terms of observed characteristics, I find substantial heterogeneity in the treatment effect based on unobserved characteristics, also referred to as essential heterogeneity. Similar to the selection based on levels (observed characteristics), I find a positive selection pattern concerning gains (unobserved resistance to treatment) for most NCS outcomes. Figure 2.2 displays the MTE curves described by equation 9 for mean values of X in my sample. The MTE curve relates the unobserved parts of the ASC treatment effect ($U_1 - U_0$) to the unobserved parts of the choice for ASC participation (U_D) – the resistance to treatment. For most non-cognitive outcomes, except conscientiousness and agreeableness, I observe a slightly falling curve. This shape of the MTE curve implies that for these outcomes, the treatment effect decreases as the resistance to treatment increases, meaning that students who are more eager to sign up for afternoon programs appear to benefit the most from them in terms of NCS. For the two schooling outcomes, maths an German grade, the flat MTE curve signifies that students with different levels of treatment resistance on average are not affected differently by ASC attendance with respect to these outcomes. Figure A.4 plots the MTE curves when applying a joint normal approach.[39] In this model, the MTE curves are more clearly downward sloping.

These patterns are also reflected in the summary treatment parameters ATE (average treatment effect), ATT (average effect of treatment on the treated), and ATUT (average treatment effect on the untreated) in Table 2.3. The overall pattern mirrors the falling curves in Figure 2.2 for NCS: The ATUT and even the ATE are negative for five of the NCS outcomes (prosociality, SDQ, extroversion, agreeableness, and emotional stability). In contrast, the ATT in these cases is positive and statistically significant in the case of prosociality, SDQ, openness, extroversion, and emotional stability. This finding suggests a positive selection into treatment in terms of personality development:

[38] As explained in section 2.4.1.1, when using parent-reported survey data on non-cognitive skills, comparisons of children with and without migration background are problematic. Hence I only distinguish between other SES characteristics.

[39] In this model, one needs to make the stronger assumption of joint normality of (U_0, U_1, V) instead of the separability assumption (Andresen, 2018; Cornelissen et al., 2016).

Figure 2.2: MTE curves



Notes: The figure plots the MTE curves (expression $\sum_{k=2}^{K} \alpha_k \hat{p}^k$ in eq. [9] for my main outcome variables evaluated at mean values of the covariates. The 90 percent confidence interval is based on standard errors clustered at the district level. *Source:* Own calculations based on SOEP v35 and administrative school data, 2003-2018.

Table 2.3: Summary treatment statistics

Ind. variable	Math grade	German grade	Prosociality	SDQ	
ATE	0.151	-0.097	-0.230	-0.481	
	(1.529)	(1.287)	(1.673)	(1.583)	
ATT	-0.046	-0.0364	1.754*	1.797**	
	(1.012)	(0.729)	(0.905)	(0.884)	
ATUT	0.237	-0.120	-1.056	-1.414	
	(2.066)	(1.65)	(2.238)	(2.018)	
LATE	0.000	-0.011	-0.002	-0.002	
	(0.004)	(0.009)	(0.005)	(0.007)	
Observations	3189	3185	3510	3502	
p (observable het.)	0.000	0.000	0.000	0.000	
p (essential het.)	0.356	0.538	0.143	0.849	
Ind. variable	Openness	Conscient.	Extroversion	Agreeable	Emotional stability
ATE	1.511	1.677	-0.350	-0.978	-1.207
	(1.602)	(1.511)	(1.613)	(1.650)	(1.276)
ATT	1.803*	1.097	2.252**	1.032	1.453*
	(0.934)	(0.920)	(0.874)	(1.061)	(0.823)
ATUT	1.397	1.923	-1.436	-1.814	-2.313
	(2.125)	(2.110)	(2.222)	(2.301)	(1.695)
LATE	0.001	-0.000	0.012***	-0.002	-0.005
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Observations	3495	3491	3495	3481	3490
p (observable het.)	0.000	0.000	0.000	0.000	0.000
p (essential het.)	0.211	0.891	0.378	0.858	0.094

Notes: The table reports the average treatment effect (ATE), the treatment effect on the treated (TT), the treatment effect on the untreated (TUT) for all outcomes and the p-value for a test of observable and essential heterogeneity. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v35 and administrative school data, own calculations.

the average student who selects into ASC benefits from it, while students who choose not to attend are indeed, on average, better off without it. Besides the large ATT, there is also a – much smaller – statistically significant local average treatment effect (LATE) on extroversion, showing that ASC had a positive and significant effect on the extroversion of instrument compliers.

Notably, the ATE, derived by equally weighting over the MTE curves in Figure 2.2 and evaluating at average values of X[40], is negative for six out of the nine outcomes, albeit not statistically significant. This result implies that, concerning these outcomes, the average student would likely not benefit from ASC. Hence, if the afternoon programs were mandatory for all elementary school students, this would likely result in adverse effects on the average child. In contrast, with the current regulation of most afternoon programs being voluntary, children selecting into them have either positive or zero effects. However, the pattern of a positive essential heterogeneity, indicating that selection into treatment is efficient, is only statistically significant at the 10 percent level for emotional stability.[41]

2.5.4 Sensitivity to alternative specifications

I conduct several sensibility checks to validate the robustness of my results. Table A6 shows the main summary parameters for the outcomes for which I observe statistically significant treatment effects in my baseline specification.[42] While for some of the specifications in Table A6 the magnitudes of the treatment effect parameters change, the overall pattern remains stable, and all robustness checks confirm a positive selection pattern, where the ATT is positive, and the ATUT is more negligible or negative. Furthermore, I check whether the pattern of the falling MTE curve is robust to consecutively leaving out more control variables to rule out that results are driven by choosing a specific set of control variables. Figure A.5 demonstrates this robustness for

[40] Cornelissen et al. (2016) and Andresen (2018) for a derivation of the weights

[41] Many of the treatment effect parameters reported, amounting to as much as 2.25 SD in the case of extroversion, would be deemed very large effect sizes by Cohen's guidelines (Cohen, 2013). This raises the question of whether the measured effects overestimate the true effect of ASC attendance, as may be the case when employing IV methods while facing a limited sample size (e.g., Becker, 2016; Bound et al., 1995). One potential reason for this overestimation is that the first stage regression reduces the variance of the endogenous explanatory variable such that the estimated coefficient increases (de Jong, 2016). Another plausible explanation is that the MTE method gives a lot of weight to individuals at the tails of the P(Z) distribution, making the ATT and the ATUT susceptible to over-representing outliers. Hence, the magnitude of the measured effects should be taken with a grain of salt. Instead, it is the relatively consistent general pattern of positive selection into treatment that should be the takeaway of this paper with respect to guiding policies on ASC.

[42] First, I restrict the P(Z) to P(Z)$\leq$ .95 and P(Z)$\leq$ .9, respectively (columns 2 and 3). Further, I cluster the standard errors on the individual level instead of on the district level (column 4).

the example of the outcome of openness. Figure A.6 additionally shows the MTE plots for the same outcome variables comparing alternative functional forms: the parametric joint normal model, third polynomial, and semiparametric estimation. The latter is an important test to check for potential misspecification in the estimated propensity score (Cornelissen et al., 2018). The shape of curves look similar for prosociality, the SDQ, openness and extroversion and differs slightly in the case of emotional stability. I also run a placebo test by estimating the MTE on the SDQ Score at age five, i.e., shortly before school entry. As shown in Figure A.7, the MTE curve for this placebo outcome is flat as expected.

2.5.5 Discussion

The finding of a positive selection into ASC based on levels and gains for most of my outcome variables stands in stark contrast to the adverse selection found by Cornelissen et al. (2018) for pre-school daycare. This result may at first glance be surprising because of the apparent similarities of the institutional setting – care facilities in Germany and the target age group (3-6 vs. 6-10). Still, the selection pattern differs from the study by Cornelissen et al. (2018) in important ways: First, ASC in Germany was created to increase equality of opportunity (BMBF, 2009); hence it is likely that teachers specifically target low-SES children and encourage their parents to register for the afternoon care. In contrast, child care centers in the 1990s and early 2000s admitted children based on their mothers' employment status and time on the waiting list, which gave high-SES families an advantage (Cornelissen et al., 2018). Second, the cultural differences that might have prevented families of immigrant ancestry from sending their children to early daycare may not be relevant for my age group. Elementary school is compulsory virtually all over the world, with many countries offering ASC schemes (European Commission, 2018). Hence there may be fewer reservations for mothers with different cultural backgrounds about sending their children to ASC. Finally, attitudes on female labor market participation and child care have changed in the last decades.

Overall, my findings suggest more substantial effects of ASC on NCS than on schooling outcomes. With a sizeable average group size and staff with partly limited pedagogical training, it seems that it is more the social aspects of ASC than the homework support that make a difference for the children. This result is in line with the findings of qualitative evaluation studies (Fischer et al., 2011; Radisch, 2009; StEG Konsortium, 2016), which also highlight the special role of ASC for social skill development of migrant children. It also strengthens the argument that during early childhood and through elementary school, children's social skills and personalities are remarkably malleable and reactive to social interactions and daycare activities (e.g., Bach et al., 2019; Kosse et al., 2020). This link is intuitive since becoming "social", "open" and "extroverted" is more easily practiced when regularly surrounded by peers. When interpreting the results on NCS, it is vital to remember that the outcomes are based on information given by the mothers. A potential limitation of these results is that I cannot rule out that the impression mothers get of their children attending ASC is affected by the shorter time window they spend together and the activities they share compared to families in the control group.[43]

My analysis also demonstrates that the LATE effects estimated by conventional IV methods differ from MTE estimates and disguise important heterogeneity patterns.

Generally, it is vital to stress that my results do not suggest global positive effects of ASC. Indeed, the ATE is negative, albeit not statistically significant, for most of my outcomes. In contrast, the ATT is positive for all NCS outcomes and statistically significant for five our of seven. Taken together, these results make a critical case not for an overall positive effect of afternoon programs but for the voluntariness of their offer since selection into ASC is positive and efficient. This means that federal states in the course of expanding the offer of ASC toward the legal entitlement of an ASC slot until 2026 should opt for the non-integrated type. Urging all elementary school students to participate in after-school programs would likely negatively affect their NCS.[44]

However, an important point to consider is that with most afternoon programs being voluntary and low-SES children selecting into ASC, a concentration of low-SES children in ASC will likely lead to a further decline in acceptance of ASC by high-SES families (Steiner, 2009). In the long run, this dynamic could increase social segregation. Hence, it should be a priority to keep ASC attractive for all students, e.g., by employing more and better trained pedagogical staff and offering more attractive recreational activities.

[43]For example, children who are regularly in school in the afternoon may be more tired when they get home, which can systematically impact how they interact with their parents. Furthermore, since they less often have to do homework at home, there may be fewer conflicts at home, leaving the parents under the impression that the child is more emotionally stable.

[44]On the other hand, making ASC mandatory for all children would likely result in a more effective lobby for better care quality on the part of the parents. The role of peer and rank effects on student achievement and non-cognitive development is contested in the literature (Burke & Sass, 2013; Denning et al., 2018; Elsner et al., 2021).

2.6 Conclusion

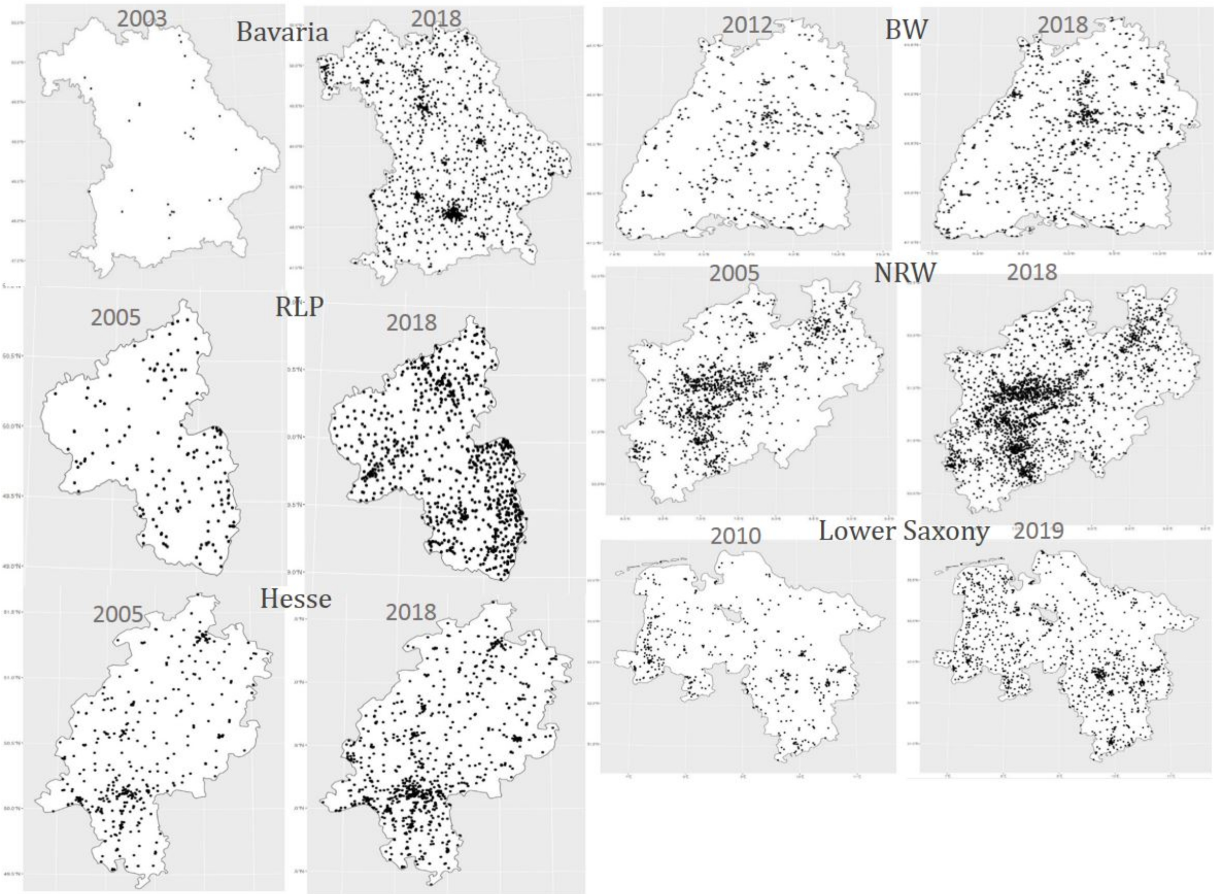
I examine the heterogeneous effects of after-school care (ASC) in Western Germany on children's cognitive and non-cognitive skill development. Employing the Marginal Treatment Effect (MTE) framework, I estimate how the effect differs along observed child characteristics and their latent propensity to attend ASC. Understanding this heterogeneity is essential to determine whether selection into ASC is efficient and, i.e., if a universal roll-out is likely to result in positive effects on those who select into treatment, and whether the offer should be compulsory for all elementary school children or not. My estimation strategy relies on spatial and time variation caused by the rapid expansion of ASC slots following an extensive investment program in Germany in 2003, instrumented by the change in the distance to the next ASC within one district.

I find that low-SES children – those of single-parent and social transfer receiving households – are more likely to attend afternoon programs during elementary school. For most of my outcomes in the area of cognitive skills (as measured by the German grade), socio-emotional development (prosociality), and the Big Five personality traits (openness, extroversion, and agreeableness), children from single-parent households – who on average have worse baseline outcomes in these categories – seem to benefit more from treatment than their peers. Heterogeneity in unobserved characteristics reinforces this pattern of positive selection into ASC: children with a low resistance to treatment, i.e., who are more likely to enroll in afternoon programs, have either zero or positive treatment effects. For most of these outcomes, the average treatment effect on the treated (ATT) is positive – and statistically significant in the case of prosociality, the SDQ, openness, extroversion, and emotional stability. In contrast, the average treatment effect on the untreated (ATUT) and the average treatment effect (ATE) tend to be negative, albeit not statistically different from zero. This result implies that the average elementary school student would likely not benefit from ASC if it were mandatory.

My findings have two imperative policy implications: First, ASC does indeed seem to benefit low-SES students and can serve as a tool to increase equality of opportunity. Second, with different organizational types of ASC currently co-existing in Germany and beyond, these results make a strong case for organizing ASC in a non-integrated way, where participation in the afternoon programs is voluntary. However, this dynamic bears the risk of increased segregation, where afternoon programs become even less attractive for high-SES students. Therefore, group composition and ASC quality should be a concern for schools and policymakers.

2.A Appendix

Figure A.1: ASC expansion in Western Germany



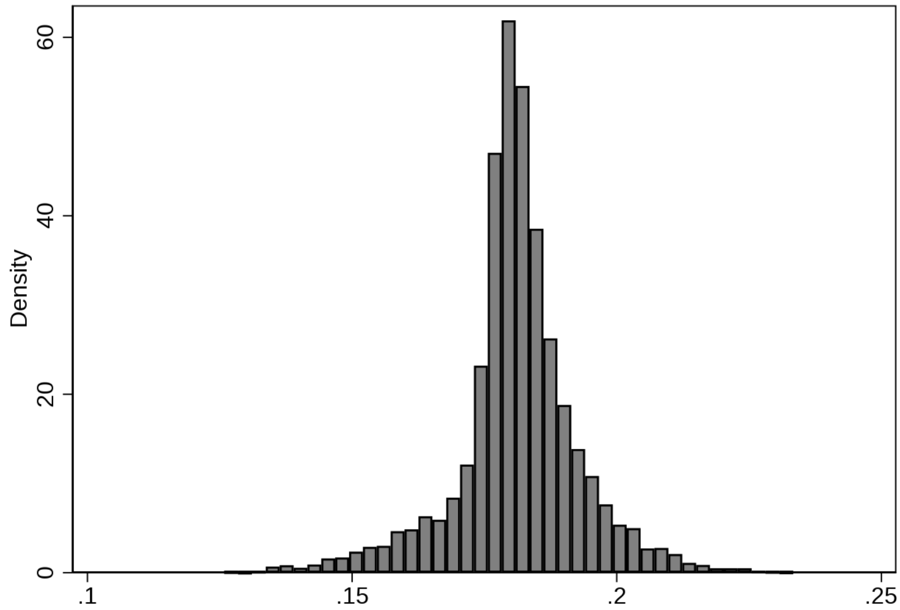
Notes: The figure plots the ASC expansion in the six federal states in my sample —Bavaria, Rhineland-Palatinate (RLP), Hesse, Baden-Wuerttemberg (BW), North Rhine-Westphalia (NRW) and Lower Saxony —in the first and last observed year, respectively. Source: administrative school data from federal statistical offices.

Table A1: The Big Five Domains

Factor	Facets	Definition of Factor	ACL[1] Marker Items for Factor
I. Openness to Experience (Intellect)	Fantasy, Aesthetics, Feelings, Actions, Ideas, Values	The degree to which a person needs intellectual stimulation, change, and variety.	Commonplace, Narrow-interest, Simple- vs. Wide-interest, Imaginative, Intelligent
II. Conscientiousness	Competence, Order, Dutifulness, Achievement striving, Self-discipline, Deliberation	The degree to which a person is willing to comply with conventional rules, norms, and standards.	Careless, Disorderly, Frivolous vs. Organized, Thorough, Precise
III. Extraversion	Warmth, Gregariousness, Assertiveness, Activity, Excitement seeking, Positive emotions	The degree to which a person needs attention and social interaction.	Quiet, Reserved, Shy vs. Talkative, Assertive, Active
IV. Agreeableness	Trust, Straight-forwardness, Altruism, Compliance, Modesty, Tender-mindedness	The degree to which a person needs pleasant and harmonious relations with others.	Fault-finding, Cold, Unfriendly vs. Sympathetic, Kind, Friendly
V. Neuroticism (Emotional Stability)	Anxiety, Angry hostility, Depression, Self-consciousness, Impulsiveness, Vulnerability	The degree to which a person experiences the world as threatening and beyond his/her control.	Tense, Anxious, Nervous vs. Stable, Calm, Contented

Notes: Source: borghans2008economics, costa1988catalog and hogan2002hogan. 1: ACL = Adjective Check List (Gough (1979)).

Figure A.2: Test for minimal assumptions



Notes: The figure plots the predicted propensity scores from a probit regression of ASC attendance
on the residualized values of Z*, i.e., the error term of a regression of Z* on [X, R, T, C].
Source: Own calculations based on SOEP v35 and administrative school data, 2003-2018.

Table A2: Monotonicity of the instrument

	Migration background		Social transfer receiving		Single parent	
	yes	no	yes	no	yes	no
Distance to ASC	-0.10***	-0.05***	-0.05**	-0.06***	-0.06*	-0.05***
	(0.04)	(0.01)	(0.03)	(0.02)	(0.03)	(0.01)
Distance squared	0.01**	0.00**	0.00	0.01***	0.01*	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Sample mean	0.32	0.24	0.35	0.20	0.29	0.24
N	923	3079	1514	2488	1006	2996
	Academic degree		Income level		Both parents work	
	yes	no	above mean	below mean	yes	no
Distance to ASC	-0.06*	-0.05***	-0.04*	-0.05**	-0.05***	-0.06***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Distance squared	0.01	0.00**	0.00*	0.01**	0.00**	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Sample mean	0.27	0.25	0.27	0.24	0.25	0.27
N	1227	2775	2519	1483	2137	1865
	Urban/rural					
	urban	rural				
Distance to ASC	-0.05***	-0.05**				
	(0.02)	(0.02)				
Distance squared	0.00**	0.00*				
	(0.00)	(0.00)				
Sample mean	0.28	0.17				
N	3073	929				

Notes: This table reports first stage results by subsamples based on household characteristics. All specifications control for time and district fixed effects. Robust standard errors are clustered at the district level and reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v35 and administrative school data, own calculations.

Table A3: Balancing Test: Avg. distance change and initial district characteristics

	Distance change in district
Distance in first year	0.852***
	(0.035)
Unemployment rate	-0.024
	(0.039)
Share of inhabitants with migration background	0.000
	(0.044)
Median household income	-0.534
	(0.580)
GDP per inhabitant	0.009
	(0.008)
Share of migrants in age group 6-9	0.025
	(0.027)
Share of school dropouts	-0.005
	(0.043)
Share of children age 6-9 in population	0.448**
	(0.207)
Share of academic track alumni	-0.030
	(0.025)
Share of lower track alumni	-0.002
	(0.014)
Share of women in the labor force	-0.042
	(0.031)
Number of observations	3464

Notes: The table displays the determinants of the expansion in ASC slots in the different districts in my sample by regressing the change in the distance to the nearest ASC between the first and last observed years on the initial average distance and baseline district characteristics. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SOEP v35 and administrative school data, own calculations.

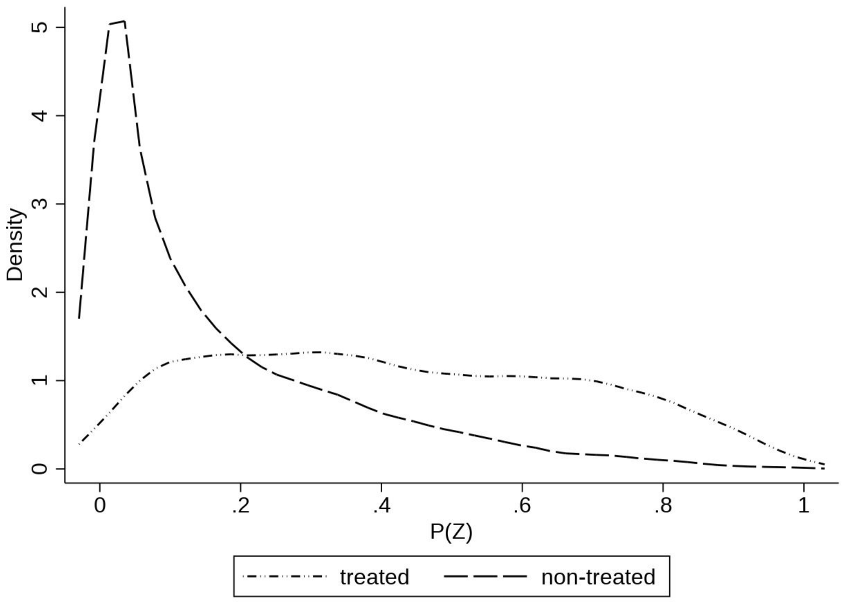
Table A4: Balancing Test: Individual determinants of ASC expansion

	Individual characteristics
Distance in first year	0.820***
	(0.043)
Social transfer	-0.014
	(0.120)
Single parent	-0.162
	(0.129)
Younger siblings	0.031
	(0.057)
At least technical degree	-0.002
	(0.069)
Academic degree	-0.086
	(0.097)
Migration background	0.089
	(0.075)
Log income	0.045
	(0.081)
Both parents work	-0.013
	(0.074)
Number of observations	3632

Notes: The reported estimates are derived by regressing the change in the average distance to the nearest ASC in the child's district between the first and last observed year on individual control variables. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v35 and administrative school data, own calculations.

Figure A.3: Common support graph



Notes: The figure plots the frequency distribution of the propensity score by treatment status. The propensity score is predicted from the baseline first-stage regression.
Source: Own calculations based on the SOEP v35 and administrative school data, 2003-2018.

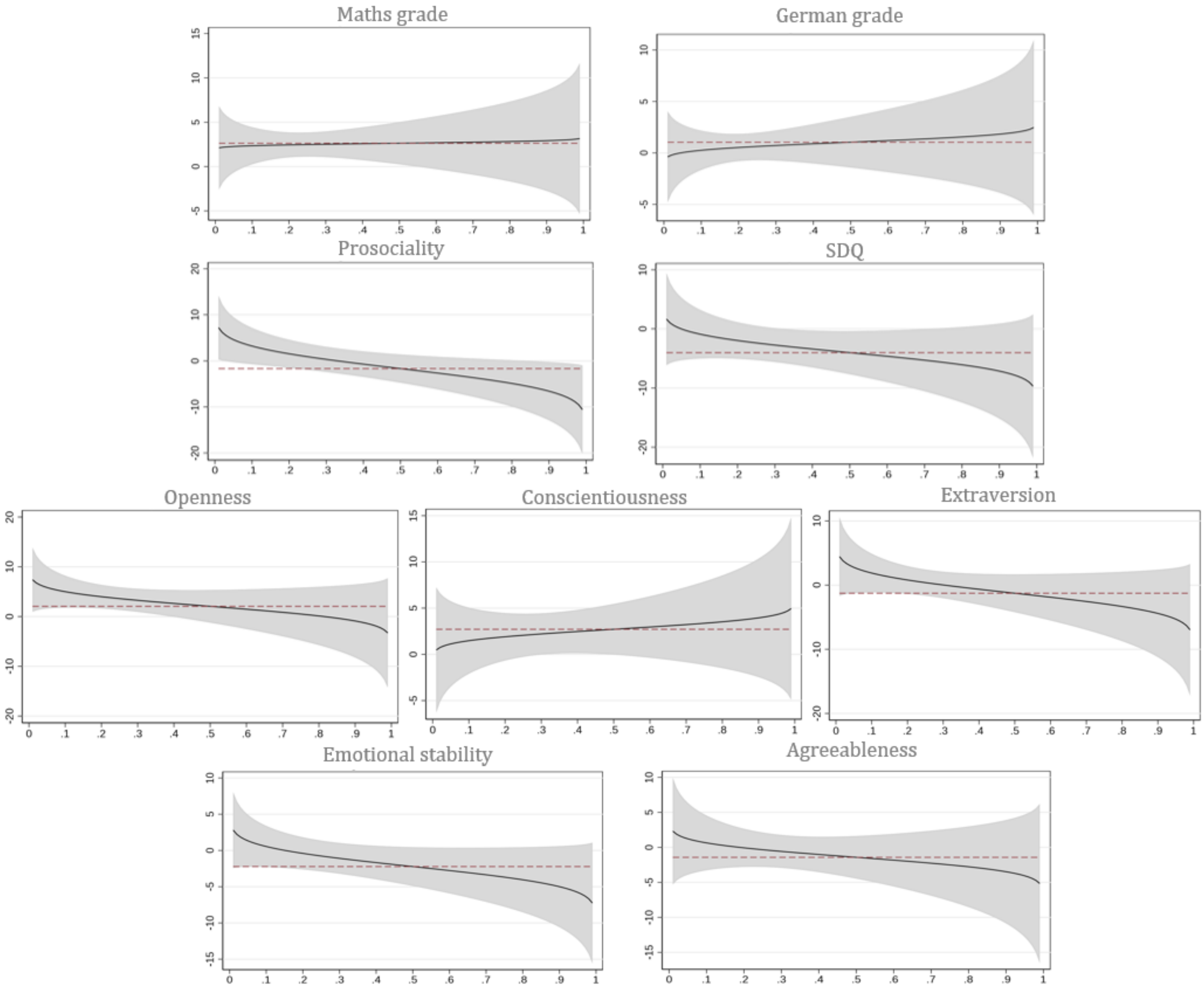
Table A5: T-test of differences in outcome variables between children attending ASC and children who do not

Outcome variables	Treatment	Control	△	t
Math grade (reversed)	3.58	3.68	-0.10***	(-4.26)
German grade (reversed)	3.52	3.62	-0.11***	(-4.95)
Prosociality	8.24	8.22	0.01	(0.33)
SDQ Scale (reversed)	28.93	30.18	-1.25***	(-7.70)
Openness	14.52	14.96	-0.44***	(-4.32)
Conscientiousness	10.45	10.64	-0.19	(-1.62)
Extraversion	14.75	14.95	-0.20*	(-2.07)
Agreeableness	12.71	12.79	-0.08	(-0.83)
Emotional stability	12.88	12.92	-0.03	(-0.30)

Notes: T-test of background characteristics between treatment and control group for the full sample. Definition of Treatment: Having attended afternoon programs for most of elementary school in elementary school. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: SOEP v35 and administrative school data, own calculations.

Figure A.4: MTE curves: joint normal model



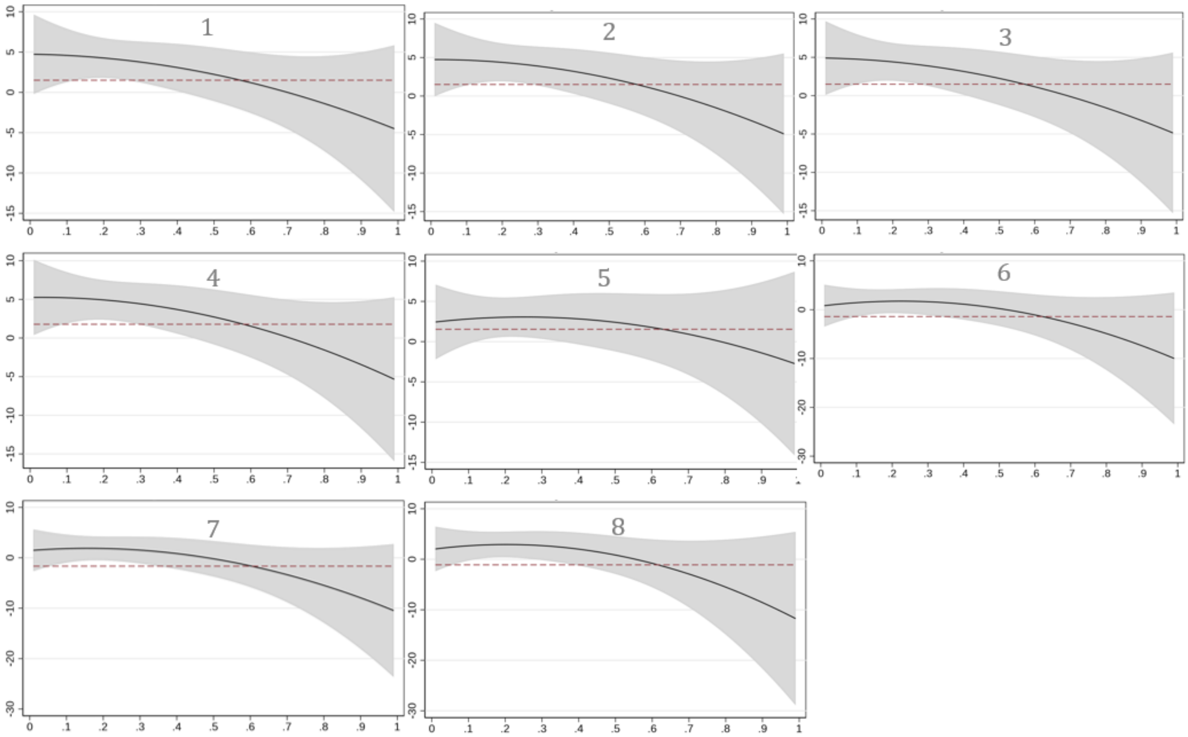
Notes: The figure plots the MTE curves (expression $\sum_{k=2}^{K} \alpha_k \hat{p}^k$ in eq. [9] for my outcome variables evaluated at mean values of the covariates. The underlying functional form is the joint normalmodel. The 90 percent confidence interval is based on standard errors clustered at the district level.
Source: Own calculations based on SOEP v35 and administrative school data, 2003-2018.

Table A6: Robustness check: ATE, TT, and TUT for main outcomes

Prosociality	Baseline	P(Z) $\leq$ 0.95	P(Z) $\leq$ 0.9	clustering
ATE	-0.230	-0.025	-0.642	-0.230
	(1.673)	(1.762)	(1.835)	(1.947)
ATT	1.754*	1.786*	1.463	1.754*
	(0.905)	(0.914)	(0.926)	(0.910)
ATUT	-1.056	-0.774	-1.506	-1.056
	(2.238)	(2.359)	(2.475)	(2.670)
LATE	-0.002	-0.001	-0.005	-0.002
	(0.005)	(0.005)	(0.005)	(0.005)
SDQ	Baseline	P(Z) $\leq$ 0.95	P(Z) $\leq$ 0.9	clustering
ATE	-0.481	-0.730	-0.703	-0.481
	(1.583)	(1.668)	(1.563)	(2.541)
ATT	1.797**	1.820**	2.108**	1.797**
	(0.884)	(0.901)	(0.933)	(0.853)
ATUT	-1.414	-1.768	-1.841	-1.414
	(2.018)	(2.124)	(1.958)	(3.507)
LATE	-0.002	-0.002	-0.001	-0.002
	(0.007)	(0.007)	(0.008)	(0.007)
Openness	Baseline	P(Z) $\leq$ 0.95	P(Z) $\leq$ 0.9	clustering
ATE	1.511	1.549	0.645	1.511
	(1.602)	(1.571)	(1.803)	(1.723)
ATT	1.803*	1.738*	1.946**	1.803*
	(0.934)	(0.948)	(0.972)	(0.999)
ATUT	1.397	1.479	0.118	1.397
	(2.125)	(2.081)	(2.418)	(2.342)
LATE	0.001	0.001	0.000	0.001
	(0.004)	(0.003)	(0.004)	(0.004)
Extraversion	Baseline	P(Z) $\leq$ 0.95	P(Z) $\leq$ 0.9	clustering
ATE	-0.350	-0.905	-2.091	-0.350
	(1.613)	(1.735)	(1.932)	(1.791)
ATT	2.252**	2.344***	2.252***	2.252**
	(0.874)	(0.881)	(0.858)	(1.076)
ATUT	-1.436	-2.250	-3.875	-1.436
	(2.222)	(2.386)	(2.630)	(2.455)
LATE	0.012***	0.012***	0.008*	0.012***
	(0.004)	(0.004)	(0.005)	(0.004)
Emotional stability	Baseline	P(Z) $\leq$ 0.95	P(Z) $\leq$ 0.9	clustering
ATE	-1.207	-1.295	-1.405	-1.207
	(1.277)	(1.351)	(1.299)	(1.577)
ATT	1.453*	1.442*	2.384***	1.453
	(0.823)	(0.828)	(0.835)	(0.949)
ATUT	-2.313	-2.424	-2.954*	-2.313
	(1.695)	(1.804)	(1.723)	(2.212)
LATE	-0.005	-0.006	-0.008*	-0.005
	(0.004)	(0.004)	(0.005)	(0.004)

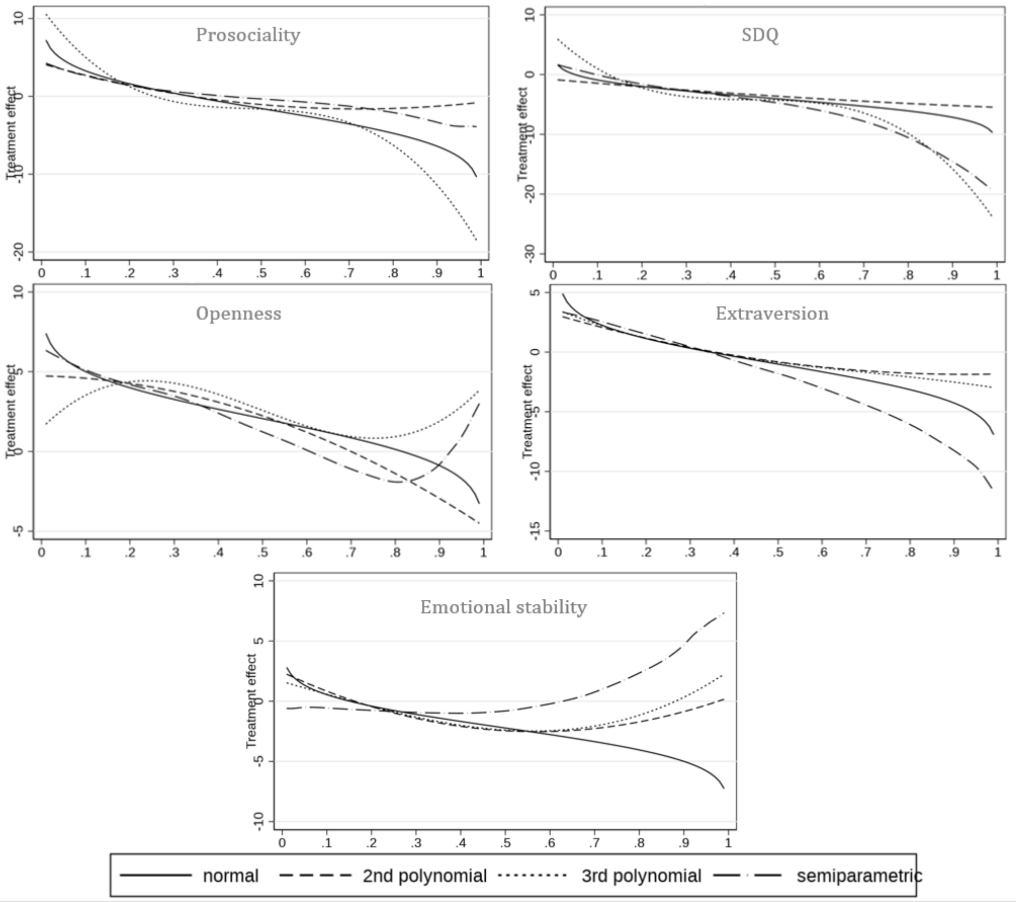
Notes: The table reports the average treatment effect (ATE), the treatment effect on the treated (TT), the treatment effect on the untreated (TUT), the local average treatment effect (LATE) and the p-value for the test of essential heterogeneity for alternative specifications of the outcomes with statistically significant results in the baseline specification. The different columns show the results of the estimations when limiting the range of P(Z) to $0 < P(Z) < 0.95$ (Column 2) and $0 < P(Z) < 0.9$ (Column 3), and when clustering on the individual level (Column 4). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source*: SOEP v35 and administrative school data, own calculations.

Figure A.5: MTE curves for openness: varying use of control variables



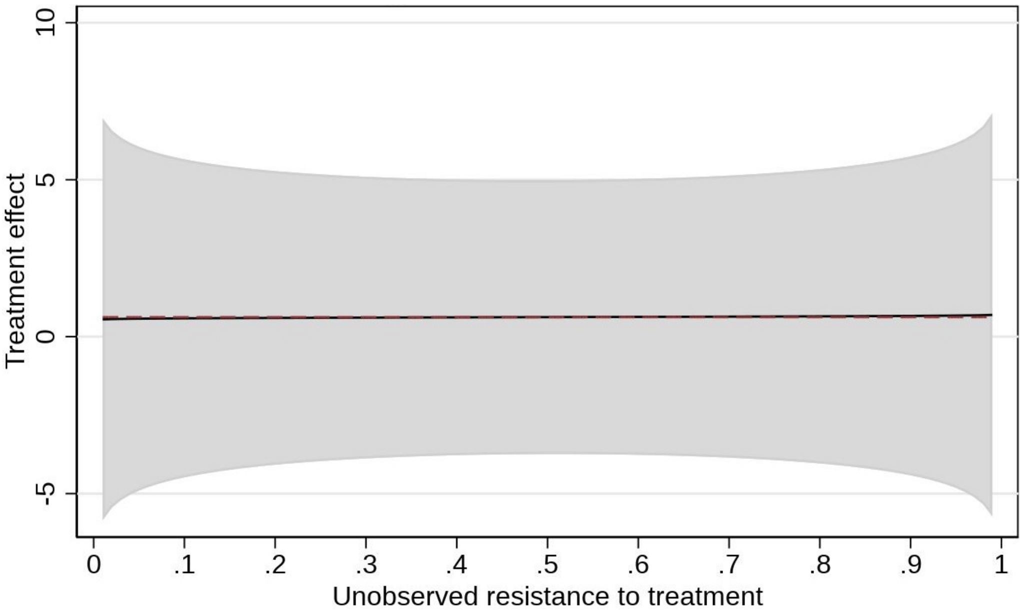
Notes: The figure displays MTE curves for the outcome of openness using a decreasing amount of control variables. While panel (1) uses the full control set, the following graphs plot the MTE curve when consecutively leaving out migration background (2), the latter plus household income (3), the latter plus amount of younger siblings (4), the latter plus the dummy on whether only one parent is present in the household (5), the latter plus whether the child additionally visits a *Hort* (6), the latter plus parental education (7), the latter plus whether or not the household receives social transfers (8). Source: Own calculations based on SOEP v35 and administrative school data, 2003-2018.

Figure A.6: MTE curves: alternative functional forms



Notes: The figure displays MTE curves for the outcomes for which my baseline specification —using a second polynomial —found statistically significant essential heterogeneity, i.e., prosociality, the SDQ, openness, agreeableness and emotional stability. The solid MTE curve refers to the joint normal model specification, the widely and finely dashed lines show the pattern of the MTE curves obtained by using a square and cubic of the propensity score, respectively, and the larger dashed line corresponds to the MTE curve resulting from a semiparametric approach.
Source: Own calculations based on SOEP v35 and administrative school data, 2003-2018.

Figure A.7: MTE curve of SDQ prior to school entry



Notes: The figure displays the MTE curve for the placebo outcome SDQ score at age five. Source: Own calculations based on SOEP v35 and administrative school data, 2003-2018.

CHAPTER 3

Sozioökonomische Zusammensetzung der Schülerschaft an Privatschulen: Wie viel erklärt die geografische Verteilung privater Schulangebote?[1]

3.1 Einleitung

In Deutschland ist es in den letzten Jahrzehnten zu einem deutlichen Anstieg der Privatschulen gekommen. Die Zahl der allgemeinbildenden privaten Schulen ist zwischen 1992 und 2020 um 86 Prozent gestiegen (Statistisches Bundesamt, 2021b). Dieser Anstieg ist zu fast der Hälfte auf die ostdeutschen Bundesländer (inklusive Berlin) zurückzuführen (Statistisches Bundesamt, 2021b). Während dort Privatschulen vor der Wiedervereinigung noch verboten waren, ist der Anteil der PrivatschülerInnen mit elf Prozent mittlerweile höher als in Westdeutschland, wo knapp neun Prozent der SchülerInnen Privatschulen besuchen.[2]

Ein häufig in Wissenschaft und Medien diskutierter Aspekt des Privatschulausbaus bezieht sich auf die sozioökonomische Zusammensetzung an Privatschulen und deren Implikationen für die Bildungsungleichheit in Deutschland. Auf privaten Schulen finden sich seltener Kinder aus einkommensschwachen Haushalten (Helbig et al., 2017b; Wrase & Helbig, 2016) und dafür häufiger Kinder, deren Eltern einen höheren beruflichen Status (Klemm et al., 2018) oder einen höheren Bildungsabschluss aufweisen (Görlitz et al., 2018; Jungbauer-Gans et al., 2012). Die Abhängigkeit des Privatschulbesuchs

[1] This chapter is joint work with Marcel Helbig (WZB, LIfBi and University of Erfurt). Since it is tailored to a German audience, it is written in German. We are grateful to C. Katharina Spieß, Felix Weinhardt and participants of the JuBilA Conference 2023 for helpful comments and feedback to earlier drafts.

[2] Diese Zahlen gehen auf eigene Berechnungen auf Basis der Daten den Sozio-oekonomischen Panel (v.36) zurück.

vom sozioökonomischen Status hat sich überdies insbesondere in Ostdeutschland mit der Zeit verstärkt (Görlitz et al., 2018; Lohmann et al., 2009).

Die deutsche Privatschullandschaft ist sehr heterogen aufgestellt. Private Ersatzschulen[3] sind in Deutschland zu einem großen Teil konfessionelle Schulen, gefolgt von Schulen mit reformpädagogischer Ausrichtung wie Waldorf- oder Montessorischulen. Außerdem existieren internationale Schulen und andere weltanschauliche Privatschulen (Klemm et al., 2018). In Deutschland stehen Privatschulen unter Aufsicht des Staates und müssen sich somit an dieselben Prüfungs- und Versetzungsordnungen halten wie öffentliche Schulen. Sie erhalten staatliche Beihilfe und können Schulgelder erheben; zudem soll der Schulträger einen Eigenanteil an der Finanzierung leisten. Die staatlichen Zuschüsse für Privatschulen sind daher niedriger als für die öffentlichen Schulen (Akkaya et al., 2019).

Bei der Auswahl der SchülerInnen müssen sich Schulen in freier Trägerschaft an das „Sonderungsverbot“ halten, nach dem „keine Sonderung nach den Besitzverhältnissen der Eltern“ gefördert werden darf (GG Art. 7 Abs. 4)[4]. Privatschulen dürfen demnach zwar generell ein Schulgeld verlangen, müssen dieses aber entweder nach dem Einkommen der Eltern beziehungsweise Sorgeberechtigten staffeln oder so niedrig ansetzen, dass es theoretisch von allen Eltern gezahlt werden kann (Köppe, 2012). Allerdings ist umstritten, inwieweit das Sonderungsverbot genaue Regelungen für die Schulgeldordnungen vorschreibt, sodass es in den Bundesländern sehr unterschiedlich ausgelegt wird (Helbig & Wrase, 2017).

In Rheinland-Pfalz gibt es beispielsweise ein Schulgeldverbot (außer an freien Waldorfschulen). In Nordrhein-Westfalen werden die Zuschüsse entsprechend nach unten korrigiert, wenn Privatschulen Schulgelder verlangen (Helbig & Wrase, 2017). Im Saarland existiert eine ähnliche Regelung wie in Nordrhein-Westfalen (Akkaya et al., 2019). Diese drei Bundesländer bilden also insofern eine Ausnahme, indem es hier eine rechtliche (Rheinland-Pfalz) beziehungsweise faktische (Nordrhein-Westfalen und Saarland) Schulgeldfreiheit gibt.[5] In anderen Bundesländern gibt es teilweise keine Begren-

[3]Neben privaten Ersatzschulen existieren noch private Ergänzungsschulen. Der Besuch einer Ersatzschule ersetzt im Gegensatz zu dem einer Ergänzungsschule den einer öffentlichen Schule (Deutscher Bundestag, 2007). Private Ersatzschulen bedürfen einer staatlichen Anerkennung und sind somit der staatlichen Aufsicht unterworfen (Klemm et al., 2018). Im vorliegenden Beitrag ist ausschließlich von privaten Ersatzschulen die Rede.

[4]Grundgesetz (GG), Artikel 7, Absatz 4 (online verfügbar, abgerufen am 13.12.2022).

[5]Es ist nicht der Fall, dass Eltern in diesen Ländern keine Beiträge für den Privatschulbesuch zahlen. Dies soll allerdings freiwillig z.B. über Fördervereine geschehen, oder in Nordrhein-Westfalen über einen sogenannten Beitrag für die Trägereigenleistung. Diese ist eine wichtige Säule der Privatschulfinanzierung, die rund 10 Prozent des Gesamtbudgets von Privatschulen ausmachen soll (Akkaya et al., 2019). Inwieweit in einzelnen privaten Schulen in diesen Ländern diese Beiträge rechtswidrig als obligatorisch anfallen, wurde empirisch bisher nicht untersucht.

zungen oder Richtwerte zum Schulgeld (z.B. Berlin, Hessen und Thüringen), oder es werden Richtwerte für ein durchschnittliches Schulgeld (z.B. Baden-Württemberg und Schleswig-Holstein) oder für Obergrenzen festgesetzt (z.B. Bayern und Hamburg). Die Streuung der veranschlagten Schulgelder ist dabei bezüglich der rechtlichen Vorgaben (Helbig & Wrase, 2017), als auch der real erhobenen Schulgelder groß (Statistisches Bundesamt, 2022). Laut Berechnungen des Statistischen Bundesamtes wurden im Jahr 2016 für 57 Prozent der Schülerinnen und Schüler an Privatschulen Schulgelder steuerlich geltend gemacht. Die restlichen 43 Prozent besuchten zumindest teilweise kostenfreie Privatschulen.[6] Diejenigen Familien, die Schulgelder steuerlich geltend machten, bezahlten im Durchschnitt 167 Euro pro Monat (Statistisches Bundesamt, 2020a).

Prinzipiell können Privatschulen aus bildungsökonomischer Sicht für einen größeren Wettbewerb sorgen und somit im besten Fall auch die Qualität auch an öffentlichen Schulen steigern (z.B. Woessmann, 2007). Je nach Ausgestaltung – an welche Regeln sich diese Schulen halten müssen, wie sie finanziert werden und wie zugänglich sie für verschiedene Schülergruppen sind – kann ein wachsender Privatschulsektor aber auch zu einer steigenden Segregation der Schülerschaft beitragen.[7]

Weitgehend ungeklärt ist bisher, welche Mechanismen trotz des Sonderungsverbots zur beschriebenen sozioökonomischen Zusammensetzung der Schülerinnen und Schüler auf Privatschulen führen. Dies gilt insbesondere für strukturelle Faktoren wie der räumlichen und sozialräumlichen Verteilung von privaten Schulen.[8] Die Distanz zur nächstgelegenen (Hoch-)Schule gilt generell als wichtiger Faktor für deren Besuch, da verkürzte Wege die zeitlichen und finanziellen Kosten des (Hoch-)Schulbesuchs verringern und Nachbarschaftseffekte für mehr Informationen sorgen (z.B. Card, 1993; Dee, 2004; Do, 2004; Helbig et al., 2017a; Jepsen & Montgomery, 2009; Spiess & Wrohlich, 2010). Wenn sich private Schulen vorwiegend dort befinden, wo Kinder aus privilegierten Verhältnissen - Haushalte, die etwa über ein hohes Bildungsniveau und/oder ein überdurchschnittliches Einkommen verfügen - wohnen, dann könnten die sozioökonomischen Ungleichheiten bei der Privatschulwahl zumindest teilweise auf die räumliche Verteilung von Schulen und Kindern zurückzuführen sein.

Bei der räumlichen Verteilung von Privatschulen sind Unterschiede anhand dreier Dimensionen denkbar: Stadt-Land Unterschiede, Unterschiede innerhalb von Städten

[6] Hier ist von einer gewissen Untererfassung auszugehen, da nicht alle Eltern eine Steuererklärung abgeben.

[7] Die Tatsache, dass der Besuch einer Privatschule mit sozioökonomischen Merkmalen zusammenhängt, belegt dies jedoch noch nicht. Die Bildungssegregation insgesamt hängt auch davon ab, welche staatlichen Schulen die Kinder, die auf Privatschulen sind, sonst besucht hätten. Denn auch zwischen staatlichen Schulen und Nachbarschaften gibt es zum Teil starke Segregation.

[8] Sozialräumlich meint hierbei die räumliche Verteilung, die sich anhand der sozialen Charakteristika der Bewohnerschaft unterscheidet.

und regionale Unterschiede. Für die ersten beiden Dimensionen finden sich einige regionalspezifische Belege für ostdeutsche Bundesländer. So zeigt beispielsweise die am Wissenschaftszentrum Berlin (WZB) erarbeitete Schulenkarte[9], dass sich private Gymnasien in Thüringen primär im städtischen Raum finden, insbesondere in Erfurt, Weimar und Jena - wo auch das Bildungsniveau deutlich über dem der Thüringer Landkreise liegt (Helbig et al., 2020). Zu ähnlichen Befunden kommen Helbig et al. (2018) auch für weitere ostdeutsche Bundesländer. In Bezug auf die zweite Dimension gibt es ebenfalls Indizien für eine räumlich ungleiche Verteilung von Privatschulen in einzelnen ostdeutschen Städten. So findet Mayer (2017), dass Privatschulen in Berlin sich vorrangig in sozioökonomisch privilegierteren Stadtteilen befinden. Für Erfurt zeigen Helbig & Mayer (2023), dass Kinder aus Haushalten mit akademischem Abschluss unterdurchschnittliche Wege zu den privaten Grundschulen haben.

Inwieweit diese Muster auch für andere ost- und westdeutsche Städte und Regionen zutrifft, ist empirisch bisher nicht untersucht worden. Im vorliegenden Beitrag wollen wir die Rolle der räumlichen Verteilung für die Zusammensetzung an privaten Schulen systematisch für ganz Deutschland sowie separat für West- und Ostdeutschland untersuchen. Hierbei spielen zwei Aspekte eine Rolle: Erstens, wo sich Privatschulen und Haushalte ansiedeln, und zweitens, ob – und für welche Gruppen im Besonderen – die Entfernung eine Rolle bei der Schulwahl spielt. Für die Untersuchung der aufgeworfenen Fragestellung verknüpfen wir Daten zu den Standorten aller allgemeinbildenden Schulen in Deutschland seit dem Jahr 2000 nach Bildungsgang und Trägerschaft mit den georeferenzierten Daten des Sozioökonomischen Panel (SOEP), welches Angaben über die Trägerschaft der besuchten Schule der Kinder und zu der sozioökonomischen Lage der Haushalte liefert. Für die Untersuchung der Rolle der räumlichen Verteilung von Privatschulen für den Privatschulbesuch verwenden wir multivariate lineare Wahrscheinlichkeitsmodelle (siehe z.B. Wooldridge, 2010).

3.2 Theorie und Stand der Forschung

3.2.1 Individuelle Perspektive

In der bildungsökonomischen Literatur ist der Zusammenhang zwischen Distanz zu Bildungsinstitutionen wie Schulen und Hochschulen und deren Besuch theoretisch wie empirisch gut belegt: Bei der Entscheidung für eine Schule sind Eltern durch eine Budgetrestriktion in Form ihres Einkommens und ihrer Zeit beschränkt, da sie ihre zur

[9] Die Schulenkate ist online abrufbar unter `https://schulenkarte.wzb.eu/#karte;c=51.256,12.3267;z=6;y=2015;s=mit_gym_os`.

Verfügung stehenden Mittel auf verschiedene Güter aufteilen müssen (Chakrabarti & Roy, 2010; Do, 2004). Unter Abwägung der finanziellen und zeitlichen Kosten und des potenziellen Nutzens sind sie eher bereit, ein Bildungsangebot dann anzunehmen, wenn sich die Wegzeit und die damit verbundenen Kosten reduzieren.[10] In zahlreichen empirischen Studien (z.B. Card, 1993; Dee, 2004; Jepsen & Montgomery, 2009) dient daher die Distanz zu verschiedenen Bildungseinrichtungen als Instrumentalvariable (Angrist et al., 1996) für deren Besuch. Die beschriebene Logik lässt sich auch auf den Besuch einer Privatschule anwenden, wofür es in der internationalen bildungsökonomischen Literatur einige Belege gibt (z.B. Fairlie & Resch, 2002; Lankford & Wyckoff, 1992). Unsere erste Hypothese (H1) lautet daher, dass eine Verringerung der Distanz zur nächsten Privatschule die Wahrscheinlichkeit, eine Privatschule zu besuchen, erhöht.

Bezüglich der „Distanzssensibilität“ bei der Privatschulwahl – wie sehr die Entfernung zur nächsten Privatschule bei der Entscheidung für den Besuch einer Privatschule ins Gewicht fällt – würde man aus werterwartungstheoretischer Perspektive (Erikson, 1996; Helbig et al., 2017a) eher erwarten, dass sich ressourcenschwächere Haushalte stärker durch die Entfernung zur nächsten (Privat-)Schule von deren Besuch abhalten lassen, weil für sie die Kosten eines längeren Schulweges stärker ins Gewicht fallen. Dass dies bei der generellen Schulwahl der Fall ist, zeigt zum Beispiel eine Studie der OECD (2017a), laut derer die Distanz für Eltern von Kindern, die sozioökonomisch benachteiligte, ländliche und öffentliche Schulen besuchen, eine überdurchschnittliche Rolle bei der Schulwahl spielt. Zudem korreliert die Wichtigkeit, die Eltern der Distanz als Kriterium für die Schulwahl beimessen, negativ mit dem PISA Ergebnis des Kindes (OECD, 2017a). Das spricht dafür, dass generell die Entfernung bei der Schulwahl für sozioökonomisch benachteiligte Familien stärker ins Gewicht fällt.

Bei Privatschulen könnte aber ein anderer Mechanismus greifen, da diese die Rolle von Ersatzschulen zu den öffentlichen Schulen erfüllen und somit die Wahl einer privaten Schule eine aktive Entscheidung gegen die „Standardschule“ darstellt. Die Bildungsentscheidung kann hier als Abwägung von Kosten, erwarteten Erträgen und Erfolgswahrscheinlichkeit gesehen werden (Erikson, 1996; Helbig et al., 2017a)[11]. Wenn

[10] Ein weiterer Mechanismus könnte in sogenannten Nachbarschaftseffekten begründet sein (siehe Do, 2004, in Bezug auf die Nähe zu “high-quality colleges”). Hierbei führt die Präsenz einer Hochschule in der Nachbarschaft sowohl zu einem „peer-group effect“ - es wird in der Nachbarschaft als normal betrachtet, eine Universität zu besuchen - als auch zu einem „information network effect“ - in der Nachbarschaft wird sich vermehrt über den Besuch einer Universität ausgetauscht, weshalb mehr Informationen darüber zur Verfügung stehen. Dieses Mechanismus wäre auch für den Besuch von Privatschulen denkbar.

[11] Im Falle der Entscheidung für den Besuch einer Privatschule sollte die Erfolgswahrscheinlichkeit keine große Rolle spielen, da es sich bei der Wahl des Trägers im Allgemeinen nicht um eine Wahl von unterschiedlich anspruchsvollen Bildungsgängen handeln sollte. Man könnte allerdings auch argumentieren, dass die Erfolgswahrscheinlichkeit z.B. ein Abitur zu erwerben auf einem privaten

der Nutzen, der sich aus dieser Abwägung für eine Bildungsinstitution ergibt, insgesamt sehr niedrig ist, dann führen auch Veränderungen der Kosteneinschätzung, die mit der Entfernung zu dieser Institution einhergehen, nicht zu einer substanziellen Veränderung der Gesamtnutzenbewertung. Wenn einkommensschwache und/oder bildungsferne Haushalte den Nutzen eines Privatschulbesuchs insgesamt als niedrig einschätzen, weil sie die Erträge (z.B. Pädagogik oder soziales Umfeld) niedrig und die Kosten (z.B. Schulgeld) subjektiv hoch bewerten (z.B. Köppe, 2012) dann spielt die Entfernung zur nächsten Privatschule auch eine geringere Rolle. Bei sozioökonomisch privilegierteren Haushalten, in welcher die Erträge höher und die Kosten subjektiv niedriger angesehen werden, spielt die Entfernung zur nächsten Privatschule demnach eine wichtigere Rolle und die Entfernung zur nächsten Privatschule beeinflusst die Bildungsentscheidung deutlich stärker. Dies lässt sich auch bildungsökonomisch ableiten. Trotz der oben beschriebenen Entwicklung ist der Besuch einer Privatschule auch für privilegierte Gruppen nicht die Regel und dementsprechend könnten gerade sie es sein, die eher auf Veränderungen von finanziellen und zeitlichen Kosten reagieren.

Auch Kristen (2005) kann man in ihrem Schulwahlmodell so interpretieren, dass nicht alle Eltern bzw. Kinder durch die Entfernung zur nächsten (Privat-)Schule in ihrer Bildungsentscheidung beeinflusst werden. Anders als bei der vertikal unterschiedlichen Schulwahl, z.B. Gymnasium vs. Real- oder Hauptschule, gibt es viele Eltern, die bei der horizontalen Schulwahl – in diesem Fall nach Trägerschaft – gar keine Wahlentscheidung treffen. Nach Kristen (2003) besteht die Schulwahl aus einem dreistufigen Prozess: erstens der Wahrnehmung von Alternativen, zweitens der Bewertung dieser Alternativen, und drittens der Auswahl durch die Schule.

Weiter oben haben wir uns vor allem auf die Bewertung von Alternativen bezogen. Dabei haben wir auf der ersten Stufe außen vorgelassen, dass sich zunächst die Frage stellt, ob Eltern überhaupt Alternativen zur nächstgelegenen und/oder zugewiesenen Schule wahrnehmen. Hierbei zeigt sich, dass die Unterscheidung zwischen wählenden und nicht-wählenden Eltern mit dem sozioökonomischen Hintergrund verbunden ist. Helbig & Mayer (2023) zeigen für die private Grundschulwahl in Erfurt, dass rund 25 Prozent der Eltern von zukünftigen Grundschulkindern unterer Bildungsschichten nicht bewusst war, dass sie eine private Schule hätten wählen können. Bei Eltern mit akademischem Abschluss waren es hingegen nur zwei Prozent. Zudem gab ein substanzieller Teil der Eltern ohne höheren Bildungsabschluss an, sich eine private Schule finanziell nicht leisten zu können. Wenn ein Großteil der Eltern ohne höheren Bil-

Gymnasium steigt, weil hier die Lernbedingungen besser sind bzw. als besser eingeschätzt werden. Dies könnte man dann aber auch als einen Ertrag bewerten, der mit der privaten Bildungsinstitution einhergeht.

dungsabschluss entweder gar keine Bildungswahl trifft (Stufe 1), oder sich von den subjektiv wahrgenommenen finanziellen Kosten eines Privatschulbesuchs abschrecken lässt (Stufe 2), dann wird nur noch für die wenigen verbliebenen Eltern ohne höheren Bildungsabschluss die Entfernung zur nächsten Privatschule ein Entscheidungskriterium nach der Werterwartungstheorie sein.

So kommen auch Helbig & Mayer (2023) für den Fall Erfurt zu dem Ergebnis, dass die Entfernung zur nächsten privaten Grundschule für Akademikereltern besonders bedeutend für die Wahl einer Privatschule ist. Leben sie in der Nähe einer privaten Grundschule, ist die Wahrscheinlichkeit sehr hoch, dass sie sich an einer privaten Grundschule bewerben. Mit steigender Entfernung zur nächsten privaten Grundschule sinkt ihre Bewerbungsquote weit überdurchschnittlich. Untere Bildungsgruppen werden in dieser Studie in der Wahl einer Privatschule überhaupt nicht durch die Entfernung zur nächsten Privatschule beeinflusst. Ein ähnliches Muster könnte auch für Familien mit Migrationshintergrund gelten. Da die größten Privatschulträger eine konfessionelle Ausrichtung haben[12], kommt ein großer Teil der Privatschulen für die in Deutschland mehrheitlich muslimisch geprägten Haushalte mit Migrationshintergrund eher nicht in Frage. Auch Informationsdefizite könnten in dieser Gruppe aufgrund von Sprachbarrieren besonders ausgeprägt sein, weshalb davon auszugehen ist, dass migrantische Eltern eher die ihnen zugewiesene öffentliche Schule für ihre Kinder auswählen.

Angelehnt an diese Erkenntnisse gehen wir in unserer zweiten Hypothese (H2) davon aus, dass der Privatschulbesuch für privilegierte Gruppen stärker durch die Entfernung zur nächsten Privatschule beeinflusst wird als bei weniger privilegierten Gruppen. Ebenso gehen wir davon aus, dass auch Kinder mit Zuwanderungsgeschichte Privatschulen seltener zu ihren Wahlalternativen zählen und für sie deshalb die Entfernung zur nächsten Privatschule eine kleinere Rolle spielt.

Wenn es vor allem das subjektiv (zu) hoch eingeschätzte Schulgeld ist, das Privatschulen bei unteren Bildungsgruppen als keine Wahlalternative erscheinen lässt, dann könnte sich dieses Muster in Bundesländern ohne Schulgeld anders darstellen. Dementsprechend sollten sich Haushalte aus sozioökonomisch benachteiligten Verhältnissen in Kontexten ohne Schulgeld in ihren Bildungsentscheidungen stärker durch die Entfernung zur nächsten Privatschule beeinflussen lassen – da diese hier die größten (zeitlichen) Kosten wiederspiegelt. Diese Annahme prüfen wir anhand einer Subanal-

[12] Die größte Organisation, der Arbeitskreis Katholischer Schulen in freier Trägerschaft in der Bundesrepublik Deutschland (AKS), umfasst aktuell 904 allgemeinbildende Schulen und 360.000 SchülerInnen (Deutsche Bischofskonferenz, 2022). 632 allgemeinbildende Schulen werden von den Trägern des Arbeitskreis Evangelische Schule in Deutschland (AKES) getragen (Wissenschaftliche Arbeitsstelle Evangelische Schule, 2021).

yse der drei Bundesländer Rheinland-Pfalz, Nordrhein-Westfalen und dem Saarland, in denen eine rechtliche bzw. faktische Schulgeldfreiheit gilt.

3.2.2 Strukturelle Perspektive

Wie bereits eingangs erwähnt gibt es Belege dafür, dass sich private Schulen zumindest in manchen Gebieten sozialräumlich ungleich verteilen. Dies gilt sowohl für die unterschiedliche Verteilung von privaten Schulen zwischen Land und Stadt und zwischen verschiedenen Städten mit unterschiedlicher Sozialstruktur als auch innerhalb von Städten. Selbst wenn sich unterschiedliche sozioökonomische Gruppen nicht danach unterscheiden, ob sie eine private Schule besuchen wollen, so würden sich aus ihren unterschiedlichen Entfernungen zur nächsten Privatschule unterschiedliche Besuchsmuster ergeben. Dementsprechend lautet unsere dritte Hypothese (H3), dass die sozioökonomische Zusammensetzung an Privatschulen teilweise über die Entfernung zur nächsten Privatschule erklärt werden kann.

Inwieweit die sozialräumliche Verteilung privater Schulen tatsächlich soziale Ungleichheiten bei ihrem Besuch erklären kann, hängt zum einen davon ab, wie sich Privatschulen räumlich verteilen, also ob sie für sozial privilegierte Schichten besser erreichbar sind. Zum anderen spielt hier eine Rolle, ob die Entfernung zur nächsten Privatschule für alle sozioökonomischen Gruppen ein ähnlich wichtiges Kriterium für den Besuch einer privaten Schule darstellt. Aus finanzieller Sicht kann es für private Schulen sinnvoll sein, sich dort anzusiedeln, wo es genügend Schülerinnen und Schüler gibt, deren Eltern in der Lage bzw. bereit sind, die von ihnen verlangten Schulgelder zu zahlen. Auf diese zusätzliche Finanzierung sind Schulen in freier Trägerschaft insoweit angewiesen, als dass sie geringere staatliche Förderung erhalten als staatliche Schulen. Inwiefern es Privatschulen durch das Beziehen von Schulgeld gelingt, sich finanziell besser aufzustellen als öffentliche Schulen, hängt stark von den Gegebenheiten im jeweiligen Bundesland und Kreis ab (Akkaya et al., 2019; Statistisches Bundesamt, 2022).

In einigen Fällen könnte es für private Schulen – in Abhängigkeit von der Höhe der Schulgelder, die diese verlangen können und wollen – aber auch finanziell sinnvoll sein, sich in sozioökonomisch schwächeren Gegenden anzusiedeln. Zum einen sind hier im Schnitt die Mieten geringer, was die Schulen dazu befähigt, mehr Geld für Personal und Ausstattung ausgeben zu können. Zum anderen wäre es gerade im Fall von Großstädten mit hoher Zuzugsrate denkbar, dass sich Privatschulen in sozioökonomisch eher schwächeren Stadtteilen ansiedeln, um dort lebenden Besserverdienenden eine Alternative zur Einzugsschule zu bieten. Durch eine verstärkte Nachfrage an Wohnraum in einzelnen, innenstadtnahen Stadtteilen sind diese Einzugsschulen häufig multikulturell und sozial heterogen geprägt. Nahegelegene Privatschulen bieten in diesen Fällen eine Möglichkeit für ressourcenstärkere Eltern, sich der Zuweisung zur Einzugsschule zu entziehen (z.B. Akbarpour et al., 2022; Jähnen & Helbig, 2022).

Damit könnten Privatschulen auch dazu beitragen, dass solche einkommensstarken Familien überhaupt erst in sozioökonomisch schwächere Stadtteile ziehen. Generell beleuchtet der Zusammenhang zwischen der Distanz zu (Privat-)Schulen und sozioökonomischen Merkmalen neben dem Aspekt der - möglicherweise strategischen - Ansiedlung von Privatschulen auch das Umzugsverhalten von privaten Haushalten. Dass die Schulqualität für viele Familien ein wichtiges Kriterium für die Wahl ihres Wohnortes darstellt, wurde vielfach empirisch belegt (siehe z.B. Goyette et al., 2014; Oeltjen & Windzio, 2022).

Große Unterschiede bei der sozialräumlichen Verteilung von Privatschulen könnte es zwischen Ost- und Westdeutschland geben. Während zur Wiedervereinigung im Osten nur eine private Schule existierte, da in der DDR Privatschulen als Ersatz für öffentliche Schulen unzulässig waren[13], wurde der Großteil (rund 50 Prozent) der Schulen mit gymnasialer Oberstufe erst ab dem Jahr 2000 gegründet.[14] Die sehr dynamischen Strukturveränderungen in Ostdeutschland deuten auf der einen Seite darauf hin, dass die sozialräumliche Verteilung der Privatschulen in Ostdeutschland ausgeprägter sein könnte als in Westdeutschland, da die ostdeutschen Privatschulstandorte stärker nach sozialräumlichen Überlegungen gegründet worden sein könnten. Auf der anderen Seite kam es in Ostdeutschland gerade im Zuge der Schulschließungen in Folge des Geburtenknicks in den frühen 1990ern ab Anfang bis Mitte der 2000er Jahre zu einer Reihe von privaten Schulneugründungen (Statistisches Bundesamt, 2020a). Diese sind zumindest in den größeren Städten teilweise auch dort entstanden, wo es zuerst zu Schulschließungen kam. Dies waren im städtischen Bereich auch einige Schulen in den Großwohnsiedlungen, die eine sozial benachteiligte Schülerklientel aufweisen (siehe Kartenmaterial von Helbig et al. (2018)). In Westdeutschland wurden (anteilig) weit weniger Privatschulen neu gegründet als im Osten. Dementsprechend sollten auch neuere sozialräumliche Ungleichheiten im Westen weniger bedeutsam für die Verteilung von Privatschulen sein.

[13] Siehe Verfassung der Deutschen Demokratischen Republik [vom 7. Oktober 1949] Art. 38.

[14] In Ostdeutschland wurden beispielsweise 50 Prozent aller privaten Schulen mit gymnasialer Oberstufe erst ab dem Jahr 2000 gegründet. In Westdeutschland wurden gerade einmal 18 Prozent der Schulen mit gymnasialer Oberstufe ab dem Jahr 2000 gegründet. 37 Prozent existierten bereits vor 1949 (bei den katholischen Schulen sogar 74 Prozent) und weitere knapp 30 Prozent entstanden bis 1989 (siehe Akkaya, 2021)

Weitere Unterschiede bei der sozialräumlichen Verteilung von Privatschulstandorten, sowohl bei der Standortwahl, als auch bei der Wahl einer solchen, sind für die unterschiedlichen Bildungsgänge zu erwarten. Gerade im Grundschulbereich spielt die Wohnortnähe eine entscheidendere Rolle für die Schulwahl der Eltern als im Bereich der weiterführenden Schulen, da Kinder und Jugendliche auf weiterführenden Schulen eher in der Lage sind, für den Schulweg selbstständig größere Distanzen zurückzulegen.[15] Dementsprechend sollte sich gerade hier eine stärkere Bedeutung der Entfernung zur nächsten Privatschule für die Schulwahl zeigen. Zudem ist gerade an den Grundschulen, die alle Kinder gemeinsam besuchen, eine höhere soziale Segregation durch Privatschulen zu erwarten und empirisch nachgewiesen (Helbig et al., 2017b; Jungbauer-Gans et al., 2012; Klemm et al., 2018) als an den Gymnasien. Gerade hier könnte das Distinktionsbedürfnis für Eltern höherer Schichten größer sein als an Gymnasien, die ohnehin von sozioökonomisch benachteiligten Kindern und solchen mit Migrationshintergrund seltener besucht werden. Zudem ist denkbar, dass manche Eltern bei der Grundschulwahl besonders nach dem Angebot spezieller pädagogischer Konzepte entscheiden, da sie sich dort einen geringeren Leistungsdruck und eine bessere individuelle Betreuung für ihr Kind erhoffen (Kraul, 2017). Andererseits sieht Artikel 7 Abs. 5 GG zusätzliche Einschränkungen für die Privatschulgründung im Primarbereich vor, weshalb private Schulen im Grundschulbereich deutlich seltener zu finden sind als im Sekundarbereich.

3.3 Daten, Operationalisierung und Methode

3.3.1 Daten

Sozio-oekonomisches Panel

Die folgenden Analysen basieren auf den Daten des Sozio-oekonomischen Panel (SOEP Goebel et al., 2019). Das SOEP ist eine repräsentative Befragung privater Haushalte, die seit 1984 jährlich durchgeführt wird. Die Anzahl der Befragten pro Welle variiert von Jahr zu Jahr und ist im Laufe der Zeit angestiegen. Im letzten Beobachtungsjahr unserer Analyse, 2019, wurden etwa 30.000 Personen befragt.

Die verwendete Stichprobe umschließt alle Kinder im SOEP, die zwischen 2002 und 2019 eine allgemeinbildende Schule besuchten. Hierbei wurde auch die Trägerschaft der Schule erfragt, die jedes Kind im Haushalt besucht. In unserer Analyse unterscheiden

[15] Dies zeigt sich auch daran, dass der durchschnittliche Schulweg im Primarbereich kleiner ist als an weiterführenden Schulen (vgl. Neumeier, 2018)

wir zwischen Grundschulen, Gymnasien und nicht-gymnasialen Sekundarschulen, zu denen wir neben Haupt- und Realschulen sowie auch Gesamtschulen zählen.

Unsere abhängige Variable bildet die Trägerschaft der besuchten Schule. Da die Frage bezüglich des Schulträgers nur in den Jahren 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019 im SOEP abgefragt wurde, können in der Untersuchung nur Beobachtungen aus diesen Jahren genutzt werden. Ein Schüler oder eine Schülerin im SOEP wird in unseren Analysen dann als PrivatschülerIn kategorisiert, wenn die Antwort auf die Frage nach der Trägerschaft „privat“, „kirchlich“ oder „andere Privatschule“ oder ähnlich lautet.[16] Die Vergleichsgruppe bilden alle Schülerinnen und Schüler, deren Eltern die Frage mit „öffentlich“ beantworteten. Zwischen 2002 und 2019 wurde diese Frage N = 22,016 Mal beantwortet, wobei einige SchülerInnen mehrfach beobachtet werden. Insgesamt gibt es in unserem Sample 8,089 Individuen, für die diese Frage beantwortet wird. SchülerInnen über 15 Jahre werden von unseren Analysen ausgeschlossen, da ab diesem Alter meist die Vollzeitschulpflicht nicht mehr greift.

Mit den anonymisierten Regionalinformationen zu den Wohnorten der SOEP-Befragten (Haushalte und Einzelpersonen) lassen sich zahlreiche Regionalindikatoren auf den Ebenen der Bundesländer, Raumordnungsregionen, Kreise und Postleitzahlen mit den SOEP-Daten verknüpfen. Seit 2006 ist es möglich, die Wohnorte der Befragten bis auf die Koordinaten des Straßenblocks zurückzuführen (Goebel et al., 2019).

Amtliche Schuldaten

Die geo-referenzierten Daten des SOEP werden mit Adressdaten aller allgemeinbildenden Schulen (ohne Förderschulen und Abendschulen) der Schuljahre 2000/01 bis 2019/20 verknüpft. Diese wurden durch eine bundesweite Anfrage 2021 bei den Kultus- bzw. Bildungsministerien beantragt. Für einige Bundesländer liegen die Adressdaten erst für spätere Jahre vor: Baden-Württemberg seit 2002, Niedersachsen seit 2004, Bayern, NRW und Saarland seit 2005, Berlin und Hamburg seit 2010.

[16] Die von uns generierte Variable wertet die Information zur Trägerschaft für jedes Jahr, in dem diese abgefragt wurde, einzeln im SOEP wide-Format aus. Die Ankreuzmöglichkeiten unterscheiden sich über die Jahre leicht. Die Kontrollgruppe bildet immer die Gruppe der SchülerInnen in Schulen “öffentlicher Trägerschaft”. Die Gruppe der PrivatschülerInnen setzt sich je nach Jahr aus den Angaben “privat”, “kirchlich”, “kirchlich-gemeinnützig”, “privat-gemeinnützig”, “privat-gewerblich” und “private, gemeinnützige Elterninitiative” zusammen.

3.3.2 Operationalisierung

Durch das Hinzuspielen der Schul-Adressdaten erweitern wir das SOEP für unsere Analysen um folgende Variablen: Euklidische Distanzen in Kilometern zur nächstgelegenen (i) Grundschule, (ii) weiterführenden Schule mit gymnasialer Oberstufe, und (iii) weiterführende Schule ohne gymnasiale Oberstufe bzw. weiterführende Schule an der auch andere Bildungsabschlüsse als das Abitur erworben werden können (z.B. Gesamtschulen, Gemeinschaftsschulen), jeweils getrennt für private und öffentliche Schulen. Für die multivariaten Regressionen werden die Distanzen in logarithmierter Form verwendet. Diese Form verbessert die Anpassung des Modells, indem die Verteilung der Merkmale in eine normal verteilte Kurve transformiert wird (z.B. Benoit, 2011).

Neben den neu erzeugten Distanz-Variablen verwenden wir eine Reihe von sozioökonomischen Merkmalen als unabhängige Variablen, um ein möglichst umfangreiches Bild von den sozioökonomischen Determinanten eines Privatschulbesuchs zu zeichnen. Genauer werden folgende Variablen in Bezug auf ihre Rolle für den Privatschulbesuch betrachtet:

(1) Bildung der Eltern: Auch wenn gerade beim Privatschulbesuch oft damit argumentiert wird, dass sich finanziell benachteiligte Haushalte davon abhalten lassen, weil dieser zu teuer ist, so zeigen sich auch an Privatschulen mit keinem oder geringen Schulgeld deutliche soziale Ungleichheiten (Helbig et al., 2017b). In einer Untersuchung zu Waldorfschulen in Deutschland zeigten Koolmann et al. (2018), dass nicht das Einkommen der Eltern den Besuch einer Waldorfschule beeinflusst, sondern in hohem Maße ihre Bildung. Das zeigen auch Lohmann et al. (2009). Wir verwenden für die Bildung der Eltern den höchsten Berufsabschluss der Eltern in drei Kategorien: kein Abschluss (0), Ausbildung (1) oder Hochschulabschluss (2).

(2) Einkommen der Eltern: Darüber hinaus sollte der Privatschulbesuch, der in Deutschland in der Regel kostenpflichtig ist, auch durch das Einkommen der Eltern beeinflusst werden. Dies messen wir über das logarithmierte Haushaltsäquivalenzeinkommen.[17][18]

[17] Ähnlich wie im Falle der Distanzvariablen verbessert die Verwendung des Logarithmus in diesem Fall die Anpassung des Modells. Wenige sehr hohe Einkommen führen dazu, dass der Mittelwert der ursprünglichen Variable stark von deren Medianwert abweicht.

[18] Das Nettoäquivalenzeinkommen richtet sich nach der OECD-Äquivalenzskala, die durch die unterschiedliche Gewichtung von Haushaltsmitgliedern Haushalte unterschiedlicher Größe und Zusammensetzung vergleichbarer macht. Hierbei erhält die erste erwachsene Person im Haushalt ein Gewicht von 1. Weitere erwachsene Personen und Jugendliche über 14 Jahren erhalten ein Gewicht von 0,5 und Kinder unter 14 Jahren ein Gewicht von 0,3 (siehe z.B. Anyaegbu, 2010).

(3) Transferleistungsbezug: Gerade die Bezieher von Sozialleistungen haben keine Möglichkeit, Schulgeld zu bezahlen. Dementsprechend müsste es entsprechend Art. 7. Abs. 4 GG eine Vollermäßigung für diese Gruppe geben (Brosius-Gersdorf, Frauke, 2017; Cremer, 2019; Wrase & Helbig, 2016). Dies ist allerdings in vielen Bundesländern nicht rechtlich festgeschrieben (Wrase & Helbig, 2016) und empirisch zeigt sich, dass dies auch vielerorts nicht umgesetzt wird.[19] Die Variable Transferleistungsbezug entspricht eins, wenn ein Mitglied des Haushaltes staatliche Transferleistungen, also Arbeitslosengeld I oder II oder Sozialhilfe, empfängt.

(4) Auch der Migrationshintergrund ist für uns von Interesse, da aus anderen Studien (Görlitz et al., 2018) bereits bekannt ist, dass Schüler mit Migrationshintergrund seltener Privatschulen besuchen. Dies könnte neben sozioökonomischen auch kulturelle und religiöse Gründe haben, da ein großer Teil der Privatschulen in Deutschland konfessionelle Schulen sind. Wir definieren Migrationshintergrund als gegeben, wenn entweder das Kind selbst oder beide Elternteile außerhalb Deutschlands geboren sind.

(5) Urbanisierungsgrad: Hier unterscheiden wir in vier Gemeindegrößenklassen: solche mit bis zu 50.000 (1), 50.000-100.000 (2), 100.000-500.000 (3), und (4) solche mit über 500.000 EinwohnerInnen (4). Diese Einteilung nehmen wir zum einen vor, weil das SOEP nur in diesen Kategorien über die Zeit vergleichbar ist. Unter 50.000 EinwohnerInnen wäre noch eine feinere Differenzierung möglich. Hierbei ist aber die Zellenbesetzung der zwei möglichen Ausprägungen (unter 5.000 und 5.000 bis 50.000) zu klein, weswegen wir sie zusammengefasst haben.

Auf die Messung des sozioökonomischen Status im Haushalt verzichten wir, weil der ISEI, der dafür in den Sozialwissenschaften oftmals verwendet wird, konzeptionell auf der Bildung und dem Einkommen aufsetzt, welches in einem Beruf erzielt wird (Ganzeboom & Treiman, 1996). Zudem zeigen Jungbauer-Gans et al. (2012) in einer multivariaten Studie, dass der ISEI bei Kontrolle auf Bildung und Einkommen der Eltern keinen Einfluss auf den Privatschulbesuch in Deutschland hat.

Weitere Kontrollvariablen bilden das Geschlecht des Kindes, das Alter des Kindes und die Anzahl der Geschwister. Gerade Familien mit mehreren Kindern könnten eine geringere Privatschulbesuchsquote aufweisen, wenn die Geschwisterzahl nicht angemessen in den Schulgeldmodellen der Privatschulen Beachtung findet.

[19] So zeigen Wrase et al. (2017) und Helbig et al. (2020), dass es diese Vollermäßigung an vielen privaten Schulen in Hessen, Berlin und Thüringen nicht gibt. Ein weiterer Beleg für diese Vermutung, dass für Kinder im Transferleistungsbezug die Zugänglichkeit zu privaten Schulen unzureichend ermöglicht wird, zeigt sich an einem Gutachten für ein sozial verträgliches Schulgeld in Baden-Württemberg, das Transferleistungsbezieher explizit außen vor lässt.(Helbig & Wrase, 2017)

3.4 Methode

Einleitend wird zunächst die sozioökonomische Zusammensetzung an Privatschulen in Form einer T-Test Tabelle mit der an öffentlichen Schulen vergleichen.

Für die Untersuchung der Rolle sozialräumlichen Verteilung von Privatschulen für den Privatschulbesuch gehen wir daraufhin in Form von mehreren multivariaten linearen Wahrscheinlichkeitsmodellen (siehe z.B. Wooldridge, 2010)[20] zwei übergeordneten Fragen nach: Erstens, wo sich Privatschulen und Haushalte ansiedeln, und zweitens, ob – und für welche Gruppen im Besonderen – die Entfernung eine Rolle bei der Schulwahl spielt. In einem ersten Schritt untersuchen wir den Zusammenhang der Distanz zur nächsten Privatschule mit sozioökonomischen Merkmalen der Haushalte. Hierbei bildet die Distanz des Kindes i im Erhebungsjahr t die abhängige Variable, welche auf die sozioökonomischen Merkmale des Kindes X_{i_t}[21] regressiert werden (Gleichung 1). Neben den beschriebenen Kontrollvariablen verwenden wir regionale fixe Effekte auf Kreisebene (κ_k) sowie fixe Jahreseffekte (τ_t). Somit werden kreis- und jahrspezifische Eigenschaften bei der Analyse herausgerechnet. Hierbei gehen Beobachtungen aus den Jahren 2000-2019 in die Berechnungen mit ein, da die Distanz und die sozioökonomischen Merkmale X_{i_t} in diesem Zeitraum in jedem Jahr beobachtet werden. Um systematische Unterschiede in der sozialräumlichen Verteilung von Privatschulen bestmöglich zu erfassen, halten wir es für sinnvoll, die Daten aus allen zur Verfügung stehenden Jahren zu untersuchen.

$$Distanz_{it} = \alpha_0 + \alpha_1 X_{ikt} + \kappa_k + \tau_t + \mu_{ikt} \qquad (3.1)$$

Im zweiten Schritt soll die Frage beantwortet werden, welche sozioökonomischen Merkmale generell multivariat mit einem Privatschulbesuch im Zusammenhang stehen. Hierbei dient der Privatschulbesuch als abhängige Variable, welche wiederum auf die sozioökonomischen Haushaltsmerkmale regressiert werden (Gleichung 3.2). Für die Berechnungen dieser Gleichung (sowie die Folgemodelle 2b-2c) werden Daten aus den Jahren 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019 verwendet, da der Pri-

[20]Statt logistischen Wahrscheinlichkeitsmodellen haben wir uns für ein lineares Modell entschieden, da hier die Ergebnisse besser zu interpretieren sind und keine bedeutenden Unterschiede im Vergleich zu Logit oder Probit Modellen zu erwarten ist.

[21]Die Kontrollvariablen bestehen aus dem Alter und dem Geschlecht des Kindes, dem höchsten Bildungsabschluss im Haushalt (kein Abschluss, Ausbildung oder Hochschulabschluss), dem Haushaltsnettoäquivalenzeinkommen (welches die Größe und Zusammensetzung des Haushalts berücksichtigt), der Anzahl der Kinder im Haushalt, dem Migrationshintergrund (beide Elternzeile im Ausland geboren), der Gemeindegröße, dem Schultyp (Grundschule, Gymnasium oder weiterführende Schule ohne gymnasiale Oberstufe) und der Entfernung zur nächstgelegenen öffentlichen Schule.

vatschulbesuch im SOEP nur in diesen Jahren erfragt wird. Daher ist die Anzahl der Beobachtungen hier kleiner als bei 3.1.

$$Privatschule_{it} = \beta_0 + \beta_1 X_{ikt} + \kappa_k + \tau_t + \epsilon_{ikt} \tag{3.2}$$

Im dritten Schritt wird Gleichung 3.2 durch die Distanz zur nächsten Privatschule ergänzt. Damit soll untersucht werden, inwieweit sich der Privatschulbesuch durch die räumliche Verteilung der Privatschulen erklären lässt (Gleichung 3.3).

$$Privatschule_{it} = \delta_0 + \delta_1 Distanz_{ikt} + \delta_2 X_{ikt} + \kappa_k + \tau_t + \eta_{ikt} \tag{3.3}$$

Im letzten Modell 3.4 soll schließlich geprüft werden, ob die Rolle der Entfernung für den Privatschulbesuch mit ausgewählten sozioökonomischen Merkmalen Z_{it} – dem Haushaltseinkommen, Bildungsgrad der Eltern und dem Migrationshintergrund – im Zusammenhang steht. Dafür werden Interaktionsterme zwischen der Distanz und diesen drei Merkmalen zu Gleichung 3.3 hinzugefügt.

$$Privatschule_{it} = \pi_0 + \pi_1 Distanz_{ikt} + \pi_2 X_{ikt} + \kappa_k + \tau_t + \pi_3 (Distanz_{ikt} * Z_{ikt}) + \nu_{ikt} \tag{3.4}$$

Die Berechnungen beinhalten darüber hinaus Haushaltsgewichte und auf der Haushaltsebene geclusterte Standardfehler.[22]

3.5 Ergebnisse

3.5.1 Deskription

Deskriptiv und univariat zeigen sich statistisch signifikante Unterschiede in Bezug auf sozioökonomische Merkmale der SchülerInnen an Privatschulen im Vergleich zu öffentlichen Schulen (Tabelle 3.1): Kinder und Jugendliche auf Privatschulen kommen häufiger aus bildungsnahen und einkommensstärkeren Haushalten, haben im Schnitt weniger Geschwister, beziehen seltener Sozialtransfers und haben seltener einen Migrationshintergrund. Dabei sind die Unterschiede bei den sozialen Merkmalen wie erwartet auf den Gymnasien geringer als an den nicht-gymnasialen Schulformen (siehe Tabellen

[22]Die Standardfehler werden auf der Haushaltsebene geclustert, da auf dieser Ebene die Entscheidung für oder gegen eine Privatschule getroffen wird. Das Clustern auf dieser Ebene ist zudem deshalb sinnvoll, da Individuen in der multivariaten Analyse mehrfach in die Analysen mit eingehen können.

3.A.1-3.A.3 im Anhang). Bereits univariat zeigt sich die Bedeutung der geografischen Nähe: PrivatschülerInnen wohnen signifikant näher an Privatschulen und im Fall der weiterführenden Schulen (besonders an Gymnasien) auch etwas weiter von der nächsten öffentlichen Schule entfernt als Gleichaltrige, die öffentliche Schulen besuchen. Im Gemeindegrößenvergleich zeigt sich zudem, dass PrivatschülerInnen insbesondere im Fall von Grundschulen und weiterführenden Schulen ohne gymnasiale Oberstufe häufiger in städtischen, bevölkerungsreichen Gegenden wohnen.

Table 3.1: Differenz der sozio-ökonomischen Zusammensetzung zwischen öffentlichen und privaten Schulen: alle Schultypen

	Besuch einer			
	Öffentl. Schule	Privatschule	Differenz	
			b	t
Individuelle Eigenschaften				
Weiblich	0.48	0.54	-0.06***	(-5.00)
Alter (Jahre)	10.92	11.11	-0.20**	(-3.04)
Haushaltseigenschaften				
Keine Ausbildung	0.08	0.02	0.06***	(16.97)
Ausbildung	0.58	0.46	0.12***	(9.43)
Uniabschluss	0.34	0.52	-0.18***	(-14.62)
Anzahl der Kinder im Haushalt	2.33	2.28	0.05	(1.76)
Migrationshintergrund	0.23	0.12	0.11***	(12.85)
Netto-Haushaltsäquivalenzeinkommen	21952	29678	-7726***	(-4.31)
Sozialtransferbezug	0.19	0.09	0.10***	(13.40)
Distanz zu Privatschule (km)	8.97	5.81	3.16***	(21.19)
Distanz zu öffentl. Schule (km)	1.75	2.24	-0.48***	(-7.21)
Gemeindegröße				
Unter 50.000 Einwohner	0.29	0.22	0.07***	(7.14)
50.000-100.000 Einwohner	0.09	0.10	-0.00	(-0.67)
100.000-500.000 Einwohner	0.31	0.33	-0.02	(-1.50)
500.000 Einwohner u.m.	0.31	0.36	-0.05***	(-4.30)
N	20262	1754	22016	

Bemerkungen: Angaben in Spaltenprozenten (sofern nicht anders vermerkt). Aus Platzgründen verwenden wir in den Tabelen das generische Maskulinum. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: Sozio-oekonomisches Panel (SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet), amtliche Schuldaten für die Jahre 2000-2019; eigene Berechnungen.

3.5.2 Multivariate Analyse

Determinanten der Privatschulentfernung

In Tabelle 3.2 untersuchen wir zunächst multivariat, wie stark die einzelnen soziodemografischen Merkmale mit der Entfernung zur nächsten Privatschule zusammenhängen (Gleichung 3.1). Hierbei zeigt sich, dass in Westdeutschland einerseits Kinder aus Haushalten mit hohem Bildungsniveau und Einkommen eine unterdurchschnittliche Entfernung zur nächsten Privatschule aufweisen. Andererseits leben auch Kinder mit Migrationshintergrund näher an privaten Schulen als Gleichaltrige ohne Migrationshintergrund. In Ostdeutschland zeigt sich in Bezug auf Migrationshintergrund ein umgekehrtes Muster: hier leben Kinder mit Zuwanderungsgeschichte weiter entfernt von Privatschulen. Darüber hinaus zeigen sich in Ostdeutschland keine statistisch signifikanten Zusammenhänge der Entfernung zu privaten Schulen mit sozioökonomischen Merkmalen, was impliziert, dass Privatschulen in den neuen Bundesländern zufälliger verteilt sind als im Westen Deutschlands. Diese Befunde zeigen sich auch dann, wenn man nur Städte (ab 50.000 EinwohnerInnen) betrachtet (siehe Tabelle 3.A.5 im Anhang). Nach Schulformen aufgeschlüsselt haben Kinder aus höher gebildeten Haushalten in Westdeutschland unterdurchschnittliche Strecken zu privaten Grundschulen und Gymnasien zurückzulegen. Für Gymnasien ist dies auch in Ostdeutschland der Fall. Kinder mit Migrationshintergrund haben in Westdeutschland zu allen Privatschulformen eine geringere Distanz (siehe Tabelle 3.A.6 im Anhang).

Table 3.2: Sozioökonomische Determinanten der Entfernung zu Privatschulen

	Entfernung zu Privatschule		
	Bundesrepublik	Westdeutschland	Ostdeutschland
Eltern: Hochschulabschluss	-0.405**	-0.572***	0.300
	(0.170)	(0.188)	(0.344)
Kind: Migrationshintergrund	-0.672***	-0.744***	1.251*
	(0.197)	(0.199)	(0.739)
Log HH-Einkommen	-0.286*	-0.264	-0.449
	(0.155)	(0.169)	(0.376)
Sozialtransfer	-0.050	-0.037	-0.064
	(0.178)	(0.197)	(0.371)
Beobachtungen	60195	49145	11050

Bemerkungen: Die Regressionen enthalten Jahres-, Kreis- und Gemeindegrößen-fixe Effekte. Standardfehler geclustert auf Haushaltsebene. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: SOEP v36 und amtliche Schuldaten, Jahre 2000-2019, eigene Berechnungen.

Determinanten des Privatschulbesuchs und die Rolle der Entfernung

Tabelle 3 fasst die Befunde zu Gleichungen 3.2-3.4 jeweils für die Bundesrepublik und danach getrennt für West- und Ostdeutschland zusammen. M1, M4 und M7 beziehen sich auf Gleichung 2a und die Frage nach der generellen sozioökonomischen Zusammensetzung an Privatschulen. Hier zeigt sich, analog zu Tabelle 1 sowie der empirischen Literatur (z.B. Görlitz et al., 2018; Jungbauer-Gans et al., 2012), dass Kinder aus Familien mit hohem Bildungsniveau und hohem Einkommen sich signifikant häufiger in Privatschulen finden, während SchülerInnen mit Migrationshintergrund und aus Sozialtransferempfängerhaushalten seltener Schulen in freier Trägerschaft besuchen. Dabei wird deutlich, dass die Bildung der Eltern (insbesondere das Vorliegen eines Hochschulabschlusses) und das Einkommen den Privatschulbesuch im Osten stärker vorhersagen als im Westen. Lediglich der Sozialtransferbezug hängt nur im Westen negativ mit dem Privatschulbesuch zusammen. Bezüglich der Gemeindegröße lassen sich keine auffälligen Unterschiede beim Privatschulbesuch beobachten. Dies deckt sich mit den Analysen von (Köppe, 2012).

Nimmt man nun die Distanzen zur nächsten privaten sowie öffentlichen Schule in die Analyse mit auf (Gleichung 3.3 und M2, M5 und M8 in Tabelle 3.3), so zeigt sich einerseits, dass die Entfernung zur nächsten Privatschule generell ein signifikanter Faktor in der Entscheidung für den Besuch einer Privatschule darstellt. Die Distanz hat nach den Schätzergebnissen ein negatives Vorzeichen, weil mit einer größeren Entfernung die Wahrscheinlichkeit eines Privatschulbesuchs sinkt. Allerdings scheint die Distanz nur geringfügig die sozioökonomischen Determinanten des Privatschulbesuchs zu erklären, was sich daran zeigt, dass sich die Koeffizienten dieser sozioökonomischen Merkmale im Vergleich zu M1, M4 und M7 nur geringfügig verändern.

In Bezug auf Gleichung 3.4 (M3, M6 und M9 in Tabelle 3.3) zeigt sich, dass die Entfernung zur nächsten Privatschule nicht für alle sozioökonomischen Gruppen die gleiche Rolle für den Privatschulbesuch spielt. Für die gesamte Bundesrepublik zeigt sich zunächst, dass Kinder von Eltern mit Hochschulabschluss durch die Entfernung zur nächsten Privatschule stärker beim Privatschulbesuch beeinflusst werden. Dies zeigt sich am negativen und statistisch signifikanten Koeffizienten des Interaktionsterms Distanz*Hochschulabschluss. Für Kinder mit Migrationshintergrund ist ein gegenteiliges Muster zu beobachten: Für sie ist die Entfernung zur nächsten Privatschule beim Privatschulbesuch weniger wichtig als für Kinder ohne Migrationshintergrund.

Table 3.3: Sozioökonomische und geografische Determinanten des Privatschulbesuchs: Alle Schularten

	Gesamtdeutschland			Westen			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.033***	-0.025***		-0.032***	-0.026**		-0.042***	-0.012
		(0.005)	(0.009)		(0.006)	(0.010)		(0.009)	(0.028)
Log Distanz zu oeff. Schule		0.016***	0.016***		0.018***	0.018***		0.005	0.006
		(0.004)	(0.004)		(0.005)	(0.005)		(0.007)	(0.007)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
50.000-100.000 Einwohner	0.020	0.012	0.013	0.016	0.008	0.008	0.039	0.024	0.024
	(0.016)	(0.016)	(0.016)	(0.019)	(0.019)	(0.019)	(0.024)	(0.023)	(0.023)
100.000-500.000 Einwohner	0.008	0.000	0.001	0.003	-0.003	-0.004	0.033	0.019	0.024
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.032)	(0.031)	(0.031)
500.000 Einwohner o.m.	0.009	-0.002	-0.002	0.006	-0.004	-0.004	0.014	-0.005	-0.005
	(0.017)	(0.017)	(0.017)	(0.019)	(0.019)	(0.019)	(0.025)	(0.026)	(0.026)
(0.003)									
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.016	0.017	0.030	0.019*	0.020*	0.043**	0.009	0.001	0.016
	(0.011)	(0.010)	(0.019)	(0.011)	(0.011)	(0.020)	(0.037)	(0.038)	(0.055)
Hochschulabschluss	0.050***	0.047***	0.079***	0.047***	0.044***	0.063***	0.078**	0.069*	0.201***
	(0.013)	(0.012)	(0.022)	(0.013)	(0.013)	(0.023)	(0.039)	(0.041)	(0.062)
Migrationshintergrund Kind	-0.029***	-0.028***	-0.053***	-0.028***	-0.028***	-0.056***	-0.084**	-0.080**	-0.120*
	(0.010)	(0.009)	(0.017)	(0.010)	(0.010)	(0.019)	(0.037)	(0.036)	(0.062)
Log HH-Einkommen	0.022**	0.021**	0.016	0.017*	0.015*	0.008	0.058***	0.059***	0.067***
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.011)	(0.022)	(0.022)	(0.025)
Sozialtransfer	-0.024***	-0.021***	-0.020***	-0.024***	-0.021**	-0.020**	-0.005	-0.008	-0.008
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.017)	(0.017)	(0.016)
Distanz * Ausbildung			-0.009			-0.014			-0.003
			(0.009)			(0.009)			(0.028)
Distanz * Hochschulabschluss			-0.021**			-0.012			-0.074**
			(0.010)			(0.011)			(0.030)
Distanz * Migrationsh.			0.016**			0.018**			0.029
			(0.007)			(0.008)			(0.032)
Distanz * hohes Einkommen			0.003			0.005			-0.013**
			(0.004)			(0.004)			(0.006)
Beobachtungen	22016	22016	22016	18330	18330	18330	3686	3686	3686

Anmerkungen: Die Regressionen enthalten Jahres- und Kreis-fixe Effekte. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Betrachtet man West- und Ostdeutschland getrennt, zeigen sich abweichende Muster. In Westdeutschland (M6) spielt die Entfernung zur nächsten Privatschule für Kinder mit Migrationshintergrund eine kleinere Rolle als für Kinder ohne Migrationshintergrund. Kinder aus hochgebildeten und einkommensstarken Haushalten unterscheiden sich in ihrer Entfernungssensibilität nicht von anderen Gruppen. Bei der Betrachtung nach Land und Stadt, sowie nach den einzelnen Schulformen (siehe Tabellen 3.A.7-3.A.11 im Anhang), zeigt sich aber auch für Grundschulen und nicht-gymnasiale Sekundarschulen sowie Schulen im ländlichen Raum in Westdeutschland, dass hoch gebildete Eltern eher durch die Entfernung zur nächsten Privatschule beeinflusst werden.

In Ostdeutschland zeigt sich, dass insbesondere für bildungsnahe und einkommensstarke Haushalte die Entfernung zur nächsten Privatschule stark mit dem Privatschulbesuch assoziiert ist. Jedoch sind die Interaktionsterme nicht über alle Schularten statistisch signifikant – was aber auch mit den kleineren Fallzahlen zusammenhängen könnte (siehe Tabellen 3.A.7-3.A.11 im Anhang).

Schulformspezifische Unterschiede

Abschließend sei noch auf ein paar Besonderheiten unserer Analysen hingewiesen, wenn man die Schulformen einzeln betrachtet (Tabellen 3.A.9-3.A.11 im Anhang). So zeigen sich die größten sozioökonomischen Differenzen beim Privatschulbesuch bei den Haupt-, Real- und Gesamtschulen. Dies liegt sicherlich auch daran, dass sich unter den privaten Schulen dieser Schulformen häufiger Schulen mit einer gymnasialen Oberstufe befinden als im öffentlichen Schulsystem, etwa Gesamtschulen oder Gemeinschaftsschulen mit gymnasialer Oberstufe. Bezüglich der Privatschulentfernung hängt diese nur für die westdeutschen Grundschulen nicht signifikant mit dem Privatschulbesuch zusammen. Verzichtet man bei diesen Analysen auf alle Befragten aus Nordrhein-Westfalen, dann zeigt sich auch für die westdeutschen Grundschulen ein signifikanter Entfernungseffekt für die privaten Grundschulen (nicht gezeigt).[23]

[23] Nordrhein-Westfalen ist bei den privaten Grundschulen ein Sonderfall. Hier gibt es nur wenige private Grundschulen, weil die kirchlichen Bekenntnisschulen, die es in Nordrhein-Westfalen in großer Zahl gibt, in staatlicher Trägerschaft sind. Zudem gibt es in Nordrhein-Westfalen eine faktische Schulgeldfreiheit, weil Schulgelder von der staatlichen Förderung abgezogen werden (Wrase & Helbig, 2016). Dadurch, dass es in Nordrhein-Westfalen nur wenige private Grundschulen gibt, die nordrheinwestfälischen SchülerInnen aber einen großen Anteil der Befragten der westdeutschen Population im SOEP ausmachen, zeigt sich der Zusammenhang aus Entfernung zur nächsten Privatschule und deren Besuch erst, wenn man die Fälle aus Nordrhein-Westfalen aus dem Untersuchungssample ausschließt.

Unterschiede zwischen Stadt und Land

Die Effekte für die Entfernung zur nächsten Privatschule bleiben auch dann bestehen, wenn man die Analysen getrennt nach Stadt und Land durchführt.[24] In Tabelle 3.A.7 im Anhang sind die Analysen für Kommunen unter 50.000 EinwohnerInnen (ländliche Gebiete) festgehalten. Hier verhalten sich die Ergebnisse für die west- und ostdeutschen Bundesländer sehr ähnlich. Analog zur Hauptanalyse (Tabelle 3.3) lässt sich beobachten, dass AkademikerInnen sich stärker durch die Entfernung zur nächsten Privatschule in ihrer Schulwahl beeinflussen lassen. Zudem ist auffällig, dass nur der Bildungsabschluss der Eltern, nicht aber Einkommen, Transferbezug oder Migrationshintergrund, den Privatschulbesuch voraussagen.

In Gemeinden ab 50.000 EinwohnerInnen (städtische Gebiete) (Tabelle 3.A.8 im Anhang) zeigen sich generell stärkere Unterschiede in der sozioökonomischen Zusammensetzung. Neben Familien mit Hochschulabschluss besuchen auch solche mit hohem Einkommen und ohne Migrationshintergrund und Sozialtransferbezug häufiger eine Schule in freier Trägerschaft. Eine besondere Sensitivität dieser Gruppen in Bezug auf die geografische Nähe lässt sich indes nur in den ostdeutschen Bundesländern beobachten.

Die Rolle des Schulgeldes

Dass sich Familien mit Migrationshintergrund im Westen und Haushalte mit geringem Einkommen und ohne hohen Bildungsabschluss im Osten Deutschlands kaum durch die Entfernung zur nächsten Privatschule beeinflussen lassen, könnte daran liegen, dass private Schulen aus anderen Gründen für sie keine Alternative sind. Möglicherweise ist das der Fall, weil die pädagogischen Modelle an privaten Schulen sie weniger stark ansprechen, oder aber, weil sie das Schulgeld als zu hoch empfinden.

Zusätzliche Analysen sollen prüfen, ob sich das beobachtete Muster auch unabhängig von der Höhe des verlangten Schulgelds zeigt. Zum einen führen wir dafür die Berechnungen gesondert für die Bundesländer mit einer langen Tradition einer Schulgeldfreiheit (Rheinland-Pfalz[25]) beziehungsweise faktischen Schulgeldfreiheit (Saarland und Nordrhein-Westfalen) durch (Tabelle 3.A.12). Zum anderen nutzen wir hierfür die Höhe des gezahlten Schulgeldes, welche in den Jahren 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019 analog zum Schulträger im SOEP abgefragt wurde. Tabelle 3.A.13

[24] Einzelanalysen nach Schulformen haben wir hierzu nicht durchgeführt, weil die Fallzahl teilweise sehr klein wurden.

[25] Eine Ausnahme bilden hier Waldorfschulen, welche Schulgeld erheben dürfen.

zeigt die Ergebnisse der Analysen, wenn wir für die Höhe des gezahlten Schulgeldes kontrollieren. In Tabelle 3.A.14 limitieren wir die Analysen auf Beobachtungen, bei denen das Schulgeld weniger als 100 Euro betrug.[26] Tatsächlich tritt in diesen Analysen ein anderes Muster hervor, was die „Entfernungssensibilität" der verschiedenen Gruppen angeht. Auffällig ist, dass in allen drei Analysen für Gesamt- und Westdeutschland Haushalte mit Einkommen oberhalb des Medians sich weniger entfernungssensibel verhalten als Haushalte mit Einkommen unterhalb des Medians. Dies könnte ein Indiz dafür sein, dass der Privatschulbesuch für untere Einkommensgruppen eher eine Wahloption darstellt, wenn kein Schulgeld erhoben wird (Tabelle 3.A.12), dieses niedrig ausfällt (Tabelle 3.A.14) oder die Rolle der Kosten herausgerechnet wird (Tabelle 3.A.13). Man kann in diesen Fällen also für Privatschulen ansatzweise das beobachten, dass auch für die generelle Schulwahl gilt (z.B. OECD, 2017a): hier ist die Distanz für ressourcenschwächere Haushalte ein wichtigeres Kriterium bei der (Privat-)Schulwahl.

In Tabellen 3.A.12 und 3.A.13 zeigt sich zudem für West- wie Ostdeutschland, dass Haushalte mit Hochschulabschluss weiterhin eine erhöhte Reaktion auf die geografische Verfügbarkeit von privaten Schulangeboten aufweisen. Auch sind in Westdeutschland Familien mit Migrationsgeschichte weniger entfernungssensibel als Familien ohne Migrationshintergrund. Zudem lassen sich in den drei Analysen weiterhin statistisch signifikante Unterschiede in der sozioökonomischen Zusammensetzung an Privatschulen im Vergleich zu öffentlichen Schulen erkennen, indem Kinder mit Migrationshintergrund und aus Sozialtransferempfängerhaushalten weiterhin dort unterrepräsentiert sind, wenn man die Rolle des Schulgeldes herausrechnet oder nur Fälle beachtet, in denen ein niedriges Schulgeld gezahlt wird. In Rheinland-Pfalz, Nordrhein-Westfalen und dem Saarland besuchen trotz Schulgeldverbots Kinder aus höher gebildeten Haushalten häufiger und Kinder mit Migrationshintergrund seltener Privatschulen (Tabelle 3.A.12). Hieran zeigt sich, dass sich sozioökonomische Unterschiede beim Privatschulbesuch bei weitem nicht nur durch das Schulgeld erklären lassen. Zu diesem Befund kamen auch Helbig et al. (2017b) für die größeren Städte in Rheinland-Pfalz.

3.6 Schlussfolgerungen

Motiviert ist der vorliegende Beitrag in erster Linie durch die Beobachtung, dass private Schulen sich in einigen Städten und Regionen ungleich entlang soziostruktureller Merkmale verteilen. So gibt es einige Belege dafür, dass sich in ländlichen Gebieten mit

[26] Die Höhe von 100 Euro ist hier willkürlich festgelegt. Die Analysen zeigen ähnliche Ergebnisse, wenn die Höhe beispielsweise auf 160 Euro begrenzt wird.

einer geringen AkademikerInnendichte, Stadtteilen mit einer hohen Armutsquote oder geringer AkademikerInnenquote weniger private Schulen befinden. Daraus ergibt sich die Frage, ob eine systematisch sozialräumlich ungleiche Verteilung von Privatschulen zumindest partiell auch die sozioökonomische Zusammensetzung an Privatschulen erklären kann. Um diese Frage zu beantworten, haben wir die Geokoordinaten aller deutschen allgemeinbildenden Schulen von 2002 bis 2019 mit den Daten des SOEP verknüpft.

In einem ersten Analyseschritt haben wir untersucht, ob sich die privaten Schulen nach verschiedenen soziodemografischen Merkmalen sozial ungleich im Raum verteilen, bzw. ob sie sich systematisch stärker in der Nähe zu bestimmten sozialen Gruppen befinden. Hier zeigte sich für Westdeutschland, dass sich private Schulen einerseits wie erwartet häufiger in geringerer Entfernung zu bildungsnahen und einkommensstarken Haushalten befinden. Privatschulen in Ostdeutschland sind dahingegen geografisch breiter und zufälliger verteilt. Nur Kinder mit Migrationshintergrund weisen hier höhere Distanzen zu Privatschulen auf als Kinder ohne Zuwanderungsgeschichte. Unterschiede, die sich etwa für Berlin und Erfurt beobachten lassen, sind also nicht systematisch für ganz Ostdeutschland feststellen. Ein Grund hierfür könnte sein, dass eine Reihe von privaten Schulen nach der Jahrtausendwende dort entstanden, wo öffentliche Schulen schlossen. Dies waren zunächst Gegenden, in denen die Schülerzahlen besonders stark zurückgingen, gleichzeitig aber auch die Kinderarmutsquote hoch war – wie z.B. in einigen Plattenbaugebieten der ostdeutschen Städte (Helbig & Jähnen, 2018; Helbig et al., 2018).

Anders als erwartet befinden sich darüber hinaus SchülerInnen mit Migrationshintergrund in Westdeutschland häufiger in der Nähe von Privatschulen. Dieses sozialräumliche Muster könnte unseres Erachtens zwei Gründe haben. Entweder befinden sich private Schulen in den westdeutschen Städten häufiger im Innenstadtbereich, um auch die Erreichbarkeit aus dem gesamten Stadtgebiet zu erhöhen. Wenn darüber hinaus auch Familien mit Migrationshintergrund häufiger im Innenstadtbereich wohnen, könnte dies unseren Befund erklären. Eine andere Möglichkeit wäre, dass private Schulen gerade dort für privilegierte Haushalte besonders attraktiv sind, wo ein hoher Anteil von Menschen mit Migrationshintergrund wohnt (z.B. Akbarpour et al., 2022; Jähnen & Helbig, 2022). Dies sollte sich aber besonders im Grundschulbereich zeigen, wo die Beschulung überwiegend in wohnortnahen Einzugsgebieten erfolgt. Ein ähnliches Muster lässt sich aber auch für Sekundarschulen beobachten.

In einem zweiten Schritt haben wir untersucht, inwieweit die Nähe zu einer privaten Schule die Wahrscheinlichkeit des Besuchs einer solchen beeinflusst, und ob darüber auch die sozioökonomische Zusammensetzung an Privatschulen teilweise erklärt werden kann. Die Analysen hierzu zeigen, dass die Entfernung zur nächsten privaten Schule zwar eine wichtige Determinante des Privatschulbesuchs ist. Je kürzer der Weg zur nächsten Privatschule, desto eher besucht ein Kind auch eine solche. Somit bestätigt sich unsere erste Hypothese (H1).

Über die Entfernung konnten wir allerdings kaum die sozioökonomische Zusammensetzung an Privatschulen aufklären. Hypothese 3 (H3), nach der sozial ungleiche Besuchsmuster von Privatschulen zumindest teilweise über die Entfernung zur nächsten Privatschule erklärt werden können, lässt sich deshalb nicht bestätigen. Dies könnte daran liegen, dass die Entfernung zur nächsten Privatschule nicht alle sozialen Gruppen gleichermaßen in ihrer Privatschulwahl beeinflusst. So zeigt sich, dass vor allem Eltern mit akademischem Hintergrund in ihrer Privatschulwahl durch die Entfernung zur nächsten Privatschule beeinflusst werden. Besonders in Ostdeutschland zeigt sich, dass hauptsächlich Akademikereltern und Eltern mit hohen Haushaltseinkommen durch die Privatschulentfernung beeinflusst werden. In Ostdeutschland werden Kinder ohne Migrationshintergrund stärker durch die Privatschulentfernung beim Privatschulbesuch beeinflusst. Somit bestätigt sich unsere zweite Hypothese (H2), dass der Privatschulbesuch für sozioökonomisch privilegiertere Gruppen stärker durch die Entfernung zur nächsten Privatschule beeinflusst wird als bei weniger privilegierten Gruppen. Die höhere „Distanzsensibilität“ von bildungsnahen Gruppen, einkommenshohen Gruppen (Ostdeutschland) und Nicht-MigrantInnen (Westdeutschland) deutet darauf hin, dass die Option, eine private Schule zu besuchen, für bildungsferne und ressourcenschwächere Gruppen und MigrantInnen gar nicht als Wahlalternative wahrgenommen wird. Dementsprechend spielt auch die Entfernung zur nächsten Privatschule für diese Eltern keine systematische Rolle bei der horizontalen Schulwahl. In zusätzlichen Analysen, welche die Berechnungen auf die schulgeldbefreiten Bundesländer Rheinland-Pfalz, Nordrhein-Westfalen und das Saarland beschränkten, zeigte sich allerdings, dass hier die einkommensschwächeren Gruppen stärker durch die Distanz zur nächsten Privatschule in ihrer Privatschulwahl beeinflusst werden. Dies kann als Indiz dafür interpretiert werden, dass in Ländern ohne Schulgeld Privatschulen auch für ressourcenschwächere Haushalte eine Wahlalternative darstellen – das Muster sich in diesen Fällen also dem annähert, was aus werterwartungstheoretischer Perspektive zu erwarten wäre.

Nach Art. 7 Abs. 4 GG sollten die Schulgelder privater Schulen so gestaltet sein, dass sich diese alle Kinder bzw. Eltern leisten können. Dafür, dass dies in der Praxis nicht überall der Fall ist, gibt es einige Hinweise (siehe Wrase & Helbig, 2016). Insgesamt zeigt sich in dieser Studie erneut, dass private Schulen häufiger von Eltern mit akademischem Hintergrund höherem Einkommen ausgewählt werden. Neben dem kulturellen und ökonomischen Kapital der Familien, sagt auch der Migrationshintergrund der Kinder den Privatschulbesuch voraus. Kinder mit Migrationshintergrund sind hier seltener zu finden, obwohl sie zumindest in Westdeutschland näher an privaten Schulen wohnen. Die Ungleichheiten aller drei Dimensionen sind darüber hinaus in den ostdeutschen Bundesländern etwas ausgeprägter als in den westdeutschen Bundesländern, obwohl Privatschulen hier geografisch zufälliger verteilt sind. Wenn für Kinder aus Haushalten mit Migrationshintergrund, mit geringen Einkommen oder ohne hohen Bildungsabschluss Privatschulen – beispielsweise wegen deren pädagogischer Konzepte – gar nicht als Alternative zu staatlichen Schulen wahrgenommen werden, würden auch restriktivere bzw. nach Einkommen gestaffelte Schulgeldmodelle nicht viel an der bestehenden sozioökonomischen Zusammensetzung an Privatschulen verändern.

Die Studie weist im Hinblick auf die Fragestellung keine zentralen Limitationen auf. Insgesamt halten wir es aber für weitergehende Forschung für sinnvoll, diejenigen Mechanismen zu untersuchen, die dazu führen, dass Kinder aus benachteiligten sozioökonomischen Verhältnissen seltener auf privaten Schulen zu finden sind. Dies betrifft dabei alle drei Stufen von Kristen's (2003) Schulwahlmodell. Diese Studie weist zumindest an der ersten Stufe des Schulwahlmodells darauf hin, dass private Schulen gar nicht als Wahlalternative für untere Bildungsgruppen gesehen werden. Welche Gründe zu diesem Befund führen, sollte weitere Forschung ebenso klären wie ungleichheitsgenerierende Mechanismen bei der Bewerbung an einer Privatschule (Stufe 2) und die Aufnahme an einer Privatschule (Stufe 3). Gerade zur letzten Stufe gibt es mit der Ausnahme von Helbig & Mayer (2023), allerdings mit einer kleinen Fallzahl, keine empirische Evidenz.

3.A Appendix

Table 3.A.1: Differenz der sozio-ökonomischen Zusammensetzung zwischen öffentlichen und privaten Schulen: Grundschulen

	Besuch einer			
	Öffentl. Schule	Privatschule	Differenz	
			b	t
Individuelle Eigenschaften				
Weiblich	0.49	0.49	0.00	(0.02)
Alter (Jahre)	8.84	8.79	0.05	(0.90)
Haushaltseigenschaften				
Keine Ausbildung	0.10	0.03	0.07***	(10.92)
Ausbildung	0.57	0.46	0.11***	(5.86)
Uniabschluss	0.33	0.51	-0.18***	(-9.79)
Anzahl der Kinder im Haushalt	2.46	2.33	0.13***	(3.39)
Migrationshintergrund	0.25	0.17	0.08***	(5.63)
Netto-Haushaltsäquivalenzeinkommen	21501	27113	-5612***	(-7.30)
Sozialtransferbezug	0.22	0.13	0.09***	(7.38)
Distanz zu Privatschule (km)	8.68	6.04	2.64***	(11.25)
Distanz zu öffentlicher Schule (km)	0.97	0.92	0.05	(1.25)
Gemeindegröße				
Unter 50.000 Einwohner	0.28	0.20	0.08***	(5.19)
50.000-100.000 Einwohner	0.09	0.09	-0.00	(-0.30)
100.000-500.000 Einwohner	0.31	0.31	0.00	(0.15)
500.000 Einwohner u.m.	0.32	0.39	-0.08***	(-4.26)
N	10439	811	11250	

Bemerkungen: Angaben in Spaltenprozenten (sofern nicht anders vermerkt).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: Sozio-oekonomisches Panel (SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet), amtliche Schuldaten für die Jahre 2000-2019; eigene Berechnungen.

Table 3.A.2: Differenz der sozio-ökonomischen Zusammensetzung zwischen öffentlichen und privaten Schulen: Gymnasien

	Besuch einer			
	Öffentl. Schule	Privatschule	Differenz	
			b	t
Individuelle Eigenschaften				
Weiblich	0.51	0.58	-0.07**	(-3.12)
Alter (Jahre)	13.05	13.10	-0.05	(-0.86)
Haushaltseigenschaften				
Keine Ausbildung	0.02	0.00	0.02***	(6.13)
Ausbildung	0.44	0.37	0.07**	(3.22)
Uniabschluss	0.54	0.63	-0.09***	(-4.15)
Anzahl der Kinder im Haushalt	2.15	2.23	-0.08	(-1.77)
Migrationshintergrund	0.13	0.06	0.07***	(6.51)
Netto-Haushaltsäquivalenzeinkommen	27551	31422	-3871***	(-4.38)
Sozialtransferbezug	0.08	0.03	0.05***	(5.77)
Distanz zu Privatschule (km)	8.95	5.63	3.31***	(12.87)
Distanz zu öffentlicher Schule (km)	3.25	3.91	-0.66***	(-4.34)
Gemeindegröße				
Unter 50.000 Einwohner	0.25	0.21	0.04*	(1.99)
50.000-100.000 Einwohner	0.08	0.09	-0.00	(-0.34)
100.000-500.000 Einwohner	0.30	0.35	-0.05**	(-2.60)
500.000 Einwohner u.m.	0.37	0.35	0.02	(1.08)
N	4681	593	5274	

Bemerkungen: Angaben in Spaltenprozenten (sofern nicht anders vermerkt).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: Sozio-oekonomisches Panel (SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet), amtliche Schuldaten für die Jahre 2000-2019; eigene Berechnungen.

Table 3.A.3: Differenz der sozio-ökonomischen Zusammensetzung zwischen öffentlichen und privaten Schulen: Sekundarschulen ohne gymnasiale Oberstufe

	Besuch einer			
	Öffentl. Schule	Privatschule	Differenz	
			b	t
Individuelle Eigenschaften				
Weiblich	0.46	0.63	-0.17***	(-6.28)
Alter	13.21	13.13	0.07	(0.92)
Haushaltseigenschaften				
Keine Ausbildung	0.10	0.02	0.08***	(8.90)
Ausbildung	0.74	0.63	0.11***	(4.05)
Uniabschluss	0.16	0.35	-0.19***	(-7.24)
Anzahl der Kinder im Haushalt	2.23	2.25	-0.02	(-0.36)
Migrationshintergrund	0.26	0.09	0.16***	(9.81)
Netto-Haushaltsäquivalenzeinkommen	17770	32665	-14894	(-1.71)
Sozialtransferbezug	0.22	0.09	0.12***	(7.57)
Distanz zu Privatschule (km)	9.60	5.58	4.03***	(14.07)
Distanz zu öffentlicher Schule (km)	1.98	2.45	-0.47***	(-3.95)
Gemeindegröße				
Unter 50.000 Einwohner	0.35	0.26	0.10***	(3.95)
50.000-100.000 Einwohner	0.11	0.13	-0.02	(-1.02)
100.000-500.000 Einwohner	0.31	0.32	-0.01	(-0.35)
500.000 Einwohner u.m.	0.23	0.30	-0.07**	(-2.71)
N	5142	350	5492	

Bemerkungen: Angaben in Spaltenprozenten (sofern nicht anders vermerkt).
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: Sozio-oekonomisches Panel (SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet), amtliche Schuldaten für die Jahre 2000-2019; eigene Berechnungen.

Table 3.A.4: Sozioökonomische Determinanten der Entfernung zu Privatschulen: Ländl. Gemeinden (unter 50.000 Einwohner)

	Entfernung zu Privatschule		
	Bundesrepublik	Westdeutschland	Ostdeutschland
Eltern: Hochschulabschluss	-0.349	-0.615	0.668
	(0.333)	(0.379)	(0.625)
Kind: Migrationshintergrund	-0.501	-0.578	-1.880
	(0.455)	(0.473)	(1.828)
Log HH-Einkommen	-0.413	-0.164	-1.027
	(0.338)	(0.385)	(0.661)
Sozialtransfer	-0.020	0.076	0.082
	(0.368)	(0.467)	(0.550)
Beobachtungen	17505	13130	4375

Bemerkungen: Die Regressionen enthalten Jahres-, Kreis- und Gemeindegrößen-fixe Effekte. Standardfehler geclustert auf Haushaltsebene. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: SOEP v36 und amtliche Schuldaten, Jahre 2000-2019, eigene Berechnungen.

Table 3.A.5: Sozioökonomische Determinanten der Entfernung zu Privatschulen Städt. Gemeinden (über 50.000 Einwohner)

	Entfernung zu Privatschule		
	Bundesrepublik	Westdeutschland	Ostdeutschland
Eltern: Hochschulabschluss	-0.480***	-0.534***	-0.207
	(0.157)	(0.172)	(0.371)
Kind: Migrationshintergrund	-0.654***	-0.692***	1.534***
	(0.176)	(0.179)	(0.580)
Log HH-Einkommen	-0.333**	-0.347**	-0.331
	(0.157)	(0.171)	(0.364)
Sozialtransfer	-0.270*	-0.332**	-0.054
	(0.160)	(0.162)	(0.418)
Beobachtungen	42690	36015	6675

Bemerkungen: Die Regressionen enthalten Jahres-, Kreis- und Gemeindegrößen-fixe Effekte. Standardfehler geclustert auf Haushaltsebene. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: SOEP v36 und amtliche Schuldaten, Jahre 2000-2019, eigene Berechnungen.

Table 3.A.6: Sozioökonomische Determinanten der Entfernung zu Privatschulen: nach Schularten

	Bundesrepublik	Westdeutschland	Ostdeutschland
	Entfernung zu privater Grundschule in km		
Eltern: Hochschulabschluss	-0.487***	-0.641***	0.285
	(0.186)	(0.203)	(0.393)
Migrationshintergrund	-0.812***	-0.933***	0.691
	(0.222)	(0.227)	(0.824)
Log HH-Einkommen	-0.101	-0.107	-0.066
	(0.192)	(0.212)	(0.393)
Sozialtransfer	-0.094	-0.012	-0.292
	(0.196)	(0.209)	(0.412)
Beobachtungen	28401	22876	5525
	Entfernung zu privatem Gymnasium in km		
Eltern: Hochschulabschluss	-0.991***	-0.903***	-1.629***
	(0.215)	(0.233)	(0.541)
Migrationshintergrund	-0.462*	-0.447*	2.250
	(0.268)	(0.271)	(1.758)
Log HH-Einkommen	-0.199	-0.123	-0.424
	(0.207)	(0.226)	(0.523)
Sozialtransfer	0.029	-0.414	0.764
	(0.272)	(0.282)	(0.600)
Beobachtungen	13267	10916	2351
	Entfernung zu privater Real- und Hauptschule in km		
Eltern: Hochschulabschluss	-0.153	-0.231	0.243
	(0.255)	(0.283)	(0.517)
Kind: Migrationshintergrund	-0.629**	-0.672***	0.336
	(0.253)	(0.258)	(0.674)
Log HH-Einkommen	-0.342	-0.367	-0.024
	(0.226)	(0.241)	(0.547)
Sozialtransfer	0.210	0.287	0.054
	(0.225)	(0.265)	(0.403)
Beobachtungen	18527	15353	3174

Bemerkungen: Die Regressionen enthalten Jahres-, Kreis- und Gemeindegrößen-fixe Effekte. Standardfehler geclustert auf Haushaltsebene. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Quelle: SOEP v36 und amtliche Schuldaten, Jahre 2000-2019, eigene Berechnungen.

Table 3.A.7: Sozioökonomische und geografische Determinanten des Privatschulbesuchs: Ländl. Gemeinden (unter 50.000 Einwohner)

		Gesamtdeutschland			Westen			Osten	
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.044***	-0.021		-0.042***	-0.022		-0.050***	-0.029
		(0.008)	(0.014)		(0.011)	(0.017)		(0.013)	(0.026)
Log Distanz zu oeff. Schule		0.018***	0.018***		0.019***	0.019***		0.012**	0.012**
		(0.005)	(0.005)		(0.006)	(0.006)		(0.006)	(0.005)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.020	0.024	0.057	0.014	0.018	0.064	0.109**	0.102**	0.090*
	(0.017)	(0.017)	(0.036)	(0.018)	(0.018)	(0.040)	(0.054)	(0.045)	(0.050)
Hochschulabschluss	0.049**	0.049**	0.165***	0.039	0.039	0.122**	0.157***	0.143***	0.342***
	(0.024)	(0.024)	(0.049)	(0.026)	(0.026)	(0.055)	(0.055)	(0.045)	(0.072)
Migrationshintergrund Kind	-0.021	-0.014	-0.061	-0.020	-0.013	-0.071	-0.031	-0.035	0.029
	(0.015)	(0.016)	(0.040)	(0.016)	(0.016)	(0.045)	(0.063)	(0.056)	(0.074)
Log HH-Einkommen	0.011	0.009	0.013	0.010	0.008	0.007	0.009	0.013	0.031
	(0.014)	(0.014)	(0.018)	(0.016)	(0.016)	(0.021)	(0.031)	(0.031)	(0.037)
Sozialtransfer	-0.010	-0.003	-0.003	-0.004	0.003	0.003	-0.024	-0.022	-0.021
	(0.014)	(0.014)	(0.014)	(0.019)	(0.018)	(0.018)	(0.024)	(0.023)	(0.022)
Distanz * Ausbildung			-0.017			-0.022			0.002
			(0.015)			(0.016)			(0.027)
Distanz * Hochschulabschluss			-0.053***			-0.038*			-0.087***
			(0.020)			(0.022)			(0.033)
Distanz * Migrationsh.			0.020			0.025			-0.041
			(0.014)			(0.016)			(0.041)
Distanz * hohes Einkommen			-0.002			0.000			-0.011*
			(0.005)			(0.006)			(0.006)
Beobachtungen	6238	6238	6238	4859	4859	4859	1379	1379	1379

Anmerkungen: Die Regressionen enthalten Jahres- und Kreis-fixe Effekte. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Table 3.A.8: Sozioökonomische und geografische Determinanten des Privatschulbesuchs: Städt. Gemeinden (über 50.000 Einwohner)

	Gesamtdeutschland			Westen			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.026***	-0.028**		-0.025***	-0.029**		-0.037***	0.022
		(0.006)	(0.012)		(0.007)	(0.013)		(0.013)	(0.055)
Log Distanz zu oeff. Schule		0.014**	0.014**		0.016***	0.016***		0.001	0.001
		(0.006)	(0.006)		(0.006)	(0.006)		(0.013)	(0.013)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
100.000-500.000 Einwohner	-0.024	-0.024	-0.023	-0.023	-0.022	-0.021	-0.024	-0.024	-0.013
	(0.020)	(0.020)	(0.020)	(0.023)	(0.023)	(0.023)	(0.040)	(0.039)	(0.040)
500.000 Einwohner o.m.	-0.062***	-0.064***	-0.063***	-0.066**	-0.068***	-0.067**	-0.021	-0.024	-0.027
	(0.024)	(0.024)	(0.024)	(0.026)	(0.026)	(0.026)	(0.038)	(0.038)	(0.039)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.016	0.016	0.014	0.020	0.020	0.026	-0.034	-0.041	-0.000
	(0.013)	(0.013)	(0.020)	(0.013)	(0.013)	(0.021)	(0.050)	(0.055)	(0.079)
Hochschulabschluss	0.051***	0.049***	0.061***	0.049***	0.047***	0.048**	0.049	0.041	0.180**
	(0.015)	(0.015)	(0.023)	(0.016)	(0.015)	(0.024)	(0.053)	(0.057)	(0.088)
Migrationshintergrund Kind	-0.034***	-0.035***	-0.049***	-0.034***	-0.034***	-0.051**	-0.144***	-0.142***	-0.153**
	(0.011)	(0.011)	(0.019)	(0.012)	(0.011)	(0.020)	(0.051)	(0.051)	(0.068)
Log HH-Einkommen	0.026***	0.024**	0.020*	0.020*	0.018*	0.011	0.075***	0.075***	0.090***
	(0.010)	(0.010)	(0.012)	(0.011)	(0.010)	(0.013)	(0.028)	(0.029)	(0.032)
Sozialtransfer	-0.025***	-0.024***	-0.023***	-0.027***	-0.025***	-0.024***	0.006	0.003	0.000
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.025)	(0.025)	(0.024)
Distanz * Ausbildung			0.001			-0.004			-0.018
			(0.011)			(0.011)			(0.055)
Distanz * Hochschulabschluss			-0.009			-0.002			-0.095*
			(0.013)			(0.013)			(0.054)
Distanz * Migrationsh.			0.011			0.013			0.002
			(0.009)			(0.009)			(0.050)
Distanz * hohes Einkommen			0.003			0.007			-0.023**
			(0.005)			(0.006)			(0.011)
Beobachtungen	15778	15778	15778	13471	13471	13471	2307	2307	2307

Anmerkungen: Die Regressionen enthalten Jahres- und Kreis-fixe Effekte. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Table 3.A.9: Sozioökonomische und geografische Determinanten des Privatschulbesuchs: Grundschulen

	Gesamtdeutschland			Westen			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.010**	0.001		-0.006	0.006		-0.027***	-0.011
		(0.005)	(0.009)		(0.006)	(0.010)		(0.010)	(0.021)
Log Distanz zu oeff. Schule		0.002	0.002		0.002	0.002		-0.002	-0.001
		(0.005)	(0.005)		(0.006)	(0.006)		(0.009)	(0.009)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
50.000-100.000 Einwohner	0.038**	0.036**	0.036**	0.036*	0.036*	0.037*	0.037	0.028	0.028
	(0.017)	(0.017)	(0.017)	(0.020)	(0.020)	(0.020)	(0.028)	(0.027)	(0.026)
100.000-500.000 Einwohner	0.009	0.006	0.007	0.011	0.009	0.010	-0.020	-0.028	-0.020
	(0.014)	(0.014)	(0.014)	(0.015)	(0.016)	(0.016)	(0.026)	(0.026)	(0.025)
500.000 Einwohner o.m.	0.021	0.018	0.020	0.019	0.017	0.019	0.008	-0.002	0.001
	(0.019)	(0.020)	(0.020)	(0.022)	(0.022)	(0.022)	(0.026)	(0.027)	(0.028)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.024**	0.024**	0.072	0.032***	0.032***	0.125	-0.019	-0.021	-0.025
	(0.011)	(0.011)	(0.074)	(0.012)	(0.012)	(0.080)	(0.038)	(0.039)	(0.183)
Hochschulabschluss	0.051***	0.050***	0.291***	0.047***	0.046***	0.249***	0.083*	0.081*	0.498**
	(0.013)	(0.013)	(0.085)	(0.014)	(0.014)	(0.089)	(0.042)	(0.043)	(0.231)
Migrationshintergrund Kind	-0.020*	-0.021*	-0.130*	-0.020*	-0.021*	-0.120*	-0.057	-0.056	-0.154
	(0.011)	(0.011)	(0.068)	(0.012)	(0.012)	(0.071)	(0.040)	(0.039)	(0.269)
Log HH-Einkommen	0.012	0.012	0.011	0.007	0.007	0.005	0.053*	0.055**	0.082**
	(0.010)	(0.010)	(0.015)	(0.011)	(0.011)	(0.016)	(0.028)	(0.028)	(0.036)
Sozialtransfer	-0.023***	-0.023***	-0.022**	-0.020**	-0.020**	-0.019*	-0.002	-0.009	-0.010
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.020)	(0.020)	(0.019)
Distanz * Ausbildung			-0.006			-0.011			0.001
			(0.008)			(0.009)			(0.022)
Distanz * Hochschulabschluss			-0.028***			-0.024**			-0.050*
			(0.010)			(0.010)			(0.027)
Distanz * Migrationsh.			0.013*			0.012			0.011
			(0.007)			(0.008)			(0.031)
Distanz * hohes Einkommen			-0.000			0.000			-0.005
			(0.001)			(0.002)			(0.004)
Beobachtungen	11250	11250	11250	9215	9215	9215	2035	2035	2035

Anmerkungen: Die Regressionen enthalten Jahres- und Kreis-fixe Effekte. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Table 3.A.10: Sozioökonomische und geografische Determinanten des Privatschulbesuchs: Gymnasien

	Gesamtdeutschland			Westen			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.058***	-0.081		-0.059***	-0.081		-0.055**	-0.092
		(0.014)	(0.065)		(0.016)	(0.066)		(0.024)	(0.062)
Log Distanz zu oeff. Schule		0.040***	0.040***		0.047***	0.046***		0.003	0.004
		(0.009)	(0.009)		(0.011)	(0.010)		(0.017)	(0.017)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
50.000-100.000 Einwohner	-0.039	-0.056	-0.056	-0.052	-0.075	-0.075	0.019	-0.009	0.006
	(0.046)	(0.046)	(0.046)	(0.053)	(0.052)	(0.052)	(0.075)	(0.074)	(0.075)
100.000-500.000 Einwohner	0.034	0.018	0.017	0.025	0.009	0.007	0.050	0.027	0.037
	(0.037)	(0.036)	(0.035)	(0.041)	(0.040)	(0.039)	(0.075)	(0.075)	(0.072)
500.000 Einwohner o.m.	-0.059*	-0.074**	-0.074**	-0.071*	-0.085**	-0.084**	0.046	0.011	0.051
	(0.035)	(0.035)	(0.035)	(0.037)	(0.038)	(0.038)	(0.094)	(0.093)	(0.097)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.015	0.025	-0.098	0.015	0.023	0.016	0.112	0.059	-0.649
	(0.064)	(0.065)	(0.560)	(0.064)	(0.065)	(0.569)	(0.125)	(0.141)	(0.484)
Hochschulabschluss	0.044	0.048	-0.194	0.037	0.040	-0.265	0.172	0.101	0.326
	(0.065)	(0.065)	(0.561)	(0.065)	(0.066)	(0.572)	(0.136)	(0.148)	(0.511)
Migrationshintergrund Kind	-0.048*	-0.043*	-0.075	-0.047*	-0.041*	-0.068	-0.039	-0.059	-0.971**
	(0.026)	(0.025)	(0.196)	(0.026)	(0.025)	(0.205)	(0.089)	(0.102)	(0.425)
Log HH-Einkommen	0.023	0.023	0.035	0.017	0.017	0.030	0.042	0.043	0.025
	(0.020)	(0.020)	(0.024)	(0.021)	(0.020)	(0.024)	(0.065)	(0.066)	(0.079)
Sozialtransfer	-0.045*	-0.032	-0.033	-0.057**	-0.045*	-0.046*	-0.003	0.001	-0.009
	(0.023)	(0.023)	(0.023)	(0.026)	(0.025)	(0.025)	(0.049)	(0.048)	(0.045)
Distanz * Ausbildung			0.016			0.002			0.093*
			(0.064)			(0.065)			(0.056)
Distanz * Hochschulabschluss			0.029			0.037			-0.012
			(0.064)			(0.066)			(0.057)
Distanz * Migrationsh.			0.004			0.003			0.110**
			(0.022)			(0.023)			(0.048)
Distanz * hohes Einkommen			-0.002			-0.002			-0.001
			(0.002)			(0.002)			(0.005)
Beobachtungen	5274	5274	5274	4427	4427	4427	847	847	847

Anmerkungen: Die Regressionen enthalten Jahres- und Kreis-fixe Effekte. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Table 3.A.11: Sozioökonomische und geografische Determinanten des Privatschulbesuchs: Sekundarschulen ohne gymnasiale Oberstufe

	Gesamtdeutschland			Westen			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.034***	-0.016		-0.034***	-0.015		-0.045***	-0.104
		(0.008)	(0.014)		(0.009)	(0.015)		(0.017)	(0.082)
Log Distanz zu oeff. Schule		0.020***	0.021***		0.023***	0.023***		0.005	0.011
		(0.006)	(0.006)		(0.007)	(0.007)		(0.010)	(0.008)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
50.000-100.000 Einwohner	0.021	0.012	0.013	0.017	0.008	0.009	0.031	0.025	0.035
	(0.026)	(0.027)	(0.027)	(0.030)	(0.031)	(0.031)	(0.043)	(0.041)	(0.042)
100.000-500.000 Einwohner	-0.025	-0.026	-0.024	-0.025	-0.025	-0.023	-0.032	-0.038	-0.019
	(0.020)	(0.020)	(0.020)	(0.021)	(0.021)	(0.021)	(0.045)	(0.044)	(0.045)
500.000 Einwohner o.m.	0.020	0.004	0.006	0.027	0.012	0.014	-0.095	-0.130	-0.150
	(0.028)	(0.027)	(0.027)	(0.029)	(0.028)	(0.028)	(0.097)	(0.096)	(0.096)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.026	0.025	0.222*	0.026	0.025	0.251**	-0.067	-0.042	-0.649
	(0.021)	(0.020)	(0.115)	(0.021)	(0.020)	(0.117)	(0.081)	(0.087)	(0.726)
Hochschulabschluss	0.092***	0.093***	0.560***	0.092***	0.091***	0.495**	0.014	0.043	0.374
	(0.028)	(0.027)	(0.186)	(0.029)	(0.029)	(0.200)	(0.085)	(0.093)	(0.765)
Migrationshintergrund Kind	-0.033**	-0.032**	-0.324***	-0.031**	-0.029**	-0.309***	-0.214**	-0.194**	-1.503*
	(0.014)	(0.014)	(0.112)	(0.014)	(0.014)	(0.116)	(0.087)	(0.090)	(0.794)
Log HH-Einkommen	0.048***	0.044***	0.045**	0.045***	0.042**	0.042**	0.059*	0.055*	0.060
	(0.016)	(0.016)	(0.019)	(0.017)	(0.017)	(0.021)	(0.033)	(0.033)	(0.040)
Sozialtransfer	-0.032**	-0.024*	-0.024	-0.038**	-0.029*	-0.029*	-0.001	0.001	0.005
	(0.015)	(0.015)	(0.014)	(0.018)	(0.017)	(0.017)	(0.028)	(0.027)	(0.025)
Distanz * Ausbildung			-0.024*			-0.027**			0.074
			(0.013)			(0.013)			(0.083)
Distanz * Hochschulabschluss			-0.055***			-0.048**			-0.031
			(0.021)			(0.023)			(0.087)
Distanz * Migrationsh.			0.034***			0.033**			0.158*
			(0.013)			(0.013)			(0.093)
Distanz * hohes Einkommen			0.000			0.000			-0.001
			(0.002)			(0.002)			(0.003)
Beobachtungen	5492	5492	5492	4688	4688	4688	804	804	804

Anmerkungen: Die Regressionen enthalten Jahres- und Kreis-fixe Effekte. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Table 3.A.12: Sozioökonomische und geografische Determinanten des Privatschulbesuchs: schulgeldbefreite vs. nicht-schulgeldbefreite Bundesländer

	NRW, RLP, SL			Westen (Rest)			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.037***	-0.034		-0.030***	0.002		-0.043***	0.017
		(0.014)	(0.022)		(0.006)	(0.006)		(0.009)	(0.032)
Log Distanz zu oeff. Schule		0.011	0.011		0.022***	0.022***		0.006	0.006
		(0.012)	(0.012)		(0.005)	(0.005)		(0.008)	(0.008)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
50.000-100.000 Einwohner	0.032	0.024	0.021	-0.002	-0.007	-0.007	0.037	0.021	0.022
	(0.044)	(0.045)	(0.045)	(0.015)	(0.015)	(0.015)	(0.025)	(0.024)	(0.023)
100.000-500.000 Einwohner	-0.006	-0.006	-0.007	0.009	0.002	0.002	0.036	0.021	0.026
	(0.041)	(0.040)	(0.040)	(0.013)	(0.012)	(0.012)	(0.033)	(0.033)	(0.032)
500.000 Einwohner o.m.	0.033	0.026	0.023	-0.005	-0.015	-0.014	0.009	-0.010	-0.010
	(0.053)	(0.051)	(0.051)	(0.013)	(0.013)	(0.013)	(0.024)	(0.025)	(0.026)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.038*	0.041*	0.035	0.011	0.011	0.045**	0.005	-0.005	0.013
	(0.022)	(0.022)	(0.052)	(0.012)	(0.011)	(0.018)	(0.036)	(0.038)	(0.053)
Hochschulabschluss	0.054**	0.051*	0.064	0.044***	0.042***	0.066***	0.076**	0.065	0.200***
	(0.027)	(0.027)	(0.061)	(0.014)	(0.014)	(0.020)	(0.039)	(0.040)	(0.060)
Migrationshintergrund Kind	-0.040**	-0.037**	-0.067**	-0.023*	-0.023**	-0.050**	-0.085**	-0.080**	-0.121*
	(0.017)	(0.017)	(0.033)	(0.012)	(0.012)	(0.021)	(0.036)	(0.036)	(0.062)
Log HH-Einkommen	0.027	0.024	-0.001	0.014	0.013	0.012	0.058***	0.060***	0.068***
	(0.019)	(0.018)	(0.023)	(0.010)	(0.010)	(0.013)	(0.022)	(0.022)	(0.025)
Sozialtransfer	-0.023	-0.021	-0.019	-0.023***	-0.018**	-0.018**	-0.003	-0.006	-0.007
	(0.017)	(0.017)	(0.017)	(0.008)	(0.008)	(0.008)	(0.017)	(0.017)	(0.016)
Distanz * Ausbildung			0.004			-0.022***			-0.004
			(0.024)			(0.007)			(0.027)
Distanz * Hochschulabschluss			-0.009			-0.016*			-0.076**
			(0.031)			(0.009)			(0.029)
Distanz * kein Mig.			-0.017			-0.017**			-0.029
			(0.015)			(0.008)			(0.032)
Distanz * hohes Einkommen			0.018*			-0.000			-0.013**
			(0.011)			(0.004)			(0.006)
Beobachtungen	5847	5847	5847	12483	12483	12483	3686	3686	3686

Anmerkungen: Die Regressionen enthalten Jahres- und Kreis-fixe Effekte. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Table 3.A.13: Sozioökonomische Determinanten des Privatschulbesuchs: Kontrolle von Schulgeld

	Gesamtdeutschland			Westen			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.027***	-0.024*		-0.027***	-0.025*		-0.027***	0.042
		(0.007)	(0.013)		(0.008)	(0.013)		(0.010)	(0.031)
Log Distanz zu oeff. Schule		0.013**	0.013**		0.015**	0.016**		-0.004	-0.003
		(0.006)	(0.006)		(0.006)	(0.006)		(0.011)	(0.011)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
50.000-100.000 Einwohner	0.014	0.005	0.004	0.009	0.000	-0.001	0.035	0.026	0.027
	(0.023)	(0.024)	(0.024)	(0.027)	(0.028)	(0.027)	(0.028)	(0.028)	(0.027)
100.000-500.000 Einwohner	-0.010	-0.017	-0.019	-0.019	-0.025	-0.026	0.046	0.037	0.026
	(0.019)	(0.018)	(0.019)	(0.020)	(0.020)	(0.020)	(0.037)	(0.037)	(0.036)
500.000 Einwohner o.m.	0.003	-0.011	-0.009	0.000	-0.012	-0.012	-0.017	-0.037	-0.029
	(0.024)	(0.024)	(0.024)	(0.026)	(0.026)	(0.026)	(0.046)	(0.046)	(0.047)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.001	0.003	0.012	0.004	0.006	0.022	0.010	0.028	0.063
	(0.016)	(0.015)	(0.027)	(0.016)	(0.016)	(0.029)	(0.042)	(0.047)	(0.049)
Hochschulabschluss	0.019	0.018	0.053*	0.018	0.016	0.040	0.047	0.066	0.191***
	(0.018)	(0.017)	(0.029)	(0.018)	(0.018)	(0.030)	(0.044)	(0.049)	(0.056)
Kinder in HH	0.005	0.004	0.004	0.003	0.002	0.002	0.020**	0.020**	0.022***
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)
Migrationshintergrund Kind	-0.044***	-0.043***	-0.068***	-0.042***	-0.041***	-0.069***	-0.051	-0.049	-0.068
	(0.011)	(0.011)	(0.021)	(0.011)	(0.011)	(0.022)	(0.038)	(0.038)	(0.047)
Log HH-Einkommen	-0.004	-0.006	-0.022*	-0.006	-0.008	-0.026**	-0.011	-0.010	-0.024
	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.013)	(0.026)	(0.025)	(0.027)
Sozialtransfer	-0.039***	-0.036***	-0.035***	-0.039***	-0.035***	-0.034***	-0.023	-0.027*	-0.026*
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.016)	(0.016)	(0.016)
Distanz * Ausbildung			-0.008			-0.011			-0.055*
			(0.012)			(0.012)			(0.030)
Distanz * Hochschulabschluss			-0.024*			-0.017			-0.110***
			(0.014)			(0.014)			(0.034)
Distanz * Migrationsh.			0.016*			0.017*			0.011
			(0.009)			(0.010)			(0.026)
Distanz * hohes Einkommen			0.011**			0.012**			0.008
			(0.005)			(0.006)			(0.011)
Beobachtungen	10856	10856	10856	9250	9250	9250	1606	1606	1606

Anmerkungen: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

Table 3.A.14: Sozioökonomische Determinanten des Privatschulbesuchs: Schulgeld unter 100 Euro

	Gesamtdeutschland			Westen			Osten		
	M1	M2	M3	M4	M5	M6	M7	M8	M9
Log Distanz zu Privatschule		-0.027***	-0.022*		-0.027***	-0.024*		-0.030***	0.047
		(0.007)	(0.013)		(0.008)	(0.013)		(0.011)	(0.029)
Log Distanz zu oeff. Schule		0.008	0.008		0.009	0.009		-0.003	-0.001
		(0.006)	(0.006)		(0.006)	(0.006)		(0.011)	(0.011)
Gemeindegröße (Referenz: unter 50.000 Ew.)									
50.000-100.000 Einwohner	0.016	0.007	0.006	0.014	0.005	0.004	0.035	0.026	0.028
	(0.022)	(0.023)	(0.023)	(0.025)	(0.026)	(0.026)	(0.025)	(0.025)	(0.025)
100.000-500.000 Einwohner	-0.004	-0.012	-0.013	-0.013	-0.020	-0.021	0.087**	0.079*	0.069*
	(0.016)	(0.016)	(0.016)	(0.018)	(0.018)	(0.018)	(0.040)	(0.040)	(0.038)
500.000 Einwohner o.m.	-0.001	-0.015	-0.014	-0.003	-0.016	-0.015	-0.005	-0.030	-0.029
	(0.024)	(0.024)	(0.024)	(0.026)	(0.026)	(0.026)	(0.037)	(0.036)	(0.037)
Elterlicher Bildungsabschluss (Referenz: kein Abschluss)									
Ausbildung	0.002	0.004	0.014	0.003	0.006	0.020	-0.017	0.003	0.037
	(0.015)	(0.015)	(0.026)	(0.015)	(0.015)	(0.027)	(0.047)	(0.054)	(0.056)
Hochschulabschluss	0.009	0.008	0.042	0.007	0.006	0.031	0.025	0.046	0.190***
	(0.017)	(0.017)	(0.028)	(0.018)	(0.017)	(0.030)	(0.053)	(0.059)	(0.066)
Migrationshintergrund Kind	-0.031***	-0.031***	-0.044**	-0.030***	-0.031***	-0.047**	-0.057	-0.055	-0.064
	(0.010)	(0.010)	(0.020)	(0.010)	(0.010)	(0.022)	(0.039)	(0.039)	(0.043)
Log HH-Einkommen	0.004	0.002	-0.011	0.004	0.002	-0.012	-0.006	-0.004	-0.011
	(0.010)	(0.010)	(0.012)	(0.011)	(0.011)	(0.013)	(0.028)	(0.028)	(0.029)
Sozialtransfer	-0.032***	-0.031***	-0.031***	-0.033***	-0.031***	-0.031***	-0.014	-0.018	-0.016
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)	(0.016)	(0.016)	(0.016)
Distanz * Ausbildung			-0.007			-0.010			-0.059**
			(0.012)			(0.012)			(0.028)
Distanz * Hochschulabschluss			-0.023*			-0.017			-0.125***
			(0.013)			(0.014)			(0.033)
Distanz * Migrationsh.			0.009			0.011			0.001
			(0.009)			(0.010)			(0.033)
Distanz * hohes Einkommen			0.009*			0.010*			0.002
			(0.005)			(0.005)			(0.011)
Beobachtungen	10437	10437	10437	8911	8911	8911	1526	1526	1526

Anmerkungen: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Quelle: SOEP v36 für die Jahre 2002, 2005, 2007, 2011, 2013, 2015, 2017 und 2019; gewichtet, amtliche Schuldaten 2000-2019; eigene Berechnungen.

CHAPTER 4

From Low Emission Zone to Academic Track: Environmental Policy Effects on Educational Achievement in Elementary School[1]

4.1 Introduction

Traffic remains a major source of air pollution in many industrialized countries. Driving restrictions are one way to target air quality in urban areas that several countries have explored. While such measures were often deemed unpopular and ineffective,[2] Germany, along with other European countries, started introducing Low Emission Zones (LEZs) in 2008, restricting vehicle access to designated inner-city areas based on emission intensity thresholds. This policy has indeed proved effective in lowering air pollution in the treated areas (e.g., Sarmiento et al., 2021; Wolff, 2014) and, in doing so, has been shown to improve health outcomes (Klauber et al., 2021; Margaryan, 2021; Pestel & Wozny, 2021). At the same time, LEZs were found to have short-term adverse effects on self-rated life satisfaction (Sarmiento et al., 2021). To comprehensively evaluate the costs and benefits of LEZs, it is essential to consider the policy's externalities on the full spectrum of socio-economic outcomes. To date, to the best of our knowledge, no study has evaluated the effectiveness of specific driving restriction policies, like LEZs, on children's educational outcomes.

[1]We thank Lukas Hörnig for his excellent support and Henri Gruhl and Ronald Bachmann as well as participants of the RWI Brown Bag, DIW internal seminar, and BeNA summer workshop for their comments. The authors gratefully acknowledge the financial support of the German Environmental Agency (UBA) under research reference number 19121040.

[2]Davis (2008), evaluating Mexico City's *Hoy no circula* (HNC) policy, points to HNC as being high-cost and largely ineffective, primarily since it incentivized car owners to buy another car to circumvent the restriction with a second license plate.

Children are particularly susceptible to the adverse health effects of air pollution, ranging from respiratory diseases to infant mortality (e.g., Chay & Greenstone, 2003; Coneus & Spiess, 2012; Jayachandran, 2009; Knittel et al., 2016; Luechinger, 2014). Recent economic literature has shown that poor air quality may also harm the human brain (Aguilar-Gomez et al., 2022), affecting individuals' cognitive performance (Archsmith et al., 2018; Künn et al., forthcoming) and leading to behavioral problems (Mortamais et al., 2019). Given these findings, it is not surprising that air quality can also affect children's test scores (Cho, 2022; Ebenstein et al., 2016; Lavy et al., 2014; Marcotte, 2017; Persico & Venator, 2021; Requia et al., 2022; Roth, 2016; Stafford, 2015) and school absence rates (Chen et al., 2018; Currie et al., 2009). Substantially less is known about the longer-term schooling effects of policies targeting air quality.

This paper studies the causal effect of the implementation of LEZs on the educational achievement of elementary school students in Germany. We focus on the transition rates of children in 4$^{\text{th}}$ grade, the last year of primary education, to a *Gymnasium*, the academic track of the secondary school system. The German school system is characterized by the early tracking (usually at age 10) of students to different secondary school tracks. This practice has been shown to determine a child's educational and professional trajectory in essential ways: once assigned to a track, upward mobility is rare (Bellenberg, 2012; Dustmann et al., 2017; Matthewes, 2021; Mühlenweg, 2008; Müller & Schneider, 2013). Being assigned to the academic track (*Gymnasium*) is highly correlated with enrolling in university education and higher earnings later in life (e.g., Dustmann, 2004). Hence, transition rates to the academic track are an exceptionally well-suited indicator for educational achievement in Germany.

We combine several data sources to comprehensively assess the link between LEZs and school track assignments. Our main analysis relies on geo-referenced administrative school-level data from North Rhine-Westphalia (NRW), Germany's most populous federal state. Knowing the exact location of elementary schools allows us to distinguish whether they lie within or outside a LEZ, and to take school heterogeneity (regarding student and neighbourhood characteristics) into account. We complement this school-specific analysis with district-level data from all of Germany to test the external validity of our results. In addition, we shed light on the underlying channels through which LEZs affect schooling outcomes using geo-referenced data from the German Socio-Economic Panel (SOEP), which allows us to distinguish between children living within or outside a LEZ.

To account for the staggered implementation of LEZs (e.g., Goodman-Bacon, 2021), we opt for two novel approaches to estimate the causal effects of LEZs on track choice besides the standard two-way fixed effects (TWFE) estimation. The first one is the stacked-by event approach (Baker et al., 2022; Cengiz et al., 2019; Deshpande & Li, 2019) and the second one is the two-way fixed effects with heterogeneous treatment effects estimator developed by de Chaisemartin & D'Haultfœuille (2020a). The main advantage of these estimators is that they bypass the vulnerability of two-way fixed effects difference-in-differences to potential heterogeneous time effects of the policy (de Chaisemartin & D'Haultfœuille, 2020a; Goodman-Bacon, 2021). This is important in our setting as the effects between the first and last introduction of LEZ may have changed, e.g., due to the changes in the vehicle fleet.

Our results based on school-level data from the state of NRW imply that the implementation of LEZs increased rates of transition to the academic track by 0.9-1.6 percentage points. Effects take some time to materialize, which is in line with the underlying channels, as the adverse effects of air pollution accumulate over time. Our analysis using the district-level data for all of Germany suggests that the effect is not merely a state-specific phenomenon. In addition, our heterogeneity analysis indicates that boys drive the results. Finally, we find suggestive evidence that a reduction in the prevalence of respiratory infections in the respective age group is a likely channel through which LEZs affect schooling outcomes. This finding is in line with Klauber et al. (2021), who find that LEZs lead to a reduction of asthma drug prescriptions for children. The more substantial schooling effect found for boys substantiates this premise since asthma is more prevalent in boys during childhood (e.g., Bjornson & Mitchell, 2000; Postma, 2007). Another potential channel could be a reduction in ADHD, which is also more prevalent in boys (Schlack et al., 2007).

Our study makes several contributions. First, our findings add to our knowledge of the efficacy of LEZs in improving health and socio-economic outcomes. Pestel & Wozny (2021) show that the introduction of LEZs in Germany reduced the number of hospitalizations due to circulatory and respiratory conditions. Margaryan (2021) further suggests that LEZs effectively lower the number of patients with cardiovascular disease by 2–3 percent, with a particularly pronounced effect on elderly patients (7–12.6 percent). Wolff (2014) provides evidence that the health benefits of the policy imply lower health expenditures. Klauber et al. (2021) find that newborns exposed to cleaner air needed less medication for respiratory diseases. Gehrsitz (2017) finds minor effects on the number of stillbirths but no impact on infant health. In contrast, looking at self-rated life satisfaction, Sarmiento et al. (2021) discover that LEZs can temporarily have adverse effects on the well-being of residents. We extend this literature by focusing on the schooling effects of LEZs.

Second, our analysis contributes to our understanding of how exposure to air pollution affects educational attainment. Thus far, several studies have focused on the immediate (Heissel et al., 2022; Lavy et al., 2014; Marcotte, 2017) and longer-term (Ebenstein et al., 2016) effects of acute short-term variations in pollution exposure. In addition, some authors have examined how exposure to lower air quality during gestation and early life affects human capital formation later in life (Almond et al., 2009; Bharadwaj et al., 2017; Black et al., 2019; Isen et al., 2017; Marcotte, 2017; Persico & Venator, 2021; Sanders, 2012). In contrast, little is known about how continuous exposure to different air quality levels affects educational success in the medium and long run. To our knowledge, Heissel et al. (2022) is the only study assessing the long-term effects of medium-term exposure to pollution on student outcomes in middle and high school by exploiting variation in wind patterns for schools within the same distance from major highways in Florida. Finding significant adverse effects of visiting a "downwind" high school on test scores, behavioral instances, and school absences, this study is thus far the only one shedding light on the channels through which pollution affects educational attainment. We add to these findings by focusing on the younger age group of elementary school children and providing both school-level estimates for a specific region and district-level estimates for all of Germany.

Third, our study contributes to the research on the factors determining school tracking choices. Early tracking systems like the one in Germany are generally associated with higher educational inequalities (e.g., Waldinger, 2007). Hence, it is necessary to understand the determinants of tracking decisions and the channels through which they lead to unequal outcomes. Besides the students' ability, various socio-economic factors have been shown to influence the probability of transitioning to the academic track.[3] On the other hand, school factors such as class size (Argaw & Puhani, 2018) and gender of the teacher (Puhani, 2018) do not seem to play a critical role. The link between school tracking and environmental factors has barely been explored in the empirical literature. This paper is the first to study how exposure to different air quality levels affects school tracking decisions.

These contributions feed into broader discussions on the well-being and (non-)cognitive development of school-age children and the role of environmental factors therein. While the adverse long-term effects of health shocks for preschool children

[3]For example, boys and younger students have lower chances of entering the highest track (Hendrik & Kerstin, 2011; Mühlenweg & Puhani, 2010). The same is true for children of immigrant ancestry (Hendrik & Kerstin, 2011), even after controlling for the grade point average (Kristen & Dollmann, 2010). While socioeconomic background (Dustmann, 2004) and risk preferences (Wölfel & Heineck, 2012) of parents influence the decision for the highest track, there is no causal effect of parental income (Tamm, 2008) and their employment status (Schildberg-Hoerisch, 2011).

(e.g., Almond et al., 2009) are well-researched, less is known about the school-age years (Heissel et al., 2022). The elementary school years are a critical period for determining later educational success (e.g., Dustmann, 2004), as well as for forming motivations and beliefs (e.g., Kosse et al., 2020). In addition, health shocks during childhood have lasting adverse consequences for later-life health and labor market outcomes (e.g., Schiman et al., 2017). Hence, the students in the focus of our study are in a decisive and malleable period of their (non-)cognitive development and are likely sensitive to environmental factors such as air pollution.

The paper proceeds as follows. In Section 4.2, we provide information on the implementation of LEZs and the education system in Germany. Section 4.3 provides an overview of the data and descriptive statistics. Section 4.4 explains the empirical strategy we use to analyze the implementation of LEZs on student attainment. In Section 4.5, we present the main results, test their robustness, and investigate heterogeneous treatment effects. Section 4.6 concludes the paper.

4.2 Background

4.2.1 Low Emission Zones in Germany

As more evidence on the health risks of air pollution was brought forward in the early 2000s, the European Commission responded with the Clean Air Directive as an unprecedented attempt to mitigate air pollution caused by fine particles, coarse particle matters (PM_{10}), nitrogen dioxide (NO_2) as well as several other air pollutants. In Germany, cities failing to comply with EU air quality standards must develop "Clean Air Plans" (*Luftreinhaltepläne*). Between 2005 and 2007, this was the case for 65 percent of all large German cities (Sarmiento et al., 2021).[4]

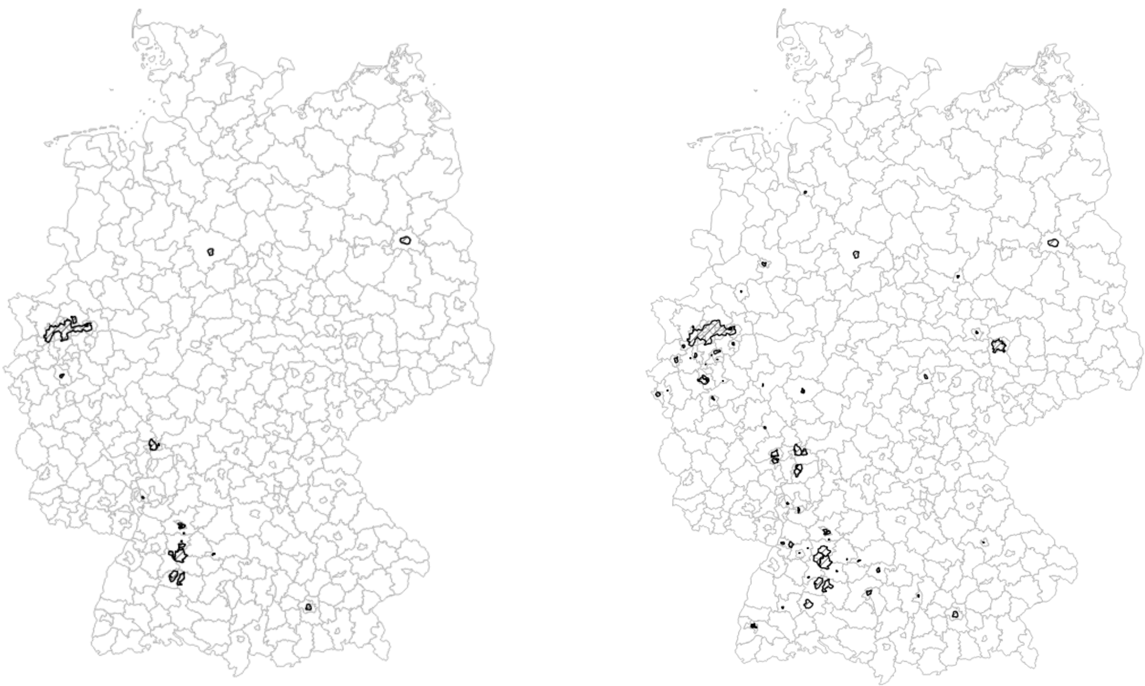
While the Clean Air Plans can consist of various measures, the most drastic has been the introduction of LEZs, which ban emission-intensive vehicles such as older diesel cars from designated areas, typically inner cities. Since vehicle traffic is a significant factor in local air pollution by particulate matter and nitrogen oxides in urban areas, restricting traffic-based pollution in the form of an LEZ was the most critical policy measure to improve air quality. The 2007 Immission Control Act (35th BImSchV) provides the legal basis for LEZs by giving local governments the right to prohibit cars not com-

[4]These legally binding standards have been in effect since 2005. Directive 2008/50/EC (EU, 2008) defines the current lawfully binding limits and detailed measurement procedures for all criteria pollutants (NO_2, SO2, PM_{10}, CO, and O3). It is a revised version of Directives 1999/30/EC (EU, 1999), 2000/69/EC (EU, 2000), and Directive 2002/3/EC (EU, 2002).

plying with specific emission standards from entering designated areas. Since the first implementation in 2008, cars must display an appropriately colored windscreen sticker based on EU-wide tailpipe emissions categories. Only vehicles bearing a respective sticker, i.e., those not exceeding predetermined levels of pollution, are allowed to enter.[5] In the first phase, bans were applied to vehicles without a sticker. In a second phase, this was gradually applied to vehicles with a red or yellow sticker (Figure A.1). Nowadays, only cars with green stickers are permitted to enter the zones.[6] The policy is enforced by the police and public order office, and violation leads to fines of EUR 100 for the vehicle driver.

Figure 4.1: Low Emission Zones in Germany, 2008 and 2018

(a) 2008 (b) 2018



Notes: Expansion of LEZs in Germany between 2008 and 2018. See Table A.2 for detailed information on the LEZs implementation dates and their stringency levels.
Source: UBA.

The introduction of LEZs is decided on a regional level involving city administrations, city councils, and local stakeholders. However, state governments can always overrule local authorities. Although the need for a Clean Air Plan and a possible LEZ depends on the previous levels of air pollution, there is idiosyncratic variation in the timing of their introduction. The decision-making process varies between different regions,

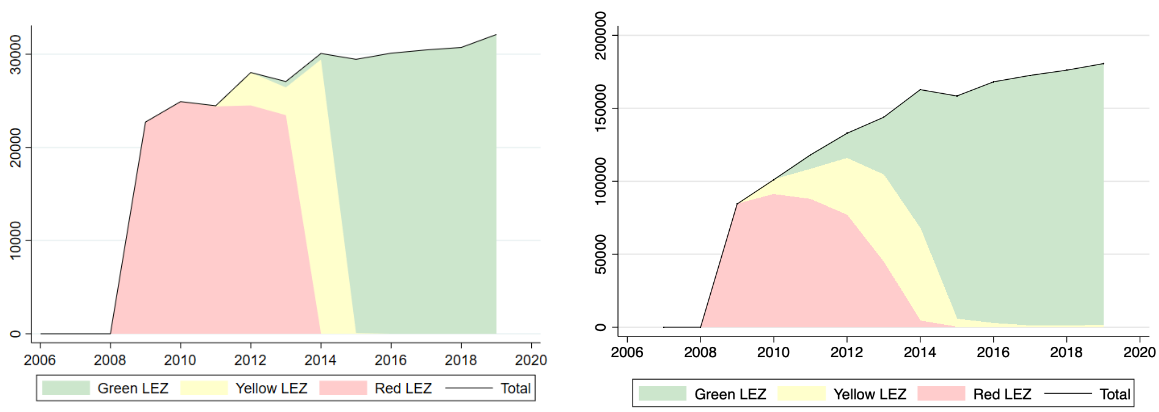
[5]Stickers are assigned based on the tax class and EURO standard recorded in the car registration book and regulated by the labeling regulation in the 35th Ordinance for the Implementation of the Federal Immission Control Act (35. BImSchV).
[6]One exception is Neu-Ulm, where yellow stickers are still allowed.

depending on conflicting interests. Further, there are several stakeholders that advocate against or in favor of LEZs. For example, lawsuits both in favor of and against the introduction have been initiated by local stakeholders (see Klauber et al., 2021, for a detailed discussion).

The first LEZs were introduced in 2008, predominantly in the largest cities (12 LEZs in 20 cities). As of 2022, this number has increased to 56 (see Table A.2). Figure 4.2 reflects the first sharp and then more gradual increase of the number of LEZs by showing the evolution of the number of 4$^{\text{th}}$ grade elementary school students living inside LEZs of different stringencies over the observation period.[7] Compared to all of Germany, the majority of LEZs in NRW were introduced within the first implementation wave (see Table A.2 for detailed information on the LEZ implementation dates and their stringency levels).

Figure 4.2: Elementary school students covered by LEZs in NRW and all of Germany

(a) NRW (b) Germany



Notes: Cumulative number of students at schools inside LEZs in NRW (Panel (a)) and in districts which contain a LEZ in Germany (Panel (b)).
Source: UBA and IT.NRW (Panel (a)); UBA and and bildungsmonitoring.de (Panel (b)).

4.2.2 School system in Germany

Education policy in Germany is decentralized and regulated by the federal states. However, while some aspects of the education system vary across states, the Standing Conference of the Ministers of Education and Cultural Affairs of the federal states (Kul-

[7]Since we only have school-level administrative data for NRW, the Germany figure depicts the number of elementary students living in districts that contain a LEZ. See section 4.4 for details.

tusministerkonferenz; KMK) harmonizes education policies between states in terms of the general structure and curriculum (KMK, 2014).

Compulsory elementary education starts when children are around six and usually lasts for four years.[8] Based on their performance in third and fourth grade, children are then divided into different tracks. In Western Germany, the secondary school system comprised three vertically ordered tracks: the basic track (*Hauptschule* lasting five years), the middle track (*Realschule* lasting six years), and the academic track (*Gymnasium* lasting eight to nine years). Over time, due to the lack of prospects for graduates of the lowest track (e.g., Helbig & Nikolai, 2015; Matthewes, 2021), many Western German federal states moved from the three-tier to a two-tier system. This system, traditionally common in Eastern Germany, merges the low and middle track while retaining the three different school-leaving certificates. Several federal states, among others NRW, have also adopted different comprehensive secondary schools (*Gesamtschulen)* where children are taught together beyond elementary school.[9] These schools offer different educational tracks at the same school, allowing students to either leave school with a general degree (*Hauptschulabschluss*) at age 15, a secondary school-leaving certificate (*Mittlere Reife*) at age 16, or to attend upper secondary school and sit the university-qualifying exams (*Abitur*, academic track).[10]

The academic track differs substantially from the non-academic track(s) in terms of curriculum and peer composition. It has the most demanding curriculum and is the only track granting access to university. In the last year of elementary school, the headteacher gives the track recommendation, which is not generally strictly binding. The exact rules again differ by the federal states. In most federal states, except for Bavaria, Brandenburg, Saxony, and Thuringia, the teacher's recommendation is not binding. However, it is usually the case that parents follow the teacher's recommendation (Bos, 2003). Once assigned to a track, mobility across tracks is rare, with upward mobility, i.e., moving from the lower to the higher track, especially difficult (e.g., Bellenberg, 2012; Dustmann, 2004; Dustmann et al., 2017). Only 2.2 percent of all students in

[8]In Berlin and Brandenburg, children remain in elementary schools for six years. In Schleswig-Holstein, even though elementary school ends after grade four, the first two years of secondary school are track independent, i.e., the tracking decision also takes place after grade six (KMK, 2014).

[9]In NRW, there are several types of comprehensive schools with minor organizational differences. Besides *Gesamtschulen*, these schools can be called *Gemeinschaftsschulen*, *Sekundarschulen*, and *Primusschulen*.

[10]In NRW, there are several types of comprehensive schools, which differ mainly in terms of the timing of the tracking. For example, integrated secondary schools (*Integrierte Sekundarschule*), introduced in 2011, teach all students together for two more years after elementary school and offer separate educational programs starting in grade seven. *Primusschulen* offer elementary and secondary school together.

grades 7 to 9 change track in NRW.[11] Further, only about 5.5 percent of all students entering 11th grade had been at one of the lower tracks in 10th grade.[12] Hence, performance in elementary schools and the subsequent tracking have broad implications for a child's educational and professional career.

4.2.3 School reforms in North-Rhine Westphalia

North Rhine-Westphalia changed the rules regarding secondary school tracking for a short period between 2006 and 2010 from a non-binding to a binding system. During these years, children whose parents disagreed with the recommendation still had the opportunity to attend three-day trial lessons. They had to pass exams in German and mathematics with specific grades to be accepted into a *Gymnasium* against the recommendation of their headteacher (Ministry of Education North Rhine-Westphalia, 2012). While this policy change could well have affected the transition rates during this period, we do not consider this to endanger our identification since there is no reason to believe this rule affected our treatment and control groups differently.

In addition, in 2006, the state government decided to reform the education system in two important ways: first, to abolish catchment areas in all municipalities in NRW as of the 2008/09 school year, and second, to decrease the number of elementary schools (Makles & Schneider, 2012). Allocation to elementary school was traditionally organized through catchment areas, making the geographical distance to the children's homes the primary determinant of school choice at the elementary school level. The dissolution of the school districts was justified, on the one hand, by the introduction of competitive elements between the schools and, on the other hand, by the desire to take parental preferences in the choice of a suitable school more into account. This was also expected to provide support for the decisions on school closures. Schools that were not in demand could be closed without major resistance.

Makles & Schneider (2012) study the determinants of school choice in the light of the 2008/09 reform in the city of Wuppertal and find that when given more freedom in school choice, students tend to favor schools that are close to their homes and that have higher transition rates to the academic track. Hence, the reform may have led to students sorting into schools with higher transition rates to *Gymnasium* and schools

[11] See Landesdatenbank NRW 21111-123is Allgemeinbildende Schulen (D12.3): Schulformwechsel in den Jahrgängen 7 bis 9 nach Geschlecht, Nationalität, Schulform und Schulform der Zielschule - Gemeinden - Schuljahr, 2021/2022

[12] See Landesdatenbank NRW: Allgemeinbildende Schulen (D12.3): Schulformwechsel in den Jahrgängen 7 bis 9 nach Geschlecht, Nationalität, Schulform und Schulform der Zielschule - Gemeinden - Schuljahr

with lower rates to have a higher likelihood of being closed. Figure D.2 indeed shows an increasing trend in transition rates to the academic track after 2007 for both treatment and control group.

In a separate analysis for all of NRW, however, Makles & Schneider (2011) show that the reform has not affected segregation measures in schools.[13] This may be seen as an indication that the reform did not lead to a concentration of children with high socio-economic status (SES) – with higher average transition rates to the academic track – in certain areas, which could potentially correlate with the location of LEZs and could endanger our identification. To further test this premise, we analyze whether, during our observation period, districts with LEZs were differently affected by school closure rates than districts with no driving restrictions. Figure B.1 provides evidence that the introduction of LEZs is not associated with the rate of school closures due to the 2006 reform.

4.3 Data and Descriptive Statistics

4.3.1 Administrative school-level data

The administrative school-level data is provided by the North Rhine Westphalian state statistics office (IT.NRW) and contains information on the number of students transitioning from elementary school (after grade 4) to the different secondary school tracks. For our main analysis we To avoid potential biases in data due to the Covid-19 pandemic, which may have affected school transitions, we restrict our analysis to the school years from 2005/06 to 2018/19. In 2005/06, there were 3,425 elementary schools in the data set, while the number was reduced to 2,720 in the school year 2018/19.[14] The data contains the total number of students graduating from each elementary school after 4$^{\text{th}}$ grade at the end of the school year (July) and which school type they are transitioning to. Those school types comprise the *Gymansium*, which is the standard academic track option, schools which also offer the academic track[15], and schools that do not offer an academic track [16]. The data further comprises public as well as private schools.[17] Moreover, the number of students can be disaggregated by

[13]However, evidence on that matter is mixed. Some analyses focusing on more narrow regional developments point in a different direction. For example, a mixed-method study for the city of Mühlheim, Ramos Lobato & Groos (2019) finds an increase in segregation as a result of the reform.

[14]See 4.2.3 for details on the reforms leading to the closure of elementary schools in NRW.

[15]*Gesamtschulen, Sekundarschulen, PRIMUS-Schulen* and *Gemeinschaftsschulen*

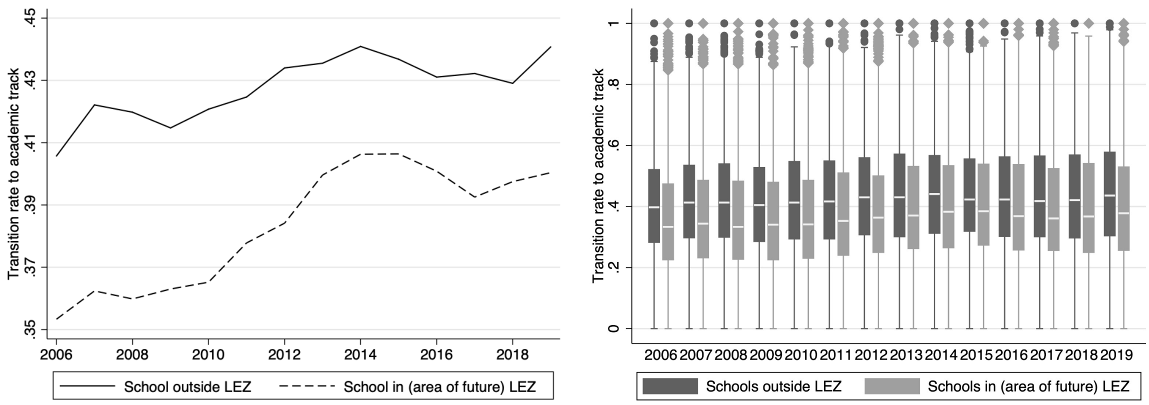
[16]*Realschulen* and *Hauptschulen*

[17]We restrict the empirical analysis to public schools since the catchment area of private schools will be larger than the neighborhood of the school. While the private school sector is growing especially

sex and nationality.[18] Table A.3 depicts the descriptive statistics for different school types inside and outside LEZs. We focus on the transition rate to the academic track (*Gymnasium*). This school type is the central one leading to the *Abitur*, which is the entrance requirement for universities.[19] Figure 4.3 depicts the transition rate to the academic track for schools which lie inside a (future) LEZ and schools outside. The average transition rate to the academic track is 43.4 percent for schools outside LEZs, while it is 38.9 percent for schools inside (future) LEZs. Moreover, as Figure 4.3 shows, the average transition rates tend to increase over time.

Figure 4.3: School level transition rates to Gymnasium in NRW by LEZ status, 2006 – 2019

(a) Mean (b) Distribution



Notes: The left panel displays the average transition rates to the academic track for schools outside of LEZs and for schools, which at any point between 2005-2018 are inside a LEZ. In the right panel, the distribution of school-level transition rates is displayed for both types of schools via boxplots. The transition rates are weighted by the number of students. The comparison group comprises large cities with > 100,000 inhabitants.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

We match the school IDs to school address lists to determine whether a school lies within a LEZ.[20] We then proceed to identify schools inside LEZs, considering the temporal and spatial dynamics of LEZs. The black dots in Figure 4.4 represent elementary schools in NRW and their location.

in Eastern Germany, it still does not play a significant role at the primary school level (e.g., Helbig, Marcel, Schmitz, Laura and Weinhardt, Felix, 2022).

[18]Nationality is coded as German nationality and non-German nationality. This data should be interpreted cautiously as the numbers of non-Germans are low, and since there have been changes to the nationality rules in Germany, identification by nationality is challenging.

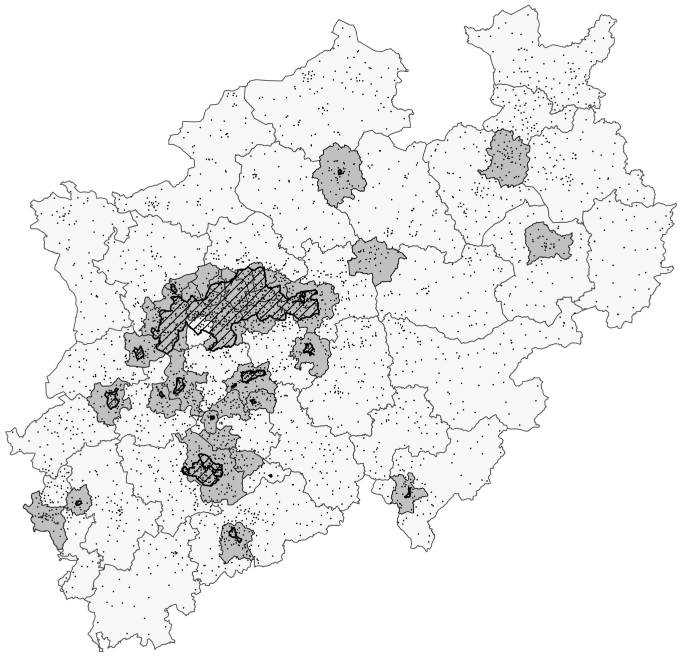
[19]See Section 4.2.2 for the institutional background.

[20]School addresses of schools that were not matched were added manually.

4.3.2 Administrative district-level data

In addition, we use aggregated district-level data for all of Germany (except for Saarland) for an additional analysis checking the external validity of the results found for NRW (Section 4.5.4). This data was provided by the respective statistical offices and is collected online.[21]

Figure 4.4: School locations, LEZs and comparison sample in NRW, 2018



Notes: Each dot represents the location of an elementary school in NRW. The main comparison sample (large cities with > 100,000 inhabitants) is shaded in grey. The dashed area represents the extent of LEZs in NRW in 2018.
Source: UBA and IT.NRW.

4.3.3 Low Emission Zones data

Data on the history of implementation, stringency (ban of Euro 1-3 vehicles), and geographic coverage is provided by the Germany Environmental Agency (UBA, Umweltbundesamt).[22] In our analysis, the main treatment variable is a binary indicator for whether a school is located inside an active LEZ area. As the implementation dates of LEZs do not necessarily coincide with the start of the school year (starting typically in August or September and lasting until June or July of the following calendar year), the LEZ treatment variable is one if at least half of the school year is treated by an active LEZ. For example, for a given school in the school year in calendar years $t/t+1$,

[21]The data can be retrieved from bildungsmonitoring.de.
[22]Table A.2 in the Appendix lists the introduction date and stringency of all LEZs in Germany.

the LEZ variable takes the value of one if the respective LEZ was introduced between 1 July and 31 December of year t and zero if it was only introduced between 1 January and 30 June of year $t+1$. Note that in the following, we refer to school years by the latter calendar year $(t+1)$ when the transition from elementary to secondary school takes place.

In terms of spatial identification, the school-level and district-level analyses differ. While we can geo-reference each elementary school and thus identify schools inside LEZs for the administrative school-level data in the state of NRW (see Section 4.3.2), the aggregated district-level data for all of Germany does not allow such a granular identification. We define "treated" districts as those districts which contain an LEZ. In case the LEZ does not cover the entire surface of the district, this should give us lower-bound estimates as areas that have not experienced air quality improvements due to the introduction of the LEZ are included.

4.3.4 Pollution data

Data on air pollution levels is provided by the air pollution monitoring system of the German Federal Environment Agency. We use data on all stations measuring the concentration of nitrogen dioxide (NO_2) and particulate matter (PM_{10}) between 2003 and 2018. We first match the station IDs to data on the exact location of the stations and merge this data with our low emission zones data via geo-coding to determine whether a school lies within a LEZ. The variables of interest are the yearly averages of pollutants.[23] Table 4.1 gives an overview of air pollution levels for stations inside and outside of LEZs. More than 800 stations measure pollution within LEZs for NO_2 and PM_{10}. The pollution levels are significantly higher within LEZs than outside LEZs.

4.3.5 RWI-GEO-GRID data

We use the RWI-GEO-GRID data (Breidenbach & Eilers, 2018) for further information on the neighborhood of each elementary school. This data set covers aggregate information for all of Germany on the 1km×1km grid cell level. The definition of grid cells follows the European INSPIRE regulation. The RWI-GEO-GRID data comprises information on the composition of the residential population regarding age, gender, nationality, and migration background. Further, there is information on

[23] Another possibility to check the impact of LEZ on the exposure of elementary school children to pollution is to exclude the summer vacation months from the analysis. A robustness check excluding the month of August yields very similar results (available upon request.

Table 4.1: Comparison of pollution levels within and outside LEZ, Germany and NRW

	unit	(Future) LEZ	No LEZ	Difference
		Germany		
Nitrogen Oxide (NO_2)	µg/m3	41.51	26.89	-14.62***
Particulate matter (PM_{10})	µg/m3	24.37	22.31	-2.05***
Number of stations		915	5548	6463
		NRW		
Nitrogen Oxide (NO_2)	µg/m3	40.32	31.07	-9.26***
Particulate matter (PM_{10})	µg/m3	25.26	24.39	-0.87*
Number of stations		296	605	901

Notes: The table shows differences of average NO_2 and PM_{10} levels for stations outside and within LEZs for Germany and NRW.
Source: UBA, years 2003-2018.

the aggregated available income, the share of households with credit failure risk, the unemployment rate, average household sizes, and the number and type of buildings. Finally, there is information on car density and the composition of cars regarding size and brand. The RWI-GEO-GRID data spans from 2005 to 2021, except for 2006 to 2008. We linearly interpolate those years to have a balanced data set. Table 4.2 depicts some descriptive statistics for key socio-economic characteristics of the grids where the elementary schools are located. The purchasing power per capita is lower, while the unemployment rate and the share of foreigners are higher at the grids inside a (future) LEZ. In sum, the neighborhoods of elementary schools outside LEZs tend to be economically better off.

Table 4.2: Comparison of grid characteristics between treatment and comparison group, NRW

	unit	(Future) LEZ	No LEZ	Difference
Purchasing Power per capita	€	19512.71	22354.96	2842.24***
Share of foreign nationals	%	15.95	10.89	-5.05***
Unemployment rate	%	12.44	7.93	-4.51***
Share of households with children	%	24.41	30.20	5.78***
Number of schools		8329	9612	17941

Notes: Tables depicts the comparison of the average grid value of schools inside a (future) LEZ vs. grid values of schools outside LEZs for the sample of large cities (> 100,000 inhabitants).
Source: UBA, IT.NRW, and RWI-GEO-GRID.

4.3.6 SOEP

We use geo-referenced data from the German Socio-economic Panel (SOEP) to examine the underlying channels of the effect of the introduction of LEZs on track choice. The SOEP is an annual, nationally representative survey covering information on demographics, household composition, educational outcomes, and labor market characteristics of nearly 13,000 households and 30,000 individuals (Goebel et al., 2019). With the anonymized regional information on the places of residence of SOEP respondents, regional indicators can be linked to the SOEP data through matching by municipality or zip codes. For all years since 2000, it is possible to trace respondents' places of residence back to the street-block coordinates. This information allows us to precisely identify children residing within LEZ, and to build a control group similar to our main specification.[24] We consider a child as treated when they have lived within a LEZ starting from age 7.[25] For our outcome variables, we use information from the mother-and-child questionnaire asking parents questions on their child's health, schooling, and well-being at age 9-10, i.e., shortly before they transition to secondary school. Hence, we observe the outcomes when the children in the treatment group have had at least two years of exposure to LEZ.

4.4 Empirical Strategy

We evaluate the changes in school-level transfer rates to the academic track following the implementation of LEZs using the difference-in-difference methodology. Until recently, using a two-way fixed effects (TWFE) model with the following form was the norm for recovering the difference-in-differences estimates of the average treatment on the treated (ATT):

$$Y_{i,t} = \beta^{TWFE} LEZ_{i,t} + \gamma\, X_{i,t} + \lambda_i + \phi_t + \varepsilon_{i,t}, \tag{4.1}$$

[24] As in our school-level analysis, we limit our sample to individuals residing in urban areas (municipalities with at least 100,000 inhabitants). In addition, we exclude special surveys, such as the M1 and M2 Migration samples and the M3 Refugee sample.

[25] Individuals who move between ages 7 and 9 are excluded from the analysis.

where $Y_{i,t}$ is the transition rate for school i in year t and is regressed on the treatment variable LEZ_{it}, school fixed effects (λ_i), year fixed effects (ϕ_t), $X_{i,t}$ a set of time-varying GRID characteristics [26] and standard errors clustered at the district level $\varepsilon_{i,t}$.[27]

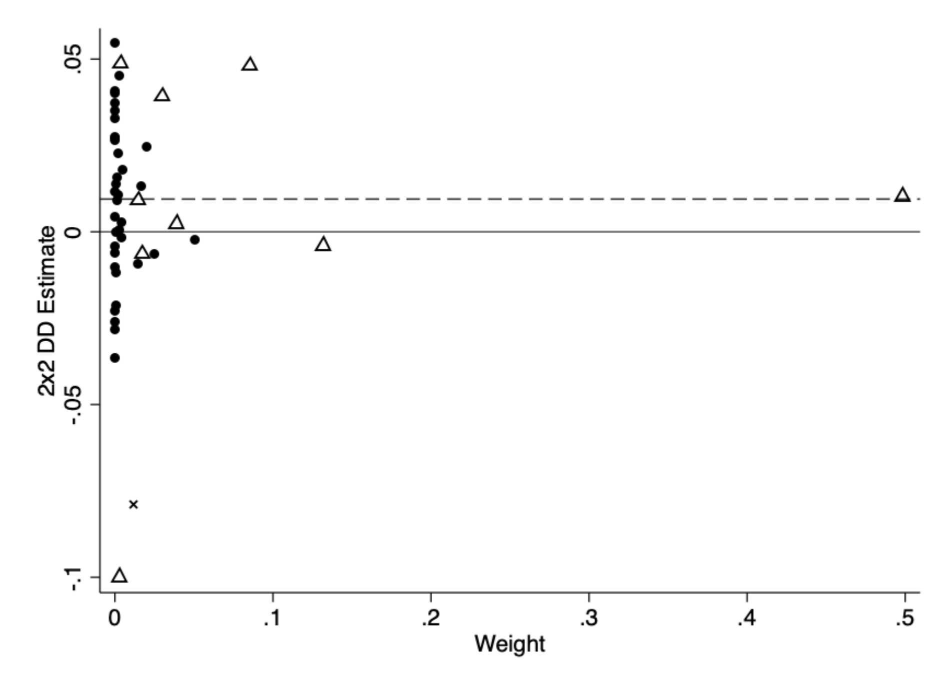
The difference-in-differences coefficient is generally thought of as the coefficient β^{TWFE}. Recent contributions have, however, highlighted potential issues with this interpretation (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020a; Goodman-Bacon, 2021; Wooldridge, 2021). In other words, when there are many periods and the treatment implementation is staggered, the β^{TWFE} may represent a biased approximation of the true underlying ATT. A weighted average of all 2×2 comparisons of "switchers" and "non-switchers" is estimated when there is variability in the treatment effects over time or between groups. These comparisons include potentially problematic comparisons such as comparing later treated to earlier treated units and "clean" comparisons between treated and not-yet-treated units (Goodman-Bacon, 2021). This may lead to negative weights in the weighted average, which may result in a downward bias or even a negative coefficient, even when all underlying ATTs are positive (de Chaisemartin & D'Haultfœuille, 2020b). These problems are more likely to occur as treatment outcomes differ between treatment groups or over time. Since the vehicle fleet's makeup changed between the first and last introduction, the staggered adoption of LEZ in our situation may have caused time-varying treatment effects.

We evaluate the extent to which our analysis may suffer from this bias by performing a Goodman-Bacon decomposition (Goodman-Bacon et al., 2019). The command produces a scatterplot of the 2×2 difference-in-differences estimations and their corresponding weights (Figure 4.5). By far, the largest weight is assigned to the 2×2 comparison of the first-wave early treated vs. the never treated group. Overall, the treated vs. never treated group receives a weight of 0.83, and the early vs. late and late vs. early groups have a weight of 0.16. The estimates by the latter (0.004) are substantially smaller than those estimated by the former (0.012), albeit not negative, indicating that our TWFE estimates may be slightly downward biased. The third group, labeled "within", tells us how much time-varying controls drive our estimates. Although this group gets the smallest weight in the Goodman-Bacon decomposition, the corresponding beta is negative, implying that controlling for the yearly grid-level covariates is important.

[26] In our case these time-varying characteristics include purchasing power per capita, unemployment rate, share of foreigners, and share of households with children.

[27] We conservatively cluster at the district level since the decision on whether a LEZ is implemented is taken at the administrative district (*Regierungsbezirk*) level in collaboration with the district/city.

Figure 4.5: Bacon decomposition



	Components	Beta	Weight
•	Early vs. Late & Late vs. Early	0.0040	0.1626
△	Treated vs. Never Treated (C)	0.0118	0.8254
×	Within	-0.0792	0.01203

Notes: This figure implements the Goodman-Bacon (2021) decomposition using the large sample and the purchasing power per capita, the unemployment rate, the share of foreigners and families (all variables at the grid cell level) as time-varying control variables. The command is run on a balanced panel.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

In recent years, numerous proposals of alternative difference-in-differences estimators that are robust to heterogeneous treatment effects across time and/or cohorts have been made (Borusyak et al., 2022; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020a; Sun & Abraham, 2021). All these estimators have in common that they only use the never-treated and the not-yet-treated as comparison groups.

In this study, we make use of a stacked event-by-event design (SD) (Baker et al., 2022; Cengiz et al., 2019; Deshpande & Li, 2019) as well as the estimator suggested by de Chaisemartin & D'Haultfœuille (2020a) (dC&D'H). We opt for this combination of estimators because the SD is the simplest and most transparent way to solve the negative weight problem. While many of the new estimators are quite restrictive in the use of fixed effects, linear trends or the inclusion of time-varying control variables, the stacked design allows for such flexibility. In this approach, we develop event-specific data sets that include the outcome variable, controls for the treated state, and controls for any other "clean controls" that do not introduce LEZs within the 9-year observation period (t = -2 to t = 6). Then, using a single set of treatment indicators, we stack these event-specific data sets in relation to time to get the average effect across all events. The dC&D'H estimator is arguably the most flexible and comprehensive approach, yielding time-specific ATTs for each time period after treatment, averaging across numerous cohorts that get treated at various intervals. An important reason for us to prefer this method over similar ones is that it allows for time-varying covariate controls, which may play a key role in our setting, as indicated by Figure 4.5. An additional advantage is that it offers the option to include non-parametric time trends for different groups.

Identification in the difference-in-differences framework relies on a number of identifying assumptions. First, we need to make the canonical difference-in-differences framework's identifying assumption – that the prospective outcomes for the untreated and treated follow parallel trends. The effect is identified even if there are shocks affecting the potential outcome, as long the severity of the shock is not correlated with the location of LEZs. Evidence that there are no substantial pre-trends may be inferred from the analysis of event study estimates (see Figure 4.6). In addition, we run balancing tests to investigate possible compositional changes which could have happened due to the introduction of the LEZs. For this, we take the control variables purchasing power per capita, unemployment rate, the share of foreigners, and the share of households with children in the respective grid cells as the dependent variables (Table B.2). The results suggest that most of these variables were not affected by the introduction of LEZs. One exception is the share of foreigners, which increased by 0.3 to 0.5 percentage points in neighborhoods within LEZs. This finding could be related to the fact that the share of foreigners in Germany generally increased in the last decade, e.g., due to the increased intake of refugees after 2015. Since students of immigrant ancestry are less likely to transition to the academic track (e.g., Hendrik & Kerstin, 2011), this result would, if anything, suggest an underestimation of the true effect of LEZ on transition rates.

To justify the identifying assumptions, we select a comparison group of schools that are not treated (i.e., which don't lie inside a LEZ) and are likely to be similar to the treatment group in (un)observable characteristics. In our main specification, we restrict the sample to large cities with more than 100,000 inhabitants, which excludes rural and less densely populated areas (see Figure 4.4). Table 4.2 depicts the differences between the characteristics of the treatment and the control group (never treated). While Table 4.2 indicates some baseline differences, identification relies on comparing trends and shocks that may be related to the treatment; hence differences in levels do not represent a problem. We further include school and administrative region-by-year fixed effects.[28] Finally, we add time-varying control variables on the 1km×1km grid cell level (see Section 4.3.5) to account for changes in the socio-economic status (SES) composition on the neighborhood level, which may influence the evolution of the transition rates to the academic track. Specifically, we include the unemployment rate, purchasing power per capita, foreign inhabitants share, and households with children within the grid cells (see Table 4.2).

4.5 Results

4.5.1 LEZ effects on air quality

Since the effectiveness of LEZs in reducing air pollution has been demonstrated widely by previous studies (e.g., Gehrsitz, 2017; Pestel & Wozny, 2021; Sarmiento et al., 2021; Wolff, 2014), we only briefly touch on this issue. We report reductions in nitrogen dioxide (NO_2) and coarse particulate matter (PM_{10}) since data coverage is the largest for these two pollutants, and they are the most relevant regarding traffic emissions and health outcomes (see Pestel & Wozny 2021 for an overview). Table 4.3 provides an overview of TWFE and dC&d'H estimates of the reduction of NO_2 and PM_{10} levels in Germany and NRW.

Our findings for all of Germany suggest that the introduction of LEZs decreases NO_2 levels by 1.6-2.1 micrograms per cubic meter ($\mu g/m^3$) or 6.5-7.5 percent of the mean. The average PM_{10} levels are reduced by 0.8-1.3 $\mu g/m^3$ or 3.7-5.7 percent of the mean, similar to the findings of Pestel & Wozny (2021). Our estimates for NRW are less precisely estimated because of the much smaller sample and large gaps in the data. For the NRW sample, only the reduction in NO_2 is statistically significant both using a TWFE design and the dC&D'H estimator, pointing to a reduction of 1.6-1.7 $\mu g/m^3$, which corresponds to 4.8-5.7 percent of the NRW mean. This marks a medium to large effect compared to similar driving restriction policies in other countries.[29] It is plausible that NO_2 is most strongly affected by the driving restriction policy since motor vehicle

[28] Administrative regions (in German *Regierungsbezirke*) for NRW are the regional administrative entities between districts and the state. This entity also serves as the upper-level supervisory school authority, which motivates its usage as a fixed effect to account for different trends across the administrative regions. We refrain from accounting for district × time trends since the LEZ of Herne spans the entire district, resulting in this district being entirely absorbed.

[29] For example, Ellison et al. (2013) find that concentrations of particulate matter within the low emission zone in London dropped by 2.46–3.07 percent. Larger effects are reported for areas with higher baseline pollution like China. Viard & Fu (2015) show that alternate-day driving restrictions in Beijing reduce particulate matter by 22 percent during every-other-day and 15 percent during one-day-per-week restrictions.

exhaust accounts for up to 80 percent of NO_2 pollution (Environmental Protection Agency, 2016). Because of its significant impacts on human health (Schneider et al., 2018; Vitousek et al., 1997) and particularly respiratory infections in children (Janke, 2014; Kampa & Castanas, 2008), we assume that any reduction in NO_2 pollution can affect educational outcomes.

Table 4.3: Impact of LEZs on air pollution in Germany

	Germany			NRW		
			NO_2			
Low Emission Zones	-1.5752***	-1.5986***	-2.1654***	-1.7114**	-1.8303**	-1.9394***
	(0.4846)	(0.2840)	(0.4125)	(0.6886)	(0.7555)	(0.6065)
Observations	4968	15501	4968	655	1178	655
			PM_{10}			
Low Emission Zone	-0.8286**	-0.7110***	-1.3116***	-0.5230	-0.1350	-0.2464
	(0.3606)	(0.2708)	(0.4305)	(0.7027)	(0.8067)	(0.7222)
Observations	4642	13987	4642	695	1191	695
TWFE	✓	-	-	✓	-	-
Stacked	-	✓	-	-	✓	-
dC & D'H	-	-	✓	-	-	✓
Grid controls	✓	✓	✓	✓	✓	✓

Notes: This table displays the results for the effect of Low Emission Zones on nitrogen dioxide (NO_2) and coarse particulate matter (PM_{10}). Each coefficient is the result of a separate regression controlling for monitor station and year fixed effects as well as time-varying grid-level controls.
*** p<0.01, ** p<0.05, * p<0.1.
Source: UBA.

4.5.2 LEZ effects on transitions to the academic track

Table 4.4 presents the results of the stacked TWFE estimation of the ATT of LEZs on academic-track transition in NRW.[30] Following Cengiz et al. (2019), specifications include event time × wave FEs and group × wave FEs to account for the stacking procedure. Gradually, we add more restrictive fixed effects, time-varying controls, and linear district trends. We find a positive and statistically significant effect of the introduction of LEZs on transition rates to the academic track ranging between 0.9 and 1.2 percentage points. To put this into perspective, the average transition rate of all schools in the sample is 42.6 percent. The effect estimate thus indicates a 2.0 to 2.8 percent increase in the transition rate due to the introduction of the LEZ. The estimated effects are smaller than effects observed regarding school entry age (Mühlenweg, 2008)

[30] Table E.1 in the Appendix displays the results for the canonical TWFE. The results are qualitatively very similar, with coefficients between 0.009 and 0.012.

or parental risk attitudes (Wölfel & Heineck, 2012). However, the observed effect indicates a large and economically meaningful effect due to the indirect relationship.

For the dC&D'H estimation, we include the same time-varying control variables, school linear trends, and administrative district-by-school-year non-parametric trends. Figure 4.6 depicts the event study graph of the evolution of transition rates to the academic track followed by the staggered introduction of LEZs. The effect becomes statistically significant in the third year and reaches a peak in the fifth year after introduction, after which it levels out at around two percentage points. Overall, the statistically significant average treatment effect on the treated (ATT) is estimated at 1.6 percentage points. This result is consistent with the findings of the TWFE analysis in Table E.1 and the Goodman-Bacon decomposition in Figure 4.5: since TWFE appears to slightly underestimate the true effects of LEZs on track choice, it ranges around 1.6 rather than 1 percentage points.

Table 4.4: Impact of LEZs transition rates to the academic track, stacked TWFE

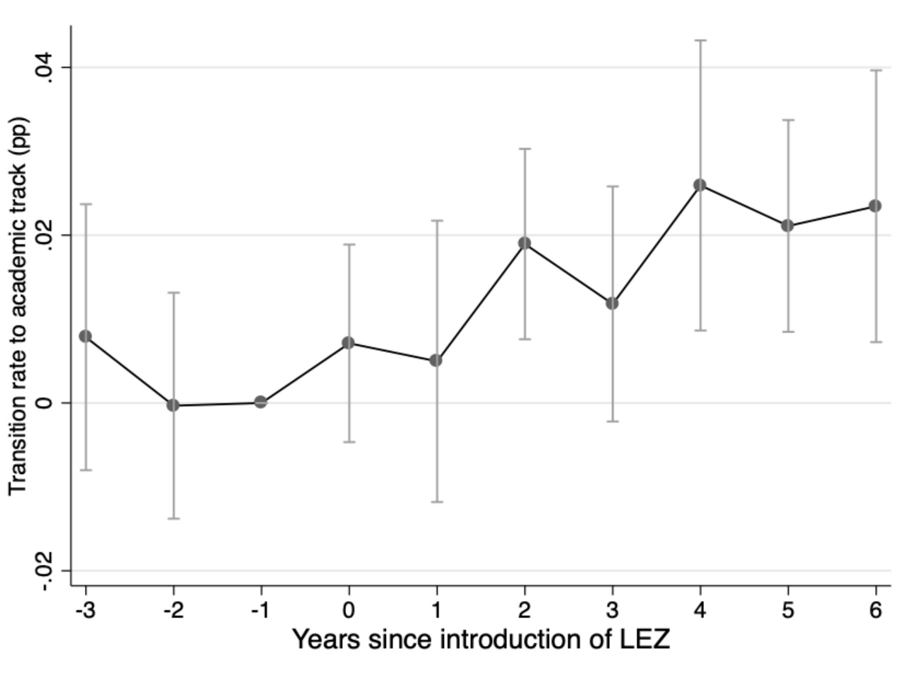
	Transition rate to academic track				
Low emission zone	0.0134***	0.0102**	0.0091**	0.0119***	0.0091**
	(0.0041)	(0.0043)	(0.0044)	(0.0045)	(0.0046)
Number of observations	47,806	47,806	47,806	47,806	47,806
Event time x Wave FEs	✓	✓	✓	✓	✓
Group x Wave FEs	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (1x1km)	–	–	✓	✓	✓
Admin. district x Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓

Notes: Sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate, and the share of households with families. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

An increase in the transition rate to the academic track implies changes to the transition rates to other school types. Given the heterogeneous school system in Germany and NRW (see Section 4.2.3), we aggregate the transition rates to three types of schools: first, the academic track (*Gymnasium*) as discussed, second, other schools which offer the option of graduating with the *Abitur*[31] and third, schools which do not

[31]Schools with "option academic track" include *Gesamtschulen, Sekundarschulen, PRIMUS-Schulen* and *Gemeinschaftsschulen.*

Figure 4.6: Event study of LEZs transition rates to the academic track



Notes: Sample: large cities(> 100,000 inhabitants). This figure depicts the dynamic difference-in-differences estimates for the effect of the introduction of LEZs on school-level transition rates to the academic track using the estimator proposed by de Chaisemartin & D'Haultfœuille (2020a) and implemented with the *did_multiplegt* Stata package. The estimation includes school linear trends and admin. district × year non-parametric trends as well as time-varying grid controls (purchasing power per capita, unemployment rate, share of foreigners, share of households with children). The plotted confidence intervals (95%) are computed using 100 bootstrap replications and are clustered at the district level. The corresponding ATT is .0164 with a standard error of 0.0060.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

offer this option.[32] Table 4.5 displays the results of the impact of the introduction of LEZs on the transition rate to the second (Panel A) and the third group (Panel B). The results indicate that while the introduction of LEZs did not significantly impact the transition rates to the comprehensive schools with academic track option, they negatively affected the share of students transitioning to the track with no option to obtain the *Abitur* (Panel B, columns 4 and 5). In addition, we amend the control group by excluding schools which also offer the option of graduating with the academic track (see Section 4.3.2). Table E.6 displays the results, which confirm our main analysis. In sum, the results indicate that the positive impact of LEZs on choosing the highest track is accompanied by a decrease in the ratio of students choosing the lowest track.

[32]Schools which do not provide an option to the academic track in NRW are *Hauptschulen* and *Realschulen*. We exclude all other schools since it is a heterogeneous group and account for less than 1 percent

Table 4.5: Impact of LEZs on transition rates to different school types except for pure academic track, stacked TWFE

Panel A: Track with *Abitur* option					
Low emission zone	-0.0020	-0.0006	-0.0011	0.0061	0.0089*
	(0.0044)	(0.0047)	(0.0047)	(0.0044)	(0.0047)
N	47,806	47,806	47,806	47,806	47,806
Panel B: Track without *Abitur* option					
Low emission zone	-0.0093	-0.0089	-0.0073	-0.0172***	-0.0171***
	(0.0061)	(0.0063)	(0.0063)	(0.0062)	(0.0059)
N	47,806	47,806	47,806	47,806	47,806
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (1x1km)	–	–	✓	✓	✓
Admin. district × Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓
Event time × Wave FEs	✓	✓	✓	✓	✓
Group × Wave FEs	✓	✓	✓	✓	✓

Notes: Schools which "Track with *Abitur* option" include *Gesamtschulen, Sekundarschulen, PRIMUS-Schulen* and *Gemeinschaftsschulen*. Schools without the *Abitur* option are *Hauptschulen* and *Realschulen*. Comparison sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate, and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

4.5.3 Effects by gender, neighborhood and school characteristics

With respect to student characteristics, our data only allows for distinguishing between girls and boys. Table 4.6 shows that the school track choice of girls and boys are differently affected by the introduction of LEZs and that the positive effect found in our principal analysis seems to be driven by boys. While for our preferred specification (columns 4 and 5), the coefficient for girls is close to and not statistically different from zero, the estimates for males are larger than the overall effect (1.7 and 1.5 percentage points increase in transition rates to the academic track) and statistically significant at the 1 and 5 percent level, respectively. In other words, the better air quality due to the introduction of LEZs seems to have a more substantial positive effect on male students than on their female peers. One way to interpret this finding is that boys react more strongly to the improvements in air quality. This could for example be the case because boys are more likely to suffer from respiratory diseases during childhood (e.g., Bjornson & Mitchell, 2000). Another reason for the stronger effect on boys could be that they tend to spend more time outside than girls of the same age, e.g. running or playing outside during the school break (Cherney & London, 2006).

Table 4.6: The impact of LEZs on the school-level transition rates to the academic track, stacked TWFE, by sex

Panel A: Female	Transition rate to academic track				
Low emission zone	0.0118**	0.0080	0.0064	0.0098	0.0060
	(0.0058)	(0.0061)	(0.0061)	(0.0061)	(0.0063)
Number of observations	47,803	47,803	47,803	47,803	47,803

Panel B: Male	Transition rate to academic track				
Low emission zone	0.0179***	0.0158**	0.0151**	0.0168***	0.0152**
	(0.0059)	(0.0066)	(0.0067)	(0.0061)	(0.0068)
Number of observations	47,801	47,801	47,801	47,801	47,801
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (1x1km)	–	–	✓	✓	✓
Admin. district × Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓
Event time × Wave FEs	✓	✓	✓	✓	✓
Group × Wave FEs	✓	✓	✓	✓	✓

Notes: Comparison sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

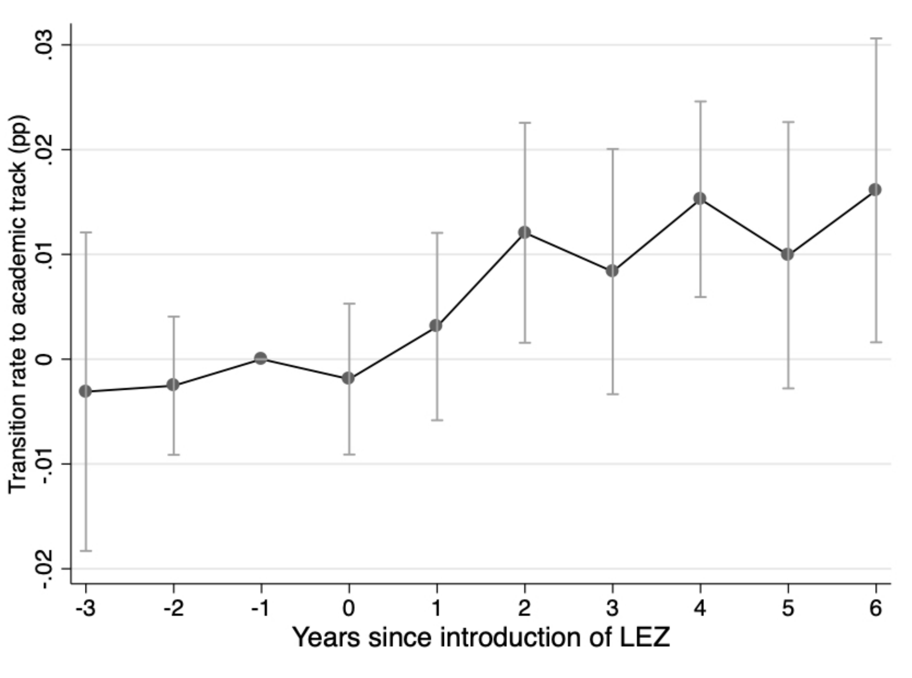
To explore further potential effect heterogeneities with respect to socioeconomic characteristics, we proxy individual with neighborhood characteristics. We find no clear heterogeneity patterns when we distinguish between above and below-median levels of grid-level characteristics (Table C.1). The estimated effect is slightly higher for socio-economically disadvantaged areas, i.e., neighborhoods with below-median purchasing power, above-median shares of unemployed and foreigners. This could be due to the fact that (baseline) air pollution tends to be higher in economically deprived regions (Lipfert, 2004). Although these differences are small and not statistically significant, we do expect our results to have implications for educational inequality. Since LEZs in Germany are located in rather deprived areas with higher baseline pollution (Table 4.1) and lower socio-economic status (Table 4.2) – positive average improvements in schooling outcomes in these areas imply an overall decrease in educational inequality.

Third, we check whether the estimated effects differ between schools that offer after-school care and those which do not. Following a large investment program in 2003 (BMBF, 2009), a rapidly increasing share of primary schools started offering afternoon programs in NRW (KMK, 2021). Since participating in after-school care implies that the children spend more time in school – usually until 3-4 pm rather than 1 pm – this could theoretically imply a stronger treatment intensity for children whose schools are located in LEZs while their homes are not. In practice, however, whether or not a school offers after-school care is unlikely to affect our results, since we expect most of the children who visit elementary schools in LEZs to also live within the same zones. In addition, more than 90 percent of all primary schools offer after-school care in NRW (KMK, 2021), and all of these schools do this in a non-integrated way, i.e., participation is voluntary. Hence, the fact that a school offers after-school care is not necessarily a strong indicator for more time spent within a LEZ. Indeed, we do not find evidence for a stronger effect of LEZ on academic-track transition rates for schools that offer after-school care (Table C.2).

4.5.4 District-level analysis

To investigate whether the results have external validity beyond North-Rhine-Westphalia, the most populous federal state (about 22 percent of all German residents live in NRW), we contrast the findings to Germany-wide district-level data as no administrative school-level data exists for all of Germany. We thus rely on aggregated district-level data where we define districts as treated once they contain a LEZ (see Section 4.3.2). We chose district-free cities as the comparison sample in this analysis as most LEZs were introduced in district-free cities, making them a suitable comparison group. The results of the district-level analysis can be found in Appendix 4.E. The dC&d'H estimator provides an average treatment effect of 0.88 percentage points which is statistically significant at the 5 percent level (see Figure 4.7). In other words, the introduction of LEZs in Germany increased the transition rate to the academic track by 0.88 percentage points. Given the treatment definition of the district-level analysis, the smaller size of the district-level coefficients is not surprising since they include areas not covered by a LEZ, which will downward bias the coefficients. It should be noted, however, that the coefficient of the district-level stacked design (0.52 percentage points) remains statistically insignificant in the most restrictive stacked specification (see Table D.3). This missing statistical significance may be the result of the small number of observations (106 districts, of which 39 were "treated" districts in 2016).

Figure 4.7: District-level event study



Notes: Comparison sample: district-free cities. The estimation includes district linear trends, as well as state-by-year non-parametric trends (to account for the educational sovereignty of the states), as well as time-varying district controls (share of foreigners, gross earnings, net migration, unemployment rate). Standard errors are computed using 100 bootstrap replications and are clustered at the district level. The ATT is 0.0088 with a standard error of 0.0040.
Source: UBA and bildungsmonitoring.de.

4.5.5 Channels

There are several channels through which a reduction in emissions as a result of LEZs could affect the educational achievement of elementary school children. First, children exposed to lower levels of air pollution may experience fewer health problems (e.g., asthma, see for example Knittel et al. 2016), leading to fewer missed school days. Second, the lower exposure to air pollution could have an effect on the students' brains, affecting the cognition of students (e.g., Graff Zivin & Neidell, 2012; Künn et al., forthcoming). Lastly, air pollutions' impact on the brain can result in mental stress and attention deficit hyperactivity disorder (ADHD) (e.g., Chen et al., 2018; Mortamais et al., 2019).

We use geo-referenced SOEP data representative for all of Germany to explore potential links between living within a LEZ and various schooling and health outcomes at age 10, i.e., at the age of the tracking decision. Since we observe the outcome variables only once per child for the relevant age group 9-10 (towards the end of elementary school) [33], we cannot apply a TWFE approach. Table 4.7 shows the results of an OLS regression controlling for district and survey year fixed effects. Respiratory diseases are the only outcome showing a statistically significant (at the 5 percent level) link to LEZ. This result implies that a reduction of respiratory infections such as asthma in LEZ – and potentially a reduction in missed school days – seems to play a role in the positive effects of LEZs on transition rates to the academic track.

Table 4.7: LEZ and individual student outcomes, pooled OLS

	Transition	Math grade	German gr.	Goal *Abitur*
Low emission zone	0.052	0.107	0.151	0.058
	(0.055)	(0.148)	(0.104)	(0.052)
Number of observations	1879	1270	1268	1390

	Good health	Resp. disease	Concentration	Enjoys school
Low emission zone	0.018	-0.126**	-0.084	0.046
	(0.036)	(0.055)	(0.225)	(0.099)
Number of observations	1430	1312	1307	1428

Notes: OLS regression controlling for district and survey year fixed effects, and individual control variables (parental education and employment status, number of children in the household, migration background, log income, social transfer receiving household). Standard errors are clustered on the district level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* SOEP v35 and UBA.

Due to the data limitations, we do not claim causality for this result. However, it should be taken as further suggestive evidence that the level of air pollution is directly related to children's health, affecting their school success. This is in line with previous findings in related studies, e.g., that children are particularly susceptible to adverse health effects of pollution (Coneus & Spiess, 2012; Stafford, 2015), and that improved air quality as a result of LEZs leads to a reduction in asthma medicine prescriptions (Klauber et al., 2021). The finding of the overall effect of LEZs on transition rates to be driven by boys presented in Section 4.5.3 further strengthens this argument

[33] Some of the outcome variables, e.g. respiratory disease are surveyed multiple times per child, i.e., at age 5/6, 7/8 and 9/10. However, the question is posed as "Has your child ever been diagnosed with one of the following diseases or disorders during a medical examination?", i.e., the variable cannot decrease over time. Hence, improvements in respiratory diseases as a result of LEZ could not be detected in a TWFE analysis.

since respiratory diseases such as asthma are more prevalent for boys at that age (e.g., Bjornson & Mitchell, 2000; Postma, 2007).[34]

4.5.6 Robustness checks

We run several sensitivity checks to test the robustness of our results. First, the expectation that the district-level estimates underestimate the true effect is further corroborated by Table 4.8, which displays the results for NRW using both the administrative school-level data as well as the district data. To make the two data sets more comparable, we use district-free cities as the comparison sample. We note that first, the district-level coefficients are smaller than the school-level coefficients, and second, the TWFE estimations are smaller than the stacked estimations, which are in turn smaller when applying the dC&d'H estimator, both accounting for the staggered treatment timing and heterogeneous treatment effects.

Table 4.8: Comparison school-level to district estimations NRW

	Transition rate to academic track		
School analysis	**TWFE**	**Stacked**	**dC & d'H**
Low emission zone	0.0103*	.0119***	0.0163**
	(0.0054)	(0.0042)	(0.0062)
Number of observations	16,118	40,055	16,118
District analysis	**TWFE**	**Stacked**	**dC & d'H**
Low emission zone	0.0047*	0.0067**	0.0111***
	(0.0026)	(0.0028)	(0.0036)
Number of observations	345	478	345
Unit FEs	✓	✓	✓
School year FEs	✓	✓	✓
Time-varying controls	✓	✓	✓
Method specific FEs	–	✓	✓

Notes: Comparison sample: district-free cities. All district-free cities in NRW have more than 100,000 residents. Only 7 large cities are not district-free cities. School analysis includes district-level time-varying controls: purchasing power per capita, the share of foreigners, and the unemployment rate. Method-specific fixed effects include event time× wave and group × wave FEs in the stacked design, and unit linear trends and admin. district/state-by-year non-parametric trends in the school and district analysis, respectively. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, bildungsmonitoring.de, INKAR (BBSR), and RWI-GEO-GRID.

Second, we amend the way we define the comparison sample by using an alternative comparison group for our analysis. We chose cities that introduced Clean Air Plans

[34] Testing the hypothesis of a stronger reduction in respiratory diseases for boys in response to LEZ with the SOEP data, we do not find a statistically significant gender effect (Table F.1. However, this could be due to the SOEP analysis being underpowered.

(*Luftreinhaltepläne*) but did not implement a LEZ, which is one possible measure to improve air quality. Clean Air Plans were introduced in cities crossing traffic exhaust-related pollutants thresholds. Consequently, the sample should be comparable in terms of air quality. Table 4.9 displays the results using this comparison sample as a robustness check. The results are very close to the sample of large cities with more than 100,000 inhabitants.

Table 4.9: Stacked TWFE, cities with Clean Air Plans

	Transition rate to academic track				
Low emission zone	0.0122***	0.0088**	0.0079*	0.0112**	0.0082*
	(0.0042)	(0.0043)	(0.0044)	(0.0046)	(0.0046)
Number of observations	50,960	50,960	50,960	50,960	50,960
Event time x Wave FEs	✓	✓	✓	✓	✓
Group x Wave FEs	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (1x1km)	–	–	✓	✓	✓
Admin. district x Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓

Notes: Comparison sample: cities that implemented a Clean Air Plan (CAP) at some point in time. Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate, and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Third, the time-varying control variables in the main results are defined at the 1km×1km grid of where the elementary school is located. We test the robustness of our results by using a broader definition of the school's neighborhood. Table E.2 re-runs the stacked specification from Table 4.4 using control variables aggregated at the zip code level. The results stay exactly the same, indicating that they are not driven by the granularity of the control variables used in the analysis.

Fourth, to avoid the negative weighting problem while sticking to a simple difference-in-differences setting, we include only the first wave of LEZs (introduced in 2008/09) in the treatment group and compare them to the group of never treated schools (Table E.3). In this setting, the coefficient is similar to the one estimated by stacking our sample but not significant in the most restrictive specification. This is likely due to the smaller number of observations.

Moreover, given that the number of schools was reduced considerably during the study period, we run the analysis on a balanced comparison sample for schools that existed for the entire period. Table E.4 displays the results. The coefficients are very similar to the unbalanced sample. In conjunction with the finding that the introduction of LEZs did not impact the likelihood of school closures (Table B.1) finding indicates that the estimations are robust to school closures.

Lastly, spillovers may violate the stable unit treatment values assumption (SUTVA) needed for causal estimates. The air quality around schools that are located just outside a LEZ is likely affected by the policy, as may be the air quality of schools just inside a LEZ (e.g. due to winds carrying air pollution). We thus create different buffers to exclude those schools from our analysis. Table E.5, Panel A displays the results excluding schools within a 500m buffer to both sides of the LEZ border, while Panel B excludes schools in a 1000m buffer outside a LEZ. In line with our expectation of positive spillovers, we find that the size of the estimate increases. This implies that our main results are conservatively estimated.

4.6 Conclusion

In 2008, several German cities started introducing Low Emission Zones, i.e., emission-intensity-based driving restrictions in urban agglomerations. The policy has been shown to effectively reduce air pollution (e.g., Gehrsitz, 2017; Wolff, 2014) and improve health outcomes (Margaryan, 2021; Pestel & Wozny, 2021) but also to exhibit social costs (Sarmiento et al., 2021). Given the large potential advantages of LEZs and the related costs of limiting mobility, it is vital to assess further spillover effects on broader areas of people's lives. One of these areas are schooling outcomes of children living within LEZs, which might be affected through different channels like better health (e.g., Klauber et al., 2021) or concentration and improved cognitive abilities (Stafford, 2015).

Children are increasingly growing up in urban centers (e.g., Bishop & Corkery, 2017; Javad, 2017), where they are exposed to high pollution levels on a daily basis. Traffic is a primary source of air pollution within cities, which has been shown to have detrimental effects on children's development and health (e.g., Klauber et al., 2021; Stafford, 2015). Therefore, policies restricting air pollution are necessary to ensure cities provide a child-friendly living environment that allows them to grow up healthy and develop their full potential.

In this paper, we study the effects of LEZs on the educational achievements of elementary school children in Germany, measured by the transition rates to the academic track. Germany is known for its rigid early tracking system, which greatly determines a child's later educational and professional trajectory (e.g., Dustmann, 2004). Since only a small fraction of students changes tracks during secondary school, the chosen track at age 10 is a meaningful indicator of educational achievement in Germany.

To identify the causal effect of LEZs, we rely on a stacked-by-event design (Cengiz et al., 2019) and the estimator developed by de Chaisemartin & D'Haultfœuille (2020a). Both estimators account for the staggered implementation of LEZs and time-varying treatment effects that potentially downward bias the two-way-fixed effect estimator (de Chaisemartin & D'Haultfœuille, 2020b; Goodman-Bacon, 2021). We use geo-coded administrative school-level data from North Rhine-Westphalia, the most populous German federal state, and district-level data for all of Germany complemented with socio-economic information on the 1×1 km grid neighborhood level and geo-referenced data from the German Socio-Economic Panel (SOEP).

Our findings for the school-level data in NRW point to an increase in transition rates to the academic track by 0.9-1.6 percentage points in response to the introduction of LEZs. Running the analysis on district-level data for all of Germany, we find a slightly weaker but positive effect, suggesting that our findings have validity beyond NRW. In the SOEP analysis, we find suggestive evidence that a reduction in respiratory infections is one channel through which LEZs improve student outcomes. The effects on transition rates are driven by boys, who more often suffer from respiratory diseases during childhood.

Our results indicate a significant and lasting causal effect of even moderate improvements in air quality on educational achievement. The effects are estimated for an industrialized country with already relatively high air quality standards. Hence, the effects in countries with worse air quality could be even more pronounced. These findings also have social equity implications since pollution exposure is not evenly distributed across socioeconomic groups. Children from low-income and migrant families are more likely to live in areas of high pollution (e.g., Barnes et al., 2019; Jerrett et al., 2001). Hence, even though we do not find stronger transition-rate effects of LEZ in more deprived areas (Table C.1), the fact that LEZs in Germany can primarily be found in more deprived areas in the first place – areas that are more polluted (Table 4.1) and have lower baseline socio-economic characteristics (Table 4.2) – implies that the policy has helped to reduce educational inequalities.

From a policy perspective, our findings show that policies that effectively target air quality in cities have wide-ranging effects on urban residents. When evaluating the potential costs and benefits of such measures, it should be considered that they are likely to have positive effects on different fundamental areas of people's lives, such as their health, well-being, and productivity. If inadequate air quality prevents the considerable and growing number of children living in urban areas from reaching their full potential, this has severe implications for human capital.

4.A Appendix

4.B LEZ Descriptives

Table A.1: Overview sticker rules and requirements

	Sticker categories			
				
	No sticker	Red	Yellow	Green
Requirements **diesel vehicles**	Euro 1 or worse	Euro 2 or Euro 1 with partcile filter	Euro 3 or Euro 2 with particle filter	Euro 4 or Euro 3 with particle filter
Requirements **gasoline vehicles**	Without 3-way catalytic converter			Euro 1 with regulated catalytic converter or better

Source: UBA.

Table A.2: Low Emission Zones in Germany

State	**City**	**Level 1**	**Level 2**	**Level 3**
BW	Freiburg	01.01.2010	01.01.2012	01.01.2013
BW	Heidelberg	01.01.2010	01.01.2012	01.01.2013
BW	Heidenheim	01.01.2012	01.01.2012	01.01.2013
BW	Heilbronn	01.01.2009	01.01.2012	01.01.2013
BW	Herrenberg	01.01.2009	01.01.2012	01.01.2013
BW	Ilsfeld	01.03.2008	01.01.2012	01.01.2013
BW	Karlsruhe	01.01.2009	01.01.2012	01.01.2013
BW	Leonberg / Hemmingen and surroundings	02.12.2013	02.12.2013	02.12.2013
BW	Ludwigsburg and surroundings	01.01.2013	01.01.2013	01.01.2013
BW	Mühlacker	01.01.2009	01.01.2012	01.01.2013
BW	Mannheim	01.03.2008	01.01.2012	01.01.2013
BW	Pfinztal	01.01.2010	01.01.2012	01.01.2013
BW	Pforzheim	01.01.2009	01.01.2012	01.01.2013
BW	Reutlingen	01.03.2008	01.01.2012	01.01.2013
BW	Schramberg	01.07.2013	01.07.2013	01.01.2015
BW	Schwäbisch Gmünd	01.03.2008	01.01.2012	01.01.2013
BW	Stuttgart	01.03.2008	01.07.2010	01.01.2012
BW	Tübingen	01.03.2008	01.01.2012	01.01.2013
BW	Ulm	01.01.2009	01.01.2012	01.01.2013
BW	Urbach	01.01.2012	01.01.2012	01.01.2013
BW	Wendlingen	02.04.2013	02.04.2013	02.04.2013
BY	Augsburg	01.07.2009	01.01.2011	01.06.2016
BY	München	01.10.2008	01.10.2010	01.10.2012
BY	Neu-Ulm	01.11.2009	05.11.2012	NA
BY	Regensburg	15.01.2018	15.01.2018	15.01.2018
BE	Berlin	01.01.2008	01.01.2010	01.01.2010
HB	Hessen	01.01.2009	01.01.2010	01.07.2011
HE	Darmstadt	01.11.2015	01.11.2015	01.11.2015
HE	Frankfurt a.M.	01.10.2008	01.01.2010	01.01.2012
HE	Limburg an der Lahn	31.01.2018	31.01.2018	31.01.2018
HE	Marburg	01.04.2016	01.04.2016	01.04.2016
HE	Offenbach	01.01.2015	01.01.2015	01.01.2015
HE	Wiesbaden	01.02.2013	01.02.2013	01.02.2013
NI	Hannover	01.01.2008	01.01.2009	01.01.2010
NI	Osnabrück	04.01.2010	03.01.2011	03.01.2012
NW	Aachen	01.02.2016	01.02.2016	01.02.2016
NW	Bonn	01.01.2010	01.07.2012	01.07.2014
NW	Düsseldorf	15.02.2009	01.03.2011	01.07.2014
NW	Dinslaken	01.07.2011	01.07.2011	01.10.2012
NW	Eschweiler	01.06.2016	01.06.2016	01.06.2016
NW	Hagen	01.01.2012	01.01.2013	01.07.2014
NW	Köln	01.01.2008	01.01.2013	01.07.2014
NW	Krefeld	01.01.2011	01.01.2011	01.07.2012
NW	Langenfeld	01.01.2013	01.01.2013	01.07.2014
NW	Mönchengladbach	01.01.2013	01.01.2013	01.07.2014
NW	Münster	01.01.2010	01.01.2010	01.01.2015
NW	Neuss	15.02.2010	01.03.2011	01.07.2014
NW	Overath	01.10.2017	01.10.2017	01.10.2017
NW	Remscheid	01.01.2013	01.01.2013	01.07.2014
NW	Ruhrgebiet	01.01.2012	01.01.2013	01.07.2014
NW	Siegen	01.01.2015	01.01.2015	01.01.2015
NW	Wuppertal	15.02.2009	01.03.2011	01.07.2014
RP	Mainz	01.02.2013	01.02.2013	01.02.2013
SN	Leipzig	01.03.2011	01.03.2011	01.03.2011
ST	Halle (Saale)	01.09.2011	01.09.2011	01.01.2013
ST	Magdeburg	01.09.2011	01.09.2011	01.01.2013

Notes: The LEZ "Ruhrgebiet" consists of LEZs in Bochum, Bottrop, Dortmund, Duisburg, Essen, Gelsenkirchen, Mülheim, Oberhausen, and Recklinghausen and was introduced 01.01.2008. The LEZ was merged and enlargened further, including Castrop-Rauxel, Gladbeck, Herten, and Herne on 01.01.2012.
Source: UBA.

Table A.3: Comparison of the number of students of schools inside and outside of (future) LEZs, NRW

	Schools outside LEZ					Schools inside (future) LEZ				
	count	mean	sd	min	max	count	mean	sd	min	max
Total graduating students	8645	54.0	20.2	1	155	6786	55.0	18.5	2	148
Gymnasium	8645	22.9	13.7	0	98	6786	21.4	13.6	0	128
Realschule	8645	13.0	8.06	0	52	6786	12.4	7.33	0	67
Hauptschule	8645	4.51	5.15	0	45	6786	4.50	5.19	0	46
Gesamtschule	8645	8.21	9.02	0	70	6786	9.65	10.6	0	72
PRIMUS Schule	3303	11.5	9.88	0	73	2935	13.7	11.1	0	58
Gemeinschaftsschule	4530	0.91	2.90	0	46	3903	0.84	2.81	0	34
Sekundarschule	3903	0.36	1.83	0	32	3420	0.41	2.03	0	31
Sonstige	8645	0.33	0.75	0	11	6786	0.44	0.91	0	11

Source: UBA and IT.NRW.

4.C Balancing Tests

Table B.1: Impact of LEZ on school closures after NRW reforms

	School closures (in percent)		
Low emission zone	0.0334	0.0076	0.0330
	(0.0260)	(0.0190)	(0.0217)
Number of observations	17,941	17,941	17,941
Admin. district x Year FE	✓	✓	✓
District FE	-	✓	✓
Time-varying grid-level controls	-	-	✓

Notes: The table depicts the effect of LEZ on the share of school closures, measured on the zip code level. Since the outcome is not measured at the school level, instead of school and year fixed effects (TWFE) we include administrative district by year FE (column 1), district FE (column2), and time-varying grid-level controls (column 3). Comparison sample: large cities (> 100,000 inhabitants). Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Table B.2: Grid controls as outcomes to investigate possible compositional changes, stacked TWFE

Purchasing power		
Low emission zone	129.6040	163.2953
	(117.4125)	(108.8304)
Number of observations	177,515	177,515
Unemployment rate		
Low emission zone	-0.1224	-0.0052
	(0.0857)	(0.0858)
Number of observations	177,515	177,515
Share of foreigners		
Low emission zone	0.3872***	0.3083*
	(0.1454)	(0.1648)
Number of observations	177,515	177,515
Share of families		
Low emission zone	0.0140	-1.0896
	(0.4289)	(0.6624)
Number of observations	177,515	177,515
Grid FEs	✓	✓
Year FEs	✓	–
GRID controls (1x1km)	✓	✓
Admin. district × Year FEs	–	✓
Linear district trends	✓	–
Event time × Wave FEs	✓	✓
Group × Wave FEs	✓	✓

Notes: This table shows the results of separate regressions using the time-varying grid control variables as outcomes. The remaining grid variables are used as controls in each regression. Comparison sample: large cities (> 100,000 inhabitants). Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

4.D Heterogeneities

Table C.1: Treatment effect heterogeneity, stacked TWFE

	Transition rate to academic track	
Purchasing power	**Low**	**High**
Low emission zone	0.0093*	0.0069*
	(0.0042)	(0.0055)
Number of observations	47,806	47,806
Unemployment rate	**Low**	**High**
Low emission zone	0.090*	0.0095*
	(0.0050)	(0.0049)
Number of observations	47,806	47,806
Share of foreigners	**Low**	**High**
Low emission zone	0.0082	0.0090**
	(0.0055)	(0.0045)
Number of observations	47,806	47,806
Share of families	**Low**	**High**
Low emission zone	0.0082*	0.0102**
	(0.0050)	(0.0051)
Number of observations	47,806	47,806
School FEs	✓	✓
School year FEs	✓	✓
GRID controls (1x1km)	✓	✓
Admin. district × Year FEs	✓	✓
Linear district trends	✓	✓
Event time × Wave FEs	✓	✓
Group × Wave FEs	✓	✓

Notes: To determine the effect heterogeneities, we first identify treated schools by LEZ implementation wave and consequently determine their median split of the variable in the year prior to the implementation of the LEZ. The control units in each wave are categorized as “low” or “high” based on the median value of the variable of the treated schools. This procedure is performed for each wave before stacking. Comparison sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate, and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

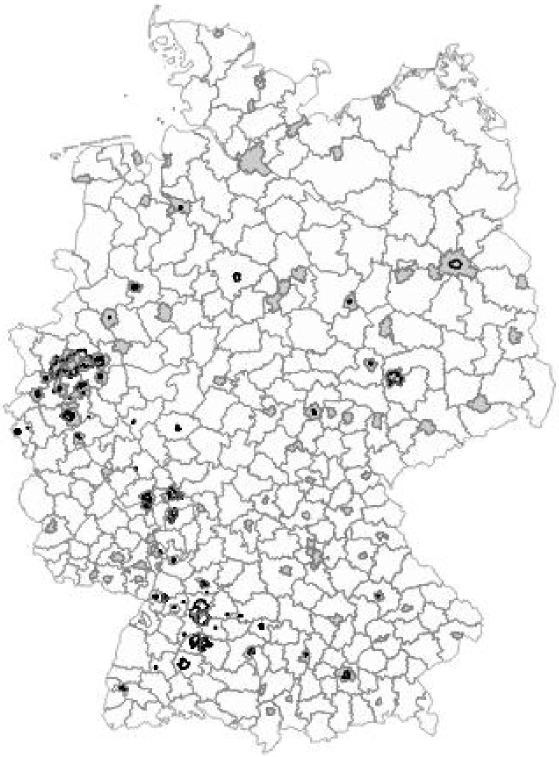
Table C.2: The role of after-school care

	Transition rate to academic track				
Low emission zone	0.0217**	0.0183**	0.0168*	0.0199*	0.0171*
	(0.0091)	(0.0090)	(0.0090)	(0.0106)	(0.0101)
After-school care	-0.0026	-0.0018	-0.0018	-0.0024	-0.0027
	(0.0024)	(0.0025)	(0.0025)	(0.0025)	(0.0025)
LEZ*after-school care	-0.0095	-0.0093	-0.0089	-0.0091	-0.0091
	(0.0095)	(0.0096)	(0.0096)	(0.0104)	(0.0102)
Number of observations	47806	47806	47806	47806	47806
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	✓	✓	✓	✓
GRID controls (1x1km)	-	✓	✓	✓	✓
Admin. district × Year FEs	-	-	✓	–	✓
District linear trends	-	-	-	✓	✓

Notes: Comparison sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate, and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

4.E District-level analysis

Figure D.1: Districts considered for the empirical analysis



Notes: Comparison sample consists of district-free cities only. Those with a LEZ are considered treated, and those without a LEZ comparison group.
Source: UBA.

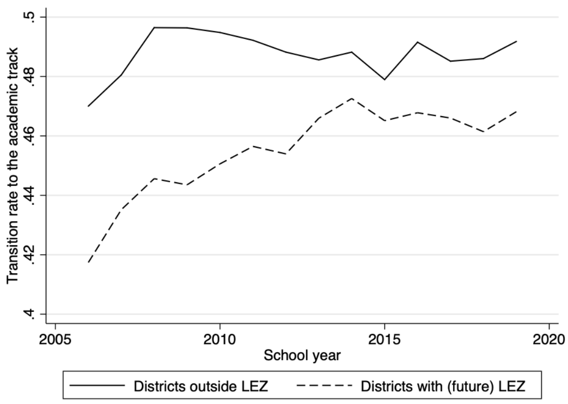
Table D.1: Comparison of district-free cities with and without LEZs

	District-free cities without LEZ					**District-free cities with LEZ**				
	count	mean	sd	min	max	count	mean	sd	min	max
Age (mean)	900	43.8	1.76	40.3	50.5	547	42.6	1.39	39.7	45.7
Unemployment rate	900	8.26	3.10	2.94	19.4	547	8.94	3.06	3.19	20.1
Gross income	900	2590.9	495.9	1697.0	5196.7	547	2779.8	451.4	1857.7	4274.9
BIP per inhabitant in €	900	45.8	20.6	18.4	188.3	547	46.1	18.1	16.6	96.0
Migration balance	900	5.57	7.20	-40.6	59.3	547	5.87	6.41	-20.8	39.9
Foreigners (%)	900	10.5	4.55	1.39	27.0	547	15.6	5.77	3.06	36.6

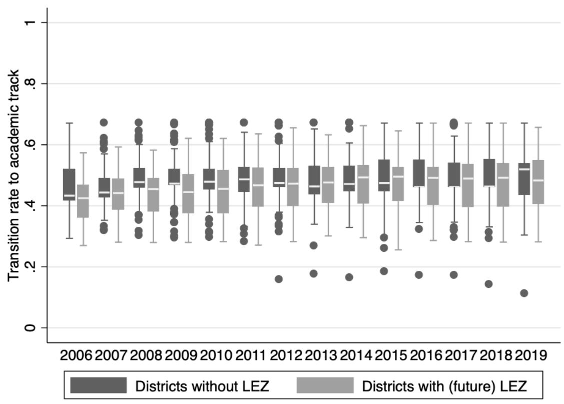
Notes: Table shows the two groups' averages between 2005 – 2018.
Source: UBA and bildungsmonitoring.de.

Figure D.2: District-level transition rates to the academic track for districts with and without a LEZ

(a) Panel A



(b) Panel B



Notes: Panel A displays the average transition rates to *Gymnasium* for districts that contain no LEZs and for districts, which at any point in time between 2005 – 2018, contain a LEZ. In Panel B, boxplots of the district-level transition rates are displayed. Students' weights applied.
Source: UBA and bildungsmonitoring.de.

Table D.2: District transition rates to the academic track (TWFE)

	Transition rate to academic track			
Low emission zone	-0.0091	0.0090*	0.0050	0.0042
	(0.0155)	(0.0050)	(0.0039)	(0.0032)
Number of observations	1438	1438	1400	1400
District FEs	-	✓	✓	✓
School year FEs	-	✓	-	-
State × Year FEs	-	-	✓	✓
District control variables	-	-	-	✓

Notes: Comparison sample: district-free cities. Time-varying district controls include district share of foreigners, district-level gross earnings, district net migration, as well as the district unemployment rate. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, bildungsmonitoring.de, and INKAR (BBSR).

Table D.3: District transition rates to the academic track, stacked TWFE

	Transition rate to academic track		
Low emission zone	0.0157***	0.0090	0.0052
	(0.0053)	(0.0077)	(0.0071)
Number of observations	4303	4303	4301
District FEs	✓	✓	✓
Year FEs	✓	–	–
Time-varying controls	✓	✓	✓
State × Year FEs	–	✓	✓
Linear district trends	–	✓	✓
Event time × Wave FEs	✓	✓	✓
Group × Wave FEs	✓	✓	✓

Notes: Comparison sample: district-free cities. Time-varying district controls include district share of foreigners, district-level gross earnings, district net migration, as well as the district unemployment rate. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, bildungsmonitoring.de, and INKAR (BBSR).

4.F Further robustness checks

Table E.1: Impact of LEZs transition rates to the academic track, TWFE

	Transition rate to academic track				
Low emission zone	0.0111**	0.0102**	0.0089*	0.0105*	0.0083
	(0.0046)	(0.0047)	(0.0052)	(0.0057)	(0.0056)
Number of observations	17,859	17,859	17,859	17,859	17,859
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	✓	✓	✓	✓
GRID controls (1x1km)	-	✓	✓	✓	✓
Admin. district × Year FEs	-	-	✓	–	✓
District linear trends	-	-	-	✓	✓

Notes: Comparsion sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Table E.2: Impact of LEZs transition rates to the academic track, stacked TWFE

	Transition rate to academic track				
Low emission zone	0.0134***	0.0102**	0.0090**	0.0119***	0.0091*
	(0.0041)	(0.0043)	(0.0044)	(0.0045)	(0.0046)
Number of observations	47,806	47,806	47,806	47,806	47,806
Event time x Wave FEs	✓	✓	✓	✓	✓
Group x Wave FEs	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (zip code)	–	–	✓	✓	✓
Admin. district x Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓

Notes: Comparison sample: large cities (> 100,000 inhabitants). Grid control variables at the zip code level variables purchasing power per capita, the share of foreigners, the unemployment rate, and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Table E.3: TWFE, only first implementation wave (2008/09)

	Transition rate to academic track				
Low emission zone	0.0148**	0.0136**	0.0121	0.0124	0.0101
	(0.0061)	(0.0064)	(0.0073)	(0.0080)	(0.0083)
Number of observations	15,968	15,968	15,968	15,968	15,968
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	✓	✓	✓	✓
GRID controls (1x1km)	-	✓	✓	✓	✓
Admin. district × Year FEs	-	-	✓	–	✓
District linear trends	-	-	-	✓	✓

Notes: Comparison sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Table E.4: Balanced comparison sample, stacked TWFE

	Transition rate to academic track				
Low emission zone	0.0121***	0.0107**	0.0094**	0.0112**	0.0092**
	(0.0042)	(0.0043)	(0.0043)	(0.0044)	(0.0044)
Number of observations	44575	44575	44575	44575	44575
Event time x Wave FEs	✓	✓	✓	✓	✓
Group x Wave FEs	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (1x1km)	–	–	✓	✓	✓
Admin. district x Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓

Notes: Comparison sample: large cities (> 100,000 inhabitants). Only schools which existed for the entire period under investigation were included. Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Table E.5: Excluding schools in buffers around LEZ, stacked TWFE

Panel A: Excluding schools within a 1000m buffer from LEZ (500m both sides)

	Transition rate to academic track				
Low emission zone	0.0149***	0.0117**	0.0104**	0.0133***	0.0104**
	(0.0051)	(0.0053)	(0.0052)	(0.0049)	(0.0051)
Number of observations	42,757	42,757	42,757	42,757	42,757

Panel B: Excluding schools within a 1000m buffer to the outside border of LEZ

	Transition rate to academic track				
Low emission zone	0.0148***	0.0121***	0.0108**	0.0143***	0.0117**
	(0.0041)	(0.0045)	(0.0045)	(0.0044)	(0.0047)
Number of observations	39,061	39,061	39,061	39,061	39,061
Event time x Wave FEs	✓	✓	✓	✓	✓
Group x Wave FEs	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (1x1km)	–	–	✓	✓	✓
Admin. district x Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓

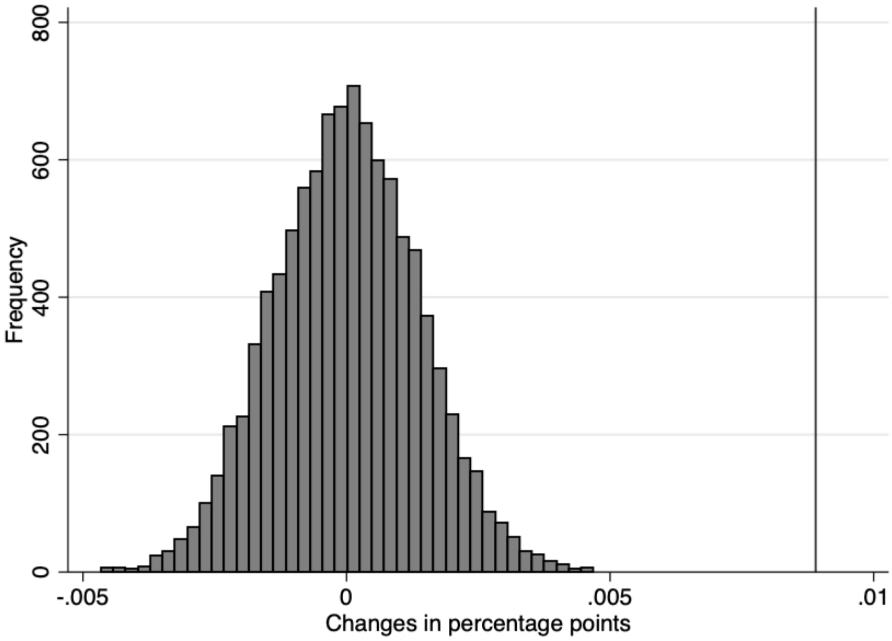
Notes: Stacked TWFE. Comparison sample: large cities (> 100,000 inhabitants). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Table E.6: Excluding comprehensive schools, stacked TWFE

	Transition rate to academic track				
Low emission zone	0.0169***	0.0141**	0.0121*	0.0206***	0.0199***
	(0.0064)	(0.0069)	(0.0069)	(0.0074)	(0.0073)
Number of observations	47,785	47,785	47,785	47,785	47,785
Event time x Wave FEs	✓	✓	✓	✓	✓
Group x Wave FEs	✓	✓	✓	✓	✓
School FEs	✓	✓	✓	✓	✓
School year FEs	✓	–	–	✓	–
GRID controls (1x1km)	–	–	✓	✓	✓
Admin. district x Year FEs	–	✓	✓	–	✓
Linear district trends	–	–	–	✓	✓

Notes: Comparison sample: large cities (> 100,000 inhabitants). Here, we compare the transition rate to the *Gymnasium* to transition rates to the *Realschule* and *Hauptschule*, thus excluding comprehensive schools (*Gesamtschulen*) which also offer the option of graduating in the academic track (see Section 4.3.2). Grid control variables include purchasing power per capita, the share of foreigners, the unemployment rate and the share of households with children. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

Figure E.1: Permutation Test



Notes: The figure displays the distribution of 10,000 placebo estimates of the TWFE regression in Column 3 in Table E.1. Sample: large cities (> 100,000 inhabitants). Grid control variables Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.
Source: UBA, IT.NRW, and RWI-GEO-GRID.

4.G Channels

Table F.1: The impact of LEZ on asthma prevalence at age 9-10

	Respiratory disease	
Low Emission Zone	-0.126**	-0.100*
	(0.055)	(0.051)
Male		0.045
		(0.038)
LEZ × male		-0.049
		(0.058)
Number of observations	1293	1293

Notes: Pooled OLS regression controlling for district and survey year fixed effects, and individual control variables (parental education and employment status, number of children in the household, migration background, log income, social transfer receiving household). Standard errors are clustered on the district level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
Source: SOEP v35 and UBA.

CHAPTER 5

Costs and Short-Term Effects of a Home-Visiting Program in *BRISE* – First Steps for a Cost-Effectiveness Analysis[1]

5.1 Introduction

It is broadly established that socio-economic inequalities emerge during early childhood or even during prenatal phases (e.g., Currie & Almond, 2011; Fernald et al., 2013; Silvestrin et al., 2020). We also know that investments during the early stages of a child's life can effectively mitigate these inequalities (e.g., Heckman et al., 2010). This knowledge is also reflected in a sharp rise in public spending on early childhood programs in most OECD countries (OECD, 2021). For example, in Germany, public spending almost doubled from 19.5 billion in 2010 to 36.9 billion Euros in 2020 (Statistisches Bundesamt, 2020b)[2] – a trend that will likely continue in the future. Regarding the effectiveness of specific policies, there is a large body of research focusing on early childcare programs aiming to nurture child development directly (e.g., Barnett, 1985; Havnes & Mogstad, 2011; Heckman et al., 2013b; Karoly et al., 2006; Kautz et al., 2014). However, substantially less research exists on the effects of interventions that target parenting skills and knowledge as a way to improve child development and parental well-being (e.g., Camehl et al., 2020; Klebanov et al., 2001; Olds, 2006).[3]

[1]This chapter is joint work with Mara Barschkett (DIW Berlin) and Sophia Schmitz (BiB Wiesbaden). We are grateful to the research associates at the University of Bremen for data access and for their excellent support. We further thank C. Katharina Spiess, Andrew Judy, Louisanne Knierim, Jonas Jessen, as well as the participants at internal seminars at DIW Berlin, BRISE consortium meetings, the 2021 meeting of the scientific advisory board of BRISE and the 2022 GEBF conference. Moreover, we gratefully acknowledge funding from the BRISE project (project number: FKZ: 01NV1601A-G).

[2]These figures correspond to the total expenditure on educational institutions under public and private sponsorship for children under six years.

[3]See Heckman & Mosso (2014) for an overview of the existing studies.

The most prominent type of these programs are home-visiting schemes which advise parents - usually mothers - on aspects of everyday life with an infant, such as mother-and-child interaction, nutrition, and ways to seek support when needed (e.g., Camehl et al., 2020). Another aspect that is often inadequately addressed by previous studies is the efficiency of such programs, i.e., how the costs relate to the benefits. In the face of scarce public resources, efficiency studies are an important tool for policymakers and can help making investments in early childhood programs more compelling (e.g., Karoly, 2012; Spieß, 2013). Studies considering the costs and effectiveness of interventions are particularly scarce in the German and European context, leading to a lack of evidence-based accountability for early childhood investments.

This paper presents novel evidence on the short-term effectiveness and costs of the home-visiting program *Pro Kind* which targets parenting styles and parental behavior during pregnancy and early childhood. It entails bi-weekly home visits starting prenatally and lasting until children turn two. The main interest of this study is to understand whether *Pro Kind* already had a significant impact on mother and child outcomes during the first seven months of the children's lives. *Pro Kind* is the first program within a systematic chain of home- and center-based preschool interventions established to support disadvantaged families from pregnancy until school entry under the Bremen Initiative to Foster Early Childhood Development ("Bremen Initiative zur Stärkung frühkindlicher Entwicklung" – *BRISE*).[4]

To establish causality, we exploit the fact that treatment status of BRISE was randomly assigned on the neighborhood level. The treatment for the families recruited in treatment areas (N=124) comprises better access and specific information on the home-based programs, while participation barriers for recruited families in the control neighborhoods (N=176) were much higher. In our empirical analysis we apply a combination of methods: We exploit the random treatment assignment in an instrumental variables (IV) approach to overcome endogeneity in the participation decision. To overcome any remaining endogeneity concerns, we combine the IV approach with entropy balancing methods to account for the observed differences in socio-economic characteristics between participating families from treatment and control neighborhoods. To analyze costs, we use comprehensive, self-collected data from a yearly cost survey following the ingredient method (Levin & McEwan, 2000) to set up a micro cost data set

[4]Launched in 2017 in a large Western German city (Bremen), the idea of *BRISE* is to systematically combine existing home- and center-based preschool programs into an intervention chain to support disadvantaged families from pregnancy until school entry. Thus, *BRISE* examines whether integrating separate schemes leads to higher cumulative effects on child development while also being less expensive. Two other programs of BRISE starting before age three, *Opstapje* and *Tipp Tapp* are not evaluated, since the data to analyze *Opstapje* is not available yet and *Tipp Tapp* participation was not randomized (BRISE Consortium, 2022; Schütte et al., 2020).

covering all costs related to the program implementation. We then proceed to compare the overall cost per child per year to other well-established early childhood programs that entail parenting skills elements.

BRISE collects data on an extensive range of outcomes covering different aspects related to maternal and child well-being, providing a comprehensive picture of the early impact of the program. For *mothers*, we focus on a number of behavioral outcomes – smoking, alcohol consumption, breastfeeding, and soothing strategies – as well as maternal well-being, including postnatal depression, measured by the Edinburgh Postnatal Depression Scale (EPDS) (Cox et al., 1996), and perception of change in living conditions. Both nicotine and alcohol consumption during pregnancy and breastfeeding is associated with several adverse infant health outcomes (e.g., Polanska et al., 2015). Similarly, maternal well-being is a strong predictor of child development (e.g., Berger & Spiess, 2011; Dahlen, 2016), positive health outcomes (e.g., Diener & Chan, 2011) and labor productivity (e.g., Oswald et al., 2015). Regarding *child* outcomes, we look at the Milestones of Normal Development in Early Years (MONDEY) score, a comprehensive child development indicator developed by psychologists (Pauen, 2011; Pauen et al., 2012).

Our results concerning the very short-term impact of *Pro Kind* on child and maternal outcomes display statistically insignificant effects. Due to the small sample sizes and many missing observations, the effects are imprecisely estimated, which does not allow us to draw conclusions. Put aside the data issues, it is not surprising to find no significant impact on child and mother outcomes in the child's first months of life. It may take several years for the effects of early childhood programs to manifest. For example, studies that evaluated *Pro Kind* in other contexts find minor improvements in some maternal and child outcomes, which materialize earliest at 12 months (e.g., Jungmann et al., 2015; Sandner, 2013, 2019; Sandner et al., 2018). Hence, future studies evaluating the effectiveness of *Pro Kind* at later stages are in a better position to conduct a sound analysis. Our cost analysis reveals that the average costs per participant per year in *Pro Kind* range between 3,468 and 3,861 Euros over the study period (2017–2020). About 80% of the total costs can be attributed to personnel costs. In comparison to other programs, *Pro Kind* belongs to the less costly programs – only one program displays lower costs per child per year (Sure Start in the UK, Cattan et al., 2021), while the prominent Perry Preschool program (Heckman et al., 2010) is about 5.5 times as expensive as *Pro Kind* .

Our study makes several contributions. First, our study adds to the literature on the effects of early childhood education and care programs (e.g., Barnett, 2011; Cunha et al., 2010; Heckman & Mosso, 2014), especially those targeting parenting skills (e.g., Camehl et al., 2020; Lindsay et al., 2011). More specifically, we add to the growing evidence on the effectiveness of home-visiting programs (e.g., Doyle et al., 2015; Olds, 2006; Sandner, 2019) – as opposed to center-based programs (e.g. Triple P, Camehl et al., 2020) – on improving child and maternal outcomes. So far, studies of this kind have primarily focused on Anglo-American programs (e.g., Heckman et al., 2017; Love et al., 2005; Olds, 2006), with little evidence existing in the European and German context. Exceptions include evaluations of the Swiss counterpart of Parents as Teachers (PAT) (Schaub et al., 2019), the German family-supporting prevention program "Keiner fällt durchs Netz" (KfdN; "Nobody Slips Through the Net") (Sidor et al., 2013) and previous research on *Pro Kind* in three German federal states (e.g., Jungmann et al., 2015; Sandner, 2013, 2019; Sandner et al., 2018). While the program has been evaluated with respect to its effectiveness on several maternal and child outcomes, this is the first study to estimate effects on the MONDEY score (e.g., Pauen, 2011; Pauen et al., 2012) and the Edinburgh Postnatal Depression Scale (EPDS) (Cox et al., 1996). The MONDEY score is an observational tool to document early childhood development until age three, comprising eight domains, e.g., gross and fine motor skills and visual perception. As such, it is an extremely comprehensive indicator of early childhood development. The EPDS is a widely established and reliable measure of postnatal depression. In total, we consider an extensive range of outcomes covering different aspects related to maternal and child well-being, providing a comprehensive picture of the early impact of the program. Lastly, we make use of a unique cost panel data set based on a cost survey developed by family and education economists (Barschkett & Schmitz, 2020). The cost database breaks down costs into different components and records a 100% response rate of the program providers on a yearly basis. As such, this study builds a valuable basis for future even more detailed cost-efficiency and cost-benefit studies within BRISE.

The remainder of this paper is structured as follows. Section 5.2 summarizes the related literature. Next, we describe the institutional background, including the *BRISE* programs. Section 5.4 presents our data set, control and outcome variables. After that, we outline our empirical strategy and provide descriptive statistics. Section 5.6 presents our empirical findings on the effectiveness of the programs and costs. Lastly, Section 5.7 concludes.

5.2 Literature

There is a large and growing literature focusing on the effectiveness of early childcare programs (e.g., Baker et al., 2008; Carneiro & Ginja, 2014; Cornelissen et al., 2018; Currie & Almond, 2011; Heckman et al., 2010). Especially targeted programs such as the Perry Preschool Program and the Abecedarian Project, including both center- and home-based elements, were found to have large medium and long-term effects on participants' (non-)cognitive and labor market outcomes (e.g., Barnett & Masse, 2007; Campbell & Ramey, 1991; Heckman et al., 2010). However, also universal programs, such as Sure Start, a center-based early childhood intervention in the UK offering a range of services including childcare and parenting support, were shown to positively affect children's health (Cattan et al., 2021) and other related outcomes such as family functioning and behavioral problems (e.g., Sammons et al., 2015). Similarly, Triple P (Positive Parenting Program), a multi-level parenting and family support strategy mostly relying on center-based interventions, had positive effects on child and maternal outcomes (e.g., Camehl et al., 2020; Kim et al., 2018). Slightly less empirical research exists on the effectiveness of home-based programs that seek to improve child development through enhanced parenting skills. These programs are referred to as home-visiting, prevention, or home-based parenting programs. They usually target low-SES families during pregnancy or shortly after childbirth and consist of regular home visits by nurses, social workers, or paraprofessionals. Table 5.1 offers a summary of selected home-visiting programs in terms of their effects on child and maternal outcomes. The studies are chosen based on program similarities to *Pro Kind* (home-based, age group) and the way effects are measured (age at measurement, outcomes, (quasi-)experimental design).[5]

A large majority of these home-visiting programs that have been evaluated in terms of their effectiveness are located in the United States. Famous examples include the Nurse Family Partnership (NFP) (Olds et al., 2002) and Early Head Start (Love et al., 2005). Many of these programs have proven to be effective concerning child development, maternal outcomes, and parenting behavior (Table 5.1), although most effects could only be measured in the medium to long term. For example, Early Head Start parents showed higher emotional engagement and support (e.g., Love et al., 2005) and the NFP led to a reduction in smoking, increased maternal employment, and more mother-child interactions (Olds et al., 2002) and increased home investments, parenting attitudes and mental health for mothers of infants at age two (Heckman, 2007). Similar results

[5]We do not claim to offer a complete overview. Other (less recent) literature overviews can be found in, for example, Sweet & Appelbaum (2004), Peacock et al. (2013) and MacMillan et al. (2009).

were found for the programs Minding the Baby (Sadler et al., 2013) and Parents as Teachers (PAT) (Wagner & Clayton, 1999). One notable exception where significant effects on parenting behavior were measured at a very early stage is the Home visiting program in Queensland: At six weeks, mothers receiving the home visits had significant reductions in postnatal depression, demonstrated more positive interactions with their infants, and achieved higher scores in maternal-infant secure attachment, among others (Armstrong et al., 1999).

Table 5.1: Overview of the effects of selected home-based early childhood interventions

	Child development			Maternal outcomes					
Program	Cognitive	Socio-emotional	Health	Mental health	Health behavior	Employment	Fertility	Parenting	Age at measurement
Anglo-American programs									
Nurse Family Partnership	+○	+○		+	+	+	-		0-2, 6, 12 y.
Preparing for Life (PFL)			+						6, 12, 18, 24, 36 m.
Early Head Start	+	+	○	+					3 y.
Minding the Baby			○	○			-	+	4, 12, 24 m.
CCDP	○	○	○	○	○	○		○	3-5 y.
Home visiting (Queensland)			○	+				+	6 w.
Home visiting (UK)			○	○				+	2, 6, 12 m.
FDRP	+	+				○			3 y.
PAT (USA)			○					+	2-3 y.
European programs									
PAT (Switzerland)	+○	+	○						3 y.
KfdN		+						+	1 y.
Pro Kind									
- All	+○	○	○	+	+	-	+		0-2 y.
- Girls	+								0-2 y.
- Boys	○								0-2 y.

Note: The table reports effects of home-visiting programs found in the literature, where + indicates positive effects (improvement), ○ indicates null effects, - indicates negative effects (deterioration). The following papers found the basis of this depictions: NFP: Olds et al. (2002), Heckman et al. (2017), Olds (2006); PFL: Doyle et al. (2015); Early Head Start: Love et al. (2005); Minding the Baby: Sadler et al. (2013); CCDP: St. Pierre & Layzer (1999); Home visiting (Queensland): Armstrong et al. (1999); Home visiting (UK): DuMont et al. (2010), McIntosh et al. (2009); FDRP: Besharov et al. (2011); PAT (USA): Wagner & Clayton (1999); PAT (Switzerland): Schaub et al. (2019); KfdN: Sidor et al. (2013); Pro Kind: Sandner et al. (2018), Sandner (2019), Sandner (2013), Sandner & Jungmann (2017), Jungmann et al. (2015).
Source: Own depiction.

While positive effects were also recorded on child outcomes, they usually take even longer to materialize. For example, at three years old, Early Head Start participants demonstrated improved cognitive and language development (Love et al., 2005). Regarding the NFP in Memphis, participants showed enhanced cognitive skills for both genders and improved socio-emotional skills for females at age six (Heckman, 2007). At the age of three, children in primarily Spanish-speaking Latino communities showed significant gains in cognitive, communication, social, and self-help development as a result of participating in PAT (Wagner & Clayton, 1999). The Irish program Preparing for Life (PFL) (Doyle et al., 2015) recorded positive effects on child health after 18 and 24 months. At six months, a small effect was found for the level of immunizations. Another large-scale home-visiting program, the Comprehensive Child Development

Program (CCDP), did not record any statistically significant effects on either child or mother outcomes (St. Pierre & Layzer, 1999).

Less evidence is available on the effectiveness of home-visiting programs outside the Anglo-American region. The Swiss counterpart to the PAT showed improved children's adaptive behavior, developmental status, and language skills at the age of three (Schaub et al., 2019). The German family-supporting prevention program "Keiner fällt durchs Netz" (KfdN; "Nobody Slips Through the Net") targets psychosocially stressed families in a controlled trial setting. Sidor et al. (2013) shows that children in the intervention group showed improved social development scores and were judged by their mothers to be less "difficult." In addition, the dysfunctionality of the mother-child interaction was reduced in this group compared to the control group. No intervention effects were found for the degree of maternal stress or maternal sensitivity towards the child.

Multiple studies have evaluated the effectiveness of *Pro Kind* in improving different parental and child outcomes at different points in time. For example, Jungmann et al. (2015) find minor positive effects on parental self-efficacy, feelings of attachment, social support, and maternal oral health. The effects were measured at different times (between pregnancy and age two of the children). However, up until age two, the authors do not find economically meaningful effects on maternal or child health. Sandner (2013) find small increases in infants' cognitive developments at 12 months, which fade out at 24 months. For other outcomes, they do not find effects at this early stage (Sandner, 2013). Sandner & Jungmann (2017) add that the effects on cognitive development were driven by girls, explained by greater parental investment for girls than for boys. Concerning health, Sandner et al. (2018) find positive effects on maternal mental health but no effect on other health outcomes such as healthcare utilization, health behaviors, and physical health of mother and child (measured at multiple points in time between pregnancy and age two of the child). At 36 months postnatally, Sandner (2019) points out a decrease in maternal employment, an increase in subsequent birth, and positive effects on maternal well-being and life satisfaction. These findings suggest that the *Pro Kind* program entails mid or long-term benefits rather than short-term effects.

Home-visiting programs are labor-intensive and hence produce high costs. Therefore, cost-effectiveness or cost-benefit studies are essential for policymakers to justify the introduction or continuation of such programs. However, evidence on both costs and effectiveness of home-visiting programs is scarce. So far, cost-effectiveness studies focusing on home-visiting schemes during early childhood mainly exist in the Anglo-

American context (Schmitz & Kröger, 2017).[6] The most widely researched program in this respect is the Nurse Family Partnership (NFP) program in the US that has been implemented at multiple sites (e.g., Glazner et al., 2004; Karoly, 2017; Karoly et al., 1998; Miller & Hendrie, 2015). The analyses are net-cost analyses from the standpoint of government spending, i.e., the benefits to the government are monetized in the way that the government needs to provide less support to nurse-visited families (e.g., reduction in social and health care costs). In the long run, NFP's benefits exceed the costs; however, this threshold is only reached in children's teenage years (Glazner et al., 2004; Karoly, 2017; Karoly et al., 1998). Additionally, a home-visiting program in the UK was shown to improve maternal sensitivity and infant cooperativeness at an incremental cost of 3264 pounds per woman (Barlow et al., 2007; McIntosh et al., 2009).

We contribute to this small but growing strand of the literature by focusing on the German context. We analyze both the effectiveness as well as incurring costs for *Pro Kind*. In Europe, such programs' (cost-)effectiveness is likely different from the US-American case. In most European countries, an established social welfare system exists in addition to such programs. Furthermore, in the US, childcare as well as parenting support programs often target the most in-need groups, whereas European countries usually adopt a universal childcare approach. We examine the effectiveness and costs of a targeted parenting scheme in an institutional context with a generous social welfare system and universal childcare.

5.3 Institutional Background

5.3.1 Design of BRISE

Launched in 2017, the project *BRISE* is a cooperation of the city of Bremen with several research institutions.[7] The idea of the *BRISE* program is to systematically combine existing home-based and preschool programs into a chain of intervention measures to support families from pregnancy until school entry (see Figure 5.A.1). Findings in the international literature suggest that time-limited individual measures often have only

[6] One of the reasons for this is that North American countries have traditionally spent less on their welfare systems than European countries, creating the need for evidence on the efficiency of alternative policies (Korpi & Palme, 1998).

[7] The scientific consortium is composed of the University of Bremen, Leibniz Institute for Science and Mathematics Education (IPN), the German Institute for Economic Research (DIW), The Federal Institute for Population Research (BiB), the Leibniz Institute for Research and Information in Education (DIPF), the Max-Planck Institute for Education Research, the Leibniz Institute for Educational Trajectories (LIfBi), University of Heidelberg, and the University of Bamberg.

small effects (e.g., Puma et al., 2012) and that periods of lack of support during the first years of a child's life should be avoided (e.g., Schütte et al., 2020). As a result, the systematic integration of regionally already established programs into an intervention chain is a potentially promising and cost-effective means to achieve sustainable positive effects on child development. BRISE tests this hypothesis in a large-scale quasi-experimental field study conducted in close collaboration with the political and administrative level in a city whose population structure is characterized by a substantial proportion of socially and culturally disadvantaged families. Measured against the national median, Bremen has the highest at-risk-of-poverty rate of all German states (IAW, 2018). On average, Bremen records one of the highest shares of students who fall below minimum requirements both in primary (Stanat et al., 2017) and middle school (Stanat et al., 2019). Within the scope of *BRISE,* the city has expanded the provision of programs belonging to the support chain. Specifically, 70 additional places have been created for *Pro Kind*, corresponding to an expansion of 78 percent (Schütte et al., 2020).

BRISE was rolled out in 27 districts in Bremen, which were selected based on data from the State Statistical Office to identify districts with a comparatively high proportion of disadvantaged families. Specifically, the following indicators were taken into account: the proportion of the population under the age of fifteen receiving basic unemployment benefits (*Arbeitslosengeld II*), the proportion of the population with a qualifying school-leaving certificate, and the proportion of the population with an immigrant background. Furthermore, the political relevance was considered; on the one hand, the rate of relocations from the district was included here, and on the other hand, the birth rate. In addition, the implementation conditions were taken into account, such as the existing infrastructure of programs for early childhood development and whether there is political interest and willingness to reorganize the local supply structure. A large number of local actors familiar with the situation on site were actively involved in this process. A network of multipliers has also been established to recruit participating families (Schütte et al., 2020). The 27 BRISE districts were then randomly divided into treatment (10) and control (17) districts such that the two groups were similar in terms of the criteria mentioned above. As shown in Table 5.A.1, at the time this selection was made, there was no statistically significant difference between the two groups. One exception is the number of births, which was slightly higher in the control districts. *BRISE* targets low-SES expectant parents, which are ideally recruited during the last trimester of pregnancy but at the latest when the children are ten weeks old. Families were recruited based on the following eligibility criteria:

one or both parent(s) should have a migration background[8], or low education[9], or low income. Another precondition was that the children should not be severely ill upon program entry, i.e., during the end of pregnancy or shortly after postpartum. Since the program had difficulties recruiting enough families, however, these criteria became less strict over time. In total, *BRISE* has recruited 404 families with 456 participating children, following a randomized controlled trial (RCT) design with randomization at the neighborhood level.

When comparing the *BRISE* sample with a representative sample of the German population (SOEP)[10] (Table 5.A.2), we see that *BRISE* families are on average slightly more disadvantaged than SOEP families. Specifically, there is a mixed picture regarding the educational status of the mothers: While *BRISE* mothers are more likely to have no school degree, there is also a higher share of mothers with an academic school degree (Abitur) in the *BRISE* groups than in the average SOEP respondent mother. One potential explanation for the relatively high share of highly educated mothers could be that we have a high share of mothers with a foreign school degree which complicates the comparison of education levels. In most remaining SES characteristics, the *BRISE* sample appears indeed more disadvantaged than the average SOEP respondent. For example, *BRISE* families have a lower net income and more often have an immigrant background.

The treatment can be regarded as a combination of access and information treatment: *BRISE* provides financial support to the treatment districts to scale up their offer of the programs forming the intervention chain. In addition, family counselors (researchers of the University of Bremen) inform the families in the treatment neighborhoods about the relevant *BRISE* programs, arrange contacts to the practice sites, and support the families of the treatment group in taking advantage of the continuous support. Figure 5.A.2 gives an example of a flyer handed out to the families. BRISE children in the treatment districts are guaranteed a place in the programs. In sum, the process is designed to keep the practical costs and administrative barriers for families to register as low as possible. The families in the control group are free to use these programs but receive no such information or organizational support. Both groups have access to other regular German healthcare services or other programs.

[8]This criteria was defined as fulfilled if at least one parent was born outside of Germany.

[9]Low education was defined as having less than a high-school degree, i.e., no school degree or a lower secondary school degree.

[10]We select a sample that includes families in urban areas, with children below one, and excludes the SOEP migration (M1-M5), low-income (L2) samples and all observations surveyed before 2010.

Three home visiting schemes are offered during the first year of *BRISE*, forming the first part of the intervention chain. The following section describes these programs, focusing on *Pro Kind*, which is the subject of this study.

5.3.2 Pro Kind (PK)

Pro Kind, run by the German Red Cross, is modeled after the US Nurse-Family-Partnership (NFP) project, which has been shown in evaluations to be effective in improving child and maternal outcomes (for an overview, see Eckenrode et al., 2017). Like NFP, *Pro Kind* begins in the last trimester of pregnancy and ends when the child turns two. During pregnancy, first time mothers are counseled about nutrition, the importance of avoiding alcohol and nicotine, and typical warning signs of complications. After childbirth, *Pro Kind* staff, i.e., midwives, pediatric nurses, or social pedagogues, provide counseling about appropriate childcare and soothing techniques, health, and nutrition, as well as how to interact with the child in a way that promotes child development. The frequency of the home visits varies between weekly, biweekly, and monthly, summing up to around 52 home visits, each lasting on average 90 minutes. The *Pro Kind* teaching materials are closely linked to NFP guidelines and structure the theme of each home visit, although *Pro Kind* staff may decide to adapt the contents to the specific needs of the families (Sandner et al., 2018). In addition, *Pro Kind* covers public transportation costs to prenatal checkups and hands out monetary thank-you gifts of 25 Euros for participation in the interviews.

5.3.3 Other home visiting programs in *BRISE*

Opstapje is also administered by the German Red Cross and is a home-based intervention for socioeconomically disadvantaged families, focusing on improving parent-child interactions by strengthening parenting skills and resources in the home (Sann & Thrum, 2005). *Opstapje Baby* usually begins when the child is two months old and ends when they turn three years. In Bremen, however, *Opstapje* is designed to start when the child turns six months (BRISE Consortium, 2013). Since we currently only have data up to the age of seven months, we cannot evaluate *Opstapje* in terms of its effectiveness yet.

Tipp Tapp takes up the postnatal concept of NFP and offers early prevention in at-risk families. Here, parents receive counseling from a nurse at three points in time (after birth, after six months, and after one year) as part of an announced home visit. The counseling covers nutritional issues, care, design of the child's living environment, accident prevention, prophylaxis, vaccinations, and participation in early detection programs. Unfortunately, a causal evaluation of *Tipp Tapp* is impossible since the program proactively contacts expectant low-SES families in all of Bremen, not distinguishing between treatment and control neighborhoods. As a result, the setting does not meet the standards for a quasi-experimental evaluation design. Consequently, participation in *Tipp Tapp* only enters our analysis as a control variable (e.g., BRISE Consortium, 2013).

5.4 Data

The sample used in this study comprises the first 300 children (born 2017–2019) and their families who participated in *BRISE*.[11] Families were surveyed between 2017 and 2020. The *BRISE* project conducts regular surveys in the participating households that build the basis for this evaluation. After the families apply for the program and are considered a potential fit based on the official criteria, a screening interview takes place surveying essential SES background variables. When the final decision is made, and the family is officially part of the program, they are surveyed again shortly after giving birth (t0), and when the child is three months (t1), seven months (t2), and twelve months (t3) old, respectively.[12] The survey instruments are mainly based on well-established questions from the Germany Socio-Economic Panel (SOEP) (Goebel et al., 2019) and the National Educational Panel Study (NEPS) (Blossfeld & Von Maurice, 2011), combined with specific questions for the *BRISE* project.

5.4.1 Outcome variables

Our outcome variables on maternal and child outcomes are currently only available for the first two waves (children's age: 3 months (t1) and 7 months (t2)). In the analysis, we standardize all outcome variables except for binary variables.[13]

Mother outcomes
We consider six variables describing maternal behavior and well-being. First, *BRISE* surveys smoking and alcohol consumption during pregnancy and after childbirth. Both

[11] Recruitment only ended in 2022. Thus, future studies can draw on the full sample of families participating in BRISE.

[12] More regular surveys occur at later stages until the child turns six. However, this study focuses on the first year after childbirth; hence, only these first five interviews up to t3 are relevant.

[13] In many of our outcome variables, we observe a non-trivial amount of missing values. These appear to be random in terms of socio-economic characteristics but negatively correlate with the participation in *Pro Kind*. Tables showing the correlations between missings in the outcomes, the control variables, treatment status and the program participation are available upon request.

nicotine and alcohol consumption during pregnancy and breastfeeding is associated with several adverse infant health outcomes (e.g., Polanska et al., 2015). In both cases, the outcome variable takes value one when the mother indicates that, at the time of the interview, she regularly smokes or drinks alcohol, respectively. Second, mothers are also asked about breastfeeding habits. Our outcome variable takes value one if the mother indicates that she is still *breastfeeding* at t1 or t2. Third, we evaluate maternal "*soothing strategies*". *BRISE* surveys whether and how often mothers apply each of the following techniques: "carrying the baby," "leaving the baby cry," "giving the baby medication," "smacking the baby," "breastfeeding," "cradling the child in one's arms," "shaking the baby," "playing music," and "singing for the baby." The variables range on a standard Likert scale (Likert, 1932) from value 0 (never) to value 5 (multiple times per day). We recode the variables such that higher values indicate better soothing strategies and build an index by adding the different items. The index ranges between 32 and 54, with a mean of 48.4 in t1 and 47.0 in t2.

Fourth, besides these variables covering different aspects of the maternal behavior towards the child, we also focus on outcomes related to *maternal well-being*. Besides the individual and societal relevance of well-being, e.g., enhancing productivity (DiMaria et al., 2020), maternal well-being is also linked to child development, e.g., improving verbal skills and reducing socio-emotional problems (Berger & Spiess, 2011). An essential aspect of this is whether the mother has experienced postnatal depression. A total of ten items, e.g., "I was feeling so sad that I had trouble sleeping" and "Things became too much for me," yield the Edinburgh Postnatal Depression Scale (Cox et al., 1996). This scale ranges from 0 (no depression) to 30 (very high risk of depression). As suggested by Cox et al. (1996), we build an indicator variable taking the value 1 for values between 0 and 9 (low risk of depression), 2 for values between 10 and 12 (medium risk of depression), and 3 for all values greater or equal to 13 (high risk of depression). Additionally, we analyze whether the mother perceived a change in her living conditions. This category comprises nine items, which we recode such that they are all positively phrased and add up to build an index.[14].

Child outcomes

"MONDEY" (Pauen, 2011; Pauen et al., 2012) includes a description of 111 mile-

[14]Change in living conditions consists of the following items: 1. "Raising my child brings me joy," 2. "I am often at the end of my strength," "I am satisfied with my new role as a mother," 4. "I often do not feel up to the new tasks and requirements," 5. "I am concerned about my child's health," 6. "My living conditions have changed very much," 7. "Giving my child much tenderness is very important to me," 8. "I suffer from being limited to my role as a mother," 9. "I also get to know others through the child and make new contacts ."These items range on a four-point Likert scale from value 0 (do not agree at all) to 3 (completely agree) (Likert, 1932; Siegle, 2020). The resulting index ranges between 17 and 32 has a mean of 26.4 in t1 and 27.1 in t2.

stones, each assigned to one of eight areas to measure child development (i.e., gross and fine motor development, perception and cognition, language, social relations, self-regulation, and emotions). Together these eight areas offer a standardized inventory to monitor child development from zero to three. MONDEY is conceptionally similar to the Bayley Scales of Infant Development (BSID) (Benson & Haith, 2010). As such, MONDEY is a comprehensive measure for child development that is exceptionally suited to track infant development during the first year of the children's lives. Many other child development indicators only focus on later outcomes. We use standardized developmental scores for each of these eight areas and an overall average score as our outcome variables.[15]

5.4.2 Program participation

BRISE collects very detailed data on the *Pro Kind* participation of the families. Specifically, we have information for each family on the number of home visits, the date of the first and the last visit, whether there was an interruption of program participation, and if so, during which period, and the age of the children when they started and finished the program. This information is collected by the providers of *Pro Kind*. Based on this information, we build a treatment variable. In order to do so, first, we define the number of scheduled visits at each survey time (15 at t1 and 23 at t2, assuming that visits start three months before birth and take place bi-weekly). Second, we define the individual maximum number of visits possible at each survey time by taking the age of children at the program start into account. Third, we estimate the individual participation rate by dividing the number of home visits by the individual maximum number of visits. In the main specification, the participation rate serves as our measure for program participation. In a robustness check, we employ a dummy variable that takes value one if the participation rate is at least 50% and zero otherwise.

5.4.3 Control variables

In our empirical analysis, we control for the following individual and household characteristics: Age of the child in days on the day of the survey, sex of the child, a dummy taking the value one if it is the first-born child of the family, a dummy for whether a doctor assessed the pregnancy to be a high-risk pregnancy, household net income[16],

[15]However, since we do not find effects in any of the separate developmental areas, we only present the results on the overall score (Table 5.4).

[16]Due to issues with missing values, a part of the observations of family income are imputed. We use mean values at different educational levels of the mother to impute missing values. We then divide

and parental characteristics[17]. Besides the mother's age, we include the school degree of the mother[18], the training level of the mother[19], the labor market participation of both parents during the screening phase, i.e., pre-birth[20]. In addition, we control for migration background[21] and self-rated satisfaction of the mother with the family situation during the screening interview, i.e., before the treatment started. Our set of control variables includes the most important socio-economic characteristics which are either exogenous or measured before the treatment occured.

Descriptive statistics of outcome and control variables and program participation for the full sample of families are depicted in column 2 of Table 5.2. In the full sample, 8–9% of children participate in *Pro Kind* whereof the majority lives in treatment districts. The relatively low participation rate may be explained by the fact that only first time mothers (56% of the sample) are eligible for *Pro Kind* participation. The means of the outcomes are reported for both measurement times: three months (t1) and seven months (t2). While smoking is relatively constant across measuring times (12–13%), the share of mothers who drink alcohol doubles from t1 to t2. Postnatal depressions, change in living conditions and soothing strategies are relatively constant across measuring times. Except for soothing strategies in t2, there are no statistically significant differences in the outcomes between treatment and control group.

5.5 Empirical strategy

5.5.1 Effectiveness analysis

In a first step, we estimate simple OLS regressions of the following form:

$$y_i = \beta_1 + \beta_2 PK_i + X_i'\beta_3 + \mu_i \tag{5.1}$$

the observed and imputed observations into six income categories: below 750 euros (1), 750-1500 euros (2), 1500-2500 euros (3), 2500-3500 euros (4), 3500-5000 euros (5), and over 5000 euros (6).

[17] The control variables except for the labor force status are only available for the mother.

[18] Here, we distinguish between having no school degree (1) as the base category having a general school degree (2), i.e., below 12 years of education, and having a high school degree (3), i.e., having obtained the German *(Fach-)Abitur* or completed at least 12 years of schooling within or outside of Germany.

[19] In this variable, no training (1) forms the base category, apprenticeship/technical training takes value 2, and academic degrees takes value 3.

[20] Here, we apply the following categories: not being in the labor force (1) as the base category, working part-time (2), and working full-time (3).

[21] This variable takes value zero if no parent has a migration background, one if one parent was either born abroad or has an indirect migration background, and two if the latter applies to both parents.

where y_i are the different child and maternal outcome variables. The variable of interest, program participation (PK_i), is a continuous variable that indicates the share of *Pro Kind* visits child i participated in, ranging from zero to 100 percent.[22] X_i' is our vector of control variables, as described in section 5.4. However, employing the OLS model in Equation 5.1 does not necessarily produce estimates that can be interpreted as causal. Identifying a causal effect of program participation on child and maternal outcomes faces potential endogeneity threats. The choice for participating might be influenced by unobserved characteristics that also affect the outcome variables, causing an omitted variables bias. One example of such an unobserved variable is the parents' openness to advice in parenting style. This degree of openness could influence the likelihood of participation and directly affect our outcomes (e.g., maternal smoking behavior). Another threat could be reverse causality; for example, maternal well-being might influence how much support from such programs mothers need and thus demand. Thus, estimating Equation 5.1 might lead to a biased and inconsistent estimate of program participation and would not reflect a causal effect.[23]

To overcome these endogeneity issues, we exploit the fact that BRISE randomly assigned an information and access treatment on the neighborhood level. Formally, we exploit this random variation within an instrumental variable (IV) framework (e.g., Angrist et al., 1996). Thus, we predict the variation in program participation using the assigned treatment status on the neighborhood level as an instrument that determines the endogenous regressor (PK_i) but only affects the dependent variables (y_i) through its effect on this independent variable (program participation).

Validity of the instrument. In order for the access and information treatment to qualify as a valid instrument, it must fulfill several conditions: The relevance and the exogeneity assumptions. Relevance means that the instrument must sufficiently correlate with the endogenous regressor, i.e., program participation. Arguably, the access and information treatment satisfies the relevance condition as it enhances the popularity of the programs and lowers entry barriers. The correlation between the instrument and the endogenous regressor is empirically tested in the first stage regression, where the endogenous variable is regressed on the instruments and the exogenous covariates

[22]Note, the variable can take values above 100 percent as we assumed conservative number of maximum visits. Thus, families participating very regularly, can achieve more than the assumed maximum number of visits.

[23]There are reasons to expect both upwardly and downwardly biased OLS estimates. For example, if only mothers open for advice participate, we expect the OLS estimator to be upward biased. Alternatively, if we expect mothers with low subjective well-being to be more likely to seek support and thus participate, the OLS estimator would be downward biased. We cannot account for the endogeneity issues by including all confounding factors as control variables, as some of them are not observed in the data at hand or might be unknown.

(columns 1 and 4 in Table 5.4). The robust first stage F-statistics displayed in the main regression table in section 5.6 (Table 5.4) are mostly around 15 to 20. This result supports our argument.

The more critical assumption is the exogeneity assumption of the instrument, which requires that the instrument is not correlated with the error term and thus influences the outcome variable only through the endogenous regressor. It seems plausible that the access and information treatment influences our outcomes only through program participation if the randomization worked perfectly and the treatment and control group are balanced in their socio-economic characteristics. On the neighborhood level, the randomization worked well, i.e., there are no statistically significant differences in socio-economic characteristics between treatment and control districts except for the number of births (see section 5.3 and Table 5.A.1). Columns 3 and 4 in Table 5.2 display means of relevant socio-economic characteristics comparing the families from treated and control neighborhoods. It becomes apparent that the two groups exhibit significant differences in some key SES characteristics: Specifically, individuals in the treatment group are significantly more likely to have only a basic school degree, no professional training, to be unemployed (before birth), to have a lower income and to be younger, and less likely to be employed full-time.

In order to account for these differences, we combine our IV and reduced form estimations with entropy balancing (Hainmueller, 2012), a matching strategy that balances pre-treatment controls more effectively than comparable propensity score methods. This matching step is conducted before running the IV or reduced form estimations. The main idea of entropy balancing is to assign a weight to observations in the control group, causing the control group's distributions of the selected covariates to match those of the treatment group in the first two moments, i.e., on mean and variances. As a result, the selected covariates in the treatment and control groups have the same means and variances. If several weighting schemes fulfill this balancing criterion, entropy balancing chooses the weighting scheme where all weights are non-negative and deviate the least from uniform weights (Hainmueller, 2012).

Two-Stage Least Squares. Next, we apply our instrument in a 2SLS approach to estimate the causal effect of program participation. In the first stage, we regress the program participation variable that we assume to be endogenous on our instrument and the exogenous control variables:

$$PK_i = \gamma_1 + \gamma_2 T_i + X_i' \gamma_4 + \varepsilon_i \tag{5.2}$$

Table 5.2: Descriptive statistics

	Unit range	BRISE mean	Treatment mean	Control mean	Difference b
Pro Kind participation					
Participation (t1)	1/0	0.08	0.17	0.01	-0.16***
Participation (t2)	1/0	0.09	0.19	0.02	-0.17***
Control variables					
Mother: no school degree	1/0	0.09	0.12	0.06	-0.06
Mother: General school degree	1/0	0.35	0.42	0.31	-0.11*
Mother: Highschool degree	1/0	0.56	0.46	0.63	0.17**
Mother: no training	1/0	0.26	0.34	0.21	-0.13*
Mother: Apprenticeship degree	1/0	0.40	0.36	0.43	0.07
Mother: Academic degree	1/0	0.34	0.30	0.37	0.07
Mother: Not in the labor force	1/0	0.45	0.53	0.40	-0.13*
Mother: Working part-time	1/0	0.29	0.27	0.30	0.03
Mother: Working full-time	1/0	0.26	0.20	0.30	0.10*
Father: Not in the labor force	1/0	0.18	0.16	0.19	0.03
Father: Working part-time	1/0	0.14	0.15	0.13	-0.02
Father: Working full-time	1/0	0.68	0.69	0.68	-0.01
First child	1/0	0.56	0.59	0.53	-0.05
Both parents born in Germany	1/0	0.50	0.44	0.54	0.10
One parent born outside Germany	1/0	0.20	0.23	0.17	-0.06
Both parents born outside Germany	1/0	0.31	0.33	0.29	-0.04
Single mother	1/0	0.10	0.08	0.11	0.03
Risk pregnancy	1/0	0.32	0.30	0.34	0.04
Household net income	Euros	2586.93	2393.13	2723.47	330.35*
Age Mother	years	31.08	30.20	31.71	1.51*
Satisfaction with family situation	0-2	1.86	1.83	1.87	0.04
Outcomes					
Smoking (t1)	1/0	0.12	0.14	0.11	-0.03
Smoking (t2)	1/0	0.13	0.16	0.11	-0.06
Alcohol (t1)	1/0	0.09	0.09	0.09	-0.00
Alcohol (t2)	1/0	0.18	0.17	0.18	0.01
Breastfeeding (t1)	1-4	3.48	3.39	3.55	0.15
Breastfeeding (t2)	1-3	2.66	2.58	2.71	0.13
EPDS (t1)	0-30	6.65	6.90	6.48	-0.42
EPDS (t2)	0-30	5.92	6.26	5.69	-0.57
Change in living cond. (t1)	8-32	26.43	26.15	26.61	0.46
Change in living cond. (t2)	8-32	27.05	27.04	27.05	0.02
Soothing strategies (t1)	9-54	48.42	48.11	48.62	0.51
Soothing strategies (t2)	9-54	47.00	45.71	47.88	2.17***
MONDEY milestones (t1)	0-26	18.10	18.37	17.91	-0.46
MONDEY milestones (t2)	0-53	32.77	32.90	32.68	-0.23
N		300	124	176	300

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns two to four show the mean values of major SES characteristics for the whole BRISE sample and separately for the treatment and control group. Column five indicates whether the difference between the two is statistically significant. Column one indicates the unit rage of the respective variable. 1/0 indicates a binary variable. *Satisfaction with the family situation* can take values 0 (not satisfied) to 2 (very satisfied). *Breastfeeding* can take values 1 (has never breastfed), 2 (breastfed only in the fist 4 weeks), 3 (breastfed longer than 4 weeks but not anymore) to 4 (I still breastfeed) in t1, and values 1 (never breastfed), 2 (only in the fist 4 weeks), and 3 (still breastfeed) in t2. The variable *change in living conditions* is composed of eight separate items which can take values ranging from 1 (do not agree) to 4 (completely agree), e.g. "raising my child brings me joy". Soothing strategies comprises nine separate items, e.g. I carry my child around when they cry. The *MONDEY milestones* count the number of "milestones" reached at three (t1) and seven (t2) months, e.g. whether the child is able to hold an object.
Source: BRISE (2017-2020), own calculations.

where T_i equals one if the family lives in a treatment neighborhood, PK_i, and X_i' are defined as above in Equation 5.1. The first stage regression is estimated using weighted least squares using the weights from entropy balancing. Since the dependent variable is binary, this corresponds to a linear probability model (LPM). In the second stage, the fitted values of the linear probability model from the first stage $\widehat{PK}_i$ are included as the main explanatory variable:

$$y_i = \beta_1 + \beta_2 \widehat{PK}_i + X_i'\beta_3 + \mu_i \tag{5.3}$$

In this regression, y_i are the different child and maternal outcome variables described in section 5.4. X_i' is again our vector of control variables that is the same as in the first stage regression. β_2 is our coefficient of interest and reflects the 2SLS estimator. Per definition, it estimates the local average treatment effect (LATE)[24] and thus depicts the effect of program participation on our outcomes.[25]

5.5.2 Cost analysis

Cost-efficiency analyses compare the "input" and "output" of measures. On the input side, this means the costs incurred by the measures in question (Spieß, 2013). In our case, this requires analyzing the complete picture of the costs required to run the home visiting program *Pro Kind*. To date, there is no standardized approach to capture the costs of programs (Karoly, 2012; Schmitz & Kröger, 2017) but the most rigorous approach is the ingredients-based method (Levin & McEwan, 2000). This approach begins by collecting detailed information on the types and quantities of resources used and then goes on to attach market or shadow prices to these resources. This means that besides the more obvious cost factors such as personnel costs, it is necessary to also account for "indirect" costs such as in-kind resources used and the opportunity costs (Spieß, 2013).

We conducted yearly cost surveys following the ingredient method, sending out questionnaires to the German Red Cross in Bremen, which is the executing agency of the *Pro Kind* intervention. The surveys take place in a Pen-and-paper Personal Interview (PAPI) version annually between spring and autumn, and the contact persons are asked to provide retrospective estimates of the time allocations of *Pro Kind* employees in the previous calendar year. The cost survey comprises detailed questions on different cost items, such as non-administrative and administrative personnel costs, volunteer

[24] It measures the effect on the compliers, i.e., those families whose program participation is induced by the access and information treatment.

[25] The robust standard errors μ_i are clustered at the household level.

activities, material resources used, investment goods, capital costs, cost of contractual services from third parties, costs for buildings and premises, training measures, overheads, as well as on the planned and actual utilization of the offered services. The information provided by the respondents is then fed into a micro database and analyzed (*BRISE* cost database). The database relies on a 100% response rate of the providers. The current market price for each employee is determined by the collective agreement for the public service (*TVöD*). Material resources that last for multiple years, e.g., office supplies like printers and pedagogical resources like books and toys, are annualized over five years, i.e., their average lifetime. Similarly, professional training measures are averaged over five years.

5.6 Empirical results

5.6.1 Effectiveness analysis

Table 5.3 reports the OLS results, i.e., coefficients obtained by simply regressing the different maternal and child outcomes measured when the child is three and seven months old on the *Pro Kind* participation rate. Across all outcomes, the point estimates are small in size and statistically not significant.[26]

Next, we turn to the results of the 2SLS estimations using the access and information treatment assigned on the neighborhood level as an instrument for program participation (Table 5.4). The first column shows first stage coefficients, namely the effect of living in a treated neighborhood with easier access and information provision on the *Pro Kind* participation rate at three months. Column two shows the reduced form estimates, i.e., the effect of living in treated neighborhoods on child and maternal outcomes at 3 months independent of actual *Pro Kind* program take-up (intention-to-treat effect). Lastly, column three presents the IV estimates, namely the causal effect of participation on child and maternal outcomes at three months. This IV-estimate constitute a LATE-effect, i.e. it estimates the causal effect for the subgroup of compliers, that is, the effect of *Pro Kind* participation for families who participate in the program because of the information provision and easier access to *Pro Kind* but would not have done so otherwise. Columns (4) - (6) show the respective results when the child is seven months old. All regressions are estimated using entropy balancing weights such that control group's distributions of the selected covariates match those of the treatment group in the first two moments.

[26] Due to missing values in the outcome variables, our sample size diminishes from the original 300 observations to 168-259 observations, depending on the outcome and specification.

Table 5.3: OLS results

	3 months	7 months	3 months	7 months
	Maternal smoking		Maternal alcohol consumption	
Pro Kind	0.001	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	247	205	244	198
	Breast feeding		Postnatal depression	
Pro Kind	-0.003	-0.002	0.001	-0.002
	(0.004)	(0.003)	(0.003)	(0.003)
Observations	249	168	259	259
	Change in living conditions		Soothing strategies	
Pro Kind	-0.001	0.005	0.003	-0.002
	(0.003)	(0.004)	(0.004)	(0.004)
Observations	229	172	232	198
	Child development: MONDEY score			
Pro Kind	0.005^{+}	0.001		
	(0.003)	(0.003)		
Observations	235	200		

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the full set of individual control variables. Robust standard errors in parentheses.
Source: BRISE (2017-2020), own calculations.

The first stage coefficients are highly significant and of similar magnitude across specifications: Living in a treatment neighborhood increases the *Pro Kind* participation rate by about 12 to 18 percentage points (depending on the outcome and age of the child). This suggests that the easier access and the information on this program provided to the parents in treated neighborhoods successfully nudged families to participate in *Pro Kind* .

The first panel shows the results for maternal smoking behavior. The reduced form and IV estimates are positive, but very imprecisely estimated: The size of the coefficients in column (3) and (6) amount to a 0.1 percentage point increase in maternal smoking at three months and a 0.2 percentage point increase at seven months if *Pro Kind* participation rates are increased by 10 percentage points.

Next, the second panel displays the results on maternal alcohol consumption. Again, we do not detect any significant effects. Coefficients are negative when the child is three months old, suggesting maternal consumption might slightly decrease in response to the treatment/*Pro Kind* participation. In contrast, coefficients turn positive and are of similar magnitude when children are seven months old. An opposing picture is visible for breastfeeding behavior: positive coefficients at three months and negative coefficients at seven months. As for the other outcomes, the effects are insignificant due to the small sample size and too imprecisely estimated to draw any conclusions.

Furthermore, there are also no significant effects on maternal postnatal depression and mothers' perception of the change in living conditions. However, also these results are too imprecisely estimated to draw conclusions. The last outcome concerning maternal behavior is the index characterizing the frequency of applying soothing strategies. Here, we depict negative and insignificant effects at three months and negative and statistically significant effects at seven months. Thus, the results suggest that living in the treatment districts decreases the application of soothing strategies by about 0.4 standard deviations and participating in *Pro Kind* by 0.024 standard deviations.

Finally, the last panel shows the results of the child development indicator MONDEY. Coefficients are positive at three months and negative at seven months but statistically not significant. We can establish with 95% certainty that the IV effects are not larger than 0.02 and not smaller than -0.023 standard deviations for both measuring times. Thus, despite the small sample size, we can conclude that *Pro Kind* participation – at least in the very short run – does not substantially impact child development.[27]

The results in our preferred specification are based on the full sample (including first and not first time mothers) and the continuous definition of *Pro Kind* participation and employing entropy balancing techniques. Additionally, we employ a binary definition, i.e., turning one if the participation rate is at least 50%. The results presented in Table 5.A.4 are very similar to our main results, i.e., displaying insignificant effects across all outcomes except for soothing strategies at seven months. Only first time mothers are eligible for *Pro Kind participation.* Since in our sample only a bit more than half of all mothers are first time mothers, a significant share of the treatment group (living in treatment districts) are not eligible for *Pro Kind*, and thus by definition non-compliers. In our main specification we use the full sample to increase the sample size. Table 5.A.5 reports the results based on a sample restricted to first time mothers. As expected, the first stage coefficients increase: Living in a treatment district increases program

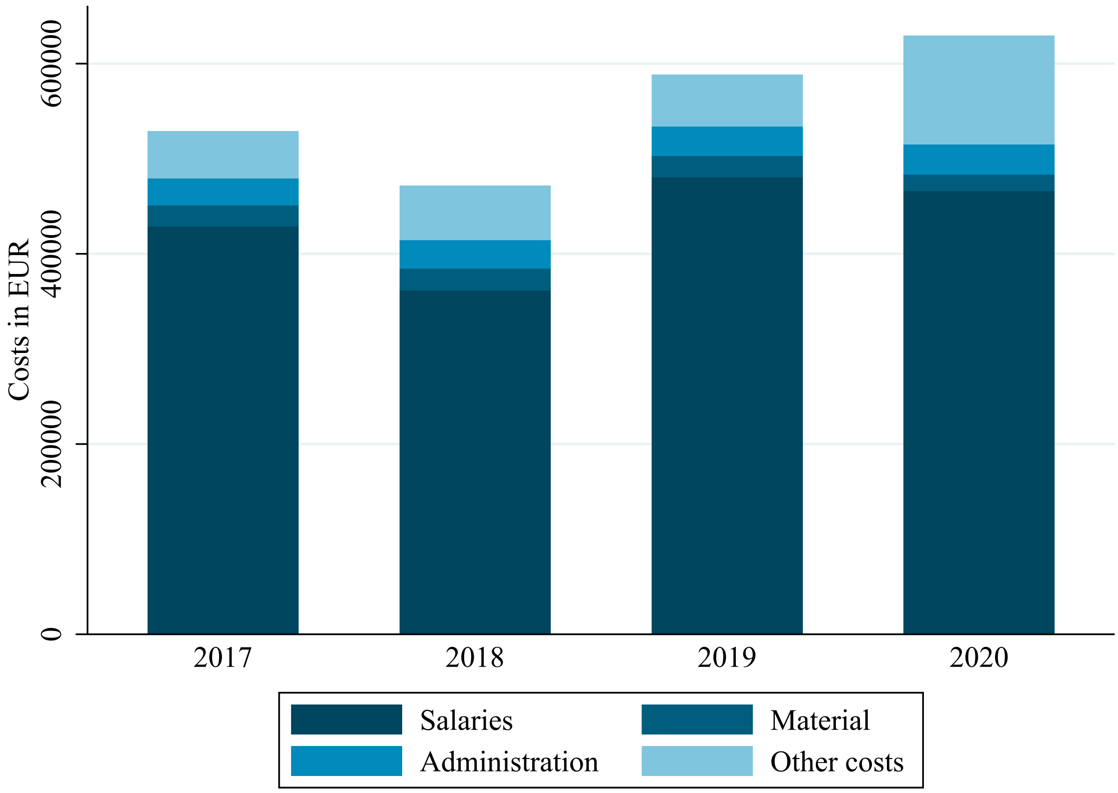
[27]Regressions on the separate MONDEY domains and items yielding the "Mother's perception of change in living conditions" and "Soothing strategies" indices also do not yield statistically significant estimates. Results are available upon request.

participation by 21 to 32 percentage points. However, the coefficients obtained by the reduced form and IV estimations remain statistically insignificant for all outcomes.

5.6.2 Program costs

Figure 5.1 displays the mean costs of *Pro Kind* based on our detailed yearly cost surveys (2017 - 2020). The costs refer to the total costs of *Pro Kind* in the city of Bremen, of which an increasing share participates in *BRISE*.[28] Personnel costs constitute by far the largest cost share, making up around 80 percent of the total costs. This is in line with similar programs, which are generally labor intensive (e.g., Workman, 2018). In 2020, the *Pro Kind* staff had to move offices, which generated above average "other costs". Table 5.A.3 in the Appendix provides a more detailed breakdown of the costs.

Figure 5.1: Cost development *Pro Kind*



Notes: Costs in Euro per year.
Source: BRISE 2017-2020, own calculations.

For the years 2017 through 2020, the average costs per participant per year in *Pro Kind* ranged between 3,468 and 3,861 Euros, and costs per home visit ranged between

[28]While in 2017, only five BRISE children participated in *Pro Kind*, this number rose to 63 in 2020.

Table 5.4: IV results

	First stage	Reduced form	IV	First stage	Reduced form	IV
			Maternal smoking behavior			
	3 months			7 months		
Info Treatment	11.574***	0.013		13.866***	0.035	
	(2.849)	(0.045)		(3.392)	(0.049)	
Pro Kind			0.001			0.002
			(0.004)			(0.003)
Observations	247	247	247	205	205	205
$F-statistic$			18.732			19.462
			Maternal alcohol consumption			
	3 months			7 months		
Info Treatment	11.930***	-0.010		14.997***	0.011	
	(2.901)	(0.041)		(3.685)	(0.060)	
Pro Kind			-0.001			0.001
			(0.003)			(0.004)
Observations	244	244	244	198	198	198
$F-statistic$			19.214			18.903
			Breast feeding			
	3 months			7 months		
Info Treatment	11.564***	0.008		15.240***	-0.063	
	(2.825)	(0.141)		(4.199)	(0.167)	
Pro Kind			0.001			-0.004
			(0.011)			(0.010)
Observations	249	249	249	168	168	168
$F-statistic$			18.978			14.727
			Maternal postnatal depressions			
	3 months			7 months		
Info Treatment	12.175***	0.073		14.110***	0.134	
	(2.773)	(0.131)		(2.981)	(0.129)	
Pro Kind			0.006			0.009
			(0.010)			(0.009)
Observations	259	259	259	259	259	259
$F-statistic$			21.766			24.978
			Mother's perception of change in living conditions			
	3 months			7 months		
Info Treatment	11.888***	-0.081		17.643***	-0.081	
	(3.117)	(0.139)		(4.033)	(0.158)	
Pro Kind			-0.006			-0.005
			(0.011)			(0.009)
Observations	229	229	229	172	172	172
$F-statistic$			16.518			19.943
			Soothing strategies			
	3 months			7 months		
Info Treatment	11.754***	-0.035		15.920***	-0.398**	
	(3.030)	(0.154)		(3.653)	(0.152)	
Pro Kind			-0.003			-0.024*
			(0.012)			(0.010)
Observations	232	232	232	198	198	198
$F-statistic$			16.753			21.088
			Child development: MONDEY score			
	3 months			7 months		
Info Treatment	11.614***	-0.015		16.315***	0.014	
	(3.089)	(0.138)		(3.846)	(0.153)	
Pro Kind			-0.001			0.001
			(0.011)			(0.009)
Observations	235	235	235	200	200	200
$F-statistic$			15.704			20.457

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. All regressions include the full set of individual control variables. Regressions are weighed with entropy balancing weights.
Source: BRISE (2017-2020), own calculations.

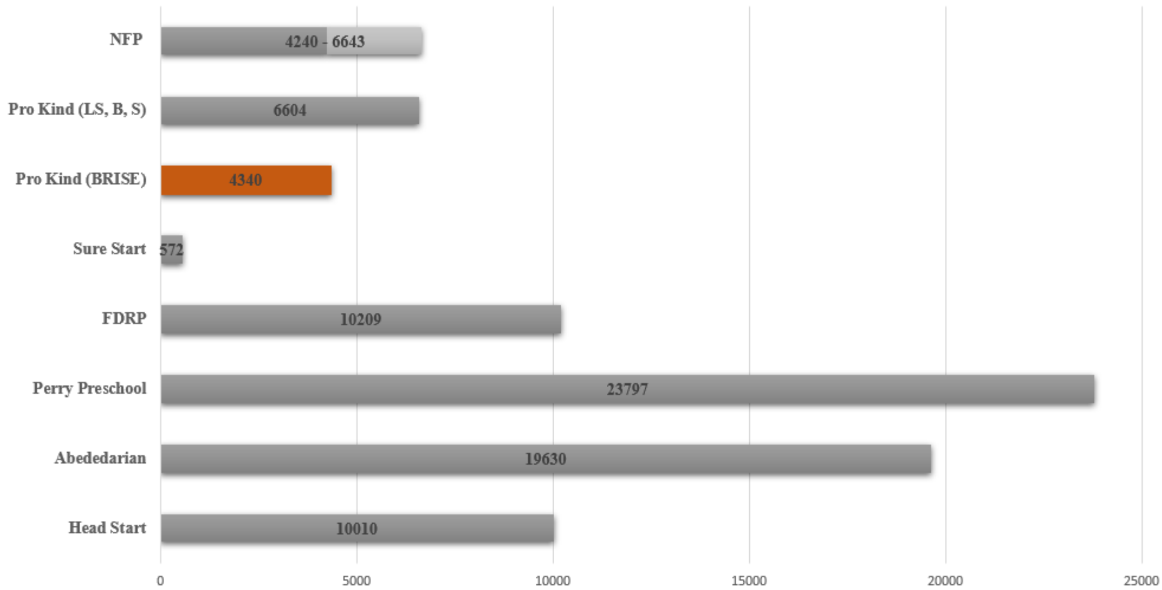
265 and 325 Euros (Table 5.5). Figure 5.2 relates the total cost per participant per year to other early childhood programs that also entail parenting support elements. The comparison reveals that *Pro Kind* in Bremen is less expensive than the average cost estimated by Maier-Pfeiffer et al. (2013) for *Pro Kind* in three German federal states. In fact, *Pro Kind* within BRISE has lower average costs per child than any comparable program except Sure Start, which is a less intensive non-targeted program. The prominent Perry Preschool program is roughly 5.5 times as expensive as *Pro Kind*.

Table 5.5: Pro Kind cost summary: 2017-2020

	2017	2018	2019	2020
Costs per participant	3861.2	3467.6	3644.4	3702.7
Costs per home visit	324.53	304.1	309.9	264.815

Notes: Costs are reported in 2021 EUR.
Source: BRISE (2017-2020), own calculations.

Figure 5.2: Comparison of per child costs of selected programs in 2021 USD



Notes: Costs are per child per year and are inflated to 2021 USD. The cost estimates may refer to different data collection methods, due to lack of a harmonized approach (Karoly, 2012).
Sources: Head Start: 7000$ in 2004 USD (U.S. Department of Health & Human Services, 2004); Abecedarian project: 13000$ in 2002 USD (Barnett & Masse, 2007); Perry Preschool: 17759 $ in 2006 USD (Heckman et al., 2010); FDRP: 7345$ in 2005 USD (Besharov et al., 2011); Sure Start: 416GBP in 2021 GBP (Cattan et al., 2021); *Pro Kind* (Brise): €3669 in 2021 EUR (BRISE, own calculation); *Pro Kind* (LS= Lower Saxony, B= Bremen, S= Saxony): €4353 in 2012 EUR (Maier-Pfeiffer et al., 2013); NFP: 3420-5358$ in 2010 USD (Miller & Hendrie, 2015).

5.7 Conclusion

Investments in early childhood have the potential to effectively mitigate socio-economic inequalities (e.g., Heckman et al., 2010). While center-based daycare programs play an important role (e.g., Barnett, 1985; Havnes & Mogstad, 2011; Heckman et al., 2013b; Karoly et al., 2006; Kautz et al., 2014), home-visiting programs targeting parenting skills and knowledge are an equally important – and in the European context under-researched – component of early childhood education and care. These programs advise parents on aspects of everyday life with an infant, such as mother-and-child interaction, nutrition, and ways to seek support when needed. An often neglected aspect of impact evaluations is the cost of these programs. Comprehensively assessing both the benefits and costs of investments builds the basis for later cost-effectiveness and cost-benefit analyses.

In this study, we present novel evidence on the early effects and costs of the home-visiting program *Pro Kind* under the Bremen Initiative to Foster Early Childhood Development (BRISE). We find no economically meaningful or statistically significant effects of *Pro Kind* on an extensive range of outcomes covering different aspects related to maternal and child well-being at three and seven months after childbirth. To establish causality, we exploit a random information and access treatment to this program at the neighborhood level that induced families to participate in *Pro Kind*.

Our cost analysis reveals that the average costs per participant per year in *Pro Kind* range between 3,468 and 3,861 Euros over the study period (2017–2020). In international comparison, *Pro Kind* belongs to the less costly programs, with its cost corresponding to about 18 percent of the per-child-per-year expenditure of the Perry Preschool program.

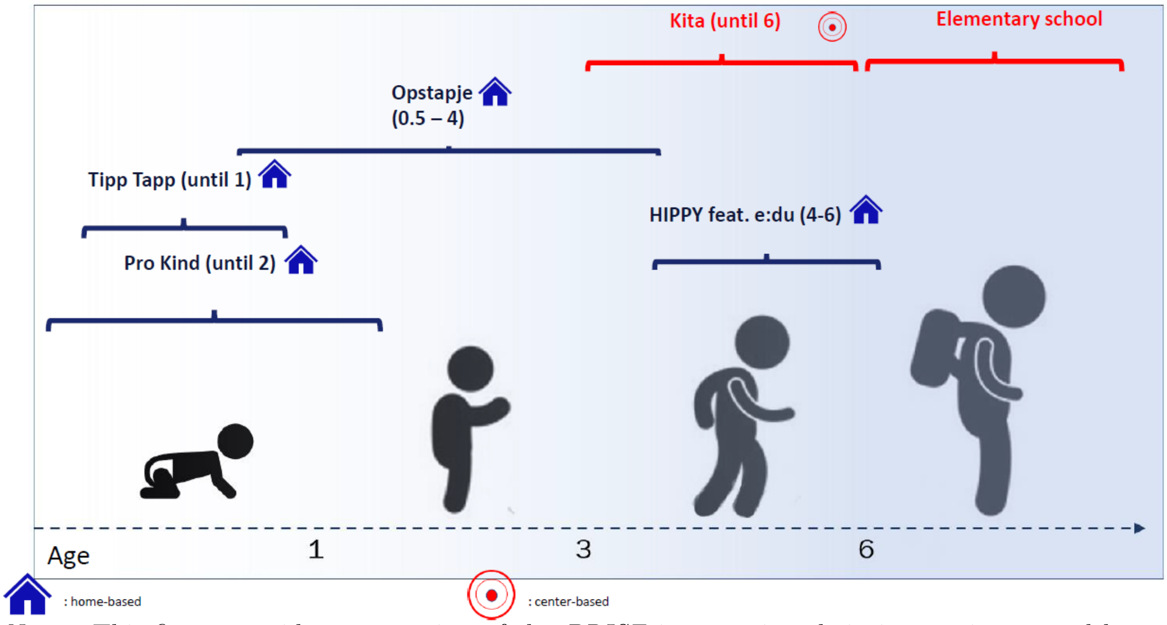
Our findings regarding *Pro Kind*'s effectiveness in improving mother and child outcomes should be interpreted in the light of the data challenges we faced: With just 300 total observations, we have a relatively small sample, which is further diminished by missing values in important control variables. This forces us to impute variables in some cases (e.g., household income) and to exclude other potentially important variables, such as the father's education. This leads to imprecisely estimated coefficients which do not allow us to derive conclusions regarding the early effects of *Pro Kind*. Lastly, randomization of the treatment on the neighborhood level was non-perfect as families significantly differ in some socio-economic characteristics (Table 5.2). We tackle this problem by employing entropy balancing. However, perfect randomization is always cleaner than imposed randomization, especially given the incomplete set of control variables.

Furthermore, it is not surprising that *Pro Kind* does not significantly impact child and mother outcomes in a child's first months. It usually takes several years for the effects of such programs to materialize (e.g., Jungmann et al., 2015; Sandner, 2013, 2019; Sandner et al., 2018). Hence, future studies evaluating the effectiveness of *Pro Kind* at later stages are better positioned to conduct a sound analysis. Our analysis paves the way for later cost-effectiveness and cost-benefit analyses within *BRISE*. At later measurement times and with a larger sample size, it should be possible to not only achieve more meaningful effectiveness estimations for *Pro Kind* but also to measure the effects of *Opstapje* - another program that starts shortly after birth and is part of the chain of interventions set up within BRISE. Along with the yearly collected cost data for all programs within *BRISE*, this allows to conduct a cost-effectiveness study, which is only possible when comparing at least two separate programs (Karoly, 2012; Spieß, 2013).

5.A Appendix

5.A.1 Additional information on *BRISE*

Figure 5.A.1: *BRISE* intervention chain



Notes: This figure provides an overview of the *BRISE* intervention chain integrating several home-based and center-based programs from birth to school.

Source: *BRISE* consortium 2022.

Figure 5.A.2: Pro Kind Flyer

Notes: This figure depicts the information flyer for the program *Pro Kind* provided to *BRISE* families by the family counselors.

Source: *BRISE* consortium.

5.A.2 Additional results

Table 5.A.1: Randomization by city districts

	Control group mean	Treatment group mean	Difference b
School degree	83.82	84.16	-0.342
Unemployment transfers	34.68	40.61	-5.928
Relocation from district	35.41	30.43	4.976
Share of inhabitants with migration background	19.78	23.17	-3.394
Birth rate	100.98	110.64	-9.658
Number of births	72.12	95.40	-23.282*
Complete social Index	537.550	572.69	-35.137
N	17	10	27

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Source: Administrative data from the city of Bremen, own calculations.

Table 5.A.2: Descriptive statistics: Brise vs. SOEP

	BRISE mean	SOEP mean
Mother: no school degree	0.09	0.01
Mother: Basic school degree	0.35	0.48
Mother: Highschool degree	0.56	0.51
Mother: no training	0.26	0.18
Mother: Apprenticeship degree	0.40	0.50
Mother: Academic degree	0.34	0.32
Mother: Not in the labor force	0.45	0.22
Mother: Working part-time	0.29	0.44
Mother: Working full-time	0.26	0.34
Father: Not in the labor force	0.18	0.19
Father: Working part-time	0.14	0.14
Father: Working full-time	0.68	0.67
No migration background	0.50	0.70
One parent with migration background	0.20	0.19
Both parents with migration background	0.31	0.11
Household net income	2586.93	3818.75
Age Mother	31.08	31.91
Single mother	0.10	0.16
Satisfaction with family situation	1.86	1.83
First child	0.56	0.34
N	300	494

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The first two columns show the mean values of major SES characteristics for the treatment and control group. Column three indicates whether the difference between the two is statistically significant. Column four shows the mean value for the same variables in the SOEP, restricting the sample to urban households, families with children below one and dropping the SOEP migration (M1-M5), low-income (L2) samples and all observations surveyed before 2010.

Source: BRISE (2017-2020), SOEP v.36 (2010-2019), own calculations.

Table 5.A.3: Pro Kind cost summary: 2017-2020

Variable	Obs	Mean	Std. Dev.	Min	Max
Resources used					
Labor costs (excl. admin.)					
Number of employees (excl. administration)	4	16.75	1.5	15	18
Weekly hours employees (without administration)	4	306.356	45.582	246.766	357.678
Weekly hours per employee	4	18.24	1.589	16.451	19.871
Total personnel costs (excl. admin.)	4	426493.3	45843.69	361623	466005
Wage per employee	4	25507.61	2460.844	23823.69	29125.31
Personnel costs (admin.)					
Number of employees (administration)	0	.	.	.	.
Weekly hours employees (administration)	0	.	.	.	.
Total personnel costs (admin.)	4	30266.51	1444.765	28470.46	31898.64
Material costs					
Total value of expenses material	4	21268.81	2674.629	17274.39	22808.41
Capital goods					
Expenditure on capital goods	4	.75	.5	0	1
Purchase price of capital goods	2	4792.655	469.766	4460.48	5124.83
Rental/lease expenses capital goods	1	615.96	.	615.96	615.96
Amount of capital costs	3	601.317	313.288	247.22	842.48
Costs for rooms					
Rental costs for rooms	4	26425.96	7951.809	14995.05	32745.59
Other ressources					
Costs for other resources	4	16556.56	11006.9	9819.29	32992.46
Costs for further training	4	10009	3589.392	4741.65	12787.69
Overheads					
Total overheads (excl. admin.)	4	30266.51	1444.765	28470.46	31898.64
Use of the offers					
Planned utilization					
Places with committed funding (planned)	4	138.75	2.5	135	140
Home visits per family (planned)	4	22	0	22	22
Actual utilization					
Places used (realized)	4	149	16.021	136	170
Number of regularly participating families	2	54	16.971	42	66
Number of families who joined later	2	107.5	4.95	104	111
Total home visits (realized)	4	1839.25	373.126	1551	2377
Home visits per family (realized)	2	22	0	22	22
Average number of home visits (reg.)	2	18.765	1.082	18	19.53
Average number of home visits (irreg.)	2	10.23	.325	10	10.46
Number of home visits by phone, videocall, walk	1	1221	.	1221	1221
General information					
Total costs in Bremen	4	546908.1	65633.71	471592.5	629464.4
Total costs within BRISE	4	141128.2	99852.28	19306	233272.1
Personnel as a share of total costs	4	.795	.053	.74	.861
Proportion of admin. in total costs	4	.056	.005	.051	.063
Share of materials in total costs	4	.04	.009	.027	.048
Share of other costs in total costs	4	.124	.041	.093	.182
Costs per participant	4	3668.978	162.532	3467.592	3861.201
Costs per home visit	4	300.837	25.51	264.815	324.53

Notes: The table displays mean, standard deviation, minimum and maximum of costs reported in EUR.
Source: BRISE (2017-2020), own calculations.

5.A.3 Robustness

Table 5.A.4: IV Results: treatment-dummy

	First stage	Reduced form	IV	First stage	Reduced form	IV
	Maternal smoking behavior					
	3 months			7 months		
Info Treatment	0.121***	0.013		0.150***	0.035	
	(0.034)	(0.045)		(0.043)	(0.049)	
Pro Kind			0.098			0.219
			(0.336)			(0.301)
Observations	247	247	247	205	205	205
$F-statistic$			14.264			13.565
	Maternal alcohol consumption					
	3 months			7 months		
Info Treatment	0.125***	-0.010		0.175***	0.011	
	(0.035)	(0.041)		(0.047)	(0.060)	
Pro Kind			-0.075			0.060
			(0.301)			(0.311)
Observations	244	244	244	198	198	198
$F-statistic$			14.685			15.265
	Breast feeding					
	3 months			7 months		
Info Treatment	0.121***	0.008		0.165**	-0.063	
	(0.034)	(0.141)		(0.050)	(0.167)	
Pro Kind			0.062			-0.360
			(1.056)			(0.913)
Observations	249	249	249	168	168	168
$F-statistic$			14.402			12.244
	Maternal postnatal depressions					
	3 months			7 months		
Info Treatment	0.126***	0.073		0.150***	0.126	
	(0.033)	(0.131)		(0.037)	(0.121)	
Pro Kind			0.547			0.826
			(0.961)			(0.803)
Observations	259	259	259	259	259	259
$F-statistic$			16.209			18.159
	Mother's perception of change in living conditions					
	3 months			7 months		
Info Treatment	0.126***	-0.081		0.194***	-0.081	
	(0.037)	(0.139)		(0.051)	(0.158)	
Pro Kind			-0.594			-0.409
			(0.988)			(0.779)
Observations	229	229	229	172	172	172
$F-statistic$			13.278			14.558
	Soothing strategies					
	3 months			7 months		
Info Treatment	0.124***	-0.035		0.174***	-0.398**	
	(0.036)	(0.154)		(0.045)	(0.152)	
Pro Kind			-0.263			-2.212*
			(1.133)			(0.960)
Observations	232	232	232	198	198	198
$F-statistic$			13.296			15.816
	Child development: MONDEY score					
	3 months			7 months		
Info Treatment	0.122***	-0.015		0.180***	0.014	
	(0.036)	(0.138)		(0.048)	(0.153)	
Pro Kind			-0.112			0.075
			(1.039)			(0.786)
Observations	235	235	235	200	200	200
$F-statistic$			12.536			15.651

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. All regressions include the full set of individual control variables. Regressions are weighed with entropy balancing weights.
Source: BRISE (2017-2020), own calculations.

Table 5.A.5: IV Results: first-time mothers

	First stage	Reduced form	IV	First stage	Reduced form	IV
	Maternal smoking behavior					
	3 months			7 months		
Info Treatment	21.106***	0.029		28.244***	0.027	
	(4.848)	(0.053)		(5.695)	(0.060)	
Pro Kind			0.001			0.001
			(0.002)			(0.002)
Observations	137	137	137	110	110	110
$F - statistic$			19.561			25.783
	Maternal alcohol consumption					
	3 months			7 months		
Info Treatment	21.624***	-0.006		30.555***	0.070	
	(4.935)	(0.040)		(6.249)	(0.082)	
Pro Kind			-0.000			0.002
			(0.002)			(0.002)
Observations	136	136	136	106	106	106
$F - statistic$			19.795			25.082
	Breast feeding					
	3 months			7 months		
Info Treatment	21.324***	-0.051		30.966***	-0.053	
	(4.853)	(0.203)		(7.474)	(0.209)	
Pro Kind			-0.002			-0.002
			(0.009)			(0.006)
Observations	137	137	137	89	89	89
$F - statistic$			19.827			18.257
	Maternal postnatal depressions					
	3 months			7 months		
Info Treatment	21.781***	-0.106		27.897***	0.113	
	(4.719)	(0.166)		(5.016)	(0.169)	
Pro Kind			-0.005			0.004
			(0.007)			(0.006)
Observations	142	142	142	142	142	142
$F - statistic$			21.596			31.278
	Mother's perception of change in living conditions					
	3 months			7 months		
Info Treatment	22.062***	-0.340+		31.363***	-0.058	
	(5.450)	(0.179)		(6.413)	(0.211)	
Pro Kind			-0.014+			-0.002
			(0.008)			(0.006)
Observations	127	127	127	99	99	99
$F - statistic$			17.771			25.470
	Soothing strategies					
	3 months			7 months		
Info Treatment	20.663***	0.110		29.587***	-0.439*	
	(5.160)	(0.232)		(6.063)	(0.212)	
Pro Kind			0.005			-0.014*
			(0.010)			(0.007)
Observations	128	128	128	112	112	112
$F - statistic$			16.434			24.085
	Child development: MONDEY score					
	3 months			7 months		
Info Treatment	22.189***	0.064		31.802***	-0.093	
	(5.287)	(0.211)		(6.330)	(0.219)	
Pro Kind			0.003			-0.003
			(0.008)			(0.006)
Observations	132	132	132	110	110	110
$F - statistic$			18.129			25.041

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. All regressions include the full set of individual control variables. Regressions are weighed with entropy balancing weights.
Source: BRISE (2017-2020), own calculations.

CHAPTER 6

Conclusion

This dissertation explores the impact of education and environmental policies on human capital formation and inequalities therein. It pays particular attention to the role of the school, home, and "natural" environment (i.e., environmental factors such as air pollution) and the interplay between these areas. The analyses encompasses various facets of human capital, such as cognitive and non-cognitive development measures, and health. The different chapters investigate the effectiveness of a family policy on mothers and children during early childhood, the effects of changes in school inputs and improvements in air quality during primary schooling, and school choice patterns during primary and secondary schooling. The results indicate that public policies can affect human capital development in diverse ways. This final chapter concludes by briefly discussing the limitations and policy implications of the different studies.

Chapter 2 analyzes the effects of the expansion of after-school care (ASC) on the skill development of elementary school children in Germany. The provision of institutionalized ASC is often advocated as a means to offer homework support to children who lack it at home, thereby promoting equality of opportunity. However, despite being of considerable policy interest, it remains unclear whether these afternoon programs benefit child development and whether the selection mechanism for attendance is efficient, meaning whether students who benefit the most choose to attend. This paper explores the effects of ASC on elementary school children's academic and non-cognitive skill development. Using a Marginal Treatment Effect (MTE) framework and regional and temporal variations caused by a comprehensive reform in Germany, I instrument after-school care attendance with the change in distance to the next school offering ASC within a district. The results indicate that children from lower socio-economic backgrounds (SES), who are more likely to opt for treatment, receive higher ASC premiums. The treatment effects on the non-cognitive skills of those receiving ASC are more pronounced than those who do not, suggesting that the selection mechanism into ASC is positive and efficient. A universal voluntary offer of ASC is likely to help alleviate educational inequality.

These findings have significant policy implications that are relevant for the ongoing expansion of ASC in Germany. First, it confirms that low-SES students benefit from the increased time spent in school in ASC. Consequently, a universal offer of ASC slots could prove instrumental in promoting equality of opportunity. Second, positive selection into treatment suggests that participation in afternoon programs should be voluntary, rather than mandatory, for all students. However, the current trend of primarily low-SES students using these offers risks exacerbating segregation, rendering such programs unappealing for high-SES students. Thus, investments in their quality are necessary to ensure the efficacy of ASC programs across diverse socio-economic backgrounds. This also becomes apparent by the fact that I do not find positive effects on German and math grades or overall positive effects on non-cognitive skill formation. Investments in the quality of ASC entail employing more and better-qualified personnel, which will likely constitute a significant challenge given that ASC offers are currently still expanding at a fast pace. At the same time, Germany already faces a shortage of teachers and educators in many regions.

The unique combination of the SOEP with administrative school data allows the estimation of MTEs for a range of interesting child development outcomes. One likely reason that the MTE literature is still relatively small is the particular data requirements necessary to estimate MTEs (see, e.g., Cornelissen et al., 2016). While the unique combination of the SOEP with administrative school address data meets most of these requirements, the relatively small sample size limits the precision of the estimates. MTE is data-hungry method since it heavily weighs individuals at the extreme ends of the propensity score distribution (see, e.g., Andresen, 2018). Therefore, my study could be replicated using a more extensive data set – which arguably is difficult, especially for non-cognitive skills of elementary school children in the German context. Another potential limitation of my results on non-cognitive skills is that I cannot rule out that the impression mothers have of their children attending ASC is affected by the shorter time window they spend together and the activities they share compared to families in the control group. For example, children regularly attending school in the afternoon may be more tired when they get home, which can systematically impact how they interact with their parents. Furthermore, since they have to do homework less often at home, there may be fewer conflicts at home, leaving the parents under the impression that the child is more emotionally stable. Finally, while the care received in ASC is certainly more homogeneous than the care children receive at home, quality will likely differ across schools and regions. Hence, even though I control for district and survey-year fixed effects, I cannot discount the possibility that some of the effect heterogeneity patterns I find are due to differences in ASC quality.

Chapter 3 delves into the topic of the expansion of the private school sector in Germany. Article 7(4) of the Basic German Law requires private schools to adjust their fees based on parents' income to avoid discrimination based on socio-economic status. Despite this regulation, socio-economically disadvantaged children are significantly under-represented in private schools. This study aims to investigate the role of the geographical distribution of private schools in explaining this unbalanced selection pattern in private schools. We utilize geo-referenced data from the Socio-economic Panel and address data for all German schools from 2000 to 2019 and estimate linear probability models. Our results suggest that high-SES households are not necessarily located closer to private schools, but are more sensitive to distance when making decisions about private school enrollment. Our findings indicate that personal preferences and a lack of information on alternative schools, in addition to school fees, may deter low-SES students from attending private schools and that the spatial distribution of private schools plays a subordinate role in this regard.

Separate analyses for federal states with regulations that forbid schools from charging fees show that less educated households behave more "distance-sensitively" than in other states. This finding demonstrates that costs matter as to whether private schools are a viable option for low-SES children. Consequently, one policy takeaway from Chapter 3 is that uniform and binding standards regarding income-based school fees or upper limits could help reduce social inequality. This may also necessitate the provision of private schools with financial support similar to that of public schools. The success of state-funded and autonomous "Charter Schools" in the USA serves as evidence that this model can be successful, fostering healthy competition in the education system without resulting in increased segregation (e.g., Angrist et al., 2013).

However, evidence from tuition-free federal states suggests that social inequalities in private school attendance are not solely due to school fees, since the overall attendance patterns are similar to the rest of Germany. If children from low-income households or those with limited education do not perceive private schools as viable alternatives to public schools, even more restrictive school fee models would do little to address these inequalities. In such cases, making information on school choice alternatives in the private school system more accessible may be helpful. This would require a higher level of transparency in the private school system. Private schools are not part of public education reporting despite being mostly publicly funded. Educational research needs to gain more knowledge of the selection processes, teacher training and compensation, and competency development in private schools. Policies may be implemented demanding private schools to become part of public education reporting to create more transparency. Ultimately, the top priority should be to make the public education system more attractive to households with privileged backgrounds so that they do not increasingly turn to the private sector. Public schools should be equipped to face the increasing competition through private schools.

The caveat of the empirical analysis in Chapter 3 is that it is specific to the German context and may lack external validity. Because the private school system is heterogeneous, it is challenging to derive general conclusions, even in the German context. Since information on the type of school (public vs. private) is only surveyed in selected years in the SOEP, our sample size is limited and does not allow us to conduct separate analyses on different private school types, distinguishing, for example, confessional schools and schools with special pedagogical concepts, such as Waldorf schools. Hence, we can only identify general patterns in the role of the spatial distribution of private schools for the socio-economic composition thereof, and it may be the case that our overall results do not hold for specific private school types.

In **Chapter 4**, we focus on the effects of reductions in air pollution on student attainment in elementary school. Low Emission Zones (LEZs) have been demonstrated to enhance public health by limiting the access of emission-intensive vehicles to designated areas, thereby curbing local air pollution. Nonetheless, the impact of driving restriction policies on other aspects of life is still a matter of little knowledge. This paper investigates the effect of LEZs on the academic attainment of elementary school students in Germany, measured by the transition rate to secondary schools. We employ school-level data from North-Rhine Westphalia (NRW), Germany's most populous federal state, and leverage the staggered implementation of LEZs since 2008 within a difference-in-differences framework. Our findings indicate that LEZs augmented the transition rates to the academic track by 0.9-1.6 percentage points in NRW. Our district-level results for the entirety of Germany confirm the external validity of these findings. Furthermore, using geo-referenced data from the SOEP, we provide suggestive evidence that a decrease in the prevalence of respiratory infections is a vital mechanism through which LEZs affect educational outcomes.

Our findings are significant for policymaking in several respects. Most importantly, they reveal that social and ecological aims can complement each other. Discourse on green transition often contends that ambitious environmental and climate policies are both costly and unjust, disproportionately burdening economically disadvantaged households. Although this may hold in certain instances, our research shows that environmental policies can yield beneficial social outcomes beyond their intended objectives in other instances. In the case of LEZs, besides improving the health outcomes of the affected population, they advance the formation of human capital – indeed, *benefiting* disadvantaged households disproportionately because they face higher baseline air pollution levels on average. An exhaustive assessment of the potential ways in which environmental policies can impact people's lives is imperative for evaluating their costs and benefits. Pointing out favorable "externalities" of policies like LEZs that may initially be unpopular can engender political support for such measures. This highlights the need for more in-depth research on the socio-economic effects of environmental policies.

While academic track transition rates are a crucial metric for measuring educational achievement in the German context, our analysis would benefit from including additional outcome variables, such as standardized yearly test scores. Moreover, examining administrative health insurance data could yield more robust insights into the underlying mechanisms driving the positive impact of LEZs on (elementary) school student performance. Employing an expansive administrative data set would also facilitate the study of heterogeneous effects, e.g., whether it is predominantly children with preexisting respiratory conditions who derive the most significant benefit from LEZs. Finally, a relevant question left to answer concerns age-specific responses to the effects of pollution on human capital: is it the duration of exposure or the age at exposure that exerts a more significant influence?

Lastly, I focus on the most critical caregiver, the parents. In **Chapter 5**, we examine how *Pro Kind*, a parenting program involving home visits by trained nurses within *BRISE*, affects short-term maternal and child developmental, behavioral, and health outcomes. Additionally, we build a detailed micro cost database on program costs and analyze the different cost components. Our results do not provide evidence of the effects at this early stage of a child's life. The cost analysis reveals that *Pro Kind* is one of the cheaper programs among comparable programs in other countries.

It comes as no surprise that *Pro Kind* does not significantly impact child and maternal outcomes in the earliest stages of a child's life, given that evidence from another context suggests that effects typically require several years to materialize (e.g., Jungmann et al., 2015; Sandner, 2013, 2019; Sandner et al., 2018). Furthermore, significant data challenges were encountered in our study. With a mere 300 observations, our sample size is limited and further reduced by missing values in critical variables. Additionally, imperfect randomization within the trial results in disparities in socio-economic characteristics between treatment and control families. Thus, the current data is insufficient to estimate precise effects.

Despite our data challenges, our study provides a promising basis for future cost-effectiveness studies. At later measurement times and with larger sample sizes, the study setting allows us to derive more meaningful estimates of the program's effectiveness. Furthermore, later measurement times will also allow us to derive effect estimates for other ECEC programs within *BRISE*. Together with the detailed cost data for each program, comparing the programs allows for cost-effectiveness analysis. Cost-effectiveness studies are highly relevant for informing policy debate, as they can guide policymakers' decisions regarding alternative programs. For example, if *Pro Kind* has a positive impact on child and maternal outcomes, the relatively low costs of *Pro Kind* compared to other programs make the program an attractive option to support children from disadvantaged backgrounds, thereby leveling the playing field at an early stage in their lives.

Despite the challenges posed by our data, our study provides a promising basis for future cost-effectiveness analyses. With a larger sample size and later measurement times, the *BRISE* cost-effectiveness module is set to produce more meaningful estimates of the program's efficacy in the future. The close, frequent, and detailed monitoring of *BRISE* children and their families provides a wealth of information that exceeds usual survey data. In addition, the close collaboration with local policymakers makes *BRISE* a suitable model project for other municipalities seeking to better support socio-economically disadvantaged young families. For instance, if *Pro Kind* is found to have a positive impact on child and maternal outcomes, its relatively low cost compared to other programs makes it an attractive option for supporting underprivileged children.

In conclusion, this dissertation presents essential insights into the efficacy of various public policies in promoting human capital formation and mitigating educational disparities. In doing so, I contribute to pertinent policy debates on education, social policy, and the social-ecological transformation. Furthermore, the different chapters of this dissertation introduce promising avenues for future research.

Bibliography

Aguilar-Gomez, S., H. Dwyer, J. Graff Zivin, and M. Neidell (2022): "This is Air: The "Nonhealth" Effects of Air Pollution," *The Annual Review of Resource*, 14.

Aizer, A. (2004): "Home alone: Supervision after school and child behavior," *Journal of Public Economics*, 88, 1835–1848.

Akbarpour, M., A. Kapor, C. Neilson, W. van Dijk, and S. Zimmerman (2022): "Centralized School choice with unequal outside options," *Journal of Public Economics*, 210, 104644.

Akkaya, P. (2021): "Der gymnasiale Privatschulmarkt Deutschlands. Exploration der Charakteristika von Privatschulen mit gymnasialer Oberstufe sowie deren Wechselwirkungen mit sozialen und regionalen Ungleichheiten in Ost-und Westdeutschland," Ph.D. thesis, Otto-Friedrich-Universität Bamberg, Fakultät Sozial-und Wirtschaftswissenschaften.

Akkaya, P., M. Helbig, and M. Wrase (2019): "Voraussetzung sozialer Verantwortung-Privatschulfinanzierung in den deutschen Bundesländern: Darstellung und Vergleich der Finanzierungssysteme für allgemeinbildende Ersatzschulen in den 16 Ländern," *WZB Discussion Paper*.

Algan, Y., E. Beasley, F. Vitaro, R. E. Tremblay, et al. (2014): "The impact of non-cognitive skills training on academic and non-academic trajectories: From childhood to early adulthood," *Sciences Po Working Paper*.

Almlund, M., A. L. Duckworth, J. Heckman, and T. Kautz (2011): "Personality psychology and economics," in *Handbook of the Economics of Education*, Elsevier, vol. 4, 1–181.

Almond, D., L. Edlund, and M. Palme (2009): "Chernobyl's subclinical legacy: prenatal exposure to radioactive fallout and school outcomes in Sweden," *The Quarterly Journal of Economics*, 124, 1729–1772.

ANDRESEN, M. E. (2018): "Exploring marginal treatment effects: Flexible estimation using Stata," *The Stata Journal*, 18, 118–158.

ANGRIST, J., S. DYNARSKI, T. KANE, P. PATHAK, AND C. WALTERS (2010): "Inputs and impacts in charter schools: KIPP Lynn," *American Economic Review*, 100, 239–43.

ANGRIST, J. AND G. IMBENS (1995): "Identification and estimation of local average treatment effects," *Econometrica*, 62, 467–475.

ANGRIST, J. D., G. W. IMBENS, AND D. B. RUBIN (1996): "Identification of causal effects using instrumental variables," *Journal of the American Statistical Association*, 91, 444–455.

ANGRIST, J. D. AND A. B. KRUEGER (1991): "Does compulsory school attendance affect schooling and earnings?" *The Quarterly Journal of Economics*, 106, 979–1014.

ANGRIST, J. D., P. A. PATHAK, AND C. R. WALTERS (2013): "Explaining charter school effectiveness," *American Economic Journal: Applied Economics*, 5, 1–27.

ANGRIST, J. D. AND J.-S. PISCHKE (2010): "The credibility revolution in empirical economics: How better research design is taking the con out of econometrics," *Journal of Economic Perspectives*, 24, 3–30.

ANYAEGBU, G. (2010): "Using the OECD equivalence scale in taxes and benefits analysis," *Economic & Labour Market Review*, 4, 49–54.

ARCHSMITH, J., A. HEYES, AND S. SABERIAN (2018): "Air quality and error quantity: Pollution and performance in a high-skilled, quality-focused occupation," *Journal of the Association of Environmental and Resource Economists*, 5, 827–863.

ARGAW, B. A. AND P. A. PUHANI (2018): "Does class size matter for school tracking outcomes after elementary school? Quasi-experimental evidence using administrative panel data from Germany," *Economics of Education Review*, 65, 48–57.

ARMSTRONG, K., J. FRASER, M. DADDS, AND J. MORRIS (1999): "A randomized, controlled trial of nurse home visiting to vulnerable families with newborns," *Journal of Paediatrics and Child Health*, 35, 237–244.

BACH, M., J. KOEBE, AND F. PETER (2019): "Long Run Effects of Universal Childcare on Personality Traits," *DIW Berlin Discussion Paper*, 1815.

Baker, A. C., D. F. Larcker, and C. C. Wang (2022): "How much should we trust staggered difference-in-differences estimates?" *Journal of Financial Economics*, 144, 370–395.

Baker, M., J. Gruber, and K. Milligan (2008): "Universal child care, maternal labor supply, and family well-being," *Journal of Political Economy*, 116, 709–745.

——— (2015): "Non-cognitive deficits and young adult outcomes: The long-run impacts of a universal child care program," *National Bureau of Economic Research.*

——— (2019): "The long-run impacts of a universal child care program," *American Economic Journal: Economic Policy*, 11, 1–26.

Barlow, J., H. Davis, E. McIntosh, P. Jarrett, C. Mockford, and S. Stewart-Brown (2007): "Role of home visiting in improving parenting and health in families at risk of abuse and neglect: Results of a multicentre randomised controlled trial and economic evaluation," *Archives of Disease in Childhood*, 92, 229–233.

Barnes, J. H., T. J. Chatterton, and J. W. Longhurst (2019): "Emissions vs exposure: Increasing injustice from road traffic-related air pollution in the United Kingdom," *Transportation Research Part D: Transport and Environment*, 73, 56–66.

Barnett, W. S. (1985): "Benefit-cost analysis of the Perry Preschool Program and its policy implications," *Educational Evaluation and Policy Analysis*, 7, 333–342.

——— (2010): "Universal and targeted approaches to preschool education in the United States," *International Journal of Child Care and Education Policy*, 4, 1–12.

——— (2011): "Effectiveness of early educational intervention," *Science*, 333, 975–978.

Barnett, W. S. and L. N. Masse (2007): "Comparative benefit–cost analysis of the Abecedarian program and its policy implications," *Economics of Education Review*, 26, 113–125.

Barro, R. J. (2001): "Human capital and growth," *American Economic Review*, 91, 12–17.

Barschkett, M. and L. Schmitz (2020): "The cost-effectiveness in BRISE," Unpublished poster.

Bartik, T. J. (2015): "From preschool to prosperity: The economic payoff to early childhood education," .

BAUMEISTER, R. F. AND J. L. ALQUIST (2009): "Self-regulation as a limited resource: Strength model of control and depletion." in *Sydney Symposium of Social Psychology, 10, University of New South Wales, Sydney, Australia*, Psychology Press.

BAVARIAN MINISTRY FOR EDUCATION AND CULTURAL AFFAIRS (2010): "Der Übertritt auf einen Blick," Tech. rep., https://web.archive.org/web/20100615105141/http://www.km.bayern.de/km/schule/schularten/allgemein/uebertritt/, accessed: 2021-05-06.

BECKER, G. S. (1964): "Human capital: A theoretical and empirical analysis, with special reference to education," *National Bureau of Economic Research*.

——— (1994): "Human capital revisited," in *Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education, Third Edition*, The University of Chicago Press, 15–28.

BECKER, M., A. BROSE, G. CAMEHL, D. GRAEBER, M. HUEBENER, L. VON KEYSERLINGK, C. KREKEL, K. MAAZ, J. MARCUS, S. MARGARYAN, ET AL. (2019): *Nicht-monetäre Erträge von Bildung in den Bereichen Gesundheit, nicht-kognitive Fähigkeiten sowie gesellschaftliche und politische Partizipation*, 137, DIW Berlin: Politikberatung kompakt.

BECKER, S. O. (2016): "Using instrumental variables to establish causality," *IZA World of Labor*.

BEKKERS, R., N. D. DE GRAAF, ET AL. (2005): "Field of education and prosocial behavior," *Marktdag Sociologie*.

BELLENBERG, G. (2012): *Schulformwechsel in Deutschland: Durchlässigkeit und Selektion in den 16 Schulsystemen der Bundesländer innerhalb der Sekundarstufe I*, Bertelsmann-Stiftung.

BENOIT, K. (2011): "Linear regression models with logarithmic transformations," *London School of Economics, London*, 22, 23–36.

BENSON, J. B. AND M. M. HAITH (2010): *Language, memory, and cognition in infancy and early childhood*, Academic Press.

BERGER, E. M. AND C. K. SPIESS (2011): "Maternal life satisfaction and child outcomes: Are they related?" *Journal of Economic Psychology*, 32, 142–158.

Besharov, D., P. Germanis, C. Higney, D. Call, and P. Germans (2011): "Assessments of twenty-six early childhood evaluations," *College Park, MD: University of Maryland, School of Public Policy.*

Bharadwaj, P., M. Gibson, J. G. Zivin, and C. Neilson (2017): "Gray matters: Fetal pollution exposure and human capital formation," *Journal of the Association of Environmental and Resource Economists*, 4, 505–542.

Bishop, K. and L. Corkery (2017): *Designing cities with children and young people: beyond playgrounds and skate parks*, Taylor & Francis.

Björklund, A. and R. Moffitt (1987): "The estimation of wage gains and welfare gains in self-selection models," *The Review of Economics and Statistics*, 42–49.

Bjornson, C. L. and I. Mitchell (2000): "Gender differences in asthma in childhood and adolescence," *The Journal of Gender-specific Medicine*, 3, 57–61.

BKJ (2005): "Kulturelle Bildung und Ganztagsschulen: Rahmenbedingungen und Umsetzung von Kooperationen in den Ländern. Eine Ländersynopse. Stand 15.11. 2005," .

Black, S. E., A. Bütikofer, P. J. Devereux, and K. G. Salvanes (2019): "This Is Only a Test? Long-Run and Intergenerational Impacts of Prenatal Exposure to Radioactive Fallout," *The Review of Economics and Statistics*, 101, 531–546.

Blau, D. and J. Currie (2006): "Pre-school, day care, and after-school care: who's minding the kids?" *Handbook of the Economics of Education*, 2, 1163–1278.

Blossfeld, H.-P. and J. Von Maurice (2011): "Bildung als lebenslanger Prozess," *Zeitschrift für Erziehungswissenschaft*, 14, 19–34.

BMBF (2009): "Investitionsprogramm Zukunft Bildung und Betreuung. Ganztagsschulen. Zeit für mehr." Tech. rep., http://ganztag.nepomucenum.de/offen/praesentation/imagbroschuere$_bmbf.pdf$, *accessed* : $2019-08-12$.

——— (2021): "Gesetz zur ganztätgigen Förderung von Kindern im Grundschulalter (Ganztagsförderungsgesetz - GaFöG)," Tech. rep., https://www.bmfsfj.de/bmfsfj/service/gesetze/gesetz-rechtsanspruch-ganztagsbetreuung-grundschulen-178966, accessed: 2021-08-12.

Borghans, L., A. L. Duckworth, J. Heckman, and B. Ter Weel (2008): "The economics and psychology of personality traits," *Journal of Human Resources*, 43, 972–1059.

Borusyak, K., X. Jaravel, and J. Spiess (2022): "Revisiting Event Study Designs: Robust and Efficient Estimation," *Available at SSRN 2826228.*

Bos, W. (2003): *Erste Ergebnisse aus IGLU*, Waxmann Verlag.

Bound, J., D. A. Jaeger, and R. M. Baker (1995): "Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variable is weak," *Journal of the American Statistical Association*, 90, 443–450.

Bradbury, B., M. Corak, J. Waldfogel, and E. Washbrook (2015): *Too many children left behind: The US achievement gap in comparative perspective*, Russell Sage Foundation.

Braun, S. T. and J. Stuhler (2018): "The transmission of inequality across multiple generations: testing recent theories with evidence from Germany," *The Economic Journal*, 128, 576–611.

Breidenbach, P. and L. Eilers (2018): "RWI-GEO-GRID: Socio-economic Data on Grid Level," *Jahrbücher für Nationalökonomie und Statistik*, 238, 609–616.

Breuing, K. (2014): *Schulwahl und Schulwettbewerb im dualen System: Zur Aufhebung der Berufsschulbezirke in Nordrhein-Westfalen*, Springer-Verlag.

BRISE Consortium (2013): "Bremer Initiative zur Stärkung frühkindlicher Entwicklung (Bremen Initiative on Early Childhood Development). Project Proposal (unpublished)," .

——— (2022): "BRISE - Bremer Initiative zur Stärkung frühkindlicher Entwicklung," `https://www.brise-bremen.de/#top`, accessed: 2022-12-12.

Brosius-Gersdorf, Frauke (2017): "Das missverstandene Sonderungsverbot für private Ersatzschulen (Art. 7 Abs. 4 Satz 3 Halbs. 2 GG)," *Rechtsgutachten im Auftrag der Friedrich-Naumann-Stiftung für die Freiheit, Hannover, Juli 2017, S. 77*, 83.

Brunello, G. and M. Schlotter (2011): "Non-cognitive skills and personality traits: Labour market relevance and their development in education & training systems," *IZA Discussion paper.*

Buckingham, J., K. Wheldall, and R. Beaman-Wheldall (2013): "Why poor children are more likely to become poor readers: The school years," *Australian Journal of Education*, 57, 190–213.

Burke, M. A. and T. R. Sass (2013): "Classroom peer effects and student achievement," *Journal of Labor Economics*, 31, 51–82.

Callaway, B. and P. H. C. Sant'Anna (2021): "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, 225, 200–230.

Camehl, G. F., C. K. Spiess, and K. Hahlweg (2020): "The effects of a parenting program on maternal well-being: Evidence from a Randomized Controlled Trial," *The BE Journal of Economic Analysis & Policy*, 20.

Campbell, F. A. and C. T. Ramey (1991): "The Carolina Abecedarian Project," *ERIC*.

Cannon, J. S., M. R. Kilburn, L. A. Karoly, T. Mattox, A. N. Muchow, and M. Buenaventura (2018): "Investing early: Taking stock of outcomes and economic returns from early childhood programs," *Rand Health Quarterly*, 7.

Card, D. (1993): "Using geographic variation in college proximity to estimate the return to schooling," *NBER Working Paper 4483*.

——— (1994): "Earnings, schooling, and ability revisited," *National Bureau of Economic Research Cambridge*.

——— (1999): "The causal effect of education on earnings," *Handbook of Labor Economics*, 3, 1801–1863.

Carneiro, P. and R. Ginja (2014): "Long-term impacts of compensatory preschool on health and behavior: Evidence from Head Start," *American Economic Journal: Economic Policy*, 6, 135–73.

——— (2016): "Partial insurance and investments in children," *The Economic Journal*, 126, F66–F95.

Carneiro, P., J. Heckman, and E. Vytlacil (2011): "Estimating marginal returns to education," *American Economic Review*, 101, 2754–81.

Carneiro, P., M. Lokshin, and N. Umapathi (2017): "Average and marginal returns to upper secondary schooling in Indonesia," *Journal of Applied Econometrics*, 32, 16–36.

Cattan, S., G. Conti, C. Farquharson, R. Ginja, and M. Pecher (2021): "The health effects of universal early childhood interventions: Evidence from Sure Start," *CEPR Discussion Paper*, DP16730.

Cawley, J. and C. J. Ruhm (2011): "The economics of risky health behaviors," in *Handbook of Health Economics*, Elsevier, vol. 2, 95–199.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019): "The effect of minimum wages on low-wage jobs," *The Quarterly Journal of Economics*, 134, 1405–1454.

Chakrabarti, R. and J. Roy (2010): "The economics of parental choice," *Economics of Education*, 3, 336–342.

Chay, K. Y. and M. Greenstone (2003): "The Impact of Air Pollution on Infant Mortality: Evidence from Geographic Variation in Pollution Shocks Induced by a Recession," *The Quarterly Journal of Economics*, 118, 1121–1167.

Chen, S., C. Guo, and X. Huang (2018): "Air Pollution, Student Health, and School Absences: Evidence from China," *Journal of Environmental Economics and Management*, 92, 465–497.

Cherney, I. D. and K. London (2006): "Gender-linked differences in the toys, television shows, computer games, and outdoor activities of 5-to 13-year-old children," *Sex roles*, 54, 717–726.

Cho, H. (2022): "Does Particulate Matter Affect Cognitive Performance?: Evidence from the City of Seoul," *American Journal of Health Economics*, 8, 301–322.

Clotfelter, C. T. (1976): "School desegregation," tipping," and private school enrollment," *Journal of Human Resources*, 28–50.

Cohen, J. (2013): *Statistical power analysis for the behavioral sciences*, Academic press.

Coneus, K. and C. K. Spiess (2012): "Pollution exposure and child health: Evidence for infants and toddlers in Germany," *Journal of Health Economics*, 31, 180–196.

Conti, G., J. Heckman, and S. Urzua (2010): "The education-health gradient," *American Economic Review*, 100, 234–38.

Cornelissen, T., C. Dustmann, A. Raute, and U. Schönberg (2016): "From LATE to MTE: Alternative methods for the evaluation of policy interventions," *Labour Economics*, 41, 47–60.

——— (2018): "Who benefits from universal child care? Estimating marginal returns to early child care attendance," *Journal of Political Economy*, 126, 2356–2409.

Cox, J. L., G. Chapman, D. Murray, and P. Jones (1996): "Validation of the Edinburgh Postnatal Depression Scale (EPDS) in non-postnatal women," *Journal of Affective Disorders*, 39, 185–189.

Cremer, W. (2019): "Die Gründungs-und Betätigungsfreiheit von Ersatzschulen im Kontext ihrer verfassungsfundierten sozialstaatlichen Imprägnierung," *Informationsschrift Recht und Bildung des Instituts für Bildungsrecht und Bildungsforschung e. V., Ausgabe*, 1, 3–17.

Cunha, F. and J. Heckman (2007): "The technology of skill formation," *American Economic Review*, 97, 31–47.

Cunha, F., J. J. Heckman, and S. M. Schennach (2010): "Estimating the technology of cognitive and noncognitive skill formation," *Econometrica*, 78, 883–931.

Currie, J. (2001): "Early childhood education programs," *Journal of Economic Perspectives*, 15, 213–238.

Currie, J. and D. Almond (2011): "Human capital development before age five," in *Handbook of Labor Economics*, Elsevier, vol. 4, 1315–1486.

Currie, J., E. A. Hanushek, E. M. Kahn, M. Neidell, and S. G. Rivkin (2009): "Does Pollution increase School Absences?" *The Review of Economics and Statistics*, 91, 682–694.

Currie, J. and E. Moretti (2003): "Mother's education and the intergenerational transmission of human capital: Evidence from college openings," *The Quarterly journal of economics*, 118, 1495–1532.

Cutler, D. M. and A. Lleras-Muney (2010): "Understanding differences in health behaviors by education," *Journal of Health Economics*, 29, 1–28.

Dahlen, H. M. (2016): "The impact of maternal depression on child academic and socioemotional outcomes," *Economics of Education Review*, 52, 77–90.

Datta Gupta, N. and M. Simonsen (2010): "Non-cognitive child outcomes and universal high quality child care," *Journal of Public Economics*, 94, 30–43.

Davis, L. W. (2008): "The effect of driving restrictions on air quality in Mexico City," *Journal of Political Economy*, 116, 38–81.

De Chaisemartin, C. (2017): "Tolerating defiance? Local average treatment effects without monotonicity," *Quantitative Economics*, 8, 367–396.

de Chaisemartin, C. and X. D'Haultfœuille (2020a): "Difference-in-Differences Estimators of Intertemporal Treatment Effects," SSRN Scholarly Paper ID 3731856, Social Science Research Network, Rochester, NY.

——— (2020b): "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, 110, 2964–96.

de Jong, E. (2016): "A simple procedure for estimating the effect size in IV regressions," *Available at SSRN 2761526*.

De Walque, D. (2007): "Does education affect smoking behaviors?: Evidence using the Vietnam draft as an instrument for college education," *Journal of Health Economics*, 26, 877–895.

Dee, T. S. (2004): "Are there civic returns to education?" *Journal of public economics*, 88, 1697–1720.

Dehos, F. T. and M. Paul (2021): "The effects of after-school programs on maternal employment," *Journal of Human Resources*, 0120–10651R1.

Deming, D. J. (2017): "The growing importance of social skills in the labor market," *The Quarterly Journal of Economics*, 132, 1593–1640.

Denning, J. T., R. Murphy, and F. Weinhardt (2018): "Class rank and long-run outcomes," *The Review of Economics and Statistics*, 1–45.

Deryugina, T. and D. Molitor (2021): "The causal effects of place on health and longevity," *Journal of Economic Perspectives*, 35, 147–170.

Deshpande, M. and Y. Li (2019): "Who is screened out? Application costs and the targeting of disability programs," *American Economic Journal: Economic Policy*, 11, 213–48.

Deutsche Bischofskonferenz (2022): "Katholische Schulen," `https://www.dbk.de/katholische-kirche/aufgaben/schulen`, accessed: 2022-09-30.

Deutscher Bundestag (2007): "Privatschulen im voruniversitären (allgemeinbildenden) Bildungsbereich in Deutschland," *Wissenschaftliche Dienste*.

Diener, E. and M. Y. Chan (2011): "Happy people live longer: Subjective well-being contributes to health and longevity," *Applied Psychology: Health and Well-Being*, 3, 1–43.

DiMaria, C. H., C. Peroni, and F. Sarracino (2020): "Happiness matters: Productivity gains from subjective well-being," *Journal of Happiness Studies*, 21, 139–160.

DJI (2008): "Zahlenspiegel 2007–Kindertagesbetreuung im Spiegel der Statistik," *Deutsches Jugendinstitut.*

Do, C. (2004): "The effects of local colleges on the quality of college attended," *Economics of Education Review*, 23, 249–257.

Doyle, O., N. Fitzpatrick, J. Lovett, and C. Rawdon (2015): "Early intervention and child physical health: Evidence from a Dublin-based randomized controlled trial," *Economics & Human Biology*, 19, 224–245.

Doyle, W. R. and B. T. Skinner (2017): "Does postsecondary education result in civic benefits?" *The Journal of Higher Education*, 88, 863–893.

Duckworth, A. L., C. Peterson, M. D. Matthews, and D. R. Kelly (2007): "Grit: perseverance and passion for long-term goals." *Journal of Personality and Social Psychology*, 92, 1087.

DuMont, K., K. Kirkland, S. Mitchell-Herzfeld, S. Ehrhard-Dietzel, M. L. Rodriguez, E. Lee, C. Layne, and R. Greene (2010): "A randomized trial of Healthy Families New York (HFNY): Does home visiting prevent child maltreatment," *Rensselaer, NY: New York State Office of Children & Family Services and Albany, NY: The University of Albany, State University of New York.*

Duncan, G. J., J. Brooks-Gunn, and P. K. Klebanov (1994): "Economic deprivation and early childhood development," *Child development*, 65, 296–318.

Dustmann, C. (2004): "Parental background, secondary school track choice, and wages," *Oxford Economic Papers*, 56, 209–230.

Dustmann, C., P. A. Puhani, and U. Schönberg (2017): "The long-term effects of early track choice," *The Economic Journal*, 127, 1348–1380.

Ebenstein, A., V. Lavy, and S. Roth (2016): "The long-run economic consequences of high-stakes examinations: Evidence from transitory variation in pollution," *American Economic Journal: Applied Economics*, 8, 36–65.

Eckenrode, J., M. I. Campa, P. A. Morris, C. R. Henderson Jr, K. E. Bolger, H. Kitzman, and D. L. Olds (2017): "The prevention of child maltreatment through the Nurse Family Partnership program: Mediating effects in a long-term follow-up study," *Child Maltreatment*, 22, 92–99.

Ellison, R. B., S. P. Greaves, and D. A. Hensher (2013): "Five years of London's low emission zone: Effects on vehicle fleet composition and air quality," *Transportation Research Part D: Transport and Environment*, 23, 25–33.

Elsner, B., I. E. Isphording, and U. Zölitz (2021): "Achievement rank affects performance and major choices in college," *The Economic Journal*, 131, 3182–3206.

Environmental Protection Agency (2016): "Integrated Science Assessment for Oxides of Nitrogen-Health Criteria," Tech. rep.

Erikson, R. (1996): "Explaining Class Inequality in Education: the Swedish Test Case in Erikson, R and Jonsson, JO, eds," *Can Education Be Equalized? The Swedish Case in Comparative Perspective.*

European Commission (2018): "The Organisation of School Time in Europe. Primary and General Secondary Education 2018/19." *Publications Office of the European Union.*

Fairlie, R. W. and A. M. Resch (2002): "Is there "white flight" into private schools? Evidence from the National Educational Longitudinal Survey," *Review of Economics and Statistics*, 84, 21–33.

Feinstein, L. (2003): "Inequality in the early cognitive development of British children in the 1970 cohort," *Economica*, 70, 73–97.

Felfe, C. and R. Lalive (2018): "Does early child care affect children's development?" *Journal of Public Economics*, 159, 33–53.

Felfe, C. and L. Zierow (2014): "After-school center-based care and children's development," *The BE Journal of Economic Analysis & Policy*, 4, 1299–1336.

——— (2018): "From dawn till dusk: Implications of full-day care for children's development," *Labour Economics*, 55, 259–281.

Fernald, A., V. A. Marchman, and A. Weisleder (2013): "SES differences in language processing skill and vocabulary are evident at 18 months," *Developmental Science*, 16, 234–248.

Fischer, M., M. Karlsson, and T. Nilsson (2013): "Effects of compulsory schooling on mortality: Evidence from Sweden," *International Journal of Environmental Research and Public Health*, 10, 3596–3618.

Fischer, N., H. P. Kuhn, and I. Züchner (2011): "Entwicklung von Sozialverhalten in der Ganztagsschule. Wirkungen der Ganztagsteilnahme und der Angebotsqualität," *Ganztagsschule: Entwicklung, Qualität, Wirkungen: Längsschnittliche Befunde der Studie zur Entwicklung von Ganztagsschulen (StEG)*, 246–266.

Fletcher, J. M. (2013): "The effects of personality traits on adult labor market outcomes: Evidence from siblings," *Journal of Economic Behavior & Organization*, 89, 122–135.

Florens, J.-P., J. J. Heckman, C. Meghir, and E. Vytlacil (2008): "Identification of treatment effects using control functions in models with continuous, endogenous treatment and heterogeneous effects," *Econometrica*, 76, 1191–1206.

Frölich, M. and M. Huber (2017): "Direct and indirect treatment effects–causal chains and mediation analysis with instrumental variables," *Journal of the Royal Statistical Society. Series B (Statistical Methodology)*, 1645–1666.

Gambaro, L., J. Marcus, and F. Peter (2019): "School entry, afternoon care, and mothers' labour supply," *Empirical Economics*, 57, 769–803.

Ganzeboom, H. B. and D. J. Treiman (1996): "Internationally comparable measures of occupational status for the 1988 International Standard Classification of Occupations," *Social Science Research*, 25, 201–239.

Gehrsitz, M. (2017): "The effect of low emission zones on air pollution and infant health," *Journal of Environmental Economics and Management*, 83, 121–144.

Glazner, J., J. Bondy, D. Luckey, and D. Olds (2004): "Effect of the Nurse Family Partnership on Government Expenditures for Vulnerable First-Time Mothers and Their Children in Elmira, New York, Memphis, Tennessee, and Denver, Colorado (# 90XP0017): Final Report to the Administration for Children and Families," *Washington, DC: US Deptartment of Health and Human Services.*

Goebel, J., M. M. Grabka, S. Liebig, M. Kroh, D. Richter, C. Schröder, and J. Schupp (2019): "The German socio-economic panel (SOEP)," *Jahrbücher für Nationalökonomie und Statistik*, 239, 345–360.

Goldin, C. D. (2016): "Human capital," *Springer Verlag.*

GOODMAN, R. (1997): "The Strengths and Difficulties Questionnaire: A research note," *Journal of Child Psychology and Psychiatry*, 38, 581–586.

GOODMAN, R., H. MELTZER, AND V. BAILEY (1998): "The Strengths and Difficulties Questionnaire: a pilot study on the validity of the self-report version," *European child & adolescent psychiatry*, 7, 125–130.

GOODMAN-BACON, A. (2021): "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 225, 254–277.

GOODMAN-BACON, A., T. GOLDRING, AND A. NICHOLS (2019): "BACONDECOMP: Stata module to perform a Bacon decomposition of difference-in-differences estimation," Tech. rep., https://EconPapers.repec.org/RePEc:boc:bocode:s458676, accessed: 2022-12-13.

GÖRLITZ, K., C. K. SPIESS, AND E. ZIEGE (2018): "Fast jedes zehnte Kind geht auf eine Privatschule: Nutzung hängt insbesondere in Ostdeutschland zunehmend vom Einkommen der Eltern ab," *DIW Wochenbericht*, 85, 1103–1111.

GOUGH, H. G. (1979): "A creative personality scale for the adjective check list." *Journal of Personality and Social Psychology*, 37, 1398.

GOYETTE, K., J. ICELAND, AND E. WEININGER (2014): "Moving for the kids: Examining the influence of children on white residential segregation," *City & Community*, 13, 158–178.

GRAFF ZIVIN, J. AND M. NEIDELL (2012): "The impact of pollution on worker productivity," *American Economic Review*, 102, 3652–73.

GROSSMAN, M. (2006): "Education and nonmarket outcomes," *Handbook of the Economics of Education*, 1, 577–633.

GUNDERSON, M. AND P. OREOPOLOUS (2020): "Returns to education in developed countries," in *The Economics of Education*, Elsevier, 39–51.

HAINMUELLER, J. (2012): "Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies," *Political Analysis*, 20, 25–46.

HAJAT, A., C. HSIA, AND M. S. O'NEILL (2015): "Socioeconomic disparities and air pollution exposure: a global review," *Current Environmental Health Reports*, 2, 440–450.

Hanushek, E. A. and D. D. Kimko (2000): "Schooling, labor-force quality, and the growth of nations," *American Economic Review*, 90, 1184–1208.

Haveman, R. and B. Wolfe (1995): "The determinants of children's attainments: A review of methods and findings," *Journal of economic literature*, 33, 1829–1878.

Havnes, T. and M. Mogstad (2011): "No child left behind: Subsidized child care and children's long-run outcomes," *American Economic Journal: Economic Policy*, 3, 97–129.

Heckman, J., R. Pinto, and P. Savelyev (2013a): "Understanding the mechanisms through which an influential early childhood program boosted adult outcomes," *American Economic Review*, 103, 2052–86.

——— (2013b): "Understanding the mechanisms through which an influential early childhood program boosted adult outcomes," *American Economic Review*, 103, 2052–86.

Heckman, J., S. Urzua, and E. Vytlacil (2006a): "Understanding instrumental variables in models with essential heterogeneity," *The Review of Economics and Statistics*, 88, 389–432.

Heckman, J. and E. Vytlacil (1999): "Local instrumental variables and latent variable models for identifying and bounding treatment effects," *Proceedings of the national Academy of Sciences*, 96, 4730–4734.

——— (2005): "Structural equations, treatment effects, and econometric policy evaluation 1," *Econometrica*, 73, 669–738.

Heckman, J. J. (2000): "Policies to foster human capital," *Research in economics*, 54, 3–56.

——— (2006): "Skill formation and the economics of investing in disadvantaged children," *Science*, 312, 1900–1902.

——— (2007): "The economics, technology, and neuroscience of human capability formation," *Proceedings of the National Academy of Sciences*, 104, 13250–13255.

Heckman, J. J. and P. Carneiro (2003): "Human capital policy," *National Bureau of Economic Research Cambridge.*

Heckman, J. J., M. L. Holland, K. K. Makino, R. Pinto, and M. Rosales-Rueda (2017): "An analysis of the memphis nurse-family partnership program," *National Bureau of Economic Research Working Paper*, w23610.

Heckman, J. J., J. E. Humphries, and G. Veramendi (2018): "Returns to education: The causal effects of education on earnings, health, and smoking," *Journal of Political Economy*, 126, S197–S246.

Heckman, J. J. and D. V. Masterov (2007): "The productivity argument for investing in young children," *National Bureau of Economic Research Cambridge, Mass., USA*.

Heckman, J. J., S. H. Moon, R. Pinto, P. A. Savelyev, and A. Yavitz (2010): "The rate of return to the HighScope Perry Preschool Program," *Journal of Public Economics*, 94, 114–128.

Heckman, J. J. and S. Mosso (2014): "The economics of human development and social mobility," *National Bureau of Economic Research Working Paper*, w19925.

Heckman, J. J., J. Stixrud, and S. Urzua (2006b): "The effects of cognitive and noncognitive abilities on labor market outcomes and social behavior," *Journal of Labor economics*, 24, 411–482.

Heckman, J. J. and E. Vytlacil (2001): "Policy-relevant treatment effects," *American Economic Review*, 91, 107–111.

Heineck, G. and S. Anger (2010): "The returns to cognitive abilities and personality traits in Germany," *Labour Economics*, 17, 535–546.

Heissel, J. A., C. Persico, and D. Simon (2022): "Does Pollution Drive Achievement? The Effect of Traffic Pollution on Academic Performance," *Journal of Human Resources*, 57, 747–776.

Helbig, M., L. Frank, S. Huber, K. Rompczyk, and K. Salomo (2020): "Zweiter Sozialstrukturatlas für den Freistaat Thüringen mit der Fokussierung auf „Armut und Armutsprävention in Thüringen ","* Thüringer Ministerium für Arbeit, Soziales, Gesundheit, Frauen und Familie*.

Helbig, M. and S. Jähnen (2018): "Wie brüchig ist die soziale Architektur unserer Städte? Trends und Analysen der Segregation in 74 deutschen Städten," Tech. rep., WZB Discussion Paper.

Helbig, M., S. Jähnen, and A. Marczuk (2017a): "Eine Frage des Wohnorts," *Zeitschrift für Soziologie*, 46, 55–70.

Helbig, M., M. Konrad, and R. Nikolai (2018): "Entwicklung von Schulen in Ostdeutschland 1992–2015," *Berlin: Wissenschaftszentrum Berlin für Sozialforschung*.

Helbig, M. and T. Mayer (2023): "Soziale Ungleichheiten beim Privatschulzugang. Die Wahl einer privaten Grundschule aus Elternsicht am Beispiel einer deutschen Großstadt," *Journal of Educational Research online*.

Helbig, M. and R. Nikolai (2015): *Die Unvergleichbaren: Der Wandel der Schulsysteme in den deutschen Bundesländern seit 1949*, Julius Klinkhardt.

Helbig, M., R. Nikolai, and M. Wrase (2017b): "Privatschulen und die soziale Frage: Wirkung rechtlicher Vorgaben zum Sonderungsverbot in den Bundesländern," *Leviathan*, 357–380.

Helbig, M. and M. Wrase (2017): "Übersicht über die Vorgaben zur Einhaltung des Sonderungsverbots in den Bundesländern: Aktualisierte und ergänzte Fassung auf der Grundlage der in NVwZ 2016 entwickelten Kriterien," *WZB Discussion Paper*.

Helbig, Marcel, Schmitz, Laura and Weinhardt, Felix (2022): "Selbst wenn Privatschulen in der Nähe sind: Sozial benachteiligte Schüler*innen sind dort kaum vertreten," *DIW Wochenbericht*, 683–690.

Hendrik, J. and S. Kerstin (2011): "Why Young Boys Stumble: Early Tracking, Age and Gender Bias in the German School System," *German Economic Review*, 12, 371–394.

Hille, A. and J. Schupp (2015): "How learning a musical instrument affects the development of skills," *Economics of Education Review*, 44, 56–82.

Hoxby, C. M. (2003): "School choice and school productivity. Could school choice be a tide that lifts all boats?" in *The Economics of School Choice*, University of Chicago Press, 287–342.

IAW (2018): "Armutspolitik in Bremen," Tech. rep., Institut Arbeit und Wirtschaft, `https://www.soziales.bremen.de/soziales/armuts-und-reichtumsberichte-70849`, accessed: 2022-12-13.

Isen, A., M. Rossin-Slater, and W. R. Walker (2017): "Every Breath You Take—Every Dollar You'll Make: The Long-Term Consequences of the Clean Air Act of 1970," *Journal of Political Economy*, 125, 848–902.

Jähnen, S. and M. Helbig (2022): "The dynamics of socio-economic segregation: What role do private schools play?" *Urban Studies*, 00420980221119385.

Janke, K. (2014): "Air pollution, avoidance behaviour and children's respiratory health: Evidence from England," *Journal of Health Economics*, 38, 23–42.

Javad, S. (2017): "Stadtkinder. Städte in Deutschland werden immer mehr zum Lebensraum für Familien," *Berlin: Friedrich Ebert Stiftung (FES).*

Jayachandran, S. (2009): "Air quality and early-life mortality evidence from Indonesia's wildfires," *Journal of Human resources*, 44, 916–954.

Jepsen, C. and M. Montgomery (2009): "Miles to go before I learn: The effect of travel distance on the mature person's choice of a community college," *Journal of uUrban Economics*, 65, 64–73.

Jerrett, M., R. T. Burnett, P. Kanaroglou, J. Eyles, N. Finkelstein, C. Giovis, and J. R. Brook (2001): "A GIS–environmental justice analysis of particulate air pollution in Hamilton, Canada," *Environment and Planning A*, 33, 955–973.

Jungbauer-Gans, M., H. Lohmann, and C. Katharina Spiess (2012): "Bildungsungleichheiten und Privatschulen in Deutschland," *Soziologische Bildungsforschung*, 64–85.

Jungmann, T., T. Brand, V. Dähne, P. Herrmann, H. Günay, M. Sandner, and S. Sierau (2015): "Comprehensive evaluation of the Pro Kind home visiting program: A summary of results," *Mental Health & Prevention*, 3, 89–97.

Kalil, A. (2015): "Inequality begins at home: The role of parenting in the diverging destinies of rich and poor children," *Families in an era of increasing inequality: Diverging destinies*, 63–82.

Kamhöfer, D. A., H. Schmitz, and M. Westphal (2019): "Heterogeneity in marginal non-monetary returns to higher education," *Journal of the European Economic Association*, 17, 205–244.

Kampa, M. and E. Castanas (2008): "Human health effects of air pollution," *Environmental Pollution*, 151, 362–367.

Karoly, L. A. (2012): "Toward standardization of benefit-cost analysis of early childhood interventions," *Journal of Benefit-Cost Analysis*, 3, 1–45.

——— (2017): "Investing in the Early Years," Tech. rep., Rand Cooperation.

Karoly, L. A., P. W. Greenwood, S. S. Everingham, J. Houbé, and M. R. Kilburn (1998): *Investing in our children: What we know and don't know about the costs and benefits of early childhood interventions*, Rand Corporation.

Karoly, L. A., M. R. Kilburn, and J. S. Cannon (2006): *Early childhood interventions: Proven results, future promise*, Rand Corporation.

Kaufmann, K. M. (2014): "Understanding the income gradient in college attendance in Mexico: The role of heterogeneity in expected returns," *Quantitative Economics*, 5, 583–630.

Kautz, T., J. Heckman, R. Diris, B. Ter Weel, and L. Borghans (2014): "Fostering and measuring skills: Improving cognitive and non-cognitive skills to promote lifetime success," *National Bureau of Economic Research Working Paper*, w20749.

Kettunen, J. (1997): "Education and unemployment duration," *Economics of Education Review*, 16, 163–170.

Kim, J. H., W. Schulz, T. Zimmermann, and K. Hahlweg (2018): "Parent–child interactions and child outcomes: Evidence from randomized intervention," *Labour Economics*, 54, 152–171.

Klauber, H., F. Holub, N. Koch, N. Pestel, N. Ritter, and A. Rohlf (2021): "Killing Prescriptions Softly: Low Emission Zones and Child Health from Birth to School," IZA Discussion Papers 14376, Institute of Labor Economics (IZA).

Klebanov, P. K., J. Brooks-Gunn, and M. C. McCormick (2001): "Maternal coping strategies and emotional distress: Results of an early intervention program for low birth weight young children." *Developmental Psychology*, 37, 654.

Klemm, K., L. Hoffmann, K. Maaz, and P. Stanat (2018): "Privatschulen in Deutschland," *Trends und Leistungsvergleiche. Berlin: Friedrich-Ebert-Stiftung.*

Klemm, K. and D. Zorn (2017): *Gute Ganztagsschule für alle: Kosten für den Ausbau eines qualitätsvollen Ganztagsschulsystems in Deutschland bis 2030*, Bertelsmann Stiftung.

KMK (2014): "The education system in the Federal Republic of Germany 2012/13," *Eurydice National Dossier*.

KMK (2021): "Allgemeinbildende Schulen in Ganztagsform in den Ländern in der Bundesrepublik Deutschland – Statistik 2011 bis 2019," https://www.kmk.org/dokumentation-statistik/statistik/schulstatistik/allgemeinbildende-schulen-in-ganztagsform.html, accessed: 2021-08-12.

Knittel, C. R., D. L. Miller, and N. J. Sanders (2016): "Caution, drivers! Children present: Traffic, pollution, and infant health," *Review of Economics and Statistics*, 98, 350–366.

Koolmann, S., L. Petersen, and P. Ehrler (2018): *Waldorf-Eltern in Deutschland: Status, Motive, Einstellungen, Zukunftsideen*, Beltz Juventa.

Köppe, S. (2012): "Wahlfreiheit und Nutzerrollen im deutschen Bildungssystem," *WSI-Mitteilungen*, 65, 206–215.

Korpi, W. and J. Palme (1998): "The paradox of redistribution and strategies of equality: Welfare state institutions, inequality, and poverty in the Western countries," *American Sociological Review*, 661–687.

Kosse, F., T. Deckers, P. Pinger, H. Schildberg-Hörisch, and A. Falk (2020): "The formation of prosociality: causal evidence on the role of social environment," *Journal of Political Economy*, 128, 434–467.

Kraul, M. (2017): *Pädagogischer Anspruch und Soziale Distinktion*, Springer.

Kristen, C. (2003): *School choice and ethnic school segregation: Primary school selection in Germany*, Waxmann Verlag.

Kristen, C. and J. Dollmann (2010): "Sekundäre Effekte der ethnischen Herkunft: Kinder aus türkischen Familien am ersten Bildungsübergang," in *Vom Kindergarten bis zur Hochschule*, Springer, 117–144.

Kroh, M., S. Kühne, J. Jacobsen, M. Siegert, and R. Siegers (2017): "Sampling, nonresponse, and integrated weighting of the 2016 IAB-BAMF-SOEP Survey of Refugees (M3/M4)," Tech. rep., SOEP Survey Papers.

Kuehnle, D. and M. Oberfichtner (2017): "Does early child care attendance influence children's cognitive and non-cognitive skill development?" *IZA Discussion Paper*.

——— (2020): "Does starting universal childcare earlier influence children's skill development?" *Demography*, 57, 61–98.

Künn, S., J. Palacios, and N. Pestel (forthcoming): "Indoor Air Quality and Strategic Decision-Making," *Management Science*.

Lang, F. R., D. John, O. Lüdtke, J. Schupp, and G. G. Wagner (2011): "Short assessment of the Big Five: Robust across survey methods except telephone interviewing," *Behavior Research Methods*, 43, 548–567.

LANKFORD, H. AND J. WYCKOFF (1992): "Primary and secondary school choice among public and religious alternatives," *Economics of Education Review*, 11, 317–337.

LAVY, V., A. EBENSTEIN, AND S. ROTH (2014): "The impact of short term exposure to ambient air pollution on cognitive performance and human capital formation," Tech. rep., National Bureau of Economic Research.

LESEMAN, P. P. AND P. L. SLOT (2020): "Universal versus targeted approaches to prevent early education gaps. The Netherlands as case in point," *Zeitschrift für Erziehungswissenschaft*, 23, 485–507.

LEVIN, H. M. AND P. J. MCEWAN (2000): *Cost-effectiveness analysis: Methods and applications*, vol. 4, Sage.

LEVINE, D., A. PACE, R. LUO, K. HIRSH-PASEK, R. M. GOLINKOFF, J. DE VILLIERS, A. IGLESIAS, AND M. S. WILSON (2020): "Evaluating socioeconomic gaps in preschoolers' vocabulary, syntax and language process skills with the Quick Interactive Language Screener (QUILS)," *Early Childhood Research Quarterly*, 50, 114–128.

LEVINE, P. B. AND D. J. ZIMMERMAN (2010): *Targeting investments in children: Fighting poverty when resources are limited*, University of Chicago Press.

LIKERT, R. (1932): "A technique for the measurement of attitudes." *Archives of Psychology*, 22140.

LINDSAY, G., S. STRAND, AND H. DAVIS (2011): "A comparison of the effectiveness of three parenting programmes in improving parenting skills, parent mental-well being and children's behaviour when implemented on a large scale in community settings in 18 English local authorities: the parenting early intervention pathfinder (PEIP)," *BMC Public Health*, 11, 1–13.

LIPFERT, F. W. (2004): "Air pollution and poverty: does the sword cut both ways?" *Journal of Epidemiology & Community Health*, 58, 2–3.

LLERAS-MUNEY, A. (2005): "The relationship between education and adult mortality in the United States," *The Review of Economic Studies*, 72, 189–221.

LOCHNER, L. (2011): "Non-production benefits of education: Crime, health, and good citizenship," *National Bureau of Economic Research.*

LOHMANN, H., C. K. SPIESS, AND C. FELDHAUS (2009): "Der Trend zur Privatschule geht an bildungsfernen Eltern vorbei," *DIW Wochenbericht*, 76, 640–646.

LOSSEN, K., K. TILLMANN, H. G. HOLTAPPELS, W. ROLLETT, AND J. HANNEMANN (2016): "Entwicklung der naturwissenschaftlichen Kompetenzen und des sachunterrichtsbezogenen Selbstkonzepts bei Schüler/-innen in Ganztagsgrundschulen. Ergebnisse der Längsschnittstudie StEG-P zu Effekten der Schülerteilnahme und der Angebotsqualität," *Zeitschrift für Pädagogik*, 62, 760–779.

LOVE, J. M., E. E. KISKER, C. ROSS, H. RAIKES, J. CONSTANTINE, K. BOLLER, J. BROOKS-GUNN, R. CHAZAN-COHEN, L. B. TARULLO, C. BRADY-SMITH, ET AL. (2005): "The effectiveness of Early Head Start for 3-year-old children and their parents: lessons for policy and programs." *Developmental Psychology*, 41, 885.

LUECHINGER, S. (2014): "Air pollution and infant mortality: a natural experiment from power plant desulfurization," *Journal of Health Economics*, 37, 219–231.

MACMILLAN, H. L., C. N. WATHEN, J. BARLOW, D. M. FERGUSSON, J. M. LEVENTHAL, AND H. N. TAUSSIG (2009): "Interventions to prevent child maltreatment and associated impairment," *The Lancet*, 373, 250–266.

MAIER-PFEIFFER, A., K. KUTZ, AND M. SANDNER (2013): "Kosten, Nutzen und Finanzierung in den Frühen Hilfen," *Kinder schützen, Familien stärken*, 111–124.

MAKLES, A. AND K. SCHNEIDER (2011): "Segregation in primary schools-Do school districts really matter," *Evidence from Policy Reforms. Schumpeter Discussion Papers*.

——— (2012): "Freie Wahl der Grundschule: Wie entscheiden sich Eltern und welche Konsequenzen hat die Schulwahl für die Segregation?" *DDS–Die Deutsche Schule*, 104, 332–346.

MARCOTTE, D. E. (2017): "Something in the air? Air quality and children's educational outcomes," *Economics of Education Review*, 56, 141–151.

MARCUS, J., J. NEMITZ, AND C. K. SPIESS (2016): "Veränderungen in der gruppenspezifischen Nutzung von ganztägigen Schulangeboten-Längsschnittanalysen für den Primarbereich," *Zeitschrift für Erziehungswissenschaft*, 19, 415–442.

MARGARYAN, S. (2021): "Low emission zones and population health," *Journal of Health Economics*, 76, 102402.

MARGARYAN, S., A. PAUL, AND T. SIEDLER (2021): "Does education affect attitudes towards immigration? Evidence from Germany," *Journal of Human Resources*, 56, 446–479.

Matthewes, S. H. (2021): "Better together? Heterogeneous effects of tracking on student achievement," *The Economic Journal*, 131, 1269–1307.

Mayer, T. (2017): "School choice and the urban neighbourhood: segregation processes in the German primary sector with special reference to private schools," *Private schools and school choice in compulsory education: global change and national challenge*, 153–175.

McCrae, R. R. and P. T. Costa Jr (2008): "The five-factor theory of personality," in *Handbook of Personality: Theory and Research*, The Guilford Press, vol. 3, 159–181.

McIntosh, E., J. Barlow, H. Davis, and S. Stewart-Brown (2009): "Economic evaluation of an intensive home visiting programme for vulnerable families: A cost-effectiveness analysis of a public health intervention," *Journal of Public Health*, 31, 423–433.

Miller, T. R. and D. Hendrie (2015): "Nurse Family Partnership: Comparing costs per family in randomized trials versus scale-up," *The Journal of Primary Prevention*, 36, 419–425.

Mincer, J. (1991): "Education and unemployment," *National Bureau of Economic Research Cambridge, Mass., USA*.

Ministry of Education North Rhine-Westphalia (2012): "Grundschulempfehlung," .

Mortamais, M., J. Pujol, G. Martínez-Vilavella, R. Fenoll, C. Reynes, R. Sabatier, I. Rivas, J. Forns, N. Vilor-Tejedor, S. Alemany, et al. (2019): "Effects of prenatal exposure to particulate matter air pollution on corpus callosum and behavioral problems in children," *Environmental Research*, 178, 108734.

Mühlenweg, A. M. (2008): "Educational effects of alternative secondary school tracking regimes in Germany," *Journal of Contextual Economics*, 128, 351.

Müller, S. and T. Schneider (2013): "Educational pathways and dropout from higher education in Germany," *Longitudinal and Life Course Studies*, 4, 218–241.

Mühlenweg, A. M. and P. A. Puhani (2010): "The Evolution of the School-Entry Age Effect in a School Tracking System," *Journal of Human Resources*, 45.

Neal, D. (1997): "The effects of Catholic secondary schooling on educational achievement," *Journal of Labor Economics*, 15, 98–123.

Nemitz, J. (2015): "The effect of all-day primary school programs on maternal labor supply," *Available at SSRN 2706505*.

Neumeier, S. (2018): "Erreichbarkeit von Regelschulen in Deutschland: eine Betrachtung des ersten Bildungswegs nach Primarstufe, Sekundarstufe 1 und Sekundarstufe 2," Tech. rep., Thünen Working Paper.

Noreisch, K. (2007): "School catchment area evasion: the case of Berlin, Germany," *Journal of Education Policy*, 22, 69–90.

Nybom, M. (2017): "The distribution of lifetime earnings returns to college," *Journal of Labor Economics*, 35, 903–952.

OECD (2017a): "School choice and school vouchers: An OECD perspective," *OECD Publishing Paris*.

——— (2017b): "Starting Strong 2017: Key OECD Indicators on Early Childhood Education and Care," *OECD Publishing Paris*.

——— (2019): "Germany: Country Health Profile 2019, State of Health in the EU," *OECD Publishing Paris*, accessed: 2022-12-13.

——— (2021): "Education at a Glance 2021: OECD Indicators on Early Childhood Education and Care," *OECD Publishing Paris*, accessed: 2022-12-13.

Oeltjen, M. and M. Windzio (2022): "Nachbarschaften als Bildungskontexte und die Dynamiken räumlicher Mobilität von Familien," in *Differenz im Raum: Sozialstruktur und Grenzziehung in deutschen Städten*, Springer, 113–140.

Olds, D. L. (2006): "The nurse–family partnership: An evidence-based preventive intervention," *Infant Mental Health Journal*, 27, 5–25.

Olds, D. L., J. Robinson, R. O'Brien, D. W. Luckey, L. M. Pettitt, C. R. Henderson Jr, R. K. Ng, K. L. Sheff, J. Korfmacher, S. Hiatt, et al. (2002): "Home visiting by paraprofessionals and by nurses: A randomized, controlled trial," *Pediatrics*, 110, 486–496.

Oreopoulos, P. and K. G. Salvanes (2011): "Priceless: The nonpecuniary benefits of schooling," *Journal of Economic Perspectives*, 25, 159–184.

Oswald, A. J., E. Proto, and D. Sgroi (2015): "Happiness and productivity," *Journal of Labor Economics*, 33, 789–822.

PARK, J. (2017): "Hot temperature and high stakes exams: Evidence from new york city public schools," *URL: https://scholar. harvard. edu/files/jisungpark/files/papernycaejep. pdf.*

PAUEN, S. (2011): "Vom Baby zum Kleinkind," in *Vom Baby zum Kleinkind*, Springer, 1–11.

PAUEN, S., L. HEILIG, D. DANNER, J. HAFFNER, A. TETTENBORN, AND J. ROOS (2012): "Milestones of Normal Development in Early Years (MONDEY)," *Frühe Bildung.*

PEACOCK, S., S. KONRAD, E. WATSON, D. NICKEL, AND N. MUHAJARINE (2013): "Effectiveness of home visiting programs on child outcomes: A systematic review," *BMC public health*, 13, 1–14.

PERSICO, C. AND J. VENATOR (2021): "The Effects of Local Industrial Pollution on Students and Schools," *Journal of Human Resources*, 56, 406–445.

PESTEL, N. AND F. WOZNY (2021): "Health effects of Low Emission Zones: Evidence from German hospitals," *Journal of Environmental Economics and Management*, 109, 102512.

PETER, F. (2016): "The effect of involuntary maternal job loss on children's behaviour and non-cognitive skills," *Labour Economics*, 42, 43–63.

PETER, F. H., P. S. SCHOBER, AND K. C. SPIESS (2016): "Early birds in day care: The social gradient in starting day care and children's non-cognitive skills," *CESifo Economic Studies*, 62, 725–751.

PLANTENGA, J. AND C. REMERY (2015): "Provision of childcare services: A comparative review of EU member states," *CESifo DICE Report*, 13, 20–24.

POLANSKA, K., J. JUREWICZ, AND W. HANKE (2015): "Smoking and alcohol drinking during pregnancy as the risk factors for poor child neurodevelopment – A review of epidemiological studies," *International Journal of Occupational Medicine and Environmental Health*, 28, 419.

POSTMA, D. S. (2007): "Gender differences in asthma development and progression," *Gender Medicine*, 4, S133–S146.

PUHANI, P. A. (2018): "Do boys benefit from male teachers in elementary school? Evidence from administrative panel data," *Labour Economics*, 51, 340–354.

Puma, M., S. Bell, R. Cook, C. Heid, P. Broene, F. Jenkins, A. Mashburn, and J. Downer (2012): "Third Grade Follow-Up to the Head Start Impact Study: Final Report. OPRE Report 2012-45." *Administration for Children & Families.*

Radisch, F. (2009): *Qualität und Wirkung ganztägiger Schulorganisation: theoretische und empirische Befunde*, Beltz Juventa.

Ramos Lobato, I. and T. Groos (2019): "Choice as a duty? The abolition of primary school catchment areas in North Rhine-Westphalia/Germany and its impact on parent choice strategies," *Urban Studies*, 56, 3274–3291.

Redmond, C. (2014): "Heterogeneity, Selection and Advantage in the Graduate and Non-Graduate Labour Market," *Unpublished manuscript.*

Reiss, K., M. Weis, E. Klieme, and O. Köller (2019): *PISA 2018: Grundbildung im internationalen Vergleich*, Waxmann.

Requia, W. J., C. C. Saenger, R. E. Cicerelli, L. Monteiro de Abreu, and V. R. Cruvinel (2022): "Air quality around schools and school-level academic performance in Brazil," *Atmospheric Environment*, 279, 119125.

Richter, D., M. Metzing, M. Weinhardt, and J. Schupp (2013): "SOEP scales manual," *SOEP Survey Papers.*

Richter, D., J. Rohrer, M. Metzing, W. Nestler, M. Weinhardt, and J. Schupp (2017): "SOEP scales manual (updated for SOEP-Core v32. 1)," *SOEP Survey Papers.*

Riddell, W. C. and X. Song (2011): "The impact of education on unemployment incidence and re-employment success: Evidence from the US labour market," *Labour Economics*, 18, 453–463.

Roth, S. (2016): "The contemporaneous effect of indoor air pollution on cognitive performance: Evidence from the UK," *Unpublished manuscript.*

Rouse, C. E. (1995): "Democratization or diversion? The effect of community colleges on educational attainment," *Journal of Business & Economic Statistics*, 13, 217–224.

Runge, R. A. and R. Soellner (2019): "Measuring children's emotional and behavioural problems: are SDQ parent reports from native and immigrant parents comparable?" *Child and adolescent psychiatry and mental health*, 13, 1–15.

Sadler, L. S., A. Slade, N. Close, D. L. Webb, T. Simpson, K. Fennie, and L. C. Mayes (2013): "Minding the baby: Enhancing reflectiveness to improve early health and relationship outcomes in an interdisciplinary home-visiting program," *Infant Mental Health Journal*, 34, 391–405.

Sammons, P., J. Hall, R. Smees, J. Goff, K. Sylva, T. Smith, M. Evangelou, N. Eisenstadt, and G. Smith (2015): *The impact of children's centres: studying the effects of children's centres in promoting better outcomes for young children and their families: Evaluation of Children's Centres in England (ECCE, Strand 4)*, Department for Education.

Sander, W. (1999): "Private schools and public school achievement," *Journal of Human Resources*, 697–709.

Sanders, N. J. (2012): "What doesn't kill you makes you weaker: prenatal pollution exposure and educational outcomes," *Journal of Human Resources*, 47, 826–850.

Sandner, M. (2013): "Effects of early childhood intervention on child development and early skill formation: Evidence from a randomized controlled trail," *Hannover Economic Papers (HEP)*, 518.

——— (2019): "Effects of early childhood intervention on fertility and maternal employment: Evidence from a randomized controlled trial," *Journal of Health Economics*, 63, 159–181.

Sandner, M., T. Cornelissen, T. Jungmann, and P. Herrmann (2018): "Evaluating the effects of a targeted home visiting program on maternal and child health outcomes," *Journal of Health Economics*, 58, 269–283.

Sandner, M. and T. Jungmann (2017): "Gender-specific effects of early childhood intervention: Evidence from a randomized controlled trial," *Labour Economics*, 45, 59–78.

Sann, A. and K. Thrum (2005): "Opstapje–Schritt für Schritt," *Abschlussbericht des Modellprojekts, München: DJI.*

Sarmiento, L., N. Wägner, and A. Zaklan (2021): "Effectiveness, spillovers, and well-being effects of driving restriction policies," *DIW Discussion Paper*.

Sauerwein, M. N., N. Thieme, and E. Chiapparini (2019): "Wie steht es mit der Ganztagsschule? Ein Forschungsreview mit sozialpädagogischer Kommentierung," *Soziale Passagen*, 1–17.

Schaub, S., E. Ramseier, A. Neuhauser, S. C. Burkhardt, and A. Lanfranchi (2019): "Effects of home-based early intervention on child outcomes: A randomized controlled trial of Parents as Teachers in Switzerland," *Early Childhood Research Quarterly*, 48, 173–185.

Schildberg-Hoerisch, H. (2011): "Does parental employment affect children's educational attainment?" *Economics of Education Review*, 30, 1456–1467.

Schiman, J. C., R. Kaestner, and A. T. L. Sasso (2017): "Early childhood health shocks and adult wellbeing: Evidence from wartime Britain," *National Bureau of Economic Research.*

Schlack, R., H. Hölling, B.-M. Kurth, and M. Huss (2007): "Die Prävalenz der Aufmerksamkeitsdefizit-/Hyperaktivitätsstörung (ADHS) bei Kindern und Jugendlichen in Deutschland," *Bundesgesundheitsblatt-Gesundheitsforschung-Gesundheitsschutz*, 50, 827–835.

Schmitz, S. and A. Kröger (2017): "Effizienzanalysen frühkindlicher Bildungs-und Betreuungsangebote: Mehr differenzierte Analysen für Deutschland erforderlich," Tech. rep., DIW Roundup: Politik im Fokus.

Schmitz, S., C. K. Spiess, and J. F. Stahl (2017): "Day care centers: Family expenditures increased significantly at some points between 1996 and 2015," *DIW Economic Bulletin*, 7, 411–423.

Schneider, A., J. Cyrys, S. Breitner, U. Kraus, A. Peters, V. Diegmann, and L. Neunhäuserer (2018): "Quantifizierung von umweltbedingten Krankheitslasten aufgrund der Stickstoffdioxid-Exposition in Deutschland," *Umweltbundesamt*, 1, 2018.

Schultz, T. W. (1963): "The economic value of education," *New York: Columbia Press.*

Schütte, K., O. Köller, Y. Anders, and F. Petermann (2020): "Empirische Arbeit: Kontinuität als Schlüssel zu wirksamer Förderung frühkindlicher Entwicklung benachteiligter Kinder," *Psychologie in Erziehung und Unterricht.*

Seidlitz, A. and L. Zierow (2020): "The Impact of All-Day Schools on Student Achievement-Evidence from Extending School Days in German Primary Schools," Tech. rep., CESifo Working Paper.

Sidor, A., E. Kunz, A. Eickhorst, and M. Cierpka (2013): "Effects of the early prevention program "Keiner faellt durchs Netz"("Nobody slips through the net") on child, mother, and their relationship: A controlled study," *Infant Mental Health Journal*, 34, 11–24.

SIEGLE, D. (2020): "Likert Scales," Tech. rep., `https://researchbasics.education.uconn.edu/likert_scales/`, accessed: 2022-12-13.

SILVESTRIN, S., V. N. HIRAKATA, C. H. DA SILVA, AND M. Z. GOLDANI (2020): "Inequalities in birth weight and maternal education: A time-series study from 1996 to 2013 in Brazil," *Scientific reports*, 10, 1–8.

SMITH, A. (1937): *The wealth of nations [1776]*, vol. 11937.

SMITH, J. R., J. BROOKS-GUNN, P. K. KLEBANOV, ET AL. (1997): "Consequences of living in poverty for young children's cognitive and verbal ability and early school achievement," *Consequences of growing up poor*, 132, 189.

SOEP (2021): "The SOEP Samples in Detail," http://companion.soep.de/Target Population in Detail.html, accessed: 2019-08-12.

SON, J. AND J. WILSON (2012): "Volunteer work and hedonic, eudemonic, and social well-being," in *Sociological forum*, Wiley Online Library, vol. 27, 658–681.

SONDHEIMER, R. M. AND D. P. GREEN (2010): "Using experiments to estimate the effects of education on voter turnout," *American Journal of Political Science*, 54, 174–189.

SPIESS, C. K. (2013): "Effizienzanalysen frühkindlicher Bildungs-und Betreuungsprogramme," *Zeitschrift für Erziehungswissenschaft*, 16, 333–354.

——— (2015): "Early childhood education and care services and child development: Economic perspectives for universal approaches," *Emerging Trends in the Social and Behavioral Sciences: An Interdisciplinary, Searchable, and Linkable Resource*, 1–14.

SPIESS, C. K. AND K. WROHLICH (2010): "Does distance determine who attends a university in Germany?" *Economics of Education Review*, 29, 470–479.

ST. PIERRE, R. G. AND J. I. LAYZER (1999): "Using home visits for multiple purposes: the Comprehensive Child Development Program," *The Future of children*, 134–151.

STAFFORD, T. M. (2015): "Indoor air quality and academic performance," *Journal of Environmental Economics and Management*, 70, 34–50.

STANAT, P., S. SCHIPOLOWSKI, N. MAHLER, S. WEIRICH, AND S. HENSCHEL (2019): *IQB-Bildungstrend 2018*, Münster; New York: Waxmann.

STANAT, P., S. SCHIPOLOWSKI, C. RJOSK, S. WEIRICH, AND N. HAAG (2017): *IQB-Bildungstrend 2016. Kompetenzen in den Fächern Deutsch und Mathematik am Ende der 4. Jahrgangsstufe im zweiten Ländervergleich*, Münster; New York: Waxmann.

STATISTISCHES BUNDESAMT (2020a): *Privatschulen in Deutschland — Fakten und Hintergründe 2020*, Statistisches Bundesamt, Wiesbaden.

——— (2020b): "Statistischer Bericht - Budget für Bildung, Forschung und Wissenschaft 2019/2020," Tech. rep., https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Bildung-Forschung-Kultur/Bildungsfinanzen-Ausbildungsfoerderung/Publikationen/Downloads-Bildungsfinanzen/statistischer-bericht-budget-bildung-5217108207005.html?nn=209224, accessed: 2022-12-13.

——— (2021a): "Bevölkerung nach Nationalität und Bundesländern," Tech. rep., `https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Bevoelkerung/Bevoelkerungsstand/Tabellen/bevoelkerung-nichtdeutsch-laender.html`, accessed: 2021-08-12.

——— (2021b): *Private Schulen - Fachserie 11 Reihe 1.1 - Schuljahr 2020/21*, Statistisches Bundesamt, Wiesbaden.

——— (2022): "Interaktive Karten zu Privatschulen und Schulgeld," *Statistisches Bundesamt, Wiesbaden.*

STEG KONSORTIUM (2016): "Ganztagsschule: Bildungsqualität und Wirkungen außerunterrichtlicher Angebote. Ergebnisse der Studie zur Entwicklung von Ganztagsschulen 2012–2015," *Frankfurt am Main: StEG Konsortium.*

STEINER, C. (2009): "Mehr Chancengleichheit durch die Ganztagsschule?" in *Ganztägige Bildung und Betreuung*, Beltz, 81–105.

SU, L., Y. TU, AND A. ULLAH (2015): "Testing additive separability of error term in nonparametric structural models," *Econometric Reviews*, 34, 1057–1088.

SUN, L. AND S. ABRAHAM (2021): "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 225, 175–199.

SWEET, M. A. AND M. I. APPELBAUM (2004): "Is home visiting an effective strategy? A meta-analytic review of home visiting programs for families with young children," *Child Development*, 75, 1435–1456.

TAMM, M. (2008): "Does money buy higher schooling?: Evidence from secondary school track choice in Germany," *Economics of Education Review*, 27, 536–545.

TODD, P. E. AND K. I. WOLPIN (2007): "The production of cognitive achievement in children: Home, school, and racial test score gaps," *Journal of Human Capital*, 1, 91–136.

UNITED NATIONS (1948): "Universal declaration of human rights," *UN General Assembly*, 302, 14–25.

U.S. DEPARTMENT OF HEALTH & HUMAN SERVICES (2004): "Head Start Program Facts: Fiscal Year 2004," Tech. rep., https://eclkc.ohs.acf.hhs.gov/archive/about-us/article/head-start-program-facts-fiscal-year-2004, accessed: 2022-12-13.

VAN BERGEN, E., T. VAN ZUIJEN, D. BISHOP, AND P. F. DE JONG (2017): "Why are home literacy environment and children's reading skills associated? What parental skills reveal," *Reading Research Quarterly*, 52, 147–160.

VIARD, V. B. AND S. FU (2015): "The effect of Beijing's driving restrictions on pollution and economic activity," *Journal of Public Economics*, 125, 98–115.

VITOUSEK, P. M., J. D. ABER, R. W. HOWARTH, G. E. LIKENS, P. A. MATSON, D. W. SCHINDLER, W. H. SCHLESINGER, AND D. G. TILMAN (1997): "Human alteration of the global nitrogen cycle: sources and consequences," *Ecological applications*, 7, 737–750.

WAGNER, M. M. AND S. L. CLAYTON (1999): "The Parents as Teachers program: Results from two demonstrations," *The Future of Children*, 91–115.

WALDINGER, F. (2007): "Does Ability Tracking Exacerbate the Role of Family Background for Student's Test Scores?" .

WASHINGTON-NORTEY, P.-M., F. ZHANG, Y. XU, A. B. RUIZ, C.-C. CHEN, AND C. SPENCE (2020): "The Impact of Peer Interactions on Language Development Among Preschool English Language Learners: A Systematic Review," *Early Childhood Education Journal*, 1–11.

WEIS, M., K. MÜLLER, J. MANG, J.-H. HEINE, N. MAHLER, AND K. REISS (2018): "Soziale Herkunft, Zuwanderungshintergrund und Lesekompetenz," *Pisa*, 129–162.

WESTPHAL, M., D. KAMHÖFER, AND H. SCHMITZ (2020): "Marginal college wage premiums under selection into employment," *IZA Discussion Paper No. 13382*.

WHITAKER, A. A., G. BAKER, L. J. MATTHEWS, J. S. MCCOMBS, AND M. BARRETT (2019): *Who Plays, who Pays?: Funding for and Access to Youth Sports*, RAND.

WISSENSCHAFTLICHE ARBEITSSTELLE EVANGELISCHE SCHULE (2021): "Statistik Evangelische Schulen: Empirische Befunde und Perspektiven 2017 bis 2019," https://www.ekd.de/ev-schulen-ses-statistik-42364.htm, accessed: 2023-02-20.

WOESSMANN, L. (2007): "International evidence on school competition, autonomy, and accountability: A review," *Peabody Journal of Education*, 82, 473–497.

WOLFF, H. (2014): "Keep your clunker in the suburb: low-emission zones and adoption of green vehicles," *The Economic Journal*, 124, F481–F512.

WOODHEAD, M. AND P. MOSS (2007): *Early childhood and primary education: Transitions in the lives of young children*, 2, Open University.

WOOLDRIDGE, J. (2010): "Linear Probability Model for Binary Response. Econometric Analysis of Cross Section and Panel Data," .

——— (2021): "Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators," *Available at SSRN 3906345*.

WORKMAN, S. (2018): "Where does your child care dollar go? Understanding the true cost of quality early childhood education," *Washington: Center for American Progress*.

WRASE, M. AND M. HELBIG (2016): "Das missachtete Verfassungsgebot-Wie das Sonderungsverbot nach Art. 7 IV 3 GG unterlaufen wird," *NVwZ-Neue Zeitschrift für Verwaltungsrecht*, 35, 1591–1598.

WRASE, M., L. JUNG, AND M. HELBIG (2017): "Defizite der Regulierung und Aufsicht von privaten Ersatzschulen in Bezug auf das Sonderungsverbot nach Art. 7 Abs. 5 S. 3 GG," Tech. rep., WZB Discussion Paper P 2017-003. Berlin: Wissenschaftszentrum Berlin für Sozialforschung.

WÖLFEL, O. AND G. HEINECK (2012): "Parental risk attitudes and children's secondary school track choice," *Economics of Education Review*, 31, 727–743.

ZHOU, X. AND Y. XIE (2019): "Marginal treatment effects from a propensity score perspective," *Journal of Political Economy*, 127, 3070–3084.