Social Policies, Within-Family Investments and Children's Human Capital: Four Essays in Family and Education Economics

DISSERTATION

zur Erlangung des Grades eines Doktors der
wirtschaftlichen Staatswissenschaften
(Dr. rer. pol.)

des Fachbereichs Rechts- und Wirtschaftswissenschaften
der Johannes Gutenberg-Universität Mainz

vorgelegt von
Elena Ziege, M.Sc.

in Mainz
im Jahre 2026

Erstgutachterin: Prof. Dr. C. Katharina Spieß

Zweitgutachterin: Prof. Ariel Kalil, Ph.D.

Drittgutachter: Prof. Dr. Jan Marcus

Tag der mündlichen Prüfung: 30.01.2026

Acknowledgments

Contents

Abstract

This dissertation consists of four independent chapters contributing to the literature of family and education economics. They study the impact of social policies and within-family investments on family's human capital and behavior. These chapters are preceded by an introduction outlining the motivation for the investigated research questions and highlighting the shared contributions of the four empirical research chapters. The dissertation conludes with a final chapter that makes policy recommendations, discusses potential limitations, and points towards future research.

Chapter 2 demonstrates that most parents overestimate their children's skills and performance in school. As parental misperceptions are more frequently found in less-advantaged families, they can exacerbate educational inequality, since parents' beliefs about their children's current performance influence their investments in their children's skill development. We capitalize on exogenous variation in report card distribution across federal states in Germany to better understand how schools as organizations can communicate with parents. We examine whether information from teachers regarding a child's performance affects parental beliefs and behavior and if so how this differs by subject and group. Our findings indicate that school information boosts parents' behavioral investments in child skill-building while having a limited impact on their beliefs. This finding suggests that receiving information from schools can be valuable as it reinforces the importance of educational activities for parents. We further find that numerical information treatments are more impactful than verbal treatments, that subsequent treatments are less potent than the initial treatment, and that school information only boosts parental investment when teachers hold accurate beliefs about children's skills.

About one in five children across OECD countries lives in single-parent households, many of which receive no financial support from the non-resident parent. To address this, several countries have introduced public advance child support schemes. **Chapter 3** investigates the impact of such payments on the financial well-being, labor supply decisions, and household spending patterns of single-parent families, drawing on a ma-

jor reform to Germany's advance child support program that substantially expanded both benefit duration and eligibility in 2017. Using representative data from the German Microcensus and the Income and Expenditure Survey, we find these payments to improve families' financial situations without crowding out private child support. Since eligibility was tied to economic independence of single-parent households, the reform also led to a decrease in the probability of receiving welfare benefits, which appears to be driven by exits from welfare due to increases in labor supply at the intensive rather than the extensive margin. We also find changes in expenditure patterns of affected families, with increased spending on food and beverages as well as goods related to the human capital development and well-being of children.

Chapter 4 analyzes family spillovers of birthright citizenship in Germany. By using difference-in-differences and event study methodologies on large-scale survey datasets, I examine the direct impact of citizenship on immigrant children and its spillover effects on the educational achievements of their older siblings who were born before the reform. The findings reveal educational benefits for immigrant children, and positive spillover effects on their older siblings' academic achievements. Children are 13 percentage points more likely, and their older siblings are 6 percentage points more likely, to complete secondary school with the highest degree. The spillovers can be attributed to a considerable increase in parental investments in the siblings' education and increased naturalization of parents and older siblings. Consequently, this study suggests that previous evaluations of citizenship have underestimated its benefits.

Health and well-being in the family context can be affected by care giving arrangements. Following parental care and daycare, grandparents are the third most important care givers for children in many Western societies. Despite the relevance of grandparental care, there is little evidence on the causal effects of this care mode on the next generations' health and well-being. In **Chapter 5**, we fill this gap by investigating the causal impact of regular grandparental care on the self-reported health and (domain-specific) satisfaction of both parents and children. To do so, we exploit geographic distance to grandparents as a source of arguably exogenous variation and use representative German panel data for families with children under the age of eleven. Our results suggest positive effects on parental satisfaction with the child care situation, as well as mothers' satisfaction with their leisure time. However, we also find negative effects on children's health, particularly for elementary school aged children and for boys.

Zusammenfassung

Diese Dissertation umfasst vier unabhängige Kapitel, die zur Literatur im Bereich der Familien- und Bildungsökonomik beitragen. Sie untersuchen die Auswirkungen von Sozialpolitik und innerfamiliären Investitionen auf das Humankapital und das Verhalten von Familien. Den Kapiteln geht eine allgemeine Einleitung voraus, die die Motivation für die behandelten Forschungsfragen darlegt und die gemeinsamen Beiträge der vier empirischen Kapitel hervorhebt. Die Arbeit schließt mit einem Kapitel ab, das politische Handlungsempfehlungen formuliert, mögliche Limitationen diskutiert und auf zukünftige Forschungsfragen verweist.

Kapitel 2 zeigt, dass die meisten Eltern die Fähigkeiten und schulischen Leistungen ihrer Kinder überschätzen. Da solche Fehleinschätzungen besonders häufig in benachteiligten Familien auftreten, können sie Bildungsungleichheiten verstärken: Die elterlichen Vorstellungen über die Leistungsfähigkeit ihrer Kinder beeinflussen maßgeblich, wie stark sie in die Weiterentwicklung der kindlichen Kompetenzen investieren. Um besser zu verstehen, wie Schulen mit Eltern kommunizieren können, nutzen wir exogene Variation im Zeitpunkt der Zeugnisausgabe zwischen den Bundesländern in Deutschland. Wir untersuchen, ob und in welcher Weise Informationen von Lehrkräften über die Leistungen der Kinder die Einschätzungen und das Verhalten der Eltern beeinflussen und ob sich diese Effekte nach Schulfach oder Bevölkerungsgruppe unterscheiden. Unsere Ergebnisse zeigen, dass schulische Informationen die Investitionen von Eltern in die Kompetenzentwicklung ihrer Kinder verstärken, während ihre Überzeugungen nur in begrenztem Maße beeinflusst werden. Dies deutet darauf hin, dass die Rückmeldungen von Schulen wertvoll sein können, da sie die Bedeutung von Bildungsaktivitäten für Eltern unterstreichen. Zudem finden wir, dass Zeugnisse in Form von numerischen Informationen wirkungsvoller sind als verbale Lernstandsberichte, dass spätere Zeugnisse weniger stark wirken als das erste Zeugnis und dass schulische Informationen nur dann zu mehr elterlichen Investitionen führen, wenn die Lehrkräfte zutreffende Einschätzungen über die kindlichen Fähigkeiten haben.

In OECD-Ländern wächst etwa jedes fünfte Kind bei einem alleinerziehenden Elternteil auf, von denen viele keine finanzielle Unterstützung vom nicht im Haushalt lebenden Elternteil erhalten. Um diesem Problem zu begegnen, haben mehrere Länder öffentliche Unterhaltsvorschusszahlungen eingeführt, die alleinerziehende Elternteile erhalten, wenn das andere Elternteil nicht den vollständigen Unterhalt zahlt. **Kapitel 3** untersucht die Auswirkungen solcher Zahlungen auf die finanzielle Situation, die Erwerbsentscheidungen sowie den Konsum von Alleinerziehenden. Grundlage ist eine umfassende Reform des deutschen Unterhaltsvorschussgesetzes aus dem Jahr 2017, die sowohl die Bezugsdauer als auch den Kreis der Anspruchsberechtigten deutlich ausweitete. Unter Verwendung repräsentativer Daten des Mikrozensus und der Einkommens- und Verbrauchsstichprobe zeigen wir, dass die Zahlungen die finanzielle Situation der betroffenen Familien verbessern, ohne private Unterhaltsleistungen zu verringern. Da die Anspruchsberechtigung an die wirtschaftliche Unabhängigkeit der Haushalte geknüpft war, führte die Reform zudem zu einer geringeren Wahrscheinlichkeit, Sozialleistungen zu beziehen. Dieser Effekt scheint vor allem auf eine Erhöhung des Arbeitsangebots in bereits bestehenden Beschäftigungsverhältnissen zurückzuführen. Darüber hinaus finden wir Veränderungen im Konsumverhalten der betroffenen Familien: So stiegen insbesondere die Ausgaben für Nahrungsmittel und Getränke sowie für Güter, die zur Humankapitalentwicklung und zum Wohlbefinden der Kinder beitragen.

Kapitel 4 evaluiert die Einführung des Geburtsortsprinzips in Deutschland, das dazu führte, dass in Deutschland geborene Kinder zugewanderter Eltern unter bestimmten Bedingungen bei der Geburt die deutsche Staatsangehörigkeit erhielten. Mithilfe eines Differenz-von-Differenzen-Ansatzes und von Event-Study-Methoden untersuche ich auf Basis umfangreicher Umfragedaten die direkten Effekte der Staatsangehörigkeit auf Kinder mit Zuwanderungsgeschichte sowie die Auswirkungen auf die Bildung ihrer älteren Geschwister, die vor der Reform geboren wurden. Die Ergebnisse zeigen, dass die Staatsangehörigkeit nicht nur Bildungsvorteile für die Kinder selbst hat, sondern sich auch positiv auf die schulischen Leistungen ihrer älteren Geschwister auswirkt. Die Kinder haben dadurch eine um 13 Prozentpunkte höhere Wahrscheinlichkeit, die Sekundarschule mit dem höchsten Abschluss zu beenden, während ihre älteren Geschwister eine um 6 Prozentpunkte höhere Wahrscheinlichkeit aufweisen. Die Effekte auf die Geschwister lassen sich auf eine deutliche Zunahme elterlichen Engagements für die Bildung der Geschwister sowie auf eine höhere Einbürgerungsrate von Eltern und älteren Geschwistern zurückführen. Somit legt die Studie nahe, dass frühere Studien zur Staatsangehörigkeit deren Vorteile unterschätzt haben.

Gesundheit und Wohlbefinden von Familien können maßgeblich durch Betreuungsarrangements beeinflusst werden. Neben der elterlichen Betreuung und institutioneller

Kindertagesbetreuung zählen Großeltern in vielen westlichen Gesellschaften zur drittwichtigsten Gruppe von Betreuungspersonen für Kinder. Trotz dieser Bedeutung gibt es bislang nur wenig belastbare Evidenz zu den kausalen Effekten großelterlicher Kinderbetreuung auf die Gesundheit und das Wohlbefinden nachfolgender Generationen. In **Kapitel 5** schließen wir diese Forschungslücke, indem wir den kausalen Einfluss regelmäßiger Betreuung durch Großeltern auf die selbst eingeschätzte Gesundheit sowie die (bereichsspezifische) Zufriedenheit von Eltern und Kindern untersuchen. Als weitgehend exogene Variation nutzen wir dabei die geografische Distanz zu den Großeltern und greifen auf repräsentative deutsche Paneldaten von Familien mit Kindern unter elf Jahren zurück. Unsere Ergebnisse deuten auf positive Effekte auf die elterliche Zufriedenheit mit der Betreuungssituation sowie die Zufriedenheit von Müttern mit ihrer Freizeitgestaltung hin. Gleichzeitig zeigen sich jedoch negative Effekte auf die Gesundheit der Kinder, insbesondere bei Grundschulkindern und bei Jungen.

List of Figures

List of Tables

CHAPTER 1

Introduction

1.1 Motivation

Advancing the human capital of future generations is a central concern for researchers and policymakers in many countries. Human capital theory, introduced by Becker (1962), predicts that investments in skills are made when the expected future benefits outweigh the costs. These skills include cognitive and non-cognitive abilities as well as health (Heckman, 2007).[1] From a societal perspective, (non-)cognitive skills and health are important for labor market productivity and economic growth (e.g., Card, 1999; Frijters et al., 2014; Hanushek and Woessmann, 2012; Well, 2007), making education and health key targets for policy intervention.

The urgency of investing in human capital has intensified in recent years, partly due to a growing shortage of skilled labor. In OECD countries, 75% of firms now report talent shortages – this share was only at 30% a decade ago (OECD, 2023d). Such shortages threaten both economic growth and social stability. Policies that strengthen the human capital of the next generation could help alleviate these pressures by increasing the supply of qualified workers.

Educational and health inequalities highlight the importance of targeted investments. In OECD countries, family background remains a strong and persistent determinant of educational outcomes (e.g., Lindahl et al., 2015). Blanden et al. (2023) document substantial gaps in PISA test scores at age 15 and in university attendance based on parental education. Additionally, there is evidence of significant disparities in university attendance linked to parental income (e.g., for the US Chetty et al., 2017). Certain

[1] According to the World Health Organization's definition of health, well-being is an essential dimension of health (WHO, 1948). In this dissertation, I therefore examine well-being as a component of health, and consequently, as an element of human capital.

groups are particularly disadvantaged, such as children from single-parent households, who exhibit lower cognitive and non-cognitive outcomes when exposed to single parenthood at a young age (e.g., Harkness and Salgado, 2018). Similarly, immigrant students consistently underperform their non-immigrant peers (e.g., OECD, 2023c). Comparable inequalities are also observed in health outcomes, with significant gaps based on parental education (e.g., Currie and Moretti, 2003; Huebener, 2025; Kemptner and Marcus, 2013) and household income (e.g., Case et al., 2002). Targeting policies toward disadvantaged families may therefore be an efficient means of addressing both skill gaps and labor shortages.

Beyond the macroeconomic implications, education and health are recognized as universal human rights in the United Nations' Universal Declaration of Human Rights (Articles 25 and 26). At the individual level, investments in human capital can shape a wide range of life outcomes. Education raises earnings and lowers the risk of unemployment or welfare dependency (e.g., Mincer, 1974; Oreopoulos and Salvanes, 2011), while poor health reduces wages, working hours, and overall employment (e.g., Frijters et al., 2014; Pelkowski and Berger, 2004). The effects extend well beyond the labor market: education reduces mortality, divorce rates, teenage parenthood, and incarceration, while enhancing well-being and civic participation, such as voting (e.g., Lochner and Moretti, 2004; Oreopoulos and Salvanes, 2011). Health is also a crucial input into the accumulation of human capital itself (e.g., Currie, 2020). Because both education and health are strongly persistent across the life cycle and transmitted across generations (e.g., Coneus and Spiess, 2012; Datta Gupta et al., 2023; Dickson et al., 2016; Oreopoulos et al., 2006), advantages or disadvantages experienced by one family member often spill over to children, siblings, and parents. Understanding the family-level dynamics is therefore essential to fully capture the impact of policies and environments on human capital formation.

According to the model of skill formation proposed by Cunha and Heckman (2007), children's skills are shaped by a combination of genetic endowments, childhood environments, and parental investments. While genes matter, previous research suggests that they account for less than half of the variation in outcomes such as education, earnings and wealth (Duncan et al., 2023), highlighting the pivotal role of environmental factors and family influence. Childhood environments include formal educational institutions such as daycare centers and schools, which often dominate political and societal debates. However, informal education within the family setting is equally crucial. Parents, as the primary caregivers, serve as the child's first and most consistent educators. During the first year of life, for example, parental leave often makes parents the sole or principal source of care. Even after children enter formal education,

the family environment continues to have a strong influence. Despite the expansion of daycare and all-day schools in countries like Germany (e.g., BMBF, 2024; Gambaro et al., 2019; OECD, 2024a; Spieß, 2022), children spend substantial amounts of time at home, and their experiences within the household shape their educational trajectories and human potential.

The importance of parental investments in children's skill development has long been emphasized in economics (e.g., Becker, 1965; Hill and Stafford, 1974; Leibowitz, 1974, 1977). However, these investments are not evenly distributed by socioeconomic background. Higher-educated mothers (e.g., Kalil et al., 2012; Marcus et al., 2021), higher-income parents (e.g., Waldfogel and Washbrook, 2011), native-born parents (e.g., Antony-Newman, 2019) and married couples (e.g., Kalil et al., 2014) consistently devote more time and resources to their children than their less advantaged counterparts. According to the technology of skill formation, such disparities matter greatly because early investments yield the highest returns and are most effective when reinforced by subsequent ones (Cunha and Heckman, 2007). This interplay between early and later inputs underscores the central role of parental involvement and the potential for targeted policies to narrow gaps in investments and skills.

Understanding the determinants of these investments is equally important. Parents make decisions about both the amount and the type of investments they make in their children, which can be in the form of money or time. In his seminal theory of the allocation of time, Becker (1965) formally introduced the cost of time into household's utility maximization framework. Time, like money, is a scarce resource, and its opportunity cost shapes how families allocate time. This framework is central to understanding parental investment behavior.

Human capital theory predicts that parents allocate resources according to the expected returns on their investments (Becker, 1962; Becker and Tomes, 1979), but their decisions are shaped by a complex set of constraints and influences. Parental income determines the monetary budget as well as the cognitive bandwidth parents have for decision-making (e.g., Mani et al., 2013). Employment status and working hours influence the amount of time that can be devoted to children. Parents' own skills influence their capacity for educational inputs and their preferences can shape the nature of these investments (see Blanden et al., 2023). In families with more than one child, these decisions also involve an intra-family allocation problem: how to distribute resources among siblings. Such decisions depend on parental preferences for equal treatment or compensation for differences in children's initial endowments (see Becker and Tomes, 1976; Lundberg and Pollak, 2007).

Parental beliefs are another critical determinant of investments (e.g., Francesconi and Heckman, 2016). In theory, parents choose the optimal investments in the human capital of their children based on children's endowments (Becker and Tomes, 1976, 1979). However, market failures like imperfect information can lead to non-optimal investments in human capital. Parents often have imperfect information about their children's abilities, and many overestimate performance (e.g., Dizon-Ross, 2019). Such misperceptions – more prevalent among disadvantaged families (e.g., Boneva and Rauh, 2018) – can reduce engagement in learning activities (e.g., Kinsler and Pavan, 2021) and thus exacerbate educational inequalities. Understanding how these beliefs are formed and the extent to which policies can shape them is essential for designing interventions that influence parental behavior and, ultimately, skill development.

Parents' behaviors are shaped not only by their own circumstances but also by the policy environment in which they operate. One example are educational policies. Improved information provision from school, for example, might shape parental beliefs about their children's abilities and needs, thereby affecting the type and intensity of parental engagement. Beyond educational policies, other public policies can also impact parental investments in their children. Child-related transfer payments like advance child support might expand the financial resources available for developmental investments. Policies that change families' long-term legal and economic perspectives, such as citizenship laws, may shift parental expectations, aspirations, and behaviors. Parental allocation and response to social policies depends on whether they view these policies and their own investments as substitutes or complements (Heckman and Mosso, 2014). Examining how policies with different primary objectives influence parental investment behavior offers valuable insights into the mechanisms underlying parental decision-making.

Another important within-family investment comes from grandparents. In many OECD countries, grandparents are the third most important providers of child care, following parents and daycare or school (e.g., OECD, 2019). From a household production perspective, grandparental child care is an allocation of time that can either substitute for or complement parental investments. Such care can free up parents' limited time for labor market work, leisure, or other forms of investment in their children, while also directly influencing the developmental outcomes of the children in their care. In this way, grandparental involvement redistributes resources within the family, potentially affecting the human capital of multiple generations (Becker, 1965).

The central role of families in shaping children's skills highlights the need to examine not only the effects on children, but also their broader impacts at the family level. This

dissertation investigates how social policies and within-family investments influence the development of children's human capital, accounting for both direct effects on children and indirect effects operating through parental beliefs, behaviors, and well-being. The analyses address five key aspects: First, they examine how parents allocate time and financial resources across children and domains, and how these investments respond to policy interventions. Second, they investigate the direct effects of social policies and within-family investments on children's human capital. Third, they analyze spillovers within families that reach beyond the individuals directly targeted. Fourth, they place particular emphasis on disadvantaged households. Finally, they consider how policy design shapes both intended benefits and unintended consequences.

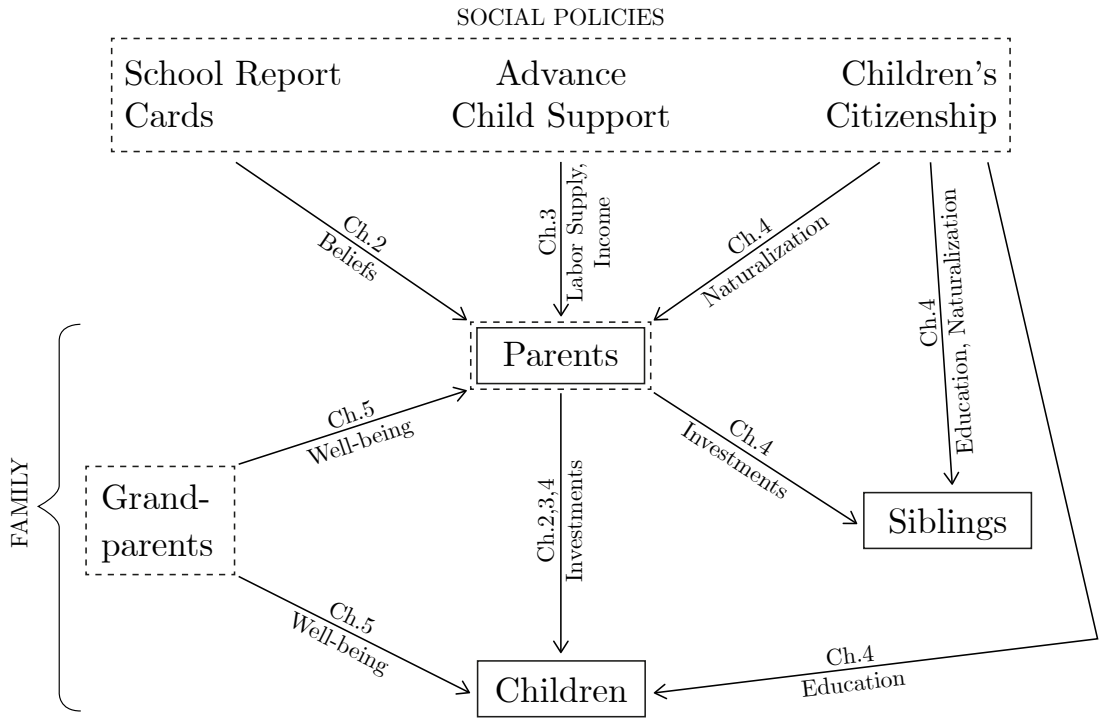
1.2 Overview and Summary

The dissertation consists of four empirical research articles in family and education economics, each addressing distinct but interrelated questions. While the chapters differ in focus, they share a common framework: families allocate time and money across family members, and these allocations are influenced by public policy and have implications for the production of human capital.

Chapters 2, 3, and 4 evaluate three German social policies: the timing and format of school report cards, a reform of advance child support payments, and a reform to children's citizenship law. The chapters assess the policies' effects on children's human capital and on parental beliefs and behavior. Parental investments in children are a central theme across these three chapters. **Chapter 5** examines another form of within-family investment: child care provided by grandparents and its implications for parents and children's human capital in the form of their health and well-being.

Figure 1.1 illustrates the connections between the chapters. The arrows represent the outcomes studied in the different chapters. The dashed square at the top shows the three social policies evaluated in **Chapters 2 to 4**. These policies are evaluated as inputs into the family system, influencing outcomes for different family members. The bottom of the diagram depicts the family members considered in the analyses: children, parents, siblings, and grandparents. The figure highlights whether they are studied as inputs (dashed line) or outputs (solid line). Children and siblings are primarily considered as "outputs" (i.e., recipients of investments by parents and grandparents or of social policies). Grandparents on the other hand, are – like the social policies – examined as "inputs" in **Chapter 5**, influencing parental and child outcomes. Parents take both roles: all chapters study the impacts on parents themselves, and **Chapters 2 to 4** also examine them as investors whose behaviors affect their children's development.

Figure 1.1: Connections Between Chapters



Source: Own illustration

Table 1.1 summarizes the key features of each chapter, including the research question, empirical strategy, data sources, and main findings. Below, I briefly outline the focus of each chapter.

In **Chapter 2**, we examine parents (mis-)perceptions of their children's skills and how these are shaped by information from schools. We show that around 40% of parents overestimate their children's abilities, with misperceptions being more prevalent in less advantaged families. This may exacerbate educational inequality, as parents' beliefs about their children's performance influence their investments in their skill development. We therefore investigate the role of school-provided information by exploiting exogenous variation in the timing of report card distributions across German federal states. The chapter assesses whether information affects parents' beliefs and educational investments at home. The German context provides a unique opportunity to compare federal states using numerical grades versus qualitative progress reports to investigate the importance of the information modality. Our sample includes children in their first year of elementary school in Germany, utilizing data from the 2013 National Educational Panel Study (NEPS) starting cohort 2. The dataset includes evaluations

Table 1.1: Overview and Summary of the Chapters

	Chapter 2	**Chapter 3**	**Chapter 4**	**Chapter 5**
Title	How information affects parents' beliefs and behavior: Evidence from first-time report cards for German school children	The Economic Impact of Advance Child Support: Financial Well-being, Labor Supply, and Consumption of Single-Parent Households	Passport to Progress: The Effects of Birthright Citizenship on Siblings' Education	The Generational Gift: The Effects of Grandparental Care on the Next Generations' Health and Well-being
Research question	How does school-provided information about children's skills impact parental beliefs about the child's skills and parental investments?	What are the effects of advance child support payments on single-parent households' financial well-being, labor supply and expenditure?	How does a child's birthright citizenship impact children's and their older siblings' education and siblings' and parents' behavior?	What is the effect of grandparental child care on parental and child health and well-being?
Empirical approach	Ordinary Least Squares	Difference-in-differences, event study	Difference-in-differences, event study	Instrumental variable approach
Data	NEPS SC 2	MZ, EVS	MZ, NEPS SC 3 and 4	Pairfam
Main finding	Increase in parental investments; parental beliefs only updated through numerical report cards (and not verbal progress reports)	Increase in family income, maternal working hours, private support payments, and expenditures on food, beverages, and goods related to children's human capital development	Children's and siblings' educational outcomes improve; enhanced naturalization of parents and siblings; increased parental investments in siblings	Improvements in parents' satisfaction with child care and mothers' satisfaction with leisure; slight negative effects on children's health

Notes: NEPS = National Educational Panel Study (SC = starting cohort), MZ = German Microcensus, EVS = Income and Expenditure Survey, Pairfam = Panel Analysis of Intimate Relationships and Family Dynamics.
Source: Own illustration.

of children's skills from both parents and teachers along with scores from standardized tests. This chapter represents an important extension of the prior literature by examining parental responses to new information about their children's achievements during the crucial first year of formal schooling. It also considers whether information modality (numeric or verbal) moderates this impact.

Findings indicate that school-provided information has limited influence on parents' beliefs: on average, school report cards do not reduce misperceptions. However, report cards affect parental behavior, leading to a significant increase in reading frequency with their children post-distribution. We show that the format and timing of in-

formation presentation are crucial. Parents receiving numerical grades or scales are more likely to adjust their perceptions and increase engagement with their children. Our analysis suggests that initial report cards are particularly effective in changing parental beliefs and behaviors. We further emphasize teacher bias, noting that about one-third of teachers overestimate their students' skills. Report cards only mitigate parental overestimation and enhance investments when they are unbiased. Our results remain robust even after excluding parents who may skew the treatment categorization based on their responsiveness. Finally, increased parental investments are observed only among those receiving negative report cards. The findings contribute to the policy discourse in Germany and other European countries regarding the efficacy of numerical versus qualitative reporting systems. The results indicate that verbal progress report cards, which are in place in the early school years in several countries, are ineffective in reducing parental misperceptions or increasing their investments in their children's education.

About one in five children across OECD countries live in single-parent households, and a large proportion of these receive no financial support from the non-resident parent. In response, several countries have implemented public advance child support schemes. Despite their widespread adoption – particularly in Europe – the impacts of these schemes remain largely unexplored. **Chapter 3** contributes to the literature by providing the first quasi-experimental evidence on the effects of public child support on single-parent families. We investigate the economic impact of such payments on families' financial situations, labor supply decisions, and household spending patterns. This chapter specifically analyzes child support payments made by non-resident parents and resident parents' expenditures on goods related to human capital development as indicators of parental investments in their children. To obtain causal estimates, we exploit a major reform to Germany's advance child support system in 2017, which expanded eligibility for substantial advance child support payments to children over the age of 11 and removed the previous 72-month payment cap. Utilizing representative microdata from the German Microcensus (MZ) and the Income and Expenditure Survey (EVS), we estimate difference-in-differences models comparing outcomes for single mothers with newly eligible children to those whose children's eligibility remained unchanged.

The evaluation shows that the reform improved families' financial situations without crowding out private child support. As eligibility was linked to the economic independence of single-parent households, the reform also increased working hours among mothers – especially those previously working 25 to 37 hours a week – and reduced reliance on unemployment benefits. Event study graphs support the causal interpretation of the estimated relation. Additionally, evidence based on the EVS indicates

that the reform significantly altered expenditure patterns, with increased expenditures on food, beverages, and goods related to childrens' human capital development and well-being. These findings offer valuable insights for policymakers, emphasizing the importance of policy design. Tying eligibility to some degree of parental employment can mitigate work disincentives while supporting children's well-being.

In **Chapter 4**, I analyze family spillover effects of birthright citizenship in Germany. In recent decades, Germany and other countries have reformed their citizenship laws, with an increasing number adopting birthright citizenship. Research indicates that birthright citizenship enhances the human capital of targeted immigrant children. However, less attention has been directed toward the spillover effects on other household members, such as older siblings. Investigating these effects is essential for evaluating the complete costs and benefits of citizenship reforms. This chapter examines spillover effects of citizenship from focal children to their older siblings by studying the introduction of birthright citizenship in Germany. This granted citizenship to immigrant children born since January 1, 2000, if at least one parent resided in Germany for a minimum of 8 years. Using a difference-in-differences approach, I compare changes in outcomes between immigrant and nonimmigrant families before and after the reform. Spillover effects are identified by comparing older siblings born between 1990 and 1998 in households with an eligible focal child (born 2000 or later) to those with an ineligible focal child (born before 2000). The analysis is based on German MZ and NEPS data.

The findings reveal that granting birthright citizenship improves the educational outcomes for both the focal children and their older siblings. Citizenship increases the likelihood of completing secondary education on the academic track by 13 percentage points for focal children and by 6 percentage points for their older siblings – about half of the direct effect. Event study graphs strengthen the common trend assumption, thus supporting a causal interpretation of the reform estimates. The reform also increased naturalization among parents and siblings, indicating that some spillovers are direct effects of the sibling's access to citizenship. Families with a citizen child also exhibit increased German language use. The results further suggest that spillovers stem from greater parental investment in older siblings, particularly through participation in school events, teacher meetings, and assistance with presentations, while investments in focal children remain constant. Consequently, this chapter suggests that previous evaluations of citizenship have underestimated its benefits. This chapter significantly contributes to the literature by being the first to investigate the effects of focal children's citizenship on family members other than parents. It is the first to consider how one family member's citizenship access influences the naturalization decisions of other family members. Finally, it provides insights into parental investment allocation

across children based on citizenship status, drawing on rich data that enable a detailed analysis of parental behavior.

Chapter 5 analyzes the impact of within-family investments provided by grandparents, specifically investigating how grandparental child care influences the health and well-being of both parents and children. Despite the relevance of grandparental care in many Western societies, there is limited evidence regarding its causal effects on the health and well-being of subsequent generations. This chapter contributes to the literature by taking a double-generation perspective and estimating the causal impact of regular grandparental care on self-reported health and (domain-specific) satisfaction of both parents and children – areas that have not been previously studied. Our analysis is based on a sample of families with children below 11 using the Panel Analysis of Intimate Relationships and Family Dynamics (pairfam), a representative panel data set for Germany collected between 2009 and 2020. To identify a causal relationship between grandparental care and our outcomes, we employ distance to grandparents as an instrumental variable (IV), assuming that proximity affects parental and child outcomes solely through the care provision. We provide evidence that major concerns regarding the exogeneity of the instrument can be excluded and demonstrate that our results remain robust even when relaxing the exclusion restriction.

Our results indicate that grandparental care is particularly beneficial for maternal well-being. Specifically, we find that it increases maternal satisfaction with child care and leisure by 9% and 11%, respectively, compared to the mean. Furthermore, fathers' satisfaction with child care increases by 19%. However, we find no evidence that grandparental care impacts parental health, life satisfaction, or other satisfaction domains. Conversely, our analysis reveals a negative impact of grandparental care on children's health, which may be explained by differences in afternoon programs organized by schools or daycare centers compared to those provided by grandparents. Overall, this chapter emphasizes that family health and well-being can be influenced by inter-familial interactions. It suggests that policymakers should take into account informal care alongside formal care programs when seeking to enhance human capital.

The four chapters are followed by a conclusion in Chapter 6, which discusses the limitations of the dissertations, potential areas for future research, and overall policy recommendations.

1.3 Contribution

This dissertation makes several important contributions to the existing literature. While each empirical chapter makes individual contributions, as outlined above and in the respective introductions of the chapters, their overall and joint contributions are highlighted in this chapter. These are connected to the fields of (i) within-family investments, (ii) children's human capital development, (iii) spillover effects within families, and (iv) implications for disadvantaged families.

All four chapters contribute to the literature on **within-family investments** by examining both how they are shaped and how they affect other family members. A central theme is parental investment in children's development, a key determinant of human capital formation (Cunha and Heckman, 2007). **Chapters 2, 3, and 4** specifically examine how social policies shape these investments. The chapters approach this theme from complementary perspectives: Chapter 2 analyzes how parents allocate their time, Chapter 3 investigates financial resource allocation, and Chapter 4 considers both dimensions jointly. **Chapter 2** explores educational activities, such as reading and playing, conducted by first-grade parents and how these investments are influenced by information from schools. **Chapter 3** examines expenditures of separated parents, including spending by resident parents on products and services related to human capital development of their children and child support payments by non-resident parents. **Chapter 4** investigates how parents allocate time and money between children in the household and how the allocation is defined by unequal access to citizenship, focusing on the child with citizenship and older siblings. It addresses educational resources, such as computers and study materials, and time investments, like participation in school events or studying at home. Given the central role of parental investments in children's skill development (e.g., Becker, 1965; Hill and Stafford, 1974), these investigations are crucial for advancing our understanding of parental decision-making. By distinguishing between various forms of investments and examining how each is shaped by different social policies, this dissertation provides an important extension of the existing literature, especially as parents remain an often-overlooked factor in evaluations.

Connected to this, this dissertation investigates a second form of within-family investments. **Chapter 5** focuses on investments made by grandparents in the form of child care and demonstrates their impact on the well-being of both parents and children. The findings indicate that within-family investments are a crucial mechanism underlying interdependencies and transmissions among family members. Consequently, the results offer valuable insights for effective policymaking, as grandparental child care

represents a major source of care in many countries but is often overlooked in policy discussions that primarily emphasize formal daycare and school provision.

Additionally, the dissertation investigate other important parental outcomes with implications for children's human capital. **Chapter 2** examines how school-provided information shapes parental beliefs – an important determinant of parental investments (Francesconi and Heckman, 2016) and, in turn, children's skill development. This chapter extends the existing literature by exploring parental responses to different forms of school report cards in the crucial first year of school. **Chapter 3** provides first evidence on the labor supply responses of single mothers to public child support payments, thereby examining an outcome with important implications for household income and child development. Meanwhile, **Chapter 4** investigates how parents' naturalization behavior responds to their children's access to citizenship. Since parental citizenship affects family integration and their children's citizenship status, these decisions have important long-term implications for children. Finally, **Chapter 5** examines the impact of grandparental care on parental well-being. Maternal well-being, in particular, not only directly affects child outcomes (Datta Gupta et al., 2023) but is also increasingly recognized as a policy priority (see, e.g., Helliwell et al., 2024; OECD, 2023a; United Nations, 2022). This chapter is the first to investigate parental well-being in the context of grandparental care.

A second strand of literature to which this dissertation contributes concerns the **human capital development of children**. Human capital plays a central role in shaping life outcomes, and early-life conditions are widely recognized as critical determinants of long-term outcomes (e.g., Card, 1999; Hanushek and Woessmann, 2012; Heckman and Mosso, 2014). In addition to examining indirect effects on children that operate through changes in parental behavior, beliefs, and well-being, **Chapters 4 and 5** analyze direct effects on children themselves. **Chapter 4** examines educational attainment, specifically whether children complete secondary school with the highest degree. This measure is of particular relevance in Germany where school-leaving qualifications determine eligibility for tertiary education and channel students into distinct vocational or academic pathways. Obtaining the highest secondary degree increases chances of securing higher-paying occupations (Nordin et al., 2020). The chapter builds on existing literature which has demonstrated positive short and medium term effects of citizenship on children's education (e.g., Cygan-Rehm, 2018; Felfe et al., 2020; Gathmann et al., 2021; Sajons and Clots-Figueras, 2014). However, some students may catch up later or overly ambitious goals might not be realized, which makes an investigation of the development over the entire period of secondary school necessary. This chapter is the first to study the completion of secondary school. Children's health – investigated

in **Chapter 5** – is not only a valuable outcome in itself but also a strong predictor of later educational achievement and labor market success (e.g., Currie, 2020). Despite its importance, the potential influence of grandparental care on child health has not been systematically studied before.

In addition to these two strands of outcomes, this dissertation extends the existing literature by examining how social policies and within-family investments affect individuals who are not their primary intended targets. While a growing body of research explores **spillover effects within families** (e.g., Bettinger et al., 2014; Dahl et al., 2024; Karbownik and Oezek, 2023; Landerso et al., 2020), these indirect effects remain relatively underexplored. **Chapter 2** studies the impact of school report cards, which are often designed with the children in mind, on the beliefs and behaviors of the children's parents. **Chapter 3** evaluates public child support payments aimed at alleviating poverty among children of separated parents. While these transfers are explicitly directed at children, the chapter focuses on parental responses, specifically, how resident parents adjust their labor supply and expenditure decisions and how non-resident parents adjust their private child support payments. **Chapter 4** investigates the effects of children's access to citizenship, assessing not only its influence on the children's own educational attainment but also on other family members. This includes its impact on parental behaviors, such as naturalization and educational investments, and on the education and naturalization outcomes of older siblings. Finally, **Chapter 5** analyzes not only how grandparent-provided child care influences the children receiving care but also how it affects the well-being of their parents. Examining effects on all potentially affected family members is essential for forming a complete picture of the costs and benefits of social policies and caregiving arrangements. Given the substantial interdependencies within families, it is likely that most policies and environments influence multiple family members, either directly or indirectly (Lundberg and Pollak, 2007).

The dissertation additionally contributes to the literature by placing particular emphasis on **disadvantaged families**, a subgroup of the population that is both highly relevant for policy-making and empirically challenging to study. Because such families constitute only a (small) share of the population, they are often underrepresented in large-scale datasets, making it difficult to obtain precise and causal estimates of their behaviors and outcomes. This dissertation addresses that gap by drawing on rich, representative survey data and exploiting policy changes that disproportionately affect these households. Several chapters specifically focus on disadvantaged groups. **Chapter 2** examines parental misperceptions of children's skills, which are more prevalent among less-advantaged families, and investigates whether school-provided information

can mitigate these gaps in beliefs and investments. **Chapter 3** analyzes single-parent households, a group at high risk of poverty, social exclusion, and material deprivation. It assesses how public child support payments influence their financial well-being, labor supply, and spending patterns. **Chapter 4** studies the impact of birthright citizenship on the education and behavior of immigrant families, who on average have lower educational attainment, earnings, and employment rates compared to non-immigrant families (Algan et al., 2010). By explicitly examining these groups across multiple policy and family contexts, the dissertation offers novel evidence on how different social policies influence families most at risk of economic and social disadvantage, thereby helping to inform effective interventions.

In addition to these four main contributions, this dissertation makes three further substantial contributions related to the approaches, datasets, and contexts.

By examining a broad range of age groups and educational stages – from early childhood through adolescence and into early adulthood – this dissertation contributes to a more comprehensive understanding of how policy interventions and family environments shape children's developmental trajectories and long-term outcomes. **Chapter 2** investigates children in the first grade of elementary school, typically aged 6 to 7. **Chapter 3** analyzes adolescents between the ages of 12 and 17 years. **Chapter 4** explores the long-term effects of birthright citizenship on children from grade 5 through the end of high school, covering an age range of approximately 10 to 21 years. Finally, **Chapter 5** examines children from birth up to age 10. The chapters focus on early childhood and school years and span the different stages to capture the dynamic process of skill formation, in which early investments are most effective when reinforced by subsequent ones (Cunha and Heckman, 2007).

Drawing on a range of high-quality survey datasets allows the inclusion of subjective measures reported by individuals, such as parental time use, beliefs, and well-being, which are often unavailable in administrative records. This enables the analysis of detailed outcomes alongside objective indicators. The NEPS, used in **Chapters 2 and 4**, provides exceptionally detailed educational data, including rich information on parental investments and beliefs, for multiple age groups. The EVS, employed in **Chapter 3**, contains an unparalleled record of household income and expenditure patterns, which is not available in any other German dataset, and which makes it uniquely suited to study financial decision-making in single-parent households. The pairfam data used in **Chapter 5** offers detailed insights into family dynamics and well-being, enabling an analysis of how grandparental child care influences both parents and children. The use of a range of complementary datasets allows each chapter to draw

on the most suitable data for the studied population – especially with the focus on disadvantaged and underrepresented groups. **Chapters 3 and 4** are based on MZ data – the largest survey in Germany, covering 1% of the population – whose scale enables the study of relatively small subgroups such as single mothers and immigrant families. **Chapter 3** is also based on the EVS, which is the largest of its kind in the European Union and a quota sample, ensuring sufficient coverage of single mothers for detailed expenditure analyses. **Chapters 2 and 4** use the NEPS, which follows specific cohorts over time and thus provides large subgroup samples. This design yields, for example, a sufficiently large number of children affected by the German citizenship reform in **Chapter 4**, enabling precise causal estimation.

All chapters in this dissertation deepen our understanding of the German context while shedding light on social policies and within-family investments. Germany is both comparable to other European countries and distinct from the Anglo-American contexts that dominate much of the literature, making it a valuable case for transferable and complementary insights. **Chapter 2** shows that numerical grades in school report cards are more effective than qualitative feedback in guiding parental investments. As many European countries introduce numerical grades only later in elementary school (European Commission Directorate-General for Education, Youth, Sport and Culture and Eurydice, 2025), these findings are relevant beyond Germany. **Chapter 3** evaluates advance child support systems, which are prevalent across Europe. It shows that higher public child support payments – intended to be recovered from non-resident fathers – lead to higher private payments, suggesting stronger compliance when enforcement is state-led. This has implications for countries such as the US and UK, where the current focus is on enforcement rather than advance child support (Hakovirta and Mesiäislehto, 2022; Pilarz and Cuesta, 2025). **Chapter 4** demonstrates that granting birthright citizenship to one child benefits the whole family, including siblings' education. As of 2020, 31% of states worldwide granted birthright citizenship, allowing children born in the country to acquire citizenship regardless of their parents' citizenship (Gathmann and Garbers, 2023), making these findings relevant across contexts. Studying grandparental care in Germany in **Chapter 5** makes the findings applicable to other countries, as formal child care trends reflect those seen elsewhere, while informal child care usage aligns with the European average (Zanasi et al., 2023). The dissertation's focus on disadvantaged families is particularly relevant in the German context, where substantial inequalities exist (e.g., OECD, 2023c), despite the country's generous social welfare system (e.g., OECD, 2025b).

CHAPTER 2

How Information Affects Parents' Beliefs and Behavior: Evidence from First-Time Report Cards for German School Children[1]

2.1 Introduction

The importance of parental investments in children's skill development has long been recognized by economists (e.g., Becker, 1965; Hill and Stafford, 1974; Leibowitz, 1974, 1977) and other social scientists (e.g., Coleman et al., 1966). Parents' knowledge and beliefs about their child's academic achievement shape parents' decisions about investing in their child's education (e.g., Francesconi and Heckman, 2016). The standard economic model assumes that parents have perfect information about their children's actual performance when investing in children's learning. But many parents misperceive their child's academic progress, as seen in the large share of parents who overestimate their children's performance (Dizon-Ross, 2019). Parents who overestimate their children's performance spend less time in learning activities with their children (Kinsler and Pavan, 2021). As misinformation is worse for less-advantaged families (Boneva and Rauh, 2018), parents' imperfect information can increase educational inequalities.

[1]This chapter is joint work with Ariel Kalil (University of Chicago). This work was supported by the Joachim Herz Foundation. This paper uses data from the National Educational Panel Study (NEPS; see Blossfeld Roßbach, 2019), Remote Access. The NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi, Germany) in cooperation with a nationwide network. This chapter was pre-published as a working paper as *Ziege, E., and Kalil, A. (2025). How Information Affects Parents' Beliefs and Behavior: Evidence from First-Time Report Cards for German School Children. University of Chicago, Becker Friedman Institute for Economics Working Paper, 2025-54.*

Early childhood is a critical period for child skill development, and parents are theorized to be the primary drivers of these skills (Cunha and Heckman, 2007). However, there is almost no evidence in the social sciences on the causal effect of information treatments on parents' investments in children's skills in the initial school year. This lack of evidence is unfortunate because children's skill development is especially malleable in the early years and because of the dynamic complementarity between early and later skill-building (Heckman, 2006).

This paper uses behavioral insights to better understand how schools as organizations can communicate with parents. We aim to investigate the causal impact of teacher-provided information about children's achievements on parents' beliefs and investments (i.e., time spent with children in learning activities) in the crucial first year of formal schooling. We do so by exploiting differences across federal states in Germany in the timing and nature of information provided to parents about children's performance. Due to its federal structure and the German school system, the German setting offers a rare laboratory to examine these questions within a common political and economic framework.

Prior research suggests that resolving parental misbeliefs about students' test scores, grades and school-related activities such as absences and uncompleted homework can increase parental investments, change child behaviors, and improve child skills (see Andrabi et al., 2017; Barrera-Osorio et al., 2020; Bergman, 2021; Bergman and Chan, 2021; Cobb-Clark et al., 2021; De Walque and Valente, 2023; Dizon-Ross, 2019; Gan, 2021; Kraft and Rogers, 2015; Robinson et al., 2018). There are several pathways through which this might occur. First, information from the school might correct parents' biased beliefs or overconfidence, and this information theoretically increases parental investments in a compensatory fashion. Information might also increase investments without necessarily changing beliefs if it serves simply as a reminder about the importance of education and the child's academic experiences, i.e., a priming effect. Bringing the child's skills to parents' top of mind could increase investments.

Nonetheless, it is also possible that no or negative effects on parental investments will arise from providing parents with information about their child's achievement. Parents may engage in motivated reasoning and ignore information about their child's achievement if the information is ego-depleting or identity-diminishing. A negative surprise about the child's achievement could be ego-depleting for parents or increase anxiety, which could depress the quantity or quality of parent-child engagement. Very negative information could anger or upset parents, with a similar possibility of ill effects on parental beliefs and behaviors. Parents may think report cards are biased,

subjective, or unrelated to relevant future outcomes. Motivated reasoning has been well-established in the literature (Amelio and Zimmermann, 2023; Bénabou and Tirole, 2002; Ryan and Deci, 2000; Zimmermann, 2020). Our unique data allow us to distinguish between parents who likely received a "negative" surprise from their counterparts who did not.

Better information will not yield more optimal investments if parents have misbeliefs about the production function of child skill development (see Cunha et al., 2013). Parents may underestimate the returns to investments for children performing poorly relative to children performing well (Caucutt et al., 2017). This underestimation likely stems from the fact that many people have a "fixed mindset" – i.e., a belief that individuals are born with specific invariant characteristics that cannot change with experience – instead of a "growth mindset" – i.e., a belief that performance can be improved over time (Dweck, 2006). Because performance can improve with investment, the growth mindset is more appropriate. However, having a fixed mindset could cause some parents to give up on children they find out are low-performing instead of making the (high-return) investments in those children. Higher-SES parents have more of a growth mindset (Agee and Crocker, 1996; Boneva and Rauh, 2018), and a growth mindset is positively correlated with parental investments in children's learning and greater child achievement (Andersen and Nielsen, 2016; Haimovitz and Dweck, 2016; Rowe and Leech, 2019). This paper examines the role of parental education and immigration status in treatment heterogeneity. Immigrants to Germany are, on average, less educated and have lower incomes than native Germans (Straub et al., 2024).

Beyond whether providing information changes parent behavior, an important question concerns how to convey that information. It is well-known that information alone seldom changes behavior; one reason is inattention to information. Modifying the design and presentation of information (in modality, frequency, and appearance) may affect whether the information captures individuals' attention and prompts behavior change (Allcott and Rogers, 2014; Castleman, 2015; Shah et al., 2025; Thaler and Sunstein, 2009). Chang et al. (2024) convincingly show that individuals prefer quantitative information, experimentally choosing the options that dominate on the dimension described numerically. This finding suggests that parental investments may respond more strongly to student achievement data presented numerically rather than verbally. The German federal structure allows us to compare states using numerical grades and scales with states using qualitative progress reports to investigate the importance of the information modality.

This paper demonstrates that more than 40 % of parents in Germany overestimate their child's skills.[2] Our analysis of school report cards shows that information provided by teachers has only a limited impact on parents' beliefs. On average, school report cards do not reduce parental misperceptions. However, our study reveals that school report cards can influence parents' behavior without significantly altering their beliefs. Specifically, parents substantially increase the frequency of reading with their children after receiving school report cards. We also emphasize the role of teacher bias, noting that approximately one third of teachers tend to overestimate their students' skills. Notably, report cards only mitigate parental overestimation and enhance parental investments when they are unbiased. Furthermore, we illustrate that the format and timing of the information presentation are crucial within the institutional context. Parents who receive information in the form of numerical grades or scales are more likely to decrease their overestimation and increase their engagement in reading and playing. Our analysis indicates that the initial report card is particularly effective in changing parental beliefs and behaviors. The results remain robust even after excluding parents who are either very easy or very hard to contact, as these groups may selectively fall into the pre- or post-treatment categories based on their availability. Finally, we find that increases in parental investments are only observed among parents who likely received a negative report card, rather than among all parents.

Four prior papers are especially relevant to our work. First, Cobb-Clark et al. (2021) used Australian data to show that parents of children in grades 3-9 who received their child's standardized test scores increased private tutoring and reduced their child's leisure time in extracurricular activities, especially in families where parents received unexpected negative information. At the same time, information did not impact the number of activities parents and children do together, suggesting that parents of older children outsource support for children's learning. Our paper looks at younger children at a developmental stage when parent engagement is paramount for children's skill development. It also has the advantage of investigating the first instance in which parents receive formalized information on their child' skills as our focus is the first year of elementary school and daycare centers do not distribute report cards. Using international data from the PISA, Bergbauer et al. (2018) compared the effect of introducing standardized tests that compare outcomes across schools and students versus the impact of localized, internal test information (i.e., report cards) for 15-year-olds, finding treatment impacts on child achievement for the former and not the latter. Again, these results may not extend to young children in their first year of formal schooling,

[2]We also show that around 20 % of parents underestimate their child's skills. Given that this proportion is substantially smaller than the proportion overestimating and most of the existing theory focuses on overconfidence, we concentrate our analysis on the effects on parental overestimation.

for whom there are no national standardized tests available in Germany. Andrabi et al. (2017) tested the impact of introducing report cards with individual and school-level data in a large population of Pakistani 3rd graders in rural provinces. The treatment increased subsequent student test scores by 0.11 SD. Survey data show that the treatment did not arise through changes in parents' time and money investments but rather through changes in teacher behavior. Nonetheless, the political and economic context of the low-literacy rural setting in that study is not comparable to the population in our German data. Finally, in an unpublished dissertation using the NEPS (the same data we use here), Penny (2019) studied grading reforms in the 1970s that affected cohorts born between 1944 and 1987 in West Germany. Penny assesses the change from a six-point numerical grading scale to a general verbal assessment on adult retrospective reports of their childhood schooling transitions. In general, this policy change had few effects on educational attainment. Our research uses contemporary data to study this question.

This research represents an important extension of the prior literature on parental responses to new information about their child's achievements. To our knowledge, this is the first paper that examines the impact of an information treatment in the crucial first year of children's formal schooling. Second, we examine whether the modality of the information (numeric or verbal) moderates the impact of the treatment. Modality is relevant both for understanding attention processes and for contemporary international policy debates. Third, our unique data allow us to compare the effect of the first to the second information treatment. Doing so allows for new insights into novelty effects. To our knowledge, no other study has investigated this question. Fourth, our data will enable us to test relevant treatment heterogeneity by parental education and immigrant status. Parents with low education and economically disadvantaged immigrant parents may be especially likely to overestimate their child's skills due to a lack of knowledge about the German school system and thus increase their investments in light of report card information. Finally, we can distinguish whether the information conveyed in this initial report card likely contains a positive or a negative surprise. We do this with unique survey data that includes evaluations of the child's skills from parents and teachers and children's scores from a standardized test conducted as part of the survey.

This topic is especially timely as educators and researchers in Germany are actively debating the merits of numerical report card grades versus qualitative progress reports (e.g., Hübner et al., 2024; Olbrisch, 2024). Some stakeholders argue that numerical grades offer limited information and lack comparability (e.g., Brügelmann, 2006). In response, several German federal states have initiated pilot programs allowing selected schools to refrain from using numerical grades in the early years of schooling –

sometimes extending this practice up to grade nine (e.g., Bürgerschaft der Freien und Hansestadt Hamburg, 2024; Landtag Baden-Württemberg, 2022).

This discussion is not unique to Germany. In several other countries – particularly across Europe – numerical grades are introduced only in the later years of elementary school.[3] Many countries have also revised their grading policies over time, reflecting broader political and societal debates about assessment and educational equity. Sweden, for example, previously began grading in year eight but now introduces numerical grades from year six onward. Similarly, Luxembourg did not use numerical grades in elementary school until a policy change in 2016. Our findings indicate that the grading system adopted in many countries only in later school years – numerical grades or performance scales – is precisely the one that drives parental investment for those children who could benefit the most.

We organize the rest of this article as follows: Section 2.2 details the context of report cards in Germany. Section 2.3 describes the data and empirical strategy. In Section 2.4, we present the estimates of the main impacts alongside heterogeneity analyses and robustness checks. Section 2.5 discusses the interpretation of our treatment effect estimates and concludes.

2.2 Report Cards in German Elementary Schools

In Germany, children generally receive two report cards per school year: one in winter and one in summer. This paper focuses on the summer report card, distributed at the end of the school year.

The distribution of report cards occurs at staggered times across different federal states, coinciding with the start of the summer holidays, which also begin at varying times. The staggered schedule of the summer holidays aims to mitigate adverse effects on traffic and the demand for accommodations in holiday destinations. The Standing Conference of the Ministers of Education and Cultural Affairs of the Länder establishes the schedule (Kultusministerkonferenz, 2012). This system divides the federal states into five groups: (i) Brandenburg, Berlin, Hamburg, Mecklenburg-Vorpommern, and Schleswig-Holstein; (ii) Bremen, Lower Saxony, Saxony, Saxony-Anhalt, and Thuringia; (iii) North Rhine-Westphalia; (iv) Hesse, Rhineland-Palatinate, and Saarland; and (v)

[3] In several European countries, the use of numerical grades becomes compulsory in later years of primary or lower secondary education. These include: Bulgaria and Latvia (from year 2); Greece, Malta, Montenegro, and Switzerland (from year 3); Finland (from year 4); Ukraine (from year 5); Estonia, Italy, Liechtenstein, and Sweden (from year 6); and Denmark and Norway (from grade 8, see European Commission Directorate-General for Education, Youth, Sport and Culture and Eurydice, 2025).

Baden-Württemberg and Bavaria (Kultusministerkonferenz, 1999). In 2013, the year examined in this paper, report cards were distributed between June 18 and July 30. Berlin was the first federal state to distribute report cards in the summer of 2013, on June 18, while Bavaria was the last, with report cards issued on July 30. We illustrate the specific dates of distribution in Figure 2.1.

The summer report card includes teachers' assessments of the child's school performance. Teachers evaluate the child's success in various subjects and their social skills. In the first year of elementary school in 2013, report cards in nearly all states provided a verbal description[4] of the child's progress. One state (Baden-Württemberg) used numerical grades ranging from 1 (best grade) to 6 (worst grade), while two states (Berlin and Schleswig-Holstein) let schools decide whether they use a verbal description or a scale from 1 to 4 for each skill. Additionally, one state (Saarland) issued report cards with verbal descriptions and required teachers to hold one-on-one meetings with parents in conjunction with the report card.

In addition to the end-of-year report card, children in Germany receive a report card after the first half of the school year (in winter). This winter report card is distributed across all federal states between the end of January and the beginning of February. However, in six federal states, children do not receive a report card in winter during their first year of school, and in two federal states (Schleswig-Holstein and Hamburg), schools can decide whether to distribute report cards in winter.

2.3 Data and Empirical Strategy

2.3.1 National Educational Panel Study

The analysis is based on the German National Educational Panel Study (NEPS) starting cohort 2. The NEPS is a multi-cohort panel study that tracks the educational paths of six cohorts of children and adults in Germany. The starting cohort 2 began with a survey of children in daycare centers in 2011. Our analysis focuses on data from the third wave, conducted in 2013, when these children entered elementary school. This wave's sample includes children surveyed in the first two waves, along with a refresh-

[4]In most states, this verbal description is based on templates that teachers can choose from, depending on the child's skill level. For instance, in the topic of "reading aloud," a child with the highest skill level (level 1) would receive the description, "$First name$ can read unknown texts fluently and independently." A child with slightly lower skills (level 2) would be described as, "$First name$ can read unknown texts independently." Conversely, a child with the lowest skills (level 5) would receive the description, "$First name$ does not yet know all the letters and their corresponding sounds, so $First name$ can only read short, familiar, phonetic words." Descriptions for levels 3 and 4 would fall between these examples. The templates differ between federal states.

ment sample (Hellrung et al., 2013). The existing sample from the previous waves experienced a high rate of panel attrition, limiting the observation of most children from daycare centers in first grade. However, the refreshment sample sampled entire first-grade classes, providing a representative sample of elementary school children in Germany and enabling comparison among classmates. At the time of the survey, the children in this wave were on average seven to eight years old.

In wave 3 of NEPS starting cohort 2, surveys were administered to elementary school students, their parents, teachers, and school headmasters. As part of the student survey, several competency tests were conducted, the results of which we utilize in this paper. Students were surveyed and tested between February 18 and June 20.[5] Parents were surveyed between May and October 2013. This interview was announced via letter and subsequently conducted over the phone. Notably, the timing of the parents' survey was independent of the distribution of report cards. In 90.13 % of cases, mothers participated in the survey.[6] Teachers were surveyed concurrently with their students' participation in the competency tests, that is, also between February 18 and June 20.

We restrict our sample to children in classes where at least 50 % of the students participated in the NEPS survey and competency tests, representing about a quarter of the surveyed children. The rationale behind this restriction is that we can only accurately assess a child's rank within their class if we know the abilities of a sufficient number of other students in the class. This leaves us with a sample size of 1,316 to 1,531 parents for the different outcomes.

2.3.2 Children's Competency Tests

During the third wave of the NEPS, various tests were conducted in school over two days to assess children's competencies. The tests lasted 105 minutes and employed a picture-based answer format (FDZ-LIFBI, 2019). We focus on the results from four specific tests: vocabulary, grammar, mathematical competence, and scientific literacy. Vocabulary competencies were evaluated through a 20-minute test that assessed students' listening comprehension at the word level. This test, conducted in the NEPS, is analogous to the Peabody Picture Vocabulary Test and requires students to select the correct picture corresponding to each predetermined word from a set of four options. Grammar skills, specifically students' listening comprehensions at the sentence

[5]We exclude 68 children who were tested close to the date of report card distribution, as they might have been tested after receiving their report cards, which could influence their self-esteem and, consequently, their test performance.

[6]If we exclude any fathers who responded to the survey, our results are very similar (see panel D of Tables 2.A.3 and 2.A.5 in Appendix 2.A.2).

level, were examined in a 15-minute test. This assessment corresponds to a shortened TROG-D "Tests for Reviewing Grammatical Understanding" (Fox-Boyer, 2006). In this test, a sentence is read aloud, and students must choose the correct picture from four displayed options. A 30-minute test was employed to assess students' mathematical competence based on the concept of mathematical literacy as defined in frameworks such as PISA. The final test evaluated students' scientific literacy. This test also lasted 30 minutes and adhered to the PISA framework. We utilize students' scores from these tests to calculate their ordinal rank within their class.

2.3.3 Parental Overestimation and Investments

To measure parental overestimation, we compare parents' beliefs about their child's skills with the child's rank in the NEPS competency tests. Parents evaluate their child's skills relative to children of the same age on a scale from 1 (much poorer) to 5 (much better). Specifically, we compare parents' evaluations of German language skills with the child's rank in vocabulary and grammar skills tests. Additionally, we analyze parental estimations of math skills relative to the child's rank in the math test and scientific skill evaluations (i.e., knowledge about animals, plants, and the environment) with the child's rank in the science test. Binary variables define the outcomes capturing overestimation, equal to 1 if (i) the parent reports that the child is at least as skilled in the field as other children of the same age while the child's ordinal rank is below the median, or (ii) the parent reports that the child possesses higher skills in the field than their peers and the child's ordinal rank is below or equal to the median rank. In all other cases, the variable is equal to 0.[7]

As a second group of outcomes, we investigate parental investments, measured by the frequency with which parents report undertaking certain activities with their children. In the NEPS survey, parents report how often they or another household member engage in various activities at home with their child. These activities include reading aloud, telling stories (e.g., made-up or retold fairy tales), listening to or making music, painting, drawing, or doing crafts, playing sports or games that involve physical activity, playing with the child, and visiting the playground or outdoor areas. Response options range from 1 (never) to 8 (several times daily). Since these measures are self-reported, they may be influenced by social desirability bias, reflecting more what parents claim to do than their actual behaviors. Nonetheless, they provide insights into what parents consider essential.

[7]Using an ordinal variable representing parental beliefs in relation to the child's rank in the competency test as the outcome variable produces results that are very similar to those of our main specification. This result is available upon request.

2.3.4 Teacher's Ranking of Child's Skills

Teachers of the sampled classes assess each child's abilities and skills compared to those of other children of the same age. Specifically, teachers evaluate the child's language skills (e.g., vocabulary, sentence construction), written language abilities (e.g., ability to understand and write texts), scientific abilities (e.g., knowledge of animals, plants and the environment), and mathematical skills (e.g., working with numbers and amounts). Just like the parents, they classify these skills from 1 (much worse) to 5 (much better) compared to other children of the same age. We use the teachers' evaluation of the child's skills relative to other children of the same age as a proxy for the type of report card the parents would receive. Since teachers were surveyed in the months leading up to the distribution of report cards, we can reasonably assume that these evaluations are a good approximation of the actual report card evaluations.[8]

2.3.5 Empirical Strategy

To estimate the effect of information provision by teachers on parents, we estimate the following OLS regression:

$$Y_i = \alpha + \beta \, \text{Post}_m + X_i'\gamma + \mu_m + \delta_c + \zeta_y + \tau_s + \rho_t + \varepsilon_{im} \tag{2.1}$$

Where Y_i corresponds to the outcome variables: parental overestimation and investment as described in Section 2.3.3. To understand how the school's information affects parental beliefs and behaviors, we leverage the variation in the timing of report card distribution across German federal states, as outlined in Section 2.2, alongside the timing of parental surveys conducted around the distribution. The variable $Post_m$ measures whether the parents were surveyed before or after receiving information from teachers about their children's academic success. It equals 1 for parents surveyed after the information provision and 0 for those surveyed before. The timing of the parental survey is known only every month. Therefore, we exclude parents surveyed in the same month as the report card distribution unless the report card was distributed before the 6^{th} or after the 24^{th} day of the month. In these cases, we assume that parents were surveyed after the 5^{th} – and are thus classified as post-treatment – or before the 24^{th} – and are classified as pre-treatment. This quasi-experimental variation in the timing

[8]School laws in Germany stipulate that teachers grade students according to predefined standards or learning objectives. However, in some federal states, these laws also permit teachers to consider the performance level of the learning group (see, for example, § 57 BbgSchul). Consequently, we assume that teachers' evaluations serve as a reliable approximation of school grades.

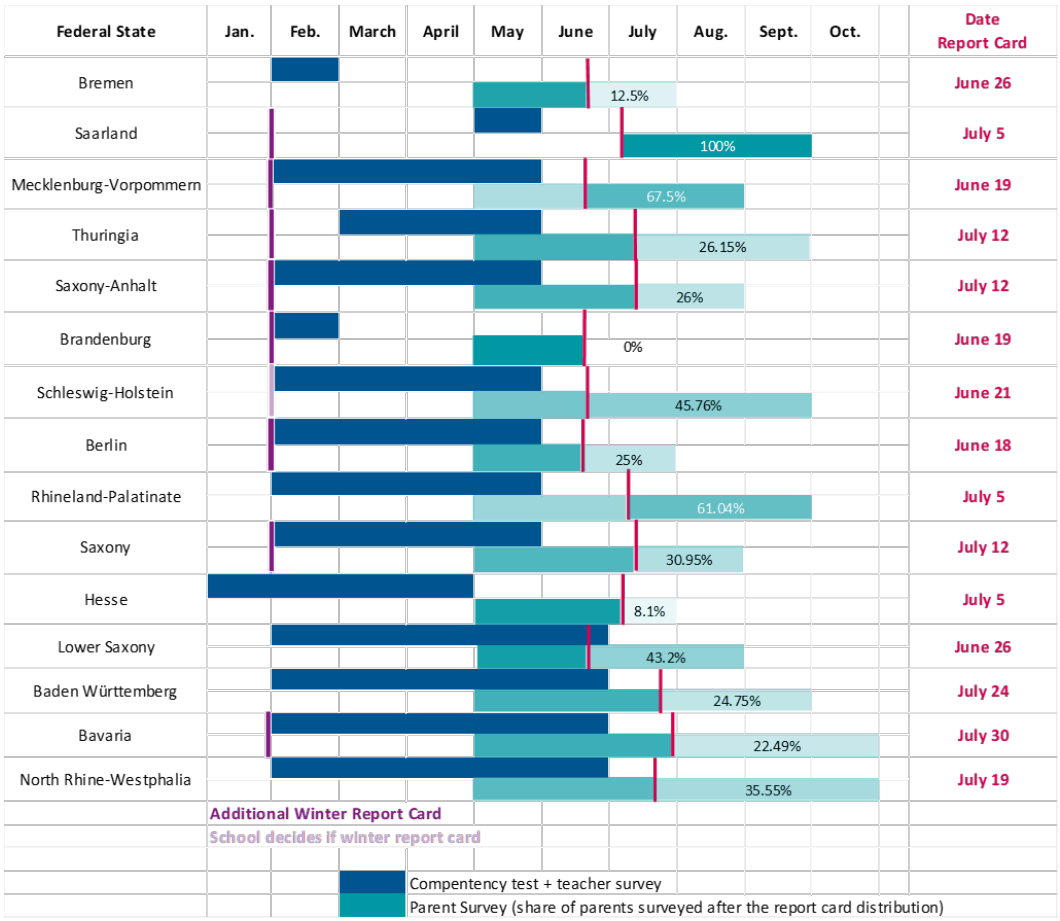
of information allows us to compare parents' perceptions of their child's skills before and after receiving the teacher's evaluation. We include a monthly fixed effect (μ_m) for the parental survey to account for the timing within the school year when parents are surveyed. As the school year progresses, parents may receive more information from the school regarding different learning activities with their child, such as reading. Additionally, the regressions incorporate class room fixed effects (δ_c) to control for specifics related to the classroom setting and the teacher. Additionally, the regressions incorporate fixed effects for the child's birth year (ζ_y), the federal state (τ_s) and the number of contact attempts necessary to survey the parents (ρ_t). This ensures that parents who are more challenging to reach and may constitute a selected group do not bias the analysis. The vector of control variables X_i includes the mother's education and labor force status, the child's immigrant status, the child's gender, and the child's ordinal rank within class.[9] We cluster the standard errors at the classroom level. This approach is necessary because children are sampled within the classroom context, which may lead to correlated standard errors within a single class. The regressions are weighted to account for regional characteristics that are not available in the NEPS but were utilized in constructing the weights.

Figure 2.1 presents the setup of this methodology. It illustrates the date of report card distribution in each federal state and the period during which students were tested and teachers and parents were surveyed. The student competency tests and the teacher survey occur before the distribution of report cards. The timing of parental surveys, conducted from May to October, varies across federal states, resulting in different proportions of pre- and post-treatment parents in each state. In our sample, 34 % of parents were surveyed after their child received the report card, while 66 % were surveyed beforehand. The smallest share of parents surveyed after the school's information treatment is observed in Brandenburg, where all parents were surveyed before issuing report cards. In contrast, the highest share is found in Saarland, where 100 % of parents were surveyed after the distribution of report cards. The federal states are ordered by population size; for example, Saarland constitutes only 1 % of the German population, and Brandenburg makes up 3 %. Although these two states offer no identifying variation in interview timing relative to report card distribution, their limited population size means that their influence on the overall analysis is minimal. Notably, in the German states with the largest populations, a substantial share of parents is surveyed before and after the report card distribution. This variation in timing allows us to identify the effect of the information treatment. The figure also

[9]When analyzing effects on parental investments, we control for the child's rank in the tests assessing German skills.

indicates which states issue report cards only in summer and which distribute them in summer and winter. Approximately half of the states distribute report cards in winter, while the other half only do so in summer.[10] This distinction enables us to compare the effects of the first report cards that parents receive with those of the second report card, allowing conclusions about novelty effects.

Figure 2.1: Dates of Report Card Distribution and NEPS Surveys



Notes: This figure displays when report cards were distributed in different German federal states relative to the NEPS surveys. The dark blue bars show the period of the competency tests, and the green bars show the period of the parent survey. The percentage shows how many parents were surveyed in the post-treatment period in this state. The red line shows the timing of the report card distribution, and the purple line reflects whether schools in this state also distributed a report card in winter. *Source:* Own visualization based on information by the Ministries of Education and Cultural Affairs of the Länder and NEPS SC2 (2013).

Identifying a causal effect of report cards on parental overestimation and investment relies on two assumptions. First, the timing of the parent interviews is unrelated to

[10]Schleswig-Holstein allows schools to decide whether to issue report cards in winter, a policy also observed in Hamburg. However, due to the sample restrictions outlined in Section 2.3.1, our sample does not include any families residing in Hamburg. It is worth noting that the population of Hamburg represents 2 % of the total German population.

the report card date. This assumption is plausible, as the NEPS survey structure was designed so that parents were surveyed in two episodes: from mid-May until the end of July and from the end of July until the beginning of October.

To further test this assumption, we compare various pre-determined characteristics of families in the pre- and post-treatment groups, as shown in Table 2.1. We observe that the two groups are balanced across most predetermined characteristics. However, children in the post-treatment group are, on average, two and a half months older than those in the pre-treatment group. This difference arises from the sample definition, as parents in the post-treatment group are surveyed later than those in the pre-treatment group. As expected, parents in the post-treatment sample needed to be contacted significantly more often before participating in the survey, highlighting the importance of controlling for the number of attempts required to survey the parents. As a robustness test, we also exclude parents who required the most (and the fewest) contact attempts to mitigate potential sample selection bias, demonstrating that our results are robust to this sample definition (see Tables 2.A.4 and 2.A.6 in the Appendix).

Table 2.1: Comparison of Pre- and Post-Treatment Groups

Variable	Pre-treatment	Post-treatment	Difference	
Mother has university degree	0.26 (0.44)	0.21 (0.41)	-0.051**	-0.038
Immigrant student	0.22 (0.42)	0.20 (0.40)	-0.024	-0.003
Child is female	0.50 (0.50)	0.51 (0.50)	0.006	0.011
Child's age (in months)	87.09 (4.7)	89.72 (4.79)	2.629***	2.529***
Two-parent household	0.91 (0.29)	0.88 (0.32)	-0.026	-0.022
Mother works full-time	0.14 (0.35)	0.15 (0.35)	0.007	-0.003
Attempts to survey parents	5.08 (5.32)	9.33 (10.58)	4.251***	4.876***
Observations	951	566		
Controlling for Federal State				✓

Notes: The table displays the averages in pre-determined variables for the pre- and post-report card groups and the difference between the two groups plus the difference when controlling for the federal state. Standard deviations in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. *Source:* NEPS SC2 (2013), own calculations.

Additionally, there is a slight difference between the two groups regarding mothers' education: the share of mothers with a bachelor's degree in the pre-treatment sample is five percentage points higher than in the post-treatment sample. Following Cobb-Clark et al. (2021), we control for the federal state in the next step, as the federal state influenced the NEPS sampling framework. When controlling for the federal state, there is no longer a statistically significant difference in mothers' education.

Second, our approach relies on the assumption that overestimation and investment would not have evolved differently for pre- and post-report card interviews in the absence of the report cards. One concern regarding this assumption is that most parents surveyed in the post period are interviewed during the school holidays. While this timing should not affect their beliefs, it may influence their behavior, as they might have more time to spend with their children if they are on vacation themselves or less time if children attend summer camps. Previous research indicates that US educational inequalities increase during vacation periods (e.g., Alexander et al., 2007; Downey et al., 2004). Additionally, parental time allocation varies between school and vacation periods (e.g., Cowan et al., 2023, for the US). Consequently, different socio-economic groups of parents may vary in their investment strategies during these breaks. However, the results in our main specification suggest increases in some activities and decreases in others, indicating that there was not an overall increase in time spent with children, which supports our assumption.

We conduct a sensitivity test in Section 2.4.4, comparing parents who received a negative with parents who received a positive information treatment. The rationale behind this test is that only parents who receive negative information should change their behavior. We do so by comparing two subsamples: parents whose children were ranked by teachers as lower than their peers (low relative rank) and parents of children ranked as the same as or better than their peers (high relative rank). A child ranked lower than the peers likely also received a less favorable report card, indicating that the parents received negative information about their child's performance. This test demonstrates that parental reading investments increased only for the subsample of parents who received a negative information treatment and strengthens our argument (see Tables 2.A.3 and 2.A.5 in the Appendix).

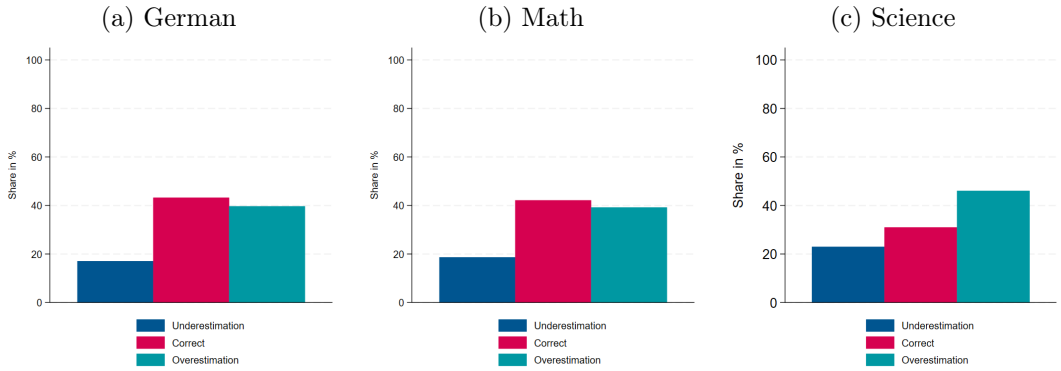
2.4 Results

2.4.1 Parental Overestimation and Investments

As a first step, we investigate how parental overestimation and investments are distributed within our sample and whether there are differences among various groups. Figure 2.2 distinguishes parental beliefs by whether the parents underestimate, correctly estimate or overestimate their child's skills. This is shown for (a) German, (b) math and (c) science. The figure illustrates that nearly 40 % of parents overestimate their child's skills in German and math. Specifically, about 40 % of parents report that their child is performing at least as well as same-aged children, despite the child

achieving an ordinal rank below the median on the NEPS test, or that their child is performing better than children of the same age despite scoring at the median rank. Approximately the same proportion of parents hold accurate beliefs regarding their child's skills: in the sample, 43 % and 44 % of parents correctly evaluate their child's abilities in German and math, respectively. The proportion of parents who overestimate their child's scientific skills is even higher, with 46 % overestimating their child's knowledge. By contrast, only 31 % of parents correctly assess their child's scientific knowledge. The figure also demonstrates that overestimation plays a greater role than underestimation. The percentage of parents who believe their child is ranking lower than same-aged children, despite the child scoring at or above the median on the NEPS test, is 17 % for German, 19 % for math, and 23 % for science. Given that overestimation significantly outweighs underestimation, we focus on parental overestimation in the remainder of our analyses.

Figure 2.2: Parental Beliefs About Their Child's Skills



Notes: This figure shows the share of parents in the overall sample who are under- or overestimating or correctly evaluating their child's skills. This is shown for the sample of parents surveyed before distributing the report cards. *Source:* NEPS SC2 (2013), weighted, own calculations.

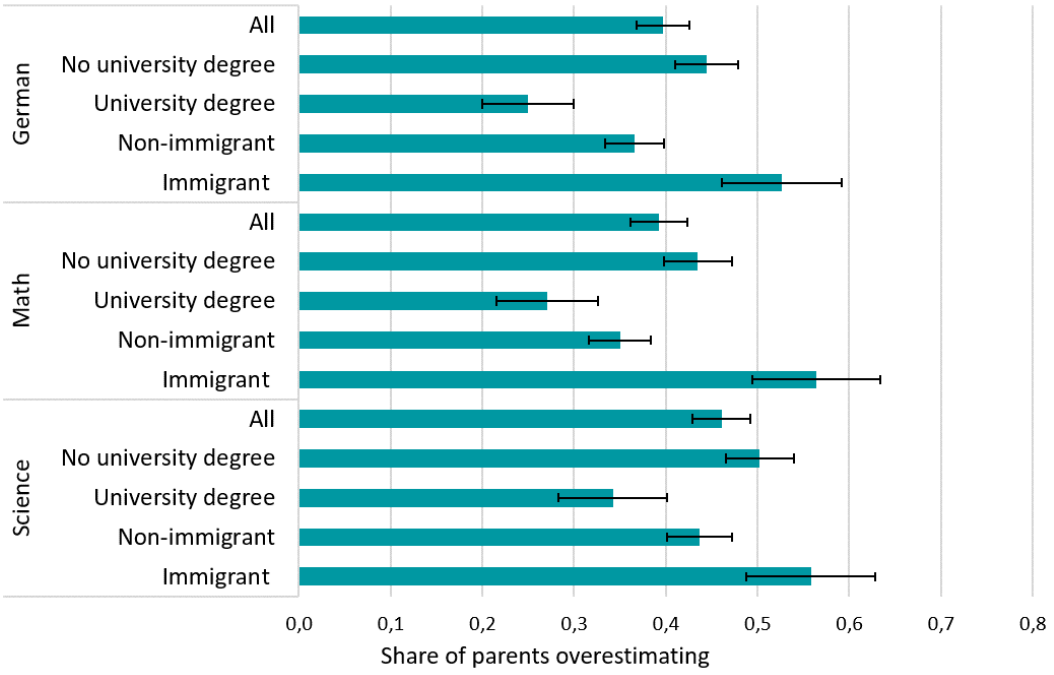
When differentiating by mother's education and parents' immigrant status in Figure 2.3, we observe between-group differences for all three subjects: lower-educated mothers are 20 percentage points more likely than higher-educated mothers to overestimate their child's German skills and 16 percentage points more likely to overestimate their child's math or science skills. Among immigrant families[11], the proportion of those overestimating their child's skills is 16 percentage points higher for German, 21 percentage points higher for math, and 12 percentage points higher for scientific knowledge. An additional analysis examining education and immigration status together reveals that differences in education cannot solely explain the disparities related

[11]Immigrant families are defined as families in which at least one parent was born abroad and immigrated to Germany. These parents migrated mostly from countries such as Poland, Turkey, the former Soviet Union, the former Yugoslavia, and other European countries.

to migration. The differences in immigrant status are consistent with previous literature, which has demonstrated that immigrant parents in Germany and other countries often have higher educational aspirations than their non-immigrant counterparts (e.g., Gresch et al., 2012).

The figure also demonstrates that parental misbeliefs in science are most pronounced. The group differences highlight the importance of investigating parental investments and how they are influence by institutional settings. If less educated and immigrant parents are more likely to overestimate their child's abilities and consequently invest less in their children's education (see Kinsler and Pavan, 2021), this may exacerbate existing inequalities (e.g., Bradbury et al., 2015).

Figure 2.3: Share of Parents Overestimating Their Child's Skills

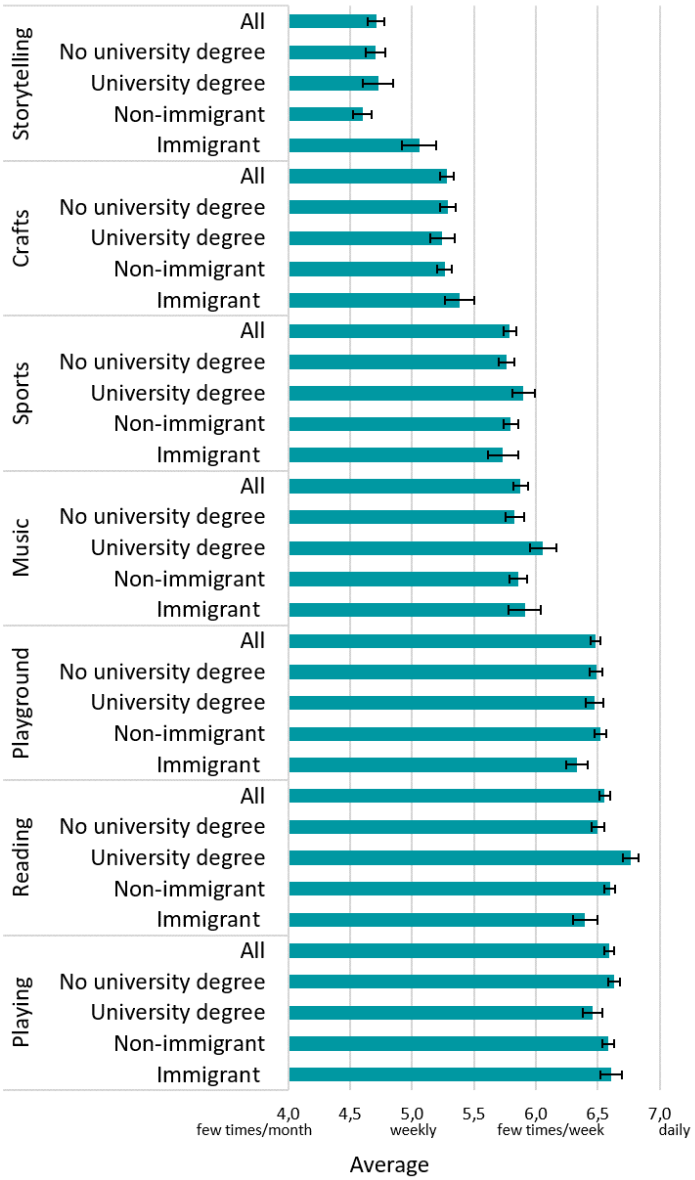


Notes: This figure shows the share of parents in the overall sample ("all"), in the sample of families in which the mother does not have a university degree ("no university degree"), in the sample of families in which the mother has at least a university degree ("university degree"), in the sample of families in which both parents were born in Germany ("non-immigrant") and in the sample of families in which at least one parent was born abroad and migrated to Germany ("immigrant") who are overestimating their child's skills. This is shown for the sample of parents surveyed before the report card distribution. *Source:* NEPS SC2 (2013), weighted, own calculations.

In Figure 2.4, we compare the average frequency with which parents engage in activities with their children and how these frequencies differ across various groups of parents. Notably, the frequency of engagement varies significantly among different activities. Parents spend the least time telling stories to their children at home, which occurs on average between several times a month and once a week. In contrast, parents

typically paint, draw, or do crafts with their children every week. Sports and music activities take place several times per week. Parents also engage in outdoor activities, visit playgrounds, read, or play with their children between several times a week and daily.

Figure 2.4: Average Parental Investments



Notes: This figure shows the average frequency at which parents undertake the different activities with their child on a scale from 1 (never) to 8 (several times a day) in the overall sample ("all"), in the sample of families in which the mother does not have a university degree ("no BA"), in the sample of families in which the mother has at least a university degree ("BA"), in the sample of families in which both parents were born in Germany ("non-migrant") and in the sample of families in which at least one parent was born abroad and migrated to Germany ("migrant"). This is shown for the sample of parents surveyed before the report card distribution. *Source:* NEPS SC2 (2013), weighted, own calculations.

The frequency of participation in these activities is relatively similar across different groups of parents. However, the most significant difference is that less-educated parents read to their children considerably less often than higher-educated parents, and immigrant parents read with their children less frequently than parents born in Germany. This finding aligns with previous German and US research (Autorengruppe Bildungsberichterstattung, 2018; Festa et al., 2014). The differences in misperceptions and reading time related to education and immigrant status underscore the importance of investigating whether these gaps are increased or decreased through teacher information. In the next step, we investigate the effects of report cards distributed in schools.

2.4.2 Effects of Teacher-Provided Information on Parents' Beliefs and Behavior

Our results in Table 2.2 indicate that report cards do not generally reduce parental misperceptions. When comparing parents before and after they received information from schools, we do not observe a reduction in their overestimations. Their overestimation with respect to German and Science skills remains the same. With respect to math skills, on average, there even seems to be an increase in overestimation.

In Panels B and C, we differentiate between federal states that use a report card containing a verbal description of the child's performance (Panel B) and states that use numerical grades, ordered scales, or additional meetings with parents in which teachers and parents discuss the report cards (Panel C). These results show that report cards containing numerical grades or scales can reduce parental overestimation of children's German skills. A comparison of states that additionally distribute a winter report card (making the report card under investigation the second one) with states where the report card under investigation is the first that parents receive (Panels D and E) suggests that first report cards can effectively reduce the overestimation of German skills. Additionally, the increase in overestimation of math skills can be attributed to the first report cards.

The analyses suggest that certain report cards reduce the overestimation of German skills. A potential explanation for the effect in German but not the other subjects is the focus on German in German elementary schools, as the recommended minimum instruction time allocated to reading, writing, and literature is 26.8 %. In comparison, only 20.6 % is allocated to mathematics (European Commission and European Education and Culture Executive Agency, 2023).

Table 2.2: Effects of Report Cards on Parental Overestimation

	(1) German	**(2)** Math	**(3)** Science
Panel A: Overall effects			
Post	-0.11 (0.08)	0.16** (0.07)	-0.11 (0.08)
Observations	1,517	1,329	1,316
Pre-treatment mean	0.40	0.40	0.48
Panel B: States using verbal description			
Post	-0.00 (0.10)	0.10 (0.08)	-0.16 (0.10)
Observations	1,251	1,116	1,104
Pre-treatment mean	0.40	0.40	0.46
Panel C: States using numerical grades, scale, additional meeting			
Post	-0.37** (0.14)	0.18 (0.25)	-0.03 (0.29)
Observations	266	213	212
Pre-treatment mean	0.42	0.41	0.56
Panel D: Second report card (states with winter report card)			
Post	0.02 (0.14)	0.06 (0.11)	-0.01 (0.22)
Observations	496	446	446
Pre-treatment mean	0.40	0.39	0.46
Panel E: First report card (states without winter report card)			
Post	-0.19** (0.08)	0.24*** (0.09)	-0.07 (0.08)
Observations	964	842	829
Pre-treatment mean	0.40	0.41	0.48

Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. Standard errors clustered at the classroom level in parentheses. * p <0.10, ** p <0.05, *** p <0.01.
Source: NEPS SC2 (2013), weighted, own calculations.

Looking at the effects on parental investments in Table 2.3, it is evident that while parental beliefs do not change much on average, there is a notable change in parental investments. Parents surveyed after receiving the report cards report a higher frequency of reading with their child than those surveyed before the information was provided. This effect is substantial, corresponding to an increase of half a standard deviation, or a shift from several times a week and daily to daily.

Further investigation into the modality of information provision reveals that parents who receive information as numerical grades, ordered scales, or in a meeting increase their reported reading and playing time. In contrast, parents receiving the information as a verbal description even decrease the time spent playing (Panels B and C). The results in Panels D and E illustrate the existence of a novelty effect: parental investments in reading and playing are substantially more affected the first time they receive information from the school. Parental storytelling increases even if the report card is the second one parents receive.

Table 2.3: Effects of Report Cards on Parental Investments

	(1) Reading	(2) Storytelling	(3) Music	(4) Crafts	(5) Sports	(6) Playing	(7) Playground
Panel A: Overall effects							
Post	0.58*	-0.20	-0.06	-0.12	0.05	0.45	-0.04
	(0.34)	(0.21)	(0.29)	(0.26)	(0.30)	(0.38)	(0.20)
Observations	1,531	1,530	1,530	1,528	1,529	1,528	1,527
Panel B: States using verbal description							
Post	-0.14	-0.27	-0.28	-0.41	-0.19	-0.42*	-0.04
	(0.27)	(0.27)	(0.28)	(0.28)	(0.22)	(0.22)	(0.22)
Observations	1,263	1,262	1,262	1,260	1,261	1,260	1,259
Panel C: States using numerical grades, scales, additional meeting							
Post	1.38*	-0.02	0.00	0.39	0.22	2.36***	-0.10
	1.38*	-0.02	0.00	0.39	0.22	2.36***	-0.10
Observations	268	268	268	268	268	268	268
Panel D: Second report card (states with winter report card)							
Post	0.48	1.14*	0.36	-0.10	-0.11	0.52	-0.23
	(0.36)	(0.50)	(0.84)	(0.49)	(0.38)	(0.70)	(0.45)
Observations	498	498	498	498	498	498	496
Panel E: First report card (states without winter report card)							
Post	1.23**	-0.16	0.30	0.33	0.46	1.10**	0.06
	(0.54)	(0.50)	(0.67)	(0.53)	(0.62)	(0.48)	(0.31)
Observations	975	974	974	972	973	972	973

Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal German rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. Standard errors (in parentheses) are clustered at the classroom level. * p <0.10, ** p <0.05, *** p <0.01. *Source:* NEPS SC2 (2013), weighted, own calculations.

2.4.3 Heterogeneous Treatment Impacts

Our data also allow us to investigate heterogeneous treatment impacts based on parental education, immigrant status, and child gender. The results indicate that the information treatment decreases parental overestimation of their child's science skills in households where the mother does not have a bachelor's degree and parental overestimation of their child's math skills increases in higher educated households (see Table 2.A.1 in the Appendix). Second, we find suggestive evidence for larger treatment effects on parental investments for less educated and immigrant parents (see Table 2.A.2 in the Appendix). Third, there is no clear pattern with respect to gender.

Additionally, we investigate how teachers' biases may influence treatment effects. Figure 2.A.1 in the Appendix illustrates the share of teachers who overestimate a child's skills. Overestimation is prevalent among teachers. Twenty-7 % of teachers overestimate children's German skills, 32 % overestimate children's math skills, and 39 % overestimate children's science skills. When differentiating by parental education, we find that teachers particularly overestimate the skills of children with less educated mothers compared to those with more educated mothers. Teachers' overestimation also seems to be higher for immigrant children compared to children whose parents were born in Germany, even though the difference is not statistically significant. This finding is in line with the findings of Bredtmann et al. (2024) that elementary school teachers have a positive evaluation bias toward ethnic minority students.

Because teacher misperceptions are highly prevalent, we compare treatment effects for children whose teacher overestimates their skills with treatment effects for children whose teacher assesses their performance correctly or underestimates their skills (see Panels G and H in Appendix Tables 2.A.1 and 2.A.2). This comparison indicates that biased assessments by teachers weaken the information value of report cards. Parental misperceptions about the child's German skills diminish and parental reading increases only if report cards assess the child correctly or underestimate the child's skills.

These results suggest that teacher information may narrow the inequalities by education and immigrant status discussed in Section 2.4.1. However, our results emphasize that teacher bias as well as the modality of information provision (see Section 2.4.2) also play a role.

2.4.4 Robustness

We conduct several sensitivity tests to ensure the exogeneity of our identification which we show in Tables 2.A.3 to 2.A.6 in the Appendix. First, we compare the treatment

effects for parents who likely received an unfavorable report card – proxied by the teacher ranking the child lower than other children the same age – with parents who likely received a neutral or positive report card – proxied by a teacher ranking the child as performing equally well or better than their peers (see Panels B and C in Tables 2.A.3 and 2.A.5). The rationale behind this test is that only parents who receive a negative information treatment should change their beliefs and behaviors, while these should remain constant for all other parents. This test is critical because most parents surveyed after the information treatment were surveyed during the school holidays. Our comparison reveals that parents who receive a negative information treatment update their beliefs and are less likely to overestimate their child's German skills when surveyed after receiving the negative information, compared to parents who received a neutral or positive information. These parents are also the ones who are increasing the frequency of reading with their child.

Second, we exclude the 10 % of fathers who participated in the survey to examine whether this impacts our results (see Panel D in Appendix Tables 2.A.3 and 2.A.5). The results of this analysis closely resemble those of our main specification.

Third, we exclude parents by the number of contact attempts necessary to contact them. We do so because parents in the post-treatment group had to be contacted significantly more often before participating in the interview, which could indicate that the pre- and post-treatment groups are not comparable. We exclude the first 10/15 % who participated after being contacted and the last 5/10/15 % who responded. We cannot exclude only the first 5 % of parents since 10.3 % participated after the first contact. This analysis, as shown in Tables 2.A.4 and 2.A.6 in the Appendix, yields very similar effect sizes although less statistically significant results due to smaller sample sizes.

The different robustness tests support the identification of causal effects using our empirical strategy and emphasize that report cards have only a limited effect on parental misperceptions. At the same time, we find substantial impacts on parental time investments especially with respect to reading.

2.5 Conclusion

Parents' knowledge and beliefs about their child's academic achievement are essential, as they shape decisions regarding investments in their children and may, therefore, increase or decrease educational inequalities. Our results indicate that many parents overestimate their child's skills, particularly among more disadvantaged families. This

result highlights the importance of understanding the formation of parental misperceptions.

This study investigates the causal impact of teacher-provided information about children's performance on parents' beliefs and investments during the first year of formal schooling. We exploit differences across federal states in Germany regarding the timing and nature of information provided to parents about their children's achievements. The used detailed survey data includes information on parents' and teachers' evaluations of children's skills and scores on standardized tests conducted as part of the survey, allowing us to investigate parental and teacher misperceptions about children's objective performance. Our approach provides insights into the effects of information treatments in the crucial first year of formal schooling and the importance of the modality of information (numeric or verbal) and novelty effects (first or second report cards). This contributes to a better understanding of communication between schools and parents.

Our analysis reveals that information from teachers has relatively little effect on parents' beliefs. Heterogeneity analyses show that report cards correct the overestimations of less-educated parents regarding their child's science skills. We further demonstrate that information spurs meaningful parental investments in activities such as reading and playing. Following the school information treatment, parents increase their reading time from several days a week to daily. Reading and playing are likely the most natural educational activities that parents increase after receiving information from schools. Plausibly causal evidence suggests that increases in these parental activities improve child skills (Kalb and Van Ours, 2014; Kalil et al., 2025). This demonstrates that providing comprehensible information to parents boosts their investments and impacts their behavior, aligning with previous research on information and parental behavior (e.g., Andrabi et al., 2017; Bergman, 2021; Borger et al., 2024).

Our results highlight two additional aspects regarding the institutional context of distributing information about a child's achievements through report cards. First, we show that the format of the report card matters. The type of report card prevalent in most German states – a verbal description of the child's skills – proves ineffective. By contrast, parents receiving information in numerical grades, ordered scales, and one-on-one meetings with teachers substantially increase their reading and playing time with their child and decrease their overestimation of the child's German skills. Conversely, parents receiving information in the form of a verbal description even decrease the frequency of playing with their child.

Additionally, our results indicate that the timing of receiving information is crucial. Increases in parental investments, such as reading and playing with their child, and

decreases in parental overestimation of children's German skills are driven by those who receive the first report card of their child's formal schooling. We find no statistically significant impacts for parents who have already received a report card before the one examined in this paper, suggesting that the novelty effect of the information diminishes over time, making subsequent information treatments less effective. This result emphasizes the need to focus mainly on report cards in the early elementary school years, which have a greater potential to inform and impact parental behavior, as parents are more attentive to novel information.

Educators – including parents – in several countries, particularly across Europe, are debating the merits of providing numerical grades as report cards compared to qualitative progress reports in the first school years. Our results indicate that the approach often viewed less favorably in public and political discourse – using numerical grades or scales in the early school years – is in fact the one that most effectively shapes parental beliefs and behaviors, especially for children who could benefit the most. The utilization of verbal descriptions in report cards, as seen in Germany and several other European countries, may unintentionally hinder parents from obtaining a clear understanding of their child's performance and may reduce their likelihood of making educational investments. If the primary objective of report cards is to inform parents and encourage their investments, then verbal descriptions do not seem to be an effective means to achieving this goal.

However, in most cases, school report cards serve multiple purposes. Therefore, it is essential to consider that the manner in which information is presented in report cards does not adversely affect the students. For instance, Collins and Lundstedt (2024) demonstrate that a more granular grading scale in Swedish schools negatively impacted high school graduation rates, as students became discouraged and lowered their self-belief. Consequently, when drawing conclusions about improving communication between schools and parents, it is crucial to simultaneously consider the effects on parents and students.

Despite the unique dataset and quasi-experimental design, several limitations remain, warranting caution in interpreting the results as strictly causal. First, parents surveyed after report cards were interviewed mostly during school holidays, while those surveyed beforehand were interviewed during the school year. Differences in available time for parental investments may thus confound the estimates, although we show that increases are mainly driven by parents receiving negative information. Second, even though survey timing is in general determined by the NEPS structure, some parents may select into the post-period only after repeated contact attempts. While excluding

parents which are difficult to contact does not change the results, residual selection cannot be ruled out.

An additional limitation of our analysis is that, because all parents in this setting eventually receive report cards during the summer, we cannot identify the long-term effects on parental beliefs and behaviors. We are investigating the immediate response of parents. Furthermore, the set of investment outcomes available in the data is limited. The NEPS does not include measures specific to parents' assistance with math and science, which restricts our analysis to general investment measures and reading as an indicator of investment in German. Finally, while analyses of parental involvement at school, such as attendance at parent-teacher conferences or parent-child conflicts, would be valuable, these analyses are not feasible with our available data.

2.A Appendix

2.A.1 Subsample Analyses

Table 2.A.1: Effects of Report Cards on Parental Overestimation: Subsample Analyses

	(1) German	(2) Math	(3) Science
Panel A: Mothers without a university degree			
Post	-0.11 (0.09)	0.09 (0.08)	-0.18* (0.10)
Observations	1,152	1,011	1,005
Pre-treatment mean	0.46	0.45	0.53
Panel B: Mothers with a university degree			
Post	0.04 (0.22)	0.44** (0.19)	0.27 (0.27)
Observations	365	318	311
Pre-treatment mean	0.25	0.28	0.33
Panel C: Immigrant families			
Post	-0.20 (0.22)	-0.12 (0.27)	-0.02 (0.29)
Observations	326	294	288
Pre-treatment mean	0.25	0.28	0.55
Panel D: Non-immigrant families			
Post	-0.05 (0.09)	0.26** (0.13)	-0.09 (0.09)
Observations	1,191	1,035	1,028
Pre-treatment mean	0.37	0.35	0.45
Panel E: Boys			
Post	-0.14 (0.15)	0.16 (0.14)	-0.03 (0.13)
Observations	751	671	663
Pre-treatment mean	0.40	0.37	0.49
Panel F: Girls			
Post	0.12 (0.11)	0.18 (0.11)	-0.09 (0.11)
Observations	766	658	653
Pre-treatment mean	0.40	0.43	0.47
Panel G: Teacher overestimates child's skills			
Post	0.01 (0.13)	0.02 (0.17)	-0.03 (0.10)
Observations	418	406	487
Pre-treatment mean	0.91	0.91	0.96
Panel H: Teacher assesses child's skills correctly or underestimates			
Post	-0.18** (0.09)	0.25** (0.10)	-0.05 (0.09)
Observations	1,072	856	747
Pre-treatment mean	0.21	0.15	0.15

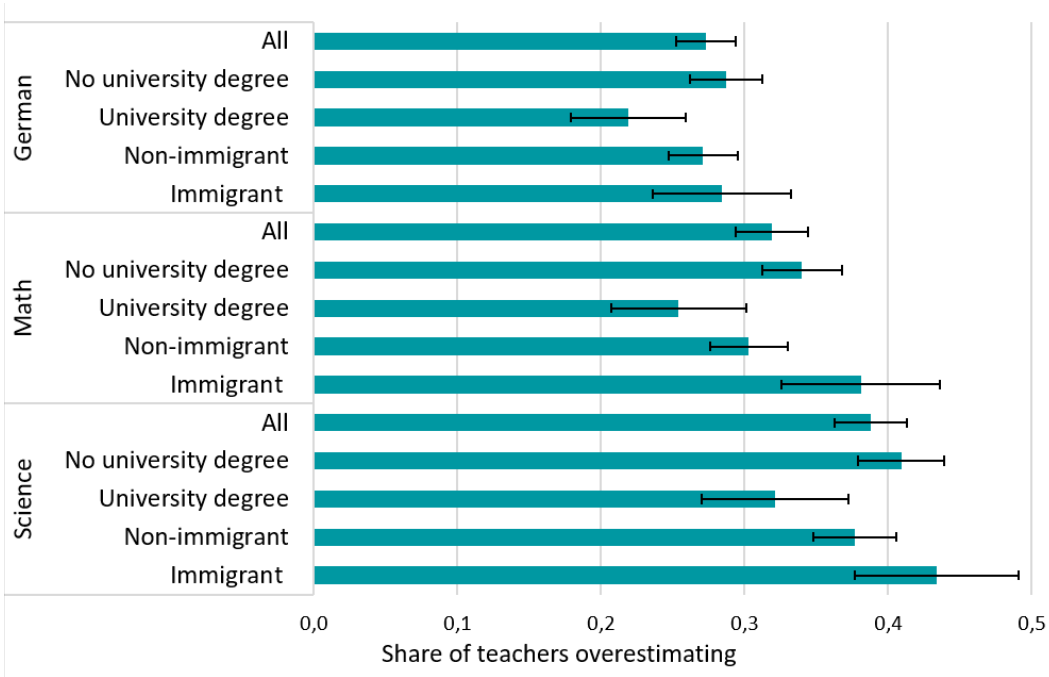
Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. Standard errors (in parentheses) are clustered at the classroom level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Source: NEPS SC2 (2013), weighted, own calculations.

Table 2.A.2: Effects of Report Cards on Parental Investments: Subsample Analyses

	(1) Reading	(2) Storytelling	(3) Music	(4) Crafts	(5) Sports	(6) Playing	(7) Playground
Panel A: Mothers without a university degree							
Post	0.59	-0.27	-0.19	-0.24	-0.03	0.67	-0.12
	(0.41)	(0.26)	(0.34)	(0.30)	(0.35)	(0.42)	(0.23)
Obs.	1,162	1,161	1,161	1,159	1,161	1,159	1,148
Panel B: Mothers with a university degree							
Post	0.41	-0.50	0.14	0.46	0.19	-1.34	-0.45
	(0.45)	(0.48)	(0.92)	(0.68)	(0.54)	(0.89)	(0.32)
Obs.	369	369	369	369	368	369	369
Panel C: Immigrant families							
Post	1.53	0.23	0.44	0.72	-0.07	0.77	-0.33
	(1.11)	(0.60)	(0.76)	(0.87)	(0.69)	(0.99)	(0.55)
Obs.	331	331	331	329	331	329	330
Panel D: Non-immigrant families							
Post	0.25	-0.47**	0.19	-0.22	0.26	0.27	0.24
	(0.38)	(0.23)	(0.32)	(0.28)	(0.37)	(0.44)	(0.24)
Obs.	1,200	1,199	1,199	1,199	1,198	1,199	1,197
Panel E: Boys							
Post	0.04	0.05	-0.63	-0.37	-0.59*	0.52	0.24
	(0.40)	(0.45)	(0.47)	(0.47)	(0.35)	(0.70)	(0.43)
Obs.	758	758	757	757	757	757	756
Panel F: Girls							
Post	0.75	-0.04	0.05	-0.15	0.12	0.47	-0.15
	(0.58)	(0.30)	(0.34)	(0.29)	(0.46)	(0.51)	(0.25)
Obs.	773	772	773	771	772	771	771
Panel G: Teacher overestimates child's skills							
Post	-1.70	-0.66	-0.63	0.56	0.00	0.83	-0.51
	(1.13)	(1.05)	(0.95)	(0.58)	(0.67)	(1.48)	(0.79)
Obs.	421	421	421	421	421	421	421
Panel H: Teacher assesses child's skills correctly or underestimates							
Post	0.80*	-0.20	0.11	-0.43	-0.16	0.13	-0.03
	(0.41)	(0.25)	(0.39)	(0.31)	(0.31)	(0.35)	(0.21)
Obs.	1,083	1,082	1,082	1,080	1,081	1,080	1,079

Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. Standard errors (in parentheses) are clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. *Source:* NEPS SC2 (2013), weighted, own calculations.

Figure 2.A.1: Share of Teachers Overestimating a Child's Skills by Parental Characteristics



Notes: This figure shows the share of teachers overestimating a child's skills. It is shown for the overall sample ("all"), for children whose mother does not have a university degree ("no university degree"), for children whose mother has at least a university degree ("university degree"), for children of two German-born parents ("non-immigrant"), and for children of at least one parent born abroad ("immigrant"). *Source:* NEPS SC2 2013, weighted, own calculations.

2.A.2 Robustness

Table 2.A.3: Effects on Parental Overestimation: Robustness Tests

	(1) German	(2) Math	(3) Science
Panel A: Main specification			
Post	-0.11 (0.08)	0.16** (0.07)	-0.11 (0.08)
Observations	1,517	1,329	1,316
Pre-treatment mean	0.40	0.40	0.48
Panel B: Teacher ranks child lower than peers			
Post	-0.41* (0.24)	0.46 (0.58)	0.24 (0.62)
Observations	397	233	148
Pre-treatment mean	0.52	0.46	0.65
Panel C: Teacher ranks child equal or higher than peers			
Post	0.00 (0.07)	0.10 (0.09)	-0.08 (0.09)
Observations	1,093	1,029	1,106
Pre-treatment mean	0.36	0.39	0.45
Panel D: Excluding fathers			
Post	-0.12 (0.08)	0.15 (0.10)	-0.10 (0.09)
Observations	1,365	1,196	1,182
Pre-treatment mean	0.40	0.39	0.47

Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. Standard errors clustered at the classroom level in parentheses. * p <0.10, ** p <0.05, *** p <0.01. *Source:* NEPS SC2 (2013), weighted, own calculations.

Table 2.A.4: Effects on Parental Overestimation: Excluding Parents With Fewest and Most Attempts Needed to Survey Them

	(1) German	(2) Math	(3) Science
Panel A: Main specification			
Post	-0.11 (0.08)	0.16** (0.07)	-0.11 (0.08)
Observations	1,517	1,329	1,316
Pre-treatment mean	0.40	0.40	0.48
Panel B: Excluding first 10 %			
Post	-0.05 (0.10)	0.22* (0.12)	-0.10 (0.09)
Observations	1,331	1,169	1,157
Pre-treatment mean	0.40	0.40	0.48
Panel C: Excluding first 15 %			
Post	-0.06 (0.10)	0.22 (0.15)	-0.09 (0.11)
Observations	1,054	934	920
Pre-treatment mean	0.41	0.40	0.50
Panel D: Excluding latest 15 %			
Post	0.18 (0.08)	0.15* (0.07)	-0.06 (0.09)
Observations	1,326	1,156	1,144
Pre-treatment mean	0.40	0.40	0.46
Panel E: Excluding latest 10 %			
Post	-0.08 (0.08)	0.15** (0.06)	-0.08 (0.09)
Observations	1,376	1,197	1,185
Pre-treatment mean	0.41	0.40	0.47
Panel F: Excluding latest 5 %			
Post	-0.12 (0.08)	0.15** (0.07)	-0.11 (0.08)
Observations	1,447	1,261	1,249
Pre-treatment mean	0.40	0.40	0.47

Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. The first 5 % of parents cannot be excluded individually since 10.3 % of parents participated after the first contact. Standard errors (in parentheses) are clustered at the classroom level. * p <0.10, ** p <0.05, *** p <0.01. *Source:* NEPS SC2 (2013), weighted, own calculations.

Table 2.A.5: Effects on Parental Investments: Robustness Tests

	(1) Reading	(2) Storytelling	(3) Music	(4) Crafts	(5) Sports	(6) Playing	(7) Playground
Panel A: Main Specification							
Post	0.58*	-0.20	-0.06	-0.12	0.05	0.45	-0.04
	(0.34)	(0.21)	(0.29)	(0.26)	(0.30)	(0.38)	(0.20)
Obs.	1,531	1,530	1,530	1,528	1,529	1,528	1,527
Panel B: Teacher ranks child lower than peers							
Post	1.62*	-0.03	0.44	-0.45	-0.21	0.26	0.01
	(0.84)	(0.70)	(0.80)	(0.58)	(0.61)	(0.63)	(0.41)
Obs.	404	404	404	401	404	402	403
Panel C: Teacher ranks child equal or higher than peers							
Post	0.19	-0.11	-0.43	0.10	0.32	0.64	0.15
	(0.45)	(0.32)	(0.29)	(0.35)	(0.50)	(0.62)	(0.26)
Obs.	1,100	1,099	1,099	1,100	1,098	1,099	1,097
Panel D: Excluding fathers							
Post	0.64*	-0.31	0.03	-0.21	0.05	0.39	-0.08
	(0.34)	(0.24)	(0.31)	(0.26)	(0.31)	(0.35)	(0.20)
Obs.	1,379	1,378	1,378	1,378	1,377	1,377	1,375

Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. Standard errors (in parentheses) are clustered at the classroom level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Source:* NEPS SC2 (2013), weighted, own calculations.

Table 2.A.6: Effects on Parental Investments: Excluding Parents With Fewest and Most Survey Attempts Needed to Survey Them

	(1) Reading	(2) Storytelling	(3) Music	(4) Crafts	(5) Sports	(6) Playing	(7) Playground
Panel A: Main Specification							
Post	0.58*	-0.20	-0.06	-0.12	0.05	0.45	-0.04
	(0.34)	(0.21)	(0.29)	(0.26)	(0.30)	(0.38)	(0.20)
Obs.	1,531	1,530	1,530	1,528	1,529	1,528	1,527
Panel B: Excluding first 10 %							
Post	0.61	-0.22	-0.44*	-0.20	0.15	0.53	0.00
	(0.45)	(0.28)	(0.26)	(0.35)	(0.45)	(0.46)	(0.26)
Obs.	1,345	1,344	1,345	1,342	1,343	1,342	1,341
Panel C: Excluding first 15 %							
Post	0.73	-0.23	-0.41	-0.23	0.25	0.20	-0.01
	(0.53)	(0.31)	(0.30)	(0.31)	(0.53)	(0.27)	(0.28)
Obs.	1,068	1,067	1,068	1,065	1,066	1,065	1,065
Panel D: Excluding latest 15 % of parents							
Post	0.69	-0.17	-0.02	0.06	0.15	0.58	0.11
	(0.43)	(0.19)	(0.33)	(0.33)	(0.36)	(0.46)	(0.24)
Obs.	1,340	1,339	1,339	1,338	1,338	1,337	1,337
Panel E: Excluding latest 10 % of parents							
Post	0.62	-0.21	-0.04	-0.02	0.12	0.53	0.04
	(0.42)	(0.21)	(0.33)	(0.31)	(0.35)	(0.46)	(0.24)
Obs.	1,390	1,389	1,389	1,387	1,387	1,387	1,387
Panel F: Excluding latest 5 % of parents							
Post	0.56	-0.29	0.03	-0.03	0.10	0.51	-0.04
	(0.39)	(0.21)	(0.31)	(0.31)	(0.34)	(0.42)	(0.21)
Obs.	1,461	1,460	1,460	1,458	1,459	1,458	1,457

Notes: Coefficients from Equation 2.1. Post equals 1 for parents surveyed after the information provision and 0 for those surveyed before. All regressions control for mother's education, labor force status, child's immigrant status, child's gender, child's ordinal German rank within class, the number of contact attempts necessary to survey the parents, survey month fixed effects, child's birth year fixed effects, federal state fixed effects, class room fixed effects. The first 5 % of parents cannot be excluded individually since 10.3 % of parents participated after the first contact. Standard errors (in parentheses) clustered at the classroom level. * p<0.10, ** p<0.05, *** p<0.01. *Source:* NEPS SC2 (2013), weighted, own calculations.

CHAPTER 3

The Economic Impact of Advance Child Support: Financial Well-being, Labor Supply, and Consumption of Single-Parent Households[1]

3.1 Introduction

Single-parent households represent a substantial share of families in many developed countries. Across OECD nations, around 17 % of children live in single-parent households – a proportion that has remained high or even increased in some countries in recent decades (e.g., Nieuwenhuis and Maldonado, 2018; OECD, 2024c). These families face elevated risks of poverty, social and material deprivation, stress and family conflict, with serious implications for children's development and well-being (e.g., Moullin and Harkness, 2021; Nieuwenhuis and Maldonado, 2018; OECD, 2024b). Child support payments[2] may reduce the economic disadvantages connected to single parenthood. However, many resident parents – mostly mothers – do not receive consistent child support. Only about 25 % of single mothers in the US and UK, and 47 % in Germany, receive full or regular payments (Beblo et al., 2025; Hakovirta and Mesiäislehto, 2022;

[1]This chapter is joint work with Sophia Schmitz (Federal Institute for Population Research (BiB)). This work was supported by the Joachim Herz Foundation. Thanks also go to the RDC of the Federal Statistical Office and Statistical Offices of the Federal States of Germany for the provision of the German Microcensus and the Income and Expenditure Survey. During the preparation of this chapter, the authors used ChatGPT to enhance readability and check the language. Following the use of this tool, the authors reviewed and edited the suggestions as needed.

[2]Child support is a legal obligation requiring non-resident parents – typically post-divorce or separation – to contribute financially to their children until adulthood or another legally defined endpoint. They are mandated across all OECD countries, although varying in design and enforcement (OECD, 2025a).

Pilarz and Cuesta, 2025). In response, many European countries have introduced public advance child support programs to ensure that children receive financial support when the non-resident parent fails to pay. These programs aim to stabilize household income, reduce child poverty, and encourage parental responsibility, with states typically seeking reimbursement from the liable parent.

Despite their widespread adoption – especially across Europe – very little is known about the economic impacts of public child support schemes. An evaluation of advance child support, therefore, requires answers to three questions. First, do public child support payments genuinely improve the financial well-being of single-parent families, or do they merely substitute for private support payments from non-resident parents? Second, how does the design of such programs – particularly when they are tied to other welfare benefits or are means-tested – influence labor supply decisions of the resident parent? Third, how do changes in child support payments, along with any resulting shifts in labor market behavior, affect family spending patterns, especially on goods and services that promote children's human capital and overall well-being?

A priori, the answer to these question is not obvious. Public advance child support payments may improve financial well-being by providing income stability in the absence of reliable private transfers, though they risk crowding out private support if not carefully designed. When such programs interact with other welfare benefits, they can create either work incentives (by conditioning support on employment), or disincentives (by acting as a partial income replacement), thus influencing labor supply decisions. Changes in income and employment resulting from these programs may, in turn, affect household consumption patterns, particularly in relation to expenditures on children, given that the payments are earmarked as child-specific support.

One challenge in studying the causal effects of child support is that selection into single parenthood – and into receipt of public advance child support (often due to the absence of private payments from the non-resident parent) – is not random. In this paper, we overcome these endogeneity concerns by exploiting a major reform to Germany's advance child support system in 2017, thereby providing first quasi-experimental evidence on how public child support affects single-parent families. The reform expanded eligibility for guaranteed and substantial advance child support payments to include children over the age of 11 and eliminated the previous 72-month payment cap. As a result, the number of single-parent families receiving advance child support more than doubled. Using representative microdata from the German Microcensus (MZ) and the Income and Expenditure Survey (EVS), we estimate difference-in-differences models that compare outcomes for single mothers with children newly eligible for ad-

vance child support to those whose eligibility was unchanged. Our results show that the reform improved families' financial situations without crowding out private support payments. Since eligibility was tied to economic independence of single-parent households, the reform also led to an increase in working hours among mothers – particularly those previously in the middle of the working hours distribution – and reduced welfare dependency. Evidence based on the Income and Expenditure Survey demonstrates that the reform also significantly altered expenditure patterns of affected families, with increased expenditures on food, beverages, and goods related to the human capital development and well-being of children. These findings are robust to a wide range of sensitivity checks, including accounting for regional policy changes, using a placebo treatment group, alternative sample definitions, and sample selection around reform implementation.

The findings add to several strands of the literature. First, our findings contribute to the literature on post-separation finances of single parents, particularly the role of child support and alimony payments. Most research on child support has examined how stronger enforcement and higher obligations affect fertility and relationship dynamics.[3] and several studies also examine parental behavior.[4] With respect to family finances and mother's labor supply, structural models predict that higher child support smooths consumption around divorce and modestly reduces maternal labor supply (Rangel, 2006), yet experimental evidence from Wisconsin finds no such reduction (Cuesta and Cancian, 2015). A review of US studies shows that stronger enforcement is linked to lower welfare use (Huang and Han, 2012). Research on alimony policies shows that reducing post-marital support increases female labor force participation in the US, particularly among educated women (Verma and Iyer, 2021). Consistent with this, introducing post-separation alimony for cohabiting couples in Canada and Brazil led to declines in female labor force participation (Chiappori et al., 2017; Rangel, 2006).[5] Similar effects are observed in Germany, where women at risk of alimony loss –

[3]Stronger enforcement is associated with lower nonmarital fertility (Huang and Han, 2012), and was shown to reduce conflict and divorce rates (Nixon, 1997; Zhylyevskyy, 2012), as well as decrease abortion and increase marriage rates among unmarried pregnant women (Tannenbaum, 2020). Higher child support obligations are shown to reduce separation and divorce (Brown et al., 2025; Walker and Zhu, 2006), decrease cohabitation with non-biological fathers (Cancian and Meyer, 2014), and increase fertility with new partners (Kim et al., 2017; Rossin-Slater and Wüst, 2018).

[4]Fathers are more likely to pay child support when they contribute a larger share of income in their new household (Ermisch and Pronzato, 2008). While higher child support obligations increase transfers to mothers, they also reduce father-child co-residence (Rossin-Slater and Wüst, 2018). At the same time, stronger enforcement is associated with greater paternal involvement (Huang and Han, 2012). Moreover, reduced child support disregards have been shown to increase in-kind transfers, indicating strategic responses by non-resident parents (Gunter, 2013).

[5]Additional findings suggest that reduced alimony also lowers marriage and in-wedlock fertility (Fahn et al., 2016), increases divorce rates, and decreases women's expected lifetime income (Verma and Iyer, 2021).

especially younger cohorts and those with career interruptions – responded with higher participation and working hours (Schaubert, 2023; Usman, 2024), while other studies find no significant average effects (Bredtmann and Vonnahme, 2019). None of these studies considers the general impact of public advance child support or the interaction of it with private payments – despite its widespread adoption. We fill this gap by providing first causal evidence on the effects of public child support payments on single-mother households, complementing and extending the insights from the previously private-focused perspective.

Second, we add to research on the impacts of transfers on the financial situation and consumption patterns of single-mother households. Hoynes and Patel (2018) show that Earned Income Tax Credit expansions reduce poverty among single mothers, though effects are small at the very lowest income levels. Similarly, Gruber (2004) finds that Aid to Families with Dependent Children cash welfare helped divorced mothers smooth short-term consumption, boosting spending on essentials like food and housing with little crowding-out of other support. Evidence from programs not explicitly targeted at single mothers in the UK, Canada, Germany and Mexico also show that higher transfers to families tend to increase everyday and child-related expenditures, including transport, food, housing, clothing, and education (Angelucci and Attanasio, 2009; Gregg et al., 2006; Hoddinott and Skoufias, 2004; Jones et al., 2019; Najjarrezaparast and Pendakur, 2021; Raschke, 2016; Stichnoth et al., 2018).[6] Experimental evidence from Macedonia further suggests that transfers to mothers increase food spending more than transfers to fathers (Armand et al., 2020).

Third, this paper contributes to the literature on the labor supply behavior of single parents. Numerous studies show that employment-focused policies – such as the Working Families Tax Credit and the New Deal for Lone Parents in the UK, the Earned Income Tax Credit in the US, the Canada Child Tax Benefit, and welfare in Norway – have increased employment and working hours among single mothers while reducing welfare reliance (Bastian and Lochner, 2022; Blundell et al., 2008; Gregg et al., 2003; Løken et al., 2018; Milligan and Stabile, 2007; Mogstad and Pronzato, 2012). Similarly, the introduction of the US Food Stamp program in the 1960s-70s had the opposite effect, reducing employment and hours worked among single mothers (Hoynes et al., 2016). Ortigueira and Siassi's (2023) structural model further highlights that optimal support should vary by child age, advocating greater income support for mothers with preschoolers and stronger work incentives for those with school-aged children. We extend this literature by examining how transfers targeted at children over the age of 11, which are linked to mothers' economic independence, affect maternal labor market

[6]Stichnoth et al. (2018) also find some substitution toward tobacco among male smokers.

behavior. Specifically, we investigate both employment decisions and the number of hours worked, assessing how mothers at various points in the working hours distribution – such as those working few, moderate, or many hours – adjust their labor supply in response to the policy.

In sum, we believe that this paper makes two key contributions to the literature. First, it provides first causal evidence on the impact of advance child support payments on single-parent families – a policy instrument widely implemented across European countries but rarely examined empirically. Our findings show that, when carefully designed to account for interactions with the broader welfare system as well as private child support enforced through legal mechanisms, such payments can effectively mitigate the economic disadvantages associated with single parenthood. We thereby complement existing studies on post-separation finances which are primarily focused on private child support and alimony payments. Second, we contribute more broadly to the literature on labor supply and consumption in single-parent families, showing that additional benefits to mothers are primarily allocated toward enhancing children's well-being, while the associated additional income does not appear to exert a significant disincentive on labor supply.

The paper proceeds as follows: Section 3.2 outlines Germany's advance child support scheme; Section 3.3 describes the data; Section 3.4 explains the empirical strategy; Section 3.5 presents results, mechanisms, and robustness checks; Section 3.6 concludes.

3.2 Advance Child Support Payments in Germany

In Germany, approximately 20 % of children live in single-parent households and around 82 % of single-parent families are led by mothers (Beblo et al., 2025). These households face significant financial challenges: many single mothers struggle to cover basic expenses (see Appendix Figure 3.A.1 and 3.A.2), despite the country's generous social welfare system (OECD, 2025b). The risk of poverty among single parents is estimated to be nearly twice that of the average for all individuals with children (Beblo et al., 2025).

To reduce the incidence of child poverty in single-parent households, German law constitutes that both parents are financially responsible for minors who have not yet completed their education or if they are under the age of 21, living in their parents' household and are still in education. If parents live apart and the child primarily resides with one parent, the resident parent fulfills their obligation by providing day-to-day care and the non-resident parent typically bears the full financial responsibility

by providing child support (§1606 (3) BGB).[7] In practice, however, it is estimated that 28 % of eligible single mothers in Germany receive no child support, and 25 % receive less than the full amount (Beblo et al., 2025).[8] Rates are even lower in the US and UK, where only about a quarter of single mothers receive regular or full payments (Hakovirta and Mesiäislehto, 2022; Pilarz and Cuesta, 2025).

To address this, Germany implemented a guaranteed[9] advance child support scheme (*Unterhaltsvorschuss*) which provides state-funded, substantial monthly payments to single[10] parents upon application when the non-resident parent fails to make regular payments. This system is similar to programs found in other OECD countries, such as Sweden's maintenance support (*underhållsstöd*) and France's family support allowance (*Allocation de soutien familial*).[11] The payment amount is not means-tested and as of 2025, corresponds to a maximum amount of 227€ for children under the age of 6, 299€ for children aged 6-11, and 394€ for children aged 12-17.[12] Upon receiving advance payments, the resident parent transfer their legal claim for child support to the state, which then seeks reimbursement from the non-resident parent through instruments such as wage garnishment or debt collection.[13] Recovery attempts by the German state only have limited success – only around 20 % of the amount is actually reclaimed (BMFSFJ,

[7] The amount of child support depends on the income of the parent obliged to pay, the child's age, and the number of children entitled to support. It is derived from the child's tax-exempt subsistence minimum and is recalibrated biennially by the Federal Ministry of Justice and varies by age group as follows: 87 % of the subsistence minimum for children up to the age of 5, 100 % for children aged 6 to 11 and 117 % for children aged 12 to 17 (§1612a BGB).

[8] Collecting child support payments from non-resident parents can be logistically and emotionally challenging for resident parents: In a German study, more than one-third of single mothers (35 %) not receiving child support reported forgoing child support to avoid straining their relationship with the non-resident parent (Hubert et al., 2020).

[9] In advance schemes, the state covers payments and later seeks reimbursement from the debtor parent. Guaranteed schemes provide support regardless of repayment success (OECD, 2025a).

[10] To be eligible, the resident parent must be unmarried – specifically single, widowed, or divorced – and not have remarried.

[11] These schemes are widespread across Europe (see Appendix Figure 3.A.3) but less common in North-America or Australia, where unpaid support typically leaves children reliant on general social welfare. In the UK and some US states, agencies assist resident parents in collecting child support from non-resident parents, but no public advance payments are provided.

[12] Payments correspond to the statutory minimum child support determined under §1612a (1) BGB and are reduced by the amount of child benefit payable for a first child (§2 UhVorschG), the amount of child support payments from the non-resident parent, and orphan's benefits. Thus, advance child support payments are lower on average than regular child support payments, which are also based on the non-resident parent's income. Advance child support payments are also deducted from other benefits such as Social Allowance (Sozialgeld), Welfare Benefits (ALG II), Social Assistance (Sozialhilfe), or Housing Benefit (Wohngeld), except for the Child Supplement (Kinderzuschlag), where only 45 % is offset (BMFSFJ, 2025).

[13] During the application process, the parent applying for advance child support must disclose information on the non-resident parent's contact details, employment situation, income, assets and number of children. Once accepted, the non-resident parent is contacted for the repayment. At this stage, the non-resident parent is required to demonstrate his inability to pay the advance child support payments back to the state.

2023). In practice, this effectively turns the scheme into a tax-funded transfer to single parents.

Germany's advance child support scheme was substantially altered by a reform which was announced on August 17, 2017 and made effective retrospectively as of July 1, 2017. The reform – which is used to identify causal effects – expanded benefit duration and eligibility to older children. Prior to 2017, advance child support was limited to children under age 12 and payed for a maximum duration of 72 months (Deutscher Bundestag, 2018). The 2017 reform extended eligibility to all children below age 18 and removed the time limit on these payments. However, children aged 12 to 17 – who became newly eligible under the reform – must meet additional conditions: they qualify only if neither they nor their resident parent receive welfare benefits (SGB II), or if the resident parent receiving such benefits has a gross monthly income of at least €600 (§1 UhVorschG).

This reform resulted in a substantial increase in the number of beneficiaries of advance child support payments, as illustrated in Figure 3.1. While the number of benefiting children under the age of 6 remained relatively stable – and even declined slightly – over time, the number of beneficiaries aged 6-11 increased following the reform. Most striking, the reform added approximately 200,000 to 300,000 beneficiaries aged 12 to 17 each year. This resulted in an increase in yearly public spending of about 150 Billion (see Appendix Figure 3.A.4). As a result, it ranks as the third-largest line item in the family policy budget, following spending on parental leave benefits.[14]

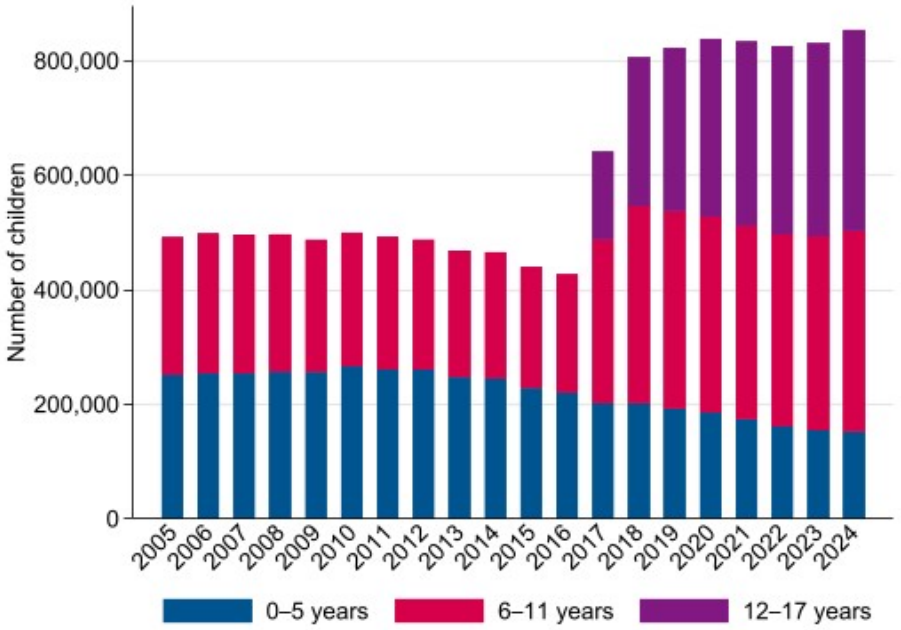
3.3 Data

3.3.1 Microcensus

We use German Microcensus data from 2012 to 2021 to examine the impact of the reform of advance child support payments on the family's financial situation and mother's labor supply. The Microcensus is a representative, annual survey of 1 % of all German households residing in private households or communal accommodations (Statistisches Bundesamt, GESIS, 2022). Its large sample size and mandatory participation help reduce selection and compositional bias, which might be particularly relevant when studying single-parent families in repeated cross-sectional data.

[14] A comparison of total government expenditures (Appendix Figure 3.A.4) with the maximum payment levels (Appendix Figure 3.A.5) and the number of beneficiaries (Figure 3.1) indicates that nearly all children receive the full amount of advance child support each month.

Figure 3.1: Number of Beneficiaries of Advance Child Support Payments



Notes: The figure shows the number of beneficiaries of advance child support payments over time and by age group.
Source: UVG Geschäftsstatistik (2025), own calculations.

Our main sample consists of single mothers – defined as women living with at least one child under the age of 18 whose father is not part of the household – who have not remarried, thus potentially meeting the eligibility criteria for advance support payments. Mothers cohabiting with a new partner without being married remain eligible for advance child support and are therefore included in our sample.[15] The treatment group comprises mothers with children aged 12–17, while the control group includes those with children aged 0–5. Both groups may also have children aged 6–11, though we show that our results remain robust when these families are excluded (see Appendix Table 3.A.3). Our final sample size corresponds to 60,813 observations (38,727 in the treatment group and 22,086 in the control group).

[15]We focus on single mothers, who account for 82 % of all single parents in Germany (Beblo et al., 2025), as analyzing mothers and fathers jointly would mask important differences due to mothers' significantly lower average (full-time) employment rates. Unfortunately, the Microcensus does not provide information on custody arrangements or the frequency of contact between children and non-resident parents. Consequently, we are unable to identify the small subset of families in which parenting responsibilities are equally shared (about 5 - 8 %, Beblo et al., 2025; Walper et al., 2020) – circumstances that would typically render the mother ineligible for advance child support payments. We exclude the small subset of mothers who live with both a child whose father is present in the household and another child whose father is not. Similarly, we exclude widowed mothers as any orphan's allowance received by the child substantially reduces the amount of advance child support.

We consider two main sets of outcome variables: First, we examine the reform's impact on outcomes reflecting the family's *financial situation* using two main measures: (i) mother's total income, comprising income from employment as well as public transfers, such as advance child support payments,[16] and (ii) private support payments – including child support, alimony from former partners, and other forms of private financial assistance. Since 2020, private support payments are only surveyed in the EU-SILC Microcensus subsample (12 %). They refer to the previous calendar year[17] and explicitly only ask for child support payments from the non-custodial parent. Reassuringly, the shares are very similar over time, indicating that our measure primarily captures child support payments in all years.[18] Unfortunately, the Microcensus only contains information on receiving advance child support within the 12 % EU-SILC subsample since 2020. We use data from the Income and Expenditure Survey (described in the following) to assess this.

Second, we assess maternal *labor supply responses* and *welfare reliance*, using four outcome measures: (i) a binary indicator for being employed, and (ii) a binary indicator for receipt of welfare benefits (Arbeitslosengeld II).[19] To study labor supply responses at the intensive margin, we look at (iii) a continuous measure of weekly working hours, set to zero for non-employed mothers; and (iv) a binary variable for near full-time employment, defined as working 30 or more hours per week.

Table 3.1 presents descriptive statistics for our full estimation sample, as well as separately for the treatment (households with children above age 11) and control groups (households with only children below age 6). 39 % of single mothers report receiving private support payments. Average (log) income is 7.2, corresponding to 1,339€. Overall, 73 % of mothers are employed, with substantial variation between groups: 83 % in the treatment group and 56 % in the control group. Accordingly, 25 % receive welfare benefits. Regarding employment intensity, 47 % of mothers are employed for 30 hours or more per week, with an average of 23 hours overall – again showing clear

[16] In the Microcensus, personal income is reported in categorical ranges. In our analysis, we take the logarithm of the median of each income range to reduce skewness and the influence of outliers, better meet the assumptions of linear regression, and allow for a more intuitive interpretation of coefficients as percentage changes.

[17] We use the 2020 and 2021 surveys to capture payments in 2019 and 2020, respectively. This means our 2019 sample combines two waves, while the 2020 sample – based solely on 2021 data – is smaller. Robustness checks (Table 3.A.3 in the Appendix) confirm that our findings hold without the EU-SILC waves.

[18] The share of parents obtaining child support also aligns with findings in Hartmann (2014)

[19] As with private support, after 2020, benefit receipt is only captured in the EU-SILC subsample. Results are robust to excluding these waves (Appendix Table 3.A.3). As an additional check, we use a variable indicating whether welfare benefits constitute the mother's primary income source – which is available for the full sample for the whole study period – and find similar results (see Appendix Table 3.A.3).

group differences (26 hours in the treatment group vs. 16 hours in the control group). These differences largely stem from our treatment definition, which is based on the child's age. This is also reflected in maternal age with mothers in the treatment group being substantially older than those in the control group (44 vs. 31 years). Relatedly, treatment group mothers are more likely to have completed post-secondary education. The share of mothers holding tertiary degrees as well as the immigration share and the number of children is however remarkably similar across the two groups.

Table 3.1: Descriptive Statistics

	Overall Sample	Control Group	Treatment Group
Outcome variables			
Private support payments	0.38 (0.49)	0.40 (0.49)	0.37 (0.48)
Log(income)	7.21 (0.58)	7.01 (0.61)	7.33 (0.53)
Employed	0.73 (0.44)	0.56 (0.50)	0.83 (0.38)
Receives unemployment benefits	0.25 (0.43)	0.38 (0.49)	0.18 (0.38)
Working hours	22.55 (16.46)	16.45 (16.66)	26.02 (15.29)
Near full-time employment	0.47 (0.50)	0.34 (0.47)	0.55 (0.50)
Control variables			
Age (in years)	39.34 (8.85)	30.90 (6.59)	44.14 (5.87)
Post-secondary degree			
No	0.25 (0.43)	0.34 (0.47)	0.19 (0.39)
Vocational	0.62 (0.49)	0.54 (0.50)	0.67 (0.47)
Tertiary	0.13 (0.34)	0.12 (0.32)	0.14 (0.35)
Immigrated to Germany	0.19 (0.39)	0.19 (0.39)	0.19 (0.39)
Number of children in the household			
1 child	0.66 (0.47)	0.66 (0.47)	0.66 (0.47)
2 children	0.28 (0.45)	0.27 (0.44)	0.28 (0.45)
3+ children	0.06 (0.24)	0.07 (0.25)	0.06 (0.24)
Observations	60,813	22,086	38,727

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The numbers represent shares if no unit is given. Standard deviations in parentheses. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. *Source:* German Microcensus (2012-2021), weighted, own calculations.

3.3.2 Income and Expenditure Survey (EVS)

We additionally use data from the Income and Expenditure Survey (EVS) in Germany to investigate the impact on single-parent household spending (Federal Statistical Office of Germany, 2025). This administrative survey is collected every five years (we use the 2008, 2013 and 2018 wave), with around 60,000 private households participating, making it the largest survey of its kind within the European Union. Given the high

social policy relevance of the EVS – particularly as a statistical basis for determining standard needs for benefits such as unemployment support, child benefits, and advance child support payments – the survey is considered to be of very high quality. In addition, the EVS is a quota sample, meaning all households are selected and surveyed according to a predefined quota plan. This allows us to obtain a sufficiently large sample of single mothers. We apply similar sample restrictions as in the Microcensus data, leaving us with 3,469 observations (2,673 the in treatment and 796 in the control group).

For our outcomes, we mainly rely on information collected in detailed household diaries, in which participating households record all income and expenditures over a three-month period.The EVS groups spending on individual items into 14 broader categories according to COICOP (Classification of Individual Consumption by Purpose).[20] To simplify our analysis, we combine a number of these broad categories into eight groups to assess the reform's impact on family spending composition: food and beverages; clothing, shoes, health and care products; rent (including heating); traffic; education; leisure, entertainment and culture; household appliances and home repairs; other goods and services. We also present results for a set of more narrowly defined goods and services that are particularly relevant to children's well-being or their learning and development: (i) kids clothes and shoes, (ii) tutoring, (iii) toys, (iv) books and stationery and (v) extracurricular activities. Most importantly, the EVS contains information on the amount of advance child support payments received by each household member, which we use to document the substantial post-reform increase in these payments. Descriptive statistics are presented in Table 3.A.1.

3.4 Empirical Strategy

To identify the causal effect of advance child support payments on families' financial well-being, mothers' labor supply and welfare dependency as well as household spending, we exploit the expansion of eligibility to children aged 12 and older, along with the extension of benefit duration in 2017. In our main specification, we estimate a difference-in-differences (DiD) model, comparing single mothers with children aged 12

[20] These categories are: expenditures for housing and energy; transportation; post and telecommunications; health and personal care; clothing and shoes; interior furnishings, household appliances and items; ongoing household management; leisure, entertainment, and culture; restaurants, canteens, hotels, inns; food, beverages, tobacco products; education and child care; other goods and services; insurance contributions; formation of financial assets; final payments, installment payments, debit and overdraft interest; new loan acquisition and future outstanding payments.

to 17 ($Treat = 1$) to those with children aged 0 to 5, before and after the reform:[21]

$$Y_{it} = \beta_0 + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + X'_{it}\beta_4 + \delta_{rt} + \epsilon_{it} \tag{3.1}$$

Here, Y_{it} denotes the outcome variables capturing family finances, maternal labor supply, welfare dependency, and household spending for family i in year t. The indicator variable $Post_t$ equals 1 for periods after July 1, 2017, and 0 otherwise. The vector of control variables X'_{it} includes the mother's age (linear and squared), migration background, education level, and fixed effects for the number of children in the household. Additionally, the specification includes a full set of year-by-federal state fixed effects (δ_{rt}) to fully capture state-specific heterogeneity due to e.g. education or family reforms at the state level.[22] Under the assumption of common trends in absence of the reform and no-anticipatory effects, β_3 identifies the causal reform impact on the outcomes considered. We discuss and empirically examine threats to identification in Section 3.5.4.

We also explore distributional reform effects on mothers' working hours by assessing heterogeneity across the outcome distribution using quantile treatment effect models proposed by Havnes and Mogstad (2015) and Firpo et al. (2009). This method assesses how mothers below a given percentile respond to the reform, relative to the control group. The RIF-DiD estimator relies on the identifying assumption that, in the absence of the reform, changes in the proportion of mothers at specific percentiles would have been the same across treatment and control groups.

To examine dynamic treatment effects and assess the validity of the parallel trends assumption, we estimate event-study specifications based on Equation 3.1, interacting the treatment indicator with year-specific dummy variables t:

$$Y_{it} = \gamma_0 + \gamma_1 Treat_i + \sum_{t \neq 2017} \gamma_2^t (Treat_i \times Year_t) + X'_{it}\gamma_3 + \lambda_t + \mu_r + \delta rt + \epsilon_{it} \tag{3.2}$$

In this specification, the year 2017 is omitted and serves as the reference period, as the reform took effect on July 1, 2017, and advance child support payments were

[21] We compare within single-mother families rather than against couple households, since single mothers with children of different ages still face similar institutional environments whereas couple families are subject to fundamentally different support and constraints, such as joint taxation and intra-household insurance. This makes comparable trends and responses to other policies and societal changes far more plausible within single mothers.

[22] Including state-by-year fixed effects improves the precision of our estimates, though results are very similar when using only state fixed effects (see Appendix Table 3.A.3).

primarily made starting in 2018 due to administrative staff shortages (Rehwald, 2018).[23] The coefficients γ_2^t capture the differential outcomes for the treatment group in each year relative to 2017, thereby allowing us to assess both pre-trends and post-reform dynamics in mothers' labor supply, financial situation, and household spending. The treatment indicator, control variables, and fixed effects are defined as in Equation 3.1.

3.5 Results

3.5.1 Effects on the Families' Financial Situation

We begin by estimating the reform's impact on the likelihood of receiving advance child support and on the average payment amount (Column 1 and 2 in Table 3.2). Consistent with aggregate numbers in Figure 3.1, we find that the reform significantly increased the likelihood of receiving advance child support payments of single-parent households with children above the age of 11: estimates amount to about 27 percentage points (with a pre-reform mean of 5 %[24]). The average monthly amount received increased by about 70 € in 2008 prices. Combining theses numbers suggests that families who receive advance child support payments post-reform almost all get the full amount. Appendix Figure 3.A.6 confirms that the share and the amount obtained by treatment and control group move in parallel before reform implementation and only increased sharply in 2018 for our treatment group while the control group exhibits no change in trends.

We next examine potential spillovers on private support payments. Understanding how private transfers respond to publicly funded child support is critical, as such programs may alter fathers' incentives to contribute. On the one hand, the reform could discourage payments by reducing the perceived urgency if mothers are already receiving public support. On the other hand, it may encourage compliance as the state – unlike individuals – may have stronger enforcement mechanisms to recover owed amounts.[25] Moreover, fathers may become aware of mothers' application for advance support, increasing the likelihood of voluntary payment from the outset to avoid legal disputes. Finally, some fathers may face moral or social pressure to contribute once the state assumes their obligations, perceiving public involvement as a negative signal of non-compliance.

[23] Figure 3.A.4 also demonstrates that public spending only began to increase substantially in 2018.

[24] The pre-reform mean is above zero because families in the treatment group are allowed to have children aged 6 to 11 as well who might have been eligible before the reform.

[25] When mothers receive advance payments, they transfer their child support claim to the state, which then seeks reimbursement from the non-resident parent through wage garnishment or debt collection.

Results in Table 3.2 indicate no crowding-out of private child support payment. On the contrary, the estimate in Column 3 shows an increase in private support payments by about 5 percentage points for mothers with children aged 12 to 17, relative to those with children under the age of 6 (14 % of the pre-reform mean, 14 % of a standard deviation).

Table 3.2: Effects on Public and Private Support Payments and Mother's Income

	(1)	(2)	(3)	(4)
	Advance child support payments		Private support payments	Log(Income)
	yes	amount		
Treat × Post	0.271***	67.9***	0.052***	0.033***
	(0.052)	(12.2)	(0.009)	(0.009)
Observations	3,469	3,469	61,798	74,198
Pre-reform mean	0.05	10.15	0.37	7.28

Notes: Difference-in-differences coefficients from Equation 3.1 using data from the Income and Expenditure Survey in Columns 1 and 2 and data from the Microcensus in Columns 3 and 4. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Amount of advance support payments in EUR and 2008 prices. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Income and Expenditure Survey (2008, 2013, 2018), German Microcensus (2012-2021), weighted, own calculations.

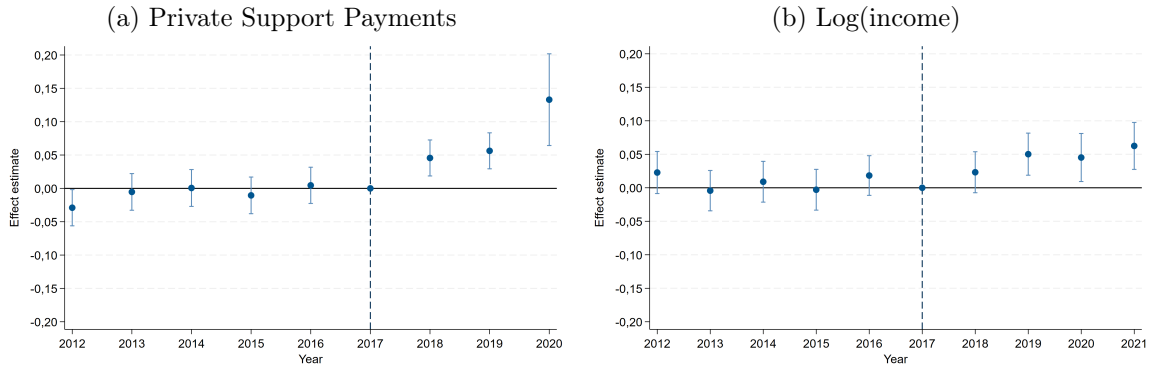
As a second aspect of evaluating whether the additional cash transfer improves the financial situation of single-parent families, we examine mothers' income, which includes public payments such as advance child support. Column 4 of Table 3.2 indicates that the increase in both public and private support resulted in a 3% rise in mothers' net income. This relatively small effect size may arise from opposing influences: while advance child support provides additional funds to families, the requirement for parents to be employed to receive these payments may reduce welfare dependence, thereby diminishing the overall impact on household income (see Chapter 3.5.2) .

Figure 3.2 illustrates the dynamic treatment effects on private support payments and income. It plots the interaction between the treatment group and each year relative to 2017, the final pre-reform year. For private support payments, the pre-reform coefficients (2012–2016) show no statistically significant differences, supporting the parallel trends assumption and thus a causal interpretation. Post-reform, all interaction coefficients are positive and statistically significant, indicating increases in private support payments of 5 to 14 percentage points.[26] Event study graph B likewise shows no statistically significant pre-trends for income. Treatment effects become statistically

[26] As noted in Section 3.3, the 2020 sample for this variable is considerably smaller, resulting in wider confidence intervals. This also explains why the overall DiD estimate (5.2 percentage points) aligns more closely with the 2018 and 2019 effects and is not strongly influenced by the 2020 estimate (see Appendix Table 3.A.3).

significant from 2019 onward, ranging between 5 and 6 percentage points annually – nearly twice the magnitude of the overall treatment effect reported in Table 3.2. The delayed response in income aligns with the notion that labor supply adjustments are subject to short-term rigidities in working hours.

Figure 3.2: Event Study Analysis: Support Payments and Income



Notes: This figure presents event study estimates of the reform's impact on private support payments and log(income), relative to the reference year 2017, along with 90 % confidence intervals. The treatment group consists of single mothers with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021), weighted, own calculations.

Heterogeneity analyses in Appendix Table 3.A.2 suggest that effects are driven by families with only one child – specifically, families where the eligibility for advance child support depends solely on this child – compared to families that may additionally have a younger child (aged 6–11) who could have been eligible for advance payments prior to the reform. The effects are also slightly larger in East Germany and among mothers without a university degree. We note, however, that coefficients are not significantly different across subgroups due to smaller sample sizes, implying that the analysis should be interpreted as suggestive evidence. However, the larger effects observed for mothers without a university degree indicate that the reform particularly benefited lower-income mothers (the average pre-reform income for those without a university degree was 1,360€ compared to 2,230€ for those with a degree) and may have contributed to reducing income inequality among single-parent households.

To sum up, the reform not only lead to an increase in public advance child support payments but also increases in private payments to single-mothers, improving their overall financial situation.

3.5.2 Effects on Maternal Labor Supply and Welfare Dependency

We next turn to the effects of the reform on resident parents' labor supply, with particular attention to the extent to which the reform's design shaped both labor supply behavior and welfare dependency. A condition for advance child support payments is that children qualify only if neither they nor their resident parent receives welfare benefits (SGB II), or if the resident parent receiving such benefits has a gross monthly income of at least €600 (§1 UhVorschG). This income threshold may incentivize parents to seek employment or increase their working hours to maintain access to the child support benefit.[27] Conversely, these payments may serve as a partial income replacement, potentially reducing the urgency to work or making part-time employment more financially viable.

The DiD estimates in Table 3.3 suggest no significant impact on the extensive margin of labor supply: coefficients on mothers' employment probability are small in magnitude and precisely estimated. However, we find that mothers with children aged 12–17 are 2 percentage points less likely to receive welfare benefits after the reform compared to those with children under 5 (13 % compared to the pre-reform mean, 6 % of sd, Column 2). This decrease in welfare dependency seems to be primarily driven by an increase in working hours, and thus labor income. While the DiD coefficient in Column 3 of Table 3.3 suggests a modest, and quite imprecisely estimated, average impact on weekly working hours of about half an hour (1.7 % relative to pre-reform mean, 3 % of sd), we find much stronger effects for the probability to work 30 hours or more post-reform (3 percentage point increase, corresponding to 5 % of the pre-reform mean and 5 % of a standard deviation).

To more formally assess potential non-linearities in the impact of advance child support payments on working hours, we present quantile treatment effects in Figure 3.3 using the RIF-DiD estimator (see e.g., Havnes and Mogstad, 2015; Huebener et al., 2017). The reform seems to have the largest impact on the 45th and 70th percentiles of the working hour distribution – corresponding to 25 to 37 weekly hours – where mothers increased their weekly hours by 0.75 to 1 hour. This suggests that the reform primarily encouraged mothers who were already moderately attached to the labor market to extend their working hours, likely because the greater financial stability provided by advance payments allowed them to take on slightly longer schedules. By contrast, at the upper end of the distribution, i.e. mothers working 41 hours or more, we observe

[27] Eligibility for welfare benefits depends on whether household income is sufficient to meet subsistence needs, which varies with household circumstances (e.g., number of children). Consequently, individuals can maintain eligibility while employed, provided their income remains insufficient.

Table 3.3: Effects on Maternal Labor Supply and Welfare Dependency

	(1) Employed	(2) Welfare Benefits	(3) Working Hours	(4) Near Full-time Employment
Treat × Post	0.010	-0.024***	0.440*	0.027***
	(0.007)	(0.008)	(0.233)	(0.007)
Observations	75,277	61,798	75,233	75,233
Pre-reform mean	0.82	0.19	25.61	0.53

Notes: Difference-in-differences coefficients from Equation 3.1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: German Microcensus (2012-2021), weighted, own calculations.

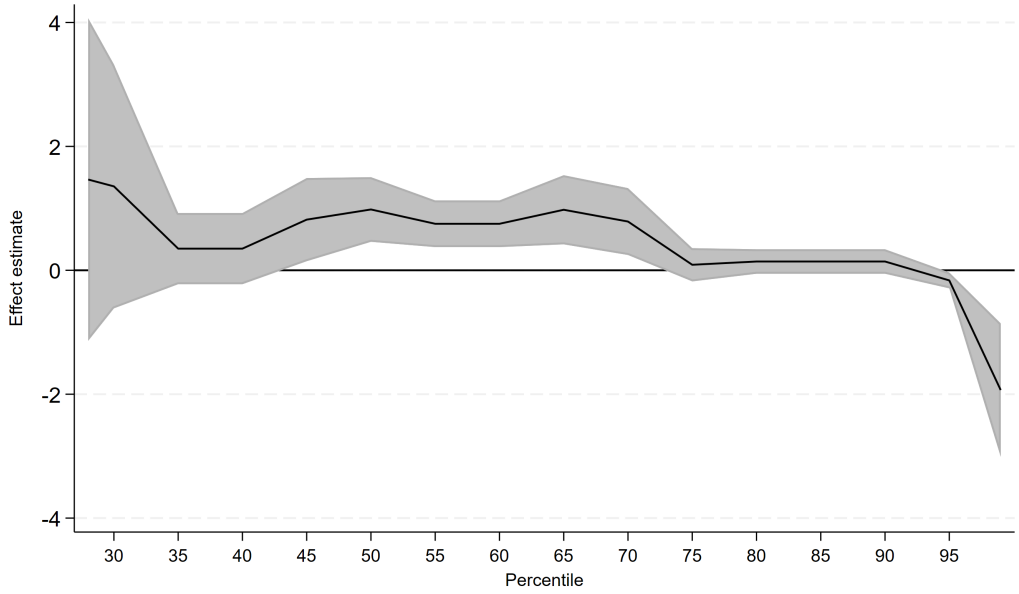
a reduction in hours, with those at the 95th percentile (around 54 hours) cutting back by 2 hours per week. This implies that some mothers with very high labor supply may have used the additional income to reduce extreme workloads, potentially substituting it for more time with their children. Meanwhile, we observe a precise null effect for mothers at the 75th to 90th percentiles (38 to 42 hours), indicating no change in this group's working hours, which may suggest contractual or time constraints limiting further increases.[28]

Figure 3.4 presents the event study estimates for labor market outcomes, examining both pre-trends and dynamic post-reform effects. In line with the DiD results, we observe no significant effect on mothers' employment rates. For welfare benefits, the pre-trends are flat, supporting the parallel trends assumption. Following the reform, we see a decline in benefit receipt, which is strongest in 2020.[29] Conclusions remain unchanged when using a variable indicating whether welfare benefits constitute the mother's primary income source – which is available for the full sample (see Appendix Table 3.A.3). For working hours, there is no consistent pre-trend in either total hours worked or the likelihood of near full-time employment, aside from a positive outlier in 2016. In 2019, we observe a statistically significant increase in total working hours of about one hour per week, while other post-reform coefficients are not statistically significant. The likelihood of near full-time employment starts to increase in 2019 – corresponding to a 5 to 6 percentage point increase each year. The slight delay in the

[28] There is also some indication of labor market entry, as coefficients at the lower end of the distribution are positive, though imprecisely estimated and not statistically significant, which is in line with our small and insignificant DiD estimate.

[29] As noted in Section 3.3, in 2020 this variable was only captured in the EU-SILC subsample, resulting in a smaller sample size and a wider confidence interval in this year. This also explains why the overall DiD estimate (2.4 percentage points) aligns more closely with the 2018 and 2019 effects and is not strongly influenced by the 2020 estimate.

Figure 3.3: Quantile Treatment Effects: Mother's Working Hours

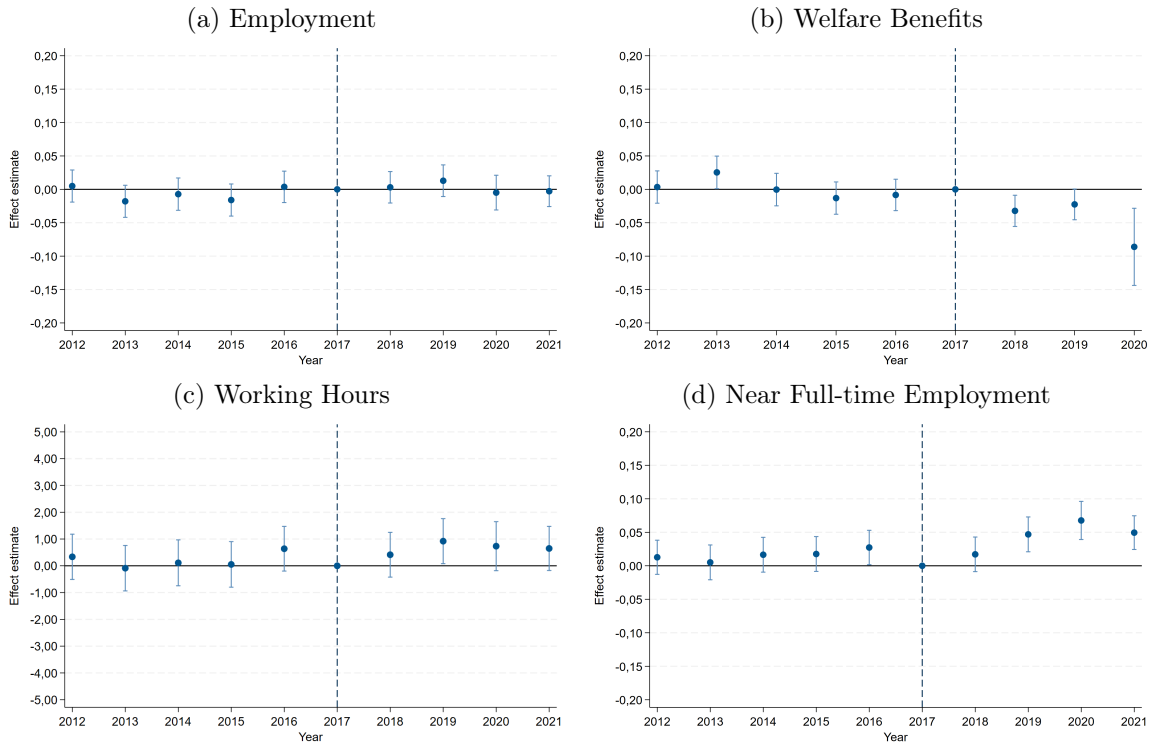


Notes: This figure displays quantile treatment effect (QTE) estimates from the RIF-DiD estimator, along with 90 % confidence intervals. The treatment group consists of single mothers with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021), weighted, own calculations.

response of working hours can be attributed to the requirement that individuals must formally request adjustments to their work schedules. Consequently, an immediate adjustment would have been implausible.

Consistent with the outcomes reported in Section 3.5.1, we find that the effects of the reform are more pronounced for single-mothers with lower education (Appendix Table 3.A.2). Mothers without university education – those with vocational training or no post-secondary degree – experience gains on both the extensive and intensive margins of labor supply: they are more likely to be employed, work more hours and are less likely to receive welfare benefits (Panel C of Table 3.A.2). For single mothers with only one child, we additionally see an increase at the extensive margin of employment. When differentiating by East and West Germany, we find increases in labor supply in both regions, with larger effect sizes in the East. Notably, pre-reform labor market outcomes among single mothers are very similar in both regions, although these regions differ markedly in norms regarding maternal labor supply in the general population (e.g., Gambaro et al., 2023; Jessen et al., 2024). It should be noted that coefficients differ significantly across subgroups only for welfare benefits when comparing East and West

Figure 3.4: Event Study Analysis: Maternal Labor Supply and Welfare Dependency



Notes: This figure presents event study estimates of the reform's impact on employment, receipt of social benefits, and working hours, relative to the reference year 2017, along with 90 % confidence intervals. The treatment group consists of single mothers with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021), weighted, own calculations.

Germany or by educational attainment. For all other outcomes, subgroup differences are not statistically significant and therefore remain merely suggestive.

In sum, the reform reduced mothers' reliance on welfare benefits, driven by increases in working hours at the intensive rather than at the extensive margin: mothers previously working 25-37 hours per week expanded their hours, while average employment rates remained unchanged.

3.5.3 Effects on Family Spending and Child-Related Investments

Finally, we examine how changes in child support payments, along with any resulting shifts in labor market behavior, affect family spending patterns, especially on goods and services that promote children's human capital and overall well-being. Previous research documents both tighter budget constraints of single-parent families (see also Appendix Figure 3.A.1 and 3.A.2) and systematic differences in spending between

single- and two-parent households (e.g. Ziol-Guest et al., 2006), suggesting that additional transfers might play a major role in shaping expenditure patterns.

Table 3.4 reports estimates for family's spending composition and spending on child-specific goods. Each row shows the results for a different dependent variable. We find the reform to significantly increase monthly spending on food and beverages by about 32€ relative to a pre-reform mean of 342€ (9 %).[30] The estimate on rent is of similar size, but much less precisely estimated and thus insignificant. For all other spending categories, we fail to detect any significant changes, though we note the limited data size and the fact that in the EVS only one post-reform period (2018) is available yet. Thus, we might fail to detect changes in consumption that require an adjustment period, such as shifts in rent expenditures when moving to a new flat.[31] An alternative to examining the total spending amount is to look at the share of expenditures for each spending category. Results are reported in Appendix Table 3.A.4 and show that budget shares were unaffected by the reform, suggesting that it did not alter consumption preferences in relative terms (i.e., the marginal utility derived from each consumption bundle).

For expenditures related more directly to children's human capital and overall well-being, results in Table 3.4 suggest that the reform increased spending on tutoring as wells as books/stationery and toys. Post-reform treatment families spend about 12€ more on tutoring and 3€ more on toys (40 % relative to pre-reform mean).[32]

Overall, these findings indicate that additional transfer payments to mothers are primarily allocated toward everyday and child-related expenditures, consistent with previous evidence (e.g., Armand et al., 2020; Gruber, 2004).

3.5.4 Robustness of the Results

In this section we conduct a number of robustness checks to support the validity of our empirical findings. Event-study results in Figures 3.2 and 3.4 already confirmed that outcomes for single-mothers with children above the age of 11 relative to single-mothers with children below age 6 followed parallel trends before reform implementation, and only started deviating post-reform.

[30] Examining single goods within this categories shows that a small part of this increase is also driven by an increase in expenditures for cigarettes. This increase in temptation goods is in line with findings from the child benefit literature in Germany (Stichnoth et al., 2018).

[31] We are also not able to show meaningful event study results when using EVS data as we only have 3 survey periods in total. Note that the EVS weighting and sampling procedure follows mandatory MZ sampling, implying that assumptions valid for MZ are also likely to apply to the EVS data.

[32] Note that we have to base our estimation results for these child-specific goods on a different control group, namely two-parent families with children aged 12-17. The reason is that almost all child-specific goods are not relevant for children below age 6.

Table 3.4: Effects on Family Spending Composition and Child Specific Goods

Dep. variable	(1) Pre-reform mean	(2) Reform effect	(3) N
Food and beverages	342.4	32.0*** (11.5)	3,469
Clothes, shoes, health, care	194.6	5.5 (12.8)	3,469
Kids clothes and shoes	50.1	2.0 (2.55)	13,273
Rent (incl. heating, etc.)	726.4	25.1 (20.6)	3,469
Traffic	244.7	-80.0 (86.7)	3,469
Education	29.8	-10.8 (7.8)	3,469
Tutoring	16.6	12.3*** (2.8)	13,273
Leisure, entertainment, culture	305.6	4.5 (22.7)	3,469
Toys	9.2	3.5** (1.5)	13,273
Books and stationery	16.1	2.2* (1.3)	13,273
Extracurricular activities	14.2	0.8 (1.9)	13,273
Household appliances, home repairs, etc.	105.2	-15.8 (19.0)	3,469
Other goods and services	169.5	6.9 (10.8)	3,469

Notes: Difference-in-differences coefficients from Equation 3.1. For broader consumption categories, the treatment group consists of single mothers with children aged 12–17, while the control group comprises single mothers with children aged 0–5. For child-specific goods, the control group instead consists of two-parent families with children aged 12–17, since these goods are not relevant for younger children and thus. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Income and Expenditure Survey (2008, 2013, 2018), weighted, own calculations.

Although we do not observe statistically significant pre-trends, one potential concern is that treatment and control groups may have been affected differently by broader societal or political developments that roughly coincide with reform-implementation. This concern might be particularly relevant given their differing age structures (mean age of mothers of 39 vs. 31 years; see Table 3.1). To address this, we first note that our estimates are not sensitive to including state-by-year fixed effects, i.e. when flexibly controlling for any family or educational reforms implemented at the state level such as changes in state parental allowance (Gathmann and Sass, 2018), abolishment of daycare fees (Huebener et al., 2020) or changes in the child benefit supplement[33] which are likely to impact treatment and control groups differently (see Panel a in Appendix Table 3.A.3). Controlling for this regional heterogeneity also considers broader economic shocks to local labor markets. Second, we use mothers with children aged 12 to 17 who live with the child's father as a placebo treatment group. These couple-families are more

[33]Since 2005, Germany has provided a child benefit supplement (Kinderzuschlag) to families whose income is sufficient to cover their own needs but not those of their children. Starting in 2016, the benefit was gradually increased to account for inflation and rising living costs by about 10€ yearly. It is estimated that around 2.5 % of households with children receive it. This gradual and small increase should not impose any threat to our identification, though we note that the share of receiving households is likely to be higher in our control group due to lower average working hours and income.

similar in terms of age but are not affected by the reform (see Panel b in Appendix Table 3.A.3). These estimates are significantly different (smaller in magnitude in absolute terms) than our main estimates, except for overall working hours and labor supply at the extensive margin where we find no significant overall effect in our main estimation. For income, we also observe a positive effect in the placebo test, even substantially larger than our main estimate. When examining event-study versions of the effect on income using this placebo group, it is evident that the significant DiD coefficient is driven by a non-parallel trend which is smooth around reform implementation (see Appendix Figure 3.A.7).[34] Third, we allow the correlation with our set of control variables to be different pre- and post reform by interacting each control variable with a post dummy (Panel c in Appendix Table 3.A.3). Overall, these checks indicate that our results are not driven by time-varying confounders, such as policy changes, compositional shifts or other factors that differentially affect outcomes of the treatment or control group around reform implementation.

Another important possible violation of our identifying assumption is that changes in public child advances may affect the likelihood of parental divorce or separation, thus leading to selection into our primary analysis sample. However, in our data we find no such evidence: marriage probabilities of mothers with children above age 11 vs. mothers with children below age 6 do not change discontinuously around the date of reform implementation (DiD coefficient: -0.0017 with se of 0.0020, 0.2% relative to pre-reform mean of 78%).

Next, using data from Google searches[35] in Germany, we investigate population awareness and provide evidence against anticipatory effects. Appendix Figure 3.A.8 displays two peaks in Google searches for the term *Unterhaltsvorschuss*: one in January, when the reform was discussed and the draft law was proposed, and another in July 2017, when the law was adopted and became effective. As we consider the entire year of 2017 as the pre-reform period in our event studies, the first peak in January 2017 should not pose a threat to our identification strategy. The graph also indicates that prior to 2017, the level of search interest was very low and stable, and following the

[34] The placebo specification on private support payments yields a positive effect, but the estimate is substantially smaller than in our main analysis and statistically different (χ^2-test p-value = 0.001). The estimated effect on the receipt of social benefits is positive in this specification – opposite in direction from our main estimate. The effect on overall working hours in this placebo test is similar to our main result, which raises some doubts about the robustness of this particular outcome. However, for the probability of near full-time employment, the placebo effect is considerably smaller, statistically insignificant, and significantly different from our main estimate.

[35] Google Trends data have been shown to be reliable proxies for individual behavior in various contexts, including job searches (Baker and Fradkin, 2017) and domestic violence (Anderberg et al., 2022).

peaks in 2017, the level remained elevated, demonstrating that overall awareness of advance child support substantially increased with reform implementation.

We also test wether our estimates are sensitive to the exclusion of families with children above age 5 and below age 12.[36] Children in this middle age group were already eligible for advance child support before the reform, as eligibility previously extended to age 11 and was limited to a maximum of six years. Thus, if parents separated later in the child's life, the child likely received support pre-reform. However, the reform eliminated the duration limit, potentially expanding eligibility for children whose parents separated shortly after birth. As a result, the expected reform effect for this group is ambiguous, and the inclusion of these families may introduce some bias into our main estimates. To assess this, we conduct a robustness check excluding families with children aged 6–11. As shown in Appendix Table 3.A.3 (Panel d), the estimated effects become even larger under this stricter definition, supporting the validity of our main results.

Finally, we assess whether the exclusion of the year 2020 affects our estimates. The rationale for this exclusion is that, since 2020, private support payments and welfare receipt are only surveyed in the 12 % EU-SILC Microcensus subsample and refer to the previous calendar year. Consequently, we utilize the 2020 and 2021 surveys to capture these outcomes for the years 2019 and 2020, respectively, which results in our 2019 sample combining two waves, whereas the 2020 sample – based solely on 2021 EU-SILC data – is smaller. The robustness check presented in Panel e in Appendix Table 3.A.3 confirms that our findings hold without the EU-SILC waves. For welfare benefits, we test whether using an alternative outcome variable, which indicates whether welfare benefits constitute the mother's primary income source and is available for the full sample throughout the study period, changes our results. The estimation in Appendix Table 3.A.3 (Panel f) is very similar to our main estimate. Additionally, results in Panel g of Table 3.A.3 also indicate that results remain stable when not applying the provided survey weights.

3.6 Conclusion

The question on how to effectively mitigate economic disadvantages to single parenthood is of perennial importance given the high and increasing share of single-parent households across many industrialized countries and the disadvantages these families

[36] Remember that our treatment and control groups are defined as mothers whose children fall into a specific age range (12–17 vs. 0–5 years), excluding those with children in both age groups. However, both groups may include mothers who also have children aged 6–11.

face along various dimensions (e.g. Boll and Schüller, 2023; Harkness, 2022). We provide first causal evidence on the economic impact of public advance child support payments, a policy instrument widely implemented across (European) countries but scarcely examined in the previous literature. For identification, we exploit a reform to Germany's advance child support system that extended the duration of benefits and expanded eligibility to older children in 2017. Our findings show that the reform improved the financial situations of single mothers without crowding-out private child support contributions. On the contrary: private support payments increase post-reform, likely reflecting greater awareness and stronger enforcement threats faced by fathers due to the transfer of legal child support claims from the mother to the state. We also find that the design of advance child support programs plays a major role: since eligibility for public payments was tied to the economic independence of single-parent households, we observe a decrease in the probability of receiving welfare benefits, which appears to be driven by exits from welfare due to increases in labor supply at the intensive rather than the extensive margin. We also find that changes in child support payments, along with the resulting shifts in labor market behavior, affect family spending patterns. The reform led to higher household spending on food, beverages, and goods closely tied to child development and well-being.

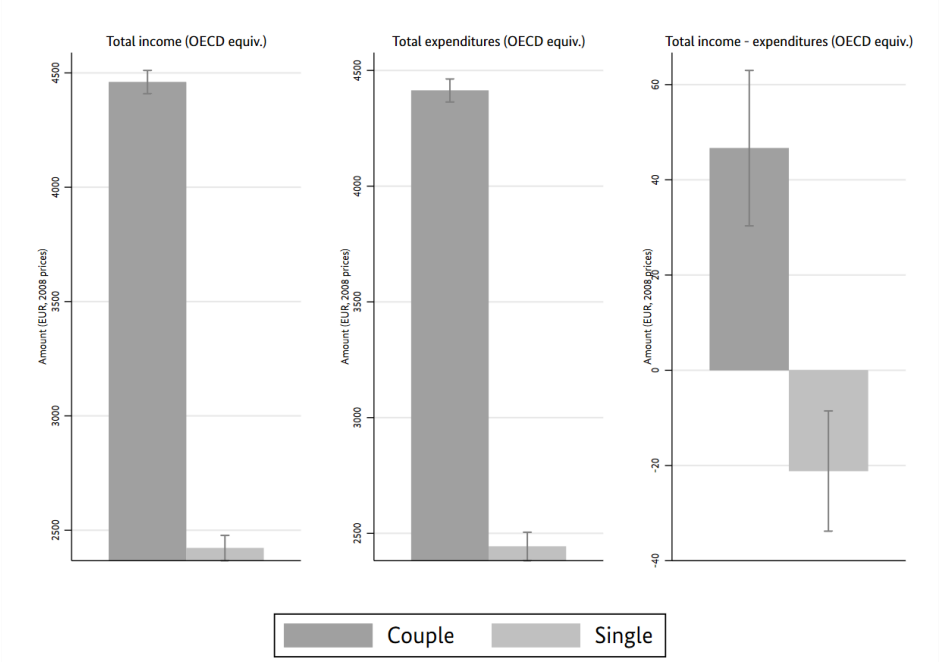
Taken together, this study highlights a number of novel findings on the role of advance child support schemes. First, we establish their economic importance for single-parent households. Second, we demonstrate that program design is pivotal, particularly in terms of (i) the interaction between child support schemes and welfare systems, and (ii) enforcement arrangements. These findings carry significant implications for child support policy design more broadly, including in countries such as the US and UK, where the current focus is on enforcement rather than on pre-financing mechanisms. Descriptive evidence from Huang and Han (2012) shows that improvements in US child support enforcement led to a 12 % increase in support orders and a 14 % increase in payments. Our results underscore the central role of enforcement and suggest that state-led collection is more effective than leaving the responsibility to resident parents.

When interpreting the results of our study, several limitations have to be kept in mind. First, because the extension of payment duration and the expansion to older children were implemented simultaneously, we cannot disentangle these two effects, although such differentiation would likely be of substantial interest to policymakers. Second, we estimate effects for single-parent families with children above age 11. Results might differ when policies target younger children or low income single-parent households, such as schemes in Belgium or Spain (Hakovirta et al., 2022). Third, a comprehensive policy evaluation should assess the impacts on children's outcomes, as

mother's employment and job stability are linked to less harsh parenting and fewer behavioral problems in children (Dunifon et al., 2003; Johnson et al., 2012). Therefore, increases in mother's working hours and reductions in welfare dependency may have significant implications for children, beyond the more indirect effect associated with changes in households consumption that we find. Fourth, regular public child support payments provide income stability to single-parent households, which may substantially reduce family stress – a key pathway through which the effects of poverty and single parenthood are transmitted to children (Heintz-Martin et al., 2022). Future evaluations should also consider related parental outcomes. Fifth, while we find that higher public child support payments also raise private payments, we cannot explain fathers' motives. Given low recovery rates in Germany and elsewhere (BMFSFJ, 2023), future research could examine fathers' responses. This will likely require collection of additional data, as sample sizes of non-resident fathers are too small in the existing datasets.

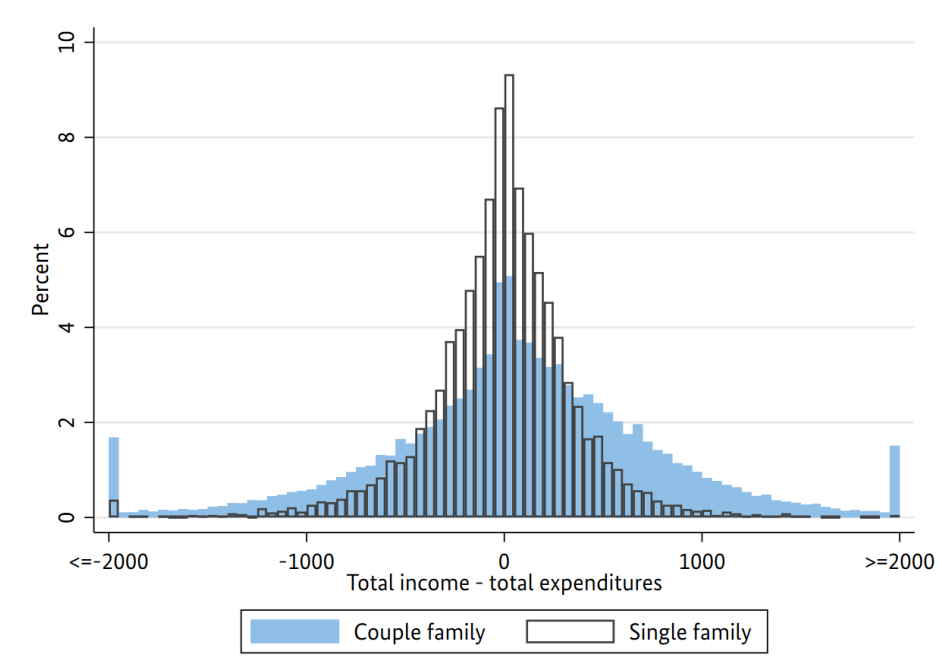
3.A Appendix

Figure 3.A.1: Income and Expenditures by Household Type



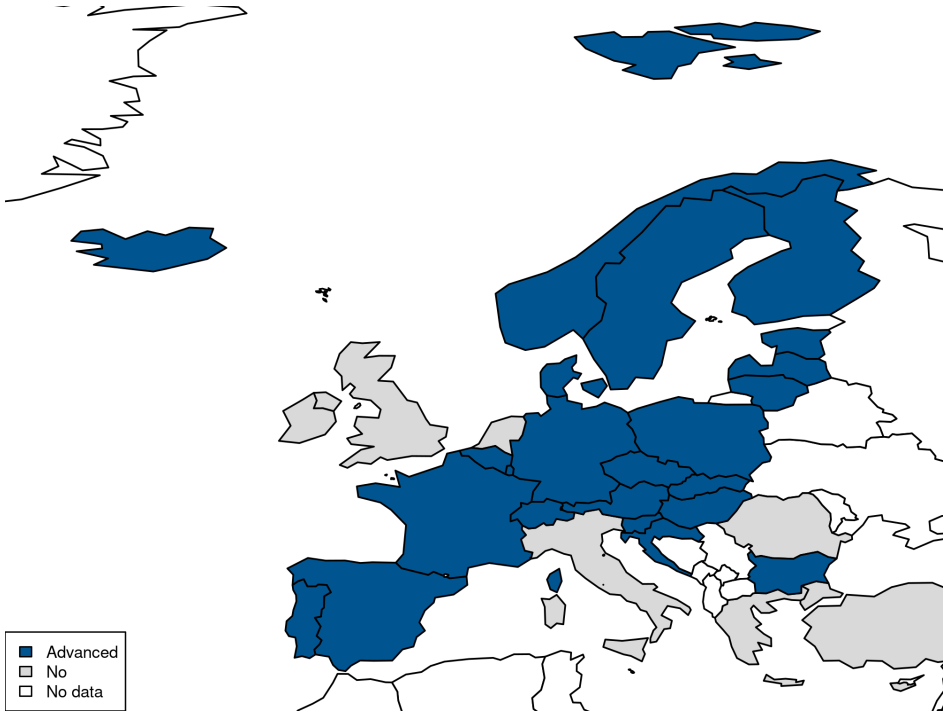
Notes: The figure shows average total incomes and expenditures (deflated in 2008 prices and OECD equ. adjusted) for single and couple households with children below age 18. Total income is composed of income from employment, both dependent and self-employed, income from assets (e.g., interest, rents), public transfer payments (e.g., pensions, social assistance, housing allowance, etc.) as well as private transfer payments and income from subletting. Total expenditures contain all consumption outlays together with certain recurring obligations. This measure constitutes the basis for poverty and distributional analyses, as well as for constructing the consumer price index basket. This data is collected in detailed household diaries (see Section 3.3.2). *Source:* Income and Expenditure Survey (2008, 2013, 2018), weighted, own calculations.

Figure 3.A.2: Distribution of Total Income - Expenditures by Household Type



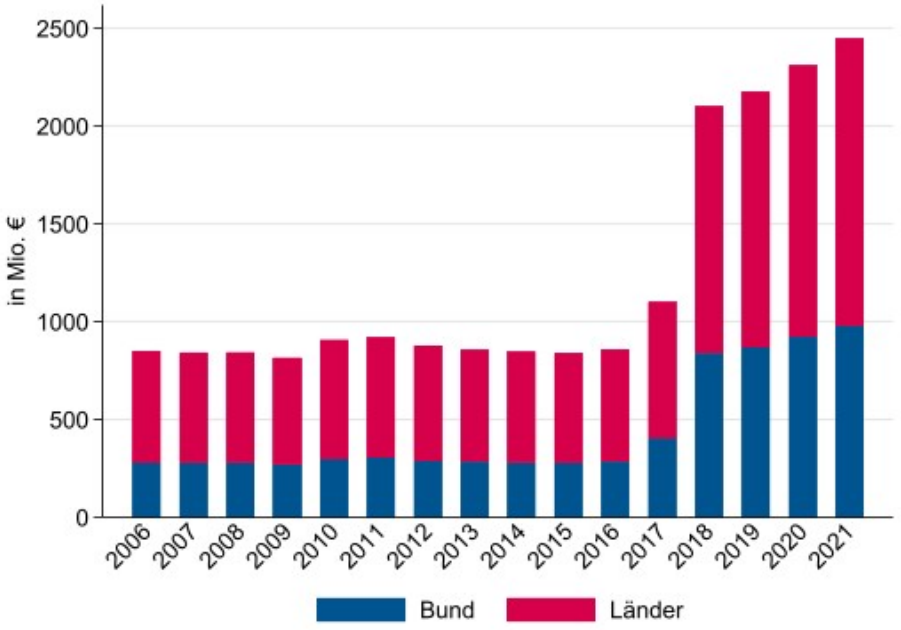
Notes: The figure shows the distribution of total incomes minus expenditures (deflated in 2008 prices) for single and couple households with children below age 18. For a detailed explanation on what these measures compose see Appendix Figure 3.A.1. *Source:* Income and Expenditure Survey)2008, 2013, 2018), weighted, own calculations.

Figure 3.A.3: Advance Child Support in Europe 2023/2024



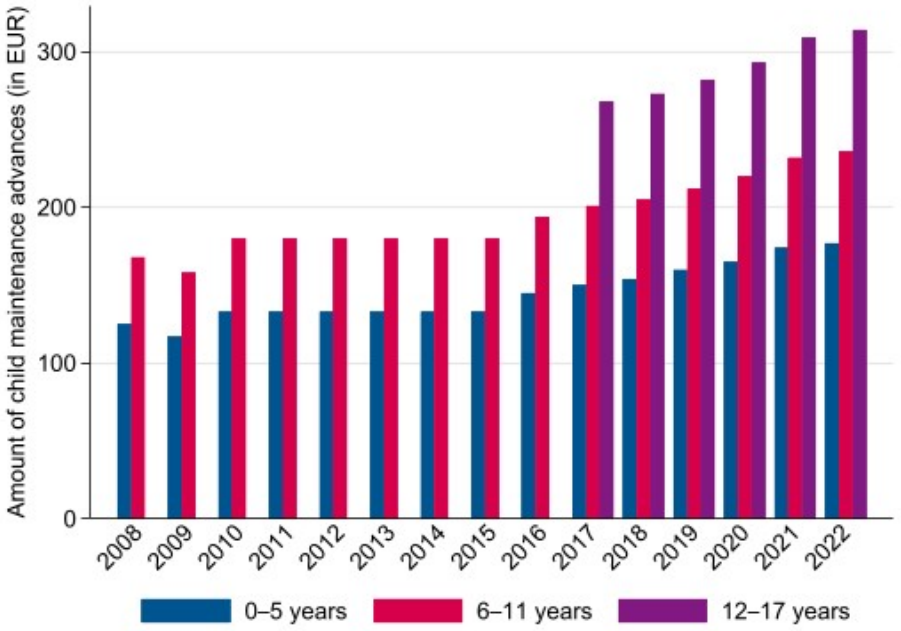
Notes: The figure shows which countries have an advance child support system in place. *Source:* OECD (2025a) based on National governments and National Legislative Framework, own visualization.

Figure 3.A.4: Public Spending on Advance Child Support Over Time



Notes: The figure shows the amount spend by the federal government (*Bund*) and by the federal states (*Länder*) on public child maintenance over time (in Mio. €). *Source:* UVG Geschäftsstatistik (2025), own calculations.

Figure 3.A.5: The Evolution of the Amount of Advance Child Support Over Time



Notes: The figure shows the maximum amount of advance child support by age group and over time. *Source:* UVG Geschäftsstatistik (2025), own calculations.

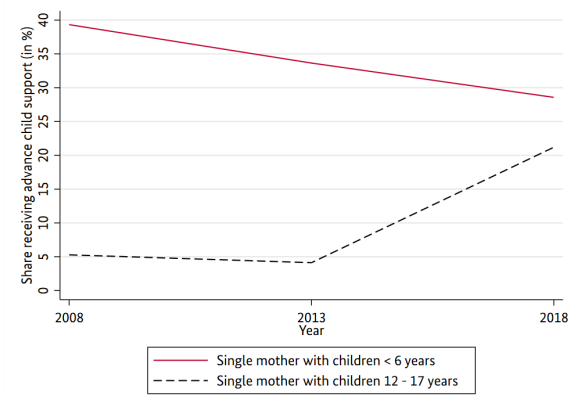
Table 3.A.1: Descriptive Statistics EVS

	Overall Sample	Treatment Group	Control Group
Outcome variables			
Advance child support payments			
Advance child support	0.16 (0.37)	0.10 (0.30)	0.37 (0.48)
Monthly amount of advance child support (EUR)	35.63 (99.52)	26.44 (94.98)	66.49 (107.94)
Monthly expenditures by category (in EUR)			
Food and beverages	329.4 (135.9)	347.8 (136.3)	267.9 (115.1)
Clothes, shoes, health, care	180.2 (180.6)	192.5 (187.3)	139.0 (149.0)
Kids clothes and shoes	43.8 (58.4)	43.9 (63.3)	43.5 (37.8)
Rent (incl. heating, etc.)	695.7 (266.4)	727.2 (273.7)	589.8 (208.0)
Traffic	234.2 (585.3)	241.4 (494.3)	210.0 (820.1)
Education	35.0 (71.6)	29.4 (69.4)	53.7 (75.8)
Tutoring	11.7 (61.9)	13.7 (65.5)	4.7 (47.0)
Education and Care	24.8 (77.5)	11.5 (52.3)	69.7 (120.2)
Leisure, entertainment, culture	290.0 (260.9)	316.2 (275.3)	202.1 (179.7)
Toys	11.6 (27.0)	10.1 (26.7)	16.6 (27.5)
Books and stationery	15.3 (23.2)	16.4 (24.3)	11.6 (18.4)
Extracurricular activities	12.9 (30.7)	14.8 (33.6)	6.4 (16.4)
Household appliances, home repairs, etc.	102.1 (222.0)	107.6 (241.1)	83.4 (138.2)
Other goods and services	162.0 (129.6)	170.6 (137.9)	133.2 (90.8)
Control variables			
Age (in years)	38.38 (7.91)	40.93 (6.09)	29.80 (7.23)
Post-secondary degree			
No	0.22 (0.41)	0.21 (0.41)	0.26 (0.44)
Vocational	0.61 (0.49)	0.61 (0.49)	0.59 (0.49)
Tertiary	0.17 (0.38)	0.18 (0.38)	0.15 (0.36)
German Citizenship	0.99 (0.12)	0.99 (0.11)	0.98 (0.15)
Number of children in the household			
One child	0.55 (0.50)	0.51 (0.50)	0.66 (0.47)
2 children	0.36 (0.48)	0.39 (0.49)	0.27 (0.44)
3+ children	0.09 (0.28)	0.09 (0.29)	0.07 (0.26)
Observations	3,469	2,673	796

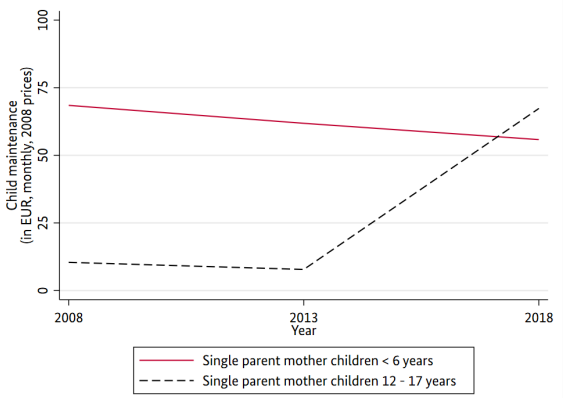
Notes: The numbers represent shares if no unit is given. Standard deviations in parentheses. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All monetary variables in EUR and expressed in 2008 prices. *Source:* Income and Expenditure Survey (2008, 2013, 2018), weighted, own calculations.

Figure 3.A.6: Advance Child Support Payments by Treatment Status

(a) Share (in %)



(b) Monthly amount (in €)



Notes: This figure displays (a) the share of single-mother households with at least one member receiving advance child support payments, and (b) the average monthly amount of advance child support received per household. *Source:* Income and Expenditure Survey (2008, 2013, 2018), weighted, own calculations.

Table 3.A.2: Heterogeneous Treatment Effects on Private Support, Income, Labor Supply, and Welfare Dependency

	(1) Private support	(2) Log(Income)	(3) Employed	(4) Welfare Benefits	(5) Working Hours	(6) Near full-time Employment
Panel A: By number of children						
One child	0.062***	0.047***	0.016**	-0.026***	0.558*	0.025***
	(0.011)	(0.011)	(0.008)	(0.009)	(0.295)	(0.009)
Observations	40,805	48,891	49,545	40,805	49,515	49,515
Pre-reform mean	0.34	7.26	0.84	0.17	27.21	0.59
Two or more children	0.030**	0.010	-0.007	-0.020	0.171	0.029**
	(0.015)	(0.016)	(0.012)	(0.014)	(0.385)	(0.012)
Observations	20,993	25,307	25,732	20,993	25,718	25,718
Pre-reform mean	0.42	7.33	0.78	0.25	22.49	0.43
Panel B: East vs. West Germany						
East Germany	0.069***	0.059***	0.020	-0.069***	0.875*	0.038***
	(0.017)	(0.015)	(0.013)	(0.015)	(0.473)	(0.014)
Observations	15,921	19,447	19,596	15,921	19,586	19,586
Pre-reform mean	0.32	7.21	0.78	0.29	25.88	0.60
West Germany	0.046***	0.023**	0.006	-0.008	0.262	0.021**
	(0.010)	(0.011)	(0.008)	(0.009)	(0.267)	(0.008)
Observations	45,877	54,751	55,681	45,877	55,647	55,647
Pre-reform mean	0.38	7.30	0.83	0.17	25.54	0.51
Panel C: By university degree						
No university degree	0.053***	0.035***	0.015*	-0.034***	0.645**	0.030***
	(0.009)	(0.010)	(0.008)	(0.009)	(0.260)	(0.008)
Observations	53,545	63,606	64,567	53,545	64,530	64,530
Pre-reform mean	0.36	7.22	0.80	0.22	24.55	0.51
University degree	0.037	0.011	0.011	0.011	0.058	0.0285
	(0.024)	(0.027)	(0.015)	(0.013)	(0.621)	(0.021)
Observations	8,253	10,592	10,710	8,253	10,703	10,703
Pre-reform mean	0.40	7.71	0.93	0.05	32.44	0.69

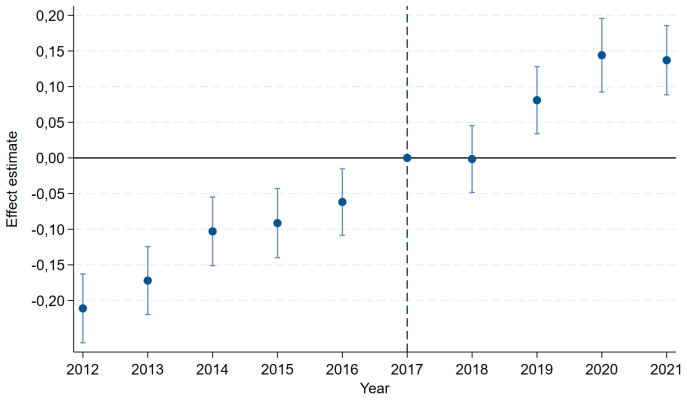
Notes: Difference-in-differences coefficients from Equation 3.1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* German Microcensus (2012-2021), weighted, own calculations.

Table 3.A.3: Robustness of Reform Effects on Private Support, Income, Labor Supply, and Welfare Dependency

	(1) Private support	(2) Log(Income)	(3) Employed	(4) Welfare Benefits	(5) Working Hours	(6) Near full-time Employment
Main estimate	0.052***	0.033***	0.010	-0.024***	0.440*	0.027***
	(0.009)	(0.009)	(0.007)	(0.008)	(0.233)	(0.007)
Observations	61,798	74,198	75,277	61,798	75,233	75,233
a) Excluding state-by-year FE	0.0507***	0.0305***	0.00703	-0.0194***	0.296	0.0220***
	(0.00870)	(0.00905)	(0.00660)	(0.00752)	(0.233)	(0.00718)
Observations	61798	74198	75277	61798	75233	75233
b) Placebo treatment	0.035***	0.204***	0.007	0.025***	0.415**	0.009
	(0.007)	(0.014)	(0.006)	(0.007)	(0.203)	(0.006)
Observations	181,551	219,173	222,623	181,551	222,500	222,500
Chi-test (p)	0.001	0.000	0.457	0.000	0.877	0.001
c) Interacting controls with post indicator	0.0421***	0.0267**	0.00791	-0.0376***	0.593*	0.0366***
	(0.0132)	(0.0132)	(0.0100)	(0.0113)	(0.358)	(0.0112)
Observations	61798	74198	75277	61798	75233	75233
d) Excluding families with 6-11 year olds	0.055***	0.047***	0.017**	-0.033***	0.668**	0.031***
	(0.010)	(0.011)	(0.007)	(0.008)	(0.268)	(0.008)
Observations	49,055	58,852	59,682	49,055	59,645	59,645
e) Excluding 2020	0.0492***			-0.0222***		
	(0.00883)			(0.00764)		
Observations	61,078			61,078		
f) Primary income source: WB				-0.0228***		
				(0.00631)		
Observations				75,277		
g) Not weighted	0.051***	0.031***	0.006	-0.023***	0.336	0.023***
	(0.009)	(0.009)	(0.006)	(0.007)	(0.226)	(0.007)
Observations	61,798	74,198	75,277	61,798	75,233	75,233

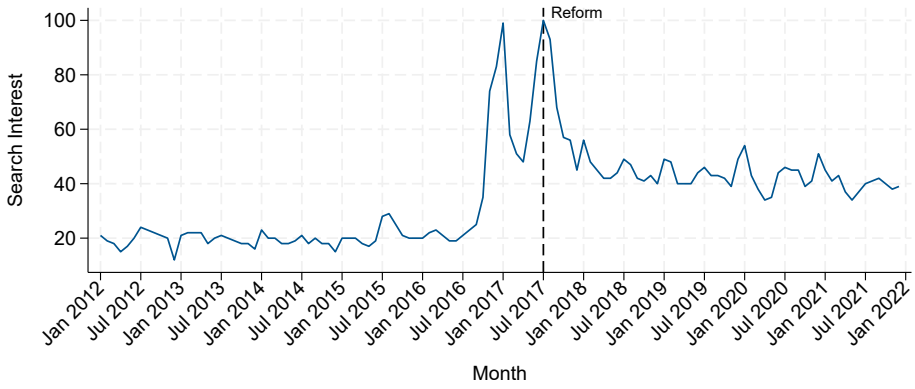
Notes: Difference-in-differences coefficients from Equation 3.1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* German Microcensus (2012-2021), weighted, own calculations.

Figure 3.A.7: Placebo Event Study: Income



Notes: This figure presents event study estimates of the reform's impact on mother's income, relative to the reference year 2017, along with 90 % confidence intervals. The treatment group consists of couple families with children aged 12–17, while the control group comprises those with children aged 0–5. Post-reform years are defined as 2018 and onwards, while all years up to 2017 are considered pre-reform. The regression controls for the mother's age (linear and squared), migration background, education level, and include fixed effects for the number of children in the household, year, federal state, and state-by-year interactions. *Source:* German Microcensus (2012-2021), weighted, own calculations.

Figure 3.A.8: Google Search Trends



Notes: The figure shows monthly Google search interest for the term "Unterhaltsvorschuss". The values represent search interest relative to the peak popularity of the term (100 = maximum interest). *Source:* Google analytics (retrieved 08/2025).

Table 3.A.4: Effects on Budget Shares: Family Spending and Child Specific Goods

Dep. variable: shares relative to total expenditures)	(1) Pre-reform mean	(2) Reform effect	(3) N
Food and beverages	0.11	0.01 (0.01)	3,469
Clothes, shoes, health, care	0.06	-0.00 (0.00)	3,469
Rent (incl. heating, etc.)	0.25	0.01 (0.01)	3,469
Traffic	0.07	-0.00 (0.01)	3,469
Education	0.01	-0.01 (0.00)	3,469
Leisure, entertainment, culture	0.09	-0.01 (0.01)	3,469
Household appliances, home repairs, etc.	0.03	-0.00 (0.00)	3,469
Other goods and services	0.06	0.01** (0.00)	3,469

Notes: Difference-in-differences coefficients from Equation 3.1. The treatment group includes single mothers with children aged 12-17, while the control group includes those with children aged 0-5. All regressions control for mother's age (linear and squared), migration background, education level, and number of children in the household fixed effects, year fixed effects, federal state fixed effects and state-by-year fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *Source:* German Microcensus (2012-2021), weighted, own calculations.

CHAPTER 4

Passport to Progress: The Effects of Birthright Citizenship on Siblings' Education[1]

4.1 Introduction

In most OECD countries, the number of immigrant[2] students is steadily growing – in Germany it rose from 13% to 26% over the past decade (OECD, 2022, 2023c). At the same time, immigrant students consistently obtain lower test scores compared with

[1]This work was supported by the Stiftung Ravensburger Verlag and Joachim Herz Foundation. This paper uses data from the National Educational Panel Study (NEPS; see Blossfeld and Roßbach, 2019), Remote Access. The NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi, Germany) in cooperation with a nationwide network. During the preparation of this chapter, the author used editGPT and ChatGPT to enhance readability and check the language. Following the use of this tool, the author reviewed and edited the suggestions as needed. This chapter was pre-published as a working paper as *Ziege, E. (2024). Passport to Progress: The Effects of Birthright Citizenship on Siblings' Education. BiB Working Paper 7/2024.* The policy report *Gambaro, L., Gutu, L., Schmitz, S., Spiess, C.K., and Ziege, E. (2024). Mütter mit Zuwanderungsgeschichte. Ihre Erwerbs- und Sorgearbeit, Geschlechternormen und schulischen Unterstützungsleistungen. BiB.Bevölkerungs.Studien 1/2024.* was published based on parts of this chapter.

[2]Following the definition provided by PISA, this article uses the term "immigrant children/students" to refer to children whose both parents immigrated to Germany. "Nonimmigrant children/students" refers to children with at least one parent born in Germany.

their nonimmigrant peers (at age 15, see OECD, 2023c).[3] The education gap between immigrant and nonimmigrant students is particularly large and persistent in Germany, despite a policy commitment to foster integration[4] since 2005 (Ammermueller, 2007). This disparity is concerning from an integration perspective and might exacerbate skilled-labor shortages. Labor shortages in OECD countries have grown sharply, with talent shortages reported by 75% of firms, up from 30% a decade ago (OECD, 2023d). Implementing policies that promote the educational attainment of immigrant children could increase the availability of skilled labor and alleviate labor shortages. The German government also sees closing immigrant-nonimmigrant education gaps as part of their strategy to tackle the talent shortage (BMAS, 2022).

In recent decades, countries like Germany have reformed their citizenship laws, with more nations adopting birthright citizenship. Globally, 31% of countries grant citizenship at birth, while 8% require a minimum period of parental residence (Gathmann and Garbers, 2023). Research shows that birthright citizenship can improve immigrant children's social integration and educational outcomes. However, less attention has been paid to potential spillover effects on other household members, such as older siblings.

Investigating spillover effects on siblings is crucial for evaluating the costs and benefits of citizenship reforms. As the per-case costs[5] of citizenship are relatively low in Germany – when compared to permanent residency, the counterfactual in this analysis – and are unlikely to exceed the associated benefits, the analysis focuses on assessing the magnitude and nature of those benefits. If citizenship policies that are advantageous for children also benefit their siblings, the policy evaluations would underestimate the benefits. Alternatively, they would overestimate them in the case of negative spillovers on siblings. Investigating spillovers can also enhance our understanding of the mechanisms underlying the effects of citizenship. Older siblings are also an important target population as they can serve as role models for younger siblings and their integration can make the family (economically) more stable.

[3]The gaps hold after accounting for relevant characteristics like the parents' socio-economic status.

[4]"Gesetz zur Steuerung und Begrenzung der Zuwanderung und zur Regelung des Aufenthalts und der Integration von Unionsbürgern und Ausländern" (see SVR, 2018).

[5]Most costs incurred through migration are borne by the state upon the granting of a permanent residence permit. Social benefits, for example, which constitute a major cost factor, can be accessed with a permanent residence permit and, therefore, do not represent additional costs of citizenship. The potential costs of citizenship are linked to the additional benefits mentioned in Section 4.2 like costs associated with an individual's participation in elections. Additionally, the formal act of naturalization incurs administrative costs.

Several reasons exist why citizenship may impact the older sibling's educational attainment. Citizenship in Germany provides several advantages[6] which are only granted to the focal child, who has received citizenship and do not extend to their siblings – unless they naturalize themselves.[7] First, a child's citizenship could reduce information barriers about citizenship and increase naturalization[8] among parents and siblings. Parents might perceive greater benefits from naturalization once one child is a citizen, prompting them to apply for citizenship themselves and, consequently, for other underage children. Older siblings might also naturalize as adults or their parents might apply for their citizenship via the transition rule.[9] If older siblings are more likely to gain access to citizenship, they may benefit directly like the focal child. This can be seen as a second spillover, as the effect on siblings' citizenship status may have significant additional consequences for their integration and other aspects of their lives.

Second, citizenship may foster family integration and increase the use of German within the household. Avitabile et al. (2013) find that a child's birthright citizenship raises parents' likelihood of interacting with Germans by 14 percentage points and of reading German newspapers by 30 percentage points in the early years after birth. While they do not observe significant changes in language use during this initial period, increased interaction and media consumption could impact parents' German proficiency and usage over time. Greater use of German at home may, in turn, enhance older siblings' language skills, potentially benefiting their academic performance.

Third, a child's citizenship might influence parental behavior and resource allocation. Avitabile et al. (2014) show that a child's citizenship decreases the parents' subsequent fertility, potentially increasing parents' financial and time resources per child. Parents may adjust how they divide this additional monetary and time budget between their children, depending on whether they see citizenship and their own investments as substitutes or complements (see Almond and Mazumder, 2013; Heckman and Mosso, 2014). If seen as complements, parents might focus on the focal child upon perceiving that the economic returns to investing in this child will be greater, potentially reducing

[6]These advantages include the right to pursue any profession, including civil service and the ability to work and study visa-free in all European Union (EU) countries (Integrationsbeauftragte, 2023, §9 AufenthG).

[7]Throughout the paper, I will use the term "focal child" to define children born around the citizenship reform in 2000 who were the main targets of the reform. The term "(older) sibling" refers to children in the same household as the focal child who were born before the focal child and before the citizenship reform in 2000.

[8]Naturalization is the legal process by which a non-citizen of a country acquires citizenship of that country.

[9]This transition rule enabled parents to apply for citizenship on the behalf of their children who were born between 1990 and 1999 under the same conditions as the studied birthright citizenship. For more details see Section 4.2.

support for older siblings. As parental involvement is strongly related to students' academic achievement (e.g., Fan and Chen, 2001), this could negatively impact siblings' education. If seen as substitutes, parents may prioritize older siblings to compensate for unequal external inputs – potentially having positive implications for siblings' education. Berry et al. (2020) show that parents are averse to inequality between their children and willing to forego earnings to equalize inputs. These dynamics may also change with child age or based on expectations of sibling support (see Becker and Tomes, 1976; Cunha et al., 2010).

Fourth, the younger sibling's access to citizenship may influence older siblings' identity and perceptions of their environment. Awareness of their sibling's citizenship could strengthen their identification with German society and motivate greater educational effort as a path to integration in the long-run. Additional channels not explored in this paper may also affect siblings, such as changes in sibling dynamics, which are often especially close in immigrant families (Nauck and Kohlmann, 1999). Changes in the focal child's endeavors may impact their siblings' academic pursuits. Greater success for the focal child may reduce the older siblings' need to provide academic support, as many immigrant parents expect their children to assist younger siblings (Nauck, 2000).

This paper investigates spillover effects of citizenship from focal children to their older siblings and potential mechanisms by studying the introduction of birthright citizenship in Germany. The reform automatically granted citizenship to immigrant children born since January 1, 2000, if at least one parent had resided in Germany for a minimum of 8 years. The implementation of this reform substantially increased the number of focal children with German citizenship (Statistisches Bundesamt, 2022b). Exploiting the exogenous access to citizenship, I use a difference-in-differences method comparing changes in outcomes between immigrant (treatment) and nonimmigrant (control) families before and after the reform to investigate the effects on focal children's and their older siblings' education. Spillover effects are identified by comparing older siblings born between 1990 and 1998 in households with an eligible focal child – born 2000 or later – to those with an ineligible focal child – born before 2000. I also employ an event study approach to test for differential pre-trends between the treatment and control groups. The analysis is based on data from the German Microcensus – a 1% population sample – and the German National Educational Panel Study (NEPS).

The findings demonstrate that granting birthright citizenship improves both the focal children's and their older siblings' educational outcomes. Citizenship increases the probability of completing secondary education on the academic track by 13 percentage points for focal children and by 6 percentage points for their older siblings

– about half the direct effect. Event study graphs strengthen the common trend assumption, thus supporting a causal interpretation of the reform estimates. The reform also increased naturalization among parents and siblings. Thus, some spillovers are direct effects of the sibling's access to citizenship. Additionally, families with a citizen child speak German more often. The results also suggest that spillovers stem from greater parental investment in older siblings, particularly parents attending school events, meeting teachers and helping with presentations, while investments in focal children remain constant.

This paper makes several important contributions to the literature. First, it is the first to investigate the effects of focal children's citizenship on family members other than parents. While prior studies explore how citizenship affects parents – regarding labor market participation, fertility, marriage, out-migration, and integration (Avitabile et al., 2013, 2014; Gathmann and Keller, 2018; Sajons, 2019) – I study its influence on older siblings of children granted birthright citizenship. This contributes to the small but growing body of research on the sibling spillover effects of political reforms. Previous studies have explored spillover effects in areas such as early childhood education, college major choice, grade retention, school performance, and school starting age (see Bennhoff et al., 2024; Bettinger et al., 2014; Dahl et al., 2024; Figlio et al., 2023; Goodman et al., 2015; Karbownik and Oezek, 2023; Landerso et al., 2020), with most studies focusing on younger siblings. By contrast, this paper analyzes spillovers from younger to older siblings, showing positive effects of birthright citizenship on older siblings' education and underscoring the importance of considering all potentially affected family members.

Second, the paper adds to research regarding the influence of citizenship on family's integration. Previous studies demonstrate that citizenship acquisition affects labor market participation and success (Bratsberg et al., 2002; Catron, 2019; Gathmann and Keller, 2018; Govind, 2021; Hainmueller et al., 2019), social integration (Hainmueller et al., 2017) and fear of deportation (Hainmueller et al., 2023).[10]. Further research focuses on implications of immigrants' legal status and deferred deportation under DACA or IRCA in the US (Cortes, 2013; Gihleb et al., 2023; Hsin and Ortega, 2018; Pope, 2016). Research focusing on children's educational achievement shows positive effects of (birthright) citizenship in the short- and medium-run, focusing on achievements at school (Cygan-Rehm, 2018; Felfe et al., 2020; Gathmann et al., 2021; Sajons and Clots-Figueras, 2014). However, we lack evidence regarding whether students complete school with improved accomplishments, such as attaining the highest academic track.

[10]For an overview of the previous literature, see Gathmann and Garbers (2023)

This study addresses this gap by investigating the long-term effects of citizenship up to high school completion.

Finally, the paper enhances our comprehension of the mechanisms underlying citizenship effects. It is the first to investigate how one family member's citizenship access affects naturalization decisions of other family members. It also provides insights into how parents allocate investments across children based on citizenship, drawing on rich data that enable a detailed analysis of parental behavior. Dahl et al. (2022) find that birthright citizenship reduces parental academic support for Muslim immigrant girls but increases it for non-Muslim immigrant boys.

The remainder of this paper is organized as follows: Section 4.2 provides an overview of the institutional background. Section 4.3 outlines the empirical strategy and Section 4.4 provides details on the data sets. Section 4.5 presents the results, examines possible mechanisms, and tests the robustness of the empirical strategy. Finally, Section 4.6 concludes the paper.

4.2 Institutional Background

4.2.1 German Nationality Act and the Reform in 1999

German citizenship can be acquired through birth, ethnic German repatriation, adoption, marriage, or naturalization.

On July 15, 1999, the German parliament amended the Nationality Act. Effective on January 1, 2000, this reform brought about several changes. The reform introduced birthright citizenship (*ius soli*), replacing the previous *ius sanguinis* rule, which granted citizenship only by descent. This resulted in the right of immigrant children born in Germany after January 1, 2000, to acquire German citizenship at birth if at least one parent had been legally residing in Germany for at least 8 years at the time of birth and had unlimited right of residence. Eligible children automatically receive citizenship at birth upon registration at the civil registry. There is no requirement to apply separately for citizenship or the option to refuse citizenship during the registration process (BMI, 2023a).[11] Therefore, this citizenship acquisition is automatic and can

[11] As citizenship depends on registration at the civil registry office, it is theoretically possible for a child not to be registered and, consequently, not to receive citizenship. Registration is mandatory, and hospitals transmit birth records, making non-registration highly unlikely. An individual living in Germany without registration would face significant restrictions regarding access to social welfare, education, and employment. Furthermore, since children benefiting from the reform are eligible for dual citizenship, the reform does not create any incentives for parents to avoid registering their child.

be exploited as an exogenous variation. By contrast, individuals have to actively apply for standard naturalization which means that naturalization is pursued by a selected group of individuals.

Initially, the law stated that children who obtained German citizenship through the reform had to choose between German and their parents' foreign nationality when they turned 18. However, as of December 20, 2014, children can retain both nationalities as long as they grew up in Germany (BMI, 2023b).[12] Considering that focal children who are studied for the post-reform period were born between 2000 and 2002, none had to choose between two nationalities before the 2014 law reform. Additionally, since August 28, 2007, citizens of EU member states and Switzerland who are naturalized in Germany are permitted to hold multiple nationalities.

Children born between 1990 and 1999 could also benefit from a transition rule allowing retroactive citizenship if parents met the same residency requirement and filed the application on their child's behalf between January 1 and December 31, 2000 (§40b StAG). This transition rule might also have affected focal children's older siblings and may have benefited all older siblings studied in this paper – as they were all born between 1990 and 1998 – whose parents were eligible. If an older sibling qualified for the transition rule their parents also had to apply on their behalf for citizenship in 2000. This rule should not confound my analysis as siblings' eligibility for the transition rule did not depend on whether the focal child was born before or after the reform or on whether the focal child obtained citizenship through the reform. Use of this rule was limited: only around 20% of eligible children were naturalized (Felfe et al., 2020; Worbs, 2008). In a robustness check in Table 4.A.7 I also show that my results do not change if I exclude focal children or older siblings that made use of the transition rule.

The second part of the reform shortened the residency requirement for naturalization from 15 to 8 years (BMI, 2023b). Unlike in the case of birthright citizenship, for naturalization, adults must renounce their previous citizenship when naturalized[13] and meet certain requirements[14]. This reform component is not studied in this paper. The reduction in the residency requirement is not connected to the focal child's birth date and applies equally to focal children in the treatment group born before and after the cutoff. It should therefore not affect the estimation strategy in this paper.

[12]Growing up in Germany is defined as (i) having lived in Germany for 8 years, (ii) having visited a German school for 6 years, (iii) having obtained a German high school diploma, or (iv) having completed a German vocational training when turning 21 (§ 29 Absatz 1 a StAG).

[13]The new German reform which became effective on June 27, 2024 introduced the possibility of dual citizenship: From 2024 onward, individuals do not have to renounce their previous citizenship.

[14]Requirements include legal residency, language proficiency, financial self-sufficiency, no criminal convictions, and alignment with democratic values (§10 StAG).

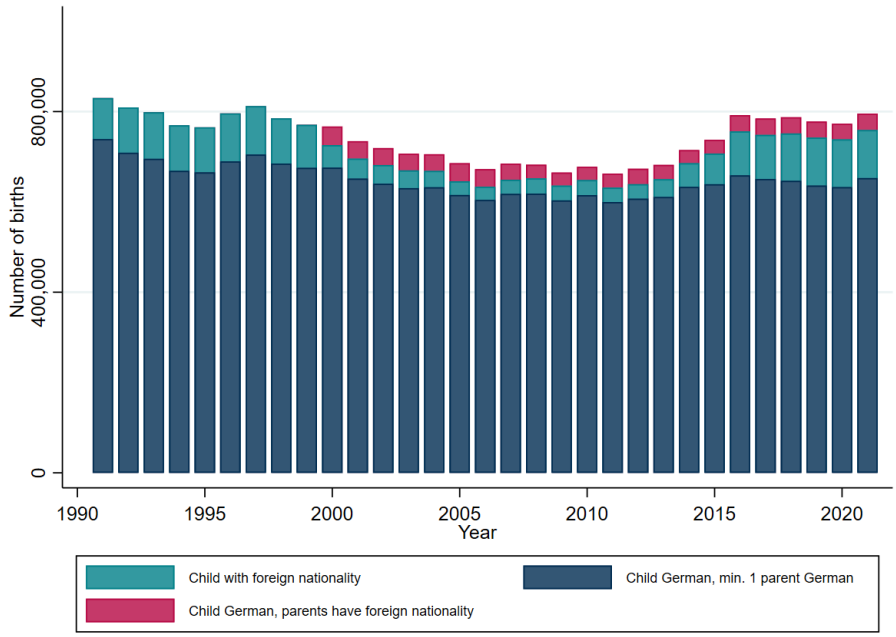
Immigrant children who do not receive German citizenship at birth obtain the status of either temporary or permanent residents. Individuals with citizenship and those considered permanent residents enjoy some of the same privileges: They have an unlimited right to stay in Germany and are eligible for social assistance, unemployment benefits, daycare, child benefits, parental benefits, and alimony advances (Riphahn et al., 2013).

Citizenship offers key advantages over permanent residency, especially during adolescence – the age studied in this paper. It provides access to all professions, including life tenure as a civil servant in institutions like the police or judiciary, as well as the ability to work as a doctor or start a business without immigration restrictions (Integrationsbeauftragte, 2023, §9 AufenthG). Citizenship also allows individuals to work, study, and travel visa-free in all European Union countries, broadening labor market and employment opportunities.[15] Furthermore, individuals are eligible to vote in national and EU elections and run for political office. They also benefit from travel and visa facilitation outside the EU, protection against deportation, and assistance from German embassies abroad. Finally, individuals enjoy an unlimited right of residency in Germany, while permanent residency expires after a six-month stay outside the country. These advantages apply only to focal children, while siblings benefit only if they naturalize themselves. Thus, these advantages might explain spillovers on siblings if the sibling naturalizes.

Figure 4.1 shows the number of births per year in Germany by the focal child's and parents' citizenship. A large share of children born in Germany receive German citizenship at the time of birth, typically because one parent is a German citizen. Between 1990 and 1999, most immigrant children were born without German citizenship (around 10% of all focal children). The introduction of birthright citizenship can also be observed in the figure: since 2000, approximately 5% of children have received German citizenship at birth despite neither parent being a citizen – roughly half the share of the previous non-citizens. Despite the reform, some individuals still do not possess German citizenship at birth due to the eight-year residency requirement imposed on their parents. With new families immigrating to Germany each year, the proportion of families who do not meet the eight-year residency requirement remains stable. From 2015 onward, as refugee arrivals increased, so did the share of children born without German citizenship.

[15] This is an additional advantage for focal children whose parents did not emigrate from another EU country.

Figure 4.1: Number of Births by Focal Child's and Parents' Citizenship Status



Source: German Federal Statistical Office (2023), own calculations.

4.2.2 The German Secondary School System

In Germany, children attend primary school for 4 years[16] before moving on to different school tracks based on their abilities and academic potential. At the end of primary school (around age 10), students receive recommendations from their teachers regarding the secondary school track they should pursue. There are three main tracks in German secondary schooling: the lowest track (Hauptschule), the intermediary track (Realschule), and the academic track (Gymnasium). The academic track prepares students for higher education. Most schools of all three types are public and tuition-free (Goerlitz et al., 2018).

Children in grades 12 or 13 can obtain a university entrance qualification ((Fach-) Hochschulreife) if they pass a final examination.[17] This qualification can be obtained at a Gymnasium, comprehensive schools[18] and certain vocational schools, certifying

[16] In the federal states Berlin and Brandenburg, children visit primary school for 6 years, but the total years of schooling to acquire certain school leaving certificates are the same.

[17] Whether the child obtains a higher education entrance qualification after 12 or 13 years depends on the school the child attends. Children visit a Gymnasium for 12 years and comprehensive schools for 13 years. The duration of the Gymnasium was reduced from 13 to 12 years in the 2000s. Some federal states have extended the duration to 13 years in recent years.

[18] Comprehensive schools include the low, middle, and academic tracks, prepare for all three degrees, and are at least until grade 10.

the child to attend a higher education institution (university or university of applied sciences). Completing higher education grants individuals access to certain professions that require a university degree.

Investigating the type of school degree is important because different degrees can result in considerably different employment opportunities, and tertiary education eligibility increases chances of securing higher-paying occupations (Nordin et al., 2020). There exists an immigrant-nonimmigrant gap in school degrees: 57% of nonimmigrant and 50% of immigrant individuals aged 18 to 25 finished secondary school with the university entrance qualification (Statistisches Bundesamt, 2022a).

4.3 Identification Strategy

To identify the causal effect of birthright citizenship on the education of focal children and their older siblings, I exploit the exogenous and automatic access to citizenship for focal children born after January 1, 2000. Unlike naturalization, which is pursued by a selective portion of the population, the German reform contains two provisions that make the eligibility for birthright citizenship exogenous: citizenship is granted automatically; and is not influenced by parental decisions.[19] I employ event study and difference-in-differences approaches and individual level data on secondary school degrees and school tracks for both individuals directly affected by the reform (focal children) and their older siblings.

4.3.1 Event Study Analysis

I use an event study design to estimate the effects on the focal child's and sibling's education in which I interact the treatment group indicator with the focal child's birth year:

$$Y_{it} = \gamma_0 + \gamma_1 Treat_i + \sum_{b \neq 1999} \gamma_2 b_i + \sum_{b \neq 1999} \gamma_3 Treat_i \times b_i + X'_{it}\gamma_4 + \mu_m + \delta_t + \zeta_s + \eta_{st} + \epsilon_{itb}. \quad (4.1)$$

In this equation, Y_{it} represents the completion of the academic school track for focal child or sibling i in year t. It is an indicator which equals 1 if the individual has obtained a university entrance qualification or is currently attending a school that leads to such

[19] The reform of the German nationality law from January 1, 2000, entails that all immigrant children born in 2000 or after to non-Germans will be granted German citizenship if at least one parent has been legally residing in the country for at least 8 years at the time of the child's birth and if the child is registered at the civil registry office (see Section 4.2 for a discussion on this).

qualification at ages 17-22 and 0 otherwise. $Treat_i$ is the treatment indicator, that is set to 1 if both parents were born outside Germany (immigrant households) and 0 if at least one parent was born in Germany (nonimmigrant households). I include focal child's birth year indicators b and exclude 1999 – the year before the implementation of the reform – as the reference year. The parameter of interest, γ_3, is the interaction of the treatment indicator $Treat_i$ with the focal child's birth year indicators b from 1992 to 2002, and represents the effect of the different birth cohorts relative to the 1999 cohort.[20] Using an event study approach allows me to assess diverging pre-trends of the treatment and control groups.

Following Buckles and Hungerman (2013), who found differences in outcomes for children born in different months of the year, I include a set of the focal child's birth month indicators μ_m, to account for any seasonal effects. I also include survey year (δ_t), federal state (ζ_s) and survey year times state (η_{st}) fixed effects to control for remaining differences in the states' school systems like the length of elementary school and different types of schools in the lower school tracks as well as differential time trends. The vector of control variables X_{it} includes the child's age and gender, the mother's age at the child's birth, and the parents' highest educational level. For the sibling spillover analysis, it additionally includes the birth order of the siblings, the age difference between the focal child and the older sibling and the older sibling's birth month fixed effects.

The employed identification strategy involves comparing families in which the focal child was born around the cutoff date of January 1, 2000. This approach relies on the assumption that these families are otherwise very similar. Table 4.A.1 contrasts older siblings whose younger sibling was born pre-reform (1994–1999) with those born post-reform (2000–2002). By design, the birth years of focal children in the two groups differ. Due to the sibling sample being restricted to birth years 1990–1998, the average age gap is larger in the post-reform group (5 vs. 3 years), since younger siblings born after 2000 can only have older siblings born before 1999. Despite these differences, the groups are similar across key characteristics, such as gender, maternal age, and parental education. The main distinction is a higher share of immigrant families in the post-reform group, consistent with broader demographic trends and increasing immigration in Germany.

[20] For the analysis of sibling spillovers, the birth cohorts of focal children are limited to the years 1994 to 2002 to ensure that the majority of each focal child's older siblings are included in the sample. See Section 4.4 for a discussion of this.

4.3.2 Difference-in-differences Approach

In the second step, I estimate a difference-in-differences approach based on the event study methods described in Equation 4.1 to estimate the effects on the focal child's and the sibling's academic school track completion. I also use difference-in-differences methods to analyze the mechanisms in Section 4.5.3, as it is the preferred specification and the NEPS is a cohort study and does not support comparisons across birth cohorts.

The approach takes the following form:

$$Y_{it} = \beta_0 + \beta_1 Treat_i + \beta_2 Post_b + \beta_3 Treat_i \times Post_b + X'_{it}\beta_4 + \sum_{b \neq 1999} \beta_5 b_i + \mu_m + \delta_t + \zeta_s + \epsilon_{it}. \tag{4.2}$$

The definition of the outcome variable Y_{it}, the treatment indicator $Treat_i$, the birth year b_i, the fixed effects and control variables corresponds to those in Equation 4.1. $Post_b$ equals 1 if the focal child i was born after January 1, 2000, and 0 for focal children born before December 31, 1999.

The parameter of interest is β_3, which identifies changes in outcomes for focal children and siblings in the treatment group (children born to two born-abroad parents) compared to the control group (children born to at least one parent born in Germany) between the pre- and post-reform periods, assuming a common trend. This estimator corresponds to an intention-to-treat (ITT) estimator for three reasons: First, the pretreatment sample includes sibling pairs with a focal child qualifying for citizenship based on their parents' citizenship (if the parents naturalized before birth) or the transition rule. Second, the post-treatment sample includes sibling pairs with focal children who did not receive birthright citizenship due to their parents not meeting the 8-year residency requirement or lacking legal residency status. Third, the control group may include sibling pairs with focal children without German citizenship because their parents were born in Germany but whose grandparents immigrated.[21] However, as the proportion of focal children in the control group with citizenship at birth appears to be 100 across birth cohorts (see Figure 4.2), the proportion of children of German-born parents without German citizenship seems negligible. All these factors indicate that the estimate represents a conservative estimate of the impact of birthright citizenship on focal children and older siblings. As a result, I later scale the estimated main effects by the take-up rate of birthright citizenship in the treatment group, as estimated in

[21] Using parents migration history and not citizenship status for the definition of the treatment group is an approximation because I only observe the parents' citizenship status and length of residency for parents living in the same household. Additionally, a definition based on parents' citizenship status would be endogenous, as parents' naturalization is an endogenous choice.

the first stage regression (Table 4.A.2) to obtain an average treatment effect on the treated (ATT).

4.4 Data

4.4.1 German Microcensus

The main analysis uses German Microcensus data from 2010 to 2021. The Microcensus is a representative survey of 1% of households (Statistisches Bundesamt, GESIS, 2022). It includes individuals in private households or communal accommodations, using a rotating sample design where each household is surveyed for four consecutive years. This setup primarily allows for cross-sectional analyses. The advantage of using the Microcensus is its large sample size and mandatory participation, which decreases sample selection biases. One household member responds for all members, and data are only collected for individuals living in the same household. Thus, older siblings are included only if they reside with their parents. Section 4.5.5 addresses possible sample selection bias due to this restriction.

The main outcome measures educational attainment of focal children and their older siblings as a binary indicator equal to 1 if the individual has or is currently pursuing a university entrance qualification ((Fach-)Hochschulreife). This includes individuals enrolled in the academic secondary track that grants university access, capturing those who may not have completed school by 2021 (for a similar approach, see Piopiunik, 2014). This measure is also unaffected by variations in school duration across German states and is assessed for individuals aged 17–22, the typical age of completion. Each wave of the Microcensus measures this outcome variable.

The treatment group consists of focal children and their siblings with two foreign-born parents. The reform only applied to children born to two non-German citizens. Throughout the study period, 15% of children in the sample have two foreign-born parents (regardless of their parents' citizenship status). The control group comprises all focal children and their siblings with at least one German-born parent.

The sample of older siblings is defined as those born between 1990 and 1998, ensuring they were born before the reform. Each must have a sibling (the focal child) born between 1994 and 2002, with at least a one-year age gap. The focal child cohort range ensures that the majority of each focal child's older siblings are included in the sample,

given a median four-year age difference between siblings in the Microcensus.[22] Only older siblings born in Germany are included to ensure uniform eligibility under the transition rule. Adoptive and step-siblings are included if co-residing with the focal child.

Older siblings may appear multiple times in the data – once for each focal child they are matched to – based on when their sibling was born. A focal child can also be included multiple times depending on the number of older siblings.[23]

All older siblings in my sample are themselves not directly affected by the reform but could have received citizenship retroactively in 2000 under the transition rule, provided their parents legally resided in Germany for at least 8 years at the time of birth and applied for German citizenship retroactively on the sibling's behalf between January 1 and December 31, 2000 (§40b StAG). Between 2000 and 2007, 49,169 children obtained citizenship through this rule, about 20% of those eligible (Felfe et al., 2020; Worbs, 2008). These siblings would have obtained citizenship later in childhood (at age 2 or later), unlike focal children who acquired it at birth.

The samples of focal children and older siblings include all individuals aged 17 to 22, regardless of current status (in school, working, studying, or neither). Missing values for control variables are imputed with means for continuous variables and defined as a separate category for categorical ones. First-generation immigrant children and ethnic German repatriates[24] are excluded, as they were unaffected by the reform and have different citizenship pathways (see Piopiunik and Ruhose, 2017).

In the mechanisms section, I explore how birthright citizenship might have spillovers on siblings through two channels using the Microcensus. First, I investigate naturalization behavior by analyzing citizenship status of parents and siblings. Additionally, I assess whether siblings received citizenship via the transition rule (see Section 4.2 for a more detailed description), using a binary variable equal to 1 for individuals born before 2000 who acquired citizenship in 2000 or 2001 – because 89% of children who benefited from the reform were naturalized in these years (see Worbs, 2008). Second, I examine family language use using an indicator for whether German is predominantly spoken at home.

[22] This means the sample includes at least 50% of older siblings for each focal child. For example, older siblings born in 1990-1993 are included for the focal child cohort of 1994, while those born in 1990-1998 are included for the focal child cohort of 2002.

[23] Due to this sample setup, it is important to cluster standard errors at the family level. I show this test in Table 4.A.7 and show that the results still hold. However, the effects on older siblings of the focal child child birth cohort 2002 is no longer statistically significant.

[24] Ethnic German repatriates refer to individuals who migrated to Germany and are descendants of Germans from the former Soviet Union and other Eastern European countries.

Table 4.1 compares predetermined characteristics of the treatment and control groups for older siblings born between 1990 and 1998 who are matched to a focal child born between 1994 and 2002. Key variables, such as the focal child's gender and birth month are similar across groups. However, there as some small statistically significant differences in terms of the focal child's birth year and the older sibling's age. Notable differences also exist in terms of parental characteristics, highlighting the importance of evaluating pre-trends. Immigrant mothers are on average younger at the time of childbirth than nonimmigrant mothers (25 vs. 28 years), and control group parents have higher levels of education compared with those in the treatment group (30% with a university degree vs. 8%). Parents' regions[25] of origin also differ. While most control group parents are German-born,[26] 46-50% of treatment group parents were born in Turkey, followed by families from the Balkans and EU-12 countries. These distributions reflect broader demographic patterns in Germany. (see e.g., Schuehrer, 2018).

4.4.2 National Educational Panel Study

To further disentangle the channels through which birthright citizenship spillovers on older siblings, I use data from the National Educational Panel Study (NEPS; see Blossfeld and Roßbach, 2019), a multi-cohort panel study tracking educational trajectories of six cohorts in Germany. The focus is on cohorts 3 and 4, which align with the birth cohorts of focal children and their older siblings studied in the Microcensus. I use cohort 3 – surveying fifth-graders (ages 10-11) in 2010 – to analyze focal children, and cohort 4 – surveying ninth-graders (ages 14-15) in 2010 – to study older siblings (Skopek et al., 2012). The surveys are annual, and participants are followed through school. Participants leaving the sampled school or class due to grade retention or graduation are tracked individually. However, children who had repeated or skipped a grade before the survey began are not included.[27] I use data from child and parent surveys.

The analysis includes focal children born between 1999 and 2000[28] and older siblings born in 1994-1997[29] with a younger sibling (focal child) born 1998-2001. Since NEPS

[25] The region of origin is approximated using the current or previous citizenship, as the country of origin is only surveyed in a few waves in the Microcensus.
[26] Some control group parents are misreported as foreign-born due to survey errors, so I use the cleaned and validated migration status from the Microcensus in my analysis.
[27] Felfe et al. (2020) show that birthright citizenship reduces grade retention by 24.5%, potentially causing slight sample selection among focal children.
[28] 93% of the cohort 3 sample was born in 1999 and 2000: 40.2% in 1999, and 52.5% in 2000.
[29] 0.3% born in other years are excluded from the analysis.

Table 4.1: Comparison of the Treatment and Control Groups

	Control Group	Treatment Group	Difference
	Mean/Percentage		
Focal child is male	51.93 %	52.04 %	0.11
Birth month (focal child)	6.45 (3.38)	6.44 (3.42)	-0.01
Birth year (focal child)	1998.06 (2.32)	1998.43 (2.32)	0.37***
Older sibling's age (in years)	19.16 (1.59)	19.28 (1.60)	0.11***
Age difference between siblings (in years)	3.83 (2.01)	4.24 (2.24)	0.41***
Mother's age at birth (in years)	28.10 (4.02)	24.68 (4.61)	-3.35***
Highest post-secondary degree in the household			
No degree	5.21 %	48.16 %	42.95***
Vocational training	60.87 %	39.64 %	-21.23***
University	29.61 %	8.04 %	-21.57***
Missing	4.3 %	4.2 %	-0.0014
Mother's region of origin			
German	88.83 %	0.23 %	-88.59***
Turkey	0.48 %	49.82 %	49.34***
Eastern Europe	0.94 %	4.18 %	3.24***
Balkan	0.43 %	12.31 %	11.88***
Eu 12	0.99 %	8.23 %	7.24***
Other	1.69 %	20.57 %	18.88***
Missing	6.64 %	4.66 %	-1.99***
Father's region of origin			
German	75.12%	0.29 %	-74.83***
Turkey	0.48 %	45.90 %	45.41***
Eastern Europe	0.33 %	3.53 %	3.21***
Balkan	0.46 %	11.43 %	10.96***
Eu 12	1.28 %	7.78 %	6.5***
Other	1.12 %	19.03 %	17.91***
Missing	21.20 %	12.05 %	-9.16***
Observations	76,237	13,778	

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard deviation in parentheses. The statistics are based on the sample of older siblings born 1990-1998 matched to a focal child born 1994-2002. *Source:* German Microcensus (2010-2021), weighted, own calculations.

samples cohorts individually, parents are not surveyed about both children, making within-family investigations of the division of parental investments impossible. As in the Microcensus, treatment group children have two foreign-born parents, while control group children have at least one German-born parent. The focal child sample includes 21,669 control and 3,136 treatment cases (13% treatment), while the sibling sample includes 3,028 control and 170 treatment cases (5% treatment).

To measure parental involvement, I construct an overall index along with four subindices that reflect different investment domains. Using indices ensures that the results are not skewed by a single survey question that captures only one aspect of parental investment. The first counts the number of educational resources at home: a desk, educational software, books for homework, and a computer.[30] A second subindex measures the frequency of school-related conversations – such as discussing class topics and addressing problems in school – between the child and their parents.[31] With a third subindex, I investigate the frequency of parental support, which includes purchasing learning materials, searching for information for class, and assisting with presentation preparation.[32] And the final subindex captures the frequency of contact between parents and the school, including attending parent-teacher conferences, participating in school events, engaging in the Parent-Teacher Association, or contacting teachers.[33] Higher values across all variables indicate greater parental involvement in education.

As an additional channel, I investigate the older sibling's identity and expected discrimination. Identity is measured through agreement with six statements about connection to Germany and German society. Responses are coded as binary indicators (1 = identifies with Germany).[34] To understand the siblings' expected discrimination, I use responses to whether individuals expect to be disadvantaged in finding an apprenticeship due to a foreign name, foreign appearance, wearing a headscarf, or limited German skills. Responses indicating rather yes or yes are defined as expecting discrimination (=1).[35] This outcome is relevant as it reflects the siblings' anticipated discrimination in the labor market and may shape educational and career choices.

[30] Focal children are surveyed across waves 1-5, while older siblings are surveyed in wave 1.

[31] This index is derived from child surveys conducted in waves 7-8 for focal children and waves 4-9 for older siblings.

[32] Parents are surveyed on this in waves 5-7 for focal children and waves 1-6 for older siblings. Due to data availability, the variable for older siblings only includes the frequency of purchasing materials and the frequency of helping with presentations.

[33] This variable is measured from the parent's perspective in wave 5 for focal children and wave 3 for older siblings.

[34] These questions were surveyed in wave 7.

[35] All students were surveyed this question in wave 2 independent of their intentions to search for an apprenticeship.

4.5 Results

4.5.1 Effects on Birthright Citizenship

The first step in the analysis is to determine whether the reform increased citizenship at birth for focal children born after the birth date cutoff. Figure 4.2 displays the percentage of children with German citizenship in a given birth quarter, separately for treatment and control groups.

As expected, all children in nonimmigrant households received German citizenship at birth regardless of birth date. By contrast, in immigrant households, the share increased substantially after the reform – from 30-50% before 2000 to about 75% afterwards. Before the reform, they acquired citizenship because one of their parents naturalized before childbirth or because their parents applied for citizenship under the transition rule on their behalf. The share does not reach 100% because eligibility also requires that at least one parent had legally resided in Germany for 8 years at the time of birth.[36] In general, there is a slight upward trend before and after the reform, with children born in later quarters more likely to have German citizenship at birth.

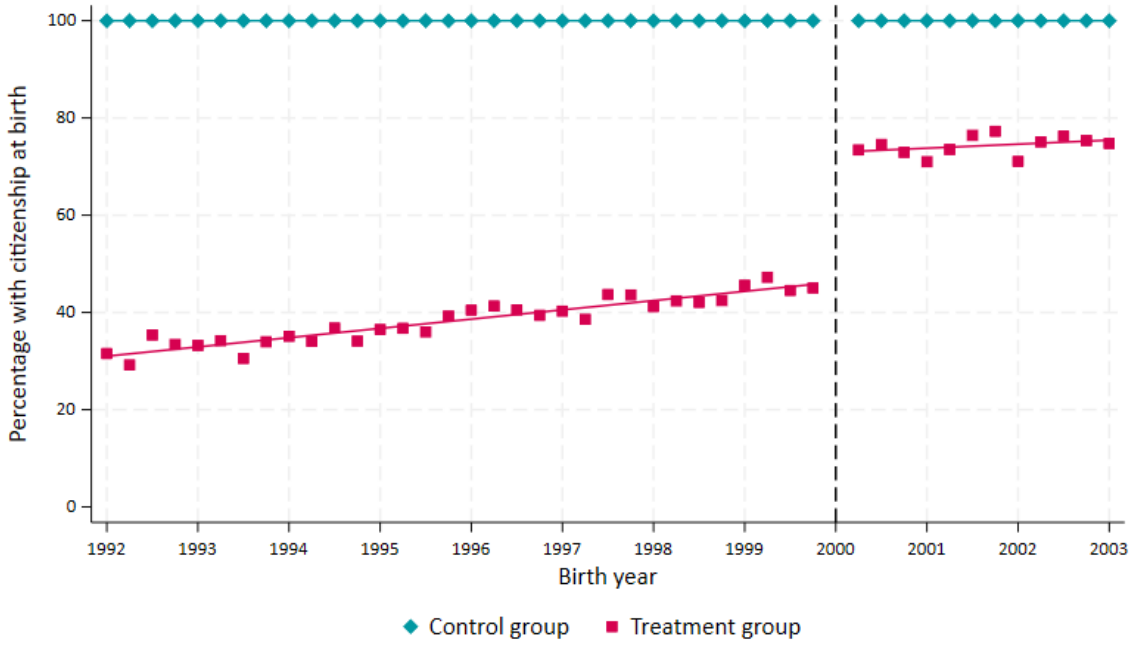
Table 4.A.2 presents the first-stage regression results for the treatment group. This is shown for the focal children and the sample of siblings matched to focal children – both over a longer time frame (birth cohorts 1992-2002 and 1994-2002) and a short window around the birth date cutoff (1999-2000). These samples are the same as those used in the main analysis in Section 4.5.2. The sample size in column 3 corresponds to the treatment group size in Table 4.1, but is slightly smaller due to missing citizenship data. The smaller sample size applies to all columns.[37]

I observe a 36 percentage point increase in the share of focal children in the treatment group obtaining German citizenship at birth for those born between 1992 and 2002, and a 25 percentage point increase for those born between 1999 and 2000. In the matched sample of older siblings, the increase is 37 percentage points for focal children born 1994-2002, and 33 percentage points for those born 1999-2000. The increase is more pronounced with a larger bandwidth due to the overall upward trend in citizenship at birth. Differences in effect sizes are more pronounced across the two focal child samples

[36] Limiting the sample to children with one parent resident for at least eight years raises the post-reform citizenship rate to over 80%. However, it does not reach 100% due to measurement errors, the availability of data only for cohabiting parents, and the lack of legal status information in the Microcensus. This is particularly relevant as legal residency with an unlimited right of residence for eight years is a requirement.

[37] In Table 4.A.7, I estimate my main analysis using the sample of children with known citizenship status. The sample size is slightly smaller, as expected, but the results are robust.

Figure 4.2: Percentage of Children with Citizenship at Birth by Birth Quarter

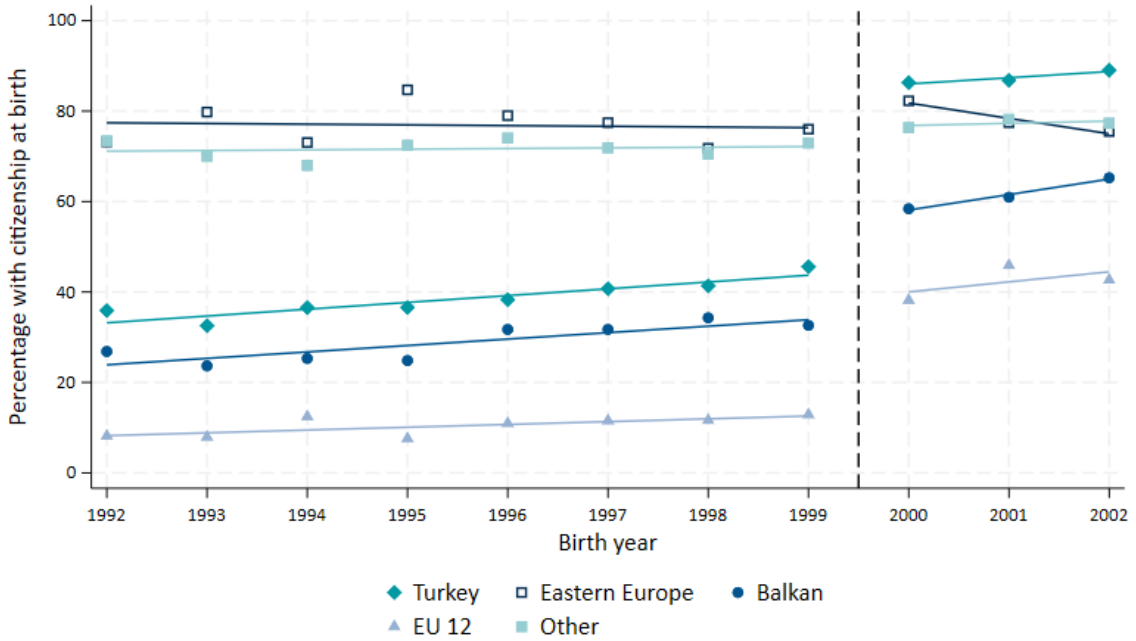


Notes: The graph shows the share of focal children with German citizenship at birth for the treatment and control group by birth quarter. The treatment group consists of children whose parents were both born outside of Germany, whereas the control group comprises children with at least one parent born in Germany. *Source:* German Microcensus (2010-2021), weighted, own calculations.

than between the sibling samples. This is due to the sibling sample restriction to those born in Germany, which ensures that the treatment group consists only of families who migrated before the older sibling's birth. As a result, these families are more likely to have met the eight-year residence requirement by the time of the reform and when the younger sibling (the focal child) was born. In the main analysis, I estimate an ITT effect and scale the effects estimated in the different specifications using the complier rate for each sample (see Section 4.3).

Figure 4.3 breaks down the treatment group by maternal region of origin. The largest increase in citizenship is seen among children of Turkish-origin mothers – almost 50 percentage points, followed by children of mothers from the Balkans and EU12 countries, while no substantial change occurred for those from Eastern Europe or other countries. These differences reflect varying migration histories: eligibility requires parental residence in Germany for at least eight years prior to childbirth, which was more common among Turkish and Balkan migrants. A slight decline among Eastern European families after 2000 is likely due to post-2000 migration increases from these countries, particularly ahead of the EU's 2004 enlargement (Statistisches Bundesamt, 2025).

Figure 4.3: Percentage of Children with Citizenship at Birth by Birth Year – By Mother's Region of Origin



Notes: The graph shows the share of focal children with German citizenship at birth for different groups within the treatment group by birth year. *Source:* German Microcensus (2010-2021), weighted, own calculations.

4.5.2 Effects on Focal Children's and Siblings' Education

After establishing that the reform increased German citizenship for immigrant children, this section assesses its effects on focal children and their older siblings.

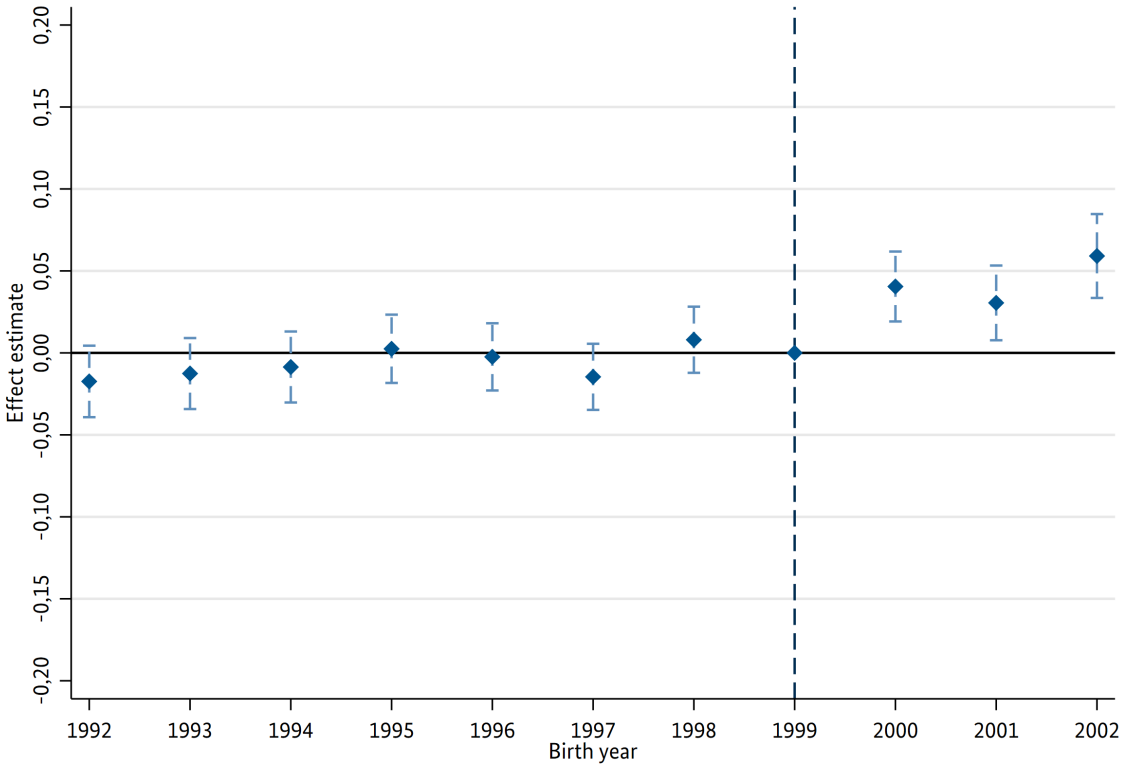
As shown in Figure 4.4, I examine the impact of the reform on focal children's completion of the academic school track using the event study approach. Completion is measured as a binary indicator, equal to 1 if the individual holds a university entrance certificate or is enrolled in a secondary school track that leads to such a certificate. The graph plots the interaction between the treatment variable and the birth cohorts relative to 1999 – the birth year before the reform. For pre-reform cohorts (1992-1998) the estimates show no statistically significant differences in pre-trends, supporting the assumption of parallel trends and thus the causal interpretation. The interaction coefficients of the birth cohorts from 2000 to 2002 show a statistically significant increase in focal children's academic school track completion of 3 to 6 percentage points, which remains consistent for the first three birth cohorts.[38]

These results confirm benefits for focal children. I next assess whether the reform had spillover effects on the academic school track completion of older siblings. Figure 4.5 presents the effects of the birthright citizenship reform for older siblings born between 1990 and 1998. It again plots the interaction of the treatment variable $Treat_i$ with the focal child's birth year in comparison with the pre-reform year (1999). Pre-trends appear to be parallel, although the 1996 cohort shows a large but insignificant coefficient. The results also reveal a statistically significant increase in the likelihood of older siblings completing the academic school track if the focal child is born after the reform cutoff by 3 to 4 percentage points.

In Table 4.2, I confirm the results using a difference-in-differences approach. Consistent with the analyses presented in Figures 4.4 and 4.5, the outcome variable is a binary indicator equal to 1 if the individual possesses a university entrance certificate or is enrolled in a secondary school track that leads to one. Columns 1 to 4 show that the birthright citizenship reform significantly increases the probability of the focal child completing school at the academic track. This effect persists when I limit the sample to children born in the 2 years around the cutoff year (1999-2000, columns 3-4). This narrower sample includes children on both sides of the cutoff who are likely more comparable than when comparing children born several years before and after the reform,

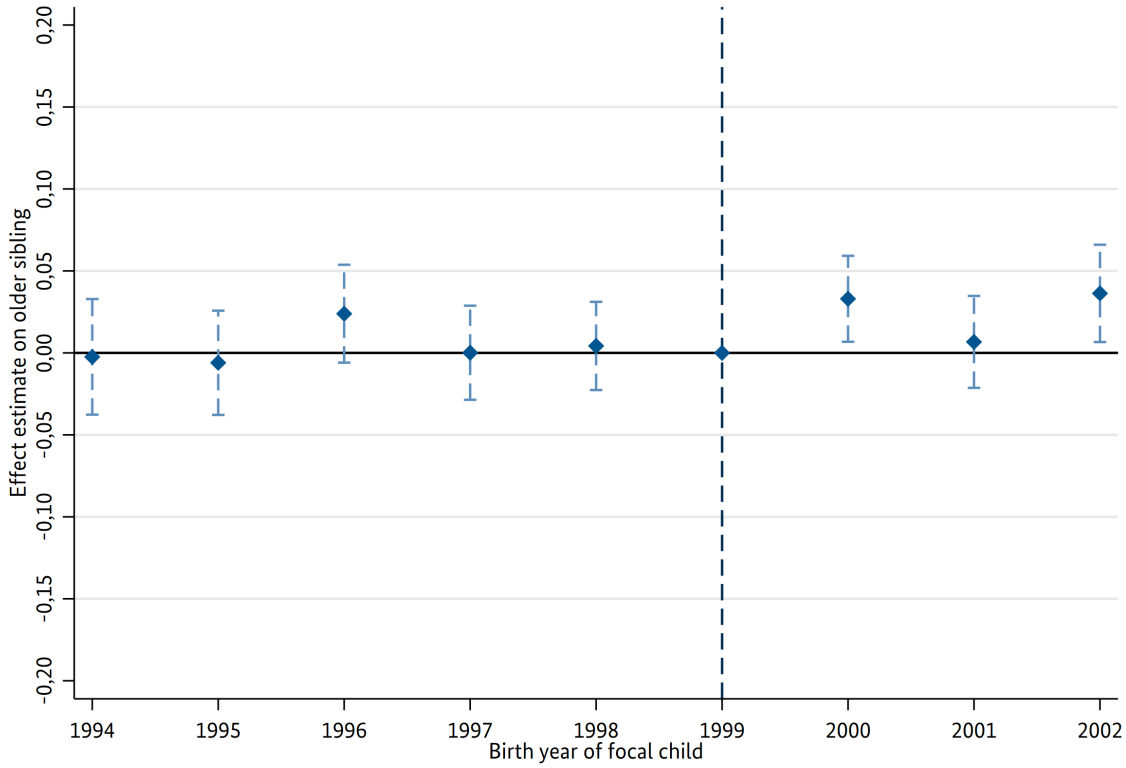
[38] I cannot investigate any later birth cohorts because 2021 is the most recent available data, and younger cohorts would not yet be old enough.

Figure 4.4: Effect on Focal Children's Academic School Track Completion



Notes: These coefficients are estimated using the event study approach described in Equation 4.1. The figure displays the coefficients of the interaction of the treatment variable $Treat_i$ with the birth cohorts compared to the last pre-reform year (1999). The outcome variable captures whether the focal child (aged 17-22) obtained a university entrance qualification or attends a school track leading to university entrance qualification compared to lower school degrees and school tracks. The treatment group includes children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. The regression includes year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month. N=307,494. 90% confidence intervals based on robust standard errors are shown. *Source:* German Microcensus (2016-2021), weighted, own calculations.

Figure 4.5: Effect on Siblings' Academic School Track Completion



Notes: These coefficients are estimated using the event study approach described in Equation 4.1. The figure displays the coefficients of the interaction of the treatment variable $Treat_i$ with the focal child's birth cohorts compared to the last pre-reform year 1999. The outcome variable captures whether the sibling (aged 17-22) either obtained a university entrance qualification or currently attends a school track leading to university entrance qualification. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. The regression includes year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree, the focal child's birth month, the birth order, the age difference between siblings, and the sibling's birth month. N=90,015. 90% confidence based on robust standard errors intervals shown. *Source:* German Microcensus (2010-2021), weighted, own calculations.

and therefore less prone to cohort-related differences.[39] It also persists when excluding socio-demographic and socio-economic controls and fixed effects (columns 1 and 3).

Citizenship increases a focal child's likelihood of obtaining a university entrance qualification or attending a school track which provides this qualification by 4.7 percentage points (or 3.9 percentage points when restricting the birth cohorts). This represents a 6-7% increase over the pre-reform average. Scaling the effect by the first-stage complier rate (36 and 25 percentage points), yields a local average treatment effect of 13 (cohorts 1992-2002) or 16 (cohorts 1999-2000) percentage points – or 19-23% compared with the pre-reform mean.

The results in columns 5-6 show that granting citizenship to the focal child increases the probability of older siblings completing secondary school at the academic track. Restricting the sample to older siblings of focal children born in 1999 or 2000 (columns 7-8) produces an even larger treatment effect.[40] The effect sizes are very similar irrespective of whether I control for socio-demographics and fixed effects or not.

The spillover effect on older siblings is smaller than the direct effect on the focal child, at 2.1 to 3.3 percentage points (or 3-6% over the pre-reform average). Scaling the spillover effect by the complier rate (37 or 33 percentage points) to calculate the local average treatment effect yields an effect size of 6 (focal child cohorts 1994-2002) or 10 (focal child cohorts 1999-2000) percentage points. The spillover effect corresponds to 46 (focal birth cohorts 1994-2002) to 63% (focal birth cohorts 1999-2000) of the size of the direct effect on the focal child.

4.5.3 Mechanisms

This section investigates whether increased naturalization of parents and siblings, greater use of German in the household, changes in parental investments, or shifts in the sibling's identity and perception of discrimination can explain the substantial effects on older siblings. The positive spillover effects on the older siblings' education could be a direct effect of the focal child's education on them. Karbownik and Oezek (2023) show that being among the oldest children in class improves academic performance, and benefits younger siblings, while effects on older siblings are less clear.

[39] I do not restrict the sample further to only several months before and after the reform cutoff, as Buckles and Hungerman (2013) demonstrate that children born in the first few months of the year are for example more likely to be born to teenage mothers and mothers with lower educational attainment. Consequently, children born before and after January 1 are not comparable.

[40] The larger effect size is likely attributable to the sibling sample composition, as the average age difference between siblings is greater in this restricted sample (4.1 vs. 3.5 years) due to the restriction of the sample to the 1999 and 2000 birth cohorts of the younger siblings. As demonstrated in Table 4.A.4, the effects are more pronounced for siblings with a larger age difference.

Table 4.2: Effects on Focal Child's and Older Sibling's High School Completion - Difference-in-differences

	Focal Child				Older Sibling			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	0.039***	0.047***	0.033**	0.039***	0.022**	0.021**	0.038**	0.033**
	(0.007)	(0.007)	(0.013)	(0.013)	(0.010)	(0.009)	(0.017)	(0.016)
Observations	307,494		57,100		90,015		25,669	
Pre-reform Mean	0.656		0.681		0.611		0.617	
Birth cohorts (focal c.)	1992-2002		1999-2000		1994-2002		1999-2000	
Controls and FE		✓		✓		✓		✓

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. Columns 2, 4, 6 and 8 include year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month, columns 6 and 8 also control for the birth order, the age difference between siblings, and the sibling's birth month. The pre-reform mean shows the sample average for the treatment group born before the reform. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Therefore, there are likely other channels through which citizenship impacts older siblings' education.

Naturalization. I first examine whether other family members naturalize more often. Access to birthright citizenship may reduce information barriers regarding citizenship for parents and siblings. Consequently, parents may naturalize, enabling their children to gain citizenship. Alternatively, parents may apply for citizenship on behalf of the older siblings via the transition rule or older siblings might naturalize independently at age 18. The results in Table 4.3 (columns 1-3) show that the focal child's citizenship increases the likelihood that other family members are also citizens – 3.8 percentage points for older siblings[41], 2.5 percentage points for mothers, and 1.8 percentage points for fathers. The larger increase for siblings compared to the parents indicates that some siblings naturalized independently or via the transition rule. However, the results in column 4 shows that the increase is not driven by the transition rule.[42] This finding aligns with research by Felfe et al. (2020), who find low take-up of the transition rule. Many siblings therefore likely chose to naturalize after turning 18.

German Proficiency. A child's citizenship might motivate parents to integrate into society and learn German, influencing German use at home. Table 4.3, column 5, demonstrates a 3.6 percentage points increase in German as the main household language.[43] This supports findings by Avitabile et al. (2013) that a child's birthright

[41]The sample size for this analysis differs from that presented in Table 4.2 due to missing citizenship data for a portion of the Microcensus sample.

[42]For this analysis, I use a proxy defining all individuals born between 1990 and 1999 who received German citizenship through naturalization in 2000 or 2001 as beneficiaries of the transition rule.

[43]The sample size of this analysis is smaller than that of the other columns because the spoken languages were surveyed only in certain waves of the Microcensus.

citizenship increases parents' frequency of reading German newspapers in early years – an effect that seems to translate into German usage in general and to persist. This finding is also plausible given that the parents of the affected children had already lived in Germany for at least eight years at the time of the child's birth, and for over twenty years by the time the German usage was measured in the Microcensus.

Table 4.3: Effects on Naturalization of Family Members and German Language Usage

	Citizenship			Transition Rule	German in
	Mother	Father	Sibling	Sibling	the Household
	(1)	(2)	(3)	(4)	(5)
Treat × Post	0.025***	0.018***	0.038***	0.001	0.036***
	(0.006)	(0.007)	(0.013)	(0.005)	(0.012)
Observations	604,774	491,640	62,596	62,596	51,566
Pre-reform Mean	0.319	0.370	0.575	0.035	0.456

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. All regressions include year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month, columns 3-5 also include the birth order, the age difference between siblings, and the sibling's birth month as controls. The outcome variable "Citizenship" captures whether the mother (col. 1), the father (col. 2) or the older sibling (col. 3) has German citizenship at the time of the survey and "Transition rule" indicates whether individuals made use of the transition rule described in Section 4.2. This is a binary variable which equals 1 for individuals who are born before 2000 and who received their German citizenship in 2000 or 2001 (as 89% of children who benefited from the rule naturalized in 2000 or 2001, see Worbs, 2008). "German in the Household" captures whether the household speaks predominantly German. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Parental Investments. Parents' behavior may also be impacted by citizenship access, potentially leading to different investments in children with or without citizenship. Panel A of Table 4.4 shows birthright citizenship increases parents' overall educational investments in the focal child, especially via material resources like a computer or books for homework.[44] Panels B and C, reveal that parents invest more in the focal child – in the form of educational resources and school-related conversations, particularly educational software and homework books (see also Table 4.A.3 in the Appendix) – when they have only one child but investments in focal children remain unchanged in families with siblings.

However, parents' educational investments in older siblings increase substantially, outpacing investments in focal children who do not have siblings (panel D). This finding is attributable to an increase in the frequency of parental support and contact between parents and schools. Examining the individual items of these indices reveals

[44]The sample sizes differ for the different outcome variables as they were surveyed in different waves of the NEPS (see Section 4.4 for a detailed description).

a particular increase in the frequency of one-on-one parent-teacher meetings and increased attendance at parent-teacher conferences and parent council meetings as well as an increased support for school presentations (see Table 4.A.3 in the Appendix). Part of the increase in support for older siblings may stem from parents' improved German proficiency, making it easier to attend school events or help with assignments.

Table 4.4: Effects on Parental Investments

	Parental Investments				
	Overall	Educational resources	Frequency conversations	Frequency support	Frequency contact school
	(1)	(2)	(3)	(4)	(5)
Panel A: Investment in children					
Treat × Post	0.089***	0.075**	0.06	0.043	0.127
	(0.034)	(0.035)	(0.055)	(0.051)	(0.095)
N	24,805	16,158	8,371	16,283	3,864
Pre-reform Mean	2.529	2.306	2.961	2.717	1.811
Panel B: Investment in children (No siblings)					
Treat × Post	0.187***	0.289***	0.204**	0.034	0.177
	(0.057)	(0.080)	(0.092)	(0.075)	(0.107)
N	9,358	3,789	3,864	7,907	2,944
Pre-reform Mean	2.603	2.586	2.973	2.661	1.810
Panel C: Investment in children (At least one sibling)					
Treat × Post	0.040	0.023	-0.003	0.054	-0.026
	(0.041)	(0.041)	(0.081)	(0.063)	(0.184)
N	12,348	10,015	3,572	8,105	920
Pre-reform Mean	2.542	2.286	2.943	2.733	1.812
Panel D: Investment in siblings					
Treat × Post	0.286***	0.0157	0.343	0.399*	0.571***
	(0.107)	(0.134)	(0.234)	(0.210)	(0.181)
N	3169	1109	984	1980	1170
Pre-reform Mean	2.536	3.432	2.615	1.971	1.347
Min - Max	0-4	0-4	1-4	1-4	0-4

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the school level in parentheses. All regressions control for the child's gender, the mother's age, the family's highest educational degree, the survey year, the federal state, and year × state fixed effects. Analyses of parental investments in the focal child for the child's birth month and analyses of parental investments in older siblings control for the birth order, the age difference and sibling's birth month. *Source:* NEPS SC3 and SC4 (2010-2016), weighted, own calculations.

Sibling's identity and expected discrimination. The last mechanism investigated in this paper concerns changes in siblings' attitudes and perceptions. Granting citizenship to one family member could impact the family's sense of belonging and their

perception of the surrounding society and discrimination. First, I explore how siblings identify with Germany and the German society. Panel A of Table 4.5 shows no significant effect on siblings' identification with Germany or Germans – the coefficients are small and inconsistent. Consequently, changes in siblings' identity do not appear to explain the spillover effects on siblings' education. Panel B tests whether siblings expect discrimination in apprenticeship applications based on name, appearance, headscarf, or German skills. The analysis reveals no statistically significant effect on expected discrimination; if anything, there seems to be a slight increase, as all coefficients are positive. Hence, the focal child's citizenship does not reduce siblings' perceived labor market discrimination. It's also plausible that siblings don't expect strangers – who may not know their family's citizenship status – to treat them differently.

Table 4.5: Effects on Sibling's Identity and Expected Discrimination

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Sibling's Identity						
	Connection with Germany	Unpleasant to be Associated with Germans	Important to be Associated with Germany	Comfortable around Germans	Part of German Society	Connected-ness with Germans
Treat × Post	0.156	0.011	-0.047	0.018	-0.035	-0.088
	(0.151)	(0.081)	(0.137)	(0.103)	(0.115)	(0.078)
N	831	832	818	811	828	824
Pre-reform Mean	0.640	0.080	0.833	0.833	0.792	0.917
Panel B: Sibling's expected discrimination during applications because of...						
	Foreign Name	Foreign Appearance	Headscarf	German Skills		
Treat × Post	0.066	0.042	0.111	0.084		
	(0.129)	(0.113)	(0.144)	(0.080)		
N	1,033	1,033	993	1,069		
Pre-reform Mean	0.382	0.343	0.531	0.912		

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the school level in parentheses. All regressions control for the child's gender, the mother's age, the family's highest educational degree, the survey year, the federal state, year × state fixed effects, the birth order, the age difference and the sibling's birth month. All outcome variables are binary variables which equal 1 if the individual agrees with the statement and 0 if the individual disagrees. *Source:* NEPS SC4 (2011-2016), weighted, own calculations.

In summary, the results reveal three channels through which birthright citizenship has spillovers on older siblings: (i) increased naturalization of siblings and parents, (ii) greater use of German at home, and (iii) increased parental investment in older siblings, without reducing support for focal children. Other possible channels – such as changes in siblings' own educational investments, support from younger siblings, a reduced need for older siblings to provide academic support to their younger siblings, or younger siblings being role models for older siblings – could also matter but are beyond the scope of available data.

4.5.4 Heterogeneity

This section explores effect heterogeneity to better understand the underlying mechanisms. Table 4.A.4 presents the results for various subsamples. Panel A and B show that birthright citizenship benefits focal boys and older brothers in particular. This is noteworthy as prior research has shown that immigrant girls in Germany have higher educational attainment compared with boys (Kristen and Granato, 2007). Citizenship may help address gender differences. Additionally, older siblings benefit slightly more when the focal child is female.

Panel C reveals that spillovers are driven by older siblings who are more than 4 years (the median) older than the focal child. These siblings, aged 4 to 12 when the focal child was born, may have been more aware of the focal child's citizenship acquisition, boosting their motivation to integrate and invest in their own education.

Dahl et al. (2022) document improved well-being among boys from non-Muslim households and a decline in well-being for girls from Muslim households following the birthright citizenship reform. Building on their findings, I examine heterogeneous effects by parental country of origin, distinguishing between predominantly Muslim and non-Muslim countries.[45] The results indicate that the positive effects on focal children and their older siblings are largely driven by families originating from Muslim-majority countries. [46] Two possible explanations may account for this pattern: First, families with more precarious or uncertain immigration experiences – such as refugees from Balkan countries – may place greater value on stable prospects in the host country. Second, children from Muslim households had a lower baseline probability of completing high school on the academic track prior to the reform, leaving more room for improvement.

Panel E replicates the interaction explored in Dahl et al. (2022) – child gender and Muslim country of origin – but reveals a different pattern: effect sizes are broadly similar across subgroups, with two exceptions – focal girls and older brothers from non-Muslim households – who do not appear to experience any meaningful benefits. Additionally, I find no consistent differences in treatment effects between families from EU and non-EU countries (Panel F).[47]

[45] A country is classified as Muslim if the majority of its population is Muslim. Children are included in this group if at least one parent migrated from a Muslim country.

[46] An analysis based on the mother's country of origin yields similar results. By contrast, the differences between Muslim and non-Muslim countries are less pronounced when using the father's origin (results available upon request).

[47] A robustness check using the mother's EU origin yields similar results. When using the father's origin instead, effects on focal children appear slightly more pronounced for EU countries (results available upon request).

Table 4.A.5 examines five regional groups by parental[48] origin: Turkey, Eastern Europe, the Balkans, EU12, and other countries.[49] Focal children with parents from Turkey, the Balkans, and EU12 benefit the most. Interestingly, although children of Turkish mothers experienced the biggest increase in citizenship (see Figure 4.3), this did not lead to proportionally larger educational benefits. Among older siblings, the strongest effects appear for those whose parents came from the Balkans. By contrast, while focal children from EU12 families improved, their older siblings benefited not as much. A possible explanation for this discrepancy could be differing parental responses to the reform. Analyses by country of origin (available upon request), indicate that EU12 parents increased educational investments in focal children but reduced them for older siblings. This suggests a reallocation of limited resources toward the child perceived to benefit most from the reform (e.g., Becker and Tomes, 1976). In relatively well-integrated EU12 families, where baseline educational engagement was already high, the reform may have led parents to prioritize the newly entitled child over older siblings.

A key issue in Germany's citizenship debate is the regulation of dual citizenship. Until the recent reform effective June 27, 2024, most naturalizing individuals were required to renounce their previous citizenship, a known barrier to naturalization (Weinmann, 2022). Exemptions applied to migrants from EU countries, Switzerland, or states that prohibit renunciation. In Table 4.A.6, I compare treatment effects on parents' and siblings' naturalization between these exempt groups and others to assess whether renunciation requirements deterred naturalization. Surprisingly, the treatment effects are stronger among families previously subject to the renunciation rule. One possible explanation is that, for these families, the cost of naturalizing was initially too high, but once their child or younger sibling acquired citizenship via birthright, the benefits became clearer, and outweighed the cost of giving up their original citizenship.[50] Therefore, this result cannot be seen as an overall effect of dual citizenship restrictions.

[48] The results do not meaningfully differ when using the mother's versus the father's country of origin.

[49] Eastern Europe includes Moldova, Russia, Slovakia, Ukraine, Hungary, Poland, the Czech Republic, Belarus, Bulgaria, Romania, and individuals born in the Soviet Union. Albania, Bosnia, Croatia, Kosovo, Montenegro, North Macedonia, Serbia, and Slovenia are classified as Balkan countries. The EU12 countries consist of Belgium, Denmark, France, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, and the UK.

[50] This is further evidenced by the data, which shows that 20-24 % of parents naturalized only after their child was born, despite having fulfilled the residency requirement prior to birth. By the time the child finishes high school, 65 % of parents still do not possess German citizenship. Conditional on naturalization, mothers naturalized on average 1.6 years and fathers 1.2 years after their child's birth.

In sum, focal boys and children with Balkan or EU12 backgrounds benefited most. Spillovers were strongest for older brothers, siblings with younger sisters, siblings at least 4 years older than the focal child, and those of Balkan origin.

4.5.5 Robustness of the Results

Common trend assumption. The main explanatory variable, the interaction of the treatment and post indicators, identifies the causal effect of birthright citizenship under the assumption of a common trend between treatment and control groups. Figures 4.4 and 4.5 support this, showing no significant pre-trends. Furthermore, no reforms or events took place during the study period which affected only the control or treatment group. The 2014 repeal of the "Optionspflicht", which originally required children granted citizenship via the reform to choose between German citizenship and their second nationality once they reached 18 years of age, did not affect the sample as none of the focal children was 18 by 2014.

Treatment group definition. Figure 4.3 reveals no first-stage effects on birthright citizenship for children with parents from Eastern European or other countries. Consequently, no effects on education or other outcomes should be anticipated for these families. Excluding these families from the treatment group produces even stronger treatment effects (Table 4.A.7).

Control group definition. Although no visible pre-trends are detected, and there were no reforms aimed to differentially affect the control and treatment groups, time-varying policies or developments, such as the influx of other migrants, may have impacted especially children with two parents born in Germany differently than those with two parents born abroad. Additionally, native children may be unsuitable controls for outcomes like naturalization, which is universally 1 for them. To address this, I use two alternative control groups.[51] First, I define children with one German-born parent and one foreign-born parent as the control group, excluding those with two German-born parents. Results (Tables 4.A.7 and 4.A.8) confirm that effects on education and German usage are robust under this specification. Effects on mothers' and siblings' citizenship are also similar, while effects on fathers' citizenship status are smaller and no longer statistically significant. Second, I classify children of mothers born in (i) Eastern Europe or (ii) other countries as the control group. As I do not observe a first-stage effect for this group (see Figure 3), it constitutes a valid control

[51] The NEPS sample size is too small to reliably apply these two approaches to outcomes related to parental investments, identity, and discrimination. While the data indicates that these variables also exhibit variation within the control group, some concerns about robustness and interpretability remain and cannot be addressed using these methods.

group expected to evolve similarly over time. The effects on focal children's educational outcomes are slightly smaller but remain statistically significant. By contrast, the effects on older siblings' education are similar in magnitude but lose statistical significance due to the substantially smaller sample size (see Table 4.A.7). The effects on other family members' naturalization remain robust under this specification (see Table 4.A.8), whereas no significant effects on German language usage in the household are observed.

Manipulation around the cutoff. A potential threat is strategic childbirth timing to qualify for citizenship. Such manipulation would occur if the reform had an impact on fertility rates. In general, parents had no incentive to postpone childbirth until 2000. Instead, eligible parents of children born between 1990 and 1999 could apply for German citizenship until December 31, 2000. Although fertility declined post-reform (Avitabile et al., 2014), this occurred only after 2001. Therefore, the main effects are based on the 2000 cohort and effects for later cohorts are interpreted with caution. Due to declining fertility, spillovers on younger siblings (born post-reform) cannot be assessed.

Transition rule. Some pre-2000 children received citizenship via the transition rule. Thus, some children may have been misclassified as untreated in this paper even if they received German citizenship under the transition rule. Yet, only 20% of eligible children used this rule (Felfe et al., 2020), and I scale effects accordingly. I also limit the sibling analysis to those born 1990–1998, ensuring equal exposure to this rule. Table 4.3 shows that the transition rule use does not explain spillovers. Excluding focal children and older siblings who benefited from the transition rule, also does not change the results (Table 4.A.7). If it did, this would serve as an explanatory mechanism rather than a threat to the identification strategy. In an additional test, I include only older siblings born before 1990, who were therefore not affected by the transition rule. Although the estimate is less precise due to a smaller sample size, its size is even larger – likely due to a larger age difference between siblings.

Residency requirement. The reform required that the child must be born after 2000 and that at least one parent must have resided in Germany for a minimum of eight years and held a legal residence title at the time of the child's birth. In the main specifications, I focus solely on the timing of childbirth, rather than the length of the parents' residency due to (i) substantial measurement error in the residency variable, (ii) data availability limited to cohabiting parents, and (iii) the absence of information regarding legal residency titles in the Microcensus. As a robustness check, I exclude

children whose parents don't meet the residency criterion. The results presented in Table 4.A.7 indicate that this exclusion does not alter the estimated effects.

Sample selection (household residence). The Microcensus only includes co-residing individuals, which excludes older siblings who moved out of the parental household. Therefore, my analysis shows the effects of birthright citizenship on older siblings completing the academic track while living in the parental household. The sample exclusion may bias estimates if residential mobility is correlated with education. To address this, I conduct a bounding analysis following Felfe et al. (2020). I boost the sample by incorporating the average share of individuals not living in the parental household by age group according to official statistics (Statistisches Bundesamt, 2024).[52] I then estimate the effects under four scenarios that assign academic track completion outcomes to these unobserved individuals: (i) none of the added individuals in either the treatment or control group complete the academic track, (ii) added individuals in the treatment group do not complete, while those in the control group do, (iii) added individuals in the treatment group complete, while those in the control group do not, and (iv) all added individuals in both groups complete the academic track. Under these varying assumptions, the estimated spillovers remain sizable and statistically significant in all cases except the scenario where all added treatment group observations are assigned academic track completion and all added control group observations are assigned non-completion (Table 4.A.9).[53] Therefore, sample selection should not drive the estimated treatment effects.

Return migration. Sajons (2016) shows that a child's citizenship access reduces family out-migration. Thus, the treatment group in the post period of my analysis could include families which would otherwise have left Germany, making them less comparable to families in the pre-period. However, families with children born before and after the birth date cutoff seem to be similar in predetermined characteristics (see Table 4.A.1), mitigating this concern.

Placebo test. To rule out the possibility that my effects are driven by unobservable characteristics or underlying trends, I perform two placebo reform tests. The first

[52] 2.7% for 17-year-olds, 7.1% for 18-year-olds, 17.0% for 19-year-olds, 27.9% for 20-year-olds, and 38.6% for 21-year-olds.

[53] To understand these estimated bounds compared to the main estimate, it is useful to consider how the group means respond to the artificial assignment of outcomes. Although I assign the same proportion of added observations to both treatment and control groups, the control group has a higher baseline academic track completion rate. As a result, assigning 1 to the treatment observations and 0 to the control observations reduces the observed treatment effect, because the increase in the treatment group's mean is smaller than the decrease in the control group's mean. Conversely, assigning 0 to the treatment observations and 1 to the control observations maximizes the estimated treatment effect.

employs the same event study approach and time period as in the main specification, while comparing two subgroups within the control group: children with one foreign-born parent and one German-born parent with children with two German-born parents. Both groups received German citizenship at birth due to their parents' citizenship; thus, the reform should not have affected them differently. Figures 4.A.1 and 4.A.2 in the Appendix show that no significant effect was observed in this placebo treatment group. The second test is based on the same treatment and control groups as in the main specification, but employs January 1, 1996, as the reform cutoff. These analyses shown in Figures 4.A.3 and 4.A.4 cannot detect any effects for this placebo reform.[54] These two tests strengthen the common trend assumption.

Controlling for household income. Parents' financial investments may increase due to higher household income connected to the focal child's citizenship. However, the effects on parents' investments remain robust even after controlling for household income.[55]

Standard errors. I test robustness to clustering standard errors: at the birth month/ year level and at the family level – as multiple focal children and older siblings can be included in the sample. Both tests confirm statistical significance of the results (see Table 4.A.7).

4.6 Conclusion

This paper investigates the spillover effects of Germany's birthright citizenship reform on older siblings of children who received citizenship at birth. It demonstrates that access to birthright citizenship increases immigrant children's probability of completing the academic secondary track by 13-16 percentage points, which is a smaller effect size compared with medium-term effects identified in previous studies (19 pp., Felfe et al., 2020).[56] The reform also benefits older siblings, raising their likelihood of obtaining a university entrance qualification by about half as much. As a comparison: Karbownik and Oezek (2023) find siblings spillover effects of educational achievements with a magnitude that is 30% of the magnitude of the direct effects on focal children.

The spillovers appear to be partly driven by increased naturalization among parents and older siblings – who appear to pursue citizenship independently – suggesting that

[54] The coefficients for the effects on siblings are less precisely estimated for the birth cohorts 1991-1991 as they are based on fewer siblings.

[55] The results are available from the author upon request.

[56] Felfe et al. (2020) restrict the sample to children born in 1999 and 2000 and therefore their effect size corresponds to my effect size of 16 percentage points which I find for the sample of children born 1999-2000.

some effects are direct outcomes of citizenship access for siblings. There likely also is an increase in German usage at home – although this is not robust across all specifications – impacting children's German proficiency and parents' ability to support their children academically. I additionally provide suggestive evidence that parents increase investments in older siblings while maintaining support for the focal child. These results provide valuable insights into parental behavior and family dynamics. The patterns suggest that parents recognize the benefits of German citizenship and strategically adjust both naturalization and educational investments, treating them as substitutes (Heckman and Mosso, 2014). Gaining citizenship for one child reduces informational barriers, prompting parents to naturalize and extend benefits to older siblings or compensate for unequal access through greater educational investment. This behavior reflects findings from Berry et al. (2020), showing parents' willingness to equalize opportunities across children. Additionally, parents may update their beliefs about the family's long-term prospects in Germany, thereby enhancing their expectations for the returns on education not only for the focal child but also for older siblings (Becker, 1962). With higher expected returns to education for older siblings, parents may choose to invest more in their education.

Heterogeneity analyses highlight the importance of the migration context. While birthright citizenship increased most among children with Turkish parents, educational gains were larger for children from Balkan families, many of whom arrived as refugees fleeing the war in former Yugoslavia (Alscher et al., 2015). As Fasani et al. (2024) show, citizenship has a greater impact on the labor market outcomes of forced migrants, likely because their integration takes generally longer. For refugee families, citizenship may represent stability. Additionally, educational gains were similar for non-EU and EU migrant families, even though non-EU children gained broader EU labor market access. This supports findings by Govind and Sirugue (2023) that EU migrants are likelier to naturalize in France than non-EU migrants despite limited direct benefits, suggesting that citizenship carries symbolic value beyond legal rights.

These findings confirm that early access to citizenship has significant benefits, greater than previously estimated. These benefits ar important with respect to educational inequalities and talent shortages in the long run (see e.g. OECD, 2023d). They also highlight the relevance of current debates in Germany and elsewhere on citizenship reforms. The new German law of June 27, 2024 – which reduces the residency requirement – may also enhance educational outcomes for children and siblings. The results are also relevant for countries like the US, where older siblings who immigrate after birth might still benefit despite not having citizenship themselves.

Finally, it is important to note that the studied families immigrated at least 8 years before the child's birth and stayed through the end of secondary school – indicating a strong intention to remain and integrate. These findings may not apply to more recent migrants with no intention to stay. Future research should additionally assess labor market impacts as these children enter employment, determining whether these reforms can effectively address the demand for highly skilled labor.

4.A Appendix

4.A.1 Additional Descriptives

Table 4.A.1: Comparison of Pre- and Post-Reform Groups

	Pre	Post
	1994-1999	2000-2002
	Mean/Percentage	
Focal Child is male	51.93 %	52.00 %
Birth month (focal child)	6.54 (3.39)	6.26 (3.39)
Birth year (focal child)	1996.86 (1.63)	2000.8 (0.79)
Older sibling's age (in years)	19.22 (1.58)	19.1 (1.61)
Age difference between siblings (in years)	3.33 (1.64)	5.11 (2.29)
Mother's age at birth (in years)	27.53 (4.27)	27.38 (4.38)
Highest post-secondary degree in the household		
No degree	11.91 %	13.02 %
Vocational training	57.42 %	57.31 %
University	26.0 %	26.22 %
Missing	4.67 %	3.45 %
Mother's region of origin		
German	75.19 %	72.32 %
Turkey	8.04 %	9.73 %
Eastern Europe	1.35 %	1.74 %
Balkan	2.16 %	2.86 %
Eu 12	2.09 %	2.37 %
Other	4.6 %	5.19 %
Missing	6.57 %	5.78 %
Father's region of origin		
German	63.87 %	60.61 %
Turkey	7.37 %	9.16 %
Eastern Europe	0.77 %	1.03 %
Balkan	2.0 %	2.82 %
Eu 12	2.3 %	2.45 %
Other	3.84 %	4.52 %
Missing	19.84 %	19.41 %
Observations	60,651	29,364

Note: Standard deviation in parentheses. The statistics are based on the sample of households with older siblings born 1990-1998 and a focal child born 1994-2002. "Pre" includes all older siblings with a focal child born 1994-1999 and "post" includes all older siblings with a focal child born 2000-2002. *Source:* German Microcensus (2010-2021), weighted, own calculations.

4.A.2 First Stage

Table 4.A.2: First Stage

	Focal Child Sample		Sibling Sample	
	Focal Child Birth Cohorts			
	1992-2002	1999-2000	1994-2002	1999-2000
	(1)	(2)	(3)	(4)
Post	0.358***	0.248***	0.365***	0.326***
	(0.007)	(0.014)	(0.009)	(0.016)
Observations	24,978	5,157	12,243	3,661

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. Post equals 1 for all children born since 2000 and 0 for all children born until 1999. The regressions only include children in the treatment group (children born to two parents who were born abroad and immigrated). *Source:* German Microcensus (2010-2021), weighted, own calculations.

4.A.3 Detailed and Subsample Analyses

Table 4.A.3: Effects on Parental Investments (Detailed Analysis)

	Parental Investments			
	(1)	(2)	(3)	(4)
Panel A: Investment in children (no siblings): Educational resources				
	Desk	Educational Software	Books for Homework	Computer
Treat × Post	0.022	0.133**	0.144***	0.014
	(0.031)	(0.054)	(0.052)	(0.015)
N	3,252	3,209	3,241	3,156
Pre-reform Mean	0.941	0.491	0.718	0.973
Min - Max	0-1	0-1	0-1	0-1
Panel B: Investment in children (no siblings): Frequency conversations about...				
	Curriculum	Difficulties in School		
Treat × Post	0.226*	0.176*		
	(0.117)	(0.097)		
N	3,857	3,855		
Pre-reform Mean	2.855	3.092		
Min - Max	1-4	1-4		
Panel C: Investment in siblings: Frequency of support with...				
	Buying Study Material	Support with Presentations		
Treat × Post	0.043	0.415*		
	(0.361)	(0.219)		
N	810	1,980		
Pre-reform Mean	2.5	1.863		
Min - Max	1-4	1-4		
Panel D: Investment in siblings: Frequency of contact with the school				
	Parent-teacher conferences	Talks with Teachers	Parents Council	Help with School Events
Treat × Post	0.510	0.851***	0.638**	0.187
	(0.328)	(0.245)	(0.280)	(0.250)
N	1,113	1,167	1,167	1,130
Pre-reform Mean	3.533	2.161	1.645	2.167
Min - Max	0-4	0-4	0-4	0-4

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the school level in parentheses. All regressions control for the child's sex, the mother's age, the family's highest educational degree, the survey year, the federal state, and year × state fixed effects. Analyses of parental investments in the focal child control for the child's birth month and analyses of parental investments in older siblings control for the birth order, age difference and sibling's birth month. *Source:* NEPS SC3 and SC4 (2010-2016), weighted, own calculations.

Table 4.A.4: Effects on Focal Child's and Sibling's Academic School Track Completion - For Different Subsamples

	Focal Child		Older Sibling	
	(1)	(2)	(3)	(4)
Panel A: By Focal Child's Gender				
	Female	**Male**	**Female**	**Male**
Treat × Post	0.037*** (0.010)	0.058*** (0.010)	0.028** (0.013)	0.016 (0.013)
Observations	154,746	152,748	43,303	46,712
Mean	0.747	0.668	0.727	0.651
Panel B: By Older Sibling's Gender				
			Female	**Male**
Treat × Post			0.018 (0.013)	0.023* (0.013)
Observations			43,472	46,543
Mean			0.689	0.686
Panel C: By Age Difference Between Siblings				
			Below median	**Above median**
Treat × Post			0.016 (0.017)	0.031** (0.012)
Observations			46,535	43,480
Mean			0.712	0.662
Panel D: By Parents' Country of Origin: Non-muslim vs. Muslim				
	Non-Muslim	**Muslim**	**Non-Muslim**	**Muslim**
Treat × Post	0.027** (0.012)	0.040*** (0.009)	0.010 (0.017)	0.019* (0.011)
Observations	286,801	295,483	79,813	85,976
Mean	0.712	0.708	0.699	0.689
Panel E: By Parent's Country of Origin: Non-muslim vs. Muslim and Gender				
	Non-Muslim	**Muslim**	**Non-Muslim**	**Muslim**
			Girls	
Treat × Post	0.004 (0.017)	0.039*** (0.012)	0.018 (0.024)	0.018 (0.015)
Observations	144,213	148,688	37,929	41,350
Mean	0.752	0.748	0.743	0.730
			Boys	
Treat × Post	0.047*** (0.017)	0.042*** (0.013)	-0.001 (0.023)	0.020 (0.016)
Observations	142,588	146,795	41,884	44,626
Mean	0.671	0.668	0.660	0.651
Panel F: By Parents' Country of Origin: Non-EU vs. EU				
	Non-EU	**EU**	**Non-EU**	**EU**
Treat × Post	0.035*** (0.010)	0.038** (0.016)	0.016 (0.010)	0.022 (0.024)
Observations	299,709	282,575	87,847	77,942
Mean	0.708	0.711	0.689	0.699

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. All regressions include year and state fixed effects, year*times*state fixed effects, the child's gender, the mother's age at birth, the family's highest educational degree and the focal child's birth month; columns 3 and 4 also control for the birth order, the age difference between siblings, and the sibling's birth month. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Table 4.A.5: Effects on Focal Child's and Sibling's Academic School Track Completion - by Parents' Region of Origin

	Focal Child		Older Sibling	
	(1)	(2)	(3)	(4)
Panel A: By Mother's Region of Origin				
	Turkey	**Eastern Europe**	**Turkey**	**Eastern Europe**
Treat × Post	0.038*** (0.011)	-0.007 (0.026)	0.017 (0.013)	0.006 (0.039)
Observations	290,515	279,018	83,347	76,818
Mean	0.708	0.712	0.691	0.701
	Balkan	**EU 12**	**Balkan**	**EU 12**
Treat × Post	0.064*** (0.019)	0.064*** (0.023)	0.054** (0.025)	0.029 (0.032)
Observations	281,016	280,160	77,828	77,230
Mean	0.711	0.711	0.698	0.699
	Other		**Other**	
Treat × Post	0.016 (0.014)		0.002 (0.018)	
Observations	282,838		79,117	
Mean	0.714		0.702	
Panel B: By Father's Region of Origin				
	Turkey	**Eastern Europe**	**Turkey**	**Eastern Europe**
Treat × Post	0.027** (0.011)	0.031 (0.028)	0.029** (0.013)	0.034 (0.043)
Observations	289,323	278,635	82,812	76,736
Mean	0.709	0.712	0.691	0.701
	Balkan	**EU 12**	**Balkan**	**EU 12**
Treat × Post	0.058*** (0.020)	0.066*** (0.024)	0.064** (0.026)	0.010 (0.033)
Observations	280,614	279,902	77,715	77,161
Mean	0.711	0.711	0.698	0.700
	Other		**Other**	
Treat × Post	0.002 (0.014)		-0.009 (0.018)	
Observations	282,259		78,907	
Mean	0.713		0.702	

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. All regressions include year and state fixed effects, the child's gender, the mother's age at birth, the family's highest educational degree and the focal child's birth month; columns 3 and 4 also control for the birth order, the age difference between siblings and the sibling's birth month. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Table 4.A.6: Effects on Naturalization of Family Members – by Dual Citizenship Restrictions

	Citizenship			Transition Rule
	Mother	Father	Sibling	Sibling
	(1)	(2)	(3)	(4)
Panel A: Dual Citizenship Restricted				
Treat × Post	0.027*** (0.006)	0.017** (0.007)	0.034*** (0.009)	-0.006** (0.003)
Observations	590,159	479,155	181,769	181,769
Pre-reform Mean	0.345	0.400	0.577	0.037
Panel B: Dual Citizenship not Restricted				
Treat × Post	0.008 (0.010)	0.010 (0.012)	0.021 (0.020)	-0.005 (0.005)
Observations	408,180	434,907	159,885	159,885
Pre-reform Mean	0.215	0.241	0.390	0.013

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. All regressions include year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month, columns 3-5 also include the birth order, the age difference between siblings, and the sibling's birth month as controls. The outcome variable "Citizenship" captures whether the mother (col. 1), the father (col. 2) or the older sibling (col. 3) has German citizenship at the time of the survey and "Transition rule" indicates whether individuals made use of the transition rule described in Section 4.2. This is a binary variable which equals 1 for individuals who are born before 2000 and who received their German citizenship in 2000 or 2001 (as 89% of children who benefited from the rule naturalized in 2000 or 2001, see Worbs, 2008). *Source:* German Microcensus (2010-2021), weighted, own calculations.

4.A.4 Robustness

Table 4.A.7: Effects on Focal Child's and Older Sibling's Academic School Track Completion - Robustness Tests

	Focal Child (1)	Older Sibling (2)
Main estimate	0.047*** (0.007)	0.021** (0.009)
Observations	307,494	90,015
Treatment group:		
Excl. Eastern Europe & other countries	0.054*** (0.008)	0.026** (0.011)
Observations	293,784	84,576
Control group:		
1 German-born & 1 foreign-born parent	0.033*** (0.010)	0.044*** (0.014)
Observations	51,775	20,387
Control group:		
Eastern Europe & other countries	0.038** (0.015)	0.027 (0.019)
Observations	27,052	13,155
Excl. focal children using transition rule	0.049*** (0.007)	0.024*** (0.009)
Observations	306,643	89,657
Excl. siblings using transition rule		0.023** (0.009)
Observations		89,451
Only siblings born before 1990		0.073 (0.065)
Observations		364
Excl. families <8 years residency at birth	0.049*** (0.008)	0.017* (0.010)
Observations	298,645	87,297
SE clustered at birth month-year level	0.047*** (0.009)	0.021** (0.011)
Observations	307,494	90,015
SE clustered at family level	0.047*** (0.008)	0.021* (0.011)
Observations	307,494	90,015
Sample with citizenship information	0.056*** (0.008)	0.030*** (0.010)
Observations	302,194	88,446

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses (clustered standard errors in specifications 9 and 10). The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. All columns include year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month, columns 3 and 4 also control for the birth order, the age difference between siblings, and the sibling's birth month. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Table 4.A.8: Effects on Naturalization of Family Members and German Language Usage - Alternative Control Groups

	Citizenship			Transition Rule	German in
	Mother	Father	Sibling	Sibling	the Household
	(1)	(2)	(3)	(4)	(5)
Main estimate	0.025***	0.018***	0.038***	0.001	0.036***
	(0.006)	(0.007)	(0.013)	(0.005)	(0.012)
Observations	604,774	491,640	62,596	62,596	51,566
1 German-born & 1 foreign-born parent	0.057***	0.003	0.029***	-0.006**	0.046***
	(0.008)	(0.009)	(0.009)	(0.003)	(0.016)
Observations	128,228	107,285	46,106	46,106	14,705
Eastern Europe & other countries	0.066***	0.042***	0.038**	-0.001	-0.045
	(0.013)	(0.012)	(0.017)	(0.006)	(0.030)
Observations	76,255	67,716	30,980	30,980	8,225

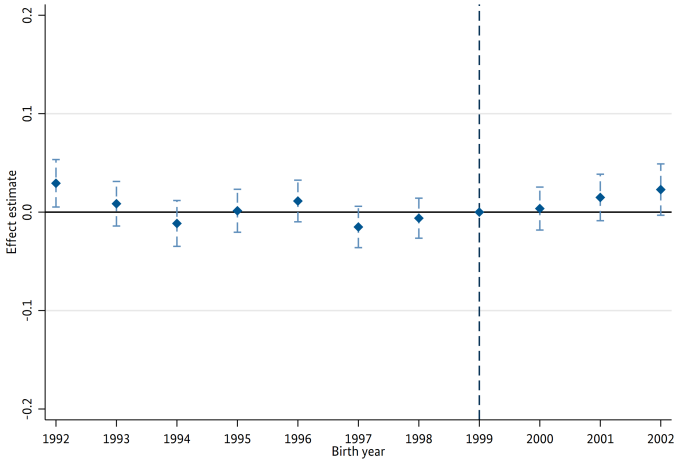
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. All columns include year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month, columns 3-5 also control for the birth order, the age difference between siblings, and the sibling's birth month. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Table 4.A.9: Effects on Older Sibling's Academic School Track Completion - Bounds

	Treat 0 Control 0	Treat 0 Control 1	Treat 1 Control 0	Treat 1 Control 1
	(1)	(2)	(3)	(4)
Treat × Post	0.024***	0.026***	0.013	0.014*
	(0.009)	(0.009)	(0.009)	(0.008)
Observations	103,907	103,907	103,907	103,907

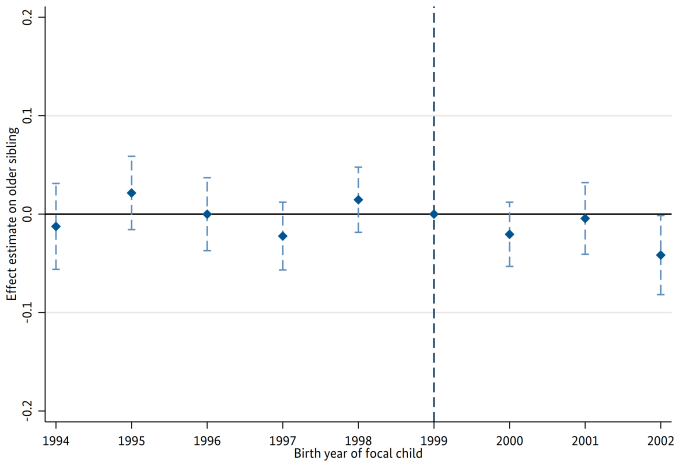
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. All columns include year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month, the birth order, the age difference between siblings, and the sibling's birth month. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Figure 4.A.1: Effect on Focal Children's Academic School Track Completion: Placebo Treatment Group



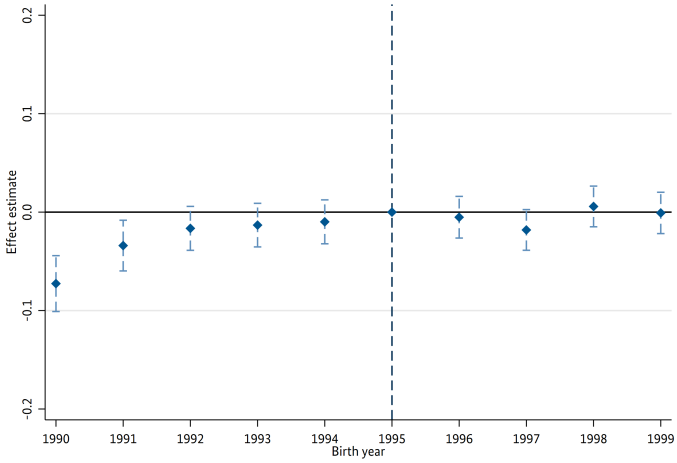
Notes: These coefficients are estimated using the event study approach described in equation 4.1. The figure displays the coefficients of the interaction of the treatment variable $Treat_i$ with the birth cohorts compared to the last pre-reform year 1999. The outcome variable captures whether the individual (aged 17-22) either obtained a university entrance qualification or currently attends a school track leading to university entrance qualification. The treatment group includes children with one German-born and one foreign-born parent. The control group includes children with two German-born parents. The regression includes year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month. N=277,299. 90% confidence intervals based on robust standard errors shown. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Figure 4.A.2: Effect on Siblings' Academic School Track Completion: Placebo Treatment Group



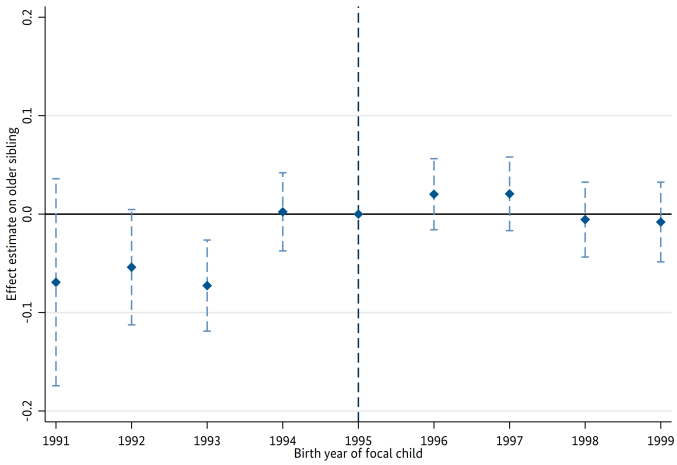
Notes: These coefficients are estimated using the event study approach described in Equation 4.1. The figure displays the coefficients of the interaction of the treatment variable $Treat_i$ with the focal child's birth cohorts compared to the last pre-reform year 1999. The outcome variable captures whether the sibling (aged 17-22) either obtained a university entrance qualification or currently attends a school track leading to university entrance qualification. The treatment group includes children with one German-born and one foreign-born parent. The control group includes children with two German-born parents. The regression includes year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree, the focal child's birth month, the birth order, the age difference between siblings, and the sibling's birth month. N=76,237. 90% confidence intervals based on robust standard errors shown. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Figure 4.A.3: Effect on Focal Children's Academic School Track Completion: Placebo Reform 1996



Notes: The coefficients are estimated using the event study approach described in equation 4.1 taking January 1,1996 as the reform cutoff. The figure displays the coefficients of the interaction of the treatment variable $Treat_i$ with the birth cohorts compared to the last pre-reform year 1995. The outcome variable captures whether the individual (aged 17-22) either obtained a university entrance qualification or currently attends a school track leading to university entrance qualification. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. The regression includes year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree and the focal child's birth month. N=289,355. 90% confidence intervals based on robust standard errors shown. *Source:* German Microcensus (2010-2021), weighted, own calculations.

Figure 4.A.4: Effect on Siblings' Academic School Track Completion: Placebo Reform 1996



Notes: These coefficients are estimated using the event study approach described in Equation 4.1 taking January 1,1996 as the reform cutoff. The figure displays the coefficients of the interaction of the treatment variable $Treat_i$ with the focal child's birth cohorts compared to the last pre-reform year 1995. The outcome variable captures whether the sibling (aged 17-22) either obtained a university entrance qualification or currently attends a school track leading to university entrance qualification. The treatment group includes all children born to two parents who were born abroad and immigrated. The control group includes all children with at least one parent born in Germany. The regression includes year and state fixed effects, year*times*state fixed effects, the focal child's gender, the mother's age at birth, the families highest educational degree, the focal child's birth month, the birth order, the age difference between siblings, and the sibling's birth month. N=46,947. 90% confidence intervals based on robust standard errors shown. *Source:* German Microcensus (2010-2021), weighted, own calculations.

CHAPTER 5

The Generational Gift: The Effects of Grandparental Care on the Next Generations' Health and Well-being[1]

5.1 Introduction

Health and well-being are central priorities for policymakers (see, for example, Helliwell et al., 2024; OECD, 2023a; United Nations, 2022), as they are not only human rights but also important drivers of human capital accumulation (e.g., Currie, 2020), labor force participation (e.g., Frijters et al., 2014), and, ultimately, economic growth (e.g., Well, 2007). In addition to the individual level, health and well-being are relevant study objects due to their intergenerational transmission and persistence (e.g., Coneus and Spiess, 2012; Datta Gupta et al., 2023). Therefore, it is important to study health in the family context and the influence of intrafamilial interactions. This provides a better

[1]This chapter is joint work with Mara Barschkett (Rheinische Friedrich-Wilhelms-Universität Bonn) and C. Katharina Spiess (Federal Institute for Population Research (BiB) Wiesbaden, Johannes Gutenberg University, Mainz). We are grateful for the financial support from the Stiftung Ravensburger Verlag. This paper was pre-published as a working paper as *Barschkett, M., Spieß, C. K. and Ziege, E. (2021). Does Grandparenting Pay off for the Next Generations? Intergenerational Effects of Grandparental Care. IZA DP, 14795*; *Barschkett, M., Spieß, C. K. and Ziege, E. (2021). Does Grandparenting Pay off for the Next Generations? Intergenerational Effects of Grandparental Care. DIW Discussion Papers, 1152*; and *Barschkett, M., Spieß, C. K. and Ziege, E. (2022). Does Grandparenting Pay off for the Next Generations? Intergenerational Effects of Grandparental Care. BiB Working Paper, 2/2022.* The policy report *Barschkett, M., Gambaro, L., Schäper, C., Spiess, C. K. und Ziege, E. (2022). Oma und Opa gefragt? Veränderungen in der Enkelbetreuung - Wohlbefinden von Eltern - Wohlergehen von Kindern. BiB Bevölkerungstudien, 1/2022.* was published based on parts of this chapter.

understanding of how the larger family impacts the health of individual members, in this case children and their parents. One important interaction is caregiving, for example grandparents caring for their grandchildren. Grandparental care is the third most important care option, following parents and daycare, in many OECD countries (OECD, 2019).[2] However, the extent of grandparental care varies considerably due to country-specific differences in daycare settings and female labor force participation. For instance, in Germany, a country with relatively low maternal employment rates and a universal daycare system, approximately one in four children under the age of eleven receives regular care from their grandparents (Section 5.2). Despite the expansion of daycare slots over the last decades in Germany, the importance of grandparental care has remained relatively stable over time (see Gambaro et al., 2024).

Despite the continuous importance of grandparents in providing child care, its effects on parental and child health and well-being have received little attention in the literature.[3] In this paper, we fill this research gap by adopting a double-generation perspective and estimating the effects of grandparental care on parental and child health and well-being.[4] Identifying a causal relationship between grandparental care and these outcomes poses a challenge due to the endogeneity of the care decision. To overcome this, we utilize distance to grandparents as an instrumental variable (IV), assuming that the proximity to grandparents only affects parental and child outcomes through the provision of care. Using this instrument, one might have concerns regarding the validity of the exclusion restriction. The main threats to validity include differences between families living closer or further away from grandparents and strategic relocation patterns. We return to this issue in detail below, providing evidence that we can exclude major concerns regarding the exogeneity of the instrument. For example, we combine the IV approach with entropy balancing to ensure comparability between families residing close and far from grandparents and we show that our results are robust to relaxing the exclusion restriction. Additionally, we demonstrate that neither parents nor grandparents strategically relocate around the time of childbirth.

[2]The term "daycare" refers to all forms of formal child care provided by professionals outside the family. The term "parental child care" refers to all child care provided by the child's mother or father. "Grandparental care" describes the situation in which grandparents care for their grandchildren on a regular basis, i.e., excluding emergency care.

[3]For a literature overview, primarily focusing on studies that display non-causal associations between grandparental care and parental and child health, see Hank et al. (2018).

[4]Health and well-being are two closely related concepts that are difficult to distinguish from each other. The WHO defines "Health is a state of complete physical, mental and social well-being and not merely the absence of disease or infirmity" (WHO, 1948). Therefore, in this study we use the terms health and well-being as measures of physical and mental health as well as overall and domain-specific life satisfaction. Such subjective evaluations are considered valid, reliable and cost-effective means of health assessment (Kaplan and Baron-Epel, 2003).

Our analysis is based on a sample of families with children below eleven using *pairfam*, a representative panel data set for Germany surveyed between 2009 and 2020. We consider outcomes at the parental and child levels. Specifically, we evaluate parents' subjective health and various dimensions of well-being, including life satisfaction and domain-specific satisfaction such as satisfaction with their leisure or the child care situation. For children, we use a parent-assessed health measure. Underlying reasons why grandparental care may have effects on children and their parents can be manifold. In terms of parental outcomes, one hypothesis is that grandparental care increases parental satisfaction with leisure activities by providing parents with more time for activities unrelated to child care. A contrasting hypothesis is that grandparental care reduces health and well-being of parents by increasing emotional stress between the grandparents and parents, as relationships within the family are prone to emotional conflicts and disagreements about child rearing (Clark et al., 2020). Additionally, grandparental care may be less stable or continuous compared to other forms of care due to the grandparents' potential illness[5] or other obligations, which could result in higher levels of parental stress. Regarding child outcomes, we consider two competing hypotheses. The first hypothesis posits that grandparental care may negatively impact a child's health. Previous research has shown mixed results for (non-)cognitive skills, with a slight tendency toward negative effects (e.g., Danzer et al., 2020; Zhang et al., 2021). Supporting this hypothesis, Ao et al. (2022) demonstrate that grandparents in China are less strict about limiting children's TV viewing time compared to parents, potentially reducing time spent on structured sports and outdoor activities, which could negatively affect the child's health. The opposing hypothesis suggests that grandparental care could have positive effects on the child's health. This hypothesis is based on the notion that grandparents may have more time to dedicate solely to the child, providing focused attention and care. In general, the intensity of grandparental care – even if provided regularly – may not be significant enough to substantially impact child and parent outcomes. Therefore, it remains an empirical question of whether grandparental care has implications for the next generations' health and well-being.

Overall, our results provide evidence that, grandparental care is beneficial, particularly for maternal well-being. We show that grandparental care increases maternal satisfaction with child care and leisure by 9 and 11 %, respectively, compared to the mean. Furthermore, we find substantial increases in fathers' satisfaction with child care (19%). The effects on satisfaction with child care are mostly driven by parents

[5]Eibich and Zai (2024) show that grandparental care has adverse effects for grandparents' self-reported health, limitations in daily activities and depressive symptoms. These health challenges may result in grandparents being more likely to cancel their caregiving commitments unexpectedly, potentially disrupting child care arrangements.

with higher education. However, we do not find evidence that grandparental care affects parental health, life satisfaction or other domains of satisfaction. By contrast, our analysis shows that grandparental care has a negative impact on children's health. These effects are more pronounced for boys and might be explained by differences in the afternoon program organized by schools/daycare centers compared to grandparents.

While the effects of grandparental care on the grandparents themselves have been studied extensively, less attention has been given to the outcomes for parents and children.[6] Therefore, our study makes three contributions. First, we contribute to the literature on the effects of various care modes, in this case grandparental care, on parental outcomes by studying the effects of grandparental care on parental health and well-being. The existing literature on the effects of other care modes – particularly daycare – on parental outcomes is vast and primarily focuses on maternal employment (for an overview, see Müller and Wrohlich, 2020), fertility (e.g., Bauernschuster et al., 2016), health (e.g., Barschkett and Bosque-Mercader, 2024; Herbst and Tekin, 2014), maternal well-being (e.g., Kröll and Borck, 2013; Schmitz, 2019) and the integration of migrant mothers (e.g., Gambaro et al., 2021). Research on grandparental care and parental outcomes has mainly centered on maternal employment and fertility, indicating that the availability of grandparents leads to an increase in maternal employment (Bratti et al., 2018; Compton and Pollak, 2014; Fenoll, 2020; Kanji, 2018) and a shift in the timing of fertility (e.g., Eibich and Siedler, 2020). We contribute to this literature by considering well-being outcomes and subjective health, thereby focusing on other aspects that may be affected by grandparental care. As discussed above and supported by related research, both positive and negative effects may be observed for the different outcomes, highlighting the importance of considering a broad range of outcome variables to get a more comprehensive understanding of the effects of grandparental care.

Second, we add to the literature on the impact of different modes of care on child outcomes, which has previously mostly focused on daycare or parental care (for studies in the German context, see e.g., Barschkett, 2022; Cornelissen et al., 2018; Felfe and Lalive, 2018). The current body of causal evidence on informal care's influence on children is limited and primarily centered around (non-)cognitive skills. Comparing

[6]Caring for grandchildren positively influences grandmother's verbal fluency (e.g., Arpino and Bordone, 2014). However, it also reduces grandparents' participation in social activities (e.g., Arpino and Bordone, 2017) and grandmother's labor supply (e.g., Backhaus and Barslund, 2021; Frimmel et al., 2020). Finally, Danielsbacka et al. (2019) find a decrease in grandparents' reported limitations with activities of daily living but no change in reported overall health, life satisfaction or depressive symptoms. By contrast, Eibich and Zai (2024) demonstrate that grandparental care adversely affects grandparents' physical functioning, subjective health and depressive symptoms, particularly among grandmothers.

children cared for by grandparents with those attending formal child care, Del Boca et al. (2018) find a positive association between grandparental care and children's cognitive skills for children from more advantaged households, while observing a negative association for children from less advantaged households. In a comparison of children cared for by grandparents and those primarily cared for by their parents, Ao et al. (2022) find that children in grandparental care exhibit a greater external locus of control. Additionally, Zhang et al. (2021) report that these children demonstrate lower abilities in walking, talking, counting, and toilet training. Danzer et al. (2020) also show that care provided by mothers or formal institutions is superior to informal care arrangements regarding children's development. Furthermore, the study by Kaufmann et al. (2022) finds an increase in preschool children's test scores when using maternal care instead of grandparental care, alongside a decrease in the test scores of 11- to 12-year-old boys when switching from grandparental care to after-school care. By contrast, evidence on the causal impact on children's health is limited.[7] We contribute to this literature by providing evidence on the causal effects of grandparental care on child health. As discussed above, the empirical evidence on (non-)cognitive skills suggests that this effect could operate in both directions and exhibit heterogeneity across groups.

Third, our study provides novel evidence on these specific outcomes for Germany, a context that offers valuable insights applicable to other countries. While existing literature has primarily focused on the United States or other European countries (for a summary see, e.g., Hank and Buber, 2009), Germany presents a particularly interesting case for several reasons. Firstly, Germany is characterized by a highly subsidized universal daycare system that has expanded significantly over recent decades, mirroring trends in many other OECD countries. Secondly, despite this expansion of publicly funded child care, we demonstrate that approximately a quarter of children are still regularly cared for by their grandparents (Figure 5.1).[8] A comparison of 26 European countries by Zanasi et al. (2023) shows that Germany's share of grandparental care is on par with the average of these countries. Thirdly, despite Germany's relatively low maternal full-time and high part-time employment rates compared to other EU countries (Eurostat, 2023), Germany has seen a trend of defamilization over the last years (e.g., Zagel and Lohmann, 2021). This unique combination of universal daycare, substantial grandparental involvement, and low full-time maternal employment provides a distinct setting for studying the effects of grandparental care.

[7] Sadruddin et al. (2019) provide a comprehensive overview of non-causal evidence on the relationship between grandparental care and child health.

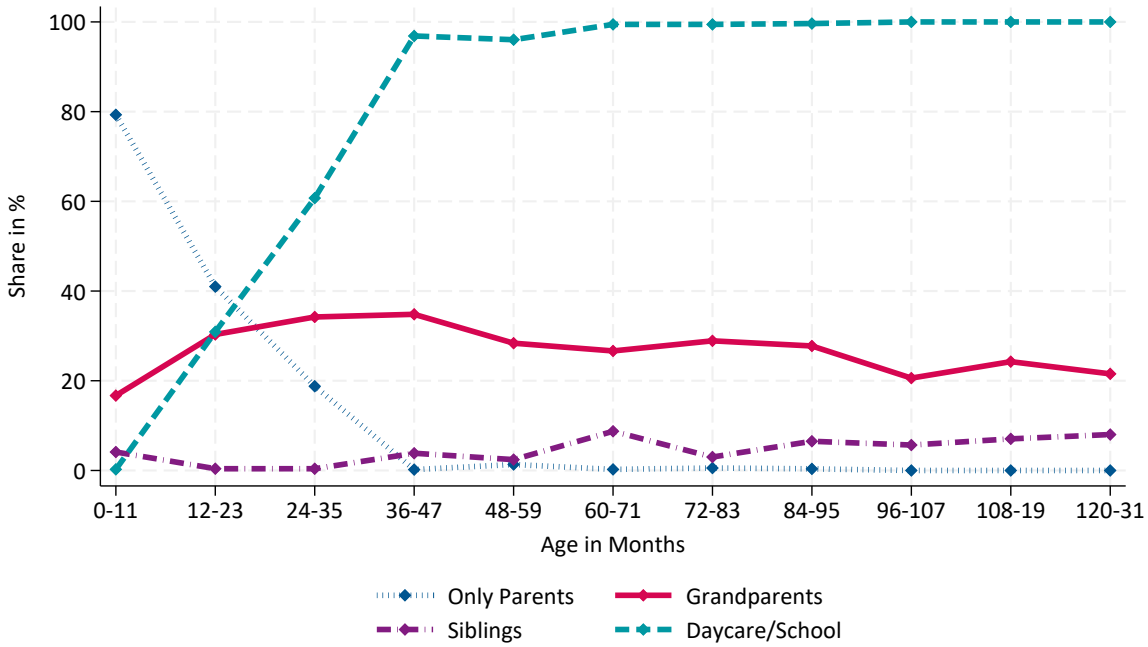
[8] These findings align with Zanasi et al. (2023), demonstrating that over 23 % of grandparents provided weekly child care in Germany in 2020.

The remainder of this paper is structured as follows: In section 5.2 we describe the institutional setting in Germany. Section 5.3 gives an overview of the data used. In section 5.4 we present the empirical strategy. Section 5.5 reports the main findings, discusses the robustness of the results and presents the results of our mechanism analysis. Finally, section 5.6 concludes the paper.

5.2 Institutional Setting

In Germany, regular grandparental care has played a significant role for many years (see Figure 5.A.1). Figure 5.1 demonstrates that in 2018/19, grandparents provided care for approximately 20 to 30 % of children under the age of eleven, across different age groups.

Figure 5.1: Actors and Institutions Involved in Care of Children Younger than 11 in Germany



Notes: A child is counted as cared for by the grandparents in this graph if the child is cared for by its grandparents in the morning or afternoon or both. The same applies for the other actors. The category 'only parents' is exclusive, as it includes only children not cared for by any other caregiver. The categories 'grandparents,' 'siblings,' and 'daycare/school' are not mutually exclusive; a child can belong to one, two, or all three of these categories. *Source:* Pairfam (2018/19), weighted, own calculations.

To understand the role of grandparental care in Germany and its evolution, it is important to consider other forms of child care that are also utilized, as well as trends in

parental employment. Historically, Germany has had low rates of female employment, while full-time employment for men has consistently been prevalent. However, in recent decades, there has been a notable increase in maternal employment in Germany. The percentage of working mothers has risen from 61.2 % in 2006 to 73.8 % in 2021 (e.g., OECD, 2023b).[9] This increase in maternal employment has been facilitated by a substantial expansion of publicly funded daycare since the 1990s (e.g., Müller and Wrohlich, 2020). While enrollment in daycare for children above three years old has become almost universal (92 % in 2022) since 2000 (Statistisches Bundesamt, 2022c), about 70 % of children are in full-time care (Autorengruppe Bildungsberichterstattung, 2020). Attendance rates for children below three years old are significantly lower but have increased from below 5 % in 1990 to approximately 35.5 % in 2022 (Statistisches Bundesamt, 2022c). Daycare fees in Germany are relatively low, and some states have even abolished them (e.g., Huebener et al., 2020; Schmitz et al., 2017). Most daycare centers in Germany are operated by non-profit organizations or municipalities (Spiess, 2008). However, during a child's first year, parents usually care for their child themselves while being on paid parental leave, which can last up to 14 months (see Figure 5.1). Other forms of regular child care that have experienced significant increases in usage in recent years are all-day schools or after-school care programs for elementary school children. The proportion of children up to age 12 in all-day schools or related programs has increased from 9.8 % in 2002/03 to 49.2 % in 2022/23 (BMBF, 2024).

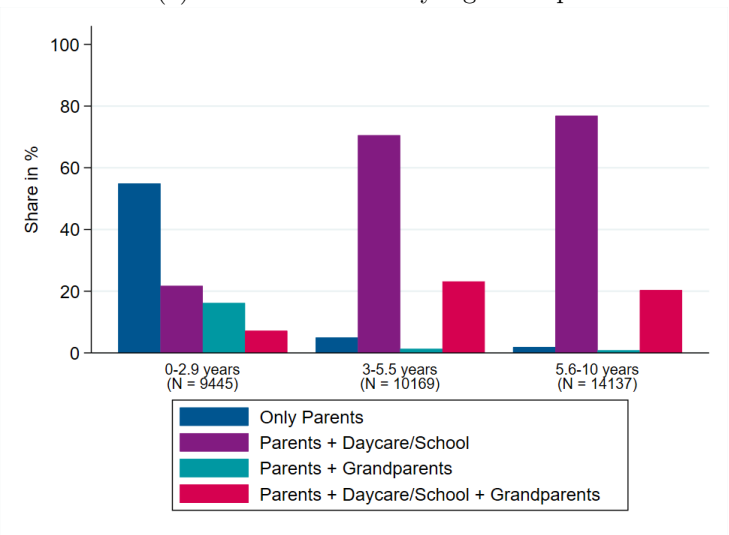
Next to formal care arrangements, grandparents play an important role in the "care puzzle" (see Gambaro et al., 2024). Figure 5.2 shows the distribution of various care modes for different age groups of children pooled from 2009 to 2020. The majority of young children (aged 0 to under 3 years) are primarily cared for by their parents. In the morning, the second most commonly used option is a combination of parental and daycare, which applies to approximately 25 % of children. This is followed by a mix of parental and grandparental care, which accounts for about 15 %. In the afternoon, the combination of parental and grandparental care is the second most frequently chosen option (20 %), while only about 10 % of children receive care from both parents and daycare. Therefore, for this age group, we define exclusive parental care as the alternative to grandparental care.

Older children (aged 3-5.5 years and 5.6-10 years) are predominantly cared for by a combination of parents and daycare/school (70-80 %). However, there are significant differences between morning and afternoon arrangements: in the morning, 90-95 % of

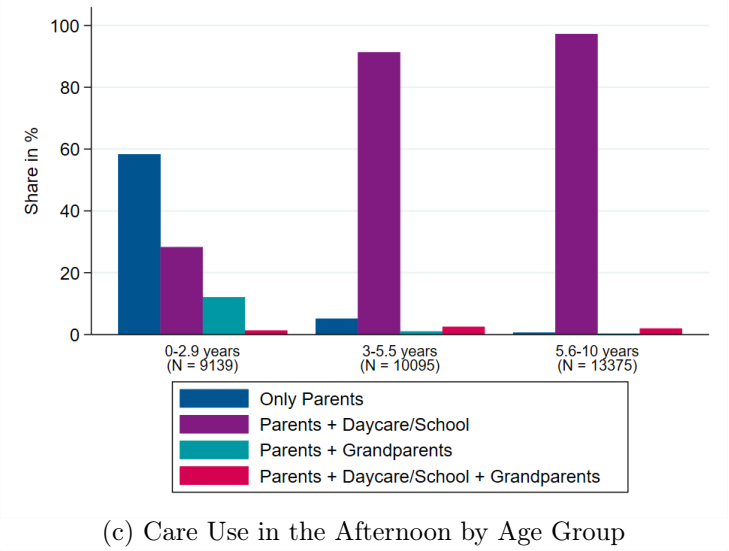
[9]In comparison, the average female employment rate in OECD countries was 72.3 % in 2021 (e.g., OECD, 2023b).

Figure 5.2: Care Patterns

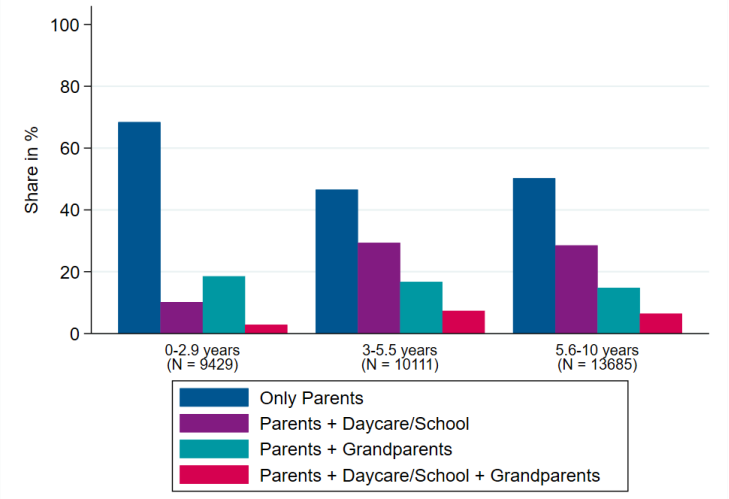
(a) Overall Care Use by Age Group



(b) Care Use in the Morning by Age Group



(c) Care Use in the Afternoon by Age Group



Notes: The figures show the care use by age group. Overall care use takes all actors either caring for the child in the morning or afternoon or both into consideration. *Source:* Pairfam (2009-2020), weighted, own calculations.

children receive care from either daycare or school, whereas in the afternoon, the majority of children are exclusively cared for by their parents (around 50 %). When parents do not provide exclusive care, it is observed that most families opt for a combination of parents and daycare/school (30 %) or a combination of parents and grandparents (almost 20 %). The least common option is a combination of daycare/school and grandparents, which is chosen by less than 10 % of families in the afternoon. Consequently, it can be concluded that the most prevalent alternative to grandparental care for older children is either sole parental care or parental care combined with daycare or a school program in the afternoon.

5.3 Data

For the analysis, the "Panel Analysis of Intimate Relationships and Family Dynamics" (*pairfam*) dataset is utilized (Huinink et al., 2011). The participants in this study are surveyed on an annual basis, which allows us to examine variations both between and within individuals (Huinink et al., 2011). While Pairfam interviews all generations separately, our study relies exclusively on information provided by parents, who report on themselves, their children, and grandparents. Additional details about the dataset can be found in Appendix 5.A.2.

Grandparental Care Variable. The main explanatory variable in our analysis is grandparental care. We have information on grandparental care for each child separately, both in the morning and afternoon. However, our data does not distinguish between grandmothers and grandfathers as caregivers. To conduct our analysis, we create a binary variable that indicates whether a child is regularly cared for by their grandparents in the morning, afternoon, or both, but we do not have information on the number of hours. In order to examine parental well-being[10] and health, we use a binary variable. This variable is set to one if at least one child of the parent in question is cared for by grandparents in the morning, afternoon, or both.[11] The use of grandparental care within families remains relatively stable over time, so most of the variation in this variable comes from comparisons between different families.

Parental Health and Well-being. We analyze various variables related to subjective parental health and satisfaction in our study. The first variable, called health, is an ordinal variable that ranges from 1 (very bad health) to 5 (very good health). This self-assessed, subjective health measure is an aggregate measure of various health

[10] Except for the satisfaction with child care, as this variable is collected at the child level.

[11] This approximation is valid because in 97 % of the households in our sample, either none or all of the children are cared for by the grandparents.

dimensions including physical and mental health. To evaluate well-being, we consider six satisfaction variables, all of which are ordinal variables measured on an 11-point Likert scale ranging from 0 (very dissatisfied) to 10 (very satisfied). The first variable measures general satisfaction with life. Additionally, *pairfam* includes several variables capturing domain-specific satisfaction, such as satisfaction with school, education, or career; satisfaction with leisure activities, hobbies, and interests; satisfaction with the relationship with one's partner; satisfaction with work-life balance; and satisfaction with the child care situation for each child. All these variables have been surveyed annually since 2009, except for satisfaction with work-life balance, which has been included only since 2013.

The table including our main results for parental outcomes (Table 5.1 in Section 5.5) includes the sample averages for all our outcome measures in column 4. In general, mothers and fathers report similar levels of health and satisfaction across most outcomes. Both groups perceive their health as good, with mothers averaging a score of 3.6 and fathers averaging 3.8. Moreover, individuals in our sample exhibit relatively high levels of satisfaction, ranging from 5.9 to 8.4. The sample sizes vary for different outcome variables as they are surveyed in different survey waves, with relationship satisfaction being surveyed only among individuals in a relationship and child care satisfaction measured at the child level. However, using a harmonized sample for outcomes surveyed in the same waves does not affect the results of our analysis.

Child Health. We also analyze the impact of grandparental care on the health of children. To measure this impact, we consider the assessment of children's health by their mother or father. Similar to parental health, this variable is ordinal, ranging from 1 (very bad health) to 5 (very good health). In Table 5.2 (Section 5.5), which reports our main regression results, column 4 displays the sample means for each age group, as well as the pooled mean across all age groups. On average, parents rate their children's health as very good, with a mean of 4.4. Interestingly, the perception of health is very similar across all three age groups.

Measurement of the instrument. We use the distance to grandparents as an instrument to measure grandparental care, as explained in Section 5.4. In the *Pairfam* data, we have information about the geographical distance between the household and all four grandparents (if they are still alive)[12]. The distance is categorized into six categories[13]. Based on this, we construct a binary variable that is equal to one if at least one grandparent lives closer than 30 minutes, and zero otherwise. We use this binary

[12]Only 0.02 % of children under the age of 11 have no living grandparents.

[13]The six categories are: "we live in one house", "less than 10 minutes", "10 minutes to less than 30 minutes", "30 minutes to less than 1 hour", "1 hour to less than 3 hours" and "3 hours or more".

variable because the relationship between distance and the extent of grandparental care provided is unlikely to be linear. For example, the difference between living 10 or 30 minutes away should have a greater impact than the difference between 3 hours and 3 hours and 20 minutes. We chose 30 minutes as the cutoff because it is a reasonable distance that still allows commuting within one day when providing care to a grandchild.[14]

The distribution of the ordinal distance variable used to construct our instrument and the grandparental care variable can be seen in Figure 5.A.2. This figure shows the percentage of children in grandparental care based on the minimum distance between the child and the grandparents. In our sample, approximately 69 % of households live less than 30 minutes away from at least one grandparent, indicating that most children live close to at least one grandparent.[15] Additionally, we observe that the percentage of households using grandparental care increases non-linearly as the distance decreases.[16]

Control Variables. To account for other observable factors that might confound the effect of grandparental care on the health and well-being of both parents and children, our models include a comprehensive set of control variables at various levels: (grand-)parental, child, and household. Specifically, we include socio-economic characteristics of the parents, such as education, age, income, gender, federal state of residence, and migration background. Furthermore, we include detailed information about the household situation, such as the number of children in the household and the age of the youngest child. For a complete list of control variables for each outcome variable, please refer to Table 5.A.1.[17] In order to ensure the robustness of our findings, we conduct additional tests where we vary the set of included control variables, such as excluding potentially endogenous variables like income, and demonstrate that our results remain consistent.

Samples. We conduct analyses at both the child and parent levels. To evaluate the effects on parental health and satisfaction, our analysis is limited to individuals

[14] In robustness checks, we test whether our results are sensitive to two different definitions of the instrument (e.g., using an ordinal instrument and one hour as the cutoff). See Appendix 5.A.5.

[15] Studies based on other data sources corroborate these findings for Germany and other European countries. For example, Isengard (2013), analyzing SHARE data, reports that 62% of adult children reside within 25 kilometers of their parents, which closely aligns with the European average of 68%.

[16] It appears that some (8%) of those households in our sample, who live further than three hours away from all grandparents, still report using grandparental care regularly. We exclude these households in a robustness check, and it does not affect our results. The results are available upon request.

[17] Several control variables in our dataset contain missing values, attributable to either non-response or the inconsistent inclusion of certain questions across survey waves. We impute time-invariant control variables (e.g., migration background) with values from previous waves. To address potential bias from systematic non-response patterns, we employ mean imputation for continuous variables and create an additional category for missing values in discrete and binary variables to include observations with non-responses in a robustness check.

who have at least one child. Each parent constitutes one observation. The analysis of parental satisfaction with child care and child health is conducted at the child level. Additionally, we only include families where at least one parent was born in Germany. If both parents were born outside of Germany, it is highly likely that none of the four grandparents lives in Germany, making regular child care unavailable (e.g., Gambaro et al., 2018). We observe the samples at the parental level and the child level from 2009 to 2020[18].

Our final sample for analyzing parental outcomes consists of 16,056 observations for fathers (corresponding to 4,043 fathers) and 19,844 observations for mothers (corresponding to 4,788 mothers). The sample for analyzing child health includes 44,339 observations, corresponding to 11,714 children. Detailed summary statistics can be found in Table 5.A.2.

5.4 Empirical Strategy

Identifying a causal effect of grandparental care on parental health and satisfaction and child health faces potential endogeneity threats. The choice for grandparental care is endogenous. Thus, it may be influenced by unobserved characteristics that also influence the outcome variables, resulting in an omitted variable bias. One example of such an unobserved variable is a grandparent's preference for taking care of their grandchild. This preference likely affects the amount of support grandparents offer and may directly impact the outcomes we are interested in. Another threat is reverse causality, where parental well-being may influence the amount of support they need from grandparents and consequently the demand for grandparental care. Similarly, the health of children likely influences the decision to seek help from grandparents. For instance, parents with children who have poor health may worry that taking care of such children would be burdensome for grandparents, or they may rely on grandparents because other non-parental care options are not feasible.

Therefore, estimating an ordinary least squares (OLS) specification may yield biased and inconsistent estimates of the effect of grandparental care, failing to capture any causal relationship. Both upward and downward biases in OLS estimators are possible. For example, if only healthy children receive grandparental care, the OLS estimator is expected to be upward biased. Conversely, if parents with low subjective well-being are more likely to seek help from grandparents due to greater need for assistance, the OLS estimator would be downward biased.

[18]For 2020, we only include households surveyed before March 15, prior to the onset of the COVID-19 pandemic in Germany.

To address the endogeneity problem, we employ an instrumental variable (IV) approach. We can use an instrument that determines the endogenous regressor (GPC_{it}), but only affects the dependent variables (y_{it}) through its effect on this independent variable (grandparental care). In this study, we use the distance to the grandparents as an instrument, which has also been utilized by Del Boca et al. (2018) and Compton and Pollak (2014).

In the first stage of our two-stage least squares (2SLS) approach, we regress the grandparental care variable on our instrument and the exogenous control variables:

$$GPC_{it} = \gamma_1 + \gamma_2 D_{it} + X'_{it}\gamma_4 + \varepsilon_{it} \tag{5.1}$$

Here D_{it} equals one if the household lives less than 30 minutes away from at least one grandparent, and 0 otherwise. The variable of interest, grandparental care (GPC_{it}), is a binary variable, and X'_{it} represents our vector of control variables (e.g., including year and state fixed effects), as shown in Table 5.A.1 and described in Section 5.3. The first stage regression is estimated using OLS. 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, denoted as $\widehat{GPC}_{it}$, are included as the main explanatory variable:

$$y_{it} = \beta_1 + \beta_2 \widehat{GPC}_{it} + X'_{it}\beta_3 + \mu_{it} \tag{5.2}$$

In this regression, y_{it} refers to different parental health, satisfaction, and child health outcomes described in Section 5.3.[19] X'_{it} is again our vector of control variables, which remains the same as in the first stage regression. The standard errors μ_{it} are clustered at the household level for the regressions of parental satisfaction with the child care situation and child health.[20]

β_2 is our coefficient of interest, which represents the 2SLS estimator. By definition, it estimates the local average treatment effect (LATE). It measures the effect on the

[19] All outcomes are measured using ordinal variables. Ferrer-i Carbonell and Frijters (2004) demonstrate that life satisfaction can be treated as cardinal, offering the advantage of easier interpretation compared to methods that treat variables as strictly ordinal. Following established practices in the literature, we treat these variables as cardinal in our analysis.

[20] This is because the observations of different children in one household might be correlated with each other, and, as a result, the i.i.d. assumption would not hold. Robust standard errors are used for all other parental outcomes.

compliers, i.e., those families whose utilization of grandparental care is (not) induced by a (large) small distance to the grandparents.

Our sample comprises three types of subjects based on their response to the instrumental variable: i) always-takers, ii) never-takers, and iii) compliers.[21] Observable always-takers are families who utilize grandparental care despite living at a considerable distance from the grandparents. Conversely, observable never-takers are individuals who do not use grandparental care even when living in close proximity to the grandparents. Compliers cannot be identified at the individual level because compliers with a large distance are indistinguishable from never-takers with a large distance, and compliers with a small distance are observably identical to always-takers with a small distance. To address this, we employ the method proposed by Marbach and Hangartner (2020). By subtracting the weighted covariate mean of observable always-takers and never-takers from the covariate mean of the entire sample, we can deduce the covariate mean for compliers.

Table 5.A.3 presents a comparison of covariate means and standard deviations across subject types. Our sample consists of 8% always-takers, 69% never-takers, and 23% compliers. While compliers share similarities with always-takers and never-takers in certain aspects, they differ significantly in others. For instance, complier mothers exhibit comparable rates of employment and high educational attainment to always-takers, whereas never-takers demonstrate substantially lower rates of employment and educational attainment. Additionally, on average complier grandparents are older than always-takers and never-takers, suggesting that – in line with previous research (e.g., Backhaus and Barslund, 2021; Frimmel et al., 2020; Tanskanen et al., 2021) – particularly grandparents who are retired deliver care to their grandchildren. If grandparents in the complier group are, on average, older, this may suggest that they are also in poorer health. Consequently, if worse grandparental health negatively affects parents' well-being, it may indicate that we are underestimating the impact on parental satisfaction. Conversely, this could mean that we are overestimating the negative effect on child health. However, an additional analysis controlling for grandparental health does not provide evidence to support this. Given that the LATE measures the treatment effect specifically for compliers, these socioeconomic disparities among the three groups suggest that extending the results to always-takers or never-takers may not be appropriate.

[21] Theoretically, the population in an IV approach consists of four groups, the fourth being defiers. Defiers are individuals who would only use grandparental care if they lived far away from the grandparents, but this scenario is unlikely in our context. As is common in the literature, we assume that there are no defiers in our sample.

For the distance to grandparents to be considered a valid instrument, it must meet several conditions. Of particular importance are the relevance and exogeneity assumptions of the instrument. Relevance means that the instrument must be sufficiently correlated with the endogenous regressor, grandparental care. Arguably, the distance to the grandparents satisfies the relevance condition as a shorter distance facilitates grandparental care. The correlation between the instrument and the endogenous regressor is shown in Figure 5.A.2 and tested in the first stage regression, where the endogenous variable is regressed on the instruments and the exogenous covariates (Table 5.A.4). The robust first stage F-statistics displayed in the main regression tables in Section 5.5 are at least 67, but in most regressions, they far exceed this value. This supports our argument.[22]

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 distance affects child health only through grandparental care. This relationship is less straightforward for parents, as for example healthier grandparents might be more likely to provide child care and grandparental health might also directly affect parents' health and satisfaction (and potentially even children's health). We provide evidence through several robustness checks that we are likely to isolate the effect of grandparental care on parental health and satisfaction and child health. In Section 5.5.3, we provide a detailed discussion on the validity of this instrumental variable approach, including the plausibility of the exogeneity assumption, as well as the robustness of our results.

5.5 Results

5.5.1 Main Results

First Stage. We begin the discussion on the effects of grandparental care by focusing on the first-stage effects. In all the specifications, the impact of distance on grandparental care is consistently significant and of similar magnitude (Table 5.A.4 in the Appendix). Living within a half-hour distance from at least one grandparent increases the likelihood of receiving grandparental care by approximately 24 percentage points (depending on the sample). This indicates that our instrument is highly relevant, i.e.,

[22]We tested three additional potential instruments using a pension reform in Germany, the parents' birth order, and the gender of the oldest sibling of both parents. All three instruments proved to be weak (small first stage F-statistic).

there is a strong correlation between the instrument (distance) and the endogenous variable (grandparental care).

Parental health and well-being. The effects of grandparental care on parental health and satisfaction are presented in Table 5.1. The findings for mothers are summarized in the upper panel, while those for fathers are presented in the lower panel. Grandparental care has a positive and statistically significant impact on three satisfaction domains of mothers: education and career, leisure, and the child care situation. Specifically, it leads to a 7 % increase in satisfaction with education/career, an 11 % increase in satisfaction with leisure, and a 9 % increase in satisfaction with the child care situation, when compared to the mean.[23] In terms of magnitude, the effect sizes are comparable to the impact of an increase in daycare availability for children three years and older on maternal life satisfaction (Schmitz, 2019). The insignificant coefficients for the other satisfaction and health variables should not be interpreted as null effects since the standard errors are sizable. Therefore, we cannot infer the direction of the effects.

The absence of a significant effect on life satisfaction can be attributed to the predictive ability of specific satisfaction outcomes in different areas for overall life satisfaction. Table 5.A.5 illustrates the correlations between life satisfaction and all the satisfaction domains examined in our study. It is clear that satisfaction with the partner relationship carries the highest explanatory weight, while satisfaction with the child care situation is of least importance. As the coefficient for the relationship and work-life balance satisfaction effect is negative (though not statistically significant), the positive effects in the three other domains are not strong enough to outweigh the importance of relationship satisfaction, ultimately resulting in an insignificant impact on life satisfaction.

A comparison of the IV and OLS estimates reveals that the OLS estimator underestimates the impact of grandparental care on satisfaction with education/career, leisure, and child care. One possible explanation for this is that parents with generally low well-being are more likely to seek assistance and therefore rely more heavily on grandparental care.

Next, we analyze the impact of grandparental care on paternal health and satisfaction, using the same variables. Similar to mothers, grandparental care has a statistically significant and positive effect on fathers' satisfaction with the child care situation. However, this effect is much larger in magnitude. Specifically, there is an increase of

[23]The percent changes are calculated by relating the coefficients to the corresponding sample means, i.e., $\frac{\beta_{IV}+Mean}{Mean}$.

Table 5.1: Effects of Grandparental Care on Parental Health and Well-being

Outcomes	Grandparental Care OLS	Grandparental Care IV	F-Statistic	Sample Mean	Obs.
Mother's Health	0.027	-0.138	456.458	3.627	9,025
	(0.027)	(0.121)			
Mother's Satisfaction with:					
Life	0.018	-0.040	456.712	7.752	9,024
	(0.042)	(0.196)			
Education, Career	0.187***	0.477*	453.623	7.179	8,875
	(0.059)	(0.273)			
Leisure	0.021	0.684**	456.622	6.344	9,024
	(0.061)	(0.274)			
Relationship	0.048	-0.110	437.976	7.596	8,338
	(0.061)	(0.275)			
Work-life balance	-0.274***	-0.071	237.039	6.428	3,293
	(0.099)	(0.368)			
Child care situation	0.069	0.736*	146.479	8.414	11,412
	(0.073)	(0.437)			
Father's Health	0.014	-0.054	281.177	3.796	6,217
	(0.031)	(0.139)			
Father's Satisfaction with:					
Life	0.025	0.162	280.507	7.785	6,215
	(0.047)	(0.216)			
Education, Career	0.067	-0.454*	291.429	7.454	6,208
	(0.057)	(0.264)			
Leisure	-0.041	-0.174	282.508	6.468	6,216
	(0.066)	(0.303)			
Relationship	-0.027	-0.260	281.892	7.695	6,212
	(0.072)	(0.344)			
Work-life balance	-0.084	-0.422	179.398	5.899	3,104
	(0.103)	(0.403)			
Child care situation	0.234***	1.567***	81.138	8.415	7,399
	(0.087)	(0.501)			

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. For the outcome "Child care", robust standard errors clustered at the household level. The outcome variables are all ordinal variables on a scale from 0 (very dissatisfied) to 10 (very satisfied). General: general life satisfaction, Education, career: satisfaction with education and career, Leisure: satisfaction with leisure and hobbies, Relationship: satisfaction with the relationship with the current partner, Work-life balance: satisfaction with the proportion of time that individuals spend on the job or for vocational training or university education relative to the time that individuals spend on personal life, Child care: satisfaction with the child care situation (on child level, all other outcomes on parental level). The regressions include the control variables listed in Table 5.A.1 column (b) for the outcome "Child care" and (c) for all other outcomes. *Source:* Pairfam (2010-2020), weighted, own calculations.

approximately 19 % compared to the average. On the other hand, when grandparents provide child care, fathers' satisfaction with their career and education decreases by 6 % compared to the average. There is no significant impact on the remaining measures of health and well-being. It is worth noting that while there is a strong positive effect on satisfaction with child care, this effect does not translate into overall life satisfaction. Satisfaction with education and career has more explanatory power than satisfaction with child care, as shown in Table 5.A.5.

Child Health. Table 5.2 presents the effects on children's health, specifically focusing on general health across three distinct age groups. It is important to remember that the alternative to grandparental care differs depending on the age group. For children under the age of three, the alternative is typically sole parental care. However, this changes for older children. For them, the alternative is either half-day daycare or school in combination with sole parental care in the afternoon, or all-day daycare and school combined with parental care.

The reported coefficients indicate that grandparental care has a negative effect on the health of children below the age of 11. Specifically, we find that grandparental care leads to an 8 % increase in children's health problems compared to the sample mean. This effect appears to be primarily driven by children of elementary school age, as the coefficient for this subgroup estimation is similarly significant and of similar magnitude to the coefficient for all children combined. However, for children in other age groups, the coefficient is not statistically significant. The decline in health is sizable, when considering that daycare attendance – on average a more intensive care mode – leads to similarly sized declines in health (Baker et al., 2008; **?**).

Table 5.2 also allows for a comparison between the OLS and IV estimates. We note that the OLS estimate (column 1) consistently underestimates the impact of grandparental care on health across all age groups. The OLS estimates suggest that there is no effect on the health of children who receive grandparental care. This finding supports our hypothesis that parents of children with poor health are less likely to seek assistance from grandparents.

Table 5.2: Results: Child Health

	Grandparental Care				
	OLS	IV	F-Statistic	Sample Mean	Obs.
Child health					
0-2.9 years	0.026	-0.404	66.900	4.450	1,904
	(0.047)	(0.255)			
3-5.5 years	0.030	-0.180	111.015	4.414	3,257
	(0.036)	(0.166)			
5.6-10 years	-0.025	-0.385***	170.219	4.406	7,093
	(0.032)	(0.135)			
0-10 years	0.000	-0.343***	199.120	4.406	12,254
	(0.026)	(0.127)			

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the household level in parentheses. The general health variable is an ordinal variable on a scale from 1 (bad health) to 5 (good health). The regressions include the control variables listed in Table 5.A.1 column (a). *Source:* Pairfam (2010-2020), weighted, own calculations.

5.5.2 Mechanisms

In the following section we provide suggestive evidence for different potential mechanisms which might drive the effects of grandparental care on family health and well-being. To this end, we conduct various subsample analyses to investigate treatment heterogeneity and examine the relationship between the use of formal and grandparental care.

First, in Table 5.A.6, we examine the relationship between attending (all-day) daycare or school and receiving care from grandparents.[24] This analysis shows that children who are cared for by their grandparents are less likely to attend all-day daycare or school. This negative relationship aligns with Figure 5.2, suggesting that in the afternoon, children are typically cared for by only two caregivers: either parents and grandparents or parents and daycare/school. Additionally, there is a shortage of (all-day) daycare and all-day school slots (Gambaro et al., 2024), indicating that grandparents fill the gap in formal care in the afternoon for children three years and older and in

[24]Similar to Figure 5.1, the different care settings are not mutually exclusive; for example, a child may attend all-day daycare or school while also receiving care from their grandparents.

the morning for children below the age of three. If children who receive care from their grandparents in the afternoon are less likely to attend afternoon programs at school, the negative effect of grandparental care on the health of elementary school-aged children may be attributed to the different activities organized by grandparents compared to schools. While all-day schools include homework supervision, sports, arts, music, and playtime in their afternoon curriculum (e.g., Ministry of Education of North Rhine Westphalia, 2024), less is known about the activities carried out by grandparents and their grandchildren. If grandparents focus less on sports (and other outdoor) activities than schools do, this might explain why grandparental care has negative effects on child health. Additionally, around 10% of children aged three and older are cared for by parents, grandparents, and daycare/school in the afternoon. Bratsch-Hines et al. (2015) demonstrate that social competencies of children are lower when they experience greater instability of caregivers across child care settings. Consequently, if children interact with multiple caregivers in different care environments within a single afternoon, this may be too stressful for some, potentially leading to poorer (mental) health outcomes.

To further discuss possible mechanisms, we now proceed with subsample analyses based on child age, parental education, gender of the child, and grandparental age, providing suggestive evidence for different mechanisms. As we only interpret the estimates in the main specification that are statistically significant, we conduct subsample analyses only for these outcomes. We begin by analyzing child health within two specific subgroups. As established in the literature (e.g., Conti et al., 2016), there are known differences in child outcomes based on the child's gender. Therefore, we estimate separate models for boys and girls. Our findings indicate that the negative health effects are primarily associated with boys, as the coefficient for boys is larger in magnitude and statistically more significant (see Table 5.A.9). Next, we divide the sample based on the median grandparental age, which is approximately 64 years. Table 5.A.9 demonstrates that health issues resulting from grandparental care are most noticeable among children who are cared for by grandparents below the median age. This might be due to the fact that grandparents below the median age are more likely to be actively employed, whereas older grandparents are more likely to be retired. As retirement frees up the time resources of grandparents and facilitates the provision of child care (e.g., Tanskanen et al., 2021), retired grandparents may have more time for outdoor activities, cooking healthy meals, and experience less stress, all of which could lead to a higher quality of care.

Additionally, we estimate the effects of grandparental care on parents for different child age groups. The results are presented in Tables 5.A.7 and 5.A.8. Grandparental

care has a positive effect on mothers' satisfaction with education and career, particularly when they have very young children. Moreover, the effect on satisfaction with leisure is mostly observed among mothers with children of elementary school age (5.5 to 10 years). This finding could possibly be explained by the limited availability of all-day school slots and the opportunity for mothers to schedule their leisure time in the afternoon with the assistance of grandparental care – as also seen in Table 5.A.6. The effect on maternal satisfaction with child care is mostly driven by mothers with children below school age, while for fathers, the estimates for satisfaction with the child care situation are at least significant on the 5% significance level across all age groups and especially large in magnitude for fathers with children below the age of 3. By contrast, we cannot conclude which child age group drives the results for satisfaction with education and career.

Differentiating by parental educational degrees reveals that the positive effect on mothers' satisfaction with education/career and child care is more pronounced for mothers with a university degree compared to those without (Table 5.A.7). This may be because highly educated mothers tend to work longer hours and consequently face more challenges in balancing child care and work responsibilities without support from grandparents. Hence, it appears that grandparents are more likely to provide support to highly educated mothers in reconciling child care and work commitments. This finding is in line with other research which shows that the availability of grandparental care leads to an increase in mother's labor supply (Bratti et al., 2018; Compton and Pollak, 2014; Fenoll, 2020; Kanji, 2018). As for fathers, the situation is similar: the positive effect on satisfaction with child care is mainly observed among fathers who have a university degree (Table 5.A.8).

5.5.3 Validity of the Instrument and Robustness

In this section, we provide several robustness checks to further corroborate our findings and test the validity of the instrument. The results of the robustness checks are depicted in Table 5.A.11 for mothers, Table 5.A.12 for fathers, and Table 5.A.13 for children and show that our findings are generally stable across specifications, although sometimes less statistically significant. Effects on mothers' satisfaction with leisure and child care, fathers' satisfaction with child care, and children's health turn out to be robust across specifications, while the effects on parents' satisfaction with education and career are not as robust.

First, it can be argued that demand for child care increases the likelihood of families living closer to their grandparents (e.g., Chen and Zhang, 2018). To test this hypoth-

esis, we investigate whether the distance between parents and grandparents decreases around the time of a child's birth, indicating that either parents moved closer to the grandparents or grandparents moved closer to the parents. The reason for a systematic moving behavior could be to facilitate grandparental child care, which would make distance an endogenous variable. However, our investigation of moving behavior in the year before and after the birth of the firstborn or any child shows no systematic movement towards the grandparents (see Table 5.A.10). We further restrict the sample to households that did not move during the observation period. This ensures that we exclude any households that may have moved closer to the grandparents specifically to facilitate child care. The coefficients in our analysis remain stable and significant, with the exception of satisfaction with education and career.

As households living close to the grandparents and those living further away seem to differ in some of their characteristics (see Table 5.A.14), we combine our IV estimation with entropy balancing (Hainmueller, 2012), a matching strategy that balances controls more effectively than propensity score methods. First, we conduct this matching step and then we run our regular IV estimations. The main idea of entropy balancing is to assign a weight to observations in the "control group" (families living further than 30 minutes away), causing the "control group's" distributions of the selected covariates to match those of the "treatment group" (families living closer than 30 minutes) on the mean. Consequently, our set of covariates has the same means in both groups. These weights are then applied to our IV estimations. The effects remain very similar; we still depict highly significant effects on children's health, parental satisfaction with child care, and maternal satisfaction with leisure. However, the negative effect on paternal satisfaction with education and career is no longer statistically significant.

We provide additional evidence that our specification isolates the impact of grandparental care on parental well-being by utilizing a sample of childless households. Specifically, we directly regress our outcomes on the distance to the "grandparents"[25] which should not exhibit an effect if grandparental care is the only important channel. The results reveal that the point estimates are negligible in magnitude and that there are no statistically significant effects of distance on well-being for both childless women and men.[26]

[25]Referring to the parents or parents-in-law of childless adults, who represent our grandparent generation.

[26]Because individuals in childless households are generally younger than parents in households with children in *pairfam*, we exclude the youngest quartile of the sample in additional regressions to ensure comparability between the childless sample and our main sample. Even in these analyses, we still do not find any effects of distance on well-being.

Next, we only consider the distance to an individual's parents-in-law (rather than the distance to any grandparent) as an instrument when estimating the effects of grandparental care on parental outcomes. The rationale behind this is that the relationship extends beyond child care and is typically closer to one's own parents than to one's parents-in-law (e.g., Del Boca et al., 2018). Therefore, if the distance to one's own parents has any effect on parental satisfaction through factors other than child care that we cannot account for, this should be eliminated when using the distance to the parents-in-law. In general, the results are similar to our main findings, but they are less statistically significant due to substantially smaller sample sizes. The effects on parental satisfaction with their career are smaller and no longer statistically significant.

Although we have demonstrated that grandparental care appears to be the primary channel through which distance affects child and parental outcomes, we further test the robustness of our results by relaxing the exclusion restriction. Following the approach of Conley et al. (2012), we provide bounds on the second-stage effect of grandparental care on parental and child outcomes, allowing for a degree of endogeneity in the instrument. We begin by estimating the reduced form effect of the instrument (Tables 5.A.11, 5.A.12, and 5.A.13, "Relaxing exclusion restriction" column 2). Subsequently, we calculate the bounds for the second-stage effects (Tables 5.A.11, 5.A.12, and 5.A.13, "Relaxing exclusion restriction" columns 3 and 4), permitting the direct effect of the instrument on our outcomes (γ) to range from zero (perfectly exogenous) up to 30% of the reduced form effect.[27] The bounds for the second-stage estimate exclude zero as long as the direct effect of the instrument (γ_{max}) is less than 30% of the reduced form effect. However, child health is only robust to allowing the direct effect to be at most 21% of the reduced form effect, while parental satisfaction with education and career is less robust to endogeneity. We conclude that most of of our effects remain robust even under a substantial degree of instrument endogeneity.

To account for the fact that the length (in years) of exposure to grandparental care may affect the results, we construct a grandparental care variable that averages past (over the past three years) and current grandparental care.[28] The findings closely align with our baseline estimates for most outcomes.

Additional robustness checks, such as employing a placebo outcome, excluding households living further than 3 hours away but using regular grandparental care, using different definitions of the instrument, and including or excluding additional control

[27] We implement this using Stata's *plausexog* command, employing 30% of the reduced form effect as γ_{max} (Clarke and Matta, 2018).

[28] In this regression, we use the average distance between grandparents and the family over the past three years as the instrument.

variables or imputing missing values, further support the robustness of our results. Correcting for multiple hypothesis testing yields a p-value of 0.077 for maternal satisfaction with leisure while parental satisfaction with education and career is no longer significant. This demonstrates that our results for maternal satisfaction with leisure hold up even when accounting for the number of hypotheses tested.[29] Overall, the results regarding parental satisfaction with child care, maternal satisfaction with leisure, and child health are the most robust, while the results regarding parental satisfaction with career should be interpreted with caution.

5.6 Conclusion

Our analysis contributes to the existing literature on the determinants of health in the family context and the intergenerational effects of regular grandparental care on the outcomes of parents and children. Our results are particularly interesting because grandparental care continues to play an important role in the "care puzzle" for many families, despite efforts to expand formal care options. Furthermore, we extend the literature on the effects of grandparental care by estimating the causal effects on family health and well-being, as measured by the subjective health and well-being of mothers, fathers, and children. To overcome endogeneity between grandparental care and our outcomes, we employ an instrumental variable approach, using the distance to the grandparents as our instrument, which we combine with entropy balancing. We also provide various robustness checks to support the validity of our instrument.

Using a representative German panel data set, our results provide evidence of mainly positive effects on various aspects of parental satisfaction, but negative effects on children's subjective health. Specifically, we show that grandparental care leads to improvements in parental satisfaction with child care and maternal satisfaction with leisure. Therefore, grandparenting is beneficial for the parent's generation, particularly for mothers. This is plausible because, on average, mothers are still the primary caretakers and thus benefit the most. Through this effect, it may also have long-term benefits for the child's development, as maternal well-being has been found to positively impact child outcomes (see Datta Gupta et al., 2023). In the short term, however, grandparental care seems to have adverse effects on children's health.

[29] To implement the correction for multiple hypothesis testing, we apply the Romano Wolf procedure (Romano and Wolf, 2005) using the Stata command *rwolf*. As *rwolf* can only be conducted within one data set, we ran the test only for parental satisfaction outcomes (excluding satisfaction with child care as it is part of another subsample of the data set), separately for mothers and fathers.

The positive effects of grandparental care on parents' satisfaction with child care and mothers' satisfaction with leisure are highly robust, regardless of the various specifications, sample restrictions, and instruments used. However, the effects observed for parental satisfaction with their education and career are less stable and should therefore be interpreted with caution. When comparing our effects to the impact of daycare attendance on maternal life satisfaction, as illustrated for instance by Schmitz (2019), it becomes evident that our effects, ranging from 9 to 11 %, are of similar magnitude.

Additionally, we provide evidence that grandparental care has a negative impact on the health of elementary school children (8 %). Studies on the health effects of other forms of care, such as daycare, yield mixed results. For instance, in their study, Cornelissen et al. (2018) identify positive health effects of daycare that are of larger magnitude compared to our findings. Specifically, they report a 25 % reduction in the need for "compensatory sports" upon entering school. On the other hand, Baker et al. (2008) uncover adverse health effects resulting from a major expansion of daycare in Quebec, Canada, amounting to 9 % compared to the average. Similarly, **?** demonstrates that daycare attendance results in short-term increases in infectious disease prevalence, followed by comparable long-term decreases, with effects similar in magnitude (5-6 %) to those observed for grandparental care. Since this is the first piece of causal evidence concerning the impact of grandparental care on overall child health, it is not possible to make direct comparisons with other estimates on this care mode.

Overall, our results indicate that in addition to parental care and daycare, regular child care provided by informal caregivers such as grandparents also has an impact on parents, children, and the family as a whole. In addition to our analysis, to fully understand the underlying mechanisms behind these effects, a more detailed analysis is needed. To investigate further, for instance, data that includes the activities grandparents engage in with their grandchildren would be necessary (Sadruddin et al., 2019). Additionally, like other forms of care, more information on the quality of the time spent in child care is required (Milovanska-Farrington, 2021). Lastly, it is important to examine the long-term effects to determine whether the positive effects on maternal satisfaction improve child outcomes and other maternal factors, thereby demonstrating additional indirect effects of grandparental care. The method employed in this paper is an instrumental variable approach. Since this method only reveals effects for compliers, and we demonstrate that in our setting compliers differ from always-takers and never-takers in specific characteristics, caution is necessary when generalizing the results to other populations. A key advantage of studying the research question within the German context is that the development of formal child care mirrors trends in many

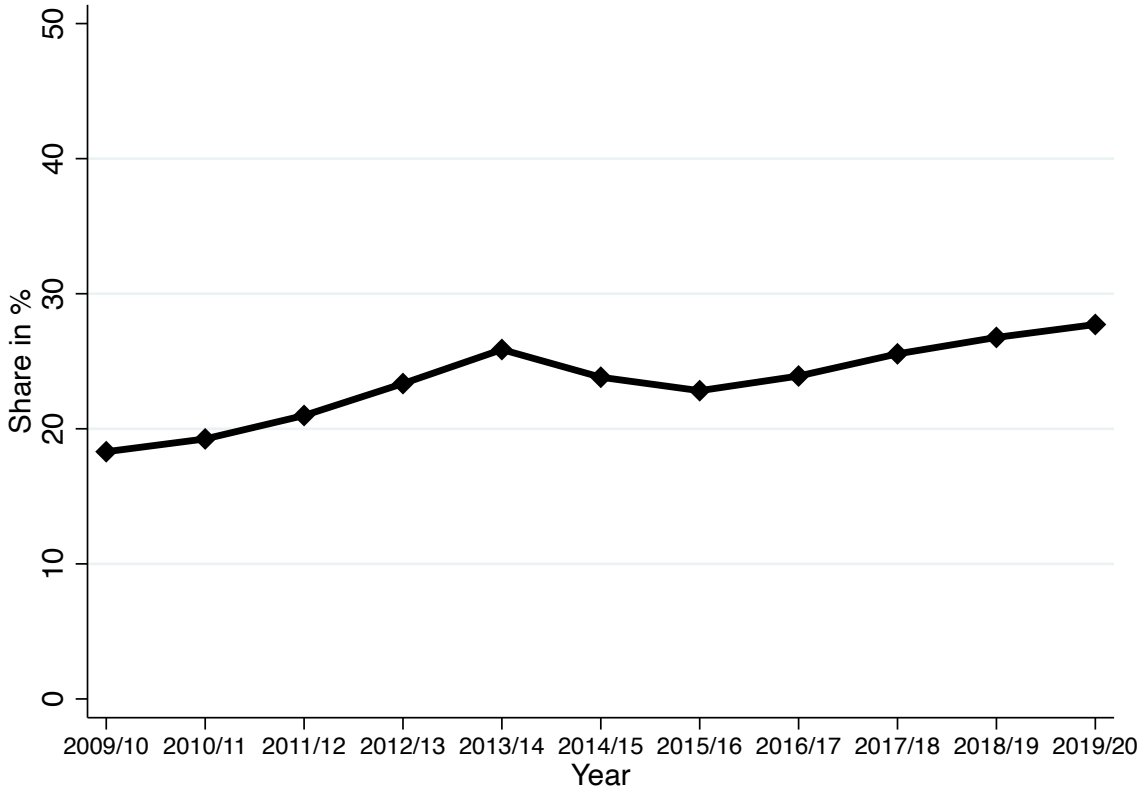
other countries, while the use of informal child care aligns with the European average. This similarity facilitates the extension of our findings to other European countries.

Our paper provides evidence that family health and well-being can be affected by interfamilial interactions. Hence, policymakers should not only focus on daycare, but also on informal care when aiming to improve societal health. Considering the positive effects of grandparental care on parental well-being, discussions could be held on implementing national insurance credits for grandparents who care for dependent children, which would contribute to their retirement income, similar to what has been done in the UK. Additionally, introducing grandparental leave and benefits, as seen in Sweden (Olsen, 2024), could be measures to support grandparental care. However, considering the mixed findings regarding the effects of grandparental care on grandparents' health and well-being (Danielsbacka et al., 2019; Eibich and Zai, 2024), it is crucial to also account for the grandparents' perspective before implementing policies that promote grandparental care. Considering the negative effects of grandparental care on some children's health, our results might also suggest that too many care modes in one day could have negative effects on children. To address this, politicians could consider policies that support longer daycare hours, shorter working days, or other measures to reduce the "child penalty" that employed parents may face when the opening hours of daycare centers do not align with their working schedules (e.g., Jessen, 2022).

5.A Appendix

5.A.1 Graph on Grandparental Care

Figure 5.A.1: Development of Grandparental Care (2009-2020)



Notes: The graph shows the development of grandparental care for children below the age of 6. A child is counted as cared for by the grandparents if the child is cared for by its grandparents in the morning or afternoon or both. *Source:* Pairfam (2009-2020), weighted, own calculations.

5.A.2 Further Information on the Data

Pairfam. *Pairfam* respondents are equally distributed among the birth cohorts 1991–1993, 1981–1983, and 1971–1973 and the first wave of the sample consisted of 12,400 respondents (Huinink et al., 2011). These individuals are called "anchor persons." Approximately one half of the anchors are male, and the other half are female. In addition, if anchors and anchors' partners agreed, partners were surveyed from the first wave onwards. The response rate for partners lies at about 52 %.[30] *Pairfam* is a multi-actor

[30] Analyses show that anchors whose partners participate and anchors whose partners do not participate do not differ systematically in most of their socio-economic characteristics. Thus, the partner sample can be considered as good as random.

survey. In addition to anchors and partners, children (aged 8 to 15 years) and parents of anchors are surveyed separately. Furthermore, anchors and partners are questioned about their children (biological, adopted, foster, and stepchildren of anchors living in one household) and parents in their own questionnaires in detail (Huinink et al., 2011). This detailed information on three generations makes *pairfam* particularly suitable for our analysis. Since the child survey only includes children above the age of 7 and the parent survey suffers from a low response rate, we focus on the information obtained from the anchor and partner questionnaires in our analysis.

Summary Statistics. Table 5.A.2 includes summary statistics of selected control variables (based on the sample on child level), showing mean and standard deviation across all observations. It can be seen that 23 % of children in the sample are cared for by the grandparents on a regular basis[31] and almost 70 % of families live close to at least one grandparent.

The sample is, on average, highly educated, as in almost half the families at least one partner holds a university degree. Generally, *pairfam* includes a slightly more highly educated sample than the German population (Wetzel et al., 2021). In terms of migration background, 12 % of children have at least one parent who was born outside Germany. Half the children in the sample are girls and children are on average 5 years old. Furthermore, 91 % of parents are cohabiting.

[31] In the *pairfam* wave 12, parents of school children are only questioned about care arrangements in the afternoon. Thus, we defined school children in wave 12 to be cared for by grandparents only if they are cared for by them in the afternoon. This means that there is a very small share of children that are cared for by the grandparents in the morning before school that are counted as not in grandparental care if they are not also in grandparental care in the afternoon. Figure 5.2 shows that this is only a very small share of school children.

Table 5.A.1: Control Variables

Variable	Definition	Type	To estimate effects on Children's (a)	Parents' (b)	Parents' (c)
Parental Variables					
Age	Mother's age	Cont	✓		
	Individual age	Cont		✓	✓
Post-secondary education	Highest degree in household, 1-3	Ord	✓		
	Individual education, 3 levels	Ord		✓	✓
Religion	One parent religious	Bin	✓		
	Individual religion, 1-7	Cat		✓	✓
Migration background	One parent has direct background	Bin		✓	
	Individual has direct background	Bin		✓	✓
Partner information	Partner answered questionnaire	Bin	✓		
Parental goals	Importance nutrition and exercise, 1-10	Ord	✓		
Pregnancy	Mother is pregnant	Bin	✓	✓	✓
Cohabitation	Parents live together	Bin	✓	✓	✓
Widowhood	One parent is widowed	Bin	✓	✓	
	Individual is widowed	Bin			✓
Only child	At least one parent is only child	Bin	✓		
	Individual is only child	Bin		✓	✓
Child Variables					
Sex	Child's sex	Bin	✓	✓	
	Children in HH: male, female, mixed	Cat			✓
Child age	In months	Cont	✓	✓	
	Age of youngest child in months	Cont			✓
Number children in HH	Total	Cont	✓	✓	
	Nr. children 0-2 years	Cont			✓
	Nr. children 3-5 year	Cont			✓
	Nr. children 6-10 year	Cont			✓
	Nr. other children	Cont			✓
Birth order	Age in comparison to sibling's age	Ord	✓	✓	
Daycare use	Child (0-5 years) in daycare	Bin	✓	✓	
	Number of children (0-5 years) in daycare	Cont			✓

Continued on the next page

Table 5.A.1 continued

Variable	Definition	Type	To estimate effects on Children's (a)	Parents' (b)	(c)
Grandparent Variables					
School education	Anchor's mother, 1-3	Ord	✓	✓	✓
	Anchor's father, 1-3	Ord	✓	✓	✓
Age	Mean of all living grandparents	Cont	✓	✓	✓
Household (HH) Variables					
Household income	logarithmic, in 1000€	Cont	✓	✓	✓
Year	number according to wave number	Cat	✓	✓	✓
Federal state	1-16	Cat	✓	✓	✓
Community size	1-7	Ord	✓	✓	✓

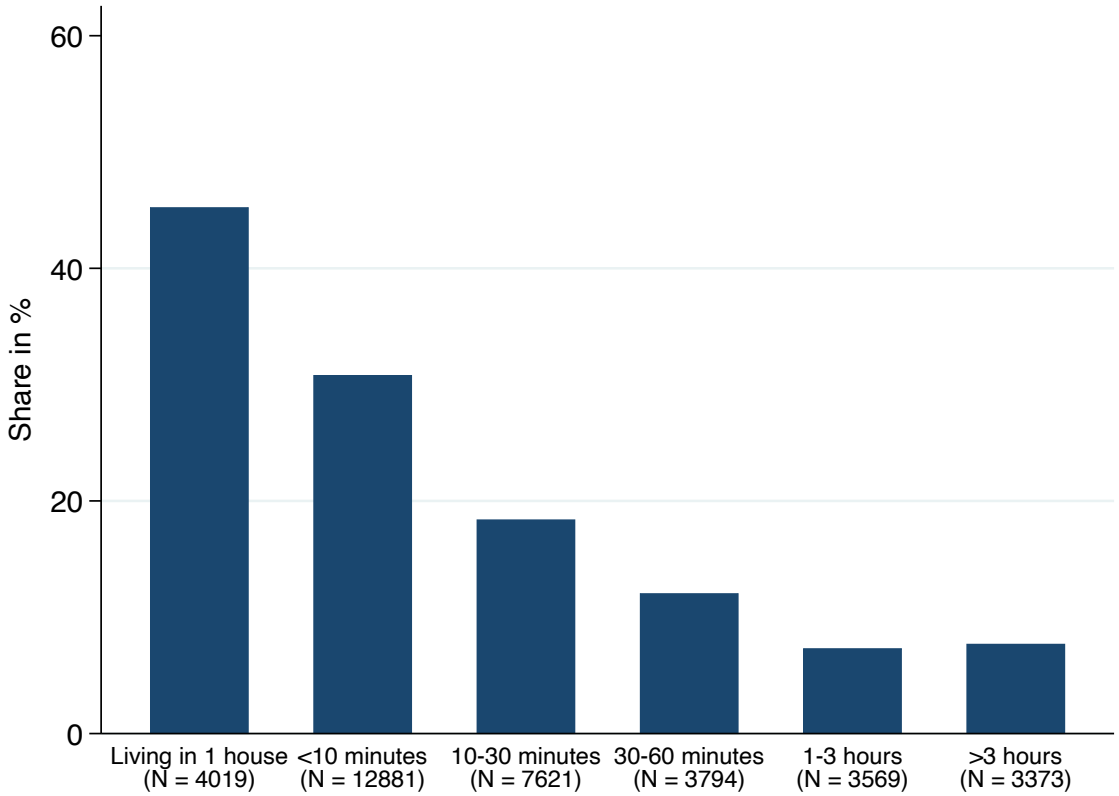
Notes: This table shows which variables are used to estimate the effect of grandparental care on: (a) Child's health (b) Parental satisfaction with child care (c) Other parental satisfaction outcomes. Types: Bin (binary), Cat (categorical), Cont (continuous), Ord (Ordinal). *Source*: Pairfam, 2009-2019.

Table 5.A.2: Summary Statistics

	Percentage/Mean (SD)
Grandparent care	23.32 %
Grandparents live 30 min or closer	68.85 %
Mother's labour force status (in %)	
Mother not working	36.33 %
Mother working part-time	42.67 %
Mother working full-time	18.88 %
Household's highest parental school degree (in %)	
No/ lower secondary degree	5.92 %
Upper secondary/vocational degree	45.51 %
University degree	48.57 %
One parent has migration background	11.90 %
Household net income (in Euro)	3416.56 (2430.79)
Age mother (in years)	34.02 (7.90)
Sex child: male	50.88 %
Number of children in household	2.04 (0.99)
Age child (in years)	4.90 (3.10)
Cohabitation with partner	91.07 %
Observations	29,177

Note: Conditional on non-missing sample. *Source:* Pairfam 2010-2020, weighted, own calculations.

Figure 5.A.2: Grandparental Care by Distance



Notes: The figures show the share of children cared for by grandparents by the distance between the child's household and the closest living grandparent. A child is counted as cared for by the grandparents in this graph if the child is cared for by its grandparents in the morning or afternoon or both. *Source:* Pairfam (2009-2020), weighted, own calculations.

Table 5.A.3: Profiling Compliers

Variable	Always-taker	Never-taker	Complier
Proportion	0.081	0.694	0.225
Mother not working	0.220 (0.015)	0.404 (0.004)	0.241 (0.010)
Mother working part-time	0.447 (0.018)	0.392 (0.004)	0.481 (0.010)
Mother working full-time	0.281 (0.015)	0.187 (0.003)	0.273 (0.009)
No/ lower secondary degree	0.040 (0.007)	0.050 (0.002)	0.016 (0.004)
Upper secondary/vocational degree	0.312 (0.017)	0.509 (0.004)	0.225 (0.011)
University degree	0.647 (0.017)	0.442 (0.004)	0.760 (0.011)
One parent has migration background (share)	0.094 (0.010)	0.071 (0.002)	0.124 (0.006)
Household net income (in Euro)	3723.667 (82.227)	3275.983 (18.304)	3912.180 (49.352)
Age mother (in years)	33.201 (0.375)	33.982 (0.055)	36.277 (0.172)
Number of children in household	1.767 (0.031)	2.151 (0.008)	2.075 (0.020)
Age child (in years)	4.691 (0.103)	4.971 (0.027)	5.073 (0.065)
Cohabitation with partner (share)	0.831 (0.014)	0.911 (0.002)	0.931 (0.007)
Grandparents' age	63.637 (0.298)	62.470 (0.063)	65.238 (0.173)

Note: Conditional on non-missing sample, except grandparental health. Means and standard deviations of the different groups are produced using Stata's command *ivdesc*. SDs in parentheses. *Source:* Pairfam 2010-2020, weighted, own calculations.

5.A.3 First Stage Results

Table 5.A.4: First Stage Results

	Sample:		
	Child	Mother	Father
Distance	0.241***	0.239***	0.238***
	(0.017)	(0.013)	(0.016)
Observations	12,254	6,196	4,489
F-statistic	198.640	484.553	365.483
Adjusted R-squared	0.106	0.151	0.154

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The outcome variable in these regressions is grandparental care and the variable of interest is our instrument, the minimum distance between the family and the grandparents. The different columns show the first stage regression for three different samples: children, mothers, fathers. Robust standard errors in parentheses. Conditional on no missings in the outcome and control variables (see Table 5.A.1). *Source:* Pairfam (2010-2020), weighted, own calculations.

5.A.4 Additional Analyses

Table 5.A.5: Domain-specific and Life Satisfaction

Satisfaction with	Mothers	Fathers
Education, Career	0.140***	0.190***
	(0.015)	(0.015)
Leisure	0.148***	0.107***
	(0.014)	(0.014)
Relationship	0.274***	0.240***
	(0.015)	(0.015)
Work-life balance	0.075***	0.090***
	(0.011)	(0.011)
Child care situation	0.070***	0.064***
	(0.016)	(0.014)
Obs.	5,628	5,947

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows results of regressions of mother's and father' life satisfaction on variables capturing domain specific satisfaction. Robust standard errors in parentheses. *Source:* Pairfam (2010-2020), weighted, own calculations.

Table 5.A.6: Relationship Between Formal and Informal Care Usage

	OLS	Obs.
0-2 years in daycare	-0.075**	2,884
	(0.022)	
3-5.5 years in all-day daycare	-0.100***	4,201
	(0.021)	
5.5-10 years in all-day school	-0.068**	6,826
	(0.020)	
3-10 years in all-day daycare/school	-0.075**	12,439
	(0.022)	

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. OLS regression with daycare/ all-day care or school as dependent variable and grandparental care as independent variable. The regressions include the control variables listed in Table 5.A.1 column (a). Robust standard errors in parentheses. *Source:* Pairfam (2010-2020), weighted, own calculations.

Table 5.A.7: Subsample Analysis: Mothers' Satisfaction

	Grandparental Care	F-Statistic	Sample Mean	Obs.
Satisfaction with education, career				
Children's age				
0-2.9 years	1.108** (0.509)	133.319	7.130	3,059
3-5.5 years	0.223 (0.421)	166.761	7.227	3,442
5.6-10 years	0.644* (0.375)	258.128	7.156	4,676
Mother's education				
University degree	0.540* (0.286)	258.383	7.657	3,233
No university degree	0.542 (0.480)	186.995	6.947	5,642
Satisfaction with leisure				
Children's age				
0-2.9 years	0.760 (0.514)	137.629	6.087	3,161
3-5.5 years	0.326 (0.430)	167.983	6.181	3,503
5.6-10 years	1.051*** (0.388)	256.239	6.374	4,738
Mother's education				
University degree	0.653** (0.323)	260.417	6.378	3,295
No university degree	0.791* (0.448)	188.761	6.314	5,729
Satisfaction with child care				
Children's age				
0-2.9 years	1.002 (0.969)	45.329	8.550	2,015
3-5.5 years	1.055* (0.635)	71.036	8.436	3,071
5.6-10 years	0.561 (0.439)	140.636	8.347	6,326
Mother's education				
University degree	1.061* (0.544)	71.454	8.445	5,995
No university degree	0.810 (0.832)	66.700	8.372	5,417

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. For the outcome satisfaction with "Child care", robust standard errors clustered at the household level. The outcome variables are all ordinal variables on a scale from 0 (very dissatisfied) to 10 (very satisfied). Leisure: satisfaction with leisure and hobbies, Child care: satisfaction with the child care situation (on child level, all other outcomes on parental level). The regressions include the control variables listed in Table 5.A.1 column (b) for the outcome satisfaction with "Child care" and (c) for all other outcomes. *Source:* Pairfam (2010-2020), weighted, own calculations.

Table 5.A.8: Subsample Analysis: Fathers' Satisfaction

	Grandparental Care	F-Statistic	Sample Mean	Obs.
Satisfaction with education, career				
Children's age				
0-2.9 years	0.317 (0.350)	162.288	7.473	2,545
3-5.5 years	-0.353 (0.292)	239.321	7.435	2,550
5.6-10 years	0.308 (0.332)	201.099	7.372	3,059
Father's education				
University degree	0.086 (0.202)	369.099	7.734	2,858
No university degree	-0.360 (0.452)	120.324	7.174	3,327
Satisfaction with child care				
Children's age				
0-2.9 years	2.073** (0.809)	43.094	8.654	1,426
3-5.5 years	1.152** (0.465)	82.938	8.371	2,071
5.6-10 years	1.651*** (0.504)	65.136	8.327	3,902
Father's education				
University degree	1.490*** (0.451)	77.731	8.406	4,273
No university degree	1.370 (0.904)	30.941	8.394	3,126

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. For the outcome "Child care", robust standard errors clustered at the household level. The outcome variables are all ordinal variables on a scale from 0 (very dissatisfied) to 10 (very satisfied). Education/career: Satisfaction with education and career, Child care: satisfaction with the child care situation (on child level, all other outcomes on parental level). The regressions include the control variables listed in Table 5.A.1 column (b) for the outcome "Child care" and (c) for all other outcomes. *Source:* Pairfam (2010-2020), weighted, own calculations.

Table 5.A.9: Subsample Analysis: Child Health

	Grandparental Care	F-Statistic	Sample Mean	Obs.
Child's gender				
Boys	-0.428** (0.180)	126.285	1.622	6,614
Girls	-0.231 (0.143)	145.452	1.540	6,385
Grandparents' health				
GP age above median	-0.305** (0.147)	138.292	1.550	7,257
GP age below/equal median	-0.459** (0.204)	84.372	1.622	5,742

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. GP = Grandparent. Robust standard errors clustered at the household level in parentheses. The general health variable is an ordinal variable on a scale from 1 (bad health) to 5 (good health). The regressions include the control variables listed in Table 5.A.1 column (a). *Source:* Pairfam (2010-2020), weighted, own calculations.

5.A.5 Robustness Checks

Table 5.A.10: (Grand-)parental Moving Behavior Before and After the Birth of a Child

In the year before child birth	General movement	Move towards	Move away from
Considering any grandparents	0.0037	0.0032	-0.0098
	(0.019)	(0.017)	(0.016)
Observations	22,251	22,251	22,251
Considering mother's parents	0.0182	0.0193	-0.0034
	(0.017)	(0.013)	(0.012)
Observations	22,250	22,250	22,250
Considering father's parents	-0.0126	-0.0162	0.0004
	(0.016)	(0.013)	(0.012)
Observations	20,904	20,904	20,904
In the year after child birth	General movement	Move towards	Move away from
Considering any grandparents	0.0033	0.0154	-0.0038
	(0.015)	(0.013)	(0.013)
Observations	22,251	22,251	22,251
Considering mother's parents	0.0220	0.0099	0.0114
	(0.013)	(0.011)	(0.010)
Observations	22,250	22,250	22,250
Considering father's parents	-0.0104	0.0057	-0.0136
	(0.013)	(0.011)	(0.010)
Observations	20,904	20,904	20,904

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Estimated using OLS. Standard errors in parentheses. All regressions include individual and household controls described in Table 5.A.1 column (a). The sample size differs to that in the main specification as the analysis includes also parents before child birth. *Source:* Pairfam (2009-2020), own calculations.

Table 5.A.11: Robustness: Mothers' Satisfaction

	Grandparental Care	F-Statistic	Sample Mean	Obs.
Satisfaction with education, career				
Main estimate	0.477* (0.273)	453.623	7.179	8,875
Exclusion of movers	0.308 (0.277)	473.168	7.255	6,894
Entropy balancing	0.743** (0.343)	348.973	7.089	8,875
Childless households	0.145 (0.131)		7.337	1,132
Distance to parents-in-law	0.149 (0.372)	193.423	7.272	4,730
Average current and past GPC	0.550* (0.305)	633.553	7.199	5,545
Exclusion of HH far away using GPC	0.477* (0.273)	453.623	7.179	8,875
Discrete instrument	0.234 (0.227)	652.290	7.179	8,875
Binary instrument ($<$ 1h vs $\geq$ 1h)	0.079 (0.323)	376.465	7.179	8,875
Including emot. closeness	0.315 (0.279)	433.025	7.177	8,866
Including freq. contact	0.228 (0.329)	307.670	7.179	8,875
Including pre-birth satisfaction	1.300** (0.525)	98.061	7.087	2,348
Including labor force status	0.371 (0.271)	440.043	7.181	8,829
Excluding income	0.559* (0.288)	370.413	7.209	8,241
Controls with replaced missings	0.509** (0.202)	952.475	7.047	17,872
	Reduced form effect	$\beta_{lowerbound}$	$\beta_{upperbound}$	γ_{max}
Relaxing exclusion restriction	0.115* (0.066)	-0.212	1.044	0.035
Satisfaction with leisure				
Main estimate	0.684** (0.274)	456.622	6.344	9,024
Exclusion of movers	0.417 (0.286)	468.028	6.363	7,006
Entropy balancing	0.869** (0.345)	351.303	6.281	9,024
Childless households	0.087 (0.151)		7.062	1,134
Distance to parents-in-law	0.818** (0.392)	189.783	6.461	4,805
Average current and past GPC	0.402 (0.313)	640.455	6.312	5,654
Exclusion of HH far away using GPC	0.684** (0.274)	456.622	6.324	9,024
Discrete instrument	0.536** (0.226)	661.348	6.335	9,024
Binary instrument ($<$ 1h vs $\geq$ 1h)	0.544* (0.323)	378.558	6.335	9,024
Including emot. closeness	0.538* (0.280)	436.855	6.335	9,015
Including freq. contact	0.610* (0.335)	311.422	6.335	9,024
Including pre-birth satisfaction	0.606 (0.555)	96.607	6.237	2,415
Including labor force status	0.661** (0.277)	445.549	6.338	8,978
Excluding income	0.964*** (0.295)	374.334	6.358	8,405
Controls with replaced missings	0.524** (0.205)	957.031	6.328	18,120
	Reduced form effect	$\beta_{lowerbound}$	$\beta_{upperbound}$	γ_{max}
Relaxing exclusion restriction	0.187*** (0.067)	0.004	1.373	0.056

Continued on the next page

Table 5.A.11 continued

	Grandparental Care	F-Statistic	Sample Mean	Obs.
Satisfaction with child care				
Main estimate	0.736* (0.437)	146.479	8.414	11,412
Exclusion of movers	0.969* (0.538)	104.940	8.467	8,414
Entropy balancing	1.322** (0.590)	142.183	8.322	11,412
Distance to parents-in-law	0.756 (0.684)	38.427	8.468	5,886
Average current and past GPC	0.735 (0.530)	119.062	8.446	6,678
Exclusion of HH far away using GPC	0.736* (0.437)	146.479	8.407	11,412
Discrete instrument	0.725** (0.365)	202.672	8.407	11,412
Binary instrument ($<$ 1h vs $\geq$ 1h)	0.805 (0.534)	129.650	8.407	11,412
Including emot. closeness	0.604 (0.436)	139.005	8.406	11,407
Including freq. contact	0.638 (0.536)	97.592	8.407	11,412
Including labor force status	0.747* (0.445)	145.824	8.453	10,058
Excluding income	0.941** (0.448)	130.162	8.426	12,318
Controls with replaced missings	1.014** (0.417)	143.409	8.359	13,131
	Reduced form effect	$\beta_{lowerbound}$	$\beta_{upperbound}$	γ_{max}
Relaxing exclusion restriction	0.155*** (0.053)	0.024	1.229	0.046

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. For the outcome "Child care", robust standard errors clustered at the household level. The outcome variables are all ordinal variables on a scale from 0 (very dissatisfied) to 10 (very satisfied). Child care: satisfaction with the child care situation (on child level, all other outcomes on parental level), Leisure: satisfaction with leisure and hobbies. The regressions include the control variables listed in table 5.A.1 column (b) for the outcome "Child care" and (c) for all other outcomes. *Source:* Pairfam (2010-2020), weighted, own calculations.

Table 5.A.12: Robustness: Fathers' Satisfaction

	Grandparental Care	F-Statistic	Sample Mean	Obs.
Satisfaction with education and career				
Main estimate	-0.454* (0.264)	291.429	7.454	6,208
Exclusion of movers	-0.499** (0.250)	351.779	7.460	5,064
Entropy balancing	-0.356 (0.357)	218.880	7.425	6,185
Childless households	-0.147 (0.212)		7.715	938
Distance to parents-in-law	0.131 (0.256)	256.907	7.411	4,404
Average current and past GPC	-0.528* (0.281)	438.966	7.525	4,033
Exclusion of HH far away using GPC	-0.462* (0.264)	290.426	7.421	6,185
Discrete instrument	-0.047 (0.217)	435.389	7.421	6,185
Binary instrument ($<$ 1h vs $\geq$ 1h)	-0.723*** (0.271)	323.697	7.421	6,185
Including emot. closeness	-0.648** (0.277)	264.468	7.421	6,183
Including freq. contact	-0.951*** (0.347)	181.016	7.421	6,185
Including pre-birth satisfaction	-0.638* (0.377)	95.704	7.646	2,079
Including labor force status	-0.512* (0.263)	283.768	7.422	6,168
Excluding income	-0.540** (0.267)	309.134	7.413	6,520
Controls with replaced missings	0.152 (0.224)	643.003	7.259	14,760
	Reduced form effect	$\beta_{lowerbound}$	$\beta_{upperbound}$	γ_{max}
Relaxing exclusion restriction	-0.134** (0.063)	-1.089	0.121	-0.040
Satisfaction with child care				
Main estimate	1.567*** (0.501)	81.138	8.415	7,399
Exclusion of movers	1.595*** (0.526)	82.004	8.438	5,702
Entropy balancing (binary instrument)	2.012*** (0.495)	84.412	8.269	7,399
Distance to parents-in-law	1.898*** (0.551)	53.900	8.417	5,328
Average current and past GPC	1.342** (0.554)	89.261	8.407	4,441
Exclusion of HH far away using GPC	1.567*** (0.501)	81.138	8.401	7,399
Discrete instrument	1.576*** (0.415)	103.139	8.401	7,399
Binary instrument ($<$ 1h vs $\geq$ 1h)	1.719*** (0.587)	73.219	8.401	7,399
Including emot. closeness	1.411*** (0.520)	73.587	8.401	7,399
Including freq. contact	1.460** (0.644)	52.007	8.401	7,399
Including labor force status	1.480*** (0.481)	84.420	8.473	6,720
Excluding income	1.575*** (0.496)	81.147	8.394	7,781
Controls with replaced missings	1.513*** (0.457)	119.702	8.301	13,756
	Reduced form effect	$\beta_{lowerbound}$	$\beta_{upperbound}$	γ_{max}
Relaxing exclusion restriction	0.334*** (0.058)	0.561	2.112	0.100

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses. For the outcome "Child care", robust standard errors clustered at the household level. The outcome variables are all ordinal variables on a scale from 0 (very dissatisfied) to 10 (very satisfied). Child care: satisfaction with the child care situation (on child level, all other outcomes on parental level), Leisure: satisfaction with leisure and hobbies. The regressions include the control variables listed in table 5.A.1 column (b) for the outcome "Child care" and (c) for all other outcomes. *Source:* Pairfam (2010-2020), weighted, own calculations.

Table 5.A.13: Robustness: Child Health

IV: Grandparental Care		F-Statistic	Sample Mean	Obs.
Main estimate	-0.343*** (0.127)	199.120	4.406	12,254
Exclusion of movers	-0.236* (0.139)	198.456	4.431	9,594
Entropy balancing (binary instrument)	-0.279** (0.121)	278.104	4.418	12,999
Average current and past GPC	-0.293** (0.138)	201.649	4.441	8,949
Exclusion of HH far away using GPC	-0.322** (0.128)	210.135	4.418	12,999
Discrete instrument ($<$ 30min vs $\geq$ 30 min)	-0.221** (0.105)	265.742	4.418	12,999
Binary instrument ($<$ 1h vs $\geq$ 1h)	-0.245 (0.153)	178.608	4.418	12,999
Including emot. closeness	-0.369*** (0.132)	195.532	4.418	12,996
Including freq. contact	-0.469*** (0.162)	140.999	4.418	12,999
Including labor force status	-0.310** (0.128)	209.655	4.429	11,701
Excluding income	-0.320** (0.130)	194.716	4.417	13,866
Controls with replaced missings	-0.141 (0.098)	310.305	4.417	28,397
	Reduced form effect	$\beta_{lowerbound}$	$\beta_{upperbound}$	γ_{max}
Relaxing exclusion restriction	-0.074** (0.030)	-0.566	0.026	-0.022
Placebo: Birth weight	156.638 (250.410)	111.613	3,395.2	7,842
Placebo: Low birth weight	-0.037 (0.096)	111.613	0.054	7,842

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the household level in parentheses. The general health variable is an ordinal variable on a scale from 1 (bad health) to 5 (good health). The regressions include the control variables listed in Table 5.A.1 column (a). *Source:* Pairfam (2010-2020), weighted, own calculations.

Table 5.A.14: Balancing Table

	Distance
No / lower school degree	-0.0458 (0.0381)
University degree	-0.161*** (0.0272)
Migration background	-0.104*** (0.0385)
log(income) in 1000€	-0.0395* (0.0237)
Age	-0.0058* (0.0033)
Children's sex	0.0057 (0.0138)
Nr. children 0-2	0.0681** (0.0269)
Nr. children 3-5.5	0.0688*** (0.0260)
Nr. children 5.5-10	0.0191 (0.0169)
Nr. other children	0.0008 (0.0170)
Grandparent's age	0.0012 (0.0023)
Pregnant	0.0307 (0.0235)
Cohabitation with partner	0.0338 (0.0298)
Widowed	0.140 (0.0862)
Mother is single child	0.0526* (0.0279)
No school degree (grandm.)	0.0366 (0.0589)
Upper school degree (grandm.)	-0.0458 (0.0337)
No school degree (grandf.)	-0.0570 (0.0718)
Upper school degree (grandf.)	-0.0642** (0.0327)
Children <6 in Kita	-0.0441** (0.0174)
Age youngest child	0.0005 (0.0004)
Observations	9,259

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Estimated using OLS based on the sample used in the regressions for maternal satisfaction. Robust standard errors clustered at the household level in parentheses. Regression includes individual and household controls described in Table 5.A.1 column (c). Source: Pairfam (2009-2020), own calculations.

CHAPTER 6

Conclusion

This dissertation investigates how social policies and within-family investments shape the development of children's human capital. It concentrates on two central dimensions of human capital: health and educational attainment. The analyses consider both direct effects on children and indirect effects transmitted through parental beliefs, behaviors, and well-being. Taking a multi-generational perspective, it highlights that policies and family investments affect not only individuals' outcomes but also those of their parents, children and siblings, with effects frequently extending beyond the targeted individuals. The findings demonstrate that family investments and public interventions shape the distribution of resources within households and highlight the role of policies in supporting disadvantaged families and reducing inequalities.

6.1 Discussion, Limitations and Future Research

While each empirical chapter individually discusses the internal and external validity of the empirical approach and avenues for future research, the following subchapter discusses the most important points and summarizes the main findings of each chapter.

Chapter 2 examines parents' misperceptions of their children's skills and the role of school-provided information in shaping parental beliefs and investments. The analysis reveals that report cards, on average, do little to correct widespread misperceptions. Differentiations between various types of report cards shows that numerical grades and scales facilitate belief updating, while verbal progress reports are largely ineffective. Additionally, numerical grades and scales are associated with increased engagement in reading and playing. The findings also emphasize the importance of timing, as parents respond most strongly to the first report cards.

The quasi-experimental design employed in this chapter allows the estimated effects to be interpreted as close to causal. Nonetheless, several limitations warrant caution. First, most parents surveyed after the distribution of report cards were interviewed during school holidays, whereas those surveyed beforehand were interviewed during the school year. Differences in available time for parental investments may therefore confound the estimates. Even though robustness tests demonstrate that increased investments are primarily driven by parents receiving negative information rather than by all parents, some concerns remain. Additionally, although survey periods are generally determined by the NEPS structure, some parents may selectively enter the post-period due to difficulties in being contacted. While the results are not driven by parents who required repeated contact, some residual selection cannot be ruled out.

Different limitations in the approach and the dataset also present opportunities for future research. First, because all parents in this setting eventually receive report cards over the summer, the chapter only studies parents' immediate responses rather than long-term effects on beliefs and behaviors. Future research on the long-term causal effects of different forms of information provision would be a valuable extension. Second, the set of investment outcomes available in the data is limited. The NEPS does not include measures specific to parents' assistance with math and science, which restricts the analysis to general investment measures and reading as an indicator of investment in German. Additionally, the outcomes are restricted to educational activities within the home. A more comprehensive assessment would also capture parental involvement at school, such as participation in school events. Including such measures could also shed light on potential general equilibrium effects, as greater parental engagement at school might increase teachers' workloads. Third, as children are surveyed and tested before report cards are distributed, impacts on the children themselves cannot be assessed. School reports serve multiple purposes, making it important to consider their effects on both parents and children. Prior evidence shows that more more precise grading scales in Swedish high schools discouraged students and reduced attainment (Collins and Lundstedt, 2024), while shifting from numerical to verbal assessments in Germany appears to have no impact on long-term educational attainment (Penny, 2019). Future work should examine short-term effects on children including motivation, aspirations, and other behavioral responses. This would allow to link these outcomes with parental responses and provide a more comprehensive understanding of the channels connecting school communication to child development in the crucial early period of formal schooling.

The generalizability of the findings is shaped by the German school system in which the usage of numerical grades versus descriptive report cards in the first years of elemen-

tary school depends on the federal state. Similarly, several other European countries introduce numerical grades only in later years, using written reports or no report cards in early grades. The results should therefore be applicable to contexts with delayed numerical grading but less relevant for countries that provide no descriptive reports. Future research could investigate whether the results hold in a different country with a different school system which also uses descriptive progress reports in the first years of elementary school.

Chapter 3 provides first causal evidence on the economic impact of public advance child support payments, a policy instrument widely implemented across (European) countries. The findings show that increased public child support improved the financial situations of single-mothers and even increased private child support contributions. Since eligibility for public payments is tied to the economic independence of single-parent households in Germany, the increase in advance child support payments led to a decrease in the probability of receiving unemployment benefits. This appears to be driven by exits from welfare due to increases in labor supply at the intensive rather than the extensive margin. Households spent the additional income on food, beverages, and goods closely tied to child development and well-being.

Although Chapter 3 demonstrates that substantial concerns regarding the internal validity of the results can be mitigated, the analysis nevertheless has several limitations that leave scope for future research. First, since the extension of payment duration and the expansion to older children were implemented simultaneously, these two effects cannot be disentangled, even though such a differentiation would be of considerable interest to policymakers. Second, the effects are estimated for single-parent families with children above age 11. Results may differ when policies target younger children or lower-income single parents, such as those in Belgium or Spain (Hakovirta et al., 2022). Both issues could be addressed in future evaluations of reforms to advance child support payments in other countries where systems operate differently or target different groups. Third, a comprehensive policy evaluation should assess the impacts on children's outcomes, as mothers' employment and job stability are linked to less harsh parenting and fewer behavioral problems in children (Dunifon et al., 2003; Johnson et al., 2012). Therefore, an increase in mother's working hours and a reduction in welfare dependency could have significant implications for children. Fourth, regular public child support payments provide income stability to single-parent households. Additional evaluations should focus on other parental outcomes, such as stress, since family stress plays a central role in transmitting the effects of poverty and single parenthood to children (Heintz-Martin et al., 2022). Fifth, while the results show that higher public child support payments also raise private payments, the analysis does

not help in explaining fathers' motives. Given the low recovery rates of public payments from non-resident parents in Germany and elsewhere (BMFSFJ, 2023), future research should investigate fathers' responses. This will likely require the collection of additional data, as the sample sizes of non-resident fathers in existing datasets are too small.

The evaluation in Chapter 3 offers insights relevant and applicable to a wide range of countries. Advanced child support payments are common across Europe, and although institutional designs differ, the main conclusions should be generalizable. In Anglo-American countries such systems are less common, yet the context is comparable in the sense that few single mothers receive full payments from fathers (Beblo et al., 2025; Hakovirta and Mesiäislehto, 2022; Pilarz and Cuesta, 2025). The results show that fathers' contributions respond more strongly to state-led collection than to collection by mothers themselves, a finding with important implications for countries like the UK or US that currently emphasize enforcement over advance payments.

Chapter 4 investigates the spillover effects of Germany's birthright citizenship reform on the older siblings of children who received citizenship at birth. The analysis shows that birthright citizenship increases both the targeted children's and their older siblings' probability of completing the academic secondary school track. These spillovers appear to be driven partly by greater parental naturalization and, even more so, by naturalization among older siblings. I also provide evidence that parents increase investments in older siblings while maintaining support for the focal child.

The analysis faces several limitations regarding internal and external validity. First, the families in the sample immigrated at least eight years before the child's birth and remained in Germany through the end of secondary school, indicating a strong intention to stay and integrate. The results may therefore not be generalizable to more recent migrants with no intention to stay. Future evaluations of the most recent reform of Germany's citizenship law – which reduced the residency requirement to five years – could shed light on whether families with shorter residence histories respond differently to citizenship access. Second, an increased fertility in birth cohorts from 2001 onward as a response to the reform prevents me from studying impacts on younger siblings. However, for understanding parental investment responses, it would be important to compare effects on older versus younger siblings, as birth order and sibling relationships may matter. Finally, future research should additionally assess labor market impacts as the targeted children and their siblings enter employment, determining whether these reforms can effectively address the demand for highly skilled labor.

In terms of external validity, the findings should be relevant beyond Germany, particularly for the 31% of countries which grant birthright citizenship to children born in the country regardless of their parents' citizenship (Gathmann and Garbers, 2023), but also for countries considering the introduction. Given that immigrant students achieve lower scores than their native peers in most OECD countries (OECD, 2023c), the results should be applicable to many contexts. Even though no such disparities by immigrant status exist in the US, the findings are especially relevant there: older siblings who immigrate to the US after birth face much greater hurdles to citizenship than their siblings born in the country. However, my analysis shows that they can still benefit even without acquiring citizenship themselves.

Finally, **Chapter 5** examines the impact of grandparental child care on parents and children, finding positive effects on maternal satisfaction with child care and leisure and on fathers' child care satisfaction. There are no effects on parental health, life satisfaction, or other domains. However, children's health deteriorates, possibly due to differences between structured school or daycare programs and grandparental care. The method employed in this chapter is an instrumental variable (IV) approach. As discussed in Chapter 5, this instrument faces several threats to its exogeneity assumption which is a limitation with respect to the internal validity. However, as shown in the bounding test, even with substantial degree of endogeneity of the instrument, there still are significant impacts on families, increasing the credibility of the IV approach. It needs to be noted, though, that the bounding test also shows that effect sizes could be different from the ones estimated in case there is endogeneity. Additionally, the employed method only reveals effects for compliers, and complier parents are on average older and more educated than other population groups in this setting. This means that the findings should be interpreted as applicable primarily to comparable subgroups rather than generalized to the entire population.

Through heterogeneity analyses the chapter provides suggestive evidence for the mechanisms driving the effects on family well-being. To properly disentangle the mechanisms, future research covering activities grandparents engage in with their grandchildren, as well as the quality of the time spent in informal child care, would provide an important extension. These investigations would likely require the collection of additional data (see Milovanska-Farrington, 2021; Sadruddin et al., 2019). Additionally, the estimated effects of grandparental care are only short-term effects. However, formal child care has, for example, long-term impacts on children (Havnes and Mogstad, 2015). Therefore, future research should examine the long-term effects of grandparental care to determine whether the positive effects on maternal satisfaction improve child and other maternal outcomes in the long run – as, for example, the importance of maternal

well-being for child development is shown in previous research (e.g., Berger and Spieß, 2011).

Studying the research question within the German context means that the external validity of the findings is limited to countries with similar institutional contexts. The development of formal child care mirrors trends in other European countries and the prevalence of grandparental care in Germany equals the average prevalence in Europe. This facilitates the extension of the findings to other – especially Western – European countries. However, it is not possible to extend the conclusions to all countries, also due to differences in culture and norms.

6.2 Policy Implications and General Conclusions

This subchapter discusses some joint policy conclusions that address the overarching research questions outlined in Chapter 1, based on evidence from the four empirical chapters. In summary, these are connected to (i) within-family investments, (ii) children's human capital development, (iii) impacts of policies on family members of targeted individuals, (iv) disadvantaged families, and (v) the importance of policy design.

A first key insight concerns **within-family investments**, with a particular focus on parental investments. The dissertation examines how parents allocate time and financial resources and how these allocations respond to different policy interventions. By doing so, it sheds light on ways to strengthen parental investments in children's human capital and to reduce inequalities in such investments.

Chapter 2 analyzes the impact of teacher-provided information on parental time investments. The results show that parental engagement can increase even without average changes in beliefs, suggesting that school feedback serves as a reminder of the importance of investing in education. Clear numerical grades stimulate additional reading and playtime, whereas descriptive progress reports have no measurable effect. This finding is consistent with evidence by Chang et al. (2024), who show that individuals prefer quantitative over qualitative information. Timing also matters: the strongest responses occur when parents receive their child's first report card, while subsequent reports have weaker effects. These results show that schools can influence parental investments through an existing practice – report cards – provided they are designed effectively.

Chapter 3 evaluates the impact of public advance child support payments on resident parents' consumption patterns and non-resident parents' child support contri-

butions. The analysis demonstrates that public advance child support increases non-resident fathers' private child support payments. This likely reflects greater awareness and stronger enforcement threats due to the transfer of legal child support claims from mothers to the state. The fact that non-resident fathers respond more strongly to collection by the state than by mothers suggests that this form of investment is less voluntary. On the resident parent side, higher child support payments and resulting increases in labor supply translate into greater household spending on food, beverages, and goods closely tied to child development and well-being. These can be seen as a form of parental monetary investment. Consistent with previous research (e.g., Moullin and Harkness, 2021; Nieuwenhuis and Maldonado, 2018; OECD, 2024b), the findings demonstrate that many single mothers struggle to cover basic expenses. The evaluation indicates that as soon as the very low income of these mothers increases they allocate a portion of it toward their children's needs. The findings highlight differences in investment behavior between resident and non-resident parents, as well as between mothers and fathers, reflecting underlying social norms, institutional incentives, and financial constraints.

In **Chapter 4**, I study how parents allocate time and money across siblings when access to citizenship is unequal within the household. I find that parents increase investments in older siblings while maintaining support for the focal child with citizenship. These patterns suggest that parents recognize the advantages of citizenship and strategically adjust their educational investments, effectively treating them as substitutes for institutional endowments (Heckman and Mosso, 2014). This behavior aligns with theoretical considerations by Becker and Tomes (1976) and empirical evidence by Berry et al. (2020) that parents aim to equalize opportunities across children. Increases in investments in older siblings occur primarily in the form of time – more frequent support and school event participation – whereas monetary investments remain unchanged. By contrast, in one-child families where the child receives citizenship, both time investments through school-related conversations and monetary investments in educational resources rise. This suggests a particularly targeted allocation towards older siblings, as the observed time inputs are concentrated on the individual child, whereas monetary investments like a computer could benefit all children in the household.

As a whole, the dissertation highlights the central role of parents as key investors in their children's human capital. Policy measures that provide parents with comprehensible information, ease financial constraints, or alter institutional opportunities can meaningfully shape how families allocate time and money. The evidence also reveals that parental investments are not fixed: they respond to external signals and resources, and parents actively balance investments both across domains and between

children. This underscores the importance of designing policies that not only support parental capacity to invest but also take into account the dynamic and strategic nature of parental decision-making.

Chapter 2 investigates the allocation of grandparents' time in the form of grandparent-provided child care. It demonstrates mainly positive effects on various aspects of parental satisfaction, but negative effects on children's subjective health. Through its positive impact on maternal well-being, it may also have long-term benefits for the child's development, as maternal well-being has been found to positively impact child outcomes (see Datta Gupta et al., 2023). In the short term, however, grandparental care seems to have adverse effects on children's health. The results show that family health and well-being can be affected by intra-familial interactions. Therefore, policymakers should not only focus on formal educational and care settings but also on within-family investments when aiming to improve societal well-being.

The thematic area for which the dissertation provides conclusions are the direct impacts on **children's human capital**. As one aspect of children's human capital, **Chapter 4** focuses on educational attainment and demonstrates that birthright citizenship has positive long-term effects on both targeted children's and their older siblings' education: both groups are significantly more likely to graduate from high school on the academic track. The degree obtained on this track qualifies children in Germany for tertiary education and increases their chances of securing higher-paying occupations (Nordin et al., 2020). This indicates that the educational benefits of birthright citizenship which have previously been estimated (e.g., Cygan-Rehm, 2018; Felfe et al., 2020; Gathmann et al., 2021; Sajons and Clots-Figueras, 2014) persist in the long run. Remarkably, these effects even extend to older siblings, demonstrating that educational spillovers between siblings can occur not only from older to younger siblings (e.g., Bennhoff et al., 2024; Dahl et al., 2024; Figlio et al., 2023; Goodman et al., 2015; Karbownik and Oezek, 2023; Landerso et al., 2020) but also from younger to older siblings. **Chapter 5** focuses on children's health, demonstrating that grandparental care adversely affects it. The results also suggest that having too many caregiving arrangements in one day could adversely affect children. To address this issue, policymakers could consider measures that support longer daycare hours, shorter working days, or other initiatives aimed at reducing the "child penalty" that employed parents face when daycare center hours do not align with their work schedules (e.g., Jessen, 2022). These results emphasize the importance of policies that account for both institutional structures and family contexts in promoting children's long-term human capital.

A third set of conclusions concerns the fact that most policies generate benefits and costs that **extend beyond the directly targeted individuals**. This is particularly relevant in the family context, where intra-familial transmission plays a central role. **Chapter 2** examines school report cards, which are primarily designed for children, and demonstrates that their format influences parents' beliefs and behaviors. **Chapter 3** evaluates public child support payments aimed at reducing child poverty and demonstrates that parents respond as well. Resident mothers increase their working hours and consumption of goods closely tied to child development, while non-resident fathers raise their child support contributions. **Chapter 4** investigates the effects of children's access to citizenship, showing that it not only benefits the children themselves but also improves educational outcomes for older siblings, increases parental and sibling naturalization, and shifts parental investments in children's human capital. **Chapter 5** shows that grandparent-provided child care not only affects the children receiving care but also improves parental well-being. Together, these findings highlight that all of the policies studied had meaningful consequences for family members beyond the direct beneficiaries and suggest that similar spillover effects are likely to occur in most policy contexts. Studies focusing exclusively on the targeted individuals therefore risk underestimating both the full benefits and the potential costs of such interventions. Policymakers should thus aim at considering all intended and unintended consequences when designing and evaluating social policies.

A fourth strand of conclusions concerns the situation of **disadvantaged families**, who are a central focus throughout this dissertation. These families often face financial constraints and limited access to resources, which restrict their ability to invest in their children's human capital. Across the empirical chapters, the findings show that policies which ease these constraints can have particularly large effects. **Chapter 2** shows that parents without a university degree are far more likely to overestimate their children's abilities, highlighting a key mechanism through which educational inequalities can persist. On average, report cards only weakly correct these misperceptions, whereas structured and precise formats, such as numerical grades, are more effective in aligning parental beliefs and encouraging engagement, particularly among disadvantaged families. **Chapter 3** demonstrates that expanding public child support payments improves the financial well-being of single-parent households which are at higher risk of poverty, without crowding out private transfers. It also shows that advance child support payments encourage greater labor supply among mothers and raises expenditures on goods tied to children's development. **Chapter 4** shows that granting birthright citizenship to immigrant children, who on average have lower educational attainment, has larger benefits than previous studies have estimated. Birthright citizenship not

only improves the children's own educational outcomes but also generates spillovers to older siblings through families' naturalization and increased parental investments. Within disadvantaged immigrant families, heterogeneity analyses demonstrate the importance of the migration context. They show particular advantages for families who likely arrived as refugees, which is in line with previous research (Fasani et al., 2024). Taken together, these findings emphasize that disadvantaged families respond strongly to well-designed policies and can benefit greatly. For policymakers this suggests that interventions targeted at disadvantaged households can be highly effective in strengthening parental investments and reducing inequalities in children's outcomes, if they are carefully tailored to the specific barriers faced by each group.

Finally, this dissertation highlights the importance of **policy design**. Debates around school report cards often focus on children's motivation, yet **Chapter 2** shows that some reporting formats are far less effective at informing parents. Because disadvantaged parents are more prone to misperceptions about their children's abilities, poorly designed report cards risk reinforcing educational inequalities. **Chapter 3** demonstrates that the design of public advance child support payments is equally crucial. In particular, the interaction with existing welfare systems and the mode of enforcement shape both economic outcomes and parental behavior. The results reveal that state-led collection is more effective than leaving enforcement to resident parents, and that linking child support to parents' economic independence has implications for maternal labor supply. **Chapter 4** further illustrates the importance of considering the broader household context, since granting unequal opportunities to children might impact the way parents invest in these children. Overall, these findings reveal that careful attention to policy design – including information modality, enforcement mechanisms, and household spillovers – as well as to the interaction of different policies is essential to maximize intended benefits and avoid unintended adverse effects.

In summary, this dissertation demonstrates that children's human capital is influenced not only by individual policies but also by family responses. Parental investments are dynamic and adaptive: parents adjust both time and financial resources across children and domains based on the signals and support they receive. Policies that provide clear information, alleviate financial constraints, or modify institutional opportunities can significantly enhance these investments, particularly for disadvantaged families. Additionally, support from the extended family network can relieve pressures on parents, but it may also negatively impact children's development if the interactions between formal and informal care and education are not taken into account. Furthermore, the effects of interventions extend beyond the individuals directly targeted, creating important spillovers within families that must be considered to fully

evaluate policy effectiveness. Finally, the findings emphasize that careful policy design – which includes attention to household context – is essential to maximize benefits, minimize unintended consequences, and target support toward the most vulnerable families. Together, these insights provide guidance for designing social policies that efficiently promote children's human capital development.

Bibliography

Agee, M. D. and T. D. Crocker (1996): "Parents' Discount Rates for Child Quality," *Southern Economic Journal*, 36–50.

Alexander, K. L., D. R. Entwisle, and L. S. Olson (2007): "Lasting Consequences of the Summer Learning Gap," *American Sociological Review*, 72, 167–180.

Algan, Y., C. Dustmann, A. Glitz, and A. Manning (2010): "The Economic Situation of First and Second-Generation Immigrants in France, Germany and the United Kingdom," *The Economic Journal*, 120, F4–F30.

Allcott, H. and T. Rogers (2014): "The Short-Run and Long-Run Effects of Behavioral Interventions: Experimental Evidence from Energy Conservation," *American Economic Review*, 104, 3003–3037.

Almond, D. and B. Mazumder (2013): "Fetal Origins and Parental Responses," *Annual Review of Economics*, 5, 37–56.

Alscher, S., J. Obergfell, and S. R. Roos (2015): "Migrationsprofil Westbalkan: Ursachen, Herausforderungen und Lösungsansaetze," *Bundesamt für Migration und Fluechtlinge Working Paper*, 63.

Amelio, A. and F. Zimmermann (2023): "Motivated Memory in Economics – A Review," *Games*, 14, 15.

Ammermueller, A. (2007): "Poor Background or Low Returns? Why Immigrant Students in Germany Perform so Poorly in the Programme for International Student Assessment," *Education Economics*, 15, 215–230.

Anderberg, D., H. Rainer, and F. Siuda (2022): "Quantifying Domestic Violence in Times of Crisis: An Internet Search Activity-Based Measure for the COVID-19 Pandemic," *Journal of the Royal Statistical Society Series A: Statistics in Society*, 185, 498–518.

Andersen, S. C. and H. S. Nielsen (2016): "Reading Intervention With a Growth Mindset Approach Improves Children's Skills," *Proceedings of the National Academy of Sciences*, 113, 12111–12113.

Andrabi, T., J. Das, and A. I. Khwaja (2017): "Report Cards: The Impact of Providing School and Child Test Scores on Educational Markets," *American Economic Review*, 107, 1535–1563.

Angelucci, M. and O. Attanasio (2009): "Oportunidades: Program Effect on Consumption, Low Participation, and Methodological Issues," *Economic Development and Cultural Change*, 57, 479–506.

Antony-Newman, M. (2019): "Parental Involvement of Immigrant Parents: A Meta-Synthesis," *Educational Review*, 71, 362–381.

Ao, X., X. Chen, and Z. Zhao (2022): "Is Care by Grandparents or Parents Better for Children's Non-Cognitive Skills? Evidence on Locus of Control from China," *China Economic Review*, 71, 101734.

Armand, A., O. Attanasio, P. Carneiro, and V. Lechene (2020): "The Effect of Gender-Targeted Conditional Cash Transfers on Household Expenditures: Evidence From a Randomized Experiment," *The Economic Journal*, 130, 1875–1897.

Arpino, B. and V. Bordone (2014): "Does Grandparenting Pay Off? The Effect of Child Care on Grandparents' Cognitive Functioning," *Journal of Marriage and Family*, 76, 337–351.

——— (2017): "Regular Provision of Grandchild Care and Participation in Social Activities," *Review of Economics of the Household*, 15, 135–174.

Autorengruppe Bildungsberichterstattung (2018): *Bildung in Deutschland 2018. Ein indikatorengestützter Bericht mit einer Analyse zu Wirkungen und Erträgen von Bildung*, Bielefeld: WBV Publikation.

——— (2020): *Bildung in Deutschland 2020. Ein indikatorengestützter Bericht mit einer Analyse zu Bildung in einer digitalisierten Welt*, Bielefeld: WBV Publikation.

Avitabile, C., I. Clots-Figueras, and P. Masella (2013): "The Effect of Birthright Citizenship on Parental Integration Outcomes," *The Journal of Law and Economics*, 56, 777–810.

——— (2014): "Citizenship, Fertility, and Parental Investments," *American Economic Journal: Applied Economics*, 6, 35–65.

Backhaus, A. and M. Barslund (2021): "The Effect of Grandchildren on Grandparental Labor Supply: Evidence from Europe," *European Economic Review*, 137, 103817.

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.

Baker, S. R. and A. Fradkin (2017): "The Impact of Unemployment Insurance on Job Search: Evidence from Google Search Data," *Review of Economics and Statistics*, 99, 756–768.

Barrera-Osorio, F., K. Gonzalez, F. Lagos, and D. J. Deming (2020): "Providing Performance Information in Education: An Experimental Evaluation in Colombia," *Journal of Public Economics*, 186, 104185.

Barschkett, M. (2022): "Age-Specific Effects of Early Daycare on Children's Health," *DIW Berlin Discussion Paper*, 2028.

Barschkett, M. and L. Bosque-Mercader (2024): "Building Health Across Generations: Childbirth, Childcare and Maternal Health," *HEDG University of York WP*, 24/08.

Bastian, J. and L. Lochner (2022): "The Earned Income Tax Credit and Maternal Time Use: More Time Working and Less Time With Kids?" *Journal of Labor Economics*, 40, 573–611.

Bauernschuster, S., T. Hener, and H. Rainer (2016): "Children of a (policy) revolution: The introduction of universal child care and its effect on fertility," *Journal of the European Economic Association*, 14, 975–1005.

Beblo, M., C. Boll, M. Berg, R. Geene, M. Kreyenfeld, A. Lenze, K. Scheiwe, P. S. Schober, and H. Stichnoth (2025): *Zehnter Familienbericht: Unterstützung allein-und getrennterziehender Eltern und ihrer Kinder – Bestandsaufnahme und Handlungsempfehlungen*, Bundesministerium für Familie, Senioren, Frauen und Jugend.

Becker, G. S. (1962): "Investment in Human Capital: A Theoretical Analysis," *Journal of Political Economy*, 70, 9–49.

——— (1965): "A Theory of the Allocation of Time," *The Economic Journal*, 75, 493–517.

Becker, G. S. and N. Tomes (1976): "Child Endowments and the Quantity and Quality of Children," *Journal of Political Economy*, 84, S143–S162.

——— (1979): "An Equilibrium Theory of the Distribution of Income and Intergenerational Mobility," *Journal of Political Economy*, 87, 1153–1189.

Bénabou, R. and J. Tirole (2002): "Self-Confidence and Personal Motivation," *The Quarterly Journal of Economics*, 117, 871–915.

Bennhoff, F. H., J. L. García, and D. E. Leaf (2024): "The Dynastic Benefits of Early-Childhood Education: Participant Benefits and Family Spillovers," *Journal of Human Capital*, 18, 44–73.

Bergbauer, A. B., E. A. Hanushek, and L. Woessmann (2018): "Testing," *Journal of Human Resources*, 59, 349–388.

Berger, E. M. and C. K. Spiess (2011): "Maternal life satisfaction and child outcomes: Are they related?" *Journal of Economic Psychology*, 32, 142–158.

Bergman, P. (2021): "Parent-Child Information Frictions and Human Capital Investment: Evidence from a Field Experiment," *Journal of Political Economy*, 129, 286–322.

Bergman, P. and E. W. Chan (2021): "Leveraging Parents through Low-Cost Technology: The Impact of High-Frequency Information on Student Achievement," *Journal of Human Resources*, 56, 125–158.

Berry, J., R. Dizon-Ross, and M. Jagnani (2020): "Not Playing Favorites: An Experiment on Parental Fairness preferences," *NBER Working Paper*, 26732.

Bettinger, E., T. Haegeland, and M. Rege (2014): "Home With Mom: The Effects of Stay-at-Home Parents on Children's Long-Run Educational Outcomes," *Journal of Labor Economics*, 32, 443–467.

Blanden, J., M. Doepke, and J. Stuhler (2023): "Educational Inequality," Elsevier, vol. 6 of *Handbook of the Economics of Education*, 405–497.

Blossfeld, H.-P. and H.-G. Rossbach (2019): *Education as a Lifelong Process: The German National Educational Panel Study (NEPS). Edition ZfE (2nd ed.)*, Springer VS.

Blundell, R., M. Brewer, and M. Francesconi (2008): "Job Changes and Hours Changes: Understanding the Path of Labor Supply Adjustment," *Journal of Labor Economics*, 26, 421–453.

BMAS (2022): *Fachkraeftestrategie der Bundesregierung*, Berlin: Bundesministerium für Arbeit und Soziales.

BMBF (2024): "Neue KMK-Statistik für Ganztagsschulen 2022/2023," https://www.ganztagsschulen.org/SharedDocs/Kurzmeldungen/de/m-o/neue-kmk-statistik-fuer-ganztagsschulen-2022-2023.html, accessed: 2024-09-04.

BMFSFJ (2023): "Staat holt sich deutlich mehr Unterhaltsvorschuss-Zahlungen zurück," https://www.bmbfsfj.bund.de/bmbfsfj/aktuelles/alle-meldungen/staat-holt-sich-deutlich-mehr-unterhaltsvorschuss-zahlungen-zurueck-214300, accessed: 2025-07-25.

——— (2025): *Der Unterhaltsvorschuss – Eine Hilfe für Alleinerziehende und ihre Kinder*, Bundesministerium für Familie, Senioren, Frauen und Jugend.

BMI (2023a): "Erwerb der deutschen Staatsangehoerigkeit durch Geburt in Deutschland," https://www.bmi.bund.de/DE/themen/verfassung/staatsangehoerigkeit/optionspflicht/optionspflicht-node.html, accessed: 2023-01-04.

——— (2023b): "Staatsangehoerigkeitsrecht," https://www.bmi.bund.de/DE/themen/verfassung/staatsangehoerigkeit/staatsangehoerigkeitsrecht/staatsangehoerigkeitsrecht-node.html, accessed: 2023-01-04.

Boll, C. and S. Schüller (2023): "The Economic Well-Being of Nonresident Fathers and Custodial Mothers Revisited: The Role of Paternal Childcare," *Journal of Family and Economic Issues*, 44, 836–853.

Boneva, T. and C. Rauh (2018): "Parental Beliefs About Returns to Educational Investments – The Later the Better?" *Journal of the European Economic Association*, 16, 1669–1711.

Borger, M., G. Elacqua, I. Jacas, C. Neilson, and A. S. W. Olsen (2024): "Report Cards: Parental Preferences, Information and School Choice in Haiti," *Economics of Education Review*, 102, 102560.

Bradbury, B., M. Corak, J. Waldfogel, and E. Washbrook (2015): *Too Many Children Left behind: The U.S. Achievement Gap in Comparative Perspective*, New York: Russell Sage Foundation.

Bratsberg, B., J. F. Ragan, Jr, and Z. M. Nasir (2002): "The Effect of Naturalization on Wage Growth: A Panel Study of Young Male Immigrants," *Journal of Labor Economics*, 20, 568–597.

Bratsch-Hines, M. E., I. Mokrova, L. Vernon-Feagans, F. L. P. K. Investigators, et al. (2015): "Child Care Instability From 6 to 36 Months and the Social Adjustment of Children in Prekindergarten," *Early Childhood Research Quarterly*, 30, 106–116.

Bratti, M., T. Frattini, and F. Scervini (2018): "Grandparental Availability for Child Care and Maternal Labor Force Participation: Pension Reform Evidence From Italy," *Journal of Population Economics*, 31, 1239–1277.

Bredtmann, J., S. Otten, and C. Vonnahme (2024): "Discrimination in Grading? Evidence on Teachers' Evaluation Bias Towards Minority Students," *Ruhr Economic Papers*, 1122.

Bredtmann, J. and C. Vonnahme (2019): "Less Money After Divorce – How the 2008 Alimony Reform in Germany Affected Spouses' Labor Supply, Leisure and Marital Stability," *Review of Economics of the Household*, 17, 1191–1223.

Brown, M., C. J. Flinn, and J. Mullins (2025): "Family law effects on divorce, fertility, and child investment," *Journal of Labor Economics*, 43, S351–S397.

Brügelmann, H. (2006): "Sind Noten nützlich - und nötig? Ziffernzensuren und ihre Alternativen im empirischen Vergleich. Eine Expertise der Arbeitsgruppe Primarstufe an der Universität Siegen im Auftrag des Grundschulverbands e. V., Frankfurt (Kurzfassung)," in *Pädagogische Leistungskultur*, ed. by H. Bartnitzky, H. Brügelmann, U. Hecker, and G. Schönknecht, Frankfurt am Main: Grundschulverband - Arbeitskreis Grundschule e.V., 17–46.

Buckles, K. S. and D. M. Hungerman (2013): "Season of Birth and Later Outcomes: Old Questions, New Answers," *Review of Economics and Statistics*, 95, 711–724.

Bürgerschaft der Freien und Hansestadt Hamburg (2024): *Schriftliche kleine Anfrage Der Abgeordneten Sabine Boeddinghaus (DIE LINKE) vom 09.07.24 Und Antwort des Senats*, Drucksache 22/15775.

Bürgerliches Gesetzbuch (2002): "§1603, §1606, §1612a BGB," `https://www.gesetze-im-internet.de/bgb/`.

Cancian, M. and D. R. Meyer (2014): "Testing the Economic Independence Hypothesis: The Effect of an Exogenous Increase in Child Support on Subsequent Marriage and Cohabitation," *Demography*, 51, 857–880.

Card, D. (1999): "The Causal Effect of Education on Earnings," Elsevier, vol. 3 of *Handbook of Labor Economics*, 1801–1863.

Case, A., D. Lubotsky, and C. Paxson (2002): "Economic Status and Health in Childhood: The Origins of the Gradient," *American Economic Review*, 92, 1308–1334.

Castleman, B. L. (2015): "Prompts, Personalization, and Pay-Offs: Strategies to Improve the Design and Delivery of College and Financial Aid Information," in *Decision making for student success*, Routledge, 79–101.

Catron, P. (2019): "The Citizenship Advantage: Immigrant Socioeconomic Attainment in the Age of Mass Migration," *American Journal of Sociology*, 124, 999–1042.

Caucutt, E. M., L. Lochner, and Y. Park (2017): "Correlation, Consumption, Confusion, or Constraints: Why do Poor Children Perform so Poorly?" *The Scandinavian Journal of Economics*, 119, 102–147.

Chang, L. W., E. L. Kirgios, S. Mullainathan, and K. L. Milkman (2024): "Does Counting Change What Counts? Quantification Fixation Biases Decision-Making," *Proceedings of the National Academy of Sciences*, 121, e2400215121.

Chen, Y. and X. Zhang (2018): "When mommies become nannies: The effects of parental retirement across generations," *SSRN*, 3146737.

Chetty, R., J. N. Friedman, E. Saez, N. Turner, and D. Yagan (2017): "Mobility Report Cards: The Role of Colleges in Intergenerational Mobility," *NBER Working Paper*, 23618.

Chiappori, P.-A., M. Iyigun, J. Lafortune, and Y. Weiss (2017): "Changing the Rules Midway: The Impact of Granting Alimony Rights on Existing and Newly Formed Partnerships," *The Economic Journal*, 127, 1874–1905.

Clark, S. J., D. C. Singer, S. L. Schultz, A. Gebremariam, and G. L. Freed (2020): "When Parents and Grandparents Disagree," *C.S. Mott Children's Hospital National Poll on Children's Health, University of Michigan.*, 36, August 2020.

Clarke, D. and B. Matta (2018): "Practical Considerations for Questionable IVs," *The Stata Journal*, 18, 663–691.

Cobb-Clark, D. A., T. Ho, and N. Salamanca (2021): "Parental Responses to Children's Achievement Test Results," *IZA Discussion Paper*, 14663.

Coleman, J. S., E. Q. Campbell, C. J. Hobson, J. McPartland, A. M. Mood, F. D. Weinfeld, and R. L. York (1966): *Equality of Educational Opportunity*, Washington, DC: U.S. Department of Health, Education, and Welfare, Office of Education.

Collins, M. and J. Lundstedt (2024): "The Effects of More Informative Grading on Student Outcomes," *Journal of Economic Behavior & Organization*, 218, 514–549.

Compton, J. and R. A. Pollak (2014): "Family Proximity, Childcare, and Women's Labor Force Attachment," *Journal of Urban Economics*, 79, 72–90.

Coneus, K. and C. K. Spiess (2012): "The Intergenerational Transmission of Health in Early Childhood – Evidence from the German Socio-Economic Panel Study," *Economics & Human Biology*, 10, 89–97.

Conley, T. G., C. B. Hansen, and P. E. Rossi (2012): "Plausibly Exogenous," *Review of Economics and Statistics*, 94, 260–272.

Conti, G., J. J. Heckman, and R. Pinto (2016): "The Effects of Two Influential Early Childhood Interventions on Health and Healthy Behaviour," *The Economic Journal*, 126, F28–F65.

Cornelissen, T., C. Dustmann, A. Raute, and U. Schoenberg (2018): "Who Benefits From Universal Child Care? Estimating Marginal Returns to Early Child Care Attendance," *Journal of Political Economy*, 126, 2356–2409.

Cortes, K. E. (2013): "Achieving the DREAM: The Effect of IRCA on Immigrant Youth Postsecondary Educational Access," *American Economic Review*, 103, 428–432.

Cowan, B., T. R. Jones, and J. Swigert (2023): "Parental and Student Time Use Around the Academic Year," *Journal of Economic Behavior & Organization*, 224, 66–110.

Cuesta, L. and M. Cancian (2015): "The Effect of Child Support on the Labor Supply of Custodial Mothers Participating in TANF," *Children and Youth Services Review*, 54, 49–56.

Cunha, F., I. T. Elo, and J. Culhane (2013): "Eliciting Maternal Expectations About the Technology of Cognitive Skill Formation," *NBER Working Paper*, 19144.

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. (2020): "Child Health as Human Capital," *Health Economics*, 29, 452–463.

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.

Cygan-Rehm, K. (2018): "Estimating the Effect of Early-Childhood Citizenship on Education Using Policy Changes as Instruments," *Applied Economics Letters*, 25, 1426–1431.

Dahl, G. B., C. Felfe, P. Frijters, and H. Rainer (2022): "Caught Between Cultures: Unintended Consequences of Improving Opportunity for Immigrant Girls," *The Review of Economic Studies*, 89, 2491–2528.

Dahl, G. B., D.-O. Rooth, and A. Stenberg (2024): "Intergenerational and Sibling Spillovers in High School Majors," *American Economic Journal: Economic Policy*, 16, 133–173.

Danielsbacka, M., A. O. Tanskanen, D. A. Coall, and M. Jokela (2019): "Grandparental Childcare, Health and Well-Being in Europe: A Within-Individual Investigation of Longitudinal Data," *Social Science & Medicine*, 230, 194–203.

Danzer, N., M. Halla, N. Schneeweis, and M. Zweimüller (2020): "Parental Leave, (In)Formal Childcare and Long-Term Child Outcomes," *Journal of Human Resources*, 57, 1826–1884.

Datta Gupta, N., J. Jessen, and C. K. Spiess (2023): "Maternal Life Satisfaction and Child Development from Toddlerhood to Adolescence," *IZA Discussion Paper 16155*.

De Walque, D. and C. Valente (2023): "Incentivizing School Attendance in the Presence of Parent-Child Information Frictions," *American Economic Journal: Economic Policy*, 15, 256–285.

Del Boca, D., D. Piazzalunga, and C. Pronzato (2018): "The Role of Grandparenting in Early Childcare and Child Outcomes," *Review of Economics of the Household*, 16, 477–512.

Deutscher Bundestag (2018): *Bericht der Bundesregierung über die Wirkungen der Reform des Unterhaltsvorschussgesetzes ein Jahr nach dem Inkrafttreten zum 1. Juli 2017*, Drucksache 19/3960, 19. Wahlperiode, Unterrichtung durch die Bundesregierung.

Dickson, M., P. Gregg, and H. Robinson (2016): "Early, Late or Never? When Does Parental Education Impact Child Outcomes?" *The Economic Journal*, 126, F184–F231.

Dizon-Ross, R. (2019): "Parents' Beliefs about Their Children's Academic Ability: Implications for Educational Investments," *American Economic Review*, 109, 2728–2765.

Downey, D. B., P. T. Von Hippel, and B. A. Broh (2004): "Are Schools the Great Equalizer? Cognitive Inequality During the Summer Months and the School Year," *American Sociological Review*, 69, 613–635.

Duncan, G., A. Kalil, M. Mogstad, and M. Rege (2023): "Investing in Early Childhood Development in Preschool and at Home," *Handbook of the Economics of Education*, 6, 1–91.

Dunifon, R., A. Kalil, and S. K. Danziger (2003): "Maternal Work Behavior Under Welfare Reform: How Does the Transition from Welfare to Work Affect Dhild Development?" *Children and Youth Services Review*, 25, 55–82.

Dweck, C. S. (2006): *Mindset: The New Psychology of Success*, Random house.

Eibich, P. and T. Siedler (2020): "Retirement, Intergenerational Time Transfers, and Fertility," *European Economic Review*, 124, 103392.

Eibich, P. and X. Zai (2024): "Are the Grandparents Alright? The Health Consequences of Grandparental Childcare Provision," *Journal of Population Economics*, 37, 71.

Ermisch, J. and C. Pronzato (2008): "Intra-Household Allocation of Resources: Inferences From Non-Resident Dathers' Child Support Payments," *The Economic Journal*, 118, 347–362.

European Commission and European Education and Culture Executive Agency (2023): *Recommended Annual Instruction Time in Full-Time Compulsory Education in Europe 2022/2023*, Publications Office of the European Union.

European Commission Directorate-General for Education, Youth, Sport and Culture and Eurydice (2025): "Eurypedia: European Encyclopedia on National Education Systems," https://eurydice.eacea.ec.europa.eu/eurypedia, accessed: 2025-07-22.

Eurostat (2023): "Part-Time and Full-Time Employment – Statistics," https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Part-time_and_full-time_employment_-_statistics#Data_sources, accessed: 2025-03-15.

Fahn, M., R. Rees, and A. Wuppermann (2016): "Relational Contracts for Household Formation, Fertility Choice and Separation," *Journal of Population Economics*, 29, 421–455.

Fan, X. and M. Chen (2001): "Parental Involvement and Students' Academic Achievement: A Meta-Analysis," *Educational Psychology Review*, 13, 1–22.

Fasani, F., T. Frattini, and M. Pirot (2024): "From Refugees to Citizens: Labor Market Returns to Naturalization," *Centro Studi Luca d'Agliano Development Studies Working Paper*, 489.

FDZ-LIFBI (2019): *Information on Competence Testing. NEPS Starting Cohort 2 — Kindergarten. From Kindergarten to Elementary School. Wave 3: Grade 1*, Bamberg: Leibniz Institute for Educational Trajectories (LIfBi).

Federal Statistical Office of Germany (2025): "Income and Expenditure Survey (EVS) 2008-2018 – Scientific Use File," Provided by the Research Data Centre (RDC) of the Federal Statistical Office and Statistical Offices of the Federal States of Germany.

Felfe, C. and R. Lalive (2018): "Does Early Child Care Affect Children's Development?" *Journal of Public Economics*, 159, 33–53.

Felfe, C., H. Rainer, and J. Saurer (2020): "Why Birthright Citizenship Matters for Immigrant Children: Short-and Long-Run Impacts on Educational Integration," *Journal of Labor Economics*, 38, 143–182.

Fenoll, A. A. (2020): "The Uneven Impact of Women's Retirement on Their Daughters' Employment," *Review of Economics of the Household*, 18, 795–821.

Ferrer-i Carbonell, A. and P. Frijters (2004): "How Important is Methodology for the Estimates of the Determinants of Happiness?" *The Economic Journal*, 114, 641–659.

Festa, N., P. D. Loftus, M. R. Cullen, and F. S. Mendoza (2014): "Disparities in Early Exposure to Book Sharing Within Immigrant Families," *Pediatrics*, 134, e162–e168.

Figlio, D. N., K. Karbownik, and U. Oezek (2023): "Sibling Spillovers May Enhance the Efficacy of Targeted School Policies," *NBER Working Paper*, 31406.

Firpo, S., N. M. Fortin, and T. Lemieux (2009): "Unconditional Quantile Regressions," *Econometrica*, 77, 953–973.

Fox-Boyer, A. (2006): *TROG-D: Test zur Überprüfung des Grammatikverständnisses*, Idstein, Germany: Schulz-Kirchner Verlag.

Francesconi, M. and J. J. Heckman (2016): "Child Development and Parental Investment: Introduction," *The Economic Journal*, 126, F1–F27.

Frijters, P., D. W. Johnston, and M. A. Shields (2014): "The Effect of Mental Health on Employment: Evidence From Australian Panel Data," *Health Economics*, 23, 1058–1071.

Frimmel, W., M. Halla, B. Schmidpeter, and R. Winter-Ebmer (2020): "Grandmothers' Labor Supply," *Journal of Human Resources*, 57, 1645–1689.

Gambaro, L., M. Kreyenfeld, D. Schacht, and C. K. Spiess (2018): "Lebenszufriedenheit von Gefluechteten in Deutschland ist deutlich geringer, wenn ihre Kinder im Ausland leben," *DIW Wochenbericht*, 85, 905–916.

Gambaro, L., J. Marcus, and F. Peter (2019): "School Entry, Afternoon Care, and Mothers' Labour Supply," *Empirical Economics*, 57, 769–803.

Gambaro, L., G. Neidhöfer, and C. K. Spiess (2021): "The Effect of Early Childhood Education and Care Services on the Integration of Refugee Families," *Labour Economics*, 72, 102053.

Gambaro, L., C. Schäper, and C. K. Spiess (2024): "Crowded-Out? Changes in Informal Childcare During the Expansion of Formal Services in Germany," *Social Policy & Administration*, 59, 383–398.

Gambaro, L., C. K. Spiess, K. Wrohlich, and E. Ziege (2023): "Should Mama or Papa Work? Variations in Attitudes Towards Parental Employment by Country of Origin and Child Age," *Comparative Population Studies*, 48.

Gan, T. (2021): "How Parents' Beliefs About Their Children's Academic Ability Affect Educational Investments," *Unpublished Manuscript*.

Gathmann, C. and J. Garbers (2023): "Citizenship and Integration," *Labour Economics*, 82, 102343.

Gathmann, C. and N. Keller (2018): "Access to Citizenship and the Economic Assimilation of Immigrants," *The Economic Journal*, 128, 3141–3181.

Gathmann, C. and B. Sass (2018): "Taxing Childcare: Effects on Childcare Choices, Family Labor Supply, and Children," *Journal of Labor Economics*, 36, 665–709.

Gathmann, C., C. Vonnahme, A. Busse, and J. Kim (2021): "Marginal Returns to Citizenship and Educational Performance," *Ruhr Economic Papers*, 920.

Gesetz zur Sicherung des Unterhalts von Kindern alleinstehender Mütter und Väter durch Unterhaltsvorschüsse oder -ausfallleistungen (UhVorschG) (1979): "§1 UhVorschG," `https://www.gesetze-im-internet.de/uvg/`.

Gihleb, R., O. Giuntella, and J. Lonsky (2023): "Dreaming of Leaving the Nest? Immigration Status and the Living Arrangements of Dacamented," *European Economic Review*, 156, 104447.

Goerlitz, 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.

Goodman, J., M. Hurwitz, J. Smith, and J. Fox (2015): "The Relationship Between Siblings' College Choices: Evidence From one Million SAT-Taking Families," *Economics of Education Review*, 48, 75–85.

Govind, Y. (2021): "Is Naturalization a Passport for Better Labor Market Integration? Evidence From a Quasi-Experimental Setting," *Unpublished manuscript*.

Govind, Y. and L. Sirugue (2023): "To Become or Not to Become French: Conscription, Naturalization, and Labor Market Integration," *Unpublished manuscript*.

Gregg, P., S. Harkness, and S. Smith (2003): "Welfare Reform and Lone Parents' Employment in the UK," in *The Labour Market under New Labour: The State of Working Britain*, ed. by R. Dickens, P. Gregg, and J. Wadsworth, Basingstoke: Palgrave Macmillan, 55–74.

Gregg, P., J. Waldfogel, and E. Washbrook (2006): "Family Expenditures Post-Welfare Reform in the UK: Are Low-Income Families Starting to Catch Up?" *Labour Economics*, 13, 721–746.

Gresch, C., K. Maaz, M. Becker, and N. McElvany (2012): "Zur hohen Bildungsaspiration von Migranten beim Übergang von der Grundschule in die Sekundarstufe: Fakt oder Artefakt," *Soziale Ungleichheit in der Einwanderungsgesellschaft: Kategorien, Konzepte, Einflussfaktoren*, 56–67.

Gruber, J. (2004): "Is Making Divorce Easier Bad for Children? The Long-Run Implications of Unilateral Divorce," *Journal of Labor Economics*, 22, 799–833.

Gunter, S. P. (2013): "Effects of Child Support Pass-Through and Disregard Policies on In-Kind Child Support," *Review of Economics of the Household*, 11, 193–209.

Haimovitz, K. and C. S. Dweck (2016): "What Predicts Children's Fixed and Growth Intelligence Mind-Sets? Not Their Parents' Views of Intelligence But Their Parents' Views of Failure," *Psychological Science*, 27, 859–869.

Hainmueller, J. (2012): "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies," *Political Analysis*, 20, 25–46.

Hainmueller, J., E. Cascardi, M. Hotard, R. Koslowski, D. Lawrence, V. I. Yasenov, and D. Laitin (2023): "Does Access to Citizenship Confer Socio-Economic Returns? Evidence From a Randomized Control Design," *IZA Discussion Paper*, 16173.

Hainmueller, J., D. Hangartner, and G. Pietrantuono (2017): "Catalyst or Crown: Does Naturalization Promote the Long-Term Social Integration of Immigrants?" *American Political Science Review*, 111, 256–276.

Hainmueller, J., D. Hangartner, and D. Ward (2019): "The Effect of Citizenship on the Long-Term Earnings of Marginalized Immigrants: Quasi-Experimental Evidence From Switzerland," *Science Advances*, 5, eaay1610.

Hakovirta, M., L. Cuesta, M. Haapanen, and D. R. Meyer (2022): "Child Support Policy Across High-Income Countries: Similar Problems, Different Approaches," *The ANNALS of the American Academy of Political and Social Science*, 702, 97–111.

Hakovirta, M. and M. Mesiäislehto (2022): "Lone Mothers and Child Support Receipt in 21 European Countries," *Journal of International and Comparative Social Policy*, 38, 36–56.

Hank, K. and I. Buber (2009): "Grandparents Caring for Their Grandchildren: Findings from the 2004 Survey of Health, Ageing, and Retirement in Europe," *Journal of Family Issues*, 30, 53–73.

Hank, K., G. Cavrini, G. Di Gessa, and C. Tomassini (2018): "What do We Know About Grandparents? Insights From Current Quantitative Data and Identification of Future Data Needs," *European Journal of Ageing*, 15, 225–235.

Hanushek, E. A. and L. Woessmann (2012): "Do Better Schools Lead to More Growth? Cognitive Skills, Economic Outcomes, and Causation," *Journal of Economic Growth*, 17, 267–321.

Harkness, S. (2022): "The Accumulation of Economic Disadvantage: The Influence of Childbirth and Divorce on the Income and Poverty Risk of Single Mothers," *Demography*, 59, 1377–1402.

Harkness, S. and M. F. Salgado (2018): "Single Motherhood and Child Development in the UK," in *The triple bind of single-parent families*, Policy Press, 101–124.

Hartmann, B. (2014): "Unterhaltsansprüche und deren Wirklichkeit: Wie groß ist das Problem nicht gezahlten Kindesunterhalts?" *SOEPpapers on Multidisciplinary Panel Data Research*, 660.

Havnes, T. and M. Mogstad (2015): "Is Universal Child Care Leveling the Playing Field?" *Journal of Public Economics*, 127, 100–114.

Heckman, J. J. (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 S. Mosso (2014): "The Economics of Human Development and Social Mobility," *Annual Review of Economics*, 6, 689–733.

Heintz-Martin, V., C. Recksiedler, and A. N. Langmeyer (2022): "Household Debt, Maternal Well-Being, and Child Adjustment in Germany: Examining the Family Stress Model by Family Structure," *Journal of Family and Economic Issues*, 43, 338–353.

Helliwell, J. F., R. Layard, J. D. Sachs, J.-E. De Neve, L. B. Aknin, and S. Wang (2024): *World Happiness Report 2024*, vol. 1, University of Oxford: Wellbeing Research Centre.

Hellrung, M., J. Bockelmann, C. Schneider, A. Waschk, and P. Hillen (2013): "Methodenbericht. NEPS Startkohorte 2. Haupterhebung – Frühjahr 2013," *NEPS Survey Papers, Wave 3*.

Herbst, C. M. and E. Tekin (2014): "Child Care Subsidies, Maternal Health, and Child–Parent Interactions: Evidence from Three Nationally Representative Datasets," *Health Economics*, 23, 894–916.

Hill, C. R. and F. P. Stafford (1974): "Allocation of Time to Preschool Children and Educational Opportunity," *The Journal of Human Resources*, 9, 323.

Hoddinott, J. and E. Skoufias (2004): "The Impact of PROGRESA on Food Consumption," *Economic Development and Cultural Change*, 53, 37–61.

Hoynes, H., D. W. Schanzenbach, and D. Almond (2016): "Long-Run Impacts of Childhood Access to the Safety Net," *American Economic Review*, 106, 903–934.

Hoynes, H. W. and A. J. Patel (2018): "Effective Policy for Reducing Poverty and Inequality? The Earned Income Tax Credit and the Distribution of ncome," *Journal of Human Resources*, 53, 859–890.

Hsin, A. and F. Ortega (2018): "The Effects of Deferred Action for Childhood Arrivals on the Educational Outcomes of Undocumented Students," *Demography*, 55, 1487–1506.

Huang, C.-C. and K.-Q. Han (2012): "Child Support Enforcement in the United States: Has Policy Made a Difference?" *Children and Youth Services Review*, 34, 622–627.

Hubert, S., F. Neuberger, M. Sommer, et al. (2020): "Alleinerziehend, alleinbezahlend? Kindesunterhalt, Unterhaltsvorschuss und Gründe für den Unterhaltsausfall," *Zeitschrift für Soziologie der Erziehung und Sozialisation*, 40, 19–38.

Hübner, N., M. Jansen, P. Stanat, T. Bohl, and W. Wagner (2024): "Alles eine Frage des Bundeslandes? Eine mehrebenenanalytische Betrachtung der eingeschränkten Vergleichbarkeit von Schulnoten," *Zeitschrift für Erziehungswissenschaft*, 27, 517–549.

Huebener, M. (2025): "The Effects of Education on Health: An Intergenerational Perspective," *Journal of Human Resources*, 60, 743–779.

Huebener, M., S. Kuger, and J. Marcus (2017): "Increased Instruction Hours and the Widening Gap in Student Performance," *Labour Economics*, 47, 15–34.

Huebener, M., A. Pape, and C. K. Spiess (2020): "Parental Labour Supply Responses to the Abolition of Day Care Fees," *Journal of Economic Behavior & Organization*, 180, 510–543.

Huinink, J., J. Brüderl, B. Nauck, S. Walper, L. Castiglioni, and M. Feldhaus (2011): "Panel Analysis of Intimate Relationships and Family Dynamics (pairfam): Conceptual Framework and Design," *ZfF–Zeitschrift für Familienforschung/Journal of Family Research*, 23.

Integrationsbeauftragte (2023): "Einbuergerung," `https://www.integrationsbeauftragte.de/ib-de/ich-moechte-mehr-wissen-ueber/einbuergerung`, accessed: 2023-01-19.

Isengard, B. (2013): "" Der Apfel lebt nicht weit vom Stamm": Wohnentfernungen zwischen Eltern und ihren erwachsenen Kindern in Europa," *Comparative Population Studies*, 38, 263–290.

Jessen, J. (2022): "Culture, Children and Couple Gender Inequality," *European Economic Review*, 150, 104310.

Jessen, J., S. Schmitz, and F. Weinhardt (2024): "Immigration, Female Labour Supply and Local Cultural Norms," *The Economic Journal*, 134, 1146–1172.

Johnson, R. C., A. Kalil, and R. E. Dunifon (2012): "Employment Patterns of Less-Skilled Workers: Links to Children's Behavior and Academic Progress," *Demography*, 49, 747–772.

Jones, L. E., K. Milligan, and M. Stabile (2019): "Child Cash Benefits and Family Expenditures: Evidence From the National Child Benefit," *Canadian Journal of Economics/Revue canadienne d'économique*, 52, 1433–1463.

Kalb, G. and J. C. Van Ours (2014): "Reading to Young Children: A Head-Start in Life?" *Economics of Education Review*, 40, 1–24.

Kalil, A., H. Liu, S. Mayer, D. Rury, and R. Shah (2025): "A Digital Library for Parent-Child Shared Reading Improves Literacy Skills for Young Disadvantaged Children," *Becker Friedman Institute for Economics Working Paper*, 2025-27.

Kalil, A., R. Ryan, and E. Chor (2014): "Time Investments in Children Across Family Structures," *The ANNALS of the American Academy of Political and Social Science*, 654, 150–168.

Kalil, A., R. Ryan, and M. Corey (2012): "Diverging Destinies: Maternal Education and the Developmental Gradient in Time with Children," *Demography*, 49, 1361–1383.

Kanji, S. (2018): "Grandparent Care: A key factor in Mothers' Labour Force Participation in the UK," *Journal of Social Policy*, 47, 523–542.

Kaplan, G. and O. Baron-Epel (2003): "What Lies Behind the Subjective Evaluation of Health Status?" *Social Science & Medicine*, 56, 1669–1676.

Karbownik, K. and U. Oezek (2023): "Setting a Good Example?: Examining Sibling Spillovers in Educational Achievement Using a Regression Discontinuity Design," *Journal of Human Resources*, 58, 1567–1607.

Kaufmann, K., Y. Özdemir, and H. Ye (2022): "Spillover Effects of Old-Age Pension Across Generations: Family Labor Supply and Child Outcomes," *CIZA Discussion Paper*, 15388.

Kemptner, D. and J. Marcus (2013): "Spillover Effects of Maternal Education on Child's Health and Health Behavior," *Review of Economics of the Household*, 11, 29–52.

Kim, Y., M. Cancian, and D. R. Meyer (2017): "Child Support and Subsequent Nonmarital Fertility With a New Partner," *Journal of Family Issues*, 38, 151–176.

Kinsler, J. and R. Pavan (2021): "Local Distortions in Parental Beliefs over Child Skill," *Journal of Political Economy*, 129, 81–100.

Kraft, M. A. and T. Rogers (2015): "The Underutilized Potential of Teacher-to-Parent Communication: Evidence From a Field Experiment," *Economics of Education Review*, 47, 49–63.

Kristen, C. and N. Granato (2007): "The Educational Attainment of the Second Generation in Germany: Social Origins and Ethnic Inequality," *Ethnicities*, 7, 343–366.

Kröll, A. and R. Borck (2013): "The Influence of Child Care on Maternal Health and Mother-Child Interaction," *CESifo Working Paper Series*, 4289.

Kultusministerkonferenz (1999): "286. Plenarsitzung der Kultusministerkonferenz am 27. und 28. Mai 1999 in Leipzig," `https://www.kmk.org/aktuelles/artikelansicht/286-plenarsitzung/`

-der-kultusministerkonferenz-am-27-und-28-mai-1999-in-leipzig.html, accessed: 2025-01-09.

——— (2012): "Langfristige Sommerferienregelung 2011 bis 2017. Stand: Januar 2012. Beschluss der Kultusministerkonferenz vom 15.05.2008," https://www.kmk.org/fileadmin/Dateien/pdf/Ferienkalender/Sommerferienregelung_2011-2017.pdf, accessed: 2025-01-29.

Landerso, R. K., H. S. Nielsen, and M. Simonsen (2020): "Effects of School Starting Age on the Family," *Journal of Human Resources*, 55, 1258–1286.

Landtag Baden-Württemberg (2022): "Modellprojekt zur Grundschule ohne Noten startet bald," https://www.landtag-bw.de/de/aktuelles/dpa-nachrichten/modellprojekt-zur-grundschule-ohne-noten-startet-bald-409722, accessed: 2025-01-29.

Leibowitz, A. (1974): "Home Investments in Children," *Journal of Political Economy*, 82, S111–S131.

——— (1977): "Parental Inputs and Children's Achievement," *The Journal of Human Resources*, 12, 242–251.

Lindahl, M., M. Palme, S. S. Massih, and A. Sjögren (2015): "Long-Term Intergenerational Persistence of Human Capital: an Empirical Analysis of four Generations," *Journal of Human Resources*, 50, 1–33.

Lochner, L. and E. Moretti (2004): "The Effect of Education on Crime: Evidence From Prison Inmates, Arrests, and Self-Reports," *American Economic Review*, 94, 155–189.

Løken, K. V., K. E. Lommerud, and K. H. Reiso (2018): "Single Mothers and Their Children: Evaluating a Work-Encouraging Welfare Reform," *Journal of Public Economics*, 167, 1–20.

Lundberg, S. and R. A. Pollak (2007): "The American Family and Family Economics," *Journal of Economic Perspectives*, 21, 3–26.

Mani, A., S. Mullainathan, E. Shafir, and J. Zhao (2013): "Poverty Impedes Cognitive Function," *Science*, 341, 976–980.

Marbach, M. and D. Hangartner (2020): "Profiling Compliers and Noncompliers for Instrumental-Variable Analysis," *Political Analysis*, 28, 435–444.

Marcus, J., C. K. Spiess, S. Waights, and A. Judy (2021): "Akademikerinnen engagieren sich häufiger als andere Mütter in der Schule ihrer Kinder," *DIW Wochenbericht*, 88, 739–747.

Milligan, K. and M. Stabile (2007): "The Integration of Child Tax Credits and Welfare: Evidence From the Canadian National Child Benefit Program," *Journal of Public Economics*, 91, 305–326.

Milovanska-Farrington, S. (2021): "The Effect of Parental and Grandparental Supervision Time Investment on Children's Early-Age Development," *Research in Economics*, 75, 286–304.

Mincer, J. A. (1974): "Schooling and Earnings," in *Schooling, Experience, and Earnings*, NBER, 41–63.

Ministry of Education of North Rhine Westphalia (2024): "Ganztags- und Betreuungsangebote," `https://www.schulministerium.nrw/ganztags-und-betreuungsangebote`, accessed: 2024-04-12.

Mogstad, M. and C. Pronzato (2012): "Are Lone Mothers Responsive to Policy Changes? Evidence From a Workfare Reform in a Generous Welfare State," *The Scandinavian Journal of Economics*, 114, 1129–1159.

Moullin, S. and S. Harkness (2021): "The Single Motherhood Penalty as a Gender Penalty: Comment on Brady, Finnigan, and Hübgen," *American Journal of Sociology*, 127, 621–636.

Müller, K.-U. and K. Wrohlich (2020): "Does Subsidized Care for Toddlers Increase Maternal Labor Supply? Evidence From a Large-Scale Expansion of Early Childcare," *Labour Economics*, 62, 101776.

Najjarrezaparast, P. and K. Pendakur (2021): "How Did the Canada Child Benefit Affect Household Spending?" *Canadian Public Policy*, 47, 479–496.

Nauck, B. (2000): "Eltern-Kind-Beziehungen in Migrantenfamilien—ein Vergleich zwischen griechischen, italienischen, tuerkischen und vietnamesischen Familien in Deutschland," Springer, Familien auslaendischer Herkunft in Deutschland: Empirische Beitraege zur Familienentwicklung und Akkulturation, 347–392.

Nauck, B. and A. Kohlmann (1999): "Kinship as Social Capital: Network Relationships in Turkish Migrant Families," *New Qualities in the lifecourse. intercultural aspects*, 199–218.

Nieuwenhuis, R. and L. Maldonado (2018): *The Triple Bind of Single-Parent Families: Resources, Employment and Policies to Improve Well-Being*, Policy Press.

Nixon, L. A. (1997): "The Effect of Child Support Enforcement on Marital Dissolution," *Journal of Human Resources*, 159–181.

Nordin, M., G. Heckley, and U.-G. Gerdtham (2020): "Impact of a Tertiary Eligibility Threshold on Tertiary Education and Earnings: A Discontinuity Approach," *Economic Inquiry*, 58, 401–424.

OECD (2019): *Education at a Glance 2019*, Paris: OECD Publishing.

——— (2022): *International Migration Outlook 2022*, Paris: OECD Publishing.

——— (2023a): *Health at a Glance 2023*, Paris: OECD Publishing.

——— (2023b): *OECD family database: LMF1.2. Maternal employment rates*, Paris: OECD Publishing.

——— (2023c): *PISA 2022 Results (Volume I): The State of Learning and Equity in Education*, Paris: OECD Publishing.

——— (2023d): *Retaining Talent at All Ages, Ageing and Employment Policies*, Paris: OECD Publishing.

——— (2024a): *Education at a Glance 2024: OECD Indicators*, Paris: OECD Publishing.

——— (2024b): *OECD Family Database, CO2.2: Child Poverty*, Paris: OECD Publishing.

——— (2024c): *OECD Family Database, SF1.2: Children in Families*, Paris: OECD Publishing.

——— (2025a): *OECD Family Database, PF1.5 Child Maintenance (Child Support)*, Paris: OECD Publishing.

——— (2025b): *Public social spending*, Paris: OECD Publishing.

Olbrisch, M. (2024): "Wie vergleichbar sind Schulnoten wirklich?" *Der Spiegel*, 2024-02-21.

Olsen, J. M. (2024): "Swedes Take a New Step in Parental Leave. Grandparents Can Now Get Paid to Take Care of Grandkids," *The Associate Press*, 2024-07-01.

Oreopoulos, P., M. E. Page, and A. H. Stevens (2006): "The Intergenerational Effects of Compulsory Schooling," *Journal of Labor Economics*, 24, 729–760.

Oreopoulos, P. and K. G. Salvanes (2011): "Priceless: The Nonpecuniary Benefits of Schooling," *Journal of Economic Perspectives*, 25, 159–184.

Ortigueira, S. and N. Siassi (2023): "On the Optimal Reform of Income Support for Single Parents," *Journal of Public Economics*, 225, 104962.

Pelkowski, J. M. and M. C. Berger (2004): "The Impact of Health on Employment, Wages, and Hours Worked Over the Life Cycle," *The Quarterly Review of Economics and Finance*, 44, 102–121.

Penny, M. (2019): "Four Essays in the Economics of Education," *Unpublished Dissertation*.

Pilarz, A. R. and L. Cuesta (2025): "Trends in Child Support Receipt and Regularity in the United States, 1996-2018," *Journal of Marriage and Family*, 87, 797–811.

Piopiunik, M. (2014): "Intergenerational Transmission of Education and Mediating Channels: Evidence From a Compulsory Schooling Reform in Germany," *The Scandinavian Journal of Economics*, 116, 878–907.

Piopiunik, M. and J. Ruhose (2017): "Immigration, regional conditions, and crime: Evidence from an allocation policy in Germany," *European Economic Review*, 92, 258–282.

Pope, N. G. (2016): "The effects of DACAmentation: The Impact of Deferred Action for Childhood Arrivals on Unauthorized Immigrants," *Journal of Public Economics*, 143, 98–114.

Rangel, M. A. (2006): "Alimony Rights and Intrahousehold Allocation of Resources: Evidence from Brazil," *The Economic Journal*, 116, 627–658.

Raschke, C. (2016): "The Impact of the German Child Benefit on Household Expenditures and Consumption," *German Economic Review*, 17, 438–477.

RDC of the Federal Statistical Office and Statistical Offices of the Federal States, Microcensus, 10.21242/12211.2020.00.00.1.1.0 (2010-2020): *own calculations*.

Rehwald, R. (2018): "Reform des Unterhaltsvorschusses: Armutsrisiko dank Amtschaos," *taz – die tageszeitung*, 2018-02-06.

Riphahn, R. T., M. Sander, and C. Wunder (2013): "The Welfare Use of Immigrants and Natives in Germany: The Case of Turkish Immigrants," *International Journal of Manpower*, 34.

Robinson, C. D., M. G. Lee, E. Dearing, and T. Rogers (2018): "Reducing Student Absenteeism in the Early Grades by Targeting Parental Beliefs," *American Educational Research Journal*, 55, 1163–1192.

Romano, J. P. and M. Wolf (2005): "Exact and Approximate Stepdown Methods for Multiple Hypothesis Testing," *Journal of the American Statistical Association*, 100, 94–108.

Rossin-Slater, M. and M. Wüst (2018): "Parental Responses to Child Support Obligations: Evidence from Administrative Data," *Journal of Public Economics*, 164, 183–196.

Rowe, M. L. and K. A. Leech (2019): "A Parent Intervention With a Growth Mindset Approach Improves Children's Early Gesture and Vocabulary Development," *Developmental Science*, 22, e12792.

Ryan, R. M. and E. L. Deci (2000): "Intrinsic and Extrinsic Motivations: Classic Definitions and New Directions," *Contemporary Educational Psychology*, 25, 54–67.

Sadruddin, A. F., L. A. Ponguta, A. L. Zonderman, K. S. Wiley, A. Grimshaw, and C. Panter-Brick (2019): "How do Grandparents Influence Child Health and Development? A Systematic Review," *Social Science & Medicine*, 239, 112476.

Sajons, C. (2016): "Does Granting Citizenship to Immigrant Children Affect Family Outmigration?" *Journal of Population Economics*, 29, 395–420.

——— (2019): "Birthright Citizenship and Parental Labor Market Integration," *Labour Economics*, 57, 1–22.

Sajons, C. and I. Clots-Figueras (2014): "Birthright Citizenship and Education – Do Immigrant Children Need a Passport to Thrive?" *Unpublished Manuscript*.

Schaubert, M. (2023): "Do Alimony Regulations Matter Inside Marriage? Evidence From the 2008 Reform of the German Maintenance Law," *Journal of Labor Research*, 44, 145–178.

Schmitz, S. (2019): "The Impact of Publicly Funded Childcare on Parental Well-Being: Evidence from Cut-Off Rules," *European Journal of Population*, 1–26.

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.

Schuehrer, S. (2018): *Tuerkeistaemmige Personen in Deutschland: Erkenntnisse aus der Repraesentativuntersuchung Ausgewaehlte Migrantengruppen in Deutschland 2015 (RAM)*, Bundesamt fuer Migration und Fluechtlinge.

Shah, R., A. Kalil, and S. Mayer (2025): "Engaging Parents with Preschools: Evidence From a Field Experiment," *Applied Economics*, 1–14.

Skopek, J., S. Pink, and D. Bela (2012): *Starting Cohort 3: 5th Grade (SC3)–SUF Version 1.0.0. Data Manual.*

Spiess, C. K. (2008): "Early childhood education and care in Germany: The status quo and reform proposals," *Zeitschrift für Betriebswirtschaftslehre*, 1–21.

Spiess, C. K. (2022): "Kita-ökonomik – eine Perspektive für Deutschland," *Perspektiven der Wirtschaftspolitik*, 23, 25–37.

Statistisches Bundesamt (2022a): *Bevoelkerung mit Migrationshintergrund - Ergebnisse des Mikrozensus 2021 - Fachserie 1 Reihe 2.2 - 2021 (Erstergebnisse)*, Wiesbaden: Statistisches Bundesamt.

——— (2022b): *Geburten in Deutschland*, Wiesbaden: Statistisches Bundesamt.

——— (2022c): *Statistiken der Kinder- und Jugendhilfe Kinder und tätige Personen in Tageseinrichtungen und in öffentlich geförderter Kindertagespflege am 01.03.2022*, Wiesbaden: Statistisches Bundesamt.

——— (2024): "Mehr als ein Viertel der 25-Jaehrigen wohnte 2023 noch im Haushalt der Eltern," `https://www.destatis.de/DE/Presse/Pressemitteilungen/2024/06/PD24_N028_12.html`, accessed: 2025-06-02.

——— (2025): *Auslaender: Deutschland, Stichtag, Geschlecht/ Altersjahre/ Familienstand, Laendergruppierungen/ Staatsangehoerigkeit*, wiesbaden: Statistisches Bundesamt.

Statistisches Bundesamt, GESIS (2022): *Wichtige Informationen zur Nutzung des Mikrozensus Scientific Use Files 2019.*

Stichnoth, H., S. Camarero Garcia, P. Dörrenberg, C. Neisser, L. Riedel, M. Ungerer, and N. Wehrhöfer (2018): *Kommt das Geld bei den Kindern an?*, Gütersloh: Bertelsmann Stiftung.

Straub, S., S. Harun, N. Sander, N. Milewski, C. K. Spiess, and M. Weinmann (2024): *Die Bevölkerung mit Migrationshintergrund neu entdecken*, Wiesbaden: Bundesinstitut für Bevölkerungsforschung (BiB).

SVR (2018): *Steuern, was zu steuern ist: Was koennen Einwanderungs-und Integrationsgesetze leisten?: Jahresgutachten 2018*, Berlin: Sachverstaendigenrat deutscher Stiftungen fuer Integration und Migration.

Tannenbaum, D. I. (2020): "The Effect of Child Support on Selection Into Marriage and Fertility," *Journal of Labor Economics*, 38, 611–652.

Tanskanen, A. O., M. Danielsbacka, H. Hämäläinen, and A. Solé-Auró (2021): "Does transition to retirement promote grandchild care? Evidence from Europe," *Frontiers in Psychology*, 12, 738117.

Thaler, R. H. and C. R. Sunstein (2009): *Nudge: Improving Decisions About Health, Wealth, and Happiness*, Penguin.

United Nations (2022): *Transforming our World: The 2030 Agenda for Sustainable Development*, `https://sdgs.un.org/publications/transforming-our-world-2030-agenda-sustainable-development-17981`, accessed: 2022-12-13.

Usman, S. (2024): "Preparing for the Worst: Post-Divorce Instability Risk and Economic Behaviour of Households," *Unpublished manuscript*.

Verma, A. P. and V. Iyer (2021): "Female Labor Supply Response to Alimony: Evidence From Massachusetts," *SSRN*, 3899981.

Waldfogel, J. and E. Washbrook (2011): "Early Years Policy," *Child Development Research*, 2011, 343016.

Walker, I. and Y. Zhu (2006): "Child Support and Partnership Dissolution," *The Economic Journal*, 116, C93–C109.

Walper, S., C. Entleitner-Phleps, and A. N. Langmeyer (2020): "Betreuungsmodelle in Trennungsfamilien: Ein Fokus auf das Wechselmodell." *Journal for Sociology of Education & Socialization/Zeitschrift für Soziologie der Erziehung & Sozialisation*, 40.

Weinmann, M. (2022): "Barriers to Naturalization: How Dual Citizenship Restrictions Impede Full Membership," *International Migration*, 60, 237–251.

Well, D. N. (2007): "Accounting for the Effect of Health on Economic Growth," *The Quarterly Journal of Economics*, 122, 1265–1306.

Wetzel, M., N. Schumann, and C. Schmiedeberg (2021): "New Weights for the pairfam Anchor Data," *Pairfam Technical Paper*, 17.

WHO (1948): *Constitution of the World Health Organization*, `https://apps.who.int/gb/bd/PDF/bd47/EN/constitution-en.pdf?ua=1`, accessed: 2025-08-25.

Worbs, S. (2008): "Die Einbuergerung von Auslaendern in Deutschland: Integrationsreport, T.3." *BAMF-FZ, Working Paper*, 17.

Zagel, H. and H. Lohmann (2021): "Conceptualising State-Market-Family Relationships in Comparative Research: A Conceptual Goodness View on Defamilization," *Journal of Social Policy*, 50, 852–870.

Zanasi, F., B. Arpino, V. Bordone, and K. Hank (2023): "The Prevalence of Grandparental Childcare in Europe: A Research Update," *European Journal of Ageing*, 20, 37.

Zhang, J., S. Appleton, L. Song, and B. Liu (2021): "Who Looks After the Kids? The Effects of Childcare Choice on Early Childhood Development in China," *Oxford Bulletin of Economics and Statistics*, 83, 619–640.

Zhylyevskyy, O. (2012): "Spousal Conflict and Divorce," *Journal of Labor Economics*, 30, 915–962.

Zimmermann, F. (2020): "The Dynamics of Motivated Beliefs," *American Economic Review*, 110, 337–363.

Ziol-Guest, K. M., T. DeLeire, and A. Kalil (2006): "The Allocation of Food Expenditure in Married- and Single-Parent Families," *Journal of Consumer Affairs*, 40, 347–371.

Curriculum Vitae