

Hermes, Henning; Lergetporer, Philipp; Peter, Frauke; Wiederhold, Simon

Working Paper

Application Barriers and the Socioeconomic Gap in Child Care Enrollment

IZA Discussion Papers, No. 16915

Provided in Cooperation with:

IZA – Institute of Labor Economics

Suggested Citation: Hermes, Henning; Lergetporer, Philipp; Peter, Frauke; Wiederhold, Simon (2024) : Application Barriers and the Socioeconomic Gap in Child Care Enrollment, IZA Discussion Papers, No. 16915, Institute of Labor Economics (IZA), Bonn

This Version is available at:

<https://hdl.handle.net/10419/295938>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

DISCUSSION PAPER SERIES

IZA DP No. 16915

**Application Barriers and the
Socioeconomic Gap in Child Care
Enrollment**

Henning Hermes
Philipp Lergetporer
Frauke Peter
Simon Wiederhold

APRIL 2024

DISCUSSION PAPER SERIES

IZA DP No. 16915

Application Barriers and the Socioeconomic Gap in Child Care Enrollment

Henning Hermes

ifo Institute and IZA

Philipp Lergetporer

*Technical University of Munich,
TUM School of Management and IZA*

Frauke Peter

*German Centre for Higher Education
Research and Science Studies (DZHW)*

Simon Wiederhold

*Halle Institute for Economic Research (IWH),
Halle University and IZA*

APRIL 2024

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Application Barriers and the Socioeconomic Gap in Child Care Enrollment*

Why are children with lower socioeconomic status (SES) substantially less likely to be enrolled in child care? We study whether barriers in the application process work against lower-SES children — the group known to benefit strongest from child care enrollment. In an RCT in Germany with highly subsidized child care (N = 607), we offer treated families information and personal assistance for applications. We find substantial, equity-enhancing effects of the treatment, closing half of the large SES gap in child care enrollment. Increased enrollment for lower-SES families is likely driven by altered application knowledge and behavior. We discuss scalability of our intervention and derive policy implications for the design of universal child care programs.

JEL Classification: I21, J13, J18, J24, C93

Keywords: child care, early childhood, application barriers, information, educational inequality, randomized controlled trial

Corresponding author:

Henning Hermes
ifo Institut – Leibniz-Institut für Wirtschaftsforschung
an der Universität München e.V.
Poschingerstr. 5
81679 Munich
Germany
E-mail: hermes@ifo.de

* This paper is an updated version of IZA DP No. 14698 (2021). We thank the editor, Andrea Weber, four anonymous referees, as well as Sascha O. Becker, Peter Bergman, Eric Bettinger, Alexander Cappelen, David Card, Thomas Cornelissen, E. Glenn Dutcher, Simone Haeckl, Eric A. Hanushek, Sabrina Herzog, Jonas Jessen, Lukas Kiessling, Fabian Kosse, Rafael Lalive, Fanny Landaud, John A. List, Susanna Loeb, Katrine Løken, Magne Mogstad, Philip Oreopoulos, Jonas Radbruch, Anna Raute, Mari Rege, Sven Resnjanskij, Chris Roth, Jens Ruhose, Kjell G. Salvanes, Malte Sandner, Amelie Schiprowski, Nikki Shure, C. Katharina Spiess, Matthias Sutter, Bertil Tungodden, Gregory Veramendi, and Ludger Woessmann for suggestions and comments. We received valuable feedback from various seminar audiences at the ifo Institute Munich, University of Chicago, University of Essex, University of Erlangen-Nuremberg, University of Innsbruck, University of Kiel, WU Vienna, and numerous workshops and conferences. We thank our team of research assistants, interviewers, and back-office staff for excellent work, and the Ministry of Education of the State of Rhineland-Palatinate for supporting the study. Special thanks go to Fabian Mierisch for excellent support in the implementation of the survey with child care center managers. We gratefully acknowledge funding from the Jacobs Foundation, the ifo Institute Munich, and support from our home institutions and the University of Mainz. Henning Hermes acknowledges financial support by the Norwegian Research Council through its Centers of Excellence Scheme, project 262675. The study was preregistered in the AEA RCT Registry (AEARCTR-0003181). IRB approval was obtained from the Joint Ethics Committee of the Goethe University Frankfurt and the Johannes Gutenberg University Mainz.

1. Introduction

In many countries around the world, enrollment rates in universal child care are substantially lower for more disadvantaged children (OECD, 2019; Cascio, 2022; Flood et al., 2021). For instance, in Germany, the gap in early child care enrollment between parents with and without a college entrance qualification amounts to 14 percentage points (37% lower enrollment), although child care programs in Germany are universally offered, of high quality, and heavily subsidized (Jessen et al., 2020). This socioeconomic gap in enrollment is highly policy relevant, because it may render efforts to promote educational equality through offering or expanding public child care programs (see, e.g., OECD, 2011; Cascio, 2022) ineffective.¹ Yet, the causal determinants of the socioeconomic gap in child care enrollment are largely unexplored.

A growing literature argues that behavioral and informational barriers are important determinants of socioeconomic gaps in educational choices and outcomes. In particular, individuals with lower socioeconomic status (SES) may lack important information about the costs and benefits of different educational programs, the application process, or their own suitability and eligibility for such programs (Currie and Gahvari, 2008; Jensen, 2010; Bettinger et al., 2012; Hoxby and Turner, 2015). They are also more susceptible to behavioral patterns such as present bias and overreliance on routines or defaults (Lavecchia et al., 2016). These factors may distort the educational choices of lower-SES individuals and thereby exacerbate educational inequality, especially when application processes for educational programs are complex and competitive. In this paper, we study whether helping families overcome such barriers can promote socioeconomic equality in early child care enrollment.

We implemented a randomized controlled trial with more than 600 families in two large cities in western Germany. We sampled families with children aged less than one year from official birth registry data. Our setting is characterized by large socioeconomic gaps in child care enrollment despite universal, high-quality child care availability.² At the same time, application processes are decentralized, complex, and nontransparent, and

¹The positive effects of (universal) early child care programs are often larger for more disadvantaged children (e.g., Currie, 2001), which implies that they can foster equality of educational opportunity and societal equality (Bjoerklund and Salvanes, 2011; Heckman, 2011). Relatedly, Cornelissen et al. (2018) and Kline and Walters (2016) provide evidence for Germany and the U.S., respectively, that children with greater potential gains from child care programs are less likely to participate.

²Similarly, for Denmark, Heckman and Landersø (2021) highlight that advantaged families are better able to access and utilize universally available education programs, such as universal child care, and Walters (2018) shows that disadvantaged children are less likely to attend charter schools, a publicly funded and non-selective type of school in the U.S. In general, many features of the German child care

competition for a child care slot is high as, on average, demand exceeds supply (see Section 2 for details). Our treatment was designed to alleviate potential barriers to acquiring a slot in early child care (i.e., center-based child care for children below the age of three years), which we refer to as *application barriers*. Specifically, we provided information about the child care system and the application process, and offered customized assistance from a trained expert to help families to navigate the child care application process. Importantly, the treatment was based on the premise of not changing parents' preferences related to child care, which is confirmed in our data. Our randomized treatment allows us to study the causal effect of alleviating application barriers in the child care application process on application behavior and enrollment. In light of the observed SES gap in child care enrollment, we investigate treatment effects separately for lower- and higher-SES families (more than 40% of the sample are categorized as lower-SES).³

We find large, equity-enhancing effects of our treatment on child care application and enrollment. Nine months after treatment, treated lower-SES families are 21 percentage points (pp) more likely to apply for a child care slot and are 16 pp more likely to have their child enrolled in child care, compared to lower-SES families in the control group. Higher-SES parents are not affected by the treatment. In consequence, our intervention fully closes the SES gap in child care application and more than halves the SES gap in enrollment observed in the control group. Treatment effects on enrollment tend to be stronger for those lower-SES families who are likely to benefit more from alleviating application barriers (e.g., families with low initial knowledge about the child care application process).

We also analyze potential mechanisms underlying the treatment effect on child care enrollment for lower-SES families. We find that the treatment increases application knowledge and the probability of visiting a child care center on-site during the application process — an important factor for securing a child care slot, as documented in our complementary survey among child care center managers in Germany (see Section 4.3). We also observe that treated lower-SES families have a higher likelihood to obtain a child care slot *conditional on applying*. This is presumably driven by improved application knowledge and behavior; in fact, lower-SES applicants in the treatment group behave very similar to higher-SES applicants in the control group. In contrast, lower-SES applicants

system are also present in other countries, which speaks to the transferability of our findings to other contexts (see Appendix Table A1).

³ Our intervention focuses on demand-side factors contributing to the SES gap in child care enrollment. Additional supply-side factors, such as discrimination against lower-SES families, might also exacerbate this gap (Hermes et al., 2023). See Section 5 for a discussion.

in both the treatment and control groups have comparable sociodemographic attributes and baseline knowledge, indicating that selection effects are unlikely to play a role in the manifestation of the treatment effects.

We confirm our main findings in a series of robustness checks. Our main results hold when using official enrollment confirmations issued by the child care centers instead of parent-reported enrollment status. Moreover, the treatment effects remain significant when correcting for multiple hypothesis testing and when using randomization inference. In our main specification, we classify parents without a college entrance qualification as lower-SES but results are robust to applying alternative SES definitions based on the educational attainment of both parents, household income, and single-parent status. Analyzing sample attrition (which is generally low in our study), we show that attrition is independent of treatment status, and that results are robust to accounting for attrition using inverse probability weighting and bounding analysis. In addition, we investigate whether treatment effects on enrollment are driven by negative spillovers on families in the control group. Leveraging data on the exact home locations of the families in our sample, we find that child care enrollment rates in the control group are unrelated to the share of treated families in close geographical proximity. This suggests that treatment effects do not reflect displacement effects. Finally, evidence from our nationwide survey with child care center managers indicates that the use of currently unfilled child care slots (due to inefficiencies in the process of slot allocation) constitutes a likely reason why our treatment does not induce displacement effects.

The paper contributes to several streams of literature. First, we add to the vibrant strand of research that implements educational interventions informed by behavioral economics principles to improve educational choices, performance, or attainment (see Lavecchia et al. (2016); Damgaard and Nielsen (2018) for reviews of the behavioral economics of education and Dynarski et al. (2022) for the importance of non-financial barriers to college enrollment). One set of these interventions aims to overcome informational barriers that may lead students to underinvest in education by providing them with information, for instance, about the costs and benefits of education or program eligibility (Jensen, 2010; Oreopoulos and Dunn, 2013; Wiswall and Zafar, 2015). Although especially disadvantaged individuals often lack important information (e.g., Hoxby and Turner, 2015), purely information-based interventions tend to show only modest (equity-enhancing) effects (e.g., McGuigan et al., 2016; Carrell and Sacerdote, 2017; Kerr et al., 2020; Bergman and Chan, 2021; Lergetporer et al., 2021; Peter et al., 2021). Another set of studies directly targets behavioral patterns, such as present bias or overreliance on defaults, which

may yield suboptimal educational choices, particularly in situations where many choices are available and decision processes are complex (e.g., Oreopoulos and Ford, 2019; Dynarski et al., 2021). Only a small fraction of these studies offer customized application assistance, as we do; yet, this work focuses exclusively on college decisions (Bettinger et al., 2012; Castleman and Page, 2015).

Second, we contribute to the literature that targets parents to enhance children’s skills and educational success. Several interventions aim at directly improving the productivity of parental investments in children, for instance, through intensive text-messaging interventions, financial incentives, training, or information provision (Fryer et al., 2015; Bergman et al., 2018; Bergman and Chan, 2021; Cortes et al., 2019; Doss et al., 2019; Mayer et al., 2019; York et al., 2019; Bergman, 2021; Bettinger et al., 2021). This literature generally finds that modifying parental investment behavior improves child outcomes. Instead of targeting direct parental inputs into children’s human capital production, our intervention supports parents in realizing their preferences at their first decisive educational choice on behalf of their children — enrollment into early child care.⁴

Finally, our study is related to the large literature on the causes and consequences of early child care participation. Consistent with the notion that skill formation involves a process of “dynamic complementarities” (e.g., Cunha et al., 2006; Cunha and Heckman, 2007), there is ample international evidence that participation in both targeted and universal (early) child care programs can improve skills and other life outcomes, especially for children from disadvantaged backgrounds (e.g., Havnes and Mogstad, 2015; Cornelissen et al., 2018; Felfe and Lalive, 2018; Drange and Havnes, 2019; Cappelen et al., 2020).⁵ Importantly, however, little attention has been paid to the selection into child care. While some studies document substantial SES gaps in early child care attendance (e.g., Magnuson and Waldfogel, 2016; Stahl and Schober, 2018; Jessen et al., 2020), the causal

⁴In that sense, our paper is also related to the recent literature studying how complementing parental investment with a university-student mentor affects outcomes such as prosociality or labor-market prospects (Kosse et al., 2020; Resnjanskij et al., 2023).

⁵A common rationale for stronger effects on disadvantaged children’s outcomes is that these children are exposed to less stimulating home environments, and that child care attendance therefore yields a relatively larger increase in care quality for them (Cascio, 2015). Consistently, our baseline data show that children from lower-SES families are much less likely than children from higher-SES families to engage in conducive activities at home, such as reading or singing with their parents (see Section 3.2). While a substantial body of literature explores the impact of child care attendance on children’s development, with consistently positive effects found for lower-SES children (see Schmutz, 2024, for a review), less focus has been given to how specific elements of early child care programs influence their effectiveness. As such, delving into factors such as child care quality, or curricular content, and isolating exposure effects from starting-age effects represent promising avenues for future research.

determinants of these gaps are not yet well understood. We contribute to this literature by showing experimentally that reducing application barriers can substantially mitigate the SES gap in child care enrollment.⁶

The remainder of the paper is structured as follows. Section 2 describes the institutional background of early child care in Germany, explains the setting of our study, and discusses (potential) barriers in the child care application process. Section 3 presents details on the implementation of our RCT and outlines the empirical strategy. Section 4 reports our main results, subgroup analyses, mechanisms analysis, and extensive robustness checks. Section 5 concludes by discussing the implications of our results for the design of universal social policies.

2. Institutional Background

In this section, we first provide a brief overview of the institutional background of early child care in Germany. Then, we introduce our study setting and discuss potential barriers in the child care application process.

2.1. Early Child Care in Germany

In the German child care system, provision of child care is available at two age levels: early child care for children under the age of three years (*Krippe*) and child care for children aged three years and older (*Kindergarten*). The universal provision of child care for these two age levels has historically grown and has not been in place for the same period of time. Examining the evolution of the German child care market over the past 30 years, it is striking to see that today’s situation for children under age three mirrors the market in the 1990s for children aged three and above. In West Germany in the 1990s, child care slots were severely rationed, free slots were mainly granted to older children (Spiess, 2008; Alt et al., 2018), and child care attendance varied substantially by age

⁶A related strand of literature studies the impacts of extended parental leave on child development, which is closely connected to the literature on child care (see Datta Gupta and Jessen, 2023; Houmark et al., 2022). The effects of extended parental leave are mixed, with many studies finding no average effects (e.g., Rossin-Slater, 2017). Intriguingly, however, there are SES-related disparities: Longer leave periods tend to benefit higher-SES children (e.g., Liu and Skans, 2010; Danzer and Lavy, 2018) and might even disadvantage lower-SES children (Canaan, 2022). While the research on child care effects and parental leave does not align perfectly — given that the alternative to center-based child care is typically a mix of parental and informal care (Houmark et al., 2022) — there is a consistent pattern of SES differences in both fields. This consistency might be attributed to the often less stimulating home environments experienced by lower-SES children.

group. For example, in 1991, only 33% of all three-year-old and 70% of all four-year-old children attended formal child care.

One of the first “milestone” reforms to implement the universal child care system for all children aged *three years or older* was the introduction of a legal entitlement for a slot in highly subsidized child care in 1996. Due to the expansion of child care slots, the provision of slots for this age group is not rationed anymore today. In 2013, a legal entitlement to a highly subsidized child care slot for children *older than 12 months* was implemented. This provision of early child care is under scrutiny today, because somewhat similar to the situation back in the 1990s, early child care attendance today varies substantially by age group and child care slots remain severely rationed due to a supply shortage.

Early child care provision for under three year-olds (*Krippe*) is universal, that is, targeted at all children. However, actual usage is far from universal, with about one-third of children under the age of three years attending child care. Attendance rates increase sharply with age, from only 1% for children below the age of one year, 29% for one-year-olds, to 55% for two-year-olds (Autorengruppe Bildungsberichterstattung, 2020). By the time children start school (around the age of six years), almost all children in Germany have attended some form of child care.⁷ Thus, the most likely counterfactual to early child care attendance is later attendance, not completely abstaining from child care. The importance of the timing of early child care attendance is also demonstrated in a recent study by Drange and Havnes (2019), which shows that decreasing the starting age by 4 months (from 19 months to 15 months) has pronounced positive effects on child development.

Early child care in Germany is heavily publicly subsidized, with the public sector bearing about three-quarters of the total cost (Spiess, 2013). On average, parents pay effectively about 250 EUR (about 300 USD) per month for a child care slot (Felfe and Lalive, 2018), and low-income families are eligible for lower fees or fee exemptions. In the majority of cases, child care is provided either by municipalities (32%) or by publicly subsidized, privately operated non-profit organizations such as ecclesiastical or charitable organizations (50%). The remainder of child care slots are offered by private for-profit providers or companies that provide child care for their employees on their premises (Autorengruppe Bildungsberichterstattung, 2020). In general, the quality of early child care is rather homogeneous across Germany and relatively high compared with other

⁷Consistently, 99% of parents in our sample want to enroll their child into child care by the age of six years; actually, as many as 97% want to enroll their child by the age of three.

countries, for example, in terms of group sizes and child-staff ratios (e.g., Felfe and Lalive, 2018).

The early child care market in Germany is characterized by rationing and decentralized admission decisions (Jessen et al., 2020). While market characteristics vary substantially across regions, average demand for early child care slots exceeds supply: Across Germany, 44% of parents express demand for a child care slot, but only 31% of children are actually enrolled (Jessen et al., 2020). Because of their decentralized organization, child care admission processes differ across region, by type of provider, and even across individual child care centers. In cases where admission criteria are communicated, these often include (full-time) employment of both parents, single-parent status, whether a child’s sibling(s) already attend the same child care center, and — for ecclesiastical providers — religious affiliation (Herzog and Klein, 2018). Given the unstructured and nontransparent nature of child care application processes, it is often very difficult for parents to navigate the child care admission system — particularly for those who lack knowledge, time, financial resources, or social capital.

In consequence, the allocation process of child care slots is criticized as being inefficient. On the one hand, some families spend years on waiting lists before they find a child care slot, despite their legal entitlement (Carlsson and Thomsen, 2015).⁸ On the other hand, because admission decisions are often not coordinated among child care centers, some families receive offers from multiple centers for their child, blocking access and increasing waiting times for other families (Fugger et al., 2017).⁹ These inefficiencies could be the reason why a surprisingly large number of child care slots in Germany remain unfilled, despite the large excess demand for child care slots on average (see Section 4.5 for evidence on the extent of unfilled slots).

Finally, differences in child care participation by socioeconomic background are pronounced in Germany. For example, Jessen et al. (2020) document that children of parents without college entrance qualification are 14 pp ($\sim 37\%$) less likely to be enrolled in early child care compared with children of parents with college entrance qualification (this gap remains sizeable even after accounting for SES differences in demand for child care, see Jessen et al., 2020). Our paper investigates the extent to which reducing barriers in the application process causally affects this socioeconomic gap in child care enrollment.

⁸Only very few families ($<1\%$) try to sue for their legal entitlement to a child care slot in court (Jessen et al., 2020).

⁹In the past few years, some municipalities (including the cities that we study; see Section 2.3) have introduced centralized online application systems, but admission decisions usually remain decentralized and uncoordinated (Fugger et al., 2017).

2.2. Parental Leave System

In the context of family policy in Germany, parental leave and child care enrollment are closely intertwined. The policies aim at helping parents to re-enter the workforce after parental leave by offering access to publicly subsidized slots in early child care. Even before the legal right to a child care slot was expanded from children aged three to those aged one, the child care system was closely linked with the parental leave system.

The German parental leave system allows parents who were employed pre-birth the right to return to their job for up to 36 months. Introduced in 2007, the paid parental leave, known as *Elterngeld*, offers up to 12 months (or 14 months if both parents take at least 2 months of leave) at on average 67% of the pre-birth income. If both parents work only part-time, this paid leave period can be extended up to 32 months (*Elterngeld Plus*). In reality, mothers predominantly utilize these benefits. While unpaid leave is almost exclusively taken by mothers, about three-quarters of paid parental leave recipients are also mothers (2022: 77%). Even when fathers do take a paid leave, the leave period is typically shorter than for mothers (3.1 months vs. 14.0 months; see Destatis, 2022). The average duration of leave, especially for mothers, has increased due to the growing popularity of *Elterngeld Plus* (Destatis, 2022).

These institutions set the stage for parents' decisions regarding their labor market participation, child care usage, and intra-household division of both.

2.3. Study Setting

We implemented our experiment in two large cities (population > 100,000) in the federal state of Rhineland-Palatinate.¹⁰ The share of children in Rhineland-Palatinate enrolled in early child care (31%) is the same as the Germany-wide enrollment rate (Autorengruppe Bildungsberichterstattung, 2020), while enrollment in the cities that we study is somewhat lower (20–30%). There are about 200 early child care centers in the two cities combined. Regulatory and quality standards for child care centers, as well as the admission processes, are similar in both cities. Both cities use a centralized online application system, but admission decisions are taken at the center level and are not coordinated across centers. Child care in Rhineland-Palatinate is free of charge for children aged two years and older. Before that age, fees are comparatively low and vary with family income,

¹⁰The sociodemographic characteristics of the population in the two cities are relatively close to the Germany-wide average; for instance, average age is 42.4 years (compared with 44 years in Germany), mean equivalent household income is 1,610 EUR (compared to 1,880 EUR in Germany), and the fertility rate is exactly at the German average of 1.6 (cf. INKAR, 2017). We address the issue of representativeness in more detail in Section 4.5.

the number of children in the household, and the number of requested child care hours per week. Thus, in our setting, it is unlikely that child care costs are a major barrier to child care participation for lower-SES families.

2.4. *Application Barriers to Child Care Enrollment*

Application barriers may play an important role in the process of acquiring a child care slot. Competition for slots is high, application processes are complex and non-transparent, and admission decisions are decentralized. To be successful in this market environment, parents need to apply early, potentially send out several applications, and have to keep track of the various deadlines, admission decisions, and waiting lists. The process also requires completing lengthy paperwork, filing various legal documents, and coping with setbacks and rejections. Acquiring a slot is likely to be more difficult for lower-SES parents, because they usually have fewer resources available to invest in child care applications (e.g., in terms of money, social capital, or time to make major life decisions). In addition, lower-SES parents are more likely to be eligible for means-tested child care fee reductions, and applying for these reductions involves additional paperwork. Furthermore, lower-SES individuals are more likely to exhibit behavioral patterns such as present bias and overreliance on routines (e.g., Lavecchia et al., 2016), making it harder to succeed in the application process. Lower-SES parents also tend to lack relevant information about the child care application process: Our baseline data show that, compared with higher-SES parents, they are significantly less likely to know that they have a legal entitlement to a child care slot, that they do not have to apply at the nearest child care center, and that fees are waived for children aged two years and older (see Section 3.2 and Appendix Table B2).

These information asymmetries exist even though the information we provide is generally publicly available and is usually shared with parents. In the two sample cities, for example, when a child is born, the authorities distribute brochures to all parents informing them of the legal entitlement to a child care slot. The existing information asymmetries may thus — at least in part — be due to behavioral biases in the sense that (some) parents do not internalize this information. Another behavioral channel through which information interventions can work is to reduce inattention by increasing the salience of the targeted decision (Bettinger et al., 2021). Thus, the barriers that our intervention was designed to address comprise several behavioral and non-behavioral barriers that may occur in the application process. We refer to these barriers simply as *application barriers*.

For these reasons, we hypothesize that reducing application barriers by providing parents with information and application assistance can mitigate the existing socioeconomic

gap in child care enrollment.¹¹ We consider barriers in the application process as adding to other possible explanations of the SES gap in early child care enrollment, which have been found to not fully explain the gap (e.g., SES differences in parental demand, local supply shortages, and fees; see Jessen et al., 2020).

3. Study Design

In this section, we first describe the recruitment of our sample and the data collection process. Then, we present sample descriptives, the design of the treatment, randomization procedure and balancing, and the empirical strategy. We obtained IRB approval from the Joint Ethics Committee of the Goethe University Frankfurt and the Johannes Gutenberg University Mainz in July 2017, and preregistered our trial in July 2018, that is, prior to the start of the baseline data collection (including treatment design, main outcomes, targeted sample size, and a detailed pre-analysis plan for the collected data, AEARCTR-0003181).

3.1. Recruitment of Sample and Data Collection

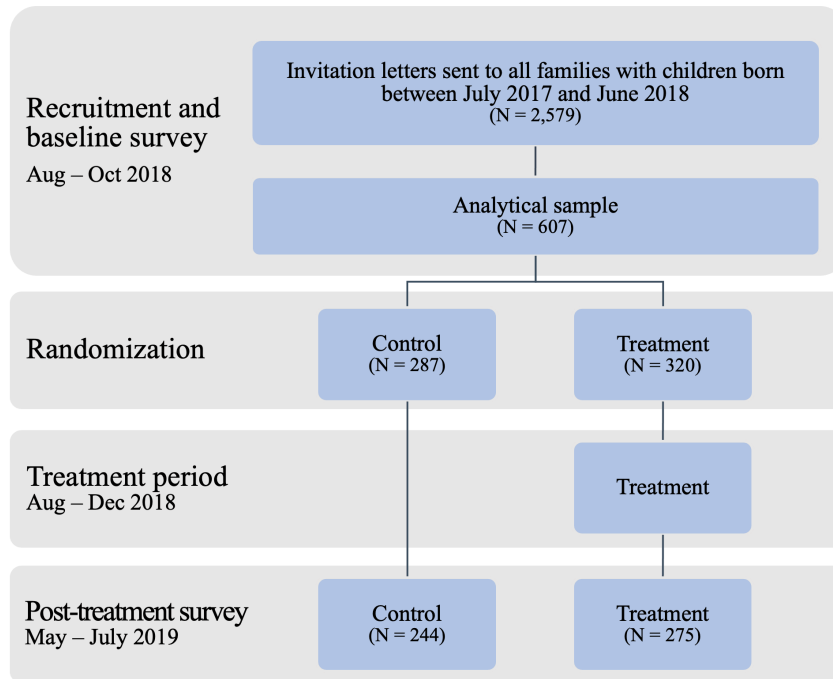
Figure 1 depicts the timeline of the study. The sample frame consists of all 2,579 families with children born between July 1, 2017, and June 30, 2018, in the two sample cities. Sampling was based on official birth registry data for the entire cohort obtained from the municipal administrations. To initiate the recruitment of the sample, all families received a postal invitation letter that informed them of the possibility of participating in a university research project on “the life of parents of young children”. The letter provided some basic information about the study, and announced that a staff member would visit the families at home to conduct the first interview.¹²

Baseline Survey. Sample recruitment and the baseline survey were conducted between August and October 2018 by 10 specially trained interviewers. We randomly assigned each

¹¹Consistent with the notion that these barriers can impede child care access, previous surveys in Germany show that parents consider a lack of information about the application process and deadlines to be an important obstacle (Camehl et al., 2018; Stahl et al., 2018). Similarly, studying barriers to child care subsidies in the U.S., Shlay et al. (2004) find that many parents do not apply because they erroneously think that they are not eligible or because they want to avoid the “hassles” of accessing the subsidy system.

¹²The letter was addressed to the child’s mother, except in the rare cases where the child lived only with the father. It included information about the study timeline, the institutions involved, the state Ministry of Education’s support of the study, and that participation was voluntary. Importantly, it did not reveal any details about the research question or the experimental nature of the study design. When communicating with parents, we always referred to the study as “ELFE-Studie (Eltern, Leben, Familie, und Erziehung)” (Parents, Life, Family, and Education), and used a professional corporate design developed by a marketing company (see Appendix Figure G1).

Figure 1: Study Timeline and Procedural Details



family in the sample frame to one of the interviewers, who visited the families at their home address. To achieve broad geographic coverage, we specified to each interviewer a different address from which to commence the recruitment tour. We instructed interviewers to give recruitment priority to parents whom we had identified as first-time parents in the registry data, because we expected that their lack of experience as parents would make them especially vulnerable to barriers in the child care application process.¹³ When first meeting a parent (in most cases the biological mother), the interviewer first inquired about her willingness to participate in the study and asked her to read and sign the consent form. Subsequently, the interviewer conducted the computer assisted baseline interview. The median interview duration was 23 minutes, and parents were paid a participation fee of 20 EUR in cash. We recruited a total of 607 families (from a population of 2,579 families) into our analytical sample, a participation rate of 24% (see Section 3.2 for a discussion of

¹³Specifically, we provided each interviewer with two unique address lists. The first list comprised only first-time parents, and interviewers were instructed to focus recruitment efforts on this list. After contacting each address on the first list, interviewers received their second list of non-first-time parents.

selection into our sample).¹⁴ The randomized treatment was administered immediately after the baseline survey (see Section 3.3).

Post-treatment Survey and Outcome Measures. We measured post-treatment outcomes between May and July 2019 through computer-assisted telephone interviews, conducted by newly recruited, trained interviewers using a university telephone laboratory. The survey was timed to capture detailed application behavior and enrollment for the child care year beginning in August/September 2019, which was the target of our intervention.¹⁵

Due to German data protection law, it is not possible to access individual administrative data on child care applications or enrollment (even if parents would consent to data linkage). Therefore, our main analysis is primarily based on parent-reported data. We took great care to ensure that the information provided was as reliable as possible: Parents were asked to provide the name(s) and address(es) of the child care center(s) to which they applied or in which their child is enrolled. Therefore, parents could not easily misreport our main outcomes of interest, and almost all parents provided the name(s) and address(es) of existing child care centers (96% for applications and 93% for enrollment).¹⁶ Our results are robust to considering only parents as having applied or as having their child enrolled who could name an existing child care center (see Appendix Table F4). Moreover, note that the question about enrollment refers to *actual* enrollment, not the *intention* to enroll. Specifically, we ask parents whether they have “accepted a slot” offered to them, which in Germany is done by signing a legally binding, written contract with the child care center. Finally, and most importantly, we verified the parent-reported enrollment information by obtaining official enrollment confirmations issued by the child care centers from the parents. Our results hold when we consider only those children as being enrolled for whom we received official enrollment confirmations (see Section 4.5 for details).

The median interview duration was 40 minutes. We paid parents a 15 EUR participation fee with vouchers for online or grocery stores or by bank transfer. Of the families who participated in the baseline survey, 86% (519 out of 607) were reinterviewed in the post-treatment survey. This is a comparatively high participation rate in general, but even

¹⁴An additional 15 families participated in the baseline survey. We subsequently had to exclude them from the study for the following reasons: four children had severe disabilities, two were not living with their parents but in assisted living groups, and nine moved out of the study area after the baseline survey.

¹⁵In Germany, the main intake of children in early child care takes place each fall. Therefore, a “child care year” is very similar to a “school year”.

¹⁶Reassuringly, there are no differences in the likelihood of naming child care centers by treatment status, conditional on having applied and being enrolled, respectively.

more so when considering the relatively high share of lower-SES families in our sample (see below). Moreover, our robustness analysis in Section 4.5 shows that our experimental results are not affected by selective attrition.¹⁷

3.2. Sample Description

This section discusses basic sample characteristics, selection into the analytical sample, and differences between lower- and higher-SES families.

Sample Characteristics. Column (1) of Table 1 shows the characteristics of our sample. All values refer to the time of the baseline survey. In total, 48% of children are female, and the average age is 6.9 months. Of those parents interviewed, 94% are the child’s biological mother, the remaining 6% are the biological father. We focus our analysis on the full sample but results are almost identical when only considering mothers (see Appendix Table F5). Respondents’ average age is 31.5 years, 40% of them have a migration background (i.e., were not born in Germany). 58% of parents have a college entrance qualification, and average net equivalent household income per month is 1,380 EUR.¹⁸ In terms of the work status at baseline, 78% of mothers are on parental leave and 7% are currently working. The table also demonstrates that the vast majority of parents (98.5%) intend to use child care before their child starts school (i.e., below age six), while 82% plan to use early child care (i.e., below age three).¹⁹ Interestingly, we find no strong link between planned enrollment age and parental leave thresholds (see Section 2.2): Only a minority of parents plan to enroll their child in child care at the end of paid parental leave, i.e., after 12 months (22% of higher-SES and 7% of lower-SES parents); merely 1% of parents plan to enroll their child after 14 months, which is the extended paid parental leave period if both parents take at least 2 months of leave. In contrast, by far the most frequent planned enrollment age in our sample is 24 months, both for higher-SES (53%) and lower-SES parents (42%), which is likely due to the fact that child care becomes

¹⁷We took several steps to minimize sample attrition. As a general survey maintenance measure, we sent all families holiday greetings cards using the study’s corporate design between both survey waves, reminded parents to participate in the post-treatment survey, and contacted them at home if barriers to participating via telephone existed. Furthermore, we conducted 21 (shorter) online interviews with participants who could not be reached by phone. Our results are robust to adding survey mode fixed effects and to excluding parents interviewed online.

¹⁸The equivalent household income is intended to reflect differences in household size and composition. We divide total household income by the number of “equivalent adults”, using the OECD equivalence scale. This scale gives a weight of 1 to the first adult in the household, 0.5 to each other person in the household aged 14 years and older, and 0.3 to each child under the age of 14 years.

¹⁹As expected, in families not planning to use early child care, mothers were less attached to the labor market prior to birth, do not plan to take up work soon, and also had lower pre-birth earnings.

Table 1: Sample Descriptives and Balancing Tests (Baseline)

	All (1)	Control (2)	Treatment (3)	$\Delta(3)-(2)$ (4)	p-val for (4) (5)	p-val by SES (6)
Pre-treatment outcomes						
Enrolled in child care	0.015	0.014	0.016	0.002	0.863	0.688
Application knowledge (# corr.)	3.448	3.477	3.422	-0.055	0.627	0.594
Application knowledge (Index)	-0.018	0.000	-0.035	-0.035	0.691	0.560
Child characteristics						
Age of child (in months)	6.869	6.992	6.758	-0.235	0.412	0.700
Child is female	0.484	0.495	0.475	-0.020	0.627	0.836
Parent characteristics						
Parent is mother	0.937	0.941	0.934	-0.006	0.745	0.559
Age of parent (in years)	31.51	31.19	31.79	0.60	0.165	0.419
Migration background	0.402	0.397	0.406	0.009	0.821	0.970
Household income	1380.7	1329.5	1426.6	97.1	0.120	0.343
No school degree	0.049	0.045	0.053	0.008	0.656	
Lower secondary degree	0.135	0.139	0.131	-0.008	0.771	
Middle secondary degree	0.229	0.237	0.222	-0.015	0.660	
College entrance qualification	0.577	0.557	0.594	0.036	0.368	
Mother currently on parental leave	0.779	0.780	0.778	-0.002	0.944	0.810
Mother currently working	0.072	0.073	0.072	-0.001	0.951	0.542
Intention to use child care (< 6y)	0.985	0.982	0.987	0.005	0.615	0.565
Intention to use child care (< 3y)	0.818	0.795	0.838	0.043	0.180	0.311
N	607	287	320			

Notes: Table reports mean values for sociodemographic characteristics in our analytical sample at baseline. Column (1) reports mean values for the full sample, Column (2) mean values for the control group, and Column (3) mean values for the treatment group. In Column (4), we display the difference between treatment and control group, and Column (5) shows the corresponding p-value of a two-sided t-test of the null hypothesis that values in Columns (2) and (3) are equal. In Column (6), we test whether there are treatment–control differences in the respective variable within SES subgroups. To do so, we regress the variable on the treatment indicator, a higher-SES dummy, and their interaction. Column (6) reports the p-value of an F-test of joint significance of the coefficients on the treatment indicator and its interaction with the higher-SES dummy. Enrolled in child care is a dummy equal to one if parents report that their child is enrolled in child care, zero otherwise. Application knowledge (# corr.) is the average number of correctly answered questions about the child care application process (out of six questions in total). Application knowledge (Index) combines answers to all six application knowledge questions to an average of z-scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007). Age of the child is the child’s age measured in months on August 1, 2018. Female is a dummy equal to one if the child is female, zero otherwise. Parent is mother is a dummy equal to one if the interviewee is the child’s biological mother, zero otherwise (remaining cases are all biological fathers). Migration background is a dummy equal to one if the parent was not born in Germany, zero otherwise. Mother currently on parental leave is a dummy variable equal to one if the mother is on parental leave at baseline, zero otherwise. Mother currently working is a dummy equal to one if the mother was working at baseline (part-time or full-time), zero otherwise. Household income is the monthly equivalent household income in EUR. No school degree, Lower secondary degree, Middle secondary degree (“MSA”), and College entrance qualification (“Abitur”) are all dummy variables indicating the parent’s highest school degree. Intention to use child care (< 6y) is a dummy variable equal to one if parents report that they want any child care for their child, zero otherwise. Intention to use child care (< 3y) is a dummy variable equal to one if parents report that they want to enroll their child into child care below the age of three, zero otherwise.

free of charge when children turn two years old in the study’s federal state. Regarding pre-treatment values of the outcomes of interest, only a small share, 1.5%, of children are enrolled in child care at time of the baseline survey.²⁰ Application knowledge at baseline is relatively low: on average, parents provide a correct answer to only 3.4 out of 6 (57%)

²⁰We deliberately chose not to elicit application behavior in the baseline survey to avoid putting too much emphasis on the study objective and thus shield against potential experimenter demand effects.

knowledge questions about the child care application process (see Appendix Table D3 for the wording of the questions).

Selection into Sample. The fact that we obtained birth registry data for the entire cohort gives us the rare opportunity to examine selection into our analytical sample. Appendix Table C1 depicts the characteristics of families who participated in the study (Column (1)) and those who did not (Column (2)), as well as the difference between the two groups (Column (3)) and the p-values from a t-test of equality of the group means (Column (4)). The samples do not differ in terms of detailed area of residence (zip-code level) or in whether the child lives with both parents. In line with our sampling strategy, the share of first-time mothers and first-time fathers in our sample is higher than in the rest of the birth cohort, and the number of siblings and the average age of parents are lower (age differences are significant only for mothers). We succeeded in recruiting a large share (41%) of children with migration background.²¹ Migration background is still higher among non-participants (51%), and the share of children with German citizenship is slightly lower (79% vs. 83%). In sum, our sample represents the characteristics of the full birth cohort well, with a slight over-representation of first-time parents (which was intended) and of non-migrant families.²²

Lower- and Higher-SES Families. As specified in our pre-analysis plan, our study sets a particular focus on differences by SES. Following previous literature on educational inequality (e.g., Bjoerklund and Salvanes, 2011; Jessen et al., 2020), we categorize families' SES based on parental education. Specifically, those 58% of responding parents ($N = 350$) who have a college entrance qualification ("Abitur") are classified as higher-SES, whereas the 42% of parents without a college entrance qualification ($N = 257$) are classified as lower-SES (see Table 1).²³ The proportion of lower-SES families in our sample reflects

²¹Note that in the registry data on the entire birth cohort, we have information on migration background for the *child*, but not for the parents (the latter information is only available for families who participated in our study). Similarly, the registry data do not contain information on education or income.

²²We also check whether the observed differences between our baseline sample and the full birth cohort affect our results. We apply propensity score weights reflecting the probability of participating in our study. The re-weighted treatment effect estimates are very similar to those in the unweighted regression, suggesting that selective participation in our study does not bias our results (see Online Appendix C and Appendix Table C2 for details).

²³Information on parental education is missing for six parents. To avoid losing these observations in our analysis, we impute missing information in five cases using information on the partner's education level, which was elicited in the post-treatment survey. This imputation is based on the idea of educational assortative mating in Germany (Eika et al., 2019). For the remaining case, we assume that the respective parent has a college entrance qualification, as this degree represents the modal education level in our data.

the German-wide share well: For example, in the representative German Socio-Economic Panel (SOEP), 47% of mothers with children aged 0–1 years are lower-SES according to our classification (Goebel et al., 2019; SOEP, 2019). Similarly, in the German Child Care Study, which is an annual representative survey of households with children under the age of three years, 48% of children are from a lower-SES household (Jessen et al., 2020). Furthermore, our results are confirmed by alternative classifications of families’ SES based on both parents’ education, household income, and single-parent status (see Section 4.5).

Using baseline data on application knowledge and parent–child activities, we can study differences between lower- and higher-SES families. The results underscore the suitability of our SES classification: on average, lower-SES parents give 0.6 fewer correct answers to our application knowledge questions (see Appendix Table B2), which is consistent with SES gaps in knowledge documented in the existing literature (Bleemer and Zafar, 2018). Moreover, children from lower-SES families are 7–12 pp less likely to regularly look at picture books, read stories, or sing songs, whereas they are 21 pp more likely to frequently watch TV or videos (see Appendix Table B3). These results suggest that lower-SES children are exposed to less stimulating home environments, and thus attending child care may yield a relatively larger increase in care quality for them (Cascio, 2015).

3.3. Treatment

The treatment was designed to address potential barriers in the child care application process (discussed in Section 2.4). It includes two components: information provision and customized application assistance.²⁴

To address parental knowledge gaps about the child care application process, each parent in the treatment group was shown a four-minute information video on the interviewers’ tablet computer, immediately after the baseline survey. The video informed that (i) all parents in Germany have a legal entitlement to a child care slot after the child’s first birthday, (ii) child care in Rhineland-Palatinate is free for all children aged two years and

Our results are robust to dropping observations with missing education information and to applying alternative imputation schemes that classify all missing cases as lower-SES or as higher-SES (results available upon request).

²⁴ A main design question when setting up the field experiment was whether to bundle both components into one treatment or have two separate treatments. While having two treatments would allow to separately assess the impact of information and assistance provision, it also comes at the cost of reduced statistical power. Based on an ex-ante power analysis, we ultimately decided for one bundled treatment to minimize the risk of obtaining imprecise and thus uninformative estimates. Therefore, our treatment effects show how the combination of information and assistance influences child care usage. Due to providing such comprehensive treatment, we are not able to causally disentangle the effects of alleviating different dimensions of application barriers (for a similar approach, see Dynarski et al., 2021).

older, with fee reductions (e.g., for lower-income families) available for children below the age of two years, and (iii) applying early and to more than one child care center increases the chance of getting a slot. The goal of our treatment was to mitigate barriers when searching for a child care slot, not to *persuade* parents to enroll their child into early child care. To respect parents' preferences, the video emphasized that (center-based) child care is only one out of several care arrangements, and it is the parents' decision alone which one to choose. Online Appendix G.2 shows screenshots of the video and the transcript translated from German to English. Treated parents also received a link and password to a non-googleable website where they could look at the information from the video again. Interviewers did not know the treatment status of the parents they were interviewing during the baseline survey. Only after parents had completed the survey, interviewers were informed by an on-screen message whether the video would be shown. We recruited new interviewers for the post-treatment survey, and the post-treatment survey was identical in treatment and control group. Thus, interviewers in the baseline and post-treatment surveys were blind to the treatment assignment, which eliminates concerns that treatment-specific interviewer effects bias our results.

Parents in the treatment group were also offered customized application assistance. We hired six student assistants and gave them intensive training on how to help parents achieve their preferred child care arrangement. The assistants' task was to provide tailored support to address the specific issues faced by each family. For instance, services included scrutinizing possible options on how to organize child care, gathering information about child care centers and their application procedures, helping with paperwork and filing applications, and reminding parents of important dates, such as open houses at child care centers or application deadlines. At the same time, assistants were instructed not to provide child care services themselves or to assist parents with tasks unrelated to child care, such as job applications. Moreover, they were instructed not to persuade parents of a particular child care arrangement. Assistants received a detailed manual that described the activities they were supposed to perform for the parents and what activities were outside the scope of their assistance duties. In addition, they were required to fill out a documentation form to provide detailed information about the tasks performed for each family they assisted.

Assistants contacted treated parents in the days following the baseline survey to inquire about demand for their services. Thus, customized application assistance was implemented as an opt-in design, giving parents the opportunity to freely choose whether to use the assistance. One-third (33%) of families in the treatment group took up the offer

of assistance, with no difference in take-up rates between lower- and higher-SES families (32% vs. 33%). Although we observe several qualitative differences between assistance takers and non-takers, only lower application knowledge is statistically significant in predicting higher assistance take-up. The treatment required a relatively modest effort from the assistants for each family involved. Based on the detailed documentation provided by the assistants, the key component of the assistance treatment was customized information delivered via telephone. Specifically, the most frequent modes of interaction between families and assistants were telephone calls (used by all families) and emails (used by two out of three families); in-person meetings were used to a lesser extent (by one out of four families). For families who took up the assistance, the median number of contacts with the assistant (phone calls, emails, or in-person meetings) was four, and the median time an assistant invested per family was 1.5 hours (mean: 2 hours). With regard to the content of the assistance, most of the interaction time was dedicated to providing parents with customized information, such as details about nearby child care centers, about costs (as well as fee exemptions), and about the legal entitlement in general. Other tasks, such as directly assisting with applications by filling out forms, occurred less frequently. Consistently, assistants themselves reported that providing customized information was most effective in helping parents secure a child care slot.

3.4. Randomization and Balancing

We used stratified randomization to assign each family in the sample frame to either the control or treatment group (Athey and Imbens, 2017). Using the birth registry data, we defined strata based on the following characteristics: city of residence (two categories), child’s birth quarter (four categories), whether the child lives with both parents (two categories), and first-time parent status (two categories).²⁵ Within these strata, we randomized families between both experimental conditions with 50% probability. In the analytical sample, the control group comprises 287 families (47%) and the treatment group 320 families (53%). The share of lower-SES families is similar in both groups (44% in the control group and 41% in the treatment group).

Table 1 shows that the randomization successfully balanced observable characteristics between the control and treatment groups (see Columns (2)–(5)). Among the 14 pairwise comparisons of pre-treatment outcomes, child characteristics, and parent characteristics, none is statistically significant at the 10% level. Because our analysis places a particular

²⁵ In the birth registry data, we observe whether the mother or father has other children up to six years of age and interpret this as a proxy for first-time parent status.

focus on treatment effects by families’ SES, we also test for balancing within the groups of lower- and higher-SES families. To do so, we regress each variable in Table 1 on the treatment indicator, a higher-SES dummy, and their interaction. Column (6) reports the p-values of F-tests for joint significance of the coefficients on the treatment indicator and the interaction term. Because we categorize families’ SES based on parental education, we do not include parental education variables in this balancing test. Reassuringly, none of the F-tests is significant, implying that the randomization procedure achieved balancing within both SES groups as well.

The balancing tests in Table 1 are based on all families who participated in the baseline survey, but baseline characteristics are also well-balanced among those participating in the post-treatment survey (see Appendix Table F7).

3.5. Empirical Strategy

We estimate the intention-to-treat effects of our intervention by ordinary least squares (OLS)²⁶ using the following regression model:

$$Y_i = \alpha + \beta_1 Treatment_i + \beta_2 Treatment_i \times HigherSES_i + \beta_3 HigherSES_i + \mathbf{X}'_i \delta + \varepsilon_i \quad (1)$$

Y_i is the outcome variable of interest for family i . As our main outcomes, we focus on binary indicators of child care application and actual enrollment which we measured in the post-treatment survey. The application dummy takes a value of one if family i has applied for a child care slot, zero otherwise. The enrollment dummy takes a value of one if family i ’s child attends child care or if the family secured a child care slot for the future, zero otherwise (see Section 3.1 for details on how we elicit and verify these two outcome variables). The variable $Treatment_i$ is the treatment indicator. $HigherSES_i$ is an indicator for higher-SES families (from the baseline survey), which takes a value of one if the respondent has obtained a college entrance qualification (“higher-SES”), and zero otherwise (“lower-SES”) (also see Section 3.2). The intention-to-treat effect of the treatment for lower-SES participants is given by β_1 . The coefficient β_2 indicates how the treatment effect differs between higher- and lower-SES participants, whereas the treatment effect on higher-SES families is given by $\beta_1 + \beta_2$.

Following our pre-analysis plan, we include a vector of control variables obtained from the baseline survey, X_i , in order to increase the precision of our treatment effect estimates. The control variables include strata fixed effects, baseline values of the respective

²⁶Logit and probit estimations yield qualitatively similar results.

outcome (if available)²⁷, child age and gender, age, gender, and migration background of the responding parent, baseline values of parental employment status, log equivalent household income, as well as zip-code and survey date fixed effects.²⁸ In the few cases in which control variables have missing values, we impute missings with the sample mean, and add imputation dummies to the regressions.

Finally, ε_i denotes the error term. Inference is based on robust standard errors. The results also hold when using randomization inference, which randomly reassigns treatment status within strata, and when correcting for multiple hypothesis testing (see Section 4.5).

4. Results

This section presents the experimental results. We begin by estimating the effects of our treatment on the probability of applying for child care and enrollment in child care (Section 4.1). Then, we examine heterogeneity in treatment effects across subgroups (Section 4.2), present a mechanism analysis (Section 4.3), and conduct a series of robustness tests (Section 4.5).

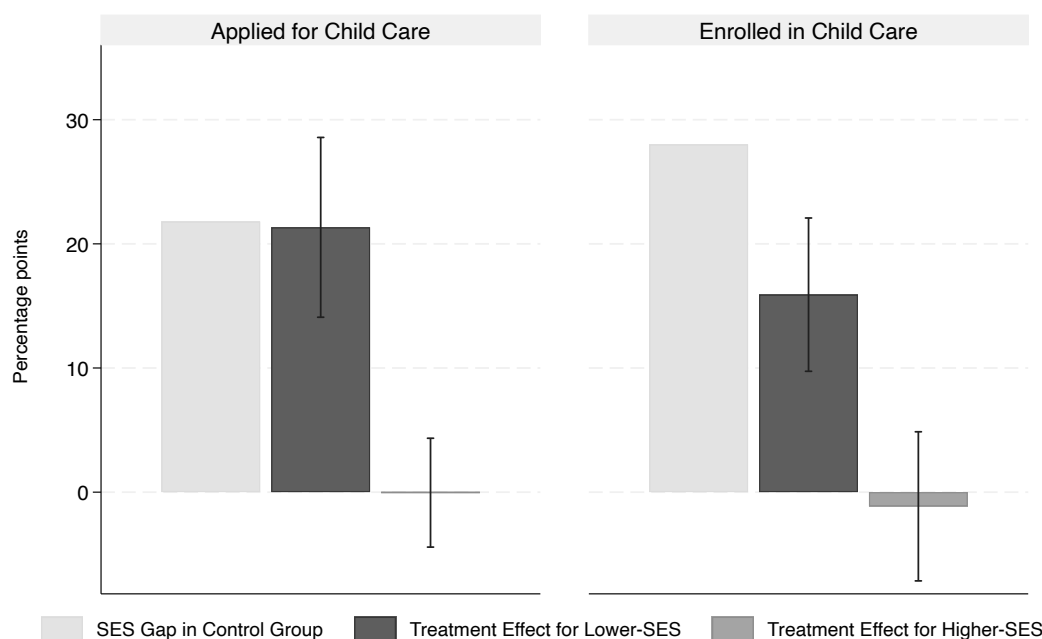
4.1. Main Results

Figure 2 presents our main results. The left panel of the figure is based on Column (2) of Table 2 and depicts treatment effects on the probability of applying for a child care slot, separately for lower- and higher-SES families. For lower-SES families, the treatment increases application rates by 21 pp (blue bar). The treatment does not affect higher-SES families’ application rates (green bar). Thus, the intervention almost entirely closes the control-group SES gap in application rates of 22 pp (grey bar; see the bottom part of Table 2 for details).

²⁷Since we did not elicit application rates in the baseline survey to conceal the aim of our study, we use baseline enrollment as the pre-treatment outcome for application. The results are almost identical without this control.

²⁸ Survey date fixed effects refer to the post-treatment survey. We include them because the data collection phase was relatively long (from May to July 2019), and timing is important in the child care application process. In particular, enrollment decisions in the German child care market exhibit a “bulky” pattern, concentrated mainly around May/June each year, creating an inherently non-linear timing effect that can only be adequately captured using detailed fixed effects. However, we checked the robustness of our results to the inclusion of survey dates in various ways. Treatment effects are robust to controlling for survey date time trends instead of fixed effects, to restricting the sample to the first half of the sampling period (in which survey dates are less spread out in time), and to restricting the sample to survey dates with an above-average number of observations per date. Results are also robust to not controlling for survey dates.

Figure 2: Treatment Effects on Child Care Application and Enrollment



Notes: Figure shows intention-to-treat effects for the subgroups of lower- and higher-SES families, based on OLS models shown in Table 2. The left-hand panel depicts treatment effects on the share of parents who applied for a child care slot; the right-hand panel shows treatment effects on the share of parents who enrolled their child in child care. Outcomes are collected in the post-treatment survey nine months after the treatment. To benchmark the size of treatment effects, we also plot the raw SES gap in the control group (i.e., the difference in the shares of lower- vs. higher-SES parents who applied for or enrolled their child in child care). For both outcomes, the treatment effects for lower-SES families are significantly larger than for higher-SES families ($p = .013$ for applied and $p = .046$ for enrolled, see Columns (2) and (4) of Table 2). Error bars show robust standard errors.

The right panel of Figure 2, which is based on Column (4) of Table 2, shows treatment effects on actual child care enrollment. The treatment increases enrollment rates for lower-SES families by 16 pp (blue bar), and again does not affect higher-SES families (green bar). In consequence, our intervention closes more than half of the control-group SES gap in enrollment rates of 28 pp (grey bar).²⁹ We discuss potential reasons why the treatment

²⁹We also estimate the treatment effect on the average weekly hours that children spend in child care (with hours of non-enrolled children counted as zero). We find that the treatment increases child care attendance of lower-SES children by 4.4 hours, closing more than half of the control-group SES gap of 7.6 hours (see Appendix Table D1). This treatment effect is completely driven by families enrolling their child in child care (extensive margin) instead of enrolled families extending the number of child care hours (intensive margin).

Table 2: Treatment Effects on Child Care Application and Enrollment

	Nine Months After Treatment			
	Applied (1)	Applied (2)	Enrolled (3)	Enrolled (4)
Treatment	0.078 (0.038)	0.213 (0.072)	0.051 (0.044)	0.159 (0.062)
Treatment \times Higher-SES		-0.214 (0.086)		-0.171 (0.085)
Higher-SES	0.069 (0.045)	0.179 (0.066)	0.133 (0.048)	0.221 (0.064)
Strata FE	Yes	Yes	Yes	Yes
Further Controls	Yes	Yes	Yes	Yes
Treatment Effect Higher-SES		-0.000 (0.044)		-0.011 (0.060)
Control Mean Higher-SES		0.857		0.497
Control Mean Lower-SES		0.639		0.216
Control Group SES Gap		0.218		0.280
N	519	519	519	519

Notes: Table shows intention-to-treat effects on child care application and enrollment, all models are estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. In Columns (1) and (2), the outcome variable takes a value of one if respondents state that they have applied for child care and zero otherwise; in Columns (3) and (4), the outcome variable takes a value of one if respondents state that their child is enrolled in child care and zero otherwise. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. We additionally report p-values based on randomization inference and correcting for multiple hypothesis testing in Panel A of Appendix Table F2. Robust standard errors in parentheses.

entirely closes the SES gap in child care application but only partially closes the SES gap in enrollment in Section 5.

While we pre-specified to focus our analysis on separate treatment effects by families' SES, Columns (1) and (3) of Table 2 also present average treatment effects in the full sample. Naturally, average treatment effects on child care application (8 pp; $p = .041$) and enrollment (5 pp; $p = .249$) are between the separate effects for lower- and higher-SES families. However, these average effects conceal the high effectiveness of our intervention for lower-SES families.

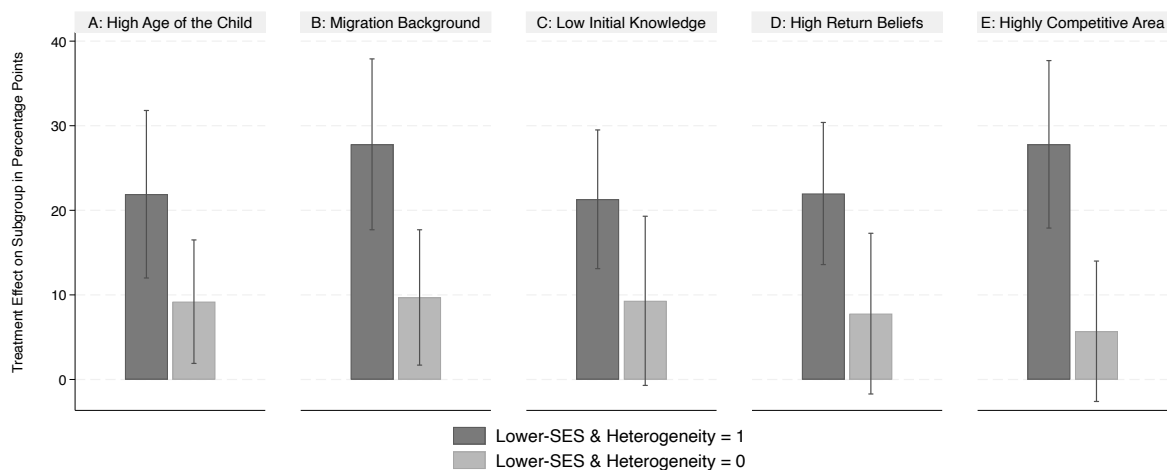
Why are the treatment effects on the application probability of lower-SES families larger than those on the enrollment probability (see Figure 2)? To understand this pattern, recall that the child care market is rationed and child care center managers have considerable leeway in their decision whom to enroll (see Section 2). Thus, not every application will lead to actual enrollment, despite the legal entitlement for a child care slot. In fact, in the control group, the enrollment probability, *conditional* on applying, is 58.0% for higher-SES families and 33.8% for lower-SES families (see bottom of Table 2). If this pass-through rate from applications to enrollment was not affected by the treatment, we would expect a treatment effect on enrollment for lower-SES families equal to the application effect (21.3 pp; see Column (2) of Table 2) multiplied by the success rate for lower-SES families in control group (33.8%), resulting in 7.3 pp. However, the actual treatment effect on enrollment is more than twice as large (15.9 pp; see Column (4) of Table 2), implying that the treatment improved enrollment probabilities conditional on applying. The higher success rate in the treatment group compared to the control group may be due to selection effects (i.e., families with an above-average success probability apply for child care) or improved application knowledge and behavior. We discuss the relevance of these two explanations in Section 4.3.

4.2. Heterogeneity of Treatment Effects

As an explorative analysis, we now investigate potential treatment effect heterogeneities for lower-SES families along baseline characteristics that might moderate the effects of our intervention: family characteristics such as child age, migration background, baseline knowledge about child care, beliefs about the degree to which child care promotes child development, and competition for child care slots in the families' neighborhood, as well as a set of maternal pre-birth employment characteristics. For each of these variables, we add a triple interaction between treatment, SES, and an indicator for the respective subsample (using median splits when possible) to Equation (1).

Family Characteristics. First, we present treatment effect heterogeneities by several characteristics potentially associated with the severity of application barriers faced by families. Figure 3 depicts the results for lower-SES families, whereas the full estimation results are presented in Appendix Table D2. For all considered dimensions of heterogeneity, we find that treatment effects on enrollment tend to be considerably larger for those lower-SES families who are likely to benefit more from alleviating application barriers. While treat-

Figure 3: Heterogeneous Treatment Effects on Child Care Enrollment By Family Characteristics



Notes: Figure shows heterogeneous treatment effects (ITT) on child care enrollment for different subgroups within the lower-SES sample, based on OLS models using triple interactions (see Appendix Table D2 for details). Within each panel, the left-hand bar shows the estimated treatment effect for the subgroup of lower-SES families to which the respective heterogeneity applies (e.g., those with children with above-median age in Panel A); the right-hand bar shows the treatment effect for the remaining lower-SES families (e.g., those with children with below-median age in Panel A). Outcomes are collected in the post-treatment survey nine months after the treatment. We additionally report p-values based on randomization inference and correcting for multiple hypothesis testing in Panel B of Appendix Table F2. Error bars show robust standard errors.

ment effects for lower-SES families in the other subsamples remain positive, they are rather modest in size, and none of them is statistically significant.³⁰

As parental demand for an early child care slot increases sharply with child age (Jessen et al., 2020), our treatment is likely to be more relevant for parents of relatively older children. Splitting the sample by median age, we find that the treatment effect for lower-SES children of above-median age (on average, 20 months in post-treatment survey) is 21.9 pp ($p = .027$, see Panel A of Figure 3). The heterogeneity by child age might be particularly pronounced in our study setting, as child care in Rhineland-Palatinate is free of charge for children aged two years and older (see Section 2.3).

Families who are less familiar with the child care market may benefit more from the support our intervention provides. One such group may be families with migration

³⁰A similar pattern of heterogeneous treatment effects emerges for child care applications. However, differences are generally less pronounced than for enrollment. Results are available upon request.

background, for example, due to potential language barriers or lower social capital.³¹ Measuring migration background by whether the interviewed parent was born in Germany or elsewhere, we find a substantial treatment effect of 27.8 pp for lower-SES migrant families ($p = .006$, see Panel B of Figure 3). As a more direct proxy for familiarity with the child care market, Panel C splits the sample along baseline application knowledge. The treatment significantly increases enrollment by 21.3 pp for lower-SES families with below-median knowledge ($p = .010$). Overall, these heterogeneities suggest that the treatment may be compensating for a lack of familiarity with the child care market.

The treatment may also be more effective for parents who believe that child care yields high returns for their child with respect to the cognitive and social development. To analyze heterogeneities by parental beliefs about the returns to child care attendance (measured at baseline), we use parents’ agreement with the following statement (elicited on a 7-point Likert scale): “Children who have attended child care are better developed at school entry than children who have not.” Panel D of Figure 3 shows a treatment effect of 22.0 pp on enrollment rates for lower-SES parents with above-median return beliefs to child care attendance ($p = .009$). These results are in line with the literature highlighting the importance of parental beliefs about the technology of skill formation in explaining variation in early child care investment (e.g., Cunha et al., 2022).

The effectiveness of our intervention may also depend on the child care market conditions in a family’s neighborhood. In particular, we expect treatment effects to be larger in areas with greater competition for child care slots, because shortcomings in child care applications (e.g., lacking relevant information or missing deadlines/visiting days) may be particularly costly in these tight markets. To calculate a proxy for competition for slots, we use detailed spatial information on the location of the child care centers and the entire cohort of families with children aged 0–1 years at baseline (i.e., our sample families and their potential “competitors”). We divide the number of child care centers within a one-mile (1.6 km) radius of each family’s home by the number of children aged 0–1 years at baseline living in that area. On average, there are 6.4 child care centers per 100 children (median: 6.5), and the 10–90 percentile range is 2.6–9.8. We define areas with “high” competition as those with a within-city below-median number of child care centers per 100 children. The treatment effect on enrollment for lower-SES families living in highly competitive areas is 27.8 pp ($p = .005$, see Panel E of Figure 3). Thus, addressing

³¹In Germany, enrollment rates for families with migration background are 12 pp lower than for native families, despite very similar demand for child care (see Figure 2 in Jessen et al., 2020).

barriers in the child care application process is particularly helpful when it is difficult for (lower-SES) families to acquire a slot as a result of strong competition.

Maternal Pre-Birth Employment Characteristics. Early child care may also be a means for parents, in particular, for mothers, to be able to get back to the labor market (also see Section 4.4 below). We thus also investigate effect heterogeneities with respect to maternal pre-birth employment, elicited in the baseline survey (note that we observe pre-birth employment characteristics for mothers only). Mothers were generally quite strongly attached to the labor market before child birth, as 69% of mothers worked prior to giving birth and 46% of them were in full-time employment (i.e., worked at least 30 hours per week; see OECD, 2021). There were also evident SES disparities in maternal employment. Specifically, with an employment rate of 63%, lower-SES mothers were 11 percentage points less likely to be employed than their higher-SES counterparts (74%). This disparity is even somewhat larger for full-time employment.

Intriguingly, Appendix Figure D1 reveals that treatment effects on enrollment do not crucially depend on pre-birth work status (Panels A and B). This result aligns with the observation that a non-negligible share of mothers who did *not* work pre-birth (e.g., because of older children) plans to join the workforce within the next year. These mothers will likely require child care to realize their employment aspirations. Panel C reinforces this idea, showing pronounced treatment effects among lower-SES mothers who plan to (re-)join the workforce within the next year, and negligible effects for mothers who plan to return to the labor market later or not at all. Finally, Panel D shows that the treatment was more effective for lower-SES mothers in the lower earnings bracket pre-birth. This resonates with Figure 3, highlighting greater benefits from the treatment for migrants and those less acquainted with the child care system — groups typically represented by low-wage earners.

Our explorative heterogeneity analysis reveals several plausible channels through which our treatment effects on enrollment materialize, which may be important for designing policies to improve equity in child care admissions. Despite large heterogeneities across different subgroups, sample sizes and statistical power for subgroup analyses are limited, so these results should be interpreted with some caution. In fact, despite the economically large differences in treatment effects across most of the considered subgroups of lower-SES families in Figure 3 and Appendix Figure D1, the difference is statistically significant at conventional levels only when comparing areas with low vs. high competition for child care slots ($p = .094$; for mothers who plan to return soon vs. later or not at all, the difference is close to statistical significance, $p = .123$).

4.3. Potential Mechanisms

In this section, we examine possible mechanisms through which the treatment affects child care enrollment. First, we analyze treatment effects on pre-specified intermediate outcomes. We primarily consider application knowledge and application behavior as potential mechanisms, as these dimensions are addressed by our intervention and are likely to be important for securing a child care slot. We then offer a descriptive, exploratory analysis of application behavior. This analysis reveals that treated lower-SES applicants exhibit behaviors remarkably similar to higher-SES applicants in the control group, but different from lower-SES applicants in the control group. Finally, we show that, in line with the design of our intervention (see Section 3.3), treatment effects are very unlikely driven by changes in parental preferences.

Application Knowledge. A key component of our intervention is reducing potential knowledge barriers to child care usage by providing relevant information about the child care application process. Using a standardized index of application knowledge in the post-treatment survey as an outcome, we do indeed find strong positive treatment effects on parental knowledge (see Column (1) of Table 3). For lower-SES parents, the treatment increases the application knowledge index by 30% of a standard deviation. However, the treatment effect for higher-SES parents is even (albeit non-significantly) larger at 40% of a standard deviation, which suggests that treatment-induced knowledge improvements are unlikely to be the main reason why the intervention mitigates the SES gap in child care enrollment.³²

Application Behavior. Since the treatment involves information about application strategies and customized application assistance, altered application behavior is another possible mechanism underlying the treatment effect on child care enrollment. Aside from the large treatment effect on application rates (see Section 4.1), we focus here on actions during the application process that are generally not mandatory, but may still be beneficial for obtaining a child care slot: on-site visits to child care centers (e.g., at open houses), as well as phone calls and emails sent to child care centers. We hypothesize that on-site visits, in particular, are important for obtaining a slot, which is based on anecdotal evidence and on corroborating results from our survey with 440 child care center managers across

³²Appendix Table D3 shows that results hold when using alternative methods for calculating the knowledge index, that is, an index based on a principal component analysis allowing for unequal weighting of the six knowledge questions (Column (2)) and the simple sum of correct answers (Column (3)). Examining the effects on each knowledge question separately in Columns (4)–(9) reveals that the treatment primarily improves knowledge about the age for the legal entitlement and child care costs.

Table 3: Treatment Effects on Application Knowledge and Behavior

	Application Knowl.	Application Behavior		
	Index (1)	Visited (2)	Called (3)	Emailed (4)
Treatment	0.299 (0.151)	0.259 (0.078)	0.086 (0.077)	-0.079 (0.058)
Treatment \times Higher-SES	0.103 (0.190)	-0.272 (0.095)	-0.083 (0.094)	0.146 (0.072)
Higher-SES	0.064 (0.135)	0.190 (0.074)	0.170 (0.070)	-0.051 (0.057)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect Higher-SES	0.402 (0.115)	-0.013 (0.055)	0.002 (0.058)	0.068 (0.044)
Control Mean Higher-SES	0.143	0.731	0.397	0.116
Control Mean Lower-SES	-0.217	0.495	0.258	0.208
Control Mean SES Gap	0.361	0.236	0.140	-0.092
N	519	515	516	515

Notes: Table shows intention-to-treat effects on application behavior, all models are estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. In Column (1), the outcome is an index that combines answers to all six application knowledge questions to an average of z -scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007). In Column (2), the outcome is a dummy equal to one if parents visited a child care center on-site during the application process, zero otherwise. We also report results for dummy variables equal to one if parents called a child care center (Column (3)) or contacted a child care center by email (Column (4)) during the application process (zero otherwise). *Further controls* include baseline outcome value for application knowledge (in Column (1)), survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. We additionally report p-values based on randomization inference and correcting for multiple hypothesis testing in Panel C of Appendix Table F2. Robust standard errors in parentheses.

Germany.³³ Column (2) of Table 3 shows that the treatment significantly increases the probability for lower-SES families to visit child care centers on-site during the application process by 26 pp. The treatment does not affect the probability of visits from higher-SES

³³In the survey, 74% of child care center managers state that visits are “very” or “somewhat” important for obtaining a child care slot in their facility. The respective figures for phone calls and emails are 48% and 38%; see Online Appendix F.4 for details about the survey. Comparing the frequency of use of these communication channels in the control group of our experimental sample (Columns (2)–(4) of Table 3), we observe that, on average, lower-SES parents are less likely than higher-SES parents to visit a child care center on-site but are more likely to send emails to child care centers).

families, implying that it closes the substantial control-group SES gap in on-site visits of about 24 pp. By contrast, the intervention has no effect on calling or emailing child care centers, suggesting that these actions do not constitute channels through which the treatment affects child care enrollment.

An alternative to estimating treatment effects on binary indicators of child care application and application behavior is to use the *numbers* of applications, visits, phone calls, and emails as outcome variables. Consistent with our findings for the binary outcomes, Appendix Table D4 shows that the treatment increases the number of applications and on-site visits for lower-SES families. This is also in line with the fact that our treatment explicitly encouraged parents to apply to more than one child care center (see Section 3.3).³⁴

Why Are Treated Lower-SES Families More Successful? We can think of two primary reasons to explain the elevated success rate of lower-SES families in the treatment group relative to the control group: Selection effects (i.e., families with an above-average success probability decide to apply for child care) and enhanced application quality. Focusing on applicants for child care, Appendix Table D7 juxtaposes lower-SES families in treatment and control groups in terms of both baseline characteristics and post-treatment knowledge and application behavior. As a benchmark, the table also includes higher-SES families in the control group.

The comparison in Appendix Table D7 showcases that lower-SES applicants in treatment and control groups are very similar in all sociodemographic attributes and baseline knowledge, suggesting that selection effects are unlikely to matter for the treatment effects to materialize. Higher-SES applicants, on the other hand, want to enroll their children at an earlier age and tend to be older, wealthier, and more knowledgeable about the child care market at baseline than lower-SES applicants in both treatment and control groups.

Instead, Appendix Table D7 emphasizes that the treatment led to changes in post-treatment application knowledge and behavior, consistent with the previous discussion in this section. Compared to the control group, lower-SES applicants in the treatment group exhibit greater application knowledge ($p = .017$), are more inclined to visit child care centers ($p = .130$), and send fewer emails to these centers ($p = .001$). Remarkably,

³⁴Note, however, that these count variables are prone to dynamic selection bias, as families are likely to stop sending applications and contacting child care centers once they have obtained a slot (see, e.g., Marinescu and Skandalis, 2021, for a discussion of dynamic selection in the context of job search behavior). Therefore, these results need to be interpreted with some caution.

when considering post-treatment knowledge and application behavior, treated lower-SES applicants are statistically indistinguishable from higher-SES applicants in the control group. This is true for each individual post-treatment variable in Appendix Table D7 and when jointly testing for differences in all five post-treatment variables listed in the table ($F = 0.4$, see Column (6)). These results indicate that the increased enrollment success among lower-SES families in the treatment group stems from improvements in application behavior and quality, rather than selection effects.

Moreover, the fact that the treatment increases application behavior conducive to enrollment that was *not* mentioned in the information video, such as the importance of on-site visits, combined with the observation that the key component of the assistance was customized information (see Section 3.3), point to the importance of the assistants for the treatment effects to materialize. Instead, providing generic information *alone* was unlikely sufficient to increase child care enrollment for lower-SES families. This finding echoes recent evidence from Bergman et al. (2023), who explore the impact of tailored support on low-income families moving to high-upward-mobility neighborhoods.

Parental Preferences. Information about the legal entitlement for a child care slot or subsidized child care fees could, in principle, affect parents' plans if and when to enroll their child in early child care or their willingness to pay (WTP) for child care. While the goal of our treatment was to address the potential application barriers that lower-SES families might face when trying to obtain a child care slot, but *not* to persuade parents to adopt a particular child care arrangement (see Section 3.3 for details), these plans could nevertheless have changed and, as such, explain why treatment effects have materialized. However, Appendix Table D5 shows that the treatment does not affect parents' plans when to enroll their child in child care, measured either as the planned enrollment age in months or as indicator variables for the age cutoffs of two or three years (Columns (1)–(3)). Similarly, parents' stated WTP for a child care slot does not differ between treatment and control groups (Column (4)). These findings suggest that changes in parental preferences are unlikely to be a channel through which the treatment effect on enrollment materializes.³⁵

³⁵Additional evidence that our treatment has not changed parents' child care intentions comes from Appendix Table D6, which shows treatment effects by enrollment intentions reported at baseline. We observe strong treatment effects for lower-SES families intending to enroll their child by the age of two years (Column (1)). These families can be expected to be especially responsive to the treatment because they were likely about to start or amidst the child care application process at the time of our intervention. The treatment effect of 15.3 percentage points is close to the effect in the entire lower-SES group (see Column (4) of Table 2), indicating that this group of lower-SES families (more than three-quarters (76.5%)

4.4. *Exploratory Analysis of Short-run Effects on Child and Family Outcomes*

In an exploratory analysis, we also evaluate the short-run effects of our treatment on child and family outcomes. We draw on parent-reported items on child development and parent-child interactions included in our post-treatment survey.³⁶

Results are reported in Appendix Tables E2 and E4. First, for lower-SES children, our intervention improves an index of child development by 25% of a standard deviation. Treatment effects are somewhat more pronounced for motor skills than for cognitive skills. Second, for lower-SES families, our intervention tends to increase an index of conducive parent-child activities by 28% of a standard deviation, suggesting that parents view child care centers as a complement to (rather than a substitute for) their own investments in their children. In Online Appendix E, we provide further results, a critical assessment of parent-reported measures of child development, and a discussion of potential explanations for the existence of short-run treatment effects.

In addition, our intervention also affects lower-SES families more broadly. In a separate follow-up survey 18 months post-treatment (when targeted children were 2–3 years old), we collected information on families’ labor market outcomes (see Hermes et al., 2022). We find that our intervention boosts lower-SES mothers’ full-time employment (+9 pp or +160% relative to the control group mean), their earnings (+22%), and family income (+10%). Further, analyzing the intra-household division of working hours, care hours, and earnings between mothers and fathers, the intervention increases an index of gender equality within the household by 40% of a standard deviation.

Taken together, these treatment effects on child and family outcomes underscore the relevance of our intervention for important life outcomes beyond child care enrollment. Additionally, our findings closely resonate with existing literature on the impacts of child care on child development, parental investment, and parental employment (e.g., Baker et al., 2008; Gelber and Isen, 2013; Bauernschuster and Schlotter, 2015; Felfe and Lalive, 2018; Cornelissen et al., 2018), further bolstering the external validity of our research.

4.5. *Robustness*

Official Enrollment Confirmations. Our results thus far are based on parent-reported data. As discussed in Section 3.1, we have asked parents to name the child care centers

of families) were driving the treatment effect on child care enrollment. In contrast, the effect is small and statistically insignificant for parents who planned to enroll their child after age two (Column (2)).

³⁶Both sets of questions are taken from nationally representative surveys. We compare our experimental data against these large, nationwide samples in Appendix Tables E1 and E3 (see Online Appendix E for details).

(and addresses) to which they applied or in which their child is enrolled in order to increase data reliability. First of all, we do not find treatment differences in the reporting of names or addresses of child care centers.³⁷ Further, if we only count children as having applied or being enrolled if their parents name an existing child care center, results do not change (see Appendix Table F4). However, misreporting could still be a concern. Since data protection laws in Germany prohibit the matching of administrative records of child care enrollment, we asked parents at the very end of the post-treatment survey to send us copies of their child’s official enrollment confirmation, a formal document which is issued by the child care centers (we offered parents a cash incentive of 10 EUR to compensate them for their effort). Overall, 49% of parents who reported that their child was enrolled sent us such a confirmation.³⁸ This share does not differ significantly by treatment status (conditional on reporting enrollment, 51% in the treatment group and 46% in the control group send the official enrollment confirmation, $p = .426$), which again mitigates concerns about biased reporting in the treatment group (e.g., due to experimenter demand effects). In Appendix Table F1, we estimate treatment effects on child care enrollment when considering only those children as enrolled whose parents sent an official enrollment confirmation. Children of parents who reported enrollment but did not provide confirmation are either classified as not enrolled (Column (1)) or, given that we cannot confirm their enrollment status with official documents, excluded from the analysis (Column (2)). Reassuringly, the treatment effect remains large and highly significant in both specifications. In fact, in Column (2), the treatment effect is virtually identical to that in the main specification (see Column (4) of Table 2), despite the substantially smaller sample. Overall, we interpret these additional results as strong support for the reliability and unbiasedness of our parent-reported outcomes.

Multiple Hypothesis Testing and Randomization Inference. Because we use multiple outcomes and test for several heterogeneous treatment effects (see Figure 3), we have to adjust for multiple hypothesis testing (MHT). We run three different versions of MHT corrections, namely those suggested by List et al. (2019), Westfall and Young (1993), and

³⁷Conditional on having applied, 96% in the treatment group name at least one center to which they have applied, 96% in the control group do so; conditional on being enrolled, 94% in the treatment group name the center where their child is enrolled, 92% in the control group do so.

³⁸Because child care enrollment documents contain sensitive information, it is not surprising that a sizable share of parents chose not to provide us with the official enrollment confirmation. The fraction of parents willing to share this confirmation is consistent with the fractions observed in the literature on individuals’ willingness to share private information (see, e.g., Acquisti, Alessandro and John, Leslie K. and Loewenstein, George, 2013; Schudy and Utikal, 2017).

Romano and Wolf (2005, 2016). As an additional robustness check, we conduct randomization inference (cf. Young, 2019) which, in essence, randomly reassigns the treatment status within strata and tests the “true” treatment effect against the distribution of randomly reassigned treatment effects. Results and details of the methodology are presented in Appendix Table F2. All treatment effects for lower-SES families remain significant at the 5%-level or better when applying MHT corrections and randomization inference, respectively.³⁹

Alternative Definitions of Lower-SES Background. Our classification of families’ SES based on parental education follows a standard approach in the literature on educational inequality (e.g., Bjoerklund and Salvanes, 2011; Jessen et al., 2020). Below, we test the robustness of our findings when applying alternative definitions of “lower-SES”. Our main definition of lower-SES families used throughout the paper is families in which the responding parent does not have a college entrance qualification (SES-1, 42% of the sample). We provide results for three alternative definitions: i) neither of the two parents has a college entrance qualification (SES-2, 30% of the sample), ii) either SES-2 or equivalent household income is below the poverty line (SES-3, 45% of the sample, based on Falk et al. (2021)), and iii) either SES-3 or single-parent status (SES-4, 46% of the sample, following Kosse et al. (2020)). Appendix Table F3 presents our main results when applying these different SES definitions. We find that the treatment effects for lower-SES families remain statistically significant for all SES definitions. While the application effects of our treatment decrease somewhat when expanding the group of lower-SES families (for definitions SES-3 and SES-4), enrollment effects remain very similar across the alternative SES definitions and are always significant at the 1%-level. Overall, this exercise demonstrates that the documented treatment effects on lower-SES families are not driven by any particular definition of lower-SES background.

Attrition. A detailed attrition analysis in Online Appendix F.1 shows that the probability of participating in the post-treatment survey is independent of treatment status and is not selective with respect to baseline outcomes in the full sample or in the subsamples of lower- and higher-SES families. Moreover, observable characteristics remain well-balanced across treatment and control groups among those parents who participated in the post-treatment survey. We also show the robustness of our results to re-weighting the observed

³⁹The only exception is the treatment effect on application knowledge, which is significant only at the 15%-level when correcting for MHT ($p = .134, p = .112, p = .113$, respectively). However, randomization inference still yields a statistically significant treatment effect on application knowledge ($p = .051$).

data using inverse probabilities of participation in the post-treatment survey and to the bounding approach suggested by Lee (2009). Hence, attrition is unlikely to bias our treatment effect estimates.

External Validity. Next, we assess the external validity of our results. In Section 3.2, we showed that our findings are generalizable to the full cohort of families in the two cities from which we recruited our sample. However, the families in our sample may not be representative of an average family in Germany because we conducted the study only in two cities located in the same federal state. While the sociodemographic characteristics of the population in the two sample cities are quite similar to the Germany-wide average (see Section 2.3), this does not necessarily hold for families with young children, which are in the focus of our study. Thus, we use data from a representative sample of families in Germany to construct weights based on migration background, SES, equivalent household income, and current employment status. The re-weighted treatment effect estimates are very similar to the unweighted results, speaking in favor of the external validity of our results for families with young children in Germany overall (see Online Appendix F.2 for details). However, we refrain from making stronger claims about the generalizability of our results because the lack of official statistics makes it impossible to judge the representativeness of our sample with respect to other important characteristics relevant in the child care application process, such as parental application knowledge, degree of rationing of child care slots, and specific processes for how slots are allocated.

Displacement Effects. In a rationed child care market (see Section 2) with a fixed number of available child care slots, a positive treatment effect for lower-SES families on child care enrollment could potentially imply that enrollment chances of other families (treated higher-SES families, control group families, or out-of-sample families) suffer (i.e., that they are “displaced” by treated lower-SES families). Because our data include the exact home locations of the entire birth cohort from which we recruited our sample, we are able to analyze whether such displacement effects matter in our setting. We test for displacement effects as follows. First, for each household in our sample, we compute the share of households in the full cohort within a one-mile (1.6 km) radius that (i) participates in our study and (ii) is randomly assigned to the treatment group. Across households that participate in our study, the share of other households participating within a 1-mile radius varies between 0 and 34% (mean: 23%), and the share of treated households varies between 0 and 19% (mean: 13%). In a second step, we regress whether a child in the control group is enrolled in child care on the share of households in the treatment group, controlling

for the share of households participating in the study. The existence of displacement effects would imply that the likelihood that control-group children are enrolled in child care *decreases* in the share of treated households in the control-group family’s surrounding area. However, the results in Appendix Table F12 provide no evidence for displacement effects of our treatment, as the likelihood that a control-group family’s child is enrolled in child care is unrelated to the share of treated households in the family’s vicinity. The same is true when we conduct the displacement analysis separately for lower- and higher-SES families in the control group.⁴⁰

While it is reassuring that the chance of enrolling in child care for the control group is not systematically related to the share of treated households nearby, it is not immediately clear how treated families in a rationed market were able to secure child care slots without negatively affecting other families. A potential channel is that inefficiencies in slot allocation in the German child care market (see Section 2) lead to slots being unfilled, for example, temporarily unused slots or slots that are not assigned to parents right away. Because there are no official statistics reporting the current number of unfilled child care slots in Germany, we conducted a nationwide survey among child care center managers ($N = 440$) to learn about the extent of potential inefficiencies in slot allocation (see Online Appendix F.4). Child care center managers report a substantial number of unfilled slots for children below the age of three: On average, there was at least one unfilled slot in more than half of the child care centers (58%), and about one-third of centers (37%) reported more than five unfilled child care slots. In fact, these figures are even larger for the specific region in which we conducted our study; however, owing to the limited sample size, we report only the more conservative Germany-wide numbers. Our findings from the child care center manager survey imply that inefficiencies exist in the allocation of child care slots, leaving room for an increased number of slots to be allocated to the treatment group without impeding the control group (or out-of-sample families). For example, treated lower-SES families being more likely to visit child care centers on-site and sending more applications might be channels that help them to find (temporarily) unfilled slots.

⁴⁰These results also imply that the estimated treatment effect is not exaggerated owing to the control group facing a *lower* chance of child care enrollment because of the treatment.

5. Conclusion

We conducted a randomized controlled trial to study whether reducing barriers in the application process increases the use of early child care, particularly for disadvantaged families. Our intervention was designed to address application barriers by randomly providing child-care-related information and offering customized application assistance. For lower-SES families, the treatment increases the probability of applying for a child care slot by 21 pp and of eventually enrolling their children in child care by 16 pp. Higher-SES families do not react to the treatment. Thus, alleviating application barriers substantially reduces the large socioeconomic gap in child care enrollment.

Of course, the addressed barriers in the application process are only one of several explanations for the large observed SES gap in child care usage. We observe that, conditional on applying, the enrollment probability for lower-SES families is substantially smaller than for higher-SES families. This suggests that additional factors not addressed by our intervention prevent lower-SES families from child care usage. These factors may include complementary demand-side reasons, such as SES differences in the quality of applications or general application strategies (e.g., narrow vs. wide scope of search), as well as supply-side reasons, such as discrimination against lower-SES families by child care providers in their admission decisions. In fact, in another large-scale field experiment, we demonstrate the existence of substantial discrimination against migrant families in the German child care market (see Hermes et al., 2023). We believe that exploring these and other factors leading to unequal access to universal social services is an important avenue for future research.

Germany is an interesting country to study barriers to early child care, because many features of the German child care system are mirrored in other countries. Most of the largest European countries and the U.S. also exhibit characteristics such as underrepresentation of lower-SES families, fee reductions for lower-income families, rationed slots, the existence of application barriers, and decentralized admission decisions (see Appendix Table A1). The countries also generally align in terms of monthly fees and enrollment rates (outliers are the UK and the U.S. with higher fees, and the Nordic countries with higher enrollment rates). Given these institutional parallels, our findings are potentially transferable to other industrialized countries. Yet, the extent to which addressing (country-specific) application barriers actually reduces the socioeconomic gap in child care enrollment in other countries is ultimately an empirical question for future research.

Drawing on the recent literature on the scalability of RCTs (e.g., Al-Ubaydli et al., 2017, 2019), several aspects of our experiment indicate the *possibility* that our intervention

is scalable beyond the immediate study context. First, implementation costs are low. The information video can be scaled almost cost-free, for example, through online delivery, and the time use of assistants was very modest (we estimate per-family costs for the assistant to be far below USD 50). Second, the specific content of the assistance treatment is conducive to scaling. According to the documentation provided by the assistants, most direct interactions focused on providing personalized information via telephone, which can be delivered at scale with relative ease and moderate costs. Third, our sample matches the population characteristics well (see Section 3.2). Lastly, our displacement analysis indicates little evidence for displacement effects (see Section 4.5), suggesting that increased enrollment rates due to our treatment likely result from inefficiencies in the slot allocation process. Naturally, the extent to which this holds true when implementing the intervention on a nationwide scale may largely depend on the specific rollout approach.⁴¹

From a policy perspective, our findings have implications for the design of equity-oriented social programs. Aiming to improve educational opportunities for disadvantaged children, several countries, including the U.S., have recently expanded choice in their education systems (e.g., through charter schools, school-voucher programs, or universal child care). A fundamental challenge with such formally non-selective programs is that lower-SES families are less able to access and utilize these programs (e.g., Heckman and Landersø, 2021) or are more likely to be put on waiting lists (e.g., Bouek, 2023; DeLuca et al., 2013). In consequence, the programs' effects on educational equality often fall short of expectations. Our results show that an important mechanism behind this pattern is that lower-SES families have difficulties navigating complex application processes. This finding is in line with the more general result in Ko and Moffitt (2022), who highlight the importance of administrative burden to explain imperfect take-up of social benefits, and specifically with Walters (2018), who suggests that a lack of familiarity with application processes may explain why lower-SES children are often underrepresented in U.S. charter schools. Our results imply that merely expanding universal child care offerings, as has been observed in several countries including Germany in recent years, without addressing the child care application process, is unlikely to fully equalize access to early childcare.⁴²

⁴¹We are fairly optimistic that displacement effects would remain modest if an opt-in design is employed, similar to our study (recall that around 600 out of over 2,500 eligible families chose to participate in our study, and approximately one-third of those in the treatment group took up the assistance). However, if policymakers were to make program participation mandatory for all families, displacement effects and other general-equilibrium effects detrimental to scaling could become relevant.

⁴² Indeed, previous expansions of early child care in Germany predominantly increased enrollment among children from higher-SES families (Felfe and Lalive, 2018). In line with this result, disadvantaged

This raises the question of how policymakers can reform application processes for early child care to increase equity in child care participation. While several features of our study suggest that our specific intervention could be successfully scaled up, we rather view our work as a *proof of concept* that reducing barriers in the child care application process can promote early educational equality. Rather than providing information and application assistance to families on a large scale within a complex and inefficient application system, policymakers can enhance access directly by implementing *structural* changes to the application process itself. These changes may include minimizing paperwork and administrative burdens, pre-filling application forms using administrative records, making relevant information easily accessible, and centralizing the admission system (which would also prevent child care centers from using waiting lists as an additional opportunity for discretionary decision-making). Exploring the effectiveness of such reforms would also be a compelling direction for future studies.

groups often forego participation in (non-oversubscribed) social programs for which they are eligible (for an overview of the evidence, see Ko and Moffitt, 2022).

References

- Acquisti, Alessandro and John, Leslie K. and Loewenstein, George (2013). What Is Privacy Worth? *Journal of Legal Studies* 42(2), 249–274.
- Al-Ubaydli, O., J. A. List, and D. Suskind (2019). The Science of Using Science: Towards an Understanding of the Threats to Scaling Experiments. *International Economic Review* (forthcoming).
- Al-Ubaydli, O., J. A. List, and D. L. Suskind (2017). What Can We Learn from Experiments? Understanding the Threats to the Scalability of Experimental Results. *American Economic Papers and Proceedings* 107(5), 282–286.
- Alt, C., B. Gedon, S. Hubert, K. Hüsken, and K. Lippert (2018). DJI-Kinderbetreuungsreport 2018 - Inanspruchnahme und Bedarfe bei Kindern bis 14 Jahren aus Elternperspektive - ein Bundesländervergleich. DJI 2018, Deutsches Jugendinstitut, München.
- Athey, S. and G. W. Imbens (2017). The Econometrics of Randomized Experiments. In A. V. Banerjee and E. Duflo (Eds.), *Handbook of Economic Field Experiments*, Volume 1, pp. 73–140. North Holland, Amsterdam.
- Autorengruppe Bildungsberichterstattung (2020). Bildung in Deutschland 2020: Ein Indikatorengestützter Bericht mit einer Analyse zur Bildung in einer Digitalisierten Welt (Education in Germany 2020). Bielefeld: wbv, W. Bertelsmann Verlag.
- Baker, M., J. Gruber, and K. Milligan (2008). Universal child care, maternal labour supply, and family well-being. *Journal of Political Economy* 116(4), 709–745.
- Bauernschuster, S. and M. Schlotter (2015). Public child care and mothers' labor supply - evidence from two quasi-experiments. *Journal of Public Economics* 123(C), 1–16.
- Bergman, P. (2021). Parent-Child Information Frictions and Human Capital Investment: Evidence from a Field Experiment. *Journal of Political Economy* 129(1), 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(18), 125–158.
- Bergman, P., R. Chetty, S. DeLuca, N. Hendren, L. F. Katz, and C. Palmer (2023). Creating Moves to Opportunity: Experimental Evidence on Barriers to Neighborhood Choice. *American Economic Review* (forthcoming).
- Bergman, P., C. Edmond-Verley, and N. Notario-Risk (2018). Parent Skills and Information Asymmetries: Experimental Evidence from Home Visits and Text Messages in Middle and High Schools. *Economics of Education Review* 66, 92–103.
- Bettinger, E. P., N. Cunha, G. Lichand, and R. Madeira (2021). Are the Effects of Informational Interventions Driven by Salience? Working Paper No. 350, University of Zurich.

- Bettinger, E. P., B. T. Long, P. Oreopoulos, and L. Sanbonmatsu (2012). The Role of Application Assistance and Information in College Decisions: Results from the H&R Block FAFSA Experiment. *Quarterly Journal of Economics* 127(3), 1205–1242.
- Bjoerklund, A. and K. G. Salvanes (2011). Education and Family Background: Mechanisms and Policies. In E. A. Hanushek, S. Machin, and L. Woessmann (Eds.), *Handbook of the Economics of Education*, Volume 3, pp. 201–247. North Holland, Amsterdam.
- Bleemer, Z. and B. Zafar (2018). Intended College Attendance: Evidence from an Experiment on College Returns and Costs. *Journal of Public Economics* 157(C), 184–211.
- Bouek, J. W. (2023). The Wait List as Redistributive Policy: Access and Burdens in the Subsidized Childcare System. *The Russell Sage Foundation Journal of the Social Sciences* 9(5), 76–97.
- Camehl, G. F., P. S. Schober, and C. K. Spiess (2018). Information Asymmetries between Parents and Educators in German Childcare Institutions. *Education Economics* 26(6), 624–646.
- Canaan, S. (2022). Parental leave, household specialization and children’s well-being. *Labour Economics* 75, 102127.
- Cappelen, A., J. List, A. Samek, and B. Tungodden (2020). The Effect of Early-Childhood Education on Social Preferences. *Journal of Political Economy* 128(7), 2739–2758.
- Carlsson, S. and S. L. Thomsen (2015). Improving the Allocation of Spots in Child Care Facilities for Toddlers in Germany: A Mechanism Design Approach. IZA Discussion Paper No. 8976, IZA, Bonn.
- Carrell, S. and B. Sacerdote (2017). Why do college-going interventions work? *American Economic Journal: Applied Economics* 9(3), 124–151.
- Cascio, E. (2015). The Promises and Pitfalls of Universal Early Education. *IZA World of Labor* 116(January).
- Cascio, E. (2022). Early Childhood Education in the United States: What, When, Where, Who, How, and Why. In B. P. McCall (Ed.), *Routledge Handbook of the Economics of Education*, pp. 30–72. London and New York: Routledge.
- Castleman, B. L. and L. C. Page (2015). Summer Nudging: Can Personalized Text Messages and Peer Mentor Outreach Increase College Going among Low-Income High School Graduates? *Journal of Economic Behavior & Organization* 115, 144–160.
- 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(6), 2356–2409.
- Cortes, K. E., H. D. Fricke, S. Loeb, D. S. Song, and B. N. York (2019). When Behavioral Barriers are Too High or Low – How Timing Matters for Parenting Interventions. NBER Working Paper 25964, National Bureau of Economic Research, Cambridge, MA.

- Cunha, F., I. Elo, and J. Culhane (2022). Maternal Subjective Expectations about the Technology of Skill Formation Predict Investments in Children one Year Later. *Journal of Econometrics* 231(1), 3–32.
- Cunha, F. and J. J. Heckman (2007). The Technology of Skill Formation. *American Economic Review Papers and Proceedings* 97(2), 31–47.
- Cunha, F., J. J. Heckman, L. Lance, and D. Masterov (2006). Interpreting the Evidence on Life Cycle Skill Formation. In E. A. Hanushek and F. Welch (Eds.), *Handbook of the Economics of Education*, Volume 1, pp. 697–812. North Holland, Amsterdam.
- Currie, J. (2001). Early Childhood Education Programs. *Journal of Economic Perspectives* 15(2), 213–238.
- Currie, J. and F. Gahvari (2008). Transfers in Cash and In-Kind: Theory Meets the Data. *Journal of Economic Literature* 46(2), 333–83.
- Damgaard, M. T. and H. S. Nielsen (2018). Nudging in Education. *Economics of Education Review* 64, 313–342.
- Danzer, N. and V. Lavy (2018). Paid Parental Leave and Children’s Schooling Outcomes. *Economic Journal* 128(608), 81–117.
- Datta Gupta, N. and J. Jessen (2023). Maternity leave versus early childcare: What are the long-term consequences for children? Technical report.
- DeLuca, S., P. M. Garboden, and P. Rosenblatt (2013). Segregating Shelter: How Housing Policies Shape the Residential Locations of Low-Income Minority Families. *Annals of the American Academy of Political and Social Science* 647(1), 268–299.
- Destatis (2022). Personen in Elternzeit. <https://www.destatis.de/de/themen/arbeit/arbeitsmarkt/qualitaet-arbeit/dimension-3/elternzeit.html>. German Federal Statistical Office, Wiesbaden.
- Doss, C., E. M. Fahle, S. Loeb, and B. E. York (2019). More Than Just a Nudge: Supporting Kindergarten Parents with Differentiated and Personalized Text Messages. *Journal of Human Resources* 54(3), 567–603.
- Drange, N. and T. Havnes (2019). Early Childcare and Cognitive Development: Evidence from an Assignment Lottery. *Journal of Labor Economics* 37(2), 581–620.
- Dynarski, S., C. J. Libassi, K. Michelmore, and S. Owen (2021). Closing the Gap: The Effect of Reducing Complexity and Uncertainty in College Pricing on the Choices of Low-Income Students. *American Economic Review* 111(6), 1721–1756.
- Dynarski, S., A. Nurshatayeva, L. C. Page, and J. Scott-Clayton (2022). Addressing Non-Financial Barriers to College Access and Success: Evidence and Policy Implications. Working Paper 30054, National Bureau of Economic Research.

- Eika, L., M. Mogstad, and B. Zafar (2019). Educational Assortative Mating and Household Income Inequality. *Journal of Political Economy* 127(6), 2795–2835.
- Falk, A., F. Kosse, P. Pinger, H. Schildberg-Hoerisch, and T. Deckers (2021). Socio-Economic Status and Inequalities in Children’s IQ and Economic Preferences. *Journal of Political Economy* 129(9), 2504–2545.
- Felfe, C. and R. Lalive (2018). Does Early Child Care Affect Children’s Development? *Journal of Public Economics* 159, 33–53.
- Flood, S., J. F. McMurry, A. Sojourner, and M. J. Wiswall (2021). Inequality in Early Care Experienced by U.S. Children. Working Paper 29249, National Bureau of Economic Research.
- Fryer, R. G., S. D. Levitt, and J. A. List (2015). Parental incentives and early childhood achievement: A field experiment in Chicago heights. NBER Working Paper 21477, National Bureau of Economic Research, Cambridge, MA.
- Fugger, N., T. Klein, and T. Riehm (2017). Dezentrale Kitaplatzvergabe ohne Warteschlange: Ein Leitfaden. *ZEWpolicybrief* 4, 709–745.
- Gelber, A. and A. Isen (2013). Children’s schooling and parents’ behavior: Evidence from the head start impact study. *Journal of Public Economics* 101, 25–38.
- Goebel, J., M. M. Grabka, S. Liebig, M. Kroh, D. Richter, C. Schroeder, and J. Schupp (2019). The German Socio-Economic Panel (SOEP). *Journal of Economics and Statistics* 239(2), 345–360.
- Havnes, T. and M. Mogstad (2015). Is Universal Child Care Leveling the Playing Field? *Journal of Public Economics* 127, 100–114.
- Heckman, J. J. (2011). The Economics of Inequality: The Value of Early Childhood Education. *American Educator* 35, 31–47.
- Heckman, J. J. and R. Landersø (2021). Lessons from Denmark about Inequality and Social Mobility. NBER Working Paper 28543, NBER.
- Hermes, H., M. Krauß, P. Lergetporer, F. Peter, and S. Wiederhold (2022). Early Child Care and Labor Supply of Lower-SES Mothers: A Randomized Controlled Trial. Working Paper No. 10178, CESifo.
- Hermes, H., P. Lergetporer, F. Mierisch, F. Peter, and S. Wiederhold (2023). Discrimination on the Child Care Market: A Nationwide Field Experiment. CESifo Working paper Series 10368, Center for Economic Studies.
- Herzog, S. and T. Klein (2018). Matching Practices for Childcare - Germany. *Matching in practice, MiP Country Profile* 26, 1–13.
- Houmark, M. A., C. M. Løchte Jørgensen, I. L. Kristiansen, and M. Gensowski (2022). Effects of Extending Paid Parental Leave on Children’s Socio-Emotional Skills and Well-Being in Adolescence. IZA Discussion Paper No. 15421, IZA, Bonn.

- Hoxby, C. and S. Turner (2015). What High-Achieving Low-Income Students Know about College. *American Economic Review: Papers & Proceedings* 105(5), 514–517.
- INKAR (2017). Indikatoren und Karten zur Raum- und Stadtentwicklung. Technical report, Bundesinstitut für Bau-, Stadt- und Raumforschung, Bonn.
- Jensen, R. (2010). The (Perceived) Returns to Education and the Demand for Schooling. *Quarterly Journal of Economics* 125(2), 515–548.
- Jessen, J., S. Schmitz, and S. Waights (2020). Understanding Day Care Enrolment Gaps. *Journal of Public Economics* 190, 104252.
- Kerr, S. P., T. Pekkarinen, M. Sarvimäki, and R. Uusitalo (2020). Post-Secondary Education and Information on Labor Market Prospects: A Randomized Field Experiment. *Labour Economics* 66, 101888.
- Kline, P. and C. R. Walters (2016). Evaluating Public Programs with Close Substitutes: The Case of Head Start. *Quarterly Journal of Economics* 131(4), 1795–1848.
- Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental Analysis of Neighborhood Effects. *Econometrica* 75(1), 83–119.
- Ko, W. and R. A. Moffitt (2022). Take-up of Social Benefits. Working Paper 30148, National Bureau of Economic Research.
- Kosse, F., T. Deckers, P. Pinger, H. Schildberg-Hoerisch, and A. Falk (2020). The Formation of Prosociality: Causal Evidence on the Role of Social Environment. *Journal of Political Economy* 128(2), 434–467.
- Lavecchia, A., H. Liu, and P. Oreopoulos (2016). Behavioral Economics of Education: Progress and Possibilities. In E. Hanushek, S. Machin, and L. Woessmann (Eds.), *Handbook of the Economics of Education*, Volume 5, pp. 1–74. Elsevier.
- Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *Review of Economic Studies* 76(3), 1071–1102.
- Lergetporer, P., K. Werner, and L. Woessmann (2021). Does Ignorance of Economic Returns and Costs Explain the Educational Aspiration Gap? Representative Evidence from Adults and Adolescents. *Economica* 88, 624–670.
- List, J. A., A. M. Shaikh, and Y. Xu (2019). Multiple Hypothesis Testing in Experimental Economics. *Experimental Economics* 22(4), 773–793.
- Liu, Q. and O. N. Skans (2010). The Duration of Paid Parental Leave and Children’s Scholastic Performance. *The B.E. Journal of Economic Analysis & Policy* 10(1).
- Magnuson, K. and J. Waldfogel (2016). Trends in Income-Related Gaps in Enrollment in Early Childhood Education. *AERA Open* 2(2), 1–13.

- Marinescu, I. and D. Skandalis (2021). Unemployment Insurance and Job Search Behavior. *Quarterly Journal of Economics* 136(2), 887–931.
- Mayer, S. E., A. Kalil, P. Oreopoulos, and S. Gallegos (2019). Using Behavioral Insights to Increase Parental Engagement: The Parents and Children Together Intervention. *Journal of Human Resources* 54(4), 900–925.
- McGuigan, M., S. McNally, and G. Wyness (2016). Student Awareness of Costs and Benefits of Educational Decisions: Effects of an Information Campaign. *Journal of Human Capital* 10(4), 482–519.
- OECD (2011). *Doing Better for Families*. Paris, France: Organisation for Economic Co-Operation and Development.
- OECD (2019). OECD Family Database: Enrolment in Childcare and Pre-School (PF3.2). http://www.oecd.org/els/soc/pf3_2_enrolment_childcare_preschool.pdf, OECD, Paris.
- OECD (2021). Part-Time Employment Rate (Indicator). <https://data.oecd.org/emp/part-time-employment-rate.htm>, OECD, Paris.
- Oreopoulos, P. and R. Dunn (2013). Information and College Access: Evidence from a Randomized Field Experiment. *Scandinavian Journal of Economics* 115(1), 3–26.
- Oreopoulos, P. and R. Ford (2019). Keeping College Options Open: A Field Experiment to Help All High School Seniors Through the College Application Process. *Journal of Policy Analysis and Management* 38(2), 299–329.
- Peter, F., C. K. Spiess, and V. Zambre (2021). Informing Students about College: Increasing Enrollment Using a Behavioral Intervention? *Journal of Economic Behavior & Organization* 190, 524–549.
- Resnjanskij, S., J. Ruhose, S. Wiederhold, L. Woessmann, and K. Wedel (2023). Can Mentoring Alleviate Family Disadvantage in Adolescence? A Field Experiment to Improve Labor-Market Prospects. *Journal of Political Economy* (forthcoming).
- Romano, J. P. and M. Wolf (2005). Stepwise Multiple Testing as Formalized Data Snooping. *Econometrica* 73(4), 1237–1282.
- Romano, J. P. and M. Wolf (2016). Efficient Computation of Adjusted P-Values for Resampling-Based Stepdown Multiple Testing. *Statistics & Probability Letters* 113, 38–40.
- Rossin-Slater, M. (2017). Maternity and Family Leave Policy. In S. L. Averett, L. M. Argys, and S. D. Hoffman (Eds.), *The Oxford Handbook of Women and the Economy*, pp. 323–342. North Holland, Amsterdam.
- Schmutz, R. (2024). Is universal early childhood education and care an equalizer? a systematic review and meta-analysis of evidence. *Research in Social Stratification and Mobility* 89, 100859.
- Schudy, S. and V. Utikal (2017). ‘You Must Not Know about Me’—On the Willingness to Share Personal Data. *Journal of Economic Behavior & Organization* 141, 1–13.

- Shlay, A. B., M. Weinraub, M. Harmon, and H. Tran (2004). Barriers to Subsidies: Why Low-Income Families Do Not Use Child Care Subsidies. *Social Science Research* 33(1), 134–157.
- SOEP (2019). Socio-Economic Panel (SOEP) 2019, Years 1984-2017, Version 34. doi:10.5684/soep.v34, DIW Berlin, Berlin.
- Spiess, C. K. (2008). Early childhood education and care in Germany: The status quo and reform proposals. *Zeitschrift für Betriebswirtschaftslehre* 2008 67, 1–20.
- Spiess, C. K. (2013). Investments in Education: The Early Years Offer Great Potential. *DIW Economic Bulletin* 3(10), 3–10.
- Stahl, J. and P. S. Schober (2018). Convergence or Divergence? Educational Discrepancies in Work-Care Arrangements of Mothers with Young Children in Germany. *Work, Employment & Society* 32(4), 629–649.
- Stahl, J., P. S. Schober, and C. K. Spiess (2018). Parental Socio-Economic Status and Childcare Quality: Early Inequalities in Educational Opportunity? *Early Childhood Research Quarterly* 44, 304–317.
- Walters, C. R. (2018). The Demand for Effective Charter Schools. *Journal of Political Economy* 126(6), 2179–2223.
- Westfall, P. H. and S. S. Young (1993). *Resampling-Based Multiple Testing: Examples and Methods for P-Value Adjustment*. Wiley Series in Probability and Mathematical Statistics. New York: Wiley.
- Wiswall, M. and B. Zafar (2015). Determinants of College Major Choice: Identification Using an Information Experiment. *Review of Economic Studies* 82(2), 791–824.
- York, B. E., S. Loeb, and C. Doss (2019). One Step at a Time: The Effects of an Early Literacy Text-Messaging Program for Parents and Preschoolers. *Journal of Human Resources* 54(3), 538–566.
- Young, A. (2019). Channeling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results. *Quarterly Journal of Economics* 134(2), 557–598.

Online Appendix A. Cross-Country Comparison of Early Child Care Provision

Table A1: Cross-Country Comparison of Early Child Care Provision

Country	Features of early child care provision								
	SES enrollment gap	Reduced fees	Slots rationed	Application required for enrollment	Application barriers	Decentralized admission decisions	Average fees per month	Enrollment rates	Enrollment rates by starting age
Austria	No ^{a1}	Yes ^{b1}	Yes ^{c1}	Yes ^{d1}	Yes ^{e1}	Both ^{f1}	100-440 EUR ^{g1}	Age group 0-2 years: 23% ^{h1}	Age 1: 19%, Age 2: 48% ⁱ¹
Belgium	Yes ^{a2}	Yes ^{b2}	Yes ^{c2}	Yes ^{d2}	Yes ^{e2}	Yes ^{f2}	285 EUR ^{g2}	Age group 0-2 years: 56% ^{h2}	Age 1: 57%, Age 2: 74% ⁱ²
Denmark	No ^{a3}	Yes ^{b3}	No ^{c3}	Yes ^{d3}	No ^{e3}	No ^{f3}	282 EUR ^{a.g3}	Age group 0-2 years: 66% ^{h3}	Age 1: 82%, Age 2: 92% ⁱ³
Finland	Yes ^{a4}	Yes ^{b4}	No ^{c4}	Yes ^{d4}	No ^{e4}	Yes ^{f4}	0-289 EUR ^{g4}	Age group 0-2 years: 38% ^{h4}	Age 1: 41%, Age 2: 67% ⁱ⁴
France	Yes ^{a5}	Yes ^{b5}	Yes ^{c5}	Yes ^{d5}	Yes ^{e5}	Yes ^{f5}	133-301 EUR ^{g5}	Age group 0-2 years: 51% ^{h5}	Age 1: 62%, Age 2: 57% ⁱ⁵
Germany	Yes^{a6}	Yes^{b6}	Yes^{c6}	Yes^{d6}	Yes^{e6}	Yes^{f6}	250 EUR^{g6}	Age group 0-2 years: 31%^{h6}	Age 1: 31%, Age 2: 56%ⁱ⁶
Ireland	Yes ^{a7}	Yes ^{b7}	Yes ^{c7}	Yes ^{d7}	Yes ^{e7}	Yes ^{f7}	771 EUR ^{g7}	Age group 0-2 years: 41% ^{h7}	Age 1: 36%, Age 2: 68% ⁱ⁷
Italy	Yes ^{a8}	Yes ^{b8}	Yes ^{c8}	Yes ^{d8}	Yes ^{e8}	No ^{f8}	-	Age group 0-2 years: 26% ^{h8}	Age 1: 25%, Age 2: 43% ⁱ⁸
Netherlands	Yes ^{a9}	Yes ^{b9}	No ^{c9}	Yes ^{d9}	Yes ^{e9}	Yes ^{f9}	577 EUR ^{g9}	Age group 0-2 years: 65% ^{h9}	Age 1: 66%, Age 2: 79% ⁱ⁹
Norway	Yes ^{a10}	Yes ^{b10}	No ^{c10}	Yes ^{d10}	Yes ^{e10}	No ^{f10}	0-244 EUR ^{a.g10}	Age group 0-2 years: 58% ^{h10}	Age 1: 76%, Age 2: 93% ⁱ¹⁰
Poland	Yes ^{a11}	No ^{b11}	Yes ^{c11}	Yes ^{d11}	Yes ^{e11}	Both ^{f11}	-	Age group 0-2 years: 10% ^{h11}	Age 1: 9%, Age 2: 21% ⁱ¹⁰
Sweden	No ^{a12}	Yes ^{b12}	No ^{c12}	Yes ^{d12}	Yes ^{e12}	No ^{f12}	0-119 EUR ^{a.g12}	Age group 0-2 years: 53% ^{h12}	Age 1: 62%, Age 2: 96% ⁱ¹¹
Switzerland	Yes ^{a13}	Yes ^{b13}	Yes ^{c13}	Yes ^{d13}	No ^{e13}	Both ^{f13}	0-2476 EUR ^{a.g13}	Age group 0-2 years: 37% ^{h13}	-
Spain	Yes ^{a14}	Yes ^{b14}	Yes ^{c14}	Yes ^{d14}	Yes ^{e14}	No ^{f14}	59-328 EUR ^{g14}	Age group 0-2 years: 57% ^{h14}	Age 1: 66%, Age 2: 84% ⁱ¹
UK	Yes ^{a15}	Yes ^{b15}	No ^{c15}	Yes ^{d15}	Yes ^{e15}	Both ^{f15}	805-1,228 EUR ^{a.g15}	Age group 0-2 years: 45% ^{h15}	-
US	Yes ^{a16}	Yes ^{b16}	Yes ^{c16}	Yes ^{d16}	Yes ^{e16}	Both ^{f16}	1147 EUR ^{a.g16}	Age group 0-2 years: 28% ^{h16}	-

Notes: The table shows features of early child care systems of the 15 largest European countries by GDP and the U.S. **Lower-SES underrepresented:** “Yes” if families with lower socioeconomic status are underrepresented in early child care. **Reduced fees for lower-income families:** “Yes” if lower-income families are eligible for fee reductions or exemptions. **Slots rationed:** “Yes” if average demand for a child care slot exceeds supply. **Application required for enrollment:** “Yes” if enrollment requires application related actions by parents (i.e., enrollment is not automatic). **Application barriers:** “Yes” if administrative barriers exist (i.e., effort related to paperwork) that may deter parents from applying. **Decentralized admission decision:** “Yes” if admission decisions are taken at the child care center level (subjective decision process, i.e., child care center managers decide) and “Both” if decisions are taken at the child care center level by private providers, but not by public providers. **Average fee per month:** Effective average fees that parents pay per month. **Enrollment rates:** Average enrollment rates for early child care. **Enrollment rates by starting age:** Average enrollment rates differentiated by starting age in the age group 0-2 (these numbers are only available for EU countries and Norway). Regarding the number for the US and Italy, current numbers on average enrollment rates in the US are not available (numbers reported are from 2011, see OECD, 2023) nor current information on application barriers in Italy (numbers reported are from 2012, see Abitare a Roma, 2013).

^aFor comparison, fees in country-specific currency have been converted to Euro using exchange rate as of May 29, 2023.

Sources: ^{a6,e6,g6} Jessen et al. (2020); ^{a9,a10,a13,a15,b5,b16} OECD (2020); ^{a1-a4,a7,a8,a11,a12,a14,a15} OECD (2023); ^{a16} Cui, J. and Natzke, L. and Grady, S. (2021); ^{e6,g6} Felfe and Lalive (2018); ^{g1-g5,g9,g10,g12-g15,b1-b5,b8-b13,b15,c1-c15} Eurydice (2019); ^{g16} World Population Review (2023); ^{b2,b8,b12} Hufkens et al. (2020); ^{c15} Department of Education (2021); ^{c7} Independent.ie (2022); ^{c16} Malik, R. and K. Hamm and L. Schochet and C. Novoa and S. Workman (2018); ^{d15,e15,f15} Renfrewshire Council (2023); ^{d5,e5,f5} Expat in France (2022); ^{d8,f8} Del Boca et al. (2016); ^{e8} Abitare a Roma (2013); ^{d14,e14,f14} Expatica (2022); ^{d9,e9,f9} City of Amsterdam (2023); ^{d13,e13,f13} Stadt Bern (2023); Kinderkrippen-Online.ch (2023); ^{d11,e11} Gov.pol (2022); ^{f11} Nursery Complex (2023); ^{d2,e2,f2} The Bulletin (2015); The Village (2023); ^{d7,e7,f7} Irish Examiner (2023); The Journal (2022); ^{d10,e10,f10} Eurydice (2022); ^{e1,g1} Arbeiterkammer Oberösterreich (2023); ^{e1} Linz.at (2023); ^{d3,e3,f3} City of Copenhagen (2023); ^{d4,e4,f4} Pilke (2022); ^{d16,f16} NYC Department of Education (2023); Catalyst Kids (2023); ^{e16} Esping-Andersen et al. (2012); ChildCare.gov (2023); ^{h10,h13,h15,h16} OECD (2023); ^{h1-h9,h11,h12,i1-i9,i11,i12,i14} Flisi et al. (2022); ⁱ¹⁰ Nordic Health & Welfare Statistics (2022).

Online Appendix B. Socioeconomic Gaps at Baseline

Table B2: SES Gaps in Application Knowledge (Baseline)

	Indices		Correct Answer to Question on...					
	Mean (1)	# Correct (2)	Legal claim (3)	Age legal claim (4)	Financial support (5)	Age cost free (6)	Apply to 1 center (7)	Nearest center (8)
Lower-SES	-0.436 (0.086)	-0.602 (0.113)	-0.114 (0.036)	-0.054 (0.027)	-0.198 (0.040)	-0.025 (0.040)	-0.177 (0.040)	-0.034 (0.028)
Mean Higher-SES	0.166	3.703	0.803	0.151	0.657	0.620	0.586	0.886
N	607	607	607	607	607	607	607	607

Notes: Table shows SES gaps in application knowledge at baseline, all models are estimated by OLS. We regress each outcome on a dummy variable indicating lower-SES. The constant is reported as 'Mean Higher-SES'. In Column (1), the outcome is an index that combines answers to all six application knowledge questions to an average of z -scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007). In Column (2), the outcome is a simple count of the number of correct answers. In Columns (3)–(8), we report results for each knowledge question separately (using linear probability models, the outcome variable is equal to one if answered correctly and zero otherwise). Robust standard errors in parentheses.

Table B3: SES Gaps in Parent-Child Activities (Baseline)

	Read Picture Books (1)	Read Stories (2)	Sing Songs (3)	Watch TV/Videos (4)	Paint/Play at Home (5)	Walk Outside (6)	Go to Playground (7)	Visit Other Families (8)
Lower-SES	-0.075 (0.041)	-0.069 (0.041)	-0.122 (0.026)	0.209 (0.039)	0.046 (0.041)	0.019 (0.012)	0.103 (0.040)	0.078 (0.041)
Mean Higher-SES	0.603	0.638	0.954	0.257	0.506	0.966	0.298	0.422
N	588	589	601	599	596	606	586	604

Notes: Table shows SES gaps in parent-child activities at baseline, all models are estimated by OLS. We regress each outcome on a dummy variable indicating lower-SES. The constant is reported as 'Mean Higher-SES'. The outcome in each column is a dummy variable equal to one if parents report that they conduct the respective activity "daily" or "several times a week", zero otherwise (the residual category includes "once a week" and "never"). Robust standard errors in parentheses.

Online Appendix C. Sample Selection

Table C1: Selection into Analytical Sample (Baseline)

	Participants (1)	Non-participants (2)	$\Delta(2)-(1)$ (3)	p-value (4)
Child characteristics				
Age of child (in months)	6.869	7.220	0.351	0.030
Child is female	0.484	0.484	-0.001	0.980
Migration background	0.409	0.515	0.106	0.000
German citizenship	0.832	0.792	-0.040	0.029
Parent characteristics				
Age of mother (in years)	31.36	30.66	-0.70	0.006
Age of father (in years)	34.85	34.55	-0.30	0.463
Family background				
Child with both parents in data	0.712	0.720	0.008	0.688
Child lives with both parents	0.695	0.683	-0.013	0.557
Mother is first-time parent	0.680	0.570	-0.111	0.000
Father is first-time parent	0.648	0.541	-0.107	0.000
Number of siblings	0.369	0.475	0.106	0.000
Area of Residence				
Zip code area 1	0.076	0.092	0.016	0.224
Zip code area 2	0.105	0.097	-0.008	0.561
Zip code area 3	0.170	0.170	0.000	0.991
Zip code area 4	0.094	0.090	-0.004	0.785
Zip code area 5	0.063	0.057	-0.005	0.627
Zip code area 6	0.079	0.081	0.002	0.871
Zip code area 7	0.125	0.123	-0.002	0.897
Zip code area 8	0.061	0.077	0.016	0.196
Zip code area 9	0.066	0.056	-0.010	0.352
Zip code area 10	0.071	0.060	-0.010	0.352
Zip code area 11	0.021	0.030	0.009	0.242
Zip code area 12	0.069	0.066	-0.003	0.778
N	607	1972		

Notes: Table compares mean values for sociodemographic characteristics and regional distribution between our analytical sample ($N = 607$) and the remaining birth cohort ($N = 1,972$) in the two participating cities at baseline. Column (1) reports mean values for our analytical sample, Column (2) reports mean values for the remaining sample from the birth cohort that is not part of our analytical sample, and Column (3) reports the difference between both groups. In Column (4), we display the corresponding p-value for a two-sided t-test of the null hypothesis that values in Columns (1) and (2) are equal. Age of the child is the child's age measured in months on August 1, 2018. Female is a dummy equal to one if the child is female, zero otherwise. Migration background is a dummy equal to one if the child has any citizenship other than the German (more than one citizenship is possible), zero otherwise. German citizenship is a dummy equal to one if the child has a German citizenship, zero otherwise. Child with both parents in data is a dummy equal to one if the child is linked to both parents in the administrative data, zero otherwise. Child lives with both parents is a dummy equal to one if both parents linked with the child are registered at the same address, zero otherwise. Mother/father is first-time parent is equal to one if the mother/father is not linked with another child in the administrative data, zero otherwise; note that these data contain information only on children born after June 1, 2012 (i.e., six years before the youngest children in our study are born, see Section 3.2) Number of siblings is the number of other children (born after June 1, 2012) linked with the parents of children in our sample.

Full-Cohort Weights. To check whether the observed differences between our baseline sample and the full birth cohort affect our results, we apply propensity score weights reflecting the probability of participating in our study. Weights are estimated from a probit model of a binary participation variable (indicating whether the family participated in our baseline survey) regressed on all variables included in Table C1. After applying these weights, all observed differences between our baseline sample and the sample of non-participating families become very small and statistically insignificant, suggesting a high quality of the propensity score matching. The re-weighted treatment effect estimates are reported in Table C2. The treatment effects are very similar to those in the unweighted regression, suggesting that selective participation in the study does not bias our results.

Table C2: Treatment Effects on Child Care Application and Enrollment Using Full-Cohort Weights

	Applied		Enrolled	
	Unweighted	Full Cohort Weights	Unweighted	Full Cohort Weights
Treatment	0.213 (0.072)	0.209 (0.072)	0.159 (0.062)	0.166 (0.060)
Treatment \times Higher-SES	-0.214 (0.086)	-0.199 (0.087)	-0.171 (0.085)	-0.202 (0.085)
Higher-SES	0.179 (0.066)	0.166 (0.066)	0.221 (0.064)	0.228 (0.065)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	-0.000	0.010	-0.011	-0.037
Higher-SES	(0.044)	(0.045)	(0.060)	(0.061)
Control Mean Higher-SES	0.857	0.835	0.497	0.487
Control Mean Lower-SES	0.639	0.642	0.216	0.210
Control Mean SES Gap	0.218	0.192	0.280	0.277
N	519	519	519	519

Notes: Table shows treatment effects using propensity score weights to account for selection into our study. The dependent variables are child care application in Columns (1) and (2) and child care enrollment in Columns (3) and (4). Regressions in Columns (2) and (4) are re-weighted according to the full birth cohort in the two sample cities. Weights are derived from a probit model of a binary participation variable (indicating whether the family participated in the baseline survey) regressed on all variables included in Table C1. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. In Columns (2) and (4), control means are re-weighted according to the full birth cohort. Robust standard errors in parentheses.

Online Appendix D. Further Results

Table D1: Treatment Effect on Hours and Months in Child Care

	Hours in Child Care (1)	Months in Child Care (2)
Treatment	4.452 (2.097)	4.214 (1.655)
Treatment \times Higher-SES	-4.919 (2.847)	-3.529 (2.370)
Strata FE	Yes	Yes
Further controls	Yes	Yes
Treatment Effect Higher-SES	-0.467 (2.049)	0.686 (1.745)
Control Mean Higher-SES	13.756	12.283
Control Mean Lower-SES	6.116	5.053
Control Mean SES Gap	7.640	7.230
N	497	507

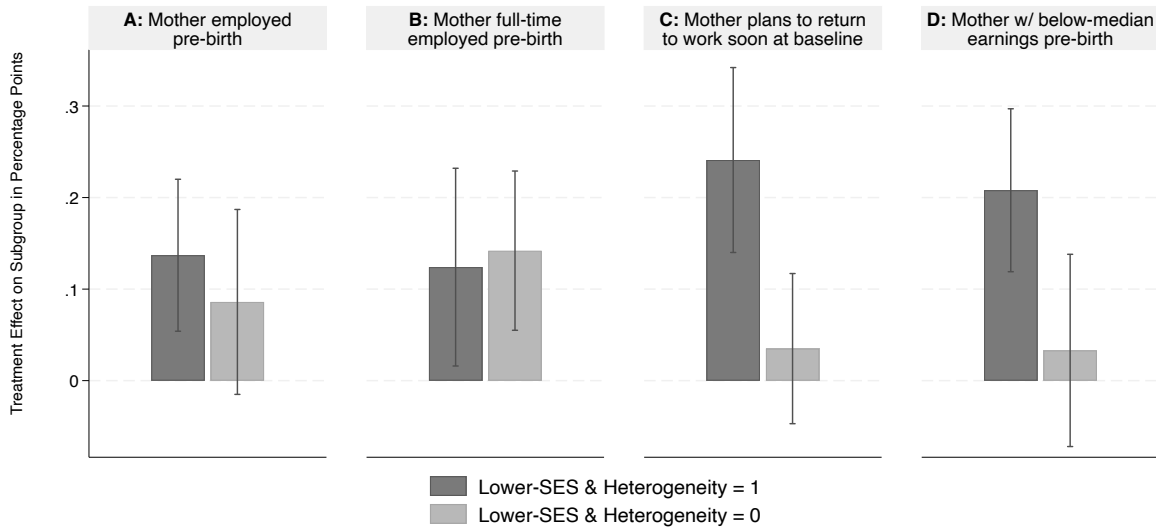
Notes: Table shows intention-to-treat effects on hours and months in child care, estimated by OLS. Both outcomes are measured in the post-treatment survey nine months after the treatment. In Column (1), for children enrolled in child care, hours are measured as the average number of hours per week that the child spends in child care (realized or planned); for children not enrolled in child care, hours are zero. In Column (2), for children enrolled in child care, expected number of months in early child care is calculated as the difference between (realized or planned) enrollment date and August 1, 2021 (i.e., when most children transfer from below-age-of-3 to above-age-of-3 child care, see Section 2.3 for details); for children not enrolled in child care, months are zero. The number of observations is smaller than in our main specification because the information on hours in child care and enrollment date is missing for some families. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table D2: Heterogeneous Treatment Effects on Child Care Enrollment

	High Child Age (1)	Migration Background (2)	Low Application Knowledge (3)	High Return Beliefs (4)	High Competition (5)
Treatment (Lower-SES, heterogeneity = 1)	0.219 (0.099)	0.278 (0.101)	0.213 (0.082)	0.220 (0.084)	0.278 (0.099)
Treatment × Higher-SES	-0.120 (0.133)	-0.398 (0.126)	-0.162 (0.121)	-0.271 (0.118)	-0.319 (0.124)
Higher-SES	0.153 (0.097)	0.332 (0.092)	0.202 (0.092)	0.359 (0.086)	0.279 (0.085)
Below-median (Low) child age	-0.120 (0.157)				
Treatment × Low child age	-0.128 (0.128)				
Higher-SES × Low child age	0.113 (0.131)				
Treatment × Higher-SES × Low child age	-0.087 (0.175)				
No migration background (Native)		0.152 (0.088)			
Treatment × Native		-0.181 (0.131)			
Higher-SES × Native		-0.174 (0.124)			
Treatment × Higher-SES × Native		0.362 (0.166)			
Above-median (High) Application knowledge			0.124 (0.097)		
Treatment × High knowledge			-0.121 (0.133)		
Higher-SES × High knowledge			0.020 (0.127)		
Treatment × Higher-SES × High knowledge			0.017 (0.176)		
Below-median (Low) return beliefs				0.121 (0.092)	
Treatment × Low return beliefs				-0.142 (0.129)	
Higher-SES × Low return beliefs				-0.283 (0.127)	
Treatment × Higher-SES × Low return beliefs				0.225 (0.169)	
Below-median (Low) competition					0.108 (0.091)
Treatment × Low competition					-0.222 (0.132)
Higher-SES × Low competition					-0.111 (0.125)
Treatment × Higher-SES × Low competition					0.288 (0.181)
Strata FE	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes
Treatment (Lower-SES, heterogeneity = 0)	0.092 (0.073)	0.097 (0.080)	0.093 (0.100)	0.078 (0.095)	0.057 (0.083)
Treatment (Higher-SES, heterogeneity = 1)	0.100 (0.090)	-0.121 (0.085)	0.051 (0.092)	-0.052 (0.085)	-0.041 (0.080)
Treatment (Higher-SES, heterogeneity = 0)	-0.115 (0.080)	0.060 (0.077)	-0.053 (0.076)	0.031 (0.079)	0.026 (0.090)
N	519	519	519	519	519

Notes: Table shows heterogeneity in intention-to-treat effects on enrollment in child care, all models estimated by OLS. Child care enrollment is measured in the post-treatment survey nine months after the treatment. Column (1) reports heterogeneity based on a dummy variable indicating above-median age of the child. In Column (2), migration background is equal to one if the parent was *not* born in Germany, zero otherwise. In Column (3), the dummy variable indicating low application knowledge is equal to one for parents with a below-median application knowledge index at baseline, zero otherwise. Beliefs about the returns to child care in Column (4) are based on the following question from the baseline survey: “Children who have been enrolled in child care show better development when starting school than children who have not been enrolled”. The dummy variable indicating high return beliefs is equal to one for parents with above-median return beliefs, zero otherwise. Column (5) reports our findings for treatment effect heterogeneity by the level of competition for child care slots. To calculate competition for slots, we divide the number of child care centers within a one-mile radius of each family’s home by the number of children aged 0–1 years at baseline living in that area. The dummy variable indicating high competition for child care slots is equal to one if parents live in an area with a below-median number of child care centers per 100 children (within each of the two sample cities), zero otherwise. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. Robust standard errors in parentheses.

Figure D1: Heterogeneous Treatment Effects by Maternal Pre-Birth Employment Status



Notes: Figure shows heterogeneous treatment effects (ITT) on child care enrollment for different subgroups within the sample of lower-SES mothers, based on OLS models shown in Table 2 in the paper using triple interactions. Panel A depicts heterogeneity based on a dummy variable indicating whether the mother worked pre-birth ($N = 128$ lower-SES mothers). In Panel B, we show heterogeneity based on a dummy variable indicating whether the mother worked full-time (i.e., at least 30 hours per week) pre-birth ($N = 74$ lower-SES mothers). Panel C shows effect heterogeneity for whether the mother planned to return to (full-time) work or to school, university, or vocational training within the next year after the survey ($N = 100$ lower-SES mothers). In Panel D, heterogeneity is based on a dummy variable indicating whether the mother's monthly net salary was below the SES-specific median net salary or if the mother did not work pre-birth at all ($N = 105$ lower-SES mothers). All maternal employment or earnings information was elicited in the baseline survey. Information on maternal pre-birth employment status is missing when the father participated in the survey (full sample size is therefore $N = 484$ in Panels A–C, and $N = 477$ in Panel D due to missings in reported pre-birth earnings). Within each panel, the left-hand bar shows the estimated treatment effect for the subgroup of lower-SES mothers to which the respective heterogeneity applies (e.g., mothers who worked pre-birth in Panel A); the right-hand bar shows the treatment effect for the remaining lower-SES mothers (e.g., mothers who did not work pre-birth in Panel A). Error bars show robust standard errors.

Table D3: Detailed Treatment Effects on Application Knowledge

	Indices			Correct Answer to Question on...					
	Mean (1)	PCA (2)	# Correct (3)	Legal Claim (4)	Age Legal Claim (5)	Financial Support (6)	Age Cost Free (7)	Apply to 1 Center (8)	Nearest Center (9)
Treatment	0.299 (0.151)	0.305 (0.147)	0.378 (0.190)	-0.099 (0.061)	0.157 (0.056)	0.010 (0.079)	0.118 (0.061)	0.035 (0.045)	0.129 (0.077)
Treatment × Higher-SES	0.103 (0.190)	0.092 (0.182)	0.117 (0.234)	0.170 (0.074)	-0.027 (0.077)	0.013 (0.098)	0.053 (0.077)	-0.001 (0.056)	-0.049 (0.095)
Higher-SES	0.064 (0.135)	0.042 (0.130)	0.089 (0.169)	-0.049 (0.058)	0.078 (0.052)	0.035 (0.074)	0.001 (0.059)	-0.023 (0.039)	0.036 (0.072)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Effect	0.402 (0.115)	0.398 (0.106)	0.495 (0.140)	0.071 (0.043)	0.131 (0.053)	0.023 (0.060)	0.171 (0.048)	0.034 (0.027)	0.079 (0.054)
Higher-SES									
Control Mean Higher-SES	0.143	0.141	3.837	0.830	0.163	0.673	0.626	0.912	0.633
Control Mean Lower-SES	-0.217	-0.213	3.330	0.856	0.062	0.619	0.402	0.918	0.474
Control Group SES Gap	0.361	0.354	0.507	-0.026	0.101	0.055	0.224	-0.006	0.158
N	519	519	519	519	519	519	519	519	519

Notes: Table shows intention-to-treat effects on application knowledge, all models are estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. In Column (1), the outcome is an index that combines answers to all six application knowledge questions to an average of z-scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007). In Column (2), the outcome is based on a principal component analysis using all six knowledge questions, which allows for unequal weights for the different items. The outcome in Column (3) is the simple count of correct answers. In Columns (4)–(9), we report results for each individual knowledge question (using linear probability models, the outcome variable is equal to one if the respective question was answered correctly, zero otherwise). ‘Legal Claim’ in Column (4) refers to the item “Parents have a legal claim to a child care slot.” (answer categories: true/false/don’t know). ‘Age Legal Claim’ in Column (5) refers to the item “At what age do you think a child has a legal claim for a child care slot?” (answer categories: [...] years/don’t know). ‘Financial Support’ in Column (6) refers to the item “Low-income families are financially supported by the state in paying the fees for a slot in child care.” (answer categories: true/false/don’t know). ‘Age Cost Free’ in Column (7) refers to the item “At what age is enrollment in a child care center in Rhineland-Palatinate free of charge?” (answer categories: from the day of birth/from the age of one year/from the age of two years/from the age of three years/never/don’t know). ‘Apply to 1 Center’ in Column (8) refers to the item “Parents may only apply for a child care slot at one child care center at a time.” (answer categories: true/false/don’t know). ‘Nearest Center’ in Column (9) refers to the item “Parents are required by law to always choose the nearest child care center for their children.” (answer categories: true/false/don’t know). *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table D4: Treatment Effects on Application Behavior — Count Variables

	#Applications (1)	#Visits (2)	#Calls (3)	#Emails (4)
Treatment	1.305 (0.602)	0.896 (0.383)	0.501 (0.504)	-0.344 (0.379)
Treatment \times Higher-SES	-0.744 (0.798)	-1.047 (0.450)	-0.503 (0.666)	0.563 (0.526)
Higher-SES	1.010 (0.569)	0.405 (0.359)	1.026 (0.461)	0.070 (0.412)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	0.562 (0.537)	-0.150 (0.261)	-0.003 (0.451)	0.218 (0.321)
Control Mean Higher-SES	3.821	2.007	1.897	0.671
Control Mean Lower-SES	2.600	1.454	1.134	0.821
Control Mean SES Gap	1.221	0.553	0.763	-0.150
N	511	515	514	514

Notes: Table shows intention-to-treat effects on application behavior measured by count variables, all models are estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. In Column (1), the outcome is the number of applications that parents have sent to child care centers (winsorized at the 95th percentile). In Column (2), the outcome is the number of child care centers that parents have visited on-site during the application process (winsorized at the 95th percentile). Columns (3) and (4) report results for the number of calls and emails that parents have sent to child care centers during the application process (both winsorized at the 95th percentile). *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table D5: Treatment Effects on Parents' Child Care Preferences

	Nine Months After Treatment			
	Enrollment Age	Enrollment Age <2y	Enrollment Age < 3y	Willingness to Pay
	(1)	(2)	(3)	(4)
Treatment	0.668 (1.603)	-0.068 (0.052)	-0.001 (0.068)	0.740 (21.651)
Treatment \times Higher-SES	0.485 (2.014)	-0.010 (0.067)	0.012 (0.081)	-17.212 (26.685)
Higher-SES	-1.229 (1.501)	0.041 (0.052)	0.041 (0.061)	36.473 (18.503)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	1.153 (1.208)	-0.078 (0.044)	0.011 (0.041)	-16.472 (14.347)
Control Mean Higher-SES	23.327	0.408	0.850	213.397
Control Mean Lower-SES	28.258	0.175	0.691	144.459
Control Mean SES Gap	-4.931	0.233	0.160	68.937
N	519	519	519	437

Notes: Table shows intention-to-treat effects on parental preferences for child care enrollment, all models are estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. Column (1) uses the planned enrollment age (in months) on a continuous scale as an outcome. In Column (2) (Column (3)), the outcome is a dummy variable equal to one if parents plan to enroll their child to child care (or have done so already) below the age of two (three) years, zero otherwise. In Column (4), the outcome is the stated willingness to pay for a child care slot (in EUR) per month. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table D6: Treatment Effects by Planned Enrollment Age

	Nine Months After Treatment	
	Enrolled	Enrolled
	(Planned Enrollment Age \leq 2y) (1)	(Planned Enrollment Age $>$ 2y) (2)
Treatment	0.153 (0.078)	-0.036 (0.098)
Treatment \times Higher-SES	-0.101 (0.105)	0.236 (0.214)
Higher-SES	0.177 (0.080)	-0.166 (0.160)
Strata FE	Yes	Yes
Further controls	Yes	Yes
Treatment Effect	0.052	0.200
Higher-SES	(0.069)	(0.161)
Control Mean Higher-SES	0.585	0.138
Control Mean Lower-SES	0.281	0.091
Control Mean SES Gap	0.303	0.047
N	397	122

Notes: Table shows intention-to-treat effects on child care enrollment by planned enrollment age at baseline, all models are estimated by OLS. Outcome is measured in the post-treatment survey nine months after the treatment. In Column (1), sample is restricted to families who planned to enroll their child at an age of 2 years or below at baseline; in Column (2), sample consists of families who planned to enroll their child at an age of above 2 years. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table D7: Characterization of Applicants for Child Care

	Lower-SES control (Mean) (1)	Lower-SES treatment (Mean) (2)	Higher-SES control (Mean) (3)	Pairwise t-test (p-value) (1)-(2)	Pairwise t-test (p-value) (1)-(3)	Pairwise t-test (p-value) (2)-(3)
Baseline variables						
Planned enrollment age (in months)	24.296	23.576	20.796	0.515	0.002	0.008
Age of child (in months)	7.582	7.455	6.511	0.826	0.054	0.063
Child is female	0.419	0.468	0.500	0.565	0.300	0.661
Parent is mother	0.952	0.937	0.968	0.707	0.573	0.286
Age of parent (in years)	29.572	31.025	32.659	0.119	0.000	0.019
Migration background	0.323	0.367	0.389	0.585	0.378	0.756
Parent currently working	0.097	0.063	0.111	0.465	0.766	0.253
Household income	1129.567	1166.373	1600.902	0.710	0.000	0.000
Application knowledge (baseline)	-0.007	-0.106	0.283	0.536	0.039	0.006
Post-treatment knowledge & appl. behavior						
Application knowledge (post-treatment)	-0.079	0.286	0.281	0.017	0.015	0.970
Number of applications	3.244	3.102	3.514	0.773	0.615	0.389
Visited center	0.774	0.872	0.855	0.130	0.171	0.736
Called center	0.403	0.359	0.464	0.595	0.434	0.142
Emailed center	0.328	0.103	0.136	0.001	0.002	0.483
Joint F-test for baseline variables				0.555	4.886	3.441
Joint F-test for post-treatment variables				3.029	3.723	0.387

Notes: Table provides a characterization of families who applied for child care by SES. Column (1) reports mean values for the lower-SES control group ($N = 62$), Column (2) mean values for the lower-SES treatment group ($N = 79$), and Column (3) mean values for the higher-SES control group ($N = 126$). Columns (4), (5), and (6) show the p-values of two-sided t-tests of the null hypothesis that values in Columns (1) and (2), (1) and (3), and (2) and (3), respectively, are equal. The upper part of the table reports baseline variables. Age of the child is the child's age measured in months on August 1, 2018. Child is female is a dummy equal to one if the child is female, zero otherwise. Parent is mother is a dummy equal to one if the interviewee is the child's biological mother, zero otherwise (remaining cases are all biological fathers). Migration background is a dummy equal to one if the parent was not born in Germany, zero otherwise. Parent currently working is a dummy equal to one if the parent was working at baseline (part-time or full-time), zero otherwise. Household income is the monthly equivalent household income in EUR. Application knowledge combines answers to all six application knowledge questions to an average of z-scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007). In terms of post-treatment variables (shown in the lower part of the table), we report the index of application knowledge (based on answers given in post-treatment survey), number of applications sent to child care centers, and three binary variables capturing application behavior: dummy variables equal to one if parents visited a child care center on-site, called a child care center, or contacted a child care center by email during the application process (zero otherwise).

Online Appendix E. Analysis of Child and Family Outcomes

Our study is motivated by robust evidence that participation in early child care has strong positive effects on the development of disadvantaged children (Felfe and Lalive, 2018; Schmutz, 2024). In this section, we present an exploratory analysis showing that our intervention, in line with these previous findings, tends to improve important short-run outcomes for lower-SES children and families.

To measure children’s development, our post-treatment survey includes seven survey items from the National Educational Panel Study (NEPS; see Blossfeld and Roßbach, 2019), a large longitudinal educational study in Germany. The items asked parents to state whether their child can perform various actions pertaining to cognitive skills (e.g., understanding instructions) and motor skills (e.g., walking up stairs). The full list of items is presented in Appendix Table E1. The table also compares the average responses to these items between the nationally representative NEPS (Columns (1) and (2)) and the control group of our sample (Columns (3) and (4)). Although parents in our sample generally report slightly higher skill levels (note that children in our sample are also slightly older than in the NEPS sample), it is reassuring that the difference between the overall mean and the lower-SES mean is very similar in both samples.

We find that our intervention tends to improve children’s skills. The index of child development that combines all seven child outcomes is 25% of a standard deviation higher for treated lower-SES children than for lower-SES children in the control group ($p = .035$; Column (1) of Table E2). Effects are somewhat more pronounced for motor skills ($p = .077$; Column (3)) than for cognitive skills ($p = .146$; Column (2)). Overall, these results suggest that lower-SES children benefited from the intervention in terms of their development. However, it is important to note that parent-reported child development measures must be interpreted cautiously because of potential reporting bias.

The observed short-run treatment effects could emerge directly from attending early child care centers or indirectly due to shifts in how parents invest in their children (Gelber and Isen, 2013). In terms of direct effects, our intervention yields an estimated increase of 4.2 months in early child care enrollment for lower-SES children (see Column (2) of Appendix Table D1), a magnitude for which Drange and Havnes (2019) find strong and positive long-term effects of child care attendance on children’s cognitive skills. Yet, indirect effects in the form of parental investments are likely crucial for treatment effects on child development to emerge, since only 40% of the children enrolled had actually started child care at the time of the post-treatment survey (and those who already started only

had a comparatively short exposure to child care). Below, we provide further evidence supporting this channel.

When considering the interplay between child care enrollment and parental investment, parents might either view child care enrollment as a *substitute* for or a *complement* to their own investments in their children. This perception influences whether they will increase or decrease their own investments once they secure a child care slot. Previous literature suggests that parents perceive child care as complementary to their own investments (Gelber and Isen, 2013; Jessen et al., 2022), and there are in fact multiple channels through which parents could learn *how* to improve their investments when applying for child care: They may learn about conducive activities with children from interactions with child care center educators during the application process, shift their attention towards certain behaviors prompted by questions during the application process, or gain insights from peer-to-peer interaction with other parents they meet while searching for a child care slot (also see Gelber and Isen, 2013). Moreover, by enrolling their child in a child care center, parents could potentially lighten their daily responsibilities, creating opportunities for more focused, quality interactions with their child (Jessen et al., 2022).

To investigate how our intervention affects parent-child interactions, we utilize data on the frequency of different parent-child activities gathered in the post-treatment survey. The survey items, derived from the German Socio-Economic Panel (SOEP), asked parents whether they engage in different activities with their children “daily”, “several times a week”, “once a week“, or “never”. In Table E3, we provide descriptive statistics on parent-child activities in the control group and benchmark our data with data from the SOEP, which is representative for the overall German population. Despite somewhat different age groups, our data and the SOEP data are well aligned in terms of both overall means and the difference to the lower-SES means.

Table E4 shows that our intervention leads to an increase in an index of conducive parent-child activities by 28% of a standard deviation ($p = .096$) for lower-SES families. Examining the individual items, treated lower-SES parents tend to be more likely to engage in conducive activities (i.e., reading picture books and stories) more than once a week, and less likely to engage in less conducive activities such as watching TV or tablet use. This evidence aligns with the notion that parents view their input as a complement to child care enrollment, leading them to increase their investments upon securing a child care slot. Such a pattern echoes findings in Gelber and Isen (2013), who show that granting randomly-chosen pre-school children the opportunity to attend Head Start substantially increases parental engagement with their children.

Table E1: Comparison of Child Development Outcomes with National Educational Panel Study

	NEPS		Control group	
	(average child age 16 months)		(average child age 17 months)	
	Mean	Mean	Mean	Mean
	All	Lower-SES	All	Lower-SES
	(1)	(2)	(3)	(4)
Overall cognitive skills	0.765	0.767	0.873	0.892
Cognitive skill (negation, shake head (y/n))	0.771	0.806	0.857	0.901
Cognitive skill (draw attention, show sth (y/n))	0.898	0.874	0.923	0.936
Cognitive skill (understand instructions (y/n))	0.878	0.871	0.923	0.914
Cognitive skill (fetch object when asked (y/n))	0.510	0.526	0.782	0.811
Overall motor skills	0.582	0.568	0.766	0.759
Motor skill (climbing up, e.g. chair (y/n))	0.705	0.672	0.850	0.830
Motor skill (walking up stairs (y/n))	0.612	0.641	0.841	0.840
Motor skill (building tower from two or more blocks (y/n))	0.433	0.397	0.582	0.570

Notes: Table shows means of parent-reported child development outcomes as the shares of parents reporting ‘yes’ to a specific item. Overall cognitive (motor) skills report the average of all four (three) items measuring cognitive (motor) skills. The analysis uses data from the representative National Educational Panel Study (NEPS; see Blossfeld and Roßbach, 2019; NEPS, 2023). Columns (1) and (2) show weighted averages for all children ($N = 2,778$) and children from lower-SES families respectively ($N = 1,022$) from a nationwide representative survey of newborn children. The so-called “newborn starting cohort” (SC 1) of the National Educational Panel Study (NEPS) sampled newborn children in 2012/2013. Children in this comparison sample are on average 16 months old when measuring child development, and 49% in the weighted sample are from lower-SES families. Columns (3) and (4) show the averages for the control group of our sample ($N = 234$) as well as for lower-SES families in the control group ($N = 94$). Children in our sample are on average 17 months old when measuring child development, and 40% are from lower-SES families.

Table E2: Treatment Effects on Child Development

	Child Development Indices		
	All Items (1)	Cognitive Skills (2)	Motor Skills (3)
Treatment	0.252 (0.119)	0.190 (0.131)	0.210 (0.119)
Treatment \times Higher-SES	-0.160 (0.151)	-0.006 (0.167)	-0.266 (0.154)
Higher-SES	0.035 (0.113)	-0.097 (0.133)	0.173 (0.119)
Strata FE	Yes	Yes	Yes
Further controls	Yes	Yes	Yes
Treatment Effect Higher-SES	0.092 (0.093)	0.185 (0.102)	-0.056 (0.105)
Control Mean Higher-SES	-0.024	-0.054	0.020
Control Mean Lower-SES	0.036	0.081	-0.030
Control Mean SES Gap	-0.060	-0.135	0.051
N	499	499	499

Notes: Table shows intention-to-treat effects on parent-reported child development outcomes, all models are estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. In Column (1), the outcome is an index that combines answers to all seven child outcome questions to an average of z-scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007). Analogously, the outcome in Column (2) (Column (3)) is an average of the z-scores of the four cognitive (three motor) child outcome questions (standardized to mean = 0 and SD = 1 in the control group). The number of observations for child outcomes is smaller compared to the full sample because the $N = 20$ participants who used the online questionnaire were not surveyed about child development. In case of item non-response, we use the mean of the remaining items. If we restrict the sample to only parents who answered *all* items, we lose a maximum of $N = 27$ observations, and treatment effects are very similar. *Further controls* include child care enrollment at baseline and a vector of sociodemographic controls (see Section 3.5); we also include child age squared to account for non-linear associations between child development and age. Imputation dummies for missing values in control variables are included. Robust standard errors in parentheses.

Table E3: Comparison of Parent-Child Activities with Socio-Economic Panel Study

	SOEP (average child age 28 months)		Control group (average child age 17 months)	
	Mean All (1)	Mean Lower-SES (2)	Mean All (3)	Mean Lower-SES (4)
Overall parent-child activities	0.807	0.814	0.721	0.730
Reading stories to the child	0.854	0.808	0.825	0.745
Looking a picture books with child	0.935	0.919	0.859	0.787
Watching Video or tv with child	0.633	0.715	0.479	0.660
Using tablet/smartphone with child	–	–	0.291	0.362

Notes: Table shows means of parent-reported parent-child activities with children as the share of parents reporting that they conduct the respective activity “daily” or “several times a week”, as compared to “once a week” and “never”. Overall parent-child activities refer to the averages of the first three activities (the tablet/smartphone item was only used in later SOEP cohorts which are not analyzed here). Columns (1) and (2) show weighted averages for all children ($N = 670$) and children from lower-SES families respectively ($N = 265$) from a nationwide representative household survey Socio-Economic Panel Study (SOEP, see Goebel et al., 2019). In the comparable SOEP sample generated from the surveys in 2017, 2018 or 2019, children are on average 28 months old when measuring parent-child activities, and 40% in the weighted sample are from lower-SES families. Columns (3) and (4) show the averages for the control group of our sample ($N = 234$) as well as for lower-SES families ($N = 94$). Children in our sample are on average 17 months old when measuring parent-child activities, and 40% are from lower-SES families.

Table E4: Treatment Effects on Conducive Parent-Child Activities

	Frequency of Parent-Child Activities				
	Index (1)	Read Picture Books (2)	Read Stories (3)	Watch TV/Videos (4)	Use Tablets (5)
Treatment	0.281 (0.168)	0.115 (0.062)	0.042 (0.071)	-0.065 (0.071)	-0.094 (0.072)
Treatment \times Higher-SES	-0.337 (0.195)	-0.148 (0.072)	-0.051 (0.083)	0.091 (0.090)	0.087 (0.088)
Higher-SES	0.448 (0.151)	0.098 (0.053)	0.134 (0.062)	-0.196 (0.068)	-0.098 (0.067)
Strata FE	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes
Treatment Effect	-0.057 (0.099)	-0.033 (0.038)	-0.009 (0.042)	0.026 (0.055)	-0.007 (0.050)
Control Mean Higher-SES	0.227	0.907	0.879	0.357	0.243
Control Mean Lower-SES	-0.338	0.787	0.745	0.660	0.362
Control Mean SES Gap	0.565	0.120	0.134	-0.302	-0.119
N	495	495	495	495	495

Notes: Table shows intention-to-treat effects on various parent-child activities, all models are estimated by OLS. All outcomes are measured in the post-treatment survey nine months after the treatment. In Column (1), the outcome is an index of conducive parent-child activities that combines all four activities to an average of z-scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007); less conducive activities (i.e., watch TV/videos and tablet use) are reversed when constructing the index. Columns (2)–(5) report treatment effects for each parent-child activity separately. In each column, the outcome is a dummy variable equal to one if parents report that they conduct the respective activity “daily” or “several times a week”, zero otherwise (the residual category includes “once a week” and “never”). The number of observations is smaller compared to the full sample because the $N = 20$ participants who used the online questionnaire were not surveyed about parent-child activities, and some additional item non-response. *Further controls* include child care enrollment at baseline and a vector of sociodemographic controls (see Section 3.5); we also include child age squared to account for non-linear associations between parent-child activities and age. Imputation dummies for missing values in control variables are included. Robust standard errors in parentheses.

Online Appendix F. Robustness Checks

Table F1: Treatment Effect on Enrollment Using Official Enrollment Confirmations

	Nine Months After Treatment	
	Enrolled (non-confirmed = 0) (1)	Enrolled (non-confirmed = dropped) (2)
Treatment	0.114 (0.047)	0.160 (0.053)
Treatment \times Higher-SES	-0.053 (0.068)	-0.092 (0.080)
Higher-SES	0.127 (0.049)	0.177 (0.058)
Strata FE	Yes	Yes
Further controls	Yes	Yes
Treatment Effect	0.061 (0.052)	0.068 (0.063)
Control Mean Higher-SES	0.238	0.321
Control Mean Lower-SES	0.082	0.095
Control Mean SES Gap	0.156	0.226
N	519	417

Notes: Table shows intention-to-treat effects on child care enrollment, estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. In Column (1), the outcome variable takes a value of one if respondents verify that their child is enrolled in child care by providing us with an official enrollment confirmation issued by the child care center, and zero otherwise. In Column (2), we exclude parents that do not verify their enrollment status with an official enrollment confirmation. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table F2: Randomization Inference and Corrections for Multiple Hypothesis Testing

	Coefficient (1)	Rand. Inference (2)	List-Shaikh-Xu (3)	Westfall-Young (4)	Romano-Wolf (5)
Panel A: Child Care Application and Enrollment (Table 2)					
Applied pooled	0.078	0.044	0.090	0.083	0.072
Enrolled pooled	0.051	0.218	0.265	0.237	0.239
Applied Lower-SES	0.213	0.005	0.007	0.004	0.005
Enrolled Lower-SES	0.159	0.008	0.007	0.005	0.006
Panel B: Heterogeneity Results (Figure 3 & Table D2), Treatment \times Lower-SES \times Heterogeneity					
Child age high	0.219	0.036	0.025	0.025	
Migration background	0.278	0.008	0.011	0.022	
Low knowledge	0.213	0.003	0.023	0.022	
High return beliefs	0.220	0.016	0.015	0.019	
High competition	0.278	0.007	0.016	0.022	
Panel C: Application Knowledge and Behavior (Table 3)					
Application knowledge	0.299	0.051	0.134	0.112	0.113
Visits (y/n)	0.259	0.002	0.005	0.006	0.007
Calls (y/n)	0.087	0.240	0.275	0.284	0.284
Emails (y/n)	-0.079	0.134	0.320	0.284	0.284

Notes: Table shows p-values for our main results when using randomization inference and adjusting for multiple hypothesis testing. All p-values $< .10$ are printed in **bold**. For comparison, Column (1) displays coefficients and significance stars representing p-values from robust standard errors as reported in the main tables. Randomization inference (RI) p-values in Column (2) are obtained from RI with 1,000 permutations, assigning the treatment status randomly within strata (using the Stata command ‘ritest’ by Hess, 2017). In Columns (3)–(5), we implement three different methods to correct for multiple hypothesis testing (controlling the family-wise error rates) using bootstrap resampling techniques. Column (3) uses the method by List et al. (2019), Column (4) the stepdown-approach by Westfall and Young (1993), and Column (5) the approach by Romano and Wolf (2005, 2016). The procedures by Westfall-Young (using the Stata command ‘wyoung’ by Julian Reif) and Romano-Wolf (using the Stata command ‘rwolf’ by Clarke et al. (2020)) account for the stratified randomization, that is, bootstrap samples are selected within each stratum. In Panel A, we correct for the fact that we use two main outcomes, child care application and enrollment. In Panel B, we correct for the multiple subgroups tested. In Panel C, we correct for the four different intermediate outcomes. For Romano-Wolf, we do not report values for interacted models because the Stata command does not allow for a heterogeneity analysis. Note that some corrected p-values are smaller than the original p-values because they are based on bootstrap methods. We do not report adjusted p-values for higher-SES families because of their insignificance in the main analysis. All control variables from the respective baseline specification are included.

Table F3: Treatment Effects on Child Care Application and Enrollment with Alternative SES Definitions

	Applied				Enrolled			
	SES-1 (main) (1)	SES-2 (2)	SES-3 (3)	SES-4 (4)	SES-1 (main) (5)	SES-2 (6)	SES-3 (7)	SES-4 (8)
Treatment	0.213 (0.072)	0.209 (0.085)	0.113 (0.065)	0.122 (0.064)	0.159 (0.062)	0.203 (0.067)	0.163 (0.058)	0.154 (0.058)
Higher-SES-1 (main = responding parent has college entrance qualification)	0.179 (0.066)				0.221 (0.064)			
Treatment \times Higher-SES-1 (main)	-0.214 (0.086)				-0.171 (0.085)			
Higher-SES-2 (at least one parent in HH has college entrance qualification)		0.119 (0.097)				0.107 (0.092)		
Treatment \times Higher-SES-2		-0.182 (0.096)				-0.212 (0.087)		
Higher-SES-3 (HH has college entrance qualification and not poor)			0.033 (0.083)				0.137 (0.087)	
Treatment \times Higher-SES-3			-0.062 (0.079)				-0.199 (0.086)	
Higher-SES-4 (HH has college entrance qualification, not poor, and no single parent)				0.046 (0.078)				0.145 (0.083)
Treatment \times Higher-SES-4				-0.079 (0.078)				-0.186 (0.085)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Effect Higher-SES	-0.000 (0.044)	0.027 (0.042)	0.051 (0.045)	0.043 (0.045)	-0.011 (0.060)	-0.009 (0.056)	-0.036 (0.064)	-0.032 (0.064)
Control Mean Higher-SES	0.857	0.837	0.865	0.870	0.497	0.488	0.586	0.588
Control Mean Lower-SES	0.639	0.611	0.658	0.655	0.216	0.139	0.144	0.150
Control Group SES Gap	0.218	0.226	0.207	0.215	0.280	0.349	0.442	0.437
N	519	519	519	519	519	519	519	519

Notes: Table shows our main results from Table 2 for different definitions of lower-SES families, see Section 4.5 for details. Columns (1) and (5) report results for our main definition of lower-SES families used throughout the paper (the responding parent not having a college entrance qualification, SES-1). In Columns (2) and (6), we define lower-SES as *neither* parent having a college entrance qualification (SES-2, 30% of the full sample). Columns (3) and (7) extend the definition for SES-2 by adding families with an equivalent household income below poverty line to the lower-SES sample (SES-3, 45% of the full sample, based on Falk et al. (2021)). Finally, Columns (4) and (8) extend the definition for SES-3 by adding single-parent families to the lower-SES sample (SES-4, 46% of the full sample, following Kosse et al. (2020)). *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table F4: Treatment Effects on Application and Enrollment If Respondents Named a Child Care Center

	Nine Months After Treatment			
	Applied (1)	Applied (named CC) (2)	Enrolled (3)	Enrolled (named CC) (4)
Treatment	0.213 (0.072)	0.225 (0.072)	0.159 (0.062)	0.160 (0.059)
Treatment \times Higher-SES	-0.214 (0.086)	-0.254 (0.085)	-0.171 (0.085)	-0.168 (0.083)
Higher-SES	0.179 (0.066)	0.210 (0.066)	0.221 (0.064)	0.197 (0.061)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect Higher-SES	-0.000 (0.044)	-0.029 (0.045)	-0.011 (0.060)	-0.007 (0.060)
Control Mean Higher-SES	0.857	0.837	0.497	0.456
Control Mean Lower-SES	0.639	0.588	0.216	0.196
Control Mean SES Gap	0.218	0.249	0.280	0.260
N	519	519	519	519

Notes: Table shows intention-to-treat effects on child care application and enrollment, all models are estimated by OLS. Columns (1) and (3) show our main treatment effects from Table 2. In Columns (2) and (4), the dummy variables indicating child care application and enrollment are equal to one if parents could name a specific child care center to which they have applied to or in which their child is enrolled; zero otherwise. Specifically, 96% of parents who stated that they applied for a child care slot named at least one child care center (95.7% in the treatment group, 95.5% in the control group); 93% of parents who stated that their child is enrolled in child care named the specific child care center (94.3% in the treatment group, 91.5% in the control group). *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table F5: Treatment Effects on Child Care Application and Enrollment — Mothers Only

	Nine Months After Treatment			
	Applied	Applied	Enrolled	Enrolled
Treatment	0.080 (0.038)	0.213 (0.073)	0.035 (0.047)	0.141 (0.065)
Treatment \times Higher-SES		-0.213 (0.086)		-0.167 (0.092)
Higher-SES	0.058 (0.046)	0.169 (0.065)	0.114 (0.053)	0.201 (0.070)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect		0.000		-0.027
Higher-SES		(0.043)		(0.064)
Control Mean Higher-SES	0.871	0.871	0.514	0.514
Control Mean Lower-SES	0.656	0.656	0.233	0.233
Control Mean SES Gap	0.216	0.216	0.281	0.281
N	484	484	484	484

Notes: Table shows intention-to-treat effects on child care application and enrollment, all models are estimated by OLS. Outcomes are measured in the post-treatment survey nine months after the treatment. This table reports results when restricting the sample to biological mothers. In Columns (1) and (2), the outcome variable takes a value of one if respondents state that they have applied for child care and zero otherwise; in Columns (3) and (4), the outcome variable takes a value of one if respondents state that their child is enrolled in child care and zero otherwise. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Online Appendix F.1. Attrition Analysis

All families who participated in the baseline survey were invited to take part in the post-treatment survey about nine months after the treatment. In total, 86% of the families (519 out of 607) decided to do so. In this section, we examine robustness to attrition in several ways. First, Table F6 shows that attrition is not selective based on treatment assignment or baseline outcomes. To construct the table, we regress an indicator of participation in the post-treatment survey on the treatment indicator, a higher-SES indicator, the baseline outcomes (i.e., enrollment in child care and application knowledge), and the interactions of the treatment indicator with the baseline variables. All coefficients are small and statistically insignificant. Hence, there is no evidence for selective attrition.

Second, Table F7 shows that the sample participating in the post-treatment survey is well balanced on baseline characteristics. This suggests that attrition is not causing treatment and control groups to differ on pre-existing characteristics.

Taken together, the evidence in Tables F6 and F7 indicates that treatment and control groups remain comparable in the post-treatment survey. However, we further examine the sensitivity of our results to attrition in two additional robustness checks.

Inverse Probability Weighting (IPW). We re-weight the observed data using the inverse probability of participation in the post-treatment survey. Due to this re-weighting, non-attriters become observationally similar to attriters. In a first step, we calculate the predicted probability of responding to the post-treatment survey from a probit model of a binary participation indicator regressed on treatment assignment (potentially interacted with the higher-SES indicator) and the baseline outcome. Then, we estimate our baseline models with attrition weights (i.e., the inverse of the probabilities obtained from the probit model). The re-weighted results for child care application and enrollment are shown in Table F8. Treatment effect sizes are virtually identical to the unweighted results.

Lee Bounds. IPW relies on the assumption that, conditional on observables, attrition is independent of the outcome. This arguably strong assumption is relaxed in the trimming procedure suggested by Lee (2009). This approach yields an interval for the true value of the treatment effect in the presence of non-random attrition. Interval estimates are based on extreme assumptions about selection: In the group that suffers less from attrition, either the largest or the smallest values of the outcome are regarded as “excess observations” and are excluded from the analysis. In Table F9, we show Lee bounds when we correct for attrition that differs between treatment and control group. Lee bounds reported in Table F10 conservatively correct for treatment–control differences in attrition

separately in the lower- and higher-SES samples. Because attrition is always very similar in treatment and control groups (also when we split the sample by SES), only few “excess observations” need to be excluded from the estimation samples (four in Table F9 and eleven in Table F10). Thus, the lower and upper bounds are both reasonably close to the point estimates of our treatment effects displayed in Table 2, and are statistically significant at 5% or better.

Table F6: Check for Selective Attrition

	Participation Post-Treatment		
	(1)	(2)	(3)
Treatment	-0.004 (0.017)	0.029 (0.026)	0.031 (0.029)
Treatment \times Higher-SES		-0.057 (0.036)	-0.064 (0.039)
Higher-SES	0.016 (0.021)	0.045 (0.028)	0.044 (0.030)
Treatment \times Enrolled in child care (baseline)			0.114 (0.187)
Enrolled in child care (baseline)			-0.067 (0.055)
Treatment \times Application knowledge (baseline)			0.015 (0.024)
Application knowledge (baseline)			0.018 (0.020)
Strata FE	Yes	Yes	Yes
Further controls	Yes	Yes	Yes
N	607	607	607
<u>Attrition F-test p-values</u>			
Overall sample	0.828	0.281	0.460
Lower-SES sample		0.268	0.373
Higher-SES sample		0.235	0.598

Notes: Table shows results from OLS models. The outcome variable takes a value of one if the family participates in the post-treatment survey nine months after treatment, zero otherwise. *Further controls* include survey date fixed effects and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. F-test p-values report p-values from joint significance tests of all treatment-related coefficients for the indicated sample. Robust standard errors in parentheses.

Table F7: Sample Descriptives and Balancing Tests of Baseline Variables in Post-Treatment Sample

	All (1)	Control (2)	Treatment (3)	$\Delta(3)-(2)$ (4)	p-val for (4) (5)	p-val by SES (6)
Pre-treatment outcomes						
Enrolled in child care	0.013	0.016	0.011	-0.005	0.594	0.349
Application knowledge (# corr.)	3.545	3.545	3.545	0.000	0.997	0.796
Application knowledge (Index)	0.057	0.047	0.065	0.018	0.837	0.714
Child characteristics						
Age of child (in months)	6.839	7.012	6.684	-0.328	0.293	0.592
Child is female	0.487	0.480	0.495	0.015	0.733	0.934
Parent characteristics						
Parent is mother	0.933	0.943	0.924	-0.019	0.387	0.447
Age of parent (in years)	31.74	31.33	32.11	0.78	0.091	0.203
Migration background	0.380	0.373	0.385	0.013	0.770	0.829
Household income	1421.4	1376.0	1461.6	85.6	0.221	0.353
No school degree	0.039	0.029	0.047	0.019	0.266	
Lower secondary degree	0.112	0.111	0.113	0.002	0.941	
Middle secondary degree	0.231	0.242	0.222	-0.020	0.591	
College entrance qualification	0.611	0.602	0.618	0.016	0.715	
Mother currently on parental leave	0.775	0.754	0.793	0.039	0.296	0.557
Mother currently working	0.083	0.086	0.080	-0.006	0.803	0.509
Intention to use child care (< 6y)	0.986	0.979	0.993	0.014	0.201	0.436
Intention to use child care (< 3y)	0.822	0.797	0.844	0.047	0.172	0.320
N	519	244	275			

Notes: Table reports mean values for outcomes and sociodemographic characteristics at baseline in our sample of respondents who participated in the post-treatment survey nine months after treatment. Column (1) reports mean values for the full sample, Column (2) mean values for the control group, and Column (3) mean values for the treatment group. In Column (4), we display the difference between treatment and control group, and Column (5) shows the corresponding p-value for a two-sided t-test of the hypothesis that values in Columns (2) and (3) are equal. In Column (6), we test whether there are treatment-control differences in the respective variable within SES subgroups. To do so, we regress the variable on the treatment indicator, the higher-SES dummy, and their interaction. Column (6) reports the p-value of an F-test of joint significance of the coefficients on the treatment indicator and its interaction with the higher-SES dummy. Enrolled in child care is a dummy equal to one if parents report that their child is enrolled in child care, zero otherwise. Application knowledge (# corr.) is the average number of knowledge questions about the child care application process answered correctly (out of six questions in total). Application knowledge (Index) combines answers to all six application knowledge questions to an average of z-scores (standardized to mean = 0 and SD = 1 in the control group, Kling et al., 2007). Age of the child is the child’s age measured in months on August 1, 2018. Female is a dummy equal to one if the child is female, zero otherwise. Intention to use child care (< 6y) is a dummy variable equal to one if parents report that they want any child care for their child, zero otherwise. Intention to use child care (< 3y) is a dummy variable equal to one if parents report that they want to enroll their child into child care below the age of three, zero otherwise. Parent is mother is a dummy equal to one if the interviewee is the child’s biological mother, zero otherwise (the remaining cases are all biological fathers). Migration background is a dummy equal to one if the parent was not born in Germany, zero otherwise. Mother currently on parental leave is a dummy variable equal to one if the mother is on parental leave at baseline, zero otherwise. Mother currently working is a dummy equal to one if the parent was working at baseline (part-time or full-time), zero otherwise. Household income is the monthly equivalent household income in EUR. No school degree, Lower secondary degree, Middle secondary degree (“MSA”), and College entrance qualification (“Abitur”) are all dummy variables indicating a parent’s highest school degree. Intention to use child care (< 6y) is a dummy variable equal to one if parents report that they want any child care for their child, zero otherwise. Intention to use child care (< 3y) is a dummy variable equal to one if parents report that they want to enroll their child into child care below the age of three, zero otherwise.

Table F8: Treatment Effects on Child Care Application and Enrollment Using Inverse Probability Weighting

	Applied			Enrolled		
	Unweighted (1)	Baseline Sample Weights I (2)	Baseline Sample Weights II (3)	Unweighted (4)	Baseline Sample Weights I (5)	Baseline Sample Weights II (6)
Treatment	0.213 (0.072)	0.213 (0.072)	0.213 (0.072)	0.159 (0.062)	0.159 (0.062)	0.156 (0.061)
Treatment × Higher-SES	-0.214 (0.086)	-0.213 (0.086)	-0.214 (0.086)	-0.171 (0.085)	-0.170 (0.085)	-0.169 (0.085)
Higher-SES	0.179 (0.066)	0.178 (0.066)	0.178 (0.066)	0.221 (0.064)	0.220 (0.064)	0.221 (0.064)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Effect Higher-SES	-0.000 (0.044)	-0.000 (0.044)	-0.001 (0.045)	-0.011 (0.060)	-0.011 (0.060)	-0.014 (0.060)
Control Mean Higher-SES	0.857	0.857	0.857	0.497	0.497	0.497
Control Mean Lower-SES	0.639	0.639	0.639	0.216	0.216	0.216
Control Mean SES Gap	0.218	0.218	0.218	0.280	0.280	0.280
N	519	519	519	519	519	519

Notes: Table shows treatment effects using inverse probability weighting to account for attrition. The dependent variable is child care application in Columns (1)–(3) and child care enrollment in Columns (4)–(6). Coefficients are weighted least squares estimates, with attrition weights being the inverse of the predicted probability of responding in the post-treatment survey. In Columns (2) and (5), the probability of responding is derived from a probit model of the binary participation indicator as function of treatment assignment and pre-treatment outcome; in Columns (3) and (6), the probability of responding is derived from a probit model of the binary participation indicator as function of treatment assignment, higher-SES indicator, their interaction, and pre-treatment outcome. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table F9: Treatment Effect Bounds (Correcting for Potentially Selective Attrition by Treatment Status)

	Applied		Enrolled	
	Lower Bound (1)	Upper Bound (2)	Lower Bound (3)	Upper Bound (4)
Treatment	0.211 (0.072)	0.227 (0.073)	0.163 (0.062)	0.165 (0.062)
Treatment \times Higher-SES	-0.213 (0.086)	-0.219 (0.086)	-0.191 (0.085)	-0.175 (0.086)
Higher-SES	0.181 (0.066)	0.184 (0.066)	0.227 (0.064)	0.223 (0.064)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect Higher-SES	-0.002 (0.044)	0.007 (0.043)	-0.028 (0.060)	-0.010 (0.060)
Control Mean Higher-SES	0.857	0.857	0.497	0.497
Control Mean Lower-SES	0.639	0.639	0.216	0.216
Control Mean SES Gap	0.218	0.218	0.280	0.280
N	515	515	515	515

Notes: Table shows treatment effect bounds for child care application (Columns (1) and (2)) and enrollment (Columns (3) and (4)). The bounds are estimated using the procedure suggested by Lee (2009), which involves trimming observations from the group that experienced less attrition (i.e., either the largest or the smallest values of the outcome are regarded as “excess observations” and are excluded from the sample). The trimming analysis accounts for differences in attrition between treatment and control group. The difference in participation rates between treatment and control groups is 0.9 pp, with a participation rate in the treatment group of 85.9%. Therefore, we trim $.9/85.9 = 1.1\%$ of the treated observations ($N = 4$), with the lower bound occurring when we exclude families who do apply or enroll, and the upper bound when we exclude families who do not apply or enroll. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Table F10: Treatment Effect Bounds (Correcting for Potentially Selective Attrition by Treatment Status and SES)

	Applied		Enrolled	
	Lower Bound (1)	Upper Bound (2)	Lower Bound (3)	Upper Bound (4)
Treatment	0.200 (0.074)	0.246 (0.072)	0.121 (0.061)	0.176 (0.064)
Treatment \times Higher-SES	-0.193 (0.088)	-0.266 (0.086)	-0.118 (0.085)	-0.201 (0.087)
Higher-SES	0.173 (0.067)	0.197 (0.066)	0.210 (0.064)	0.227 (0.065)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect Higher-SES	0.007 (0.045)	-0.020 (0.042)	0.003 (0.060)	-0.025 (0.061)
Control Mean Higher-SES	0.853	0.881	0.483	0.510
Control Mean Lower-SES	0.639	0.639	0.216	0.216
Control Mean SES Gap	0.214	0.242	0.266	0.294
N	508	508	508	508

Notes: Table shows treatment effect bounds for child care application (Columns (1) and (2)) and enrollment (Columns (3) and (4)). The bounds are estimated using the procedure suggested by Lee (2009), which involves trimming observations from the group that experienced less attrition (i.e., either the largest or the smallest values of the outcome are regarded as “excess observations” and are excluded from the sample). The trimming analysis accounts for differences in attrition between treatment and control groups by SES. The difference in participation rates between treatment and control groups is 4.4 pp in the lower-SES sample (leading to the exclusion of 5.4% ($N = 7$) treated lower-SES observations) and is -2.4 pp in the higher-SES sample (leading to the exclusion of 2.6% ($N = 4$) control group higher-SES observations). The lower bound occurs when we trim observations for parents who do apply or enroll, and the upper bound when we trim observations for parents who do not apply or enroll. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. Robust standard errors in parentheses.

Online Appendix F.2. Representativeness

To further investigate the representativeness of our results, we use data from the German Socio-Economic Panel (SOEP) (Goebel et al., 2019). These annually collected data are representative of the German population, with about 15,000 households and 30,000 individuals participating in the survey. We restrict the SOEP sample to the target population of our study, that is, mothers with children born in 2017 and 2018 in Germany ($N = 502$ mothers). We add our study sample to the SOEP data and construct propensity score weights from a probit model of a binary variable indicating whether the family participated in our baseline survey regressed on migration background, SES, net equivalent household income, and current employment status. Treatment effect estimates with these SOEP population weights are very similar to the unweighted estimates (see Table F11).

Table F11: Treatment Effects on Child Care Application and Enrollment Using SOEP Population Weights

	Applied		Enrolled	
	Unweighted	SOEP Weights	Unweighted	SOEP Weights
Treatment	0.213 (0.072)	0.212 (0.071)	0.159 (0.062)	0.154 (0.066)
Treatment \times Higher-SES	-0.214 (0.086)	-0.214 (0.085)	-0.171 (0.085)	-0.174 (0.091)
Higher-SES	0.179 (0.066)	0.176 (0.066)	0.221 (0.064)	0.217 (0.067)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	-0.000 (0.044)	-0.002 (0.043)	-0.011 (0.060)	-0.020 (0.063)
Control Mean Higher-SES	0.857	0.867	0.497	0.516
Control Mean Lower-SES	0.639	0.651	0.216	0.237
Control Mean SES Gap	0.218	0.216	0.280	0.279
N	519	519	519	519

Notes: Table shows treatment effects using propensity score weights derived from representative German survey data (German Socio-Economic Panel, SOEP). The dependent variable is child care application in Columns (1) and (2) and child care enrollment in Columns (3) and (4). Regressions in Columns (2) and (4) are re-weighted such that our sample is representative of the overall population of mothers with young children in Germany. Weights are derived from a probit model of a binary variable (0: SOEP mothers with children born in 2017 and 2018; 1: families participating in our baseline survey (see Section 3.1)) regressed on migration background, SES, net equivalent household income, and current employment status. *Further controls* include baseline outcome value, survey date fixed effects, and a vector of sociodemographic controls (see Section 3.5 for details). Imputation dummies for missing values in control variables are included. *Control Mean Higher-SES (Lower-SES)* is the mean of the respective outcome in the control group in the post-treatment survey for higher-SES (lower-SES) parents; *Control Group SES Gap* reports the difference between control means of higher- and lower-SES parents. In Columns (2) and (4), control means are re-weighted with the SOEP weights. Robust standard errors in parentheses.

Online Appendix F.3. Potential Displacement Effects

Table F12: Analysis of Potential Displacement Effects

	Enrolled into Child Care					
	Control Group (1)	Only Lower-SES (2)	Only Higher-SES (3)	Control Group (4)	Only Lower-SES (5)	Only Higher-SES (6)
Share of households in treatment	0.004 (0.015)	-0.002 (0.028)	0.008 (0.019)			
High share of households in treatment				-0.038 (0.061)	-0.035 (0.101)	-0.039 (0.082)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
(High) Share of households in sample	Yes	Yes	Yes	Yes	Yes	Yes
N	244	97	147	244	97	147

Notes: Table analyzes potential displacement effects of treated participants on participants in the control group, all models are estimated by OLS. The outcome variable takes a value of one if respondents state that they are enrolled in child care and zero otherwise, measured in the post-treatment survey nine months after the treatment. In Columns (1) and (4), we estimate models for the full control group; in Columns (2) and (5) (Columns (3) and (6)), we only use the subsample of lower-SES (higher-SES) families. In this table, we use the exact location information about all households with a child aged 0–1 years in the cities we study. In Columns (1)–(3), we control for the share of households that are in the treatment group within in an area of one mile around the responding household; values range from 0 to 19%, mean: 13%. To account for areas with a generally higher or lower participation in the study, we additionally control for the share of participating households (i.e., in treatment plus control group) in this area; values range from 0 to 34%, mean: 23%. Columns (4)–(6) use — instead of the continuous variables from Columns (1)–(3) — dummy variables taking a value of one if, within an area of one mile around the responding household, there is an above-median share of households in the treatment group and in the full sample, respectively (zero otherwise). In addition, we control for baseline outcome values and strata fixed effects. Robust standard errors in parentheses.

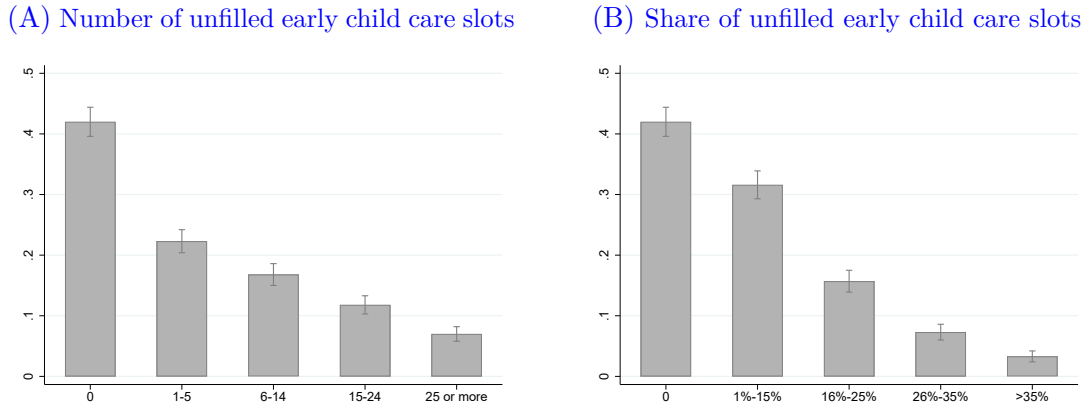
Online Appendix F.4. Survey of Child Care Center Managers in Germany

In Section 4.5, we provide evidence against the existence of displacement effects by showing that the likelihood that a control-group family’s child is enrolled in child care is unrelated to the share of treated households in the family’s vicinity. This raises the question of how treated families in a rationed market were able to secure child care slots without affecting other families’ chances of enrolling in child care. One potential reason for the absence of displacement effects of our treatment are inefficiencies in the allocation of slots in the German market for early child care, discussed in Section 2. These inefficiencies, which mainly result from decentralized, non-transparent admission processes, could lead to unfilled slots in child care — leaving room for treated families to enroll their child in child care without negatively affecting other families’ enrollment chances.

Unfortunately, information about unfilled child care slots is not publicly available in Germany. In an effort to better understand the process of allocating early child care slots, we conducted a Germany-wide survey with child care center managers responsible for selecting and admitting children to their centers. The sample was drawn from a commercially available data set comprising contact details for nearly the universe of child care centers in Germany. The data set comprises about 35,000 child care centers, and approximately 90% of them provide child care for children below the age of three years. We drew a randomly selected subset of child care centers among those targeting children below the age of three years, and sent out email invitations to participate in our online survey to 6,000 child care centers. We specifically asked the child care center *manager* to take part in our survey. Managers who completed the survey received an unconditional average cash incentive of 5 EUR. The survey was conducted in fall 2020, and we sent out an initial invitation plus two reminder emails. In total, 440 child care center managers participated in the survey.

The aim of the survey was to improve our understanding about the process of allocating child care slots in Germany. For example, child care center managers reported the number of unfilled slots and rated the importance of several application strategies, such as on-site visits to the center. The distribution of unfilled slots for children below the age of three years is displayed in Figure F1. On average, at the time of the survey, there was at least one unfilled slot in more than half of the early child care centers (58%); about one-third of the centers (37%) even report more than five open slots (see Panel A of Figure F1). A very similar picture emerges if we look at the share of unfilled slots at early child care centers (see Panel B of Figure F1): for about one-third of child care centers (32%), up to 15% of their slots are unfilled, and around 10% of child care centers even report a

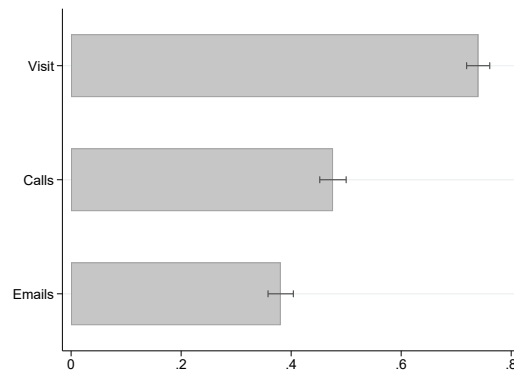
Figure F1: Unfilled Early Child Care Slots



share of unfilled slots of more than 25%. Overall, the data suggest that having unfilled slots (even several of them) is common for child care centers. Because the early child care market is generally characterized by demand for slots exceeding supply, the large numbers of unfilled slots that we document indicate substantial market inefficiencies in slot allocation. These inefficiencies leave room for an increased number of slots being allocated to treated families without negatively affecting families in the control group (or out-of-sample families).

In addition, we asked child care center managers to rate — on a scale from 1 “very important” to 5 “very unimportant” — the importance of several application strategies for successfully obtaining a child care slot in their center. Figure F2 shows the share of child care center managers who consider a strategy to be very important or important (values of 1 and 2). Almost three-quarters (74%) of managers emphasize that *personal on-site visits at the child care center* are important or very important for securing a slot; a

Figure F2: Importance of Different Application Strategies for Obtaining Child Care Slot



much smaller share of managers considers *phone calls* (48%) or *emails* (38%) as important or very important. These survey results indicate the relevance of on-site visits to child care centers for obtaining a child care slot.

Online Appendix G. Experimental Material

Online Appendix G.1. Corporate Design

Figure G1: Study Logo and Examples for Design of Materials for Parents

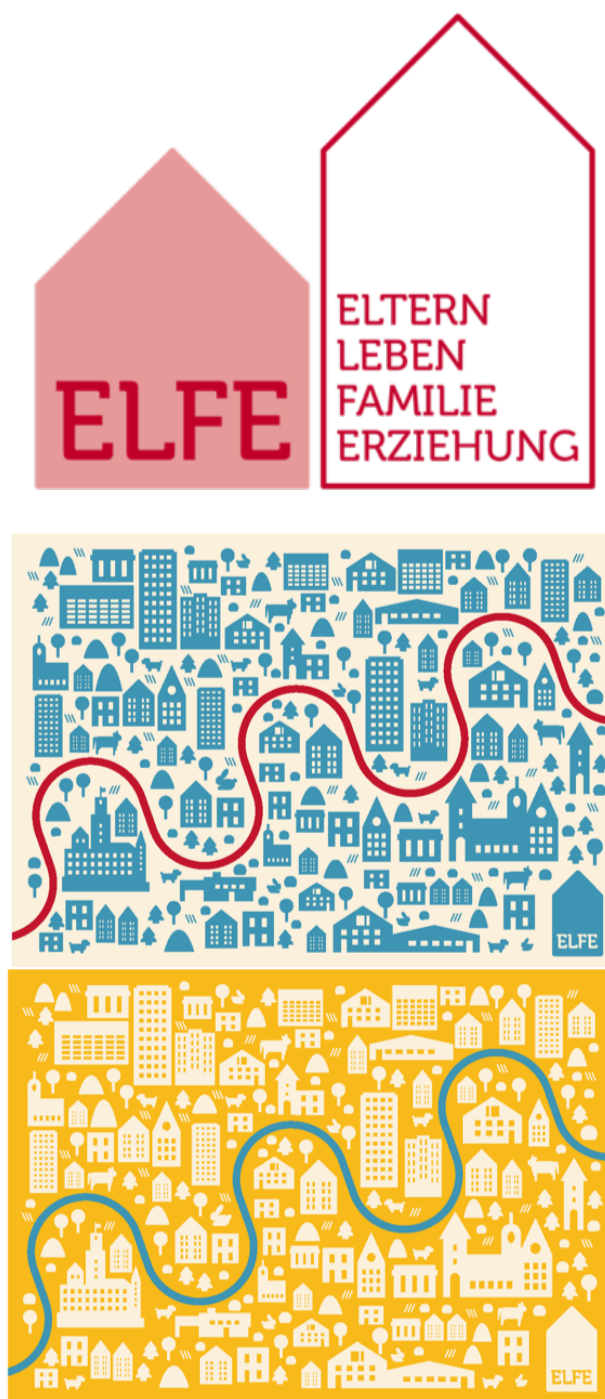


Figure G2: Slide 1 of the Information Video Shown to the Treatment Group



Audio Slide 1: Who should take care of my child and where? Almost all parents with young children in Germany face these questions.

Figure G3: Slide 2 of the Information Video Shown to the Treatment Group



Audio Slide 2: This short video summarizes the most important information about child care for you. There are many different ways to care for your child. You can choose! Basically, you can look after your child yourself at home, your child could attend a child care center, or, for example, a nanny could take care of your child. The decision is entirely up to you.

Figure G4: Slide 3 of the Information Video Shown to the Treatment Group



Audio Slide 3: Access to a child care slot.

Figure G5: Slide 4 of the Information Video Shown to the Treatment Group



Audio Slide 4: Many parents think that their child cannot attend child care because there are no slots available or it is too expensive. But is this really the case? In Germany, all parents have a legal entitlement to a child care slot for their children from their first birthday onward. This applies without exceptions. And the legal entitlement also applies regardless of whether parents work or not.

Figure G6: Slide 5 of the Information Video Shown to the Treatment Group



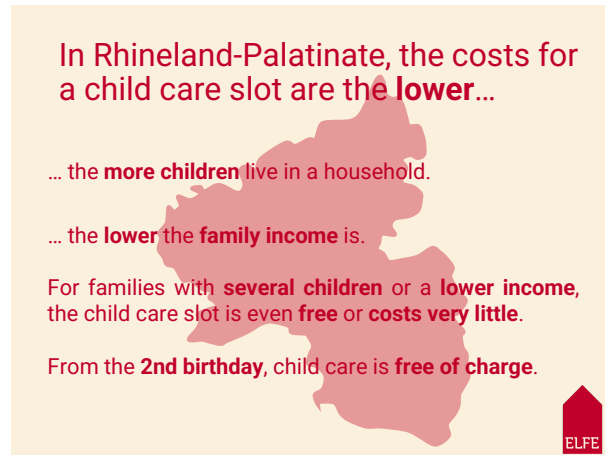
Audio Slide 5: You can choose the child care slot yourself. If you cannot find a slot in a child care center of your choice, the following applies: your city must offer you an alternative slot in child care, for example, at another child care center.

Figure G7: Slide 6 of the Information Video Shown to the Treatment Group



Audio Slide 6: Child care costs: Many parents think that a child care slot is very expensive. But how much does a child care slot really cost?

Figure G8: Slide 7 of the Information Video Shown to the Treatment Group



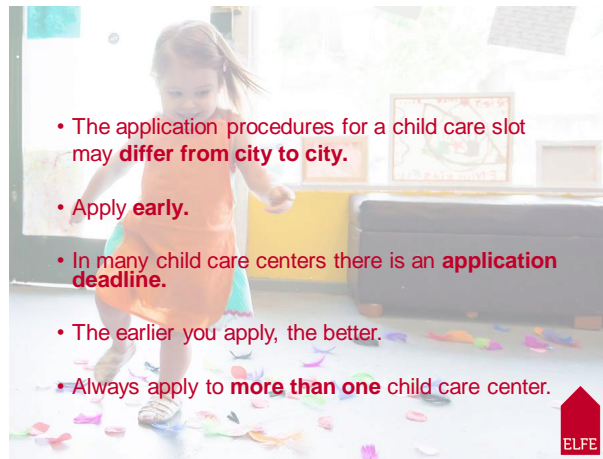
Audio Slide 7: In Rhineland-Palatinate, the costs for a child care slot are the lower the more children live in a household and the lower the family income is. For families with several children or a lower household income, child care is often even free or costs very little. From a child's second birthday, child care is even free of charge.

Figure G9: Slide 8 of the Information Video Shown to the Treatment Group



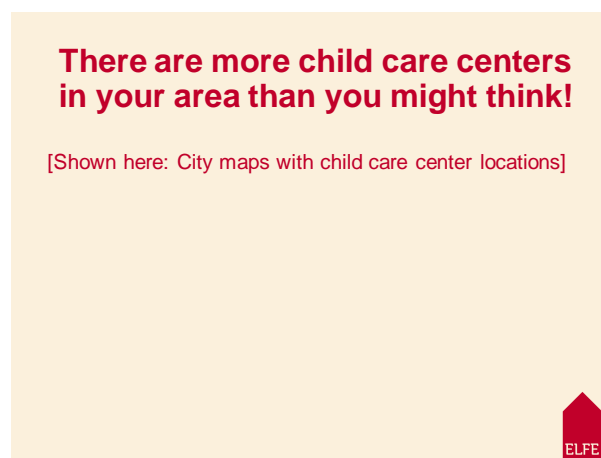
Audio Slide 8: Application for a child care slot.

Figure G10: Slide 9 of the Information Video Shown to the Treatment Group



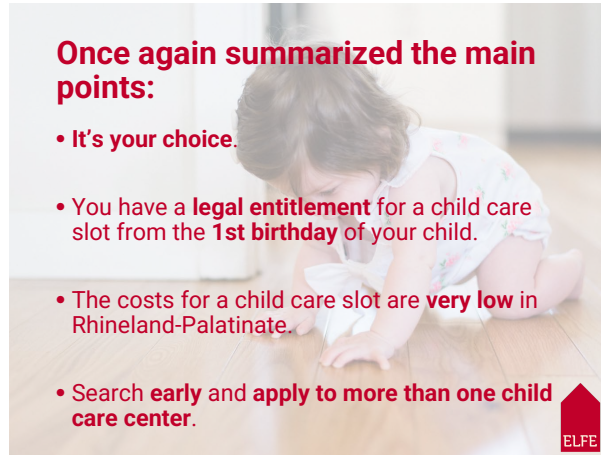
Audio Slide 9: If you decide to apply for child care, finding a slot is really not rocket science. The application procedures for a child care slot may differ from city to city. However, the following always applies: apply early. This increases your chances of finding a slot. In many child care centers, there is an application deadline. But even if there is no application deadline, the earlier you apply, the better. In any case, apply to more than one child care center! This will increase your chances of getting a slot at the child care center of your choice.

Figure G11: Slide 10 of the Information Video Shown to the Treatment Group



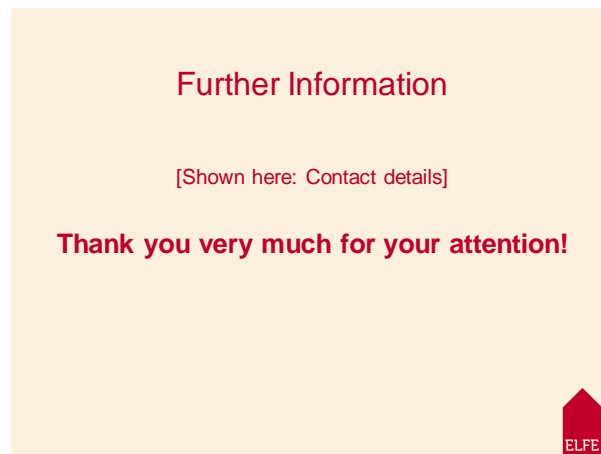
Audio Slide 10: Here you can see that there are more child care centers in your area than you might think!

Figure G12: Slide 11 of the Information Video Shown to the Treatment Group



Audio Slide 11: Once again, we summarize the main points. It's your choice whether you want to care for your child yourself at home or whether you want to enroll your child in child care. You have a legal entitlement to child care from the day your child is one year old. The costs for child care are very low in Rhineland-Palatinate, and, from the day your child is two years old, child care is even free of charge. If you would like to enroll your child in child care, search early and apply to more than one child care center. If you have any questions or need support regarding child care, please contact our staff. We will gladly help you!

Figure G13: Slide 12 of the Information Video Shown to the Treatment Group



Audio Slide 12: Further information can also be found at [webpage]. Thank you very much for your attention! We wish you and your family all the best and thank you for participating in the ELFE study.

References

- Abitare a Roma (2013). Asilo nido, adesso l'iscrizione passa dal Caf. <https://abitareroma.it/asilo-nido-adesso-liscrizione-passa-dal-caf/>.
- Arbeiterkammer Oberösterreich (2023). Krabbelstube & Co: Betreuungsmöglichkeiten für unter 3-Jährige. https://ooe.arbeiterkammer.at/beratung/bildung/kindergartenundco/Krabbelstube___Co.html.
- Blossfeld, H.-P. and H.-G. Roßbach (Eds.) (2019). *Education as a Lifelong Process: The German National Educational Panel Study (NEPS)*. Wiesbaden: Springer VS.
- Catalyst Kids (2023). Infant & Toddler Programs. <https://www.catalystkids.org/programs/infant-toddler/>.
- ChildCare.gov (2023). Look, Listen, and Ask. Tips for Choosing a Quality Child Care Center. <https://childcare.gov/sites/default/files/2022-03/Choosing-a-Child-Care-Center.pdf>.
- City of Amsterdam (2023). Childcare. <https://www.amsterdam.nl/en/family-youth/childcare/>.
- City of Copenhagen (2023). Public childcare. <https://international.kk.dk/live/childcare-and-school/childcare-services/public-childcare>.
- Clarke, D., J. P. Romano, and M. Wolf (2020). The Romano-Wolf Multiple-Hypothesis Correction in Stata. *The Stata Journal* 20(4), 812–843.
- Cui, J. and Natzke, L. and Grady, S. (2021). Early Childcare Program Participation: 2019. First Look. , National Center for Education Statistics at IES, Washington D.C.
- Del Boca, D., C. Pronzato, and G. Sorrenti (2016). When Rationing Plays a Role: Selection Criteria in the Italian Early Childcare System. *CESifo Economic Studies* 62(4), 752–775.
- Department of Education (2021). Survey of Childcare and Early Year Providers: Main Summary, England. , Department of Education, London.
- Drange, N. and T. Havnes (2019). Early Childcare and Cognitive Development: Evidence from an Assignment Lottery. *Journal of Labor Economics* 37(2), 581–620.
- Esping-Andersen, G., I. Garfinkel, W. J. Han, K. Magnuson, S. Wagner, and J. Waldfogel (2012). Child care and school performance in Denmark and the United States. *Children and Youth Services Review* 34(3), 576–589.
- Eurydice (2019). Key Data on Early Childhood Education and Care in Europe. Eurydice Report, Publications Office of the European Union, Luxembourg.
- Eurydice (2022). Norway. Early Childhood Education and Care. <https://eurydice.eacea.ec.europa.eu/national-education-systems/norway/early-childhood-education-and-care>.

- Expat in France (2022). How to get a place in a crèche for your baby. <https://expat-in-france.com/place-creche/>.
- Expatica (2022). Daycare and preschool in Spain. <https://www.expatica.com/es/living/family/preschool-spain-107654/>.
- Falk, A., F. Kosse, P. Pinger, H. Schildberg-Hoerisch, and T. Deckers (2021). Socio-Economic Status and Inequalities in Children’s IQ and Economic Preferences. *Journal of Political Economy* 129(9), 2504–2545.
- Felfe, C. and R. Lalive (2018). Does Early Child Care Affect Children’s Development? *Journal of Public Economics* 159, 33–53.
- Flisi, S., Z. Blasko, and E. Stepanova (2022). Indicators for early childhood education and care – Reconsidering some aspects of the Barcelona target for younger children. , Publications Office of the European Union, Luxembourg.
- Gelber, A. and A. Isen (2013). Children’s schooling and parents’ behavior: Evidence from the head start impact study. *Journal of Public Economics* 101, 25–38.
- Goebel, J., M. M. Grabka, S. Liebig, M. Kroh, D. Richter, C. Schroeder, and J. Schupp (2019). The German Socio-Economic Panel (SOEP). *Journal of Economics and Statistics* 239(2), 345–360.
- Gov.pol (2022). Zapisz dziecko do żłobka. <https://www.gov.pl/web/gov/zapisz-dziecko-do-zlobka#:~:text=Co%20musisz%20zrobi%C4%87,%2C%20w%20innych%20E2%80%93%20przez%20internet.>
- Hess, S. (2017). Randomization Inference with Stata: A Guide and Software. *Stata Journal* 17(3), 630–651.
- Hufkens, T., F. Figari, D. Vandelanootte, and G. Verbist (2020). Investing in subsidized childcare to reduce poverty. *Journal of European Social Policy* 30(3), 306–319.
- Independent.ie (2022). Parents are warned of two-year wait to secure a creche place for their child. <https://www.independent.ie/irish-news/parents-are-warned-of-two-year-wait-to-secure-a-creche-place-for-their-child-42016038.html>.
- Irish Examiner (2023). “We have 180 people on our waiting list”: childcare sector crisis continues. <https://www.irishexaminer.com/news/spotlight/arid-41033759.html>.
- Jessen, J., S. Schmitz, and S. Waights (2020). Understanding Day Care Enrolment Gaps. *Journal of Public Economics* 190, 104252.
- Jessen, J., C. Spiess, and S. Waights (2022). Centre-based care and parenting activities. *Oxford Bulletin of Economics and Statistics* 84(6), 1356–1379.

- Kinderkrippen-Online.ch (2023). KiTa-Portal für Kinderbetreuung und KiTa Stellen in der Schweiz. <https://www.kinderkrippen-online.ch/>.
- Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental Analysis of Neighborhood Effects. *Econometrica* 75(1), 83–119.
- Kosse, F., T. Deckers, P. Pinger, H. Schildberg-Hoerisch, and A. Falk (2020). The Formation of Prosociality: Causal Evidence on the Role of Social Environment. *Journal of Political Economy* 128(2), 434–467.
- Lee, D. S. (2009). Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects. *Review of Economic Studies* 76(3), 1071–1102.
- Linz.at (2023). Allgemeine Informationen der Krabbelstuben. https://www.linz.at/serviceguide/viewchapter.php?chapter_id=122078.
- List, J. A., A. M. Shaikh, and Y. Xu (2019). Multiple Hypothesis Testing in Experimental Economics. *Experimental Economics* 22(4), 773–793.
- Malik, R. and K. Hamm and L. Schochet and C. Novoa and S. Workman (2018). America’s Child Care Deserts in 2018. , Center for American Progress, Washington D.C.
- NEPS (2023). NEPS Starting Cohort 1: Newborns (SC1 10.0.0). National Educational Panel Study, <https://doi.org/10.5157/NEPS:SC1:10.0.0>, IIfBi Leibniz Institute for Educational Trajectories, Bamberg, Germany.
- Nordic Co-operation (2023). Childcare in Sweden. <https://www.norden.org/en/info-norden/childcare-sweden>.
- Nordic Health & Welfare Statistics (2022). Early childhood education and care. <https://nhws.org/families-and-children/early-childhood-education-and-care>.
- Nursery Complex (2023). The Nursery Complex of the Capital City of Warsaw. <https://zlobki.waw.pl/>.
- NYC Department of Education (2023). Pre-K. <https://www.schools.nyc.gov/enrollment/enroll-grade-by-grade/pre-k>, New York.
- OECD (2020). Is Childcare Affordable? Policy Brief on Employment, Labour and Social Affairs. <http://oe.cd/childcare-brief-2020>, OECD, Paris.
- OECD (2023). OECD Family Database: Enrolment in Childcare and Pre-School (PF3.2). http://www.oecd.org/els/soc/PF3_2_Enrolment_childcare_preschool.pdf, OECD, Paris.
- Pilke (2022). Päiväkotiin hakeminen alkaa tutustumiskäynnillä. <https://www.pilkepaivakodit.fi/paivakotipaikan-hakeminen-ja-tutustumiskaynti/>.

- Renfrewshire Council (2023). Apply for a free nursery or childcare place. <https://www.renfrewshire.gov.uk/article/9741/Apply-for-a-free-nursery-or-childcare-place>.
- Romano, J. P. and M. Wolf (2005). Stepwise Multiple Testing as Formalized Data Snooping. *Econometrica* 73(4), 1237–1282.
- Romano, J. P. and M. Wolf (2016). Efficient Computation of Adjusted P-Values for Resampling-Based Stepdown Multiple Testing. *Statistics & Probability Letters* 113, 38–40.
- Schmutz, R. (2024). Is universal early childhood education and care an equalizer? a systematic review and meta-analysis of evidence. *Research in Social Stratification and Mobility* 89, 100859.
- Stadt Bern (2023). Kitas Stadt Bern. FAQ. <https://www.bern.ch/themen/kinder-jugendliche-und-familie/kinderbetreuung/kitas-stadt-bern/faq>.
- The Bulletin (2015). Mind the baby: Childcare in Belgium. <https://www.thebulletin.be/mind-baby-childcare-belgium>.
- The Journal (2022). “It’s put me off having more kids”: Your stories of childcare in Ireland during the pandemic. <https://www.thejournal.ie/readers-childcare-stories-good-information-project-5665363-Jan2022/>.
- The Village (2023). Finding daycare for your baby in Brussels. <https://www.thevillage.be/parenting/daycare/>.
- Westfall, P. H. and S. S. Young (1993). *Resampling-Based Multiple Testing: Examples and Methods for P-Value Adjustment*. Wiley Series in Probability and Mathematical Statistics. New York: Wiley.
- World Population Review (2023). Child Care Costs by State 2023. <https://worldpopulationreview.com/state-rankings/child-care-costs-by-state>.