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ABSTRACT

Behavioral Barriers and the Socioeconomic Gap in Child Care Enrollment*

Children with lower socioeconomic status (SES) tend to benefit more from early child care, but are substantially less likely to be enrolled. We study whether reducing behavioral barriers in the application process increases enrollment in child care for lower-SES children. In our RCT in Germany with highly subsidized child care (n > 600), treated families receive application information and personal assistance for applications. For lower-SES families, the treatment increases child care application rates by 21 pp and enrollment rates by 16 pp. Higher-SES families are not affected by the treatment. Thus, alleviating behavioral barriers closes half of the SES gap in early child care enrollment.

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

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

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1. Introduction

There is mounting evidence that early childhood programs promote child development and other important life outcomes (e.g., Currie and Almond, 2011; Fryer et al., 2020). The 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). However, in many countries, child care enrollment rates tend to be lower for disadvantaged children (OECD, 2019, 2020; Cascio, 2021). For instance, in Germany, the socioeconomic gap in early child care enrollment is 14 pp, although child care programs in Germany are universally offered, of high quality, and heavily subsidized (Jessen et al., 2020).[1] 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, 2021) ineffective. Yet, the causal determinants of the socioeconomic gap in child care enrollment are largely unexplored.

A growing literature argues that behavioral 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 (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 behavioral 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 child care availability.[2] At the same time,

[1] This gap refers to the enrollment difference between parents with and without a college entrance qualification ("Abitur"; i.e., the school degree qualifying for university studies) for children below the age of three years. 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.

[2] Similarly, 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

application processes are decentralized, complex, and nontransparent, and competition for a child care slot is high as average demand exceeds supply (see Section 2 for details). Our treatment was designed to alleviate potential behavioral barriers to acquiring a slot in early child care (i.e., center-based child care for children below the age of three years). 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.[3] Importantly, the treatment was based on the premise of not changing parents' preferences related to child care (which is also confirmed in our data). Our randomized treatment allows us to study the causal effect of alleviating behavioral 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. In our main specification, we classify parents without a college entrance qualification as lower-SES (more than 40% of the sample are categorized as lower-SES).

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 behavioral 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 not only application rates, but also 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). Our mediation analysis indicates that almost half of the overall treatment effect on enrollment can be attributed to these two mediators. We also find that the treatment sig-

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.

[3]Our treatment is similar to the approach of Bergman et al. (2019), who investigate the effect of randomly providing customized assistance on moving to high-upward-mobility neighborhoods among 430 low-income families.

nificantly increases application knowledge. However, when considered jointly with child care application and on-site visits, application knowledge accounts for very little of the treatment effect. This finding suggests that improving application knowledge alone might not be sufficient to increase enrollment chances of lower-SES families.

We confirm our main findings in a series of robustness checks. The treatment effects remain significant when correcting for multiple hypothesis testing and when using randomization inference. Our results are also 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 the literature in three dimensions. 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 (for extensive reviews of the behavioral economics of education, see Lavecchia et al., 2016; Damgaard and Nielsen, 2018). One set of 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; Dinkelman and Martinez, 2014; Hastings et al., 2015; 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; Kerr et al., 2020; Bergman and Chan, 2021; Lergetporer et al., 2021; Peter et al., 2021). A second 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. These interventions usually induce small changes to the choice environment, for instance, by simplifying application processes, providing application assistance, or reducing uncertainty related to admission or aid, and often yield large positive effects (Bettinger et al., 2012; Hoxby and Turner, 2013; Castleman and Page, 2015; Pallais, 2015; Castleman and Long, 2016; Marx and Turner, 2019; Oreopoulos and Ford, 2019; Dynarski et al., 2021). While this literature almost exclusively focuses on school or college choices, our paper is the first to show that behavioral barriers play a crucial role in shaping educational decisions about early child care use.[4]

Second, we contribute to the literature that targets parents to improve children's skill development and educational success. Parental inputs are particularly decisive in the first years of a child's life, and early socioeconomic differences in these inputs exacerbate future inequalities (Suskind, 2015). Consequently, several interventions aim at improving the productivity of parental investments in very young children, for instance, through text-messaging interventions that break down complex parenting tasks into small steps, or by providing financial incentives and training to engage parents in behavior that supports skill development (Fryer et al., 2015; Cortes et al., 2019; Doss et al., 2019; Mayer et al., 2019; York et al., 2019). Related studies, focusing on school-aged children, provide parents with information about their child's academic progress to correct biased beliefs and reduce monitoring costs (Bergman et al., 2018; Bergman, 2020; Bergman and Chan, 2021; Bettinger et al., 2021). This literature generally finds that modifying parental investment behavior yields large returns in terms of children's cognitive and non-cognitive skills. Instead of targeting direct parental inputs into children's human capital production, our intervention focuses on the alleviation of behavioral barriers that parents may face when considering applying for a child care slot.[5]

[4]The fact that our intervention includes information provision about the child care application process raises the interpretive question of whether it addresses mainly *behavioral* aspects as opposed to standard information asymmetries. Note that the information we provide is publicly available and is generally shared with parents. In the two sample cities, for example, officials distribute leaflets to all parents when a child is born, informing them of their legal entitlement to a child care slot for their child. In principle, it is possible that the potential failure of lower-SES parents to process this information reflects behavioral biases, such as cognitive overload or recall bias (e.g., Mullainathan and Shafir, 2013). 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). Ultimately, we remain agnostic on this interpretative issue, and, for simplicity, refer to the discussed barriers that parents may face when considering to apply for child care as *behavioral barriers* (see Section 2.3 for details).

[5]In 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., 2021).

Finally, our study adds to the large literature on the effects of early child care on child outcomes.[6] 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).[7] However, relatively 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 determinants of these gaps are not yet well understood. We contribute to this literature by showing experimentally that reducing behavioral barriers can mitigate the SES gap in child care enrollment.

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) behavioral 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 behavioral barriers in the child care application process.

2.1. Early Child Care in Germany

In Germany, early child care provision for under three year-olds (*Krippe*) is universal, that is, targeted at all children. Each child has a legal entitlement to a child care slot starting from the age of one year. Similar to other countries with universal child

[6]The effectiveness of early educational investments is often rationalized by the notion that skill formation involves a process of "dynamic complementarities" by which skills attained early in life make later human capital investment more productive (e.g., Cunha et al., 2006; Cunha and Heckman, 2007).

[7]A 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).

care provision, such as Denmark (Heckman and Landersø, 2021), actual usage is far from universal. About one-third of children under the age of three years attend 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. Thus, the most likely counterfactual to early child care attendance is later attendance, not completely abstaining from child care.[8]

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 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

[8] Recent evidence shows that even small advances in the timing of the start of early child care, for instance, from the age of 19 months to 15 months in Drange and Havnes (2019), have pronounced positive effects on child development.

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).[9] 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).[10] 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 4.4 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 (∼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 behavioral barriers in the application process causally affects this socioeconomic gap in child care enrollment.

2.2. Study Setting

We implemented our experiment in two large cities (population > 100,000) in the federal state of Rhineland-Palatinate.[11] 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

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

[10]In the past few years, some municipalities (including the cities that we study; see Section 2.2) have introduced centralized online application systems, but admission decisions usually remain decentralized and uncoordinated (Fugger et al., 2017).

[11]The 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 4.4.

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, 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.

2.3. Behavioral Barriers to Child Care Enrollment

Behavioral 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., Mullainathan and Shafir, 2013), 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 A1).

For these reasons, we hypothesize that reducing behavioral barriers by providing parents with information and application assistance can mitigate the existing socioeconomic gap in child care enrollment.[12] We consider behavioral barriers in the application process

[12] Consistent 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.

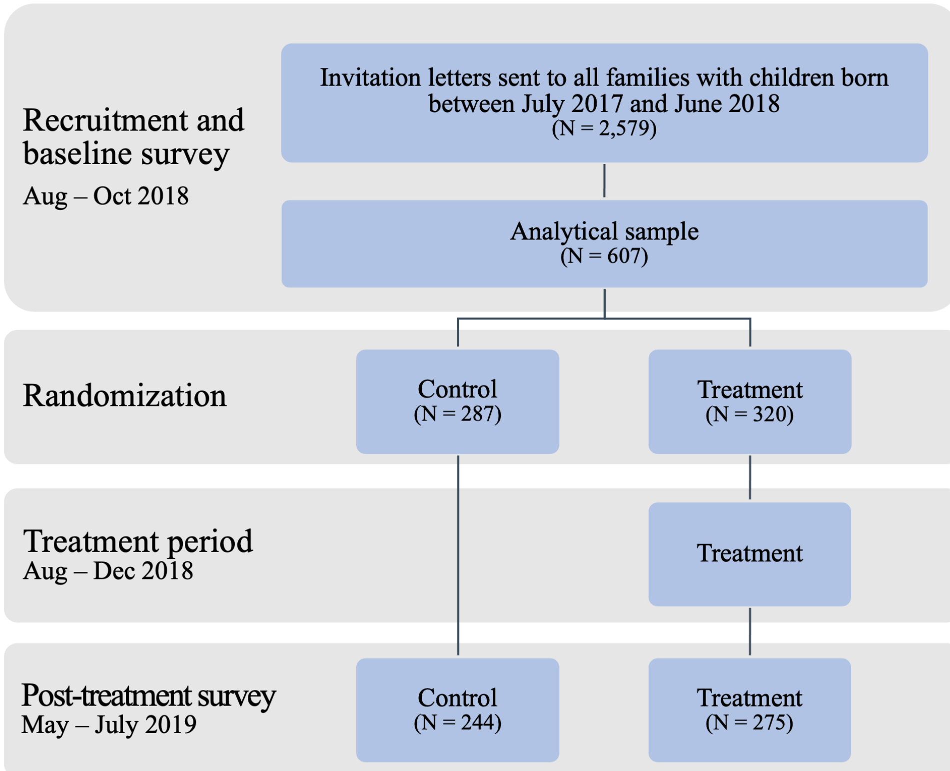


Figure 1: Study Timeline and Procedural Details

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.[13]

Sample recruitment and the baseline survey were conducted between August and October 2018 by 10 specially trained interviewers. We randomly assigned each 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.[14] 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 selection into our sample).[15] The randomized treatment was administered immediately after the baseline survey (see Section 3.3).

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

[13]The 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 F1).

[14]Specifically, 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.

[15]An 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.

behavior and enrollment for the child care year beginning in August/September 2019, which was the target of our intervention.[16] 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, 85.5% (519 out of 607) were reinterviewed in the post-treatment survey. This is a comparatively high participation rate in general, but even more so when considering the relatively high share of lower-SES families in our sample (see below). Moreover, our robustness analysis in Section 4.4 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), their average age is 31.5 years, 40% of them have a migration background (i.e., were not born in Germany), and 9% are currently working. 58% of parents have a college entrance qualification, and average net equivalent household income per month is 1,380 EUR.[18] 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.[19] Application knowledge at baseline is relatively low: on average, parents provide a correct answer to only 3.4 out of 6 (57%) knowledge questions about the child care application process (see Table C3 for the wording of the questions).

[16]In 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".

[17]We 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.

[18]The 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.

[19]We 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.

Table 1: Sample Descriptives and Balancing Tests (Baseline)

	All (1)	Control (2)	Treatment (3)	Δ(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 female	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
Parent currently working	0.087	0.094	0.081	-0.013	0.578	0.713
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	
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. 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. 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.

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 B1 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.[20] 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.[21]

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 57.7% of responding parents ($n = 350$) who have a college entrance qualification ("Abitur") are classified as higher-SES, whereas the 42.3% of parents without a college entrance qualification ($n = 257$) are classified as lower-SES (see Table 1).[22] The proportion of lower-SES families in our sample reflects 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 4.4).

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

[20] Note 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.

[21] We 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 Appendix B and Table B2 for details).

[22] Information 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. 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).

to our application knowledge questions (see Appendix Table A1), 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 A2). 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 behavioral barriers in the child care application process (discussed in Section 2.3). It includes two components: information provision and customized application assistance.[23]

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 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.[24] Appendix F.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.

[23]Based on an ex-ante power analysis, we decided to combine both components into one treatment.
[24]In Section 4.4, we show that indeed the treatment does not affect parents' child care preferences.

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 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, assistants 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.

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.[25] Our treatment involved a relatively modest effort for the assistants. The median number of contacts between assistants and parents (i.e., in-person meetings, phone calls, or emails) was four, and the median time an assistant invested per family was 1.5 hours (mean: 2 hours).

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).[26] Within these strata, we randomized families between both experimental conditions with 50% probability. In

[25] In addition, assistance takers tend to have higher beliefs about returns to child care and lower income than non-takers. Moreover, they tend to be more often female and with a migration background.

[26] 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.

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 group (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 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 E5).

3.5. Empirical Strategy

We estimate the intention-to-treat effects of our intervention by ordinary least squares (OLS) 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.[27] The variable $Treatment_i$ is the treatment indicator. $HigherSES_i$ is

[27] We took great care to ensure that parents' self-reported outcomes are as reliable as possible. For example, we also ask the parents to provide the name(s) of the child care center(s) they have applied to or have enrolled their child in. Therefore, parents could not easily misreport our main outcomes of interest, and almost all parents do provide the name(s) of specific child care center(s). Moreover, there is no differential (mis)reporting by treatment status, as the share of parents who name specific child care

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 $\beta_1 + \beta_2$.[28]

As outlined in 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 outcome (if available)[29], 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.[30] 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 4.4).

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 4.4).

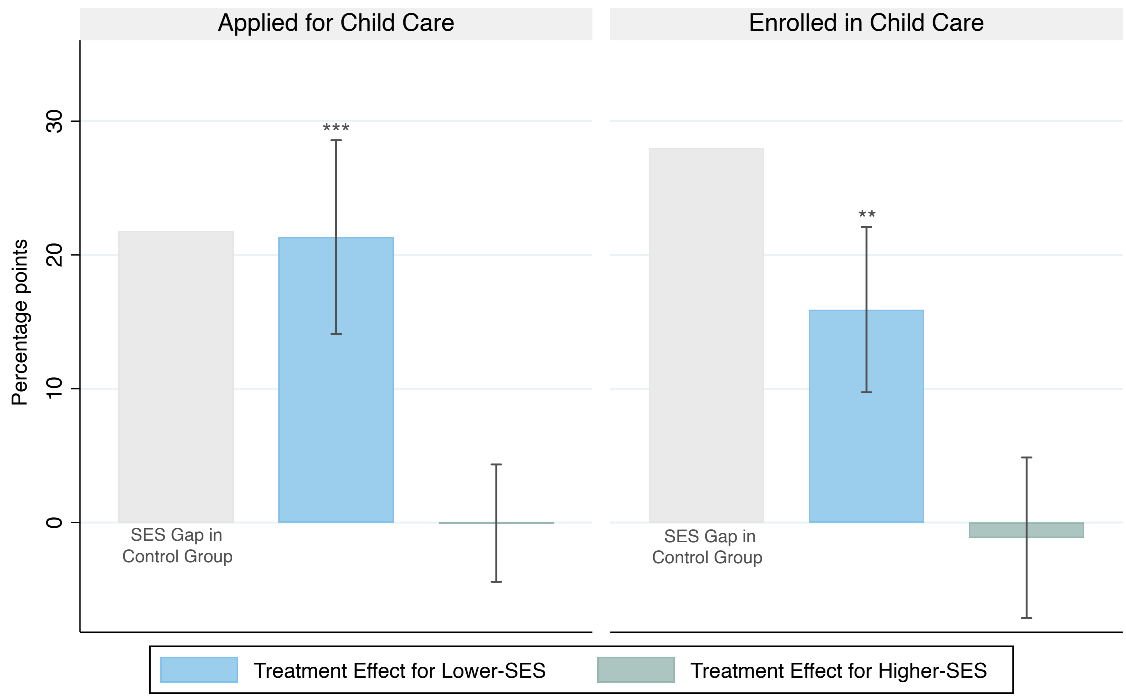
center(s) is very similar in treatment and control group. Finally, our results are robust when we consider parents as having applied or having their child enrolled only if they name a specific child care center (see Appendix Table E3).

[28]Note that we refrain from running instrumental variable models to estimate treatment effects on the treated (TOT) for families who take up the assistance because the exclusion restriction is unlikely to hold in our setting (as we cannot rule out that the information we provide in the video directly affects outcomes).

[29]Since 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.

[30]Survey date fixed effects refer to the post-treatment survey. We include them because the data collection phase was relatively long (about two months), and timing is important in the child care application process (e.g., because some child care centers may allocate slots based on the application date or have certain deadlines after which they allocate available slots). Treatment effects are robust to excluding survey date fixed effects.

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. Significance levels: * $p<.10$, ** $p<.05$, *** $p<.01$

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).

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**	0.213***	0.051	0.159**
	(0.038)	(0.072)	(0.044)	(0.062)
Treatment × Higher-SES		-0.214**		-0.171**
		(0.086)		(0.085)
Higher-SES	0.069	0.179***	0.133***	0.221***
	(0.045)	(0.066)	(0.048)	(0.064)
Strata FE	Yes	Yes	Yes	Yes
Further Controls	Yes	Yes	Yes	Yes
Treatment Effect		-0.000		-0.011
Higher-SES		(0.044)		(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 E1. Robust standard errors in parentheses. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$.

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).[31] We discuss potential reasons why the treatment

[31] We 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 C1). This treatment effect is completely driven by families enrolling their

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.

4.2. Heterogeneity of Treatment Effects

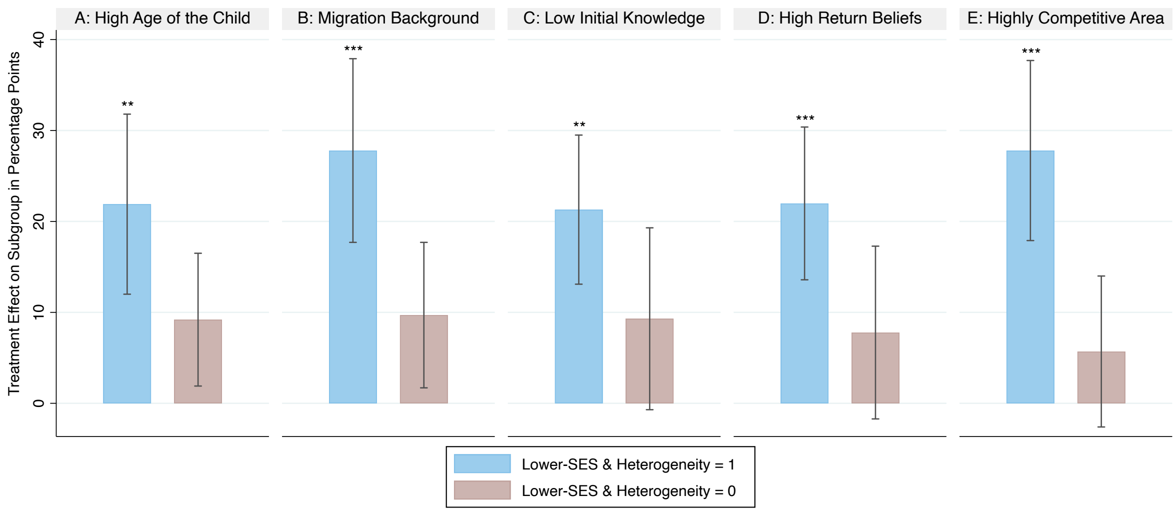
Next, we investigate potential treatment effect heterogeneities for lower-SES families along baseline family characteristics that might moderate the effects of our intervention: child age, migration background, baseline knowledge about child care, beliefs about the degree to which child care promotes child development, and local competition for child care slots. For each of these characteristics, we add a triple interaction between treatment, SES, and an indicator for the respective subsample (using median splits when possible) to Equation (1). We present the heterogeneity results for lower-SES families in Figure 3, whereas the full estimation results are presented in Appendix Table C2. 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 behavioral barriers. While treatment effects for lower-SES families in the other subsamples remain positive, they are rather modest in size, and none of them is statistically significant.

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 2.2).

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

child in child care (extensive margin) instead of enrolled families extending the number of child care hours (intensive margin).

Figure 3: Heterogeneous Treatment Effects on Child Care Enrollment



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 C2 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 E1. Error bars show robust standard errors. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$

background, for example, due to potential language barriers or lower social capital.[32] 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

[32] In 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).

(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., 2020).

In addition to parental characteristics, the effectiveness of our intervention may also depend on local child care market conditions. 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 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.

Our heterogeneity analysis reveals several plausible moderators of the treatment effects on enrollment, 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 large economic differences in treatment effects across the considered subgroups of lower-SES families, the difference is statistically significant at conventional levels only for child care competition ($p = .094$; see Column (5) of Appendix Table C2).

4.3. Mechanisms Analysis

In this section, we examine possible mechanisms through which the treatment affects child care enrollment. First, we analyze treatment effects on pre-specified intermediate

Table 3: Treatment Effects on Application Knowledge and Behavior

	Application Knowl.	Application Behavior		
	Index	Visited	Called	Emailed
	(1)	(2)	(3)	(4)
Treatment	0.299**	0.259***	0.086	-0.079
	(0.151)	(0.078)	(0.077)	(0.058)
Treatment × Higher-SES	0.103	-0.272***	-0.083	0.146**
	(0.190)	(0.095)	(0.094)	(0.072)
Higher-SES	0.064	0.190**	0.170**	-0.051
	(0.135)	(0.074)	(0.070)	(0.057)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	0.402***	-0.013	0.002	0.068
Higher-SES	(0.115)	(0.055)	(0.058)	(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 E1. Robust standard errors in parentheses. Significance levels: * $p<$ .10, ** $p<$ .05, *** $p<$ .01.

outcomes. We primarily consider application knowledge and application behavior as potential mediators, as these dimensions are addressed by our intervention and are likely to be important for securing a child care slot. Second, we follow the approach developed by Heckman et al. (2013) to decompose the overall treatment effect into shares attributed to these different mediators.

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.[33]

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 Germany.[34] 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 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,

[33]Appendix Table C3 shows that results hold when using alternative methods for calculating the knowledge index, that is, an index based on factor 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.

[34]In 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 Appendix E.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).

Appendix Table C4 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).[35]

Parental Preferences. The goal of our treatment was to address the potential behavioral 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). Influencing parental preferences would not only be critical because it would complicate the (normative) interpretation of our results, but would also be ethically questionable. Reassuringly, Appendix Table C5 shows that the treatment does not affect parents' plans for when to enroll their child in child care (Columns (1) and (2)), or their stated willingness to pay for a child care slot (Column (3)). These findings imply that changes in parental preferences do not operate as a channel through which the treatment effect on enrollment materializes.

Mediation Analysis. Based on these results for intermediate outcomes, we conduct a mediation analysis to study the channels through which the treatment affects child care enrollment for lower-SES families. Following the approach by Heckman et al. (2013) and Heckman and Pinto (2015) (applied, for instance, by Oreopoulos et al., 2017; Kosse et al., 2020; Resnjanskij et al., 2021), we decompose the treatment effect into a share explained by k observed mediator variables, and a remaining share explained by unobserved mediator variables. Assuming that the outcome is a linear function of our observed k mediators (M_i^k) and a vector of sociodemographic controls (X_i'), we extend our estimation Equation (1) to:

$$\begin{aligned} Y_i = \alpha + \beta_1^{residual} Treatment_i + \beta_2^{residual} Treatment_i \times HigherSES_i \\ + \beta_3 HigherSES_i + \sum_k \theta^k M_i^k + \sum_k \lambda^k M_i^k \times HigherSES_i + \mathbf{X_i'}\delta + \mu_i \end{aligned} \quad (2)$$

Because we find strong heterogeneity in treatment effects by SES, we also allow effects of mediators to differ by SES (θ^k for lower-SES families and $\theta^k + \lambda^k$ for higher-SES fami-

[35]Note, 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.

lies).[36] Furthermore, we make the assumption required for such mediation analyses that any unobserved mediator (included in the error term μ_i) that is affected by the treatment is orthogonal to the observed mediators.[37] Then, $\beta_1^{residual}$ indicates the treatment effect on lower-SES families net of the observed mediators, and the share of the treatment effect explained by all observed mediators is $1 - \beta_1^{residual}/\beta_1$ (with β_1 from Equation (1)).

The following intermediate outcomes are significantly affected by the treatment and are thus considered as mediators of the treatment effect on child care enrollment for lower-SES families: (i) whether a family has applied for a child care slot, (ii) the application knowledge index, and (iii) whether a family has visited child care centers on-site during the application process. Appendix Table D1 shows the results from estimating Equation (2) with the three mediators. In addition, to assess the relative contribution of mediator M_i^k, we use the estimate of the treatment effect on the respective mediator (see Column (2) of Table 2 and Columns (1) and (2) of Table 3):

$$M_i^k = \gamma_0^k + \gamma_1^k Treatment_i + \gamma_2^k Treatment_i \times HigherSES_i + \gamma_3^k HigherSES_i + \mathbf{X_i'}\gamma_\mathbf{4}^\mathbf{k} + \eta_i^k \quad (3)$$

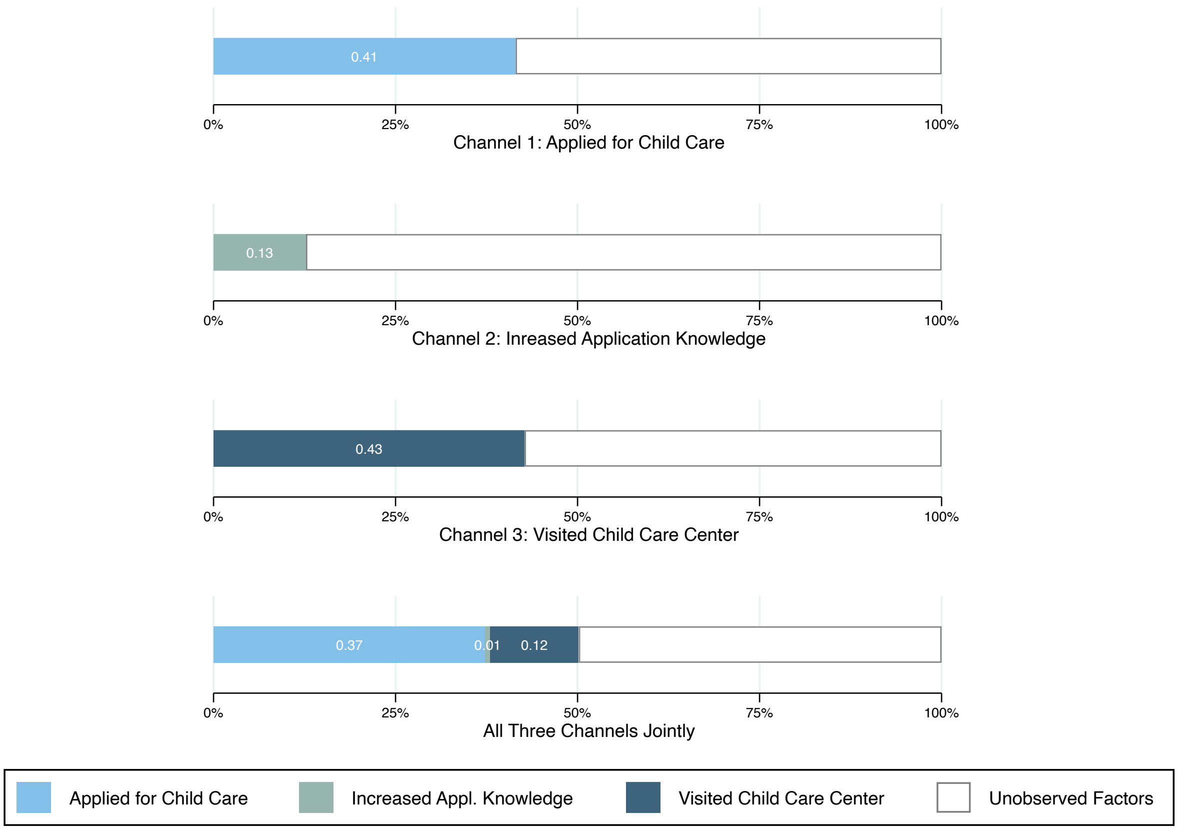
The share of the treatment effect for lower-SES families attributed to mediator M^k can be expressed as $m_k = \theta^k \gamma_1^k / \beta_1$, with θ^k estimated from Equation (2), γ_1^k from Equation (3), and β_1 from Equation (1).

Figure 4 depicts the results of our mediation analysis for lower-SES families. Considering each mediator separately in the upper three bars, we find that changes in the probability to apply for child care and in having visited child care centers on-site both account for more than 40% of the overall treatment effect on child care enrollment. By contrast, increased application knowledge only explains 13% of the estimated treatment effect. Accounting for all three mediators jointly in the bottom bar of Figure 4, we can

[36]Decomposing the outcome variable as in Equation (2) assumes that the θ^ks do not differ between treatment and control group. For example, this means that, conditional on our other mediators and control variables, applications are equally effective for child care enrollment in both treatment and control group. We can test this assumption by interacting the mediators with the treatment dummy and adding them to the model in Equation (2) (see, for instance, Oreopoulos et al., 2017). Likewise, the decomposition assumes that the effect of the control variables in $\mathbf{X_i'}$ on child care enrollment does not differ by treatment status. We again test this assumption by interacting control variables with the treatment dummy and adding them to the model estimated in Equation (2). Reassuringly, in both models none of the interaction terms is statistically significant at conventional levels (all $p > .10$). Thus, both assumptions cannot be rejected in our data.

[37]Note that a violation of this arguably strong assumption, such that unobserved mediators are positively related to both observed mediators and outcomes, would upward bias the estimated share of the treatment effect explained by our mediators in Figure 4.

Figure 4: Share of Lower-SES Treatment Effect on Enrollment Attributed to Mediators



Notes: Figure shows the share of the treatment effect on child care enrollment for lower-SES parents that can be attributed to the respective mediator. The upper three bars show the contribution of a single mediator, while the bottom bar shows the contribution of all three mediators when they are jointly included. Detailed results are reported in Appendix Table D1.

explain more than half of the treatment effect on enrollment. The largest share of 37% is attributed to having applied for child care, and another 12% to on-site visits to child care centers. However, increased application knowledge explains only a negligible share of the treatment effect (<1%), once we include the other two mediators. The idea that application knowledge is unlikely to be a main driver of the treatment effect on enrollment is in line with our result that the treatment — despite substantially increasing higher-SES parents' application knowledge — does not affect child care enrollment of higher-SES families.

In sum, our mechanism analysis provides the intuitive result that the main channel through which the treatment affects child care enrollment of lower-SES families is the increased likelihood that these families apply for a slot. Moreover, the treatment effect on enrollment also materializes through increased on-site visits to child care centers during the application process, improving the chances of obtaining a slot conditional on having applied. By contrast, increased application knowledge explains only little of the overall treatment effect on enrollment, which suggests that customized application assistance is a particularly important component of our intervention.[38] Note, however, that the results of our mediation analysis do not necessarily rule out that knowledge about the child care application process matters in our setting. First, it may be that the treatment affects relevant knowledge components that are not captured by our six application knowledge questions, such as specific information provided by the assistant. Second, it could be that changing application knowledge affects enrollment rates by altering application behavior, thus attributing the knowledge effect to the other mediators.

4.4. Robustness

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 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 E1. 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

[38] Consistent with this result, Bettinger et al. (2012) find that providing information about the Free Application for Federal Student Aid (FAFSA) without complementary help to complete the application is ineffective in increasing college enrollment rates and other outcomes.

[39] The 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 ($p = .051$).

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 E2 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 Appendix E.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 group among those parents who participated in the post-treatment survey. We also show the robustness of our results to re-weighting the observed 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 2.2), 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 Appendix E.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 E10 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 un-

[40]These 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.

filled, 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 Appendix E.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.

5. Conclusion

We conducted a randomized controlled trial to study whether reducing behavioral barriers in the application process increases the use of early child care, particularly for disadvantaged families. Our intervention was designed to address behavioral 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 behavioral barriers substantially reduces the large socioeconomic gap in child care enrollment in the control group.

Of course, behavioral barriers in the application process are only one of several explanations for the large observed SES gap in child care usage. We observe that the estimated treatment effect on lower-SES families' application rates does not fully translate into higher enrollment rates, suggesting that additional factors not addressed by our intervention prevent lower-SES families from child care usage, even *conditional* on having applied for a slot. 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. We consider studying such additional factors an important component for future research.

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). 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 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. Therefore, alleviating behavioral barriers by simplifying application processes (e.g., reducing paperwork, centralizing the admission system, and providing more accessible information) can be a simple but effective strategy to strengthen the desired equity-enhancing impacts of universal social programs.

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Appendix A. Socioeconomic Gaps at Baseline

Table A1: SES Gaps in Application Knowledge (Baseline)

	Indices		Correct Answer to Question on...					
	Mean	# Correct	Legal claim	Age legal claim	Financial support	Age cost free	Apply to 1 center	Nearest center
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lower-SES	-0.436***	-0.602***	-0.114***	-0.054**	-0.198***	-0.025	-0.177***	-0.034
	(0.086)	(0.113)	(0.036)	(0.027)	(0.040)	(0.040)	(0.040)	(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. Significance levels: * $p<$ .10, ** $p<$ .05, *** $p<$ .01.

Table A2: SES Gaps in Parent–Child Activities (Baseline)

	Picture books	Read stories	Sing songs	Watch TV/videos	Paint/play at home	Walk outside	Go to playground	Visit other families
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lower-SES	-0.075*	-0.069*	-0.122***	0.209***	0.046	0.019	0.103**	0.078*
	(0.041)	(0.041)	(0.026)	(0.039)	(0.041)	(0.012)	(0.040)	(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," "less than once a week," and "never"). Robust standard errors in parentheses. Significance levels: * $p<$ .10, ** $p<$ .05, *** $p<$ .01

Appendix B. Sample Selection

Table B1: Selection into Analytical Sample (Baseline)

	Participants (1)	Non-participants (2)	Δ(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 B1. 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 B2. 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 B2: 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***	0.209***	0.159**	0.166***
	(0.072)	(0.072)	(0.062)	(0.060)
Treatment × Higher-SES	-0.214**	-0.199**	-0.171**	-0.202**
	(0.086)	(0.087)	(0.085)	(0.085)
Higher-SES	0.179***	0.166**	0.221***	0.228***
	(0.066)	(0.066)	(0.064)	(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 B1. *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. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$.

Appendix C. Further Results

Table C1: Treatment Effect on Hours in Child Care

	Hours in Child Care
	(1)
Treatment	4.374**
	(2.055)
Treatment × Higher-SES	-4.761*
	(2.798)
Higher-SES	6.438***
	(2.138)
Strata FE	Yes
Further controls	Yes
Treatment Effect	-0.387
Higher-SES	(1.993)
Control Mean Higher-SES	13.756
Control Mean Lower-SES	6.116
Control Mean SES Gap	7.640
N	497

Notes: Table shows intention-to-treat effects on hours in child care, estimated by OLS. The outcome is measured in the post-treatment survey nine months after the treatment. 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. The number of observations is smaller than in our main specification because the information on hours in child care 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. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$.

Table C2: 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**	0.278***	0.213***	0.220***	0.278***
	(0.099)	(0.101)	(0.082)	(0.084)	(0.099)
Treatment × Higher-SES	-0.120	-0.398***	-0.162	-0.271**	-0.319**
	(0.133)	(0.126)	(0.121)	(0.118)	(0.124)
Higher-SES	0.153	0.332***	0.202**	0.359***	0.279***
	(0.097)	(0.092)	(0.092)	(0.086)	(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	0.097	0.093	0.078	0.057
	(0.073)	(0.080)	(0.100)	(0.095)	(0.083)
Treatment (Higher-SES, heterogeneity = 1)	0.100	-0.121	0.051	-0.052	-0.041
	(0.090)	(0.085)	(0.092)	(0.085)	(0.080)
Treatment (Higher-SES, heterogeneity = 0)	-0.115	0.060	-0.053	0.031	0.026
	(0.080)	(0.077)	(0.076)	(0.079)	(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. Significance levels: * $p<$ .10, ** $p<$ .05, *** $p<$ .01.

Table C3: Detailed Treatment Effects on Application Knowledge

	Indices			**Correct Answer to Question on...**					
	Mean	Factor	# Correct	Legal Claim	Age Legal Claim	Financial Support	Age Cost Free	Apply to 1 Center	Nearest Center
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.299**	0.036**	0.378**	-0.099	0.157***	0.010	0.118*	0.035	0.129*
	(0.151)	(0.015)	(0.190)	(0.061)	(0.056)	(0.079)	(0.061)	(0.045)	(0.077)
Treatment × Higher-SES	0.103	0.004	0.117	0.170**	-0.027	0.013	0.053	-0.001	-0.049
	(0.190)	(0.018)	(0.234)	(0.074)	(0.077)	(0.098)	(0.077)	(0.056)	(0.095)
Higher-SES	0.064	0.006	0.089	-0.049	0.078	0.035	0.001	-0.023	0.036
	(0.135)	(0.013)	(0.169)	(0.058)	(0.052)	(0.074)	(0.059)	(0.039)	(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***	0.040***	0.495***	0.071	0.131**	0.023	0.171***	0.034	0.079
Higher-SES	(0.115)	(0.011)	(0.140)	(0.043)	(0.053)	(0.060)	(0.048)	(0.027)	(0.054)
Control Mean Higher-SES	0.143	-0.003	3.837	0.830	0.163	0.673	0.626	0.912	0.633
Control Mean Lower-SES	-0.217	-0.042	3.330	0.856	0.062	0.619	0.402	0.918	0.474
Control Group SES Gap	0.361	0.039	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 factor 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. Significance levels: * $p<$ .10, ** $p<$ .05, *** $p<$ .01.

Table C4: Treatment Effects on Application Behavior — Count Variables

	#Applications (1)	#Visits (2)	#Calls (3)	#Emails (4)
Treatment	1.305**	0.896**	0.501	-0.344
	(0.602)	(0.383)	(0.504)	(0.379)
Treatment × Higher-SES	-0.744	-1.047**	-0.503	0.563
	(0.798)	(0.450)	(0.666)	(0.526)
Higher-SES	1.010*	0.405	1.026**	0.070
	(0.569)	(0.359)	(0.461)	(0.412)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	0.562	-0.150	-0.003	0.218
Higher-SES	(0.537)	(0.261)	(0.451)	(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. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$.

Table C5: Treatment Effects on Parents' Child Care Preferences

	Enrollment Age < 3y (1)	Enrollment Age (2)	Willingness to Pay (3)
Treatment	0.053	0.679	0.740
	(0.071)	(1.815)	(21.651)
Treatment × Higher-SES	-0.019	0.261	-17.212
	(0.085)	(2.209)	(26.685)
Higher-SES	0.037	-0.717	36.473**
	(0.065)	(1.624)	(18.503)
Strata FE	Yes	Yes	Yes
Further controls	Yes	Yes	Yes
Treatment Effect	0.034	0.940	-16.472
Higher-SES	(0.044)	(1.255)	(14.347)
Control Mean Higher-SES	0.857	22.728	213.397
Control Mean Lower-SES	0.711	27.351	144.459
Control Group SES Gap	0.146	-4.623	68.937
N	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. In Column (1), 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 three years, zero otherwise. Column (2) uses the planned enrollment age (in months) on a continuous scale as an outcome. In Column (3), 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. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

Appendix D. Mediation Analysis

Table D1: Treatment Effects on Child Care Enrollment with Mediators

	Main (1)	Channel 1: Applied (2)	Channel 2: Knowledge (3)	Channel 3: Visits (4)	All Three Channels (5)
Treatment	0.157**	0.092	0.137**	0.090	0.078
	(0.062)	(0.062)	(0.062)	(0.063)	(0.062)
Treatment × Higher-SES	-0.171**	-0.106	-0.187**	-0.100	-0.107
	(0.086)	(0.083)	(0.086)	(0.084)	(0.084)
Higher-SES	0.223***	-0.000	0.220***	0.102	0.023
	(0.065)	(0.069)	(0.066)	(0.074)	(0.077)
Applied (y/n)		0.264***			0.224*
		(0.056)			(0.114)
Application knowledge			0.034		0.002
			(0.032)		(0.035)
Visited (y/n)				0.224***	0.058
				(0.060)	(0.118)
Mediator × Higher-SES	Yes	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes
N	515	515	515	515	515

Notes: Table shows the models estimated from Equation (2), which are the basis for calculating the share of the treatment effect explained by specific mediators in our mediation analysis (see Section 4.3). We consider the following mediators: *Applied (y/n)* takes a value of one if respondents state that they have applied for child care, zero otherwise; *Application knowledge* 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); *Visited (y/n)* is a dummy equal to one if parents visited a child care center on-site during the application process, zero otherwise. Column (1) replicates the main treatment effect on enrollment in child care from Column (4) of Table 2, using the sample for which we have information on all mediators ($N = 515$). In Columns (2)–(4), we add each mediator separately (corresponding to the upper three bars in Figure 4). In Column (5), we include all three mediators jointly (corresponding to the bottom bar of Figure 4). In all specifications, we also include interaction terms of the mediator variable with the higher-SES dummy, allowing the mediators to differentially affect enrollment for lower- and higher-SES 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. Robust standard errors in parentheses. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$.

Appendix E. Robustness Checks

Table E1: Randomization Inference and Corrections for Multiple Hypothesis Testing

	Coefficient (1)	Rand. Inference (2)	List-Shaikh-Xu (3)	Westphal-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 C2), Treatment × Lower-SES × 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 (* $p< .10$, ** $p< .05$, *** $p< .01$) 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 E2: Treatment Effects on Child Care Application and Enrollment with Alternative SES Definitions

	Applied				**Enrolled**			
	SES-1 (main)	SES-2	SES-3	SES-4	SES-1 (main)	SES-2	SES-3	SES-4
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.213***	0.209**	0.113*	0.122*	0.159**	0.203***	0.163***	0.154***
	(0.072)	(0.085)	(0.065)	(0.064)	(0.062)	(0.067)	(0.058)	(0.058)
Higher-SES-1 (main = responding parent has college entrance qualification)	0.179***				0.221***			
	(0.066)				(0.064)			
Treatment × Higher-SES-1 (main)	-0.214**				-0.171**			
	(0.086)				(0.085)			
Higher-SES-2 (at least one parent in HH has college entrance qualification)		0.119				0.107		
		(0.097)				(0.092)		
Treatment × Higher-SES-2		-0.182*				-0.212**		
		(0.096)				(0.087)		
Higher-SES-3 (HH has college entrance qualification and not poor)			0.033				0.137	
			(0.083)				(0.087)	
Treatment × Higher-SES-3			-0.062				-0.199**	
			(0.079)				(0.086)	
Higher-SES-4 (HH has college entrance qualification, not poor, and no single parent)				0.046				0.145*
				(0.078)				(0.083)
Treatment × Higher-SES-4				-0.079				-0.186**
				(0.078)				(0.085)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Effect	-0.000	0.027	0.051	0.043	-0.011	-0.009	-0.036	-0.032
Higher-SES	(0.044)	(0.042)	(0.045)	(0.045)	(0.060)	(0.056)	(0.064)	(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 4.4 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. Significance levels: * $p<$ .10, ** $p<$ .05, *** $p<$ .01

Table E3: Treatment Effects on Application and Enrollment If Respondents Named a Child Care Center

	Nine Months After Treatment			
	Applied	Applied (named CC)	Enrolled	Enrolled (named CC)
	(1)	(2)	(3)	(4)
Treatment	0.213***	0.225***	0.159**	0.160***
	(0.072)	(0.072)	(0.062)	(0.059)
Treatment × Higher-SES	-0.214**	-0.254***	-0.171**	-0.168**
	(0.086)	(0.085)	(0.085)	(0.083)
Higher-SES	0.179***	0.210***	0.221***	0.197***
	(0.066)	(0.066)	(0.064)	(0.061)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	-0.000	-0.029	-0.011	-0.007
Higher-SES	(0.044)	(0.045)	(0.060)	(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. Significance levels: * $p<.10$, ** $p<.05$, *** $p<.01$.

Appendix E.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, 85.5% of the families (519 out of 607) decided to do so. In this section, we examine robustness to attrition in several ways. First, Table E4 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 E5 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 group to differ on pre-existing characteristics.

Taken together, the evidence in Tables E4 and E5 indicates that treatment and control group 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 E6. 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 E7, we show Lee bounds when we correct for attrition that differs between treatment and control group. Lee bounds reported in Table E8 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 group (also when we split the sample by SES), only few "excess observations" need to be excluded from the estimation samples (four in Table E7 and eleven in Table E8). 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 E4: Check for Selective Attrition

	Participation Post-Treatment		
	(1)	(2)	(3)
Treatment	-0.004	0.029	0.031
	(0.017)	(0.026)	(0.029)
Treatment × Higher-SES		-0.057	-0.064
		(0.036)	(0.039)
Higher-SES	0.016	0.045	0.044
	(0.021)	(0.028)	(0.030)
Treatment × Enrolled in child care (baseline)			0.114
			(0.187)
Enrolled in child care (baseline)			-0.067
			(0.055)
Treatment × 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
Attrition F-test p-values			
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. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$

Table E5: Sample Descriptives and Balancing Tests of Baseline Variables in Post-Treatment Sample

	All (1)	Control (2)	Treatment (3)	Δ(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 female	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
Parent currently working	0.098	0.107	0.091	-0.016	0.553	0.651
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	
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. 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. 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. 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.

Table E6: Treatment Effects on Child Care Application and Enrollment Using Inverse Probability Weighting

	Applied			**Enrolled**		
	Unweighted	Baseline Sample Weights I	Baseline Sample Weights II	Unweighted	Baseline Sample Weights I	Baseline Sample Weights II
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.213***	0.213***	0.213***	0.159**	0.159**	0.156**
	(0.072)	(0.072)	(0.072)	(0.062)	(0.062)	(0.061)
Treatment × Higher-SES	-0.214**	-0.213**	-0.214**	-0.171**	-0.170**	-0.169**
	(0.086)	(0.086)	(0.086)	(0.085)	(0.085)	(0.085)
Higher-SES	0.179***	0.178***	0.178***	0.221***	0.220***	0.221***
	(0.066)	(0.066)	(0.066)	(0.064)	(0.064)	(0.064)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Effect	-0.000	-0.000	-0.001	-0.011	-0.011	-0.014
Higher-SES	(0.044)	(0.044)	(0.045)	(0.060)	(0.060)	(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. Significance levels: * $p<.10$, ** $p<.05$, *** $p<.01$

Table E7: 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***	0.227***	0.163***	0.165***
	(0.072)	(0.073)	(0.062)	(0.062)
Treatment × Higher-SES	-0.213**	-0.219**	-0.191**	-0.175**
	(0.086)	(0.086)	(0.085)	(0.086)
Higher-SES	0.181***	0.184***	0.227***	0.223***
	(0.066)	(0.066)	(0.064)	(0.064)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	-0.002	0.007	-0.028	-0.010
Higher-SES	(0.044)	(0.043)	(0.060)	(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 group 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. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$

Table E8: Treatment Effect Bounds (Correcting for Potentially Selective Attrition by Treatment Status and SES)

	Applied		**Enrolled**	
	Lower Bound	Upper Bound	Lower Bound	Upper Bound
	(1)	(2)	(3)	(4)
Treatment	0.200***	0.246***	0.121**	0.176***
	(0.074)	(0.072)	(0.061)	(0.064)
Treatment × Higher-SES	-0.193**	-0.266***	-0.118	-0.201**
	(0.088)	(0.086)	(0.085)	(0.087)
Higher-SES	0.173**	0.197***	0.210***	0.227***
	(0.067)	(0.066)	(0.064)	(0.065)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	0.007	-0.020	0.003	-0.025
Higher-SES	(0.045)	(0.042)	(0.060)	(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 group by SES. The difference in participation rates between treatment and control group 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. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$

Appendix E.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 E9).

Table E9: Treatment Effects on Child Care Application and Enrollment Using SOEP Population Weights

	Applied		**Enrolled**	
	Unweighted	SOEP Weights	Unweighted	SOEP Weights
Treatment	0.213***	0.212***	0.159**	0.154**
	(0.072)	(0.071)	(0.062)	(0.066)
Treatment × Higher-SES	-0.214**	-0.214**	-0.171**	-0.174*
	(0.086)	(0.085)	(0.085)	(0.091)
Higher-SES	0.179***	0.176***	0.221***	0.217***
	(0.066)	(0.066)	(0.064)	(0.067)
Strata FE	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes
Treatment Effect	-0.000	-0.002	-0.011	-0.020
Higher-SES	(0.044)	(0.043)	(0.060)	(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. Significance levels: * $p<.10$, ** $p<.05$, *** $p<.01$.

Appendix E.3. Potential Displacement Effects

Table E10: Analysis of Potential Displacement Effects

	Enrolled into Child Care					
	Control Group	Only Lower-SES	Only Higher-SES	Control Group	Only Lower-SES	Only Higher-SES
	(1)	(2)	(3)	(4)	(5)	(6)
Share of households in treatment	0.004	-0.002	0.008			
	(0.015)	(0.028)	(0.019)			
High share of households in treatment				-0.038	-0.035	-0.039
				(0.061)	(0.101)	(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. Significance levels: * $p< .10$, ** $p< .05$, *** $p< .01$

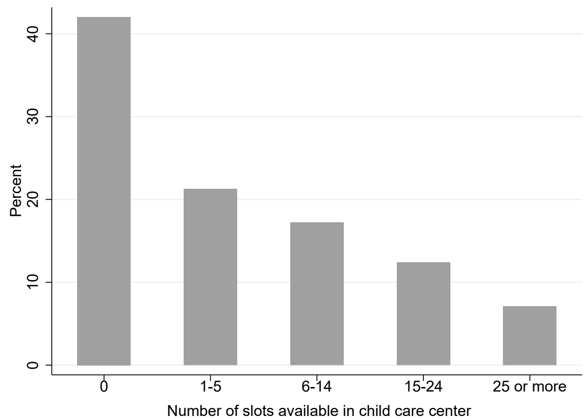
Appendix E.4. Survey of Child Care Center Managers in Germany

In Section 4.4, 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 E1. 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. 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

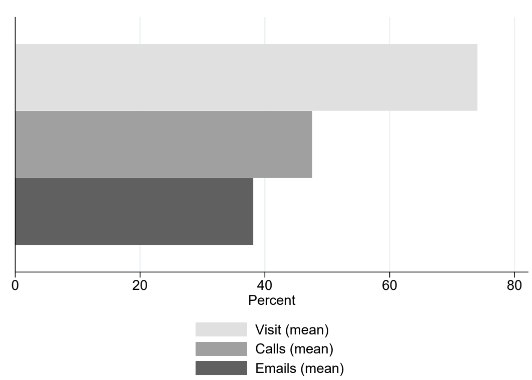
Figure E1: Number of Unfilled Early Child Care Slots



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 E2 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 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.

Figure E2: Importance of Different Application Strategies for Obtaining Child Care Slot



Appendix F. Experimental Material

Appendix F.1. Corporate Design

Figure F1: Study Logo and Examples for Design of Materials for Parents

Appendix F.2. Information Video Slides and Text

Figure F2: 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 F3: 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.

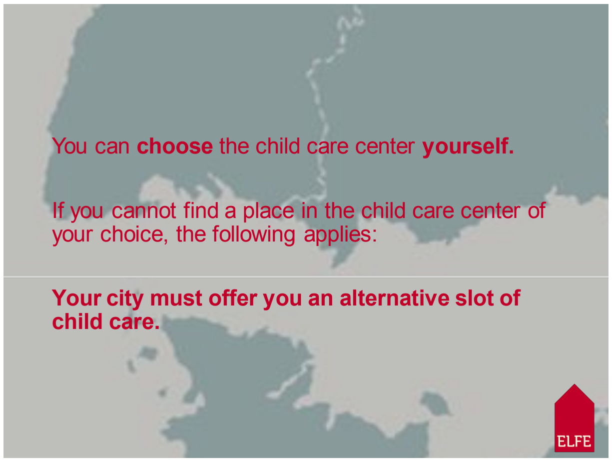
Figure F4: Slide 3 of the Information Video Shown to the Treatment Group

Audio Slide 3: Access to a child care slot.

Figure F5: 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 F6: Slide 5 of the Information Video Shown to the Treatment Group

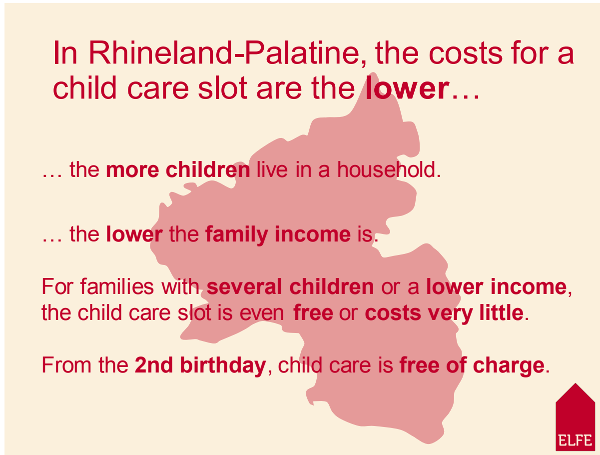


Audio Slide 5: You can choose the child care slot yourself. If you cannot find a slot in a child care center of your choice, the following applies: your city must offer you an alternative slot in child care, for example, at another child care center.

Figure F7: 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 F8: Slide 7 of the Information Video Shown to the Treatment Group

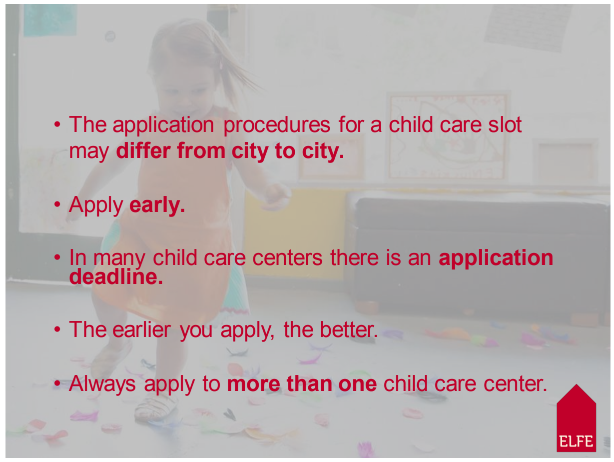


Audio Slide 7: In Rhineland-Palatinate, the costs for a child care slot are the lower the more children live in a household and the lower the family income is. For families with several children or a lower household income, child care is often even free or costs very little. From a child's second birthday,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

Figure F9: Slide 8 of the Information Video Shown to the Treatment Group

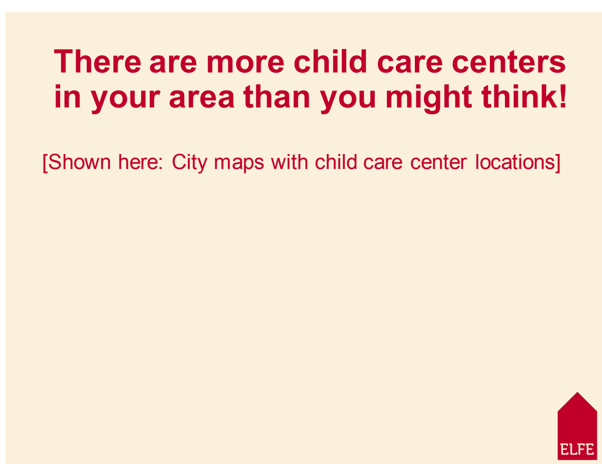
Audio Slide 8: Application for a child care slot.

Figure F10: Slide 9 of the Information Video Shown to the Treatment Group



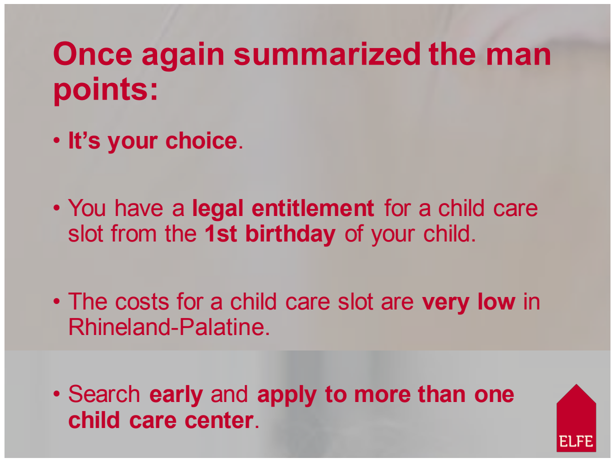
Audio Slide 9: If you decide to apply for child care, finding a slot is really not rocket science. The application procedures for a child care slot may differ from city to city. However, the following always applies: apply early. This increases your chances of finding a slot. In many child care centers, 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 F11: Slide 10 of the Information Video Shown to the Treatment Group



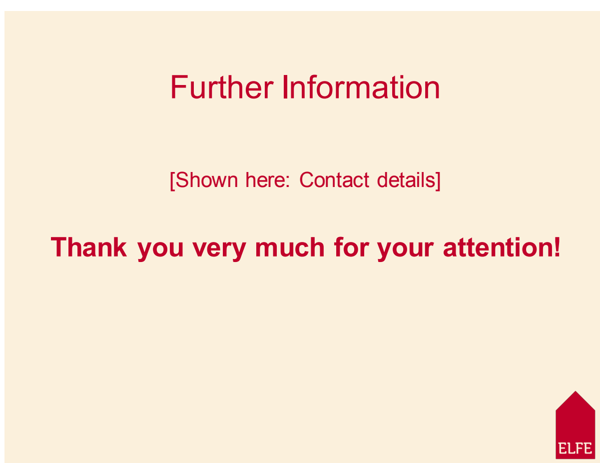
Audio Slide 10: Here you can see that there are more child care centers in your area than you might think!

Figure F12: 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 F13: 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.