Journal of Public Economics 159 (2018) 33–53

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Does early child care affect children's development?☆

Christina Felfe[a,b], Rafael Lalive[b,c,d,e,f,*]

[a] University of St. Gallen, Varnbüelstrasse 14, St. Gallen CH-9000, Switzerland
[b] CESifo, Germany
[c] Department of Economics, University of Lausanne, Lausanne-Dorigny CH-1015, Switzerland
[d] CEPR, United States
[e] NBER, United States
[f] IZA, Germany

ARTICLE INFO

JEL classification:
J13
I21
I38

Keywords:
Early child care
Child development
Marginal treatment effects
Rationing

ABSTRACT

We study how early child care (ECC) affects children's development in a marginal treatment effect framework that allows for rich forms of observed and unobserved effect heterogeneity. Exploiting a reform in Germany that induced school districts to expand ECC at different points in time, we find strong but diverging effects on children's motor and socio-emotional skills. Children who were most likely to attend ECC benefit in terms of their motor skill development. Children who were least likely to attend ECC gain in terms of their socio-emotional skill development, especially boys and children from disadvantaged families, such as those with low education or migration backgrounds. Simulating expansions of ECC, we find that a moderate expansion fosters motor skills for all children and language skills for boys and immigrant children. A progressive expansion of ECC improves all children's socio-emotional development but neither their motor skills nor their language skills.

1. Introduction

In light of the rapidly increasing demand for early child care (ECC) – care offered to children under the age of 3 – many countries have placed reforms to the ECC system high up on their political agenda. However, whether ECC helps or hinders children's development is hotly debated by politicians and scholars alike. ECC provides stimulating environments in which children meet other children on a regular basis and are cared for by certified pedagogical staff. However, these staff might not be able to devote sufficient attention to each child, particularly not to children with special needs.

Our aim is to assess whether ECC affects children's development and how expanding ECC supply affects children who occupy newly created slots. To answer these questions, we adopt a marginal treatment effect (MTE) framework that provides information on how the ECC effect varies across children in terms of their observable characteristics and in terms of their latent propensity to attend ECC. In our context, a child's latent propensity to attend ECC likely depends on both parents' preferences to send their child to ECC and constraints, e.g., features of the rationing system that allocates slots to children. The MTE framework is well suited to capture the full range of ECC effects and can be used to simulate effects of alternative reforms to the ECC system and thus to generate important and policy-relevant information.

We study ECC in Schleswig-Holstein, the northernmost German state that features excess demand for ECC: in 2005, 36% of all parents sought to place their child in ECC, while only 7% could be accommodated. From 2005 onward, German authorities channeled substantial funding into school districts to expand ECC. The expansion

☆ We would like to thank Olof Aslund, Michael Baker, Mark Dusheiko, Martin Eckhoff Andresen, Maria Fitzpatrick, Eric French, Martin Huber, Steven Lehrer, Edwin Leuven, Michael Kosfeld, Patrick Kline, Claudia Senik, Marianne Simonsen, Mark Stabile, Sergio Urzua, Chris White, Ludger Wössmann, Danny Yagan, the editor and three anonymous referees for their valuable comments on earlier versions of this paper. We presented the paper to seminar audiences at Ifo Institute Munich, UC Berkeley, University of Lausanne, University of Mannheim, University of Toronto, CIREQ Montreal, and Barcelona Summer Forum. We are indebted to the MSGWG (Ministry of Social Affairs, Health, Science and Equality) of Schleswig Holstein/Germany, which granted us access and further information on their annual reports on the school entry examinations. In particular, we would like to thank our cooperation partners, Heidrun Thaiss, Ute Thyen and Sabine Brehm, for their support and thoughtful input. This publication benefited from the support of the Swiss National Centre of Competence in Research LIVES –Overcoming vulnerability: Life course perspectives (NCCR LIVES), which is financed by the Swiss National Science Foundation (grant number: 51NF40-160590). The authors are grateful to the Swiss National ScienceFoundation for its financial assistance. Christina Felfe acknowledges financial support from the Economic Policy Profile Area of the University of St. Gallen. Rafael Lalive thanks the UC Berkeley Center for Labor Economics for its hospitality while rewriting substantial parts of this paper. All remaining errors are our own.
* Corresponding author at: Department of Economics, University of Lausanne, CH-1015 Lausanne-Dorigny, Switzerland.
E-mail addresses: Christina.Felfe@unisg.ch (C. Felfe), Rafael.Lalive@unil.ch (R. Lalive).

https://doi.org/10.1016/j.jpubeco.2018.01.014
Received 25 September 2015; Received in revised form 29 January 2018; Accepted 31 January 2018

process initially occurred unevenly across school districts. Two years after the reform, some districts offered an ECC slot to 25% of all children – henceforth called fast-expansion districts – while other districts offered a slot to only 5% – the slow-expansion districts. Four years after the reform, however, all districts were able to accommodate approximately 25% of all children. Hence, the timing of the expansion, but not the targeted level of ECC slots, differed across fast- and slow-expansion districts.

While not random, the variation in the timing of the expansion is plausibly exogenous. The fact that the targeted level of ECC does not differ between fast- and slow-expansion districts is consistent with this claim. There are no pre-reform differences between fast- and slow-expansion districts in child development or socio-demographic aspects. The exception is the number of children aged 0–2, one of the criteria determining the order according to which funding was distributed across districts. Socio-demographics and care center quality follow similar trends in fast- and slow-expansion districts. We are also unaware of any other policy change during the period under study that may have differentially affected fast- and slow-expansion districts.

Official records from school entry examinations (SEE) – mandatory medical assessments of children just before they enter primary school – provide information on language, motor, and socio-emotional skills. For our analysis, we use SEE data on 6 cohorts, 2 born before the reform and 4 born after the reform. In a first step, we estimate the individual probability of being enrolled in ECC – the propensity score. The propensity score model contains a set of interaction terms that capture the plausibly exogenous change in ECC attendance triggered by the 2005 reform. In a second step, we estimate how child development varies with the propensity score. The resulting estimates ultimately allow us to produce estimates of the MTEs.

The results indicate a weak effect of ECC on language skills. We observe positive effects of ECC on children's motor and socio-emotional skills. The patterns, however, differ across these two skill dimensions. Effects on motor skills are strong for children with a high latent propensity, but significantly lower for children with a low latent propensity to attend ECC. The pattern for children's socio-emotional skills is exactly the opposite: while children with a low latent propensity exhibit significant gains in this skill dimension, children with a high latent propensity benefit substantially, but not statistically significantly, less from attending ECC. The differential patterns observed for motor and socio-emotional skills could be due to differences in the underlying skill production functions: motor skills are the explicit target of the ECC curricula and can be fostered using playful activities that both centers and parents can provide equally; socio-emotional skills, in turn, are shaped primarily by adult and peer relationships (Ladd, 2005), which are readily available in centers but not necessarily to the caregiver at home.

How ECC expansions affect children who occupy the new slots is a central policy question. Based on our MTE results, we simulate two alternative policy scenarios. In the first scenario, we simulate the effects of the ECC expansion that occurred between the first cohort and the last cohort in our data. This expansion increases average ECC attendance from 7% to 27%, a “modest” expansion. In the second scenario, we simulate what would happen if all school districts expanded ECC as much as the strongly expanding districts did in our observation period, those at the 90th decile of the distribution of ECC expansions in our data. This “progressive” reform implies an increase in ECC attendance to 50%, up from the current 27%.

Overall, we find that expanding ECC improves the motor skill development of the average child and that a moderate reform exerts stronger effects than a progressive reform. A progressive reform stimulates moreover the socio-emotional skill development of the average child. Examining sub-groups, we find that boys benefit considerably more from ECC expansions than girls: a moderate reform stimulates boys' language and motor skills, while a progressive reform stimulates their motor and socio-emotional skills. Girls benefit only in terms of their motor skills in the modest expansion, but not in the progressive expansion. Effects by education and migration background suggest that a modest expansion fosters the motor skills of all children, independently of their socio-demographic background. In addition, the modest expansion helps improve immigrant children's language skills and thus points to the leveling effects of ECC. A progressive expansion promotes the socio-emotional development of disadvantaged children, both children from a low education background and children with migrant ancestry. This result highlights the relevance of expanding the ECC supply because expansion could provide access to children whose parents may underestimate the benefits of ECC.

Two main strands of the literature discuss how child care affects children's development. The first strand investigates the effects of universally accessible child care on children's skill acquisition. Most studies focus on preschool-age children (3–6 years old) and generally find neutral or positive effects (Berlinski et al., 2009; Cascio, 2009; Felfe et al., 2015; Fitzpatrick, 2008; Gormley Jr. et al., 2008; Havnes and Mogstad, 2011; Magnuson et al., 2007). Research on the effects of non-parental care on younger children (0–2 years old – the age range on which we focus) is less abundant and inconclusive. Several studies find negative effects on children's development. Focusing on the Canadian province of Québec, Baker et al. (2008) find that reducing the out-of-pocket cost of public child care increases its use but crowds out existing private care arrangements. While stimulating maternal employment, the subsidy leads to more negative parenting styles, resulting in a deterioration of child well-being. Exploiting the summer dip in child care utilization as an instrument for attendance, Herbst (2013) finds negative effects of non-parental care arrangements on children's cognitive test scores. A recent study for Italy by Fort et al. (2017) confirms the negative effects of non-parental care arrangements on intellectual strength, particularly for girls. What these studies have in common is the relatively low quality of center-based care in comparison to the quality of the counterfactual care mode (i.e., home care or private child care arrangements). In contrast, studies focusing on countries with high-quality, center-based care come to a more positive conclusion. In the context of Denmark, Datta-Gupta and Simonsen (2010) find that children benefit more from center-based care than from lower-quality informal day care. Furthermore, in Chile, center-based care targeted at children aged 5–14 months carries substantial benefits, particularly in terms of motor and cognitive skills (Noboa Hidalgo and Urzúa, 2012). Drange and Havnes (2014) exploit child care assignment lotteries in Norway and identify a positive effect of starting child care, on average, four months earlier (at 15 months instead of at 19 months) on children's medium-run cognitive skills.

Another strand of literature dealing with very young children is the literature on the effects of maternal employment on child development. This strand of the literature is relevant for our study, as center-based care represents the main care mode for working mothers. Maternal employment can improve children's intellectual performance by increasing household income (Blau and Grossberg, 1992), but it may also negatively affect it (Baum, 2003; James-Burdumy, 2005), depending on the family's background (see Ruhm, 2004 and Brooks-Gunn et al., 2002 for an overview). Several recent studies exploit changes in time spent with parents due to reforms of the length of parental leave to assess the role of time with parents for children's long-term development. Wuertz-Rasmussen (2010), Liu and Skans (2010), Baker and Milligan (2012), and Dustmann and Schoenberg (2012) find no effects, whereas Carneiro et al. (2014) and Danzer and Lavy (forthcoming) detect some positive long-term effects on education and labor market outcomes.

We complement the literature adopting an MTE framework that allows for differences in the average treatment effect along observed dimensions and along the individual latent propensity to be treated. The MTE approach is useful to understand and predict the effects of child care, but to date, it has rarely been adopted in this context. Kline and Walters (2016) and Cornelissen et al. (2016b) use an MTE framework to discuss the effects of care offered to children aged 3 to 6, specifically, Head Start in the US and Kindergarten in Germany.[1] Eckhoff Andresen (2016) focuses on the same age group as we do and studies the effects of ECC on test scores at age 10 in Norway. We identify ECC's effects not only on cognitive skills but also on non-cognitive skills. This distinction is important, as different inputs may matter for different skill dimensions.

The remainder of this paper is structured as follows. Section 2 describes the institutional setting. Section 3 introduces the data and provides the descriptive analysis. Section 4 presents the conceptual framework of our analysis. Section 5 contains the results, and Section 6 concludes.

2. The child care system in Germany

2.1. Early child care

Germany offers child care at two levels.[2] ECC is available for children aged 0–2, and Kindergarten is available for children aged 3–6. Since 1996, every child has been legally entitled to a place in Kindergarten from age 3 until primary school. As a result, more than 90% of all children aged 3–6 attend Kindergarten.

ECC is offered by care centers, which have a clear educational mission and follow strict guidelines to develop children's analytical, language, and motor skills. Center staff engage children in playful activities, such as circle play, reading, painting, or physical activities. While centers do not have the explicit aim of improving other skills, they are likely to nourish children's cultural, ethical and social skills through interactions with the staff and other children.

Centers are subject to strict quality regulations concerning opening hours, group size, the staff-child ratio, and staff qualifications. Centers must remain open for at least 4 h 5 days per week. Groups can contain up to 10 children and must be supervised by at least one certified pedagogue and one or two assistants. To work as a group leader in a care center, a caregiver must have two years of theoretical training and at least two years of practice in a care center. Care centers tend to comply with these regulations: over the period under study, groups accommodated, on average, 10.1 children,[3] the average child-staff ratio was approximately 3:1, and 61.9% of the employed staff had a degree in ECC education (see Table 1).

ECC is highly subsidized. In 2006, for instance, public subsidies covered 78.9% of the total cost of center-based care. Parents and other private organizations contributed 14.0% and 7.1% of the total operating cost of 14.1 billion Euros, respectively. Parental fees amount to 250 Euros per month on average – a price well below the actual costs (approximately 1070 Euros per month; see Felfe and Stern, 2015). Further price reductions are awarded to large families and families with low incomes.

West Germany is characterized by strong excess demand for ECC.[4] In 2005, for instance, centers could offer a slot to approximately 7% of all children aged 0–2, whereas 36% of all parents had sought to place their child in ECC (Bien et al., 2006). To address this situation, centers use waiting lists. Slots are allocated on a first-come-first-served basis, with the exception of families with special needs; i.e., children who live with a single parent or who have previously enrolled siblings enjoy priority. Parents can place their child on a waiting list as soon as they find out about the upcoming birth, and children are accepted on a rolling basis (children move on to Kindergarten on their 3rd birthday, which frees ECC slots at various times during the calendar year). Centers organize open days for parents to visit and to decide whether placing their child in a center is the best option for them.

Depending on the school district, parents either register via a centralized system organized by the respective youth welfare service or apply directly for a slot at the care center.[5] Parents can apply for a slot outside their district of residence, but strict zoning rules prevent them from obtaining subsidies, and new slots are offered to local children first. This system reduces incentives for parents to sign their child up outside the district of residence.[6] Parents who do not obtain a slot in center-based care via waiting lists can report their needs to the local youth welfare service; however, only approximately 2% of all families do so.[7]

On average, children aged 24–48 months who were not enrolled in ECC were cared for by grandparents for 4.1 h per week, by other members of the extended family for 0.8 h and by informal modes of paid care for 3.1 h per week in 2011. Other care modes played a minor

Table 1
Descriptive statistics of key variables.
Source: School entry examination, Schleswig-Holstein 2009–2014, our calculations.

	All	ECC	No ECC	Diff	z-Ratio
A. Child development outcomes					
Language skills (D)	0.711	0.733	0.707	0.026	(3.697)
Motor skills (D)	0.795	0.809	0.793	0.016	(2.639)
Socio-emotional maturity (D)	0.778	0.796	0.775	0.021	(2.936)
B. Child characteristics					
Child age (months)	73.774	72.924	73.918	−0.995	(−8.961)
Boy (D)	0.519	0.519	0.519	0.000	(−0.031)
Low birth weight (D)	0.066	0.055	0.068	−0.014	(−5.087)
Foreign (D)	0.207	0.186	0.210	−0.024	(−3.104)
C. Family characteristics					
Mother is single (D)	0.125	0.139	0.123	0.016	(3.237)
Mother has tertiary education (D)	0.321	0.392	0.309	0.008	(10.528)
Number of siblings (children)	1.185	1.013	1.214	−0.201	(−17.41)
D. Care center characteristics					
Group size (children)	10.082	9.904	10.112	−0.208	(−3.029)
Staff age (years)	41.299	41.458	41.272	0.187	(5.349)
Staff with degree (%)	61.903	61.894	61.905	−0.011	(−0.053)
Full-time staff (%)	28.078	28.635	27.983	0.652	(3.013)
Full-time children (%)	45.386	48.212	44.905	3.307	(7.088)
Children	61,265	8914	52,351		

Notes: This table reports child, family, and care center background characteristics. The "All" column is the sample average, the "ECC" column refers to children with exposure to early center-based care, and the "No ECC" column refers to children with no exposure to early center-based care. "Diff" calculates the difference between the "ECC" and "No ECC" columns, and the "z-Ratio" column provides the asymptotic z-statistic for a test of no difference between the two columns adjusted for clustering at the school district level.

[1] Further studies concerned with the distributional effects of preschool programs have relied on quantile regressions: Bitler et al. (2013) study the distributional effects of Head Start; Havnes and Mogstad (2015) study universal preschool in Norway. MTEs have been studied mostly in other settings. Doyle (2007) measures the MTEs of foster care on child outcomes. Maestas et al. (2013) and French and Song (2014) estimate MTEs of disability benefit receipt on the labor supply. Brinch et al. (2017) extend the MTE framework to a setting with multiple discrete instruments and analyze the interaction of the quantity and the quality of children.

[2] This section draws on Huesken (2010).

[3] Groups with more than ten children are allowed in cases of mixed-age groups that host children up to age 6.

[4] East Germany offers a comprehensive ECC system, a remnant of the former German Democratic Republic.

[5] Most districts offer one center. In the event that a district features several care centers, parents can register simultaneously with several centers and thus maximize their likelihood of receiving a slot.

[6] One might believe that such a system would encourage migration. In earlier work, we find that few parents of newborn children move and that mobility decisions are uncorrelated with the availability of ECC (Felfe and Lalive, 2012).

[7] The introduction of the legal claim on a slot in ECC from August 2013 onward and the subsequent financial consequences for districts acted like an enforcement mechanism and led to a strong improvement in the efficiency of the ECC slot allocation mechanisms.

role.[8] These numbers suggest that mothers are the main care providers for children who are not in center-based care, and there are several reasons for this result. First, the German parental leave system was extremely generous during the period considered: parents enjoyed a job guarantee of 36 months (where up to 24 months were at least partially paid)[9] that therefore lasted until their child's entry into Kindergarten. Second, women worked in occupations with rather flat wage profiles and therefore faced lower opportunity costs of interrupting their careers after childbirth. Third, traditional views of parental roles remained widespread in West Germany: parents who did not care for their children personally were commonly referred to as "raven parents", a judgment that became even more severe if parents enrolled their children in care by an unlicensed child care worker.

2.2. Expansion of early child care

Beginning in 2005, West Germany embarked on a strong expansion of the ECC supply.[10] In 2005, the federal government committed to creating 230,000 additional ECC slots by 2010 (*Tagesbetreuungsausbaugesetz*). In 2007, a summit of the federal government, the states, and the counties reinforced the aim of the 2005 mandate and established the target of achieving a coverage rate of 35% nationwide by 2013. Finally, in December 2008, the law on support for children (*Kinderförderungsgesetz*) announced that all children aged 1 year or older would have a legal claim to an ECC slot by August 2013. As a consequence, the availability of ECC slots has increased since then: the coverage rate amounted to 2.4% in 2002, 8.0% in 2006, 17.5% in 2010 and to 28.1% in 2015, on average, in West Germany.

Despite this remarkable expansion, coverage remains below demand: in 2005, 36% of all mothers with children aged 0–2 expressed a desire for an ECC slot (Bien et al., 2006). Stated demand remained fairly stable over time and still exceeds supply: in 2012, for instance, 37% of all mothers with children aged 0–2 expressed a desire for an ECC slot, according to BMFSFJ (2012). Thus, the large-scale policy intervention under study has apparently not affected demand for ECC, a possible issue with our empirical approach. The ECC expansion was accompanied by a slight improvement in care center quality, but prices remained constant.

The ECC expansion was financed by the federal and the state governments, with subsidies amounting to approximately 80 million Euros per year in Schleswig-Holstein. Funds were allocated to school districts in two steps. In the first step, the state government allocated subsidies to counties in line with the number of children in the ECC age bracket. In the second step, each county's youth welfare service allocated subsidies to school districts with a low supply of ECC, a large number of children in the ECC age bracket, and a convincing expansion plan.[11] Districts faced two main barriers to submitting a convincing expansion plan: space and staff. Space regulations required centers to offer care to each group in rooms that offered at least 2.5 square meters per child. This regulation restricted the set of appropriate properties and prolonged time until a district could submit an expansion plan. In addition, Germany had a deficit of 45,000 ECC workers, twice the number of yearly graduates (approximately 20,000 per year) and almost ten times the yearly net number entering the sector (approximately 15,000 leave the sector per year).[12] Applications to expand ECC exceeded the available funds, and youth welfare services used lotteries or waiting lists to select districts served within the same calendar year. All remaining districts were served the following calendar year.

There was substantial geographic variation in the timing of the expansions. Some districts needed only to present a convincing expansion plan to obtain the financing to initiate a rapid expansion. Other districts took considerably longer to meet the eligibility requirements. Our empirical analysis exploits variation in the timing of districts' ECC expansion in order to assess ECC's effects on child development. Thus, the key question is whether the timing was exogenous, i.e., unrelated to child development. Two concerns caution against treating the timing as exogenous. First, districts that were more prepared to expand may have also been those districts that lobbied more strongly for expansion, particularly because of a particularly highly educated constituency with large gains from mothers entering the labor market. Second, the expansion of ECC supply may have been accompanied by changes in care center quality or women's labor market outcomes. In this case, the effects on child development may not necessarily be explained by having greater access to ECC but rather by better ECC or higher household income due to increased maternal employment. To assess these important concerns, we discuss below whether fast- and slow-expansion districts differ in terms of (trends in) their socio-economic composition, the quality of their care centers or their female labor force participation.

3. Data and descriptive analysis

This section provides a description of the data source, descriptive statistics of the children included in our empirical analysis and a comparison of the fast- and slow-expansion districts.

3.1. School entry examination data

Our main data source is based on official SEEs. In Germany, in the year prior to school entry (i.e., when turning 6 years old between July of the previous year and June of the year of school entry) all children undergo a mandatory medical screening.[13] The purpose of this medical screening is to assess children's mental and physical development, diagnose anomalies and prescribe treatment if necessary. The SEE informs parents and schools about children's readiness to follow the curriculum taught in primary schools. The SEE is organized by counties' youth welfare services and executed by official pediatricians at the school district level.

Pediatricians assess children's development with regard to language, motor, and socio-emotional skills in the form of a medical diagnosis.[14] The language assessment concerns articulation and expression of thoughts. Children describe a story presented in pictures and repeat several pseudo-words and sentences. The diagnosis regarding motor skills concerns coordination and motor capacity. Children are asked to stand on one leg, jump on one leg, and jump left and right. The socio-emotional assessment is based on the pediatrician's observations and on the so-called strength and difficulties questionnaire, which was designed to identify behavioral problems, emotional instability,

[8] Felfe and Lalive (2012) report these statistics based on data from the German Socio-Economic panel.

[9] In 2007, the federal parental benefit scheme was adjusted, and since then, parents can claim a higher – up to 60% of their net salary – benefit for up to 12 months.

[10] The aim of the government was to increase fertility and stimulate the female labor supply. Indeed, Bauernschuster et al. (2016) show that fertility rates reacted to the policy efforts but mostly at the intensive margin and only from 2007 onward. We discuss female labor force participation in the next section.

[11] This discussion is based on several interviews with deputies of the Youth Welfare Service in *Schleswig-Holstein*. Officials responsible for implementing the reform provided us with rich personal information on how the allocation process was implemented. We particularly thank Stefanie Krueger-Johns for providing us with detailed information.

[12] Attracting qualified staff was particularly difficult for economically strong areas, not least due to the imbalance between the low wages paid in the child care sector – employees working in the child care sector belong to the lowest pay scale groups – and high subsistence costs in these areas.

[13] Children turning 6 years old between July and December of the same year of school entry are allowed to be examined one year before the official SEE would have taken place. We assign children to their original birth cohort to address the endogeneity of early assessment. Children who are not ready for school in a given year take a special examination one year later and thus are not included in the baseline SEE.

[14] The diagnosis can take five forms: "negative, no problems", "positive, but no treatment is necessary", "positive, already in treatment", "positive, treatment necessary", and "positive, problem will reduce the child's performance in school".

hyperactivity and peer relationships (Goodman, 1997).[15] The assessed dimensions are relevant predictors of later socio-economic success. Duncan et al. (2007), for instance, show that the dimensions assessed in the SEE are key to predicting later educational achievements. In a similar vein, Gregg and Machin (1999, 2001) and Grissmer et al. (2010) discuss the relevance of children's early cognitive and motor abilities for their later achievements.

The primary caregiver, which is the mother in most cases, completes a questionnaire providing information on child and family background and on the number of years in center-based care the child has completed. We use this information, as well as the age of the child when the SEE is conducted, to impute whether a child was placed in center-based care before the age of 3.[16] Children who attend ECC transfer automatically to Kindergarten and thus may spend more time in Kindergarten than children who do not attend ECC. Effects on outcomes at the onset of primary school can thus be due to the time spent in ECC, the time spent in Kindergarten, or both. Because the estimated effects are triggered by attending child care early in life, we will refer to them as ECC effects.[17]

We use data for 6 school entry cohorts (2009–2014) in Schleswig-Holstein. These data contain identifiers for 360 school districts located in 8 (out of 15) counties.[18] We restrict our analysis to school districts with at least 5 children per cohort because we construct school-district-level information by aggregating individual information. Finally, we merge data on center quality, socio-demographics and labor market information by county and cohort.[19]

3.2. Descriptive statistics

Table 1 provides descriptive statistics for the full estimation sample according to ECC status. Of all children in the sample, 14.6% (8914 children) had some exposure to ECC – we refer to this group as *treated children*.[20] The remaining 85.4% (52,351 children) were not exposed to ECC, and we refer to this group as *control children*.

In comparing children in terms of their skill development, we observe that treated children outperform control children in all dimensions (see Panel A): 73.3% of treated children and 70.7% of control children do not have any language problems; in the case of motor skills, these shares amount to 80.9% and 79.3%, respectively; and in the case of socio-emotional skills to 79.6% and 77.5%, respectively All differences are significant at the 1% level, even though treated children are approximately one month younger than control children (6 years and 1 month versus 6 years and 2 months, see Panel B), which is likely due to our ECC classification. A lower share of low-birth-weight children (5.5% versus 6.8%) and migrant children (18.6% versus 21.0%) among treated children, however, points towards positive selection into treatment. This claim is furthermore supported by the fact that 39.2% of treated children but only 30.9% of control children are raised by a mother with tertiary education (see Panel C). Differences in terms of household composition – treated children have fewer siblings than control children (1 sibling versus 1.2 siblings) and are more likely to live with one parent only (13.9% versus 12.3%) – are likely the result of certain children bypassing the waiting list due to priority criteria.

Our main analysis abstracts from children being given priority access to ECC. The underlying reason is that selection into ECC follows different rules depending on a child's priority status. Recall that priority is given to children with single parents or those with older siblings enrolled in ECC. We allocate children to the *non-priority* group if they reside with both parents and have at most one sibling. All remaining children – those with a single parent or more than one sibling – are assigned to the group of *priority* children. Because the SEE is conducted when children are 6 years old and thus does not provide us with information from the time the parents applied for a slot in ECC, our allocation is not perfect, but it is reasonably accurate.[21]

Table 1 reveals several differences between treated and control children. As a result, the simple mean comparison of child development outcomes in Panel A of Table 1 is likely biased. We therefore provide OLS estimates of the ECC effects on child development adjusting for child and family characteristics (see Table 2). Note that the presented OLS estimates also account for the full set of cohort and school district fixed effects and county cohort interactions. While being slightly weaker than raw mean differences, these results indicate a positive correlation between children's ECC attendance and their development: treated children have an advantage over control children on the order of 2.5 percentage points (ppts) in language skills and 1.4 ppts in motor skills. There are no significant differences in socio-emotional skills.

Table 2
Early child care and child development: OLS estimates.
Source: School entry examination, *Schleswig-Holstein* 2009–2014, our calculations.

	Language	Motor	Socio-emotional
Early child care attendance (D)	0.025***	0.014***	0.004
	(0.006)	(0.005)	(0.005)
Mother has tertiary education (D)	0.093***	0.052***	0.070***
	(0.004)	(0.004)	(0.004)
Adjusted R2	0.113	0.089	0.097
Children	61,265	61,265	61,265

Notes: All estimates are based on OLS estimations, controlling for the full set of child and family characteristics, cohort and school district dummies and county cohort interactions. Inferences are based on standard errors that allow for clustering at the school district level.
Significance levels are indicated as follows: *0.01 percent,**0.05 percent and *0.1 percent

3.3. Fast- versus slow-expansion districts

We study the ECC effects in a setting where ECC supply expands substantially over time, but unequally across space. The expansion under study was triggered by the 2005 reform, which affected some but not all cohorts in our data. Specifically, the reform did not affect the

[15] The assessment of socio-emotional skills might be affected by subjective perceptions of the primary caregiver or by non-response problems. Because the pediatrician reassesses children's socio-emotional skills and because a medical diagnosis is available in 93% of all cases, reporting bias and non-response bias are not a major concern in our context.

[16] Specifically, children with ECC exposure are those who, at age 3, had already been in center-based care, i.e., children who were 5 years old at the time of the SEE and had completed 2 or more years in center-based care or children who were 6 years old at the time of the SEE and had completed 3 or more years in center-based care. Our imputation assumes that children start center-based care on their birthdays, which is plausible for Germany: parental leave benefits are paid for either 12 or 24 months after childbirth, parental leave ends after 36 months, and all children are entitled to attend Kindergarten at age 3. Women tend to return to work and thus send their children to child care when the child turns 1, 2 or 3 years old. Bauernschuster and Schlotter (2015) or Schoenberg and Ludsteck (2014) document this pattern.

[17] This issue arises in any dynamic evaluation in which an initial program may place individuals on different trajectories (Eberwein et al., 1997).

[18] We observe a child's district of residence at the SEE date. Because post-birth mobility is low in West Germany, the district of residence at the SEE date is unlikely to have changed between birth and school entry for most children in our sample.

[19] District identifiers are anonymous in the SEE data and therefore prevent us from merging socio-economic data or information on center quality at that level.

[20] This share grows over time and corresponds to 5.4% in birth cohort 2002/03, 7.2% in birth cohort 2003/04, 13.1% in birth cohort 2004/05, 16.9% in birth cohort 2005/06, 20.5% in birth cohort 2006/07 and 26.0% in birth cohort 2007/08.

[21] We do not know the exact age of the siblings, and here, we assess how many of the non-priority children may have been prioritized because they had an older sibling in ECC. Of the non-priority children, 27% have no siblings, while 73% have one sibling. Data from the German Socio-Economic Panel reveal the following: the probability of children with one sibling to have an older sibling is approximately two-thirds (67.6%), and the probability of this sibling being in the age range of 0–2 years old and thus potentially enrolled in ECC is approximately one-quarter (26.6%). As such, at most 13% (= 0.266 * 0.676 * 0.730) of all children are misclassified as non-priority children. ECC attendance among the non-priority children was at most 14.6%, and thus, 2% of the children with one sibling are likely to have an older sibling enrolled in ECC and may have benefitted from priority access to ECC.

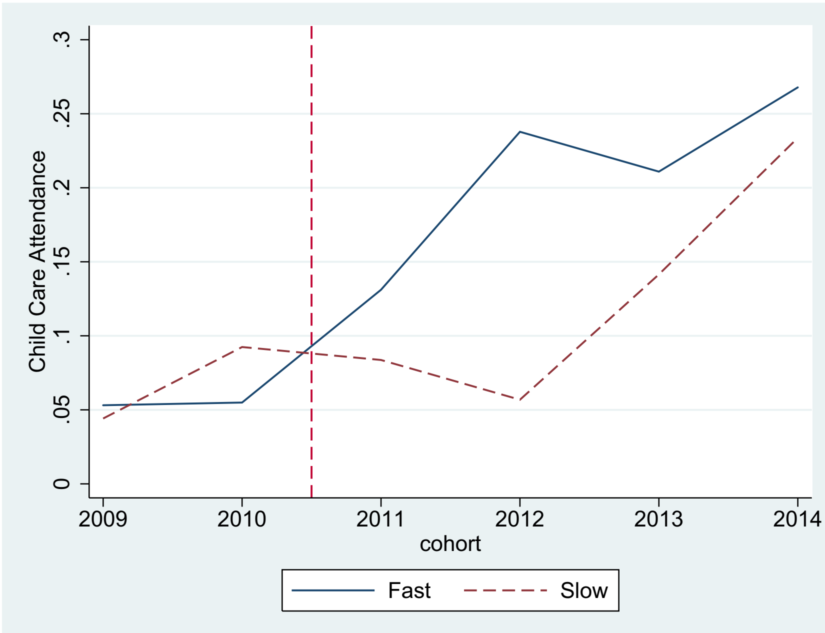

Fig. 1. Expansion of child care attendance across school entry cohorts. Note: This figure reports ECC attendance for school districts that expand rapidly – above median expansion for the 2011 and 2012 cohorts – and school districts that expand more slowly – below median expansion for the 2011 and 2012 cohorts. The x-axis refers to the 2009 (born in 2002/03), 2010 (born in 2003/04), 2011 (born in 2004/05), 2012 (born in 2005/06), 2013 (born in 2006/07) and 2014 (born in 2007/08) school entry cohorts.

2009 and 2010 school entry cohorts, which were born between July 2002 and June 2004, because they were already in Kindergarten by the time the reform took effect. The consecutive cohorts, examined for school entry between 2011 and 2014 and born between July 2005 and June 2008, may have been affected by the reform. Inspired by Havnes and Mogstad (2011, 2015), we exploit this reform to construct a difference-in-differences-type strategy. Specifically, we define the group of fast-expansion districts as those districts that, in the initial years after the reform – and thus for the 2011 and 2012 school entry cohorts – expanded their ECC by more than the median expansion. All remaining districts are allocated to the slow-expansion group. The fast-expansion group consists of 190 districts, and the slow-expansion group consists of 170 districts.[22]

Fig. 1 shows the proportion of children in ECC for fast- and slow-expansion districts. While starting at a similar ECC attendance level (see 2009 and 2010 cohorts), the two groups differ by construction in terms of their expansion speed. Among all children belonging to the 2012 cohort and residing in a fast-expansion district, 25% attended ECC, while only 5% of the children residing in a slow-expansion district did so. Importantly, however, there is no difference in ECC attendance among children belonging to the 2014 cohort: in both groups, 25% were enrolled in ECC. This evidence suggests that demand for ECC is similar in fast- and slow-expansion districts, while the time required to attain that level differs.

The crucial question is whether the timing of the ECC expansion relates to child development outcomes. Table 3 describes the slow- and fast-expansion districts for the cohorts not yet affected by the expansion. Child development outcomes do not differ between the two groups (Panel A). As such, there is no indication that districts with unfavorable child development lobbied for faster ECC expansion. Fast-expansion districts have somewhat lower ECC attendance than control districts: 5.7% in fast-expansion districts vs 7.5% in slow-expansion districts (Panel B). School entry cohorts are also larger, with 30% more children in fast-expansion districts compared to the school entry cohorts in slow-expansion districts (see Panel C). These differences are consistent with the background on the expansion provided in Section 2: districts with larger cohorts and relatively low initial ECC supply received preferential treatment in the allocation of funds. Importantly, however, slow- and fast-expansion districts are statistically identical in terms of further socio-demographic characteristics, including the proportion of college-educated mothers and thus potential labor market gains from sending children to ECC. In sum, fast-expansion districts are statistically similar to slow-expansion districts, whereas children in ECC are not statistically similar to children who are not in ECC (see Table 1). As such, our strategy of comparing districts improves on comparing children directly (shown in Table 2).

One remaining concern is that fast- and slow-expansion districts may have evolved differently over time. Fig. 2 addresses this point and

Table 3
Pre-reform differences between fast- and slow-expansion districts.

Variable	Fast	Slow	p-Value
A. Child development outcomes			
Language skills	0.717	0.728	0.634
Socio-emotional maturity	0.79	0.795	0.569
Motor skills	0.799	0.816	0.347
B. ECC attendance			
ECC attendance	0.057	0.075	0.000
C. District characteristics			
Log children in school district cohort	4.009	3.700	0.001
Age (Avg)	74.356	73.984	0.284
Boys (Fract)	0.518	0.511	0.120
Low birth weight (Fract)	0.070	0.065	0.102
Foreign (Fract)	0.198	0.184	0.247
Number of siblings (Avg)	1.179	1.191	0.861
Single mothers (Fract)	0.122	0.117	0.364
Mother has tertiary education (Fract)	0.309	0.279	0.130
Districts	190	170	

Notes: This table reports district averages for the 2009 and 2010 school entry cohorts.

[22] The number of observations is not identical since we split the sample before imposing further sample selection conditions. Median growth in ECC attendance for the 2011 and 2012 cohorts is approximately 2 ppts. The results are not sensitive to changing this threshold to 6 ppts.

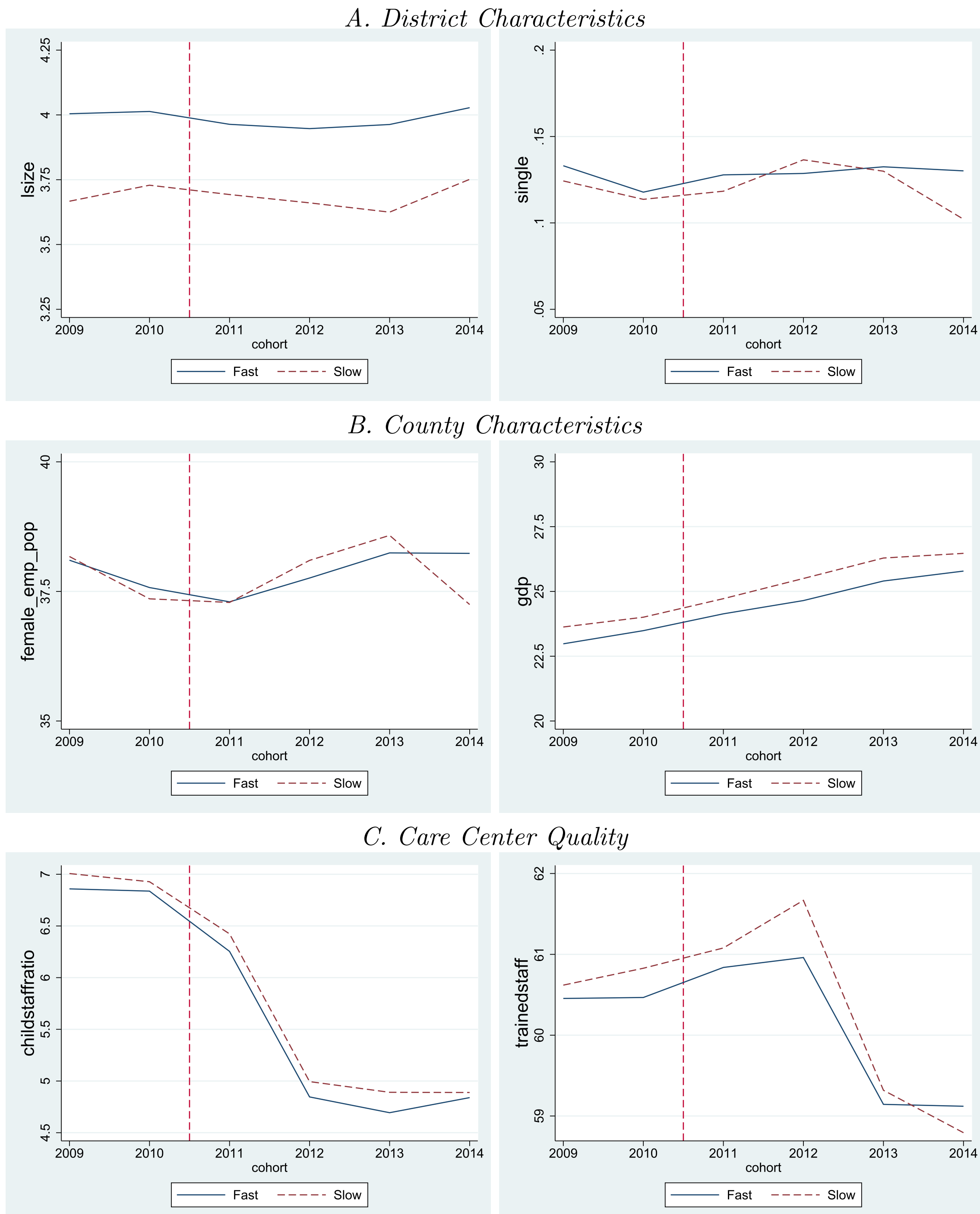

Fig. 2. Cohort trends for fast- and slow-expansion districts. Notes: Panel A shows the school district level log size of the school entry cohort and proportion of single mothers. Panel B shows the cohort trends in county female employment to population and gross domestic product. Panel C shows the county average child-to-staff ratio and the proportion of staff with a childhood education degree.

shows cohort trends for district composition, county economic conditions, and county average center quality.[23] Fast-expansion districts are larger, but the gap remains constant throughout all cohorts (see Panel A). The share of children living in a single-parent household is comparable between fast- and slow-expansion districts, and both groups follow very similar trends over time (the exception is the 2014 cohort,

[23] Fig. 2 displays trends for county characteristics weighted by the number of school districts. County characteristics are identical for slow- and fast-expansion districts within counties, and thus, any differential changes over time at the district level are mitigated by county characteristics. However, note that school districts are not perfectly balanced within counties, with the share of children in fast-expansion districts ranging from 19.5% to 86%. A county with more fast-expansion districts will thus contribute more strongly to

(*footnote continued*)
the estimation of the fast-expansion line than the slow-expansion line, and vice versa. Examining county characteristics averaged by districts nonetheless strikes us as useful in assessing trends.

in which the share of single mothers in slow-expansion districts is somewhat lower than that in fast-expansion districts). Female labor force participation evolves very similarly in fast- and slow-expansion districts for the 2009 to 2013 cohorts, with the 2014 cohort having lower female employment in the slow-expansion group than in the fast-expansion group – possibly as a result of the faster expansion of ECC in the latter districts (see Panel B). Slow-expansion districts are, on average, situated in wealthier counties, but the gap in GDP remains constant over time.

Fig. 2 also shows trends in two aspects of ECC quality (see Panel C). The child-to-staff ratio declines meaningfully over all examination cohorts. In the cohorts unaffected by the reform – those examined in 2009 and 2010 – there was one early care worker for approximately every 7 children, but there was one for fewer than 5 children from the 2012 to 2014 cohorts onward. Importantly, these quality improvements were symmetric between fast- and slow-expansion districts. Finally, in slow-expansion districts until the 2012 cohort, the share of child care workers with a degree lies above the share observed in fast-expansion districts, but the differences are small, less than one-half of 1%, and disappear in the 2013 and 2014 cohorts. The share of trained staff also does not strictly follow parallel trends, but the differences are again so small that they are unlikely to account for a large difference in the effects.

While fast- and slow-expansion districts are similar at the district level, capturing developments at the county level appears crucial. Youth welfare services at the county level control and implement state quality regulations. Counties are also natural local labor market areas. Our empirical analysis will therefore address developments at the county level and allow for a flexible cohort trend in each county.

4. Empirical strategy

This section introduces the models of potential outcomes and treatment choice and defines the MTE.[24] The section also discusses the assumptions necessary for identification and describes the details of the estimation.

4.1. Model set-up

We are interested in the effects of ECC on children's development. Let $D = 1$ for a child who attended ECC and $D = 0$ otherwise. The potential child development outcomes are thus Y_1 for children with ECC exposure and Y_0 for children without ECC exposure. We model potential outcomes, Y_1 and Y_0, and the effects of ECC, $Y_1 - Y_0$, as follows

$$Y_0 = X'\beta_0 + R'\alpha + C_M'\gamma + U_0 \tag{1}$$

$$Y_1 = X'\beta_1 + R'\alpha + C_M'\gamma + U_1 \tag{2}$$

$$Y_1 - Y_0 = X'\beta + U_1 - U_0 \tag{3}$$

where $\beta \equiv \beta_1 - \beta_0$. The levels of potential outcomes, Eqs. (1) and (2), depend on individual child and family characteristics, such as a child's age, gender, and migrant status, as well as the mother's education, all contained in vector X. Importantly, the impact of individual child and family characteristics on the levels of potential outcomes may vary depending on treatment status, and the vector β captures treatment effect heterogeneity in terms of child and family characteristics. The levels of potential outcomes further vary across school districts, captured by the vector R, and across cohorts within counties, captured by the vector C_M containing interactions between cohort and county indicators. In our baseline specification, school districts and country-cohort trends do not enter the treatment effect. In a sensitivity analysis, however, we explore a specification that allows treatment effects to vary across counties and cohorts. Finally, the levels of potential outcomes depend on a set of unobservable components that are likely to vary between treated and untreated individuals, U_1 and U_0.

We use the following latent variable discrete choice model for selection into treatment

$$D = \mathscr{I}[Z'\pi_Z - V] \tag{4}$$

where $\mathscr{I}[]$ is the indicator function. Z contains the same set of covariates (X,R,C_M) as the potential outcome Eqs. (1) and (2). In addition, Z needs to contain a set of instruments $\widetilde{Z}$, which are excluded from the potential outcomes, Eqs. (1) and (2). We will discuss our choice of instruments in the next paragraph. The error term V enters the choice equation with a negative sign and thus embodies unobserved characteristics that make an individual less likely to receive treatment, which is frequently interpreted as the resistance to be treated or distaste regarding treatment (Cornelissen et al., 2016a). In our setting, V captures both parental preferences regarding ECC and constraints, e.g., features of the rationing mechanism that allocates ECC slots to children.

Our choice of instruments, $\widetilde{Z}$, relies on the staggered implementation of the 2005 reform.[25] Since each district was in charge of formulating an expansion plan, we capture district-specific expansions as the pre-to-post expansion change in ECC attendance in each district. Fig. 1 suggests that the 2005 reform affected the 2011 to 2014 school entry cohorts in fast-expansion districts and the 2013 to 2014 school entry cohorts in slow-expansion districts. We define a dummy $Post = 1$ for cohorts 2011 to 2014 in fast-expansion districts, $Post = 1$ for cohorts 2013 and 2014 in slow-expansion districts, and $Post = 0$ otherwise. The set $\widetilde{Z}$ contains interaction terms between the district-specific post-expansion period and the respective district, $\widetilde{Z} \equiv Post \cdot R$.[26] These interaction terms capture the district-specific expansions of ECC in response to the 2005 reform. We discuss why $\widetilde{Z}$ can be excluded from the outcome equations below (see the paragraph on Assumptions).

Given our models of potential outcomes and treatment, the treatment effect $Y_1 - Y_0$, Eq. (3), may vary across individuals with different observed characteristics X and among individuals with the same observed characteristics X but different values of the unobserved components U_1 and U_0. While our model does not specify why parents decide to send their child to ECC, it does not preclude the possibility that parents base their choice on personal knowledge of their child's potential outcomes. Any such knowledge will be reflected through dependence between the potential outcomes, Y_0 and Y_1, and the unobserved component of treatment choice, V.

In order to trace the dependence between the treatment effect and the unobserved component of the treatment choice, the MTE literature customarily relies on the quantiles of the distribution of V rather than its absolute values. For this, the following transformation of the selection rule in Eq. (4) is useful: $Z'\pi_Z - V \geq 0 \Leftrightarrow Z'\pi_Z \geq V \Leftrightarrow F(Z'\pi_Z) \geq F(V)$, with $F()$ denoting the *c.d.f.* of V. The term $F(Z'\pi_Z)$, also denoted by $P(Z)$, is the propensity score or – put differently – a child's probability of

[24] The MTE framework was first explored by Bjorklund and Moffitt (1987) and developed further by Heckman and Vytlacil (1999, 2001, 2005). For a recent overview of the literature estimating MTEs, please refer to Cornelissen et al. (2016a).

[25] Earlier work that exploits spatio-temporal variation in child care supply includes Havnes and Mogstad (2011), who use interactions between municipalities and time, given the lack of supply or attendance data; Felfe and Lalive (2014), who use information on attendance; and Cornelissen et al. (2016b), who use survey data on supply.

[26] The vector $\widetilde{Z}$ contains one element for each district. The element for the child's district of residence is one if the child belongs to the post-expansion cohorts and 0 otherwise. All remaining elements of the vector are 0. This set of instruments is discrete. Brinch et al. (2017) discuss the interpretation, identification, and estimation of MTEs with discrete instruments. We adopt methods designed for continuous instruments, as the variation in ECC attendance is essentially continuous (Fig. 3). In a classical DD setting, we would instrument with an interaction term, $Post \cdot T$, where T is a set of "treated" districts. Our setting differs from the classical setting in two respects. First, since we are looking for variation in the number of children entering care, we interact the Post indicator with every school district. Moreover, all districts experience an expansion in ECC at some point, meaning that there are no control districts.

Table 4
First-stage estimates.

	ECC attendance
Child age (months)	−0.007***
	(0.000)
Boy (D)	0.001
	(0.003)
Mother has tertiary education (D)	0.031***
	(0.004)
Foreign (D)	−0.029***
	(0.005)
Constant	0.573***
	(0.035)
District dummies	Yes
Cohort dummies	Yes
County * Cohort dummies	Yes
District * Post-expansion period dummies	Yes
District * Post-expansion period dummies, first stage F	19.84
District * Post-expansion period dummies, first stage p	0.00
Adj. R2	0.14
Children	38,589

Notes: The table reports estimates of the model for ECC attendance (Eq. (4)). The table shows parameters associated with child and family characteristics X. Cohort and district fixed effects and county-cohort interactions are included in the model but not shown. Interactions between districts and their respective expansion periods are included and displayed graphically in Fig. 3 below. "District*Post-expansion period dummies, F-stat" reports the F-statistic and "District*Post-expansion period dummies, p-value" the p-value of a test of joint significance of the district and post-expansion period interactions. The sample is restricted to priority children (38,589 in total), that is, children who live with both parents and have at most one sibling.
Significance levels are indicated as follows: *0.01 percent,**0.05 percent and *0.1 percent

attending ECC. We now define $U_D \equiv F(V)$. U_D is the quantile of the distribution of V and captures a child's latent propensity or – put differently – his or her latent resistance to attend ECC. The MTE is

$$MTE(X = x, U_D = p(Z)) = E(Y_1 - Y_0 | X = x, U_D = p(Z)) \tag{5}$$

and represents the average treatment effect conditional on the set of covariates X and indexed by a child's latent resistance to attend ECC U_D.

4.2. Assumptions

The set of excluded interactions, $\widetilde{Z}$, must fulfill two conditions. First, the interactions must be strong predictors of ECC attendance. We test for the presence of many weak instruments, as $\widetilde{Z}$ is composed of several hundred interaction terms.[27] This step is important because weak instruments induce bias in two-stage least squares (2SLS) estimates. Specifically, we use the approach by Skeels and Windmeijer (2016) to obtain exact critical values for models with any number of instruments.[28] Based on the 5% exact critical value of 10.52, which results for our application with 318 instruments and a 10% maximum bias of IV relative to OLS, we reject the null that our set of instruments is weak (see Section 5, specifically Table 4 for details).[29]

Second, instruments need to be excludable, i.e., they need to be orthogonal to the unobserved components of the potential outcomes, U_1 and U_0, as well as to the unobserved component of treatment choice, V, conditional on the set of covariates X,R and C_M, i.e.,

$$U_1, U_0, V \perp\!\!\!\perp \widetilde{Z} | X, R, C_M \tag{6}$$

This identifying assumption means that, barring ECC expansions, child development outcomes and ECC attendance would have developed according to the same trend across all districts. The evidence provided in Section 3.3 suggests that fast- and slow-expansion districts are similar in terms of pre-reform observables (Table 3) and observable trends (Fig. 2). As such, the timing of the expansion is likely unrelated to district composition. However, to further probe the identifying assumption (6), we conduct a sensitivity analysis allowing for district-specific linear time trends. In this specification, identification is based on structural breaks in ECC expansion, specifically for all cohorts examined after 2011 in fast-expansion districts and for the cohorts examined in 2013 and 2014 in slow-expansion districts. We also provide estimates when additionally controlling for districts' socio-demographic composition and conduct placebo estimations using low birth weight, an obvious pre-treatment outcome, as an outcome variable.

As in other MTE applications (Brinch et al., 2017; Carneiro et al., 2011), we impose the assumption that the unobserved component of the MTE does not depend on the observed covariates X. Formally, this separability assumption implies that

$$\begin{aligned} MTE(X = x, U_D = p(Z)) &= E(Y_1 - Y_0 | X = x, U_D = p(Z)) \\ &= x'(\beta_1 - \beta_0) + E(U_1 - U_0 | U_D = p(Z)) \end{aligned} \tag{7}$$

Separability implies that individuals can be ordered according to their latent propensity to be treated V (conditional on observables). It allows identifying the MTE over the unconditional support of the propensity score, jointly generated by the instruments and the covariates, as opposed to the support of the propensity score conditional on $X = x$. As such, it considerably reduces data requirements for estimating the MTE curve and hence facilitates estimation.[30]

4.3. Estimation

The MTE can be estimated from the model for the observed outcome

$$Y = D \cdot X'\beta + X'\beta_0 + R'\alpha + C_M'\gamma + D \cdot (U_1 - U_0) + U_0 \tag{8}$$

Taking expectations of this equation allows us to obtain estimates regarding effect heterogeneity in terms of observables, X, and in terms of unobservables, summarized by the function $K(p(Z)) \equiv p(Z)E(U_1 - U_0|p(Z))$:

$$E[Y|p(Z), X, R, C_M] = p(Z)^* X'\beta + X'\beta_0 + R'\alpha_r + C_M'\alpha_{cm} + K(p(Z)) \tag{9}$$

The derivative of Eq. (9) with respect to the propensity score is the MTE

$$MTE(X, p(Z) = U_D) = X'\beta + \frac{\partial K(p(Z))}{\partial p(Z)} \tag{10}$$

Our estimation approach proceeds in two stages. We first estimate the propensity score $p(Z)$ using a linear probability model.[31] In a second

[27] Angrist and Krueger's (1991) analysis alerted the literature to the weak instruments problem.

[28] The critical values published by Stock and Yogo (2005) are inappropriate for our purposes because they apply only to models with up to 30 instruments.

[29] Our data contain information on 360 districts, but only 318 districts are observed before and after the expansion date. Only districts observed throughout the expansion period contribute to the identification of the propensity score.

[30] In sub-sample estimates, we relax the separability assumption somewhat and allow the unobserved component of the MTE to differ between boys and girls, between children with and without migrant backgrounds, and between children of mothers who have completed college and children of those who have not. For instance, in the analysis by gender, we estimate a baseline polynomial for girls, $K_G(p)$, and a separate polynomial for boys, $K_B(p)$. We test for differences in polynomial terms and report estimates for boys and girls. We proceed in the same way in the analysis by immigrant status and mothers' education level. While these analyses do not allow for full flexibility, they allow us to assess the extent to which a restrictive MTE specification could be misleading.

[31] A probit model assumes that no predicted probability is 0 or 1. This restriction poses a challenge for our first stage since in several districts, there are cohort combinations in which no or all children attend ECC. A linear probability model allows for perfect prediction and strikes us as appropriate for our setting. 2SLS also assumes a linear probability model for the first stage with binary endogenous regressors. Individual propensity scores

stage, we estimate models of the child development outcomes and approximate the unknown shape of $K(p(Z))$ using a third-order polynomial in the propensity score. In a sensitivity analysis, we explore a semi-parametric specification of the MTE (Heckman et al., 2006). The propensity score is a generated regressor, and we base our statistical inferences using the non-parametric bootstrap of both the first and second stages with 100 replications and cluster at the district level.

5. Results

This section discusses the results for the selection into ECC and for the marginal effects of ECC on child development outcomes. The baseline results refer to the non-priority children (38,589 children) who received a slot through the waiting list. We also present results for the priority children (22,676 children), who bypassed the waiting list because they either lived in a single-parent household or had older siblings in ECC. We then turn to a discussion of the effects of alternative policy reforms, where we devote particular attention to the effects for relevant subgroups such as boys and girls, children of college- and non-college-educated mothers (who we also refer to as high- and low-educated mothers) and children with and without migrant ancestry (who we also refer to as immigrants and natives).

5.1. Selection into early child care – first stage

Table 4 shows the parameter estimates of the model explaining ECC attendance for non-priority children (Eq. (4)). The model contains a set of child and family characteristics (X),[32] a set of cohort and district fixed effects, and a full set of cohort-county interactions (estimates not shown in Table 4). The model also includes 318 interaction terms between the district fixed effects and the district-specific post-expansion period (estimates also not shown in Table 4 but displayed graphically in Fig. 3), which serve as our set of instruments.

The results indicate that children's socio-demographic background is a decisive determinant of their ECC attendance. Specifically, children of low-educated mothers are less likely to attend ECC than children of high-educated mothers, and immigrant children are less likely to attend ECC than their native counterparts. Older children are less likely to attend ECC, yet this is probably a result of our way of imputing ECC attendance (see Section 3.1 for details).

The histogram shown in Fig. 3 displays the distribution of the coefficients belonging to our instruments – the 318 interactions between the districts and their respective post-expansion periods – that result when estimating our first stage; thus it allows us to discuss our source of identification. There is substantial variation in ECC attendance that remains even after netting out the average district level and flexible county-specific trends.[33] Table 4 reports a test of the strength of our set of instruments. The two rows "District * Post-Expansion Period Dummies" report the F-statistic and the associated p-value resulting from a test of the joint significance of the full set of interactions. We reject the null of weak instruments because the F-statistic of the interaction terms amounts to 19.84 and is larger then the exact critical value of 10.52 calculated using the approach by Skeels and Windmeijer (2016). The district-specific expansions are strong predictors of ECC attendance and will thus allow us to estimate the unobserved component of the MTEs.

Before turning to our main outcome models, we report non-parametric evidence on child development and ECC attendance. Fig. 4 displays the relationship between the district-specific shocks to ECC attendance, documented above in Fig. 3, and the corresponding district-specific "reactions" in child development outcomes. The "reactions" in child development outcomes correspond to the interactions between districts and their specific post-expansion period in a model that includes the same regressors as Eq. (9), except for the terms involving the propensity score. Fig. 4 shows a binned scatter of the average "reaction" in the outcome on a 5-percentage-point ECC attendance "shock" grid, where the size of the dots represents the number of children in each district-period cell. Fig. 4 also superimposes a quadratic regression line that uses the raw district-period effects, weighted by the size of each district-period cell. The slope of the regression line provides evidence on the MTE that does not rely on the parametric structure assumed in Eq. (9).

The regression line in Fig. 4 suggests that average language skills decrease slightly when more children attend ECC. The inverted U shape of the graph that plots average motor skills against average ECC attendance suggests that the effects of ECC attendance on the marginal child's motor skills are initially positive and then turn negative. The shape of the graph plotting average socio-emotional skills against average ECC attendance in turn suggests positive returns to ECC attendance on the marginal child's socio-emotional skills throughout.

5.2. Child development outcomes – non-priority children

Table 5 reports the estimates of all MTE parameters, specifically the polynomial of the propensity score and the interactions between the individual child and family characteristics and the propensity score, along with various tests shown at the bottom of the table. All models control for a full set of individual child and family characteristics, a set of cohort and district fixed effects and a full set of county-cohort interactions (the full set of estimates resulting from Eq. (9) is shown in Table A1 in the Appendix). MTEs are identified by excluding the interactions between the districts and their respective post-expansion periods. Results are shown for non-priority children only and thus for a setting in which rationing plays an important role.

Table 5 shows the extent to which ECC effects vary depending on children's observed socio-demographic characteristics, specifically between boys and girls, natives and immigrants, and children with high-educated mothers and those with low-educated mothers, all captured by the vector β in Eq. (9). Boys benefit significantly more from attending ECC than girls across all development dimensions. In fact, ECC attendance reduces the gap between boys and girls in language and socio-emotional skills by approximately one-half and the gap in motor skills by approximately one-third. There are no differential effects of ECC attendance along any of the other observed socio-demographic characteristics, with the exception of children of high-educated mothers, who benefit significantly more in terms of their motor skill development than children of low-educated mothers.[34]

Table 5 displays the coefficients of the polynomial of the propensity score determining the shape of the unobserved component of the MTE, $K(p(Z))$. The row " F-Test, Polynomial pZ, p-value " provides the respective tests for the joint significance of all higher-order terms of the propensity score. The results provide weak support for MTE

(*footnote continued*)
may be invalid, i.e., smaller than 0 or larger than 1. The average propensity score in a district, however, will always be valid, given the saturated model specification. We do not adjust invalid individual propensity scores since the variation in propensity scores across districts identifies the marginal treatment effects.

[32] The model controls for the child's age at examination, gender, and migrant ancestry, and for the education level of the child's mother. Further child and family characteristics such as the number of siblings and single motherhood characterize priority status and thus are not controlled for.

[33] Note that the underlying estimation contains a full set of district and cohort dummies and a full set of county-cohort interactions. As such, Fig. 3 displays the extent to which the pre-to-post expansions across districts deviate from the respective county-cohort average. The few negative interaction terms arise because the associated district expanded ECC less than the average expansion in the respective county and cohort combination.

[34] The row " F-Test, Interactions pZ * (Child-Family), p-value " provides p-values of a joint significance test of all interactions between the propensity score and the child and family background variables, rejecting the null hypothesis for all – except motor skill development – at the 10% significance level.

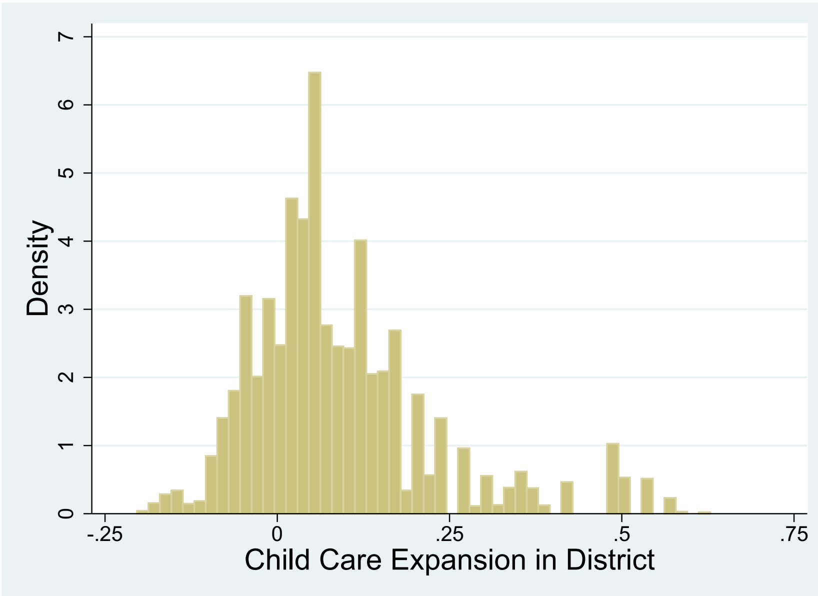

Fig. 3. Source of identification – pre-to-post-reform expansion. Notes: This figure displays the coefficients of the interactions between the districts and the district-specific expansion period – equal to one for all cohorts examined from 2011 to 2014 for the fast-expansion districts and for the latter two cohorts examined in 2013 and 2014 for the slow-expansion cohorts – resulting from estimating Eq. (4). Note that this equation also controls for a full set of cohort and district dummies and county-cohort interactions in addition to individual child and family characteristics.

heterogeneity with respect to U_D for motor skills, the null is rejected only at the 10% significance level. The results neither support effect heterogeneity for language skills nor that for socio-emotional skill development.

A central focus of our paper is whether or not children benefit from attending ECC and how this situation varies with the latent propensity to attend ECC. Fig. 5 shows the MTEs of attending ECC for children sorted according to their latent propensity to attend ECC, U_D, with lower values of U_D representing children who are first to attend ECC or – put differently – children with the lowest resistance to attend ECC and higher values of U_D representing children who are last to attend ECC, given available slots.[35] Overall, the pattern of the baseline MTE estimates displayed in Fig. 5 is very much consistent with the descriptive evidence – the slope of the regression line shown in Fig. 4 – suggesting that the baseline specification captures fairly well how ECC affects child development outcomes.

An interesting pattern for children's motor skill development can be observed: among children who were first to attend ECC, the share of children without any problems in motor skill development increases by approximately one-quarter after attending ECC. The effect decreases with children's latent resistance to attend ECC and is essentially 0 for children who are placed in ECC only when supply exceeds stated demand (at approximately $U_D > 0.4$). The pattern is reversed for children's socio-emotional skills. Children who are among the first to be placed in ECC, $U_D < 0.3$, do not seem to benefit substantially from attending ECC in terms of their socio-emotional development. On the contrary, children who are placed in ECC only when the number of available slots approaches stated demand, specifically when $U_D > 0.3$, experience strong gains in their socio-emotional development. Among these children, the share who experience socio-emotional issues declines by up to 25 ppts after attending ECC. Fig. 5 does not indicate considerable heterogeneity in the unobserved component of the returns to ECC attendance in terms of children's language skill development.

One explanation for why MTEs for language skills and, particularly, motor skills differ from MTEs for socio-emotional skills may be that care centers do a better or a worse job at substituting for home care in fostering specific skill dimensions. Staff employed in care centers follow strict curricula to promote language and motor skill development – e.g., by using circle play, reading or physical activities – activities that most parents also provide. Centers also nourish children's socio-emotional skills via social interactions with other children in the center. The relationships that children form with their peers from very early on exert an enormous influence on their subsequent development, particularly on their social competence (Ladd, 2005). Parents likely to send their children to ECC may understand what is required to foster their children's social skills, e.g., meeting with other families regularly or visiting the public playground, if their children are not enrolled in ECC. Parents unlikely to send their child to ECC may underestimate the development gains from peer interactions and may thus promote their children's socio-emotional skills much less than the center would. As such, an explanation for why the MTEs for socio-emotional skills of children with a high resistance to attend ECC are positive is that home care likely offers them fewer peer interactions than center-based care does.

5.3. Child development outcomes – priority children

Fig. 6 shows the results for priority children (a total of 22,676 children who either live with a single parent or have two or more siblings).[36] These children are given priority access to ECC and are thus served first when new ECC slots open. Fig. 6 shows that the effects are imprecisely estimated, and the point estimates are close to 0 across all dimensions. Even more striking is that there is essentially no variation in the MTEs across the distribution of U_D. This evidence suggests that the rationing mechanism is an important element of the latent propensity to attend ECC. Nevertheless, an alternative explanation for the absence of any ECC effects for priority children may be the fact that many priority children have two or more siblings and may thus not experience as much of a difference between home and ECC care in terms of exposure to peers.

[35] We report MTE estimates on a support range between 0 and 0.7, as 99% of our observations lie in this range.

[36] We completely separate first- and second-stage estimates for priority children from estimates for non-priority children.

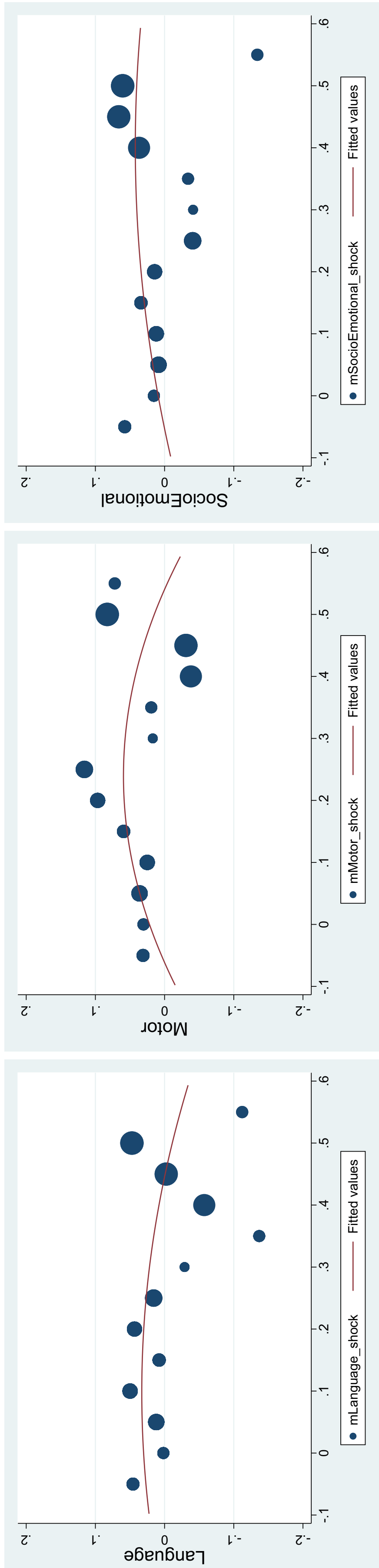

Fig. 4. Descriptive evidence. Note: This figure plots the coefficients of the district-expansion-period interactions estimated in the different skill development models against the coefficients of the district-expansion-period interactions estimated in the ECC attendance model. The scatter plots average the outcome coefficients in 5 ppt bins of ECC attendance, with the dot size reflecting the number of observations. The regression uses the original data and is weighted to reflect the number of children in each district * expansion period interaction cell and provides a quadratic plot of outcomes in ECC expansion. The slope of the regression line corresponds to the MTE.

Table 5
Baseline MTE parameter estimates.

	Language	Motor	Socio-emotional
pZ	0.149	0.298**	0.063
	(0.143)	(0.149)	(0.133)
pZ^2	−0.127	0.019	−0.068
	(0.439)	(0.388)	(0.395)
pZ^3	−0.123	−0.474	0.251
	(0.405)	(0.390)	(0.364)
pZ * Child age	−0.006	−0.005	0.008*
	(0.005)	(0.004)	(0.004)
pZ * Boy	0.065**	0.054*	0.061**
	(0.026)	(0.029)	(0.025)
pZ * Foreign	0.053	−0.044	0.034
	(0.042)	(0.031)	(0.033)
pZ * Tertiary	−0.027	0.072**	−0.023
	(0.033)	(0.034)	(0.028)
Individual and family characteristics	Yes	Yes	Yes
Cohort dummies	Yes	Yes	Yes
District dummies	Yes	Yes	Yes
Country * Cohort dummies	Yes	Yes	Yes
F-test interactions pZ * (Child-Family), p-value	0.043	0.141	0.052
F-test polynomial pZ, p-value	0.274	0.059	0.213
R-squared	0.102	0.085	0.091
Children	38,589	38,589	38,589

Notes: The table reports estimates of the coefficients associated with the propensity score and the respective child or family characteristics. "F-test interactions pZ*(Child-Family), p-value" reports the p-value of a test of joint significance of all interactions between the propensity score and the child or family characteristics. "F-test polynomial pZ, p-value" reports the p-value of a test of joint significance of the second- and third-order propensity score polynomials. Tests and standard errors are based on non-parametric bootstraps with 100 replications. The sample is restricted to non-priority children (38,589 in total), that is, children who live with both parents and have at most one sibling.
Significance levels are indicated as follows: *0.01 percent,**0.05 percent and *0.1 percent

5.4. Sensitivity analysis

We now assess the sensitivity of our baseline results. Fig. 7, Panel A, probes sensitivity to the functional form of $K(p)$, implementing the semi-parametric approach proposed by Heckman et al. (2006).[37] As in the baseline results, the MTEs for language skills do not exhibit heterogeneity in the unobserved component. Contrary to the baseline results, the MTEs for motor skills do not exhibit any notable heterogeneity in the unobserved component.[38] The MTEs for socio-emotional skills, albeit somewhat weaker, are largely in line with the baseline results. Panel B in Fig. 7 shows estimates that allow for effect heterogeneity across cohorts and counties, C_M, in addition to effect heterogeneity in terms of individual child and family characteristics. This specification captures variation in the ECC effects due to, e.g., differences in care center quality, which is regulated by the youth welfare services located at the county level and may vary over time. The resulting estimates are remarkably similar to our baseline results, with the exception of motor skills, which remain positive independent of the value of U_D.

A key threat to our identification strategy is the district-specific time trends in the absence of the reform. Fig. 8, Panel A reports the

[37] The child development function is a partial linear model with a parametric linear index component and a non-parametric function $K(p(Z))$. We estimate the derivative of $K(p(Z))$ in two steps. First, we remove all effects of observed characteristics, district fixed effects, and county-cohort interactions and retain the residuals. We then non-parametrically regress the development residuals on the propensity score using a local second-order polynomial estimator for the mean and its first derivative, with an ad-hoc bandwidth of 0.20.

[38] We also assessed the functional form using fourth-order polynomial approximations. The baseline results for motor skills are similar to the third-order polynomial results. The results are available upon request.

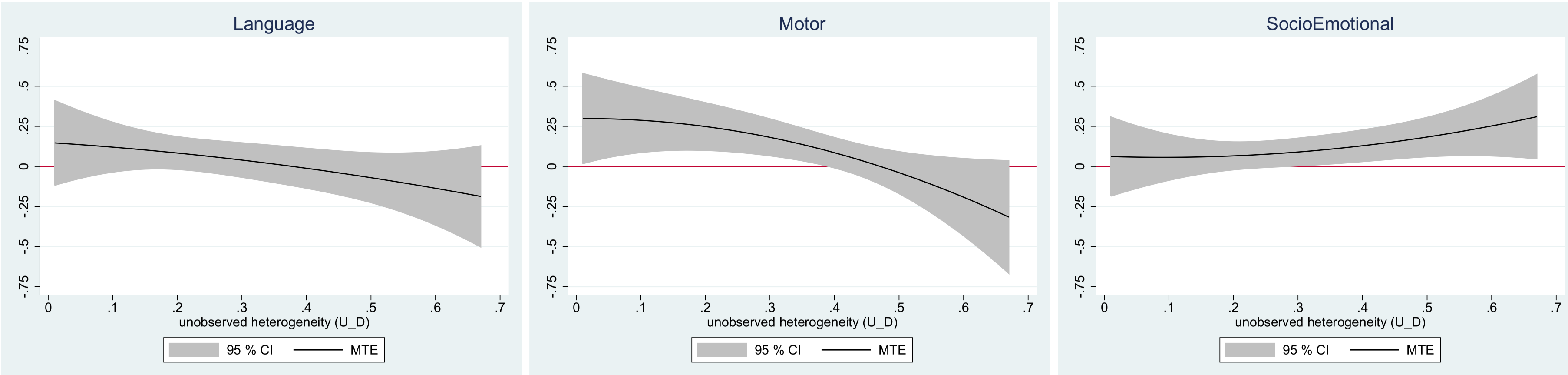

Fig. 5. MTEs of ECC on child development for non-priority children. Note: These figures report the MTEs of ECC attendance on the skill development of non-priority children who enter child care via the regular rationing mechanism (38,589 non-priority children who either live with both parents or have at most one sibling). The underlying equation includes child and family characteristics, cohort and district fixed effects, and county-cohort interactions. We use a linear model to estimate the propensity score with interactions between the districts and their respective post-expansion periods as instruments. MTEs are thus identified from district and post-expansion period interactions, starting in 2011 for fast-expansion districts and in 2013 for slow-expansion districts. U_D is children's latent propensity – or resistance – to attend ECC. Low values of U_D indicate children who are most likely, or least resistant, to attend ECC. Standard errors are based on a non-parametric bootstrap with 100 replications.

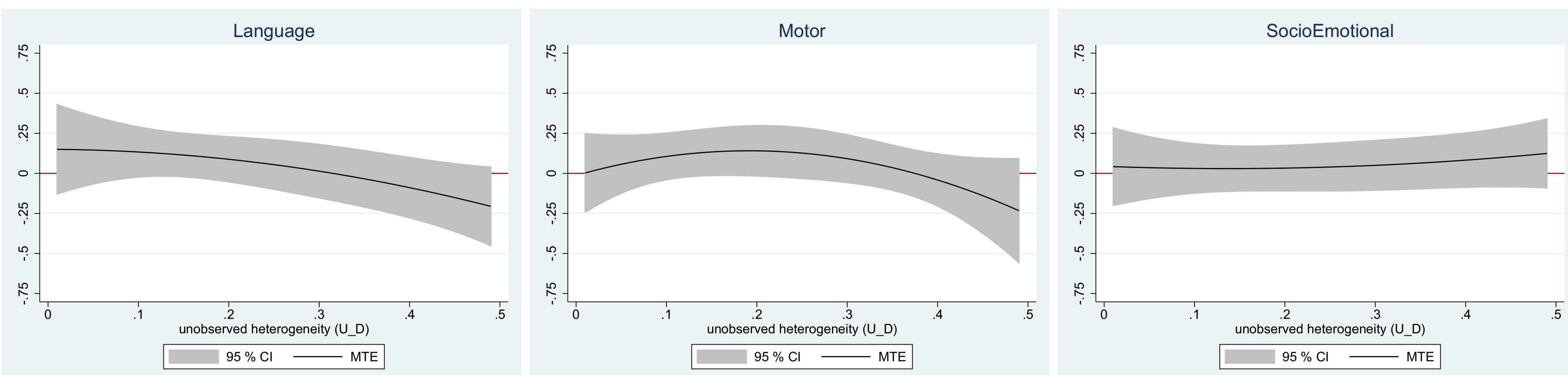

Fig. 6. ECC effects on child development for priority children. Note: These figures report the MTE of ECC attendance on the skill development of priority children – children who are given priority access to ECC either because they live with a single mother or because they already have one sibling in ECC. The underlying estimation includes child and family characteristics, cohort and district fixed effects, and county-cohort interactions. We use a linear model to estimate the propensity score with interactions between the districts and their respective post-expansion periods as instruments. "F-test polynomial pZ, p-value" reports the p-value of a test of joint significance of the second- and third-order propensity score polynomials. MTEs are thus identified from district * expansion period interactions, beginning in 2011 for fast-expansion districts and 2013 for slow-expansion districts. U_D is a child's latent propensity to attend ECC. Low values of U_D identify children with a low resistance to attend ECC. Standard errors are based on a non-parametric bootstrap with 100 replications.

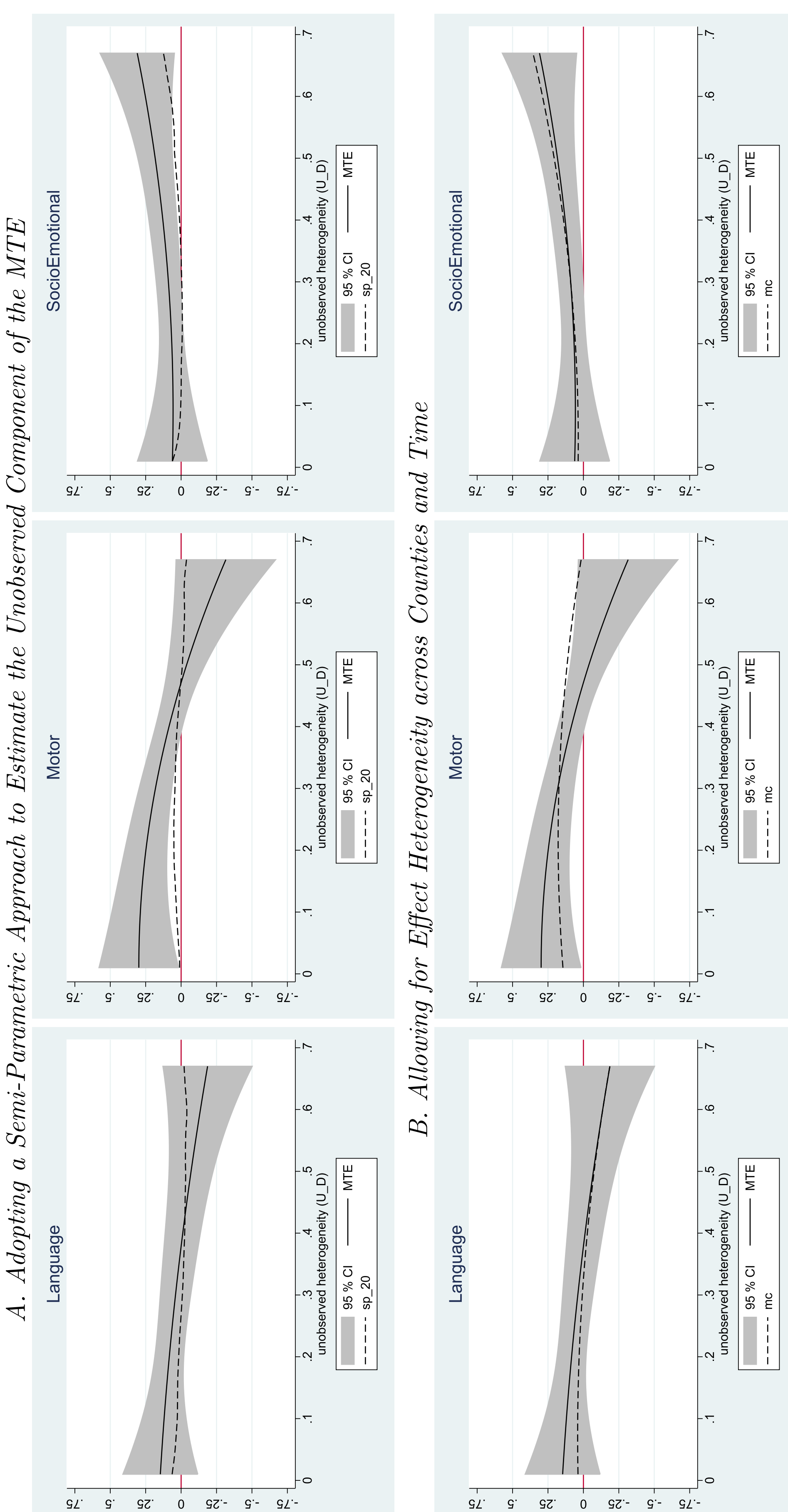

Fig. 7. Sensitivity: alternative specifications of the MTE. Note: Panel A shows MTE results when adopting a semi-parametric approach based on the partial linear model and obtaining the non-parametric slope estimates with locpoly2, with a bandwidth of 0.20 (indicated by the line "sp_20"). Panel B allows for further effect heterogeneity along an observed component, namely, counties and cohorts (indicated by the line "mc"). All figures also report the baseline specification. The sample is restricted to non-priority children (38,589 in total), that is, children who live with both parents and have at most one sibling.

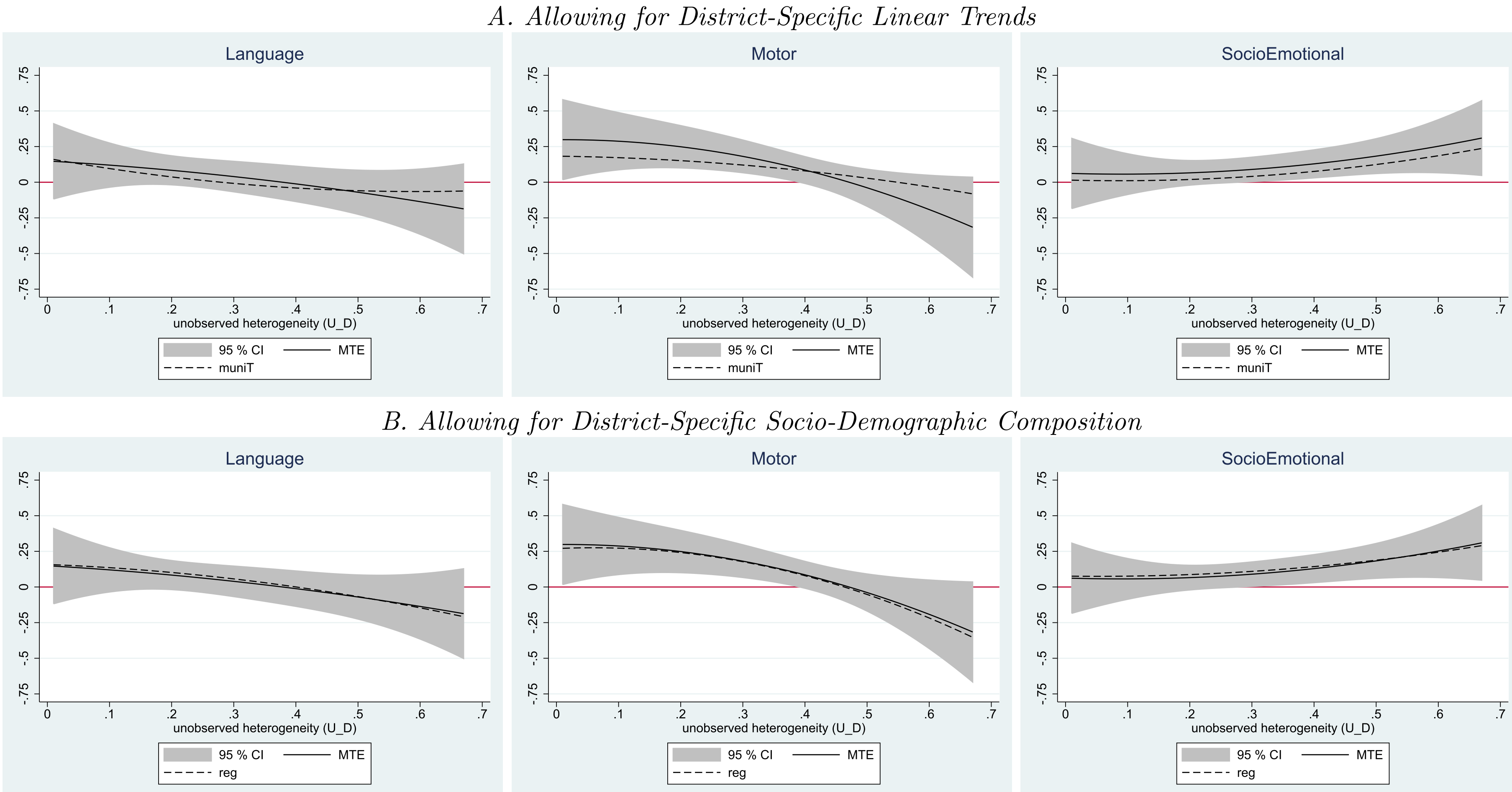

Fig. 8. Sensitivity: assessing parallel trends. Note: Panel A reports MTE results when including district-specific linear trends in both the first- and the second-stage equations (indicated by the line "muniT"). Panel B reports MTE results when adding school-district-level averages of child and family variables to the set of control variables in both the first- and second-stage equations (indicated by the line "reg"). The sample is restricted to non-priority children (38,589 in total), that is, children who live with both parents and have at most one sibling.

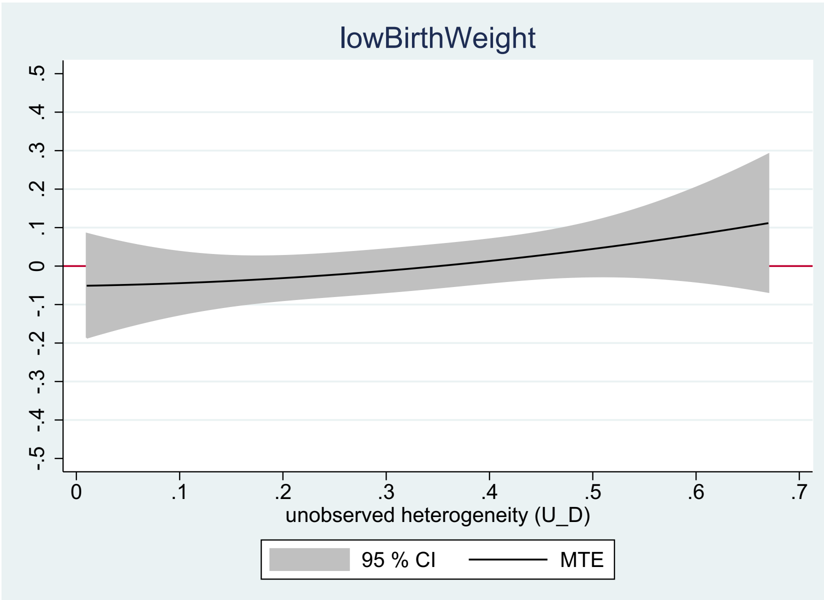

Fig. 9. Sensitivity: placebo – incidence of low birth weight. Note: The figure reports the MTE of ECC attendance on the incidence of birth weight. The underlying estimation includes child and family characteristics, cohort and district fixed effects, and county-cohort interactions. We use a linear model to estimate the propensity score with interactions between district dummies and their respective expansion periods as instruments. MTEs are thus identified from district * expansion period interactions, beginning in 2011 for fast-expansion districts and in 2013 for slow-expansion districts. U_D is the latent propensity to attend ECC. Low values of U_D indicate children with a low resistance to attend ECC. Standard errors are based on a non-parametric bootstrap with 100 replications. The sample is restricted to non-priority children (38,589 in total), that is, children who live with both parents and have at most one sibling.

Table 6
Policy-relevant treatment effects.

	Moderate reform		Progressive reform	
	7 to 27% (Existing supply)		27 to 50% (Beyond existing supply)	
Panel A. All				
Language	0.091	(0.070)	−0.010	(0.066)
Motor	0.258	(0.060)**	0.087	(0.052)*
Socio-emotional	0.065	(0.062)	0.129	(0.065)**
Panel B. Gender				
Boys				
Language	0.140	(0.071)**	0.012	(0.076)
Motor	0.285	(0.065)**	0.154	(0.059)**
Socio-emotional	0.096	(0.068)	0.169	(0.074)**
Girls				
Language	0.037	(0.075)	−0.034	(0.069)
Motor	0.230	(0.061)**	0.017	(0.053)
Socio-emotional	0.032	(0.063)	0.087	(0.065)
Panel C: Maternal education				
Less than college				
Language	0.104	(0.068)	0.050	(0.077)
Motor	0.241	(0.062)**	0.127	(0.058)**
Socio-emotional	0.055	(0.065)	0.157	(0.074)**
College or more				
Language	0.068	(0.078)	−0.119	(0.067)*
Motor	0.279	(0.064)**	0.014	(0.062)
Socio-emotional	0.076	(0.067)	0.071	(0.067)
Panel D: Migrant background				
Immigrant				
Language	0.147	(0.090)*	0.119	(0.110)
Motor	0.223	(0.076)**	0.107	(0.074)
Socio-emotional	0.069	(0.069)	0.179	(0.080)**
Native				
Language	0.072	(0.074)	−0.045	(0.065)
Motor	0.265	(0.059)**	0.080	(0.051)
Socio-emotional	0.061	(0.064)	0.113	(0.067)*

Notes: This table shows the average effects for all children attending ECC when average ECC attendance is expanded from 7 to 27% (modest reform corresponding to the average expansion observed between the earliest cohort (2009) and the latest cohort (2014) in our data) and from 27 to 50%, on average (a progressive reform). Standard errors are shown in parentheses. The sample is restricted to non-priority children (38,589 in total), that is, children who live with both parents and have at most one sibling.
Significance levels are indicated as follows: *0.01 percent,**0.05 percent and *0.1 percent

unobserved component of the MTE when allowing for district-specific cohort trends along with the baseline estimates and the associated 95% confidence interval. The results of this sensitivity analysis are similar to those of our baseline analysis, albeit somewhat weaker. Panel B reports results that control for the socio-demographic composition at the district level along with baseline estimates and the associated confidence interval. The results are remarkably similar to the baseline results, consistent with our earlier finding that the composition of districts evolved similarly across districts and were independent of the timing of the expansion. Fig. 9 reports the results of our placebo analysis, the "effects" of ECC attendance on the incidence of low birth weight. Birth weight is a central determinant of children's development (Currie and Moretti, 2007), but it is a pre-treatment outcome and thus by default not influenced by ECC. There is clearly no "effect" of ECC on attendance.

5.5. Policy simulations – heterogeneity

MTEs can be aggregated to simulate the effects of policy changes, the so-called policy-relevant treatment effects (PRTEs).[39] We discuss the average effects of two alternative policy reforms. The first expansion of ECC is the one that occurred between the first cohort (2009) and last cohort (2014) in our data. This reform shows the extent to which the expansion under study contributed to child development, an expansion of ECC attendance from 7% to 27% and hence a "modest" expansion. The second expansion simulates what would happen if all school districts had expanded ECC by as much as those at the 90th decile of the distribution of ECC expansions in our data (compare Fig. 3 for the distribution of ECC expansions observed in our data). This "progressive" reform implies that ECC attendance increases to 50%

[39] Consider expanding the supply of ECC to raise ECC attendance from a baseline level $\overline{p}$ to a new level $\overline{p}'$, where $\overline{p} = 1/N\sum_i p_i$ is the average propensity score under the baseline policy and $\overline{p}'$ is the propensity score under the new policy. The $PRTE[\overline{p}', \overline{p}]$ is the average treatment effect for children who attend ECC if $p(z_i) = p_i'$ but not if $p(z_i) = p_i$. We estimate the PRTE as follows

$$PRTE = \frac{1}{N}\sum_i \frac{(p_i' - p_i)}{\overline{p}' - \overline{p}} X_i' \beta + \sum_{u=1}^{100} E(U_1 - U_0 | U_D = u/100)\left(\frac{Pr(p' > u/100) - Pr(p > u/100)}{(\overline{p}' - \overline{p})100}\right)$$

where β and $E(U_1 - U_0|U_D) = \partial K(p(Z))/\partial p(Z)$ are estimated.

from the current 27%. Note that the PRTEs are the result of an extrapolation based on estimated MTEs without considering possible general equilibrium effects or financing issues.

Table 6, Panel A displays the effects of the alternative reforms on the average non-priority child attending ECC only when new slots are created. Children who are served first when new slots are opened benefit in terms of their motor skill development, while children who are served only when sufficient ECC slots are available reap the greatest benefits in terms of their socio-emotional skill development. Centers have clear guidelines to develop motor skills and may promote motor skill development as well as parents do. In contrast, socio-emotional skills are more difficult to target, and parents may not adequately take socio-emotional skills into account when deciding whether to send their child to ECC.

Whether ECC helps to close development gaps between children belonging to disadvantaged groups is of central policy interest. The following subgroups are the target of frequent discussions of academic scholars and politicians alike: boys and girls and children from advantaged and disadvantaged backgrounds, including having low-educated parents and migrant backgrounds. We therefore stratify our analysis along these lines and estimate MTEs separately for boys and girls, children with a college-educated mother and those without, and children with and without migrant ancestry.[40] Here, we discuss the PRTEs obtained for the different subgroups. The underlying MTEs are shown in Fig. A1 in the Appendix.

Panel B of Table 6 shows results by gender. Boys benefit from attending ECC across all development dimensions. The effects on boys' motor skills are particularly sizable and observed independent of the specific policy change: a moderate reform leads to an improvement in motor skills for approximately 29 out of 100 boys, while a progressive reform still helps 15 out of 100 boys to improve their motor skills. A moderate reform is also beneficial for the language skill development of 14 out of 100 boys, while a progressive reform is beneficial for the socio-emotional skill development of 17 out of 100 boys. Girls, in contrast, benefit only in terms of their motor skill development: a moderate reform improves motor skills for 23 out of 100 girls. This effect, however, vanishes once ECC supply is expanded further. The differential patterns by gender suggest that boys are more sensitive to external stimuli than girls are. Girls appear to lose out more than boys when forgoing important one-on-one time with their parents (Cisbra and Gergely, 2009, 2011) – a result reflected by the absence of any gains for girls' socio-emotional skill development and largely in line with the results of a recent study by Fort et al. (2017).

Panel C of Table 6 shows PRTEs when stratifying the analysis by mothers' education level. Expanding ECC implies substantial gains for the development of children from a less-educated family background. Specifically, a moderate reform stimulates the motor skill development of approximately 24 out of 100 children with a low-educated mother, while 13 benefit from a progressive reform. A similar pattern is observed for the motor skill development of children with a high-educated mother. The estimated effects for this subgroup are somewhat stronger – a moderate reform leads to an improvement in motor skill development for 28 out of 100 children but decrease quickly, as a progressive reform does not entail any significant benefits for the motor skills of children with a high-educated mother. The differences between children with and those without a college-educated mother are more striking in terms of the effects on children's socio-emotional skill development. While there are no significant effects for the socio-emotional skill development of children with a high-educated mother, expanding ECC entails sizable effects for the socio-emotional skill development of children with a low-educated mother: approximately 16 children out of 100 children with a low-educated mother improve their socio-emotional skills as a result of a progressive reform.

The differential pattern observed for children from different education backgrounds is in line with the arguments outlined above: low-educated mothers, particularly those who are rather undecided regarding whether to send their children to ECC, may underestimate the relevance of peer contacts for their children's socio-emotional development and thus ultimately fail to sufficiently foster their children's exposure to peers if they do not send them to ECC. High-educated mothers, in contrast, may well be aware of the relevance of peer contacts and thus may reach out and meet other families on a frequent basis if they do not send their children to ECC (Hsin and Felfe, 2014). A progressive reform, however, harms the language skill development of children from more-educated families. This result highlights the relevance of the time spent one-on-one with parents, particularly in the case of college-educated mothers. In fact, one reason for college-educated mothers not to send their children to ECC includes their children experiencing developmental deficits and mothers' beliefs in making up for such deficits created by the staff in center-based care.

Panel D of Table 6 reports results by migrant ancestry. A modest reform boosts the language skill development of 15 of 100 immigrant children, while a progressive reform still fosters the language skill development of 12 of 100 immigrant children. These effects, even if partially lacking significance,[41] highlight that ECC contributes to the integration of immigrant children – a result in line with policy makers' intention. Moreover, a progressive reform leads to an improvement in the socio-emotional skill development of 18 of 100 immigrant children. This effect is likely due to native peers conveying important information about the host country's social and cultural capital. Interestingly, a progressive reform also fosters the socio-emotional skill development of native children, but to a lesser extent (11 of 100 native children benefit from a progressive reform in terms of their socio-emotional skill development). A moderate reform improves the motor skill development of both immigrant and native children, with approximately 22 of 100 immigrant children and 27 of 100 native children having fewer problems with their motor skill development when attending ECC. The effects, however, vanish once ECC supply exceeds the stated demand.

6. Conclusions

We assess the effects of center-based care offered to children aged 0–2 on children's development in West Germany, a context where the supply of ECC centers was rationed using waiting lists. We adopt an MTE approach that provides us with estimates of how the effects of ECC vary along observed characteristics and children's unobserved latent propensity to attend ECC. Understanding effect heterogeneity is essential because ECC may both help and harm children.

Our identification strategy relies on a recent reform in Germany that triggered a substantial expansion of ECC slots. We exploit the differential timing in the implementation of the required expansion, with some districts expanding their ECC supply immediately after the reform was announced and other districts following soon thereafter. Districts offer the same amount of ECC at the beginning and the end of our sample period, suggesting that the two groups of districts differ only in their ability to immediately react to the reform but are otherwise comparable. Based on this observation, we develop an empirical strategy that exploits plausibly exogenous within-district variation in ECC attendance.

We find, first, that ECC fosters the development of boys more strongly than that of girls. As a result, boys catch up with girls in terms

[40] Importantly, these estimations not only address heterogeneity in the MTEs across certain child and family background characteristics but also relax, to some extent, the restrictive assumption that the unobserved component $K_X()$ does not vary with X, and thus, $K_X() = K()$. In other words, for each comparison, we estimate a separate polynomial for the mutually exclusive groups, e.g., for girls, $K_G()$, and boys, $K_B()$.

[41] The lack of significance may be partially due to the fact that our measure of language skills does not strictly measure language proficiency but rather articulation and comprehension problems.

of their language, motor and socio-emotional skill development. Second, ECC fosters the integration of immigrant children who are likely to be sent to ECC; in particular, ECC attendance helps immigrant children catch up in terms of their language skills. Immigrant children who are sent to ECC only once sufficient slots are available benefit in terms of their socio-emotional skill development. This effect is also true for children from a low-educated family background and thus highlights the relevance of social interactions, which are readily available in the center but potentially less so at home.

Our findings show that ECC enables disadvantaged children to catch up with their peers. Expansions of ECC could thus help "[level] the playing field" (Havnes and Mogstad, 2011). However, the effects of progressive expansions of ECC are somewhat disappointing. Skills that feature in the curriculum of every center – language skills and motor skills – develop less strongly among children who join ECC only when sufficient slots are available. On the bright side, however, children entering ECC after a progressive expansion reap of the strongest gains in terms of their socio-emotional maturity, a skill dimension that could be important for later life success. As such, evaluations of ECC should take into account a large array of skills, including skill dimensions that lie beyond the curriculum.

Appendix A

Table A1
Baseline MTE parameter estimates – full results.

	Language	Motor	Socio-emotional
pZ	0.149	0.298**	0.063
	(0.143)	(0.149)	(0.133)
pZ^2	−0.127	0.019	−0.068
	(0.439)	(0.388)	(0.395)
pZ^3	−0.123	−0.474	0.251
	(0.405)	(0.390)	(0.364)
pZ * Child age	−0.006	−0.005	0.008*
	(0.005)	(0.004)	(0.004)
pZ * Boy	0.065**	0.054*	0.061**
	(0.026)	(0.029)	(0.025)
pZ * Foreign	0.053	−0.044	0.034
	(0.042)	(0.031)	(0.033)
pZ * Tertiary education	−0.027	0.072**	−0.023
	(0.033)	(0.034)	(0.028)
Child age (months)	0.011***	0.013***	0.005***
	(0.001)	(0.001)	(0.001)
Boy (D)	−0.096***	−0.169***	−0.107***
	(0.005)	(0.006)	(0.006)
Foreign (D)	−0.143***	0.019**	0.008
	(0.009)	(0.008)	(0.008)
Mother tertiary education (D)	0.080***	0.024***	0.060***
	(0.008)	(0.008)	(0.007)
Cohort 2010 (D)	0.031	0.018	0.058
	(0.044)	(0.034)	(0.037)
Cohort 2011 (D)	−0.013	−0.027	0.073*
	(0.045)	(0.038)	(0.040)
Cohort 2012 (D)	0.019	−0.088**	0.092**
	(0.042)	(0.038)	(0.037)
Cohort 2013 (D)	0.027	−0.071*	0.107***
	(0.038)	(0.040)	(0.040)
Cohort 2014 (D)	0.018	0.028	0.125*
	(0.086)	(0.055)	(0.067)
Constant	−0.033	−0.084	0.486***
	(0.089)	(0.089)	(0.087)
District dummies	Yes	Yes	Yes
Country * Cohort dummies	Yes	Yes	Yes
F-test interactions pZ * (Child-Family), p-value	0.043	0.141	0.052
F-test polynomial pZ, p-value	0.274	0.059	0.213
R-squared	0.102	0.085	0.091
Children	38,589	38,589	38,589

Notes: The table reports the full set of results when estimating Eq. (9). "F-test interactions pZ * (Child-Family), p-value" reports the p-value of a test of joint significance of all interactions between the propensity score and the child or family characteristics. "F-test polynomial pZ, p-value" reports the p-value of a test of joint significance of the second- and third-order propensity score polynomials. Tests and standard errors are based on non-parametric bootstraps with 100 replications. The sample is restricted to non-priority children (38,589 in total), that is, children who live with both parents and have at most one sibling.
Significance levels are indicated as follows: *0.01 percent,**0.05 percent and *0.1 percent

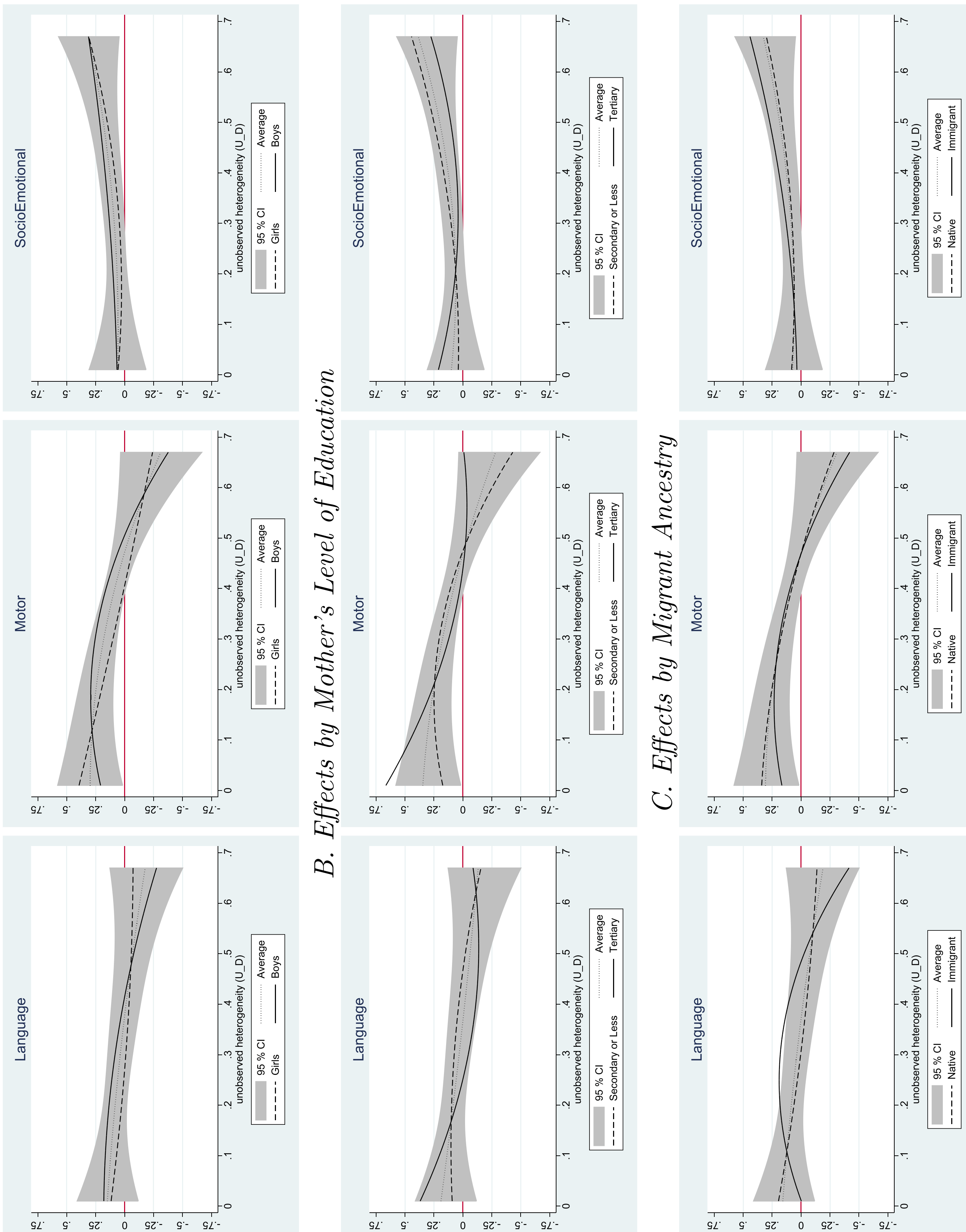
Language
Motor
SocioEmotional
95 % CI
Average
Girls
Boys
unobserved heterogeneity (U_D)
B. Effects by Mother's Level of Education
Secondary or Less
Tertiary
C. Effects by Migrant Ancestry
Native
Immigrant
.75
.5
.25
0
-.25
-.5
-.75
0
.1
.2
.3
.4
.5
.6
.7

Fig. A1. Heterogeneity analysis. Note: This figure reports the results when estimating the child development Eq. (9) while allowing the unobserved component of the MTE – the function *K*– to differ between boys and girls (Panel A), children whose mother completed college and those whose mother did not (Panel B) and children with migrant ancestry and those without (Panel C). All subfigures display the baseline estimate and the corresponding 95% confidence interval. The sample is restricted to *priority* children (38,589 in total), that is, children who live with both parents and have at most one sibling. MTEs are statistically different for boys and girls (at the 5% significance level) and for the motor and socio-emotional skill development of children whose mothers have completed college education and those whose mothers have not (at the 10% level of significance). The differences are not significant between children with immigrant ancestry and those without. The sample is restricted to *non-priority* children (38,589 in total), that is, children who live with both parents and have at most one sibling.

References

Angrist, J., Krueger, A.B., 1991. Does compulsory school attendance affect schooling and earnings? Q. J. Econ. 106 (4), 979–1014.

Baker, M., Gruber, J., Milligan, K., 2008. Universal child care, maternal labor supply, and family well-being. J. Polit. Econ. 116 (4), 709–745.

Baker, M., Milligan, K., 2012. Maternity leave and children's cognitive and behavioral development. In: NBER Working Paper. NBER.

Bauernschuster, S., Hener, T., Rainer, H., 2016. Children of a (policy) revolution: the introduction of universal child care and its effect on fertility. J. Eur. Econ. Assoc. 14 (4), 975–1005.

Bauernschuster, S., Schlotter, M., 2015. Public child care and mothers' labor supply - evidence from two quasi-experiments. J. Public Econ. 123 (469-505), 1–16.

Baum, C., 2003. Does early maternal employment harm child development? An analysis of the potential benefits of leave taking. J. Labor Econ. 21 (2).

Berlinski, S., Galiani, S., Gertler, P., 2009, February, February. The effect of pre-primary education on primary school performance. J. Public Econ. 93 (1-2), 219–234.

Bien, W., Rauschenbach, T., Riedel, B., 2006. Wer betreut deutschlands kinder? dji-kinderbetreuungsstudie. In: Technical report. Weinheim/Basel.

Bitler, M., Hoynes, H., Domina, T., 2013. Experimental evidence on distributional effects of head start. In: Technical report. UC Berkeley.

Bjorklund, A., Moffitt, R., 1987, February, February. The estimation of wage gains and welfare gains in self-selection. Rev. Econ. Stat. 69 (1), 42–49.

Blau, F., Grossberg, A., 1992. Maternal labor supply and children's cognitive development. Rev. Econ. Stat. 77, 231–249.

BMFSFJ, 2012. Dritter zwischenbericht zur evaluation des kinderfrderungsgesetzes. In: Technical report. mimeo.

Brinch, C.N., Mogstad, M., Wiswall, M., 2017. Beyond late with a discrete instrument. J. Polit. Econ. 125 (4), 985–1039.

Brooks-Gunn, J., Han, W.-J., Waldfogel, J., 2002. The effects of early maternal employment on child cognitive development. Demography 39 (2), 369–392.

Carneiro, P., Heckman, J., Vytlacil, E., 2011, October, October. Estimating marginal returns to education. Am. Econ. Rev. 101, 2754–2781.

Carneiro, P., Loken, K., Salvanes, K.G., 2014. A flying start? Long term consequences of time investments in infants in their first year of life. J. Polit. Econ. 123, 365–412.

Cascio, E., 2009, May, May. Do investments in universal early education pay off? Long-term effects of introducing kindergartens into public schools. In: Working Paper 14951. NBER.

Cisbra, G., Gergely, G., 2009. Natural pedagogy. Philos. Trans. R. Soc. B 13, 143–153.

Cisbra, G., Gergely, G., 2011. Natural pedagogy as evolutionary adaptation. Trends Cogn. Sci. 366, 1149–1157.

Cornelissen, T., Dustmann, C., Raute, A., Schönberg, U., 2016a. From LATE to MTE: alternative methods for the evaluation of policy interventions. Labour Econ. 41 (C), 47–60.

Cornelissen, T., Dustmann, C., Raute, A., Schönberg, U., 2016b. Who benefits from universal child care? estimating marginal returns to early child care attendance. In: Technical report. mimeo.

Currie, J., Moretti, E., 2007. Biology as destiny? Short and long-run determinants of intergenerational transmission of birth weight. J. Labor Econ. 25 (2), 231–264.

Danzer, N., Lavy, V., 2016. Parental leave and children's schooling outcomes: quasi-experimental evidence from a large parental leave reform. Econ. J (forthcoming).

Datta-Gupta, N., Simonsen, M., 2010. Non-cognitive child outcomes and universal high quality child care. J. Public Econ. 94 (1-2), 30–43.

Doyle Jr., J.J., 2007. Child protection and child outcomes: measuring the effects of foster care. Am. Econ. Rev. 97 (5), 1583–1610.

Drange, N., Havnes, T., 2014. Early and bright? Child care for toddlers and early cognitive skills. In: Technical report. mimeo.

Duncan, G., Dowsett, C., Claessens, A., Magnuson, K., Huston, A., Klebanov, P., Pagani, L., Feinstein, L., Engel, M., Brooks-Gunn, J., Sexton, H., Duckworth, K., Japel, C., 2007. School readiness and later achievement. Dev. Psychol. 43 (6), 1428–1446.

Dustmann, C., Schoenberg, U., 2012. The effect of expansions in leave coverage on children's long-term outcomes. Am. Econ. J. Appl. Econ. 4, 190–224.

Eberwein, C., Ham, J.C., Lalonde, R.J., 1997. The impact of being offered and receiving classroom training on the employment histories of disadvantaged women: evidence from experimental data. Rev. Econ. Stud. 64 (4), 655–682.

Eckhoff Andresen, M., 2016. Who benefits from early care? Effects on test scores under essential heterogeneity. Mimeo, Oslo University.

Felfe, C., Lalive, R., 2012. Early child care and child development: for whom it works and why. In: Discussion Paper 7100. IZA.

Felfe, C., Lalive, R., 2014. Does early child care help or hurt childrens's development? In: Working Paper Series 2014:22. IFAU - Institute for Evaluation of Labour Market and Education Policy.

Felfe, C., Nollenberger, N., Rodriguez-Planas, N., 2015. Can't buy mommy's love? universal child care and children's long-run cognitive development. J. Popul. Econ. 26 (3), 983–1005.

Felfe, C., Stern, S., 2015. Krippenkosten und -finanzierung im internationalen vergleich. Soz. Sicherh. CHSS 05.

Fitzpatrick, M.D., 2008. Starting school at four: the effect of universal pre-kindergarten on children's academic achievement. B.E. J. Econ. Anal. Policy 8 (1), 46.

Fort, M., Ichino, A., Zanella, G., 2017. The cognitive cost of daycar 0–2 for children in advantaged familis. In: Technical report. mimeo.

French, E., Song, J., 2014. The effect of disability insurance receipt on labor supply. Am. Econ. J. Econ. Policy 6 (2), 291–337.

Goodman, R.N., 1997. The strengths and difficulties questionnaire: a research note. J. J. Child Psychol. Psychiatry (38), 581–586.

Gormley Jr., W.T., Phillips, D., Gayer, T., 2008. THE EARLY YEARS: preschool programs can boost school readiness. Science 320 (5884), 1723–1724.
Gregg, P., Machin, S., 1999. Childhood disadvantage and success or failure in the labour market. In: Blanchflower, D., Freeman, R. (Eds.), Youth Employment and Joblessness in Advanced Countries. National Bureau of Economic Research, Cambridge, MA.
Gregg, P., Machin, S., 2001. The relationship between childhood experiences, subsequent educational attainment and adult labour market performance. In: Vleminckx, K., Smeeding, T. (Eds.), Child Well Being in Modern Nations: What do we Know?.
Grissmer, D., Grimm, K., Aiyer, S., Murrah, W., Steele, J., 2010. Fine motor skills and early comprehension of the world: two new school readiness indicators. Dev. Psychol. 46 (5), 1008–1017.
Havnes, T., Mogstad, M., 2011. No child left behind: universal child care and children's long-run outcomes. Am. Econ. J. Econ. Pol. 3 (2), 97–129.
Havnes, T., Mogstad, M., 2015. Is universal child care leveling the playing field? Evidence from non-linear difference-in-differences. J. Public Econ. 127, 100–114.
Heckman, J., Urzua, S., Vytlacil, E., 2006. Understanding instrumental variables in models with essential heterogeneity. Rev. Econ. Stat. 88 (3), 389–432.
Heckman, J., Vytlacil, E., 1999. Local instrumental variables and latent variable models for identifying and bounding treatment effects. Proc. Natl. Acad. Sci. 96 (8), 4730–4734.
Heckman, J., Vytlacil, E., 2001. Policy relevant treatment effects. Am. Econ. Rev. 91 (2), 107–111.
Heckman, J., Vytlacil, E., 2005. Structural equation, treatment effects, and econometric policy evaluation. Econometrica 73 (3), 669–738.
Herbst, C., 2013. The impact of non-parental child care on child development: evidence from the summer participation dip. J. Public Econ. 105, 86–105.
Hsin, A., Felfe, C., 2014. When does time matter? Maternal employment and children's time with parents and child development. Demography 51 (5), 1867–1894.
Huesken, K., 2010. Kita vor Ort: Betreuungsatlas auf Eene der Jugendamtsbezirke. Deutsches Jugendinstitut e.v.
James-Burdumy, S., 2005. The effect of maternal labor force participation on child development. J. Labor Econ. 23 (1), 177–211.
Kline, P., Walters, C.R., 2016. Evaluating public programs with close substitutes: the case of head start. Q. J. Econ. 131 (4), 1795–1848.
Ladd, G., 2005. Children's Peer Relations and Social Competence: A Century of Progress (Current Perspectives in Psychology). Yale University Press, New Haven.
Liu, Q., Skans, O., 2010. The duration of paid parental leave and children's scholastic performance. B.E. J. Econ. Anal. Policy (Contributions) 10 (1), 3.
Maestas, N., Mullen, K.J., Strand, A., 2013. Does disability insurance receipt discourage work? Using examiner assignment to estimate causal effects of ssdi receipt. Am. Econ. Rev. 103 (5), 1797–1829.
Magnuson, K.A., Ruhm, C.J., Waldfogel, J., 2007. Does prekindergarten improve school preparation and performance? Econ. Educ. Rev. 26, 33–51.
Noboa Hidalgo, G., Urzúa, S., 2012. The effect of participation in public childcare centers: evidence from Chile. J. Hum. Cap. 6 (1).
Ruhm, C., 2004. Parental employment and child cognitive development. J. Hum. Resour. 39 (1), 155–192.
Schoenberg, U., Ludsteck, J., 2014. Maternity leave legislation, female labor supply and the family wage gap. J. Labor Econ. 32 (469-505).
Skeels, C., Windmeijer, F., 2016. On the stock-yogo tables. In: Bristol Economics Discussion Papers. Department of Economics, University of Bristol, UK.
Stock, J., Yogo, M., 2005. Testing for Weak Instruments in Linear IV Regression. Cambridge University Press, New York, pp. 80–108.
Wuertz-Rasmussen, A., 2010. Increasing the length of parents' birth-related leave: the effect on children's long-term educational outcomes. Labour Econ. 17 (1), 91–100.