Does Universal Child Care Matter?
Evidence from a Large Expansion in Pre-School Education

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This Draft: March 1st, 2013

Abstract

This paper studies the effect of a German universal child care program (aimed at 3- to 6-year-olds) on school readiness indicators. We draw on unique administrative data for the entire population of children who are about to start school in one large region. Our identification strategy exploits a federal policy reform which entitles every child to a child care slot from her third birthday to school entry, leading to a staggered expansion in child care facilities across municipalities. We find that longer public child care attendance strongly and robustly reduces language and motor skill problems and improves overall school readiness for children of immigrant ancestry (i.e., children who themselves or whose parents were born outside Germany), but has no significant effects for children of native ancestry. We offer three explanations for these findings. First, non-linear returns to the length of attendance may lead to higher overall effects for children of immigrant ancestry, as the reform had a differential impact on the child care starting age for children of native ancestry. Second, the two groups of children differ with respect to the counterfactual care arrangements. Third, due to their lack of proficiency in the host country language, children of immigrant ancestry may benefit more from public child care and exposure to other children. Our findings suggest that universal child care programs help to narrow the achievement gap between children of immigrant and native ancestry, potentially reducing life-time inequalities between these two groups.

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1 Introduction

Pre-school and early childhood programmes are often thought to be an important and effective way to influence the development of children (see e.g. Ruhm and Waldfogel, 2011 and Almond and Currie, 2011 for excellent recent surveys). Not only is learning in early childhood particularly productive because of the longer pay-off period (Becker, 1964) and because many skills are best learnt when young (e.g. Shonkoff and Phillips, 2000), but there may also be important "dynamic complementarities" of early learning with acquisition of human capital at later stages (Heckman, 2007 and Cunha and Heckman, 2007). While publicly-provided, universal pre-school programs aimed at promoting the child's social and cognitive development exist in most European countries, including the UK, France, Germany, and all Nordic countries, they are typically not provided in the US. An important goal of the current Obama administration, as stated in the Zero to Five plan, is to create universal, state-funded, pre-school programs similar to those in Europe. Despite the enormous policy interest, evidence on the effectiveness of such programs is, however, still scarce.

In this paper, we study the effects of a universal child care program that is aimed at 3- to 6-year-olds for the case of Germany. The German universal child care program is similar to that in other countries in that it is a half-day program with strict nation-wide quality standards, where learning is mostly informal and play-oriented and carried out in the context of day-to-day social interactions between children and teachers. To measure the impact of exposure to that program, we make use of a unique feature of the German school system, where all children have to undergo an extensive battery of school readiness tests, administered by pediatricians, before entering elementary school. This provides us with rare and detailed administrative data on a series of school readiness indicators, such as fine or gross motor skill problems or language deficits, for the entire population of children who are about to start school in one large region. These indicators are, as recent research shows (Grissmer et al., 2010), stronger predictors for later academic success than early cognitive test scores, which are typically used in the literature (see Section 2 for a review). An important additional advantage of our data is that

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1 Such programs (often referred to as pre-K) currently exist in Georgia, Florida, New Jersey, New York and Oklahoma.
2 See Section 2 for a detailed review of the literature.
our outcome variables are standardized assessments by health professionals as opposed to subjective assessments by parents commonly used in the literature.

To deal with the non-random sorting of children into public child care, we make use of a federal policy reform which entitles every child to a largely subsidized 4-hour child care slot from her third birthday to school entry. In the region we study, this mandate lead to a staggered timing and intensity of the construction of child care facilities across municipalities—which is the variation we exploit in our instrumental variable estimation.

An important contribution of our work is to contrast the effects of time spent in public child care for two groups of children: children of native ancestry, and children of immigrant ancestry, defined as children who themselves or whose parents were born outside Germany. Children of immigrant ancestry are more likely to come from a disadvantaged background and to perform significantly worse at school than children of native ancestry (see e.g., Dustmann et al., 2012), and often do not speak German at home. We find very different effects for these two groups of children: Whereas longer public child care attendance strongly and robustly reduces language and motor skill problems and generally improves school readiness for immigrant children, it has no significant effects on school readiness outcomes for native children.

We offer three explanations for these differential effects. First, they might be driven by non-linear returns to public child care attendance with respect to age or length of attendance. Prior to the expansion in child care facilities, the typical child of native ancestry entered public child care at the age of 4 and thus attended public child care for two years before starting school at the age of 6. For these children, the expansion in child care facilities therefore primarily increased the share of children who enrol in public child care at the age of 3 and thus attend public child care for three years. In contrast, prior to the expansion in child care facilities, many children of immigrant ancestry attended public child care for only one year or not at all. For these children, the expansion in child care facilities therefore

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3 For simplicity, we also refer to the first group as "native children" and to the second group as "immigrant children".  
4 See e.g. Casey and Dustmann (2008) who report significant language deficiencies for the largest traditional immigrant communities in Germany (i.e., Turkey, Spain, Italy, former Yugoslavia and Greece).
increased both the 1-, 2-, and 3-year attendance rate. At the same time, we find some suggestive evidence that for immigrant children the first year in public child care (when children are between 4 or 5 years old) improves proficiency in the host country language and overall school readiness by more than the third year in public child care (when children are 3 years old).

Second, the larger benefits of public child care attendance for immigrant than native children may be explained by differences in the counterfactual child care arrangement. Our findings suggest that for children of immigrant ancestry public child care attendance primarily crowds out maternal care while for children of native ancestry it primarily replaces alternative child care like that by a child minder—which is likely to be more similar to public child care than maternal care, in particular with respect to the exposure to other children.

Third, immigrant children are, due to a lack of proficiency in the host country language, likely to be particularly perceptive to the informal and play-oriented learning environment which public child care provides, especially if the counterfactual care arrangement is the home environment. Host country language proficiency has been shown to be an important determinant of success not only in elementary school (see e.g., Dustmann, Machin, and Schönberg, 2010, Bleakley and Chin, 2008) but also for other aspects of life (see e.g., Bleakley and Chin 2010 on social assimilation). The exposure to other children in public child care is likely to quickly reduce this deficit. As host country language proficiency may also be complementary to learning in general, it might lead to positive spillover effects, thus explaining the positive effects of child care attendance on other measures of early achievement.

Our findings thus emphasize that the benefits of large scale, publicly provided child care programs are far from uniform. In our study, they are concentrated among a specific group of disadvantaged children, suggesting that universal child care programs are successful at narrowing the achievement gap between children of native and immigrant ancestry at school entry (and possibly beyond), and hence may help children of immigrant ancestry to assimilate into the host society. Our findings further highlight that the welfare effects of universal child care programs crucially depend on which children the program draws into child care, which type of child care arrangement public child care replaces, and
which margins of the child care attendance distribution (i.e., the one-, two-, or three-year attendance rates) are affected.

The structure of the paper is as follows. In the next section, we briefly discuss the existing literature, and how our paper contributes to it. In Section 3.1, we illustrate the possible channels through which changes in child care supply may affect child outcomes. We then outline the German system of public child care and the child care reform that we analyze (Sections 3.2 and 3.4). In Section 4, we describe our data and provide a descriptive analysis on the differences in outcomes between children of immigrant and native ancestry and the potential selection into child care. We then discuss our identification strategy and investigate in detail the validity of the assumptions we impose (Section 5). We report results in Section 6 and conclude with a brief discussion of our results in Section 7.

2 Contribution to Existing Literature

There is a large literature which evaluates early intervention programs, such as Head Start or the Perry Preschool Project, that specifically target children from disadvantaged backgrounds and combine a variety of different services (see e.g. Blau and Currie, 2006, for a survey). This literature finds strong positive effects of these programs in the short-run, and conflicting evidence in the longer run.\(^5\)

Less is known, however, about the benefits of large scale, publicly provided universal child care programs targeted at all children—which is the focus of this paper (see Baker, 2011 for a comprehensive survey).\(^6\) Much of this literature has found that such programs have positive effects on children. For

\(^5\)According to The Head Start Impact Study, Head Start improved cognitive and non-cognitive achievement measures of its participants in the short-term, but the strong improvements however quickly dissipate. Garces, Thomas and Currie (2002), however, find that participation in Head Start increases high school completion and college enrolment among whites. Deming (2009) finds large long-term gains of Head Start participation for disadvantaged children despite the fading-out of test score gains. Using a regression discontinuity design, Carneiro and Ginja (2012) show that participation in Head Start reduces the incidence of behavioral problems, serious health issues and obesity for adolescents and criminal activity among young adults. Re-evaluating the Perry Preschool Project, Heckman, Moon, Pinto, Savelyev and Yavitz (2010a,b) find strong positive effects of the program that last well into adulthood.

\(^6\)Related research evaluates the effects of attending center-based care that is typically privately provided, with a large variance in quality. For instance, Loeb, Bridges, Bassok, Fuller, and Rumberger (2007) and Magnuson, Lahaie, and Waldfogel (2007) document a positive association between attending center-based care and short-run cognitive test scores, but a negative association between attending center-based care and behavioral outcomes. To deal with the endogenous selection into center-based care, Bernal and Keane (2011) exploit exogenous variation in welfare policy rules, and find—contrary to the observational studies described above—that an additional year in child care reduces child test scores.
instance, Berlinski, Galiani and Gertler (2009) find positive effects of an expansion in pre-elementary education in Argentina on performance in elementary school. Similarly, Gormley and Gayer (2005) show a positive effect of a prekindergarten-program for 4–5 year-olds in the U.S. on cognitive, motor and language skills measured shortly after the program. There is also some evidence on longer term effects: Havnes and Mogstad (2011a) find that an expansion of subsidized child care for 3–6 year-olds in Norway had strong positive effects on long-run adult outcomes, such as educational attainment and labor market participation. Berlinski, Galiani and Manacorda (2008) also report positive effects of a rapid expansion of pre-school child care facilities in Uruguay on completed education, while Cascio (2009a), exploiting variation in the introduction of Kindergarten programs for 5-year-olds into public schools across different states in the U.S., finds a decrease in high school drop-out rates and institutionalization rates as adults for white, but not for black children. Evidence of positive effects is not unequivocal, however: Baker, Gruber and Milligan (2008) study the introduction of highly-subsidized universal child care in Quebec and find negative short-run effects on behavioral and health outcomes, while Datta Gupta and Simonsen (2010) show that enrollment in pre-school in Denmark does not improve child outcomes at the age of seven. In a similar vain, Drange, Havnes and Sandsor (2012) find that mandating kindergarten for 5-6 year-olds in Norway had little impact on school performance at the end of compulsory schooling.

We first add to this literature by focusing on a particularly interesting group of children for which the benefits of public child care may be high: children of immigrant ancestry who themselves or whose parents were born outside Germany. While several studies analyze the differential effects of universal or targeted child care programs for disadvantaged and advantaged children or across racial and ethnic groups7, children of immigrant ancestry—who often do not speak the language of the host country at home—have received relatively little attention, despite their growing share in the population.8

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7For instance, Havnes and Mogstad (2011a) find larger long-term benefits of public child care attendance for children of low-educated parents. Gormley and Gayer (2005) show that Hispanics experienced the biggest effects on cognitive, motor skills and language scores followed by blacks, whereas the effects for whites were mostly small and statistically insignificant. Evaluating an intervention program targeted at disadvantaged children (Head Start), Currie and Thomas (1995, 1999) show that Hispanic and white children benefit more from the program with respect to schooling outcomes than black children.

8Studies which specifically report findings for immigrant children include Magnuson, Lahaie and Waldfogel (2006) who show that participation in head start is associated with higher gains for children of low educated immigrant mothers than for children of low-educated non-immigrant mothers, and Gormley (2008) who finds that a pre-kindergarten program in
We further contribute to the literature by investigating possible non-linearities of the effects of child care attendance with respect to years or age of attendance. Most existing studies which evaluate an expansion of a universal child care program (e.g., Berlinski, Galiani and Gertler, 2009, Havnes and Mogstad, 2011a, and Baker, Gruber and Milligan, 2008) estimate linear models which restrict the impact of child care attendance to be linear in child care duration and to be homogenous by age. We show that these studies implicitly identify a weighted average of the duration- or age-specific marginal treatment effects (see Section 6.4). The very few studies which explicitly analyze the non-linear impact of child care attendance on child outcomes (albeit not in the context of an exogenous expansion in child care coverage) reach conflicting conclusions. While Berlinski, Galiani and Manocorda (2008) find that a preschool program in Uruguay positively affects school attainment primarily at the extensive margin, with small and statistically insignificant effects at the intensive margin, Behrman, Cheng and Todd (2004) report that a Bolivian Preschool program boosted child outcomes only for children with at least seven months of exposure, and find increasing marginal impacts with longer program attendance.\(^9\) We find suggestive evidence that for children of immigrant ancestry, the first year in child care reduces the language deficits and deferment by more than the third year in child care.

Finally, one channel through which an expansion in child care facilities may affect children is through a change in maternal labor supply. Most studies, including Baker, Gruber and Milligan (2008) and Lefebvre and Merrigan (2008) for Canada, Havnes and Mogstad (2011b) for Norway and Berlinski and Galiani (2007) for Argentina, find that increases in the supply of child care places have much smaller effects on maternal labor supply than on child care utilization.\(^10\) In line with this literature, we show the U.S. improved pre-reading, pre-writing, and pre-math skills in particular for Hispanic children whose parents speak Spanish at home or whose parents were born in Mexico. Drange and Telle (2010) report that the introduction of a free pre-school program for five-year-olds in Oslo improved test scores of immigrant girls.

Loeb, Bridges, Bassok, Fuller and Rumberger (2007) find the greatest association of center-based care with reading and maths scores for children who start at age 2 or 3 rather than at younger or older ages and negative effects on behavior to be greater the younger the starting age.\(^9\) In a similar vein, Gathmann and Saß (2012) find that a subsidy given to parents who do not enrol their child in formal child care lead to a much larger reduction in child care attendance rates than in maternal employment rates. González (2012), in contrast, finds that a universal child benefit reform lead to an increase in maternal time at home as well as a decrease in day-care utilization during the first year after birth. Gelbach (2002), using age of school entry cut-off rules, finds sizeable effects of child care availability on the labor supply of married women, and particularly for single women without younger children - a result that is similar to Cascio (2009b) who finds that the introduction of Kindergarten programs for 5-year-olds lead to sizeable employment responses for single mothers. See Blau and Currie (2006) for a comprehensive survey.
that the expansion in child care facilities had little effect on the labor supply of mothers of both native and immigrant children. Hence, in our setting, the effect of child care attendance on child outcomes through an increase in maternal labor supply is small for both groups of children.

3 Background

3.1 Child Care and Child Development: Channels

The following discussion illustrates the channels through which public child care attendance may affect child outcomes. For simplicity, we distinguish two age periods only; period 1 corresponds to the years when the child is 3 or 4 years old, while period 2 corresponds to years when the child is 5 or 6 years old. In each period, parents can make four types of investment into their child: time investments by the parents themselves, time investments by alternative care givers such as child minders or grandparents, time investments in public child care, and monetary investments such as toys or books.

Suppose that initially, public child care is rationed (as it was the case in Germany in the early 1990s). Parents optimally choose investments by maximizing a combination of their own and their child’s utility (which is a function of the child’s achievement), subject to a time and budget constraint, as well as subject to the slot constraint in public child care. Now suppose that the government expands public child care for children in period 2 (i.e., when children are 5 or 6 years old). Consider a child who was not offered a public child care slot in period 2 in the absence of the expansion, but now is. Panel A of Figure 1 illustrates the channels through which child care attendance in period 2 may affect child development at the end of period 2. First, public child care attendance may directly reduce time investments by the mother as well as by alternative care givers. Second, child care attendance may also lower monetary investments if public child care attendance is costly. Second, child care attendance may increase maternal labor supply, as it frees up some of the mother’s time. This in turn may decrease

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\[\text{Here, it is important to emphasize that one additional hour in public child care does not necessarily reduce time investments by the mother or by alternative care givers by the same amount. For instance, the mother may use the time the child spends in child care to do much of the household work and hence not reduce the active time in which she plays with and teaches her child; see e.g. Felle and Lalive (2012) for empirical evidence along these lines. It is even possible that parents increase the active play and learning time with their child because of child care attendance, as they receive new information about their child’s development from teachers; see e.g. Gelber and Isen (2012) for empirical evidence.}\]
time investments by the mother, but may increase time investments by alternative care givers as well as monetary investments through an increase in household income. These arguments highlight that the impact of public child care attendance on child development depends on the extent to which public child care replaces care by parents as opposed to care by others, and whether child care attendance is accompanied by an increase in the mother’s labor supply and income. To ease the interpretation of our estimates, we shed some light on these issues in Section 6.5.

Next, suppose that the government expands public child care also for children in period 1 (i.e., when children are between 3 and 4 years old). Consider a child who was not able to attend public child care in period 1 in the absence of the expansion, but now is able to do so. Panel B of Figure 1 illustrates the dynamic effects which child care attendance in period 1 may have on child development at the end of period 2. First, child care attendance in period 1 will affect the child’s achievement at the end of period 1—which, as discussed above, depends on the relative marginal returns of the different types of investments, and the extent to which public child care attendance crowds out or complements these investments. Note that the effect of child care attendance in period 1 on child achievement in that period may differ from that in period 2, most notably because the relative marginal returns of the different types of investments may differ by the child’s age when she starts child care. In particular, the psychology literature emphasizes that child care attendance may be more beneficial for older children, as for young children the exposure to multiple care givers may lead to behavioral anomalies and difficulties in social relations with others (e.g. Belsky, 1988; Belsky and Eggeben, 1991; Bowlby, 1969).

In addition, investments in public child care in period 1 may affect the marginal returns to investments in period 2, leading to dynamic effects. These dynamic effects are in principle ambiguous. On the one hand, as emphasized for instance by Cunha and Heckman (2007), a higher stock of skills at the end of period 1 may increase the marginal return to investments in period 2, which in turn may induce parents to increase their investments in period 2. Such dynamic complementarities in the form of "skills beget skills" imply that investments in public child care are more productive in period 1 than in period 2. On the other hand, the return to investments in public child care may be decreasing in
the number of years the child has already spent in child care, so that the third and fourth year in child care is less productive than the first or second year in child care. Since child care is an absorbing state (i.e., once the child enters child care, she typically stays in child care until entering elementary school), it is impossible to disentangle such dynamic effects from heterogeneous effects of child care attendance by age. We discuss these dynamic or heterogeneous effects in Section 6.4.

3.2 Child Care Provision in Germany

In Germany, formal child care provision of children aged between 3 and 6 is mostly public.\textsuperscript{12} Child care provision is further characterized by strict country-wide quality standards: in all child care institutions, the student-teacher ratio must not exceed 2 teachers for 25 children. Teachers must have at least completed a two-year state-certified vocational programme followed by a one-year internship (children’s carers). Further regulations exist regarding the space provided by the institution for each child. As a consequence, the quality of child care is relatively homogeneous.

In terms of quality standards, Germany occupies an intermediate position in an international context. The student-teacher ratio of 12.5:1 lies in between UK center-based programs with a ratio of 8:1 for 3-7-year olds, the US Head Start program with a maximum ratio of 10:1 and France with a ratio of 25:1 (OECD, 2006). Annual expenditure per child in 2002 is estimated to be $4,998 for Germany and is comparable to other continental European universal child care programs, such as for France ($4,512) or for the Netherlands ($4,923), but lies below high quality intensive programs like Head Start, which invests about $7,200 per child (OECD, 2005, 2006).

Similar to most universal child care programs, the majority of children in Germany (i.e., more than 90\%\textsuperscript{13}) attend child care part-time, for 4 hours in the morning. As it is typical for the age group considered, learning is mostly informal and play-oriented, and carried out in the context of day-to-day social interactions between children and teachers. The main learning goals emphasize the personal and emotional development and social skills through for instance role play, structured discussions and the

\textsuperscript{12}For instance, in 2007 only 21 out of the 4264 institutions were private in the region we consider.

\textsuperscript{13}The calculation is based on data from the Statistical report on child care institutions from the Lower-Saxonian Statistical office (Niedersächsisches Landesamt fuer Statistik, 2004, p. 19).
implementation of social rules; the development of cognitive abilities and positive attitudes towards learning through for instance puzzle solving and memory games; physical development through for instance gymnastics or soccer lessons; language and communication though for instance story telling, songs, and rhymes; knowledge and understanding of the world through for instance excursions and looking after plants and animals; life skills through for instance encouraging independence and self-hygiene; mathematical understanding through for instance recognizing and sorting shapes and quantities; and creative development through for instance handicrafts, music and staging a theater play. These education and care elements are found in the curricula of many early childhood programs, such as the HighScope (Ypsilanti) program (OECD, 2004) or the Early Years foundation stage in the U.K. (Department of Education, 2012).

An important further element of German formal child care (as of similar programs) is the cooperation with parents, in order to communicate and inform parents about the child’s developmental and learning progress and provide them with educational guidance.

3.3 The Legal Mandate and Financing of Child Care

In Germany, child care is heavily subsidized, with parental fees covering only about 10% of the overall child care costs\(^{14}\); the remaining child care costs are shared by the municipality and the state government. In the early 1990s, the responsibility between municipality or state for the provision of child care was only vaguely defined by law and subsidies for the construction of formal child care institutions were limited. This lead to a severe rationing of child care slots, as reflected by the long waiting lists. Rationing was most severe for 3-year old children and children of non-employed mothers, as the age of the child and the employment status of the mother were the key admission criteria. In 1991 and 1992, the rationing of child care slots and the burden it imposed on families was at the center of the political discussion. In August 1992, the federal government introduced a mandate which, by January 1st 1996, entitled every child to a subsidized 4-hour child care slot from her third birthday until school entry. While the slot

\(^{14}\text{A typical fee paid by parents is, depending on the parent’s income, between 54 and 129 Euros per month for a 4-hour slot in 2006.}\)
was to be provided by the municipality of residence, some financial aid was available from the state.

In particular, the state of Lower Saxony (the state to which the region we consider in the empirical analysis belongs) introduced subsidies in 1993 to cover construction costs of newly established institutions by about 25%, besides subsiding center staff expenses by 20%. Hence, in Lower Saxony (as well as in most other West German states) municipalities with low initial coverage rates and thus a need for a greater expansion of child care slots, were eligible for, in absolute terms, the highest subsidies. Moreover, unlike the situation prior to the legal mandate, municipalities were allowed to go into debt to finance the construction of new child care slots, and the debt of municipalities was partly leveled through equalization transfers from richer to poorer municipalities. It is important to stress that the additional expenditure on child care in the municipality did not reduce other social spending, such as social and unemployment benefits, in the municipality, as these are regulated at the federal level and therefore independent from local government funds. Furthermore, municipalities could not finance the increased expenditure on child care through an increase in income tax rates, as these are likewise set at the federal level. This limits the concern that the expansion in child care crowded out other public expenditure, or reduced household income, which might have negatively affected child development.

Despite the financial aid from the state, the construction of child care slots imposed too strong constraints on the region we consider (as well as on most regions in West Germany) so that providing a child care slot to every 3-year-old by January 1996 was not realizable. The state government of Lower Saxony therefore allowed for exceptions from the universal legal claim to a child care slot until December 31, 1998. This lead to a staggered timing and intensity of the construction of child care slots across municipalities—which we will exploit in our empirical analysis described in Section 5.1.

### 3.4 Expansion in Child Care Facilities

In the following, we describe how the federal mandate affected the child care attendance rates of children of native and immigrant ancestry in the particular region we consider in our empirical analysis (i.e., the region for which we have access to the school entry examinations). This region, the Weser-Ems
region, is part of the state of Lower-Saxony, and shares a border with the Netherlands. It is mostly rural; the largest cities are Osnabrueck (around 270,000 inhabitants), Oldenburg (around 160,000 inhabitants), Wilhelmshaven (around 80,000 inhabitants), and Emden (around 50,000 inhabitants). The region consists of 139 municipalities, with an average population size of 17,825.\textsuperscript{15} In 2004 the region counted 2,473,998 inhabitants in total.

Overall, between 1994 and 2002 more than 14,600 new child care slots for children aged between 3 and 6 were constructed in this region, increasing the ratio between the number of available child care slots and the number of children aged 3 to 6 from 62\% to 78\%.\textsuperscript{16} We provide more details in Figure 2 where we plot the average number of years children spent in public child care by birth cohort, separately for immigrant and native children, for our estimation sample (see Section 4 for a data description). Prior to the expansion in child care facilities, children of native ancestry spent on average 0.7 more years in public child care than children of immigrant ancestry (i.e., 2.12 years vs 1.43 years for the 1988 birth cohort). For native children, average attendance is approximately 0.3 years higher for the 1998 birth cohort which fully benefited from the expansion in child care facilities than for the 1988 birth cohort which was not affected by the expansion. The increase in the number of years in child care between the 1988 and 1998 birth cohort was considerably larger for immigrant than for native children (0.7 years vs 0.3 years), but even for the 1998 birth cohort their average attendance lacks behind that of native children (2.48 vs 2.15 years for the 1998 birth cohort).

In Figure 3, we break down the average number of years in child care into the share of children who do not attend child care at all, and who attend child care for 1, 2 or 3 years. For children of native ancestry, the increase in average attendance is primarily driven by an increase in the 3-year attendance rate, which increased from 34\% for children born in 1988 to about 56\% for children born in 1998. The increase in the 3-year attendance rate was accompanied by a decrease in predominantly the 2-year attendance rate, although the 1-year attendance rate also slightly declined. This reflects that for native

\textsuperscript{15} Calculations based data on total number of inhabitants in 2006 provided by the Lower-Saxonian Statistical Office.

\textsuperscript{16} Calculations based on data from the Statistical reports on child care institutions and population from the Lower-Saxonian Statistical Office (1994 and 2004).
children, the share of children attending child care for at least one or at least two years was already high before the expansion in child care slots (95% and 81%, respectively for the 1988 birth cohort), suggesting that for these children, the rationing of child care slots mostly affected 3-year-old children, and not 4- and 5-year-old children. Note that the increase in the 3-year attendance rate is concentrated among birth cohorts 1990 to 1994 who are most affected by the legal mandate, suggesting that the increase is indeed driven by the slackening of the supply constraint.

The picture for children of immigrant ancestry is different. Similar to children of native ancestry, the 3-year attendance rate is markedly higher for children born in 1998 than for children born in 1988 (44% versus 18%). However, before the expansion in child care slots immigrant children were much more likely not to attend child care at all, or to attend child care for only one year, than native children (22% vs 5% and 55% vs 19%, respectively, for the 1988 birth cohort). Consequently, in contrast to native children, the increase in the 3-year attendance rate for immigrant children was accompanied by a decrease in both the non-attendance, 1-year-attendance and ultimately the 2-year attendance rate. Note that the non-attendance rate started to decline for the 1990 birth cohort (i.e., the first cohort affected by the construction of new child care slots), while the 1-year attendance rate started to decrease one year later, for the 1991 birth cohort. Due to the decline in the non- and 1-year attendance rate, the 2-year attendance rate initially rose, but eventually started to fall.

Overall, the expansion in child care slots increased child care attendance rates of immigrant children by more, and at different margins, than of native children. One explanation for this is that, due to the lower employment rates of their mothers (see also Table 10), 4- and 5-year-old immigrant children were more affected by the rationing of child care slots than 4- and 5-year-old native children. An alternative explanation is that the federal mandate increased awareness among immigrant parents (relative to native parents) about the availability of highly subsidized public child care and also improved the knowledge about the process of how to apply for child care places.17

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17 In line with this argument, local health authorities contacted parents whose child was not enrolled in child care the year before school entry after the legal mandate was passed.
4 Data and Descriptive Evidence

4.1 School Entrance Examination

A unique feature of the German school system is that all children undergo a compulsory school entry examination in the year they are legally required to start elementary school. The main purpose of the examination is to assess children’s school readiness. The examination typically takes place in the nearby elementary school of the child’s municipality of residence in the months between February and June before school entry in August, lasts about 45 minutes, is conducted by government pediatricians, and includes a battery of tests of gross and fine motor skills as well as an assessment of the child’s speech development. Our empirical analysis draws on administrative data reporting results of these examinations for one large region in West Germany (the Weser-Ems region; see Section 3.4 for a brief description of this region) for the years 1994 to 2006.

We focus on four test outcomes: Language or speech difficulties for children of native ancestry and proficiency in the host country language for children of immigrant ancestry; fine motor skill problems; gross motor skill problems; as well as an overall measure for school readiness. Similar indicators are commonly used to assess the child’s school readiness in the U.S.\textsuperscript{18}, and have been found to be strong predictors of later academic success (e.g. Duncan et al., 2007; Grissmer et al., 2010 and Pagani et al., 2010). In particular motor skills are influential for cognitive development early on (e.g. Pagani and Messier, 2012) and research suggests that they are a stronger overall predictor for later test scores than even early reading or math test scores (Grissmer et al., 2010). An important further advantage of our data is that our outcome variables are standardized assessments by health professionals as opposed to subjective assessments by parents, which may be subject to a number of sources of bias.\textsuperscript{19}

Language and speech difficulties are identified in our data through the conversation between the pediatrician and the child during the examination, as well as through three tests where the child is

\textsuperscript{18}The National Education Goals Panel in US defines school readiness by physical well-being and motor development; social and emotional development; approaches towards learning; language development; and cognition (National Education Goals Panel, 1995). Many of these are measured in the school entry examinations.

\textsuperscript{19}In an appendix, Baker, Gruber and Milligan (2008) provide a detailed discussion on this issue.
asked to articulate words correctly, to describe a picture, and to repeat sentences. Since these elaborate tests are often conducted only among native speakers, we use this outcome only for native children. The outcome we use for immigrant children instead is an indicator whether the pediatrician recommends German training because the child’s German and speech comprehension is insufficient.

The diagnosis of fine motor skills problems is based on drawing tasks (e.g. drawing of shapes, such as triangles or crosses, or of a person), testing finger dexterity, as well as on tasks testing the hand-eye coordination and diadochokinesia (the capacity to bring a limb alternately into opposite positions), for example by asking the child to demonstrate the movement of trying a door knob or screwing in a light bulb and performing the finger-thumb-test, in which the child touches each finger of one hand to the thumb of the same hand in an alternating pattern. Tests that check for gross motor skill and coordination problems include standing on one leg for at least 10 seconds without shaking and holding on to something, jumping on one leg, astride and scissor jumps, hopping to the side, balancing on a line, and exercises with a ball.20

Based on all tests the pediatrician gives a recommendation to parents and the elementary school whether to hold the child back from immediate school attendance. This outcome may thus be seen as an assessment on the overall school readiness of the child. It is an interesting outcome variable also because deferment from school entry may lower lifetime earnings, due to delayed entry into the labor market.21

In addition to test outcomes, our data include information on the number of years the child spent in public child care. Other than migration background, parental background characteristics are included in our data only from 2002 onwards. We merge to this data source the number of inhabitants, social welfare recipients and unemployed in the municipality, obtained from the statistical office in Lower Saxony, as well as characteristics of the local labor market in the municipality (such as the median wage or the share of university graduates in the workforce), obtained from Social Security Records.

20 In our data, motor skill problems and language and speech difficulties take four values depending on the severity of the anomaly. As very severe levels are a rare outcome and the multi-valued outcome variable is lacking a meaningful cardinal scale (see Cunha and Heckman, 2008), we have transformed variables into binary outcome variables.

21 Deming and Dynarski (2008) find that in the U.S. "red-shirting", i.e. the delayed entry into first grade or Kindergarten, reduces educational attainment and lifetime earnings.
4.2 Sample and Test Outcomes

From this data base, we select school entry cohorts who were most affected by the expansion in child care slots, which are children examined between 1994 to 2002\textsuperscript{22}. We restrict the sample to children born between January and June. We impose this restriction because children born between July and December may opt to start school one year earlier than implied by the age of school entry cut-off rule (which foresees that every child that turns 6 before the 30th of June enters elementary school in the fall of the respective year, whereas every child that turns 6 after that cut off enters elementary school in the following year). As a consequence, some children born between July and December are examined in the year they turn 6, while others are examined in the year they turn 7, when they are legally required to start school. In contrast, all children born between January and June are examined in the year they turn 6.\textsuperscript{23} In addition, we exclude 21 municipalities which, due to IT problems, provided the data only partially. Our baseline sample consists of 118 municipalities.

Table 1 provides a first overview of our sample and outcome variables, separately for children of native and immigrant ancestry. Overall, our sample consists of about 78,000 native children and about 10,000 immigrant children, although the number of observations slightly varies according to the outcome variables. Children from the former Soviet Union form the largest immigrant group, making up more than 40\% of immigrant children in our sample. Most of these children are ethnic Germans and their parents arrived in Germany predominantly in the early 1990s, starting in the late 1980s, after the breakdown of the communist regimes in Eastern Europe. Children of Turkish descend form the second largest immigrant group, making up roughly one quarter of immigrant children in our sample. The Turkish arrived in Germany predominantly in the 1960s and 1970s. Hence, the parents of children of Turkish descend have resided in Germany much longer than the parents of the children who arrived from the former Soviet Union, but nevertheless often do not use German at home (see footnote 4 for evidence).

\textsuperscript{22}The 2002 examination cohort was 3 years old in 1999, which is the first year, when the legal claim was fully binding.  
\textsuperscript{23}Including children born between July and December, and controlling for age at test in a flexible manner, has little impact on our results.
Developmental delays are common in our sample. 22.2% of native children show language and speech difficulties, and pediatricians recommend German language training to more than one third of immigrant children. While fine and gross motor skill problems are equally common among native and immigrant children (roughly 17% of children experience some problems), recommendations for deferment from immediate school entry are almost twice as likely for immigrant than for native children (12.7% versus 22.6%). In the last row, we compute a summary measure that is equal to 1 if the child either has some fine or gross motor skill problems or shows some speech or language difficulties (native children) or is recommended for German training (immigrant children). Overall, 38.7% of native children and 46.4% of immigrant children experience any kind of problem, indicating that the different outcome measures are not perfectly correlated. With the exception of German training for immigrant children, developmental problems are more pronounced for boys than for girls for both native and immigrant children.\textsuperscript{24}

4.3 Parental Background and Child Care Attendance

Table 2 provides additional information about child care attendance and parental characteristics of the two groups of children. Since parental background characteristics are included in our data only from 2002 onwards, numbers refer to the years 2002 to 2006 and hence after the expansion in child care facilities. On average, children of immigrant ancestry children come from a more disadvantaged background than children of native ancestry: Mothers and fathers of immigrant children are less educated, younger, have more children, and are less likely to work than mothers and fathers of native children. For native children, there is a strong positive selection into child care attendance in terms of observable parental background characteristics, in particular with respect to parental education: While the share of mothers and fathers with no post-secondary education is 16.7% and 6.0%, respectively, for children who do attend child care for one year only, it is only 5.6% and 2.5%, respectively, for children who attend child care for three years or more. Similarly, while only 7.3% and 12.6% of mothers and fathers of children who attend child care for one year only have some tertiary education, the share is 18.7% and 29.5% for

\textsuperscript{24}Deming and Dynarski (2008) also find for the U.S. that red-shirting is more common for boys than for girls.
mothers and fathers of children who spend three years or more in child care.

Interestingly, selection into child care, in particular with respect to parental education, is much less pronounced for immigrant children. For instance, the share of mothers and fathers with some tertiary education of children who attend child care for one year is 12.5% and 13.9%, respectively, compared to 14.6% and 17.6% for mothers and fathers of children who spend three years or more in child care. This will be important for the interpretation of our results below.

5 Identification Strategy

Next, we describe our identification strategy to obtain causal estimates of the impact of child care attendance on child outcomes.

5.1 Baseline Specification

Consider a linear causal model that links outcomes of child $i$ in municipality $m$ and school examination cohort $t$ ($Y_{imt}$) to the number of years the child has spent in child care ($Care_{imt}$), and allows the impact of child care attendance on child development to differ for the two groups of children (which we index by subscript $g$):

$$Y_{imt} = a^g_0 + a^g_1 Care_{imt} + X^g_{imt} \alpha^g_2 + f^g_m + \gamma^g_t + A^g_{imt} \beta^g_2 + u_{imt}. \quad (1)$$

Here, $X_{imt}$ is a vector of observed child characteristics, $f^g_m$ and $\gamma^g_t$ denote municipality and school examination cohort fixed effects (which, in the immigrant sample, we allow to differ by ethnic group), $A^g_{imt}$ is a vector of unobserved (to the econometrician) factors that may affect child outcomes and child care attendance alike, and $u_{imt}$ is an i.i.d. error term. The coefficient of interest is $a^g_1$, which captures the impact of one additional year spent in public child care on child outcomes. Note that this specification restricts the impact of child care attendance to be linear in child care duration. This does not only rule out that the impact of public child care on child development varies by age, but also ignores possible

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$^{25}$We control for gender, age at examination, month of birth dummies, and (for immigrants) ethnic group dummies as well as ethnic-year dummies.
dynamic effects. In Section 6.4, we show that if the effect of public child care on child development is non-linear, the IV estimate of $a_1$ in equation (1) may be interpreted as a weighted average of the marginal effects at a given age. In that section, we also provide some evidence on the non-linearity of returns to child care.

The problem with estimating this regression by OLS is the potential selection of children into child care based on unobservable characteristics $A_{imt}^0$, leading to a potential bias in our estimates. From Table 2, we expect, due to the strong positive selection into child care, the OLS estimates to be upward biased for native children. The bias is likely to be smaller for children of immigrant ancestry for whom the positive selection into child care is much less pronounced than for native children.

To obtain plausibly causal estimates, we exploit the staggered timing and intensity of the construction of child care slots across municipalities, induced by the federal mandate which entitles every child to a 4-hour child care slot from her third birthday up to school entry. We implement our estimator as an instrumental variable estimator, where we instrument individual-specific child care attendance $Care_{imt}$ in equation (1) with the aggregate child care attendance rate for children in municipality $m$ who are examined in year $t$, which we compute separately for children of immigrant and native ancestry: $Care^g_{imt}$.26 Thus, compared to OLS estimation, IV estimation discards all child-specific variation in child care attendance within municipalities and cohorts—which is likely to be driven by endogenous selection into child care—, and exploits only variation in the aggregate child care attendance rate of native and immigrant children within municipalities across cohorts—which, due to the severe rationing of child care slots at the beginning of our sample period, is likely to be driven by the construction of new child care places induced by the federal policy reform that differed in intensity and timing across municipalities. We discuss the key identification assumptions in detail in Section 5.2.

We compute aggregate attendance rates separately for children of immigrant and native ancestry because, due to the lower initial attendance rates of immigrant children, they benefited from the ex-
pansion in child care slots more than children of native ancestry (see Section 3.2 and Figures 2 and 3). Consequently, the aggregate attendance rates at the municipality-cohort level of immigrant and native children, conditional on child characteristics as well as municipality and cohort fixed effects, are with a correlation coefficient of 0.12 only weakly correlated.\textsuperscript{27}

In order to increase precision in the aggregate attendance rates, and since children are most likely to compete for child care slots with other children in their school entry cohort, we compute aggregate attendance rates for the whole sample (i.e. all children who are examined in year \( t \)), although our estimation sample includes only children born between January and June (see Section 4).\textsuperscript{28} For native children, the average and median numbers of children per municipality and year of school examination cohort are 374 and 231, compared to 100 and 53 for immigrant children. In order to make sure that our findings are not driven by small municipalities, we exclude these in our robustness checks (see the first column in Tables 6a and 6b). Throughout the empirical analysis, we cluster standard errors at the municipality level to allow for an arbitrary auto-correlation of residuals within municipalities.

We report first stage estimates of the impact of the aggregate attendance rate on the individual attendance rate, conditional on child characteristics as well as municipality and cohort fixed effects, in Table 3. Since we compute aggregate child care attendance rates for all children, but restrict the estimation sample to children born between January and June, the first stage coefficient is slightly lower than 1 (0.954 for native children, and 0.903 for immigrant children). For both groups of children, the first stage is highly significant, with an F-statistic above 1000 for native children, and above 400 for immigrant children. For immigrant children, changes in the aggregate attendance rate explain 4\% of the total variation in individual child care attendance rates, compared to only 1.4\% for native children.

\textsuperscript{27}If we use the overall aggregate attendance rate as an instrument the point estimates for children of immigrant ancestry are in line with those reported here, but are measured imprecisely as we discard a lot of useful variation aggregate child care attendance rates.

\textsuperscript{28}Computing aggregate attendance rates based on our estimation sample has little impact on our findings.
5.2 Exogenous Expansion in Child Care Facilities

Our baseline linear IV estimates (equation (1)) identify the causal impact of child care attendance on child development if the native- and immigrant-specific aggregate child care attendance rate in the municipality and school examination cohort, $\bar{C}are_{int}$, is uncorrelated with the unobserved child characteristics, $A_{int}$, conditional on observable child characteristics, $X_{int}$, as well as municipality fixed effects, $f_m$, and school examination cohort fixed effects, $\gamma_{i}$—which, in the immigrant sample, we allow to differ by ethnicity.\textsuperscript{29} There are three reasons why this assumption might be violated. First, the timing and intensity of the expansion in child care facilities might not have been exogenous, but correlated with underlying region-specific time trends in child development. Second, the variation in the aggregate child care attendance rate across school examination cohorts within municipalities might be driven by changes in the composition of children and their parents, and not by the expansion in child care slots. Third, the expansion in child care slots might have lead to a deterioration in child care quality and hence did not only affect children who are pulled into child care because of the reform, but also children whose child care attendance is unaffected by the reform. Next, we provide evidence in support of the assumption that the timing and intensity of the expansion in child care facilities was exogenous. In Section 6.2, we provide an extensive number of robustness checks in support of the other two assumptions behind our identification strategy: no changes in parental composition, and no deterioration in child care quality (see also Tables 6a, 6b and 7).

Note that unlike most of the existing literature, which exploits variation in the expansion of child care facilities across broadly defined local labor markets, we exploit regional variation at a much finer local level, within one large local labor market (see Section 3.4 for statistics on the size of the region). The common time trend assumption therefore is more plausible in our study than in many existing studies. Nevertheless, even in our study the intensity and timing of the expansion in child care slots could be non-random and driven by events specific to the municipality that themselves have a direct impact on child outcomes. In order to get a first idea which municipalities in our sample experienced an above

\textsuperscript{29}We distinguish three groups, immigrant children of Turkish descent, immigrant children from the former Soviet Union (the two largest groups) and all others.
average expansion in child care slots between 1994 and 2002, we regress in Table 4 the change in the aggregate child care attendance rate between 1994/95 and 2001/2002 on the initial child care attendance rate in 1994/95, separately for native (column (1)) and immigrant children (column (2)), as well as on a number of baseline municipality characteristics. By far the most important determinant of the increase in the native- and immigrant-specific aggregate child care attendance rate in a municipality is the initial attendance rate: municipalities with low initial aggregate attendance rates—which, in absolute terms received larger subsidies from the state, and for which the political pressure to construct new child care slots was higher—experience a larger increase in aggregate attendance rates than municipalities with high initial aggregate attendance rates.

Interestingly, the baseline aggregate child care attendance rate for native children has a strong positive impact on the change in the aggregate child care attendance rate for immigrant children (a coefficient of 0.58), suggesting that a larger share of the newly created child care slots falls to immigrant children if the initial aggregate child care attendance rates of native children is higher and their demand for additional child care slots is thus lower. We do not observe a similar pattern for native children. Importantly, neither the median wage, the share of medium- and high-skilled individuals, the share of individuals on social benefits, nor the number of inhabitants or the share of immigrants in the workforce, help to predict the change in the aggregate attendance rate for native or immigrant children. Only one of the 14 variables we include, the unemployment rate in the regression for native children, has a statistically significant coefficient at the 5% level.

In order to further investigate whether the timing and intensity of the construction of new child care slots is quasi-random, we regress in Tables A1a and A1b the native- and immigrant-specific aggregate child care attendance rate on the same municipality characteristics as in Table 4, while controlling for child characteristics, municipality and cohort fixed effects. The municipality characteristics refer to the year when the child is 5 and 2 years old, i.e. to 1 and 4 years before the school entry exam. Focusing on the aggregate child care attendance rate in years (column (1)), all but one municipality characteristics (the number of inhabitants at age 5) are statistically insignificant for both immigrant
and native children. This supports our assumption that the size and the timing of the expansion in child care slots is unrelated to underlying trends in child outcomes. Nevertheless, to make sure that our estimates reflect causal effects, we conduct a battery of robustness checks, such as controlling for observable municipality characteristics or allowing linear municipality-specific time trends. We describe the robustness checks in detail in Section 6.2 and report them in Tables 6a and 6b.

6 Results

6.1 Public Child Care and Child Development: Baseline Results

How does public child care attendance affect child development? We report our baseline results in Table 5, separately for native (Panel A) and immigrant children (Panel B). In each panel, we display OLS estimates based on equation (1) in the first row, and IV estimates where we instrument individual child care attendance with the native- and immigrant-specific aggregate child care attendance rate, \( \text{Care}^a_{mt} \), in the second row. For native children, OLS estimates consistently indicate that public child care attendance improves child development: One additional year in child care lowers the probability of deferment by 5.1 percentage points, and reduces the probability of speech and language difficulties and fine and gross motor skill problems by 2.5, 2.9 and 2.4 percentage points, respectively. However, the positive impact of child care attendance on child development disappears if we exploit the staggered timing and intensity of the construction of child care slots across municipalities in the IV estimates in the second row. In fact, in contrast to the OLS estimates, the IV estimates indicate that one additional year in public child care may increase developmental problems—although the effects are imprecisely estimated and not statistically significant. In line with the evidence in Table 2, the difference between OLS and IV estimates points to a positive selection of native children into child care.\(^30\)

For immigrant children, in contrast, OLS and IV estimates are similar in magnitude. This is once again consistent with the evidence in Table 2 which shows that the selection of immigrant children

\(^30\) Alternatively, the two estimators may place different weights on the marginal effects, which we will discuss in Section 6.4. However, our calculations show that OLS and IV weights are very similar (see Table 9, column (1)). The difference between the OLS and IV estimates in Table 5 are therefore largely due to a positive selection bias in the OLS estimates.
into child care is small in terms of observable family background characteristics. Both OLS and IV estimates consistently suggest that public child care attendance improves child outcomes. According to our baseline IV estimates, one additional year in child care reduces the probability of receiving German training by 15 percentage points, lowers the probability of fine and gross motor skill problems by 8.2 and 2.2 percentage points, and decreases the probability of deferment by 8.7 percentage points. Evaluated at the baseline, these effects are large and imply that a one-year increase in the child care attendance rate lowers the probability of receiving German training by 43.7 percent, reduces the probability of fine and gross motor skill problems by 50.3 and 15.6 percent, and decreases the probability of deferment by 38.4 percent.

These results are based on all municipalities in the region, some of which are very small. To make sure that our findings are not driven by small municipalities with few children, we exclude in column (1) of Table 6a (children of native ancestry) and Table 6b (children of immigrant ancestry) all municipality-school examination cohort cells with less than 100 native children (Table 6a) or less than 20 immigrant children (Table 6b). This has little impact on our findings.

6.2 Robustness Checks: IV Assumptions

Next, we report a number of additional robustness checks which support the three key assumptions behind our IV strategy: the randomness of the expansion of child care facilities, no changes in parental composition, and no deterioration in child care quality.

Random Expansion In columns (2) to (5) of Tables 6a (native children) and 6b (immigrant children), we probe the assumption behind our IV estimates that the intensity and timing of the construction of new child care slots was quasi-random. Results in column (2) correspond to our baseline specification, but we now add a wide array of time-varying municipality characteristics (the same ones as in Tables A1a and A1b) referring to 1 and 4 years before the school entry examination when children were 5 and 2 years old, respectively. The specification in column (3) allows for municipality-specific linear time trends. In column (4), we control for a fully flexible time trend with respect to the child
care attendance rate in the baseline school examination cohort (the average group-specific aggregate child care attendance rate over 1994/1995), the key determinant for the construction of new child care slots (see Table 4). In a similar spirit, in column (5) we allow for a fully flexible time trend with respect to observable municipality characteristics (the same ones listed in Table 4) in the base year. All specifications yield similar results: For native children, our findings point to a small negative and mostly insignificant effect of child care attendance on child development. For immigrant children, findings based on the robustness checks are close to our baseline estimates in Panel B of Table 5 and indicate that one additional year in child care significantly reduces the probability of developmental problems.

**Changes in composition** Another assumption behind our IV strategy is that the variation in the aggregate child care attendance rate across school examination cohorts within municipalities is only driven by the (exogenous) expansion in child care slots, rather than by (possibly endogenous) changes in the composition of children and their parents. Compositional changes could arise due to migration to the municipality.\(^{31}\) In column (6), we therefore drop children who moved to the municipality within the last two years. We then exclude all municipalities that experienced an inflow of immigrants (measured as the percentage point change in share of immigrants in the working population) between 1991 and 2002 above the median (column (7)). Our results are largely unchanged.

If changes in the immigrant-specific aggregate child care attendance rate across school examination cohorts within municipalities is driven by changes in the composition of children, we would expect these to be correlated with changes in observable characteristics of immigrants. In column (8), we therefore account for time-varying municipality characteristics which, in contrast to column (2), we now allow to vary between immigrants and natives. This has little impact on our findings for both immigrant and native children. Our results are also robust to the inclusion of a fully flexible district-specific time trend (our region consists of 18 districts, column (9)), indicating that our findings are not driven by changes in parental composition across districts. Finally, in column (10) we exploit variation only in

\(^{31}\)Among native children, moves to the municipality are rare: only 3.08% of children have moved to the municipality (either from abroad or from another municipality) over the last 2 years. Among children of Turkish ancestry, moves to the municipality are similarly rare (4.72%). Among children from the Former Soviet Union, in contrast, 19.72% of children have moved to the municipality, most of them from abroad.
the intensity, and discard any variation in the differential timing, of the expansion in child care facilities across municipalities, by using only two time periods at the beginning and end of our sample. The point estimates for immigrant children are similar to the baseline, but slightly more noisily estimated. Note that these last three findings not only speak against changes in parental composition, but also support our assumption that the expansion in child care facilities was quasi-random.

In sum, we are confident that our results are not biased because of non-randomness in the expansion of child care slots or because of changes in parental composition.

**Changes in the Quality in Child Care and Spillover Effects**  A third assumption behind our IV strategy is that the expansion in child care slots lead to a deterioration in child care quality and hence did not only affect children who are pulled into child care because of the reform, but also children whose child care attendance is unaffected by the reform. A decline in child care quality is discussed by Baker et al. (2008) as a possible reason for the large negative effects on child development of the introduction of subsidized universal child care in Quebec. Unlike in the Canadian setting, a sharp decline in quality seems unlikely in our case, since strict quality standards, like the teacher-child ratio and teacher qualifications, were in place throughout our observation period (see Section 3.2). Moreover, municipalities were given sufficient time to meet the mandate, precisely in order to avoid a large reduction in child care quality. In order to get an idea whether the expansion in child care coverage, and the necessary recruitment of child care teachers, lead to a decline in teacher quality, we have examined changes in teacher characteristics between 1990 and 2002, using Social Security Records on all child care teachers in our region. Over this period, the number of full- and part-time teachers has increased from 4,734 to 6,902, corresponding to an increase in 1,217 full-time positions (or 31%). We find no evidence that teacher quality, measured as the share of teachers with at least a bachelor or vocational degree or the share of teachers with at least 2 years of experience in the industry, decreased.

If the quality of care nevertheless deteriorated due to the rapid construction of new child care places, we would expect the decline to be most pronounced in municipalities in which the construction of new child care slots happened particularly fast. In Panel A, we therefore exclude municipalities which
experienced a particularly large increase in aggregate child care attendance rates between two years in Panel A of Table 7. This has hardly any impact on our estimates for both immigrant and native children.

Another reason for spillover effects to children whose child care attendance is unchanged by the expansion in child care facilities are peer effects. This could be first because the expansion increased child care attendance rates of immigrant children by more than that of native children (see Figures 2 and 3), so that native children in child care are exposed to more immigrant children after than before the expansion, with possible consequences to their development. We investigate this in Panel B of Table 7, by including the aggregate attendance rate of immigrant children in years as an additional control variable in our baseline IV regressions (Panel B). This variable has only a small impact on developmental problems of native children, and its inclusion has little impact on our IV estimates for native children. Spillover effects from immigrant to native children therefore appear to be small.

Yet another reason why children whose child care attendance rates are unchanged by the expansion in child care slots are nevertheless affected by the reform could be their increased exposure to younger (i.e., 3-year old) children–since the reform primarily drew in 3-year old children. We investigate this possibility by including the (approximated) share of 3-year-olds in child care in the municipality and cohort that is examined one year later (i.e. the cohort that is one year younger and attends the same child care facilities as the older cohorts) as an additional control variable in our baseline IV regressions (Panel C). For native children, an increased exposure to 3-year-olds generally increases the probability of a developmental problem. This effect is, however, statistically significant for only one outcome variable (gross motor skills). The inclusion of the share of 3-year-olds also reduces the magnitude of the negative point estimate of public child care attendance on language and gross motor skill problems, pointing to the possibility that the negative estimated coefficient for native children may partly be driven by negative spillover effects from younger to older children. This interpretation is however only suggestive, as the differences between these estimates and our baseline estimate are not statistically significant.

For immigrant children, an increased exposure to 3-year-olds seems to increase the probability of a
German training recommendation, even though the effect is not statistically significant, and has little impact on the other outcome variables. Furthermore, the inclusion of the attendance rate of 3-year-olds in the younger cohort has only a very small impact on the IV estimates for all outcome variables. Thus, these findings suggest that for immigrant children, the spillover effects from younger to older children did not lead to a large bias in our estimates.

6.3 Heterogenous Effects by Gender and Ethnicity

Do the effects of child care attendance differ by gender? In Table 8, we report results based on our baseline specification (equation (1)) estimated separately by gender, where we instrument the individual child care attendance rate with the native- and immigrant-specific overall aggregate child care attendance rate. For native children, there is some suggestive evidence that child care attendance increases the probability of deferment for boys, but not for girls. For all other outcomes, point estimates are similar for boys and girls.

For immigrant children, the absolute impact of spending one additional year in child care is somewhat larger for boys than for girls. For instance, while one additional year in child care reduces the probability that the pediatrician recommends German training by 17.3 percentage points for boys and 11 percentage points for girls. Recall, however, from Table 1 that developmental problems (with the exception of German training) are more pronounced for boys than for girls, which mitigates the difference in effects between boys and girls.32

Our sample of children of immigrant ancestry includes diverse groups, with children from the former Soviet Union whose parents recently migrated to Germany, and from Turkey whose parents, or even grandparents, migrated to Germany primarily in the late 1960s and 1970s forming the two largest groups. At the bottom of Panel B in Table 8, we report results based on our baseline specification (equation (1)) estimated separately for these two groups, where we instrument the individual child care

32 Unlike our findings here, several studies, in particular those studying the effects of targeted intervention programs (see e.g. Anderson, 2008), often find larger improvements in cognitive outcomes for girls than for boys. One reason why we find somewhat larger effects for boys than girls could be that in our setting, the learning environment is informal and play-oriented and focuses on school readiness in terms of physical and language skills rather than on early academic training. This may be an environment which is particularly beneficial for immigrant boys.
attendance rate with the overall immigrant-specific aggregate child care attendance rate. Interestingly, public child care attendance reduces the incidence of developmental problems, including the incidence that the pediatrician recommends German training, for both groups of immigrant children, suggesting that child care attendance does not only benefit children who recently migrated to Germany, but also children whose parents were born in Germany or migrated to Germany many years ago.

6.4 Non-linear Returns to Public Child Care Attendance

Our analysis so far has restricted the impact of time spent in child care to be linear in child care duration and to be homogenous by age. In this section, we first show that if the effect of public child care on child development is non-linear, our IV estimates may be interpreted as a weighted average of the marginal effects at a given age. We then provide evidence on the non-linearity of returns to the length of child care attendance.

To keep the notion simple, we drop the superscript \( g \). Let \( Y_l - Y_{l-1} \) denote the marginal impact of attending child care for \( l \) instead of \( l - 1 \) years. Due to the near perfect correlation between the number of years spent in child care \( (l) \) and the age at which the child starts child care (since child care attendance is an absorbing state), \( Y_l - Y_{l-1} \) also refers to the impact of attending child care when the child is between \( 6 - l \) and \( 6 - l + 1 \) years old. The IV estimate of \( a_1 \) in equation (1) may then be interpreted as a weighted average of these marginal effects (see also Løken, Mogstad, and Wiswall, 2012 and in particular Mogstad and Wiswall, 2011):\(^{33}\)

\[
\text{plim } \hat{a}^{IV}_1 = \sum_{l=1}^{3} (Y_l - Y_{l-1})w^{IV}_l, \tag{2}
\]

\[
w^{IV}_l = \frac{\text{Cov}(D^{l}_{imt}, \tilde{Care}_{mt})}{\text{Cov}(Care_{imt}, \tilde{Care}_{mt})}
\]

where \( D^{l}_{imt} \) is an indicator variable that is equal to 1 if the child spends at least \( l \) years in public child care (or started child care at the age \( 6 - l \)), and \( \tilde{Care}_{mt} \) is the residual from a regression of \( \tilde{Care}_{mt} \)

\(^{33}\)Løken et al. (2012) use a generalized decomposition of the linear IV estimand, which was originally developed by Angrist and Imbens (1995) for the case of a binary instrument.
on municipality and school examination cohort fixed effects and the same set of control variables as in equation (1). The weights sum up to 1 and, provided that the monotonicity assumption holds, are non-negative. The IV estimator places more weight on the marginal effects at years of child care attendance (or at the age at which the child attends child care) that are most affected by the instrument. The OLS estimator may also be interpreted as a weighted average of the marginal effects, where the highest weight is given to the most common margins (not explained by the control variables).

In the first column of Table 9, we report the weights given to each marginal treatment effect for immigrant and native children in the OLS and IV estimation. Both estimation methods yield similar weights. The marginal return of attending child care for more than 2 versus 2 years receives the highest weight for native children (53.9% in the IV estimation), followed by the marginal return of attending child care for 2 versus 1 year (29.93%) and for 1 versus 0 years (16.17%). For immigrant children, in contrast, the IV estimator places larger weights on the marginal return of attending child care for 2 versus 1 year (39.13% vs 29.93% for native children) and on 1 versus 0 years (22.69% vs 16.17% for native children), suggesting that, in line with Figure 3, the expansion in child care slots affected immigrant children more often at the extensive margin (1 versus 0 years) and at a low level of the intensive margin (2 versus 1 year) of the child care attendance distribution than native children. Hence, if the benefits of public child care attendance are larger when the child is older (or decreasing in child care duration), the fact that the expansion affected immigrants children at lower margins of attendance may help to explain why the overall benefits of public child care attendance are larger for immigrant than for native children.

Next, we provide evidence on the possible non-linearity of returns to public child care attendance. To do this, we replace in equation (1) the number of years the child has spent in child care ($Care_{imt}$) with three indicator variables that are equal to 1 if the child has spent at least 1, at least 2, or at least 3 years in child care, respectively. The weights are given by:

$$w_{i}^{OLS} = \frac{\text{Cov}(D_{imt}^{l}, \bar{Care}_{imt})}{\text{Cov}(\text{Care}_{imt}, \text{Care}_{imt})}$$

Here, $\bar{Care}_{imt}$ is the residual from a regression of $Care_{imt}$ on the same set of control variables as above.

Note that the weights cannot be directly inferred from Figure 3, as it does not account for municipality fixed effects.
3 years in child care respectively \((\text{Care}_{imt}^j, j = 1, 2, 3)\):

\[
Y_{imt} = a_0^g + \sum_{j=1}^{3} a_j^g \text{Care}_{imt}^j + X_{imt}^g a_2^g + f_m^g + \gamma_t^g + A_{imt}^g a_2^g + u_{imt} \tag{3}
\]

The coefficients on these variables, \(a_j^g\), pick up the marginal impact of one additional year in child care, when the child is 5, 4, or 3 years old, respectively. We then instrument these variables with the aggregate share of immigrant and native children in the municipality and school examination cohort who spend at least 1, 2, or 3 years in child care \((\text{Care}_{gm}^j, j = 1, 2, 3)\).\(^{37}\)

We report OLS and IV estimates based on equation (3) in columns (2) to (6). For native children, OLS estimates suggest that the first, second, and third year in child care reduces developmental problems by roughly the same amount. Turning to the IV estimates, no clear picture emerges and returns are imprecisely estimated.

For immigrant children, OLS and IV estimates tend to be similar in magnitude, confirming once again that for these children the bias due to selection into child care is small. For two outcomes—recommendation of training in the German language and deferment—both OLS and IV estimates are suggestive of decreasing marginal returns to child care attendance. While the first year in child care (i.e. when the child starts child care at the age of 4 or 5) reduces the probability of a recommendation in German training by about 23.9 percentage points, the third year (i.e. when the child starts child care at the age of 3) lowers the probability by only 13.9 percentage points and the effect is statistically insignificant. This is consistent with the hypothesis that children younger than the critical period for language acquisition (around puberty, see e.g. Lenneberg, 1967) learn a new language very easily, and that two years of native language exposure prior to school is sufficient to avoid language deficits.

\(^{37}\)Compared to our baseline specification (equation (1)), this non-linear specification requires the slightly stronger assumption for identification that the share of native and immigrant children in the municipality and school examination cohort who attend child care for at least 1, 2, or 3 years (as opposed to the mean aggregate attendance \(\text{Care}_{imt}^g\)) in the baseline specification is uncorrelated with the unobserved child characteristics \(A_{imt}\), conditional on observable child characteristics, \(X_{imt}\), as well as municipality and school examination cohort fixed effects. To probe this assumption, we regress in columns (2) to (4) in Tables A1a and A1b the shares of native and immigrant children who attend child care for at least 1, 2, or 3 years on municipality characteristics, while controlling for child characteristics, municipality and cohort fixed effects. Only 4 out of 39 municipality characteristics for native children, and 2 out of 39 municipality characteristics for immigrant children, help to statistically predict aggregate attendance rates. This is consistent with our assumption that the size and the timing of the expansion in child care slots is unrelated to underlying trends in child outcomes.
Similarly, while the first and second year in child care decreases the probability of deferment by about 14.3 and 9.8 percentage points, the third year reduces the probability only by about 4.2 percentage points, an effect that is again statistically insignificant. Hence, for these two outcomes, non-linear returns to public child care attendance coupled with different weights given to the marginal treatment effects help to explain why immigrant children benefit more from spending an additional year in public child care than native children.

For fine motor skill problems, in contrast, the marginal effect of attending an additional year in child care attendance is highest in the third year when children are between 3 and 4.\textsuperscript{38} One explanation for this finding is that children’s fine motor skills rapidly progress around this age. Hence, it may be particularly important to encourage fine motor skills at that age to prevent problems later on.\textsuperscript{39}

These non-linear estimates correspond to our baseline linear specification in Table 5. We have performed the same types of robustness checks as discussed in Section 6.2 also for the non-linear specification. Results, which are available on request, are similar.

6.5 The Expansion in Child Care Coverage on Mothers’ Labor Supply

Our discussion in Section 3.1 highlights that an important indirect channel through which child care attendance may affect child outcomes is maternal labor supply (see also Panel A of Figure 1). Next, we investigate the impact of the expansion in child care facilities on maternal employment, using data from the Microcensus on all mothers residing in West Germany.\textsuperscript{40} First note that maternal employment is low by international standards: In 1991, only 38% of mothers in West Germany with a 3- or 4-year old

\textsuperscript{38}We have also run alternative specifications with quadratic terms, using the individual attendance and its square, instrumented by the attendance rate and squared attendance rate for each group. These estimates paint a similar picture as the results reported above. In particular, for minority children the coefficient on the quadratic term is positive and statistically significant for German training recommendation and deferment (pointing towards decreasing marginal returns), and negative and statistically significant for fine motor skills (pointing towards increasing marginal returns).

\textsuperscript{39}Gallahue and Ozmun (2005) stress that early childhood is the fundamental phase of motor development, where motor skills develop fast and children initially develop and refine basic movement skills, which are the basis for developing more complex movement skills at later childhood.

\textsuperscript{40}To increase the sample size, we include, unlike in the school entry examination data, children born in the second half of the year in our sample. We find similar results when we restrict the sample to children born in the first half of the year. When we restrict our sample to Northern German states to make our sample more similar to our sample on children, we also obtain very similar results.
child were employed full-time or part-time.\footnote{In the US and Sweden, 63.1\% and 75\% of mothers with a 4-5 year old child were participating in the labor market; these numbers refer to 1988 for the US and 1984 for Sweden (Gustafsson and Stafford, 1994). Piketty (2005) reports that in 1994 in France the labor force participation rate of mothers with two children, of which at least one is below 3 years old, is 69\%.}

Figure 4 gives a first impression of the link between the expansion in child care slots and mothers’ employment rates for native mothers. In Panel A of the figure, we plot child care attendance rates of native children who in April were 3 or 4 years old over time. As expected, the attendance rate of 3- and 4-year-olds sharply rose between 1993 and 2001, by about 20 percentage points, respectively. In Panel B of the figure, we plot the corresponding labor force participation rates of mothers with 3- or 4-year old children ("treatment") and mothers whose youngest child is 11- or 12-year old ("control").\footnote{We use mothers of children, whose youngest child is 11 or 12 years as our control group to ensure that the mothers were not affected by the increase in public child care supply through their younger children. Using mothers with children in a similar age range as a control group leads to similar results.} Labor force participation rates of mothers with a 3- or 4-year old child (for whom child care attendance rates increased sharply) rose by 8.7 percentage points between 1993 and 2001. However, labor force participation rates of mothers whose youngest child is 11- or 12-year old (who were thus unaffected by the construction of new child care slots) increased by almost the same amount over the same time period. This suggests that the increase in the labor force participation rates of German mothers with 3- or 4-year old children is mostly due to an aggregate time trend, and not caused by the expansion in child care facilities.

We provide additional information in Table 10 based on the same data set separately for native (Panel A) and immigrant mothers (Panel B). For native mothers, the difference-in-difference ("DinD") estimate, which calculates the differential increase in employment rates between the treatment group over the control group, points to a small positive, but statistically insignificant, effect of the expansion in child care slots on maternal employment: a 10 percentage point increase in the attendance rate of 3- to 4-year-olds leads to an increase in employment rates of their mothers by 0.63 percentage points (0.012/0.19-10). The findings in Panel B of Table 10 further indicate that the expansion in child care slots also only had a small effect on labor force participation rates of immigrant mothers: While the child care attendance rate of 3- or 4-year old immigrant children rose by 23 percentage points between 1993
and 2001, the labor force participation rates of their mothers, as well as the labor force participation rates of mothers with a 11- or 12-year old child, remained roughly constant.

Hence, in our setting, the effect of child care attendance on child outcomes through an increase in maternal labor supply is small for both native and immigrant children. This may not be surprising, given that universal child care in Germany is mostly part-time (4 hours per day) and mothers therefore need to complement universal child care with some alternative care even for a typical part-time position. Since public child care is heavily subsidized, with parental fees contributing to only 10% of the total costs of child care (see Section 3.2), we also expect the impact of child care attendance on monetary investments into the child to be small. Public child care attendance therefore affects child outcomes primarily directly, as well through its direct effects on time investments made by the mother and by alternative care givers (see Section 3.1 and Panel A of Figure 1).

Does public child care primarily replace maternal care or alternative care? In Panel C of Table 10, we provide some evidence on the counterfactual care arrangement, by separately reporting the 2001-1993 change in the share of mothers with a 3- or 4-year old child who are working and use child care—these mothers are likely to have used alternative forms of child care prior to the expansion in child care slots and switched to public child care because of the expansion—and the change in the share of mothers who are not working and yet use child care—these mothers are likely to have taken care of their child themselves prior to the expansion in child care slots. The results reveal an interesting difference between native and immigrant mothers. For native mothers, the share of children in child care increases much more for working than for non-working mothers, suggesting that for native children public child care primarily replaces alternative care. For immigrant mothers, in contrast, the increase in the share of mothers who are not working and yet use child care is twice as large as the increase in the share of mothers who are working and use child care. Hence, for immigrant children public child care primarily crowds out maternal care. This partially reflects the much lower labor force participation rates of

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43 Black, Devereux, Løken and Salvanes (2012) use a regression discontinuity approach to analyze the impact of child care subsidies on child care utilization rates and mothers’ labor force participation rates and find small effects on both. The positive effects of the subsidy on child outcomes found in this paper are therefore driven by the income effect of the subsidy.
immigrant mothers than native mothers (see Panel A of Table 10).\footnote{Unfortunately, our data do not allow us to directly investigate the impact of the expansion in child care slots on time investments made by parents and alternative care givers. Even if immigrant children who are pulled into public child care due to the expansion in child care slots would otherwise have been cared for by their mother, public child care attendance might nevertheless increase time investments by the mother. In line with this hypothesis, Gelber and Isen (2011) find that Head Start attendance caused a substantial increase in parents’ involvement with their children. Recent research by Aizer and Cunha (2012) further investigates these adjustment mechanisms.}

7 Discussion and Conclusion

In this paper, we study the impact of time spent in public or universal child care on children who are about to enter school. The program we consider is similar to universal child care programs in other countries: it is a half-day program aimed at 3-6 year-olds with strict nation-wide quality standards where, as it is typical for this age group, learning is mostly informal and play-oriented. In our empirical analysis, we draw on rare administrative data on the population of all children for one large region in Germany. Our outcome variables include indicators for developmental problems (such as language deficits or gross or fine motor skill problems) that may interfere with the child’s success at school. Similar indicators have been found to be strong predictors of later academic success (e.g., Duncan et al., 2007).

To deal with the possibly non-random selection of children into public child care, we make use of a federal policy reform which entitles every child to a largely subsidized 4-hour child care slot from her third birthday until school entry. In the region we study, this mandate lead to a staggered timing and intensity of the construction of child care slots across municipalities—which is the variation we exploit in our analysis.

We study the effects of public child care attendance separately for children of immigrant and native ancestry, and find very different effects for the two groups. While public child care attendance strongly and robustly reduces language and motor skill problems and improves school readiness for immigrant children, it has no significant effects for native children. We explore a number of possible explanation for these differences. A first reason may be non-linearities in returns to length of child care attendance.

Due to the lower initial child care attendance rates of children of immigrant ancestry, the expansion in
child care slots increased both the one-, two-, and three-year attendance rate for immigrant children, but affected primarily the three-year attendance rate for children of native ancestry. At the same time, we find for some outcomes, like language proficiency and overall school readiness, some suggestive evidence for decreasing marginal returns to child care attendance for immigrant children.

A second reason may be different counterfactual care arrangements for immigrant and native children. Our findings suggest that for immigrant children, public child care attendance primarily crowds out maternal care, while for native children it primarily replaces alternative care—which is likely to be more similar to public child care than to maternal care, in particular with respect to the exposure and interaction with other children.

A third explanation for the larger benefits of public child care attendance for immigrant than for native children is that the returns to investments in public child care, relative to investments made by the mother or alternative care givers, are higher for immigrant children. In our setting, immigrant children are more likely than native children to come from a disadvantaged background, and thus from a group for which several studies have found the benefits of formal child care attendance to be particularly large (see e.g. Havnes and Mogstad, 2011a). Even among disadvantaged children, immigrant children may be, due to the specific deficiencies they face, particularly perceptive to the learning environment provided by public child care. Foremost is here a lack in proficiency in the language of instruction. Deficiencies in language proficiency may lead to serious under-performance at early elementary education, with potentially damaging consequences not only far into the educational curriculum, but also for many other aspects of life (see e.g. Bleakley and Chin 2008, 2010). The exposure to other children in public child care, especially children whose first language is the host country language, is likely to quickly reduce this deficit. Moreover, host country language proficiency may facilitate learning in other areas and thus lead to complementarities and positive spillover effects—hence explaining the positive effects of child care attendance on other measures of early achievement besides on language proficiency. The strong positive effects on immigrant children’s school readiness could also be partly due to an improved interaction of the mother with the child as a result of the advice and guidance by the kindergarten
teacher.

Overall, our results emphasize that the benefits of universal child care programs may not be uniform but concentrated among specific groups of disadvantaged children who are particularly perceptive to the informal learning environment which public child care provides. This suggests that such programs may be successful at "leveling the playing field" at school entry (and possibly beyond), and may thus, as stressed by Currie (2001), be a more cost-effective way for governments to reduce life-time inequalities rather than compensating for unequal outcomes later in life.
References


