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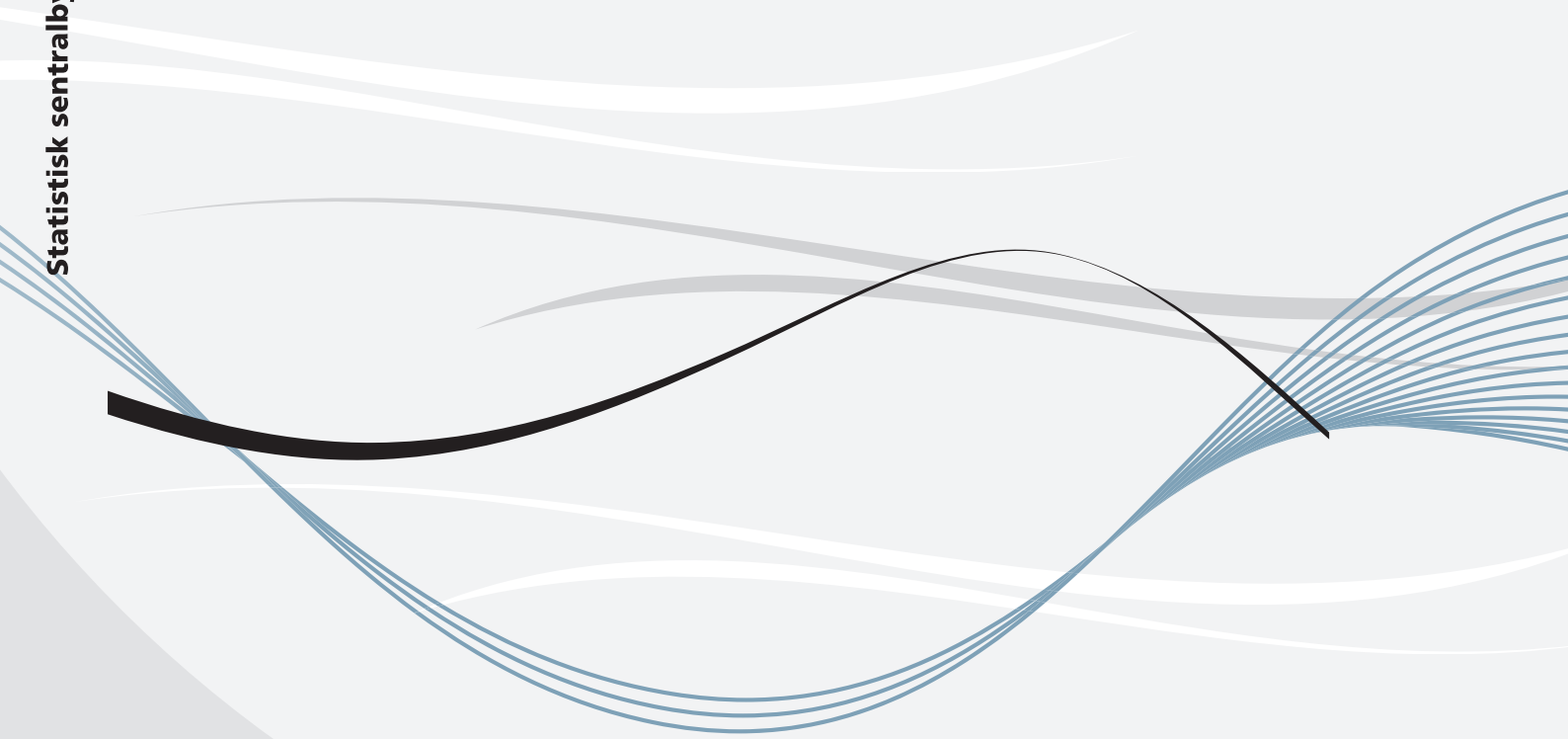
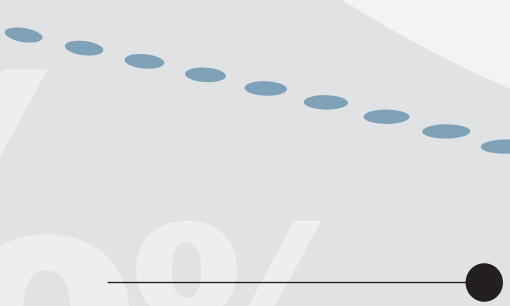
Effects of free child care on child enrollment and parental employment





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Abstract
Proficiency in the language spoken by the majority population may be crucial for the cognitive development of children from immigrant families. High-quality child care is believed to promote such language skills, and it is thus of concern that children from immigrant families are underrepresented in formal child care across OECD countries. How can we increase their participation, and can such participation improve family integration? We study an intervention in some districts of Oslo where children aged four and five were eligible for twenty hours of free childcare weekly. Taking advantage of the intervention being available in some city districts and not in others, we estimate the effect of the intervention on the enrollment of children and on their parents' employment and education, using outcomes measured for the same family before and after the child's age of eligibility. We find that the intervention increased the participation for children from immigrant families by 15 percent. However, we do not find support for effects on parental employment or education. The performance in tests at school entry (age six) for children from immigrant families in city districts with free child care is better than that of similar children in comparison districts. Overall, our results suggest that subsidizing center based child care can improve the cognitive development of children from immigrant families.

Keywords: child care; education; immigrant children; integration; assimilation

JEL classification: J13, J15, H52, I28

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Address: Postboks 8131 Dep., 0033 Oslo Statistics Norway, Research Department.
E-mail: ndr@ssb.no

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Sammendrag

Ferdigheter i språket som snakkes av majoritetsbefolkningen kan være avgjørende for den kognitive utviklingen til barn fra innvandrerfamilier. Barnehage av høy kvalitet kan fremme slike språkkunnskaper, og det er derfor bekymringsfullt at barn fra innvandrerfamilier er underrepresentert i barnehage-institusjoner i OECD-landene. Hvordan kan vi øke deres deltakelse, og kan slik deltakelse forbedre familiens integrering?

Vi studerer en intervensjon i enkelte bydeler i Oslo hvor barn i alderen fire og fem år fikk tilbud om tjue timer gratis i barnehagen ukentlig. Barna ble rekruttert av representanter fra bydelene gjennom helsestasjonene, bydelenes servicetorg samt ved informasjon sendt til barnas hjem.

Vi estimerer effekten av intervensjonen på bruk av barnehage, samt på foreldrenes arbeid og utdanning i familier med innvandrerbakgrunn. Metodisk utnytter vi at gratis barnehage var tilgjengelig i enkelte bydeler og ikke i andre. Dermed kan vi sammenligne barnehagebruk, arbeid og utdanning for samme familie før og etter barnet når alderen som kvalifiserer til gratis barnehage i bydeler med og uten gratis kjernetid. Videre forsøker vi å isolere effekten av tilbudet på barnas prestasjoner på kartleggingsprøver i første og andre klasse. Vi gjør flere robusthetstester, blant annet undersøker vi om resultatene holder seg når vi inkluderer en gruppe av barn uten innvandrerbakgrunn.

Vi finner at intervensjonen økte bruken av barnehage for barn fra innvandrerfamilier med 15 prosent. Vi finner ikke støtte for effekter på foreldrenes arbeid eller utdanning. Barna med innvandrerbakgrunn i bydeler med gratis kjernetid gjør det bedre på kartleggingsprøvene i første og andre klasse ved skolestart (seks år), sammenlignet med barn med slik bakgrunn i bydeler uten tilbud. Dette resultatet er robust for inkludering av barn uten innvandrerbakgrunn.

1 Introduction

The child care center is the first public institution in which most children spend time without parental presence. The time in child care coincides with a phase in the child's life that is crucial for the formation of its values and skills (Shonkoff, Phillips, and Council, 2000; Almond and Currie, 2011). The child care center is for most children the first step of the education system, and can thus lay the foundation for subsequent performance. A number of studies show that formal childcare during early childhood is important for child development (excellent recent overviews are provided in e.g. Almond and Currie (2011), Ruhm and Waldfogel (2012) and Baker (2011)). Moreover, for some children, formal child care might be of particular importance. For children who speak another language than the majority language, early and intensive exposure to the language spoken by the majority can be crucial for educational and emotional development (Bleakley and Chin, 2009).

Inequality in educational attainment of native and immigrant groups has been of great concern to policymakers in the US and Europe for a long time (Taguma, Shewbridge, Huttova, and Hoffman, 2009; Schnepf, 2007; Grigorenko and Takanishi, 2009; Dustmann, Raute, and Schønberg, 2013). The low enrollment of children from immigrant families in preschool programs enhances the fear that the current educational inequality will persist into future generations (Norwegian Ministry of Education and Research, 2010; J.Hernandez, Denton, and Macartney, 2009). We explore whether offering free center based child care might bridge this enrollment gap at an early age, and whether it affects parental employment and education. Furthermore, we investigate links between higher enrollment and cognitive child development.

Since 2006 the Norwegian government has allocated significant funding of an intervention offering four hours daily of free child care in five city districts in Oslo. The city districts in question have a particularly high share of children from immigrant families, a group of children with lower participation rates in child care institutions in Norway as well as in a number of other countries (Drange and Telle, 2010; Dustmann, Raute, and Schønberg, 2013). The main purposes have been to facilitate participation and provide systematic language stimulation of the children, as well as to raise awareness among their parents of the importance of language development and the crucial role played by the child care institution in promoting such development.

Our empirical strategy takes advantage of the fact that free child care for four- and five-year-olds was available in some city districts and not in others. This enables us to estimate the effect of the intervention on the enrollment of children, as well as on parental labor force attachment and educational attainment, comparing outcomes for each child (or parent) before and after eligibility age in city districts with and without the intervention. Moreover, detailed registry information on pre-intervention family characteristics allows us to explore how the effects vary across families with high vs. low family income, high

vs. low parental education, as well as across child gender and sibling parity. We find that the intervention indeed succeeded in recruiting children from immigrant families to child care centers. In treated city districts, there is an increase in enrollment rates at about 11.5 percentage points. For native children, we find no such differences, implying that the offer of free child care might help to bridge the gap between children with and without an immigrant background (see Figure 1 in Section 5.1). Although the intervention increased the enrollment of children from immigrant families, parental outcomes are to a very little extent affected. Looking at test scores at school entry (age six) we find that children with an immigrant background in city districts with free child care perform better than children in comparison districts. This result is robust for the inclusion of native children in a difference-in-difference model.

To our knowledge, very few studies have focused on child care and the particular group of children with an immigrant background. One recent exception is a study from Germany exploring a large increase in subsidized child care slots for children between three and six years old, finding positive effects on child development (cognitive and non-cognitive) for children from immigrant families (Dustmann, Raute, and Schønberg, 2013). Another study looks at the introduction of free child care for five-year-olds in two city districts in Oslo in 1998, and finds that girls (no effects for boys) of immigrants perform better at the end of primary school, ten years after the intervention (Drange and Telle, 2010).[1] The main contribution of this current study is as follows. While Dustmann, Raute, and Schønberg (2013) look at effects of an intervention that expands the number of subsidized child care slots and a legal right to child care (from a situation where demand exceeds supply), we look at effects of an intervention that is implemented in an environment with little or no rationing of slots and where child care has been heavily subsidized for several years. The intention of the intervention we study was to recruit the children from immigrant families who had not already enrolled in child care, despite the long lasting availability of heavily subsidized child care of high quality.[2] Indeed, in our setting child care was offered free of charge and public servants actively recruited non-enrolled children. Recruiting these children might be of particular importance if children who benefit the most from attending child care, are hard to recruit. Our findings also indicate that effects on test scores of attending child care is high for this margin of children. Thus, while the previous studies have focused on effects on children's cognitive (and non-cognitive) outcomes measured in school, we focus on how an intervention intentionally directed at

[1]The data available to Drange and Telle (2010) did not allow studying of uptake because the data did not contain enrollment for the affected cohorts. Furthermore, treatment was limited to fewer city districts, resulting in a smaller sample size. Lastly, treatment differed in nature and intensity, as e.g. free child care was only available one year prior to school start.

[2]The intervention considered in Dustmann, Raute, and Schønberg (2013) provided child care at a fee of 54-129 euros per month for 20 hours a day, which is similar to the fee of 500-1200 NOK that had prevailed in Oslo for several years when the free child care intervention was introduced (1€ is about 8 NOK).

children from immigrant families affects their enrollment in child care. In doing so we utilize that we have access to the outcome variable (child enrolled in care or not) for each child from before to after eligibility (at age four), which enables us to control for any time-invariant child characteristic.[3]

2 Background

2.1 Previous literature

Lack of participation in child care might delay the child's language development, in particular when parents have limited proficiency in the language spoken by the majority (Bleakley and Chin, 2009). Several of the studies examining the effects of child care on children's later school achievement and other subsequent outcomes in the general population, find that girls and children from families with low socioeconomic status benefit the most from attending formal child care (Cascio, 2009; Havnes and Mogstad, 2011b; Berlinski, Galiani, and Gertler, 2009; Anderson, 2008). Studies of the effect on school performance or further education of children from immigrant families are scarce. Dustmann, Raute, and Schønberg (2013) examine how children of immigrants are affected when child care for children aged 3-6 is legally ensured through a universal program with subsidized, but not free, child care. The authors take advantage of the staggered implementation of a federal policy change in one region in Germany, which entitled all children to a child care slot from their third birthday and until school entry. The German child care institutions are similar to the Norwegian in that they are subsidized, follow consistent national policies regarding quality, and have a focus on learning through play. The outcome measure for the children of immigrant background is whether they need additional training in the German language at school entry. In addition, the children are tested for fine and gross motor skills. Comparing outcomes of cohorts of children who are differently exposed to the child care expansion across municipalities, Dustmann, Raute, and Schønberg (2013) find positive effects on language skills for children with immigrant background of the increased availability of child care, and no effects for children without such a background. The authors accredit this difference to the fact that the expansion in the child care enrollment of children with and without immigrant ancestry was affected on different margins. Children without immigrant background did enroll in child care centers before the increase of subsidized slots, whereas the children from immigrant families previously had less exposure to child care.

While child care has proved beneficial for child development in many studies, it is not

[3] On the other hand, with a focus on children's cognitive and non-cognitive outcomes, which are only measured after child care age, previous studies have utilized variation in uptake across cohorts (before and after intervention) at the municipal level, making them vulnerable to possibly endogenous compositional changes in the cohorts and disabling controls for unobservable child characteristic.

always clear what the mechanisms are. One suggested mechanism might be that child care attendance frees up time for the parent who prior to enrollment spent time at home with the child (usually the mother). If this parent joins the workforce, family income will increase.[4] Some studies suggest that family income might affect child development positively. Dahl and Lochner (2012) find evidence that child development is affected by increases in family income. Black, Devereux, Løken, and Salvanes (2014) study a subsidy cut-off in Norway, and find that children in families with incomes just below the cut-off (i.e. receiving a larger subsidy) perform better in junior high school, whereas their attendance rates are not affected. Hence, when exploring effects of child care policies, it seems relevant to take the possible effect on maternal employment into account.

2.2 Content of the intervention

The intervention of free child care was implemented in five city districts in Oslo (Alna, Bjerke, Grorud, Stovner and Søndre Nordstrand). The reason why these city districts were chosen over the other remaining 10 city districts in Oslo, was primarily that their populations consisted of a large share of immigrant children and children with an immigrant background (Bogen and Reegård, 2009). It started the autumn 2006 with free child care for children in Stovner, and since autumn 2007 the intervention has covered all four- and five-year-olds in the five city districts, i. e. the last two years before starting school (Bogen and Reegård, 2009).[5] Total annual expenditure has been about 50 million NOK (approx. 6,25 million Euros, 1 Euro=8 NOK). Children become eligible from August in the calendar year she/he turns four, and hence all children in the same cohort becomes eligible at the same time. The main purposes have been to facilitate participation and provide systematic language stimulation of the children,[6] as well as to raise awareness among their parents of the importance of language development and the crucial role played by the child care institution in promoting such development (Bogen and Reegård, 2009). While the target group has been children from immigrant families, all children in treatment city districts pay a reduced child care fee according to the policy.

Free child care has only been available for the families residing within the treated city districts, and hence none of the neighboring districts have been affected. If the child spends more that four hours per day in child care, the parents have to pay for the

[4]Studies on this topic are inconclusive. While Havnes and Mogstad (2011a) find negligible effects on mothers labor force attachment following a large expansion in universal and subsidized child care in Norway in the 1970s, other studies, such as Bauernschuster and Schlotter (2015) for Germany, Baker, Gruber, and Milligan (2008) for Canada and Brewer, Cattan, Crawford, and Rabe (2014) for England, find substantial effects on maternal labor supply.

[5]In the city district of Gamle Oslo, a similar policy was introduced for children born 2007 and onwards. This implies that Gamle Oslo will be considered a comparison city district for children born 2004-2006, and an intervention city district for children born 2007.

[6]For example, some city districts report that they hired multi-lingual teachers to meet the demand of the new minority language children (Bogen and Reegård, 2009).

additional time. Thus parents of children already in full- or part-time child care have had lower expenditures on child care compared to parents in comparison city districts. Information about free child care has been actively conveyed through health care centers, by district civil servants and social services (Bogen and Reegård, 2009). Apart for the recruitment efforts, the intervention city districts aim to provide enrolled children with a sufficient pedagogical program, tailored to the needs of minority language children. As an element of the efforts directed towards engaging parents, language training (in particular for mothers) has been offered at hours when the children can spend time in child care. These courses have typically been available for mothers with younger children as well, but free child care has only been available for the four- and five-year-olds. Some of the city districts had an offer of language training in place prior to the introduction of free child care, but have expanded their courses post intervention. While the offer of language training is available for all parents, it is up to the individual family whether they want to participate. Thus, the intervention is to some extent a "package" that consists of an active recruitment approach towards families with an eligible child, a free part-time child care slot, an increased focus on tailored educational content for the group of children with an immigrant background, and an offer of language courses for parents. It should be noted, however, that the vast majority of funding received by the city districts has been used to cover the reduced fees from parents. In 2008, the price of a full time child care slot in Oslo was about 800 NOK (100 Euro) per month for parents with a family income below 150 000 NOK (18 750 Euro), 2100 NOK for a family income between 150 000 NOK and 300 000 (37 500 Euro) and 2350 if the family income was above 300 000. Prices have been quite stable after 2008. If we perform a back-of-the-envelope calculation, and assume that the average amount of fee paid in treated city districts was about 2000 per child per month, parental fees covered by the intervention would be 1000 NOK per month, and 11 000 NOK per year (the child should have at least 4 weeks of holiday every year, so a family typically pays for 11 months yearly) per child. In 2012, 4980 four- and five-year-old children were registered as residing in the treated city districts. About 85 % of these children were enrolled in child care. Thus, financing the free/subsidized slots amounted to about 46 563 000 NOK, or 93 % of the total budget.

3 Empirical strategy

We do not have access to individual information on child care use in Oslo before free child care was introduced. Moreover, our data measuring child development is available for the cohorts born 2004 and onwards only.[7] However, for cohorts born 2004–2007 we observe child care enrollment for each child each year, enabling us to look at changes in enrollment

[7]The last cohort not affected by the intervention was born in 2000 for Stovner and 2001 for the remaining treated city districts.

from the child is below the age of eligibility (<4 years old) to it is eligible (>4). We can thus compare child care enrollment in city districts with and without an offer of free child care, before and after the age of eligibility.

We start by estimating a model of the effect of the intervention on child care enrollment. The model is estimated by OLS on a sample of children registered as living in a city district at the beginning of the year they turn four (eight months before they become eligible). Formally, the difference-in-difference approach compares uptake in child care before and after age of eligibility in treated and comparison districts.

$$\text{(1a) } CC_{it} = \alpha_i + \lambda age_t + \delta_4 treatdistrictxage4_{it} + \delta_5 treatdistrictxage5_{it} + \varphi X_{it} + \varepsilon_{it}$$

CC_{it} measures whether child i is enrolled in child care at a certain age t and α_i is a set of dummies capturing any time-invariant child characteristics like the residential city district of the child. The variable age_t is a set of dummies capturing the age of child i (takes the value 0,1,..,5). The variables $treatdistrictxage4_{it}$ and $treatdistrictxage5_{it}$ are dummies set to 1 if the child lived in a treated district (measured before the child is eligible i.e. before age four; see Section 4 for details) and was four or five years old, respectively. Thus the parameters of interest, δ_4 and δ_5, capture the treatment effects, i.e. the difference in child care use at age four and five (compared with the years before) in city districts with intervention (compared with districts without intervention). The vector X_{it} includes observable child and family variables, which reduces to a full set of calendar year dummies when child fixed effects are included. In some models where we do not include child fixed effects, X includes dummies for city district of residence (measured before age four), cohort fixed effects and a number of child and family characteristics (measured before the child is born). ϵ_{it} is an error term with conditional expectation zero.

When we look at effects of the free child care intervention on parental outcomes, we estimate the same model on parental employment and education before and after eligibility age of the child.[8]

An assumption for the above approach to yield causal effects is that the change in child care use (from before to after age four) among children in the comparison districts is a good measure of the counterfactual. If families in comparison districts have a different pattern of child care use before the children are old enough to be eligible for free child care, it suggests that they are not a good measure of the counterfactual. We hence investigate the enrollment at various ages in the different types of city districts. However, the difference-in-differences approach on a balanced panel of observations for the

[8]While we do believe it is an interesting outcome, and a potential mechanism for possible effects on child cognitive development, we have unfortunately not been able to obtain information on parents participation in language courses.

same individual before and after the eligibility age, differences out any time-invariant child characteristics, implying that we depend solely on within child variation in child care enrollment before and after the age of eligibility. Moreover, including child fixed effects in addition, expands this argument to also cover unbalanced panels. Thus, possible compositional changes in the comparison and treatment group over time, which can be a serious concern in the typical application of difference-in-differences on repeated cross sections (i.e. when the the outcome variable is only measured in one time period, like we do in Eqs. 1b and 2b below), is not a concern here.

We might still note, however, that our identification strategy would be challenged if there are, for example, age-specific preferences among parents in different city districts. Even a fixed effect model cannot rule out the possibility that parents in city districts with free child care may prefer the child care start age to be age four unrelated to the intervention, whereas the parents in comparison city districts prefer it to be later (or the other way around). We can explore it to some extent by looking closely at how estimates vary when adding covariates. Such different preferences could be correlated with some observable characteristics (such as decade of immigration and country of origin). If this is the case, estimates may not be stable for the inclusion of certain covariates. While we can never entirely rule out such different age-verifying preferences, it seems far-fetched that they are important in our setting.

Data on child care attendance is only available for the cohorts affected by the intervention, disabling us from applying a difference-in-differences model comparing child care participation of cohorts of children born before the intervention with cohorts of children born after the intervention, across treated and comparison districts. However, we do have access to parental records for cohorts of children born prior to the intervention. As a robustness test we estimate such a classic difference-in-differences model where we include the two last cohorts of parents whose children were not eligible for free child care, i.e. born 2000 and 2001,[9] and hence starting school the year when the intervention was introduced.[10] In this specification we hence include parents with immigrant background having had children over the years 2000–2007.

$$\text{(1b) } Y_{it} = \alpha + \lambda district_i + \delta cohort_t + \beta treatdistrictxpost_{it} + \varphi X_i + \varepsilon_{it}$$

Y_{it} measures income and education of parent i in year t. $district_i$ is a set of dummy variables for each city district of residence of the child (measured before age four). $cohort_t$ is a set of dummy variables for the calendar year of birth of the child (2000–2007). The vector X_i includes covariates (measured before age four), described in the data section.

[9] We could have included even earlier child cohorts, but a municipality reform introduced in 2004 complicates this somewhat. While Oslo prior to 2004 had 25 city districts, it had 15 after this reform.

[10] Note that since children in the city district of Stovner became eligible one year prior to the other city districts, parents with children residing here will be considered treated also when born in 2001.

The variable $treatdistrictxpost_{it}$ is a dummy variable set to one if the child lived in a treated district (before age four) and belonged to the cohorts covered by the intervention (typically 2002-2007). The parameter of interest, β , captures the treatment effect, i.e. the effect on parental outcomes of having a child eligible for the intervention, after the intervention (compared with having a non-eligible child after the intervention).

Furthermore, we are interested in how a potential increase in child care enrollment affects the gap in cognitive achievement between children with and without an immigrant background. A naive but simple model compares test scores at school entry of children from immigrant families living in treated districts with the scores of children from immigrant families in comparison districts.

$$\text{(2a) } Y_i = \alpha + \beta treatdistrict_i + \varphi X_i + \varepsilon_i$$

Y_i is the score of child i on cognitive tests at school entry and $treatdistrict_i$ is a dummy equal to 1 if child i lives in a treated city district (before age 4). X_i is a vector of covariates measured before age four, which includes, for example, cohort and calendar year fixed effects. ϵ_i is an error term with conditional expectation zero.

Due to the before mentioned data limitations, this is a first difference approach, and has two main drawbacks compared with the difference-in-differences approach of Eq. 1a. First, although we can control for a rich set of observable characteristics of children and their families, there might still be selection into city districts along an unobservable dimension, like parents' preferences for education. Second, using the first difference estimate, it is hard to credibly assign differences in test scores to the intervention. As we will see from descriptive statistics, the immigrant families residing in the intervention districts are generally more resourceful than those in the comparison districts. For example, the educational level of the parents is higher, from which we may suspect that these children would have performed better in school regardless of the intervention. Moreover, the intervention districts may also have undertaken other remedial efforts than the intervention, like higher quality of child care or school.[11]

One way to account for differences in school and/or child care quality, is to look at another group of children in the treated city districts that might be affected to the same extent as children from immigrant families. One such group is children without an immigrant background. General differences in child care quality will typically affect children of immigrants and native children in the same direction. By including children without an immigrant background as a second difference in a difference-in-differences model, we can account for other shared characteristics in the different city districts. This impose

[11]Note that these concerns do not apply to the proposed strategy to obtain estimates of child care use. In this case we have pre-eligibility measures of child care use in both groups of city districts since we know whether families used child care for their 1, 2 and 3 year-old, enabling control for any time-invariant child characteristics.

another assumption, namely that free child care does not affect school performance among children without an immigrant background. This may at first seem unlikely given that all children in districts offering free child care are eligible for the subsidized child care. However, there is evidence suggesting that children without immigrant background are not affected by policies like the free child care intervention (see Drange and Telle 2010, Bogen and Drange 2012 and Dustmann, Raute, and Schønberg (2013)). The main reason is that native children have a very high enrollment in child care at all ages, and at ages four and five in particular, regardless of whether child care is free or not. Thus, the intervention is unlikely to affect uptake, possibly ruling out that it improved the development of native children through a higher exposure to child care. If we find that child care use among native children is largely unaffected by the intervention (using Eq. 1a), we can assess the relationship between outcomes of children with and without an immigrant background across city districts with and without intervention using the following specification:

$$\text{(2b) } Y_i = \alpha + \beta district_i + \lambda imm_i + \mu treatdistrictximm_i + \varphi X_i + \varepsilon_i$$

where Y_i is a measure of child i's score on assessment tests at school entry, *imm* is a dummy indicating that the child is from an immigrant family, and *district* is a set of dummies indicating the city district of residence (before age four). $treatdistrictximm_i$ is a dummy equal to 1 if child i is from an immigrant family who lived in a treated district (at the beginning of the year in which she turns four), and μ is thus the parameter of interest. The X_i is a vector of covariates measured the year before the child turns four years old, and are more closely described in the data section. ϵ_i is an error term with expectation zero.

The main identifying assumption is that the difference in test scores between treated and comparison districts for the native children is a reasonable counterfactual for this difference for the immigrant children. There are some institutional features of the Norwegian child care system that support this. First, the municipality of Oslo is the largest owner of child care institutions in both treated and comparison city districts, and hence sets the standard for how the operation of child care centers shall be conducted in all districts. Second, child care institutions are heavily regulated on a national basis, and need to comply with regulations concerning child/teacher ratios, play area and educational content. Still, we cannot rule out that selection into city districts may differ across immigrant and native parents, or other policy initiatives (than free child care) could have affected native children and children with immigrant background differently. If this is the case, and the included controls for background fail to pick this up, our estimates will be biased. We will therefor be more hesitant in giving these results a causal interpretation.

Before we describe the data in the next section, we would like to note two things. First, we could have used $treatdistrictximm_i$ as an instrument for child care enrollment in an

instrumental variable (IV) approach, estimating the effect of enrollment on test scores. This would require, however, that there is no direct effect of the intervention on the test scores. This exclusion restriction can be hard to defend, if, for example, the intervention also affected the language training of the children who attended child care regardless of the intervention. There are some indications that this was the case (Bogen and Drange, 2012), and an IV would then channel all of the overall effect on test scores to the children being enrolled, which suggest that such a scaling of our estimate could involve serious upward bias. Nevertheless, in Section 5.3. we include a brief note on the magnitude of such a rescaled estimate.

Second, we will use standard errors that allow for dependence between observations within city districts, i.e. we cluster on city district. Cameron and Miller (2015) provide an overview of how and when to cluster, and we follow the literature using clustered standard errors with residuals corrected for few clusters ($\sqrt{G/G-1}$) and critical values based on the t-distribution with G-1 degrees of freedom (Brewer, Crossley, and Joyce, 2013). In addition, Cameron and Miller (2015) argue for cluster-specific fixed effects, which we also account for. These adjustments have been shown to be crucial to reduce serious downward biased standard errors in difference-in-difference studies (Cameron and Miller, 2015), though some concern may remain in our case where there are relatively few clusters. In simulations Cameron and Miller (2015) find some over-rejection of the null when there are few clusters, especially when there are no more than 10 clusters. Brewer, Crossley, and Joyce (2013) on the other hand, using a wide range of simulations, find that the bias-adjusted cluster-robust standard error with inference based on t-distribution with G-1 degrees of freedom, which we apply, provides tests of the correct size as long as there are at least 10 clusters. Both papers suggest that the wild cluster t-bootstrap performs similarly well, and Webb (2014) and MacKinnon and Webb (2014) refine this bootstrap procedure to also perform well when the number of clusters is below 10 and when the number of treated and non-treated clusters are very different.[12]

4 Data

4.1 Dataset and variables

To conduct the analysis we employ data from several sources that can be linked through a personal identifier. First, we have access to a unique data set from the municipality of Oslo where all institutional child care use for cohorts born from 2004 and onwards is registered. Second, we have information on the background characteristics of the children and their

[12]Also in our data, the standard errors we present in the paper are substantially larger than classical standard errors not accounting for clustering and few cluster bias. We have also confirmed that our main result remain clearly statistically significant in the wild cluster t-bootstrap procedure; see Cameron, Gelbach, and Miller (2008) for details.

families from registers provided by Statistics Norway. Lastly we have access to 1st and 2nd grade test score records provided by the education authority in the municipality of Oslo.[13]

The sample includes all children (and their parents) born 2004-2007,[14] who lived in one of the intervention or comparison districts of Oslo. The child's city district of residence is defined by a dummy variable taking the value 1 if the child lives in this city district by the start of the calendar year it turns four (i.e. eight months before it becomes eligible for the free child care in August of the same calendar year). Since three of the districts are geographically located quite far from the intervention districts, and since they also differ along demographic and socioeconomic dimensions (see Appendix Table 8), we include 12 of Oslo's 15 city districts in the main analytic sample. Five of the districts are in the treatment group whereas seven are in the comparison group. We will perform robustness checks to ensure that our main results are robust to the choice of districts in our comparison group.

The three sets of outcome variables (enrollment, parental employment and education, and children's test scores) are defined as follows. Enrollment in child care at a given age is defined by a dummy set to 1 if a child is enrolled 31st of December the calendar year it turns 0 (very few is enrolled in child care at this age), 1, 2, 3, 4 and 5 years old respectively (referred to as enrolled at age 0–5 in the text).[15]

Parental employment defined by whether the mother/father earns above a certain threshold. We construct dummy variables for whether the mother/father has positive earnings, and whether the mother/father earns more than 1, 2, 4 and 6 times the "basic amount", labeled G, in the Norwegian Social Insurance Scheme. Following previous studies, these dummies are set to capture any employment (1G), part time (2G), full time (4G) and full time for high earners (6G), respectively.[16] Education measures are constructed as dummies; and set to 1 if the mother (father) has finished high school, and another set to one if the mother (father) has finished college. Education is missing for some of the immigrants who did not undertake the education in Norway.[17]

Measures of children's test scores are retrieved from nationwide tests in reading and

[13] While these tests are taken nationwide, test results are to the best of our knowledge only available for children in the municipality of Oslo, and for cohorts born 2004 and onwards.

[14] Due to a restrictive storage policy in the municipality, data on children born in January and February 2004 were deleted from the application data base before we got access to it. We are therefore not able to include these children in our sample.

[15] If a child (or its parent) is no longer living in Norway, its observations are then set to missing. Very few children attend a child care center in another city district than the one they reside in, but as long as the center is situated in Oslo the enrollment will be included in our data. If the child attends a child care center in another city, which is very rare, we will not be able to register the enrollment.

[16] Employment status is available for every year that a person resides in Norway.

[17] A considerable share with missing on educational achievement is not surprising for these parents, since they are born outside Norway. Moreover, survey data indicate that education of immigrants is not well captured and typically under reported in register data (Blom and Henriksen, 2008).

mathematics during their first (age 6-7) and second (age 7-8) year in school.[18] The tests are meant to identify weak students in order to secure that the school allocates sufficient compensating resources to these children (by law, students this age cannot fail class in Norway). Hence most children score close to the maximum of 105 points (reading) and 50 (mathematics).[19] The threshold is set on the basis of the nationwide sample to capture the bottom 20 percent. We have access to test scores and to the threshold for the different tests. In mathematics there is defined one threshold for the entire test, and we set a dummy to one if the child scores better than the threshold (denoted *>limit* in tables). In reading there are several thresholds for different parts of the test. The reading dummy is constructed as scoring better than the threshold in all parts of the test (denoted *>limit* in tables). In addition we normalize the actual score on both tests (mean zero and standard deviation 1, labeled *reading* and *mathematics*). As a summary measure of cognitive skills, we also consider the unweighted average of the standardized test scores in language and mathematics (labeled *Score*). Finally we set a dummy to one if the child scores better than the threshold in both reading and mathematics (denoted *>limit* in tables).

Based on the data sources, we construct a number of variables capturing child and family characteristics. We define children with an immigrant background by a dummy set to one if the child is born in or outside Norway, with both parents born outside Norway, or it is born in Norway with four foreign-born grandparents (zero otherwise). All children not in this category will be denoted *native* or *without immigrant background.*

In regressions of Eq. 1a that are run without child fixed effects, where we estimate effects on child care enrollment and parental employment/education, the following covariates are measured the year before the child is born to secure that they are not endogenous to child care enrollment and employment/education in the pre-eligibility period. Similarly, in regressions of Eq. 2 includes covariates are measured the year before eligibility. As children born early in the year are older when graduating, we add dummies for quarter of birth. We also add a dummy capturing if the child has a younger sibling, as well as a gender dummy. For both parents, we construct a dummy to capture if the mother (father) was younger than 22 when she (he) had the child. Furthermore, we include two measures of employment for each of the parents; a dummy capturing if the mother (father) had positive earnings, and a covariate measuring linear earnings. In addition we construct a dummy measuring whether the mother (father) received welfare support. To measure the parents' education, we construct a dummy set to 1 if the mother (father) has finished high school and college respectively, and in addition a dummy capturing if the mother (father)

[18] Children born 2007 have not yet completed 2nd grade, and the 2nd grade outcome is hence not available for this cohort. Moreover, test scores will be missing if, for example, the child no longer lives in Oslo or the result of the test is for some other (rare) reason not registered.

[19] For the cohort born 2007 the test were somewhat different, and the maximum score was 77 points on the language test and 50 points on the maths test. We account for this by including cohort fixed effects in our regressions.

has a missing observation on education. To account for single parenthood we construct a dummy that captures if the parents are living together. We also include dummies capturing the mother's country of origin. Finally we include a covariate measuring the number of years that the mother has been residing in Norway. This is constructed as a dummy for the decade that the mother immigrated to Norway the first time.

4.2 Summary statistics

In column 2-3 in Table 1 we report summary statistics for the background characteristics of children included in the estimation sample we employ to explore enrollment and parental outcomes: Children from immigrant families in intervention and comparison city districts. The fourth column in the table reports the difference between the background characteristics of children in the two groups. In the subsequent columns we report the overall statistics for the population (for the same cohorts of children, but also including natives) in the intervention and comparison city districts.

Turning first to the children with immigrant background,we see that the share of the children who are immigrants themselves is slightly lower in treatment districts. Parental income in treatment districts is somewhat higher, and the share of mothers with a high school degree is higher. The share of parents with unknown education is high in both groups, but lower in the treatment districts. The share of fathers who has completed high school seems to be rather similar. There is also a difference between the two groups of city districts when it comes to whether the parents live together. Overall, it becomes clear that children with an immigrant background residing in the treated city districts on average come from a somewhat more resourceful background. Turning to the last three columns of the table, we see that this picture changes when we look at the entire population of children in the relevant cohorts. The share of children with an immigrant background is about 56 percent in intervention districts, and 24 percent in comparison districts. Income and education for both mothers and fathers are clearly lower in intervention city districts. Given the high share of immigrants that tend to earn less and have a lower educational level than the general population (Bogen and Drange, 2012), this might not be too surprising.

Table 1: Summary statistics for the children with immigrant background only (and their families)

	Immigrant population			Entire population		
	Intervention	**Comparison**	**Difference**	**Intervention**	**Comparison**	**Difference**
Girl	0,478	0,494	-0,015	0,483	0,495	-0,012
Has younger sibling(s)	0,447	0,500	-0,053	0,430	0,447	-0,017
Immigrant	0,076	0,153	-0,077	0,042	0,037	0,006
Parents are immigrants	0,795	0,787	0,008	0,443	0,189	0,254
Grandparents are immigrants	0,129	0,060	0,069	0,072	0,014	0,057
Mother has a high school degree	0,250	0,203	0,048	0,457	0,692	-0,236
Mothers education unknown	0,508	0,612	-0,104	0,321	0,192	0,129
Mother income	129110	98984	30127	167723	242102	-74379
Mother working	0,609	0,471	0,138	0,703	0,806	-0,103
Mother on welfare	0,008	0,004	0,005	0,007	0,004	0,004
Mother <22 at first birth	0,266	0,262	0,004	0,192	0,099	0,093
Mother and father are living together	0,835	0,670	0,165	0,572	0,557	0,015
Father has a high school degree	0,324	0,332	-0,009	0,472	0,686	-0,215
Fathers education unknown	0,352	0,398	-0,046	0,253	0,168	0,085
Father income	321739	258052	63687	265612	342055	-76444
Father working	0,895	0,791	0,104	0,848	0,861	-0,012
Father on welfare	0,030	0,054	-0,023	0,015	0,012	0,003
Father <22 at first birth	0,078	0,064	0,013	0,059	0,033	0,027
N	5721	3229		10266	13441	

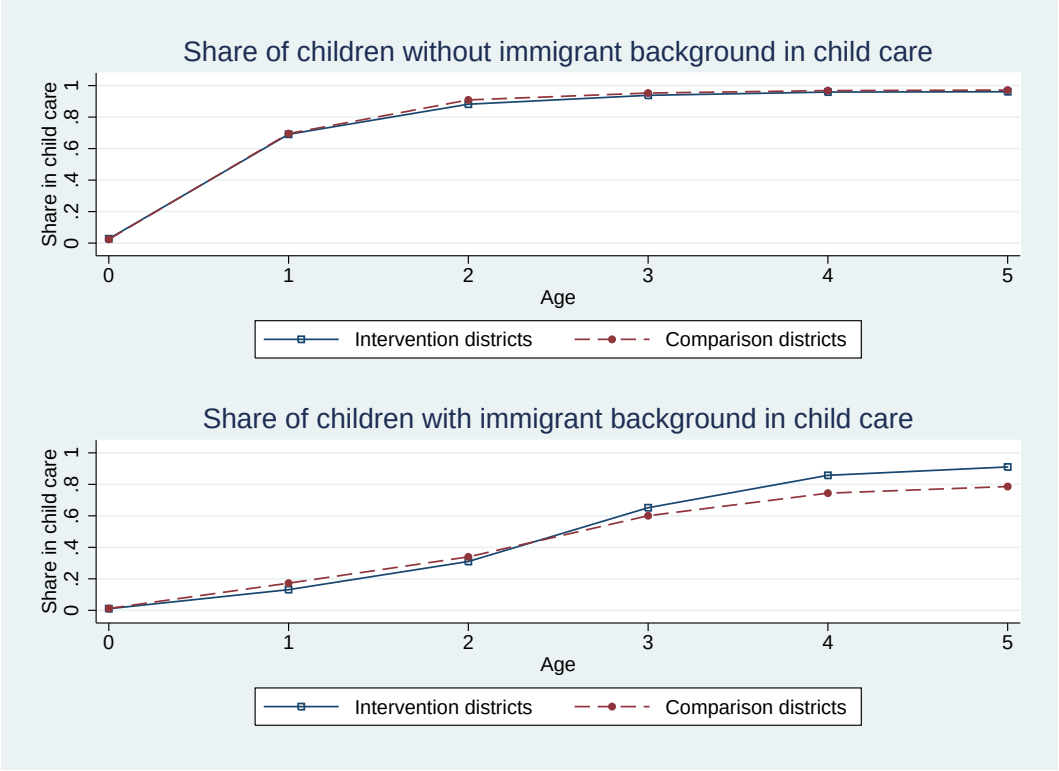
5 Results

5.1 Effects on enrollment in child care

We start by looking at how the share of children in child care centers varies between treated and comparison districts, and between families with and without an immigrant background. The top panel of Figure 1 shows how child care enrollment for children with a native background varies with child age across treatment (line with circle symbols) and comparison (line with square symbols) districts. Very few children are enrolled in child care by the end of the children's birth year. However, already by the end of the calendar year the child turns one, more than 60 percent of children are enrolled in child care. There is little evidence of differences in child care use across treatment and comparison districts for native children. The bottom panel of Figure 1 displays the corresponding figures for children with an immigrant background. Notably we see that the pattern among young children differs considerably from the pattern we observed in the top panel. Child care use among children with an immigrant background is much lower at early ages compared with children with native background. Child care use among children from immigrant families, however, does not seem to differ much across treatment and comparison districts until the child turns four and five. During these years there is a higher use of child care

in treated city districts in line with what we would expect if the offer of free child care succeed in recruiting a higher share of children. Remember that we did not see such a difference in the top panel, suggesting that the offer of free child care does not affect the child care use among native families.

Figure 1: Enrollment in child care



We now turn to Table 2 where we report results from a regression where the dependent variable is whether the child is enrolled in child care at ages four and five respectively. The difference in differences model (cf. Eq. 1a) corresponds to the difference in Figure 1 between the line with circles and the line with squares before and after the free child care becomes available at age four. In Table 2 we report results from specifications with child fixed-effects, and results are reported separately at age four and five (compared with before age four). In other words, we rely on within-child variation in child care use before and after eligibility age in city district with and without free child care. From the first column, where we restrict the sample to children from immigrant families, it is clear that there are significant differences in child care use before and after age four across treatment and comparison districts — in line with what we would expect if free child care is a successful way of recruiting children to child care centers. The offer of free child care increases enrollment by about 11.5 percentage points, which corresponds to a relative increase of about 15 percent (given a counterfactual baseline similar to the child care use of about 75 percent in the comparison districts at age four and five).

Turning to robustness checks, we first explore whether there might be some differ-

ences inside the comparison or treatment districts that affect uptake at age four and five, unrelated to the intervention. To the extent, however, that such age-related changes are affecting both native and immigrant families, we can remove this variation by taking the difference between these two groups of families. As seen in Figure 1, the child care use for children with native backgrounds looked very similar across treatment and comparison districts. From Column 2 of Table 2, where we look at children with a native background, we see that native children in treatment and comparison districts have a very similar pattern of child care attendance before and after eligibility age, though there is indication of a slightly - but economically uninteresting - higher share of enrolled four- and five-year-olds in treated city districts. Given this very small effect for the native children, we would not expect results from such a difference-in-difference-in-differences model to affect our main results much. This is also what we find in Column 3 of Table 2.

Table 2: Effect of intervention on child enrollment in child care at age four and five

	With imm. background	**Without imm. background**	**Diff-in-diff-in-diffs**
Four years	0.115*	0.014+	0.104*
	(0.022)	(0.008)	(0.022)
Five years	0.119*	0.015+	0.108*
	(0.023)	(0.007)	(0.023)
N	51253	84451	135704

Note: Each column provides main results from one regression. Sample is children with immigrant background only in first column; children with native background only in second column; and all children in third column. All estimates are based on model given in Eq. 1a (child fixed effects). Standard errors allowing for dependency within districts (clustered on city district) in parentheses. + p<0.10, * p<0.05

Second, the main results are remarkably consistent if we omit the child-fixed effects and instead include varying number of covariates (see Appendix Table 9 for details), suggesting that unobserved characteristics are not creating bias. This strengthens our trust in the pattern we already have seen in Figure 1, that the intervention in fact did increase child care attendance among children with an immigrant background. Third, as discussed above, is not obvious what city districts to include in the comparison group. Therefore, we have checked that our results are robust to including alternative districts in the comparison group. Specifically, we have included all non-treated city districts in Oslo in the comparison group, as well as the districts with 20 percent and higher share of children from immigrant families. Results are very similar across these variations (see Appendix Table 10 for results). As a final robustness check we implement a specification with a placebo intervention for two- and three-year-olds as well, where we interact age two and age three dummies with residing in intervention city districts. Neither the age two nor the age three estimate is significantly different from zero at the five percent level (see last column of Appendix Table 10 for results).[20]

[20]Though not even statistically significant at the ten percent level, the age three estimate may be

5.2 Child care enrollment across sub-samples

We turn now to Table 3 where we explore how enrollment in child care differs across children from various backgrounds. We find, in general, little evidence that take-up varies by sub-groups, although there seems to be a tendency for a higher take-up rate among children with non-working mothers.[21] This is in line with what we would see if working mothers already have enrolled their children in child care. If this is the case, free child care might be less likely to affect enrollment.[22]

Table 3: Differences in enrollment across sub-samples

	Share	Four years		Five years		N
		b	(se)	b	(se)	
Girl	0,48	0.115*	(0.022)	0.110*	(0.023)	24855
Boy	0,52	0.115*	(0.024)	0.127*	(0.024)	26398
Mother not working	0,49	0.131*	(0.020)	0.153*	(0.024)	24670
Mother working	0,51	0.099*	(0.028)	0.088*	(0.030)	25218
Mother no high school	0,52	0.111*	(0.042)	0.125*	(0.023)	12387
Mother has high school	0,48	0.086*	(0.033)	0.108*	(0.044)	11374
Family income<median income	0,49	0.118*	(0.016)	0.132*	(0.021)	24359
Family income>median income	0,51	0.106*	(0.036)	0.107*	(0.037)	25529
No younger siblings	0,53	0.107*	(0.022)	0.120*	(0.023)	27361
Has younger siblings	0.47	0.125*	(0.025)	0.121*	(0.028)	23892

Note: Each line provides main results from one regression. Sample is children with immigrant background. All estimates are based on model given in Eq. 1a (child fixed effects). Standard errors clustered on city district in parentheses, + p<0.10, * p<0.05

5.3 Effects on test scores?

We now proceed to the analysis that aims to get closer to answering whether the provision of free child care affects subsequent child cognitive outcomes. In Table 4 we see that the children of immigrants in treated city districts do better at 1st grade tests than do children of immigrants in comparison districts. The columns show results in reading (standardized scores), the likelihood of scoring above the critical threshold in reading,

considered of noteworthy magnitude (0.06). Note, however, that a slightly higher enrollment rate at age three in intervention city districts will scale down the age four and age five estimates in our preferred specification.

[21] Drange and Telle (2010) found that another intervention of free child care affected girls grades positively, but they found no effect for boys. Their data did not allow for studying the take-up of free child care, and hence they could not distinguish whether the positive effect for girls was related to a possible higher take-up rate, or whether the enrollment in child care was just not affecting the school performance of the boys. In the table, we see that take-up rates do not differ by gender, suggesting that girls and boys are as likely to enroll in child care due to the intervention.

[22] We have also estimated similar models for the seven largest immigrant groups (by mother's country of origin). We find strong enrollment effects on children with a mother from Morocco, Somalia and to some extent Pakistan and Iraq, whereas we find little evidence of effects on enrollment of children with a mother from Turkey, Sri Lanka or Vietnam. Since these sub-samples of children can be small, we should, however, interpret the results with caution.

math results (standardized scores), the likelihood of scoring above the critical threshold in mathematics, the average of the score in reading and mathematics (standardized) and, lastly, the likelihood of scoring above the critical threshold in both reading and mathematics. For each line we add controls for observable characteristics cumulatively, and we see that this reduces the standard errors, but the point estimates remain similar. With all covariates included (last line) we see that children in intervention city districts score about 10 percent of standard deviation better on the first grade tests in reading and mathematics. There are also signs of an increase in the share of children scoring above the critical threshold, from the last column we see that children in intervention city districts are about 5 percentage points more likely to score above the threshold in both reading and mathematics.[23]

Turning now to the second grade outcomes in Panel B, we see that differences are somewhat smaller, but children in intervention city districts are still performing significantly better on the assessment tests both in reading and mathematics. There are also a higher share of children scoring above the critical threshold in both subjects.

As discussed in the empirical strategy section, these results can only be given a causal interpretation under very strong assumptions (unconfoundedness). Thus, the observed differences in early cognitive skills among children in treated and comparison city districts might be caused by other factors than the intervention, for instance unobserved characteristics of the children or differences in the quality of child care centers or schools. By including native children in a difference in difference analysis we can remove such differences between treatment and comparison districts that are similar for children with native and immigrant backgrounds. Since the intervention did not affect take-up of native children (cf. Column 2 of Table 2), we can arguably also assume that it did not have an effect on the test scores of native children. In Panel D in Table 4 we look at how child cognitive outcomes are affected by the intervention in a difference-in-differences model. For native children in Panel C, test scores are similar across treatment and comparison districts — as we would expect since the intervention did not affect their enrollment in child care. Thus, the difference-in-differences estimates in Panel D confirms our findings from Panel A, that children with immigrant background in treated districts do better than similar children in comparison districts. When we compare with the estimate in Panel A, we see that the results are very similar. If there are systematic differences in child care or school quality in the different groups of districts, this should be accounted for in this difference-in-difference analysis. The estimates in Panel D thus support a causal interpretation of the findings from Panel A.

How should we interpret the economic significance of the findings in Table 4? Keeping in mind the uncertainty related to whether we manage to isolate the causal effect, we

[23]These findings are robust to variations in the districts used in the comparison group; see Appendix Table 11.

should be cautious. Moreover, if we assume that the entire effect on test scores goes through the channel of increased enrollment, we might want to consider scaling the point estimates by the take-up rate. Assuming one channel and causality are both rather bold in this context, so we should interpret the exercise as providing us with an upper bound to the results. Easiest to interpret is perhaps the likelihood that the child scores better than the threshold (denoted *>limit* in tables). The point estimates in the last column of Table 4, Panel D, show that children in treated city districts are about .04 percentage points more likely to score above the limit in both reading and mathematics. Scaling by the take-up (about 0.115 percentage points) leaves us with an estimates of about 0.35. In non-treated city districts, the gap between children with and without an immigrant background is .31. Thus, according to the upper bound, the intervention more than closes the gap between children with and without an immigrant background after scaling by the take-up. Keeping in mind that this interpretation rests on two strong assumptions, we should be very cautious in concluding further in this matter. Firstly, we know that other aspects of the intervention, such as courses for parents and a pedagogical content more tailored towards the needs of children with an immigrant background, may have affected other children than the ones recruited by the intervention. Furthermore, all children in treated city districts got reduced child care fees. If, as some studies suggests, increased family income in itself may affect child development, scaling by the uptake provides an inflated estimate.[24] Lastly, as already discussed in Section 3, we cannot rule out that the effect estimates in Table 4 are biased due to the fact that we do not have access to pre-reform outcomes.

[24]Dahl and Lochner (2012) find evidence that child development is affected by increases in family income. Black, Devereux, Løken, and Salvanes (2014) study a subsidy cut-off in Norway, and find that children in families with incomes just below the cut-off (i.e. receiving a larger subsidy) perform better in junior high school.

Table 4: Differences in test scores among children of immigrants in treated and comparison districts

Panel A: First grade outcomes						
Adding covariates:	Reading	>limit	Math	>limit	Score	>limit
Birthyear only	0.114	0.039	0.128+	0.045	0.132+	0.061
	(0.067)	(0.031)	(0.067)	(0.027)	(0.070)	(0.038)
Child characteristics	0.127+	0.046	0.145*	0.050+	0.147*	0.069+
	(0.059)	(0.028)	(0.060)	(0.024)	(0.060)	(0.034)
Mother characteristics	0.114*	0.040	0.131*	0.046+	0.133*	0.062+
	(0.050)	(0.024)	(0.053)	(0.021)	(0.051)	(0.030)
Father characteristics	0.095+	0.037	0.116*	0.040+	0.112*	0.056+
	(0.044)	(0.023)	(0.048)	(0.020)	(0.044)	(0.027)
Mothers continent of origin and imm. decade	0.098*	0.039	0.106*	0.036+	0.111*	0.056*
	(0.037)	(0.023)	(0.045)	(0.019)	(0.037)	(0.025)
N	6639	6639	6641	6641	6605	6605
Panel B: Second grade outcomes						
b	0.055+	0.037*	0.086*	0.020*	0.067*	0.034*
(se)	(0.028)	(0.012)	(0.037)	(0.008)	(0.029)	(0.015)
N	4638	4638	4673	4673	4616	4616
Panel C: Children without immigrant background						
b	-0.005	-0.007	0.032	0.004	0.005	-0.001
(se)	(0.024)	(0.013)	(0.042)	(0.011)	(0.029)	(0.017)
N	11114	11114	11118	11118	11098	11098
Panel D: Difference-in-difference including children with and without immigrant background						
b	0.103*	0.025	0.092*	0.019	0.114*	0.040+
(se)	(0.035)	(0.023)	(0.030)	(0.015)	(0.035)	(0.019)
N	17753	17753	17759	17759	17703	17703

Note: Each column and line provide main results from one regression, and for each line new control variables are added. Sample is children with immigrant background only in Panel A and B; children with native background only in Panel C; and all children in Panel C. Estimates in Panels A, B and C are based on model given in Eq. 2a, and in Panel C on model given in Eq. 2b (difference-in-differences). Standard errors clustered on city district in parentheses, + p<0.10, * p<0.05

As we have already discussed, free child care might affect children differently depending on their background. Previous studies typically find that children from families with low income or low education benefit from child care experience (see, for example, Havnes and Mogstad (2011b)). We consider a sub-sample analysis in Table 5 below. We see that the associations are stronger for girls at the critical threshold margin in reading. But although estimates are not similarly strong for boys they still point in a positive direction. Furthermore, we see that results are driven by children with mothers who are not attached to the labor force. This result could be related to the larger take up effects found for such children in Table 3, though the result would also occur if attending child care is more beneficial for these children. The same pattern can be observed for children whose family income is respectively below vs above the median. Again as with uptake in Table 3, it appears to be a large estimate for children whose family income is below the median. Given the large standard errors in the sub-sample table, as well as the similar

patterns across groups for the uptake effects in Table 3, we want to be cautious with conclusions. However, we note that our estimates point in the same direction as previous findings in the literature — in general there is a stronger effect of child care experience for children from less resourceful families (Almond and Currie, 2011).

Table 5: Differences in test scores among children of immigrants in treated and comparison districts, sub samples

	Share	Reading	>limit	Math	>limit	Score	>limit
Girl	0.50	0.098*	0.049*	0.129*	0.035	0.121*	0.062+
		(0.044)	(0.019)	(0.054)	(0.025)	(0.048)	(0.029)
Boy	0.50	0.105*	0.026	0.086	0.036	0.106+	0.048
		(0.046)	(0.038)	(0.053)	(0.022)	(0.048)	(0.037)
Mother not working	0.40	0.161*	0.030	0.157*	0.040*	0.177*	0.058*
		(0.041)	(0.022)	(0.033)	(0.016)	(0.030)	(0.020)
Mother working	0.60	0.042	0.046+	0.063	0.033	0.055	0.052
		(0.038)	(0.026)	(0.057)	(0.021)	(0.047)	(0.030)
Mother not finished high school	0.55	0.043	0.074*	0.036	0.033	0.037	0.075*
		(0.061)	(0.031)	(0.053)	(0.029)	(0.054)	(0.032)
Mother has finished high school	0.45	0.042	0.012	0.107	0.044	0.068	0.041
		(0.044)	(0.031)	(0.068)	(0.028)	(0.051)	(0.036)
Family income<median income	0.50	0.120*	0.040	0.104*	0.031	0.128*	0.065*
		(0.040)	(0.023)	(0.040)	(0.020)	(0.037)	(0.024)
Family income>=median income	0.50	0.045	0.037	0.077	0.035+	0.060	0.039
		(0.038)	(0.031)	(0.057)	(0.019)	(0.044)	(0.031)
No younger siblings	0.55	0.114*	0.042	0.136*	0.042*	0.131*	0.063*
		(0.045)	(0.029)	(0.042)	(0.014)	(0.040)	(0.027)
Has younger siblings	0.45	0.086	0.034	0.074	0.030	0.093	0.047
		(0.053)	(0.025)	(0.061)	(0.025)	(0.058)	(0.029)

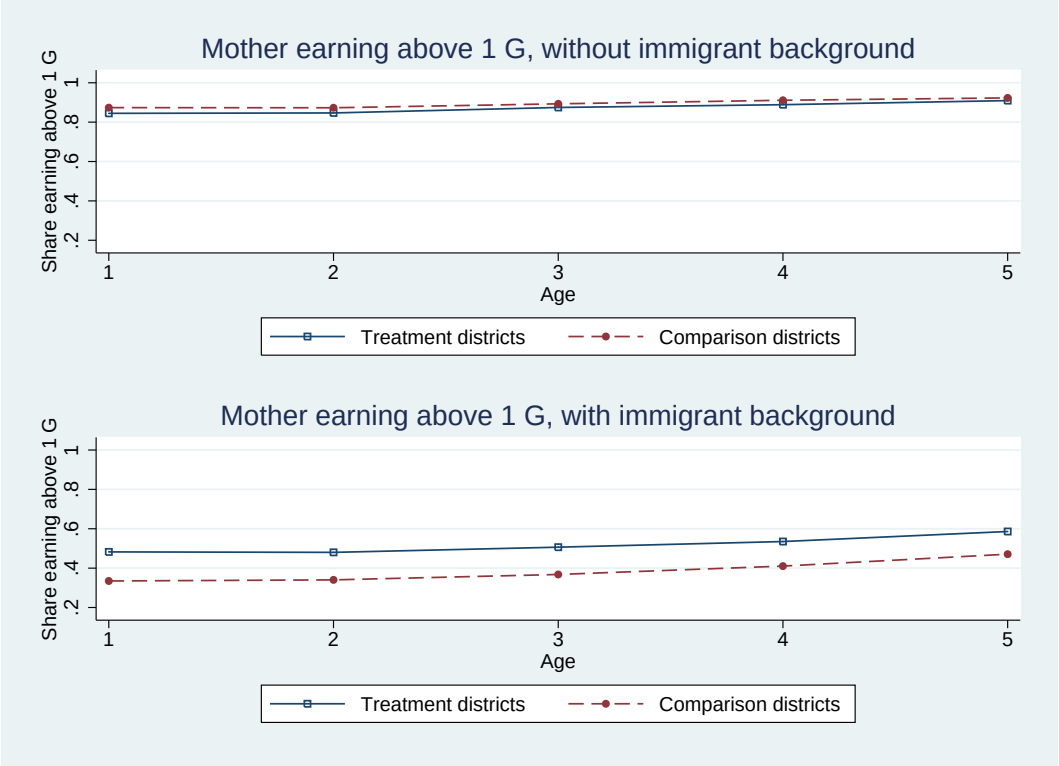
Note: Each column and line provide main results from one regression. Sample is children with immigrant background. All estimates are based on model given in Eq. 2a. Standard errors clustered on district in parentheses, + p<0.10, * p<0.05

5.4 Parental outcomes

Keeping in mind that increased child care enrollment might also affect parental employment and/or education, we now turn to explore how parents fare when their children become eligible. The top panel of Figure 2 shows how labor force participation of native mothers varies with child age across intervention (line with circle symbols) and comparison (line with square symbols) districts. There is little evidence of differences in maternal labor force attachment across intervention and comparison districts for native children, although it seems that mothers without immigrant background are slightly less likely to be attached to the labor force if they reside in an intervention districts. The bottom panel of Figure 2 displays the corresponding figures for mothers with an immigrant background. While there seems to be a tendency for both groups of mothers to become more attached to the labor force as their child grows older, there are no clear differences around eligibility

age. If anything, there might be a sign of a slightly larger increase in attachment among mothers in the comparison districts at ages four and five, contradictory to what we would expect if free child care induced mothers to work more.

Figure 2: Mothers' labor force participation, measured as earning more than one G



In Table 6 we compare parental outcomes before and after eligibility age of their child, in districts with and without free child care. Panel A reports results for whether mothers earn more than 1, 2, 4 and 6 times the basic amount G (see Section 4 for details). It is clear that the free child care intervention did not succeed in securing a higher participation in the labor force. If anything, it seems like eligible mothers are slightly less likely to participate in the labor market (1G) when their child becomes four and five years old and becomes eligible for the free child care. A similar pattern is observed in Panel B. Fathers are not more likely to work more if they have an eligible child after eligibility. Neither the mother nor the father are more prone to finish education on high school or college lever, as observed in Panel C. It seems, thus, that offering free child care and some activities for parents is not sufficient to spur a detectably higher labor force attachment or education for the parents with eligible children. We should, however, note that we only observe completed parental education the year the child turns five. It could be that completing education takes a somewhat longer time, and that we might be able to see an effect when we can include a longer time span in the analysis.

Table 6: Parental outcomes

Panel A: Mothers' labor force participation				
	> 1G	**> 2G**	**> 4G**	**> 6G**
4 years	-0.015	-0.007	0.007	0.006
	(0.011)	(0.011)	(0.009)	(0.004)
5 years	-0.027+	-0.011	0.002	0.002
	(0.015)	(0.011)	(0.011)	(0.006)
Panel B: Fathers' labor force participation				
	> 1G	**> 2G**	**> 4G**	**> 6G**
4 years	-0.004	-0.011	-0.008	-0.005
	(0.008)	(0.011)	(0.013)	(0.007)
5 years	-0.011	-0.019	-0.008	-0.006
	(0.012)	(0.013)	(0.015)	(0.008)
Panel C: Parental education				
	M HS	**M College**	**F HS**	**F College**
4 years	0.003+	0.001	0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
5 years	0.003	0.000	0.001	0.002
	(0.002)	(0.003)	(0.003)	(0.002)
N	29653	29653	33828	33828

Note: Each panel provides results from four regressions on the given outcome variable. Sample is parents of children with immigrant background. All estimates are based on model given in Eq. 1a (child fixed effects). Standard errors clustered on district in parentheses, + p<0.10, * p<0.05 N=42888 in Panel A and B.

In Table 7 we show results from robustness checks. In Panel A–C we display results that mirrors results in Table 6, but where native parents are included in a difference-in-difference-in-differences model (similar to the last column of Table 2). We see that results in Panel A–C largely confirm the findings in Panels A–C in Table 6. There is a small but imprecisely estimated negative effect for low earning for mothers. Results for fathers rule out any negative effect. There is also quite precise zero effects on education. The only estimate that stands out as different from the findings in Table 6, is a small, positive effect on very high earnings (6G). Turning now to the last two Panels, we consider how the results hold in a difference-in-differences model over cohorts as described in Eq. 1b. Estimates are small and not significant, except for small, negative effect of fathers earning more than 6G. Overall, we do not find strong support for robust effects on parental outcomes.

Table 7: Parental outcomes: Robustness

Panel A: Mothers' labor force participation, diff-in-diff with natives				
	> 1G	**> 2G**	**> 4G**	**> 6G**
4 years	-0.016	-0.019	-0.011	0.017*
	(0.013)	(0.013)	(0.013)	(0.006)
5 years	-0.033	-0.023	-0.006	0.021+
	(0.019)	(0.016)	(0.015)	(0.009)
Panel B: Fathers' labor force participation, diff-in-diff with natives				
	> 1G	**> 2G**	**> 4G**	**> 6G**
4 years	0.005+	0.003	-0.001	-0.008*
	(0.003)	(0.003)	(0.006)	(0.004)
5 years	0.004	0.001	-0.003	-0.007
	(0.004)	(0.004)	(0.005)	(0.008)
Panel C: Parental education, diff-in-diff with natives				
	M HS	**M College**	**F HS**	**F College**
4 years	0.002	0.001	0.004*	0.001
	(0.001)	(0.002)	(0.001)	(0.001)
5 years	0.002	0.001	0.003*	0.000
	(0.002)	(0.003)	(0.001)	(0.002)
N	100852	100852	101936	101936
Panel D: Labor force participation, before and after intervention				
	> 1G	**> 2G**	**> 4G**	**> 6G**
Mother	-0.022	-0.012	-0.003	0.001
	(0.013)	(0.010)	(0.009)	(0.006)
Father	-0.006	-0.010	-0.023+	-0.020*
	(0.012)	(0.010)	(0.011)	(0.008)
Panel E: Education, before and after intervention				
	M HS	**M College**	**F HS**	**F College**
	0.004	-0.003	0.003	0.003
	(0.008)	(0.007)	(0.007)	(0.004)
N	18284	18284	20576	20576

Note: Each panel provides results from four regressions on the given outcome variable. All parents are included in panels A–C, whereas panels D–E are based on a sample with parents of children with immigrant background. Estimates are based on model given in Eq. 1a (child fixed effects) in panels A-C and Eq. 1b in panels D–E. N=115 985 in Panels A–B and N=24 155 in Panel D. Standard errors clustered on city district in parentheses, + p<0.10, * p<0.05

6 Conclusion

We estimate effects of an intervention offering child care free of charge for four- and five-year-olds in several city districts in Oslo in a situation where child care was heavily subsidized and not rationed. The main purposes of the intervention was to recruit children from immigrant families to care centers and provide them with systematic language stimulation, as well as to raise awareness among their parents of the importance of language development and the crucial role played by the child care institution in promoting such development. Taking advantage of the fact that the policy was introduced in

certain districts and for children of a certain age, leaving other districts and age groups unaffected, we find that the offer of twenty hours of free childcare weekly for four- and five-year-olds, succeeded in increasing child care enrollment of children with immigrant background by almost 12 percentage points. The gap in enrollment between immigrant and native children in comparison city districts amounted to 0.22 and 0.19 percentage points for four- and five-year-olds. If the enrollment gap in intervention and comparison city districts is a good counterfactual of enrollment gap in in the intervention districts, this implies that the intervention more than halved the attendance gap between children with and without immigrant background.

Children without an immigrant background were not affected by the intervention, most likely a result of very high enrollment rates already at age three for this group. We consider several specifications to explore possible effects on parental labor force attachment and education, and we are not able to document that the intervention succeed in engaging parents in work or education activities. Looking at children's first and second grade test scores, we find that children in city districts with free child care perform better than children in comparison districts in both reading and mathematics. Results are stronger for children from a disadvantaged background. We perform a number of robustness tests, and results are consistent. Children in intervention districts are about 4 percentage points more likely to score above the concern-threshold in reading and mathematics, compared to a group of native children in the two groups of city districts. Making the strong assumption that our estimate for test scores is representative of the causal effect, as well as assuming that this entire effect was channeled through the increase in enrollment, we can obtain an upper bound of the effect. Then children in treated city districts were about 0.04 percentage points more likely to score above the concern-threshold in both reading and mathematics. Scaling up with the take-up (about 0.115 percentage points) leaves us with an estimates of about 0.35. In non-treated city districts, the gap between children with and without an immigrant background was 0.31. Thus, according to the upper bound, the intervention more than closed the gap between children with and without immigrant background in the share with concerningly poor scores in reading and mathematics. As we strongly suspect that other aspects of the intervention, such as courses for parents and a pedagogical content more tailored towards the needs of children with an immigrant background, may have affected other children than the ones recruited by the intervention, we expect the true effect of the intervention to be smaller. The lower bound in this context — still maintaining the strong assumption that we can give the difference-in-difference estimate a causal interpretation — indicates that the intervention closed about 13 percent of the achievement gap between children with and without an immigrant background.

Our results suggest that free child care can help bridge the gap in formal child care attendance between native and immigrant groups. The Norwegian setting under which the program was introduced, is, however, different from setting elsewhere in OECD coun-

tries. The child care was and is of high quality and heavily subsidized, yet there was no real rationing of slots. The families who did not send their children to care under these very generous conditions, may be a group with particular characteristics. For this group, it might be that the fact that the child care was entirely free could have been crucial. Moreover, the active recruitment efforts of the municipal public servants may also have been important and possibly more difficult to replicate elsewhere. While these characteristics of the intervention may make it hard know exactly how offering free child care will affect uptake and child development in other settings, our results do demonstrate that a voluntary public policy can be effective in recruiting children to child care. Reaching these children might be crucial to improve their development and to reduce societal segregation in the long run.

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Appendix Tables

Table 8: City district characteristics

City district	Treat	Comparison	Share immigrants	Father mean income	Number of children
Gamle Oslo	Partly	Partly	.46	160888	1026
Grünerløkka	No	Yes	.36	162743	676
Sagene	No	Yes	.29	157062	411
St. Hanshaugen	No	Yes	.20	157619	214
Frogner	No	No	.19	168399	280
Ullern	No	No	.10	222445	165
Vestre Aker	No	No	.08	264494	203
Nordre Aker	No	Yes	.10	175507	258
Bjerke	Yes	No	.45	208179	793
Grorud	Yes	No	.57	221005	826
Stovner	Yes	No	.65	239608	1056
Alna	Yes	No	.61	238279	1547
Østensjø	No	Yes	.25	217990	620
Nordstrand	No	Yes	.11	218337	272
Søndre Nordstrand	Yes	No	.55	225358	1251

Table 9: Effects of free child care provision on enrollment in child care

	Model 1	Model 2	Model 3	Model 4	Model 5
4 years	0.115*	0.113*	0.113*	0.113*	0.113*
(se)	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)
5 years	0.116*	0.115*	0.115*	0.115*	0.115*
(se)	(0.021)	(0.021)	(0.021)	(0.021)	(0.020)
Covariates:					
Birth year	x	x	x	x	x
Child characteristics		x	x	x	x
Mother characteristics			x	x	x
Father characteristics				x	x
M. country of origin & imm. decade					x

Note: Sample is children with immigrant background. All estimates are based on model given in Eq. 1a, and include given covariates described in Section 4. Covariates are added cumulatively, starting in Model 1 with birth year dummies only, and including all covariates described in Section 4 in Model 5. Standard errors clustered on district in parentheses, + $p<0.10$, * $p<0.05$. N=51253

Table 10: Enrollment

	Model 1	**Model 2**	**Model 3**	**Model 4**
Two years				-0.013
				(0.027)
Three years				0.064
				(0.040)
Four years	0.115*	0.124*	0.112*	0.129*
	(0.022)	(0.020)	(0.026)	(0.034)
Five years	0.119*	0.128*	0.114*	0.132*
	(0.023)	(0.021)	(0.027)	(0.034)
N	51253	54637	48326	51253

Note: Sample is children with immigrant background. All estimates are based on model given in Eq. 1a, and include given covariates described in Section 4. Model 1 replicates the main specification. Model 2 includes all non-treated city districts in Oslo in the comparison group. Model 3 includes the 5 city districts with a share of immigrants 20 % and above as comparison city districts. Model 4 includes interactions for agexintervention for two- and three-year-olds as well, using the main sample. Standard errors clustered on city district in parentheses, + p<0.10, * p<0.05.

Table 11: Child outcomes

Panel A: First grade outcomes, all 10 city districts included in comparison group					
	Reading	>limit	Math	>limit	Score
	0.075+	0.039+	0.094*	0.032+	0.089*
	(0.036)	(0.021)	(0.040)	(0.017)	(0.035)
N	6992	6992	6999	6999	6958
Panel B: First grade outcomes, 5 city districts with immigrant share >=20 included in comparison group					
	0.115*	0.039	0.108+	0.037	0.124*
	(0.042)	(0.025)	(0.050)	(0.020)	(0.043)
N	6294	6294	6296	6296	6262

Note: Sample is children with immigrant background. All estimates are based on model given in Eq. 2a, and include given covariates described in Section 4. Panel A includes all non-treated city districts in Oslo in the comparison group. Panel B includes the 5 city districts with a share of immigrants 20 % and above as comparison city districts. Standard errors clustered on city district in parentheses, + p<0.10, * p<0.05.

Statistics Norway

Postal address:
PO Box 8131 Dept
NO-0033 Oslo

Office address:
Akersveien 26, Oslo
Oterveien 23, Kongsvinger

E-mail: ssb@ssb.no
Internet: www.ssb.no
Telephone: + 47 62 88 50 00

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Statistics Norway

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