

*Nina Drange and Kjetil Telle*

**The effect of preschool on the  
school performance of children  
from immigrant families**  
Results from an introduction of free  
preschool in two districts in Oslo

**Abstract:**

Two districts of Oslo started to offer five-year-old children free preschool four hours a day. We analyze the effect of this intervention on the school performance of the children from immigrant families 10 years later (age 16). Our difference-in-difference approach takes advantage of the variation caused by the intervention being implemented in two districts in Oslo, leaving other similar districts unaffected. The grade point average of girls increases substantially more in the intervention districts than in the comparison districts; resulting in an effect estimate of more than a quarter of a standard deviation. There is no significant change in boys' performance, and no support for disadvantageous effects on non-cognitive outcomes.

**Keywords:** preschool, immigrants, early intervention, school performance

**JEL classification:** J13, J15, H52, I28

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**Address:** Nina Drange, University of Stavanger, Norway. E-mail: [nina.e.drange@uis.no](mailto:nina.e.drange@uis.no)

Kjetil Telle, Statistics Norway, Research Department. E-mail: [kjetil.telle@ssb.no](mailto:kjetil.telle@ssb.no)

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**Discussion Papers**

comprise research papers intended for international journals or books. A preprint of a Discussion Paper may be longer and more elaborate than a standard journal article, as it may include intermediate calculations and background material etc.

# 1 Introduction

Inequality in the educational attainment of natives and immigrant groups have been of great concern to policymakers in the US and Europe for a long time (Shnepf 2007, Schneeweis 2009, Fredriksson et al. 2010, Grigorenko and Takanishi 2009, OECD 2009). The low enrollment of children from immigrant families in preschool programs enhances the fear that the current educational inequality will persist into future generations (NOU 2010, Hernandez et al. 2009, St. meld. 1996-97). In the present paper we try to advance the understanding of how publicly provided preschool can improve the educational destinies of children from immigrant families.

There is a growing consensus that investment in early childhood is promising and important to improve intergenerational mobility.<sup>1</sup> Non-enrollment in preschool programs may, for example, delay the children's language development, especially when the parents' proficiency in the language spoken by the majority is poor (Bleakly and Chin 2008, Schølberg et al. 2008).

A number of studies investigate the effect of preschool on subsequent outcomes of the children. It is common to find particularly beneficial effects for girls and children from disadvantaged families (Cascio 2009, Havnes and Mogstad 2009, Berlinski et al. 2009, Melhuish et al. 2004, Cambell et al. 2002, Schweinhart et al. 1993, Anderson 2008), though there is some indication that universal childcare/preschool has deleterious effects on non-cognitive short-run outcomes (Loeb 2007, Magnuson 2007, Baker et al. 2008).

There are, however, few studies on effects of childcare/preschool on immigrant groups.<sup>2</sup> Currie and Thomas (1999) use sibling variation to look at impacts of preschool (Head Start) on Hispanic children in the US. They find substantial benefits among children with US-born mothers. However, they are surprised to find much smaller (or no) effects on children with foreign-born mothers. Given that foreign-born mothers are less proficient in English than US-born mothers, early exposure to English through preschool may therefore be more important for the language development of children of foreign-born mothers. If so, benefits of preschool should be higher for children of foreign-born compared to US-born mothers.

In the present paper we consider children whose both parents are born outside Norway, and we estimate effects of preschool (age 5) on later school performance (age 16). Our study exploits an

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<sup>1</sup> Excellent contributions include e.g. Chetty et al. (2010), Dahl and Lockner (2008), Duncan et al. (2010), Heckman et al. (2010), Deming (2009), Heckman (2006), Garces et al. (2002), Duncan and Brooks-Gunn (1997). Almond and Currie (2010) provides an overview of the literature on children below age five.

<sup>2</sup> We follow Currie and Thomas (1999) who look at effects of preschool on outcomes of the children. There are a few studies exploring the related question of how the achievement gap between persons with immigrant and native background is affected by preschool/kindergarten; see next section (Fredriksson et al. 2010, Becker and Tremel 2006, Spiess 2003).

intervention in the Norwegian capital Oslo in 1998 where two districts offered five-year-old children to spend four hours daily in preschool free of charge. The main motivation for the intervention was to improve the opportunities of children with immigrant background by increasing their language performance and by connecting their families closer to the local society.<sup>3</sup> We analyze the effect of this intervention on the children's educational outcomes when they graduate from compulsory education (age 16). We employ a difference-in-difference approach taking advantage of the variation caused by the intervention being implemented in two districts in Oslo, leaving other similar districts unaffected.

We find a substantial positive effect on the girls' cognitive skills, measured by the grade point average at graduation. The effect corresponds to more than a quarter of a standard deviation. In line with some previous studies (Joo 2010, Anderson 2008, Cambell et al. 2002, Schweinhart et al. 1993) we find no similar effects for boys. Some previous studies on more universal childhood intervention programs have indicated unfavorable impacts on non-cognitive outcomes, at least in the short-run (Loeb 2007, Magnuson 2007, Baker et al. 2008). We, however, find no support for any negative effects on non-cognitive outcomes measured at age 16.

A main assumption for a causal interpretation of our findings is that the educational outcomes in the two intervention districts would have developed in the same way as in the comparison districts in the absence of the intervention. We explore the reliability of this assumption in a number of ways, and the results from various specification and placebo tests are reassuring.

The richness of our data allows us to explore the plausibility of possible mechanisms. First, improved language skills are often considered a main advantage of preschool attendance for children from a disadvantaged background. Our effect estimates are substantial on girls' marks in language, which indicates that this intention of the program was largely achieved for the girls. Second, by reducing the time spent caring for the children, the intervention might also have improved the abilities of the immigrant parents to work or attain education. However, we find no effect on parents' earnings or educational attainment, suggesting that the benign impact on the girls is not a result of improved labor force participation or education of their parents. Finally, we discuss possible reasons why the intervention is more beneficial to girls than boys. Similar gender differences are found in several previous studies on the overall population of children (Anderson 2008, Havnes and Mogstad 2009), which may suggest that the immigrant background of the children in our study is irrelevant in explaining this gender difference.

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<sup>3</sup> Children from immigrant families were underrepresented in preschool institutions in Oslo at the time cf. St. meld. 1996-97. The two districts chosen for the intervention had a particularly high population share of immigrants.

## 2 Theory and related empirical studies

There are a number of ways in which time spent in preschool might affect later outcomes. According to Heckman (2006), early investment in children's development provides a foundation for later learning. Preschool cognitive and socio-emotional capacities are key ingredients for human capital acquisition during the school years. Within this framework, "learning begets learning" with early capacities enhancing the productivity of school-age human capital investments.

Language is a particularly important prerequisite for "learning to beget learning". The child psychology literature underlines the crucial role of language development in the child's learning processes, and the amount and quality of speech of adults with whom the child interacts is believed to be paramount to the development of language and thinking (Vygotsky 1962, Huttenlocher et al. 1991, Hart and Risley 1995, Huttenlocher et al. 1998, Bryant 2005, Santrock 2009, Kalil et al. 2010). Attending preschool may be particularly beneficial for our children whose parents typically have limited proficiency in Norwegian (NOU 2010, Schølberg et al. 2008, St. meld 1996-1997). As it seems well-documented that the ability to learn a new language is higher at younger ages (Hakuta et al. 2003), exposing the children to the new language one year earlier can enhance their learning throughout the early school years. Bleakly and Chin (2008), for example, find that immigrant children in the US with poor proficiency in English are less likely to attend preschool; and more likely to experience grade retention and high-school drop-out.

The child may also benefit from effects on the parents of taking the child to preschool. The presence of young children in the household typically requires that the parent(s) spend time at home. This time spent in home production could be reduced when the child gets to stay in preschool four hours a day, which makes it more attractive to allocate time towards market production (Becker 1991). If the mother used to stay at home with the child,<sup>4</sup> access to free preschool would make labor market participation or attending school more attractive. A free slot in preschool will lower her reservation wage since she no longer needs to pay for child care if employed (or in school) four hours daily.

Making employment or education more attractive for the parents may affect the performance of the child in several ways. First, the household income may increase. A higher household income can affect the child's performance through the improved consumption opportunities of the family (Duncan et al. 2010). Low income is not uncommon among families with immigrant background. To the extent that cognitively enriching early home environments lay the groundwork for success in preschool and beyond, parents' ability to purchase books, toys, and enriching activities is vital (Yeung et al. 2002). Low income and its attendant stressors have the potential to shape the neurobiology of the

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<sup>4</sup> According to summary statistics the mothers in our sample do not have a strong attachment to the labor market.

developing child, which may lead directly to poorer outcomes later in life (Knudsen et al. 2006). Economic insecurity can also affect parental abilities by influencing parents' mental health.

Second, employment or participation in education could lead to a stronger integration of the parents into society, for example through more interaction with native co-workers/students and enhanced understanding of the society. Their language skills are likely to improve, which could make it easier for the child to learn the language. Improved language skills could also better enable the parents to seek help and advice in school and society, and possibly reduce parents' frustration and stress related to lack of understanding and ability to navigate the school system. Parental psychological stress or harsh parenting behaviors can be especially detrimental during early childhood (Bronfenbrenner and Morris 1998, Godfrey and Barker 2000, Shonkoff and Philips 2000, Waters and Sroufe 1983). Moreover, improved integration of the family into the Norwegian society could also affect the child's pool of peers. The family may, for example, move to a school district with better teachers and peers.

We now turn to existing empirical studies on effects of preschool. There are many studies on early intervention programs (Almond and Currie 2010), but few that consider effects on children from immigrant families.

General studies on childcare and preschool suggest particularly beneficial effects for girls and children from disadvantaged families (Havnes and Mogstad 2009, Berlinski et al. 2009, Melhuish et al. 2004, Cambell et al. 2002, Schweinhart et al. 1993, Anderson 2008). Anderson (2008) conducts a reanalysis of the major American early-intervention programs (Perry Preschool, Abecedarian and the Early Training Project). He argues that the long-term effects of these interventions all relates to girls and that the effects are biggest for total years of education. He finds no significant long-term effects for boys. When it comes to non-cognitive outcomes of the child, several studies have revealed a negative effect of time spent in childcare (Loeb 2007, Magnuson 2007, Baker et al. 2008).<sup>5</sup>

In a recent Norwegian study, Havnes and Mogstad (2009) provide evidence on long-run outcomes from a universal childcare program. Their study takes advantage of an Act passed in 1975 assigning the responsibility for childcare to local municipalities, and causing a large increase in childcare slots with substantial local variation. They find a strong, positive effect on educational attainment and on subsequent labor market attachment. Some effects are heterogeneous, for instance is the main part of the decrease in the probability of being a low and average earner related to girls' improved achievements.

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<sup>5</sup> For the children with particularly disadvantaged family backgrounds being included in the Perry Preschool and Abecedarian programs, there is little evidence suggesting deleterious long-run effects on non-cognitive outcomes.

Despite this growing literature on general effects of childcare and preschool programs, little is known about how such programs influence children from immigrant families. Currie and Thomas (1999) use sibling fixed effects models to study the effect of the US preschool program Head Start on Hispanic children. Their findings suggest that Head Start had large positive effects on test scores and school attainment. They find substantial benefits among children with US-born mothers, but much smaller (or no) effects on children with foreign-born mothers. The authors recognize that it is surprising that the effect is larger for the children with US-born mothers than for the children with foreign-born mothers. Given that children with foreign-born mothers have poorer proficiency in English than those with US-born mothers (Bleakly and Chin 2008), we would expect early exposure to English through preschool to particularly be beneficial for the language development of the children with foreign-born mothers. In this case, benefits of preschool should be higher for children of foreign-born compared to US-born mothers.

In addition to Currie and Thomas (1999), we are aware of a few studies which consider the influence of preschool on the *achievement gap* between persons with native and immigrant background (Fredriksson et al. 2010, Becker and Tremel 2006, Spiess 2003).<sup>6</sup>

Fredriksson et al. (2010) examine the impact of preschool on the cognitive achievement gap between persons with native and immigrant background in Sweden. They observe that the parents whose children were in childcare hold favorable socioeconomic characteristics, and they restrain from trying to credibly estimate the effect of attending childcare on the outcome of children with immigrant background. Instead they estimate the improvement of attending childcare for persons with immigrant background relative to the same improvement for natives; thereby relying on an assumption that the unobservable characteristics determining selection into childcare affect children with native and immigrant background similarly. The results show that childcare attendance reduces the gap in language skills at age 13 between children from native and immigrant families, while the gap in educational attainment by age 24 is unaffected.

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<sup>6</sup> Bakken (2009) compares the school performance (at age 16) of children from immigrant families in Norway who started compulsory school at age six and seven. He finds that the cohort with ten years of schooling gets better grades than the preceding cohort with nine years of schooling.

## 3 The preschool intervention

### 3.1 Preschool in Norway

In Norway children start school in August of the calendar year they turn 6 and graduate from compulsory school in June of the calendar year they turn 16.<sup>7</sup> Childcare institutions serve children aged 1 to 5, but children are usually divided into groups of 1-3 year olds and 3-5 year-olds. Childcare institutions in Norway receive substantial public subsidies. The part of the costs that are covered by the parents differed somewhat across the country in 1998. In Oslo the parents typically had to pay around 6000 US\$ a year for a full-time slot, and some part-time slots were available at a somewhat (but not proportionally) lower cost (Grünerløkka 1998).

The childcare centers we consider in this paper are generally of high quality. It is required that all age groups have a trained teacher. To work as a preschool teacher, a college degree is required; and with a limited additional college course, preschool teachers can teach in primary school (grades 1-4). The year before starting school, the five-year-olds will spend time in groups separated from the younger children.

These preschools have to follow a national curriculum<sup>8</sup>. The curriculum describes what kind of subjects that should be covered and how the pedagogical content should be implemented. Among the requirements, the curriculum from 1995 stated that the child should develop its verbal communication to be able to communicate effectively.<sup>9</sup> In addition to the regular curriculum, the city district of Gamle Oslo developed a more comprehensive pedagogical plan aimed at reaching the immigrant children with their particular language- and cultural background.<sup>10</sup>

The counterfactual mode of care, i.e. the type of care the children from immigrant families would be exposed to in the absence of the intervention, is likely to comprise private arrangements. Before 2010 there have been too few slots at daycare centers to meet the demand, requiring some working parents to privately hire nannies or get help from relatives. These private arrangements are not subject to any public certification or subsidies, and the quality is presumed to vary a lot. Among the immigrants not in formal daycare, survey data suggest that the children would typically to be looked after by their mothers or relatives (Holm and Henriksen 2008). The language skills of these foreign-born mothers and relatives are limited, and the motivation for the intervention in Oslo was to

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<sup>7</sup> Cohorts born 1990 and before started school at age 7, and graduated at the same age (16). They thus have completed 9 years of schooling upon graduation from compulsory school, while our later cohorts complete 10 years.

<sup>8</sup> Lov av 5. mai 1995 nr. 19 om barnehager. (Act of 5<sup>th</sup> of Mai 1995 no. 19 about child care institutions.)

<sup>9</sup> Rammeplan for barnehagen, 1995. (Curriculum for childcare institutions, 1995).

<sup>10</sup> Informasjon om prosjektet gratis kortidsbarnehage i bydel Gamle Oslo (Plan for the project free part time pre-school in Gamle Oslo city district.) Gamle Oslo city district.



improve the children's opportunities to learn Norwegian and facilitate integration. It is thus likely that the type of care the children from immigrant families would be exposed to in the absence of the intervention, is largely characterized by modest to no possibilities to develop full proficiency in Norwegian.

### **3.2 The intervention in two districts in Oslo**

The number of children from immigrant families, i.e. where both parents are foreign-born, residing in Norway has increased substantially from the 1970s and onwards (St. meld. 1996-97). While there were 2 544 pupils with an immigrant background in 1977, this had increased to 28 000 in 1996/97. Children from immigrant families amounted to 25 % of the pupils in Oslo in 1996/7, a far higher number than in the rest of Norway. They were underrepresented in attending preschool in Oslo at the time, and the two city districts called Gamle Oslo (GO) and Grünerløkka (GL) had the highest share of immigrant families with children in preschool age. These two administrative districts (GO and GL) got funding from the municipality of Oslo and the Norwegian Government to offer preschool free of charge to all five-year-olds four hours a day. The intervention started in August 1998, meaning that children residing in the two districts and born in 1993 got the offer. Children in the two districts who were born in 1992 (or earlier), did not get the offer (they started *school* in August 1998). The aim of the intervention was to improve the children's language skills and promote integration by providing all children with experience from preschool before starting school at age 6. Preschool free of charge was only available for the families residing within the district of GO and GL, and hence none of the neighboring districts were affected.

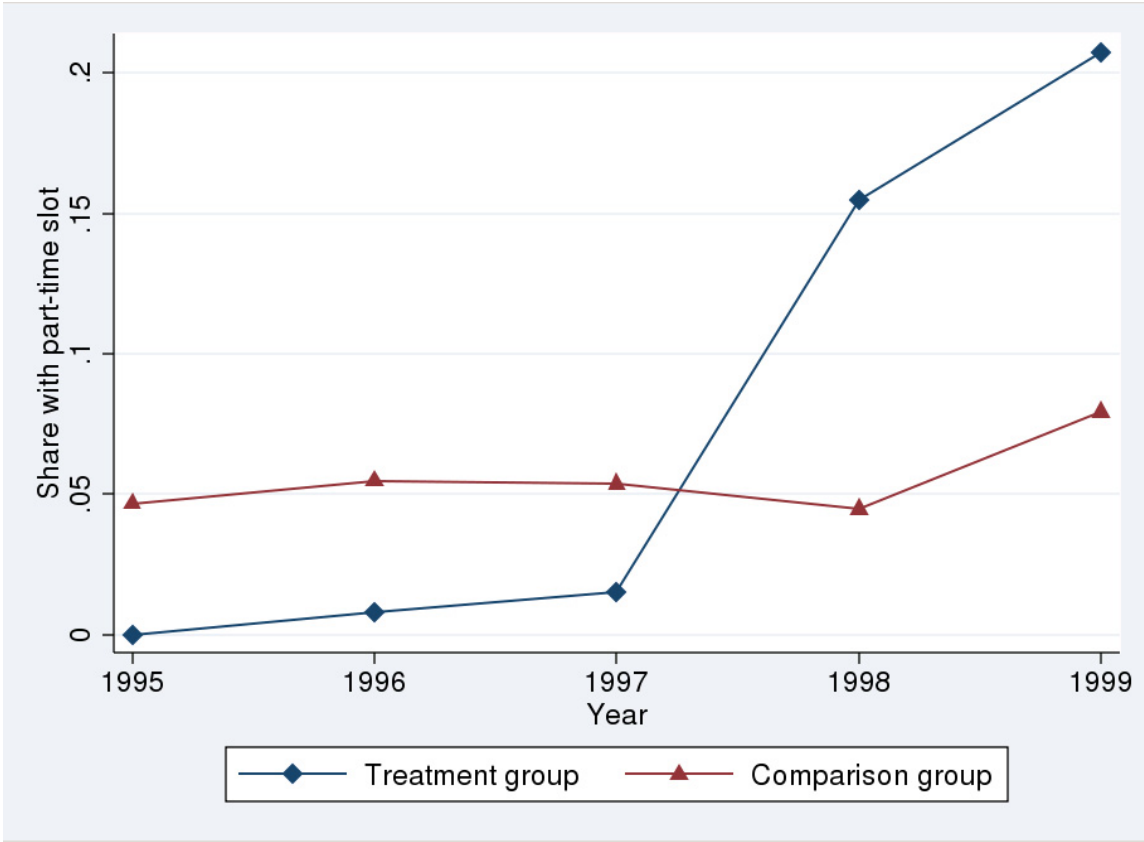
If the child spent more than the four daily free hours in preschool, the parents had to pay for the additional hours. Thus parents of children already in full- or part-time preschool experienced a decrease in expenditures on preschool, while parents with children at home could now send their child to preschool four hours every day free of charge. To meet the anticipated surge in demand, the number of available preschool slots in the two districts was expanded, in particular the part-time slots. Figure 1 shows the development of the share of all five-year-old children in the districts that held a part-time preschool slot; separately for the two intervention districts (labeled "Treated") and comparable districts in Oslo (labeled "Comparison"; see Section 5 for definition). The number of slots is measured at the end of the given year, and we note how the 1998-intervention in GO and GL increased the supply of part-time slots considerably.

Note that the figures in Figure 1 may underestimate the effect of the intervention on the increase in preschool attendance. Some families who accepted a free short-time slot may have decided to expand it to a full time slot. Qualitative reports from GL suggest that several families actually did

“upgrade” to a full time slot (Grünerløkka 2006). Unfortunately, individual data on the children who did attend preschool, as well as on the number of hours per week attended, is not available. We know, however, from an evaluation of the intervention in GO, that approximately 100 five year-olds started pre-school as a result of the policy, amounting to 60 % of all children not in pre-school in the city district autumn 1998 (Nergård 2002).

The two districts GO and GL had somewhat different sources of funding for their interventions. The main features are however shared by the districts. The target group was children from immigrant families, and information about the free preschool was actively conveyed through health care centers, by district civil servants and social services. The information was distributed in several languages. The district administration took effort to make sure that every family knew that they could send their child to preschool free of charge. Indeed, families with five year-old children that had not applied for a preschool slot were systematically approached by representatives from the city district in order to enhance preschool participation. In addition, a substantial effort was put into giving the children a sufficient pedagogical offer, and a number of the preschool teachers were multi-lingual. See Appendix 1 for further details on the intervention.

**Figure 1. Share of five-year-old children in the district that held a part-time preschool slot**



Data source: Statistics Norway.

## 4 Empirical strategy

Our empirical analysis aims to uncover the effect of attending formal preschool at age five on the school performance at age 16 of children from immigrant families. We utilize the variation from the provision of free preschool in 1998 in two districts in Oslo, leaving other similar districts unaffected.

A difference-in-difference estimator ( $\lambda$ ) of the effect of the preschool intervention on later school performance ( $Y$ ) can be expressed as follows:

$$(1) \quad \lambda = (Y_{T,93} - Y_{T,92}) - (Y_{C,93} - Y_{C,92})$$

where  $Y$  is a measure of school performance for the cohort born in 1992 and 1993 residing in the treatment ( $T$ ) or comparison ( $C$ ) area. The first parenthesis measures the difference between outcomes of children living in the treatment districts 31<sup>st</sup> of December the calendar year before they turn 5 years old: Those born in 1993 are affected by the preschool free of charge intervention, while those born in 1992 are not (already in school when the intervention takes place). The second parenthesis measures the corresponding difference between those living in the comparison area (of whom none are affected by the intervention) who are born in 1993 and 1992.

We proceed to consider the formal presentation of the regression model (estimated by OLS):

$$(2) \quad Y_{i,t} = \alpha + \beta \text{treated}_i + \delta \text{post}_t + \lambda(\text{treated} * \text{post})_{i,t} + \eta X_i^\tau + \varepsilon_{i,t}$$

$Y_{i,t}$  is the school performance measure of child  $i$  born in year  $t$ .  $\text{treated}_i$  is a dummy variable equal to 1 if child  $i$  lived in the treatment area, and 0 if child  $i$  lived in the comparison area.  $\text{post}_t$  is a dummy variable equal to 1 if child  $i$  is born 1993 (i.e. five in 1998 and graduating in 2009) and 0 if child  $i$  is born 1992 (i.e. five in 1997 and graduating in 2008). The vector  $X$  includes covariates, and the superscript  $\tau$  indicates at what time the covariates are measured (we will measure them at the time the child is four).

The parameter of interest is  $\lambda$ , which captures the change in the school performance in the treatment area between the pre-cohort (1992) and the intervention-cohort (1993) relative to the same change in the comparison area. This difference-in-difference estimator is intended to capture the causal effect of the intervention on the school performance of the children with immigrant background. The main identifying assumption is that the trend in school performance of the children in the treatment area would have been the same as for the children in the comparison area, in the absence of the intervention.

There are several reasons why this assumption may not hold. First, the assumption may not hold if the composition of families in the comparison and treatment area changes. For example, families in the comparison area may move to the intervention area to get access to preschool free of charge. We handle this by relying on the district of residence when the child was four years old. To the extent that some families moved after the child was four, relying on district of residence at age four could introduce some attenuation bias.

The composition of families in the comparison and treatment area might have changed for other reasons too. During the 1990s there was an increase in housing prices across Oslo, and the increase was particularly high in the centre areas. Our intervention districts are located very close to the center and it is not unlikely that housing became relatively more expensive in our treatment area. This may have caused the poorest immigrant families to move out to the suburbs, leading to an increase in low-income families in the comparison districts relative to the treated districts. Assuming that low-income families have children who perform poorer at school, this selection mechanism may bias our effect estimate upwards. We explore the relevance of such housing selection in several ways. We construct different sets of comparison districts to explore whether results are robust across several specifications. Since low-income families are unlikely to move to upper-class districts, this housing selection should affect our effect estimate less when comparing to such districts than when comparing to low-income suburbs. We run regressions with the comparison group born 1991 (instead of 1992). If there is a trend causing children with poor school performance to leave our intervention districts, the effect estimate should increase when we use older children in the comparison group. We also control for district-specific linear trends, and, finally, we perform placebo tests. There is no reason to expect housing selection to only affect the change between our two cohorts (1992 and 1993). If we find an effect using cohorts 1991 and 1992 (instead of 1992 and 1993) this indicates the presence of a secular trend (like housing selection) not related to our intervention.

Second, the identifying assumption may not hold if some children are excluded from our dataset for reasons related to the intervention. For example, the intervention might have affected emigration from Norway. We are not able to measure school performance for children who do not live in Norway when they graduate. The intervention intended to improve the integration of the children and their families, and we might therefore imagine that the intervention causes some families that would otherwise have emigrated to remain in Norway. If these children perform below average in school, our effect estimate may be downward biased. We explore this by estimating effects of the intervention on emigration.

Another example relates to grade retention or skipping a year. In the Norwegian school system children are virtually never retained in a grade due to poor performance. Instead, students who are not

performing well are supposed to be followed up closely and given special tutoring. Students are allowed to graduate even with the lowest possible grade. As a consequence, it is very unlikely that the composition of the children has changed due to changes in grade retention because of the intervention.<sup>11</sup> Note also that dropping out of school prior to 10<sup>th</sup> grade is legally prohibited in Norway.

In principle, it is possible that the preschool intervention could improve the performance of some children to the extent that they get to skip a grade and graduating one year earlier. While this might happen, it is extremely rare in Norway to skip a grade.<sup>12</sup> Our dataset only includes children who graduate on-time, which implies that we exclude possible early-graduating students from our sample. To the extent that these children perform better than average at graduation, excluding them would tend to bias our effect estimate downwards.

A sort of opposite bias may occur if the preschool intervention affected who actually got to start school at age 6. The treated children get in contact with the educational system earlier, and one could imagine that the preschool teachers could identify children that were not sufficiently mature to start school at age 6. If they start school one year later, this would change the composition of cohorts in the treatment group before and after the intervention (the composition of the comparison group would be unaffected). After the intervention the children who are not mature enough are retained one year, and they graduate a year later and are hence not observed in our data. To the extent that immature children perform worse at graduation, this selection will bias our effect estimate upwards.<sup>13</sup> We explore such selection in and out of our dataset by checking that the intervention has no effect on the number of children who reside in Norway at age 16 and who do not graduate on-time.

Third, the identifying assumption may not hold if there are other concurrent programs that could have affected the school performance of children in the treatment or comparison area.

The intervention in GO and GL occurred around the same time as a bigger program called Plan of Action Oslo Centre/East (PAOCE). Most relevant for our children, PAOCE included homework support and outdoor activities.<sup>14</sup> Note that while the preschool intervention was introduced in 1998, the school-related activities in PAOCE started in 1997. Hence there is nothing that suggests

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<sup>11</sup> In the 1992 cohort, among all children with immigrant background residing in the whole of Norway at age four (like in our main analytic sample) only 39 children graduated one year too late. 22 of these were born in the last quarter of the year.

<sup>12</sup> It is somewhat less uncommon to start one year earlier at school and therefore graduating at age 15. Note, however, that in our setting the reason for graduating one year early could not be that these children started school at age 5 (since then they would not be in preschool).

<sup>13</sup> Rather than a source of bias, this selection may also be considered a mechanism. If waiting one year to start school improves the school performance of such immature children, we may consider this an attractive feature of the intervention. We are unable to investigate this further since we do not have data on school performance of children starting school one year later.

<sup>14</sup> A more elaborative coverage of the activities directed towards children under the Plan of Action Oslo Centre/East is found in Appendix 2.

that children born 1992 (pre-intervention cohort) and children born 1993 (post-intervention cohort) should be affected differently by the other PAOCE activities. The PAOCE expired in 2007 implying that the children affected by the preschool intervention (1993 cohort who graduated in 2009) were treated one year less by the PAOCE activities than the pre-intervention cohort (1992 cohort who graduated in 2008). I.e. if PAOCE actually affected the school performance of children in the intervention districts positively, the fact that our post-cohort (from the preschool intervention) is exposed one year less should, if anything, contribute to a downward bias in our effect estimate. We still explore this as follows. PAOCE affected children from native and immigrant families similarly, while the intervention was mainly directed towards children from immigrant families. Thus, if our estimate of the effect of the preschool intervention were in fact driven by PAOCE, we should expect to see similar effects for children from native and immigrant families. Moreover, we are able to account for PAOCE by estimating a dif-in-dif-in-dif, which utilizes that children from native families were largely unaffected by the preschool intervention.<sup>15</sup>

Another relevant policy reform took place throughout Norway in 1998. In August 1998 a cash-for-care subsidy was introduced in Norway giving all parents that did not send their 1 year-old child to publicly subsidized daycare a monthly cash subsidy amounting to 3000 NOK. From 1999 the subsidy was expanded to also cover 2 year-olds. The subsidy might affect the children in our sample through an increase in household income if they have a young sibling. Note that this increase in income would also accrue to the families of the pre-intervention cohort (born 1992) with a younger sibling, but for one year less (cash-for-care started in 1998 and is still available). More importantly, this subsidy was equally available in our treatment and comparison areas. The reform would therefore only affect the reliability of our empirical strategy if the mothers of the treated children are more likely to have a new baby than mothers of non-treated children in the treatment or comparison area. Indeed, the presence of younger children, who make the family eligible for cash-for-care, needs to be different across native and immigrant families for our dif-in-dif-in-dif estimate (with children from native families) to be biased.

A final notable challenge to our empirical strategy relates to the measurement of the outcome variable. School performance at graduation (10<sup>th</sup> grade) is assessed on the school level. Although the teachers are legally instructed to mark the students due to objective criteria, it might be a chance that every cohort is graded by the general level of the children at the school. Thus, if several children in a

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<sup>15</sup> Note that this dif-in-dif-in-dif could also be interpreted as an investigation of mechanism. The native children in preschool also got subsidized by the intervention (four hours free of charge daily), but since most of them were already attending preschool, this is a pure income effect. By showing an effect on children with immigrant background even after differencing out the effect on native children, this may be taken to indicate that it is not the income effect of the intervention that causes the school performance of children from immigrant families to improve.

particular cohort perform particularly well, the teachers may lower the marks of other children in order to retain a similar level as earlier cohorts (or require better achievements than previous years to provide a given mark). This might lead to effect estimates that are downwards biased. We are able to explore this in two ways. First, the graduating students must take an exam in one or two randomly drawn subjects. These exams are evaluated by school external personnel and should thus not be affected by changes in performance related to our intervention. Second, we know what school each student graduated from, which enables us to include school fixed effects.<sup>16</sup>

We also explore some possible mechanisms behind an effect on the school performance of children from immigrant families. We will look at language skills of the children using the regression in (2) above (just changing the outcome variable). To look at labor market attachment and education of the parents, we use the same regression (2) on the sample of the children's parents.<sup>17</sup>

## 5 Data and definitions

### 5.1 Definitions of treatment and comparison areas

Oslo was organized in 25 districts at the time of the preschool intervention (1997/1998); see Statistisk kontor (1998) for definitions. Children in the treatment area who are born in 1993 and turning five in 1998 were eligible for the free preschool. We hence define the post-reform population as children from immigrant families who are born 1993 and who reside in a city district in Oslo on the 31<sup>st</sup> of December the year they turn 4 (1997). In our main analytic sample the pre-reform sample is similarly defined as all children from immigrant families who are born 1992 and who reside in a city district in Oslo on 31<sup>st</sup> of December the year they turn 4 (1996).<sup>18</sup> We use the district of residence at age four (and not five or later) to reduce the possibility of selection bias that may occur if families moved to one of the intervention districts to get free preschool slot. Hence if a family moved from the comparison area to the treatment area during the year the child turned 5 (or later), the child is still put in our comparison area. Relocation after the child has started school is not relevant for our results as

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<sup>16</sup> Note, however, that we do not include school fixed effects in our *main* regressions since what school a student attends may be endogenous to the intervention: If the intervention improves school performance of the child or the integration of the parents, it is possible that the family might be better at identifying and getting the child into a higher-quality school. Public schools in Norway are generally of high quality, and there are very few private schools. The public schools have geographically defined catchment areas and getting into a school outside the catchment area is rarely possible. Segregation by socio-economic status results in the abilities of the students varying substantially across schools, but this is largely compensated by more public funding directed towards schools with low-performing students (Hægeland et al. 2005).

<sup>17</sup> While the outcomes of children are only measured once, we have yearly income and education data for the parents. We can thus follow the same parents in treatment and comparison area for several years, enabling application of an alternative dif-in-dif estimator which exploits variation across treatment/comparison groups and variation over time for the same parents. We did apply this alternative dif-in-dif estimator for the parents of children born in 1993, and it yielded similar results.

<sup>18</sup> In some analyses we explicitly also include earlier cohorts, which are then defined analogously.

long as they remain within Norway past graduation. If they leave the country before graduating (10<sup>th</sup> grade), no measure of school performance is available to us.

Further, we define children from immigrant families in the two intervention districts (GO and GL) as living in the treatment area (again relying on the district of residence at age 4). How to define the comparison area is not similarly clear. In our main definition of comparison area, we rely on information about the share of five-year-old children with immigrant background in the district (age four). In table 1 we list the 25 districts in Oslo according to this share of children of immigrant families for the cohort born in 1993. We see that the two intervention districts (GO and GL) are the city districts with the highest share of children from immigrant families – more than half of the children in these districts belonged to immigrant families. The other districts have shares from about 40 % and lower, and for our main analytic sample we set the cut-off for inclusion in the comparison group at 15 %. Table 1 also includes a ranking of districts based on the average earnings of fathers and the children's GPA. To make sure our results are not sensitive to exactly which districts are included in the comparison group, we will show robustness results where we vary which districts are included in the comparison group. The countries of origin of the children are similar in the treated and comparison district. The biggest group in both the districts is people with a Pakistani background (30-40 percent), and the subsequent four biggest groups have Turkish, Moroccan, Sri Lankan and Vietnamese background.

## 5.2 Dataset and Variables

We utilize the database called *FD-trygd* which is a combination of a number of Norwegian registries. It contains records for every Norwegian resident from 1992 to 2005. The data set provides individual demographic information (marital status, parental and child identifiers, spouse identifier, immigrant status, gender, age), socio-economic data (years of education, earnings), employment status, indicators of participation on Norway's welfare programs and geographic identifiers for municipality and district of residence. We restrict our main analytic sample to children with immigrant background, where a child is defined to have an immigrant background if the mother and the father are born abroad.

In addition, Statistics Norway maintains an education database containing information on 10<sup>th</sup> graders' school performance at time of graduation from compulsory education at age 16. The education dataset can straightforwardly be merged with *FD-trygd* since both datasets contain the identical personal identifier. The evolving dataset includes information and school performance of children with immigrant background who are born in 1988-1993, i.e. who graduate over 2004-2009.

We can measure school performance in several ways. When graduating from compulsory school (10<sup>th</sup> grade), Norwegian children get marks in 12 subjects. There are 8 theoretical subjects



(written and oral Norwegian<sup>19</sup>, written and oral English, mathematics, nature and science, social science and religion) and 4 practical subjects (home economics, physical education, music and hand-craft). In addition they get an evaluation on behavior (non-cognitive). They typically also have one written and one oral exam in two randomly drawn theoretical subjects. The two exams are graded by an external evaluator, typically an experienced teacher from another school.

We construct a summary measure of each 10<sup>th</sup> grader's performance in the 12 graduating subjects. Grades in individual subjects are awarded on a scale from one to six, where six indicates excellence and one indicates very little competence (it is legally impossible to fail). Our main measure of cognitive outcomes is grade point average (GPA), which is the average mark of the 12 subjects 10<sup>th</sup> graders get a final assessment of.<sup>20</sup> In addition to GPA we construct a language measure (average of written and oral Norwegian and written and oral English), a math/science measure (average of mathematics and nature and science) and a measure for the other subjects (average of social science, religion, home economics, physical education, music and hand-craft). Finally we construct a measure of the exam results (average of the two exams).<sup>21</sup>

The point of departure for constructing measures of non-cognitive outcomes is two marks that capture the ability to organize and the ability to keep order. They are graded on a scale of 3 possible outcomes, but the vast majority of students get the best possible grade. We construct a dummy variable that is 1 if the student gets the best possible outcome in both marks, and 0 if the grade in any of them is not the best possible.

To account for possible observable changes in composition between the areas, we include a number of covariates  $X_t^r$ . The covariates are measured in  $\tau$ , i.e. in the year when the child was 4, in order to secure that the covariates are not endogenous to the treatment. A gender dummy is included, and a dummy that captures whether the mother (father) was younger than 22 when she (he) had the child (and a dummy set to one if the age of the parent is missing). We include two measures of labor force participation for each of the parents; a dummy capturing that the mother (father) had non-minor earnings in the three previous years, 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

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<sup>19</sup> In addition, most children from native families get marks in a version of written Norwegian called nynorsk. Children with immigrant background will typically be exempted from this subject, and hence we do not include it.

<sup>20</sup> Some records contain missing observations for one or more marks. As there was no legal opportunity to fail students in this age group, the missing observations are most likely due to problems with registration. If there are more than 5 missing observations on an individual we exclude the record. This similarly applies for the other measures defined below, though below we only require that they have at least one mark to be included. It is not obvious how individual marks should be aggregated into a summary measure; see Hægeland et al. (2005).

<sup>21</sup> Marks from exams and marks on behavioral outcomes are only available for the cohorts born 1992 and 1993.

in addition a dummy capturing if the mother (father) has a missing observation on education. We also include a dummy of whether the child is born outside Norway (or whether only the parents are born outside Norway), and a dummy of whether the child has a mother (father) from an OECD country. As children born early in the year are older when graduating, we add dummies for quarter of birth.

We also rely on outcomes of the parents to explore the possibility of some mechanisms. It is possible that the preschool free of charge affected the parents' integration into society. To look at this, we estimate the effect of the intervention on the parents' earnings and education. The vector of covariates is the same as above, and the outcome is measured in the year the child was 6 years old.<sup>22</sup>

### 5.3 Summary Statistics

In Table 2 we present summary statistics on our main analytic sample of children with immigrant background. The dataset comprises 508 children in the treatment area, and 1292 children in the comparison area.

From Table 2 we see that the treatment and comparison areas are somewhat different on some of the observables. Parents of children in the treatment areas are less likely to work and more likely to receive welfare than are the parents in the comparison areas. We keep in mind that our common trend assumption does not require that the levels are the same, only that the trends in the two areas are similar. There seem to be at least one difference in trends. For once there is an increase in mothers with a completed high school degree in the treated areas. In the comparison area there is a decline, resulting in a raw difference between the two districts the relevant years of 10 percentage points. We also note that there are fewer mothers with missing on education in the treated area in 1993 than in 1992, while this number is increasing in the comparison area. However, there is in general a very high share of parents with missing on education,<sup>23</sup> which makes it difficult to disentangle the relevance of the somewhat differing trends in this variable in the treatment and comparison area. There is also a decline in welfare recipients in the treatment area (particularly among fathers), with no corresponding decline in the comparison area. In our regressions we will handle such possible observable changes in composition by including covariates (unless otherwise noted).

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<sup>22</sup> While the outcomes of children are only measured once, we have yearly income and education data for the parents. We can thus follow parents in treatment and comparison area for several years, enabling application of a dif-in-dif estimator that exploits variation across treatment/comparison groups and variation over time for the same parents. We did apply this alternative dif-in-dif estimator for the parents of children born in 1993, and it yielded similar results.

<sup>23</sup> It is not surprising that educational achievement is missing for a number of these parents, since they are born outside Norway - and typically in developing countries like Pakistan and Turkey; cf. Section 5.1. Moreover, survey data indicates that education of immigrants is not well captured and typically underreported in register data (Holm and Henriksen 2008).

# 6 Results

## 6.1 Descriptive results on GPA

In Table 3 we provide the mean GPA for girls and boys in the treatment and comparison area for the cohort born before and after the preschool intervention. For the girls in the treatment area, the GPA increases significantly from 3.8 in the cohort before the intervention to 4.1 in the affected cohort. There is no similar increase in the comparison area, where it in fact declines from 4.1 to 4.0. Such a clear improvement in the relative performance of treatment-area girls from 1992 to 1993 is in line with the intention of the preschool intervention. To the extent that this relative increase can be attributed to the preschool intervention, it implies that the intervention improved the mean school performance of the girls by 0.35 grades. For the boys, there is no improvement in school performance.

Figure 2: Mean of grade point average (GPA) for Cohorts Born 1988-1993.



Note: Grade point average (GPA) at graduation from compulsory school (age 16) for children with immigrant background born in given year. See Section 5.1. for definition of comparison group. Cohorts born in 1990 and before had 9 years of schooling at graduation, while later cohorts had 10.

In Figure 2 we graph the development over time in the mean GPA in the treated and comparison area for our girls and boys. The x-axis provides the birth year of the children, and recall that the intervention occurred for those born in 1993 (i.e. attended preschool in 1998 and graduated from compulsory school in 2009).

The upper part of Figure 2 shows the mean of GPA for girls born in 1988-1993. The trend in the years just before the intervention is fairly similar,<sup>24</sup> but something clearly happens with the performance of the treatment-area girls from 1992 to 1993. Such a clear improvement in the performance of treatment-area girls born 1993 compared to 1992 is in line with the intention of the preschool intervention.

The lower part of Figure 2 shows the corresponding changes in GPA for the boys. Again, the trend in the years just before the intervention is fairly similar, but contrary to the graph for the girls, the treatment-area boys are not catching up with the comparison-area boys in the last cohort. This is not what we would expect to see if the preschool intervention had beneficial impacts on the school performance of the affected boys.

## 6.2 Main regression results

In Table 4 we report our main regression results. We have estimated effects for girls and boys separately (by interactions in one regression), including covariates as outlined in Section 5.2. The first column reports results for the grade point average (GPA). We see that the girls' results improve by 0.26 grades and the result is significant at the 5 % level. This amounts to about a whole grade in three of the graduating subjects, or 29 % of the standard deviation.<sup>25</sup> This is a substantial effect.<sup>26</sup> While 29 % of a standard deviation appears higher than what is found in some previous studies, there are also previous studies on children from disadvantaged families that find bigger effects (Almond and Currie 2010, Anderson 2008, Deming 2009).

Turning to the boys we see that in spite of a negative coefficient, the estimate is not statistically significant. It seems that the boys' GPA is not substantially affected by them being exposed to the free preschool. Again, it is not uncommon in previous studies to find substantial benign

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<sup>24</sup> Note that the cohorts born 1990 and before started school as 7 year-olds and are hence not necessarily comparable to the cohorts born 1991 and onwards.

<sup>25</sup> The mean and belonging standard deviation is computed with the reference group treated children pre reform.

<sup>26</sup> All eligible children from immigrant families may not have attended the free preschool, but qualitative evidence suggests that most of them did (cf. Section 4). Our estimation model produces intention-to-treat (ITT) effects, as we estimate the reduced form impact on all children in the 1993 cohort who reside in the intervention districts (i.e. not only on the children who actually attended preschool). An advantage of the ITT parameter is that it captures the full impact of the intervention of changes in both preschool and other childcare (private, parental), as well as peer effects. However, since this parameter averages the intervention effect over *all* children with immigrant background in the intervention districts, it may understate the effect per preschool slot (see Baker et al. 2008 or Havnes and Mogstad 2009 for elaborations).

effects for girls and smaller or no effects for boys (Anderson 2008, Cascio 2009, Havnes and Mogstad 2009, Schweinhart et al. 1993).

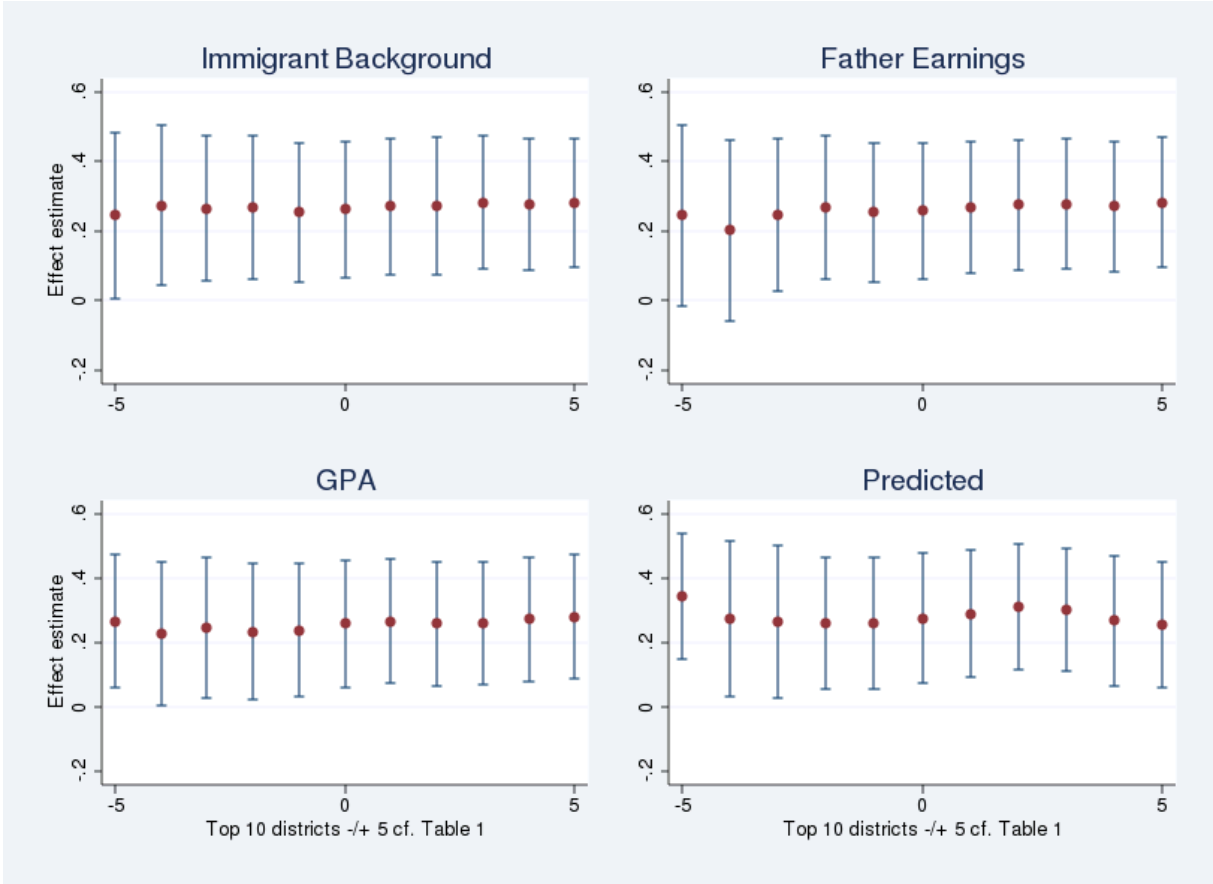
In the second column of Table 4 we report effect estimates on our non-cognitive outcome measure (behavior). This result indicates no deleterious effect of the preschool intervention on our long-run measure of non-cognitive outcome, and this holds for girls and boys alike.

### **6.3 Robustness and specification tests**

We want to make sure that our main regression result on GPA is robust across related specifications and samples. What city districts to include in the comparison area is not obvious, and we therefore want to check that our main regression result is not sensitive to the definition of comparison area (see Section 4). In Figures 3 and 4 we report the effect estimates (with 95 percent confidence intervals) for girls and boys with differing districts included in the comparison area. In each of the four panels, 0 on the x-axis indicates that the comparison group comprises the top 10 non-treated districts in the corresponding column of Table 1. The number on the x-axis indicates how many more or fewer districts are included in the redefined comparison group. For example, the comparison group of our main analytic sample includes the top 10 non-treated districts given in column 1 of Table 1 (cf. definition in Section 5). The effect estimate provided at 0 in the upper left panel of Figure 3 is thus our main regression result (as given in column 1 of Table 4). Further, -3 indicates that we have removed the three districts in our comparison group with lowest share of children with immigrant background; and 3 indicates that we have added the three districts outside our comparison group with highest share of children with immigrant background.

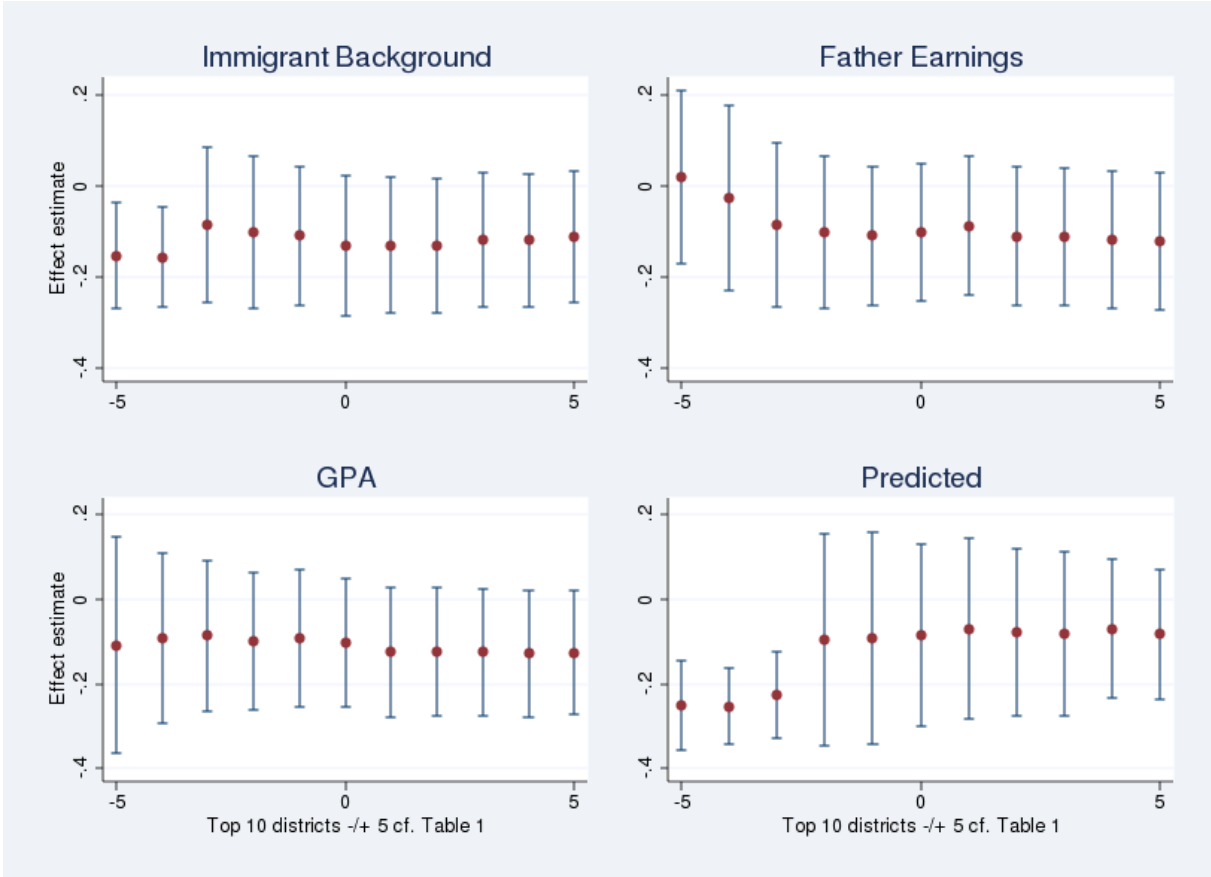
The other three panels of Figures 3 and 4 present similar robustness checks based on other definitions of the comparison group; cf. columns 2-4 in Table 1. We see from Figure 3 that our main regression result for the girls persists across these variations in definition of comparison area. The standard errors tend to increase somewhat as the number of districts included in the comparison area declines, but, overall, our main result seems robust to alternative definitions. For the boys (Figure 4), the effect estimate is more sensitive to variations in the comparison group. In most cases the effect estimate is negative but insignificant. In a few cases, however, the negative effect estimate becomes statistically significant, but there is also an example where it becomes positive. This makes it hard to conclude on how the intervention affected the school performance of the boys.

**Figure 3: Girls. Robustness of main result to variations in what city districts are included in the comparison group**



Note: Comparison group redefined to be the top 10 non-treated districts in the indicated columns of Table 1 (at 0) and adding/dropping one to five of the districts closest in rank in the same column. The upper left panel (cf. column 1 of Table 1) thus replicates our main regression result (first column of Table 4) at x-axis value 0; and -5 (5) indicates that we have dropped (added) the five districts with lowest (highest) immigrant background share from (to) our comparison group.

**Figure 4: Boys. Robustness of main result to variations in what city districts are included in the comparison group**



Note: Comparison group redefined to be the top 10 non-treated districts in the indicated columns of Table 1 (at 0) and adding/dropping one to five of the districts closest in rank in the same column. The upper left panel (cf. column 1 of Table 1) thus replicates our main regression result (first column of Table 4) at x-axis value 0; and -5 (5) indicates that we have dropped (added) the five districts with lowest (highest) immigrant background share from (to) our comparison group.

We recall from Table 2 that there were some differences in the development of some of the observables in the two areas over time. In Table 5 we explore how our main result is affected by excluding covariates from the regression. None of our numerous covariates seem to have any substantial impact on the effect estimate. Excluding the education of the mother and father, however, moves the estimate somewhat. This may be taken to suggest that a part of the increase in girls’ achievements can be explained by a change in the composition of fathers and/or mothers with respect to their education level. Recalling from Table 2 that educational achievement is missing for more than half of the parents, it is seems difficult to disentangle whether changes in this variable reflects actual changes in composition or some difference over time and space in registration of actual educational achievement for persons with missing on education in the registry. In any case, there should be little need to worry too much as the point estimate increases less than one standard error.

As discussed in Section 4, we may be concerned that there is a secular trend causing children with poor school performance to leave our intervention districts. We now explore this in several ways.

First, if our main results is driven by an underlying trend that increases school performance more in the treatment than the comparison area (catching up), we should see larger effect estimates when we move the comparison cohort away from the treatment cohort. The first column of Table 6 provides our main result from Table 4 (first column). In the second column of Table 6, we report estimates where we have substituted the 1992 pre-intervention cohort with the 1991 cohort; and in the third column we have included data for all available cohorts (1988-1993) and included all previous cohorts<sup>27</sup> in the pre-intervention group. We see that the effect estimate for girls remains in the similar magnitude, though it declines somewhat. The effect estimate for boys is close to zero and insignificant.

Second, if there is a tendency for school performance in the treatment area to catch up with performance in the comparison area, we would expect our effect estimate to decline when we control for district-specific linear trends. In column four of Table 6 we have included data for all available cohorts (1988-1993) and replicated our main regression result on this extended sample. In column five we have included district-specific dummies, and in column six we have also added district-specific linear trends. We note that our effect estimate is about 0.2 for the girls on this sample of all cohorts, and, importantly, that this effect estimate changes little when we control for district dummies and district specific linear trends.

In the last column of Table 6 we report effect estimates when allowing for dummies for the school of graduation and year dummies (and interactions between school and year dummies). If teachers respond to better student achievements by increasing the achievement necessary to obtain a given grade, then our effect estimate may be biased downwards. Such relative grading should be largely accounted for by controlling for school fixed effects, and the result in the last column of Table 6 indicates that this is not biasing our effect estimate. Table 7 provides further indication that relative grading is not a problem for us. All children graduating from 10<sup>th</sup> grade have about one written and one oral exam, which are evaluated by school external personnel. In Table 7 we report effect estimates using results from the exam as outcome variable, and we see that the effect estimate is similar to the one in our main regression (Table 4, column 1).

Third, if there is a catch up going on, we would not only expect positive effect estimates between our two cohorts (1992 and 1993), but also between previous cohorts for which there was no preschool intervention. In the first column of Table 8 we report a placebo test where we have

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<sup>27</sup> Note that the cohorts born 1990 and before started school as 7 year-olds and are hence not entirely comparable to the cohorts born 1991 and onwards as they have completed only 9 years of primary education compared to 10 years for the later cohorts.



incorrectly set the reform to affect the 1992 cohort, and we use the 1991 cohort as the pre-reform cohort. We see that there are no indications of a significant treatment effect before the intervention happened.

As discussed in Section 4, we may be concerned that our effect estimates are driven by other programs being introduced at the same time as the preschool intervention. The Plan of Action Oslo Centre/East (PAOCE) was ongoing in the intervention districts at the same time of the preschool intervention (cf. discussion in Sections 3 and 4). PAOCE similarly affected children from native and immigrant families, while the intervention was mainly directed towards children with immigrant background. Thus, if our estimate of the effect of the preschool intervention were in fact driven by PAOCE, we should expect to see similar effects for children from native and immigrant families. In the second column of Table 8 we report results based on a dataset of children from native families that is constructed analogous to our main analytic sample. We see that the effect estimate is virtually zero (and insignificant) for both boys and girls.

In column three of Table 8 we report effect estimates from a dif-in-dif-in-dif regression utilizing a sample that is the union of our main analytic sample and the analogous sample of children from native families (used in column two). We see that the effect estimate on our girls with immigrant background declines somewhat compared to our main specification, but it remains large (0.2) and statistically significant.<sup>28</sup> The cash-for-care reform could potentially also interfere with our effect estimate if there were more children with younger siblings in the relevant age in the treatment area. Since children from native families were similarly affected by the cash-for-care reform, the dif-in-dif-in-dif results in column two should largely account for this.

In Table 9 we report results of similar placebo tests based on a dataset covering all 1988-1993 cohorts. This matrix of “treatment effects” is reported for all cohorts from 1989 to 1993 relative to the excluded cohort (1992). Again, we see that the only positive and significant effect is the one for cohort of girls actually exposed to the preschool intervention (1993). It is worth noticing the results for the boys. Although never significant, the effect estimate is negative in all years, indicating that the boys perform particularly well in the excluded cohort (1992). This suggests that one should be careful in attributing the negative coefficient in the main result for the boys to the intervention.

Another concern discussed in Section 4 relates to “attrition” due to missing on our measures of school performance. If children, who would have emigrated in the absence of the intervention,

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<sup>28</sup> Note that this dif-in-dif-in-dif could also be interpreted as an investigation of a mechanism. The native children in preschool also got subsidized by the preschool intervention (four hours free of charge daily), but since most of them were already attending preschool, this is a pure income effect. By showing an effect on girls with immigrant background even after differencing out the effect on native children, this may be taken to indicate that it is not the income effect of the intervention that causes the school performance of girls with immigrant background to improve.

remained in Norway, this may bias our effect estimate (because such children did not remain in Norway in the comparison groups). To explore this we construct a dataset that is the union of our main analytic sample and the analogous sample of all children with immigrant background who emigrated between preschool and age 16. Based on this dataset, we estimate the effect of the preschool intervention on emigration. We see from the first column of Table 10 that there is no indication that this is the case.

Attrition could also be a problem if there is a relationship between not graduating on-time and the intervention. To explore this we construct a dataset that is the union of our main analytic sample and the analogous sample of all children with immigrant background who have missing on school performance and who lived in Norway at age 16. Based on this dataset, we estimate the effect of the preschool intervention on not graduating on-time (second column, Table 10). Overall, there is no indication of a relationship between exposure to the intervention and being excluded from our sample.

## **7 Discussion of Mechanisms**

Our results suggest that girls from immigrant families are positively affected by free preschool. As indicated in Section 2, benign effects of preschool may have a number of reasons, and here we discuss the plausibility of potential mechanisms.

The motivation for the intervention was to improve the language skills of the children from immigrant families. Previous literature, both in economics and other fields, have emphasized that the development of language skills at early ages are paramount to later educational achievements (Bleakly and Chin 2008, Hakuta et al. (2003), Vygotskij 1962). Given the emphasis on language skills, we may expect to see large effects on grades in language than in other subjects. However, to the extent that development of language is a prerequisite for any learning, beneficial effects of the reform should not be restricted to language subjects only. In Table 11 we report results on our main analytic sample where GPA is broken down on the three sub-categories Language (first column), Science/Math (second column) and Other Subjects (last column).

For the girls we see that there are large and positive effect estimates across all sub-categories. For the boys, the effect estimates are negative but never statistically significant. The substantial effect on girls' grades in language suggests that the intention of the preschool intervention was largely achieved for the girls. These findings provide no clear evidence that improved development of language skills in preschool is crucial for later school performance, but the possibility of a particularly important role for early stimulation of language development remains.

The provision of free preschool four hours a day may also improve the parents' integration into the Norwegian society, for example through their ability to attain work or attend education. In

Table 12 we report results from the estimation of the effect of the preschool intervention on the mothers' and fathers' earnings and education. We find no significant effect on the parents' earnings or education. This suggests that the effect on the girls' school performance is not due to an increase in parents' integration into the labor market. The lack of effect on earnings indicates that the effect on girls' school performance is not due to improved household income.<sup>29</sup> Overall, the data provides little reason to believe that integration of the parents is an important contributor to the beneficial effect on the girls' school performance.

Finally, we discuss possible reasons why the intervention is more beneficial to girls than boys. Other studies of early interventions that look at non-immigrant children have also found more beneficial effects for girls than for boys (Havnes and Mogstad 2009, Joo 2010, Anderson 2008, Campbell et al. 2002, Schweinhart et al. 1993). This indicates that the immigrant background of our children may not be important in explaining this gender difference. It is still possible that the bigger effect for the girls could be a result of different degrees of treatment for girls and boys. If, for example, almost all boys with immigrant parents were already attending preschool before the intervention, then all the new preschool slots introduced by the intervention were taken up by girls. If so, it is not surprising that we see no effect for the boys (since the intervention did not affect their attendance) and a substantial effect for the girls (they now benefit from attending just like the boys already benefited). Unfortunately, we do not have individual information on the gender of the children who actually took up the new slots, though qualitative reports do not indicate that there were far more girls than boys. Moreover, this interpretation does appear unlikely to be very important given that similar gender differences also appear in previous studies with random assignment (Anderson 2008).

Overall, however, we cannot really tell whether a benign effect for girls, but not for boys, is because the girls benefit more from *attending* preschool *or* because the intervention made more girls than boys *start* attending preschool. The distinction between these two channels can have important policy implications for the boys. To the extent that the result for the girls are solely driven by higher attendance rates, then we may expect future interventions which are able to increase the attendance rates of both girls and boys, to improve the outcome of girls and boys alike. However, to the extent that the result is driven by different effects of preschool on boys and girls, then there is little reason to expect future interventions to also benefit the boys. For the girls, however, to be able to discriminate between these two channels has limited importance for policy. The intervention improved the school

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<sup>29</sup> The native children in preschool also got subsidized by the preschool intervention (four hours free of charge daily), but since most of them were already attending preschool, this is a pure income effect. By showing an effect on girls with immigrant background even after differencing out the effect on native children, as we did in Table 8 (third column), may be taken to indicate that it is not the income effect of the intervention that causes the school performance of girls with immigrant background to improve.

performance of the girls, which, from a policy perspective, is attractive regardless of whether it happened because the girls benefit more from attending preschool or because the intervention made them attend preschool more.

## 8 Conclusion

It is important for policy makers to improve the integration and social mobility of children from immigrant families. Not only does an increase in human capital of an ethnic group affect the future for the individual, but according to Borjas (1992), it also affects future generations within this particular group. A lack of proficiency in the language of the majority seem to be one important determinant of the often unfavorable destiny of children from immigrant families (Bleakly and Chin 2008).

We analyze the effect of an intervention meant to improve the language skills and integration of children from immigrant families through providing free preschool. Using a difference-in-difference approach we find a positive effect of the preschool intervention on girls' cognitive skills, measured by the grade point average at graduation from compulsory school (age 16). In line with previous studies there are no similar effects for boys, and we find no support for any negative effect on non-cognitive outcomes.

We perform a number of robustness and specification checks, of which none are at odds with our main identifying assumption of a common trend in comparison and treatment group. Several mechanisms may explain an effect of preschool on school performance. A large effect on girls' grades in language suggests that the intention of the preschool intervention was largely achieved for the girls. On the other hand, the earnings and education of the parents seem unaffected by the preschool intervention, suggesting that better integration of the parents into the Norwegian society is not important in explaining the improved educational achievements of the girls.

Why was the intervention more beneficial to girls than to the boys? Strictly speaking, our data do not really enable us to say whether this gender difference occurs because the girls benefit more than boys from attending preschool or because they attend preschool more. We do, however, have little indication of significant gender differences in take-up, and previous studies of early interventions towards non-immigrant children typically also find more beneficial effects for girls than for boys (Havnes and Mogstad 2009, Joo 2010, Anderson 2008, Cambell et al. 2002, Schweinhart et al. 1993). This indicates that the immigrant background of our children may not be important in explaining this gender difference.

From a policy perspective, an intervention that improves the school performance of girls from immigrant families seems attractive; regardless of whether the girls benefit more than boys from attending preschool or because they attend preschool more. We still think that the strong differing

effect on school performance by gender remains somewhat puzzling. A more thorough understanding of why early interventions often seem to be more beneficial for girls than for boys would be helpful when designing future policies aimed at improving the destinies of children from disadvantaged families.

Preschool free of charge was offered to children in particularly challenged districts in Oslo with a high share of immigrant families. Thus, our results do not necessarily generalize to the population of children from immigrant families in general. However, as most western cities with a substantial population of immigrant families experience a concentration in particular neighborhoods, our findings might suggest that provision of free preschool could be a powerful weapon in the battle for improved educational outcomes, integration and social mobility of girls from immigrant families in such areas.

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## Details on preschool interventions in Oslo around 1998

Several reports from the early 1990s and onwards emphasized the large differences in living conditions among the city districts in Oslo.<sup>30</sup> This was followed by a white paper from the government on immigration and the multi-cultural Norway in 1997 (St. meld 1996-97). One recommendation from the report that was later agreed upon in the parliament was the funding and provision of 4 hours of preschool free of charge to 5 year-olds in one city district in Oslo. The city district chosen was Gamle Oslo, the most disadvantaged of the center/east city districts.

### Gamle Oslo (GO)<sup>31</sup>

Initially, GO had a low coverage of preschool slots, and the city district was chosen to become a pilot project financed by The Norwegian Ministry for Children and the Family. The Ministry contributed with 10 million NOK yearly (autumn 1998 approximately 5 million), and the funding should cover the building of new preschool slots and provide the children with preschool free of charge. The city district itself states in a report from 1999 that the objective was to provide all children with experience from preschool before they started school.<sup>32</sup> One of the new preschool centers was aimed at the new starters and offered part-time slots only.

GO estimated that 166 children in the relevant age group were not attending pre-school before the intervention. During the autumn 1998 about 100 new children started pre-school, this amounts to 60 % of all children not in pre-school at the start of the year.

Although all children received the offer of a free part time slot, the target group for the intervention was children from immigrant families. In GO approximately 90 % of the new pre-schoolers came from immigrant families. It was emphasized that the pedagogical content of the preschool should emphasize improving the language skills of the children. Multilingual preschool teachers and assistants were hired. Importantly, families with 5 year-old children who had not applied for a preschool slot were systematically approached by representatives from the city district in order to enhance participation.

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<sup>30</sup> See for instance Hagen et al. (1994): *Oslo: den delte byen?* Faforapport nr. 16

<sup>31</sup> Unless anything else is specified, the content of this sub-section is based on information from the qualitative evaluation report "Gratis barnehage for alle femåringer i bydel Gamle Oslo" by Trude Brita Nergård (NOVA report 3/02).

<sup>32</sup> From "Rapport februar 1999": Prosjekt gratis korttidsplass for alle femåringer i bydel Gamle Oslo.

## **Grünerløkka (GL)**

Paralleled with the expansion in GO, another and more extensive program was initiated called “Plan of Action Oslo Centre/East” (Handlingsprogram Oslo indre øst). The targeted area was the city districts GO, GL and Sagene-Torshov. A part of this program involved the provision of free preschool for preschoolers aged five. While GO already had their preschool expansion fully financed by the Ministry, the two remaining city districts received funding for their preschool offer through the project. GL started their project 1<sup>st</sup> of October 1998<sup>33</sup>. All five-year-old children in the district that were not already in preschool received the offer.<sup>34</sup> During the first year the new children were integrated into regular preschool centers. This was funded on the idea that “language stimulation and the acquiring of social skills are best implemented across country origin/ethnicities and by teachers qualified to work with these particular challenges”<sup>35</sup>. According to the annual report on the intervention in the city district, families of several of the children who started pre-school when receiving the offer, decided to increase the time the child attended to full time because they observed improvements in the child’s language development. Families with 5 year-old children that had not applied for a preschool slot were systematically approached by representatives from the city district in order to enhance participation. GL did, in line with GO, employ multilingual teachers to meet the new children.

## **Sagene-Torshov (ST)**

ST was also a part of the Plan of Action, but they did not start offering preschool free of charge until spring 1999<sup>36</sup>. Moreover, very few children seem to have started on the program, and families of children who did not apply for preschool seem not to have been approached by representatives from the city district (like they were in GO and GL). Indeed, documents from the budgetary process for year 2000 underlines that the program has not been successful in recruiting children: “The city district council note that the demand for the 5-year old preschool offer has not been as expected. The city district council therefore suggests that an evaluation is implemented during the first quarter of 2000”<sup>37</sup>.

Thus, the intervention in ST does not seem to be comparable to the ones undertaken in GO and GL. In ST it did not officially start before spring 1999. Moreover, it seems to have been largely unsuccessful in recruiting immigrant children to attend preschool throughout 1999. This indicates that

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<sup>33</sup> City district council case 136/98 and 137/98.

<sup>34</sup> Handlingsprogram for Oslo indre øst, årsrapport 1999 (Plan of Action Oslo Centre/East, annual report 1999)

<sup>35</sup> Satsinger på oppvekst i bydel Grünerløkka under Handlingsprogram Oslo indre øst 1997-2005. En intern evalueringsrapport. Internal evaluation report.

<sup>36</sup> City district council meeting 26<sup>th</sup> of November 1998.

<sup>37</sup> Suggestion from the children and youth committee to the City district meeting 16<sup>th</sup> of December 1999.

there was no real intervention going on in ST, and we have therefore excluded ST from the analysis. In the Appendix Table 1 below we still report our main results (Table 4) with i) ST excluded from the sample (as in Table 4), ii) ST included in the treatment group, and iii) ST included in the comparison group. As we might expect, we see that the point estimate declines modestly when ST is included in the treatment group. Including ST in the comparison group, however, has a negligible impact on our main result from Table 4.

**Table A1. Sagene/Torshov**

	ST excluded from sample	ST in treatment group	ST in comparison group
Treatxpostxgirl	0.26 (0.10)*	0.21 (0.10)	0.27 (0.10)*
Treatxpostxboy	-0.13 (0.08)	-0.12 (0.08)	-0.13 (0.07)
Observations	1800	1918	1918
R-squared	0.16	0.16	0.16
Mean of dep. var.	3.85	3.85	3.85

*Note:* All models are OLS estimates of the effect on GPA of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text)

**Stovner (SR)<sup>38</sup>**

Some time during the mid-nineties the city district SR started offering a small-scale child care program for the children that were not attending the regular child care centers. The target group was children from immigrant families, but children from native families were also allowed to attend. Children from age 3 could participate, and they could continue until they started school. The idea was to facilitate the language learning process of children from immigrant families as well as to introduce their parents to the institutions in the city district before the children started school. The offer was not free, but parents paid a very low fee.

The small-scale offer amounted to 16 hours of child care every week. The groups would consist of approximately 18 children, and there would be 2 teachers in each group. The activities were slightly less intensive than in regular child care centers, with fewer excursions and fewer teachers per child. Approximately 60 children attended the small scale program each year. As the city district of SR

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<sup>38</sup> This sub-section is based on an interview with the former unit director for child care centers in Stovner city district, Mary Kristensen.

changed its borders in 2004, we have not been able to retain detailed written information on this small-scale child care program.

We have decided to include the city district in the comparison group because the program most likely was in place when the treatment districts (GO and GL) introduced their program in 1998. Our difference-in-difference analysis should hence handle the fact that SR had already implemented a possibly somewhat similar program.

In the Appendix Table 2 below we still report our main results (Table 4) with i) SR included in the comparison group (as in Table 4), ii) SR excluded from the sample, and iii) SR included in the treatment group. We see that the point estimate increases modestly if we exclude SR from the sample or include SR in the treatment group.

**Table A2. Stovner**

	Stovner in comparison group	Stovner excluded from sample	Stovner in treatment group
Treatxpostxgirl	0.26 (0.10)*	0.29 (0.10)*	0.29 (0.07)*
Treatxpostxboy	-0.13 (0.08)	-0.15 (0.09)	-0.15 (0.09)
Observations	1800	1617	1800
R-squared	0.16	0.16	0.16
Mean of dep. var.	3.85	3.83	3.85

*Note:* All models are OLS estimates of the effect on GPA of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text)

## **Initiatives under the Plan of Action Oslo Centre/East (PAOCE)**

From 1997-2006 a number of initiatives were directed towards children in the three city districts Gamle Oslo (GO), Grünerløkka (GL) and Sagene-Torshov (ST). We should keep in mind that only way PAOCE may bias effect estimates of the preschool intervention is if children in the pre-treatment cohort is differently affected than the post-treatment cohort.

### **1. School-related activities**

The schools in GO, GL and ST carried out a number of different smaller projects during the 10 years of the funding from PAOCE. Many schools offered homework assistance where the children could do their homework under supervision of teachers after school. Some schools offered subsidized after-school activities. A number of schools expanded their libraries, giving the children greater access to literature. And lastly, a couple of schools upgraded their computer rooms and even let the children stay behind after school using the equipment for homework.

### **2. Culture and outdoor activities**

Several smaller projects directed towards children were completed during the period. School bands got financial support, one school started a choir and theatre groups were founded. One school received funding for film equipment, and used this to improve the children's knowledge of language and culture.

A substantial funding was given to promote outdoor activities, and a center was established in GO organizing trips and activities for all the schools in the three districts.

## Tables

**Table 1: City districts ranked by given characteristic of each district**

Share of children with immigrant background			Average of fathers' earnings		Average of GPA		Predicted probability of being treated	
City district	N	Share	City district	Earn.	City district	GPA	City district	Pr.
Gamle Oslo	348	0,59	Gamle Oslo	131 902	Gamle Oslo	3,78	Ekeberg	0.30
Grunerløkka	282	0,48	Grunerløkka	141 264	Romsås	3,79	Majorstua	0.30
Stovner	281	0,42	Sagene <sup>1</sup>	178 388	Lambertseter	3,85	Gamle Oslo	0.27
Sagene <sup>1</sup>	209	0,36	Romsås	184 763	Grunerløkka	3,90	Ullern	0.26
Helsfyr	189	0,36	Helsfyr	188 666	Furuset	3,93	Grunerløkka	0.25
Hellerud	193	0,34	Lambertseter	191 897	Hellerud	3,95	Søndre Nordstrand	0.22
Grorud	202	0,33	Stovner	204 231	Søndre Nordstrand	4,01	Sagene <sup>1</sup>	0.21
Søndre Nordstrand	626	0,31	Furuset	215 059	Stovner	4,01	Lambertseter	0.21
Romsås	126	0,31	Hellerud	216 060	Helsfyr	4,04	Hellerud	0.21
Furuset	426	0,29	Søndre Nordstrand	220 071	Bjerke	4,09	Nordstrand	0.20
Lambertseter	110	0,25	Grorud	229 197	Østensjø	4,09	Furuset	0.20
Bjerke	284	0,22	Bjerke	227 038	Sagene <sup>1</sup>	4,10	Grefsen	0.20
Bøler	173	0,18	Østensjø	257 723	Grorud	4,10	Helsfyr	0.20
Ekeberg	219	0,14	St. Haugen	263 814	Bøler	4,17	St. Hanshaugen	0.20
Manglerud	135	0,10	Bøler	269 412	Bygdøy	4,27	Romsaas	0.19
St. Hanshaugen	214	0,09	Manglerud	288 903	Manglerud	4,29	Røa	0.19
Røa	293	0,09	Sogn	297 166	Majorstua	4,32	Stovner	0.19
Østensjø	183	0,09	Majorstua	324 625	Ekeberg	4,32	Bjerke	0.19
Sogn	221	0,09	Grefsen	330 661	Nordstrand	4,34	Grorud	0.19
Majorstua	156	0,08	Ekeberg	343 674	Grefsen	4,36	Bygdoy	0.18
Ullern	336	0,07	Nordstrand	371 522	Ullern	4,37	Bøler	0.18
Bygdøy	131	0,06	Røa	394 405	Røa	4,39	Manglerud	0.18
Vindern	287	0,06	Ullern	449 202	St. Hanshaugen	4,45	Østensjø	0.17
Grefsen	217	0,04	Vinderen	465 334	Sogn	4,48	Sogn	0.13
Nordstrand	230	0,04	Bygdøy	497 258	Vinderen	4,50	Vinderen	0.13

*Note:* Characteristics of the districts in the pre-reform cohort born in 1992. N is the overall number of children born 1992 registered living in the respective district 31<sup>st</sup> of December 1996 (age 4). The share of children with immigrant background is the share of all these children (N) with immigrant background. The average of fathers' earnings is the mean of the earnings of the fathers of all these children (N). Average GPA is the mean of the GPA of all these children in 2008 (missings are excluded). The predicted probability of being treated is the mean of the individual probability predicted from a logit regression of living in the treatment area (in 1996) on all the covariates included in our main regressions (cf. Section 5.2.). Note, however, that the sample in this regression is based solely on children with immigrant background (like in our main regression in Table 4; cf. Section 5).<sup>1</sup> This district is excluded from all samples employed in the paper, cf. Appendix 1.

**Table 2: Main analytic sample: Summary statistics of covariates**

	Treatment area			Comparison area			
	1992 cohort	1993 cohort	Diff	1992 cohort	1993 cohort	Diff	Diff-In-Dif
M below 22	0.11	0.12	0.01	0.08	0.09	0.01	-0.00
M age miss	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Father below 22	0.03	0.02	-0.01	0.02	0.01	-0.01	0.00
F age miss	0.01	0.02	0.01	0.02	0.02	0.00	0.01
M finished HS	0.06	0.11	0.05*	0.17	0.13	-0.05*	0.10*
M educ. miss	0.71	0.66	-0.05	0.54	0.56	0.02	-0.07
F finished HS	0.21	0.20	-0.01	0.29	0.25	-0.04	0.03
F educ. miss	0.49	0.46	-0.03	0.37	0.35	-0.02	-0.01
M on welfare	0.19	0.16	-0.03	0.09	0.09	0.00	-0.03
F on welfare	0.42	0.35	-0.07	0.29	0.30	0.02	-0.08
M working	0.28	0.31	0.03	0.44	0.44	0.00	0.03
F working	0.70	0.71	0.01	0.78	0.79	0.01	0.00
M earnings	26 517	29 760	3 242	52 869	57 167	4 299	-1 056
F earnings	109 851	125 047	15 195	139 909	153 655	13 746	1 450
M from OECD	0.11	0.09	-0.03	0.11	0.12	0.01	-0.03
F from OECD	0.11	0.09	-0.02	0.09	0.11	0.02	-0.03
First gen	0.10	0.07	-0.03	0.07	0.07	0.00	-0.03
Birth quarter 1	0.26	0.26	-0.01	0.26	0.25	-0.01	-0.00
Birth quarter 2	0.26	0.24	-0.02	0.25	0.25	0.00	-0.02
Birth quarter 3	0.21	0.26	0.06	0.25	0.27	0.02	0.03
Birth quarter 4	0.27	0.24	-0.03	0.24	0.22	-0.02	-0.01
N	247	261		641	651		

Note: Mean or share of indicated variable, with differences. All variables are measured when the child is 4 years old. Earnings are in current prices (NOK). \* indicates significance at the 5 percent level (two-sided t-test).



**Table 3: Mean of GPA in the four groups**

	Treatment area			Comparison area			Diff-in-Diff
	1992 cohort	1993 cohort	Diff	1992 cohort	1993 cohort	Diff	
Girls	3.817 (0.071) n=124	4.104 (0.066) n=130	0.287* (0.097)	4.100 (0.047) n=313	4.037 (0.042) n=310	-0.062 (0.063)	0.349* (0.116)
Boys	3.556 (0.067) n=123	3.488 (0.066) n=131	-0.069 (0.094)	3.702 (0.046) n=328	3.749 (0.043) n=341	0.047 (0.063)	-0.116 (0.113)

Note. Grade point average (GPA) at time of graduation from compulsory school (10<sup>th</sup> grade, i.e. age 16), i.e. in 2008 for the 1992-cohort and in 2009 for the 1993-cohort. Standard errors in parenthesis. \* indicates significance at the 5 percent level (two-sided t-test).

**Table 4: Regression Results: Grade point average and theoretical subjects**

	GPA	1	Behavior
Effect estimate girls	0.26 (0.10)*		0.01 (0.02)
Effect estimate boys	-0.13 (0.08)		0.01 (0.04)
Observations	1800		1800
R-squared	0.16		0.05
Mean of dep. var.	3.85		0.94

Note: All models are OLS estimates of the effect on the given measure of school performance (10<sup>th</sup> grade) of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text)

**Table 5: Robustness: Excluding more and more covariates**

	With all covariates	<b>Dropping</b> parents age	and fathers income	and mothers income	and parents education	and parents welfare measure	and first generation dummy	and quarter of birth	No covariates
Effect estimate girls	0.26 (0.10)*	0.27 (0.10)*	0.26 (0.10)*	0.26 (0.09)*	0.33 (0.08)*	0.33 (0.08)*	0.35 (0.07)*	0.34 (0.07)*	0.35 (0.07)*
Effect estimate boys	-0.13 (0.08)	-0.13 (0.08)	-0.14 (0.08)	-0.15 (0.08)	-0.17 (0.09)	-0.13 (0.09)	-0.14 (0.09)	-0.13 (0.09)	-0.12 (0.09)
Observations	1800	1800	1800	1800	1800	1800	1800	1800	1800
R-squared	0.16	0.16	0.15	0.15	0.10	0.08	0.08	0.07	0.06
Mean of dep. var.	3.85	3.85	3.85	3.85	3.85	3.85	3.85	3.85	3.85

*Note:* The first column replicates our main result (i.e. column 1 of Table 4). All models are OLS estimates of the effect on GPA of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts.

**Table 6: Further robustness**

	1992 as pre-reform cohort	1991 as pre-reform cohort	1988-1992 as pre-reform cohorts	1988-1992 as pre-reform cohorts	1988-1992 as pre-reform cohorts	1988-1992 as pre-reform cohorts	1988-1992 as pre-reform cohorts
Effect estimate girls	0.26 (0.10)*	0.18 (0.10)	0.21 (0.06)*	0.21 (0.06)*	0.21 (0.06)*	0.24 (0.14)	0.21 (0.08)*
Effect estimate boys	-0.13 (0.08)	0.02 (0.06)	-0.02 (0.05)	-0.02 (0.05)	-0.01 (0.05)	0.01 (0.11)	-0.04 (0.07)
Cohorts included in sample	1992, 1993	1991, 1993	1988-1993	1988-1993	1988-1993	1988-1993	1988-1993
Cohort dummy omitted	1992	1991	1988-1992	1992	1992	1992	1992
Additional covariates included				Year fixed effects	Year and district fixed effects	Year and district fixed effects, and district specific trends	Year and school fixed effects (with interactions)
Observations	1800	1805	4945	4945	4945	4945	4945
R-squared	0.16	0.15	0.13	0.14	0.14	0.14	0.27
Mean of dep. var.	3.85	3.83	3.78	3.78	3.78	3.78	3.78

*Note:* The first column replicates our main result (i.e. column 1 of Table 4). Results in the second column based on the sample of cohorts born 1991 and 1993 only, while results in the other columns are based on the sample of all cohorts (born 1988-1993). All models are OLS estimates of the effect on GPA of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts.

**Table 7: Exam results**

	Written exam	Oral exam	Average of written and oral exam
Effect estimate girls	0.18 (0.11)	0.27 (0.25)	0.17 (0.09)
Effect estimate boys	-0.13 (0.25)	-0.06 (0.08)	-0.12 (0.18)
Observations	1767	1619	1784
R-squared	0.13	0.10	0.14
Mean of dep. var.	3.18	4.10	3.59

*Note:* All models are OLS estimates of the effect on the given exam as measure of school performance (10<sup>th</sup> grade) of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text).

**Table 8: Placebo reform and effect on native children**

	Placebo reform in 1992	Children from native families	Diff-in-dif-in-dif
Effect estimate girls	-0.08 (0.06)	0.02 (0.15)	0.22 (0.08)*
Effect estimate boys	0.14 (0.09)	-0.03 (0.11)	-0.10 (0.16)
Observations			
R-squared	1781	3780	5580
Mean of dep. var.	0.15	0.25	0.23

*Note:* First column reports OLS estimates of the “effect” on GPA of exposure to a placebo preschool intervention in 1992 (using children with immigrant background born in 1991 as the pre-intervention cohort). Second column reports OLS estimates of the effect on GPA of children with native background (cohorts 1992 and 1993). Third column reports OLS estimates of the effect on GPA of children with immigrant background where we have also differenced out the effect on children with native background in a dif-in-dif-in-dif regression (cohorts 1992 and 1993). \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text).

**Table 9: Placebo matrix**

	1992 baseline
“Effect estimate” girls born 1988	0.116 (0.115)
“Effect estimate” girls born 1989	0.127 (0.150)
“Effect estimate” girls born 1990	-0.029 (0.103)
“Effect estimate” girls born 1991	0.087 (0.059)
“Effect estimate” girls born 1993	0.264 (0.096)*
“Effect estimate” boys born 1988	-0.012 (0.188)
“Effect estimate” boys born 1989	-0.142 (0.090)
“Effect estimate” boys born 1990	-0.210 (0.119)
“Effect estimate” boys born 1991	-0.130 (0.096)
“Effect estimate” boys born 1993	-0.118 (0.078)
Observations	4945
R-squared	0.14
Mean of dep. var.	3.77811

*Note:* All models are OLS estimates of the “effect” on GPA of exposure to a placebo intervention (cohorts born before 1993) or the actual preschool intervention (1993 cohort). The reference year is 1992. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text).

**Table 10: Effect of intervention on attrition**

	Emigration	Be resident and have missing on GPA
Effect estimate girls	0.01 (0.05)	0.01 (0.04)
Effect estimate boys	0.03 (0.05)	0.02 (0.04)
Observations	2068	2021
R-squared	0.05	0.02
Mean of dep. var.	0.13	0.10

*Note:* Result in first column based on a dataset that is the union of our main analytic sample and analogous children with immigrant background who emigrated before age 16. Result in second column based on a dataset that is the union of our main analytic sample and analogous children with immigrant background who was not registered as emigrated at age 16 and who still had missing on GPA. All models are OLS estimates of the effect on school performance (10<sup>th</sup> grade) of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text).

**Table 11: Mechanisms: Different subjects**

	Language	Math/science	Other subjects
Effect estimate girls	0.30 (0.11)*	0.35 (0.11)*	0.21 (0.10)
Effect estimate boys	-0.08 (0.10)	-0.12 (0.16)	-0.17 (0.08)
Observations	1800	1800	1800
R-squared	0.18	0.13	0.13
Mean of dep. var.	3.67	3.85	4.09

*Note:* All models are OLS estimates of the effect on school performance (10<sup>th</sup> grade) of exposure to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text). Fewer observations in the education regressions reflect missing information on education.

**Table 12: Mechanisms: Parental outcomes**

	Mothers' earnings	Mother finished high school	Fathers' earnings	Father finished high school
Effect estimate girls' parents	965 (5457)	0.02 (0.04)	-2 368 (11620)	0.08 (0.05)
Effect estimate boys' parents	627 (8 669)	0.05 (0.05)	3 019 (8 895)	-0.00 (0.02)
Observations	1800	1076	1800	1273
R-squared	0.54	0.68	0.61	0.84
Mean of dep. var.	71 957	0.32	164 708	0.41

*Note:* All models are OLS estimates of the effect on parents' earnings and education of the children being exposed to the preschool intervention. \* denotes significance at the 5 percent levels. Robust standard errors (in parentheses) are corrected for non-independent residuals within city districts. All models includes cohort and year fixed effects, as well as covariates capturing individual (child, mother and father) characteristics (described in text).