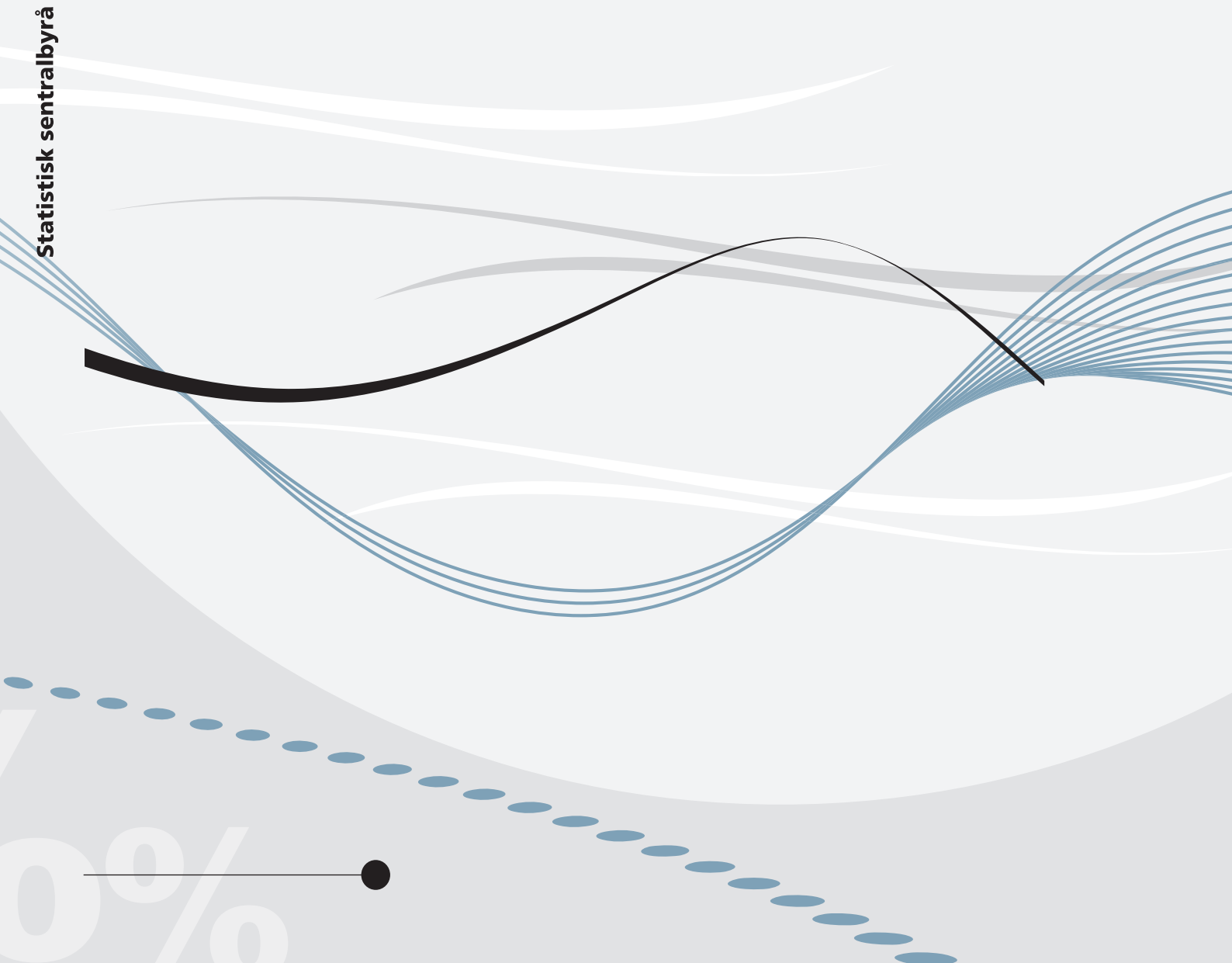


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Kindergarten for all: Long run effects of a universal intervention



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Abstract:

Theory and evidence points towards particularly positive effects of high-quality child care for disadvantaged children. At the same time, disadvantaged families often sort out of existing programs. To counter differences in learning outcomes between children from different socioeconomic backgrounds, European governments are pushing for universal child care. However, empirical evidence on the effects of universal programs is scarce. We provide evidence on the long-run effect on schooling of mandating kindergarten at age 5--6. Our identifying variation comes from a reform that lowered school starting-age from 7 to 6 in Norway in 1997. Our precise DD estimates reveal hardly any effect, both overall, across subsamples, and over the grading distribution. A battery of specification checks supports our empirical strategy.

Keywords: kindergarten, early childhood intervention, distributional effects, difference-in-differences, child care, child development

JEL classification: J13, H40

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Sammendrag

Studier av intensive programmer som fremmer tidlig læring har vist gode effekter på utviklingen til barn fra vanskeligstilte familier. Imidlertid vet vi mindre om hvordan store, universelle læringsprogrammer påvirker barns utvikling. Vi tar utgangspunkt i Reform 97, en reform som gjorde det obligatorisk for seksåringene å starte på skolen i et førskolelignende program som vektla læring gjennom lek. Bakgrunnen for å innføre skolestart for seksåringene var at man ville gi alle barn et likeverdig pedagogisk tilbud uavhengig av bosted og sosioøkonomisk bakgrunn. Vi studerer effektene av reformen på barns senere karakterer når de går ut av ungdomsskolen. Vi ser også på om reformen påvirket barnas sannsynlighet for å fullføre videregående på normert tid, eller om den førte til at flere valgte akademisk spesialisering.

Fordi de fleste allerede fulgte et førskoletilbud i barnehagene eller på skolen, og slik ikke opplevde noen stor forskjell før og etter reformen, estimerer vi effekten på den lille gruppen som ikke hadde noe førskoletilbud før 1997. Selv om mange av disse barna kom fra familier med lav inntekt og lav utdanning, finner vi at tidligere skolestart påvirket deres utvikling i svært liten grad. Vi gjør en rekke tester, og finner at resultatene våre er robuste. På bakgrunn av andre funn i litteraturen om tidlig læring er en mulig årsak til de manglende positive effektene av tidligere skolestart at programmet var lite intensivt og strukturert.

1 Introduction

Recent research suggests that universally available child care of high quality can have a beneficial impact on children’s development, also in the long run.¹ Such early childhood interventions are found to have particularly high returns for children from disadvantaged families.² At the same time, children from disadvantaged families are underrepresented in existing universally available programs. Such sorting into the existing programs coupled with particularly large estimated benefits among disadvantaged children, suggests a potentially strong social gradient in expanding or mandating early childhood interventions (Barnett and Belfield, 2006). Indeed, in an effort to counter differences at school entry depending on social background, many countries are currently moving towards subsidized kindergarten or child care available for the general population.³

Policies and proposals promoting universal interventions in early child care pose a challenge to the existing literature, which has reserved most of its attention for programs targeted at disadvantaged children. While a handful of recent studies consider large-scale early childhood interventions (Baker, Gruber, and Milligan, 2008; Havnes and Mogstad, 2011b), these usually exploit the introduction of programs or differences in local availability for exogenous variation. The estimates in these studies therefore derive largely from children in families that are early adopters of child care, revealing a strong preference for out-of-home care. Since both theory and evidence point towards important heterogeneity in the effects of early childhood interventions (see e.g. Blau and Currie, 2006; Cunha, Heckman, and Schennach, 2010; Havnes and Mogstad, 2010), it remains an open question how well the current evidence can inform about the impact of truly universal interventions.

In the current paper, we provide first evidence on the long-run effect on schooling of mandating kindergarten at age 5–6. Specifically, we first consider the impact on children’s school performance at the end of compulsory schooling at age 15–16. To uncover effects that may not be revealed in measures of school performance, we also consider the impact on high school dropout and on enrollment in the academic track in upper secondary school. Our identifying variation comes from a 1997-reform in Norway that lowered school starting-age from 7 to 6. The goal of the reform was to counter differences in learning out-

¹For recent reviews of this rapidly expanding literature, see Almond and Currie (2010); Ruhm and Waldfogel (2011), or Baker (2011).

²Havnes and Mogstad (2010) document large heterogeneity in the effects on adult outcomes from child care for 3–6 year old children in the late 1970s in Norway. Ludwig and Miller (2007) interpret the effects of the US Head Start as an upper bound because children are among the most disadvantaged. Further, effects found in the targeted Perry Preschool project (e.g. Karoly, Kilburn, and Cannon, 2005) are larger than what could plausibly be expected in the general population.

³For instance, the European Union Commission proclaims that early childhood education and care “is the essential foundation for successful lifelong learning, social integration, personal development and later employability” (European Union, 2011, p. 1). In their 2002 Barcelona Declaration, the European Union aims “to provide childcare by 2010 to at least 90 % of children between 3 years old and the mandatory school age and at least 33 % of children under 3 years of age” (European Union, 2002, p. 13).

comes between children from different socioeconomic backgrounds. While disadvantaged children were thought to benefit most from kindergarten programs, they were strongly underrepresented in the existing voluntary programs prior to the reform. The activities in the new program were aimed to prepare children for school by learning through play, similar to activities previously offered in voluntary kindergarten programs.

An advantage of our study is that access to kindergarten was not rationed prior to the reform. Before 1997, the majority of six year olds were enrolled in subsidized child care institutions, while the remaining children were mainly cared for by a parent (Norwegian Ministry of Child and Family Affairs, 1996). This first implies that parental care is the dominant counterfactual mode of care, as opposed to the alternative informal non-parental care. In comparison, it is often difficult to disentangle whether previous estimates of the effect of kindergarten and child care programs mostly reflect shifts from parental care or shifts from informal care.⁴ Second, since kindergarten is not rationed prior to mandating, the estimated effect derives from the particular group of children that do not voluntarily enroll. This implies that there is little risk of confounding estimates of mandating kindergarten from effects of admitting children that are merely rationed. We are to our knowledge the first to study the effects of kindergarten enrollment on this particular group. Our paper therefore also speaks to the broader debate on state interventions in private decision-making.

Our baseline empirical strategy is based on a difference-in-differences (DD) approach. Ideally, we want to compare outcomes before and after the implementation of the mandatory kindergarten reform, of children who enroll in voluntary kindergarten at age six (i.e. the control group) and children who do not enroll in voluntary kindergarten at age six (i.e. the treatment group). After the reform, this is unobservable, however, since all children are enrolled at age six. Instead, we use enrollment in kindergarten at age five to determine treatment. This should be a good proxy since most children who are enrolled in kindergarten at age five are also enrolled at age six. The validity of our DD strategy hinges on the assumption that the trend in school performance among children who do not voluntarily enroll in kindergarten would have been the same as for children who voluntarily do enroll in kindergarten, in the absence of mandating. The richness of our registry data allows us to condition on a large set of observable characteristics, and to challenge the plausibility of our identification strategy in a number of ways. In order to focus on the particular issue of mandating and rationing, we base our analysis on children from non-immigrant families, which constitute about 96% of the population. Our paper does not, therefore, speak to the debate on early interventions to provide language training among non-native speakers.

⁴For instance, Baker, Gruber, and Milligan (2008) interpret their estimates to reflect about one third shifts from parental care to formal care and two thirds shifts from informal care to formal care. In contrast, Havnes and Mogstad (2011b) interpret their estimates as reflecting 96 % shifts from informal care.

Results suggest that extending the reach of kindergarten in the general population by making it compulsory, does little to counter differences in learning outcomes between children from different socioeconomic backgrounds when access to kindergarten is substantial. In our baseline estimation, the estimated effect on the child's grade is negative but below 2 % of a standard deviation. Meanwhile, we find a modest increase in high school dropout rates, and no impact on academic tracking in upper secondary school.

Of course, mean impact estimations always risk masking potentially substantial but counteracting effects in different parts of the population, or over the distribution of the outcome. To address this, we first estimate effects in a large number of subsamples, reflecting the child's gender, birth order and number of siblings; as well as parental education, income, welfare dependence, and age at first birth. While sometimes imprecise, estimates across subsamples are generally small, confirming our main findings. Second, we estimate the impact of the reform on different segments of the grading distribution, again finding no evidence of important heterogeneity.

To improve our confidence in the estimates, and to explore alternative channels by which an effect may work, we challenge our empirical approach in different ways. First, we confront the key identifying assumption of a common trend between the treatment and comparison group underlying all DD estimation. Second, we consider whether there may be a separate effect of the reform on our comparison group that may attenuate effects on our treatment group. Third, we investigate whether there might be a delayed effect of the reform on later cohorts. Fourth, we consider some alternative and less aggregated school outcomes to understand whether there may be effects on some particular sets of skills that are washed out in our aggregated measure of school performance. Finally, we investigate whether the reform may have had an effect on the labor supply of mothers, estimating effects on both the extensive and the intensive margin.

Three alternative explanations for the weak effect of the reform stand out. On the one hand, the results may suggest that parents sort relatively efficiently into the existing kindergarten programs, so that children that are not in such programs in fact may opt out partly because they will benefit little. Second, the results may suggest that the children affected by the program are too old to benefit, and that universal programs must start at earlier ages. In both cases, our results would suggest that to universalize kindergarten at age 5–6 is a misguided strategy for improving outcomes of disadvantaged children. However, the weak effect could also imply that it was the particular program that failed to generate benefits. This could suggest that the focus on learning through play, emphasized in the implemented program, is not successful in generating learning gains in this group of children.

The paper proceeds as follows. We first discuss relevant literature in Section 2. We then proceed to discuss our analysis of the 1997-reform in Section 3, starting with the institutional background before turning to the details of our empirical approach. Section

4 describes our data and gives descriptive statistics, before Section 5 presents our main results. Section 6 presents a battery of specification checks and investigates potential mechanisms, and Section 7 concludes.

2 Early childhood interventions and child outcomes

A large and rapidly increasing literature on early childhood intervention suggests that investments in early childhood have high returns, particularly for disadvantaged children (Almond and Currie, 2010).⁵ However, most studies have focused on targeted interventions, offered to children from particularly disadvantaged families.⁶ As noted by Baker (2011), the evidence base for universally available large-scale programs is still small. There are several reasons why effects from programs targeted at disadvantaged children could differ importantly from more universal programs. First, the effect of such programs are related to the alternative mode of care had the programs not been in place. Since disadvantaged children would be expected to have poorer alternatives, they likely have more to gain from interventions (Knudsen, Heckman, Cameron, and Shonkoff, 2006). Second, the targeted interventions that have been studied are often quite intensive, including home visits, nutritional advice and several years of daily activities. In comparison, a program serving a large part of the population will necessarily have to provide a less intensive intervention. This might produce effects from large-scale and universal programs that differ distinctly from the effects of intensive small-scale interventions.⁷

Findings from the existing studies of universally available large-scale programs are mixed. Evidence from a large expansion of universal child care in Canada points towards a negative impact on child outcomes in the short run: Baker, Gruber, and Milligan (2008) report negative mean impacts on behavior, while Lefebvre and Merrigan (2008) find negative effects for cognitive outcomes. Gupta and Simonsen (2010) exploit municipality differences in guaranteed access to child care centers. They find that compared to parental

⁵Studies in neuroscience and development psychology indicate that learning is easier in early childhood than later in life (Shonkoff, Phillips, and Council, 2000). In the economics literature, Becker (1964) points out that the returns to investments in early childhood are likely to be relatively high, simply because of the long time to reap the rewards, while Carneiro and Heckman (2003) argue that investments in human capital have dynamic complementarities, implying that learning begets learning.

⁶The Perry Preschool and Abecedarian programs are examples of targeted randomized programs (see Barnett (1995) and Karoly, Kilburn, and Cannon (2005) for surveys of the literature.), while the US Head Start program provides an example of a targeted non-randomized program (see e.g. Currie (2001) or McKey, Condelli, Ganson, Barrett, McConkey, and Plantz (1985) for a review of the findings).

⁷See Baker (2011) for a thorough discussion. Our paper also relates to the literature on early enrollment into formal schooling (see e.g. Leuven, Lindahl, Oosterbeek, and Webbink (2010) or Black, Devereux, and Salvanes (2011) for an overview). An important issue in this literature has been to resolve the collinearity of age at test and age at school start. This is not an issue in our case, since age at test is both common across treatment groups and unaffected by the reform. However, the literature on child care and early childhood interventions may in general be said to face a similar collinearity between age at program start and years of enrollment. As in the rest of the literature, we estimate the combined effect of an additional year in kindergarten and lower age of entry.

home care, being enrolled in a child care center at age three did not improve non-cognitive child outcomes at age seven. However, longer hours in non-parental care lead to poorer child outcomes.

In contrast, Fitzpatrick (2008) finds modest increases in test scores (12 percent of a standard deviation) for disadvantaged children in less densely populated areas following a large expansion in Universal Pre-K in Georgia. In the same vein, Havnes and Mogstad (2010) find strong, positive effects for a number of long-run adult outcomes when exploring an increase in the availability of child care for three to six year olds in Norway in the 1970s. The effects are particularly strong for children from disadvantaged families. Similarly, comparing siblings in Uruguay following a rapid expansion in the supply of kindergarten places, Berlinski, Galiani, and Manacorda (2008) find an increase in years of education and in the likelihood of being in school, with the strongest effects for children of low-educated mothers. Meanwhile, Cascio (2009) finds a decrease in subsequent high school drop out rates and in the likelihood of being institutionalized for white children, but no such effect for blacks,⁸ when exploiting the variation in kindergarten subsidies across different states in the US from the 1960s.

As emphasized by Blau and Currie (2006), the expected effect of a child care intervention depends crucially on the alternative mode of care, usually parental care in the home (parental care) or informal sources of non-parental care (informal care).⁹ A shift into formal care from informal care, often of low quality, may be expected to yield benefits for children (see e.g. Havnes and Mogstad, 2011b). Meanwhile, the alternative shift from parental care may potentially be less beneficial.¹⁰

Unfortunately, existing evidence is often unable to disentangle which shifts are most relevant for the interventions. This is one potential explanation for the seemingly contradicting effects found in parts of the literature. Baker, Gruber, and Milligan (2008), for instance, note that about a third of the children enrolling in child care after the expansion were shifted from informal sources of care. The effect they estimate should be a mix, therefore, two thirds of the effect of moving children from parental care to formal care, and one third the effect of moving children from informal care to formal care. In comparison, the estimates in Havnes and Mogstad (2010; 2011b) can be interpreted as being driven almost solely by shifts from informal to formal out-of-home care. It should therefore not necessarily come as a surprise that the effects in the latter studies paint a brighter picture of how child care affects children's outcomes than do those in Baker,

⁸Cascio (2009) argues that this is likely due to the fact that the new program crowded out investments in federally funded early education among the poorest five year olds.

⁹Informal care refers to care by an unlicensed, usually untrained, caretaker. Informal care can be both paid and unpaid, and usually take place in the child's home or in the home of the caretaker. Examples are nannies, neighbors, relatives, or unlicensed child-minders.

¹⁰Gupta and Simonsen (2010) can distinguish between the shift from parental care to formal care and from parental care to informal care. They report no effect on child outcomes of shifts from home care to formal care, but a negative effect of shifts from home care to informal care for disadvantaged boys.

Gruber, and Milligan (2008).

As emphasized by Baker (2011) we need more evidence on the impacts of programs that serve large parts of the population. Our paper adds to this literature in two distinct ways. First, from extensive studies conducted prior to the reform, we can be confident that we estimate the effect mostly of shifts from parental care. While some studies have been able to plausibly isolate effects from shifting children from informal care (e.g. Havnes and Mogstad, 2011b), we are not aware of studies that isolate the shift from parental care to formal care. Second, since kindergarten is not rationed prior to mandating, the estimated effect should derive from the particular group of children that do not voluntarily enroll. We are to our knowledge the first to study the effects of kindergarten on this particular group.

3 Analyzing the 1997 compulsory schooling law

3.1 Background

Until 1997, Norwegian children started school in August the year they turned seven. This was late compared to children in most western countries.¹¹ At the same time, slots in child care institutions were widely available following a child care reform in 1975. In 1996, 89 % of non-immigrant families enrolled their six year olds in a kindergarten program.¹² However, from the mid-1980s, there was widespread worry that children entered school on different footings, depending on their socioeconomic background.

Figure 1 shows the strong social gradient in school performance and kindergarten enrollment. In Panel (a), we draw the average grade of students at exams administered at the end of compulsory school, in the deciles of family income at age five. The figure shows a strong positive relationship between the two: On average, children in the lowest decile, with family income of about USD 16,000, receive a grade of less than 3.5, while children in the upper decile, with family income of about USD 170,000, receive a grade of almost 4.5.¹³ This difference in exam performance is equivalent to a difference of just under one standard deviation (cf. Table 2 below). At the same time, children in lower deciles have a much lower probability of being enrolled in kindergarten. Panel (b) of Figure 1 shows that enrollment in kindergarten at age five among children in the lowest decile of family income is just over 50 %, compared to over 90 % for deciles 6–10.¹⁴

While children enrolled in formal child care were offered school preparation in kindergarten groups within their child care center, this was not available for children not enrolled

¹¹For instance, school starting age in Germany, France and the US was six, while England had a starting age of five.

¹²For simplicity, we use age a to refer to the year the child turns a years old in the below.

¹³Throughout, we refer to 2011-USD adjusted using the consumer price index.

¹⁴For details, see table A1 in Appendix A.

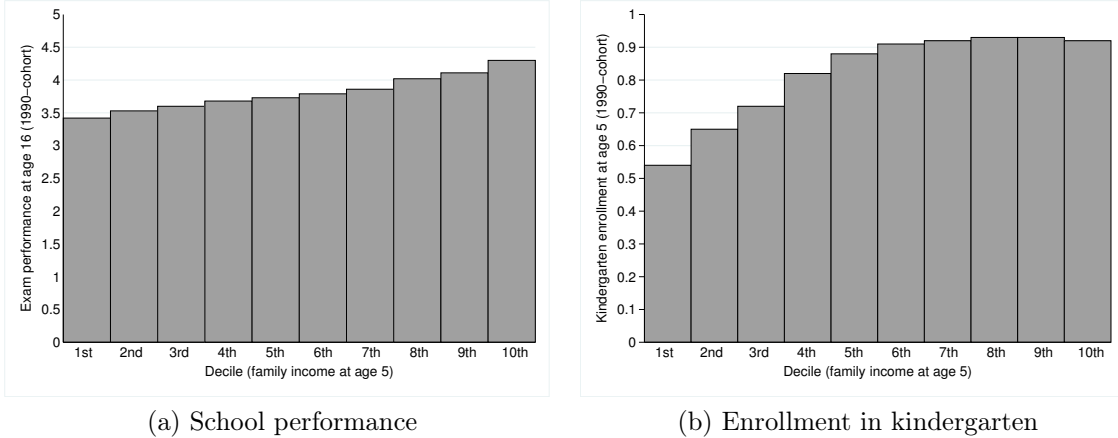


Figure 1: Social gradient in school performance and enrollment in kindergarten among children born in 1990.

Notes: Family income is measured in 1996, when the child is five years old, adjusted for CPI-growth, and converted to *USD* using $USD/NOK = 6$. Average exam grade and enrollment in kindergarten refers to the mean among children from families with income in each decile of the distribution of family income. Data and variable definitions are in Section 4.

in formal child care.¹⁵ On this background, a proposal to lower the mandatory school starting age from seven to six was widely discussed. Compulsory programs for six year olds, as opposed to voluntary kindergarten programs, would expose all children to the same educational program, and was argued to counter differences in learning outcomes between children from different socioeconomic backgrounds. A reform was finally proposed in a government White Paper published in the spring of 1993 (Norwegian Ministry of Education, 1992-93), and passed the Norwegian Parliament in May 1994 (Norwegian Ministry of Education, 1993-94). The reform was implemented in August 1997, at the start of the 1997–1998 school year. The first children affected were those born in 1991, who started school in August 1997, the year they turned six years old. The document refers to the fact that 80 % of children were enrolled in a kindergarten program on a voluntary basis (in 1993), either in school or integrated into a child care center.

Note that the cutoff for school starting age in Norway is January 1st. In Norway, as in most of Europe, schools employ strict enrollment rules, and nearly all children start school the year they turn the school starting age. Any exemption from this rule requires a formal application from the parents which then has to be approved by specialists and decided upon by the local government.

Educational content. To facilitate the comparison of the kindergarten programs before and after the 1997-reform, Table 1 summarizes some important characteristics of the

¹⁵Voluntary programs for six year olds were allowed on school grounds from 1991, managed by kindergarten teachers (Norwegian Ministry of Education, 1990-91, Ot.prp. nr. 57). Government support was the same as for six year olds enrolled in kindergarten, and the parental co-payment and educational content of the program was essentially the same as that offered in regular child care institutions. In the remainder, we do not distinguish between the two, as we cannot identify in which program a particular child was enrolled.

Table 1: Characteristics of kindergarten programs before and after the 1997-reform.

	Pre-reform	Post-reform
Adults in a group with 20 children	Two kindergarten teachers	Two kindergarten teachers
Structured learning activities	Learning through play integrated into primary schools or child care centers	Learning through play four hours/day
Other activities	Free play (supervised) in the child care center or the after-school program	Free play (supervised) in the after-school program
Peers	Three to six years old	Six years old in kindergarten, six to nine years old in the after-school program
Payment	290–630 USD/month depending on income	170 USD/month on average (given enrollment in an after-school program)
State subsidies	4,600 USD/year	4,100-4,700 USD/year

Source: Norwegian Ministry of Education (1992-93) and Norwegian Ministry of Child and Family Affairs (1995). Costs are measured in 2011-USD.

programs, discussed further below.

The educational content of the mandatory kindergarten program was aimed at combining the best of school and kindergarten traditions. These were grounded in the social pedagogy tradition which had also dominated child care practices in subsidized child care institutions.¹⁶ Learning through play was stated as essential, and the White Paper preceding the reform referred to research that gave little credit to formal learning for the particular group of six year olds (Norwegian Ministry of Education, 1992-93). Kindergarten teachers were for the first time allowed to teach in school, and a new national curriculum was introduced focusing on providing care and an educational environment adapted to age and level of maturity. The curriculum for the new first graders ensured consistent teaching goals for all six year olds across the country.¹⁷

¹⁶The social pedagogy tradition for early education has been especially influential in the Nordic countries and Central-Europe. In contrast, a so-called pre-primary pedagogic approach to early education has dominated many English and French-speaking countries, favoring formal learning processes to meet explicit standards for what children should know and be able to do before they start school.

¹⁷The curriculum for the remaining nine years in compulsory schooling was revised, and implemented for grades 1, 2, 5, and 8 in 1997, grades 3, 6 and 9 in 1998 and the remaining grades in 1999 (Norwegian Ministry of Education, 1996b). Children in our main sample, born 1990–1991, therefore were subject to the new curriculum through primary school. The implementation was such, however, that the cohorts in our extended sample, were introduced to the new curriculum at somewhat different times. Specifically, children born in 1987, 1990 and 1991 were placed on the new curriculum track at the outset of the reform in 1997. Children born in 1988 were placed on the new curriculum track in 1998, while children born in

The new curriculum strengthened the focus on developing social, language and physical skills through free play and “learning-by-playing”. Play was considered to be a necessity for young children, contributing to learning and development, and the time devoted to play was gradually phased out as the child grew older. The new curriculum specifically stated that “*The first year [of primary school] is to have a distinct kindergarten character, and one has to emphasize learning through play and age-mixed activities throughout lower secondary school*” (Norwegian Ministry of Education, 1996a).¹⁸ In an international context, the program thus bears strong resemblance to a kindergarten program rather than to a formal school program. This is important to keep in mind when interpreting the results.

In the new mandatory kindergarten program, the minimum requirement was one teacher or kindergarten teacher for every 18 children. By comparison, the minimum requirement for pedagogical staff in child care centers for six year olds was one per 14-18 children. Beyond this, the municipalities should themselves judge the need for further staff, but they had to secure sufficient care for the children (Norwegian Ministry of Child and Family Affairs, 1995). In addition to the kindergarten teacher, assistants were hired depending on the size of the group and the municipal regulations.¹⁹

The teaching requirement for the new first graders was set to 20 hours per week (Norwegian Ministry of Education, 1992-93). Beyond this the new first graders could enroll in an after-school program if their parents needed further care for their child. The after-school programs were in most cases situated on school premises. The programs offered free play under the supervision of non-qualified adults, with no educational content.

Reform costs. The costs of the reform were largely due to the added investments in school buildings in order to house the new first graders, estimated to 891 million USD. This amounts to about 82 million USD per year. In addition, there were costs associated with developing and purchasing books for the new curriculum, additional teaching resources, school transportation and after-school programs each year (Norwegian Ministry of Regional Affairs, 1995-96).

The counterfactual mode of care. Parental care seems to be the alternative for the vast majority of six year-old children not enrolled in a kindergarten program. Families who wanted to enroll their six year-old in kindergarten faced little rationing in the years just

1989 were placed on the new track only in 1999, when they were in the fourth grade.

¹⁸To ensure the care of six year olds during normal work hours but outside school hours, the government also expanded the access to after-school programs. These programs required co-payment, and prioritized younger children. After-school programs were subject to similar requirements as regular child care providers.

¹⁹The preschool teacher education is a college degree, and the head teacher was responsible for planning, observation, collaboration and evaluation of the work, under the requirements specified in the strict regulations for subsidized child care. Teachers typically worked closely with one or two assistants, and were responsible for the educational programs in separate groups of 6–18 children and for day-to-day interaction with parents. There were no educational requirements for assistants.

prior to the reform. Of course, not all families wanted to enroll their child in kindergarten. In a survey from 1992, about 70 % of parents said that they had enrolled their six year old in a kindergarten program. 20 % reported that the child was cared for by a parent, and 3 % reported close relatives as the main care taker during the day. 5 % were cared for by an unlicensed care taker or by a nanny (Norwegian Ministry of Child and Family Affairs, 1996). As also suggested in Panel (a) of Figure 2 below, rationing of kindergarten does not seem to have been a factor at the time the reform was implemented.

Other reforms. We may worry that there were other reforms that could also have affected our cohorts differently. However, the closest reform in primary education prior to the 1997-reform was implemented in 1986, while there was no additional reforms until the start of the school year 2007–2008. This ensures that the 1990 and 1991 cohorts completed their entire compulsory schooling with the national curriculum introduced in 1997. Also, a nationwide cash-for-care reform was implemented in 1998, and expanded in 1999, paying families with children below two years old (from 1998) and three years old (from 1999) that did not utilize subsidized child care a substantial monthly cash allowance.²⁰ While this reform did not affect the children in our sample directly, it could have had an effect on younger siblings and therefore an indirect effect on the children in our sample.²¹ However, it seems unlikely that the impact of the reform differ much between children born in 1990 and 1991, which constitute our baseline estimation sample.²²

3.2 Empirical strategy

To estimate the relationship between kindergarten and children’s long-term outcomes, we exploit the temporal and spatial variation in pre-reform kindergarten enrollment in a difference-in-differences setup. Ideally, we want to compare the child outcomes before and after the implementation of the mandatory kindergarten reform of children who would enroll in voluntary kindergarten at age six (i.e. the control group) and children who would not enroll in voluntary kindergarten at age six (i.e. the treatment group). Our basic difference-in-differences (DD) model estimated by OLS, can then be expressed as

$$Y_{it} = \alpha_t + \gamma_1 Treated_i + \lambda Treated_i \times Post_t + X'_{it}\beta + \epsilon_{it} \quad (1)$$

²⁰See Schøne (2004) or Drange and Rege (2012) for a detailed description of the cash-for-care reform.

²¹Bettinger, Rege, and Haegeland (2011) find that school performance improves among children with siblings eligible for the Cash-for-Care subsidy.

²²To investigate this directly, we have estimated a baseline DD model in equation (1) using as dependent variable a dummy equal to one if the child has a younger sibling born 1996 or later (i.e. partly or fully eligible for the subsidy) and zero otherwise. The estimate is almost exactly zero (0.004) and not statistically significant at conventional levels. This suggests that the cash-for-care reform does not pose a threat to our empirical strategy.

where i indexes child, t indexes cohort, $Post_t$ is a dummy equal to one if the child is affected by the reform (i.e. $t \geq 1991$) and zero otherwise, and $Treated_i$ is a dummy equal to one if the child is in the treatment group. Note that the cohort-specific constant term consumes the separate effect of the $Post$ -dummy. We estimate the model with and without a large set of control variables for child and parental characteristics X_{it} , including the child's sex, the mother's and her spouse's age, years of education, and family size (see also Section 4). We also include municipality fixed-effects to capture potentially differing labor market environments. All control variables are measured prior to the impact of the reform and standard errors are robust to heteroskedasticity.

In practice, whether a child would enroll in voluntary kindergarten cannot be observed for post-reform cohorts, since all children are enrolled at age six. To estimate equation (1), we therefore use enrollment in kindergarten at age five to determine treatment. This should be a good proxy since most children who are enrolled in kindergarten at age five are also enrolled at age six. That is, children who are enrolled in child care the year they turn five are placed in the control group, while children who are not enrolled in child care at age five are placed in the treatment group.

Panel (a) of Figure 2 displays the trend in kindergarten enrollment at age five and six in our estimation sample. We note the close relationship between the two series over time in the pre-reform period, where the two lines are virtually parallel. This suggests that enrollment at age five captures the counterfactual evolution of enrollment at age six well. Furthermore, we note that there is no spike in the enrollment of five year olds following the reform, when children age six are no longer taking up places in child care centers. This suggests that there was no discernible rationing of kindergarten for these age groups in our period of study. Finally, we note that kindergarten enrollment in pre-reform years is around 85 %, giving a treatment group of about 15 % of the total sample.

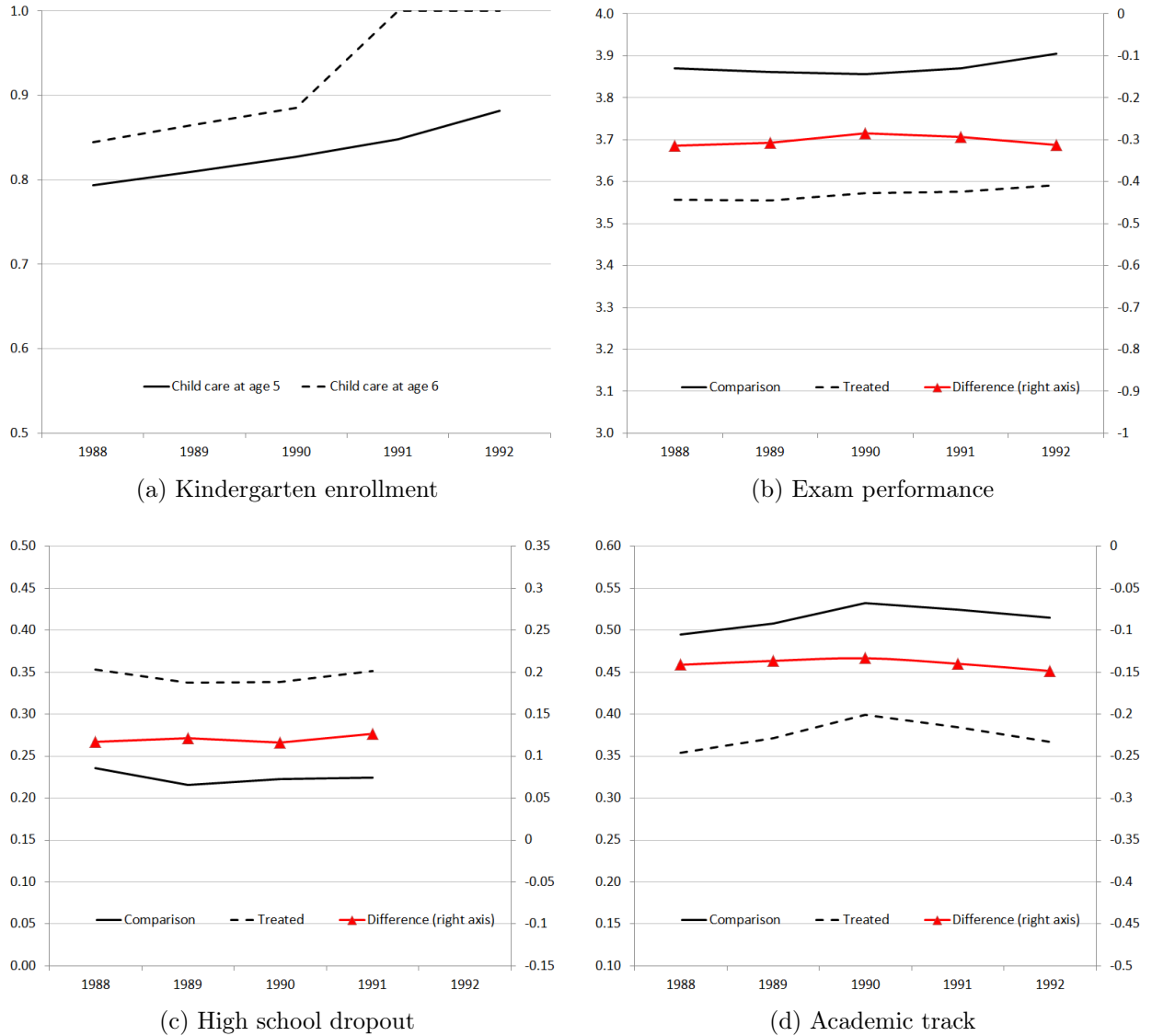


Figure 2: Kindergarten enrollment and children’s schooling outcomes by treatment for cohorts born 1988–1992.

Notes: Vertical axes are scaled to approximately one standard deviation. High school dropout is not yet available for the 1992-cohort. Variables are defined in Section 4

The validity of our DD strategy hinges on the assumption that the trend in school performance among children in the treatment group would have been the same as for children in the control group, in the absence of the reform. As emphasized by Besley and Case (2000), this essentially assumes common time effects and no compositional changes between the treatment and control group. The richness of our registry data allows us to condition on a large set of observable characteristics, to investigate changes in the composition of the groups.

To investigate the time effects, Panels (b)–(d) of Figure 2 display mean outcomes of cohorts born 1988–1992 separate for the treatment and control group, and the difference between the two groups over time (on the right axis). The vertical axes are scaled to

about one standard deviation in all the figures. The trends are quite flat and strikingly similar across the treatment and control group throughout the period. The similarity of the trend in the pre-reform period, supports the assumption of common time effects. That there is no jump in the treatment group from the 1991-cohort onwards, nor a divergence in the trends in the post-reform period, is first evidence that the reform had little impact on children’s school performance.

Though the documentation suggests little change in the contents, we could also worry that the new kindergarten program integrated in schools in fact was different from the former program, and thus could have had an effect also on children in the comparison group. We pay close attention to this in our robustness analysis provided in Section 6, finding no support for an effect on the comparison group. To further challenge the validity of our empirical specification, Section 6 also reports results from a series of specification checks, including a placebo reform and treatment-specific trends.

The difference in enrollment between five and six year olds should not be a threat to the internal validity of our estimates. Higher enrollment at age six than at age five may, however, dilute the estimated treatment effect by misplacing some children in the treatment group who enroll in kindergarten only at age six. Our estimates may therefore be interpreted similar to intention-to-treat estimates, and should be scaled in order to arrive at the average treatment effect on the treated. In 1990, 48 % of children who are not enrolled at age five are enrolled at age six, suggesting that only 52 % of the treatment group are in fact affected by the reform. In interpreting our results, we should bear this in mind, scaling the estimated effects by a factor of $1/.52 = 1.9$ to arrive at the average effect on the treated.²³

4 Data

Dataset and variables. Our data are based on administrative registers from Statistics Norway. Specifically, we use a rich longitudinal database which covers every resident from 1992 to 2007. It contains individual demographic information (e.g. sex, age, immigrant status, marital status, number of children), socioeconomic data (e.g. years of education, income, employment status), and geographic identifiers for municipality of residence. Information on school performance, educational attainment and school enrollment for every individual is based on annual reports from Norwegian educational establishments. Income and employment data are collected from tax records and other administrative registers. The household information is from the Central Population Register, which is updated annually by the local population registries and verified by the Norwegian Tax Authority. We also have access to registry data on municipal child care coverage reported by the

²³The opposite misclassification is almost completely absent: More than 97 % of children who are enrolled in kindergarten at age 5 are also enrolled at age 6.

child care institutions themselves. The reliability of Norwegian register data is considered to be very good, as is documented by the fact that they received the highest rating in a data quality assessment prepared for the OECD by Atkinson, Rainwater, and Smeeding (1995).

Measuring kindergarten enrollment. There is, unfortunately, no register of individual kindergarten or child care enrollment. However, parents may claim the cost of child care as a deduction on their earned income. To identify whether a child is enrolled in kindergarten, we therefore use a binary variable equal to one if the child's parents claimed a tax deduction for child care at child age five. Of course, if a child has siblings, we cannot verify which of the children the deduction is claimed for (if not all). To get around this issue, we assume that child care enrollment is monotonous in age, such that older children are in child care whenever younger children are in child care. This ensures that at least the older child in child care age is enrolled whenever the parents claim the deduction. Finally, because we want to identify enrollment at age five, we exclude from our sample children that have a sibling born one year previous. To verify that our measure of kindergarten enrollment is sound, we have calculated the municipal enrollment rates implied by this measure.²⁴ We then compare these rates to the actual enrollment rates from administrative registers, reported by the child care institutions themselves. The correlation between the two is near perfect at about 0.94.

Estimation sample. We start with the universe of children born 1990–1991, who reside in Norway the year they turn five years old and who graduate from lower secondary school in 2005–2006. We then restrict our sample to children born to native-born parents, constituting about 96% of the population, in order both to focus our study on the effect of mandating, and to sidestep problems of comparability between native and immigrant children. We also exclude children with missing values on our dependent variable. Rather than exclude children with missing values on control variables, we construct dummy variables for missing and include these in our regressions. As mentioned, we also exclude children with a one year older sibling in order to more cleanly identify kindergarten enrollment from tax records.²⁵ Our main sample then consists of 111,397 individuals, of which just over 16 % are in the treatment group. In our extended sample, we consider the analogous population of children born 1988–1992.

Measuring school performance. Our main outcome is an average of grades on nationally

²⁴While the vast majority of deductions claimed are for child care costs, some other costs may also be claimed under the same statute, e.g. outlays for support for disabled children or registered nannies (“dagmamma”). In 1992, 5 percent of parents reported in a survey to have their six year-old in informal care with a nanny or child minder (Norwegian Ministry of Child and Family Affairs, 1996). About 1/5 of these reported to have a registered nanny (Ellingsæter and Gulbrandsen, 2003), and only these could deduct expenses from their taxes. This implies that about 1 % of the sample may be wrongfully registered as being in child care when they were in fact cared for by a nanny.

²⁵Results are unchanged if we include these children.

administered end-of-school exams. At graduation from compulsory school, children are tested on two or three exams in randomly drawn theoretical subjects—one or two written exams and one oral exam. The written exam is uniform across the country and provided by the Central Education Authority, and is corrected by external evaluators. The oral exam is also evaluated by an external examiner, and takes place at the school at which the child is enrolled, usually with a local teacher present. Grades are awarded on a scale from one to six, where six indicates excellence and one indicates very little competence. Grade retention is illegal, hence all children are allowed to graduate regardless of their grades. In addition, teachers assign each child grades in 12–13 subjects, based on performance throughout the year. There are nine theoretical subjects and four practical subjects.²⁶

Measuring high school drop out. In our data we can observe whether an individual is enrolled in education and how many years they have successfully completed. We define high school drop out as either not being enrolled in education, or not being on year for age in graduating year. That is, we code high school dropout as not being registered in your 13th year of education in the fall of the year you turn 18.²⁷ Note that this definition is somewhat strict, since it requires that children are not delayed.

Measuring academic track: In Norway, children are first tracked when they start upper secondary school. There are two main tracks (which are divided into 13 more specialized sub-disciplines): The academic track which is required for entry into university and college studies, and the vocational track which qualifies for a practical occupation. To consider whether the reform had an impact on academic tracking, we use a dummy equal to one if the child started on the academic track in the year following graduation from compulsory schooling. Note that enrollment in upper secondary school is almost complete in these cohorts, with about 94 % of children enrolling in one of the two tracks. We have also estimated the effect on the decision to enroll, finding no impact of the reform. If a child does not enroll in upper secondary school the year following graduation, he or she is excluded from these estimations.

Measuring maternal labor supply: We include three measures of maternal labor supply in our analysis. First, we measure earnings in 2011-USD. Second we construct two dummies, one variable capturing whether the mother is employed at all, and one variable capturing if the mother work full time. In our dataset we can observe whether an individual works 4–19 hours, 20–29 hours or more than 30 hours per week. We define a mothers as being employed if she is registered in one of these three categories in the outcome year. Because

²⁶Theoretical subjects are written and oral Norwegian, written and oral English, mathematics, nature and science, social science, and religion. Practical subjects are home economics, physical education, music, and arts and crafts.

²⁷Final graduation from high school should occur the year they turn 19 in the academic track and the year they turn 20 in the vocational track. This information is not yet available in the data.

of lags in the submission of employee information by firms, some individuals are recorded as being in full-time employment despite the records also indicating very low or even zero earnings. We correct this by defining mothers as full time employed if they work more than 30 hours per week and earn more than two times the basic amount in the Norwegian pension system.²⁸

Covariates. To account for possible observable changes in composition between years, we include a number of child and parent characteristics in our analysis, measured when the child is five years old. Child characteristics include municipality of residence, gender, number of siblings, and finally a dummy measuring if the child lived in a densely populated area. Background characteristics include a dummy measuring if the mother/father worked full time, a dummy for whether the mother/father completed high school and a dummy indicating if the mother/father finished a college education. In addition, we include a dummy capturing missing observations on mothers/fathers education. Further, we include a dummy that captures whether the mother/father was younger than 22 when the child was born. We also include a dummy for having missing observations on either the mother or the father. If both parents are missing we exclude the observation. Finally, we include a dummy capturing if one or both parents received welfare benefits, a dummy measuring if the family was low income (defined as earnings below the 10th decile in the family income distribution in the cohort born in 1990), and a dummy capturing if the child lives with only one of its parents.

Descriptive statistics. Means of the outcome variables were presented in Figure 2. In table 2, we present characteristics for the entire sample in the first two columns, and differences between the two groups by year in the remaining columns. We see no evidence of changes over time for characteristics of children or their parents between the treated and the comparison group. As discussed above, it is clear, however, that the treated children to a greater extent come from families with younger and less educated parents, and are more likely to belong to a family on welfare and/or to a single parent family. They are also overrepresented in the low income family group. This suggests that the children in our treatment group have a more disadvantaged background, in line with both what was expected when the reform thought to promote social mobility was introduced (Norwegian Ministry of Education, 1992-93) as well as to what has been found in related early intervention literature (Barnett and Belfield, 2006).

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The basic amount of the Norwegian Social Insurance Scheme are used to define labor market status, and determine eligibility for unemployment benefits as well as disability and old age pension. In 2011, one basic amount was about USD 13,000.

Table 2: Summary statistics

	Mean (SD)		1988	1989	1990	1991	1992
	<i>Treat (T)</i>	<i>Comp. (C)</i>	<i>T - C</i>	<i>T - C</i>	<i>T - C</i>	<i>T - C</i>	<i>T - C</i>
A. Child and family characteristics							
Female	0.49 (0.50)	0.49 (0.50)	0.00	0.00	0.00	0.01	0.01
1 sibling	0.40 (0.49)	0.51 (0.50)	-0.11	-0.10	-0.11	-0.12	-0.12
2 siblings	0.28 (0.45)	0.24 (0.43)	0.04	0.04	0.04	0.05	0.02
3 siblings +	0.12 (0.32)	0.06 (0.23)	0.05	0.06	0.06	0.07	0.06
Big school	0.57 (0.49)	0.58 (0.49)	0.00	0.00	0.00	0.00	0.00
On welfare	0.19 (0.40)	0.08 (0.26)	0.13	0.13	0.12	0.14	0.16
Low income	0.08 (0.27)	0.00 (0.05)	0.08	0.08	0.07	0.08	0.09
Single parent	0.29 (0.46)	0.17 (0.38)	0.12	0.13	0.12	0.11	0.14
B. Mother characteristics							
Employed	0.18 (0.38)	0.70 (0.46)	-0.55	-0.54	-0.52	-0.53	-0.64
– full time	0.07 (0.26)	0.33 (0.47)	-0.26	-0.27	-0.26	-0.28	-0.32
High sch.	0.34 (0.47)	0.54 (0.50)	-0.21	-0.22	-0.20	-0.22	-0.26
College	0.13 (0.34)	0.28 (0.45)	-0.17	-0.16	-0.15	-0.16	-0.20
Teenage mother	0.21 (0.41)	0.13 (0.33)	0.10	0.10	0.08	0.07	0.11
C. Father characteristics							
Employed	0.63 (0.48)	0.73 (0.44)	-0.10	-0.10	-0.10	-0.10	-0.11
High sch.	0.49 (0.50)	0.60 (0.49)	-0.13	-0.12	-0.11	-0.10	-0.12
College	0.18 (0.39)	0.27 (0.44)	-0.10	-0.09	-0.09	-0.08	-0.10
Teenage father	0.07 (0.26)	0.05 (0.21)	0.04	0.04	0.03	0.03	0.05

Notes: The treatment group include the children whose parents do not report tax deduction for child care expenses the year the child turns five. Outcome and control variables are defined in Section 4. Standard deviations are in parentheses.

5 Empirical results

In this section, we first report estimated mean effects of mandating kindergarten on children’s long-term schooling. All specifications are estimated with municipality fixed effects to account for time-invariant differences between municipalities. To address concerns about compositional changes we have estimated the baseline model with and without the set of covariates capturing important child and parent characteristics. We then investigate potential heterogeneity in the effects, reporting first estimated effects on school performance across the grading distribution, and then estimated effects in subsamples defined from child and family characteristics from our baseline model including covariates.

Mean effect. Table 3 reports our difference-in-differences estimates based on equation (1) from the sample of children born 1990–1991. In Panel A, we report the estimated effects on exam performance at the end of compulsory school, with and without the set of covariates. The estimates indicate that the reform had little effect on children’s school performance, with a precisely estimated point estimate of about 1 % of a standard deviation. Excluding covariates in the second row of Panel A hardly moves the estimate. This indicates that

Table 3: Mean effects on school performance, high school dropout rates and academic track in upper secondary school.

	Cohort-difference		DD		Mean [SD]
	Coeff	SE	Coeff	SE	
A. School performance					
Baseline			-0.01	(0.014)	0 [1]
No covariates			-0.013	(0.016)	
B. High school dropout					
Baseline			0.013	(0.008)	0.33 [0.47]
No covariates			0.014	(0.008)	
C. Academic track					
Baseline			-0.008	(0.007)	0.40 [0.49]
No covariates			-0.009	(0.008)	

Notes: $N = 111,397$ ($N = 107,707$ for academic track). UPDATE??? Estimations are based on OLS on equation (1). The controls are listed in table 2 and the dependent variables are defined in section 4 and 5. In Panel A, coefficients are standardized to the standard deviation of the dependent variable. Scaled refers to the estimate scaled by the take-up, see Section 3. Mean refers to pre-reform mean in the treatment group. Standard errors (SE) are robust for heteroskedasticity and all models include municipality fixed effects.

there are no important compositional changes between the two cohorts, as expected from historical reports and descriptive statistics. Given the precision of the estimate and scaling for take-up, we can rule out effects above 3.3% and below -7.1 % of a standard deviation at a confidence level of 5 %.

While studies of how early interventions affect child cognitive outcomes often find positive effects in the short run, these effects are usually found to dissipate over time (see e.g. Knudsen, Heckman, Cameron, and Shonkoff (2006)). At the same time, persistent effects are often found on outcomes that may also reflect non-cognitive traits.²⁹

In Panels B and C, we consider effects on high school dropout rates and enrollment in the academic track in upper secondary school, where earlier studies have often found an improvement from early intervention programs. However, again we find little evidence of any substantial effect, whether or not we include covariates. Indeed, if anything, we find a small negative impact on children’s schooling of mandating kindergarten, with a slight rise in high school dropout rates of 1.3 percentage points (from a pre-reform mean of about 33 % in the treatment group).

Heterogeneous effects. Though we find little support for an effect of mandating kindergarten on mean school performance, high school drop out or choice of academic track, we have already emphasized the general expectation of heterogeneous effects of early childhood interventions. A worry may therefore be that estimated mean effects mask large, and potentially countervailing, effects among different groups of children. On the one

²⁹Knudsen, Heckman, Cameron, and Shonkoff (2006) report that the early randomized interventions from the US (Perry Preschool and the Abecedarian Project) lowered the likelihood of grade repetition and increased the likelihood of finishing high school for treated children.

Table 4: Distributional effects on school performance

	Coeff	SE	Perc. value
5th percentile	0.001	(0.005)	2.0
10th percentile	-0.002	(0.006)	2.5
25th percentile	-0.011	(0.006)	3.0
50th percentile	-0.002	(0.006)	4.0
75th percentile	-0.005	(0.005)	4.5
90th percentile	-0.005	(0.003)	5.0
95th percentile	-0.002	(0.002)	5.5

Notes: $N = 111,397$. Estimations are based on OLS on equation (1). The controls are listed in table 2 and the dependent variables are defined in section 4 and 5. Percentile values refer to pre-reform percentiles in the treatment group. Standard errors (SE) are robust for heteroskedasticity and all models include municipality fixed effects.

hand, we may believe that the reform would be particularly beneficial in the lower parts of the grading distribution. On the other hand, we may believe that the reform may have quite different effects depending on characteristics of the child, the family or the local school. For instance, the overview provided by Almond and Currie (2011) suggests that girls and children with low educated parents benefit more than other children.

To address the first concern, we have estimated the impact of the reform on school performance at every point in the grading distribution. Specifically, we estimate equation (1) over the sample of children born 1990–1991, where the outcome variable is a dummy equal to one if the child’s school performance at end of compulsory school is above the given percentile, and zero otherwise. Estimates should then be interpreted as the percentage point change following the reform in the probability of performing above a given percentile for a child in the treatment group compared to a child in the comparison group.³⁰

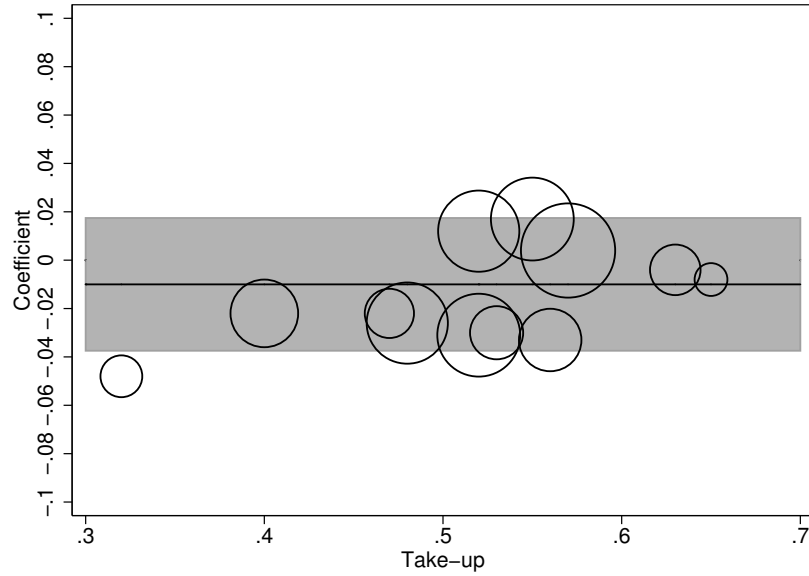
In Table 4, we report estimated effects at a set of percentiles of the grading distribution covering the bottom, the middle and the top of the distribution.³¹ Results show that there is essentially no heterogeneity across the grading distribution in how mandatory kindergarten affects children’s exam performance at end of lower secondary school. The estimates are almost uniformly negative, but small and precisely estimated. They are also not statistically significant at conventional levels, except for the small negative effect on the probability of performing above the 25th percentile.

To address the second concern, we have estimated separate reform effects for all outcomes in subsamples defined from a number of background characteristics. The estimates are reported in Table 5. To facilitate comparison of estimates across subsamples, we also report the mean outcome in the subsample among treated children from the pre-reform cohort. We also report the share of treated children and the take-up rate in the subsample

³⁰This procedure is essentially the first step in the RIF-procedure proposed by Firpo, Fortin, and Lemieux (2009) and applied to the DID-framework by Havnes and Mogstad (2010).

³¹We have estimated effects at all percentiles, which yields the same picture.

Figure 3: Estimated reform effect and take-up across subsamples



Note: The horizontal line and the shaded area correspond to the baseline estimate and its 95% confidence interval. Circles mark the estimates and take-up rates for subgroups reported in Table 5. The size of the circle indicates the size of the subgroup among treated children born in 1990. Take-up is defined as the probability that a child born in 1990 was not enrolled in child care at age five but was enrolled in kindergarten at age six, see Section 3.2. Coefficients and take-up rates for subgroups are reported in Table 5.

in the final two columns.³²

The results give little indication of important heterogeneity in the impact of mandatory kindergarten, which is estimated to be very small, and almost uniformly negative. However, lower sample size implies less precision, and some patterns in the point estimates may warrant comment. First, girls seem to benefit more than boys, in line with what is often found in the literature on cognitive impact of early childhood interventions (Anderson, 2008). Second, though very imprecise, we also note a pattern that children that initially perform well, as measured by mean exam grade pre-reform, may tend to receive the most harm from mandatory kindergarten. In particular, children of higher educated families on average experience a quite substantial negative effect of the reform. Though estimates are too imprecise to provide much confidence, this could be interpreted as an indication that parents with high levels of human capital provide a good alternative to preschool, in line with Cunha, Heckman, and Schennach (2010) and estimates in Havnes and Mogstad (2010).

³²As discussed in Section 3, take-up is defined as the probability that a child who does not enroll in child care at age five, and is therefore in our treatment group, would enroll in kindergarten at age six, and should therefore not be affected by the reform.

Table 5: Heterogeneous responses

	School performance			High school dropout			Academic track			Treated	Take-up
	Coeff	SE	Mean	Coeff	SE	Mean	Coeff	SE	Mean		
A. Child and family characteristics											
Girls	0.012	(0.190)	3.73	0.008	(0.009)	0.22	0.000	(0.011)	0.58	0.16	0.52
Boys	-0.031	(0.022)	3.44	0.016	(0.010)	0.26	-0.015	(0.011)	0.44	0.16	0.52
Single parent	-0.033	(0.025)	3.31	0.016	(0.015)	0.39	0.002	(0.015)	0.40	0.24	0.56
Low income	-0.008	(0.046)	3.30	-0.006	(0.022)	0.45	0.012	(0.022)	0.34	0.45	0.65
Young mother	-0.03	(0.032)	3.31	0.019	(0.018)	0.37	0.003	(0.018)	0.35	0.24	0.53
On welfare	-0.004	(0.042)	3.22	-0.001	(0.020)	0.43	0.015	(0.019)	0.34	0.35	0.63
B. Mother characteristics											
No high school	0.004	(0.016)	3.35	0.020	(0.010)	0.41	-0.006	(0.010)	0.330	0.22	0.57
High school	-0.022	(0.024)	4.00	-0.003	(0.010)	0.21	-0.012	(0.012)	0.59	0.11	0.40
College	-0.048	(0.042)	4.28	0.014	(0.014)	0.14	0.003	(0.018)	0.72	0.08	0.32
C. Father characteristics											
No high school	0.017	(0.023)	3.31	0.016	(0.011)	0.43	-0.002	(0.011)	0.29	0.20	0.55
High school	-0.026	(0.021)	3.84	0.005	(0.008)	0.25	-0.013	(0.010)	0.51	0.14	0.48
College	-0.022	(0.036)	4.18	0.007	(0.012)	0.17	-0.029	(0.016)	0.70	0.12	0.47

Notes: $N = 111,397$ ($N = 107,707$ for academic track). Estimations are based on OLS on equation (1). The subsamples are defined in section 4. The controls are listed in table 2 and the dependent variables are defined in section 4. Standard errors (SE) are robust for heteroskedasticity and all models include municipality fixed effects.

6 Specification checks

To improve our confidence in the estimates, and to explore alternative channels by which an effect may work, this section aims to challenge our empirical approach in different ways. First, we confront the key identifying assumption of our empirical strategy, namely the common trend assumption. Second, we consider whether there may be a separate effect of the reform on our comparison group that may attenuate effects on our treatment group. Third, we investigate whether there might be a delayed effect of the reform on later cohorts. Fourth, we consider some alternative and less aggregated school outcomes to understand whether there may be effects on some particular sets of skills that are washed out in our aggregated measure. Finally, we investigate whether the reform may have had an effect on the labor supply of mothers, estimating effects on both the extensive and the intensive margin. For brevity, we focus on school performance in reporting of the specification checks. Results are similar for high school dropout and enrollment in the academic track, for which results are reported in Tables A2 and A3 in the Appendix.

Common trend assumption. The primary threat in DD estimation is that the change in the observed outcome in the comparison group in the absence of the reform differs from the change in the potential outcome of the treatment group in the absence of the reform. To investigate this, we start by considering a placebo reform, pretending that the reform was implemented in the pre-reform period. The first row of Panel A in Table 6 reports the estimate from equation (1) estimated over the sample of children born 1989–1990, where $Post_t$ is redefined to be equal to one for children born in 1990 and zero otherwise. A significant estimate in this specification would put in doubt our identifying assumption. However, the estimate is almost precisely zero and nowhere near statistical significance.

Allowing treatment and comparison groups to follow separate trends is another way to challenge the common trend assumption. By extrapolating pre-reform trends into the post-reform period, we essentially restrict our estimates to reflect how outcomes deviate from the pre-reform trajectory. As emphasized by Besley and Case (2000), this is a simple yet potentially powerful test, which can often kill otherwise large and significant DD estimates.

To allow estimation of a trend, we extend the estimation sample to the start of our data series in 1988, and include the 1992-cohort, which is the last cohort that we can confidently use due to the cash-for-care reform in 1998. We then set $Post_t = 1$ for $t = 1991$ and $t = 1992$, and zero otherwise. For a correct comparison, results on this sample using our main regression equation (1) are reported in the first row of Panel B. Estimates conform to those in the baseline. In row 2 of panel B, we include a linear treatment-specific trend, while row 3 includes a second-order polynomial treatment-specific trend. Both specifications confirm the baseline estimates of essentially no effect of introducing mandatory kindergarten.

Table 6: Robustness – Exam grade

	Sample	Post	Coeff	SE	N
A. Key specification check					
Placebo	1989–1990	1990	0.005	(0.016)	110,171
B. Treatment-specific trends					
Extending pre-reform	1988–1992	1991–92	-0.004	(0.010)	267,745
Linear trend	1988–1992	1991–92	-0.016	(0.017)	267,745
Quadratic trend	1988–1992	1991–92	-0.013	(0.022)	267,745
C. Flexible trends					
Trend \times covar	1988–1992	1991–92	0.018	(0.009)	267,745
Year FE \times covar	1988–1992	1991–92	0.011	(0.009)	267,745
D. Other					
1st diff.: Treatment	1990–1991	1991	-0.007	(0.013)	18,108
1st diff.: Comparison	1990–1991	1991	0.004	(0.008)	93,288
Delayed effect	1988–1992	1991	-0.005	(0.011)	267,745
		1992	-0.004	(0.014)	

Notes: Column 2 gives the estimation sample. In all estimations, $Post_t = 1$ for t is given in Column 3. In Panel A estimation is based on OLS on equation (1). In Panel B, estimations are based on equation (1), including a linear (row 2) and a quadratic (row 3) treatment-specific trend. In Panel C, estimations are based on equation (1), including a linear trend (row 1) or cohort dummies (row 2) interacted with a set of baseline covariates (school size; mother’s and father’s education level; municipal income; urban area). In rows 1 and 2 of Panel D, estimations are based on equation (2), while row 3 is based on equation (3). The controls are listed in table 2 and the dependent variables are defined in section 4. Standard errors (SE) are robust for heteroskedasticity and all models include municipality fixed effects.

As an alternative, we can instead follow Duflo (2001) in allowing children to follow different trends depending on underlying characteristics. Specifically, we first estimate equation (1) including a linear trend interacted with baseline covariates.³³ We then relax the assumption of a linear trend, interacting instead the baseline covariates with the cohort fixed effects. Results are reported in Panel C, again confirming our baseline estimate of hardly any impact of the mandatory kindergarten reform on children’s school performance at end of compulsory schooling.

Effect on comparison group. We could also worry that the new kindergarten program integrated in schools in fact was different from the former program, and thus could have had an effect also on children in the comparison group. If the kindergarten program offered prior to the reform was of lower quality than the program offered after the reform, then the comparison group would experience a positive impact of the reform. In this case, the new mandatory kindergarten program may in fact have a positive effect for children

³³The baseline covariates are measured the year the child turns 5 years old, and include an overall measure of school size, the education level of the mother and father, the average income in the municipality of residence, and a dummy indicating whether the child lives in an urban area.

in the treatment group, that is simply netted out in our DD-setup against a positive effect in the treatment group (of similar size). Similarly, a negative impact on the comparison group could mask a negative impact in the comparison group.

To investigate this, we consider the two groups of children separately, to reveal whether there are in fact substantial changes in the grades of children around the implementation of the reform. Looking back at Panel (b) of Figure 2, we see no indication of such changes in neither the treatment nor the comparison group. More formally, and including covariates, we estimate first difference regressions separately for the two groups based on

$$Y_{it} = \alpha + \lambda Post_t + X'_{it}\beta + \epsilon_{it} \quad (2)$$

Results are reported in rows 1–2 of Panel D, and give no reason to believe that mandating kindergarten had much impact on the comparison group, nor the treatment group.³⁴

Delayed effect. A further worry may be that a positive effect of the mandatory kindergarten reform was offset, completely or in part, by adjustment problems in the year of implementation. Unfortunately, the cash-for-care reform implemented in late 1998 (see Schøne (2004) or Drange and Rege (2012)), creates problems for identifying effects on cohorts born 1993 and onwards. We can however plausibly estimate effects on children born in 1992. To ensure that the comparison group is not smaller than the treatment group, and to provide better identification of control variables, we also use the extended pre-reform period used above. We then extend the post-reform period to include in the estimation also children born in 1992. To allow for different treatment effects on children born in the two years, we expand on equation (1) to include a separate interaction term for the 1992-cohort. Specifically, we estimate the following regression using OLS on the sample of children born 1988–1992,

$$Y_{it} = \alpha_t + \gamma_1 Treated_i + \lambda_{91} Treated_i \times 1(t = 1991) + \lambda_{92} Treated_i \times 1(t = 1992) + X'_{it}\beta + \epsilon_{it} \quad (3)$$

where $1(t = s)$ is an indicator function equal to one if $t = s$ and zero otherwise. Results are reported in the final row of Panel D, again revealing no evidence that the reform had an important impact on children’s school performance at end of compulsory schooling.

Alternative outcomes. We may also worry that the dependent variable is not picking up the relevant margin of the effect. For instance, if kindergarten affects mostly oral skills or mostly skills that are relevant in one or a few particular subjects, then our estimate may be small simply because it is diluted by including subjects in our outcome that test skills that are not affected. To investigate this, we consider alternative outcomes that should

³⁴Note that grading is performed by external sensors who typically grade exams from several schools. Grading on a curve should therefore not be of much concern with these grades, at least in the short run.

reflect different sets of skills.

We start by separating the written and oral exams that make up our main dependent variable. Since there are usually two written for each oral exam, any effect on the oral exam may be diluted by a zero or counteracting effect on the written exam. In Panel A of Table 7, we report results from our baseline regression where the dependent variable is replaced by first the average of written exam grades and then by the grade on the oral exam. Estimated effects are virtually identical.

In addition to the externally graded written and oral exams used in our main analysis, we also have access to children’s teacher-assigned grades at end of compulsory school in all subjects (see Section 4). We can then run our main specification (1) on a number of measures based on the grades the children earn when they graduate. Estimates are reported in Panel B of Table 7.

We start by estimating the effect on the overall grade point average (GPA; the mean grade across all subjects), which is reported in the first row of Panel B. Not surprisingly, the estimated effect on the overall GPA is virtually identical to the effect on the average exam grade used in our main analysis. We then separate out the subjects that are tested on the written and oral exams used in our main analysis, and those that are not tested on these exams.³⁵ Estimates from our baseline specification using these as dependent variables, are virtually identical to the baseline. Finally, we group subjects according to the types of skills expected to determine the performance. Specifically, we group subjects into the following categories: “Sciences” (Mathematics, Natural science, and Social science), “Languages” (written and oral Norwegian, and written and oral English), and “Culture” (Religion, Music, Home Economics and Arts and crafts). Estimates are reported in rows 4–6 in Panel B of Table 7, and are again virtually identical. We conclude, therefore, that there is no evidence of substantial effects that were not picked up in our main analysis.

Maternal labor supply. An alternative effect of the mandatory kindergarten reform is an increase in maternal labor supply and family income. This is important in itself because any expansions in the tax base may offset the financial costs of subsidizing child care, particularly because the dominant pre-reform mode of care was parental care. At the same time, higher maternal labor supply would also affect family income, and could indirectly affect child performance (Dahl and Lochner, forthcoming; Løken, Mogstad, and Wiswall, 2010).³⁶

³⁵Subjects tested are written and oral Norwegian, written and oral English, mathematics, nature and science, social science and religion. Subjects not tested are home economics, physical education, music, and arts and crafts.

³⁶In a survey of the early literature, Blau and Currie (2006) report elasticities of maternal employment with respect to the price of child care ranging from 0 to -1. More recently, using more plausible identification, Baker, Gruber, and Milligan (2008) find a positive effect on maternal labor supply following the introduction of heavily subsidized universally available child care in Quebec. Meanwhile, Lundin, Mork, and Ockert (2008) find no such effect when studying a childcare reform which capped childcare prices in

Table 7: Alternative outcomes

	Coeff	SE	N	Dep mean
A. Separating written and oral exams				
Exam, written subjects	-0.011	(0.015)	108,473	3.21
Exam, oral subjects	-0.012	(0.015)	105,224	4.05
B. Teacher-assigned grades				
Grade point average	-0.013	(0.014)	111,185	3.79
Exam subjects	-0.013	(0.014)	111,021	3.64
Non-exam subjects	-0.007	(0.015)	111,038	4.12
Sciences	-0.011	(0.014)	111,225	3.57
Languages	-0.015	(0.015)	110,951	3.66
Culture	-0.008	(0.014)	111,157	4.11
C. Mothers labor supply, child age 7				
Earnings (2011-USD)	1,540	(914)	46,742	20,110
Employment	-0.002	(0.010)	46,742	0.23
Full time	-0.000	(0.008)	46,742	0.10

Notes: In Panel A and B estimations are based on OLS on equation (1) including covariates. The controls are listed in table 2 and the dependent variables are defined in section 4. “Sciences” includes Mathematics, Natural science, and Social science; “Languages” includes written and oral Norwegian, and written and oral English; “Culture” includes Religion, Music, Home Economics and Arts and crafts. In Panel C estimations are based on OLS on equation (1), and the sample is restricted to mothers with youngest child of relevant age. Covariates included are listed in table 2, excluding measures of mothers employment and including municipality-specific unemployment rates. The dependent variables are defined in section 4. Standard errors (SE) are robust for heteroskedasticity and all models include municipality fixed effects.

In line with related literature (Gelbach, 2002; Havnes and Mogstad, 2011a), we restrict our analysis to mothers with their youngest child born in 1990 or 1991. This is where we would expect the strongest labor supply responses. As our outcome of interest, we consider labor supply of mothers when the child turns seven years old. That is, ideally we consider the labor supply of mothers whose youngest child was enrolled or was not enrolled in kindergarten in the months January through late August.³⁷

To estimate the effects on maternal labor supply, we apply an analogous empirical strategy to the one in our main analysis. Specifically, we estimate the DD-model in equation (1) where t refers to the cohort of the youngest child, $Treat$ is equal to one for mothers of children who enroll in child care at age five, while $Post$ is equal to one if the youngest child is born in 1991. We also include the same covariates we use when measuring children’s outcomes.

Results are reported in Panel C of Table 7. We first estimate the impact of the reform Sweden. See also Schlosser (2005); ?); Cascio (2009); Havnes and Mogstad (2011b) and Berlinski and Galiani (2007). For a review of the literature, see Blau and Currie (2006).

³⁷We have also estimated effects when the child is six (when the child may be enrolled September through December), finding no impact of the reform.

on annual earnings, normalized to 2011-USD. The results show a modest impact on annual earnings ($p = 0.092$). To explore whether mandating kindergarten had an effect on the extensive or the intensive margin of maternal labor supply, we next estimate the impact on the probability of being employed (row 2) or being in full time employment (row 3) finding no impact on either margin. Thus, in spite of a marginally significant increase in earnings, we find little to suggest that the reform improved labor market attachment of mothers in our sample.

7 Concluding remarks

Evidence on the impact of child care interventions has been dominated by estimates from targeted programs. These may be hard to apply to the general population. Recent research provides some insight into the effects of large-scale programs. However, knowledge on the impact of the universal programs advocated in many western countries is still scarce (Baker, 2011). This is particularly worrisome given the heterogeneity created by wide differences in individual alternatives to subsidized care. The high returns found for children from disadvantaged families, coupled with much lower participation rates in existing programs compared to children from more advantaged backgrounds, suggests a potentially strong social gradient in expanding or mandating early childhood interventions (Barnett and Belfield, 2006). Indeed, in an effort to counter differences at school entry depending on social background, many countries are currently moving towards subsidized kindergarten or child care available for the general population.

In the current paper, we provide first evidence on the effect of mandating kindergarten at age 5–6 on children’s schooling outcomes. Specifically, we consider the impact on school performance at the end of compulsory schooling at age 15–16, on high school dropout and on the likelihood of enrolling in an academic track. Our identifying variation comes from a 1997-reform in Norway that lowered school starting-age from seven to six. The goal of the reform was to counter differences in learning outcomes between children from different socioeconomic backgrounds. Our results suggest that extending the reach of kindergarten in the general population by making it compulsory, does little to counter differences in schooling outcomes between children from different socioeconomic backgrounds when access to child care is substantial. In our baseline estimation, the precisely estimated effect on the child’s exam performance is below 2 % of a standard deviation, with negligible impacts also on high school dropout and academic tracking. Estimates are similarly small when we consider effects across the grading distribution and in different subsamples defined from characteristics of the child or parents. A number of specification checks lend support to our empirical strategy.

It is also noteworthy that estimated effects are similarly negligible when we consider some alternative and less aggregated school outcomes that could reveal effects on some

particular sets of skills that are washed out in an aggregated measure. We further find little evidence for an impact on maternal labor supply.

Three alternative explanations for the weak effect of mandating kindergarten stand out. On the one hand, the results may suggest that parents sort relatively efficiently into the existing kindergarten programs, so that children that are not in such programs in fact may opt out partly because they will benefit little. Second, the results may suggest that the children affected by the program are too old to benefit, and that universal programs must start at earlier ages. In both cases, our results would imply that to universalize kindergarten at age 5–6 is a misguided strategy for improving outcomes of disadvantaged children. However, the weak effect could also suggest that it was the particular program that failed to generate benefits in this age group. This could imply that the focus on learning through play, emphasized in the implemented program, is not successful in generating learning gains in this group of children.

Finally, the conclusion that mandating kindergarten had little impact on children’s school performance may cut both ways: While the large benefits expected by proponents can be firmly rejected, our results also lend little support to claims of strong negative effects from opponents. This is true even though the reform implemented a fully mandated program affecting families that did not voluntarily enroll their children, and who would otherwise care for their children themselves. It should be noted, however, that these estimates are driven mostly by children from relatively lower socioeconomic backgrounds, and may not be representative for children from higher socioeconomic backgrounds.

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A Appendix

Table A1: School performance and enrollment in kindergarten by family income decile at age five, children born in 1990.

Decile	Family income	School performance	Enrollment in kindergarten
1	15,912 (12,640)	3.42 (1.00)	0.54 (0.50)
2	45,124 (4,462)	3.53 (0.98)	0.65 (0.48)
3	57,408 (2,962)	3.60 (0.97)	0.72 (0.45)
4	66,968 (2,576)	3.68 (0.97)	0.82 (0.38)
5	75,547 (2,380)	3.73 (0.95)	0.88 (0.33)
6	83,524 (2,271)	3.79 (0.93)	0.91 (0.29)
7	91,695 (2,462)	3.86 (0.95)	0.92 (0.27)
8	101,363 (3,249)	4.02 (0.93)	0.93 (0.26)
9	116,100 (5,757)	4.11 (0.91)	0.93 (0.26)
10	169,179 (90,272)	4.30 (0.89)	0.92 (0.27)

Notes: This table corresponds to figure 1. School performance is measured as the average exam performance at end of compulsory schooling (age 16). Enrollment in kindergarten is measured at age five. Standard deviations are in parentheses.

Table A2: Robustness – High school drop out

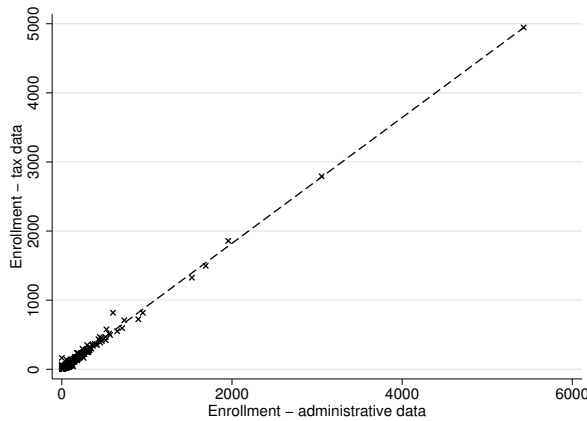
	Sample	Post	Coeff	SE	N
A. Key specification check					
Placebo	1989–1990	1990	-0.001	(0.007)	110,171
B. Treatment-specific trends					
Extended sample	1988–1991	1991	0.013	(0.005)	218,485
Linear trend	1988–1991	1991	0.011	(0.009)	218,485
Quadratic trend	1988–1991	1991	0.019	(0.023)	218,485
C. Flexible trends					
Trend \times covar	1988–1991	1991	0.005	(0.006)	213,472
Year FE \times covar	1988–1991	1991	0.008	(0.006)	213,472
D. Other					
1st diff.: Treatment	1990–1991	1991	0.017	(0.007)	18,108
1st diff.: Comparison	1990–1991	1991	0.004	(0.003)	93,288

Notes: Column 2 gives the estimation sample. In all estimations, $Post_t = 1$ for t is given in Column 3. In Panel A estimation is based on OLS on equation (1). In Panel B, estimations are based on equation (1), including a linear (row 2) and a quadratic (row 3) treatment-specific trend. In Panel C, estimations are based on equation (1), including a linear trend (row 1) or cohort dummies (row 2) interacted with a set of baseline covariates (school size; mother’s and father’s education level; municipal income; urban area). In rows 1 and 2 of Panel D, estimations are based on equation (2), while row 3 is based on equation (3). The controls are listed in table 2 and the dependent variables are defined in section 4. Standard errors (SE) are robust for heteroskedasticity and all models include municipality fixed effects.

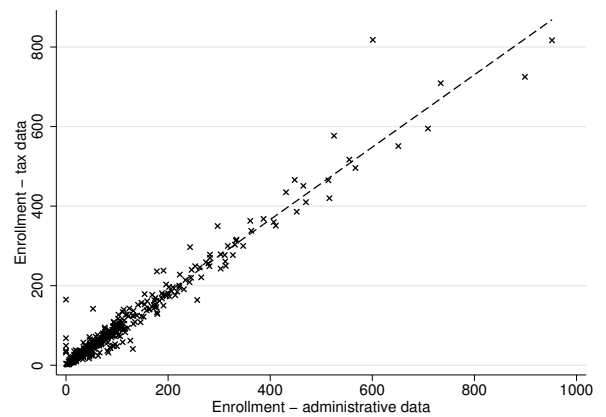
Table A3: Robustness – Academic track

	Sample	Post	Coeff	SE	N
A. Key specification check					
Placebo	1989–1990	1990	-0.003	(0.008)	105,894
B. Treatment-specific trends					
Extending pre-reform	1988–1992	1991–92	-0.002	(0.005)	258,112
Linear trend	1988–1992	1991–92	-0.008	(0.009)	258,112
Quadratic trend	1988–1992	1991–92	-0.018	(0.011)	258,112
C. Flexible trends					
Trend \times covar	1988–1992	1991–92	0.006	(0.005)	252,495
Year FE \times covar	1988–1992	1991–92	0.003	(0.005)	252,495
D. Other					
1st diff.: Treatment	1990–1991	1991	-0.019	(0.007)	17,203
1st diff.: Comparison	1990–1991	1991	-0.011	(0.003)	90,504
Delayed effect	1988–1992	1991	-0.009	(0.006)	258,112
		1992	0.007	(0.007)	

Notes: Column 2 gives the estimation sample. In all estimations, $Post_t = 1$ for t is given in Column 3. In Panel A estimation is based on OLS on equation (1). In Panel B, estimations are based on equation (1), including a linear (row 2) and a quadratic (row 3) treatment-specific trend. In Panel C, estimations are based on equation (1), including a linear trend (row 1) or cohort dummies (row 2) interacted with a set of baseline covariates (school size; mother’s and father’s education level; municipal income; urban area). In rows 1 and 2 of Panel D, estimations are based on equation (2), while row 3 is based on equation (3). The controls are listed in table 2 and the dependent variables are defined in section 4. Standard errors (SE) are robust for heteroskedasticity and all models include municipality fixed effects.

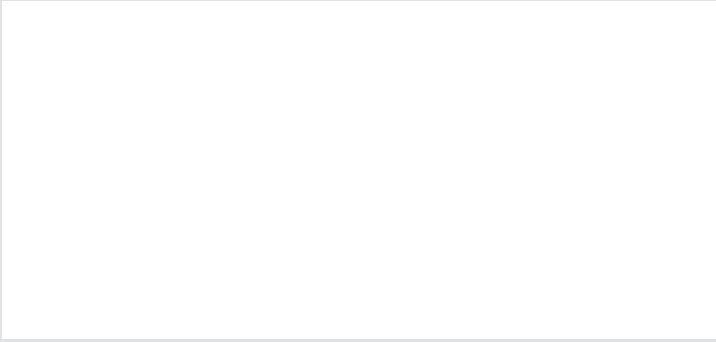


(a) Full sample



(b) Truncated sample

Figure A1: Enrollment – tax data and administrative data



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