**Universal Early Childhood Education and Care for Toddlers and Achievement
Outcomes in Middle Childhood**

Henrik Daae Zachrisson^{a*}, Eric Dearing^{a,b}, Nicolai Topstad Borgen^a, Astrid Marie Jorde
Sandsør^a Lynn Karoly^{a,c}

^a Department of Special Needs Education, University of Oslo

^b Lynch School of Education and Human Development, Boston College

^c RAND Corporation and the Pardee RAND Graduate School

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*Correspondence concerning this article should be addressed to Henrik Daae Zachrisson, Department of Special Needs Education, University of Oslo, PB 1140 Blindern, 0318 Oslo, Norway. Email: henrikdz@uio.no

Abstract

In this study, we estimate the effects of the scale-up of Norway's universal ECEC program—expanding access to 1- and 2-year olds starting in the early 2000s—on standardized math and achievement tests in 5th grade (age 10) using a population-based survey sample (Norwegian Mother, Father, and Child Cohort Study, MoBa, $n = 102,352$), linked with national administrative records of child achievement test scores. These data support using fixed-effects regressions and instrumental variable regressions to make inferences about the causal impact of ECEC scale-up on middle childhood achievement. We find the scale-up of ECEC starting in the second year of life improved test scores, especially for children from families with low levels of parental education, thereby reducing the achievement gap between children of parents with the highest and lowest education by up to 50%.

Keywords: Early Childhood Education and Care, Universal Child Care, Toddler Care, Achievement, Instrumental Variables, Fixed-Effects, MoBa

Universal Early Childhood Education and Care for Toddlers and Achievement

Outcomes in Middle Childhood

Universal publicly-funded Early Childhood Education and Care (ECEC) programs for preschoolers are implemented in many European countries, and also increasingly so in the US, where programs targeted at disadvantaged children have been more common (Cascio, 2021). Proponents of universal ECEC argue that—by avoiding confusion over the eligibility rules required for targeted programs and the stigma that can arise with programs focused on serving at-risk children only—participation rates will be higher among children who can benefit most; that disadvantaged children gain from being in more diverse child groups; that middle-income families struggle to pay out-of-pocket for ECEC programs and their children can also benefit from a high-quality early learning experience; that universal programs attract stronger public support; and that there is a stronger focus on program quality when socioeconomically advantaged children also attend (e.g., Barnett, 2010; Karoly, 2009). In the US, most of the debate around effectiveness of ECEC, and on universal programs specifically, has focused on programs for preschoolers (Barnett, 2010; Farran, 2016; Phillips et al., 2017). However, recent policy proposals have introduced the idea of universal ECEC programs for infants and toddlers as well (e.g., Warren, 2019). While controversial in the US, universal programs for 1- and 2-year-olds have been implemented in Northern Europe over the last decades (Dearing, Zachrisson, Mykletun, & Toppelberg, 2018; OECD, 2006), and evidence on their achievement effects is expanding.

A meta-analysis of 27 quasi-experimental studies of universal programs in Europe and the US found overall positive short- and long-term effects, strongly driven by programs with high structural quality (in terms of staff education level and child:staff ratios), with effects most pronounced for socioeconomically disadvantaged children (van Huizen & Plantenga, 2018). These results are consistent with a recent narrative review on the same topic

(Blau, 2021). Yet, evidence on longer-term effects of universal programs that begin in infancy or toddlerhood is scarce, as studies have mainly examined outcomes measured during or just after the end of the program for this age group (van Huizen & Plantenga, 2018).

In this article, we add to the literature by estimating the effects of universal ECEC beginning when children are toddlers, on national achievement tests when children are 10 years old (i.e., 5th grade), examining differential effects by family socioeconomic status indicators. To do so, we exploit the national scale-up of universal ECEC in Norway that occurred during the 2000s. We use data from a national health survey, the Norwegian Mother, Father, and Child Cohort Study (MoBa; Magnus et al., 2016), covering the most crucial years of the policy expansion, linked with national registries on achievement outcomes, demographics, income, and residency, as well as public records on the proportion of the age group enrolled in ECEC.

The policy expansion leveraged to identify ECEC effects in this study has previously been used by Dearing et al. (2018). They found that the ECEC expansion reduced language gaps at age 3 between children from low- and high-income families. Their study, however, relied on parent-reported language outcomes at a very young age when children were still attending ECEC. To extend their analyses, we used national achievement tests that are not subject to parent-report bias (and attrition due to parent non-response) in addition to measuring outcomes many years after ECEC exposure. We found positive long-term effects of ECEC attendance on math and reading test scores in 5th grade, suggesting that the effects found by Dearing et al. (2018) persist into primary education. Moreover, the effects were larger for children from families with low parental education, reducing the achievement gap between children of parents with the highest and lowest education by up to 50%. ECEC effects were also found to vary by parental income, but the statistical significance of income interaction effects are lower and less consistent than those for parental education.

Effects of ECEC and alignment with children's development

Meta-analyses have, quite consistently, shown targeted preschool programs – for 3 to 4 year old children – to be effective in promoting pre-academic cognitive skills in the short run, with effect sizes averaging around 20% to 30% of a standard deviation (Camilli, Vargas, Ryan, & Barnett, 2010; Duncan & Magnuson, 2013). There is also some meta-analytic evidence of persistent effects throughout adolescence and early adulthood on outcomes such as grade retention and special education placement (McCoy et al., 2017). The same is, as mentioned, true for universal preschool programs in cases where structural quality is high, with effects evident primarily among socioeconomically disadvantaged children (van Huizen & Plantenga, 2018).

There are, however, notable exceptions. Most prominent are quasi-experimental studies of Quebec's scale-up of universal ECEC (Baker, Gruber, & Milligan, 2008; Michael Baker, Gruber, & Milligan, 2019; Kottelenberg & Lehrer, 2017). Notably, this scale-up covered children aged 0 to 4. These studies found mixed short- and long-term effects on cognitive- and academic outcomes (for example, negative effects of about 20% of a standard deviation of program exposure on a Canadian national test in math and reading for ages 13 and 16, yet with positive effects of about 10-30% for PISA math and reading scores; Baker et al., 2019). They also found short- and long-term negative effects on socio-emotional and health outcomes. Consistent with meta-analytic evidence on effects of universal ECEC being conditional on quality (van Huizen & Plantenga, 2018), observed quality in Quebec at the time was very low, especially among the youngest children (Japel, Tremblay, & Cote, 2005).

While the majority of universal ECEC programs in high-income countries include preschool-age children (aged 3 or 4), developmental scientists have long argued that the earliest years (ages 0 to 3) are foundational in terms of brain development and, thereby, cognitive and language development (e.g., Shonkoff & Phillips, 2000). Socioeconomic

disparities in development are evident already during children's earliest years, exemplified but not restricted to language development. In vocabulary learning and language processing at age 1 ½ and 2 years, children of low-educated parents in the US lag six months behind their counterparts with highly educated parents (Fernald, Marchman, & Weisleder, 2013). As initially suggested by Hart and Risley (1995), this early socioeconomic discrepancy in language development may be a function of the quantity and quality of language exposure in the home. More recently, language-enriched environments have been shown to be consequential for early brain development (Romeo et al., 2018). Socioeconomic gaps in pre-academic skills like vocabulary are evident as early as age 2 also in several European countries, including progressive welfare states like Norway and Germany. In Germany, these gaps remain quite stable into school age (e.g., Passaretta & Skopek, 2018; Skopek & Passaretta, 2020), while they increase throughout school age in Norway (Sandsør, Zachrisson, & Karoly, 2021). This evidence points to the potential value of intervening early, suggesting that compensatory interventions like enriched ECEC should start as early as toddlerhood.

Evidence is much more scarce with regard to the effectiveness of ECEC programs that begin prior to the preschool years, in infancy or toddlerhood, compared to those that begin in preschool. Some of the evidence comes from correlational studies in contexts without universal access policies. For example, a series of studies in the US have used data from the National Institute for Child Health and Human Development Study of Early Child Care and Youth Development (NICHD SECCYD) to examine a range of available ECEC settings, including center-based, family daycare, and home care. In these studies, time spent in center-based ECEC during the first three years of life was found to be positively associated with language- and school readiness scores at age 3 (NICHD Early Child Care Research Network, 2000). Collapsed across setting types, observed high-quality ECEC prior to age 3 was also found to be more strongly associated with age 3 assessments of language- and school

readiness skills among low-income children, compared to their more affluent peers (McCartney, Dearing, Taylor, & Bub, 2007). In the same dataset, Dearing et al. (2009) found that larger doses of high-quality ECEC (not exclusively center care) closed the achievement gap between children from high- and low-income families in middle childhood, and closed income- and graduation gaps in early adulthood (Bustamante, Dearing, Zachrisson, & Vandell, 2021). Notably, neither of these studies focus exclusively on ECEC starting in toddlerhood.

Consistent with these studies, other scholars using the SECCYD have found that time spent in high-quality settings is associated with higher scores on long-term achievement tests at ages 12 and 15, yet with no effects on achievement outcomes for proportion of time spent in center care (Belsky et al., 2007; Vandell, Belsky, Burchinal, Steinberg, & Vandergrift, 2010). Using a different US dataset (National Longitudinal Survey of Youth), Jaffee, Van Hulle, and Rodgers (2011) report positive associations between math and reading scores at ages 11-13 and entry into ECEC in the first year of life, as well as in the second and third years, with coefficients around 30% of a standard deviation. Notably, none of these associations were upheld when adding sibling fixed-effects in the models.

Stronger causal evidence for effects of universal ECEC beginning in infancy or toddlerhood comes from a few experimental and quasi-experimental studies. Results from a large randomized study of a high-quality program in the US (The Infant Health and Development Program) were extrapolated to population-level effects of either a universal program, or a program targeted at low-income children, starting at birth or age 1 and continuing until age 3 (Duncan & Sojourner, 2013). In both cases, these programs were estimated to reduce socioeconomic gaps in cognitive abilities between children from high- and low-income families. The effect sizes were large; for example, the difference in IQ scores between low-income children attending and not attending the programs was 86% of a

standard deviation. In a French quasi-experimental study, Berger, Panico, and Solaz (2021) found short-term effects at age 2 of early ECEC attendance beginning at age 1 using season of birth in instrumental variable (IV) analysis. Children who started ECEC at age 1 had about 30% of a standard deviation better language skills compared to those in home care, but also more behavior problems. Effect sizes were about half the size when using conventional regression analyses models, compared to season of birth in instrumental variable (IV) analyses, while standard errors were about 7 times smaller in the regression analyses.

A study from Norway exploited a lottery allocating children to slots in public childcare (Drange & Havnes, 2019). Children assigned by the lottery started ECEC at approximately age 1, while those not assigned started, on average, 6 months later. Those starting early scored 16% and 11% of a standard deviation better in reading and math, respectively, on screening tests in first grade (age 6). Moreover, for children of low-educated parents, the effects were about 60% larger than the average effects, fully closing the gap observed between children from high- and low educated families in reading, while reducing it by 25% in math. Finally, an Italian study found positive effects on 5th-grade language test scores of ECEC-attendance for toddlers, but only for children with immigrant backgrounds, and no effects on math scores (Corazzini, Meschi, & Pavese, 2021). These authors exploited variations across municipalities over time in ECEC availability in an instrumental variable analysis, finding a treatment effect for immigrants of about 60% of a standard deviation.

There are also two examples of studies exploiting policy reforms of expanding ECEC for toddlers to identify short-term effects of ECEC, with empirical strategies similar to the one we use in the present study (instrumental variables; IV). In Germany, Felfe and Lalive (2018) found the expansion of ECEC slots to improve language- and motor skills for boys and immigrant children at age 6 with about 20% and 33% of a standard deviation, respectively. In Norway, as already mentioned, Dearing et al. (2018) used the same dataset that we use in the

present study, and found the expansion of ECEC impacting toddlers. They reported effects on language skills at age 3 of 89% of a standard deviation for low-income children attending ECEC at age 1 ½, 31% for middle-income children, and 29% (not statistically significant) for high-income children.

Taken together, the evidence from across contexts and research designs seems to suggest that universal ECEC starting in late infancy or during the toddlers years (with sufficiently high quality) is effective in improving language, cognitive, and achievement-related outcomes in the short run. Also, these programs may have added achievement benefits for children from low socioeconomic backgrounds. Yet, it remains uncertain to what extent these effects persist throughout school age, or whether they fade out. Fadeout is a concern with regard to ECEC and other early childhood interventions in general (Bailey, Duncan, Odgers, & Yu, 2017). There are examples of convergence in achievement scores across school age between children who attended and not attended preschool (Puma et al., 2012; Yoshikawa, Weiland, & Brooks-Gunn, 2016), as well as reversals of initial effects, eventually favoring children in the control group (Lipsey, Farran, & Durkin, 2018). The present study is therefore critical in, to our knowledge, being among the first to address persistent effects of universal ECEC for toddlers well into school age.

ECEC Policy and Scale-Up in Norway

Since the mid-70's Norway has gradually scaled up universal, publicly-subsidized ECEC. The first large expansion was a result of the Kindergarten Act of June 1975 that greatly increased government funding for investment and operating costs, leading to a substantial expansion of ECEC coverage for 3- to 6-year olds (see Havnes & Mogstad [2011] for details). In the 1990s, Norwegian ECEC policy was focused on establishing federal regulations for quality of care (e.g., teacher educational requirements and an educational content framework) for both center-based care and family daycare (Ministry of Education,

2014). Meanwhile, ECEC for 1- and 2-year olds gradually became a policy focus. In 2002, a mandate required Norwegian municipalities to provide access for this age group in ECEC centers and/or family daycare units, leading to an increasing number of ECEC slots for 1- and 2-year olds. The 2002 mandate legally anchored the incremental progression toward universal access, and with this mandate, public spending increased while corresponding family fees decreased. A maximum fee for full-time care was introduced in 2004 (2750 NOK or ~USD 400/month, 2004 exchange rate) and lowered in 2006 (2250 NOK or ~USD 360/month, 2006 exchange rate). The maximum fee was lower for low-income households, and the sliding scales below the maximum fee varied at the discretion of municipalities. Driven primarily by the mandate and these additional policies, the national coverage rate of ECEC for 1- and 2-year olds expanded from 38% in 2001 to 77% in 2009. The coverage rate is defined as the number of 1- and 2- year olds enrolled in ECEC divided by the number of 1- and 2- year olds in the municipality.

Progress toward universal access, however, varied considerably across municipalities-- Norway has a two-tiered local governing structure of 19 counties divided into 428 municipalities--due to a wide range of idiosyncratic local circumstances, which we exploit in an IV framework. Obstacles to increased ECEC slots were primarily due to: a) lack of available spaces for building new centers, high building cost, and lack of available contractors (particularly for the major cities); b) lack of qualified staff (particularly for the smaller municipalities); c) concerns in some municipalities about “over-coverage” due to year-by-year variations in birth-rates and for some municipalities conservative predictions of demand; and, d) local concerns over the availability of long-term earmarked funding from the government (Rindfuss, Guilkey, Morgan, Kravdal, & Guzzo, 2007; Rindfuss, Guilkey, Morgan, & Kravdal, 2010; Asplan Virak, 2006; 2009). In Figure 1, we show that the median coverage rate within municipalities in Norway increased from 41% of all 1- and 2-year olds in

2002 to 76% in 2008 (the time span covered in our study), while the variability across municipalities was considerable. In 2002, municipalities at the 25th percentile had a coverage rate of 31%, while the 75th percentile was 53%. In 2008, these gaps had narrowed, with the 25th percentile being 69% and the 75th percentile being 82%. This disparity, leading to different rates of change in ECEC coverage over time across municipalities, is the basis for our key identification strategy, described in detail below.

Fig. 1. Municipality-level variation in coverage of ECEC for 1- and 2-year olds, across years 2002-2008. The coverage rate is defined as the number of 1- and 2- year olds enrolled in ECEC divided by the number of 1- and 2- year olds in the municipality.

It is also worth noting that the expansion of ECEC for 1- and 2-year olds occurred within the context of progressive Norwegian parental leave policies. During the time of the expansion, parents received up to one year of paid leave, and as a result, few children entered non-parental care prior to 9 months of age. After parental leave, parents had a choice between enrolling their child in publicly subsidized ECEC or receiving cash benefits (approximately

NOK 3.500/month, or ~USD 560/month using the 2006 exchange rate) for staying home with their children (Ministry of Children, 2014). Parents were eligible for cash benefits until the child turned 3 years old, and could switch between ECEC and cash benefit at their own discretion.

In terms of quality regulations, several aspects of quality and content of Norwegian ECEC were, and still are, regulated at a national level. In the 2000's, structural quality regulations stipulated that at least 30-35% of the staff should be ECEC teachers (with a 3-year university college degree) and that there should be a leader with ECEC teacher education in each center. Adult:child ratios of 1:3 for those under 3 and 1:6 for those older were also recommended, but not enforced by law. In ECEC centers, most children under age 3 attended infant-toddler groups of about nine children with three staff, one of them a trained ECEC teacher. Yet, variations in group size and age composition were allowed, as long as the staff requirements were met.

The pedagogical content was guided by a "Framework" curriculum plan with national guidelines regarding the values and purpose of ECEC centers, their curricular objectives, and educational approaches (Ministry of Education, 2006). The curriculum plan specified there to be stimulating verbal and non-verbal interactions in all everyday situations, age-appropriate use of learning materials, and a learning-rich environment that included work with symbols, books, and reading.

Although Norwegian ECEC could be considered as a national program, its implementation varied between ECEC centers within the limits of the legal requirements for structural quality and the "Framework" plan. Evidence of such quality variation in Norway is somewhat mixed. One study has indicated that structural quality standards mostly followed regulations and recommended standards for the period covered by the present study (Winsvold & Guldbrandsen, 2009), while others suggest that quality standards were not met

in all centers across Norway (Brenna et al., 2010; OECD, 2015). Moreover, observed quality for more recent cohorts (than those covered by the present study) find that most classrooms meet only mediocre quality standards, as assessed with the ITERS-R (Harms, Cryer, Clifford, & Yazejian, 2017), in particular concerning hygiene, safety, and access to play materials (Bjørnstad & Os, 2018).

Present study

In the present study, we hypothesized that attending ECEC from the second year of life would improve math and reading achievements in 5th-grade. We also hypothesized that these effects would reduce achievement gaps between children from lower- and higher socioeconomic status backgrounds (i.e., low family income and/or low parental education level).

We first conducted regression analyses, conditional on family-level covariates as well as municipality and birth cohort fixed effects. Our extensive set of covariates, along with exploiting within-municipality and -cohort variation, restricted the potential for omitted variable bias at the family- and municipal level. Yet, these estimates are still susceptible to bias caused by unobserved between-family heterogeneity. Thus, we also estimated two-stage least squares IV regressions, including fixed effects for municipality and cohort. In these analyses, we instrumented ECEC use at age 1½ years on the within municipality-by-cohort coverage rate of ECEC for 1- and 2- year olds.

Methods

We used data from the Norwegian Mother, Father, and Child Cohort Study (MoBa; for complete details, see Magnus et al., 2016, and www.fhi.no/morogbarn; Magnus, Irgens, Haug, Nystad, Skjaerven, Stoltenberg, et al., 2006), a population-based pregnancy cohort study conducted by the Norwegian Institute of Public Health. Participants were recruited from all over Norway from 1999-2008. The full sample included 114,500 children, 95,200 mothers,

and 75,200 fathers at baseline. The current study is based on version 12 of the quality-assured data files released for research in January 2019. For the present study, MoBa was linked (through person identification numbers) with Norwegian administrative records, provided by Statistics Norway. This study included all children in MoBa born in 2002-2008, resulting in a sample of 102,352 children. Of those invited to participate in MoBa, 41% agreed to participate. Yet, although all women giving birth in Norway were the target population, only hospitals and birth units with more than 100 births annually were included, and the roll-out of the study was gradual across the first years (Magnus, Irgens, Haug, Nystad, Skjaerven, & Stoltenberg, 2006). Analyses based on our linked data show that between 16.9% (in 2002) and 31.3% (in 2006) of all births in Norway were included in MoBa. In Appendix 1, we show the income- and education-distributions of families participating in MoBa, compared to the whole population of families with children born across 2002-2008 (Figure S1 and Table S1, see description of measures below). As previously indicated (Maguns et al., 2006), families with lower income and low levels of education are under-represented in MoBa. However, the associations between achievement test scores in 5th grade, and income and education, respectively, are almost identical in the MoBa sample and in the comparable population (see online Appendix 1).

Retention rates in MoBa were 66.4% at age 1.5 and 53.5% at age 3. In the present study, we used data collected at 0.5 years (family-level covariates), 1.5 years (child care use and family-level covariates), and at age 3 (family-level covariates). The MoBa was linked to national registries covering the entire population, including the population registry (demographic information), the income registry (family income), and the education registry (parents' education level and child test scores from national tests in 5th grade). Linkage, via person identification numbers, was available from the birth cohorts of 2002 and onwards.

The establishment of MoBa and initial data collection was based on a license from the Norwegian Data Protection Agency and approval from The Regional Committees for Medical and Health Research Ethics. The MoBa cohort is now based on regulations related to the Norwegian Health Registry Act. The current study was approved by The Regional Committees for Medical and Health Research Ethics. The data linkage with administrative records used in the present study was approved by The Regional Committee for Medical Research Ethics and the Data Protection officer at the first author's host institution.

Measures

National test scores, 5th grade. Compulsory national assessments for children in 5th grade have been administered every fall in Norway since 2007, in the subjects reading (Norwegian), mathematics, and English (the first two included in the present study). Test development is commissioned by the Directorate of Education and Training and carried out by subject experts at universities in Norway along with psychometric experts in the Directorate (see <https://www.udir.no/eksamen-og-prover/prover/nasjonale-prover>). The tests are designed to capture the full range of subject skills among students in 5th grade. About 96% of all students in Norway take the test; students with special needs and those following introductory language courses may be exempt. Data from 2007 and onwards are available as a score summing up correct responses. In addition, from 2014 (for the 2004 and later cohorts), a scaled score based on a 2-parameter IRT model is available (for details, see <https://www.udir.no/globalassets/filer/vurdering/nasjonaleprover/metodegrunnlag-for-nasjonale-prover-august-2018.pdf>). In the present study, we standardized the summed test scores within year and conducted robustness checks with scaled scores.

ECEC arrangements. Mothers reported the type of child care used at age 1.5 years, representing the child's primary care arrangement. Choices included "at home with mother or father", "at home with unqualified child minder", "family day care", and "center care". From

these reports, we computed a dummy variable indicating whether children were in center-based ECEC versus any of the other arrangements. Center-based ECEC was the target of the expansion, and thus the policy-relevant focus given the recent discussions toward universal ECEC.

Municipality-level ECEC coverage. For our instrumental variable analyses, we used municipality-level ECEC coverage rates as an instrument. Coverage rates were derived from public records of the total ECEC coverage for children aged 1 and 2 in municipalities by year (<https://www.ssb.no/en/statbank/table/12562/>). Because the municipalities fund public ECEC, these public records are an accurate representation of the availability of ECEC. We link the municipality-by-year coverage rate to children via the child's residential municipality at age 1.

Household income and family education. We used annual tax records for each household member in the child's primary household (tracked through the population registry) to measure household income. We combined the after-tax income for each household member (18 years or older), and adjusted incomes for the consumer price index to 2018-levels. The lowest 2% of incomes (in the population) were set to missing and imputed (see description of imputations below) because very low or negative incomes can indicate that losses from private companies have been deducted on the tax records rather than their reported income being low. Moreover, incomes were truncated at the 98th percentile to remove the influence from outliers. We then averaged household income over the first three years of the child's life and centered it at the 2nd percentile of the income distribution (approximately NOK 180,000, or USD 22,100, 2018 average exchange rate), which was the lowest observed income, and divided it by 100,000.

For the municipality level analyses (as part of instrument validation, see below), we calculated a ratio of family income-to-needs to account for variations in family size. For each

child across the three first years of life, we divided total after-tax income by the OECD poverty line for each particular year (50% of the median income, adjusted for family size; OECD, 2011).

The highest family education was drawn from the education registry, recording the highest completed level of education for mothers and fathers. We converted the ordinal scale into years of education, following the standard guidelines from Statistics Norway. We defined the family education variable as the highest value of either the mother's or father's education. For analyses, we centered highest level of education on 9 years, i.e., completed compulsory schooling, which is the lowest common level of education.

Covariates from registries. From the registries, we include demographic information: Child gender; Maternal age (in years, truncated at 3rd and 97th percentile); Child age at testing); Immigrant status: one western immigrant indicator (0, 1) for being born or having at least one parent born in Europe (except Norway), North America, or Oceania, and one non-western immigrant indicator (0, 1) for being born or having at least one parent born in the rest of the world (except Norway); Number of children in the household (count of household members ≤ 18 years as an average across the child's first 3 years of life, truncated at 4. i.e., the 99th percentile); Number of adults in the household (count of household members > 18 years as an average across the child's first 3 years); and birth month dummy variables.

For selected covariates, we compute aggregated municipality-by-year-level covariates based on all families within a municipality-by-year cluster with a child aged 1 for the instrument validation (see details below). These aggregated variables included proportion of families with 1) less than 12 years of education; 2) more than 16 years of education; 3) low income (income-to-needs of 1.5 or less); 4) high income (income-to-needs of 2.5 or above); 5) filing for any unemployment benefit in the child's first two years; 6) western- and non-western immigrants. A few small municipalities have extreme univariate outliers on the

demographic variables. We capped these aggregate variables at the 95th percentile of the municipality-weighted distribution.

From the Medical Birth Registry, we included birth weight (dichotomized [0.1]: < 2500 and ≥ 2500 grams), Apgar score (five minutes after birth), and multiple birth (i.e., twins).

Covariates from MoBa. Mothers reported on their anxious/depressive symptoms (Tambs & Moum, 1993) and partner/spouse relationship satisfaction (Rosand, Slinning, Eberhard-Gran, Roysamb, & Tambs, 2011) when children were ages 0.5, 1.5, and 3. We included the averages of these scales across the three time points.

Statistical analyses

We used birth cohort by municipality fixed effects (FE) regressions and instrumental variables (IV) regressions. The FE regression is an efficient estimator for effects of ECEC on achievement outcomes. These analyses provide unbiased estimates under the assumption that ECEC use is as-random after accounting for a detailed list of child and family covariates and fixed effects. Our IV analyses rely on a different set of assumptions. We use municipality by cohort variations in availability of ECEC as an instrument for ECEC use. Assuming a strong and valid instrument, these models provide unbiased estimates of the effect, although less efficient than our FE regression estimates.

Birth cohort by municipality fixed effect regressions. Our first set of analyses estimated the regression coefficient for the individual child's ECEC use conditional on covariates (W) and municipality (α) and cohort (γ) fixed effects, where i , m , and c index individual, municipality, and cohort:

$$Outcome_{imc} = \alpha_m + \gamma_c + \beta_1 ECEC_{imc} + \beta_2 W_{imc} + \epsilon_{imc} \quad (1)$$

The covariates included all child- and family variables listed above and shown in Table 1 (also education and income), and indicators for birth month (to account for the main uptake in ECEC in August). After estimating the main effects of ECEC use, we included

product terms that allow the estimated effects of ECEC to vary by family income and family education.

$$Outcome_{imc} = \alpha_m + \gamma_c + \beta_1 ECEC_{imc} + \beta_2 SES_{imc} + \beta_3 ECEC_{imc} * SES_{imc} + \beta_4 W_{imc} + \epsilon_{imc} \quad (2)$$

Following recommendations by Duncan & Magnuson (2003), our primary analyses included modeling interactions with parental income and education separately, under the assumption that these moderately correlated indicators of socioeconomic indicators (.38 in our data) capture financial and human capital differentially. As a robustness check, we also included them simultaneously.

The FE regression models are based on the assumption that after accounting for observed family and individual characteristics, outcomes of children who did not attend ECEC can serve as a counterfactual for the children who attended ECEC within the same municipality by birth cohort group. While these FE regression models provide an efficient estimator of the treatment effect, we must still be concerned about unobserved family selection factors occurring within municipality. We probed the omitted variable risk by testing the robustness or fragility of our estimates in supplementary sensitivity analyses, using the coefficient of proportionality method (as described in Dearing & Zachrisson, 2019; Oster, 2019). This method provides an indication of how large the impact of unobserved selection factors would need to be relative to unobserved variables to nullify our FE regression results.

Instrumental variable regressions. In our second set of analyses, we estimated two-stage least squares IV regression models with municipality and cohort fixed effects. Our instrument was the coverage rate of ECEC in each of Norway's municipalities in each of the study years. Hence, variation in our instrument is time by municipality variation in ECEC coverage. There were considerable differences between municipalities in the extent to which coverage increased across the time period 2002-2008 (see Figure 1). In Figure 2 below, we

illustrate the differences in rates of change. We graph four groups of change rates in ECEC coverage, created by subtracting the within-municipality coverage in 2008 from that in 2002, and then splitting the rate of change into quartiles. From 2002 through 2008, the median increase in coverage was 34.2 percentage points, while the 25 percentile was 23 and the 75 percentile was 43.

Figure 2. Changes in municipality-level ECEC coverage rate averaged for municipalities within quartiles of change (from those with the lowest change rate [starting on top] to the highest [starting at the bottom]).

The first and second stages of our IV models are shown in equations 3 and 4 below. In the first stage (Eq. 3), ECEC use was regressed on our instrument (i.e., the ECEC coverage rate in the corresponding cohort by municipality cluster for child i) and our covariate set (W). In the second stage (Eq. 4), the outcomes were regressed on predicted values of ECEC from the first stage ($ECEC'_{imc}$) in addition to covariates.

$$ECEC_{imc} = \alpha_m + \gamma_c + \beta_1 ProportionECEC_{mc} + \beta_2 W_{imc} + v_{imc} \quad (3)$$

$$Outcome_{imc} = \alpha_m + \gamma_c + \beta_1 ECEC'_{imc} + \beta_2 W_{imc} + \mu_{imc} \quad (4)$$

Our covariate set included all individual-level variables used in the previous models. Finally, as with the previous models, we tested interactions with family income and parent's education. For the IV models, we followed the simultaneous equation setup from Wooldridge (2010); the treatment and the instrument were multiplied with the moderator, and both interaction terms were included in the estimated models. Thus, we have two first-stage equations, one for the main effect of the treatment and one for the product of the treatment and the SES indicator. In each of the first-stage regressions, the covariate list includes the instrument, the product of the instrument and the SES indicator, and the observed covariates (Eq. 5 and Eq. 6). The second stage is the outcome regressed on the predicted values from these two equations as well as covariates (Eq. 7).

$$ECEC_{imc} = \alpha_m + \gamma_c + \beta_1 ProportionECEC_{mc} + \beta_2 ProportionECEC_{mc} * SES_{imc} + \beta_3 W_{imc} + v_{imc} \quad (5)$$

$$ECEC_{imc} * SES_{imc} = \alpha_m + \gamma_c + \beta_1 ProportionECEC_{mc} + \beta_2 ProportionECEC_{mc} * SES_{imc} + \beta_3 W_{imc} + v_{imc} \quad (6)$$

$$Outcome_{imc} = \alpha_m + \gamma_c + \beta_1 ECEC'_{imc} + \beta_2 (ECEC_{imc} * SES_{imc})' + \beta_3 W_{imc} + \mu_{imc} \quad (7)$$

To address instrument validity, we followed the analytical strategies of Cornelissen, Dustmann, Raute, and Schönberg (2018), who used a similar ECEC expansion instrument. The first assumption is that the policy expansion causes variation in the probability for treatment, conditional on covariates (i.e., instrumental relevance). We find this to be the case; the instrument was strongly associated with our treatment, with the first-stage F-test of the instrument being highly significant ($F[1, 117.60] = 141.49$), and where a 10 percentage point increase in coverage was associated with a 4 percentage point higher rate of ECEC use on average across municipality and cohort clusters, conditional on covariates. This relatively

strong first-stage in a just identified (i.e., one instrument) IV model reduces the risk of bias in the second stage, given that the instrument is otherwise plausibly random (Angrist & Kolesár, 2021).

The second assumption is that the instrument must be independent of the error term of the outcome, conditional on covariates and the municipality and cohort fixed effects. This assumption can be further separated into the ignorable treatment assignment assumption – unobserved variables do not confound the effects of the instrument on the outcome – and the exclusion restriction assumption – the instrument influences the outcome only through the treatment (Angrist, Imbens, & Rubin 1996). Our main logic is that we restrict our estimates to be within municipality to exploit this within-municipality variation. Without municipality and cohort fixed effects, these assumptions would probably not hold; municipalities or cohorts with high coverage rates may differ from units with low coverage rates in several ways. For example, urban areas have higher ECEC coverage rates than rural areas *and* urban area children typically have higher test scores (net of observed family characteristics, i.e., ignorable treatment assignment does not hold). We include municipality and cohort fixed effects in the IV model to account for such potential bias.

We tested the second assumption, that the instrument must be independent of the error term, by following the strategy outlined by Cornelissen et al. (2018). A concern could be that the ECEC expansion would be different across municipalities that differed across a range of demographic characteristics. If the expansion was associated with these differences, they could also be correlated with the error term. We regressed the change in ECEC coverage between 2002 and 2008 on initial coverage in 2002. As can be seen in Table S3, first column, the change in coverage was strongly negatively related to the coverage rate in 2002. To test whether other municipality characteristics were associated with the expansion, we added a number of baseline (2002) municipality-weighted characteristics for the entire child

population, the municipality size, and the fraction of children in the municipality participating in MoBa. The set of covariates did not affect the association between initial coverage in 2002 and change in coverage from 2002 to 2008, and none of the municipality level covariates in 2002 were significantly related to coverage that year. A joint significance test for the full set of covariates (except change in ECEC coverage) was far from statistically significant. This supports the notion that from the onset, the expansion we are exploiting is unrelated to municipality characteristics. Moreover, ECEC coverage in the first year of our time window (2002) was not correlated with either of the outcomes measured that year (not shown). Finally, second-born children with a sibling in ECEC can receive ECEC slot priority, and thus having an older sibling in care may influence the selection process. We thus tested the robustness of our models by including only first-born children in the robustness checks. As we report below, including only first-born children leads to the same results (Table S5).

We then addressed covariate balance for the instrument. While observed covariates are controlled for in the analyses, and hence will not bias the estimate, we would be more concerned about imbalance on unobserved variables if there were an imbalance on observed variables. To check this, we regressed our ECEC coverage instrument on municipality-level aggregated covariates. None of the municipality or family covariates were significant, except for the proportion of children within the municipality participating in MoBa, where a higher rate was positively associated with ECEC coverage (Table S4). As a supplement to the balance test, we also substituted the treatment (ECEC use) in the first stage equation with income and education, respectively, to test whether the multivariate combination of ECEC coverage and other covariates predicted these outcomes (not shown). In both cases, F-values were below 5, which indicates that ECEC coverage and the covariates in combination are associated with income and education to a negligible degree.

In sum, we find no evidence that our instrument violates any assumptions, which means that our IV estimates can be interpreted as causal effects. Our IV model identify the average effect of ECEC attendance for the compliers that are induced to attend ECEC because of the scale-up of Norway's universal ECEC program, known as Local Average Treatment Effects (LATE). Such complier treatment effects may differ from the average treatment effect identified by the fixed effect regression models.

Missing data. Despite nearly complete data on some key indicators such as family income and child test scores, there was considerable missing data due to attrition for ECEC use: 33% of children were missing ECEC data at 1.5 years. Although likelihood of having missing values on the study covariates was by and large unrelated to the treatment of interest, likelihood of attrition was slightly higher for more socially disadvantaged families. To account for this attrition, our statistical models were estimated using multiple imputation with 20 datasets for missing values, combining estimates and standard errors according to Rubin's Rules (Rubin. 1987). Our imputation models included both achievement measures and child- and family-level covariates listed in Table 1, in addition to the municipality-level covariates described above. All models were estimated using complete data only (listwise deletion) as a sensitivity check.

Results

Descriptive statistics can be seen in Table 1, including rates of missing data. Here is worth noting a few descriptive aspects of the key variables in our study. With regard to our treatment of interest, across all years, about 53% of children in the sample attended ECEC by 18 months. With regard to our moderators of interest, average (after tax) family income was NOK 663,000, in 2018 values (median income in 2019 for couples with children ages 0-7 was NOK 822,700, and for single parent with children 0-17, NOK 438,400; Statistics Norway, 2021), and average highest family education was 14.37 years (SD 2.68).

Table 1. Descriptive statistics. MoBa data (N=102,352)

	% missing	M(SD)/%	Min - Max
Achievement 5 th grade			
Math (unscaled)	5.77	0.18(0.98)	-2.98 - 2.36
Reading (unscaled)	4.63	0.18(0.97)	-3.08 - 2.20
Math (scaled – from 2004)	8.55	51.95(9.54)	19.36 - 79.66
Reading (scaled – from 2004)	9.59	51.94(9.49)	18.45 - 75.68
ECEC			
18 months	33.00	52.97%	
Socioeconomics			
Family income (NOK 100,000's)	0.04	6.63(2.24)	1.81 – 12.80
Family education (highest)	5 56	14.37(2.68)	0.00 - 23.00
Family characteristics			
Non-western immigrant	0.00	4.71%	
Western immigrant	0.00	9.51%	
Maternal age	0.00	-0.03(4.55)	-10.65 - 9.35
Maternal anxiety/depression	18.88	1.28(0.34)	1.00 - 4.00
Relationship satisfaction	20.06	5.15(0.74)	1.00 - 6.00
Number of children	0.00	1.79(0.62)	1.00 - 4.00
Number of adults	0.00	1.86(0.26)	0.33 - 4.33
Child characteristics			
Boy	0.00	51.27%	
APGAR 5 min	0.12	9.42(0.81)	0.00 - 10.00
Low birth weight	0.05	4.31%	
Twin birth	0.00	3.36%	
Birth month (average)	0.00	6.42(3.39)	1.00 – 12.00
Birth cohort			
2002	0.00	9.07%	
2003	0.00	13.05%	
2004	0.00	14.95%	
2005	0.00	15.89%	
2006	0.00	17.74%	
2007	0.00	16.53%	
2008	0.00	13.77%	

Estimating the Effects of ECEC on Achievement

Our primary results from both the FE regression and IV models are presented in Table 2. We first explain the FE regression results and then turn to the IV results.

Birth cohort by municipality fixed effect regressions. The left-hand panels in Table 2 provide results from fixed effect regression analyses. Although excluded from the table for brevity, the full set of covariates listed in Table 1 were estimated in these models (also, due to restrictive publication policies by the MoBa, we are not allowed to report coefficients for covariates). In the first row of Table 2, we report results from models regressing math and reading 5th-grade test scores on ECEC use. In the remaining rows, we report interactions between ECEC and both family income and parent education.

In these models, the effect of ECEC was statistically significant for math scores ($p < .01$), but above conventional levels for reading scores ($p = .07$). In terms of effect size, children attending ECEC had, on average, 2.6% of a standard deviation higher scores on math, compared to children not attending ECEC. In comparison, this difference was about 1.5% of a standard deviation for reading scores.

Table 2. Fixed Effects- and Instrumental Variable Regression Estimates of ECEC at 18 Months on 5th Grade Test Scores, for 2002-2008 Cohorts ($n = 102,352$) using Multiple Imputed Data ($n = 20$). Main effects and interactions with income and education.

	Fixed-Effects		Instrumental Variable	
	Math	Reading	Math	Reading
	Est. (se.)	Est. (se.)	Est. (se.)	Est. (se.)
ECEC Main Effect	.026(.009)**	.015(.008) [†]	.233(.136) [†]	.257(.137) [†]
Interactions w/income				
ECEC	.049(.018)**	.044(.018)*	.336(.130)**	.305(.131)*
Income	.042(.003)***	.022(.003)***	.051(.008)***	.021(.008)**
ECECxIncome	-.005(.003)	-.006(.003) [†]	-.027(.011)*	-.012(.011)
Interactions w/education				
ECEC	.071(.016)***	.062(.016)***	.446(.124)***	.455(.125)***
Education	.083(.002)***	.079(.002)***	.099(.007)***	.093(.007)***
ECECxEducation	-.008(.002)***	-.009(.002)***	-.048(.009)***	-.045(.010)***

Note: Covariates in all models are: Child gender, APGAR 5 min, low birth weight, twin births, highest family education, family income, maternal age, number of children and adults, western- and non-western immigrant status, maternal anxiety and depression, maternal relationship satisfaction, birth month, cohort indicators.

[†] $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$

Turning to the interaction effects, family income did not significantly moderate ECEC estimates for math, while it did for reading (but only significant at the 10% level). On the other hand, the estimated effects of ECEC on both math and reading were moderated by parent education: test scores were more strongly and positively associated with ECEC use for children from homes with lower levels of education. Among children of parents with 9 years of education, those attending ECEC scored 7.1% and 6.2% of a standard deviation higher in math and reading than those not attending. For each additional year of parental education, this effect decreased by .08% of a standard deviation for math, and by .09% of a standard deviation for reading.

The treatment effect heterogeneity by parental education highlights universal ECEC's potential for closing SES gaps. To illustrate, consider the marginal effects of ECEC by varying levels of parental education (holding all other covariates at their mean values), which can be seen in Figure 3, with the horizontal lines representing 20% of a standard deviation (i.e., the math standard deviation is 9.5). For example, ECEC reduces the expected test score gap between children of parents with 9 versus 18+ years of education by approximately 10%.

Figure 3. Test Scores for Math for Children Attending and not Attending ECEC by Levels of Parental Education, Results from Fixed Effects Regression Analyses. The Y-axis is converted to scaled test scores.

However, when we tested how sensitive our fixed effects regression analyses were to unobserved selection bias – following the strategy outlined by Dearing and Zachrisson (2019) and Oster (2019) – they appeared fragile and potentially altered by even weak omitted

variable bias. Specifically, in examining how much unobserved selection bias it would take to nullify our findings, we find that unobserved confounders as little as 6% to 12% as powerful as our observed covariates could nullify our findings in these models. This sensitivity to potential omitted variables in the FE models underscores the internal validity advantages of our IV estimates, discussed next.

Instrumental variable regressions. The right-hand panels in Table 2 report results from our IV regressions. These IV models also included the full set of covariates listed in Table 1. For the main effects of ECEC, the effect sizes were considerably larger in the FE analyses (with treatment effects of 23% and 26% of a standard deviation for math and reading, respectively), compared to the FE regressions, yet the standard errors were also considerably larger, yielding statistically significant main effects only at a 10% level. To illustrate, the 95% confidence intervals range from reducing test scores by 3.5% (math) and 1.5% (reading) to increasing test scores by 47.5% (math) and 49.5% (reading).

With regard to interactions in the IV models, we found evidence of differential effects of ECEC by family income for math but not reading. Children of parents with the lowest observed income level scored 33.6% of a standard deviation higher in math if they attended ECEC compared to those who did not. For each additional NOK 100,000 (about USD 12,000) earned by the parents, this effect decreased by 2.7% of a standard deviation.

In addition, we found differential effects of ECEC use across levels of family education on both math and reading. These were largest for children of parents with 9 years of education, children who entered ECEC by 18 months had math scores 45% of a standard deviation higher and reading scores 46% of a standard deviation higher than those who were not in ECEC by this age ($p < .001$ in both cases). For each additional year of parental education, the ECEC effect decreased by about 5% of a standard deviation. The marginal effects of ECEC attendance on scaled test scores at varying levels of parental education can

be seen in Figure 4 (again with mean levels of all other covariates), with the horizontal lines representing 20% of a standard deviation. Considering the point estimates, the differences in test scores between children of parents with nine versus 18+ years of education is about 50% smaller for children attending ECEC. The figure also shows that the marginal effects are not statistically significant at a 5%-level for children of parents with 13 years of education and above. When including interactions of ECEC with both income and education in one model, the interaction with income was no longer significant, while the interaction with education remained unchanged (not shown).

Figure 4. Test Scores for Math for Children Attending and not Attending ECEC by Levels of Parental Education, Results from Instrumental Variable Analyses. The Y-axis is converted to scaled test scores.

Robustness checks. We conducted several robustness checks for these analyses. For both fixed effects regressions and instrumental variable analyses, we found the results to be substantively identical when: a) we tested the interaction with the log of income (mean centered), to probe whether the association with ECEC use was stronger at the lower end of the income distribution; b) using complete data only (i.e., listwise deletion), c) restricting analyses to first-born children only (approximately 45,000 children), d) using the scaled test scores for math and reading, available for the 2004 through 2008 cohorts. All robustness checks are detailed in Appendix 3, Table S5.

Discussion

In this study, we found positive effects of universal ECEC starting in toddlerhood on math and reading skills in 5th grade. The effects were largest among children from disadvantaged backgrounds, particularly for children whose parents had low levels of education. To our knowledge, this is the first large-scale study examining whether universal ECEC beginning in toddlerhood has long-term effects, documenting persistent effects on achievement well into school age for socially disadvantaged children.

Our analyses are based on a unique combination of survey and administrative data, covering a time period with a sharp increase in the availability of ECEC for toddlers. This allows us to take two different analytical approaches to estimating ECEC effects, FE regression models (i.e., comparing children within regions and birth cohorts) and IV regressions. Although we prefer our IV model results for internal validity reasons, the results from our two analytical approaches are consistent in several respects. Attending ECEC positively affects math and reading achievement in 5th grade, and most strongly so for children from parents with the least education. The positive effects of ECEC seem to be concentrated among children of parents with 12 years of education or less in both models. These convergencies certainly strengthen our confidence in our main conclusions about the

compensatory effects of ECEC for toddlers on achievement gaps at age 10. Moreover, the consistency in direction of both main effects and interactions supports our confidence in the validity of our instrument. While the main conclusion about direction and specificity of effects for children of parents with low education are consistent across these two approaches, the estimated effect sizes differ considerably, and also require distinct substantive interpretations. In the following, we discuss the results of each of them, before juxtaposing the results.

Our FE regression models estimated the total effect on test scores in 5th grade of being in ECEC at age 1½ years, conditional on a rich set of child- and family covariates. The municipality- and cohort fixed-effects restricted the comparisons to be children born in the same year, living in the same municipality. We found an average treatment effect (ATE) on math of 2.6% of a standard deviation, and a smaller effect on reading scores of 1.5%, which were only statistically significant at a 10%-level. For children of parents with 9 years of education, the effect was 7.1% of a standard deviation on math, and 6.2% for reading (both statistically significant at $p < .001$). The effects decreased by about 1% of a standard deviation for each additional year of parental education. In effect, the estimates from our FE regression models indicate that ECEC attendance reduced the gap in, for example, math achievement between children of the most- and least-educated parents by about 10%. The strength of the FE regression models is that they are based on an efficient estimator, exploiting all variability in ECEC in our data. The major limitation is the risk of omitted variable bias, underscored by our sensitivity analyses showing that these estimates were fragile, susceptible to being altered by fairly weak potential omitted variables; hence, we prefer estimates from the IV models.

While IV estimates were less efficient and restricted only to the variability in ECEC attendance caused by the municipality-by-year coverage changes, our IV approach provided a more rigorous causal test. These models provided consistent evidence that ECEC beginning in

toddlerhood led to substantive improvements in achievement, especially for children whose parents had low levels of education. Notably, the effect sizes were considerably larger for our IV models (as were the standard errors) than for our FE regression models. In these models, we estimated the effect of ECEC on children who enter as a function of availability of care (LATE), within cohort and municipality. The main effects for math and reading were 23% and 26% of a standard deviation, respectively, and only statistically significant at a 10%-level. Yet, for children of the least educated parents, the LATE was about 45% of a standard deviation for both math and reading ($p < .001$), with an estimated reduction of effects of about 4.5% of a standard deviation for each additional year of parental education.

The heterogeneous treatment effects suggest that publicly funded universal ECEC has great potential for reducing socioeconomic achievement gaps, perhaps by as much as 50% as indicated by the interaction effects for parental education in our IV models. Even if we more conservatively estimate reduction of achievement gaps by comparing the extremes of our 95% confidence intervals, these effects were of considerable practical importance. For example, we can compare the upper bound of the 95% CI of the estimate for children of low-educated parents not attending ECEC with the lower bound for children of high-educated parents (also not attending ECEC), and then use the lower bound of the 95% CI for children attending ECEC as an estimate of the treatment effect. Using these points of comparison, attending ECEC was estimated to close the gap between children of high- and low-educated parents by approximately 34%.

How do these effect sizes compare to previous studies? As mentioned, we are not aware of directly comparable studies of school-age outcomes of universal ECEC for toddlers. From the preschool literature, we know that immediate effects tend to decrease during the first few years in school (e.g., Barnett, 2011). With this in mind, the effect sizes from the instrumental variable regressions are in line most other studies. With a similar design to ours,

Felfe et al (2018) found effect sizes of 20%-30% of a standard deviation at age 6, and only for certain sub-groups, which is smaller than what we found for 5th graders. In Norway, Dearing and colleagues (2018) found effect sizes as large as 89% of a standard deviation on language skills at age 3 for low-income children attending ECEC at age 1½. The positive effects are larger than those reported by Drange and Havnes (2019), who found effect sizes of about 15% to 25% of a standard deviation in first grade, as a function of starting ECEC at age 1 versus age 1½ for children of low educated parents in their lottery design. Both of these latter findings from Norway may underestimate the true effect, as their outcome measures were screening tests primarily designed for detecting low-performing children. In contrast, we use standardized national tests covering the full range of abilities. Moreover, Drange and Havnes (2019) evaluate treatment effects of a very short intervention period, although their findings may suggest that our results could have been even larger if we were able to specifically test ECEC use for even younger children.

Our results in 5th grade must be seen in relation to these studies at earlier ages with potential fadeout in mind. Barnett (2011) summarized meta-analytic evidence from high-quality trials of preschool programs. Measured at the end of the programs, effect sizes for cognitive outcomes were about 70% of a standard deviation. At age 5-10, this had decreased to 35%, with a further decrease at ages 10 and older to a little less than 30% of a standard deviation. As these were primarily of programs targeted at low-income children, they may be most comparable to those effect sizes we found of about 45% for children of parents with low educated parents (while the 30% of a standard deviation effect size is well within the 95% CI of our estimate). This reduction of effect sizes with about 50% into middle school age is consistent with what we observe when comparing those reported by Dearing et al. (2018) at age 3 and the results we report. This is obviously a very imprecise comparison, as the constructs (language skills vs. math and reading) and measurement (maternally reported

questionnaires vs. tests) are very different. Nevertheless, the effect sizes from our instrumental variable models seem to be—by-and-large—not very different from those often reported in other studies with strong designs for causal inference, yet from very different contexts and, in many cases, from interventions including older children.

Somewhat surprisingly, we found the effects of ECEC to be more consistently differentiated as a function of parental education than family income. And, when both income and education were included in the same models, the effect of income disappeared, while the effect of education was unchanged. Moderation of ECEC effects by education is in itself reported in several other studies (e.g., Drange & Havnes, 2019), while moderation by parental income was tested in other studies, for example by Dearing et al. (2018), using the same sample. This leads us, however, to speculate about the type of socioeconomic disadvantage for which the consequences are buffered by ECEC, as well as the sociopolitical context of this particular study.

Parental income and education are correlated, but to a varying degree across sociopolitical contexts. For example, from our own calculations, the two are correlated about .70 in the NICHD SECCYD, but only about .40 in Norwegian registry data. Parental income and education are supplementary indicators of SES (Duncan & Magnuson, 2003), and may affect the home learning environment for children differently (Duncan, Magnuson, & Votruba-Drzal, 2015). Parental income may affect developmental outcomes through investment- and stress pathways (e.g., Longo, McPherran Lombardi, & Dearing, 2017; Yoshikawa, Aber, Bergman, & Beardslee, 2012), while the mechanisms through which parental education affects developmental outcomes may be more specifically through home learning environment, educational aspirations, and conversational styles (e.g., Magnuson, 2007). From our results, it seems to be the case that ECEC in Norway compensates for disadvantages stemming from lower parental education rather than income. The exact

developmental processes for which ECEC compensates (at least in Norway) are yet to be determined.

Limitations

Despite several strengths, including large and nation-wide survey data linked with administrative records on both parental income and education, and test scores, our study has limitations that must be taken into consideration. The first of these concerns external validity. MoBa includes a restricted sample, with underrepresentation of the least educated parents, and those with the lowest incomes, as well as children with immigrant background. While the associations between the socioeconomic indicators and test scores are very similar among MoBa participants and the relevant population, we cannot rule out the possibility that there is a differential selection into MoBa of parents with children most likely to benefit from the expansion. If the low-educated or low-income parents participating in MoBa are, in some way, more resourceful and take better advantage of access to ECEC, this can be the case. On the other hand, our estimates may be a lower bound of the true effect of the scale up if children of the least resourceful parents are, in fact, those who benefit the most from early ECEC attendance.

Moreover, our results are necessarily linked to a certain, and in one sense peculiar, context of a quite rapid expansion of ECEC for infants and toddlers in a well resourced and quite egalitarian country. There was a concern in Norway, at the time, that this expansion came at the expense of quality—that an increasingly larger number of toddlers put pressure on structural resources in the ECEC system, like teachers and space, ultimately resulting in lower process quality (Gulbrandsen & Eliassen, 2012). If this is the case, it may be reasonable to consider our effect sizes as a lower bound, and that ECEC in Norway will have seen quality improvements as more teachers are trained, and the ECEC system at large consolidates to its

larger coverage. We do, however, not have any evidence on whether this actually has happened.

Implications

In this study, we demonstrate that universal ECEC starting in toddlerhood can contribute considerably to closing the achievement gap in 5th grade between children from high- and low SES backgrounds. Our findings provide supporting evidence for what is often considered to be one of the primary arguments favoring ECEC programs for preschoolers (both targeted and universal)—closing of achievement gaps—and expand this also to include programs starting at earlier ages. Our IV estimates identified the effects of scale-up and the corresponding reduction in socioeconomic gaps for the children induced to ECEC because of the scale-up. This treatment effect is relevant for countries like the US, where there are discussions about increasing ECEC coverage. If ECEC treatment effects are heterogeneous, which all of our results suggest, then such expansion may substantially reduce gaps for those induced into ECEC by the expansion. In contrast, the fixed-effects regression analysis (assuming that they are robust to omitted variable biased) shows the effects of ECEC for the average child, irrespective of the scale-up. Also these results indicate that ECEC could lower socioeconomic gaps. In sum, our findings support universal ECEC from an early age as a means to ensure that children from different socioeconomic background have more equal opportunities.

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APPENDICES

Appendix 1. Distribution of family income and education in MoBa and in the population
(parents with children born in 2002-2008)

Figure S1. The distribution of family income in MoBa and in the general population

Table S1. Years of Education in MoBa and in the general population

	MoBa (%) (n=96,733)	Population (%) (n=411,427)
<9	0.02	.02
9	6.65	11.24
11	1.95	3.12
12	31.21	34.92
13	3.69	3.66
16	43.03	35.31
18	12.96	10.83
23	0.48	.62

Table S2. Test scores in Math and Reading, regressed on income and education, and covariates, in MoBa and in the population.

	MoBa (n=91,720)		Population (n=330,559)	
	Math	Reading	Math	Reading
	Est.(se.)	Est.(se.)	Est.(se.)	Est.(se.)
Education	.071(.001)	.073(.001)	.075(.001)	.077(.001)
Income	.040(.001)	.017(.002)	.041(.001)	.020(.001)

Note: Within-municipality and cohort fixed-effects regressions, Conditional on number of adults and children in the family, Immigrant background, and birth months.

Appendix 2. Instrument validation

Table S3. Changes in ECEC coverage predicted by municipality Characteristics in 2002

	Est (se)	Est (se)
ECEC coverage (%)	-.85(.03)***	-.85(.04)***
Municipality level covariates (share)		
Low family education		-4.33(6.59)
High family education		1.21(13.01)
Western immigrant		2.93(10.09)
Non-western immigrant		5.56(12.17)
Unemployment benefits		-6.26(5.36)
Low Income-to-needs		12.66(9.61)
High Income-to-needs		16.65(9.61)
Small number of children in municipality		5.57(.00)
Medium number of children in municipality		4.96(7.53)
Fraction of children in MoBa		3.48(2.91)
Constant	70.47(1.23)***	62.76(8.14)***
<i>p</i> -value for joint significance tests of other covariates (excluding coverage rate)		.34

* $p < .05$, ** $p < .01$, *** $p < .001$

Table S4. Covariate balance, municipality (n=432) and family level (n=102,251) covariates predicting ECEC coverage

Municipality covariates	
	Est (se)
Low family education	7.37(2.59)
High family education	.05(4.95)
Western immigrant	.58(4.43)
Non-western immigrant	5.83(4.43)
Unemployment benefits	-3.54(2.13)
Low Income-to-needs	-4.44(2.42)
High Income-to-needs	.66(3.62)
Small number of children in municipality	-9.88(7.82)
Med. number of children in municipality	-6.74(7.41)
Fraction of children in MoBa	6.15(1.60)***
Family level covariates	
Family education (highest)	-.01(.01)
Income	-.01(.01)
Non-western immigrant	-.01(.08)
Western immigrant	.05(.06)
Maternal age	-.01(.00)*
Maternal anxiety/depression	.08(.06)
Relationship satisfaction	-.02(.03)
Number of children	.05(.03)
Number of adults	.09(.06)
Boy	.00(.03)
APGAR 5 min	-.01(.02)
Low birth weight	.12(.09)
Twin birth	-.05(.10)

Appendix 3. Robustness checks

Table S5. Robustness checks

a) Interactions with log of income				
	Two-Way Fixed-Effects		Instrumental Variable	
	Math	Reading	Math	Reading
ECEC	.063(.032) [†]	.073(.032)*	1.342(.904)	1.596(.909) [†]
Income	.136(.028)***	.057(.028)*	.58(.240)*	.527(.241)*
ECECxIncome(Log)	-.023(.018)	-.035(.018)*	-.734(.504)	-.882(.506) [†]
b) Listwise deletion				
	Two-Way Fixed-Effects		Instrumental Variable	
	Math	Reading	Math	Reading
	Est. (se.)	Est. (se.)	Est. (se.)	Est. (se.)
ECEC Main Effect	.024(.008)**	.014(.008) [†]	.204(.205)	.295(.202)
Interactions w/income				
ECEC	.053(.019)**	.045(.019)*	.367(.185)*	.353(.182) [†]
Income	.040(.003)***	.019(.003)***	.058(.012)***	.019(.012)
ECECxIncome	-.006(.003)	-.006(.003) [†]	-.043(.015)**	-.015(.015)
Interactions w/education				
ECEC	.071(.016)***	.062(.016)***	.563(.164)**	.617(.162)***
Education	.083(.002)***	.079(.002)***	.113(.011)***	.102(.011)***
ECECxEducation	-.008(.002)***	-.009(.002)***	-.072(.014)***	-.063(.014)***
c) Only first borns				
	Two-Way Fixed-Effects		Instrumental Variable	
	Math	Reading	Math	Reading
	Est. (se.)	Est. (se.)	Est. (se.)	Est. (se.)
ECEC Main Effect	.037(.012)**	.022(.012) [†]	.233(.136) [†]	.257(.137) [†]
Interactions w/income				
ECEC	.084(.024)**	.058(.025)*	.341(.175) [†]	.280(.175)
Income	.045(.004)***	.020(.004)***	.054(.004)***	.026(.011)*
ECECxIncome	-.011(.005)*	-.008(.004) [†]	-.031(.017) [†]	-.022(.016)
Interactions w/education				
ECEC	.076(.024)**	.053(.025)*	.451(.162)**	.399(.160)*
Education	.078(.003)***	.072(.003)***	.101(.010)***	.094(.010)***
ECECxEducation	-.008(.004)*	-.006(.004)	-.057(.014)***	-.051(.014)***
d) Scaled scores (2004-2008 cohorts)				
	Two-Way Fixed-Effects		Instrumental Variable	
	Math	Reading	Math	Reading
	Est. (se.)	Est. (se.)	Est. (se.)	Est. (se.)
ECEC Main Effect	.227(.097)*	.129(.092)	1.808(1.864)	3.714(1.938) [†]
Interactions w/income				
ECEC	.287(.210)	.422(.218) [†]	2.613 (1.792)	4.114(.131)*
Income	.416(.029)***	.228(.029)***	.498(.113)***	.175(.116)
ECECxIncome	-.012(.036)	-.058(.035)	-.196(.148)	-.010(.051)
Interactions w/education				
ECEC	.631(.169)**	.578(.168)**	4.030(1.637)*	5.452(1.709)***
Education	.793(.022)***	.767(.021)***	1.037(.106)***	.897(.106)***
ECECxEducation	-.078(.026)**	-.087(.026)**	-.550(.136)***	-.429(.136)**

Note: Covariates in all models are: Child gender, APGAR 5 min, low birthweight, twin births, highest family education, family income, maternal age, number of children and adults, western- and non-western immigrant status, maternal anxiety and depression, maternal relationship satisfaction, birth month, cohort indicators.
† $p < .1$, * $p < .05$, ** $p < .01$, *** $p < .001$