What is the Added Value of Preschool? Long-Term Impacts and Interactions with an Infant Health Intervention*

Maya Rossin-Slater†

Miriam Wüst‡

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Abstract

We study the impact of targeted high quality preschool over the life cycle and across generations, and examine its interaction with an infant health intervention. Using administrative data from Denmark together with variation in the timing of program implementation between 1933 and 1960, we find lasting benefits of access to high quality preschool at age 3 on adult educational attainment, earnings, and survival beyond age 65. Further, the benefits persist to the next generation, who obtain more education by age 25. However, exposure to a nurse home visiting program in infancy reduces the added value of preschool by 88 percent for years of schooling and by 80 percent for survival past age 65. Our findings suggest that high quality preschool can compensate for early life health disadvantage.

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†University of California at Santa Barbara, Department of Economics; NBER; IZA. E-mail: maya.rossin-slater@ucsb.edu.

‡The Danish National Centre for Social Research (SFI). E-mail: miw@sfi.dk.

1 Introduction

A growing body of evidence traces the origins of adult well-being to individuals' early life circumstances (Almond *et al.*, 2017; Almond and Currie, 2011; Barker, 1990). This evidence, combined with the fact that the majority of parents in developed countries participate in the labor force when their children are young, has prompted fervent discussions among both researchers and policymakers on the importance of high quality preschools, especially for low-income children.

This paper contributes to the debate on publicly funded preschools—and early interventions more broadly—in three ways. First, we use administrative population-level data on nearly 1 million individuals to deliver estimates of the long-term effects on individuals' human capital and health outcomes through age 65 of a large high quality preschool program that targeted poor children in early and mid-20th century Denmark. Unlike other studies that have examined more recent universal programs in Scandinavia attended by children from *all* socio-economic backgrounds (Havnes and Mogstad, 2011, 2015; Bingley *et al.*, 2015), our analysis may be especially relevant to the current policy landscape in the United States, where discussions have mostly centered around *targeted* rather than universal preschools.[1] Despite important differences between modern United States and Denmark in the first half of the 20th century, the two settings are arguably similar in their relative dearth of public programs for working parents and young children (e.g., no paid maternity leave and no universal health insurance), especially when compared to current policies in Scandinavia.[2] Our paper builds on prior studies of the long-term effects of targeted programs in the U.S.—including Head Start, the Perry Preschool program, and the Abecedarian Project—which have documented benefits for individuals into their 30s using data sets with relatively small

[1]For instance, President Obama's "Plan for Early Education" extended federal funds to provide all four-year-olds from U.S. families with incomes at or below 200 percent of the poverty line with a high quality public preschool (see: https://www.whitehouse.gov/the-press-office/2013/02/13/fact-sheet-president-obama-s-plan-early-education-all-americans). Additionally, even state-level "universal pre-kindergarten (pre-K)" programs are often not universal in practice, as budget constraints require targeting to limit enrollment (for information on the share of four-year-olds enrolled in public preschool by state, see table 1 in https://www2.ed.gov/documents/early-learning/matter-equity-preschool-america.pdf).

[2]The period that we study is prior to the large-scale expansion of the Danish social safety net in the 1960s and 70s. For instance, Denmark introduced universal paid maternity leave of 14 weeks in 1960 (DICE Database, 2015) and universal health insurance in 1973 (Vallgårda *et al.*, 2001).

sample sizes of less than 3,500 (and, in many cases, less than 150) individuals (Garces *et al.*, 2002; Ludwig and Miller, 2007; Deming, 2009; Carneiro and Ginja, 2012; Campbell *et al.*, 2014; Schweinhart *et al.*, 2005; Heckman *et al.*, 2010a,b).[3]

Second, we provide some of the first evidence on the *intergenerational* impacts of preschool on the educational attainment of children of women who had access to high quality preschool in early childhood. While there is a vast literature documenting intergenerational correlations in measures of socio-economic status such as income, education, and IQ in both the U.S. and Europe (e.g.: Solon, 1992; Bauer and Riphahn, 2004; Lee and Solon, 2009; Black *et al.*, 2009; Chetty *et al.*, 2014), less is known about the impacts of an intervention that causally increases educational attainment in one generation on the education of the next generation.[4]

Third, we examine the *added value* of preschool for a population that received an earlier health intervention in infancy. This analysis is informative because different early childhood programs often have similar eligibility criteria, and children can be exposed to more than one intervention in the early life period.[5] Knowledge about the signs and magnitudes of the potential interaction effects between programs is imperative for efficient policy design (Kline and Walters, 2016; Robling *et al.*, 2016; Olds, 2016), as well as for understanding the determinants of human capital production in a framework with different stages of early childhood (Cunha and Heckman, 2007; Heckman and Masterov, 2007; Heckman and Mosso, 2014). As noted by Almond and Mazumder (2013), the analysis of interactions between multiple interventions in an observational setting requires "lightning to strike twice"; one needs two independent, quasi-exogenous interventions affecting the same children at adjacent developmental stages. We argue that we have found one such exceptional setting—in Denmark, high quality preschool access was gradually expanded around the same time as a

[3]See Section 2 for more details on the sample sizes in these studies.

[4]We are aware of one other concurrent paper that is examining the intergenerational effects of Head Start using samples from the National Longitudinal Survey of Youth (NLSY): Barr and Gibbs (2016) use a sibling fixed effects design as well as some variation in Head Start funding across counties, finding that children of mothers with Head Start exposure have higher educational attainment and lower incidence of risky behaviors. However, small sample sizes limit the statistical power in this analysis. Related, Black *et al.* (2013) use Norwegian data to show that children of individuals who were exposed to radiation *in utero* have lower IQ scores.

[5]For an overview of current early childhood programs in the U.S., see Currie and Rossin-Slater, 2015.

nurse home visiting (NHV) program for new mothers and infants was introduced. Importantly, some municipalities implemented the NHV program before the preschool program, while others implemented the preschool program before the NHV program, and the timing of each program's rollout was independent of the other.

We merge unique historical program data to individual-level administrative data on the population of Danish individuals born in 1930-1957. We exploit policy variation in the expansion of government-approved and regulated preschools across 140 Danish municipalities that established such a preschool by 1960.[6] These preschools offered a high quality early learning environment (relative to the typical informal care arrangement during the time), nutritious meals, and access to health check-ups to disadvantaged children for four years before the start of primary school at age 7. We find that, relative to cohorts without access to a government-approved preschool in early life, cohorts born in municipalities with such a preschool by age 3 have 0.07 more years of schooling (0.6 percent at the sample mean) and are 1.3 percentage points more likely to have more than nine years of compulsory education (1.9 percent at the sample mean). We also find that access to high quality preschool leads to a 0.5 percentage point increase in the likelihood of surviving beyond age 65 (0.6 percent at the sample mean), and may result in a 1.6 percent increase in average wage income over ages 30 to 60 (although the result on income is not as robust as the other estimates). Further, we document that the benefits of high quality preschool extend to the next generation—children of women with access to a government-approved preschool by age 3 are 1.2 percentage points more likely to have more than a compulsory education by age 25 (1.5 percent at the sample mean).

Finally, we study whether the added value of preschool varies with earlier exposure to NHV.[7] We find statistically significant *negative* interaction effects between exposure to NHV

[6] To receive government approval, a preschool must follow strict quality regulations mandated by the government. See Section 2 for more details. Individuals born in the 140 ever-implementing municipalities account for approximately 53 percent of the Danish population born during this time period.

[7] As prior work has comprehensively analyzed the long-run effects of the NHV program, we do not focus on them here. Hjort *et al.* (forthcoming) show that NHV decreased mortality at ages 45 to 64, as well as the probability of being diagnosed with a cardiovascular disease and the average number of nights spent in the hospital during the same age range. We have also estimated the main effects of NHV using the sample and specifications from the current paper, finding similar results on diagnoses and hospitalizations. We discuss them briefly in Section 5.

and preschool, suggesting that the marginal benefit of preschool is lower for children who had already been exposed to NHV as infants. Having access to NHV reduces the positive impact of preschool by 88 percent for years of schooling and by 80 percent for survival beyond age 65. We find suggestive evidence that the impacts on wage income of the first generation and the education of the second generation are reduced as well.

Our results on the interaction between preschool and NHV are consistent with other studies documenting that the impacts of preschool are largest for the *least* advantaged children (Bitler *et al.*, 2014; Havnes and Mogstad, 2011, 2015; Cascio, 2015; Herbst, Forthcoming). However, unlike prior work, we are able to identify heterogeneous returns to preschool across individuals who differ in their early life environments due to a more exogenous, program-driven source of variation. Our estimates imply that high quality preschool can compensate for initial disadvantages in health.

Our findings also relate to a few recent studies pointing out that program treatment effects may be under-estimated when close substitutes exist (Kline and Walters, 2016; Robling *et al.*, 2016; Olds, 2016). In our context, the NHV program provided parents of infants with education about nutrition, parent-child interactions, and the overall home environment, and facilitated referrals to health care professionals. Thus, the preschool environment—which incorporated some similar elements but at older ages—may constitute a less significant treatment for cohorts already exposed to NHV.

Our analysis is further relevant to economic models of human capital formation, which posit dynamic complementarities between multiple investments at different stages of childhood and across different parameters of the child production function (Cunha and Heckman, 2007; Heckman and Masterov, 2007; Heckman and Mosso, 2014). Since we do not observe important parameters of the human capital production function in our data (e.g., parental investments and initial conditions), we do not view our results as arguing against these models. The large negative interaction effects between NHV and preschool exposure are consistent with two other recent papers studying interactions across different types of early life investments in very different settings (Bangladesh (Gunnsteinsson *et al.*, 2014) and Mexico (Adhvaryu *et al.*, 2015)). But we note that substitutability in health-related investments in early childhood does not rule out the possibility of dynamic complementarities at a later stage (e.g., educational investments during school age, as in Johnson and Jackson, 2017 and Gilraine, 2017).

The rest of the paper proceeds as follows. Section 2 provides relevant background and reviews the existing literature. Section 3 describes our data sources and sample, while Section 4 discusses our empirical methods. Section 5 presents our main results and robustness tests, and provides a discussion of the magnitudes and the possible mechanisms underlying the effects we find, while Section 6 concludes.

2 Background and Related Literature

2.1 The Danish Preschool Expansion

The Danish preschool system dates back to the 19th century, when philanthropic organizations operated preschools to serve children from poor families, whose mothers had to work (Pedersen *et al.*, 2011a). These preschools were not regulated and exhibited large heterogeneity in quality. Preschool quality improved and became more uniform as a result of a series of laws passed between 1919 and 1951 regarding government approval and financial support of all existing and new preschools (see Skjernbæk, various years).

In 1919 the government began offering subsidies to preschools that could be used to cover ongoing expenses (e.g., staff wages or rent) or to establish, improve, or expand existing centers (Skjernbæk, various years; Pedersen *et al.*, 2011b).[8] To receive a subsidy, a new or existing preschool had to first obtain government approval. In addition to providing adequate hygienic facilities, approved preschools had to satisfy four main requirements: (1) have a preschool head, staff, and a board of members with expertise on children; (2) be open for at least four hours each working day; (3) provide services exclusively or predominantly to children from poor families; and (4) charge fees that could cover food and milk provided to children (very poor parents could apply for an exemption).[9]

[8]Subsidies ranged between 30 and 50 percent for expenses related to daily operations, and were around 50 percent for expenses related to the establishment or improvement/expansion of existing institutions. Both the national and municipal governments were involved in the financing of preschools. If a municipality ran a preschool or subsidized at least 30 percent of its expenses, the subsidy from the national government was around 40 percent in the 1940s and 1950s.

[9]A preschool could either be run by a municipality (which employed staff with expertise on children) or

Further, regulations regarding the educational requirements for preschool staff, their wages, and the child-to-teacher ratio were put in place in the early 1930s.[10] Preschool staff were trained to teach pedagogical content inspired by the principles of influential educators such as Friedrich Fröbel and Maria Montessori, with an emphasis on providing a stimulating environment for children. The government also incentivized preschools to work together with local physicians and dentists to monitor children's health, reimbursing expenses related to these check-ups. The link between preschools and local health care providers facilitated access to vaccinations.[11]

In sum, government-regulated preschools provided poor Danish children aged 3-6 with a higher quality of care, early education, nutrition, and health services than they would have otherwise received. During this time period, poor mothers who had to work would typically leave small children alone at home, under the supervision of older siblings, or in the care of other relatives or neighbors (Pedersen *et al.*, 2011a, p728).[12]

As we show below, there was substantial variation across Danish municipalities in the timing of the first government approval of a preschool. We exploit this variation in our main analysis and discuss the identifying assumptions in detail in Sections 4 and 5.

2.2 Nurse Home Visiting in Denmark

In 1937, the Danish parliament passed a bill that regulated the content and funding of a NHV program serving all families with newborns. The Danish National Board of Health (DNBH) had developed the program to address the relatively high infant mortality rate of around

be run by a private organization with a board of members with expertise on children (e.g., a pediatrician, a teacher, etc.). The vast majority of preschools were run by private non-profit organizations (Pedersen *et al.*, 2011b). Preschools were allowed to be closed for up to four weeks during the summer and a total of two weeks around holidays such as Christmas or Easter. Finally, the requirement for serving children from poor families was loosened over time—beginning in the late 1940s, preschools that did not predominantly serve poor families could also receive smaller subsidies from the national and municipal governments (for a total of around 35 percent of all costs) (Skjernbæk, various years).

[10] Trade unions that focused on pedagogical work—and lobbied for adequate educational programs and higher wages for the preschool staff—were established as well.

[11] In the 1930-1960 period, vaccines against the following diseases were available to all children through the national vaccination program: smallpox (mandatory since 1931), diphteria (1943), tuberculosis (1946), tetanus (1949), and polio (1955). The vaccination program was expanded by the Danish National Board of Health and the Serum Institute. See `http://www.ssi.dk/Vaccination/Boernevaccination/Sygdomsforekomst%20foer%20og%20efter%20vaccination.aspx` for more information.

[12] During the first half of the 20th century, the Danish female labor force participation rate was between 30 and 40 percent (Olivetti, 2013).

6.5 percent at the time (DNBH, various years). As a considerable share of infant mortality was due to preventable causes—among them, infectious diseases caused by the improper treatment of cows' milk—the DNBH designed the program to promote breastfeeding and a safe home environment. Highly trained nurses were assigned to visit newborns and their mothers approximately 10 times in the first year of life. Nurses provided information on infant care, monitored infants' development and referred ill infants to doctors for treatment (for more details on the program see DNBH, 1970; Buus, 2001; Wüst, 2012; Hjort *et al.*, forthcoming).

While DNBH centrally designed the program and the Danish government co-funded it, implementation was under municipal discretion. To implement NHV and be eligible for a 50 percent refund of program expenses from the government, municipalities had to find trained nurses and get approval for their implementation plan from the DNBH. Variation in the timing of program implementation across municipalities largely stemmed from the lengthy accreditation process at the DNBH. Another source of variation came from differences in the preferences of local general practitioners, who in some places promoted the initiation of NHV but in other places opposed it as it was undermining their authority (Buus, 2001).

2.3 Related Literature

Preschool programs. Evidence on the long-term impacts of preschool in Scandinavia on outcomes through age 30 comes from more recent expansions in universal programs offered to children from all socio-economic groups. Havnes and Mogstad (2011) study an expansion of universal preschool in Norway in the 1970s and find positive impacts on educational attainment and labor market participation. In Denmark, Bingley *et al.* (2015) use variation in preschool openings from the same time period to instrument for maternal employment, and find large positive impacts on children's schooling and adult earnings. Examining even more recent cohorts, Datta Gupta and Simonsen (2016) find positive medium-run effects of universal preschools in Denmark on children's test scores in ninth grade.[13]

[13] The evidence on the shorter-term effects of universal preschools comes from a wider set of countries and offers mixed results. In the U.S., studies of state-level universal pre-kindergarten programs show improvements in cognitive test scores at least for some groups (Gormley and Gayer, 2005; Fitzpatrick, 2008; Cascio and Schanzenbach, 2013; Weiland and Yoshikawa, 2013); similar evidence has been found in Argentina (Berlinski *et al.*, 2009). By contrast, a universal program in Quebec, Canada has been shown to have no

However, evidence from recent decades in Scandinavia may not be applicable to current debates about public preschools in countries without expansive social safety net supports for working parents and young children, such as the United States. Moreover, as pointed out in an overview of the literature by Cascio (2015) and documented in a recent study on the long-term impacts of a universal childcare program during World War II in the U.S. (Herbst, Forthcoming), the benefits of universal programs are consistently larger for disadvantaged children, while children from higher income families often experience zero or even negative consequences. These findings raise the question of whether programs explicitly targeting poor children have larger returns than universal ones.[14]

The literature on targeted preschool programs has focused on the U.S., using data sets with relatively small sample sizes.[15] Evaluations of Head Start—the largest U.S. federal program offering preschool education to low-income children—find positive short-term impacts on test scores (Office of Planning, Research, and Evaluation, 2010; Bitler *et al.*, 2014), which may dissipate by the end of first grade (Office of Planning, Research, and Evaluation, 2010). Yet despite the possible test score "fade-out" in Head Start, existing research documents positive medium-run impacts on young adult outcomes: Children who attended Head Start are less likely to be placed in special education or retained in a grade, are more likely to graduate high school and attend college, have higher earnings in their 20s, and are less likely to be booked or charged with a crime than their non-Head-Start-exposed siblings (Currie

or even adverse impacts for both cognitive and non-cognitive outcomes (Baker *et al.*, 2008, 2015; Haeck *et al.*, 2015); recent Danish universal preschools have been found to have minimal impacts on children's non-cognitive outcomes as well (Datta Gupta and Simonsen, 2010). Program quality and the availability of alternative options likely account for the differences in estimated impacts (see also Herbst and Tekin, 2010 for evidence of adverse impacts of childcare subsidies on child outcomes).

[14]Recent evidence from the U.S. suggests that state-level universal pre-K programs are more effective at improving reading test scores at age 4 when compared with targeted pre-K programs (Cascio, 2017). However, since the decision to operate a universal or targeted program is not random, unobserved sources of heterogeneity across states could contribute to these disparate effects. The impacts on longer-term outcomes could be different as well. In another related U.S.-based study, Cascio (2009) estimates that the introduction of universal kindergartens into U.S. public schools reduced white children's high school dropout rate, but had no impacts on other outcomes such as employment, college attendance, and earnings. She finds no impacts for black children, possibly due to a crowd-out of participation in other federal programs.

[15]Studies of the long-term effects of Head Start use data from the National Longitudinal Survey of Youth (NLSY) (Currie and Thomas, 1995; Deming, 2009; Carneiro and Ginja, 2012; Barr and Gibbs, 2016), which has at most 3,500 individuals in the estimation sample, and the Panel Study of Income Dynamics (PSID) (Garces *et al.*, 2002), which has under 1,800 individuals in the estimation sample. The Perry Preschool sample size is 123 (Schweinhart *et al.*, 2005; Belfield *et al.*, 2006; Heckman *et al.*, 2010a,b), while the Abecedarian Project sample size is 122 (Anderson, 2008; Masse and Barnett, 2002; Campbell *et al.*, 2014).

and Thomas, 1995; Garces *et al.*, 2002; Deming, 2009). Other evidence suggests that Head Start reduces childhood mortality (Ludwig and Miller, 2007), while ongoing research shows that Head Start may improve educational attainment and reduce risky behaviors in the next generation (Barr and Gibbs, 2016). Smaller-scale intensive targeted preschool interventions such as the Perry Preschool Program and the Abecedarian Project have even larger positive impacts on a variety of medium-run outcomes (Schweinhart *et al.*, 2005; Belfield *et al.*, 2006; Anderson, 2008; Heckman *et al.*, 2010a,b; Masse and Barnett, 2002; Campbell *et al.*, 2014).

Our study builds on this literature to provide the first evidence on the persistence of impacts of access to a high quality targeted preschool on educational, labor market, and health outcomes through age 65 and on the education of the next generation, using administrative population-level data from Denmark.

NHV. Moreover, we examine how the added value of preschool varies with exposure to an earlier health intervention. A separate literature has established mixed effects for home visiting programs. In the U.S., several programs have been evaluated using experimental designs. Results indicate that the success of these programs depends on the level of program intensity (i.e., frequency of visits, curriculum breadth, etc.) and on the professional qualifications of the home visitors (i.e., local community members versus professional trained nurses).[16]

Wüst (2012) and Hjort *et al.* (forthcoming) have studied the short- and long-term impacts of the Danish NHV program that we examine here.[17] Wüst (2012) finds that access to NHV led to a significant increase in infant survival of about 5-8 lives saved per 1000 live births. NHV accounted for about 17-29 percent of the overall decreases in diarrhea-related mortality over this time period in Denmark. These results suggest that the program worked in the intended ways, and the survivors of treated cohorts likely experienced fewer severe illnesses and enjoyed a better home environment and nutrition. Hjort *et al.* (forthcoming) document

[16]For example, see St. Pierre and Layzer (1999) for evidence on the Comprehensive Child Development Program (CCDP); Harding *et al.* (2007) for a review of the literature on the Healthy Families America (HFA) program; Olds (2006) for results on the Nurse Home Visiting Parternship (NHVP) program; and Duncan and Sojourner (2013) for evidence on the Infant Health and Development Program (IHDP).

[17]Two other papers study the long-term effects of similar programs in Sweden and Norway. Bhalotra *et al.* (2015) show that the Swedish program substantially reduced mortality through age 75, while Bütikofer *et al.* (2014) document that the Norwegian program had lasting positive effects on education and adult earnings.

that the positive health effects persist into adulthood—individuals who were exposed to NHV at birth are less likely to die at ages 45-64. Further, they show that treated individuals are less likely to be diagnosed with cardiovascular diseases and are admitted for fewer hospital nights in the same age range. These findings are in line with other research that has documented the long-run benefits of improving early life health and nutrition for reducing later life incidence of cardiovascular diseases (Forsdahl, 1979; Barker, 1990; Bhalotra and Venkataramani, 2012; Hoynes *et al.*, 2016).[18]

Interactions between interventions. Our analysis of the interaction between preschool and NHV can be motivated by three strands of literature. First, as noted above, there is growing evidence that the benefits of preschool (both in universal and targeted programs) are largest for the least advantaged individuals. Using quantile regression methods, Bitler *et al.* (2014) show that the effects of Head Start on test scores are concentrated among students at the bottom of the distribution; Havnes and Mogstad (2015) show that the benefits of Norwegian universal preschool are concentrated in the lower and middle parts of the earnings distribution; while Herbst (Forthcoming) demonstrates that the effects of U.S. universal childcare during World War II on long-term labor market outcomes are driven by those at the bottom of the distribution. Similarly, Havnes and Mogstad (2011) find that the positive effects of Norwegian preschool are greater for children with less educated mothers than for children with higher educated mothers, while Cascio and Schanzenbach (2013) document that universal programs in Georgia and Oklahoma only improve test scores for children from low-income families. Yet since variation in baseline human capital and socio-economic status is not random, it is difficult to disentangle the various unobservable sources of heterogeneity that may contribute to differences in the returns to preschool across children. We address this challenge by examining whether the impact of preschool exposure is different across individuals who differ in their early life health environments as a result of an arguably exogenous, program-driven source of variation.

Second, by studying the interaction between two public programs, we also contribute to a set of studies that consider how treatment effects may vary with the presence of alternative

[18] Hjort *et al.* (forthcoming) also consider the long-run effects on educational and labor market outcomes, finding less consistent and much smaller effects.

options. Kline and Walters (2016) develop a theoretical and empirical framework to account for the fact that about one-third of Head Start non-participants attend other competing preschool programs, and show that doing so substantially increases estimated rates of return. Related, evaluations of home visiting programs find that their impacts can be small or non-existent when these interventions are implemented on top of existing health and social care programs (Robling *et al.*, 2016; Olds, 2016). Given that the Danish preschool program that we study features a significant health component (nutritious meals, health check-ups, referrals to doctors, etc.), one can view it as a possible partial substitute for NHV. Our analysis sheds light on how the treatment effects of preschool change with exposure to an alternative health program earlier in a child's life.

Third, the study of interactions between two interventions at adjacent developmental stages is broadly motivated by a model of human capital formation with dynamic complementarities (see, e.g.: Cunha and Heckman, 2007; Heckman and Masterov, 2007; Cunha *et al.*, 2010; Heckman and Mosso, 2014). A key feature of the model is the idea that human capital and investments in one period raise the productivity of investments in a subsequent period. Yet it is difficult to produce causal empirical evidence of such complementarities; studies that support this hypothesis show that the effects of preschool interventions are larger for those with higher measures of initial endowments (Aizer and Cunha, 2012; Heckman *et al.*, 2013), which is different from other evidence finding larger effects for more disadvantaged groups (discussed above). Our work is related to a small set of studies that instead overlay two sources of plausibly exogenous variation. Several of these studies find either zero or negative interaction effects in early childhood investments across very different settings (Malamud *et al.*, 2015; Gunnsteinsson *et al.*, 2014; Adhvaryu *et al.*, 2015).[19]

[19] Malamud *et al.* (2015) combine variation from an abortion reform and a regression discontinuity design in school quality in Romania to study whether cohorts born after abortion was made legal (who likely experienced higher parental investments) benefited more from school quality than cohorts born before. They estimate negative interactions between the reform and school quality, although not all are statistically significant. Gunnsteinsson *et al.* (2014) study an interaction between a tornado and a randomized vitamin supplementation program in Bangladesh, and show that infants who received vitamin supplementation at birth were protected from the negative effects of exposure to the tornado *in utero* in terms of their morbidity and anthropometric measures at ages 0-6 months. Adhvaryu *et al.* (2015) analyze an interaction between rainfall shocks and conditional cash transfers under the *Progresa* experiment in Mexico, showing that the transfers can mitigate about 80 percent of the adverse effect of rainfall on later educational attainment. By contrast, recent studies on educational investments during school ages find evidence of complementarities between them (Johnson and Jackson, 2017; Gilraine, 2017).

3 Data and Sample

We merge data from several sources. First, we use information on the geographical and administrative structure of Denmark over 1920-1955. Second, we collect data on the establishment and approval of preschools and the implementation of NHV. Third, we compile a set of historical municipality control variables. Fourth, we use administrative individual-level data on adult outcomes for cohorts born in 1930-1957 and their children.

Data on Denmark's historical administrative structure. We use data from the "DigDag" project (Digital Atlas of the Danish Historical and Administrative Geography) to link several historical Danish administrative entities, including parishes and municipalities.[20] As births in Denmark are registered at the parish level, we use the "DigDag" data together with information on individuals' parishes of birth in our long-run outcomes data to merge individuals to their municipalities of birth (and thus to assign individuals' treatment status).

Data on preschools. We have collected information on all approved Danish preschools for children aged 3-6 years that existed over the 1921-1960 period from nine books published in 1921, 1924, 1927, 1936, 1942, 1946, 1950, 1956 and 1960 (Skjernbæk, various years).[21] These data contain information on the preschool's first registered exact address (i.e., we can assign preschools to municipalities), the year of establishment and the year of approval, and the number of children registered in each of the given nine years. As noted above, in our main analysis, we use variation in the timing of government *approval* of preschools.[22]

[20] For more information, please see: `www.digdag.dk`. In the period that we study, Denmark consisted of over 1,300 municipalities that were heterogeneous in their size, population density, and composition. Each municipality contained one or more parishes. The vast majority of rural municipalities only had one parish each. The approximately 86 urban municipalities—also known as "*Købstæder*," or market towns—consisted of multiple parishes.

[21] The majority of preschools served children starting at age 3 and through the year when they turned 7 (i.e., the school starting age). A minority of preschools in our data also accepted younger children.

[22] By studying the impacts of preschool approval rather than establishment, we can estimate the impacts of access to a regulated, high quality preschool (and not just any preschool). In practice, however, the years of establishment and approval are the same for many preschools. We show that our results are similar if we use the year of establishment to assign treatment in Section 5. Additionally, we use the original address of the preschool even though some preschools move. Usually, preschools only moved within the same municipality, e.g., to get more space. The records for the total number of slots per preschool are unfortunately incomplete;

Out of the 1,354 Danish municipalities that existed between 1930 and 1960, 140 had at least one approved preschool by 1960. Figure 1 depicts these municipalities in a map of Denmark (using its 1950 administrative structure). As we show in Table 1, the municipalities without approved preschools are mostly very small and rural; the 140 municipalities with at least one approved preschool had ten times higher average population counts in 1930 than the other municipalities. Thus individuals born in municipalities with at least one approved preschool account for 53 percent of the population we observe in our administrative individual-level data (described below). Table 1 also shows that there are substantial differences between the municipalities with and without approved preschools by 1960 in terms of politics, average income, and industrialization. Therefore, we limit all of our analysis to the relatively homogeneous sample of 140 municipalities that ever had a government-approved preschool by 1960. These 140 municipalities are still fairly small entities, with a median population of 4,606 in 1930.

Figure 2 shows the evolution of preschools in these 140 municipalities.[23] In 1933, only about 20 percent of municipalities in our sample had at least one approved preschool, whereas by 1960, all of them did. Most municipalities only ever have one approved preschool—the median number of preschools per municipality is one, while the 75th percentile is two. Only 18 municipalities in our data ever had more than five approved preschools. Thus, most of our analysis uses variation in the initial preschool approval (changing from zero to one approved preschool).

Data on the NHV program. We use information on the date of NHV program approval from the DNBH in the period 1937-1949 from records stored in the Danish National Archives.[24] We also obtained aggregate data from Skjernbæk (various years), which contain lists of NHV-treated municipalities. For municipalities that did not implement an NHV program by 1949, we assign a (somewhat less precise) treatment date using these lists.[25]

we only have data on the number of enrolled children in each preschool in each of the nine years.

[23] We begin the graph in 1933 as our oldest cohorts are born in 1930 and we measure preschool exposure at age 3.

[24] Program approval date indicates the date starting with which municipalities were eligible for a 50% state refund for program expenses (for further details see Hjort *et al.*, forthcoming).

[25] Out of our 140 analysis municipalities, 28 do not implement an NHV program by 1949. We assign either (i) the year of the previous publication to municipalities that are listed as treated in a given publication or

Approval was only granted to municipalities with sufficient coverage, i.e., if the number of nurses matched the estimated demand (number of infants). Thus, we create an indicator for an approved program being in place.[26]

Appendix Figure 1 depicts the variation in preschool and NHV availability by birth year. Access to preschool is measured at age 3, while access to NHV is measured at birth. For cohorts born in 1930, about 80 percent of municipalities did not provide an approved preschool and NHV was not yet established. As the preschool and NHV programs expanded, the percentage of municipalities with both programs increased from zero for cohorts born in 1936 to 86 percent for cohorts born in 1957 in our sample. But, until 1948, between 20 and 50 percent of municipalities only had preschool and no NHV. In the late 1940s, nearly 10 percent of municipalities only had NHV and no approved preschool. In sum, during our analysis time frame, some cohorts were exposed to neither preschool nor NHV, other cohorts were exposed to either only preschool or only NHV, while still others were exposed to both programs.

Data on municipality-level demographics, live births, and infant deaths. We use municipality-level data on population from the quinquennial censuses. Data on other controls come from the *Statistical Commune Data Archive* (Danish Data Archive), and contain information on municipal characteristics such as the share of left-wing voters at several national and local elections, the share of females, the share of workers in the industrial sector, and the share of property tax payers. As we only have control variables for a subset of our sample years (election and census years), we interpolate these data for some of our analyses.[27]

Additionally, we use data on the annual number of live births and infant deaths for the 86 urban municipalities for years 1933-1950 (DNBH, 1933-1950). These data are unfortunately not available for the (much smaller) rural municipalities during this time period. In the urban municipalities, the median number of live births over 1933-1950 was 146.

(ii) a "never treated" status for municipalities that are not featured on the lists. We test the robustness of our main results to dropping cohorts born after 1949 in these 28 municipalities with less precise NHV treatment dates.

[26]The archive data on the number of nurses is incomplete and of poor quality. Moreover, we assume that NHV program implementation is "an absorbing state". The vast majority of municipalities have the NHV program in place continuously once it was implemented.

[27]Where necessary (e.g., data on votes), we constrain our linear interpolation to values in the 0-100 range.

Individual-level administrative data on outcomes of the first generation. We use administrative population register data available for years 1980-2012. As outcomes, we consider several measures of human capital and health. First, we construct two measures of educational attainment at age 50 (when we can observe all of our cohorts in the outcome data): years of schooling and an indicator for having more than nine years of compulsory schooling.[28]

Second, we examine adult income. Our main specifications focus on log average wage income (in 2012 terms) over all ages observable between 30 and 60 in our data, as a proxy for lifetime earnings.[29] We also calculate the total present discounted value (PDV) of earnings over ages 30-60 (again, based on all observable ages in this range).[30] To study potential heterogeneous effects on income throughout the lifecycle, we calculate individual-level three-year moving averages (e.g., an average over ages 49-51) at 5-year age intervals, as well as indicators for any wage income in these age ranges. We create the same measures using total income, which includes income from any public or retirement benefits in addition to earnings.

Third, we study survival beyond ages 55, 60, and 65. For these outcomes, we left-censor the data such that all individuals in our analysis sample enter the risk period that we consider at age 50.[31]

Individual-level administrative data on outcomes of the second generation. We have data on the fertility of women born in 1935-1957 in our sample.[32] We examine several fertility outcomes for the women in the 1935-1957 cohorts—an indicator for no children, total

[28] We have also considered an indicator for university graduation as an outcome and did not find significant effects on this higher margin of educational attainment.

[29] More precisely, for each individual, we calculate an average wage income over all observable ages between 30 and 60 and then take logs.

[30] We use a 3 percent annual real discount rate, following Chetty *et al.* (2011).

[31] Since our outcomes data begin in 1980, individuals enter our sample at different ages. As such, our oldest cohorts must have survived to age 50 to be observed in the data, while our youngest cohorts must have only survived to age 23. When studying survival, we limit our analysis to only those individuals who have survived to at least age 50. The resulting sample is right-censored, but this type of censoring is taken into account by our cohort fixed effects.

[32] In the Danish register data, it is possible to link all cohorts born in 1960 and later to their parents (Pedersen *et al.*, 2006). Unfortunately, we cannot link treated children to their families; i.e., we cannot examine whether access to preschool or NHV impacted the fertility patterns of the mothers of treated children.

number of children, maternal age at first birth, and an indicator for the father's information being missing.

We then link all mothers in our sample to their oldest children, for whom we can observe educational outcomes at age 25. In the second generation, we can observe years of schooling, an indicator for more than compulsory education, and an indicator for graduating gymnasium (academic high school after the nine years of compulsory education). Given that the average age at graduation from university is in the late 20s in Denmark, we lack power to examine this last margin of the educational distribution.

Sample construction and selection. We limit our sample to individuals born in Denmark between 1930 and 1957. In addition, to be a part of our analysis, individuals have to meet two criteria: (1) the individual must have a valid code for his/her parish of birth that allows us to assign treatment status; and (2) the individual must be observed in our post-1980 outcome data.

We can match around 90 percent of Danish-born individuals in our outcome data to a parish of birth.[33] Since we can only study the outcomes of survivors who are observed in the register data—i.e., those who were aged 23 to 50 in 1980—we are concerned with endogenous sample selection due to effects on mortality or emigration before 1980. We address this concern in two ways. First, we compare our analysis sample to annual aggregate data on live births and infant deaths in Denmark, which is available for years 1933-1950. Appendix Figure 2 illustrates the percentage of "missing" Danish-born individuals in our outcome data (including individuals who are missing due to invalid parish codes) by year of birth.[34] Not surpisingly, we miss more individuals from older than younger cohorts—about 13 percent of the 1930 cohort and only 4 percent of the 1951 cohort are missing from our outcome data. However, using only the younger cohorts with fewer missing observations, we show

[33]We omit the following groups with invalid parishes: individuals with errors in their parish of birth registration (such as those who are registered using post-1970 municipality information that cannot be matched to the pre-1970 municipal structure), individuals who were registered by religious minorities such as Catholics, and individuals with undocumented parish codes. Also, individuals who were born in hospitals cannot be merged to their municipalities of birth, and they are omitted from our sample as well. Hospital births for these cohorts were very rare—only 5.5 percent of our sample—as home births were the norm in Denmark up until the 1960s.

[34]We calculate this percentage as: (# of Danish-born observations in register data)/(# of live births - # infant deaths). Aggregate data on live births and infant deaths come from DNBH (various years).

that statistically significant mortality impacts of preschool only materialize around age 60 (see Appendix Figure 8). Thus we do not believe that selection due to mortality prior to age 50 has a meaningful impact on our results.

Second, we use our municipality-level data on live births and infant deaths for 86 urban municipalities for years 1933-1950. We correlate the share of "not missing" Danish-born individuals in our outcome data relative to all first-year survivors with our key treatment variable, an indicator for an approved preschool in the municipality×year. Appendix Table 1 reports the results from various specifications of this regression, showing no statistically significant relationships.

Our analysis sample of Danish-born individuals with valid parish codes consists of $1,657,399$ observations. When we limit to individuals born in the 140 municipalities with an approved preschool by 1960, we are left with $879,647$ observations.

4 Empirical Methods

Our analysis exploits the municipality×year variation in preschool approvals (and the NHV rollout) to create difference-in-difference and event-study designs. To ease the computational burden and to estimate models at the level of variation, we take a two-step approach. First, we run an auxiliary regression, in which we regress each of our outcome measures on all available pre-determined individual-level control variables (i.e., gender and month of birth indicators) as well as municipality×birth-year fixed effects.[35] The municipality×birth-year fixed effects from this regression yield the conditional mean outcomes in each municipality×birth-year cohort, after controlling for the micro-covariates. Then, we collapse our data into $3,918$ municipality×birth-year-cells and use the conditional means as dependent variables in our regressions, which we weight by cell size.[36]

To analyze the effects of preschool access, we estimate versions of the following equation:

[35]When we analyze maternal fertility and intergenerational outcomes, we exclude gender dummies from the auxiliary regression since the first generation only consists of women in these analyses.

[36]Donald and Lang (2007), among others, show the asymptotic equivalence between this two-step group-means estimator and the micro-data counterpart. In Section 5 we show that our results are very similar when we estimate individual-level regressions instead. These types of estimators have been used in many other papers: e.g., Shapiro (2006); Angrist and Lavy (2009); Albouy (2009); Notowidigdo (2011); Currie *et al.* (2015); Isen *et al.* (Forthcoming).

$$Y_{ymc} = \alpha_0 + \alpha_1 PreschoolAge3_{ym} + \lambda_m + \gamma_y + \delta' X_{ym} + \nu_c \times y + \epsilon_{ymc} \quad (1)$$

for cohorts born in year y, municipality m, and county c.[37] Y_{ymc} is an outcome of interest such as education or survival beyond age 65 (conditional on the micro-covariates described above). $PreschoolAge3_{ym}$ is an indicator equal to one for cohorts that had at least one approved preschool in their municipality of birth at age 3, and zero otherwise.[38] λ_m are municipality fixed effects, while γ_y are year of birth fixed effects. X_{ym} is a vector of municipality time-varying controls for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. We also add county-specific linear time trends denoted by $\nu_c \times y$.[39] ϵ_{ymc} is the error term, which we cluster by municipality. The key coefficient of interest, α_1, identifies the effect of having a government-approved preschool in one's municipality of birth at age 3 on the outcome of interest.

We also estimate event-study regressions to analyze the effects of preschool access by age of exposure:

$$\begin{aligned} Y_{ymc} = \kappa_0 + \sum_{a=-2}^{a=6} \tau^a Preschool^a_{ym} + \sum_{a=8}^{a=11} \tau^a Preschool^a_{ym} \\ + \rho' \mathbf{1}[AgeAtPreschool < -2]_{ym} + \sigma' \mathbf{1}[AgeAtPreschool > 11]_{ym} \\ + \lambda_m + \gamma_y + \delta' X_{ym} + \nu_c \times y + \epsilon_{ymc} \quad (2) \end{aligned}$$

Here, $Preschool^a_{ym}$ is an indicator equal to one for cohorts that were age a in the year of the first preschool approval in their municipality of birth and zero otherwise. We include indicators for ages -2 to 6 and 8 to 11 (with age 7, the age at which children start school

[37] Counties are the next-largest geographical entities after municipalities. In the period that we study, there are 23 counties and the capital Copenhagen, which had special status in the county structure (i.e., was a separate administrative entity). Counties contain between two and eight municipalities.

[38] These analyses implicitly assume that the municipality of birth is also the municipality of residence during early childhood.

[39] We test the robustness of our results to the exclusion of county time trends and to the inclusion county×year fixed effects, as described further below.

and can no longer attend preschool, as the omitted category). $\mathbf{1}[AgeAtPreschool < -2]_{ym}$ is an indicator for cohorts born more than two years after the preschool approval (i.e., they were aged less than -2 at the time of approval), while $\mathbf{1}[AgeAtPreschool > 11]_{ym}$ is an indicator for cohorts who were older than age 11 at the time of approval. The remainder of the variables is the same as in equation (1). The event-study specification allows us to test for differences in effects by the number of potential years of exposure: cohorts who were aged 3 or less at the time of approval could attend preschool for four years until they turned 7, while cohorts who were older could only attend for fewer years, or none at all. Moreover, this regression contains a placebo test as we can check whether preschool access is correlated with the outcomes of cohorts who were too old at the time of preschool approval.

To examine interactions between preschool and NHV, we estimate:

$$Y_{ymc} = \beta_0 + \beta_1 PreschoolAge3_{ym} + \beta_2 NHV_{ym} + \beta_3 PreschoolAge3_{ym} \times NHV_{ym}$$

$$+ \lambda_m + \gamma_y + \delta' X_{my} + \nu_c \times y + \epsilon_{ymc} \tag{3}$$

Here, NHV_{ym} is an indicator equal to one for cohorts that had the NHV program in their municipality in their year of birth and zero otherwise. All of the other variables and coefficients are the same as in equation (1). β_1 measures the impact of access to preschool at age 3 for cohorts without NHV, while β_2 measures the impact of access to NHV at birth for cohorts without preschool at age 3. β_3 identifies the interaction effect between the two programs.

Identifying assumptions. Our empirical strategy yields estimates of the causal effects of early access to a high quality preschool and the interaction effects between access to preschool and NHV under the assumptions that: (1) the timing of preschool approvals is uncorrelated with other municipality time-varying characteristics that also predict our long-run and intergenerational outcomes of interest; and (2) the timing of preschool approvals is uncorrelated with the NHV program rollout.[40]

[40] We also need for the timing of the NHV program rollout to be exogenous with respect to the outcomes of interest. Evidence on this point has been provided by Wüst (2012) and Hjort *et al.* (forthcoming).

With regard to assumption (1), our estimation approach addresses several concerns: Our year-of-birth fixed effects control for overall trends in cohort and intergenerational outcomes, while the municipality fixed effects control for all time-invariant differences between municipalities. Our county linear trends allow for the outcomes of cohorts born in each of the 24 counties in our data to follow distinct trends. Further, we include all available municipality time-varying controls for our time period of analysis.

The period that we study calls for a discussion of the role of World War II and its possible influence on our sources of variation. We would face a problem if the effect of the war varied across municipalities in a systematic way that correlated with the preschool and NHV expansions. Historical accounts make clear that Denmark—unlike many other European countries—was not very severely impacted by the German occupation between 1940 and 1945. As noted in several publications, cooperation with the German forces with respect to political and economic decisions during the war resulted in a minimal impact of the occupation (Pedersen, 2009; Poulsen, 2002). While coffee, tobacco and some other goods were rationed, there was nevertheless a stable supply of food for all Danish citizens (e.g., milk and bread were not rationed, see Poulsen, 2002). According to Pedersen (2009), "among all occupied countries, Denmark was the country with the smallest decrease in the standard of living and the country where everyday life was least impacted." (authors' translation, p. 404 in Pedersen, 2009). As such, we do not believe that World War II is a confounding factor for our analysis. Moreover, we find no evidence of disruptions in the spread of preschools or NHV during the war years.

Additionally, while the first identifying assumption remains inherently untestable, we conduct some indirect tests to evaluate its plausibility. Specifically, to test for a correlation between the timing of preschool approvals and other time-varying municipality-specific factors, we first regress each of our municipality characteristics on an indicator for preschool approval, municipality and year fixed effects, as well as county-specific linear trends. Panel A in Table 2 presents the results, which show that preschool approval is positively correlated with the percent of the population that is urban, and negatively correlated with the percent of the population that is agricultural and the percentage of property tax payers. These associations imply that preschool approvals occurred in urban areas earlier than in rural areas on average (rural areas are more likely to have property tax payers). When we include linear trends interacted with an urban/rural municipality indicator in these specifications in Panel B of Table 2, the correlations become insignificant at the 5% level. We also show that our main results are robust to controlling for urban/rural municipality indicators interacted with linear trends in Section 5.[41]

As another test of the first identifying assumption, we ask whether *predicted* outcomes based on observable municipality time-varying characteristics are correlated with preschool access. We regress each of our four main outcomes (which are already conditional on gender and month of birth fixed effects)—years of schooling at age 50, indicator for more than compulsory education at age 50, log average wage income over ages 30-60, and survival beyond age 65—on all of the municipality characteristics, and then estimate equation (1) with the predicted outcomes as dependent variables. Table 3 presents the results, which are not significant at the 5% level. The only marginally significant coefficient (at the 10% level) is for predicted survival, and it is in the opposite direction of our main results described in Section 5.

We can directly test the second identifying assumption with our data. In Table 4, we present results from correlating access to an approved preschool with access to NHV. Specifically, in column (1), we estimate a version of equation (1), using an indicator for having the NHV program at birth as the dependent variable. In column (2), we instead regress an indicator for having access to an approved preschool at age 3 on an indicator for having access to the NHV program at birth. In both specifications, we find little evidence for any statistically significant (or economically meaningful) relationship between the two programs.

5 Results

5.1 Long-Run Effects of Preschool on the First Generation

We begin with results on the long-term impacts of access to a government-approved preschool for the first generation. Table 5 presents results from estimating versions of equation (1) using

[41] Another source of potentially confounding variation is a schooling reform in 1958, which increased access to academic-track high schools for rural students. We show that our results are robust to controlling for this reform in Section 5.

the following outcomes as dependent variables: years of schooling at age 50, an indicator for more than compulsory education at age 50, log mean wage income between ages 30 and 60, and an indicator for survival beyond age 65.

We show results from four different specifications. In the first column, we begin with a model that only controls for municipality and birth year fixed effects. In column (2), we add in all of the available municipality time-varying characteristics. In column (3), we also include county linear time trends. Finally, column (4) includes county×year fixed effects instead of county linear trends.

Table 5 shows that access to a high quality preschool improves long-term well-being. The coefficients are reduced in magnitude once we control for the municipality time-varying characteristics (i.e., moving from column (1) to column (2)), but all remain statistically significant. In our most preferred (and most conservative) specification in column (3), we find that, relative to the comparison cohorts, individuals who had an approved preschool in their municipality of birth by age 3 have 0.07 more years of schooling (0.6 percent at the sample mean), are 1.3 percentage points (1.9 percent) more likely to have completed more than compulsory education, have 1.6 percent higher income (although this coefficient is only marginally significant at the 10% level), and are 0.5 percentage points (0.6 percent) more likely to survive beyond age 65.[42]

Appendix Table 2 explores the effects of preschool on alternative measures of adult income: average age 30-60 wage income in levels, log of the PDV of age 30-60 wage income, log average age 30-60 total income, log average age 49-51 wage income (i.e., around age 50, when all of our cohorts are observed), and an indicator for any positive wage income at ages 49-51. While we find positive coefficients on exposure to preschool for all of these outcomes, the results are not robust across all models that we consider. As such, we view our evidence for impacts on adult income as more suggestive than our results for the other outcomes.

Figures 3 and 4 present the event-study graphs for years of schooling and more than

[42] As further discussed in Section 5.3, we have also examined the effects of preschool exposure on hospitalizations and some diagnoses (cardiovascular disease, heart disease, and diabetes) in adulthood. We find that preschool exposure reduces the number of nights individuals spend in the hospital between ages 55 and 64, but no consistent effects on the diagnoses. Finally, we have also explored differences in the effects on our main outcomes by gender. The estimated coefficients for wage income are higher for men, while the estimated coefficients for survival beyond age 65 are higher for women. However, these differences across gender are not statistically significant and we therefore do not focus on them here.

compulsory education, respectively. We plot the τ^a coefficient estimates from equation (2) and the corresponding 95% confidence intervals. Both figures show an improvement in educational attainment for cohorts aged 3 years or less at the time of preschool approval. Cohorts aged 4 to 6 in the year of preschool approval also seem to benefit from access to a government-approved preschool, consistent with the fact that they were eligible to attend for at least some of their preschool years. The coefficients on exposure at ages 8 to 11 are statistically insignificant, suggesting that there are no pre-existing trends in the outcomes of cohorts who were too old to attend preschool.

Appendix Figures 3 and 4 present the event-study graphs for log mean wage income at ages 30-60 and survival beyond age 65, respectively. While we still do not see any significant pre-trends for cohorts aged 8 to 11, the estimated coefficients at younger ages are also not statistically different from zero. We note that the event-study design may be particularly underpowered for analyzing survival beyond age 65, which is an outcome that exhibits less variation than the other main outcomes in our data.

Robustness. Our analysis rests on an assumption that, conditional on municipality and birth year fixed effects, all available time-varying municipality characteristics, and county-specific time trends, the timing of preschool approvals is exogenous to other determinants of long-run outcomes. We would face a problem, if, for example, cohorts born in municipalities with earlier approved preschools were experiencing a more positive trend in their outcomes than cohorts born in municipalities with later preschool approvals. Our event-study figures suggest that differences in outcome trends across municipalities are unlikely to bias our results—we find no evidence that cohorts who were aged 8 to 11 at the time of the first preschool approval experienced any changes in their outcomes, despite the fact that slightly younger children in those same municipalities did benefit from preschool access. We perform a number of other specification checks to test the robustness of our results and the validity of our identification strategy in Appendix Table 3.

Column (1) presents results where we only include a balanced panel of municipalities with observations in every cohort birth-year in our data; results remain largely unchanged. In columns (2)-(5), we explore alternative specifications that deal with differences across urban and rural areas. In column (2), instead of county linear trends, we include urban/rural municipality indicators interacted with linear trends. In column (3), we include county×urban/rural indicators interacted with linear trends (i.e., we allow urban and rural municipalities within each county to follow distinct trends). In column (4), we drop Copenhagen, the largest municipality and city in Denmark. In column (5), we include an interaction between an indicator for cohorts born in 1946 or later and an indicator for a rural municipality to control for the impact of the 1958 Danish schooling reform, which increased access to academic-track high schools for rural students (for details, see Arendt, 2008). Our results are robust to all of these changes. The fact that our results are robust to allowing urban and rural areas to follow differential trends suggests that the correlations in Table 2 (which showed that urban municipalities tended to approve preschools earlier than rural ones) are not driving our main results.

Columns (6)-(8) test the robustness of our results to further sample limitations. Column (6) drops municipalities that had an approved preschool at the beginning of our sample period in 1933. We drop these municipalities because many of the earliest approvals took place in preschools that had been introduced by philantropic organizations, and there may be a concern that these organizations also introduced other initiatives that benefitted children (e.g., vaccination programs). Column (7) limits the analysis to cohorts born in 1937-1957, i.e., after the initial introduction of NHV program. Column (8) only includes municipalities that ever implement an NHV program in our sample. Across all of these specifications, the results remain generally consistent with our baseline model.

Column (9) of Appendix Table 3 estimates regressions where we replace the baseline indicator treatment variable with a variable for the fraction of years a cohort was exposed to an approved preschool between the ages of 3 and 6.[43] The results from this alternative specification again suggest that greater exposure to preschool improves long-run outcomes.

In Appendix Table 4, we show that our effects on education, income, and mortality are very similar when we use the underlying micro-data to estimate our regressions rather than the two-step approach described in Section 4.[44] We have also tested the robustness of our

[43]This variable is equal to 1 for those aged 3 and younger in the year of preschool approval; 0.75 for those aged 4; 0.5 for those aged 5; 0.25 for those aged 6; 0 for those aged 7 and older.

[44]Here, we use outcome-specific samples, which omit the individuals with missing values for education (6

results to different ages of follow-up. Since we do not observe all cohorts at all ages, this analysis can shed light on whether we see similar effects across different cohorts. Appendix Figures 5 and 6 show positive statistically significant effects of preschool on educational attainment at ages 30 through 55. The coefficients become insignificant for our oldest cohorts who are observed at ages 60 and 65, which is consistent with other evidence (briefly discussed below) that the estimated impacts of preschool are larger in municipalities that approved preschools in later years rather than earlier years. Appendix Figure 7 demonstrates that the effect on log wage income is positive at ages 30 through 55, although it is only statistically significant for cohorts observed at age 45. We therefore continue to view the result for adult income as only suggestive. Appendix Figure 8 shows that the positive effect on survival materializes around age 60 and not earlier.

Lastly, we have checked whether our results are sensitive to defining treatment based on the year of first preschool establishment rather than the first government approval. In principle, municipalities where the first approval happened after the year of initial preschool establishment could be used to distinguish the effects of access to *any* preschool from a change in preschool *quality* resulting from government approval. In practice, however, only 62 municipalities have an approval year that is later than the initial establishment year, and we do not have enough power to detect separate impacts of the two treatments. Appendix Table 5 shows that our results remain similar if we use the year of establishment to define treatment.

Magnitudes. To assess the magnitudes of our estimates, we compare our findings to the existing literature on the effects of preschool on educational attainment. To begin with, we compare our results to two studies on the impacts of universal preschool expansions in Scandinavia in the 1960s and 1970s. Havnes and Mogstad (2011) and Bingley *et al.* (2015) find 0.06 and 0.07 year increases in average years of schooling in Norway and Denmark, respectively. Our estimated 0.07 year increase in years of schooling is very similar.

Just as in Havnes and Mogstad (2011) and in Bingley *et al.* (2015), our estimates represent intent-to-treat (ITT) impacts, since we do not observe whether individuals in our outcome

percent of the sample) and income (10 percent of the sample). We have also estimated these regressions using a sample with non-missing information across all outcomes, with similar results.

data actually attended preschool. However, given that we study a targeted rather than universal program, our treatment-on-the-treated (TOT) effects are likely to be different.

To calculate approximate TOT effects for our setting, we must first estimate a preschool enrollment rate, which we can do for the 86 urban municipalities in our sample. We use data on the number of children enrolled in each preschool from the nine book publications, interpolate to get estimates of enrollment in every year, and then aggregate to the municipality×year level. Next, we use data on the number of survivors past age one in each of the 86 municipalities and calculate the share of children aged 3 to 6 who were enrolled in preschool in every year between 1939 and 1950.[45] Lastly, we estimate "first stage" regressions on this limited sample, regressing the share of children enrolled on an indicator for at least one approved preschool in a municipality×year cell, controlling for municipality and year fixed effects. Appendix Table 6 presents the results. When we consider all 86 municipalities in column (1), we find that moving from zero to at least one approved preschool in an average urban municipality leads to about an 8 percentage point increase in the share of children enrolled in preschool. In column (2), we drop 9 municipalities that had at least one preschool established before 1939 that was not yet approved; the first stage estimate becomes a 14 percentage point increase in the share of children enrolled. This higher estimate may be more applicable to the other municipalities that are included in our main analysis (but excluded from these "first stage" regressions), which are more rural and less likely to have had a previously established preschool. In column (3), we focus on the "switcher" municipalities that move from zero to one approved preschool over the 1939-1950 time period—the first stage estimate here is an 11 percentage point increase, but is no longer statistically significant ($p = 0.14$) due to the substantially reduced sample size.

The above analysis suggests that one can divide our estimates by between 0.08 and 0.14 to get approximate TOT effect sizes. Although the resulting TOT magnitudes may seem large when compared with the more recent Scandinavian studies on universal programs, it is likely that the disadvantaged children targeted by the preschools in our study may have had the most to gain from early education and improved nutrition and health care. In fact, our TOT

[45] We begin in 1939 since that is the first year when we can observe all living 6-year-olds (our earliest data on births are from 1933). We do not have municipality-level data on births past 1950.

estimates are actually not out of step with the U.S. literature on participation in targeted preschool programs. For example, Garces *et al.* (2002) find that Head Start participation increases the likelihood of high school completion by 20 percentage points among whites. Our 1.3 percentage point increase in the likelihood of having more than compulsory education translates into an approximate TOT effect of 9 ($\frac{0.013}{0.14}$) to 16 ($\frac{0.013}{0.08}$) percentage points, which is quite comparable. Heckman *et al.* (2010a) find even larger impacts of participation in the Perry Preschool program for females—the highest grade completed is increased by almost one year, while the likelihood of high school graduation is nearly 50 percentage points higher for the treatment group than the control group. Again, our estimated TOT magnitudes of a 0.5 ($\frac{0.07}{0.09}$) to 0.9 ($\frac{0.07}{0.06}$) year increase in years of schooling and a 9 to 16 percentage point increase in more than compulsory education are within these bounds.

5.2 Effects of Preschool on the Second Generation

Having shown that preschool access has large and persistent positive effects on adult well-being throughout the life cycle, we proceed to examine whether these benefits transmit to the next generation.

Before doing so, we first test whether preschool exposure affected the fertility behavior of women in our analysis sample. In Appendix Table 7, we present results from specifications that limit the sample to women born in 1935-1957 for whom we have complete fertility data. As outcomes, we consider: an indicator for having no children, the total number of children, the mother's age at first birth, and an indicator for the father's information being missing from any of the children's birth certificates. None of the effects is significant at the 5% level, although we do find a marginally significant increase in the age at first birth by 0.08 years. These results suggest that any selection into fertility—and hence into our sample of second generation outcomes—is likely to be small.

Our analysis of second generation outcomes focuses on the oldest children of the mothers in our baseline sample. Table 6 presents results for educational outcomes measured when these children are age 25. We see positive impacts on the second generation's educational attainment—years of schooling increases by about 0.03 years (0.2) percent, which seems to be driven by a 1 percentage point (1.5 percent) increase in the likelihood of having more than a compulsory education. These magnitudes are substantially smaller than those found in Barr and Gibbs (2016)'s concurrent study of the intergenerational effects of access to Head Start, who report a 12 percentage point increase in the likelihood of high school graduation and a 16 percentage point increase in the likelihood of having some college education.[46]

We can place our results for the second generation in the context of the literature on intergenerational transmission of socio-economic status (e.g.: Solon, 1992; Bauer and Riphahn, 2004; Lee and Solon, 2009; Black *et al.*, 2009; Chetty *et al.*, 2014). If preschool mostly affects the education of the second generation through an improvement of the first generation's education level, the ratio of the coefficients for the two generations can approximate an intergenerational transmission coefficient for education. Note that since not all first generation women have children, we have also estimated our first generation models on the sample of mothers that we use to create our second generation sample. In our preferred specification (with county linear trends and all municipality time-varying controls), we find that access to preschool at age 3 increases years of schooling by 0.08 years.[47] Combining this result with the estimated coefficient for years of schooling for the second generation (0.03) suggests a transmission coefficient of around 0.4 ($\frac{0.031}{0.077}$).[48] Thus, our findings present novel evidence that high quality preschools have the potential to mitigate some of the intergenerational transmission of low educational attainment and socio-economic status more broadly.

5.3 Interaction Effects Between Preschool and NHV

Next, we analyze whether access to the NHV program in infancy—which has been shown to have significant impacts on infant and long-term health outcomes by Wüst (2012) and Hjort *et al.* (forthcoming)—enhances or diminishes the positive long-term and intergenerational returns to preschool. Before proceeding, Appendix Table 8 confirms that NHV has long-term effects on health outcomes in our sample of 140 municipalities that ever have an approved

[46]Barr and Gibbs (2016) acknowledge that their estimates seem implausibly large, possibly due to spillover effects or concerns about endogenous program adoption.

[47]The results are available upon request.

[48]The delta method yields an approximate standard error of 0.3 for this transmission coefficient. If we instead calculate the transmission coefficient using an indicator for having more than a compulsory education as the outcome, we obtain an estimate of 0.86 with a standard error of 0.42. Black *et al.* (2013), who study the intergenerational effects of *in utero* exposure to radiation in Norway, use a similar strategy to calculate an intergenerational transmission coefficient of 0.625 for male IQ.

preschool.[49] Additionally, in Appendix Tables 9 and 10 we report estimates from regressions that only include the main effects of preschool and NHV, and not the interaction. We continue to find strong positive effects of preschool on education, income, and survival, while the main effects of NHV are concentrated on health outcomes. In fact, the main effects of preschool are very similar to our results from specifications where we do not control for NHV exposure, consistent with our finding that the rollout of each program is uncorrelated with the other.

Table 7 presents results from estimating equation (3) for our main outcomes of interest in the first generation. In these specifications, the main effects of both preschool and NHV point to substantial improvements in education, income, and the likelihood of survival for cohorts who were only exposed to either preschool or NHV.[50] However, the interaction coefficients are consistently opposite-signed. For cohorts who had NHV at birth, the positive impact of access to preschool at age 3 on years of schooling is reduced by 88 percent, while the increase in the likelihood of having more than compulsory education is reduced by 70 percent. The increase in the likelihood of survival past age 65 is lowered by 80 percent. When we consider adult income, the interaction coefficient is insignificant but similarly negative.[51]

While the magnitudes of our interaction effects may seem large, they are surprisingly similar to two other recent studies in different contexts. In particular, Gunnsteinsson *et al.* (2014) show that vitamin A supplementation at birth mitigates 100 percent of the adverse impact of *in utero* exposure to a tornado on children's health in the first year of life in Bangladesh. Adhvaryu *et al.* (2015) find that cash transfers under the *Progresa* program in Mexico reduce the adverse effect of rainfall shocks on educational attainment by 80 percent. Our estimates, which come from an analysis of two early childhood interventions in Denmark, are comparable.

To test the robustness of the interaction analysis, Appendix Tables 11 through 13 show

[49] Just like Hjort *et al.* (forthcoming), who use a different analysis sample, we find effects on the number of nights spent in the hospital and on the incidence of diagnoses for cardiovascular diseases and heart conditions. We do not find statistically significant effects of NHV on long-term survival in our sample.

[50] We note that the main effect estimates in Table 7 should not be directly compared to the main effects of either preschool or NHV in regressions without interactions (in Appendix Tables 9 and 10), as the main effects in Table 7 are conditional on the other program not being present.

[51] We have also examined interaction effects for education of the second generation. The interaction coefficients are consistently opposite-signed from the main effects, but are statistically insignificant.

results from a number of specifications that vary the control variables and sample. We exclude county linear trends (Appendix Table 11), include urban/rural indicators interacted with trends (Appendix Table 12), and drop post-1949 cohorts in the 28 municipalities with worse NHV program data (Appendix Table 13). While the coefficients vary slightly in magnitude and statistical significance, the overall story remains the same: There are strong positive main effects of preschool and NHV on adult well-being, while the interaction effects of the two programs are negative.

An additional concern with the interaction results stems from the possible selective survival of weak infants due to NHV exposure. Wüst (2012) finds that NHV increased infant survival by 0.5-0.8 percent. If the surviving infants have worse health and are less responsive to the benefits of preschool, then our negative interaction effect may be in part driven by this change in the composition of the sample. To address this issue, before collapsing the data, we randomly drop one percent of individuals with the lowest possible educational attainment (seven years or less) who were born in NHV-treated municipalities, and estimate the interaction model on this constrained sample. Appendix Table 14 presents the results from this exercise, which are very similar to our main findings. We conclude that selective survival of NHV-exposed individuals is unlikely to explain our negative interaction effects.

Discussion of interaction effects. Our interaction results suggest that high quality preschool can compensate for initial disadvantages (as identified by a lack of exposure to NHV). Put differently, our findings imply that two multifaceted early childhood programs—NHV and preschool—may partially substitute for one another. While we do not have data to observe the underlying mechanisms, one hypothesis is that the two programs have overlapping informational and health components. For instance, since NHV educated parents on hygiene, infant nutrition, and parent-child interactions, their children may have grown up in a healthier home environment. Furthermore, NHV facilitated parents' contact to GPs and thus children may have received better preventive and acute health care. As such, preschool offered a less significant treatment to NHV-exposed children than to their non-NHV counterparts. Our estimate of a large opposite-signed interaction coefficient when we consider our primary health outcome—survival past age 65—is consistent with the "overlapping health component" interpretation.[52]

Finally, we have examined the interaction effects separately in the early and later periods in our analysis time frame. Since child health was improving throughout the 20th century, one would expect greater scope for health improvements (and substitution between programs) for older cohorts in our data. We find suggestive indication that the interaction effects are larger—especially for survival as the outcome—in the early period, although these estimates are imprecise and not statistically different from one another.[53]

Of course, alternative explanations are also possible. First, since both programs were implemented at the municipality level, there may be concerns about "overlapping labor markets". While one might worry that NHV program implementation limited nurses' ability to work at preschools (leading to a reduction in the effectiveness of preschool), this concern is not relevant to our setting since NHV nurses were highly specialized with additional training beyond standard nurse certification. Thus NHV nurses were not in the relevant pool of preschool personnel.

Second, our estimates cannot speak to potential parental behavioral responses to these public interventions. Given that preschool reduces the costs of maternal employment, parental private investments into children may have become lower once high quality preschool became available. Moreover, if parents of children who had both NHV at birth and access to preschool at subsequent ages reduced their private investments by more than parents of children who only had one program, then our negative interaction effects may be in part driven by this response. Unfortunately, we do not have any data on parental investment behaviors for the cohorts in our analysis, and thus cannot explore this possibility.

5.4 Cost-Benefit Calculations

In this section, we present some back-of-the-envelope calculations that assess whether the estimated benefits of preschool outweigh its costs, and discuss how the cost-benefit analyses

[52] We have also estimated the interaction model using the hospitalization and diagnoses outcomes from Appendix Table 8. We find similar evidence of opposite-signed interaction effects for the number of nights spent in the hospital between ages 55 and 64, but our results for diagnoses are imprecise.

[53] Results available upon request. Also, we have explored whether the treatment effect of preschool varies across different time periods. We find evidence that the "late approvers" (i.e., municipalities that had a first approved preschool in 1940 or later) experienced larger long-term impacts of preschool than the "early approvers".

compare across the two programs we consider. All amounts are reported in 2012 dollars. According to a historical report, the total cost of a preschool slot in 1949-50—which includes spending by the national government, municipalities, philanthropic organizations, and parents—ranged between $1,661 and $2,291 for preschools outside and within Copenhagen, respectively (Børnesagens tidende, 1952).[54] Thus, the per-slot cost of attending preschool for the full four years ranged from $6,644 to $9,164, implying a total cost of preschool between $525,993,720 and $725,497,660 for the cohorts that we study.[55]

Since preschool costs increased substantially over time, our measure based on the 1949-50 estimates is an upper bound for the average cost over our entire period of analysis (1933-1960), and our cost-benefit calculations are likely to be conservative.[56] Notably, the Danish preschool cost is much lower than the per-child cost of the two-year U.S. Perry Preschool Program, which is estimated to be $20,225 (Heckman *et al.*, 2010b).

To calculate the benefits of preschool, we use our estimates of the impacts on survival and earnings. Since most individuals in our sample are still alive, we cannot calculate survival gains using impacts on age at death. Thus we conservatively assume that access to an approved preschool increases survival by one additional year beyond age 65. We can then calculate that preschool exposure saved about 396 life-years for the cohorts in our analysis.[57] Using a recent estimate of a non-quality-adjusted value of a year of life of $76,482 (Lee *et al.*, 2009), our calculations suggest a lower bound for the monetary benefit due to life-years saved of about $30,274,723.

To calculate the benefits due to higher earnings, we assume that individuals work from

[54] A preschool slot was more expensive in Copenhagen for several reasons: among other things, Copenhagen preschools had, on average, longer opening hours and were smaller.

[55] We calculate these numbers by multiplying the cost per child by the number of treated children in our time period. The number of treated children is our analysis sample (879,647) multiplied by the approximate share of individuals aged 3-6 who attended preschool (0.09; see the dependent variable mean in Appendix Table 6). Thus, we arrive at: $6,644*879,647*0.09=$525,993,720 and $9,164*879,647*0.09=$725,497,660.

[56] To examine how the costs of preschools changed over time, we used the Statistical Yearbook for the Kingdom of Denmark and the Statistical Yearbook for Copenhagen (Statistics Denmark, various years; The municipality of Copenhagen, various years) to calculate the total costs of a preschool slot for the period 1942-1951. We find similar costs for the 1949-50 year to those reported in Børnesagens tidende (1952), which is reassuring. We also find that the costs per preschool slot increased by approximately 80 percent per child over 1945-1950, as a result of new construction, longer opening hours, and more highly paid and highly qualified staff.

[57] To arrive at this number, we multiply our analysis sample (879,647) by the effect on survival past age 65 (0.005) and scale by the approximate share of individuals who attended preschool (0.09).

age 21 to 60, and that average annual earnings over ages 30-60 reflect average annual lifetime earnings.[58] With these assumptions, our (most conservative) 0.016 effect on adult earnings translates to a total benefit of about $1,041,645,181 in net present value terms.[59]

With regard to the cost-benefit analysis for NHV, we rely on prior calculations detailed in Wüst (2012) and Hjort *et al.* (forthcoming). They estimate a total cost of NHV of $23,805,000.[60] Conservatively assuming that treated individuals survive one additional year beyond age 60, they also estimate that NHV saved between 690 and 2,070 life-years in 1935-1949.[61] These numbers imply a total benefit in the range of $52,772,580 to $158,317,740.

For both programs, it is evident that the benefits—even under the most conservative assumptions—substantially outweigh the costs. The NHV program, which is much cheaper than the preschool program, seems more cost-effective according to these back-of-the-envelope calculations. However, it is important to highlight that preschool access has other effects that we ignore here. For instance, we find that access to preschool increases educational attainment, which can have effects on outcomes we do not observe, such as criminal behavior.[62] We also do not account for the direct effects of preschool on the mothers (or other family members) of the cohorts we analyze. Lastly, we ignore the benefits for the next generation.

[58] We follow Fredriksson *et al.* (2013) and Bütikofer *et al.* (2014), who use similar assumptions in their cost-benefit calculations.

[59] We calculate this benefit as follows. We use mean age 30-60 wage income (273,662.9 DKK in our data), and convert it to 2012 dollars, i.e., $35,576.18. The NPV per person over ages 21-60 then amounts to $822,335.22. We multiply the per-person NPV by our effect size of 0.016, and then scale by the approximate number of individuals who attended preschool (0.09 * 879,647).

[60] Wüst (2012) estimates the yearly costs per nurse at 6,002DKK (in 1941). Assuming that each nurse provided services for the recommended 250 infants, this figure suggests a per-child cost of the program of 24DKK. In 2012, this figure amounts to a per child cost (per visit cost) of 533DKK (52DKK) or $69 ($6.9). This figure is broadly in line with estimates for the cost of well-baby visits reported in Bütikofer *et al.* (2014), who estimate the costs of the (similar but center-based) Norwegian program to be $22 (in 2014 USD). The calculations in Wüst (2012) are based on the assumption that all costs of the nurse program were realized in the first year of the child's life and do not factor in costs related to additional doctor visits of treated children.

[61] As mentioned earlier, Hjort *et al.* (forthcoming) calculate benefits based on the long-term health effects of NHV, since they do not find consistent impacts on education or labor market outcomes.

[62] Prior research documents that high quality preschool reduces criminal activity in adulthood (Heckman *et al.*, 2010a,b).

6 Conclusion

Although the existing literature has documented the importance of early childhood interventions, the question as to whether their effects persist over the life cycle and across generations remains open. Additionally, we know very little about the added value of a program in a population that is exposed to more than one intervention. In this paper, we shed light on these questions with (i) some of the first quasi-experimental evidence on the very long-run and intergenerational effects of a high quality targeted preschool program, and (ii) an analysis of the interaction between exposure to preschool and a health intervention in infancy.

Using data on the timing of preschool approvals across Danish municipalities together with administrative data on outcomes for nearly one million Danish people born between 1930 and 1957, we document positive long-term effects of access to a high quality targeted preschool program. Cohorts with access to a government-approved preschool by age 3 have 0.6 percent more years of schooling, are 1.9 percent more likely to have more than a compulsory education, and are 0.6 percent more likely to survive past age 65. We also find suggestive evidence of a 1.6 percent increase in average wage income over ages 30 to 60. Moreover, we show intergenerational impacts—children of women with preschool access have 0.4 percent more years of schooling and are 1.5 percent more likely to have more than a compulsory education.

However, when we interact preschool access at age 3 with access to the NHV program in infancy, we find that the individuals only exposed to preschool benefit more from it than individuals who were also exposed to NHV. For example, for people who had NHV at birth, the positive impact of preschool on years of schooling is reduced by 88 percent. The increase in the likelihood of survival past age 65 is lowered by 80 percent.

Our findings imply that the marginal benefit of a high quality preschool program is substantially reduced in a population that was exposed to an earlier health intervention. This result means that, in a setting with limited public resources, it may be efficient to design programs that specifically target populations without prior exposure to other interventions. For instance, while many over-subscribed programs for low-income children allocate slots at random or on a "first-come, first-serve" basis, our evidence suggests that an allocation mechanism that considers (the lack of) participation in earlier programs as potentially leading to greater program benefits.

Our results also imply that a high quality preschool program with a health component can compensate for low initial health in a cost-effective way. Although low socio-economic status children suffer from disadvantages at birth in terms of health and parental resources, our findings suggest that preschool interventions can work against some of these initial shortcomings and potentially reduce inequalities in outcomes over the life cycle and across generations (Currie, 2011; Chen *et al.*, 2014; Aizer and Currie, 2014).

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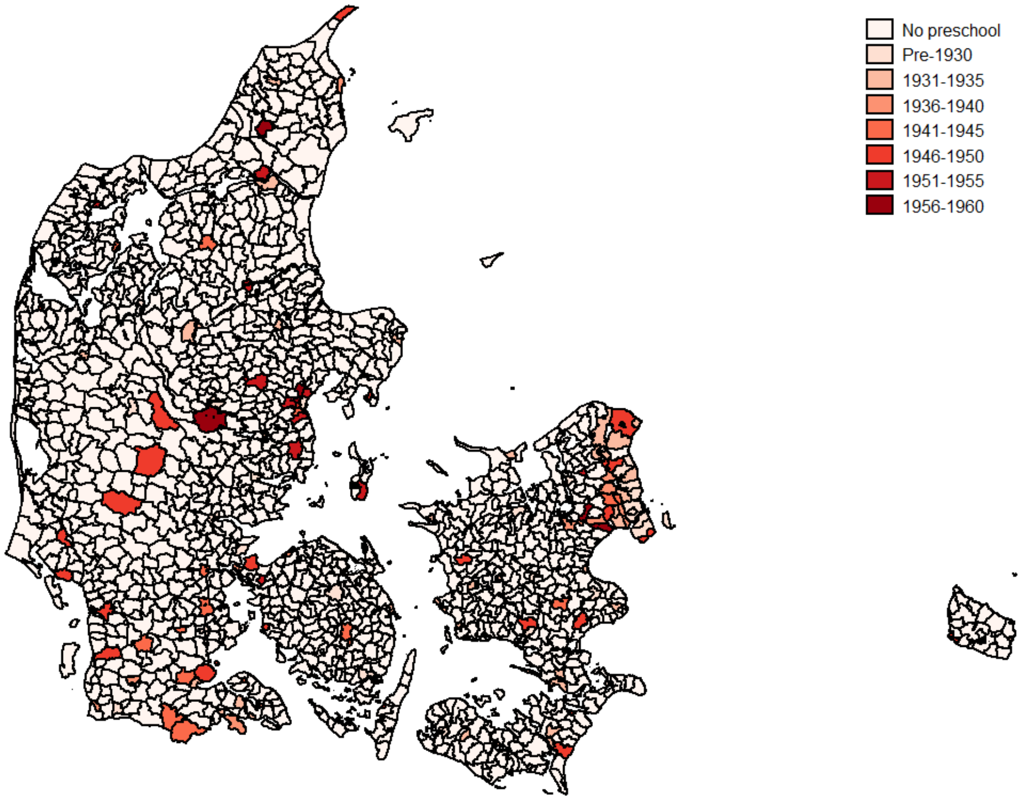
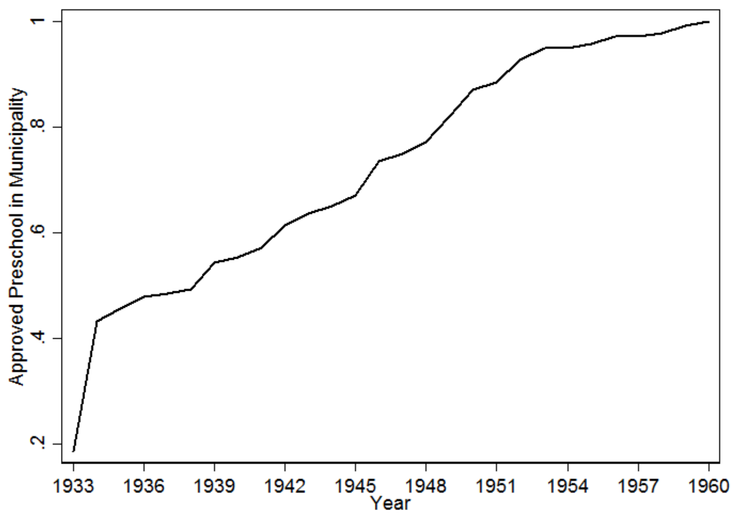
Figure 1: Map of Danish Municipalities with an Approved Preschool by 1960



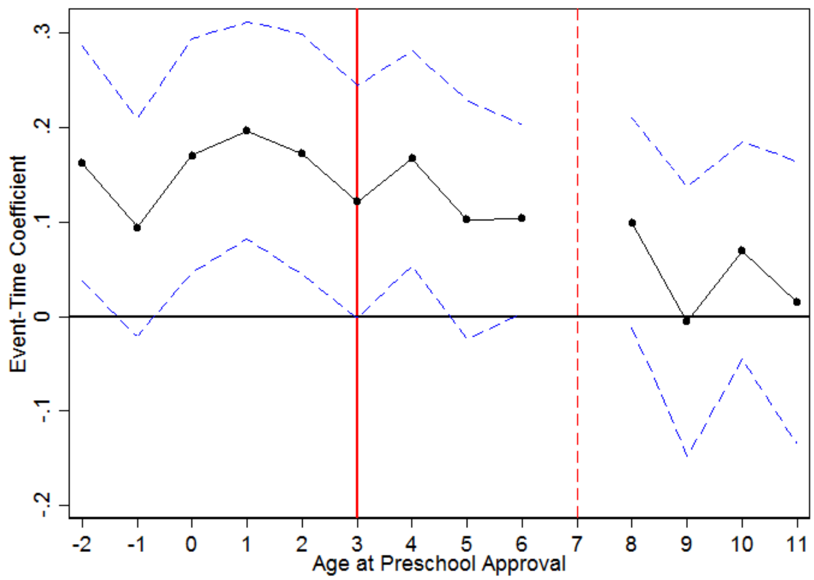
Notes: This map shows the evolution of preschool approvals across Danish municipalities through 1960. Our analysis sample is limited to the 140 municipalities that ever had an approved preschool by 1960.

Figure 2: Percent of Municipalities with an Approved Preschool by Year



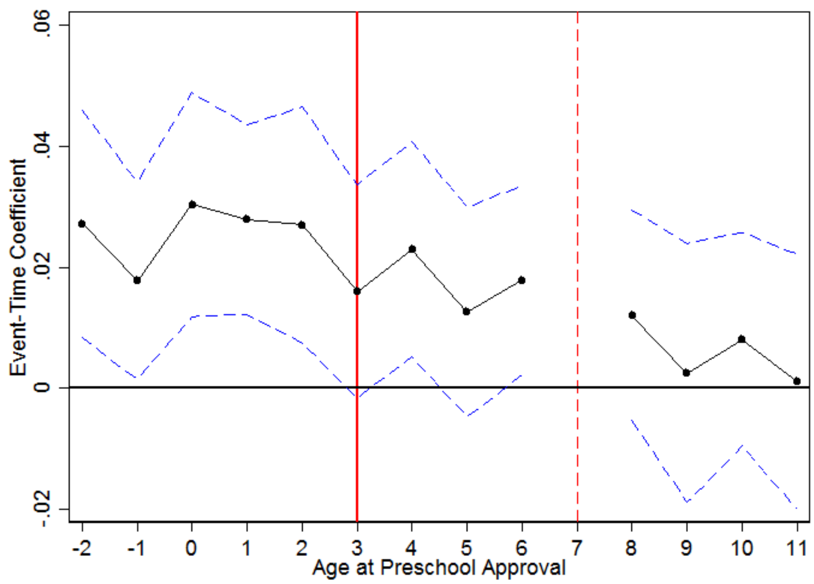
Notes: The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. This graph shows the percent of municipalities that had an approved preschool in each year.

Figure 3: Effect of Access to Preschool on Years of Education at Age 50 by Age of Exposure



Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The regression includes indicators for the cohorts' single years of age in the year of the preschool approval in their municipality of birth between -2 and 11 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born more than two years after the preschool approval (i.e., age less than -2) and an indicator for cohorts being older than age 11 at the time of approval. The regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. The regression also includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. The regression is weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Figure 4: Effect of Access to Preschool on Indicator for More than Compulsory Education at Age 50 by Age of Exposure



Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The regression includes indicators for the cohorts' single years of age in the year of the preschool approval in their municipality of birth between -2 and 11 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born more than two years after the preschool approval (i.e., age less than -2) and an indicator for cohorts being older than age 11 at the time of approval. The regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. The regression also includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. The regression is weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Table 1: Municipality Characteristics in 1929-1930

	(1) All Munis	(2) Ever Approved Preschool	(3) No Approved Preschool
Avg. Population	2650.0	13629.5	1383.9
Pct Female	48.76	51.47	48.43
Pct Social Demo	25.55	46.72	23.05
Pct Radical Lib	14.47	8.453	15.18
Pct Agrarian Lib	47.29	21.09	50.39
Pct Conservatives	9.761	18.90	8.680
Pct Industrial	17.39	35.27	15.28
Pct Urban	19.99	80.90	12.78
Pct Agricultural	57.08	17.62	61.75
Rural	0.938	0.521	0.986
Pct Paying Income Tax	23.72	28.48	23.16
Log Taxable Income	6.585	8.276	6.385
Pct Paying Property Tax	5.806	5.254	5.871
Num. Munis	1,354	140	1,214

Notes: Column (1) reports the means of municipality characteristics for all Danish municipalities with available data. Column (2) limits the sample to the 140 municipalities that ever had an approved preschool by 1960. Column (3) limits the sample to the other municipalities that never had an approved preschool by 1960.

Table 2: Correlation Between Municipality Characteristics and Timing of Preschool Approval

A. With County-Specific Linear Trends	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log Pop	Pct Fem	Pct Urb	Pct Ind	Pct Ag	Pct Inc Tax	Log Tax Inc	Pct Prop Tax
Any Approved	0.0155	0.135	1.351**	-0.366	-1.404***	0.402	-0.0366	-0.243**
Preschool	[0.0244]	[0.150]	[0.664]	[0.642]	[0.390]	[0.464]	[0.0394]	[0.0998]
Mean, dept. var.	10.96	52.36	96.96	46.26	5.537	36.99	11.29	5.224
Observations (cells)	3918	3918	3918	3918	3918	3918	3918	3918
B. With Rural/Urban Municipality Trends								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log Pop	Pct Fem	Pct Urb	Pct Ind	Pct Ag	Pct Inc Tax	Log Tax Inc	Pct Prop Tax
Any Approved	0.0611	-0.312	0.725	0.424	-0.621*	-0.0818	-0.0858	-0.417*
Preschool	[0.0524]	[0.258]	[0.532]	[0.698]	[0.354]	[0.720]	[0.0565]	[0.236]
Mean, dept. var.	10.96	52.36	96.96	46.26	5.537	36.99	11.29	5.224
Observations (cells)	3918	3918	3918	3918	3918	3918	3918	3918

Notes: Each coefficient is from a separate regression. The outcomes are the following time-varying municipality characteristics (interpolated for years without data): log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, and percent paying property tax. The units of analysis are municipality×birth-year cells. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions include municipality and year-of-birth fixed effects. Panel A also includes county-specific linear time trends. Panel B instead includes linear time trends interacted with rural/urban municipality dummies. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 3: Correlation Between Access to Preschool at Age 3 and **Predicted** Outcomes

	(1) Yrs. School	(2) More than Comp.	(3) Log Wage Inc	(4) Survival beyond age 65
Any Approved Preschool at Age 3	-0.00692 [0.0350]	0.000162 [0.00386]	-0.00423 [0.00707]	-0.00281* [0.00169]
Observations (cells)	3918	3918	3918	3918

Notes: Each coefficient is from a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort. Then, as outcomes, we use predicted variables from a regression of each conditional outcome on the following time-varying municipality characteristics: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, and percent paying property tax. The units of analysis are municipality×birth-year cells. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions include municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 4: Correlation between Access to the NHV at Birth and Access to Preschool at Age 3

	(1) NHV at Birth	(2) Any Approved Preschool at Age 3
Any Approved Preschool at Age 3	0.00862 [0.0433]	
NHV at Birth		0.00578 [0.0292]
Mean, dept. var.	0.733	0.909
Observations (cells)	3918	3918

Notes: Each column reports the results from a separate regression. The units of analysis are municipality×birth-year cells. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions include municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 5: Effect of Access to Preschool at Age 3 on Education, Income, and Survival

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Yrs. School	0.281*** (0.087)	0.127** (0.052)	0.073** (0.034)	0.092** (0.042)
Mean of dep. var.	12.075	12.075	12.075	12.075
No. of obs.	3918	3918	3918	3918
More than Compulsory Educ.	0.037*** (0.011)	0.018*** (0.006)	0.013*** (0.004)	0.014** (0.006)
Mean of dep. var.	0.701	0.701	0.701	0.701
No. of obs.	3918	3918	3918	3918
Log Avg Age 30-60 Wage Inc	0.060*** (0.019)	0.035*** (0.012)	0.016* (0.009)	0.018** (0.009)
Mean of dep. var.	12.230	12.230	12.230	12.230
No. of obs.	3918	3918	3918	3918
Survival beyond Age 65	0.005** (0.002)	0.005*** (0.002)	0.005** (0.002)	0.005* (0.002)
Mean of dep. var.	0.903	0.903	0.903	0.903
No. of obs.	3918	3918	3918	3918
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County $\times$ Year FE	No	No	No	Yes

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. The units of analysis are municipality$\times$birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality$\times$birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality$\times$birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions are weighted by the number of observations in each municipality$\times$birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 6: Effect of Access to Preschool at Age 3 on the Education of the Next Generation

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Child's Years of Schooling	0.056**	0.036	0.031	0.027
	(0.025)	(0.022)	(0.022)	(0.024)
Mean of dep. var.	12.338	12.338	12.338	12.338
No. of obs.	3197	3197	3197	3197
Child Has More than	0.022***	0.016***	0.012**	0.010*
Compulsory Ed.	(0.006)	(0.005)	(0.005)	(0.005)
Mean of dep. var.	0.775	0.775	0.775	0.775
No. of obs.	3197	3197	3197	3197
Child Has Completed	0.023***	0.017***	0.007	0.005
Gymnasium	(0.007)	(0.006)	(0.005)	(0.006)
Mean of dep. var.	0.253	0.253	0.253	0.253
No. of obs.	3197	3197	3197	3197
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County × Year FE	No	No	No	Yes

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on month-of-birth indicators for the first generation, as well municipality×birth-year (of the first generation) fixed effects. We thus obtain conditional mean second generation outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

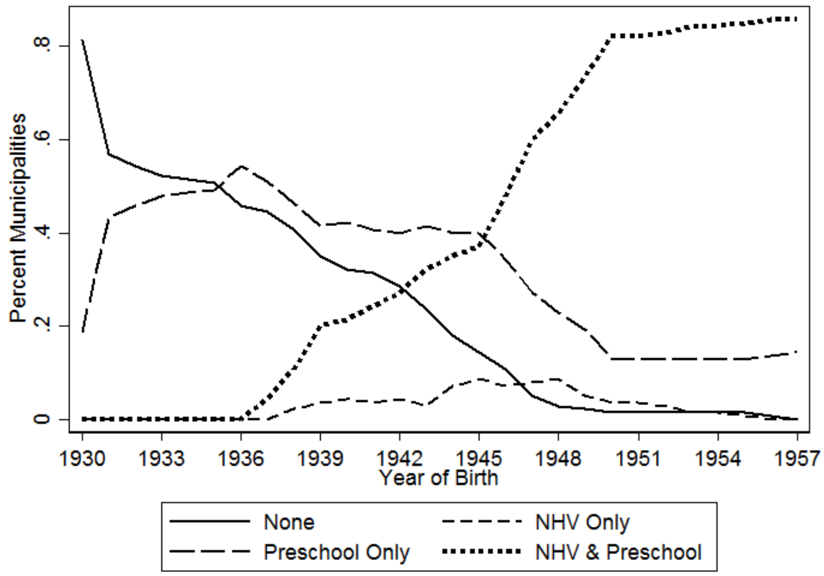
Table 7: Interaction Effect between Access to NHV at Birth and Access to Preschool at Age 3 on Education, Income, and Survival

	(1) Yrs. School	(2) More than Comp	(3) Log Wage Inc	(4) Survival beyond age 65
Any Approved	0.0892**	0.0155***	0.0180*	0.00639**
Preschool at Age 3	[0.0392]	[0.00528]	[0.0104]	[0.00251]
NHV at Birth	0.104**	0.0128**	0.0109	0.00649**
	[0.0418]	[0.00552]	[0.0155]	[0.00263]
Preschool x NHV	-0.0808**	-0.0108**	-0.0108	-0.00504*
	[0.0405]	[0.00526]	[0.0160]	[0.00262]
Mean, dept. var.	12.07	0.701	12.23	0.903
Observations (cells)	3918	3918	3918	3918

Notes: Each column reports the results from a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions include municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. All regressions include municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

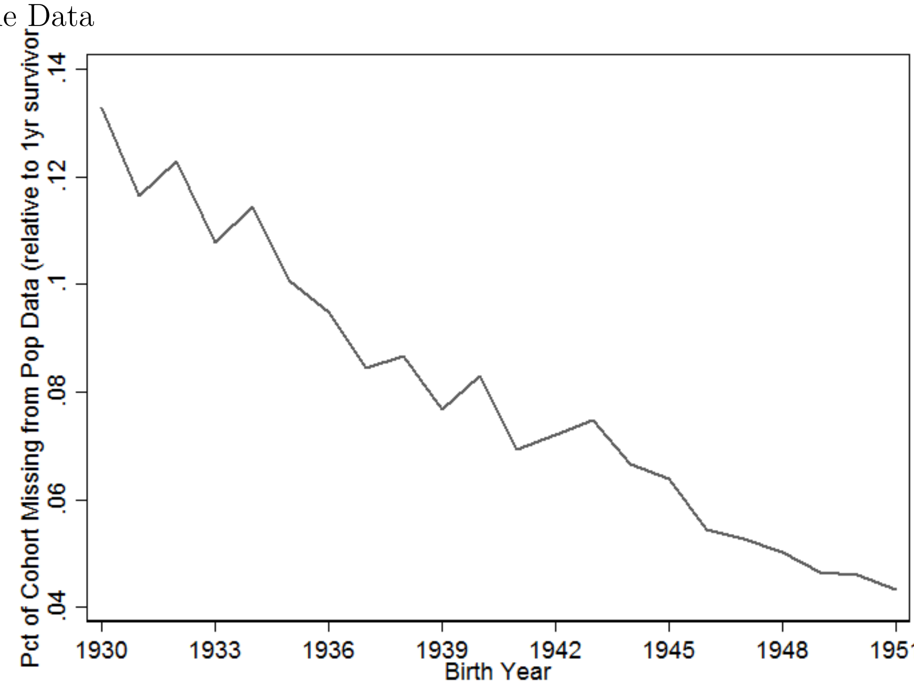
A Additional Results

Appendix Figure 1: Variation in Preschool and NHV Availability by Year of Birth

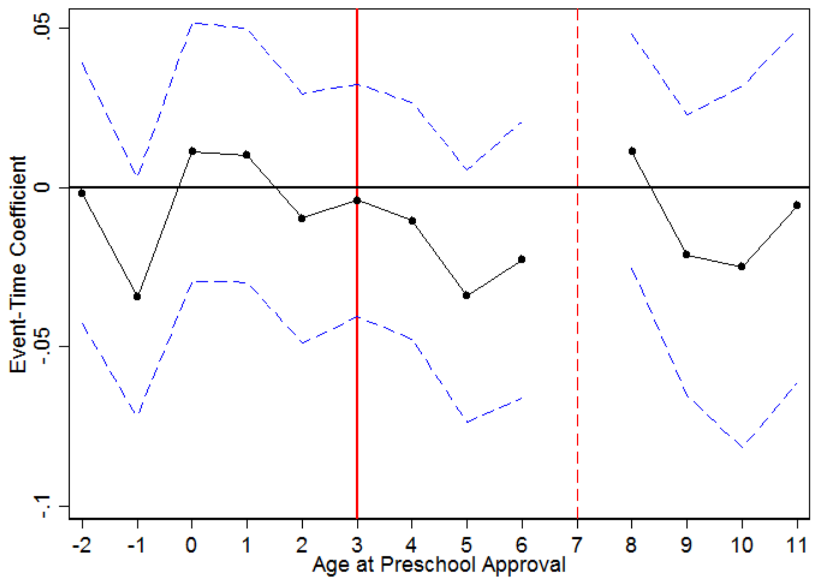


Notes: This graph shows for each cohort the percent of municipalities that had: (1) no preschool at age 3 and no NHV at birth in red; (2) preschool at age 3 but no NHV at birth in green; (3) NHV at birth but no preschool at age 3 in blue; and (4) preschool at age 3 and NHV at birth in orange. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960.

Appendix Figure 2: Comparison of First-Year Survivors to All Danish-Born Individuals in the Outcome Data

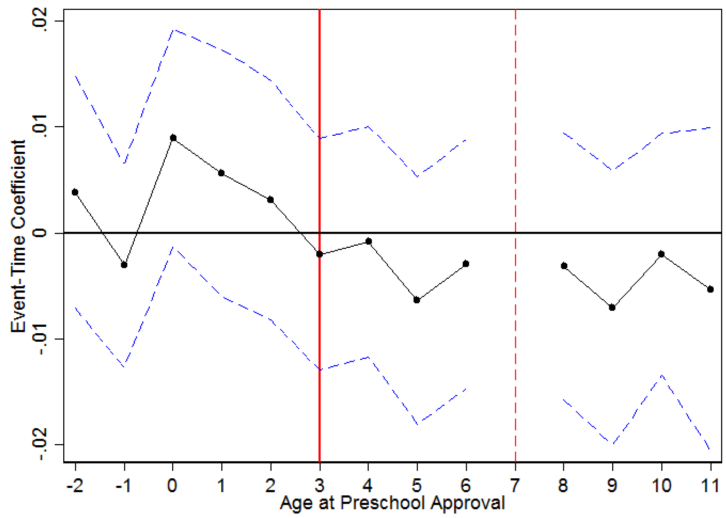


Appendix Figure 3: Effect of Access to Preschool on Log Mean Wage Income between Ages 30 and 60 by Age of Exposure



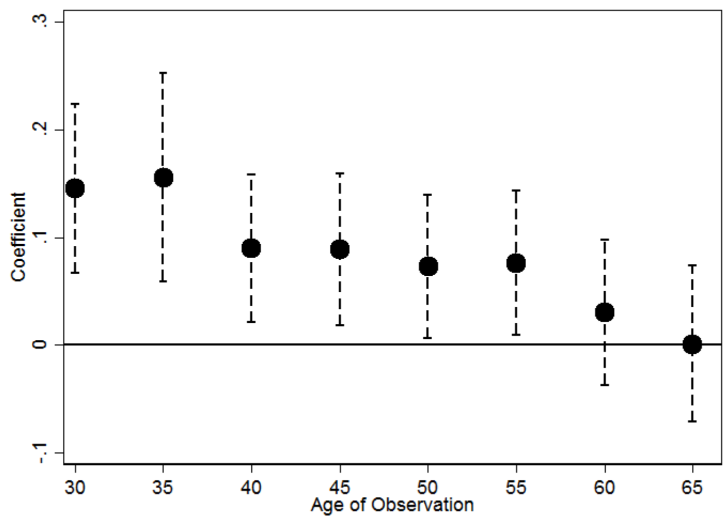
Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The regression includes indicators for the cohorts' single years of age in the year of the preschool approval in their municipality of birth between -2 and 11 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born more than two years after the preschool approval (i.e., age less than -2) and an indicator for cohorts being older than age 11 at the time of approval. The regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. The regression also includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. The regression is weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Appendix Figure 4: Effect of Access to Preschool on Survival beyond Age 65 by Age of Exposure



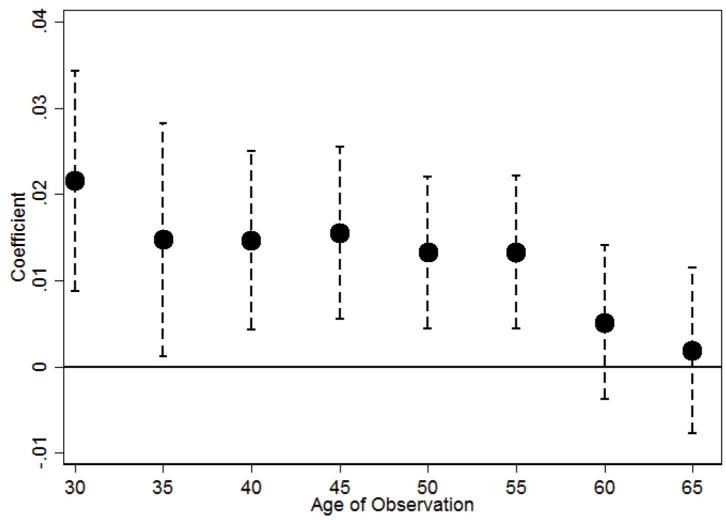
Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The regression includes indicators for the cohorts' single years of age in the year of the preschool approval in their municipality of birth between -2 and 11 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born more than two years after the preschool approval (i.e., age less than -2) and an indicator for cohorts being older than age 11 at the time of approval. The regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. The regression also includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. The regression is weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Appendix Figure 5: Effect of Access to Preschool at Age 3 on Years of Schooling by Age of Follow-Up



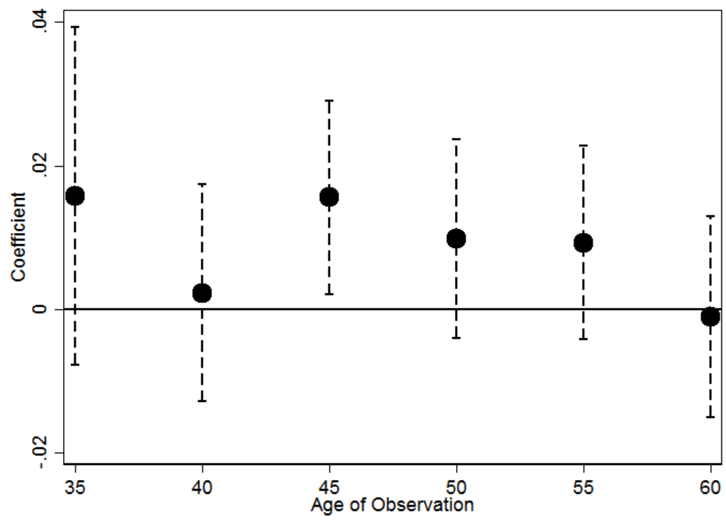
Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The coefficients plotted estimate the effect of access to preschool at age 3 on the outcome listed observed at the age reported on the x-axis. Each regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. Each regression includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Appendix Figure 6: Effect of Access to Preschool at Age 3 on Indicator for More than Compulsory Education by Age of Follow-Up



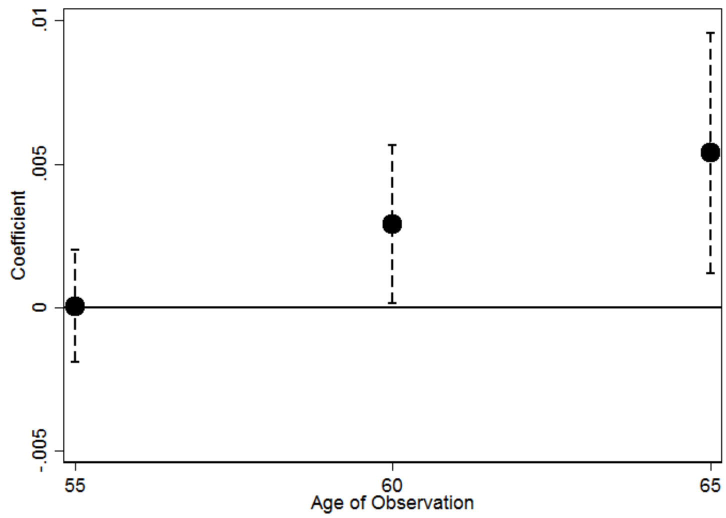
Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The coefficients plotted estimate the effect of access to preschool at age 3 on the outcome listed observed at the age reported on the x-axis. Each regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. Each regression includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Appendix Figure 7: Effect of Access to Preschool at Age 3 on Log Wage Income by Age of Follow-Up



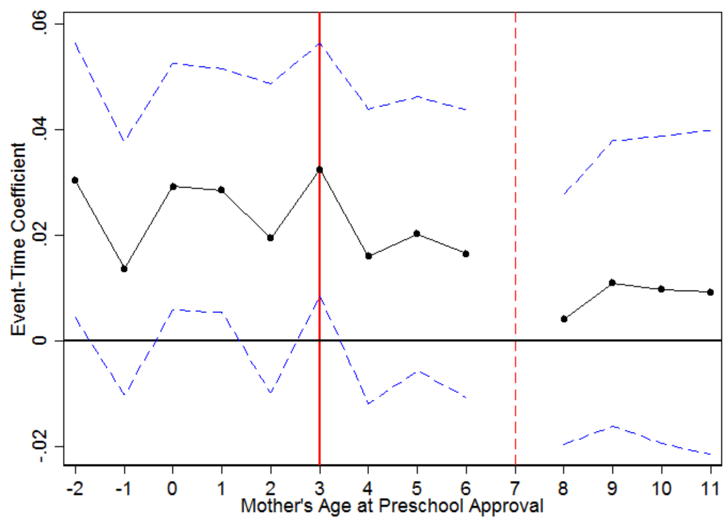
Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The coefficients plotted estimate the effect of access to preschool at age 3 on the outcome listed observed at the age reported on the x-axis. Each regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. Each regression includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Appendix Figure 8: Effect of Access to Preschool at Age 3 on Survival by Age of Follow-Up



Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. Additionally, the sample is limited to only those individuals who have survived to at least age 50. The coefficients plotted estimate the effect of access to preschool at age 3 on the outcome listed observed by the age reported on the x-axis. Each regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. Each regression includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Appendix Figure 9: Effect of Access to Preschool on Indicator for More than Compulsory Education **for the Next Generation** by Age of Exposure



Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year collapsed data. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on month-of-birth indicators (of the first generation), as well municipality×birth-year fixed effects (of the first generation). We thus obtain conditional mean second generation outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. The regression includes indicators for the maternal cohorts' single years of age in the year of the preschool approval in their municipality of birth between -2 and 11 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born more than two years after the preschool approval (i.e., age less than -2) and an indicator for cohorts being older than age 11 at the time of approval. The regression includes municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. The regression also includes municipality and year-of-birth fixed effects as well as county-specific linear time trends. The regression is weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Appendix Table 1: Correlation between Share of Cohort "Not Missing" and Access to Preschool; Urban Municipalities

	(1) (Urb.)	(2) (Urb.)	(3) (Urb.)	(4) (Urb.)
Any Approved Preschool at Age 3	-0.064	-0.015	0.112	0.167
	(0.160)	(0.093)	(0.085)	(0.106)
Mean of dep. var.	0.921	0.921	0.921	0.921
No. of obs.	1548	1548	1548	1548
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County $\times$ Year FE	No	No	No	Yes

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. The units of analysis are municipality×birth-year cells. The sample is limited to the 86 urban municipalities that ever had an approved preschool by 1960. The outcome is the ratio of observations in our outcome data to the number of 1-year survivors (i.e., # of live births - # infant deaths) in each municipality×year cell. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 2: Effect of Access to Preschool at Age 3 on Different Adult Income Measures

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Avg Age 30-60 Wage Inc	7706.289**	3513.988*	1439.914	2207.618
	(3290.127)	(2027.730)	(1504.928)	(1582.414)
Mean of dep. var.	273662.9	273662.9	273662.9	273662.9
No. of obs.	3918	3918	3918	3918
Log Age 30-60 PDV Wage Inc	0.060***	0.036***	0.016*	0.020**
	(0.018)	(0.011)	(0.009)	(0.009)
Mean of dep. var.	14.831	14.831	14.831	14.831
No. of obs.	3918	3918	3918	3918
Log Avg Age 30-60 Tot Inc	0.042***	0.026***	0.010	0.015***
	(0.014)	(0.009)	(0.006)	(0.006)
Mean of dep. var.	12.531	12.531	12.531	12.531
No. of obs.	3918	3918	3918	3918
Log Avg Age 49-51 Wage Inc	0.032***	0.019**	0.008	0.018**
	(0.011)	(0.009)	(0.008)	(0.008)
Mean of dep. var.	12.429	12.429	12.429	12.429
No. of obs.	3777	3777	3777	3777
Any Wage Inc., Age 49-51	0.009**	0.006*	0.004	0.003
	(0.004)	(0.003)	(0.002)	(0.002)
Mean of dep. var.	0.898	0.898	0.898	0.898
No. of obs.	3777	3777	3777	3777
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County × Year FE	No	No	No	Yes

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 3: Robustness: Effect of Access to Preschool at Age 3 on Education, Income, and Survival

Outcome	(1) (Balanced)	(2) (All)	(3) (All)	(4) (No CPH)	(5) (Edu. Ref.)	(6) (No Already App.)	(7) (1937-1957)	(8) (Ever-NHV)	(9) (Frac Yrs)
Yrs. School	0.073**	0.103**	0.076	0.074**	0.117**	0.049	0.090**	0.066*	0.148***
	(0.034)	(0.048)	(0.049)	(0.034)	(0.049)	(0.032)	(0.036)	(0.036)	(0.048)
Mean of dep. var.	12.075	12.075	12.075	12.038	12.075	11.824	12.236	12.095	12.075
No. of obs.	3864	3918	3918	3890	3918	3190	2939	3359	3918
More than Compulsory Educ.	0.013***	0.014**	0.010	0.013***	0.016***	0.010**	0.015***	0.012**	0.023***
	(0.004)	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Mean of dep. var.	0.701	0.701	0.701	0.695	0.701	0.672	0.720	0.703	0.701
No. of obs.	3864	3918	3918	3890	3918	3190	2939	3359	3918
Log Avg Age 30-60 Wage Inc	0.016*	0.033***	0.027**	0.014	0.036***	0.006	0.020**	0.012	0.015
	(0.009)	(0.011)	(0.011)	(0.009)	(0.012)	(0.010)	(0.009)	(0.010)	(0.010)
Mean of dep. var.	12.230	12.230	12.230	12.225	12.230	12.202	12.264	12.231	12.230
No. of obs.	3864	3918	3918	3890	3918	3190	2939	3359	3918
Survival beyond Age 65	0.005**	0.005***	0.006***	0.005**	0.005***	0.005**	0.002	0.006***	0.006**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Mean of dep. var.	0.903	0.903	0.903	0.906	0.903	0.906	0.912	0.903	0.903
No. of obs.	3864	3918	3918	3890	3918	3190	2939	3359	3918
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trends									
County	Yes	No	No	Yes	No	Yes	Yes	Yes	Yes
Urban/Rural	No	Yes	No	No	No	No	No	No	No
Urban × County	No	No	Yes	No	No	No	No	No	No

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.

Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Appendix Table 4: Effect of Access to Preschool at Age 3 on Education, Income, and Survival; Individual-level Micro-Data

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Yrs. School	0.280***	0.125**	0.072**	0.091**
	(0.085)	(0.050)	(0.033)	(0.037)
Mean of dep. var.	12.064	12.064	12.064	12.064
No. of obs.	820197	820197	820197	820197
More than Compulsory Educ.	0.037***	0.017***	0.013***	0.014***
	(0.011)	(0.006)	(0.004)	(0.005)
Mean of dep. var.	0.696	0.696	0.696	0.696
No. of obs.	820197	820197	820197	820197
Log Avg Age 30-60 Wage Inc.	0.039***	0.022**	0.009	0.014**
	(0.013)	(0.009)	(0.007)	(0.006)
Mean of dep. var.	12.556	12.556	12.556	12.556
No. of obs.	782985	782985	782985	782985
Survival beyond Age 65	0.005**	0.005***	0.005**	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
Mean of dep. var.	0.903	0.903	0.903	0.903
No. of obs.	879647	879647	879647	879647
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County × Year FE	No	No	No	Yes

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. The units of analysis are individual-level observations. The sample is limited to individuals born in the 140 municipalities that ever had an approved preschool by 1960. All regressions include indicators for gender and month of birth. Standard errors are clustered on the municipality level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 5: Effect of Access to Preschool at Age 3 on Education, Income, and Survival: Using Year of Establishment

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Yrs. School	0.343***	0.169***	0.123***	0.119**
	(0.093)	(0.060)	(0.043)	(0.053)
Mean of dep. var.	12.075	12.075	12.075	12.075
No. of obs.	3918	3918	3918	3918
More than Compulsory Educ.	0.046***	0.023***	0.019***	0.017**
	(0.012)	(0.007)	(0.006)	(0.007)
Mean of dep. var.	0.701	0.701	0.701	0.701
No. of obs.	3918	3918	3918	3918
Log Avg Age 30-60 Wage Inc	0.066***	0.039***	0.023**	0.018*
	(0.018)	(0.012)	(0.009)	(0.010)
Mean of dep. var.	12.230	12.230	12.230	12.230
No. of obs.	3918	3918	3918	3918
Survival beyond Age 65	0.005**	0.006***	0.005**	0.004
	(0.002)	(0.002)	(0.002)	(0.002)
Mean of dep. var.	0.903	0.903	0.903	0.903
No. of obs.	3918	3918	3918	3918
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County × Year FE	No	No	No	Yes

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. Treatment here is defined using the first year of establishment rather than approval. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Appendix Table 6: "First Stage": Access to Preschool and Share of Children Aged 3-6 Enrolled; 1939-1950; Urban Municipalities

	Share Enrolled, Ages 3-6		
	(1)	(2)	(3)
	1939-50, All Urb.	No Non-App. Preschool	1939-1950, Switchers, Urb.
Any Approved Preschool	0.0829** [0.0349]	0.136** [0.0581]	0.107 [0.0692]
Mean, dept. var.	0.0936	0.0938	0.0874
Observations (cells)	1032	924	216

Notes: Each cell presents the coefficient for the treatment indicator for a separate regression. The units of analysis are municipality×birth-year cells. The sample is limited to the 86 urban municipalities that ever had an approved preschool by 1960, observed in years 1939-1950. Column (2) further drops municipalities that had at least one established but not approved preschool over this time period. Column (3) only uses municipalities that approved a preschool between 1939 and 1950. The outcome is the share of children aged 3-6 who are enrolled in preschool. To calculate this variable, we use data on the number of children enrolled in each preschool in each of the nine years of book publications, interpolate to get estimates of enrollment in every year, and then aggregate to the municipality×year level. We then use data on the number of survivers past age one in each of the 86 urban municipalities as the denominator for years 1939-1950. We begin in 1939 since that is the first year when we can observe all living 6-year-olds (as our earliest data on births are from 1933). Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 7: Effect of Access to Preschool at Age 3 on the Fertility Outcomes of Females born in 1935-1957

	(1)	(2)	(3)	(4)
	No Kids	Num. Kids	Age at Fst. Birth	Dad Ever Miss.
Any Approved Preschool at Age 3	-0.00519 [0.00354]	0.000590 [0.0139]	0.0800* [0.0454]	-0.00302 [0.00344]
Mean, dept. var.	0.115	1.904	24.15	0.154
Observations (cells)	3207	3207	3202	3207

Notes: Each column reports the results from a separate regression. The sample is limited to females who were born in 1935-1957 in the 140 municipalities that ever had an approved preschool by 1960. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. All regressions include municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. All regressions include municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 8: Effect of NHV on Long-Term Health in Preschool Analysis Sample

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Hospital Nights, Age 55-64	-0.285**	-0.252*	-0.273*	-0.275
	(0.139)	(0.138)	(0.158)	(0.188)
Mean of dep. var.	8.596	8.596	8.596	8.596
No. of obs.	3918	3918	3918	3918
Cardio Diagnosis by Age 60	-0.006**	-0.005*	-0.004	-0.006*
	(0.002)	(0.003)	(0.003)	(0.003)
Mean of dep. var.	0.228	0.228	0.228	0.228
No. of obs.	3918	3918	3918	3918
Heart Condition Diagnosis by Age 60	-0.004***	-0.002*	-0.002*	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Mean of dep. var.	0.061	0.061	0.061	0.061
No. of obs.	3918	3918	3918	3918
Diabetes Diagnosis by Age 60	-0.001	-0.001	-0.001	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Mean of dep. var.	0.041	0.041	0.041	0.041
No. of obs.	3918	3918	3918	3918
Cancer Diagnosis by Age 60	-0.001	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Mean of dep. var.	0.076	0.076	0.076	0.076
No. of obs.	3918	3918	3918	3918
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County $\times$ Year FE	No	No	No	Yes

Notes: Each cell presents the coefficient for the treatment indicator—which is equal to 1 if the NHV program was operating in a given municipality$\times$birth-year and 0 otherwise—for a separate regression. The units of analysis are municipality$\times$birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality$\times$birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality$\times$birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions are weighted by the number of observations in each municipality$\times$birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 9: Main effects of Access to Preschool and to NHV on on Education, Income, and Survival

Outcome/Program	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Yrs. School: Preschool	0.279***	0.125**	0.072**	0.090**
	(0.086)	(0.051)	(0.034)	(0.041)
Yrs. School: NHV	0.037	0.033	0.031	0.042
	(0.033)	(0.026)	(0.027)	(0.042)
Mean of dep. var.	12.075	12.075	12.075	12.075
No. of obs.	3918	3918	3918	3918
More than Compulsory Educ.: Preschool	0.037***	0.017***	0.013***	0.014**
	(0.011)	(0.006)	(0.004)	(0.006)
More than Compulsory Educ.: NHV	0.002	0.004	0.003	0.003
	(0.004)	(0.003)	(0.003)	(0.005)
Mean of dep. var.	0.701	0.701	0.701	0.701
No. of obs.	3918	3918	3918	3918
Log Avg Age 30-60 Wage Inc: Preschool	0.060***	0.035***	0.016*	0.018**
	(0.019)	(0.012)	(0.009)	(0.009)
Log Avg Age 30-60 Wage Inc: NHV	0.007	0.002	0.001	0.012
	(0.008)	(0.008)	(0.008)	(0.009)
Mean of dep. var.	12.230	12.230	12.230	12.230
No. of obs.	3918	3918	3918	3918
Survival beyond Age 65: Preschool	0.005**	0.005***	0.005**	0.004*
	(0.002)	(0.002)	(0.002)	(0.003)
Survival beyond Age 65: NHV	0.003	0.001	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Mean of dep. var.	0.903	0.903	0.903	0.903
No. of obs.	3918	3918	3918	3918
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County × Year FE	No	No	No	Yes

Notes: Each panel presents the coefficients for the two treatment indicators (preschool and NHV) included in the same regression (without the interaction). The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 10: Main effects of Access to Preschool and to NHV on Long-Term Health Outcomes

Outcome/Program	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Hosp. Age 55-64: Preschool	-0.475**	-0.431**	-0.377*	-0.456**
	(0.213)	(0.205)	(0.199)	(0.194)
Hosp. Age 55-64: NHV	-0.266*	-0.236*	-0.263*	-0.248
	(0.137)	(0.139)	(0.158)	(0.189)
Mean of dep. var.	8.596	8.596	8.596	8.596
No. of obs.	3918	3918	3918	3918
Diagnosed Cardio: Preschool	0.003	0.003	0.002	0.001
	(0.003)	(0.003)	(0.002)	(0.003)
Diagnosed Cardio: NHV	-0.006**	-0.005**	-0.004	-0.006*
	(0.002)	(0.003)	(0.003)	(0.003)
Mean of dep. var.	0.228	0.228	0.228	0.228
No. of obs.	3918	3918	3918	3918
Diagnosed Heart: Preschool	-0.004**	-0.003*	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Diagnosed Heart: NHV	-0.003**	-0.002*	-0.002*	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)
Mean of dep. var.	0.061	0.061	0.061	0.061
No. of obs.	3918	3918	3918	3918
Diagnosed Diabetes: Preschool	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Diagnosed Diabetes: NHV	-0.001	-0.001	-0.001	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Mean of dep. var.	0.041	0.041	0.041	0.041
No. of obs.	3918	3918	3918	3918
Diagnosed Cancer: Preschool	-0.002	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.002)	(0.002)
Diagnosed Cancer: NHV	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)
Mean of dep. var.	0.076	0.076	0.076	0.076
No. of obs.	3918	3918	3918	3918
Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Muni Controls (ipolated)	No	Yes	Yes	Yes
Linear County Trends	No	No	Yes	No
County × Year FE	No	No	No	Yes

Notes: Each panel presents the coefficients for the two treatment indicators (preschool and NHV) included in the same regression (without the interaction). The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 11: Interaction Effect between Access to NHV at Birth and Access to Preschool at Age 3 on Education, Income, and Survival; No Time Trends

	(1) Yrs. School	(2) More than Comp	(3) Log Wage Inc	(4) Survival beyond age 65
Any Approved	0.148**	0.0203***	0.0386***	0.00624***
Preschool at Age 3	[0.0577]	[0.00678]	[0.0136]	[0.00222]
NHV at Birth	0.138***	0.0180***	0.0199	0.00565**
	[0.0474]	[0.00609]	[0.0157]	[0.00249]
Preschool x NHV	-0.116**	-0.0149**	-0.0202	-0.00473**
	[0.0500]	[0.00594]	[0.0168]	[0.00237]
Mean, dept. var.	12.07	0.701	12.23	0.903
Observations (cells)	3918	3918	3918	3918

Notes: Each column reports the results from a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions include municipality and year-of-birth fixed effects. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 12: Interaction Effect between Access to NHV at Birth and Access to Preschool at Age 3 on Education, Income, and Survival; Rural/Urban Time Trends

	(1) Yrs. School	(2) More than Comp	(3) Log Wage Inc	(4) Survival beyond age 65
Any Approved Preschool at Age 3	0.123**	0.0168***	0.0370***	0.00630***
	[0.0539]	[0.00622]	[0.0132]	[0.00225]
NHV at Birth	0.128***	0.0166***	0.0192	0.00568**
	[0.0456]	[0.00582]	[0.0155]	[0.00248]
Preschool x NHV	-0.109**	-0.0139**	-0.0197	-0.00474**
	[0.0483]	[0.00567]	[0.0167]	[0.00237]
Mean, dept. var.	12.07	0.701	12.23	0.903
Observations (cells)	3918	3918	3918	3918

Notes: Each column reports the results from a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions include municipality and year-of-birth fixed effects as well as linear time trends interacted with rural/urban dummies. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level. Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 13: Interaction Effect between Access to NHV at Birth and Access to Preschool at Age 3 on Education, Income, and Survival; Drop Post-1949 Cohorts in 28 Municipalities with Worse NHV Data

	(1) Yrs. School	(2) More than Comp	(3) Log Wage Inc	(4) Survival beyond age 65
Any Approved	0.0821**	0.0145***	0.0168	0.00650**
Preschool at Age 3	[0.0385]	[0.00509]	[0.0108]	[0.00258]
NHV at Birth	0.0976**	0.0120**	0.0105	0.00633**
	[0.0420]	[0.00552]	[0.0154]	[0.00269]
Preschool x NHV	-0.0788*	-0.0105**	-0.0110	-0.00496*
	[0.0403]	[0.00516]	[0.0160]	[0.00263]
Mean, dept. var.	12.07	0.701	12.23	0.903
Observations (cells)	3694	3694	3694	3694

Notes: Each column reports the results from a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. For the 28 municipalities that do not establish NHV by 1949 in our data, we drop cohorts born in 1950-1957 since we do not have precise information on NHV initiation in those years. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions include municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 14: Interaction Effect between Access to NHV at Birth and Access to Preschool at Age 3 on Education, Income, and Survival; Drop NHV-Treated Observations at Bottom of Education Distribution

	(1) Yrs. School	(2) More than Comp	(3) Log Wage Inc	(4) Survival beyond age 65
Any Approved	0.0884**	0.0154***	0.0116	0.00618**
Preschool at Age 3	[0.0390]	[0.00529]	[0.00853]	[0.00250]
NHV at Birth	0.106**	0.0130**	0.0229**	0.00621**
	[0.0424]	[0.00567]	[0.0102]	[0.00263]
Preschool x NHV	-0.0817**	-0.0110**	-0.0110	-0.00473*
	[0.0409]	[0.00536]	[0.00975]	[0.00263]
Mean, dept. var.	12.08	0.702	12.56	0.903
Observations (cells)	3918	3918	3778	3918

Notes: Each column reports the results from a separate regression. The units of analysis are municipality×birth-year cells. Before collapsing, we estimate an auxiliary regression on the individual-level data, where we regress each outcome on gender and month-of-birth indicators, as well municipality×birth-year fixed effects. We thus obtain conditional mean outcomes for each municipality×birth-year cohort, and use them as dependent variables. Additionally, before collapsing the data, we randomly drop one percent of NHV-treated individual observations with seven years of schooling or less (i.e., those with the lowest education in our data). The sample is limited to the 140 municipalities that ever had an approved preschool by 1960. When studying survival beyond age 65, the sample is limited to only those individuals who have survived to at least age 50. All regressions include municipality×year controls (interpolated for years without data) for: log population, percent female, percent urban, percent industrial, percent agricultural, percent paying income tax, log taxable income, percent paying property tax, percent voting for the social democratic party, percent voting for the radical liberal party, percent voting for the agrarian liberal party, and percent voting for the conservative party. All regressions include municipality and year-of-birth fixed effects as well as county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01