Are Different Early Investments Complements or Substitutes? Long-Run and Intergenerational Evidence from Denmark*

Maya Rossin-Slater†

Miriam Wüst‡

June 2016

Abstract

This paper presents quasi-experimental evidence on interactions between two different early-life investments: high quality preschool childcare and a nurse home visiting (NHV) program. We use administrative and historical data from Denmark together with variation in the timing of program implementation across municipalities between 1933 and 1960. Our results point to lasting benefits of access to childcare at age 3 on outcomes through age 65—educational attainment increases, income rises (for men), and the probability of survival increases (for women). Further, the benefits persist to the next generation, who have higher educational attainment by age 25. However, the interaction effects between childcare and NHV are *negative*, suggesting that the two interventions are substitutes rather than complements. Cohorts who did *not* receive NHV at birth experience larger positive impacts of childcare than those who did—NHV reduces the effect of childcare by 85 percent for years of schooling (of the first generation) and by 86 percent for adult income among men.

*We thank Hoyt Bleakley, Shelly Lundberg, Michael Mueller-Smith, Jeffrey Smith, and seminar participants at UC Santa Barbara, University of Iowa, the University of Michigan, the University of Copenhagen, and Santa Clara University for helpful comments. We are grateful to Peder Dam and the "DigDag" project for invaluable help with the data on Denmark's historical administrative structure. Ida Lykke Kristiansen provided excellent research assistance. The Danish Data Archive provided the data from the "Statistical Commune Data Archive." We gratefully acknowledge financial support from the Danish Council for Independent Research (grant # 4003-00007).

†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 and Currie, 2011; Barker, 1990). This research has prompted fervent discussions among both researchers and policymakers on the importance of early childhood interventions. For example, in his 2013 State of the Union address, President Obama argued for expansions in access to high-quality preschool childcare "for every child in America". The Affordable Care Act (ACA) of 2010 created the Maternal, Infant, and Early Childhood Home Visiting grant program, which provides states with funding for home visiting programs designed to improve children's early-life health and parent-child interactions.

Programs that target the early childhood period are varied in structure and scope (see Currie and Rossin-Slater, 2015 for an overview of current programs in the U.S.). They include nutrition supplementation programs, nurse home visiting programs, large-scale preschool childcare programs like Head Start and universal pre-K, small and intensive early education interventions modelled after the Perry Preschool and the Carolina Abecedarian Project, and many others. Given that many of these programs target low-income families and have similar eligibility criteria, children are likely to be exposed to more than one intervention in their early lives. Yet while the existing research has focused on the effectiveness of these programs individually, the evidence on the potential *interactions* between different early childhood interventions is limited.[1]

As highlighted by Almond and Mazumder (2013), estimating interactions between different interventions in an observational setting is particularly challenging as the researcher is asking for "lightning to strike twice": one needs two independent, quasi-exogenous interventions affecting the same cohort but at adjacent developmental stages. We argue that we

[1]For evidence on the short- and long-run effects of individual programs, see for example: Bitler and Currie (2005); Joyce *et al.* (2008); Hoynes *et al.* (2011); Rossin-Slater (2013); Hoynes *et al.* (2012); Currie and Rajani (2014) on nutritional programs like the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) and Food Stamps; Harding *et al.* (2007); Olds (2006); Wüst (2012); Hjort *et al.* (2014); Bhalotra *et al.* (2015); Bütikofer *et al.* (2014) on home visiting; Office of Planning, Research, and Evaluation (2010); Currie and Thomas (1995); Garces *et al.* (2002); Ludwig and Miller (2007); Deming (2009); Bitler *et al.* (2012); Carneiro and Ginja (2012) on Head Start; Gormley and Gayer (2005); Hustedt *et al.* (2008); Wong *et al.* (2008) on universal pre-K; Schweinhart *et al.* (2005); Belfield *et al.* (2006); Anderson (2008); Heckman *et al.* (2010) on the Perry Preschool; and Masse and Barnett (2002); Campbell *et al.* (2014) on the Abecedarian Project.

have found such an exceptional setting in early 20th century Denmark, which—in addition to allowing us to analyze interactions—also offers an opportunity to study the persistence of an early intervention's impact throughout the life cycle and across generations.

Specifically, we examine two typical early-life interventions: a state-regulated, means-tested preschool childcare program and a nurse home visiting (NHV) program for all new mothers and infants. Both programs were gradually introduced in Denmark in the 1930s, 40s, and 50s, and we have collected unique historical data to document the timing of each program's rollout across Danish municipalities. Importantly, some municipalities implemented the NHV program before the childcare program, while others implemented the childcare program before the NHV program, and we provide evidence that the timing of each program's rollout is independent of the other. We are able to estimate causal interaction effects since we can observe individuals in four groups: those with no exposure to either program in early childhood, those with exposure to only childcare and not NHV, those with exposure to only NHV and not childcare, and those with exposure to both programs.

Our paper has three main contributions. First, we extend the literature on preschool childcare by delivering estimates of the long-term impacts of a large targeted program on outcomes of individuals in their 50s and 60s. Prior studies on childcare in both Scandinavia and the U.S. have thus far only documented benefits for individuals into their 30s (Havnes and Mogstad, 2011; Bingley *et al.*, 2015; 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.*, 2010). Moreover, the Scandinavian evidence comes from much more recent universal programs, whereas we shed light on the consequences of an earlier Danish program that explicitly targeted disadvantaged children who may have had the most to gain from regulated center-based childcare.[2]

We merge our historical program data to individual-level administrative data on cohorts born in 1930-1957 and exploit variation in the timing of childcare center openings across

[2] In Scandinavia, Havnes and Mogstad (2011) analyze the impacts of a Norwegian childcare reform in the late 1970s, while Bingley *et al.* (2015) examine the effects of Danish childcare expansions over 1966-1976. Both papers study outcomes of individuals in their early 30s. In the U.S., studies on Head Start have examined impacts on teenagers and young adults through their early 20s (Garces *et al.*, 2002; Ludwig and Miller, 2007; Deming, 2009; Carneiro and Ginja, 2012). The most recent study of the Abecedarian Project follows individuals until their mid-30s (Campbell *et al.*, 2014), while evaluations of the Perry Preschool program have outcomes through age 40 (Schweinhart *et al.*, 2005; Heckman *et al.*, 2010).

140 Danish municipalities that established a state-approved and regulated formal childcare center by 1960.[3] We find that, relative to cohorts without access to childcare in early life, cohorts born in municipalities with a childcare center by age 3 have 1.6 percent more years of schooling and are 9.7 percent less likely to have only nine years of basic compulsory education. We also find that early childcare leads to a 2 percent increase in total income measured around age 50 for males. For females, we find a 0.8 percent increase in the likelihood of surviving beyond age 65, possibly driven by a decrease in heart disease diagnoses.

Our second contribution is to provide some of the first evidence on the *intergenerational* impacts of preschool childcare.[4] We find that children of women who had access to childcare by age 3 have 0.4 percent more years of schooling and are 6 percent less likely to only have a compulsory education by age 25, relative to the children of women without early childcare exposure.

Our third contribution is to test whether access to NHV at birth amplifies or diminishes the positive long-term effects of early childcare.[5] The question of whether and how these two interventions interact can be motivated from at least two perspectives. On the one hand, modern economic models of human capital formation suggest that there are dynamic complementarities between multiple investments at different stages of childhood (Cunha and Heckman, 2007)—investments at one stage of development may make subsequent investments more productive. Applying this logic to our setting suggests that children who are exposed to the positive health effects of the NHV program in infancy may benefit more from access to childcare than those without prior NHV exposure. On the other hand, studies of heterogeneous effects of interventions often find that the *least* advantaged children are the ones who benefit the most (see, e.g.: Bitler *et al.*, 2012; Havnes and Mogstad, 2011; Meyer

[3]In order to receive state approval, a childcare center must follow strict quality regulations mandated by the government. See Section 2 for more details. Individuals born in these 140 municipalities account for approximately 53 percent of the Danish population born during this time period.

[4]We are aware of only one study that has examined the intergenerational impacts of early-life conditions on the cognitive outcomes of the next generation: Black *et al.* (2013) show that children of individuals who were exposed to radiation *in utero* have lower IQ scores.

[5]Prior work has comprehensively analyzed the long-run effects of the NHV program that we study, and we therefore do not focus on them here. Hjort *et al.* (2014) show that NHV decreased mortality at ages 45-57 and the probability of being diagnosed with cardiovascular disease during the same age range. In Appendix B, we present estimates for the main effects of NHV based on the sample and measures used in the current paper. Our estimates are in line with the results presented in Hjort *et al.* (2014) and suggest strong health effects of NHV.

and Wherry, 2012; Carneiro *et al.*, 2011). This evidence suggests that different types of interventions could actually be substitutes—in our context, children who are not exposed to the NHV program may have more to gain from access to childcare than those who already experienced NHV in infancy.

We find statistically significant *negative* interaction effects between NHV and preschool childcare exposure, suggesting that the two early childhood interventions are substitutes rather than complements. Having access to NHV at birth reduces the positive impact of childcare at age 3 by 85 percent for years of schooling (of the first generation) and 86 percent for income among men. We also present suggestive evidence that the impact on women's survival beyond age 65 and the education of the second generation is reduced as well. In sum, access to high quality childcare at age 3 is much more consequential for individuals (and their children) who had *not* already received the NHV intervention at birth. Interestingly, these large negative interaction effects between NHV and preschool childcare are very consistent with two other concurrent papers studying interactions across different types of early-life investments in Bangladesh (Gunnsteinsson *et al.*, 2014) and Mexico (Adhvaryu *et al.*, 2015).[6]

The rest of the paper proceeds as follows. Section 2 provides more details on each of the two programs we study and reviews the relevant 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

We use variation from two natural experiments in our analysis. Here, we describe the details of these natural experiments and the related literature.

[6]Specifically, Gunnsteinsson *et al.* (2014) study an interaction between a tornado and a randomized vitamin supplementation program in Bangladesh, while Adhvaryu *et al.* (2015) analyze an interaction between rainfall shocks and conditional cash transfers in Mexico. Section 2 discusses these papers in more detail.

2.1 Childcare Expansion

Center-based childcare provides parents with an opportunity to work while leaving their children in an organized and professional group care setting. Early childcare programs vary in their pedagogical methodologies, but generally aim to promote school readiness among young children. In addition, formal childcare may provide disadvantaged children with better nutrition and health services than they would have otherwise received in their homes or in the care of relatives.

The Danish childcare system goes back to 1828, when the first childcare center was founded. These early childcare centers—called *"Asylums"*—were run by philanthropic organizations to exclusively serve children from poor families. "Kindergardens" (*"Folkebørnehave"*) emerged in the 1910s and accounted for the majority of new centers from that time onwards. In the period that we study, both types of centers still primarily served children from disadvantaged families—childcare was supposed to provide poor mothers with the opportunity to work and contribute to family income.[7]

Until 1919, however, childcare centers were not regulated and exhibited substantial variation in program quality. A series of laws passed between 1919 and 1951 regulated state approval and financial support of childcare centers (see Skjernbæk, various years). Specifically, from 1919 onward, all existing and new childcare centers could apply for state approval and state subsidies. To acquire state approval, childcare centers had to provide adequate facilities, have a board and a qualified center head, and meet state requirements for fees paid by parents.[8]

Once a childcare center became approved by the state, it could apply for a subsidy.[9] To receive a subsidy, an approved center had to satisfy four main requirements:

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

[8]Fees were between 3 Danish crowns per week in the 1930s and 8-10 Danish crowns per week in the 1950s. These fees covered the food and milk that was provided to children at most centers.

[9]These subsidies could be used to cover the daily expenses associated with running the center (e.g., wages and rent) or to establish, improve, or expand an existing center. Subsidies from the state 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. From 1951 onwards, both the state and the municipalities were involved in the financing of childcare centers. If a municipality ran a childcare center or subsidized at least 30 percent of its expenses, the state subsidy was raised to 40 percent.

1. **Ownership and childcare expertise.** The childcare center had to be either run by a municipality (which employed staff with expertise on children) or be run by a private organization with a board of members with expertise on children (e.g., a pediatrician, a teacher, etc.).

2. **Opening hours.** The center had to be open for at least four hours each working day. The center was 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.

3. **Targeting.** The center had to provide services exclusively or predominantly to children from poor families, who were eligible for social benefits. Over time, this requirement was loosened.[10]

4. **User fees.** The center had to charge a fee that at least matched the value of the meals provided to children. Parents who could not afford the fee could apply for an exemption.

Throughout the time period we consider, the state regulated and monitored childcare centers' hygienic conditions, and encouraged centers to work together with local physicians and dentists to monitor children's health. The state reimbursed expenses related to these health check-ups.[11] Further, regulations regarding the educational requirements for childcare staff, their wages, and the child-to-teacher ratio were also in place.

The pedagogical content in Danish childcare from the 1930s onwards was inspired by the principles of influential educators such as Friedrich Fröbel and Maria Montessori, with an emphasis on providing a stimulating environment for children. Childcare center staff were required to be formally educated. During the period that we consider, trade unions that focused on pedagogical work were established.[12] In sum, childcare centers approved by the state during this time period likely provided much higher quality care than the alternative

[10]Beginning in the late 1940s, approved centers that did not predominantly serve poor families could also receive smaller subsidies from the state and muicipality (for a total of around 35 percent of all costs) (Skjernbæk, various years).

[11]As noted in Skjernbæk (various years), a shortage of dentists resulted in a rather low take-up of the dental services.

[12]The unions lobbied for adequate educational programs and higher wages for the childcare staff.

options; poor mothers would otherwise typically leave small children alone at home, under the supervision of older siblings, or in the care of other families.

Our analysis uses variation in the timing of state approvals of childcare centers across Danish municipalities between 1930 and 1960.[13] An important concern for our identification strategy is that the timing of childcare center approvals may be endogenous and correlated with other municipality characteristics. We examine this possibility in detail in Sections 4 and 5, with evidence suggesting that program endogeneity does not pose serious threats to our identification strategy.

Existing literature on childcare. The existing literature on the impacts of childcare programs is large, but has mostly focused on their short- and medium-term effects in childhood and young adulthood and has found mixed results. There are a number of studies that find that preschool childcare improves short-run cognitive test scores (Loeb *et al.*, 2007; Gormley and Gayer, 2005; Fitzpatrick, 2008; Berlinski *et al.*, 2009). Similarly, 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, concentrated among the most disadvantaged children (Office of Planning, Research, and Evaluation, 2010; Bitler *et al.*, 2012). However, other papers suggest that these short-term impacts dissipate by the end of first grade (Magnuson *et al.*, 2007; Office of Planning, Research, and Evaluation, 2010), while still others find no or adverse impacts of childcare on cognitive and non-cognitive child development (Baker *et al.*, 2008; Herbst and Tekin, 2010; Datta Gupta and Simonsen, 2010; Baker *et al.*, 2015). Overall, the mixed nature of the results implies that program quality and the availability and quality of alternative options for childcare are likely important determinants of program success.

Despite the mixed evidence on short-run impacts of childcare, the research points to positive medium-term effects of some targeted programs on a variety of outcomes. For

[13] Our data have information on both the year of establishment and the year of approval for each center. For many centers, these years are the same. Some centers were originally established as low-quality and unregulated *"Asylums"* and obtained state approval in a later year. However, given that state approval entails a more uniform and regulated treatment that leads to a large improvement in childcare quality, we focus on the effects of access to formally approved childcare centers rather than the effects of access to any childcare.

example, 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 and Thomas, 1995; Garces *et al.*, 2002; Deming, 2009). Head Start may also reduce childhood mortality (Ludwig and Miller, 2007). Smaller-scale intensive preschool interventions such as the Perry Preschool Program and the Abecedarian Project have even larger positive impacts on medium-run outcomes (Schweinhart *et al.*, 2005; Belfield *et al.*, 2006; Anderson, 2008; Heckman *et al.*, 2010; Masse and Barnett, 2002; Campbell *et al.*, 2014).[14]

Universal publicly-subsidized childcare programs have mixed medium-run impacts. Cascio (2009) finds that the introduction of kindergartens into U.S. public schools reduced the highschool dropout rate, but had no impacts on other outcomes such as employment, college attendance, and earnings. In Norway, Havnes and Mogstad (2011) study a childcare expansion in the 1970s and find positive impacts on educational attainment and labor market participation. In Denmark, Bingley *et al.* (2015) use variation in childcare center openings from the same time period to instrument for maternal employment, and find large positive impacts on children's schooling and adult earnings.

We contribute to this literature by providing the first evidence on the persistence of impacts of access to early childcare on educational, labor market, and health outcomes through age 65 and on the education of the next generation, and by studying how preschool childcare interacts with another popular early-life intervention, a nurse home visiting program.

2.2 Nurse Home Visiting Program

While childcare centers may improve children's early circumstances through education and high-quality care, home visiting programs target the early-life environment by serving both parents and children at the same time. These programs seek to promote child development and provide new parents with education about parenting skills, health, nutrition, and resource availability through regular home visits by program-trained paraprofessionals, nurses,

[14] There is also some evidence that low-quality childcare programs have negative effects on non-cognitive outcomes in the medium-term, in terms of health, life satisfaction, and crime (Baker *et al.*, 2015).

or other child development professionals. The visits usually begin when the woman is pregnant or shortly after childbirth, and can continue for several years.

We study an early nurse home visiting program for all infants and their mothers in Denmark. The Danish National Board of Health (DNBH) developed the program due to a relatively high infant mortality date of around 6.5 percent in the 1930s (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 proper home environment. Approved and trained nurses were assigned to visit newborns and their mothers approximately 10 times in the first year of life and teach mothers about the basics of infant care, as perceived at the time: "calmness, orderliness, and cleanliness". Nurses monitored infants' development and referred ill infants to doctors for treatment (for more details on the program see Buus, 2001; Wüst, 2012; Hjort *et al.*, 2014).

While DNBH centrally designed and co-funded the program, implementation was under municipal discretion. Municipalities had to find suitable candidate nurses and get approval for their choice of nurses and their municipal programs at the DNBH. Implementation began in 1937. A key factor responsible for the variation in the exact timing of program implementation from 1937 onwards was the sometimes lengthy accreditation process that was necessary for national co-funding. Other factors appear to have been the local preferences of key actors such as general practictioners (GPs), who in some places promoted the initiation of NHV but in other places opposed it as it was undermining their authority (Buus, 2001).

As with the childcare expansion, the endogeneity of the NHV rollout poses a potential threat to our identification strategy. Both Wüst (2012) and Hjort *et al.* (2014)—who study the short- and long-run health effects of the program, respectively—examine this issue thoroughly. They find that the inclusion of pre-treament controls does not impact the conclusions from their analyses. They conclude that the rollout of the NHV program serves as a valid natural experiment for identifying its causal effects.

Existing literature on home visiting. In the U.S., several home visiting programs have been evaluated using experimental designs. The results are mixed, and it seems 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. For example, the Comprehensive Child Development Program (CCDP), which was implemented with low-income families at 24 different sites in 22 states in 1990, involved biweekly home visits by paraprofessionals, who were local community members with limited post-high school education. The evidence suggests that CCDP had little effect on the health and development of treated children (St. Pierre and Layzer, 1999).[15]

By contrast, a more targeted program called the Nurse Home Visiting Partnership (NHVP) has had much greater success. NHVP enrolls pregnant women who satisfy at least two of the following three criteria: they are unemployed, unmarried, or have less than 12 years of education. The families receive weekly home visits (which are gradually decreased in frequency to monthly) until the children are 2 years old. A key distinction of the NHVP is that all of the home visitors are certified nurses with formal training in women's and children's health. At each visit, the nurses are required to follow detailed guidelines that are specific to different stages of pregnancy and periods of child development. The program has been experimentally evaluated at three sites, and across all sites, the positive effects on children are large and lasting. Relative to children in control groups, treatment children experienced fewer health problems throughout childhood, had higher GPAs and scored higher on math and reading achievement tests at age 9 (Olds, 2006).

As noted above, Wüst (2012) and Hjort *et al.* (2014) have studied the short- and long-term impacts of the Danish NHV program that we examine here.[16] 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. She also shows that NHV accounted for about 17-29 percent of the overall decreases in

[15] Another similar program, called Healthy Families America (HFA), was begun in 1992 and provides participants with weekly home visits up to five years. HFA's visitors are more educated than those in CCDP—most have at least some college education with specializations in child development, social work, education, and nursing. The evidence on HFA is mixed, with some sites showing positive effects on infant health and reductions in rates of intimate partner violence and child abuse, while others showing no impacts (see Harding *et al.*, 2007 for a review). The variation in effects across HFA sites likely stems from differences in program implementation and management.

[16] Ongoing work is studying 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.

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 better nutrition. Hjort *et al.* (2014) document that the positive health effects persist into adulthood—individuals who were exposed to NHV at birth have fewer hospitalizations and are less likely to die at ages 45-57. Furthermore, they show that treated individuals are less likely to be diagnosed with cardiovascular diseases in the same age range. This finding is 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.*, 2012).[17]

2.3 Evidence on Interactions

The growing interest in interactions between different types of interventions is in part motivated by the work of James Heckman and his co-authors, who developed a model that extends the seminal model of Becker and Tomes (1986) on parental investments in human capital (see, e.g.: Cunha and Heckman, 2007; Heckman and Masterov, 2007; Cunha *et al.*, 2010). A key feature of the model is the idea of dynamic complementarities, where human capital and investments in one period raise the productivity of investments in a future period. In other words, skills beget skills. However, despite the extensive theoretical work underpinning this argument, rigorous empirical evidence on complementarities between investments is scarce.

Typically, studies arguing in favor of complementarities show that the effects of some particular intervention are larger for those with higher measures of initial human capital or skill. For example, Heckman *et al.* (2013) find that the Perry Preschool program had the largest impacts on cognitive achievement among children at the top of the distribution. Similarly, Aizer and Cunha (2012) show that children with higher measures of cognitive development at the age of eight months experience the largest gains in IQ as a result of Head Start participation. However, as pointed out by Almond and Mazumder (2013), these

[17]Hjort *et al.* (2014) also consider the long-run effects on educational and labor market outcomes, finding less consistent and much smaller effects.

studies are limited by the fact that the variation in initial human capital is not random. In other words, there may be unobserved differences between children with high and low baseline cognitive measures that are contributing to the differences in the returns to preschool in both Heckman *et al.* (2013) and Aizer and Cunha (2012).[18]

Moreover, there are other studies showing evidence that is more consistent with different investments being *substitutes* rather than complements. These studies show that the benefits of early childhood interventions are largest for the least advantaged children. Bitler *et al.* (2012) use quantile regression methods to show that the effects of Head Start on test scores are concentrated among students at the bottom of the distribution. Havnes and Mogstad (2011) find that the positive effects of early childcare in Norway are greater for children with low educated mothers than for children with higher educated mothers. Meyer and Wherry (2012) document that early-life access to Medicaid reduced the mortality rates of black but not white teens. Carneiro *et al.* (2011) show that the long-run effects of maternity leave are largest for children with low educated mothers. Dahl and Lochner (2012) find that the positive impacts of income from the Earned Income Tax Credit (EITC) are largest for children from disadvantaged families. Of course, just as in the studies on complementarities, other unobservable measures of heterogeneity could explain these differences in effects. Additionally, studies that measure access to programs (and not program participation) cannot distinguish between heterogeneous effects of programs conditional on participation from the heterogeneity in program take-up (e.g., less advantaged families may benefit more from the EITC simply because they are more likely to qualify for and take-up the program than their more advantaged counterparts).

To identify interaction effects across investments, one clearly needs at least *two* sources

[18]A related strand of literature studies the relationship between initial endowments of children and subsequent parental investments. These studies do not estimate the interaction effects between investments and endowments on long-run outcomes, but rather ask whether parents invest in a reinforcing or compensating manner. Adhvaryu and Nyshadham (2014) find evidence of reinforcing investments, showing that children exposed to an iodine supplementation program in Tanzania are more likely to receive vaccinations and are breastfed longer. Bharadwaj *et al.* (2013) use data from Chile and Norway and find no differences in parental responses between children who are more or less likely to receive medical intervention at birth. In contrast, Sievertsen and Wüst (2015) show that in Denmark parents do respond to medical interventions at birth—mothers who are discharged from the hospital on the day of giving birth are less likely to breastfeed at four months than mothers who are discharged at a later time. See also Almond and Mazumder (2013) for a review.

of quasi-exogenous variation. We are aware of only a handful of recent working papers that take this approach. Bhalotra and Venkataramani (2012) overlay state-year variation in access to sulfa drugs (antibiotics used to treat pneumonia) in the 1930s with variation in several measures of racial segregation. They show that African-American cohorts born in the U.S. in the 1930s experienced higher human capital gains from access to sulfa drugs when they faced less segregation, better schooling, and overall opportunity for social mobility. However, while suggestive of complementarities between investments, the potential endogeneity of the racial segregation measures remains a concern.

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 investment levels on average) 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.

The other two concurrent papers in this small but growing literature find stronger evidence of substitutability across different types of investments and interventions. 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.

Our analysis contributes to this literature by delivering causally identified estimates of an interaction between two commonly implemented early-life interventions in the modern developed world—high-quality preschool childcare and an NHV program—and by studying a wide range of very long-term and intergenerational outcomes.

3 Data and Sample

We merge data from several sources for our analysis. First, we use information on the geographical and administrative structure of Denmark in the 1920-1955 period to assign treatment status to individuals in our sample. Second, we collect data on the expansion and approval of childcare centers and on the implementation of the NHV program. 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) that provides a link across several historical Danish administrative entities, including parishes and municipalities.[19] In the period that we study, Denmark consisted of over 1,300 municipalities that were rather 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.

The information on parishes is relevant in our context, as births in Denmark are registered at the parish level. Thus, we use the "DigDag" project data together with information on individuals' parishes of birth available in our long-run outcomes data to merge them to their municipalities of birth.

Data on childcare centers. We have collected information on all approved Danish childcare centers for children aged 3-7 years old 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).[20] These data contain information on the type of childcare center ("*Asylum*" or "*Kindergarden*"), the first registered exact address, the year of establishment and the year

[19] For more information, please see: `www.digdag.dk`.

[20] The majority of childcare centers served children between ages 3 and 7. A minority of the centers in our data also accepted younger children.

of approval, and the number of children registered in each of the given nine years.[21] We use the address data to match centers to the municipalities in which they were located.

Out of the 1,354 Danish municipalities that existed between 1930 and 1960, 140 had at least one approved childcare center 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 childcare centers are mostly very small and rural; the 140 municipalities with at least one approved childcare center had ten times higher average population counts in 1930 than the other municipalities. Table 1 also shows that there are substantial differences between the municipalities with and without childcare centers 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 state-approved childcare center by 1960. We should note that these 140 municipalities are still fairly small entities, with a median population of 4,606 in 1930. Individuals born in these municipalities account for 53 percent of the population we observe in our administrative individual-level data (described below).

Figure 2 shows the evolution of childcare centers in these 140 municipalities.[22] In 1933, only about 20 percent of municipalities in our sample had at least one approved childcare center, whereas by 1960, all of them did. Most municipalities only ever have one approved childcare center—the median number of childcare centers per municipality is one, while the 75th percentile is two. Only 18 municipalities in our data ever had more than five approved childcare centers. Thus, most of our analysis uses variation in the initial childcare center approval (changing from zero to one approved childcare center).

Data on the Nurse Home Visiting program. We have collected information on the date of NHV program approval from the DNBH for all implementing municipalities over

[21] We use the original address of the childcare center even though some centers move. Usually, centers only moved within the same parish/municipality, e.g., to get more space. We also should note that the records for the total number of slots per institution are unfortunately incomplete; we only have data on the actual number of children enrolled in each center in the given nine years.

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

1937-1949 from records stored in the Danish National Archives.[23] 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.[24]

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 instead of analyzing the impact of the number of nurses per municipality.[25]

Figure 3 depicts the variation in childcare and NHV program availability by birth year. In this figure (and in most of our analysis), access to childcare 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 childcare and NHV was not yet established. As the childcare and NHV programs expanded, the percentage of municipalities with both childcare and NHV 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 childcare. In the late 1940s, nearly 10 percent of municipalities only had NHV. In sum, during our analysis time frame, some cohorts were exposed to neither childcare nor NHV, other cohorts were exposed to either only childcare 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 the total population and other control variables. The population data are available for all municipalities from the quinquennial censuses. The data on other controls come from the *Statistical Commune Data Archive* (Danish Data Archive), and contain information on municipal characteristics (collected from various archives and censuses)

[23]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.*, 2014).

[24]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 (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.

[25]Also, 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.

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.[26]

Additionally, we have collected data on the annual number of live births and infant deaths for the 86 urban municipalities for years 1933-1950 from the DNBH. 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.

Individual-level administrative data on outcomes of the first generation. We use administrative register data on outcomes available for years 1980-2012/2013. We study a variety of outcomes observed at different ages for our cohorts. First, we construct three measures of educational attainment: years of schooling, and indicators for basic education (nine years of compulsory schooling only) and higher education (more than compulsory).

Second, we examine several labor market and income variables. Our main specifications focus on outcomes measured around age 50; at this age, individuals are well established in their careers and we can observe all of our cohorts in the outcome data.[27] We consider log total income, log wage income, an indicator for any wage income, and an indicator for having a blue-collar occupation. For each of the income variables, we calculate individual-level three-year moving averages (i.e., an average over ages 49-51) before taking logs. The indicator for any wage income similarly refers to any positive income in the three-year age range. We study labor market outcomes averaged for each individual over a set of ages rather than outcomes measured at a particular age (e.g., age 50) in order to minimize any residual variance or measurement error in the observed employment and earnings distributions and to ameliorate concerns that any effects we see are driven by a contemporaneous shock in any particular earnings year. We also examine income and labor market outcomes at younger ages using data on the younger cohorts in our sample.

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

[27] As we write below, our analysis focuses on individuals born between 1930 and 1957. As such, we can observe all of our cohorts at age 50 in 1980-2007.

Third, we study survival beyond age 65 and hospitalizations by age 60.[28] When we analyze survival, we left-censor the data such that all individuals in our analysis sample enter the risk period that we consider at age 50.[29] Additionally, we study the probability of being diagnosed with one of the following conditions by age 60: cardiovascular conditions, heart disease, diabetes, and cancer. Data on diagnoses come from hospital records available over 1980-2012, which we merge to the population register.[30]

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.[31] We examine several fertility outcomes for the women in the 1935-1957 cohorts—an indicator for no children, total number of children, maternal age at first birth, and an indicator for the father's information being missing from the birth certificate.

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 basic education, an indicator for gymnasium education (academic high school after the nine years of compulsory education), and an indicator for higher 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. Our sample is limited to individuals who were 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

[28]Hospitalizations are measured as the number of nights in the hospital.

[29]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. We still study right-censored data but this type of censoring is taken into account by our cohort fixed effects.

[30]For the period 1980-1993, we only have information on diagnoses for patients who are admitted as inpatients. From 1994 onwards we also observe outpatient diagnoses. As we only have information on whether there was any outpatient contact (and not on how many contacts the given outpatient spell required), we cannot study the number of outpatient contacts.

[31]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 childcare or NHV impacted the fertility patterns of the mothers of treated children.

birth that allows us to assign treatment status; and (2) the individual must be observed in our post-1980 outcome data.

As Appendix Figure 1 shows, 88-90 percent of Danish-born individuals in our outcome data can be matched to a parish of birth. Older cohorts are less likely to be matched to valid parish codes.[32]

Since we can only study the outcomes of survivors who are observed in the register data—i.e., those who were aged 23-50 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. Appendix Figure 2 illustrates the percentage of "missing" Danish-born individuals in our outcome data (including individuals with no valid parish code).[33] The figure indicates that 4-13 percent of Danish-born individuals are missing in the post-1980 data due to mortality or emigration, and that we miss—as expected—more individuals from older cohorts.[34] However, using only the younger cohorts with fewer missing observations, we show below in Section 5 that statistically significant mortality impacts only materialize around age 60 for women (and not at all for men). 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 childcare center in the municipality×year. Appendix Table 1 reports the results from various specifications of this regression, showing no statistically significant relationships.

[32]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.

[33]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).

[34]Hjort *et al.* (2014) present a similar table in their analysis of the long-run effects of NHV. It also supports the finding that a relatively low and stable number of individuals are missing from the post-1980 data.

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 that implemented an approved childcare center by 1960, we are left with $879,647$ observations. We collapse our data to $3,918$ municipality×birth-year-cells.

4 Empirical Methods

Our analysis exploits the municipality×year variation in childcare center approvals and the NHV program rollout to create difference-in-difference and event-study designs. As noted above, to ease the computational burden, we collapse our individual-level data into birth-municipality×birth-year cells and weight by cell size.[35] For some specifications, we also present results on heterogeneous effects by gender, using data collapsed to the gender×birth-municipality×birth-year level.

To analyze the effects of early-life childcare access, we estimate versions of the following equation:

$$Y_{ymc} = \alpha_0 + \alpha_1 ChildCareAge3_{ym} + \lambda_m + \gamma_y + \nu_c \times y + \epsilon_{ymc} \tag{1}$$

for cohorts born in year y, municipality m, and county c.[36] Y_{ymc} is an outcome of interest such as education or adult income. $ChildCareAge3_{ym}$ is an indicator equal to one for cohorts that had at least one approved childcare center in their municipality of birth at age 3, and zero otherwise.[37] λ_m are municipality fixed effects, while γ_y are year of birth fixed effects. We also add county-specific linear time trends denoted by $\nu_c \times y$.[38] ϵ_{ymc} is the error term, which we cluster by municipality. The key coefficient of interest, α_1, identifies the effect of having childcare 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 childcare access by age

[35]This method is equivalent to estimating the corresponding individual-level regressions with no individual-level controls.

[36]Counties are the next-largest geographical entities after municipalities. In our sample of 140 municipalities, 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.

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

[38]We test the robustness of our results to the exclusion of county time trends and to the inclusion of interpolated municipality characteristics and municipality-specific pre-trends, as described further below.

of exposure:

$$Y_{ymc} = \kappa_0 + \sum_{a=0}^{a=6} \tau^a ChildCare^a_{ym} + \sum_{a=8}^{a=10} \tau^a ChildCare^a_{ym} + BornAfter_{ym} + Older_{ym}$$

$$+ \lambda_m + \gamma_y + \nu_c \times y + \epsilon_{ymc} \qquad (2)$$

Here, $ChildCare^a_{ym}$ is an indicator equal to one for cohorts that were age a in the year of the first child care center approval in their municipality of birth and zero otherwise. We include indicators for ages 0 to 6 and 8 to 10 (with age 7, the oldest age at which a child could attend childcare, as the omitted category). $BornAfter_{ym}$ is an indicator for cohorts born after the childcare center approval (i.e., they were aged less than 0 at the time of approval), while $Older_{ym}$ is an indicator for cohorts who were older than age 10 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 formal childcare for five years until age 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 childcare access is correlated with the outcomes of cohorts who were too old at the time of center approval.

To examine interactions between childcare and NHV, we estimate:

$$Y_{ymc} = \beta_0 + \beta_1 ChildCareAge3_{ym} + \beta_2 NHV_{ym} + \beta_3 ChildCareAge3_{ym} \times NHV_{ym}$$

$$+ \lambda_m + \gamma_y + \nu_c \times y + \epsilon_{ymc} \qquad (3)$$

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 childcare at age 3 for cohorts without NHV, while β_2 measures the impact of access to NHV at birth for cohorts without childcare 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 childcare and the interaction effects between access to childcare and access to NHV under the assumptions that: (1) the timing of childcare center 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 childcare center approvals is uncorrelated with the NHV program rollout.[39]

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. Further, our county linear trends allow for the outcomes of cohorts born in each of the 24 counties in our data to follow distinct trends.

The time period that we study calls for a discussion of the role of World War II and its possible influence on our sources of variation. This influence would be a concern if it varied across municipalities, and was therefore not accounted for by our cohort fixed effects. 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 other goods were rationed, there was nevertheless a stable supply of food for all Danish citizens (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 childcare or NHV during the war years.

While the first identifying assumption remains inherently untestable, we conduct some indirect tests to evaluate its plausibility. Specifically, we estimate versions of model (1) using interpolated municipality characteristics as outcome variables to test for a correlation be-

[39] We also need for the timing of the NHV program rollout to be quasi-exogenous and uncorrelated with municipality time-varying characteristics. Evidence on this point has been provided by Wüst (2012) and Hjort *et al.* (2014), as we noted in Section 2.

tween the timing of childcare center approvals and other time-varying municipality-specific factors. Table 2 presents the results, which show that childcare center 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 childcare center 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 Appendix Table 2, the correlations become insignificant at the 5% level. To address this issue further, we also show that our main results are robust to: (a) including urban/rural municipality indicators interacted with linear trends, (b) including municipality fixed effects interacted with linear pre-trends, and (c) including all of the interpolated municipality characteristics presented in Table 1 as control variables. See Section 5 for more details.

We can directly test the second identifying assumption with our data. In Table 3, we present results from correlating access to childcare 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 childcare 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 Childcare on the First Generation

We begin with results on the long-term impacts of early-life access to childcare for the first generation. Table 4 presents results from estimating versions of equation (1) using the following outcomes as dependent variables: years of schooling at age 50, an indicator for basic education at age 50, log mean total income between ages 49 and 51, and an indicator for survival beyond age 65.

We show results from four different specifications. In the first column, we present a baseline model with only municipality and birth year fixed effects. In column (2), we also include county linear time trends. In column (3), we add in all of the available interpolated municipality characteristics as time-varying controls. Finally, column (4) includes municipality-specific linear pre-trends instead of the county linear trends. We use municipality trends *based on the pre-treatment data only* because we believe that our treatment effects likely consist of changes in both outcome *levels* and *trends*. First, take-up of childcare might increase gradually after the first approved center opens, meaning that cohorts born several years after a center is approved experience greater benefits than cohorts exposed to childcare immediately after the approval. Second, there may be a linear effect of each additional year of exposure between ages 3 and 7. As a result, municipality-specific time trends that use both pre- and post-treatment data will "over-control" and absorb an important part of the treatment effect we are trying to estimate.

Across all four outcomes, we see evidence that early childcare improves long-term well-being. In columns (1), (2), and (4), the coefficients are all of the same sign and similar magnitude. For instance, in column (2), we find that, relative to the comparison cohorts, individuals who had an approved childcare center in their municipality of birth by age 3 have 0.19 more years of schooling (1.6 percent at the sample mean), are 2.9 percentage points (9.7 percent) less likely to only have a basic education, have 1.5 percent higher income, and are 0.5 percentage points (0.6 percent) more likely to survive beyond age 65. In column (3)—where we include the interpolated municipality controls—the coefficients are qualitatively similar to those in the other columns, but reduced in magnitude for some of the outcomes. One potential explanation for this reduction in effect size is that some of the municipality-level variables observed after childcare center approval could actually be endogenous—for example, the presence of formal childcare could affect voting behavior or average incomes (since women may be more likely to work). As such, we take the model in column (2) as our preferred specification for much of the subsequent analysis.

Figures 4 and 5 present the corresponding event-study graphs for years of schooling and basic education, respectively. We plot the τ^a coefficient estimates from equation (2) and the corresponding 95% confidence intervals. Both figures show a marked improvement in educational attainment for cohorts aged 0 to 3 at the time of the state-approved childcare center opening. The magnitudes of the coefficients for individuals aged 4 to 6 in the year of childcare center approval are smaller and consistent with a possibly linear effect of each additional year of childcare exposure. Importantly, the coefficients on exposure at ages 8 to 10 are statistically insignificant, suggesting that there are no pre-existing trends in the outcomes of cohorts who were too old to attend childcare.

Table 5 explores the effects of childcare on other labor market outcomes using the same four specifications as in Table 4. In column (2), we see a marginally significant 1.5 percent increase in log wage income. We find no statistically significant effects on the likelihood of having any wage income or on the likelihood of having a blue-collar occupation.

When we split our sample by gender in Tables 6 and 7, we see that the income and labor market effects are driven entirely by males, while the survival effect is driven by females. The educational effects, by contrast, are similarly strong for both males and females. Men who are exposed to childcare at age 3 have 2.4 percent higher incomes at age 50, while women with early childcare access are 0.8 percent more likely to survive beyond age 65. Figures 6 and 7 plot the corresponding event-study figures for male income and female survival, respectively. Again, we see an improvement in outcomes for cohorts who were aged 0 to 3 at the time of childcare center approval, and no evidence of pre-trends for cohorts older than age 7 in the year of center approval. We should note, however, that the individual age coefficients are less precisely estimated in these figures than those in the figures for educational outcomes, perhaps because of reduced power when we limit the sample to only one gender.

In Table 8, we examine impacts on health diagnoses separately by gender. For women, our results suggest that early-life childcare access may reduce the incidence of diagnoses for heart disease at age 60, which can perhaps explain the increased survival beyond age 65. For men, we see a reduction in hospitalizations for cancer.[40]

Effects by age of follow-up. We next explore how the observed effects of childcare on education, income, and health evolve over the life cycle. Figure 8 presents the coefficients and 95% confidence intervals from seven separate regressions that use educational attainment observed at ages 35-65 (in five-year intervals) as dependent variables. We see a consistent

[40]We also examined hospitalizations at younger ages, finding insignificant effects.

positive effect on years of schooling at all of these ages. These results are not surprising as most individuals complete their education by age 35. However, they are also reassuring as they demonstrate the robustness of the education effect across many possible ages of observation and across different cohorts (given that we do not observe all cohorts at all ages).

Figure 9 shows how the positive effect on log total income evolves over the life cycle for males. The coefficients are positive at all observed ages, and with overlapping confidence intervals (i.e., we cannot reject that they are the same across all age groups). The impact of childcare increases male income at age 35, and this effect seems to persist until retirement age.[41]

In Figure 10 we analyze heterogeneity in the effect on female survival by age of follow-up. We find that statistically significant effects on survival materialize around age 60 for women.

Robustness. Our analysis of childcare rests on an assumption that, conditional on municipality and year fixed effects and county-specific time trends, the timing of childcare center 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 childcare centers were experiencing a more positive trend in their outcomes than cohorts born in municipalities with later childcare. 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 10 at the time of the first childcare center approval experienced any changes in their outcomes, despite the fact that slightly younger children in those same municipalities did benefit from childcare access. We have also shown that our results are mostly robust to the inclusion of different controls for trends—county linear trends, municipality-specific pre-trends, and municipality time-varying controls. 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.

[41]The official age of retirement in Denmark is 65.

In columns (2)-(4), we explore alternative specifications that deal with differences across urban and rural areas. Specifically, 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. 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 childcare centers earlier than rural ones) are not driving our main results.

Columns (5)-(7) test the robustness of our results to further sample limitations. Column (5) drops municipalities that had an approved childcare center at the beginning of our sample period in 1933. Omitting the earlierst implementers likely omits all municipalities with childcare centers that had been introduced by philantropic organizations. For those organizations we may be worried that they also introduced other initiatives that benefitted treated children (such as vaccation programs). Column (6) limits the analysis to cohorts born in 1930-1949, a narrower window of years surrounding the childcare variation. Column (7) only includes municipalities that ever implement an NHV program. Across all of these specifications, the results remain generally consistent with our baseline model.

Finally, column (8) 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 formal childcare between the ages of 3 and 7.[42] The results from this alternative specification again suggest that greater exposure to childcare improves long-run outcomes.

Magnitudes. To assess the magnitudes of our estimates, we compare our findings to two strands of literature on the effects of childcare. First, we compare our results to the two studies on the impacts of childcare expansions in Scandinavia. Havnes and Mogstad (2011) find that access to the Norwegian childcare program increases years of schooling by 0.5 percent, and reduces the likelihood of being a "low earner" at age 35 by 3.2 percent. Bingley

[42]This variable is equal to 1 for those aged 3 and younger in the year of the center approval; 4/5 for those aged 4; 3/5 for those aged 5; 2/5 for those aged 6; 1/5 for those aged 7; and 0 for those aged 8 and older.

et al. (2015) report much larger impacts from the more recent Danish childcare expansion in the 1960s and 1970s—they find a 17 percent increase in years of schooling and a 25 percent increase in age-35 earnings.

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 data actually attended childcare. However, the other two Scandinavian papers study expansions in universal childcare, while we study a more targeted program. As such, we also calculate approximate treatment-on-the-treated (TOT) effects and relate our estimates to a second line of research on the effects of targeted programs.

Our ITT estimates show a 1.6 percent increase in years of schooling, a 9.7 percent increase in having more than a basic (compulsory) education, and a 1.5 percent increase in income around age 50. To calculate TOT effects, we must estimate a childcare coverage rate, which we can do for the 86 urban municipalities in our sample. Specifically, we use data on the number of children enrolled in each childcare center in each of the nine years of book publications. We interpolate these data to get estimates of enrollments in every year and aggregate to the municipality×year level. Then, using our data on the number of first-year survivors in each of the 86 urban municipalities, we can calculate the average share of children aged 3-7 years old who were enrolled in childcare in every year between 1940 and 1950. Appendix Table 4 estimates that approximately 10 percent of all living children aged 3-7 were enrolled in childcare in the urban municipalities during this time period. This figure is in line with available aggregate numbers on childcare coverage (DST, 2008), and reflects the targeting of state-approved childcare centers during our study period. As described in Section 2, until the 1960s, childcare centers had to focus on children from disadvantaged households to be approved for state subsidies (Skjernbæk, various years).

The above analysis suggests that one can scale our estimates by 10 to get approximate TOT effect sizes. Although the approximate TOT magnitudes may seem very large, it is important to highlight that they are based on the most disadvantaged children for whom we may expect the largest gains from professional care and improved nutrition in state-regulated childcare. Furthermore, our TOT estimates are actually not out of step with the U.S. literature on participation in targeted early childcare programs. For example, Garces *et al.*

(2002) find that Head Start participation increases the likelihood of high school completion by 26 percent and raises the likelihood of college attendance by 28 percent among whites, while Schweinhart *et al.* (2005) report that participation in the Perry Preschool program increased the age-40 earnings of males by 30 percent and of females by 20 percent. Heckman *et al.* (2010) estimate a lifetime earnings impact of participation in the Perry Preschool program of $145,461 for males and $211,651 for females (in undiscounted 2006 dollars).[43]

5.2 Effects of Childcare on the Second Generation

Having shown that early access to childcare 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 childcare exposure affected the fertility behavior of women in our analysis sample. In Appendix Table 5, 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 0.6 percentage point decline in the likelihood of having no children and an 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 9 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 0.4 percent, which seems to be driven by a 6 percent reduction in the likelihood of only having a basic education and about a 3.7 percent increase in the likelihood of having a gymnasium education. While the magnitudes of these effects are smaller than the education effects for the first generation, our results present novel evidence that early-life interventions have the potential to mitigate some of the intergenerational transmission of low socio-economic status.

[43]This estimate is based off interpolations that use earnings data collected at ages 27 and 40.

5.3 Interaction Effects Between Childcare 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.* (2014)—enhances or diminishes the positive long-term and intergenerational impacts of childcare.[44]

Table 10 presents results from estimates of equation (3) for our main outcomes of interest in the first generation. In these specifications, the main effects of both childcare and NHV are statistically significant and point to substantial improvements in education and adult income and improvements in survival rates for cohorts who were only exposed to either childcare or NHV.[45] However, the interaction coefficients are consistently opposite-signed. For cohorts who had NHV at birth, the positive impact of access to childcare at age 3 on years of schooling is reduced by 85 percent, while the decrease in the likelihood of only having a basic education is reduced by 83 percent. The increase in adult income is lowered by a marginally significant 89 percent. When we consider survival beyond age 65, the interaction coefficient is insignificant but similarly negative.

Table 11 shows the interaction results separately for males and females. As with the main effects of childcare, the impacts on education are similar across the two genders. Again, the estimated effects on income are driven by males. The interaction effects consistently point toward substitutability between childcare and NHV—cohorts who are only exposed to childcare at age 3 benefit more from it than cohorts who are also exposed to NHV at birth.

Figures 11, 12, and 13 explore the heterogeneity in interaction effects by age of follow-up. The interaction effects for education are negative at all ages and statistically significant at ages 45-65. The interaction effects for adult income among males are negative beginning around age 40, and statistically significant around ages 50 and 55. The interaction effects for survival among females are negative at all ages 55 to 65, but imprecise.

Appendix Tables 6 through 10 show that our interaction results are robust across a

[44] In Appendix B, we show that these health effects of NHV also hold in our sample of 140 municipalities that ever implement childcare.

[45] We should note that these estimates should not be directly compared to the main effects of either childcare or NHV in regressions without interactions, as the main effects in Table 10 are conditional on the other program not being present.

number of specifications that vary the control variables and sample. We exclude county linear trends (Appendix Table 6), include urban/rural indicators interacted with trends (Appendix Table 7), include municipality-specific linear pre-trends (Appendix Table 8), control for all available interpolated municipality variables (Appendix Table 9), and drop post-1949 cohorts in the 28 municipalities with worse NHV program data (Appendix Table 10). While the coefficients vary slightly in magnitude and statistical significance, the overall story remains the same. There are strong positive main effects of childcare and NHV on adult well-being, while the interaction effects of the two programs are negative.

Finally, Table 12 presents the interaction effects for the educational outcomes of the second generation. The main effect of childcare remains statistically significant and positive, while the interaction effects are opposite-signed (although insignificant).

Discussion. The estimated negative interaction effects between NHV and childcare are consistent with the interpretation that these two interventions are substitutes—rather than complements—and that this relationship persists in the long-run and possibly into the next generation. While we cannot directly observe the channels through which these interaction effects operate, we briefly discuss the potential mechanisms and alternative explanations here.

The NHV program likely affected infants' exposure to disease and nutrition during the first year of life through its focus on the importance of breastfeeding and having a sanitary home environment. Regulated childcare centers probably affected children's lives in multiple ways: Many low-income children of working mothers likely received higher quality care and early education than they would have in alternative care arrangements (e.g., in the care of relatives or neighbors). In addition, as described in Section 2, the centers provided children with a nutritious diet and health monitoring. Further, incomes in families with childcare access may have increased because mothers could work more. Our negative interaction results suggest that the marginal benefits of these different aspects of childcare were larger for children who did not already receive the NHV health benefits than for children whose health was improved by NHV.

However, alternative explanations are possible. First, since the two programs were gradually rolled out over a fairly long time period and neither program was discontinued in any municipality, our variation creates a setting where municipalities that approved childcare centers in later years were more likely to also have NHV than municipalities with earlier childcare center approvals. If the treatment effect of childcare is lower in municipalities with later approvals than in municipalities with earlier approvals, then the negative interaction between childcare and NHV may in part pick up some of this treatment effect heterogeneity. In Appendix Table 11, we test whether the main effect of childcare is different between early and late implementing municipalities. Specifically, we augment equation (1) to include an interaction term between treatment and an indicator for a municipality being a "late implementer" (defined as having a first childcare center approval in 1940 or later). We see no statistically significant differences between early and late implementers; if anything, the signs of the interaction coefficients suggest that the treatment effects of childcare were actually larger among municipalities with later approvals than among those with earlier approvals.

Second, since both programs were implemented at the municipality level, there is a concern about "overlapping labor markets". In particular, one might worry that NHV program implementation limited nurses' ability to work at childcare centers (since nurses could serve on child care center boards), leading to a reduction in the effectiveness of childcare. However, this concern is not especially relevant to our setting since NHV program nurses were highly specialized and required additional training beyond standard nurse certification. It is unlikely that NHV nurses were also in the relevant pool of childcare personnel.

Third, our estimates cannot speak to potential parental behavioral responses to these public interventions. Given that childcare reduces the costs of maternal employment, it is possible that parental private investments into children became lower once high-quality childcare became available and mothers went to work. Moreover, if parents of children who had both NHV at birth and childcare 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 parental response. Unfortunately, we do not have any data on parental investment behaviors during this time period and thus cannot address this possibility in our analysis.

Finally, while the magnitudes of these interaction effects may seem large, they are actually quite similar to the two other concurrent studies that report evidence of substitutability across early investments in completely 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 very different types of interventions in Denmark, are surprisingly comparable.

6 Conclusion

Although the existing literature has largely reached a consensus on the importance of early-life investments, whether and how different types of investments interact in their long-term effects remains an open question. In this paper, we begin to shed light on this issue with some of the first quasi-experimental evidence on interactions between two typical early interventions—a high quality preschool childcare program and a nurse home visiting program. Our natural experiments take place in early 20th century Denmark, providing us with a unique opportunity to study their long-run consequences for life-long and intergenerational outcomes.

Using historical data on the timing of child care center approvals across Danish municipalities together with administrative data on outcomes for nearly 1 million Danish people born between 1930 and 1957, we document strong positive long-term effects of early access to childcare. Cohorts with access to formal childcare by age 3 have 1.6 percent more years of schooling and are 9.7 percent less likely to only have a compulsory education. For males, income is increased by 1-3 percent between the ages of 35 and 60, while females are 0.8 percent more likely to survive beyond age 65. We also find evidence of persistent intergenerational impacts—children of women with early-life access to childcare have 0.4 percent more years of schooling and are 6 percent less likely to only have a compulsory education.

However, when we interact childcare access at age 3 with access to the NHV program in infancy—which was previously shown to have large positive effects on infant and adult health (Wüst, 2012; Hjort *et al.*, 2014)—we find that the two interventions act as substitutes rather than complements. Individuals only exposed to childcare 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 childcare on years of schooling is reduced by 85 percent. For men, the increase in adult income is lowered by 86 percent. While the interaction effects for female survival and the education of the second generation are statistically insignificant, they are consistent with substitutability between childcare and NHV as well.

Similar to other studies showing that the most disadvantaged children experience the biggest gains from early-life interventions (Bitler *et al.*, 2012; Havnes and Mogstad, 2011; Meyer and Wherry, 2012; Carneiro *et al.*, 2011), our results suggest that individuals who did *not* benefit from the positive health impacts of NHV have larger impacts of childcare relative to those who did. Our evidence implies that a high quality preschool childcare program can compensate for low initial health, which is reassuring in light of the substantial disparities in infant health across socio-economic groups today in the United States (Currie, 2011; Chen *et al.*, 2014; Aizer and Currie, 2014). Although low socio-economic status children suffer from substantial disadvantages at birth in terms of health and parental resources, our findings imply that early public interventions can work against some of these initial shortcomings and potentially reduce inequalities in outcomes over the life cycle and across generations.

While our study provides some of the first causal evidence on interactions between early interventions, our results cannot speak to the possible role of later interventions. For example, are programs targeting low-income teenagers complements or substitutes to earlier investments? Future research may explore how investments at different points in the life cycle interact with one another.

References

Adhvaryu, A., Molina, T., Nyshadham, A. and Tamayo, J. (2015). Helping children catch up: Early life shocks and the *Progresa* experiment, University of Michigan, unpublished manuscript.

— and Nyshadham, A. (2014). Endowments at birth and parents' investments in children. *The Economic Journal*.

AIZER, A. and CUNHA, F. (2012). The production of human capital in childhood: endowments, investments and fertility, Unpublished manuscript, Brown University.

— and CURRIE, J. (2014). The intergenerational transmission of inequality: Maternal disadvantage and health at birth. *Science*, **344** (6186), 856–861.

ALMOND, D. and CURRIE, J. (2011). Chapter 15: Human capital development before age five. In O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, *Handbook of Labor Economics*, vol. 4, Part 2, Elsevier, pp. 1315–1486.

— and MAZUMDER, B. (2013). Fetal origins and parental responses. *Annual Review of Economics*, **5** (1), 37–56.

ANDERSON, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American Statistical Association*, **103** (484).

BAKER, M., GRUBER, J. and MILLIGAN, K. (2008). Universal child care, maternal labor supply, and family well-being. *Journal of Political Economy*, **116** (4), 709–745.

—, — and — (2015). *Non-Cognitive Deficits and Young Adult Outcomes: The Long-Run Impacts of a Universal Child Care Program*. Working Paper 21571, National Bureau of Economic Research.

BARKER, D. J. (1990). The fetal and infant origins of adult disease. *BMJ: British Medical Journal*, **301** (6761), 1111.

BECKER, G. S. and TOMES, N. (1986). Human capital and the rise and fall of families. *Journal of Labor Economics*, **4** (3), S1–S39.

BELFIELD, C. R., NORES, M., BARNETT, S. and SCHWEINHART, L. (2006). The high/scope perry preschool program cost–benefit analysis using data from the age-40 followup. *Journal of Human Resources*, **41** (1), 162–190.

BERLINSKI, S., GALIANI, S. and GERTLER, P. (2009). The effect of pre-primary education on primary school performance. *Journal of public Economics*, **93** (1), 219–234.

BHALOTRA, S., KARLSSON, M., NILSSON, T. *et al.* (2015). *Infant health and longevity: evidence from a historical trial in Sweden*. Discussion Paper 2015-08, IZA, Institute for Social and Economic Research.

BHALOTRA, S. R. and VENKATARAMANI, A. (2012). Shadows of the captain of the men of death: Early life health interventions, human capital investments, and institutions, University of Essex, unpublished manuscript.

BHARADWAJ, P., LOKEN, K. V. and NEILSON, C. (2013). Early life health interventions and academic achievement. *American Economic Review*, **103** (5), 1862–91.

BINGLEY, P., JENSEN, V. M. and NIELSEN, S. S. (2015). Maternal employment, childcare, and long-run child outcomes, SFI, The Danish National Centre for Social Research, unpublished manuscript.

BITLER, M., DOMINA, T. and HOYNES, H. W. (2012). *Experimental evidence on distributional effects of head start*. Tech. rep.

Bitler, M. P. and Currie, J. (2005). Does wic work? the effects of wic on pregnancy and birth outcomes. *Journal of Policy Analysis and Management*, **24** (1), 73–91.

Black, S. E., Butikofer, A., Devereux, P. J. and Salvanes, K. G. (2013). *This Is Only a Test? Long-Run Impacts of Prenatal Exposure to Radioactive Downfall*. Working Paper 18987, National Bureau of Economic Research.

Bütikofer, A., Løken, K. V. and Salvanes, K. (2014). Long-term consequences of access to well-child visits, Norwegian School of Economics, unpublished manuscript.

Buus, H. (2001). *Sundhedsplejerskeinstitutionens Dannelse [The Introduction of the Danish Home Visiting Program]*. Museum Tusculanum Press.

Campbell, F., Conti, G., Heckman, J. J., Moon, S. H., Pinto, R., Pungello, E. and Pan, Y. (2014). Early childhood investments substantially boost adult health. *Science*, **343** (6178), 1478–1485.

Carneiro, P. and Ginja, R. (2012). *Long Term Impacts of Compensatory Preschool on Health and Behavior: Evidence from Head Start*. IZA Discussion Papers 6315, Institute for the Study of Labor (IZA).

—, Loken, K. V. and Salvanes, K. G. (2011). *A flying start? Maternity leave benefits and long run outcomes of children*. Discussion Paper 5793, IZA.

Cascio, E. U. (2009). *Do investments in universal early education pay off? long-term effects of introducing kindergartens into public schools*. Working Paper 14951, National Bureau of Economic Research.

Chen, A., Oster, E. and Williams, H. (2014). Why is infant mortality in the us higher than in europe?, University of Chicago, unpublished manuscript.

Cunha, F. and Heckman, J. (2007). The technology of skill formation. *The American Economic Review*, **97** (2), 31–47.

—, Heckman, J. J. and Schennach, S. M. (2010). Estimating the technology of cognitive and noncognitive skill formation. *Econometrica*, **78** (3), 883–931.

Currie, J. (2011). Inequality at birth: Some causes and consequences. *The American Economic Review*, **101** (3), 1–22.

— and Rajani, I. (2014). *Within-Mother Estimates of the Effects of WIC on Birth Outcomes in New York City*. Working Paper 20400, National Bureau of Economic Research.

— and Rossin-Slater, M. (2015). Early-life origins of life-cycle well-being: Research and policy implications. *Journal of Policy Analysis and Management*, **34** (1), 208–242.

— and Thomas, D. (1995). Does head start make a difference? *American Economic Review*, **85** (3), 341–364.

Dahl, G. B. and Lochner, L. (2012). The impact of family income on child achievement: Evidence from the earned income tax credit. *The American Economic Review*, **102** (5), 1927–1956.

Danish Data Archive (). *Statistical Danish Commune Archive*. Archive data material.

Datta Gupta, N. and Simonsen, M. (2010). Non-cognitive child outcomes and universal high quality child care. *Journal of Public Economics*, **94** (1), 30–43.

Deming, D. (2009). Early childhood intervention and life-cycle skill development: Evidence from head start. *American Economic Journal: Applied Economics*, pp. 111–134.

DNBH (various years). *Medical Report for the Kingdom of Denmark*. The Danish National Board of Health.

DST (2008). *60 år i tal - Danmark siden 2. verdenskrig [60 years in numbers - Denmark since WWII]*. Statistics Denmark.

Fitzpatrick, M. D. (2008). Starting school at four: The effect of universal pre-kindergarten on children's academic achievement. *The BE Journal of Economic Analysis & Policy*, **8** (1).

Forsdahl, A. (1979). Are poor living conditions in childhood and adolescence an important risk factor for arteriosclerotic heart disease? *British Journal of Preventive and Social Medicine*, **31** (2), 91–95.

Garces, E., Thomas, D. and Currie, J. (2002). Longer-term effects of head start. *The American Economic Review*, **92** (4), 999–1012.

Gormley, W. T. and Gayer, T. (2005). Promoting school readiness in oklahoma an evaluation of tulsa's pre-k program. *Journal of Human resources*, **40** (3), 533–558.

Gunnsteinsson, S., Adhvaryu, A., Christian, P., Labrique, A., Sugimoto, J., Shamim, A. A. and West, K. P. J. (2014). Resilience to early-life shocks, University of Michigan, unpublished manuscript.

Harding, K., Galano, J., Martin, J., Huntington, L. and Schellenbach, C. J. (2007). Healthy families america® effectiveness: A comprehensive review of outcomes. *Journal of Prevention & Intervention in the Community*, **34** (1-2), 149–179.

Havnes, T. and Mogstad, M. (2011). No child left behind: Subsidized child care and children's long-run outcomes. *American Economic Journal: Economic Policy*, **3** (2), 97–129.

Heckman, J. and Masterov, D. (2007). The productivity argument for investing in young children. *Applied Economic Perspectives and Policy*, **29**.

—, Pinto, R. and Savelyev, P. (2013). Understanding the mechanisms through which an influential early childhood program boosted adult outcomes. *The American Economic Review*, **103** (6), 1–35.

Heckman, J. J., Moon, S. H., Pinto, R., Savelyev, P. A. and Yavitz, A. (2010). The rate of return to the highscope perry preschool program. *Journal of Public Economics*, **94** (1), 114–128.

Herbst, C. M. and Tekin, E. (2010). Child care subsidies and child development. *Economics of Education Review*, **29** (4), 618–638.

Hjort, J., Sø lvsten, M. and Wüst, M. (2014). *Universal investments in infant and long-run health - Evidence from Denmark's 1937 home visiting program*. SFI Working Paper 08:2014.

Hoynes, H., Page, M. and Stevens, A. H. (2011). Can targeted transfers improve birth outcomes?: Evidence from the introduction of the wic program. *Journal of Public Economics*, **95** (7), 813–827.

HOYNES, H. W., SCHANZENBACH, D. W. and ALMOND, D. (2012). *Long Run Impacts of Childhood Access to the Safety Net*. Working Paper 18535, National Bureau of Economic Research.

HUSTEDT, J. T., BARNETT, W. S., JUNG, K. and FIGUERAS, A. (2008). *Impacts of New Mexico Pre-K on Children's School Readiness at Kindergarten Entry: Results from the Second Year of a Growing Initiative*. Report, National Institute for Early Education Research, Rutgers University.

JOYCE, T., RACINE, A. and YUNZAL-BUTLER, C. (2008). Reassessing the wic effect: Evidence from the pregnancy nutrition surveillance system. *Journal of Policy Analysis and Management*, **27** (2), 277–303.

LOEB, S., BRIDGES, M., BASSOK, D., FULLER, B. and RUMBERGER, R. W. (2007). How much is too much? the influence of preschool centers on children's social and cognitive development. *Economics of Education Review*, **26** (1), 52–66.

LUDWIG, J. and MILLER, D. (2007). Does head start improve children's life chances? evidence from a regression discontinuity design*. *The Quarterly Journal of economics*, **122** (1), 159–208.

MAGNUSON, K. A., RUHM, C. and WALDFOGEL, J. (2007). Does prekindergarten improve school preparation and performance? *Economics of Education Review*, **26** (1), 33–51.

MALAMUD, O., POP-ELECHES, C. and URQUIOLA, M. (2015). Understanding interactions between family and school environments in human capital formation, Columbia University, unpublished manuscript.

MASSE, L. N. and BARNETT, W. S. (2002). A benefit-cost analysis of the abecedarian early childhood intervention. *Cost-Effectiveness and Educational Policy, Larchmont, NY: Eye on Education, Inc*, pp. 157–173.

MEYER, B. D. and WHERRY, L. R. (2012). *Saving teens: Using a policy discontinuity to estimate the effects of medicaid eligibility*. Working Paper 18309, National Bureau of Economic Research.

OFFICE OF PLANNING, RESEARCH, AND EVALUATION (2010). *Head Start Impact Study Final Report*. Report.

OLDS, D. L. (2006). The nurse–family partnership: An evidence-based preventive intervention. *Infant Mental Health Journal*, **27** (1), 5–25.

OLIVETTI, C. (2013). *The female labor force and long-run development: the American experience in comparative perspective*. Working Paper 19131, National Bureau of Economic Research.

PEDERSEN, C. B., GOTZSCHE, H., MØ LLER, J. O. and MORTENSEN, P. B. (2006). The danish civil registration system. *Danish Medical Bulletin*, **53** (4), 441–450.

PEDERSEN, J. (2009). *Danmarks økonomiske historie 1910-1960 [Denmark's economic history 1910-1960]*.

POULSEN, H. (2002). *Besættelsesårene 1940-1945 [The years of occupation 1940-1945]*.

ROSSIN-SLATER, M. (2013). WIC in your neighborhood: New evidence on the impacts of geographic access to clinics. *Journal of Public Economics*, **102** (0), 51 – 69.

Schweinhart, L. J., Montie, J., Xiang, Z., Barnett, W. S., Belfield, C. R. and Nores, M. (2005). *Lifetime effects: The High/Scope Perry Preschool study through age 40*. Ypsilanti, MI: High/Scope Press.

Sievertsen, H. H. and Wüst, M. (2015). Discharge on the day of birth, parental response, and health and schooling outcomes, SFI, The Danish National Centre for Social Research, unpublished manuscript.

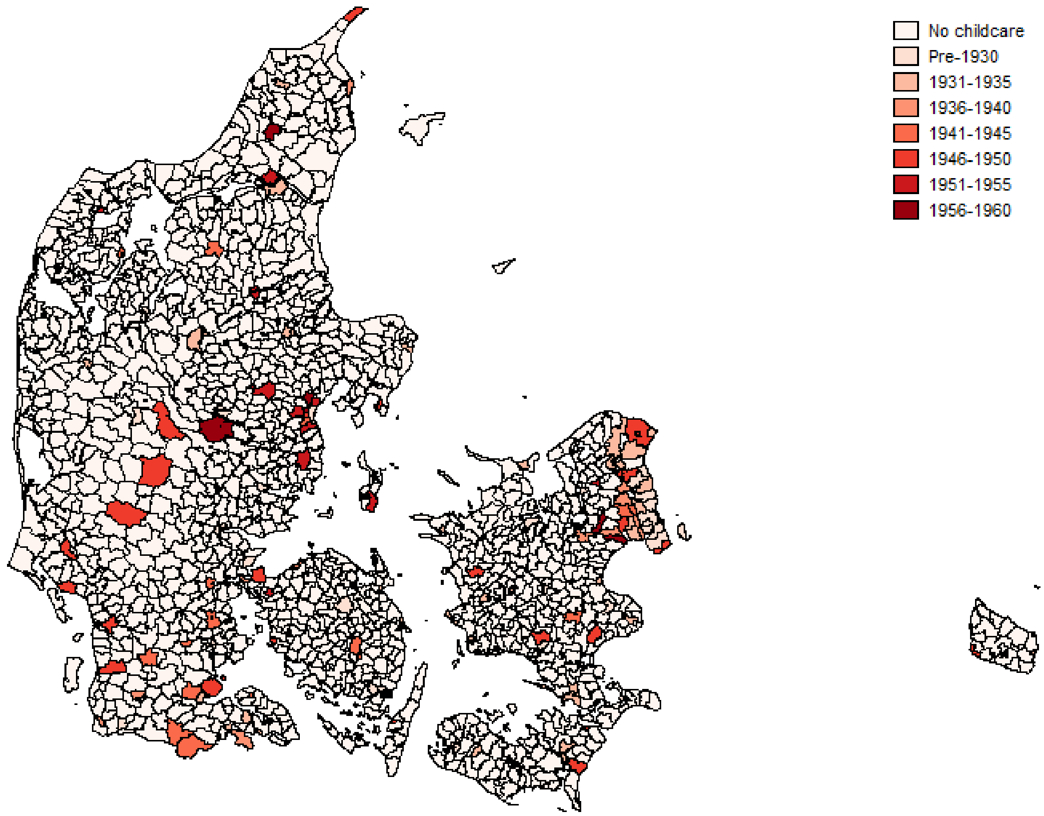
Skjernbæk, O. J. (various years). *Institutioner til værn for børn og unge i Danmark [Institutions for the protection of children and youth in Denmark]*.

St. Pierre, R. G. and Layzer, J. I. (1999). Using home visits for multiple purposes: the comprehensive child development program. *Future of Children*, **9**, 134–151.

Wong, V. C., Cook, T. D., Barnett, W. S. and Jung, K. (2008). An effectiveness-based evaluation of five state pre-kindergarten programs. *Journal of Policy Analysis and Management*, **27** (1), 122–154.

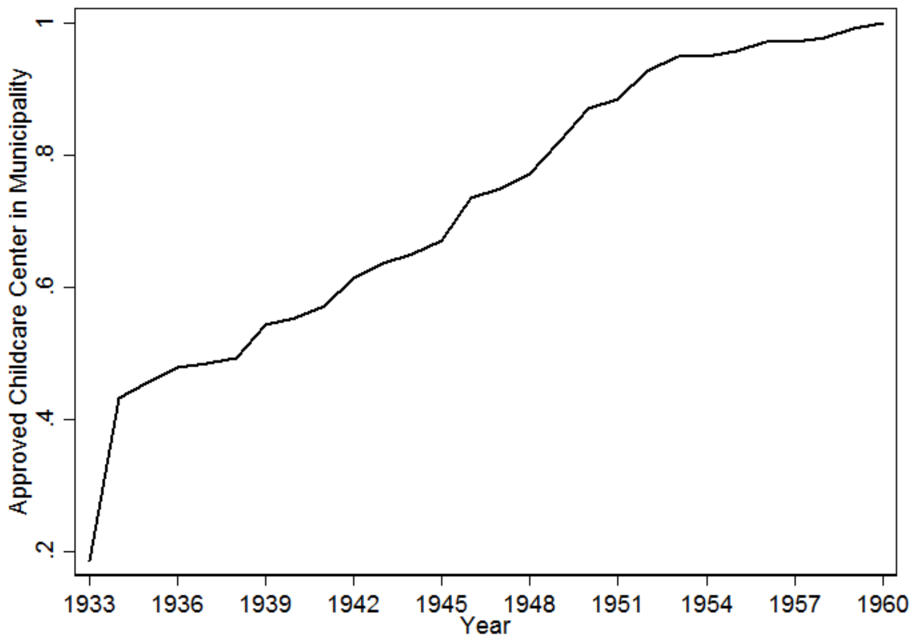
Wüst, M. (2012). Early interventions and infant health: Evidence from the danish home visiting program. *Labour Economics*, **19**, 484–495.

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



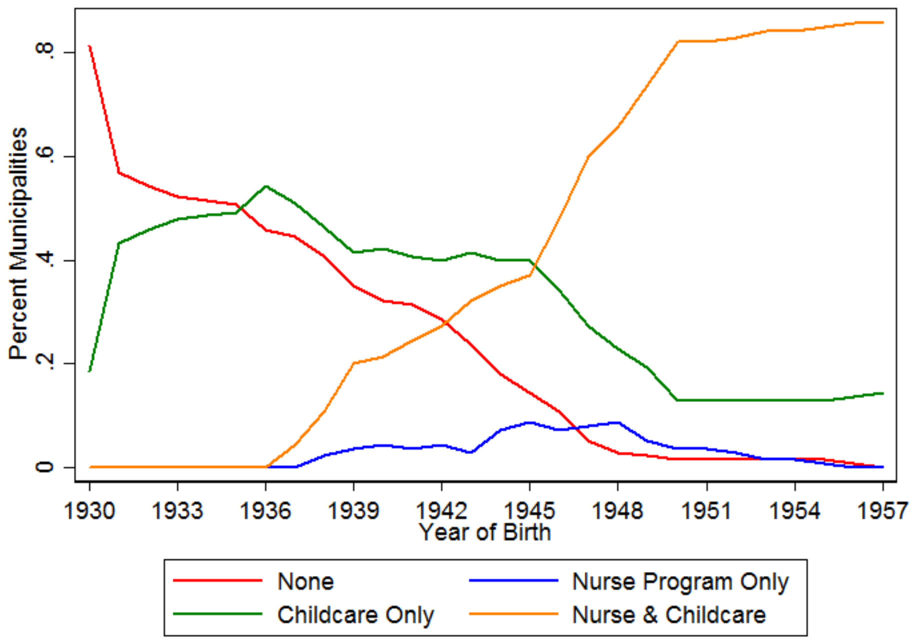
Notes: This map shows the evolution of childcare center approvals across Danish municipalities through 1960. Our analysis sample is limited to the 140 municipalities that ever had an approved childcare center by 1960.

Figure 2: Percent of Municipalities with an Approved Childcare Center by Year



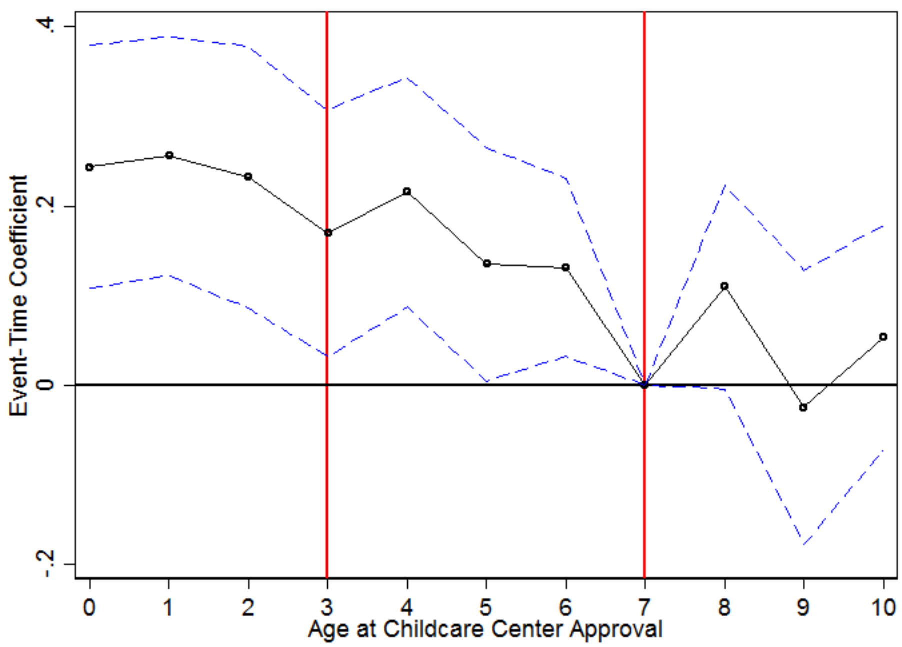
Notes: The sample is limited to the 140 municipalities that ever had an approved childcare center by 1960. This graph shows the percent of municipalities that had an approved childcare center in each year.

Figure 3: Variation in Childcare and Nurse Program Availability by Year of Birth



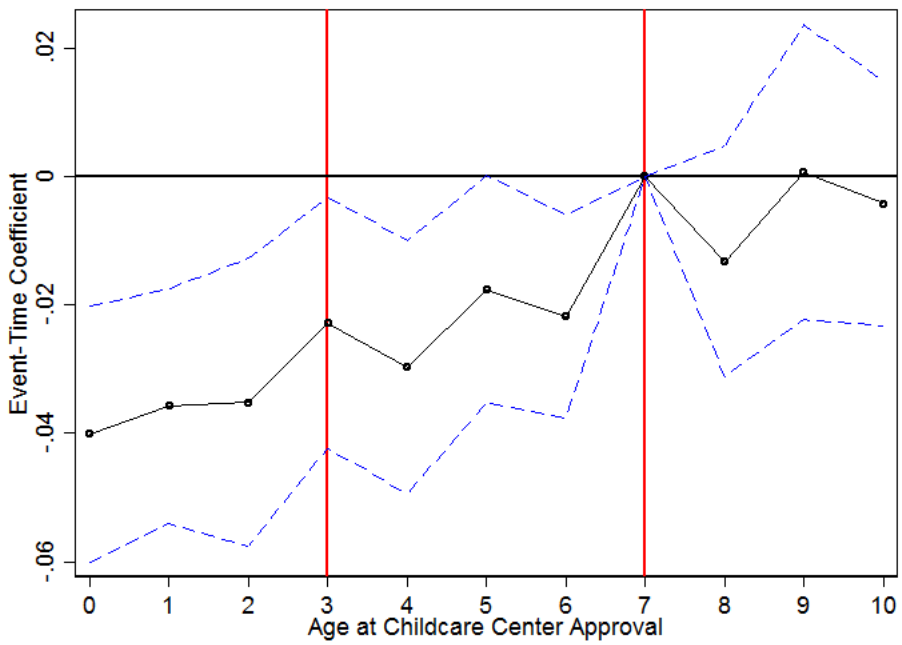
Notes: This graph shows for each cohort the percent of municipalities that had: (1) no childcare at age 3 and no nurse program at birth in red; (2) childcare at age 3 but no nurse program at birth in green; (3) nurse program at birth but no childcare at age 3 in blue; and (4) childcare at age 3 and nurse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

Figure 4: Effect of Access to Childcare on Years of Education at Age 50 by Age of Exposure



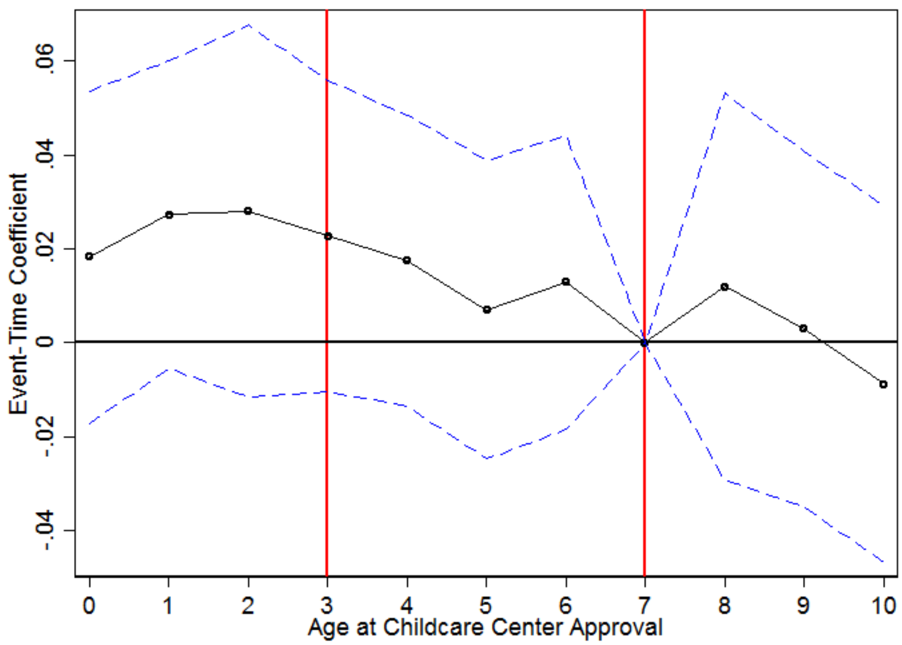
Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year collapsed data. The sample is limited to the 140 municipalities that ever had a childcare center by 1960. The regression includes indicators for the cohorts' single years of age in the year of the childcare center approval in their municipality of birth between 0 and 10 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born after the childcare center approval (i.e., age less than 0) and an indicator for cohorts being older than age 10 at the time of approval. The regression 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 5: Effect of Access to Childcare on Indicator for Basic Education at Age 50 by Age of Exposure



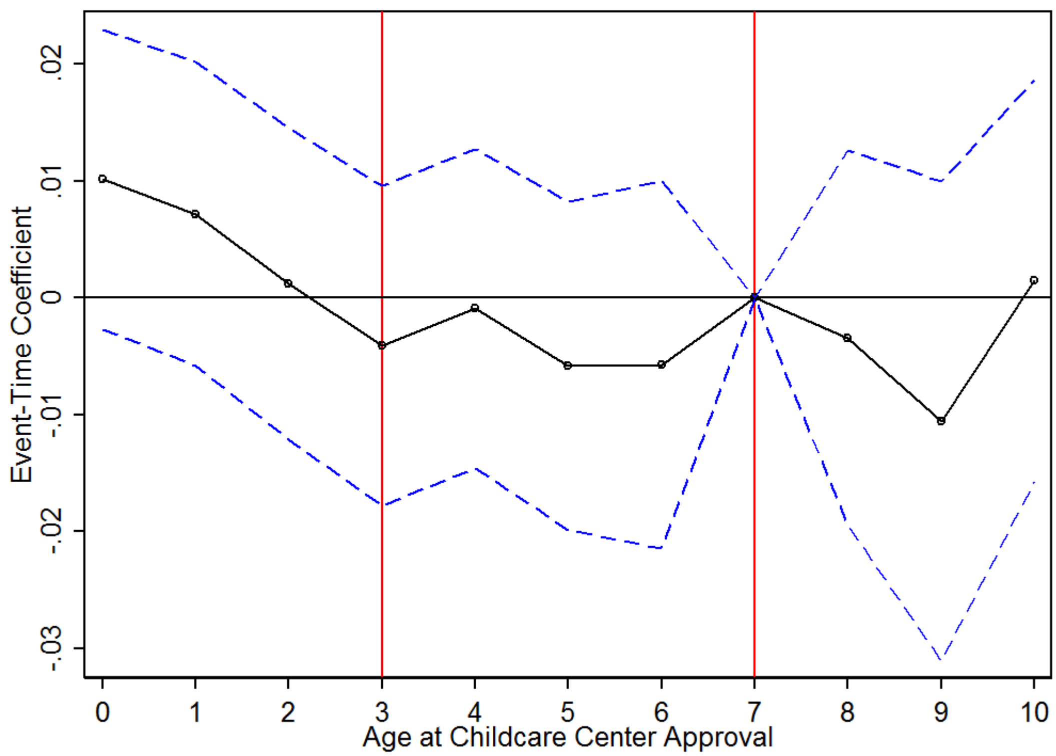
Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year collapsed data. The sample is limited to the 140 municipalities that ever had a childcare center by 1960. The regression includes indicators for the cohorts' single years of age in the year of the childcare center approval in their municipality of birth between 0 and 10 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born after the childcare center approval (i.e., age less than 0) and an indicator for cohorts being older than age 10 at the time of approval. The regression 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 6: Effect of Access to Childcare on Log Mean Total Income between Ages 49 and 51 by Age of Exposure for Males



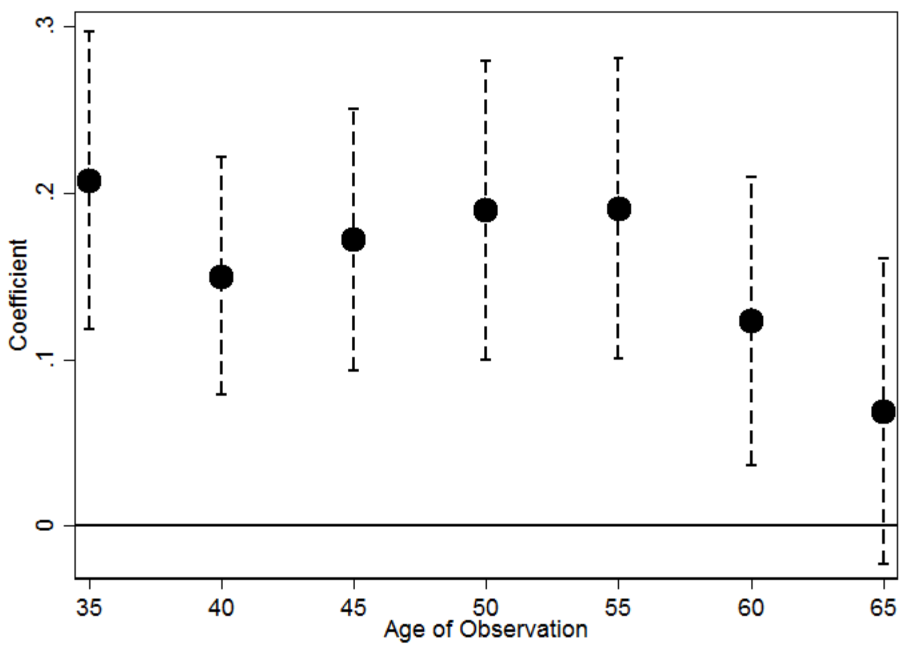
Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year×gender collapsed data. The sample is limited to males born in the 140 municipalities that ever had a childcare center by 1960. The regression includes indicators for the cohorts' single years of age in the year of the childcare center approval in their municipality of birth between 0 and 10 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born after the childcare center approval (i.e., age less than 0) and an indicator for cohorts being older than age 10 at the time of approval. The regression 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×gender cell. Standard errors are clustered on the municipality level.

Figure 7: Effect of Access to Childcare on Survival beyond Age 65 by Age of Exposure for Females



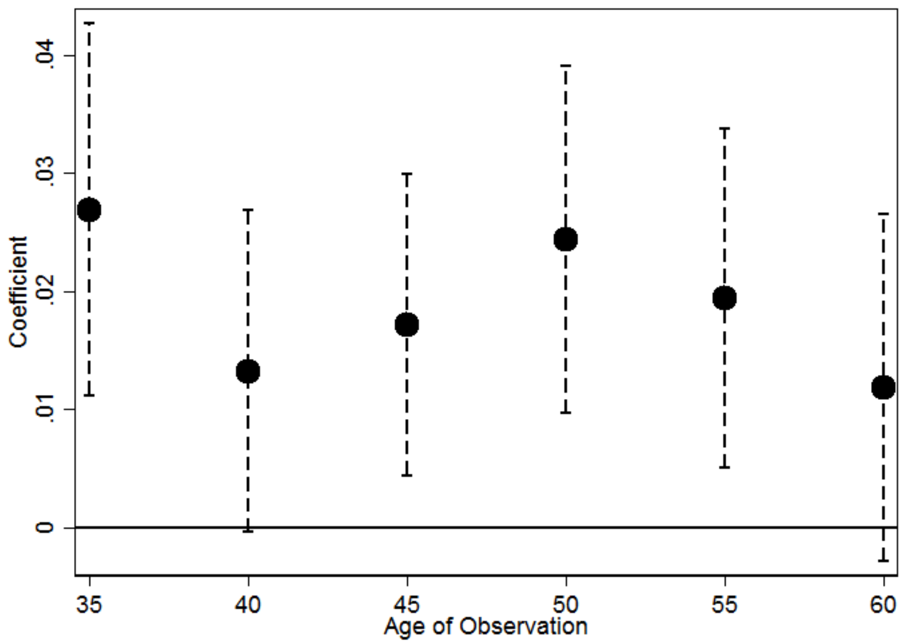
Notes: This figure shows the coefficients and 95% confidence intervals from an event-study regression estimated on the municipality×birth-year×gender collapsed data. The sample is limited to females born in the 140 municipalities that ever had a childcare center by 1960. Additionally, the sample is limited to only those individuals who have survived to at least age 50. The regression includes indicators for the cohorts' single years of age in the year of the childcare center approval in their municipality of birth between 0 and 10 (with age 7 as the omitted category). The regression also includes an indicator for cohorts being born after the childcare center approval (i.e., age less than 0) and an indicator for cohorts being older than age 10 at the time of approval. The regression 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 8: Effect of Access to Childcare at Age 3 on Years of Education by Age of Follow-Up



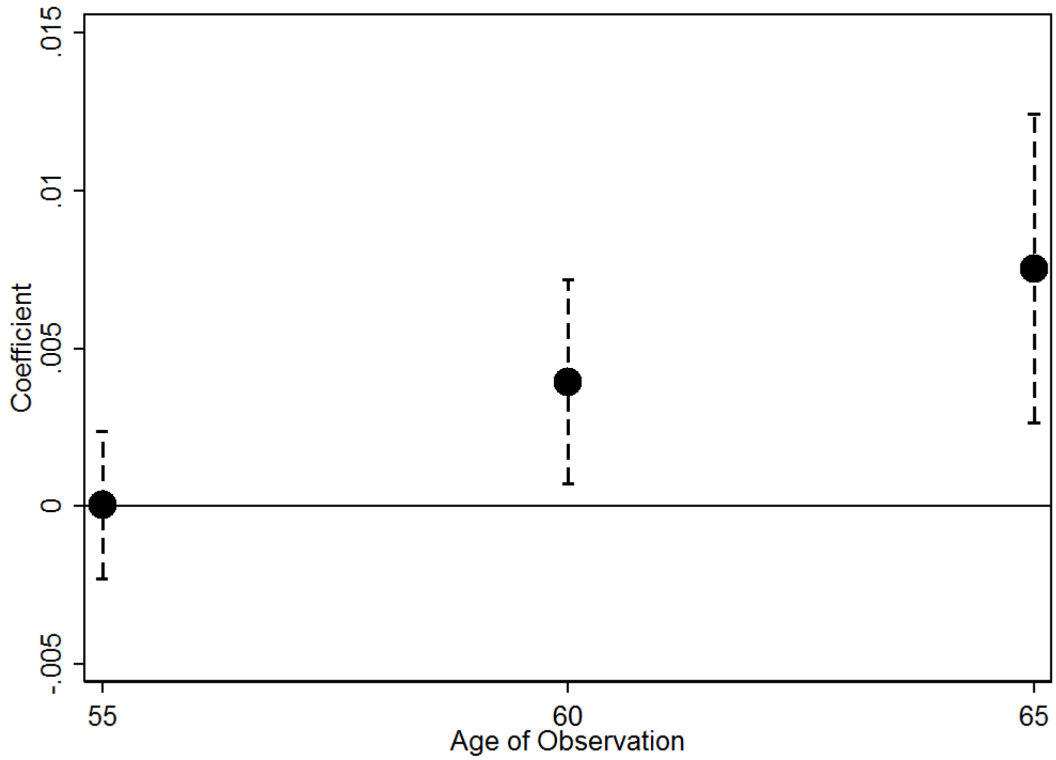
Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year collapsed data. The sample is limited to the 140 municipalities that ever had a childcare center by 1960. The coefficients plotted estimate the effect of access to childcare at age 3 on the outcome listed observed at ages reported on the x-axis. 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.

Figure 9: Effect of Access to Childcare at Age 3 on Log Mean Total Income by Age of Follow-Up for Males



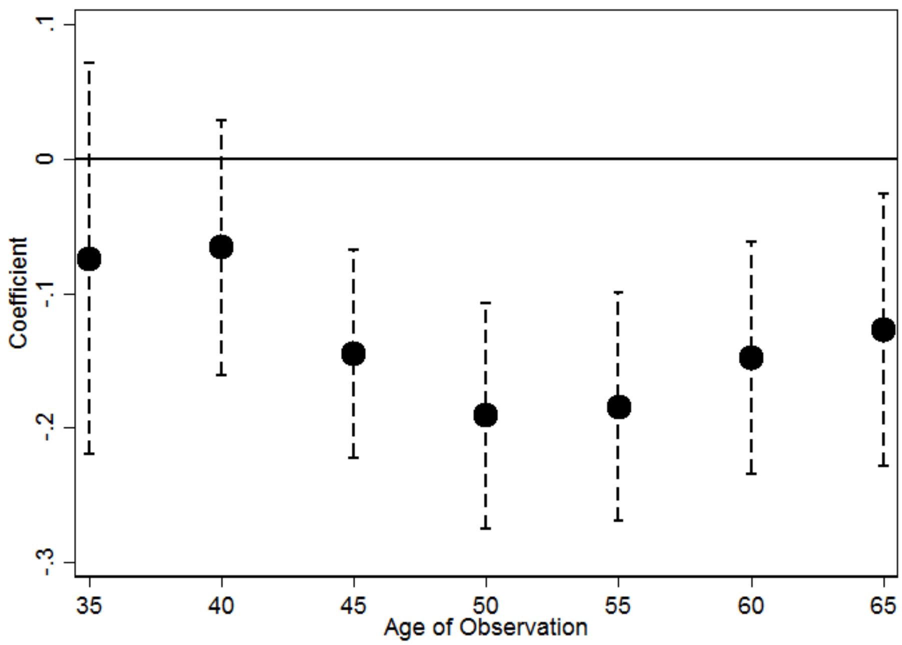
Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year×gender collapsed data. The sample is limited to males born in the 140 municipalities that ever had a childcare center by 1960. The coefficients plotted estimate the effect of access to childcare at age 3 on the outcome listed observed at ages reported on the x-axis. Log mean total income is calculated by taking the 3-year moving average of annual incomes surrounding each age reported on the x-axis (e.g., age 34-36 for age 35). 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×gender cell. Standard errors are clustered on the municipality level.

Figure 10: Effect of Access to Childcare at Age 3 on Survival by Age of Follow-Up for Females



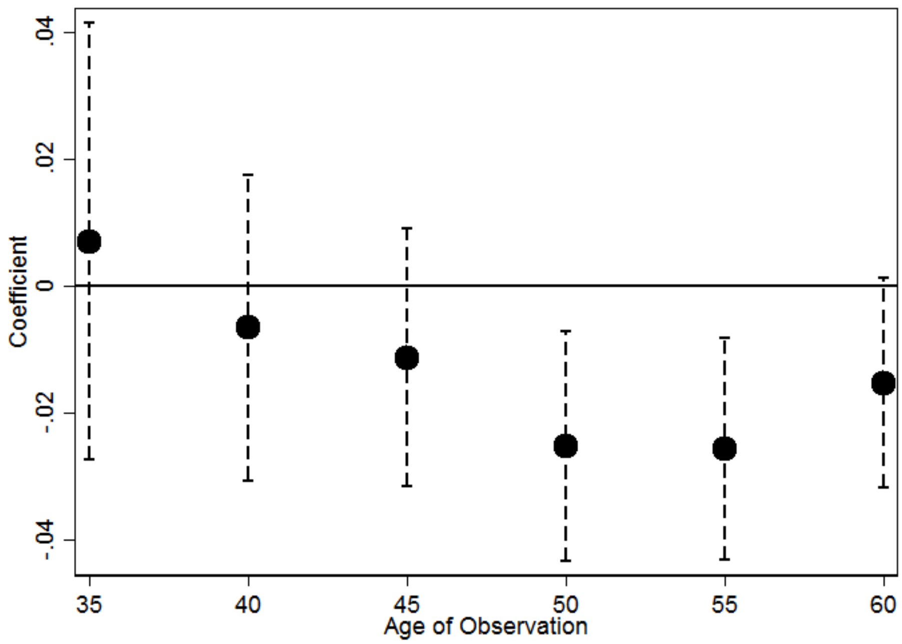
Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year×gender collapsed data. The sample is limited to females born in the 140 municipalities that ever had a childcare center by 1960. Additionally, the sample is limited to only those individuals who have survived to at least age 50. The coefficients plotted estimate the effect of access to childcare at age 3 on the outcome listed observed by the age reported on the x-axis. 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.

Figure 11: Interaction Effect of Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education by Age of Follow-Up



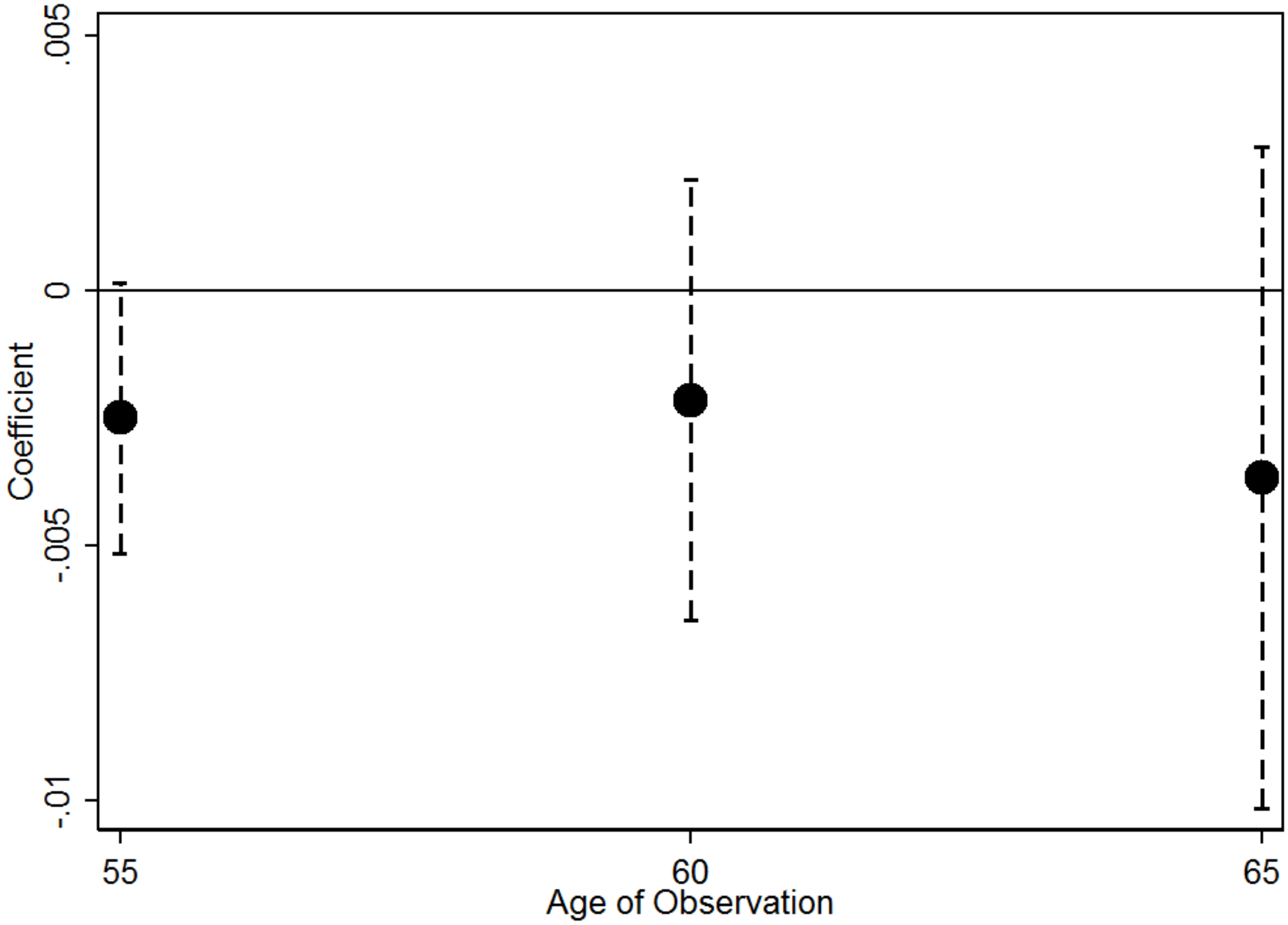
Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year collapsed data. The sample is limited to the 140 municipalities that ever had a childcare center by 1960. For the 28 municipalities that do not establish a nurse program by 1949 in our data, we drop cohorts born in 1950-1957 since we do not have information on nurse program initiation in those years. The coefficients plotted estimate the interaction between the effect of access to childcare at age 3 and access to higher schooling on the outcome listed observed by the age reported on the x-axis. 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.

Figure 12: Interaction Effect of Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Log Total Income by Age of Follow-Up for Males



Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year×gender collapsed data. The sample is limited to males born in the 140 municipalities that ever had a childcare center by 1960. The coefficients plotted estimate the interaction between the effect of access to childcare at age 3 and access to higher schooling on the outcome listed observed by the age reported on the x-axis. 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×gender cell. Standard errors are clustered on the municipality level.

Figure 13: Interaction Effect of Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Survival by Age of Follow-Up for Females



Notes: This figure shows the coefficients and 95% confidence intervals from separate regressions estimated on the municipality×birth-year×gender collapsed data. The sample is limited to females born in the 140 municipalities that ever had a childcare center by 1960. Additionally, the sample is limited to only those individuals who have survived to at least age 50. The coefficients plotted estimate the interaction between the effect of access to childcare at age 3 and access to higher schooling on the outcome listed observed by the age reported on the x-axis. 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×gender cell. Standard errors are clustered on the municipality level.

Table 1: Municipality Characteristics in 1929-1930

	(1) All Munis	(2) Ever Open Childcare	(3) No Open Childcare
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 a childcare center by 1960. Column (3) limits the sample to the other municipalities that never had a childcare center by 1960.

Table 2: Correlation Between Municipality Characteristics and Timing of Childcare Center Approval

	(1) Log Pop	(2) Pct Fem	(3) Pct Urb	(4) Pct Ind	(5) Pct Ag	(6) Pct Inc Tax	(7) Log Tax Inc	(8) Pct Prop Tax
Any Approved	0.0155	0.135	1.351**	-0.366	-1.404***	0.402	-0.0366	-0.243**
Childcare Center at Age 3	[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

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 a childcare center 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 individuals in our data who were born in each municipality in 1930. 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 the Nurse Program at Birth and Access to Childcare at Age 3

	(1) Nurse Program at Birth	(2) Any Operating Childcare Center at Age 3
Any Approved Childcare Center at Age 3	0.00862 [0.0433]	
Nurse Program 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 a childcare center 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: Effect of Access to Childcare at Age 3 on Education, Income, and Survival

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Yrs. School	0.284***	0.189***	0.074**	0.206**
	(0.088)	(0.046)	(0.034)	(0.087)
Mean of dep. var.	12.075	12.075	12.075	12.075
No. of obs.	3918	3918	3918	3918
Basic Educ.	-0.038***	-0.029***	-0.013***	-0.037***
	(0.011)	(0.006)	(0.005)	(0.012)
Mean of dep. var.	0.299	0.299	0.299	0.299
No. of obs.	3918	3918	3918	3918
Log Tot Inc.	0.040***	0.015**	0.009	0.024*
	(0.014)	(0.007)	(0.008)	(0.013)
Mean of dep. var.	12.557	12.557	12.557	12.557
No. of obs.	3778	3778	3778	3778
Survival beyond age 65	0.005**	0.005**	0.005**	0.004
	(0.002)	(0.002)	(0.002)	(0.004)
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
Muncipality:				
FE	Yes	Yes	Yes	Yes
X (ipolated)	No	No	Yes	No
FE × pre-trend	No	No	No	Yes
Linear county time trends	No	Yes	Yes	No

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 140 municipalities that ever had a childcare center 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

Table 5: Effect of Access to Childcare at Age 3 on Adult Labor Market Outcomes

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Log Wage Inc.	0.033***	0.015*	0.008	0.014
	(0.012)	(0.008)	(0.008)	(0.017)
Mean of dep. var.	12.429	12.429	12.429	12.429
No. of obs.	3777	3777	3777	3777
Any Wage Inc.	0.009**	0.004	0.003	0.005
	(0.004)	(0.002)	(0.003)	(0.006)
Mean of dep. var.	0.898	0.898	0.898	0.898
No. of obs.	3777	3777	3777	3777
Blue Collar Occ.	-0.006	-0.004	-0.006	-0.021
	(0.007)	(0.006)	(0.005)	(0.031)
Mean of dep. var.	0.336	0.336	0.336	0.336
No. of obs.	2380	2380	2380	2380
Cohort FE	Yes	Yes	Yes	Yes
Muncipality:				
FE	Yes	Yes	Yes	Yes
X (ipolated)	No	No	Yes	No
FE $\times$ pre-trend	No	No	No	Yes
Linear county time trends	No	Yes	Yes	No

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 140 municipalities that ever had a childcare center 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 6: Effect of Access to Childcare at Age 3 on Education, Income, and Survival; By Gender

	Yrs. School		Basic Ed.		Log Tot Inc		Survival beyond age 65	
	(1) Male	(2) Fem.	(3) Male	(4) Fem.	(5) Male	(6) Fem.	(7) Male	(8) Fem.
Any Approved Childcare Center at Age 3	0.204***	0.176***	-0.0305***	-0.0282***	0.0244***	0.00936	0.00158	0.00753***
	[0.0510]	[0.0500]	[0.00625]	[0.00703]	[0.00750]	[0.00958]	[0.00318]	[0.00249]
Mean, dept. var.	12.47	11.68	0.249	0.351	12.78	12.34	0.885	0.923
Diff/p-val	0.0282	0.513	-0.00229	0.709	0.0150	0.160	-0.00596	0.124
Observations (cells)	3903	3902	3903	3903	3762	3764	3905	3904

Notes: Each pair of columns presents first the estimate for males and then the estimate for females. The "Diff/p-val" row shows in the odd-numbered columns the corresponding differences in the childcare coefficients ($\beta^{Male} - \beta^{Female}$) and in the even-numbered columns the *p*-value associated with the test of equality across the two coefficients. The units of analysis are municipality×birth-year×gender cells. The sample is limited to the 140 municipalities that ever had a childcare center 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 county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year×gender cell. Standard errors are clustered on the municipality level.

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

Table 7: Effect of Access to Childcare at Age 3 on Adult Labor Market Outcomes; By Gender

	Log Wage Inc.		Any Wage Inc.		Blue Collar Occ.	
	(1) Male	(2) Fem.	(3) Male	(4) Fem.	(5) Male	(6) Fem.
Any Approved Childcare Center at Age 3	0.0250** [0.0105]	0.00589 [0.0102]	0.00634** [0.00309]	0.00229 [0.00318]	-0.00969 [0.00774]	0.00442 [0.00659]
Mean, dept. var.	12.62	12.22	0.924	0.873	0.451	0.221
Diff/p-val	0.0192	0.145	0.00404	0.288	-0.0141	0.105
Observations (cells)	3760	3759	3761	3763	2373	2368

Notes: Each pair of columns presents first the estimate for males and then the estimate for females. The "Diff/p-val" row shows in the odd-numbered columns the corresponding differences in the childcare coefficients ($\beta^{Male} - \beta^{Female}$) and in the even-numbered columns the *p*-value associated with the test of equality across the two coefficients. The units of analysis are municipality×birth-year×gender cells. The sample is limited to the 140 municipalities that ever had a childcare center 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×gender cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 8: Effect of Access to Childcare at Age 3 on Hospital Diagnoses by Age 60; By Gender

	Cardio		Heart		Diabetes		Cancer	
	(1) Male	(2) Fem.	(3) Male	(4) Fem.	(5) Male	(6) Fem.	(7) Male	(8) Fem.
Any Approved Childcare Center at Age 3	0.00536	-0.00191	0.000428	-0.00311*	0.0000883	-0.00220	-0.00363**	0.00139
	[0.00387]	[0.00390]	[0.00285]	[0.00165]	[0.00204]	[0.00153]	[0.00155]	[0.00223]
Mean, dept. var.	0.248	0.206	0.0844	0.0372	0.0515	0.0302	0.0617	0.0911
Diff/p-val	0.00727	0.199	0.00354	0.275	0.00228	0.311	-0.00502	0.0408
Observations (cells)	3905	3904	3905	3904	3905	3904	3905	3904

Notes: Each pair of columns presents first the estimate for males and then the estimate for females. The "Diff/p-val" row shows in the odd-numbered columns the corresponding differences in the childcare coefficients ($\beta^{Male} - \beta^{Female}$) and in the even-numbered columns the *p*-value associated with the test of equality across the two coefficients. The units of analysis are municipality×birth-year×gender cells. The sample is limited to the 140 municipalities that ever had a childcare center 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×gender cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 9: Effect of Access to Childcare at Age 3 on the Education of the Next Generation

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Child's years of schoolong	0.056**	0.043**	0.030	0.103
	(0.025)	(0.021)	(0.022)	(0.080)
Mean of dep. var.	12.338	12.338	12.338	12.338
No. of obs.	3197	3197	3197	3197
Child has basic education	-0.022***	-0.014***	-0.011**	-0.028*
	(0.006)	(0.004)	(0.005)	(0.017)
Mean of dep. var.	0.225	0.225	0.225	0.225
No. of obs.	3197	3197	3197	3197
Child has completed gymnasium	0.023***	0.009*	0.008	0.016
	(0.007)	(0.005)	(0.005)	(0.022)
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
Muncipality:				
FE	Yes	Yes	Yes	Yes
X (ipolated)	No	No	Yes	No
FE × pre-trend	No	No	No	Yes
Linear county time trends	No	Yes	Yes	No

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 140 municipalities that ever had a childcare center 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 10: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education, Income, and Survival

	Outcomes at Age 50			
	(1)	(2)	(3)	(4)
	Yrs. School	Basic Ed.	Log Tot Inc	Survival beyond age 65
Any Approved Childcare Center at Age 3	0.224***	-0.0344***	0.0178**	0.00505**
	[0.0490]	[0.00636]	[0.00839]	[0.00237]
Nurse Program at Birth	0.193***	-0.0257***	0.0270***	0.00586**
	[0.0504]	[0.00645]	[0.0100]	[0.00256]
Childcare x Nurse Program	-0.191***	0.0285***	-0.0159*	-0.00312
	[0.0428]	[0.00589]	[0.00927]	[0.00251]
Mean, dept. var.	12.07	0.299	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. The sample is limited to the 140 municipalities that ever had a childcare center 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 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 11: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education, Income, and Survival by Gender

	Yrs. School		Basic Ed.		Log Tot Inc		Survival beyond age 65	
	(1) Male	(2) Fem.	(3) Male	(4) Fem.	(5) Male	(6) Fem.	(7) Male	(8) Fem.
Any Approved	0.241***	0.209***	-0.0351***	-0.0340***	0.0292***	0.0121	0.00201	0.00820***
Childcare Center at Age 3	[0.0529]	[0.0542]	[0.00666]	[0.00758]	[0.00837]	[0.0112]	[0.00361]	[0.00274]
Nurse Program at	0.204***	0.176**	-0.0237***	-0.0268***	0.0319***	0.0215	0.00904**	0.00307
Birth	[0.0585]	[0.0680]	[0.00770]	[0.00880]	[0.0103]	[0.0163]	[0.00403]	[0.00327]
Childcare x Nurse	-0.199***	-0.184***	0.0251***	0.0318***	-0.0251***	-0.0147	-0.00268	-0.00368
Program	[0.0501]	[0.0618]	[0.00687]	[0.00818]	[0.00926]	[0.0164]	[0.00389]	[0.00331]
Mean, dept. var.	12.47	11.68	0.249	0.351	12.78	12.34	0.885	0.923
Observations (cells)	3903	3902	3903	3903	3762	3764	3905	3904

Notes: Each pair of columns presents first the estimate for males and then the estimate for females. The units of analysis are municipality×birth-year×gender cells. The sample is limited to the 140 municipalities that ever had a childcare center 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 county-specific linear time trends. All regressions are weighted by the number of observations in each municipality×birth-year×gender cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 12: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on the Education of the Next Generation

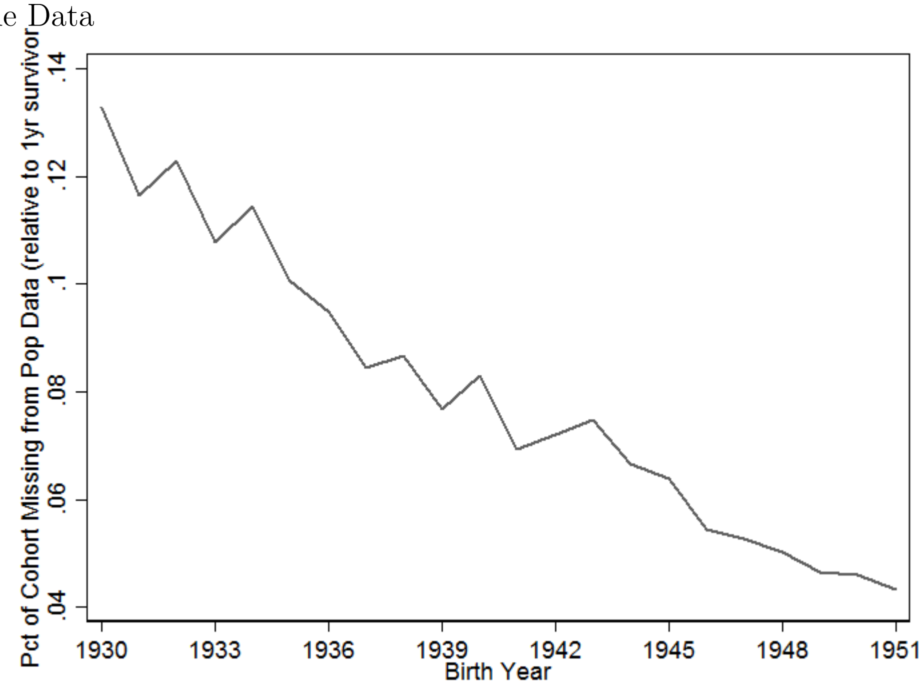
	Child Outcomes at Age 25		
	(1) Yrs.School	(2) Basic	(3) Gym.
Any Approved Childcare Center at Age 3	0.0464**	-0.0163***	0.0103*
	[0.0223]	[0.00503]	[0.00551]
Nurse Program at Birth	0.0166	-0.00960	0.00748
	[0.0275]	[0.00593]	[0.00753]
Childcare x Nurse Program	-0.0139	0.00825	-0.00442
	[0.0240]	[0.00526]	[0.00753]
Mean, dept. var.	12.34	0.225	0.253
Observations (cells)	3197	3197	3197

Notes: Each column reports the results from a separate regression. The sample is limited to the children of females who were born in 1935-1957 in the 140 municipalities that ever had a childcare center by 1960. The units of analysis are municipality×birth-year cells of the 1st generation of females with children. 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: Valid Parish Codes Among Danish-Born Individuals in the Outcome Data

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



Appendix Table 1: Correlation between Share of Cohort "Not Missing" and Access to Childcare. Sample: 86 Urban Municipalities.

	Share Not Missing			
	(1)	(2)	(3)	(4)
	Year/Muni FE	+ County Trends	+ Muni Chars	Muni FE Pre-Trends
Any Approved	-0.0639	0.129	0.112	0.110
Childcare Center	[0.160]	[0.0989]	[0.0847]	[0.218]
Mean, dept. var.	0.921	0.921	0.921	0.921
Observations (cells)	1548	1548	1548	1548

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 86 urban municipalities that ever had a childcare center 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 include municipality and year fixed effects. Column (2) adds county-specific linear time trends. Column (3) includes interpolated municipality×year controls. Column (4) includes municipality-specific linear pre-trends instead of county linear trends. All regressions are weighted by the number of observations in each municipality×year cell. Standard errors are clustered on the municipality level.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Appendix Table 2: Correlation Between Municipality Characteristics and Timing of Childcare Center Approval, Controlling for Rural/Urban Time Trends

	(1) Log Pop	(2) Pct Fem	(3) Pct Urb	(4) Pct Ind	(5) Pct Ag	(6) Pct Inc Tax	(7) Log Tax Inc	(8) Pct Prop Tax
Any Approved	0.0611	-0.312	0.725	0.424	-0.621*	-0.0818	-0.0858	-0.417*
Childcare Center at Age 3	[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 a childcare center by 1960. 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 individuals in our data who were born in each municipality in 1930. 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 Childcare at Age 3 on Education, Income, and Survival

Outcome	(1) (Balanced)	(2) (All)	(3) (All)	(4) (No CPH)	(5) (No always open)	(6) (1930-1949)	(7) (Ever-NHV)	(8) (Frac Yrs)
Yrs. School	0.189***	0.179***	0.146**	0.189***	0.085**	0.083*	0.170***	0.306***
	(0.046)	(0.067)	(0.066)	(0.047)	(0.039)	(0.044)	(0.048)	(0.060)
Mean of dep. var.	12.075	12.075	12.075	12.038	11.824	11.803	12.095	12.075
No. of obs.	3864	3918	3918	3890	3190	2798	3359	3864
Basic Educ.	-0.029***	-0.024***	-0.016**	-0.029***	-0.015***	-0.013**	-0.026***	-0.046***
	(0.006)	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)
Mean of dep. var.	0.299	0.299	0.299	0.305	0.328	0.323	0.297	0.299
No. of obs.	3864	3918	3918	3890	3190	2798	3359	3864
Log Tot Inc.	0.015**	0.032***	0.029**	0.014*	0.001	0.008	0.013	0.020**
	(0.007)	(0.012)	(0.012)	(0.007)	(0.007)	(0.009)	(0.008)	(0.008)
Mean of dep. var.	12.557	12.557	12.557	12.553	12.532	12.484	12.559	12.557
No. of obs.	3726	3778	3778	3751	3076	2658	3239	3726
Survival beyond age 65	0.005**	0.005**	0.007***	0.004**	0.005**	0.006**	0.006***	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Mean of dep. var.	0.903	0.903	0.903	0.906	0.906	0.883	0.903	0.903
No. of obs.	3864	3918	3918	3890	3190	2798	3359	3864
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municip FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Trends								
County	Yes	No	No	Yes	Yes	Yes	Yes	Yes
Urban/rural	No	Yes	No	No	No	No	No	No
Urban × county	No	No	Yes	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. 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: Annual Averages: Number of Childcare Enrollees; Number of Relevant Children Aged 3-7; Share of Children in Childcare. Sample: 86 Urban Municipalities.

Year	**No. of Children Enrolled**	**No. of Children Aged 3-7**	**Share in Childcare**
1940	177.71	2280.12	0.09
1941	184.20	2409.94	0.09
1942	182.23	2342.54	0.12
1943	190.25	2462.59	0.12
1944	205.16	2556.71	0.12
1945	215.46	2705.71	0.12
1946	215.47	2781.50	0.11
1947	232.12	2998.12	0.10
1948	246.52	3116.83	0.09
1949	260.07	3255.67	0.09
1950	252.01	2991.87	0.08
Total	216.50	2737.46	0.10

Notes: The sample is limited to urban municipality×year cells over 1940-1950 with an approved childcare center.

Appendix Table 5: Effect of Access to Childcare at Age 3 on the Fertility Outcomes of Females born in 1935-1957

	(1) No Kids	(2) Num. Kids	(3) Age at Fst. Birth	(4) Dad Ever Miss.
Any Approved	-0.00592*	0.000673	0.0793*	-0.00217
Childcare Center at Age 3	[0.00340]	[0.0138]	[0.0428]	[0.00335]
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 a childcare center by 1960. The units of analysis are municipality×birth-year cells. 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 6: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education, Income, and Survival; No Time Trends

	Outcomes at Age 50			
	(1)	(2)	(3)	(4)
	Yrs. School	Basic Ed.	Log Tot Inc	Survival beyond age 65
Any Approved Childcare Center at Age 3	0.319***	-0.0428***	0.0426***	0.00532**
	[0.0896]	[0.0109]	[0.0149]	[0.00223]
Nurse Program at Birth	0.257***	-0.0342***	0.0412***	0.00506**
	[0.0670]	[0.00843]	[0.0134]	[0.00238]
Childcare x Nurse Program	-0.232***	0.0336***	-0.0251**	-0.00290
	[0.0541]	[0.00721]	[0.0116]	[0.00232]
Mean, dept. var.	12.07	0.299	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. The sample is limited to the 140 municipalities that ever had a childcare center 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 7: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education, Income, and Survival; Rural/Urban Time Trends

	Outcomes at Age 50			
	(1) Yrs. School	(2) Basic Ed.	(3) Log Tot Inc	(4) Survival beyond age 65
Any Approved Childcare Center at Age 3	0.200***	-0.0273***	0.0339***	0.00522**
	[0.0701]	[0.00881]	[0.0129]	[0.00220]
Nurse Program at Birth	0.187***	-0.0251***	0.0359***	0.00500**
	[0.0585]	[0.00726]	[0.0119]	[0.00234]
Childcare x Nurse Program	-0.145***	0.0221***	-0.0189*	-0.00283
	[0.0509]	[0.00680]	[0.0107]	[0.00231]
Mean, dept. var.	12.07	0.299	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. The sample is limited to the 140 municipalities that ever had a childcare center 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 8: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education, Income, and Survival; with Municipality-Specific Pre-Trends

	Outcomes at Age 50			
	(1)	(2)	(3)	(4)
	Yrs. School	Basic Ed.	Log Tot Inc	Survival beyond age 65
Any Approved	0.206**	-0.0378***	0.0267*	0.00456
Childcare Center at Age 3	[0.0901]	[0.0120]	[0.0148]	[0.00393]
Nurse Program at	0.0315	0.00164	0.0195	0.00807
Birth	[0.0662]	[0.00958]	[0.0253]	[0.00619]
Childcare x Nurse	-0.0245	-0.00267	-0.00824	-0.00568
Program	[0.0684]	[0.00986]	[0.0266]	[0.00675]
Mean, dept. var.	12.07	0.299	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. The sample is limited to the 140 municipalities that ever had a childcare center 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 county-specific linear time trends. The regressions also include municipality fixed effects interacted with linear pre-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 9: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education, Income, and Survival; with All Municipality Controls

	Outcomes at Age 50			
	(1)	(2)	(3)	(4)
	Yrs. School	Basic Ed.	Log Tot Inc	Survival beyond age 65
Any Approved Childcare Center at Age 3	0.0892**	-0.0155***	0.00983	0.00640**
	[0.0396]	[0.00535]	[0.00919]	[0.00252]
Nurse Program at Birth	0.107**	-0.0131**	0.0232**	0.00642**
	[0.0431]	[0.00578]	[0.0106]	[0.00261]
Childcare x Nurse Program	-0.0788*	0.0106*	-0.00774	-0.00513**
	[0.0419]	[0.00552]	[0.0102]	[0.00259]
Mean, dept. var.	12.07	0.299	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. The sample is limited to the 140 municipalities that ever had a childcare center 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 county-specific linear time trends. The regressions also 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 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: Interaction Effect between Access to the Nurse Program at Birth and Access to Childcare at Age 3 on Education, Income, and Survival; Drop Post-1949 Cohorts in 28 Munis with Worse NHV Data

	Outcomes at Age 50			
	(1)	(2)	(3)	(4)
	Yrs. School	Basic Ed.	Log Tot Inc	Death by 65
Any Approved Childcare Center at Age 3	0.200***	-0.0308***	0.0174*	-0.00836***
	[0.0487]	[0.00623]	[0.00889]	[0.00282]
Nurse Program at Birth	0.182***	-0.0240***	0.0269***	-0.00928***
	[0.0491]	[0.00640]	[0.0102]	[0.00321]
Childcare x Nurse Program	-0.172***	0.0255***	-0.0156	0.00580*
	[0.0433]	[0.00590]	[0.00958]	[0.00324]
Mean, dept. var.	12.07	0.299	12.56	0.138
Observations (cells)	3694	3694	3554	3694

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 a childcare center by 1960. For the 28 municipalities that do not establish a nurse program by 1949 in our data, we drop cohorts born in 1950-1957 since we do not have precise information on nurse program 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 11: Effect of Access to Childcare at Age 3 on Education, Income, and Survival; Early vs. Late Implementers

	Outcomes at Age 50			
	(1) Yrs. School	(2) Basic Ed.	(3) Log Tot Inc	(4) Survival beyond age 65
Any Approved	0.0938	-0.0122	0.00422	0.0145***
Childcare Center at Age 3	[0.127]	[0.0155]	[0.0319]	[0.00499]
Any Childcare x	0.115	-0.0204	0.0119	-0.0120**
Late Implementer	[0.127]	[0.0162]	[0.0314]	[0.00519]
Mean, dept. var.	12.07	0.299	12.56	0.903
Observations (cells)	3918	3918	3778	3918

Notes: Each column reports the results from a separate regression. The regression includes an interaction between the treatment variable and an indicator for the municipality being a "late implementer" (i.e., first childcare center approval in 1940 or later). The units of analysis are municipality×birth-year cells. The sample is limited to the 140 municipalities that ever had a childcare center 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 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

B Additional Results: Main Effects of NHV in the Ever-Childcare Analysis Sample

Appendix Table 12: Effect of Access to NHV in the Sample of Municipalities that Ever Implement Childcare on Education, Income, and Survival

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Yrs. School	0.053	0.016	0.037	-0.004
	(0.037)	(0.031)	(0.027)	(0.026)
Mean of dep. var.	12.075	12.075	12.075	12.075
No. of obs.	3918	3918	3918	3918
Basic Educ.	-0.005	0.001	-0.004	0.002
	(0.005)	(0.004)	(0.003)	(0.004)
Mean of dep. var.	0.299	0.299	0.299	0.299
No. of obs.	3918	3918	3918	3918
Log Tot Inc.	0.020***	0.013**	0.016***	0.011**
	(0.007)	(0.005)	(0.005)	(0.005)
Mean of dep. var.	12.557	12.557	12.557	12.557
No. of obs.	3778	3778	3778	3778
Survival beyond age 65	0.003	0.003	0.002	0.003
	(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
Muncipal:				
FE	Yes	Yes	Yes	Yes
X (ipolated)	No	No	Yes	No
FE × pre-trend	No	No	No	Yes
Linear county time trends	No	Yes	Yes	Yes

Notes: Each cell presents the coefficient for a separate regression. The units of analysis are municipality×birth-year cells. The sample is limited to the 140 municipalities that ever had a childcare center 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 13: Effect of Access to NHV in the Sample of Municipalities that Ever Implement Childcare on Survival at Different Ages

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Survival beyond age 55	0.001	0.001*	0.001	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
Mean of dep. var.	0.975	0.975	0.975	0.975
No. of obs.	3918	3918	3918	3918
Survival beyond age 60	0.002	0.002	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Mean of dep. var.	0.941	0.941	0.941	0.941
No. of obs.	3918	3918	3918	3918
Survival beyond age 65	0.003	0.003	0.002	0.003
	(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
Muncipal:				
FE	Yes	Yes	Yes	Yes
X (ipolated)	No	No	Yes	No
FE × pre-trend	No	No	No	Yes
Linear county time trends	No	Yes	Yes	Yes

Notes: Each cell presents the coefficient for the NHV indicator for a separate regression. The units of analysis are municipality×birth-year cells. The sample is limited to the 140 municipalities that ever had a childcare center 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 14: Effect of Access to NHV in the Sample of Municipalities that Ever Implement Childcare on Diagnoses at Age 60

Outcome	(1) (All)	(2) (All)	(3) (All)	(4) (All)
Age 55-64	-0.282**	-0.287*	-0.270*	-0.223
	(0.139)	(0.148)	(0.157)	(0.167)
Mean of dep. var.	8.596	8.596	8.596	8.596
No. of obs.	3918	3918	3918	3918
Diagnosed Cardio	-0.005**	-0.006**	-0.004	-0.005**
	(0.003)	(0.002)	(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	-0.003**	-0.003**	-0.002	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Mean of dep. var.	0.061	0.061	0.061	0.061
No. of obs.	3918	3918	3918	3918
Diagnosed Diabetes	-0.001	-0.001	-0.001	0.000
	(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	-0.001	-0.001	-0.001	-0.001
	(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
Muncipal:				
FE	Yes	Yes	Yes	Yes
X (ipolated)	No	No	Yes	No
FE × pre-trend	No	No	No	Yes
Linear county time trends	No	Yes	Yes	Yes

Notes: Each cell presents the coefficient for the NHV indicator for a separate regression. The units of analysis are municipality×birth-year cells. The sample is limited to the 140 municipalities that ever had a childcare center 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