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SOURCES OF GENERATIONAL PERSISTENCE IN THE EFFECTS OF EARLY-LIFE HEALTH INTERVENTIONS

Sara Sofie Abrahamsson Aline Bütikofer Katrine V. Løken Marianne E. Page

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ABSTRACT

We document that the long-run economic benefits of a low-cost early-life health intervention transmit to later generations, but only for children of exposed mothers. We provide novel evidence that the program improved mothers' marriage outcomes but had limited effects on fathers' partnering decisions. Changes in assortative mating patterns may, therefore, be an important mechanism behind program-induced intergenerational spillovers. We also show that the intervention significantly increased economic mobility across three generations, suggesting that early health interventions may be important candidates for reducing the cycle of poverty.

Sara Sofie Abrahamsson CeFH at Norwegian Institute of Public Health Postboks 222 Skøyen Oslo N-0213 Norway sofiesara.abrahamsson@fhi.no

Aline Bütikofer Department of Economics Norwegian School of Economics Hellev. 30, N-5035 Bergen Norway aline.buetikofer@nhh.no Katrine V. Løken Department of Economics Norwegian School of Economics Helleveien 30 5045 Bergen Norway katrine.loken@nhh.no

Marianne E. Page Department of Economics University of California, Davis Davis, CA 95616-8578 and NBER mepage@ucdavis.edu

1 Introduction

Economic status is highly persistent across generations (see, e.g., Clark et al., 2014; Lindahl et al., 2015; Solon, 2018). Relative to more advantaged children, those born to loweducated, low-earning parents are at substantially higher risk of growing up to have lower educational attainment and earnings in adulthood. Recent research suggests that at least part of this association is causal-that is, that external or policy-induced changes in parents' education or financial resources also affect the economic success of their offspring (Bastian and Michelmore, 2018; Aizer et al., 2016; Hoynes et al., 2016; Bailey et al., 2020; Oreopoulos et al., 2006, 2008). This suggests that public investments that successfully alter one generation's human capital or labor market outcomes may be important levers for breaking the cycle of poverty. As yet, however, we know little about the extent to which positive interventions impact multi-generational linkages or the mechanisms that generate them.¹

We address this gap in the literature by leveraging the staggered adoption of an infant health care program. A rapidly expanding literature documents that early childhood health and nutrition interventions have positive effects on children's later life outcomes, making such programs strong candidates for intergenerational spillovers (Bailey et al., 2020; Brown et al., 2020; Bütikofer et al., 2019; Miller and Wherry, 2019; Bitler et al., 2018; Cohodes et al., 2016; Hoynes et al., 2016; Fitzsimons and Vera-Hernández, 2013; Bharadwaj et al., 2013; Bhalotra and Venkataramani, 2012; Chay et al., 2009; Almond et al., 2007). Results from a handful of studies are consistent with this prior. For example, East et al. (2022) find that girls with prenatal exposure to the 1980s Medicaid expansions later gave birth to healthier infants, and Almond and Chay (2006) find improved birth outcomes among the offspring of Black mothers who gained access to integrated hospitals during their first year of life. Baker et al. (2023) analysis of a randomized trial that extended preventative care to toddlers also finds suggestive evidence of intergenerational birthweight effects. Since previous studies have linked birth outcomes to future earnings (Royer, 2009; Black et al., 2007), it is likely that the benefits of these early interventions also extended to human capital and economic outcomes of the next generation, but explorations of these causal linkages have been constrained by data limitations.² Our administrative data, spanning

¹There is some evidence that negative shocks to the in utero environment, such as exposure to disease, malnutrition, and radioactivity, have harmful effects that persist to later generations (see, e.g., Almond et al., 2010; Painter et al., 2005; Richter and Robling, 2016; Black et al., 2019).

²Closest to this study are Barr and Gibbs (2022) and Rossin-Slater and Wüst (2020), who find that children of mothers who were exposed to targeted preschool programs that provided a combination of education and health services, had better human capital outcomes in adulthood than children whose mothers

three generations, enables direct investigation of a program's economic spillovers, and the processes by which they occur.

Importantly, intergenerational spillovers may result from a policy's direct effect on the exposed generation's resources or through an indirect marriage market channel, with potential ramifications for treated individuals' fertility choices and the abilities of their offspring. The relative importance of these two channels has critical implications for interpreting policies' long-lasting effects. In particular, if the children of treated cohorts have better economic outcomes because treatment enables individuals to select partners with "better" innate characteristics, then the partners of untreated individuals will come from a lower part of the innate skill distribution, and differences in outcomes between their offspring could reflect a zero-sum game, rather than improvements that all children would experience if the treatment were extended to everyone. To date, studies that examine the extent to which assortative mating contributes to intergenerational mobility have been conducted independently of those that consider the causal effects of policies on later generations (see, e.g., Ermisch et al., 2006; Collado et al., 2022; Fernández and Rogerson, 2001). We provide a unified framework that combines these two lines of research and, in doing so, we provide important insights into the complex processes that contribute to generational persistence.

Our analyses capitalize on variation in program exposure that resulted from Norway's staggered adoption of mother-child health care centers. The centers, established during the 1930s, 1940s, and 1950s, provided free medical exams during the first year of life and information about nutrition, disease, and other aspects of early child development. Bütikofer et al. (2019) document that "first-generation" children (henceforth denoted G1) who gained access to the centers experienced substantially better health and economic outcomes in adulthood. This is an important starting point for any investigation of "second-generation" (G2) effects.³ As well, the geographic variation in program availability experienced by the first generation was no longer present at the time of the second generation's birth, as infant healthcare was then offered nationwide, with universal access. Together, these features of our natural experiment provide an ideal setting for exploring the questions at hand.

were not exposed. Two other relevant studies are Bütikofer and Salvanes (2020) who investigate a Norwegian tuberculosis intervention targeted at adolescents and find that the intervention reduced generational persistence in educational attainment, and Colmer and Voorheis (2020) who consider the grandchildren of cohorts who benefited from reductions in pollution exposure following the 1970 Clean Air Act Amendments, finding that they had better educational outcomes.

³Studies of similar programs in Sweden and Denmark have also found evidence of positive long-run effects (Bhalotra et al., 2017; Wüst et al., 2018; Hjort et al., 2017).

We offer three sets of key results. First, infant health centers had substantial spillovers onto the next generation's education and income, but only among the offspring of exposed (G1) mothers. Estimates for G2 children of exposed fathers are small and are not statistically significant. This result is somewhat surprising because the centers had larger effects on G1 men than women (Bütikofer et al., 2019). Moreover, spillovers from G1 women to their children are large: the program increased G1 mothers' years of education by 0.09, and it increased their offspring's years of education by 0.07, implying a generational persistence parameter of nearly 80 percent.

Second, the large spillovers from exposed mothers to their children can be explained by a combination of the centers' effects on a) mothers' human capital and b) partner choice. In particular, we provide novel evidence that exposed mothers' improved marriage market outcomes were an essential source of the intergenerational spillovers, and we show that the impact of the centers on G1 partner selection differed substantially by gender. Women exposed to the program were more likely to choose a higher educated, higher earning partner, while men who were exposed selected partners with similar education and earnings as the partners of the men who were not exposed. As a result, children of treated mothers grew up in higher-resource families because the centers directly affected mothers' own human capital and through the higher resources of their chosen partner.

In further analysis, we examine how the program influenced the innate characteristics of G1 individuals' partners. We present empirical evidence suggesting that the program had a modest impact on assortative mating based on innate skills, as proxied by the educational attainment of the parents of exposed individuals' partners (G0). However, the positive effect on assortative mating in innate skills is primarily observed among men, making it unlikely to account for the spillover effects seen in children of exposed mothers. Additionally, a mediation analysis indicates that the impact of the program on family resources, rather than the innate skills of the partner, is the primary driver of these spillover effects.

We further illustrate this by documenting the program's effect on households' total years of education (the exposed G1 individual's years of education plus that of their partner). We show that relative to control children, those who grew up with exposed fathers did not live in households with significantly higher levels of total parental education, but children with exposed mothers grew up in households with 0.27 additional years of education. This estimate is much larger than the estimated effect on exposed mothers' education alone (.09). Therefore, from the household perspective, the persistent impact of the program across generations is about 0.26, and the large difference between the estimated householdchild persistence parameter (0.26) and mother-child persistence parameter (0.78) can be explained by treated mothers' choice of partners.

Finally, we show that the program increased intergenerational mobility. While existing studies suggest that early life interventions have larger effects on children from the lower end of the income and education distributions (Bütikofer et al., 2019), few have examined whether this pattern persists for later generations or whether transmission mechanisms operate differently for high- and low-income families. We find that the intergenerational correlation in education between the parents of the exposed generation (G0) and the exposed generation (G1) is reduced by 7 percent. In comparison, the correlation between the parents of the exposed generations' offspring (G2) is reduced by 8 percent. These results suggest that early-life health interventions can significantly alter intergenerational mobility. Hence, our analyses indicate that policies that expand access to early-life health care can contribute to sustainably increasing economic opportunities across multiple generations.

Our paper makes several important contributions. First, we quantify generationally persistent economic spillovers resulting from an early-life health intervention. Second, we distinguish between spillovers generated through maternal vs. paternal exposure and show that this distinction is key to understanding the sources of persistence. We also make significant progress towards identifying the processes through which policy persistence may occur. In particular, we decompose our treatment effects into a direct component from the policy's effects on family resources and an indirect component from policy-induced changes in assortative mating. We provide empirical evidence that gender differences in the program's impact on partner choice explain why spillovers are observed only when the mother is exposed. These changes in assortative mating also explain why the intergenerational spillovers are large relative to the policy's impact on the exposed generation.

Finally, we provide empirical evidence that changes in both assortative mating on innate skills and increases in family resources are likely to contribute to an intervention's generational persistence. In our case study of Norwegian mother-child centers, the latter channel is the most important. By quantifying the program's persistent effects across three generations, we demonstrate that policies that promote early-life health can be valuable levers for increasing intergenerational mobility.

2 Background and Data

2.1 Infant Health Centers

In the late 19th and early 20th centuries infant mortality was high in both Europe and the United States: in 1930, nearly 45 out of 1000 Norwegian infants did not survive their first year of life (Backer, 1963). This sparked public demands for government investments targeting infant health and led to local initiatives by a philanthropic institution called the Norwegian Women's Public Health Association (NKS), which established mother-child health centers across the country. The centers were run according to national state-of-theart medical guidelines, and all services were free of charge. While the centers were mainly targeted at poor families, they were open to everyone and quickly became widely popular among mothers of all socioeconomic backgrounds. By 1939, 80 percent of infants in the capital city of Oslo were receiving check-ups, and the centers were an important part of the city's universal health care services (Schiøtz, 2003).

On average, a child would visit a well-child center three to four times during their first year of life. The centers provided two main services: infant check-ups by doctors and nurses and advice for mothers on adequate infant nutrition, infant hygiene measures, and adequate infant clothing. Centers' medical equipment was limited to what was needed for standard check-ups; ill infants were, therefore, referred to doctors or hospitals. As breastfeeding rates in Norway were relatively low and declining in the first half of the 20th century (Liestøl et al., 1988), staff at the centers promoted breastfeeding, and mothers were taught to make adequate milk formulas. The centers' costs were mainly comprised of doctors' and nurses' salaries and traveling expenses, and expenses related to printing information materials for mothers. The costs were financed by funds from the state lottery, financial support from philanthropic contributions, local governments, counties, and the state.

Using variation in the timing of the centers' rollout across municipalities, Bütikofer et al. (2019) document that infants who gained access to health care experienced substantially better health and economic outcomes in adulthood. They also document that the centers reduced diarrhea-related infant deaths, consistent with the goal of improving nutrition. These "first stage" results are a critical foundation for our identification of second-generation effects.

Also noteworthy is that, while access to infant health care varied substantially across geographic areas during the initial rollout period, this variation no longer existed when treated cohorts gave birth to their children. By 1960, all infants had access to free infant check-ups, and centers also provided guidance and simple check-ups for pregnant women. Therefore, this study's estimates will capture an indirect effect of "first generation" health care access on the second generation.

Starting in the 1960s, centers began to switch from private to public ownership, and since 1972, all Norwegian municipalities have been obliged by law to provide public health-care centers for infants, regulated by the Health Directorate's official guidelines. Thus, municipality-run centers are still an important and integral part of universal healthcare provision for infants, small children, and mothers. Today, the centers provide 14 free check-ups between the ages of 0 to 5, along with vaccinations and basic health education. They are staffed by pediatricians, nurses, midwives, physiotherapists, and psychologists.

2.2 Data

We link historical data on the centers' rollout with individual-level administrative data. In particular, we use a compilation of different Norwegian administrative registers, including the central population register, the education register, the birth registry, and the tax and earnings register. These linked administrative data cover the Norwegian population up to 2018 and provide information about individuals' place of birth and residence, educational attainment, labor market status, birth outcomes, earnings, and a set of demographic variables. In addition, a multi-generational register matches Norwegian children with their parents and grandparents.⁴ As a result, we can link earnings, education, and birth outcomes data over several generations. The historical data on the health centers are collected from private archives. In what follows, we briefly describe the historical data, summarize the sample definitions and the registry data, and describe the variables and summary statistics for our sample.

2.3 Historical Data

The main data sources we use to document the rollout of mother and child health care centers are two surveys that the Norwegian Women's Public Health Association (NKS) sent to all centers in 1939 and 1955. The surveys collected data on each center's exact address and date of establishment. In addition, we collected data from centers' yearly

 $^{^{4}}$ We have information on schooling for about 65% of the first generation's parents (G0). This information is only available for individuals who survived until the late 1950s. Replacing missing information on education with the lowest level of education does not alter our results.

reports, including the number of children served. Comparing the size of a birth cohort in a municipality with the number of children examined at a health care center each year, we find that the uptake rate was about 40 percent in the year of the center opening and about 60 percent two to three years after the opening. Figure 1 shows the timing of the rollout of the mother and child health care centers across Norwegian municipalities between 1935–1955.⁵

2.3.1 Administrative Data

The central population register contains individuals' municipality of birth. We assign access to health care centers during the primary years of the centers' rollout (1936–1955) based on the municipality and year in which the directly exposed generation was born.⁶ We call these birth cohorts the "first generation" (G1). The main sample of interest consists of the children born to the first generation, whom we call the "second generation" (G2). This second-generation sample includes individuals born between 1961 and 1988. We restrict the sample to those whose children were born after 1960 because we wish to focus our second-generation analyses on children who were born after the rollout was fully completed and when there was no longer any regional variation in center access. Limiting the sample to children born before 1988 allows us to estimate the program's effect on the second generation's education and income after age 30. These limitations simultaneously ensure that no G2 individuals were directly impacted by the rollout (which continued until the 1960s, when it became mandatory for municipalities to offer these services) while including all births that occurred to the first generation between the ages of 25 and 33.⁷ The second-generation sample consists of 773,764 individuals with a mother born between 1936 and 1955, for whom we observe at least education or annual earnings; 438,144 of these individuals have a treated mother. 741.318 individuals have a father born between 1936 and 1955, for whom we observe at least education or average earnings. Out of these, 417,290 have a treated father.⁸

⁵Bütikofer et al. (2019) provide a more detailed description of the historical data.

⁶Note that the first center opened in Oslo in 1914. Only nine municipalities, including Oslo, had a center before 1936. We therefore focus on the rollout period beginning after 1936.

⁷This sample restriction means that our analyses do not include G2 individuals whose G1 parents were born at the beginning of the rollout period (late 1930s and early 1940s) and who gave birth at young ages. The resulting imbalance in parental age at birth across cohorts does not drive our results, as the estimates are very similar when we restrict the analyses to second-generation individuals whose mothers were older than 25 when they gave birth.

⁸The exact number of observations varies across specifications because some outcomes are missing observations.

We focus on generational spillovers onto common measures of socioeconomic success, including measures of educational attainment and earnings. In some analyses, we also use the education of the parents of the first generation (G0). Educational attainment for the first and the second generations is taken from the educational registry database and is measured up to 2018 (when the youngest cohort is 30 years old). We consider total years of education, and we examine whether the individual was enrolled in an academic track during high school (vs. the vocational track or no high school). The data are based on school reports sent directly from educational institutions to Statistics Norway, minimizing any measurement error due to misreporting. For G0, we use education data from the 1960 Census. Note that these measures are, therefore, only available for G0 individuals who are still alive in 1960. Restricting our main first and second generation analyses to individuals for whom G0 education information is available does not impact our main conclusions.

Annual earnings data are obtained from the tax registry and include labor earnings, taxable sickness benefits, unemployment benefits, and parental leave payments. They are not top-coded. For both the G1 and G2 cohorts, we use average earnings from 1967 to 2017, measured in 2015 Norwegian Kroner (NOK).

After documenting the program's impact on the second generation's education and earnings, we consider potential underlying mechanisms, including changes in health at birth and fertility patterns, partner choice, and migration decisions. We examine standard infant health measures available from the Medical Birth Registry, which contains records for all births since 1967.⁹ The records include information on the date of birth and variables related to infant health at birth, including the child's birth weight, whether the child was below the low birth weight threshold (under 2500 grams), and whether the child was born prematurely (before 37 weeks).

To investigate the program's impact on fertility patterns, we consider the first generation's likelihood of ever having a child, number of births, and age at first birth. We obtain these outcomes from the central population registry. All of these variables can potentially influence our second-generation results directly or indirectly through selection into the sample.

Further, we consider whether the program influenced the first generation's location decisions. Information on individuals' municipality of residence is available annually from the central population register. First, we consider whether an exposed parent was more

 $^{^9\}mathrm{Restricting}$ the second generation sample to cohorts born after 1967 does not substantively change our main results.

likely to move to one of Norway's seven largest urban areas before the first child was born.¹⁰ Second, we conduct a similar exercise based on municipality-level variables and analyze whether individuals were more likely to move out of a low-income municipality where the average income was below the median. The average income for each municipality is taken from the 1930 population census.¹¹ We multiply all binary variables by 100 to ease interpretability and avoid rounding issues when displaying coefficients.

Finally, we investigate whether program exposure influenced first-generation marriage market outcomes using the information on the G1 partner's years of education, the likelihood of enrolling in an academic high school track, average earnings, age, and the partner's treatment status, all provided in the registries described above. As a substantial share of Norwegian couples cohabit without being married, we identify an individual's partner as the father/mother of their firstborn child. This information is provided on their birth certificate or in the population records.

3 Empirical Strategy and Sources of Generational Persistence

Our identification strategy is based on variation in health care access resulting from the program's staggered rollout across municipalities. For the first generation, we estimate the following reduced form model:

$$Y_{ict} = \alpha + \gamma * Treat_{ct} + \beta X_{ict} + \lambda_c + \theta_t + \varepsilon_{ict}, \tag{1}$$

where y_{itc} are the outcomes of interest for individual *i* born in municipality *c* at time *t*. $Treat_{ct}$ is an indicator variable equal to one if an individual is born in the year before or the years after the center opening in their municipality of birth.¹² Individuals born a year

¹⁰These urban areas are Oslo, Bergen, Trondheim, Stavanger, Drammen, Fredrikstad, and Skien.

¹¹There are four municipalities lacking information on the average earnings in 1930: Ski, Ringeriket, Arendal, and Hammerfest.

¹²This classification is somewhat different from Bütikofer et al. (2019), who classify individuals as treated if they were born in the same year, or after a center opened. We classify an individual as treated if she was born in the year before a center opened because infants were eligible for services until the age of one. Hence, many infants born in the year before a center opening had access to the mother-child centers during their first year of life. Another difference from Bütikofer et al. (2019) is that our main specifications include never treated municipalities as part of the control group. Bütikofer et al. (2019)'s sample only includes individuals in ever-treated municipalities. The decision to include never-treated municipalities as part of the control group is driven by our interest in investigating the policy's effect on partner choice (partners may come from never-treated municipalities). We expect these municipalities to bias our estimates downward since

before or earlier than a health center opened in their municipality of birth or individuals born in a municipality who never experienced an opening of an infant healthcare center are classified as the control group, with $Treat_{ct}$ set equal to zero. X_{ict} controls for the individual's gender. We control for common time effects by including cohort fixed effects θ , and for non-time-varying differences across municipalities by including municipality fixed effects λ . Standard errors are clustered on the municipality of birth level.

Assuming that the rollout is quasi-random and the treatment effects are homogeneous across treatment groups, Equation 1 will produce unbiased estimates. Bütikofer et al. (2019) provide a discussion on the quasi-randomness of the rollout, showing that trends in economic or other development variables cannot predict the timing of a center opening in our period of interest. Moreover, we consider the importance of these assumptions using the method proposed by Callaway and Sant'Anna (2020).

To understand whether the benefits associated with the first generation's access to infant health centers spill over to their children, we estimate the following model separately for children of exposed mothers and exposed fathers:

$$Y_{jc_pt_p} = \alpha + \delta * TreatParent_{c_pt_p} + \beta X_{jc_pt_p} + \lambda_{c_p} + \theta_{t_p} + \varepsilon_{jc_pt_p}, \tag{2}$$

where $Y_{jc_pt_p}$ are the outcomes of interest for individual j born to parent p who is born at time t and in municipality c. In these second-generation regressions, X_{jct} also includes an indicator for the offspring's gender and fixed effects denoting the offspring's cohort. The variable of interest is δ , which shows the effect of having either a mother or a father with access to an infant health center. Similar to Equation 1, we include municipality fixed effects for the parent's municipality of birth λ and cohort fixed effects for the parent's birth year θ . For comparability of the estimates across generations, the regressions are weighted such that each first-generation parent gets equal weight. Standard errors are clustered at the municipality level to account for correlated errors within a municipality.

3.1 Potential Channels for Intergenerational Transmission

As shown in Bütikofer et al. (2019) and replicated in this paper, the reform affected the first generation's education and earnings. Transmission of these effects onto later generations could come about for a number of reasons (Blanden et al., 2023). Before turning to these potential mechanisms, however, we discuss a few that are not relevant in our context. First,

some never-treated municipalities may have had a mother-child center that was operated by a philanthropic group other than the NKS.

G1 cohorts were treated after birth. Hence, the reform did not affect their innate skills. Second, all G2 children had access to infant healthcare (see discussion in Section 2.1), and the vast majority were born in a specialized maternity ward in a hospital. Therefore, our results are unlikely to be driven by municipality-level differences in the second generation's access to infant health care.

We distinguish between two primary sources of generational persistence. First, the reform increased parental inputs such as education, income, and health. Improvements in these inputs could have positively impacted children's outcomes through increases in parents' monetary investments or time allocations, knowledge, or information that was transmitted to the children. They may have also changed parental preferences or aspirations that subsequently affected the next generation's human capital investments. Access to the centers also reduced the likelihood of experiencing metabolic syndrome and cardio-vascular disease in adulthood (Bütikofer et al., 2019; Hjort et al., 2017). The presence of such health conditions during pregnancy increases the risk of poor birth outcomes (Catalano and Ehrenberg, 2006), which can, in turn, negatively impact individuals' later life education and labor market success (see, e.g., Almond et al., 2018; Black et al., 2007; Figlio et al., 2014). Second, centers' positive effects on the first generation's health and economic outcomes may have altered their preferences (and opportunities) for partners with certain types of characteristics, with implications for fertility, children's skills, and household resources.

4 Results

In this section, we present our main findings. First, we show that our first-generation sample and slightly modified estimation strategy yield results that are very similar to Bütikofer et al. (2019). Second, we document that the program had large spillover effects on the second generation's educational outcomes, but only among second-generation individuals with an exposed mother. We show that our results hold up to robust difference-in-differences methods proposed by Callaway and Sant'Anna (2020) and discuss results from further specification checks.

4.1 Direct Effects on the First Generation

Table 1 provides our replication of Bütikofer et al. (2019). Panel A is based on a sample that, like Bütikofer et al. (2019), is restricted to individuals who were born in municipalities

that received a center during the analysis period. We find that the reform increased the first generation's education by 0.23 years, which is about 2 percent of the pre-reform mean. Exposure to the reform also increased average earnings by about 3 percent. These estimates align with Bütikofer et al. (2019) who use a slightly different set of cohorts.

In Panel B, we show that extending our sample to include untreated municipalities produces similar results. This change to the sample is necessary because we will eventually examine whether and how marriage market dynamics feed into the reform's second-generation effects, and treated parents may partner with individuals born in treated, eventually treated, or never-treated municipalities. As expected, these estimates are somewhat smaller than the estimates in Panel A because it is likely that some "untreated" municipalities had centers that were provided by philanthropic groups other than the NKS.¹³

Panel C clarifies that restricting the sample to first-generation individuals who had children born between 1961 and 1988 (about 86 percent of those in Panel B) has little additional impact on the estimates. The similarities between Panels B and C suggest that the effects of healthcare access are similar for individuals with and without children.¹⁴

Panels D and E provide results separately by gender for those with children in the second generation and document that the reform's effects were larger for men than for women. First-generation men who had access to the program experienced a 1.6 percent increase in their years of schooling, whereas the increase for women was closer to 0.8 percent. The smaller effects for women are unsurprising given that labor market opportunities differed substantially for men and women born in the late 1930s–1950s, but it is important to keep this in mind as we consider the reform's impact on the next generation.

We then move to understand whether our estimates are contaminated by the presence of heterogeneous treatment effects, following the approach proposed by Callaway and Sant'Anna (2020) (see Online Appendix Figure A.1 and Figure A.2). Overall, these stacked event-study analyses indicate that heterogenous treatment effects are not driving the estimates in Table 1 for our main sample with children in the second generation. There is also no evidence of pre-trends in the years prior to healthcare center openings.¹⁵

¹⁵Note that the plots in Figure A.1 and Figure A.2 cannot necessarily be interpreted analogously to the

¹³Organizations such as the Norwegian Red Cross also offered similar healthcare centers in the later part of the roll-out phase. Our data sources are from NKS's private archive and, therefore, provide no information on when and where non-NKS centers were located.

¹⁴Note that we also examined whether the program affected selection into parenthood. The results are available in Online Appendix Table A.4 and are discussed in more detail in Section 5. Overall, the findings indicate that the program had minimal effects on fertility. Moreover, the second-generation regressions are weighted such that each first-generation parent gets equal weight (see Section 3). Hence, our second-generation results are not biased by differences in family size across treatment and control groups.

Online Appendix Table A.1 shows that our results are robust to several additional specification checks. As the Nazis occupied Norway from April 1940 to May 1945, some foods (e.g., sugar, meat, milk) were rationed during this time. Second-generation individuals whose parents were born during World War II might, therefore, differ from those whose parents were born during other periods because of differences in their parents' nutritional endowments. Excluding these individuals does not affect the estimates. Next, we estimate a version of the baseline model that controls for changes in Norwegian compulsory schooling laws that increased the educational attainment of cohorts born during the second half of the rollout period (1946–1961) as discussed by Black et al. (2005a).¹⁶ This check is important because some individuals in our sample were simultaneously exposed to the healthcare centers and the new compulsory schooling requirements. We control for this potential contaminant by including an indicator equal to 1 when an individual and municipality of birth is affected by the school reform. Including this control does not substantively change the estimates.

4.2 Spillovers to the Second Generation

Table 2 presents our second generation (G2) results, which are generated by estimating Equation 2, and are based on the offspring of G1 individuals. Panel A displays the estimates generated by separate regressions for those whose mothers had access to well-child centers during their first year of life and those who had exposed fathers. It is immediately apparent that the program's impacts spilled over onto the next generation's human capital, but only for those in the second generation with an exposed mother. Estimates for those with an exposed father are substantially smaller, and they are not statistically different from zero at conventional levels, despite the large treatment effects found for first-generation men in Table 1. Data constraints have restricted most multi-generational studies to the examination of maternal linkages, leaving the role of paternal transmission largely unexplored, but our findings are consistent with Black et al. (2005b) who find that an increase in maternal education has a larger effect on children than a similar increase in paternal education.

Relative to children whose mothers did not have access to health centers during their first year of life, those whose mothers were exposed grew up to have 0.07 additional years of

two-way fixed effects estimates (Roth, 2024).

 $^{^{16}}$ In 1959, the Norwegian Parliament increased the number of years of compulsory schooling from seven to nine years. The reform was gradually implemented across the country between 1960 and 1972.

education (a 0.5 percent increase).¹⁷ We also find that the program had positive spillovers on the second generation's average earnings (1.5 percent relative to the pre-reform mean). Online Appendix Table A.2 shows that our results are robust to the same specification checks we discussed for the first generation in Section 4.1.

These estimates are large. To give a sense of magnitudes, they are similar to the estimated impacts of a two-pupil reduction in class size among Swedish children born in roughly the same period (Fredriksson et al., 2013). Another study focusing on similar Norwegian cohorts finds that a 10 percent increase in birth weight increases both the probability of completing high school and subsequent earnings by about a 1 percent (Black et al., 2007). Using this metric, our earnings estimate is comparable to the effects of a 14 percent increase in birth weight. As these studies focus on uncovering long-run impacts on *treated* cohorts, our estimates of the spillovers onto the next generation are substantial.

The estimates are also large relative to our estimates for the first generation. When we compare the estimated effect of maternal exposure on the next generation's years of schooling (Column 1) to the reform's direct effect on mothers' own educational attainment (0.09 in Table 1), we obtain a persistence parameter of 78 percent. The persistence parameter for earnings is even larger because the estimated effect of the reform on mothers' earnings is small and insignificant. We will return to this issue in Section 5.

As in Section 4.1, we present figures based on the stacked event-study approach introduced by Callaway and Sant'Anna (2020). The results are available in Online Appendix Figures A.3, and A.4. There is no evidence that pre-existing differences in municipalities' trends drive the estimated effects of maternal exposure on second-generation outcomes.

Panel B of Table 2 shows how the estimated effects vary across second-generation children who fall into three different types of families: those with two treated parents, those with a treated mother and an untreated father, and those with an untreated mother and a treated father. Children with no treated parents serve as the reference group. Specifically, we estimate

$$Y_{jt_m t_f c_m c_f} = \alpha + \eta * TreatMother_{c_m t_m} + \rho * TreatFather_{c_f t_f} + \sigma * TreatBoth_{c_m c_f t_m t_f} + \beta X_{jt_m t_f c_m c_f} + \lambda_{c_m} + \lambda_{c_f} + \theta_{t_m} + \theta_{t_f} + \varepsilon_{jt_m t_f c_m c_f},$$
(3)

where $Y_{jt_mt_fc_mc_f}$ are the outcomes of interest for individual j born to mother m and father f born at times t and in municipalities c. The variables of interest are η , which is the effect of only having a mother with access to a center, ρ , which is the effect of only having a father

¹⁷We have also investigated whether the magnitude of the spillovers differs for second-generation girls and boys, and find no evidence of significant differences. Results are available on request.

with access, and σ , which is the effect of having two parents who were exposed. We include the same variables in X_{jct} , and employ the same weighting and approach to standard error clustering as in Equation 2. Different from Equation 2, we include municipality fixed effects for the mother's and father's municipalities of birth λ , and cohort fixed effects for the mother's and father's birth years, θ . Figure 2, which shows how we group the children by couple types, provides additional intuition. Starting with the lowest row, there are four different groups of children: those who have two exposed parents, those who have only an exposed mother, those who have only an exposed father, and those with no exposed parents. If the reform affects G1 partner choice (second row), then there will be no pure control group in this specification anymore. That is, couples in which neither parent is exposed may have different underlying characteristics in the pre- and the post-period. Indeed, we will show that this is the case later.

Although these estimates should be interpreted cautiously, provide insights into relative effects and may provide helpful information when we turn to the potential role of assortative mating in Section 5. Here, we see that maternal exposure has spillover effects onto the next generation's education outcomes, even in the absence of a treated father.¹⁸ In other words, the effects of maternal exposure do not merely result from a tendency for exposed mothers to partner with exposed fathers. The pattern is similar for earnings, but the estimate is only statistically significant in the case where both parents gained access to the centers. These results support our earlier finding that maternal exposure leads to intergenerational spillovers.

5 Mechanisms for Spillovers

What are the mechanisms generating these spillovers, and why do they operate largely through treated mothers? Our finding that treated mothers matter more than treated fathers is somewhat puzzling because the human capital and economic returns to center exposure were larger for first-generation men than women. The dominance of maternal effects could result from any of several factors, including program-induced improvements in the second generation's in utero environment, changes in the first generation's selection into motherhood, or changes in other maternal behaviors that result from the program's

¹⁸While in this specification, the estimated effect of having a treated father on the likelihood of attending an academic high school is positive and statistically significant, it is still much smaller than the estimated effect of having a treated mother.

impacts on the first generation's health and human capital.¹⁹

We use available data to explore these possibilities. First, we consider the program's impact on measures of the second generation's health at birth, including birth weight, the likelihood of being born prematurely (see Online Appendix Table A.3). We find no evidence of spillovers onto the second-generation's health at birth for children with treated mothers, and limited effects for children with treated fathers (although there is a small decrease in the likelihood of being born with low birth weight). The limited impact on infant health outcomes may reflect the continuing evolution of Norwegian healthcare after the roll-out period: by the 1960s, pre-natal and infant check-ups following national guidelines had been established in all municipalities, and the vast majority of births occurred in hospitals. Hence, regional variation in the first generation's access to infant healthcare had evened out for the second generation, which may have partly compensated for differences between treated and untreated mothers' health and human capital.²⁰

Our results are also unlikely to be driven by a treatment effect on the first generation's selection into parenthood. Estimates in Online Appendix Table A.4 provide no evidence that the program affected the probability of first-generation women ever having a child. We do find that exposure to the program had a very small, negative effect on the first generation's completed fertility and age at first birth, but importantly, our second-generation regressions are weighted so that each first-generation parent has the same weight (see Section 3). Hence, our second-generation results cannot be biased by differences in family size across treatment and control groups. It is possible that the decrease in family size leads to better child outcomes, but Black et al. (2005a) find little causal evidence that family size impacts children's long term outcomes (in Norway).

Bütikofer et al. (2019) show that access to maternal and child healthcare centers reduces the infant mortality rate by 0.8 percentage points. These results have two key implications for the long-term effects observed. First, a 'selection effect' may arise, as lower infant mortality could result in a greater number of unhealthy survivors, potentially introducing a downward bias in the estimates. Second, if infant mortality serves as a proxy for the overall disease environment, a lower rate should correspond to improved health outcomes. Therefore, we estimate lower bounds for the policy effect, dropping one percent of the

¹⁹Another possibility is that mothers have a higher tendency to direct resources towards their children, but to explore this possibility more directly, we need information on household expenditures that we do not have.

 $^{^{20}}$ Note that data on birth outcomes are only available for cohorts born after 1967. Nevertheless, limiting the second generation to cohorts born after 1967 does not alter our main results, described in Section4.2.

treatment group at different percentiles of the predicted distribution and re-estimate our main equation. Online Appendix Tables A.5 and A.6 indicate that our main estimates for the second generation are not due to selection resulting from the reform's effect on the first generation's likelihood of surviving into adulthood. Finally, Online Appendix Tables A.7 and A.8, which replicates our results for non-movers (first-generation mothers who remained in their municipality of birth until they had their first child), indicates that our results are not driven by program-induced selection into migration.

An alternative possibility is that treatment changed the first generation's partnering decisions. This could have happened (for example) because the additional education received by treated women changed their probability of meeting certain type of partners or altered their partner preferences. Previous work suggests that marital sorting can have important implications for the intergenerational transmission of socioeconomic status (Holmlund, 2022; Butler et al., 2008; Mare and Maralani, 2006). Under this scenario, the program's impact on the second generation would reflect both the direct effects of the reform on mothers' skill levels and any indirect effects resulting from changes in her partner's characteristics. We turn to this possibility below.

5.1 Partner Choice and Assortative Mating

We use Equation 2 to estimate the effect of the first generation's access to health centers on several outcomes of later life partner characteristics, including completed years of schooling, average earnings, relative age, and treatment status. Table 3 documents that treated mothers partnered with better educated and higher earnings men than they would have partnered with in the absence of the reform. Specifically, Panel A shows that the partners of exposed mothers had 0.16 more years of education than the partners of mothers who were not exposed. We also see that relative to the partners of un-exposed mothers, exposed mothers' partners' earnings were about 2 percent higher. Moreover, we observe that they match with partners where the difference in relative age is slightly smaller and where the partner is also likely to be treated. These results suggest that treated mothers' positive impacts on their offspring's outcomes reflect the combined effects of program-induced increases in their own human capital, as well as improvements in the characteristics of their partners. Panel B shows that exposed fathers did not select better-educated or higherearning partners, although their partner is more likely also to be treated, and their relative age is slightly higher. Together, the patterns in Table 3 indicate that the reform's effects on partner choice differed significantly by gender.

Where did exposed women meet these better quality partners? In Online Appendix Table A.9, we show that the positive effect of healthcare centers on education is concentrated in high school completion and not in additional post-secondary schooling. This suggests that partner selection is unlikely to emerge from changes in college-level exposure to potential partners. Changes in partner matching more likely happened at earlier education stages or through other arenas, such as the labor market.

5.2 Partner Selection

So far, we have demonstrated that a policy's effect on assortative mating may contribute to intergenerational spillovers. We still do not have enough information, however, to determine what this implies about the policy's potential to improve equality of opportunity and reduce inequality among future generations. Because our measures of partner characteristics are observed after individuals were exposed to the centers, the estimated effects of the reform on partners' education and earnings likely reflect a combination of two distinct effects, each of which leads to a different interpretation of what the second generation estimates in Table 2 are capturing. One possibility is that the higher education and earnings we observe among exposed mothers' partners reflect the effect of the treatment on mothers' ability to draw their partners from a higher place in the innate skill distribution. In this case, the reform's observed benefits to second-generation individuals with exposed mothers would partly reflect negative consequences for the children of unexposed mothers, who, as a result of the policy, must have matched with partners further down the distribution. The program's estimated "effects" on the next generation could then represent a zero-sum game across all second-generation individuals and just a reallocation from those with unexposed mothers to those with exposed mothers. This type of reform-induced increase in partner selectivity on innate skills could even increase inequality among later generations.

Alternatively, treated mothers may have chosen partners with the same latent characteristics as they would have chosen in the absence of treatment, with their partners' higher education and earnings resulting directly from the reform. In this case, the estimates in Table 2 suggest that access to infant health care could have long-term potential for increasing equality of opportunity and reducing inequality.

To gain further traction on the relative importance of these two effects, we consider the effect of treatment on proxies for a partner's innate skills: the education of the partner's mother and father (separately and jointly). These partner characteristics are present at birth and, therefore, not directly affected by the reform. The results of this exercise are

shown in Table 4, and document that treated mothers partnered with men with the same innate skills as untreated mothers. However, there is some evidence that treatment changed fathers' partner selection along this dimension.²¹

As a further exploration, we perform a mediation analysis where we re-estimate Equation 2, including step-wise controls for different parental and grand-parental characteristics. The results of this exercise are shown in Table 5. Panel A includes controls for the education of grandparents–our proxy for innate skills. The estimated effects are not very different– even slightly higher–from our main estimates in Table 2. This confirms our earlier finding that sorting on innate skills is not a driver of the second-generation results.

The estimates in Panel B are based on a model that includes controls for parental education. In contrast to Panel A, controlling for parental education leads to a dramatic decline in the magnitude of the estimated spillovers. The estimates in Panel C are based on a version of the model that controls for household earnings instead of parental education. Including household earnings also reduces the estimated effect of the reform on the second generation's education but by less than when we control for parental education. Finally, Panel D includes all of the controls from Panels A–C. Including all of the controls changes the education estimates very little relative to Panel B, but further reduces the earnings estimates. Taken together, these results suggest that the reform's positive effects on households' financial resources and human capital–operating through the reform's effects on both individual outcomes and partner choices–were important drivers of the reform's generational persistence.²²

5.3 Interpretation of Intergenerational Policy Spillovers

Next, we consider how changes in exposed parents' choice of partner affected the level of *household* resources available to their children. To do this, we replace the dependent variable in Equation 2 with either the sum of mother and father's education, at least one

 $^{^{21}}$ Note that we do not observe G0's education for all individuals. Hence, the samples in Tables 1, Table 2, and Table 4 are not identical. Table A.10 displays the results for our main first-generation and second-generation regressions for the sample for which we observe G0 outcomes. The estimated effects for the individuals for whom we observe G0 outcomes are larger than for the entire sample. The main results persist.

 $^{^{22}}$ While Table 5 is based on observations for whom relevant control variables are available, Table A.11 also includes observations for whom relevant controls are missing, and the regressions include a dummy variable indicating which variables contain incomplete information. The estimated coefficients in Tables 5 and A.11 are similar; the estimates in Table A.11 are, however, more precise.

parent having academic education, or the sum of mother and father's average earnings.²³ The results, shown in Table 6, indicate that second-generation children of exposed mothers grew up in households with higher total years of education (1.5 percent) and household average earnings (2.5 percent). We find no evidence that the children of exposed fathers grew up in households with more resources.

Importantly, these household-level estimates are substantially larger than the reform's estimated effects on mother's education and earnings. Therefore, the estimates of the reform's generational persistence described in Section 4.2, which do not incorporate mothers' partner choice, likely overstate the true level of direct persistence. To adjust the persistence parameters for the reform's effect on assortative mating, we compare the estimated impact of maternal exposure on second-generation outcomes (Panel A of Table 2) to the estimated effects of maternal exposure on total household education and earnings (Table 6). When the full impact of maternal exposure on household resources is taken into account, the estimate of the policy's generational persistence falls from 0.78 to 0.26 (0.07/0.27) for years of education and 0.54 (4424/8275) for earnings. These estimates are comparable to persistence parameters that have been calculated in other Nordic studies and indicate that the large persistence parameters calculated in Section 4.2 result from a failure to incorporate the reform's effects on first-generation women's partnering decisions.²⁴

5.4 Policy Persistence and Long-Run Effects on Intergenerational Mobility

Finally, having shown that the first generation's access to infant health care had a direct effect on the second generation's outcomes, we consider the reform's impact on intergenerational mobility in education. We do this by regressing individuals' years of schooling on their parents' or grandparents' years of education, along with a dummy variable indicating whether the first-generation parent was exposed and an interaction term between the exposure dummy and parents'/grandparents' education. The coefficient of this interaction term indicates how the reform affected educational persistence across generations.

 $^{^{23}}$ Note that taking the perspective of the second generation to look at outcomes for the first generation will be somewhat different from studying the first generation directly. This means that we cannot directly compare the individual treatment effects in Table 1 to these combined treatment effects.

²⁴The persistence parameter we estimate for education is similar to intergenerational correlations in education that have been estimated in other studies of Nordic countries (see, e.g., Black et al., 2005b; Karlson and Landersø, 2021). It is also in the same ballpark as estimates of the generationally persistent effect of in utero exposure to nuclear fallout on cognitive ability Black et al. (2019).

Table 7 documents the reform's impact on mobility across three generations, focusing on the directly exposed first generation (G1), their parents (G0), and their offspring (second generation, G2). Panel A shows that access to the centers weakened the correlation in educational outcomes between the directly exposed generation and their fathers by about 7 percent. This is in line with Bütikofer et al. (2019), and suggests that access to infant health care had a larger impact on children from lower socioeconomic backgrounds.

Panel B shows how the reform affected the correlation between G0's education and the education of their G2 grandchildren.²⁵ In Column (1), we focus on correlations between G2 children and their maternal grandfathers, and in Column 2, we focus on correlations between G2 children and their paternal grandfathers. Unsurprisingly, compared to educational persistence across adjacent generations, persistence across two generations is substantially lower (0.14 instead of 0.4). It is striking, however, that the correlation between G0 and G2 is about 19 percent lower among G2 children who had exposed mothers. This indicates that the centers not only increased economic mobility within the exposed generation but also increased mobility among the children of exposed mothers.²⁶ In contrast, among second-generation children who had exposed fathers, the interaction term's coefficient estimate is smaller and insignificant. Taken together, these results suggest that, in at least some settings, early-life health policies can improve social mobility across three generations and help sustainably improve opportunities.

6 Conclusion

Recent studies document that early childhood health interventions have long-lasting effects on individuals' outcomes, making such policy interventions strong candidates for increasing mobility and reducing poverty among later generations. To date, however, data constraints have limited researchers' opportunities for causal examination of intergenerational spillovers, the channels through which they occur, and their implications for intergenerational mobility. We take advantage of a unique opportunity to shed light on these gaps by leveraging geographic variation in the opening of Norwegian mother and child health centers during the 1930s-1950s. Several features of this natural experiment make it ideal

 $^{^{25}}$ Note that the results are very similar when controlling for G1's education (see Table A.12).

 $^{^{26}}$ This finding differs from Nybom and Stuhler (2013), who find that an educational reform in Sweden increased educational mobility between parents and their directly affected children but decreased mobility for the following generation. The results align with Bütikofer et al. (2022), who show that the 1970's Norwegian oil boom lowered intergenerational persistence across three generations.

for investigating multi-generational spillovers. First, the timing of the program's rollout ensures that there is enough time after the intervention's initial implementation to observe the second generation's long-run economic outcomes. Another important aspect of our setting is that infant healthcare had become available nationwide by the time the second generation was born. This means that the regional distributions of health care access experienced by the first and second generations differed. To exploit this opportunity, we harness Norwegian registry data, which allow us to link individuals to both their mothers and fathers across multiple generations.

We find compelling evidence that the positive impacts of infant health care were transmitted to the next generation, but only among those who had treated mothers. We find no evidence of intergenerational spillovers among those with exposed fathers. This asymmetry suggests that the education and earnings gains experienced by both treated mothers and fathers cannot be the only channel by which the policy benefits were transmitted to the second generation. We also provide novel evidence that the program improved treated mothers' marriage-market outcomes, but had little impact on fathers' choice of partners. Treated mothers were more likely to match with higher earnings and more highly educated men.

When we take the reform's effect on assortative mating into account, our estimate of the persistence parameter for years of education falls by two-thirds, from 78 to 26 percent. Nevertheless, the direct policy impacts on the second generation are still substantial and essential for meaningful cost-benefit calculations. Finally, we document that access to mother-child centers reduced generational persistence in educational outcomes for both the treated generation and their offspring. In other words, making infant healthcare widely available effectively disrupted the cycle of poverty.

Our study provides three key takeaways. First, the benefits of a low-cost early-life health intervention can extend to later generations. Second, mothers, and their choice of partners, are a essential channel generating intergenerational spillovers. Third, policies that benefit individuals from disadvantaged backgrounds have the potential to improve mobility across multiple generations. Program benefit calculations that disregard these potential spillovers will be inaccurate, which has important policy implications for the optimal allocation of resources.

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7 Tables and Figures

Figure 1: Mother and Child Healthcare Center Openings by Municipality and Decade



Notes: The map displays Norway's 428 municipalities. The different colors indicate when the first NKS mother and child healthcare center was opened in these municipalities. There were no NKS mother and child healthcare centers opened in the white municipalities during the period of interest.

Figure 2: Family Types



	Total years of education	Average earnings 1967–2017			
	(1)	(2)			
	Panel A: Eventuall	Panel A: Eventually treated municipalities			
Center exposure	.23***	6365.3***			
-	(.04)	(1992.6)			
Observations	505,871	508,187			
Pre-reform mean	11.1	207981			
	Panel B: Untreated m	unicipality as control group			
Center exposure	.14***	4436.1***			
	(.04)	(1660.8)			
Observations	$781,\!354$	784,364			
Pre-reform mean	11.5	227212			
	Panel C: Sample with children in second generation				
Center exposure	.14***	3749.3**			
	(.05)	(1509.8)			
Observations	669,918	670,799			
Pre-reform mean	11.5	229768			
	Panel D: Women with children in second generation				
Center exposure	.09**	1626.5			
	(.04)	(1114.9)			
Observations	$340,\!811$	340,399			
Pre-reform mean	11.1	145998			
	Panel E: Men with children in second generation				
Center exposure	.19***	5325.5**			
	(.06)	(2319.9)			
Observations	329,107	$330,\!400$			
Pre-reform mean	11.9	316593			

Table 1: Impacts of Center Exposure on First Generation Education and Labor Market Outcomes

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The estimation sample includes cohorts born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual in Panel A–C. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years	Average earnings
	of education	1967 - 2017
	(1)	(2)
	Panel A: Indi	vidual parental effects
Treated mother	0.07***	4424.2**
	(.02)	(2049.6)
Observations	771,078	772,768
Pre-reform mean	12.8	301679
Treated father	.01	1494.7
	(.02)	(2031.7)
Observations	738,613	739,996
Pre-reform mean	12.9	299120
	Panel B: Jo	pint parental effects
Treated mother/untreated father	.10***	3281.1
	(.03)	(2029.9)
Treated father/untreated mother	.04	-2344.3
,	(.03)	(2384.4)
Both parents treated	.07**	5079.0* [*]
-	(.04)	(2288.6)
Observations	492,207	492,872
Pre-reform mean	12.9	305094

Table 2: Impacts of First Generation Center Exposure on Second Generation Education and Labor Market Outcomes

Notes: Each parameter in Panel A is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. The parameters in Panel B are from a regression of the second generation outcome variable on a set of indicator variables covering the set of possible maternal-paternal/exposed-unexposed pairs. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988. The first row in Panel A uses a sample of individuals whose mothers were born between 1936 and 1955. The second row in Panel A is based on a sample of individuals whose fathers were born between 1936 and 1955. Panel B presents results based on a sample of individuals whose mothers and fathers were both born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years of education (1)	Academic high school track (2)	Average earnings 1967–2017 (3)	Relative age (4)	Partner treated (5)
	Panel A: Part	tner characteristic	s of women with chi	ldren in seco	ond generation
Center exposure	$.16^{***}$ (.06)	1.7^{***} (.56)	6274.6^{**} (2594.9)	12^{**} (.05)	9.47^{***} (1.02)
Observations	335,025	335,025	337,587	338,284	338,284
Pre-reform mean	11.7	35.1	300912	3.1	24.2
	Panel B: Pa	rtner characteristi	ics of men with child	lren in secor	nd generation
Center exposure	.04	.8	1402.3	.07*	10.57***
-	(.03)	(.64)	(1101.1)	(.04)	(.99)
Observations	$328,\!335$	328,335	328,669	330,203	330,203
Pre-reform mean	11.4	41.5	156362	-2.9	34

Table 3: Impacts of Center Exposure on First Generation Partner Characteristics

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and adjusted for clustering at the level of the municipality of birth. The sample includes first generation individuals born between 1936–1955, with second generation children born between 1961–1988. Most healthcare centers opened between 1936 and 1955 (1914 in Oslo). Our earnings outcome is average discounted earnings from 1967 to 2017. Academic high school track reflects the likelihood of enrolling in an academic rather than a vocational track and is multiplied by 100. All specifications include a full set of cohort and municipality fixed effects. The sample is based on individuals with children in second generation. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Partner G0 household education (1)	Partner G0 mother's education (2)	Partner G0 father's education (3)
	Panel A: Partner char	cacteristics of women w	ith children in second generation
Center exposure	.04 (.05)	03 (.02)	.02 (.03)
Observations	182,529	209,315	196,198
Pre-reform mean	16.0	7.7	8.3
	Panel B: Partner cha	aracteristics of men wit	h children in second generation
Center exposure	.08**	0	.05*
	(.04)	(.02)	(.03)
Observations	203,774	$224,\!655$	214,089
Pre-reform mean	16.2	7.8	8.4

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and adjusted for clustering at the level of the municipality of birth. The estimation sample is based on first generation individuals born between 1936–1955, with children born between 1961–1988. Most healthcare centers opened between 1936 and 1955 (1914 in Oslo). When information is available for both of the partner's parents' education then the partner's G0 household education is calculated as the sum of the partner's mother and father's education. If education data for both partner's parents' is missing, the observation is dropped. Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years of education (1)	Average earnings 1967–2017 (2)		
	Panel A: Baseline			
Treated mother	0.07***	4424.2**		
Observations	(.02) 771,078	(2049.6) 772,768		
Treated father	.01	1494.7		
Observations	(.02) 738.613	(2031.7) 739,996		
	Panel B: Contro	olling for G0 education		
Treated mother	.09* (.05)	7366.2* (3850.4)		
Observations	239,118	239,267		
Treated father	.02	2406.5		
Observations	239,119	239,268		
	Panel C: Contro	olling for G1 education		
Treated mother	.03	4081**		
	(.03)	(1993.2)		
Observations	489,606	490,240		
Treated father	.01	1461.5		
Observations	(.02) 489.606	(1055.7) 490.240		
	Panel D: Contr	olling for G1 earnings		
Treated mother	.05	3577.9*		
	(.03)	(1953.5)		
Observations	490,646	491,284		
Treated father	.02	1697.9		
	(.02)	(1657.2)		
Observations	490,646	491,284		
	Panel E: Controlling for G0 education, and G1 education and earnings			
Treated mother	01	5012.4		
	(.05)	(3666.5)		
Observations	237,829	237,972		
Treated father	02	1911		
Observations	(.03) 237,830	(2250.7) 237,973		

Table 5: Mediation Analysis: Impacts of First Generation Center Exposure on Second Generation Education and Labor Market Outcomes

Notes: Each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988, whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Household total years of education (1)	Household average earnings 1967–2017 (2)
Treated mother	.27**	8275.2**
	(.13)	(3814.1)
Observations	772,991	773,643
Pre-reform mean	18.1	327213
Treated father	.08	1949.9
	(.1)	(3221.9)
Observations	$740,\!576$	740,927
Pre-reform mean	19.4	413735

 Table 6: Impacts of First Generation Center Exposure on Household Resources Available

 to Second Generation Children

Notes: Each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988, whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Household level variables are calculated as the sum of the outcomes for the mother (father) and their partner. If either parent is missing information, the total is calculated based on the available data from the household member with complete information. Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years of education			
	Panel A: Ge	Panel A: Generation 0 to 1		
Father's years of education	.4	06***		
Center exposure	(. .41	.007) 58***		
Center exposure $\times {\rm Father}$'s years of education	.) 0 (28*** 008)		
Observations	(.008) 433,245			
	Panel B: Generation 0 to 2			
	Maternal grandfather Paternal grandfather			
Maternal/or paternal grandfather's education	0.144***	0.138***		
	(0.005)	(0.004)		
Treated mother/or father	0.171*** 0.097**			
	(0.053) (0.048)			
Treated mother/or father×G0's education	-0.012* -0.007			
	(0.006) (0.005)			
Observations	447,381	502,449		

Table 7: Policy-Induced Changes in Intergenerational Mobility in Education

Notes: Panel A is based on a sample of first generation individuals with children in the second generation. Column 1, Panel B provides intergenerational estimates for second generation children with treated versus untreated mothers, and Column 2, Panel B provides intergenerational estimates for second generation children with treated versus untreated fathers. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The first generation is born between 1936–1955, and the second generation is born between 1961–1988. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual. The second-generation specification also controls for second-generation year-of-birth fixed effects. Second-generation regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

A Online Appendix

Table A.1: Robustness Checks: Impacts of Center Exposure on First Generation Education and Labor Market Outcomes

	Total years of education	Average earnings 1967–2017	
	(1)	(2)	
	Excluding cohorts born during WWII		
	Panel A: Sample with	children in second generation	
Center exposure	.16***	3607.8**	
-	(.06)	(1789.9)	
Observations	522,169	522,921	
Pre-reform mean	11.5	233487	
	Panel B: Women with	children in second generation	
Center exposure	.11**	1792.9	
	(.05)	(1250.6)	
Observations	$266,\!644$	266,495	
Pre-reform mean	11.2	151038	
	Panel C: Men with ch	ildren in second generation	
Center exposure	.20***	4877.5*	
	(.07)	(2683.7)	
Observations	255,522	256,423	
Pre-reform mean	11.8	319294	
	Controlling	for school reform	
	Panel D: Sample with children in second generation		
Center exposure	.15***	3723.3**	
	(.05)	(1509.6)	
Observations	669,918	670,799	
Pre-reform mean	11.4	224380	
	Panel E: Women with	children in second generation	
Center exposure	.10**	1665.6	
	(.04)	(1118.6)	
Observations	340,811	$340,\!399$	
Pre-reform mean	11	139398	
	Panel F: Men with ch	ildren in second generation	
Center exposure	.19***	5243**	
	(.06)	(2317.4)	
Observations	329,107	330,400	
Pre-reform mean	11.8	311967	

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The estimation sample includes cohorts born between 1936–1955. In Panels A—D, cohorts born between 1939–1945 are excluded. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual in Panel A and D. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years of education (1)	Average earnings 1967–2017 (2)			
	Panel A: Excluding co	Panel A: Excluding cohorts born during WWII			
Treated mother	.05*	4332.5*			
	(.03)	(2467.6)			
Observations	589,210	590,370			
Pre-reform mean	12.9	299347			
Treated father	.02	931.4			
	(.03)	(2472.8)			
Observations	560,568	561,509			
Pre-reform mean	12.8	296084			
	Panel B: Controlling for school reform				
Treated mother	.07***	4381.8**			
	(.02)	(2061.8)			
Observations	771,078	772,768			
Pre-reform mean	12.8	301679			
Treated father	.01	1468.7			
	(.02)	(2045.1)			
Observations	738,613	739,996			
Pre-reform mean	12.9	299120			

 Table A.2: Robustness Checks: Impacts of First Generation Center Exposure on Second

 Generation Education and Labor Market Outcomes

Notes: Each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. In Panel A, mothers (fathers) born between 1939–1945 are excluded. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings.Significance levels: *** 1% level, ** 5% level, * 10% level.

	Birth weight (1)	Low birthweight (2)	Premature (3)
Treated mother	6.37	-0.05	-0.07
	(6.73)	(0.18)	(0.26)
Observations	$617,\!061$	$617,\!061$	$592,\!848$
Pre-reform mean	3503	4.1	8.9
Treated father	6.23	-0.36**	0.03
	(6.09)	(0.16)	(0.23)
Observations	$639,\!840$	$639,\!840$	612,777
Pre-reform mean	3507	4.1	8.7

 Table A.3: Impacts of First Generation Center Exposure on Second Generation Health

 Outcome

Notes: Each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Low birthweight measures the probability of being born below the 2500 gram cutoff. Premature measures the probability of being born before the 37th week of pregnancy. Outcome variables in Columns 2 and 3 are multiplied by 100. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Ever having a child (1)	Number of children (2)	Age at first child (3)	Moved to urban area (4)	Moved out of low income area (5)
	Pane	el A: Women	with childre	n in second ge	eneration
Center exposure	09	05**	-0.11*	3.13**	6.75***
	(.27)	(.02)	(.06)	(1.32)	(1.74)
Observations	387,515	$342,\!220$	342,220	$342,\!186$	$342,\!186$
Pre-reform mean	90.8	2.6	23.5	21.2	21.3
	Pa	Panel B: Men with children in second generation			
Center exposure	.70*	04***	20**	1.57**	5.26***
-	(.39)	(.02)	(.08)	(.72)	(1.5)
Observations	400,916	330,470	330,470	330,443	330,443
Pre-reform mean	85.4	2.5	26.2	19	21.2

Table A.4: Impacts of Center Exposure on First Generation Fertility and Mobility

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The estimation sample includes cohorts born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Ever having a child measures the probability of having a child at any point. Moving to an urban area and moving out of a low-income area measure the probability of relocating before the first child. Outcome variables in Columns 1, 4 and 5 are multiplied by 100. All specifications include a full set of cohort and municipality fixed effects. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.5: Infant Mortality Bounds: Impacts of Center Exposure on First Generation's Total Years of Education

	Baseline (1)	Dropping 60 th percentile (2)	Dropping 70 th percentile (3)	Dropping 80 th percentile (4)	Dropping 90 th percentile (5)	Dropping 100 th percentile (6)
		Pane	l A: Sample with	children in secon	d generation	
Center exposure	.14***	$.14^{***}$.14***	.14***	.14***	$.14^{***}$
Observations	669,918	663,408	663,462	665,350	663,366	663,618
	Panel B: Women with children in second generation					
Center exposure	.09** (.04)	$.09^{**}$ (.04)	$.09^{**}$ (.04)	$.09^{**}$ (.04)	$.09^{**}$ (.04)	$.10^{**}$ (.04)
Observations	340,811	339,143	337,739	338,384	337,314	337,456
	Panel C: Men with children in second generation					
Centre exposure	$.19^{***}$ (.06)	.18*** (.06)	.19*** (.06)	$.19^{***}$ (.06)	.18*** (.06)	$.18^{***}$ (.06)
Observations	$329,\!107$	326,000	324,812	328,064	$325,\!614$	326,036

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The estimation sample includes cohorts born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Column 1 show baseline estimates from Table 1 Column 1. In Column 2 we drop individuals in the 60th percentile of the predicted education distribution. In Columns 3–6 this procedure is repeated for individuals in the 70th, 80th, 90th and 100th percentile of the predicted education distribution. All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual in Panel A. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.6: Infant Mortality Bounds: Impacts of First Generation Center Exposure on Second Generation's Total Years of Education

	Baseline (1)	Dropping 60 th percentile (2)	Dropping 70 th percentile (3)	Dropping 80 th percentile (4)	Dropping 90 th percentile (5)	Dropping 100 th percentile (6)	
Treated mother	.07***	.07***	.07***	.07***	.07***	.07***	
	(.02)	(.03)	(.03)	(.02)	(.03)	(.03)	
Observations	771,078	763,263	763,421	763,322	763,333	$763,\!450$	
Treated father	.01	.00	.01	.01	.01	.00	
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	
Observations	$738,\!613$	$731,\!325$	$730,\!683$	$730,\!628$	$730,\!655$	730,721	

Notes: Each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Column 1 show baseline estimates from Table 2 Column 1. In Column 2 we drop individuals in the 60th percentile of the predicted education distribution for mothers (fathers). In Columns 3–6 this procedure is specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years of education (1)	Average earnings 1967–2017 (2)	
	Panel A: Sample with c	hildren in second generation	
Center exposure	.13***	3415.5**	
	(.04)	(1457.6)	
Observations	564,912	565,492	
Pre-reform mean	11.2	219854	
	Panel B: Women with children in second generation		
Center exposure	.06	402.3	
	(.04)	(1264.3)	
Observations	$283,\!837$	$283,\!325$	
Pre-reform mean	10.9	136305	
	Panel C: Men with children in second generation		
Center exposure	.21***	6064.4***	
	(.06)	(2304.3)	
Observations	281,074	282,166	
Pre-reform mean	11.6	303969	

Table A.7: Impacts of Center Exposure on First Generation Education and Labor Market Outcomes for Non-Movers

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The estimation sample includes cohorts born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Our earnings outcome is average discounted earnings from 1967 to 2017. Non-movers are defined as individuals living in their municipality of birth after having their first child. All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual in Panel A. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years of education (1)	Average earnings 1967–2017 (2)
Treated mother	.06**	5362**
	(.03)	(2279.4)
Observations	$651,\!271$	$652,\!827$
Pre-reform mean	12.8	300770
Treated father	.02	2019.4
	(.02)	(2406.3)
Observations	$637,\!876$	639,121
Pre-reform mean	12.8	302753

Table A.8: Impacts of First Generation Center Exposure on Second Generation Education and Labor Market Outcomes for Non-Movers

Notes: Each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Our earnings outcome is average discounted earnings from 1967 to 2017. Non-movers are defined as mothers (fathers) living in their municipality of birth after having their first child. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Primary education (1)	High school (2)	Academic high school track (3)	Advanced education (4)
	Panel A:	Women wi	th children in seco	ond generation
Center exposure	1.92 (1.34)	1.45^{**} (.64)	1.85^{**} (.8)	09 (.21)
Observations	340,811	340,811	340,811	340,811
Pre-reform mean	88.1	27.2	38.7	5.1
	Panel B	: Men with	children in secon	d generation
Center exposure	2.53**	3.1***	2.27***	.35
	(1.16)	(.93)	(.64)	(.36)
Observations	$329,\!107$	$329,\!107$	329,107	$329,\!107$
Pre-reform mean	88	49.2	37.6	10

Table A.9: Impacts of Center Exposure on First Generation Education Outcomes

Notes: Each parameter is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The estimation sample includes cohorts born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Primary education indicates the probability of completing nine years of schooling, high school corresponds to completing 12 years, academic high school track reflects the likelihood of enrolling in an academic rather than a vocational track, and advanced education represents the probability of completing 16 years of schooling. Outcome variables in Column 1–4 are multiplied by 100. All specifications include a full set of cohort and municipality fixed effects. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.10: Impacts of Center Exposure on First and Second Generation Education	on and
Labor Market Outcomes for a Sample with Non-Missing Information on G0 Father	r's Ed-
ucation	

	Total years of education (1)	Average earnings 1967–2017 (2)	
	Panel A: Sample with children in second generation		
Center exposure	.23***	9580.7***	
	(.08)	(3057.6)	
Observations	433,245	434,045	
Pre-reform mean	11.7	245451	
	Panel B: Women with	children in second generation	
Center exposure	.16***	5354.2***	
	(.06)	(2060.5)	
Observations	203,090	203,206	
Pre-reform mean	11.4	158404	
	Panel C: Men with children in second generation		
Center exposure	.28***	11398.8***	
	(.1)	(3993.7)	
Observations	$230,\!155$	$230,\!839$	
Pre-reform mean	11.9	317883	
	Panel D: S	econd generation	
Treated mother	0.08***	5647.4**	
	(0.03)	(2376.49)	
Observations	447,381	448,159	
Pre-reform mean	12.9	295202	
Treated father	0.03	3366.5^{*}	
	(0.03)	(2029.38)	
Observations	323,290	$323,\!592$	
Pre-reform mean	12.9	293588	

Notes: Each parameter in Panel A-C is from a separate regression of the outcome variable on a variable indicating access to a mother and child healthcare center. In Panel D each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the (first generation's) municipality of birth. The estimation sample includes cohorts born between 1936–1955 in Panel A-C. In Panel D, the estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. In Panel A-C, individuals with missing information on fathers education (G0) are excluded. In Panel D, mothers (fathers) with missing G0 education are excluded. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of (first generation) cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual in Panel A. Panel D additionally includes second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions in Panel D are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

	Total years of education (1)	Average earnings 1967–2017 (2)	
	Panel A: Baseline		
Treated mother	0.07***	4424.2**	
	(.02)	(2049.6)	
Observations	771,078	772,768	
Treated father	.01	1494.7	
	(.02)	(2031.7)	
Observations	738,613	739,996	
	Panel B: Control	ling for G0 education	
Treated mother	.10***	5044.6**	
	(.03)	(2244.3)	
Observations	771,078	772,768	
Treated father	.02	1700.4	
	(.03)	(2135.7)	
Observations	738,613	739,996	
	Panel C: Control	ling for G1 education	
Treated mother	.04*	3669*	
	(.02)	(1873)	
Observations	771,078	772,768	
Treated father	01	1174.3	
	(.02)	(1798.1)	
Observations	738,613	739,996	
	Panel D: Controlling for G1 earnings		
Treated mother	.06**	3955.4^{*}	
	(.02)	(2085.7)	
Observations	771,078	772,768	
Treated father	.0	1347	
	(.02)	(1829.6)	
Observations	738,613	739,996	
	Panel E: Control	ling for G0 education,	
	and G1 education and earnings		
Treated mother	.05**	3324.3*	
	(.02)	(1878.7)	
Observations	771,078	772,768	
Treated father	0	1034.9	
	(.02)	(1658.8)	
Observations	$738,\!613$	739,996	

Table A.11: Mediation Analysis: Impacts of First Generation Center Exposure on Second Generation Education and Labor Market Outcomes when Controlling for Missings in G0 and G1 Education and Earnings

Notes: Each parameter is from a separate regression of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the first generation's municipality of birth. The estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

Table A.12: Policy-Induced Changes in Intergenerational Mobility in Education when Controlling for G1 Education

	Total years of education		
	Panel A: Generation 0 to 2		
	Maternal grandfather	Paternal grandfather	
Maternal/or paternal grandfather's education	0.054***	0.044***	
	(0.004)	(0.004)	
Treated mother/or father	0.121^{***}	-0.010	
	(0.046)	(0.039)	
Treated mother/or father×G0's education	-0.011**	0.000	
	(0.005)	(0.004)	
Observations	447,381	$502,\!449$	

Notes: Column 1, Panel A provides intergenerational estimates for second generation children with treated versus untreated mothers, and Column 2, Panel A provides intergenerational estimates for second generation children with treated versus untreated fathers. Robust standard errors are shown in parentheses and are adjusted for clustering at the level of the municipality of birth. The first generation is born between 1936–1955, and the second generation is born between 1961–1988. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, a dummy variable controlling for the gender of the second generation individual, and a control variable for the mother's (father's) education. All regressions are weighted by the number of siblings. Significance levels: *** 1% level, ** 5% level, * 10% level.

Figure A.1: Impacts of Center Exposure on First Generation Results for Total Years of Education: Callaway and Sant'Anna



∾ -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 11 Years since first exposure ◇ Callaway–Sant'Anna ATT: 0.69 (0.11))

(c) Men with children in second generation

Notes: Each figure presents event-study point estimates (circles) and 95% confidence intervals (whiskers) for relative time periods t = -5 to t = +10, using Callaway and Sant' Anna estimation method. More extreme relative time periods are estimated but not shown in the figures. The estimation sample includes cohorts born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual in Figure A.

Figure A.2: Impacts of Center Exposure on First Generation Results for Average Earnings 1967–2017: Callaway and Sant'Anna



(c) Men with children in second generation

Notes: Each figure presents event-study point estimates (circles) and 95% confidence intervals (whiskers) for relative time periods t = -5 to t = +10, using Callaway and Sant' Anna estimation method. More extreme relative time periods are estimated but not shown in the figures. The estimation sample includes cohorts born between 1936–1955. Most healthcare centers opened between 1936 to 1955 (1914 in Oslo). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of cohort and municipality fixed effects, and a dummy variable controlling for the gender of the individual in Figure A.

Figure A.3: Impacts of First Generation Center Exposure on Second Generation Results for Treated Mothers: Callaway and Sant'Anna



(b) Average earnings 1967–2017

Notes: Each figure presents event-study point estimates (circles) and 95% confidence intervals (whiskers) for relative time periods t = -5 to t = +10, using Callaway and Sant' Anna estimation method. Point estimates show the effect of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. The estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings.

Figure A.4: Impacts of First Generation Center Exposure on Second Generation Results for Treated Fathers: Callaway and Sant'Anna



(b) Average earnings 1967–2017

Notes: Each figure presents event-study point estimates (circles) and 95% confidence intervals (whiskers) for relative time periods t = -5 to t = +10, using Callaway and Sant' Anna estimation method. Point estimates show the effect of the second generation outcome variable on a variable indicating whether the individual's mother (father) had access to a mother and child healthcare center. The estimation sample includes second generation cohorts born between 1961–1988 for a sample of individuals whose mothers (fathers) were born between 1936 and 1955. Most healthcare centers opened between 1936 and 1955 (Oslo in 1914). Our earnings outcome is average discounted earnings from 1967 to 2017. All specifications include a full set of first generation cohort and municipality fixed effects, second generation year-of-birth fixed effects, and a dummy variable controlling for the gender of the second generation individual. All regressions are weighted by the number of siblings.